Why Are All-Star Analysts Influential?

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December 2024

Abstract:

Evidence indicates *Institutional Investor's (I/I)* star analysts are more influential than non-stars, but the reason is unclear. We investigate two hypotheses: influence stems from *status* or superior *quality*. Our findings support the quality hypothesis, showing no evidence that status affects influence. Analysts making influential recommendations are likelier to become stars. We introduce *"blunder"* as negative performance measure, demonstrating how voters in *I/I's* ranking can balance their interests with *I/I's* by adjusting weights on influential recommendations and blunders. This system encourages reliability in ordinary analysts and bold research from stars. Additionally, we observe seasonal voting pattern consistent with seasonal analyst behavior.

Keywords: All-Star Analysts; Security Analysts; Stock Recommendations

JEL Codes: G14, G20, G24, M49.

[1] We thank seminar participants at Korea University, University of Melbourne, University of Oklahoma, Texas Tech University, and Wake Forest University. For their comments, we thank Kose John, William Megginson, Marios Panayides, Ajay Patel, Tyler Ransom, Pradeep Yadav, and Mu-Jeung Yang. Aharoni is at gaharoni@unimelb.edu.au, Lim is at blim@unimelb.edu.au, Litov is at litov@ou.edu, Price is at richard.price@ou.edu, and Zhang is at zhanglin@rowan.edu. All errors are our own.

1. Introduction

Ample empirical evidence suggests that "star" analysts – analysts who are voted as "All-American" in the annual survey of *Institutional Investor* (*I/I*) magazine – are more influential than ordinary analysts. This finding is reported across various dimensions, including price impact (Stickel, 1992), buying activity (Irvine, Lipson, and Puckett, 2007), and cost of capital (Kelly and Ljungqvist, 2012). Most relevant for our paper, Loh and Stulz (2011) develop a quantifiable metric of analyst influence – recommendation revisions that generate a significant market response (henceforth, "influential recommendations" or *IR*) – and show that star analysts are more likely to be influential than non-stars. However, the literature does not provide a definitive explanation for why star analysts exert such influence. The empirical challenge is that an analyst's star status and influence are measured simultaneously, complicating the identification of causality.

In this study, we employ a novel research design to investigate the relation between influence and star status. Building upon the regression framework and the *IR* taxonomy from Loh and Stulz (2011), we exploit both changes in star status and the time lag between the voting on and the publication of star status to determine the direction(s) of causality. Our key finding is that star status itself does not have any measurable effect on analysts' influence. Instead, we find the opposite causal relation: influential recommendations are correlated with subsequent selection as a star. Stickiness in star selection gives rise to the documented correlation between current star status and *IR*. Drilling down into the star selection process, we find that first-time stars are selected for their reliability, while reigning stars are incentivized to produce bold research. We posit that voters use this discriminatory criterion to satisfy their and *I/I*'s competing interests.

The motivating question for our paper follows its title: Why are star analysts influential? We test two non-mutually exclusive hypotheses. The first, which we call *status*, posits that the

higher influence of star analysts is due to their star status itself, rather than the quality of their research output. The second, which we call *quality*, posits that analysts who produce higher-quality research are more likely both to be selected as stars and to have stronger influence. In this context, "quality" represents the intersection of an analyst's innate cognitive ability, effort, interpersonal skills, and brokerage resources. While these factors vary over time, the premise of the *quality* hypothesis is that the aggregation of them is serially correlated, which leads to a contemporaneous correlation between star status and influence. In the context of analyst research, the *status* hypothesis emphasizes the importance of *who* writes the research, while the *quality* hypothesis emphasizes the importance of *what* is contained in the research.

Existing literature is ambiguous about whether one hypothesis dominates the other. Considerable circumstantial evidence supports *status*. Gleason and Lee (2003) report that recommendations from stars are incorporated in stock prices faster than comparable recommendations from non-stars. Emery and Li (2009) argue that star status generates recognition and suggest that stars are likely to have higher influence because of this higher recognition. Aharoni et al. (2023) show that acquirers accept a low-synergy deal when a star analyst is affiliated with the acquirer's advisor and covers the target. Beyond the realm of analysts, Gold, Kadous, and Leiby (2024) find that auditors mistakenly believe high-status specialists are more competent in an experimental setting than low-status specialists. Conversely, both Leone and Wu (2007) and Fang and Yasuda (2014) argue that star analysts are elected for their higher skills, consistent with *quality*. Fang and Yasuda (2008) suggest that star analysts are less likely to bias their reports in response to their brokerage house needs because they are more concerned about their personal reputations, implying that, consistent with *quality*, their status is conditional on the caliber of their research output.

Our identification strategy centers on changes in star status. While both *status* and *quality* predict higher influence for current stars, they differ in their predictions when an analyst changes their status. Consider, for example, an analyst who is demoted from being a star in year *t*. Votes are collected by *I/I* around May of year *t*, with voting presumably based on the analyst's performance since the last vote was collected in May of *t-1*. *I/I* publishes the results much later, in October of *t*. The *status* hypothesis implies that the analyst will lose their influence after the demotion becomes public in October of *t*. However, the *quality* hypothesis implies that the analyst's influence will have diminished well before the publication date – during the evaluation period ending in May of *t*. According to *quality*, it is the lower caliber of the analyst's recommendations that affects both the analyst's diminished influence and subsequent loss of star status.

In our empirical analyses, we find no evidence in support of *status* and strong evidence in support of *quality*. We begin by replicating the result in Loh and Stulz (2011) that recommendation revisions from star analysts are more likely to be influential than those from non-stars. Having established that baseline result, we then test the *status* hypothesis. We focus on two groups that experience the most consequential changes in status – stars who are demoted and non-stars who become stars for the first time[2] – and compare the likelihood of *IR* in the periods before and after the publication of their status change. The results indicate that a change in status does not affect the probability of issuing *IR*. Specifically, recommendations from a demoted star do not become less influential after the demotion is publicly announced, and recommendations from a first-time star do not become more influential after the promotion becomes public. The results are robust to different windows before and after the publication of the *I/I* list. We also use a stacked difference-

[2] We exclude observations involving non-star analysts who have previously been stars but have since been demoted, given the potential ambiguity of their perceived status.

in-difference design to compare demoted (promoted) stars to their non-demoted (non-promoted) peers before and after the publication of their next-year's status. Again, the results do not support the *status* hypothesis.

We next test the *quality* hypothesis by effectively inverting the *status* tests and investigating whether analyst performance affects their subsequent star status. Our goals are twofold. First, we ask whether higher influence leads to a higher probability of star selection. Second, we attempt to identify which parts of analysts' performance other than influence that might be perceived as high quality by voters. For the first goal, we follow the insight of Loh and Stulz (2011) that while the market reaction to most recommendations is small and indistinguishable from noise, an important minority of them is followed by a large market reaction, and we posit that these recommendations are consequential to analysts' careers. Our key empirical innovation is to isolate these recommendations, aggregate them over the annual voting evaluation period, and then test whether this annual aggregate predicts future star status. Formally, we regress analysts' star status in year *t* on *Annual_IR*, an indicator variable equaling 1 if the analyst had at least one *IR* in the 12-month evaluation period ending in May of *t*. We find that the coefficient on *Annual_IR* is positive and significant, suggesting that having at least one *IR* increases the likelihood of being elected as a star in the subsequent year, consistent with the prediction of *quality*.

For the second goal of identifying which parts of analysts' performance are perceived as high quality by voters, we note that a voting criterion based solely on *IR* will incentivize analysts to gamble by making high-risk recommendations. Since voters attempt to identify the best analysts rather than the luckiest analysts, using *Annual_IR* as the only criterion is insufficient. With this in mind, we introduce a novel performance measure called *Annual_blunder*, an indicator variable that equals 1 if the analyst had at least one recommendation that was followed by a sharp price

movement in the *opposite* direction. Blunders take the intuition in Loh and Stulz (2011) to the opposite tail of the *IR* distribution. When we include *Annual_blunder* in our regression, we find that the coefficient on *Annual_IR* remains positive and significant, while the coefficient on *Annual_blunder* is negative and significant. Notably, the magnitude of coefficient on *Annual_blunder* is roughly twice as large as that on *Annual_IR*, suggesting that voters strongly deter analysts from playing a high-risk gambling strategy that would generate both *IR* and blunders. When we control for the average abnormal announcement return, the magnitude and significance of the coefficients on *Annual_IR* and *Annual_blunder* remain intact, while the coefficient on the average announcement return is statistically insignificant. The results are consistent with Loh and Stulz (2011) by showing that voters measure analysts based on their most visible and consequential recommendations.

The *I/I* ranking is unique in that analysts are elected not by a specific, observable criteria but by a vote of buy-side professionals. There is no explicit compensation to voters, but one possibility is that their voting reflects the interests of not only *I/I* magazine but also themselves. The tests for the *quality* hypothesis suggest that *I/I* and its readership prefer the best, rather than the luckiest, analysts to be selected as stars. However, fund managers and buy-side analysts also rely on sell-side analysts for their information. The literature has documented multiple channels through which information production by sell-side analysts benefits the buy-side analysts. For example, Irvine et al. (2007) and Klein, Saunders and Wong (2019) show that institutional investors and hedge funds benefit when analysts, especially star analysts, "tip" their recommendations to institutions prior to announcing them publicly. Brown et al. (2016) interview buy-side analysts and report that they often meet with high-quality sell-side analysts to gather firm specific and industry information. Hobbs and Singh (2015) find that buy-side recommendations

are most profitable when stocks have high analyst coverage and low dispersion in earnings forecasts. In all these cases, buy-side professionals are likely to benefit more from information that is timely, bold and related to firms with a weak information environment.[3]

Based on the above discussion, the interests of the *I/I* magazine and the voters are not aligned: penalizing blunders to identify high-quality analysts incentivizes competing analysts to be conservative, which is contrary to the voters' objectives. To satisfy both their own and the magazine's interests, one possibility is that voters apply different criteria to different analyst cohorts. We separate analysts into two groups to investigate this possibility. The first group, "Never Stars", consists of analysts who have not been named as a star within the last five years. The second group, "Current Stars", consists of analysts who are stars at the year of *I/I* publication. Our results indicate that voters value reliability for Never Stars. Never Stars are judged both on *IR* and *blunder*, but the penalty for the latter is larger in magnitude than the benefit from the former. However, once analysts have sufficiently demonstrated their reliability and been elected as stars, the criteria for re-election change. We find that Current Stars are judged only on *IR*, not *blunder*. The results across the two analyst groups suggest that voters use a two-competition structure in which they can both identify quality analysts who are first-time stars by punishing blunders, and at the same time incentivize analysts who have been elected as stars to adopt bolder strategies by *not* punishing blunders. This voting strategy provides a parsimonious explanation for the findings in Jung, Lu, and Wu (2019) that newly-elected stars adopt bolder strategies and add harder-to-predict stocks to their portfolios: Stars play the strategy which voters value.

[3] Several papers support the view that buy side analysts benefit from high quality sell-side analysts' stock reports. For example, Hobbs and Singh (2015) show that buy-side portfolios with greater sell-side analyst coverage have higher abnormal returns. Survey evidence in Williams, Moyes and Park (1996) shows that buy-side analysts find sell-side analyst reports to be more important than private information from management. Survey evidence from Boni and Womack (2002) documents (their Table 3) that among the most valuable sell-side attributes is the production of detailed research report.

The preceding results suggest that certain analyst behavior documented in the literature may be driven in part by the criteria of *I/I* voters. For example, both Dolvin, Pyles and Wu (2009) and Lo and Wu (2018) report that analysts are more optimistic and less accurate during spring and summer months. Our findings suggest that stars and ordinary analysts are ranked differently, but *I/I* rankers only know which analysts are selected as stars in October, following publication of the latest *I/I* rankings. One possibility, then, is that the rankers concentrate more on the latter half of the evaluation period, after October. Accordingly, we split the evaluation year into two half-years, one prior to and one after publication in October, and recalculate our *IR*- and *blunder*-based indicator variables for each half-year. We find that performance in the later half-year – most prominently, blunders by non-star analysts and *IR* by star analysts – are extremely important to analysts' likelihood of (re)election. The lesser importance of the first half-year, which spans spring and summer, may explain why analysts are more optimistic and less accurate during this period.

Our paper makes several contributions to the literature. First, we show that the elevated influence of star analysts' recommendations is not driven by their status. Prior literature on the effects of star status suggests that star analysts' influence is due to their status (e.g., Emery and Li, 2009; Gleason and Lee, 2003). We find no evidence that status itself affects analysts' influence. Instead, we find that analyst performance is positively correlated with the probability of becoming a star. A natural interpretation is that investors process analyst research output on the merits of the output itself, not the status of its author.[4]

Second, we document the salience of extreme results when evaluating analysts. We show that Loh and Stultz's (2011) intuition about the importance of the right tail of analyst

[4] This interpretation presumes that the channel through which star status affects influence is investors. However, there may be other channels, such as the behavior of the analyst themselves or of their competitors, that lead to alternative interpretations.

recommendations *(IR)* extends to the left tail of the distribution as well (*blunder*). Our results indicate that an analyst's consequential recommendations matter to voters, not his or her average or aggregate performance. As such, we shed light on two seemingly opposing strands of literature on star selection. One strand shows that quantitative measures predict star selection (e.g. Stickel, 1992; Leone and Wu, 2007). The other strand documents that voters themselves claim they value *qualitative* characteristics of analysts (Brown et al., 2015; Brown et al., 2016). Our results indicate that voters value *IR* and *blunder* but not average announcement returns, and we provide a parsimonious connection between the two seemingly opposing views. *IR* and *blunder* represent notable and memorable outcomes that voters may not consciously classify as quantitative but nevertheless value when voting. For example, voters routinely rank "professionalism" as a top attribute influencing the *I/I* rankings (Bradshaw 2011; Bradley et al. 2017), and one possibility is that voters consider a high-variance gambling strategy that produces blunders to be unprofessional.

Finally, by focusing on extreme observations, we identify sophisticated voting criteria through which voters cater to the interests of both *I/I* and themselves. Our paper is the first to argue that the interests of multiple participants in the *I/I* tournament affect the criteria used to select analysts. We document how voters can not only select high-quality analysts to become first-time stars but also simultaneously incentivize those who have already been identified as high-quality analysts (i.e., stars) to produce bold research simply by altering the weights on *IR* and *blunder* for different analyst cohorts. Furthermore, our paper is the first to document that the voting criteria exhibits a seasonal component, one which is largely consistent with seasonal trends previously documented in the literature. Overall, our results suggest that voters are more creative and strategic in their criteria than has been previously understood in the literature.

The remainder of this paper is organized as follows. In Section 2, we provide institutional background information and discuss the data. In Section 3, we investigate the *status* and *quality* hypotheses. In Section 4, we test whether voters additionally serve their own interests through their voting. Section 5 investigates seasonality in voting criteria. Section 6 concludes.

2. Institutional Background and Data

2.1 *Institutional Investor* Rankings

Each year, *Institutional Investor* magazine publishes rankings of sell-side analysts ranked in the top three in an industry to an "All-America Research Team" with others chosen as runners-up. To create its rankings, *I/I* sends a questionnaire to directors of research and fund managers and asks them to vote for sell-side analysts who provided the best service during the year. In 2020, *I/I* received responses from more than 3,667 individuals from 1,446 firms.[5] To identify eligible voters, heads at the brokerage houses submit client lists to *I/I* before the voting period. Brokerages may also inform investors who may be interested and eligible to vote and refer them to *I/I*; these investors contact *I/I* directly and *I/I* determines their voting eligibility. An eligible voter (whether an investor or a director/manager affiliated with a brokerage) must meet an undisclosed threshold of assets under management.

The polling period starts about five to six months before publication and lasts four weeks. The rankings are based on analyst performance in the months before the end of the voting period, typically in May. Voting is confidential and results are published in October. Analysts are grouped by industry and each analyst receives a composite score, ranging from 0 to 40 and commissions or assets under management are used as weights. *I/I* then determines the ranks using these scores and

[5] See https://www.iiresearch.com/article/2cztop8v1yu851ujaoglc/the-new-stars-of-the-52nd-annual-all-america-research-team for detailed information regarding the 2023 All-America Research Team.

weights. The sell-side analysts in the first three places are named "All-Stars", and all other analysts whose scores are within 35% of third place are named "runner-up."

2.2 Data

Our data is comprised of four primary datasets over the period from 2001 to 2020. First, we obtain the names of star analysts from *I/I*'s annual reporting of the outcome of the all-star analyst vote. Following Loh and Stulz (2011), our measure of star status includes all analysts ranked in the top 3 as well as runners up. Second, we obtain analyst recommendations from IBES and merge the all-star status with IBES data for the period during which the all-star status is effective (from November 1 of year *t* to October 31 of year *t+1*). The process of matching IBES and *I/I* data involves a comparison of analyst names and brokerages reported in both sources. IBES analyst information includes the last name and first initial along with the brokerage that employs the analyst. *I/I* includes the full first and last names along with the industry sector in which the analyst received star status as well as the brokerage house that employs the analyst. The merge of these two datasets was checked extensively for accuracy. Third, we obtain financial statement information from Compustat and use the data from the fiscal quarter preceding the recommendation in our tests. Fourth, we obtain stock returns from CRSP. We utilize CRSP returns to measure firm-specific volatility and returns following analyst recommendations.

2.3 Key Performance Measures

The key measures of analyst performance in our paper are based on Loh and Stultz's (2011) taxonomy of "influential recommendations" (*IR*): recommendation revisions that are accompanied by significant market reactions in the same direction as the revision. That is, an upgrade

(downgrade) is accompanied by significant positive (negative) abnormal returns. A revision is determined by comparing one recommendation with the preceding recommendation for the same firm issued by the same analyst. We require that the prior recommendation is no older than 365 days, such that the measured revision is meaningful. We calculate the recommendation change, $recch_{ijt}$, as the difference between the recommendation at time *t* for firm *j* issued by analyst *i*, rec_{ijt}, and the preceding recommendation issued within the past 365 days, rec_{ijt-1}; rec_{ijt} is coded from 1 to 5 with values of 1 (5) representing strong sell (buy) recommendations.

Loh and Stulz (2011) argue that using average abnormal cross-sectional returns may not be a meaningful way to determine whether analyst recommendations are influential. Rather, they determine whether a specific analyst's recommendation is influential by calculating a z-statistic comparing the two-day abnormal return to return volatility. The abnormal return, $abret_{ijt}$, is measured following Loh and Stulz (2011) for the (0, +1) return window. Residual volatility, $\sigma_{\varepsilon ijt}$, is measured over a three-month period from day -69 to day -6 for each analyst-recommendation observation.[6] If the abnormal return is in the same direction as the recommendation change, and the ratio of the abnormal return to $\sigma_{\varepsilon ijt}*\sqrt{2}$ exceeds 1.96, then the recommendation is considered influential. The following shows the determination of influential recommendation revisions, $influential_{ijt}$:

$$influential_{ijt} = 1 \; if \left|\frac{abret_{ijt}}{\sigma_{\epsilon ijt\sqrt{2}}}\right| > 1.96 \text{ and } sign(abret_{ijt}) = sign(recch_{ijt})$$

$$influential_{ijt} = 0 \; otherwise$$

[6] Loh and Stulz calculate the abnormal return following Daniel, Grinblatt, Titman, and Wermers (2012) (henceforth DGTW) and the volatility of abnormal returns using the Fama-French three-factor model. For two reasons, we use the Fama-French model for both calculations. The first is to impose consistency within the calculated z-statistic. The second is that we are calculating daily returns, and the standard procedure for calculating DGTW returns corresponds to monthly returns.

We take Loh and Stulz's (2011) intuition to the opposite end of the distribution and generate a novel performance variable, *blunder*. *Blunder* is constructed similarly to *influential* but requires the sign of the abnormal announcement return to be the *opposite* of the sign of the change in recommendation. Put simply, *blunder* represents an extremely poorly received recommendation change. Given the unlikeliness of extreme price movements in the opposite direction of the recommendation, we employ a lower threshold for *blunder* than *influential*:

$$blunder_{ijt} = 1 \; if \; \left|\frac{abret_{ijt}}{\sigma_{\epsilon ijt\sqrt{2}}}\right| > 1.645 \text{ and } sign(abret_{ijt}) = -sign(recch_{ijt})$$

$$blunder_{ijt} = 0 \; otherwise$$

Following Loh and Stulz (2011), we exclude recommendations where multiple analysts issued recommendations on the same day. Also, recommendations that confirm a past recommendation (i.e., $recch_{ijt}$ = 0) are excluded from the analysis to allow for unambiguously measuring a change in analyst expectations. Finally, we exclude recommendations issued in the 3-day window surrounding either earnings announcements or management earnings guidance. Thus, we are able to attribute the reaction to the analyst as opposed to other events that are likely to occur concurrently with recommendations.

3. Status vs Quality

The focus of our paper is to understand why star analysts are influential, and we investigate two non-mutually exclusive hypotheses. The first hypothesis, *status*, states that star status itself drives high influence. According to *status*, after being selected as a star, the analyst becomes more influential. One possibility is that star status provides analysts with more exposure. Another one is that markets perceive star status as an indicator of high quality and that stars' recommendations are better informed, regardless of whether star status truly reflects their quality (Gold, Kadous, and

Leiby 2024). Whatever the underlying mechanism might be, we do not take a stance.[7] The second hypothesis, *quality,* states that higher-quality analysts tend to have more influential recommendations and are more likely to be selected as stars. Here, "quality" represents not only the analyst's innate ability and work ethic but also their interpersonal skills and brokerage resources. We assume the sum of them is dynamic but serially correlated. According to the *quality* hypothesis, star analysts are more influential because of their high quality, and their influence is an outcome of their superior performance.

While both hypotheses predict higher influence for current star analysts, they differ in their predictions when analysts change their status. The *status* hypothesis states that analysts' influence is contemporaneously correlated with their status, implying that an analyst's influence could change only after the publication of their status in October. The *quality* hypothesis suggests that changes in star status are driven by changes in the analyst's performance. However, for this change in performance to affect the published status of the analyst in October in year *t*, it must happen before the end of the voting period, which is typically in April or May of year *t*. The precise evaluation interval used by *I/I* voters is unknown, but given the annual nature of these rankings, we assume that it covers the 12 months before the end of the voting period (i.e., May_{t-1} to April_t), consistent with the approach used by Leone and Wu (2007).[8]

Figures 1 and 2 illustrate the timeline in which each hypothesis predicts a change in influence for promoted (Figure 1) and demoted (Figure 2) analysts. For promoted analysts (Figure

[7] The channel does not need to be investors. One possibility is that analysts themselves put more effort after becoming stars. While such behavior certainly qualifies as the analysts' quality improving, the predictions from this mechanism still follow the *status* hypothesis.

[8] Leone and Wu (2007) use a pre-regulation Fair Disclosure sample to examine whether star analysts are lucky or high quality. They examine the performance of first-time stars before and after the end of the voting period in April. Their results show that newly elected stars have higher profitability of recommendations and higher accuracy compared with ordinary analysts. They do not find material differences between the period before and after the end of the voting period.

1), the status hypothesis predicts that the influence increases after the publication of the rankings in October, indicated by a solid line showing a sharp rise after publication. In contrast, the quality hypothesis predicts that increased influence should occur before the publication of the rankings, represented by a dashed line showing an earlier rise in influence for promoted analysts. Similarly, for demoted analysts (Figure 2), the *status* hypothesis predicts a decrease in influence after rankings are published in October, while the quality hypothesis predicts a reduction in influence during the evaluation period ending in April.

3.1 *Status* Hypothesis

To investigate the *status* hypothesis, we modify the research design in Loh and Stulz (2011) and test whether the status of the analyst at the time of the recommendation impacts the probability of issuing influential recommendations *among the subset of analysts whose status has changed*. Specifically, we compare the influence of the analyst just before and after the publication of the new *I/I* list in October. Our test samples consist of individual analyst recommendations made by analysts who are either promoted or demoted.

We estimate the following probit regression model:

$$\begin{aligned} Pr\,(influential_{ijt}=1) = {} & \beta_0 + \beta_1 post + \beta_2 influential_any_{it} + \beta_3 influential_same_{ijt} + \\ & \beta_4 away_from_consensus_{ijt} + \beta_5 momentum_pre_{ijt} + \beta_6 logbm_{ijt} + \\ & \beta_7 avg_logturn_pre_{ijt} + \beta_8 std_resid_pre_{ijt} + \beta_9 logsize_{ijt} + \beta_{10} \\ & ana_count_{ijt} + \varepsilon_{ijt} \end{aligned} \quad (1)$$

where *influential* follows the definition in Loh and Stulz (2011) that we discussed earlier. Our variable of interest is *post*, which is an indicator variable equal to 1 if the recommendation occurred after the analyst's promotion or demotion was announced in October, and 0 otherwise. If status drives influence, there should be a significant effect of the change in status on influence around the publication of all-star rankings, and the coefficient on *post* should be significant.

Our control variables are comprised of the subset of control variables in Loh and Stulz (2011) that they found to be statistically significant in their tests of *IR*. These variables include: *influential_any (influential_same)*, an indicator variable set to 1 if the analyst had at any time previously issued an influential recommendation for any stock (the same stock), and 0 otherwise; *away_from_consensus* is an indicator variable set to 1 if the analyst's recommendation moves away from the consensus, and 0 otherwise; *momentum_pre* is the cumulative returns in the three months prior the recommendation; *logbm* is the natural log of the book-to-market ratio; *avg_logturn_pre* is the average of the log of share turnover; *std_resid_pre* is the standard deviation of idiosyncratic returns in the period prior to the recommendation; *logsize* is the natural log of the market value of equity; *ana_count* is the natural log of the number of recommendations issued for all analysts for the three months preceding the recommendation.

3.1.1 Summary Statistics of Analyst-Recommendation Data

Table 1 presents summary statistics for recommendation-level variables. Column (1) includes the full sample, and in Columns (2) and (3), we divide the sample into two groups: analysts who are stars at the time of the recommendation (Current Stars) and analysts who have not been selected as stars in the previous five years at the time of the recommendation (Never Stars). Column (4) presents the differences between the two groups. We exclude ex-stars from our tests except for analysts who are in the first year of demotion, as we are interested in their change of status. That is, we exclude analysts from the second to the fifth year after losing status because their expected influence is ambiguous.[9] There are 94,769 recommendations during our sample period, of which 10,972 (11.6%) are issued by star analysts.

[9] This group may have some residual star status, the effect of which is unclear under either of our hypotheses.

[Table 1 about here]

The results show that 13.5% of all recommendations are influential and that Current Stars exhibit stronger influence, as 17.1% of their recommendations are influential. Current Stars have a 32.5% higher probability than Never Stars to have an influential recommendation (17.1% vs. 12.9%). Current Stars are more likely to have issued an influential recommendation (*influential_any* = 84.4%) compared with Never Stars (64.5%). Similarly, Current Stars are more likely to have issued an influential recommendation for the same stock (*influential_same* = 20.6%) than Never Stars (14.2%).

Consistent with previous studies (Clarke et al., 2007; Emery and Li, 2009; Lobo et al., 2020), we find that Current Stars cover larger firms, as *logsize* being significantly larger for stars. However, there are no significant differences in pre-recommendation daily log turnover (*avg_logturn_pre*) or pre-recommendation returns (*momentum_pre)* between firms covered by Current Stars and by Never Stars.

3.1.2 Loh and Stulz (2011) Replication

Our baseline test replicates a key finding in Loh and Stulz (2011): a recommendation issued by a star analyst is more likely to be influential. The replication follows Equation (1) but replaces the *post* variable with *star*, the analyst's star status at the time of the recommendation. The results in Table 2 Column (1) are consistent with those in Loh and Stulz (2011): the coefficient on *star* is positive and statistically significant, confirming that star analysts are more likely to issue an influential recommendation than ordinary analysts in our sample.

[Table 2 about here]

star is a binary variable, but *I/I* provides four rankings: 1st, 2nd, 3rd, and runners up. We next extend Loh and Stulz (2011) to investigate whether a higher rank is correlated with influence. In Column (2), we include *ranked_3rd*, *ranked_2nd*, and *ranked_1st*, which are dummy variables corresponding to the analyst's rank at the time the recommendation was made. These variables measure the differential likelihood of a star analyst's recommendation being influential relative to that of a star analyst ranked runner up. The coefficient on *star* remains positive and significant, while none of the additional "*ranked_*" variables are statistically significant. The result suggests that a recommendation from a star analyst of any rank is more likely to be influential than that from an ordinary analyst, but recommendations from higher-ranked star analysts are not significantly more likely to be influential than those from lower-ranked star analysts.

Although a star analyst's current rank does not affect the influence of their recommendations, their accumulated rank potentially could be. That is, an analyst who has been elected as a star multiple times may be more likely to be influential than an analyst elected as a star for the first time. We re-estimate the baseline model from Column (1) but include *num_star_last5yr*, an integer between 0 and 5 that measures the number of times an analyst has been a star in the past five years. The results in Column (3) indicate that, similar to current rank, accumulated rank is not associated with higher likelihood of issuing an influential recommendation.

In sum, our baseline and extended tests of Loh and Stulz (2011) confirm that recommendations from star analysts tend to be more influential, but neither the analyst's current nor accumulated rank is correlated with higher likelihood of generating an influential recommendation.

3.1.3 Probit Regressions

According to the *status* hypothesis, we should observe an increase in the likelihood of *IR* after being promoted to a star and a decrease in the likelihood after a demotion. In Equation (1), the *status* hypothesis predicts that the coefficient of the *post* variable should change in the same direction as the change in status.

In Column (1) of Table 3, we limit the sample to demoted stars and examine their influence in the six months before and after the October of their demotion. We find that the coefficient on *post* is small and insignificant, suggesting that the influence of demoted analysts changes little after the publication of the new *I/I* list. To ensure that our results are not sensitive to the selection of the pre-publication period, we test an alternative six-month period in Column (2): November of year *t-1* to April of year *t*. This alternative period mitigates the concern that investors can predict future rankings, and it also reduces the impact of other seasonality effects as we are comparing the probability of generating an *IR* between November and May in two consecutive years. The results in Column (2) show that the coefficient on *post* remains statistically insignificant.

[Table 3 about here]

Next, we focus on the other group of analysts whose status changes in October: ordinary analysts who become stars. To ensure a clean test, we require that the analyst has not been selected as a star in the previous five years. According to the *status* hypothesis, the coefficient on *post* should be positive and significant. However, the results in Columns (3) and (4) show that the coefficient on *post* is not statistically significant, where these two sets of results follow the same time-period specifications as in Columns (1) and (2). The results for promoted first-time stars are similar to those for demoted stars: a change in status does not correspond with a change in the likelihood of generating *IR* in the same direction.

We also run two sets of robustness tests. First, to ensure that the results are not driven by the specific observations windows we use, we vary the observation windows in the 18 months before and after October. Second, to check whether status matters more for stocks which are difficult to value, we rerun our tests on two subsamples of recommendations: those for stocks with high idiosyncratic volatility and those for stocks with high total volatility.[10] The untabulated results show that the coefficient on *post* remains statistically insignificant. Across all our tests, we find no evidence that changes in status are accompanied by changes in analysts' influence.

3.1.4 Stacked Difference-in-Difference

To further examine whether status impacts analyst influence, we conduct difference-in-difference (DID) tests by comparing demoted and promoted analysts to analysts with the same ranking prior to the change in status. For each publication year *t*, we split the year into two half-years: May_t to October_t, and November_t to April_{t+1}. The annually recurring DID "event" is the publication of the list in October. We modify Equation (1) to estimate the following regression:

$$\begin{aligned} Pr\,(influential_{ijt}=1) = {} & \beta_0 + \beta_1 post_{it} + \beta_2 treatment_{it} + \beta_3 post \times treatment_{it} + \\ & \beta_4 num_star_last5yr_{it} + \beta_5 influential_any_{it} + \beta_6 influential_same_{ijt} + \\ & \beta_7 away_from_consensus_{ijt} + \beta_8 momentum_pre_{ijt} + \beta_9 logbm_{ijt} + \\ & \beta_{10} avg_logturn_pre_{ijt} + \beta_{11} std_resid_pre_{ijt} + \beta_{12} logsize_{ijt} + \\ & \beta_{13} ana_count_{ijt} + \varepsilon_{ijt} \end{aligned} \qquad (2)$$

where *post* is an indicator variable set to 1 if the recommendation occurs in the six months post-publication, and 0 otherwise. *Treatment* is an indicator variable for recommendations made by the "treatment" group, a subset of analysts who experience a specific change in status. The variable of interest is the interaction term *post* × *treatment*, which reflects the impact of the publication of

[10] For each recommendation, we calculate the total and idiosyncratic volatility of the stock over a (−69, −6) window using the Fama-French 3-factor model. For each month and each volatility measure, we rank stocks by their volatility. To ensure we have sufficient observations, we categorize recommendations in the top 1/3 of each volatility rank as having high total/idiosyncratic volatility.

rankings for the treatment group. For demoted analysts, *num_star_last5yr* is a control variable that measures the number of times an analyst has been a star in the past five years. The remaining control variables are the same as in Equation (1).

We estimate three specifications. In the first one, we limit the sample to analysts who are stars in May of year *t* (i.e., those elected as stars in year *t-1*), with *treatment* corresponding to the subset who are demoted in October of that year. This approach allows us to determine whether demoted stars experience a change in influence relative to what their non-demoted peers experience after the publication of the updated lists each October. Results for this specification (sample = stars in May; treatment = demoted in October) are presented in Table 4 Column (1). The *status* hypothesis suggests that demoted analysts lose their influence after the publication of the all-star list. Therefore, the prediction is that the coefficient on *post* × *treatment* is significantly negative. The results in Column (1) shows a negative and significant coefficient on *treatment* but not on *post* × *treatment.* Therefore, recommendations from demoted analysts are less likely to generate *IR* than those from analysts who remain stars, but contrary to the prediction of *status* hypothesis, there is no difference in this likelihood after the demotion is publicly announced. Moreover, while the regression is not a formal test of the *quality* hypothesis, the negative and significant coefficient for *treatment* is nevertheless consistent with it: relative to their non-demoted peers, the to-be demoted analyst's influence is diminished *before* their demotion is made public.

[Table 4 about here]

A potential concern is that analysts might retain some residual status even after being demoted or promoted. In the second specification, we limit the sample to stars who are demoted in October of year *t,* with *treatment* group representing the subset demoted analysts who were elected as stars for the first time in year *t-1*. This allows us to investigate whether upon publication

of their demotion, demoted first-time stars experience a greater loss in influence than demoted multi-year stars, who would presumably have higher accrued status. Results for the second specification (sample = demoted in October of year *t*; treatment = first-time star in October *t-1* demoted after one year) are presented in Table 4 Column (2). The results show that the coefficient on *post* × *treatment* is negative but not significant, suggesting that there is not a significant difference in the loss of influence for first-time demoted vs. multi-time demoted stars.

In the final specification, we limit the sample to analysts who, as of May of year *t*, have not been elected as a star in the prior five years (Never Stars). The *treatment* group includes the subset of analysts who will be elected as stars for the first time in October of that year. Results for this specification (sample = Never Star in May of year *t*; treatment = first-time star in October *t*) are presented in Table 4 Column (3). While the coefficient on *treatment* is positive and significant, the coefficient on the interaction term is not significant. The results here are consistent with those for the first DID specification: 1) While recommendations from first-time promoted stars are more likely to be influential than their non-promoted peers, there is no significant change in that likelihood after promotion occurs;[11] and 2) although it is not a formal test of *quality*, the positive and significant coefficient on *treatment* is consistent with it: relative to their non-promoted peers, first-time stars' influence increases before their promotion is made public. Overall, we find no evidence in our stacked DID results that status affects star analysts' influence.

3.2. *Quality* Hypothesis

[11] The sample here includes all analysts who have not been elected as a star within the previous five years, a group that includes only lightly active analysts who are essentially ineligible to become stars. To address this concern, we re-estimate the model but limit the sample to recommendations from analysts who cover at least five (5) stocks. In untabulated results, the coefficient estimates are qualitatively similar.

The results in the preceding subsection suggest that changes in status are not related to the probability of issuing an *IR*, leaving the *quality* hypothesis as an intuitive causal channel between *IR* and star status. Indeed, the results from the DID tests (Section 3.1.4) provide some preliminary evidence supporting the *quality* hypothesis: revisions from demoted (promoted) analysts are less (more) likely to be influential *before* the change in status is announced. A plausible interpretation is that analysts who can produce influential recommendations are voted as stars in the upcoming year, consistent with the *quality* hypothesis.

A key assumption in this interpretation is that influential recommendations are part of the *I/I* voters' criteria, and our first test of *quality* is based on this assumption. We effectively reverse the dependent and independent variables from our *status* tests and explore the lead-lag relationship between star status and lagged measures of quality. In our *status* tests, we estimated the effect of star status on the contemporaneous influence of a given recommendation. In our *quality* tests, we examine the effect of past influence on future star status.

For each analyst-year observation, star status in October of year *t* is the dependent variable. The independent variables are constructed from recommendations that the analyst issued over the preceding evaluation period from May of year *t-1* to April of year *t*. We generate measures of each analyst's observable performance over the prior evaluation period. The first performance measure is *Annual_IR,* an indicator variable equal to 1 if the analyst had at least one *IR* in year *t-1*; 0 otherwise. The second measure is *Annual_blunder*, an indicator equal to 1 if the analyst had at least one recommendation immediately followed by a significant return in the *opposite* direction of the recommendation in year *t-1*, 0 otherwise. The final measure is *avg_ret,* the average abnormal announcement returns for all of the analyst's recommendations in year *t-1*. We estimate the following probit regression model:

$$Pr\ (star_{jt}=1) = \gamma_0 + \gamma_1 Annual_IR_{jt-1} + \gamma_2 Annual_blunder_{jt-1} + \gamma_3 avg_ret_{t-1} + \gamma_4 star_{jt-1} + \gamma_5 log(Nstocks_{jt-1}) + \gamma_6 log(Nrec_{jt-1}) + \gamma_7 pctbuy_{jt-1} + \gamma_8 risklevel_{jt-1} + \gamma_9 inforatio_{jt-1} + \gamma_{10} accuracy_{jt-1} + \gamma_{11} boldness_{jt-1} + \gamma_{12} optimism_{jt-1} + \gamma_{13} experience_{jt-1} + \varepsilon_{jt} \quad (3)$$

Our control variables follow Emery and Li (2009). While we focus on announcement returns, they use a measure of long-term returns (*inforatio*) based on a hypothetical portfolio constructed of the analyst's recommendations over the prior year. $star_{jt-1}$ is an indicator variable equal to 1 if the analyst was an all-star in the previous year, and 0 otherwise. $Nstocks_{jt-1}$ and $Nrecs_{jt-1}$ are measures of industry knowledge (Jacob et al. 1999). $Nstocks_{jt-1}$ is the number of unique stocks that analyst *j* covers in year *t*. $Nrecs_{jt-1}$ is the number of recommendations analyst *j* issues in year *t*. Logged versions of $Nstocks_{jt-1}$ and $Nrecs_{jt-1}$ are used in the regression analysis to control for skewness in the measures. Two measures of analyst bias control for the possibility that analyst bias may help analysts get promoted (Hong and Kubic, 2003). First, $pctbuy_{jt-1}$ is the ratio of buy recommendations to all recommendations that analyst *i* issues in the prior year; second, $optimism_{jt-1}$ is a measure of the relative optimism of an analyst's earnings forecasts. Analyst aggressiveness may also affect the likelihood of being selected as a star. Controlling for these possibilities, $risklevel_{jt-1}$ is the beta of analysts' recommendation portfolio from an industry index regression, and $boldness_{jt-1}$ is a score measure based on the ranking of deviation from earnings consensus. $accuracy_{jt-1}$ is a measure of relative earnings forecast accuracy developed by Hong et al. (2000). Finally, we include $experience_{jt-1}$, which is the number of years an analyst has been submitting reports to IBES.

3.2.1 Descriptive Statistics of Analyst-Year Data

Panel A of Table 5 presents summary statistics for our main variables. Column (1) includes the full sample of 27,715 analyst-year observations. In Column (2), the sample includes analysts

who are named as stars during the evaluation period (Current Stars). In Column (3), the sample includes analysts who have not been named to an all-star list in the five years up to and including the evaluation year (Never Stars). Column (4) presents the differences in means between Current Stars and Never Stars.

[Table 5 about here]

The average number of recommendations issued by an analyst (*Nrecs*) is 12.47 for the full sample. Star analysts issue 35% more recommendations than ordinary analysts (15.46 vs. 11.43). The higher number of recommendations issued by stars explains why the proportion of recommendations by stars (10.5%) is higher than their proportion in the analyst community. Star analysts also follow more stocks compared with ordinary analysts (19.97 vs. 14.82).[12]

Our primary performance metrics all indicate that Current Stars perform better than Never Stars. Star analysts are both more likely to issue at least one influential recommendation (*Annual_IR* is 0.457 for stars vs. 0.364 for ordinary analysts) and less likely to issue at least one blunder recommendation (*Annual_blunder* is 0.074 for stars vs. 0.095 for ordinary analysts). Star analysts are associated with higher average announcement returns (*avg_ret* is 0.027 for star analysts vs. 0.025 for ordinary analysts). Additionally, we observe lower percentages of buy recommendations (0.400 vs. 0.463), lower levels of risk (0.440 vs. 0.537), higher accuracy (51.4 vs. 50.3), and more experience for star analysts (17.92 vs. 12.84).

Panel B of Table 5 presents the transition matrix for star status. Notably, star status is extremely sticky: 80% of current star analysts continue to be stars in the subsequent year.

3.2.2 Probit Regressions

[12] The number of covered stocks is based on earnings forecasts, not recommendations, since analysts typically issue the former more frequently than the latter.

Table 6 Panel A reports the probit regression results of Equation (3); the corresponding marginal effects are also reported in Panel B. We begin by testing whether influence affects star status in the following year. Loh and Stulz (2011) show that although analyst recommendations are on average not impactful, a narrow set of influential recommendations warrant attention. Motivated by their insight, we include only *Annual_IR* as a proxy for analyst quality along with other control variables in Column (1). The coefficient on *Annual_IR* is both positive and significant at the 1% level. This result provides initial direct evidence for the *quality* hypothesis: Having an *IR* in a given year increases the probability of being selected as a star in the upcoming *I/I* rankings. Given the stickiness of star status (shown in Panel B of Table 5), one explanation for the finding in Loh and Stulz (2011), which we replicate in Table 2 of our paper, that current star status is positively correlated with the likelihood of having an *IR* is that serial correlation in star selection is the product of serial correlation in the quality of a star analyst's research output, at least as perceived by *I/I* voters.

[Table 6 about here]

In terms of control variables, the coefficient on $star_{t-1}$ is positive and statistically significant, suggesting that previous stars are more likely to be re-elected even after controlling for the profitability of recommendations and forecast accuracy. To the extent that lagged star status is a proxy for quality, this result is consistent with Brown et al. (2015, 2016) that quantitative measures capture only a small part of "star quality" considered by buy-side analysts.[13] The coefficient on *Nstocks* is also positive and highly significant, indicating that covering more firms increases an analyst's chances to be elected (Jacob et al., 1999; Emery and Li, 2009).

[13] Several studies suggest that star analysts are likely to repeat their selection even if their quality is not better than other analysts. For example, Emery and Li (2009) find that once elected, stars have more recognition from money managers. Similarly, Clarke et al. (2007) and Dunbar (2000) report that star analysts attract more investment banking business.

One potential issue with the preceding test is that *IR* alone is unlikely to be a sufficient proxy for whatever dimensions of analyst quality that *I/I* voters value. The rewards for star status are considerable (e.g., Groysberg et al., 2011), and a criterion based on *Annual_IR* alone would reward analysts for gambling: making high-variance, low-likelihood-of-success recommendations. In this scenario, *I/I* star status would largely be conferred to the luckiest, as opposed to best, analysts. To mitigate this concern, *I/I* voters could employ a criterion to punish analysts for gambling. One possibility is to punish analysts for committing the opposite of *IR:* blunders.

In Column (2), when we include *Annual_blunder* in our regression specification, we find the coefficient on it is negative, significant, and roughly twice the magnitude of the coefficient on *Annual_IR*. Punishing a blunder lightly or even equivalently relative to the rewarding of *IR* would allow low-quality analysts to make risky recommendations and potentially make the cut for all-star status, weakening the case for *quality*. Punishing a blunder more than rewarding *IR* serves as a gambling deterrence mechanism and increases the likelihood that elected analysts are in fact high quality.

Annual_IR and *Annual_blunder* represent the extremes of the distribution of recommendation announcement returns, and one possibility is that they proxy for the overall performance of an analyst. With this in mind, we additionally control for *avg_ret*, the average of the abnormal announcement returns for all recommendations made by the analyst over the evaluation period. The results in Column (3) indicate that *Annual_IR* and *Annual_blunder* are not subsumed by *avg_ret.* [14] Notably, *avg_ret* itself is not significantly correlated with subsequent star status, suggesting that the voting criteria is based more on salient, memorable research by an analyst rather than the analyst's average performance.

[14] In unreported results, we alternatively measure *avg_ret* using only non-influential and non-blunder recommendations. The results are quantitatively similar.

Overall, the results in Table 6 strongly support the *quality* hypothesis. *I/I* voters reward influence but punish blunders severely. That gambling behavior is strongly discouraged suggests that the voters are trying to separate analysts who are truly capable of producing consistent, high-quality research from simply those who take on high risk.

4. Voter Interests

The prior tests for *quality* are based on the premise that the sole objective of *I/I* voters is to identify the highest-quality analysts. However, *I/I* voters may have an additional and self-interested objective: to incentivize high-quality analysts to reveal novel and high-impact information in a timely manner.

Voters fall into two broad categories: buy-side analysts and fund managers. For buy-side analysts, sell-side analysts are a valuable channel of information.[15] The transfer of information can be direct (e.g., Brown et al. (2016) show that buy-side analysts meet with quality sell-side analysts to gather both firm and industry information), or indirect (e.g., Hobbs and Singh (2015) provide evidence that buy-side recommendations are most profitable when stocks have high analyst coverage and low dispersion in earnings forecasts). For fund managers, sell-side analysts can be a particularly valuable and profitable source of information. Irvine et al. (2007) and Klein, Saunders, and Wong (2019) demonstrate that institutional investors derive benefits from analysts' advance notice of their recommendations to institutions prior to their public release, a practice known as "tipping". Harford et al. (2019) also provide evidence that analysts devote more effort (e.g., more frequent and more accurate earnings forecasts) to firms in their portfolio that have greater

[15] Buy-side analysts have an inherent disadvantage in competing for firm specific information as they cover much larger portfolios than sell-side analysts (e.g. Brown et al., 2016)

institutional ownership. In all these cases, voters are more likely to benefit from information that is timely, bold, and related to firms with weak information environment.

The interests of *I/I* and its voters are therefore not aligned. Identifying high-quality analysts by punishing blunders can lead to more conservative strategies, discouraging high-quality analysts from promptly disclosing potentially high-impact information that voters seek. This raises the question: How can voters both identify high-quality analysts and encourage them to make bold recommendations?

While the previous tests follow established literature and assume that the *I/I* ranking criteria is fixed for all analysts regardless of star status, in this section we explore the possibility that *I/I* voters are more sophisticated than previously assumed. One potential mechanism for voters to satisfy both *I/I*'s and their own interests is to use different criteria for different analyst cohorts. The criteria for ordinary analysts vying for first-time star selection serves to separate high-quality analysts from low-quality or lucky analysts. Those who have demonstrated their quality and been selected as stars are assessed with different criteria. These criteria no longer require proof of competence but rather maximal utilization of competence: bringing new and material information to the financial markets. To incentivize stars to release their information in a timely manner rather than waiting to verify their information (and risk the information becoming stale), voters can choose not to punish star analysts for their mistakes.

Similar to Emery and Li (2009), we divide the sample into two groups – "Never Stars" vying for first-time star selection in the last five years and "Current Stars" seeking re-election – and re-estimate our quality hypothesis regressions for each group. The subsamples exclude reigning stars who left the IBES database the next year and ex-stars. Results are reported in Table 7. Columns (1) and (2) include the "Never Stars" sample, and Columns (3) and (4) include the

"Current Stars" sample. The results indicate a significant difference between how *I/I* rankers assess the two groups. For Never Stars, the key results are qualitatively identical to those reported in Table 6 Panel A, which is not surprising given that the overall sample is overwhelmingly composed of Never Stars. In both models, the coefficient on *Annual_IR* is positive and significant, and the coefficient on *Annual_blunder* is negatively significant, and more than 50% larger in magnitude than the coefficient on *Annual_IR*. For Current Stars, however, the results are notably different. The coefficient on *Annual_IR* increases relative to Never Stars, indicating that being influential is important for re-election, but the coefficient on *Annual_blunder* decreases in magnitude and is no longer statistically significant. Current Stars are assessed on their successes (*IR*), not their failures (blunders*)*. The results suggest that Current Stars, who have already proven their ability, are incentivized to use that quality freely to its best effect: generating bold and new information.

[Table 7 about here]

The different criteria for Current Stars provide a novel and intuitive explanation for why newly-minted stars change their behavior. Jung et al. (2019) document that stars are bolder and less accurate compared with ordinary analysts. Our results imply that the change in their behavior relates directly to the change in their incentives: chances of re-election can be greatly improved with an influential recommendation.

In sum, we find that voters apply two sets of criteria for *I/I* rankings – Never Stars are evaluated based on their competence, while Current Stars are judged almost exclusively on *IR* – which allow them both to identify quality analysts for first-time star status and to incentivize reigning star analysts to produce bold information.

5. Seasonality

The results in the preceding section suggest that some previously documented behavior of analysts might be explained by the incentives embedded in the criteria set by *I/I* voters. Building on this, we now investigate whether the observed seasonality in analyst forecasts, characterized by reduced optimism in the winter and increased optimism in the spring and summer (Dolvin et al. 2009; Lo and Wu, 2018), can be linked to incentives created by voters.

The evaluation period spans from approximately May_t to $April_{t+1}$. We conjecture that there are at least three potential reasons why recommendations issued in the first half year (May_t to $October_t$) might carry less weight than those issued in the second half year ($November_t$ to $April_{t+1}$). First, our previous results indicate that analysts are evaluated based on different criteria depending on their current status, but their status in the first half of the evaluation period is ambiguous since the rankings are published only in October. As analysts cannot be incentivized without knowing which incentives apply to them, assigning higher weight to the post-publication period better aligns analysts' incentives with the voters' interests. Second, recency effects (Tversky and Kahnemann, 1974; Malmendier, 2021) may play a role. In this case, recency effects refer to voters placing higher value to events that occur closer to the voting date. Lastly, as a concession to the brokerage houses that employ the contending analysts, voters could offer a "grace period" in which analysts are less accountable for their research outputs, allowing analysts to issue optimistic valuations that benefit their respective brokerages without suffering negative consequences for their *I/I* rankings.

To investigate the seasonality effect, we employ a modified version of Equation (3) and regress next-year star status on the same set of independent variables but splitting *Annual_IR* and *blunder* into two sub-period indicator variables: *early_IR, late_IR, early_blunder,* and *late_blunder*. The *early* variables represent analysts' recommendations made between May_t and $October_t$, and the *late* variables correspond to those made between $November_t$ and $April_{t+1}$. For

example, *early_IR* equals 1 if the analyst had at least one *IR* between May_t and $October_t$, and 0 otherwise.

Probit coefficients are presented in Panel A of Table 8. In Column (1), we use the full sample and find that the magnitude of the coefficients for both *Annual_IR* and *Annual_blunder* are larger in the late period compared with the early period. Given the associated standard errors, however, the differences are not statistically significant. The results for the separated analyst cohorts are more important. In Column (2), when we limit the sample to Never Stars, the coefficient on *late_blunder* is roughly three times the coefficient on *early_blunder*. In Column (3), when we limit the sample to Current Stars, the coefficient on *late_IR* is roughly one-third larger than the coefficient on *early_IR*, though the difference is not statistically significant.

To understand the economic significance of the coefficients, we report the corresponding marginal effects in Table 8 Panel B. A *late_blunder* by a "Never Star" analyst reduces the probability of election by roughly 68 basis points, a considerable drop given the unconditional 1.4% probability of election (reported in Column 3 of Table 5). Similarly, a *late_IR* by a "Current Star" increases the probability of election by 6.77 percentage points, a considerable increase given the unconditional 19.4% likelihood of being demoted (inferred from Column 2 of Table 5). Overall, the evidence is consistent with voters employing a criterion that incentivizes analysts – specifically, the non-stars who make up the majority of the analyst population – to be more conservative in the later post-publication period. The result is consistent with prior literature documenting analyst conservatism in winter months (Dolvin et al. 2009; Lo and Wu, 2018).

[Table 8 about here]

6. Conclusion

This paper investigates the reasons behind star analysts' high influence in financial markets. We suggest two non-mutually exclusive explanations for this phenomenon. The *status* hypothesis argues that the higher influence of star analysts is related strictly to their status rather than the quality of their research output. Under this hypothesis, investors are more responsive to star analysts' recommendations than those of their non-star counterparts. The *quality* hypothesis posits that high-quality analysts are more likely to issue influential recommendations and to be selected as stars than their lower-quality peers.

Exploiting changes in the status of analysts, we find no evidence in support of the *status* hypothesis. The likelihood of influential recommendations for both promoted and demoted analysts remains the same in the months before and after the publication of their changes in status. Conversely, we find strong evidence in support of the *quality* hypothesis. High-performing analysts – measured by their ability both to generate IR and to avoid blunders – have a higher probability of being selected as a star in the next *I/I* rankings. Rather than status driving influence, as the *status* hypothesis states, our results indicate that influence drives status, which aligns with the *quality* hypothesis.

In addition, *I/I* voters, seeking both to identify high-quality analysts and to incentivize analysts who they have deemed high-quality to reveal their information quickly, effectively run two competitions for two distinct cohorts of analyst: analysts who have never been stars ("Never Stars") and those who are currently stars ("Current Stars"). Never Stars must demonstrate competence, while Current Stars must demonstrate boldness. Overall, our results suggest that all-star rankings are not just "popularity contests" but rather the outcome of a sophisticated voting process that balances the interests of both *I/I* and its voters.

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Figure 1: Quality vs. Status Hypotheses for Promoted Analysts

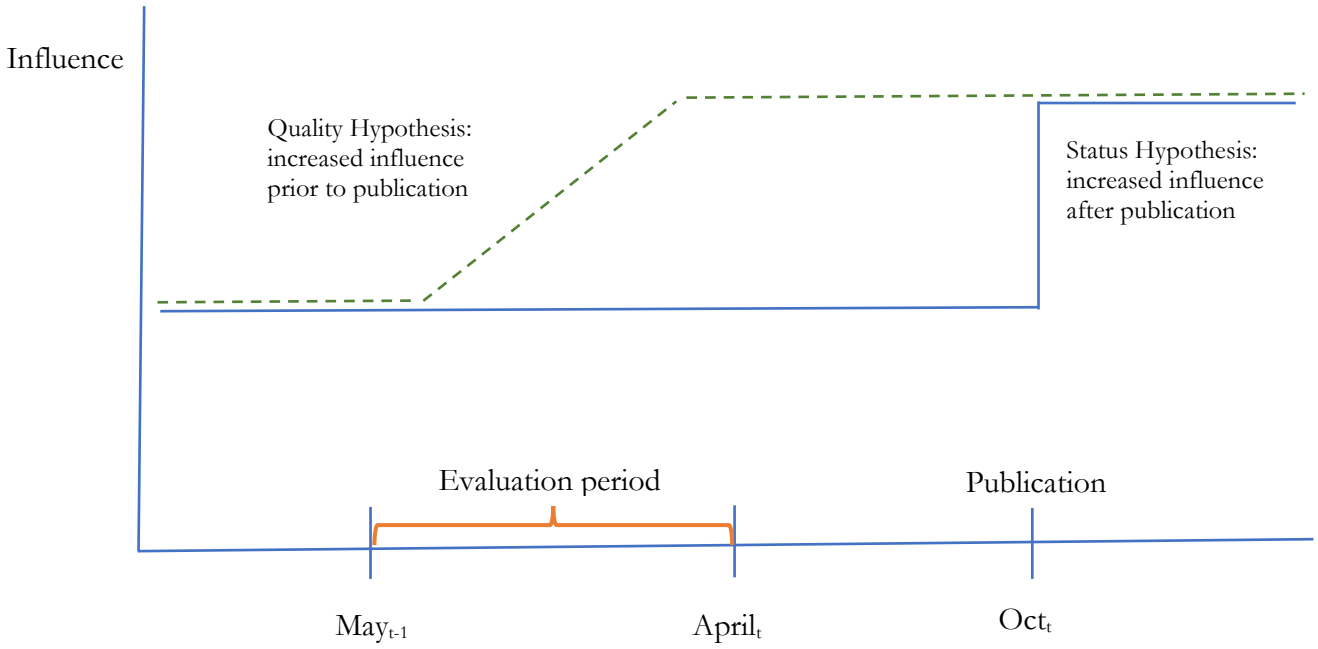


Figure 2: Quality vs. Status Hypotheses for Demoted Analysts

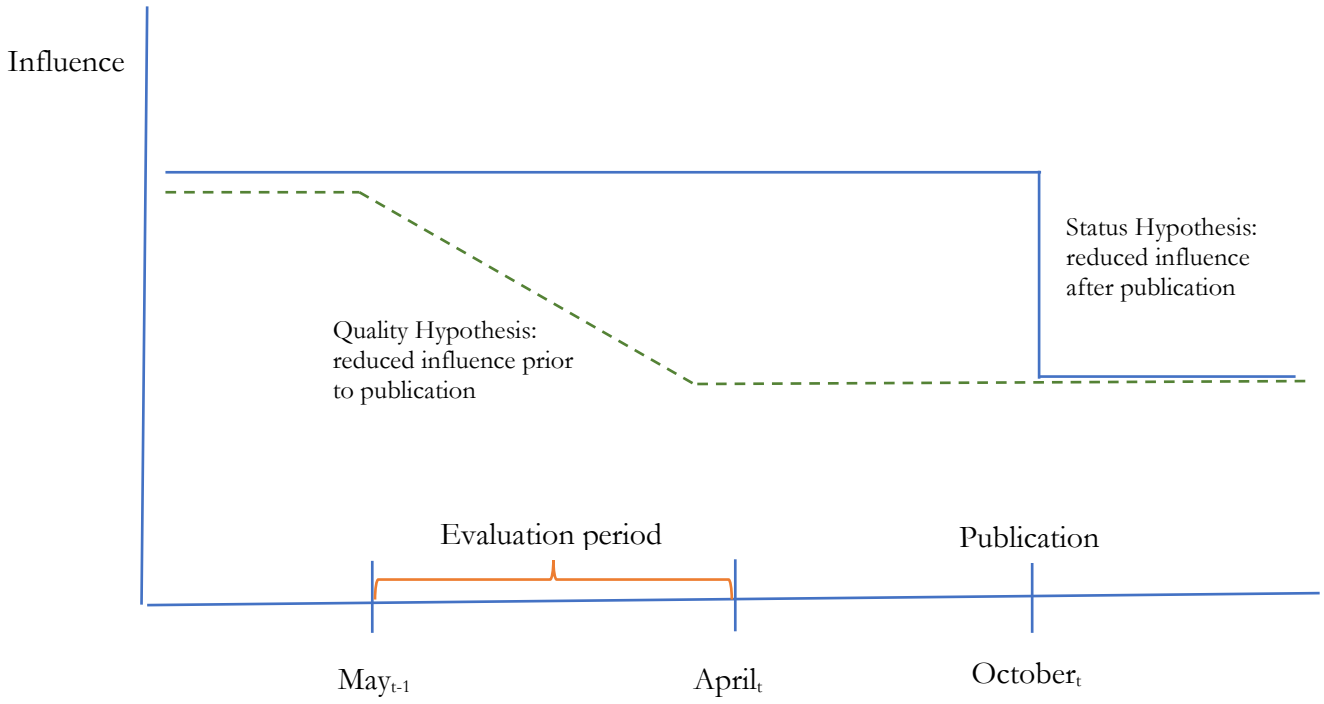


Table 1: Recommendation-Level Summary Statistics

	Full Sample n=94,769		Current Stars n=10,972		Never Stars n=81,501		
	(1)		(2)		(3)		(4)
	Mean	Std	Mean	Std	Mean	Std	(2) – (3)
influential	0.135	0.341	0.171	0.377	0.129	0.336	0.042***
influential_any	0.675	0.468	0.844	0.363	0.645	0.478	0.199***
influential_same	0.15	0.357	0.206	0.404	0.142	0.349	0.064***
momentum_pre	0.024	0.249	0.022	0.224	0.024	0.253	-0.002
logbm	-0.878	0.802	-0.855	0.801	-0.88	0.801	0.025***
avg_logturn_pre	-4.707	0.839	-4.734	0.757	-4.707	0.853	-0.027
std_resid_pre	0.023	0.015	0.02	0.013	0.023	0.015	-0.003***
logsize	14.91	1.574	15.468	1.473	14.815	1.571	0.653***

Each observation corresponds to an analyst *i* in year *t*. Variable definitions are in Appendix A.

Table 2: Star Status and Influential Recommendations

	Dependent variable = *influential*		
	(1)	(2)	(3)
star	0.198***	0.166***	0.194***
	(0.016)	(0.025)	(0.020)
ranked_3rd		0.032	
		(0.040)	
ranked_2nd		0.055	
		(0.039)	
ranked_1st		0.062	
		(0.039)	
num_star_last5yr			0.002
			(0.006)
influential_any	0.087***	0.087***	0.087***
	(0.012)	(0.012)	(0.012)
influential_same	0.031**	0.031**	0.031**
	(0.015)	(0.015)	(0.015)
away_from_consensus	0.137***	0.137***	0.137***
	(0.010)	(0.010)	(0.010)
momentum_pre_wins	-0.394***	-0.393***	-0.394***
	(0.023)	(0.023)	(0.023)
logbm_wins	-0.050***	-0.050***	-0.050***
	(0.007)	(0.007)	(0.007)
avg_logturn_pre	0.007	0.007	0.007
	(0.007)	(0.007)	(0.007)
std_resid_pre	-13.962***	-13.964***	-13.961***
	(0.523)	(0.523)	(0.523)
logsize_wins	-0.103***	-0.103***	-0.103***
	(0.005)	(0.005)	(0.005)
ana_count	-0.016***	-0.016***	-0.016***
	(0.001)	(0.001)	(0.001)
Intercept	0.727***	0.728***	0.728***
	(0.080)	(0.080)	(0.080)
Pseudo R^2	0.032	0.032	0.032
Observations	94,676	94,676	94,676

This table presents results from probit regressions examining whether star status increases the probability of generating influential recommendations. Each observation corresponds to a recommendation, and the dependent variable, *influential* is an indicator variable set to 1 if the recommendation is influential and 0 otherwise. *star* is an indicator variable corresponding to the analyst's star status at the time that the recommendation was issued. *ranked_3rd, ranked_2nd,* and *ranked_1st* are indicator variables set to 1 corresponding to the analyst's ranking at the time of the recommendation. *num_star_last5yr* represents the number of years in the prior five years the analyst had been elected as a star. Variable definitions are in Appendix A. Continuous variables are winsorized. Standard errors are shown in parentheses. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 3: Changes in Star Status and Influential Recommendations

	Dependent variable = *influential*			
Sample	Demoted Stars		First-time Stars	
Pre-Period	May_t-Oct_t	Nov_{t-1}-Apr_t	May_t-Oct_t	Nov_{t-1}-Apr_t
Post-Period	Nov_t-Apr_{t+1}	Nov_t-Apr_{t+1}	Nov_t-Apr_{t+1}	Nov_t-Apr_{t+1}
	(1)	(2)	(3)	(4)
post	0.033	0.040	-0.083	0.006
	(0.088)	(0.084)	(0.093)	(0.094)
influential_any	0.105	0.212*	0.126	-0.159
	(0.134)	(0.120)	(0.150)	(0.136)
influential_same	-0.155	-0.025	0.022	0.220*
	(0.116)	(0.110)	(0.121)	(0.120)
away_from_consensus	0.114	0.173**	0.147	0.155*
	(0.087)	(0.082)	(0.092)	(0.093)
momentum_pre	-0.717***	-0.559**	-0.276	-0.365
	(0.239)	(0.223)	(0.211)	(0.233)
logbm	-0.094	0.024	0.001	-0.047
	(0.058)	(0.054)	(0.058)	(0.061)
avg_logturn_pre	0.030	0.003	0.067	0.014
	(0.074)	(0.064)	(0.080)	(0.078)
std_resid_pre	-24.323***	-23.409***	-16.355***	-16.398***
	(5.850)	(4.687)	(5.263)	(5.415)
logsize	-0.191***	-0.188***	-0.043	-0.117***
	(0.041)	(0.038)	(0.043)	(0.042)
ana_count	-0.022**	-0.004	-0.027**	-0.013
	(0.011)	(0.009)	(0.011)	(0.011)
Intercept	2.500***	2.089***	0.475	1.317*
	(0.751)	(0.695)	(0.752)	(0.760)
Pseudo R^2	0.059	0.05	0.028	0.031
Observations	1,361	1,559	1,076	1,092

This table presents results from probit regressions examining whether star status increases the probability of generating influential recommendations. Each observation corresponds to a recommendation, and the dependent variable, *influential* is an indicator variable set to 1 if the recommendation is influential and 0 otherwise. *post* is an indicator variable equal to 1 if the recommendation occurred after the analyst's promotion or demotion was announced in October, and 0 otherwise. In Columns (1) and (2), the sample is limited to star analysts who are demoted in year *t*. In Columns (3) and (4), the sample is limited to recommendations made by First-Time Stars: those elected to the list in year *t* with no prior election in the previous five years. For a promotion or demotion in year *t*, two sets of time periods are used. In Columns (1) and (3), the sample consists of recommendations made six months prior to (May_t to Oct_t) and six months post (Nov_t to Apr_{t+1}) the end of October when the analyst's status change was published. Columns (2) and (4) shift the prior period back six months (Nov_{t-1} to Apr_t). Variable definitions are in Appendix A. Continuous variables are winsorized. Standard errors are shown in parentheses. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 4: Difference-in-Differences

	Dependent variable = *influential*		
Sample	Stars in May_t	Demoted in Oct_t	Never Stars in May_t
Treatment	Demoted in Oct_t	Demoted First-Time Star	First-Time Star in Oct_t
	(1)	(2)	(3)
post	-0.018	-0.009	-0.016
	(0.032)	(0.106)	(0.011)
treatment	-0.114*	-0.198	0.222***
	(0.063)	(0.157)	(0.046)
*treatment * post*	-0.031	-0.056	-0.027
	(0.092)	(0.190)	(0.065)
num_star_last5yr	-0.013	-0.059*	
	(0.009)	(0.035)	
influential_any	0.139***	0.119	0.080***
	(0.050)	(0.138)	(0.013)
influential_same	0.016	-0.151	0.037**
	(0.037)	(0.117)	(0.016)
away_from_consensus	0.228***	0.106	0.119***
	(0.030)	(0.087)	(0.011)
momentum_pre_wins	-0.407***	-0.750***	-0.384***
	(0.076)	(0.241)	(0.025)
logbm_wins	-0.055***	-0.089	-0.051***
	(0.020)	(0.058)	(0.007)
avg_logturn_pre	-0.005	0.030	0.011
	(0.024)	(0.074)	(0.007)
std_resid_pre	-19.893***	-23.620***	-13.438***
	(1.798)	(5.839)	(0.552)
logsize_wins	-0.105***	-0.185***	-0.105***
	(0.014)	(0.042)	(0.005)
ana_count	-0.016***	-0.023**	-0.016***
	(0.004)	(0.011)	(0.001)
Intercept	0.964***	2.613***	0.784***
	(0.243)	(0.747)	(0.086)
Pseudo R^2	0.038	0.062	0.031
Observations	10,237	1,361	82,587

This table presents difference-in-differences results from probit regressions examining whether star status increases the probability of generating influential recommendations. Each observation corresponds to a recommendation, and the dependent variable, *influential*, is an indicator variable set to 1 if the recommendation is influential and 0 otherwise. In Column (1), the sample is composed of all observations from May_t to $April_{t+1}$ in which the analyst was an all-star as of May_t. The treatment variable in these columns is $Demoted_t$, corresponding to those analysts who lost their star status in $October_t$. In Column (2), the sample is composed of all observations from May_t to $April_{t+1}$ in which the analyst lost their star status in $October_t$. In Column (3), the sample is composed of observations in which the analyst had not been a star within the five years prior to May_t ("Never Stars"). *post* is an indicator variable equal to 1 if the observation occurs post-$October_t$ (i.e., recommendations between $November_t$ and $April_{t+1}$). *num_star_last5yr* represents the number of years in the prior five years the analyst had been elected as a star. Standard errors are shown in parentheses. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 5: Analyst-Year Observations

Panel A: Summary Statistics

	Full Sample n=27,715		Current Stars n=2,853		Never Stars n=19,412		
	(1)		(2)		(3)		(4)
	Mean	Std	Mean	Std	Mean	Std	(2) - (3)
Annual_IR$_{t-1}$	0.381	0.486	0.457	0.498	0.364	0.481	0.093***
Annual_blunder$_{t-1}$	0.099	0.299	0.074	0.262	0.095	0.294	-0.021***
avg_ret$_{t-1}$	0.025	0.058	0.027	0.044	0.025	0.062	0.002***
star$_{t-1}$	0.103	0.304	0.816	0.387	0.014	0.118	0.802***
Nstocks$_{t-1}$	15.15	8.20	19.97	8.26	14.82	8.17	5.15***
Nrecs$_{t-1}$	12.47	8.40	15.46	10.54	11.43	8.23	4.03***
pctbuy$_{t-1}$	0.460	0.209	0.400	0.178	0.463	0.210	-0.063***
risklevel$_{t-1}$	0.517	0.376	0.440	0.309	0.537	0.387	-0.097***
inforatio$_{t-1}$	0.217	1.039	0.172	1.052	0.193	1.026	-0.021
accuracy$_{t-1}$	50.5	15.0	51.4	10.6	50.3	15.1	1.1***
boldness$_{t-1}$	49.9	15.2	49.6	10.9	50.0	15.3	-0.4
optimism$_{t-1}$	0.526	0.229	0.520	0.186	0.525	0.229	-0.005
experience$_{t-1}$	13.226	8.786	17.919	8.309	12.840	9.007	5.079***

Each observation in Panel A corresponds to an analyst *i* in year *t*. Following Emery and Li (2009), *pctbuy*, *risklevel*, *inforatio*, *accuracy*, *boldness*, *optimism*, and *experience* are calculated on calendar year *t*. All other variables are calculated based on *ranking* year *t*, which runs from May$_t$ to April$_{t+1}$.

Panel B: Star Status Transition Matrix

		star$_t$	
		0	1
star$_{t-1}$	0	24,333	529
	1	524	2,329

Each observation in Panel B corresponds to an analyst *i* in years *t-1* and *t*.

Table 6: Determinants of Star Status

Panel A: Probit Regressions

	Dependent Variable = $star_t$		
	(1)	(2)	(3)
$Annual_IR_{t-1}$	0.087***	0.087***	0.102***
	(0.032)	(0.032)	(0.035)
$Annual_blunder_{t-1}$		-0.180***	-0.197***
		(0.056)	(0.058)
avg_ret_{t-1}			-0.388
			(0.344)
$star_{t-1}$	2.807***	2.804***	2.807***
	(0.034)	(0.034)	(0.034)
$log_nstocks_{t-1}$	0.248***	0.245***	0.245***
	(0.033)	(0.033)	(0.034)
log_nrecs_{t-1}	0.024	0.038	0.037
	(0.025)	(0.025)	(0.026)
$pctbuy_{t-1}$	-0.359***	-0.359***	-0.353***
	(0.081)	(0.081)	(0.082)
$risklevel_{t-1}$	-0.106**	-0.107**	-0.104**
	(0.044)	(0.044)	(0.044)
$inforatio_{t-1}$	0.004	0.006	0.005
	(0.015)	(0.015)	(0.015)
$accuracy_{t-1}$	0.002	0.002	0.002
	(0.001)	(0.001)	(0.001)
$boldness_{t-1}$	0.002	0.002	0.002
	(0.001)	(0.001)	(0.001)
$optimism_{t-1}$	-0.084	-0.087	-0.088
	(0.075)	(0.075)	(0.075)
$experience_{t-1}$	0.005**	0.004**	0.004**
	(0.002)	(0.002)	(0.002)
Intercept	-2.740***	-2.751***	-2.748***
	(0.137)	(0.138)	(0.138)
Pseudo R^2	0.582	0.582	0.583
Observations	27,742	27,742	27,715

Panel B: Marginal Effects

	Dependent Variable = $star_t$		
	(1)	(2)	(3)
$Annual_IR_{t-1}$	0.0072***	0.0072***	0.0084***
	(0.003)	(0.003)	(0.003)
$Annual_blunder_{t-1}$		-0.0149***	-0.0163***
		(0.005)	(0.005)
avg_ret_{t-1}			-0.0321
			(0.028)
$star_{t-1}$	0.2334***	0.2321***	0.2324***
	(0.007)	(0.007)	(0.007)
$log_nstocks_{t-1}$	0.0206***	0.0203***	0.0203***
	(0.003)	(0.003)	(0.003)
log_nrecs_{t-1}	0.002	0.0032	0.003
	(0.002)	(0.002)	(0.002)
$pctbuy_{t-1}$	-0.0299***	-0.0297***	-0.0292***
	(0.007)	(0.007)	(0.007)
$risklevel_{t-1}$	-0.0088**	-0.0089**	-0.0086**
	(0.004)	(0.004)	(0.004)
$inforatio_{t-1}$	0.0004	0.0005	0.0005
	(0.001)	(0.001)	(0.001)
$accuracy_{t-1}$	0.0001	0.0001	0.0001
	(0.0000)	(0.0001)	(0.0001)
$boldness_{t-1}$	0.0001	0.0001	0.0001
	(0.0001)	(0.0001)	(0.0001)
$optimism_{t-1}$	-0.007	-0.0072	-0.0073
	(0.006)	(0.006)	(0.006)
$experience_{t-1}$	0.0004**	0.0004**	0.0004**
	(0.0000)	(0.0000)	(0.0000)

These tables present the regression results for tests of the *quality* hypothesis. Each observation corresponds to an analyst *i* in year *t*. Panel A reports the results of the probit model, while Panel B reports the corresponding marginal effects. The dependent variable $star_t$ is a dummy variable equal to 1 if the analyst is named an all-star in October of year *t*, and 0 otherwise. *Annual_IR* is a dummy variable set to 1 if the analyst has at least one influential recommendation over the ranking year, and 0 otherwise. *Annual_blunder* is a dummy variable set to 1 if the analyst had at least one blunder recommendation during the ranking year; 0 otherwise. *avg_ret* is the average abnormal (0,1) announcement return for all recommendations over the ranking year. Variable definitions are in Appendix A. Continuous variables are winsorized. Standard errors are shown in parentheses. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 7: Separate Competitions

	Dependent Variable = $star_t$			
Sample	Never Stars		Current Stars	
	(1)	(2)	(3)	(4)
$Annual_IR_{t-1}$	0.138***	0.149***	0.141**	0.173***
	(0.051)	(0.055)	(0.057)	(0.062)
$Annual_blunder_{t-1}$	-0.238**	-0.247**	-0.084	-0.117
	(0.096)	(0.098)	(0.103)	(0.105)
avg_ret_{t-1}		-0.209		-0.976
		(0.489)		(0.669)
$log_nstocks_{t-1}$	0.375***	0.375***	0.048	0.056
	(0.055)	(0.056)	(0.071)	(0.071)
log_nrecs_{t-1}	0.057	0.055	-0.083*	-0.087**
	(0.043)	(0.044)	(0.043)	(0.043)
$pctbuy_{t-1}$	-0.791***	-0.783***	0.385**	0.391**
	(0.135)	(0.135)	(0.160)	(0.160)
$risklevel_{t-1}$	-0.085	-0.083	-0.111	-0.100
	(0.068)	(0.068)	(0.089)	(0.090)
$inforatio_{t-1}$	-0.009	-0.008	0.015	0.015
	(0.024)	(0.024)	(0.026)	(0.026)
$accuracy_{t-1}$	0.002	0.002	0.001	0.001
	(0.002)	(0.002)	(0.003)	(0.003)
$boldness_{t-1}$	0.002	0.003	0.004	0.003
	(0.002)	(0.002)	(0.003)	(0.003)
$optimism_{t-1}$	-0.128	-0.138	0.075	0.082
	(0.125)	(0.126)	(0.148)	(0.149)
$experience_{t-1}$	-0.001	-0.001	0.003	0.002
	(0.003)	(0.003)	(0.003)	(0.003)
Intercept	-3.146***	-3.144***	0.502*	0.512*
	(0.230)	(0.230)	(0.304)	(0.304)
Pseudo R^2	0.051	0.051	0.007	0.008
Observations	19,432	19,412	2,855	2,853

This table presents results from probit regressions investigating the two separate competitions for ordinary analysts vying to be first-time stars and current stars trying to repeat. Each observation corresponds to analyst *i* in year *t*. The dependent variable $star_{it}$ is a dummy variable equal to 1 if the analyst is named an all-star in October of year *t*; 0 otherwise. In Columns (1) and (2), the sample is limited to analysts who have not been a star in the prior five years. In Columns (3) and (4), the sample is limited to analysts who are stars in year *t-1*. Variable definitions are in Appendix A. Continuous variables are winsorized. Standard errors are shown in parentheses. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 8: Seasonality

Panel A: Probit Coefficients

	Dependent Variable = $star_t$		
	Full Sample	Never Stars	Current Stars
	(1)	(2)	(3)
$early_IR_{t-1}$	0.094**	0.117*	0.187**
	(0.041)	(0.064)	(0.075)
$late_IR_{t-1}$	0.150***	0.154***	0.258***
	(0.037)	(0.059)	(0.069)
$early_blunder_{t-1}$	-0.082	-0.082	-0.115
	(0.079)	(0.129)	(0.148)
$late_blunder_{t-1}$	-0.192**	-0.246*	0.058
	(0.083)	(0.140)	(0.163)
avg_ret_{t-1}	-0.305	0.003	-0.952
	(0.318)	(0.428)	(0.634)
$star_{t-1}$	2.810***		
	(0.034)		
$log_nstocks_{t-1}$	0.248***	0.379***	0.051
	(0.034)	(0.056)	(0.072)
log_nrecs_{t-1}	0.014	0.033	-0.125***
	(0.026)	(0.045)	(0.044)
$pctbuy_{t-1}$	-0.346***	-0.779***	0.422***
	(0.082)	(0.135)	(0.161)
$risklevel_{t-1}$	-0.102**	-0.082	-0.086
	(0.044)	(0.068)	(0.090)
$inforatio_{t-1}$	0.004	-0.009	0.013
	(0.015)	(0.024)	(0.026)
$accuracy_{t-1}$	0.002	0.002	0.001
	(0.001)	(0.002)	(0.003)
$boldness_{t-1}$	0.002	0.002	0.003
	(0.001)	(0.002)	(0.003)
$optimism_{t-1}$	-0.087	-0.137	0.094
	(0.075)	(0.125)	(0.149)
$experience_{t-1}$	0.004**	-0.001	0.003
	(0.002)	(0.003)	(0.003)
Pseudo R^2	0.583	0.051	0.013
Observations	27,715	19,412	2,853

Panel B: Marginal Effects

	Dependent Variable = $star_t$		
	Full Sample	Never Stars	Current Stars
	(1)	(2)	(3)
$early_IR_{t-1}$	0.0078**	0.0032*	0.0492**
	(0.003)	(0.002)	(0.02)
$late_IR_{t-1}$	0.0124***	0.0043***	0.0677***
	(0.003)	(0.002)	(0.018)
$early_blunder_{t-1}$	-0.0068	-0.0023	-0.0302
	(0.007)	(0.004)	(0.039)
$late_blunder_{t-1}$	-0.0159**	-0.0068*	0.0152
	(0.007)	(0.004)	(0.043)
avg_ret_{t-1}	-0.0252	0.0001	-0.2501
	(0.026)	(0.012)	(0.166)
$star_{t-1}$	0.2325***		
	(0.007)		
$log_nstocks_{t-1}$	0.0205***	0.0105***	0.0133
	(0.003)	(0.001)	(0.019)
log_nrecs_{t-1}	0.0012	0.0009	-0.0329***
	(0.002)	(0.001)	(0.012)
$pctbuy_{t-1}$	-0.0286***	-0.0217***	0.1107***
	(0.007)	(0.004)	(0.042)
$risklevel_{t-1}$	-0.0084**	-0.0023	-0.0227
	(0.004)	(0.002)	(0.024)
$inforatio_{t-1}$	0.0003	-0.0003	0.0035
	(0.001)	(0.001)	(0.007)
$accuracy_{t-1}$	0.0001	0.0001	0.0002
	(0.0001)	(0.0001)	(0.001)
$boldness_{t-1}$	0.0001	0.0001	0.0009
	(0.0001)	(0.0001)	(0.001)
$optimism_{t-1}$	-0.0072	-0.0038	0.0246
	(0.006)	(0.003)	(0.039)
$experience_{t-1}$	0.0004**	0	0.0007
	(0)	(0.0001)	(0.001)

This table presents results from probit regressions investigating the seasonality in the research output of analysts and the corresponding marginal effects. Each observation corresponds to analyst *i* in year *t*. In Panel A, the dependent variable $star_{it}$ is a dummy variable equal to 1 if the analyst is named an all-star in October of year *t*, and 0 otherwise. In Column (2), the sample is limited to observations in which the analyst had not been a star in the prior five years. In Column (3), the sample is limited to analysts who were stars in year *t*. $early_IR_{t-1}$ is a dummy variable equal to 1 if

the analyst had at least one influential recommendation between May and October in year *t-1*; 0 otherwise. $late_IR_{t-1}$ is a dummy variable equal to 1 if the analyst had at least one influential recommendation between November in year *t-1* and April in year *t*; 0 otherwise. $early_blunder_{t-1}$ is a dummy variable equal to 1 if the analyst had at least one blunder recommendation between May and October in year *t-1*; 0 otherwise. $late_blunder_{t-1}$ is a dummy variable equal to 1 if the analyst had at least one blunder recommendation between November in year *t-1* and April in year *t*; 0 otherwise. Variable definitions are in Appendix A. Continuous variables are winsorized. Standard errors are shown in parentheses. ***, **, and * denote significance at the 0.01, 0.05, and 0.10 levels, respectively.

Appendix Table A: Variable Definitions

Panel A. Recommendation-based variables

Variable	Definition
influential	An indicator variable equal to 1 if the absolute value of the recommendation's abnormal return exceeds 1.96 * $\sqrt{2}$ * [standard deviation of the stock's idiosyncratic volatility] and the abnormal return has the same sign as the change in recommendation; 0 otherwise.
influential_any	An indicator variable equal to 1 if the analyst had ever made an influential recommendation on any stock prior to the current recommendation; 0 otherwise.
influential_same	An indicator variable equal to 1 if the analyst had ever previously made an influential recommendation on the same stock as the current recommendation; 0 otherwise.
star	An indicator variable equal to 1 if the analyst was ranked in the top 4 of the *Institutional Investor* survey for any category (in the top 3 or runner-up); 0 otherwise
away_from_consensus	An indicator variable set to 1 if the revision is away from the consensus; 0 otherwise.
momentum_pre	Raw return of the company measured over the (−69, −6) window
logbm	Log of firm's book-to-market ratio.
avg_logturn_pre	Average of pre-recommendation daily log turnover for the recommended stock. Daily log turnover is measured as the natural log of 0.00000255 (a small constant to avoid problems with zero values) plus the percentage of outstanding shares traded that day. The average of daily log turnover is calculated over the (−69, −6) window. Volume of Nasdaq stocks is divided by two.
std_resid_pre	Pre-recommendation idiosyncratic volatility measured as the standard deviation of the residual of a regression of the daily excess return on the three Fama-French factors for the (−69, −6) window.
logsize	Log of firm's market capitalization from the most recent fiscal quarter.
ana_count	Log of the number of recommendations for all analysts over the three months preceding the recommendation.

Panel B. Analyst-year-based variables

Variable	Definition
Annual_IR$_{t-1}$	An indicator variable equal to 1 if the analyst had at least one influential recommendation between May in year *t* -1 and April in year *t*; 0 otherwise.
early_IR$_{t-1}$	An indicator variable equal to 1 if the analyst had at least one influential recommendation between May and October in year *t-1*; 0 otherwise.
late_IR$_{t-1}$	An indicator variable equal to 1 if the analyst had at least one influential recommendation between November in year *t*-1 and April in year *t*; 0 otherwise.

Annual_blunder$_{t-1}$	An indicator variable equal to 1 if the analyst had at least one recommendation in year *t-1* for which the abnormal return was both a) greater than 1.645 * $\sqrt{2}$ * standard deviation of idiosyncratic volatility and b) signed in the *opposite* direction as the change in recommendation.
early_blunder$_{t-1}$	An indicator variable equal to 1 if the analyst had at least one blunder recommendation between May and October in year *t-1*; 0 otherwise.
late_blunder$_{t-1}$	An indicator variable equal to 1 if the analyst had at least one blunder recommendation between November in year *t-1* and April in year *t*; 0 otherwise.
avg_ret$_{t-1}$	The average abnormal return to all recommendations made between May in year *t*-1 and October in year *t.*
star$_{t-1}$	An indicator variable equal to 1 if the analyst was named a star in October of year *t-1*; 0 otherwise.
Nstocks$_{t-1}$	Number of unique stocks the analyst covered in year *t-1*.
Nrecs$_{t-1}$	Number of recommendations the analyst issued in year *t-1*.
pctbuy$_{t-1}$	Ratio of buy recommendations to all recommendation the analyst issued, expressed as a percentage in year *t-1*.
inforatio$_{t-1}$	Following Emery and Li (2009), we construct a nominal portfolio constructed from the analyst's recommendations over the prior year. *Inforatio* is the t-statistic for the intercept of a regression of the daily analyst recommendation portfolio returns on the industry portfolio in year *t-1*.
risklevel$_{t-1}$	The beta of the analyst's recommendation portfolio from an industry index regression in year *t-1*.
accuracy$_{t-1}$	A measure of relative earnings forecast accuracy developed by Hong et al. (2000), calculated here on a one-year basis for year *t-1*.
boldness$_{t-1}$	A measure of boldness in earnings forecasts developed by Hong et al. (2000), calculated here on a one-year basis for year *t-1*.
optimism$_{t-1}$	A measure of relative optimism in earnings forecasts developed by Hong and Kubic (2003), calculated here on a one-year basis for year *t-1*.
experience$_{t-1}$	Number of years the analyst has submitted reports to IBES for year *t-1*.