How does reputational capital affect professional behavior?
Evidence from analysts who become all-stars.

Michael J. Jung
University of Delaware
mjung@udel.edu

Yiqing Lu
New York University Shanghai
yiqing.lu@nyu.edu

Hong Wu
School of Accounting and Finance
Hong Kong Polytechnic University
hhkpuwu@polyu.edu.hk

July 2019

Acknowledgements: We thank Yiwei Dou, Kose John, Stephannie Larocque, Lian Fen Lee (discussant), Jeffrey Ng and workshop participants at New York University, University of Hong Kong, and the MIT Asia Conference for helpful suggestions and comments.

How does reputational capital affect professional behavior? Evidence from analysts who become all-stars.

Abstract

We examine whether increased reputational capital affects the behavior of capital market professionals using the setting of sell-side equity analysts and annual all-star rankings. Our identification strategy is based on a novel dataset and a quasi-experimental design to compare the forecast properties of, and the market reactions to, analysts who rank just high enough to be considered all-stars to analysts who rank just below and are not considered all-stars. We find that newly-ranked all-star analysts become more optimistic, bold, and frequent in their forecasts, but are less accurate; they also add more stocks to their coverage universe and generate higher market reactions from their forecast revisions. Overall, our results suggest that increased reputational capital emboldens professionals to take bolder actions to exert their market influence.

JEL Code: G23, G40, M41

Keywords: sell-side financial analysts, reputational capital, market reaction, professional behavior, all-star award, forecast optimism

1. Introduction

Winning a significant industry or professional award typically provides the recipient recognition, respect, opportunity, and influence, among other things (Malmendier and Tate, 2009). In this paper, we refer to these positive outcomes as an increase in reputational capital for the recipient. From an ex ante perspective, the tournament for professional awards could attract the most talented employees and encourage their best effort (Holmström 1982). However, from an ex post perspective, while the recipient may have strong incentives to maintain their reputation as it becomes a more valuable asset (Diamond 1989), winning the award could lead to unexpected subsequent behaviors. For example, increased reputational capital for a stock market professional could lead him or her to "exploit that capital" in ways (previously unavailable) intended to influence investment decisions and stock returns (Jensen and Meckling, 1976). As professional awards are prevalent in the financial industry, we believe understanding whether they change the behavior of capital market professionals is important to a broad audience.

We examine this question in the setting of sell-side equity analysts, where we use annual "all-star" rankings from *Institutional Investor* publication as the award that increases reputational capital. We posit that after an analyst becomes highly ranked as an all-star, he or she feels emboldened to exert their newfound market influence in ways that further their standing and visibility among institutional investors, as well as generate higher market reactions and trading commissions for their employing brokerage firm. In particular, we test whether the research output of newly-ranked analysts becomes more optimistic, bold, frequent, and associated with higher market reactions. We also examine whether these analysts expand their research coverage and continue on their career trajectory.

Our identification strategy is based a novel dataset and a quasi-experimental design to examine whether new annual rankings actually cause changes in the behavior of analysts who become all-stars. At the heart of our analyses are granular voting data from *Institutional Investor (II)* publication's annual All-American Research rankings and a regression discontinuity (RD) design that allow us to estimate the treatment effect of being a top-three-ranked, all-star analyst. The top three vote-getters are widely considered "all-stars," while those just below third place are called "runner-ups." As first illustrated by Thistlethwaite and Campbell (1960), a research design that compares the winners of an award to those who scored just below a cutoff or threshold and did not receive the award, can be used to estimate the treatment effect of the award itself across various outcomes. Important underlying assumptions in the design, which we believe hold in the setting of sell-side analysts, are that participants do not know, ex ante, what their scores or threshold will be, and thus, they cannot self-select to be just above the threshold.

To briefly illustrate our setting and data, we have analyst all-star voting data from 2001 to 2014, and with an average of 61 industries per year, there are 849 industry-years in which sell-side analysts are ranked. Each sell-side analyst earns a composite score (typically from 0 to 40) that is weighted by assets under management of each buy-side analyst and portfolio manager who casts a vote. The sell-side analyst with the highest composite score in each industry-year is named "First-Place All-American," followed by the "Second-Place" and "Third-Place" analysts. All other analysts whose score are within 35% of third place are named "Runner-Up." There can be over fifty analysts in a given industry-year, and the vast majority of them do not earn a score high enough to be named an all-star or runner-up. For reasons that we explain later in this paper, comparing the third-place analyst with the first runner-up analyst (i.e., the fourth-place analyst) provides the best matched-pair for the RD design. Their composite scores, as a measure of their value to institutional investors, differ by only a few points, with the mean and median differences being 1.57 and 1.28, respectively. To insure that we compare analysts with a small absolute difference in scores,[1] we require in our RD design that the analysts' score differences be within narrow bandwidths of 1.5 and 1.0, as well as an optimal bandwidth estimated by a non-parametric approach proposed by Calonico, Cattaneo, and Titiunik (2014).

We begin our analyses by validating the assumption that within an industry-year, a third-place analyst is comparable to the first runner-up analyst across several dimensions *prior* to the rankings. First, we show that in the year *before* the rankings, measures of forecast properties, such as earnings forecast optimism, accuracy, boldness, and frequency, do not predict the current year's all-star status. That is, the two analysts covering the same firm in the same year who scored very closely in the *II* voting do not differ significantly in their forecast properties prior to the rankings. Second, we validate the assumption of the continuity of composite score differences between third-place and first runner-up analysts, as well as show that the requirement to have score differences within narrow bandwidths still allows for a large and significant share of the analysts to be included in our analyses. Overall, we do not find a systematic pattern in scores that is suggestive of manipulation or self-selection. Our conclusion from the several validation tests is that the matched-pair of analysts are similar across all characteristics except that one has become a top-three-ranked all-star and the other has not.[2]

Next, we test whether earnings forecast properties differ between the matched-pair of analysts *after* the rankings. We focus on earnings forecast properties because they have been used in prior studies as proxies for changes in analyst behaviors (Hong, Lim, and Stein 2000; Hong and Kubic 2003), but more importantly, they should be correlated with other, more ideal outputs of

---

[1] Even with these small average differences, there could still be instances in which a third-place analyst receives a score of, say 20, and a first runner-up receives a score of 13, which is still within 35% of 20 but is arguably not close in absolute terms. Therefore, we implement bandwidths with small absolute differences in our empirical tests.
[2] Throughout this paper, we often refer to the first runner-up analyst as a non-all-star, although it should be noted that the first runner-up analyst still ranks much more highly than the average analyst.

analyst research that institutional investors care about, but which are unobservable.[3] We conjecture that changes in forecast properties are reflective of the newly-ranked all-star analyst leveraging his or her increase in reputational capital to begin publishing research that stands out more from consensus opinion, possibly at the expense of accuracy, and "flexing" their increased influence to generate trading commissions for his or her employing brokerage firm (Jackson 2005). We find that newly-ranked all-star analysts become more optimistic, bold, and frequent in their forecasts, but are also less accurate, relative to non-all-star analysts. We also find weaker evidence that suggests all-star analysts are more positive in their recommendation levels than non-all-star analysts. Our analysis is robust to parametric estimation including polynomials of various orders, following Lee and Lemieux (2010) using the full sample, and non-parametric estimation based on narrow bandwidths of 1.5 and 1.0, and the optimal bandwidth approach proposed by Calonico et al. (2014).

To further corroborate our hypothesis, we test whether market reactions to forecast revisions differ between the matched-pair of analysts *after* the rankings. We find that market reactions are greater for forecast revisions by all-star analysts relative to non-all-star analysts. This result suggests that after the rankings, the third-place analyst can "move the market" more than a first runner-up analyst, consistent with the idea that award-winning analysts have greater reputational capital. Given that the third-place analyst and first runner-up analyst were similar in forecast properties prior to the rankings, and that their composite scores were very close in the rankings, our results suggest a causal relation between all-star distinction and a change in analyst forecast behavior, as well as investors' reactions to that change in behavior.

[3] We discuss in the next section the attributes that institutional investors care most about when voting for analysts in the annual all-star rankings.

In additional analyses, we find that our main result on all-star status and increases in forecast optimism is more pronounced for analysts who were an all-star in the past, which suggests that past reputational capital could reinforce the effect of increased reputational capital in the present. Other additional tests show that a new all-star analyst increases the number of stocks they cover, compared to a non-all-star analyst. They are also more likely to change employers, and descriptive evidence suggests that a majority of third-place analysts continue their career momentum to advance higher in subsequent rankings.

This study makes several contributions. First, our findings provide novel evidence that an increase in reputational capital for capital markets professionals can lead to changes in professional behavior. In particular, our results suggest that after sell-side analysts become all-stars in the annual *Institutional Investor* rankings, they tend to change their forecasting behavior and generate greater market reactions, consistent with them using their increased reputational capital to influence the markets. A few prior studies have examined a similar question in the setting of corporate CEOs and find that they tend to increase their perquisite consumption following an award. For example, Malmendier and Tate (2009) find that award-winning CEOs subsequently underperform, both relative to their prior performance and to a matched sample of non-winning CEOs, suggesting that an award is associated with lower shareholder wealth. Our paper complements this literature by showing that award-winning analysts may have similar changes in behavior as CEOs to exploit their reputational capital.

Second, this paper contributes to the analyst literature by making causal inferences between the rankings and analyst forecast behavior. We use a more detailed dataset and rigorous research design to alleviate the problem caused by analyst and firm-level omitted variables that confound the effect of analysts' all-star status on analyst behavior. For example, our causal inferences confirm prior positive associations between all-star status and forecast frequency and stock price impact (Stickel 1992), as well as associations between forecast optimism and boldness with career outcomes (Hong and Kubik 2003). However, our findings on a decline in relative accuracy is inconsistent with prior studies that suggest all-star analysts are generally more accurate (Stickel 1992) or that forecast accuracy is a primary driver of analyst promotions (Mikhail et al. 1999; Hong el at. 2000; Hong and Kubic 2003). Instead, our archival data-based findings are consistent with other studies that use proprietary data from one or two brokerage firms (Groysberg et al. 2011) or survey respondent data (Brown et al. 2015) that suggest analysts are not incentivized, compensated, or promoted based on forecast accuracy.

Third, our study contributes to the broader literature on the effect of rankings. Prior studies have examined the causal effects of rankings on the investment decisions of finance professionals (Dijk et al. 2014; Kirchler et al. 2018). These studies were experimental in nature and used laboratory subjects, while our analysis is based on large-sample archival data. Finally, we believe our study is generalizable to isolating and identifying the causal effects of relative performance rankings on other capital market participants.

The rest of this paper is organized as follows. Section 2 reviews the institutional background and develops our empirical predictions. Section 3 describes our empirical research design and sample construction. Section 4 discusses the results, and Section 5 concludes.

2. Background and Hypotheses Development

2.1 Background on Institutional Investor Rankings

Institutional Investor (II) is a trade publication in the asset management industry. Each year in October, it publishes a survey of institutional investors (portfolio managers and buy-side analysts) who vote for the “best” sell-side analysts. The analysts are grouped into industries, the votes are weighted by assets under management, and rankings are created. The sell-side analyst with the highest score in each industry-year is named "First-Place All-American," followed by the "Second-Place" and "Third-Place" analysts. Collectively, these analysts have been referred to in the literature as "all-stars" or "all-Americans" (e.g., Hong et al. 2000, Hong and Kubik 2003).

Voters consistently cite industry knowledge, professionalism, accessibility, access to management, and special services as the most important reasons in voting for particular analysts (Bradshaw 2011). However, these attributes are generally unobservable and arguably time-invariant, and thus, prior studies have focused on more observable and time-varying factors such as written reports, stock recommendations, and earnings forecasts when examining changes in analyst behaviors and incentives. These factors are also cited by institutional investors as criteria for voting, and although they rank slightly lower in voters' minds, they should be correlated with the attributes voters care most about. Therefore, in our study, we also focus on earnings forecast properties as proxies for changes in analyst behaviors.

Academic research has shown that the rankings are an important industry recognition. Early research showed a positive association between all-star analysts and forecast frequency, accuracy, and market reaction to forecast revisions (Stickel 1992), although there was no definitive conclusion about causality. Firm managers appear to respond favorably to all-star analysts, as Soltes (2014) finds that all-star analysts are associated with a higher tendency to meet with management in a given month, and Mayew (2008) finds that all-star analysts are less likely to be shut out of earnings conference calls even if they have a negative rating on a firm. Such access to management is consistent with the attributes of sell-side analysts valued by institutional investors (Bradshaw 2011). Brokerage firm employers also view the distinction positively, as all-star analysts contribute to the performance of their investment banks by generating higher trading commissions (Jackson 2005) and attracting investment-banking clients (Dunbar 2000, Krigman et al. 2001, Clarke et al. 2007).

Brown at al. (2015) interview sell-side analysts to ask their views about all-star rankings. One analyst described it as "your external stamp of approval" and, consistent with prior research (Mayew 2008, Soltes 2014), said that because the *II* results are visible to outsiders, "Your access to management teams is greatly increased by your *II* ranking." Another said, "The *II* rankings . . . give you significant leverage within your own firm" because *II*-rated analysts can easily find employment elsewhere. Analysts indicate that, although broker votes are more important than *II* rankings for their career advancement, both forms of recognition provide analysts with valuable benefits (Groysberg et al. 2011). In summary, the rankings published by *II* are viewed by institutional investors, firm managers, and the sell-side analysts themselves as an important industry recognition that has been shown to be associated with compensation and career advancement. Therefore, in our study, if an analyst achieves all-star status (i.e., one of the top 3 places) in a given year, we view that analyst as having experienced an increase in reputational capital, relative to the next analyst (i.e., first runner-up or fourth place).

2.2 Prior Literature on Behaviors of Finance Professionals

The causal effect of relative performance rankings on the incentives and behaviors of financial professionals has been the focus of several experimental studies. Dijk et al. (2014) find that a desire for a higher ranking—driven by social status rather than monetary payoffs—influences subjects to alter their attitudes towards risk and portfolio choice. Outperformers, or those with an above-average ranking, are more likely to select negatively skewed assets, while underperformers prefer positively skewed assets. The results indicate that those at the top of the rankings reduce their risk to stay near the top, while those at the bottom increase their risk in an effort to move toward the top. Similarly, Kirchler et al. (2018) find that laboratory subjects who are finance professionals, but not students, respond to information about their relative performance rankings; underperformers increase their risk-taking behaviors. Both Dijk et al. (2014) and Kirchler et al. (2018) show that monetary incentives have little effect above and beyond the rank incentives, which alone resonate with finance professionals' concern for social status and relative standing among their peers.

While the effect of rankings on the incentives and behaviors of sell-side analysts, specifically, have not been examined in prior studies, there have been studies examining the association between career concerns and earnings forecast properties. Analyst career concerns are of interest to researchers and practitioners because conflicts of interests can cause analysts to bias their research (Michaely and Womack 1999, Jackson 2005). Hong et al. (2000) find that career concerns motivate inexperienced analysts to herd in their forecasts, rather than deviate from the consensus. In other words, experienced analysts are bolder in their forecasts than inexperienced analysts. Hong and Kubik (2003) find that analysts who make optimistic forecasts are more likely to move to higher-status brokerage houses. The results of both studies suggest that sell-side analysts are concerned about their reputation and are motivated by the status that comes with working for a more prestigious brokerage house.

2.3 Empirical Predictions

In this subsection, we discuss our predictions for several commonly-examined properties of analyst forecasts—optimism, accuracy, boldness, and frequency—subsequent to an increase in reputational capital for an all-star analyst relative to an analyst who just missed the all-star distinction. In addition, we develop a prediction for how investors react to forecast revisions from all-stars relative to non-all-stars.

Sell-side analysts have incentives to generate trading commissions for their employing brokerage firm (Hayes 1998, Jackson 2005). Analysts who are more positive on the stocks they cover tend to generate higher trading commissions. One reason is that, with or without short-sale constraints, investors are typically limited in the number of shares they can sell of a particular stock when analysts are negative, relative to the number of shares they can buy when analysts are positive. However, analysts who are repeatedly opportunistic in biasing their forecasts upward face negative repercussions to their reputation among institutional investors (Jackson 2005). Thus, an analyst considers a trade-off between being optimistic to generate trading commissions and not being overly optimistic to maintain or build their reputation. In our setting, where an analyst experiences a significant increase in reputational capital, we expect that the analyst now has more leeway to be optimistic. Therefore, our first prediction is that after an analyst becomes an all-star, their forecasts become more optimistic, on average, relative to the next lower-ranked analyst who did not become an all-star.

With increased optimism in forecasts, however, it is not clear whether ranked analysts compromise on their forecast accuracy. For example, consider the following scenario. The consensus forecast (or the mean forecast for multiple non-all-star analysts) for a given firm is $0.50 earnings per share, while an all-star analyst has a relatively more optimistic forecast of $0.55. If the firm reports EPS of $0.52 or less, then the all-star analyst is more optimistic but *less* accurate than the consensus. In contrast, if the firm reports EPS of $0.53 or more, then the all-star analyst is more optimistic and *more* accurate than the consensus. Therefore, we test for changes in forecast accuracy between ranked and unranked analysts without a directional prediction.

Another commonly examined analyst forecast property is boldness, which is a measure of how far an analyst's forecast for a given firm deviates from the consensus forecast (Clement and

Tse 2005; Clarke and Subramanian 2006; Jegadeesh and Kim 2009). Analysts whose forecasts are near the consensus are viewed as herding with the crowd, which tends to occur when an analyst is concerned about the loss of reputation from being incorrect (Hong et al. 2000, Hong and Kubik 2003). However, we conjecture that analysts who experience an increase in reputational capital are likely to be emboldened to stand apart from the herd. Doing so enables an analyst to be more visible to buy-side clients (i.e., institutional investors). In fact, there is empirical evidence that some analysts systematically anti-herd by underweighting the consensus forecasts (Bernhardt et. al 2006). Therefore, we predict that after an analyst becomes an all-star, their forecasts become bolder, on average, relative to the next lower ranked analyst who did not become an all-star.

Next, we examine whether an increase in reputational capital affects an analyst's forecast frequency. Assuming that analysts who have become all-stars have gained industry influence in the stocks they cover, each time they revise their expectations should lead to a market reaction in those stocks. Therefore, for reasons similar to the previously-discussed relationship between forecast optimism and trading commissions, we expect that after an analyst becomes an all-star, the frequency of their forecasts increases, on average, relative to the next lower ranked analyst who did not become an all-star.

Finally, we examine whether an increase in an analyst's reputational capital changes how the market reacts to the analyst's research. The *II* rankings are a direct measure of institutional investors' recognition of individual analysts, and those who have received the all-star distinction should be perceived to be the most influential in their respective industries. Therefore, we expect to observe greater abnormal stock returns to the forecast revisions of all-star analysts relative to non-all-star analysts.

3. Empirical Design and Sample Construction

3.1 Implementing RD Design to Measure Analyst Ranking Effects

In this sub-section, we describe how we implement the regression discontinuity (RD) design to measure the effect of analyst ranking on forecast boldness. We use a similar procedure to examine other outcome variables such as forecast optimism, error, frequency, recommendation level, and market reaction. We provide additional background and details of RD designs in Appendix 1.

Consider the case in which we wish to test whether analyst A ranked in the i^{th} place has significantly different forecast boldness than analyst B ranked in the $(i+1)^{th}$ place; note that the i^{th} place is ahead of the $(i+1)^{th}$ place. Also, let $y_{A(B)}$ refer to the forecast boldness for the analyst A(or B) and $Score_{A(B)}$ refer to the score for analyst *A(or B)*. For every pair of analysts who are adjacent in their rankings in i^{th} and $(i+1)^{th}$ place, let z_{AB} be the difference in their composite scores. From the i^{th} analyst (or analyst A)'s perspective, z_{AB} is a positive value, and from the $(i+1)^{th}$ analyst (or analyst B)'s perspective, z_{AB} is a negative value. Then select the bandwidth θ for the maximum absolute value z_{AB}. The bandwidth can be an arbitrarily small value, and different values can be used in robustness checks. Next, retain observations in which the score differences are within the bandwidth θ. The RD design compares observations for which $0 < z_{AB} < \theta$ with those for which $-\theta < z_{AB} < 0$. Thus, we retain observations for which $|z_{AB}| < \theta$. Finally, estimate the ranking effect by using both a parametric approach that utilizes the full sample (also called a global regression) and a non-parametric regression that utilizes a subset of the data within the bandwidth (also called a local regression).

For the parametric approach, we follow Lee and Lemieux (2010) to approximate the underlying relationship between analyst forecast boldness $y_{A(B)}$ (the dependent variable) and score difference z_{AB} with the inclusion of four polynomials of the first and second order. The ranking effect is represented by coefficient β in regression Equation 1 below.

$$y_{A(B)} = \alpha + \beta \cdot 1(pos_{A(B)} = i + 1) + p(z_{AB}, r) + p(z_{AB}, l) + f(A, B) + \text{Controls} + \varepsilon_i \qquad (1)$$

The term $1(pos_{A(B)} = i + 1)$ is an indicator variable for whether the analyst is in the lower ranking (i.e., first runner-up or fourth place), $p(z_{AB}, r)$ includes two polynomials of the first and second order of score differences for observations on the right-hand side of the ranking (i.e., positive score difference), and $p(z_{AB}, l)$ includes two polynomials of the first and second order of score differences for observations on the left-hand side of the ranking (i.e., negative score difference). The polynomials capture any continuous relationship between z_{AB} and y_j , in particular, the effect of any confounding factors that are correlated both with the score and analyst characteristics in a continuous way.[4] In equation (1), β is the ranking effect of interest and the term $f(A, B)$ includes a set of fixed effects and controls. As noted earlier, we examine multiple dependent variables, some are at the analyst-firm-forecast level, some are aggregated to the analyst-firm-year level, and some are at analyst-year level. For the first two types of variables, the set of fixed effects includes firm-year fixed effects and brokerage-year fixed effects. The former fixed effects insure that we compare third-place and first runner-up analysts who cover the same firm in the same year, while the latter is included because brokerage houses experience mergers from time to time and vary in resources, client bases, and investment banking affiliations. At the analyst-year level, we control for only brokerage-year fixed effects. Appendix 3 contains all variable definitions.

For the non-parametric approach, we follow Calonico et al. (2014), who approximate the regression function on either side of the ranking by a weighted polynomial regression. The weights

[4] We also checked that all results are robust to using polynomials of higher orders.

are computed by applying a kernel function on z_{AB}. The coefficient β is then estimated as the difference between these non-parametric regression functions on either side of the ranking. This approach improves on earlier non-parametric estimators by calculating the optimal bandwidth that overcomes limitations of earlier non-parametric RD estimators (which tend to lead to bandwidths that are too large). Another practical implementation of RD involves finding these limiting values non-parametrically using a local regression, often simply a local linear regression within a pre-specified bandwidth θ and then assessing sensitivity to several bandwidths. We choose bandwidths of 1.5 and 1.0 for additional checks. As we will see, the results are fairly similar across these non-parametric estimation approaches. In these local regressions, we also include $p(z_{AB}, r)$ and $p(z_{AB}, l)$ of order 1 to control for the variation in the forcing variable in two sides of the ranking.

In Section 4.1, we discuss additional validation tests to provide further support for our RD design. Furthermore, it is important to note that for an RD design to be valid, it should be the case that the only source of discontinuity is the treatment (i.e., the all-star distinction in our case). One consequence of this condition is that RD is invalidated if there is self-selection near the threshold. That is, if an analyst: i) knew where the threshold was to become an all-star, ii) knew that he or she was near the threshold, and iii) could exert extra effort to exceed the threshold, then the RD design in the analyst ranking context would not be valid. Thus, it is vital to examine the institutional details of the *II* voting process to ensure that self-selection is not likely.

Based on our discussions with the *II* representative who provided our data, as well as several sell-side analysts who follow the rankings, it is our understanding that prior to the rankings being published each year, analysts are not aware of their own composite score, any other analyst's composite score, or the threshold score required to be ranked within the top three places with their industry. Furthermore, even after the rankings are published and analyst can observed everyone's composite scores, the votes cast by individual buy-side analysts and portfolio managers are not disclosed. This inability to observe individual votes satisfies the conditions for validity of an RD designed specified in Hartmann, Nair, and Narayanan (2011), with the analyst being uncertain about their composite score and score differences between analysts. Therefore, it would not appear possible for any individual analyst to know, *ex ante*, that he or she is near the threshold to be an all-star or to strategically self-select to be just above or below that threshold. Absent any self-selection, researchers can consider analysts just above and just below the all-star threshold (or any arbitrary threshold) to be nearly equivalent in terms of underlying abilities, and any difference between the limiting values of the outcomes on the two sides of the threshold can be entirely attributed to the all-star distinction. For these reasons, we believe our setting is ideal compared to other settings using RD designs in which the cutoff is publicly known in advance (e.g., Cunat, Gine and Guadalupe, 2012; Chen et al., 2013).

3.2 Sample Construction and Summary Statistics

We obtain annual sell-side analyst ranking data directly from *Institutional Investor (II)*. While the printed edition typically contains the names of the analysts who placed in the top three and runner-up positions for each industry-year, the data we obtain also includes their composite scores, which are computed from buy-side votes weighted by assets under management.[5] The composite score is a continuous variable, and the score difference between the third-place analyst and first runner-up analyst serves as the forcing variable in our RD design.

We have analyst rankings data from 2001 to 2014 for all industries. The number of industries per year declines gradually from 71 early in the sample period to 57 by the end of the

[5] *Institutional Investor* announced in March 2018 that it was discontinuing the print edition of its magazine, opting to become a fully digital publication starting April 1, 2018.

period due to *II* consolidating the number industries over time.[6] With an average of about 61 industries per year, there are a total of 849 industry-years of rankings data. Recognition went to 849 first-place analysts, 851 second-place analysts, 845 third-place analysts, 754 first runner-up analysts, and 1,032 other runner-up analysts.[7] However, since an analyst may rank highly in multiple years and even rank in multiple industries within the same year, the number of unique analysts in our data is not a simple sum of the placements. After consolidating to unique analyst names, we identify 879 unique analysts in the *II* rankings data. Table 1, Panel A contains summary statistics of our sample of analysts.[8]

[Insert Table 1 about here]

Table 1 Panel B contains descriptive statistics of analyst composite scores by rank. The mean composite score for first-place analysts is 19.76, followed by second-place analysts at 14.70, third-place analysts at 11.79, first runner-up analysts at 9.89, and all other runner-up analysts at 8.12. For our RD design, we focus on comparing the third-place analyst and first runner-up analyst in each industry-year. The mean and median difference between the composite scores between these analysts is 1.57 and 1.28, respectively.

We then match each analyst from the *II* rankings data to the IBES datasets. Using the Detail Recommendations file, we manually match each analyst's last name, first initial, and brokerage firm affiliation, in order to obtain their unique IBES identifier. Not all brokerage firms are covered by IBES, but we are able to match 861 (about 98%) of the analysts from the *II* rankings to the

[6] We exclude the entire "Macro" sector because the analysts do not cover specific firms; instead, they provide advice on macroeconomic strategies. The industries included in this sector are Accounting & Tax Policy, Convertibles, Economics, Equity Derivatives, Equity-Linked Strategies, Portfolio Strategy, Quantitative Research, Small Companies, Technical Analysis, and Washington Research.

[7] There are a few instances of ties for second-place, no third-place due to lack of requisite number of votes, and runner-ups must be within 35% of third-place to be recognized. See Appendix 2 for details of the voting procedure.

[8] Our consolidation of analysts down to unique analyst names accounts for instances in which an analyst changed her last name from a maiden name to a married name, as well as analysts who changed their preferred first name.

IBES datasets. The primary reason for the high successful match rate is that the vast majority of ranked analysts work for the major bulge bracket investment banks and brokerage firms, and these banks and analysts are well covered in the IBES datasets. Table 1 Panel C shows descriptive statistics of the scores of third-place analysts, first runner-ups, and their difference, after merging the II-ranking data with IBES data. The scores are very similar to those presented in Panel B, indicating that the requirement to match the *II* rankings data to the IBES data does not induce any bias.

[Insert Table 2 about here]

In Table 2, we report the summary statistics of forecast errors, absolute value of forecast errors, boldness, forecast frequency, and recommendation levels of the third-place analysts and the first runner-up. *Forecast Error* is constructed for each annual EPS forecast issued by an analyst; it is defined as the forecast minus the actual value of annual EPS for the *same* fiscal year period. We include all forecasts announced before the release of the actual EPS.[9] To construct *Forecast Boldness*, we rank all the analysts covering the same company by absolute deviation from the consensus mean of forecasts of all analysts for the same firm each year. Rankings are then scaled such that boldness ranking is valued from 100 to 0, with higher values indicated more boldness (Clarke and Subramanian, 2006). For *Forecast Frequency*, we count the number of annual forecasts made by each analyst in each year for a given firm. For *Recommendation Level*, we use coding of 5=Strong Buy, 4=Buy, 3=Hold, 2=Sell, and 1=Strong Sell to compute the average rating an analyst has for all his or her covered firms. Table 2 shows that, in the year *prior* to the rankings,

[9] If the forecast is made very late, then the error is usually small. If the forecast is made very far in advance, then the error is usually large. So the timing of when the forecast is made is important. Thus, in regression analyses, we also control for the time (in months) between the announcement of rankings and the announcement of forecasts.

forecast error for third-place analysts is on average 0.190, and they make 4.48 forecasts each year. First runner-up analysts have on average forecast error of 0.226 and make 4.83 forecasts each year.

4. Results

4.1.Validation

Underlying our identification approach are two assumptions: the lack of pre-existing differences in analyst forecast ability and the continuity of score difference in the neighborhood of the two positions in the rankings (third-place and first runner-up). In this section, we provide validation tests to show that the two assumptions behind our identification strategy hold.

[Insert Table 3 about here]

In Table 3, we investigate whether there are any systematic pre-existing differences in forecast characteristics during the year before the rankings are published. The main assumption of the design is that there are no systematic differences in characteristics from third-place analysts who are ranked marginally higher, relative to the first runner-up analysts who are ranked marginally lower. We test whether their forecast errors, the absolute value of their forecast errors, forecast boldness, and forecast frequency could significantly predict rankings in the next year.

For forecast errors and their absolute values, we compute their means at analyst-firm-year level to predict whether either analyst is more likely to be ranked as the first runner-up next year. We restrict our comparison to analysts whose score differences are within optimal bandwidths, and introduce polynomials, different for the third-place and runner-up analysts, up to order 1 in all specifications. We include brokerage house×year fixed effects in all specifications and firm×year fixed effects in Column 2. We find that analysts in the close neighborhood of the two positions do not differ significantly in any of the forecast characteristics. This absence of statistically significant differences around the discontinuity lends support to our identification strategy.[10]

[Insert Figure 1 about here]

We also examine our data visually to check for continuity. First, Figure 1a shows the distribution of score differences within the sample. Third-place analysts have a positive score difference from first runner-ups, thus represented in the right half of the distribution. First runner-up analysts have a negative score difference from the third-place analysts, thus represented in the left half of the distribution. We note two observations. First, as the differences are the same for each pair of third-place and first runner-up analysts, the average and median score difference is zero. Around 50% of the observations fall within the (−1, +1) bandwidth, and around 75% of the observations fall within the (−1.5, +1.5) bandwidth. This coverage implies that our RD coefficient is estimated from a large and significant share of the third-place and first runner-up analysts and, hence, can be thought of as representative of the effect of rankings. Second, the fact that much of the density is concentrated around zero rejects the possibility of systematic self-selection by third-place analysts to obtain a score significantly higher than the first runner-up. If there were self-selection, then we would see very few observations around zero and a spike in density in both left and right sides around zero.

Second, based on McCrary (2008), we estimate in Figure 1b the density function and do not find a discernible spike in density in both left and right sides around zero, suggesting that there is no strategic self-selection. Our validation tests are consistent with the institutional details discussed in Section 3.2 in which it is not likely that analysts near an unobservable threshold can self-select into a third-place ranking.

[10] In untabulated univariate tests, we find that earnings forecast optimism, accuracy, boldness, and frequency, as well as recommendation levels, do not differ statistically between the third-place and runner-up analysts.

4.2. Forecast Behavior

4.2.1. Forecast Optimism

In this section, we test our first prediction that all-star analysts, relative to non-all-star analysts, become more optimistic in their forecasts. We estimate regression equation (1) at the analyst-firm-forecast level and present the results in Table 4.

[Insert Table 4 about here]

The dependent variable in Table 4 is *Forecast Error*, the difference of each forecast and the actual value of annual EPS for the same fiscal year forecasted. Models in Columns (1) and (2) follow parametric estimation and use the full sample. Models in Columns (3) and (4) follow non-parametric estimation using the bandwidths generated by the approach proposed by Calonico et al. (2014) with triangular kernel functions and take the optimal bandwidths. Models in Columns (5) and (6) follow parametric estimation and use the sample of analysts whose scores fall within bandwidths of (−1.5, +1.5), and Models in Columns (7) and (8) use analysts whose scores fall within bandwidths of (−1, +1). We introduce polynomials, different for the third-place and first runner-up analysts, up to order 2 in Columns (1) and (2), and up to order 1 in Columns from (3) to (8). In Columns (2), (4), (6), and (8) we control for the time (in months) between the announcement of rankings and the announcement of forecasts. All standard errors are reported in parenthesis under each coefficient estimate.

We find that, across almost all models, first runner-up (third-place) analysts make less (more) optimistic forecasts, and the differences are statistically significant. For example, when the full sample is used, Column (2) indicates that third-place analysts, on average, make forecast errors that are $0.086 higher than first runner-up analysts. When the optimal bandwidth is imposed, Column (4) suggests that third-place analysts, on average, make forecast errors that are $0.166

higher than first runner-up analysts. Overall, the findings in Table 4 indicate that analysts become more optimistic in their forecasts after becoming an all-star, relative to the next ranked analyst who did not become an all-star, which is consistent with our prediction.

4.2.2. Forecast Accuracy

In this section, we explore whether all-star analysts are more or less accurate than the non-all-star analysts. We estimate the difference in absolute forecast errors for third-place analysts and the first runner-up analysts whose scores are sufficiently close, and present the results in Table 5.

[Insert Table 5 about here]

Using the same specifications in Table 4, we find that across the local regressions (Columns (3) to (6)), first runner-up (third-place) analysts make more (less) accurate forecasts, and the differences are statistically significant. For example, when the optimal bandwidth is imposed, Column (4) suggests that the absolute forecast errors of third-place analysts are greater than first runner-up analysts by $0.114. The findings in Table 5, along with Table 4, suggest that analysts who become all-stars sacrifice some forecast accuracy to be more optimistic.

4.2.3. Forecast Boldness

We next test for differences in forecast boldness between third-place and first runner-up analysts. To construct the boldness measure, we first calculate the absolute deviation of the mean of each analyst's forecasts from the consensus mean of forecasts of all analysts for the same firm. Following Clarke and Subramanian (2006), we rank all the analysts covering the same firm by the absolute deviation computed above. The analyst that is farthest in absolute value from the consensus mean is considered the most bold, and all other analysts covering the same firm in the same year are ranked accordingly. Rankings are then scaled such that boldness ranking is valued from 100 to 0, with higher values indicated more boldness. We estimate regression equation (1) at the analyst-firm-year level and present the results in Table 6.

[Insert Table 6 about here]

The results in Table 6, across all specifications, indicate that the forecasts of first runner-up analysts are less bold than the forecasts of third-place analysts. The coefficients for *First runner-up* are negative and significant at the 1% or 5% level. The results are consistent with our expectation that an analyst with an increase in reputational capital seeks to stand out from his or her peers and not herd in their forecasts.

4.2.4. Forecast Frequency

Thus far, our results indicate that ranked analysts become more optimistic and bold but less accurate in their forecasts relative to unranked analysts. In this section, we examine whether such changes in forecasting behavior is also accompanied by more frequent forecast revisions. We estimate regression equation (1) at the analyst-firm-year level and present the results in Table 7.

[Insert Table 7 about here]

The dependent variable is *Frequency*, measured by the number of forecasts made by each analyst in each year. We find mixed results, with the coefficient on *First runner-up* being positive in the global regression shown in column (1), and the coefficients being negative in the local regressions shown in columns (2), (3), and (4). However, the global regression is not as rigorous as the local regressions in controlling for unobservable differences, and thus, we place more emphasis on the latter results. When the optimal bandwidth is imposed, Column (4) suggests that third-place analysts, on average, make 1.445 more forecasts than first runner-up analysts. Overall, our findings in this section suggest that analysts tend to make more frequent forecast revisions after becoming an all-star analyst.

4.2.5. Recommendation Level

We also examine whether all-star status affects analysts' recommendation levels. Table 8 presents the results of our analysis, conducted at the analyst-firm-forecast level. Only in columns

(7) and (8) do we find a significant negative coefficient for *First runner-up,* suggesting that first runner-up analysts are more negative in the recommendation levels. One possible explanation for the lack of significant coefficient in the other specifications is that recommendation levels are sticky in that the vast majority of recommendations are reiterations (Chen, Jung, and Ronen 2017). In other words, analysts do not change recommendation levels as often as earnings forecasts. Therefore, there is likely less power to detect differences in recommendation levels between third-place and first runner-up analysts.

[Insert Table 8 about here]

4.3.Market Influence and Career Prospect

4.3.1 Stock Market Reaction

In this section, we study the underlying incentives for all-star analysts to become more optimistic, bold, and frequent in their forecasts. We posit that this change in forecasting behavior is an effort to generate greater market reactions and trading commissions for his or her employing brokerage firm. To examine this explanation, we test whether market reactions to forecast revisions differ between the third-place and first runner-up analysts after the rankings.

[Insert Table 9 about here]

The dependent variable in Table 9 is the 7-day cumulative abnormal returns (CARs) over a window of [-3, 3] around the announcement of each forecast revision. We identify each analyst's forecast *Revision* by subtracting from each forecast its previous forecast made by the same analyst for the same firm and further scale it by the firm's stock price at the fiscal year beginning. To avoid confounding effects, we drop forecast revisions that are announced by both the first runner-up and the third-place analysts within the same 7-day window. Because greater revision could lead to greater market reaction, we estimate the sensitivity of market reaction to each forecast revision by introducing the interaction term of forecast *Revision* and *First runner-up*, while controlling for the respective main effects. While the coefficient of *First runner-up* indicates the level effect of the all-star award on market reaction, the coefficient of the interaction term indicates the difference in market reactions between the third-place analyst and the first runner-up analyst per one unit of revision in analyst forecasts. Panel A reports results of market reactions in the [-3, +3] window around the forecast revision announcement dates; for robustness, Panel B reports results of 5-day market reactions in the [-2, +2] window around the forecast revision announcement dates.

We find that being ranked third place is associated with greater market reaction to forecast revisions. In all columns, we find that the coefficients on *Revision* are positive and significant, consistent with prior studies. We also find that the coefficients on *First runner-up* × *Revision* are negative and significant, suggesting that market reactions to forecast revisions by third-place analysts are more positive than those by first runner-up analysts. Our finding is consistent with our earlier results that third-place analysts make more optimistic, bold, and frequent forecasts, likely to exert their market influence.

4.3.2. Career Prospect

In this section, we study whether all-star analysts change their coverage of stocks or change their employer. Specifically, we examine new stock coverage and job turnover in Panel A and B of Table 10, respectively.

[Insert Table 10 about here]

Specifications in Table 10 are at the analyst-year level. For each analyst-year, new stock coverage is computed as the number of new stocks covered in that year scaled by the total number of stocks covered in that year. Job turnover is a dummy variable that equals one if the analyst moves to a different brokerage house within one year after the ranking. We control for brokerage house-year fixed effects in all specifications.

Panel A of Table 10 shows that all-star analysts cover significantly more new stocks than first runner-up analysts. For example, when imposing the optimal bandwidth, we find that an all-star analyst covers 5% more new stocks than a first runner-up. This finding suggests that all-star analysts are able to expand their job domain. In Panel B, we find that all-star analysts are more likely to change their employer than the non-all-star analysts. Although we are not able to tell whether these analysts receive a more favorable employment contract from their new employer, combining the two results of Table 10, we believe that all-star analysts are more likely to gain greater career opportunities than the first runner-up.

Overall, we believe that increased reputational capital brings analysts better career prospect in terms of new stock coverage and more turnover opportunities. Consistent with the increased influence in the stock market, our results suggest that the all-star analyst award brings material benefits, potentially making all-star analysts less concerned about the loss of reputation from exploiting the increased market status.

4.4.Cross-sectional Analysis: Past All-star Analysts

Previous analysis shows that the increased reputational capital could embolden all-star analysts to make more optimistic forecasts. We conjecture that such effect should be more pronounced for all-star analysts who in past years also earned an all-star status (i.e., repeat winners), as they have accumulated more reputational capital than an analyst who just won the award for the first-time.

[Insert Table 11 about here]

In Table 11, we test this cross-sectional effect. *Past All-Star* is a dummy variable that equals 1 if the analyst was an all-star in any of the past three years. We interact this dummy variable with another dummy variable that equals 1 if the analyst is ranked the third place this year. Other specifications follow Table 4. Indeed, we find that past reputational capital emboldens an all- star analyst to make more optimistic forecasts than a first-time all-star analyst. This result suggests that past reputational capital could reinforce the effect of increased reputational capital on analyst forecast behavior.

4.5.Next Year's Outcome

The results from the prior subsections paint the story that after an analyst becomes ranked as an all-star, he or she becomes more optimistic, bold, and frequent in their forecasts, but are less accurate. A natural question that arises in this scenario is how does the all-star analyst fare in next year's rankings? To shed light on this question, we traced next year's outcomes for the third-place and first runner-up analysts used in our RD design. However, we caution that while our RD design is an effective approach to estimate the treatment effect of becoming an all-star in the current year, it does not estimate the determinants of becoming an all-star in the following year. For example, becoming an all-star for the second time may be a function of becoming an all-star for the first time, and thus, there are other confounding factors that determine the stickiness of all-star status. Therefore, we view our tracing of next year's outcome as descriptive in nature.

In untabulated results, we find that 56.4% of third-place analysts remained within the top-three places, with 18.9% moving into the second-place and 7.6% moving into first-place. This compares with 36.6% of first runner-up analysts who became an all-star (one of the top-three places), with 11.2% moving into the second-place and 4.5% moving into first-place. This descriptive evidence suggests that a majority of third-place analysts continue their career momentum to advance further in their careers.

5. Conclusion

In this paper, we use the setting of sell-side equity analysts and all-star rankings to examine whether increased reputational capital affects the behavior of capital markets professionals. We find that newly-ranked all-star analysts become more optimistic, bold, and frequent in their forecasts, but are less accurate. Our analysis is robust to parametric estimation including polynomials of various orders, following Lee and Lemieux (2010) using the full sample, and non-parametric estimation based on narrow bandwidths of 1.5 and 1, and the optimal bandwidth approach proposed by Calonico et al. (2014).

We then test whether market reactions to forecast revisions differ between the matched-pair of analysts *after* the rankings. We find that market reactions are greater for forecast revisions by all-star analysts relative to non-all-star analysts. This result, coupled with the prior results on forecast optimism, accuracy, boldness, and frequency, suggests that after the rankings, the third-place analyst can move the market more than a first runner-up analyst. Given that the third-place analyst and first runner-up analyst were similar across forecast properties prior to the rankings, our results suggest a causal relation between all-star distinction and a change in analyst forecast behavior, as well as investors' reactions to that change in behavior. Overall, our results suggest that increased reputational capital emboldens professionals to take bolder actions to exert their market influence.

One caveat for our study is that we believe we capture the *ex post* effect of a high rank realization rather than an *ex ante* incentive effect of competing for the high rank. In other words, we do not examine the types of behaviors or changes in behaviors that cause analysts to become all-stars.

References

Bernhardt, D., Campello, M., & Kutsoati, E. (2006). Who herds? *Journal of Financial Economics,* 80 (3): 657-675.

Bradshaw, M. T. (2011). Analysts' forecasts: what do we know after decades of work? Unpublished working paper, Boston College. Available at SSRN: https://ssrn.com/abstract=1880339.

Brown, L. D., Call, A. C., Clement, M. B., & Sharp, N. Y. (2015). Inside the "black box" of sell-side financial analysts. *Journal of Accounting Research,* 53 (1): 1-47.

Calonico, S., Cattaneo, M. D., & Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica,* 82 (6): 2295-2326.

Carhart, M. M. (1997). On persistence in mutual fund performance. *Journal of Finance,* 52 (1): 57-82.

Chen, J., Jung, M.J., & Ronen, J. (2017). The confirmation effect of analyst recommendation reiterations. *Journal of Accounting, Auditing & Finance,* 32 (4): 576-592.

Chen, Y., Ebenstein, A., Greenstone, M., & Li, H. (2013). Evidence on the impact of sustained exposure to air pollution on life expectancy from China's Huai River policy. Proceedings of the National Academy of Sciences, 110(32), 12936-12941.

Clarke, J., & Subramanian, A. (2006). Dynamic forecasting behavior by analysts: Theory and evidence. *Journal of Financial Economics,* 80 (1): 81-113.

Clarke, J., Khorana, A., Patel, A., & Rau, P. R. (2007). The impact of all-star analyst job changes on their coverage choices and investment banking deal flow. *Journal of Financial Economics,* 84 (3): 713-737.

Clement, M., and S. Tse. (2005). Financial analyst characteristics and herding behavior in forecasting. *Journal of Finance*, 60 (1): 307−341.

Cuñat, V., Gine, M., & Guadalupe, M. (2012). The vote is cast: The effect of corporate governance on shareholder value. The journal of finance, 67(5), 1943-1977.

Diamond D. W. (1989). Reputation acquisition in debt markets. Journal of Political Economy 97:828–62

Dijk, O., Holmen, M., & Kirchler, M. (2014). Rank matters–The impact of social competition on portfolio choice. *European Economic Review,* 66, 97-110.

Dunbar, C. G. (2000). Factors affecting investment bank initial public offering market share. *Journal of Financial Economics*, 55 (1): 3-41.

Fama, E. F., & French, K. R. (1996). Multifactor explanations of asset pricing anomalies. *Journal of Finance,* 51 (1): 55-84.

Groysberg, B., Healy, P. M., & Maber, D. A. (2011). What drives sell-side analyst compensation at high-status investment banks? *Journal of Accounting Research,* 49 (4): 969–1000.

Hahn, J., Todd, P., & Van der Klaauw, W. (2001). Identification and estimation of treatment effects with a regression discontinuity design. *Econometrica*, 69 (1): 201-209.

Hartmann, W., Nair, H. S., & Narayanan, S. (2011). Identifying causal marketing mix effects using a regression discontinuity design. *Marketing Science,* 30 (6): 1079-1097.

Hayes, R.M. (1998). The Impact of Trading Commission Incentives on Analysts' Stock Coverage Decisions and Earnings Forecasts. *Journal of Accounting Research*, 36 (2): 299-320.

Holmström B. R. (1982). Managerial incentive problems: A dynamic perspective. In Essays in economics and management in honour of Lars Wahlbeck, 209–30. Helsinki, Finland: Swedish School of Economics.

Hong, H., & Kubik, J. D. (2003). Analyzing the analysts: Career concerns and biased earnings forecasts. *Journal of Finance,* 58 (1): 313-351.

Hong, H., Lim, T., & Stein, J. C. (2000). Bad news travels slowly: Size, analyst coverage, and the profitability of momentum strategies. *Journal of Finance,* 55 (1): 265-295.

Imbens, G. W., & Lemieux, T. (2008). Regression discontinuity designs: A guide to practice. *Journal of Econometrics*, 142 (2): 615-635.

Jackson, A. R. (2005). Trade generation, reputation, and sell-side analysts. *Journal of Finance,* 60 (2): 673-717.

Jegadeesh, N., and W. Kim. 2009. Do analysts herd? An analysis of recommendations and market reactions. *Review of Financial Studies*, 23 (2), 901–937.

Kirchler, M., Lindner, F., & Weitzel, U. (2018). Rankings and risk-taking in the finance industry. *Journal of Finance*, 73 (5): 2271-2302.

Krigman, L., Shaw, W. H., & Womack, K. L. (2001). Why do firms switch underwriters? *Journal of Financial Economics*, 60 (2-3): 245-284.

Lee, D. S., & Lemieux, T. (2010). Regression discontinuity designs in economics. *Journal of Economic Literature,* 48 (2): 281-355.

Malmendier, U., & Tate, G., 2009. Superstar CEOs. *The Quarterly Journal of Economics*, 124(4): pp.1593-1638.

Mayew, W. J. (2008). Evidence of management discrimination among analysts during earnings conference calls. *Journal of Accounting Research,* 46 (3): 627-659.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics,* 142 (2): 698-714.

Michaely, R., & Womack, K. L. (1999). Conflict of interest and the credibility of underwriter analyst recommendations. *Review of Financial Studies,* 12 (4): 653-686.

Mikhail, M.B., Walther, B.R., & Willis, R.H. (1999). Does forecast accuracy matter to security analysts? *The Accounting Review*, 74 (2): 185-200.

Soltes, E. (2014). Private interaction between firm management and sell-side analysts. *Journal of Accounting Research*, 52 (1): 245-272.

Stickel, S. E. (1992). Reputation and performance among security analysts. *Journal of Finance, 47*(5): 1811-1836.

Thistlethwaite, D. L., & Campbell, D. T. (1960). Regression-discontinuity analysis: An alternative to the ex post facto experiment. *Journal of Educational Psychology,* 51 (6): 309.

Figure 1

Notes: Figure 1a shows the distribution of score differences within the sample. The right half of the distribution is the score difference of the third-place analysts from first runner-ups. The left half of the distribution is the score difference of the first runner-ups from the third-place analysts. In Figure 1b, we estimate the density function based on McCrary (2008).

Figure 1a: Distribution of score differences between third-place analysts and the first runner-up

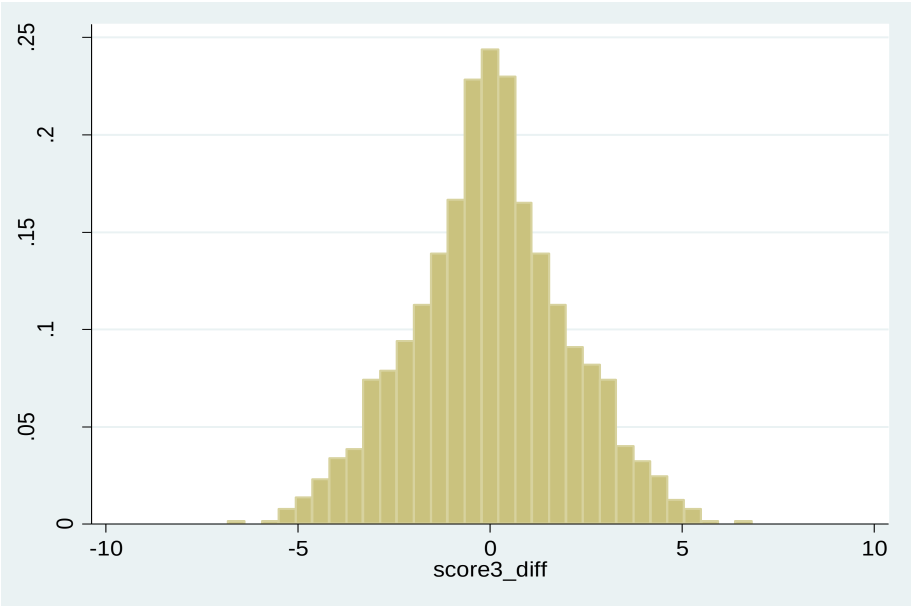

Figure 1b. Continuity of score differences

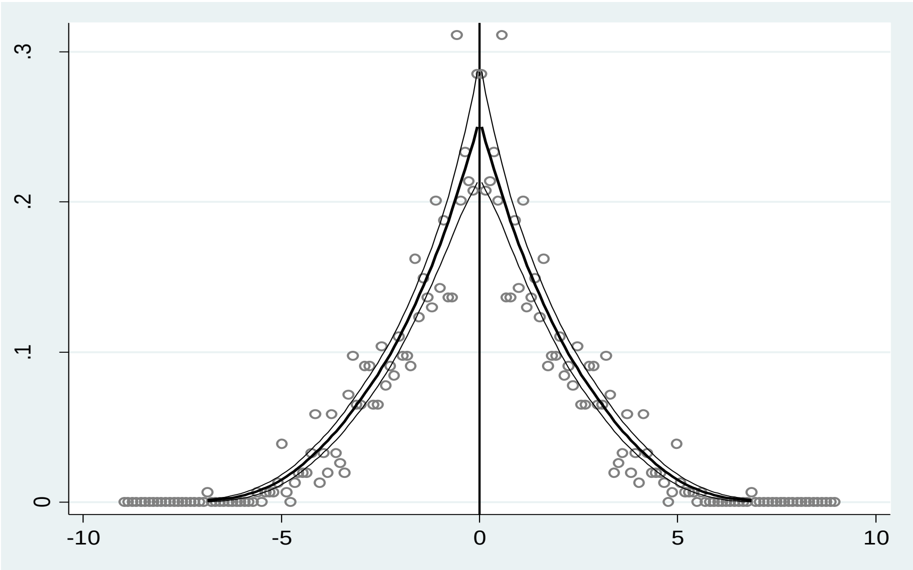

Table 1. Sample Composition

Table 1 presents the summary statistics of our sample. Panel A contains the number of industries, the number of analysts in each position of rankings, and the number of unique analysts by year. Panel B contains descriptive statistics of analyst composite scores by rank, before merging the II-ranking data with IBES data. Panel C shows descriptive statistics of the composite scores of third-place analysts, first runner-ups, and their difference, after merging the II-ranking data with IBES data.

Panel A: II-Ranked Analysts

Year	Number of Industries	First-Place	Second-Place	Third-Place	First Runner-Up	Other Runner-Ups	Number of Unique Analysts
2001	71	71	72	70	65	105	352
2002	68	68	68	68	63	99	343
2003	62	62	62	62	56	70	304
2004	62	62	64	60	57	67	293
2005	62	62	62	62	57	79	300
2006	62	62	61	61	50	68	278
2007	58	58	58	58	47	58	262
2008	58	58	58	58	42	46	249
2009	57	57	57	57	55	59	260
2010	58	58	58	58	51	70	270
2011	58	58	58	58	54	78	277
2012	58	58	58	58	55	80	278
2013	58	58	58	58	51	81	272
2014	57	57	57	57	51	72	265
Total		849	851	845	754	1032	879

Panel B: II-Ranked Analyst Composite Scores

Rank	N	Mean Score	Min	1st Quartile	Median	3rd Quartile	Max
First-Place	849	19.76	9.24	16.24	19.09	22.57	44.60
Second-Place	851	14.70	6.98	12.44	14.28	16.52	29.34
Third-Place	845	11.79	6.39	10.12	11.47	13.19	22.58
First Runner-Up	754	9.89	5.09	8.56	9.71	10.95	19.11
Other Runner-Ups	1,032	8.12	4.37	7.15	7.99	9.10	15.50
Difference between Third-Place and First Runner-Up	750	1.57	0.00	0.54	1.28	2.37	6.82

Panel C: II-Ranked Analyst Composite Scores after Matching with IBES data

Rank	N	Mean Score	Min	1st Quartile	Median	3rd Quartile	Max
Third-Place	842	11.79	6.39	10.11	11.48	13.21	22.58
First Runner-Up	743	9.90	5.09	8.56	9.73	10.96	19.11
Difference between Third-Place and First Runner-Up	736	1.57	0.00	0.54	1.26	2.37	6.82

Table 2. Summary Statistics

In Table 2, we report summary statistics of forecast errors, the absolute value of their forecast errors, forecast boldness, forecast frequency, and recommendation levels of third-place and first runner-up analysts.

Rank	Variables	N	Mean	Std Dev	Min	Median	Max
Third-Place	Forecast error	16,803	0.190	1.312	-2.610	-0.010	10.510
	Abs (forecast error)	16,803	0.518	1.565	0.000	0.120	14.050
	Boldness	2,238	48.801	25.981	0.000	50.000	100.000
	Frequency	3,669	4.483	3.752	1.000	4.000	21.000
	Recommendation	10,911	3.402	0.927	1.000	3.000	5.000
First Runner-Up	Forecast error	19,402	0.226	1.473	-2.610	-0.010	10.510
	Abs (forecast error)	19,402	0.600	1.835	0.000	0.120	14.050
	Boldness	2,463	50.744	26.415	0.000	50.000	100.000
	Frequency	3,880	4.833	3.835	1.000	4.000	21.000
	Recommendation	9,540	3.398	0.898	1.000	3.000	5.000

Table 3. Validation Tests: Pre-existing Differences

In Table 3, we investigate the systematic pre-existing differences in forecast characteristics between third-place and first runner-up analysts. We compute the mean of forecast errors and the mean of the absolute value of their forecast errors for each firm an analyst covers in each year; forecast boldness and forecast frequency are computed at analyst-firm-year level following the definition in Appendix 3. We use these characteristics of an analyst all one year before the ranking to predict whether the analyst is ranked as the first runner-up next year. More specifically, we restrict our comparison to analysts whose score differences are within optimal bandwidths. We introduce polynomials, different for the third-place and runner-up analysts, up to order 1 in all specifications. We control brokerage house-year fixed effects in all specifications and also firm-year fixed effects in Column 2. Standard errors are included in the parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dep. Var.	Next year Ranking=4	
	(1)	(2)
Error	-0.000	0.139
	(0.047)	(0.285)
Abs (Forecast error)	0.059	-0.034
	(0.037)	(0.336)
Frequency	-0.002	-0.006
	(0.004)	(0.006)
Boldness	0.000	-0.001
	(0.001)	(0.001)
Polynomials	Yes	Yes
Firm*Year fixed effects	No	Yes
Brokerage*Year fixed effects	Yes	Yes
R-squared	0.421	0.910
N	1,629	621

Table 4. Regressions of Forecast Error and Analyst Ranking

Table 4 presents the regression analysis of the effect of ranking on forecast optimism at analyst-firm-forecast level. The dependent variable is forecast error. For each analyst, it is calculated as the forecast annual EPS minus the actual value of annual EPS for the same fiscal year forecasted. Independent variable is a dummy that equals 1 if the analyst is the first runner-up, and 0 for a third-place analyst. Models in Columns 1 and 2 follow parametric estimation and use full sample. Models in Columns 3 and 4 follow non-parametric estimation using the bandwidths generated by the approach proposed by Calonico, Cattaneo, and Titiunik (2014) with triangular kernel functions. Models in Columns 5 and 6 and models in Columns 7 and 8 follow parametric estimation and use the sample of analysts whose scores differences fall within bandwidths of (−1.5, 1.5) and (−1, 1), respectively. We introduce polynomials, different for the third-place and runner-up analysts, up to order 2 in Columns 1 and 2, and up to order 1 in Columns from 3 to 8. In Columns 2, 4, 6, and 8, we control for the time (in months) between the announcement of rankings and the announcement of forecasts. In all regressions, we control firm-year fixed effects and brokerage house-year fixed effects. Standard errors are included in the parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dep. Var.: Forecast Error	Global		Local					
			Optimal bandwidth		(-1.5, +1.5)		(-1, +1)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
First runner-up	-0.067	-0.086*	-0.151***	-0.166***	-0.154**	-0.157**	-0.135*	-0.133*
	(0.044)	(0.044)	(0.050)	(0.050)	(0.069)	(0.068)	(0.076)	(0.076)
Distance to announcement	No	Yes	No	Yes	No	Yes	No	Yes
Polynomials	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm*Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Brokerage*Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.755	0.758	0.717	0.720	0.698	0.700	0.694	0.696
N	18,666	18,666	13,891	13,891	11,518	11,518	10,123	10,123

Table 5. Regressions of Forecast Accuracy and Analyst Ranking

Table 5 presents the regression analysis of the effect of ranking on forecast accuracy at analyst-firm-forecast level. The dependent variable is the absolute value of error of each forecast. Independent variable is a dummy that equals 1 if the analyst is the first runner-up, and 0 for a third-place analyst. Models in Columns 1 and 2 follow parametric estimation and use full sample. Models in Columns 3 and 4 follow non-parametric estimation using the bandwidths generated by the approach proposed by Calonico, Cattaneo, and Titiunik (2014) with triangular kernel functions. Models in Columns 5 and 6 and models in Columns 7 and 8 follow parametric estimation and use the sample of analysts whose scores differences fall within bandwidths of (−1.5, 1.5) and (−1, 1), respectively. We introduce polynomials, different for the third-place and runner-up analysts, up to order 2 in Columns 1 and 2, and up to order 1 in Columns from 3 to 8. In Columns 2, 4, 6, and 8, we control for the time (in months) between the announcement of rankings and the announcement of forecasts. In all regressions, we control for firm-year fixed effects and brokerage house-year fixed effects. Standard errors are included in the parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dep. Var.: Abs (Forecast error)	Global		Local					
			Optimal bandwidth		(-1.5, +1.5)		(-1, +1)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
First runner-up	0.048	0.013	-0.117*	-0.114*	-0.100*	-0.106**	-0.105*	-0.102*
	(0.036)	(0.035)	(0.062)	(0.060)	(0.053)	(0.051)	(0.057)	(0.056)
Distance to announcement	No	Yes	No	Yes	No	Yes	No	Yes
Polynomials	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm*Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Brokerage*Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.816	0.830	0.803	0.814	0.799	0.810	0.803	0.814
N	18,666	18,666	9,812	9,812	11,518	11,518	10,123	10,123

Table 6. Regressions of Forecast Boldness and Analyst Ranking

Table 6 presents the regression analysis of the effect of ranking on forecast boldness at analyst-firm-year level. The dependent variable is boldness. Following Clarke and Subramanian (2006), we rank all the analysts covering the same company by absolute deviation from the consensus mean of forecasts of all analysts for the same firm. Rankings are then scaled such that boldness ranking is valued from 100 to 0, with higher values indicated more boldness. Model in Columns 1 follows parametric estimation and use full sample. Model in Column 2 follows non-parametric estimation using the bandwidths generated by the approach proposed by Calonico, Cattaneo, and Titiunik (2014) with triangular kernel functions. Models in Columns 3 and 4 follow parametric estimation and use the sample of analysts whose scores differences fall within bandwidths of (−1.5, 1.5) and (−1, 1), respectively. We introduce polynomials, different for the third-place and runner-up analysts, up to order 2 in Column 1, and up to order 1 in Columns from 2 to 4. In all regressions, we control for firm-year fixed effects and brokerage house-year fixed effects. Standard errors are included in the parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dep. Var.: Boldness	Global	Local		
		Optimal bandwidth	(-1.5, +1.5)	(-1, +1)
	(1)	(2)	(3)	(4)
First runner-up	-22.01***	-24.39***	-18.03**	-20.27**
	(6.896)	(7.597)	(8.637)	(9.817)
Polynomials	Yes	Yes	Yes	Yes
Firm*Year fixed effects	Yes	Yes	Yes	Yes
Brokerage*Year fixed effects	Yes	Yes	Yes	Yes
R-squared	0.575	0.080	0.128	0.060
N	1,234	906	616	432

Table 7. Regressions of Forecast Frequency and Analyst Ranking

Table 7 presents the regression analysis of the effect of ranking on forecast frequency at analyst-firm-year level. The dependent variable is forecast frequency. For each firm, we count the number of forecasts made by each analyst in each year. Model in Columns 1 follows parametric estimation and use full sample. Model in Column 2 follows non-parametric estimation using the bandwidths generated by the approach proposed by Calonico, Cattaneo, and Titiunik (2014) with triangular kernel functions. Models in Columns 3 and 4 follow parametric estimation and use the sample of analysts whose scores differences fall within bandwidths of (−1.5, 1.5) and (−1, 1), respectively. We introduce polynomials, different for the third-place and runner-up analysts, up to order 2 in Column 1, and up to order 1 in Columns from 2 to 4. In all regressions, we control for firm-year fixed effects and brokerage house-year fixed effects. Standard errors are included in the parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dep. Var.: Frequency	Global	Local		
		Optimal bandwidth	(-1.5, +1.5)	(-1, +1)
	(1)	(2)	(3)	(4)
First runner-up	1.207**	-1.445**	-1.425**	-1.322*
	(0.555)	(0.620)	(0.643)	(0.771)
Polynomials	Yes	Yes	Yes	Yes
Firm*Year fixed effects	Yes	Yes	Yes	Yes
Brokerage*Year fixed effects	Yes	Yes	Yes	Yes
R-squared	0.795	0.847	0.837	0.806
N	1,039	532	483	320

Table 8. Regressions of Recommendation Level and Analyst Ranking

Table 8 presents the regression analysis of the effect of ranking on recommendation at analyst-firm-forecast level. The dependent variable is the recommendation. Independent variable is a dummy that equals 1 if the analyst is the first runner-up, and 0 for a third-place analyst. Models in Columns 1 and 2 follow parametric estimation and use full sample. Models in Columns 3 and 4 follow non-parametric estimation using the bandwidths generated by the approach proposed by Calonico, Cattaneo, and Titiunik (2014) with triangular kernel functions. Models in Columns 5 and 6 and models in Columns 7 and 8 follow parametric estimation and use the sample of analysts whose scores differences fall within bandwidths of (−1.5, 1.5) and (−1, 1), respectively. We introduce polynomials, different for the third-place and runner-up analysts, up to order 2 in Columns 1 and 2, and up to order 1 in Columns from 3 to 8. In Columns 2, 4, 6, and 8, we control for the time (in months) between the announcement of rankings and the announcement of forecasts. In all regressions, we control for firm-year fixed effects and brokerage house-year fixed effects. Standard errors are included in the parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dep. Var.: Recommendation	Global		Local					
			Optimal bandwidth		(-1.5, +1.5)		(-1, +1)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
First runner-up	-0.027	-0.022	-0.044	-0.038	-0.011	-0.005	-0.098**	-0.094*
	(0.037)	(0.037)	(0.040)	(0.039)	(0.045)	(0.045)	(0.049)	(0.049)
Distance to announcement	No	Yes	No	Yes	No	Yes	No	Yes
Polynomials	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm*Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Brokerage*Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.708	0.710	0.707	0.708	0.719	0.720	0.734	0.736
N	16,925	16,925	13,584	13,584	12,037	12,037	10,560	10,560

Table 9. Regressions of Market Reaction and Analyst Ranking: Forecast Revisions

Table 9 presents the regression analysis of the effect of ranking on market reaction at analyst-firm-forecast level. The dependent variable is the 7-day cumulative abnormal returns (CARs) over a window of [-3, 3] around the announcement of each forecast revision. CARs are estimated using a four-factor model based on Fama and French (1996) and Carhart (1997). We identify each analyst's forecast revisions by subtracting from each forecast its previous forecast for the same firm scaled by the stock price at the fiscal year beginning. Variable first runner-up is a dummy that equals 1 if the analyst is the first runner-up, and 0 for a third-place analyst. We estimate the sensitivity of market reaction to each forecast revision by introducing the interaction term of forecast revision and first runner-up while controlling for the respective main effects. Panel A examine a five day window, and Panel B four day window. We drop forecasts revisions that are announced by the first runner-up and the third place analysts within the respective window horizons. . Models in Columns 1 and 2 follow parametric estimation and use full sample. Models in Columns 3 and 4 follow non-parametric estimation using the bandwidths generated by the approach proposed by Calonico, Cattaneo, and Titiunik (2014) with triangular kernel functions. Models in Columns 5 and 6 and models in Columns 7 and 8 follow parametric estimation and use the sample of analysts whose scores differences fall within bandwidths of $(-1.5, 1.5)$ and $(-1, 1)$, respectively. We introduce polynomials, different for the third-place and runner-up analysts, up to order 2 in Columns 1 and 2, and up to order 1 in Columns from 3 to 8. In Columns 2, 4, 6, and 8, we control for the time (in months) between the announcement of rankings and the announcement of forecasts. In all regressions, we control for brokerage house-year fixed effects and firm-year fixed effects. Standard errors are included in the parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dep. Var.: CAR	Global		Local					
			Optimal bandwidth		(-1.5, +1.5)		(-1, +1)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Panel A: (-3, +3)					
First runner-up	0.000	0.000	-0.003	-0.004	-0.009	-0.009	-0.010	-0.010
	(0.008)	(0.008)	(0.008)	(0.008)	(0.010)	(0.010)	(0.011)	(0.011)
First runner-up * Revision	-0.231***	-0.230***	-0.405***	-0.404***	-0.274***	-0.272***	-0.453***	-0.452***
	(0.044)	(0.044)	(0.059)	(0.059)	(0.076)	(0.076)	(0.101)	(0.101)
Revision	0.252***	0.249***	0.426***	0.425***	0.296***	0.294***	0.475***	0.474***
	(0.041)	(0.041)	(0.057)	(0.057)	(0.074)	(0.074)	(0.099)	(0.099)
Distance to announcement	No	Yes	No	Yes	No	Yes	No	Yes
Polynomials	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm*Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Brokerage*Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.262	0.262	0.303	0.303	0.307	0.307	0.306	0.306
N	7,811	7,811	5,775	5,775	4,965	4,965	4,325	4,325

				Panel B: (-2, +2)				
First runner-up	-0.001	-0.001	-0.002	-0.002	-0.009	-0.009	-0.011	-0.011
	(0.007)	(0.007)	(0.007)	(0.007)	(0.010)	(0.010)	(0.010)	(0.010)
First runner-up * Revision	-0.264***	-0.262***	-0.382***	-0.381***	-0.294***	-0.292***	-0.458***	-0.456***
	(0.040)	(0.040)	(0.055)	(0.055)	(0.071)	(0.071)	(0.094)	(0.094)
Revision	0.273***	0.270***	0.391***	0.390***	0.304***	0.301***	0.468***	0.465***
	(0.037)	(0.037)	(0.052)	(0.052)	(0.069)	(0.069)	(0.092)	(0.092)
Distance to announcement	No	Yes	No	Yes	No	Yes	No	Yes
Polynomials	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm*Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Brokerage*Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.281	0.281	0.318	0.318	0.311	0.312	0.311	0.311
N	7,812	7,812	5,695	5,695	4,966	4,966	4,325	4,325

Table 10. Regressions of New Stock Coverage and Job Turnover and Analyst Ranking

Table 10 presents the regression analysis of the effect of ranking on analyst's new stock coverage and job turnover at analyst-year level. The dependent variable in Panel A is new stock coverage. For each analyst-year, new stock coverage is computed as the number of new stocks covered in that year scaled by the total number of stocks covered in that year. The dependent variable in Panel B is job turnover. Job turnover is a dummy variable that equals one if the analyst moves to a different brokerage house one year after the ranking. is forecast frequency. For each firm, we count the number of forecasts made by each analyst in each year. Model in Columns 1 follows parametric estimation and use full sample. Model in Column 2 follows non-parametric estimation using the bandwidths generated by the approach proposed by Calonico, Cattaneo, and Titiunik (2014) with triangular kernel functions. Models in Columns 3 and 4 follow parametric estimation and use the sample of analysts whose scores differences fall within bandwidths of (−1.5, 1.5) and (−1, 1), respectively. We introduce polynomials, different for the third-place and runner-up analysts, up to order 2 in Column 1, and up to order 1 in Columns from 2 to 4. In all regressions, we control for firm-year fixed effects and brokerage house-year fixed effects. Standard errors are included in the parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Global	Local		
		Optimal bandwidth	(-1.5, +1.5)	(-1, +1)
	(1)	(2)	(3)	(4)
		Panel A: New stock coverage		
First runner-up	-0.052*	-0.054*	-0.058*	-0.066**
	(0.029)	(0.029)	(0.029)	(0.031)
Polynomials	Yes	Yes	Yes	Yes
Brokerage*Year fixed effects	Yes	Yes	Yes	Yes
R-squared	0.479	0.509	0.512	0.539
N	899	721	668	571
		Panel B: Job turnover		
First runner-up	-0.049	-0.056*	-0.058*	-0.071*
	(0.033)	(0.034)	(0.035)	(0.037)
Polynomials	Yes	Yes	Yes	Yes
Brokerage*Year fixed effects	Yes	Yes	Yes	Yes
R-squared	0.476	0.453	0.383	0.399
N	1,141	762	833	696

Table 11. Cross-sectional Analysis of Past All-star Analysts

Table 11 presents the cross-sectional analysis of being an All-star analyst in the past analysis. The dependent variable is forecast error. For each analyst, it is calculated as the forecast annual EPS minus the actual value of annual EPS for the same fiscal year forecasted. Independent variable is a dummy that equals 1 if the analyst is the first runner-up, and 0 for a third-place analyst. Models in Columns 1 and 2 follow parametric estimation and use full sample. Models in Columns 3 and 4 follow non-parametric estimation using the bandwidths generated by the approach proposed by Calonico, Cattaneo, and Titiunik (2014) with triangular kernel functions. Models in Columns 5 and 6 and models in Columns 7 and 8 follow parametric estimation and use the sample of analysts whose scores differences fall within bandwidths of (−1.5, 1.5) and (−1, 1), respectively. We introduce polynomials, different for the third-place and runner-up analysts, up to order 2 in Columns 1 and 2, and up to order 1 in Columns from 3 to 8. In Columns 2, 4, 6, and 8, we control for the time (in months) between the announcement of rankings and the announcement of forecasts. In all regressions, we control firm-year fixed effects and brokerage house-year fixed effects. Standard errors are included in the parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dep. Var.: Forecast error	Global		Local					
			Optimal bandwidth		(-1.5, +1.5)		(-1, +1)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Third-place	-0.0428	-0.0123	-0.0449	-0.0361	0.0357	0.0335	-0.0643	-0.0704
	(0.0693)	(0.0688)	(0.104)	(0.104)	(0.115)	(0.114)	(0.148)	(0.148)
Past all-star	-0.110*	-0.120**	-0.0854	-0.101	-0.0378	-0.0623	0.0238	-0.00616
	(0.0573)	(0.0569)	(0.115)	(0.114)	(0.136)	(0.136)	(0.180)	(0.180)
Third-place*Past all-star	0.223***	0.203**	0.293*	0.289*	0.236	0.246	0.376*	0.387*
	(0.0838)	(0.0832)	(0.155)	(0.155)	(0.168)	(0.168)	(0.224)	(0.223)
Distance to announcement	No	Yes	No	Yes	No	Yes	No	Yes
Polynomials	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm*Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Brokerage*Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.762	0.765	0.704	0.706	0.705	0.706	0.700	0.702
N	18,282	18,282	11,807	11,807	11,154	11,154	9,759	9,759

Appendix 1. Regression Discontinuity Design

Regression discontinuity (RD) designs can be used to measure treatment effects when a treatment is based on whether an underlying continuous variable (or forcing variable) crosses a threshold. Under the condition that there is no other source of discontinuity, the treatment effect induces a discontinuity in the outcome of interest at the threshold. Thus, the outcomes on the two sides of the threshold are unequal and the difference between these two directional limits measures the treatment effect. A necessary condition for the validity of the RD design is that the forcing variable itself is continuous at the threshold (Hahn, Todd, and Van der Klaauw, 2001).

Formally, let y denote the outcome of interest and z the forcing variable, with $\bar{z}$ being the threshold above which there is treatment. Further, we define the two limiting values of the outcome variable as follows:

$$y^{+} = \lim_{\delta \to 0} E[y \mid z = \bar{z} + \delta]$$

$$y^{-} = \lim_{\delta \to 0} E[y \mid z = \bar{z} - \delta]$$

Then the local average treatment effect is given by

$$d = y^{+} - y^{-}$$

In the analyst ranking context, the forcing variable is the *II*-voting composite score earned by a given analyst in a given industry-year, and the ideal threshold is the one in which scores just above the threshold earn an analyst all-star distinction and scores just below the threshold do not earn the distinction. Given the institutional details of the *II* voting process, we argue that the best way to study the treatment effect of rankings is to compare the outcomes (e.g., forecast properties) of the third-place analyst and the first runner-up (i.e., fourth-place) analyst. There are several reasons for choosing this pair of analysts within each industry-year. First, in the analyst profession, as well as in the academic literature, only the analysts ranked in the top three places are considered all-stars (e.g., Hong et al. 2000, Hong and Kubik 2003). Second, the required minimum number of votes to achieve first place is higher than for all other places (see Appendix 2 for details of *II*'s voting process), making a first-place rank not exactly comparable to other ranks. Third, the difference in composite scores between the top three places are not guaranteed to be close, while by construction, the composite scores of runner-up analysts must be within at least 35% of the third-place analyst. Therefore, in the spirit of Thistlethwaite and Campbell (1960), the third-place analyst is the award winner and the first runner-up is the next analyst who just missed the award.

Formally, let analyst A be ranked i in the voting process and analyst B be ranked $i+1$ (one place below A), then it must be the case that

$$Score_A > Score_B$$

or equivalently

$$\Delta Score_{AB} = Score_A - Score_B > 0$$

Besides the score levels, the difference in scores can be used as the forcing variable for the RD design, and the threshold for the treatment can be 0. The RD design measures the treatment effect by comparing outcomes for situations when $\Delta Score_{AB}$ is just above zero and when it is just below zero. This comparison achieves the quasi-experimental design that underlies RD, with the latter set of observations acting as a control for the former.

More details on estimating causal effects using RD designs, including the difference between sharp and fuzzy RD designs, the selection of nonparametric estimators for $y^+$and y^-, and the choice of bandwidths can be found in Imbens and Lemieux (2008) and Lee and Lemieux (2010).

Appendix 2. Voting Procedure

The polling period begins roughly five months before publication and lasts four weeks. *Institutional Investor* (*II*) will disclose approximate polling start and end dates. Voting is unprompted and confidential. Results are published in October. The polling universe is compiled from the following sources: sell-side input, buy-side inquiries, and other internal databases and directories. The universe also includes the majority of firms on the following: benchmark lists published in *Institutional Investor*: II 300 (July) and Hedge Fund 100.

Before the voting period, any head of research at a sell-side institution may submit a client list to the operations group at II. During the voting period, sell-side firms should refer investors interested in voting to II. Interested investors must contact the operations group directly, determines voter eligibility. An eligible voter should meet an Assets Under Management (AUM) threshold, but the AUM cutoff is not disclosed. The cutoff may be lower for hedge funds. Voters must also be an active manager and manage money for institutional clients.

Each vote is weighted by AUM of the buy-side institution and calculated into weighted scores. Weighted scores are used to produce the Research Team (i.e., analyst) rankings. For example, a 1st place vote from the largest portfolio receives 24 points, while a 4th place vote from the smallest portfolio receives 1 point. A voting firm cannot exceed its assigned asset weight by increasing its number of voters. Votes are fractionated when multiple voters within a firm vote in the same sector and place rank. The voting firm's asset weight is then averaged across the number of its voters. For example, if two voters from the same firm in the largest portfolio class vote in the Chemicals sector for a given analyst for 1st place, each voter's asset weight is divided by two to equal 12 points.

A minimum number of votes is required for an analyst/team to be ranked in a sector. First-place teams require 15 votes for a sector to be published. All other top-ranked analysts/teams (2nd, 3rd and RU) require ten votes. To be ranked as runners-up, analysts' score should be within 35% of the 3rd-place score. To be ranked below runner-up, an analyst/team requires five votes. All sectors/positions are treated equally. The underlying calculation to determine the positions within each sector is based on a percentage of the weighted vote.

Appendix 3. Variable Definition

Variable	Definition
Composite score	A score that the Institutional Investor uses to produce All-American Research ranking. It is constructed based on the votes cast by buy-side analysts and portfolio managers weighted by their assets under management.
First runner-up	A dummy that equals 1 if the analyst is the first runner-up, and 0 for a third-place analyst.
Third-place	A dummy that equals 1 if the analyst is in the third place, and 0 for a second-place analyst.
Scores difference	We compute the score difference of the third-place analysts by subtracting the composite score of the first runner-up in the same industry from their composite scores. We compute the score difference of the first runner-up analysts by subtracting the composite scores of the third-place analyst in the same industry from their composite scores.
Forecast error	For each analyst, it is calculated as the forecast annual EPS minus the actual value of annual EPS for the same fiscal year forecasted.
Abs (forecast error)	The absolute value of error of each forecast
Forecast revision	We subtract from each forecast its previous forecast made by the same analyst for the same firm, and scale it by the stock price at the fiscal year beginning
Boldness	Following Clarke and Subramanian (2006), we rank all the analysts covering the same company by absolute deviation from the consensus mean of forecasts of all analysts for the same firm. Rankings are then scaled such that boldness ranking is valued from 100 to 0, with higher values indicated more boldness.
Forecast frequency	For each firm, we count the number of forecasts made by each analyst in each year.
Recommendation	A variable that takes on value between 1-5, with 5 indicating Strong Buy, 4 Buy, 3 Hold, 2 Sell, and 1 Strong Sell.
New stock coverage	For each analyst-year, new stock coverage is computed as the number of new stocks covered in that year scaled by the total number of stocks covered in that year.
Job turnover	A dummy variable that equals one if the analyst moves to a different brokerage house one year after the ranking.
Past all-star	A dummy variable that equals 1 if the analyst has won an All-star award in the past three years.
Distance to announcement	The time (in months) between the announcement of rankings and the announcement of forecasts.
CAR	The 5- and 7-day cumulative abnormal returns (CARs) over a window of [-2, 2] and [-3, 3] around the announcement of each forecast revision. CARs are estimated using a four-factor model based on Fama and French (1996) and Carhart (1997).
Order 1_pos	Equals scores difference if scores difference is positive, and zero if scores difference is non-positive
Order 1_neg	Equals scores difference if scores difference is negative, and zero if scores difference is non-negative
Order 2_pos	The square of Order 1_pos
Order 2_neg	The square of Order 2_neg