Hiring Cheerleaders: Board Appointments of "Independent" Directors*

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ABSTRACT

We provide evidence that firms appoint independent directors who are overly sympathetic to management, while still technically independent according to regulatory definitions. We explore a subset of independent directors for whom we have detailed, micro-level data on their views regarding the firm *prior* to being appointed to the board: sell-side analysts who are subsequently appointed to the boards of companies they previously covered. We find that boards appoint overly optimistic analysts who are also poor relative performers. The magnitude of the optimistic bias is large: 82.0% of appointed recommendations are strong-buy/buy recommendations, compared to 56.9% for all other analyst recommendations. We also show that appointed analysts' optimism is stronger at precisely those times when firms' benefits are larger. Lastly, we find that appointing firms are more likely to have management on the board nominating committee, appear to be poorly governed, and increase earnings management and CEO compensation following these board-appointments.

JEL Classification: G20, G24, G30

Key words: Independent directors, appointments, analysts, board members

What makes a good monitor? Embodied in recent regulatory requirements is the notion that independent directors provide a particular type of objective, shareholder-minded monitoring.[1] At the same time, little is known about the characteristics of independent directors, or the factors that influence the selection process of these directors. It seems almost necessary that a senior officer or board member has some relationship with, or prior knowledge of, a potential independent director in order to ensure they have enough information to be able to recommend the member for board election. This reality need not be problematic, as although it could be that this relationship skews the view of these "independent" directors, it could also reduce information asymmetries regarding the potential value of the director for the given board.[2]

In this paper we exploit a unique, hand-collected database of independent directors to test the hypothesis that boards appoint directors who, while technically independent according to regulatory definitions, nonetheless may be overly sympathetic to management. To do so we investigate a subset of independent directors for whom we have detailed, micro-level data on their views regarding the firm *prior* to being appointed to the board. We use these track records to compare the roles of optimism (i.e., hiring a cheerleader for management) versus skill (i.e., hiring an objective and able observer) in the board appointment process. Focusing on ex-ante, observable characteristics of the independent directors themselves allows us to directly evaluate the objectivity and potential efficacy of independent directors based solely on their actual opinions about the firm in question.

The agents we examine are former sell-side analysts who end up serving on the board of companies they previously covered. Motivating our empirical strategy is the fact that 91% of the board members of the appointing firms in our sample are on the board both at the time the analyst was covering the firm *and* at the time of the subsequent board appointment, suggesting that the firm-analyst relationships we explore exhibit a great deal of continuity. Further, unlike former CEOs or other senior executives who serve on corporate boards, for whom past performance attribution is complicated by the fact that firm performance is difficult to disentangle from individual performance, sell-side analysts' opinions and performance can be easily assessed. We can explicitly compute measures of skill/ability and optimism by examining the composition and stock return performance of analysts' buy/sell recommendations. In doing so we find evidence that boards appoint overly optimistic analysts who are also poor relative performers.

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[1] See, for example, the SEC's press release on November 4, 2003, in which the SEC approved new rules proposed and adopted by the New York Stock Exchange and the Nasdaq Stock Market requiring widespread strengthening of corporate governance standards for listed companies. The new rules "establish a stricter, more detailed definition of independence for directors and require the majority of members on listed companies"boards to satisfy that standard...Pursuant to NYSE Section 303A(2) of the NYSE Manual, no director would qualify as "independent" unless the board affirmatively determines that the director has no material relationship with the company (either directly or as a partner, shareholder or officer of an organization that has a relationship with the company)." See http://www.sec.gov/rules/sro/34-48745.htm for more details. See also Duchin et al. (2008) for a review of recent changes to the regulatory requirements for corporate boards.

[2] See Adams and Ferreira (2007) for a discussion of the tradeoffs involved in board construction.

In particular, board-appointed analysts issue significantly more positive recommendations on companies that subsequently appoint them to the board; both relative to the other stocks they cover, and relative to other analysts covering these same stocks. The magnitude of this result is large: 82.0% of these recommendations are strong-buy or buy recommendations, compared to 56.9% for all other analyst recommendations (an over 25% difference). In regressions of recommendation levels (1=Strong Sell, 5=Strong Buy) on an appointment dummy (equal to 1 if the analyst recommending the given stock is subsequently appointed to the board of directors of that firm), the coefficient on appointment implies an increase in favorableness of rating from between a Hold and a Buy for the average recommendation to between a Buy and Strong Buy for appointed recommendations. This result is nearly three times as large as the optimism effect associated with affiliation (here a dummy variable equal to 1 if the given firm has an underwriting relationship with the analyst's brokerage house), which is the subject of a vast analyst literature (see, for example, Lin and McNichols (1998), Lin et al. (2005), Michaely and Womack (1999), Hong and Kubik (2003)).

Additionally, we find that board-appointed analysts exhibit poor relative performance on their stock recommendations on appointing firms. For example, we find that appointed analysts issue 60-90% more directionally incorrect calls on stock recommendations for firms that appoint them compared to the typical analyst recommendation. Finally, we show that appointed analysts demonstrate inferior *overall* forecasting ability, both for earnings forecasts and for stock recommendations, across the entire portfolio of stocks they cover. Thus, while it is true that an optimistic analyst may simply be more likely to accept a board seat than an otherwise similar analyst, our results imply that firms are either appointing the wrong analysts, or that firms should simply demand zero analysts if the only willing supply consists of biased and poor performing analysts.

Of course, appointing overly bullish analysts need not imply bad monitoring. It is possible that optimistic directors might facilitate productive cooperation and communication among board members, or have ideas on new strategies and directions for growth. To explore these issues, we first examine the characteristics of appointing firms, and then explore the behavior and performance of appointing firms after these appointments. We find that firms that appoint cheerleaders have a strikingly different composition of their nominating committee (at the time of these appointments) relative to other firms; in particular, their share of independent directors on the nominating committee is much lower, and their CEO is far more likely to be on the nominating committee. The economic magnitudes of these effects are substantial: appointing firms have nominating committees with over 30% fewer independent members and are over 5 times as likely to have the CEO on the nominating committee. We also find that appointing firms score significantly worse on common measures of governance quality (e.g., the Gompers, Ishii, Metrick (2003) governance index).

Further, we show that appointing firms engage in increased questionable behavior after the appointment of these analysts: appointing firms significantly increase their earnings management behavior,

reporting higher discretionary accruals post-appointment relative to the pre-appointment period. To overcome the potential endogeneity of analyst board appointments, we instrument for the appointment of a cheerleader analyst by exploiting the post-Global Settlement time period when analysts were widely scrutinized and hence in low demand to serve as potential directors. We use this exogenous shock to the desirability/availability of analysts in the pool of potential directors as an instrument for the actual appointment of cheerleader analysts. We first show that Global Settlement did have a significant negative impact on the appointment of analysts to boards of directors (first-stage). We then find that the instrumented-appointment of analysts to the board led to a large and significant increase in earnings management post-(instrumented)-appointment. We also examine CEO compensation in this IV framework, and find that appointing firms significantly increase CEO compensation post-appointment of a cheerleader analyst.

Overall, our findings provide new evidence on the board selection process, and on the characteristics of independent directors. Our unique micro-level data on analyst board appointments enables us to investigate the track records of a subset of independent directors in a clean and direct way in order to investigate their optimism and expertise with respect to the appointing firms. To our knowledge, although papers have used measures of director relationships, this paper is the first to *empirically* document this phenomenon of firms actively appointing board "cheerleaders" (i.e., board members who have an empirically documented optimistic view of the firm/management, but who possess little skill in assessing the firm or its prospects, suggesting them to be unsuitable monitors). Since these cheerleaders are of course technically labeled as independent directors, our findings call into question the idea that increasing the representation of independent directors on the board is by definition a positive step.

Additionally, our results on the characteristics and behavior of the appointing firms suggest that exploring the past track records and backgrounds of board members is a useful way to identify cross-sectional variation in firm governance quality. Although we focus on a subset of board appointments in this paper, we believe our results help shed light on independent directorships in general. Even in this pool of former sell side analysts of the firm, who are potentially very informed and skilled monitors, firms either seem to be demanding (or at the very least settling) for overly optimistic analysts who are poor relative performers. Collectively our results suggest that the board appointment process involving other classes of independent directors, where the same potential monitoring skill might not be present, could be even more problematic.

I. Background and Motivation

Our data and approach allow us to investigate the micro foundations of several competing views on how boards function. Specifically, by looking at observable measures of the optimism and ability of a subset of board appointees, we can directly test the hypothesis that boards engage in a type of "window-dressing"

when appointing independent directors. This view, embraced by many skeptics of recent regulatory reforms and articulated by Romano (2005), maintains that setting numerical targets for independent directors will not improve corporate governance (nor have any effect on firm performance) because managers can still appoint directors who are independent according to regulatory definitions, but nonetheless still overly sympathetic to management.

A competing viewpoint, which forms the foundation of recent regulatory changes (including the Sarbanes-Oxley Act of 2002 (SOX), as well as rules enacted by the Securities and Exchange Commission (SEC), New York Stock Exchange (NYSE), and National Association of Securities Dealers (NASD)), argues that independent directors are objective, shareholder-focused monitors of management, and therefore that increasing their representation on boards should uniformly improve corporate governance. Independent directors, under this view, are custodians of shareholder interests, whose presence on the board helps reduce agency problems and improve firm performance.

Yet another hypothesis suggests that boards are optimally constructed so as to maximize shareholder value, such that any mandated increases in board independence will likely hurt firm performance. Not surprisingly, since all three of these theories have predictions on how changes in board independence may affect future performance, the typical approach in the literature to evaluating these stories has been to relate measures of board independence (e.g., increases in the percentage of independent directors on a board) to future performance of the firm. The problem with this strategy is that board composition is endogenous, so identifying a link between board independence and firm performance is difficult (even if one exists) if poor performance causes an increase in board independence (as in Hermalin and Weisbach (1998)), or if other factors cause comovement in board composition and firm performance (as in Harris and Raviv (2007)). Recent theory also suggests that board independence is unlikely to have a uniform effect across firms, and that the effectiveness of independent directors may depend on the information environment of the firm (see Hermalin and Weisbach (1998), Raheja (2005), Adams and Ferreira (2007), and Harris and Raviv (2007)).[3]

Perhaps as a result of these issues, many studies fail to find a strong relation between board independence and firm performance (see, for example, Bhagat and Black (2002), Hermalin and Weisbach (2003), Fields and Keys (2003)). However, more recent studies (see, for example, Dahya and McConnell (2007) and Duchin et al. (2008)) identify exogenous changes in board structure by exploiting shifts in regulatory environments and provide evidence that increases in board independence precede improvements in firm performance. In particular, Duchin et al. (2008) find that the effect of outside directors on firm performance is small on average; however, consistent with the recent theory above, the effect of outside directors on firm performance varies according to the information environment of a firm: outside directors

[3] Note that incorporating information considerations into evaluations of board composition builds off a long-understood notion (see Berle and Means (1932), Fama and Jensen (1983), Jensen (1993)) that the effectiveness of outside directors may be limited by their inferior information relative to corporate insiders.

are effective when the cost of acquiring information about a firm is low, but ineffective when the cost of acquiring information is high.

The paper most closely related to ours is perhaps Brickley et al. (1999), who investigate a sample of former CEOs who end up on boards of companies after they retire as CEOs.[4] Their focus is on the managerial incentives that these possible future board appointments provide for CEOs during their tenures, but they do provide evidence that boards may consider ability and merit when selecting directors by showing that the likelihood of post-retirement board service by a CEO is positively related to the recent accounting performance of that CEO's firm during her tenure.[5] The problem of course with using CEOs and senior executives is that past performance attribution is complicated by the fact that firm performance is difficult to disentangle from individual performance.[6] By contrast, we can explicitly compute measures of skill/ability and optimism for each individual analyst with respect to the appointing firm (and with respect to her entire portfolio); in doing so, we can directly test the true track record and implicit firm motivation for our sample of appointed independent directors.

II. Data

The data in this study are collected from several sources. We obtain biographical information and past employment history for directors and senior company officers from Boardex of Management Diagnostics Limited. The Boardex data contain relational links among boards of directors and other corporate officials. Links in the dataset are constructed by cross-referencing employment history, educational background and professional qualifications. For each firm, we use the link file to reconstruct the annual time series of identities of board members and senior officers of the firms.

We use analysts' stock recommendation data from the I/B/E/S historical recommendation detail file, which codes recommendations on a common scale from 1 to 5, where 1=Strong Buy, 2=Buy, 3=Hold, 4=Sell, and 5=Strong Sell. We search public filings and other miscellaneous information available over the World Wide Web to identify security analysts that are subsequently appointed to the board of directors of the companies they follow. We start by identifying all analysts on the I/B/E/S tape who provide at least one recommendation on a domestic stock between 1993 and 2006. For each analyst, I/B/E/S provides a numeric identifier, the analyst's last name, the initial of his/her first name, and the analyst's brokerage house. Since our

[4] See also Lee (2007) for more recent evidence on post-retirement board service by former CEOs. In addition to this work, Stern and Westphal (2006) use survey evidence to find that managers who engage in ingratiatory behavior toward CEOs are more likely to receive appointments on boards with the CEO.

[5] See also Kaplan and Reishaus (1990) and Gilson (1990), as well as a body of empirical research (summarized in Yermack (2006)) that argues that what matters for firm performance are the qualifications of outside directors, such as financial expertise (DeFond et al. (2005)), business knowledge and experience (Fich (2005)), and the time commitments of outside directors (Fich and Shivdasani (2006)).

[6] See Bertrand and Schoar (2003).

data construction methodology involves name searches, we delete observations with multiple names for a given analyst numeric identifier or multiple analyst and brokerage identifiers for a given name. Finally, we discard teams, as attribution of the recommendation is less clean in these cases.

We look at analysts exiting the industry during our sample period and generate an initial list of potential hires by matching the analyst's initials and last name to the names of all board members of all firms covered by the analyst during her tenure. For example if analyst J. Smith covered stock ABC and XYZ between 1994 and 1998 and exits the industry in 1998, we search the list of directors of ABC and XYZ for board members named J* SMITH appointed in or after 1998. Finally we hand-check each entry from this initial list in order to positively identify analysts appointed to the board of firms they used to cover. To do so, we search press releases regarding the appointment (which usually describe the board member's background and prior employment) and Zoominfo.com, a search engine that specializes in collecting and indexing biographical and employment data from publicly available documents over the Web. We also use a variety of other sources on a case-by-case basis, including contacting the company to confirm the identity and the background of the board member. We use a conservative approach and only retain entries for which we can positively identify the board member as a former security analyst from multiple sources.

We match our recommendation data to accounting and stock return data from CRSP/COMPUSTAT. We also utilize data on firm-level governance measures, drawn from the IRRC database available through WRDS.

We can positively identify 51 unique situations where analysts exiting the industry are later appointed to the board of directors of a firm that they themselves previously covered.[7] Collectively these analysts cover a total of 1,163 firms issuing 4,130 recommendations between 1993 and 2006. Our identification relies on the fact that these analysts cover a large number of stocks and produce numerous recommendations. Also, firms appointing former analysts to their board are covered by many other analysts: a total of 1,212 analysts making 4,716 recommendations on these firms. We therefore exploit variation within and across analysts to identify systematic differences in recommendations. We find that firms that appoint analysts to the board are slightly larger than other firms and have a slightly higher percentage of independent directors, but these differences are not statistically significant. In addition, analysts who are appointed to the board tend to work for slightly larger brokerage houses and cover more stocks than other analysts, but again the differences are not significant. These appointments are spread across a wide range of industries (29 of the Fama-French 49 industries), with the two largest appointment shares coming from Finance and Trading (10%) and the Petroleum and Natural Gas industry (10%). The total frequency (68) is greater than the total number of firm-appointments (51), as a number of the firms switch industry classifications throughout our sample period.[8] As we do analyses both pre- and post- appointment, we retain all industries that are represented. Finally, the

[7] See the online Appendix Table A1 for additional summary statistics for our sample.
[8] These breakdowns by industry are shown in the online Appendix Table A2.

time-series distribution of appointments over our sample is relatively uniform, with no noticeable decreases in the incidence of analyst appointments after the enactment of Sarbanes-Oxley (in July 2002), or after the enactment of new exchange rules (in 2004) requiring that nominating committees be independent.[9]

III. Bias in Appointed Recommendations

A. Distribution of recommendations

The mere fact that analysts are subsequently appointed to boards of firms that they previously covered may not be unreasonable from a shareholder's perspective. Analysts spend years (and in some cases their entire careers) covering a small set of stocks, and so may be expected to have relative expertise on these firms. They may be the types of informed agents that shareholders would like as representatives on the board of directors. However, motivations based *solely* on this expertise carry no prediction on the level of recommendations. Actions based on window-dressing motives by firms, in contrast, do. In this section we examine the stock recommendations of analysts on firms that subsequently appoint them to their board of directors.

Table I presents the distribution of analysts' recommendations and tests the hypothesis that analysts hired by the firm they formerly covered issued more optimistic recommendations on these firms. Panel A reports the distribution of recommendations issued by analysts on firms who subsequently appoint them to the board of directors (i.e., if analyst Jim Smith covers firm XYZ and he is later hired by XYZ to serve on the board, we report the distribution of his recommendations on XYZ in Panel A). We refer to these as "Appointed recommendations."

We compare this distribution to three benchmarks. Panel B reports the distribution of all other recommendations on the I/B/E/S tape. Panel C reports the distribution of recommendations by analysts who are *not* appointed to the board, on those same firms that do appoint an analyst to the board. (I.e. we report recommendations on XYZ by all other analysts, excluding the appointed analyst Jim Smith). Panel D reports the distribution of recommendation by analysts who are appointed to the board, on all the stocks they cover *excluding* the firm who appoints them to the board. (I.e. we report Jim Smith's recommendations on all other firms he covered, excluding the appointing firm XYZ).

Comparing Panel A and Panel B reveals that appointed recommendations are significantly more optimistic than the I/B/E/S population. Roughly 42% of recommendations issued by analysts subsequently hired by the firm they cover are Strong Buy recommendations compared with only 25% for the whole sample. Similarly, over 82% of appointed recommendations are buys (Buy or Strong Buy), compared with only 57% of all of the non-appointed recommendations; we are able to safely reject the null hypothesis of no difference between the two distribution (Chi-square statistic=39.2, p-value<0.001). Panels C and D report

[9] See Appendix Table A3 for the year-by-year breakdowns of analyst appointments.

very similar results in comparison to the Appointed recommendations of Panel A (Chi-square tests in both cases reject equal distributions with p-values<0.001). To summarize, we find that analysts hired by the firm they previously covered issue significantly more optimistic recommendations on these firms relative to: 1) the universe of all sell side analysts, 2) recommendations on all other firms that they themselves issue, 3) recommendations on the appointing firm issued by all other analysts.[10]

B. Regression results on the positive bias in board-appointed analyst recommendations

In this section we run panel regressions on analyst recommendations to control for other determinants of recommendation levels. The dependent variable is the recommendation level of (1-5), which we reverse-score such that 1=Strong Sell, 2=Sell, 3=Hold, 4=Buy, and 5=Strong Buy.[11] The key independent variable of interest is a categorical variable (*Appointed Rec*) that is equal to 1 if the recommendation is issued by an analyst who is subsequently appointed by the given firm as a board member, and 0 otherwise. A positive coefficient on this variable indicates that the appointed analyst issues more optimistic stock recommendations on the appointing firm relative to all other recommendations.

We include a number of firm-level controls: size, book-to-market, past 1-month, and past 1-year returns (from month t-12 to t-2). In addition, control variables for analyst and brokerage house include: two measure of analyst experience, the number of years an analyst has been issuing recommendations on I/B/E/S, and the number of years the analyst has been issuing recommendations on the given stock; an affiliation dummy, equal to one if the analyst is employed by a bank that has an underwriting relationship with the given firm; an All-Star dummy variable, equal to one if the analyst is listed as an "All-Star" in the October issue of Institutional Investor magazine in that year;[12] a measure of brokerage size, equal to the total number of analysts employed by the brokerage house, a measure of if the analyst shares an alumni connection with any of the senior officers in the firm (CEO, CFO, or Chairman of the Board) (see Cohen, Frazzini, and Malloy (2010)); and fixed effects for recommendation month, analyst, firm, and industry, where indicated.[13] Standard errors are clustered at the recommendation month level.

Table II reports the regression results. Consistent with the results in Table I, in every specification the coefficient on *Appointed Rec* is positive and highly significant, indicating that the appointed recommendations are significantly more optimistic. The interpretation of the coefficient in the first column,

[10] We also identified an additional 55 analysts who were later appointed to boards of firms they did *not* previously cover; these analysts issued a total of 2,642 recommendations. In the online Appendix Table A4, we show that recommendations by these analysts are not optimistically biased, helping to rule out the possibility that overly optimistic analysts are more likely to serve on boards in general (and not necessarily on boards of firms they previously covered).

[11] Note that on I/B/E/S, Strong Buys are coded equal to 1, and Strong Sells are coded equal to 5; we reverse this convention and set Strong Buys=5 and Strong Sell=1, and so on, such that increases in recommendation levels correspond to increases in optimism.

[12] The list of affiliated analysts and all-star analysts are from Ljungqvist at al. (2006, 2007).

[13] We use a 48-industry classification from Ken French's website.

equal to 0.48 (t=5.90), is that analysts' recommendations are shifted half of a rating higher on firms that subsequently appoint them as board members; so while the mean rating is between a Buy and a Hold (3.74), the appointed analyst's recommendation rises to between a Strong Buy and a Buy (4.22) on firms to which he is subsequently appointed. The appointment effect is largely unaffected by other firm-level, analyst-level, and brokerage-level controls. The effect does not seem to be driven by a certain time period of overly positive recommendations (month fixed-effects), by recommendations in a specific industry (industry fixed-effects), by something specific about analysts appointed to boards (analyst fixed-effects), or by something specific about the firms that appoint covering analysts to their boards (firm fixed-effects).[14] Finally, in the last column we run the same regression specification, but as an ordered logit, and find nearly identical results.[15]

To get an idea of the magnitude of the *Appointed Rec* effect, we compare it with a well-documented conflict of interest effect: underwriting affiliation of a given analyst's investment bank with the firm in question (Lin and McNichols (1998), Lin, McNichols, and O'Brien (2005)). This literature shows that analysts have positively biased recommendations on these affiliated firms to which their investment banks do business. We include this affiliation effect in the regressions (Columns 2-9), and find that affiliation does have a positive effect on recommendations. However, it has no impact on the appointment effect (*Appointed Rec*), and the affiliation effect magnitude is 3 to 4 times smaller than the appointment effect (0.11 to 0.14 vs. 0.36 to 0.44).

In Columns 6-9, we include a dummy variable (*Connected to Firm*) that is equal to one if the analyst is connected to a senior officer through a school alumni link to control for the possibility that social ties may be driving the bias in recommendations that we observe for appointed analysts. The coefficient on *Appointed Rec* is virtually unchanged, while the coefficient on *Connected to Firm* is small and insignificant.[16]

We also break up our sample and examine our main result both before and after Regulation Fair Disclosure (Reg FD). Columns 7 and 8 show that the coefficient on *Appointed Rec* is very similar both before and after Reg FD, suggesting that changes in the information environment that may have accompanied the imposition of this law had virtually no impact on the appointment effect that we document here.

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[14] Given that we include fixed effects in all the regressions, constants are not reported. We have also run all the tests in the paper clustering at the firm- or analyst-level. These results, which are very similar to those reported here, are available on request. For example, replicating the full specification of Table II but adjusting the standard errors for clustering at the firm- (analyst-) level gives a t-stat on *Appointed Rec* of 2.57 (3.13), significant at the one-percent level. We have also included firm age (which is highly correlated with size) in the regressions as a robustness check, and the results are virtually identical in terms of magnitude and significance.

[15] When the coefficients are transformed back into marginal effects, the predicted appointment effect is 0.42. We only report one set of coefficients, while the coefficients in an ordered logit can theoretically change for each increment of the dependent variable (1 to 2, 2 to 3, etc.). We have checked this, especially for *Appointed Rec*, and the coefficient estimates are nearly identical across the increments.

[16] Cohen et al. (2010) find, as we do here, that the social ties have no effect on recommendation levels. Note that we are only able to match 20% of analysts' education data (roughly 70,000 recommendations versus the full sample of 400,000). Our power is thus slightly reduced in Columns 6-9 where connections are included, which explains the slightly smaller (though still significant) t-stats.

IV. Performance of Appointed Analysts

A. Performance on the appointing firm

In this section we explore appointed analyst predictive ability. Under the hypothesis that analysts are selected to serve on the board on the basis of their perceived ability, potential efficacy, and general understanding of the appointing firm, one might expect that appointed analysts would demonstrate higher predictive ability on their stock recommendations on the appointing firm.

To test this conjecture, we run panel regressions where the dependent variable is a dummy variable (*Wrong Bet*) equal to one if the return in the year immediately following the analyst's recommendation is the opposite sign from that implied by the recommendation. For example, if the subsequent annual stock return is negative (positive) and the recommendation is a strong buy or buy (strong sell or sell), then the variable *Wrong Bet* is set equal to one.[17] The mean of *Wrong Bet* across all analyst recommendations is approximately 26%, meaning that 74% of the time the analysts predict the subsequent return direction correctly. On the right-hand side of these regressions we control for known determinants of stock returns such as size, book-to-market, past 1-month, and past 1-year returns (from month t-12 to t-2), as well as the complete set of analyst-level controls used in Table II.

The first two columns of Table III indicate that appointed analysts' recommendations on appointing firms are incorrect significantly more often than the typical analyst recommendation. For example, the coefficient on *Appointed Rec* in Column 2 of 0.154 (t=2.67) is positive and highly significant. As the mean for *Wrong Bet* is 26%, the coefficient on *Appointed Rec* here of 0.154 indicates that appointed analysts' calls on appointed firms are over 50% more likely to be incorrect than the typical analyst recommendation, controlling for firm- and analyst-level characteristics (15.4%/26%).

We also employ a similar set of tests for *changes* in recommendations. The changes we examine are upgrades from the consensus recommendation (*Upgrade*), and downgrades from the consensus recommendation (*Downgrade*). Here, *Wrong Bet* is defined such that if the subsequent annual stock return is negative (positive) and the recommendation is an upgrade (downgrade), then the variable *Wrong Bet* is set equal to one. Columns 3 and 4 of Table III indicate that appointed analysts' upgrades and downgrades on appointing firms are wrong bets significantly more often than the typical recommendation change.[18] In Column 4, for instance, while the mean for *Wrong Bet* across all analyst recommendation changes is

[17] We have also run these regressions where *Wrong Bet* is defined relative to positive and negative 4-factor alphas, rather than returns. For example, if an analyst recommends a strong buy and the stock experiences a negative alpha over the next year, then *Wrong Bet* would be equal to one. Not surprisingly, since we already control for the known determinants of returns on the right-hand side of these regressions, the results using alphas are virtually identical to those reported here; for example, the coefficient on *Appointed Rec* using alphas as the threshold variable in Table III column 1 is 0.183 (t=3.46).

[18] Since appointed analysts issue very few holds, sells, and downgrades, the results here in Table III are driven largely by the large number of incorrect calls on buys and upgrades by appointed analysts; restricting our definition of *Wrong Bet* to include only the performance on buys and upgrades yields very similar results.

approximately 18%, the coefficient on *Appointed Rec* here (0.165, t=3.11) indicates that appointed analysts' calls on appointed firms are around 90% more likely to be incorrect than the typical analyst recommendation change (16.5%/18%).[19]

Overall, whether we look at the number of incorrect calls, or whether we look at recommendation levels or changes, we find a similar pattern of relative underperformance by appointed analysts on the firms that appoint them. In fact, we cannot find anything in the track records of appointed analysts to suggest that these analysts would be particularly effective monitors of the firms that appoint them.[20]

B. *Overall performance of appointed analysts*

One argument that could be made in response to the results above is that perhaps analysts are selected to serve on the board on the basis of their *overall* perceived ability, and not necessarily on their stock return performance on a single firm. Under this hypothesis, one might expect these analysts to outperform other analysts in a more general sense. To explore this idea, we examine the overall earnings forecasting ability and the overall stock return forecasting ability of appointed analysts.

Our first tests examine overall earnings forecasting ability. From the point of view of a firm hiring an analyst to serve on its board, the predictions in terms of analyst ability would seemingly apply to earnings forecasting ability as well as stock return forecasting ability. To conduct these tests, we compute the identical score measure used in Hong and Kubik (2003) in order to rank analysts across all the firms they cover in a given year. Specifically, we rank each analyst on each firm based on their absolute forecast error, computed as the absolute difference between her forecast for firm j in year t and the actual EPS of the firm, scaled by the stock price. For each analyst, we choose her most recent earnings per share forecast of year-end earnings issued by analyst i on firm j between January 1st and July 1st of year t. As in Hong and Kubik (2003), we then transform these rankings into a score measure (*Score_EPS*) where an analyst with a rank of one in terms of the lowest absolute forecast error receives a 100, while the least accurate analyst receives a score of 0; the median and mean score for a firm in a year is 50.[21] This relative measure of earnings forecasting ability allows us to compare all analysts, regardless of coverage, on the same scale. We take the average of this score measure

[19] In the Appendix Table A5, we run another set of panel regressions where the dependent variable is the actual return to the recommendation in the year immediately following a recommendation, rather than the dummy variables designed to capture right or wrong bets used in Table III, and again find that appointed analysts exhibit poor return performance on their recommendations on appointing firms.

[20] We have also conducted all these *Wrong Bet* tests defining *Wrong Bet* relative to: 1-month post recommendation return, 6-month post recommendation return, and post-recommendation horizon return (i.e., the stock return until the analyst's next recommendation on the stock, or the return over a maximum of one year if the analyst does not make another recommendation on the stock). Using all three alternative measures, appointed analysts make significantly more (both economically and statistically) *Wrong Bet*. For instance, the coefficient on *Appointed Rec* in the analog to Column 2 of Table III using the post-recommendation horizon definition of *Wrong Bet* is 0.158 (t=2.61), which implies an over 50% increase in *Wrong Bet*.

[21] As in Hong and Kubik (2003), we compute this measure as: $SCORE_{i,j,t}=100 - [(Rank-1)/(Number\ of\ Analysts_{j,t} - 1)]*100$.

across all the firms an analyst covers in a given year.[22] We then run panel regressions of these annual analyst-level score measures on the same set of control variables used in Table II, except that these control variables are now averaged across all firms that an analyst covers in a given year. Thus, observations are at the analyst-year level.[23]

Columns 5 and 6 of Table III present the results from these tests. The coefficient on *Appointed Rec* is strongly negative, indicating that appointed analysts perform worse overall on their earnings forecasts, across all the firms they cover, than other analysts. The mean of the left-hand side variable equals 50 by construction, so the magnitude of the coefficients in Column 5-6 imply that appointed analysts' earn a ranking that is approximately 7-11% worse than the average analyst ranking. Column 5's coefficient on *Appointed Analyst* of -5.64 (t=4.16), for instance, implies that appointed analysts earn an over 11% worse ranking than the average analyst.

We adopt a similar analyst-level ranking procedure in order to compare the overall stock return forecasting ability of appointed analysts. To do so, we compute the variable *Wrong Bet* as defined earlier for each recommendation for each analyst; in these tests *Wrong Bet* is set equal to one if the recommendation is an upgrade (downgrade) and the subsequent year's stock-level four-factor alpha is negative (positive). We then sum across each analyst for each year to compute each analyst's proportion of incorrect calls in a given year; we then rank analysts inversely by this proportion, and then transform these rankings into a score measure (*Score_Rec*) similar to the one described above. This variable *Score_Rec* again varies between 0 and 100, where the most accurate analyst (i.e., with a rank of 1 in terms of the lowest proportion of incorrect calls) receives a score of 100, and the least accurate analyst receives a score of 0; the median and mean score for a firm in a year is 50.

The last two columns of Table III present the results from these tests. The coefficient on *Appointed Rec* is again strongly negative, indicating that appointed analysts also perform worse overall on their entire set of recommendations relative to other analysts. The magnitude of the coefficient is similar to the coefficient on the earnings rankings, again implying that appointed analysts earn a ranking that is about 6-11% lower than the average analyst ranking. The Column 7 coefficient on *Appointed Rec* of -5.51 (t=4.07) implies that appointed analysts earn a recommendation ranking over 11% worse than the average analyst.

Taken together, the findings in Table III indicate that appointed analysts are not only poor relative performers on their stock recommendations on appointing firms, but are also poor relative performers in a much broader sense as well. Specifically, appointed analysts perform poorly on both their earnings forecasts

[22] Results are not sensitive to using multiple-year averages to compute annual analyst-level score measures.

[23] As we are now collapsing and evaluating at the analyst level in the tests in Columns 5-8, our *Appointed Rec* variable will not change for a given analyst over time (the analyst either is, or is not, subsequently appointed to a board), so we cannot include analyst fixed effects (nor firm fixed effects, as everything is collapsed to the analyst level). We can, and do, include year fixed effects in the regressions (Time) and all standard errors are adjusted for clustering at the ranking year level.

and their stock recommendations, across the entire portfolio of firms that they cover.

V. The Timing of Positive Recommendations

In this section we examine the dynamics of appointed analysts' recommendations. Specifically, we identify situations where firms may find a positive recommendation especially advantageous, and examine the behavior of the appointed analysts versus all other analysts at these times. The three situations we examine are: i.) periods preceding large amounts of stock issuance by the firm, ii.) periods following especially high short interest in the firm, and iii.) periods where the last analyst's recommendation downgraded the stock from the consensus.

We begin using the same framework as in Table II: the dependent variable is the level of the recommendation, and as before the variable *Appointed Rec* measures the recommendations of analysts on the firms that subsequently appoint them to the board. All of the control variables from Table II are included (but unreported) in Table IV. In addition to these variables we include the following dummy variables: *Last Rec. Downgrade*, which equals 1 when the prior recommendation by the last analyst was a downgrade from consensus; *High Short Interest*, which equals 1 if the firm had above median short interest level in the month prior to the recommendation being issued; and *High Future Issuance*, which is equal to 1 if the firm has above median stock issuance in the 6 months following the recommendation. The results are in Columns 1-4 of Table IV. From Column 1, the average analyst's recommendation is significantly more negative following times of high short interest (i.e., the coefficient on high short interest is negative and significant). However, Column 2 shows that analysts who are subsequently appointed to boards of the firms they cover have the complete opposite behavior and issue significantly more *positive* recommendations following months of high short interest on these firms. From Column 3, these same analysts also issue especially positive forecasts when the appointing firm has a large amount of stock issuance in the near future. Specifically, the coefficients on [*High Short*Appointed Rec*] of 0.334 (t=2.19) and on [*High Issue*Appointed Rec*] of 0.306 (t=2.11) imply that the appointed analysts issue recommendations roughly twice as upwardly biased at these times. Lastly, Column 4 shows that these subsequently appointed analysts also tend to issue more positive recommendations following a downgrade. The coefficient of 0.245 (t=1.63) is sizable, but not statistically significant.

Columns 5 and 6 explore this behavior in more depth, with a slightly different specification. Here, we examine whether the analysts subsequently appointed to the boards of directors are able to fight the well-documented herding behavior around downgrades of the firm to which they are later appointed. To do this, we use a new dependent variable, *Downgrade*, which is a categorical variable equal to 1 if the given recommendation is a downgrade from the current consensus estimate. First, while the average analyst downgrades 42% of the time, the coefficient on *Appointed Rec* in Column 5 of -0.17 (t=3.08) indicates that

appointed analysts downgrade only 25% of the time, or about 40% less often (17%/42%) on firms to which they are subsequently appointed to the board. In Column 6, we see that consistent with prior findings on analyst herding, the average analyst is about 7% more likely to downgrade from consensus if the prior analyst downgraded. Analysts later appointed to boards again do the exact opposite: they are especially *unlikely* to downgrade the firms they are appointed to at exactly those times when the last analyst downgraded from the consensus. To get an idea of the magnitude of the difference in behavior, when the prior recommendation was a downgrade, the average analyst will downgrade roughly 49% of the time (42%+7%), while analysts later appointed to the boards of firms will only downgrade these appointing firms roughly 20% of the time (42%+7%-1%-28%), making them roughly 60% less likely to downgrade. In sum, following other analysts' downgrading of their future appointing firm, Columns 4-6 suggest that the appointed analysts' recommendation bias is seen primarily in a lower propensity to downgrade, rather than in a higher absolute recommendation.

All of these tests point to the same types of behaviors: not only do analysts who are subsequently appointed to boards of firms they cover issue significantly more positive recommendations, but they have especially large positive biases at precisely those times likely to be most valuable to these firms.[24]

VI. The Governance Characteristics and Post-Appointment Behavior of Appointing Firms

In this section we explore the governance characteristics of appointing firms, as well as the impact of appointing a former analyst to the board.

A. Nominating Committees of Appointing Firms

In this section we explore in more detail the characteristics of the nominating committees of the appointing firms in our sample. To do so, we collect the complete time-series list of nominating committee members for our appointing firms. To do this, for each firm-year we obtain data from *The Corporate Library* which offers annual board composition data beginning in 2001. We determine each nominating committee director's membership, whether the director is independent, and whether the CEO is on the nominating committee from this data. For firm-year observations that are not present in *The Corporate library* dataset we refer to the *RiskMetrics Directors Legacy* dataset which includes data fields "Employment Title – CEO", "Board Affiliation (E-employee, I-Independent, L-linked)" and "Nominating Committee Member," from which it is straightforward to obtain the variable in which we are interested.

[24] In Appendix Table A6, we also sort our appointed analysts into groups based on their gap between last coverage and appointment; we find analysts who most recently covered the firm in question are more optimistic on the appointing firm than analysts with a larger gap between appointment (although both groups are optimistically biased).

We manually hand-collect remaining missing information that is not available in either of the above datasets from the website of the Securities Exchange Commission (SEC).[25] We obtain information about a firm's nominating committee using SEC form DEF-14A, which most often states explicitly the members of the firm's nominating committee.

In sum, we construct three measures regarding the nominating committee make-up. First, we create a variable *% Indep,* which is the percentage of the nominating committee made up of independent directors. The second is *Maj Indep*, which is a categorical variable equal to one if the nominating committee is majority independent, and zero otherwise. Third, we use *CEO On,* which is a categorical variable equal to one if the CEO is a member of the nominating committee.

We then run tests to explore the nominating committee composition of our appointing firms. Specifically, in Panel A of Table V, we run regressions of these three dependent variables on characteristics of firms. The main independent variable of interest is *Appointing Firm*, which is equal to one for firms that appoint former analysts in the year that these analysts are nominated to join the board, and zero otherwise. A number of other firm controls, and fixed effects from Table II are also included. These tests deliver a consistent message: firms that appoint these optimistic analysts have significantly less independent nominating committees (and the CEO is much more likely to be a nominating committee member). To give an idea of the magnitude, from Column 4, the coefficient of -0.289 (t=2.79) implies that appointing firms nominating committees have (on average) only 57% of independent members vs. 86% independent for all other firms. The coefficient in Column 5 of -0.312 (t=2.58) implies that they are over 30% less likely to be majority independent. This translates to only 61% of appointing firms vs. 92% of non-appointing firms having majority independent boards. Further, from Column 6, the coefficient of 0.227 (t=3.01) implies that appointing firms are roughly 5 times as likely to have the CEO on the nominating committee (28% vs. 5%).

B. Governance Characteristics of Appointing Firms

We next examine the level of governance, more generally, at the appointing firms. We do this using a number of commonly established measures of governance, namely those from Gompers, Ishii, and Metrick (2003). We focus on two main measures. The first, *GIndex*, is a composite of 24 unique governance provisions, in which one point is added for each provision added. Higher values of the *GIndex* indicate fewer shareholder rights, and thus higher values are often associated with weaker shareholder governance (Gompers, Ishii, and Metrick (2003)). We also focus on the *Delay* index of governance. *Delay* is meant to capture provisions that slow down hostile takeovers.[26] The reason we isolate this sub-index is that the legal

[25] http://www.sec.gov/edgar/searchedgar/companysearch.html.

[26] Delay is composed of four unique provisions: i.) blank check (a special class of preferred stock the board has control over), ii.) classified boards (staggered board terms of directors, preventing a complete ousting of the board at any

literature has argued that given the modern characteristics of the takeover market, this index subsumes all others in importance (see Coates (2000) and Daines and Klausner (2001)).

In Panel B of Table V we run regressions of these measures of governance on a number of control variables, and on a variable that measures those firms that appoint their former sell side analysts to their boards. The independent variable, *Appointing Firm*, is equal to 1 for firms who appoint their former analysts at some point over our sample period, and zero otherwise. This variable thus captures how much better (or worse) the governance is at the firms who engage in appointing their past analysts to their boards, controlling for the other firm, industry, and year effects in the regression. Column 1 shows a regression using *GIndex* as the governance measure. The positive and significant coefficient on *Appointing Firm* (0.284, t=4.06) implies that firms who appoint former analysts to their boards do have relatively weaker governance. To get an idea of the magnitude, the unconditional mean of *GIndex* is 8.98, so this coefficient represents a roughly 3% increase.

Column 2 uses the measure of the *Delay* index. This sub-index takes a value of between 0 and 4, with an unconditional mean of 2.17. The coefficient on *Appointed Rec* in Column 2 of 0.243 (t=11.00) thus represents over an 11% increase in these important hostile takeover defenses by those firms appointing analysts. To further understand exactly which of these provisions to delay hostile takeovers is driving the strong relationship with *Delay*, we separately test the relationship between *Appointed Rec* and each of the four component provisions (these components are described in detail in footnote 25). We find that two of the four governance provisions have an especially strong relationship with this propensity to appoint analysts, in terms of magnitude and significance. These are the Classified Board (*CBoard*) and Limits to Written Consent (*Limits*). The coefficient in Column 3 implies that appointing firms are 16% more likely to have the delay provision of classified boards than firms that do not (.093 relative to a mean of .58). The coefficient in Column 4 of 0.138 implies that appointing firms are over 38% more likely to have the delay provision of required written consent than firms that do not appoint their analysts to their boards (.138 relative to a mean of .358).

C. Behavior of Appointing Firms in the Post-Appointment Period

As noted earlier, appointing overly bullish analysts need not imply bad monitoring. It is possible that optimistic directors might facilitate productive cooperation and communication among board members, or have ideas on new strategies and directions for growth. On the other hand, if firms are simply hiring cheerleaders for the current management and board, we might expect these firms to engage in more potentially questionable activities for shareholders once having the cheerleader on the board. One of these

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election), iii.) special meeting provisions (make it difficult or impossible for bidders to call a special meeting to replace board members or alter takeover defenses), and iv.) limits to written consent (make difficult or completely disallow action by written consent, making it more difficult and time consuming for potential bidders).

questionable behaviors that is both well documented and established in the literature is earnings management. Specifically, we focus on the portion of earnings management that is discretionary, and that has been shown to have a positive short-term impact on a firm's stock price: discretionary accruals (Sloan (1996)).

In this section we use the actual appointment dates (shown in Appendix Table A3) to identify changes in behavior and valuation implications.[27] We present the results from regressions that test whether firms change their behavior after appointing their former analyst in Appendix Table A9. Specifically, we regress a firm's discretionary accruals[28] on *After Appointment*, a categorical variable equal to 1 if the former analyst is a board director, and zero otherwise. In these tests, we include only those firms that do appoint analysts as board members to isolate the pre- and post-appointment effect on their behavior (a total of 402 firm-year observations). In addition, we include year fixed effects and firm fixed effects, as we want to capture solely the marginal effect of having the former analyst on the board, within a given firm, and controlling for sample time trends. We also include a number of firm-level control variables, which given the firm and year fixed effects, can be interpreted as the effect of these variables after firm averages and time period trends have been removed. The most important control variable is *Total Accruals*.[29] With total accruals included, the coefficient on *After Appointment* can be interpreted as follows: given the same level of actual accruals before and after appointment, how much more of the accruals are discretionary (earnings management) after the appointment.

We find a positive and significant coefficient on *After Appointment* (=0.022, t=2.06) in Column 1 of Table A9, implying that firms have significantly higher discretionary accruals (do significantly more earnings management), once the former analyst joins the board. To get an idea of magnitude, the unconditional average of accruals in the sample is -0.01 (with a median of 0), while 0.022 represents a move to the 75th percentile, so the entire upper-quartile spread. Controlling for the level of *Total Accruals* has no effect on the magnitude or significance of *After Appointment* (Column 3 of Table A9). In the model including all controls (even current year's earnings level), the estimated change in behavior even increases in point-estimate and significance level, with *After Appointment* having a coefficient of 0.030 (t=2.51).

D. Instrumental Variables Regressions

To overcome the potential endogeneity of analyst board appointments, and specifically the possibility that poor anticipated performance might lead to analyst appointments (and that increases in earnings

[27] In the next section we address the endogeneity of these appointment dates using an instrument for appointment dates.
[28] Discretionary accruals are computed using the modified Jones model described in Dechow, Sloan, and Sweeney (1995), and are equal to the residuals from firm-level regressions of total accruals on non-discretionary accruals (where non-discretionary accruals are equal to the change in sales minus the change in receivables plus gross property, plant, and equipment, all scaled by last year's total assets); each firm must have a minimum of ten years of data to be included in these regressions.
[29] *Total Accruals* are the total annual amount of accruals of the firm, calculated as in Healy (1985).

management might reflect efforts to mitigate poor stock price performance that would have occurred whether the analyst appointment took place or not), we also instrument for the appointment of a cheerleader analyst. To be clear, the regressions in Appendix Table A9 on firm behavior include *only* firms that appoint analysts, and examine the changes in behavior of solely these firms pre- and post-appointment. Thus, the only way that an endogeneity concern could be driving the effects we document there is through a pure timing explanation. In other words, a firm could decide to appoint a former friendly analyst to the board at precisely the time when it foresees a future deterioration in performance and future need for earnings management. In this interpretation, the change in earnings management behavior we document is concurrent with, but not a result of, the analyst's appointment. As this explanation is plausible, in this section we specifically instrument for the timing of analyst board member appointments to get around the potential endogeneity problems regarding the interpretation of the changes in firm behavior in Appendix Table A9.

We do so by exploiting the post-Global Settlement time period when analysts were widely scrutinized and scorned and hence in low demand to serve as potential directors. We use this exogenous shock to the desirability/availability of analysts in the pool of potential directors as an instrument for the actual appointment of cheerleader analysts.[30] This shock involved penalties for conflicts of interest stemming from several brokerage houses' relationships with, and behavior toward, investment banking clients, and is thus plausibly exogenous to any given firm's accounting reporting decision regarding discretionary accrual behavior.

Our first-stage regression, shown in Column 1 of Panel A Table VI, is a regression of actual analyst appointment months on a categorical variable *Post-Global Settlement* that is equal to 1 for those firm-months after the Global Settlement (April 2003-April 2005) and zero otherwise, plus the same control variables used in Appendix Table A9. We run this predictive regression in the symmetric four year-window pre- and post-Global Settlement (so April 2001 - April 2005).[31] Column 1 confirms that this post-Global Settlement variable is a negative and significant predictor of analyst appointment dates (-0.014, (t=2.57)). Note that this regression, like those in Appendix Table A9, uses only those firms that appoint an analyst as a board member, since the potential endogeneity problem, as described above, concerns the *timing* of cheerleader appointment; we do address firm selection issues below in a matched sample framework. We use the predicted values of this first-stage regression as estimates of the probability that a firm will appoint a cheerleader in any given month; for each firm we then take the firm-month with the highest predicted probability of appointment, and use this as our instrumented appointment date. We then define the variable *Instrumented After Appointment* as a

[30] See Guner, Malmendier and Tate (2008) for a similar instrumental variables approach that uses the banking crisis in the late 1970s and early 1980s to identify times when commercial bankers were perceived to be less suitable candidates for corporate board directorships.

[31] We have experimented with this window, using six-, eight-, and ten-year windows around the Global Settlement, and the results are very similar in significance, with slightly smaller, but similar magnitudes. These results are available on request. We choose to show the four-year window as it isolates the tightest band of negative sentiment around the Global Settlement.

categorical variable equal to 1 for all firm-years after the firm has its highest predicted probability of appointment, and 0 otherwise.

We use these instrumented appointments to examine the pre- and post-appointment effect on firm behavior. Specifically, the second stage regression in Column 2 of Panel A Table VI is a regression of discretionary accruals on the variable *Instrumented After Appointment* plus the same control variables used in Column 1. Column 2 shows that instrumented appointments are positive and significant predictors of increases in discretionary accruals. From Column 2, the positive and significant coefficient of 0.018 (t=2.71) implies that firms have significantly higher discretionary accruals (do significantly more earnings management), once a former analyst is predicted to join the board. This instrumented coefficient is similar in magnitude, and more precisely estimated than that in Appendix Table A9.

As a falsification test for our instrumentation technique, we look at the exact same instrumented analyst board appointments, but instead examine their impact on non-discretionary accruals. Non-discretionary accruals are the portion of accruals that firms have no ability to manipulate to create a better reflection of current earnings. Thus, the appointment of an analyst should have no impact on the non-discretionary accruals of a firm. If, in contrast, our instrumented analyst appointments are simply picking up a spurious relationship with firm accruals, we would expect to see this exhibited in both the discretionary and non-discretionary portions. We therefore run the exact same regression as in Column 2, but with the dependent variable now being non-discretionary accruals. In contrast to discretionary accruals (earnings management), Column 3 illustrates that instrumented appointments do *not* predict any increases in non-discretionary accruals.

E. Matched Sample Approach

To address the possibility that the particular types of firms that choose to appoint former analysts may be the types of firms that would engage in earnings management regardless of whether or not they appointed a former analyst, we also employ a matched sample approach. We construct our matched sample by matching appointing firms to other firms that in the year leading up to the appointments were: i.) in the same Fama-French 49 industry category, ii.) in the same size quintile, iii.) in the same book-to-market quintile, and iv.) in the same discretionary accruals quintile as the appointing firms; but who did *not* appoint a cheerleader in the year of a cheerleader appointment. Note that these tests are designed to examine the composition of firms appointing cheerleader analysts, as opposed to the timing of when appointing firms choose to appoint cheerleader analysts (which we examined in our IV tests above).

Column 4 of Table VI repeats the same result shown in Column 6 of Appendix Table A9, which regresses discretionary accruals on the *After Appointment* variable described in Appendix Table A9 for the sample of appointing firms. Column 5 of Table VI runs the identical regression, but this time on our matched sample of non-appointing firms designed to mimic the characteristics of the appointing firms.

Unlike the appointing firms, which engage in significantly more earnings management post-appointment, the matched firms with similar characteristics (including engaging in the same level of earnings management in the period pre-appointment) exhibit no change in their earnings management following these cheerleader appointments.[32]

VII. Discussion

In this section we discuss the interpretation of our results in greater depth. In particular we focus on the distinction between optimism in general versus sympathy towards management, the interplay between the supply and demand for biased analysts, and the equilibrium interpretation of our findings.

A. Optimism Versus Sympathy Towards Management

From a shareholder perspective, there is an important distinction between purely optimistic analysts versus those analysts that are specifically sympathetic (cheerleaders) to management, in terms of ability to perform their duty of monitoring management. Most of our evidence on analyst recommendations to this point is consistent with both possibilities (although the opportunistic timing of especially positive recommendations from Table IV appears less supportive of unconditional optimism, and more supportive of an analyst who is sympathetic toward management). In addition, our IV evidence on increased earnings management behavior following appointment appears more consistent with a cheerleader for management. We provide two additional tests to help further distinguish between exactly which explanation (optimism toward firm vs. cheerleader toward management) better describes our appointed analysts.

The first test has to do with optimism in earnings forecasts. If the analyst were simply optimistic about the future prospects of the firm, we would expect to see this expressed in both positive views in recommendations *and* earnings forecasts. Contrast this with an analyst who is a cheerleader for management. Here we would expect to observe positively biased recommendations as these clearly benefit the firm (Womack (1996)). By contrast, the direction of the cheerleader's bias is much less clear with respect to earnings forecasts. There is some positive effect at the time of increasing a consensus estimate, however this effect is likely mitigated by the negative effect of making it more difficult for the firm to beat earnings consensus at the time of earnings announcement. Consistent with this conjecture, we find no evidence of appointed analyst optimism on one- and two-year earnings forecasts.[33] The fact that we find little evidence of optimism in the earnings forecasts of appointed analysts helps to rule out the possibility that appointed analysts are simply optimistic about firm prospects, as opposed to behaving in a specific way that benefits the

[32] This result is robust to variations in the way we construct our matching sample. For example, matching in addition on prior stock return performance gives the same result.

[33] For example, when we employ regressions using the identical specification as in Table II, but replacing the dependent variable with one-year earnings forecast optimism (measured as: (forecast-actual)/actual), the coefficient on *Appointing Forecast* is nearly zero, 0.005 (t=0.13).

managers of the firm (i.e., sympathetic to management).

We provide additional evidence on this distinction between being optimistic per se versus being sympathetic towards management by exploring post-appointment CEO compensation. For example, analysts who are purely optimistic about a firm's prospects may be especially hard on managers who fail to operate the business at a level that meets analysts' expectations. In contrast, cheerleaders for management are, by definition, beholden to the managers of the firm. Using the instrumental variables framework described in Section VI above, we test the idea that boards with former analysts behave differently towards the CEO in the post-appointment period. Specifically, Panel B of Table VI repeats the instrumental variable regressions in Columns 1 and 2 of Panel A, except that the control variable for total accruals is removed in both stages, and the dependent variable in the second-stage regression in Column 2 is now the log of total CEO compensation (the variable TDC2 as reported in the ExecuComp Database). Column 2 of Panel B indicates that firms *increase* CEO compensation post-appointment of a former cheerleader analyst. The coefficient of 0.745 (t=2.65), represents an increase of roughly one-half of a standard deviation in total compensation.

B. Supply and Demand for Biased Analysts

On the firm side, the question remains as to why firms choose to appoint these particular analysts to serve on their board of directors. Even if the only willing supply consists of these biased, relatively poor-performing analysts, the firms are still making an active decision to appoint them. Thus while it is true that an optimistic analyst may simply be more likely to accept a board seat than an otherwise similar analyst, our results imply that firms are either appointing the wrong analysts, or that firms should simply demand zero analysts if the only willing supply consists of biased analysts. One might still argue that even a biased (and poor performing) analyst may be better than the next best alternative director for the firm. However, our IV results on the increased levels of earnings management, and the fact that these appointments are concentrated in significantly more poorly governed firms suggest that the board appointments of the former analysts are not incredibly effective monitoring choices from the view of shareholder interests.

On the analyst side, a similar question arises as to why analysts would want to bias their recommendations given their incentives to produce accurate forecasts (see Stickel (1992) and Mikhail, Walther, and Willis (1999)). In this case, the poor *overall* relative performance of these appointed analysts suggests that these analysts are unlikely to reap the rewards of good performance, and hence the cost of biasing recommendations on a single firm may be low.

C. Incentive Compatible Equilibrium

The equilibrium interpretation of our results is as follows. Out of the pool of available directors, boards knowingly choose to appoint optimistic, management-friendly directors who are technically viewed as

independent. This problem is a general one, and our empirical strategy simply highlights a specific, empirically verifiable, example of this problem. The appointment of cheerleaders to the board has a causal impact on earnings management and CEO salary increases, as demonstrated in our IV tests, which show that firms increase earnings management and CEO-friendly behavior post-appointment. The firms that choose to appoint cheerleaders are those that have significantly fewer independent directors on their board nominating committees, are much more likely to have the CEO on the nominating committee, and are generally poorly governed. Finally, biased directors are appointed in advance of poor stock price performance, but the causal impact of analyst appointments on stock price performance is modest.[34]

These results represent an incentive-compatible equilibrium. CEOs (and inside directors) are happy to appoint cheerleaders who de-facto further their control of the firm. We see evidence of this in increased CEO pay following appointment, and the increase in ability to shift earnings. Supporting evidence of this arrangement is that appointing firms have significantly less independent nominating committees (and the CEO is much more likely to be a nominating committee member). While hiring sympathetic board members benefits management in terms of compensation and board control, it has only a modestly negative causal impact on firm value. Thus, it is likely to be a net-positive ex-ante behavior for management (who are thus incentive-aligned to appoint cheerleaders).

Further, from the shareholders perspective, the modest impact on firm value makes it difficult for shareholders to pinpoint, and thus likely organize against management, to take action against the hiring of cheerleaders. Add to this the plurality voting system used by most US firms, and pervasive use of solely board nomination (working through the nominating committee), and the hiring of cheerleaders can be a stable, observed equilibrium.

VIII. Conclusion

This paper provides evidence that firms appoint independent directors who are overly sympathetic to management, while still technically independent according to regulatory definitions. We do so by exploiting a unique, hand-collected database of former sell-side analysts who are appointed to the boards of companies they previously covered; importantly, our data provides us with information on these directors' views regarding the firm *prior* to being appointed to the board. Our empirical strategy thus allows us to directly

[34] In the Appendix Table A7, we examine the abnormal stock returns following analyst appointments. Using actual appointment dates, we find significant negative abnormal returns following analyst appointments. However, when we employ the IV strategy from Section VI in order to isolate the *causal* impact of appointments on firm performance, we estimate the causal impact of appointments to be negative but modest in economic terms (ranging from 12 to 45 basis points per month in the year following appointment) and statistically unreliable; this suggests that some of the negative poor post-appointment performance was anticipated and that biased directors were appointed in advance of this poor performance. In Appendix Table A8 we also show insignificant announcement effects around these analyst appointments.

evaluate the objectivity and potential effectiveness of a class of independent directors based solely on their *observable* opinions about the firm in question. We use these analysts' track records to examine the roles of optimism and ability in the board appointment process. In doing so we find evidence that boards appoint overly optimistic analysts (i.e., cheerleaders for management) who exhibit little skill in evaluating the firm itself, or in evaluating firms in general. While the literature has explored measures of director relationships, this paper is the first to empirically document this phenomenon of firms actively appointing board cheerleaders.

The magnitude of the optimistic bias is large: 82.0% of appointed recommendations are strong-buy/buy recommendations, compared to 56.9% for all other analyst recommendations. At the same time, board-appointed analysts exhibit poor relative performance on their recommendations on appointing firms. They also demonstrate poor overall relative performance on their stock recommendations and earnings forecasts across all the firms that they cover. Additionally, these appointed analysts appear to be especially optimistic at times that are most favorable to the appointing firms (e.g., prior to stock issuances). Lastly, we examine the behavior of these appointing firms following the analyst's appointment, and find that appointing firms significantly increase their earnings management activities and CEO compensation in the post-appointment period.

We believe that our results, when taken as a whole, shed new light on the views and characteristics of independent directors, and of the firms who appoint them. Further, the post-appointment behavior of the appointing firms in our sample suggest that exploring the past track records and backgrounds of *all* board members (beyond simply independent vs. inside) may be a useful way to identify cross-sectional variation in firm governance quality. Before the question of whether independent boards benefit shareholders can be adequately addressed, more research is needed to determine the true nature of "independence" within corporate boards, which begins with an understanding of the true independence of directors.

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Table I: Recommendations of Analyst Appointees

This table reports the distribution of recommendations of analysts. There are five distinct levels of recommendations, ranging between Strong Sell and Strong Buy. Panel A reports the distribution of recommendations issued by analysts who are appointed to the board, on those firms that appoint the analyst to the board. Panel B reports the distribution of all other recommendations on the I/B/E/S tape. Panel C reports the distribution of recommendations by analysts who are *not* appointed to the board, on those firms that appoint an analyst to the board. Panel D reports the distribution of recommendation by analysts who are appointed to the board, on all the stocks they cover excluding the firm who appoints them to the board. Chi-square tests for equality of distributions between the comparison groups are given in each panel, along with p-values.

	Panel A		Panel B			Panel C			Panel D		
	Appointed recommendations		All other recommendations			All recommendations on firms appointing analysts to the board			All recommendations by analysts appointed to a board		
	%	Cum %	%	Diff	Cum %	%	Diff	Cum %	%	Diff	Cum %
Strong Buy	41.7	41.7	25.2	16.5	25.2	24.8	17.0	24.8	26.6	15.1	26.6
Buy	40.3	82.0	31.7	8.6	56.9	31.6	8.7	56.4	38.2	2.1	64.8
Hold	15.8	97.8	37.0	-21.2	93.9	38.0	-22.2	94.4	31.5	-15.6	96.2
Sell	2.2	100.0	4.0	-1.8	97.9	3.6	-1.5	98.0	2.8	-0.7	99.0
Strong Sell	0.0	100.0	2.1	-2.1	100.0	2.0	-2.0	100.0	1.0	-1.0	100.0
Chi-square				39.2			39.8			23.6	
P-value				0.00			0.00			0.00	

Table II: Appointed Analyst Recommendations

The dependent variable in each regression is the level of recommendation, which ranges between 1 and 5, and which we reverse-score such that 1=Strong Sell, 2=Sell, 3=Hold, 4=Buy, and 5=Strong Buy. The key variable of interest is in the first row: *Appointed Rec* equals 1 if the analyst recommending the stock in question is subsequently appointed to the board of directors of that firm, and 0 otherwise. The other independent variables are as follows: *Size* measures the log(ME) and *B/M* measures the log(BE/ME), of the firm being recommended. *Past Month Return* and *Past Year Return* measure the given stock's return in the prior month, and 11-months prior to that month respectively, from the recommendation date. *Brokerage Size* is the total number of analysts that work at the given analyst's brokerage house. At the time of each recommendation, *Experience* measures an analyst's history of recommending stocks on I/B/E/S (in years), while *Exper. Rec. Firm* measures the number of years an analyst has been recommending a given stock. *All Star* is a categorical variable equal to 1 if the analyst was voted an all star analyst in the October issue of Institutional Investor magazine for the given year. *Connected to Firm* is a categorical variable equal to 1 if the analyst attended the same school as one of the senior officers of the firm being recommended. *Affiliation* is a categorical variable that measures whether or not the given firm has an underwriting relationship with the analyst's brokerage. Column 7 and 8 split our sample period to pre- and post-Reg FD (October 2000). Column 9 runs an ordered logit regression, where the left hand side variable is the recommendation level (1-5). Fixed effects for recommendation month (Time), for industry (Industry) using the Fama-French industry definitions, for the firm (Firm), and for the analyst (Analyst), are included where indicated. All standard errors are adjusted for clustering at the recommendation month level, and t-stats using these clustered standard errors are included in parentheses below the coefficient estimates. 1%, 5%, and 10% statistical significance are indicated with ***, **, and *, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
							Pre- Reg FD	Post-Reg FD	Logit
Appointed Rec	0.477***	0.426***	0.363***	0.435***	0.435***	0.417***	0.412**	0.465**	1.027***
	(5.90)	(5.14)	(4.70)	(5.19)	(5.14)	(3.21)	(2.10)	(2.28)	(4.15)
Size		0.004	0.039***	0.008***	0.079***	0.092***	0.088***	0.120***	0.020**
		(1.44)	(12.54)	(3.14)	(8.13)	(7.53)	(4.76)	(6.83)	(2.38)
B/M		-0.037***	-0.017***	-0.027***	-0.022***	-0.006	-0.020	0.013	-0.120***
		(9.10)	(4.66)	(7.82)	(3.38)	(0.57)	(0.88)	(0.97)	(9.45)
Past Month Return		0.224***	0.191***	0.221***	0.144***	0.116***	0.162***	0.008	0.283***
		(8.60)	(8.50)	(8.72)	(6.50)	(3.06)	(2.83)	(0.16)	(2.65)
Past Year Return		0.121***	0.106***	0.119***	0.095***	0.082***	0.056***	0.089***	0.283***
		(12.69)	(12.77)	(12.67)	(12.29)	(8.51)	(4.03)	(6.36)	(11.25)
Brokerage Size		-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	0.000	-0.002***	-0.003***
		(6.93)	(4.43)	(6.94)	(6.33)	(8.23)	(0.29)	(8.55)	(8.78)
Experience		0.004***	-0.024***	0.003***	0.002	0.002	-0.002	0.001	-0.008
		(3.25)	(3.22)	(2.84)	(1.63)	(1.04)	(0.40)	(0.51)	(1.60)
Exper. Rec. Firm		-0.034***	-0.035***	-0.034***	-0.024***	-0.027***	-0.030***	-0.016***	-0.050***
		(13.55)	(15.44)	(14.08)	(9.41)	(6.25)	(3.61)	(3.41)	(6.45)
All Star		-0.013	0.004	-0.012	-0.007	0.003	0.066**	-0.084***	0.220***
		(1.08)	(0.33)	(0.94)	(0.52)	(0.14)	(2.22)	(3.03)	(4.44)
Connected to Firm						-0.015	-0.025	-0.011	-0.001
						(1.26)	(1.16)	(0.71)	(0.04)
Affiliation		0.136***	0.108***	0.129***	0.115***	0.115***	0.098***	0.137***	0.576***
		(8.53)	(6.50)	(7.85)	(6.56)	(5.04)	(3.09)	(3.23)	(10.59)
Fixed Effect		Time	Time	Time	Time	Time	Time	Time	
Fixed Effect			Analyst	Industry	Firm	Firm	Firm	Firm	
R^2	0.01	0.07	0. 17	0.07	0.14	0.19	0.22	0.20	
Observations	421,099	371,947	371,947	371,947	371,947	65,908	20,391	44,999	65,908

Table III: Wrong Bets and Analyst Ability Ranking

The dependent variable in Columns 1-4 is *Wrong Bet*. *Wrong Bet* measures incorrect calls by analysts, and is a categorical variable equal to 1 if i.) the analyst recommends a Buy or Strong Buy on the given stock and the price declines over the following year or ii.) the analyst recommends a Sell or Strong Sell on the given stock and the price rises over the following year. For Columns 1 and 2, these bets are measured using recommendations. *Wrong Bet* is defined equivalently in Columns 3-4 with the addition that the given recommendation is an upgrade or downgrade from the prevailing consensus recommendation, and then tracking subsequent performance of the stock. The dependent variable in Columns 5-8 is *Analyst Ability Rank*, a measure of an analyst's rank relative to the rest of her peer analysts. In these four columns, this rank is normalized to between 1-100, with the higher ranks meaning better performance (with 100 being the top performer, 1 being the poorest). In Columns 5-6, analysts are ranked according to their earnings forecast ability following Hong and Kubik (2003). In Columns 7-8, analysts are ranked according to the predictive ability of their recommendations (upgrades and downgrades) for future returns, using the measure *Wrong Bets* as defined above. In both measures the rankings are averaged across all stocks an analyst issues forecasts (or gives recommendations) on in a given year, giving an analyst-level ranking for that year. Thus, observations are at an analyst-year level, so that every analyst-year will represent one observation. The independent variable of interest is *Appointed Rec*, equal to 1 if the analyst recommending the stock in question is subsequently appointed to the board of directors of that firm, and 0 otherwise. The other independent variables are as follows: *Size, B/M, Past Month Return, Past Year Return, Brokerage Size, Experience, Exper. Rec. Firm, All Star, and Affiliation* are included as controls in each regression, and are described in Table II. *Average Rec* is the average level of all an analyst's recommendations for a given year, which ranges between 1=Strong Sell and 5=Strong Buy. Fixed effects for time (Time), monthly for wrong bet, annual for Analyst Ability, and for the firm (Firm) are included where indicated. All standard errors are adjusted for clustering at the recommendation month level, and t-stats using these clustered standard errors are included in parentheses below the coefficient estimates. 1%, 5%, and 10% statistical significance are indicated with ***, **, and *, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Var:	Wrong Bet				Analyst Ability Rank			
	Rec	Rec	Up/Down	Up/Down	Earnings	Earnings	Recommend	Recommend
Appointed Rec	0.134***	0.154***	0.159***	0.165***	-5.640***	-3.896***	-5.514***	-3.014**
	(2.60)	(2.67)	(3.45)	(3.11)	(4.16)	(3.26)	(4.07)	(2.03)
Size		0.136***		0.064***		8.339***		-1.315***
		(16.87)		(14.40)		(12.95)		(5.57)
B/M		-0.020***		-0.002		1.021		0.615*
		(5.33)		(0.51)		(1.52)		(1.69)
Past Month Return		0.018		0.029**				
		(0.99)		(2.20)				
Past Year Return		0.023***		0.009***				
		(8.12)		(4.30)				
Brokerage Size		0.000***		0.000***		0.012*		0.008**
		(3.30)		(4.72)		(1.80)		(1.95)
Experience		-0.001		-0.002***		-0.414***		-0.741***
		(1.60)		(3.92)		(3.18)		(6.19)
Exper. Rec. Firm		-0.002		0.002		-1.344***		-0.099
		(1.63)		(1.64)		(4.35)		(0.39)
All Star		0.014***		0.015***		-2.401***		-5.745***
		(3.18)		(2.89)		(3.33)		(11.78)
Affiliation		0.028*		0.021*		-5.528		7.309
		(1.69)		(1.78)		(0.86)		(1.46)
Average Rec						3.658***		0.153
						(9.12)		(0.17)
Num Analysts						-1.153***		-0.428***
						(5.50)		(5.30)
Fixed Effect	Time	Time	Time	Time	Time	Time	Time	Time
Fixed Effect	Firm	Firm	Firm	Firm				
R^2	0.18	0.19	0.13	0.11	0.01	0.10	0.01	0.03
Observations	220,572	188,425	220,572	188,425	44,362	31,890	40,821	39,834

Table IV: Timing of Analysts' Positive Recommendations

This table reports panel regressions of analyst recommendations. The dependent variable in columns 1-4 is the level of recommendation (*Rec*), which ranges from 1=Strong Sell to 5=Strong Buy. In columns 5 and 6, the dependent variable is *Downgrade*, which is a categorical variable equal to 1 if the recommendation is a downgrade from the current consensus, and 0 otherwise. The independent variable *Appointed Rec* is a categorical variable that is equal to 1 if the analyst recommending the given stock is subsequently appointed to the board of directors of the firm, and 0 otherwise. *Last Rec. Downgrade* is equal to 1 if the last recommendation on the stock (before the given analyst's recommendation) was a downgrade, and 0 otherwise. *High Short Interest* is equal to 1 if short interest in the month prior to the given recommendation was higher than the median, and 0 otherwise. *High Future Issuance* is equal to 1 if the firm being recommended has higher than median issuance over the 6 months following recommendation, and 0 otherwise. Interaction effects are included where shown. *Size*, *B/M, Past Month Return, Past Year Return*, *Brokerage Size*, *Experience*, *Exper. Rec. Firm, All Star*, and *Affiliation* are also included as controls in every regression, and are described in Table II. Fixed effects for recommendation month (Time) and the firm (Firm) are included where indicated. All standard errors are adjusted for clustering at the recommendation month level, and t-stats using these clustered standard errors are included in parentheses below the coefficient estimates. 1%, 5%, and 10% statistical significance are indicated with ***, **, and *, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Var:	Rec	Rec	Rec	Rec	Downgrade	Downgrade
Appointed Rec	0.381***	0.279***	0.231*	0.240*	-0.173***	-0.013
	(4.25)	(2.51)	(1.74)	(1.86)	(3.08)	(0.18)
Last Rec. Downgrade	-0.065***	-0.065***	-0.065***	-0.065***	0.070***	0.070***
	(13.75)	(13.75)	(13.75)	(13.77)	(19.78)	(19.79)
High Short Interest	-0.083***	-0.083***	-0.083***	-0.083***	0.026***	0.026***
	(14.17)	(14.19)	(14.18)	(14.17)	(8.83)	(8.83)
High Future Issuance	0.066***	0.066***	0.066***	0.066***	-0.018***	-0.018***
	(15.26)	(15.26)	(15.25)	(15.25)	(7.53)	(7.53)
High Short*Appointed Rec		0.334**				
		(2.19)				
High Issue*Appointed Rec			0.306**			
			(2.11)			
Last Rec Down*Appointed Rec				0.245		-0.275***
				(1.63)		(3.05)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effect	Time	Time	Time	Time	Time	Time
Fixed Effect	Firm	Firm	Firm	Firm	Firm	Firm
R^2	0.14	0.14	0.14	0.14	0.06	0.06
Observations	324,518	324,518	324,518	324,518	323,673	323,673

Table V: Nominating Committee and Governance of Firms That Appoint Analysts

Panel A of this table reports panel regressions of the makeup of the Nominating Committee of firms in our sample from 1993-2006. The dependent variables are as follows: *% Indep* is the percentage of independent directors on the nominating committee; *Maj Indep* is a categorical variable equal to 1 if the given nominating committee is majority independent; *CEO On* is a categorical variable equal to 1 if the CEO is a member of the nominating committee. The independent variable of interest in Panel A is *Appointing Firm,* and is equal to 1 in the year the appointing firm's given nominating nominates the analyst for the board position, and 0 otherwise Panel B shows panel regressions of firm governance characteristics over our sample period, from 1993 to 2006. The dependent variables are as follows: in column 1, the governance index (*GIndex*) from Gompers, Ishii, and Metrick (2003); in column 2, the delay index (*Delay)*, also Gompers, Ishii, and Metrick (2003), meant to capture provisions that slow hostile bidders; in column 3, a dummy variable equal to one if the firm has a classified board (*CBoard*); in column 4, a dummy variable equal to one if the firm has limits to written consent (*Limits*). The independent variable of interest in Panel B is *Appointing Firm*, and is equal to 1 if the firm appoints an analyst who previously covered the firm to its board of directors at some point over the sample, and 0 otherwise. *Size* measures the log(ME) and *B/M* measures the log(BE/ME), of the firm being recommended. *Return Volatility* measures the given stock's standard deviation of monthly returns over the past year, and *Past Year Return* measure the given stock's return from months *t*-12 to *t*-2. Fixed effects for industry (Industry) using the Fama-French industry definitions, and for year (*Time*), are included where indicated. All standard errors are adjusted for clustering at the year level, and t-stats using these clustered standard errors are included in parentheses below the coefficient estimates. 1%, 5%, and 10% statistical significance are indicated with ***, **, and *, respectively.

Panel A: Composition of the Nominating Committee

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Var:	% Indep	Maj Indep	CEO On	% Indep	Maj Indep	CEO On
Appointing Firm	-0.287***	-0.311***	0.215**	-0.289**	-0.312***	0.227***
	(2.84)	(3.01)	(2.19)	(2.79)	(2.58)	(3.01)
Size	-0.001	-0.003*	0.001	-0.001	-0.003	0.000
	(1.27)	(1.80)	(1.39)	(0.62)	(1.21)	(0.21)
B/M	-0.002	-0.009***	0.004	-0.002	-0.009*	0.004
	(1.15)	(3.76)	(2.56)	(0.67)	(1.89)	(1.52)
Return Volatility	-0.039	-0.023	0.056*	-0.028	-0.015	0.031
	(2.82)	(0.80)	(1.76)	(0.93)	(0.44)	(1.29)
Past Year Return	0.000	-0.010	-0.003	0.000	-0.010	-0.001
	(0.13)	(1.61)	(0.53)	(0.10)	(1.35)	(0.20)
Fixed Effect				Industry	Industry	Industry
Fixed Effect				Time	Time	Time
R^2	0.01	0.01	0.01	0.03	0.02	0.03
Observations	5,764	5,764	5,764	5,764	5,764	5,764

Panel B: Governance Characteristics

	(1)	(2)	(3)	(4)
Dependent Var:	GIndex	Delay	CBoard	Limits
Appointing Firm	0.284***	0.243***	0.093***	0.138***
	(4.06)	(11.00)	(7.60)	(11.07)
Controls	Yes	Yes	Yes	Yes
Fixed Effect	Industry	Industry	Industry	Industry
Fixed Effect	Time	Time	Time	Time
R^2	0.02	0.05	0.01	0.04
Observations	21,058	21,058	21,058	21,058

Table VI: Post-Appointment Behavior: Instrumental Variables and Matched Samples

The first three columns of Panel A report results from two-stage least squares regressions that instrument for the appointment for a cheerleader. These regressions include only those firms that appoint an analyst as a board member. The first stage of this regression (Column 1) is a regression of actual appointment months (of cheerleader analysts) on a categorical variable *Post-Global Settlement* that is equal to 1 for those years directly after the Global Settlement (April 2003-April 2005), and zero otherwise, plus the same control variables used in Table A9. The firm-month with the maximum predicted value of appointment is then designated as the instrumented appointment date, and the variable *Instrumented After Appointment* is then a categorical variable equal to 1 for those firm-years after the instrumented appointment date. The second stage regression in Column 2 (Column 3) is a regression of discretionary accruals (non-discretionary accruals) on the variable *Instrumented After Appointment* plus the same control variables used in Column 1. Columns 4 and 5 of Panel A compare the results in Table A9 to those obtained from a matched sample panel regression of discretionary accruals on the same *After Appointment* variable described in Table A9. Column 4 repeats the same result shown in Column 6 of Table A9, which regresses discretionary accruals on the *After Appointment* variable described in Table A9 for the sample of appointing firms; Column 5 runs the identical regression, but this time on a matched sample of firms designed to mimic the characteristics of the appointing firms. The matched sample is constructed by matching appointing firms to firms in the same Fama-French 49 industry category, same size quintile, same book-to-market quintile, and same discretionary accruals quintile, but who did *not* appoint a cheerleader in the year of a cheerleader appointment. Panel B repeats the instrumental variables regressions in Columns 1 and 2 of Panel A, except that the control variable for total accruals is removed in both stages, and the dependent variable in the second-stage regression in Column 2 is now the log of total CEO compensation (the variable TDC2 as reported in Execucomp). Firm fixed effects (*Firm*) and year fixed effects (*Time*) are included where indicated. All standard errors are adjusted for clustering at the year level (month level in Column 1), and t-stats using these clustered standard errors are included in parentheses below the coefficient estimates.

Panel A: IV Accruals Behavior and Matched Sample

	IV Estimation			Matched-Sample	
	1st Stage	2nd Stage	2nd Stage	Our Sample	Matched Sample
Dependent Variable:	Appoint Date	Disc Accr	Non-Disc	Disc Accr	Disc Accr
	(1)	(2)	(3)	(4)	(5)
Post Global Settlement	-0.014***				
	(2.57)				
Instrumented After Appointment		0.018***	-0.028		
		(2.71)	(0.94)		
After Appointment				0.030**	0.003
				(2.51)	(0.75)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed Effect		Time	Time	Time	Time
Fixed Effect	Firm	Firm	Firm	Firm	Firm
R^2	0.02	0.29	0.28	0.29	0.19
Observations	1440	346	346	346	1366

Panel B: IV Compensation Behavior

	1st Stage	2nd Stage
Dependent Variable:	Appoint Date	Compensation
	(1)	(2)
Post Global Settlement	-0.012***	
	(2.62)	
Instrumented After Appointment		0.745***
		(2.65)
Controls	Yes	Yes
Fixed Effect		Time
Fixed Effect	Firm	Firm
R^2	0.02	0.62
Observations	1564	283