Call Me by Your Name: The Effect of Analyst-CEO First Name Commonality on Analyst Forecast Accuracy

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Abstract

In this paper we document that the earnings forecasts of security analysts who share a first name with the CEO of a covered firm (referred to as 'matched' analysts) are more accurate, on average, than those of analysts who do not share a first name (referred to as 'unmatched' analysts). This result is consistent with findings in psychology which show that individuals have an affinity for those who share first names and suggests that the CEO is more likely to share private information with a matched analyst. We find this phenomenon to be concentrated among those matched analysts with less common first names, perhaps because the salience of sharing a first name is lower for analysts with more common names. It is also stronger in situations where there is greater information asymmetry between management and analysts.

JEL Codes: G14, G24, G40, M41

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"Using data on nearly four million speeding tickets in Florida, we examined…whether something as trivial as having the same first name as a police officer could affect a driver's likelihood of receiving a costly speeding ticket.

It can." [1]

I. Introduction

As illustrated by the above quote from a *New York Times* article, names play an important, and sometimes outsized, role in society. Research in accounting and finance is replete with examples of this. Jung, Kumar, Lim, and Yoo (2019), for instance, find that the perceived favorability of an individual analyst's surname can predict the price reaction to the analyst's forecast revisions, despite there being no significant association between forecast accuracy and surname favorability. Kumar, Niessen-Ruenzi, and Spalt (2015) document that the flow of funds into U.S. mutual funds is significantly lower for managers with foreign-sounding surnames than for those with American-sounding surnames, holding fund performance constant. Tan, Xiao, Zeng, and Zou (2018) document a negative relation between the sharing of surnames among the members of a firm's board of directors and firm value, suggesting that surname sharing creates personal ties between otherwise unrelated persons, compromising board effectiveness. Cooper, Dimitrov, and Rau (2001) find that during the dot com boom, changing a firm's name to an Internet-related name elicited a positive price reaction, regardless of the firm's involvement with the Internet, while Cooper, Gulen, and Rau (2005) document that mutual funds that changed their name to reflect a currently-hot investment style received an influx of funds, even though performance did not improve.[2]

[1] "The Benefit of Having the Same Names as a Police Officer," New York Times, August 5, 2018, Section SR, p. 9.

[2] There is another literature that examines the effect of corporate names and corporate stock tickers on financial markets. For example, Green and Jame (2013) find that firms whose names are considered to be more fluent are associated with greater breadth of ownership, greater liquidity, and higher Tobin's q and market-to-book ratios. Xing, Anderson, and Hu (2016) find a positive association between the likeability/pronounceability of a firm's stock ticker and its Tobin's q ratio.

In this paper we examine whether an analyst's earnings forecast accuracy is affected by whether the analyst and the firm's CEO share a first name. Our work is grounded in research in psychology which suggests that people have a greater liking of, and attraction to, individuals with a first name similar to their own and a greater willingness to respond favorably to requests made by such individuals. (See Jones, Pelham, Mirenberg, and Carvallo, 2004, and Garner, 2005.) This finding is considered to be a manifestation of *implicit egotism*, whereby individuals express a preference for people, places, and things that they unconsciously associate with themselves (see, for example, Jones, Pelham, Mirenberg, and Hetts, 2002, and Pelham, Mirenberg, and Jones, 2002).[3] We conjecture that the greater affinity arising from the sharing of a first name will lead to a greater willingness on the part of the CEO to share private information with an analyst and will, in turn, manifest itself in increased forecast accuracy.

To test this, we use the annual earnings forecasts issued by analysts between 1992 and 2018. For each firm and year, we calculate the forecast accuracy of each analyst covering that firm relative to that of all other analysts covering the firm that year. Consistent with our conjecture, we find that the relative accuracy of forecasts issued by analysts with the same first name as that of the firm's CEO is significantly greater, on average, than that of forecasts issued by analysts with a first name different from that of the CEO. Further, for a given analyst we find

[3] This phenomenon appears to be quite pervasive. Nuttin (1985) found that individuals have a preference for the letters in their own name, especially the initials of their first and last names. (This is called the "name letter effect.") Kooti, Magno, and Weber (2014) found that Twitter users are more likely to follow other users with the same first name than to follow users with other names. Jones et al. (2004) found that people were disproportionately more likely to marry someone sharing the beginning letters of their first name (for example, James is more likely to marry Juliette than to marry Deborah; David is more likely to marry Daniel than to marry Adam). Pelham et al. (2002) provided evidence that individuals are more likely to choose a profession whose name shares the first few letters with the individuals' first name (for example, Denny is more likely to be a dentist than a lawyer; Laura is more likely to be a lawyer than an engineer). Gallucci (2003) and Simonsohn (2011) questioned some of the conclusions in Pelham et al. (2002) regarding the link between names and major life decisions. Pelham, Carvalo, DeHart, and Jones (2003) and Pelham and Carvalo (2011) provided rebuttals.

that relative forecast accuracy is higher, on average, for those covered firms where the analyst's first name matches that of the CEO than for those where there is no match.

To provide further support for our results, we examine the change in forecast accuracy when there is CEO turnover. At such a time, previously matched analysts become unmatched (except in the rare circumstance where the new CEO has the same first name as the previous one), while some previously unmatched analysts may become matched. We expect the relative forecast accuracy of the former group to decrease post-turnover, while that of the latter group to increase. Both of these conjectures are borne out by the data, supporting our conclusion that the relative forecast accuracy of matched analysts exceeds, on average, that of unmatched analysts.

To rule out the possibility that ethnic and/or gender ties are driving the observed forecasting superiority of matched analysts, we add controls for ethnic and gender ties into our analysis. Doing so, we find that the forecast accuracy of matched analysts remains significantly greater than that of unmatched analysts. As an alternative test, we exclude from our sample all analyst-CEO pairs in which both are either women or have the same ethnicity. Again, we find that matched analysts exhibit significantly greater forecast accuracy than do unmatched analysts. These findings allow us to conclude that neither gender nor ethnic ties are driving our results.

Analysts and CEOs with very common first names are accustomed to meeting people with the same name; for them, the salience of sharing a first name is likely to be lower. As a result, we expect that the positive effect of a first name match on relative forecast accuracy will be more pronounced the less common that name. To test this we partition our matched name subsample into two groups – analysts with more common names and analysts with less common names, using data on first name frequency provided by the Social Security Administration. We find that the forecasts of the matched analysts with less common names are significantly more accurate than are those of unmatched analysts. In contrast, the relative accuracy of the forecasts of the matched analysts with more common names is not significantly different from that of unmatched analysts. These results are consistent with our expectations.

If matched analysts have access to private information, then we would expect their forecasting superiority to be greater when there is more information asymmetry between the CEO and analysts. We examine this by partitioning our sample into those firm-years where management has issued their own earnings forecast (which would lessen information asymmetry) and those where they have not. Consistent with our conjecture, we find that the forecast accuracy of matched analysts is significantly higher than that of unmatched analysts when there is no management forecast. In contrast, there is no significant difference in accuracy when a management forecast has been released. For the set of firm-years without management forecasts, we go one step further and partition firms according to market capitalization. We expect information asymmetry to be higher among the relatively smaller firms and, therefore, for there to be a greater effect of a first name match on relative forecast accuracy. Again consistent with our conjecture, matched analysts' forecast accuracy is significantly higher than that of unmatched analysts for the smaller firms in our sample, but not for the largest firms.

We conjecture that the superior forecasting accuracy of a matched analyst relative to that of an unmatched analyst will decrease as analyst experience covering the firm increases. There are two reasons why this might happen. The first is that as analysts, whether matched or unmatched, gain experience with a covered firm, their expertise in forecasting earnings increases (see Clement, 1999, and Mikhail, Walther, and Willis, 1997), thereby reducing the importance of sharing a first name with the CEO. The second is that, as time passes, the affinity between an analyst and CEO due just to their sharing of a first name may lessen. This is consistent with the finding of List (2003) that, in a market setting, psychological biases diminish with participant experience. Our conjecture is supported by the data; compared to non-matched analysts with the same experience covering the same firm, the forecast superiority of matched analysts decreases over time.

Our analysis further reveals that the matched analysts' forecasting superiority remains significant after the implementation of Regulation Fair Disclosure (Reg FD). This regulation prohibits public firms from selectively disclosing 'material' private information to market professionals and certain shareholders. If Reg FD was fully effective, then matched analysts' forecasting superiority should have disappeared after its implementation. There is reason to believe, though, that Reg FD has not completely halted the selective disclosure of material information. This is because the regulation does not prohibit management from meeting privately with an individual analyst to discuss details of the business, which may allow the analyst to complete a 'mosaic' of information that could be material to the analyst (see Cooley Godward, 2000). These meetings also allow the analyst to take advantage of nonverbal communication in order to make inferences about the manager's private information (see the survey results of Brown, Call, Clement, and Sharp, 2015). Empirical evidence consistent with this is provided by Bushee, Jung, and Miller (2013) who find that investors make profitable trades subsequent to one-on-one meetings with managers at invitation-only investor conferences, even after the implementation of Reg FD. Additionally, Solomon and Soltes (2013), using a dataset of one-on-one meetings for a NYSE-traded firm, find that those investors who meet privately with management make more informed investment decisions. That matched analysts' forecasting superiority remains after the implementation of Reg FD provides additional suggestive evidence that the regulation has not been completely effective at prohibiting the selective dissemination of private information.

Our work contributes to a body of research examining the effect of shared characteristics between analysts and CEOs on analysts' forecasts and recommendations. Jannati, Kumar, Niessen-Ruenzi, and Wolfers (2016), for example, find that male analysts issue more optimistic forecasts for firms with male CEOs than for firms with female CEOs. They also find that consensus forecasts are higher for firms with domestic CEOs and for firms with CEOs whose political contributions are made predominantly to the Republican party. The authors explain these latter results by noting that most analysts in the *IBES* database are American and that most of them donate primarily to the Republican party. Cohen, Frazzini, and Malloy (2010) show that the buy recommendations issued by analysts graduating from the same university as one of the senior officers of the firm that is being recommended significantly outperform those of analysts who do not have a university connection.[4] The authors conjecture that this superior performance stems from the willingness of firm management to share private information with analysts with whom they have university ties. They present evidence consistent with their conjecture.[5]

The structure of this paper is as follow. In section 2 we discuss our sample construction procedure. Variable definitions and descriptive statistics are presented in section 3. Our analysis

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[4] No significant difference in performance is found for sell recommendations. They also do not find any differences between the accuracy of earnings forecasts issued by analysts who share a university tie and those issued by analysts who do not.

[5] The impact of social ties has been studied in other settings as well. For example, Hwang and Kim (2009) find that corporate boards where a majority of directors have a tie with the firm's CEO (alma mater, military service, regional origin, academic discipline, or industry) award higher pay to that CEO than do boards whose members do not have these ties. Cohen, Frazzini, and Malloy (2008) find that mutual fund managers make larger investments in firms for which they share a university tie with a senior official. The mutual fund managers also earn a significantly higher return on their investment in those firms, suggesting that there is a flow of private information from the firm to fund managers when there exists a university tie.

of forecast accuracy for matched and unmatched analysts appears in section 4. A summary and conclusions section ends the paper.

II. Sample Construction

Table I summarizes the process of arriving at our final sample. Our initial sample consists of the approximately 1.9 million annual earnings forecasts that are recorded in the *IBES* forecast database over the period 1992 through 2018 and for which the name of the firm's CEO appears in the *Execucomp* database (which covers the S&P 1500 firms). Of these forecasts, we retain only those for whom we can determine the first name of the analyst issuing the forecast. The forecast database does not directly allow us to ascertain the analyst's first name since it only contains an analyst code. To find the analyst's name, we use the *IBES* recommendations database, which provides the analyst code and analyst last name and first initial for each analyst who has issued a recommendation. Using the analyst's last name and first initial, and the name of the firm for which the analyst worked, we search the *Capital IQ* database, as well as consult various Internet sources (such as LinkedIn) to find the analyst's full first name. We are able to identify the first name of the analysts for about half of our initial sample of forecasts. From that reduced sample, we delete forecasts with missing announcement date, those issued either after the earnings announcement or more than a year before the earnings announcement, and forecasts for which the earnings announcement date is more than six months after the fiscal year-end. This leaves us with 904,748 annual earnings forecasts. Of these forecasts, we retain just the last forecast issued by an analyst for a given fiscal year. If a firm-year has fewer than five analysts disseminating forecasts, that firm-year is dropped from our sample.[6] After dropping those

[6] Dropping only those observations with fewer than three analysts does not affect our results.

observations, the final number of last-of-the-year annual earnings forecasts in our sample is 193,698.

In determining whether an analyst and CEO have the same first name, we allow for variations in spelling. (Our results hold if we restrict matches to be those where the names are spelled identically.) For example, an analyst with the name Allen is considered to share a first name with a CEO named Allan and with a CEO named Alan; an analyst with the name Steven is considered to share a first name with a CEO named Stephen. We do not extend this to nicknames, though; an analyst and CEO are not considered to have the same first name if one uses a formal first name and the other uses a nickname of that formal first name. For example, an analyst with the name David is not considered to share a first name with a CEO named Dave. One reason for not extending matches to nicknames is that it is not always clear what nicknames are associated with any given name. Another reason is that two formal names might share one nickname. For example, Al could be a nickname for both Allen and Albert. It is unclear that Allen and Albert would consider themselves to have the same first name.

As reported in Table II, panel A, our final sample is comprised of 4,890 unique equity analysts, 592 (12.1%) of whom share their first name with at least one CEO at some point during the analyst's career. Our sample also consists of 4,380 unique CEOs, 677 (15.5%) of whom share their first name with at least one analyst at some point during the CEO's tenure. There are 69,514 unique analyst-CEO pairs, of which 921 (1.3%) share their first names. Table II, panel B lists all of the matched names, and the number of times that each name appears as part of a match. As seen in the table, the top five names, John, David, Michael, James, and Robert comprise approximately 53% of all of the matched names.

III. Variable definitions and descriptive statistics

III. a. Dependent variable

Our dependent variable is relative forecast accuracy. For each analyst *i* covering firm *f* in year *t*, we first calculate the analyst's earnings forecast error for the firm in that year, FE_{ift}. It is equal to the difference between the firm's realized earnings for that year and the analyst's last annual earnings forecast preceding the release of those earnings. (Using the analyst's last forecast prior to the end of the period is common in empirical studies. See, for example, Clement, 1999, Clement and Tse, 2005, O'Brien, 1990, and Sinha, Brown, and Das, 1997.) The analyst's relative forecast accuracy, RFA_{ift}, is then given by:

$$RFA_{ift} = \frac{Abs(FE_{ift}) - \overline{Abs(FE_{ft})}}{\overline{Abs(FE_{ft})}} \qquad (1)$$

where $Abs(FE_{ift})$ is the absolute value of FE_{ift} and $\overline{Abs(FE_{ft})}$ is the average of the absolute forecast errors of all the analysts following firm *f* in year *t*. (This is the same measure of relative forecast accuracy used by Clement, 1999, Malloy, 2005, and Bae, Stulz, and Tan, 2008.) A negative (positive) value of RFA_{ift} reflects a forecast that is more (less) accurate than the average. For our analysis we winsorize observations of relative forecast error at the 99th percentile. If $\overline{Abs(FE_{ft})}$ is equal to zero, meaning that all analysts have a zero forecast error, we set RFA_{ift} equal to 0.[7] As reported in Table III, the mean value of *RFA* is zero (by definition), and ranges from -1 (when the analyst's forecast error is zero) to a maximum value of 18.85.

[7] There are very few such observations. Excluding them from our analysis has no impact on our results.

III. b. Independent Variables

There are several independent variables that are common across all or most of our regressions. The first is $MATCH_{ift}$, which is an indicator variable equal to one if analyst *i* has the same first name (as defined above) as the CEO of covered firm *f* in year *t*, and is equal to zero, otherwise.[8] Of the 193,698 forecasts in our sample, 1.4% of them were issued by matched analysts (untabulated). Our next independent variable is $COMMON_i$, which is an indicator variable equal to one if analyst *i*'s first name is classified as common (as defined below), and is equal to zero, otherwise. We control for name commonness in our analysis because matched names are also likely to be common names. Research in psychology has found that individuals with common names are considered more attractive and are more liked than those with uncommon names (Crisp, Apostal, and Luessenheide, 1984, West and Shults, 1976, Lawson, 1971, Mehrabian, 1992, 1997, and Christopher, 1998). Being more liked, they may get preferential treatment in hiring (see Cotton, O'Neill, and Griffin, 2008). If so, this would suggest that the qualifications of analysts hired with common names might not be as strong as the qualifications of those hired with uncommon names, which could lead to lower forecast accuracy for the former set of analysts.

To determine whether a first name is common, we collect data from the Social Security Administration on the distribution of first names of those born in each decade from the 1950s through the 1980s.[9] We classify names with a frequency of 2% or higher as more common (or simply "common") names and those with a frequency of less than 2% as less common (alternatively, "uncommon") names. We do this separately for male and female names. In

[8] For a firm that experiences CEO turnover in year *t*, the CEO that we use for our analysis is the person who served in that position for the longest period of time during the year.
[9] See https://www.ssa.gov/oact/babynames/decades/index.html.

determining these frequencies, we do not aggregate spelling variants of a given name.[10] To determine whether an analyst has a more common or less common name, we assume that each analyst is 30 years old when first appearing on *IBES* and then check whether the analyst's first name was above or below 2% in frequency 30 years prior to the analyst's first appearance on *IBES*. We recognize that the choices of a cutoff frequency of 2% and a 30-year look-back period are arbitrary. To ensure the robustness of our results, we alternatively (1) use a cutoff frequency of 1.5% and (2) rerun our analysis, using a definition of commonness based on the frequency of analyst first names in the *IBES* database (see Section IV. a).

Our next independent variable, $HORIZON_{ift}$, is the number of calendar days between the last forecast of analyst *i* for covered firm *f* during year *t* and the date of the release of the firm's earnings for the year. The mean number of days between the last forecast and the earnings release is 108, ranging from a low of one day to a high of 364 days. The variable EXP_{it} is the number of years of experience that analyst *i* has had as an analyst as of year *t*, starting from the year in which the analyst first appears on *IBES*. Analysts in our sample have, on average, 11 years of experience, with a maximum of 37 years. The variable $\#FIRMS_{it}$ is equal to the number of firms for which analyst *i* has issued at least one forecast of annual earnings during year *t*. On average, the number of firms covered by a single analyst is 17.4, with a low of 1 and a high of 107.[11] All of the definitions of these dependent and independent variables appear in the Appendix. There are other independent variables that are specific to particular regressions; they are defined as they are introduced.

[10] There is a fair degree of stability in the common first names of males over the decades; the five most common first names among males in the 1950s remain on the top-10 lists for all the decades. This is not true for the common first names among females; no name appears on the top-10 lists for all four decades (untabulated).

[11] Such a large number of covered firms likely reflects cases where a top analyst at a firm puts his or her name on the reports that were actually written by a number of different associates in the firm.

IV. Analyst forecast accuracy

IV. a. Central results

We begin by estimating the following regression:

$$\begin{aligned} RFA_{ift} = \alpha_0 + \alpha_1 MATCH_{ift} + \alpha_2 HORIZON_{ift} + \alpha_3 EXP_{it} + \alpha_4 \# FIRMS_{it} + \alpha_5 COMMON_i \\ + \sum \beta_t * Year\ dummy\ variables + \sum \gamma_f * Firm\ dummy\ variables + \varepsilon_{ift} \end{aligned} \tag{2}$$

where all variables have been defined in the previous section. In this regression we include year and firm fixed effects. The results of estimating this regression are reported in column 1 of Table IV. The coefficient on *MATCH* is significantly negative. As conjectured, the relative accuracy of an analyst's forecast is significantly greater when that analyst shares a first name with the firm's CEO than when they have different first names. The magnitude of the coefficient, -0.049, means that having the same name as a CEO is associated with a 4.9 percentage point increase in an analyst's relative accuracy as compared to having a different name. The magnitude of the matched analysts' relative accuracy advantage is similar to that of local analysts over non-local analysts, as documented in Bae et al. (2007).

As expected, the coefficient on *HORIZON* is significantly positive, indicating that an analyst's forecast is more accurate the closer in time it is issued relative to the date of the earnings announcement. Also as expected, the coefficient on *EXP* is significantly negative, meaning that an analyst's forecast accuracy increases with experience. The coefficient on *#FIRMS* is significantly negative, indicating that relative forecast accuracy increases with the number of firms that an analyst covers. This result stands in contrast to some other studies in the literature, which have found a negative relation between forecast accuracy and the number of covered firms. It should be noted, though, that the magnitude of the coefficient is small, suggesting that the economic effect of the number of firms covered on relative forecast accuracy is marginal. The coefficient on the indicator variable *COMMON* is significantly positive; the relative accuracy of the forecasts of analysts with more common names is lower than that of analysts whose names are less common. Consistent with prior research showing that individuals with common names have an advantage in the hiring process, this result suggests that analysts with common names who are hired are less qualified, in general, than are those with uncommon names, leading to lower forecast accuracy for the common-named analysts.

To control for differences across analysts in the timing of their last forecast of the year, we re-estimate regression (2) using only those last forecasts that are issued within 120 days of the earnings announcement. This restriction causes us to lose about one-third of our observations. (Using an even shorter interval of 90 days would have caused us to lose about three-quarters of our original sample.) In untabulated results, we find that the relative accuracy of the forecasts of the matched analysts remains significantly higher, on average, than that of the unmatched analysts when we narrow the variation in forecast horizons across analysts in this manner (p-value < 5%).

To validate that our results are not driven by analyst-specific effects, we examine whether, for a given analyst-year, the forecasts for firms in which the analyst's first name matches that of the CEO are more accurate than those for which there is no match. To do so, we re-estimate regression (2), replacing the year fixed effect in the regression with an analyst-year fixed effect. We drop the independent variables for analyst experience and number of firms covered, along with the common name indicator variable, since they are constant for each analyst-year. The results of re-estimating this regression are reported in column 2 of Table IV. Consistent with our prior results, the relative accuracy of an analyst's forecasts is greater for the subset of firms where the analyst's first name matches that of the CEO than for the subset of firms where it does not.

CEO turnover provides an additional means by which to test whether a matched name is associated with higher relative forecast accuracy. Given our prior finding that matched analysts provide more accurate forecasts, we expect that following a change in CEO, previously matched analysts who are now unmatched will exhibit a decrease in forecast accuracy relative to unmatched analysts who remain unmatched. Similarly, those previously unmatched analysts who are now matched should exhibit an increase in forecast accuracy relative to those who remain unmatched.

To implement this test, we use the *Execucomp* database, which provides information on CEO turnover for all S&P 1500 firms. For our test we include all forecasts for the year prior to and for the year subsequent to the CEO change. We do not include the forecasts for the year of the change, when both the prior and new CEO were in place. The observations used for regression (3), below, are comprised of the forecasts of two sets of analysts: one set consisting of all analysts who had the same name as the CEO before the turnover and a different name afterwards, and the other consisting of all analysts who had a name different from that of the CEO both before and after the turnover. The observations used for regression (4), below, are comprised of the forecasts of two sets of analysts: one set consisting of all analysts who had a name different from that of the CEO before the turnover and the same name afterwards, and the other consisting of all analysts who had a name different from that of the CEO both before and after the turnover. These two regressions take the following forms:

$$\begin{aligned} RFA_{im} = \alpha_0 + \alpha_1 MATCH\_UNMATCH_{im} * POST_{im} + \alpha_2 MATCH\_UNMATCH_{im} \\ + \alpha_3 POST_m + Controls + \varepsilon_{im} \end{aligned} \tag{3}$$

and

$$\begin{aligned} RFA_{im} = \alpha_0 + \alpha_1 UNMATCH\_MATCH_{im} * POST_{im} + \alpha_2 UNMATCH\_MATCH_{im} \\ + \alpha_3 POST_m + Controls + \varepsilon_{im} \end{aligned} \tag{4}$$

where the subscript i denotes analyst i and the subscript m denotes CEO turnover event m; $MATCH\_UNMATCH_{im}$ equals one if analyst i had the same name as the CEO before turnover event m and a different name afterwards, and zero, otherwise; $UNMATCH\_MATCH_{im}$ equals one if analyst i had a name different from that of the CEO before turnover event m and the same name afterwards, and zero, otherwise; and $POST_m$ equals one for an analyst forecast that is made in the year after CEO turnover event m and is equal to zero, otherwise. All control variables are as in the previous regressions.

The coefficient on $MATCH\_UNMATCH$ in regression (3) captures the pre-turnover year difference between the relative forecast accuracy of a matched analyst and an unmatched analyst. The coefficient on *POST* captures the effect of CEO turnover on the relative forecast accuracy of those analysts who were unmatched before the turnover and remain unmatched after the turnover. The coefficient on $MATCH\_UNMATCH * POST$ captures the additional amount by which relative forecast accuracy changes for analysts who were matched pre-turnover, but became unmatched post-turnover. The coefficients on $UNMATCH\_MATCH$ and $UNMATCH\_MATCH * POST$ are interpreted analogously.

For these regressions, we include only those analysts who are following the firm both before and after the CEO turnover. We also make a small change in the definition of relative forecast accuracy. Since we are now analyzing how relative forecast accuracy changes *over time*, if we had continued to use the previous definition of *RFA*, then relative forecast accuracy would change with CEO turnover just because the denominator (average absolute forecast error) changes over time. To abstract from this effect, we use as the denominator of *RFA* the average of the absolute forecast errors over the three years around the CEO turnover.

As reported in Table V, columns one and two, the coefficient on *POST* is significantly positive, meaning that relative forecast accuracy, over both matched and unmatched analysts, decreases in the year after a CEO turnover. This reflects the increased difficulty of forecasting earnings when a new CEO takes over. The coefficient on $MATCH\_UNMATCH * POST$ in column 1 is positive and significant. While relative forecast accuracy generally decreases after a change in CEO, it decreases more for an analyst whose first name matched that of the previous CEO, but does not match that of the current CEO. The coefficient on $UNMATCH\_MATCH * POST$ in column 2 is negative and significant. Relative forecast accuracy after a change in CEO decreases less for an analyst whose first name did not match that of the previous CEO, but matches that of the new CEO. These findings provide additional evidence that matched analysts issue more accurate forecasts than do unmatched analysts.

IV. b. Controlling for gender and ethnicity matches

To the extent that our matches are comprised of analysts and CEOs who are both female or who come from the same ethnic background, it is possible that the forecasting superiority of matched analysts is due, at least in part, to gender and/or ethnic ties. To rule out the possibility that these ties are driving our results, we add a control for these matches in our regression. It is straightforward to control for gender matches since (1) we collected gender information for each analyst at the same time that we searched for the analyst's first name and (2) the *Execucomp* database provides the gender of each CEO. To control for ethnicity matches, we follow Gompers, Mukharlyamov, and Xuan (2016) and classify each analyst and CEO into one of five ethnic groups: East Asian, Indian, Jewish, Middle Eastern, and all others.[12] We assign each

[12] East Asian includes Chinese, Japanese, and Korean last names. Middle Eastern includes Arabic last names, but not Jewish last names.

analyst and CEO to an ethnic group by checking his or her last name against the list of ethnic last names for each ethnic group as provided by FamilyEducation.com.[13] Table VI, panel A provides information on the distribution of gender and ethnicity of the analysts and CEOs in our sample. As reported in the table, 11.41% (2.67%) of the analysts (CEOs) are female. Of the analysts (CEOs) in our sample, 4.11% (1.78%) are East Asian, 2.70% (0.84%) are Indian, 9.35% (8.15%) are Jewish, and 0.08% (0.07%) are Middle Eastern, non-Jewish.

We estimate the following regression:

$$\begin{aligned} RFA_{ift} = {} & \alpha_0 + \alpha_1 MATCH_{ift} + \alpha_2 SAME\_ETHNICITY_{ift} + \alpha_3 BOTH\_FEMALE_{ift} \\ & + \alpha_4 HORIZON_{ift} + \alpha_5 EXP_{it} + \alpha_6 \#FIRMS_{it} + \alpha_7 COMMON_i \\ & + \sum \beta_t * Year\ dummy\ variables + \sum \gamma_f * Firm\ dummy\ variables + \varepsilon_{ift} \end{aligned} \tag{5}$$

where $SAME\_ETHNICITY_{ift}$ is an indicator variable equal to one if analyst *i* has the same ethnicity as the CEO of covered firm *f* in year *t*, and is equal to zero, otherwise, and $BOTH\_FEMALE_{ift}$ is an indicator variable equal to one if analyst *i* and the CEO of covered firm *f* in year *t* are both female, and is equal to zero, otherwise.

As reported in Table VI, panel B, the coefficient on *MATCH* is negative and significant. It is identical in magnitude to that reported in Table IV, column 1. Controlling for gender and ethnicity matches, analysts who share a first name with the CEO of a covered firm continue to demonstrate superior forecasting accuracy over those whose first name does not match that of the CEO. Interestingly, the coefficient on $BOTH\_FEMALE$ is significantly negative, suggesting that the forecasts of female analysts are more accurate for firms where the CEO is also a woman. In contrast, the coefficient on $SAME\_ETHNICITY$ is insignificantly different from zero. In an untabulated analysis, we exclude all analyst-CEO pairs where both are women, or where both are

[13] See https://www.familyeducation.com/baby-names/browse-origin/surname.

East Asian, Indian, Jewish, or Middle Eastern, non-Jewish and re-run regression (5). The coefficient on *MATCH* remains significantly negative.

IV. c. Common name matches vs. uncommon name matches

We might expect that there would be variation in the forecast superiority of matched analysts depending on the commonness of an analyst's name. If an analyst and CEO have a very common name, then they are likely used to meeting people with the same name. As a consequence, the enhanced affinity between the two due to the sharing of a first name may be attenuated. (To be clear, this argument is distinct from the one used to justify the inclusion of name commonness as an independent variable in all of our regressions.) If so, we would expect that the matched name effect would be smaller the more common the name. To test this, we estimate the following regression:

$$\begin{aligned} RFA_{ift} = {} & \alpha_0 + \alpha_1 UNCOMMON\_MATCH_{ift} + \alpha_2 COMMON\_MATCH_{ift} \\ & + \alpha_3 COMMON_i + \alpha_4 HORIZON_{ift} + \alpha_5 EXP_{it} + \alpha_6 \#FIRMS_{it} \\ & + \sum \beta_t * Year\ dummy\ variables + \sum \gamma_f * Firm\ dummy\ variables + \varepsilon_{ift} \end{aligned} \tag{6}$$

where $UNCOMMON\_MATCH_{ift}$ equals 1 if analyst *i* following firm *f* in year *t* has the same name as the firm's CEO and that first name is classified as uncommon, and is equal to zero, otherwise; $COMMON\_MATCH_{ift}$ equals 1 if analyst *i* following firm *f* in year *t* has the same name as the firm's CEO and that first name is classified as common, and is equal to zero, otherwise. (Recall that a first name is classified as common if it has a frequency of 2% or more during the decade that is 30 years prior to the analyst's first appearance on *IBES*, according to Social Security Administration records.) Of the 592 matched analysts in our sample, 340 of them have a common name.

In this regression the coefficient on *COMMON* captures the incremental accuracy of an unmatched analyst with a common name over that of one with an uncommon name. The coefficient on $UNCOMMON\_MATCH$ ($COMMON\_MATCH$) is the incremental accuracy of a matched analyst over that of an unmatched analyst, both of whom have an uncommon (a common) name.

Table VII, column 1 reports the results of estimating regression (6). The negative and significant coefficient on $UNCOMMON\_MATCH$ indicates that the relative forecast accuracy of a matched analyst with an uncommon first name is significantly higher, on average, than that of an unmatched analyst who also has an uncommon name. The difference is an economically large 8.2 percentage points. Given that the coefficient on *COMMON* is positive, it is also significantly higher, on average, than that of an unmatched analyst with a common name. In contrast, the coefficient on $COMMON\_MATCH$ is insignificantly different from zero; a matched analyst with a more common first name has a forecast accuracy that is not significantly different from that of an unmatched analyst who also has a common first name. In column 2 of Table VII we report the results of re-estimating regression (6), defining a name as common if it has a frequency of greater than 1.5%. (Under this definition, 402 of our matched analysts have common names.) The results are little changed.

As a robustness check on the source used to determine the commonness of first names, we again re-estimate regression (6), this time using as a measure of commonness the frequency with which names appear on the *IBES* database. We compute the distribution of analyst first names in the *IBES* database and define a common name as one that is shared by more than 50 analysts (there are 21 such names). Under this definition, 490 of our matched analysts have common names.

The results of re-estimating regression (6) using this alternative definition of name commonness appear in column 3 of Table VII. The coefficient on $UNCOMMON\_MATCH$ remains negative and becomes much larger in magnitude than that when the Social Security Administration data is used to classify names. Using the *IBES* classification, conditional on having an uncommon name, sharing a first name with the CEO is associated with a 14.0 percentage point increase in an analyst's forecast accuracy, on average, relative to that of an analyst not sharing a first name. The coefficient on $COMMON\_MATCH$ is now significantly negative as well, although much smaller in magnitude than that on $UNCOMMON\_MATCH$.

IV. d. The effect of information asymmetry

An implication of matched analysts being more likely to receive private information is that their forecasting superiority should be greater in situations where there is higher information asymmetry, ex-ante. To test this, we partition our sample into those firm-years where management had issued an earnings forecast and those where no management earnings forecast was issued. With information asymmetry arguably higher in the absence of a managerial earnings forecast, we expect that the effect of a first name match on relative forecast accuracy will be higher in firm-years without a managerial forecast. Table VIII, panel A reports the results of re-estimating regression (2) for the subset of firm-years without management forecasts (column 1) and for the subset of firm-years with management forecasts (column 2). For the subsample with no management forecasts, the coefficient on *MATCH* is significantly negative and an economically large -7.3 percentage points. In contrast, it is insignificantly different from zero for the subset with management forecasts. These findings are in line with our expectations.

Within the subset of firm-years without management forecasts, we further partition the firms into deciles according to market capitalization at the end of June of each calendar year, using NYSE breakpoints (Fama and French, 1993).[14] Since all of our firms are in the S&P 1500, firm size is skewed to the right; the median size decile of our firms is 8. We partition our sample of firms into three groups: the first group consists of all firms in the lowest 3 deciles of market cap; the second consists of all firms in deciles 4 through 7; and the third group consists of all firms in deciles 8 through 10. We expect there to be more information asymmetry among the smaller firms and, therefore, a greater impact of first name match on relative forecast accuracy. Table VIII, panel B reports the results of re-estimating regression (2) for each of these three groups. The coefficient on *MATCH* across the groups is again consistent with our expectation: it is significantly negative for the smallest and middle groups of firms, and insignificantly different from zero for the largest firms.

We alternatively partition the subset of firm-years without management forecasts according to firm book-to-market ratio at the end of June of each calendar year, again using NYSE size breakpoints and measuring book value at the end of the previous fiscal year-end. We form three groups: the first group consists of the firms in the lowest 3 deciles of book-to-market ratio; the second consists of the firms in deciles 4 through 7; and the third group consists of the firms in decile 8 through 10. We expect that information asymmetry would be the highest among the firms in the lowest deciles (the growth firms) and, therefore, that the effect of a first name match on forecast accuracy would be the greatest. Table VIII, panel C reports the results of re-estimating regression (2) for each of these groups. Somewhat surprisingly, there does not appear to be any meaningful difference among the coefficients on *MATCH* across the three groups. Further analysis of these groups reveals that the firms in the lowest book-to-market

[14] These break points are provided by Ken French on his website: https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/Data_Library/det_me_breakpoints.html.

group are larger on average (average market cap of 7.66, untabulated) than those in the other two groups (average market cap of 6.91, untabulated). These size differences reduce the ability of our tests to detect any relation that might exist between firms' book-to-market ratios and the coefficient on *MATCH* in these regressions.

IV. e. Additional tests

In this subsection we conduct two additional tests. The first test examines whether matched analysts' relative forecast superiority changes the longer the match has been in effect. We expect a reduction over time. There are two reasons for this. The first is that as analysts gain experience covering a firm, their expertise in forecasting earnings increases, thereby reducing the importance of sharing a first name with the CEO. The second is that, as time passes, the affinity between an analyst and CEO due solely to their sharing of a first name is likely to diminish (consistent with the results of List, 2003, in a different setting).

To test this conjecture, we modify regression (2) as follows:

$$\begin{aligned} RFA_{ift} &= \alpha_0 + \alpha_1 MATCH_{ift} * CEO\_EXP_{ift} + \alpha_2 MATCH_{ift} + \alpha_3 CEO\_EXP_{ift} \\ &+ \alpha_4 COMMON_i + \alpha_5 HORIZON_{ift} + \alpha_6 EXP_{it} + \alpha_7 \# FIRMS_{it} \\ &+ \sum \beta_t * Year\ dummy\ variables + \sum \gamma_f * Firm\ dummy\ variables + \varepsilon_{ift} \end{aligned} \tag{7}$$

where CEO_EXP_{ift} is equal to the number of years that analyst *i* has been covering firm *f* under the same CEO as of year *t*. Included in regression (7) is an interaction term between *MATCH* and CEO_EXP. For this analysis, we exclude the year of CEO turnover (when both the previous and new CEO held the position for a portion of the year). Table IX reports the results of estimating this regression. The negative and significant coefficient on CEO_EXP is not surprising. It means that the relative accuracy of an analyst's forecast increases, on average, the longer the analyst has been following the firm (as long as the CEO has not changed). The positive and significant coefficient on the interaction term between *MATCH* and CEO_EXP indicates that the accuracy advantage a matched analyst has over an unmatched analyst with the same experience decreases the longer the analysts have been covering the firm.

Our second test in this subsection addresses the question of whether the implementation of Reg FD affected the forecasting superiority of matched analysts. If it had been fully effective at halting the selective disclosure of private information, we would have expected their forecasting superiority to disappear after its implementation in October 2000. There is evidence, though, that the selective disclosure of private information has continued post-Reg FD. One reason for this is that the regulation does not prohibit management from meeting privately with an individual analyst to discuss the firm's business. Such meetings allow an analyst to complete a 'mosaic' of information that could be material to the analyst. Another reason is that these meetings allow the analyst to observe management's nonverbal communication, which may yield additional inferences about the firm's prospects.

To examine whether the implementation of Reg FD had any impact on the forecasting superiority of matched analysts, we divide our sample into forecasts made for fiscal years beginning in or after October, 2000 and those made for fiscal years beginning prior to October, 2000. Table X, columns 1 and 2 report the results of estimating regression (2) for the earlier and later time periods, respectively. The coefficient on *MATCH* is significantly negative in both periods. While the coefficient is greater in magnitude prior to the implementation of Reg FD than after, the difference is not significant. It appears that Reg FD has not had a meaningful impact on the benefit of a matched name to relative forecast accuracy. This result also provides additional evidence that Reg FD has not fully eliminated the selective sharing of private information between CEOs and analysts.

V. Summary and conclusions

In this paper we examine whether the sharing of a first name between a security analyst and the CEO of a covered firm affects the accuracy of the analyst's forecasts. We conjecture that the greater affinity between an analyst and CEO that arises due to the sharing of a first name will lead to a greater willingness on the part of the CEO to share private information with the analyst and that this, in turn, will manifest itself in increased forecast accuracy.

Consistent with our conjecture, we find that the accuracy of the forecasts issued by those analysts who share a first name with a covered firm's CEO is higher than those of analysts who do not share a first name. We find matched analyst forecast superiority to be concentrated among those analysts with less common first names, perhaps because the salience of sharing a first name is lower for analysts with more common names. Supportive of our results, we find that at the time of CEO turnover the forecast accuracy of previously matched analysts who are now unmatched falls relative to that of analysts who were unmatched both pre- and post-turnover, while the forecast accuracy of analysts who become matched increases relative to that of those who remain unmatched. Consistent with the notion that increased forecast accuracy is due to the sharing of private information between a CEO and a matched analyst, we also find that forecasting superiority is greater in situations where there is greater information asymmetry.

Analysts consider many factors when deciding whether to initiate coverage of a firm. Obvious ones are the extent to which coverage will lead to future investment banking business and the amount of trading commissions that it will generate. Our research suggests a less obvious, but still important, factor – the CEO's first name. Choosing a firm with a CEO who shares the analyst's first name could lead to an informational advantage over the other analysts covering that firm.

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Table I
Sample Construction

This table provides details on the construction of our final sample of annual forecasts.

	Number of Observations
IBES annual earnings per share forecasts for firms in the Compustat Execucomp database (1992 to 2018)	1,937,627
Less:	
Forecasts with analyst first name not found	(989,791)
Forecasts missing announcement date	(5,136)
Forecasts issued either after earnings announcement or more than a year before earnings announcement	(37,493)
Forecasts for which earnings announcement date is more than six months after fiscal year end	(459)
Number of annual forecasts after deletions	904,748
Less:	
Forecasts other than the last of the year issued by an analyst	(675,521)
Forecasts for firm-years with fewer than five analysts	(35,529)
Final sample	193,698

Table II
Descriptive Statistics on Matches

Panel A provides information on the number of analyst-CEO first name matches in our sample. Spelling variants of the same name are considered to be matches. Panel B lists the matched analyst names in our sample, in descending order of frequency. In determining this ranking, all spelling variants of the same name are combined.

Panel A. Numbers of matched analysts and matched CEOs

	Number
Unique analysts	4,890
Unique analysts with at least one match	592
Unique CEOs	4,380
Unique CEOs with at least one match	677
Unique analyst-CEO pairs	69,514
Unique analyst-CEO pairs with same first name	921

Panel B. List of Matched Analyst Names

First Name	Frequency	First Name	Frequency	First Name	Frequency	First Name	Frequency
John	82	Jeffery	7	Edward	2	Howard	1
David	71	Charles	6	Elizabeth	2	Ian	1
Michael	64	Eric	6	Glenn	2	Ivan	1
James	49	Kevin	6	Jason	2	Jay	1
Robert	45	Alan	5	Keith	2	Joe	1
Steven	34	Peter	5	Larry	2	Joel	1
William	31	Anthony	4	Patrick	2	Jonathan	1
Mark	24	Douglas	4	Ronald	2	Laura	1
Richard	21	Gregory	4	Timothy	2	Leo	1
Thomas	14	Christopher	3	Jack	1	Martin	1
Paul	11	Frank	3	Albert	1	Philip	1
Scott	11	Matthew	3	Benjamin	1	Roger	1
Brian	9	George	3	Bruce	1	Samuel	1
Gary	8	Brad	2	Donald	1	Todd	1
Andrew	7	Craig	2	Greg	1		
Joseph	7	Daniel	2	Henry	1	Total	592

Table III
Summary Statistics

This table provides descriptive statistics for our final sample of analyst forecasts of annual earnings for the years 1992-2018. Each observation is an analyst-firm-year. All variables are defined in the Appendix.

Variable	Obs	Mean	SD	min	p25	p50	p75	max
Relative forecast accuracy (RFA)	193,698	0.00	1.05	-1.00	-0.63	-0.22	0.22	18.85
HORIZON	193,698	108.36	83.88	1.00	48.00	96.00	118.00	364.00
EXP	193,698	11.26	8.19	0.00	4.00	10.00	17.00	37.00
#FIRMS	193,698	17.43	9.55	1.00	12.00	16.00	21.00	107.00

Table IV
First Name Match and Forecast Accuracy

This table reports the estimated coefficients from regressions of *Relative forecast accuracy* (*RFA*) on *MATCH* and other independent variables. The regression reported in column 1 uses year and firm fixed effects. The regression reported in column 2 uses firm and analyst-year fixed effects. Each coefficient's *t*-statistic appears directly below the coefficient estimate. All variables are defined in the Appendix. The dependent variable is winsorized at the 99th percentile. Robust standard errors are clustered at the analyst-CEO pair level. Statistical significance at the 1%, 5%, and 10% levels are denoted by ***, **, and *, respectively.

Independent variables	(1) *RFA*	(2) *RFA*
MATCH	-0.049***	-0.042**
	(-2.80)	(-2.36)
HORIZON	0.0048***	0.0036***
	(134.47)	(77.70)
EXP	-0.0029***	
	(-9.73)	
#FIRMS	-0.00058**	
	(-2.06)	
COMMON	0.019***	
	(3.19)	
Constant	-0.47***	-0.41***
	(-17.50)	(-81.79)
Observations	193,698	188,152
R-squared	0.174	0.366
Year FE	YES	NO
Firm FE	YES	YES
Analyst-Year FE	NO	YES

Table V
Forecast Accuracy Around CEO Turnover

This table reports the estimated coefficients from regressions with *Relative forecast accuracy* (*RFA*) as the dependent variable, before and after CEO turnover. Each coefficient's *t*-statistic appears directly below the coefficient estimate. *MATCH_UNMATCH* is an indicator variable equal to one if the analyst has the same first name as the CEO in the year before turnover but does not share the CEO's first name in the year after turnover, and is equal to zero, otherwise. The independent variable *UNMATCH_MATCH* is an indicator variable equal to one if the analyst does not have the same first name as the CEO before turnover but shares the first name after turnover, and is equal to zero, otherwise. *POST* is an indicator variable equal to one if the analyst's forecast is made in the year after CEO turnover, and is equal to zero, otherwise. All other variables are defined in the Appendix. Forecasts made in the turnover year are not included in the regressions. The dependent variable is winsorized at the 99th percentile. Robust standard errors are clustered at the analyst-CEO pair level. Statistical significance at the 1%, 5%, and 10% levels are denoted by ***, **, and *, respectively.

Independent variables	(1) *RFA*	(2) *RFA*
*MATCH_UNMATCH * POST*	0.19*	
	(1.80)	
MATCH_UNMATCH	-0.16**	
	(-2.23)	
*UNMATCH_MATCH * POST*		-0.23*
		(-1.84)
UNMATCH_MATCH		0.022
		(0.22)
POST	0.14***	0.15***
	(8.97)	(9.11)
HORIZON	0.0049***	0.0048***
	(30.82)	(30.57)
EXP	-0.0028***	-0.0031***
	(-2.68)	(-2.93)
#FIRMS	-0.0011	-0.00084
	(-1.08)	(-0.86)
COMMON	0.0043	0.0023
	(0.21)	(0.11)
Constant	-0.63***	-0.63***
	(-4.28)	(-4.30)
Observations	18,862	18,842
R-squared	0.175	0.173
Year FE	YES	YES
Firm FE	YES	YES

Table VI
Controlling for Gender and Ethnicity Matches

This table reports the results of controlling for gender and ethnicity matches. Panel A provides information on the distribution of gender and ethnicity of the analysts and CEOs in our sample. Panel B presents the estimated coefficients from a regression of *Relative forecast accuracy* (*RFA*) on *MATCH* and other independent variables, including controls for gender and ethnicity matches. Each coefficient's *t*-statistic appears directly below the coefficient estimate. $SAME\_ETHNICITY_{ift}$ is an indicator variable equal to one if analyst *i* has the same ethnicity as the CEO of firm *f* in year *t*, and is equal to zero, otherwise. Our sample is divided into five ethnic groups: East Asian, Indian, Jewish, Middle Eastern (non-Jewish) and all others. $BOTH\_FEMALE_{ift}$ is an indicator variable equal to one if analyst *i* and the CEO of firm *f* in year *t* are both female, and is equal to zero, otherwise. All other variables are defined in the Appendix. The dependent variable is winsorized at the 99th percentile. Robust standard errors are clustered at the analyst-CEO pair level. Statistical significance at the 1%, 5%, and 10% levels are denoted by ***, **, and *, respectively.

Panel A. Distribution of analysts' and CEOs' ethnicity and gender

	Analyst		CEO	
Gender/Ethnicity	Number	Percent	Number	Percent
Female	558	11.41%	117	2.67%
East Asian	201	4.11%	78	1.78%
Indian	132	2.70%	37	0.84%
Jewish	457	9.35%	357	8.15%
Middle Eastern (non-Jewish)	4	0.08%	3	0.07%
Total Number of Analysts/CEOs	4,890		4380	

Panel B. Regression results, controlling for gender and ethnicity matches

Independent variables	*RFA*
MATCH	-0.049***
	(-2.77)
SAME_ETHNICITY	-0.025
	(-1.18)
BOTH_FEMALE	-0.089**
	(-2.26)
HORIZON	0.0048***
	(134.48)
EXP	-0.0029***
	(-9.73)
#FIRMS	-0.00058**
	(-2.08)
COMMON	0.019***
	(3.15)
Constant	-0.47***
	(-17.49)
Observations	193,698
R-squared	0.174
Year FE	YES
Firm FE	YES

Table VII
Common Name Match, Uncommon Name Match, and Forecast Accuracy

This table reports the estimated coefficients from regressions of *Relative forecast accuracy* (*RFA*) on *UNCOMMON_MATCH, COMMON_MATCH*, and other independent variables. Each coefficient's *t*-statistic appears directly below the coefficient estimate. $UNCOMMON\_MATCH_{ift}$ $(COMMON\_MATCH_{ift})$ is an indicator variable equal to one if analyst *i* covering firm *f* in year *t* has the same first name as the firm's CEO and that name is an uncommon (common) one, and is equal to zero, otherwise. All other variables are defined in the Appendix. In column 1 (column 2), a common name is defined as having a frequency of at least 2% (1.5%) during the decade that is 30 years prior to the analyst's first appearance on *IBES*, according to Social Security Administration records. In column 3, a common name is defined as one that is shared by 50 or more analysts appearing in *IBES*. In determining these frequencies, we do not aggregate spelling variants of a given name. The dependent variable is winsorized at the 99th percentile. Robust standard errors are clustered at the analyst-CEO pair level. Statistical significance at the 1%, 5%, and 10% levels are denoted by ***, **, and *, respectively.

	Social Security 2%	Social Security 1.5%	IBES definition
	(1)	(2)	(3)
Independent variables	*RFA*	*RFA*	*RFA*
UNCOMMON_MATCH	-0.082***	-0.10***	-0.14***
	(-2.90)	(-3.30)	(-3.03)
COMMON_MATCH	-0.032	-0.029	-0.038**
	(-1.43)	(-1.36)	(-2.03)
HORIZON	0.0048***	0.0048***	0.0048***
	(134.47)	(134.46)	(134.53)
EXP	-0.0029***	-0.0029***	-0.0029***
	(-9.73)	(-9.62)	(-9.55)
#FIRMS	-0.00058**	-0.00058**	-0.00057**
	(-2.07)	(-2.08)	(-2.03)
COMMON	0.018***	0.014***	0.021***
	(3.00)	(2.68)	(4.30)
Constant	-0.47***	-0.47***	-0.48***
	(-17.50)	(-17.49)	(-17.62)
Observations	193,698	193,698	193,698
R-squared	0.174	0.174	0.174
Year FE	YES	YES	YES
Firm FE	YES	YES	YES

Table VIII
Information Asymmetry and Forecast Accuracy

This table reports the estimated coefficients from regressions of *Relative forecast accuracy* (*RFA*) on *MATCH* and other independent variables. Each coefficient's *t*-statistic appears directly below the coefficient estimate. In panel A, the first column reports the results for the subset of analyst forecasts made in firm-years with no management forecast of annual earnings per share. The second column reports the results for the subset of analyst forecasts made in firm-years with at least one management forecast of annual earnings per share. For the subset of firm-years without management forecasts, panel B, columns 1, 2, and 3, report the results for the subset of analyst forecasts made by firms in the bottom 30%, middle 40%, and top 30%, respectively, of market capitalization, using NYSE breakpoints. For the subset of firm-years without management forecasts, panel C, columns 1, 2, and 3, report the results for the subset of analyst forecasts made by firms in the bottom 30%, middle 40%, and top 30%, respectively, of book-to-market ratio, using NYSE breakpoints. All variables are defined in the Appendix. The dependent variable is winsorized at the 99th percentile. Robust standard errors are clustered at the analyst-CEO pair level. Statistical significance at the 1%, 5%, and 10% levels are denoted by ***, **, and *, respectively.

Panel A. Sample split according to whether a management forecast is released

	No management forecast released	Management forecast released
	(1)	(2)
Independent variables	*RFA*	*RFA*
MATCH	-0.073***	-0.018
	(-3.09)	(-0.68)
HORIZON	0.0048***	0.0049***
	(103.74)	(86.28)
EXP	-0.0027***	-0.0032***
	(-6.43)	(-7.48)
#FIRMS	0.00013	-0.0019***
	(0.37)	(-3.98)
COMMON	0.020***	0.018**
	(2.59)	(2.09)
Constant	-0.47***	-0.59***
	(-16.46)	(-4.22)
Observations	112,236	81,462
R-squared	0.178	0.174
Year FE	YES	YES
Firm FE	YES	YES

Panel B. Sample of firm-years with no management forecast, split according to firm size

	Firm size decile		
	Bottom 30%	Middle 40%	Top 30%
	(1)	(2)	(3)
Independent variables	*RFA*	*RFA*	*RFA*
MATCH	-0.14**	-0.11***	-0.046
	(-2.31)	(-2.87)	(-1.44)
HORIZON	0.0048***	0.0047***	0.0050***
	(34.98)	(61.84)	(76.33)
EXP	0.00093	-0.00040	-0.0050***
	(0.83)	(-0.58)	(-8.44)
#FIRMS	-0.0021*	-0.0013**	0.0010**
	(-1.80)	(-2.17)	(2.27)
COMMON	0.0069	0.033**	0.018*
	(0.29)	(2.34)	(1.79)
Constant	-0.62	-0.36***	-0.48***
	(-1.53)	(-3.11)	(-15.64)
Observations	12,193	39,738	60,305
R-squared	0.186	0.173	0.184
Year FE	YES	YES	YES
Firm FE	YES	YES	YES

Panel C. Sample of firm-years with no management forecast, split according to book-to-market ratio

	Book-to-market decile		
	Bottom 30%	Middle 40%	Top 30%
	(1)	(2)	(3)
Independent variables	*RFA*	*RFA*	*RFA*
MATCH	-0.071*	-0.074*	-0.077*
	(-1.86)	(-1.80)	(-1.72)
HORIZON	0.0051***	0.0050***	0.0046***
	(64.53)	(64.09)	(51.53)
EXP	-0.0023***	-0.0035***	-0.0025***
	(-3.07)	(-5.06)	(-3.71)
#FIRMS	0.000084	0.0014**	-0.0016**
	(0.14)	(2.56)	(-2.55)
COMMON	0.037***	0.026**	-0.0069
	(2.91)	(2.07)	(-0.50)
Constant	-0.52***	-0.52***	-0.33***
	(-10.76)	(-11.33)	(-4.06)
Observations	42,484	40,157	29,595
R-squared	0.187	0.188	0.169
Year FE	YES	YES	YES
Firm FE	YES	YES	YES

Table IX
Analyst Experience and Forecast Accuracy

This table reports the estimated coefficients from a regression of *Relative forecast accuracy* (*RFA*) on *MATCH*, *CEO_EXP*, the interaction term between *MATCH* and *CEO_EXP*, and other independent variables. Each coefficient's *t*-statistic appears directly below the coefficient estimate. CEO_EXP_{ift} is equal to the number of years that analyst *i* has been covering firm *f* under the same CEO, as of year *t*. All other variables are defined in the Appendix. CEO turnover years are excluded from this analysis. The dependent variable is winsorized at the 99th percentile. Robust standard errors are clustered at the analyst-CEO pair level. Statistical significance at the 1%, 5%, and 10% levels are denoted by ***, **, and *, respectively.

Independent variables	*RFA*
*MATCH * CEO_EXP*	0.014**
	(2.13)
MATCH	-0.10***
	(-3.72)
CEO_EXP	-0.010***
	(-8.84)
HORIZON	0.0048***
	(129.02)
EXP	-0.0022***
	(-6.76)
#FIRMS	-0.00044
	(-1.51)
COMMON	0.019***
	(3.13)
Constant	-0.47***
	(-17.31)
Observations	179,156
R-squared	0.173
Year FE	YES
Firm FE	YES

Table X
Forecast Accuracy Before and After the Implementation of Reg FD

This table reports the estimated coefficients from regressions of *Relative forecast accuracy* (*RFA*) on *MATCH* and other independent variables. Each coefficient's *t*-statistic appears directly below the coefficient estimate. The first column reports the results for the subset of forecasts made for fiscal years beginning prior to October, 2000 (the month in which Reg FD went into effect). The second column reports the results for the subset of forecasts made for fiscal years beginning in or after October, 2000. All variables are defined in the Appendix. The dependent variable is winsorized at the 99th percentile. Robust standard errors are clustered at the analyst-CEO pair level. Statistical significance at the 1%, 5%, and 10% levels are denoted by ***, **, and *, respectively.

	Before Reg FD	After Reg FD
	(1)	(2)
Independent variables	*RFA*	*RFA*
MATCH	-0.083**	-0.043**
	(-2.07)	(-2.14)
HORIZON	0.0044***	0.0050***
	(57.75)	(121.78)
EXP	-0.0065***	-0.0025***
	(-5.44)	(-7.98)
#FIRMS	0.0028***	-0.0024***
	(5.99)	(-6.80)
COMMON	0.035***	0.018***
	(2.97)	(2.65)
Constant	-0.47***	-0.58***
	(-16.00)	(-31.78)
Observations	34,820	158,878
R-squared	0.165	0.179
Year FE	YES	YES
Firm FE	YES	YES

Appendix
Variable Definitions

Variable	Definition
$MATCH_{ift}$	Indicator variable equal to one if analyst *i* covering firm *f* in year *t* has the same first name as the CEO of covered firm *f* during year *t*, and is equal to zero, otherwise.
Relative forecast accuracy (RFA_{ift})	Relative forecast accuracy for analyst *i* covering firm *f* in year *t* is equal to $\left[Abs(FE_{ift})-\overline{Abs(FE_{ft})}\right]/\overline{Abs(FE_{ft})}$, where FE_{ift} is equal to the difference between the firm's realized earnings for the year and the analyst's last annual earnings forecast preceding the release of those earnings; $Abs(FE_{ift})$ is the absolute value of FE_{ift}; and $\overline{Abs(FE_{ft})}$ is the average of the absolute forecast errors of all the analysts following firm *f* during year *t*.
$HORIZON_{ift}$	The number of days from the time that the last forecast for year *t* is issued by analyst *i* covering firm *f* until the time that the earnings for year *t* are released.
EXP_{it}	The number of years of experience that analyst *i* forecasting year *t*'s earnings has had, from the year in which the analyst's forecasts first appear on *IBES* until year *t*.
$\#FIRMS_{it}$	The number of firms for which analyst *i* has issued a forecast in year *t*.
$COMMON_{i}$	Indicator variable equal to one if analyst *i*'s first name is classified as common, and is equal to zero, otherwise. A first name is classified as common if it has a frequency of 2% or more during the decade that is 30 years prior to the analyst's first appearance on *IBES*, according to Social Security Administration records. We alternatively define name commonness by (1) having a frequency of 1.5% or more or (2) being one of the top 50 names in terms of frequency in the *IBES* database.