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Authors

Even-Tov, Omri

Huang, Kanyuan

Trueman, Brett

et al.

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Sharing names and information: Incidental similarities between CEOs and analysts can lead to favoritism in information disclosure

Omri Even-Tov[a,1], Kanyuan Huang[b], Brett Trueman[c], Jonathan E. Bogard[d,1], and Noah J. Goldstein[e]

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When two people coincidentally have something in common (such as a name or birthday), they tend to like each other more and are thus more likely to offer help and comply with requests. This dynamic can have important legal and ethical consequences whenever these incidental similarities give rise to unfair favoritism. Using a large-scale, longitudinal natural experiment, covering nearly 200,000 annual earnings forecasts over more than 25 y, we show that when a CEO and a securities analyst share a first name, the analyst's financial forecast is more accurate. We offer evidence that name matching improves forecast accuracy due to CEOs privately sharing pertinent information with name-matched analysts. Additionally, we show that this effect is especially pronounced among CEO–analyst pairs who share an uncommon first name. Our research thus demonstrates how incidental similarities can give way to special treatment. Whereas most investigations of the effects of similarity consider only one-shot interactions, we use a longitudinal dataset to show that the effect of name matching diminishes over time with more interactions between CEOs and analysts. We also point to the findings of an experiment suggesting that favoritism born of sharing a name may evade straightforward regulation in part due to people's perception that name similarity would exert little influence on them. Taken together, our work offers insight into when private disclosures are likely to be made. Our results suggest that the effectiveness of regulatory policies can be significantly impacted by psychological factors shaping the context in which they are implemented.

similarity | favoritism | information disclosure

Significance

Incidental similarities (e.g., sharing a name) can cause people to feel affinity toward each other, which can thus engender preferential treatment. This favoritism becomes problematic when it leads people to cross legal or ethical lines. In this paper, using a large-scale natural field experiment, we show that when a CEO and a securities analyst happen to share a first name, the analyst's forecast is more accurate. We offer converging evidence that the name-matching advantage is likely due to the private disclosure of information, despite it being illegal for CEOs to privately share information with securities analysts. We consider the psychological challenges associated with regulating this kind of name-based favoritism as compared to other kinds of more overt similarities.

What explains when and why people are sometimes willing to unethically, even illegally, show favoritism toward others? In the present investigation, we consider this question in the context of CEOs privately sharing corporate information to the advantage of certain securities analysts. Such private information then provides a competitive advantage for favored analysts to make more accurate earnings forecasts; indeed, securities analysts' compensation and job prospects are tied to the accuracy of their forecasts (1). Yet this kind of selective disclosure by CEOs is illegal: In response to rampant cronyism, the United States Securities and Exchange Commission (SEC) passed Regulation Fair Disclosure (Reg FD) in 2000, making it illegal for firms to privately disclose material information. Since that time, any pertinent information that a CEO shares individually with a securities analyst must also be publicly disclosed. However, because most CEO–analyst meetings occur in private, and because the definition of "material" information remains imprecise, the effects of Reg FD have been mixed. Mounting research has shown that this mandate is routinely flouted, and private information continues to flow between CEOs and securities analysts (2).

It is therefore important to understand what leads CEOs to disregard Reg FD and reward some analysts by privately divulging important information. Here, we offer part of the answer: We show that an incidental similarity between CEOs and analysts—sharing a first name—leads to more accurate analyst forecasts likely due to the private sharing of information. Decades of research has shown that similarities like sharing names can lead to greater affinity (3, 4), and greater affinity typically leads to greater helping behavior. In the context of the present investigation, this may result in greater compliance, as when analysts ask privileged questions, and/or greater voluntary disclosure, as when CEOs choose to privately share unsolicited information with matched analysts. This private information may then help analysts build more accurate forecasts. For these reasons, we predicted that when analysts and CEOs share a first name, CEOs would be more likely to share critical information and thus name-matched analysts would make more accurate forecasts. Moreover, because naming conventions are associated with race and gender, the considerable financial rewards for this additional accuracy entail that name matching may

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[1]To whom correspondence may be addressed. Email: omri_eventov@berkeley.edu or bogard@wustl.edu.

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serve as an additional source of unfair treatment of traditionally disadvantaged groups.

Related Literature

Name-Matching and Affinity. When people feel more similar to others, they tend to feel a stronger sense of closeness (5, 6). Heider argued that incidental similarities (e.g., mutual interests, backgrounds, preferences, and so on) can cause two people to feel part of a "unit relationship" (7). This association then becomes the basis for greater attraction and mutual liking. Extending this idea, Pelham and colleagues follow Byrne in suggesting that name-similarity in particular can lead to increased attraction and positive evaluation (3, 8). Perhaps explaining this, some evidence shows that the positive evaluations people have of themselves (9) can spill over onto their evaluations of other things closely associated with their personal identity (10). This greater affinity, in turn, can cause people to have stronger preferences for people, places, and things with which they have tighter personal associations (11, 12). In other words, people will tend to hold especially positive evaluations of things that are more similar to their names (13), which may affect their behavior (14). For instance, research has shown that people are more likely to follow Twitter accounts of others with the same name (15) and purchase from brands whose names match their initials (16). Two people who share a name are therefore likely to feel closer and like each other more compared to two people who, ceteris paribus, do not share a name. We therefore expect CEOs to feel more positive about and closer with a securities analyst who happens to share their first name compared to those who do not. This closeness then serves as the basis for a stronger relationship.

Affinity and Favoritism. People tend to show favoritism toward members of their in-group (17). Illustrating such a bias, Cohen and colleagues show that mutual fund managers invest more in firms with senior officials who attended their alma mater (18). Similarly, Hwang and Kim document that when the majority of a company's governing board has something personal in common with the CEO (e.g., regional origin), the CEO receives more generous compensation (19). Even people's charitable donations tend to privilege beneficiaries with similar names (20–22). As another demonstration of this effect, a recent randomized field experiment showed that sellers on a second-hand online marketplace were more willing to agree to price reductions when requested by buyers whose name began with the same letter (23). Across many domains, incidental similarities—as when two people share a first name—can thus engender preferential treatment. While no experimental evidence exists to support the claim that major life decisions (e.g., where to move, whom to marry, what job to take) are influenced by a person's name, decades of experimental research do show that incidental similarities have powerful effects on affinity and helping (5, 17, 22, 24). It is on this latter literature that our hypotheses theoretically rest.

Similarities can also cause people to become more willing to act unethically. For instance, increased affinity can increase the perception that others are deserving of help (25), which can in turn increase people's willingness to violate ethical rules (26). More directly, experimental research has shown that people are willing to take morally dubious actions to benefit others who they like more. As an example, Cadsby and others find that people are more willing to cheat if doing so would help members of their in-group (ref. 27, see also ref. 28). Similarly, empathizing with other people—which increases with affinity (e.g., ref. 29)—can lead to an increased willingness to override justice concerns in order to benefit individuals (30). For all these reasons, whenever sharing a name causes CEOs to feel similar to a name-matched analyst, they may be more likely to flexibly disregard ethical or legal prohibitions against private disclosure.

Further, the increased closeness between name-matched CEOs and analysts may even lower the perceived risk of illegally sharing private information. Illustrating this possibility, Zimmer and colleagues find that greater relationship trust can alleviate perceptions of risk associated with disclosing private information (31). In fact, recent research has suggested that analysts whose faces are judged more trustworthy make more accurate forecasts (32). Peng and colleagues speculate that this is driven by leaders selectively disclosing information to trustworthy-seeming analysts, noting that the advantage of these analysts soars after in-person conferences. If name-matching leads CEOs and analysts to form stronger bonds, which in turn increases trust, this may similarly lead CEOs to privately share more information with name-matched analysts. And because what counts as a material disclosure is not strictly defined, there may be enough situational ambiguity for CEOs to bend moral and legal rules without feeling the "ethical dissonance" of having transgressed (33).

Hypotheses

For all these reasons, despite Reg FD's prohibition of sharing private information, we predicted that analysts' forecasts would be more accurate when they shared a first name with the CEO of a firm whose performance they were forecasting. We test this hypothesis both by a) comparing forecasts of the same firm made by name-matched and mismatched analysts, and by b) comparing forecasts made by the same analyst when matched versus when unmatched with the CEOs of various firms whose performance they forecast. Further, because uncommon similarities are especially likely to engender stronger connections (7, 24), we predicted that the name-match effect on forecast accuracy would be stronger when the shared name is relatively uncommon. Conversely, if an analyst and CEO have a very common name, they are likely used to meeting people with the same name. As a consequence, any enhanced affinity due to sharing a first name may be attenuated, and we would therefore expect the name-matching effect to be smaller for names that are more common.

We hypothesize that the increased accuracy of name-matched analyst's forecasts is attributable to CEOs selectively sharing private information with name-matched analysts. To support this conjecture, we assessed the strength of the relationship between name matching and forecast accuracy under different conditions of informational asymmetry (and hence value). In other words, we test whether there is a stronger advantage of name matching in situations in which more information about a firm's performance is privately known relative to what has been publicly disclosed. For example, we test whether there is a stronger name-matching effect in years when firms do not issue their own earnings forecasts, which reduces the amount of information publicly available to all analysts, compared to years when firms issue their own public forecasts. Further, consistent with the proposed mechanism, we show that the advantage of sharing a name diminishes over time for a given CEO–analyst pair. The more meetings between a CEO and an analyst, the more likely that sharing a name (or not) becomes just one of many dozens of similarities (or not) that may be shared. Thus, the diminishing effect of name-matching over the course of a CEO–analyst relationship is consistent with an information-sharing explanation of the name matching effect.

Our hypotheses are tested on a large-scale longitudinal dataset, covering nearly 200,000 annual earnings forecasts over more than 25 y. This empirical context and sample provide an ideal setting for studying the real, consequential effects of incidental similarity (i.e., name matching) while accounting for other variables that have confounded prior research in this domain. Below, we describe the field context and analytic strategy for testing our hypotheses, then we describe our results: We first demonstrate the effect of name matching on forecast accuracy, offering several additional tests of our hypotheses using alternative specifications. We then provide evidence indicating that private information sharing is an important mechanism for the similarity effects we observe. We conclude by drawing general lessons about similarity, unethical behavior, and successful policy regulation of unfair favoritism.

Materials and Methods

Data. Our initial sample consists of the approximately 1.9 million annual earnings forecasts that are recorded in the Institutional Brokers' Estimate System (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). We follow prior literature and retain just the last forecast of a given company issued by an analyst for a given fiscal year (e.g., ref. 34), resulting in a final sample of 193,698 earnings forecasts. It should be noted that CEOs do not select the analysts who follow their firms. Rather, portfolio managers at brokerage firms typically assign each analyst to cover all the major firms for an entire sector or industry.

In determining whether an analyst and a CEO have the same first name, we allow for variations in spelling. 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. However, our results hold if we restrict matches to those where names are spelled identically (93% of matches in our dataset; see *SI Appendix*, Table S4, Panel B, for further discussion). Our final sample is composed 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. This sample 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. The full details of our data construction, variable definitions, and descriptive statistics can all be found in *SI Appendix*.

Measures.

Dependent variable. We use relative forecast accuracy as our key dependent measure. 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. We use this to then construct each analyst's relative forecast accuracy, RFA_{ift}, given by:

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

where $Abs(\overline{FE_{ft}})$ is the average of the absolute forecast errors of all the analysts who followed firm f in year t, and $Abs(FE_{ift})$ is the absolute value (for each analyst, i) of FE_{ift}. This is the same measure of relative forecast accuracy used by others (e.g., ref. 35) with the order of the numerator terms swapped for rhetorical ease. Thus, a positive (negative) value of RFA_{ift} reflects a forecast that is more (less) accurate than the average. For our analyses, we winsorize observations of relative forecast error at the 99th percentile, a common practice for this kind of analysis since data errors can sometimes yield outliers thousands of SD from the mean (36). If $Abs(\overline{FE_{ft}})$ is equal to zero, meaning that all analysts have a zero forecast error, we set RFA_{ift} equal to 0 (though excluding the small number of such instances has no discernable impact on our results). The mean value of *RFA* is zero (by definition), and we observe ranges from 1 (when the analyst's forecast error is zero) to −18.85.

Independent variables. While *SI Appendix* precisely describes how all of the variables are defined, we pause to give explanations of the independent variables that are common across all or most regressions in our analyses. Our primary independent variable of interest ($MATCH_{ift}$) is an indicator variable corresponding to whether the analyst i has the same first name as the CEO of covered firm f in year t. Additionally, we include several control variables in the following models. First, we control for the number of calendar days between a) the date of the last forecast made by analyst i for covered firm f during year t and b) the date of the release of the firm's earnings for that year ($HORIZON_{ift}$). We include this as a control variable since we expected that the further out from an earnings release that a forecast was issued, the lower its relative accuracy. Second, we control for 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 (EXP_{it}). We included this control since we expected that greater overall experience would correspond with greater relative forecast accuracy. Third, we control for the number of firms for which analyst i has issued at least one forecast of annual earnings during year t ($\#FIRMS_{it}$). While this variable should correspond with analyst experience (and presumably skill) and thus accuracy, others have found that covering more firms is associated with lowered forecast accuracy due to limited time for analysts to thoroughly research each covered firm. Either way, we include this variable as a control because it is likely related to forecast accuracy. Finally, in the following regressions, we control for $COMMON_i$, which is an indicator variable equal to one if analyst i's first name is classified as common and is equal to zero otherwise.

We control for the commonness of a name for several reasons. First, we wanted to account for the fact that the names most likely to be matched are most likely to be common names. Moreover, the commonness of a name might plausibly be related to forecast accuracy. Most directly, research has found that individuals with common names are considered more attractive and are more liked than those with uncommon names (37–39). This stronger liking (independent of matching) may directly result in better forecasts for similar reasons as name-matching (i.e., private information disclosure due to increased affinity). Moreover, there is an indirect reason why people with more common names may have different forecasting accuracy than those with less common names. If having a common name causes people to be better liked, all else equal, these people may get preferential treatment in hiring (40, 41). If so, this artificial inflation of candidate attractiveness 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. For all these reasons, we include $COMMON_i$ as a control variable in the following regressions. As with all of the aforementioned controls, we note that our results are qualitatively similar when not including $COMMON_i$ as a control variable.

Throughout, we typically cluster SE in our analyses at the level of CEO—analyst pairs to account for serial correlation in cases when, for example, a given analyst follows a CEO over several years. However, results remain qualitatively unchanged if instead SE are clustered at the firm-year level.

Results

Name Matching and Forecast Accuracy. We begin by testing for a first-order effect of CEO–analyst pairs sharing a first name on analyst forecast accuracy. To do so, we estimate the following regression:

$$\begin{aligned} RFA_{ift} = {} & \alpha_0 + \alpha_1 MATCH_{ift} + \alpha_2 HORIZON_{ift} + \alpha_3 EXP_{it} + \alpha_4 \#FIRMS_{it} \\ & + \sum \beta_t * \text{Year dummy variables} \\ & + \sum \gamma_f * \text{Firm dummy variables} + \varepsilon_{ift}. \end{aligned} \qquad [2]$$

In this regression, we include year and firm fixed effects. The results of estimating this regression are reported in column 1 of Table 1.

For a given firm, forecast accuracy is higher among name-matched analysts (versus unmatched analysts). As conjectured, an analyst's relative forecast accuracy is significantly greater when that analyst shares a first name with the firm's CEO than when they have different first names. Before estimating regression (2), we find that an uncontrolled regression (i.e., comparison of cell means) accounting for both analyst–year and firm fixed effects shows that forecast accuracy is higher for name-matched analysts (RFA = 7.0%) compared to those who are unmatched (*RFA* = 2.5%;

Table 1. First-name match and forecast accuracy

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

Note: All variables are defined in the Appendix. The dependent variable is winsorized at the 99th percentile. Each coefficient's t-statistic appears directly below the coefficient estimate. Robust SE are clustered at the analyst–CEO pair level. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, and *, respectively.

t = 2.40, P = 0.008). Further, when including the controls in Eq. **2**, as can be seen in Table 1, the coefficient on *MATCH* is significantly positive. The magnitude of the coefficient 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 nonlocal analysts, as documented by others (42). Additionally, when reestimating Eq. **2** using only those last forecasts that are issued within 120 d of the earnings announcement, we find that the relative accuracy of the forecasts of matched analysts remains significantly higher, on average, than that of the unmatched analysts (b = 0.41, t = 2.01).

Results hold when controlling for race and gender matches. Many of the matches we observe are composed of CEO–analyst pairs who come from the same ethnic background or are of the same gender. To rule out the possibility that demographic ties are driving our results, we revise our model specification to account for whether a pair matches on ethnicity and/or gender. To do so, we constructed a dummy variable corresponding to whether each CEO–analyst pair match for each identified ethnic group. Similarly, we constructed a dummy variable representing whether each CEO–analyst pair are of the same gender. Given this coding scheme, the regression thus utilizes a reference category of analysts who are unmatched on both ethnicity and gender. The resulting effect of matching is significantly positive in this regression (b = 0.048, t = 2.74, P < 0.001), signifying that a first-name match is associated with forecasting superiority above and beyond any effect of sharing a gender or ethnicity.

For a given analyst, forecast accuracy is higher among firms with name-matched CEOs (versus firms with unmatched CEOs). To validate that our results are not driven by analyst-specific effects, we examine, for a given analyst-year, whether each analyst's forecasts are more accurate for firms led by CEOs with whom the analyst shares a first name (compared to firms lead by CEOs whose name does not match the analyst's). To do so, we reestimate regression (2), replacing the year fixed effect in the regression with an analyst-year fixed effect (column 2 of Table 1). Consistent with our other results, the relative accuracy of a given 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.

When there is CEO turnover, forecast accuracy of formerly matched analysts decreases while accuracy for newly matched analysts increases. CEO turnover represents an exogenous shock to the set of name-matched and unmatched pairs, providing an alternative means by which to test whether a matched name is associated with higher relative forecast accuracy. We expected that following a change in CEO, previously matched analysts who become unmatched will exhibit a decrease in forecast accuracy relative to unmatched analysts who remain unmatched. Conversely, those previously unmatched analysts who become matched should show an increase in forecast accuracy relative to those who remain unmatched.

To test this, we first compared two sets of analysts covering each firm in our dataset: one set of analysts whose names were matched preturnover then became unmatched postturnover, and another set of analysts whose names were unmatched with CEOs both before and after the turnover. We then regressed relative forecast accuracy on 1) an indicator variable corresponding to the forecast being issued the year after CEO turnover, 2) an indicator variable corresponding to being matched preturnover then becoming unmatched postturnover (versus being unmatched pre- and post-turnover), and 3) their interaction, as well as 4) the same set of controls as in regression 2 above. Furthermore, we also estimated a similar regression, this time comparing those analysts who were mismatched preturnover but who became matched postturnover versus those analysts who were unmatched both before and after turnover. Critically, the interaction term in each of these regressions represents the extent to which the change in relative forecast accuracy is different for CEOs whose matching status switches as a result of the turnover (versus those whose status did not switch).

First, perhaps unsurprisingly, we find that relative forecast accuracy overall decreases after a change in CEO (b_{post} = −0.14, t = −8.97, P < 0.001), reflecting the increased noise in forecasting a firm's performance immediately after a new CEO takes over. Of note, however, it decreases even more for an analyst whose first name matched that of the previous CEO but does not match that of the current CEO ($b_{match\_unmatch}$ = −0.19, t = -1.80, P < 0.10). We find that relative forecast accuracy after CEO turnover decreases significantly less—and directionally, though not statistically significantly, increases—for an analyst whose first name comes to match that of the new CEO ($b_{unmatch\_match}$ = 0.23, t = 1.84, P < 0.10). These findings provide additional evidence that matched analysts issue more accurate forecasts than do unmatched analysts.

Stronger Effects for Less Common Names. To provide further evidence that the matched analysts' superior performance is driven by their sharing a first name, we turn our focus to the popularity of shared first names. As noted in the introduction, we expected that the effect of name matching would be greater for less common names compared to more common names. Specifically, we expected that the previously documented name-matching effect is driven by those CEO–analyst pairs who share less common names.

To test this, we regressed relative forecast accuracy on 1) an indicator variable representing whether the CEO and analyst matched and shared a name classified as uncommon (*SI Appendix*), as well as 2) an indicator variable representing whether the CEO and analyst matched and shared a common name. In addition to controlling for the same variables as in Eq. **2**, we also included a control for whether the analyst's name is classified as common (capturing the incremental accuracy of an unmatched analyst with a common name over that of one with an uncommon name). We were primarily interested in the relative forecast accuracy of matched analysts who have uncommon names (versus unmatched analysts with uncommon names). We were also interested in exploring whether the effect of matching persisted even for CEO–analyst pairs with highly common names (compared to CEOs and analysts who both had common, but not matching names).

We find that, among analysts with uncommon names, there is a strong, economically large 8.2 percentage point accuracy advantage to name matching (b = 0.082, t = 2.90, $P < .001$). Moreover, since we find that the effect of having a common (unmatched) name is significantly negatively related to forecast accuracy (b = −0.018, t = −3.00, $P < 0.001$), matched analysts with an uncommon first name are also significantly more accurate, on average, compared to unmatched analysts with a common name. To rule out the concern that sharing an uncommon name could be confounded with CEOs and analysts sharing gender and/or ethnicity, we replicate this analysis while also controlling for gender- and ethnicity-matching controls (as described below). The effects for both uncommon matches (b = 0.080, t = 2.85) and common matches (b = 0.031, t = 1.41) remain qualitatively unchanged. In contrast, we find that a matched analyst with a common first name has a forecast accuracy that is not detectably different from that of an unmatched analyst who also has a common first name. In *SI Appendix*, we show that the accuracy advantage of matching with uncommon names is robust to strict ways of classifying a name as (un)common. In other words, the effect of name matching seems to persist for all but the absolute most common names.

Of course, securities analysts have a different distribution of names than the general population, so a given analyst has a different chance of matching names with a randomly chosen CEO than with a person randomly drawn from the American population. As another robustness check on the measure of a name's commonness, we reestimated the above regression, this time using the frequency with which names appear on the IBES database as the measure of a name's popularity. This measure of a name's popularity may arguably better capture how likely a given analyst (CEO) is to work with a CEO (analyst) of the same name. On this specification, we find even more extreme results as reported above: 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 (b = 0.14, t = 3.03, $P < 0.001$). We also note that the effect of matching with a common name also becomes significantly positive on this specification, although much smaller in magnitude than for uncommon names (b = 0.038, t = 2.03). Because we theoretically expected uncommon names to be especially causally potent in affecting affinity, these results provide further evidence that sharing first names positively affects an analyst's forecasting accuracy.

Mechanism: Private Disclosure of Information. We surmise that the increased accuracy of name-matched analysts' forecasts is attributable to CEOs selectively sharing private information with name-matched analysts. By definition, the private meetings between CEOs and analysts are not observed, so this hypothesis cannot be directly tested. Instead, what follows is a series of tests that offer converging evidence that name matching facilitates selective sharing of information. We show that in cases where the value of information is greater—in years without firm-generated forecasts and for smaller firms with less publicly available information—the effect of name matching is greater.

Stronger in years without firm-generated forecasts. Our claim that matched analysts are more likely to receive private information implies that their forecasting superiority should be greater in situations where there is higher information asymmetry between firms and analysts. Put otherwise, when an abundance of vital information is publicly available to all analysts, the advantage of name matching should be smaller. To wit, past research has found that interpersonal relationships are especially important in similar situations under conditions of information asymmetry (43). To test this, we partition our sample into those firm-years in which management issued a public earnings forecast versus those years in which no management earnings forecast was issued. With information asymmetry higher in the absence of a managerial earnings forecast, we expected that the effect of a name match would be higher in firm-years without a managerial forecast.

Table 2 reports the results of reestimating 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). We observe a pattern of results consistent with our hypothesized mechanism. In years for which no management forecast was issued, the coefficient on *MATCH* is significantly positive and economically considerable (7.3 percentage points). In contrast, the effect of name matching is not significantly different from zero for years in which management forecasts were made publicly available.

As an additional test of this hypothesis, we can exploit variation in the number of management forecasts issued in a year. While CEOs are constantly updating their sense of their firms' financial

Table 2. Subsample analyses depending on whether or not there was a management-issued financial forecast of firm performance

Independent variables	*(1) RFA*	*(2) RFA*
Subsamples	*No management forecast*	*Forecast issued*
MATCH	0.073***	0.019
	(3.11)	(0.69)
HORIZON	−0.0048***	−0.0049***
	(−103.52)	(−86.49)
EXP	0.0027***	0.0032***
	(6.36)	(7.55)
#FIRMS	−0.00014	0.0019***
	(−0.41)	(4.02)
COMMON	−0.021***	−0.018**
	(−2.62)	(−2.07)
Constant	0.47***	0.59***
	(16.46)	(4.22)
Observations	111,650	82,048
R-squared	0.178	0.174
Year FE	YES	YES
Firm FE	YES	YES

Note: 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 SE are clustered at the analyst−CEO pair level. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, and *, respectively.

performance as the year progresses, there is wide variability in the number of updated forecasts that firms choose to publish in a given year. Table 3 considers the effect of name matching as a function of the number of management-issued forecasts in the year. As can be seen, the effect of matching on forecast accuracy becomes increasingly negative (i.e., the advantage of name matching diminishes) with an increase in the number of management-issued forecasts. In other words, as firms publicly offer more up-to-date information for all analysts to access, the advantage of name matching with CEOs diminishes. Conversely, when there is a greater amount of pertinent information privately known to firms but not publicly available to all analysts, the effect of name matching increases. We take this to be further evidence that, compared to mismatched analysts, name-matched analysts have more accurate forecasts because they privately receive more information germane to their forecasts. If the accuracy effect of name matching was explained by another channel besides private information disclosure, we would not expect an interaction with the amount of information that the firm chooses to publicly disclose.

Stronger for smaller firms. In line with the preceding section, we also hypothesized that forecasts of smaller firms would exhibit a stronger name-matching effect. The larger the firm, others have shown, the more information that is publicly known about the firm and its performance (44, 45). However, for smaller firms with less publicly available information, we expected that the value of privately shared information would thus be greater. Moreover, we speculate that the larger the firm, the less likely it is that an analyst would be able to meet with the CEO and thus the less likely it is that name-matching could possibly have an effect. Therefore, we expected to see a larger effect of name matching on forecast accuracy for smaller firms. To test this hypothesis, we measured the extent to which the name-match effect depends on the size of the firm being forecasted. Since all of the firms in our dataset are in the S&P 1500, firm size is right-skewed; the median firm size is in the 80th percentile of all American firms. We operationalize firm size in the following analysis by capturing (the logarithm of) each firm's market capitalization for each year in which a forecast was issued.

To test these predictions, we reestimated regression (2) including an interaction term between firm size and CEO–analyst name matching within the subset of firm-years without management forecasts. We find, as expected, that the accuracy advantage of name-matched analysts decreases as the firm being covered gets larger ($b = -0.027$, $t = -2.37$). As before, there are larger name-matching effects in situations with a higher value of private information. In fact, partitioning firms into deciles according to their market capitalization, we find a significantly positive effect of name matching across deciles up to the eighth decile of firm size. Once again, we take this to indicate that name-match effects are driven by privately disclosed information.

Attenuation with experience. We expected that matched analysts' relative forecast superiority would diminish the longer the match had been in place. There are two reasons for this prediction. First, as analysts gain experience covering a firm, their expertise in forecasting that firm's earnings increases, thereby attenuating the relative importance of informational advantages from sharing a first name with the CEO. Second, as time passes, the affinity between an analyst and CEO due solely to their sharing of a first name is likely to diminish, as previously argued. To test this conjecture, we added a variable to regression (2) corresponding to the number of years that an analyst had been covering a given firm under the same CEO. Critically, we also interacted this variable with the indicator for whether or not the analyst and CEO matched names.

Unsurprisingly, we find a positive and significant effect of experience, indicating that the relative accuracy of an analyst's forecast increases, on average, the longer the analyst has been following the firm under the same CEO ($b = 0.010$, $t = 8.84$). Of note, however, we find that analyst experience negatively interacts with name matching ($b = -0.14$, $t = -2.13$). This suggests that the accuracy advantage that a matched analyst has over an unmatched analyst with the same firm experience decreases the longer the analysts have been covering the firm. It appears, then, that whatever early advantages might be conferred to name-matched analysts get swamped by subsequent experiences as the relationship unfolds over time. This is consistent with our overall hypothesis that incidental similarities early in a relationship can beget greater affinity and thus preferential treatment, all else equal.

Table 3. Regression accounting for number of management-issued forecasts

Independent variables	*RFA*
*MATCH * # MGMT FORECASTS*	−0.011*
	(−1.73)
MATCH	0.070***
	(3.27)
# MGMT FORECASTS	0.0011
	(0.86)
HORIZON	−0.0048***
	(−134.47)
EXP	0.0030***
	(9.74)
#FIRMS	0.00058**
	(2.06)
COMMON	−0.019***
	(−3.19)
Constant	0.47***
	(17.50)
Observations	193,698
R-squared	0.174
Year FE	YES
Firm FE	YES

Discussion

In this paper, we show that when a securities analyst and the CEO of a firm share a first name, the accuracy of the analyst's forecast is considerably higher compared to a) that same analyst's forecasts of other firms and b) the forecasts of that firm made by analysts who do not share the CEO's name. In support of these analyses, we additionally show that when there is CEO turnover, relative forecast accuracy of that company falls for the formerly matched analyst while forecast accuracy increases for newly matched analysts who come to share a name with the new CEO. Consistent with our theorizing, we find that this effect is even more pronounced when the name shared by the CEO and analyst is less common. Further still, we offer several converging pieces of evidence that the name-matching effects we observe are driven by the selective disclosure of private information to name-matched analysts. Supporting this claim, we show in various ways that when the value of private information is higher so too is the advantage of name matching. We conjecture that the effect of name matching is owed to the heightened affinity felt between two people who share something personal in common. This affinity, we predict, then leads CEOs to share information privately with these analysts to whom they feel a greater connection.

Our results bear on an ongoing academic controversy about the validity and generalizability of similarity effects. Where much of the foundational literature demonstrating "implicit egotism" has been called into question (46–48), our research offers a robust test of the effects of name matching. Using a large-scale natural field experiment, we avoid the selective testing issues identified by Gallucci (46) and the reverse-causality concerns identified by Simonsohn (48) that have plagued some earlier investigations of the effects of people's names on important choices. Of course, there are associations with a person's first name (e.g., culture, socioeconomic status, geography) that we cannot observe in our data. Nonetheless, we find advantages of name matching above and beyond the potentially confounding effects of age cohort,[*] gender-matching, and ethnicity-matching as demonstrated by Simonsohn (47). Furthermore, extending previous research, we observe a time series of CEO–analyst interactions, with a dataset covering more than 25 y, to assess the effects of name similarity in repeated, ongoing relationships. Contrary to the prediction that name matching would create a permanent advantage for matched pairs, we find that the effect of name matching seems to diminish over time. This sheds light on the effects and psychological drivers of implicit egotism.

As noted, we do not find evidence that name-matching effects are driven by either ethnicity- or gender-matching. Nevertheless, it may be important to consider how these demographic factors relate to norms for choosing names. As reported in *SI Appendix*, the vast majority of CEOs in our dataset were white men. Because naming conventions are associated with race and gender, analysts named George and Brad were far more likely to match names with CEOs than analysts named Jerel or Fatima were. Illustrating this point: Over 50% of all matches we observe are attributable to just five typically white, typically male names. Given the considerable financial advantages to name matching, not all races or genders are equally likely to enjoy the advantages of name matching. Compounding this, because white male analysts are the most likely to match with CEOs of additional firms they begin covering as their careers progress, and because name-matched analysts are likely to be rewarded for their resulting superior forecast accuracy, gender and racial gaps in payment and promotion may continue to widen.

The present research represents a demonstration of a name-matching effect on information disclosure, doing so in a context in which such private disclosures are both extremely profitable and illegal. Of course, we do not observe the one-on-one interactions between CEOs and analysts so cannot directly measure private information disclosure. A name-match effect could therefore be driven by additional mechanisms we cannot rule out. For instance, it is possible that name-matched analysts spend more time and try harder on the forecasts of firms led by name-matched CEOs compared to their forecasts of firms run by unmatched CEOs. We do not take this explanation to be likely, however, since CEOs do not typically observe the forecasts made by analysts, so name-matched analysts face no economic or social incentive for trying harder on these particular forecasts. Nonetheless, a limitation of the present investigation is that we are only able to indirectly test for evidence of our proposed mechanism. An additional limitation of the present investigation is that we only observe the names of approximately half of all analysts who issued forecasts during the 26-y observation period. There are likely unobserved differences between these groups; for instance, we are more likely to observe the names of analysts who are especially active (i.e., those who cover more firms or issue more forecasts). Nonetheless, it is difficult to imagine that our results could be explained by any systematic differences between those analysts whose names we do versus do not observe.

Finally, as documented in *SI Appendix*, we show that the observed name-match effect may have been moderately reduced by, but nonetheless persists even after, the enactment of Reg FD, which expressly prohibits the disclosure of material information to some analysts but not others. Bearing on these results, we conducted a lab study that found that people do not expect name matching to influence their decisions ($M = -1.86$) compared to sharing an alma mater ($M = -0.08$, $b = 1.78$, $t(682) = 11.32$, $P < 0.001$). The experiment further showed that, when given a hypothetical situation, 97% of participants reported expecting that they would be more likely to be accused of unfair favoritism when favoring someone with whom they share an alma mater rather than a first name (95% CI [0.96, 1.00]). Very little past research has tested for differences in the effects of different sources of similarity, and future research should explore such variance. For instance, the more central the source of similarity to a person's identity, the more that similarity may influence their decisions. On the one hand, this could in turn mean that more central similarities give rise to greater disclosure due to greater affinity. On the other hand, people may more readily anticipate that such similarities could potentially bias them. If this is so, more central similarities may thus give rise to less disclosure if people recognize and compensate for such a bias.

This latter prediction entails that people's lay beliefs about the forces that are likely to bias them—and, critically, those they think are not likely to bias them—may moderate the effects of similarity. Our research invites further questions about the interaction between the effects of various sources of similarity (e.g., shared race versus alma mater versus first name) and people's beliefs about their effects. As an example, it is an open question whether people are any more likely to recognize the biasing potential of the sources of similarity that in fact tend to have the strongest effects. Indeed, such sources of similarity may have stronger effects because they are underdetected. Additionally, the causal relationship between a) the potency of a given similarity and b) people's awareness of its biasing potential is unresolved. Thus, further research should consider whether similarity effects can be weakened by making people aware of their biasing potential.

An important contribution of the present research is to demonstrate that subtler, more unexpected forms of bias may evade straightforward efforts to forestall unethical favoritism. Laws that regulate different kinds of favoritism may thus be more or less effective depending on the psychological context in which they are implemented. Perhaps one reason it is so hard to prevent selective information disclosure in the context of Reg FD is specifically due to the fact that people are mostly unaware of the biasing effects of name matching. Since selective disclosure of information has already been legally banned in this context, perhaps all that can be done to discourage favoritism is thus to bring awareness to the problem. Indeed, acknowledgment of a bias might be necessary for individuals to correct their behavior. In this way, we seek to draw attention to a heretofore-neglected source of bias in hopes that it might help reduce such bias and favoritism in the future. Understanding this psychology

[*]For elaboration on how we rule out confounding by age cohort as an alternative explanation of the name-matching effects we observe, see the *SI Appendix*.

takes on especially heightened importance for settings in which minor advantages can translate into massive payoffs for privileged individuals.

Data, Materials, and Software Availability. Anonymized data and all other open science materials for the experiment only have been deposited in Research Box (https://researchbox.org/960&PEER_REVIEW_passcode=NEQUDC) (49).

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Author affiliations: [a]Department of Accounting, Haas School of Business, University of California, Berkeley, CA 94720; [b]Department of Accounting, School of Management and Economics, Chinese University of Hong Kong-Shenzhen, Shenzhen 518172, China; [c]Department of Accounting, Anderson School of Management, University of California, Los Angeles, CA 90095; [d]Department of Organizational Behavior, Olin Business School, Washington University in St. Louis, St. Louis, MO 63130; and [e]Department of Management and Organizations, Anderson School of Management, University of California, Los Angeles, CA 90095

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