An Analyst by Any Other Surname: Surname Favorability and Market Reaction to Analyst Forecasts*

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Abstract

We find that forecast revisions by analysts with more favorable surnames elicit stronger market reactions. The effect is stronger among firms with lower institutional ownership and for analysts with non-American first names. Following the 9/11 terrorist attacks, and France and Germany's opposition to the Iraq War, revisions by analysts with Middle Eastern and French or German surnames, respectively, generated weaker market reaction. Surname favorability is not associated with forecast quality, but it has complementary effects with forecast performance on analysts' career outcomes. Surname favorability mitigates under-reaction to forecast revisions. These findings are distinct from the effects of ethnic, cultural proximity, or in-group bias.

Keywords: Equity analyst, surname favorability, earnings forecast, market reaction, motivated reasoning.

JEL Codes: G14, G24, J15, J71.

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We find that forecast revisions by analysts with more favorable surnames elicit stronger market reactions. The effect is stronger among firms with lower institutional ownership and for analysts with non-American first names. Following the 9/11 terrorist attacks, and France and Germany's opposition to the Iraq War, revisions by analysts with Middle Eastern and French or German surnames, respectively, generated weaker market reaction. Surname favorability is not associated with forecast quality, but it has complementary effects with forecast performance on analysts' career outcomes. Surname favorability mitigates under-reaction to forecast revisions. These findings are distinct from the effects of ethnic, cultural proximity, or in-group bias.

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1. Introduction

The first name of an individual is given at birth but the surname is typically inherited from parents and reflects the person's ancestry (Hanks, 2003). For instance, an individual with the surname "Yamamoto" is likely to be of Japanese origin, and the surname "Volkmann" might reflect German origins. In fact, according to U.S. historical immigration records, 99.0% (85.8%) of U.S. immigrants who have the surname "Yamamoto" ("Volkmann") have come from Japan (Germany). Since cultural beliefs and preferences are transmitted across generations (e.g., Guiso et al., 2006; Fernández and Fogli, 2009), stereotypes associated with various countries may automatically get activated and alter people's perceptions about an individual based on how favorably they perceive the country of origin associated with that individual's surname.

In this study, we investigate whether name-induced bias, such as surname favorability, affects the information processing and the investment decisions of stock market participants. Specifically, we examine whether analysts with favorable surnames generate a stronger market reaction to their forecast revisions. Our key conjecture is that investors assess the credibility of information in an analyst's forecast revision more favorably if the analyst has a favorable surname, and a more favorable assessment would lead to a stronger market reaction to the analyst's forecast revision.

This prediction is motivated by two closely related streams of literature in psychology. Studies on motivated reasoning show that people's desire to arrive at a particular conclusion biases their judgment (e.g., Kunda, 1990; Kunda and Sinclair, 1999; Sinclair and Kunda, 1999). One of the main mechanisms for motivated reasoning is that people are likely to reduce unpleasant dissonance among conflicting cognitions (e.g., Kunda, 1990). It suggests that investors who have favorable views toward an analyst because of his or her surname are motivated to assess the analyst's forecasts as being more credible and of higher quality. Making this conclusion reduces the unpleasant inconsistency between their favorability toward the analyst and their belief about the analyst's forecast quality.

Another stream of psychology literature shows that people rely on their subjective feelings when making judgments (Klauer and Stern, 1992; Loewenstein et al., 2001; Slovic et al., 2007; Greifeneder et al., 2011; for a review, see Kunda, 1999). In particular, people assign more positive evaluations to a subject who has a favorable attribute even though the attribute may be irrelevant to their evaluations, because people naturally desire to seek consistency in their attitudes and judgments (e.g., Klauer and Stern, 1992). This tendency is also related to the halo effect, where an impression created in one area biases the evaluations of the object's other qualities (e.g., Thorndike, 1920; Nisbett and Wilson, 1977). The effects of motivation and affect on judgement are closely related to each other, because affect often drives an individual's motivation to draw a particular conclusion, and both predict investors are more likely to believe that analysts with favorable surnames issue high-quality forecasts. As a result, investors react more strongly to forecast revisions by analysts with favorable surnames.

Stock market reaction to analysts' earnings forecasts provides a good empirical setting in which to examine whether people's subjective opinions about an individual based on his or her surname affects their evaluation of the individual as well as the information provided by the individual. Analyst forecasts are an important source of information for stock investment decisions, and investors' evaluation of an analyst, such as forecasting ability, influences the strength of the market's reaction (e.g., Park and Stice, 2000; Clement and Tse, 2003). Investors can easily observe an analyst's surname when forecasts are released. Consequently, their favorability of the analyst, formed through their opinion about the countries of origin inferred from the surname, can influence how they evaluate and process the information in the analyst's forecasts.

Following Pan et al. (2017), we first identify countries of origin associated with a surname by using the U.S. historical immigration records available on ancestry.com. We then measure the level of Americans' favorability toward an analyst's surname by taking the weighted average of the favorability ratings from Gallup poll data for countries associated with the surname.[1]

Using 762,780 forecast revisions issued by U.S. equity analysts from 1996 to 2014, we find that market responds more strongly to forecast revisions issued by analysts who have more favorable surnames. The surname favorability effect holds after controlling for a large number of analyst, firm, and forecast characteristics known to affect the strength of market reactions. Further, our results are economically significant. For example, we find that a one-standard-deviation increase in the favorability of an analyst's surname translates into a 6.30% stronger return-revision relation. This effect is comparable to those resulting from a change in other major analyst characteristics, such as the number of years an analyst has worked in the profession (5.56%) and the number of firms an analyst covers in a year (-6.61%).[2]

We perform a series of robustness checks by altering sample restrictions or adopting different measures of favorability. Our results remain qualitatively the same when we use analysts' last forecasts for a firm-fiscal year only, exclude forecast revisions issued concurrently with other peer analysts' forecasts or corporate disclosures, use different definitions of surname favorability, and measure favorability based on full names. We also run a falsification test to confirm that our results are not a statistical artifact.

In the next set of tests, we find that the effect is more pronounced for firms that are largely

[1] We describe our measure of surname favorability in more detail in Section 2.4.
[2] We offer a detailed interpretation in Section 3.1.

held by individual investors, for analysts whose countries of origin are easily inferred from their surname, and for negative forecast revisions. We further find that the surname favorability effect is weaker for all-star analysts, female analysts, and analysts who use American first names. We find that our results are robust to including a host of fixed effects.

Unlike static name traits, such as ethnicity and foreignness, that prior studies examine, surname favorability has both cross-sectional and time variation. To better establish the causal relation between the favorability of an analyst's surname and market reactions to forecast revisions, we use two exogenous shocks that adversely affected Americans' favorability toward certain countries: the September 11, 2001 (9/11) terrorist attacks and the French and German governments' opposition to the Iraq War. We find that the strength of the market's reaction to forecast revisions significantly decreases for analysts with Middle Eastern and French or German surnames after the 9/11 terrorist attacks and the French and German opposition to the Iraq War, respectively, compared to control analysts. The results strengthen our identification of surname favorability effects.

Next, we find that there is no significant relation between the favorability of an analyst's surname and his or her forecast quality, which is measured by accuracy, bias, and timeliness. The evidence suggests that investors' subjective perception, such as surname favorability, biases their information processing despite no systematic difference in forecast quality.[3] In terms of career outcomes, we find that, conditional on good forecasting performance, having a favorable surname makes it more likely for an analyst to be elected as an all-star or survive in the profession after his or her brokerage house's merger or closure. Thus, the results suggest that surname favorability

[3] Several theory papers show that the effect of irrational trading can persist over time, as investors who irrationally trade can, on average, earn higher expected profits compared with fully rational traders (e.g., De Long et al., 1991; Kyle and Wang, 1997; Fischer and Verrecchia, 1999; Verrecchia, 2001; Hirshleifer and Teoh, 2003).

serves as a catalyst for career progression.

To identify specific channels that generate the surname favorability effect, we decompose our measure of surname favorability into individual components associated with in-group bias, cultural, ethnic and linguistic proximity, and the perceived level of corruption in countries associated with a surname. We find that the favorability component associated with the foreignness of an analyst's name significantly increases the market's reaction to forecast revisions. The result suggests that in-group bias against foreigners (Kumar et al., 2015) plays a significant role in our finding.

More interestingly, we find strong evidence that the residual component of favorability significantly and positively affects the market's reaction to forecast revisions. It suggests that the unexplained portion of surname favorability unrelated to cultural, ethnic, and country factors is an important driver for the market's reaction to forecast revisions. This finding distinguishes our work from prior studies that examine the effect of cultural proximity or in-group bias in financial markets (e.g., Kumar et al., 2015; Du et al., 2017; Jia et al., 2017).

Finally, we examine whether surname favorability mitigates investor underreaction to forecast revisions. Using a subsample with a clear manifestation of post-revision price drifts (Gleason and Lee, 2003), we find that surname favorability is associated with a significantly weaker post-revision price drift. This suggests that a stronger immediate market reaction to a forecast revision by an analyst with a favorable surname mitigates investor underreaction to the forecast revision.

These results contribute to several strands of the accounting and finance literature. Recent studies use peoples' names to identify their cultural origins and examine how their cultural origins affect their behavior and performance (e.g., Brochet et al., 2016; Du et al., 2017; Ellahie et al., 2017; Jia et al., 2017). For example, Brochet et al. (2016) use surnames to identify CEOs from individualistic cultures. They find that CEOs from individualistic cultures disclose information more optimistically and in a self-referencing manner. Du et al. (2017) also use surnames to identify analysts with Chinese origin. They find that Chinese analysts issue more accurate forecasts for Chinese firms. Our study is distinct from these studies as we use names to capture others' subjective evaluation of an individual rather than the cultural beliefs of the individual.

Our study is closely related to Kumar et al. (2015), except with important differences. They focus on Americans' in-group bias against foreigners and find that fund managers with foreign-sounding names have lower fund flows than do others. In contrast, we focus on the favorability of a name based on countries of origin that are associated with that name and examine its effect on the strength of market reaction. While the foreignness of a name is dichotomous and does not vary over time, surname favorability has a significant cross-sectional and time variation even among the same group of foreigners. We also confirm that the effect of surname favorability is not fully explained by Americans' in-group bias like in Kumar et al. (2015).

Our findings also contribute to the literature on the determinants of stock market reaction to analyst forecast revisions. Prior studies on market reaction to forecast revisions have mostly focused on the effects of analyst attributes that are related to forecast quality, such as forecasting experience and accuracy (e.g., Park and Stice, 2000; Clement and Tse, 2003). Extant studies have found that institutional ownership tends to strengthen the effect of these attributes on market reactions to forecast revisions (e.g., Bonner et al., 2003), suggesting that more sophisticated investors are better able to utilize the information regarding the quality and credibility of an analyst's forecasts. In contrast, our paper provides a new insight that market reaction to an analyst's forecasts is influenced by investors' perception of that analyst's surname, which is not related to the analyst's forecasting ability. It is noteworthy that, unlike most analyst attributes related to forecasting performance, surname favorability mainly affects investment decisions of relatively less sophisticated individual investors.

We also contribute to the growing literature that investigates how economic outcomes, such as the price discount of securities or cross-border mergers and acquisitions, are affected by the popularity of a country (e.g., Hwang, 2011). In addition, our findings have implications for the labor market outcomes of these practitioners (e.g., Mikhail et al., 1999). We show that having a favorable surname has a complementary relation with forecast performance and serves as a catalyst in career progression.

2. Data and Summary Statistics

In this section, we describe our sample selection procedure and introduce two main datasets: (1) a hand-collected dataset of countries of origin for equity analysts in the United States and (2) a dataset of Americans' favorability of foreign countries, complied using Gallup survey results. We also describe how we construct our measure of surname favorability using the two datasets, and we present descriptive statistics for the main variables.

2.1. Sample Selection

We begin our sample construction by collecting data on equity analysts' one-year-ahead forecasts of annual earnings and the U.S. firms' actual earnings from Thomson Reuters' Institutional Brokers Estimate System (I/B/E/S). To avoid the potential rounding problems pointed out by Diether et al. (2002), we directly adjust the I/B/E/S estimates using adjustment factors in the Center for Research on Security Prices (CRSP) without rounding to the nearest penny. We drop an earnings forecast if it is issued after a firm's actual earnings announcement date, because the forecast is likely to be subject to data error. We also delete earnings forecasts associated with more than one analyst surname in I/B/E/S in order to establish a clear link between the identity of an analyst and the market's reaction to the analyst's forecast revision.

We merge the analyst data with Compustat, CRSP, and Thomson Reuters' Institutional (13F) Holdings file to obtain information on firms' annual fundamentals, stock price, and institutional ownership, respectively. After deleting observations with missing values, we have the final sample of 762,780 firm-year-analyst-forecast horizons for 5,516 unique analysts and 6,495 unique firms from 1996 to 2014.

2.2. Data on Countries of Origin for the Surnames of Equity Analysts in the United States

Following Pan et al. (2017), we identify the countries of origin associated with a surname using U.S. historical immigration records retrieved from ancestry.com. For each surname of an equity analyst in our sample, we identify the nationalities of all immigrants who entered the United States through the port of New York between 1820 and 1957 and have the same surname.[4]

In our study, we have 5,516 unique analysts (3,983 unique surnames) in the sample and collect 13,353,663 immigration records from U.S. immigrants whose surname is identical to our sample analysts'. We then drop immigration records with a missing nationality and manually check and correct minor typos. Also, we standardize and regroup some nationalities if appropriate (e.g., England, Scotland, and Wales into Great Britain).[5] This procedure allows us to reclassify 1,629 unique nationalities in the raw dataset of the immigration records into 116 countries of origin.[6] In

[4] Since we examine the effect of the market perception of analyst surnames, our study does not rely on the countries of origin associated with an analyst's surname correctly representing the analyst's actual countries of origin.
[5] We refer to the data-processing procedure in Pan et al. (2017).
[6] Among the 116 countries in our classification, we have "USA," "Unidentifiable" (for non-missing but indiscernible nationalities), and other uninformative categories that refer to either geographic regions (e.g., Asia and Central America) or ethnic groups (e.g., Hispanic and Jewish). We do not use these categories in our empirical analyses, unless

Appendix A, we list the 116 countries of origin and provide summary statistics for the distributions of countries of origin that are associated with the surnames of equity analysts in our sample.

2.3. Data on Favorability of Foreign Countries

We measure investors' favorability toward analysts' countries of origin inferred from their surnames using responses to a particular question from a Gallup survey: "I'd like your overall opinion of some foreign countries. Is your overall opinion of the following country very favorable, mostly favorable, mostly unfavorable, or very unfavorable?" We use Gallup data from 1996, because Gallup Analytics covers few countries prior to 1996, and the analyst data (e.g., forecast dates) in I/B/E/S are also known to be relatively inaccurate for the early 1990s (Clement and Tse, 2003; Cooper et al., 2001; Jiang et al., 2016).[7]

Table 1 reports the summary statistics for Americans' favorability of foreign countries from Gallup Analytics from 1996 to 2014. Each number indicates the average percentage of survey participants who selected a given item on the five-point favorability rating scale. Notably, Americans' perceptions about favorable foreign countries are considerably varied. For example, Iran is perceived as "Very Favorable" by only 1.8% of Americans participating in the Gallup surveys, whereas Canada is viewed as "Very Favorable" by 45.0% of Americans.

In our study, we use the total percentage of survey participants who answered "Very Favorable" or "Mostly Favorable" as a primary measure of Americans' favorability toward the country. Figure 1 depicts the distribution of Americans' favorability across countries. Each bar

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otherwise stated. When an immigrant has a dual nationality (e.g., Russia and Poland), we select the former by default. However, we choose the latter (1) when the former is USA but the latter is not, or (2) when the latter is the only country covered in Gallup surveys.

[7] Prior to 1996, Gallup carried out its surveys covering 11, 17, 6, and 3 countries in 1989, 1991, 1992, and 1993, respectively. It has conducted a survey almost every year since 1996.

indicates the average level of Americans' favorability toward a country during the sample period from 1996 to 2014.

2.4. A Measure of Surname Favorability and Summary Statistics

We now construct a measure of surname favorability (*FavSurname*) using the two datasets described in the previous sections. Specifically, for an individual analyst, we measure the favorability of the analyst's surname (*FavSurname*) as the weighted average of the Gallup's favorability ratings for countries that match the nationalities of U.S. immigrants who share the analyst's surname.[8] We compute our measure of surname favorability using the most recent favorability ratings available in the Gallup surveys as of each forecast revision date. Following Pan et al. (2017), we assign a weight to each of the countries associated with a surname according to the frequency of the country reported as the nationality of U.S. immigrants sharing the same surname. The sum of the weights equals one and we only consider countries with non-missing favorability ratings. In Appendix B, we provide examples of analysts' surnames across the deciles of the mean level of surname favorability (*FavSurname*). For brevity, we report only the 10 surnames with the lowest mean favorability levels in each decile.

One notable feature of our measure of surname favorability (*FavSurname*) is that it enables us to capture time-series variation in Americans' opinions about a given analyst's surname. In Table 2, we provide descriptive evidence for the time variation in our measure of surname favorability. In Panel A of Table 2, we illustrate a real-life example of the most and least variable

[8] We note that South Korea and North Korea are not distinguished in the U.S. historical immigration records, and Gallup Analytics provides favorability ratings on South and North Korea separately. In our study, we assume that all immigrants who entered the United States from Korea between the 1800s and the 1900s are South Koreans, because North Korean refugees have only been recently allowed to enter the United States after the North Korean Human Rights Act was passed in October 2004. In untabulated tests, we drop immigration records related to Korea and find that our results remain unchanged.

surnames in our sample. The most and least variable surnames are determined based on the standard deviation of yearly mean levels of surname favorability (*FavSurname*) for each surname. In Panel B, we report the mean spread (i.e., highest level – lowest level) of our measure of surname favorability for all analysts' surnames in our sample across the deciles of the variation of surname favorability. The deciles are ranked by the standard deviation of yearly mean levels of surname favorability (*FavSurname*). The results in Table 2 suggest that a large number of analysts experience considerable time variations in their surname favorability. For example, analysts in the top-three deciles of variability have an average favorability spread over 10% (13.2%~28.6%).

We report summary statistics for our main variables in Table 3. Panel A of Table 3 shows that the favorability of an analyst's surname, *FavSurname*, has the mean and the median of 0.784 and 0.808, respectively. This suggests that our sample analysts, on average, have surnames originating from countries favorably viewed by about 80% of Americans. Overall, we find that summary statistics for control variables are consistent with prior studies (Clement et al., 2011; Jiang et al., 2016).[9]

In Panel B of Table 3, we divide the sample into two groups based on the sample median of surname favorability (*FavSurname*) and compare analyst, forecast, and firm characteristics across the two groups. We find that there is no significant difference in terms of forecast quality, such as *Accuracy* and *Forecast bias*, across analysts in the two groups. For other characteristics, we find that analysts with more favorable surnames tend to update their forecasts more quickly following the issuance of others' forecasts (*Days since last forecast*), make forecasts earlier in a fiscal year (*Forecast horizon*), and have more years of experience in the profession (*General*

[9] We take the natural logarithm of some control variables, such as brokerage size, days since last forecast, forecast horizon, forecast frequency, firm size, firm-specific experience, and general experience, to adjust their skewed distributions. In untabulated tests, we find that our results are not affected by the logarithm transformation.

experience). They also tend to cover larger firms with a higher book-to-market ratio and higher institutional ownership, compared to analysts with less favorable surnames. Overall, Panel B of Table 3 emphasizes the importance of controlling for analyst, forecast, and firm characteristics in our regression analyses.

3. Surname Favorability and Stock Market Reaction

In this section, we examine whether analysts with more favorable surnames elicit stronger market reactions to their earnings forecast revisions. We also provide a battery of robustness checks and investigate cross-sectional variations in our findings.

3.1. Market Reaction Regression Estimates: Baseline Results

A large volume of studies in psychology show that motivation and affect can strongly influence people's judgment.[10] In particular, people are motivated to make certain conclusions to seek consistency in their attitudes and beliefs, because conflicting cognitions generate unpleasant feelings (e.g., Kunda, 1990). Thus, we conjecture that investors' favorability toward an analyst's surname influences their evaluation of the analyst and further biases their cognitive processing of information provided by the analyst, leading to different market reactions to forecast revisions.

Our main hypothesis is that analysts with more favorable surnames elicit stronger market reactions to their earnings forecast revisions. To examine the relation between the favorability of analysts' surnames and market reactions to their forecast revisions, we estimate a baseline ordinary least squares (OLS) regression in which the dependent variable is a size-adjusted cumulative abnormal return (*CAR*) around the forecast revision date.[11] Following recent studies (e.g., Jiang

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[10] Kunda (1990), Klauer and Stern (1992), Kunda and Sinclair (1999), Sinclair and Kunda (1999), Loewenstein et al. (2001), Slovic et al. (2007), and Greifeneder et al. (2011). See Kunda (1999) for a review.
[11] In untabulated tests, we find that results are essentially unaffected when we use an equal-weighted market return

et al., 2016), we measure the market's reaction over four different estimation windows, beginning on trading day -1 and ending on trading day +1, +3, +5, and +10 of the revision date. We focus on the coefficient estimate of the interaction term between the favorability of an analyst's surname and her forecast revision (*FavOrigin×Revision*) in our regressions. We predict a positive coefficient in accordance with our hypothesis.

Our set of control variables include firm, analyst, and forecast characteristics known to affect the market's response to forecast revisions (e.g., Clement and Tse, 2003; Gleason and Lee, 2003; Clement et al., 2011; Jiang et al., 2016). Specifically, we include firm characteristics such as firm size, book-to-market ratio, institutional ownership, past 12-month returns preceding a forecast revision date (*Momentum*), and the number of analysts following a firm. As controls for analyst characteristics, we use an analyst's gender, brokerage size, forecast frequency, firm-specific experience, general experience, and the number of firms and the number of industries an analyst follows. We also control for the past forecast accuracy of an analyst for a given firm (i.e., lagged accuracy) to rule out the possibility that the stronger market reactions we capture are driven by the analyst's superior forecasting ability. Lastly, we control for forecast characteristics, such as the number of days elapsed since the last forecast was issued (*Days since last forecast*), as a proxy for the new information content in a forecast revision (Cooper et al., 2001) and the forecast horizon to capture a walk-down pattern in analyst forecasts (e.g., Ke and Yu, 2006). In all regression specifications, we include firm and year fixed effects to capture unobservable and time-invariant firm and year attributes. Following Bradshaw et al. (2013), we cluster standard errors at the analyst level to allow for correlations in residuals within each analyst group.[12]

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as the return benchmark and calculate a buy-and-hold abnormal return (*BHAR*) instead of a *CAR*.

[12] In untabulated tests, we find that results hold, regardless of whether standard errors are clustered at the firm level or at the analyst-firm level.

In Panel A of Table 4, we estimate the baseline OLS regression to examine the impact of the favorability of analysts' surnames on market reactions to their forecast revisions. Consistent with our prediction, we find that the coefficient on the interaction term between *Revision* and *FavSurname* is statistically significant and positive across all columns. In untabulated tests, we find qualitatively the same results using a rank or dichotomized variable of *FavSurname* (e.g., quintile ranks, or an indicator for the surname favorability that is greater than the sample median). Our results suggest that, all else being equal, investors more strongly respond to forecast revisions issued by analysts who have more favorable surnames. Our results are also economically meaningful. For example, based on the result in column (2), which includes the entire set of control variables, a one-standard-deviation increase in *FavSurname* translates into a 6.30% increase in the return-revision relation.[13] The magnitude (6.30%) of the change in the return-revision relation due to surname favorability is quite comparable to those resulting from other major analyst characteristics, such as *General experience* (5.56%) and *Number of firms* (-6.61%), which are two important proxies for analysts' skills and task complexity (e.g., Clement, 1999).[14]

3.2. Alternative Explanations and Robustness Checks

In this section, we examine alternative explanations for our findings and provide robustness checks. First, following O'Brien (1990) and Clement and Tse (2003), we re-estimate our baseline regressions using the last forecast of an analyst for a firm-fiscal year pair and report the results in Panel B of Table 4. Although the sample size considerably shrinks by 72.00% to

[13] 0.431 (coefficient on *Revision*×*FavSurname*) × 0.113 (standard deviation of *FavSurname*) ÷ 0.773 (coefficient on *Revision*) = 0.063

[14] We find that the incremental change in adjusted R^2, resulting from the inclusion of our measure of surname favorability, is 0.01%, which may look small but is highly comparable to that from other major analyst characteristics (e.g., *General experience* = 0.01%, *Number of firms* = 0.00%). We believe that such small changes in adjusted R^2 are partly attributable to abnormal stock returns, which contain a large amount of unexplainable variability.

213,549 firm-year-analysts in Panel B, we still find statistically significant results.

Second, we address the concern that our results might be spuriously driven by market reactions contaminated by other analysts' forecasts or corporate announcements. We drop forecast revisions issued on firm-days during which other analysts' forecasts, the firm's quarterly earnings, or managerial forecasts are released. We retrieve the actual dates of firms' quarterly earnings announcements and managerial forecasts from I/B/E/S and First Call, respectively.[15] As reported in Panel C of Table 4, we find that results remain similar to those obtained in Panel A. The results suggest that our findings are not driven by the confounding effects of other analysts' forecasts and corporate announcements.

Third, we address concerns that our results may be sensitive to the way we construct our measure of surname favorability. In the Gallup surveys, Americans' favorability of a country is assessed using five-point Likert scales: Very Favorable, Mostly Favorable, Mostly Unfavorable, Very Unfavorable, and Others. One possible concern is that survey results based on Likert scales can be subject to the central tendency bias (e.g., Peer et al., 2014), which refers to the tendency of survey participants to choose moderate Likert items, such as "Mostly Favorable" and "Mostly Unfavorable," when they have a neutral or undecided opinion about a question. To address this concern, we consider survey responses for the most extreme Likert item, "Very Favorable," to measure the favorability of a country. Results, which are reported on the first row of Panel D in Table 4, look very similar to our main results in Panel A.[16]

In addition, we construct an alternative measure of favorability by taking into account the

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[15] In this analysis, we limit our sample to the period up to 2010 because of the data availability of managerial forecasts in the First Call's Company Issued Guidance (CIG) database.

[16] In untabulated tests, we further construct a measure of surname unfavorability (*Un_FavSurname*) using the most extreme item on the opposite side, "Very Unfavorable," and find significant and negative coefficients on *Revision*×*Un_FavSurname* across all columns.

entire distribution of the Gallup survey responses. Following Hwang (2011), we compute a composite score of favorability as 4 × (%Very Favorable) + 3 × (%Mostly Favorable) + 2 × (%Mostly Unfavorable) + 1 × (%Very Unfavorable). We report results using this composite score of favorability on the second row of Panel D in Table 4.[17] Overall, the results reported in Panel D of Table 4 show that our results are not sensitive to how we construct our measure of surname favorability.

Next, we also consider a subset of dominant country origins that are strongly associated with a given surname in calculating the surname favorability. In Panel E of Table 4, we find that our results are essentially unaffected by using either one or three of the most dominant origins associated with a surname.

Finally, we perform a falsification test. We construct a placebo measure of surname favorability, *FavSurname (P)*, using the Gallup's favorability ratings for falsified countries that come immediately after the actual countries of origin in an alphabetically ordered list of 116 countries shown in Appendix A (e.g., Germany is replaced with Great Britain). We re-estimate the baseline OLS market reaction regressions using the placebo measure and report the results in Table 5. We find no statistical significance for the coefficients on the interaction term, *Revision*×*FavSurname (P)*, across all columns. This provides further reinforcement that our finding about the surname favorability effect is not a mere statistical artifact.

3.3. Subsample Analyses

Thus far, we have established that investors more strongly respond to forecast revisions

[17] In untabulated tests, we find that the results remain qualitatively the same when we use a different composite score, measured as 2 × (%Very Favorable) + 1 × (%Mostly Favorable) -1 × (%Mostly Unfavorable) -2 × (%Very Unfavorable).

issued by analysts who have more favorable surnames. This finding naturally poses a question of whether the extent to which surname favorability influences investors' reactions varies with the level of investor sophistication.

Prior studies have found evidence that individual investors are less sophisticated compared with institutional investors, in that they have limited ability to correctly process information and incorporate its implication into stock prices, thereby biasing market reactions to the information to a greater extent (e.g., Bartov et al., 2000; Bonner et al., 2003; Collins et al., 2003). Following the aforementioned studies, we use the percentage of common shares held by institutional investors as a proxy for investor sophistication and predict that the surname favorability effect will be more pronounced for firms that are largely owned by individual investors. This approach is based on the assumption that the stock price is determined by a weighted average of all investors' beliefs, as shown in several theoretical models (e.g., Kim and Verrecchia, 1994, 1997; Hirshleifer and Teoh, 2003). Under this assumption, the beliefs of institutional investors (sophisticated investors) should have a stronger influence on the stock price of a firm with higher institutional ownership.

We divide the sample into two groups based on the sample median of institutional ownership and report the results of our baseline regressions for each subsample in Panel A of Table 6. Consistent with our prediction, we find a significant surname favorability effect using the subsample of low institutional ownership, whereas the results are mostly insignificant using the subsample of high institutional ownership.[18] The evidence indicates that individual investors, who are less sophisticated, are more susceptible to subjective feelings or opinions, including surname favorability.

[18] In untabulated tests, we divide the sample into tertiles or quintiles of institutional ownership. We find a monotonic relationship between the surname favorability effect and institutional ownership.

Next, we investigate whether the surname favorability effect varies with the level of difficulty in inferring a country of origin from a surname. For example, if an analyst has a surname commonly used by people in a certain country, investors would be able to easily infer countries of origin from the surname and use the information to form their perception about the surname. In contrast, if a surname is associated with a large number of countries, investors find it more difficult to clearly infer the country of origin associated with the surname. For example, an analyst has the surname "Yamamoto," and 99.0% of U.S. immigrants sharing the same surname are from Japan, according to U.S. historical immigration records from ancestry.com. In this case, we believe that investors would infer that the analyst is of Japanese origin. In contrast, it is not straightforward to identify a specific country of origin for an analyst whose surname is "Boris." According to U.S. immigration records, Poland is the most common country of origin for the surname Boris, yet it only accounts for 12.8% of the U.S. immigrants sharing the same surname.

We assume that it is easier to infer origins from a surname when a single country accounts for a greater fraction of the nationalities of the U.S. immigrants who have the same surname (e.g., 99.0% Japanese for Yamamoto). We divide all analysts into two subsamples by the sample-median fraction of the U.S. immigrants whose nationalities match one single most common country for a surname. In the subsample of easy names, we find that coefficients on *Revision*×*FavSurname* are positive and significant across all columns (Panel B of Table 6). However, in the subsample of difficult names, we only find marginally significant or insignificant results. The results are consistent with our prediction that surname favorability would play a greater role when investors can easily infer countries of origin from a surname.

We now investigate whether the surname favorability effect is weaker for analysts who are more visible and have a higher reputation. Our conjecture is that the importance of having a favorable surname will diminish for analysts who are already well recognized in the profession. To test this conjecture, we construct two subsamples based on whether an analyst has been ranked as an all-star analyst in *Institutional Investor* magazine (i.e., all-star vs. non-all-star analysts). Consistent with our conjecture, we find stronger results in the subsample of non-all-star analysts compared to the subsample of all-star analysts (Panel C of Table 6).[19,20]

Next, we examine whether the surname favorability effect varies with the sign of news contained in forecast revisions. According to the theory of motivated reasoning, people find creative ways to distort information in self-serving ways in order to arrive at their desired conclusions (Kunda, 1990). In other words, people take information that is consistent with their preferences at face value, but attempt to scrutinize and discredit information that is inconsistent with their preferences (e.g., Kunda, 1990; Sinclair and Kunda, 1999; Kunda and Spencer, 2003; Hales, 2007; Kadous et al., 2008). For example, Sinclair and Kunda (1999) find in their experiment that a black doctor who delivers praise is viewed as a doctor, but the same doctor is viewed as a black person and rated as relatively incompetent if he criticizes the participants. Their evidence suggests that when people receive information containing bad news, they attempt to dispel the information by discrediting the individual who delivers the information.

As shown in Panel A, Table 6, the surname favorability effect is predominantly driven by individual investors who face greater short-sale constraints compared to institutional investors

[19] We alleviate concern that stronger results for non-all-star analysts are attributable to its relatively larger sample size (558,444 obs.). To make its sample size more comparable to that of all-star analysts (204,336 obs.), we identify a subset of non-all-star analysts who have never been elected an all-star despite many years of having worked in the profession as an analyst (e.g., General experience greater than 10 years). This yields a smaller subsample of non-all-star analysts (173,113 obs.), in which we still find a strong surname favorability effect (untabulated).

[20] In untabulated tests, we divide all-star analysts into two groups based on the degree of their reputation, which is proxied by the total number of all-star elections they have received before each forecast revision date. The weak, but significant, results for all-star analysts in Panel C are mostly concentrated on those who have been elected four times or less. The results suggest that the surname favorability effect gradually dissipates even within an all-star analyst group as analysts gain recognition.

(Nagel, 2005). Thus, it is quite reasonable to assume that downward forecast revisions for the stock they have purchased would be against their preferences, making them prone to discrediting the analyst who issued the unfavorable forecasts by activating negative perceptions associated with the analyst's surname. This implies that the surname favorability effect will be stronger for negative forecast revisions. Consistent with our conjecture, we find stronger results in the subsample of negative forecast revisions in Panel D of Table 6.

We further examine a cross-sectional variation of our finding across analysts' gender in Panel E of Table 6. We only find significant results in the subsample of male analysts.[21] We have two possible explanations for insignificant results for female analysts. First, it could be due to an intensified measurement error arising from the widespread cultural practice of women changing their surnames upon marriage. Second, it might reflect a gender variation in our surname favorability effect. In a related context, prior studies have found that the degree of racial discrimination or stereotypes varies with gender (e.g., Honora, 2002; Swanson et al., 2003; Chavous et al., 2008). For example, Honora (2002) and Swanson et al. (2003) find that ethnic minority men receive more negative treatment than women of the same ethnic group from others, suggesting that having an unfavorable surname could be more detrimental to male analysts than to female analysts.

Lastly, we examine whether our finding weakens when analysts use American first names. Considering that first names may convey information about which American generation the

[21] To mitigate the concern that stronger results for male analysts are due to the relatively larger sample size of male analysts, in untabulated tests, we further confine our test to a subset of male analysts' forecast revisions that are analogous in sample size and in characteristics to those of female analysts. We use a coarsened exact matching (CEM) and the following matching covariates: forecast accuracy, brokerage size, forecast horizon, forecast frequency, general experience, lagged accuracy, and number of firms. We identify 95,637 male analysts' forecast revisions matched with 95,637 female analysts' and still find significant results for male analysts.

individual is (e.g., foreign-born first generation vs. American-born second generation), we predict that having an American first name, such as John and Jane, will dampen the effect of surname favorability. We define a first name as Americanized if it appears on the list of 100 most popular first names for male and female babies born in the United States over the past 100 years (1917–2016) (the list is available from the Social Security website at www.ssa.gov). Consistent with our prediction, we find weaker results using analysts who have Americanized first names (Panel F of Table 6). Despite a relatively larger sample size for analysts with Americanized first names, the coefficient estimates for *Revision×FavSurname* are only significant in two of the four columns in which control variables are included. In contrast, all columns that include control variables (columns (2), (4), (6), and (8)) show significant results for analysts with non-Americanized first names.[22]

4. Additional Identification Tests

In this section, we further enhance the identification of the surname favorability effect by using various fixed effects and two natural experiments.

4.1. Variations within Analyst-Year, Firm-Year, or Analyst-Firm

An analyst's earnings forecasts for a firm are revised several times throughout a year, and Gallup usually conducts a survey on perceptions of country favorability once or twice a year. Motivated by these features of our sample, we use various fixed effects models to further control for any confounding effects arising at the analyst-year, firm-year, or analyst-firm level.

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[22] As robustness checks, we define Americanized first names using the top-50 most popular male and female names and find similar results. We also conduct a subsample analysis using analysts' full name. We divide the sample into American-sounding full names and foreign-sounding full names using our measure of *Foreignness*. We find strong results using the subsample of foreign-sounding full names whose sample size is more than 10 times smaller than its counterpart.

In Table 7, we re-estimate the baseline regression after including various combinations of additional fixed effects.[23] In columns (1) and (2), we control for analyst×year fixed effects and/or industry fixed effects, which absorb the effects of analyst-year factors, such as an analyst's background, education, and experience. Strong evidence for the surname favorability effect confirms that our results are not driven by confounding effects of unobservable analyst characteristics. Similarly, we also include firm×year fixed effects or analyst×firm fixed effects in the remaining columns of Table 7 to additionally control for other unobservable factors at the firm-year or analyst-firm level, including firm characteristics and the connection between the analyst and the firm. Overall, we find that results remain qualitatively the same after controlling for unobservable factors at the analyst-year, firm-year, or analyst-firm level.

4.2. Two Natural Experiments for Surname Favorability

To better establish a causal relation between the favorability of an analyst's surname and the market's reaction to his or her forecast revisions, we carry out difference-in-differences tests using two natural experiments: (1) the 9/11 terrorist attacks and (2) the French and German governments' opposition to the Iraq War.

In the aftermath of the 9/11 terrorist attacks, the Federal Bureau of Investigation (FBI) reported a surge in hate crimes and harassment against Muslims, Arabs, and others who were thought to be of Middle Eastern origins (Anderson, 2002). Academic articles also report evidence that discrimination and prejudice against Muslims substantially rose following the 9/11 attacks (Sheridan, 2006; Kumar et al., 2015). In Panel A of Figure 2, we plot the average level of Americans' favorability of Middle Eastern countries. The figure shows a 6.3% drop in favorability

[23] Results are qualitatively similar when we measure the dependent variable over different return windows, such as [-1,+3], [-1,+5], and [-1,+10]. For brevity, we omit the results from Table 7.

in 2002 following the 9/11 attacks, which corresponds to a 23.0% decrease relative to its favorability rating in 2001 (27.4%). Thus, we use the 9/11 attacks as our first natural experiment in which we can exploit an exogenous shock that adversely affected Americans' favorability toward Middle Eastern surnames.

As for the second natural experiment, we utilize the French and German governments' opposition to the U.S.-led Iraq War around February and March 2003. Their opposition prompted Americans to boycott French and German products and worsened bilateral trades between the United States and the two countries (Chavis and Leslie, 2009; Michaels and Zhi, 2010). Panel B of Figure 2 also shows supportive evidence that the average level of Americans' favorability of France and Germany fell by 39.3%, reaching a record low of 41.4% in 2003 from its highest, 80.7%, in 2002.

We construct a matched sample for each of our two natural experiments. Recent studies find that propensity score matching (PSM) is subject to the random matching problem and generates fragile results that are sensitive to fairly minor changes in design choices (DeFond et al., 2016; Shipman et al., 2016). Thus, we use a coarsened exact matching (CEM) algorithm that outperforms PSM by achieving better covariate balance between the treatment and control groups (Iacus et al., 2011).

In the 9/11 terrorist attacks setting, our treatment analysts are those who have Middle Eastern surnames (Middle Eastern analysts, hereafter). We define a surname as Middle Eastern if more than 30% of U.S. immigrants sharing the same surname are either from a Middle Eastern country or identified as "Arab" or "Muslim," according to U.S. immigration records.[24] The list of

[24] In untabulated tests, we use different cutoffs, such as 20%, 40%, and 50%, and find qualitatively the same results. When we set a cutoff higher than 40%, fewer than 10 individual analysts are identified as Middle Eastern analysts at the end of 2000, making our empirical analyses almost infeasible.

Middle Eastern countries includes Afghanistan, Egypt, Iran, Iraq, Jordan, Kuwait, Libya, Pakistan, Palestine, Saudi Arabia, Syria, Turkey, and Yemen. In untabulated tests, we also include South Asian countries, such as India and Indonesia, and find that their inclusion does not alter the inferences of our results.

We identify 16 Middle Eastern analysts at the end of year 2000. We match Middle Eastern analysts and control analysts on the following matching covariates: surname favorability (*FavSurname*), mean accuracy of the last forecasts for firms covered, brokerage size, forecast frequency, general experience, and the number of firms covered in a year. To retain a sufficient number of matches and to avoid the curse of dimensionality issues, we exclude firm-specific experience and the number of industries covered in a year from the set of matching covariates.[25]

Following the CEM approach, we temporarily coarsen each of the covariates into four equal-sized intervals and discard any observation whose stratum does not have at least one Middle Eastern analyst and one control analyst (Iacus et al., 2011). Given the small number of Middle Eastern analysts, we conduct one-to-many matching by allowing a Middle Eastern analyst to have multiple control analysts that are similar on the matching covariates.[26] The procedure yields a matched sample of 13 Middle Eastern (treatment) and 204 control analysts. Panel A of Table 8 reports descriptive statistics for matching covariates between the two analyst groups. We find that none of the matching covariates are significantly different across our treatment and control analyst groups.

In our second setting of the French and German governments' opposition to the Iraq War,

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[25] As a robustness check, we further include firm-specific experience and the number of industries as additional matching covariates and find that our results remain similar.

[26] Whenever we use a one-to-many matched sample, we compensate for the different sizes of strata by imposing CEM weights calculated based on the formula in Iacus et al. (2011).

we define analysts having French or German surnames as our treatment analysts (French and German analysts, hereafter). Given the relatively higher number of French and German analysts in our sample, we impose stricter conditions on the matching procedure: First, we conduct a one-to-one matching.[27] Second, we use a cutoff of 40% to define a surname as French or German. All other matching criteria and covariates are the same as the 9/11 setting. We conduct the matching at the end of 2002, which immediately precedes the French and German governments' opposition to the Iraq War. Using the same set of matching covariates, we successfully identify 248 control analysts whose characteristics are highly analogous to those of 248 French and German analysts. In Panel B of Table 8, we report descriptive statistics for matching covariates between our treatment and control analysts. All matching covariates, except for the initial level of surname favorability, are insignificantly different across two groups.[28]

To check the validity of our natural experiments, we investigate whether surname favorability significantly decreases for our treatment analysts after a shock, relative to the change for their control analysts. We construct the annual favorability level of an analyst's surname using the most recent Gallup survey data available. In Panel C of Table 8, we estimate OLS regressions in which the dependent variable is the annual surname favorability (*FavSurname*) and the independent variable of interest is *Treatment*×*Post-shock*. We confine our difference-in-differences tests to analyst-year observations in the pre- and post-shock periods with equal five-year length, excluding those in the transition year of 2001 (2003) for the 9/11 terrorist attacks (the

[27] In untabulated tests, we also perform a one-to-many matching. Treatment analysts (248) are matched with 1,152 control analysts. We find weaker but qualitatively similar results.

[28] In a difference-in-differences test, we focus on a time-series change in the market's reaction to French and German analysts, compared to before, after French and German opposition to the Iraq War. Thus, we believe that the higher initial level of *FavSurname* for our control group is of less concern. As a robustness check, in untabulated tests, we coarsen *FavSurname* more strictly into six equal-length intervals and find similar results.

French and German opposition to the Iraq War) setting. We find that the coefficients on the interaction term between *Treatment* and *Post-shock* are significantly negative. The results suggest that Middle Eastern (French and German) analysts experience a significant decline in the level of their surname favorability after the 9/11 attacks (the French and German opposition to the Iraq War), compared to control analysts during contemporaneous periods. Overall, the empirical evidence in Panel C of Table 8 provides a further justification for using the two settings as our natural experiments for surname favorability.

Next, to examine whether market responses to forecast revisions by treatment analysts become weaker after the shock, we carry out difference-in-differences tests using our matched samples. We first retrieve all forecast revisions that our treatment and control analysts have issued in the pre- and post-shock periods. Like in Panel C of Table 8, we exclude forecast revisions in the transition year of 2001 (2003) and define the pre- versus post-shock periods as 1996–2000 versus 2002–2006 (1998–2002 vs. 2004–2008) for the 9/11 terrorist attacks (the French and German opposition to the Iraq War) setting. We estimate the same baseline regression model used in Table 4, except that we replace *FavSurname* with *Treatment*, *Post-shock*, and *Treatment*×*Post-shock*. We report the results in Panel D of Table 8.[29]

Consistent with our conjecture, we find negative and significant coefficients on the three-way interaction term of *Revision*×*Treatment*×*Post-shocks* across all columns for two different matched samples. They suggest that Middle Eastern (French and German) analysts experience a significant decrease in the magnitude of market responses to their forecast revisions after the 9/11 attacks (the French and German governments' opposition to the Iraq War), as compared to their

[29] For brevity, we omit our results based on the dependent variables using different return windows, such as [-1,+3], [-1,+5] and [-1,+10]. In untabulated tests, we find qualitatively similar results for those windows.

control analysts during contemporaneous periods.[30] We believe that strong results from two different natural experiments, involving different sets of analysts (i.e., Middle Eastern analysts and European analysts), considerably strengthen our identification of the surname favorability effect.

5. Surname Favorability, Forecast Quality, and Career Outcomes

5.1. Forecast Accuracy, Bias, and Timeliness

Although we use baseline regression models that control for a number of analyst- and forecast-specific characteristics, including past forecast accuracy, we now directly investigate whether surname favorability is associated with the quality of forecast revisions, such as forecast accuracy, bias, and timeliness. If we find that analysts with more favorable surnames issue more accurate, less biased, or timelier earnings forecasts, our surname favorability effect on market reactions may be attributable to investors' rational information processing that puts a greater weight on higher-quality forecasts. On the other hand, if forecast quality is not associated with the level of an analyst's surname favorability, it will lend further support to our interpretation of the results that investors make biased judgments based on their subjective attitudes or perception (favorability) about an analyst's surname.

We first examine the relation between an analyst's surname favorability and his or her forecast accuracy. We measure forecast accuracy as the negative value of the absolute difference between an analyst's last one-year-ahead forecast of annual earnings and the actual earnings, scaled by the stock price two trading days prior to the forecast date. In our regression model of forecast accuracy, we control for firm, analyst, and forecast characteristics known to affect forecast

[30] In untabulated tests, we divide each matched sample into two subsamples by the sign of the forecast revisions. We find that our results are slightly stronger in the subsamples of negative forecast revisions.

accuracy (e.g., Clement, 1999; Kumar, 2010; Jiang et al., 2016): book-to-market ratio, brokerage size, days since last forecast, female analyst, forecast horizon, forecast frequency, firm size, firm-specific experience, general experience, institutional ownership, lagged accuracy, number of analysts, number of firms, and number of industries.[31] To exclude the effects of unobservable firm characteristics and time trends, we further include firm and year fixed effects in the regressions.

Panel A of Table 9 reports results for the accuracy tests. We find that the coefficient estimate of *FavSurname* is close to zero and statistically insignificant, regardless of the model specifications used. The results suggest that the positive surname favorability effect on the market's reactions are due to investors' subjective perceptions rather than rational weighting based on forecast quality.

Next, we examine the relation between surname favorability and forecast bias, which is measured as the difference between an analyst's last one-year-ahead forecast of annual earnings and the actual earnings (i.e., signed forecast error), scaled by the stock price two trading days prior to the forecast date (e.g., Easterwood and Nutt, 1999). Panel B of Table 9 shows that *FavSurname* is not significantly associated with forecast bias in column (3), where control variables and fixed effects are added to the regression model, suggesting no relation between the two.

Lastly, we check another aspect of forecast quality, the timeliness of a forecast revision (e.g., Cooper et al., 2001) in Panel C of Table 9. We capture the timeliness of a forecast revision using *Days since last forecast*, which is measured as the natural logarithm of one plus the number of days elapsed since the most recent earnings forecast for a firm was issued by another analyst.[32]

[31] In untabulated tests, we also control for a firm's past 12-month return (i.e., momentum) and find qualitatively the same results.

[32] Cooper et al. (2001) find that earnings forecasts issued by lead analysts have a greater impact on stock prices compared with those issued by follower analysts who tend to make forecasts immediately after the release of lead analysts' forecasts. They attribute the finding to follower analysts' free riding on the information produced by lead

We do not find evidence that the favorability of an analyst's surname is significantly associated with forecast timeliness.

Overall, the results in Table 9 show that forecast quality does not systematically vary with the favorability of an analyst's surname. It suggests that our main findings reflect investors' biased information processing due to their subjective opinions about the analysts based on their surnames.[33]

5.2. Career Outcomes

We now investigate the labor market consequences of analysts' having favorable surnames. If investors more strongly react to analysts with more favorable surnames, it is possible that these analysts experience positive career outcomes and succeed in the profession.[34] Furthermore, as Fang and Huang (2017) point out, career advancement requires not only superior job performance but also the favorable subjective evaluations by others. This argument suggests a possible interaction between performance and surname favorability.

In this section, we consider three different measures of analysts' career outcomes (e.g., Fang and Huang, 2017): *All-star* equals 1 if an analyst is ranked as an all-star analyst by *Institutional Investor* magazine in the following year and 0 otherwise. *Post-closure/merger termination* equals 1 if an analyst disappears from the I/B/E/S within three years after his or her brokerage house goes out of business (closure) or goes through a merger and 0 otherwise.[35]

analysts.

[33] We acknowledge that it is not possible to encompass the entire spectrum of forecast quality with observable forecast characteristics. Thus, it is still possible that surname favorability is associated with other unknown aspects of forecast quality or analyst ability (e.g., analysts' ability to communicate with the management) that are value relevant, yet not captured in observable forecast characteristics.

[34] One possible mechanism is through investors' higher subscriptions to the forecast reports of analysts who have favorable surnames.

[35] We manually complied the list of mergers and closures of brokerage houses between 1996 and 2008 by referring to the 1984–2005 list of brokerage mergers in Hong and Kacperzyk (2010) and the 2000–2008 list of brokerage

Covering visible stocks equals 1 if an analyst covers at least one stock that ranks in the top decile by total analyst coverage across all stocks in the following year and 0 otherwise.

We estimate pooled probit regressions in which the dependent variable is one of the three measures of career outcomes. We use the following independent variables of interest in the regression model: *High FavSurname* equals 1 if an analyst has a surname that ranks in the top tercile of surname favorability among all analysts in a year and 0 otherwise. *Mean Accuracy* is the mean accuracy of an analyst's last forecasts issued for all firms he or she covered in a year, and *Mean Accuracy×High FavSurname* is the interaction term between the two variables. We control for brokerage size, female analyst, general experience, number of firms, number of industries, and year fixed effects (e.g., Fang and Huang, 2017).[36] Since an analyst's career outcomes are defined every year, we use observations at the analyst-year level.

Table 10 reports results for the career outcome regressions. Across all columns for three different measure of career outcomes, we find that coefficients on *Mean Accuracy* are positive (negative) and statistically significant for *All-star* and *Covering visible stocks* (*Post-closure/merger termination*).[37] This is consistent with prior findings that forecast accuracy is positively associated with analysts' career outcomes (e.g., Mikhail et al., 1999). As for surname favorability, coefficients on *High FavSurname* are not significant across most of the six columns. However, interestingly, we find that coefficients on the interaction term *Mean Accuracy×High*

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closures in Kelly and Ljungqvist (2012). As a robustness check, we confine our test to brokerage house closures only, where analysts have no option but to get a new job at a different brokerage house. In untabulated tests, we find that results remain inferentially the same as those in Table 10.

[36] In untabulated tests, we find that results remain qualitatively the same when we additionally control for brokerage house fixed effects in the regression models.

[37] In columns (3) and (4), we only estimate the regression models using an analyst's yearly observations from year *t*-1 to *t*+3, where year *t* includes the date of his or her brokerage house's merger or closure. This is to more clearly capture the analyst's job termination resulting from an unexpected shock, such as redundancies (Fang and Huang, 2017). We include an analyst's observation in year *t*-1 to capture a job termination occurring in year *t*.

Surname are statistically significant and positive (negative) for good (bad) career outcomes, such as *All-star* (*Post-closure/merger termination*). The results suggest that surname favorability has a complementary relation with forecasting performance, and, thus, acts as a catalyst in furthering analysts' career progression.

6. Additional Tests

In this section, we summarize the results of additional tests reported in the Online Appendix. First, we identify underlying factors that form the favorability of a surname. We find that surname favorability is positively associated with the ethnic and linguistic similarities between the United States and an analyst's country of origin reflected in his or her surname, whereas it is negatively associated with the foreignness of an analyst's name, cultural distance, and the level of perceived corruption in the countries associated with the surname. Further, we find that the residual component of surname favorability not explained by the aforementioned factors still has significant explanatory power for investor reactions to forecast revisions.

Second, we examine the impact of surname favorability on post-revision price drifts (Gleason and Lee, 2003). Using a subsample of firms and analyst forecasts that has significant drifts, we find that surname favorability is associated with a weaker underreaction to forecast revisions, suggesting that surname favorability also affects price efficiency.

Lastly, we bolster the robustness of our findings by using an alternative measure and data source. We use a name-based ethnicity classification provided by OnoMap, which derives data from telephone directories and electoral registers in 26 countries (Mateos et al., 2011). We find that our results are robust to alternative measures of surname or full name favorability, constructed using the ethnicity classifications in OnoMap.

7. Summary and Conclusion

We examine whether investors' favorability of an analyst's surname affects their reaction to the analyst's forecast revisions. Using U.S. historical immigration records and Gallup survey data, we construct a measure of surname favorability and find strong evidence that analysts with more favorable surnames elicit stronger market reactions to their forecast revisions. The result is consistent with investors' biasing their judgments in an attempt to seek consistency between their perception of an analyst's surname and their evaluation of the analyst's forecast quality, as predicted by the theory of motivated reasoning in the psychology literature.

We strengthen our identification of the surname favorability effect using two natural experiments, the 9/11 terrorist attacks and the French and German opposition to the Iraq War, both of which adversely affected Americans' perceptions of certain countries and thus certain surnames. Further, we find that a favorable perception of an analyst's surname is not associated with the analyst's forecast quality, such as accuracy, bias, and timeliness. Lastly, we find that surname favorability is a complement to forecasting performance, helping analysts proper in their profession, and is negatively associated with the post-revision price drifts when there is a significant underreaction to forecast revisions.

Our study sheds light on a new aspect of a name, the favorability of a surname, which varies over time, unlike other static name traits, such as ethnicity and foreignness. We demonstrate that surname favorability not only influences people's information processing in the capital market but also leads to different labor market consequences for professionals. We do not find clear evidence of market over- or underreaction related to surname favorability in the overall sample. Thus, we cannot completely rule out the possibility that our findings reflect investors' rational response. Using evidence from other related economic settings, future research may determine more clearly whether market response to surname favorability reflects irrational or rational investor behavior.

Another interesting avenue for future research would be to determine whether the effect of surname favorability persists out-of-sample. The market impact of surname favorability could be reinforced as in a Keynesian beauty contest (Keynes, 1936) if investors make decisions based on what they think the market's reaction to an analyst's forecast revision will be based on the analyst's surname. However, the effect can also weaken over time if investors become more aware of the mispricing associated with surname favorability and trade against it to exploit that mispricing.

Future research may also explore other types of name-induced bias. For example, the market may perceive an individual who shares their name with a popular professional athlete or a celebrity more favorably. Lastly, it may be interesting to examine the effects of name-induced bias for other market participants and corporate stakeholders.

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Appendix A
Distribution of countries of origin for the surnames of the U.S. equity analysts

Country of origin	Percentage of analysts whose surname is associated with at least one U.S. immigrant from the country (1)	Mean percentage of U.S. immigrants from the country, conditional on their surnames identical to the analysts' in column (1) (2)
Afghanistan	0.40	0.44
Africa	33.68	0.37
Albania	5.73	0.61
Algeria	0.82	0.09
Arab World	8.96	0.75
Argentina	24.55	0.18
Armenia	18.51	0.38
Asia	11.02	0.76
Australia	40.92	0.17
Austria	60.26	1.45
Austrian	2.39	0.02
Barbados	4.44	0.01
Belgium	42.31	0.76
Bermuda	15.46	0.05
Bolivia	2.01	0.04
Bosnia	7.90	0.11
Brazil	30.46	0.25
Bulgaria	12.65	0.30
Burma	0.36	0.12
Canada	65.08	1.52
Central America	0.80	0.01
Chile	21.65	0.14
China	27.45	10.42
Colombia	16.12	0.17
Costa Rica	7.38	0.06
Croatia	25.18	0.68
Cuba	38.65	0.54
Cyprus	0.20	0.04
Czechoslovakia	48.88	1.41
Denmark	43.51	0.64
Dominican Republic	8.77	0.11
Ecuador	3.66	0.10
Egypt	6.93	0.96
El Salvador	0.89	0.02
Estonia	15.30	0.24
Ethiopia	1.41	0.02
Finland	36.24	0.67
France	70.99	3.06
Germany	84.94	13.79
Great Britain	86.53	27.68
Greece	39.98	1.25
Grenada	0.04	0.00
Guatemala	3.14	0.05
Haiti	5.35	0.11
Hispanic	0.44	0.04
Honduras	13.65	0.11
Hungary	53.01	1.98
Iceland	6.36	0.02
India	23.17	3.05
Indonesia	1.00	0.89
Iran	2.48	0.39
Iraq	3.21	2.53
Ireland	71.30	10.71
Israel	18.06	0.82
Italy	67.84	9.18
Jamaica	17.89	0.03
Japan	16.99	1.42
Jewish	60.46	7.67
Jordan	1.02	0.99
Korea	2.52	0.59
Latin America	31.07	0.44

Latvia	17.73	0.28
Lebanon	2.52	0.37
Liberia	5.73	0.01
Lithuania	27.56	0.48
Luxembourg	0.05	0.03
Macedonia	2.96	0.15
Malaysia	7.31	0.29
Mexico	33.88	0.36
Mongolia	1.74	0.19
Montenegro	6.27	0.13
Morocco	1.25	0.79
Muslim	0.44	0.63
Native American	62.06	4.38
Netherlands	60.48	2.27
New Zealand	15.30	0.02
Nicaragua	4.41	0.02
Norway	49.40	1.03
Pacific Islander	20.98	0.17
Pakistan	1.27	3.38
Palestine	5.33	0.12
Panama	19.83	0.05
Paraguay	1.03	0.07
Peru	10.30	0.06
Philippines	29.51	0.17
Poland	57.74	2.90
Polynesia	2.86	0.02
Portugal	32.14	0.75
Puerto Rico	32.31	0.48
Romania	40.28	0.99
Russia	62.00	3.40
Samoa	0.56	0.03
Scandinavia	61.62	2.46
Senegal	0.34	0.19
Serbia	15.19	0.26
Singapore	1.14	0.02
Slovenia	23.04	0.45
Somalia	0.02	0.03
South Africa	24.71	0.06
South America	4.33	0.01
Spain	62.60	1.90
Sudan	1.90	0.07
Sweden	55.93	1.44
Switzerland	47.08	0.95
Syria	22.90	1.44
Thailand	0.45	0.11
Tunisia	0.45	0.17
Turkey	24.44	0.93
USA	91.01	18.90
Ukraine	6.18	0.04
Unidentifiable	77.52	2.13
Uruguay	4.62	0.03
Venezuela	22.03	0.22
Vietnam	1.81	0.25
West Indies	11.89	0.02
Yugoslavia	9.05	0.14

This appendix shows summary statistics and a distribution of 116 countries of origin for the surnames of U.S. equity analysts over the sample period between 1996 and 2014. We obtain the U.S. historical immigration records of passengers arriving in the port of New York between 1820 and 1957 from ancestry.com. For 3,983 unique surnames that are associated with 5,516 individual analysts in the sample, we collect 13,353,663 immigration records without a missing nationality. We reclassify 1,629 original nationalities in the raw immigration record files into 116 countries of origin, following Pan, Siegel, and Wang (2017). We exclude USA, Unidentifiable, and other uninformative country classifications that indicate geographic regions (e.g., Africa, Arab World, Asia, and Central America) or ethnic groups (e.g., Hispanic and Jewish) from our empirical analyses, unless otherwise stated.

Appendix B
Examples of the U.S. equity analysts' surnames

	Favorability of an analyst's surname (*FavSurname*)									
	Decile 1	Decile 2	Decile 3	Decile 4	Decile 5	Decile 6	Decile 7	Decile 8	Decile 9	Decile 10
No. of unique surnames =	398	398	399	398	398	399	398	399	398	398
Mean of *FavSurname* (%) =	50.1	66.5	72.8	76.1	78.6	80.4	82.3	85.2	87.3	88.7
	(Least favorable)									(Most favorable)
Surnames	Agah	Aydin	Alling	Bout	Klauer	Culp	Beardsley	Brennan	Wooten	Thackray
	Coiro	Zale	Pfeifer	Loff	Lorenz	Thind	Logue	Brundage	Bacon	Willoughby
	Franqui	Jakubik	Dan	Hasse	Nadol	Beago	Keaney	Moran	Wilkin	Garvey
	Crabs	Teitelbaum	Hackman	Philippi	Damasco	Notardonato	Bowyer	Myers	Mohan	Summers
	Vucenovic	Heikkinen	Pinchot	Schneider	Schlang	Tenbrink	Murchie	Dickson	Gould	Billingsley
	Aftahi	Blaustein	Szymczak	Hanus	Pitzer	Blaeser	Lindberg	Stanley	Aney	McIlree
	Ghazi	Min	Fontana	Bauer	Albrecht	Hardt	Shepler	Dann	Lennan	Crowell
	Georgieva	Zinsmeister	Solotar	Wlodarczak	Augenthaler	Noto	Ederer	Corty	Howard	Whelan
	Hormozi	Karim	Ladha	Bergman	Helmig	Daubenspeck	Lipman	Crooks	McGinnis	Holmstead
	Manlowe	Klugman	Groshans	Prehn	Pantazis	Marx	Purvis	Hand	Baugh	Barrett

This appendix shows 10 surnames of U.S. equity analysts across deciles of surname favorability (*FavSurname*). We rank 3,983 unique analyst surnames into deciles using the mean favorability of a surname over the sample period from 1996 to 2014. Each decile contains approximately 400 unique surnames. We report 10 analyst surnames that have the lowest mean favorability levels in each decile.

Appendix C
Variable definitions

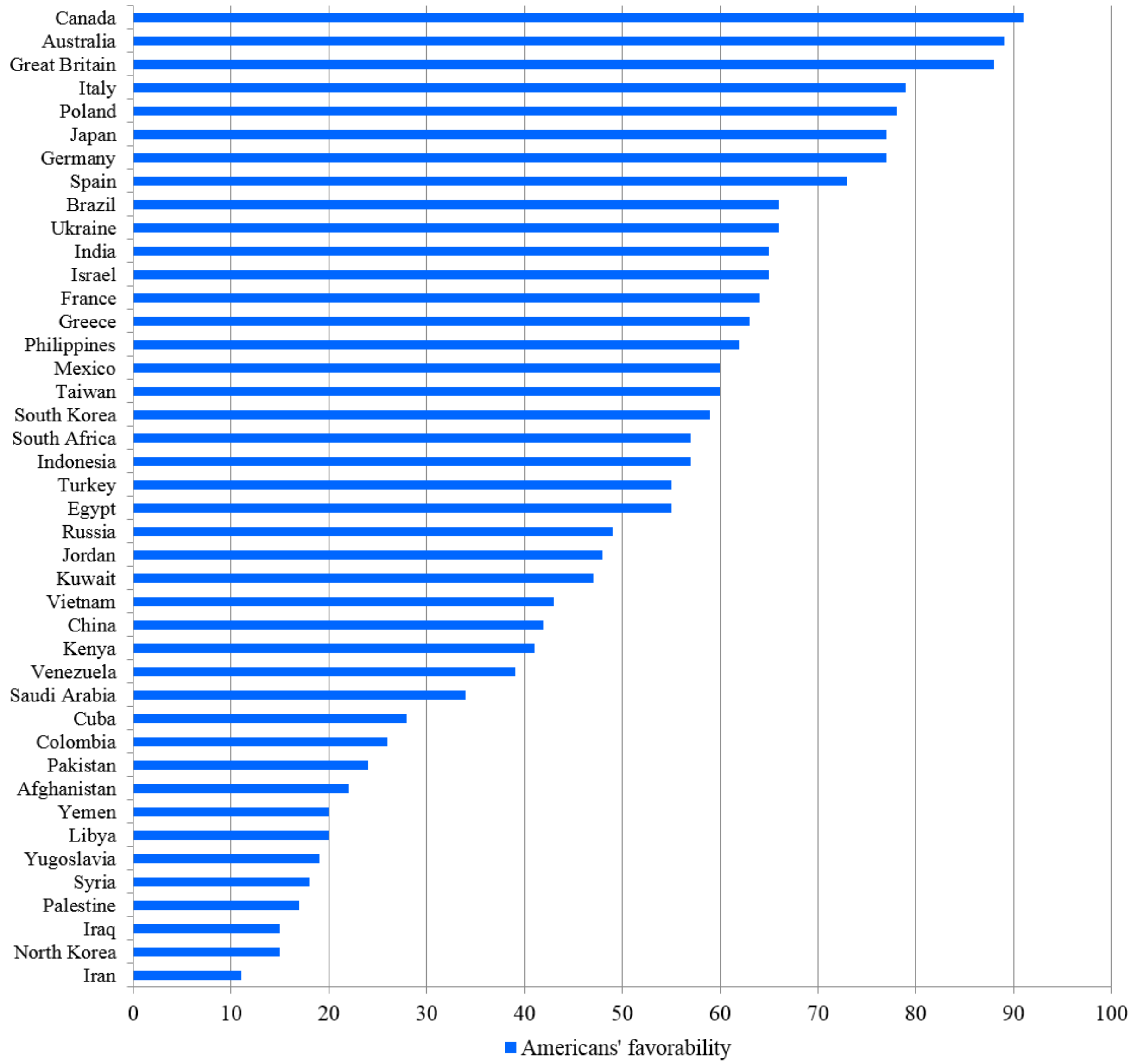
Variable name	Description	Source
Variables of interest		
FavSurname	Americans' favorability of an analyst's surname, measured as the weighted average of favorability ratings for the analyst's countries of origin that are associated with his or her surname through the nationalities of the U.S. immigrants. The favorability rating is the percentage of survey respondents who answered "Very Favorable" or "Mostly Favorable" to the following question in a Gallup survey: "I'd like your overall opinion of some foreign countries. Is your overall opinion of the following country very favorable, mostly favorable, mostly unfavorable, or very unfavorable?" Most recent favorability ratings are used as of each forecast date. Countries with non-missing favorability ratings are assigned a weight based on the frequency of nationalities that the U.S. immigrants with the same surname reported	Gallup, IBES, Immigration
Treatment	Indicator variable for an analyst who has a Middle Eastern (French or German) surname in the natural experiment setting of the 9/11 terrorist attacks (the French and German governments' opposition to the Iraq War). A surname is defined as Middle Eastern if more than 30% of the U.S. immigrants with the surname came from Middle Eastern countries, such as Afghanistan, Egypt, Iran, Iraq, Jordan, Kuwait, Libya, Pakistan, Palestine, Saudi Arabia, Syria, Turkey, and Yemen, or identified as Arab or Muslim. A surname is defined as French or German if more than 40% of the U.S. immigrants with the surname came from France or Germany	IBES, Immigration
Post-shock	An indicator variable for a forecast revision issued after September 11, 2001 (February 14, 2003) in the natural experiment setting of the 9/11 terrorist attacks (the French and German governments' opposition to the Iraq War)	IBES
Revision	The forecast revision, measured as the difference between the current earnings forecast and the preceding forecast, scaled by the stock price two trading days prior to the current forecast date	CRSP, IBES
Dependent variables		
Accuracy	A negative value of the absolute difference between an analyst's last one-year-ahead earnings forecast and the actual earnings, scaled by the stock price two trading days prior to the forecast date	CRSP, IBES
All-star	A dummy variable equal to 1 if an analyst is ranked as an all-star analyst in the next year's *Institutional Investor* magazine and 0 otherwise	IBES, II
BHAR [+m, +n]	Size-adjusted buy-and-hold abnormal return over the next six-month or one-year window, starting from trading day *m* (*m* = +2 or 11) and ending on the *n*-th trading day (*n* = +127 or 253), where trading day 0 is the forecast revision date. Size-decile breakpoints are computed at the beginning of every calendar quarter using all NYSE firms. Benchmark return is the equal-weighted return for all NYSE/AMEX/NASDAQ firms in the same size-decile portfolio	CRSP
CAR [-1,+n]	The size-adjusted cumulative abnormal return over the window starting one trading day before and ending on the *n*-th trading day (n = +1, 3, 5, and 10) following a forecast revision date. Size-decile breakpoints are computed at the beginning of every calendar quarter using all NYSE firms. Benchmark return is the equal-weighted return for all NYSE/AMEX/NASDAQ firms in the same size-decile portfolio	CRSP
Covering visible stocks	A dummy variable equal to 1 if an analyst covers at least one stock that ranks in the top decile by total analyst coverage in the following year and 0 otherwise	IBES
Forecast bias	An analyst's last one-year-ahead earnings forecast for the fiscal year minus the actual earnings, scaled by the stock price two trading days prior to the forecast date.	CRSP, IBES
Post-closure/merger termination	A dummy variable equal to 1 if an analyst disappears from the I/B/E/S within 3 years after the brokerage house he or she works for goes out of business (i.e., closure) or goes through a merger (as either an acquirer or a target) and 0 otherwise	Factiva, IBES

Appendix C (*Continued*)
Variable definitions

Variable name	Description	Source
Control variables		
Book-to-market	The ratio of book equity to market equity for a firm, measured at the most recent December preceding the forecast date	Compustat, CRSP
Brokerage size	The natural logarithm of one plus the number of analysts in a brokerage house in a year	IBES
Days since last forecast	The natural logarithm of one plus the number of days elapsed since the most recent earnings forecast for a firm was issued by another analyst	IBES
Female analyst	A dummy variable equal to 1 if an analyst is female and 0 otherwise	IBES, Nelson
Forecast horizon	The natural logarithm of one plus the number of days between a firm's earnings announcement date and an analyst's earnings forecast date for the firm	IBES
Forecast frequency	The natural logarithm of one plus the number of one-year-ahead earnings forecasts an analyst issues in a year	IBES
Firm size	The natural logarithm of a firm's market capitalization (in thousands) measured at the end of the month prior to an analyst's forecast date.	CRSP, IBES
Firm-specific experience	The natural logarithm of one plus the number of years an analyst has issued one-year-ahead earnings forecasts for a firm	IBES
General experience	The natural logarithm of one plus the number of years an analyst has appeared in I/B/E/S	IBES
Institutional ownership	The percentage of shares held by institutions in the most recent quarter-end 13F filing	13F
Lagged accuracy	One-year lagged accuracy, defined as the accuracy of an analyst's last earnings forecast for a firm in the preceding year	CRSP, IBES
Momentum	The past 12-month return for a firm, measured at the end of the prior month of the forecast date	CRSP
Number of analysts	The number of analysts following a firm in a year	IBES
Number of firms	The number of firms an analyst follows in a year	IBES
Number of industries	The number of (two-digit SIC code) industries an analyst follows in a year	IBES

This appendix provides the variable definitions. We construct variables using the following data sources: Amazon Mechanical Turk (AMT) platform, Compustat, Center for Research on Security Prices (CRSP), Factiva, Gallup Analytics (Gallup), *Institutional Investor* magazine (II), Nelson's directory (Nelson), Thomson Reuters' Institutional Brokers Estimate System (IBES), Thomson Reuters' Institutional 13F Holdings file (13F), and the U.S. immigration records on ancestry.com (Immigration).

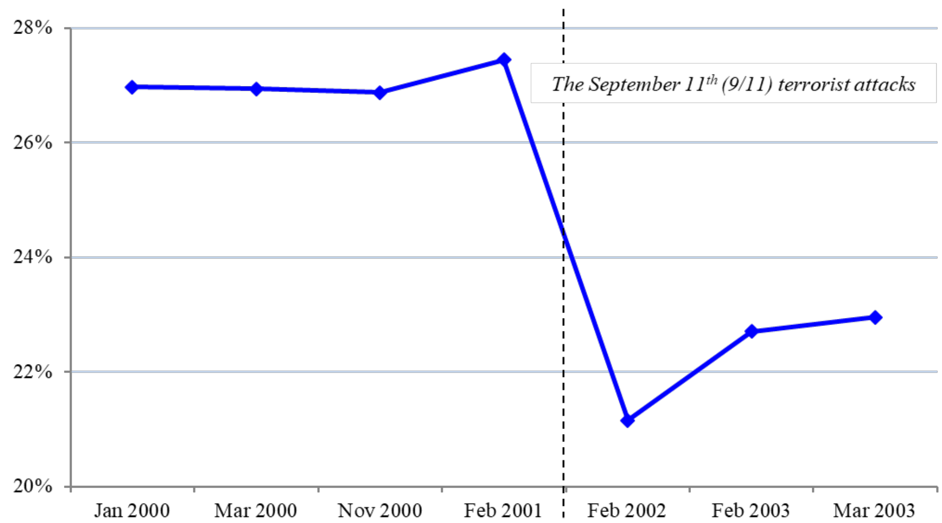
Figure 1
Distribution of Americans' favorability of foreign countries



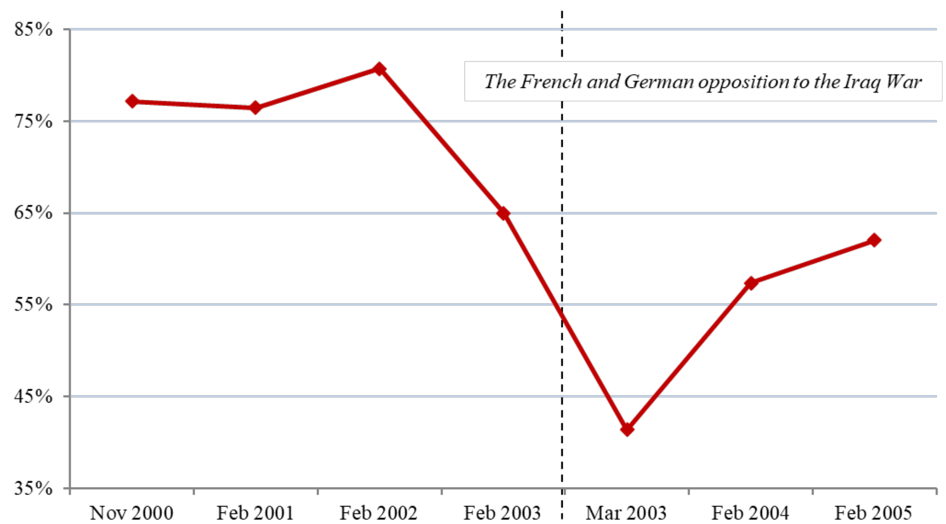
This figure shows the average level of Americans' favorability toward foreign countries during the sample period between 1996 and 2014. Favorability is measured as the total percentage of survey respondents who answer "Very Favorable" or "Mostly Favorable" to the following question in a Gallup survey: "I'd like your overall opinion of some foreign countries. Is your overall opinion of the following country very favorable, mostly favorable, mostly unfavorable, or very unfavorable?"

Figure 2
Changes in Americans' favorability of foreign countries

Panel A: The September 11th (9/11) terrorist attacks



Panel B: The French and German governments' opposition to the Iraq War



This figure plots the time-series change in Americans' favorability of Middle Eastern countries around the 9/11 terrorist attacks (September 2001) and of France and Germany around the French and German governments' opposition to the U.S. troops' invasion of Iraq (March 2003). Panel A reports the average American's favorability of Middle Eastern countries. Panel B reports the average Americans' favorability of France and Germany. We require countries to be covered in two Gallup surveys that immediately preceded and followed the event date in each panel. We use data in the closest year when the favorability rating is missing in a year.

Table 1
Summary statistics for Americans' favorability of foreign countries

Country	%Very favorable	%Mostly favorable	%Mostly unfavorable	%Very unfavorable	%Others	Country	%Very favorable	%Mostly favorable	%Mostly unfavorable	%Very unfavorable	%Others
Afghanistan	2.9	18.9	43.8	28.7	5.7	Kuwait	5.7	41.1	29.6	12.3	11.3
Australia	43.9	45.2	3.3	2.0	5.6	Libya	2.4	17.1	41.8	24.8	13.9
Brazil	11.1	55.2	13.9	3.7	16.1	Mexico	11.6	47.9	24.9	10.3	5.3
Canada	45.0	45.6	4.4	1.7	3.3	North Korea	2.3	12.3	35.5	42.7	7.2
China	5.9	35.7	35.8	15.9	6.7	Pakistan	2.5	21.2	45.5	22.3	8.5
Colombia	4.2	22.2	40.2	19.2	14.2	Palestine	2.4	14.9	43.6	26.9	12.2
Cuba	3.7	24.3	42.6	22.4	7.0	Philippines	10.4	51.8	20.7	5.8	11.3
Egypt	7.8	47.1	25.8	7.3	12.0	Poland	17.1	60.8	8.2	1.3	12.6
France	15.4	48.7	20.4	9.5	6.0	Russia	5.6	43.9	32.4	10.9	7.2
Germany	18.9	58.4	11.5	4.5	6.7	Saudi Arabia	4.1	30.3	40.5	17.6	7.5
Great Britain	40.2	47.4	5.2	2.4	4.8	South Africa	11.2	46.3	23.6	8.9	10.0
Greece	11.3	51.2	23.5	5.2	8.8	South Korea	11.3	47.2	22.7	9.0	9.8
India	10.2	55.1	19.3	5.7	9.7	Spain	16.1	57.3	8.1	2.7	15.8
Indonesia	6.1	50.7	22.8	4.8	15.6	Syria	2.4	15.8	40.9	25.3	15.6
Iran	1.8	9.1	42.3	41.0	5.8	Taiwan	10.3	49.2	17.8	6.8	15.9
Iraq	2.3	12.9	38.5	41.4	4.9	Turkey	6.5	48.6	23.6	5.8	15.5
Israel	20.0	45.3	19.9	7.4	7.4	Ukraine	8.7	57.6	15.2	3.1	15.4
Italy	21.4	58.0	7.1	3.4	10.1	Venezuela	7.0	32.3	28.0	19.1	13.6
Japan	19.6	57.7	12.1	4.6	6.0	Vietnam	5.1	37.6	31.7	13.0	12.6
Jordan	7.5	40.6	27.3	9.5	15.1	Yemen	1.9	18.6	37.0	19.4	23.1
Kenya	4.8	36.7	27.8	9.6	21.1	Yugoslavia	2.1	16.6	45.5	26.7	9.1

This table shows summary statistics for Americans' favorability of foreign countries from Gallup Analytics. Favorability is rated on a five-point Likert scale of Very Favorable, Mostly Favorable, Mostly Unfavorable, Very Unfavorable, and Others. The "Others" response includes uninformative rating items, such as "Don't Know," "Refused," "Never Heard Of," and "Can't Rate." Numbers indicate the mean percentage of survey participants for each rating scale from 1996 to 2014.

Table 2

Time variation in the weighted measure of surname favorability

Panel A: A real-life example of the most and least variable analyst surnames		
Analyst surname:	Most variable surname *"Tournier"*	Least variable surname *"Garcea"*
	Weighted measure of surname favorability, % (in year)	
Highest level	79.14 (in 2002)	74.38 (in 2002)
Lowest level	44.01 (in 2003)	73.64 (in 2007)
Spread of *FavSurname* (highest - lowest) (%)	35.13	0.74
Standard Deviation of *FavSurname*	0.194	0.003
Three most common nationalities in the U.S. immigrants (%)	France (63.64)	Italy (47.06)
	Netherlands (23.82)	Spain (28.99)
	Germany (3.13)	Cuba (5.04)

Panel B: Mean spread of surname favorability for the sample analysts' surnames										
	Mean spread of the weighted measure of surname favorability (*FavSurname*)									
	Decile 1	Decile 2	Decile 3	Decile 4	Decile 5	Decile 6	Decile 7	Decile 8	Decile 9	Decile 10
No. of unique surnames =	451	333	339	307	223	258	319	284	237	276
Mean Std. Dev. of *FavSurname* =	0.004	0.013	0.020	0.025	0.029	0.032	0.037	0.046	0.063	0.099
	(Least variable)									(Most variable)
Mean spread of *FavSurname* (highest - lowest) (%)	1.11	3.69	5.26	6.79	8.22	8.58	9.27	13.17	20.8	28.58

This table shows descriptive evidence for the time variation of our weighted measure of surname favorability (*FavSurname*). In Panel A, we illustrate a real-life example of the most and least variable analyst surnames in our sample. The most and least variable surnames are determined based on the standard deviation of yearly mean levels of surname favorability (*FavSurname*), calculated at the surname level. In Panel B, we rank analysts' surnames into deciles by their standard deviation of yearly mean levels of surname favorability. We report the mean spread (i.e., highest – lowest) of our measure of surname favorability (*FavSurname*) for surnames in each decile. Surnames that appear in at least three different years in the sample are used in the analysis. Variable definitions are provided in Appendix C.

Table 3
Summary statistics for variables

Panel A: Summary statistics							
Variable	Mean	Std dev	10th pctl	25th Pctl	Median	75th pctl	90th pctl
Variables of Interest							
FavSurname	0.784	0.113	0.626	0.747	0.808	0.870	0.889
Revision	-0.002	0.013	-0.011	-0.003	0.000	0.002	0.006
Dependent Variables							
Accuracy	-0.013	0.026	-0.030	-0.011	-0.004	-0.001	0.000
All-star	0.108	0.311	0	0	0	0	1
CAR [-1,+1]	-0.002	0.070	-0.078	-0.031	-0.001	0.031	0.074
CAR [-1,+3]	-0.002	0.079	-0.089	-0.037	0.000	0.036	0.085
CAR [-1,+5]	-0.002	0.085	-0.097	-0.041	0.000	0.041	0.093
CAR [-1,+10]	-0.001	0.098	-0.113	-0.049	0.000	0.050	0.111
Covering visible stocks	0.824	0.381	0	1	1	1	1
Forecast bias	0.003	0.025	-0.011	-0.003	0.000	0.004	0.020
Post-closure/merger termination	0.474	0.499	0	0	0	1	1
Control Variables							
Book-to-market	0.552	0.437	0.149	0.258	0.440	0.714	1.062
Brokerage size	3.857	0.995	2.565	3.178	4.007	4.673	4.942
Days since last forecast	1.781	1.152	0.693	0.693	1.386	2.639	3.584
Female analyst	0.125	0.331	0	0	0	0	1
Forecast horizon	5.014	0.689	4.304	4.654	5.236	5.557	5.684
Forecast frequency	4.334	0.563	3.638	3.989	4.344	4.691	5.037
Firm size	14.909	1.775	12.613	13.620	14.849	16.205	17.246
Firm-specific experience	1.694	0.515	1.099	1.386	1.609	2.079	2.398
General experience	2.233	0.537	1.386	1.792	2.303	2.639	2.890
Institutional ownership	0.703	0.202	0.412	0.585	0.736	0.854	0.945
Lagged accuracy	-0.005	0.013	-0.011	-0.004	-0.001	-0.001	0.000
Momentum	0.138	0.485	-0.403	-0.157	0.089	0.345	0.683
Number of analysts	19.345	10.859	6	11	18	26	35
Number of firms	17.730	7.295	10	13	17	21	27
Number of industries	3.726	2.392	1	2	3	5	7

Table 3 (*Continued*)
Summary statistics for variables

Panel B: Analyst, forecast, and firm characteristics conditional on favorability of analysts' surnames						
	High *FavSurname* N = 381,470		Low *FavSurname* N = 381,310		Test of differences	
Variable	Mean	Median	Mean	Median	*t*-statistic	*z*-statistic
FavSurname	0.863	0.870	0.705	0.747	(48.24)***	(756.36)***
Accuracy	-0.012	-0.004	-0.013	-0.004	(1.18)	(9.98)***
Forecast bias	0.003	0.000	0.004	0.000	(-0.10)	(3.61)***
Book-to-market	0.563	0.450	0.541	0.431	(2.61)***	(25.63)***
Brokerage size	3.834	3.970	3.879	4.025	(-1.21)	(-21.36)***
Days since last forecast	1.765	1.386	1.797	1.386	(-2.00)**	(-14.69)***
Female analyst	0.115	0	0.136	0	(-1.63)	(-27.00)***
Forecast horizon	5.022	5.236	5.006	5.226	(4.68)***	(11.02)***
Forecast frequency	4.342	4.344	4.327	4.331	(0.62)	(9.41)***
Firm size	14.953	14.886	14.865	14.812	(2.13)**	(20.74)***
Firm-specific experience	1.700	1.609	1.689	1.609	(1.04)	(12.30)***
General experience	2.258	2.303	2.207	2.303	(3.04)***	(41.29)***
Institutional ownership	0.715	0.748	0.691	0.723	(6.57)***	(49.60)***
Lagged accuracy	-0.005	-0.001	-0.005	-0.002	(1.66)*	(6.53)***
Momentum	0.129	0.083	0.147	0.094	(-4.78)***	(-12.70)***
Number of analysts	19.543	18	19.146	18	(1.44)	(16.33)***
Number of firms	17.607	17	17.854	16	(-0.87)	(2.05)**
Number of industries	3.698	3	3.754	3	(-0.63)	(-8.38)***

This table shows summary statistics for our main variables in the sample. In Panel A, we report summary statistics for variables over the sample period from 1996 to 2014. In Panel B, we compare analyst, forecast, and firm characteristics across two groups of surname favorability. We divide our sample into two groups by the sample-median favorability of analysts' surnames (*FavSurname*). *t*-statistics for mean difference tests are based on standard errors clustered by analyst. *z*-statistics for Wilcoxon signed-rank median difference tests do not account for intra-group correlations in residuals per analyst. All continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are provided in Appendix C. ***, **, * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 4
Market reaction regression estimates

Panel A: Main results								
	Dependent variable: Size-adjusted CAR							
	[-1,+1]		[-1,+3]		[-1,+5]		[-1,+10]	
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Revision	0.888***	0.773***	0.919***	0.759***	0.968***	0.854***	0.902***	0.566*
	(7.308)	(2.886)	(7.032)	(2.704)	(7.513)	(2.923)	(6.797)	(1.800)
Revision×FavSurname	0.413***	0.431***	0.451***	0.472***	0.412**	0.443***	0.465***	0.507***
	(2.660)	(2.804)	(2.707)	(2.878)	(2.507)	(2.696)	(2.739)	(2.988)
FavSurname	0.001	0.001	0.001	0.001	0.001	0.001	0.000	0.000
	(0.960)	(0.787)	(1.183)	(0.992)	(0.571)	(0.468)	(0.273)	(0.299)
Revision×Book-to-market		-0.296***		-0.305***		-0.289***		-0.283***
		(-13.697)		(-12.978)		(-11.671)		(-10.324)
Revision×Brokerage size		0.127***		0.128***		0.138***		0.145***
		(7.417)		(7.053)		(7.546)		(7.486)
Revision×Days since last forecast		0.050***		0.069***		0.082***		0.095***
		(5.094)		(6.417)		(7.097)		(7.437)
Revision×Female analyst		0.010		0.072		0.067		0.107
		(0.151)		(1.099)		(1.063)		(1.602)
Revision×Forecast horizon		0.115***		0.120***		0.115***		0.137***
		(6.987)		(6.427)		(5.549)		(5.864)
Revision×Forecast frequency		-0.273***		-0.268***		-0.285***		-0.293***
		(-5.724)		(-5.366)		(-5.685)		(-5.553)
Revision×Firm size		-0.005		-0.004		0.001		0.016
		(-0.387)		(-0.295)		(0.075)		(0.961)
Revision×Firm-specific experience		-0.071**		-0.065*		-0.057		-0.040
		(-2.121)		(-1.828)		(-1.532)		(-1.030)
Revision×General experience		0.080**		0.094**		0.073*		0.081**
		(2.323)		(2.550)		(1.918)		(1.991)
Revision×Institutional ownership		1.081***		1.103***		1.049***		0.980***
		(17.308)		(16.223)		(14.641)		(12.328)
Revision×Lagged accuracy		9.223***		10.237***		10.166***		10.816***
		(19.263)		(19.170)		(17.341)		(17.577)
Revision×Momentum		0.521***		0.594***		0.672***		0.764***
		(17.977)		(18.578)		(19.534)		(20.013)
Revision×Number of analysts		-0.006***		-0.009***		-0.011***		-0.016***
		(-3.103)		(-4.108)		(-4.518)		(-6.102)
Revision×Number of firms		-0.007*		-0.009**		-0.008**		-0.009**

		(-1.886)		(-2.235)		(-2.038)		(-2.257)
Revision×Number of industries		0.071***		0.077***		0.081***		0.089***
		(8.447)		(8.619)		(9.038)		(9.391)
Book-to-market		0.000		0.000		-0.000		0.001
		(0.315)		(0.911)		(-0.014)		(0.832)
Brokerage size		-0.000		-0.000		-0.000		0.000
		(-0.456)		(-0.360)		(-0.340)		(0.595)
Days since last forecast		0.000		-0.000		-0.000**		-0.000***
		(0.416)		(-1.353)		(-1.971)		(-3.214)
Female analyst		0.000		0.000		0.000		0.000
		(0.588)		(0.764)		(0.850)		(0.631)
Forecast horizon		0.002***		0.002***		0.002***		0.002***
		(13.728)		(12.358)		(11.292)		(7.912)
Forecast frequency		-0.001**		-0.001**		-0.001*		-0.001***
		(-2.304)		(-2.130)		(-1.754)		(-2.576)
Firm size		-0.012***		-0.014***		-0.017***		-0.023***
		(-35.020)		(-37.759)		(-40.486)		(-45.713)
Firm-specific experience		-0.000		-0.000		-0.000		-0.000
		(-0.664)		(-0.505)		(-0.672)		(-1.122)
General experience		-0.000		-0.000		-0.000		0.000
		(-0.384)		(-0.750)		(-0.125)		(0.422)
Institutional ownership		-0.009***		-0.008***		-0.010***		-0.011***
		(-8.745)		(-7.259)		(-7.743)		(-7.421)
Lagged accuracy		0.015		0.017		0.015		0.012
		(1.550)		(1.481)		(1.162)		(0.784)
Momentum		-0.000		-0.002***		-0.002***		-0.004***
		(-1.528)		(-5.204)		(-7.346)		(-9.167)
Number of analysts		-0.000		-0.000		-0.000		-0.000
		(-1.439)		(-1.239)		(-0.671)		(-0.750)
Number of firms		0.000		0.000		0.000		0.000
		(1.311)		(1.343)		(0.336)		(0.431)
Number of industries		0.000		0.000		0.000		0.000
		(0.884)		(0.899)		(1.198)		(1.348)
Intercept	0.003***	0.171***	0.004***	0.208***	0.004***	0.243***	0.006***	0.330***
	(3.773)	(33.759)	(4.276)	(36.277)	(4.394)	(39.106)	(5.165)	(44.307)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	762,780	762,780	762,780	762,780	762,780	762,780	762,780	762,780
Adjusted R^2 (%)	7.77	9.74	7.16	9.20	6.61	8.71	5.65	8.00

Table 4 (*Continued*)
Market reaction regression estimates

Panel B: Using analysts' last forecasts for a firm-year only								
	Dependent variable: Size-adjusted CAR							
	[-1,+1]		[-1,+3]		[-1,+5]		[-1,+10]	
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Revision×FavSurname	0.411**	0.441**	0.349	0.368*	0.401*	0.428*	0.597**	0.645**
	(1.984)	(2.260)	(1.484)	(1.682)	(1.647)	(1.868)	(2.139)	(2.500)
Controls and fixed effects	Identical to Panel A of Table 4							
Number of observations	213,549	213,549	213,549	213,549	213,549	213,549	213,549	213,549
Adjusted R^2 (%)	9.68	11.51	9.47	11.27	9.14	10.96	8.58	10.56

Panel C: Excluding forecasts made on days when other analysts' forecasts, quarterly earnings, or managerial forecasts for the firm are released

Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Revision×FavSurname	0.286*	0.275*	0.405**	0.405**	0.334*	0.355**	0.308	0.364
	(1.959)	(1.927)	(2.299)	(2.386)	(1.806)	(1.961)	(1.323)	(1.622)
Controls and fixed effects	Identical to Panel A of Table 4							
Number of observations	209,467	209,467	209,467	209,467	209,467	209,467	209,467	209,467
Adjusted R^2 (%)	3.45	4.15	3.24	4.07	3.21	4.20	3.1	4.45

Panel D: Defining the level of favorability using different survey response items

Coefficient estimates on Revision×FavSurname	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Using "very favorable (%)" only	0.588***	0.424***	0.658***	0.482***	0.624***	0.452**	0.693***	0.520***
	(3.551)	(2.578)	(3.727)	(2.766)	(3.501)	(2.559)	(3.727)	(2.838)
Using a composite score	0.248***	0.206***	0.275***	0.232***	0.262***	0.223***	0.296***	0.261***
	(3.720)	(3.117)	(3.867)	(3.299)	(3.690)	(3.151)	(3.993)	(3.543)
Controls and fixed effects	Identical to Panel A of Table 4							

Panel E: Using dominant origins of a surname only

Coefficient estimates on Revision×FavSurname	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Using the most dominant origin	0.324**	0.333***	0.354**	0.360***	0.302**	0.315**	0.323**	0.340**
	(2.482)	(2.581)	(2.518)	(2.594)	(2.180)	(2.249)	(2.265)	(2.345)
Using three most dominant origins	0.407***	0.426***	0.444***	0.465***	0.400**	0.431***	0.442***	0.484***
	(2.696)	(2.850)	(2.744)	(2.915)	(2.499)	(2.684)	(2.677)	(2.924)
Controls and fixed effects	Identical to Panel A of Table 4							

Table 4 (*Continued*)
Market reaction regression estimates

This table shows the estimates of pooled OLS market reaction regressions. In Panel A, we estimate the baseline OLS regressions in which the dependent variable is the size-adjusted cumulative abnormal return (*CAR*) over the window from trading day -1 to trading day *n* (*n* = 1, 3, 5, and 10), where trading day 0 is an analyst's forecast revision date. *FavSurname* is Americans' favorability of an analyst's surname. *Revision* is the difference between an analyst's current and preceding earnings forecast for a firm, scaled by the stock price two trading days prior to the current forecast date. Panels B, C, D, and E report results from estimating the same OLS regressions whose model specifications are identical to those in Panel A, but each panel is different from Panel A in terms of either a sample composition or a measurement of *FavSurname*. In Panel B, we estimate the regressions using the subsample of analysts' last forecasts for each firm-fiscal year. In Panel C, we exclude forecasts made on days when other analysts' forecasts, the firm's quarterly earnings, or the firm's managerial forecasts are released. In Panel D, we use different items from Gallup surveys to measure the level of Americans' favorability: we either use the most extreme rating scale, "Very Favorable," only or make a composite score of favorability using all four rating scales. The composite score is computed as 4 × (%Very Favorable) + 3 × (%Mostly Favorable) + 2 × (%Mostly Unfavorable) + 1 × (%Very Unfavorable), following Hwang (2011). In Panel E, we calculate *FavSurname* by considering one or three of the most dominant countries of origin associated with an analyst's surname. In parentheses below coefficient estimates are *t*-statistics based on standard errors clustered by analyst. All continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are provided in Appendix C. ***, **, * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 5
Falsification test: Market reaction regression estimates

	Dependent variable: Size-adjusted CAR							
	[-1,+1]		[-1,+3]		[-1,+5]		[-1,+10]	
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Revision	1.187***	1.070***	1.185***	1.049***	1.196***	1.096***	1.145***	0.795**
	(11.073)	(4.074)	(10.236)	(3.792)	(10.290)	(3.758)	(9.263)	(2.561)
Revision×FavSurname (*P*)	0.035	0.118	0.124	0.210	0.137	0.219	0.176	0.251
	(0.227)	(0.794)	(0.751)	(1.356)	(0.825)	(1.410)	(1.001)	(1.545)
FavSurname (*P*)	0.000	0.001	0.000	0.001	0.001	0.001	0.000	0.001
	(0.374)	(0.899)	(0.524)	(1.071)	(0.557)	(1.117)	(0.407)	(1.065)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	740,566	740,566	740,566	740,566	740,566	740,566	740,566	740,566
Adjusted R^2 (%)	7.77	9.74	7.17	9.22	6.63	8.73	5.69	8.03

This table shows the results from falsification tests for the market reaction regression estimates. We estimate the baseline OLS regressions in which the dependent variable is the size-adjusted cumulative abnormal return (*CAR*) over the window from trading day -1 to trading day n (n = 1, 3, 5, and 10), where trading day 0 is an analyst's forecast revision date. *Revision* is the difference between an analyst's current and preceding earnings forecast for a firm, scaled by the stock price two trading days prior to the current forecast date. *FavSurname (P)* is a placebo measure of surname favorability, measured as the weighted average of favorability ratings for falsified countries of origin. Falsified country of origin is the country that appears immediately after the true country of origin in the alphabetically ordered list of 116 countries in Appendix A (e.g., Germany → Great Britain). Control variables and fixed effects are identical to those in Panel A of Table 4. In parentheses below coefficient estimates are *t*-statistics based on standard errors clustered by analyst. All continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are provided in Appendix C. ***, **, * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 6

Subsample analyses for market reaction regression estimates

Panel A: Investor sophistication

	Dependent variable: Size-adjusted CAR							
	[-1,+1]		[-1,+3]		[-1,+5]		[-1,+10]	
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<u>*High institutional ownership*</u>								
Revision×FavSurname	0.279	0.315	0.312	0.353	0.352	0.410*	0.336	0.406
	(1.093)	(1.333)	(1.177)	(1.418)	(1.336)	(1.651)	(1.239)	(1.572)
Controls and fixed effects	Identical to Panel A of Table 4							
Number of observations	381,397	381,397	381,397	381,397	381,397	381,397	381,397	381,397
Adjusted R^2 (%)	9.11	11.42	8.35	10.7	7.61	10.17	6.47	9.21
<u>*Low institutional ownership*</u>								
Revision×FavSurname	0.425***	0.489***	0.467***	0.532***	0.387**	0.456***	0.504***	0.577***
	(2.915)	(3.360)	(2.944)	(3.333)	(2.402)	(2.739)	(2.854)	(3.090)
Controls and fixed effects	Identical to Panel A of Table 4							
Number of observations	381,383	381,383	381,383	381,383	381,383	381,383	381,383	381,383
Adjusted R^2 (%)	8.95	10.60	8.41%	10.1	8.01	9.77	7.31	9.39

Panel B: Difficulty in inferring countries of origin

	Dependent variable: Size-adjusted CAR							
	[-1,+1]		[-1,+3]		[-1,+5]		[-1,+10]	
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<u>*Origins easily inferred by name*</u>								
Revision×FavSurname	0.481**	0.482**	0.483**	0.488**	0.420**	0.440**	0.522**	0.538**
	(2.379)	(2.381)	(2.212)	(2.247)	(1.961)	(2.054)	(2.372)	(2.442)
Controls and fixed effects	Identical to Panel A of Table 4							
Number of observations	381,397	381,397	381,397	381,397	381,397	381,397	381,397	381,397
Adjusted R^2 (%)	8.04	10.05	7.39	9.45	6.87	9.03	5.85	8.25
<u>*Origins difficult to infer by name*</u>								
Revision×FavSurname	0.346	0.366	0.431	0.449*	0.421	0.450*	0.411	0.458*
	(1.406)	(1.529)	(1.639)	(1.782)	(1.604)	(1.734)	(1.521)	(1.691)
Controls and fixed effects	Identical to Panel A of Table 4							
Number of observations	381,383	381,383	381,383	381,383	381,383	381,383	381,383	381,383
Adjusted R^2 (%)	7.72	9.70	7.17	9.25	6.59	8.70	5.68	8.05

Panel C: Analyst reputation

	Dependent variable: Size-adjusted CAR							
	[-1,+1]		[-1,+3]		[-1,+5]		[-1,+10]	
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<u>*All-star analysts*</u>								
Revision×FavSurname	0.510	0.412	0.498	0.364	0.526	0.427	0.793**	0.676**
	(1.526)	(1.411)	(1.478)	(1.205)	(1.484)	(1.346)	(2.046)	(1.973)
Controls and fixed effects	Identical to Panel A of Table 4							
Number of observations	204,336	204,336	204,336	204,336	204,336	204,336	204,336	204,336
Adjusted R^2 (%)	7.53	9.49	6.96	9.05	6.38	8.53	5.42	7.85
<u>*Non-all-star analysts*</u>								
Revision×FavSurname	0.401**	0.448***	0.457**	0.507***	0.403**	0.456**	0.412**	0.475***
	(2.323)	(2.674)	(2.421)	(2.769)	(2.194)	(2.523)	(2.213)	(2.613)
Controls and fixed effects	Identical to Panel A of Table 4							
Number of observations	558,444	558,444	558,444	558,444	558,444	558,444	558,444	558,444
Adjusted R^2 (%)	7.92	10.00	7.32	9.45	6.76	8.95	5.82	8.24

Table 6 (*Continued*)
Subsample analyses for market reaction regression estimates

Panel D: The sign of revision news								
	Dependent variable: Size-adjusted CAR							
	[-1,+1]		[-1,+3]		[-1,+5]		[-1,+10]	
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Positive forecast revisions								
Revision×FavSurname	0.463**	0.365*	0.485*	0.387	0.432	0.325	0.384	0.284
	(1.984)	(1.647)	(1.897)	(1.642)	(1.558)	(1.284)	(1.213)	(0.987)
Controls and fixed effects	Identical to Panel A of Table 4							
Number of observations	377,046	377,046	377,046	377,046	377,046	377,046	377,046	377,046
Adjusted R^2 (%)	8.44	9.49	8.03	9.24	7.83	9.15	7.41	8.98
Negative forecast revisions								
Revision×FavSurname	0.271**	0.280**	0.327**	0.330**	0.292**	0.300**	0.423***	0.426**
	(2.221)	(2.214)	(2.318)	(2.282)	(2.047)	(2.007)	(2.666)	(2.570)
Controls and fixed effects	Identical to Panel A of Table 4							
Number of observations	382,946	382,946	382,946	382,946	382,946	382,946	382,946	382,946
Adjusted R^2 (%)	8.56	10.18	7.56	9.27	6.91	8.70	6.00	8.06

Panel E: Analyst gender								
	Dependent variable: Size-adjusted CAR							
	[-1,+1]		[-1,+3]		[-1,+5]		[-1,+10]	
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Male analysts								
Revision×FavSurname	0.407**	0.427***	0.465***	0.484***	0.436**	0.463***	0.508***	0.548***
	(2.449)	(2.652)	(2.597)	(2.816)	(2.454)	(2.668)	(2.785)	(3.066)
Controls and fixed effects	Identical to Panel A of Table 4							
Number of observations	667,067	667,067	667,067	667,067	667,067	667,067	667,067	667,067
Adjusted R^2 (%)	7.79	9.77	7.13	9.19	6.58	8.69	5.62	7.99
Female analysts								
Revision×FavSurname	0.368	0.251	0.280	0.108	0.181	0.057	0.126	-0.007
	(0.779)	(0.530)	(0.579)	(0.210)	(0.398)	(0.119)	(0.253)	(-0.014)
Controls and fixed effects	Identical to Panel A of Table 4							
Number of observations	95,713	95,713	95,713	95,713	95,713	95,713	95,713	95,713
Adjusted R^2 (%)	7.99	10.06	7.71	9.80	7.34	9.58	6.30	8.77

Panel F: Analyst first name								
	Dependent variable: Size-adjusted CAR							
	[-1,+1]		[-1,+3]		[-1,+5]		[-1,+10]	
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Americanized first names								
Revision×FavSurname	0.312	0.263	0.399*	0.341	0.431*	0.389*	0.586**	0.554**
	(1.470)	(1.219)	(1.756)	(1.488)	(1.918)	(1.669)	(2.560)	(2.329)
Controls and fixed effects	Identical to Panel A of Table 4							
Number of observations	528,551	528,551	528,551	528,551	528,551	528,551	528,551	528,551
Adjusted R^2 (%)	7.98	9.99	7.38	9.45	6.82	8.96	5.87	8.23
Non-Americanized first names								
Revision×FavSurname	0.523**	0.622***	0.522**	0.624***	0.399	0.511**	0.298	0.428*
	(2.185)	(2.760)	(2.036)	(2.610)	(1.582)	(2.177)	(1.126)	(1.742)
Controls and fixed effects	Identical to Panel A of Table 4							
Number of observations	234,229	234,229	234,229	234,229	234,229	234,229	234,229	234,229
Adjusted R^2 (%)	7.58	9.53	7.03	9.08	6.48	8.59	5.44	7.86

Table 6 (*Continued*)
Subsample analyses for market reaction regression estimates

This table shows the estimates of pooled OLS market reaction regressions using subsamples based on investor sophistication (Panel A), the difficulty in inferring countries of origin from a surname (Panel B), analyst reputation (Panel C), the sign of revision news (Panel D), analyst gender (Panel E), and analysts' first name (Panel F). The dependent variable is the size-adjusted cumulative abnormal return (*CAR*) over the window from trading day -1 to trading day *n* (*n* = 1, 3, 5, and 10), where trading day 0 is an analyst's forecast revision date. *FavSurname* is Americans' favorability of an analyst's surname. *Revision* is the difference between an analyst's current and preceding earnings forecast for a firm, scaled by the stock price two trading days prior to the current forecast date. In Panel A, we divide the sample into two subsamples by the sample median of institutional ownership. In Panel B, we divide the sample into two subsamples by the sample median of the fraction of U.S. immigrants whose nationality matches the most common country for a surname, conditional on the U.S. immigrants having the same surname. We assume that it is easier to infer the origin of an analyst from his or her name when a higher fraction of the U.S. immigrants with the analyst's surname come from a single country. In Panel C, we divide the sample into two subsamples by whether an analyst has ever been ranked as an all-star analyst in *Institutional Investor* magazine. In Panel D, we divide the sample into two subsamples by the sign of a forecast revision. In Panel E, we divide the sample into two subsamples by analysts' gender. In Panel F, we divide the sample into two subsamples by whether an analyst uses an Americanized first name. We define a first name as Americanized if it appears on the list of 100 most popular first names for male and female babies born in the United States between 1917 and 2016 (the list is available on the Social Security website at www.ssa.gov). Control variables and fixed effects are identical to those in Panel A of Table 4. In parentheses below coefficient estimates are *t*-statistics based on standard errors clustered by analyst. All continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are provided in Appendix C. ***, **, * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 7
Variations within analyst-year, firm-year, or analyst-firm

	Dependent variable: Size-adjusted CAR [-1,+1]					
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)
Revision	0.983***	0.984***	0.687***	0.707***	1.156***	1.166***
	(3.375)	(3.378)	(2.601)	(2.673)	(3.859)	(3.876)
Revision×FavSurname	0.446***	0.446***	0.280**	0.285**	0.493***	0.488***
	(2.672)	(2.675)	(2.092)	(2.125)	(2.823)	(2.776)
FavSurname	-0.024***	-0.024***	0.000	-0.000	0.001	-0.010***
	(-3.901)	(-3.915)	(0.325)	(-0.307)	(0.302)	(-2.687)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	No	Yes	No	No	No	No
Brokerage fixed effects	No	No	No	Yes	No	No
Year fixed effects	No	No	No	No	No	Yes
Analyst×Year fixed effects	Yes	Yes	No	No	No	No
Firm×Year fixed effects	No	No	Yes	Yes	No	No
Analyst×Firm fixed effects	No	No	No	No	Yes	Yes
Number of observations	762,780	762,780	762,780	762,780	762,780	762,780
Adjusted R^2 (%)	8.23	8.26	22.72	22.72	10.36	10.52

This table shows the estimates of pooled OLS market reaction regressions. We estimate the baseline OLS regressions in which the dependent variable is the size-adjusted cumulative abnormal return (*CAR*) over the window from trading day -1 to trading day +1, where trading day 0 is an analyst's forecast revision date. *FavSurname* is Americans' favorability of an analyst's surname. *Revision* is the difference between an analyst's current and preceding earnings forecast for a firm, scaled by the stock price two trading days prior to the current forecast date. The set of control variables is identical to that in Panel A of Table 4. In parentheses below coefficient estimates are *t*-statistics based on standard errors clustered by analyst. All continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are provided in Appendix C. ***, **, * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 8
Two natural experiments

Panel A: Matching covariates for the sample of the 9/11 terrorist attacks

	Middle Eastern analysts N = 13	Non-Middle Eastern analysts N = 204	Test of differences
Matching covariates	Mean	Mean	*t*-statistic
FavSurname	0.519	0.548	(-0.68)
Mean accuracy	-0.007	-0.009	(0.72)
Brokerage size	4.032	3.866	(0.58)
Forecast frequency	3.508	3.542	(-0.24)
General experience	1.681	1.636	(0.33)
Number of firms	9.461	11.683	(-1.48)

Panel B: Matching covariates for the sample of the French and German opposition to the Iraq War

	French and German analysts N = 248	Non-French/Non-German analysts N = 248	Test of differences
Matching covariates	Mean	Mean	*t*-statistic
FavSurname	0.814	0.853	(-11.88)***
Mean accuracy	-0.009	-0.008	(-0.59)
Brokerage size	3.996	4.007	(-0.11)
Forecast frequency	3.722	3.680	(0.79)
General experience	1.979	1.975	(0.09)
Number of firms	13.011	12.556	(0.90)

Panel C: Americans' favorability toward analysts' surnames around the events of two natural experiments

	Dependent variable: FavSurname			
Natural experiment:	The 9/11 terrorist attacks		The French and German opposition to the Iraq War	
Treatment = 1, for	Middle Eastern analysts		French and German analysts	
Post-shock = 1, after	September 11, 2001		February 14, 2003	
Independent variables	(1)	(2)	(3)	(4)
Treatment	0.007	0.014	-0.045***	-0.045***
	(0.115)	(0.224)	(-13.082)	(-12.962)
Treatment×Post-shock	-0.218***	-0.224***	-0.047***	-0.047***
	(-4.004)	(-4.129)	(-14.809)	(-14.886)
Post-shock	0.077***	0.093***	0.018***	0.084***
	(5.926)	(6.395)	(10.009)	(42.224)
Intercept	0.555***	0.561***	0.819***	0.782***
	(36.512)	(34.275)	(249.112)	(243.804)
Year fixed effects	No	Yes	No	Yes
Number of observations	2,136	2,136	4,920	4,920
Adjusted R^2 (%)	8.79	12.55	26.37	44.26

Table 8 (*Continued*)
Two natural experiments

Panel D: Difference-in-differences tests using two natural experiments				
	Dependent variable: Size-adjusted CAR [-1,+1]			
Natural experiment:	The 9/11 terrorist attacks		The French and German opposition to the Iraq War	
Treatment = 1, for	Middle Eastern analysts		French and German analysts	
Post-shock = 1, after	September 11, 2001		February 14, 2003	
Independent variables	(1)	(2)	(3)	(4)
Revision	0.658***	-1.608	1.071***	0.531
	(3.059)	(-0.849)	(11.629)	(0.720)
Revision×Treatment	1.112**	1.259**	0.169	0.075
	(2.503)	(2.011)	(1.378)	(0.607)
Revision×Treatment×Post-shock	-1.643***	-1.863***	-0.687***	-0.500**
	(-2.877)	(-2.597)	(-3.055)	(-2.410)
Revision×Post-shock	0.974***	1.250***	0.665***	0.599***
	(3.618)	(5.139)	(4.447)	(3.950)
Treatment×Post-shock	-0.004	0.000	-0.001	-0.001
	(-1.032)	(0.091)	(-1.088)	(-1.039)
Treatment	0.003	0.001	0.001	0.001
	(0.997)	(0.313)	(1.019)	(1.196)
Post-shock	-0.003	0.011	0.006***	0.015***
	(-0.701)	(1.209)	(3.430)	(7.806)
Controls	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Number of observations	25,519	25,519	67,120	67,120
Adjusted R^2 (%)	11.02	13.51	9.14	11.68

Table 8 (*Continued*)
Two natural experiments

This table shows summary statistics for matching covariates and OLS regression results for difference-in-differences tests using the matched samples of our two natural experiments: (1) the 9/11 terrorist attacks and (2) the French and German opposition to the Iraq War. For the natural experiment of the 9/11 terrorist attacks (the French and German opposition to the Iraq War), treatment analysts are those who have a Middle Eastern (French or German) surname. A surname is defined as Middle Eastern when more than 30% of the U.S. immigrants having the surname come from Middle Eastern countries, such as Afghanistan, Egypt, Iran, Iraq, Jordan, Kuwait, Libya, Pakistan, Saudi Arabia, Syria, Turkey, and Yemen, or are identified as Arab or Muslim, according to the U.S. historical immigration records. A surname is defined as French and German when more than 40% of the U.S. immigrants with the surname come from either France or Germany. In Panel A, we report summary statistics for matching covariates for the sample of the 9/11 terrorist attacks. The sample is constructed by matching Middle Eastern analysts and control analysts on the following matching covariates using a coarsened exact matching (CEM) algorithm: *FavSurname* (Americans' favorability of an analyst's surname), mean accuracy (mean accuracy of an analyst's last forecasts for firms in a year), brokerage size, forecast frequency, general experience, and number of firms. We conduct the matching at the end of year 2000. In Panel B, we report summary statistics for matching covariates for the sample of the French and German opposition to the Iraq War. We match French and German analysts with control analysts using the same set of matching covariates used in Panel A. We conduct a one-to-one matching at the end of year 2002. In Panels C and D, we restrict the sample period to the window from 1996 (1998) to 2006 (2008), excluding the transition year of 2001 (2003) for tests using the natural experiment of the 9/11 terrorist attacks (the French and German opposition to the Iraq War). In Panel C, we use observations at the analyst-year level and estimate OLS regressions in which the dependent variable is *FavSurname*. At the end of every December, we update *FavSurname* for all analysts in the matched samples using the most recent survey data from Gallup Analytics. In Panel D, we use observations at the forecast revision level and estimate the pooled OLS market reaction regressions in which the dependent variable is the size-adjusted cumulative abnormal return (*CAR*) over the window from trading day -1 to trading day +1, where trading day 0 is an analyst's forecast revision date. *Revision* is the difference between an analyst's current and preceding earnings forecast for a firm, scaled by the stock price two trading days prior to the current forecast date. In the sample of the 9/11 terrorist attacks (the French and German opposition to the Iraq War), *Treatment* and *Post-shock* are indicator variables equal to 1 for Middle Eastern (French and German) analysts and for observations after September 11, 2001 (February 14, 2003), respectively. The set of control variables is identical to that in Panel A of Table 4. In parentheses below coefficient estimates are *t*-statistics based on standard errors clustered by analyst. All continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are provided in Appendix C. ***, **, * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 9
Forecast quality

Panel A: Forecast accuracy and favorability of an analyst's surname

	Dependent variable: Accuracy		
Independent variables	(1)	(2)	(3)
FavSurname	0.000	-0.000	0.000
	(0.298)	(-0.581)	(0.941)
Book-to-market		-0.007***	-0.006***
		(-28.975)	(-17.443)
Brokerage size		-0.000***	-0.000
		(-3.288)	(-1.470)
Days since last forecast		0.000	-0.000***
		(1.305)	(-4.404)
Female analyst		-0.000	-0.000*
		(-0.943)	(-1.677)
Forecast horizon		-0.002***	-0.003***
		(-28.128)	(-34.931)
Forecast frequency		-0.001***	0.001***
		(-3.526)	(7.337)
Firm size		0.003***	0.009***
		(43.166)	(43.017)
Firm-specific experience		-0.001***	-0.000***
		(-4.169)	(-3.833)
General experience		0.000**	0.000**
		(2.155)	(2.181)
Institutional ownership		0.006***	0.004***
		(14.883)	(7.523)
Lagged accuracy		0.432***	0.143***
		(37.239)	(12.579)
Number of analysts		-0.000***	-0.000***
		(-23.722)	(-21.606)
Number of firms		0.000***	-0.000***
		(6.787)	(-3.799)
Number of industries		-0.000	0.000
		(-0.011)	(0.825)
Intercept	-0.004***	-0.030***	-0.111***
	(-9.520)	(-25.196)	(-40.360)
Firm fixed effects	Yes	No	Yes
Year fixed effects	Yes	No	Yes
Number of observations	213,549	213,549	213,549
Adjusted R^2 (%)	30.92	16.42	37.79

Panel B: Forecast bias and favorability of an analyst's surname

	Dependent variable: Forecast bias		
Independent variables	(1)	(2)	(3)
FavSurname	0.001	0.001**	0.000
	(1.169)	(2.043)	(0.759)
Controls and fixed effects	Identical to Panel A of Table 9		
Number of observations	213,549	213,549	213,549
Adjusted R^2 (%)	20.86	2.49	21.62

Panel C: Forecast timeliness and favorability of an analyst's surname

	Dependent variable: Days since last forecast		
Independent variables	(1)	(2)	(3)
FavSurname	-0.044	-0.014	0.012
	(-0.909)	(-0.281)	(0.266)
Controls and fixed effects	Identical to Panel A of Table 9 (*Days since last forecast* is omitted)		
Number of observations	213,549	213,549	213,549
Adjusted R^2 (%)	20.68	18.61	24.42

Table 9 (*Continued*)
Forecast quality

This table reports the estimates of pooled OLS forecast quality regressions. In Panel A, the dependent variable is *Accuracy*, measured as the negative value of the absolute difference between an analyst's last one-year-ahead earnings forecast and the actual earnings, scaled by the stock price two trading days prior to the forecast date. *FavSurname* is Americans' favorability of an analyst's surname. In Panel B, the dependent variable is *Forecast bias* (signed forecast error), measured as an analyst's last one-year-ahead earnings forecast minus the actual earnings, scaled by the stock price two trading days prior to the forecast date. In Panel C, the dependent variable is *Days since last forecast*, measured as the natural logarithm of one plus the number of days elapsed since the most recent earnings forecast for a firm was issued by another analyst. The set of controls and fixed effects are identical to those used in Panel A. In parentheses below coefficient estimates are *t*-statistics based on standard errors clustered by analyst. All continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are provided in Appendix C. ***, **, * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 10
Career outcomes

	Dependent variable:					
	All-star		Post-closure/merger termination		Covering visible stocks	
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)
High FavSurname	-0.075	-0.034	-0.172***	-0.084	-0.027	0.004
	(-1.415)	(-0.624)	(-2.814)	(-1.265)	(-0.702)	(0.090)
Mean Accuracy	7.157***	3.581*	-5.226***	-5.707***	7.297***	5.908***
	(4.710)	(1.757)	(-2.634)	(-2.776)	(7.107)	(5.773)
Mean Accuracy×High FavSurname	6.767**	7.744**	-7.590**	-5.603*	0.364	0.956
	(2.279)	(2.393)	(-2.445)	(-1.736)	(0.214)	(0.560)
Brokerage size		0.837***		-0.044		0.175***
		(21.563)		(-1.477)		(10.016)
Female analyst		0.120*		0.125*		-0.010
		(1.756)		(1.781)		(-0.187)
General experience		0.418***		0.361***		0.123***
		(11.007)		(6.912)		(3.907)
Number of firms		0.025***		-0.105***		0.054***
		(7.563)		(-14.346)		(14.386)
Number of industries		-0.004		0.002		-0.087***
		(-0.330)		(0.123)		(-9.318)
Intercept	-0.951***	-5.518***	-1.583***	-0.715***	1.264***	-0.008
	(-20.881)	(-28.318)	(-9.499)	(-2.867)	(26.326)	(-0.076)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	29,685	29,685	5,218	5,218	26,800	26,800
Pseudo R^2 (%)	2.09	25.43	2.71	15.25	3.28	11.24

This table shows the estimates of career outcome regressions. We estimate pooled probit regressions for three dependent variables of analyst career outcomes: *All-star* equals 1 if an analyst is ranked as an all-star analyst by *Institutional Investor* magazine in the following year and 0 otherwise; *Post-closure/merger termination* equals 1 if an analyst disappears from the I/B/E/S within 3 years after his or her brokerage house goes out of business (i.e., closure) or goes through a merger (as either an acquirer or a target) and 0 otherwise; and *Covering visible stocks* equals 1 if an analyst covers at least one stock that ranks in the top decile by total analyst coverage across all stocks in the following year and 0 otherwise. *High FavSurname* is an indicator variable equal to 1 for an analyst whose surname favorability (*FavSurname*) ranks in the top tercile among all analysts in a year and 0 otherwise. *Mean Accuracy* is the mean accuracy of an analyst's last forecasts issued for all firms he or she covers in a year. In parentheses below coefficient estimates are *t*-statistics based on standard errors clustered by analyst. All continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are provided in Appendix C. ***, **, * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Online Appendix to

"An Analyst by Any Other Surname: Surname Favorability and Market Reaction to Analyst Forecasts"

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In this online appendix, we report results for several additional tests. Specifically, we identify underlying factors that form the favorability of a surname. We also examine the impact of surname favorability on post-revision price drifts. Lastly, we provide the robustness of our findings to using an alternative data source and measure of full name favorability.

1. Decomposing Surname Favorability

Several factors can influence surname favorability. For example, Americans' in-group bias against foreigners may be an underlying factor for surname favorability (e.g., Kumar et al., 2015). Also, cultural or ethnic similarities between the United States and the country associated with a surname or the perceived level of the country's corruption are likely to affect surname favorability.

Following prior studies (Hwang, 2011; Kumar et al., 2015), we construct five variables to capture the underlying factors of surname favorability: *Foreignness* captures whether the name of an analyst sounds foreign from American perspective. *Same ancestry* captures the similarity in ancestry between U.S. citizens and people in the countries associated with an analyst's surname. *Same language* captures the proportion of countries associated with an analyst's surname that speak English as the official language. *Cultural distance* captures the mean absolute difference in six dimensions of the Hofstede cultural index between the United States and the countries associated with an analyst's surname. Lastly, *Country corruption* captures the mean level of perceived corruption in countries associated with an analyst's surname.

We first examine the relation between surname favorability and the five aforementioned variables. Panel A of Table O.1 reports results from pooled OLS regressions in which the dependent variable is Americans' favorability of an analyst's surname (*FavSurname*), and the independent variables are each or all of the five underlying factors. Consistent with our conjecture, we find that surname favorability is positively associated with the ethnic and linguistic similarities between the United States and an analyst's country of origin reflected in his or her surname, whereas it is negatively associated with the foreignness of an analyst's name, cultural distance, and the level of perceived corruption in the countries associated with the surname.

Next, we investigate which underlying factor of surname favorability plays the most important role in driving our findings. Using the coefficient estimates and the residual obtained from the pooled OLS regression of *FavSurname* on *Foreignness*, *Same ancestry*, *Same language*, *Cultural distance*, and *Country corruption*, we measure the six components of surname favorability explained by each of these five factors and the residual. For example, we measure the component of surname favorability explained by the foreignness of an analyst's name, *FavSurname* (*Foreignness*), as the coefficient estimate of *Foreignness* times the value of *Foreignness*. The residual component, *FavSurname* (*Residual*), is measured as the residual obtained from the regression.

Panel B of Table O.1 reports results for the baseline regressions using the six components of favorability. In column (1), we find that the component of surname favorability associated with the foreignness of an analyst's name has a significant and positive effect on market reactions to forecast revisions. This result suggests that Americans' in-group bias triggered by foreign-sounding names is an important driver for the estimated effects of surname favorability. We do not find significant effects of favorability components associated with other four factors.

Interestingly, we find significant results using the residual component of surname favorability, *FavSurname* (*Residual*), even after controlling for all other favorability components.[1] The evidence implies that the component of favorability not explained by foreignness, cultural proximity, and country corruption still has important explanatory power for investor reactions to

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[1] In untabulated tests, we include these five factors as control variables in our baseline regression model. We still find significant results for the surname favorability effect.

forecast revisions. This finding clearly distinguishes our work from prior studies examining the effect of in-group bias and cultural proximity in the capital market.

2. Post-revision Price Drift

We now examine whether surname favorability mitigates investors' underreaction to forecast revisions. Gleason and Lee (2003) show that analysts' forecast revisions predict future abnormal returns over the next six months or one year, which is referred to as the post-revision price drift. If the market underreacts to forecast revisions, we should find that surname favorability is associated with weaker underreaction, because it has a positive effect on the immediate reaction to forecast revisions.

Following Gleason and Lee (2003), we measure the abnormal drift return to a forecast revision as the size-adjusted buy-and-hold return (*BHAR*) over the window from trading day $+m$ (m = 2 or 11) to trading day $+n$ (n = 127 or 253), where trading day 0 is the forecast revision date.[2] In Table O.2, we estimate the post-revision price drift regression in which the dependent variable is an abnormal drift return and the main independent variable of interest is the interaction term between *Revision* and *High FavSurname*. *High FavSurname* equals 1 if an analyst has a surname that ranks in the top tercile of surname favorability among all analysts in a calendar quarter and 0 otherwise.

In columns (1) to (4), we report results using our full sample. We do not find significant post-revision price drifts using the full sample, making it difficult for us to make a clear conclusion about the effect of surname favorability on investor underreaction.[3] Thus, as the next step, we

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[2] We use *BHAR* as *CAR* implicitly assumes daily rebalancing and may lead to an upward bias in the return over long periods (Roll, 1983). In untabulated tests, we find that our results are robust to using *CAR* for the post-revision price drift tests.

[3] One potential explanation for not finding a significant drift using the full sample could be because our sample period spans more recent years from 1996 to 2014. Gleason and Lee (2003) use data in the 1990s. Another possible

confine our tests to a subsample of firms and analyst forecasts that are likely to have a significant underreaction to forecast revisions. According to Gleason and Lee (2003) and Zhang (2006), the post-revision price drift is likely to be greater as a firm's information environment is less transparent, when forecasts are issued by analysts who are less visible (i.e., non-all-star), and when forecasts provide more information to investors (i.e., non-herding forecasts). Thus, we construct a subsample by excluding firms in the top tercile of total analyst coverage or institutional ownership and dropping herding forecasts and forecast revisions issued by all-star analysts.[4] We re-estimate the drift regression using the subsample and report results in columns (5) to (8) of Table O.2.

Coefficients on *Revision* are positive and statistically significant across all four columns, suggesting significant drifts in this subsample. More importantly, we also find negative and significant coefficients on *Revision×High FavSurname*, suggesting that surname favorability is associated with a weaker underreaction to forecast revisions.

The results in Table O.2 show that surname favorability also affects price efficiency within a subsample with significant post-revision price drifts. However, the fact that we do not find significant post-revision price drifts nor the effect of surname favorability on drifts in the overall sample raises a question about the extent to which the surname favorability affects market mispricing.

3. Robustness to Alternative Measures

We further test the robustness of our findings to using an alternative measure and a different data source. First, we use a name-based ethnicity classification provided by OnoMap,

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explanation is that we include more control variables and fixed effects.

[4] We find a significant surname favorability effect for the market's immediate reaction to forecast revisions in this subsample (regression models in Panel A of Table 4). In untabulated tests, we find that excluding firms in the top quartile or quintile of analyst coverage or institutional ownership does not change our results. In addition, we find that results are robust to removing the conditions on total analyst coverage or herding forecasts.

which derives data from telephone directories and electoral registers in 26 countries (Mateos et al., 2011). Using the ethnicity classifications in OnoMap, we construct an alternative measure of surname favorability, *FavSName* (*OnoMap*), and find that our main findings remain qualitatively the same. The results are provided in Panel A of Table O.3. Second, using a hand-collected dataset of analysts' full names and the ethnicity classifications in OnoMap, we construct a measure of full name favorability, *FavFName* (*OnoMap*). We find that our results are robust to measuring the favorability of an analyst's full name using a different data source. The results are provided in Panel B of Table O.3.

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Table O.1
Decomposition of surname favorability

Panel A: Potential underlying factors of surname favorability

	Dependent variable: FavSurname					
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)
Foreignness	-0.174***					-0.021***
	(-26.026)					(-5.362)
Same ancestry		0.989***				-0.037
		(52.485)				(-1.252)
Same language			0.167***			-0.093***
			(57.277)			(-13.943)
Cultural distance				-0.010***		-0.014***
				(-86.795)		(-28.657)
Country corruption					-0.054***	-0.003*
					(-47.952)	(-1.677)
Intercept	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	31,443	31,443	31,443	31,443	31,443	31,443
Adjusted R^2 (%)	25.12	43.42	46.92	72.47	54.57	76.05

Panel B: Effects of individual components of surname favorability on market reactions to forecast revisions

	Dependent variable: Size-adjusted CAR [-1, +1]						
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Revision	1.144***	1.086***	1.109***	1.130***	0.941***	1.139***	1.488***
	(4.896)	(4.572)	(4.676)	(4.839)	(3.675)	(4.847)	(4.093)
Revision×FavSurname (*Foreignness*)	4.011*						4.525*
	(1.956)						(1.793)
Revision×FavSurname (*Same ancestry*)		-0.576					-1.668
		(-0.341)					(-0.619)
Revision×FavSurname (*Same language*)			0.102				1.383*
			(0.310)				(1.724)
Revision×FavSurname (*Cultural distance*)				0.124			0.622
				(0.913)			(1.628)
Revision×FavSurname (*Country corruption*)					-3.611		0.669
					(-1.640)		(0.170)
Revision×FavSurname (*Residual*)						1.027***	0.973***
						(4.007)	(3.900)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	753,714	759,544	762,780	762,244	762,568	750,044	750,044
Adjusted R^2 (%)	9.74	9.73	9.73	9.73	9.73	9.74	9.75

Table O.1 (*Continued*)
Decomposition of surname favorability

This table shows results from pooled OLS regressions. In Panel A, we estimate OLS regressions in which the dependent variable is Americans' favorability of an analyst's surname (*FavSurname*), and the independent variables are five potential underlying factors of surname favorability and year fixed effects. *Foreignness* is the percentage of the Amazon Mechanical Turk (AMT) workers who indicate that the name of the analyst is foreign sounding (Kumar et al., 2015). *Same ancestry* is the weighted average of the percentage of U.S. citizens whose ancestors came from countries associated with the analyst's surname. We obtain the ancestry of the U.S. citizens from the U.S. Census Bureau. *Same language* is the weighted average of *English* dummy for countries associated with the analyst's surname. The *English* dummy variable is equal to 1 if English is the official or the most popular language for a country according to the World Factbook by the Central Intelligence Agency (CIA) and 0 otherwise. *Cultural distance* is the weighted average of the culture difference for countries associated with the analyst's surname. The culture difference is measured as the mean value of the absolute differences in the Hofstede index between the United States and the country in question, across all six cultural dimensions. *Country corruption* is the weighted average of negative one times the Corruption Perception Index (CPI) published by Transparency International for countries associated with the analyst's surname. Weights are computed based on the frequency of the nationality of U.S. immigrants who have the same surname as an analyst. In Panel B, we estimate the pooled OLS market reaction regressions in which the dependent variable is the size-adjusted cumulative abnormal return (*CAR*) over the window from trading day -1 to trading day +1, where trading day 0 is an analyst's forecast revision date. *Revision* is the difference between an analyst's current and preceding earnings forecast for a firm, scaled by the stock price two trading days prior to the current forecast date. *FavSurname* (*Component*) is measured as the value of *Component* times the coefficient estimate of *Component*, obtained from the pooled OLS regression of *FavSurname* on *Foreignness*, *Same ancestry*, *Same language*, *Cultural distance* and *Country corruption*. *FavSurname* (*Residual*) is the residual value obtained from the OLS regression. In Panel B, for brevity we suppress coefficient estimates of the stand-alone variables, *FavSurname* (*Component*) and *FavSurname* (*Residual*). The set of control variables is identical to that in Panel A of Table 4. In parentheses below coefficient estimates are *t*-statistics based on standard errors clustered by analyst. All continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are provided in Appendix C. ***, **, * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table O.2
Post-revision price drift

	Dependent variable: Size-adjusted BHAR							
Return window:	[+2,+127]	[+11,+127]	[+2,+253]	[+11,+253]	[+2,+127]	[+11,+127]	[+2,+253]	[+11,+253]
Sample composition:	Full sample				Subsample with significant drifts			
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Revision	-0.570	-0.590	0.749	0.450	2.201**	2.661***	5.607***	6.121***
	(-0.952)	(-1.040)	(0.844)	(0.517)	(2.382)	(3.028)	(4.304)	(4.799)
Revision×High FavSurname	-0.044	-0.057	-0.087	-0.119	-0.209*	-0.201**	-0.324**	-0.330**
	(-0.549)	(-0.751)	(-0.713)	(-0.997)	(-1.951)	(-1.990)	(-2.029)	(-2.137)
High FavSurname	-0.000	0.000	-0.002	-0.002	0.000	0.000	-0.000	-0.000
	(-0.051)	(0.039)	(-0.847)	(-0.809)	(0.094)	(0.036)	(-0.062)	(-0.137)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	762,780	762,780	762,780	762,780	195,530	195,530	195,530	195,530
Adjusted R^2 (%)	15.12	14.16	23.86	23.16	21.88	20.52	32.61	31.67

This table shows results from pooled OLS regressions. The dependent variable is the size-adjusted buy-and-hold abnormal return (*BHAR*) over the next six-month or one-year window, starting from trading day m (m = 2 or 11) and ending on trading day n (n = 127 or 253), where trading day 0 is an analyst's forecast revision date. *Revision* is the difference between an analyst's current and preceding earnings forecast for a firm, scaled by the stock price two trading days prior to the current forecast date. *High FavSurname* is an indicator variable that equals 1 if an analyst ranks in the top tercile by surname favorability (*FavSurname*) among all analysts in a calendar quarter and 0 otherwise. In columns (5), (6), (7) and (8), we estimate the OLS regressions for post-revision price drifts after excluding firms in the top tercile of total analyst coverage or institutional ownership and dropping herding forecasts or forecasts issued by all-star analysts. We define a forecast as herding if it is between the analyst's own prior forecast and the consensus (Gleason and Lee, 2003). The set of control variables is identical to that in Panel A of Table 4. In parentheses below coefficient estimates are t-statistics based on standard errors clustered by analyst. All continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are provided in Appendix C. ***, **, * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table O.3
Alternative measures

Panel A: OnoMap-based measure of surname favorability

	Dependent Variable: Size-adjusted CAR							
	[-1,+1]		[-1,+3]		[-1,+5]		[-1,+10]	
Independent Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Revision	0.889***	0.892**	0.978***	0.984***	1.052***	1.039***	1.007***	0.879**
	(5.334)	(2.428)	(5.863)	(2.615)	(6.420)	(2.636)	(6.112)	(2.099)
Revision×FavSName (*OnoMap*)	0.416**	0.607***	0.390*	0.588***	0.325	0.542***	0.354*	0.587***
	(2.058)	(3.019)	(1.922)	(2.925)	(1.630)	(2.712)	(1.763)	(2.837)
FavSName (*OnoMap*)	0.001	0.001	0.001	0.001	0.000	0.000	-0.001	-0.001
	(0.902)	(0.914)	(0.622)	(0.597)	(0.168)	(0.144)	(-0.486)	(-0.363)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	436,821	436,821	436,821	436,821	436,821	436,821	436,821	436,821
Adjusted R^2 (%)	7.90	9.93	7.36	9.46	6.84	9.01	5.90	8.33

Panel B: OnoMap-based measure of full name favorability

	Dependent Variable: Size-adjusted CAR							
	[-1,+1]		[-1,+3]		[-1,+5]		[-1,+10]	
Independent Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Revision	0.456**	0.073	0.493***	0.110	0.598***	0.213	0.540***	-0.167
	(2.457)	(0.220)	(2.631)	(0.321)	(3.207)	(0.600)	(2.580)	(-0.427)
Revision×FavFName (*OnoMap*)	0.909***	1.143***	0.943***	1.199***	0.840***	1.127***	0.874***	1.208***
	(4.126)	(5.425)	(4.223)	(5.550)	(3.781)	(5.189)	(3.525)	(4.953)
FavFName (*OnoMap*)	0.001	0.001	0.001	0.001	0.000	0.000	-0.001	-0.001
	(0.849)	(0.740)	(0.550)	(0.438)	(0.327)	(0.149)	(-0.574)	(-0.592)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	604,536	604,536	604,536	604,536	604,536	604,536	604,536	604,536
Adjusted R^2 (%)	7.87	9.91	7.28	9.41	6.73	8.93	5.79	8.22

This table shows results for the market reaction regression estimates, using alternative measures of name favorability. We estimate the baseline OLS regressions in which the dependent variable is the size-adjusted cumulative abnormal return (*CAR*) over the window from trading day -1 to trading day n (n = 1, 3, 5, and 10), where trading day 0 is an analyst's forecast revision date. *Revision* is the difference between an analyst's current and preceding earnings forecast for a firm, scaled by the stock price two trading days prior to the current forecast date. In Panel A, we construct an alternative measure of surname favorability, *FavSName* (*OnoMap*), using the surname-ethnicity classifications in OnoMap. In Panel B, we construct an alternative measure of full name favorability, *FavFName* (*OnoMap*), using the full name-ethnicity classifications in OnoMap. Control variables and fixed effects are identical to those in Panel A of Table 4. In parentheses below coefficient estimates are t-statistics based on standard errors clustered by analyst. All continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are provided in Appendix C. ***, **, * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.