An analyst by any other last name: Country favorability and market reaction to analyst forecasts*

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April 24, 2017

Abstract

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Keywords: Equity analysts, country favorability, earnings forecasts, market reaction, social bias.

JEL Codes: G14, G24, J15, J71.

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1. Introduction

Last name of an individual is likely to convey information about the person's ancestry, as the last name is inherited from either one or both parents. For instance, the last name "Yamamoto" is thought to be of Japanese origin, while the last name "Volkmann" is likely to indicate German origin. In fact, according to the U.S. historical immigration records, 99.0 percent (85.8 percent) of the U.S. immigrants with the last name "Yamamoto" ("Volkmann") came from Japan (Germany). Because the culture of the country of origin is known to influence the beliefs and preferences of immigrants to the U.S. over several generations (e.g., Guiso, Sapienza, and Zingales, 2006; Fernández and Fogli, 2009), people may assign certain stereotypes and alter their perceptions about an individual based on that person's country of origin inferred from her last name.

A number of studies in psychology demonstrate that people rely on their subjective feelings in their judgments (Klauer and Stern, 1992; Loewenstein, Weber, Hsee, and Welch, 2001; Slovic, Finucane, Peters, and MacGregor, 2007; Greifeneder, Bless, and Pham, 2011; see Kunda, 1999 for a review). In particular, people assign more positive evaluations to an object they perceive as being more favorable, possibly owing to their desire for consistency in their feelings and judgments (e.g., Klauer and Stern, 1992). Such tendency is also related to the halo effect where overall feelings toward an object bias the evaluations of the object's specific qualities (e.g., Thorndike, 1920; Nisbett and Wilson, 1977).

Motivated by these earlier findings in psychology, in this study, we investigate whether name-induced biases affect the information gathering activities of stock market participants. Specifically, we examine whether market participants assess the information content of an analyst's forecast revision based on the favorability of analyst's country of origin and whether the stock market reaction to the forecast revision is also affected by the analyst's country of origin. Our key conjecture is that analysts with more favorable countries of origin will be evaluated by investors more favorably, which, in turn, would generate stronger market reaction to forecast revisions by those analysts.

Stock market reaction to analyst forecasts provides a good empirical setting to examine whether people's overall feeling about an individual's country of origin affects their evaluation of the individual and the information provided by the individual. Analyst forecasts are important sources of information for investors in their stock investment decisions, and investors' evaluations of the analyst such as forecasting ability influence the strength of their reactions (e.g., Park and Stice, 2000; Clement and Tse, 2003). Investors can easily observe the last name of the analyst when analyst forecasts are released. Consequently, their opinion about the analyst's country of origin inferred from the last name can influence how they evaluate and process the information in the analyst's forecasts.

Following Pan, Siegel, and Wang (2015), we identify countries of origin associated with an equity analyst's last name based on the U.S. historical immigration records from *Ancestry.com*. We then match each analyst's countries of origin and the Gallup poll data on Americans' favorability toward foreign countries. Using 901,751 analyst forecast revisions in the U.S. from 1996 to 2014, we find that market responds more strongly to forecast revisions issued by analysts whose countries of origin are perceived as being more favorable. The finding holds after controlling for a number of analyst, firm, and forecast characteristics that are known to affect the strength of market reactions. Further, our results are economically significant. For example, we find that one standard deviation increase in the favorability of an analyst's country origin translates into a 5.64 percent stronger return-revision relation.[1]

We provide a battery of robustness checks by altering sample restrictions and by adopting different measures of favorability. In particular, the results remain qualitatively similar when we only use the analyst's last forecast for the firm-fiscal year, exclude forecast revisions issued concurrently with forecasts by other analysts or corporate disclosures, and measure favorability in alternative ways. The evidence further supports our conjecture that investors' information processing and investment decisions are affected by their subjective perception such as the favorability of analysts' countries of origin. We also run a falsification test using a placebo measure of favorability. Insignificant results in the falsification test provide further reinforcement that the estimated effect of favorability is not a statistical artifact.

In the next set of tests, we investigate how our results vary in the cross-section. We find that the country of origin effect is more pronounced among firms that have lower institutional ownership. The evidence suggests that individual investors are more strongly influenced by favorability in their investment decisions. These results are in line with prior findings that individual investors are relatively less sophisticated than institutional investors and exhibit stronger biases in processing information (e.g., Bartov, Radhakrishnan, and Krinsky, 2000; Bonner, Walther, and Young, 2003).

Next, we investigate whether our finding is affected by the difficulty in inferring the country of origin based on an analyst's last name. If the majority of U.S. immigrants with the same last name as an analyst come from a single country, it would be easier for investors to infer the analyst's country of origin. As a result, market participants would be more strongly influenced by the analyst's country favorability in their investment decisions. In our subsample

[1] The interpretation is based on column (2) in Panel A of Table 3: 0.360 (coefficient on *Revision*×*FavOrigin*) × 0.112 (standard deviation of *FavOrigin*) ÷ 0.715 (coefficient on *Revision*) = 0.0564

analysis, we find that the favorability effect is stronger when the analyst has a last name whose country of origin is easier to infer.

In additional tests, we examine whether the favorability effects vary with the level of analyst reputation. We conjecture that investors will be influenced less by an analyst's country of origin if the analyst is already well-recognized in the profession. Consistent with our conjecture, we find that forecast revisions issued by all-star analysts are less strongly affected by perceptions of favorability about their countries of origin. Also, we find that the favorability effects exhibit an asymmetry across the sign of revision news: country favorability is more important when analysts deliver bad news in forecast revisions than good news.

We conduct several additional tests to further identify the favorability effect. Since analysts issue multiple forecast revisions for a firm in a given year and the favorability rating for a particular country is updated when a new Gallup poll is conducted, we add additional fixed effects in our empirical specification. Specifically, we include analyst×year, firm×year or analyst×firm fixed effects to eliminate the confounding effects of unobservable factors that are constant within analyst-year, firm-year, or analyst-firm combination. These unobservable factors include analyst background, firm characteristics, and cultural commonalities between an analyst and the firm she follows (e.g., a Chinese analyst follows a Chinese firm in the U.S.). We find that the favorability effects are robust to including those fixed effects.

Our second identification strategy is motivated by the observed time-series variation in the country favorability. To better establish the causal relation between favorability of an analyst's country of origin and market reactions to forecast revisions, we use the September 11 (9/11) terrorist attacks as a natural experiment that adversely affected perceptions of favorability toward Middle Eastern countries (e.g., Anderson, 2002; Kumar, Niessen-Ruenzi, and Spalt, 2015). In this test, we define treatment analysts as those having last names of Middle Eastern origins and identify control analysts using Coarsened Exact Matching (CEM) algorithm (Iacus, King, and Porro, 2011; DeFond, Erkens, and Zhang, 2016). Using the matched sample, we implement a difference-in-difference test and find a significant decline in the strength of market reactions to forecast revisions by analysts with Middle Eastern origins after the 9/11 attacks compared to the control group.

In the next set of tests, we examine whether analysts' forecast quality and career outcomes are associated with the favorability of their countries of origin. We find that there is no significant relation between the favorability of analysts' countries of origin and their forecast quality such as accuracy, bias, and the timeliness. This evidence suggests that investors' subjective perceptions such as country favorability affect their reaction to information even when it is not related to objective measures of information quality. We also find no systematic relation between favorability of analysts' countries of origin and their career outcomes. This result suggests that employers at a brokerage house are not affected by the favorability of analysts' counties of origin when evaluating their performance.

To identify the specific channels that generate our findings, we decompose the favorability measure into components that are associated with various factors. As potential factors that can affect perceptions of country favorability, we consider the foreignness of an analyst's name (Kumar, Niessen-Ruenzi, and Spalt, 2015), the similarities in ancestry, language, and culture between the U.S. and an analyst's country of origin (Hwang, 2011; Du, Yu, and Yu, 2016; Jia, Wang, and Xiong, 2016), and perceived level of corruption in the analyst's countries of origin (Hwang, 2011). Using five coefficient estimates and the residual obtained from the regression of favorability on these five factors, we examine a total of six different components of favorability that are explained by these factors.

We find that the favorability component associated with the foreignness of an analyst's name has a significant positive relation to market reaction to forecast revisions. This result suggests that in-group bias is likely to be an important driver of our results. More interestingly, we find strong evidence that the residual component of favorability, which is orthogonal to these underlying factors, significantly and positively affects market reaction to forecast revisions. This evidence indicates that the component of favorability that is not explained by the aforementioned factors is an important determinant of market reactions to forecast revisions. This finding also distinguishes our work from prior studies that examine the impact of cultural proximity or in-group bias on financial markets (e.g., Kumar, Niessen-Ruenzi, and Spalt, 2015; Du, Yu, and Yu, 2016; Jia, Wang, and Xiong, 2016).

These findings contribute to a growing literature in accounting, economics, and finance that examines the impact of cultural origins on the behavior of capital market participants. In particular, Hwang (2011) finds that a country's favorability in the U.S. is negatively related to the price discount of securities from that country in the U.S. market, suggesting that favorability affects investment decisions. Further, Kumar, Niessen-Ruenzi, and Spalt (2015) show that names can generate an in-group bias against foreigners as people adopt positive opinions about members of their own group compared to those who are outside of their group. Consequently, mutual fund investors are less likely to invest in mutual funds that are managed by individuals with foreign sounding names.

More recently, Brochet, Miller, and Yu (2016) find that managers' cultural background affects their disclosure tone but that market investors do not distinguish between the cultural backgrounds of managers, conditional on the disclosure tone. Du, Yu, and Yu (2016) show that

U.S. analysts with Chinese ethnic origins issue more accurate forecasts and elicit stronger market reaction for Chinese firms because of their informational advantage over non-Chinese analysts. Jia, Wang, and Xiong (2016) find that local (foreign) investors respond more strongly to stock recommendations by local (foreign) analysts in China.

Our study is most closely related to Kumar et al. (2015). Like their paper, our results suggest biases induced by a person's name, but there are significant differences. First, we focus on the effect of name-induced perception on investor reaction to analyst forecast revisions, while Kumar et al. (2015) focus on the effect of name on investment choices of mutual fund investors. Second, we use perceptions of favorability toward different countries rather than focus on the in-group bias. We also confirm that the estimated effect of favorability is not fully explained by in-group bias that is triggered by the foreignness of an analyst's name. In addition, by using the Gallup surveys conducted over years, we are able to capture time-variation in perceptions of favorability toward a given country. This is distinct from prior studies that rely on time-invariant innate traits such as ethnicity and race (e.g., Chinese vs. non-Chinese groups) (Kumar, Niessen-Ruenzi, and Spalt, 2015; Du, Yu, and Yu, 2016; Jia, Wang, and Xiong, 2016).

Our findings also contribute to the literature on the determinants of stock market reaction to analyst forecast revisions. Prior studies have mostly focused on the effects of analyst attributes in their professional domains such as forecasting experience and accuracy on market reaction to forecast revisions (e.g., Park and Stice, 2000; Clement and Tse, 2003). These attributes are related to the credibility and the information content of forecast revisions. Accordingly, rational investors have incentives to react more strongly to forecast revisions of analysts who are superior in such attributes. Indeed, prior studies find that institutional ownership tends to strengthen the effect of these attributes on market reactions to forecast revisions (e.g., Bonner, Walther, and Young, 2003), suggesting that more sophisticated investors are better able to utilize the information regarding the forecast quality and credibility. In contrast, our paper offers a new insight into the market reaction to analyst forecast that is unrelated to the information content of the forecast signal. We suggest that investors' overall favorability of analysts' cultural origins may influence market reactions to forecast revision even though it is not significantly related to analysts' forecasting ability. It is noteworthy that unlike most analyst attributes related to forecasting performance, country favorability mainly affects investment decisions of relatively less sophisticated individual investors.

2. Data and Summary Statistics

In this section, we describe our sample selection procedure and introduce two main datasets: (1) a hand-collected dataset on countries of origin for equity analysts in the U.S. and (2) the dataset of Americans' favorability of foreign countries in Gallup survey data. We also present descriptive statistics for 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 (IBES). To avoid the potential rounding problems pointed out by Diether, Malloy, and Scherbina (2002), we directly adjust the IBES 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 as the forecast is likely subject to data error. We also delete earnings forecasts associated with more than one analyst's last name in the IBES in order to establish a clear link between the identity of an analyst and market reactions to the analyst's forecast revision. Then, 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 901,751 firm-year-analyst-forecast horizons for 7,765 unique analysts and 6,611 unique firms from 1996 to 2014. We restrict the sample to begin in 1996 because data on Americans' favorability of foreign countries in Gallup surveys, which we discuss in Section 2.3, are relatively scarce in its coverage prior to 1996. We winsorize all continuous variables at the 1% and 99% levels.

2.2. Countries of Origin for Equity Analysts in the U.S.

Following Pan, Siegel, and Wang (2015), we construct a proxy for the countries of origin for equity analysts in our sample based on the U.S. historical immigration records retrieved from *Ancestry.com*. Using the last names of individual analysts in IBES, we collect the nationality of all U.S. immigrants whose last names are identical to those of our sample analysts and who immigrated to the U.S. through the port of New York between 1820 and 1957. Specifically, we identify 7,765 unique analysts in the sample and collect 14,883,568 immigration records of the U.S. immigrants whose last names are identical to our sample analysts' and have non-missing nationality information (5,197 unique last names). We then manually check the correctness of nationalities in the immigration records. For example, we correct trivial spelling errors such as Franch, Filipino, and Mecican to France, Philippines, and Mexico, respectively. Next, we assign some nationalities to a country name (e.g., England, Scotland, and Wales into Great Britain).[2]

[2] We refer to the data processing procedure in Pan, Siegel, and Wang (2015).

This procedure allows us to reclassify 1,708 unique nationalities in the raw file of the immigration records into 116 countries of origin.[3] In Appendix A, we provide summary statistics and a distribution of 116 countries of origin for the U.S. immigrants with the same last names as the analysts in our sample.

2.3. Country of Origin and Perception of Favorability

We measure investors' favorability toward analysts' countries of origin using the responses of Gallup survey participants to the following question: "*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?*" While Gallup provides a time-series dataset on Americans' subjective opinion about a total of 42 foreign countries dating back to 1989, we use data from 1996 because Gallup Analytics cover relatively few countries prior to 1996 and the forecast dates in IBES are often inaccurate in the early 1990s (Clement and Tse, 2003; Cooper, Day, and Lewis, 2001; Jiang, Kumar, and Law, 2016).[4]

Table 1 reports the summary statistics for Americans' favorability of foreign countries in Gallup Analytics from 1996 to 2014. Each number indicates the average percentage of survey participants who selected a given item on the five favorability rating scales.[5] It is noteworthy that there is a considerable variation in Americans' favorability toward foreign countries. For example, Iran is perceived as "Very Favorable" by only 1.8 percent of Americans participating in

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[3] Among the 116 countries in our classification, we have "USA", "Unidentifiable" (if a nationality is non-missing but indiscernible), and other uninformative categories indicating geographic locations (e.g., Asia and Central America) or ethnic groups (e.g., Hispanic and Jewish). We do not use these categories throughout the empirical analyses, unless otherwise stated. If an immigrant has a dual nationality (e.g., USA and France), we select the former. However, we choose the latter (1) if the former is USA whereas the latter is not or (2) if the latter is the only one that is covered by the Gallup surveys.

[4] Prior to 1996, Gallup has carried out the surveys on 11, 17, 6, and 3 countries in 1989, 1991, 1992 and 1993, respectively. The survey was conducted every year in the 2000s.

[5] We combine four uninformative rating scales in Gallup surveys such as Don't Know, Refused to Answer, Never Heard Of, and Can't Rate into Others.

the Gallup surveys, while Canada is viewed as "Very Favorable" by 45.0 percent of the Americans.

In the study, we use the 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 indicates the average level of Americans' favorability toward a country during the sample period from 1996 to 2014. Canada and Iran are ranked as the most and least favored foreign countries by Americans with the mean favorability level of 90.57 and 10.87 percent, respectively.

For each individual analyst in the sample, we match the countries of origin associated with the analyst's last name and the Americans' favorability of foreign countries.[6] For each forecast revision of an analyst, we compute the weighted average of the most recent favorability ratings toward countries associated with the analyst's last name. We use this value as our measure of investors' favorability toward the analyst's countries of origin (*FavOrigin*). Similar to Pan, Siegel, and Wang (2015), for countries associated with a given last name, we only retain countries having a non-missing favorability data in Gallup Analytics and assign a weight to each country based on the frequency of nationalities that the U.S. immigrants with the same last name have. A distinct feature of our measure is that we are able to observe a time-series variation in Americans' perception of a given analyst's countries of origin. This is different from the recent studies on the effects of cultural origins that rely on a time-invariant trait such as ethnicity (Du, Yu, and Yu, 2016; Jia, Wang, and Xiong, 2016) or foreignness of names (Kumar, Niessen-Ruenzi,

[6] We note that South Korea and North Korea are not distinguished in the U.S. historical immigration records from *Ancestry.com*, while Gallup Analytics provides favorability ratings on South and North Korea separately. In the paper, we assume that U.S. immigrants with the nationality of Korea in 1800s and 1900s are from South Korea because North Korean refugees have been allowed to enter the U.S. after the passage of the *North Korean Human Rights Act* in October 2004. In untabulated tests, we drop immigration records related to Korea and find that our results remain unchanged.

and Spalt, 2015).

2.4. Summary Statistics

We report the summary statistics for our variables in Table 2. Panel A of Table 2 shows that Americans' favorability toward analysts' countries of origin, *FavOrigin*, has the mean and median of 0.787 and 0.809, respectively. This suggests that analysts in our sample have last names originated from countries that are viewed favorably by about 80 percent of Americans. Note that 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.[7] Overall, we find that summary statistics for control variables are consistent with prior studies (Clement, Hales, and Xue, 2011; Jiang, Kumar, and Law, 2016).

In Panel B of Table 2, we divide the sample into two groups based on the sample median of *FavOrigin*, and compare analyst, forecast and firm characteristics of the two groups. According to the results from mean difference *t*-tests, we find that there are no significant differences between analysts with more and less favorable countries of origin in their average forecast quality such as forecast accuracy and bias. For other variables, we find that analysts with more favorable countries of origin tend to work for smaller brokerage houses, update forecasts more quickly after others' forecasts, issue forecasts earlier (longer forecast horizons), and have longer experience in the profession. They also tend to cover larger firms with a higher book-to-market ratio and higher institutional ownership, compared to analysts with less favorable countries of origin. Overall, Panel B of Table 2 emphasizes the importance of controlling for

[7] In untabulated tests, we find that our results are not affected by the logarithm transformation.

analyst, forecast, and firm characteristics in our regression analyses.

3. Country Favorability and Stock Market Reaction

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

3.1. Market Reaction Regression Estimates: Baseline Results

Attitudes or affective feelings such as favorability toward an object exert strong influence on people's judgments (Klauer and Stern, 1992; Loewenstein, Weber, Hsee, and Welch, 2001; Slovic, Finucane, Peters, and MacGregor, 2007; Greifeneder, Bless, and Pham, 2011). In particular, people tend to seek consistency in their feelings and evaluations (Klauer and Stern, 1992) and are susceptible to the so-called halo effect in which people extrapolate overall favorability to the evaluations of other attributes (Thorndike, 1920; Nisbett and Wilson, 1977). Thus, we conjecture that investors' favorability toward an analyst's countries of origin influences their evaluations of the analyst and her forecasts, affecting their reactions to her forecast revisions.

Our main hypothesis is that analysts with last names originated from more favorable countries would elicit stronger market reactions to their earnings forecast revisions. To examine the relation between the favorability of analysts' countries of origin 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.[8] Following recent studies (Gleason and Lee, 2003; Jiang, Kumar, and Law, 2016),

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[8] In untabulated tests, we find that results are essentially unaffected by using an equally-weighted market return as

we measure market reactions over four different estimation windows, beginning on trading day -1 and ending on trading day +1, +3, +5, and +10 of the revision date.

Our main research question concerns the effect of investors' favorability toward an analyst's countries of origin on market reactions to her forecast revisions. Thus, we focus the coefficient estimate of the interaction term between the favorability of an analyst's countries of origin and her forecast revision (*FavOrigin*×*Revision*) in our regressions. According to our conjecture that investors react more strongly to forecast revisions issued by analysts with more favorable countries of origin, we predict the coefficient to be positive.

As for control variables, we include firm, analyst, and forecast characteristics that are known to affect market responses to forecast revisions (Clement and Tse, 2003; Gleason and Lee, 2003; Clement, Hales, and Xue, 2011; Jiang, Kumar, and Law, 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 (i.e., momentum), and number of analysts following the firm. A set of controls for analyst characteristics includes brokerage size, forecast frequency, firm-specific experience, general experience, and the number of firms and the number of industries the analyst follows. We also include an analyst's forecast accuracy for a given firm in the previous year (i.e., lagged accuracy) to rule out the possibility that stronger market reactions to analysts with more favorable countries of origin are driven by their superior forecast quality. Lastly, we control for forecast characteristics such as number of days since last forecast as a proxy for the new information content in forecast revisions (Cooper, Day, and Lewis, 2001) and forecast horizon to capture the walk-down pattern in analyst forecasts (e.g., Ke and Yu, 2006).

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

In all regression specifications, we include firm fixed effects and year fixed effects to capture unobservable and time-invariant firm and year attributes. Following Bradshaw, Brown, and Huang (2013), we cluster standard errors at the analyst level to allow for correlations in residuals within each analyst group (Petersen, 2009).[9]

In Panel A of Table 3, we estimate the baseline OLS regression to examine the impact of the favorability of analysts' countries of origin on market reactions to their forecast revisions. Model specifications vary across columns in terms of the set of control variables included and the return estimation window for the dependent variable.

Consistent with our prediction, we find that the coefficient on the interaction term between *Revision* and *FavOrigin* is significant and positive across all columns. In untabulated tests, we also find that the results do not change when we replace *FavOrigin* with its quintile rank or a dichotomized variable (i.e., one if the value is above the sample median and zero otherwise). Our results suggest that, all else being equal, investors respond more strongly to forecast revisions issued by analysts with last names originated from more favorable countries. Our results are also economically meaningful. For example, based on column (2) which includes the entire set of control variables, one standard deviation increase in *FavOrigin* translates into a 5.64 percent increase in return-revision relation.[10] Overall, the finding in Panel A of Table 3 suggests that the favorability of analysts' countries of origin affects market responses to forecast revisions.

3.2. Alternative Explanations and Robustness Checks

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

[10] 0.360 (coefficient on *Revision*×*FavOrigin*) × 0.112 (standard deviation of *FavOrigin*) ÷ 0.715 (coefficient on *Revision*) = 0.0564

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. We report results from the last forecast sample in Panel B of Table 3. All model specifications are identical to those used in the corresponding columns of Panel A. For brevity, we only present the coefficient estimates of the interaction term between *Revision* and *FavOrigin*. Although the sample size shrinks considerably by 72.23% to 250,405 firm-year-analysts in Panel B, we find slightly weaker but statistically significant results across all columns.

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 IBES and First Call, respectively.[11] As reported in Panel C of Table 3, we find that results are similar to those obtained in Panel A. All coefficient estimates of the interaction term between *FavOrigin* and *Revision* are statistically significant except for columns (7) and (8) that use [-1, +10] window for CARs. The results suggest that our findings are not driven by any confounding effects of other analysts' forecasts and corporate announcements.

Third, we address a possible concern that our results may be sensitive to the way we construct the country favorability measure. In Gallup Analytics, Americans' favorability of a country is measured on the five-point Likert scales: Very Favorable, Mostly Favorable, Mostly Unfavorable, Very Unfavorable, and Others. One possible concern is that survey data based on

[11] In this analysis, we restrict the sample period up to 2010 due to the availability of managerial forecast data in the First Call's Company Issued Guidance (CIG) database.

the Likert scales may be subject to a central tendency bias (e.g., Peer, Vosgerau, and Acquisti, 2014). Central tendency bias refers to the tendency of survey participants to select moderate Likert items, such as "Mostly Favorable" and "Mostly Unfavorable" in the Gallup survey of country favorability, when they have a neutral or unsure opinion about a question.

To alleviate the possibility that we assess favorability partly based on survey participants' neutral or unsure opinions, we now solely consider survey responses to the most extreme Likert item, "Very Favorable," to measure the favorability of a country. The first row of Panel D of Table 3 shows that the results using this alternative measure of favorability are very similar to those obtained in Panel A. In untabulated tests, we alternatively construct an inverse measure of the favorability (*UNFavOrigin*) using the most extreme item on the opposite side, "Very Unfavorable", and find that the coefficient on *Revision*×*UNFavOrigin* is negative and significant across all columns.

We also use another alternative definition of the level of favorability toward a country by taking into account the entire distribution of 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 in the second row of Panel D of Table 3.[12] Overall, the results reported in Panel D of Table 3 alleviate the concern that our results may be sensitive to the way we define the country favorability measure.

Next, we investigate whether our results are robust to considering only a small number of dominant country origins that are strongly associated with a given last name when computing

[12] 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).

the favorability of an analyst's country origins. In Panel E of Table 3, we find that our results are essentially unaffected by considering either one or three most dominant origins associated with each last name.

Finally, we conduct a falsification test by using a placebo measure of favorability of an analyst's countries of origin constructed as follows. For each country of origin, we use the next country in the alphabetically ordered list of 116 countries in Appendix A as a placebo country of origin. Then the placebo measure of favorability, *FavOrigin (P),* is computed using the favorability ratings of the analyst's placebo countries of origin. We re-estimate the baseline OLS market reaction regressions using this placebo measure of *FavOrigin (P)* and report the results in Table 4. We find no statistical significance on the interaction term, *Revision×FavOrigin (P)*, across all but column (8) which is marginally significant at the 10 percentile level. This provides further reinforcement that our finding is not a mere statistical artifact. Taken together, the evidence in this section shows the robustness of our finding that the favorability of analysts' countries of origin is positively associated with the strength of market reactions to their forecast revisions.

3.3. Subsample Analyses

Thus far, we have established that investors respond more strongly to forecast revisions issued by analysts who have more favorable countries of origin. This finding naturally poses a question of whether the effect of favorability on investors' reactions would differ with their levels of sophistication. Numerous prior studies find evidence that individual investors are less sophisticated compared to institutional investors, and have limited ability in correctly processing information and incorporating its implication into stock prices, and thereby biasing market reactions to the information (e.g., Bartov, Radhakrishnan, and Krinsky, 2000; Bonner, Walther, and Young, 2003; Collins, Gong, and Hribar, 2003). Thus, we predict that the positive favorability effect on market reactions will be more pronounced for firms that have higher levels of ownership by individual investors. We use the level of common shares held by institutional investors as a proxy for investor sophistication (e.g., Collins, Gong, and Hribar, 2003).

We divide the sample into two groups based on the sample median of institutional ownership and re-estimate our baseline market reaction regressions for each subsample separately. In Panel A of Table 5, we report the results for the two subsamples formed by institutional ownership. For brevity we only present the coefficient estimate of the *Revision*×*FavOrigin* interaction term. Consistent with our prediction, we find a clear distinction between the two subsamples in statistical significance for the coefficient estimate of the *Revision*×*FavOrigin* interaction term. While the results using the subsample of low institutional ownership continue to remain significant, we do not find significant results using the subsample of high institutional ownership.[13] The evidence indicates that our findings are predominantly driven by individual investors, suggesting that their reactions to forecast revisions are more likely to be susceptible to subjective opinions about analysts' countries of origin.

Next, we investigate whether the favorability effect on market reactions varies depending on the difficulty in inferring countries of origin from a given last name. If an analyst has a last name commonly used by people in a certain country, it should be easier to infer countries of origin from the analyst's last name. In contrast, if the analyst's last name is used in several countries without one particularly dominating country, it would be difficult for investors to infer the countries of origin from the last name. For example, one of our sample analysts has a last name of Yamamoto and 99.0 percent of the U.S. immigrants with the same last name came

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

from Japan according to the U.S. historical immigration records. Thus, it would be easy for investors to infer that the analyst with the last name Yamamoto is of Japanese origin. In contrast, another analyst in our sample has a last name of Boris and this last name does not have one particular dominating country of origin. According to the U.S. immigration record dataset, Poland is the most common country of origin for Boris and yet it only accounts for 12.76 percent of the U.S. immigrants with the same last name.

We construct a proxy for the difficulty level of inferring a last name's origin as follows. We assume that it is easier to infer the origin of a last name when a higher percentage of the U.S. immigrants with the same last name came from its most common country of origin for the name (e.g., Japan for Yamamoto). We divide the sample into two subsamples according to the sample median of the percentage of U.S. immigrants from one single most common country. We re-estimate our baseline regressions using the two subsamples separately and report results in Panel B of Table 5. Using the subsample of easy names to infer, we find that the coefficient estimate of the *Revision*×*FavOrigin* interaction term is positive and significant across all columns. In contrast, using the subsample of difficult names to infer, we find that the coefficient estimates are insignificant in five columns and marginally significant in the remaining three columns. The results suggest that favorability plays a greater role when investors can easily infer analysts' countries of origin from their last names.

We now investigate whether the favorability effect is weaker for analysts who have greater reputation. Our conjecture is that the importance of an analyst's countries of origin will diminish when the analyst is well-recognized in the profession. To test this conjecture, we construct two subsamples based on analyst reputation, proxied by an indicator variable for whether an analyst has been ranked as an all-star analyst in the *Institutional Investor* magazine (i.e., all-star vs. non-all-star analysts). In Panel C, we re-estimate the baseline market reaction regressions and find asymmetric results across two subsamples. The favorability effects are very weak or insignificant for all-star analysts, while the effects remain strong for the others.[14] The results are consistent with our conjecture that country favorability does not have strong effects on investors' evaluation of the analysts who are already well-recognized in the profession.

Finally, we test if there a variation in the favorability effect conditional on the signs of forecast revisions. We divide the sample into two subsample based on whether an analyst's forecast revision is positive or negative. In Panel D, we re-estimate the baseline market reaction regressions using the two subsamples. We find that the favorability effects are more pronounced when forecast revisions contain bad news than good news.

4. Additional Identification Tests

In this section, we further develop the identification of the favorability effects by using various fixed effects and a natural experiment.

4.1. Variations within analyst-year, firm-year, or analyst-firm

Analysts issue multiple forecast revisions for a firm-year pair and country favorability ratings are updated at the date of each Gallup survey which normally takes place in every February or occasionally twice a year. Motivated by these features of our sample, we use fixed effects models to exclude confounding effects at the analyst-year, firm-year, or analyst-firm levels.

[14] To alleviate the concern that strong results for non-all-star analysts are owing to its larger sample size, we make a smaller subsample of non-all-star analysts by selecting analysts who have never been ranked as an all-star analyst despite a sufficient amount of general experience (i.e., greater than 10 years) to be nominated. This yields the subsample of 223,157 observations, which is more comparable in size to the subsample of all-star analysts in Panel C. In untabulated tests, we still find strong results for non-all-star analysts.

In Table 6, we re-estimate the baseline market reaction regressions after including combinations of additional fixed effects.[15] In columns (1) and (2), we control for analyst×year fixed effects and/or industry fixed effects, which will absorb the effects of analyst-year factors such as an analyst's background, education, and experience. The evidence on strong favorability effects 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 6 to control for other unobservable factors at the firm-year or analyst-firm levels such as firm characteristics and the connection between the analyst and the firm. Across all columns, we find strong results for the favorability effect, suggesting that favorability of an analyst's countries of origin is indeed an important determinant of market reactions to forecast revisions.

4.2. Natural Experiment: 9/11 Terrorist Attacks

To better establish a causal relation between the favorability of analysts' origin and market reaction to forecast revisions, we carry out a difference-in-differences test using the September 11 terrorist attacks as a natural experiment.

In the aftermath of the terrorist attacks on September 11, 2001, the Federal Bureau of Investigation (FBI) reported a surge in hate crimes and harassments against Muslims, Arabs, and others who are thought to be of Middle Eastern origins (Anderson, 2002). Academic articles also report evidence that discrimination and prejudice against Muslims rose substantially following September 11, 2001 (Sheridan, 2006; Kumar, Niessen-Ruenzi, and Spalt, 2015). Thus, the September 11 attacks provide a natural experiment in which we can exploit an exogenous shock

[15] In untabulated tests, we find that results are qualitatively similar when the dependent variable is measured over different return windows such as [-1,+3], [-1,+5], and [-1,+10]. For brevity, we do not display the results in Table 6.

that adversely affects Americans' favorability of Middle Eastern origins. In Figure 2, we plot the average level of Americans' favorability of Middle Eastern countries whose favorability data are available prior to the 9/11 attacks. Figure 2 shows a drop in the favorability of Middle Eastern origins around the 9/11 attacks.

We construct a matched sample to compare the change in market reactions to forecast revisions around the 9/11 attacks between analysts of Middle Eastern origins and those of other origins. Recent studies find that one of the most popular matching methods, Propensity Score Matching (PSM), generates results that are fragile and sensitive to fairly minor changes in design choices (DeFond, Erkens, and Zhang, 2016; Shipman, Swanquist, and Whited, 2016).[16] Thus, in our analyses, we use Coarsened Exact Matching (CEM) algorithm that outperforms PSM by achieving better covariate balance between the treatment and control groups (Iacus, King, and Porro, 2011). Our treatment group consists of analysts who have last names of Middle Eastern origins (Middle Eastern analysts, hereafter).

We define a last name as Middle Eastern if more than 30 percent of the U.S. immigrants with the same last name have a nationality that corresponds to a Middle Eastern country, "Arab" or "Muslim" in the U.S. immigration records.[17] The list of 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 the inclusion does not alter the inferences of our results.

We identify 16 Middle Eastern analysts at the end of year 2000, which immediately

[16] In addition, PSM is subject to the random matching problem which occurs when treatment groups are matched with control groups based on a scalar (i.e., propensity score), ignoring the dimensionality of matching covariates (e.g., DeFond, Erkens, and Zhang, 2016).
[17] In untabulated tests, we use different cutoffs such as 20, 40, and 50 percent and find qualitatively the same results. When we set a cutoff higher than 40 percent, less than 10 individual analysts are identified as Middle Eastern analysts at the end of year 2000, making our empirical analyses almost infeasible.

precedes the September 11 terrorist attacks in 2001. We match Middle Eastern analysts and control analysts on the following matching covariates: favorability level of the analyst's origin (*FavOrigin*), mean accuracy of the last forecasts across firms, brokerage size, forecast frequency, general experience, and 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 number of industries covered in a year from the set of matching covariates.[18]

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, King, and Porro, 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. For all empirical analyses in this section, we compensate for the different sizes of strata by imposing CEM weights to each stratum based on the formula introduced in Iacus, King, and Porro (2011).

Next, to capture a change in the favorability effect on market reactions before and after the 9/11 attacks, we retrieve all forecast revisions of the matched analysts issued between 1996 and 2006. To alleviate any confounding effects, we exclude the transition year of 2001 from our empirical analyses and set the pre- and post-9/11 periods with five-year equal length (1996-2000 vs. 2002-2006). The matching procedure yields a matched sample of 26,032 firm-year-analyst-forecast horizons, which consists of 14 Middle Eastern (treatment) and 219 control analysts.

Panel A of Table 7 reports descriptive statistics for matching covariates between the two analyst groups. We find that none of the matching covariates are significantly different between the two analyst groups, indicating that our matching procedure effectively identifies control

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

analysts that are highly analogous to Middle Eastern analysts.

We then demonstrate the validity of this setting as a natural experiment. Specifically, we investigate whether Americans' favorability toward the origins of Middle Eastern analysts decrease after the 9/11 terrorist attacks, relative to the change in the favorability toward those of the control analysts. In every December during the pre- and post-9/11 periods, we compute the favorability of origins of all treatment and control analysts in the sample, using the most recent survey data available in Gallup Analytics. In Panel B of Table 7, we estimate OLS regressions in which the dependent variable is the favorability level of an analyst's origins (*FavOrgin*). We find that the coefficients on the interaction term between Middle Eastern and Post-9/11 attack are significantly negative. The results suggest that Middle Eastern analysts experience a significant decline in the favorability level after the 9/11 attacks, as compared to the control analysts during contemporaneous periods. Overall, the empirical evidence in Panel B provides a further justification to using the 9/11 terrorist attacks as an exogenous shock that adversely affects Americans' favorability of Middle Eastern origins.

Next, to examine whether market responses to forecast revisions by Middle Eastern analysts become weaker after the 9/11 terrorist attacks, we carry out a difference-in-differences test using the CEM matched sample. We estimate the same baseline regression model used in Table 3, with the only difference that we replace *FavOrigin* with two indicator variables, *Middle Eastern* and *Post-9/11 attacks,* and their interaction term. We report the results in Panel C of Table 7.[19] Consistent with our conjecture, we find a negative and significant coefficient on the three-way interaction term of *Revision*×*Middle Eastern*×*Post-9/11 attacks* across all columns,

[19] For brevity, we do not display results based on the other two dependent variables, *CAR* [-1,+3] and *CAR* [-1,+5]. In untabulated tests, we find qualitatively the same results.

suggesting that Middle Eastern analysts experience a significant decrease in market responses to their forecast revisions after the 9/11 attacks, as compared to the control analysts whose last names are not originated from Middle Eastern countries. We note that, in columns (3) and (6), the results remain strong even after we additionally include analyst fixed effects and their interaction terms with *Revision*, which considerably boosts our identification of the favorability effects. Our results are also economically significant: According to the coefficient estimates in column (1), the positive return-revision relation becomes weaker by 89.52 percent in its magnitude after the 9/11 attacks relative to before for Middle Eastern analysts.[20] Overall, the evidence in Table 7 provides further credence to our finding that Americans' favorability of analysts' origins influences market responses to forecast revisions.

5. Country Favorability, Forecast Quality, and Career Outcomes

In this section, we examine the effect of the favorability of analysts' countries of origin on their forecast quality or career outcomes.

5.1. Forecast Accuracy, Bias, and Timeliness

Thus far, we have found positive favorability effects using the baseline regressions that control for the analyst's past forecast accuracy. As a further test to distinguish the effect of favorability from that of forecast quality, we examine the relation of forecast accuracy, bias or timeliness to the favorability of analysts' countries of origin. If we find that analysts from more favorable countries of origin issue more accurate, less biased, or timelier earnings forecasts, the positive favorability effect may be attributable to investors' rational decision-making that puts higher weights on better quality forecasts. On the other hand, if we find no significant relation

[20] $1.581 \div (0.678+1.088)=0.8952$

between such forecast quality and the favorability level of an analyst's countries of origin, the result will lend a further support to our explanation that subjective perception such as favorability affects investors' processing of information.

First, we examine whether analysts with more favorable countries of origin issue more accurate earnings forecasts. 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 the forecast accuracy regression models, we control for a host of firm, analyst, and forecast characteristics that are known to affect forecast accuracy (e.g., Clement, 1999; Kumar, 2010; Jiang, Kumar, and Law, 2016): Book-to market, brokerage size, days since last forecast, 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.[21] To capture any unobservable firm characteristics or time trends, we control for firm and year fixed effects in the regressions.

Panel A of Table 8 reports results for the accuracy tests. We find that the coefficient estimate of *FavOrigin* is almost close to zero and statistically insignificant, regardless of model specifications. The results suggest that favorability of analysts' countries of origin is not systematically associated with forecast accuracy, supporting the behavioral explanation that our findings on the positive favorability effect on market reactions are driven by investors' subjective perception rather than rational weighting based on forecast quality.

Next, we measure forecast bias 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

[21] In untabulated tests, we find that results are virtually not affected when we include a firm's past 12-month return (i.e., momentum).

the stock price two trading days prior to the forecast date (e.g., Easterwood and Nutt, 1999). Then, we regress forecast bias on *FavOrigin* and the same set of control variables used in Panel A of Table 8. Results are reported in Panel B of Table 8. Similar to the forecast accuracy results in Panel A, we find that *FavOrigin* is not significantly related to forecast bias across all columns.

Lastly, we test another aspect of forecast quality, the timeliness of a forecast revision (e.g., Cooper, Day, and Lewis, 2001). 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.[22] In Panel C, we report results for the timeliness tests. We regress *Days since last forecast* on the same set of independent variables that are used in Panel A of Table 8. We do not find the evidence that the favorability of an analyst's countries of origin is significantly associated to the forecast timeliness.

Overall, the evidence in Table 8 shows that analysts with more favorable countries of origin do not show better forecasting performance. It suggests that investors are influenced by their overall opinions about analysts' countries of origin when processing the information in analyst forecasts, even though the favorability of the analysts' countries of origin is not significantly related to their forecasting performance.

5.2. Career Outcomes

We now investigate whether career advancement of an analyst is affected by the favorability of the analyst's countries of origin. Following prior studies (Mikhail, Walther, and

[22] Cooper, Day, and Lewis (2001) find that earnings forecasts issued by lead analysts have a greater impact on stock prices than 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 analysts.

Willis, 1999; Jiang, Kumar, and Law, 2016), we consider five dimensions of analyst career outcome: *All-star* equals one if the analyst is ranked as an all-star analyst by the *Institutional Investor* magazine in the following year and zero otherwise; *Turnover* equals one if an analyst moves to another brokerage house or leaves the profession in the following year and zero otherwise; *Promotion* (*Demotion*) equals one if an analyst moves to a larger (smaller) brokerage house in the following year, conditional on the analyst remaining in IBES, and zero otherwise; and *Termination* equals one if an analyst disappears from IBES in the following year and zero otherwise.

We estimate pooled logit regressions and linear probability models for each of the five dependent variables of analyst career outcome. An analyst may have different values of favorability of origin (*FavOrigin*) in a given year.[23] Thus, we use the mean value of *FavOrigin*, measured at the analyst-year level, as the key independent variable. We control for forecast accuracy, brokerage size, days since last forecast, forecast horizon, forecast frequency, general experience, number of firms, and number of industries that are known to affect analysts' career outcomes (e.g., Jiang, Kumar, and Law, 2016). Since analysts' career outcomes are defined every year, we use the mean value of forecast accuracy, days since last forecast, and forecast horizon using an analyst's last forecasts across firms in a year.

Table 9 reports results for the career outcome regression estimates. We estimate pooled logit regressions for results without brokerage fixed effects and use linear probability models for results with brokerage fixed effects. Overall, we do not find evidence that the favorability of analysts' origin is associated with their career outcomes. The results in Table 9 suggest that analysts' employers at brokerage houses are not affected by the favorability of analysts' origin

[23] Gallup surveyed Americans' favorability of foreign countries more than once in 1999, 2000, and 2003. Starting from 2004, the survey was conducted in February of each year.

when evaluating analysts' performance. This is in line with the result in Panel A of Table 5 that institutional investors are less strongly influenced by the favorability of analyst origins.

6. Additional Tests

In this section, we attempt to identify the channels through which favorability affects market reaction. We focus on the different components of favorability associated with foreignness, cultural proximity, and country corruption. We also examine the impact of favorability on post-revision price drift.

6.1. Decomposition of Country Favorability

There are several factors that can influence country favorability. For example, Americans' in-group bias against foreigners may be an underlying factor for the favorability of a country (e.g., Kumar, Niessen-Ruenzi, and Spalt, 2015). Also, cultural similarities between the U.S. and the country and the perceived corruption level of the country are likely to affect the favorability of the country (Hwang, 2011).

Thus, following prior studies (Hwang, 2011; Kumar, Niessen-Ruenzi, and Spalt, 2015), we construct five variables to capture underlying factors of favorability of an analyst's country of origin: *Foreignness* captures whether the name of an analyst sounds foreign from the perspective of U.S. citizens. *Same ancestry* captures the similarity in ancestry between the analyst and the U.S. citizens. *Same language* captures whether the official languages of an analyst's countries of origin are English or not. *Cultural distance* captures the cultural differences in six dimensions of the Hofstede index between the U.S. and the analyst's countries of origin. Lastly, *Country corruption* captures the level of perceived corruption in the analyst's countries of origin.[24]

[24] See Appendix B for the definitions of these variables.

We first examine the relation between favorability and the aforementioned five variables. Panel A of Table 10 reports results from pooled OLS regressions in which the dependent variable is Americans' favorability of an analyst's countries of origin (*FavOrigin*) and the independent variables are each or all of the five underlying factors. Consistent with our conjecture, we find that *FavOrigin* is positively associated with the similarities in an analyst's ancestry and language to that of Americans, while it is negatively associated with the foreignness of an analyst's name, cultural distance, and the level of perceived corruption in an analyst's countries of origin.

Next, we examine which underlying factors of country favorability play more important roles in our findings. Using the coefficient estimates and the residual obtained from the pooled OLS regression of *FavOrigin* on *Foreignness*, *Same ancestry*, *Same language*, *Cultural distance*, and *Country corruption*, we measure individual components of favorability that are explained by each of these five factors and the residual that is not explained by them. For example, we measure the favorability component associated with the foreignness of an analyst's name, *FavOrigin* (*Foreignness*), by multiplying the coefficient estimate of *Foreignness* and the value of *Foreignness*. The residual component of favorability that is not explained by any of the five underlying factors, *FavOrigin* (*Residual*), is the residual from the regression.

Panel B of Table 10 reports results for the baseline market reaction regressions using the six components of favorability. In column (1), we find that the component of favorability associated with the foreignness of an analyst's name has a significant 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 favorability. We do not find strong evidence that favorability components associated with other factors drive our results. Interestingly, we find significant results using the residual component of favorability, *FavOrigin*

(*Residual*), which is orthogonal to all of the aforementioned five underlying factors.[25] The evidence implies that the component of favorability not explained by foreignness, cultural proximity, and country corruption is indeed an important driver of investor reactions to forecast revisions. It clearly distinguishes our work from prior studies examining the effect of in-group bias and cultural proximity in the capital market.

6.2. Post-Revision Price Drift

We also examine whether favorability also affects the post-revision price drifts (e.g., Gleason and Lee, 2003). Following Gleason and Lee (2003), we measure abnormal drift return to a forecast revision as the size-adjusted buy-and-hold return (*BHAR*) over the window from trading day +2 to trading day n (n= 21, 127, and 253), where trading day 0 is the forecast revision date.

In untabulated tests, we estimate post-revision price drift regressions in which the dependent variable is abnormal drift return and the independent variables are identical to those of the baseline regression models used in Panel A of Table 3. We find that there is no significant drift in our sample after we include controls, making it hard for us to draw clear implications about the effects of favorability on the post-revision price drifts.[26] The coefficients on the interaction term between *Revision* and *FavOrigin* are statistically insignificant as well.

7. Summary and Conclusion

We examine whether investors' perception of an equity analyst's country of origin affects their responses to forecast revisions by the analyst. We identify an analyst's country of

[25] In untabulated tests, we include these five factors as control variables. We still find significant effects of favorability of an analyst's countries of origin on market reactions to forecasts.

[26] It could be attributable to our sample period spanning more recent years than Gleason and Lee (2003) that use the data in 1990s. Another possible explanation is that we include more control variables and fixed effects.

origin using the U.S. historical immigration records at the port of New York and measure investors' perception of the analyst's origins using the Gallup survey data on Americans' favorability toward foreign countries. We find that analysts with more favorable countries of origin generate stronger market responses to their forecast revisions.

The effect of favorability on market reaction is more pronounced when the firm is largely held by individual investors and when the origins of analyst last names are easier to infer. Also, the favorability plays a greater role for forecast revisions that are issued by analysts with lower reputation or that contain bad news. To further identify the favorability effects, we exploit variations within a given analyst-year, firm-year, or analyst-firm and use a natural experiment of 9/11 terrorist attacks. The strong results alleviate concerns about correlated omitted variable bias.

We find no evidence that favorable origins are significantly associated with forecast quality, suggesting that our results are driven by investors' subjective perception of an analyst's origin rather than information quality. We also find that favorability does not affect analysts' career outcomes. The evidence, coupled with the weak favorability effect for firms with high institutional ownership, implies that analysts' origin does not exert strong influence on the judgments of sophisticated professionals in the capital market. Further, we identify the channels through which favorability affects market reaction to forecast revisions. We find that favorability associated with the foreignness of an analyst's name and the residual component of favorability exert significant influence on market reactions to forecast revisions.

Collectively, these results contribute to the understanding of market reaction to analyst forecasts and the effect of cultural origins in capital markets. We demonstrate that investors' subjective perceptions of analysts' cultural origins affect how investors process the information in analyst forecasts.

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Appendix A

Distribution of countries of origin for the U.S. equity analysts

This appendix shows summary statistics and a distribution of 116 countries of origin for the U.S. equity analysts over the sample period between 1996 and 2014. We obtain the U.S. historical immigration records for passengers arriving in the port of New York between 1820 and 1957 from *Ancestry.com*. For 5,197 unique last names that are associated with 7,765 individual analysts in the sample, we collect 14,883,568 immigration records without a missing nationality. We reclassify 1,708 original nationalities in the immigration records into 116 countries of origin, following Pan, Siegel, and Wang (2015). We exclude USA, Uninformative, and other country classifications that indicate geographic regions (e.g., Africa, Arab World, Asia, and Central America) or ethnic groups (e.g., Hispanic and Jewish) in the empirical analyses unless otherwise stated.

Country of origin	Percentage of analysts whose last name 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 last names identical to the analysts' in column (1) (2)
Afghanistan	0.40%	0.44%
Africa	34.48%	0.37%
Albania	5.64%	0.58%
Algeria	0.82%	0.09%
Arab World	9.80%	0.86%
Argentina	24.59%	0.16%
Armenia	18.79%	0.40%
Asia	12.23%	0.76%
Australia	40.91%	0.18%
Austria	59.52%	1.40%
Austrian	2.29%	0.02%
Barbados	4.75%	0.01%
Belgium	42.28%	0.77%
Bermuda	15.94%	0.05%
Bolivia	1.93%	0.04%
Bosnia	8.38%	0.09%
Brazil	30.51%	0.29%
Bulgaria	12.74%	0.29%
Burma	0.58%	0.11%
Canada	64.56%	1.50%
Central America	0.77%	0.01%
Chile	21.71%	0.15%
China	29.25%	12.37%
Colombia	16.64%	0.15%
Costa Rica	7.93%	0.05%
Croatia	25.18%	0.66%
Cuba	39.40%	0.56%
Cyprus	0.22%	0.03%
Czechoslovakia	48.60%	1.37%
Denmark	43.22%	0.63%
Dominican Republic	9.29%	0.13%
Ecuador	3.70%	0.09%
Egypt	7.23%	0.98%
El Salvador	0.97%	0.04%
Estonia	15.66%	0.21%
Ethiopia	1.46%	0.01%
Finland	36.92%	0.64%
France	70.83%	3.15%
Germany	84.21%	13.24%
Great Britain	85.98%	28.21%
Greece	40.43%	1.28%
Grenada	0.03%	0.00%
Guatemala	3.18%	0.04%
Haiti	5.64%	0.08%
Hispanic	0.50%	0.12%
Honduras	14.33%	0.12%
Hungary	52.72%	1.92%
Iceland	6.88%	0.02%
India	24.59%	3.18%
Indonesia	1.11%	1.34%

Iran	2.47%	0.53%
Iraq	3.34%	2.16%
Ireland	71.01%	10.56%
Israel	17.39%	0.78%
Italy	68.31%	9.28%
Jamaica	18.64%	0.03%
Japan	17.95%	1.43%
Jewish	59.90%	7.02%
Jordan	1.10%	1.11%
Korea	3.10%	0.67%
Latin America	31.69%	0.45%
Latvia	17.72%	0.24%
Lebanon	2.68%	0.49%
Liberia	6.16%	0.01%
Lithuania	27.47%	0.46%
Luxembourg	0.04%	0.03%
Macedonia	2.83%	0.15%
Malaysia	7.95%	0.27%
Mexico	34.40%	0.37%
Mongolia	2.18%	0.14%
Montenegro	6.34%	0.11%
Morocco	1.26%	0.60%
Muslim	0.43%	0.57%
Native American	62.34%	4.63%
Netherlands	60.45%	2.20%
New Zealand	15.97%	0.02%
Nicaragua	4.50%	0.02%
Norway	49.50%	1.02%
Pacific Islander	22.01%	0.20%
Pakistan	1.31%	2.71%
Palestine	5.01%	0.12%
Panama	20.71%	0.06%
Paraguay	0.99%	0.06%
Peru	10.30%	0.08%
Philippines	30.28%	0.19%
Poland	57.55%	2.98%
Polynesia	3.19%	0.02%
Portugal	33.01%	0.78%
Puerto Rico	32.71%	0.49%
Romania	40.34%	1.04%
Russia	61.52%	3.33%
Samoa	0.61%	0.03%
Scandinavia	61.74%	2.57%
Senegal	0.39%	0.22%
Serbia	15.58%	0.24%
Singapore	1.21%	0.01%
Slovenia	22.81%	0.52%
Somalia	0.04%	0.73%
South Africa	24.95%	0.06%
South America	4.25%	0.02%
Spain	62.41%	2.16%
Sudan	1.97%	0.08%
Sweden	55.83%	1.45%
Switzerland	47.03%	0.94%
Syria	23.23%	1.42%
Thailand	0.45%	0.12%
Tunisia	0.46%	0.16%
Turkey	24.40%	0.86%
USA	90.55%	18.46%
Ukraine	6.43%	0.03%
Unidentifiable	76.83%	2.10%
Uruguay	4.64%	0.03%
Venezuela	22.19%	0.26%
Vietnam	1.80%	0.24%
West Indies	12.47%	0.02%
Yugoslavia	9.27%	0.12%

Appendix B
Variable definitions

We construct variables using the following data sources: Amazon Mechanical Turk (AMT) platform, Corruption P erception Index (CPI) published by Transparency International, Compustat, Center for Research on Security Prices (CRSP), Gallup Analytics (Gallup), Hofstede index (Hofstede), *Institutional Investor* magazine (II), Thomson Reuters' Institutional Brokers Estimate System (IBES), Thomson Reuters' Institutional 13f Holdings file (13F), the U.S. Census Bureau (US Census), the U.S. immigration records on *Ancestry.com* (Immigration), and the World Factbook by the Central Intelligence Agency (CIA). The table below shows variable definitions.

Variable Name	Description	Source
Variables of Interest		
FavOrigin	Americans' favorability of an analyst's countries of origin, measured as the weighted average of favorability ratings across the analyst's countries of origin. Favorability rating is the percentage of survey respondents who answered Very Favorable or Mostly Favorable to the following question in 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 the nationality that U.S. immigrants with the last name have.	Gallup, IBES, Immigration
Middle Eastern	Dummy variable for Middle Eastern analyst. It is equal to one if more than 30 percent of the U.S. immigrants with the same last name as the analyst are either 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 and Muslim, and zero otherwise.	IBES, Immigration
Post-9/11 attacks	Dummy variable equal to one if a forecast revision is made after September 11, 2001 and zero otherwise.	IBES
Revision	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	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	Dummy variable equal to one if an analyst is ranked as an all-star analyst in the next year's *Institutional Investor* magazine and zero otherwise.	IBES, II
CAR [-1,+n]	Size-adjusted cumulative abnormal return over the window starting a 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
Demotion	Dummy variable equal to one if an analyst moves to a smaller brokerage firm in the following year, conditional on the analyst remaining in IBES, and zero 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
Promotion	Dummy variable equal to one if an analyst moves to a bigger brokerage firm in the following year, conditional on the analyst remaining in IBES, and zero otherwise.	IBES
Termination	Dummy variable equal to one if an analyst disappears from IBES in the following year and zero otherwise.	IBES
Turnover	Dummy variable equal to one if an analyst moves to another brokerage firm or leave the profession and zero otherwise.	IBES

Appendix B (*Continued*)
Variable definitions

Variable Name	Description	Source
Control Variables		
Book-to-market	Ratio of book equity to market equity for a firm, measured at the most recent December preceding the forecast date.	Compustat, CRSP
Brokerage size	Natural logarithm of one plus the number of analysts in a brokerage house in a year.	IBES
Country corruption	Weighted average of negative one times the Corruption Perception Index (CPI) for countries associated with the analyst's last name. Weights are computed based on the frequency of the nationality of U.S. immigrants who have the same last name as the analyst's.	CPI, Immigration
Cultural distance	Weighted average of the culture difference for countries associated with the analyst's last name. The culture difference is measured as the mean value of the absolute differences in the Hofstede index between the U.S. and the country in question, across all six cultural dimensions (i.e., individualism, power distance, masculinity, uncertainty avoidance, long-term orientation and indulgence). Weights are computed based on the frequency of the nationality of U.S. immigrants who have the same last name as the analyst's.	Hofstede, Immigration
Days since last forecast	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
Forecast horizon	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	Natural logarithm of one plus the number of one-year-ahead earnings forecasts an analyst issues in a year.	IBES
Foreignness	Percentage of the electronic Amazon Mechanical Turk (AMT) workers who indicate that the name of the analyst is foreign-sounding, from Kumar, Niessen-Ruenzi, and Spalt (2015).	AMT, IBES
Firm size	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	Natural logarithm of one plus the number of years an analyst has issued one-year-ahead earnings forecasts for a firm.	IBES
General experience	Natural logarithm of one plus the number of years an analyst has appeared in IBES.	IBES
Institutional ownership	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	Past 12-month return for a firm, measured at the end of the prior month of the forecast date.	CRSP
Number of analysts	Number of analysts following a firm in a year.	IBES
Number of firms	Number of firms an analyst follows in a year.	IBES
Number of industries	Number of (two-digit SIC code) industries an analyst follows in a year.	IBES
Same ancestry	Weighted average of the percentage of U.S. citizens whose ancestors came from countries associated with the analyst's last name from Census. Weights are computed based on the frequency of the nationality of U.S. immigrants who have the same last name as an analyst's.	Immigration, US Census
Same language	Weighted average of English dummy variable for countries associated with the analyst's last name. The English dummy variable is equal to one if English is the official or the most popular language for a country and zero otherwise. Weights are computed based on the frequency of the nationality of U.S. immigrants who have the same last name as an analyst's.	CIA, 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 Gallup surveys: "*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?*"

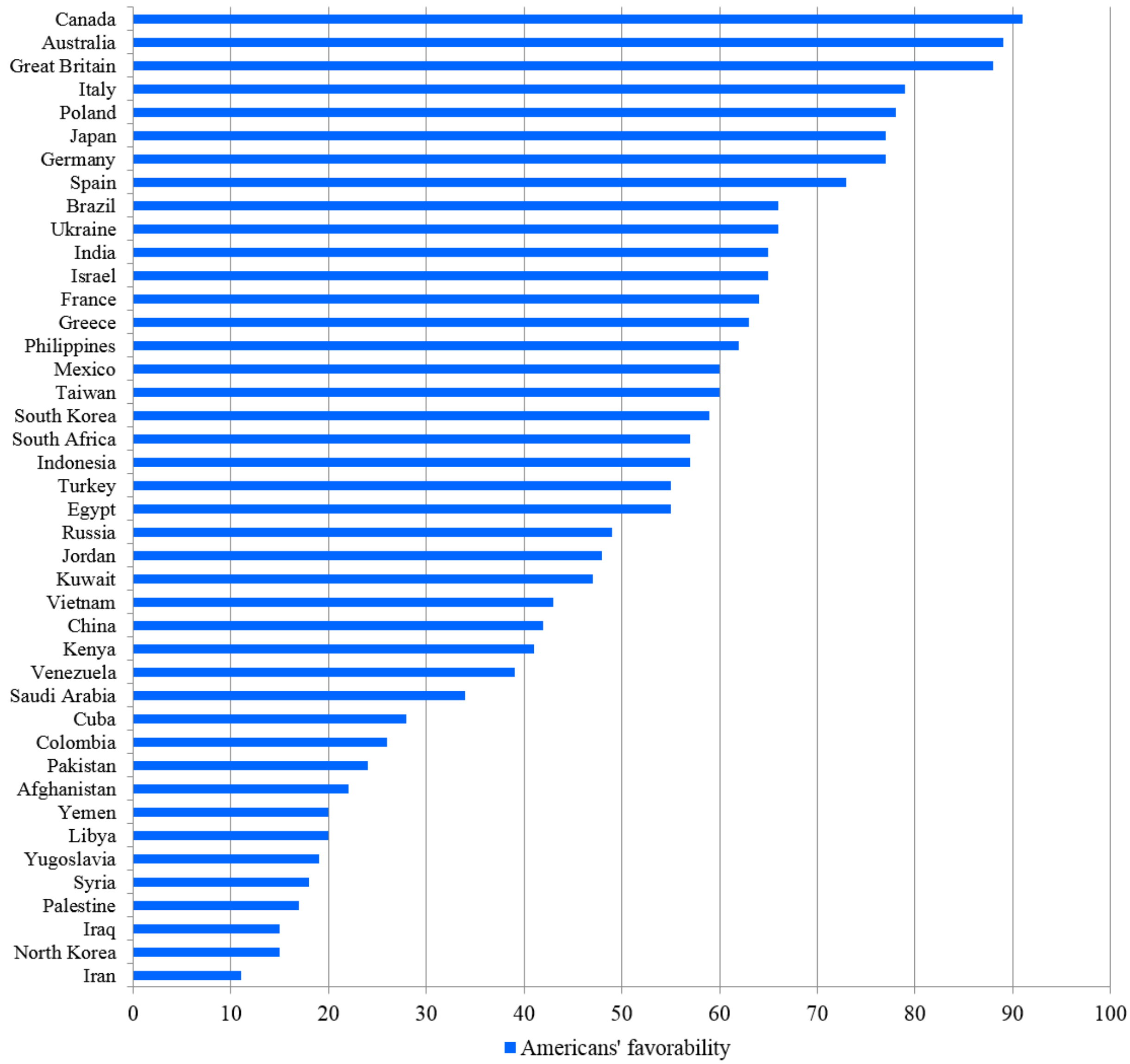

Figure 2

Change in Americans' favorability of Middle Eastern countries

This figure shows the change in Americans' favorability of Middle Eastern countries around the September 11th (9/11) terrorist attacks in 2001. As of each survey date, we compute the mean value of favorability for Middle Eastern countries using the most recent data available in Gallup Analytics. We use favorability ratings of Middle Eastern countries whose data are available prior to the September 11th terrorist attacks.

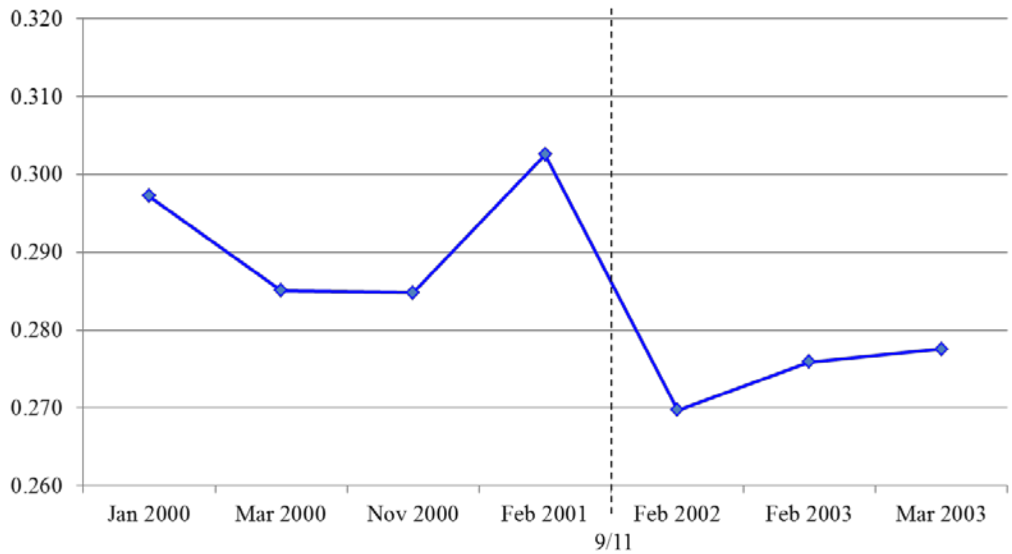

Table 1
Summary statistics for Americans' favorability of foreign countries

This table shows summary statistics for Americans' favorability of foreign countries in Gallup Analytics. Favorability is rated on the five Likert scale from Very Favorable, Mostly Favorable, Mostly Unfavorable, Very Unfavorable, and Others. Others 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 from 1996 to 2014.

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

Table 2
Summary statistics for variables

This table shows summary statistics of the variables we use in our analyses. In Panel A, we report summary statistics for main variables over the sample period from 1996 to 2014. In Panel B, we provide comparisons in analyst, forecast and firm characteristics between the two favorability groups. We divide the sample into two groups according to the sample median of favorability of an analyst's countries of origin (*FavOrigin*). *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 1 and 99 percentile levels. Variable definitions are provided in Appendix B.

Panel A: Summary statistics							
Variable	Mean	Std Dev	10th Pctl	25th Pctl	Median	75th Pctl	90th Pctl
Variables of Interest							
FavOrigin	0.787	0.112	0.631	0.749	0.809	0.871	0.890
Revision	-0.002	0.013	-0.012	-0.003	0.000	0.002	0.006
Dependent Variables							
Accuracy	-0.013	0.027	-0.030	-0.012	-0.004	-0.001	0.000
All-star	0.090	0.286	0	0	0	0	0
CAR [-1,+1]	-0.002	0.070	-0.078	-0.031	-0.001	0.030	0.074
CAR [-1,+3]	-0.002	0.078	-0.088	-0.037	0.000	0.036	0.084
CAR [-1,+5]	-0.002	0.084	-0.097	-0.041	0.000	0.040	0.093
CAR [-1,+10]	-0.001	0.097	-0.112	-0.049	0.000	0.050	0.110
Demotion	0.004	0.061	0	0	0	0	0
Forecast Bias	0.004	0.026	-0.011	-0.003	0.000	0.005	0.020
Promotion	0.090	0.286	0	0	0	0	0
Termination	0.151	0.358	0	0	0	0	1
Turnover	0.244	0.430	0	0	0	0	1
Control Variables							
Book-to-market	0.556	0.437	0.151	0.262	0.446	0.723	1.069
Brokerage size	3.820	0.984	2.485	3.135	3.951	4.644	4.898
Days since last forecast	1.773	1.153	0.693	0.693	1.386	2.639	3.584
Forecast horizon	5.020	0.687	4.331	4.663	5.236	5.565	5.684
Forecast frequency	4.321	0.583	3.611	3.989	4.331	4.682	5.037
Firm size	14.896	1.767	12.613	13.617	14.831	16.182	17.227
Firm-specific experience	1.656	0.508	1.099	1.099	1.609	2.079	2.398
General experience	2.164	0.553	1.386	1.792	2.197	2.565	2.890
Institutional ownership	0.707	0.201	0.418	0.590	0.740	0.857	0.946
Lagged accuracy	-0.006	0.014	-0.012	-0.004	-0.002	-0.001	0.000
Momentum	0.141	0.481	-0.398	-0.153	0.093	0.348	0.683
Number of analysts	19.332	10.889	6	10	18	26	35
Number of firms	17.427	7.224	10	13	16	21	26
Number of industries	3.683	2.388	1	2	3	5	7

Table 2 (*Continued*)
Summary statistics for variables

Panel B: Analyst, forecast and firm characteristics conditional on favorability of countries of origin						
	High FavOrigin N=450,893		Low FavOrigin N=450,858		Test of Differences	
Variable	Mean	Median	Mean	Median	t-statistic	z-statistic
FavOrigin	0.865	0.871	0.708	0.749	(53.96)***	(822.38)***
Accuracy	-0.013	-0.004	-0.013	-0.004	(0.55)	(7.31)***
Forecast bias	0.004	0.000	0.004	0.000	(0.18)	(5.15)***
Book-to-market	0.570	0.457	0.542	0.433	(3.79)***	(35.60)***
Brokerage size	3.790	3.871	3.850	4.007	(-1.80)*	(-29.83)***
Days since last forecast	1.753	1.386	1.793	1.386	(-2.76)***	(-19.51)***
Forecast horizon	5.028	5.242	5.013	5.231	(4.78)***	(11.47)***
Forecast frequency	4.330	4.344	4.313	4.317	(0.78)	(13.68)***
Firm size	14.940	14.863	14.853	14.803	(2.34)**	(21.55)***
Firm-specific experience	1.661	1.609	1.652	1.609	(0.87)	(11.20)***
General experience	2.182	2.197	2.146	2.197	(2.24)**	(30.99)***
Institutional ownership	0.721	0.754	0.693	0.725	(8.77)***	(64.21)***
Lagged accuracy	-0.006	-0.002	-0.006	-0.002	(1.16)	(2.31)**
Momentum	0.131	0.087	0.151	0.099	(-5.77)***	(-14.81)***
Number of analysts	19.526	18	19.142	18	(1.55)	(17.21)***
Number of firms	17.291	16	17.562	16	(-1.10)	(-1.96)**
Number of industries	3.651	3	3.715	3	(-0.81)	(-10.84)***

Table 3
Market reaction regression estimates

This table shows the estimates of market reaction pooled OLS 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. *FavOrigin* is Americans' favorability of an analyst's countries of origin associated with the analyst's last name. *Revision* is the difference between the 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 sample construction or measurement of *FavOrigin*. 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 two alternative measures of favorability. The first alternative measure only uses the most extreme rating item, "Very Favorable". The second alternative measure is a composite score of favorability, following Hwang (2011). The composite score is computed as 4×(%Very Favorable)+3×(%Mostly Favorable)+2×(%Mostly Unfavorable)+1×(%Very Unfavorable). In Panel E, we measure *FavOrigin* by only considering either one or three most dominant countries of origin associated with a last name. In parentheses below coefficient estimates are *t*-statistics based on standard errors clustered by analyst. All continuous variables are winsorized at the 1 and 99 percentile levels. Variable definitions are provided in Appendix B.

Table 3 (*Continued*)
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	0.938***	0.715***	0.968***	0.699***	0.991***	0.755***	0.952***	0.461*
	(8.782)	(3.026)	(8.438)	(2.804)	(8.792)	(2.912)	(8.194)	(1.669)
Revision×FavOrigin	0.311**	0.360***	0.345**	0.394***	0.340**	0.398***	0.355**	0.421***
	(2.291)	(2.655)	(2.371)	(2.729)	(2.375)	(2.764)	(2.402)	(2.820)
FavOrigin	0.001	0.000	0.001	0.001	0.000	0.000	-0.000	-0.000
	(0.790)	(0.528)	(0.832)	(0.558)	(0.283)	(0.083)	(-0.159)	(-0.221)
Revision×Book-to-market		-0.294***		-0.293***		-0.278***		-0.265***
		(-15.123)		(-13.997)		(-12.650)		(-10.692)
Revision×Brokerage size		0.130***		0.137***		0.146***		0.153***
		(8.303)		(8.292)		(8.762)		(8.624)
Revision×Days since last forecast		0.035***		0.052***		0.066***		0.080***
		(3.985)		(5.366)		(6.383)		(7.050)
Revision×Forecast horizon		0.108***		0.111***		0.105***		0.129***
		(7.218)		(6.550)		(5.604)		(6.029)
Revision×Forecast frequency		-0.227***		-0.230***		-0.240***		-0.248***
		(-5.628)		(-5.403)		(-5.636)		(-5.494)
Revision×Firm size		-0.010		-0.008		-0.004		0.011
		(-0.823)		(-0.596)		(-0.268)		(0.753)
Revision×Firm-specific experience		-0.051		-0.045		-0.032		-0.009
		(-1.638)		(-1.338)		(-0.911)		(-0.237)
Revision×General experience		0.099***		0.112***		0.090***		0.100***
		(3.252)		(3.436)		(2.661)		(2.753)
Revision×Institutional ownership		1.032***		1.060***		1.007***		0.934***
		(18.403)		(17.413)		(15.824)		(13.102)
Revision×Lagged accuracy		8.547***		9.455***		9.439***		10.227***
		(21.214)		(20.996)		(19.023)		(19.688)
Revision×Momentum		0.491***		0.554***		0.625***		0.719***
		(19.530)		(19.937)		(20.992)		(21.770)
Revision×Number of analysts		-0.008***		-0.011***		-0.012***		-0.017***
		(-4.148)		(-5.088)		(-5.302)		(-6.949)
Revision×Number of firms		-0.009***		-0.010***		-0.010***		-0.010***
		(-2.625)		(-2.859)		(-2.790)		(-2.770)
Revision×Number of industries		0.070***		0.076***		0.080***		0.088***

		(9.440)		(9.756)		(10.121)		(10.444)
Book-to-market		0.000		0.000		-0.000		0.000
		(0.069)		(1.021)		(-0.121)		(0.500)
Brokerage size		-0.000		0.000		-0.000		0.000
		(-0.329)		(0.051)		(-0.167)		(0.870)
Days since last forecast		0.000		-0.000		-0.000*		-0.000***
		(0.711)		(-1.304)		(-1.884)		(-3.359)
Forecast horizon		0.002***		0.002***		0.002***		0.002***
		(14.249)		(12.697)		(11.454)		(8.102)
Forecast frequency		-0.001***		-0.001**		-0.001**		-0.001**
		(-2.744)		(-2.473)		(-1.998)		(-2.537)
Firm size		-0.012***		-0.015***		-0.017***		-0.023***
		(-38.733)		(-41.712)		(-44.458)		(-49.592)
Firm-specific experience		-0.000		-0.000		-0.000		-0.000
		(-0.740)		(-0.768)		(-0.651)		(-1.095)
General experience		-0.000		-0.000		0.000		0.000
		(-0.117)		(-0.028)		(0.323)		(0.763)
Institutional ownership		-0.009***		-0.008***		-0.010***		-0.011***
		(-9.494)		(-7.820)		(-8.277)		(-7.845)
Lagged accuracy		0.017**		0.022**		0.015		0.011
		(2.015)		(2.185)		(1.307)		(0.857)
Momentum		-0.000		-0.002***		-0.003***		-0.004***
		(-1.189)		(-5.541)		(-8.111)		(-9.742)
Number of analysts		-0.000*		-0.000*		-0.000		-0.000
		(-1.847)		(-1.896)		(-1.113)		(-1.235)
Number of firms		0.000		0.000		0.000		-0.000
		(1.422)		(1.246)		(0.090)		(-0.103)
Number of industries		0.000		0.000		0.000		0.000*
		(1.353)		(1.451)		(1.576)		(1.752)
Intercept	0.003***	0.174***	0.004***	0.211***	0.005***	0.248***	0.007***	0.333***
	(4.382)	(37.563)	(5.056)	(40.141)	(5.204)	(42.916)	(6.132)	(47.973)
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	901,751	901,751	901,751	901,751	901,751	901,751	901,751	901,751
Adjusted R^2	7.72%	9.66%	7.10%	9.12%	6.56%	8.65%	5.61%	7.93%

Table 3 (*Continued*)

Market reaction regression estimates

Panel B: Using the analyst's last forecast for the firm-fiscal 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×FavOrigin	0.376**	0.408**	0.360*	0.375*	0.409**	0.434**	0.532**	0.563**
	(2.094)	(2.369)	(1.787)	(1.958)	(1.970)	(2.191)	(2.232)	(2.496)
Controls and fixed effects	Identical to Panel A of Table 3							
Number of observations	250,405	250,405	250,405	250,405	250,405	250,405	250,405	250,405
Adjusted R^2	9.67%	11.46%	9.41%	11.17%	9.12%	10.91%	8.52%	10.44%

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×FavOrigin	0.279**	0.270**	0.409**	0.401**	0.354**	0.365**	0.198	0.236
	(2.042)	(1.991)	(2.529)	(2.542)	(2.107)	(2.197)	(0.924)	(1.123)
Controls and fixed effects	Identical to Panel A of Table 3							
Number of observations	231,991	231,991	231,991	231,991	231,991	231,991	231,991	231,991
Adjusted R^2	3.30%	4.02%	3.14%	4.00%	3.13%	4.13%	3.07%	4.38%

Panel D: Using a different definition of FavOrigin

Coefficient estimates on Revision×FavOrigin	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Using "Very Favorable (%)" only	0.441***	0.321**	0.504***	0.374**	0.502***	0.375**	0.521***	0.392**
	(3.068)	(2.237)	(3.309)	(2.470)	(3.268)	(2.443)	(3.234)	(2.453)
Using a composite score	0.196***	0.172***	0.222***	0.195***	0.223***	0.200***	0.236***	0.216***
	(3.395)	(2.962)	(3.608)	(3.188)	(3.629)	(3.238)	(3.672)	(3.355)
Controls and fixed effects	Identical to Panel A of Table 3							

Panel E: Using dominant origins for a last name

Coefficient estimates on Revision×FavOrigin	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Using the most dominant origin	0.273**	0.300***	0.301**	0.325***	0.286**	0.316***	0.286**	0.314**
	(2.366)	(2.657)	(2.439)	(2.686)	(2.350)	(2.595)	(2.276)	(2.483)
Using three most dominant origins	0.313**	0.368***	0.347**	0.403***	0.335**	0.398***	0.344**	0.415***
	(2.370)	(2.793)	(2.456)	(2.866)	(2.399)	(2.836)	(2.386)	(2.851)
Controls and fixed effects	Identical to Panel A of Table 3							

Table 4

Falsification test: Market reaction regression estimates

This table shows the results from falsification (placebo) 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. *FavOrigin (P)* is a placebo measure of *FavOrigin*, computed using favorability ratings of placebo countries origins. A placebo country of origin is the country that appears right after the actual country of origin in the alphabetically ordered list of 116 countries in Appendix A. The set of controls and fixed effects are identical to those in Panel A of Table 3. In parentheses below coefficient estimates are *t*-statistics based on standard errors clustered by analyst. All continuous variables are winsorized at the 1 and 99 percentile levels. Variable definitions are provided in Appendix B.

	Dependent Variable: Size-adjusted CAR							
	[-1,+1]		[-1,+3]		[-1,+5]		[-1,+10]	
Independent Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Revision	1.175***	0.968***	1.171***	0.947***	1.173***	0.965***	1.105***	0.637**
	(12.591)	(4.191)	(11.608)	(3.880)	(11.636)	(3.762)	(10.292)	(2.346)
Revision×FavOrigin (*P*)	0.014	0.087	0.101	0.177	0.126	0.205	0.188	0.258*
	(0.107)	(0.659)	(0.698)	(1.281)	(0.869)	(1.479)	(1.217)	(1.764)
FavOrigin (*P*)	0.000	0.000	0.000	0.001	0.000	0.001	0.001	0.002
	(0.178)	(0.617)	(0.266)	(0.750)	(0.470)	(0.957)	(0.729)	(1.267)
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	876,973	876,973	876,973	876,973	876,973	876,973	876,973	876,973
Adjusted R^2	7.73%	9.68%	7.12%	9.15%	6.59%	8.68%	5.66%	7.98%

Table 5
Subsample analyses for market reaction regression estimates

This table shows the estimates of market reaction pooled OLS regressions using subsamples based on investor sophistication (Panel A), the level of difficulty in inferring countries of origin (Panel B), analyst reputation (Panel C), and the sign of revision news (Panel D). 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. *FavOrigin* is Americans' favorability of an analyst's countries of origin, associated with the analyst's last name. *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. Model specifications are identical to those in Panel A of Table 3. In Panel A, we divide the sample into two subsamples according to the sample median of institutional ownership. In Panel B, we divide the sample into two subsamples according to the sample median of the fraction of U.S. immigrants whose nationality matches the most common country for a last name, conditional on the U.S. immigrants having the same last name. We assume that it is easier to infer the origin of an analyst when a higher fraction of the U.S. immigrants with the analyst's last name come from a single country. In Panel C, we divide the sample into two subsamples according to whether an analyst has ever been ranked as an all-star analyst in the *Institutional Investor* magazine. In Panel D, we divide the sample into two subsamples according to the sign of a forecast revision. In parentheses below coefficient estimates are *t*-statistics based on standard errors clustered by analyst. All continuous variables are winsorized at the 1 and 99 percentile levels. Variable definitions are provided in Appendix B.

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×FavOrigin	0.177	0.245	0.186	0.257	0.242	0.327	0.246	0.330
	(0.788)	(1.155)	(0.794)	(1.148)	(1.045)	(1.473)	(1.039)	(1.448)
Controls and fixed effects	Identical to Panel A of Table 3							
Number of observations	450,884	450,884	450,884	450,884	450,884	450,884	450,884	450,884
Adjusted R^2	9.06%	11.34%	8.29%	10.65%	7.56%	10.10%	6.45%	9.17%
<u>*Low Institutional Ownership*</u>								
Revision×FavOrigin	0.313**	0.403***	0.360***	0.448***	0.326**	0.417***	0.363**	0.454***
	(2.428)	(3.115)	(2.589)	(3.191)	(2.312)	(2.871)	(2.335)	(2.779)
Controls and fixed effects	Identical to Panel A of Table 3							
Number of observations	450,867	450,867	450,867	450,867	450,867	450,867	450,867	450,867
Adjusted R^2	8.94%	10.55%	8.40%	10.09%	8.04%	9.78%	7.26%	9.32%

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>*Easy Names to Infer Origins*</u>								
Revision×FavOrigin	0.353**	0.407**	0.349*	0.401**	0.305*	0.373**	0.371*	0.435**
	(2.003)	(2.294)	(1.840)	(2.105)	(1.647)	(1.994)	(1.955)	(2.266)
Controls and fixed effects	Identical to Panel A of Table 3							
Number of observations	451,379	451,379	451,379	451,379	451,379	451,379	451,379	451,379
Adjusted R^2	7.98%	9.98%	7.30%	9.35%	6.82%	8.96%	5.77%	8.14%
<u>*Difficult Names to Infer Origins*</u>								
Revision×FavOrigin	0.260	0.295	0.347	0.382*	0.393*	0.429*	0.340	0.389
	(1.204)	(1.412)	(1.505)	(1.732)	(1.696)	(1.890)	(1.414)	(1.631)
Controls and fixed effects	Identical to Panel A of Table 3							
Number of observations	450,372	450,372	450,372	450,372	450,372	450,372	450,372	450,372
Adjusted R^2	7.65%	9.58%	7.11%	9.15%	6.53%	8.63%	5.66%	8.01%

Table 5 (*Continued*)
Subsample analyses for market reaction regression estimates

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)
All-star analysts								
Revision×FavOrigin	0.470	0.368	0.448	0.334	0.483	0.407	0.740*	0.671*
	(1.439)	(1.260)	(1.346)	(1.103)	(1.380)	(1.288)	(1.936)	(1.958)
Controls and fixed effects	Identical to Panel A of Table 3							
Number of observations	207,688	207,688	207,688	207,688	207,688	207,688	207,688	207,688
Adjusted R^2	7.42%	9.35%	6.87%	8.94%	6.29%	8.42%	5.34%	7.76%
Non-all-star analysts								
Revision×FavOrigin	0.283*	0.356**	0.332**	0.403***	0.323**	0.398***	0.297*	0.375**
	(1.920)	(2.478)	(2.075)	(2.581)	(2.075)	(2.592)	(1.886)	(2.436)
Controls and fixed effects	Identical to Panel A of Table 3							
Number of observations	694,063	694,063	694,063	694,063	694,063	694,063	694,063	694,063
Adjusted R^2	7.87%	9.91%	7.25%	9.35%	6.72%	8.88%	5.77%	8.16%

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×FavOrigin	0.392*	0.339*	0.420*	0.368*	0.409*	0.353	0.311	0.271
	(1.880)	(1.701)	(1.838)	(1.722)	(1.646)	(1.531)	(1.075)	(1.007)
Controls and fixed effects	Identical to Panel A of Table 3							
Number of observations	444,378	444,378	444,378	444,378	444,378	444,378	444,378	444,378
Adjusted R^2	8.59%	9.67%	8.13%	9.38%	7.93%	9.30%	7.50%	9.10%
Negative forecast revisions								
Revision×FavOrigin	0.239**	0.253**	0.301**	0.310**	0.297**	0.309**	0.362***	0.366**
	(2.230)	(2.276)	(2.440)	(2.431)	(2.387)	(2.373)	(2.626)	(2.535)
Controls and fixed effects	Identical to Panel A of Table 3							
Number of observations	454,105	454,105	454,105	454,105	454,105	454,105	454,105	454,105
Adjusted R^2	8.47%	10.03%	7.47%	9.12%	6.84%	8.57%	5.94%	7.93%

Table 6

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

This table shows the estimates of market reaction pooled OLS 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. *FavOrigin* is Americans' favorability of an analyst's countries of origin associated with the analyst's last name. *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 controls is identical to that used in Panel A of Table 3. In parentheses below coefficient estimates are *t*-statistics based on standard errors clustered by analyst. All continuous variables are winsorized at the 1 and 99 percentile levels. Variable definitions are provided in Appendix B.

	Dependent Variable: Size-adjusted CAR [-1,+1]					
Independent Variables	(1)	(2)	(3)	(4)	(5)	(6)
Revision	0.919***	0.919***	0.617***	0.637***	1.106***	1.114***
	(3.564)	(3.563)	(2.689)	(2.770)	(4.176)	(4.194)
Revision×FavOrigin	0.375**	0.374**	0.245**	0.249**	0.401***	0.397**
	(2.549)	(2.547)	(2.121)	(2.157)	(2.588)	(2.548)
FavOrigin	-0.024***	-0.024***	-0.000	-0.000	0.000	-0.011***
	(-4.019)	(-4.022)	(-0.095)	(-0.585)	(0.042)	(-3.149)
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	901,751	901,751	901,751	901,751	901,751	901,751
Adjusted R^2	8.11%	8.14%	22.76%	22.77%	10.29%	10.44%

Table 7
Natural experiment: The September 11 terrorist attacks

This table shows summary statistics and results from OLS regressions using a matched sample around the September 11 terrorist attacks in 2001. An analyst is considered a Middle Eastern (treatment) analyst when more than 30 percent of the U.S. immigrants with the same last name as the analyst are either from Middle Eastern countries such as Afghanistan, Egypt, Iran, Iraq, Jordan, Kuwait, Libya, Pakistan, Saudi Arabia, Syria, Turkey, and Yemen or identified as Arab or Muslim according to the U.S. historical immigration records. In Panel A, we match Middle Eastern analysts and control analysts on the following matching covariates using the Coarsened Exact Matching (CEM) algorithm: *FavOrigin* (Americans' favorability of an analyst's countries of origin), mean accuracy (mean accuracy of the last forecasts across firms an analyst follows), brokerage size, forecast frequency, general experience, and number of firms. Matching is carried out at the end of year 2000. In panel A, we report matching covariate balance across the two analyst groups in the matched sample. *t*-statistics for mean difference tests are based on standard errors clustered by analyst. In Panel B and Panel C, we restrict the sample period between 1996 and 2006 and exclude the transition period of year 2001 in order to make the pre- and post-9/11 attacks period in equal lengths of five years. In Panel B, we estimate OLS regressions in which the dependent variable is *FavOrigin. Middle Eastern* is an indicator variable that equals one if an analyst is defined as a Middle Eastern analyst and zero otherwise. *Post-9/11 attacks* is an indicator variable that equals one if an observation belongs to the period after September 11, 2001. At the end of every December, we measure *FavOrigin* for every analyst in the matched sample using the most recent survey data in Gallup Analytics. In Panel C, we retrieve all forecast revisions made by analysts in the matched sample and estimate the market reaction pooled 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 or +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. The set of controls is identical to that in Panel A of Table 3. In parentheses below coefficient estimates are *t*-statistics based on standard errors clustered by analyst. All continuous variables are winsorized at the 1 and 99 percentile levels. Variable definitions are provided in Appendix B.

Panel A: Comparison of matching covariates used in CEM approach

	Middle Eastern analysts N=14	Non-Middle Eastern analysts N=219	Test of Differences
Matching covariates	Mean	Mean	*t*-statistic
FavOrigin	0.512	0.543	(-0.73)
Mean accuracy	-0.010	-0.009	(-0.36)
Brokerage size	4.065	4.062	(0.01)
Forecast frequency	3.451	3.484	(-0.21)
General experience	1.759	1.695	(0.43)
Number of firms	9.428	11.143	(-1.43)

Panel B: Americans' favorability toward analysts' countries of origins around the 9/11 terrorist attacks

	Dependent Variable: FavOrigin	
Independent Variables	(1)	(2)
Middle Eastern	0.007	0.014
	(0.112)	(0.238)
Middle Eastern×Post-9/11 attacks	-0.225***	-0.232***
	(-4.242)	(-4.393)
Post-9/11 attacks	0.080***	0.095***
	(6.329)	(6.708)
Intercept	0.547***	0.553***
	(37.471)	(34.913)
Year fixed effects	No	Yes
Number of observations	2,289	2,289
Adjusted R^2	9.42%	13.22%

Table 7 (*Continued*)

Natural experiment: The September 11 terrorist attacks

Panel C: Difference in differences test using the natural experiment of the 9/11 terrorist attacks						
	Dependent Variable: Size-adjusted CAR					
		[-1,+1]			[-1,+10]	
Independent Variables	(1)	(2)	(3)	(4)	(5)	(6)
Revision	0.678***	-1.499	-4.360*	0.727***	-0.581	-3.468
	(3.046)	(-0.908)	(-1.843)	(3.203)	(-0.278)	(-1.019)
Revision×Middle Eastern	1.088**	1.285**		1.616**	1.766***	
	(2.470)	(2.133)		(2.263)	(2.637)	
Revision×Middle Eastern×Post-9/11 attacks	-1.581***	-1.774**	-2.244**	-1.818**	-1.861**	-3.413***
	(-2.877)	(-2.560)	(-2.134)	(-2.470)	(-2.546)	(-4.128)
Revision×Post-9/11 attacks	0.888***	1.214***	0.871**	0.915***	1.312***	1.413***
	(3.544)	(5.419)	(2.177)	(4.129)	(4.648)	(2.947)
Middle Eastern×Post-9/11 attacks	-0.005	-0.000	0.001	0.004	0.011	0.016
	(-1.185)	(-0.094)	(0.148)	(0.413)	(1.016)	(1.234)
Middle Eastern	0.004	0.002		-0.005	-0.007	
	(1.151)	(0.576)		(-0.600)	(-0.953)	
Post-9/11 attacks	-0.006	0.008	0.017*	-0.018***	0.020	0.052***
	(-1.287)	(0.886)	(1.960)	(-2.972)	(1.451)	(3.179)
Controls	No	Yes	Yes	No	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Analyst fixed effects	No	No	Yes	No	No	Yes
Revision×Analyst fixed effects	No	No	Yes	No	No	Yes
Number of observations	26,032	26,032	26,032	26,032	26,032	26,032
Adjusted R^2	10.87%	13.36%	16.08%	9.19%	11.74%	13.35%

Table 8

Forecast quality

This table reports the estimates of forecast quality pooled OLS regressions. In Panel A, the dependent variable is *Accuracy*, measured as 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. *FavOrigin* is Americans' favorability of an analyst's countries of origin, associated with the analyst's last name. 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 1 and 99 percentile levels. Variable definitions are provided in Appendix B.

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

	Dependent Variable: Accuracy		
Independent Variables	(1)	(2)	(3)
FavOrigin	0.000	-0.000	0.000
	(0.006)	(-0.212)	(0.825)
Book-to-market		-0.008***	-0.007***
		(-32.237)	(-18.918)
Brokerage size		-0.000***	-0.000**
		(-3.912)	(-2.231)
Days since last forecast		0.000*	-0.000***
		(1.807)	(-4.386)
Forecast horizon		-0.002***	-0.003***
		(-30.705)	(-37.492)
Forecast frequency		-0.001***	0.001***
		(-3.014)	(8.561)
Firm size		0.003***	0.009***
		(47.217)	(46.876)
Firm-specific experience		-0.001***	-0.000***
		(-4.261)	(-3.885)
General experience		0.001***	0.000***
		(3.832)	(3.196)
Institutional ownership		0.006***	0.004***
		(15.964)	(7.605)
Lagged accuracy		0.431***	0.145***
		(41.677)	(14.625)
Number of analysts		-0.000***	-0.000***
		(-26.047)	(-22.486)
Number of firms		0.000***	-0.000***
		(5.777)	(-4.484)
Number of industries		-0.000	0.000
		(-0.010)	(0.912)
Intercept	-0.004***	-0.032***	-0.114***
	(-9.116)	(-27.404)	(-43.460)
Firm fixed effects	Yes	No	Yes
Year fixed effects	Yes	No	Yes
Number of observations	250,405	250,405	250,405
Adjusted R^2	31.19%	16.80%	37.92%

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

	Dependent Variable: Forecast bias		
Independent Variables	(1)	(2)	(3)
FavOrigin	-0.000	0.001	-0.000
	(-0.003)	(1.098)	(-0.444)
Controls and fixed effects	Identical to Panel A of Table 8		
Number of observations	250,405	250,405	250,405
Adjusted R^2	20.58%	2.54%	21.31%

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

	Dependent Variable: Days since last forecast		
Independent Variables	(1)	(2)	(3)
FavOrigin	-0.041	-0.026	0.010
	(-0.928)	(-0.561)	(0.252)
Controls and fixed effects	Identical to Panel A of Table 8 (*Days since last forecast* is omitted)		
Number of observations	250,405	250,405	250,405
Adjusted R^2	20.70%	18.66%	24.44%

Table 9
Career outcomes

This table shows the estimates of career outcome regressions. We use pooled logit regressions in odd columns (1, 3, 5, 7 and 9) and linear probability models in even columns (2, 4, 6, 8 and 10). We use five dependent variables of analyst career outcome: *All-star* equals one if an analyst is ranked as an all-star analyst by the *Institutional Investor* magazine in the following year and zero otherwise; *Turnover* equals one if an analyst moves to another brokerage house or leaves the profession in the following year and zero otherwise; *Promotion* equals one if an analyst moves to a larger brokerage house in the following year, conditional on the analyst remaining in IBES, and zero otherwise; *Demotion* equals one if an analyst moves to a smaller brokerage house in the following year, conditional on the analyst remaining in IBES, and zero otherwise; and *Termination* equals one if an analyst disappears from IBES in the following year and zero otherwise. Mean $(\cdot)$ is a function that computes the mean value of a forecast-specific variable based on the last forecasts across the firms an analyst follows in a year.

	Dependent Variable:									
	All-star		Turnover		Promotion		Demotion		Termination	
Independent Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Mean (FavOrigin)	-0.324	-0.008	-0.202	-0.009	-0.366*	-0.008	0.000	0.000	-0.019	-0.001
	(-0.873)	(-0.313)	(-1.368)	(-0.439)	(-1.944)	(-0.504)	(0.000)	(0.083)	(-0.109)	(-0.079)
Mean (Accuracy)	8.282***	0.140*	-6.728***	-1.322***	-3.408***	-0.623***	-5.368**	-0.067**	-5.350***	-0.986***
	(3.023)	(1.868)	(-7.444)	(-8.866)	(-3.417)	(-4.734)	(-2.089)	(-2.155)	(-5.949)	(-7.630)
Brokerage size	1.628***	0.035***	-0.017	-0.030***	-0.112***	-0.053***	-0.068	-0.000	0.081***	0.014*
	(25.215)	(5.219)	(-1.075)	(-3.161)	(-5.627)	(-6.291)	(-0.881)	(-0.112)	(4.272)	(1.817)
Mean (Days since last forecast)	-0.252***	-0.004*	-0.082***	-0.015***	-0.071***	-0.006**	0.001	-0.000	-0.067***	-0.012***
	(-5.598)	(-1.777)	(-4.493)	(-4.998)	(-2.926)	(-2.419)	(0.014)	(-0.816)	(-3.386)	(-4.734)
Mean (Forecast horizon)	-0.194***	-0.004	1.120***	0.137***	1.812***	0.121***	1.254***	0.004***	0.670***	0.051***
	(-3.325)	(-1.457)	(26.525)	(25.130)	(21.158)	(20.259)	(4.029)	(3.163)	(18.892)	(11.987)
Forecast frequency	0.996***	0.061***	-2.024***	-0.326***	-0.335***	-0.063***	-1.640***	-0.009***	-3.068***	-0.313***
	(11.903)	(9.443)	(-35.591)	(-43.569)	(-4.267)	(-9.499)	(-5.597)	(-5.542)	(-45.452)	(-49.469)
General experience	0.839***	0.065***	0.322***	0.050***	0.222***	0.016***	0.489***	0.003***	0.335***	0.041***
	(11.939)	(13.429)	(10.651)	(11.147)	(6.417)	(4.978)	(3.661)	(3.529)	(8.942)	(10.710)
Number of firms	-0.011	0.000	0.093***	0.013***	0.034***	0.005***	0.087***	0.000***	0.119***	0.010***
	(-1.209)	(0.497)	(17.250)	(20.517)	(5.583)	(8.597)	(4.124)	(3.819)	(18.763)	(20.342)
Number of industries	0.017	0.001	-0.066***	-0.009***	-0.097***	-0.007***	0.043	0.000	-0.005	-0.004***
	(0.855)	(1.016)	(-8.010)	(-7.865)	(-9.665)	(-7.713)	(1.006)	(0.229)	(-0.490)	(-4.255)
Intercept	-11.982***	-0.319***	-1.049***	0.528***	-9.417***	-0.184***	-8.772***	0.006	2.500***	0.742***
	(-17.950)	(-8.172)	(-3.210)	(9.691)	(-15.389)	(-3.572)	(-3.712)	(0.561)	(7.729)	(17.147)
Brokerage fixed effects	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	35,463	35,463	35,463	35,463	30,546	30,546	28,941	30,546	35,463	35,463
Adjusted R^2	0.298	0.247	0.218	0.271	0.126	0.153	0.122	0.027	0.299	0.254

Table 10

Decomposition of country 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 countries of origin (*FavOrigin*) and the independent variables are five potential underlying factors of country 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, Niessen-Ruenzi, and Spalt 2015). *Same ancestry* is the weighted average of the percentage of U.S. citizens whose ancestors came from countries associated with the analyst's last name. *Same language* is the weighted average of English dummy for countries associated with the analyst's last name. The English dummy variable is equal to one if English is the official or the most popular language for a country and zero otherwise. *Cultural distance* is the weighted average of the culture difference for countries associated with the analyst's last name. The culture difference is measured as the mean value of the absolute differences in the Hofstede index between the U.S. 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) for countries associated with the analyst's last name. Weights are computed based on the frequency of the nationality of U.S. immigrants who have the same last name as an analyst's. In Panel B, we estimate the market reaction pooled 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. *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. *FavOrigin* (*Variable*) is computed by multiplying the value of *Variable* times the coefficient estimate of *Variable* obtained from the pooled OLS regression of *FavOrigin* on *Foreignness*, *Same ancestry*, *Same language*, *Cultural distance* and *Country corruption*. *FavOrigin* (*Residual*) is the residual value obtained from the OLS regression. We include the stand-alone variable of *FavOrigin* (*Variable*) in the regression model if its interaction with *Revision* is used. We use the same set of controls used in Panel A of Table 3. In parentheses below coefficient estimates are *t*-statistics based on standard errors clustered by analyst. All continuous variables are winsorized at the 1 and 99 percentile levels. Variable definitions are provided in Appendix B.

Table 10 (*Continued*)
Decomposition of country favorability

Panel A: Potential underlying factors of country favorability

	Dependent Variable: FavOrigin					
Independent Variables	(1)	(2)	(3)	(4)	(5)	(6)
Foreignness	-0.178***					-0.019***
	(-29.913)					(-5.402)
Same ancestry		0.983***				-0.046*
		(57.714)				(-1.726)
Same language			0.168***			-0.096***
			(63.163)			(-15.684)
Cultural distance				-0.010***		-0.014***
				(-96.581)		(-32.669)
Country corruption					-0.055***	-0.003**
					(-53.510)	(-2.209)
Intercept	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	36,826	36,826	36,826	36,826	36,826	36,826
Adjusted R^2	25.99%	42.74%	46.44%	72.79%	54.80%	76.69%

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

	Dependent Variable: Size-adjusted CAR [-1, +1]						
Independent Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Revision	1.081***	0.961***	0.994***	1.012***	0.858***	1.067***	1.417***
	(5.098)	(4.537)	(4.716)	(4.851)	(3.801)	(5.003)	(4.295)
Revision*FavOrigin *(Foreignness)*	4.872**						5.659**
	(2.397)						(2.237)
Revision*FavOrigin *(Same ancestry)*		-0.120					-0.505
		(-0.080)					(-0.198)
Revision*FavOrigin *(Same language)*			0.082				1.246*
			(0.290)				(1.730)
Revision*FavOrigin *(Cultural distance)*				0.082			0.518
				(0.684)			(1.490)
Revision*FavOrigin *(Country corruption)*					-3.206		1.831
					(-1.559)		(0.489)
Revision*FavOrigin (*Residual*)						0.938***	0.906***
						(4.036)	(3.995)
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	862,398	897,257	901,751	901,097	901,539	857,636	857,636
Adjusted R^2	9.70%	9.65%	9.65%	9.65%	9.66%	9.69%	9.70%