Individualism, deeply-rooted overconfidence, and analyst information production

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Abstract. In this paper, we argue that cultural heritage shapes the extent to which an analyst is subject to overconfidence (which we label deeply-rooted overconfidence). We find that deeply-rooted overconfidence, measured by Hofstede's (2001) Individualism score, affects the accuracy of analyst forecasts as these analysts allocate too much weight to their private information. We further document a lower informativeness of forecast and recommendation revisions, consistent with deeply-rooted overconfident analysts overestimating the quality of their private information. At the firm level, an exogenous shock in analyst coverage that results in an increase in the average overconfidence of analysts covering the firm causes a deterioration in the information environment in the form of higher earnings surprises and information asymmetry. Our findings suggest the existence of culturally-transmitted behavioral biases among analysts that influence their information production.

Keywords: Equity analysts, cultural heritage, overconfidence, forecast accuracy.

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1. Introduction

Overconfidence - the tendency of individuals to overweigh their private information - is one of the most salient behavioral biases in financial decision making (see Daniel and Hirshleifer (2015) and Malmendier and Tate (2015) for recent surveys), and is widely observed among financial agents.[1] Previous literature demonstrates that analysts are also prone to overweighing their private information (e.g., Bernhardt et al. 2006; Chen and Jiang 2005), and indeed Hilary and Menzly (2006) provide evidence that overconfidence affects analyst forecasts. Specifically, they document that analysts who made accurate predictions in the recent past make less accurate subsequent predictions. In this paper, we extend the literature on analyst overconfidence by focusing on a persistent source of overconfidence that is induced by an analyst's cultural heritage (which we label *deeply-rooted overconfidence*) and examine whether analyst forecast performance is affected by this deeply-rooted overconfidence.

The idea that culture shapes the behavior and practices of economic agents is not new (e.g., Hofstede 2001; Guiso et al. 2006). Psychology literature makes a strong connection between overconfidence and one specific dimension of culture: *individualism* (defined as the degree to which people focus on their internal attributes, such as their own abilities, to differentiate themselves from others (Hofstede 2001)). Markus and Kitayama (1991) and Heine et al. (1999), for instance, find that people in individualistic cultures tend to believe that their abilities are above average, while people in collectivistic cultures do not have this belief. Empirical evidence supports the idea of using individualism to capture a predisposition to overconfidence. Chui et al. (2010) and Dou et al. (2016) connect individualism to an excessive reliance on private information due to an overconfidence-induced self-attribution bias and document a relation with price and earnings momentum, respectively. Ferris et al. (2013) link individualism to CEO overconfidence in international merger and acquisition activity, while Antonczyk and Salzmann (2014) document that managerial overconfidence (measured by individualism) causes an upward bias in a firm's perception of supportable leverage ratios. Cheon and Lee (2018) demonstrate that individualism, as a proxy for overconfidence, is associated with an overpayment for stocks with lottery-like features.

[1]For instance, Odean (1998) shows that overconfident investors trade too much. Malmendier and Tate (2008) show that overconfident CEOs overpay for target firms and engage in value-destroying acquisitions. Friedman (2007) shows that overconfident individuals have a higher propensity to begin startup activities.

In this paper, we use individualism to capture deeply-rooted overconfidence of analysts. In line with recent literature that uses ancestry to determine a person's cultural heritage, we use surnames to identify an analyst's ancestry (Fernández 2011; Liu 2016; Du et al. 2017; Pan et al. 2017; Merkley et al. 2019; among others).[2] Based on the argument that culture is transmitted and shapes the beliefs and values of individuals over generations (Fernandez and Fogli 2009; Guiso et al. 2006; Robalino and Robson 2013), we associate individualism to an analyst based on her country of ancestry. We consider historical census records of foreign-born migrants (see Liu 2016), common American Asian family names (Lauderdale and Kestenbaum 2000), and the Oxford Dictionary of American Family Names, which allows us to identify the ancestry of 88.35% of the 13,605 unique analysts in our sample.

Our core hypothesis is that the private information revealed by overconfident analysts is less informative as overconfident analysts, due to a self-attribution bias, allocate too much weight to their private information. More specifically, we expect that forecasts based on private information are less accurate when made by analysts that are more individualistic because they, in part, reveal their overconfidence. We follow Clement and Tse (2005) and use bold forecasts (either above both the analyst's prior forecast and the consensus forecast before the analyst's forecast, or below both) to identify instances where analysts use private information to form their forecasts.[3] We empirically investigate our main conjecture using a large sample of earnings forecasts (855,604 analyst-firm-year forecasts) over the period 1994-2015, controlling for common determinants of forecast accuracy.

We find that bold forecasts of analysts with higher individualism scores are less accurate than other analysts covering the same firm in the same year. This result is consistent with analysts that are culturally more predisposed to overconfidence overestimating the precision of their private information. Setting all other variables to their mean values, an interquartile change in deeply-rooted overconfidence (individualism) reduces the benefit of using private information on forecast error by about 11%. This effect is of a similar magnitude to gaining three years of firm-specific experience, working for a top brokerage

[2]As surnames are inherited over generations, they convey information about an individual's ancestry and ethnic background. Name-based ethnicity classification methods have been used extensively in public health and population genetics literature, as reviewed by Mateos (2007). Mateos (2007) concludes that last name classifications overcome many of the issues of ethnicity classification based on other classification approaches.

[3]Prior literature shows that bold forecasts are, on average, more accurate and elicit stronger market reactions, suggesting that they contain valuable private information (Clement and Tse 2005; Gleason and Lee 2003).

house, or adding three other industries to the analyst's portfolio. It also compares well with other determinants of forecast accuracy recently uncovered in the analyst literature (e.g., Bradley et al. 2017a; Harford et al. 2018; Merkley et al. 2019). The relation between analyst deeply-rooted overconfidence and forecast accuracy is arguably causal as cultural preferences are shaped in the formative years of an individual and a consequence of parents' upbringing of their children.

We further show that the relation we document is stronger for bolder forecasts (i.e., forecasts by individualistic analysts that are further away from the consensus forecast are less accurate). Moreover, the effect of deeply-rooted overconfidence on forecast accuracy is mainly observed when individualistic analysts issue more optimistic forecasts than the consensus (rather than analyst issuing more pessimistic forecasts). This is consistent with Chen and Jiang (2005), who argue that analysts tend to overweigh the more favorable of the public or private information.

To demonstrate the robustness of our results, we conduct a battery of validity checks. Hilary and Menzly (2006) document that analysts who have predicted earnings more accurately than the median analyst in the recent past tend to be less accurate and further from the consensus forecast in their subsequent earnings prediction. Our results are robust to controlling for this short-lived overconfidence. Our results also remain when we use alternative culture frameworks (e.g., House et al. 2004; or Schwartz 2006), and are not sensitive to the choice of the surname library we use to determine the countries of ancestry of analysts and hold across subsamples excluding specific countries of ancestry. We next run a placebo test, where we replace Hofstede's individualism scores by random scores taken from within a plausible range of values (we generate 2,500 placebo coefficients). The distribution of placebo coefficients suggests that our results are not the product of randomness and depend on the analysts' individualism scores specific to their countries of ancestry. We further show that our results are specific to the individualism dimension, i.e., the moderating effects of other cultural dimensions on bold forecasts have no impact on forecast accuracy, which supports the interpretation of our findings as being the result of deeply-rooted overconfidence.

We further examine the informativeness of analyst forecasts and recommendation revisions, measured by the market reactions they elicit (e.g., Bradley et al. 2017a; Green et al. 2014; Harford et al. 2018; Loh and Stulz 2018). Consistent with overconfident analysts overestimating the quality of their private information, we expect revisions issued by overconfident analysts, when they are motivated by private signals, to have a lower information content (to elicit weaker market reactions). We document supportive evidence of this, lending support to our main conjecture of overconfident analysts overestimating the information content of their private signals.

Finally, we turn our attention to potential firm-level consequences of an analyst's deeply-rooted overconfidence. Analysts fulfil a key role in financial markets by producing and distributing financial information to market participants (e.g., Womack 1996; Asquith et al. 2005; Bradley et al. 2014; Huang et al. 2017; Loh and Stulz 2018). Prior studies show that the intensity of analyst coverage can influence a firm's information environment (e.g., Brennan and Subrahmanyam 1995; Hong and Kacperczyk 2010; Kelly and Ljungqvist 2012; Derrien and Kecskés 2013). We expect that, ceteris paribus, the lower accuracy and quality of the information issued by overconfident analysts may translate into a poorer information environment at the firm level. We examine the effect of an increase in the average individualism of analysts covering the firm on a firm's information environment. Following prior literature, we measure the quality of a firm's information environment using earnings surprises and illiquidity as a proxy for information asymmetry (e.g., Harford et al. 2018; Bradley et al. 2017a). To better identify the causal impact of an increase of analyst individualism on the firm's information environment, we use a natural experiment that provides an exogenous shock to analyst coverage. Specifically, we consider brokerage house mergers and closures resulting in drops in analyst coverage (e.g., Hong and Kaspersky 2010; Kelly and Ljungqvist 2012; Derrien and Kecskés 2013; Merkley et al. 2017). In line with prior studies, we find that a decrease in analyst coverage deteriorates the firm's information environment (larger earnings surprises and greater information asymmetry). More importantly, the negative effect of a reduction of analyst coverage on the firm's information environment is stronger when it results in greater individualism among the analysts covering the firm. This result is consistent with a real effect of deeply-rooted overconfidence at the firm level.

Our paper makes several contributions to the extant literature. We first add to the literature on analyst overconfidence. While Hilary and Menzly (2006) focus on short-lived overconfidence resulting from prior short-term forecast performance, we demonstrate that analysts are also subject to deeply-rooted overconfidence, which persistently differs across analysts depending on their cultural heritage.

Our paper adds more broadly to the literature on analyst behavior. Recent studies show that analysts are subject to behavioral biases such as anchoring bias (Cen et al. 2013), weather-induced inactivity (Dehaan et al. 2017), availability heuristic (Bourveau and Law 2018), the affect heuristic (Antoniou et al. 2018), distraction (Pisciotta 2018; Driskill et al. 2018), and decision fatigue (Hirshleifer et al. 2019). These analyst biases are, in turn, material to explain several important stock price anomalies (Grinblatt et al. 2018). Our results on deeply-rooted overconfidence expand this list of behavioral biases affecting analyst information production.

Finally, our paper contributes to an emerging literature on the effect of culture on analyst performance. Du et al. (2017) find that analysts who are Chinese immigrants or of recent Chinese descent issue more accurate forecasts on Chinese firms than other analysts. Their results suggest that cultural proximity affects information asymmetry in financial markets. We confirm their findings by showing that, unconditionally, analysts with high individualism scores issue more accurate forecast for U.S. firms (the world's most individualistic country), consistent with cultural proximity reducing information asymmetry. Controlling for this proximity, we show that analysts with high individualism scores are more predisposed to overconfidence about their private information which results in less accurate forecasts. Merkley et al. (2019) document that cultural diversity improves the accuracy of analysts' consensus forecasts and reduces the optimism bias and dispersion. Controlling for the effect of cultural diversity in our empirical analysis by looking at differences in forecast accuracy within the same firm-year, we show that a specific aspect of culture (individualism) explains persistent differences in analyst forecast performance.[4]

The remainder of this paper is as follows. Section 2 discusses the relevant literature and develops our main hypotheses. Section 3 presents our data, sample, and empirical design. Section 4 presents and discusses the results of our empirical analysis. Section 5 concludes.

[4]Jung et al. (2019) find that forecast revisions of analysts with surnames associated with more favorable countries of origin generate stronger market reactions. Their finding supports the notion that market participants assess the information content of an analyst's forecast revision based on the favorability of analyst's country of origin. While the authors uncover a bias in the market's reaction to analyst forecasts based on the analysts' country of origin, they do not expressly relate their finding to the culture of the country of ancestry.

2. Background and hypothesis development

2.1. Overconfidence and analyst performance

Overconfidence captures the tendency of individuals to overestimate the precision of their own information (Fischhoff et al. 1977; Alpert and Raiffa 1982). Overconfidence is widely observed among financial stakeholders and has material consequences for investment decisions and corporate policies. For instance, Malmendier and Tate (2005) show that investments of overconfident CEOs are more responsive to cash flow, particularly in equity-dependent firms. Malmendier and Tate (2008) find that CEO overconfidence influences merger decisions. Ben-David et al. (2007) document that overconfident CFOs use lower discount rates to value cash flows, invest more, use more debt, are less likely to pay dividends, are more likely to repurchase shares, and use more long-term debt. Grinblatt and Keloharju (2009) provide empirical evidence showing that overconfident investors trade more often. Friedman (2007) shows that the propensity of an individual to initiate startup activities is positively associated with an individual's level of overconfidence.

The psychology and behavioral economics literature document an important source of overconfidence: *self-attribution bias*. Individuals subject to self-attribution bias accredit too much of their success to their "superior" ability while attributing failures to external factors (Langer and Roth 1975; Miller and Ross 1975; Hirshleifer and Luo 2001). Self-attribution bias has been used to explain various overconfidence phenomena in finance and accounting (e.g., Daniel et al. 1998; Gervais and Odean 2001; Malmendier and Tate 2005; Billett and Qian 2008; Libby and Rennekamp 2012). In the analyst literature, prior studies document that analysts also tend to overweigh their own private information (e.g., Chen and Jiang, 2005; Bernhardt et al., 2006). The most direct evidence for this is provided by Hilary and Menzly (2006), who show that analysts become more overconfident after a short series of accurate predictions, and consequently are more likely to be inaccurate in subsequent forecasts. In this paper, we investigate another source of overconfidence among analysts, namely a culturally induced one.

2.2. Cultural heritage and deeply-rooted overconfidence

An economic agent's behavior, to some extent, depends on her cultural origins (see Hofstede (2001)). Defining culture as "*the collective programming of the mind distinguishing the members of one group or category of people* *from others*", Hofstede (2001, p.9) originally categorized national cultures into four dimensions: individualism, masculinity, power distance, and uncertainty avoidance.[5] These dimensions have been used widely to capture persistent differences across economic agents such as investors (e.g., Chui et al. 2010; Siegel et al. 2011; Aggarwal et al. 2012; Eun et al. 2015), banks (e.g., Giannetti and Yafeh 2012), corporates (e.g., Shao et al. 2010; Zheng et al. 2012; Ahern et al. 2015), CEOs (e.g., Bryan et al. 2015; Pan et al. 2017), directors (e.g., Frijns et al. 2016; Giannetti and Zhao 2019), consumers (Steenkamp et al. 1999), and individuals (e.g., Guiso et al. 2008).[6]

While culture has been shown to have an impact on economic and financial decisions and outcomes, a more recent literature has emerged focusing on how cultural heritage affects individuals' preferences. As Guiso et al. (2006) highlight, culture is a set of those *"customary beliefs and values that ethnic, religious, and social groups transmit fairly unchanged from generation to generation"* (p.23). One key aspect is that cultural values and preferences are instilled in the formative years of an individual and remain relatively unchanged in adulthood (Bisin and Verdier, 2000). Another aspect is that cultural transmission is persistent over generations. Moreover, Bisin and Verdier (2000) argue that minority groups have strong incentives to pass their cultural values on to their children.

Based on the notion that culture is persistent and transmitted over generations, various recent studies focus on the effect that cultural heritage has on the behavior and preferences of economic agents. Liu (2016), for instance, focuses on corruption culture and corporate misconduct and uses the cultural heritage of corporate directors and insiders to capture the corruption culture within a firm. Brochet et al. (2018) assess how managers' ethnic backgrounds affect their communications with investors, and document that more individualistic managers use a more optimistic tone, exhibit greater self-reference and make fewer apologies in their discussions. Giannetti and Zhao (2019) focus on board ancestral diversity and show that high ancestral diversity results in high firm performance volatility. All these studies use individuals' names

[5] *Individualism* captures how much value people place on taking care of themselves rather than prioritizing the collective. *Masculinity* measures the importance people place on achievement, assertiveness, and material reward for success. *Power Distance* captures people's acceptance of an unequal distribution of power within a society. *Uncertainty Avoidance* represents the degree to which people are uncomfortable with uncertainty and ambiguity.
[6] See Karolyi (2016) for a recent survey of the literature.

to deduce their cultural heritage. Mateos (2007) reviews the literature on name-based ethnicity classification methods and concludes that these methods have several advantages over other approaches.

In this paper, we focus on one specific dimension of culture: *individualism*. Hofstede's (2001) individualism dimension measures the degree to which people focus on their internal attributes, such as their own abilities, to differentiate themselves from others, and has been linked to overconfidence.[7] For instance, Markus and Kitayama (1991) and Heine et al. (1999) find that people in individualistic cultures, such as the U.S., tend to believe that their abilities are above average, while people in collectivistic cultures, such as Japan, do not have this belief. As Van den Steen (2004) discusses, when individuals have different priors and are overoptimistic about their abilities, they tend to overestimate the precision of their predictions, which aligns with the notion of overconfidence discussed in Daniel et al. (1998). Empirical evidence supports this connection between overconfidence and individualism. Chui et al. (2010) use individualism to proxy for overconfidence in their study on momentum returns in international stock markets. They argue that overconfident investors underreact to public news and overreact to private information, resulting in gradual incorporation of information into stock prices and therefore a momentum effect. They indeed find that momentum profits observed in a country are strongly related to overconfidence as proxied by individualism. Ferris et al. (2013) show that individualism positively influences the likelihood that a CEO will be overconfident and affects their activity in international mergers and acquisitions (overconfident managers make more offers on targets, and more cash-financed acquisitions). Antonczyk and Salzmann (2014), focus on corporate managers and conclude that managers from individualistic countries tend to be more overconfident and that this overconfidence affects the capital structure of the firm. Based on the same argument as Chui et al. (2010), Dou et al. (2016) document a strong relation between earnings momentum (post-earning-announcement drift) and individualism (overconfidence). Likewise, Cheon and Lee (2018) demonstrate that individualism, as a proxy for overconfidence, is associated with the overpayment of stocks with lottery-like features and can explain cross-sectional differences in stock return across different countries.

[7]The distinction between individualistic and collectivistic cultures pertains to the degree to which people have an independent rather than an interdependent self-image. In individualistic cultures, individuals tend to view themselves as autonomous and independent, while in collectivistic cultures, individuals view themselves "not as separate from the social context but as more connected and less differentiated from others" (Markus and Kitayama, 1991).

Prior literature makes a strong connection between individualism and overconfidence. In this paper, we extend this literature by assessing whether culturally-induced overconfidence also affects the forecast ability of analysts. To do so, we focus on an analyst's ancestry to examine whether an analyst's cultural heritage results in overconfidence, or as we label it, *deeply-rooted overconfidence*. Various studies document the importance of deeply-rooted values and use ancestry to capture these deeply-rooted values (e.g., Liu, 2016; Brochet et al., 2018; Giannetti and Zhao, 2019; Merkley et al., 2019). We conjecture that deeply-rooted overconfidence manifests itself in a poorer information production when overconfident analysts rely on private information. In this setting, overconfident analysts believe that their private information is more accurate than it is and hence put too much weight on it (Kraemer et al. 2006). We thus expect culturally-induced overconfidence of analysts, conditional on the use of private information, to translate into a lower forecast accuracy relative to other analysts.

3. Empirical design

3.1. Data sources

We obtain data from multiple sources. Analyst earnings forecasts and recommendations for U.S. firms are from I/B/E/S. Firm characteristics and stock returns are obtained from COMPUSTAT and CRSP. Information on institutional ownership is from the Thomson 13F database. To determine ancestry based on surnames, we obtain reference libraries of surnames from census data provided by IPUMS (Integrated Public Use Microdata Series), Asian American family names from Lauderdale and Kestenbaum (2000), and common American family names from the Oxford Dictionary of American Family Names.

3.2. Identifying the country of ancestry of U.S. analysts

To determine countries of ancestry for U.S. analysts, we start by collecting analyst surnames from the I/B/E/S detailed recommendation file for U.S. firms over the period 1994-2015. We use the recommendation file as it provides the most accurate data on sell-side analysts' surnames. We select all analysts with valid surnames in the recommendation file and exclude anonymous analysts, analysts with missing or incomplete names, and instances where surnames have been replaced by a country or industry name. We drop hyphenated and invalid names (including, for instance, parentheses, "_" or "&"), and remove titles from surnames (such as "Ph.D.", "MD", "CFA", "Jr.", "Dr."). After cleaning, we end up with a sample of 13,605 unique analysts and 9,714 unique surnames.

To map analyst surnames to countries of ancestry, we use three reference libraries. Our main reference library is based on historical census records of foreign-born U.S. residents (see, e.g., Liu, 2016). For this reference library, we obtain census data from IPUMS.[8] The IPUMS project collects and digitizes data from historical censuses. The censuses prior to and including 1940 are not anonymous, and so IPUMS provides data containing names, as well as countries of birth. As full count data are not available through the IPUMS web-portal, we requested data from IPUMS on all foreign-born individuals in each census year (we include the years 1850, 1880, 1900, 1910, 1920, 1930 and 1940). From these records, we remove surnames that contain non-alphabetical characters that would not occur in surnames (such as "?", "*", etc.). We also remove the records of married females, as these would likely adopt their husband's surname. Overall, we end up with a reference library that contains 6,182,373 unique surnames representing 68,134,313 individuals from 199 countries of ancestry.

While comprehensive, this census-based reference library has two drawbacks. First, it underrepresents Asian migrants, as there was relatively less migration from Asia towards the U.S. prior to 1940. Second, it does not identify people of Jewish descent, as they would have migrated from various other countries. To address these issues, we obtain two additional reference libraries. The first is a reference library of common Asian American surnames developed by Lauderdale and Kestenbaum (2000). This reference library contains 20,693 common surnames for Asian Americans for six major Asian American ethnic groups (Chinese, Japanese, Filipino, Korean, Indian, and Vietnamese). The second reference library is the Oxford Dictionary of American Family Names. This dictionary contains most likely regions of origin (sometimes countries, other times broader regions, such as Scandinavia) for close to 70,000 most common American family names. This library identifies whether a surname is a common Jewish name.

We match analysts' surnames with those in the respective reference library. For the census-based reference library, we collect the number of occurrences of that surname, the most likely country of ancestry

[8] https://usa.ipums.org/usa/

(i.e., the country with the highest number of surnames from that country) and the percentage of surnames from that country. For the other reference libraries, we match surnames and obtain the origin of that surname. We then implement the following algorithm to robustly identify the most likely origin of a surname. If a surname has more than 100 unique entries in the census-based reference library, and more than 60% of those surnames come from one country, we assign that country as the most likely country of ancestry for that surname. In all other cases, we cross-check the surname in the different reference libraries. If the surname has the same occurrence in two or more of the reference libraries, then we take that country of ancestry. If we find no match in the reference libraries, and we have an entry in the Asian reference library, we use that country of ancestry. If we only have one entry in one of the reference libraries, we use that entry. After matching all surnames, we run a cross-check in the Oxford Dictionary of American Family Names to determine whether that surname is a common Jewish name and if so, we replace the origin of that surname. This last filter is important as we would otherwise misclassify the origin of a substantial part of our sample of surnames.[9] After identifying the country of ancestry based on the three reference libraries, we match each analyst with Hofstede's individualism score.[10] In total, we match 88.35% of the analysts in our sample (12,465 of the 13,605 unique analysts).

3.3. Analyst forecasts

We next link the analysts for which we can identify their cultural heritage to the I/B/E/S dataset of one-year-ahead earnings forecasts of annual earnings using the analyst identifier code in the recommendation file (AMASKCD). As customary, we focus on forecasts that are issued between 360 to 30 days before the earnings announcement date (Clement, 1999; Clement et al. 2007; Harford et al., 2018). We limit our sample to fiscal years after 1994 because analyst data on recommendations is not widely available on I/B/E/S

[9] For instance, the most common Jewish surname in the census records is "Cohen" with 102,016 entries. If we were to rely on the census records, we would attribute this surname to Russian origin (over 65% of people with this surname migrated to the US from Russia). The Oxford Dictionary of America Family Names identifies this surname as Jewish.

[10] While our approach is comprehensive, we acknowledge that our procedure may incorrectly infer the ancestry of married females who adopt their spouse's surname. Since we do not have gender data on analysts, we cannot address this issue. However, we argue that the lack of this data has little impact on our results. First, Kumar (2010) shows that female analysts only make 16% of analysts over his sample period 1983-2005. Of those females, only a certain proportion will be married, and of those married females, only a certain proportion would adopt their husband's surname, and of those females who adopt their husband's surname, only a certain proportion would marry a husband with a different cultural background. Hence, we expect the misclassification to affect a relatively small proportion of analysts. Second, as this misclassification is likely to be random and it does not introduce a systematic bias. Such a random misclassification should bias our estimates towards zero.

prior to this and end our sample at the end of 2015. To be included in our sample, we require an analyst to provide at least two recommendations and to cover at least two firms over the sample period. We further require firms covered by I/B/E/S to also be covered by CRSP and drop firms covered by less than two analysts. We exclude firms belonging to the utilities (two-digit SIC code = 49) or the financial industry (one-digit SIC code = 6). Our final sample comprises of 855,604 analyst-firm-year unique forecasts (9,545 unique analysts covering 11,582 unique firms) over the 1994-2015 period.

3.4. Methodology

In line with prior literature on analyst forecast performance and overconfidence, we use a setting in which we test whether the private information revealed by analysts that are culturally more predisposed to overconfidence result in less accurate forecasts. To capture forecasts likely to be rich in private information, we use bold forecasts. We follow Clement and Tse (2005) and classify forecasts as bold if they are both above or below the analyst's prior forecast and the consensus forecast immediately before the analyst's forecast (*Bold*). Clement and Tse (2005) document that bold forecasts are, on average, more accurate than herding forecasts, even after controlling for analyst characteristics. Gleason and Lee (2003) find that stock price reactions are stronger for forecast revisions that deviate more from the prior consensus than for forecast revisions that herd towards the consensus. Thus, bold forecasts appear to reflect private information to a greater extent than non-bold (herding) forecasts (Clement and Tse, 2005).

To complement our approach, we also quantify the extent to which private information is incorporated into the analyst's prediction by using the relative distance of her forecast to the prior consensus (*RDC*). This distance measures the magnitude of an analyst's private information that prompted the forecast revision (e.g., Zitzewitz, 2001; Hilary and Menzly, 2006; Jegadeesh and Kim, 2009). We compute *RDC* as the absolute distance to consensus of a forecast minus the average absolute distance to the prior consensus of the other analysts covering the same firm-year divided by the average absolute distance to the prior consensus of the other analysts covering the same firm-year. A *RDC* of 10% indicates that the forecast distance from the prior consensus is 10% higher than it is, on average, for the cluster of firm-year forecasts.

To study analyst forecast performance, we consider the relative forecast error as our main dependent variable (e.g., Clement, 1999; Malloy, 2005; Clement et al., 2007; De Franco and Zhou, 2009; Green et al., 2014; Bradley et al., 2017b; Harford et al., 2018). Specifically, for analyst *i*, firm *j* and year *y*, we compute the relative forecast error (RFE_{ijy}) as per Clement (1999), i.e.,

$$RFE_{ijy} = \frac{(AFE_{ijy} - \overline{AFE}_{jy})}{\overline{AFE}_{jy}},$$

where AFE_{ijy} is the absolute forecast error, i.e. the absolute difference between analyst *i*'s earnings forecast of the end-of-the-fiscal-year earnings of firm *j* in year *y*, and $\overline{AFE}_{jy}$ One advantage of this measure is that it makes forecast error comparable across analysts for the same firm-fiscal year, as it measures an analyst's forecast error relative to all analyst covering the same firm and thus controls for differences across companies, time, and industries (Clement 1999; Ke and Yu, 2006).

We employ an OLS model to formally test whether analysts subject to deeply-rooted overconfidence issue less accurate forecast as a result of the overestimation of the precision of their private information. Our baseline model is as follows:

$$RFE_{ijy} = \beta_0 + \beta_1 Bold_{ijy} + \beta_2 Individualism_i + \beta_3 Individualism_i\ X\ Bold_{ijy} + \beta\ X_{iy} + \beta\ Y_{ijy} + Firm\ Year\ Fixed\ Effects_{jy} + \epsilon_{ijy} \quad (1)$$

where $Bold_{ijy}$ is a dummy variable that takes the value one if the forecast of analyst *i* for firm *j* and fiscal year *y* is bold.[11] $Individualism_i$ is Hofstede's individualism score for analyst *i*. Our main variable of interest is the interaction term $Individualism_i\ X\ Bold_{ijy}$. The interaction term captures instances where analysts whose cultural background predispose them to overconfidence issue forecast based on relatively more private information. We expect the coefficient on this interaction term to be positive because overconfident analysts tend to overestimate the precision of their private information.

[11] We use the most recent I/B/E/S weekly consensus forecast. Our results are similar if we use the prior consensus forecast based on all forecasts issued up to the one of the analyst, or the average forecast issued over the last 180 days.

X_{iy} represent several time-varying analyst characteristics that have been identified as important factors explaining forecast performance. Specifically, we control for general forecast experience (*General Experience*), measured by the total number of years that analyst *i* appears in I/B/E/S. Clement (1999) and Clement et al. (2007) document that general forecast experience translates into more accurate forecasts. We also control for the resources available to an analyst using an indicator variable that is one if the analyst works for a top-decile brokerage house based on the number of analysts employed (*Top 10 Brokerage*), and zero otherwise. We further control for analyst portfolio complexity measured by the number of firms in analyst *i*'s portfolio (*Portfolio Size*) in year *y* and the number of 2-digit SIC codes of these firms (*Portfolio Complexity*). Clement (1999) shows that portfolio size has an adverse impact on analyst earnings forecast accuracy. In addition, Cohen et al. (2017) find that analysts with larger portfolios are less likely to ask questions on firms' earnings conference calls. Y_{ijy} represent several time-varying analyst-stock specific characteristics that previous research identifies as important factors explaining forecast performance. We control for the number of days between analyst *i*'s forecast for firm *j* and the firm's earnings announcement (*Days Before Announcement*). Forecasts issued closer to the earnings announcement are likely to incorporate more information about the expected earnings and thus to be more accurate. Indeed, Clement (1999) shows that relative forecast errors are positively associated with the number of days between the forecast and earnings announcement earnings date. Prior studies also show that forecast horizon is strongly associated with analyst forecast bias, as analysts generally issue optimistic forecasts and gradually walk down their forecasts during the year (Richardson et al. 2004; Ke and Yu 2006; Bernhardt and Campello 2007). We also control for the number of forecasts issued by an analyst for a given firm-year (*Forecast Frequency*). This variable has been used previously to capture analyst effort (e.g., Jacob et al. 1999; Merkley et al. 2019). We finally control for firm-specific forecast experience (*Firm Experience*) of analyst *i* in year *y* measured by the total number of years that analyst *i* has been covering firm *j*.[12] Mikhail et al. (2003) measure analyst firm-specific forecast experience as the number of prior quarters for which the analyst has issued earnings forecasts for the firm and document that analysts become more accurate with experience.

[12]Because the I/B/E/S database is left censored, we cannot determine how much experience analysts have prior to the first year of available data. To mitigate this problem, we follow Clement (1999) and exclude analysts which appear in the first year of the database (1983). Forecast made in 1984 are also excluded from our analysis because there would be little variation in the experience variables for that year.

The inclusion of *Individualism* as a main effect arguably captures the effect of cultural proximity (Du et al. 2017). Since the U.S. is the most individualistic country on the Hofstede scale, a higher individualism score reflects closer proximity to the U.S., and in line with Du et al. (2017), we expect the direct effect of *Individualism* on relative forecast error to be negative.[13] We further control for firm-year fixed effects that capture observable and unobservable factors affecting analysts' forecast accuracy which do not vary within a group of analysts issuing earnings forecasts for the same firm-fiscal year. These firm-year fixed effects control for the effect of firm time-varying factors that prior literature has identified as potential determinants of analyst forecast accuracy such as size, book-to-market, stock momentum, trading volume, analyst coverage, and institutional ownership (e.g., Bradley et al. 2017b; Harford et al. 2018), but also controls for self-selection bias as analysts may self-select into specific companies.[14] Importantly, Merkley et al. (2019) show that analyst cultural diversity affects the quality of the consensus earnings forecast. Adding firm-year fixed effects to our baseline regression allows us to control for the effect of cultural diversity as it varies much more across firm-years than within firm-years.

We further conjecture that the effect of deeply-rooted overconfidence on forecast error is more pronounced when more private information is incorporated into the forecast. To capture this feature, we use the triple interaction term *Individualism X Bold X RDC*. We expect the coefficient on this interaction term to be positive, i.e., conditional on the use of private information, the effect of deeply-rooted overconfidence on forecast error should be magnified when more private information is incorporated into the forecast.

Finally, as Chen and Jiang (2005) show that analysts tend to overweigh the more favorable of the public or private information, we expect to observe the effect of deeply-rooted overconfidence only when the analyst makes bold forecasts that are above the consensus (reflecting and overly optimistic forecast

[13]We do not expect the unconditional effect of individualism to capture the effect of overconfidence, as overconfidence should matter much less when analysts rely more on public information (as captured by the unconditional effect). Indeed, replacing the Individualism score with other measures of cultural proximity produce very similar results to those presented in this paper.

[14]Prior literature shows that the same can be achieved by demeaning the right-hand-side variables that vary across one firm-year-quarter forecasts, i.e. X_{iq} and Y_{ijq} (Clement, 1999; Malloy, 2005; Clement et al., 2007; Bradley et al., 2017b). However, Gormley and Matsa (2014) show that this de-meaning can potentially produce inconsistent estimates and distort the results. They suggest using the actual value of the variables and controlling for fixed effects instead. For completeness, we ensure that our results are robust to both approaches.

rather than an overly pessimistic forecast). To assess this prediction, we consider bold forecasts that are above and below the consensus separately.

4. Empirical results

4.1. Descriptive statistics

Panel A of Table 1 reports the number and proportion of sample forecasts and analysts by country of ancestry. We identify 43 different countries of ancestry. In line with Merkley et al. (2019), we document that a large portion of U.S. analysts has Irish and UK ancestry (16.31% and 16.63%, respectively). We also notice the importance of other historical emigration areas, such as Germany, Italy, and Russia (15.03%, 6.77%, and 5.41%, respectively), from neighboring countries, Canada and Mexico (8.48% and 1.33%, respectively), and we note the relatively large proportion of analysts with Jewish or Chinese ancestry (7.64% and 4.72%, respectively). Panel B of Table 1 reports descriptive statistics for individualism based on different cultural frameworks for each country of ancestry. Our main measure is Hofstede's individualism score (*Individualism*), which ranges from 11 (Panama) to 90 (Australia).[15]

[*Insert Table 1 about here*]

Table 2 reports descriptive statistics for our main variables. The median analyst in our sample has a Hofstede individualism score of 70, covers a portfolio of 16 firms belonging to 1 industry, has a firm-specific experience of 2 years, and a general experience of 8 years. She issues 5 forecasts for a firm within a fiscal year. The median forecast in our sample is issued 182 days before the earnings announcement and 46 days after the last forecast by the same analyst. 60% of the forecasts are issued by analysts working for top brokerage houses. The average absolute forecast error is about 31%, and 76% of the forecasts are *bold.* These figures are similar to those documented by previous studies (e.g., Clement, 1999; Hilary and Menzly, 2006; Clement and Tse, 2005; Bradley et al., 2017a; Harford et al., 2018).

[*Insert Table 2 about here*]

[15]We added the score for the U.S. to show that this country has the highest individualism score (91).

4.2. Analyst deeply-rooted overconfidence and forecast accuracy

The first column of Table 3 reports the results for Equation (1). Standard errors are double-clustered by analyst and firm and robust to heteroskedasticity (Bradley et al., 2017a; Harford et al., 2018). Consistent with cultural proximity increasing the forecast accuracy of analysts (Du et al., 2017), *Individualism* has a significant negative association with relative forecast error. We interpret this unconditional effect of individualism on forecast accuracy as the effect of analysts being culturally closer to U.S. firms because the U.S. is the country with the highest individualism score (Hofstede, 2001). Consistent with prior findings on the effect of forecast boldness on forecast accuracy (Hilary and Menzly, 2006; Clement and Tse, 2005), we document that bold forecasts are significantly more accurate. In economic terms, bold forecasts are, on average, 14% more accurate than forecasts issued by analysts covering the same stock-year. The coefficients on the control variables all have the expected sign, and the magnitude of the effects are in line with prior literature (e.g., Bradley et al., 2017a; Clement, 1999; Clement and Tse, 2005; Green et al., 2014; Harford et al., 2018; Hilary and Menzly, 2006; Merkley et al., 2017). While more experienced analysts or those working for top brokerage houses issue, on average, more accurate forecasts, analysts covering a larger number of firms or industry, issuing forecasts further away from the earnings announcement date and issuing forecasts more often are, on average, less accurate in their forecasts.

[*Insert Table 3 about here*]

Turning to our variable of interest, the coefficient on the interaction term *Individualism X Bold* is positive and significant, i.e., bold forecasts of analysts with higher individualism scores are less accurate than other analysts covering the same firm in the same year. This result is consistent with analysts who are culturally more predisposed to overconfidence overestimating the precision of their private information. Setting all other variables to their mean values, an interquartile change in individualism (deeply-rooted overconfidence), from 55 to 80, reduces the benefit of using private information on forecast error by about 11% (from -12.5 to -11.8). This corresponds to an increase in forecast error of about 70%, which is sizeable compared to the effect of the standard determinants of an forecast accuracy, and is of a similar magnitude to gaining three years of firm-specific experience, working for a top brokerage house, or adding three other industries to an analyst's portfolio. The relation between analyst culturally-rooted overconfidence and forecast accuracy is arguably causal as cultural preferences are shaped in the formative years of an individual and a consequence of parents' upbringing of their children.

As we argued throughout the paper, we consider the effect of overconfidence conditional on the use of private information and use the indicator variable *Bold Forecast* to capture the use of private information. However, this variable does not quantify the amount of private information incorporated into the forecast. In line with our core conjecture, among overconfident analysts relying on private information, we expect those incorporating relatively more private information into their forecasts to experience a greater decrease in forecast accuracy. As discussed in Section 3, to capture the extent to which an analyst incorporates private information into her forecast, we use the relative distance to the prior consensus.

In the second column of Table 3, we report the results where we augment our core regression by the interaction term *Individualism X Bold X RDC*. Results indicate that deeply-rooted overconfidence is associated with a significantly greater forecast error when more private information is incorporated into the forecasts. These findings support the notion that the extent to which deeply-rooted overconfidence affects analyst forecast accuracy is a function of how much they rely on private information (and as such how much they overestimate their information advantage).

In the third column of Table 3, we split *Bold* into *Positive Bold* (a dummy variable equals to one when the for bold forecasts is above the consensus and zero otherwise) and *Negative Bold* (a dummy variable equals to one when the bold forecasts below the consensus and zero otherwise), and estimate the effect of two interaction terms*: Positive Bold X Individualism* and *Negative Bold X Individualism*. Chen and Jiang (2005) find that analysts overweigh their private information when issuing forecasts more favorable than the consensus, and underweigh private information when issuing forecasts less favorable than the consensus. Consistent with their finding, we find that the coefficient on the interaction term *Positive Bold X Individualism* is positive and significant, while the coefficient on *Negative Bold X Individualism* is insignificant. These results suggest that the lower accuracy of the forecasts issued by overconfident analysts mainly comes from overconfident analysts overestimating the quality of private information more favorable than the consensus, that is being too optimistic rather than being too pessimistic.

4.3. Robustness checks

In this section, we conduct a battery of tests to assess the robustness of our main result. We consider alternative measures for individualism, control for time-varying overconfidence, assess the robustness of the surname reference libraries, conduct a placebo test, and examine the role of other culture dimensions.[16] Our main result is robust to all these tests.

4.3.1. Alternative measures of individualism

The results reported so far are based on Hofstede's (2001) individualism score. To demonstrate that our results are robust to the selection of a specific culture framework, we want to ensure that our results hold when using alternative frameworks. First, we consider the cultural framework from the Global Leadership & Organizational Behavior Effectiveness (or GLOBE) project (see House et al. 2004), which, based on data from 17,300 managers in 951 organizations, devised 62 "societal cultures" and 10 "societal clusters" (e.g., Anglo cultures, Arab cultures, or Nordic Europe). It identifies nine cultural dimensions (uncertainty avoidance, future orientation, power distance, gender egalitarianism, in-group and institutional collectivism, humane orientation or rewarding of altruism, assertiveness orientation, and performance orientation or rewarding of excellence). We use GLOBE's *in-group collectivism* score to proxy for individualism, which assesses the degree to which individuals express pride, loyalty, and cohesiveness in their families. We multiply the GLOBE collectivism score by -1 to represent the level of individualism.

The culture framework of Schwartz (2006) offers a second alternative culture framework. This framework is based on an extensive list of 57 items in his survey of elementary school teachers and college students. He formulates seven key dimensions: embeddedness (versus autonomy), in which individuals find meaning through social relationships or a shared way of life, egalitarianism (versus hierarchy), in which individuals are seen as moral equals and people are socialized to internalize a commitment to cooperate, and mastery versus harmony, in which individuals seek to understand, preserve and protect a natural social

[16]In Appendix B, we report additional robustness checks to further validate the robustness of our main results at the analyst level. The effect of the interaction term *Bold X Individualism* is robust to the use of the last forecast per analyst-firm-year only (Column 1), alternative clusterings of standard errors (Columns 2 to 4), the use of log-transformed individualism scores (Column 5), to the inclusion of interaction terms of *Bold* with other control variables (Column 6), and to the inclusion analyst fixed effects (Column 7).

world rather than to change, direct or exploit it. We capture individualism using the bipolar dimensions of autonomy versus embeddedness from Schwartz (2006) and focus on embeddedness by multiplying the embeddedness score by -1.[17] In Panel A of Table 4, we estimate Equation (1) using the alternative culture frameworks. Our results are robust to the use of alternative culture frameworks.

[*Insert Table 4 about here*]

4.3.2. Controlling for time-varying overconfidence

Hilary and Menzly (2006) document that analysts who have predicted earnings more accurately than the median analyst in the previous four quarters tend to be less accurate and further from the consensus forecast in their subsequent earnings prediction. So far, we have not directly controlled for time-varying overconfidence. Following a similar approach to Hilary and Menzly (2006), we define a superior prediction as an analyst's forecast that is more accurate than the average forecast of the analysts covering the same firm-year. We then count the number of superior predictions an analyst has made in the previous firm-year. We call this variable *Number Prior Successes*. In line with Hilary and Menzly (2006), we expect analysts who have predicted earnings relatively more accurately to become overconfident about the quality of their private information in the next period and thus to issue relatively less accurate forecasts.

Panel B of Table 4 reports the results of our core regression augmented by the interaction term *Bold X Number Prior Successes* that captures the effect of time-varying overconfidence. In line with Hilary and Menzly (2006), we find that the coefficient on the interaction term *Bold X Number Prior Successes* is positive and significant. This result suggests that analysts who have predicted earnings more accurately in the past are indeed overconfident about their private information in the present period. The coefficient on the interaction term *Bold X Individualism* remains significant and positive, and of about the same magnitude as the one reported in Table 3. This indicates that the effect of deeply-rooted overconfidence is above and beyond the effect of the time-varying overconfidence documented in the literature.

4.3.3. Identifying the country of ancestry

[17]We also considered Schwartz's (2006) Affective Autonomy and Intellectual Autonomy scores and confirm that these scores produce similar results to those reported for the embeddedness score.

We next assess the robustness of our findings to changes in the way we assign countries of ancestry to our sample analysts. This step is important, because, as in previous studies (Du et al., 2017; Jung et al., 2019; Merkley et al., 2019), we rely on a specific matching algorithm to assign surnames to likely countries of origin. Moreover, when an analyst's surname is common in more than one country, or when the different sources indicate different countries of ancestry, we have to make a choice. To minimize the influence of our choices on our results, we proceed with the following complementary analysis.

We first rerun our core analysis removing the influence of countries of ancestry that have a large representation in our sample. We do this to ensure that our results are not only driven by one specific country of ancestry. Panel C of Table 4 reports the results when we exclude "Canada", "Germany", "Ireland", "Israel", "United Kingdom", or "Russia", respectively. As reported in Panel C of Table 4, in all cases we find similar results those reported in Table 3, i.e., bold forecasts are associated with greater forecast error when issued by analysts that are culturally more predisposed to overconfidence.

We next show the results for our core regression using different sources to match analyst surnames to their country of ancestry. Panel D of Table 4 reports the results. In Column 1, we only rely on IPUMS Census data to assign countries of ancestry to surnames. Arguably, this choice ensures a more homogenous classification approach compared to relying on several sources. The two other sources of family names we exploit are not based on immigration records. In Column 2, we only use the Oxford dictionary of America Family Names. In Column 3, we only use the IPUMS and Oxford dictionary and exclude the library of Asian surnames. Our result holds across the different combination of family names sources.

4.3.4. Falsification test: Placebo country scores for individualism

We next conduct a falsification test, where we replace the *Individualism* scores by random scores taken from a plausible range of values (11 to 90, see Panel B of Table 1). We rerun our main regressions (Equation (1), Column 1 of, Table 3, Panel A) using the placebo individualism scores instead of the Hofstede scores and repeat the process 2,500 times so that we obtain a distribution of placebo coefficients (see Figure 1). The average placebo coefficient for the interaction term *Bold X Placebo Individualism* is 0.006, with a standard deviation of 0.0148. The coefficient we document using Hofstede's *Individualism* score is 0.028, about two standard deviations away from the center of the distribution of placebo coefficients in the extreme right tail of the distribution. This observation suggests that our results are not the product of randomness and depend on the analysts' individualism scores specific to their countries of ancestry.

[*Insert Figure 1 about here*]

4.3.5. Other cultural dimensions

Our main conjecture is that a specific dimension of an analyst's cultural roots, namely individualism, affects her predisposition to overconfidence. Hence, we do not expect other cultural dimensions to make analysts persistently more overconfident about their private information and ultimately to affect analyst forecast accuracy. However, the effect we pick up may simply be an effect of culture in general. To assess whether the effect we pick up is due to culture itself, we re-estimate Equation (1) where we replace the individualism score with any other Hofstede culture score.

Panel E of Table 4 reports the results. As can be seen, none of the other culture dimensions have a direct association with forecast accuracy or an effect conditional on the use of private information. The specificity of the effect of individualism on top of controlling for cultural diversity (firm-year fixed effects) and cultural proximity (stand-alone cultural dimension variables) gives us comfort in interpreting our results as being a manifestation of deeply-rooted overconfidence rather than a general effect of culture (e.g., Du et al. 2017; Merkley et al. 2019).

4.4. Deeply-rooted overconfidence and forecast informativeness

We argue that deeply-rooted overconfidence results in poorer forecast accuracy because overconfident analysts tend to overestimate the quality of their private information. To further validate this argument, we study the market reaction to forecast revisions (recommendation changes) of overconfident analysts, conditional on the issuing of a bold forecast (bold recommendation). Following prior studies, such as Bradley et al. (2017a) and Harford et al. (2018), we use the market reaction to analyst information production as a proxy for how much new information a forecast revision or recommendation change contains. Within a firm-year, we expect the market to react less to a one-unit forecast or recommendation revision motivated by private information when it comes from a relatively more overconfident analyst.

We first examine the market reaction to forecast revisions. We standardize forecast revisions by the standard deviation of the forecast within a firm-year to make them comparable across firm-years. We then regress the cumulative abnormal returns (*CAR*) over the three-day window surrounding the forecast revision announcement, controlling for the usual covariates (e.g., Green et al., 2014; Bradley et al., 2017a; Harford et al., 2018; Loh and Stulz, 2018). Abnormal returns are computed as raw returns minus the CRSP value-weighted index returns.[18] To examine the market reaction to forecast revisions conditional on overconfident analyst relying on private information, we focus on the triple interaction term: *Standardized Forecast Revision X Individualism X Bold*. We expect a negative coefficient on this triple interaction term, suggesting that more overconfident analysts' forecast revisions elicit weaker market reactions when they signal their private information.

Results are reported in Panel A of Table 5, and, in line with prior literature, indicate that a one-unit change in forecast revision results in a significant abnormal market reaction of about 0.30%. Moreover, in line with Clement and Tse (2005), we find that forecast revisions that are more likely to be based on private information (bold forecasts) elicit stronger market reactions (as indicated by the coefficient on the interaction term *Standardized Forecast Revision X Bold*). A one unit forecast revision, conditional on issuing a bold forecast, elicits a greater market reaction by 1.5%, on average, which supports the market perception of valuable private information. Consistent with overconfident analysts issuing relatively less informative forecasts as they overweigh their private information, the coefficient on the triple interaction term *Standardized Forecast Revision X Bold X Individualism* is negative and significant. Setting all other variables to their mean values, an interquartile change in the deeply-rooted overconfidence of analysts (change in the *Individualism* score from 55 to 80) decreases the market reaction to a one-unit forecast revision conditional on issuing a bold forecast by 5% (from 1.35 to 1.29).

[*Insert Table 5 about here*]

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[18]As reported in column 2 of Table 6, results are similar if we use returns in excess of CAPM returns.

We next examine the market reaction to recommendation changes using a similar approach as for forecast revisions.[19] To examine the market reaction to recommendation changes conditional on an overconfident analyst overweighing private information, we include the triple interaction term *Recommendation Change X Bold Recommendation X Individualism* in a regression explaining the cumulative abnormal returns over the three-day window surrounding the announcement of the recommendation change and controlling for the usual covariates. We compute recommendation changes as the difference between the recommendation at time *t* and the last recommendation issued by analyst *i* for firm *j*. A positive (negative) change means a downward (upward) recommendation revision. For instance, a change of +1 can correspond to a change from 1 (strong buy) to 2 (buy). We do not standardize the recommendation changes because they are already comparable across firm-years. We define bold recommendations by analyst *i* for firm *j* following Jegadeesh and Kim (2009), i.e., as a recommendation that is more distant to the prior consensus recommendation than the previous one. We define the prior consensus recommendation as the average recommendation of all outstanding recommendations with at least two analysts following the stock as of the day before the revision, excluding the revising analyst.

Panel B of Table 5 shows that positive recommendation changes (downward revisions) elicit, on average, a significant negative market reaction, in line with prior studies (e.g., Jegadeesh and Kim, 2009). A one-notch increase in an analyst's recommendation is associated with an average market reaction of –1.9%. Also, in line with prior literature, a one-notch increase further away from the consensus recommendation elicits an even stronger market reaction (an additional -0.4%), which is consistent with the incorporation of private information into the recommendation change. Consistent with overconfident analysts issuing relatively less informative recommendations based on private signals, the coefficient on the triple interaction term *Recommendation Change X Bold X Individualism* is positive and significant. Setting all other variables to their mean values, an interquartile change in the exposure to deeply-rooted overconfidence (change in the *Individualism* score from 55 to 80) decreases the market reaction to a one-unit upward revision conditional on issuing a bold recommendation by about 9% (that is from -0.35 to -0.32).

[19]For recommendation changes we use firm and year fixed effects, instead of firm-year fixed effects, because there are too few recommendation changes by analysts within a firm-year.

Both findings, using either recommendation changes or forecast revisions, lead to the same conclusion that updates of analyst information driven by private information elicit weaker market reactions when they are issued by overconfident analysts, and supports the idea that analysts that are more predisposed to overconfidence because of their cultural roots, tend to overestimate the quality of their private signals and issue relatively less informative forecasts and recommendations. These results support our main conjecture that deeply-rooted overconfidence affects the information production of analysts.

4.5. Firm-level consequences of analysts' deeply-rooted overconfidence

Our results so far document that deeply-rooted overconfidence affects analyst forecast accuracy, and that these recommendations and forecast revisions result in weaker market reactions. This reduced forecast accuracy of overconfident analysts can have firm-level consequences when firms are mostly covered by overconfident analysts. Since sell-side analysts fulfil a key role in financial markets by producing and distributing financial information (e.g., Womack 1996; Asquith et al. 2005; Bradley et al. 2014; Huang et al. 2017; Loh and Stulz 2018),[20] the intensity of analyst coverage influences a firm's information environment (e.g., Brennan and Subrahmanyam 1995; Hong and Kacperczyk 2010; Kelly and Ljungqvist 2012; Derrien and Kecskés 2013). We thus expect that the lower accuracy and informativeness of the information produced by overconfident analysts translate into a poorer information environment at the firm level.

To identify the effect of deeply-rooted overconfidence, we examine the effect of a reduction in analyst coverage on a firm's information environment. The relation between analyst coverage and a firm's information environment may arise endogenously as analysts are arguably not randomly assigned across firms. We hence exploit a quasi-natural experiment connected to brokerage house mergers and closures. Prior studies use brokerage house mergers and closures to examine the effect of a decrease in the number of analysts following a firm on its information environment and policies (e.g., Hong and Kacperczyk 2010; Kelly and Ljungqvist 2012; Derrien and Kecskés, 2013). These studies conclude that these changes are exogenous and compare firms with analyst drops to those without drops. We identify analyst drops

[20] Huang et al. (2017) show that analysts play information intermediary roles by discovering information beyond corporate disclosures and by clarifying and confirming corporate disclosures.

following these prior studies.[21] We create an indicator variable, *Drop*, which we set to one if we identify an analyst drop due to a broker merger or closure and 0 otherwise. We then break *Drop* into two parts: *Drop – Increase in Individualism,* which takes the value of one for drops in analyst coverage resulting in a greater average individualism of the remaining analysts and 0 otherwise, and *Drop – No Increase in Individualism,* which takes the value one for drops in analyst coverage not resulting in a greater average individualism of the remaining analysts and 0 otherwise. In our test, we concentrate on the effect of analyst drops that coincide with an increase in average analyst individualism (*Drop – Increase in Individualism).*

Following prior literature, we measure the quality of a firm's information environment using earnings surprises and illiquidity - as a proxy for information asymmetry (e.g., Harford et al. 2018; Bradley et al. 2017a). We compute quarterly earnings surprises as the absolute distance to the last known consensus scaled by the stock price (*Absolute SUE*). We next examine the association between an increase in the average individualism of analyst covering a firm and information asymmetry (i.e., Amihud (2002)'s illiquidity measure). We control for the usual covariates of earnings surprise and illiquidity documented in the literature such as size, market-to-book, leverage, profitability, institutional ownership, trading volume, return volatility and momentum (e.g., Harford et al., 2018). We also control for year (year-quarter) and firm fixed effects. We expect the exogenous increases in individualism to be negatively associated with the firm's information environment.

Panel A of Table 6 provides summary statistics on the firm-level variables. The mean values of the variables are in line with the ones documented in prior studies. Panel B of Table 6 reports the results of our analysis. Consistent with prior studies (e.g., Derrien and Kecskés 2013), we find that exogenous reductions in analyst coverage are associated with a deterioration in the firm's information environment (larger earnings surprises and greater information asymmetry). When we split these reductions in analyst coverage according to whether they increase the average individualism of the remaining analysts, we find that the effect of a drop in analyst coverage is much stronger (weaker) when the latter coincides (does not coincide) with an increase in average analyst individualism. When a drop in analyst coverage results in greater individualism

[21]We use drops in analysts resulting from brokerage houses closures between 1993 and 2016. More specifically, we use I/B/E/S to identify brokers that disappear and determine brokerage house closures using press releases and broker mergers using SDC Mergers and Acquisition database. We assume that an analyst disappears if there is no earnings estimate in I/B/E/S during the year after the broker disappearance date.

among analysts, it is associated with an increase of quarterly absolute earnings surprise (information asymmetry) of 0.042 (1.012), significant at the 5% level. When drops in analyst coverage do not result in greater individualism among analysts, they are associated with a much smaller increase of quarterly absolute earnings surprise (information asymmetry) of 0.011 (0.557), not significant at the 5% level. The difference in the magnitude of the effect is about three times for earnings surprise and two times for information asymmetry. These results suggest that changes in analyst coverage that give more weight to the information production of analysts with a higher predisposition to overconfidence (deeply-rooted overconfidence as captured by higher cultural individualism) negatively affect the information environment of firms.

5. Conclusion

We examine the impact of analyst overconfidence on the analyst's information production. We focus on deeply-rooted overconfidence that analysts are predisposed to through their cultural heritage. We make use of an epidemiological approach to capture this deeply-rooted overconfidence, i.e., by focusing on the cultural heritage of an analyst. Using a large sample of sell-side analysts, we document a strong negative relation between deeply-rooted overconfidence and the accuracy of analyst forecasts when they use private information. This suggests that indeed, overconfidence is partly persistent and culturally induced and indeed provides new evidence that overconfidence affects analyst forecast accuracy. These results remain in a battery of robustness tests. Consistent with overconfident analysts overestimating the quality of their private information, we find that forecast and recommendation revisions issued by overconfident analysts, when they are motivated by private information, have a lower information content from a market perspective (they elicit weaker market reactions).

While previous studies show that culture, in general, may have a positive effect through cultural proximity or cultural diversity on analyst forecast production, we show that a specific cultural dimension (individualism), in specific circumstances in which overconfidence is likely to manifest itself, has a detrimental effect on forecast accuracy. Our study contributes by showing that the influence of culture is multifaceted. Our paper also provides evidence in favor of the existence of persistent behavioral biases that affect the information production of analysts.

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Figure 1. Distribution of the placebo coefficients

Panel A: Placebo Individualism scores and Relative Forecast Error – Analyst Level

This figure shows the distribution of 2,500 placebo coefficients on the interaction term *Bold Forecast X Individualism* in the regression reported in Table 3, Panel A, Column 1. Each time, we replace the Hofstede's *Individualism* country scores by random scores taken within a plausible range of values (11 to 90).

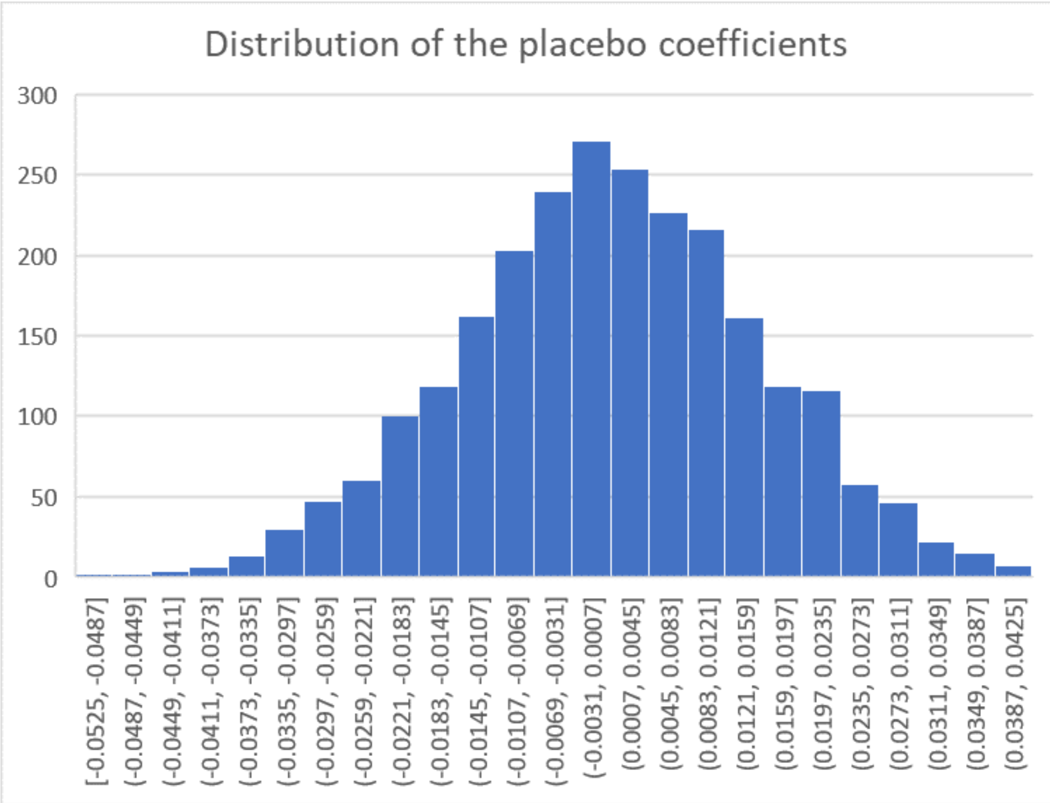

Table 1. Country-level descriptive statistics

Panel A: Number of forecasts and analysts per country of ancestry

Panel A reports the number and proportion of unique earnings forecasts and unique analysts per by country of ancestry.

Country of origin	Number of earnings forecast	Number of analysts	Proportion of earnings forecast	Proportion of analysts
Australia	238	2	0.03%	0.02%
Austria	14,828	127	1.73%	1.55%
Belgium	498	11	0.06%	0.13%
Brazil	70	2	0.01%	0.02%
Bulgaria	3	1	0.00%	0.01%
Canada	67,415	693	7.88%	8.48%
China	23,654	386	2.76%	4.72%
Czechoslovakia	7,891	80	0.92%	0.98%
Denmark	2,654	39	0.31%	0.48%
Estonia	23	1	0.00%	0.01%
Finland	2,583	21	0.30%	0.26%
France	2,712	33	0.32%	0.40%
Germany	140,933	1,228	16.47%	15.03%
Greece	6,228	55	0.73%	0.67%
Hungary	6,614	58	0.77%	0.71%
India	18,014	237	2.11%	2.90%
Iran	168	3	0.02%	0.04%
Ireland	146,387	1,333	17.11%	16.31%
Israel	76,909	624	8.99%	7.64%
Italy	60,108	553	7.03%	6.77%
Jamaica	37	2	0.00%	0.02%
Japan	1,430	31	0.17%	0.38%
Korea	3,969	59	0.46%	0.72%
Lithuania	2,409	22	0.28%	0.27%
Luxembourg	378	2	0.04%	0.02%
Malta	566	3	0.07%	0.04%
Mexico	10,624	109	1.24%	1.33%
Netherlands	6,158	41	0.72%	0.50%
Norway	6,632	87	0.78%	1.06%
Panama	3	1	0.00%	0.01%
Philippines	3,818	43	0.45%	0.53%
Poland	16,943	185	1.98%	2.26%
Portugal	2,012	32	0.24%	0.39%
Romania	610	8	0.07%	0.10%
Spain	2,877	11	0.34%	0.13%
Sweden	15,984	153	1.87%	1.87%
Switzerland	6,887	36	0.80%	0.44%
Thailand	10	1	0.00%	0.01%
Turkey	3,004	17	0.35%	0.21%
Russia	48,325	442	5.65%	5.41%
United Kingdom	141,746	1,359	16.57%	16.63%
Vietnam	634	18	0.07%	0.22%
Yugoslavia	2,618	22	0.31%	0.27%

Panel B: Individualism scores

Panel B presents the individualism score per country of origin for different culture frameworks. Definitions of the variables are provided in Appendix A.

Country of ancestry	Individualism (Hofstede)	Globe – In Group Individualism	Schwartz - Embeddedness
Australia	90.00	-4.17	-3.59
Austria	55.00	-4.85	-3.11
Belgium	75.00	.	-3.25
Brazil	38.00	-5.18	-3.62
Bulgaria	30.00	.	-3.87
Canada	80.00	-4.26	-3.46
China	20.00	-5.80	-3.74
Czechoslovakia	58.00	-3.18	-3.59
Denmark	74.00	-3.53	-3.19
Estonia	60.00	-5.63	-3.81
Finland	63.00	-4.07	-3.37
France	71.00	-4.37	-3.20
Germany	67.00	-4.02	-3.03
Greece	35.00	-5.27	-3.41
Hungary	80.00	-5.25	-3.60
India	48.00	-5.92	-3.97
Iran	41.00	-6.03	-4.18
Ireland	70.00	-5.14	-3.41
Israel	54.00	-4.70	-3.61
Italy	76.00	-4.94	-3.46
Jamaica	39.00	.	.
Japan	46.00	-4.63	-3.49
Korea	18.00	-5.54	-3.68
Lithuania	60.00	-5.63	-3.83
Luxembourg	60.00	.	.
Malta	59.00	.	.
Mexico	30.00	-5.71	-3.90
Netherlands	80.00	-3.70	-3.19
Norway	69.00	.	-3.45
Panama	11.00	-5.32	-3.49
Philippines	32.00	-6.36	-4.03
Poland	60.00	-5.52	-3.86
Portugal	27.00	-5.51	-3.43
Romania	30.00	.	-3.78
Spain	51.00	-5.45	-3.31
Sweden	71.00	-3.66	-3.12
Switzerland	68.00	-3.97	-3.34
Thailand	20.00	-5.70	-4.02
Turkey	37.00	-5.88	-3.77
Russia	39.00	-5.63	-3.81
United Kingdom	89.00	-4.08	-3.34
Vietnam	20.00	.	.
Yugoslavia	27.00	-5.43	-3.57

Table 2. Descriptive statistics

This table reports descriptive statistics for the forecast- and analyst variables. Appendix A provides the variable definitions.

Variable	Obs.	Mean	S.D.	0.25	Mdn	0.75
Forecast Error						
Absolute Forecast Error	855,604	0.24	0.52	0.03	0.08	0.23
Relative Forecast Error (%)	855,604	-1.37	72.91	-57.05	-12.26	36.95
Standardized Forecast Error (*100)	855,604	113.06	103.16	33.65	83.99	166.12
Forecast Characteristics						
Bold Forecast	855,604	0.74	0.44	0.00	1.00	1.00
Absolute Distance to Consensus	855,604	0.15	0.35	0.02	0.06	0.14
Relative Distance to Consensus	855,604	-0.01	0.80	-0.63	-0.18	0.41
Forecast Revision	561,963	-0.02	0.22	-0.06	0.00	0.05
Standardized Forecast Revision	561,931	-0.02	1.00	-0.59	0.00	0.58
Day Before Announcement	855,604	175.73	75.51	106.00	182.00	229.00
Recommendation Characteristics						
Bold Recommendation	249,543	0.47	0.50	0.00	0.00	1.00
Recommendation Change	249,543	0.08	1.31	-1.00	0.00	1.00
Days Since Last Recommendation	249,543	73.78	154.26	10.00	32.00	80.00
Market Reaction						
CAR Excess - Forecast Revision (%)	682,604	-0.05	6.62	-2.90	0.01	3.03
CAR Market - Forecast Revision (%)	682,604	-0.16	6.61	-2.96	-0.07	2.88
CAR Excess - Recommendation Change (%)	249,543	-0.29	8.22	-3.13	-0.04	3.06
CAR Market - Recommendation Change (%)	249,543	-0.38	8.18	-3.18	-0.12	2.91
Hofstede's National Cultural Dimensions						
Individualism	855,604	66.68	17.14	55.00	70.00	80.00
Uncertainty Avoidance	855,604	56.68	21.77	35.00	53.00	75.00
Masculinity	855,604	59.27	14.68	52.00	66.00	68.00
Power to Distance	855,604	40.77	20.72	28.00	35.00	50.00
Control Variables						
Days Before Announcement	855,604	175.73	75.51	106.00	182.00	229.00
Days Since Last Forecast	561,963	52.92	37.07	21.00	46.00	86.00
Firm Experience	855,604	3.32	3.65	1.00	2.00	5.00
General Experience	855,604	9.51	6.89	4.00	8.00	14.00
Top 10 Brokerage House	855,604	0.62	0.49	0.00	1.00	1.00
Portfolio Size	855,604	17.10	8.17	12.00	16.00	21.00
Portfolio Complexity	855,604	1.53	0.85	1.00	1.00	2.00
Alternative Individualism Scores						
Globe – In-Group Individualism	846,246	-4.66	0.65	-5.14	-4.70	-4.08
Schwartz – Embeddedness	853,989	-3.42	0.25	-3.60	-3.41	-3.34

Table 3. Deeply-rooted overconfidence and analyst forecast accuracy

This table reports the regression results of an analyst's *RFE* on the interaction between *Individualism* and *Bold* plus control variables. *RFE* is the percentage difference between the analyst's absolute forecast error and the average absolute forecast error of other analysts covering the same stock in the same year. Regressions include firm-year fixed effects. Standard errors are reported in parentheses. They are robust to heteroskedasticity and clustered by analyst-firm. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively. Appendix A provides variable definitions.

	RFE (%)	*RFE (%)*	*RFE (%)*
Individualism	-0.036***	-0.064***	-0.034***
	(0.012)	(0.023)	(0.012)
Bold	-14.024***	-26.953***	
	(0.785)	(1.250)	
Bold X Individualism	0.028***	0.073***	
	(0.011)	(0.018)	
RDC		30.491***	
		(1.886)	
Individualism X RDC		-0.125***	
		(0.027)	
Bold X RDC		-20.920***	
		(1.713)	
Bold X Individualism X RDC		0.057**	
		(0.025)	
Positive Bold			-20.149***
			(0.904)
Negative Bold			-7.540***
			(0.909)
Positive Bold X Individualism			0.042***
			(0.013)
Negative Bold X Individualism			0.005
			(0.013)
Days Before Announcements	0.479***	0.460***	0.480***
	(0.002)	(0.002)	(0.001)
Firm Experience	-0.196***	-0.190***	-0.197***
	(0.035)	(0.034)	(0.035)
General Experience	-0.030*	-0.033**	-0.029*
	(0.018)	(0.017)	(0.018)
Top Brokerage House	-0.879***	-1.097***	-0.946***
	(0.216)	(0.207)	(0.215)
Portfolio Size	0.044***	0.046***	0.044***
	(0.014)	(0.014)	(0.014)
Portfolio Complexity	0.292***	0.298***	0.300***
	(0.109)	(0.107)	(0.109)
Forecast Frequency	0.887***	0.842***	0.887***
	(0.067)	(0.068)	(0.066)
Observations	855,604	855,604	855,604
Firm-year Fixed Effects	Yes	Yes	Yes
Analyst-firm Cluster	Yes	Yes	Yes
Adjusted R-squared	0.18	0.19	0.18

Table 4. Robustness tests

Panel A: Alternative culture frameworks

This table reports the regression results for *RFE* on the interaction term between *Individualism* and *Bold* plus control variables. In the first column, we use Globe's In-group Individualism to proxy for individualism. In the second column, we use Schwartz's Embeddedness score. Regressions include firm-year fixed effects. Standard errors are reported in parentheses. They are robust to heteroskedasticity and clustered by analyst-firm. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively. Appendix A provides variable definitions.

RFE	Globe's In-Group Individualism	Schwartz's Embeddedness
Bold X Globe - In-group Individualism	0.755***	
	(0.290)	
Bold X Schwartz – Embeddedness		2.525***
		(0.755)
Observations	846,246	853,989
Control Variables of Table 3, Panel A	Yes	Yes
Firm-year Fixed Effects	Yes	Yes
Analyst-firm Cluster	Yes	Yes
Adjusted R-squared	0.18	0.18

Panel B: Controlling for prior analyst forecast accuracy

This table reports the regression results of *RFE* on the interaction term between *Individualism* and *Bold* plus control variables. Column 1 controls for the number of successful forecasts in the previous year and its interaction with *Bold*. Column 2 controls for the natural logarithm of the number of successful forecasts in the previous year and its interaction with *Bold*. Regressions include firm-year fixed effects. Standard errors are reported in parentheses and are robust to heteroskedasticity and clustered by analyst-firm. *, **, *** indicates significance at the 10%, 5%, and 1% levels, respectively. Appendix A provides variable definitions.

Relative Forecast Error	(1)	(2)
Individualism	-0.036***	-0.036***
	(0.012)	(0.012)
Bold	-15.214***	-15.829***
	(0.793)	(0.804)
Bold X Individualism	0.028***	0.028***
	(0.011)	(0.011)
Number Prior Successes	-2.014***	
	(0.138)	
Bold X Number Prior Successes	0.846***	
	(0.098)	
Ln(Number Prior Successes)		-6.403***
		(0.344)
Bold X Ln(Number Prior Successes)		2.812***
		(0.282)
Observations	855,604	855,604
Control Variables of Table 3, Panel A	Yes	Yes
Firm-year Fixed Effects	Yes	Yes
Firm-Analyst cluster	Yes	Yes
Adjusted R-squared	0.18	0.18

Panel C: Sensitivity to country of ancestry exclusion

Regression results of the regression reported in Table 3, Panel A, excluding analysts whose country of ancestry is "Canada", "Germany", "Ireland", "Israel", "United Kingdom", or "USSR", respectively.

RFE	Without Canada	Without Germany	Without Ireland	Without Israel	Without UK	Without USSR
Bold X Individualism	0.032***	0.026**	0.025**	0.032***	0.022**	0.032**
	(0.012)	(0.011)	(0.011)	(0.012)	(0.011)	(0.012)
Observations	788,189	714,671	709,217	778,695	713,858	807,279
Control Variables of Table 3, Panel A	Yes	Yes	Yes	Yes	Yes	Yes
Firm-year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Analyst-Firm Cluster	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.18	0.18	0.18	0.18	0.18	0.18

Panel D: Sensitivity to the choice of last-names dictionaries

Regression results of the regression reported in Table 3, Panel A, using different sources to match analyst family names to their country of origins.

RFE	Oxford dictionary only	Census data and Oxford dictionary only (excluding Asian name dictionary)	Census data only
Bold X Individualism	0.038**	0.034***	0.035***
	(0.014)	(0.012)	(0.011)
Observations	500,586	842,772	852,224
Control Variables of Table 3, Panel A, Column 1	Yes	Yes	Yes
Firm-year Fixed Effects	Yes	Yes	Yes
Analyst-firm Cluster	Yes	Yes	Yes
Adjusted R-squared	0.185	0.181	0.180

Panel E: Other cultural dimensions

This table reports the regression results of *RFE* on the interaction between Hofstede's four main cultural dimensions and *Bold* plus control variables. The regressions include firm-year fixed effects. Standard errors are reported in parentheses and are robust to heteroskedasticity and clustered by analyst-firm. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively. Appendix A provides variable definitions.

RFE	Individualism (1)	Uncertainty Avoidance (2)	Power to Distance (3)	Masculinity (4)	All (5)
Bold	-14.023***	-12.139***	-11.325***	-12.772***	-13.977***
	(0.785)	(0.531)	(0.416)	(0.801)	(1.714)
Individualism	-0.029				-0.006
	(0.018)				(0.023)
Bold X Individualism	0.028***				0.027**
	(0.011)				(0.013)
Uncertainty Avoidance		-0.010			-0.013
		(0.010)			(0.012)
Bold X Uncertainty Avoidance		-0.002			0.013
		(0.009)			(0.010)
Power to Distance			0.017*		0.016
			(0.010)		(0.011)
Bold X Power to Distance			-0.013		-0.016
			(0.009)		(0.011)
Masculinity				0.003	0.014
				(0.014)	(0.017)
Bold X Masculinity				0.009	0.002
				(0.013)	(0.014)
Observations	855,604	855,604	855,604	855,604	855,604
Control Variables of Table 3, Panel A	Yes	Yes	Yes	Yes	Yes
Firm-year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Analyst-firm Cluster	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.18	0.18	0.18	0.18	0.18

Table 5. Deeply-rooted overconfidence and market reaction to forecast and recommendation changes

Panel A: Forecast revisions

This table reports regression results of market reactions to forecast revisions on the interaction between *Individualism, Bold,* and *Standardized Forecast Revision* plus control variables. The regressions include firm-year fixed effects. The first column uses the cumulated abnormal returns over the three days surrounding the forecast revision to capture the market reaction, where abnormal returns are returns in excess of the CRSP universe value-weighted returns. In the second column, the market reaction is measured as the cumulated abnormal returns over the three days surrounding the forecast revision, where abnormal returns are returns in excess of the CAPM market model. Standard errors are reported in parentheses, and are robust to heteroskedasticity and clustered by analyst-firm. *, **, *** indicate the significance of the coefficient at the 10%, 5%, and 1% level, respectively. Appendix A provides the variable definitions.

Market Reaction	CAR excess (%)	CAR market (%)
Individualism	-0.001	-0.001
	(0.001)	(0.001)
Standardized Forecast Revision	0.364***	0.368***
	(0.104)	(0.104)
Standardized Forecast Revision X Individualism	0.001	0.001
	(0.001)	(0.001)
Bold	-0.167**	-0.166**
	(0.081)	(0.081)
Bold X Individualism	0.000	0.000
	(0.001)	(0.001)
Standardized Forecast Revision X Bold	1.490***	1.495***
	(0.112)	(0.112)
Standardized Forecast Revision X Bold X Individualism	-0.002**	-0.002**
	(0.001)	(0.001)
Days Since Last Forecast	-0.001***	-0.001***
	(0.000)	(0.000)
Days Before Announcement	0.000*	0.000**
	(0.000)	(0.000)
Firm Experience	0.005**	0.005**
	(0.002)	(0.002)
General Experience	-0.002**	-0.003**
	(0.001)	(0.001)
Top Brokerage House	-0.025	-0.024
	(0.016)	(0.016)
Portfolio size	-0.000	0.000
	(0.001)	(0.001)
Portfolio Complexity	-0.000	0.003
	(0.010)	(0.009)
Forecast Frequency	-0.004	-0.003
	(0.003)	(0.003)
Observations	446,483	446,483
Firm-year Fixed Effects	Yes	Yes
Analyst-firm Cluster	Yes	Yes
Adjusted R-squared	0.274	0.273

Panel B: Recommendation changes

This table reports regression results of market reactions to forecast revisions on the interaction between *Individualism, Bold Recommendation,* and *Recommendation Change* plus control variables. The regressions include firm and year fixed effects. In the first column, cumulated abnormal returns over the three days surrounding the recommendation change are used to capture the market reaction, where abnormal returns are returns in excess of the CSPR universe value-weighted returns. In the second column, the market reaction is measured as the cumulated abnormal returns over the three days surrounding the recommendation change, whereby abnormal returns are returns in excess of the CAPM market model. Standard errors are reported in parentheses, and are robust to heteroskedasticity and clustered by analyst-firm. *, **, *** indicate the significance at the 10%, 5%, and 1% levels, respectively. Appendix A provides the variable definitions.

	CAR excess (%)	CAR market (%)
Recommendation Change	-1.891***	-1.859***
	(0.074)	(0.074)
Individualism	-0.001	-0.001
	(0.003)	(0.003)
Recommendation Change X Individualism	0.003***	0.003***
	(0.001)	(0.001)
Bold Recommendation	-0.995***	-0.995***
	(0.128)	(0.127)
Recommendation Change X Bold Recommendation	-0.442***	-0.444***
	(0.108)	(0.108)
Individualism X Bold Recommendation	0.003	0.003
	(0.002)	(0.002)
Recommendation Change X Bold Recommendation X Individualism	0.004**	0.004**
	(0.002)	(0.002)
Firm Experience	0.006	0.006
	(0.006)	(0.006)
General Experience	0.005	0.004
	(0.003)	(0.003)
Top 10 Brokerage House	-0.075*	-0.074*
	(0.038)	(0.038)
Portfolio Size	-0.004	-0.003
	(0.002)	(0.002)
Portfolio Complexity	-0.020	-0.013
	(0.021)	(0.021)
Recommendation Frequency	-0.011	-0.022**
	(0.009)	(0.009)
Days Since Last Recommendation	0.001***	0.001**
	(0.000)	(0.000)
Observations	249,543	249,543
Year Fixed Effects	Yes	Yes
Firm Fixed Effects	Yes	Yes
Analyst-firm cluster	Yes	Yes
Adjusted R-squared	0.105	0.104

Table 6. Firm-level consequences of analyst deeply-rooted overconfidence

Panel A: Summary statistics for firm-level variables

This table reports descriptive statistics for the main variables entering the firm-level regressions. Appendix A provides the variable definitions.

Variables	Obs.	Mean	S.D.	0.25	Mdn.	0.75
Drop	65,824	0.07	0.26	0.00	0.00	0.00
Drop – Increase in Individualism	65,824	0.04	0.20	0.00	0.00	0.00
Drop – No Increase in Individualism	65,824	0.03	0.17	0.00	0.00	0.00
Absolute SUE (*100)	65,824	0.39	0.84	0.03	0.12	0.35
Information Asymmetry (*100)	22,815	6.07	19.49	0.07	0.40	2.35
Size	65,824	6.96	1.69	5.75	6.82	8.03
Market-to-book	65,824	3.44	3.83	1.59	2.50	4.07
Book Leverage	65,824	0.20	0.19	0.02	0.17	0.32
Profitability	65,824	0.03	0.14	0.01	0.05	0.09
Institutional Ownership	65,824	0.65	0.22	0.51	0.68	0.82
Trading Volume	65,824	13.67	1.55	12.62	13.64	14.70
Return Volatility	65,824	0.13	0.06	0.08	0.12	0.16
Momentum	65,824	0.20	0.56	-0.14	0.11	0.40

Panel B: Indiviudalism-increasing reduction in analyst coverage and firm information environment

This table reports results of regressions regarding the association between deeply-rooted overconfidence and the firm's information environment. *Drop in Analyst Coverage* is set to one if a firm experiences a decrease in analyst coverage following a broker's merger or closure. *Drop in Analyst Coverage – Increase in Individualism* is a dummy variable that is equal to 1 if the drop in analyst coverage leads to an increase in the average individualism of the analysts covering the firm and 0 otherwise. *Drop in Analyst Coverage – No Increase in Individualism* is a dummy variable that is equal to 1 if the drop in analyst coverage does not lead to an increase in the average individualism of the analysts covering the firm and 0 otherwise. The first and third columns report the regression results for the absolute earnings surprises (*Absolute SUE*), while the second and fourth columns report the results for the change in information asymmetry (*Information Asymmetry*). Earnings surprise (SUE) is computed as the distance of the actual EPS to the last known consensus scaled by the stock price. Information asymmetry is measured as the natural logarithm of Amihud's (2002) illiquidity measure for firms whose stock price is greater than $5. The regressions include year-quarter (year) and firm fixed effects. Standard errors are reported in parentheses and are robust to heteroskedasticity and clustered by firm. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Information Environment	(1) *Absolute SUE*	(2) *Information Asymmetry*	(3) *Absolute SUE*	(4) *Information Asymmetry*
Drop	0.028**	0.811***		
	(0.014)	(0.244)		
Drop – Increase in Individualism			0.042**	1.012***
			(0.018)	(0.320)
Drop – No Increase in Individualism			0.011	0.557*
			(0.020)	(0.336)
Size	-0.177***	-2.299***	-0.177***	-2.298***
	(0.012)	(0.340)	(0.012)	(0.340)
Market-to-book	-0.007***	0.000	-0.007***	0.000
	(0.001)	(0.022)	(0.001)	(0.022)
Leverage	0.036	4.471**	0.036	4.480**
	(0.046)	(1.766)	(0.046)	(1.766)
Profitability	-0.433***	-4.720***	-0.433***	-4.726***
	(0.066)	(1.378)	(0.066)	(1.378)
Institutional Ownership	-0.471***	-12.133***	-0.471***	-12.127***
	(0.053)	(1.282)	(0.053)	(1.282)
Trading Volume	0.040***	-3.605***	0.040***	-3.606***
	(0.011)	(0.428)	(0.011)	(0.428)
Return Volatility	1.167***	-11.812***	1.168***	-11.803***
	(0.175)	(4.232)	(0.175)	(4.233)
Momentum	-0.207***	-1.439***	-0.207***	-1.439***
	(0.011)	(0.273)	(0.011)	(0.273)
Observations	65,824	22,815	65,824	22,815
Year-quarter Fixed Effects	Yes	No	Yes	No
Year Fixed Effects	No	Yes	No	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Firm cluster	Yes	Yes	Yes	Yes
Adjusted R-squared	0.384	0.724	0.384	0.724

Appendix A. Variable definitions

Variable	Definition	Source
AFE	The absolute forecast error of analyst *i* for firm *j*, calculated as the absolute value of the difference between analyst *i*'s earnings forecast for firm *j* and the actual earnings reported by firm *j*.	I/B/E/S
Absolute SUE	Absolute quarterly earnings surprise, whereby we compute an earnings surprise as the distance to the last known consensus forecast scaled by the stock price.	I/B/E/S
Bold	Dummy variable that takes the value one if the forecast of an analyst *i* for firm *j* for the fiscal year *y* is above both the analyst's own prior forecast and the consensus forecast prior of analyst to the analyst's forecast, or else below both.	I/B/E/S
Bold Recommendation	Computed following Jegadeesh and Kim (2009) as recommendation change away from the consensus (average recommendation over the last 180 days).	I/B/E/S
Book-to-market	Book value of equity divided by the current market value of equity at the beginning of the fiscal year.	COMPUSTAT
Book-to-market	Book value of equity divided by the current market value of equity at the beginning of the fiscal year.	COMPUSTAT
CAR Excess – Forecast Revision	Cumulated abnormal returns over the three days surrounding the forecast revision. Abnormal returns are returns in excess of the value-weighted CRPS universe returns.	CRSP
CAR Excess – Recommendation Chance	Cumulated abnormal returns over the three days surrounding the recommendation change. Abnormal returns are returns in excess of the value-weighted CRPS universe returns.	CRSP
CAR Market – Forecast Revision	Cumulated abnormal returns over the three days surrounding the forecast revision. Abnormal returns are returns in excess of a CAPM market model, where the market returns are the value-weighted CRPS universe returns. We estimate the model over the 300 to the 50 days preceding the event. We require at least 200 observations to estimate the model.	CRSP
CAR Market – Recommendation change	Cumulated abnormal returns over the three days surrounding the recommendation change. Abnormal returns are returns in excess of a CAPM market model, where the market returns are the value-weighted CRPS universe returns. We estimate the model over the 300 to the 50 days preceding the event. We require at least 200 observations to estimate the model.	CRSP
Days Before Announcements	The number of days between analyst *i*'s forecast for firm *j* and the firm fiscal year end.	I/B/E/S
Days Since Last Forecast	Number of days between an analyst forecast for firm *j* for the fiscal year *y* and her last forecast for the same firm and year.	I/B/E/S
Firm Experience	The number of quarters since analyst *i*'s first earnings forecasts for firm *j* at year *y*.	I/B/E/S
Forecast Frequency	The number of forecasts issued by an analyst *i* for firm *j*'s earnings at quarter *t*.	I/B/E/S
Forecast Revision	The difference between the analyst *i*'s forecast for the firm *j*'s earnings at quarter *t* and the last analyst *i*'s forecast for the same firm and earnings at quarter *t* scaled by the last forecast.	I/B/E/S
General Experience	The total number of quarters that analyst *i* appeared in I/B/E/S at year *y*.	I/B/E/S
Globe – In Group Individualism	In group individualism score from House et al. (2004). Data are obtained from the GLOBE project website https://globeproject.com/	Globe
Hofstede - Individualism	Hofstede's (2001) individualism score for the country of origin we identify for an analyst *i*. We identify the country of origin based on the family name of the analyst. Data are from https://geerthofstede.com/	Hofstede
Hofstede - Masculinity	Hofstede's (2001) masculinity score for the country of origin we identify for an analyst *i*. We identify the country of origin	Hofstede

	based on the family name of the analyst. Data are from https://geerthofstede.com/	
Hofstede – Power Distance	Hofstede's (2001) power distance score for the country of origin we identify for an analyst *i*. We identify the country of origin based on the family name of the analyst. Data are from https://geerthofstede.com/	Hofstede
Hofstede – Uncertainty Avoidance	Hofstede's (2001) uncertainty avoidance score for the country of origin we identify for an analyst *i*. We identify the country of origin based on the family name of the analyst. Data are from https://geerthofstede.com/	Hofstede
Information Asymmetry	Amihud (2002)'s measure of illiquidity computed as the average daily ratio of absolute stock return to dollar volume over the last 250 trading days multiplied by 1000000. We exclude small stocks inferior or equal to five dollars.	CRSP
Institutional Ownership	The percentage of a firm's equity held by all institutions at the end of the previous fiscal year.	13F Thomson database
Leverage	Long-term and short-term debt divided by total assets.	COMPUSTAT
Momentum	Buy-and-hold returns over the last twelve months prior to the fiscal year end.	CRSP
Number of Prior Successes	Number of forecasts with a *Relative Forecast Error* inferior or equal to zero of analyst *i* for firm *j* over the previous year *y-1*.	
Portfolio Complexity	The number of 2-digit SICS represented by firms followed by analyst *i* in year *y*.	I/B/E/S
Portfolio Size	The number of unique firms followed by analyst *i* in year *y*.	I/B/E/S
Profitability	Return on asset at the end of the previous fiscal year.	COMPUSTAT
RFE	The difference between the absolute forecast error for analyst *i* and firm *j* in year *y* and the mean absolute forecast error for firm *j* in year *y* scaled by the mean absolute forecast error for firm *j* in quarter *y*.	I/B/E/S
Return Volatility	Standard deviation of the monthly returns over the last three years.	CRSP
Schwartz - Embeddedness	Schwartz's (2006) embeddedness score for the country of origin we identify for an analyst *i*. We identify the country of origin based on the family name of the analyst. Data are from Shalom Schwartz's ResearchGate website: https://www.researchgate.net/publication/304715744_The_7_Schwartz_cultural_value_orientation_scores_for_80_countries	Schwartz (2006)
Size	Natural logarithm of market capitalization of the covered firm (in $thousands) at the end of the previous fiscal year.	COMPUSTAT
Top 10 Brokerage House	Indicator variable that is equal to one if an analyst *i* works at a top decile brokerage house in quarter *t*.	I/B/E/S
Trading Volume	The annual trading volume (in thousand shares) for a firm *j* in the previous fiscal year.	CRSP
Volatility	Standard deviation of the monthly stock returns over the last 36 months preceding the fiscal year end.	CRSP

Appendix B. Further robustness checks

This table reports a series of additional robustness checks on our core regression results reported in Table 3, Panel A. We respectively assess the robustness of our finding to the use of last forecast per analyst-firm-year only (1), alternative standard errors clustering (2, 3, and 4), the use of the natural logarithm of Hofstede's individualism scores, analyst experience and portfolio size (5), the interaction of *Bold Forecast* with all other control variables (6), and the inclusion of analyst fixed effects (7). *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively. Standard errors are reported in parentheses. Appendix A provides the variable definitions.

Relative Forecast Error	(1) Latest forecast only	(2) Firm Cluster	(3) Analyst Cluster	(4) Firm-year Cluster	(5) Log-transformation	(6) Interaction with the other control variables	(7) Analyst fixed effects
Bold Forecast X Individualism	0.030***	0.028**	0.028**	0.028**	1.618***	0.028**	0.022**
	(0.012)	(0.012)	(0.013)	(0.011)	(0.617)	(0.011)	(0.011)
Observations	295,420	855,604	855,604	855,604	855,604	855,604	855,604
Control Variables of Table 3, Column 1	Yes	Yes	Yes	Yes	Yes	Yes	No
Firm-year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	No
Year Fixed Effects	No	No	No	No	No	No	Yes
Analyst Fixed Effects	No	No	No	No	No	No	Yes
Analyst-firm Cluster	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.20	0.18	0.18	0.18	0.16	0.16	0.21