Is the Stock Market Biased against Diverse Top Management Teams?

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Keywords: Biased Expectations, Top Management Teams, Diversity

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1. Introduction

Stock market investors are becoming increasingly vocal about the diversity of corporate leadership teams, following a global trend to engage with diversity issues at all levels of society. For example, in its 2019 proxy voting guidelines BlackRock requires companies to "take into consideration the full breadth of diversity including personal factors, such as gender, ethnicity, and age; as well as professional characteristics, such as a director's industry, area of expertise, and geographic location" when nominating new board members.[1] As an another example, a group of large investment funds including CalPERS petitioned the SEC in 2015 to enhance disclosure requirements on the diversity of corporate boards.[2] And Institutional Shareholder Services (ISS) has listed diversity "beyond gender and beyond the boardroom" as the number one item on their list of the "Top 10 Corporate Governance Topics to Watch in 2019," predicting in particular an increased focus on diversity among C-Suite managers.[3] In this paper, we propose a new approach to studying how diversity among C-suite managers matters for firms in the stock market.

The interest by practitioners in the diversity of corporate leadership teams accords well with a long-standing interest in the topic by academics. Specifically, a substantial body of prior work in finance, which we discuss in greater detail below, asks whether diverse corporate leadership affects corporate decision making. Broadly speaking, most of this work is concerned with the question of what firms with diverse leadership do and whether diversity, via its impact on corporate decision making, relates to firm performance.

Our paper departs from much of the existing literature by emphasizing that diversity could

[1]Source: https://www.blackrock.com/corporate/en-br/literature/fact-sheet/blk-responsible-investment-guidelines-us.pdf. Version of January 2019.

[2]They write: "We believe better disclosure about the board's skills, experiences, gender, race, and ethnic diversity can help us as investors determine whether the board has the appropriate mix to manage risk and avoid groupthink. For these reasons, we urge the Commission to initiate a rulemaking process to require better disclosure." The petition was submitted in 2015. The signatories included the California Public Employees Retirement System (CalPERS), the Washington State Investment Fund, the Connecticut Retirement Plans and Trust Fund, the California State Teacher's Retirement System, the Illinois State Board of Investment, the Ohio Public Employees Retirement Systems, the New York State Common Retirement Fund, and the North Carolina Department of State Treasurer. The full text can be found at: https://www.sec.gov/rules/petitions/2015/petn4-682.pdf.

[3]Source: http://clsbluesky.law.columbia.edu/2019/01/11/iss-lists-top-10-corporate-governance-topics-to-watch-in-2019/

matter for firms *over and above* affecting corporate decision making. In particular, we provide novel empirical evidence on how diverse top management teams are *perceived* in the stock market. Perception is of central importance in this setting, because stock prices reflect expectations, and because perceptions of diversity can potentially color expectations. Our results suggest that the diversity of a top management team can itself affect the stock market's expectations about firm performance, and therefore firm value, even if there is no difference in what firms with diverse leadership do. Cleanly separating the impact of diversity on corporate actions from the impact of diversity on investor perceptions thus emerges as an essential challenge for existing and future academic work on the link between diversity and shareholder value. Our new results on the potential role of biased investor perceptions about diversity contribute to an understudied part of the literature according to Adams, Haan, Terjesen, and Ees (2015).

Prior studies in finance and other fields provide evidence consistent with both costs of diversity (e.g., higher potential for frictions within the team) and benefits of diversity (e.g., more creativity in problem solving) for the efficiency of corporate decision making. The net effect of diversity on firm performance depends on the relative magnitude of benefits and costs, but the existing literature has so far not reached a consensus on the sign of the net effect, let alone on its magnitude (see e.g., Harrison and Klein (2007), Adams, Haan, Terjesen, and Ees (2015) for survey papers). Our study is motivated by the idea that, given the uncertainty surrounding whether diversity helps or hurts firm performance, there may be room for stock market participants to deviate from rational expectations. Whether biases in expectations exist for the diversity attribute in the stock market, whether such biases are quantitatively meaningful, and whether they are in favor of or against diverse teams are empirical questions we address in this paper.

Empirically investigating how diversity affects perception is challenging. One challenge is that diversity is a complex construct – while it is often defined narrowly to describe gender and race, it can have more dimensions which are relevant in the managerial context (e.g., Harrison and Klein (2007), Adams, Haan, Terjesen, and Ees (2015), Hillman (2015)). A long tradition among management scholars emphasizes the multi-dimensional nature of diversity, and argues that studying one attribute of diversity in isolation can be misleading in the presence of correlations between different attributes (e.g., Jackson, Joshi, and Erhardt (2003)). In line with this academic view, BlackRock explicitly recognizes that "diversity has multiple dimensions," which is reflected in the quote above, and in line with similarly broad definitions adopted by CalPERS and ISS. The fact that diversity can be multi-dimensional, and the fact that some of the largest stock market investors adopt broad definitions of diversity, raises the question of how to make multi-dimensional diversity measures operational in empirical work.

Our contribution on that front is to develop a new text-based measure that can capture many dimensions of diversity simultaneously. Specifically, we measure the diversity of a top management team from the cosine similarity of managerial biographies of the team's members, a procedure which exploits detailed multi-dimensional information on individual managers. An advantage of this approach is that the SEC requires all listed firms to disclose biographies of top executives. By tapping this new data source, we can capture over 6,500 unique firms over the period from 1999 to 2014, tracking over 70,000 individual executives, and assemble what is, to our knowledge, one of the largest datasets on top management team diversity in the literature. We show that our new multi-dimensional text-based measure of diversity correlates in plausible ways with a number of more traditional uni-dimensional measures including gender, race, education, and prior work experience, but we also show that the text-based measure has incremental power vis-à-vis the more traditional ones.

A second central challenge is that diversity may affect both what firms do and how they are perceived. We use two different approaches to overcome this challenge. In the first part of our paper, we study expectations directly, by looking at earnings per share forecasts by financial analysts. Our main finding is that analyst forecasts on firms with more diverse top management teams are more pessimistic than forecasts for otherwise similar homogeneous firms. We show that the implied expectational bias is economically substantial. We find similar results when we analyze one-year-ahead target price estimates. These results are consistent with analysts believing that the costs of more diversity in a top management team outweigh the benefits.

By design, our tests rule out a number of alternative explanations. First, because we focus on the difference between forecast and actual earnings, our results cannot be easily explained by diversity affecting what firms do. Second, the results are not driven by a battery of observable variables. Third, our results do not simply reflect known biases in analyst forecasts, because our identification comes from comparing differences in biases between forecasts on diverse vs. other firms. Fourth, because we include industry × date fixed effects, our results are not induced by industry-level drivers even if those drivers are unobservable and time-varying. Finally, analyst × date fixed effects show that *the same analyst at the same point in time* is issuing more pessimistic forecasts on diverse firms than other firms. This result is powerful, because it speaks against alternative explanations which relate to, for example, overall analyst skill, status as a top analyst, incentives from pay contracts, employer characteristics, and analyst demographics such as age, gender, and education.

Additional tests provide support for the view that it is really top management team diversity which affects forecasts, as opposed to some variable correlated with diversity and not captured by our extensive set of controls and fixed effects. In particular, we show that our results are not induced by top management diversity being correlated with corporate governance, diversity further down in an organization, the complexity of the firm's operations, the complexity of a firm's disclosures, the size of the team, the length of biographical texts, recent changes in the top management team, or analyst conflicts of interest.

We also find that analyst experience with a given firm substantially reduces the bias in expectations due to diversity. It is therefore unlikely that some stable, potentially unobserved, differences between more diverse and other firms which happen to be correlated with the diversity characteristic are inducing the analyst bias we document. The results are consistent with the view that inexperienced analysts make systematic mistakes in incorporating the diversity attribute into their forecasts and, on a more positive note, suggest that learning may help reduce the bias.

Overall, the first part of our paper shows that, relative to forecasts for homogeneous teams, analyst forecasts are biased against diverse top management teams. Given these results, a natural question is whether stock market investors have similar expectational biases. Two reasons suggest this may be the case. First, analysts are an important source of information in financial markets, so any biases they have may feed through to investors. Second, the average investor may be biased in the same way as the average analyst.

In the second part of our paper, we provide evidence which indicates that the bias we measure for analysts also affects a substantial fraction of investors in the stock market. We start by examining the holdings of institutional investors. We show that firms with more diverse top management teams are, all else equal, less likely held by institutional investors, even though we find no evidence suggesting that more diverse stocks are associated with lower returns. If anything, diverse stocks have higher risk-adjusted returns. Both results are consistent with the view that the average institutional investor, just like the average analyst, has downward-biased expectations about firms with diverse management teams. Apparently, what *some* investors say publicly about diversity, and how diversity features in the investment decisions of the *average* investor, are not necessarily the same thing, which is similar to patterns found in hiring decisions of women and minorities (e.g., Scheiber and Eligon (2019)). Finding that institutional equity investors are on average biased against diversity is in line with similar findings for bond market investors, retail mutual fund investors, and early stage investors (e.g., Kumar, Niessen-Ruenzi, and Spalt (2015), Ewens and Townsend (2017), Gompers and Wang (2017), Dougal, Gao, Mayew, and Parsons (2018)). Our results also obtain when we focus only on investments in non-local companies, so the patterns are not due to local bias or local information stories.

Further supporting the bias interpretation, we show that the tendency to shun diverse firms is attenuated for institutions headquartered in states with greater minority populations, more votes for the Democratic party, and states that rank lower on an index of racial animus, i.e. states in which diversity may plausibly have a less negative connotation.

While the tests on holdings and returns are informative, and consistent with biased expectations, attributing causality is hard. We therefore conduct an additional test which, we argue, substantially attenuates pertinent identification concerns. Specifically, we follow Engelberg, McLean, and Pontiff (2018) who propose a method to identify expectational biases of investors from stock returns around information release days. The core idea is that, in a model with unbiased expectations, there should be no systematic difference between stock returns of diverse firms and other firms around information releases such as earnings announcements. By contrast, downward-biased expectations predict that markets are "systematically positively surprised" about new information released to markets by diverse firms. Our results strongly support downward-biased expectations.

In sum, we believe that the combined evidence from our three sets of tests argues strongly in favor of the view that a substantial fraction of analysts and stock market investors are biased against firms with diverse top management teams, which raises many new questions for firms, analysts, investors, researchers and lawmakers.

1.1 Relation to Existing Literature

Our paper contributes to the corporate finance literature on the diversity of corporate leadership teams (see e.g., Ferreira (2010) for a survey). Examples include studies on the effects of women on boards (e.g., Adams and Ferreira (2009), Ahern and Dittmar (2012), Adams (2016), Kim and Starks (2016)), on CEO power vis-à-vis the board (e.g., Adams, Almeida, and Ferreira (2005), Fahlenbrach (2009), Bebchuk, Cremers, and Peyer (2011)), on the nationality of board members (e.g., Masulis, Wang, and Xie (2012)), on the variation in expertise and prior work history (e.g., Güner, Malmendier, and Tate (2008), Coles, Daniel, and Naveen (2015)), and studies that combine several characteristics into an index (e.g., Giannetti and Zhao (2018), Bernile, Bhagwat, and Yonker (2018), Adams, Akyol, and Verwijmeren (2018)).

There are a number of differences between our study and most of the existing literature on diversity in finance. First, our results suggest that top management team diversity induces biases in expectations among important players in the stock market, in particular analysts and institutional investors. In the existing finance literature, many papers focus on what firms with diverse top management teams do, but few study how diversity influences perceptions. Consistent with this reading of existing work, Adams, Haan, Terjesen, and Ees (2015) argue in a review article on the diversity of corporate leadership teams that studying "biases of market participants [with respect to diversity] is an interesting area for future research." To the best of our knowledge, our paper provides some of the first, and some of the cleanest empirical evidence to suggest that stock markets are biased against firms with diverse leadership teams. Our findings are consistent with, and related to, work by Gompers, Mukharlyamov, and Xuan (2016), Ewens and Townsend (2017), and Gompers and Wang (2017), which suggests early stage investors are biased against diverse leadership teams, in particular against women, but our focus on large established firms, institutional investors, and analysts in the stock market makes our paper substantially different.

Second, our paper has important methodological implications. In particular, our findings highlight that using market values or stock returns to analyze the quality of what firms with diverse top management teams do is problematic whenever the researcher cannot rule out that investor perceptions matter. Our paper suggests that investor perceptions can have a large impact on market prices, which in turn suggests that finding a way to empirically separate perception from fundamentals should feature prominently in related work on diversity.

Third, most papers in the literature use a bottom-up approach, i.e., they measure diversity by relying on (usually) one or (sometimes) several variables hypothesized to capture the relevant dimensions of group heterogeneity (e.g., age, gender, education, etc.). By contrast, our approach is top-down: we rely on similarities in biographical texts, which eliminates the need to narrow the focus down to a small pre-specified set of dimensions and allows us to capture many similarities on a very detailed level. By using a top-down approach, our study complements the bottom-up approach in the existing literature and offers a new way to address largely open questions about how different dimensions of diversity interact, and how researchers can capture their joint impact (e.g., Hillman (2015)). We show that our text-based approach contributes fundamentally new evidence on the impact of diversity on financial outcomes, not captured by more traditional measures of diversity.

Fourth, we focus on the top management team of executive officers, rather than corporate boards. While boards may matter more for the broad strategic direction of a company, the top management team is more likely relevant for the firm's day-to-day operations, which is why top management teams are potentially of interest for analysts and investors. In finance, compared with the literature on corporate boards, few papers focus on top management teams, even though good theoretical and empirical reasons suggest looking at top management teams can be incrementally valuable (see e.g., Landier, Sauvagnat, Sraer, and Thesmar (2013)), and even though the related management literature has emphasized top management team diversity much more strongly than board diversity (e.g., Nielsen (2010)). Our findings thus complement existing work on corporate boards by providing new evidence from top management teams. Our study's focus on multi-dimensional diversity measures and top management teams accords well with the ISS's observation that investors are increasingly concerned with diversity "beyond gender and beyond the boardroom."[4]

Our study is also related to the literature on the role of biased investor expectations on stock market outcomes (e.g., Barberis, Shleifer, and Vishny (1998), Daniel, Hirshleifer, and Subrahmanyam (1998), Greenwood and Shleifer (2014)). Related to our paper, Engelberg, McLean, and Pontiff (2018) study the role of biased cash-flow expectations on stock market anomalies. Engelberg, McLean, and Pontiff (2017) document that analysts have downward-biased expectations about firms in anomaly portfolios, and argue that one channel through which anomalies manifest is investors being influenced by biased analyst expectations. In a similar vein, Bouchaud, Ciliberti, Landier, Simon, and Thesmar (2016) and Asness, Frazzini, and Pedersen (2019) study biased analyst expectations as a source of the returns to "quality" investing. Our paper contributes a new and economically important setting in which analyst and investor biases in expectations manifest in the stock market.

Finally, our paper relates to a literature that shows that investors can be biased against financial assets, and thus impact financial market outcomes, because they are biased against attributes of individuals associated with the financial asset. Dougal, Gao, Mayew, and Parsons (2018) find

[4]Source: http://clsbluesky.law.columbia.edu/2019/01/11/iss-lists-top-10-corporate-governance-topics-to-watch-in-2019/

evidence to suggest that investors are biased against holding bonds issued by historically black colleges and universities, despite no difference in credit quality. Kumar, Niessen-Ruenzi, and Spalt (2015) show that investors are biased against investing into mutual funds managed by individuals with foreign-sounding names, despite no difference in performance. Our paper contributes new evidence to this literature by showing that institutional investors in the stock market are biased against holding firms with diverse top management teams, even though those firms do not deliver lower returns. Even more broadly, our findings are related to the large literature on biases against diversity in other domains including biases in hiring decisions, and biases of judges in courts of law (e.g., Bertrand and Mullainathan (2004), Abrams, Bertrand, and Mullainathan (2012)).

2. Background, Measurement, and Data

2.1 Top Management Team Diversity, Firm Performance, and the Potential for Biased Expectations

The idea that top management teams matter for firm outcomes has a long tradition in the management literature. For example, in their seminal paper, Hambrick and Mason (1984) argue that key to understanding why organizations act or perform the way they do is the analysis of the biases and disposition of their "upper echelon," i.e. their top executives. A central conjecture in Hambrick and Mason (1984) is that an executive's cognitive frame, which determines her values and beliefs, and therefore ultimately her corporate decisions, can be proxied for by observable characteristics. In a review article on the vast upper echelons literature, Hambrick (2007) writes:

> "Given the great difficulty obtaining conventional psychometric data on top executives (especially those who head major firms), researchers can reliably use information on executives' functional backgrounds, industry and firm tenures, educational credentials, and affiliations to develop predictions of strategic actions...researchers have generated substantial evidence that demographic profiles of executives (both individual executives and top management teams) are highly related to strategy and performance outcomes."

Motivated by theories like upper echelon, a substantial body of work analyzes how diverse corporate leadership teams affect corporate decisions (see Section 1.1). The sheer volume of papers written on the subject over several decades suggests that the diversity of top management teams, and their impact on firm performance, is an issue of first-order economic interest. It is therefore not surprising that prominent investors like BlackRock and CalPERS pay attention to diversity. Similarly, given that analysts spend considerable effort on determining the quality of top management teams (e.g., Du Pont Capital (2014), Brown, Call, Clement, and Sharp (2015)), they should naturally pay attention to diversity if it affects teams and therefore firm performance.

Despite a strong belief that managerial attributes in general, and diversity in particular, could potentially influence firm performance, the voluminous literatures in management and finance have so far yielded mixed evidence (e.g., Nielsen (2010), Adams, Haan, Terjesen, and Ees (2015)). While some studies document benefits of diversity (e.g., more creativity in approaching a given problem), others provide evidence consistent with substantial costs associated with it (e.g., more frictions and greater potential for conflict in the team). Hence, whether diversity helps or hurts firm performance is not ex-ante clear and depends on the relative magnitude of its benefits and costs. Consistent with the difficulty of trading-off benefits and costs accurately, studies show that the potential for biases in assessing the performance of diverse teams is particularly large (e.g., Van Dijk, Van Engen, and Van Knippenberg (2012), Lount, Sheldon, Rink, and Phillips (2015))

We argue that the combination of (i) a belief that top management team diversity can plausibly affect firm performance, and (ii) the absence of a consensus about whether, and by how much, diversity helps or hurts firm performance, creates space for analysts and investors to form biased expectations. In particular, putting too much weight on the potential costs of diversity will lead to downward-biased expectations about the performance of a firm with a diverse top management team. Potentially, such overemphasis on the costs of diversity is a contributing factor to the continuing underrepresentation of females and minority groups among directors, top executives, and university professors despite a pledge by firms and universities to increasing diversity in public pronouncements. Also, the pervasiveness and persistence of biases against diversity, i.e., the systematic overestimation of its costs relative to its benefits, may be a motivating factor for policy action, such as the quotas for women on boards which have been introduced in many countries. Against this background, finding that stock markets are biased against diversity should not be entirely surprising.

2.2 Measuring Diversity from Biographical Texts

A main innovation of our study is to propose a new way of measuring top management team diversity, which builds on recent advances in textual analysis in the finance literature.

The core of our data are biographical texts which all listed U.S. firms need to file with the SEC for each executive officer and year, under Regulation S-K of the U.S. Securities Act of 1933. Items 401(b), (c), and (e) require firms to identify each executive officer or other significant (non-director) employee, and report their principal occupations and employment over the past five years plus any material information on relevant business experience and professional competence. The collection of executive officers is what we label the top management team. The following is one example of a text firms provide in response to this SEC requirement. It is from General Electric's 2009 proxy statement and describes the company's CEO at the time, Jeffrey Immelt:

> Mr. Immelt joined GE in corporate marketing in 1982 after receiving a degree in applied mathematics from Dartmouth College and an MBA from Harvard University. He then held a series of leadership positions with GE Plastics in sales, marketing and global product development. He became a vice president of GE in 1989, responsible for consumer services for GE Appliances. He subsequently became vice president of worldwide marketing product management for GE Appliances in 1991, vice president and general manager of GE Plastics Americas commercial division in 1992, and vice president and general manager of GE Plastics Americas in 1993. He became senior vice president of GE and president and chief executive officer of GE Medical Systems in 1996. Mr. Immelt became GEs president and chairman-elect in 2000, and chairman and chief executive officer in 2001. He is a director of the Federal Reserve Bank of New York, a trustee of Dartmouth College, and was recently named a member of President Obamas Economic Recovery Advisory Board.

For each firm, information about each top management team member is provided in filings available in electronic form from the SEC on the EDGAR website. We retrieve these data going back until 1999 (coverage issues and changes in layout requirements dictate our starting year). Diversity in our study is the degree of dissimilarity in the backgrounds of a firm's executive officers, as represented in the biographies reported in the firm's filings. To measure diversity, we rely on cosine similarity, a well-established method widely used in a recent strand of the finance literature (Hanley and Hoberg (2010), Hoberg and Phillips (2016)).

Firms provide biographies either in the annual report, or in the proxy statement. We scan forms 10-K, 10-KSB, and DEF 14A in the SEC EDGAR database for each firm and year. In 10-Ks, the biographies are usually provided in Item 10 or Item 4A. In proxy statements (DEF 14A), which have a less standardized structure, the biographies can often be found in a specific section whose title refers to "Executive Officers" or "Management." We use a Python web-scraping program to collect and process the biographies. We use human intervention whenever the non-standard format of a firm's filing does not allow the program to extract the biographies. With this approach, we obtain a raw sample of 59,863 firm-year observations, consisting of 420,428 executive biography-year observations.

Next, we build the main dictionary. To this end, we take the list of all unique words used in all biographies in year t. Hoberg and Phillips (2016) restrict the attention to words classified as either nouns or proper nouns; in addition to that we also keep adjectives, because words like "international" can carry informational value in our context. Also following Hoberg and Phillips (2016), we exclude words that appear in more than 25% of all biographies in a given year because such words are unlikely to convey meaning (e.g., "company"). The resulting list of N words is the main dictionary and it is represented by a vector of length N. The n-th entry of a biography's N-vector is 0 if the n-th dictionary word is not used in the biography, or x, where x is the number of times the n-th word appears in the biography. The output is, for firm k in year t, a $M \times N$ matrix, where M is the number of executives in the top management team of firm k in year t.

Table B.1 in the appendix illustrates typical words in the biographies by showing the 100 most frequently used words in the "main dictionary" for the year 2011. As is evident from this list, texts relate to many different areas that are plausibly related to similarities between executives, including: industries ("technology", "bank", "engineering"), functional backgrounds ("operations", "marketing", "sales"), job titles ("controller", "treasurer", "CEO"), geography ("international", "global", "California"), education ("degree", "bachelor", "MBA"). Frequent words also cover dimensions of similarity that are potentially relevant, but harder to measure ("leadership", "responsibility", "governance"). Overall, the list highlights an advantage of the text-based approach: we obtain a very detailed, high–dimensional, image of similarities across executives.

Some words in the list also illustrate a potential drawback of text-based methods: measurement error. For example, the most used word in the year 2011 is "position," which is unlikely to signal similarity among executives. The Hoberg and Phillips (2016) 25% filter is designed to delete most of such common words. The word "position" in the year 2011 apparently just missed the 25% cutoff. A word like "position," which is commonly used but likely unrelated to diversity, will noise up our diversity measure, and therefore work against us finding strong effects, but it should not otherwise bias our findings.

For each biographical text associated with executive i, company k, and year t, vector T_{ikt} is a row in the $M \times N$ matrix and describes the biography's word usage. For each pair of executives i, j of company k, in year t, we then define the similarity of two biographical texts as:

$$CS_{ijkt} = \frac{T'_{ikt} T_{jkt}}{\parallel T_{ikt} \parallel \times \parallel T_{jkt} \parallel} = \frac{\sum_{n=1}^{N} T_{nikt} \times T_{njkt}}{\sqrt{\sum_{n=1}^{N_t} T_{nikt}^2} \times \sqrt{\sum_{n=1}^{N_t} T_{njkt}^2}}. \tag{1}$$

CS is the cosine of the angle between T_{ikt} and T_{jkt} in Euclidean space, and is thus bounded between 0 and 1. We then define diversity for a given firm-year as:

$$D_{kt} = 1 - \overline{CS}_{kt}, \tag{2}$$

where $\overline{CS}_{kt}$ is the average of CS_{ijkt} over all $[M \times (M-1)]/2$ executive pairs in firm k in year t. We consider firms with only one reported top executive as maximally homogeneous and set $D = 0$.

To get an intuition for the diversity measure, consider a simple example with only two executives and a word dictionary of only two words "Blue" and "Red." If executive i's biography reads "Blue," her vector T_{ikt} is (1 0). If executive j's biography is also "Blue," then $T_{jkt} = (1\ 0)$ and, using the definition above, $CS_{ijkt} = (1 \times 1 + 0 \times 0) / \left(\sqrt{1} \times \sqrt{1}\right) = 1$. Hence, if executives have identical biographies, $CS = 1$ and, therefore, $D = 0$, i.e., diversity for this top management team is zero. Suppose now that executive j's biography reads "Red." Then, $T_{jkt} = (0\ 1)$ and $CS_{ijkt} = (1 \times 0 + 0 \times 1) / \left(\sqrt{1} \times \sqrt{1}\right) = 0$. It follows that $D = 1$, which means this team of top executives is maximally diverse.

A potential concern with analyzing texts in SEC filings is that the executives may not write the biographies themselves. We do not believe this is a serious limitation in our setting for several reasons. First, irrespective of who writes the bios, the bios will be informative about actual attributes of the executive, because (i) while most executives will not be writing the bios themselves, it is likely that many, if not most, would at least read them, given this is detailed personal information to be widely distributed among investors in a formal document; (ii) the underlying biographical information (e.g., where the executive obtained her MBA or whether she has worked for a given company in the past) does not depend on who writes the biography; (iii) the SEC requires certain items to be part of the bio, so the ability to "cherry-pick" entries is limited. In sum, it is thus a priori unlikely that we would systematically label firms as diverse based on the bios, even though they are actually homogeneous – i.e., text-based diversity should be a good proxy for actual diversity no matter who writes the bios. Supporting this argument, we provide direct evidence showing that text-based diversity is correlated with observable diversity attributes in Section 2.4.

Second, we use a bag-of-words approach. By design, our diversity measure thus reflects the occurrence of words, but does not capture differences in style or syntax. The style of writing per se, therefore, cannot induce our results. Third, differences in text relating to biography length, i.e., the amount of information provided, is not a concern because it is observable and we can control for it.

Finally, any alternative hypothesis based on the idea that the presence of individuals who "write diverse biographies" for a given firm is correlated with some unobserved factor needs to explain what that factor is, why it is picked up by our bag-of-words approach, why it is not captured by our extensive set of controls and fixed effects, why that factor biases analyst expectations, why that effect is attenuated for more experienced analysts, why that unobserved factor induces institutions to shun diverse firms' shares, why this effect is particularly strong in conservative regions, why diverse firms tend to have higher average returns, and why diverse firms have systematically positive returns around information releases. We find it hard to think of a plausible alternative hypothesis along those lines.

2.3 Data

We merge the firm-level diversity measures with the CRSP-Compustat Merged database and drop all firms with missing or negative book value of equity. Our final sample, after searching for biographical texts, and after applying the filters above, has data on 73,692 individual executives, in 6,898 unique firms, and has 38,971 useable firm-years. All dependent variables and control variables are winsorized at the 1^{st} and 99^{th} percentiles. We obtain analyst data from IBES.

Table 1 presents summary statistics. For brevity, all variables are defined in the appendix. In Panel A, we present statistics for the firm-level variables. Panel B presents statistics for analysts. In Panel C, we show time-series averages for various variables of interest when firms are sorted, each year, by their diversity measure. Diverse firms are smaller on average. However, at $2.2 billion of average market capitalization, they are not small in absolute terms. Diverse firms are similar to homogeneous firms in their book-to-market ratio, their previous 12-month stock returns, and institutional ownership. They have slightly lower analysts coverage and earnings volatility, and slightly higher share turnover and number of days between the end of fiscal quarter and the earnings announcement date. Finally, diverse firms have smaller top management teams and longer biographical texts for each executive, but these differences are economically small.

To build intuition about the variation in the diversity measure, consider the stylized example of two executives who have bios with the same number of words. Assuming that each word appears at most once in a bio, a diversity measure of 0.56 (the average for the homogeneous quartile in Panel C) then implies that the two executives have 44% of the words in their bios in common. By contrast, a diversity measure of 0.94 (the average for the diverse quartile in Panel C) implies that only 6% of words overlap.

Figure D.1 in the appendix shows industry-averages of our diversity measure. Overall, the across-industry differences are not very large. The most diverse industries are business equipment and health care, while the most homogeneous are finance and utilities.

2.4 Team-Level Correlates of Text-Based Diversity

We argue that similarities in biographical texts provide meaningful information on similarities between individuals. To bolster this case, we now show that the text-based diversity measure is correlated with a range of observable characteristics of team diversity.

We obtain individual measures from BoardEx, ExecuComp, and the biographical texts themselves. We first construct two employment-related variables (details on the definitions are provided in the appendix): *company overlap* measures for each firm-year the average number of unique company names that appear in the biographies of both executives across all executive pairs, and thus captures commonality in prior work experience; *tenure overlap* captures the period over which executives have been working together in the top management team at the current company. We also include two education-related variables: *university overlap* captures whether executives on the team attended the same universities; and *elite university standard deviation* captures within-team variation on whether executives on the team attended elite universities. Finally, we include three variables on demographic diversity within the top management team: *nationality mix*, *age standard deviation*, and *gender standard deviation*.

Table 1, Panel D, presents correlations between the various individual measures and our text-based diversity measure. As expected, text-based diversity is negatively correlated with *company overlap* and *tenure overlap*, both significant at the 1% level. Also in line with expectations, diversity is lower in teams in which multiple executives are linked to the same universities (significant at the 10% level). We also find that diversity is positively related to *elite university standard deviation*. This is intuitive: mixed teams in which some members went to an elite school, while other members went to lower-ranked schools, are plausibly more diverse than teams in which all or none of their members went to elite institutions (significant at the 1% level).

Among demographic variables, we find that diverse teams are more likely to have members with different nationalities (significant at the 1% level), which accords well with intuition. Also in line with expectations, *age standard deviation* and *gender standard deviation* are positively correlated with text-based diversity (significant at the 5% and 1% level, respectively). Quantitatively, the association of our text-based measure with gender diversity is the most pronounced among the seven variables we consider ($\rho = 0.28$).

A remarkable feature of these tests is that tenure, as well as the demographic variables age, gender, and nationality are not part of the biographical texts we use (following standard approaches as in, e.g., Hoberg and Phillips (2016), we drop numbers, prefixes, and personal pronouns; and the word pair Beijing/New York is, from a textual analysis standpoint, as dissimilar as New York/Boston). Table 1, Panel D therefore shows that diversity measured from biographical texts relates to observable dimensions of diversity even if these categories are not included in the text. We find this to be particularly strong evidence consistent with the view that biographical texts, and therefore our measure of text-based diversity, are able to capture actual diversity among the individuals to which the texts relate.

While it is reassuring to see the text-based top-down measure line up with individual bottom-up measures, it bears emphasizing that a key advantage of the top-down text-based measure is that it can capture information from many dimensions simultaneously – to be precise, it can capture N dimensions, the length of the word dictionary, which is around $55,000$ in the average year in our sample and thus much larger than the number of bottom-up categories a researcher can reasonably pre-specify. The top-down measure therefore has the potential to capture a lot of information which individual bottom-up measures may miss. Because analysts and institutional investors, in particular, can know much more about executives than top line characteristics like age, race and gender, the text-based measure can potentially get one step closer to the degree of diversity that analysts and institutional investors attribute to a team when they try to determine the quality of management at a company they analyze or invest in. If this argument is true, then the text-based measure should be incrementally informative, over and above direct uni-dimensional measures. A second advantage of the text-based approach to measuring diversity is that it aggregates this large number of individual dimensions into one index via equation (2); it is therefore both detailed in the amount of information it captures, and easy to use.

3. Direct Measurement of Biased Expectations From Analyst Forecasts

To investigate if stock markets are biased against diverse top management teams we would ideally like to observe investor expectations on the stock-date level. This kind of information, however, is not available. Therefore, in a first set of tests, we turn to what we believe is the best available substitute: financial analyst forecasts.

Using analyst forecasts has important advantages. First, analyst forecasts are specific for a given stock and date. We can thus obtain a direct measure of expectational biases by comparing forecasts with ex-post realizations. Second, we can observe multiple forecasts for a given analyst and date, which helps identification because we can absorb potentially confounding variation across analysts with high-dimensional fixed effects. Because of these advantages, analyst forecasts are now frequently studied in related work on biased expectations (e.g., Bouchaud, Ciliberti, Landier, Simon, and Thesmar (2016), Asness, Frazzini, and Pedersen (2019), Engelberg, McLean, and Pontiff (2017)).

Beyond providing a comparatively clean gauge of expectational biases, studying analysts is interesting because analysts are central information intermediaries in financial markets: they gather, analyze, and produce information that investors rely on in their pricing and trading decisions (e.g., Kothari, So, and Verdi (2016)). If analysts have biased expectations, these biases may feed into investor decisions, a point to which we return in Section 4.

3.1 Baseline Results

Our baseline test looks at analyst earnings per share (EPS) forecasts from IBES. We mostly follow Hirshleifer, Lim, and Teoh (2009) in the definitions and control variables we use. For each analyst who issued at least one forecast for quarter t EPS, we retain the last forecast. We also obtain the ex-post actual EPS reported by the firm. We normalize both actual and forecast EPS by expressing them as a fraction of the stock price at the end of quarter t. If expectations are unbiased, forecasts should on average match ex-post actuals.

The prior literature documents that analyst forecasts are on average optimistic, a finding often attributed to conflicts of interest, such as analysts' desire to gain access to top management, or the reluctance to jeopardize future investment banking business with a firm covered by a bank's analysts (e.g., Michaely and Womack (1999), Hong and Kubik (2003)). To isolate the effect of diversity on expectations, we therefore base our tests on the *relative* level of forecast errors and ask whether analyst forecasts are, *all else equal*, more pessimistic for diverse than homogeneous firms. This approach of focusing on cross-sectional differences to isolate expectational biases follows prior related work on biased analyst expectations (e.g., Bouchaud, Ciliberti, Landier, Simon, and Thesmar (2016)). Intuitively, we ask whether, holding established patterns in analyst forecasts fixed, the diversity attribute induces incremental pessimism or optimism among analysts.

Our baseline regression is:

$$y_{iat} = \alpha + \beta D_{it} + \gamma' x_{iat} + \varepsilon_{iat}, \tag{3}$$

where y_{iat} is either the ex-post actual, or the forecast, or the difference between the two for stock i and analyst a at time t. The forecast as well as the difference between actual and forecast are unique to a given analyst-firm-quarter combination. D_{it} denotes top management team diversity for firm i at time t. x is a vector of control variables discussed below. All variables are defined in the appendix, and all right-hand-side variables are defined such that they are known to market participants at the time the analyst makes her final forecast.[5] We also include various combinations of fixed effects as described below and we cluster standard errors by date (year-quarter). The coefficient of interest is β, which captures the impact of the diversity attribute.

The results are reported in Table 2. Across all specifications, we find evidence of a downward bias in analyst expectations for diverse firms, relative to homogeneous firms. The first entry in column (1) ("Actual") shows that firms with diverse top management teams have, all else equal, higher earnings per share. The second entry of column (1) ("Forecast") shows, however, that forecasts do not reflect this. At best, they weakly increase in diversity. The third entry in column (1) ("A–F") combines the two previous tests and shows that, compared with what we see for homogeneous firms, analyst forecasts are more pessimistic, relative to ex post actuals, for firms with diverse top management teams.

It is important to note that, because we are focusing on forecast errors, rather than absolute levels of earnings, the results in the last row of Table 2 are not simply driven by diversity affecting what firms do. If forecasters had rational expectations, then higher ex post actual earnings should be associated with higher forecasts, with no effect on the average level of forecast errors. The results in Table 2 are thus indicative of expectational biases.

Further, our results are not simply reflecting known patterns of optimism in analyst forecasts, because our identification comes from comparing forecast errors for more diverse firms with forecast errors for less diverse firms. Hence, any deviation from rational forecasting that comes from, for example, an analyst's incentive to curry favors with the firms she covers, will not impact our results as long as those incentives do not systematically vary with the diversity of a firm's top management team. (We investigate the latter possibility in Section 3.2.6 below, and conclude it is unlikely to be inducing our results.)

The results are robust to a set of control variables which follows Hirshleifer, Lim, and Teoh (2009) and includes the natural logarithm of market capitalization, the log-book-to-market ra-

[5]The only exception is *reporting lag*, defined as the number of days between the end of a given quarter and the earnings announcement date, which may not be known to the analyst at the time she makes her forecast.

tio, the prior 12-month stock return as of the end of the fiscal quarter preceding the earnings announcement, analyst coverage (the natural logarithm of 1 plus the number of analysts covering the stock), reporting lag, reporting lag square and cube, institutional ownership, earnings volatility, earnings persistence, share turnover, as well as the number of earnings forecasts issued by the analyst in the current quarter. None of these variables induce our results.

The nature of the analyst data allows us to eliminate additional confounding variation using fixed effects. In column (2), we add calendar quarter fixed effects and find our results are unaffected. In column (3), we add analyst $\times$ calendar quarter fixed effects. The results show that *the same analyst at the same point in time* is issuing more pessimistic forecasts on diverse firms than other firms. This result is powerful, because it speaks against alternative explanations which relate to, for example, overall analyst skill, status as a top analyst, incentives from pay contracts, employer characteristics, and analyst demographics such as age, gender, and education.

Finally, we add industry $\times$ calendar quarter fixed effects in column (4) to rule out that our results are induced by industry-level drivers even if those drivers are unobservable and time-varying. We use the Fama-French 12 industry classification in these tests. Our results are thus not easily explained by stories which assume that diverse teams cluster in certain sectors at specific points in time. The expectational bias of analysts operates also within industry-dates.[6]

The results are statistically significant with t-statistics well above 3 in all specifications. They are also economically significant. For our most conservative specification in column (4), where we essentially compare two stocks in the same industry in an analyst's portfolio at the same point in time, a one standard-deviation increase in D implies that, for the same ex-post actual EPS, forecasts issued by analysts are 7.5 ($= 0.20 \times 0.378$) basis points lower when expressed as a percentage of share price. To see what this means in dollar terms, consider the following example. The average firm in our sample has a stock price of \$45 and an earnings-to-price ratio of 30 bps, which implies earnings per share of 13.5 cents. For this average firm, the estimates

[6] We cannot include firm fixed effects because the diversity measure is very persistent. Persistence stems from the fact that team level diversity is mainly driven by the identity of individuals on the team, which changes only infrequently. Moreover, even if a top management team changes, the incoming executives may have similar characteristics than the exiting executives, in which case overall diversity does not change much even though there is executive turnover. There is thus little useable variation once we include firm fixed effects.

from column (4) imply that analyst forecasts are 3.4 cents ($= 13.5 \times 7.5/30$) lower if diversity increases by one standard deviation. The downward bias in analyst expectations induced by diversity is therefore economically substantial.

An alternative way to run our regressions would be to collapse the data on the firm-quarter level. A drawback of running our analysis on the firm-quarter level is that we lose the ability to control for characteristics of individual analysts, which we absorb via analyst $\times$ date fixed effects in our baseline specification. In any case, Table C.1 shows that we obtain very similar results when we collapse the data on the firm-quarter level.

In sum, we conclude that analyst earnings forecasts exhibit a bias against firms with diverse top management teams.

3.2 Robustness and Alternative Explanations

In this section, we provide additional robustness tests and discuss potential alternative explanations.

3.2.1 Diversity as a Proxy for Corporate Governance

Better governed firms may have better top management teams, which in turn may be correlated with the diversity attribute. We now show that the bias in forecasts is not due to a correlation with governance strength.

We measure governance strength by the Total Number of Governance Strengths index from the RiskMetrics KLD STATS database. The Total Number of Governance Strengths is an index based on a set of underlying dimensions such as compensation, ownership, and transparency. The index covers individual companies on a yearly basis throughout our sample period.[7] We find that the raw correlation between governance and diversity is close to zero ($\rho = -0.04$), which is a first indication that our results on diversity are unrelated to governance.

[7]The coverage of the index is limited to S&P500 and KLD400 Social Index firms until 2000; following that year, it is progressively expanded, to cover the 3000 largest U.S. companies by market capitalization. For details on the variables in the KLD STATS dataset, please see: `http://cdnete.lib.ncku.edu.tw/93cdnet/english/lib/Getting_Started_With_KLD_STATS.pdf`.

Table 3 reproduces the actual minus forecast results from the last line in Table 2, specification (3). In specification (1) we add governance strength as an additional control variable and find that, while governance itself has no bearing on analyst biases, diversity remains strongly related to forecast errors. (Because we lose almost 20% of our sample due to data availability, both the coefficient on diversity and its statistical significance are not directly comparable to the baseline specification.) In unreported checks, we find very similar results when we reproduce this test using the Bebchuk, Cohen, and Ferrell (2009) E-Index as an alternative governance measure.

3.2.2 Diversity as a Proxy for Workforce Diversity

Top management team diversity may proxy for workforce diversity, which raises the possibility that the analyst bias we uncover is not really about top management teams. To investigate this, we measure firm-wide workforce diversity based on six non-top management related diversity strengths provided in the KLD STATS dataset. Specifically, we form an index by summing over the following 6 diversity strengths: (i) promotion of minorities and women, (ii) work-life benefits at the company, (iii) whether the firm does significant amounts of business with women or minority owned subcontractors or suppliers, (iv) employment of the disabled, (v) gay and lesbian policies, and (vi) other diversity strengths. We reproduce the test of Table 2, specification (3), while additionally controlling for firm-wide diversity, in specification (2) of Table 3. The coefficient on firm-wide diversity is negative and marginally significant; but again, our findings on top management team diversity are qualitatively unaffected. Our baseline results are therefore not driven by a firm's general workforce diversity.

3.2.3 Team Size and Organizational Capital

The size of a top management team correlates only weakly with our diversity measure, as shown in Table 1, Panel C. Nevertheless, to be conservative, we now show that our results are not due to team size. One potential reason why team size may affect analyst estimates is that team size may be related to organizational capital, and thus correlate with exposure to hard-to-forecast employment shocks (e.g., Eisfeldt and Papanikolaou (2013), Boguth, Newton, and Simutin (2016)). We control for team size in specification (4) of Table 3 and we also control for the Eisfeldt and Papanikolaou (2013) organizational capital index directly in specification (3). While both the coefficient on organizational capital as well as the coefficient on team size are significantly different from zero, they do not materially affect the size and significance of the effect of top management team diversity, which shows we are capturing a different effect.

3.2.4 Biography Length

A potential concern could be that text-level diversity is associated with longer texts, and that it is the length of the biographical text that matters, rather than its content. Arguing against this concern, we find in specification (5) of Table 3 that the coefficient on our diversity measure is completely unaffected when we control for biography length, which is itself insignificant.

3.2.5 Diversity, Textual Complexity, and Fundamental Complexity

Top management team diversity could potentially affect analyst forecasts because diverse firms are more complex, and because complexity induces a bias. For example, ambiguity averse analysts may take a more negative view of more complex firms. We now show that our previous results on diversity are not simply reflecting greater complexity.

Prima facie evidence arguing against the idea that diverse firms are harder for analysts to understand because they are inherently more complex comes from Table 1, Panel C. That panel shows that earnings volatility is not higher, but lower, for diverse firms than for homogeneous firms. Thus, if anything, earnings for diverse firms should be *less* difficult to predict by that metric.

To further investigate the issue, we use two approaches to measure the complexity of a firm and its disclosures: fundamental and text-based. The first proxy for fundamental complexity is the prior year's idiosyncratic stock return volatility, computed from daily returns using the Fama-French-Carhart four-factor model. The second one is a Herfindahl-Hirschman-index (HHI) over segment sales obtained from the Compustat Segments file as in Loughran and McDonald (2014).

As a first text-based complexity proxy, following work by Loughran and McDonald (2014), we use the readability of firm disclosures, proxied for by the 10-K file size in the SEC's EDGAR database. A second measure is the number of words in the 10-K as in Li (2008). We also examine the tone of the annual reports to measure complexity. To that end, we rely on two word lists that have recently been developed by Loughran and McDonald (2011) specifically to analyze financial texts such as those in 10-Ks. The first list contains *Uncertain Words*. Uncertain words are words denoting uncertainty, with an emphasis on the general notion of imprecision, for example: "approximate", "depend", "indefinite", or "uncertain." The second list comprises *Weak Modal Words*, i.e., words such as "could", "might", and "possibly." A greater use of uncertain or weak modal words is plausibly associated with information that is more vague and therefore potentially harder to process for investors. We obtain the relevant data from Professor Bill McDonald's website.

Table 4 presents results when we add the various measures of complexity to the baseline test in specification (3), Table 2. Specifications (1)–(7) in Table 4 show that the results on diversity are essentially unaffected in all specifications. We conclude that our baseline effect is not due to greater firm-level complexity, either fundamental or text-based.

One potential concern could be that the previous measures of complexity are based on observables and may therefore not fully control for unobservable aspects of complexity. We therefore complement the above tests with a fixed effects approach in specifications (8) and (9). In column (8) of Table 4, we use the text-based industry classification (TNIC) developed by Hoberg and Phillips (2016), which groups firms using the similarity of text reported in the 10-K product description section. We include TNIC $\times$ date fixed effects to effectively compare firms with similar product descriptions. In column (9), we control for text cluster group $\times$ date fixed effects. For this test, we create 49 text clusters (TCluster) following the approach used in Hoberg and Phillips (2016), but replacing 10-K product descriptions with full 10-K reports. Using either approach, we find that the coefficient on diversity remains largely unaffected.

The tests in specifications (8) and (9) are also informative in two other respects. First, a potential concern could be that the diversity measure captures some aspect of the style of writing of a firm's annual report. We have argued above on conceptual grounds why we do not think this should be a major concern. Our results in specifications (8) and (9) provide some empirical support for this assessment, because these tests effectively compare two firms with different diversity measures, but otherwise similar 10-Ks. Second, it is theoretically possible that diverse firms are different in some unobserved fundamental dimension that we fail to capture in all our other tests. As argued in Hoberg and Phillips (2016), using similarities of texts in 10-Ks can yield industry classifications which more closely match similarity in firm fundamentals than more traditional industry membership measures. The results in specifications (8) and (9) thus further raise the bar for any story that would attribute our findings to differences in firm fundamentals across more diverse and less diverse firms.

We conclude that our results on diversity do not obtain because diversity correlates with the complexity of a firm or its disclosures.

3.2.6 Diversity and Analyst Conflicts of Interest

A potential alternative explanation for the bias against diverse firms we document could be that conflicts of interest are related to diversity in a systematic way. Specifically, we may observe more pessimistic forecasts for diverse firms if analyst conflicts of interest, and therefore the incentive to issue optimistic forecasts, are stronger for homogeneous firms.

Our starting point for investigating this issue is the recent survey article by Kothari, So, and Verdi (2016) who summarize the main findings on the relation between analyst bias and conflicts of interest from the existing literature. They highlight two channels relevant to our study. First, prior work proposes that analyst optimism is greater in firms with less earnings predictability because analysts have an incentive to issue favorable recommendations to ensure access to management in these firms. Second, analysts with an investment banking affiliation are found to be systematically overoptimistic relative to non-affiliated ones, potentially because negative forecasts may jeopardize future investment banking business.

Our results are not induced by the earnings predictability channel. The summary statistics in Table 1, Panel C, show that diverse and homogeneous firms do not differ significantly in earnings persistence. If anything, diverse firms have lower earnings persistence, which would predict more optimistic forecasts for diverse firms and is thus the opposite from what we find. More importantly, we use earnings persistence as a control variable in all our tests, so variation on that dimension cannot explain the difference between diverse and homogeneous firms we document. Beyond earnings persistence, our tests also control for several other measures that plausibly correlate with the importance of private information for a firm, including earnings volatility, firm size, book-to-market, firm volatility, and the full slate of complexity measures in Table 4, which ensures these variables are not spuriously inducing our results.

Our results are also not due to analysts with investment banking affiliations because the analyst $\times$ date fixed effects control for investment banking affiliation status in each period.

In sum, our results are not induced by the drivers of cross-sectional variation in analyst biases from conflicts of interest identified by Kothari, So, and Verdi (2016).[8] We conclude that it is unlikely that our results are due to a systematic relation between diversity and analyst conflicts of interest.

3.2.7 Executive Turnover

Our results are not driven by firms who have recently changed the composition of their top management team. When we drop firms that had an executive turnover event in any of the previous four quarters in Table C.2, our results are essentially unchanged. This speaks against firms strategically adding diversity to their teams ahead of good news, and it speaks against concerns that our results are driven by higher values of diversity brought about by recent changes

[8] The one remaining channel that is discussed by Kothari, So, and Verdi (2016) is that personal connections between managers and analysts may drive cross-sectional variation in the strength of biases across analysts. For this channel to induce our results one would have to assume that analysts are more likely to be personally connected to firms with homogeneous top management teams conditional on all the control variables we use. It is not obvious why that would be the case, so we do not pursue this channel further.

in the strategic direction of a given firm.

3.2.8 Target Prices

The finding that analysts have downward-biased expectations on diverse firms is not specific to earnings forecasts. To show this, we compute for each stock i and month m the one-year ahead expected return implied by analysts' target price forecasts, and we compare it with the corresponding ex-post realized return. The number of observations in this test is lower, because target price forecasts are less common than earnings forecasts. Table C.3 in the appendix shows that we obtain very similar results when we use one-year ahead target price forecasts instead of earnings forecasts. The downward bias of analyst expectations is also present for target price forecasts.

3.2.9 Other Tests

Our results remain unchanged when we control for firm age. They remain similarly unchanged when we control for the location of the firm by including headquarter state $\times$ date fixed effects. These results are not reported for brevity, but available on request.

3.3 Text-Based vs. Traditional Measures of Diversity

The previous results show that diversity, measured using our text-based approach, relates to biases in analyst forecasts. In this section, we ask whether text-based diversity has incremental power to explain biases once we control for a range of more traditional observable measures of diversity.

To investigate this, we rerun our baseline regression from Table 2, specification (3), while controlling for the alternative diversity measures used in Table 1, Panel D. The results are reported in Table 5. The core finding is that text-based diversity has explanatory power over and above the uni-dimensional measures as well as a linear combination of these measures. (The fact that we lose up to more than 30% of our sample due to data availability means coefficients and t-statistics are not comparable across specifications.)

By contrast, while the results for text-based diversity are strongly consistent across specifications, the results for the uni-dimensional measures are more mixed. While some variables, like gender, appear to be significantly related to forecast errors in a way that is consistent with gender biases, other variables, such as nationality mix show the opposite sign, while yet other variables, like within-team age variation, seem unrelated to forecast errors. If we combine the seven observable measures into one index by means of their first principal component and then run a horse-race between that variable and text-based diversity, text-based diversity is strongly related to forecast errors, while the principal component has no incremental explanatory power.

One interpretation of this evidence is that the text-based measure is empirically more powerful than widely-used observable measures. This is consistent with the view that a text-based top-down measure can capture many facets of diversity that simple uni-dimensional bottom-up measures may miss. It is also consistent with the idea that, by aggregating a very large number of individual dimensions into one index, the text-based measure has a much higher signal-to-noise ratio than any of the underlying dimensions on their own.

The above results highlight a common difficulty for researchers when working with traditional uni-dimensional measures of diversity. In the absence of an established "gold standard" in the literature, there are many degrees of freedom with respect to which diversity characteristics to include, and also with respect to the most appropriate definitions. For example, should educational diversity be measured using the type of degrees obtained? Or by the subjects majored in? Or by the name of the universities attended? Or by the tier of the university attended? Or a combination of the above? If so, which combination? Including only one dimension raises the concern that results obtain because of an omitted correlated dimension. On the other hand, once a researcher includes many dimensions, as we do in Table 5, conflicting results across measures may dilute what we can learn from the exercise.

The text-based approach offers a structured and disciplined way to overcome the dimensionality problem in diversity research. While it can capture many dimensions of diversity simultaneously, it is, at the same time, very easy to use. And because cosine similarities provide a structured way of condensing the wealth of information from detailed individual dimensions into one index, the approach substantially reduces the degrees of freedom researchers have in measuring diversity, thus inducing some research discipline.

While we believe these advantages are considerable, we emphasize that the text-based approach is no panacea for two reasons. First, researchers who want to use it need to accept its main premise, which is that similar texts are an informative signal about the similarity between individuals. Second, while text-based diversity may provide an empirically powerful summary measure of diversity, it does not lend itself easily to identifying the most important individual driver of diversity. Nevertheless, to us, the combined results in this paper, and the new insights on the impact of diversity on firms we can obtain, suggest that text-based diversity is a valuable complement to more traditional measures of diversity.

3.4 The Impact of Analyst Experience

If the previous results are due to analysts systematically underweighting the net benefits of a diverse top management team, a reasonable conjecture is that analysts may learn over time that their forecasts are too pessimistic relative to otherwise similar firms. More specifically, if the gap between diverse and homogeneous firms is due to a bias, and if learning reduces the bias, then the difference in forecast errors between diverse and homogeneous firms should shrink as analysts become more experienced. Finding such an effect in the data would cast doubt on any alternative explanation for our baseline results in which differences in forecast errors between diverse and homogeneous firms reflect some stable, potentially unobserved, differences between these firms which happen to be correlated with the diversity characteristic.

Following Bouchaud, Ciliberti, Landier, Simon, and Thesmar (2016), we measure analyst experience as the number of months since an analyst in the IBES database starts following a given firm. Table 1, Panel B shows that the average analyst in our data follows 4.5 firms in 1.2 different industries. She has an overall career experience of about nine years, but only slightly over 3 years with any given firm.

We sort analysts into quartiles based on their firm-level experience, and then compare forecast errors for analysts with high (top quartile) and low (bottom quartile) experience. The full model we estimate is:

$$y_{iakt} = \alpha_{atE} + \alpha_{ktE} + \beta_1 D_{it} + \beta_2 D_{it} \times E_{iat} + \gamma_1' x_{it} + \gamma_2' x_{it} \times E_{iat} + \varepsilon_{iakt}, \tag{4}$$

where y_{iakt} is the difference between ex-post actual earnings and earnings forecast for stock i and analyst a at time t, and where k refers to the industry associated with firm i. E_{iat} is an experience group indicator for whether analyst a is in the top quartile of firm-level experience. In this test, we compare only top and bottom quartiles and drop quartiles 2 and 3 of firm-level experience. We include analyst $\times$ date $\times$ experience group and industry $\times$ date $\times$ experience group fixed effects. In this regression, β_1 captures the impact of diversity for the low experience subsample, $\beta_1 + \beta_2$ captures the impact for the high experience subsample, and β_2 captures the difference between the two.

The results are reported in Table 6. They indicate that analysts are less pessimistic on diverse firms for stocks in which they have experience, and that the gap between homogeneous and diverse firms shrinks with experience. Note that the inclusion of analyst $\times$ date fixed effects means the results are driven by variation in experience across stocks within an analyst's portfolio at a given point in time. Any stable factor at the analyst-date level, such as analyst skill, which may be systematically higher for more experienced analysts due to selection effects, cannot induce our findings.

We interpret these results as powerful evidence that diversity affects forecast errors because analysts are forming biased expectations. The results are consistent with analysts, and in particular inexperienced analysts, systematically underweighting the net benefits of diverse top management teams relative to more homogeneous teams. The results also suggest that learning may help reduce the bias.

4. Are Investors Biased Against Diverse Top Management Teams?

In this section, we provide evidence that the bias against diversity in top management teams we document for analysts is also present among a substantial fraction of investors in the financial markets.

There are two reasons to expect that this may be the case. First, investors may respond to diversity the same way as analysts. For instance, buy-side analysts may interpret information on top management teams in the same way as the sell-side analysts that were the focus of our tests in the previous sections. Second, investors may simply follow analysts, in which case they would inherit some of the analysts' bias.

4.1 Does Diversity Affect Institutional Investors' Equity Holdings?

We start by examining the holdings of institutional investors and ask whether top management team diversity matters for the probability that a given stock is held by an investor. To that end, we obtain data on the quarterly equity holdings of all institutional investors in the Thomson 13f database over our sample period. The full model we estimate is:

$$I(Hold)_{ijkt} = \alpha_{jt} + \alpha_{kt} + \beta D_{it} + \gamma' x_{it} + \varepsilon_{ijkt}, \tag{5}$$

where $I(Hold)_{ijkt}$ is an indicator for whether institutional investor j holds stock i in period t, and where k refers to the industry associated with firm i. The vector x represents a standard set of control variables (e.g., Bennett, Sias, and Starks (2003)), and includes firm i's market capitalization, its book-to-market ratio, past returns, idiosyncratic volatility, share turnover, and payout, all measured as of the beginning of period t, as well as industry $\times$ date and investor $\times$ date fixed effects.

Table 7 presents results. The key finding is that institutional investors are less likely to hold a stock when the top management team is more diverse. Because we include investor $\times$ date fixed effects, the results in Table 7 show that the *same investor at the same point in time* is more likely to invest into a homogeneous firm than a diverse firm. The industry $\times$ date fixed effects ensure that the results are not driven by any variable that does not vary within industry-dates, such as for example industry returns, or the state of the industry business cycle.

These findings are consistent with the view that institutional investors, just like analysts, have downward-biased expectations about firms with more diverse management teams, or alternatively, that analyst biases feed into institutional investors' decisions.

A potential alternative explanation is that institutions are sophisticated investors, who realize that diverse firms have particularly low expected returns. We directly investigate returns in Table 8. In Panel A, we present monthly value-weighted sorting results, and Panel B presents monthly Fama-McBeth regressions, with and without value-weighting. All these tests show that diverse firms do not have lower returns – the results in Table 8 indicate, if anything, the opposite. Hence, institutions are shunning diverse firms not *because of lower* returns, but *despite higher* returns. Finding that institutional investors in the stock market are on average biased against diversity is in line with similar findings for investors in the bond market, retail investors in mutual funds, and early stage investors, which have all been suggested to reflect investor biases against various measures of diversity (e.g., Kumar, Niessen-Ruenzi, and Spalt (2015) on foreign-sounding names; Ewens and Townsend (2017) and Gompers and Wang (2017) on gender; Dougal, Gao, Mayew, and Parsons (2018) on race).

To further support the biased expectations interpretation, we want to exploit plausibly exogenous variation in how much an investor is predisposed to disliking diversity. Intuitively, if our previous results are due to biases against diversity, we expect the tendency to shun diverse firms to be attenuated for institutions whose managers are less likely to have an unfavorable view on diversity. We implement this idea by exploiting the geographical dispersion of institutional investors and variation in attitudes to diversity across regions. Specifically, we conjecture that diversity has a more positive connotation in states with greater minority populations, and in states with more votes for the Democratic party (as diversity tends to be endorsed more by the political left). Consistent with the latter conjecture, a recent poll by the Pew Research Center shows that 70% of Democrats have a positive view of growing diversity in the U.S. compared with only 47% for Republicans (Fingerhut (2018)). Consistent with the former conjecture, a 2017 survey by the Washington Post-Kaiser Family Foundation finds that sentiment against immigrants by white Americans is greater in areas with smaller minority populations (e.g. Sacchetti and Guskin (2017)). As an additional measure, we use the racial animus index used in Dougal, Gao, Mayew, and Parsons (2018).

Table 9 presents results when we compare the bias induced by diversity across regions. In the first test, we measure political attitudes by whether Democrats or Republicans won the majority of votes in the last Presidential election. In the second test, we sort states into quartiles by the percentage of minority inhabitants in each year, and then compare institutions located in states in the top and bottom quartiles, respectively. In the third test we sort states by the Dougal, Gao, Mayew, and Parsons (2018) racial animus index. We estimate:

$$I(Hold)_{ijkt} = \alpha_{jtS} + \alpha_{ktS} + \beta_1 D_{it} + \beta_2 D_{it} \times S_{jt} + \gamma_1' x_{it} + \gamma_2' x_{it} \times S_{jt} + \varepsilon_{ijkt}, \tag{6}$$

where S_{jt} is a regional indicator for whether institutional investor j is located in a state which is in the top quartile of minority inhabitants, the racial animus index, or which votes for the Democratic party, respectively. For the minority and racial animus index tests, we compare only top and bottom quartiles and drop quartile 2 and 3. We include investor $\times$ date $\times$ regional indicator and industry $\times$ date $\times$ regional indicator fixed effects, which should absorb a lot of potentially confounding variation. In this regression, β_1 captures the impact of diversity for the Republican/Low Minority subsample, $\beta_1 + \beta_2$ captures the impact for the Democrat/High Minority subsample, and β_2 captures the difference between the two. Across all specifications in Table 9, the results indicate that investors located in areas where diversity is more likely to have a negative connotation are less likely to hold diverse stocks.

A potential question on the above results may be whether they are driven by institutions investing into local firms. They are not. Table C.4 in the appendix shows that we obtain virtually unchanged results when we drop investments into firms with headquarters in the same state as the institutional investor.

In sum, the patterns we document in this section are consistent with the view that institutional investors are biased against firms with diverse top management teams, just like financial analysts. Notice that our results do not rule out the possibility that institutional investors shun diverse stocks because they cater to customers who dislike diverse firms. In either case, the results reflect a bias against diverse firms in the stock market.

4.2 Evidence from Firm-Specific Information Releases

The previous tests are consistent with institutional investors being biased against firms with diverse top management teams. The purpose of this section is to provide evidence for a bias against diverse firms in the stock market more broadly.

To establish such a bias, and to provide direct evidence of downward-biased expectations on diverse firms, we follow an approach recently proposed by Engelberg, McLean, and Pontiff (2018). Those authors argue that stock returns on earnings announcements and other corporate news days offer a clean setting to detect stock market biases. The key idea in this test is that systematic and economically large swings in the day-to-day return differences between stocks around information release days are unlikely driven by fundamental risk (i.e. changes in discount rates), because most risk factors are unlikely to show large systematic day-to-day swings. Rather, systematic and therefore predictable changes in return differences around information-release days are indicative of mispricing.

Engelberg, McLean, and Pontiff (2018) propose, and we adopt, a specific version of a mispricing model, motivated by the literature on biased investor expectations and stock market anomalies (e.g., Barberis, Shleifer, and Vishny (1998), Daniel, Hirshleifer, and Subrahmanyam (1998)). In the model, investors have downward-biased expectations on a stock, i.e. they are too pessimistic about the fundamental performance of the firm. Upon information releases, such as earnings announcements, investors partly correct their mistake, thus inducing higher stock returns precisely on days in which a firm releases information. They show that this view is consistent with return patterns for a large set of documented anomalies from the recent finance literature. Our goal here is to see whether the returns on diverse stocks follow the same pattern, which would indicate that biased expectations are a driver of those results. Intuitively, investors who are systematically too pessimistic on diverse firms will be systematically positively surprised about results reported by those firms, and returns on information release days will then be particularly high for diverse firms.

We follow Engelberg, McLean, and Pontiff (2018) and estimate:

$$R_{it} = \alpha_t + \beta_1 D_{it} + \beta_2 Eday_{it} + \beta_3 D_{it} \times Eday_{it} + \gamma' x_{it} + \varepsilon_{it}, \tag{7}$$

where R_{it} is the return of stock i on day t, D_{it} is the diversity score for firm i, $Eday_{it}$ is an indicator variable equal to one if firm i announces earnings on day t, x_{it} is a vector of controls which mimics the one we use in our main Table 2, and α_t is a calendar-day fixed effect, absorbing any day-specific variation such as e.g. macroeconomic shocks or day-of-the-week effects.

The coefficient of interest is β_3, which measures whether return differences vary systematically with diversity around announcement days. As Engelberg, McLean, and Pontiff (2018) explain, rational expectations predict $\beta_3 = 0$, because "in the rational expectations framework, return-predictability is explained by ex-ante differences in discount rates, which should not change in a predictable manner on firm-specific information days." By contrast, $\beta_3 \neq 0$ indicates mispricing.[9]

Table 10, specification (1), presents results. The central result is that the coefficient on the interaction term β_3 is over 10 times bigger than the baseline coefficient β_1, indicating that more diverse firms outperform more homogeneous firms strongly on, and particularly on, earnings announcement days. At 5 bps (assuming a one standard deviation change in diversity), and with a t-statistic of 3.14, the incremental effect is both statistically and economically large. This

[9] Engelberg, McLean, and Pontiff (2018) examine in detail two alternative possibilities for $\beta_3 \neq 0$: (i) rationally higher correlations with the market on earnings announcement days, and (ii) data mining, i.e. a mechanical effect by which outperforming companies are those that have positive news in a given period. While the joint hypothesis problem makes it impossible to completely rule out these alternative stories, the evidence in Engelberg, McLean, and Pontiff (2018) suggests that they explain at best a part of the higher returns on earnings announcement days.

large difference constitutes strong support for a mispricing explanation, since rational risk premia would need to vary systematically and by an extremely large magnitude to explain the results. For example, using the CAPM, and assuming a risk-free rate of 1%, a market risk premium of 5%, 250 trading days, and a beta of 1 for homogeneous firms, the beta of diverse firms should increase from a level of 2 on non-announcement days to about 14 on announcement days to be consistent with the above results. And right after the announcement, the beta of diverse stocks would need to decrease again to its original level of 2. Such extreme day-to-day swings in risk premia appear implausible. Specification (2) shows that results are effectively unchanged when we replace day fixed effects by industry × day fixed effects.

While earnings days may be particularly informative, the underlying logic – that priors are updated on information release days – should apply also to other firm-specific news days. Specifications (3) and (4) thus replace the earnings announcement indicator in the above regression by an indicator variable for firm-specific news days, $Nday$. We define firm-specific news days as days for which we can find any news items linked to the firm in the Ravenpack dataset in which the company plays an important role in the main context of the story (as defined by Ravenpack). We find that the return difference between diverse and homogeneous firms is significantly higher on news days, which is again consistent with mispricing.

Because the news items used to construct $Nday$ include news about earnings announcements, specifications (5) and (6) include both the $Eday$ and $Nday$ variables and their interactions with diversity. The results are similar to specifications (1) to (4). Notably, there is no longer a baseline effect from diversity, which indicates that most of the returns on diverse stocks relative to homogeneous stocks are "earned" on information release days.

In sum, the significant interaction terms in Table 10 show that diverse firms exhibit, all else equal, *predictably* higher returns on information release days. That is hard to square with rational expectations. Instead, the results are consistent with biased expectations of investors as emphasized in Engelberg, McLean, and Pontiff (2018). In our setting, this means that diversity returns are due to investors being too pessimistic on firms with diverse top management teams, consistent with the direct evidence we presented for analysts in Section 3.1 and the evidence on institutional holdings in the previous section.

5. Conclusion

Is the market biased against diverse top management teams? The combined evidence in this paper suggests that the answer is "yes" for some of the most influential players in the stock market. We find that financial analysts systematically underestimate the earnings and future returns of firms with diverse top management teams relative to firms with homogeneous teams. We find that institutional investors are less likely to hold stocks of firms with diverse top management teams, even though these firms do not earn lower returns. And we find that the tendency to shun stocks with diverse top management teams is stronger for investors located in more conservative areas.

Our results indicate that market participants hold excessively pessimistic views on firms with diverse top management teams relative to firms with homogeneous teams. Consistent with this downward-biased expectations view, we show that, when new information arrives on the market, stock returns of firms with more diverse top management teams are systematically higher than stock returns of otherwise similar firms with homogeneous top management teams. Thus, firms with more diverse top management teams seem to release information which *systematically* surprises the market positively.

Our main methodological contribution is that we measure diversity from within-team similarities in biographical texts which firms are required to file with the SEC for each top executive. Using this new approach, we assemble a dataset which covers a total of more than 70,000 executives in over 6,500 firms from 1999 to 2014. We show that our new text-based measure, which can simultaneously capture many dimensions of diversity, is empirically powerful. Our text-based approach complements, and overcomes some of the challenges associated with, more traditional uni-dimensional measures of diversity.

Our main conceptual contribution is to highlight that stock markets react not only to what firms with diverse leadership teams do, but also to the diversity of the leadership teams itself. An important implication for much of the related literature is that researchers need to be careful when using stock market valuations to draw inferences on the quality of what firms with diverse top management do – perceptions of diversity can matter for stock market outcomes even in the absence of a difference in corporate actions. Unless perception effects are properly controlled for, our results imply that drawing inferences on corporate actions from stock market valuations is problematic.

Finally, our findings suggest that both analysts and investors overweight the potential costs of having a more diverse top management team, relative to the potential benefits. A valuable avenue for future research could be to take a closer look at the nature of these costs and the deeper drivers for why these cost items are overweighted in the minds of important stock market participants.

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Table 1: Descriptive Statistics

This table presents summary statistics. Panel A shows statistics for the main firm-level variables used in our analyses. Panel B shows statistics for analyst-level variables. In this panel, industries are based on the Fama-French 12-industry definition. Panel C shows summary statistics by diversity quartiles. The last column of this panel reports the t-statistic for the difference between diverse and homogeneous, based on standard errors clustered by firm. Panel D shows correlation coefficients between Diversity and the set of team-level variables used in Table 5. In this panel, a, b and c denote significance at the 1%, 5% and 10% levels, respectively. Definitions of all variables are provided in the appendix.

Panel A: Firm-Level Statistics

	Avg. N	Mean	St.Dev	25%	50%	75%
Diversity	2,211	0.80	0.20	0.78	0.86	0.91
Size (B$)	2,211	4.00	9.34	0.21	0.76	2.71
Book-to-Market	2,211	0.72	0.99	0.30	0.51	0.84
Return$_{m-12\rightarrow m-2}$	2,211	0.14	0.54	-0.18	0.07	0.34
Analyst Following	2,211	9.36	7.55	4.00	7.00	13.00
Reporting Lag (days)	2,211	48.04	74.26	24.00	31.00	40.00
Institutional Ownership	2,211	0.60	0.29	0.39	0.66	0.83
Earnings Persistence	2,211	0.34	0.40	0.04	0.35	0.65
Earnings Volatility	2,211	0.26	0.35	0.09	0.16	0.29
Share Turnover	2,211	1.79	1.58	0.71	1.35	2.35
Team Size	2,211	8.00	4.62	5.00	7.00	10.00
Net Biography Length (words)	2,211	42.40	24.47	24.33	38.13	54.17

Panel B: Analyst-Level Statistics

	Avg. N	Mean	St.Dev	25%	50%	75%
Firms Followed	18,523	4.49	7.10	2.00	2.00	4.00
Industries Followed	18,523	1.16	0.55	1.00	1.00	1.00
Experience (months)	18,523	108.43	82.59	40.00	91.00	162.00
Firm Experience (months)	18,523	38.68	45.49	7.00	23.00	54.00

Panel C: Summary Statistics by Diversity Quartiles

	Homogeneous	2	3	Diverse	t-stat
Diversity	0.56	0.83	0.89	0.94	
Size (B$)	6.09	4.47	3.08	2.23	9.88
Book-to-Market	0.74	0.69	0.69	0.74	0.61
Return$_{m-12 \rightarrow m-2}$	0.13	0.14	0.14	0.14	1.01
Analyst Following	9.97	9.96	9.08	8.20	7.11
Reporting Lag (days)	40.95	48.52	50.00	53.20	10.04
Institutional Ownership	0.58	0.66	0.62	0.59	1.32
Earnings Persistence	0.34	0.34	0.34	0.32	1.71
Earnings Volatility	0.29	0.26	0.25	0.25	3.46
Share Turnover	1.51	1.88	1.98	1.80	7.63
Team Size	7.76	8.53	8.28	7.33	2.98
Net Biography Length (words)	37.67	45.44	46.40	39.74	3.32

Panel D: Correlations between Team-Level Characteristics

Team Variable	Diversity	(1)	(2)	(3)	(4)	(5)	(6)
Employment-Related							
(1) Company Overlap	-0.098^a						
(2) Tenure Overlap	-0.018^a	-0.010^c					
Education-Related							
(3) University Overlap	-0.003^c	0.144^a	0.059^a				
(4) Elite University St. Dev	0.112^a	0.034^a	0.028^a	0.179^a			
Demographic							
(5) Nationality Mix	0.016^a	0.028^a	-0.039^a	0.036^a	0.053^a		
(6) Executive Age St. Dev.	0.012^b	-0.038^a	0.016^b	-0.015^b	0.007	-0.052^a	
(7) Gender St. Dev.	0.281^a	-0.045^a	-0.002	-0.000	0.130^a	0.010^b	-0.000

Table 2: Diversity and Analyst Earnings Forecasts

This table relates top management team diversity to analyst earnings forecasts. In the first row, the dependent variable is Actual, defined as announced quarterly earnings as reported by IBES, divided by the stock price at the end of the last quarter before the announcement. In the second row, the dependent variable is Forecast, computed as the 1- or 2-quarter-ahead forecast issued by an analyst covering the firm as reported by IBES, divided by the stock price at the end of the last quarter before the announcement. In the last row, the dependent variable is A − F, the difference between Actual and Forecast. For all rows, the table shows the coefficient from regressing the dependent variable on diversity. Controls include log of market capitalization, log book-to-market ratio, return$_{m-12\rightarrow m-2}$, log (1 + analyst following), reporting lag, reporting lag squared, reporting lag cubed, institutional ownership, earnings volatility, earnings persistence, share turnover, and the number of firms followed by the analyst. Date FE are based on year-quarter dates. Industries are based on the Fama-French 12-industry definition. t-statistics are based on standard errors clustered by date (year-quarter), and are shown in parentheses.

	(1)	(2)	(3)	(4)
Actual	0.565	0.535	0.476	0.401
	(2.56)	(2.51)	(3.03)	(2.56)
Forecast	−0.060	−0.071	0.057	0.023
	(−0.57)	(−0.64)	(0.63)	(0.25)
A − F	0.625	0.606	0.419	0.378
	(4.35)	(4.52)	(4.13)	(3.84)
Controls	Yes	Yes	Yes	Yes
Date FE	No	Yes	No	No
Analyst × Date FE	No	No	Yes	Yes
Industry × Date FE	No	No	No	Yes
Observations	1,029,159	1,029,159	1,029,159	1,029,159

Table 3: Robustness

In this table we regress the measure of analyst error (A − F) on diversity and other firm-level characteristics. Controls are those from Table 2. Date FE are based on year-quarter dates. t-statistics are based on standard errors clustered by date (year-quarter), and are shown in parentheses. Definitions of all variables are provided in the appendix.

	(1)	(2)	(3)	(4)	(5)	(6)
Diversity	0.305	0.296	0.388	0.466	0.415	0.320
	(3.01)	(2.95)	(3.94)	(4.46)	(4.02)	(3.01)
Governance Strengths	0.026					0.031
	(0.47)					(0.57)
Workforce Diversity		-0.090				-0.078
		(-1.86)				(-1.59)
Organizational Capital			-1.970			-0.723
			(-5.89)			(-1.89)
Team Size				-0.020		-0.009
				(-5.18)		(-3.25)
Bio Length					0.001	-0.001
					(0.57)	(-0.46)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	841,163	854,172	1,029,056	1,029,159	1,029,159	841,163
R^2	0.24	0.24	0.23	0.23	0.23	0.24

Table 4: Diversity and Complexity

In this table we regress the measure of analyst error (A − F) on diversity and various measures of firm complexity. In columns (1) through (7), we add explicit controls for text-based and fundamental complexity. In column (8), we control for the text-based industries (TNIC) of Hoberg and Phillips (2016). In the last column, we include text cluster-date FE, where text clusters (TCluster) are computed as in Hoberg and Phillips (2016), but replacing product descriptions with 10-K reports. Controls are those from Table 2. Date FE are based on year-quarter dates. *t*-statistics are based on standard errors clustered by date (year-quarter), and are shown in parentheses. Definitions of all variables are provided in the appendix.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Diversity	0.498	0.495	0.397	0.394	0.393	0.392	0.468	0.448	0.406
	(4.42)	(4.59)	(3.91)	(3.89)	(3.86)	(3.88)	(4.14)	(4.29)	(3.97)
Idiosyncratic Vol.	-1.717						-1.507		
	(-7.07)						(-8.09)		
Business HHI		-0.120					-0.114		
		(-2.33)					(-2.24)		
10-K File Size			-0.172				-0.091		
			(-4.82)				(-2.97)		
10-K Word Count				-0.069			0.065		
				(-1.84)			(1.98)		
Uncertain Words					-0.671		-2.167		
					(-1.15)		(-2.31)		
Weak Modal Words						-1.014	7.266		
						(-0.86)	(4.00)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
TNIC × Date FE	No	No	No	No	No	No	No	Yes	No
TCluster × Date FE	No	No	No	No	No	No	No	No	Yes
Observations	1,029,159	838,217	1,009,661	1,009,685	1,009,685	1,009,685	821,060	978,725	991,735
R^2	0.25	0.25	0.23	0.23	0.23	0.23	0.26	0.25	0.25

Table 5: Text-Based Diversity and Team-Level Observables

In this table we regress the measure of analyst error (A − F) on diversity and other team-level observable characteristics. Controls are those from Table 2. In the last column, we add PC1 Team Observables, which is the first principal component of the 7 alternative diversity measures we use in this table. Date FE are based on year-quarter dates. t-statistics are based on standard errors clustered by date (year-quarter), and are shown in parentheses. Definitions of all variables are provided in the appendix.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Diversity	0.390	0.381	0.386	0.385	0.376	0.229	0.338	0.277	0.279
	(3.97)	(3.84)	(3.98)	(4.01)	(3.82)	(2.68)	(3.41)	(2.54)	(2.63)
Company Overlap	0.003							0.207	
	(0.03)							(3.37)	
Tenure Overlap		0.022						0.011	
		(4.34)						(1.74)	
University Overlap			-0.664					-0.313	
			(-2.10)					(-0.61)	
Elite University St. Dev				0.102				0.176	
				(0.93)				(1.81)	
Nationality Mix					-0.136			-0.061	
					(-2.91)			(-1.64)	
Executive Age St. Dev.						-0.003		-0.004	
						(-0.86)		(-0.96)	
Gender St. Dev.							0.219	0.144	
							(3.00)	(1.65)	
PC1 Team Observables									0.019
									(0.87)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Analyst × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	965,164	822,123	965,164	965,164	821,904	883,680	966,747	690,984	690,984
R^2	0.24	0.24	0.24	0.24	0.24	0.24	0.23	0.25	0.25

Table 6: The Impact of Analyst Experience

This table relates top management team diversity to analyst earnings forecasts for different levels of analyst experience. The dependent variable is A − F, computed as in Table 2. We split the sample based on firm-level analyst experience, defined as the number of months since the analyst first started to follow the firm. We report values of A − F for analysts in the bottom quartile of experience (Inexperienced), top quartile of experience (Experienced), and the difference between the two. Controls are the same as in Table 2. t-statistics are based on standard errors clustered by date (year-quarter), and are shown in parentheses.

	(1)	(2)	(3)	(4)
Inexperienced	0.814	0.819	0.526	0.500
	(6.00)	(6.33)	(4.25)	(4.12)
Experienced	0.485	0.443	0.244	0.211
	(2.78)	(2.77)	(2.20)	(1.95)
Difference	0.330	0.376	0.282	0.289
	(2.32)	(2.76)	(2.23)	(2.25)
Controls	Yes	Yes	Yes	Yes
Date FE	No	Yes	No	No
Analyst × Date FE	No	No	Yes	Yes
Industry × Date FE	No	No	No	Yes
Observations	1,029,159	1,029,159	1,029,159	1,029,159
R^2	0.13	0.15	0.24	0.25

Table 7: Diversity and Institutional Investors' Portfolio Choices

This table presents results of regressions of the probability that a stock is in the portfolio of an institutional investor on diversity and control variables. The dependent variable is coded as 1 when a stock appears in the portfolio of an investor in a given quarter, and 0 otherwise. All control variables are defined in the appendix. Date FE are based on year-quarter dates. Industries are based on the Fama-French 12-industry definition. t-statistics are based on standard errors clustered by date (year-quarter), and are shown in parentheses.

	(1)	(2)	(3)	(4)
Diversity	−0.044	−0.017	−0.018	−0.021
	(−9.35)	(−19.01)	(−17.69)	(−20.21)
Market Capitalization		0.030	0.037	0.037
		(53.34)	(55.48)	(58.93)
Book-to-Market		0.001	0.001	0.003
		(1.48)	(3.33)	(9.21)
$\text{Return}_{m-12\rightarrow m-2}$		−0.001	0.001	0.000
		(−0.02)	(0.86)	(0.04)
Return_{m-1}			0.006	0.004
			(2.65)	(1.66)
Idiosyncratic Volatility			1.529	1.508
			(16.63)	(16.84)
Share Turnover			−0.007	−0.007
			(−22.76)	(−24.16)
Payout			0.053	0.079
			(5.87)	(8.79)
Investor × Date FE	Yes	Yes	Yes	Yes
Industry × Date FE	No	No	No	Yes
Observations (M)	407.360	407.360	407.360	407.360
R^2	0.25	0.31	0.32	0.32

Table 8: Diversity and Stock Returns

Panel A presents returns for the value-weighted portfolios of diverse firms, homogeneous firms, and the portfolio that goes long on diverse firms and short on homogeneous firms. We show raw portfolio returns, Fama-French 3-factor portfolio alphas, Fama-French-Carhart 4-factor alphas, and Fama-French 5-factor alphas. To predict returns from July year t through June year $t+1$ we use values of diversity as of December year $t-1$. Portfolios are formed using diversity quartiles. Panel B presents monthly Fama and MacBeth (1973) regressions. All control variables are defined in the appendix. Columns (1) and (2) weight observations by market capitalization in June year t. Columns (3) and (4) use equal weighting. t-statistics based on Newey and West (1987) standard errors with 12 monthly lags are shown in parentheses.

Panel A: Returns by Diversity Quartiles

	Returns			t-statistics		
	Diverse	Homogeneous	D−H	Diverse	Homogeneous	D−H
Raw returns (%)	0.90	0.43	0.46	2.65	1.31	3.14
FF 3-factor (%)	0.29	−0.09	0.38	2.77	−1.06	3.06
FF 4-factor (%)	0.32	−0.03	0.34	3.04	−0.41	2.80
FF 5-factor (%)	0.14	−0.11	0.25	1.37	−1.18	1.90

Panel B: Fama-MacBeth Regressions

	Value-Weighted		Equal-Weighted	
	(1)	(2)	(3)	(4)
Diversity	0.744	0.669	0.203	0.335
	(3.51)	(3.39)	(1.05)	(2.32)
Market Capitalization	−0.082	−0.221	−0.016	−0.168
	(−1.95)	(−3.41)	(−0.33)	(−3.02)
Book-to-Market	0.102	0.070	0.132	0.049
	(0.72)	(0.63)	(1.15)	(0.54)
$\text{Return}_{m-12\to m-2}$	−0.092	−0.193	0.031	−0.022
	(−0.16)	(−0.34)	(0.08)	(−0.06)
Return_{t-1}		−3.462		−2.724
		(−3.47)		(−4.49)
Idiosyncratic Volatility		−0.745		−0.434
		(−3.30)		(−2.30)
Share Turnover		0.015		−0.002
		(1.84)		(−0.38)
Observations	446,013	444,248	446,013	444,248
R^2	0.09	0.15	0.03	0.07

Table 9: Diversity and Institutional Investors' Portfolio Choices: Regional Variation

The dependent variable is 1 when a stock appears in the portfolio of an investor in a given quarter, and 0 otherwise. Panel A compares portfolio choices for investors headquartered in Republican versus Democrat states; Panel B compares portfolio choices for investors headquartered in states characterized by a low versus high proportion of minority population; Panel C compares portfolio choices for investors headquartered in states characterized by a low versus high racial animus. We define a state Republican (Democrat) if the majority of voters voted for the Republican (Democratic) candidate in the last Presidential election. We define a state as low (high) minority if the proportion of population in minority groups is in the bottom (top) quartile across states in that year. We classify a state as low (high) racial animus if the state is in the top (bottom) quartile of the Dougal, Gao, Mayew, and Parsons (2018) racial animus ranking. In all panels, controls are the same as in the respective column of Table 7. t-statistics are based on standard errors clustered by date (year-quarter), and are shown in parentheses.

	(1)	(2)	(3)	(4)
Panel A: Republican vs. Democrat States				
Republican	−0.063	−0.028	−0.027	−0.030
	(−11.78)	(−25.02)	(−22.52)	(−25.63)
Democrat	−0.041	−0.016	−0.017	−0.019
	(−9.43)	(−17.77)	(−16.88)	(−18.11)
R – D	−0.022	−0.011	−0.010	−0.011
	(−3.22)	(−7.92)	(−6.40)	(−7.87)
Observations (M)	315.671	315.671	315.671	315.671
Panel B: Low Minority vs. High Minority States				
Low Minority	−0.067	−0.032	−0.030	−0.035
	(−11.90)	(−27.44)	(−25.24)	(−28.21)
High Minority	−0.034	−0.012	−0.013	−0.016
	(−9.29)	(−15.26)	(−15.42)	(−19.64)
L – H	−0.033	−0.020	−0.017	−0.019
	(−4.88)	(−14.03)	(−11.93)	(−12.55)
Observations (M)	165.587	165.587	165.587	165.587
Panel C: Low Racial Animus vs. High Racial Animus States				
High Racial Animus	−0.060	−0.029	−0.028	−0.031
	(−10.67)	(−24.18)	(−21.32)	(−22.81)
Low Racial Animus	−0.042	−0.017	−0.018	−0.021
	(−9.90)	(−19.88)	(−17.58)	(−21.45)
H – L	−0.018	−0.012	−0.010	−0.010
	(−2.57)	(−8.11)	(−6.33)	(−8.56)
Observations (M)	136.652	136.652	136.652	136.652
Baseline Controls	No	Yes	Yes	Yes
Additional Controls	No	No	Yes	Yes
Investor × Date FE	Yes	Yes	Yes	Yes
Industry × Date FE	No	No	No	Yes

Table 10: Diversity Returns on Information-Release Days

This table reports results from a regression of daily returns on diversity, information-release day dummy variables, interactions between diversity and information-release day variables, calendar-day fixed effects and controls. Information-day variables are dummies equal to one on earning announcement dates (Eday), or corporate news release dates (Nday), respectively. In all columns, controls include log of market capitalization, log book-to-market ratio, return$_{m-12\rightarrow m-2}$, log $(1 +$ analysts following$)$, reporting lag, reporting lag squared and cubed, institutional ownership, earnings volatility, earnings persistence and share turnover. Industries are based on the Fama-French 12-industry definition. t-statistics are based on standard errors clustered by day, and are shown in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
Diversity	0.022	0.020	0.011	0.007	0.011	0.007
	(2.73)	(2.87)	(1.29)	(0.91)	(1.28)	(0.90)
Eday	−0.052	−0.040			−0.051	−0.034
	(−0.79)	(−0.63)			(−0.76)	(−0.54)
Eday × Diversity	0.251	0.229			0.215	0.188
	(3.14)	(2.98)			(2.67)	(2.43)
Nday			−0.002	−0.007	0.001	−0.005
			(−0.15)	(−0.61)	(0.12)	(−0.41)
Nday × Diversity			0.064	0.070	0.050	0.057
			(4.29)	(4.90)	(3.38)	(4.05)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	No	Yes	No	Yes	No
Industry × Day FE	No	Yes	No	Yes	No	Yes
Observations	5,067,758	5,067,758	5,067,758	5,067,758	5,067,758	5,067,758
Adjusted R^2	0.26	0.30	0.26	0.30	0.26	0.30

APPENDIX

A Additional Variable Descriptions

Variable	Description
Main independent variable	
Diversity	Degree of similarity among the members of the executive team. This variable is computed by applying text–based analysis to managerial biographies as described in the main text. It can take on values in the interval [0,1], with 0 representing the most homogeneous firms and 1 representing the most diverse firms. If Diversity is missing for a given firm in year t, we assign the value of Diversity in $t-1$, provided that Diversity is not missing in both year $t-1$ and $t+1$.
Variables in Table 3 and Table 4	
10-K File Size	The natural logarithm of the file size in megabytes of the SEC EDGAR "complete submission text file" for the 10-K filing.
10-K Word Count	The natural logarithm of the word count from the 10-K.
Bio Length	Natural logarithm of the average number of words in the biographies of each top management team member. Computed after applying the filter described in Section 2.2. If the variable is missing for a given firm in year t, we assign the value of the variable in $t-1$, provided that the variable is not missing in both year $t-1$ and $t+1$. Source: Executive biographies.
Business HHI	The sum of the squared business segment shares reported for the firm in the COMPUSTAT Segment database, based on company sales.
Governance Strengths	Index computed by analyzing compensation, reporting quality, political accountability, firm's involvement in public policy, and ownership. Source: RiskMetrics KLD STATS
Idiosyncratic Volatility	Standard deviation of residuals from Fama-French 4–factor model estimated from daily returns over calendar year $t-1$.
Organizational Capital	Eisfeldt and Papanikolaou (2013) measure of organizational capital.
Team Size	Natural logarithm of the number of executives constituting the top management team. If the variable is missing for a given firm in year t, we assign the value of the variable in $t-1$, provided that the variable is not missing in both year $t-1$ and $t+1$. Source: Executive biographies.
Uncertain Words	Percentage of words within the 10-K that are classified as uncertain using the Loughran and McDonald (2011) word list.
Workforce Diversity	Index computed by combining (i) promotion of minorities and women, (ii) work-life-benefits at the company (iii) whether the firm does significant amounts of business with women or minority owed subcontractors or suppliers, (iv) employment of the disabled, (v) gay and lesbian policies, and (vi) other diversity strengths. Source: RiskMetrics KLD STATS
Weak Modal Words	Percentage of words within the 10-K that are classified as weak modal using the Loughran and McDonald (2011) word list.

Additional Variable Descriptions (Continued)

Variable	Description
Variables in Table 5	
Company Overlap	For every pair of executives in a given team we compute the number of company names that appear in the biographies of both executives, then we take the average over all executive pairs. If the variable is missing for a given firm in year t, we assign the value of the variable in $t-1$, provided that the variable is not missing in both year $t-1$ and $t+1$. Source: Executive biographies.
Elite University St. Dev.	Standard deviation of an indicator variable that takes value 1 if an executive attended one of the following universities, at any academic level: MIT, Stanford University, University of Chicago, Brown University, Columbia University, Cornell University, Dartmouth College, Harvard University, Princeton University, University of Pennsylvania, Yale University, and 0 otherwise. If the variable is missing for a given firm in year t, we assign the value of the variable in $t-1$, provided that the variable is not missing in both year $t-1$ and $t+1$. Source: Executive biographies.
Executive Age St. Dev.	Standard deviation of the age of the executives on the top management team. Source: Boardex supplemented by Execucomp.
Gender St. Dev.	Within executive team standard deviation of an indicator variable that takes value 1 when the executive is a woman and 0 otherwise. If the variable is missing for a given firm in year t, we assign the value of the variable in $t-1$, provided that the variable is not missing in both year $t-1$ and $t+1$. Source: Boardex supplemented by executive biographies.
Nationality Mix	One minus the Herfindahl concentration index for nationality. Source: Boardex.
Tenure Overlap	For every pair of executives in a given team we compute the number of years that the pair has worked together on the team, then we take the average over all executive pairs. Source: Boardex supplemented by Execucomp.
University Overlap	For every pair of executives in a given team we compute the number of university names that appear in the biographies of both executives, then we take the average over all executive pairs. If the variable is missing for a given firm in year t, we assign the value of the variable in $t-1$, provided that the variable is not missing in both year $t-1$ and $t+1$. Source: Executive biographies.

Additional Variable Descriptions (Continued)

Variable	**Description**
Other variables	
Analyst Following	Natural logarithm of the number of analysts issuing an earning (target price) forecast in the current quarter (month).
Book-to-Market	The natural logarithm of the ratio of the book value of equity to the market value of equity. Book equity is total assets at the end of December year $t-1$, minus total liabilities, plus balance sheet deferred taxes and investment tax credit if available, minus preferred stock liquidating value if available, or redemption value if available, or carrying value. Market equity is price times shares outstanding at the end of December of $t-1$.
Earnings Persistence	The first-order autocorrelation coefficient of quarterly earnings using 4 years of data.
Earnings Volatility	The standard deviation during the previous 4 years of the deviations of quarterly earnings from the corresponding 1 year ago earnings.
Institutional Ownership	Computed as the portion of shares outstanding held by institutional investors in a given quarter. Values of the variable below 0.0001 and above 0.9999 are replaced with 0.0001 and 0.9999, respectively.
Market Capitalization	The natural logarithm of price times shares outstanding.
Number of Firms Followed	Natural logarithm of the number of firms for which the analyst issues an earning (target price) forecast in the current quarter.
Payout	Ratio of total dividends over the book value of assets.
Reporting Lag	Number of days between the end of the current quarter and the earnings announcement date.
$\text{Return}_{m-12\to m-2}$	Cumulated continuously compounded stock return from month $m-12$ to month $m-2$, where m is the last month of the current quarter, or the month of the forecasted return.
Return_{m-1}	Stock return in month $m-1$, where m is the last month of the current quarter, or the month of the forecasted return.
Share Turnover	Average monthly share trading volume divided by the average number of shares outstanding during a 1-year period ending at the end of the current quarter.

B Common Words in Biographies

Table B.1: Common Words in Executive Biographies

This table shows the list of the 100 most commonly occurring terms in the main dictionary based on executive biographies in the year 2011.

Rank	Word	Rank	Word	Rank	Word
1	position	35	administration	69	assistant
2	operations	36	state	70	health
3	finance	37	real	71	mba
4	public	38	science	72	communication
5	october	39	estate	73	software
6	committee	40	medical	74	subsidiary
7	firm	41	human	75	strategy
8	technology	42	information	76	oil
9	international	43	national	77	planning
10	investment	44	college	78	legal
11	degree	45	york	79	compensation
12	counsel	46	service	80	equity
13	marketing	47	american	81	association
14	secretary	48	research	82	llp
15	accounting	49	strategic	83	founder
16	sales	50	time	84	holding
17	global	51	llc	85	advisory
18	industry	52	extensive	86	addition
19	bank	53	leadership	87	institute
20	capital	54	career	88	provider
21	private	55	ceo	89	capacity
22	law	56	independent	90	america
23	engineering	57	principal	91	governance
24	division	58	banking	92	responsibility
25	school	59	california	93	shares
26	resource	60	commercial	94	effective
27	energy	61	united	95	department
28	systems	62	acquisition	96	consulting
29	product	63	consultant	97	present
30	controller	64	solutions	98	healthcare
31	treasurer	65	partner	99	market
32	responsible	66	securities	100	insurance
33	audit	67	gas		
34	bachelor	68	accountant		

C Additional Results

Table C.1: Diversity and Analyst Earnings Forecasts - Firm-Level Evidence

This table relates top management team diversity to analyst earnings forecasts. To obtain this table, we collapse our analyst-level sample at the firm-level, by taking the average of all variables defined at the analyst-level. In the first row, the dependent variable is Actual, defined as announced quarterly earnings as reported by IBES, divided by the stock price at the end of the last quarter before the announcement. In the second row, the dependent variable is Forecast, computed as the mean of all 1- or 2-quarter-ahead forecasts issued by an analyst covering the firm as reported by IBES, divided by the stock price at the end of the last quarter before the announcement. In the last row, the dependent variable is A − F, the difference between Actual and Forecast. For all rows, the table shows the coefficient from regressing the dependent variable on diversity. Controls include log of market capitalization, log book-to-market ratio, return$_{m-12\to m-2}$, log (1 + analyst following), reporting lag, reporting lag squared, reporting lag cubed, institutional ownership, earnings volatility, earnings persistence, share turnover, and the average number of firms followed by the analysts covering the firm. Date FE are based on year-quarter dates. Industries are based on the Fama-French 12-industry definition. t-statistics are based on standard errors clustered by date (year-quarter), and are shown in parentheses.

	(1)	(2)	(3)
Actual	1.021	1.076	1.103
	(3.33)	(3.56)	(4.11)
Forecast	0.165	0.188	0.398
	(1.09)	(1.22)	(3.02)
A − F	0.856	0.888	0.705
	(3.69)	(3.93)	(3.63)
Controls	Yes	Yes	Yes
Date FE	No	Yes	No
Industry × Date FE	No	No	Yes
Observations	197,304	197,304	197,304

Table C.2: Diversity and Analyst Earnings Forecasts - Excluding Turnover Events

This table relates top management team diversity to analyst earnings forecasts. In the first row, the dependent variable is Actual, defined as announced quarterly earnings as reported by IBES, divided by the stock price at the end of the last quarter before the announcement. In the second row, the dependent variable is Forecast, computed as the 1- or 2-quarter-ahead forecast issued by an analyst covering the firm as reported by IBES, divided by the stock price at the end of the last quarter before the announcement. In the last row, the dependent variable is A − F, the difference between Actual and Forecast. If a firm has an executive turnover in a given quarter, we exclude that firm from the analysis in the subsequent 4 quarters. For all rows, the table shows the coefficient from regressing the dependent variable on diversity. Controls include log of market capitalization, log book-to-market ratio, return$_{m-12\rightarrow m-2}$, log (1 + analyst following), reporting lag, reporting lag squared, reporting lag cubed, institutional ownership, earnings volatility, earnings persistence, share turnover, and the number of firms followed by the analyst. Date FE are based on year-quarter dates. Industries are based on the Fama-French 12-industry definition. t-statistics are based on standard errors clustered by date (year-quarter), and are shown in parentheses.

	(1)	(2)	(3)	(4)
Actual	0.880	0.851	0.620	0.569
	(3.60)	(3.58)	(3.25)	(2.99)
Forecast	0.190	0.180	0.221	0.200
	(1.64)	(1.54)	(1.98)	(1.78)
A − F	0.690	0.671	0.399	0.368
	(4.21)	(4.29)	(3.27)	(3.10)
Controls	Yes	Yes	Yes	Yes
Date FE	No	Yes	No	No
Analyst × Date FE	No	No	Yes	Yes
Industry × Date FE	No	No	No	Yes
Observations	791,328	791,328	791,328	791,328

Table C.3: Diversity and Analyst Target Prices

This table compares returns computed from analyst forecasts of target prices with ex-post realized returns. In the first row, the dependent variable is Actual, and it is computed as the 12 month cumulative stock return excluding dividends as reported in CRSP. In the second row, the dependent variable is Forecast, computed as $TP_{m+12}/P_m - 1$, where TP_{m+12} is the average one-year ahead target price across all active forecasts in the current month, and P_m is the current stock price. In the last row, the dependent variable is A − F, computed as the difference between actual ex-post realized return and forecast. Controls are the logarithm of market capitalization, book-to-market, return$_{m-12\to m-2}$, share turnover, idiosyncratic volatility, return$_{m-1}$, the fraction of firm shares held by institutional investors, the number of target price forecasts issued by the analyst in the current month, the logarithm of one plus analysts following, and dispersion, defined as the standard deviation of the price targets divided by the average price target. Date FE are based on year-month dates. Industries are based on the Fama-French 12-industry definition. t-statistics are based on standard errors clustered by date (year-month), and are shown in parentheses.

	(1)	(2)	(3)	(4)
Actual	0.019	0.030	−0.003	−0.004
	(1.96)	(4.24)	(−0.58)	(−0.60)
Forecast	−0.015	−0.045	−0.036	−0.036
	(−1.59)	(−5.08)	(−3.82)	(−3.76)
A − F	0.034	0.074	0.033	0.033
	(2.31)	(5.93)	(2.62)	(2.58)
Controls	Yes	Yes	Yes	Yes
Date FE	No	Yes	No	No
Analyst × Date FE	No	No	Yes	Yes
Industry × Date FE	No	No	No	Yes
Observations	683, 974	683, 974	683, 974	683, 974

Table C.4: Diversity and Institutional Investors' Portfolio Choices - Excluding Same-State Companies

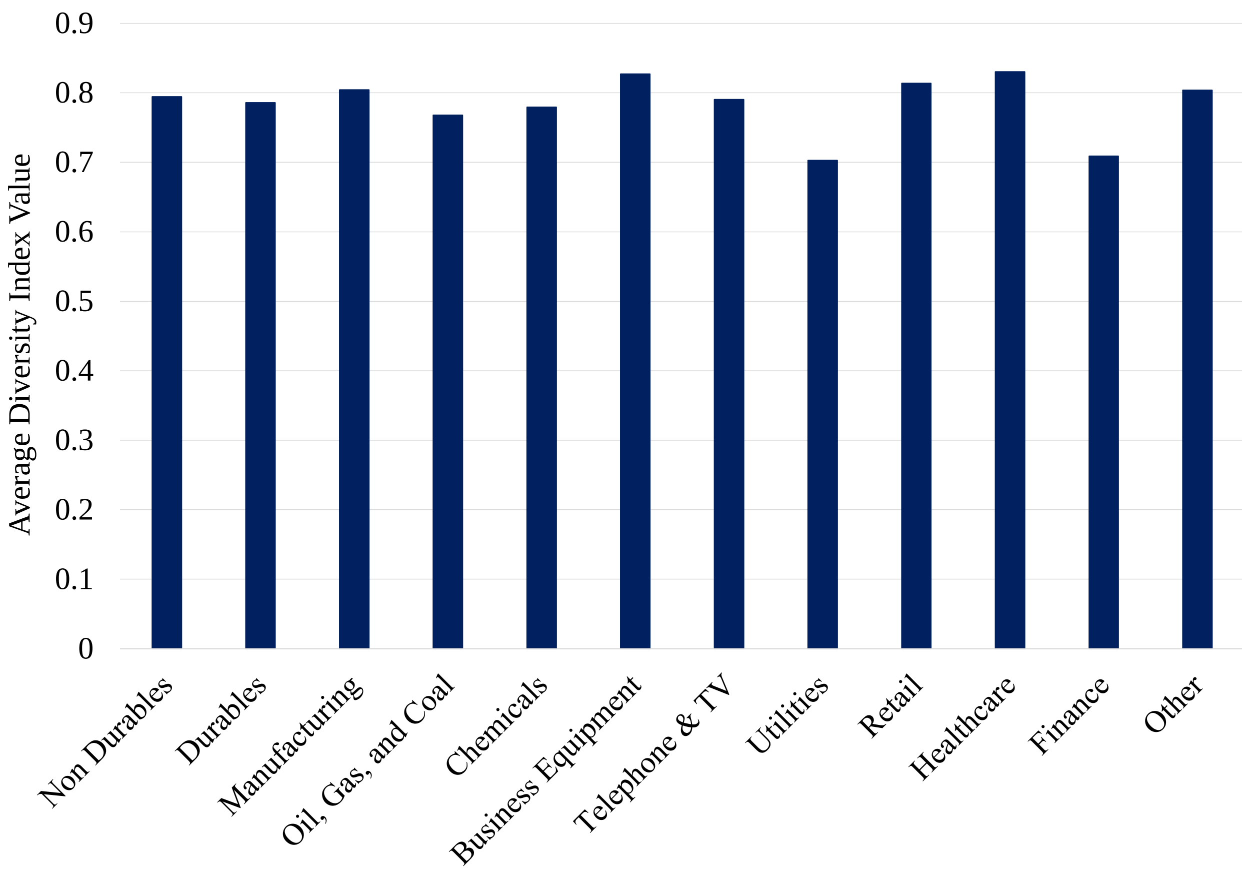
This table presents results of regressions of the probability that a stock is in the portfolio of an institutional investor on diversity and control variables. The dependent variable is coded as 1 when a stock appears in the portfolio of an investor in a given quarter, and 0 otherwise. We exclude all institutional investor-firm pairs for which the two entities are headquartered in the same state. All control variables are defined in the appendix. Date FE are based on year-quarter dates. Industries are based on the Fama-French 12-industry definition. t-statistics are based on standard errors clustered by date (year-quarter), and are shown in parentheses.

	(1)	(2)	(3)	(4)
Diversity	−0.043	−0.017	−0.017	−0.021
	(−9.26)	(−18.74)	(−17.63)	(−20.28)
Market Capitalization		0.030	0.037	0.037
		(54.71)	(57.43)	(61.37)
Book-to-Market		0.001	0.001	0.004
		(1.94)	(3.88)	(10.01)
$\text{Return}_{m-12\to m-2}$		0.001	0.001	0.000
		(0.09)	(0.90)	(0.06)
Return_{m-1}			0.007	0.004
			(2.72)	(1.73)
Idiosyncratic Volatility			1.551	1.508
			(16.63)	(16.89)
Share Turnover			−0.007	−0.007
			(−23.40)	(−25.06)
Payout			0.054	0.082
			(5.99)	(9.01)
Investor × Date FE	Yes	Yes	Yes	Yes
Industry × Date FE	No	No	No	Yes
Observations (M)	381.049	381.049	381.049	381.049
R^2	0.26	0.32	0.32	0.32

D Additional Figure

Figure D.1: Average Diversity Index by Industry

This figure shows the average of the diversity index by industry across our sample. Industries are based on the Fama-French 12-industry definition.



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