Does Crowdsourced Research Discipline Sell-Side Analysts?

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Abstract:

We examine whether increased competition stemming from an innovation in financial technology disciplines sell-side analysts. We find that firms added to Estimize, an open platform that crowdsources short-term earnings forecasts, experience reductions in short-term forecast bias relative to matched control firms. The reduction is greater when existing sell-side competition is lower, earnings uncertainty is higher, and Estimize coverage is less biased and more accurate. We also document an increase in short-term forecast accuracy and representativeness. We find no change in bias for longer-horizon forecasts or investment recommendations, suggesting competition from Estimize rather than broad economic forces drives our results.

Keywords: Sell-Side Analysts, Conflicts of Interests, Competition, Crowdsourcing, FinTech.

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1. Introduction

The role of sell-side equity analysts as key information intermediaries in capital markets has been well documented. Analyst earnings forecasts and stocks recommendations have a substantial impact on stock prices (e.g., Gleason and Lee, 2003; Womack, 1996), and analyst coverage reduces information asymmetry, resulting in a lower cost of capital (Kelly and Ljungqvist, 2012). At the same time, the sell-side research industry is fraught with conflicts of interest. Dependent on managers for information and subsidized by investment banking revenues, analysts have incentives to bias their research to please managers and facilitate investment banking activities. Several studies find that analyst research is biased, in some cases even distorting market prices and harming less sophisticated investors.[1] For instance, naïve fixation on optimistic long-term forecasts contributes to a wide range of market anomalies (Dechow and Sloan, 1997; Grinblatt, Jostova, Philipov, 2016), whereas naïve fixation on pessimistic short-term forecasts unduly increases the valuation of firms that consistently meet analyst expectations (Kasznik and McNichols, 2002). Large investors appropriately discount predictably positive stock recommendations, but small investors do not (Malmendier and Shanthikumar, 2007). The impact of biased research on capital markets and investor welfare motivates a better understanding of the forces that constrain sell-side bias.

In this study, we investigate whether increased competition from crowdsourced investment research disciplines sell-side analysts. In an attempt to capitalize on investors' increased social media participation and harness the wisdom of crowds, financial technology (FinTech)[2] companies such as Estimize and Seeking Alpha have outsourced the task of forecasting earnings and picking

[1] See Mehran and Stulz (2007) for a survey of the literature on conflicts of interest in the investment industry.
[2] As defined in Philippon (2016), FinTech includes "technology-enabled business model innovations in the financial sector" (p. 15).

stocks to large networks of people. Prior literature finds that crowdsourced research conveys useful information to capital markets (Jame, Johnston, Markov, and Wolfe, 2016; Chen, De, Hu, and Hwang, 2014), but does not explore whether it also constrains sell-side bias.

Estimize has several distinctive features that make it especially well-suited for testing the disciplining hypothesis. First, Estimize freely provides a clear, close-to-unbiased forecast benchmark, whereas other prominent sources of crowdsourced investment research provide research commentaries which must be further processed to obtain a benchmark recommendation or forecast (e.g., Seeking Alpha).[3] Second, Estimize presents crowdsourced and sell-side forecasts side-by-side, further facilitating their comparison. Finally, since the overwhelming majority of Estimize forecasts are short-term (one-quarter ahead) forecasts, the setting affords a sharp prediction about the effect of increased competition on sell-side bias: In particular, we expect Estimize to weaken sell-side analysts' propensity to issue low, easy-to-beat quarterly earnings forecasts (hereafter: short-term pessimism).

We test for a decline in pessimism using a standard difference-in-difference approach. Our treatment sample includes firms added to Estimize in 2012 (i.e., firms whose first Estimize forecast appears in 2012). Our outcome variable is the difference between short-term pessimism over the three year "after" period (2013-2015) and short-term pessimism in the three year "before" period (2009-2011). We measure pessimism as the actual earnings minus the IBES analyst consensus, scaled by stock price. For each treated firm we select a matched control firm using a propensity score model that includes size, book-to-market, turnover, sell-side coverage, and the bias and accuracy of short-term forecasts.

[3] Section 2.2.2 of Jame et al. (2016) and Chapter 5 of Egger (2014) survey key sources of crowdsourced investment research.

We find that treated firms have positive forecast errors of 0.14% in the "before" period and 0.04% in the "after" period: an economically and statistically significant 0.10 percentage point (or 70%) drop in forecast pessimism. In contrast, the control firms experience a statistically insignificant 0.03 percentage point increase in pessimism. Furthermore, the difference-in-difference estimate of -0.13 percentage points is highly significant. We find similar results when we control for firm characteristics that influence sell-side bias, implement a portfolio matching method in selecting control firms, or use alternative measures of pessimism (e.g., meet or beat indicator). Furthermore, we document a leftward shift in the entire distribution of forecast pessimism, suggesting the decline in pessimism is widespread.

Accuracy and representativeness, defined as the ability to measure the market expectation, are basic forecast attributes that should benefit from a reduction in bias. Using the same difference-in-difference design, we document that treated firms experience a statistically and economically significant decline in absolute forecast errors relative to control firms. Similarly, the relation between sell-side consensus forecast errors and earnings announcement returns strengthens for treated firms relative to control firms, consistent with Estimize making the sell-side consensus a more accurate proxy for the market expectation.

We conduct a series of tests to strengthen our inference of a causal relation between the arrival of a new competitor, Estimize, and the decline in short-term pessimism. First, we confirm that treated and control firms do not experience significant differences in pessimism in any of the twelve quarters prior to Estimize coverage, suggesting that pre-trends are unlikely to explain our results. In contrast, the difference-in-difference estimate is negative in all twelve post-Estimize quarters and statistically significant in ten quarters.

Second, we demonstrate that our results are stronger in circumstances where theory and intuition suggest a greater role for Estimize as a disciplining device. In particular, the decline in pessimism is greater when existing sell-side competition is lower, earnings volatility and return volatility are higher, and Estimize coverage is less biased and more accurate. These findings suggests that competition from Estimize is particularly influential when existing competition is low, when high earnings uncertainty makes it difficult for investors to unravel sell-side bias alone, and when Estimize is a more effective benchmark. We also find significantly greater declines in pessimism for firms in industries heavily covered by Estimize. This effect holds even among stocks with no direct Estimize coverage, suggesting that heightened industry-level competition is a distinct mechanism though which Estimize reduces sell-side bias.

Our last set of tests addresses the concern that Estimize coverage is correlated with broad unobservable forces that steer sell-side analysts toward less biased research (e.g., by increasing reputation costs or reducing dependence on management for information). This explanation predicts less short-term forecast pessimism as well as less optimistic longer-term forecasts and stock recommendations (O'Brien, 1988; Michaely and Womack, 1999). In contrast, our hypothesis predicts only a reduction in short-term forecast pessimism, as long-term forecasts are rare and stock recommendations non-existent on the Estimize platform. Consistent with our hypothesis, we find no evidence that stocks added to Estimize experience a decline in optimism for longer-horizon earnings forecasts or investment recommendations relative to matched control firms.

Our primary contribution is toward understanding the market forces that constrain sell-side conflicts of interest. While prior literature focuses on reputational considerations (e.g., Fang and Yasuda, 2009), competition among sell-side analysts (e.g., Hong and Kacperczyk, 2010; Merkley, Michaely, and Pacelli, 2017), and regulation (e.g., Barber, Lehavy, McNichols, and Trueman, 2006; Kadan, Madureira, Wang, and Zach, 2009), our results point to FinTech-engendered competition as a force upending the investment research industry and disciplining the sell-side. The arrival of Estimize is the culmination of both a decades-long trend of technology empowering investors to bypass traditional sell-side research and decades-long investor criticism of conflicts of interest in the investment research industry.

Our study helps paint a more complete picture of how FinTech is changing the process by which information is produced and revealed in capital markets. Specifically, FinTech is not only creating new sources of value-relevant information and democratizing access to investment research (Chen et al., 2014; Jame et al., 2016), it is also changing the behavior of the incumbent providers, the sell-side analysts, impelling them to produce less biased and more accurate research (this study). More broadly, our results illustrate that technological innovations that empower retail investors to produce and disseminate valuable information can disrupt the traditional Wall Street information ecosystem (Costa, 2010).

Our study also fits well in a broader literature on competition and bias in other markets. In particular, Becker and Milbourn (2011), Doherty, Kartasheva, and Phillips (2012), and Xia (2014) examine entrants in the highly regulated and non-competitive credit rating market whose organization and practices largely mirror those of the incumbents (Fitch, Egan Jones, and S&P), whereas we study an entrant in a much less regulated and more competitive market whose business model and practices dramatically differ from those of the incumbents (Estimize). Genztkow and Shapiro (2008) and Gentzkow, Glaeser, and Goldin (2006) focus on the market for news. Our study's result that technology-engendered competition to sell-side research suppliers reduces sell-side bias echoes Gentzkow, Glaeser and Goldin's (2006) result that technology-engendered competition among newspapers in the 19th century reduces newspaper bias.

2. Background and Hypothesis Development

2.1 Analyst Bias and the Moderating Role of Competition

Managers generally desire favorable sell-side coverage, and they can shape analyst incentives by rewarding optimistic analysts with investment banking business, as well as private access and information. Consistent with the sell-side succumbing to management pressures, there is much evidence that analysts issue optimistic long-term earnings forecasts and recommendations, and that this optimism is explained by incentives to acquire investment banking deals (e.g., Lin and McNichols, 1998; Michaely and Womack, 1999), and obtain valuable information (e.g., Francis and Philbrick, 1993; Das, Levine, and Sivaramakrishnan, 1998).

At the same time, managers believe the market unduly rewards firms that meet or beat the sell-side consensus, measured in the days immediately prior to earnings announcements. As a consequence, managers desire a low, beatable earnings benchmark,[4] potentially creating incentives for analysts to reduce their forecasts. Extensive evidence suggests that analysts switch from long-term optimism to short-term pessimism, and that this forecasting behavior is rewarded by management (e.g., Ke and Yu, 2006, and Feng and McVay, 2010).

Factors that moderate the extent of analyst conflicts of interest include regulation, reputational concerns, and competition. We briefly discuss the moderating role of regulation and expound on the moderating roles of reputation and competition with a view to developing our hypothesis that technology-induced competition can also reduce sell-side bias.

The extent to which analysts bias research to attract investment banking business largely depends on investment bankers' ability to influence research department budgets and research

[4] See Graham, Harvey, and Rajgopal (2005) for survey evidence that CFOs guide sell-side analyst forecasts down to increase the likelihood of meeting the consensus, and Kasznik and McNichols (2002) and Bartov, Givoly, and Hayn. (2002) for archival evidence that meeting or beating forecasts is rewarded by the market.

analyst compensation. A string of recent reforms aim to increase analyst independence from investment bankers (e.g., NASD Rule 2711, NYSE Rule 472, and the Global Settlement), and evidence suggests these reforms have reduced but not fully eliminated analyst propensity to issue biased research (Barber et al., 2006; Kadan et al., 2009).

Sell-side research is an "experience" good purchased by investors in a multi-period setting, creating a role for reputation as a disciplining device. As discussed in Fang and Yasuda (2009), publishing biased research creates a fundamental trade-off for all analysts: a reputation loss and worsened long-term career prospects versus an increase in investment banking-driven compensation. Since analysts with better reputations stand to lose more from biasing their research than other analysts, theory predicts they will bias their research less. Consistent with this hypothesis, analysts rated "All-Stars" are less likely to issue biased research when conflicts of interest are more severe (Fang and Yasuda, 2009), and analyst bias is weaker for stocks heavily owned by institutional investors, who are more likely to discern bias and impose a reputational penalty (Ljungqvist, Marston, Starks, Wei, and Yan, 2007).

Hong and Kacperczyk (2010) argue that competition can reduce analyst bias through at least two channels. First, from the firm's perspective, the cost of influencing analyst coverage is increasing in the number of analysts covering the firm. Intuitively, the supply of management time and transactions requiring investment banking services is largely fixed. As the total number of analysts covering the firm increases, a firm's ability to influence coverage is weakened. Second, greater competition can increase the diversity of incentives among suppliers, making it more likely that at least one analyst will be incentivized to remain independent and provide an unbiased forecast. Access to one or more unbiased forecasts allows investors to more easily unravel biases in forecasts issued by other analysts, resulting in reputation loss and worsened career outcomes.[5] In short, competition reduces bias by exposing and penalizing biased analysts.[6]

An implicit assumption in the arguments that underlie the second channel is that investors cannot fully unravel analyst bias by themselves. This is a plausible assumption. First, to fully unravel analyst bias, investors would need to know the exact nature of the optimization problem solved by every analyst; even the most sophisticated institutional investors are unlikely to possess such knowledge. Second, there is ample empirical evidence consistent with investor inability to fully unravel analyst bias. For instance, Dechow and Sloan (1997) find that much of the profitability of value-oriented strategies can be explained by investors naively following biased analyst forecasts. Several studies document retail investors' inability to unravel bias. Malmendier and Shanthikumar (2007) show that while large traders (presumably institutional investors) tend to discount buy recommendations from affiliated analysts, small traders (presumably retail investors) tend to interpret the buy recommendations literally. Analyzing a small sample of stocks where analysts were found to have issued misleading research, De Franco, Lu, and Vasvari (2007) find pronounced differences in trading behavior between large and small investors; by their estimates, individual investors lost "$2.2 billion, an amount that is approximately two and a half times the amount that institutions lose" (p. 72).

[5] Research in psychology suggests competition can discipline the sell-side even in the absence of a reputational penalty. According to Kunda's (1990) theory of motivated reasoning, individuals motivated to arrive at a particular conclusion try to justify their conclusion to a dispassionate observer; and they draw the desired conclusion only if they can muster up the evidence necessary to support it (pp. 482-483). Sell-side analysts are motivated to issue pessimistic, easy-to-beat forecasts; widely available, accurate, and substantially less biased, Estimize forecasts make justifying biased sell-side forecasts to investors more difficult, thus causing a decline in sell-side bias.

[6] The general idea that competition can resolve conflict of interest problems between the provider of an experience good and a customer by encouraging reputation building behavior is developed in Horner (2002). In his model, greater competition strengthens reputation incentives by making the threat that a dissatisfied customer will terminate the relationship with the seller more credible.

Empirical evidence on the role of competition in disciplining equity analysts is limited. Using broker mergers to identify exogenous changes in analyst competition, Hong and Kacperczyk (2010) find that a decline in competition results in greater optimism in longer-term earnings forecasts. Similarly, Merkley, Michaely, and Pacelli (2017) measure competition at the industry level and report that a decline in industry-level competition leads to greater forecast optimism.

In recent years, technological and institutional innovations have given rise to new competing sources of investment research. While FinTech competition is often touted in the popular press as an important disciplining mechanism,[7] its effects on the incumbents have remained unexplored in the academic literature (Philippon, 2016). In this study, we examine whether competition from Estimize, a provider of crowdsourced earnings forecasts, has a disciplining effect on sell-side analysts. We discuss key attributes of Estimize in Section 2.2 and argue these attributes generally satisfy the conditions under which competition reduces bias in Section 2.3.

2.2 Estimize

Estimize is an open platform which crowdsources earnings forecasts from a diverse set of contributors. Estimize has received significant public acclaim and is frequently listed among the top FinTech companies.[8] As of December 2015, Estimize has attracted forecasts from over 15,000 contributors, covering more than 2,000 firms.[9] Estimize forecasts tend to be short-term focused; during our sample period of 2013 to 2015, more than 90% of all estimates are forecasts of current

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[7] For example, *The Economist* writes, "*The bigger effect from the fintech revolution will be to force flabby incumbents to cut costs and improve the quality of their service. That will change finance as profoundly as any regulator has"* (*The Economist,* 9 May 2015, p. 14).
[8] See, for example, https://www.benzinga.com/news/15/04/5395774/the-2015-benzinga-fintech-award-winners
[9] Estimize has experienced dramatic growth since the end of our sample period. As of December 2016, Estimize has over 40,000 unique contributors.

quarter (i.e., one-quarter ahead) earnings. Contributors to the platform include buy-side and sell-side analysts, portfolio managers, retail investors, corporate finance professionals, industry experts, and students. Estimize forecasts are available on Bloomberg and several other financial research platforms and are regularly referenced in prominent financial media sources including Forbes, Barron's, The Wall Street Journal, Investor's Business Daily, and Businessweek. Estimize is often featured on CNBC and has signed a data-sharing agreement which allows its estimates to be presented across all CNBC platforms. Estimize also sells a feed of all estimates made on the platform though an API in real time to buy-side clients.

Estimize was founded by Leigh Drogen, a former hedge fund analyst, with the objective of "disrupting the whole sell-side analyst regime".[10] Drogen's view is that crowdsourcing estimates from a diverse community should lead to a superior consensus for two reasons. First, by capturing the collective wisdom of a large and diverse group, the consensus can convey new information to the market. Second, by encouraging participation from individuals with varied backgrounds, Estimize contributors are more likely to be free from many of the conflicts that bias the research of sell-side analysts.[11] Jame et al. (2016) find evidence that is consistent with these predictions. In particular, they document that quarterly forecasts provided by Estimize are significantly less pessimistic than sell-side forecasts. They also find that Estimize forecasts are more representative of the market's expectation of earnings and incrementally useful in forecasting earnings.

2.3 Hypothesis Development

Recall that the first mechanism through which competition reduces bias relates to the cost of influence. Estimize's entry is likely to increase the firm's cost of influencing coverage more

[10] http://www.businessinsider.com/estimize-interview-leigh-drogan-2011-12
[11] In particular, Drogen highlights his dissatisfaction with the sell-side's "tendency to skew estimates in favor of higher earnings beat rates for the companies they cover," https://www.estimize.com/beliefs

than the entry of a typical sell-side research provider because Estimize contributors are numerous, often anonymous, and do not depend on management for information: that is, they cannot be "bribed" by managers with information, private meetings for clients, and underwriting/advisory business.

The second channel through which competition reduces bias is to increase the likelihood that one or more competitors issue unbiased forecasts, thus helping investors identify and penalize biased analysts. Estimize handily meets this condition: Estimize contributors do not depend on management for information and their forecasts are significantly less biased than sell-side forecasts (Jame et al., 2016). Furthermore, the usefulness of Estimize forecasts as a benchmarking device is likely enhanced by their high accuracy. Intuitively, an unbiased, accurate benchmark forecast is more useful in debiasing the sell-side forecast than an unbiased, inaccurate benchmark forecast. Finally, the process of unraveling sell-side bias is likely facilitated by the collocation of crowdsourced forecasts and sell-side forecasts on the Estimize website, in the financial media (e.g., Bloomberg and CNBC), and in datasets sold to quantitative investors. In a world of limited attention, the task of debiasing the sell-side consensus is simplified when the consensus and the benchmark forecast are in close proximity.[12]

The above arguments suggest that competition from Estimize can reduce sell-side analysts' bias. We predict a decline in one-quarter ahead sell-side forecast pessimism for stocks covered by Estimize because the majority of Estimize forecasts concern one-quarter ahead earnings. We use

[12] The potential value of Estimize as a debiasing tool has been recognized in the financial press: "*Adjusting for bias in short-term forecasts is harder. It is tempting simply to accept the errors--after all, they tend to be off by just a little… An alternative is to look at crowdsourcing websites such as Estimize. There punters--some amateur, and some professional--are shown Wall Street consensus estimates and asked to make their own forecasts. Estimize users beat Wall Street estimates two-thirds of time*" (*The Economist*, 3 Dec. 2016, p. 64).

the absence of longer-horizon forecasts and investment recommendations on the Estimize platform to conduct "placebo" tests of whether sell-side optimism also declines.

Several factors may attenuate and, perhaps, even eliminate the disciplining effect of Estimize. First, retail investors, who are least likely to unravel sell-side bias and most likely to benefit from Estimize's arrival, may be unable to impose sufficiently large penalties to discipline sell-side analysts. While large institutional investors do have the ability to discipline sell-side analysts, they may already unravel analyst bias, or they may tolerate analyst bias if it helps them obtain private information and access to management. Second, firms may counter the creation of new sources of investment research by investing more resources to influence traditional sell-side research providers as well as their competitors. Finally, if sell-side analysts view Estimize as a fad and predict its quick demise, they may feel no need to change their forecasting behavior. In sum, it is ultimately an empirical question whether and to what extent the crowdsourcing of earnings estimates by Estimize will affect the behavior of incumbent research providers.

3. Data and Descriptive Statistics

3.1 Sample Selection and Summary Statistics

So that we can reliably measure the change in sell-side bias around the introduction of Estimize in 2012, we require continuous sell-side coverage from 2009 to 2015, as reported by IBES. We also require that firms have non-missing book value of equity and stock price above $5 in the year prior to the introduction of Estimize. Our final sample includes 1,842 firms.

We obtain Estimize forecasts of earnings announced from January 2012 through December 2015. For each forecast, the dataset contains the forecasted earnings per share, the date of the forecast, the actual earnings per share, the date of the earnings announcement, a unique id for each contributor, and the ticker symbol of the firm. Table 1 provides summary statistics regarding the breadth and depth of Estimize coverage. Of the 1,842 firms in our sample, 1,391 firms have at least one Estimize forecast during the sample period. Collectively, there are 172,566 forecasts made by 11,167 unique contributors. The mean (median) Estimize firm is covered by 9.1 (4.0) different contributors during a quarter. Estimize's coverage and contributor base have significantly grown over time. For example, the number of firm-quarters with forecasts has increased from 1,694 in 2012 to 5,011 in 2015, and the number of contributors has increased from 1,370 to 7,555 over the same period.

Panel B of Table 1 examines the characteristics of firms added to Estimize at different times. All characteristics are measured during the 2013-2015 period. We observe that firms added in 2012 are larger, have greater sell-side coverage, and are more growth-oriented (i.e., lower book-to-market ratios) than firms added in subsequent years. These firms also attract greater Estimize coverage: 11.7 contributors per quarter compared to less than 2.5 contributors for later Estimize additions.

3.2 The Properties of Estimize and IBES Quarterly Forecasts

In this section, we compare the properties of Estimize and IBES quarterly earnings forecasts. We limit the sample to the 772 firms added to Estimize in 2012 (see Panel B of Table 1), and we report forecast properties over the 2013-2015 sample period. This sample choice foreshadows subsequent analyses in which we define firms added to Estimize in 2012 as "treated firms" and define the 2013-2015 sample period as the "post-event window". We consider only forecasts issued within 120 days of the earnings announcement date (i.e., one-quarter ahead forecasts) which account for approximately 93% of all forecasts, and we exclude Estimize forecasts flagged as unreliable (roughly 1% of the sample). The resulting sample includes 8,265 firm-quarters with at least one Estimize and one IBES forecast.

For each firm-quarter, we compute four forecast characteristics: *Forecasters per Stock*, *Forecast Age*, *Bias/Prc* (i.e., forecast error), and Absolute Forecast Error (*AbsFE)*. *Forecasters per Stock* is defined as the number of unique contributors or analysts issuing a forecast, and *Forecast Age* is defined as the number of calendar days between the forecast issue date and the earnings announcement date.

Our primary measure of forecast bias for firm *j* in quarter *t* is:

$$Bias\,/\,Prc_{j,t} = \frac{Actual_{j,t} - Consensus_{j,t}}{Price_{j,t\text{-}1}} * 100\,, \tag{1}$$

where *Actual* is reported earnings, *Consensus* is the mean Estimize (or IBES) forecast, and *Price* is the closing price at the end of the prior year. In computing *Consensus*, we use only the most recent forecast by a contributor or an analyst. We winsorize *Bias/Prc* at 2.5% and 97.5%. As a robustness check, we consider two alternative measures of bias: *Bias/AbsConsensus,* which uses the absolute value of *Consensus* as an alternative scaling factor, and *MBE*, a meet or beat earnings indicator equal to 1 if *Actual* is greater than or equal to *Consensus*, and 0 otherwise. *AbsFE*, a measure of forecast accuracy, is defined as the absolute value of *Bias/Prc.*

Table 2 reports the results. On average, a stock is covered by 12.6 Estimize contributors and 14.8 IBES analysts;[13] and Estimize (IBES) forecasts are issued 9.7 days (63.8 days) prior to earnings announcements. Estimize forecasts have similar accuracy (absolute forecast errors of 0.17% versus 0.16%), but much lower bias: For instance, the average *Bias/Prc* for Estimize forecasts is 0.00% compared to 0.06% for IBES forecasts, and Estimize forecasts are more pessimistic than IBES forecasts in only 19.18% of all firm-quarters. The results using

[13] We note that the number of Estimize contributors is slightly larger than the Table 1 estimate of 11.7 because Table 2 reports the average conditional on there being at least one Estimize contributor. In contrast, the number of sell-side analysts reported in Table 2 is smaller than Table 1, because in Table 2 we exclude forecasts issued more than 120 days prior to the earnings announcement.

Bias/AbsConsensus or *MBE* yield similar conclusions. The dramatic difference in bias, however measured, is consistent with sell-side analysts having greater incentives to issue pessimistic forecasts that managers can easily beat (Richardson, Teoh, and Wysocki, 2004).

4. Empirical Design

Our central prediction is that Estimize forecasts, which are easily accessible, reasonably accurate, and substantially less biased, can exert a disciplining effect on sell-side analysts' tendency to issue pessimistic forecasts of quarterly earnings. To test this prediction, we follow a standard difference-in-difference approach, which compares changes in bias for treatment and control firms around an event window.

We define treated firms as firms that are first added to Estimize in 2012. Firms added in 2012 experience significantly greater activity on the Estimize platform than firms added in later years (see Table 1). As greater Estimize activity places more pressure on sell-side analysts, this subgroup presents a more powerful setting for documenting the disciplining effect of Estimize.[14] Candidate control firms consist of firms that have not been added to Estimize as of 2015.

We define the pre-event period as the three years prior to the introduction of Estimize (2009 to 2011) and the post-event period as the three years after Estimize (2013 to 2015). We favor a long post-event window because it may take time for an upstart to prove its viability and begin to influence incumbents, and to reduce the error with which bias is measured; but in additional tests we also analyze changes in bias in event-time at a quarterly frequency.

The exclusion restriction is that the change in bias of the treatment firms relative to control firms around the introduction of Estimize is not due to factors other than the introduction of

[14] Treated firms exhibit within-year variation in treatment date. We explore this staggered introduction in Section 6.1.

Estimize. A natural concern is that treated firms have different characteristics from control firms, and that these differences influence the time-series behavior of *Bias/Prc*, biasing our difference-in-difference estimate. To minimize this potential bias, we match each treated firm to a control firm using propensity score matching.[15]

We obtain propensity scores from a logistic regression in which the dependent variable is a dummy variable equal to one for treated firms and zero for control firms, and the independent variables include four firm characteristics: Log *(Size*), *Book-to-Market*, *Turnover,* and Log (*IBES Coverage*), and two forecast characteristics: *Bias/Prc* and *AbsFE*. We measure firm characteristics at the end of 2011 and forecast characteristics as quarterly averages over the period 2009-2011. We find that the likelihood of being included in the treated sample increases with *Size*, *Turnover*, *IBES Coverage*, and *Bias/Prc*, and decreases with *Book-to-Market* and *AbsFE*. For each treated firm, we select the control firm with the closest propensity score (i.e., nearest neighbor matching).

The propensity score model uncovers the lack of a close match for many treated firms. For example, the highest propensity score for a treated firm is 99.88% compared to 97.37% for control firms. To ensure a good match (i.e., the validity of the common support assumption), our main tests exclude 156 observations where the absolute difference in the propensity scores of the treated and matched control firm exceeds 0.50%. In robustness tests, we confirm that including the 156 treated firms that lack common support does not significantly alter conclusions.

Table 3 examines the success of the propensity score matching. Panel A of Table 3 highlights that treated firms differ substantially from the universe of candidate control firms. However, Panel B shows that treated firms are very similar to matched control firms. In particular,

[15] In robustness tests, we confirm that our results are very similar using a portfolio matching approach (see Table 5).

we find no significant difference between the treated and matched control firms across any of the eight variables considered.

5. Main Analysis

5.1 Changes in Pessimism - Baseline Results

Panel A of Table 4 reports the results from our tests of changes in *Bias/Prc* for treated firms and matched control firms after the introduction of Estimize. In the case of treated firms, the average *Bias/Prc* is 0.14% in the pre-event period and 0.04% in the post-event period. The difference of 0.10 percentage points (or 70%) is statistically significant based on standard errors double clustered by control firm and quarter.[16] In contrast, the matched control firms experience a statistically insignificant 0.03 percentage point increase in *Bias/Prc* around the event. The difference-in-difference of -0.13 percentage points is not only statistically significant but also economically large. Specifically, the cross-sectional standard deviation of *Bias/Prc* for treated firms is 0.33%; thus, the decline of 0.13 percentage points corresponds to roughly 40% of the standard deviation of *Bias/Prc*. For reference, in Hong and Kacperczyk (2010), the change in long-term bias associated with losing one analyst due to a broker merger is roughly 5% of the standard deviation of long-term bias (see Table 1 and Table 5 in their study).

To control for additional firm characteristics that influence bias, we purge *Bias/Prc* from the effects of Log (*Size*), *Book-to-Market,* Log (*IBES Coverage),* *Turnover,* Log (*Return Volatility*), *Return, Forecast Age, Guidance,* and industry and time factors by estimating the panel regression:

[16] We cluster by matched control firm because some treated firms share the same control firm, which may result in correlated residuals across these treated firms. In untabulated analysis, we find that clustering by treated firm yields slightly larger t-statistics.

$$BIAS / Prc_{jt} = \alpha + \boldsymbol{\beta}\mathbf{X_j} + IND_j + QTR_t + \varepsilon_{jt}, \tag{2}$$

where **X** is the vector of firm characteristics, *IND* is a vector of 12 Fama and French (1997) industry dummies, and *QTR* is a vector of 24 quarter dummies. Panel B of Table 4 reports the results when the regression residual, *Abnormal Bias/Prc*, is the outcome variable. We find that treated firms experience a statistically significant decline in *Abnormal Bias/Prc* of 0.04 percentage points, control firms experience a significant increase of 0.08 percentage points, and the difference-in-difference of -0.12 percentage points is highly significant.

We also assess the pervasiveness of the hypothesized effect by examining the entire distribution of forecast bias in the pre-event and post-event periods. Specifically, we plot the difference between the quarterly average *Abnormal Bias/Prc* of a treated firm and its matched control firm in the pre-and post-event window. Figure 1 presents the results. We observe a significant leftward shift in the entire distribution of forecast pessimism in the post-event window. For example, the median value falls by 0.08 percentage points and the 25th (75th) percentile falls by 0.10 (0.14) percentage points. Similarly, the percentage of forecasts where the difference in *Abnormal Bias/Prc* is greater than zero (i.e., when forecasts are more pessimistic for treated firms relative to control firms) falls from 51% in the pre-event window to 32% in the post-event window.

Collectively, the evidence suggests that treated firms experience a pervasive and economically large reduction in bias, consistent with Estimize coverage disciplining sell-side analysts into issuing less biased forecasts.

5.2 Changes in Pessimism - Robustness Results

In Table 5, we further examine the robustness of our results by estimating eight alternative specifications. For reference, the first row of Table 5 reports the estimates from the baseline specification (as reported in Table 4). In Row 2, we extend the sample to include the 156 treated firms dropped due to lack of common support and find very similar results. In Row 3, we select control firms using a portfolio matching approach. Specifically, we require that candidate control firms be in the same size quintile and book-to-market quintile, based on breakpoints estimated at the end of 2011. We then select the candidate control firm that has the smallest difference in pre-event period *Bias/Prc* (averaged across all 12 quarters in the pre-event window). We match along size and book-to-market because 1) treated firms and controls firms tend to differ significantly along both dimensions and 2) the magnitude of short-term pessimism tends to vary with both characteristics (see, e.g., Richardson, Teoh, and Wysocki, 2004); we match on pre-event bias to control for mean reversion. The results using the portfolio-matched control firms are slightly weaker but still economically large and statistically significant.

We examine the sensitivity of our findings to alternative measures of bias in Rows 4 through 6. We find similar results when bias is defined as *Bias/AbsConsensus* (Row 4) and weaker but still significant results when it is defined as *MBE* (Row 5). In Row 6, we demean *Bias/Prc* by the average *Bias/Prc* of a given analyst-firm pair over the sample period to preclude the concern that our results are driven by the entry of relatively more pessimistic analysts in the post-event period, and find that our results remain.

In Row 7, we exploit the staggered treatment setting by defining treated firms as those added to Estimize in 2013. Accordingly, we measure pre-event bias over the period 2009-2011 and post-event bias over 2014 and 2015. The difference-in-difference estimates of *Bias/Prc* and *Abnormal Bias/Prc* are -0.05% and -0.04%, the lowest in Table 5 and statistically insignificant. We attribute the lack of significance to weaker treatment: On average, firms added to Estimize in 2013 are covered by only 2.53 contributors, whereas firms added in 2012 are covered by 11.7 contributors (see Table 1).

Finally, in Rows 8 and 9 we confirm that our results hold in a subsample of firm-quarters in which management does not issue any earnings guidance, and in a subsample of sell-side forecasts issued more than 60 days prior to the earnings announcement. The first specification addresses the concern that our results are driven by a change in management guidance practices.[17] The second specification alleviates the concern that sell-side analysts herd on or learn from Estimize forecasts, as 99% of all Estimize forecasts are issued within 60 days of the earnings announcement.

5.3 Changes in Other Forecast Properties – Accuracy and Representativeness

We next investigate whether sell-side forecast accuracy and representativeness (the degree to which the sell-side consensus is representative of the market consensus) increase for stocks added to Estimize. Since lower bias generally implies higher accuracy and higher representativeness, evidence of increased accuracy and representativeness would lend additional support to our basic hypothesis of a decline in bias due to the arrival of Estimize.

In Panel A of Table 6, we repeat the analysis of Panel A of Table 4 after replacing *Bias/Prc* with *AbsFE*. We find that treated firms experience a statistically significant reduction in *AbsFE* of 0.13 percentage points, while control firms experience an insignificant decline of 0.03 percentage points. The difference-in-difference estimate of -0.10 percentage points is highly significant.

In measuring representativeness, we rely on the intuition that a superior measure of the market expectation exhibits a stronger association with returns at the time of the earnings announcement (Brown, Hagerman, Griffin, and Zmijewski, 1987). For each firm with at least six quarters of Estimize forecasts, we estimate the time-series regression:

[17] In untabulated analysis, we also find that the difference-in-difference in the frequency and bias of management guidance around the introduction of Estimize is economically small and statistically insignificant.

$$CAR_{j,t} = \alpha + \beta\left(UE_{jt}\right) + \varepsilon_t, \tag{3}$$

where *CAR* is the cumulative market-adjusted return in the three trading days around the earnings announcement date and *UE* is unexpected earnings (i.e., actual earnings less forecasted earnings) scaled by price and standardized to have mean 0 and standard deviation 1 for each firm. The slope coefficient, β (also known as the Earnings Response Coefficient), is our measure of representativeness. We winsorize β at the 1st and 99th percentile.

Panel B of Table 6 presents the changes in *Representativeness* for treated and matched control firms. We find that *Representativeness* increases significantly for treated firms but not for control firms. In particular, for treated firms, a one standard deviation increase in unexpected earnings is associated with a 2.71% three-day earnings announcement return in the pre-event period and a 4.98% return in the post-event period; for control firms, the corresponding figures are 2.62% and 2.87%. The difference-in-difference estimate of 2.03% is economically and statistically significant.[18]

6. Strengthening Causal Inference

In this section, we seek to increase confidence in the causal interpretation of our findings by demonstrating that 1) the parallel trends assumption underlying the difference-in-difference approach is valid, 2) the decline in pessimism varies as predicted by economic theory and intuition, and 3) sell-side biases that should not be affected by the arrival of Estimize are indeed unaffected.

6.1 Time-Series Patterns in the Decline of Pessimism

[18] An important concern is that the sell-side consensus becomes more accurate and representative of the market consensus because of a shorter forecast horizon. However, in untabulated analysis, we find that the average forecast horizon for treated firms increases by roughly 4 days relative to matched control firms.

The assumption of parallel trends asserts that the change in bias in the treatment and control samples would have been the same had Estimize not been created in 2012. To investigate the parallel trends assumption, we examine the difference in bias of treatment and matched control firms in event time. Demonstrating equality during the pre-event period helps alleviate the concern that the documented difference around the event reflects the continuation or the reversal of an earlier difference in trends.

Figure 2 plots the difference in *Abnormal Bias/Prc* between treated and matched control firms from quarters -12 to +12, where quarter 0 is the quarter in which the firm was first added to Estimize. A key benefit of conducting this analysis at the quarterly frequency is that it allows for a richer description of the dynamic relation between the arrival of Estimize and sell-side bias. In all 12 quarters during the pre-event window, the difference in *Abnormal Bias/Prc* between treated and matched control firms is economically small, typically less than 0.05 percentage points, and statistically insignificant, with statistical significance based on standard errors clustered by control firm. We also find that the change in the difference in *Abnormal Bias/Prc* (i.e. the difference-in-difference) from year -3 (i.e. quarters -12 to -9) to year -1 is statistically insignificant. This finding is consistent with the parallel trends assumption and suggests that pre-trends are unlikely to explain our results.

Turning to the post-event period, we find that the difference in *Abnormal Bias/Prc* between treated firms and matched control firms is negative in each quarter, with point estimates ranging from -0.06 to -0.20 percentage points. Ten of the twelve estimates are statistically significant at the 10% level, consistent with a permanent decline in pessimism. The decline in pessimism somewhat accelerates in event time. The difference in *Abnormal Bias/Prc* between treated firms and matched control firms is -0.10% in the first half of the post-event period and -0.15% in the second half, and the difference-in-difference of -0.05% is significant at a 10% level.

Approximately half (304) of the firms treated in 2012 are treated in quarters one and two (*Early 2012 Treated*) and half (312) in quarters three and four (*Late 2012 Treated*). The staggered intra-year treatment presents a testable prediction. In particular, in quarters three and four of 2012, we expect the bias in the sample of *Early 2012 Treated* firms, which have been on the platform in the first half of the year, to be smaller than the bias in the sample of matched control firms; but we do not expect the bias in the *Late 2012 Treated* sample to differ from the bias in the control firms sample.[19] Outside this window, we expect to find similar results for *Early 2012 Treated* and *Late 2012 Treated* firms. Both predictions are borne out in the data. In Figure 3, we find that in the last two quarters of 2012, the difference in bias between *Early 2012 Treated* firms and matched control firms is statistically and economically significant, whereas the difference in bias between *Late 2012 Treated* firms and control firms is not; moreover, the corresponding difference-in-difference estimate is also statistically significant. In contrast, we find no significant difference between *Early 2012 Treated* and *Late 2012 Treated* firms during the pre-event window, the first half of 2012, or the post-event window.

6.2 Cross-Sectional Patterns in the Decline of Pessimism

In this section, we explore whether the decline in pessimism is larger when sell-side competition is lower, earnings uncertainty is higher, and the Estimize consensus is less biased and more accurate (6.2.1), and when an industry has greater Estimize coverage (6.2.2). We also

[19] We pool quarters 1 and 2 and quarters 3 and 4 to increase statistical power. We acknowledge that the fourth quarter of 2012 would be post-event quarter +1 for firms treated in the third quarter. Excluding these firms-quarters (21% of the sample observations) slightly strengthens results.

examine whether the decline in pessimism varies systematically with broker reputation, analyst reputation, and analyst experience (6.2.3).

6.2.1 The Decline in Pessimism Conditional on Existing Competition, Earnings Uncertainty, and Estimize Bias and Accuracy

We expect that the disciplining effect of Estimize is greater when the level of existing sell-side competition is lower. Extending Gentzkow and Shapiro's (2008) argument to our setting, higher sell-side competition implies greater diversity of incentives among analysts, which in turn implies a greater likelihood of drawing an unbiased analyst/forecast. One or several analysts issuing unbiased forecasts would exert a disciplining effect on the rest, thus diminishing the value of Estimize as a disciplining device. As in Hong and Kacperczyk (2010), our measure of competition is the number of analysts covering a firm, calculated at the end of 2011.

Also, we suggest that the disciplining effect of Estimize is greater when earnings uncertainty is higher. The reason is that high uncertainty makes it difficult for investors to unravel sell-side bias on their own, increasing their demand for an external benchmark. We consider two proxies for earnings uncertainty: return volatility and earnings volatility (defined in the Appendix).

Finally, we conjecture that a less biased and more accurate Estimize consensus is more effective as a disciplining device. Investors should more easily unravel sell-side pessimism when they have access to a benchmark that is relatively less pessimistic and more accurate, which should put greater pressure on sell-side analysts to reduce their bias. More broadly, we suggest that Estimize is a greater threat to the sell-side and more likely to illicit a sell-side response when it is perceived by investors as a valuable information source – accuracy and unbiasedness are universally accepted determinants of information value. Estimize consensus bias (*Estimize Bias/Prc*) and Estimize consensus accuracy (*Estimize AbsFE*) are measured as in Table 2.[20]

Table 7 sorts treated firms into three groups based on breakpoints for the bottom 30% (*Low*), middle 40% (*Medium)*, and top 30% (*High*) for each of the five conditioning variables, and reports the difference-in-difference estimate for each group (computed as in Panel B of Table 4) and the *High-Low* spread. The results are consistent with our predictions. In particular, when existing sell-side coverage is low (high), the difference-in-difference estimate is -0.21 (-0.08) percentage points. When earnings volatility and return volatility are high, the difference-in-difference estimates are -0.16 and -0.20 percentage points, respectively; the corresponding figures for the low group are –0.07 and -0.06 percentage points, neither statistically different from zero. The spread in difference-in-difference estimates for the measures of benchmark effectiveness are also consistent with our expectations. In particular, when the Estimize consensus is most (least) biased, the difference-in-difference estimate is -0.02 (-0.13) percentage points, and when the Estimize consensus is most (least) accurate, the difference-in-difference estimate is -0.12 (-0.04) percentage points.[21] For all variables, we reject the null hypothesis of equality of difference-in-difference estimates in the top and bottom groups.

6.2.2 The Decline in Pessimism Conditional on Estimize Industry Coverage

In this section, we investigate whether increased availability of Estimize forecasts in an industry leads to lower bias. We expect a greater decline in bias for industries in which more firms

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[20] We drop post-event observations where the Estimize consensus includes less than three forecasts. The Estimize consensus is available on the Estimize platform next to the sell-side consensus and on external sites only if it includes three or more forecasts. While investors can calculate a consensus that comprises one or two individual Estimize forecasts, the location and limited availability of these forecasts hinder their usefulness as a disciplining device. Including these observations yields similar results for *Estimize Bias/Prc* but weaker results for *Estimize AbsFE*.

[21] A plausible conjecture is that the disciplining effect of Estimize is increasing in the size of the Estimize contributor base. However, in untabulated analysis we find no relation between contributor base size and the difference-in-difference in *Abnormal Bias/Prc.*

are covered by Estimize for two reasons. First, since common factors drive the earnings of all firms in the same industry, a greater ability to debias earnings forecasts for other firms and form a more accurate expectation of industry earnings should help with debiasing earnings forecasts for all firms in the industry. Second, analysts are generally viewed as industry experts, and they compete for better reputations (higher Institutional Investor ranking) and higher compensation against all analysts in their industry, which means that a decline in pessimism among other analysts in the industry will put pressure on an analyst to issue less pessimistic forecasts for all firms in the industry.[22]

Following Boni and Womack (2006) and Kadan, Madureira, Wang, and Zach (2012), we classify firms into 68 industries according to the Global Industry Classification Standard (GICS).[23] For each industry, we compute the total number of firms added to Estimize in 2012, scaled by the total number of firms in the industry as of 2012 (*Estimize Industry Coverage*). Figure 4 reports the 10 most and 10 least heavily covered industries. We observe significant variation in industry coverage, ranging between 67% and 83% in the 10 most heavily covered industries and between 0% and 27% in the least heavily covered. As expected, Estimize contributors favor industries that are more familiar and require less specialized knowledge (e.g., retail-oriented industries in the Consumer Staples, Consumer Discretionary, and Industrials sectors), and shy away from industries especially difficult to analyze (e.g., Financials) or with limited growth potential (e.g., Utilities).

We separately analyze *Treated Firms* (added to Estimize in 2012), *Control Firms* (not added to Estimize as of 2015), and, for completeness, *Late Treated Firms* (firms added to Estimize

[22] See Merkley, Michaely, and Pacelli (2017) for evidence that industry-level completion has a distinct disciplining effect.

[23] The classification scheme, well accepted in the literature as an accurate representation of how brokerage firms organize equity research (e.g., Bhojraj, Lee, and Oler, 2003; and Boni and Womack, 2006), includes 10 sectors, 24 industry groups, 68 industries, and 154 subindustries. Our results are similar when we assign firms to 24 industry groups.

after 2012). For *Control Firms*, we select a matched firm based on the propensity score model outlined in Section 4. For *Late Treated Firms*, we re-estimate the propensity score model after dropping treated firms and setting the dependent variable equal to one for late additions and zero for control firms. In each sample, we sort observations into low (bottom 30%), medium (middle 40%), and high (top 30%) levels of Estimize Industry Coverage and estimate the difference-in-difference for each group, as in Panel B of Table 4. We present the results in Table 8.

We consistently find that greater industry coverage leads to a greater decline in sell-side bias. For example, among *Treated Firms*, the difference-in-difference estimate in the top (bottom) group of *Estimize Industry Coverage* is -0.19 (-0.09), with the spread of -0.10 percentage points significant at a 5% level (Column 1).[24] Among *Late Treated Firms* and *Control Firms*, we find a statistically significant decline in bias *only* in the top group of *Estimize Industry Coverage*. Since these firms receive no Estimize coverage in 2012, these findings strongly point toward industry-level competition as a distinct mechanism through which Estimize disciplines the sell-side.

6.2.3 The Decline in Pessimism Conditional on Broker Reputation, Analyst Reputation, and Analyst Experience

Theory does not make clear-cut predictions about how analyst reputation and experience would influence analyst reaction to the arrival of Estimize. On one hand, analysts who have greater reputational capital may react more strongly to an increase in the likelihood of investors detecting analyst bias because they have more to lose; on the other hand, if these analysts' forecasts are already close to unbiased, the increase in the likelihood of detection may be negligible, resulting in a weaker reaction. More experienced (and older) analysts may be less likely to change behavior

[24] In untabulated findings, we find essentially the same spread for firms with below median Estimize firm coverage (0.10) and above median coverage (0.11), precluding the concern that our results are driven by differences in firm coverage.

in response to environmental change, consistent with evidence in psychology that more experienced workers adapt less well to changes in work settings (e.g., Niessen, Swarowsky, and Leiz, 2010). On the other hand, if greater experience signifies greater ability to understand and respond to "natural selection" forces then more experienced analysts may react more strongly to the introduction of Estimize. Although theory makes opposing predictions, documenting a clear pattern in the decline of pessimism would still be comforting.

We construct the outcome variable as the percentile ranking of individual analyst forecast errors in a firm-quarter (*Relative Bias*). This approach controls for any firm- or time-specific factors that affect forecast pessimism and is similar to the relative optimism measure developed in Hong and Kubik (2003). To minimize differences in pessimism that stem from different information sets, we exclude forecasts whose forecast horizon differs more than 45 days from the median horizon (~15% of all forecasts). We also exclude firm-quarters with less than three contributors. We match a treated broker-firm observation to a portfolio of control firms covered by the same broker and in the same quintile of *Relative Bias* in the pre-event period to address the concern that *Relative Bias* is mean reverting. We consider three proxies of broker reputation: broker size, broker accuracy, and the percentage of All-Star analysts employed by the broker; two proxies of analyst reputation: analyst accuracy and analyst All-Star rank; and two measures of analyst experience: overall experience and firm-specific experience (see the Appendix for detailed definitions).

In Table 9, we sort treated analyst-firm pairs into three groups, High (top 30%), Medium (middle 40%), and Low (bottom 30%), based on the distribution of the conditioning variable for each firm-quarter, and report the difference-in-difference estimate for each group, as well as the *High-Low* group spread. We observe that *Relative Bias* in the *High* reputation group declines more than that in the *Low* reputation group, and that this difference is statistically significant (at a 10% level) in four out of five cases. For both experience measures, we find that pessimism declines more for less experienced analysts, consistent with evidence in psychology that less experienced workers adapt better to change.

6.3 Placebo Tests – The Impact of Estimize on Bias on Longer-Horizon Earnings Forecasts and Recommendations

An alternative hypothesis is that reputational concerns or other broad forces mitigating analyst conflicts of interest strengthen for stocks in the treatment sample but not in the control sample. This hypothesis predicts a reduction in bias not only for short-term earnings forecasts, but also for longer-term earnings forecasts and investment recommendations. In contrast, if the reduction in short-term pessimism is driven by competition from Estimize, we would not expect a reduction in bias for longer-term forecasts (which account for less than 10% of all Estimize forecasts) or stock recommendations (which are not available on the Estimize platform).

To preclude the alternative hypothesis, we repeat the analysis in Panel A of Table 4 after replacing one-quarter ahead earnings (*Bias/Prc)* with *t*-quarter ahead earnings ($Bias_t/Prc$), where *t* ranges from two to five. In computing $Bias_2/Prc$ ($Bias_3/Prc$), we require that the forecast period indicator, as reported in IBES, is equal to '7' ('8'), and we limit the sample to forecasts issued 90-210 (180-300) days prior to the earnings announcement. The selection of the matched control firm is similar to Table 4, except we now also include the outcome variable of interest in our propensity score regressions.

Panels A through D of Table 10 report the results for $Bias_2/Prc$, $Bias_3/Prc$, $Bias_4/Prc$, and $Bias_5/Prc$, respectively. Consistent with prior literature, we find that earnings forecasts are more optimistic over longer horizons. For example, in the pre-event window, the average $Bias_2/Prc$

(*$Bias_5$/Prc*) is 0.00 (-0.21). There is no evidence that treatment firms experience a reduction in longer-horizon bias. In all four cases, the difference-in-difference estimate is statistically insignificant and economically small.[25]

We also examine recommendation bias, measured as the average recommendation level at the end of each quarter (*Rec Level*). In computing *Rec Level*, we convert recommendation categories, strong buy, buy, hold, sell/underperform, and strong sell, to numerical values, 1, 2, 3, 4, and 5, respectively. The results from Panel E of Table 10 indicate that *Rec Level* increases (i.e., recommendations become less optimistic) following the introduction of Estimize for both treated and matched control firms, and the difference-in-difference estimate is statistically insignificant. Overall, there is very little evidence that the introduction of Estimize is associated with a decline in sell-side analysts' tendency to issue optimistic longer-horizon earnings forecasts or investment recommendations. Thus, our findings suggest that direct competition from Estimize, rather than more pervasive economic forces, reduces short-term sell-side bias.

7. Conclusion

The last two decades have witnessed a sharp decline in information and communication costs as well as the creation of new sources of information; some of them directly competing with and potentially disrupting traditional sources of investment research. We examine whether this FinTech-engendered competition has a disciplining effect on sell-side analysts. We focus on Estimize, an open platform that crowdsources short-term quarterly earnings forecasts. Less

[25] To assess magnitudes, one must take into account that the standard deviation of *Bias/Prc* is increasing in forecast horizon. For example, the cross-sectional standard deviation of *$Bias_1$/Prc ($Bias_4$/Prc)* is about 0.33% (0.69%). Thus, the main effects documented in Table 4 are approximately 40% of the standard deviation of *$Bias_1$/Prc*, while the effects documented in Panel C are approximately 3% of the standard deviation of *$Bias_4$/Prc*.

pessimistic than sell-side forecasts but similarly accurate and readily available, Estimize forecasts present a unique opportunity for addressing this question.

We find robust evidence that sell-side analysts' tendency to issue pessimistic short-term forecasts significantly weakens for firms added to Estimize relative to a sample of matched control firms. The decline in sell-side forecast pessimism is accompanied by an increase in forecast accuracy and representativeness of the market expectation.

Several additional results point towards a causal relation between the arrival of a new competitor, Estimize, and the decline in sell-side bias. In the time-series, we find no evidence of a decline in pessimism in the three years prior to Estimize coverage, suggesting that pre-trends are unlikely to explain our findings. In the cross-section, we find that the decline in sell-side pessimism is larger when theory suggests a greater disciplining role for Estimize. In particular, the decline in pessimism is greater when 1) existing competition is lower, 2) earnings uncertainty is greater, and 3) Estimize is a more effective benchmark (i.e., more accurate and less biased). Furthermore, we show a decline in pessimism when Estimize firm coverage is absent but Estimize industry coverage is high, consistent with Estimize disciplining the sell-side by intensifying industry-level competition. Finally, placebo tests show that biases in longer-term earnings forecasts and investment recommendations – unlikely to be affected by the arrival of a short-term forecast provider – remain unchanged, indicating that broad economic forces are unlikely to be driving our results.

Our study has important policy implications. In particular, concerned with the adverse consequences of biased sell-side research such as inefficient prices and wealth transfers from less sophisticated to more sophisticated investors, in the last two decades regulators have comprehensively reformed sell-side analyst activities and communications with investment bankers and required extensive conflict of interest disclosures. These regulations have reduced analyst bias but at the cost of lower analyst coverage and lower research informativeness (Kadan et al., 2009). Our findings suggest that encouraging new forms of competition may be effective in both reducing investor reliance on the sell-side and in constraining sell-side bias, without the unintended adverse consequences of traditional regulatory approaches.

Appendix: Description of Variables

All variables are classified into three groups: forecast characteristics, firm characteristics, and brokerage and analyst characteristics.

A.1 Forecast Characteristics

- $Bias\ /\ Prc_{j,t} = \dfrac{Actual_{j,t} - Consensus_{j,t}}{Price_{j,t\text{-}1}} * 100.$ *Actual* is reported earnings. *Consensus* is the average forecasted earnings across all forecasters. *Price* is the stock price at the end of the prior year. We drop forecasts issued more than 120 days prior to the earnings announcement and use the most recent forecast for each forecaster. We winsorize *Bias/Prc* at 2.5% and 97.5%.
- *Abnormal* $Bias/Prc_{j,t}$ = the residual from a panel regression of *Bias/Prc* on the following characteristics: Log (*Size*), *Book-to-Market*, Log (*IBES Coverage*), *Turnover*, Log (*Return Volatility*), *Return*, *Forecast Age*, *Guidance*, and industry and quarter fixed effects. *Forecast Age* and *Guidance* are measured in period *t*, while all other characteristics are measured in period *t-1*.
- $Bias\ /\ AbsConsensus_{j,t} = \dfrac{Actual_{j,t} - Consensus_{j,t}}{\left|Consensus_{j,t}\right|}$. We winsorize |*Consensus*| at 0.02 and *Bias/AbsConsensus* at 2.5% and 97.5%.
- *MBE (Meet or Beat Earnings)* = a dummy variable equal to one for firms who report earnings greater than or equal to the consensus, and zero otherwise.
- *Relative Bias* = the demeaned percentile ranking of bias relative to all other analysts issuing a forecast for the same firm-quarter. This measure excludes forecasts whose forecast age differs more than 45 days from the median forecast horizon in the firm-quarter. This measure excludes firm-quarters with fewer than three total analysts.
- *AbsFE* (*Absolute Forecast Error*) = the absolute value of *Bias/Prc.*
- *Representativeness* (*Earnings Response Coefficient - ERC*) = the slope coefficient from the following time-series regression: $CAR_{j,t} = \alpha + \beta UE_{j,t} + \varepsilon_t$. C*AR* is the cumulative market-adjusted return in the three trading days around the earnings announcement date. *UE* is unexpected earnings, defined as actual earnings less forecasted earnings, scaled by price. We standardize *UE* to have mean 0 and standard deviation 1, and winsorize β at the 1^{st} and 99^{th} percentile. We also exclude firms with fewer than six quarters of Estimize forecasts.
- *Forecast Age* = the number of calendar days between the forecast issue date and the earnings announcement date, averaged across all forecasts in the consensus.
- *Rec Level* = the consensus recommendation level at the end of each quarter. Recommendations are converted to numeric values using the following scale: 1 for strong buy, 2 for buy, 3 for hold, 4 for sell/underperform, and 5 for strong sell.

- *Estimize Bias/Prc* = *Bias/Prc* of the Estimize consensus set to zero in pre-event quarters and set to missing in post-event quarters with fewer than three Estimize contributors. We winsorize *Estimize Bias/Prc* at 2.5% and 97.5%.
- *Estimize AbsFE* = the absolute value of *Estimize Bias/Prc*. This value is set to zero in pre-event quarters and set to missing in post-event quarters with fewer than three Estimize contributors.

A.2 Firm Characteristics

- *Size* = market capitalization computed as share price times total shares outstanding as of the end of the year prior to the earnings announcement date.
- *IBES Coverage* = the total number of sell-side analysts (in IBES) covering a firm in a year.
- *Book-to-Market* = the book value of equity for the most recent fiscal year prior to the earnings announcement year, scaled by market capitalization on December 31st of the same fiscal year. We winsorize *Book-to-Market* at the 1st and 99th percentile.
- *Turnover* = average daily turnover defined as share volume scaled by shares outstanding in the calendar year prior to the earnings announcement date. We winsorize turnover at the 99th percentile.
- *Return Volatility* = the standard deviation of daily returns over the calendar year prior to the earnings announcement date.
- *Return* = the average daily market-adjusted return over the calendar year prior to the earnings announcement date.
- *Guidance* = a dummy variable equal to one if the firm issues earnings guidance during the quarter.
- *Earnings Volatility* = the standard deviation of quarterly earnings scaled by price over a calendar year.
- *Estimize Industry Coverage* = the total number of firms in an industry added to Estimize in 2012, scaled by the total number of firms in the industry as of 2012. Industry classification is based on the GICS 68 industry classification.

A.3 Analyst and Broker Characteristics

- *Total Experience* = the number of years since the analyst issued their first forecast (as reported by IBES).
- *Firm-specific Experience* = the number of years since the analyst issued their first forecast for a given firm (as reported by IBES).
- *Analyst AbsFE* = the average percentile rank of relative absolute forecast errors (i.e., absolute value of *Relative Bias*) across all of the analyst's forecasts during 2011.
- *Analyst All-Star* = a dummy variable equal to one if the analyst is ranked as an All-American (first, second, third, or runner up) in the annual poll by Institutional Investor magazine in 2011.
- *Broker AbsFE* = the average percentile rank of relative absolute forecast errors (i.e., absolute value of *Relative Bias*) across all the brokerage firm's forecasts in 2011.

- *Broker All-Stars* = the percentage of brokerage analysts ranked as an All-American (first, second, third, or runner up) in the annual poll by Institutional Investor magazine in 2011.
- *Broker Size* = the total number of analysts working for the firm as of the end of 2011.

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Table 1: Estimize Summary Statistics

This table reports summary statistics for forecasts submitted on Estimize from January 2012 to December 2015. Panel A reports the breadth and depth of Estimize coverage across the four years in the sample. Panel B partitions Estimize firms into five groups based on the year in which the firm was first added to Estimize and reports summary statistics for each group. The sample includes 1,842 firms with 1) continuous sell-side coverage from 2009-2015, 2) a stock price of at least $5 at the end of 2011, and 3) non-missing book value of equity at the end of 2011.

Panel A: Breadth and Depth of Estimize Coverage

Year	Firms Covered	Firm-Quarters	Contributors	Forecasts	Contributors per Firm-Quarter: Mean	Contributors per Firm-Quarter: Median	Average Firms Followed
All (2012-2015)	1,391	15,120	11,167	172,566	9.05	4	8.06
2012	772	1,694	1,370	13,007	6.61	3	6.42
2013	1,271	3,781	1,612	24,750	5.88	3	9.67
2014	1,326	4,634	2,167	44,457	7.88	3	10.61
2015	1,362	5,011	7,555	90,352	13.82	6	7.05

Panel B: Characteristics of Firms Covered by Estimize

	Observations	Contributors Per Firm Quarter: Average	Contributors Per Firm Quarter: Median	% Quarters with Coverage	Average Firm Characteristics: IBES Coverage	Average Firm Characteristics: Market Cap ($Bil)	Average Firm Characteristics: Book-to-Market
2012 Additions	772	11.70	6.25	90.02%	20.17	18.62	0.41
2013 Additions	509	2.53	2.09	75.87%	12.35	3.71	0.53
2014 Additions	74	1.66	1.46	48.09%	9.14	2.24	0.43
2015 Additions	36	1.02	0.42	12.50%	8.11	1.20	0.47
Not on Estimize	451	0.00	0.00	0.00%	7.96	2.54	0.58

Table 2: A Comparison of Estimize and IBES Quarterly Forecasts

This table examines key attributes of Estimize and IBES consensus forecasts. In computing a consensus, we limit the sample to earnings forecasts issued within 120 calendar days of the earnings announcement and use the most recent forecast by a contributor or an analyst. We also exclude forecasts flagged as unreliable by Estimize. We report mean and median attribute values, as well as the percentage of times that the Estimize value exceeds the IBES value. Forecast attributes are defined in the Appendix. The sample is limited to the 772 firms that were added to Estimize in 2012. The sample includes 8,265 firm-quarters over the 2013-2015 period.

	Estimize Mean	Estimize Median	Sell-Side Mean	Sell-Side Median	% Estimize > Sell-Side
Forecasters Per Stock	12.64	6.00	14.83	14.00	23.91%
Forecast Age	9.71	6.33	63.82	66.76	1.37%
Bias/Prc	0.00	0.01	0.06	0.04	19.18%
Bias/AbsConsensus	-1.36	0.80	5.51	3.19	17.57%
MBE	55.81%	100.00%	70.02%	100.00%	-
AbsFE	0.17	0.08	0.16	0.08	45.15%

Table 3: Characteristics of Treated and Control Firms

This table compares the characteristics of treated firms and control firms. Treated firms are those added to the Estimize platform in 2012. Candidate control firms are those not added to Estimize as of 2015. We match each treated firm to a control firm with the most similar probability of being treated (i.e., a matched control firm). We obtain estimates of the probability of being treated from a logistic regression in which the dependent variable is a dummy variable equal to one for treated firms and zero for control firms, and the independent variables are Log (*Size*), *Book-to-Market*, *Turnover*, Log (*IBES Coverage*), *Bias/Prc*, and *AbsFE*. We measure firm characteristics at the end of 2011 and forecast characteristics as quarterly averages over the period 2009-2011. Detailed definitions of all variables appear in the Appendix. Panel A reports average firm and forecast characteristics for the 772 treated firms and 480 candidate control firms. It also reports the difference in means and the t-statistics from the test of difference in means. Panel B reports analogous results for treated firms and its corresponding matched control firm. Panel B excludes 156 observations where the absolute difference in the propensity scores of the treated and matched control firms exceeds 0.50%, resulting in 616 treated firms.

Panel A: Characteristics of Treated Firms and Candidate Control Firms

	Treated	Candidate Control	Treated - Control	t(Treated - Control)
Log *(Size)*	15.25	13.26	1.99	(23.23)
Log *(IBES Coverage)*	2.73	1.78	0.95	(23.09)
Book-to-Market	0.42	0.73	-0.31	(-16.97)
Turnover	12.23	7.17	5.06	(12.74)
Return Volatility	2.17	2.61	-0.44	(-9.16)
Return	0.05	0.06	-0.01	(-0.72)
Bias/Prc	0.14	0.03	0.11	(5.94)
AbsFE	0.33	0.71	-0.38	(-16.68)
Propensity Score	80.54	31.30	49.24	(32.72)

Panel B: Characteristics of Treated Firms and Matched Control Firms

	Treated	Matched Control	Treated - Matched	t(Treated - Matched)
Log *(Size)*	15.00	15.11	-0.11	(-0.44)
Log *(IBES Coverage)*	2.61	2.58	0.03	(0.31)
Book-to-Market	0.45	0.48	-0.03	(-0.90)
Turnover	11.22	12.96	-1.74	(-1.21)
Return Volatility	2.17	2.37	-0.20	(-1.23)
Return	0.06	0.05	0.01	(0.86)
Bias/Prc	0.14	0.10	0.05	(1.65)
AbsFE	0.35	0.37	-0.02	(-0.49)
Propensity Score	77.38	77.38	0.00	(0.00)

Table 4: The Effect of Estimize Coverage on Bias

This table examines sell-side bias before (from 2009 to 2011) and after the arrival of Estimize (from 2013 to 2015) for treated and matched control firms. Treated firms are those added to the Estimize platform in 2012. Candidate control firms are those not added to Estimize as of 2015. We match each treated firm to a candidate control firm with the most similar probability of being treated, estimated with a logistic regression in which the dependent variable is a dummy variable equal to one for treated firms and zero for candidate control firms, and the independent variables are Log (*Size*), *Book-to-Market*, *Turnover*, Log (*IBES Coverage*), *Bias/Prc*, and *AbsFE*. The sample includes 616 treated firms and 14,245 treated firm-quarters. Panels A and B report mean *BIAS/Prc* and *Abnormal BIAS/Prc*, respectively. *BIAS/Prc* is the difference between actual earnings and the consensus IBES forecast, scaled by stock price at the end of the previous year (and multiplied by 100). *Abnormal Bias/Prc* is the residual from a panel regression of *BIAS/Prc* on control variables (*Size*, *Book-to-Market*, *IBES Coverage*, *Turnover*, *Return Volatility*, *Return*, *Forecast Age*, *Guidance*, and industry and time fixed effects). All variables are defined in the Appendix. Reported t-statistics are based on standard errors double-clustered by matched control firm and quarter.

Panel A: Bias/Prc

	Before	*After*	*Difference*	*t(Dif.)*
Estimize	0.14	0.04	-0.10	(-4.22)
Matched Control	0.10	0.13	0.03	(0.86)
Estimize - Control	0.05	-0.09	-0.13	(-3.79)

Panel B: Abnormal Bias/Prc

	Before	*After*	*Difference*	*t(Dif.)*
Estimize	0.03	-0.02	-0.04	(-3.67)
Matched Control	0.02	0.10	0.08	(2.33)
Estimize - Control	0.00	-0.12	-0.12	(-3.53)

Table 5: The Effects of Estimize Coverage on Bias – Robustness

This table examines sell-side bias before (from 2009 to 2011) and after the arrival of Estimize (from 2013 to 2015) for treated and matched control firms using alternative specifications. Row 1 reports the baseline difference-in-difference results for *Bias/Prc* and *Abnormal Bias/Prc* as reported in Panels A and B of Table 4, respectively. Row 2 repeats the baseline analysis after including the 156 treated firms dropped in the main analysis due to lack of common support. Row 3 identifies a control firm using portfolio matching rather than propensity score matching. The portfolio-matched control firm must have (1) the same size quintile and book-to-market quintile as the treated firm, based on breakpoints estimated at the end of 2011, and (2) the smallest difference in pre-event period bias from the treated firm. Rows 4 and 5 repeat the baseline analysis after replacing *Bias/Prc* with two alternative measures of bias: *Bias/AbsConsensus* and *MBE*. In Row 6, we demean *Bias/Prc* by the average *Bias/Prc* of a given analyst-firm pair over the sample period. Row 7 repeats the baseline analysis after redefining treated firms as firms added to Estimize in 2013 and re-defining the post-event window as 2014-2015. Rows 8 repeats the baseline analysis for a subsample of firm-quarters without management guidance, and Row 9 reports the results for the subsample of sell-side forecasts issued more than 60 days prior to the earnings announcement. The reported t-statistics are computed based on standard errors double-clustered by control firm and quarter.

	Bias	*Abnormal Bias*
1. Baseline Results	-0.13	-0.12
	(-3.79)	(3.53)
Alternative Matching Approaches		
2. Propensity Score Matching –No Common Support	-0.15	-0.13
	(-3.89)	(-3.62)
3. Portfolio Matching	-0.10	-0.10
	(-2.23)	(-2.65)
Alternative Measures of Bias		
4. Bias/AbsConsensus	-0.18	-0.18
	(-3.29)	(-3.24)
5. Meet or Beat	-0.09	-0.09
	(-1.98)	(-1.87)
6. Bias/Prc with Analyst-Firm Fixed Effects	-0.09	-0.09
	(-2.36)	(-2.21)
Alternative Treatment Samples		
7. 2013 Additions (2014-2015 post period)	-0.05	-0.04
	(-1.22)	(-1.06)
Alternative Subsamples		
8. Drop Firm-Quarters with Management Guidance	-0.15	-0.14
	(-3.99)	(-3.76)
9. Drop Forecasts where Forecast Age ≤ 60 days	-0.16	-0.16
	(-3.61)	(-3.57)

Table 6: The Effect of Estimize Coverage on Accuracy and Representativeness

This table examines sell-side forecast accuracy and representativeness before (from 2009 to 2011) and after the arrival of Estimize (from 2013 to 2015) for treated and matched control firms. Treated firms are those added to the Estimize platform in 2012. Candidate control firms are those not added to Estimize as of 2015. We match each treated firm to a candidate control firm with the most similar probability of being treated, estimated with a logistic regression in which the dependent variable is a dummy variable equal to one for treated firms and zero for control firms, and the independent variables are Log (*Size*), *Book-to-Market*, *Turnover*, Log (*IBES Coverage*), *Bias/Prc*, and *AbsFE*. Accuracy is inversely related to the absolute value of the consensus forecast error (*AbsFE*), and *Representativeness* is the earnings response coefficient from a firm-specific earnings-return regression. See the Appendix for details. The sample in Panel A includes 616 treated firms and 14,245 firm-quarter observations. The sample in Panel B includes 599 treated firms and 1,198 firm observations. Reported t-statistics are based on standard errors that are double-clustered by control firm and quarter in Panel A and clustered by control firm in Panel B.

Panel A: Accuracy (*AbsFE*)

	Before	*After*	*Difference*	*t(Dif.)*
Estimize	0.34	0.21	-0.13	(-3.67)
Matched Control	0.37	0.34	-0.03	(-0.74)
Estimize - Control	-0.03	-0.13	-0.10	(-3.11)

Panel B: Representativeness (ERCs)

	Before	*After*	*Difference*	*t(Dif.)*
Estimize	2.71	4.98	2.27	(6.44)
Matched Control	2.62	2.87	0.25	(0.30)
Estimize - Control	0.09	2.11	2.03	(5.21)

Table 7: The Effect of Estimize Coverage on Bias Conditional on Competition, Earnings Uncertainty, and Benchmark Effectiveness

This table reports the mean difference-in-difference estimates of *Abnormal Bias/Prc* conditional on the level of existing sell-side competition, measured as the number of sell-side analysts covering the firm in 2011 (*IBES Coverage*); earnings uncertainty, measured as the standard deviation of actual earnings scaled by price (*Earnings Volatility*) during 2011 or the standard deviation of daily returns (*Return Volatility*) in 2011; and Estimize effectiveness as a benchmark, defined as bias (*Estimize Bias/Prc)* or accuracy (*Estimize AbsFE)* of the Estimize consensus, both estimated in the prior quarter. The value of a conditioning variable is *High*, *Medium*, or *Low*, if it is in the top 30%, middle 40%, or bottom 30%, respectively. The reported t-statistics (in parentheses) are computed based on standard errors double-clustered by control firm and quarter.

	Competition	***Earnings Uncertainty***		***Benchmark Effectiveness***	
	IBES Coverage	*Earnings Volatility*	*Return Volatility*	*Estimize Bias/Prc*	*Estimize AbsFE*
3 (High)	-0.08	-0.16	-0.20	-0.02	-0.04
	(-1.37)	(-3.71)	(-3.57)	(-0.53)	(-1.04)
2	-0.09	-0.14	-0.11	-0.12	-0.11
	(-2.17)	(-3.12)	(-3.11)	(-3.35)	(-3.09)
1 (*Low*)	-0.21	-0.07	-0.06	-0.13	-0.12
	(-4.85)	(-1.58)	(-1.52)	(-3.68)	(-3.68)
High - Low	0.13	-0.09	-0.14	0.12	0.08
	(1.97)	(-2.28)	(-3.25)	(3.53)	(2.59)

Table 8: The Effect of Estimize Coverage on Bias Conditional on Estimize Industry Coverage

This table reports the mean difference-in-difference estimate of *Abnormal Bias/Prc* conditional on *Estimize Industry Coverage*, defined as the total number of firms in an industry added to Estimize in 2012, scaled by the total number of firms in the industry in 2012. Industry classification is based on the GICS 68 industry grouping. We report results separately for firms added to Estimize in 2012 (*Treated Firms*), firms added to Estimize after 2012 but before 2015 (*Late Treated Firms*), and firms not yet added to Estimize as of the end of 2015 (*Control Firms*). *Estimize Industry Coverage* can be *Low* (bottom 30%), *Medium* (middle 40%), or *High* (top 30%). The reported t-statistics (in parentheses) are computed based on standard errors double-clustered by control firm and quarter.

	Treated Firms	*Late Treated Firms*	*Control Firms*
3 (High)	-0.19	-0.14	-0.14
	(-4.61)	(-3.03)	(-2.24)
2	-0.10	-0.03	0.11
	(-2.51)	(-0.77)	(1.91)
1 (Low)	-0.09	-0.02	0.06
	(-1.79)	(-0.53)	(0.96)
High - Low	-0.10	-0.12	-0.20
	(-2.31)	(-2.55)	(-2.28)

Table 9: The Effect of Estimize Coverage on Individual Analyst Bias Conditional on Broker Reputation, Analyst Reputation, and Analyst Experience

This table reports the mean difference-in-difference estimates of individual analyst bias conditional on measures of broker reputation, analyst reputation, and analyst experience. We measure individual analyst bias as the percentile rank of bias across all analysts issuing a forecast for the firm-quarter (*Relative Bias*). We match each treated broker-firm observation to a portfolio of control firms in the same brokerage house that are in the same quintile of *Relative Bias* in the pre-event window. The difference-in-difference is computed as the *Relative Bias* of treated firms less the *Relative Bias* of matched control firms during 2013-2015 less the corresponding difference during 2009-2011. We measure broker reputation as 1) *Broker Size* –the total number of analysts working for the firm at the end of 2011, 2) *Broker AbsFE* – the broker's percentile absolute forecast error rank, calculated as the average of all broker-issued forecasts in 2011, and 3) *Broker All-Stars* – the percentage of broker analysts ranked as an All-American by Institutional Investor magazine in 2011. We measure analyst reputation as either 1) *Analyst AbsFE* - the analyst percentile absolute forecast error rank, calculated as the average of all the analyst's forecasts issued in 2011, or 2) *Analyst All-Star* – a dummy variable equal to one if the analyst is ranked as an All-American by Institutional Investor magazine in 2011. We measure analyst experience as either 1) *Total Experience* – the number of years since the analyst issued their first forecast or 2) *Firm-specific Experience* - the number of years since the analyst issued their first forecast for a given firm. The table reports the mean difference-in-difference estimates of *Relative Bias* after partitioning observations into three groups based on breakpoints for the bottom 30% (*Low),* middle 40%, and top 30% (*High*) of the variable of interest for a firm-quarter. The reported t-statistics (in parentheses) are computed based on standard errors double-clustered by control group and quarter.

	Broker Reputation			**Analyst Reputation**		**Experience**	
	Broker Size	*Broker AbsFe*	*Broker All Star*	*Analyst AbsFE*	*All-Star Dummy*	*Total Experience*	*Firm Experience*
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
3 (High)	-1.07	1.45	-0.95	1.68	-0.74	0.58	0.43
	(-1.62)	(2.10)	(-1.12)	(2.09)	(-1.44)	(1.12)	(1.17)
2	0.05	0.58	0.07	0.40		0.23	0.01
	(0.11)	(1.48)	(0.17)	(1.24)		(0.84)	(0.04)
1 (low)	1.02	-1.62	1.38	-1.69	0.23	-0.87	-0.50
	(1.59)	(-1.99)	(2.28)	(-2.09)	(0.77)	(-1.86)	(-1.32)
High - Low	-2.08	3.08	-2.34	3.36	-0.96	1.45	0.93
	(-1.90)	(2.42)	(-1.97)	(2.42)	(-1.62)	(2.35)	(1.78)

Table 10: The Effect of Estimize Coverage on Bias in Longer-Horizon Forecasts and Recommendations
This table examines bias in sell-side analysts' longer-horizon earnings forecasts and investment recommendations before and after the arrival of Estimize in 2012. We use the difference-in-difference approach of Panel A of Table 4, except we now define the outcome variable as the bias in two- to five-quarter ahead consensus earnings forecasts (Panels A through D) or the consensus recommendation (Panel E). We augment the propensity score model used to select the matched control firm to include the corresponding outcome variable. We convert recommendations to numeric values as follows: 1 for strong buy, 2 for buy, 3 for hold, 4 for sell/underperform, and 5 for strong sell. The reported t-statistics are based on standard errors double-clustered by control firm and quarter.

Panel A: Two-Quarter Ahead Earnings				
	Before	*After*	*Difference*	*t(Dif)*
Estimize	0.00	-0.08	-0.08	(-1.26)
Matched Control	0.02	0.00	-0.02	(-0.30)
Estimize - Control	-0.02	-0.08	-0.06	(-1.10)
Panel B: Three-Quarter Ahead Earnings				
	Before	*After*	*Difference*	*t(Dif)*
Estimize	-0.11	-0.16	-0.05	(-0.47)
Matched Control	-0.02	-0.04	-0.02	(-0.26)
Estimize - Control	-0.09	-0.12	-0.03	(-0.47)
Panel C: Four-Quarter Ahead Earnings				
	Before	*After*	*Difference*	*t(Dif)*
Estimize	-0.19	-0.21	-0.02	(-0.15)
Matched Control	-0.12	-0.16	-0.04	(-0.41)
Estimize - Control	-0.06	-0.04	0.02	(0.26)
Panel D: Five-Quarter Ahead Earnings				
	Before	*After*	*Difference*	*t(Dif)*
Estimize	-0.21	-0.26	-0.05	(-0.36)
Matched Control	-0.21	-0.26	-0.05	(-0.39)
Estimize - Control	0.00	0.00	0.00	(0.00)
Panel E: Recommendation Level				
	Before	*After*	*Difference*	*t(Dif)*
Estimize	2.25	2.35	0.10	(4.67)
Matched Control	2.32	2.39	0.07	(1.66)
Estimize - Control	-0.07	-0.04	0.03	(0.56)

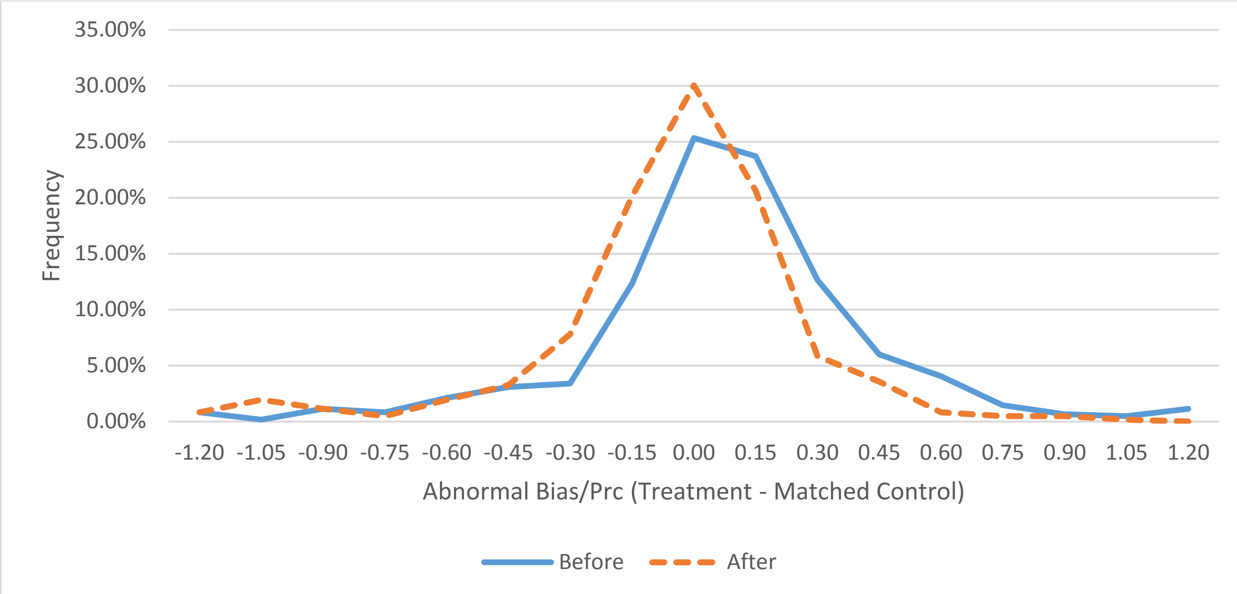

Figure 1: Distribution of the Difference in Bias of Treatment and Control Groups Before and After Estimize

This figure plots the distribution of *Abnormal Bias/Prc* of treated firms less matched control firms before (from 2009-2011) and after (from 2013-2015) the introduction of Estimize. Treated firms are those added to the Estimize platform in 2012. Candidate control firms are those not added to Estimize as of 2015. We match each treated firm to a candidate control firm with the most similar probability of being treated, estimated with a logistic regression in which the dependent variable is a dummy variable equal to one for treated firms and zero for candidate control firms, and the independent variables are Log (*Size*), *Book-to-Market*, *Turnover*, Log (*IBES Coverage*), *Bias/Prc*, and *AbsFE*. The sample includes 616 treated firms and 14,245 treated firm-quarters. *BIAS/Prc* is the difference between actual earnings and the consensus IBES forecast, scaled by stock price at the end of the previous year (and multiplied by 100). *Abnormal Bias/Prc* is the residual from a panel regression of *BIAS/Prc* on control variables (*Size*, *Book-to-Market*, *IBES Coverage*, *Turnover*, *Return Volatility*, *Return*, *Forecast Age*, *Guidance*, and industry and time fixed effects).

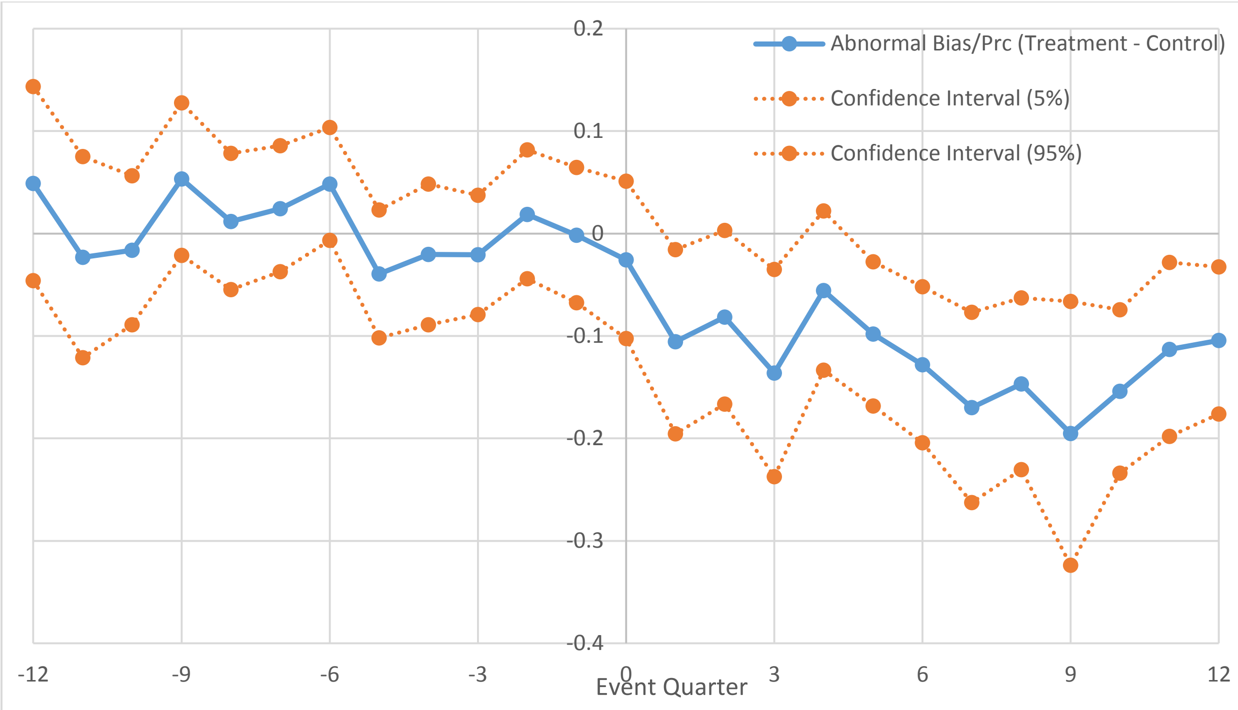

Figure 2: Differences in Bias in Event Time

This figure plots the difference in *Abnormal Bias/Prc* between treated and matched control firms from quarters -12 to +12, where quarter 0 is the quarter in which the firm is first added to Estimize. Treated firms are those added to the Estimize platform in 2012. Candidate control firms are those not added to Estimize as of 2015. We match each treated firm to a candidate control firm with the most similar probability of being treated, estimated with a logistic regression in which the dependent variable is a dummy variable equal to one for treated firms and zero for candidate control firms, and the independent variables are Log *(Size)*, *Book-to-Market*, *Turnover*, Log (*IBES Coverage*), *Bias/Prc*, and *AbsFE*. *BIAS/Prc* is the difference between actual earnings and the consensus IBES forecast, scaled by stock price at the end of the previous year (and multiplied by 100). *Abnormal Bias/Prc* is the residual from a panel regression of *BIAS/Prc* on control variables (*Size*, *Book-to-Market*, *IBES Coverage*, *Turnover*, *Return Volatility*, *Return*, *Forecast Age*, *Guidance*, and industry and time fixed effects). The sample includes 616 treated firms. The dotted orange lines plot the 90% confidence interval based on standard errors clustered by control firm.

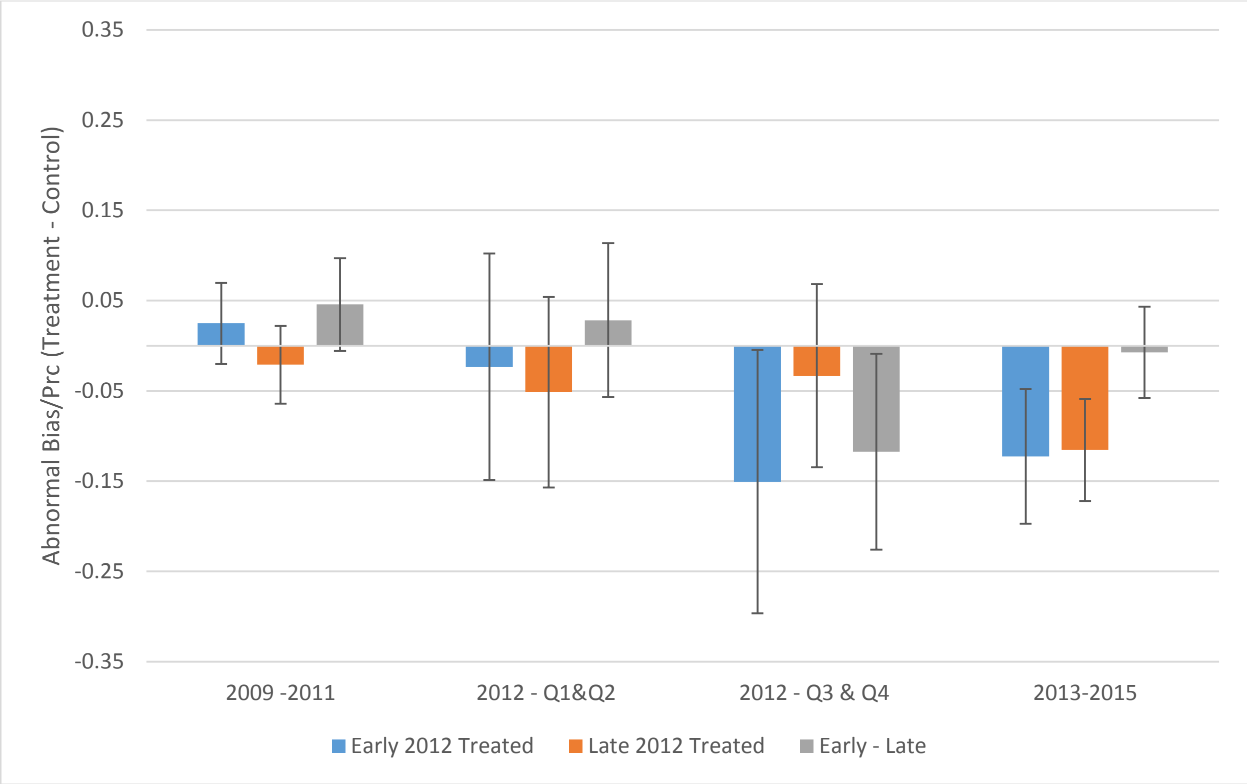

Figure 3: Differences in Bias – Early 2012 Treated versus Late 2012 Treated

This figure plots the difference in *Abnormal Bias/Prc* (as defined in Table 4) of treated firms less matched control firms during 2009-2011 ("before"), the first half of 2012, the second half of 2012, and 2013-2015 ("after"). The table partitions treated firms into the 304 treated firms added to Estimize in the first half of 2012 (*Early 2012 Treated*) and the 312 treated firms added to Estimize in the second half of 2012 (*Late 2012 Treated*). Treated firms are those added to the Estimize platform in 2012. Candidate control firms are those not added to Estimize as of 2015. We match each treated firm to a candidate control firm with the most similar probability of being treated, estimated with a logistic regression in which the dependent variable is a dummy variable equal to one for treated firms and zero for candidate control firms, and the independent variables are Log *(Size)*, *Book-to-Market*, *Turnover*, Log (*IBES Coverage*), *Bias/Prc*, and *AbsFE*. *BIAS/Prc* is the difference between actual earnings and the consensus IBES forecast, scaled by stock price at the end of the previous year (and multiplied by 100). *Abnormal Bias/Prc* is the residual from a panel regression of *BIAS/Prc* on control variables (*Size*, *Book-to-Market*, *IBES Coverage*, *Turnover*, *Return Volatility*, *Return*, *Forecast Age*, *Guidance*, and industry and time fixed effects).The error bars report the 90% confidence intervals based on standard errors clustered by control firm.

Figure 4: Estimize Industry Coverage - Most and Least Popular Industries

This figure reports *Estimize Industry Coverage* for the 10 industries most heavily covered by Estimize (Panel A) and the 10 industries least heavily covered by Estimize (Panel B). We classify firms into 68 industries (across 11 sectors) using the GICS industry definitions. For each industry, we compute *Estimize Industry Coverage* as the number of firms added to Estimize in 2012, scaled by the total number of firms in the industry as of 2012.

Panel A: 10 Industries with Highest Estimize Coverage

Rank	Sector	Industry	Estimize Industry Coverage
1	Industrials	Industrial Conglomerates	83.33%
2	Consumer Staples	Food & Staples Retailing	81.82%
3	Consumer Staples	Beverages	77.78%
4	Consumer Discretionary	Multiline Retail	75.00%
5	Consumer Discretionary	Specialty Retail	73.44%
6	Consumer Staples	Food Products	70.37%
7	Consumer Discretionary	Consumer Services	68.75%
8	Materials	Chemicals	68.00%
9	Industrials	Capital Goods	67.86%
10	Healthcare	Health Care Technology	66.67%

Panel B: 10 Industries with Lowest Estimize Coverage

Rank	Sector	Industry	Estimize Industry Coverage
1	Financials	Thrifts & Mortgage Finance	0.00%
2	Financials	Banks	5.63%
3	Financials	Insurance	6.98%
4	Real Estate	Equity REITs	8.62%
5	Utilities	Water Utilities	11.11%
6	Materials	Paper & Forest Products	14.29%
7	Telecom	Wireless Telecommunication Services	14.29%
8	Utilities	Gas Utilities	21.43%
9	Telecom	Diversified Telecommunication Services	23.08%
10	Healthcare	Biotechnology	26.67%