Does Crowdsourced Research Discipline Sell-Side Analysts?

Russell Jame, Stanimir Markov, and Michael Wolfe*

December 2018

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 a reduction in short-term forecast bias relative to matched control firms. Cross-sectional results consistently favor the disciplining hypothesis over the alternative that sell-side analysts use Estimize forecasts to improve their own forecasts. For example, we find a greater reduction in bias when the consensus includes a larger fraction of forecasts by analysts who enjoy close, mutually beneficial relationships with management but not when conditions conducive to use of Estimize forecasts exist. Finally, we find no change in bias for longer-horizon forecasts or investment recommendations, suggesting competition from Estimize rather than broad economic forces accounts for our results.

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

---

* Jame is from the Gatton College of Business and Economics, University of Kentucky, russell.jame@uky.edu. Markov is from the Cox School of Business, Southern Methodist University, smarkov@mail.smu.edu. Wolfe is from the Pamplin College of Business, Virginia Tech, mcwolfe@vt.edu. We thank Andy Call, Michael Chin, Pouyan Ghazizadeh, Clifton Green, Stephan Hollader, Patrick Hopkins, Xing Huang, Julian Kolev, Roby Lehavy, Mario Schabus, Christoph Sextroh, Rick Sen, David Veenman, and seminar participants at Arizona State University, Baruch College, Rutgers University, the Southern Methodist University, Tilburg University, University of Amsterdam, University of Illinois at Chicago, University of Kentucky, University of New South Wales, University of North Carolina at Charlotte, University of South Florida, University of Technology Sydney, West Virginia University, the 2017 AAA Annual Meeting, the 2017 Colorado Summer Accounting Conference, the 2017 Catolica|Nova Lisbon Accounting Conference, and the 2017 Financial Intermediation Research Society Conference for helpful comments, and Leigh Drogen and Josh Dulberger from Estimize for providing the data and answering our questions.

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. Consistent with sell-side analysts succumbing to management pressures, analysts have been shown to bias their research to acquire investment banking deals (e.g., Lin and McNichols, 1998), obtain valuable information (Ke and Yu, 2006), or subsequently get hired by the firm (e.g., Lourie, 2018). While biased research may at times be beneficial to some investors, the preponderance of the evidence suggests substantial costs in the form of market mispricing (Dechow and Sloan, 1997; Veenman and Verwijmeren, 2018) and wealth transfers from less sophisticated to more sophisticated investors (Malmendier and Shantikumar, 2007; De Franco, Lu, and Vasvari, 2007).[1] Consequently, a large literature in accounting, finance, and economics explores the forces moderating bias: reputational concerns (Fang and Yasuda, 2009), regulation (e.g., Kadan, Madureira, Wang, and Zach, 2009), and analyst competition (Hong and Kacperczyk, 2010).

In this study, we turn attention to a new phenomenon, crowdsourced investment research. 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 stocks to large networks of

[1] We acknowledge that not all studies find evidence of analyst research being biased and detrimental to market efficiency and investor welfare (see a survey by Mehran and Stulz (2007)).
[2] As defined in Philippon (2016), FinTech includes "technology-enabled business model innovations in the financial sector" (p. 15).

people. Crowdsourced research conveys new information to capital markets (Jame, Johnston, Markov, and Wolfe, 2016; Chen, De, Hu, and Hwang, 2014) and is less biased than sell-side research (Jame et al., 2016), raising the possibility that it exerts a disciplining effect by making it easier for investors to identify and penalize biased analysts. Given how often FinTech competition is touted in the popular press as an important disciplining mechanism,[3] an investigation of its effects on the incumbents seems long overdue (Philippon, 2016).

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 (Jame et al., 2016), 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).[4] 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

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

(2009-2011). We measure pessimism as 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, sell-side coverage, turnover, and the bias and accuracy of short-term forecasts.

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, select control firms using the coarsened exact matching or entropy balancing methods, or use alternative measures of pessimism (e.g., meet or beat indicator). We also document a leftward shift in the entire distribution of forecast pessimism, suggesting the decline in pessimism is widespread. In addition, 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 at the 10% level in ten quarters.

Sell-side analysts compete not only at the firm-level but also at the industry level (Merkeley, Michaely, and Pacelli, 2017), prompting us to explore whether Estimize influences sell-side bias by increasing competition at the industry level. We find a significantly greater decline in pessimism for firms in industries with greater Estimize coverage. This effect holds even for firms that lack any Estimize coverage, consistent with heightened industry-level competition being a distinct channel through which Estimize operates.

We present a series of results that favor the disciplining hypothesis over the alternative explanation that analysts use Estimize forecasts to improve their own forecasts. First, we document a stronger decline in bias when the consensus includes more forecasts by analysts in greater need of disciplining (those issuing favorable investment recommendations, working at an affiliated brokerage firm, asking questions on conference calls, or hosting the firm at a broker conference), but not when conditions conducive to use of Estimize forecasts exist (when sell-side analysts have access to current quarter Estimize forecasts or stronger incentives to use past quarter Estimize forecasts). Second, we show that sell-side accuracy increases *only* in the top tercile of pre-Estimize bias, consistent with the disciplining hypothesis but not the information hypothesis which predicts accuracy improvement even when pre-Estimize forecasts are unbiased. Finally, we find that sell-side analysts who have a history of pessimistic forecasts are more likely to reduce their coverage of treated firms, where forecast bias is more easily exposed, relative to control firms – a prediction made only by the disciplining hypothesis.

Another alternative hypothesis is that Estimize coverage is correlated with broad unobservable forces that steer sell-side analysts toward less biased research in general (e.g., by increasing reputation costs or reducing dependence on management for information). While this hypothesis predicts less optimistic longer horizon forecasts and stock recommendations, our hypothesis predicts only a decline in short-term pessimism. Placebo tests yield no evidence that longer horizon forecasts and stock recommendations become less optimistic.

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 et al., 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; Farrell, Green, Jame, and Markov, 2018), 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). Gentzkow 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 19^{th} century reduces newspaper bias.

2. Background and Hypothesis Development

2.1. Sell-Side Bias and the Need for Disciplining It

Managers desire favorable sell-side coverage and they have the means to shape analyst incentives. Consistent with the sell-side succumbing to management pressures, there is evidence that analysts issue optimistic long-term earnings forecasts and recommendations, and that this optimism is explained by analyst incentives to acquire investment banking deals (e.g., Lin and McNichols, 1998; Michaely and Womack, 1999), obtain valuable information (e.g., Francis and Philbrick, 1993; Das, Levine, and Sivaramakrishnan, 1998), or subsequently get hired by the firm they cover (Horton, Serafeim, and Wu, 2017; Lourie, 2018). Managers also desire a low, beatable earnings benchmark,[5] potentially creating incentives for analysts to issue low, easy to meet or beat short-term forecasts. Prior evidence suggests that analysts switch from long-term optimism to short-term pessimism, and that this forecasting behavior is rewarded by management with information (e.g., Ke and Yu, 2006; Feng and McVay, 2010) and better career opportunities (Horton, Sefareim, and Wu, 2017).[6]

The case for the undesirability of analyst bias largely rests on extant empirical evidence that investors cannot fully unravel bias, resulting in suboptimal trading and inefficient prices. For instance, Malmendier and Shanthikumar (2007) show that while large traders tend to discount buy recommendations from affiliated analysts, small traders tend to interpret the buy recommendations literally. Analyzing a small sample of stocks where analysts were found to have issued misleading

[5] 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.

[6] An alternative explanation is that analyst bias is behavioral rather than strategic. That is, analysts are simply incapable of anticipating and adjusting for discretionary accounting choices and expectations management. However, it is unclear why this analyst naiveté would be rewarded with management access.

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). Evidence that predictable analyst forecast errors and forecast optimism are inefficiently priced is reported in Dechow and Sloan (1997), Dechow, Hutton, and Sloan (2000), and So (2013), among others. More recently, Veenman and Verwijmeren (2018) show that short-term analyst pessimism is also inefficiently priced: that is, greater predictable pessimism is associated with greater (more positive) future earnings announcement returns.[7]

2.2 Factors Moderating Sell-Side Bias

Factors that moderate analyst bias 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 analyst compensation. A string of reforms, passed after the dotcom bubble burst, aim to increase analyst independence from investment bankers (e.g., NASD Rule 2711, NYSE Rule 472, and the Global Settlement). Evidence suggests these reforms have reduced but not fully eliminated

[7] When bias facilitates the flow of information from managers to analysts, analyst forecasts incorporate relatively more information, potentially benefiting investors. The actual benefits, however, still depend on investor ability to debias analyst forecasts, which many investors lack (Veenman and Verwijmeren, 2018). When bias does not facilitate the flow of information from managers to analysts, investors are only harmed. Anecdotally, neither investors nor analysts believe bias is on the whole beneficial to investors. According to Institutional Investor surveys, analyst integrity and professionalism – attributes antithetical to bias – are in the top three most desirable analyst attributes, ahead of management access, each year in the period from 2001 to 2011. According to Brown, Call, Clement, and Sharp's (2015) analyst survey (p. 4), reduced credibility with investing clients is a cost of issuing biased forecasts, consistent with bias benefitting the analyst but not her clients.

analysts' propensity to issue biased research (Barber et al., 2006; Kadan et al., 2009). More recently, Dambra, Field, Gustafson, and Pisciotta (2018) find that reforms that allow analysts to increase their level of participation in the IPO process (The JOBS Act of 2012, specifically) increase analysts' propensity to issue biased research.

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.[8] 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). Bank reputation, too, can exert a disciplining effect. Altinkilic, Balashov, and Hansen (2018) find that in the post-reforms era, bias leads to worse career outcomes, especially for analysts employed at more reputable banks, consistent with more reputable banks more effectively monitoring their analysts.

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

[8] An implicit assumption – corroborated in prior evidence – is that analysts have incentives to acquire a reputation for being accurate. Prior evidence suggests analysts who issue more accurate forecasts are more likely to be voted as all-stars (Stickel, 1992), more likely to be promoted to higher-status brokerage firms or hedge funds (Hong and Kubik, 2003; Cen, Orthanalai, and Schiller, 2017), and less likely to lose their jobs (Mikhail, Walther, and Willis, 1999); furthermore, more accurate research generates larger price reactions (Park and Stice, 2000; Chen, Francis, and Jiang, 2005) and greater trading commissions (Jackson, 2005). We acknowledge that forecast accuracy is a less important driver of analyst reputation and compensation than other analyst attributes (Bradshaw, 2011; Brown et al., 2015), but note that less important does not mean "not important." In fact, in Brown al.'s (2015) analyst survey, 24.10% of the respondents believe forecast accuracy is "very important" and 7.76% believe it is "not important."

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.[9] In short, competition reduces bias by exposing and penalizing biased analysts.

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. The arrival of another competitor whose forecasts are similarly accurate but substantially less biased would potentially make it more difficult for sell-side analysts to justify their forecasts to investors, thus causing a decline in sell-side bias.

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. More recently, Merkley, Michaely, and Pacelli (2017) measure competition at the

---

[9] 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.

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. According to a recent survey of affluent investors, nearly one in three individuals in the United States rely on social media to inform their investment decisions.[10] According to prior literature, crowdsourced earnings forecasts and investment research, freely available on Estimize and Seeking Alpha, convey new information to the market (Chen et al., 2014; Jame et al., 2016), making them viable alternatives to sell-side earnings forecasts and investment research. We discuss key attributes of Estimize in Section 2.3 and argue these attributes generally satisfy the conditions under which competition reduces bias in Section 2.4.

2.3 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.[11] As of December 2015, Estimize has attracted forecasts from over 15,000 contributors, covering more than 2,000 firms.[12] 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 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

[10] http://www.experiencetheblog.com/2013/04/four-recent-studies-on-rapid-adoption.html.
[11] See, for example, https://www.benzinga.com/news/15/04/5395774/the-2015-benzinga-fintech-award-winners
[12] Estimize has experienced dramatic growth since the end of our sample period. As of December 2018, Estimize has over 80,000 unique contributors.

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".[13] 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.[14] 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.

2.4 Hypothesis Development

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

[13] http://www.businessinsider.com/estimize-interview-leigh-drogan-2011-12

[14] 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

"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.[15]

The likely consequences of investors knowing with greater certainty that analysts put their own interests and the interests of their employers ahead of investors' interests are tarnished analysts' and analyst employers' reputations, reducing the demand for sell-side research and increasing the demand for crowdsourced research (and other sell-side alternatives). Consistent with a demand shift, Serafeim, Horton, and Wu (2015) observe that dissatisfaction with sell-side bias "explains why an increasing number of investors are conducting their own in-house analysis

[15] 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).

and rely more on the "wisdom of the crowds" by using signals that are generated by web-based technologies that aggregate individual opinions and measure the sentiment of people towards a company" (para. 4). We suggest that anticipating and reacting to these effects, analysts are likely to reduce their bias. Also, as discussed in Section 2.2, research in psychology suggests that sell-side analysts may reduce their bias upon the arrival of Estimize even if bias is not explicitly penalized. According to Kunda's theory, individuals draw a desired conclusion only if they can justify it to a dispassionate observer; the arrival of Estimize makes the justification of biased research more difficult.

The above arguments suggest that competition from Estimize can reduce sell-side analysts' bias by increasing the likelihood that bias will be detected or by making it more difficult for analysts to justify their biased forecasts. 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 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 focus on firms with continuous sell-side coverage from 2009 to 2015. We define coverage as earnings forecast availability in the IBES detail file. We also require that these 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

We examine the properties of Estimize and IBES quarterly earnings forecasts issued from 2013 to 2015 for the sample of 772 firms added to Estimize in 2012 (see Panel B of Table 1). 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 include the most recent forecast issued by an analyst/contributor 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.

We compute four forecast characteristics for each firm-quarter: *Coverage*, *Forecast Age*, *Bias/Prc* (i.e., forecast error), and Absolute Forecast Error (*AbsFE)*. Coverage is the number of unique contributors or analysts issuing a forecast. *Forecast Age* is the number of calendar days from the forecast issuance date to the earnings announcement date, averaged across all forecasts in a firm-quarter.

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

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

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. We winsorize at the 2.5th and the 97.5th percentiles to reduce the effect of extreme observations.[16] 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.*

Panels A and B of Table 2 contrast the distributional properties of Estimize and IBES forecasts, respectively. The mean number of Estimize contributors is comparable to the mean number of IBES analysts (12.64 vs. 14.38), but the median is significantly lower (6 vs. 13), consistent with large positive skewness in the distribution of Estimize coverage. The difference in forecast age between Estimize forecasts and IBES forecasts is striking. For example, the median Estimize forecast is issued less than a week prior to the earnings announcement (6.33 days), while the median IBES forecast is issued more than two months in advance of the earnings announcement (66.79 days). The location of the distribution of *Estimize Bias/Prc* is essentially zero (mean of 0 and median of 0.01), whereas the location of the distribution of *IBES Bias/Prc* is above zero (mean of 0.05 and median of 0.04), consistent with Estimize forecasts being relatively unbiased and IBES forecasts being pessimistic. The same pattern emerges when we define bias as *Bias/AbsConsensus* or *MBE*. Finally, the distribution of *AbsFE* between the two samples is similar, as evidenced by identical medians and 25th and 75th percentiles, and similar means.

4. Empirical Design

[16] Winsorizing *Bias/Prc* at the 1st and the 99th percentiles results in significant sample kurtosis (10). As a result, our difference-in-difference estimates have similar magnitudes but slightly larger standard errors.

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.[17] 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 for treated firms relative to control firms is not due to other factors. A natural concern is that systematic differences in covariates between treated and non-treated firms may lead to differences in Δ*Bias/Prc*, biasing our difference-in-difference estimates. With the functional relation between covariates and Δ*Bias/Prc* generally unknown, we control for confounding covariates by implementing the propensity score matching (PSM) method. We use PSM in all of our tests and employ two recently proposed matching

[17] Treated firms exhibit within-year variation in treatment date. We explore this staggered introduction in Section 5.3.

techniques, coarsened exact matching (CEM) and entropy balancing (EB), in our robustness tests (Section 5.2).

The basic idea behind PSM is to estimate and equate the probabilities of receiving the treatment as a function of confounding covariates. Accordingly, we estimate a logistic regression in which the dependent variable is one for treated firms and zero for control firms, and covariates include four firm characteristics: Log *(Size*), *Book-to-Market*, Log (*IBES Coverage*), and *Turnover,* 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.

Table IA.1 of the Internet Appendix tabulates our results from the estimation of the logistic regression. We find that the likelihood that a firm is treated increases with *Size*, *IBES Coverage*, *Turnover*, and *Bias/Prc*, and decreases with *Book-to-Market* and *AbsFE*. We match each treated firm to one control firm with replacement because the number of control firms is smaller than the number of treated firms; and we require that the absolute value of the difference in propensity scores is less than 0.50% to decrease the likelihood of a "poor" match and improve covariate balance.[18]

Panel A (B) of Table 3 examines covariate balance between treated and control firms before (after) matching. Before matching, treated firms significantly differ from control firms on all covariates. After matching, all covariates are balanced: the largest t-stat from the tests of equal means is 1.65. Treated and matched control firms have identical mean propensity scores of 77.38%.

5. Main Analysis

[18] We follow Shipman, Swanquist, and Whited's (2017) "best practices" suggestions in implementing PSM and in considering alternative matching approaches.

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.[19] 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). We note that Estimize's arrival reduces but does not fully eliminate sell-side bias: our estimate of *Bias/Prc* for treated firms in the post-Estimize period, 0.04, is statistically different from zero (untabulated t-statistic of 5.33).

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:

$$Bias/Prc_{jt} = \alpha + \boldsymbol{\beta}\mathbf{X_j} + IND_j + QTR_t + \varepsilon_{jt}, \qquad (2)$$

[19] 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.

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 windows. 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 25^{th} (75^{th}) 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 examine whether our results are robust to key research design choices. For reference, we first report the estimates from the baseline specification (as reported in Table 4). In Specification 1, we broaden the treatment sample to include 156 treated firms previously dropped due to lack of common support and find slightly stronger results.

We next implement two alternative matching approaches. In Specification 2, we use coarsened exact matching which matches on a coarsened range of the covariates, discarding treated firms without a match (Blackwell et al., 2009). We match on all six covariates after coarsening each into two strata using median breakpoints, and find slightly weaker but still significant results.[20] In Specification 3, we use entropy balancing which applies continuous weights to candidate control firms to equate the moments of the covariate distributions, thus retaining all treated firms (Hainmueller, 2012). We match on the first three moments of the covariates and find similar results.

In Specifications 4 through 7, we consider alternative measures of bias. We find similar results when bias is defined as the median forecast error (Specification 4) or scaled by *AbsConsensus* (Specification 5), and weaker but still significant results when it is defined as *MBE* (Specification 6). The decline in *Bias/Prc* in the post-event period could be driven by the entry of less pessimistic analysts and/or the exit of more pessimistic analysts. To preclude this explanation, we demean *Bias/Prc* by the average *Bias/Prc* of a given analyst-firm pair over the sample period, and find that our results remain (Specification 7).

In Specification 8, we conduct a placebo test to preclude the alternative explanation that Estimize firms experience a positive performance shock. While this explanation predicts a decline in the bias of a statistical forecast, our hypothesis predicts a decline only in analyst forecast bias. We construct a statistical forecast of firm *j*'s quarter *t* earnings as:

$$Stat\_Fcst = earn_{jt-4} + \theta_{j0} + \theta_{j1}(earn_{jt-1} - earn_{jt-5}), \tag{3}$$

[20] When we use Blackwell, Iacus, King, and Porro's (2009) recommended coarsening algorithm, we are able to match only 29 treated observations, prompting us to further coarsen the covariates. In the Internet Appendix, we use this algorithm to match on *any* three covariates. Across the 20 specifications, the number of matched pairs varies from 382 to 738; the difference-in-difference estimates from -0.04 to -0.19, with a median of -0.13, and the t-statistics from -1.59 to -5.29, with a median of -3.81(Table IA.2).

where $earn_{jt}$ is firm *j*'s quarter *t* earnings, and θ_{j0} and θ_{j1} are parameters of an autoregressive model in fourth difference estimated on the past 30 quarters of data. We define *Statistical Bias* as actual earnings minus the statistical forecast, scaled by the lagged stock price. We find no evidence that treated firms experience a decline in statistical bias relative to control firms.[21]

In Specification 9, we define treated firms as those added to Estimize in 2013 and measure post-event bias over 2014 and 2015. The difference-in-difference estimates are still negative but statistically insignificant.[22] The weaker results are consistent with a weaker treatment effect: 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). However, the results could also be weaker because the match between firms treated in 2013 and control firms is better than the match between firms treated in 2012 and control firms. In the Internet Appendix, we find that the decline in bias is related to treatment intensity but not to match quality. However, we acknowledge that omitted unobservable factors may cause co-variation in treatment intensity and bias decline, and that our proxy for match quality, a small difference in propensity scores between a treated firm and a control firm, is imperfect.

In Specification 10, we confirm that our results hold in a subsample of firm-quarters in which management does not issue any earnings guidance which alleviates the concern that, for unrelated reasons, managers guide down analysts less in the post-Estimize period, resulting in less pessimistic analyst forecasts.

5.3 Event-Time Analysis in the Decline of Pessimism

[21] We find similar results when we compute expected earnings using a seasonal random walk with drift or a seasonal random walk without drift.

[22] Defining treated firms as those added to Estimize in either 2012 or 2013 yields a difference-in-difference estimate of -0.10, with a t-stat of -2.92 (untabulated for brevity).

An important assumption underlying our difference-in-difference estimates is that the change in bias in the treatment and control samples would have been the same had Estimize not been created in 2012 (i.e., the parallel trends assumption). 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 firm sample.[23] 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.

5.4 Industry Spillover Effects

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

[23] 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 firm-quarters (21% of the sample observations) slightly strengthens results.

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.[24]

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).[25] 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 10 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 *Late Treated Firms* (firms added to Estimize 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

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

[25] 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; 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.

and setting the dependent variable equal to one for late additions and zero for control firms. In each sample, we sort observations into *High* (top 30%), *Medium* (middle 40%), and *Low* (bottom 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 6.

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).[26] 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. Alternative Explanations

6.1 The Information Hypothesis

Given that Estimize forecasts are incrementally useful in forecasting future earnings (Jame et al., 2016), it is possible that sell-side analysts use these forecasts to improve their own forecasts, resulting in lower bias. To disentangle the disciplining hypothesis from this alternative explanation (information hypothesis, henceforth), we derive and test differential predictions about bias decline (Section 6.1.1), accuracy improvement (Section 6.1.2), and changes in coverage (Section 6.1.3).

6.1.1 Differential Predictions about the Cross-Sectional Pattern of the Decline in Bias

[26] 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.

The information hypothesis predicts that the decline in bias is stronger when current quarter Estimize forecasts are available to sell-side analysts when forecasting current quarter earnings. The disciplining hypothesis predicts a decline in sell-side bias even when Estimize forecasts are unavailable because sell-side analysts know their own bias and react to the threat of being exposed.

Sell-side analysts may also use past Estimize forecasts to improve their own forecasts. We suggest that this phenomenon is triggered, or greatly facilitated, by the sell-side's inferior forecasting performance (relative to Estimize), and test whether the reduction in bias is greater when the sell-side consensus is less accurate than the Estimize consensus in the prior quarter or over all prior quarters with an Estimize forecast. We refer to the use of past Estimize forecasts to improve current quarter earnings forecasts as past quarter learning,[27] and the use of current quarter Estimize forecasts as current quarter learning.

The disciplining hypothesis uniquely predicts a stronger decline in bias when the consensus includes more forecasts by analysts who have close, mutually beneficial relationships with management and are, therefore, in greater need of disciplining. Empirical proxies of such a relationship include: issuing favorable recommendations (Chen and Matsumoto, 2006), being employed by the firm's lead underwriter(s) (Michaely and Womack, 1999), asking questions on conference calls (Mayew, 2008), and hosting management at conferences (Green et al., 2014).[28]

We estimate several specifications of the following panel regression:

$$Dif\ Bias/Prc_{jt} = \alpha + \beta_1 Post_t + \beta_2 Var_{jt} + \beta_3 Post * Var_{jt} + \varepsilon_{jt}. \tag{4}$$

[27] Our assumption that sell-side analysts use Estimize data after they become dissatisfied with own forecasting performance is inspired by Simon's work on satisfying behavior (1982) and consistent with Hong, Stein, and Yu's (2007) assumption that agents abandon their current forecasting model after they become dissatisfied with its performance.

[28] All four variables are positively correlated with forecast pessimism in the pre-event window, validating them as proxies of close, mutually beneficial analyst-manager relationships (untabulated for brevity).

where *Dif Bias/Prc* is the difference in *Abnormal Bias/Prc* between treated firms and their propensity-score matched control firm, *Post* is a dummy variable equal to one in the post-event window (2013-2015) and zero in the pre-event window (2009-2011), and *Var* is a conditioning dummy variable designed to test a particular empirical prediction. In the first three specifications, β_2 cannot be estimated because *Var* is observable only in the post-event period.

In Specification 1 of Table 7, we compute *Dif Bias/Prc* for two groups of forecasts each firm-quarter: (1) those preceded by at least one Estimize forecast and (2) those not preceded by any Estimize forecasts. The conditioning variable, *Current Qtr Learn*, is one when the consensus comprises forecasts that could have benefitted from availability of Estimize forecasts (Group 1), and zero otherwise. The results are inconsistent with the hypothesis of current quarter learning. If anything, pessimism somewhat increases when analysts have the opportunity to learn from current quarter Estimize forecasts (β_3 is 0.020, with a t-stat of 1.18).[29]

In all remaining specifications, we compute *Dif Bias/Prc* using all available forecasts. In Specification 2 (3), the conditioning variable, *Prior Qtr (Qtrs) Learn*, is one when the sell-side consensus is less accurate than the Estimize consensus in the prior quarter (across all prior quarters with available Estimize forecasts). We find no evidence that the decline in pessimism is larger when past Estimize forecasts have been relatively more accurate. If anything, pessimism increases, as evidenced by the positive β_3 estimate in both specifications.

The conditioning variables in Specifications 4-7 are: *Rec Optimism*, one when the fraction of analysts included in the consensus who have an outstanding Strong Buy or Buy recommendation exceeds the sample median; *Underwriting*, one when the fraction of analysts included in the consensus who are employed by the firm's lead underwriters exceeds the sample median; *CC*

[29] To alleviate the concern that learning from related firms confounds the comparison, we eliminate Group 2 forecasts issued within 60 days of earnings announcement, and still find a positive β_3.

Participation, one when the fraction of analysts included in the consensus who have participated in the firm's conference calls in the last three years exceeds the sample median; and *Conf Host*, one when the fraction of analysts included in the consensus who have hosted the firm at conferences in the last three years exceeds the sample median. As predicted by the disciplining hypothesis, we document a larger decline in bias when the consensus includes more forecasts by analysts close to management. For example, the decline in pessimism is 0.171 (0.121) when the fraction of analysts with favorable recommendations is above (below) the median.[30] In Specification 8, we include all of the conditioning variables from Specifications 2 through 7 and find that all of our results hold.[31]

6.1.2 Differential Predictions about Sell-Side Accuracy Improvement

According to the information hypothesis, increased availability of pertinent information leads to greater accuracy, whether pre-Estimize sell-side forecasts are biased or not. Conversely, the disciplining hypothesis predicts greater accuracy *only* if pre-Estimize forecasts are biased: i.e., accuracy improves as a byproduct of analysts reducing their bias for fear of being exposed.

We sort firms into three groups based on breakpoints for the top 30% (*High*), middle 40% (*Medium*) and bottom 30% (*Low*) of pre-Estimize sell-side pessimism and examine how our difference-in-difference estimates of sell-side bias (*Bias/Prc)*, accuracy (*AbsFE*, inversely related to accuracy), and representativeness (*Representativeness)*[32] vary across the three groups. We report

[30] Table 7 results suggests a larger average decline in bias than Table 3 results. The reason for the difference is that in Table 7 we exclude observations with missing values of the conditioning variables.
[31] We omit *Current Qtr Learn* from Specification 8 since the construction of the dependent variable differs across the two specifications.
[32] Forecast representativeness reflects the degree to which a forecast is representative of the market expectation of earnings. 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). Our measure of representativeness, therefore, is the slope coefficient in a firm-level regression of earnings announcement returns on unexpected earnings. See Appendix for details.

the mean values of these variables in the pre-Estimize period for the full sample, the three terciles, and the *High-Low* difference in Panel A of Table 8, and the difference-in-difference estimates in Panel B. We observe large improvements in accuracy and representativeness in the *High* bias group, where pre-Estimize bias is 0.52%. In contrast, we find no significant improvements in the *Medium* or *Low* group where pre-period pessimism is much less extreme. This finding is inconsistent with the information hypothesis which predicts improvements in accuracy and representativeness even when sell-side forecasts are unbiased.

6.1.3 Differential Predictions about Sell-Side Coverage

The disciplining hypothesis also predicts that sell-side analysts with a history of pessimistic forecasts are more likely to reduce coverage of treated firms, where their bias is more easily exposed, relative to control firms. The information story makes no such prediction because it regards Estimize forecasts as an additional information source rather than a threat.

We compute the ratio of treated firms to the sum of treated and control firms in analyst *j*'s portfolio in 2013 and 2011, labeled $Estimize\ Tilt_{j,2013}$ and $Estimize\ Tilt_{j,2011}$, respectively, and test whether analyst *j*'s greater relative pessimism in the pre-Estimize period predicts a decrease in Estimize Tilt in the post-Estimize period.[33] We measure analyst *j*'s pessimism by first ranking forecast errors for each firm-quarter in the pre-Estimize period (2009-2011) and then averaging analyst *j*'s forecast error percentile ranking across firm-quarters ($Relative\ Bias_j$). We exclude (1) forecasts with horizons that differ from the sample median by 45 days or more to address the concern that differences in analyst bias are driven by significant differences in forecast horizon and (2) analysts who issue fewer than six forecasts in order to more accurately measure bias.

[33] Results are similar when we define $\Delta Estimize\ Tilt_j$ from 2011 to 2015. The requirement that analyst *j* remains on IBES in 2015 reduces the sample size by 20%.

We sort analysts into three groups based on their *Relative Bias*: *Low* (bottom 30%), *Medium* (middle 40%), and *High* (top 30%) and report mean Δ*Estimize Tilt* for each group in Table 9. The results support our conjecture that more pessimistic analysts are more likely to tilt coverage away from Estimize firms. In particular, our estimate of Δ*Estimize Tilt* is negative *only* in the *High* group, -0.66, and it is statistically different from the corresponding estimate in the *Low* group (t-stat of -2.17). A placebo test that lags all variables by two years yields no evidence of a relation between *Relative Bias* and Δ*Estimize Tilt* (Column 2), suggesting that the documented relation is unique to the year of Estimize's arrival.

6.2 Omitted Economic Forces Mitigating Conflicts of Interests

Another 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, whereas our hypothesis predicts a reduction in bias only for short-term forecasts. The reason is that Estimize provides few longer-term forecasts (less than 10% of all Estimize forecasts) and no stock recommendations.

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, and recommendation bias, measured as the average recommendation level at quarter end (*Recommendation Level*) after converting strong buy, buy, hold, sell/underperform, and strong sell categories to numerical values, 1, 2, 3, 4, and 5, respectively. 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; Panel E of Table 10 reports the results for *Recommendation Level*. We find no evidence that treated firms experience a reduction in longer-horizon bias. In all four cases, the difference-in-difference estimates are statistically insignificant and economically small.[34] Nor do we find a statistically significant decline in recommendation optimism. We conclude that direct competition from Estimize, rather than more pervasive economic forces, accounts for the change in short-term pessimism.

6.3 Relation to the Financial Crisis

Another alternative explanation is that the decline in analyst pessimism occurs as a reaction to the financial crisis. Exactly when the financial crisis ended is debatable but market returns of -35%, 25% and 15% in 2008, 2009, and 2010 suggest that investors shed their pessimism in 2009. Given that analyst forecasts are a well-known proxy for market expectations, it is possible but unlikely that analysts would only begin to reduce their short-term pessimism in 2012, three years after the market recovery.[35] In addition, the financial crisis explanation predicts not only a decline in short-term analyst pessimism but also an increase in long-term optimism, which we do not find (Table 10).

7. Conclusion

[34] 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*.

[35] Figure 2 shows that the documented decline in pessimism in 2012 is not the continuation of a pre-event trend.

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 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, and present several additional results that suggests that competition from Estimize is disciplining sell-side analysts.

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, the decline in sell-side pessimism is greater when we expect a greater disciplining effect but not when we expect a greater learning effect. In addition, sell-side accuracy improvement strongly depends on pre-Estimize sell-side bias, suggesting that the disciplining effect drives the accuracy effect. Finally, placebo tests show that biases in longer-term earnings forecasts and investment recommendations 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 learning versus disciplining variables discussed in Section 6.1.

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 mean of all individual forecasts issued within 120 days of an earnings announcement. We retain the most recent forecast for each analyst. Price is the stock price at the end of the prior year. 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 that report earnings greater than or equal to the consensus, and zero otherwise.
- $MedianBias\ /\ Prc_{j,t} = \dfrac{Actual_{j,t} - MedianConsensus_{j,t}}{Price_{j,t\text{-}1}} * 100.$ *Actual* is reported earnings. *MedianConsensus* is the median of all individual forecasts issued within 120 days of an earnings announcement. We retain the most recent forecast for each analyst. Price is the stock price at the end of the prior year. We winsorize *MedianBias/Prc* at 2.5% and 97.5%.
- $Stat\_Fcst_{j,t} = earn_{j,t-4} + \theta_{j,0} + \theta_{j,1}(earn_{j,t-1} - earn_{j,t-5}),$

  where $earn_{jt}$ is the realized earnings for firm *j* in quarter *t* and θ_{j0} and θ_{j1} are parameters that are estimated based on (up to) the past 30 quarters of data for firm *j*.
- *Statistical Bias* = actual earnings minus *Stat_Fcst*, scaled by lagged stock price. We winsorize *Statistical Bias* at 2.5% and 97.5%.
- *AbsFE* (*Absolute Forecast Error*) = the absolute value of *Bias/Prc.*
- *Representativeness* (*Earnings Response Coefficient - ERC*) = the slope coefficient from the following time-series regression: $CAR_{j,t} = \alpha + \beta UE_{j,t} + \varepsilon_t.$ *CAR* 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 1st and 99th percentile. We also exclude firms with fewer than six quarters of Estimize forecasts.

- *Coverage* = the number of unique contributors or analysts issuing a forecast.
- *Forecast Age* = the number of calendar days between the forecast issue date and the earnings announcement date, averaged across all forecasts in the consensus.
- *Recommendation 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.

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 date, 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. We winsorize *Return Volatility* at the 99th percentile.
- *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.
- *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 Learning and Disciplining Variables

- *Current Qtr Learn* = a dummy variable equal to one when the consensus comprises forecasts that are preceded by at least one Estimize forecast and zero when the consensus excludes such forecasts.
- *Prior Qtr Learn* = a dummy variable equal to one when the Estimize consensus is more accurate than the sell-side consensus in the prior quarter.
- *Prior Qtrs Learn* = a dummy variable equal to one when the Estimize consensus is more accurate than the sell-side consensus across all prior quarters with an Estimize forecast.

- *Rec Optimism*$_{jt}$ = a dummy equal to one when the fraction of analysts included in the consensus (for firm *j* in quarter *t*) that have an outstanding Strong Buy or Buy recommendation for firm *j* exceeds the sample median.
- *Underwriting*$_{jt}$ = a dummy equal to one when the fraction of analysts included in the consensus (for firm *j* in quarter *t*) who are employed by a brokerage firm that has been the lead underwriter for an IPO or SEO for firm *j* exceeds the sample median.
- *CC Participation*$_{jt}$ = a dummy equal to one when the fraction of analysts included in the consensus (for firm *j* in quarter *t*) who have asked a question on firm j's conference call in the past three years exceeds the sample median.
- *Conf. Host*$_{t}$ = a dummy equal to one when the fraction of analysts included in the consensus (for firm *j* in quarter *t*) who are employed by a brokerage firm that has hosted firm *j* at a conference in the past three years exceeds the sample median.
- *EstimizeTilt* = the ratio of treated firms to the sum of treated and control firms in an analyst's research portfolio.
- *Relative Bias* = an analyst's average forecast error percentile ranking. We first rank forecast errors for each firm-quarter in the pre-Estimize period (2009-2011). We then calculate and average each analyst's forecast error percentile ranking across all firm-quarters. This measure excludes forecasts with a horizon that differs from the sample median by 45 days or more and analysts with fewer than six forecasts in the pre-event period.

References:

Altinkilic, O., Balashov, V., and Hansen, R., 2018, Investment bank monitoring and bonding of security analysts' research. Journal of Accounting and Economics, forthcoming.

Barber, B. M., Lehavy, R., McNichols, M., and Trueman, B., 2006. Buys, holds, and sells: the distribution of investment banks' stock ratings and the implications for the profitability of analysts' recommendations. Journal of Accounting and Economics 41, 87-117.

Bartov, E., Givoly, D., and Hayn, C., 2002. The rewards to meeting or beating earnings expectations. Journal of Accounting and Economics 33, 173-204.

Becker, B., and Milbourn, T., 2011. How did increased competition affect credit ratings? Journal of Financial Economics 101, 493-514.

Bhojraj, S., Lee, C., and Oler, D., 2003. What's my line? A comparison of industry classification schemes for capital market research. Journal of Accounting Research 41, 745-774.

Blackwell, M., Iacus, S., King, G., and Porro, G., 2009. CEM: Coarsened exact matching in Stata. Stata Journal 9, 524-546.

Boni, L., and Womack, K., 2006, Analysts, industries, and price momentum. Journal of Financial and Quantitative Analysis 41, 85-109.

Bradshaw, M. T., 2011. Analysts' forecasts: What do we know after decades of work? Working paper, Boston College.

Brown, L. D., Hagerman, R. L., Griffin, P. A., and Zmijewski, M. E., 1987. Security analyst superiority relative to univariate time-series models in forecasting quarterly earnings. Journal of Accounting and Economics 9, 61-87.

Brown, L. D., Call, A. C., Clement, M. B., Sharp, N. Y., 2015. Inside the "black box" of sell-side financial analysts. Journal of Accounting Research 53, 1-47.

Cen, L., Ornthanalai, C., and Schiller, C. M., 2017. Navigating Wall Street: Career concerns and analyst transitions from sell-side to buy side. Working paper, University of Toronto.

Chen, H., De, P., Hu, Y. J., and Hwang, B. H., 2014. Wisdom of crowds: the value of stock opinions transmitted through social media. Review of Financial Studies 27, 1367-1403.

Chen, S., and Matsumoto, D., 2006. Favorable versus unfavorable recommendations: the impact of analysts' access to management-provided information. Journal of Accounting Research 44, 657-689.

Chen, Q., Francis, J., and Jiang, W, 2005. Investor learning about analyst predictive ability. Journal of Accounting and Economics 39, 3-24.

Costa, L., 2010. Facebook for finance. Institutional Investor 44 (October, 2010), 54-93.

Dambra, M., Field, L. C., Gustafson, M. T., and Pisciotta, K., 2018. The consequences to analyst involvement in the IPO process: Evidence surrounding the JOBS Act. Journal of Accounting and Economics 65, 302-330.

Das, S., Levine, C. B., and Sivaramakrishnan, K., 1998. Earnings predictability and bias in analysts' earnings forecasts. The Accounting Review 73, 277-294.

De Franco, G., Lu, H., and Vasvari, F. P., 2007. Wealth transfer effects of analysts’ misleading behavior. Journal of Accounting Research 45, 71-110.

Dechow, P. M., and Sloan, R. G., 1997. Returns to contrarian investment strategies: tests of naive expectations hypotheses. Journal of Financial Economics 43, 3-27.

Dechow, P. M., Hutton, A. P., and Sloan, R. G., 2000. The relation between analysts’ forecasts of long-term earnings growth and stock price performance following equity offerings. Contemporary Accounting Research 17, 1-32.

Doherty, N. A., Kartasheva, A. V., and Phillips, R. D., 2012. Information effect of entry into credit ratings market: the case of insurers' ratings. Journal of Financial Economics 106, 308-330.

Egger, B.D., 2014. Social media strategies for investing: how twitter and crowdsourcing tools can make you a smarter investor. F+ W Media, Inc.

Fama, E. F., and French, K. R., 1997. Industry costs of equity. Journal of Financial Economics 43, 153-193.

Fang, L., and Yasuda, A., 2009. The effectiveness of reputation as a disciplinary mechanism in sell-side research. Review of Financial Studies 22, 3735-3777.

Farrell, M., Green, T.C., Jame, R., and Markov, S., 2018. The democratization of investment research: Implications for retail investor profitability and firm liquidity. Working paper.

Feng, M., and McVay, S., 2010. Analysts’ incentives to overweight management guidance when revising their short-term earnings forecasts. The Accounting Review 85, 1617-1646.

Francis, J., and Philbrick, D., 1993. Analysts' decisions as products of a multi-task environment. Journal of Accounting Research 31, 216-230.

Gentzkow, M., Glaeser, E. L., and Goldin, C., 2006. The rise of the fourth estate: how newspapers became informative and why it mattered. In Corruption and Reform: Lessons from America's Economic History (pp. 187-230). University of Chicago Press.

Gentzkow, M., and Shapiro, J. M., 2008. Competition and truth in the market for news. Journal of Economic Perspectives 22, 133-154.

Gleason, C. A., and Lee, C. M. C., 2003. Analyst forecasts revisions and market price discovery. The Accounting Review 78, 193-225.

Graham, J. R., Harvey, C. R., and Rajgopal, S., 2005. The economic implications of corporate financial reporting. Journal of Accounting and Economics 40, 3-73.

Green, T. C., Jame, R., Markov, S., and Subasi, M., 2014. Access to management and the informativeness of analyst research. Journal of Financial Economics 114, 239-255.

Hainmueller, J., 2012. Entropy balancing for causal effects: A multivariate reweighting method to produce balanced samples in observational studies. Political Analysis 20, 25-46.

Hong, H., and Kacperczyk, M., 2010. Competition and bias. Quarterly Journal of Economics 125, 1683-1725.

Hong, H., and Kubik, J. D., 2003. Analyzing the analysts: Career concerns and biased earnings forecasts. The Journal of Finance 58, 313-351.

Hong, H., Stein, J. C., and Yu, J., 2007. Simple forecasts and paradigm shifts. The Journal of Finance 62, 1207-1242.

Horner, J., 2002. Reputation and competition. American Economic Review 92, 644-663.

Horton, J., Serafeim, G., and Wu, S., 2017. Career concerns of banking analysts. Journal of Accounting and Economics 63, 231-252.

Jame, R., Johnston, R., Markov, S., and Wolfe, M. C., 2016. The value of crowdsourced earnings forecasts. Journal of Accounting Research 54, 1077-1110.

Jackson, A. R., 2005. Trade generation, reputation, and sell-side analysts. The Journal of Finance 60, 673-717.

Kadan, O., Madureira, L., Wang, R., and Zach, T., 2009. Conflicts of interest and stock recommendations: the effects of the global settlement and related regulations. Review of Financial Studies 22, 4189-4217.

Kadan, O., Madureira, L., Wang, R., and Zach, T., 2012. Analysts’ industry expertise. Journal of Accounting and Economics 54, 95-120.

Kasznik, R., and McNichols, M. F., 2002. Does meeting earnings expectations matter? Evidence from analyst forecast revisions and share prices. Journal of Accounting Research 40, 727-759.

Ke, B., and Yu, Y., 2006. The effect of issuing biased earnings forecasts on analysts' access to management and survival. Journal of Accounting Research 44, 965-999.

Kelly, B., and Ljungqvist, A., 2012. Testing asymmetric-information asset pricing models. Review of Financial Studies 25, 1366-1413.

Kunda, Z., 1990. The case for motivated reasoning. Psychological Bulletin 108, 480-498.

Lin, H. W., and McNichols, M. F., 1998. Underwriting relationships, analysts' earnings forecasts and investment recommendations. Journal of Accounting and Economics 25, 101-127.

Ljungqvist, A., Marston, F., Starks, L. T., Wei, K. D., and Yan, H., 2007. Conflicts of interest in sell-side research and the moderating role of institutional investors. Journal of Financial Economics 85, 420-456.

Lourie, B., 2018. The revolving door of sell-side analysts. The Accounting Review, forthcoming.

Malmendier, U., and Shanthikumar, D., 2007. Are small investors naive about incentives? Journal of Financial Economics 85, 457-489.

Mayew, W. J., 2008. Evidence of management discrimination among analysts during earnings conference calls. Journal of Accounting Research 46, 627-659.

Mehran, H., and Stulz, R. M., 2007. The economics of conflicts of interest in financial institutions. Journal of Financial Economics 85, 267-296.

Merkley, K. J., Michaely, R., and Pacelli, J. M., 2017. Does the scope of sell-side analyst industry matter? An examination of bias, accuracy and information content of analyst reports. The Journal of Finance 72, 1285-1334.

Michaely, R., and Womack, K. L., 1999. Conflict of interest and the credibility of underwriter analyst recommendations. Review of Financial Studies 12, 653-686.

Mikhail, M. B., Walther, B. R., and Willis, R. H., 1999. Does forecast accuracy matter to analysts? The Accounting Review 74, 185-200.

O'Brien, P. C., 1988. Analysts' forecasts as earnings expectations. Journal of Accounting and Economics 10, 53-83.

Park, C. W., and Stice, E. K., 2000. Analyst forecasting ability and the stock price reaction to forecast revisions. Review of Accounting Studies 5, 259-272.

Philippon, T., 2016. The FinTech opportunity. Working paper, New York University.

Richardson, S., Teoh, S. H., and Wysocki, P. D., 2004. The walk-down to beatable analyst forecasts: the role of equity issuance and insider trading incentives. Contemporary Accounting Research 21, 885-924.

Serafeim, G., Horton, J., and Wu, S., 2015. How banking analysts’ biases benefit everyone except investors. Harvard Business Review, June 5, 2015. Retrieved from https://hbr.org/2015/06/how-banking-analysts-biases-benefit-everyone-except-investors.

Shipman, J.A., Swanquist, Q.T., Whited, R.L., 2017. Propensity score matching in accounting research. The Accounting Review 92, 213-244.

Simon, H.A., 1982. Models of bounded rationality: Empirically grounded economic reason. Vol. 3. MIT Press.

Stickel, S. E., 1992. Reputation and performance among security analysts. The Journal of Finance 47, 1811-1836.

So, E. C., 2013. A new approach to predicting analyst forecast errors: Do investors overweight analyst forecasts? Journal of Financial Economics 108, 615-640.

Veenman, D., and Verwijmeren, P., 2018. Do investors fully unravel persistent pessimism in analysts’ earnings forecasts? The Accounting Review 93, 349-377.

Womack, K. L., 1996. Do brokerage analysts' recommendations have investment value? The Journal of Finance 51, 137-167.

Xia, H., 2014. Can investor-paid credit rating agencies improve the information quality of issuer-paid rating agencies? Journal of Financial Economics 111, 450-468.

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: Characteristics 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 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 the mean, median, standard deviation, and 25th and 75th percentile for each attribute. The sample includes 772 firms added to Estimize in 2012 and covered by IBES analysts and Estimize contributors from 2013 to 2015 (8,265 firm-quarters).

	Mean	*Median*	*Std Dev*	*25th*	*75th*
Panel A: Estimize Forecasts					
Coverage	12.64	6.00	26.16	3.00	13.00
Forecast Age	9.71	6.33	11.42	2.00	13.60
Bias/Prc	0.00	0.01	0.28	-0.06	0.09
Bias/AbsConsensus	-0.01	0.01	0.32	-0.05	0.07
MBE	55.81%	100.00%	49.66%	0.00%	100.00%
AbsFE	0.17	0.08	0.23	0.03	0.21
Panel B: IBES Forecasts					
Coverage	14.38	13.00	8.19	8.00	19.00
Forecast Age	63.79	66.79	21.58	48.83	79.96
Bias/Prc	0.05	0.04	0.39	-0.02	0.13
Bias/AbsConsensus	0.05	0.03	0.40	-0.02	0.11
MBE	68.85%	100.00%	46.31%	0.00%	100.00%
AbsFE	0.20	0.08	0.33	0.03	0.21

Table 3: Characteristics of Treated and Control Firms

This table compares treated firms to candidate control firms in Panel A and to matched control firms in Panel B. 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 one control firm with the most similar probability of being treated, excluding 156 observations where the absolute difference in the propensity scores of the treated and matched control firms exceeds 0.50%. We estimate the probability of being treated as a function of *Log (Size)*, *Book-to-Market*, Log (*IBES Coverage*), *Turnover*, *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 include detailed definitions in the Appendix and report propensity score model results in Table IA.1 of the Internet Appendix.

Panel A: Treated Firms (N=772) vs. Candidate Control Firms (N=451)

	Treated	Candidate Control	Treated - Control	t(Treated - Control)
Log *(Size)*	15.25	13.26	1.99	(23.23)
Book-to-Market	0.42	0.73	-0.31	(-16.97)
Log *(IBES Coverage)*	2.73	1.78	0.95	(23.09)
Turnover	12.23	7.17	5.06	(12.74)
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: Treated Firms vs. Matched Control Firms (N=616)

	Treated	Matched Control	Treated - Matched	t(Treated - Matched)
Log *(Size)*	15.00	15.11	-0.11	(-0.44)
Book-to-Market	0.45	0.48	-0.03	(-0.90)
Log *(IBES Coverage)*	2.61	2.58	0.03	(0.31)
Turnover	11.22	12.96	-1.74	(-1.21)
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*, Log (*IBES Coverage*), *Turnover*, *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 Effect of Estimize Coverage on Bias-Robustness

This table examines the sensitivity of the difference-in-difference estimates in Table 4 (tabulated for convenience in Row 1) to alternative research design choices. In Specification 1, we include the 156 treated firms excluded due to lack of common support. In Specifications 2, we identify control firms using coarsened exact matching (CEM). We match on all six covariates after coarsening each into two strata based on median breakpoints. In Specification 3, we use entropy balancing to match on the first three moments of the covariates. In Specifications 4 through 6, we replace *Bias/Prc* with *Median Bias/Prc*, *Bias/AbsConsensus* and *MBE* (meet or beat indicator variable). In Specification 7, we demean *Bias/Prc* by the average *Bias/Prc* of a given analyst-firm pair over the sample period. In Specification 8, we conduct a placebo test, with bias defined as actual earnings minus a statistical forecast, obtained from a first-order autoregressive process in fourth difference with a drift. In Specification 9, we define treated firms as firms added to Estimize in 2013 and the post-event window as 2014-2015. In Specification 10, we conduct the baseline analysis on a sample of firm-quarters without management guidance. The t-statistics (in parentheses) are based on standard errors double-clustered by control firm and quarter.

	Treated Firms	*Bias*	*Abnormal Bias*
Baseline Results	616	-0.13	-0.12
		(-3.79)	(-3.53)
Alternative Matching Approaches			
1. Propensity Score Matching - No Common Support	772	-0.15	-0.13
		(-3.89)	(-3.62)
2. Coarsened Exact Matching	585	-0.10	-0.10
		(-2.53)	(-2.65)
3. Entropy Balancing	772	-0.17	-0.17
		(-2.04)	(-2.10)
Alternative Measures of Bias			
4. Median Bias/Prc	616	-0.09	-0.09
		(-3.16)	(-3.02)
5. Bias/AbsConsensus	616	-0.18	-0.18
		(-3.29)	(-3.24)
6. MBE	616	-0.09	-0.09
		(-1.98)	(-1.87)
7. Bias/Prc with Analyst-Firm Fixed Effects	616	-0.09	-0.09
		(-2.36)	(-2.21)
8. Statistical Bias (Placebo Test)	616	0.02	0.02
		(0.41)	(0.43)
Alternative Treatment Sample			
9. Additions (2014-2015 post period)	509	-0.05	-0.04
		(-1.22)	(-1.06)
Alternative Subsamples			
10. Drop Firm-Quarters with Management Guidance	519	-0.15	-0.14
		(-3.99)	(-3.76)

Table 6: The Effect of Estimize Industry Coverage on Bias

This table reports the mean difference-in-difference estimates 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 *High* (top 30%), *Medium* (middle 40%), or *Low* (bottom 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 7: Information Hypothesis vs. Disciplining Hypothesis: Differential Predictions about the Cross-Sectional Pattern of the Decline in Bias

This table reports estimates from the panel regression**:** *Dif Bias/Prc* = $\alpha+\beta_1$Post + β_2*Var* + β_3 *Post* * *Var* + ε. *Dif Bias/Prc* is the difference in *Abnormal Bias/Prc* between treated firm *j* and its propensity-score matched control firm in quarter *t*. *Post* equals one in the post-event window (2013-2015) and zero in the pre-event window (2009-2011). In Specifications 1-3, *Var* is one when the information hypothesis predicts a greater decline in bias, and zero otherwise. In Specification 1, we compute *Dif Bias/Prc* for two groups of forecasts each firm-quarter: forecasts by analysts who can learn from Estimize (i.e., their forecasts are preceded by at least one Estimize forecast) and forecasts that are not preceded by any Estimize forecasts. *Var* is one for the first group (*Current Qtr Learn)* and zero otherwise. In all other tests, *Dif Bias/Prc* is computed using all available forecasts. In Specification 2 and 3, *Var* is one when the Estimize consensus is more accurate than the sell-side consensus in the prior quarter (*Prior Qtr Learn*) or across all prior quarters (*Prior Qtrs Learn*). In Specifications 1-3, β_2 is not estimable because Estimize forecasts are available only in the post-event period. In Specifications 4-7, *Var* is one when the disciplining hypothesis predicts a greater decline in sell-side bias: i.e., when the fraction of analysts included in the consensus displaying characteristics associated with a close relationship with management exceeds the sample median. These characteristics are: issuing a Strong Buy or Buy recommendation (*Rec Optimism*); being employed by the firm's lead underwriters (*Underwriting*); asking questions on conference calls in the last three years (*CC Participation*); and hosing the firm at broker conferences in the last three years (*Conf Host*). The reported t-statistics (in parentheses) are computed based on standard errors double-clustered by control firm and quarter.

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Post	-0.113	-0.150	-0.150	-0.121	-0.119	-0.097	-0.114	-0.046
	(-3.66)	(-3.63)	(-3.06)	(-2.92)	(-3.11)	(-1.83)	(-2.41)	(-0.89)
*Post * Current Qtr Learn*	0.020							
	(1.18)							
*Post * Prior Qtr Learn*		0.018						0.016
		(1.10)						(1.04)
*Post * Prior Qtrs Learn*			0.016					0.014
			(0.62)					(0.51)
Rec Optimism				0.023				0.024
				(2.10)				(2.15)
*Post * Rec Optimism*				-0.05				-0.049
				(-4.68)				(-4.89)
Underwriting					0.031			0.029
					(1.35)			(1.21)
*Post * Underwriting*					-0.05			-0.047
					(-2.34)			(-2.10)
CC Participation						0.038		0.029
						(1.60)		(1.29)
*Post * CC Participation*						-0.064		-0.053
						(-2.20)		(-1.98)
Conf Host							0.025	0.021
							(1.19)	(0.96)
*Post * Conf Host*							-0.056	-0.049
							(-2.01)	(-1.75)
Observations	20,026	15,093	15,093	15,093	15,093	15,093	15,093	15,093

Table 8 – Information Hypothesis vs. Disciplining Hypothesis: Differential Predictions about Accuracy and Representativeness

This table examines the effect of Estimize on forecast bias, *Bias/Prc*, accuracy, *AbsFE* and representativeness, *Representativeness*, conditional on pre-event forecast bias: *High* (top 30%), *Medium* (middle 40%), and *Low* (bottom 30%). *Representativeness* is the slope coefficient from a firm-level regression of three-day earnings announcement returns on unexpected earnings, defined as actual earnings less forecasted earnings, scaled by price (See Appendix for details). More detailed variable definitions are in the Appendix. Panel A reports the mean values of the outcome variables in pre-event 2009-2011 window. Panel B reports the difference-in-difference estimates, as computed in Panel A of Table 4, with t-statistics, in parentheses, based on standard errors double-clustered by matched control firm and quarter.

Panel A: Mean Pre-Event Values

	All	*High*	*Medium*	*Low*	*High - Low*
1. Bias/Prc	0.14	0.52	0.12	-0.13	0.65
2. AbsFe	0.34	0.68	0.22	0.40	-0.28
3. Representativeness	2.71	2.82	2.78	2.41	0.41

Panel B: Difference-in-Difference Estimates

	All	*High*	*Medium*	*Low*	*High-Low*
1. Bias/Prc	-0.13	-0.43	-0.10	0.04	-0.47
	(-3.79)	(-8.15)	(-2.50)	(0.96)	(-8.51)
2. AbsFe	-0.10	-0.39	-0.03	-0.05	-0.34
	(-3.11)	(-7.59)	(-0.74)	(-1.18)	(-6.20)
3. Representativeness	2.03	5.52	1.09	1.61	3.91
	(5.21)	(3.83)	(0.96)	(1.36)	(2.05)

Table 9: The Effect of Estimize on Coverage Decisions: Learnings versus Disciplining

This table reports the mean change in the ratio of treated firms to the sum of treated and control firms in an analyst's research portfolio, $\Delta EstimizeTilt_{2013\text{-}2011}$, conditional on the analyst's pre-event (2009-2011) *Relative Bias*. We measure each analyst's *Relative Bias* by first ranking her forecast errors each firm-quarter, then calculating and averaging her forecast error percentile rankings across all firm-quarters. We require that an analyst appear in the sample in 2013 and issue at least six forecasts in the pre-event period; and that the forecast horizon be within 45 days of the sample median. We then sort analysts into three groups based on their *Relative Bias*: *Low* (bottom 30%), *Medium* (middle 40%), and *High* (top 30%). We report t-statistics, in parentheses, based on standard errors double-clustered by matched control firm and quarter. In the Placebo Test, we shift the timing of all variables earlier by two years: i.e., we measure *Relative Bias* over the 2007-2009 period and $\Delta EstimizeTilt_j$ from 2009 to 2011. The samples in the Main and Placebo Tests include 1,898 and 1,931 analysts, respectively.

	Main Test	Placebo Test
Relative Bias	Δ $EstimizeTilt_{2013\text{-}2011}$	Δ $EstimizeTilt_{2011\text{-}2009}$
1 (*Low*)	1.29%	-1.51%
2	0.62%	0.74%
3 (*High*)	-0.66%	0.00%
High - Low	-1.95%	1.51%
	(-2.17)	(1.76)

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)

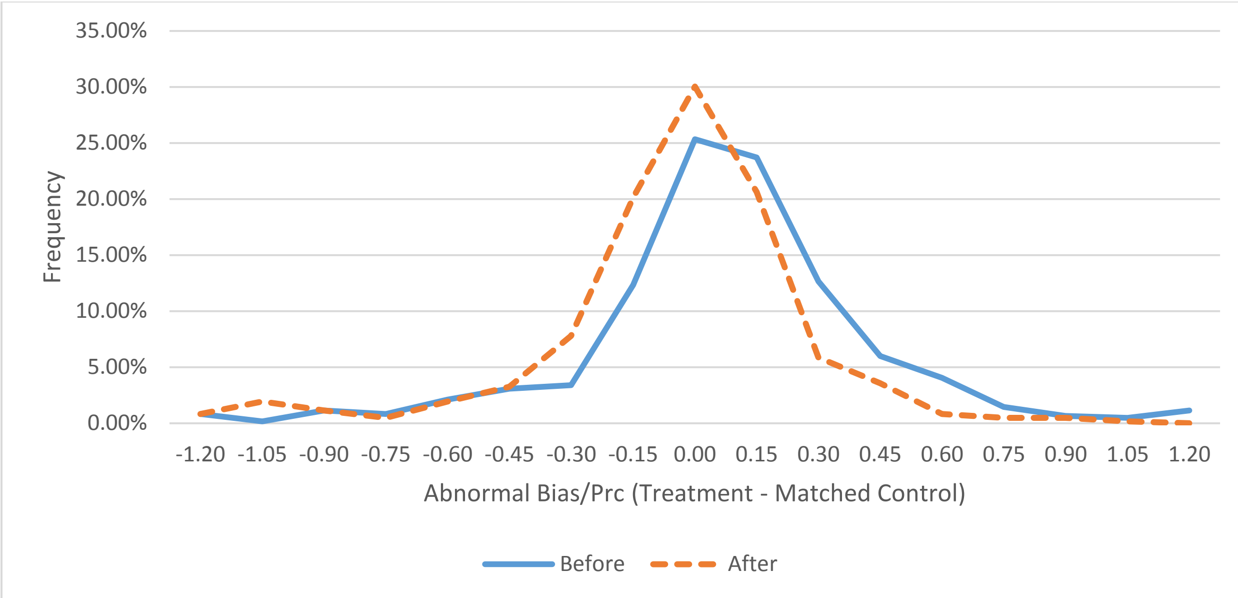

Figure 1: Distribution of the Difference in Bias of Treatment and Control Groups Before and After Estimize

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*, Log (*IBES Coverage*), *Turnover*, *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).

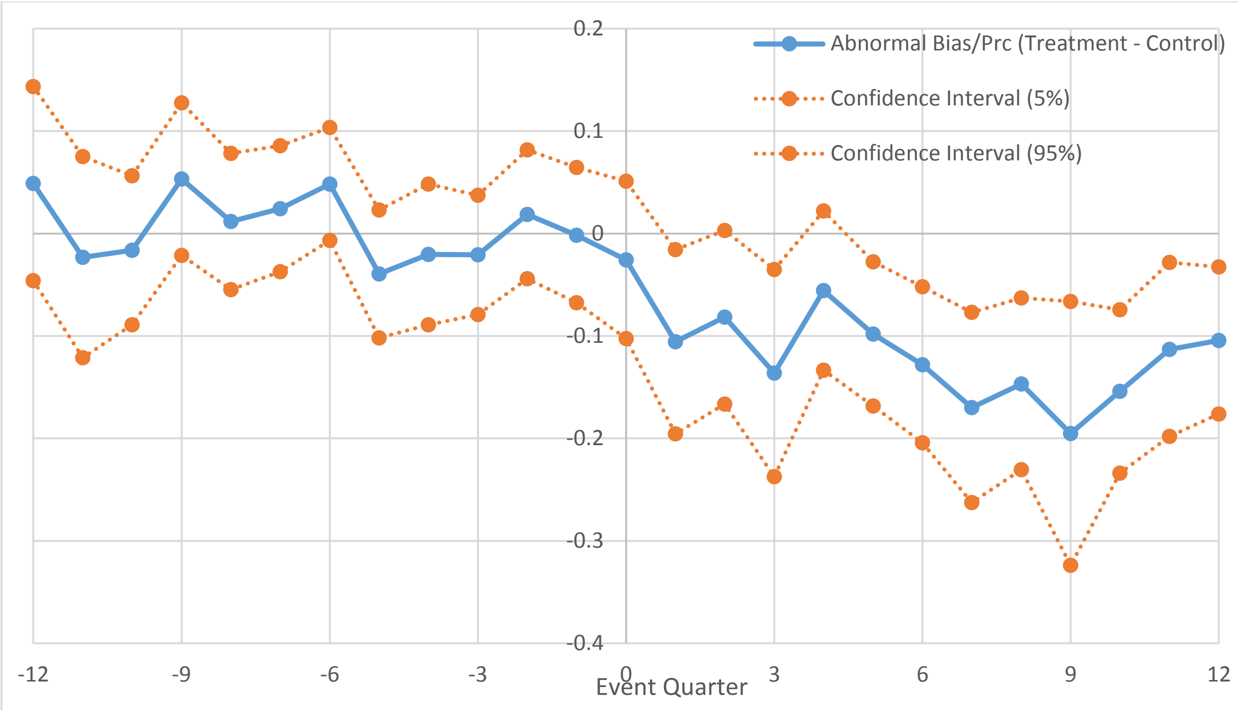

Figure 2: Differences in Bias in Event Time

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*, Log (*IBES Coverage*), *Turnover*, *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.

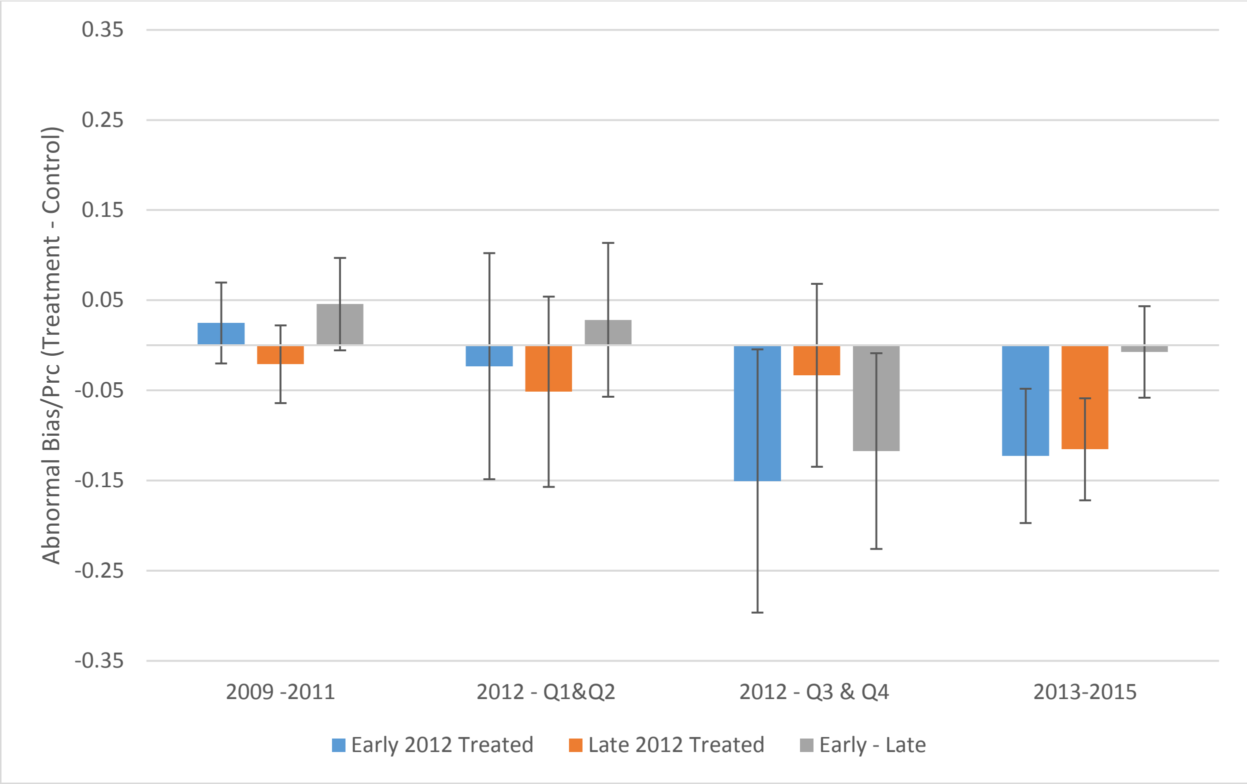

Figure 3: Differences in Bias – Early 2012 Treated versus Late 2012 Treated

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*, Log (*IBES Coverage*), *Turnover*, *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%

Internet Appendix for

"Does Crowdsourced Research Discipline Sell-Side Analysts?"

We tabulate and discuss results from select robustness and supplementary analyses referenced in the paper.

1. Propensity Score Model

In Table IA.1, we tabulate the odds ratios from the propensity score model (discussed in Section 4 and Table 3) and the corresponding z-scores (in parentheses). The three most important predictors of whether a firm is added to Estimize are *Log (Size)*, *Turnover*, and *Log (IBES Coverage).*

2. Coarsened Exact Matching: Matching on Fewer, Less Coarsened Covariates

In Specification 2 of Table 5, we use coarsened exact matching (CEM) to match on all six covariates after we coarsen each into two strata based on median breakpoints. In Table IA.2, we match on three covariates using the default coarsening algorithm recommended by Blackwell, Iacus, King, and Porro (2009).[37] We report covariate names, the number of matched pairs, and difference-in-difference estimates, with t-statistics in parentheses.

Our results remain. For example, the difference-in-difference estimates for *Bias/Prc* range from -0.04 to -0.19, with a median of -0.13, and the t-statistics range from -1.59 to -5.29, with a median of -3.81. As a reference, the corresponding difference-in-difference estimate using CEM

[37] On average, the default algorithm coarsens the variables into 11 strata.

with two strata is -0.10 (Specification 2 in Table 5), and the corresponding estimate using propensity score matching is -0.13 (Table 4).

3. Exploring the Difference in Results between Early Treated Firms and Late Treated Firms

In Specification 9 of Table 5, we show that the reduction in bias for firms treated late (added to Estimize in 2013) is smaller than that for firms treated early (added to Estimize in 2012), and suggest that the weaker results are due to weaker treatment. Indeed, firms added early (late) are covered by 11.7 (2.5) Estimize contributors. An alternative explanation is that the weaker results are due to poorer matching. Consistent with this explanation, the mean absolute value of the difference in propensity scores between firms treated in 2012 and control firms is 0.17%; the corresponding figure for firms treated in 2013 is 0.10% (untabulated for brevity).

In Table IA.3, we seek to disentangle these explanations. Pooling early treated and late treated firms, we first regress the difference in *Abnormal Bias/Prc* between treated firm *j* and its propensity-score matched control firm on *Post* (an indicator variable coded one (zero) if the observation is from 2014-2015 (2009-2011), *Early* (an indicator variable coded one (zero) if a firm is added to Estimize in 2012 (2013)) and *Post * Early*, which measures the difference in the reduction in bias between the two groups. The coefficient *on Post * Early*, -0.09, is statistically and economically significant, consistent with firms added to Estimize in 2012 experiencing a greater decline in bias than firms added in 2013. As a reference, the coefficient on *Post*, -0.05, is statistically insignificant.

In Specification 2, we interact *Post* with *Strong Treatment*, an indicator variable equal to one (zero) if the ratio of Estimize forecasts to sell-side forecasts in 2012-2013, is above (below) the median.[38] The coefficient on *Post * Early* is reduced to -0.06 and is now only marginally significant (t-stat of -1.61), suggesting that difference in treatment strength partially explains the difference in results between firms added in 2012 and those added in 2013. Treatment strength distinctly affects the decline in sell-side bias, as evidenced by the significant negative coefficient on *Post * Strong Treatment*.

In Specification 3, we interact *Post* with *Poor Match*, an indicator variable equal to one (zero) if the absolute value of the difference in propensity scores between a treated firm and its matched control firm is above (below) the median. We do not find that *Post * Poor Match* reduces the coefficient on *Post * Early* or that it contributes to explaining the decline in bias. Specifically, the coefficient on *Post * Early* is -0.08 (t-stat of -2.05), while the coefficient on *Post * Poor Match* is -0.02 (t-stat of -0.43).

In Specification 4, we include all three interaction terms and continue to find similar results. We conclude that treatment strength rather than match quality accounts for the difference in results between firms added in 2012 and firms added in 2013, subject to the caveat that our proxy of match quality, a small difference in propensity scores, is imperfect.

[38] We scale Estimize coverage by sell-side coverage because the marginal effect of a one unit increase in competition is decreasing in the level of existing competition (Hong and Kacperczyk, 2010).

Table IA.1: Propensity Score Model Results

This table reports the odds ratios from the propensity score model discussed in Table 3, with z-scores reported in parentheses. The dependent variable equals one (zero) for a treated (untreated) firm. The independent variables are Log (*Size*), *Book-to-Market*, Log (*IBES Coverage*), *Turnover*, *Bias/Prc*, and *AbsFE*. We measure firm characteristics at the end of 2011 and forecast characteristics as quarterly averages over the period 2009-2011. See Appendix for detailed definitions. We standardize all independent variables to have mean zero and unit variance.

Log (Size)	2.22
	(6.02)
Book-to-Market	0.45
	(8.31)
Log (IBES Coverage)	1.74
	(4.21)
Turnover	1.83
	(5.31)
Bias/Prc	1.48
	(4.68)
AbsFE	0.50
	(6.79)
Percent Concordant	90.30%
Observations	1,253
Psuedo R-squared	43.75%

Table IA.2: Coarsened Exact Matching: Matching on Three Covariates using the Default Coarsening Algorithm Recommended by Blackwell, Iacus, King, and Porro (2009)

This table complements Specification 2 in Table 5. We match a treated firm to a control firm on three covariates, using Blackwell, Iacus, King, and Porro's (2009) default coarsening algorithm. We report covariates names, the number of matches, difference-in-difference estimates, and t-statistics based on standard errors double-clustered by matched control firm-quarter (in parentheses).

	Covariate #1	Covariate #2	Covariate #3	Obs.	Bias/Prc	Abnormal Bias/Prc.
1	*Size*	*Book-to-Market*	*IBES Coverage*	382	-0.15	-0.16
					(-4.46)	(-4.18)
2	*Size*	*Book-to-Market*	*Turnover*	502	-0.19	-0.18
					(-4.84)	(-4.38)
3	*Size*	*Book-to-Market*	*AbsFE*	483	-0.16	-0.15
					(-4.94)	(-4.82)
4	*Size*	*Book-to-Market*	*Bias/Prc*	568	-0.12	-0.11
					(-2.51)	(-2.26)
5	*Size*	*IBES Coverage*	*Turnover*	586	-0.15	-0.15
					(-4.31)	(-4.95)
6	*Size*	*IBES Coverage*	*Bias/Prc*	640	-0.11	-0.11
					(-3.77)	(-4.12)
7	*Size*	*IBES Coverage*	*AbsFE*	457	-0.14	-0.15
					(-4.33)	(-4.47)
8	*Size*	*Turnover*	*Bias/Prc*	631	-0.14	-0.14
					(-3.97)	(-3.53)
9	*Size*	*Turnover*	*AbsFE*	554	-0.16	-0.15
					(-5.29)	(-4.80)
10	*Size*	*Bias/Prc*	*AbsFE*	653	-0.07	-0.06
					(-2.63)	(-2.32)
11	*Book-to-Market*	*IBES Coverage*	*Turnover*	505	-0.13	-0.13
					(-3.19)	(-2.87)
12	*Book-to-Market*	*IBES Coverage*	*AbsFE*	469	-0.12	-0.11
					(-3.85)	(-4.10)
13	*Book-to-Market*	*IBES Coverage*	*Bias/Prc*	596	-0.07	-0.07
					(-1.84)	(-1.77)
14	*Book-to-Market*	*Turnover*	*AbsFE*	596	-0.13	-0.12
					(-3.94)	(-3.59)
15	*Book-to-Market*	*Turnover*	*Bias/Prc*	738	-0.09	-0.10
					(-3.46)	(-3.77)
16	*Book-to-Market*	*AbsFE*	*Bias/Prc*	730	-0.04	-0.03
					(-1.59)	(-1.21)
17	*IBES Coverage*	*Turnover*	*AbsFE*	531	-0.13	-0.12
					(-3.94)	(-3.59)
18	*IBES Coverage*	*Turnover*	*Bias/Prc*	697	-0.09	-0.10
					(-3.30)	(-3.49)
19	*IBES Coverage*	*AbsFE*	*Bias/Prc*	641	-0.06	-0.06
					(-2.07)	(-2.21)
20	*Turnover*	*Bias/Prc*	*AbsFE*	715	-0.07	-0.07
					(-2.75)	(-2.36)

Table IA.3: Exploring the Difference in Results between Early Treated Firms and Late Treated Firms

This table examines whether differences in treatment strength and match quality explain the difference in results between firms added to Estimize in 2012 (early treated) and firms added in 2013 (late treated). The dependent variable, *Dif Bias/Prc*, is the difference in *Abnormal Bias/Prc* between treated firm *j* and its propensity-score matched control firm in quarter *t*. *Post* indicates whether an observation is from the post event period of 2014-2015, coded one, or from the pre-event period of 2009-2011, coded zero. *Early* is equal to one (zero) if a firm is added to Estimize in 2012 (2013). *Strong Treatment* is equal to one (zero) if the ratio of Estimize forecasts to sell-side forecasts in 2012-2013 is above (below) than the median. *Poor Match* is equal to one (zero) if the absolute value of the difference between a treated firm's propensity score and its matched control firm's propensity score is above (below) the median. The sample includes 21,407 firm-quarter observations. The reported t-statistics (in parentheses) are computed based on standard errors double-clustered by control firm and quarter.

	[1]	[2]	[3]	[4]
Post	-0.05	-0.03	-0.04	-0.03
	(-1.21)	(-0.76)	(-1.11)	(-0.68)
Early	-0.02	-0.04	-0.02	-0.40
	(-0.84)	(-1.75)	(-0.85)	(-1.80)
*Post * Early*	-0.09	-0.06	-0.08	-0.06
	(-2.14)	(-1.61)	(-2.05)	(-1.57)
Strong Treatment		0.06		0.06
		(3.05)		(3.03)
*Post * Strong Treatment*		-0.06		-0.05
		(-2.08)		(-2.02)
Poor Match			-0.01	-0.01
			(-0.19)	(-0.21)
*Post * Poor Match*			-0.02	-0.01
			(-0.43)	(-0.42)