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UZH Business Working Paper Series

Working Paper No. 396

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December 2022

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UZH Business Working Paper Series

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Are Expectations Misled by Chance? Quasi-Experimental Evidence from Financial Analysts

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December 29, 2022

Abstract: We examine whether finance professionals deviate from Bayes' theorem on the processing of nondiagnostic information when forecasting quarterly earnings. Using field data from sell-side financial analysts and employing a regression discontinuity design, we find that analysts whose forecasts have barely been met become increasingly optimistic relative to when their forecasts have barely been missed. This result is consistent with an update of analysts' expectations after observing uninformative performance signals. Our results also suggest that this behavior leads to significantly worse forecasting accuracy in the subsequent quarter. We contribute to the literature by providing important field evidence of expectation formation under uninformative signals.

Keywords: Financial Analysts; Information Processing; Uninformative Signals; Outcome Bias; Regression Discontinuity Design

JEL Classification: D81; D83; D91; G41

Funding: This research did not receive any specific grants from funding agencies in the public, commercial, or not-for-profit sectors.

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1 INTRODUCTION

In everyday life, decisions must usually be made in noisy environments. According to Bayes' theorem, people are expected to update their prior beliefs upon the arrival of new diagnostic information. Often, however, people have preferences over which state of the world is true (Eil & Rao, 2011; Möbius et al., 2014), leading to situations where new information signals appear desirable or undesirable even if they are uninformative (Kieren & Weber, 2022). Thus, individuals may falsely update their expectations after uninformative information signals, which may impair many economic decisions, such as the evaluation and compensation decisions of managers, shareholder voting at annual general meetings, or investor forecasting of portfolio risks.

Previous research on reference dependence has shown that utility from an outcome depends on both the intrinsic value and the deviation from expectations (e.g., Baillon et al., 2020; Bell, 1985; Gul, 1991; Kahneman & Tversky, 1979; Koszegi & Rabin, 2006). Building on this literature, Gagnon-Bartsch and Bushong (2022) propose a model in which individuals fail to correctly account for their reference-dependent utility and misattribute these sensations of elation and disappointment to the intrinsic value of the outcome. As a consequence, this misinterpretation of outcomes leads to biased subsequent expectations.

Kieren and Weber (2022) translate the proposition that individuals face difficulties in separating the informational content of a signal from the reference-dependent sensations to signals that are inherently uninformative. Their model predicts that the greater personal utility (disutility) of an observed desirable (undesirable) uninformative signal induces people to form more optimistic (pessimistic) expectations. Using an experimental setting, Kieren and Weber (2022) find supporting empirical evidence for their model's predictions. In particular, the authors find that individuals strongly update prior beliefs after uninformative signals and form more optimistic (pessimistic) beliefs about the objective state after observing an uninformative signal that is better (worse) than some expectational reference point. However, field evidence of how individuals update prior beliefs after uninformative signals is missing thus far. In this paper, we examine whether and how professional sell-side financial analysts update their beliefs after uninformative performance signals.

This setting offers several key advantages. First, financial analysts are key participants in the financial market (Cen et al., 2013) who shape the corporate information environment (Li et al., 2021) through the acquisition of private information and execution of prospective and retrospective analyses to interpret past events (Guo et al., 2020). Through these activities, analysts help investors and stakeholders monitor their use of committed capital (e.g., Chen et al., 2010). Being experts in their field, analysts are well educated, well trained, and well motivated to make accurate forecasts (e.g., Beyer et al., 2010; Groysberg et al., 2011; Hilary & Hsu, 2013; Stickel, 1992), which is especially important given that mistakes have economically considerable consequences for themselves (e.g., for compensation and career trajectory) and investors (e.g., for investment decisions).

Second, analysts' forecasts (e.g., forecasted earnings per share (EPS)) and actual outcomes (e.g., actual EPS) can be directly observed and verified ex post (Cen et al., 2013). Third, an analyst issues distinct forecasts for multiple firms, which enables the examination of within-analyst variation over time. Finally, a firm is typically covered by multiple analysts, allowing for the study of the expectations of an analyst relative to those of his or her peers. In this way, analysts' optimism and accuracy for a given firm can be measured over time and relative to peer analysts (Hirshleifer et al., 2019; Hirshleifer et al., 2021).

To study expectation formation after uninformative signals among financial analysts, we rely on a quasi-experimental approach. Using the individual analyst's forecasts as his or her expectational reference point, we utilize a regression discontinuity design (RDD) and exploit the quasi-random variation in terms of whether a company's actual EPS barely meets or barely falls short of an individual analyst's forecast. While the size of deviation from actual EPS is an informative signal, barely exceeding or falling short of actual EPS can be considered random. Thus, this outcome is a conditionally uninformative signal, meaning that it provides no additional information beyond being close to the actual EPS.

To illustrate our empirical approach, let us consider the following example of a fictitious firm, ABC, with actual quarterly earnings of USD 2.35 in Q1 of a given year. Analyst X estimated an EPS of 2.40, while analyst Y estimated earnings of 2.30. The analysts are assumed to compare the actual EPS of firm ABC against their expectational reference points, i.e., their own forecasts. In this example, both analysts miss the actual EPS narrowly by USD 0.05 (or 2%). Conditional on the deviation of 2%, the signal showing that the actual forecast is below or above their own forecast is uninformative.

Based on the theoretical model of Kieren and Weber (2022), we expect that analysts form more optimistic beliefs after their forecasts have been barely met relative to their forecasts being barely missed. We test this prediction by using a large sample of analyst-firm quarterly earnings forecasts obtained from the Institutional Brokers' estimate System (I/B/E/S) database between 1984 and 2019. Following Hirshleifer et al. (2021), we operationalize optimism as the standardized deviation of the individual analyst's forecast from the consensus forecast, i.e., the mean of all forecasts, and employ the change in an analyst's optimism as our dependent variable. The graphical illustration of our RDD shows a discontinuous jump in optimism change for analysts whose forecasts have been barely met compared to those whose forecasts have been barely missed. Our formal estimation results support the graphical results: analysts whose forecasts have barely been met become significantly more optimistic than analysts whose forecasts have barely been missed.

The results remain robust under different RDD robustness tests, such as those varying the bandwidth and changing the polynomial degree, as well as placebo and balance tests of observable analyst characteristics. We also address the concern that firms or analysts may select into the treatment condition by excluding all exact hit forecasts and all forecasts that are close to the consensus forecast. Finally, we find that the unwarranted upward change in expectations for analysts whose forecasts have been barely met leads to worse forecast accuracy, as measured by the absolute forecasting error, relative to analysts whose forecasts have been barely missed. Taken together, these results show that financial analysts incorporate uninformative performance signals into subsequent forecasting. Moreover, our analyses suggest that analysts update their expectations depending on the valence of these uninformative signals, consistent with the idea that positive and negative emotions are misattributed to information about the underlying quality of the firm.

Our study makes several important contributions to the literature. First, we contribute to the literature by presenting an analysis of unique and naturally occurring field data that provide a rare opportunity to empirically investigate expectation formation under uninformative signals. In line with the theoretical predictions of Kieren and Weber (2022), our results suggest that analysts' prior beliefs become more optimistic after desirable uninformative performance signals relative to undesirable uninformative signals. Thus, the misattribution of reference-dependent preferences biases expectations, suggesting that even professional analysts cannot adequately disentangle their preferences from belief-relevant information.

Second, we contribute to the literature on forecasting behavior and biases among financial analysts. In the finance literature, an increasing strand of research documents biased forecasting behavior among financial analysts (e.g., Cen et al., 2013; Hirshleifer et al., 2019; Hirshleifer et al., 2021; Jannati et al., 2020; Pursiainen, 2020; Roger et al., 2018), which is consistent with the view that even finance professionals are characterized by some behavioral biases or deviation from rational Bayesian updating, leading them to miscalculate the earnings of covered firms (Cen et al., 2013; Roger et al., 2018). In light of the "behavioral analyst", we extend the literature by showing that analysts incorporate uninformative performance information into their decision-making process. Since relatively more accurate analysts are likely to experience more favorable career outcomes than relatively less accurate analysts (e.g., Hong & Kubik, 2003), nonrational updating may lead to worse career trajectories for financial analysts. Furthermore, this bias may have adverse consequences for other stakeholders who rely on analysts' accurate aggregation and interpretation of information, such as investors or firms.

Third, our study also contributes to the literature on outcome bias, which describes the tendency that people take outcome information into account when evaluating past decisions (e.g., Baron & Hershey, 1988). As a consequence, people judge decisions more favorably following a desirable outcome compared to an undesirable outcome. While there is ample evidence on outcome bias from laboratory studies (e.g., Brownback & Kuhn, 2019; König-Kersting et al., 2021), field evidence almost uniquely stems from the context of professional sports (Gauriot & Page, 2019; Kausel et al., 2019; Lefgren et al., 2015).[1] We contribute to this stream of the literature by showing that outcome bias also translates to high-stakes market settings such as the capital market, suggesting that the effect is likely to be widespread in other economic contexts. Moreover, our results imply that outcome bias also arises depending on individuals' prior expectations, as opposed to an objective reference point such as losing or winning a game (Kausel et al., 2019; Lefgren et al., 2015) and failing or passing an exam (Meier, Flepp, Meier, & Franck, 2022). Thus, what constitutes a "good" or "bad" outcome may be subjective and contingent on an individual's own reference point, such as his or her expectation.

More broadly, our study is also related to other strands in the economic literature on Bayesian updating, such as asymmetric belief updating following good and bad news (e.g., Barron, 2021; Burton et al., 2022; Eil & Rao, 2011; Kuhnen, 2015; Möbius et al., 2014), prior-biased reasoning (e.g., Charness & Dave, 2017; Rabin & Schrag, 1999; Zimmermann, 2020), over- or underreaction to recent signals (e.g., Amir & Ganzach, 1998; Bondt & Thaler, 1990;

[1] One notable exception is Meier et al. (2022) showing evidence of outcome bias in self-evaluations using data from car driving license exams.

Bordalo et al., 2019; Bordalo et al., 2020), or the misattribution of exogenous or situational factors (e.g., Weber et al., 2001). In a related spirit, our study adds to the growing empirical literature on how luck, chance, or random factors are incorporated into decision-making, such as in the context of executive compensation (e.g., Bertrand & Mullainathan, 2001; Daniel et al., 2020), chief executive officer (CEO) turnover decisions (Flepp, 2021; Jenter & Kanaan, 2015), strategic risk-taking of CEOs (Meier, Flepp, & Oesch, 2022), and voting behavior in the political economy (e.g., Achen & Bartels, 2004; A. J. Healy et al., 2010; Wolfers, 2002).

The remainder of this paper is structured as follows. In section 2, we discuss the research design. The results are presented in section 3. Section 4 concludes the paper with a discussion.

2 RESEARCH DESIGN

2.1 DATA AND VARIABLES

We obtain data from analysts' quarterly EPS forecasts from the Institutional Brokers' Estimate System (I/B/E/S) database. Our analysis focuses on one-quarter-ahead earnings forecasts, similar to the prior literature (e.g., Brown & Caylor, 2005; Hirshleifer et al., 2021; Kumar, 2010). The sample ranges from 1984 to 2019. We follow the established literature (e.g., Hirshleifer et al., 2019; Hirshleifer et al., 2021) regarding data preparation.[2] For our identification strategy (see below), we require two consecutive quarterly forecasts and include only firm-year observations with the sufficient coverage necessary for calculating our measure of relative optimism based on analyst consensus (we require at least five distinct analysts issuing forecasts in a given quarter). After these restrictions, our sample comprises 698,824 unique analyst-firm-quarter observations. Similar to Hirshleifer et al. (2021), we measure different analyst characteristics, such as job experience, complexity and specialization, that can influence forecasting behavior. The variables are defined in Table A1.

[2] We limit the analysis to forecasts within the 90 days before the actual earnings announcement for each firm in the fiscal quarter in question and eliminate analyst codes associated with teams of analysts. We also exclude utilities and financial services firms (Standard Industrial Classification codes 4900-4999 and 6000-6999) and firm-years that we could not match to CRSP/Compustat.

Our outcome variable is set up to capture optimism in a similar spirit to prior research (e.g., Clement, 1999; Cowen et al., 2006; Hirshleifer et al., 2021; Hong & Kubik, 2003; Jacob et al., 1999). Following Hirshleifer et al. (2021), the measure is based on the relative optimism against the consensus forecast, which is the mean value of those forecasts obtained from analysts issuing forecasts of a focal firm in the same fiscal quarter. Hence, we define this measure as follows:

$$Optimism_{i,j,t} = \frac{EPS\ Forecast_{i,j,t} - Consensus_{j,t}}{Standard\ Deviation(EPS\ Forecast_{i,j,t})}$$

where $EPS\ Forecast_{i,j,t}$ equals the value of the first earnings forecast of analyst *i* for firm *j* in quarter *t*. We standardize this value across all earnings forecasts by subtracting the consensus forecast (mean of all forecasts) among all analysts issuing forecasts for firm *j* in quarter *t* and dividing it by the standard deviation of all individual forecasts made in quarter *t*. The higher the value of this variable is, the more optimistic a forecast relative to peers for a given firm in a given quarter. To utilize a measure of *change* in the outcome variable following research with a similar identification strategy (e.g., Lefgren et al., 2015), we calculate the difference between the relative optimism for analyst *i* covering firm *j* in the focal quarter *t* and the relative optimism in the preceding quarter *t*-1.[3] Thus, $Optimism\_Change_{i,j,t}$ is defined as:[4]

$$Optimism\_Change_{i,j,t} = Optimism_{i,j,t} - Optimism_{i,j,t-1}$$

2.2 IDENTIFICATION STRATEGY

Ideally, we would randomly assign financial analysts to uninformative performance signals with different degrees of valence. However, naturally, this approach is not possible in a field setting. To overcome this limitation, we employ an RDD, which is used to estimate

[3] The measure of *Optimism* in quarter *t* is based on the first forecast issued by an analyst for firm *j*. The measure of *Optimism* in quarter *t*-1 is based on the last forecast issued by an analyst, as the last forecast is likely to be the most salient among all forecasts.

[4] Similar to Lefgren et al. (2015), we also tested a dummy variable as the outcome variable, capturing whether analysts change expectations upward. The variable equals 1 if $Optimism\_Change_{i,j,t}$ is equal to or greater than zero, and 0 otherwise. Our conclusions remain the same.

treatment effects in nonexperimental settings. The distinct feature of an RDD is that the treatment is quasi-randomly assigned based on whether an observable variable (running variable) exceeds a specific cutoff value (Klein Teeselink et al., 2022). Thus, the quasi-random variation in treatment status may be interpreted as the causal effect of the treatment if all other determinants of the outcome variable vary smoothly through the threshold (Imbens & Lemieux, 2008).

To derive quasi-random variation in uninformative performance signals, we utilize the deviation from an analyst's previous-quarter EPS forecast from the firm's actual EPS. Thus, we define an analyst's forecast as his or her expectational reference point against which he or she compares the actual EPS of a firm. We calculate a variable capturing the deviation from the actual EPS of firm *j* in the preceding quarter *t*-1 from analyst *j*'s previous forecast for firm *i* in that quarter relative to the actual EPS in that quarter:

$$Rel\_Dev\_Est_{j,i.t\text{-}1} = \frac{Actual_{j,t\text{-}1} - EPS\ Forecast_{i,j,t\text{-}1}}{Actual_{j,t\text{-}1}}$$

We denote this variable, $Rel\_Dev\_Est_{i,j.t\text{-}1}$, as the relative deviation in the actual EPS of firm *j* in quarter *t*-1 from the forecast of analyst *i* for firm *j* in quarter *t*-1. If this variable is positive, then firm *j*'s actual EPS exceeds the corresponding forecast of analyst *j* in preceding quarter *t*-1. We calculate a treatment indicator, $Meet\_Beat_{i,j.t\text{-}1}$, equaling 1 if $Rel\_Dev\_Est_{i,j.t\text{-}1}$ is equal to or greater than zero and 0 otherwise.

While the deviation from the actual EPS is an informative signal (e.g., missing the forecast by a large margin), the outcome of *narrowly* exceeding or falling short is (conditionally) uninformative. Importantly, however, these signals are likely to be different in terms of their valence to the analyst: unmet expectations are likely to elicit more negative (or less positive) feelings than are met expectations (e.g., Gagnon-Bartsch & Bushong, 2022; Kieren & Weber, 2022; Zhang & Covey, 2014). If the treatment status has a significant effect on forecasting behavior, then this would provide evidence that uninformative performance signals are incorporated and that expectations depend on the valence of the signal, stemming from the comparison against analysts' expectational reference point.

Throughout our study, we focus on two sets of results. First, we investigate graphically whether there is a visible discontinuity in forecasting behavior. Our second set of results includes more formal approaches. Thus, to test our research question, we estimate the following (local) RDD baseline specification:

$$Optimism\_Change_{i,j,t} = \alpha_0 + \beta_1 \times Meet\_Beat_{i,j,t-1} + \gamma_0 \times P_n\left(Rel\_Dev\_Est_{i,j,t-1}\right) \qquad \textbf{(1)}$$

$$+ \gamma_1 \times P_n\left(Rel\_Dev\_Est_{i,j,t-1}\right) \times Meet\_Beat_{i,j,t-1} + Analyst\ Controls_{i,j,t} + FEs + \varepsilon_{i,j,t}$$

P denotes a suitable polynomial function of $Rel\_Dev\_Est_{i,j,t-1}$ of order *n*. We allow for potentially different slopes on the left-hand side of the threshold and on the right-hand side of the threshold; thus, the coefficients on the polynomial terms are indexed by 0 and 1. We focus primarily on local, lower-order polynomial regression methods to estimate the treatment effect quantitatively, as suggested by Gelman and Imbens (2019). Higher-order polynomial degrees lead to noisy estimates, sensitivity to the degree of the polynomial, and poor coverage of confidence bandwidths (Cattaneo et al., 2019). Thus, our baseline model includes a second-order polynomial; however, we also test its sensitivity to a local linear regression.

Furthermore, we use a uniform kernel and a bandwidth of plus/minus five percentage points of the running variable ($Rel\_Dev\_Est_{i,j,t-1}$) to strike an appropriate balance between bias and precision associated with bandwidth use (Villamizar‐Villegas et al., 2022), meaning that we include all analyst forecast observations that deviate at most 5% from the actual EPS, i.e., are sufficiently close to the threshold. We acknowledge that these implementation choices are subject to certain discretion in RDD analyses. Thus, we test for the sensitivity of these choices as part of our robustness tests.

Throughout the analyses, the coefficient of interest in Equation (1) is β_1, which indicates the treatment effect of a firm meeting an individual analyst's forecast in the previous quarter on subsequent forecasting. To test the hypothesis that analysts become increasingly optimistic if their forecast has been barely met, we focus on β_1 and expect it to be positive and significant. This would be indicative of the conditionally uninformative outcome of a firm barely meeting an expectational reference point, i.e., his or her forecast, being interpreted more positively and thus positively affecting optimism relative to when expectations are missed.

In our baseline analyses, we control for several analyst characteristics. Similar to Hirshleifer et al. (2021), we include different fixed effects (FEs) at the firm, year-quarter, and analyst level or a combination of the last two (analyst-year-quarter FEs). If analyst-year-quarter FEs are employed, then the number of employed covariates is reduced due to collinearity. While firm FEs are used to capture time-invariant heterogeneity at the firm level, analyst-quarter FEs reduce remaining concerns that unobservable time-variant analyst characteristics could be correlated with the treatment (or control) condition and analyst forecasts. Although covariates are conceptually not needed in an RDD, our preferred specification uses firm and analyst-year-quarter FEs to control for this possibility and to increase the precision of our estimates. Finally, in all our models, we cluster standard errors at the analyst level to allow for possible correlation between the forecasts of the focal analysts.

3 RESULTS

3.1 BASELINE RESULTS

In Table 1, we present the descriptive statistics of the variables employed in our regressions. The variables (except for dummies) are winsorized at 5% and 95% to account for outliers. We show the mean, standard deviation, 25th percentile, median, and 75th percentile.

------- *Insert Table 1 about here* -------

We start by providing graphical evidence of the RDD using a bandwidth of 5%, which is also that used in our main analysis. In Figure 1, each dot represents the mean of the optimism change from quarter *t*-1 to quarter *t* within a bin of one percentage point. The solid line equals the predicted change in optimism estimated using a polynomial of the order two, which is allowed to differ on both sides of the cutoff. The same intuition applies for Figure 2, where we display all data within plus/minus 50% of the cutoff, i.e., within a large bandwidth.

------- *Insert Figure 1 & Figure 2 about here* -------

The figures indicate a visible increase in the change in relative optimism, consistent with our conjecture, providing, first, suggestive evidence that analysts incorporate uninformative performance signals and, second, that their expectations are raised if the forecast in the preceding quarter was barely met or exceeded.

To investigate these findings more formally, we estimate the RDD using Equation (1). The results are displayed in Table 2. We estimate Equation (1) with our baseline RD approach; i.e., we use a local second-order polynomial regression, which is allowed to differ on both sides of the cutoff, and a bandwidth of 5%. First, we estimate the model without any covariates and FEs (specification I). Then, we gradually increase the complexity until our preferred full model (specification IV) is achieved. To do so, we next present the estimation results with FEs at the firm, year-quarter, and analyst levels in specification II. Next, we add covariates to the model (specification III). Finally, we estimate our preferred full model and employ a combination of firm and analyst-year-quarter FEs, given that the optimism of an analyst may vary over time (specification IV).

------- *Insert Table 2 about here* -------

The results from specifications I to IV in Table 2 are consistent, suggesting that subsequent forecasting behavior differs under uninformative performance signals with different degrees of valence. As expected, the coefficients are positive and significant, suggesting that analysts whose forecasts have barely been met exhibit a more pronounced increase in optimism (pessimism) relative to their peers. The results are robust to the inclusion of covariates, analyst FEs and quarter FEs separately or combining them into analyst-quarter FEs to account for the possibility that some analysts may be more optimistic or pessimistic in general relative to their peers or that certain analysts react differently. The effect size varies from +0.161 in specification I, without any controls, and FEs to +0.120 in specification IV, with our full set of control variables.

3.2 ROBUSTNESS TESTS

3.2.1 SENSITIVITY TO IMPLEMENTATION CHOICE

We conduct several robustness tests to substantiate our main results. First, we use the local linear method proposed by Calonico et al. (2014a), who implemented a nonparametric local polynomial estimation method with optimal bandwidth selection and robust confidence intervals.[5] For an in-depth discussion of this methodology, we refer to Calonico et al. (2014b) and Calonico et al. (2019). For empirical implementation, we utilize the Stata command *rdrobust* developed by the authors (Calonico et al., 2014a; Calonico et al., 2017). The results are presented in Table 3. Our conclusions remain similar.[6] The optimal bandwidth, as suggested by the data-driven calculation, is approximately 4% and thus close to our baseline choice of 5%.

------- *Insert Table 3 about here* -------

Second, we again use our baseline model and employ local linear regression instead of a second-order polynomial degree. The results are shown in Table 4 and remain robust.

------- *Insert Table 4 about here* -------

[5] The selection of optimal bandwidth is data driven. That is, it is determined on the basis of a nonparametric approximation that is the result of a tradeoff between lower variance (associated with a larger bandwidth) and higher bias (associated with poorer parametric polynomial approximation when using a larger bandwidth) (Calonico et al., 2014a). The authors correct for the misspecification of the confidence intervals as a consequence of larger bandwidths, providing a new theory-based and more robust confidence interval estimator for average treatment effects at the cutoff using a bias-corrected RDD estimator, together with a novel standard error estimator.

[6] For computational reasons, all FEs are excluded.

In a next step, we also test different bandwidths for our outcome variable, which involves a tradeoff between bias and precision. A larger (smaller) bandwidth increases (diminishes) the misspecification error, thus increasing (decreasing) bias, but with a smaller (larger) variance (Villamizar-Villegas et al., 2022). In Figure 3**,** we plot the treatment coefficient for the same RDD baseline approach but using different bandwidths, both larger and smaller than our baseline bandwidth. We start by using a bandwidth of plus/minus 1% and increase it gradually up to a bandwidth of 50% to test the sensitivity to larger bandwidths (i.e., using observations that are further away from the cutoff). More formal estimation results are presented in Table A2. While our results are somewhat sensitive to the very narrow bandwidths of 1% and 3%, our conclusions remain the same for larger bandwidths.

------- *Insert Figure 3 about here* -------

3.2.2 VALIDATION AND BALANCE CHECKS

One of the main advantages of an RDD is that the mechanism that assigns units to treatment and control groups is known and based on observable features (Cattaneo et al., 2019). However, one of the core assumptions of an RDD is that units cannot *precisely* manipulate their treatment status.

As Figure 4 indicates, there is bunching at zero in our running variable, suggesting that the value of $Rel\_Dev\_Est_{j,i.t-1}$ is most often zero. This pattern could be explained by the fact that analysts are skilled and incentivized professionals and, hence, are likely to be relatively accurate. Hence, it could be considered natural to expect analysts' forecasts to align with the actual situation relatively often. However, this pattern could also stem from either firms or analysts (or both) selecting into the treatment of meeting or beating the forecast. Such a pattern would raise questions in terms of credibility to causal inference in our setting.

------- *Insert Figure 4 about here* -------

One plausible way in which selection might bias inference in our setting stems from firms having the discretion to manage their earnings (e.g., P. M. Healy & Wahlen, 1999). A broad strand of literature documents that firms actively engage in earnings management, such as accrual-based or real earnings management (e.g., Choi et al., 2022). For instance, Graham et al. (2005) find that managers are willing to decrease discretionary spending (e.g., on research and development (R&D)) to meet short-term earnings expectations.

However, while a firm has certain discretion over its earnings to reach a specific target (e.g., Habib & Hansen, 2008; Payne & Robb, 2000), note that it cannot *precisely* manage its earnings to meet (all) individual analyst forecasts, particularly when there is dispersion among analyst forecasts. Our data comprise multiple analysts covering the same firm, i.e., is at the analyst-firm level and thus more granular than firm level data. Therefore, the same firm may fall both into the treatment group (i.e., analyst X) and into the control group (i.e., analyst Y). Thus, even in the presence of earnings management of the firm, there should be variation in treatment and control status at the analyst-firm level.[7]

In addition to selection at the firm level, selection may also occur at the analyst level. It is well known that analysts herd (e.g., Trueman, 1994), i.e., that analysts follow their peers in forecasting earnings. Similarly, this situation may lead to a pattern of bunching observations, especially in conjunction with firms engaging in earnings management to reach a certain target.

To address these concerns surrounding manipulation at the analyst and/or firm level, we conduct further robustness tests. First, we exclude all exact hit observations, i.e., where the

[7] In our main regression model, we compare analyst X issuing forecasts for firm ABC with him- or herself over several years. Thus, sometimes, analyst X falls into the treatment group (i.e., his or her forecast is barely met) and, other times, into the control group (i.e., forecast is barely missed). We compare if the change in the degree of optimism of analyst X for firm ABC is different depending on treatment status.

forecast of the focal analyst equals the actual situation at t-1. The formal estimation results are robust to excluding these observations (see Table 5 Panel A).[8]

------- *Insert Table 5 about here* -------

Second, we exclude all analyst-firm-year-quarter observations that are relatively close to the consensus. The consensus forecast might be a credible target of the firm in the presence of earnings management (e.g., Bhojraj et al., 2009; Gentry & Shen, 2013). If firms aim at meeting or beating the consensus forecast, then there might be a relatively higher number of forecasts that are barely met relative to those that are barely missed. Furthermore, analysts might herd toward the consensus forecast (e.g., Trueman, 1994; Welch, 2000). Repeating the same estimation approach without forecasts that are in near proximity of the standardized consensus (i.e., lie in the 45% to 55% percentile of $Optimism_{i,j,t-1}$), our results, as presented in Table 5, Panel B, remain robust. The results also remain robust when applying a more conservative threshold (e.g., using the 40% to 60% percentile) and thus excluding more observations.[9] The results also remain robust when applying both restrictions at the same time, i.e., excluding all exact hit observations and forecasts that are relatively close to the consensus.

If analysts indeed lack the ability to precisely manipulate their treatment status, the group of analyst-firm forecasts that have been barely met are likely to be similar to the group of analyst-firm forecasts that have been barely missed, except for the treatment. Thus, one remaining concern is that the treatment group whose forecast has barely been met is

[8] We also test the robustness of the results to excluding firm-year-quarter observations with a high amount of exact hits. The intuition behind this check is that uncertainty in forecasting may have been low; thus, it might have been relatively easy to forecast EPS in this situation, leading to bunching at zero. We exclude all firm-year-quarter observations where at least 50% of analysts in our sample exactly hit the EPS (i.e., $Rel\_Dev\_Est_{j,i,t-1}$ equals zero), and the results remain virtually the same. The results also remain the same when a more conservative threshold is used and all firm-year-quarter observations where at least a quarter of analysts exactly hit the EPS are excluded.

[9] We also test the robustness of our results to excluding analyst-firm-year-quarter forecasts that occur with high frequency. Instead of the consensus forecast, firms might target the mode value of EPS forecasts, i.e., the forecast that occurs the most. Salient stimuli that stand out are easier to process and, therefore, receive more attention (e.g., Kahneman, 1973). Thus, similarly, we would expect bunching at zero in the case of firms systematically targeting to beat the mode value. We repeat the same estimation when excluding forecasts if they equal the mode value. Again, our conclusions remain the same when we exclude these forecasts.

systematically different than the control group. To mitigate such concerns, we perform various balance checks. As highlighted by Cattaneo et al. (2019), we analyze our set of covariates as if they were outcome variables; i.e., we use the same local polynomial regression model (1) for our predetermined covariates. If there was evidence of the null hypothesis of no treatment being rejected, then the validity of the design would come into question.

The results presented in Table 6 Panel A (including analyst, firm, and year-quarter FEs) and Panel B (including analyst-year-quarter and firm FEs) show no evidence of a discontinuity in any of the observable covariates employed in the main results, except for weak evidence in one specification for *Forecast_Age*. Thus, the analysts on the left and right of the cutoff are, at least in terms of observable characteristics, similar, which is a further indication that our RDD is valid.

------- *Insert Table 6 about here* -------

3.2.3 PLACEBO CUTOFFS

In addition to the balance checks of covariates, an important validity check is to examine a treatment effect at placebo cutoffs, testing whether the regression function is continuous at cutoffs other than the actual treatment cutoff. However, continuity at placebo cutoffs does not necessarily imply continuity at the cutoff in the absence of the treatment (Cattaneo et al., 2019). We examine artificial cutoffs in steps of one to three percentage points. We use the placebo cutoffs +3%, -3%, +6%, -6%, +9%, -9%, +10%, -10%, +12%, -12%, +15%, and -15% to check for discontinuities outside of our main bandwidth.[10]

Figure 5 graphically illustrates the results from these falsification tests, including the actual cutoff, using the same RDD approach. More formal estimations of the falsification tests

[10] Our utilized bandwidth of five percentage points overlaps with the discontinuity surrounding the actual cutoff (0%) in the case of the narrow alternative cutoffs of +3% and -3%. To avoid contamination with the actual cutoff, we exclude the observations on the right-hand side of the actual cutoff (0%) in the case using the placebo cutoff of -3% and exclude the observations on the left-hand side of the actual cutoff (0%) in the case of the placebo cutoff of +3%.

are presented in Table A3. Overall, we see no evidence of a systematic discontinuity for the placebo cutoffs, which leaves us confident that our identification strategy is valid.

------- *Insert Figure 5 about here* -------

3.3 CONSEQUENCES OF A RELATIVE INCREASE IN OPTIMISM

In the previous sections, we document that analysts become more optimistic than analysts whose forecasts have been barely missed. In the next step, we examine whether this relative increase in optimism change is associated with impaired forecast accuracy.

Similar to Huyghebaert and Xu (2016), we calculate the absolute forecasting error to obtain a measure of actual forecasting accuracy. We define the absolute forecasting error as follows:

$$Abs\_FError_{i,j,t} = \left| \frac{EPS\ Forecast_{i,j,t} - Actual_{j,t}}{Actual_{j,t}} \right|$$

where $EPS\ Forecast_{i,j,t}$ is again the value of the first earnings forecast of analyst i for firm j in quarter t and $Actual_{j,t}$ denotes the EPS reported by firm j for quarter t. The higher the $Abs\_FError_{j,i.t}$ value is, the less accurate the forecast of analyst i for firm j in quarter t. To study whether the observed behavior leads to consequences, we estimate the same baseline estimation (i.e., Equation 1) with the only change being the use of $Abs\_FError_{i,j,t}$ as the outcome variable.

The results are presented in Table 7. The coefficient in all four specifications is positive and significant. The results suggest that analysts whose forecasts have been barely met have a significantly higher forecasting error in the subsequent quarter relative to analysts whose forecasts have been barely missed, suggesting that the relative increase in optimism of analysts whose forecasts have been barely met also leads to worse forecasting accuracy compared to that of analysts whose forecasts have been barely missed.

------- *Insert Table 7 about here* -------

4 CONCLUSIONS

We present evidence that finance professionals in the field deviate from rational Bayesian updating by incorporating uninformative performance signals into their decision-making process. The results suggest that the direction in which analysts update their expectations depends on the valence of the uninformative performance signal, stemming from the comparison against their ex ante expectations. Expectations that are met (missed) are likely to induce positive (negative) feelings, which positively affect optimism (pessimism). The relative increase in optimism among analysts whose forecasts have been barely met relative to that of analysts whose forecasts have been barely missed leads to actual consequences in the form of worse forecasting accuracy, suggesting that the relative increase in optimism is costly for analysts.

Our findings have important implications for the literature. First, our results are in line with Kieren and Weber's (2022) theoretical model on expectation formation under uninformative performance signals, indicating that the behavior extends beyond an artificial laboratory setting. Although our rich field data allow us to test the main prediction of the authors, i.e., that individuals incorporate uninformative performance signals and update their expectation in part based on the valence of the signal, our quasi-experimental setting does not allow us to draw conclusions for Kieren and Weber's (2022) additional predictions. In particular, the authors further predict that more extreme outcomes lead to a stronger updating of expectations and that individuals who are actively invested exhibit a stronger bias. We leave these topics for future research.

Our results also advance our understanding of outcome bias. Our findings indicate that the evaluation of an outcome depends on an expectational reference point. What constitutes a "good" or "bad" outcome may vary across individuals, despite similar circumstances having led to the outcome. Thus, outcome bias is not only to be expected in settings with an objective reference point but also likely to be more widespread for decisions where individuals form subjective expectations. For instance, an employee's performance, which may be objectively good or bad, might be judged differently depending on his or her manager's expectations, i.e., whether the performance exceeds or falls short of managerial expectations, which could be different from an objective reference point.

Our findings also bear practical implications. Clients of brokerages should be aware that financial analysts (similar to themselves) are human and, thus, subject to cognitive limitations and emotions that can influence their professional decision-making. Given that capital market participants rely on the accurate representation and interpretation of information, such behavior might spill over and thus lead to further consequences. Thus, brokerages may benefit from designing procedures to adjust for emotions in generating earnings forecasts, accounting for the potentially misleading expectations of financial analysts. Similarly, domain-specific training might help in debiasing these kinds of decisions (Soll et al., 2015).

Since we examine highly incentivized professionals in a high-stakes context, our results have implications for managers setting performance goals for their employees, investors forecasting portfolio risks, and customers revising expectations after purchasing a product. Addressing how chance events affect expectation formation in other settings and how biased expectations can be resolved seem to be important and a fruitful path for further research.

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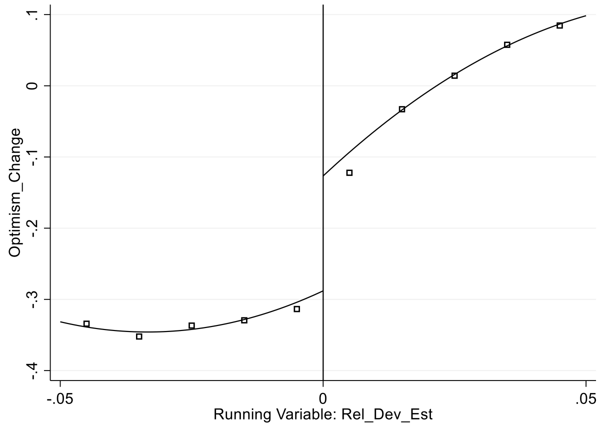
TABLE AND FIGURES

Table 1: Descriptive Statistics

Variable	N	Mean	SD	P25	Median	P75
Dependent variables						
Optimism_Change	698,824	-0.001	1.067	-0.747	0.000	0.747
Abs_FError	698,824	0.271	0.383	0.043	0.115	0.300
Independent variables						
Rel_Dev_Est	698,824	0.030	0.261	-0.030	0.036	0.133
Meet_Beat	698,824	0.704	0.456	0.000	1.000	1.000
Covered_Firms	698,824	12.602	5.451	8.000	12.000	16.000
Covered_Ind	698,824	4.456	2.882	2.000	4.000	6.000
Exp_Years	698,824	9.263	6.633	4.000	8.000	13.000
Exp_Firm	698,824	3.947	3.586	1.000	3.000	6.000
Specialization	698,824	0.560	0.496	0.000	1.000	1.000
Forecast_Age	698,824	72.315	21.499	64.000	83.000	89.000

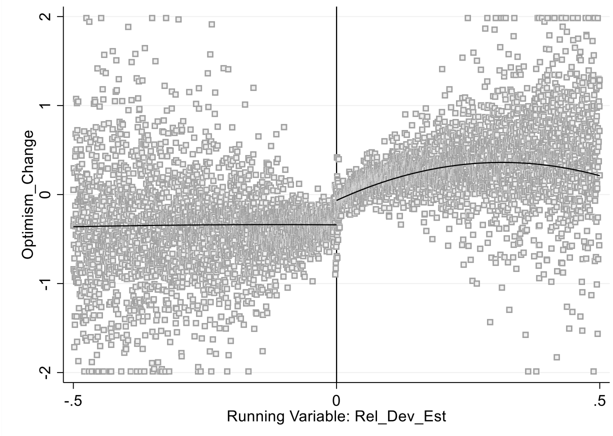
Notes: This table presents the descriptive statistics. The variables are defined in Table A1.

Figure 1: RDD Plot for Narrow Bandwidth



Notes: This figure shows the RDD plot within the bandwidth of -5% to 5% of the running variable. The local sample means of our dependent variable are plotted in the nonoverlapping bins of the running variable in steps of 1 percentage point. The model includes a second-order polynomial, which is allowed to differ on either side of the cutoff.

Figure 2: RDD Plot for Wide Bandwidth



Notes: This figure shows the RDD plot displaying all data points within the bandwidth of -50% to 50% of the running variable. The model includes a second-order polynomial, which is allowed to differ on either side of the cutoff.

Table 2: Main Results

Dependent variable: *Optimism_Change*

	I	II	III	IV
Meet_Beat	0.161***	0.165***	0.167***	0.120***
	(0.021)	(0.022)	(0.022)	(0.028)
Rel_Dev_Est	3.462**	3.855**	3.926**	9.555***
	(1.688)	(1.740)	(1.738)	(2.266)
Meet_Beat x Rel_Dev_Est	3.441*	5.051***	4.803***	-0.635
	(1.779)	(1.847)	(1.845)	(2.395)
$Rel\_Dev\_Est^2$	51.850*	41.433	40.801	138.217***
	(30.449)	(31.418)	(31.379)	(41.202)
Meet_Beat x $Rel\_Dev\_Est^2$	-99.901***	-107.123***	-104.949***	-203.757***
	(32.224)	(33.184)	(33.131)	(43.879)
Covered_firms	-	-	0.000	-
	-	-	(0.001)	-
Covered_industries	-	-	-0.001	-
	-	-	(0.002)	-
Exp_Years	-	-	0.003	-
	-	-	(0.003)	-
Exp_Firm	-	-	0.000	-0.001
	-	-	(0.001)	(0.001)
Specialization	-	-	0.011	0.004
	-	-	(0.008)	(0.012)
Forecast_Age	-	-	0.004***	0.003***
	-	-	(0.000)	(0.000)
Constant	-0.288***	-0.306***	-0.587***	-0.492***
	(0.021)	(0.021)	(0.036)	(0.032)
Firm FEs	No	Yes	Yes	Yes
Analyst-Yr.-Qr. FEs	No	No	No	Yes
Analyst FEs	No	Yes	Yes	Yes
Year-Quarter FEs	No	Yes	Yes	Yes
Bandwidth	+/- 5%	+/- 5%	+/- 5%	+/- 5%
Polynomial Degree	2	2	2	2
Controls	No	No	Yes	Yes
Observations	230,833	230,833	230,833	230,833

Notes: ***, ** and * denote significance at the 10%, 5% and 1% levels, respectively. P valueP-values are in parentheses. Standard errors are clustered at the analyst level. The estimated model corresponds to our baseline model., i.e., a local second-degree polynomial regression, which is allowed to differ on either side of the cutoff, estimated with a uniform kernel and a bandwidth of plus/minus five percentage points.

Table 3: Robust Data-Driven Inference in RDD

Dependent variable: *Optimism_Change*

		I	II
Conventional	Beta	0.179***	0.178***
		(0.014)	(0.015)
Robust	Beta	0.173***	0.172***
		(0.017)	(0.017)
	Kernel	Trian.	Trian.
	Polynomial Order	1	1
	Bandwidth	0.0407	0.0397
	#L	43,937	42,733
	#R	156,290	153,474
	Controls	No	Yes

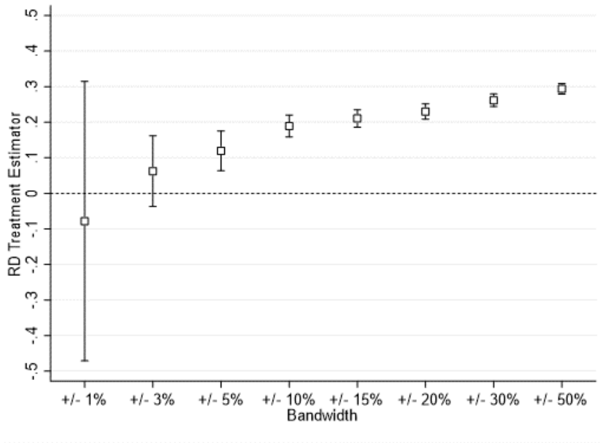
Notes: *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Standard errors are reported in parentheses. Conventional RDD estimates with a conventional variance estimator and bias-corrected RDD estimates with a robust variance estimator are reported, as suggested by Calonico et al. (2014b) and implemented by Calonico et al. (2014a). The sample includes observations within the optimal bandwidth selected by a common mean squared error (MSE)-optimal bandwidth selector (Calonico et al., 2017). The model is estimated using a triangular kernel and includes a first-degree polynomial, which is allowed to differ on either side of the cutoff.

Table 4: Local Linear Regression

	Dependent variable: ***Optimism_Change***			
	I	II	III	IV
Meet_Beat	0.197***	0.201***	0.203***	0.211***
	(0.012)	(0.012)	(0.012)	(0.016)
Firm FEs	No	Yes	Yes	Yes
Analyst-Yr.-Qr. FEs	No	No	No	Yes
Analyst FEs	No	Yes	Yes	Yes
Year-Quarter FEs	No	Yes	Yes	Yes
Bandwidth	+/- 5%	+/- 5%	+/- 5%	+/- 5%
Polynomial Degree	2	2	2	2
Controls	No	No	Yes	Yes
Observations	230,833	230,833	230,833	230,833

Notes: ***, ** and * denote significance at the 10%, 5% and 1% levels, respectively. P-values are in parentheses. Standard errors are clustered at the analyst level. The estimated model corresponds to a local linear regression, which is allowed to differ on either side of the cutoff, estimated with a uniform kernel and a bandwidth of plus/minus five percentage points.

Figure 3: Robustness to Bandwidth Selection



Notes: This figure displays the point estimators using alternative bandwidths and the respective 95% confidence intervals.

Table 5: Robustness to Exclusion of Forecasts

Dependent variable: *Optimism_Change*

Panel A
Exclusion rule: all exact hit observations (N=66,663)

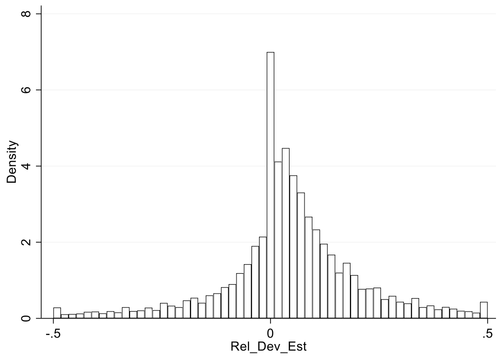
	I	II	III	IV
Meet_Beat	0.122***	0.124***	0.126***	0.078**
	(0.026)	(0.027)	(0.027)	(0.036)
Observations	164,200	164,200	164,200	164,200

Panel B
Exclusion rule: analyst-year-quarter observations when close to consensus (N=69,882)

	I	II	III	IV
Meet_Beat	0.173***	0.167***	0.169***	0.136***
	(0.024)	(0.025)	(0.025)	(0.034)
Observations	179,514	179,514	179,514	179,514
Firm FEs	No	Yes	Yes	Yes
Analyst-Yr.-Qr. FEs	No	Yes	Yes	Yes
Analyst FEs	No	No	No	No
Year-Quarter FEs	No	Yes	Yes	No
Bandwidth	+/- 5%	+/- 5%	+/- 5%	+/- 5%
Polynomial Degree	2	2	2	2
Controls	No	No	Yes	Yes

Notes: ***, ** and * denote significance at the 10%, 5% and 1% levels, respectively. P-values are in parentheses. Standard errors are clustered at the analyst level. The estimated model corresponds to our baseline model., i.e., a local second-degree polynomial regression, which is allowed to differ on either side of the cutoff, estimated with a uniform kernel and a bandwidth of plus/minus five percentage points.
In Panel A, all exact hit observations are excluded. In Panel B, all analyst-firm-year-quarter observations that are in near proximity to the consensus (i.e., lie in the 45% to 55% range of $Optimism_{i,j,t-1}$) are excluded.

Figure 4: Histogram – All Data



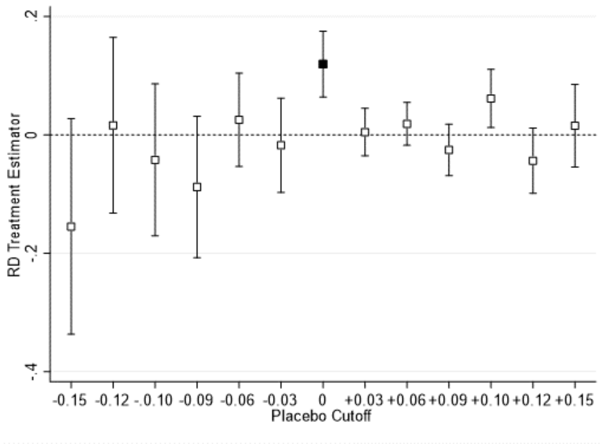
Notes: The distribution of the running variable between -50 and +50 percentage points of *Rel_Dev_Est*.

Table 6: Balance Checks

	Covered_Firms	Covered_Inustries	Exp_Years	Exp_Firm	Specialization	Forecast_Age
Panel A	I	II	III	IV	V	VI
Meet_Beat	0.010	0.004	-0.008	-0.024	0.002	-0.698*
	(0.071)	(0.030)	(0.019)	(0.056)	(0.006)	(0.419)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Analyst-Yr.-Qr. FEs	No	No	No	No	No	No
Analyst FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FEs	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	+/- 5%	+/- 5%	+/- 5%	+/- 5%	+/- 5%	+/- 5%
Polynomial Degree	2	2	2	2	2	2
Controls	No	No	No	No	No	No
Observations	230,833	230,833	230,833	230,833	230,833	230,833
Panel B	I	II	III	IV	V	VI
Meet_Beat	0.000	-0.000	0.000	-0.018	0.002	-0.408
	(0.000)	(0.000)	(0.000)	(0.069)	(0.007)	(0.512)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Analyst-Yr.-Qr. FEs	Yes	Yes	Yes	Yes	Yes	Yes
Analyst FEs	No	No	No	No	No	No
Year-Quarter FEs	No	No	No	No	No	No
Bandwidth	+/- 5%	+/- 5%	+/- 5%	+/- 5%	+/- 5%	+/- 5%
Polynomial Degree	2	2	2	2	2	2
Controls	No	No	No	No	No	No
Observations	230,833	230,833	230,833	230,833	230,833	230,833

Notes: ***, ** and * denote significance at the 10%, 5% and 1% levels, respectively. P-values are in parentheses. Standard errors are clustered at the analyst level. The estimated model corresponds to our baseline model., i.e., a local second-degree polynomial regression, which is allowed to differ on either side of the cutoff, estimated with a uniform kernel and a bandwidth of plus/minus five percentage points.

Figure 5: Falsification Checks



Notes: This figure displays the point estimators using alternative bandwidths and the respective 95% confidence intervals.

Table 7: Consequences of a Relative Increase in Optimism

	Dependent variable: ***Abs_FError***			
	I	II	III	IV
Meet_Beat	0.121***	0.042***	0.043***	0.027***
	(0.005)	(0.004)	(0.004)	(0.005)
Firm FEs	No	Yes	Yes	Yes
Analyst-Yr.-Qr. FEs	No	No	No	Yes
Analyst FEs	No	Yes	Yes	No
Year-Quarter FEs	No	Yes	Yes	No
Bandwidth	+/- 5%	+/- 5%	+/- 5%	+/- 5%
Polynomial Degree	2	2	2	2
Controls	No	No	Yes	Yes
Observations	230,833	230,833	230,833	230,833

Notes: ***, ** and * denote significance at the 10%, 5% and 1% levels, respectively. P-values are in parentheses. Standard errors are clustered at the analyst level. The estimated model corresponds to our baseline model., i.e., a local second-degree polynomial regression, which is allowed to differ on either side of the cutoff, estimated with a uniform kernel and a bandwidth of plus/minus five percentage points.

APPENDIX

Table A1: Variable Definition

Variable	Calculation	Description
Dependent variables		
Optimism_Change	$Optimism\_Change_{i,j,t} = Optimism_{i,j,t} - Optimism_{i,j,t\text{-}1}$ $Optimism_{i,j,t} = \frac{EPS\ Forecast_{\ i,j,t} - Consensus_{j,t}}{Standard\ Deviation(EPS\ Forecast_{\ i,j,t})}$	The change in the analyst's optimism from the focal quarter and the preceding quarter. *Optimism* against consensus EPS forecast of the firm in the fiscal quarter in consideration. *EPS Forecast* denotes the focal analyst's forecast. The consensus is based on the mean value of analysts issuing a forecast for the focal firm. The denominator is the standard deviation of all forecasts made by analysts forecasting earnings of the focal firm in that quarter.
Abs_FError	$Abs\_FE_{i,j,t} = \left\lvert \frac{EPS\ Forecast_{\ i,j,t} - Actual_{j,t}}{Actual_{j,t}} \right\rvert$	The absolute value of the relative difference in a firm's actual earnings in a fiscal quarter (*Actual*) and the analyst's first earnings forecast (*EPS Forecast*) for the firm in that quarter.
Independent variables		
Rel_Dev_Est	$Rel\_Dev\_Est_{j,i,t\text{-}1} = \frac{Actual_{j,t\text{-}1} - EPS\ Forecast_{\ i,j,t\text{-}1}}{Actual_{j,t\text{-}1}}$	The relative deviation in a firm's actual earnings in the preceding fiscal quarter and the analyst's last earnings forecast for the firm in that quarter.
Meet_Beat	Indicator variable equaling 1 if a firm's actual earnings in the preceding fiscal quarter meets or beats the analyst's first earnings forecast for the firm in that quarter and 0 otherwise.	-
Covered_Firms	The number of firms the analyst follows in a particular quarter.	-
Covered_Ind	The number of distinct industries (3-digit SIC) the analyst follows in a particular quarter.	-
Exp_Years	Current year—first year the analyst appears in the sample.	-
Exp_Firm	Current year—first year the analyst starts following the firm in consideration in the sample.	-
Specialization	Indicator variable equaling 1 if the analyst follows at least five firms in the same industry (3-digit SIC) and 0 otherwise.	-
Forecast_Age	Earnings announcement date minus EPS forecast date.	-

This table presents the definitions of the variables employed.

Table A2: Robustness to Bandwidth Selection

Dependent variable: *Optimism_Change*

Panel A	I	II	III	IV
Meet_Beat	-0.078	0.062	0.120***	0.189***
	(0.200)	(0.051)	(0.028)	(0.016)
Firm FEs	Yes	Yes	Yes	Yes
Analyst-Yr.-Qr. FEs	Yes	Yes	Yes	Yes
Analyst FEs	No	No	No	No
Year-Quarter FEs	No	No	No	No
Bandwidth	+/- 1%	+/- 3%	+/- 5%	+/- 10%
Polynomial Degree	2	2	2	2
Controls	Yes	Yes	Yes	Yes
Observations	79,318	159,294	230,833	359,837

Panel B	I	II	III	IV
Meet_Beat	0.211***	0.230***	0.262***	0.294***
	(0.012)	(0.011)	(0.009)	(0.008)
Firm FEs	Yes	Yes	Yes	Yes
Analyst-Yr.-Qr. FEs	Yes	Yes	Yes	Yes
Analyst FEs	No	No	No	No
Year-Quarter FEs	No	No	No	No
Bandwidth	+/- 15%	+/- 20%	+/- 30%	+/- 50%
Polynomial Degree	2	2	2	2
Controls	Yes	Yes	Yes	Yes
Observations	442,074	491,348	555,168	615,275

Notes: ***, ** and * denote significance at the 10%, 5% and 1% levels, respectively. P-values are in parentheses. Standard errors are clustered at the analyst level. The model is estimated using a uniform kernel. The estimated model corresponds to our baseline model., i.e., a local second-degree polynomial regression, which is allowed to differ on either side of the cutoff, estimated with a uniform kernel.

Table A3: Falsification Tests

Dependent variable: *Optimism_Change*

Panel A	I	II	III	IV	V	VI
Meet_Beat	-0.155*	0.016	-0.042	-0.088	0.026	-0.018
	(0.093)	(0.076)	(0.065)	(0.061)	(0.040)	(0.041)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Analyst-Yr.-Qr. FEs	Yes	Yes	Yes	Yes	Yes	Yes
Analyst FEs	No	No	No	No	No	No
Year-Quarter FEs	No	No	No	No	No	No
Cutoff	-15%	-12%	-10%	-9%	-6%	-3%
Bandwidth	+/- 5%	+/- 5%	+/- 5%	+/- 5%	+/- 5%	+/- 5%
Polynomial Degree	2	2	2	2	2	2
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	34,933	45,287	54,119	60,420	86,405	76,174

Panel B	I	II	III	IV	V	VI
Meet_Beat	0.016	-0.044	0.062**	-0.025	0.019	0.005
	(0.036)	(0.028)	(0.025)	(0.022)	(0.018)	(0.020)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Analyst-Yr.-Qr. FEs	Yes	Yes	Yes	Yes	Yes	Yes
Analyst FEs	No	No	No	No	No	No
Year-Quarter FEs	No	No	No	No	No	No
Cutoff	+15%	+12%	+10%	+9%	+6%	+3%
Bandwidth	+/- 5%	+/- 5%	+/- 5%	+/- 5%	+/- 5%	+/- 5%
Polynomial Degree	2	2	2	2	2	2
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	97,760	129,498	153,618	169,323	214,293	176,399

Notes: ***, ** and * denote significance at the 10%, 5% and 1% levels, respectively. P-values are in parentheses. Standard errors are clustered at the analyst level. The estimated model corresponds to our baseline model., i.e., a local second-degree polynomial regression, which is allowed to differ on either side of the cutoff, estimated with a uniform kernel and a bandwidth of plus/minus five percentage points.