The Use of External Performance Expectations in the Target Setting of Executive Annual Bonus Contracts

Sewon Kwon
Lecturer
Seoul National University
E-mail: k4js11@gmail.com

Sunhwa Choi
Assistant Professor
Lancaster University and *Sungkyunkwan University*
E-mail: s.choi@lancaster.ac.uk

Sunyoung Kim
Assistant Professor
Monash University
E-mail: sunyoung.kim@monash.edu

Jae Yong Shin
Professor
Seoul National University
E-mail: jshin@snu.ac.kr

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ABSTRACT

Although anecdotal evidence suggests that externally informed approaches are increasingly used in target-setting practices, only limited evidence in the literature addresses what types of external information sources are used in target setting. We examine how externally informed performance expectations, such as analysts' annual earnings forecasts, influence target setting for executive bonus contracts. Using performance target data from S&P 1500 firms, we find that analyst earnings forecasts are positively associated with firms' bonus target revisions and that target accuracy improves with the incorporation of analyst forecasts into target setting. Furthermore, we find that the use of analyst forecasts in target setting is more pronounced when the forecasts are more informative about future firm performance and when they are less likely to be influenced by managers. Our results are robust to a battery of sensitivity tests.

Keywords: *Executive compensation; external performance expectations; annual bonuses; analyst forecasts; performance targets*

Data Availability: *Data are available from public sources indicated in the text.*

1. Introduction

Performance targets are a key component of firms' management control and compensation systems, because how firms set targets and revise them over time affects executives' incentives (Indjejikian and Nanda 2002). To optimally motivate a firm's executives, their performance targets should reflect the best estimate of expected performance that can be achieved with adequate levels of effort (Van der Stede 2000). Firms rely on various sources of information when setting and revising targets because information asymmetry between firms and managers makes it difficult for firms to set accurate targets (Indjejikian and Nanda 2002, Indjejikian et al. 2014b, Bouwens and Kroos 2017).

Research on target setting shows that past performance is an important source of information used to set targets. Firms typically revise targets upward following favorable performance and downward following unfavorable performance; this practice is called "target ratcheting" (Weitzman 1980). The literature notes that the use of past performance in target setting introduces a dynamic incentive problem known as the "ratchet effect," because target ratcheting motivates managers to withhold effort in the current period to avoid higher targets in the future (Leone and Rock 2002, Bouwens and Kroos 2011).

Because the use of past performance in target setting introduces complex incentive effects and its ability to predict future performance is limited, past performance is usually complemented by other sources of information (Indjejikian et al. 2014b). However, the academic literature on target setting provides only limited evidence on the use of these other sources of information. Using data from a retail travel company, Aranda et al. (2014) provide evidence that firms use peer performance information in target setting. Bouwens and Kroos (2017) show that firms incorporate non-financial information, such as customer service, in target setting. Based on survey data, Dekker et al. (2012) report that internal planning information is widely used in target setting and that the use of internal benchmarking (i.e., the comparison of units within the firm) and external benchmarking (i.e., comparison between firms) is relatively limited.

Although these findings provide insight into how firms use information sources other than past performance in target setting for the business units of an organization (Aranda et al. 2014, Bouwens and

Kroos 2017) or for middle-level managers (Dekker et al. 2012), the literature has not yet fully explored the use of alternative information sources in target setting for executive compensation contracts at the largest firms, due to a lack of publicly available data on performance targets.

Theories and anecdotal evidence suggest that externally informed approaches can be useful in target setting. Using targets based on external standards that are not affected by managers can mitigate the ratchet effect because managers would not be able to affect next-period targets by manipulating current performance (Murphy 2000). In addition, external sources can provide incremental information that is not available from internal sources (Indjejikian et al. 2014b) and thus improve target accuracy. Recent studies provide empirical evidence that peer performance, one type of external standards, is used for target setting for divisions within a firm (Aranda et al. 2014, Bol and Lill 2015). Anecdotal evidence also supports the use of externally informed approaches in target setting. For example, the 2007 survey by Mercer (2009) indicates that 55% of participating firms use "externally informed absolute" numbers to set short-term incentive targets.

In this study, we focus on the use of externally informed performance expectations to set targets for executive annual bonus contracts. Performance expectations by external parties provide independent and direct estimates of firm-specific performance, and reflect an additional information set of external parties that provide the estimates. External performance expectations are readily available for public firms in the form of analyst earnings forecasts. Thus, in the target setting of senior executives of public firms, external performance expectations provide useful information that is independent, informative, and less costly to obtain (Holmstrom 1979, Murphy 2000).

We examine whether firms use external performance expectations to revise targets in executive bonus contracts by analyzing the performance target data from S&P 1500 firms, collected from the compensation discussion and analysis (CD&A) section of their proxy statements. We focus on earnings per share (EPS) targets in executive annual bonus contracts because EPS is the most widely used performance measure (Graham et al. 2005). As an empirical proxy for external performance expectations, we use the consensus of analysts' annual earnings forecasts that are available before the approval date of the annual bonus plan.

Using a sample of 1,179 firm-year observations for fiscal years 2006 through 2014, we find a significant association between analyst forecasts and firms' bonus target revisions, after controlling for past performance, peer performance, expected growth, the presence of management forecasts, and other control variables (Aranda et al. 2014, Indjejikian et al. 2014b, Kim and Shin 2017). This finding suggests that firms use analyst forecasts as an important external information source in revising bonus targets. Furthermore, using a test suggested by Bouwens and Kroos (2017), we provide evidence that incorporating analyst forecasts into target setting improves target accuracy.

Next, we conduct two cross-sectional tests to examine under what circumstances the use of external performance expectations in target setting is more pronounced. Based on the theory that the relative weight on each information source in target setting depends on its informativeness about the agent's actions and the extent to which the information is influenced by managers (Holmstrom 1979, Murphy 2000), we predict that firms place more emphasis on external performance expectations in target setting when they are more informative about future firm performance (Murphy 2000, Ittner et al. 2003, Bouwens and Kroos 2017) and when they are less likely to be influenced by managers (Murphy 2000). By using analysts' forecast errors, forecast dispersion, the number of analysts following a firm, and the correlation between a firm's performance and the gross domestic product (GDP) (Hutton et al. 2012) as proxies for the informativeness of analyst forecasts, we find that the use of analyst forecasts in target setting increases when analyst forecast errors are low, when forecasts are less dispersed, when the number of analysts following the firm is high, and when firm performance is strongly correlated with GDP. These findings support the value of external performance expectations that are informative about future firm performance.

In addition, using the subsample of firms for which EPS forecasts of both affiliated analysts (i.e., those with an investment banking relationship with the firm) and unaffiliated analysts exist, we find that firms put more weight on the forecasts of unaffiliated analysts in target setting than on the forecasts of affiliated analysts. We also provide evidence that the weight on analyst forecasts issued *before* management forecasts is greater than the weight on analyst forecasts issued *after* management forecasts. These findings are consistent with the notion that firms' use of external performance expectations is more pronounced when they are less likely to be influenced by managers. Overall, our main findings suggest that firms' use of external performance expectations in target setting varies in a manner consistent with the economic theory on target setting (Murphy 2000).

A potential alternative explanation for our main findings is that they merely reflect the effect of internal planning information that has been communicated to both analysts and compensation committees. To check this possibility, we conduct several tests using management forecasts as a proxy for internal planning information. The results of these tests show that our main findings hold even after controlling for the information in management forecasts, suggesting that they are not driven by the effect of management forecasts on analyst forecasts. Another potential concern is that our results may reflect that analysts revise their forecasts based on information about internal bonus targets (i.e., reverse causality).[1] However, we find no evidence that analysts revise their forecasts around the approval date of the bonus plan, mitigating the concern about reverse causality. We also examine whether the use of analyst forecasts varies depending on managers' incentives to meet or beat analysts' expectations (i.e., equity incentives) and find no evidence supporting this argument. Our results are robust to a battery of robustness checks.

We make several contributions to the literature on target setting. First, by providing large sample empirical evidence on the use of externally informed performance expectations in target setting, we respond to a call for research by Indjejikian et al. (2014b) to examine the use of externally informed performance targets in recent target-setting practices. We predict and find that external performance expectations, such as analyst forecasts, can convey information not captured by past performance and peer performance and

[1] Our use of analyst forecasts issued *before* the approval date of the annual bonus plan in the empirical tests mitigates this concern. Furthermore, bonus plan details are not typically publicly available until proxy statements are disclosed. Nevertheless, managers may communicate this information to analysts before the approval date of the bonus plan (e.g., through management forecasts) to manage market expectations and to make it easier to beat the forecasts. We explore these possibilities in Sections 6.1 and 6.2.

that they are incorporated in firms' target setting process. Furthermore, we provide empirical evidence that firms' use of external performance expectations systematically varies as predicted by the economic theory on target setting (Murphy 2000). Like Bouwens and Kroos (2017), who document the use of non-financial information in setting financial targets, we extend previous research that has exclusively focused on past performance and peer information as the sources of information in target setting.

Second, we use data on the performance targets of large U.S. public firms to shed light on how external information is incorporated into their target-setting processes for CEOs' annual bonus plans. While a growing body of research provides evidence that various information sources, such as peer performance, are used to set targets for business units (Aranda et al. 2014, Bol and Lill 2015, Bouwens and Kroos 2017), it is still unclear whether these earlier findings hold true for the target setting of top executives of public firms. For example, whereas the benefit of internal benchmarking is clear for business units conducting homogeneous business, the value of external benchmarking is questionable due to limited data on industry peer performance and the subjective nature of peer group selection (Hansen et al. 2003, Dekker et al. 2012). Furthermore, the information set available for public firms is different from that for business units, suggesting that different approaches are warranted for public firms. For example, analyst forecast information is available only at the firm level, not at the business unit level. Based on a large cross section of firms, we provide new evidence on the use of external performance expectations to set targets for executive bonus contracts in public firms.

Finally, we enhance the understanding of the relation between internal and external performance targets. Previous research has generally assumed that firms consider internal targets (for bonus determination) and external targets (for meeting market expectations) separately, with little interaction between the two.[2] For example, Matsunaga and Park (2001, p. 314) argue, "The effect of missing a quarterly earnings benchmark on a CEO's bonus is likely to result from the compensation committees' exercise of

[2] The CFOs surveyed in Graham et al. (2005) indicate that firms set higher internal targets than external targets to ensure that the latter are not difficult to attain. However, Armstrong et al. (2017) report that external targets exceed internal EPS targets in 60% of their sample.

their discretion in the allocation of the bonus pool, as opposed to the benchmark's being an explicit component of the plan." However, our evidence that firms update their internal bonus targets based on external targets suggests that firms *ex ante* incorporate the information about external benchmarks into an internal target-setting process, rather than making discretionary adjustments *ex post*. In a contemporaneous study, Armstrong et al. (2017) also examine the interaction between internal and external EPS goals as a source of CEO incentives. They find that external EPS goals provide CEOs with stronger incentives than internal EPS goals because of CEOs' significant equity-based incentives to meet analysts' expectations. Our study is different from that by Armstrong et al. (2017) in that our focus is on the target setting process, whereas Armstrong et al. (2017) focus on the relative incentive effects of external and internal targets on CEO behaviors. We consider external targets (i.e., external performance expectations) as an alternative source of information that can be used in setting internal targets to improve the efficiency of target setting. In contrast, Armstrong et al. (2017) emphasize the role of external targets in the context of meeting or beating analyst forecasts in the capital market. We thus believe that our study complements their findings in improving the understanding of the interaction between internal and external targets.

The remainder of this paper proceeds as follows. Section 2 reviews the literature and develops the hypotheses. Section 3 describes the research design and sample. Section 4 presents the descriptive statistics. Section 5 reports the empirical results. Section 6 contains additional analyses. Section 7 concludes.

2. Related Literature and Hypothesis Development

Performance targets serve as an important basis for business decision making, such as choosing investments and evaluating performance (Ittner and Larcker 2001). The goal of performance targets in incentive contracts is to provide managers with incentives to increase firm value while simultaneously compensating them competitively (Murphy 2000). Therefore, setting appropriate performance targets is important to optimally motivate managers. Given the information asymmetry between firms and managers, firms can improve their contracts by fully exploiting various sources of information to set adequate performance targets (Indjejikian and Nanda 2002, Indjejikian et al. 2014b, Bouwens and Kroos 2017). For example, firms strive to improve the accuracy of targets, because more accurate performance standards (i.e., those with less deviation from actual performance) reduce managers' compensation risk and increase their motivation, thereby improving the efficiency of incentive contracts (Murphy 2000). Prior research shows that firms can improve target accuracy by incorporating all available information when setting performance standards (Indjejikian and Nanda 2002, Bouwens and Kroos 2017).

Murphy (2000) suggests that firms determine the relative weight on each source of information in target setting based on the three criteria: the information collection cost, the ability to predict future performance (i.e., accuracy or informativeness), and the extent to which managers can influence the information. We begin by discussing each possible source of information, such as past performance, based on the prior target setting literature and then develop our hypotheses on the use of external performance expectations in target setting based on Murphy's (2000) theoretical framework.

The literature identifies past performance, internal planning, and benchmarking information as important information sources in target setting (Dekker et al. 2012). Past performance information is widely used for target setting because it is readily available and informative about future performance. Target ratcheting occurs when firms revise targets upward following favorable performance and downward following unfavorable performance (Weitzman 1980). The magnitude of the target adjustment made after a favorable performance variance is usually greater than the target adjustment made after the same magnitude of an unfavorable performance variance, suggesting an asymmetry in ratcheting (Leone and Rock 2002, Kim and Shin 2017). Target ratcheting has negative incentive effects because managers are motivated to withhold their effort in the current period to avoid difficult targets in the future (i.e., the ratchet effect). Consistent with this argument, Bouwens and Kroos (2011) show that retail store managers with favorable performance in the first three quarters reduce their sales activity in the fourth quarter, suggesting that managers manipulate real economic activities in the current period to influence future targets.[3]

[3] Firms can mitigate the ratchet effect by making a contractual commitment to disregard information about past performance when setting future targets (Laffont and Tirole 1993). Consistent with this argument, empirical studies

Past performance is typically complemented by internal planning information (e.g., firms' business plans and budgets). Internal planning information is potentially more informative about future performance than past performance because it incorporates managers' forward-looking information. Consistent with this conjecture, Dekker et al. (2012) report that internal planning information is widely used in target setting to complement past performance information. However, internal planning information is subject to manipulation and game playing because managers themselves determine it. For example, Anderson et al. (2010) document that when a performance-based bonus plan with participative goal setting was introduced in the stores of a U.S. retail firm, managers set lower goals and tended to just meet the targets. In addition, as internal planning information is typically used for multiple purposes, such as coordinating, planning, and evaluating performance, it is not clear whether this information reflects the best (unbiased) estimate of future performance, which is required for optimal target setting for incentive contracts.

External information sources that are not affected by managers can be useful in target setting because using external information in target setting mitigates the negative incentive effect of target ratcheting. Managers would have no incentive to manage current performance to achieve easy future targets when the next-period target is determined by external standards. Consistent with this argument, Murphy (2000) reports that firms with externally determined performance standards (e.g., standards based on the performance of external peer groups, timeless standards, and standards based on the cost of capital) are less likely to smooth earnings than firms with internally determined standards (e.g., standards based on budgets or past performance).

One type of external sources in target setting is peer performance. Information about peer performance can be used to assess the common components of performance across the peer group because

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provide evidence that firms do not fully consider managers' past performance when setting future targets (Indjejikian and Nanda 2002, Aranda et al. 2014, Indjejikian et al. 2014a, Bol and Lill 2015). For example, Indjejikian et al. (2014a) find that target revisions for well-performing managers are sensitive to past unfavorable performance variances and less sensitive to past favorable performance variances, suggesting that firms reward well-performing managers with rents. While these findings are consistent with the benefit of a long-term contractual commitment in addressing the ratchet effect, the commitment to disregarding information about past performance increases the demand for other sources of information.

the groups is subject to common shocks. Using data from a retail travel company, Aranda et al. (2014) find that supervisors use information about the relative performance of comparable branches in target setting. Bol and Lill (2015) show that the performance of business units relative to their peers affects the degree of target ratcheting. Although these studies provide empirical evidence that internal benchmarking within a firm is used in target setting, there is limited evidence on the use of external benchmarking in target setting, because data about industry peer performance are often costly to obtain and available only with a time lag (Hansen et al. 2003). Consistent with these limitations of external benchmarking, Dekker et al. (2012) report that external benchmarking is used much less than past performance or internal planning data to set targets for middle-level managers.

Another potential external source of information in target setting, which has not received much attention in the literature, is externally informed performance expectations. There are several reasons why external performance expectations can be useful in setting targets for incentive contracts, particularly for the senior executives of public firms. First, external performance expectations provide an independent and direct estimate of firm-specific performance. Second, they reflect an additional information set of external parties that provide the estimates. Finally, they are readily available for public firms, for example, in the form of analyst forecasts. Thus, in terms of the extent to which managers can influence the information, the informativeness, and the cost of information collection (Murphy 2000), external performance expectations should be useful for target setting.[4, 5]

Anecdotal evidence is also consistent with the use of external performance expectations in the

[4] Theory provides another reason why external performance expectations can be useful in improving the efficiency of incentive contracts. Mittendorf and Zhang (2005) provide a principal-agent model in which the principal (owner) benefits from relying on the performance forecasts of independent external agents (i.e., analysts), even when the agent (manager) provides earnings guidance based on her private earnings observation. In this model, analysts act as information intermediaries to discipline the agent because they, faced with the possibility that the manager's guidance is biased, conduct their own research, rather than repeating the manager's guidance. In other words, firms can improve their incentive contracts by using the interaction/tension between managers and external agents who provide independent estimates of firm performance.

[5] In addition, the emphasis on meeting or beating earnings benchmarks (Graham et al. 2005) and the need to justify the performance targets in incentive contracts under extensive compensation disclosure requirements (Indjejikian et al. 2014b) also contribute to the increasing use of external performance expectations in target setting.

practice of target setting. For example, the 2014 proxy statement of Biogen Inc. states that the firm considers analysts' projections for firm performance in setting annual goals in addition to internal forecasts and peer performance (see Appendix A for details). Mercer (2009), a compensation-consulting firm, also recommends the use of analysts' expectations as an important source of external information.

Based on the preceding discussion, we state our first hypothesis as follows:

> ***H1****: External performance expectations are positively associated with adjustments to performance targets in executive bonus contracts.*

Next, we discuss our cross-sectional predictions about the use of external performance expectations in target setting, based on Murphy's (2000) three criteria for evaluating the information sources in target setting: information collection cost, informativeness, and the extent to which the information is influenced by managers. To the extent that the cost of collecting information about external performance expectations is low, firms are expected to determine the weight on external performance expectations based on their informativeness and the extent to which they are influenced by managers (i.e., independence). Following the arguments of Ittner et al. (2003), we assume that a measure's predictive ability for future financial performance provides a reasonable proxy for its informativeness in contracting. Therefore, we expect firms to allocate more weight in target setting to external performance expectations when they are more informative about future firm performance. Moreover, to the extent that managers can affect external performance expectations, we predict that firms allocate more weight to external performance expectations when they are less likely to be influenced by managers. Based on these discussions, we present our second and third hypotheses as follows:

> ***H2****: The use of external performance expectations in the revision of target setting is more pronounced when they are more informative about future firm performance.*

> ***H3****: The use of external performance expectations in the revision of target setting is more pronounced when they are less likely to be influenced by managers.*

3. Research Design and Sample

3.1. Research Design

As an empirical proxy for external performance expectations, we use the consensus of analysts' annual earnings forecasts that are available before the annual bonus plan is determined. Sell-side analysts, who are employed by brokerage firms or independent research firms, provide their forecasts about firms' earnings in their research reports, along with their estimates of target prices and stock recommendations. The consensus earnings forecasts, which aggregate all available individual forecasts, are readily available from commercial websites or data vendors (e.g., I/B/E/S and Zacks).

To examine the association between target revisions and analyst forecasts, we build on target setting research (Leone and Rock 2002, Bouwens and Kroos 2011, Aranda et al. 2014, Indjejikian et al. 2014a, Kim and Shin 2017). Specifically, we estimate the following ordinary least squares (OLS) regression to test H1 (i.e., whether external performance expectations are positively associated with target revisions in executive bonus contracts):

$$\begin{aligned} \textit{Target revision}_{i,t+1} = {} & \lambda_0 + \lambda_1 \textit{ Target deviation}_{i,t} + \lambda_2 \textit{ Target deviation}_{i,t} \times \textit{D\_NEG}_{i,t} \\ & + \lambda_3 \textit{ Analyst forecast dev}_{i,t+1} + \lambda_4 \textit{ Relative-to-peers}_{i,t} + \lambda_5 \textit{ Growth}_{i,t+1} \\ & + \lambda_6 \textit{ D\_NEG}_{i,t} + \lambda_7 \textit{ Ret}_{i,t} + \lambda_8 \textit{ Guide}_{i,t+1} + \textit{Year and industry fixed effects} + \varepsilon_{i,t} \qquad (1) \end{aligned}$$

The dependent variable is *Target revision*$_{i,t+1}$, which is defined as (*Target EPS*$_{i,t+1}$ − *Target EPS*$_{i,t}$) divided by *Target EPS*$_{i,t}$. *Target deviation*$_{i,t}$ is a proxy for past actual performance relative to the performance target, defined as (*Actual EPS*$_{i,t}$ − *Target EPS*$_{i,t}$) divided by *Target EPS*$_{i,t}$. To capture any asymmetry in target ratcheting, we include an indicator variable for unfavorable performance variances, *D_NEG*$_{i,t}$, and its interaction with *Target deviation*$_{i,t}$ (Leone and Rock 2002, Kim and Shin 2017). *D_NEG*$_{i,t}$ equals 1 if *Target deviation*$_{i,t}$ is negative and 0 otherwise.

Our main variable of interest is *Analyst forecast dev*$_{i,t+1}$, analysts' estimates of future EPS over the realized EPS. Specifically, *Analyst forecast dev*$_{i,t+1}$ is defined as (*Analyst forecast*$_{i,t+1}$ − *Actual EPS*$_{i,t}$) divided by *Target EPS*$_{i,t}$, where *Analyst forecast*$_{i,t+1}$ is the average of the most recent forecasts of year t+1 earnings issued over the period from the announcement of year t earnings to the approval date of the annual bonus plan for year t+1.[6] If the approval date is missing, we use the end date of the 3 months after the fiscal year-end as the approval date. This way of measuring analyst forecasts ensures that they are not affected by stale forecasts and that analysts incorporate information about year t earnings. Figure 1 illustrates the timeline of analyst forecasts used in our analyses. A positive coefficient on $\textit{Analyst forecast dev}_{i,t+1}$ indicates that targets are revised upward when analysts expect earnings for year t+1 to be greater than the realized earnings for year t. If analyst forecasts do not convey any incremental information over other variables in the model (e.g., past performance and peer performance), λ_3 would not be different from 0.

[*Insert Figure 1 here*]

Recent studies on target setting argue that managers' past performance relative to peers affects future targets and the extent of target ratcheting, which underscores the importance of controlling for peer performance (Aranda et al. 2014, Indjejikian et al. 2014 a and b, Bol and Lill 2015). Therefore, we include a measure for a firm's performance compared to its peers ($\textit{Relative-to-peers}_{i,t}$), which we define as the firm's basic EPS for year t minus industry peers' average basic EPS for year t. We follow Albuquerque (2009) to construct industry-size-matched peer portfolios.[7] We also include firms' stock returns ($Ret_{i,t}$) to control for events that affect both future target revisions and analyst forecasts.[8] To the extent that stock returns reflect all publicly available information, controlling for stock returns can mitigate the possibility that our model merely captures an association due to the common information available to both analysts and compensation committees. $Ret_{i,t}$ is the firm's stock returns over the 12-month period that ends 3 months after fiscal year-end t. To control for the presence of management forecasts, we include $Guide_{i,t+1}$, an

[6] The approval date of the annual bonus plan for year t+1 is on average 53 days after the end of fiscal year t.
[7] To calculate peer performance, we construct peer portfolios matched on industry and firm size. First, we form annual portfolios based on two-digit standard industry classification (SIC) codes. We use all of the firms on Compustat to construct the portfolios. Second, we sort firms by beginning-of-year market value into size quartiles. Third, we match each firm with an industry-size peer group. Peer performance is the equal-weighted portfolio EPS for an industry-size peer group. When calculating portfolio EPS, we exclude the EPS of the observed firm.
[8] For example, when oil prices increase, both firms and analysts may revise their expectations of future performance downward, resulting in a positive correlation between the target revisions and analyst forecasts. By controlling for stock returns, which contain information about public news, we expect $\textit{Analyst forecast dev}_{i,t+1}$ to reflect analysts' unique information set beyond public information.

indicator variable that equals 1 if a firm issues management forecasts over the period from the announcement of year *t* earnings to the approval date of the annual bonus plan for year *t*+1.

Indjejikian et al. (2014b) suggest that the relation between target revisions and past performance may be attributable to firm-specific growth. To control for the effect of firm-specific growth in target setting (Aranda et al. 2014, Indjejikian et al. 2014 a and b, Kim and Shin 2017), we include anticipated growth in EPS ($Growth_{i,t+1}$) in our model. Specifically, we measure $Growth_{i,t+1}$ as the predicted value from the following model:

$$\begin{aligned} EPS\ growth_{i,t+1} = {} & \alpha_0 + \alpha_1\ Past\ EPS\ growth_{i,t} + \alpha_2\ Size_{i,t} + \alpha_3\ EP_{i,t} + \alpha_4\ Leverage_{i,t} \\ & + \alpha_5\ MKT_{i,t} + \alpha_6\ RD_{i,t} + \alpha_7\ CAP_{i,t} + \alpha_8\ BTM_{i,t} + \alpha_9\ Div\ yield_{i,t} \\ & + \alpha_{10}\ Past\ RET_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{2}$$

The dependent variable in Equation (2) is $EPS\ growth_{i,t+1}$, which is defined as the EPS growth between year *t* and *t*+1. Following research on the factors that affect the growth of accounting earnings and sales (Chan et al. 2003, Ciftci and Cready 2011, Gong and Li 2013), we control for past growth in EPS over the previous 3 years (*Past EPS growth*), the natural logarithm of the market value of equity (*Size*), the earnings-to-price ratio (*EP*), leverage (*Leverage*), advertising expenses divided by sales (*MKT*), the average of R&D expenses divided by sales over the previous 3 years (*RD*), the average of capital expenditures divided by total assets over the previous 3 years (*CAP*), the book-to-market ratio (*BTM*), the dividend yield ratio (*Div yield*), and stock returns over the previous 12 months (*Past RET*). We estimate Equation (2) separately for each fiscal year and two-digit SIC code group.[9]

3.2. Sample

Our initial sample consists of S&P 1500 firms for the fiscal years of 2006 to 2014. We focus on firms that use EPS as a performance measure in their CEOs' annual bonus contracts because EPS is the most widely used performance measure in annual bonus contracts and analysts' EPS forecasts are widely available. We hand-collect EPS targets, thresholds, and maximums and actual EPS for CEOs from the

[9] Section 6.4.1 discusses alternative ways to measure expected growth.

CD&A sections of proxy statements. Detailed information on each plan is disclosed in the "Short-term Incentives" section.[10] Appendix B provides an example of the CD&A section of a proxy statement for El Paso Electric Company. This company provides information on performance measures (EPS with 50% weight), performance targets ($2.30), and actual performance ($2.27) in its 2014 executive annual bonuses.

We obtain stock return data from CRSP and financial statement data from Compustat. We collect analyst forecast data from I/B/E/S. Table 1 summarizes our sample selection procedure. Of the initial sample of 13,140 firm-years, 3,303 firm-years use EPS as a performance measure for CEO annual bonus contracts. We exclude firm-years that lack EPS target information and those with missing analyst forecast information and control variables.[11] As our main regression model requires EPS target and performance data for at least two consecutive years, we further exclude firm-years that lack such data. Our final sample consists of 1,179 firm-year observations over the 2006 to 2014 period.

[*Insert Table 1 here*]

4. Descriptive Statistics

Table 2 provides the descriptive statistics for our key variables, including $\textit{Target revision}_{i,t+1}$, $\textit{Target deviation}_{i,t}$, $\textit{Analyst forecast dev}_{i,t+1}$, and several firm characteristics. For ease of interpretation, we also present the descriptive statistics of the unscaled variables: ($\textit{Target EPS}_{i,t+1} - \textit{Target EPS}_{i,t}$), ($\textit{Actual EPS}_{i,t} - \textit{Target EPS}_{i,t}$), and ($\textit{Analyst forecast}_{i,t+1} - \textit{Actual EPS}_{i,t}$). The mean (median) values of ($\textit{Target EPS}_{i,t+1} - \textit{Target EPS}_{i,t}$) and $\textit{Target revision}_{i,t+1}$ are $0.202 ($0.215) and 11.6% (9.8%), respectively, suggesting that EPS targets are typically revised upward by 20.2 cents (11.6% of prior target EPS). The mean of ($\textit{Actual EPS}_{i,t} - \textit{Target EPS}_{i,t}$) is $0.039, whereas the actual EPS is 1.7% higher than the target (i.e., $\textit{Target deviation}_{i,t}$) on average, indicating that actual performance is, on average, slightly higher than the target. The mean values of ($\textit{Analyst forecast}_{i,t+1} - \textit{Actual EPS}_{i,t}$) and $\textit{Analyst forecast dev}_{i,t+1}$ are $0.164 and

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[10] Our main analyses use the target value and thus do not incorporate information about the threshold or maximum (target) value. In an untabulated analysis, we find that thresholds and maximums are commonly set to 80% and 120% of the target value. For example, the mean (median) values of the thresholds and maximums in our sample are 82.5% (86.7%) and 119.5% (114.8%), respectively. The practice of setting thresholds and maximums to 80% and 120% of the target value is observed across most of the industries in our sample.

[11] If a stock split occurs during the year, we exclude that firm-year observation from our sample.

11.0%, respectively, indicating that analyst EPS forecasts are 16.4 cents (11%) higher than prior actual EPS on average. The means of the market value of equity and book value of total assets are $12,530 million and $16,629 million, respectively. Lastly, the mean (median) value of *Relative-to-peers*$_{i,t}$ is 0.774 (0.602).

[*Insert Table 2 here*]

Table 3 provides the correlation matrix of our key variables. (*Target EPS*$_{i,t+1}$ − *Target EPS*$_{i,t}$) is positively associated with (*Actual EPS*$_{i,t}$ − *Target EPS*$_{i,t}$), suggesting that targets tend to be revised in the same direction as past performance variances. (*Target EPS*$_{i,t+1}$ − *Target EPS*$_{i,t}$) is also positively correlated with (*Analyst forecast*$_{i,t+1}$ − *Actual EPS*$_{i,t}$), providing univariate evidence to support our first hypothesis that external performance expectations are positively associated with the revision of targets. The positive correlation between (*Target EPS*$_{i,t+1}$ − *Target EPS*$_{i,t}$) and *Growth*$_{i,t+1}$ highlights the importance of controlling for firm-specific growth in the empirical models that attempt to explain firms' target revisions (Indjejikian et al. 2014b).

[*Insert Table 3 here*]

5. Empirical Results

5.1. Association Between External Performance Expectations and the Revision of Targets (H1)

Table 4 reports the results of estimating Equation (1). Column (1) shows the baseline results without *Analyst forecast dev*$_{i,t+1}$. Consistent with previous studies, the coefficient on *Target deviation*$_{i,t}$ is significantly positive, suggesting that firms consider past performance variances in revising targets (i.e., target ratcheting). The coefficient on *Target deviation*$_{i,t}$ × *D_NEG*$_{i,t}$ is significantly negative, consistent with previous findings that targets tend to ratchet asymmetrically (Leone and Rock 2002, Bouwens and Kroos 2011, Aranda et al. 2014, Kim and Shin 2017). A significantly negative coefficient on *Relative-to-peers*$_{i,t}$ suggests that firms outperforming their peers (i.e., those with higher values of *Relative-to-peers*$_{i,t}$) experience downward target revisions in the subsequent period (Aranda et al. 2014, Indjejikian et al. 2014a, Kim and Shin 2017).

[*Insert Table 4 here*]

Column (2) of Table 4 provides the estimation results to test H1 that external performance expectations are positively associated with the target revisions in bonus contracts. The coefficient on *Analyst forecast dev*$_{i,t+1}$ is positive and significant, suggesting that analysts' performance expectations above (below) the realized past performance are associated with upward (downward) revisions in targets. This finding is consistent with the argument that firms use analyst forecasts as an external information source in revising future bonus targets, going above and beyond information about past and relative performance. In terms of economic significance, the magnitude of the coefficient (0.376) indicates that an interquartile change in *Analyst forecast dev*$_{i,t+1}$ from the first quartile (Q1) to the third quartile (Q3) is associated with an 8.08% increase in future targets. Furthermore, including the analyst forecast variable increases the adjusted R^2 from 58.03% in Column (1) to 69.68% in Column (2), highlighting the importance of analyst forecasts as an additional information source in target setting.

We also note that the coefficient on peer performance, which is significant in Column (1), becomes marginally insignificant once analyst forecast information is included in Column (2). This finding indicates that analyst forecasts subsume peer information as an information source in target setting, consistent with a limited use of external benchmarking in the target-setting practice.[12] Overall, the results in Table 4 support our first hypothesis that external performance expectations (i.e., analyst forecasts) are positively associated with target revisions.

As a supplementary analysis, we test whether incorporating analyst forecasts in target setting improves target accuracy. Based on Indjejikian and Nanda's (2002) argument, Bouwens and Kroos (2017) design a test examining whether firms set more accurate targets when non-financial information is incorporated in target setting in addition to financial information. They find that sales target deviations are less serially correlated when both financial and non-financial information are incorporated in target setting, suggesting that target accuracy improves with the inclusion of non-financial measures into target setting.

[12] In Section 6.4.1, we examine whether the insignificant results for peer performance are due to measurement errors in our approach to identifying peer groups based on firm size and industry membership. We find that using an alternative way to identify peer groups does not change the results.

Similarly, we examine whether the inclusion of analyst forecasts enhances target accuracy by comparing the extent to which current target deviations explain next-period target deviations when firms incorporate analyst forecasts into target setting, compared to when firms do not incorporate analyst forecasts. Specifically, we estimate the following equation:

$$|Target\ deviation_{i,t+1}| = \lambda_0 + \lambda_1\ |Target\ deviation_{i,t}| + \varepsilon_{i,t} \quad (3)$$

$|Target\ deviation_{i,t}|$ is the absolute value of ($Actual\ EPS_{i,t} - Target\ EPS_{i,t}$) divided by $Target\ EPS_{i,t}$. Following Bouwens and Kroos (2017), we define $|Target\ deviation_{i,t+1}|$ in two different ways. The first is the absolute value of the actual target deviation in period t+1, measured as the absolute value of ($Actual\ EPS_{i,t+1} - Target\ EPS_{i,t+1}$) divided by $Target\ EPS_{i,t}$. The second is the counterfactual target deviation in period t+1 assuming that analyst forecast information is not used in target setting, measured as the absolute value of ($Actual\ EPS_{i,t+1} - Predicted\ Target\ EPS_{i,t+1}$) divided by $Target\ EPS_{i,t}$. $Predicted\ Target\ EPS_{i,t+1}$ is estimated as the sum of $Target\ EPS_{i,t}$ and the predicted target revision for year t+1 from Equation (1) that excludes $Analyst\ forecast\ dev_{i,\ t+1}$. Because a significant serial correlation in target deviations indicates that all available information is not fully incorporated into targets (i.e., less accurate), to the extent that incorporating analyst forecasts into targets improves target accuracy, we predict that the ability of current target deviations to explain next-period target deviations (i.e., R^2 from Equation (3)) is lower when analyst forecasts are incorporated into target setting than when they are not incorporated.

Table 5 presents the results. In Column (1), in which the dependent variable is the absolute value of the actual target deviation in period t+1 (i.e., next-year targets incorporate analyst forecasts), the coefficient on $|Target\ deviation_{i,t}|$ is 0.559 and the R^2 is 15.15%. In Column (2), in which the dependent variable is the counterfactual target deviation in period t+1 (i.e., next-year targets do not incorporate analyst forecasts), the coefficient is 0.652 and the R^2 is 16.62%. The Vuong (1989) test statistic shows that the difference in the R^2 between the two models is statistically significant. The higher R^2 of the second model suggests that current target deviations explain next-period target deviations more when the targets do not incorporate analyst forecasts, supporting the idea that incorporating analyst forecasts improves target accuracy.

[*Insert Table 5 here*]

5.2. Effects of the Informativeness of External Performance Expectations on Their Use in Target Setting (H2)

Next, we test whether the use of external performance expectations in target setting is more pronounced when they are more informative about future firm performance (H2). We use two approaches to test this prediction. First, we use analysts' forecast accuracy, their dispersion, and the number of analysts following the firm to proxy for the accuracy/noisiness of analyst forecasts and predict that the use of analyst forecasts increases when analyst forecast errors are low, when forecasts are less dispersed, and when more analysts provide estimates.[13] While intuitive, this approach has the limitation that these proxies may simply reflect the overall information uncertainty of the firm (e.g., due to volatile business) instead of the informativeness of analyst forecasts. Therefore, we use the second approach of measuring the relative information advantage of analysts versus managers because it enables us to pinpoint the source of the relative advantage of analyst forecasts compared to other sources (e.g., managers' own information). Hutton et al. (2012) document that analyst forecasts are more accurate than management forecasts approximately 50% of the time and then examine the circumstances in which analysts' forecasts are more accurate than managers' forecasts. They find that analysts' information advantage comes from their superior ability to assess macroeconomic factors. Specifically, analyst forecasts are more accurate than management forecasts

[13] Although our approach using analyst forecast dispersion as a proxy for forecasting uncertainty is common in the literature, forecast dispersion can alternatively proxy for the extent to which analyst forecasts are influenced by managers (i.e., H3), as it is more difficult for managers to guide analysts when forecasts are more dispersed (Cotter et al. 2006). Therefore, one can argue that more dispersed forecasts represent more independent information and thus are given more weight in target setting, leading to an opposite prediction to ours in H2. However, the relation between managers' guidance and forecast dispersion is not clear. For example, Houston et al. (2010) report that managers are more likely to stop providing guidance when analyst forecast dispersion is high because it is difficult for managers to predict future earnings with precisions when forecast uncertainty is high (as reflected in high analyst forecast dispersion), suggesting that managers' ability to guide analysts can be negatively associated with forecast dispersion. Thus, we make no specific prediction about the relation between dispersion and analyst independence (i.e., H3). In addition, although a higher number of analysts following the firm can also indicate managers' difficulty in influencing the consensus, this leads to the same prediction as ours that the weight on analyst forecasts increases with the number of analysts following the firm.

for firms whose earnings are highly exposed to macroeconomic factors, such as GDP.[14] Based on their finding, we predict that firms put more weight on analyst forecasts in target setting when their performance is strongly correlated with macroeconomic factors, a situation in which analysts have an information advantage over managers. We estimate the following OLS regression to test these predictions:

$$
\begin{aligned}
\textit{Target revision}_{i,t+1} = {} & \lambda_0 + \lambda_1\, \textit{Target deviation}_{i,t} + \lambda_2\, \textit{Target deviation}_{i,t} \times \textit{D\_NEG}_{i,t} \\
& + \lambda_3\, \textit{Analyst forecast dev}_{i,t+1} \\
& + \lambda_4\, \textit{Analyst forecast dev}_{i,t+1} \times \textit{Forecast error quartile}_{i,t} \\
& + \lambda_5\, \textit{Analyst forecast dev}_{i,t+1} \times \textit{Forecast dispersion quartile}_{i,t+1} \\
& + \lambda_6\, \textit{Analyst forecast dev}_{i,t+1} \times \textit{Analyst following}_{i,t+1} \\
& + \lambda_7\, \textit{Analyst forecast dev}_{i,t+1} \times \textit{Cyclicality}_{i,t} + \lambda_8\, \textit{Forecast error quartile}_{i,t} \\
& + \lambda_9\, \textit{Forecast dispersion quartile}_{i,t+1} + \lambda_{10}\, \textit{Analyst following}_{i,t} \\
& + \lambda_{11}\, \textit{Cyclicality}_{i,t} + \lambda_{12}\, \textit{Relative-to-peers}_{i,t} + \lambda_{13}\, \textit{Growth}_{i,t+1} + \lambda_{14}\, \textit{D\_NEG}_{i,t} \\
& + \lambda_{15}\, \textit{Ret}_{i,t} + \lambda_{16}\, \textit{Guide}_{i,t+1} + \textit{Year and industry fixed effects} + \varepsilon_{i,t} \qquad (4)
\end{aligned}
$$

We include four variables in the model to reflect the informativeness of analyst forecasts: 1) analyst forecast errors, 2) analyst forecast dispersion, 3) the number of analysts following the firm, and 4) the degree to which GDP explains firm performance. *Forecast error quartile*$_{i,t}$ is defined as the quartile rank of analyst forecast errors for year *t*. Analyst forecast errors for year *t* are calculated as the absolute value of actual EPS minus the average of the most recent analyst forecasts of year *t* earnings issued from the announcement of year *t*-1 earnings to the announcement of year *t* earnings, scaled by *Target EPS*$_{i,t\ t}$ (Mikhail et al. 1997). *Forecast dispersion quartile*$_{i,t+1}$ is the quartile rank of analyst forecast dispersion, where forecast dispersion is measured as the standard deviation of analyst forecasts of year *t*+1 earnings, scaled by *Target EPS*$_{i,t}$ (Cheong and Thomas 2011), where we use the forecasts of year *t*+1 earnings issued over the period from the announcement of year *t* earnings to the approval date of the annual bonus plan for year *t*+1. *Analyst following*$_{i,t+1}$ is the natural logarithm of the number of analysts following the firm.

To capture the information advantage of analysts over managers, we follow Hutton et al. (2012) and construct *Cyclicality*$_{i,t}$ to capture analysts' expertise in assessing the effects of macroeconomic factors

[14] Managers' relative information advantage over analysts comes from their ability to make decisions to respond to unusual operation situations, because analysts find it difficult to anticipate these actions by managers. Hutton et al. (2012) find no difference between analysts and managers in terms of their ability to understand industry-level shocks.

on firm performance. $Cyclicality_{i,t}$ is defined as the R^2 from the following firm-specific regression over at least 4 of the prior 5 years:

$$IB_{i,t} = \beta_0 + \beta_1 GDP_t + \varepsilon_{i,t} \tag{5}$$

where $IB_{i,t}$ is income before extraordinary items and GDP_t is nominal annual GDP. A higher value of *Cyclicality* indicates that a firm's earnings are highly correlated with the overall economy.[15]

Table 6 shows our results from estimating Equation (4).[16] The coefficients on $Analyst\ forecast\ dev_{i,t+1} \times Forecast\ error\ quartile_{i,t}$ (-0.071; *t*-value = -1.66) and $Analyst\ forecast\ dev_{i,t+1} \times Forecast\ dispersion\ quartile_{i,t+1}$ (-0.215; *t*-value = -6.71) are significantly negative. This suggests that firms are less likely to rely on analyst forecasts in revising targets when forecast errors or dispersion is high. The coefficient on $Analyst\ forecast\ dev_{i,t+1} \times Analyst\ following_{i,t+1}$ is positive and significant (0.101; *t*-value = 1.71), indicating that the use of analyst forecast information increases when more analysts provide earnings forecasts for the firm.

Regarding the effect of analysts' informational advantage on firms' use of analyst forecasts in target setting, we find that the coefficient on $Analyst\ forecast\ dev_{i,t+1} \times Cyclicality_{i,t}$ is significantly positive (0.221; *t*-value = 1.75). The results indicate that when a firm's earnings are highly correlated with GDP, firms exploit analysts' information advantage by relying more on analyst forecasts in setting their annual bonus targets.

Overall, the results in Table 6 support our cross-sectional predictions that the use of analyst forecasts in target setting is more pronounced when analyst forecasts are more informative, consistent with the economic theory on target setting (Murphy 2000).

[*Insert Table 6 here*]

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[15] The value of *Cyclicality* is high when the relation between firm performance and GDP is significantly positive (i.e., cyclical) or significantly negative (i.e., countercyclical).

[16] The sample size is reduced from 1,179 in Table 4 to 1,158 in Table 6, as we require non-missing values for additional variables, such as forecast dispersion and cyclicality.

5.3. Effects of the Extent to Which External Performance Expectations are Affected by Managers on Their Use in Target Setting (H3)

While external performance expectations are assumed to be independent of managers' influence, managers may affect external performance expectations to some extent. If there is variation in the extent to which external performance expectations are affected by managers, we predict that compensation committees incorporate the variation into their weighting decision in target setting. Our use of analyst forecasts as an empirical proxy for external performance expectations enables us to test this prediction. Whereas analysts are outsiders who provide independent estimates of future earnings based on their own research, managers can affect analyst forecasts either by privately communicating with analysts or by publicly providing earnings guidance. In addition, analysts have their own incentives to issue biased (e.g., optimistic) forecasts because of their investment banking relationships with the firms they follow (Dugar and Nathan 1995) or to obtain favorable access to private information from managers (Chen and Matsumoto 2006). If managers have the incentive and ability to influence analyst forecasts downward to obtain easier targets in their bonus contracts, such as by issuing pessimistic management forecasts to lower the consensus, the benefit of relying on analyst forecasts as an external source of information would be reduced.

However, we believe that these issues do not significantly impair the *overall* independence of analysts for the following reasons. First, several regulations have been introduced since 2000 to enhance the independence of analysts' research. Specifically, Regulation Fair Disclosure enacted in 2000 prohibits selective disclosures of material information to certain groups (e.g., individual analysts), and NASD Rule 2711 and NYSE Rule 472 introduced in 2002 prohibit linking analyst compensation to investment banking business. Furthermore, the 2003 Global Analysts Research Settlement requires the separation of the research department from the investment banking department. Empirical findings suggest that analysts' independence is indeed enhanced as a result of these regulatory changes (Barniv et al. 2009, Chen and Chen 2009), mitigating the concern about analysts' independence in the recent period. Second, from the stylized fact that analyst forecasts are initially optimistic and are gradually revised downward so that firms can beat the targets when earnings are announced, we infer that managers have incentives to guide analysts' annual earnings forecasts downward toward beatable forecasts *later* in the year as they learn the actual outcome with certainty, but not *early* in the year (i.e., in the beginning of the year when compensation is determined).[17]

Although we expect analyst forecasts to be generally less likely to be affected by managers' opportunistic behaviors for obtaining easy targets, there may be cross-sectional variation in the degree to which managers affect analyst forecasts. Thus, we examine whether the use of analyst forecasts varies depending on the perceived independence of analyst forecasts. We use affiliated versus unaffiliated analysts as a proxy for the extent to which analyst forecasts are influenced by managers. Affiliated analysts are those whose brokerage firms acted as the lead underwriter or co-underwriter of the covered firm's initial public offering (IPO) or seasoned equity offering (SEO) and they are often regarded as less independent than unaffiliated analysts (Malmendier and Shanthikumar 2014). Therefore, to the extent that compensation committees view the forecasts of unaffiliated analysts as more independent, firms are predicted to put more weight in target setting on the forecasts of unaffiliated analysts than on the forecasts of affiliated analysts. To test this prediction, we identify firms for which the EPS forecasts of both affiliated and unaffiliated analysts are available and measure two different *Analyst forecast dev*$_{i,\ t+1}$ using the forecasts of affiliated and unaffiliated analysts, respectively.[18]

Column (1) of Table 7 reports the estimation results when *Analyst forecast dev*$_{i,\ t+1}$ is measured based on the forecasts of affiliated analysts, whereas Column (2) reports the results based on the forecasts of unaffiliated analysts. While the coefficients on *Analyst forecast dev*$_{i,\ t+1}$ are positive and significant in both columns, the magnitude of the coefficient (0.580) in Column (1) (i.e., affiliated analysts) is smaller than that (0.620) in Column (2) (i.e., unaffiliated analysts). This difference is statistically significant (*p*-

[17] As a result, analysts' annual forecasts with long forecast horizons are usually optimistic, whereas quarterly forecasts with short forecast horizons tend to be pessimistic. Consistent with the argument that managers' decision to issue management forecasts depends on the forecast horizon, Bergman and Roychowdhury (2008) find that managers tend to walk-down analysts' optimistic short-horizon (i.e., quarterly) forecasts, but they are disinclined to walk-down optimistic long-horizon (i.e., annual) forecasts.

[18] Due to our requirement for the presence of both affiliated and unaffiliated analysts for a firm, the sample size is reduced to 119 for the analysis in Table 7.

value = 0.029). This finding suggests that the weight placed on the forecasts of unaffiliated analysts is greater than those of affiliated analysts, consistent with our third hypothesis (H3) that the use of external performance expectations is more pronounced when they are less likely to be affected by managers.

[*Insert Table 7 here*]

As an alternative way to test H3, we divide analyst forecasts into those issued before management forecasts and those issued after management forecasts, for a subsample with available management forecasts issued over the period from the announcement of year *t* earnings to the approval date of the annual bonus plan for year *t*+1. To the extent that management forecasts can influence analyst forecasts, we view that analyst forecasts issued before management forecasts provide more independent and objective performance expectations than those issued after management forecasts. Thus, we measure two different *Analyst forecast* $dev_{i,\ t+1}$ using analyst forecasts issued before management forecasts and those issued after management forecasts, respectively, and estimate Equation (1) separately. In untabulated analyses, we find that the coefficient on *Analyst forecast* $dev_{i,\ t+1}$ based on analyst forecasts issued before management forecasts (0.724) is significantly greater than the coefficient on *Analyst forecast* $dev_{i,\ t+1}$ based on analyst forecasts issued after management forecasts (0.439) (p-value for the difference = 0.00).[19] This finding corroborates the previous result based on the affiliated versus unaffiliated analyst classification.[20]

6. Additional Analyses

6.1. Relative Importance of Analyst Forecasts Versus Management Forecasts in Target Setting

[19] As we require (i) a firm to have management forecasts issued from the announcement of year *t* earnings to the approval date of the annual bonus plan for year *t*+1 (i.e., measurement period) and (ii) both analyst forecasts issued before management forecasts and those issued after management forecasts to be available over this measurement period, the sample size is reduced to 78 for this analysis. Alternatively, when we extend the measurement period to the period from the fiscal year-end for year *t* to the approval date of the annual bonus plan for year *t*+1, we find similar results for the sample of 513 observations (untabulated).

[20] In these analyses, we compare the weight on affiliated analysts' forecasts to that on unaffiliated analysts' forecasts (or the weight on analyst forecasts before management forecasts to that after management forecasts) using two separate regressions. A possible alternative approach is to estimate one model that includes both affiliated and unaffiliated analysts' forecasts (or that includes both analyst forecasts before and after management forecasts). The results from this alternative approach are qualitatively similar to those from the main approach. However, we do not tabulate them, because the variance inflation factors for *Analyst forecast* $dev_{i,t+1}$ are greater than 10, indicating a multicollinearity problem with the alternative approach.

One can argue that internal future planning information (e.g., internal budgeting) is more important than analyst forecasts in target setting because the former reflects managers' private information. However, internal planning information is subject to managers' opportunistic behaviors. For example, previous studies find that using budgets in performance evaluation can cause managers to understate their expected productivity (Young 1985, Chow et al. 1988). Anderson et al. (2010) find that managers tend to set easy targets after the introduction of a performance-based bonus plan. Furthermore, Cassar and Gibson (2008) report that budget preparation does not improve managers' forecast accuracy because budgets are used not only for forecasting performance but also for other purposes, such as communicating objectives and motivating employees. Considering these issues, independent compensation committees should be unwilling to use the information provided by managers themselves in setting targets. Overall, whether internal future planning information is more important than analyst forecasts in target setting remains an empirical question.

To examine the relative importance of analyst forecasts versus internal planning information in target setting, we use managers' earnings forecasts as a proxy for internal planning information and include them in our regressions.[21] Specifically, for the subsample with available management forecasts, we control for the information in management earnings forecasts and estimate the following regression:

$$\begin{aligned}
\textit{Target revision}_{i,t+1} = {} & \lambda_0 + \lambda_1\, \textit{Target deviation}_{i,t} + \lambda_2\, \textit{Target deviation}_{i,t} \times \textit{D_NEG}_{i,t} \\
& + \lambda_3\, \textit{Analyst forecast dev}_{i,t+1} + \lambda_4\, \textit{Management forecast dev}_{i,t+1} \\
& + \lambda_5\, \textit{Relative-to-peers}_{i,t} + \lambda_6\, \textit{Growth}_{i,t+1} + \lambda_7\, \textit{D_NEG}_{i,t} + \lambda_8\, \textit{Ret}_{i,t} \\
& + \textit{Year and industry fixed effects} + \varepsilon_{i,t} \qquad (6)
\end{aligned}$$

Management forecast dev$_{i,t+1}$ is the difference between *Management forecast*$_{i,t+1}$ and *Actual EPS*$_{i,t}$, divided by *Target EPS*$_{i,t}$, where *Management forecast*$_{i,t+1}$ is the most recent management forecasts of year t+1 earnings issued over the period from the announcement of year t earnings to the approval date of the annual bonus plan for year t+1. If firms rely on information in analyst earnings forecasts to set targets above

[21] Previous studies have established a theoretical link between external management earnings forecasts and internal management planning information (Hemmer and Labro 2008). Such studies have used management earnings forecasts as a proxy for internal planning and budget information (Goodman et al. 2014).

and beyond the information in management earnings forecasts, the coefficient on *Analyst forecast* $dev_{i,t+1}$ (λ_3) would be positive and significant even after controlling for *Management forecasts* $dev_{i,t+1}$. As presented in Table 2, the mean (median) values of (*Management* $forecast_{i,t+1}$ – *Actual* $EPS_{i,t}$) and *Management forecast* $dev_{i,t+1}$ are \$0.052 (\$0.110) and 2.6% (5.0%), respectively. Table 3 shows a positive correlation between (*Target* $EPS_{i,t+1}$ – *Target* $EPS_{i,t}$) and (*Management forecast* ${}_{i,t+1}$ – *Actual* $EPS_{i,t}$).

Panel A of Table 8 presents the results of estimating Equation (6) for a reduced sample with available management forecast data. As a starting point, Column (1) shows the results of estimating the model without *Analyst forecast* $dev_{i,t+1}$. The coefficient on *Management forecast* $dev_{i,t+1}$ is significantly positive, suggesting that management forecasts are an important source of information in setting annual bonus targets. However, in Column (2), where both analyst and management forecast variables are included in the model, the coefficient on *Analyst forecast* $dev_{i,t+1}$ is significantly positive, whereas the coefficient on *Management forecast* $dev_{i,t+1}$ loses its significance.[22] Overall, these results suggest that firms rely more on the information in analyst earnings forecasts than on the information in management earnings forecasts.

A potential issue related to estimating Equation (6) is that analyst forecasts can be affected by management forecasts (Hassell et al. 1988), as we discuss in Section 5.3.[23] In this scenario, the information in management forecasts may be an underlying factor in the association between analyst forecasts and target revisions. To check this possibility, we use several approaches as follows.

First, we estimate Equation (1) using a subsample of firms that do not issue management earnings forecasts, in which analyst forecasts are free from the influence of management forecasts. In Column (3), the coefficient on *Analyst forecast* $dev_{i,t+1}$ remains positive and significant for the subsample without

[22] The variance inflation factors for *Analyst forecast* $dev_{i,t+1}$ and *Management forecast* $dev_{i,t+1}$ are relatively low at 2.42 and 2.50, respectively. Therefore, any multicollinearity in the model is not severe.
[23] For example, Hassell et al. (1988) document that the sign and magnitude of the news from management forecasts are positively associated with those of analyst forecast revisions.

management forecasts, suggesting that analyst forecast information itself is important in target setting, even when it is not influenced by management forecasts.[24]

Second, we measure analyst forecasts over the period *before* management forecasts are issued. When we include *Analyst forecast*$_{i,t+1}$, alternatively defined as the most recent forecasts of year t+1 earnings issued over the period from the announcement of year t earnings to the date of the management forecast issuance, the results do not change, as reported in Column (4).

Third, we also measure analyst forecasts over the period *after* management forecasts are issued to check whether potentially "walked-down" analyst forecasts provide any incremental information in target setting above management forecasts. If analyst earnings forecasts, which may be affected by management forecasts, provide no further incremental information, the coefficient on *Analyst forecast dev*$_{i,t+1}$ should be insignificant when the model controls for management forecasts. However, the results using this measure of analyst forecasts reported in Column (5) are similar to those in Column (4), suggesting that the association between analyst forecasts and target revisions that we document is independent of the influence of management forecasts.

Overall, the findings in Panel A of Table 8 lend support to the notion that analyst forecasts provide incremental information above and beyond management forecasts in explaining target revisions.

[*Insert Table 8 here*]

6.2. Potential Reverse Causality: Analyst Forecast Revisions Around the Approval Date of the Annual Bonus Plan

One may argue that the positive association between analyst forecasts and firms' bonus target revisions is derived from analysts' practice of updating their forecasts based on firms' internal targets (i.e., reverse causality). We believe that this concern is largely mitigated in our analyses because we use analyst forecasts issued *before* the approval date of the plan and bonus plan details are not typically publicly

[24] Grabner et al. (2016) find that the quality of the target-setting process is positively associated with the probability of issuing management guidance. However, they also suggest that a high-quality target-setting process does not guarantee the accuracy of management forecasts because managers use their informational advantage strategically to increase the likelihood of meeting or beating market expectations. If compensation committees recognize this phenomenon, they would prefer analyst forecasts to management forecasts when setting bonus targets.

available until proxy statements are disclosed. Nevertheless, we explore this possibility by examining individual analyst forecast revisions around the bonus plan approval dates. Specifically, we examine whether individual analysts revise their forecasts upward (downward) after the approval date if bonus targets are set higher (lower) than their own previous forecasts.

Low bonus target$_{i,j,t+1}$ (*High bonus target*$_{i,j,t+1}$) equals 1 if the bonus target for year t+1 is lower (higher) than *Analyst forecast*$_{i,j,t+1}$ and 0 otherwise, where *Analyst forecast*$_{i,j,t+1}$ represents the most recent forecasts of an individual analyst for year t+1 earnings issued before the bonus plan approval date. *Walked-down analyst forecast*$_{i,j,t+1}$ (*Walked-up analyst forecast*$_{i,j,t+1}$) equals 1 if the analyst's earliest forecast for year t+1 earnings issued within 7 days of the bonus plan approval date for year t+1 is lower (higher) than her own previous forecast issued before the approval date (i.e., *Analyst forecast*$_{i,j,t+1}$) and 0 otherwise. If individual analysts revise their forecasts around the bonus plan approval date (as the reverse causality argument implies), we should observe a positive correlation between *Low bonus target*$_{i,j,t+1}$ and *Walked-down analyst forecast*$_{i,j,t+1}$ (*High bonus target*$_{i,j,t+1}$ and *Walked-up analyst forecast*$_{i,j,t+1}$). The results in Panel B of Table 8 based on the sample of 665 observations with available data at the firm-year-analyst level show that the correlations are negative and insignificant. This suggests that reverse causality is unlikely to be a plausible alternative explanation for our main findings.[25]

6.3. Effect of Incentives to Meet or Beat Earnings Benchmarks on the Use of Analyst Forecasts in Target Setting

Our cross-sectional tests relating to H2 and H3 focus on the characteristics of analyst forecasts as an external source of information in target setting. However, firms' use of analyst forecasts in target setting may be driven by the incentive of managers to meet or beat market expectations because missing the consensus adversely affects firm value and executives' compensation, particularly equity-related incentives.

[25] Our findings on the relative information advantage of analysts provide additional (indirect) evidence that rejects the reverse causality argument. When firm performance is strongly correlated with macroeconomic factors (i.e., when cyclicality is high), analysts have a relative informational advantage over managers and are therefore less likely to revise their forecasts based on information about internal targets. Thus, the reverse causality argument predicts the association between analyst forecasts and target revisions to be weaker when cyclicality is high. However, the results in Table 6 show that the association is in fact stronger when cyclicality is high, negating the reverse causality explanation.

In this scenario, the use of analyst forecasts is expected to increase when managers have stronger incentives to meet or beat the consensus (e.g., Armstrong et al. 2017). To examine this possibility, we examine whether the use of analyst forecasts varies according to CEOs' equity incentives (e.g., the CEO's equity portfolio delta and the ratio of equity-related compensation to annual bonus). In untabulated results, we find that the interactions between these proxies for equity incentives and *Analyst forecast dev*$_{i,t+1}$ are all insignificant. The findings support the notion that firms' use of analyst forecasts is driven by their need for more informative and independent information sources to improve the contract efficiency, rather than by their need to provide incentives to meet or beat the consensus EPS forecasts.

6.4. Other Robustness Tests

6.4.1. Alternative proxies for *Growth* and *Relative-to-peer*

To ensure that our findings are robust to alternative measures of expected growth, we replace growth in EPS (i.e., the dependent variable) in Equation (2) with growth in sales, income before extraordinary items, and operating income before depreciation. As Panel A of Table 9 presents, the results of estimating Equation (1) using these alternative *Growth* proxies are qualitatively similar to those previously reported.

[*Insert Table 9 here*]

We also test the robustness of our findings by using an alternative method of identifying peers to measure *Relative-to-peer*. Specifically, we follow Jayaraman et al. (2015) and Hoberg and Phillips (2016) and define peer groups and *Relative-to-peers* based on the textual similarity of a firm's product description section in its 10-K filing with that of other firms. The results using this alternative method to measure peer performance presented in Panel B of Table 9 are similar to those previously reported.

6.4.2. Sample selection issue

Our sample selection process requires firms to have EPS-based annual bonus plans, a requirement that may introduce a sample selection issue. To mitigate this concern, we use the Heckman (1979) two-stage least squares method. Specifically, in the first stage, we estimate a probit model in which a firm's decision to use EPS in bonus plans is regressed on a set of determinant variables identified in the literature (Huang et al. 2014, Cheng et al. 2015), including firm size, book-to-market ratio, a set of corporate governance measures (i.e., institutional holdings, transient institutional holdings, percentage of female directors, and percentage of outside directors), R&D expenditures, effective tax rates, other forms of executive compensation (i.e., restricted stock awards and option awards), the number of business segments, and the number of analysts following. In addition to these variables, we further include in the first-stage regression the lagged value of the industry average percentage of firms that use EPS in bonus plans to satisfy an "exclusion restriction."[26] The second-stage regression results with the inverse Mills' ratio as an additional control variable (untabulated) are similar to our main findings, suggesting that the sample selection issue does not affect the main inferences.

6.4.3. Differences in contract- and executive-specific features

We acknowledge that a significant level of cross-sectional differences may exist in our sample in terms of the type of compensation plans (e.g., bonus and equity-based compensation), performance measures (e.g., EPS and non-EPS earnings), and executive-specific features (e.g., CEO tenure). Specifically, in untabulated analyses, we find that approximately 14.5% of our sample firms also use EPS as a performance measure for their executives' equity-based compensation plans (e.g., stock and option grants). 30.4% of our sample firms also use other non-EPS earnings in their bonus plans, such as net income, operating income, and earnings before interest and taxes. For firms that disclose the weight on EPS in bonus plans (N = 831), we also find that the average weight on EPS in the bonus plan is 54.3%. When other non-EPS earnings are also used in bonus plans, the average weights on EPS and non-EPS earnings are 35.6% and 33.2%, respectively.

[26] While the percentage of firms that use EPS in bonus plans in an industry may be correlated with individual firms' decision to use EPS as a performance measure, there is no convincing argument for why it would directly affect individual firms' target setting practices, thereby satisfying the condition for an exclusion restriction (Lennox et al. 2012).

To examine whether such differences in contract- and executive-specific features affect our main findings, we take the following approaches. First, we identify firms that use EPS for both equity-based compensation and bonus plans. We define an indicator variable that equals 1 if EPS is used for both annual bonus plans and equity-based compensation, and 0 if EPS is used only for annual bonus plans (*Equity_EPS*). We then include *Equity_EPS* and its interaction with *Analyst forecast* $dev_{i,t+1}$ in the model and find that the interaction is insignificant. Importantly, the coefficient on *Analyst forecast* $dev_{i,t+1}$ remains positive and significant. In addition, including *Equity_EPS* in the model (without its interaction) does not change the results.

Second, we identify firms that use both non-EPS earnings and EPS for annual bonus plans. We define an indicator variable that equals 1 if the annual bonus contract is linked to other non-EPS earnings and to EPS, and 0 otherwise (*Bonus_Earnings*). As in the previous approach, we include *Bonus_Earnings* and its interaction with *Analyst forecast* $dev_{i,t+1}$ in the model or include *Bonus_Earnings* as an additional control variable without the interaction.[27] We find that the coefficient on *Analyst forecast* $dev_{i,t+1}$ remains significantly positive while the interaction term is not significant.[28] Third, we measure the weight on EPS in bonus plans and include changes in the weight on EPS between year *t* and *t*+1 as an additional control variable along with its interaction with *Analyst forecast dev*. We find that the interaction is not significant and the main finding for *Analyst forecast* $dev_{i,t+1}$ does not change.

Finally, to examine whether CEO tenure affects our main findings, we divide our sample into subsamples based on the median CEO tenure and estimate Equation (1) separately for these subsamples. We find that the coefficients on *Analyst forecast* $dev_{i,t+1}$ are significantly positive for both subsamples and the coefficients are not significantly different between the two groups, indicating that firms' use of external

[27] Alternatively, we estimate the model separately for two subsamples partitioned by *Equity_EPS* or *Bonus_Earnings*. We find that the coefficients on *Analyst forecast* $dev_{i,t+1}$ are significantly positive for both groups and that they are not different between the groups.

[28] We acknowledge that we do not examine the use of external performance expectations for firms that do not use EPS in their annual bonus plans (e.g., firms that use non-EPS earnings but not EPS) because we limit our hand collection of data to firms that use EPS in their bonus plans.

performance expectations in setting future targets does not differ between CEOs with long tenure and those with short tenure.[29] In summary, the various analyses discussed above suggest that our main inferences are not affected by various contract-specific or executive-specific differences in executive compensation.

6.4.4. Time-series analysis

To provide additional evidence to corroborate our main findings from the pooled data, we estimate a firm-specific time-series regression, because Leone and Rock (2002, p. 66) suggest that "the setting of budgets is an inherently inter-temporal process" and thus ratcheting can be studied from the time-series model. Specifically, we restrict our sample to observations with no missing values of *Target revision*$_{i,t+1}$, *Target deviation*$_{i,t}$, and *Analyst forecast dev*$_{i,t+1}$ for at least 7 years during the sample period. This restriction reduces our sample size from 1,179 to 332 (45 unique firms). We estimate Equation (7) for each of these 45 firms:[30]

$$\begin{aligned} \textit{Target revision}_{i,t+1} = \lambda_0 + \lambda_1\, \textit{Target deviation}_{i,t} + \lambda_2\, \textit{Target deviation}_{i,t} \times \textit{D\_NEG}_{i,t} \\ + \lambda_3\, \textit{Analyst forecast dev}_{i,t+1} + \varepsilon_{i,t} \end{aligned} \tag{7}$$

In the untabulated results, we find that the mean of the coefficients on *Analyst forecast dev*$_{i,t+1}$ is positive and significant (0.697, Z-stat: 30.66), lending support to the results in Table 4.

6.4.5. Robustness to outliers

To check the robustness of our main findings to the effect of outliers, we perform several additional tests. First, we winsorize all of the variables at the 5% and 95% levels (instead of at the 1% and 99% levels, as used for our main results) and then re-estimate the models. Second, we estimate the models after truncating (instead of winsorizing) all of the variables at the 1% and 99% levels. Third, we restrict our sample to observations that have absolute studentized residual values of less than 2 (Belsley et al. 1980). Fourth, we delete any observations that have a Cook's D value (Cook 1977) greater than 4 divided by the

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[29] Alternatively, we partition the sample based on (i) whether the CEO is close to retirement; (ii) whether CEO tenure is less than (equal to or greater than) 3 years; and (iii) whether CEO tenure is less than (equal to or greater than) 5 years. We obtain similar results. When we include a variable for CEO tenure (logged value) and its interaction with *Analyst forecast dev*$_{i,\,t+1}$, the interaction is insignificant.

[30] Given that the number of observations for each regression is very small (e.g., N = 7), we do not include control variables in Equation (7) to conserve the degree of freedom.

number of observations. Finally, we re-estimate the models using the least absolute deviations estimation, a form of robust regression also known as median regression, which is less susceptible to the outlier problem (Indjejikian et al. 2014a). We also check whether our results hold across firms of different size by estimating the models separately for the subsamples of S&P 500 LargeCap, S&P 400 MidCap, and S&P 600 SmallCap. The untabulated results show that the coefficients on *Analyst forecast* $dev_{i,t+1}$ are significantly positive in all cases.

7. Conclusion

We examine whether firms use externally informed performance expectations to set and revise performance targets for executive annual bonus contracts. Although economic theories suggest that firms use a variety of information sources in target setting, existing studies have largely focused on the use of past performance. A growing body of research has begun to examine various external sources of information (e.g., the relative performance of peers) and their roles in target setting. However, the relevance of external performance expectations, such as analyst earnings forecasts, in target setting has received little attention, despite support from theory and practice. The results reported in this study extend our understanding of the role of external information in firms' target-setting process.

Our main results can be summarized as follows. First, we find that external performance expectations, as proxied by the consensus of analysts' EPS forecasts, are positively associated with the revision of future targets in bonus contracts. This result is robust to controlling for the effect of past performance, peer performance, expected growth, and the presence of management forecasts. This finding highlights the importance of external performance expectations as a source of information above and beyond past and peer performance. Based on the test suggested by Bouwens and Kroos (2017), we find evidence that incorporating analyst forecasts into target setting improves target accuracy.

Second, we find that a firm's reliance on external performance expectations in target revisions depends on their informativeness about future firm performance and that firms place more weight on external performance expectations when they are less likely to be affected by managers. The findings from the cross-sectional tests lend further support to the notion that the association between analyst forecasts and target revisions varies in a manner consistent with the economic theory on target setting (Murphy 2000). Our results are robust to several sensitivity tests.

We contribute to the growing literature that examines the use of information sources other than past performance in target setting (e.g., Bouwens and Kroos 2017). While we provide initial evidence that firms use externally informed performance expectations in setting internal targets for executive annual bonus contracts, future research may further investigate the costs and benefits of using externally informed performance targets, their motivational effect on managerial behaviors, and the complex interrelations between external and internal targets.

2014 Performance-Based Plans

Our executive compensation programs place a heavy emphasis on performance-based rewards. We maintain a short-term incentive plan, known as our annual bonus plan, as well as a long-term incentive plan. Awards to our NEOs under our annual bonus plan are made under our 2008 Performance-Based Management Incentive Plan, and awards under our long-term incentive plan are granted under our 2008 Omnibus Equity Plan. Awards under our annual bonus plan are directly tied to the achievement of our annual operating goals, which are aligned with the Company's short- and long-term strategic plans. Our long-term incentives are directly tied to the performance of the price of shares of our common stock, which align our executives' long-term interests with the interests of our stockholders.

In setting our annual goals, in addition to our internal forecasts, we consider analysts' projections for our performance and the performance of companies in our peer group, as well as broad economic and industry trends. We establish challenging targets that result in payouts at target levels only when Company performance warrants it. The Compensation Committee is responsible for reviewing and approving our annual Company goals, targets and levels of payout (e.g., threshold, target, and maximum) and for reviewing and determining actual performance results at the end of the performance period.

Appendix B 2014 Proxy Statement of El Paso Electric Company

2014 Annual Cash Bonus Plan

Metric	Weighting (%)	Performance Goals: Threshold	Performance Goals: Target	Performance Goals: Maximum	Performance Result: Actual Result	Performance Result: Final Payout (as % of Target Bonus): CEO	Performance Result: Final Payout (as % of Target Bonus): Other NEOs (averaged)
EPS	50	$2.20	$2.30	$2.45	$2.27	39.7	38.2
Customer Satisfaction							
3rd Party Customer Survey	10	75	78	81	81	20	17.9
Call Center Performance (%)	10	70%	80%	90%	86%	16	14.7
Reliability (SAIDI) (min)	15	45.6 min	41.9 min	39.9 min	46.6 min	0	0
Safety							
DART (measure of injuries)	3.75	1.5	1.2	0.9	1.73	0	0
Vehicle accident	3.75	3.6	2.6	1.6	1.55	7.5	6.7
Leading Indicator activities	2.5	4 points	6 points	8 points	Maximum	5	3.7
Compliance	5	N/A	Fully Compliant	N/A	Target	5	5
Total	100					93.2	86.2

Bonuses are paid in late February or early March after the Compensation Committee reviews the audited financial results and operational performance for the previous year. As reported in the Annual Report on Form 10-K for the year ended December 31, 2014, and as shown in the above table, the Company had net income of $2.27 per basic share, which includes an accrual for the cost of the bonus pool. The Company also met (or failed to meet) its customer satisfaction goals, its reliability goal, its three safety goals and its compliance goal, in each instance at the level reflected in the above table. As a result, each NEO received a bonus, as set forth in the table below and also in the Summary Compensation Table later in this proxy statement. The total bonus paid to Company employees for 2014 was approximately $7.4 million, of which approximately $1.9 million was paid to the NEOs and other executive officers.

Appendix C Variable Definitions

Variables		Definitions
Target EPS$_{i,t}$	=	Firm *i*'s target EPS used in the firm's executive bonus plan for fiscal year *t*.
Actual EPS$_{i,t}$	=	Firm *i*'s actual EPS for fiscal year *t*.
Target revision$_{i,t+1}$	=	(*Target EPS*$_{i,t+1}$ − *Target EPS*$_{i,t}$) divided by *Target EPS*$_{i,t}$.
Target deviation$_{i,t}$	=	(*Actual EPS*$_{i,t}$ − *Target EPS*$_{i,t}$) divided by *Target EPS*$_{i,t}$.
Analyst forecast dev$_{i,t+1}$	=	(*Analyst forecast*$_{i,t+1}$ − *Actual EPS*$_{i,t}$) divided by *Target EPS*$_{i,t}$. *Analyst forecast*$_{i,t+1}$ is the average analyst forecast for year *t*+1 earnings issued from the announcement of year *t* earnings to the approval date of the annual bonus plan for year *t*+1. If the approval date is missing, we use the end date of the 3 months after the fiscal year-end as the approval date. If an analyst issues multiple forecasts during this period, we only use the most recent forecast.
Management forecast dev$_{i,t+1}$	=	(*Management forecast*$_{i,t+1}$ − *Actual EPS*$_{i,t}$) divided by *Target EPS*$_{i,t}$. *Management forecast*$_{i,t+1}$ is the most recent management forecast for year *t*+1 earnings issued from the announcement of year *t* earnings to the approval date of the annual bonus plan for year *t*+1.
D_NEG$_{i,t}$	=	Equals 1 if *Target deviation*$_{i,t}$ is negative, and 0 otherwise.
Forecast error quartile$_{i,t}$	=	The quartile rank of analyst forecast errors in the previous year. Analyst forecast errors are calculated as the absolute value of actual EPS minus the average of the most recent analyst forecasts for year *t* earnings issued from the announcement of year *t*-1 earnings to the announcement of year *t* earnings, scaled by *Target EPS*$_{i,t}$.
Forecast dispersion quartile$_{i,t+1}$	=	The quartile rank of analyst forecast dispersion, where dispersion is measured as the standard deviation of analyst forecasts for year *t*+1 earnings, scaled by *Target EPS*$_{i,t}$.
Analyst following$_{i,t+1}$	=	The natural logarithm of the number of analysts who issue an EPS forecast for year *t*+1.
Cyclicality$_{i,t}$	=	The R^2 from the following firm-specific regression over at least 4 of the prior 5 years (Hutton et al. 2012): $IB_{i,t} = \beta_0 + \beta_1 GDP_t + \varepsilon_{i,t}$ (5) where *IB* is income before extraordinary items and *GDP* is the nominal annual gross domestic product.
Low (High) bonus target$_{i,j,t+1}$	=	Equals 1 if the annual bonus target for year *t*+1 is lower (higher) than *Analyst forecast*$_{i,j,t+1}$, and 0 otherwise. *Analyst forecast*$_{i,j,t+1}$ is the most recent individual analyst forecast for year *t*+1 earnings issued before the approval date of the annual bonus plan for year *t*+1.
Walked-down (up) analyst forecasts$_{i,j,t+1}$	=	Equals 1 if an analyst's earliest forecast for year *t*+1 earnings issued within 7 days of the approval date of the annual bonus plan for year *t*+1 is lower (higher) than *Analyst forecast*$_{i,j,t+1}$, and 0 otherwise. *Analyst forecast*$_{i,j,t+1}$ is the most recent individual analyst forecast for year *t*+1 earnings issued before the approval date.
Growth$_{i,t+1}$	=	The expected EPS growth is estimated from the following model:

		$$EPS\ growth_{i,,t+1} = \alpha_0 + \alpha_1\ Past\ EPS\ growth_{i,t} + \alpha_2\ Size_{i,t} + \alpha_3\ EP_{i,t} + \alpha_4\ Leverage_{i,t} + \alpha_5\ MKT_{i,t} + \alpha_6\ RD_{i,t} + \alpha_7\ CAP_{i,t} + \alpha_8\ BTM_{i,t} + \alpha_9\ Div\ yield_{i,t} + \alpha_{10}\ Past\ RET_{i,t} + \varepsilon_{i,t} \quad (2)$$ $Growth_{i,t+1}$ is the firm-level predicted value from the above cross-sectional model. Considering the factors that affect the growth of accounting earnings and sales (Chan et al. 2003, Ciftci and Cready 2011, Gong and Li 2013), we control for past growth in EPS over the previous 3 years (*Past EPS growth*), the natural logarithm of the market value of equity (*Size*), the earnings-to-price ratio (*EP*), leverage (*Leverage*), advertising expenses divided by sales (*MKT*), average R&D expenses divided by sales over the previous 3 years (*RD*), average capital expenditures divided by total assets over the previous 3 years (*CAP*), the book-to-market ratio (*BTM*), the dividend yield ratio (*Div yield*), and stock returns over the previous 12 months (*Past RET*). We compute leverage (*Leverage*) as liabilities minus cash holdings over total assets minus cash holdings. We calculate the book-to-market ratio (*BTM*) as total assets divided by the market value of equity plus long-term liabilities. The dividend yield ratio (*Div yield*) is ordinary cash dividends divided by net income before extraordinary items. We estimate Equation (2) separately for each fiscal year and two-digit SIC code group.
$Relative\text{-}to\text{-}peers_{i,t}$	=	Firm *i*'s EPS for fiscal year *t* minus peer firms' EPS for fiscal year *t*. Peer portfolios are constructed following Albuquerque (2009). To construct peer portfolios matched on industry and firm size, we first form annual portfolios based on two-digit SIC codes. We use all of the firms on Compustat to construct the portfolios. Second, we sort firms by beginning-of-year market value into size quartiles. Third, we match each firm with an industry-size peer group. Peer performance is the equal-weighted portfolio EPS for an industry-size peer group. When calculating portfolio EPS, we exclude the EPS of the observed firm.
$Ret_{i,t}$	=	The firm's stock returns over the 12-month period ending 3 months after fiscal year-end *t*.
$Guide_{i,t+1}$	=	Equals 1 if a firm issues management forecasts from the announcement of year *t* earnings to the approval date of the annual bonus plan for year *t*+1, and 0 otherwise.
$Market\ Value_{i,t}$	=	The firm's market value of equity in millions at the end of fiscal year *t*.
$AT_{i,t}$	=	The firm's total assets in millions at the end of fiscal year *t*.

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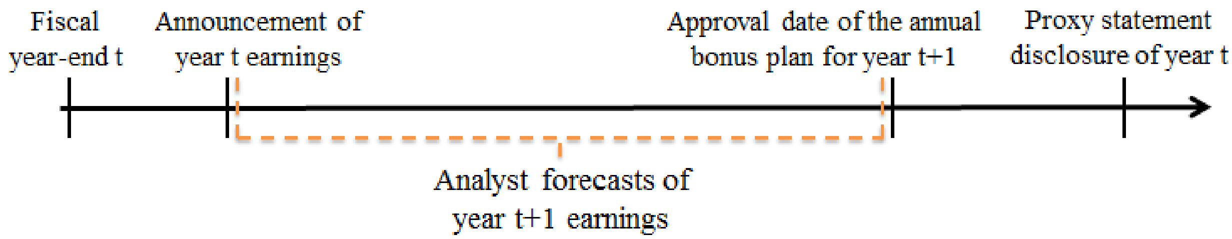
FIGURE 1
Timeline of Analyst Forecasts



This figure illustrates the timeline of analyst forecasts for year t+1 earnings issued from the announcement of year t earnings to the approval date of the annual bonus plan for year t+1. If the approval date is missing, we use the end date of the 3 months after the fiscal year-end as the approval date. If an analyst issues multiple forecasts during this period, we only use the most recent one. We average the analyst forecasts of year t+1 earnings to derive *Analyst* $forecast_{i,t+1}$. Analyst forecast data come from I/B/E/S.

TABLE 1
Sample Selection

S&P 1500 firm years from 2006 to 2014:	13,140
Less firm years that do not use EPS as one of the performance measures for their executive annual bonus contracts:	(9,837)
S&P 1500 firm years that use EPS as a performance measure for executive annual bonus contracts:	3,303
Less firm years that lack EPS target information for executive bonus contracts:	(1,232)
Sample firm years that have EPS target information:	2,071
Less firm years that lack analyst forecast information:	(597)
Sample firm years that have EPS target and analyst forecast information:	1,474
Less firm years that lack control variables (e.g., *Growth*, *Relative-to-peers*, and *Ret*):	(206)
Sample firm years that have EPS target and analyst forecast information, and control variables:	1,268
Less firm years that lack two consecutive years of target data:	(89)
Final sample	1,179

This table presents the sample selection procedure.

TABLE 2
Descriptive Statistics

Measure	N	Mean	Median	Q1	Q3	Std. Dev.
$Target\ EPS_{i,t+1}-Target\ EPS_{i,t}$	1,179	0.202	0.215	-0.045	0.490	0.728
$Target\ revision_{i,t+1}$	1,179	0.116	0.098	-0.022	0.228	0.355
$Actual\ EPS_{i,t}-Target\ EPS_{i,t}$	1,179	0.039	0.060	-0.080	0.230	0.588
$Target\ deviation_{i,t}$	1,179	0.017	0.027	-0.040	0.111	0.318
$Analyst\ forecast_{i,t+1}-Actual\ EPS_{i,t}$	1,179	0.164	0.162	-0.069	0.381	0.640
$Analyst\ forecast\ dev_{i,t+1}$	1,179	0.110	0.075	-0.034	0.181	0.380
$Management\ forecast_{i,t+1}-Actual\ EPS_{i,t}$	695	0.052	0.110	-0.150	0.270	0.638
$Management\ forecast\ dev_{i,t+1}$	695	0.026	0.050	-0.068	0.114	0.300
$Market\ value_{i,t}$	1,179	12,530	3,927	1,334	11,832	24,789
$AT_{i,t}$	1,179	16,629	4,306	1,383	12,371	39,695
$Relative\text{-}to\text{-}peers_{i,t}$	1,179	0.774	0.602	-0.324	1.629	1.824
$Ret_{\ i,t}$	1,179	0.092	0.127	-0.352	0.584	1.014
$Growth_{i,t+1}$	1,179	0.150	0.134	-0.085	0.333	0.371
$D\_NEG_{i,t}$	1,179	0.348	0.000	0.000	1.000	0.476
$Guide_{i,t+1}$	1,179	0.589	1.000	0.000	1.000	0.492

This table reports the descriptive statistics for the key variables. The sample includes 1,179 firm-year observations during the 2006 to 2014 period. Appendix C provides the variable definitions.

TABLE 3
Correlations

	$Target\ EPS_{i,t+1} - Target\ EPS_{i,t}$	(a)	(b)	(c)
(a) $Actual\ EPS_{i,t} - Target\ EPS_{i,t}$	**0.647 *****			
(b) $Analyst\ forecast_{i,t+1} - Actual\ EPS_{i,t}$	**0.384 *****	**-0.186 *****		
(c) $Management\ forecast_{i,t+1} - Actual\ EPS_{i,t}$	**0.308 *****	**-0.223 *****	**0.779 *****	
(d) $Growth_{i,t+1}$	**0.070 *****	0.003	**0.128 *****	0.061

This table shows the correlations between the key variables. The sample includes 1,179 firm-year observations during the 2006 to 2014 period. Appendix C provides the variable definitions. *** denotes statistical significance at the 1% level.

TABLE 4
Test of the Association between Analyst Forecasts and Target Revisions (H1)

$$Target\ revision_{i,t+1} = \lambda_0 + \lambda_1\ Target\ deviation_{i,t} + \lambda_2\ Target\ deviation_{i,t} \times D\_NEG_{i,t} + \lambda_3\ Analyst\ forecast\ dev_{i,t+1} + \lambda_4\ Relative\text{-}to\text{-}peers_{i,t} + \lambda_5\ Growth_{i,t+1} + \lambda_6\ D\_NEG_{i,t} + \lambda_7\ Ret_{i,t} + \lambda_8\ Guide_{i,t+1} + Year\ and\ industry\ fixed\ effects + \varepsilon_{i,t} \quad (1)$$

	Dependent variable:	$Target\ revision_{i,t+1}$			
Independent variables:	Pred.	(1)		(2)	
Intercept		-0.023		0.013	
		(-0.93)		(0.68)	
$Target\ deviation_{i,t}$	+	**1.073**	***	**0.982**	***
		(15.53)		**(15.69)**	
$Target\ deviation_{i,t} \times D\_NEG_{i,t}$	−	**-0.731**	***	**-0.355**	***
		(-6.26)		**(-2.82)**	
$Analyst\ forecast\ dev_{i,t+1}$	+			**0.376**	***
				(5.45)	
$Relative\text{-}to\text{-}peers_{i,t}$	−	**-0.019**	***	-0.006	
		(-3.84)		(-1.59)	
$Growth_{i,t+1}$	+	0.009		0.001	
		(1.08)		(0.00)	
$D\_NEG_{i,t}$	?	**-0.057**	**	**-0.038**	*
		(-2.45)		**(-1.85)**	
$Ret_{i,t}$	+	**0.174**	***	**0.101**	***
		(4.95)		**(3.43)**	
$Guide_{i,t+1}$	?	0.018		0.014	
		(0.91)		(0.80)	
Year-fixed effects		Included		Included	
Industry-fixed effects		Included		Included	
Number of observations		1,179		1,179	
Adjusted R^2		58.03%		69.68%	

This table reports the results of estimating Equation (1) using the OLS regression, where all *t*-statistics (in parentheses) are based on the standard errors clustered by firm. Appendix C provides the variable definitions. All of the variables are winsorized at the 1% and 99% levels. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

TABLE 5
Effect of Incorporating Analyst Forecasts into Target Setting on Target Accuracy

$$|Target\ deviation_{i,t+1}| = \lambda_0 + \lambda_1\ |Target\ deviation_{i,t}| + \varepsilon_{i,t} \qquad (3)$$

		Dependent variable:	$\|Target\ deviation_{i,t+1}\|$
Independent variables:	Pred.	(1)	(2)
Intercept		**0.096** ***	**0.137** ***
		(7.07)	**(9.50)**
Target deviation$_{i,t}$	+	**0.559** ***	**0.652** ***
		(6.11)	**(7.78)**
Number of observations		1,148	1,148
R^2		15.15%	16.62%
Vuong's (1989) Z-statistic		Z-statistic: 3.167 (*p*-value = 0.002)	

This table reports the results of estimating Equation (3). $|Target\ deviation_{i,t}|$ is the absolute value of (*Actual EPS$_{i,t}$* − *Target EPS$_{i,t}$*) divided by *Target EPS$_{i,t}$*. In Column (1), we define $|Target\ deviation_{i,t+1}|$ as the absolute value of (*Actual EPS$_{i,t+1}$* − *Target EPS$_{i,t+1}$*) divided by *Target EPS$_{i,t}$*. In Column (2), we define $|Target\ deviation_{i,t+1}|$ as the absolute value of (*Actual EPS$_{i,t+1}$* − *Predicted Target EPS$_{i,t+1}$*) divided by *Target EPS$_{i,t}$*. *Predicted Target EPS$_{i,t+1}$* is estimated as the sum of *Target EPS$_{i,t}$* and the predicted target revision for year *t*+1 is calculated using the following model:

$$Target\ revision_{i,t+1} = \lambda_0 + \lambda_1\ Target\ deviation_{i,t} + \lambda_2\ Target\ deviation_{i,t} \times D\_NEG_{i,t} + \lambda_3\ Relative\text{-}to\text{-}peers_{i,t}$$
$$+ \lambda_4\ Growth_{i,t+1} + \lambda_5\ D\_NEG_{i,t} + \lambda_6\ Ret_{i,t} + \lambda_7\ Guide_{i,t+1} + Year\ and\ industry\ fixed\ effects + \varepsilon_{i,t}$$

The Vuong (1989) test is a likelihood ratio-based test for model selection. The OLS regression is estimated, where all *t*-statistics (in parentheses) are based on the standard errors clustered by firm. Appendix C provides the variable definitions. All of the variables are winsorized at the 1% and 99% levels. *** denotes statistical significance at the 1% level.

TABLE 6

Effects of the Informativeness of Analyst Forecasts on the Use of Analyst Forecasts in Target Setting (H2)

$$Target\ revision_{i,t+1} = \lambda_0 + \lambda_1\ Target\ deviation_{i,t} + \lambda_2\ Target\ deviation_{i,t} \times D\_NEG_{i,t} + \lambda_3\ Analyst\ forecast\ dev_{i,t+1}$$
$$+ \lambda_4\ Analyst\ forecast\ dev_{i,t+1} \times Forecast\ error\ quartile_{i,t}$$
$$+ \lambda_5\ Analyst\ forecast\ dev_{i,t+1} \times Forecast\ dispersion\ quartile_{i,t+1}$$
$$+ \lambda_6\ Analyst\ forecast\ dev_{i,t+1} \times Analyst\ following_{i,t+1} + \lambda_7\ Analyst\ forecast\ dev_{i,t+1} \times Cyclicality_{i,t}$$
$$+ \lambda_8\ Forecast\ error\ quartile_{i,t} + \lambda_9\ Forecast\ dispersion\ quartile_{i,t+1} + \lambda_{10}\ Analyst\ following_{i,t}$$
$$+ \lambda_{11}\ Cyclicality_{i,t} + \lambda_{12}\ Relative\text{-}to\text{-}peers_{i,t} + \lambda_{13}\ Growth_{i,t+1} + \lambda_{14}\ D\_NEG_{i,t} + \lambda_{15}\ Ret_{i,t} + \lambda_{16}\ Guide_{i,t+1}$$
$$+ Year\ and\ industry\ fixed\ effects + \varepsilon_{i,t} \qquad (4)$$

	Dependent variable:	$Target\ revision_{i,t+1}$	
Independent variables:	Pred.		
Intercept		-0.061 (-0.99)	
$Target\ deviation_{i,t}$	+	**1.052** **(14.66)**	***
$Target\ deviation_{i,t} \times D\_NEG_{i,t}$	−	**-0.554** **(-4.29)**	***
$Analyst\ forecast\ dev_{i,t+1}$	+	**0.634** **(3.59)**	***
$Analyst\ forecast\ dev_{i,t+1} \times Forecast\ error\ quartile_{i,t}$	−	**-0.071** **(-1.66)**	**
$Analyst\ forecast\ dev_{i,t+1} \times Forecast\ dispersion\ quartile_{i,t+1}$	−	**-0.215** **(-6.71)**	***
$Analyst\ forecast\ dev_{i,t+1} \times Analyst\ following_{i,t+1}$	+	**0.101** **(1.71)**	**
$Analyst\ forecast\ dev_{i,t+1} \times Cyclicality_{i,t}$	+	**0.221** **(1.75)**	**
$Forecast\ error\ quartile_{i,t}$	?	0.007 (0.95)	
$Forecast\ dispersion\ quartile_{i,t+1}$	?	**0.025** **(3.41)**	***
$Analyst\ following_{i,t+1}$	?	0.002 (0.13)	
$Cyclicality_{i,t}$	?	-0.010 (-0.50)	
$Relative\text{-}to\text{-}peers_{i,t}$	−	**-0.007** **(-1.91)**	*
$Growth_{i,t+1}$	+	0.006	

		(0.77)	
$D\_NEG_{i,t}$	?	**-0.057**	***
		(-2.62)	
$Ret_{i,t}$	+	**0.080**	**
		(2.57)	
$Guide_{i,t+1}$	?	0.020	
		(1.07)	
Year-fixed effects		Included	
Industry-fixed effects		Included	
Number of observations		1,158	
Adjusted R^2		66.07%	

This table reports the results of estimating Equation (4). The sample includes 1,158 firm-year observations during the 2006 to 2014 period. The OLS regression is estimated, where all *t*-statistics (in parentheses) are based on the standard errors clustered by firm. Appendix C provides the variable definitions. All of the variables are winsorized at the 1% and 99% levels. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (one-tailed for $Analyst\ forecast\ dev_{i,t+1} \times Forecast\ error\ quartile_{i,t}$, $Analyst\ forecast\ dev_{i,t+1} \times Forecast\ dispersion\ quartile_{i,t+1}$, $Analyst\ forecast\ dev_{i,t+1} \times Analyst\ following_{i,t+1}$, and $Analyst\ forecast\ dev_{i,t+1} \times Cyclicality_{i,t}$), respectively.

TABLE 7
Effects of Analyst Affiliation on the Association Between Analyst Forecasts and Target Revisions (H3)

$$Target\ revision_{i,t+1} = \lambda_0 + \lambda_1\ Target\ deviation_{i,t} + \lambda_2\ Target\ deviation_{i,t} \times D\_NEG_{i,t} + \lambda_3\ Analyst\ forecast\ dev_{i,t+1} + \lambda_4\ Relative\text{-}to\text{-}peers_{i,t} + \lambda_5\ Growth_{\ i,t+1} + \lambda_6\ D\_NEG_{i,t} + \lambda_7\ Ret_{\ i,t} + \lambda_8\ Guide_{\ i,t+1} + Year\ and\ industry\ fixed\ effects + \varepsilon_{i,t} \quad (1)$$

Independent variables:	Pred.	(1) Analyst forecasts of affiliated analysts		(2) Analyst forecasts of unaffiliated analysts	
Intercept		0.096		0.133	
		(0.77)		(1.06)	
Target deviation$_{i,t}$	+	**0.791**	***	**0.802**	***
		(2.80)		**(2.99)**	
Target deviation$_{i,t}$ × *D_NEG$_{i,t}$*	–	-0.062		-0.012	
		(-0.22)		(-0.05)	
Analyst forecast dev$_{i,t+1}$	+	**0.580**	***	**0.620**	***
		(6.16)		**(5.92)**	
Relative-to-peers$_{i,t}$	–	**-0.040**	***	**-0.044**	***
		(-3.04)		**(-3.49)**	
Growth$_{i,t+1}$	+	-0.023		-0.021	
		(-0.67)		(-0.64)	
D_NEG$_{i,t}$	?	**-0.158**	**	**-0.149**	**
		(-2.21)		**(-2.10)**	
Ret$_{i,t}$	+	0.078		0.055	
		(1.27)		(0.95)	
Guide$_{i,t+1}$	?	-0.028		-0.012	
		(-0.45)		(-0.17)	
Chi-squared test: (1) λ_3 = (2) λ_3, 4.75 (p-value = 0.0293)					
Year-fixed effects		Included		Included	
Industry-fixed effects		Included		Included	
Number of observations		119		119	
Adjusted R^2		80.82%		81.70%	

This table reports the results of estimating Equation (1) using the OLS regression, where all *t*-statistics (in parentheses) are based on the standard errors clustered by firm. An analyst is classified as an affiliated analyst if his or her brokerage firm was a lead or co-underwriter of the covered firm's IPO and SEO over the previous 10 years (Malmendier and Shanthikumar 2014). Column (1) shows the test results using *Analyst forecast dev$_{i,t+1}$* measured based on analyst forecasts issued by affiliated analysts. Column (2) shows the test results using *Analyst forecast dev$_{i,t+1}$* measured based on analyst forecasts issued by unaffiliated analysts. Appendix C provides the variable definitions. All of the variables are winsorized at the 1% and 99% levels. *** and ** denote statistical significance at the 1% and 5% levels, respectively.

TABLE 8
Additional Analyses I
Panel A: Relative Importance of Analyst Forecasts Versus Management Forecasts in Target Setting

$$\begin{aligned} Target\ revision_{i,t+1} = \lambda_0 + \lambda_1\ Target\ deviation_{i,t} + \lambda_2\ Target\ deviation_{i,t} \times D\_NEG_{i,t} + \lambda_3\ Analyst\ forecast\ dev_{i,t+1} \\ + \lambda_4\ Management\ forecast\ dev_{i,t+1} + \lambda_5\ Relative\text{-}to\text{-}peers_{i,t} + \lambda_6\ Growth_{i,t+1} + \lambda_7\ D\_NEG_{i,t} \\ + \lambda_8\ Ret_{i,t} + Year\ and\ industry\ fixed\ effects + \varepsilon_{i,t} \end{aligned} \quad (6)$$

		Dependent variable:		*Target revision*$_{i,t+1}$		
Independent variables:	Pred.	(1)	(2)	(3)	(4)	(5)
Intercept		-0.043 (-1.38)	**0.110** *** **(3.96)**	0.000 (0.01)	-0.019 (-0.50)	**0.102** *** **(3.59)**
Target deviation$_{i,t}$	+	**1.121** *** **(10.53)**	**1.068** *** **(11.48)**	**1.255** *** **(7.30)**	**1.014** *** **(7.40)**	**1.048** *** **(11.30)**
Target deviation$_{i,t}$ × *D_NEG*$_{i,t}$	–	**-0.409** *** **(-2.86)**	-0.174 (-1.30)	**-1.069** *** **(-4.00)**	-0.240 (-1.16)	-0.112 (-0.87)
Analyst forecast dev$_{i,t+1}$	+		**0.551** *** **(6.96)**	**0.220** ** **(2.18)**	**0.712** *** **(6.11)**	**0.550** *** **(6.93)**
Management forecast dev$_{i,t+1}$	+	**0.284** *** **(4.18)**	-0.005 (-0.13)		-0.054 (-1.18)	-0.005 (-0.14)
Relative-to-peers$_{i,t}$	–	**-0.010** * **(-1.91)**	-0.005 (-1.11)	-0.010 (-1.04)	-0.016 (-1.64)	-0.004 (-0.96)
Growth$_{i,t+1}$	+	0.004 (0.61)	-0.003 (-0.61)	0.003 (0.16)	0.018 (1.21)	-0.003 (-0.63)
D_NEG$_{i,t}$	?	-0.021 (-1.22)	-0.008 (-0.46)	-0.104 (-1.64)	-0.042 (-1.49)	-0.005 (-0.33)
Ret$_{i,t}$	+	**0.110** *** **(4.04)**	**0.053** ** **(2.10)**	0.085 (1.35)	**-0.103** * **(-1.84)**	**0.057** ** **(2.34)**
Year-fixed effects		Included	Included	Included	Included	Included
Industry-fixed effects		Included	Included	Included	Included	Included
Number of observations		695	695	484	115	665
Adjusted R^2		62.02%	72.71%	61.74%	82.29%	75.30%

TABLE 8 (Continued)

Panel B: Correlations Between Low (High) Bonus Targets and Walked-Down (Walked-Up) Analyst Forecasts

	Low bonus target$_{i,t+1}$	*High bonus target*$_{i,t+1}$	*Walked-down analyst forecasts*$_{i,t+1}$
High bonus target$_{i,t+1}$	**-0.961** ***		
Walked-down analyst forecasts$_{i,t+1}$	-0.033	0.025	
Walked-up analyst forecasts$_{i,t+1}$	0.032	-0.026	**-0.994** ***

Panel A of this table reports the results of estimating Equation (6). Panel B reports the correlation matrix for the relation between low (high) bonus targets and walked-down (walked-up) analyst forecasts. In Columns (1) and (2) of Panel A, the regressions are estimated using a reduced sample of firms that issued management earnings forecasts. The results in Column (3) are based on a reduced sample of firms that did not issue management earnings forecasts. The results in Columns (4) and (5) are based on a sample of firms that issued management earnings forecasts, where *Analyst forecast dev*$_{i,t+1}$ in Column (4) is measured using analyst forecasts issued *before* management forecasts and *Analyst forecast dev*$_{i,t+1}$ in Column (5) is measured using analyst forecasts issued *after* management forecasts. The results in Panel A are based on the OLS regression, where all *t*-statistics in parentheses are based on standard errors clustered by firm. The results in Panel B are based on individual analyst-level data. The sample for Panel B is 665 observations at the firm-year-analyst level. Appendix C provides the variable definitions. All of the variables are winsorized at the 1% and 99% levels. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

TABLE 9
Additional Analyses II

Panel A: Alternative Growth Measures

$$\textit{Target revision}_{i,t+1} = \lambda_0 + \lambda_1 \textit{ Target deviation}_{i,t} + \lambda_2 \textit{ Target deviation}_{i,t} \times \textit{D\_NEG}_{i,t} + \lambda_3 \textit{ Analyst forecast dev}_{i,t+1} + \lambda_4 \textit{ Relative-to-peers}_{i,t} + \lambda_5 \textit{ Growth}_{i,t+1} + \lambda_6 \textit{ D\_NEG}_{i,t} + \lambda_7 \textit{ Ret}_{i,t} + \lambda_8 \textit{ Guide}_{i,t+1} + \textit{Year and industry fixed effects} + \varepsilon_{i,t} \quad (1)$$

		Dependent variable:	*Target revision*$_{i,t+1}$	
Growth measure based on:		Sales	Income before extraordinary items	Operating income before depreciation
Independent variables:	Pred.	(1)	(2)	(3)
Intercept		0.009 (0.47)	0.014 (0.69)	0.017 (0.72)
Target deviation$_{i,t}$	+	**0.982** *** **(15.85)**	**0.982** *** **(15.69)**	**0.983** *** **(15.69)**
Target deviation$_{i,t}$ × *D_NEG*$_{i,t}$	−	**-0.357** *** **(-2.85)**	**-0.355** *** **(-2.82)**	**-0.357** *** **(-2.79)**
Analyst forecast dev$_{i,t+1}$	+	**0.371** *** **(5.38)**	**0.376** *** **(5.46)**	**0.377** *** **(5.46)**
Relative-to-peers$_{i,t}$	−	-0.006 (-1.65)	-0.006 (-1.58)	-0.006 (-1.62)
Growth$_{i,t+1}$	+	0.145 (1.64)	-0.001 (-0.11)	-0.009 (-0.40)
D_NEG$_{i,t}$	?	**-0.039** * **(-1.89)**	**-0.038** * **(-1.85)**	**-0.039** * **(-1.88)**
Ret$_{i,t}$	+	**0.091** *** **(3.09)**	**0.102** ** **(3.42)**	**0.103** ** **(3.53)**
Guide$_{i,t+1}$	?	0.013 (0.76)	0.014 (0.80)	0.014 (0.81)
Year-fixed effects		Included	Included	Included
Industry-fixed effects		Included	Included	Included
Number of observations		1,179	1,179	1,176
Adjusted R^2		69.78%	69.68%	69.64%

TABLE 9 (Continued)

Panel B: Alternative Measure for Peer Performance

$$Target\ revision_{i,t+1} = \lambda_0 + \lambda_1\ Target\ deviation_{i,t} + \lambda_2\ Target\ deviation_{i,t} \times D\_NEG_{i,t} + \lambda_3\ Analyst\ forecast\ dev_{i,t+1} + \lambda_4\ Relative\text{-}to\text{-}peers_{i,t} + \lambda_5\ Growth_{i,t+1} + \lambda_6\ D\_NEG_{i,t} + \lambda_7\ Ret_{i,t} + \lambda_8\ Guide_{i,t+1} + Year\ and\ industry\ fixed\ effects + \varepsilon_{i,t} \quad (1)$$

	Dependent variable:	*Target revision*$_{i,t+1}$
Independent variables:	Pred.	(1)
Intercept		0.024 (0.51)
Target deviation$_{i,t}$	+	**0.985** *** **(14.35)**
Target deviation$_{i,t}$ × *D_NEG*$_{i,t}$	−	**-0.368** *** **(-2.48)**
Analyst forecast dev$_{i,t+1}$	+	**0.322** *** **(4.46)**
Relative-to-peers$_{i,t}$	−	-0.001 (-0.27)
Growth$_{i,t+1}$	+	-0.004 (-0.49)
D_NEG$_{i,t}$	?	**-0.041** * **(-1.86)**
Ret$_{i,t}$	+	**0.109** *** **(3.32)**
Guide$_{i,t+1}$	?	0.017 (0.89)
Year-fixed effects		Included
Industry-fixed effects		Included
Number of observations		1,028
Adjusted R^2		67.00%

Panel A of this table reports the estimation results of Equation (1) using alternative measures of *Growth*$_{i,t+1}$. We replace the dependent variable in Equation (2) with alternative growth proxies, such as growth in sales, income before extraordinary items, and operating income before depreciation. Panel B reports the results of estimating Equation (1) using the alternative method of identifying industry peers following Jayaraman et al. (2015) and Hoberg and Phillips (2016). The OLS regression is estimated, where all *t*-statistics (in parentheses) are based on the standard errors clustered by firm. Appendix C provides the variable definitions. All of the variables are winsorized at the 1% and 99% levels. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.