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Compensation goals and firm performance

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ABSTRACT

Using a large data set of performance goals employed in executive incentive contracts, we find that a disproportionately large number of firms exceed their goals by a small margin as compared to the number that fall short of the goal by a similar margin. This asymmetry is particularly acute for earnings goals, when compensation is contingent on a single goal, when the pay-performance relationship around the goal is concave-shaped, and for grants with non-equity-based payouts. Firms that exceed their compensation target by a small margin are more likely to beat the target the next period and CEOs of firms that miss their targets are more likely to experience a forced turnover. Firms that just exceed their Earnings Per Share (EPS) goals have higher abnormal accruals and lower Research and Development (R&D) expenditures, and firms that just exceed their profit goals have lower Selling, General and Administrative (SG&A) expenditures. Overall, our results highlight some of the costs of linking managerial compensation to specific compensation targets.

1. Introduction

In their ongoing effort to link managerial pay to performance, firms are increasingly tying non-equity *and* equity grants to achieving explicit performance goals. Institutional investors and large shareholders like Warren Buffett have been major proponents of assessing management against specific performance goals. A typical equity or non-equity grant linked to firm performance identifies threshold, target, and maximum values for one or more accounting, or stock price-based metrics. The payout from the grant or the vesting schedule of the grant is then tied to the firm achieving these particular performance goals. For example, a manager may receive no payout if performance is below the threshold and her payout may increase as performance exceeds the threshold. The slope of the pay-performance relationship (PPR) may also change at the target and the maximum value, with discontinuous slope changes generating a "kink" in the PPR.[1] In this paper, we use a comprehensive data set containing information on the performance goals employed in pay contracts to highlight some of the costs of this popular pay feature.

Rewarding managers for achieving explicit performance goals certainly has a bright side. It makes pay more transparent and offers strong incentives, especially when the goal is challenging. On the other hand, identifying explicit performance goals and having "jumps and kinks" in the PPR at the goals may also have a dark side. If there is a jump in managerial pay for achieving a performance goal, and if actual performance is close to but short of the goal, managers may be tempted to take actions – with possible negative long-term consequences – to push reported performance to (or past) the goal. In other words, managerial myopia may be exacerbated around "jump points" in the

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[1] See Appendix B for the description of a few bonus and stock grants linked to firm performance targets.

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PPR. The effect of kinks on managerial behavior is more nuanced. If the kink is concave, it may reduce the manager's incentives to improve firm performance much beyond the kink. On the other hand if the kink is convex, it will not only incentivize managers to push performance beyond the kink, but may also affect their incentives to take risk.

Explicit target performance goals may also influence reported firm performance for reasons not directly related to the payout from the grant. Managers may not want to exceed the target performance by a large amount if better current period performance results in higher targets in subsequent periods ("target ratcheting effect"). If the board focuses on the target as the expected performance and punishes underperformance, say by firing the CEO, then CEOs may want to achieve the target performance and not fall short. We call this the "forced turnover effect." We use our data to understand how goals in the incentive contracts influence reported performance. Specifically, we study the distribution of reported performance around the incentive goals and test to see if performance clusters around the goals. We also conduct tests to explore the possible reasons for such clustering.

If firms manage reported accounting performance to either beat the goal or to not exceed the goal by a large amount, then the actual performance of a disproportionate number of firms will just exceed the goal as compared to the number that just miss the goal. In other words, the distribution of reported performance will exhibit a discontinuity around the goal (Burgstahler and Dichev, 1997; Bollen and Pool, 2009). McCrary (2008) develops a test to identify if a probability density has a statistically significant discontinuity at a given point. We employ this methodology, along with the tests in Bollen and Pool (2009) and additional bootstraping techniques to test for the presence of discontinuities.[2]

We obtain data on performance goals from Incentive Lab (IL) who in turn obtain it from firms' proxy statements. We have information on all the cash, stock, and option grants awarded to a top five, highest paid executives of the 750 largest firms by market capitalization over the time period 1998–2012. We have information on the metric(s) the grant is tied to, the nature of the relationship, i.e., whether the payout or vesting schedule is tied to the metric(s), and the nature (absolute versus relative) and specific value of the performance goal. Given our interest to detect performance management, for most of the paper we focus on grants to the firm's CEO linked to an absolute accounting-based metric that we can match with actual performance as reported in Compustat. This limits the grants to those that are tied to the level or the growth of one of the following metrics: Earnings, EPS, Sales, Earnings Before Interest and Tax (EBIT), Earnings Before Interest, Taxes, Depreciation and Amortization (EBITDA), Operating income, and Funds From Operations (FFO). This results in a sample of 5,810 grants awarded by 974 firms.[3] Among the accounting metrics employed, EPS is the most popular with around 46% of the grants linked to an EPS goal. Cash and stock are the most popular modes of payout for the grants in our sample, with over 72% (28%) of the grants involving some cash (stock) payout.

We begin our empirical analysis by comparing the target performance in the pay contract to the firm's reported performance. We focus much of our analysis on the target because not only do we have information about the target for most grants, but firm performance often clusters around the target and this increases the power of our tests of discontinuity in the underlying density. We construct a variable, *Actual less target* to help us identify clustering of performance at the goal. *Actual less target* is the difference between actual performance as reported in Compustat and the target goal as identified in the pay contract. We construct this separately for EPS, sales, and profit goals and normalize each by its standard deviation before combining into a single variable. We normalize by standard deviation to adjust for possible noise in our matching of actual performance and compensation goals. We find that the density of *actual less target* has a significant discontinuity at zero. A disproportionately large number of firms exceed the performance target by a small amount as compared to the number of firms that fail to meet the performance target by a small amount. These results are confirmed by the two other methods we employ to test for discontinuity, namely, the bootstrapping test and the regression-based test.

When we focus on the individual performance measures, the (McCrary, 2008) test shows a statistically significant discontinuity only for EPS goals. The discontinuity around profit and sales goals is not statistically significant. In contrast, our bootstrapping exercise finds a discontinuity for all three measures.

Next, we study the relationship between threshold and reported performance. Here again, we find that firms are significantly more likely to beat the threshold by a small margin as compared to just miss the threshold by a small margin. Since there usually is a jump in pay at the threshold performance for most of the grants in our sample, the clustering of performance around the threshold is less of a surprise.

We perform a number of cross-sectional tests to better understand the reasons for the observed discontinuity. Many plans include multiple metrics and since metrics are generally positively correlated, it will be difficult for executives to "just barely beat" the target for all metrics simultaneously. For example, if a CEO aims to meet an EPS goal by a small margin, she might inadvertently beat the profit target by a wide margin. Therefore, if performance clusters at the target because of performance management, then we should see more clustering for grants contingent on a

[2] To the extent managerial pay discretely increases at the goal, a discontinuity in reported performance at the goal may also be consistent with managers working "very hard" when actual performance is close to the goal. We call this the "effort channel." Since we don't observe managerial effort, it is very difficult to distinguish the effort channel from the performance management channel. We compare firms that just beat and just miss benchmarks on a number of observable dimensions to characterize the firms whose performance clusters just above the goal. These tests help us understand the underlying mechanism at work.

[3] We also design placebo tests on grants linked to relative performance goals, for which we include grants tied to relative stock and accounting performance.

single metric. Consistent with this, when we divide our sample into executives that obtain grants contingent on single versus multiple metrics, we find that the discontinuity at the target is larger for executives who obtain grants contingent on a single metric. Since the methodology in McCrary (2008) does not allow for a statistical comparison of the size of two discontinuities, we employ a bootstrapping methodology and a regression-based technique to statistically compare the size of the discontinuities.

We classify the grants as concave or convex based on a comparison of the slopes of the PPR to the right and to the left of the target and compare the size of the discontinuity for both sets of grants. Consistent with concave grants reducing incentives to exceed the goal by a large amount, we find a significant discontinuity at the target only for concave grants. These findings are confirmed by both our bootstrapping exercise and regression analysis.

Grants may involve an equity or non-equity payout. To the extent the stock price is positively related to the performance metric, a grant involving equity payout is likely to introduce a convexity in the PPR. When we compare grants with equity and non-equity payouts, we find a significant discontinuity at the target only for grants that involve non-equity payouts. This is consistent with the presence of a discontinuity for concave grants and not for convex grants.

Our regression-based test to cross-sectionally compare the size of the discontinuities is similar to the test in McCrary (2008), and involves comparing the actual number of firms whose performance falls within a bin to an expected number. That is, for any metric, such as say, EPS, we use the bin size as recommended by McCrary (2008) and divide all our sample firms into bins based on reported EPS. The dependent variable in the regression is *# Firms*, defined as the logarithm of one plus the number of firms in each bin. We do a similar exercise for sales and profit measures as well. Our main independent variable is *# Goals* which is defined as the logarithm of one plus the number of firms with the target or threshold performance in a particular bin. If firms manage reported performance so as to exceed a goal, then we expect their reported performance to fall near (within the same bin as) the performance goal. We model the expected number of firms in each bin in a flexible manner by including a fourth-order polynomial of the mid-point of the bin.

In comparison to McCrary (2008), the regression analysis has a number of advantages and a few disadvantages. We discuss these in detail in Section 6.3. Our results from the regression analysis are broadly consistent with our graphical analysis. The presence of a performance goal in a bin increases the probability of an additional firm's performance falling in the bin by 20%. This effect is present for grants contingent upon a single or multiple metrics, for grants that involve a concave kink at the target, and for grants that involve a non-equity payout.

Absent a jump in pay at the target for most of the grants in our sample, there are three non-mutually exclusive reasons for the clustering of firm performance slightly higher than the target. They are the target ratcheting effect, the forced turnover effect, and reference-based preference effect. We find two pieces of evidence consistent with the target ratcheting effect. First, targets are positively related to past performance. Second, firms whose performance clusters around the target are more likely to meet their target in the next period. We also find evidence consistent with the turnover effect in that CEOs who miss their performance target are more likely to be forced out identified using the methodology of Parrino (1997). We find this effect even after we control for actual performance in a flexible manner. While reference-based preferences may also drive managers to focus on the target performance and slack off once performance exceeds the target, we do not perform any direct tests of this effect because of the difficulty in proxying for managerial preferences.

If firms meet performance goals by managing reported performance, then the tendency to just meet goals should be weaker for relative-performance goals. We find that is indeed the case. When relative-performance goals are compared to the firm's relative performance, we do not find a tendency for firms to just beat their performance goals.

To understand how firms meet their accounting performance goals, in our final set of tests we compare the levels of accruals, expenditures on both R&D and SG&A, and share repurchases for firms that just exceed the goal, that is, the firms that fall in the first bin above the performance goal (either target or threshold) and the firms that just miss the goal – that is, firms whose performance is in the two bins below the performance goal.[4] Since firms deliberately pick performance goals and may take deliberate action to meet those goals, firms that meet and miss goals are not likely to be randomly selected. To this extent, our evidence should not be interpreted as causal in nature.

We find that firms that exceed the EPS goal by a small margin have much higher abnormal accruals and smaller changes in R&D expenditure as compared to firms that miss the goal by a small margin. Firms that exceed the profit goal have significantly lower SG&A expenses as compared to firms that miss the goal by a small margin. Thus, overall our evidence is consistent with firms using both accruals and cuts to discretionary expenditures to meet EPS and profit goals, respectively (see also Graham et al., 2005; Roychowdhury, 2006).

The remainder of our paper is organized as follows: Section 2 covers the related literature, Section 3 discusses our empirical methodology, Section 4 outlines our hypotheses, Section 5 discusses our data, Section 6 presents our empirical tests, and Section 7 concludes.

2. Related literature

Our paper is most closely related to the prior literature that studies how executives manage performance to meet or beat pre-established performance goals. These

[4] Note that these bins are identified in an "optimal" manner using the procedure in McCrary (2008). Since there is a disproportionately large number of firms in the bin above the performance goal as compared to the bin below the performance goal, we include firms in the two bins below the performance goal to ensure a relatively equal number of firms that exceed and miss the goal.

include the zero EPS goal (Burgstahler and Dichev, 1997) and the consensus analyst estimates (Bartov et al., 2002). While these studies show that executives manage earnings to avoid losses or meet analyst expectations, we are the first to show that they manage performance to meet compensation plan goals. Our results highlight the costs of having *explicit* performance goals when executives exercise power over the reported performance. Furthermore, we not only know the monetary penalty managers face for not meeting a performance goal, thereby being able to design sharper cross-sectional tests, but our analysis also helps highlight the important role performance goals can play in predicting actual firm performance.

Our research is also related to the literature that studies optimal contracts in a setting where the agent can manipulate the observable performance measure. Crocker and Slemrod (2008) show that compensation contracts that are written in terms of reported earnings cannot provide incentives for managers to simultaneously maximize profits and also report those profits truthfully. Maggi and Rodríguez-Clare (1995) study a principal-agent problem in which the agent is privately informed about his marginal cost of production. In their paper, costly information distortion emerges as an equilibrium behavior. Additionally, Guttman et al. (2006) find that there exist equilibria in which kinks and jumps emerge endogenously in the distribution of reported earnings.

A large literature in accounting and finance documents a correlation between performance and performance goals. Cheng et al. (2015) find that firms may repurchase shares to manipulate EPS to achieve bonus targets. Roychowdhury (2006) and Dechow et al. (2003) find that firms may reduce discretionary expenditures, such as R&D and SG&A, to improve reported margins and avoid reporting a loss. Additionally, Graham et al. (2005) show that when surveyed, a majority of CEOs admit to sacrificing long-term value to smooth earnings. Bergstresser and Philippon (2006) provide evidence that the use of discretionary accruals to manipulate reported earnings is related to the amount of stock-based pay. In comparison, we find that firms increase accruals and cut discretionary spending to meet *highly specific* performance goals explicitly embedded in compensation contracts.

Our paper is also related to the strand of literature that studies the use of performance provisions in executive compensation. Healy (1985) examines short-term accounting-based performance goals and finds a strong association between accruals and managers' income-reporting incentives. Murphy (2000) describes and differentiates between internally and externally determined performance standards. Bettis et al. (2010, 2013) explore the usage, determinants, and implications of performance-vesting provisions in executive stock and option grants, and find that firms with such provisions have better subsequent operating performance. Gong et al. (2011) study grants that are tied to relative performance and find a weak relationship between the relative performance targets and future peer group performance. Kuang and Qin (2009) find that performance-vesting stock option plans are associated with better executive incentives among non-financial U.K. firms. Unlike these papers, we focus on the role of performance provisions that may provide incentives to manage reported performance.

Our paper is also related to the literature that highlights the costs and benefits of alternate metrics to evaluate executive performance. Holmstrom (1979) argues for the use of metrics that are most informative about CEO effort. More recently, Matêjka et al. (2009) hypothesize that metrics are chosen in response to past poor performance, while Gao et al. (2012) hypothesize that good past performance is indicative of the importance of a given metric. In comparison, our paper highlights the costs of picking metrics that can be readily managed by the executive.

In addition to the intended contribution to the literature, our paper may also add to the already active, policy-oriented, executive compensation debate. Large investors are in favor of evaluating managers against specific performance goals. There is also increasing pressure from proxy advisory firms such as Institutional Shareholder Services (ISS) and Glass Lewis for the use of explicit performance goals in executive compensation. Our results suggest that the effective use of such provisions requires more careful design and greater board oversight on firm performance to minimize executives gaming of reported performance to meet the goals.

3. Empirical methodology

In this section, we describe the tests that we perform to identify manipulation of firm performance to meet goals. All the tests look for discrepancies in the distribution of reported performance.

The first test we implement is the one described in McCrary (2008) that is designed to test for the presence of a discontinuity at a point in a density. To implement this test, we construct variables that measure the difference between actual performance and the stated goal, and test for discontinuity at zero, i.e., at the performance goal. The test involves two steps. In the first step, we obtain a "finely gridded histogram" of the underlying variable. The bins are carefully defined such that no bin includes points both to the left and to the right of zero. In the second step, we smooth the histogram by estimating a weighted regression separately on either side of zero. The midpoints of the histogram bins are treated as the regressor and the normalized counts of the number of observations falling within each bin are treated as the outcome variable. The weighing function is a triangular kernel that gives most weight to the bins nearest to where one is trying to estimate the discontinuity. The test for discontinuity is then implemented as a Wald test of the null hypothesis that the discontinuity is zero. We implement the test using the "DCdensity" function in STATA. The output of this function includes both the first-step histogram and the second-step "smoother," along with 95% confidence intervals (CI) of the second-step density.

The critical parameters in the test are the bin-size for the first-step histogram and the bandwidth used in the second-stage estimation. For our analysis, we use the default bin-size and bandwidth as recommended by the DCdensity function. The default bin-size b equals $2\sigma n^{-1/2}$, where σ is the sample standard deviation and n is the number of observations. To estimate the default bandwidth, the "DCdensity" function estimates the weighted regression described above and for each side, it computes $3.348[\tilde{\sigma}^2(b-a)/\sum \tilde{f}''(X_j)^2]^{1/5}$, and sets the bandwidth equal to the average of the two quantities. In this formula, $\tilde{\sigma}^2$ is the mean-squared error of the regression, and $b-a$ equals X_j for the right-hand regression and $-X_j$ for the left-hand regression, where X_j is the bin-size and $\tilde{f}''(X_j)^2$ is the estimated second derivative implied by the global polynomial model.

The second test that we conduct to detect performance manipulation is from Bollen and Pool (2009). This test not only serves as a robustness check on the test in McCrary (2008), but also allows us to test for discontinuities all through the density. This test is similar to McCrary (2008) and involves dividing the data into bins, estimating a smooth density, and comparing the actual number of observations to those predicted by the smooth density. The bin-size for the first-stage histogram is estimated to minimize the mean-square error and is equal to $0.7764 \times 1.364 \times \min\,[\sigma,\ \frac{Q}{1.34}] \times n^{-\frac{1}{5}}$, where σ is the standard deviation, Q the interquartile range, and n the number of observations.

In the second stage, the test uses the Gaussian kernel and estimates the smooth density. The bandwidth for the second-stage estimation is set equal to the bin size from the first stage. The test then uses an estimate of sampling variation in the histogram to determine whether the actual number of observations in a given bin is significantly different from the expected number under the null hypothesis of a smooth underlying distribution. If p denotes the probability that an observation lies in a bin (estimated by integrating the kernel density along the boundary of each bin), then according to the Demoivre-Laplace theorem the actual number of observations in a bin is asymptotically normally distributed with mean np and standard deviation $np\ (1-\ p)$, where n is the total number of observations. This is used to design the test for discontinuity all along the density.

An important limitation of the tests described above is that they do not allow one to compare the size of the discontinuities at two points in the density or across densities. To do this, we do a bootstrapping exercise and a regression-based analysis to complement the above two tests. In our bootstrapping exercise, we draw a random sample from the variable of interest and count the number of observations that lie in the first bin to the right of zero and the number of observations that lie in the first bin to the left of zero. We repeat this 1,000 times and compare the means. To do cross-sectional tests we do the sampling separately, say for single and multiple metric-based grants, and compare the size of the differences. We describe our regression-based tests in greater detail in Section 6.3.

4. Hypothesis

In this section, we outline the hypotheses that have predictions relevant for our setting. If there is a jump in managerial pay for achieving a performance goal, and if managers realize that actual performance is likely to be close to, but short of the goal, they may take actions to push reported performance past the goal. However, if the PPR is concave at the performance goal, the manager has no incentive to go beyond the goal.[5] Both these will result in a disproportionate number of firms having reported performance at or just in excess of the goal (Burgstahler and Dichev, 1997; Bollen and Pool, 2009).

Firm performance may cluster at the target value mentioned in the grant for reasons not directly related to the payout from the grant. Managers may not want to exceed the target performance by a large amount if better performance this period results in higher targets in subsequent periods ("target ratcheting effect"). Managers may also want to achieve the target performance if the board focuses on the target as the expected performance and punishes underperformance in ways other than through a lower bonus.[6] For all these reasons we expect the reported performance of a disproportionate number of firms to exceed the goal by a small margin as compared to fall short by a small margin. This forms our first prediction.

An important feature of our data is that the payout from a grant can be contingent on a single metric or multiple metrics. Since metrics are generally positively correlated, it will be difficult for managers to "just barely beat" the target for all metrics simultaneously. For example, if a CEO manages reported performance to beat an EPS goal by a small margin, she might actually beat the profit goal by a wider margin. Thus, if performance clusters at the target because of performance management, then we should see more clustering for grants contingent on a single metric. Therefore, for our second prediction, we expect a larger discontinuity in the density of underlying performance for executives that obtain grants that depend on a single metric as compared to executives that obtain grants contingent on multiple metrics.

The slope of the PPR at the performance goal may also affect the extent of performance clustering. To the extent pay increases at a slower rate when performance exceeds a kink that is concave, we expect performance to cluster around concave kinks and *not* around convex kinks. This forms our third prediction. Note that a concave kink may not necessarily result in performance clustering just above the target. A concave kink is likely to make the agent indifferent between performing at or just above the target. If there are incentives to meet or beat the target – such as retaining one's job – and if there is some uncertainty about the true reported performance, then agents are more likely to aim just above the target to ensure that they meet or beat it. Landing exactly on the target is feasible only if managers have complete control over reported performance.

Our sample includes grants for which the payout is denominated in dollars and grants, for which (some of) the payout is in terms of number of shares. To the extent the stock price is positively related to the performance

[5] Note that we will not be able to be certain of the existence of a kink at the target unless the Compensation Discussion and Analysis (CD&A) explicitly or implicitly discloses it. What we can be sure of is whether the PPR is concave or convex at the target.

[6] One example of punishment that we test is the ouster of the CEO.

metric, grants denominated in number of shares introduce a convexity in the PPR. Hence, following the previous logic, we expect the discontinuity at the performance goal to be greater for grants involving non-equity (meaning cash) payouts as compared to grants involving equity payouts.

If firm performance clusters around the target because of the "target ratcheting" effect, then not only do we expect targets in one period to be positively related to performance the previous period, but also, as in Bouwens and Kroos (2011), we expect firms whose performance falls close to the target to be more likely to beat subsequent targets. We use these predictions to test for the relevance of the "target ratcheting" effect. If, in addition, the board evaluates managers relative to the target and punishes underperformance, then we expect CEOs who fail to meet their target to be more likely to experience forced turnover.

Depending on the metric involved, managers can employ a variety of means to meet a goal. In the case of EPS goals, managers can increase abnormal accruals, cut discretionary expenditures such as R&D and SG&A, and repurchase shares to meet a goal. Managers can meet their sales goal by increasing SG&A and accounts receivables. Managers can meet profit goals by cutting discretionary expenditures. We compare the level of *Accruals, R&D/TA, SG&A/Sales* and *Repurchase* for firms that exceed the goal by a small margin to the firms that miss the goal by a small margin to test these predictions. These tests help estimate the extent to which our results are due to management of reported performance, and importantly enable us to highlight the potential dark side of those pay contracts.

5. Data

Our data come from four sources: Incentive Lab, ExecuComp, the Center for Research in Security Prices (CRSP), and Compustat.

1. Data on the metrics used to design stock and bonus awards are from Incentive Lab (hereafter IL). Similar to Standard and Poor's (S&P) (the provider of ExecuComp), IL collects grant data from firms' proxy statements. We obtain details of all the stock, option, and cash grants to all named executives of the 750 largest firms by market capitalization for the years 1998–2012. Since Securities and Exchange Commission (SEC) standardized disclosure requirements for grants of plan-based awards after 2006, for some of our analysis, we confine the sample to the time period 2006–2012. The identity of the set of largest firms also changes from year to year, so IL backfill and forward fill data to yield a total sample of 1,166 firms for the period 2006–2012. Of these firms, 1,025 tie some of their grants to a performance metric, that is, they award "performance-based grants." For our analysis, we use information on the performance metrics employed in the grant and the specific threshold, target, and maximum performance goals specified in the award.
2. We obtain data on other components of executive pay, such as salary and bonus, from ExecuComp. We carefully hand-match IL and ExecuComp using firm tickers and executive names. Since prior studies on executive compensation predominantly use ExecuComp, we ensure comparability of IL and ExecuComp in terms of the total number of stock and options awarded during the year.
3. We complement the compensation data with stock returns from CRSP and firm and segment financial data from Compustat.

Given our interest in understanding how reported accounting performance is managed to achieve executive performance goals, for most of the paper we focus on grants linked to an absolute accounting performance metric that we can match with actual performance as reported in Compustat. This limits the grants to those that are linked to the level or the growth of one of the following metrics: EPS, Earnings, Sales, EBIT, EBITDA, Earnings Before Taxes (EBT), Operating income, and FFO. This results in a final sample of 974 firms covered by both IL and ExecuComp for the time period 2006–2012. For some of our analysis, we combine the performance metrics into earnings (EPS and earnings), sales (Sales), and profit-based (EBIT, EBITDA, EBT, Operating income, and FFO) metrics. Additionally, we restrict the sample to grants to the CEO because the performance goals in grants to non-CEOs could refer to divisional instead of firm performance.[7]

Panel A of Table 1 provides the summary characteristics of the grants that we analyze. As can be seen, EPS is the most popular metric with around 45% (2,650 out of 5,810) of the grants in our sample linking some of the payout to an EPS goal. This is followed by sales, with about 33% of the grants partly tied to a sales goal. Note that the classification of grants based on the metric employed is not mutually exclusive because a single grant can be (and typically is) tied to multiple metrics. Grants can involve a cash, stock, or option payout. We break up the grants in our sample based on the nature of the payout involved. Cash is by far the most popular payout with 72.3% of grants involving some amount of cash payout. Stock is the next most popular form of payout, while very few grants involve an option payout. Grants can also involve more than one form of payout and hence, the sum of grants involving cash, stock, and option payouts will exceed the total number of grants in our sample.

We classify a grant as *long* if its final vesting occurs more than 11 months after the grant date; 11 months is the median time between grant date and final vesting date for the grants in our sample. About 15.9% of the grants in our sample are classified as long. The fraction of the grants that we classify as long is less than 50% because a large number of grants award their final payout 11 months after the grant date. We find that grants that tie their payout to EPS are more likely to be long term as compared to grants that tie their payout to other metrics. *Concave* identifies grants that involve a concavity in the PPR at the target value, i.e., those for which the slope of the PPR to the left of the target scaled by the slope to the right of

[7] Our results are robust to including non-CEOs in our sample. For brevity, these results are not included but are available upon request.

Table 1
Summary characteristics.
This table reports the summary statistics of the key variables used in our analysis. Panel A reports the summary characteristics of grants broken down based on the metric employed. Panel B reports the summary statistics of the variables that compare actual performance outcomes to corresponding performance goals in the compensation contract. All variables are defined in detail in Appendix A. The data covers the period 2006–2012. The compensation data are from Incentive Lab (IL), Compustat, CRSP, and ExecuComp.

Panel A: Summary grant characteristics

	EPS	Earnings	Sales	EBIT	EBITDA	EBT	FFO	Operating income	Total
Number of CEOs/firms	492	291	456	106	224	135	35	399	974
Number of grants	2,650	882	1,910	294	751	347	147	1,466	5,810
Percent of grants that involve									
Non-equity-based payout	0.295	0.129	0.253	0.045	0.114	0.054	0.02	0.228	0.723
Equity-based payout	0.12	0.031	0.069	0.007	0.023	0.011	0.005	0.048	0.285
Option payout	0.007	0.001	0.002	0.001	0.002	0	0	0	0.012
Long-term vesting	0.236	0.111	0.136	0.106	0.106	0.100	0.092	0.119	0.159
Convex	0.51	0.509	0.504	0.504	0.423	0.56	0.357	0.468	0.492
Concave	0.31	0.329	0.344	0.346	0.451	0.321	0.262	0.384	0.346
Linear	0.18	0.161	0.152	0.150	0.125	0.119	0.381	0.148	0.162
Multiple	0.199	0.254	0.345	0.178	0.201	0.257	0.125	0.256	0.223

Panel B: Performance goals and actual performance

Variable	*N*	Mean	SD	P25	Median	P75
Actual less target	3,805	−0.015	1.03	−0.357	0.011	0.346
Actual less threshold	2,357	0.389	1.029	−0.054	0.281	0.861
Actual less target EPS	1,606	−0.133	0.656	−0.3	0.01	0.15
Actual less threshold EPS	1,053	0.104	0.743	−0.127	0.101	0.45
Actual less target sales	948	0.025	0.097	−0.018	0.006	0.043
Actual less threshold sales	533	0.081	0.12	0.009	0.047	0.109
Actual less target profit	1,251	0.005	0.029	−0.009	0.003	0.017
Actual less threshold profit	771	0.015	0.029	−0.002	0.012	0.032

the target is more than 1.01. *Convex* identifies grants where the slope of the PPR to the left scaled by the slope to the right is less than 0.99. *Linear* identifies grants that are neither convex nor concave. Among the grants for which we are able to construct this variable, we find that 35% of the grants are concave and 49% are convex. We find that grants tied to FFO are less likely to be concave at the target as compared to grants tied to other metrics. We classify a grant as being tied to multiple metrics if more than 50% of the grant is tied to more than one metric. We find that about 22.3% of the grants in our sample are tied to multiple metrics. Sales and EBT are more likely to be used in combination with other metrics in designing performance grants.

In the next panel, we provide the summary statistics for the key variables we employ in our analysis. In this panel, we convert our data set to have one observation per CEO-year-metric. To do this, all grants to an executive linked to the same metric (i.e., EPS for 2006) are combined into one observation. Given our interest in understanding if firm performance clusters around performance goals, if more than one grant is tied to the same accounting metric for the same year and if the goals are different, we pick the goal that is closest to the actual performance.

Actual less target EPS is the difference between the reported EPS (from Compustat) and the goal identified as the target EPS in a grant to an executive of the firm. Compustat provides four different EPS estimates for the firm, (specifically, epspi, epspx, epsfi, and epsfx) that vary based on whether they are fully diluted or not, and whether they include extraordinary items or not. IL did not collect or specify which particular EPS measure the individual grant is tied to. Hence, in constructing *Actual less target EPS*, we pick the actual EPS that is closest to the target EPS specified in the grant. Note that while this is likely to concentrate the distribution of *Actual less target EPS* around zero, it is not likely to bias our tests that compare the number of firms that just exceed the goal with the number that just miss the goal.[8] Given our interest in estimating an empirical density of the variable around zero, we truncate all the variables in this table at the 5th and 95th percentiles. While the average firm performance is just short of the targeted EPS (mean value of *Actual less target EPS* is −0.133), the median performance is very close to the targeted EPS (median value of *Actual less target EPS* is 0.01).

Actual less target sales is the difference between the actual sales and sales target mentioned in the pay contract, normalized by the book value of total assets. Firms, on average, exceed the sales target as seen from the positive mean value of *Actual less target sales.* From the mean value of *Actual less target profit*, we also find that the average firm's reported profits are higher than the target profit mentioned in the pay contract. For our main tests, we combine the three separate measures into one. We do this to increase the power of our nonparametric test. To adjust for potential noise in the three variables, we normalize each by its standard deviation before combining

[8] See the *Wall Street Journal* article from June 26, 2014 entitled "Some companies alter the bonus playbook" for instances of firms using non-generally accepted accounting principles (GAAP) measures to design executive compensation.

them into a single variable, *Actual less target*. The mean and median value of *Actual less target* are very close to zero.

Actual less threshold EPS is the difference between the reported EPS (from Compustat) and the goal identified as the threshold EPS in a grant to an executive of the firm. We construct this in the same manner as we construct *Actual less target EPS* and continue to use the particular EPS measure (from Compustat) that we use in our construction of *Actual less target EPS.* Actual firm performance is, on average, greater than the threshold performance. Both the mean and median values of *Actual less threshold EPS* are positive. Along similar lines, we find that firms tend to exceed the threshold sales and profits by a larger margin as seen from the mean and median values of *Actual less threshold sales* and *Actual less threshold profit.* We have information about threshold performance for fewer grants because not all disclosures mention a threshold performance. On the other hand, for the purpose of calculating a fair value, all performance-linked grants have disclosed target performance.

We acknowledge a potential issue with our variables that compare actual performance to target and threshold performance. To the extent firms make *non-GAAP* adjustments to accounting data in designing grants, comparing performance goals to GAAP performance is likely to introduce noise in our estimate of how far actual performance is relative to the goal. We do a number of robustness tests to ensure that noise from these sources does not bias our conclusions. For example, we repeat our analysis with alternate measures of performance, such as undiluted EPS instead of the EPS that is closest to the goal to ensure that our conclusions are robust.[9] We next discuss the empirical tests of our hypothesis.

6. Empirical tests

6.1. Full sample analysis

In Panel A of Fig. 1, we plot the histogram of *Actual less target*, along with a smooth density. The bin width for this histogram is 0.029, the default suggested by the "DCdensity" procedure in STATA. The histogram is bunched around zero with a larger number of observations to the right of zero as compared to the number to the left. *Actual less target* appears to be left skewed and because of this, the smooth density estimated by STATA has a mode to the left of zero. Panel B is the output from the "DCdensity" function in STATA with the default bin width. The figure plots the density of *Actual less target*. The x-axis represents the difference between the outcome (actual) performance and the target (goal). The vertical line represents the goal, and thus dots to the right of the goal represent CEOs who surpassed the goal, while those to the left represent those who failed to meet their goal. The dark solid line represents the fitted density function while the thinner lines represent a 95% confidence interval around the fitted density line.[10] An intersection from the left and right of the vertical line indicates a non-discontinuity while a non-intersection indicates a discontinuity. From the figure, we find evidence for a discontinuity at zero (p-value $=0.01$). A disproportionately large number of firms have reported performance that just exceeds the target performance as compared to the number of firms whose reported performance falls short of the target performance. One of the critical parameters that may affect the test results is the bin width. A small bin width will result in a noisy (and volatile) empirical density and lead to identifying discontinuities where there are none, whereas a large bin width will smooth the density and result in false negatives. We find that the discontinuity at zero for *Actual less target* is not very sensitive to the bin width. The discontinuity is present and significant when we vary the bin width from 0.01 to 0.05.

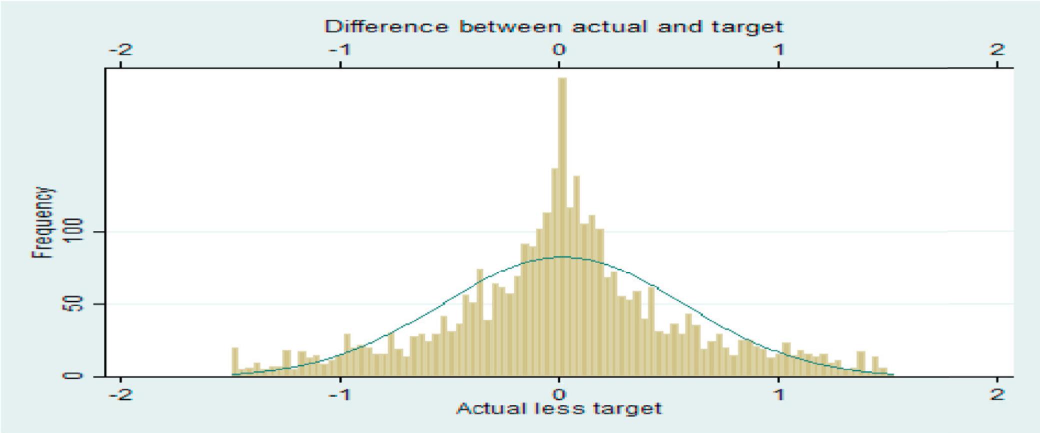
We also do a bootstrapping exercise to test if there are more observations to the right of zero as compared to the left of zero. Specifically, we draw a random sample of 50 observations of *Actual less target* and count the number of observations that lie in the first bin to the right of zero (i.e., between zero and 0.029) and the number of observations that lie in the first bin to the left of zero (i.e., between -0.029 and zero). We repeat this exercise 1,000 times and compare the means. On average, in a sample of 50, there are 0.45 more observations just to the right of zero as compared to the number of observations just to the left of zero. Statistically, this is highly significant with a t-value of 19.58.

In Panel C of Fig. 1, we present the result of a test that provides a t-statistic for the presence of a discontinuity in the density at points other than at zero. Specifically, we plot the t-statistic for the test of the difference between the actual number of observations in a bin and the number of observations that is expected based on the empirical density. The tests are similar to the ones in Bollen and Pool (2009). Similar to Bollen and Pool (2009), we pick the bin size for these tests as $0.7764 \times 1.364 \times \min\left[\sigma,\ \frac{Q}{1.34}\right] \times n^{-\frac{1}{5}}$ where σ is the empirical standard deviation, Q is the empirical interquartile range, and n is the number of observations. This results in a bin size of 0.12 for *Actual less target*. The middle, jagged line plots the t-values and the top and bottom (straight) lines identify the cutoff t-values for 99% significance. As we see, there is again a significant discontinuity at zero. The t-values are significantly large to the right of zero. This is consistent with the presence of a disproportionately large number of observations to the right of zero.

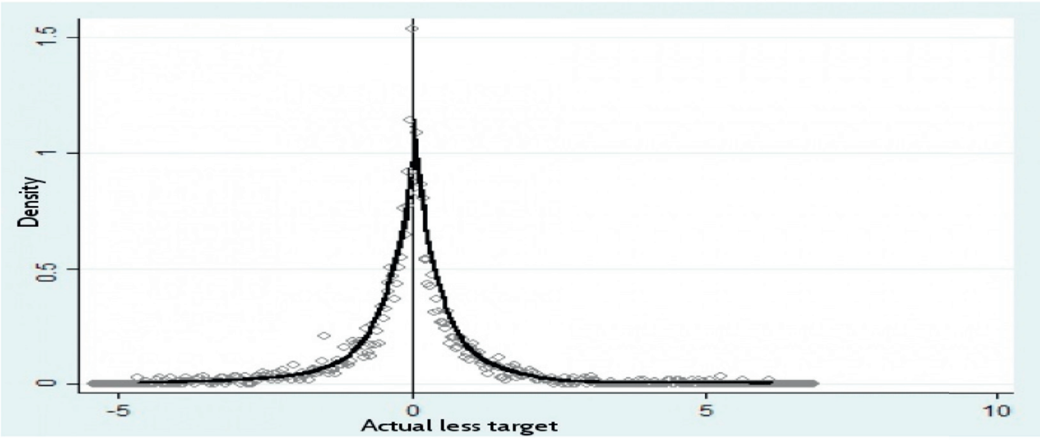
In Fig. 2, we separately test for discontinuities at zero for *Actual less target EPS, Actual less target sales*, and *Actual less target profit.* While the McCrary (2008) test shows a statistically significant discontinuity at zero only for EPS goals (p-values $=0.01$, 0.41, and 0.46 for EPS, sales, and profit, respectively), the bootstrapping exercise shows a

[9] The results of these tests are not presented to conserve space. They are available upon request.

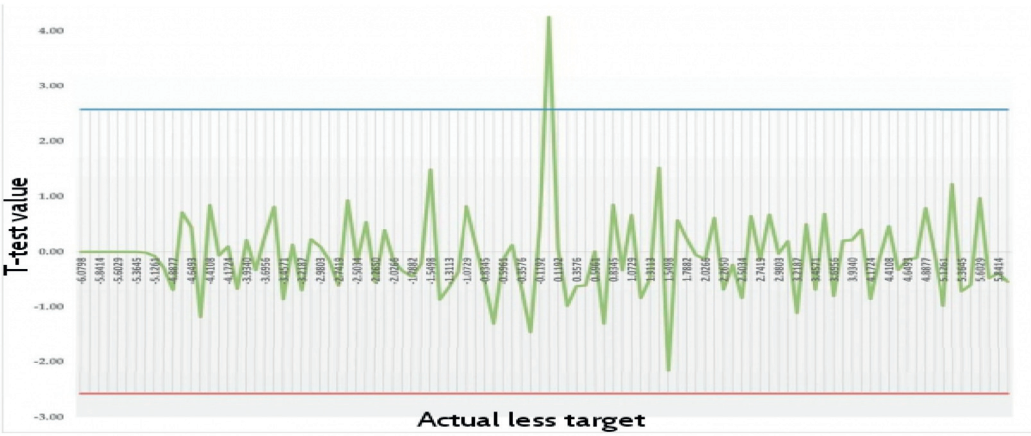
[10] The density function and the confidence interval lines are more differentiable in some of the other figures, such as Fig. 2c.



Panel A: Difference between actual and target

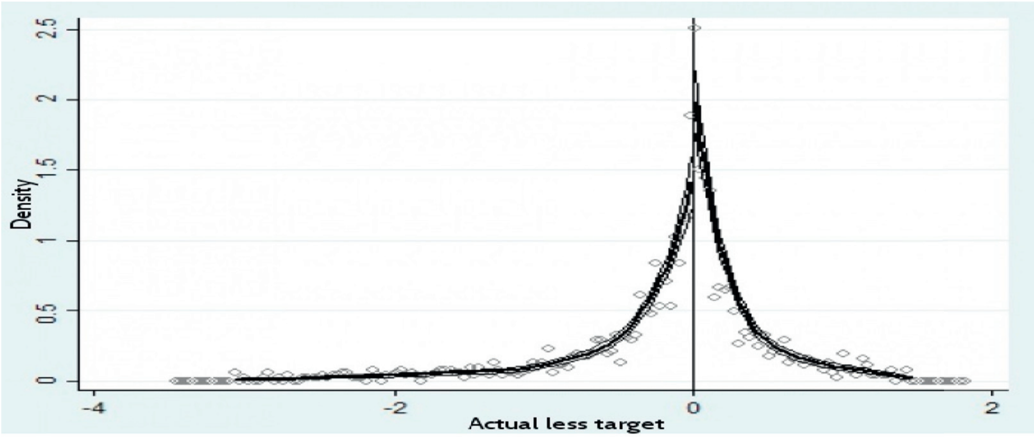


Panel B: Test of discontinuity at zero

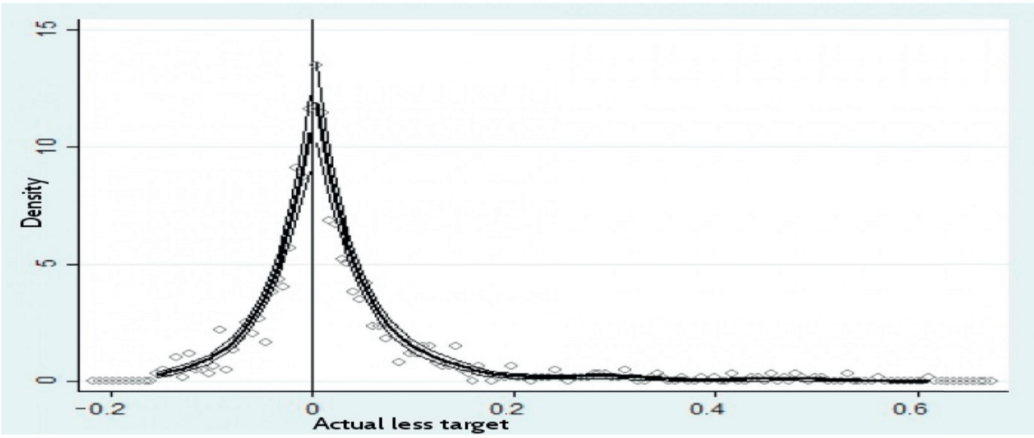


Panel C: Results of *t*-test of difference between actual and estimated density

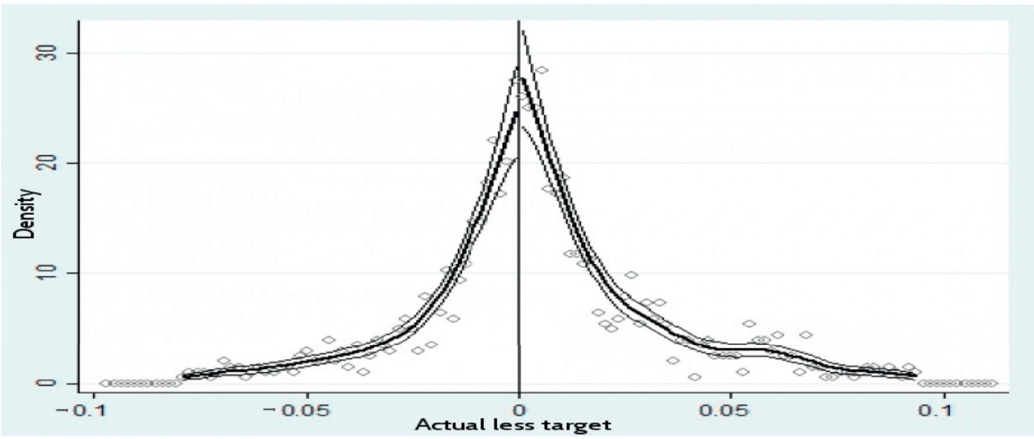
Fig. 1. Difference between actual and target: EPS/Sales/Profit. This figure presents tests for discontinuity in the density of *Actual less target* which includes the combination of the following three measures: *Actual less target – EPS, Actual less target – sales*, and *Actual less target – profit* scaled by their respective standard deviations. In Panel A we present the histogram of *Actual less target – combined* along with a smooth density. The bin width for this histogram is 0.029. Panel B presents the results of the McCrary (2008) test for the presence of a discontinuity in the empirical density at zero. The figure shows the fitted density function and 95% confidence intervals of *Actual less target*. Panel C presents the result of a test for the presence of a discontinuity in the density at points other than zero. This test is similar to the one in Bollen and Pool (2009). All variables are defined in detail in Appendix A. The data covers the period 2006–2012. The compensation data are from Incentive Lab (IL), Compustat, CRSP, and ExecuComp.



Panel A: Test of discontinuity: EPS



Panel B: Test of discontinuity: Sales



Panel C: Test of discontinuity: Profit

Fig. 2. Difference between actual and target: EPS/Sales/Profit. This figure presents tests for a discontinuity in the density of *Actual less target EPS, Sales*, and *Profits* using the McCrary (2008) test. Panels A–C present the histograms of *Actual less target EPS/Sales/Profit* along with a smooth density. The bin widths for these tests are 0.032, 0.006, and 0.002, respectively. All variables are defined in detail in Appendix A. The data covers the period 2006–2012. The compensation data are from Incentive Lab (IL), Compustat, CRSP, and ExecuComp.

significant discontinuity for *all* three metrics. Even the bootstrapping exercise shows that the discontinuity is economically larger for EPS goals as compared to profit and sales goals. Specifically, the difference in the number of observations just to the right and left of zero are 0.868, 0.309, and 0.069 for *Actual less target EPS, Actual less target sales*, and *Actual less target profit*, respectively. The corresponding t-values are: 27.59, 10.30, and 2.77. In summary, our evidence is consistent with greater performance clustering around EPS goals as compared to sales and profit goals.

A possible concern with our analysis is that firms may selectively report targets in the years in which actual performance beats the targets. To control for this, in unreported tests we repeat our analysis confining the sample to firms that report targets for a contiguous set of years. That is, we identify firms that once they begin reporting performance metrics, continue to do so until the end of the sample period. Our results are robust to confining the sample to these firms.[11]

In Fig. 3, we test for discontinuities at zero for *Actual less threshold*, which is constructed in a manner similar to *Actual less target* except that we use the threshold level of the goal instead of the target. Consistent with our target level results, we find a significant discontinuity at zero (p-value $= 0.018$). Thus, a disproportionate number of firms have performance just above the threshold as compared to the number of firms with performance just below the threshold.

6.2. Subsample analysis

In Fig. 4, we focus on *Actual less target*, and in Panels A and B our sample is divided into subsamples based on whether the grant involves a single metric or multiple metrics. We expect greater performance clustering for grants contingent on one metric because of the difficulty of "just beating" multiple metrics. Thus, we expect the size of discontinuity to be larger in Panel A as compared to in Panel B. Consistent with this, we find that the size of the discontinuity is larger in the single metric subsample (p-values $= 0.01$ for single and 0.90 for multiple).

As mentioned before, the methodology in McCrary (2008) does not allow for a statistical comparison of the size of the discontinuities. Hence, in this section we use bootstrapping to compare the size of the discontinuities. To perform a bootstrapping exercise, we draw two samples of 100 observations each from grants involving single and multiple metrics respectively. In these samples, we count the number of observations that lie just to the right of zero and the number that lies just to the left of zero. In doing this, we take care to use the same bin size as in Fig. 4. That is, we use different bin sizes for single and multiple metric-based grants. We repeat this procedure 1,000 times and compare the difference in the number of observations to the right and left of zero across single versus multiple metric-based grants. Consistent with the results in Fig. 4, we find that the discontinuity is larger for grants based on a single metric. On average, in a sample of 100, there are 0.307 more observations just to the right of zero as compared to the number just to the left of zero for single metric-based grants as compared to for multiple metric-based grants. This is highly significant statistically with a t-value of 9.44.

In Fig. 5, we perform cross-sectional tests focusing on whether the PPR at the target is concave or convex. The grant is classified by comparing the slope of the PPR to the right and left of the target value. Due to data limitations, we can only identify the nature of the slope for about 37.1% of the grants in our sample (2,153 out of 5,810 observations). Thus, the sample size is smaller for these tests. In Panels A and B of Fig. 5, we divide our sample into grants for which the PPR is concave versus grants for which the PPR is convex at the target and test for a discontinuity at zero for *Actual less target*. We find that the discontinuity at zero is present only for grants for which the PPR is concave at the target (p-values $= 0.06$ for concave and 0.11 for convex). Here again, we perform a bootstrapping exercise to statistically compare the size of the discontinuities. Our bootstrapping exercise confirms the graphical results. We find 0.37 more grants in the bin just to the right of zero relative to the bin to the left of zero for concave grants as compared to convex grants.

Finally, in Fig. 6, our sample is divided into non-equity-based grants and equity-based grants and we test for a discontinuity at zero in each of the two subsamples. As mentioned before, equity-based grants denominated in terms of number of shares introduce a convexity in the PPR. Panels A and B present *Actual less target* and find that the discontinuity at zero is present only for non-equity grants (p-values $= 0.05$ for non-equity and 0.46 for equity grants). Consistent with our graphical result, when we perform the bootstrapping exercise to statistically compare the size of the discontinuities, the discontinuity is present only for non-equity grants.

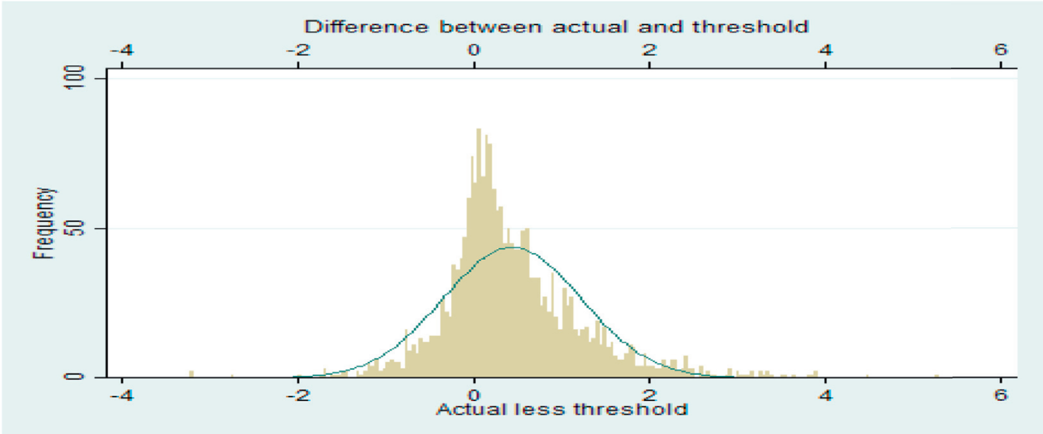
6.3. Regression analysis

To statistically compare the size of the discontinuities and also to accommodate for discontinuities at multiple points in the density, we perform a regression analysis estimating the following model:

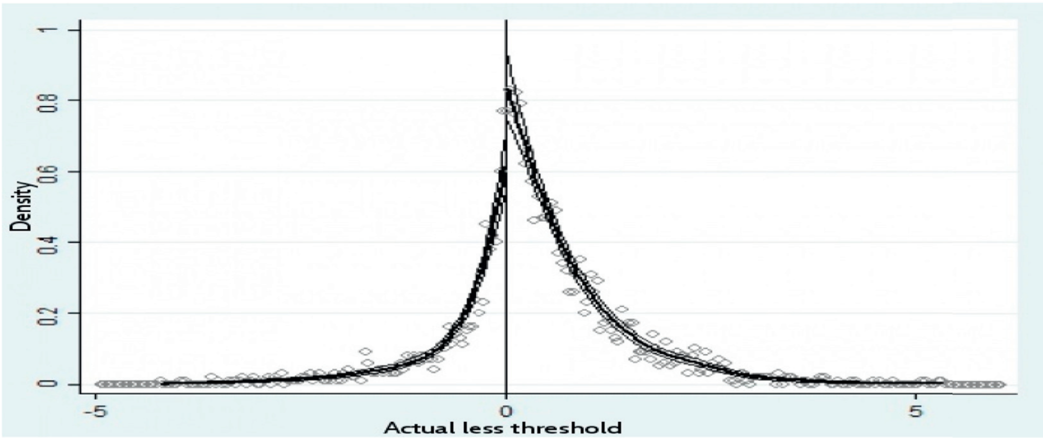
$$\begin{aligned}\text{Number of firms} = {} & \alpha + \beta_0 \text{Metric} \times \text{Mid} - \text{point} \\ & + \beta_1 \text{Metric} \times \text{Mid} - \text{point}^2 + \beta_2 \text{Metric} \\ & \times \text{Mid} - \text{point}^3 + \beta_3 \text{Metric} \times \text{Mid} - \text{point}^4 \\ & + \beta_4 \text{Number of goals} + Y \qquad (1)\end{aligned}$$

where the dependent variable, *Number of firms* is the logarithm of one plus the number of firms whose reported performance falls in a particular bin. That is, for any metric, such as say EPS, we use the bin size as recommended by McCrary (2008) and divide the firms into bins based on reported EPS. In this test, we combine the metrics so *Number of firms* also counts the number of firms whose reported sales fall within a sales-bin and the number of firms whose profit falls within a profit-bin. The bin sizes vary for the different metrics. The number of observations

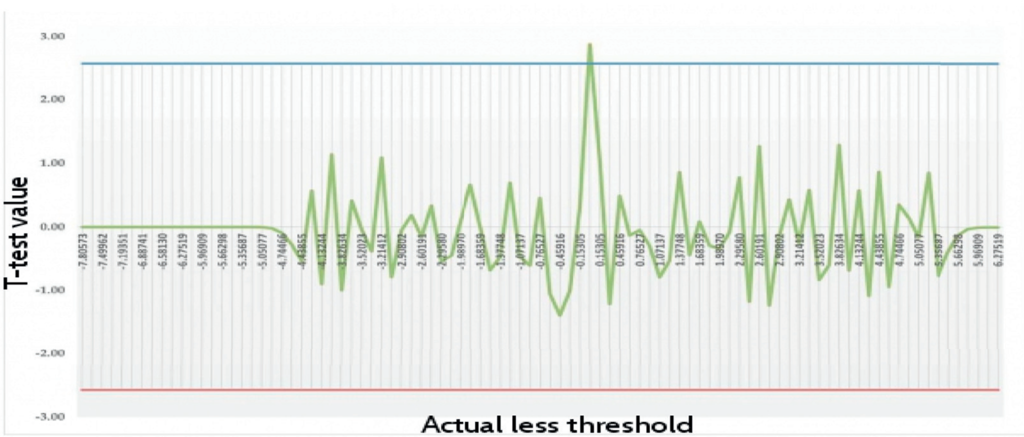
[11] Once they begin reporting performance metrics, 74.9% of firms report these metrics through the end of our sample period.



Panel A: Difference between actual and threshold

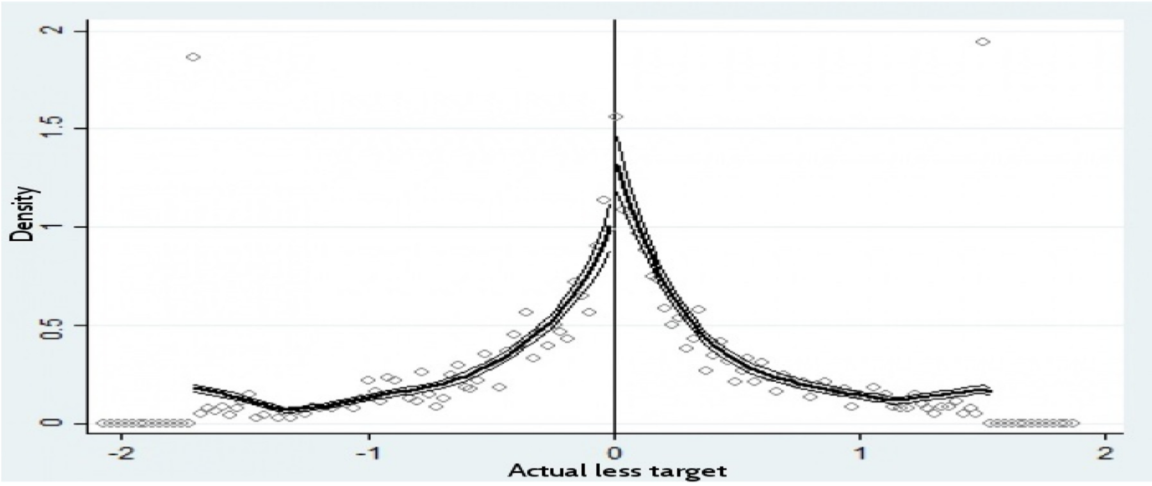


Panel B: Test of discontinuity at zero

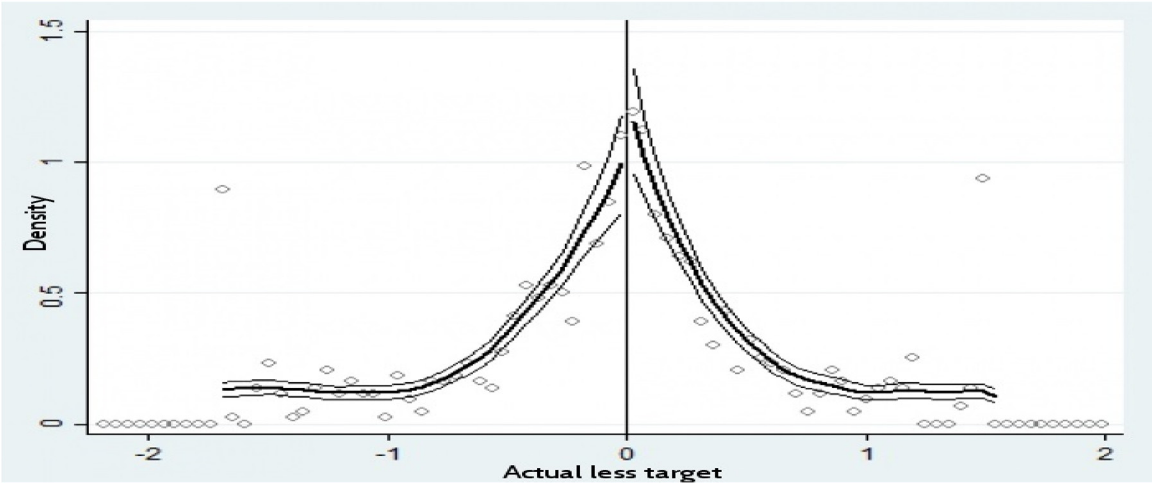


Panel C: Results of *t*-test of difference between actual and estimated density

Fig. 3. Difference between actual and threshold: EPS/Sales/Profit. This figure presents tests for discontinuity in the density of *Actual less threshold* which includes the combination of the following three measures: *Actual less threshold – EPS*, *Actual less threshold – sales*, and *Actual less threshold – profit* scaled by their respective standard deviations. In Panel A we present the histogram of *Actual less target – combined* along with a smooth density. The bin width for this histogram is 0.042. Panel B presents the results of the McCrary (2008) test for the presence of a discontinuity in the empirical density at zero. Panel C presents the result of a test for the presence of a discontinuity in the density at points other than zero. This test is similar to the one in Bollen and Pool (2009). All variables are defined in detail in Appendix A. The data covers the period 2006–2012. The compensation data are from Incentive Lab (IL), Compustat, CRSP, and ExecuComp.



Panel A: *Actual less target* – Single goal



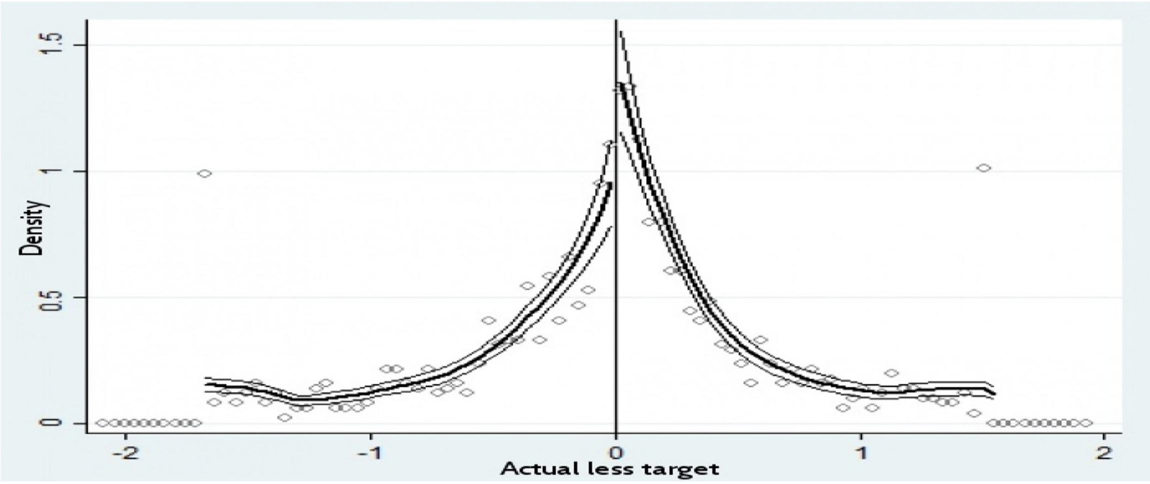
Panel B: *Actual less target* – Multiple goals

Fig. 4. Difference between actual and target performance: single vs. multiple goals. This figure tests for discontinuity in the density of variables that compare actual performance with target performance. Panels A and B present the results of the McCrary (2008) test for the presence of a discontinuity in the empirical density at zero. Panel A shows the results for grants which contain a single metric while Panel B presents the same test for grants which contain multiple metrics. All variables are defined in detail in Appendix A. The data covers the period 2006–2012. The compensation data are from Incentive Lab (IL), Compustat, CRSP, and ExecuComp.

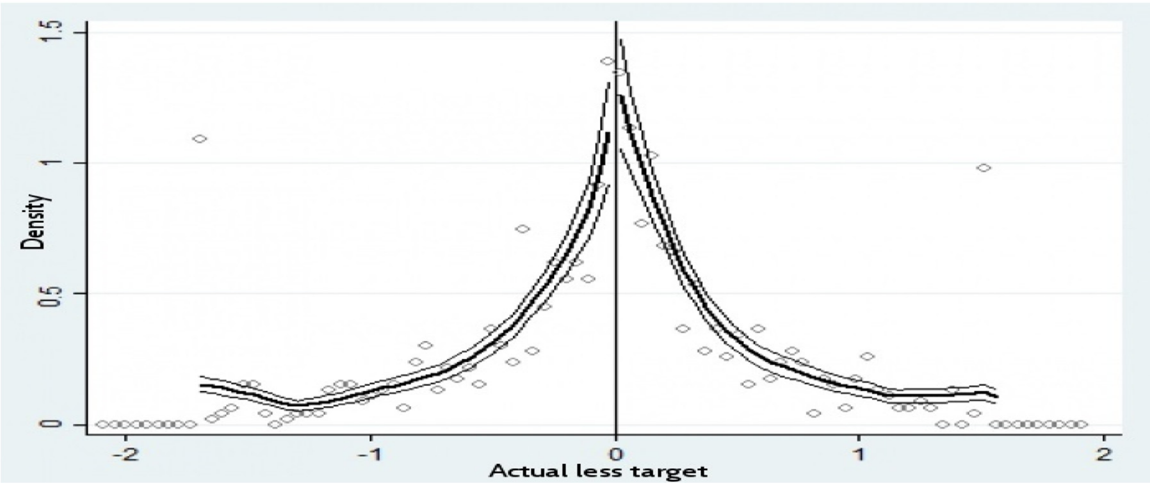
for this test for each year is the sum of the number of bins of EPS, sales, and profit. Note that the number of bins each year depends on the bin size (which is the same across years), and the maximum and minimum values of the metric.

One concern with our bin sizes is that since they are determined based on both the within- and across-firm variation in the performance metric, they may end up being too large. In other words, if the across-firm differences dominate, the bin sizes can be large, resulting in both goals and performance falling within the same bin despite not being close to each other. We think this is not a major problem. We find that our bin sizes are small both in an absolute and in a relative sense. Our bin size for earnings per share (EPS) is 4 cents and is 0.006 for sales over total assets. While these numbers are small in an absolute sense, they are also small relative to the within-firm variation in these metrics. For example, 4 cents is less than the 5th percentile of within-firm standard deviation of EPS. In other words, more than 95% of the firms in our sample have a standard deviation of EPS greater than 4 cents.

Our main independent variable is *Number of goals*, which is the logarithm of one plus the number of firms whose target or threshold performance is in a particular bin. If firms manage reported performance so as to exceed a goal, then we expect their reported performance to fall near (within the same bin as) the performance goal. This would imply a positive β_4. We model the expected number of firms in each bin in a flexible manner by including a fourth-order polynomial of the midpoint of the bin – the



Panel A: *Actual less target* – Concave

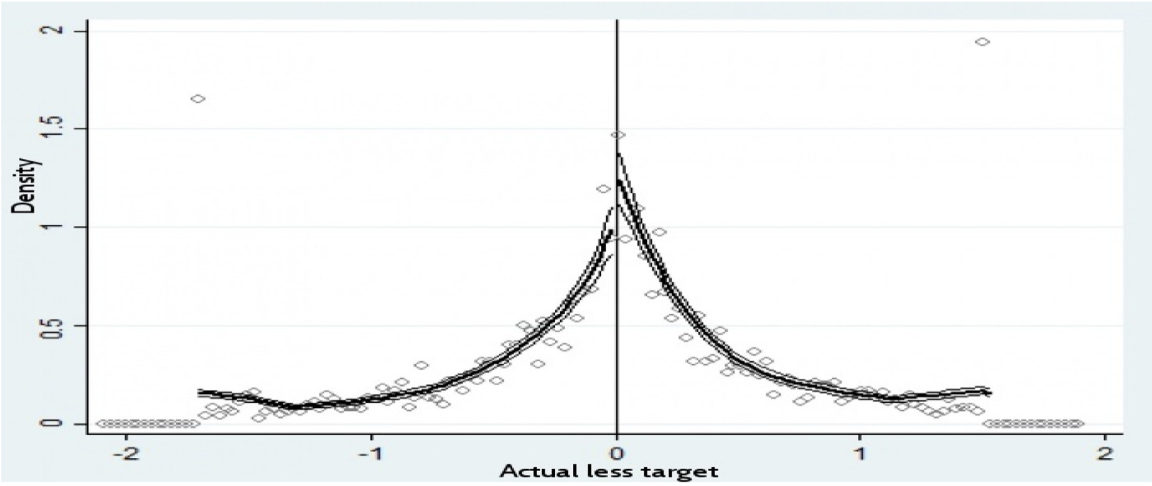


Actual less target – Convex

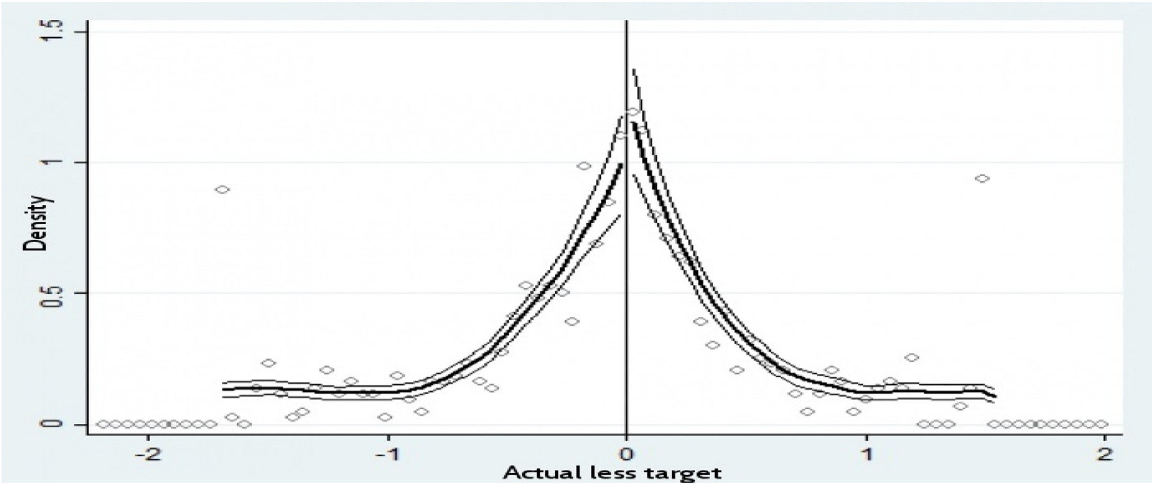
Fig. 5. Difference between actual and target performance: Concave vs. convex goals. This figure tests for discontinuity in the density of variables that compare actual performance with target performance. Panels A and B present the results of the McCrary (2008) test for the presence of a discontinuity in the empirical density at zero. Panel A shows the results for concave grants while Panel B presents the same test for convex grants. All variables are defined in detail in Appendix A. The data covers the period 2006–2012. The compensation data are from Incentive Lab (IL), Compustat, CRSP, and ExecuComp.

first four terms in the above model. We allow this model to vary across the earnings, sales, and profit metric groups by including an interaction term between *Metric,* a set of dummy variables that identify the metric group and the fourth-order polynomial in *Mid-point*. In this specification, we also include year fixed effects to control for time-series effects and cluster the standard errors at the bin level.

Note that the spirit of the test in (1) is similar to the graphical test in that it statistically compares the number of firms whose actual performance falls near the goal to some expected number. As compared to the graphical analysis, the regression approach has three advantages and two disadvantages. The first advantage is that although we combine all the metrics in the same test, we can estimate a metric-specific distribution within the same model by including an interaction term between metric fixed effects and the fourth-order polynomial. Second, the regression allows us to test for discontinuities at multiple points in the density. We can include both the threshold and target goals to construct *Number of goals*. For example, if a firm has an EPS-based grant with a threshold EPS of 0.9 and a target EPS of 1.1, then *Number of goals* will increment in both the bins that include 0.9 and 1.1. Thus, β_4 will capture firms whose managers appear to alter reported performance to exceed either the target or the threshold value. Third, the regression also allows us to perform cross-sectional tests. To test if the discontinuity is greater in cases where the grant only depends on one metric as compared to when the grant depends on multiple metrics, we divide *Number of goals* into two variables, *Single metric* and *Multiple metrics*, and repeat our estimation. *Single metric* (*Multiple metrics*) counts the number of firms that offer



Panel A: *Actual less target* – Non-equity payouts



Panel B: *Actual less target* – Equity payouts

Fig. 6. Difference between actual and target performance: non-equity vs. equity payouts. This figure tests for discontinuity in the density of variables that compare actual performance with target performance. Panels A and B present the results of the McCrary (2008) test for the presence of a discontinuity in the empirical density at zero. Panel A shows the results for grants which have non-equity payouts while Panel B presents the same test for grants which have equity payouts. All variables are defined in detail in Appendix A. The data covers the period 2006–2012. The compensation data are from Incentive Lab (IL), Compustat, CRSP, and ExecuComp.

a grant with a single (multiple) metric and whose performance goal falls within a bin. By comparing the size of the coefficient on the two variables, we can compare the marginal incentive for firms to exceed these goals.

The first disadvantage of the regression approach is that it will not be able to tell if the firm actually exceeded the goal or fell short of the goal as we only test to see if the actual performance is close to the goal. To overcome this, we rely on our prior analysis which clearly shows that whenever firm performance is close to a goal, it is more likely to be greater than the goal. The second disadvantage of the regression approach is that since we only model the total number of firms in a bin as a function of the number of goals in a bin, we will not know if the same firm has its performance and goal in the same bin. In comparison to the bootstrapping tests, which only compares the number of firms in the bins to the right and left of zero, the regression-based approach models the entire distribution. We see these two as complementing each other in helping us from our conclusions.

In Table 2, we present the results of our analysis. The positive and significant coefficient on *Number of goals* in Column 1 shows that, consistent with the graphical analysis, a disproportionate number of firms have their actual performance close to a performance goal mentioned in the pay contract. The size of the coefficient indicates that the presence of a performance goal within a bin increases the probability of an additional firm having its reported performance in that bin by 20%. Note that to the extent each grant has three goals (threshold, target, and maximum per-

Table 2
Reported performance and earnings-based pay targets.
Table 2 reports the results of an ordinary least squares (OLS) regression relating number of firms whose performance (earnings, sales, or profit) falls within a bin to the bin mid-point and the number of firms with a performance goal in the same bin. Standard errors reported in parentheses are robust to heteroskedasticity and are clustered at the bin level. All variables are defined in detail in Appendix A. The data cover the period 2006–2012. The compensation data are from Incentive Lab (IL), Compustat, CRSP, and ExecuComp. (***); (**); (*) denote statistical significance at 1%, 5%, and 10%, levels respectively.

	Number of firms (1)	Number of firms (2)	Number of firms (3)	Number of firms (4)
Number of goals	0.203**			
	(0.082)			
Single metric		0.142**		
		(0.061)		
Multiple metrics		0.212**		
		(0.108)		
Concave			0.275**	
			(0.122)	
Convex			0.114	
			(0.092)	
Non-equity				0.146*
				(0.078)
Equity				0.478***
				(0.102)
Const.	2.829***	2.917***	2.875***	2.862***
	(0.104)	(0.104)	(0.102)	(0.102)
Obs.	11,576	11,576	11,576	11,576
R-squared	0.668	0.676	0.637	0.674

formance) and actual firm performance can only fall close to one goal, even if the performance of all firms fell close to a performance goal, the coefficient is likely to be only 0.33. Against this benchmark, a coefficient of 0.20 indicates significant clustering in firm performance. Also, while we include all the control variables mentioned in (1), for brevity we do not report their coefficients. The R^2 of 0.67 highlights that the fourth-order polynomial does a reasonable job of fitting the empirical density.

In Column 2, we repeat our tests after splitting *Number of goals* into two variables, *Single metric* and *Multiple metrics*. We find that both variables are positive and significant, however, the coefficient on *Single metric* is smaller than the coefficient on *Multiple metrics*. In Column 3 we include two variables, *Concave* and *Convex*, and repeat our tests. *Concave* (*Convex*) counts the number of firms that offer a grant that involves a concave (convex) kink at the target value and whose performance goal falls within a bin. The results in Column 3 show that only the coefficient on *Concave* is significant. This is again consistent with our hypothesis and graphical tests.

Column 4 shows grants with equity and non-equity payouts. There is a significant clustering around goals for both grants that involve equity and non-equity payouts. In additional robustness tests, instead of a fourth-order polynomial in *Mid-point*, we include bin fixed effects and repeat our tests. Our results are robust to this alternate specification.

6.4. Relative performance-based awards

In Fig. 7, we focus on relative performance-based awards to test whether firms with relative performance awards have a tendency to just meet these targets as compared to just miss them. Not only do these tests inform us about firms' tendencies to beat relative performance goals, but also serve as an additional (falsification) test of our hypothesis. If firms beat performance goals by managing reported performance, then that tendency should be less prevalent for grants tied to relative performance as it is impossible to manage performance relative to a peer group whose outcome is unknown until after the performance period ends.[12] To see if this is the case, in Fig. 7, the relative performance targets are compared to the firm's actual relative-performance. Relative performance-based awards typically specify the target performance in terms of a relative rank or a percentile with respect to the peer group performance. We convert the targets into ranks and compare them to the firm's actual rank. Panel A of Fig. 7 plots the histogram of the difference between the actual rank and the target rank. Since ranks typically take on integer values, the bin size for this histogram is one and we confine the histogram to values between −20 and +20.

As can be seen, there is no tendency for firms to just beat their performance target. There are more firms that just miss the target as compared to firms that just meet the target. In Panel B, we perform the test in McCrary (2008) to test for a discontinuity at zero and do not find any statistically significant discontinuity at zero. Here again, the bin size is one. Thus, when performance benchmarks are based on relative performance, firms do not have a tendency to just meet the target. On the other hand, our prior evidence indicates that when targets are given in terms of absolute performance, firms do have a greater tendency to just meet the target as compared to just miss it. In conjunction, these two pieces of evidence are consistent with firms managing reported performance to meet the performance targets.

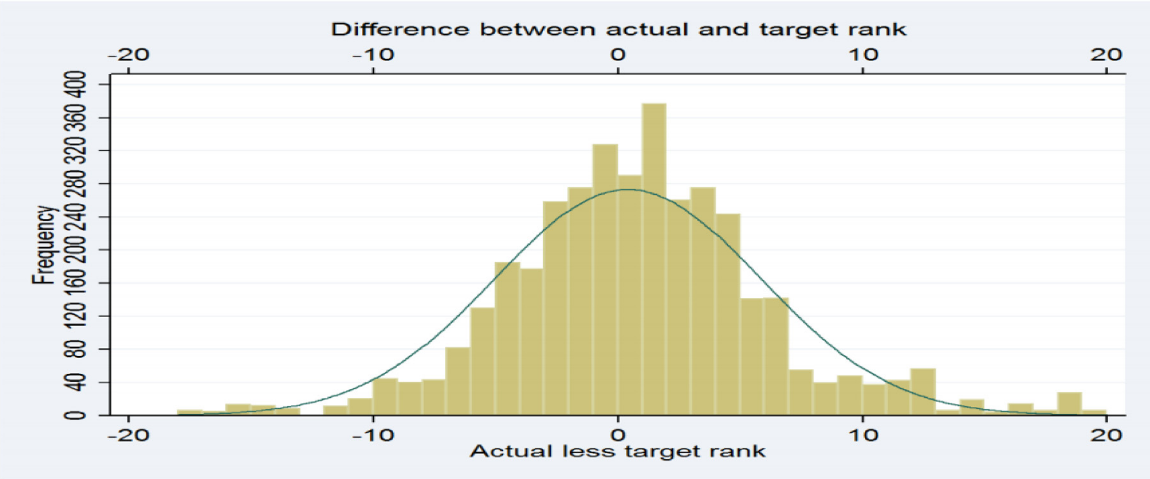
6.5. Why does performance cluster around the target?

In this section, we perform two sets of tests to better understand the reasons why firm performance clusters at the target value even in the absence of a jump in pay.

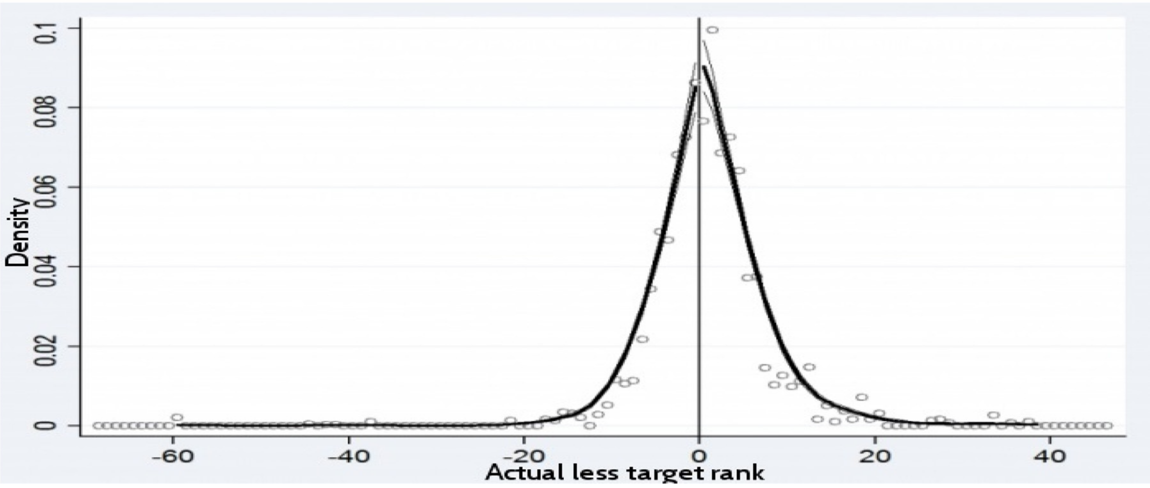
6.5.1. Target ratcheting

To test the predictions of the "target ratcheting" effect, in Panel A of Table 3 we first relate performance targets to past performance. Our sample includes one observation per metric-firm-year. We include within-industry time effects in all regressions. We define industry at the level of two-digit Standard Industrial Classification (SIC) code. Not surprisingly, we find a strong and positive relationship between targets and past performance. This relationship is present for all three metric groups (Columns 2–7), and is robust to the inclusion of firm and time fixed effects

[12] The peer group performance is not known until all the peers' audited financials are disclosed, roughly 2–3 months after the close of the fiscal year.



Panel A: Histogram of difference between actual and target rank



Panel B: Test of discontinuity at zero of the difference between actual and target rank

Fig. 7. Difference between actual and target ranks for relative performance grants. This figure tests for discontinuity in the density of Actual less target rank. In Panel A we present the histogram of Actual less target rank along with a smooth density. The bin width for this histogram is 1. Panel B presents results of the McCrary (2008) test for the presence of a discontinuity in the empirical density at zero. All variables are defined in detail in Appendix A. The data covers the period 2006–2012. The compensation data are from Incentive Lab (IL), Compustat, CRSP, and ExecuComp.

(Column 1). This result is consistent with performance influencing subsequent targets. This in turn could provide incentives for executives to understate performance in an effort to influence subsequent grants. Note that omitted variables, such as economic and industry conditions, preclude us from concluding that there is a causal link between performance and subsequent targets.

In Panel B of Table 3, we follow Bouwens and Kroos (2011) and test to see if firms whose performance clusters close to the target in one period are more likely to meet their target the next period. To ensure that outliers do not bias our estimates, we exclude firms with estimates of *Actual less target EPS*, *Actual less target sales*, and *Actual less target profit* beyond the 5th and 95th percentiles. To avoid a mechanical correlation between the targets from one period to another, we confine the grants to annual grants. Our dependent variable is a dummy variable that identifies firms that meet or beat the target this period, while the main independent variable, *Exceed target*, is a dummy variable that identifies firms that just exceed the target in the previous period (i.e., whose performance falls in the first two bins to the right of the target). Thus, *Exceed target* takes a value of zero both for firms whose prior period performance exceeds the target by a large amount and for firms whose performance falls short of the target. Apart from the control variables shown in the table, we also include within-industry time effects in this regression. We define industry at the level of three-digit SIC code. The positive and significant coefficient on *Exceed target* in Columns 2–4 show that firms are more likely to meet their target if they just beat the target the previous period. Our results are economically significant. The results in

Table 3
Evidence for target ratcheting effect.
Panel A reports the results of multivariate tests for correlation in performance targets. The dependent variable *EPS/Sales/Profit goal*, is equal to the EPS/Sales/Profit goal in the current period. The main independent variable is Lag EPS/Sales/Profit and is equal to the previous period's firm EPS/Sales/Profit. Panel B reports the results of multivariate tests that relate the probability of a firm meeting its performance target to the performance relative to target the previous year. The dependent variable is Meet target, a dummy variable that identifies firms that meet their performance target in the next period. The main independent variable is *Exceed target* (CEOs who meet the performance target in the current period). This variable takes a value one for firms whose performance is in the two bins just above the performance goal and zero otherwise. Details on the definition of the variables in this table are provided in Appendix A. Standard errors reported in parentheses are robust to heteroskedasticity and are clustered at the two-digit SIC industry level, except in Specification 1 of Panel A, which includes firm fixed effects and standard errors clustered at the firm level. The data cover the period 2006–2012. The compensation data are from Incentive Lab (IL), Compustat, CRSP, and ExecuComp. (***); (**); (*) denote statistical significance at 1%, 5%, and 10%, levels respectively.

Panel A: Previous performance and current goal levels

Goal	All (1)	EPS (2)	EPS (3)	Sales (4)	Sales (5)	Profit (6)	Profit (7)
Lag Performance	0.483***						
	(0.17)						
Lag EPS		1.08**	.728*				
		(0.05)	(0.38)				
Lag Sale				.761***	.838***		
				(0.17)	(0.19)		
Lag Profits						0.440***	0.239*
						(0.13)	(0.12)
Size			.242**		−1.576*		4.202***
			(0.97)		(0.92)		(0.79)
Constant	3.601	−0.616	−2.16**	−0.187	0.111	.514***	−.304***
	(2.98)	(1.13)	(0.92)	(1.43)	(0.69)	(0.07)	(0.07)
Observations	2,852	1,289	1,289	505	505	1,058	1,058
R-squared	0.681	0.35	0.384	0.746	0.75	0.435	0.491

Panel B: Performance goals and actual performance

	Meet Target (1)	Meet Target (2)	Meet Target (3)	Meet Target (4)
Exceed target	0.095**	0.088**	0.108**	0.105**
	(0.045)	(0.044)	(0.053)	(0.052)
Size		−0.002		−0.01
		(0.013)		(0.014)
ROA		0.650***		0.378
		(0.237)		(0.277)
Sales growth		0.443***		0.310**
		(0.121)		(0.137)
Const.	0.543***	0.460***	0.558***	0.585***
	(0.013)	(0.120)	(0.015)	(0.132)
Obs.	1,954	1,954	1,395	1,395
R-squared	0.21	0.227	0.211	0.219

Column 2 indicate that firms whose performance just exceeds the target are 8.8% more likely to meet their target the next period. In Columns 3 and 4, tests are repeated after confining the sample to firms with *Actual less target EPS*, *Actual less target sales*, and *Actual less target profit* within the 10th and 90th percentiles and we obtain consistent results.

6.5.2. CEO turnover

To test if boards evaluate managerial performance relative to the target, in Table 4 we relate the probability of a forced CEO turnover to the firm not meeting its performance target the previous period. Following Parrino (1997), all turnovers for which the press reports that the CEO is fired, is forced out, or departs due to difference of opinion or unspecified policy differences with the Board are classified as forced. Of the remaining turnovers, if the departing CEO is under age 60, it is classified as forced if either (1) the reported reason for the departure does not involve death, poor health, or acceptance of another position elsewhere or within the firm (including the chairmanship of the board), or (2) the CEO is reported to be retiring but there is no announcement about the retirement made at least two months prior to the departure. All the CEO turnovers not classified as forced or due to mandatory or planned retirements are classified as voluntary.

The sample for these tests include one observation per metric-firm-year. The main dependent variable is *Forced*, a dummy variable that identifies the years in which a firm experiences a forced CEO turnover. There are a total of 31 forced CEO turnovers in our sample. Our main independent variable is *Miss target*, a dummy variable that identifies firms that fail to meet the target performance. We also include a set of control variables that prior literature has identified as being correlated with the likelihood of a forced CEO turnover. These include *Industry*

Table 4
Performance Goals and CEO turnover.
This table reports the results of multivariate tests that relate the likelihood of forced CEO turnover to firm performance. The dependent variable is *Forced*, a dummy variable that identifies firms that experience a forced CEO turnover during the year. We identify forced CEO turnovers following the procedure first used in [parrino-97]. The main independent variable is *Miss target*, a dummy variable that identifies firms that miss their performance targets during the previous year. Details on the definition of the variables in this table are provided in the Appendix A. Standard errors reported in parentheses are robust to heteroskedasticity and are clustered at the two-digit SIC industry level. The data covers the period 2006–2012. The compensation data are from Incentive Lab (IL), Compustat, CRSP, and ExecuComp. (***); (**); (*) denote statistical significance at 1%, 5%, and 10%, levels respectively.

	Forced (1)	Forced (2)
Miss target	0.015*	0.015*
	(0.008)	(0.008)
Target	0.009	0.011
	(0.007)	(0.007)
Target X Target	0.002	0.002
	(0.004)	(0.004)
Industry return	−0.015**	−0.028**
	(0.007)	(0.013)
Return		0.012
		(0.014)
Size		0.009**
		(0.004)
Volatility		0.205**
		(0.080)
Const.	−0.0005	−0.101**
	(0.007)	(0.046)
Obs.	1,874	1,874
R-squared	0.354	0.360

ret., Return, Size, Volatility, Tenure, Age, CEO shareholding, and *Duality*.[13] Along with these, we also include a second-order polynomial of the target. Thus, what we are interested in is, controlling for firm performance (in terms of stock returns) and the performance target, does missing the performance target incrementally increase the odds of a forced turnover? The specification also includes within-industry time effects and the standard errors are clustered at the level of three-digit SIC code industry.

The positive and significant coefficient on *Miss target* indicates that CEOs who fail to meet the performance target in a year are more likely to experience a forced turnover in the next year. Our results are economically significant. The coefficient in Column 1 indicates a 1.5% increase in the annual probability of a forced turnover for failing to meet the target. For perspective, this 1.5% increase more than doubles the unconditional probability that a CEO will experience a forced turnover in our sample. In Column 2, we repeat our tests after including additional control variables and obtain consistent results. These results highlight a reason why managers may want to extend themselves to meet the performance target. A third explanation for why performance clusters around the target could be because of reference-based preferences of the CEO. While there is anecdotal evidence about the extent to which CEOs are risk averse (see Graham et al., 2013), these say nothing about the extent to which they have reference-based preferences. Hence, in the absence of proxies for managerial preference, it is very difficult to test this explanation. We interpret this as the residual that can be used to explain performance clustering that cannot be explained by other reasons.

6.6. How do firms exceed performance goals?

In our next set of tests, we compare firms that just exceed a manager's compensation goal and those that just miss a goal on a number of dimensions to understand how firms ultimately exceed performance goals in practice. These tests help us understand the extent to which firms manage accruals and discretionary expenditures to manage reported performance. Depending on the metric involved, managers can employ a variety of techniques to meet a performance goal. In the case of EPS goals, managers can use abnormal accruals, cut discretionary expenditures such as R&D and SG&A, as well as repurchase shares to meet the goal. Similarly, managers can meet their sales goals by increasing SG&A and accounts receivables. In these tests, we compare firms that just exceed their goal, that is, the firms that fall in the first bin above the performance goal (either target or threshold) and the firms that just miss their goal, that is, firms whose performance is in the two bins below the performance goal. We include two bins to the left of the performance goal because there are very few firms in the bin just below the performance goal. We separately look at EPS, sales and profit goals because the samples of firms that exceed and miss the goals are different. In Table 5, firms that exceed are compared with those that miss their performance goal.[14]

In Panel A, we focus on EPS goals. Firms that exceed the EPS goal are very similar to firms that miss their EPS goal on most observable characteristics. The two significant differences between the two sets of firms are that firms that exceed their EPS goal repurchase less shares and have smaller changes in R&D expenditures. The first result is rather surprising because if one expects firms to strategically repurchase stock to meet EPS goals, then one would expect to find greater share repurchases among firms that just beat their EPS goals. In the second panel, we compare firms that just exceed and just miss their sales goal. Apart from a higher sales growth rate for the former set of firms, we do not find any other significant difference between the two sets of firms. Finally, in the last panel we focus on profit goals and find that firms that exceed their profit goals are larger, have lower market-to-book ratios, lower sales growth, and smaller changes in SG&A as compared to firms that miss their profit goals. The smaller change in SG&A for the firms that just exceed their profit goals as compared to firms that miss their profit goals is consistent with Roychowdhury (2006) and Dechow et al. (2003), who find that firms often decrease discretionary spending in an effort to increase short term earnings.

In Table 6, we perform multivariate tests that compare firms that exceed and miss their performance goals by estimating variants of the following model:

[13] All the variables used are defined in Appendix A.

[14] Definitions of all the variables we compare in this table are provided in Appendix A.

Table 5
Univariate comparison of firms that exceed and miss performance goals.

This table compares the mean values of the key variables across the subsamples of firms that just exceed and just miss their performance goals. Performance metrics investigated are EPS, sales, and profit. The data cover the period 2006–2012. The compensation data are from Incentive Lab (IL), Compustat, CRSP, and ExecuComp. (***); (**); (*) denote statistical significance at 1%, 5%, and 10%, levels respectively. See Appendix A for variable definitions.

	Exceed EPS goal		Miss EPS goal		
	N	Mean	*N*	Mean	Difference
Size	239	8.747	265	8.799	−0.052
ROA	239	0.11	265	0.111	−0.001
Market to book	239	1.811	265	1.809	0.002
Leverage	238	0.253	265	0.25	0.003
Sales growth	237	0.04	265	0.054	−0.014
Accruals	201	0.012	225	0.009	0.003
Repurchase	239	13.703	265	18.709	−5.006*
ΔR&D	239	0.743	265	1.839	−1.096**
ΔSG&A	239	9.523	265	9.249	0.274

	Exceed EPS goal		Miss EPS goal		
	N	Mean	*N*	Mean	Difference
Size	171	8.996	231	9.092	−0.096
ROA	171	0.095	231	0.103	−0.008
Market to book	166	1.8	225	1.847	−0.047
Leverage	168	0.251	229	0.222	0.029
Sales growth	171	0.082	231	0.05	0.032**
Accruals	148	0.008	192	0.009	−0.001
Repurchase	171	16.237	231	16.613	−0.376
ΔR&D	171	2.906	231	1.766	1.14
ΔSG&A	171	11.88	231	11.026	0.854

	Exceed EPS goal		Miss EPS goal		
	N	Mean	*N*	Mean	Difference
Size	129	9.004	228	8.728	0.276*
ROA	129	0.081	228	0.084	−0.003
Market to book	126	1.617	222	1.636	−0.019*
Leverage	129	0.287	228	0.273	0.014
Sales growth	128	0.019	228	0.052	−0.033**
Accruals	104	0.007	186	0.002	0.005
Repurchase	129	12.632	228	12.772	−0.14
ΔR&D	129	0.076	228	0.674	−0.598
ΔSG&A	129	−0.129	228	5.889	−6.018**

$$y_i = \alpha + \beta_0 \text{ x Exceed EPS/Sales/Profit} + \beta_1 \text{ x Size} + \beta_2 \text{ x Market to book} + Y + \gamma_j + \varepsilon_i \quad (2)$$

where the dependent variable is one of *Accruals, R&D/TA, SG&A/Sales,* or *Repurchase*. The main independent variable is one of *Exceed EPS, Exceed sales*, or *Exceed profit*. These variables take a value of one for firms whose performance is in the bin just above the performance goal, and zero for firms whose performance is in the two bins below the performance goal. In all the regressions, we control for firm size (*Size*) and *Market to book*. In addition, for the regressions with *Accruals* as the dependent variable, we also include the standard deviation of sales growth and standard deviation of profitability as additional controls. All the regressions include year and industry fixed effects, the latter at the two-digit SIC code level, and the standard errors are clustered at the firm level. Since managers at firms are typically involved in selecting performance goals and may take deliberate actions to meet those goals, firms that meet and miss goals are not likely to be randomly selected. To this extent, our evidence should not be interpreted as being causal in nature. On the other hand, our univariate evidence did not indicate systematic differences between the two sets of firms on observable characteristics. Although changes in accruals and discretionary expenditure may allow firms to meet the goals, our tests are designed to capture the fact that firms that want to beat performance goals change the discretionary expenditure and accruals. Hence, we model these as the outcome variable and a dummy variable that identifies if firms meet or miss target as the independent variable.

Table 6
Multivariate difference between firms that exceed and miss performance goals.

This table reports the results of multivariate tests that compare firms that exceed and miss their performance goals. The dependent variables are *Accruals, ΔR&D, ΔSG&A*. The main independent variables are *Exceed EPS* and *Exceed profit*. These variables take a value one for firms whose performance is in the bin just above the performance goal and zero for firms whose performance is in the two bins below the performance goal. Details on the definition of the variables in this table are provided in Appendix A. Standard errors reported in parentheses are robust to heteroskedasticity and are clustered at the two-digit SIC industry level. The data cover the period 2006–2012. The compensation data are from Incentive Lab (IL), Compustat, CRSP, and ExecuComp. (***); (**); (*) denote statistical significance at 1%, 5%, and 10%, levels respectively.

	Accruals (1)	ΔR&D (2)	ΔSG&A (3)
Exceed EPS	0.009**	−1.009*	
	(0.004)	(0.590)	
Exceed profit			−6.22**
			(3.016)
Size	0.002	−0.046	−0.549
	(0.002)	(0.233)	(0.879)
Market to book	0.0009	2.637**	11.762***
	(0.002)	(1.090)	(2.177)
Std. dev. cash flow	−0.018		
	(0.044)		
Std. dev. sales growth	−8.44E−07		
	(9.97E−07)		
Const.	-0.016	−2.81	2.606
	(0.021)	(3.380)	(9.782)
Obs.	904	1,111	1,111
R-squared	0.139	0.068	0.231

While we estimate a full set of regressions with all combinations of independent variables (*Exceed EPS, Exceed sales*, and *Exceed profit*) and dependent variables, to conserve space, Table 6 has the results of the tests in which the coefficient on the main independent variable is significant. From Column 1, the coefficient on *Exceed EPS* is positive and significant. This indicates that firms that exceed EPS goals have higher abnormal accruals as compared to firms that miss EPS goals. We also find that firms that exceed EPS goals have lower changes in R&D expenditures. This is consistent with such firms lowering R&D expenditures more than firms that miss EPS goals. From Column 3, we find that firms that meet profit goals reduce SG&A expenses more than firms that just miss their profit goal. In summary, the evidence in Table 6 offers evidence consistent with managers using accruals and discretionary expenses to meet their incentive compensation EPS goals, and reducing discretionary expenditure to meet their profit goals.[15]

7.. Conclusion

We use a comprehensive data set containing information on the performance goals employed in equity-based and non-equity-based grants awarded by 974 firms to investigate the extent to which executives manage reported performance to meet compensation goals. Meeting compensation goals may be important either if there is a jump in pay when performance exceeds the goal or if not meeting the goal imposes penalties such as being fired. Executives may also not want to exceed goals by a large amount if either the PPR is concave at the goal or if better performance results in higher subsequent targets. We explore the validity of these hypotheses by testing for discontinuities in reported performance around the goals (McCrary, 2008).

We find evidence consistent with executives managing reported accounting performance to achieve compensation goals. A disproportionately large number of firms just exceed the goals as compared to the number of firms that just fail to meet the goals. This effect is present for earnings-based goals, and is stronger among executives who receive grants contingent on a single metric as opposed to grants contingent on multiple metrics. This effect is strong for grants with a concave kink at the target and those with non-equity payouts. We do not find a corresponding tendency for firms to beat relative performance goals. Consistent with firms understating performance to influence subsequent targets, firms that just beat performance targets are more likely to meet their subsequent targets. CEOs of firms that fail to meet performance targets are more likely to experience forced turnover. Firms that just exceed their EPS goal have higher abnormal accruals and lower R&D expenditures as compared to firms that just miss their EPS goal. Firms that just exceed their profit goal have lower SG&A expenses as compared to firms that miss their goal.

In their ongoing effort to achieve an optimal link between pay and performance, firms have increasingly resorted to linking equity and non-equity compensation to achieving explicit performance goals (Bettis et al., 2013). Our paper highlights some important costs of awarding performance-contingent grants that focus on particular accounting targets, because they may provide perverse incentives for management to "just beat" the target.

There are a number of take-aways from our study that can be used to minimize distortions from explicitly linking pay to performance. First, distortions can be avoided if firms do not identify specific performance targets. Instead firms can provide an explicit (and possibly smooth) link between performance, along any preferred dimension, and pay. If firms need to identify a specific target, say for expensing purposes, our study highlights the ways to minimize distortions. Our results indicate less performance management if goals are specified either relative to other firms or in terms of sales or profit targets. On the other hand, there appears to be greater performance management in the case of EPS-based goals. We also find that grants that involve a concavity in the PPR and grants that use cash payouts are more likely to result in performance management by CEOs. We believe that compensation committees and consultants should consider structuring performance-pay contracts that are relative versus absolute to minimize performance management.

[15] In unreported tests, we use Exceed EPS, Exceed sales and Exceed profit as the dependent variables, which are influenced by the independent variables: Accruals, ΔR&D/TA, ΔSG&A/Sales, or Repurchase. Our results are very similar, and we find a significant relationships between: (i) Exceed profit and Repurchase, (ii) Exceed profit and ΔR&D/TA, and (iii) Exceed EPS and ΔSG&A/Sales.

Appendix A. Variable definitions

The variables used in the empirical analysis are defined as follows:

- *Accruals* is signed abnormal accruals. We calculate this measure following the procedure outlined in Jones (1991).
- *Actual less target/threshold EPS* is the difference between actual EPS as reported in Compustat and the target/threshold EPS as identified in the compensation contract.
- *Actual less target/threshold profit* is the difference between the actual profit and the target or threshold profit mentioned in the compensation contract normalized by the book value of total assets.
- *Actual less target/threshold sales* is the difference between the actual sales and the target or threshold sales mentioned in the compensation contract normalized by the book value of total assets.
- *Actual less target* is either *Actual less target EPS* or *Actual less target sales* or *Actual less target profit*.
- *Actual less threshold* is either *Actual less threshold EPS* or *Actual less threshold sales* or *Actual less threshold profit*.
- *Age* is the age of the CEO.
- *CEO shareholding* is the percentage shareholding of the CEO.
- *Concave* are awards where the slope to the left of the target scaled by the slope to the right of the target is greater than 1.01.
- *Convex* are awards where the slope to the left of the target scaled by the slope to the right of the target is less than 0.99.
- *R&D* is one thousand times the year-on-year change in R&D expenditure normalized by book value of total assets.
- *SG&A* is one thousand times the year-on-year change in SG&A expenditure normalized by book value of total assets.
- *Debt/Total assets* (or *Leverage*) is the ratio of the sum of long-term and short-term debt (Compustat items: dltt and dlc) to the book value of total assets.
- *Duality* is a dummy variable that identifies firms in which the CEO is also the chairman of the board.

- *EPS goals* is one plus the natural logarithm of number of firms whose earnings goals (EPS, or earnings) fall within a bin.
- *Exceed EPS/sales/profit* take a value one for firms whose performance is in the bin just above the performance goal and zero for firms whose performance is in the two bins below the performance goal.
- *Exceed target* takes a value one for firms whose current period performance is in the two bins just above the performance goal and zero for firms whose performance is elsewhere.
- *Industry ret.* is the equal-weighted return on the three-digit SIC code industry to which the firm belongs.
- *Linear* are awards where the slope to the left of the target scaled by the slope to the right of the target is between or equal to 0.99 and 1.01.
- *Market to book* is the ratio of market value of total assets to book value of total assets.
- *Meet target* is a dummy variable set to one for firms that meet or exceed the target in the next time period and zero otherwise.
- *Miss target* is a dummy variable that takes a value one for firms whose performance is less than the compensation target and zero otherwise.
- *Number of firms* is one plus the natural logarithm of number of firms whose actual performance (EPS, Sales, EBIT, EBITDA, EBT, FFO, or Operating income) falls within a bin.
- *No. of goals* is one plus the natural logarithm of number of firms whose compensation contract goals (EPS, Sales, EBIT, EBITDA, EBT, FFO, or Operating income) fall within a bin.
- *No. of goals – Single metric (Number of goals – Multiple metrics)* is one plus the natural logarithm of number of firms whose compensation contract goals (EPS, Sales, EBIT, EBITDA, EBT, FFO, or Operating Income) that are in grants involving a single (multiple) metric fall within a bin.
- *No. of goals – Concave (Number of goals – Convex)* is one plus the natural logarithm of number of firms whose compensation contract goals (EPS, Sales, EBIT, EBITDA, EBT, FFO, or Operating income) that are in grants involving a concave (convex) kink at the target value fall within a bin.
- *No. of goals – Non-Equity (Number of goals – Equity)* is one plus the natural logarithm of number of firms whose compensation contract goals (EPS, sales, EBIT, EBITDA, EBT, FFO, or Operating income) in grants involving cash (stock) payout fall within a bin.
- *Number of metrics* is the number of different metrics (such as EPS, Sales, etc.) that the particular grant is tied to.
- *Option* is a dummy variable that takes a value of one if a grant payout is in the form of stock options and zero otherwise.
- *Profit goals* is one plus the natural logarithm of number of firms whose profit goals (EBIT, EBITDA, EBT, FFO, or Operating income) fall within a bin.
- *R&D/Total assets* is the ratio of research and development expenditure over book value of total assets. We code missing values of research and development expenditure as zero.
- *Repurchase* is the percentage change in shares outstanding with respect to the previous fiscal year.
- *Return* is the one-year percentage return for the firm's stock over the previous fiscal year.
- *ROA* is return on assets calculated as the ratio of net income to total assets.
- *Sales goals* is one plus the natural logarithm of number of firms whose sales goals fall within a bin.
- *Sales growth* is the percentage change in revenue with respect to the previous fiscal year.
- *Size* is the natural logarithm of total (book) assets.
- *Spread* is the average daily stock bid-ask spread during the previous year.
- *Std. dev. cash flow* is the standard deviation of the firm's cash flow calculated over the previous five years.
- *Std. dev. sales growth* is the standard deviation of the firm's sales growth calculated over the previous five years.
- *Stock* is a dummy variable that takes a value of one if a grant payout is in the form of stock and zero otherwise.
- *Tangibility* is the ratio of tangible assets to total assets.
- *Target* is the CEO target goal level.
- *Target X Target* is the CEO target goal level squared.
- *Tenure* is the tenure of the CEO.
- *Total assets* is the book value of total assets; *Log(Total assets)* (or *Size*) is the natural logarithm of total assets.
- *Volatility* is the stock return volatility calculated as the annualized volatility of daily stock returns during the previous year.

Appendix B. Examples of performance-linked grants

Example 1. Barnes & Noble in fiscal year 2012.

This is a cash award without interpolation (see Table A.1). The proxy reads: "Set forth below is a chart showing the payout scale on which the consolidated Adjusted EBITDA portion of incentive compensation was based."

Subsequently in the proxy statement for fiscal year 2013, the company mentions the actual payout from the award as follows: "For Fiscal 2013, the Company's actual consolidated Adjusted EBITDA was less than the minimum performance level of 50% of the consolidated Adjusted EBITDA target. Accordingly, actual consolidated Adjusted EBITDA performance resulted in a payout for this portion of the executives' annual incentive compensation of 0% of target."

Table A.1
Barnes & Noble payout level.

Level of Achievement of Consolidated Adjusted EBITDA Target	% of Target Payout
0% - less than 50%	0
50% - less than 75%	0.25
75% - less than 100%	0.625
100% - less than 112.5%	1
112.5% - less than 125%	1.085
125% or more	1.17

Table A.2
Quanta payout scale.

Percentage of Operating Income Goal Attained	Payout as a Percentage of AIP Target Incentive
Less than 75%	0
0.75	0.25
0.8	0.4
0.85	0.55
0.9	0.7
0.95	0.85
1	1
1.1	1.3
1.2	1.75
1.3	1.85
1.4	1.95
150% or greater	2

Table A.3
Sunoco performance elements and weights.

Incentive Plan Elements	Weight
Base Earnings per share	0.6
Revenue growth	0.2
Working capital improvement	0.2

Example 2. HealthNet in fiscal year 2006.

Our next example is a cash/stock award that involves interpolation. The proxy reads: “The performance share unit awards were granted pursuant to our 2006 Long-Term Incentive Plan (the “2006 LTIP”). The grants cliff vest as soon as practicable following the third anniversary of the date of grant based on achievement of minimum levels of pre-tax income and pre-tax income margin (pre-tax income as a percent of total revenues). For the Chief Executive Officer, no shares vest upon achievement of the target level of pre-tax income and pre-tax income margin, 100% of the shares vest upon achievement of the median level and 200% of the shares vest upon achievement of the maximum level (with linear interpolations for performance between the target and maximum levels), and for all other named executive officers, 50% of the shares vest upon achievement of the threshold level of pre-tax income and pre-tax income margin, 100% of the shares vest upon achievement of the target level, 150% of the shares vest upon achievement of the median level and 200% vest upon achievement of the maximum level (with linear interpolations for performance between the threshold and maximum levels). In addition, the Chief Executive Officer’s award can be settled in (i) shares of Common Stock, (ii) a cash payment equal to the fair market value of the shares earned as of the vesting date, or (iii) a combination of stock and cash.”

Example 3. Quanta in fiscal year 2012.

This is a cash award that involves interpolation. The proxy reads: “Based upon the sliding performance/payout scale adopted by the Compensation Committee, NEOs could earn cash awards under the annual incentive plan for 2012 as follows (when the attainment of the performance goal falls between the designated percentages in the table below, the cash awards are determined by interpolation).”

Example 4. Sunoco in fiscal year 2006.

This is an example of a performance-based award with multiple metrics, each with its own weight, threshold, target, and maximum levels (see Table A.2). The proxy reads: “Set forth below are the performance elements, and their respective weightings as a percentage of annual incentive compensation, the Committee used to arrive at actual 2006 bonus awards. It is the Committee’s philosophy that annual incentive plan elements should be limited to three or fewer to maximize concentration on those most critical to the success of our business in the forthcoming year. Base earnings per share, revenue growth and working capital management are all considered to be key performance variables essential to maximizing shareholder value. Base earnings per share are defined as earnings per share excluding the impact of restructuring charges and certain non-recurring, infrequent or unusual items and are used to place primary focus on year over year operating results. Revenue growth excludes revenue from acquisitions completed during the year. We believe that in most years, base earnings per share will be the most critical measure in driving share price and, in turn, shareholder value. Consequently, the Committee felt that a 60% weighting on this element was appropriate. Revenue growth was weighted at 20%. This is an important Company objective, but profitable revenue growth is of greater importance, hence the lower weighting than that for base earnings per share. The Committee added working capital improvement as a performance element in 2006 because it believed there was an opportunity to increase cash flow through reduction in our working capital requirements.”

The proxy then gives the levels required for each metric (see Tables A.3 and A.4).

Table A.4
Sunoco payout levels.

	Threshold	Target	Maximum	Actual 2006 Performance
Base Earnings per Share				
Amount	1.89	1.98	2.12	2.13
Percent of Prior Year	1	1.048	1.122	1.127
Revenue (Excl Acquisitions)				
Amount (millions)	3528.6	3652.1	3705.3	3648.4
Percent of Prior Year	1	1.035	1.05	1.034
Working capital - cash gap days				
Reduction from Prior Year	0	3.25 days	6.5 days	7.2 days

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