Compensation goals and firm performance*

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Abstract

Using a large dataset 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 and profit goals, when compensation is contingent on a single goal and is present for both long-term and short-term goals, when the pay-for-performance relationship is concave or convex and for grants with cash or stock payout. 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 EPS goals have higher abnormal accruals and lower Research and Development (R&D) expenditures and firms that just exceed their profit goals have lower SG&A expenditures. Overall, our results highlight some of the costs of linking managerial compensation to specific compensation targets.

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"Charlie and I believe that those entrusted with handling the funds of others should establish performance goals at the onset of their stewardship. Lacking such standards, managements are tempted to shoot the arrow of performance and then paint the bull's-eye around wherever it lands."

Warren Buffett

Introduction

In their ongoing effort to link managerial pay to performance, firms have increasingly resorted to tying annual bonus grants and long-term stock and option grants to achieving explicit performance goals. As noted in the quote above, investors like Warren Buffett have been major proponents of assessing management against specific performance goals. A typical cash or stock grant linked to firm performance identifies a threshold, target and maximum value 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 discontinuously change ("have kinks") at the target and the max performance.[1] In this paper, we use a comprehensive dataset containing information on the performance goals employed in pay contracts to highlight some of the costs of this 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, the "discontinuities and kinks" in the PPR at these explicit goals may also have a dark side. If there is a discontinuous 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 past the goal. In other words, managerial myopia may be

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

exacerbated around "points of discontinuity" in the PPR. The effect of kinks on managerial behavior is more nuanced. If the kink is concave, it will reduce the manager's incentives to improve firm performance much beyond the kink. Thus actual firm performance may cluster around points of discontinuities and concave kinks in the PPR.

Firm performance may also cluster at the target performance 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 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, managers may want to achieve the target performance and not fall short. We use our data to test if actual performance clusters around the goals mentioned in performance grants. We also perform tests to uncover the reasons why performance clusters around the goal.

For most of our analysis, we focus on absolute accounting based performance goals and analyze the distribution of reported performance around the goal. If reported accounting performance is managed to either beat the goal by a small amount 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 it. In other words, the distribution of reported performance will exhibit a discontinuity around the goal (Burgstahler and Dichev (1997) and 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 bootstraping techniques to test for the presence of discontinuities.[2]

We obtain data on performance goals from a dataset collected from firm's proxy statements by Incentive Lab (IL). We have information on all the cash, stock and option grants awarded to a top five highest paid executive for the 750 largest firms by market capitaliza-

[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.

tion 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 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, EBIT, EBITDA, Operating Income and FFO. This results in a sample of 29,591 grants awarded by 974 firms to 7,933 executives.[3] Among the accounting metrics employed, EPS is the most popular with around 40% 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.[4] We focus much of our analysis on the target because firm performance often clusters around the target and this increases the power of our tests of discontinuity in the underlying density. We construct three variables to help us identify discontinuity at the performance goal. *Actual less target EPS* is the difference between actual EPS as reported in Compustat and the target EPS as identified in the pay contract. Similarly, *Actual less target sales* (*Actual less target profit*) is the difference between actual sales (profit) and targeted sales (profits) normalized by the book value of total assets.[5] We find that the density of both *Actual less target EPS* and *Actual less target profit* exhibit a significant discontinuity at zero, that is, at the target value specified in the grant. 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. Interestingly, we do not find a corresponding discontinuity at zero for *Actual less*

[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.

[4] A majority of the grants in our sample have a kink in the PPR at the target and this kink is concave 65% of the time.

[5] We conduct our test separately on earnings (Earnings and EPS), sales (Sales) and profit (EBIT, EBITDA, Operating Income and FFO) based grants because the underlying distribution of these metrics are quite different and combining them in the same test will make the density estimation very noisy.

target sales. We find this effect is robust to confining the analysis to grants to CEOs and to grants that involve only a cash payout.

Next, we study the relationship between threshold performance and reported performance. Here again we find that firms are significantly more likely to beat the threshold performance by a small margin as compared to just missing the threshold by a small margin. Since there usually is a discontinuous 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 on the size of discontinuity at the target. Since influencing a single metric is easier than influencing multiple metrics, if performance clusters at the target because of performance management, then we should see more clustering for grants contingent on a 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 (which we explain presently) to statistically compare the size of the discontinuities.

Most grants in our sample have a kink in the PPR at the target performance. We classify the kinks 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. We find that while there is a discontinuity at the target for both concave and convex grants, we obtain mixed evidence about which one is larger. While our bootstrapping exercise indicates the discontinuity to be slightly larger for convex grants as compared to concave grants, our regression analysis indicates the opposite.

When we compare the extent of discontinuity at the goal for short-term and long-term grants, we find that there is significant discontinuity at the target for both sets of grants, especially for EPS and profit goals. We also find a significant discontinuity at the target both for grants involving cash payout and for grants involving some non-cash payout.

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 *Number of firms,* 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 *Number of 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 5.3. Our results from the regression analysis is broadly consistent with our graphical analysis. We find that the presence of a performance goal in a bin increases the probability of an additional firm's performance falling in the bin by 21%. We find that this effect is present for EPS and profit goals and is stronger for goals in grants contingent on a single metric, for grants that involve a concave kink at the target, for long-term grants and for grants that involve some non-cash payout.

Absent a discontinuous increase 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 effects. We find evidence consistent with the target ratcheting effect in that firms whose performance clusters around the target in one period are more likely to meet their performance 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 (using the methodology of Parrino (2007)) in the next year. 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 manager 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 we compare relative performance goals to the firm's actual 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 level of *Accruals*, $\Delta R\&D/TA$, $\Delta SG\&A/Sales$ and *Repurchase* for firms that just exceed the goal, i.e., 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.[6] 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 expense 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. (2005a); Roychowdhury (2006a)).

The remainder of our paper is organized as follows: Section 1 covers the related literature, Section 2 discusses our empirical methodology, Section 3 outlines our hypotheses, Section 4 discusses our data, Section 5 presents our empirical tests, and Section 6 concludes.

[6]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.

1 Related Literature

Our paper is most closely related to the papers that highlight the exercise of CEO power over her pay. Bebchuk and Fried (2004) and Adams et al. (2005) argue that CEO power over the pay process can explain much of the contemporary landscape of executive compensation. More managerial power leads to pay that is less sensitive to performance (what they call "compensation camouflage"). Morse et al. (2011) argue that a powerful CEO may opportunistically change performance benchmarks to increase her pay. In comparison, our paper highlights the effects of having *explicit* performance goals when executives exercise power over both the goal setting process and the reported performance.

Our paper is related to the prior literature that studies other performance goals that managers try to meet. These include the zero EPS goal (Burgstahler and Dichev (1997)) and the consensus analyst estimates (Bartov et al. (2002)). Our study differs from these in two important respects. In our setting, we know the monetary penalty managers face for not meeting a performance goal. This allows us to design sharper cross-sectional tests. Our analysis also helps highlight a dark side to the increasing use of accounting and stock based metrics to design pay contracts and the important role performance goals can play in predicting actual firm performance.

Our research is also related to the theoretical moral hazard and adverse selection literature. More specifically applicable is a strand of theoretical research on contracting settings where the agent can manipulate the observable performance measure. The main finding in Crocker and Slemrod (2008) is that compensation contracts that are written in terms of reported earnings cannot provide managers with incentives to maximize profits and at the same time provide managers with incentives to report those profits truthfully. Maggi and Rodríguez-Clare (1995) study a principal-agent setting 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 discontinuities emerge endogenously in the distribution of reported earnings.

A large literature in accounting and finance documents how executives manipulate reported performance to achieve performance goals. Cheng et al. (2010) find that firms may repurchase shares to manipulate EPS to achieve bonus targets. Roychowdhury (2006b) 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. (2005b) 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 recent literature that studies the use of performance provisions in executive compensation. 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 options plans are associated with better executive incentives among non-financial UK firms. Unlike these papers, we focus on the role of performance provisions in providing incentives to manage reported performance to meet managers own performance targets.

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, Matějka 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 more readily managed by the executive.

In addition to the intended contribution to the literature, our paper may also further stir up the already active, policy-oriented, executive compensation debate. As revealed in the opening quote from Warren Buffett, large investors are in favor of evaluating managers against specific performance goals. There is also increasing pressure from proxy advisory firms such as ISS and Glass Lewis for the use of explicit performance goals in executive compensation. Our paper highlights that the effective use of such provisions also requires greater board oversight on firm performance to minimize executives gaming of reported performance to meet the goals.

2 Empirical methodology

In this section, we describe the three tests that we perform to identify manipulation of firm performance to meet goals. All three 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, one obtains 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, one smooths 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 density. 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 $3.348[\tilde{\sigma^2}(b-a)/\Sigma\tilde{f}''(Xj)^2]^{1/5}$, 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 $\tilde{f}''(Xj)^2$ 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 $1.0585 \times \min\{\sigma, \frac{Q}{1.34}\} \times n^{\frac{1}{5}}$, 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 5.3.

3 Hypothesis

In this section, we outline the hypothesis that have predictions relevant for our setting. If there is a discontinuous 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. If alternatively, there is a "concave kink" in the PPR at the performance goal, it will reduce the manager's incentives to improve firm performance much beyond the kink point. Both these will result in a disproportionate number of firms having reported performance just in excess of the goal (Burgstahler and Dichev (1997) and 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. 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.

We expect managers to be more likely to influence performance to meet a goal if it is relatively easy to influence performance. To the extent influencing a single metric is easier than influencing multiple metrics, grants contingent on a single metric may provide greater incentives for managers to find ways to achieve the financial performance embedded in their compensation contracts. Thus, for our second prediction, we expect a larger discontinuity in the 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 nature of the kink 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 the performance clustering to be greater if the kink is concave as compared to if the kink is convex. This forms our third prediction. 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 positively depends on the performance metrics, 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 denominated in cash as compared to grants denominated in number of shares.

If firm performance clusters around the target because of the "target ratcheting" effect, then 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 this prediction 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 goals by increasing SG&A and accounts receivables. Managers can meet profit goals by cutting discretionary expenditures. We compare the level of *Accruals*, $\Delta R\&D/TA$, $\Delta SG\&A/Sales$ 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.

4 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 S&P (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 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. Since the identity of the set of largest firms changes from year to year, 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 specificed 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 managerial 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, Operating

Income and FFO. This results in a final sample of 974 firms and 7,933 executives covered by both IL and ExecuComp for the time period 2006-2012. For most of our analysis, we group the performance metrics into earnings (EPS and earnings), sales (Sales), and profit (EBIT, EBITDA, Operating Income and FFO) based metrics.

Panel A of Table 1 provides the summary characteristics of the grants that we analyze. We have a total of 29,591 grants in our sample. As can be seen, EPS is the most popular metric with around 40% of the grants in our sample (11,691 out of 29,591) linking some of the payout to an EPS goal. This is followed by sales, with about 30.5% of the grants (9,017 out of 29,591) 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. In the next three rows, 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 21,409 of the 29,591 grants involving some amount of cash payout. Stock is the next most popular form of payout, while very few grants involve 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 is greater to the left of the target as compared to the right of the target. Due to data availability, we are able to construct this variable only for 15,637 grants in our sample. Among the grants for which we are able to construct this variable, we find that 65% of the grants are concave at the target value. We find that grants tied to EBITDA 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. We find that 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 dataset to have one observation per executive-year. To do this, we combine all grants to an executive linked to the same metric (i.e., EPS for 2006) 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, (epspi, epspx, epsfi, epsfx) that vary based on whether they are fully diluted or not, and whether they include extraordinary items or not. Firms do not typically provide information on which EPS the 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.[7] 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 -0.118), the median performance is very close to the targeted EPS (median value of *Actual less target EPS* is 0). *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.* We find that actual firm performance is, on average, greater than the threshold performance. Both the mean and median values of *Actual less threshold EPS*

[7]See Wall Street Journal article from June 26, 2014 entitled "Some Companies Alter the Bonus Playbook" for instances of firms using non-GAAP measures to design executive compensation.

is positive. The *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. We find that firms, on average, exceed the sales target as seen from the positive mean value of *Actual less target sales.* Not surprisingly, as compared to the target sales, firms exceed the threshold sales by a larger margin. We have information about threshold performance for fewer grants because not all grants mention a threshold performance. On the other hand, for the purpose of calculating a fair value, all performance-linked grants mention a target performance. Finally, we find that the average firm's reported profits are higher than both the target and threshold profit mentioned in the pay contract, as can be seen from the mean value of *Actual less target profit* and *Actual less threshold profit.*

We acknowledge two potential issues with our variables that compare actual performance to target and threshold performance. First, 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. Second, to the extent we have executives below the rank of the CEO in our sample, their performance may be linked to divisional performance as opposed to firm performance. Comparing the targets of such executives to firm performance is also likely to introduce noise in our estimation. We do a number of robustness tests to ensure that noise from these sources does not bias our conclusions. In response to the first concern, 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.[8] In response to the second concern, we repeat all our tests confining our sample to grants to CEOs. We now discuss the empirical tests of our hypothesis.

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

5 Empirical tests

5.1 Full sample analysis

In panel (a) of Figure (1), we plot the histogram of *Actual less target EPS* along with a smooth density. The bin width for this histogram is 0.0121, 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 left. *Actual less target EPS* appears to be left skewed and because of this, the smooth density estimated by STATA has a mode to the left of zero. In panel (b), we present the results of the test proposed in McCrary (2008) that tests for the presence of a discontinuity in the empirical density at zero. This is the output from the "DCdensity" function in STATA with the default bin width. Panel (b) of Figure 1 plots the empirical density along with the 95% confidence intervals (CIs). Given the small standard errors, the CIs are difficult to visually distinguish from the density plot. From the figure, we find significant evidence for a discontinuity at zero. 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 EPS* 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 EPS* and count the number of observations that lie in the first bin to the right of zero (i.e. between 0 and 0.0121) and the number of observations that lie in the first bin to the left of zero (i.e. between -0.0121 and 0). We repeat this 1,000 times and compare the means. We find that on average, in a sample of 50, there are 1.31 more observations just to the right of zero as compared to the number of observations just to the left of zero. We find this is statsitically very significant with a t-value of 22.721.

In panel (c) of Figure 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 $0.7764 \times 1.364 \times \min \sigma, \frac{Q}{1.34} n^{-\frac{1}{5}}$ 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 0.0537 for *Actual less target EPS*. The green line plots the t-values and the blue 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 (small) to the right (left) of zero. This is consistent with the presence of a disproportionately large (small) number of observations to the right (left) of zero.

Interestingly, the graph in panel (c) also identifies discontinuities at places other than at zero. There are two possible reasons for this. First is the noise in our estimate of *Actual less target EPS*. This arises both from us not knowing the exact metric employed in the grant and due to the fact that for non-CEO executives the grants may be tied to division as opposed to firm performance. The second reason for us finding discontinuities at places other than zero is that the pay contract often involves kinks and discontinuities in PPR at more than one place (see Appendix A). Specifically, the manager's payoff may not only have a kink at the target value, but also have a discontinuous jump at the threshold and plateau off when the firm performance exceeds the max value. Our tests in Figure 1 only test for the presence of a discontinuity at the target performance. If firm performance is closer to the max, then the high t-values may capture the clustering of firm performance at the max performance. If the payoff from the grant plateaus off at the max value, then the manager may have little incentives to report a performance that exceeds the max performance. This is likely to happen at positive values of *Actual less target EPS* because max values are typically larger than target values. Thus, an issue with the test in panel (b) of Figure 1 is its inability to accomodate and test for discontinuities at more than one place in the density.

In Figure 2, we test for discontinuities at zero for *Actual less target sales.* The tests in Figure 2 are similar to the ones in Figure 1. From panel (a), we find that the histogram is clustered around zero, but the distribution is positively skewed with a few large positive values. The bin width for the histogram is 0.0029 From panel (b), we find that there is no significant discontinuity at zero at the 95% CI when we employ the default bin width of 0.0029. Thus, there does not appear to be a disproportionately large number of firms that beat sales goals.

Interestingly, when we do the bootstrapping exercise, we do find that there are more observations to the right of zero as compared to the left of zero. Specifically, the difference in the number of observations just to the right and left of zero is .44 and it is statistically significant with a t-value of 4.94. Note that both the size and significance of the effect with the bootstrapping exercise is smaller for *Actual less target sales* as compared to for *Actual less target EPS.* This is consistent with there being weaker evidence for performance clustering around sales goals as compared to around EPS goals. From the last panel we find that while the t-values indicate a significant discontinuity at zero, there are significant discontinuities at points other than zero as well. Here again, we believe that these are partly due to the presence of discontinuities at the max value.

Finally in Figure 3, we test for discontinuities at zero for *Actual less target profit.* From panel (a), we find that the histogram is clustered around zero, but the distribution is positively skewed with a few large positive values. The bin width for the histogram is 0.0006. From panel (b), we find that there is a significant discontinuity at zero at the 95% CI when we employ the default bin width of 0.0006. We find that the discontinuity is again not dependent on the bin width when we vary the bin width from .0001 to .001. Here again, when we perform the bootstrapping exercise, we find that there are more observations to the right of zero as compared to the left of zero. Specifically, the difference in the number of observations just to the right and left of zero is 0.115 and it is statistically significant at the 10% level with a t-value of 1.9. Finally, from the last panel we find that while the t-values indicate a significant discontinuity at zero, they also indicate discontinuities at places other than zero.

The presence of discontinuity at zero in the case of *Actual less target EPS* and *Actual less target profit*, and the weaker result for *Actual less target sales* is consistent with it being more difficulty to manage performance to meet a sales goal as compared to an earnings or a profit goal. This is reasonable if firms face fixed costs and hence can bridge a given proportional short-fall in earnings or profits with less performance management as compared to the same proportional short-fall in sales.

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 continguous 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.

In Figure 4, we confine our sample to grants to CEOs and repeat our tests for discontinuity at zero. Consistent with our evidence in Figures 1- 3, we find that while there is a significant discontinuity at zero in the case of both *Actual less target EPS* and *Actual less target profit*, there is no significant discontinuity at zero for *Actual less target sales.*

In Figure 5, we test for discontinuities at zero for *Actual less threshold EPS, Actual less threshold sales* and *Actual less threshold profit.* As mentioned before, we construct these variables by comparing actual performance with the threshold performance mentioned in the grant. Interestingly, we find that there is a significant discontinuity at zero for all three variables. Thus, a disproportionate number of firms have performance just above threshold performance as compared to the number of firms with performance just below the threshold performance.

5.2 Subsample analysis

In Figure 6, we focus on *Actual less target EPS*, and in panels (a) and (b) we divide our sample into subsamples based on whether the grant involves a single metric or multiple metrics. We expect managers are more likely to alter reported EPS if that is the *only* metric that affects a majority of the payout from the grant. 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, visually at least, the size of the discontinuity appears larger in the former subsample. In panels (c) and (d), we focus on *Actual less target sales* and find that the discontinuity at zero is insignificant both for grants involving a single metric and for grants involving multiple metrics. Finally in panels (e) and (f), we focus on *Actual less target profit* and find that similar to *Actual less target EPS*, the discontinuity at zero is larger for firms that award grants that include profit as the only metric. The discontinuity when profit is used along with other metrics is much smaller.

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 pool the values of *Actual less target EPS*, *Actual less target sales*, and *Actual less target profit* for all grants in our sample and 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 Figure 6. That is we use different bin sizes for the different metrics and for single and multiple metric based grants. We repeat this 1000 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 Figure 6, we find that the discontinuity is larger for grants based on a single metric. On average, in a sample of 100, there are 1.19 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. We find this is statsitically very significant with a t-value of 9.57.

In Figure 7, we perform cross-sectional tests focusing on whether the kink at the target value is concave or convex. We classify the kink by comparing the slope of the PPR to the right and left of the target value. Due to data availability, we can only identify the nature of the kink for about 52.8% of the grants in our sample. Thus, the sample size is smaller for these tests. In panels (a) and (b) of Figure 7, we divide our sample into grants that involve a concave versus convex kink at the target and test for a discontinuity at zero for *Actual less target EPS*. We find that the discontinuity at zero appears to be larger for grants that involve a concave kink at the target. In panels (c) and (d), we repeat our analysis with *Actual less target sales* and find that there is no discontinuity at zero for either set of grants. Finally in panels (e) and (f), we focus on *Actual less target profit* and again find that the discontinuity at zero is larger for grants that involve a concave kink at the target. Here again we perform a bootstrapping exercise to statistically compare the size of the discontinuity. Our procedure shows that the discontinuity at zero is statistically significant for both concave and convex grants. For concave grants we find that there are 1.6 more observations in the bin to the right of zero as compared to the bin immediately to the left of zero while the corresponding number is 1.98 for convex grants. Thus, interestingly the bootstrapping exercise indicates that the size of discontinuity is slightly greater for convex as compared to for concave grants. We find that this result is not robust as our regression based tests provide the opposite conclusion.

In Figure 8, we divide our sample into short-term and long-term grants and test for a discontinuity at zero in each of the two subsamples. As mentioned before, to the extent it is difficult to anticipate long-term performance as compared to short-term performance, any discontinuity at zero for long-term grants is likely to be due to managers managing ex post accounting performance as opposed to the ex ante goal. On the other hand, the discontinuity at zero for short-term grants can arise both due to setting lower goals and managing ex post accounting values. In panels (a) and (b), we focus on *Actual less target EPS* and find that the discontinuity at zero is present for both short-term and long-term grants. Recall that we classify all grants with a final payout beyond 11 months after the grant date as long-term. In panels (c) and (d), we focus on *Actual less target sales* and find that the discontinuity at zero is not present for either short-term or long-term grants. Finally in panels (e) and (f), we focus on *Actual less target profit* and find that while the discontinuity at zero is present for both long-term and short-term grants, it appears larger for short-term grants. Overall, the evidence in Figure 8 indicates that while the discontinuity is more pronounced for short-term grants especially if they are based on an earnings or profit metric, there is some discontinuity at zero even for long-term grants. The latter evidence is consistent with ex post management of accounting values to meet compensation performance goals.

Finally, in Figure 9, we divide our sample into grants that only involve cash payout and grants that involve some amount of stock payout and test for a discontinuity at zero in each of the two subsamples. As mentioned before, grants denominated in terms of number of shares introduce a convexity in the PPR. In panels (a) and (b), we focus on *Actual less target EPS* and find that the discontinuity at zero is present for both grants that involve only cash payout and for grants that involve some stock payout. In panels (c) and (d), we focus on *Actual less target sales* and find that the discontinuity at zero is not present for either cash or non-cash grants. Finally in panels (e) and (f), we find that the discontinuity at zero for *Actual less target profit* is statistically significant only for cash grants. When we perform the bootstrapping exercise to statistically compare the size of the discontinuities, we find that the discontinuity is present for both cash and non-cash grants. Specifically, for cash grants in a sample of 100, we find that there are 0.93 more observations in the bin to the right of zero as compared to the bin immediately to the left of zero while the corresponding number is 1.32 for non-cash grants. Thus, interestingly the bootstrapping exercise indicates that the size of discontinuity is actually greater for non-cash as compared to cash grants.

5.3 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. That is, we estimate the following model:

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

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 falls 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 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), the maximum and the 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, for EPS growth it is 1.5% and is .01 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 mid point of the bin – the first four terms in the above model. Also 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 we can combine all the metrics in the same test. We can estimate the 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 *Number of goals - single metric* and *Number of goals - multiple metrics* and repeat our estimation. *Number of goals - single metric* (*Number of goals - multiple metric*) counts the number of firms that offer 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 compare 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 form our conclusions.

In Table 3, 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 21%. Note that to the extent each grant has three goals (threshold, target and max performance) 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 0.21 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 0.52 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, *EPS goals, Sales goals* and *Profit goals* and find that while the coefficient on *EPS goals* and *Profit goals* is positive and significant that on *Sales goals* in insignificant. This is consistent with our results from the graphical analysis. In column (3), we repeat our tests after splitting *Number of goals* into, two variables, *Number of goals- single metric* and *Number of goals- multiple metrics.* We find that while the coefficient on both the variables is positive and significant, the one on *Number of goals- single metric* is larger than the one on *Number of goals- multiple metrics,* from the row titled Δ*Coefficient* we find that this difference is statistically significant. In column (4) we include two variables, *Number of goals- concave and Number of goals- convex*, and repeat our tests. *Number of goals- concave* (*Number of goals- 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) shows that while the coefficient on both *Number of goals - concave* and *Number of goals - convex* is positive and significant, the former is larger. This is consistent with concave grants providing weaker incentives for executives to exceed the target performance by a large amount. From the row titled Δ*Coefficient* we find that the coefficient on *Number of goals- concave* is statistically larger than that on *Number of goals- convex.*

In column (5), we compare long-term and short-term goals by including two terms, *Number of goals-short term* and *Number of goals-long term*, and surprisingly we find that while the coefficient on *Number of goals -long term* is positive and significant, the coefficient on *Number of goals-short-term* is not significant. We also find that the coefficient on the former is statistically larger than that on the latter (row titled Δ*Coefficient*). Our results are consistent with our bootstrapping exercise but counter to the observational evidence in Figure 8. Since these tests make different assumptions in modelling the density – the validity of each of which is difficult to establish – and in the case of the bootstrapping exercise, only focus on the bins around the goal, we do not pick one result over the other. Overall we interpret the evidence as being consistent with the existence of a discontinuity around both short-term and long-term goals, consistent with the presence of performance management.

Finally in column (6) we compare the discontinuity at zero for grants that only involve a cash payout to the grants that involve some stock payout. Consistent with our bootstrapping evidence, we find the discontinuity at zero is greater for grants that involve some non-cash payout as compared to for grants that only involve cash payout. This is counter to our evidence comparing concave and convex grants as non-cash grants are more likely to introduce a convexity in the PPR.

In additional robustness tests, instead of a fourth-order polynomial in *Mid-point*, we include bin fixed effects and repeat our tests. We find our results are robust to this alternate specification.

5.4 Relative performance based awards

In Figure 10, we focus on relative performance based awards to test if firms 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 difficult to manage the performance of the peer group. To see if this is the case, in Figure 10 we compare the relative performance targets 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 Figure 10 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 1 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 discontinuity at zero and do not find any statistically significant discontinuity at zero. Here again, the bin size is 1. 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.

5.5 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 discontinuous increase in pay.

5.5.1 Target ratcheting

To test the predictions of the "target ratcheting" effect, in Panel A 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. In these tests, we focus on grants to the CEO and to avoid a mechanical correlation between the targets from one period to another, we confine the grants to annual grants. Our sample includes one observation per metric-firm-year. To ensure that outliers do not bias our estimates, we exclude firms with estimates of *Actual less target EPS, Actual less target sale*, and *Actual less target profit* beyond the 5th and 95th percentiles. Our dependent variable is a dummy variable that identifies firms that meet the target this period, while the main independent variable, *Exceed target*, is a dummy variable that identifies firms that just exceed the target 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 definine 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 column (2) indicate that firms whose performance just exceeds the target are 8.2% more likely to meet their target the next period. In columns (3) - (4), we repeat our tests after confining the sample to firms with *Actual less target EPS, Actual less target sale*, and *Actual less target profit*, within the 10th and 90th percentiles and obtain consistent results.

5.5.2 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. We follow Parrino (2007) in identifying forced CEO turnovers. 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. We have 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 ret., Return, Size, Volatility, Tenure, Age, CEO Shareholding,* and *Duality.* All the variables we use here are defined in Appendix A. Along with these, we also include a second order polynomial of the actual performance relative to the target. 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. In comparison the average probability of a forced CEO turnover is 1.3% in our sample. In column (3), we repeat our tests after including observations for which *Actual less target EPS, Actual less target sale*, and *Actual less target profit*, lie within the 5th and 95th percentiles and obtain consistent results. Finally, in column (4), we repeat our tests focusing on voluntary CEO turnovers and interestingly find a negative and significant relationship between missing a performance target and voluntary turnovers. That is, CEOs are less likely to voluntarily leave a firm if they miss a performance target the previous year. These tests 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 preference. 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.

5.6 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 exceed performance goals. These tests help us understand the extent to which firms manage accruals and discretionary expenditure to manage reported performance. Depending on the metric involved, managers can employ a variety of means 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 sample of firms that exceed and miss the goals are different. In Table 5, we compare firms that exceed and those that miss their performance goal. Definitions of all the variables we compare in this table are provided in Appendix A.

In panel (a), we focus on EPS goals. We find that 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 expenditure. 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 repurchase 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, 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 (2006b) and Dechow et al. (2003) who find that firms often decrease discretionary spending, in an effort to increase short term earnings. We now present some multivariate evidence.

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

$$\begin{aligned} y_i &= \alpha + \beta_0 \times \text{Exceed EPS/Sales/Profit} + \beta_1 \times \text{Size} + \beta_2 \times \text{Market to book} \\ &+ Y + \gamma_j + \epsilon_i \end{aligned} \tag{2}$$

where the dependent variable is one of *Accruals*, Δ*R&D/TA*, Δ*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.

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, in Table 6 we present the results of the tests in which the coefficient on the main independent variable is significant. From column (1), we find that 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 change in R&D expenditure. This is consistent with such firms lowering R&D expenditure more than firms that miss EPS goals. From column (3), we find that firms that meet profit goals reduce SG&A expense 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.

6 Conclusion

We use a comprehensive dataset containing information on the performance goals employed in 29,591 stock and cash 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 discontinuous increase 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, and profit 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 present both for long-term and short-term grants, both for grants that payout in cash and grants that payout in stock and both when the PPR is concave or convex at the goal. 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 expenditure 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 annual bonus grants and long-term stock and option grants to achieving explicit performance goals. Our paper highlights an important cost to awarding performance-contingent grants that focus on a particular performance number as target performance, because they may provide perverse incentives for management to aim for the target. We believe that, at a minimum, our results suggest that it is better to include performance provisions in pay contracts in a way that they provide a more *continuous* link between pay and performance.

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* if 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*.
- *Age* is the age of the CEO.
- *CEO shareholding* is the percentage shareholding of the CEO.
- Δ*R&D* is one thousand times the year-on-year change in R&D expenditure normalized by book value of total assets.
- Δ*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/sale/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 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 to.

- *Market to book* is the ratio of market value of total assets to book value of total assets.

- *Market to book* is the ratio of market value of total assets to book value of total assets.

- *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, 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, 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, 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, FFO or Operating Income) that are in grants involving a concave (convex) kink at the target value fall within a bin.

- *No.of goals - Long-term (Number of goals - Short-term)* is one plus the natural logarithm of number of firms whose compensation contract goals (EPS, sales, EBIT, EBITDA, FFO or Operating Income) that are in short-term (long-term) grants fall within a bin.
- *No.of goals - Cash (Number of goals - Non-cash)* is one plus the natural logarithm of number of firms whose compensation contract goals (EPS, sales, EBIT, EBITDA, FFO or Operating Income) in grants involving cash (non-cash) payout fall within a bin.
- *Number of metrics* is the number of different metrics (such as EPS, sales, ROA, 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, 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 scal 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 the previous fiscal year.
- *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 cashflow 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.
- *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. 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."

Table A.1: Barnes and Nobel payout levels

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

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."

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)."

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

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 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."

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

The proxy then gives the levels required for each metric.

Table A.4: Sunco 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 (Excluding 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

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 statics 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 is from Incentive Lab (IL), Compustat, CRSP and ExecuComp.

(a) Summary grant characteristics

	EPS	Earnings	Sales	EBIT	EBITDA	EBT	FFO	Operating Income	Total
Number of firms	492	291	456	106	224	135	35	399	974
Number of executives	3,551	1,670	3,153	598	1,415	741	221	2,617	7,933
Number of grants	11,691	4,275	9,017	1,446	3,762	1,788	725	7,221	29,591
Number of grants that involve									
Cash payout	8,730	3,810	7,491	1,324	3,381	1,586	602	6,734	21,409
Stock payout	3,539	907	2,029	218	679	328	139	1,407	8,421
Option payout	204	29	70	34	58	3	1	11	367
Long-term vesting	.236	.111	.136	.106	.106	.1	.092	.119	.159
Concave	.699	.659	.647	.675	.568	.663	.727	.612	.65
Multiple	.199	.254	.345	.178	.201	.257	.125	.256	.223

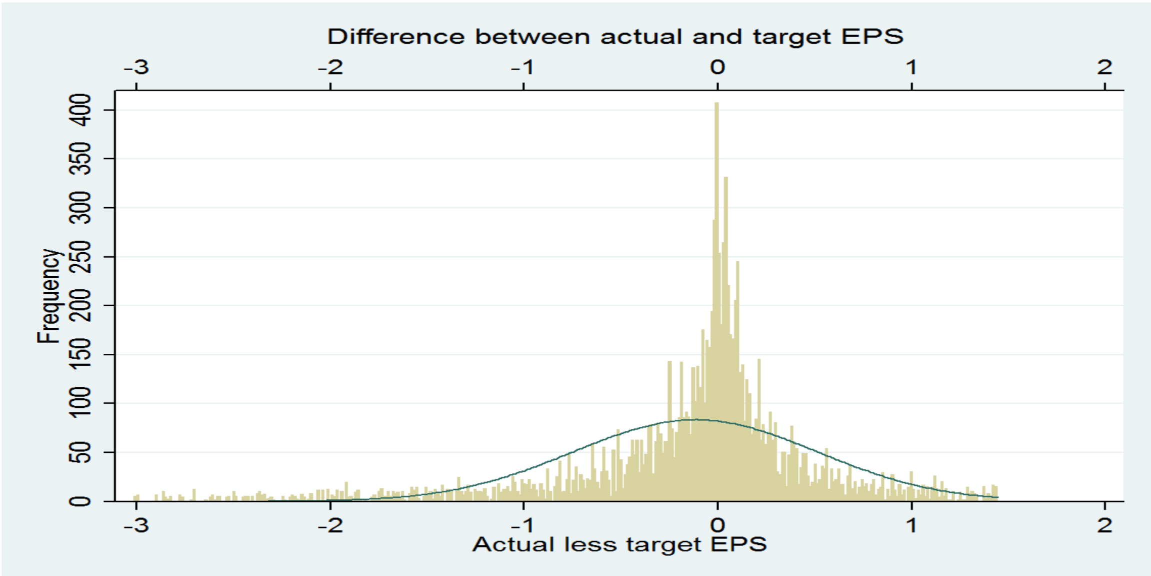
(b) **Performance goals and actual performance**

Variable	**N**	**Mean**	**SD**	**P25**	**Median**	**P75**
Actual less target EPS	10,959	-0.118	0.633	-0.287	0.000	0.150
Actual less threshold EPS	7,466	0.100	0.690	-0.120	0.100	0.420
Actual less target sales	6,830	0.034	0.122	-0.019	0.006	0.047
Actual less threshold sales	4,391	0.085	0.139	0.009	0.047	0.118
Actual less target profit	9,566	0.007	0.029	-0.008	0.003	0.017
Actual less threshold profit	6,498	0.017	0.029	-0.001	0.011	0.032

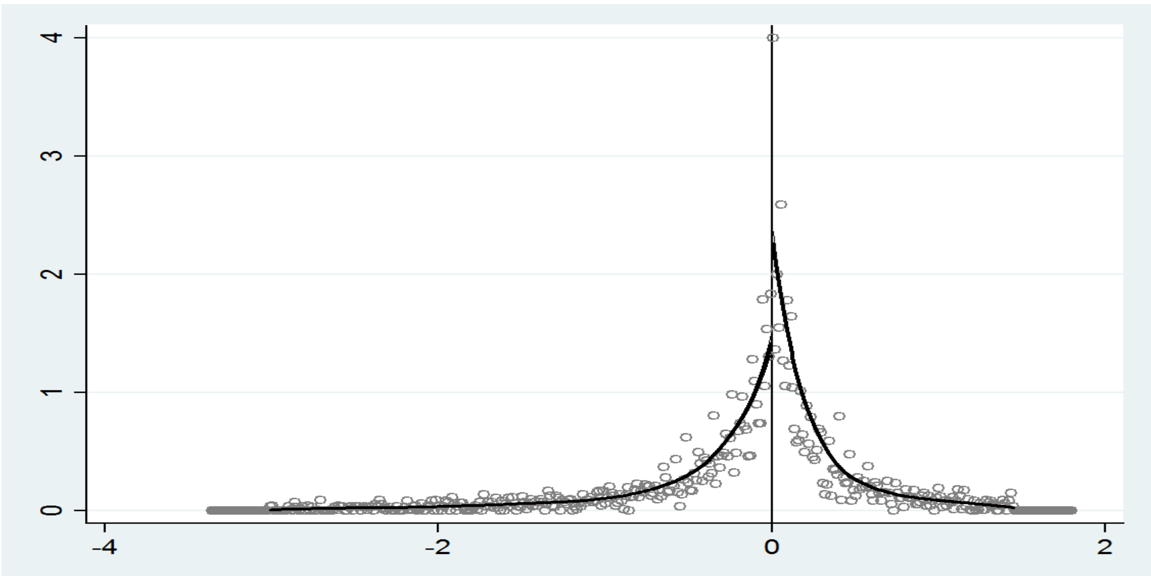
Figure 1: **Difference between actual and target EPS**

This figure tests for discontinuity in the density of *Actual less target EPS*. In Figure (a) we present the histogram of *Actual less target EPS* along with a smooth density. The bin width for this histogram is 0.0121. Figure (b) presents the results of McCrary (2008) test for the presence of a discontinuity in the empirical density at zero. Figure (c) presents the result of a test for the presence of a discontinuity in the density at points other than zero. These tests are similar to those in Bollen and Pool (2009).

(a) Histogram of difference between actual and target EPS



(b) Test of discontinuity at zero



(c) Results of t-test of difference between actual and estimated density

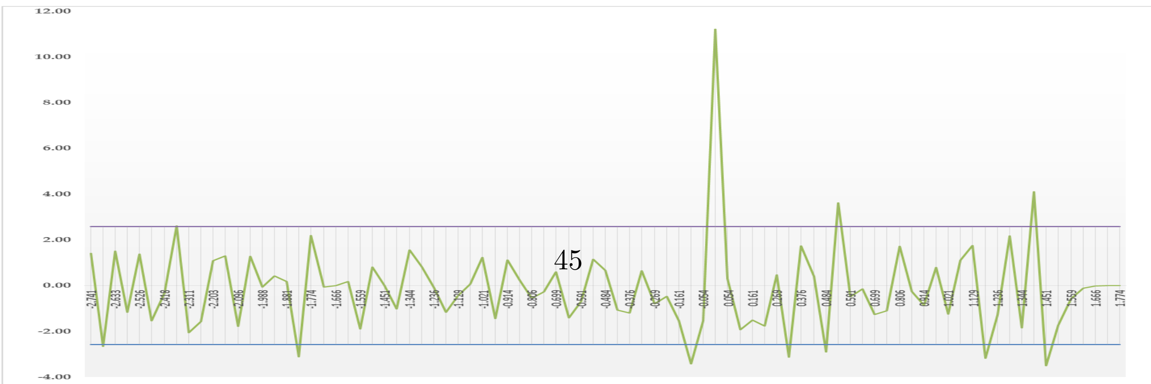
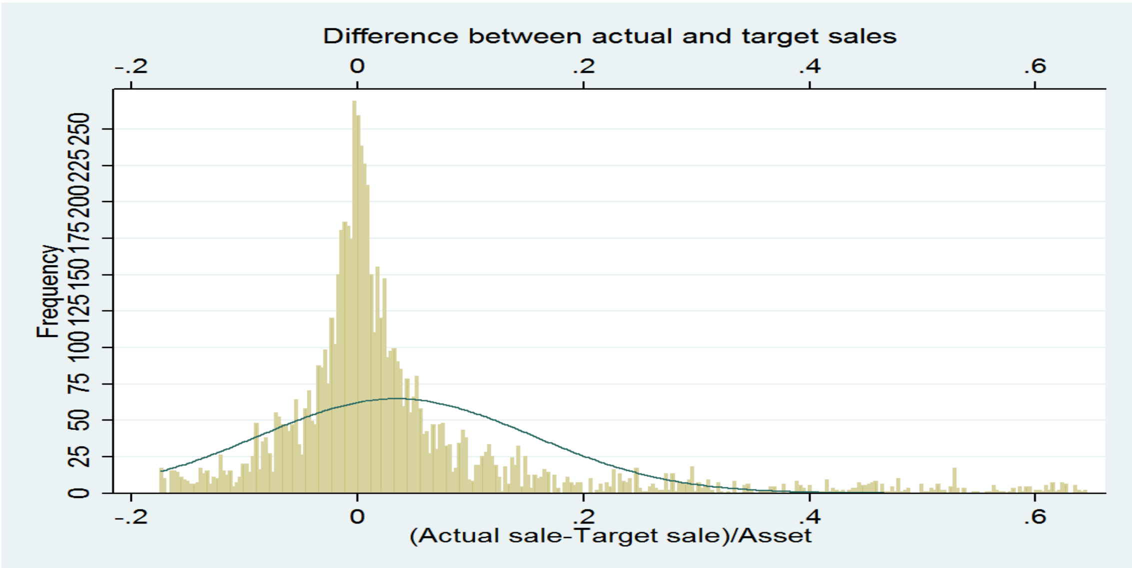


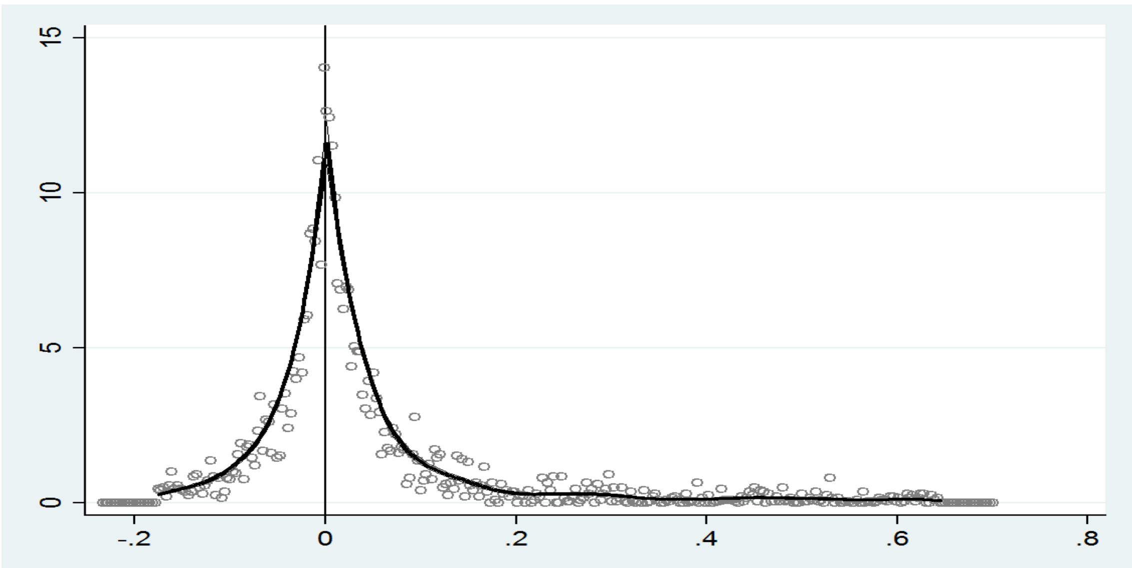
Figure 2: **Difference between actual and target sales**

This figure tests for discontinuity in the density of *Actual less target* sales. In Figure (a) we present the histogram of *Actual less target sales* along with a smooth density. The bin width for this histogram is 0.0029. Figure (b) presents the results of McCrary (2008) test for the presence of a discontinuity in the empirical density at zero. Figure (c) presents the result of a test for the presence of a discontinuity in the density at points other than zero. These tests are similar to those in Bollen and Pool (2009).

(a) Histogram of difference between actual and target sales growth



(b) Test of discontinuity at zero



(c) Results of t-test of difference between actual and estimated density

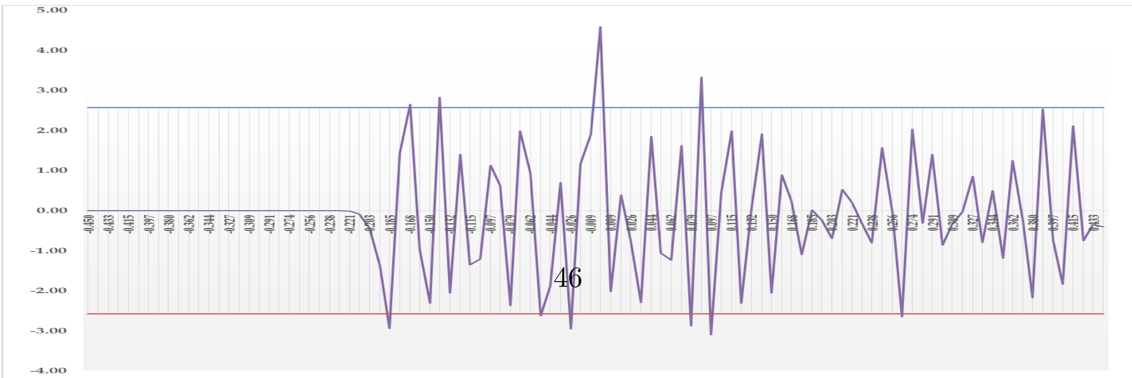
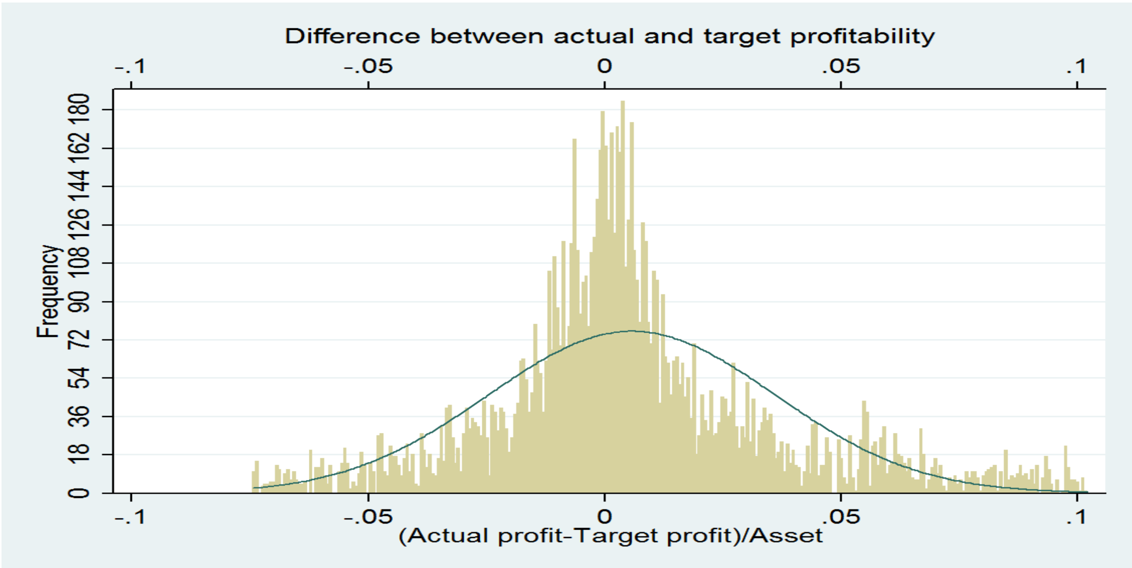


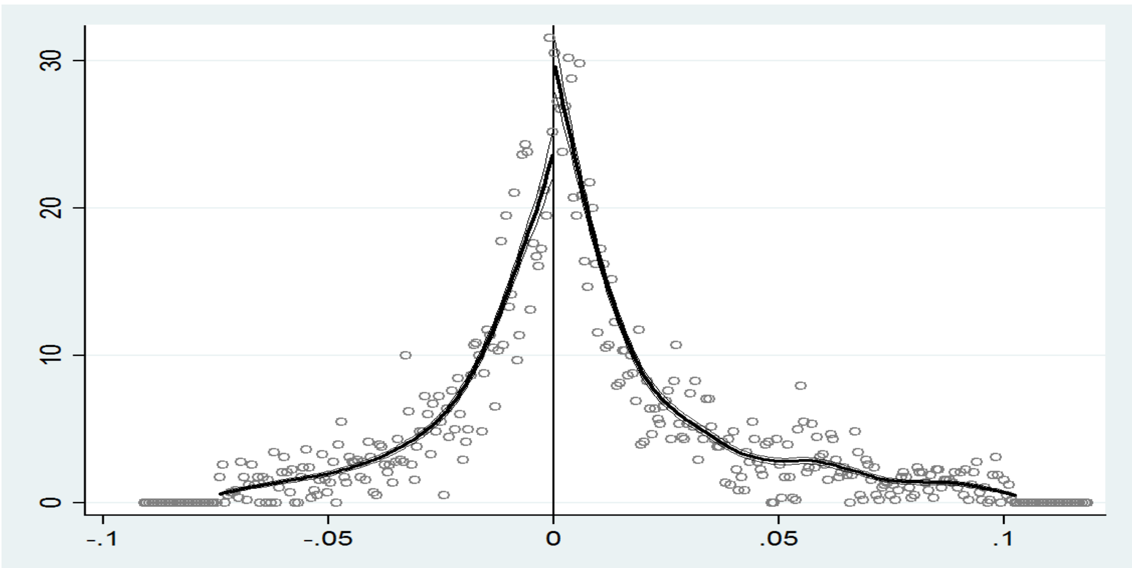
Figure 3: **Difference between actual and target profit**

This figure tests for discontinuity in the density of *Actual less target* profit. In Figure (a) we present the histogram of *Actual less target profit* along with a smooth density. The bin width for this histogram is 0.0006. Figure (b) presents the results of McCrary (2008) test for the presence of a discontinuity in the empirical density at zero. Figure (c) presents the result of a test for the presence of a discontinuity in the density at points other than zero. These tests are similar to those in Bollen and Pool (2009).

(a) Histogram of difference between actual and target sales growth



(b) Test of discontinuity at zero



(c) Results of t-test of difference between actual and estimated density

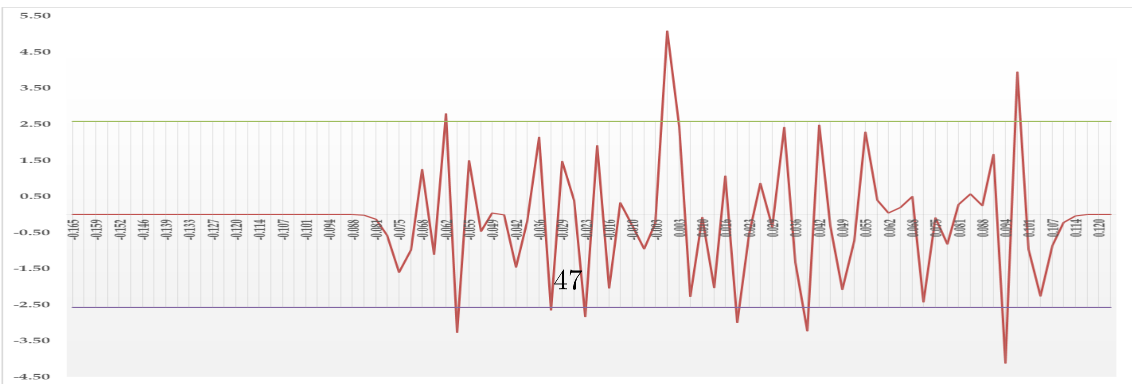
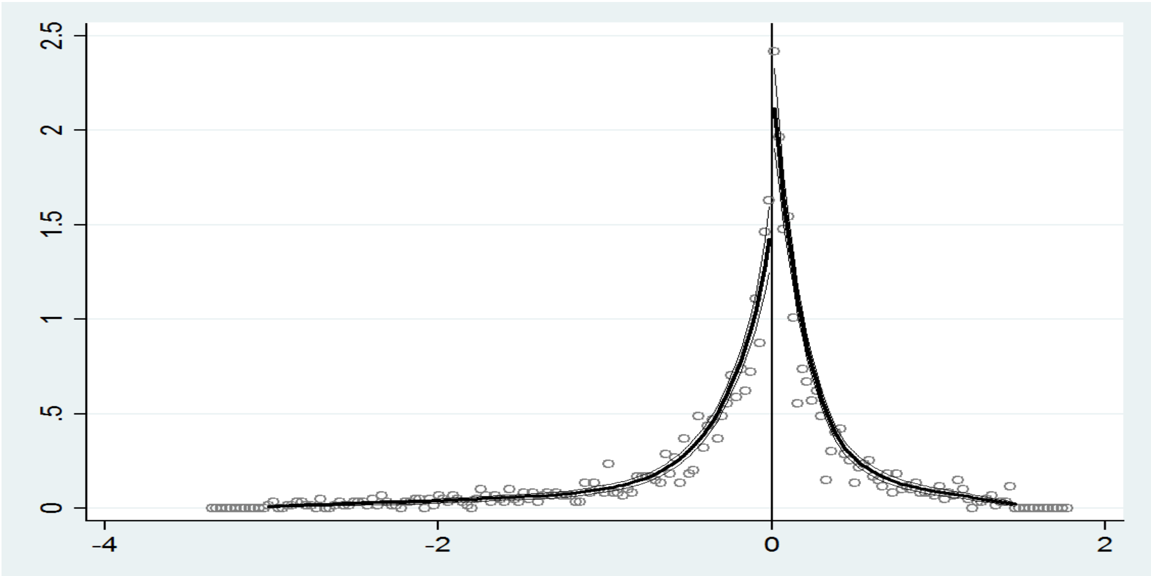


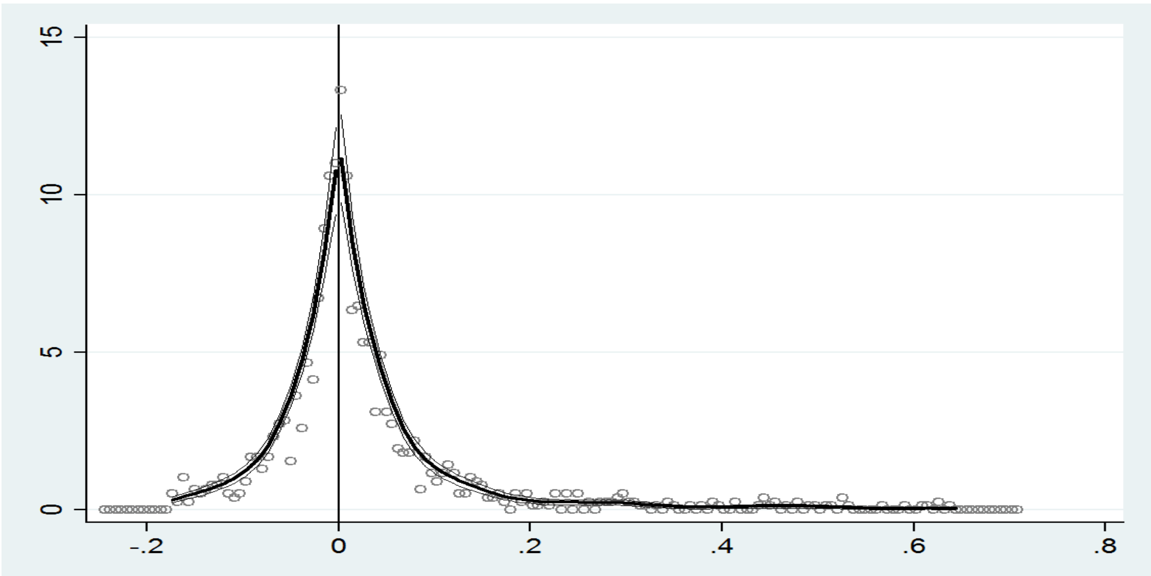
Figure 4: **Difference between actual and target performance - CEOs only**

This figure tests for discontinuity in the density of *variables that compare actual performance with target performance of grants to CEOs.* In Sub-figures (a), (b) and (c) present the results of McCrary (2008) test for the presence of a discontinuity in the empirical density at zero for *Actual less target EPS, Actual less target sales,* and *Actual less target profit,* respectively. The bin-size for these tests are .028, .006 and .0014 respectively.

(a) *Actual less target EPS* - Test of discontinuity at zero



(b) *Actual less target sales* - Test of discontinuity at zero



(c) *Actual less target profit* - Test of discontinuity at zero

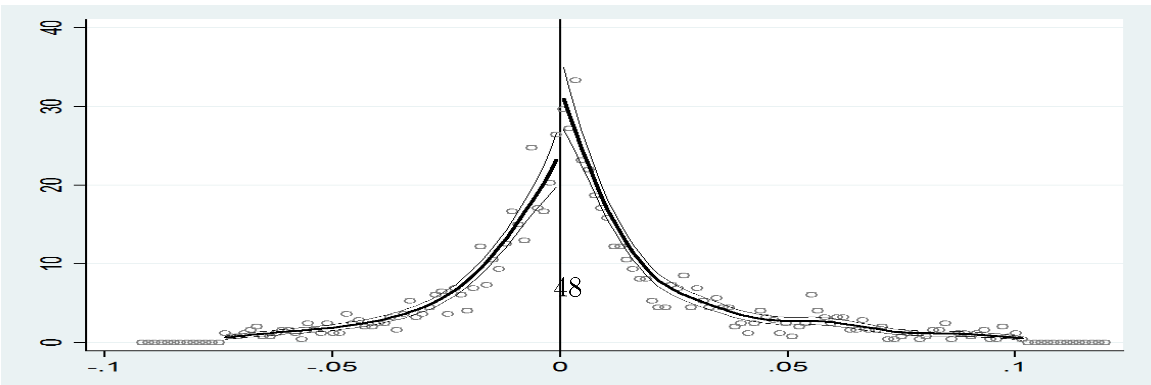
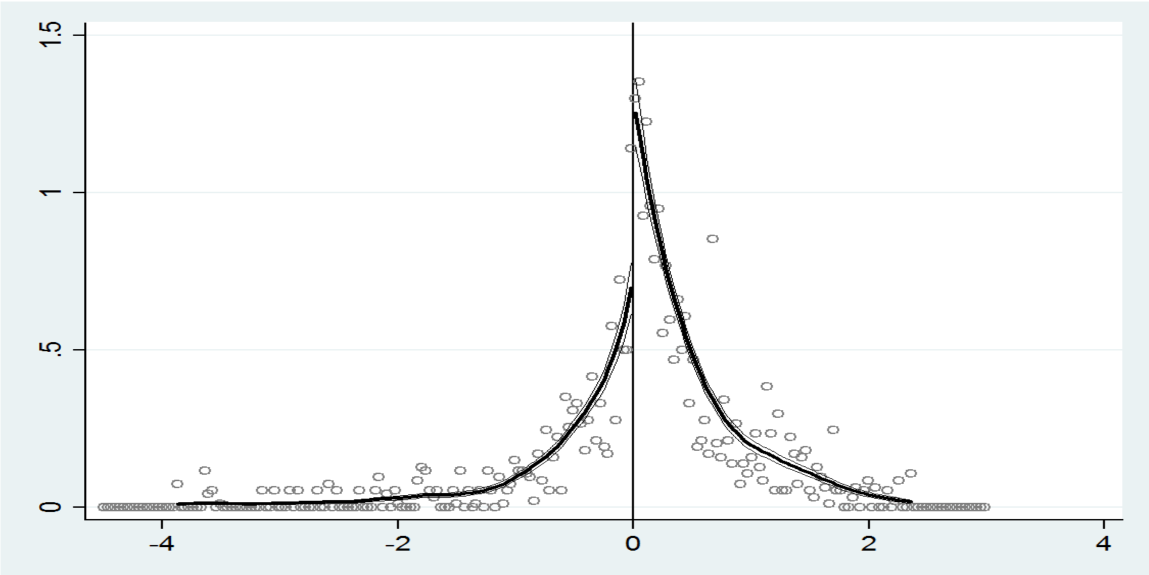


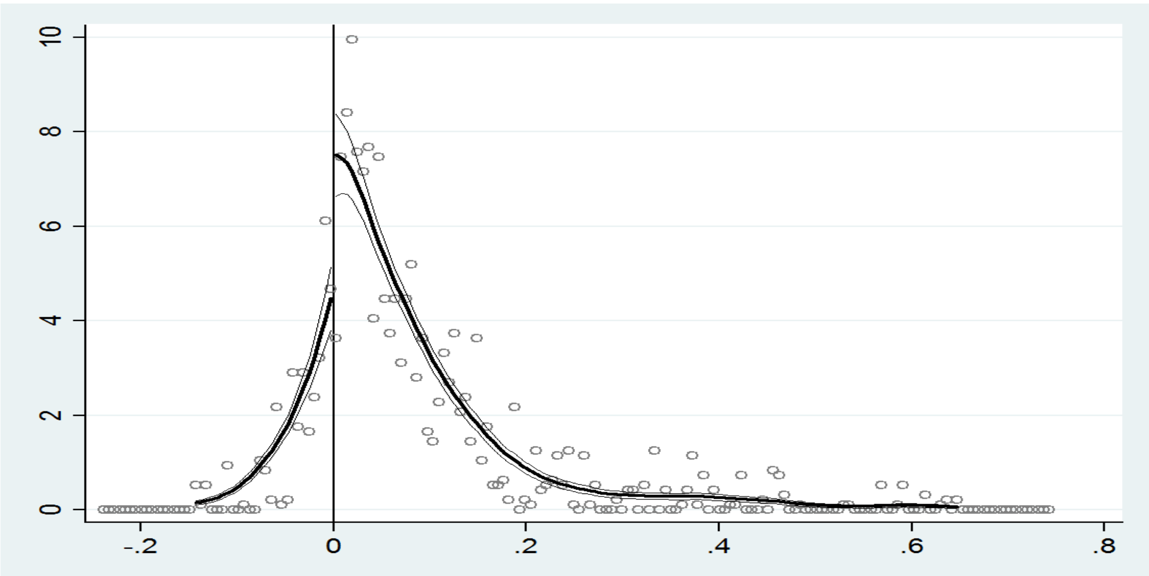
Figure 5: **Difference between actual and threshold performance**

This figure tests for discontinuity in the density of *variables that compare actual performance with threshold performance.* In Sub-figures (a), (b) and (c) present the results of McCrary (2008) test for the presence of a discontinuity in the empirical density at zero for *Actual less threshold EPS, Actual less threshold sales,* and *Actual less threshold profit,* respectively. The bin-widths for these tests are .016, .004 and .0007 respectively.

(a) *Actual less threshold EPS* - Test of discontinuity at zero



(b) *Actual less threshold sales* - Test of discontinuity at zero



(c) *Actual less threshold profit* - Test of discontinuity at zero

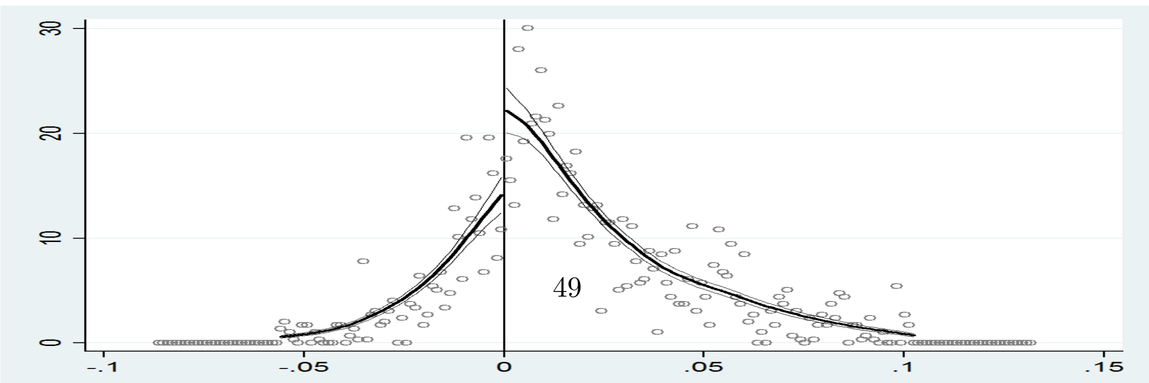
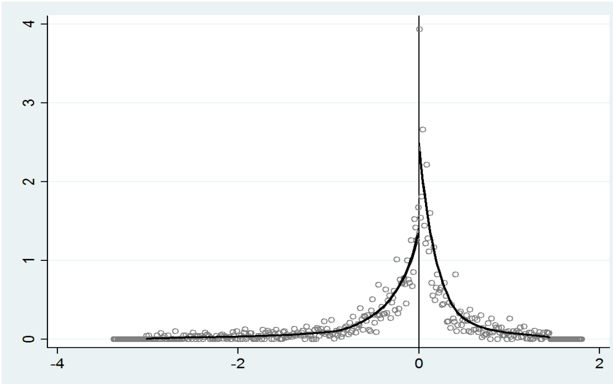


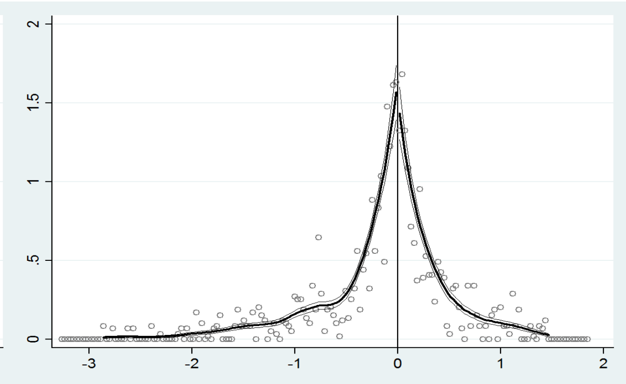
Figure 6: **Actual performance and targets: Single versus multiple metrics**

This figure presents the results of a test for a discontinuity at zero in the density of *Actual less target EPS* (panels (a-b)), *Actual less target sales* (panels (c-d)) and *Actual less target profit* (panels (e-f)). In the left-hand-side panel we focus on grants that involve a single metric while in the right-hand-side panel we focus on grants that involve multiple metrics.

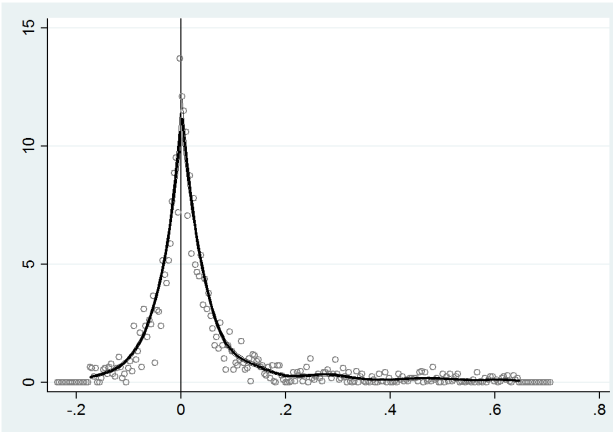
(a) Actual versus target EPS: Single metric



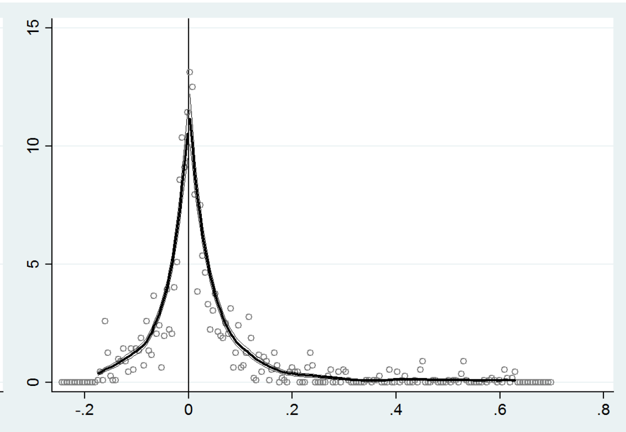
(b) Actual versus target EPS: Multiple metrics



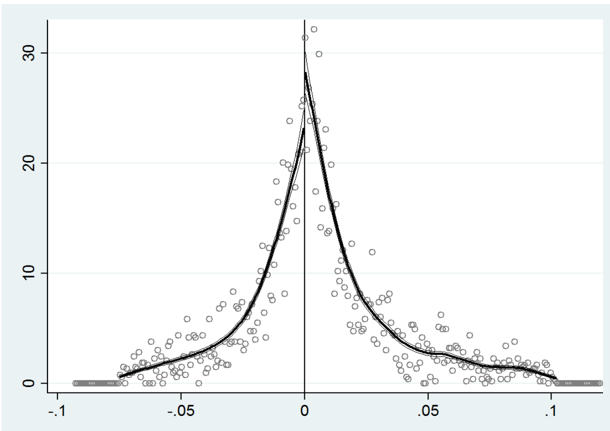
(c) Actual versus target Sales: Single metric



(d) Actual versus target Sales: Multiple metrics



(e) Actual versus target Profit: Single metric



(f) Actual versus target EPS: Short grants

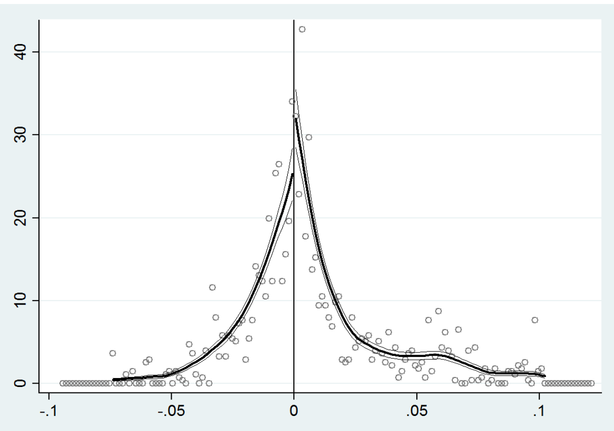
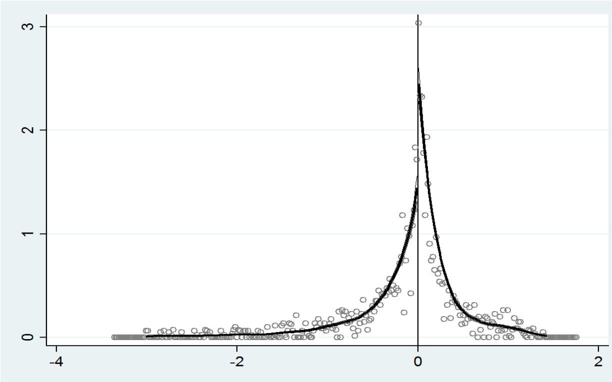


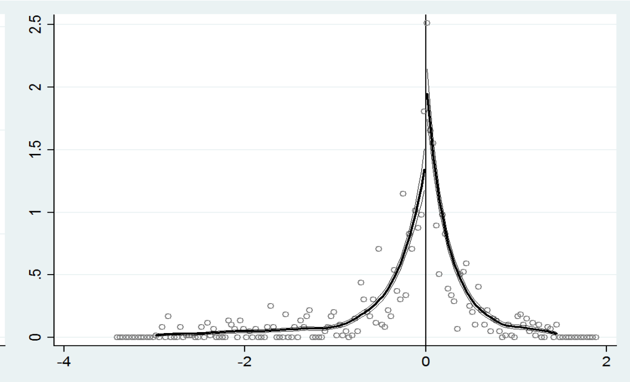
Figure 7: **Actual performance and targets: Concave versus convex grants**

This figure presents the results of a test for a discontinuity at zero in the density of *Actual less target EPS* (panels (a-b)), *Actual less target sales* (panels (c-d)) and *Actual less target profit* (panels (e-f)). In the left-hand-side panel we focus on grants that involve interpolation between the threshold and target value while in the right-hand-side panel we focus on grants that do not involve interpolation.

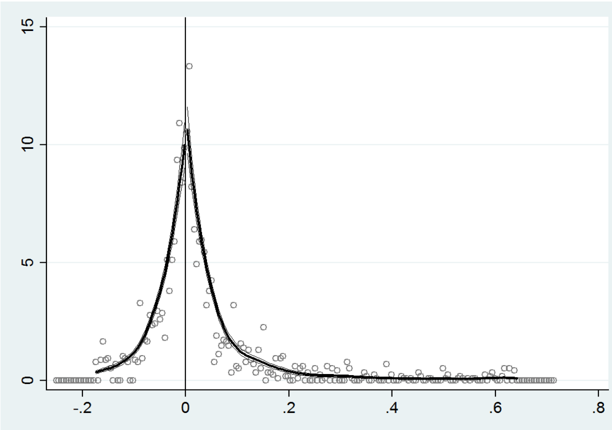
(a) Actual versus target EPS: Concave grants



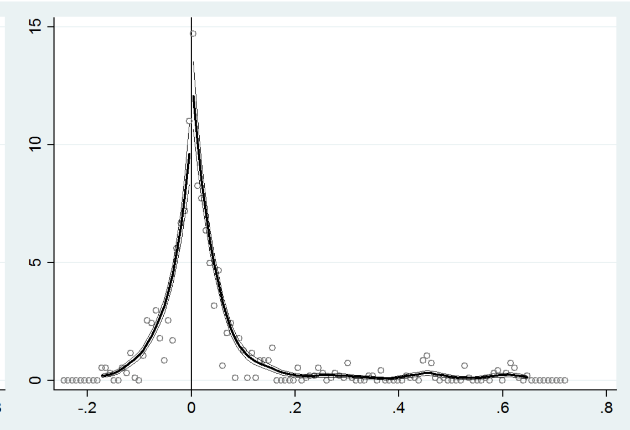
(b) Actual versus target EPS: Convex grants



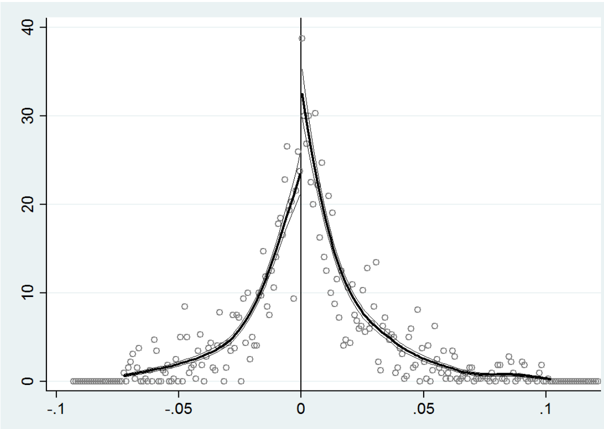
(c) Actual versus target Sales: Concave payoffs



(d) Actual versus target Sales: Convex payoffs



(e) Actual versus target Profit: Concave grants



(f) Actual versus target Profit: Convex grants

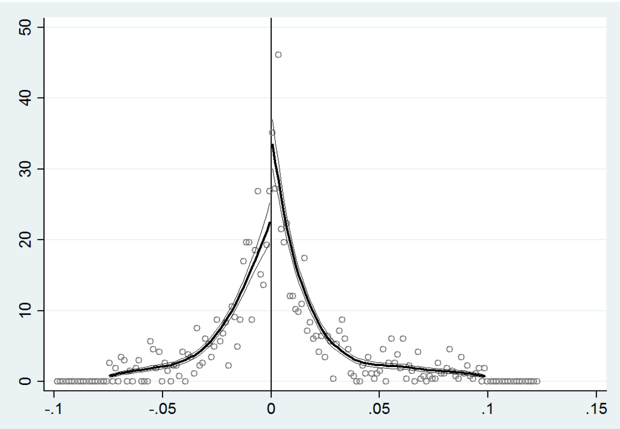
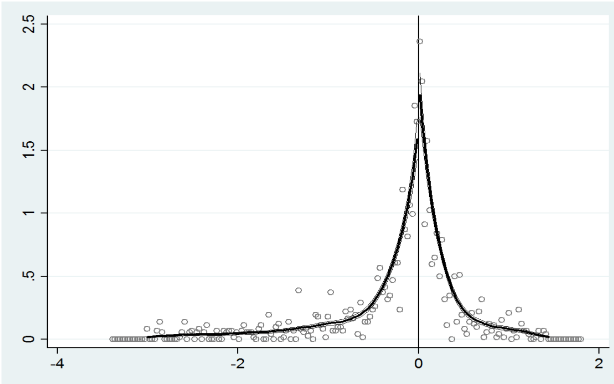


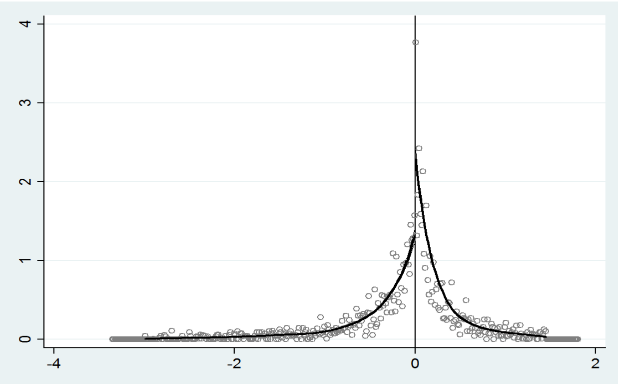
Figure 8: **Actual performance and targets: Long versus short grants**

This figure presents the results of a test for a discontinuity at zero in the density of *Actual less target EPS* (panels (a-b)), *Actual less target sales* (panels (c-d)) and *Actual less target profit* (panels (e-f)). In the left-hand-side panel we focus on long-term grants while in the right-hand-side panel we focus on short-term grants. We classify any grant with a final vesting longer than 11 months as long-term.

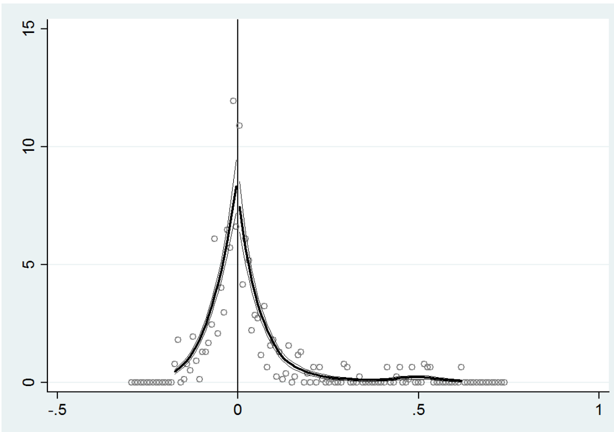
(a) Actual versus target EPS: Long grants



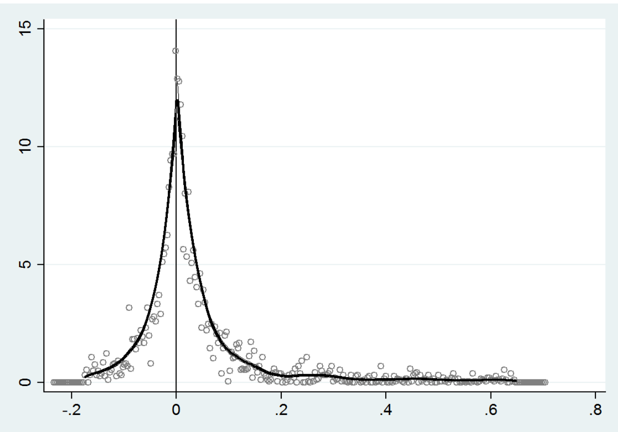
(b) Actual versus target EPS: Short grants



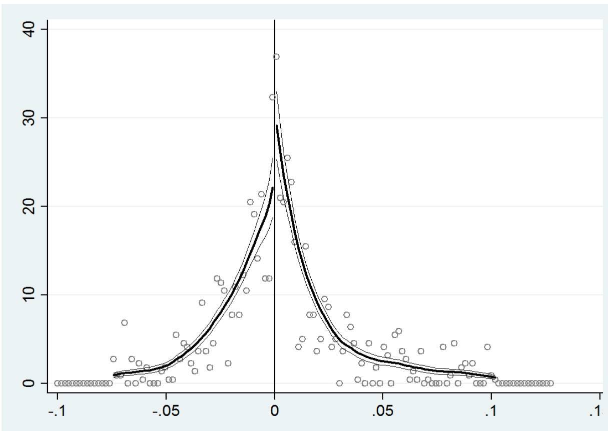
(c) Actual versus target Sales: Long grants



(d) Actual versus target Sales: Short grants



(e) Actual versus target Profit: Long grants



(f) Actual versus target Profit: Short grants

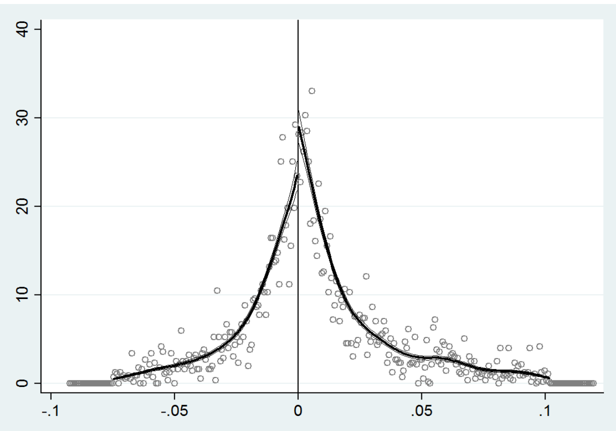
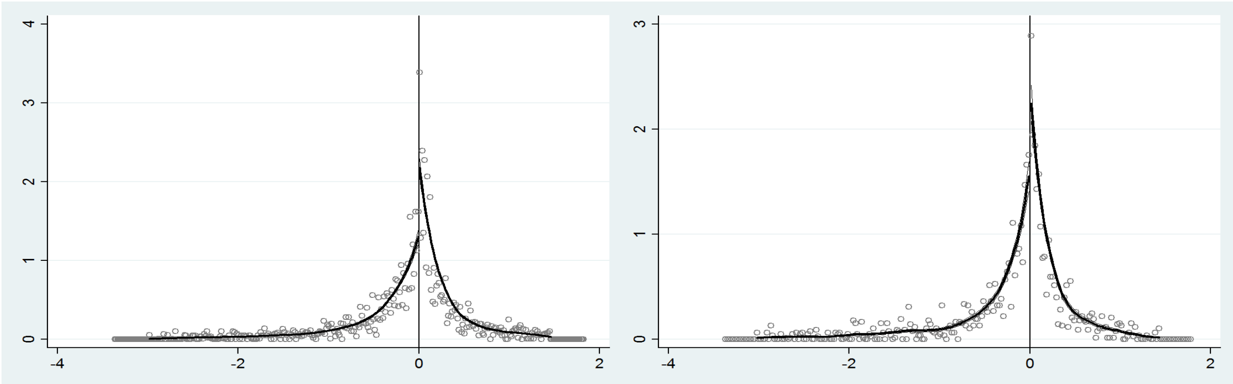


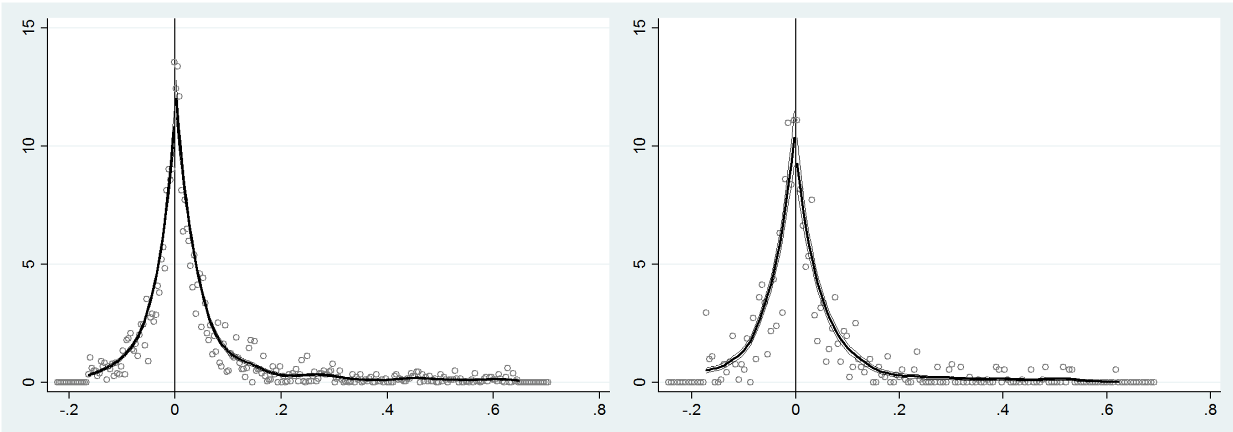
Figure 9: **Actual performance and targets: Cash versus non-cash grants**

This figure presents the results of a test for a discontinuity at zero in the density of *Actual less target EPS* (panels (a-b)), *Actual less target sales* (panels (c-d)) and *Actual less target profit* (panels (e-f)). In the left-hand-side panel we focus on grants that involve cash payout while in the right-hand-side panel we focus on grants that involve non-cash payout.

(a) Actual versus target EPS: Cash grants (b) Actual versus target EPS: Non-cash grants



(c) Percentage actual versus target sales: Cash grants (d) Percentage actual versus target sales: Non-cash grants



(e) Percentage actual versus target profit: Cash grants (f) Percentage actual versus target profit: Non-cash grants

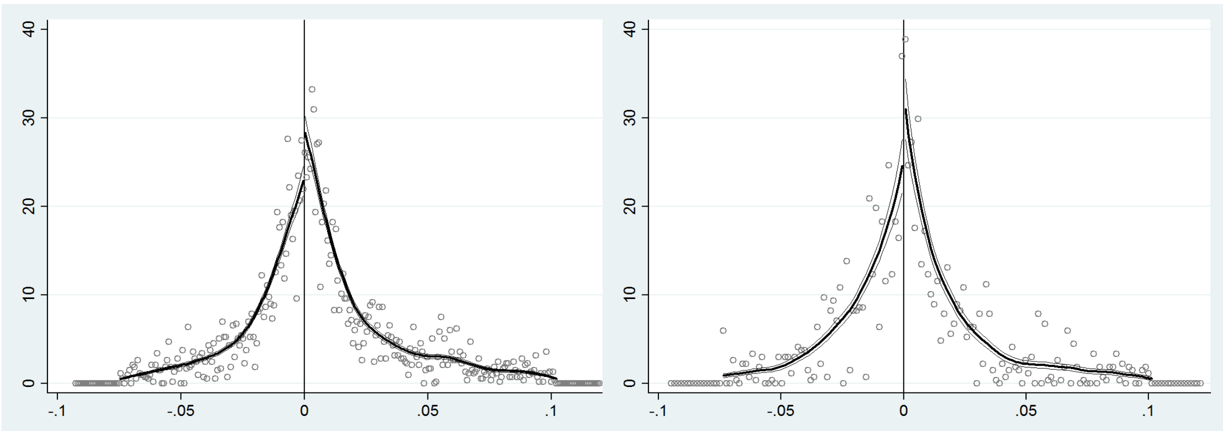
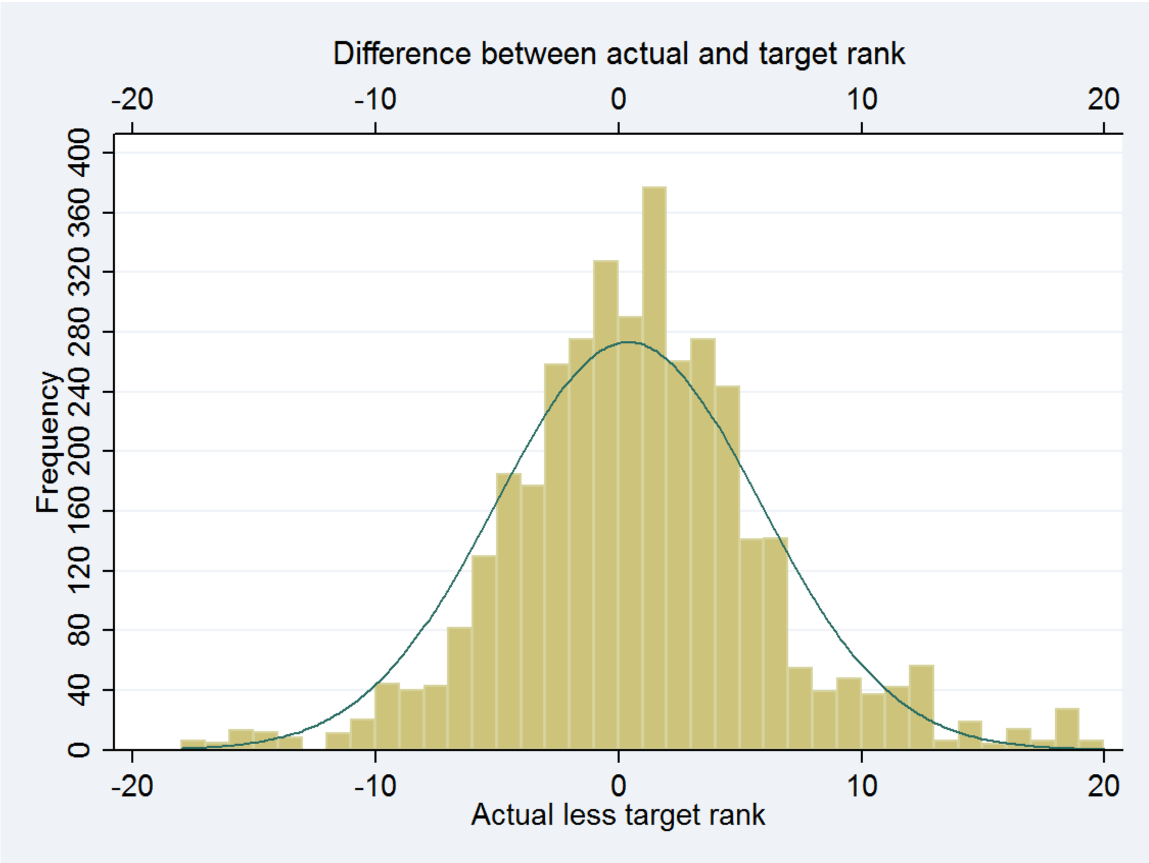


Figure 10: **Difference between actual and target ranks for relative performance grants**

This figure tests for discontinuity in the density of *Actual less target* rank. In Figure (a) we present the histogram of *Actual less target rank* along with a smooth density. The bin width for this histogram is 1. Figure (b) presents the results of McCrary (2008) test for the presence of a discontinuity in the empirical density at zero. Figure (c) presents the histogram of Actual less threshold rank along with a smooth density. The bin width for this histogram is 1.

(a) Histogram of difference between actual and target rank



(b) Test of discontinuity at zero of the difference between actual and target rank

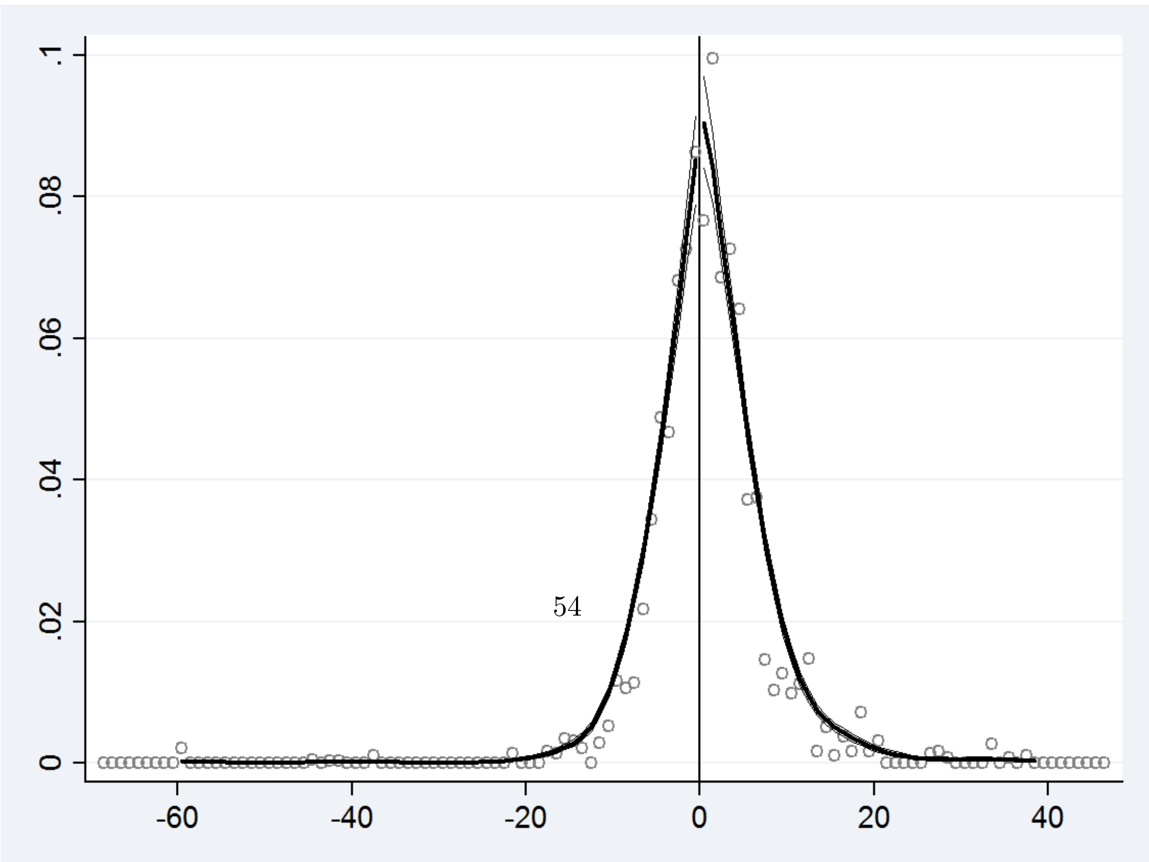


Table 2: **Reported performance and earnings based pay targets**

Table 2 reports the results of an 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 covers the period 2006-2012. The compensation data is from Incentive Lab (IL), Compustat, CRSP and ExecuComp. (***); (**); (*) denote statistical significance at 1%, 5%, and 10% levels respectively.

	Number of Firms					
	(1)	(2)	(3)	(4)	(5)	(6)
No. of goals	.210					
	(.033)***					
EPS goals		.167				
		(.068)**				
Sale goals		.021				
		(.013)				
Profit goals		.303				
		(.042)***				
No. of goals - Single metric			.183			
			(.031)***			
No. of goals - Multiple metrics			.097			
			(.038)**			
No. of goals - Concave grants				.199		
				(.035)***		
No. of goals - Convex grants				.123		
				(.042)***		
No. of goals - Long grants					.243	
					(.053)***	
No. of goals - Short grants					.113	
					(.025)***	
No. of goals - Cash only grants						.080
						(.025)***
No. of goals - Non-cash grants						.256
						(.049)***
Const.	3.226	3.416	3.231	3.223	3.223	3.247
	(.102)***	(.058)***	(.102)***	(.102)***	(.102)***	(.103)***
Obs.	10914	10914	10914	10914	10914	10914
R^2	.519	.514	.518	.519	.519	.516
Δ Coefficient			.086	.076	.130	-177
			(.044)**	(.041)*	(.053)**	(.052)***

Table 3: **Evidence for target ratcheting effect**

This table 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. The main independent variable *is Exceed target*. 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 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 is from Incentive Lab (IL), Compustat, CRSP and ExecuComp. (***); (**); (*) denote statistical significance at 1%, 5%, and 10% levels respectively.

(a) Target ratcheting

	(3)	(4)	(5)	(6)
Exceed target	.071	.082	.094	.103
	(.044)	(.044)*	(.050)*	(.050)**
Size		-.004		-.007
		(.012)		(.014)
ROA		1.245		1.155
		(.273)***		(.370)***
Sales growth		.265		.149
		(.105)**		(.129)
Const.	.538	.423	.557	.491
	(.011)***	(.114)***	(.013)***	(.134)***
Obs.	2882	2882	2072	2072
R^2	.379	.393	.394	.403

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 (2007). 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 is from Incentive Lab (IL), Compustat, CRSP and ExecuComp. (***); (**); (*) denote statistical significance at 1%, 5%, and 10% levels respectively.

	(1)	(2)	(3)	(4)
Miss target	.015	.014	.021	-.027
	(.006)**	(.007)**	(.012)*	(.015)*
Actual less target	.007	.010	.020	-.010
	(.005)	(.005)*	(.015)	(.007)
Actual less target × Actual less target	.002	.001	.008	-.0005
	(.003)	(.003)	(.008)	(.001)
Industry ret.	-.017	-.024	-.028	.003
	(.006)***	(.015)	(.018)	(.020)
Return		.015	.024	.010
		(.013)	(.015)	(.028)
Size		.004	.003	-.004
		(.005)	(.005)	(.005)
Volatility		.086	.029	.057
		(.144)	(.153)	(.160)
Tenure		-.036	-.033	-.083
		(.019)*	(.022)	(.014)***
Age		-.00003	-.0006	1.00e-05
		(.0008)	(.0005)	(.001)
CEO Shareholding		.002	.003	.005
		(.002)	(.002)	(.003)**
Duality		.010	.011	.025
		(.007)	(.008)	(.013)*
Const.	.0001	.009	.044	.185
	(.004)	(.090)	(.080)	(.073)**
Obs.	2439	2326	2098	2326
R^2	.307	.352	.338	.468

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 covers the period 2006-2012. The compensation data is from Incentive Lab (IL), Compustat, CRSP and ExecuComp. (***); (**); (*) denote statistical significance at 1%, 5%, and 10% levels respectively.

	Exceed EPS goals		**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 sales goal		**Miss sales 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 profit goal		**Miss profit 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 **

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*, Δ*R&D*, Δ*SG&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 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 is from Incentive Lab (IL), Compustat, CRSP and ExecuComp. (***); (**); (*) denote statistical significance at 1%, 5%, and 10% levels respectively.

	Accruals	ΔR&D	ΔSG&A
	(1)	(3)	(4)
Exceed EPS	.009	-1.009	
	(.004)**	(.590)*	
Exceed profit			-6.220
			(3.016)**
Size	.002	-.046	-.549
	(.002)	(.233)	(.879)
Market to book	.0009	2.637	11.762
	(.002)	(1.090)**	(2.177)***
Std. Dev. cash flow	-.018		
	(.044)		
Std. Dev. sales growth	-8.44e-07		
	(9.97e-07)		
Const.	-.016	-2.810	2.606
	(.021)	(3.380)	(9.782)
Obs.	904	1111	1111
R^2	.139	.068	.231

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