



Uncertainty about managers' reporting objectives and investors' response to earnings reports: Evidence from the 2006 executive compensation disclosures



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ABSTRACT

We examine whether the information content of the earnings report, as captured by the earnings response coefficient (ERC), increases when investors' uncertainty about the manager's reporting objectives decreases, as predicted in Fischer and Verrecchia (2000). We use the 2006 mandatory compensation disclosures as an instrument to capture a decrease in investors' uncertainty about managers' incentives and reporting objectives. Employing a difference-in-differences design and exploiting the staggered adoption of the new rules, we find a statistically and economically significant increase in ERC for treated firms relative to control firms, largely driven by profit firms. Cross-sectional tests suggest that the effect is more pronounced in subsets of firms most affected by the new rules. Our findings represent the first empirical evidence of a role of compensation disclosures in enhancing the information content of financial reports.

1. Introduction

We empirically examine whether the information content of the earnings report, as captured by the earnings response coefficient, increases when investors' uncertainty about the manager's reporting objectives decreases. The intuition behind this hypothesis is formalized by Fischer and Verrecchia (2000). In their model, the manager's reporting bias (arising from a variety of incentives) can reduce the information content of the earnings report, with the effect increasing in the degree of investors' uncertainty about her reporting objectives. This is because such uncertainty prevents investors from perfectly adjusting for the bias the manager adds to the report. Thus, the manager's biasing activity adds noise to the financial report. An implication of the model is that the information content of the earnings report should increase as investors' uncertainty about the manager's reporting objectives is reduced.

To examine this hypothesis we use the executive pay disclosures mandated by the Securities and Exchange Commission (SEC) in 2006 as an instrument to the change in investors' uncertainty about the manager's reporting objectives.¹ In doing so, we make two assumptions. First, the 2006 disclosures substantially increased the quality of information about managers' (compensation-driven)

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¹ A long line of research links reporting choices to CEO cash and equity compensation incentives (for a review, see Murphy, 2013), with survey evidence indicating that 88.6% of financial executives believe that executive pay affects firms' (mis)reporting choices (Dichev, Graham and Rajgopal, 2013). Fischer and Verrecchia (2000) explicitly mention compensation disclosures as a potential source of uncertainty about reporting objectives.

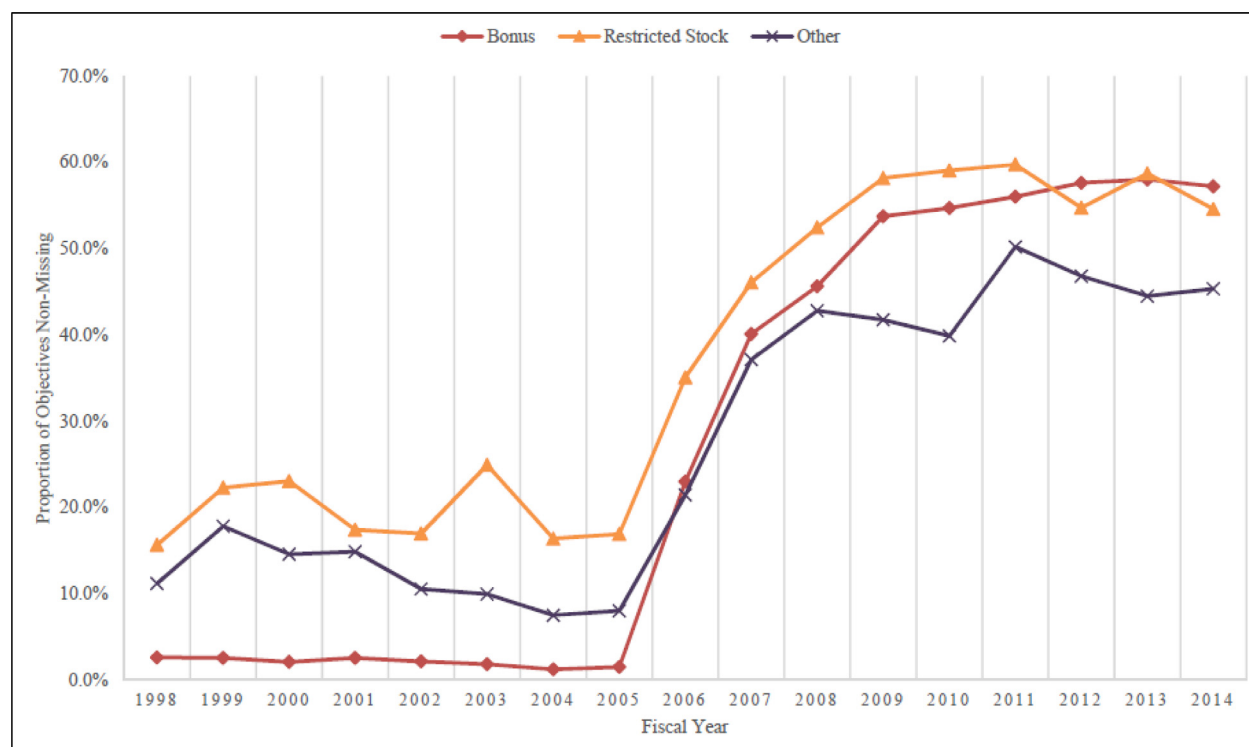


Fig. 1. Proportion of absolute performance objective targets non-missing in IncentiveLab

This is the Figure 1 of Appendix 2 in [Gipper \(2016\)](#). This figure shows the time-series trends of the proportion of non-missing absolute performance targets for bonus and short-term cash incentives, restricted stock, and other plans as reported in the ISS Incentive Lab database.

reporting objectives. One example in support of this assumption is the dramatic increase in disclosures of absolute and relative performance targets in cash and equity compensation plans after 2006 – one of the requirements of the new rules (see [Figs. 1 and 2](#)). Second, the new rules only affected the quality of compensation disclosures, without providing new information about the firm's strategy and investment opportunities, and without affecting managerial incentives (e.g. by changing compensation contracts) and thus altering the economics of the firms (we will discuss and examine this assumption later).²

Under these assumptions, the information content of the manager's financial report, as captured by the earnings response coefficient, increases as a result of a decrease in investors' uncertainty about the manager's reporting objectives caused by enhanced disclosure of the manager's compensation incentives under the 2006 rules (hereinafter 'CD&A rules').³

Our identification strategy exploits the staggered nature of the adoption of the CD&A rules. Because the rules were effective for fiscal years (FY) ending on or after December 15, 2006, the timing of their adoption depended on the firm's FY-end, which (by and large) is exogenously (pre)determined. Following [Gipper \(2016\)](#), we use December FY-end firms as the treatment sample, as these firms were the first to be subject to the new rules. In contrast, we use firms with a September, October and November FY-end as the control sample, as these firms were the last to comply with the new rules. This approach allows us to maximize the length of the overlapping period where some firms (the treated sample) were subject to the rule and some (the control sample) were not. Using this approach, we employ a difference-in-differences research design comparing the change in investors' response to quarterly earnings releases (the ERC) of treatment and control firms from the year prior to December 15, 2006 (when no firm was affected) to the subsequent year, during which treated firms were affected but control firms were not (yet). Because firms with different FY-ends may have different characteristics we use entropy balancing, a quasi-matching technique which re-weights control observations to ensure covariate balance between treatment and control firms. Unlike standard matching procedures, entropy balancing preserves the size of the control sample, which is important in studies with significant imbalance between the size of treatment and control samples (most firms have a December fiscal-year end).

² Another implicit assumption, in line with [Fischer and Verrecchia \(2000\)](#)'s rational expectations framework, is that investors use the information in compensation disclosures to understand the manager's reporting objectives. Given the documented role of compensation motives in reporting choices ([Murphy, 2013](#)), it seems natural that investors would try to understand such motives. Anecdotal evidence based on a forensic firm's report to its clients suggests that sophisticated institutional investors care about the potential effect of compensation-driven incentives on reported numbers ([Appendix 1](#)).

³ Because our analysis is based on the joint hypothesis that (i) a reduction in investors' uncertainty about the manager's reporting objectives increases the information content of earnings reports and (ii) the two assumptions above hold, a no-result could have several explanations.

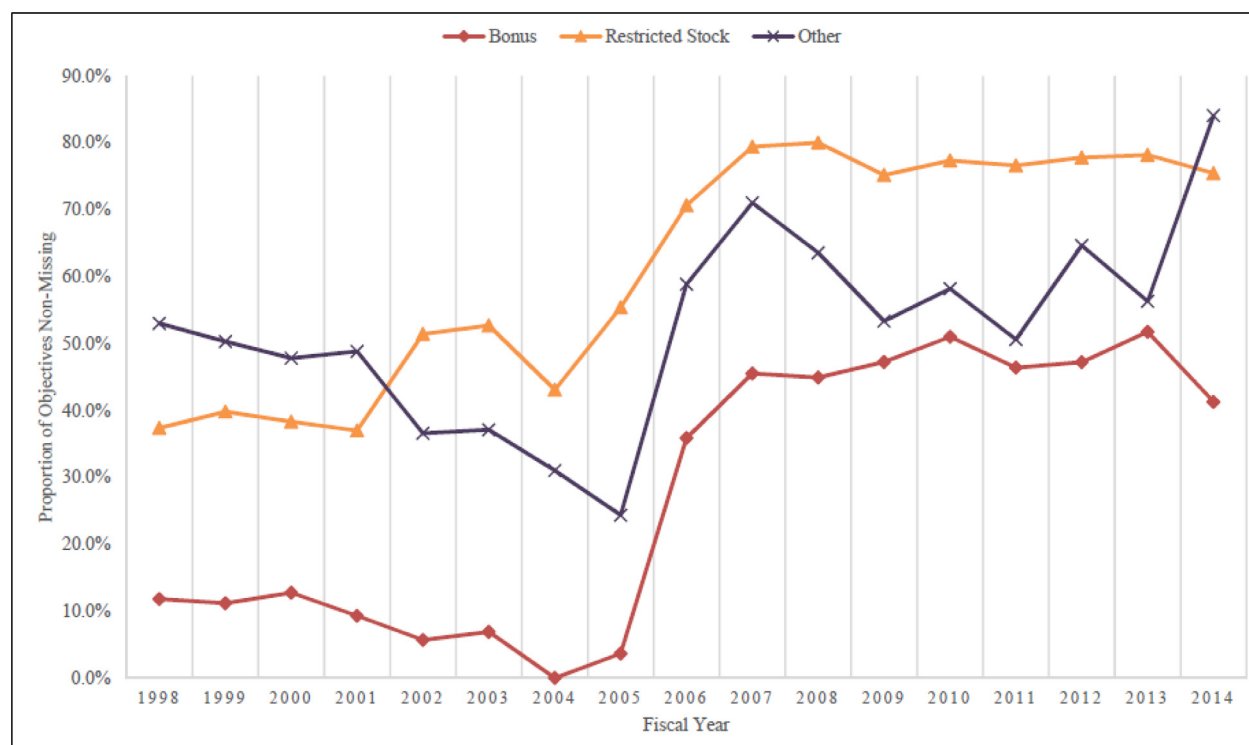


Fig. 2. Proportion of relative performance objective targets non-missing in IncentiveLab

This is the Figure 2 of Appendix 2 in [Gipper \(2016\)](#). This figure shows the time-series trends of the proportion of non-missing relative performance targets for bonus and short-term cash incentives, restricted stock, and other plans as reported in the ISS Incentive Lab database.

Consistent with our hypothesis, we find a statistically and economically significant increase in ERC for treatment firms relative to control firms of 0.925, corresponding to about a 20% increase over the pre-CD&A level of 4.64.⁴ Additional tests indicate that the increase in ERC is driven by profit firms. Among loss firms, the change in ERC does not differ between treatment and control sample. This is consistent with prior evidence that ERCs are significantly attenuated for loss firms ([Hayn, 1995](#)). It also suggests that enhanced compensation disclosures have limited usefulness in reducing investors' uncertainty about the manager's reporting objectives at loss firms, perhaps because these firms, rather than linking manager's pay to formulaic financial targets, tend to use subjective evaluation and non-financial metrics ([Gibbs et al., 2004](#); [Matějka, Merchant and Van der Stede, 2009](#)). Also, when we account for differential market response to extreme unexpected earnings, we find that treated firms experienced a relative decrease in the degree to which extreme surprises reduce the ERC. That is, better disclosures of compensation incentives appear to reduce the “discount” investors attach to large earnings surprises.

Our results are robust to extending the event window (i.e. including 2005 in the pre-CD&A period), expanding the control sample (i.e. adding firms with a FY-end between June and August) and alternative design choices (different clustering, winsorizations, etc.). In a placebo test, we find no differential change in ERC between treatment and control firms between 2005 and 2006 (instead of 2006 and 2007), providing indirect support for the parallel trend assumption underlying our design. Finally, a series of cross-sectional tests indicate that the increase in ERC is driven by firms that most improved the quality of their compensation disclosures (and thus experienced greater reduction in investors' uncertainty about the manager's reporting objectives), i.e. firms that did not receive a SEC comment letter criticizing such disclosures, firms disclosing CFO pay information for the first time, and firms with higher excess CEO pay (a proxy for greater pressure to improve pay disclosures).

As noted earlier, a key assumption in our study is that the CD&A rules *only* affected the quality of compensation disclosures. In [Section 6](#) we discuss this assumption. We first consider whether the CD&A rules affected compensation contracts and thus managerial incentives. As the rules were finalized in August 2006, both treatment and control firms may have adjusted their compensation contracts already during FY 2007 (i.e. with respect to compensation outcomes, they were both treated at the same time). If so, any effects of the CD&A rules on compensation could not explain the differential changes in ERC. Notwithstanding this argument, we examine changes from FY 2006 to FY 2007 in a number of variables capturing executive pay structure and find little evidence of differential effects between treatment and control firms. We also examine directly potential real effects of changes in managerial

⁴ As a benchmark, [Wilson \(2008\)](#) reports an ERC decrease of about 1.2 after a restatement (representing an 18% decrease), [Chen, Cheng and Lo \(2014\)](#) report an ERC decrease of about 1.8 after a *material* restatement (representing a 56% decrease) and [Gipper, Leuz and Maffett \(2015\)](#) report an ERC increase ranging between 0.8 and 1.6 around the introduction of PCAOB inspection regime.

incentives. Across a set of variables, capturing firms' investment policy and risk profile, we fail to detect any differential changes between treatment and control firms. Finally, better pay disclosures may indirectly provide new information about the firm's fundamentals and strategy. But we argue that, in the framework of [Fischer and Verrecchia \(2000\)](#), such scenario would predict a reduction in ERC, biasing against our hypothesis. In spite of the above arguments and evidence, we acknowledge we cannot fully rule out the possibility that the new rules may have had some consequences that we do not account for, with a potential effect on the ERC.

Our study is the first to link investors' uncertainty about the manager's reporting objectives to the information content of earnings reports, a question highlighted in theoretical work ([Fischer and Verrecchia, 2000](#)) but neglected in the empirical literature.⁵ It is also the first to highlight a potential role of compensation disclosures in improving the usefulness of financial reports to investors. By doing so, we contribute to a long line of research on the determinants of ERC (see [Kothari, 2001](#) for a review). While most of this research has focused on the association between ERC and firm characteristics, recent studies have begun to examine changes in ERC around exogenous shocks to the regulatory and information environment. For example, [Gipper et al. \(2015\)](#) document an increase in ERC around the introduction of the PCAOB and its inspection regime, consistent with the regulatory change causing an increase in financial reporting credibility. In a similar vein, [Wilson \(2008\)](#) and [Chen et al. \(2014\)](#) find a decrease in ERC in firms with restatements, viewed as a negative shock to reporting credibility. Our study adds to this line of research by examining the impact of a regulatory change to compensation disclosures on ERC. This type of evidence is important because the market response to unexpected earnings releases remains relatively weak (i.e. the ERC is fairly low; [Kothari, 2001](#)). In this respect, an implication of our study is that policy makers can increase the information content of financial reports not only by improving their credibility (via disclosure of higher-quality financial information and greater public oversight), but also by reducing investors' uncertainty about managers' reporting objectives (for example via better disclosures of compensation incentives).

Our findings also contribute to the literature examining compensation disclosures, and, especially, the 2006 CD&A rules. Most of these studies focus on the effect on executive pay practices (e.g. compensation peer benchmarking, [Faulkender and Yang, 2013](#); level of CEO pay, [Gipper, 2016](#); perks, [Grinstein et al., 2011](#); use of compensation consultants, [Sandino and Murphy, 2010](#)). In contrast, we highlight the effect on investors' understanding of managers' reporting incentives and, as a result, on the information content of earnings reports. In a similar vein, [Bloomfield \(2016\)](#) also uses the CD&A rules as a shock to compensation contracts' observability. He finds that revenue-based compensation is more prevalent in CEO pay packages when the benefits of committing to aggressive product market behavior are greater, but only after the CD&A rules, consistent with theoretical predictions that this approach to commitment is only effective if the CEO's pay contract is observable and credible to rivals.

[Section 2](#) provides theoretical and regulatory backgrounds. [Section 3](#) describes the research design. [Section 4](#) reports the main results, followed by cross-sectional tests in [Section 5](#). [Section 6](#) and [Section 7](#) discuss, respectively, alternative explanations for our findings and additional analyses, followed by concluding remarks in [Section 8](#).

2. Backgrounds

2.1. Theoretical background

[Fischer and Verrecchia \(2000\)](#) analytically show that the information content of the manager's earnings report is decreasing in the market's uncertainty about the manager's reporting objectives.

To illustrate the intuition behind this result, consider a firm whose true value is unknown ex ante. The prior distribution of firm value $\tilde{v} \sim N(\mu_v, \sigma_v^2)$ is common knowledge.⁶ The manager privately observes the earnings $\tilde{e} = \tilde{v} + \tilde{n}$, $\tilde{n} \sim N(0, \sigma_n^2)$, which is a noisy signal of the firm value v . The manager then decides the earnings report r . The manager's bias in the report is denoted by $b \equiv r - e$. By adding bias to the report, the manager bears a private cost $C(b) = cb^2/2$. After observing the report, investors form a conditional expectation of firm value, which determines the market price of the firm, denoted by $P(r) = E[\tilde{v}|r]$. The manager's objective is to maximize the value of $\tilde{x}P(r)$ net of the cost of biasing the report. The term $\tilde{x}P(r)$ represents the manager's benefit from obtaining a certain price as a result of biasing the report, for example, due to the structure of her compensation. Note that x can be positive or negative. For example, the manager may prefer higher earnings and thus a higher price because of his annual bonus ([Healy, 1985](#)) and equity-based pay ([Cheng and Warfield, 2005](#); [Bergstresser and Philippon, 2006](#); [Bizjak et al., 2015](#)), or may prefer lower earnings and thus a lower price because of an expected grant of new stock options ([Aboody and Kasznik, 2000](#); [Balsam et al., 2003](#); [McAnally et al., 2008](#)).

The manager observes the realization x , while investors only know the prior distribution $\tilde{x} \sim N(\mu_x, \sigma_x^2)$. In our setting, this is equivalent to assuming that only the manager knows precisely the details of his or her compensation plan and how they affect reporting objectives. Investors have only limited information about the compensation plan. Hence, σ_x^2 captures investors' uncertainty about the manager's reporting objectives. Under this scenario, [Fischer and Verrecchia \(2000\)](#) show that in equilibrium the manager chooses bias $b^* = \beta x/c$ (resulting in an earnings report $r^* = e + \beta x/c$), and the market price is: $P(r^*) = \mu_v + \beta(r^* - (\mu_v + \beta\mu_x/c))$, where β is determined by $\beta = \sigma_v^2/(\sigma_v^2 + \sigma_n^2 + \beta^2\sigma_x^2/c^2)$.

⁵ Other empirical studies (e.g. [Bird, Karolyi, Ruchti and Sudbury, 2015](#); [Fang, Huang and Wang, 2017](#); [Stein and Wang, 2016](#)) rely on the [Fischer and Verrecchia \(2000\)](#) model or variations of the model, but they do not focus on the role of investors' uncertainty about the manager's reporting objectives.

⁶ In [Fischer and Verrecchia \(2000\)](#) the prior distribution of firm value is $\tilde{v} \sim N(0, \sigma_v^2)$. Assuming a non-zero mean, i.e. $\tilde{v} \sim N(\mu_v, \sigma_v^2)$, allows us to explicitly obtain the price function expressed in terms of price change and unexpected earnings (rather than price level and earnings), which literally fits our returns-earnings surprise empirical framework.

If there is no uncertainty about the manager's reporting objectives (i.e. $\tilde{x} = \mu_x$ is common knowledge, and thus $\sigma_x^2 = 0$), then $\beta = \sigma_v^2/(\sigma_v^2 + \sigma_n^2)$. That is, β only depends on the precision of the earnings report as a signal of firm value (i.e. $1/\sigma_n^2$) and the precision of the prior on the firm value (i.e. $1/\sigma_v^2$), but does not depend on x (even though in equilibrium the manager does add bias to the report; Stein, 1989). Intuitively, because there is no uncertainty about the manager's reporting objectives, the market is able to correctly conjecture the amount of reporting bias and back out the “true” earnings. Hence, the reporting bias does not add noise to the report and does not reduce its information content. In contrast, if $\sigma_x^2 > 0$, β also depends on σ_x^2 . Because of the uncertainty about \tilde{x} , the market is unable to perfectly adjust for the bias the manager adds to the report. As a result, the market's ability to infer the true earnings from the reported earnings is weakened, resulting in an inverse relation between β and σ_x^2 .⁷

As we proceed to empirically test the relation between β and σ_x^2 , it is useful to make three observations. First, to see why β in essence captures the earnings response coefficient (ERC), the market price function can be reorganized as: $P(r^*) - \mu_v = \beta(r^* - (\mu_v + b^*))$, where $P(r^*) - \mu_v = P(r^*) - E[P(r^*)]$ represents the price change after earnings announcement, and $r^* - (\mu_v + b^*) = r^* - E[r^*]$ represents the earnings surprise. That is, to the extent that the stock price prior to the earnings announcement proxies for the expected price as a result of reported earnings, $E[P(r^*)]$, and that analysts' consensus forecasts proxy for expected reported earnings, $E[r^*]$, then empirically β is the coefficient in a regression of earnings announcement returns on earnings surprise (scaled by price), i.e. the ERC.

Second, Fischer and Verrecchia (2000) mentions compensation incentives as an example of reporting objectives, but it does not explicitly model the role of compensation disclosures. However, adjusting the model to make compensation disclosures a source of reduction of σ_x^2 is fairly simple.⁸

Finally, as the ERC is determined by $\beta = \sigma_v^2/(\sigma_v^2 + \sigma_n^2 + \beta^2\sigma_x^2/c^2)$, an increase in ERC can also result from an increase in σ_v^2 or c , and a decrease in σ_n^2 . The intuition is as follows: if prior uncertainty on firm value increases, then investors will put more weight on the earnings report signal, resulting in a higher ERC; if the marginal cost of adding bias to the earnings report increases, then managers' biasing activity and thus the associated noise in the earnings report will decline, implying a higher ERC; finally, if the inherent earnings quality increases, investors will put more weight on the earnings report signal, leading to a higher ERC. As noted in the Introduction, we initially assume these parameters are not affected by the new disclosures (i.e. the CD&A rules have no direct or indirect real effects) and defer the examination of this assumption to Section 6.

2.2. Regulatory background: the 2006 CD&A rules

On January 27, 2006, the SEC proposed revisions to the executive pay disclosure rules introduced in 1992. The objective was to design a disclosure framework that would reflect the significant changes to compensation practices since 1992 (e.g. increase in equity awards, more complex performance-based plans) and allow investors to better understand and monitor CEO compensation and its relation to performance. After receiving a record number of over 20,000 comment letters, the SEC released the final rules in August 2006, effective for FY years ending on or after December 15, 2006.

The new rules were far-reaching in scope, requiring “dramatically different disclosures” (McGuireWoods, 2007).⁹ The centerpiece is a new “Compensation Discussion and Analysis” (CD&A) section (to be filed with the proxy statement and certified by CEO and CFO). The CD&A is meant to be a narrative, in-depth description of the objectives and terms of executive compensation programs, complementing the tabular disclosures in the same way the MD&A section of a 10-K filing complements the financial statements. Especially relevant to our study, firms must disclose more details about performance metrics used (including their weight, when multiple metrics are used, and their exact definition) and performance targets (including the time over which they need to be achieved and the payouts triggered by their achievement) for any type of incentive plan (cash, equity, short-term, long-term, etc.).¹⁰ Figs. 1 and 2 (based on Gipper, 2016) highlight the impact of the new rules on the frequency of disclosure of absolute and relative performance targets in cash and equity incentive plans.

The CD&A section must also include forward-looking information (i.e. new plans adopted after the end of the FY) and explain the

⁷ If $\sigma_x^2 > 0$ (as in Fischer and Verrecchia, 2000), the precision of the earnings report as a signal of the firm value declines because the signal contains not only the endowed measurement noise \tilde{n} but also the reporting noise $\beta(\tilde{x} - \mu_x)/c$ (whose variance is $\beta^2\sigma_x^2/c^2$). Similar predictions have been obtained with additional or alternative model features including earnings-based compensation (Ewert and Wagenhofer, 2005), investor speculation (Fischer and Stocken, 2004), and voluntary disclosure (Einhorn, 2007).

⁸ Let us assume the prior distribution is $\tilde{x} \sim N(\mu_0, \sigma_0^2)$, and compensation disclosure, s , serves as a noisy signal of the manager's reporting objectives, that is, $s = \tilde{x} + \tilde{\epsilon}$, where $\tilde{\epsilon} \sim N(0, \sigma_\epsilon^2)$. Then, the posterior distribution is $\tilde{x}|s \sim N(\mu_{x|s}, \sigma_{x|s}^2)$, where $\mu_{x|s} = (\sigma_s^2\mu_0 + \sigma_0^2s)/(\sigma_s^2 + \sigma_0^2)$, $\sigma_{x|s}^2 = \sigma_s^2\sigma_0^2/(\sigma_s^2 + \sigma_0^2)$. Improved compensation disclosures can be captured by a lower σ_s^2 , which results in a lower $\sigma_{x|s}^2$ and, thus, a higher equilibrium β or ERC.

⁹ In this section we focus on the provisions most relevant to our research question. The CD&A rules also modified disclosures regarding the use of compensation consultants, related party transactions, director independence and other corporate governance matters. The final rules are available at: <https://www.sec.gov/rules/final/2006/33-8732a.pdf>.

¹⁰ Firms are allowed not to disclose performance targets if disclosure would result in competitive harm to the company. The 1992 rules had a similar provision, but the standard for non-disclosure under the 2006 rules is much tighter (it is the “Freedom of Information Act” standard that a company needs to satisfy when it requests confidential treatment of trade secrets or commercial or financial information that would otherwise be disclosed in documents filed with the SEC). The large increase in disclosures of performance targets (see Fig. 1 and Fig. 2) confirms it became more difficult to justify non-disclosure. Also, under the 2006 rules, if performance targets are not disclosed, the firm must explain how difficult it is to achieve the targets.

firm's policy regarding the timing of option grants (a provision included in response to the option backdating scandal). As a result, investors will know if the firm has a policy to grant options at the same time each year (or contingent upon the same event), which may affect their interpretation of earnings reports preceding and following the scheduled grant date.

The second important change involves tabular disclosures.¹¹ A new table (“Outstanding equity awards at fiscal year-end”) requires detailed information on each tranche of equity holdings, including their vesting schedule. Combined with the requirement on performance metrics and targets, this means that at any point in time an investor knows which equity awards are about to vest (and when), the performance criteria (if any) triggering vesting and the potential payout. Finally, the CD&A rules expand the requirements for timely disclosures via 8-K filings of compensation arrangements made during the year, further augmenting the information set available to investors.

To sum up, under the CD&A rules, at any point in time (e.g. before an earnings release) investors have substantially more precise information about the compensation consequences of reported performance to the manager, and thus, we argue, about her reporting objectives.

3. Research design and sample description

3.1. Research design

Our identification strategy exploits the staggered adoption of the CD&A rules. Because the rules were effective for FY ending on or after December 15, 2006, the timing of their adoption depended on the firm's FY-end. Following Gipper (2016), we use December FY-end firms as the treatment sample, as these firms were the first to be subject to the new rules. In contrast, we use firms with a September, October and November FY-end as the control sample, as these firms were the last to comply with the new rules.¹² This approach allows us to maximize the length of the overlapping period where some firms (the treated sample) were subject to the rule and some (the control sample) were not (yet). Thus, we can examine how investors' response to quarterly earnings releases changed for treatment versus control firms from the year prior to December 15, 2006 (when no firm was affected) to the subsequent year, when treated firms were affected but control firms were not (yet).¹³

In particular, we adopt the following difference-in-differences design using data from FY 2006 and 2007 for treatment and control firms defined as above:

$$\begin{aligned} CAR_{i,t} = & \beta_0 + \beta_1 UE_{i,t} * POST_{i,t} * TREAT_{i,t} + \beta_2 UE_{i,t} + \beta_3 TREAT_{i,t} + \beta_4 POST_{i,t} \\ & + \beta_5 TREAT_{i,t} * POST_{i,t} + \beta_6 UE_{i,t} * POST_{i,t} + \beta_7 UE_{i,t} * TREAT_{i,t} \\ & + \beta_m Controls + \beta_n UE_{i,t} * Controls + \varepsilon_{i,t} \end{aligned} \quad (1)$$

$CAR_{i,t}$ is the 3-day market-adjusted stock return around the date of quarterly earnings announcements. $UE_{i,t}$ is the unexpected earnings, which is calculated as the difference between the actual quarterly EPS and the median of one-quarter-ahead analyst forecasts of quarterly EPS scaled by stock price two days prior to the earnings announcement. $TREAT_{i,t}$ is an indicator variable which equals one if firm i has a December FY-end, and zero if firm i has a September, October or November FY-end. $POST_{i,t}$ is an indicator variable which equals one if quarter t is in FY 2007, and zero if quarter t is in FY 2006 (see Fig. 3 for a timeline).¹⁴ We focus only on 2006 and 2007 to keep the window as tight as possible and reduce the risk of capturing the effect of earlier events (e.g. the PCAOB inspection regime, FAS123R) or subsequent ones (e.g. the 2008 financial crisis).¹⁵ Our main variable of interest is β_1 , the coefficient of $UE_{i,t} * POST_{i,t} * TREAT_{i,t}$, which captures the change in ERCs for treated firms relative to control firms from the Pre (FY 2006) to the Post (FY 2007) CD&A period.

¹¹ The CD&A rules mandate disclosure of the dollar value of equity grants and a “total compensation” figure (not required under the 1992 rules) in the Summary Compensation Table. They also mandate new tabular and narrative disclosures of post-employment contracts (pensions, severance and change-in-control payments), new tabular disclosure of director compensation and greater disclosures about perks.

¹² To the extent that some control firms decided to adopt the CD&A rules “early”, the power of our tests may be reduced. We checked a random sample of 50 firms and found only one such case.

¹³ Because we are able to compare quarterly ERCs for treatment and control firms over *overlapping* periods, our specification naturally takes into account any time effect: for each quarterly earnings release by a treatment firm we have a quarterly earnings release by control firms in the adjacent months (or even weeks). Using annual ERCs would result in measuring the ERC for treatment firms and control firms at a systematically different time (with the treatment firms being measured later). This is especially a problem because the ERC for fiscal year 2007 for treatment firms would be measured in 2008 (at the onset of the financial crisis) and may be systematically different because of its timing, rather than because of the effect of the CD&A rules. Also, to keep a tight window, a test using annual ERC would be essentially based only on two observations (fiscal year 2006 and fiscal year 2007), further increasing the risk of spurious inferences.

¹⁴ The first quarter of FY 2007 for December FY-end firms is included in the Post period as long as the proxy statement is filed before the first-quarter earnings release and, thus, investors are aware of the new compensation information before the earnings release. We remove 414 observations (about 3% of the original sample) where this condition is not met.

¹⁵ Avoiding the effect of the financial crisis is also one reason why we do not perform a similar difference-in-differences study around the adoption of the CD&A rules by September–November FY-end firms in late 2007. Another reason is that the December FY-end firms continued to improve their disclosures in the second year under the CD&A rules (see Fig. 1 and 2), partly in response to SEC comment letters issued after the first year and urging for better compliance with the new rules (Robinson et al., 2011). In other words, during the second year arguably both sets of firms receive some ‘treatment’ (see Gipper (2016) for a similar argument).

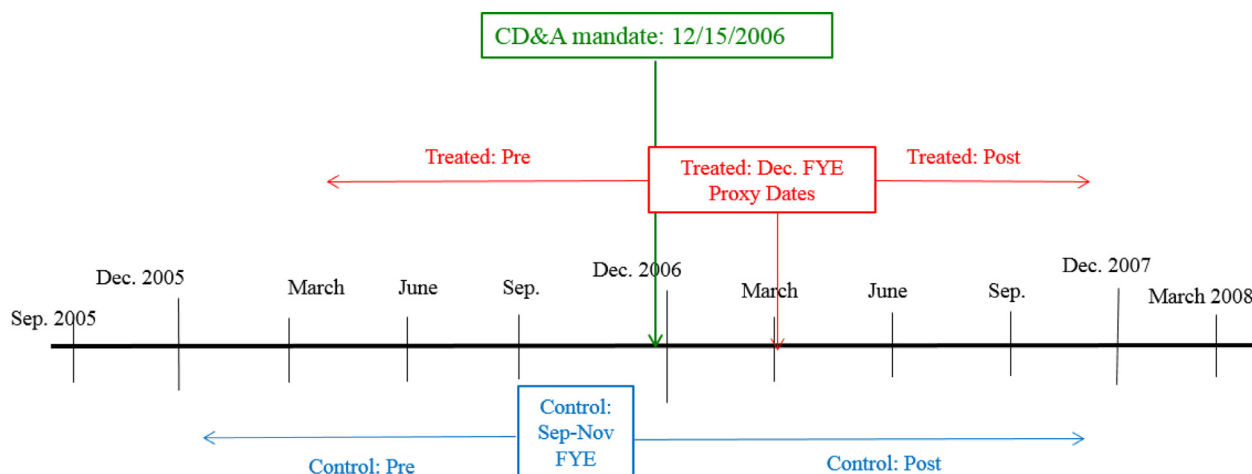


Fig. 3. Timeline

This figure shows the timeline underlying the research design. We utilize the CD&A introduction date of December 15, 2006 to design a difference-in-differences test. We use firms with December fiscal year-end as the treatment firms, and firms with September to November fiscal year-end as the control firms. In the difference-in-differences tests, we compare the changes in ERCs between treatment and control firms from the Pre to the Post Period.

Following the literature on ERCs (e.g. Collins and Kothari, 1989; Easton and Zmijewski, 1989; Gipper et al., 2015), we include several control variables and their interaction with *UE* (see Appendix 2 for details): firm size (*Size*), market-to-book ratio (*Market-to-Book*), beta (*Beta*), leverage (*Leverage*), earnings persistence (*Persistence*), a loss indicator (*Loss*) and analysts' forecast dispersion (*Dispersion*).¹⁶ We also include industry fixed effects (using Fama-French 12 industries) and fiscal year-quarter fixed effects and their interactions with *UE*, to control for differences in investors' reaction to earnings reports in different fiscal quarters (particularly the fourth quarter) and across different industries.

To mitigate concerns about outliers (especially for *UE*, Kothari, 2001), we employ a weighted-least squares “robust” regression, which places less weights on observations with large absolute residuals (e.g. Gipper et al., 2015; Leone et al., 2015). To account for cross-sectional dependencies among firms announcing on the same day, we report standard errors clustered by earnings announcement dates (DellaVigna and Pollet, 2009; Hirshleifer et al., 2009). Doing so has the added benefit of resulting in a large number of clusters and more consistent standard errors (Petersen, 2009). Our inferences are similar when we cluster by week, month or quarter (untabulated).

Note that our difference-in-differences design is well suited to overcome measurement errors in the control variables. This is especially relevant for *UE*, the unexpected earnings. Analysts' forecasts are known to be an imperfect proxy for investors' expectations, because analysts do not rationally incorporate all information in their forecasts and are subject to non-random biases (Bradshaw, 2011; Brown et al., 2015). The resulting measurement error will bias downward the ERC (Kothari, 2001). Nevertheless, if the degree to which analysts' forecasts proxy for investors' expectations does not change differentially for treatment and control firms around the CD&A rules, in a difference-in-differences design these shortcomings will not affect the estimate of the *change* in ERC (Gipper et al., 2015).

This assumption seems generally plausible, but it is worth considering two scenarios under which it may be violated. First, the CD&A rules provided both analysts and investors with more precise information potentially useful in adjusting for the manager's reporting bias at treatment firms. If analysts only partially incorporated such new information in their forecasts, then their forecasts may have become a worse proxy for investors' expectations at treatment firms in the Post period (i.e. the measurement error in *UE* may have increased for these firms). If so, our estimate of the relative change in ERC for treatment firms would be biased downward, making it more difficult to detect the hypothesized increase in ERC. Second, it is possible that analyst's incentives to bias were systematically affected by the CD&A rules (e.g. because investors are better able to detect analysts' incentives to bias when the information about manager's reporting objectives is more precise). However, in untabulated tests we examine the signed forecast error (a common proxy for analysts' bias; e.g. Francis and Philbrick, 1993) for treatment and control firms in the Pre and Post period and find that the difference-in-difference is not significant. Also, bias is a greater concern for annual earnings' forecasts issued early in the year (Richardson et al., 2004; Bradshaw et al., 2016). In contrast, we examine one-quarter ahead forecasts of quarterly earnings, where bias should be less significant to start with.

¹⁶ In unreported tests we add an indicator for “bundled” earnings releases (i.e. earnings releases bundled with a management forecast; Anilowski, Feng and Skinner, 2007; Rogers and Van Buskirk, 2013) and its interaction with *UE*, to account for the possibility that the ERC differs when a concurrent management forecast is released. However, the interaction term is not significant and our coefficient of interest ($UE \cdot POST \cdot TREAT$) is unaffected. Also, we include cumulative market-adjusted returns from the most recent analyst forecast date in I/B/E/S to two days prior to the earnings announcement, to control for any new information arriving between the last analyst report and the earnings announcement. Our inferences are unchanged.

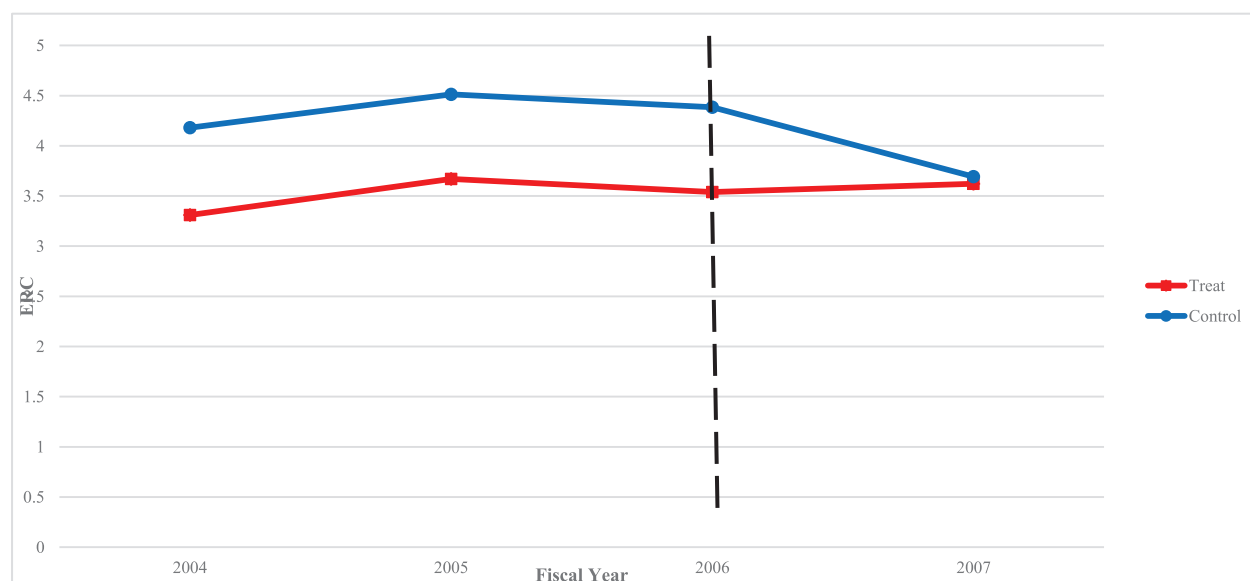


Fig. 4. Parallel trend in ERCs

This figure presents trends in ERCs of treatment and control samples from FY 2004 to FY 2007. To construct the figure, we re-run regression Eq. (1), modified to include two additional indicators to capture FY 2004 and FY 2005 (interacted with the relevant variables, similar to the indicator *POST* for the FY 2007). The ERC values plotted in Figure 4 for control firms (blue line with dots) and treatment firms (red line with squares) are the average quarterly coefficients on *UE* for each group in each FY estimated from such pooled regression. Each quarterly ERC is computed by adding (a) the coefficient of *UE*, (b) the coefficient of *UE*TREAT*, (c) the coefficient of the interaction between *UE* and that specific fiscal year-quarter fixed effect, (d) the average of the coefficients of the interactions between *UE* and industry effects, and (e) the coefficient on each interaction term of *UE* and each characteristic multiplied by the sample median of that firm characteristic. The sample includes U.S. firms used in our primary analyses (Table 3). (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

3.2. Parallel trend assumption

The key identifying assumption for the consistency of the difference-in-differences estimator is the parallel trends assumption: in the absence of treatment (the CD&A rules) treated and control firms should experience parallel *trends* in the outcome variable (ERC). While the assumption is not directly testable (since the trend in ERC absent the 2006 CD&A rules is not observable), we examine the trends in ERC *prior* to the event of interest similar to other studies using a difference-in-differences design (Roberts and Whited, 2013). A caveat is that our outcome variable, the ERC, is not directly observable but needs to be estimated (and the estimation may differ depending on various design choices). To do so, we re-run regression Eq. (1) but starting from FY 2004 (we start in 2004 to avoid the potential confounding effect of earlier regulations, e.g. Sarbanes-Oxley). That is, we include two additional indicators to capture FY 2004 and FY 2005 (interacted with the relevant variables, similar to the indicator *POST* for the FY 2007). Then, for each FY between 2004 and 2007 we plot the average quarterly ERCs for treatment and control firms estimated from such regression. As shown in Fig. 4, there is no indication of a different trend in ERCs prior to the CD&A rules.¹⁷ We also run a placebo test using a pseudo-event date of December 2005 (i.e. with FY 2005 and 2006, respectively, as Pre and Post periods), finding no evidence of a differential change in ERC for treatment firms relative to control firms (the coefficient of *UE*POST*TREAT* is negative and insignificant; untabulated). Based on this (admittedly suggestive) evidence, the parallel trend assumption seems reasonable in our setting.

Fig. 4 also anticipates the key result of the study: a relative increase in ERC for treatment firms. A potential concern is that the relative increase is driven by a decline in ERC for control firms, whose magnitude appears ‘unusual’ relative to the magnitude of the changes in the Pre period. However, as in any difference-in-differences studies, the focus is on the *relative* change, regardless of the specific change experienced by treatment and control firms (i.e. the trend in control firms is assumed to be the trend experienced by all firms absent the CD&A rules). Also, the magnitude of the change in ERC for control firms (and thus, presumably, for the entire population absent the CD&A rules) is not as unusual as the Pre period (2004–2006) in Fig. 4 may suggest. In untabulated tests, we estimated the average quarterly ERCs for each year from 1990 to 2016 in a broad sample of U.S. firms (regardless of their FY ends) and find that the ERC fluctuates between about 1.1 and 4.0, with an absolute yearly change of 0.66.

¹⁷ Treatment firms exhibit lower ERCs in the Pre period. However, differences in the *level* of ERCs do not violate the parallel trend assumption, which pertains to the *trend* in the outcome variable.

3.3. Sample selection, descriptive statistics and entropy balancing

Our sample includes all U.S. firms with the required data available on Compustat, CRSP and I/B/E/S for FY 2006 and 2007 (each firm must have at least one quarter of data in both FY 2006 and FY 2007). We exclude any earnings release occurring in calendar year 2008 to avoid the effect of the financial crisis (we will return to this issue in the robustness tests section). We exclude foreign private issuers (FPIs), since they are exempted from the CD&A rules.¹⁸ We winsorize continuous variables at 2% and 98% level, and delete firms with prices lower than \$3 in order to mitigate market microstructure concerns when calculating stock returns. Using Compustat data we define a firm as treated if it has a December FY-end and as a control if its FY ends between September and November. We drop all other firms. The resulting final sample includes 14,509 firm-quarters (2,765 distinct firms). Treatment (control) observations represent 88% (12%) of the sample. Such imbalance is common in studies where the regulation of interest applies to all firms and identification of the control samples relies on its staggered adoption based on FY-ends, since the vast majority of firms uses a December FY-end. For example, control firms represent about 10% of the sample in [Gipper \(2016\)](#), and 13% in [Ladika and Sautner \(2016\)](#).

[Table 1](#) reports summary statistics for the main variables. The descriptive statistics for the full sample are generally similar to other studies using Compustat firms (Panel A). Prior studies document that the choice of FY end is associated with industry factors, business seasonality and peer effects ([Fried and Sinha, 2008](#); [Smith and Pourciau, 1988](#)), raising concerns that by sorting based on FY ends we are selecting on firm/industry characteristics perhaps related to changes in ERC over our sample period. We analyze such characteristics for treatment and control firms in Panel B and C. Prior to the CDA& rules, treatment firms have higher leverage and market-to-book ratios, lower beta, and slightly higher dispersion in analysts' forecasts and frequency of losses (Panel B). Most industries are present in both samples but some industries are more frequent in the treatment group (e.g. Banking) and some more frequent in the control group (e.g. Electronic Equipment) (Panel C). While we partially control for these differences in our regressions by including firm characteristics (including industry membership) and their interactions with *UE*, the above evidence calls for tighter matching between treatment and control firms. Since the control observations represent only 12% of our sample, using traditional propensity score matching would greatly reduce the sample size and thus the power of our test. Instead, we use entropy balancing, a technique which preserves sample size and is often used in studies where treatment and control groups are unbalanced ([Chapman et al., 2017](#); [Shroff et al., 2017](#)).

Entropy balancing is a quasi-matching approach which re-weights each control observation so that post-weighting distributional properties of matched variables of treatment and control observations are virtually identical, thereby ensuring covariate balance ([Hainmueller, 2012](#); [McMullin and Schonberger, 2015](#)). As a first step, entropy balancing requires to select the distributional properties of interest (we focus on mean and variance) and the 'matching' variables (we choose firm size, market-to-book, beta, leverage, earnings persistence, loss indicator, analyst forecasts' dispersion and industry indicators). Next, the algorithm proceeds by first assigning possible weights (above or below one) to control observations, and then testing whether the 'balance' conditions have been met, i.e., whether the chosen distributional properties of treatment and post-weighted control observations for the matched variables are identical. The algorithm repeats this process over multiple iterations until a set of weights for control observations are found such that the balance conditions are met. [Table 2](#) shows the distributional properties of treatment and control firms before and after this process. The weights assigned to each control observation at the end of this procedure are then used in the regression analysis.

For our main analyses we present the findings both without matching and using entropy balancing. Given the importance of closely 'matching' treatment and control firms, we rely on the entropy balancing results when discussing the economic magnitude of the effects and in the subsequent robustness and cross-sectional tests.

4. Empirical results

4.1. Changes in ERC around the CD&A rules

Our main analysis relies on a difference-in-differences design comparing the change in ERC around the 2006 CD&A rules for firms who are immediately subject to the rules (December FY-end firms) and firms who are not subject to them until a later date (September–November FY-end firms). To do so, we estimate regression [Eq. \(1\)](#). [Table 3](#) reports the results, using un-weighted control observations in Panel A and re-weighted control observations (entropy balancing technique) in Panel B.

To facilitate comparisons with prior studies, we first present a 'pooled' regression for FY 2006 and FY 2007, without the indicator variables denoting the Post period and the treatment sample, (this regression is therefore the same in Panel A and B), and without firm characteristics, industry and fiscal year-quarter fixed effects. The estimated coefficient of *UE* in the pooled sample ranges

¹⁸ We considered using FPIs as a control sample. However, FPIs were subject to the Sarbanes-Oxley Act Section 404 (effectiveness of internal controls) and prior studies document an increase in ERCs in response to Section 404 disclosure ([Chen, Krishnan, Sami and Zhou, 2013](#)). Section 404 became effective at different points in time between 2006 and 2007 for different types of FPIs ("non-accelerated filers", "accelerated filers", "large accelerated filers"; for details, see pp. 224–226 from <http://www.gibsondunn.com/publications/Documents/Kelley-Sarbanes-OxleyActForeignPrivateIssuers.pdf>). Hence, it is difficult to identify a subset of FPIs that could serve as a proper control sample during our sample period. Also, FPIs were not required to do quarterly reporting (though many did), raising selection issues. However, we will use FPIs for a placebo test described in [Section 4.3](#).

Table 1
Summary statistics

Table 1 presents summary statistics for the variables used in our main analyses (Table 3). Panel A reports descriptive statistics for the full sample for the entire period (quarterly data for fiscal year 2006 and 2007). Panel B reports mean and median values for the treatment and control samples for the Pre period (quarterly data for fiscal year 2006). Also reported are the differences in mean and median values. All the variables are defined in detail in Appendix 2. Panel C reports the industry composition for treatment and control firms based on Fama-French 48 industry classification. All continuous variables are winsorized at 2% and 98% level. ***, **, * indicates significance level at 1%, 5%, and 10%, respectively.

Panel A Summary statistics for full sample						
Variable	N	Mean	Std. Dev	P25	Median	P75
CAR	14,509	−0.00059	0.07150	−0.03666	−0.00080	0.03745
UE	14,509	−0.00050	0.01073	−0.00086	0.00036	0.00185
Treat	14,509	0.88	0.32	1.00	1.00	1.00
Post	14,509	0.43	0.50	0.00	0.00	1.00
Size (Million)	14,509	4,056.41	8,588.11	348.80	973.54	2,957.51
Market-to-Book	14,509	3.19	3.36	1.56	2.34	3.79
Beta	14,509	1.13	0.61	0.72	1.09	1.49
Leverage	14,509	0.83	1.55	0.03	0.41	1.05
Persistence	14,509	0.33	0.30	0.09	0.32	0.56
Loss	14,509	0.17	0.37	0.00	0.00	0.00
Dispersion	14,509	0.01	0.03	0.00	0.00	0.00

Panel B Summary statistics: means and medians for treatment and control firms prior to the 2006 CD&A rules						
Variable(s)	Treatment: Mean	Control: Mean	Difference in Mean: Treatment - Control	Treatment: Median	Control: Median	Difference in Median: Treatment - Control
CAR	−0.00060	0.00542	−0.00602***	−0.00113	0.00269	−0.00382**
UE	−0.00025	−0.00049	0.00024	0.00040	0.00038	0.00002
Size (Million)	3,883.70	3,658.61	225.09	946.02	963.43	−17.41
Market-to-Book	3.23	2.89	0.34***	2.49	2.31	0.18***
Beta	1.20	1.29	−0.09***	1.15	1.26	−0.11***
Leverage	0.79	0.61	0.18***	0.39	0.25	0.14***
Persistence	0.33	0.34	−0.01	0.31	0.35	−0.04
Loss	0.17	0.14	0.03**	0.00	0.00	0.00**
Dispersion	0.01	0.00	0.00**	0.00	0.00	0.00***

Panel C Industry composition (based on Fama-French 48 industries classification)						
	Treatment Freq.	Percent	Control Cum.	Freq.	Percent	Cum.
Agriculture	3	0.12	0.12	0	0.00	0.00
Food Products	19	0.76	0.88	10	3.85	3.85
Candy & Soda	6	0.24	1.12	0	0.00	0.00
Beer & Liquor	5	0.20	1.32	0	0.00	0.00
Tobacco Products	5	0.20	1.52	0	0.00	0.00
Recreation	14	0.56	2.08	2	0.77	4.62
Entertainment	30	1.20	3.27	2	0.77	5.38
Printing and Publishing	9	0.36	3.63	4	1.54	6.92
Consumer Goods	21	0.84	4.47	3	1.15	8.08
Apparel	23	0.92	5.39	4	1.54	9.62
Healthcare	47	1.88	7.27	7	2.69	12.31
Medical Equipment	72	2.87	10.14	10	3.85	16.15
Pharmaceutical Products	189	7.54	17.68	8	3.08	19.23
Chemicals	43	1.72	19.40	10	3.85	23.08
Rubber and Plastic Products	12	0.48	19.88	1	0.38	23.46
Textiles	4	0.16	20.04	0	0.00	0.00
Construction Materials	32	1.28	21.32	7	2.69	26.15
Construction	25	1.00	22.32	7	2.69	28.85
Steel Works Etc	22	0.88	23.19	4	1.54	30.38
Fabricated Products	1	0.04	23.23	1	0.38	30.77
Machinery	73	2.91	26.15	14	5.38	36.15
Electrical Equipment	24	0.96	27.11	9	3.46	39.62
Automobiles and Trucks	30	1.20	28.30	1	0.38	40.00
Aircraft	10	0.40	28.70	4	1.54	41.54
Shipbuilding, Railroad Equipment	7	0.28	28.98	0	0.00	0.00
Defense	4	0.16	29.14	1	0.38	41.92
Precious Metals	4	0.16	29.30	0	0.00	0.00
Non-Metallic and Industrial Metal Mining	7	0.28	29.58	0	0.00	0.00
Coal	11	0.44	30.02	0	0.00	0.00
Petroleum and Natural Gas	132	5.27	35.29	4	1.54	43.46
Utilities	95	3.79	39.08	9	3.46	46.92

(continued on next page)

Table 1 (continued)

Panel C Industry composition (based on Fama-French 48 industries classification)						
	Treatment Freq.	Percent	Control Cum.	Freq.	Percent	Cum.
Communication	90	3.59	42.67	1	0.38	47.31
Personal Services	23	0.92	43.59	4	1.54	48.85
Business Services	255	10.18	53.77	31	11.92	60.77
Computers	61	2.44	56.21	10	3.85	64.62
Electronic Equipment	107	4.27	60.48	27	10.38	75.00
Measuring and Control Equipment	39	1.56	62.04	11	4.23	79.23
Business Supplies	24	0.96	62.99	1	0.38	79.62
Shipping Containers	8	0.32	63.31	2	0.77	80.38
Transportation	97	3.87	67.19	2	0.77	81.15
Wholesale	51	2.04	69.22	7	2.69	83.85
Retail	49	1.96	71.18	11	4.23	88.08
Restaurants, Hotels, Motels	41	1.64	72.81	3	1.15	89.23
Banking	329	13.13	85.95	11	4.23	93.46
Insurance	135	5.39	91.34	0	0.00	0.00
Real Estate	10	0.40	91.74	0	0.00	0.00
Trading	176	7.03	98.76	11	4.23	97.69
Other	31	1.24	100.00	6	2.31	100.00
Total	2,505	100		260	100	

between 1.324 (using ordinary least squares, Column 1) and 1.394 (robust regression, Column 2), generally in line with prior literature (Kothari, 2001).

We then present our difference-in-differences analysis. In Column (3) we include only the indicator variables for the Post period (*POST*) and for the treatment sample (*TREAT*) and their interactions with each other and with *UE* again without firm characteristics, fixed effects and their interactions with *UE* (essentially, a ‘univariate’ difference-in-differences analysis). The coefficient on the variable of interest, *UE*POST*TREAT*, is positive and significant at the 1% level, in both Panel A (1.155) and Panel B (1.356). Next, in Column (4) we present the full model corresponding to Eq. (1), including firm characteristics, industry and year-quarter fixed effects and their interactions with *UE*. The coefficients of the interactions of control variables with *UE* are in line with prior studies: losses are associated with lower ERC (*UE*Loss*) (Hayn, 1995; Basu, 1997); larger firms and higher-leverage firms have significantly lower ERC (*UE*Size*, *UE*Leverage*) (Collins and Kothari, 1989); growth firms have higher ERC (*UE*Market-to-Book*) (Easton and Zmijewski, 1989); firms with greater earnings persistence have higher ERC (*UE*Persistence*) (Easton and Zmijewski, 1989); firms with greater analysts’ forecast dispersion have lower ERC (*UE*Dispersion*) (Imhoff and Lobo, 1992). All the coefficients are significant

Table 2

Pre- and post-weighting distributional properties of treatment and control firms

This table presents distributional properties (mean and variance) for treatment and control firms in the original sample (Panel A) and after the re-weighting of the control sample via the entropy balancing technique (Panel B).

Panel A Original Sample				
Variable	Treatment (N = 12,778)		Control (N = 1,731)	
	Mean	Variance	Mean	Variance
Log(Size)	6.99	2.55	6.99	2.30
Market-to-Book	3.22	11.71	2.93	7.93
Beta	1.12	0.38	1.18	0.35
Leverage	0.86	2.48	0.63	1.76
Persistence	0.33	0.09	0.35	0.09
Loss	0.17	0.14	0.14	0.12
Dispersion	0.01	0.00	0.01	0.00
Panel B Post Entropy Balancing				
Variable	Treatment (N = 12,778)		Control (N = 1,731)	
	Mean	Variance	Mean	Variance
Log(Size)	6.99	2.55	6.99	2.55
Market-to-Book	3.22	11.71	3.22	11.71
Beta	1.12	0.38	1.12	0.38
Leverage	0.86	2.48	0.86	2.48
Persistence	0.33	0.09	0.33	0.09
Loss	0.17	0.14	0.17	0.14
Dispersion	0.01	0.00	0.01	0.00

Table 3

Change in ERC around the CD&A Rules

This table presents results from the estimation of Eq. (1). We regress 3-day cumulative abnormal returns around earnings announcements (CAR) on unexpected earnings (UE), indicators for treatment firms, treatment period and their interaction (TREAT, POST, POST*TREAT), control variables (firm characteristics), year-quarter fixed effects, industry fixed effects and the interactions of UE with control variables, year-quarter fixed effects, industry fixed effects and treatment indicators. Panel A reports results for a set of baseline regressions across different specifications. In Column (1), we use Ordinary Least Squares (OLS), while in Columns (2)–(6) we use a robust regression. Panel B reports the results by using entropy balancing, a quasi-matching technique which re-weights control observations to obtain better matching with the treatment observations. In Column (1) we use OLS, while in Columns (2)–(6) we use robust regressions by multiplying the robust regression weights with weights from entropy balancing method. Column (1) and (2) are identical in Panel A and B because the entropy balancing technique only applies to the specifications with indicators for treatment and control sample (Columns (3)–(6)). The coefficients for the intercepts are untabulated. All the variables are defined in detail in Appendix 2. *t*-statistics, based on robust standard errors clustered at the earnings announcement date level, are presented below the coefficient estimates. ***, **, * indicates significance level at 1%, 5%, and 10%, respectively.

Panel A Baseline regressions						
VARIABLES	(1) OLS	(2) Robust	(3) Robust	(4) Robust	(5) Robust	(6) Robust
UE*POST*TREAT			1.155*** (2.82)	0.648* (1.77)	4.403*** (5.79)	2.846*** (3.41)
UE	1.324*** (15.32)	1.394*** (24.61)	2.586*** (7.31)	4.237*** (9.60)	7.006*** (10.13)	8.860*** (12.12)
UE*POST			−1.346*** (−3.46)	−0.460 (−0.99)	−3.214*** (−4.71)	−2.835*** (−3.59)
UE*TREAT			−1.116*** (−3.08)	−0.759** (−2.32)	−3.556*** (−5.48)	−3.328*** (−4.80)
UE*Log(Size)				−0.121*** (−2.97)	−0.086** (−2.19)	−0.222*** (−4.76)
UE*Market-to-Book				0.061*** (5.16)	0.062*** (4.81)	0.058*** (4.74)
UE*Beta				0.324*** (3.77)	0.295*** (3.55)	0.362*** (4.17)
UE*Leverage				−0.052** (−2.24)	−0.052** (−2.24)	−0.019 (−0.81)
UE*Persistence				0.178 (1.00)	0.056 (0.33)	0.441** (2.35)
UE*Loss				−3.081*** (−21.12)	−6.646*** (−9.54)	−6.060*** (−8.22)
UE*Dispersion				−2.154*** (−5.58)	−3.386*** (−7.95)	−0.351 (−1.45)
UE*Loss*POST*TREAT					−4.487*** (−5.42)	−5.166*** (−5.84)
Nonlinear						−52.295*** (−3.46)
Nonlinear*TREAT						−20.278 (−1.23)
Nonlinear*POST						−1.454 (−0.09)
Nonlinear*POST*TREAT						35.078** (2.02)
Firm Characteristics	No	No	No	Yes	Yes	Yes
Firm Characteristics*UE	No	No	No	Yes	Yes	Yes
Fixed Effects	No	No	No	Year-Quarter, Industry	Year-Quarter, Industry	Year-Quarter, Industry
Fixed Effects*UE	No	No	No	Yes	Yes	Yes
Treatment Indicators (POST, TREAT)	No	No	Yes	Yes	Yes	Yes
POST*TREAT	No	No	Yes	Yes	Yes	Yes
UE*Treatment Indicators	No	No	Yes	Yes	Yes	Yes
Loss*UE interacted with Treatment Indicators	No	No	No	No	Yes	Yes
Nonlinear (= UE* UE).	No	No	No	No	No	Yes
Nonlinear*Treatment Indicators	No	No	No	No	No	Yes
Observations	14,509	14,507	14,508	14,491	14,490	14,497
Number of Treatment Obs.	12,778	12,777	12,777	12,766	12,770	12,772
Number of Control Obs.	1,731	1,730	1,731	1,725	1,720	1,725
Cluster	Date	Date	Date	Date	Date	Date
Adj. R-squared	0.041	0.061	0.064	0.148	0.152	0.179

Panel B Entropy balancing results

VARIABLES	(1) OLS	(2) Robust	(3) Robust	(4) Robust	(5) Robust	(6) Robust
UE*POST*TREAT			1.356*** (3.25)	0.925** (2.52)	3.472*** (4.35)	1.969** (2.13)

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Table 3 (continued)

Panel B Entropy balancing results						
VARIABLES	(1) OLS	(2) Robust	(3) Robust	(4) Robust	(5) Robust	(6) Robust
UE	1.324*** (15.32)	1.394*** (24.61)	2.541*** (7.05)	4.808*** (9.49)	7.197*** (9.65)	8.233*** (9.96)
UE*POST			–1.546*** (–3.92)	–0.213 (–0.45)	–2.525*** (–3.44)	–1.843** (–2.09)
UE*TREAT			–1.071*** (–2.89)	–1.168*** (–3.63)	–3.607*** (–5.18)	–3.152*** (–3.96)
UE*Log(Size)				–0.162** (–2.53)	–0.140** (–2.32)	–0.189*** (–3.24)
UE*Market-to-Book				0.082*** (4.77)	0.088*** (5.05)	0.054*** (2.70)
UE*Beta				0.346*** (2.68)	0.370*** (3.05)	0.310*** (2.77)
UE*Leverage				–0.101*** (–2.93)	–0.116*** (–3.39)	–0.070* (–1.95)
UE*Persistence				0.086 (0.33)	–0.159 (–0.63)	0.470* (1.74)
UE*Loss				–3.414*** (–16.47)	–6.242*** (–8.23)	–5.966*** (–7.25)
UE*Dispersion				–1.888*** (–3.62)	–2.804*** (–5.05)	–0.693** (–2.11)
UE*Loss*POST*TREAT					–3.051*** (–3.45)	–4.364*** (–4.60)
Nonlinear						–29.263 (–1.51)
Nonlinear*TREAT						–35.741* (–1.70)
Nonlinear*POST						–20.329 (–1.01)
Nonlinear*POST*TREAT						51.026** (2.34)
Firm Characteristics	No	No	No	Yes	Yes	Yes
Firm Characteristics*UE	No	No	No	Yes	Yes	Yes
Fixed Effects	No	No	No	Year-Quarter, Industry	Year-Quarter, Industry	Year-Quarter, Industry
Fixed Effects*UE	No	No	No	Yes	Yes	Yes
Treatment Indicators (POST, TREAT)	No	No	Yes	Yes	Yes	Yes
POST*TREAT	No	No	Yes	Yes	Yes	Yes
UE*Treatment Indicators	No	No	Yes	Yes	Yes	Yes
Loss*UE interacted with Treatment Indicators	No	No	No	No	Yes	Yes
Nonlinear (= UE* UE)	No	No	No	No	No	Yes
Nonlinear*Treatment Indicators	No	No	No	No	No	Yes
Observations	14,509	14,507	14,508	14,491	14,490	14,497
Number of Treatment Obs.	12,778	12,777	12,777	12,766	12,770	12,772
Number of Control Obs.	1,731	1,730	1,731	1,725	1,720	1,725
Cluster	Date	Date	Date	Date	Date	Date
Adj. R-squared	0.041	0.061	0.077	0.164	0.177	0.187

except *UE*Persistence*. As for the coefficient of interest, *UE*POST*TREAT*, is again positive and significant at 0.648 in Panel A (p -value = 0.077) and at 0.925 in Panel B (p -value = 0.012). These results suggest a greater relative increase of ERC for treated firms after the introduction of the CD&A rules.

In terms of economic magnitude, an increase of 0.925 (0.648) corresponds to about a 20% (15%) increase over the ERC level in the Pre period for treatment firms of 4.64 (4.44).¹⁹ This ERC figure of 4.64 is obtained as the average of the four quarterly ERCs for treatment firms in the Pre period (i.e. FY 2006), where each quarterly ERC is estimated by adding up the following: (a) the coefficient of *UE*; (b) the coefficient of *UE*TREAT*; (c) the coefficient of the interaction between *UE* and the relevant quarter fixed effect; (d) the average of the coefficients of the interactions between *UE* and industry effects (since we want to estimate an ‘average’ ERC, across all industries); (e) for each firm characteristic, the coefficient of the interaction term of *UE* with that firm characteristic multiplied by the sample median of that firm characteristic. In essence, the resulting estimate represents the average ERC (across quarters and industries) for a “typical” treatment firm (i.e. “typical” in terms of firm characteristics) in the Pre period.

¹⁹ These estimates do not coincide with the corresponding ones in Fig. 4 because they are based on a pooled regression over the FY 2006–2007 period, while the ERCs in Fig. 4 are based on a pooled regression over the FY 2004–2007 period. However, the pattern in terms of difference-in-differences is similar.

4.1.1. Change in ERC for loss firms and for extreme earnings surprises

Next, we extend our analysis to account for two critical properties of ERCs – namely, that ERCs are lower for accounting losses and for extreme values of *UE*. In particular, in Column (5), we add the interaction variables *UE*Loss*POST*, *UE*Loss*TREAT* and *UE*Loss*POST*TREAT*, to allow for the market reaction to earnings surprise for loss firms to differ between Pre and Post period and between treatment and control firms, as well as to change differently for treatment and control firms around the CD&A rules (the *UE*Loss*POST*TREAT* term). In both Panel A and B, the coefficient of *UE*POST*TREAT* (capturing the change in ERC in case of profits for treatment firms relative to control firms) is much larger than in Column (4). In contrast, the coefficient on *UE*Loss*POST*TREAT* is negative and significant, with a magnitude similar to the positive coefficient on *UE*POST*TREAT*. The net effect is close to zero and statistically not significant (untabulated). In other words, the relative increase in ERC for treatment firms is driven by firms with positive earnings, while there is no change in ERC for firms with losses, suggesting that enhanced information about managers' compensation-driven reporting incentives has limited usefulness for these firms. This is consistent with the notion that losses are transitory and earnings releases for loss firms contain less information, as evidenced by a low ERC (Hayn, 1995; Lawrence, Sloan and Sun, 2017). In a similar vein, Gipper et al. (2015) find that the increase in ERC around the introduction of the PCAOB inspection regime is concentrated among firms reporting profits. Our result is also consistent with evidence that firms operating at a loss, rather than linking manager's pay to formulaic financial targets, tend to use subjective evaluation and non-financial metrics (Gibbs et al., 2004; Matějka et al., 2009). Thus, for loss firms the CD&A rules are likely to be less effective at reducing investors' uncertainty about the manager's reporting objectives.

Next, in Column (6), to account for differential market response to extreme unexpected earnings, we add the nonlinear term *UE*|UE|* (*Nonlinear*) as well as the interaction variables *Nonlinear*POST*, *Nonlinear*TREAT* and *Nonlinear*POST*TREAT*. The coefficient on *UE*POST*TREAT* remains positive and significant. Consistent with prior literature documenting that extreme unexpected earnings are less persistent and thus are associated with lower ERCs (Freeman and Tse, 1992), the coefficient on *Nonlinear* is negative and significant. Interestingly, we find a positive and significant coefficient on the interaction variable *Nonlinear*POST*TREAT*, indicating that firms affected by the CD&A rules experienced a relative decrease in the degree to which extreme surprises reduce the ERC. With some caution, this finding suggests that a better understanding of management reporting objectives (via enhanced compensation disclosures) may reduce the “discount” that investors attach to large unexpected earnings surprises. Indirectly, it also implies that uncertainty about reporting objectives may be one of the reasons for lower ERCs in case of extreme unexpected earnings.

4.2. Interpreting the economic magnitude

In Section 4.1 we document an ERC increase of 0.925, corresponding to a 20% increase from the Pre period. Do these figures suggest that the CD&A rules resulted in a modest reduction in uncertainty about manager's reporting objectives? Or a large one? Are they plausible? Interpreting the economic significance of a change in ERC is challenging. A natural starting point is to compare our estimates to other studies examining changes in ERC. Chen et al. (2014) report a decrease of about 1.8 in the average quarterly ERC in the three years after a *material* restatement (a shock to reporting credibility), representing a 56% decrease from an initial ERC of about 3.3. Wilson (2008) reports an ERC decrease of about 1.2 by the third quarter after (*any*) restatement, from an initial level of about 6.6, representing an 18% decrease. Gipper et al. (2015) report an increase of about 0.8 in the annual ERC around the introduction of PCAOB inspection regime (with the increase rising to 1.6 in the sample of ‘full’ inspections). However, the events analyzed in these studies (e.g. restatements, PCAOB inspections) are not comparable to ours, and the models underlying the ERC estimates differ across studies. Thus, it is difficult to use prior studies to evaluate the plausibility of our estimate.

Part of the problem in interpreting a change in ERC is that we lack a benchmark for what the change in ERC increase *should* be under the hypotheses that the CD&A rules had a large impact, or no impact. A step in this direction is to express the increase in ERC in terms of the difference between the empirically observed ERC and the ‘theoretical’ ERC, i.e. the ERC under the assumption that innovations in earnings are permanent ($ERC = (1/r) + 1$, where *r* is the annual cost of equity capital; Kothari, 2001). Assuming a cost of capital of 10% and, thus, a theoretical ERC of 11 ($= 1 + 1/10\%$), we can state that an increase of 0.925 closes about 14% of the “gap” between observed (4.64) and theoretical (11) level of ERC, that is, $14\% \approx 0.925/(11-4.64)$. While useful, this approach falls short of providing a complete answer, since we do not know how much of the gap is due to σ_x^2 vis-à-vis other factors (transitory earnings, etc.). For example, if we knew that the ERC would at most increase to 6 when the $\sigma_x^2 = 0$ (with the rest of the gap due to other factors), then we could conclude that the CD&A rules removed about 68% ($\approx 0.925/(6-4.64)$) of the uncertainty about reporting objectives.

Another approach to interpret a change in (annual) ERC is offered by Gipper et al. (2015), who propose to convert it into an equivalent change in cost of capital. If the downward bias in the observed ERC relative to its theoretical value of 11 (assuming, again, a 10% cost of capital and permanent earnings innovations) remains constants over time, then for a given change in ERC one can infer the corresponding cost of equity capital by solving for *r_{new}* the following expression: $(1/r_{new}) + 1 = 11 + \text{Change in ERC}$. After solving for *r_{new}*, Gipper et al. (2015) compare it to 10% and conclude that the increase in the ERC around the introduction of the PCAOB inspection regime is equivalent to a decrease of 73 basis points in the cost of capital. Using the same approach, our 0.925 increase in (quarterly) ERC is equivalent to a decrease of 22 to 84 basis points in the cost of capital,

depending on whether quarterly earnings innovations are assumed to be permanent only on an annual basis, or on a quarterly basis.²⁰

4.3. Robustness tests

We perform a number of robustness tests (in all of them we use entropy balancing; thus the figures below should be compared to Table 3 Panel B). First, we expand our control sample to include firms with a FY-end between July and August (for these firms, we only keep in the Post period the quarterly earnings releases issued before they adopt the CD&A rules). The logic remains the same as in the main test (i.e. exploit the overlapping period to examine differential changes in ERC between treatment and control), except that for the June–August firms the overlapping period is shorter. The resulting control sample represents almost 20% of the total sample. Using this control sample in Column (4) the coefficient of $UE*POST*TREAT$, at 0.668, remains positive and significant (p -value = 0.07).

Our second set of tests (untabulated) deals with the window used to perform our analyses. In our main tests (Table 3) we employ a tight window (FY 2006 and 2007) to minimize the risk of capturing the effect of other events. Nonetheless, we obtain similar results when we expand the Pre period to include FY 2006 and 2005. For example, in Column (4) the coefficient of $UE*POST*TREAT$, at 0.685, remains positive and significant (at the 5% level). Also, in our main tests we exclude any earnings release relative to the fourth quarter of fiscal year 2007 but occurring in calendar year 2008, to avoid any effect of the 2008 financial crisis on ERCs. When we include early 2008 earnings releases, the coefficient of $UE*POST*TREAT$ in Column (4) is slightly lower, at 0.760, but still significant (at the 5% level).

Our third test aims to control for omitted factors that may differentially affect treatment and control firms around the CD&A rules. While we control for fiscal year-quarter fixed effects and the interactions with unexpected earnings, it is possible that unobservable factors differentially affect market responses to earnings news for treatment and control firms over the period we analyze, causing us to attribute to the CD&A rules a change in ERC driven instead by these factors. While it is hard to imagine why such factors would correlate with (predetermined) FY-ends, to partially mitigate this concern we conduct a placebo test using a sample of foreign private issuers (FPIs). FPIs are exempted from the CD&A rules but are subject to similar market events as well as other aspects of the U.S. regulatory regime. If our results were driven by omitted factors that for some reason affect the ERC of firms with different FY-end differently, then we should find a significant increase in ERCs for December FY-end FPIs compared to September–November FY-end FPIs. To test this conjecture, we re-run the regression Eq. (1) in a sample of 261 distinct FPI firms (identified as in Li, 2014), resulting in 1,312 firm-quarter observations. In unreported tests, we do not find a relative increase in ERC for December FY-end FPIs. However, the sample size is small, especially for September–November FY-end FPIs, and some of the coefficients unusually large, casting doubts on the reliability of the estimates.

Finally, our results are robust to a number of alternative design choices. Untabulated tests show that: (i) if we exclude industries not represented in the control sample (see Table 1, Panel C), the coefficient of $UE*POST*TREAT$ in Column (4) is 0.932 (p -value = 0.012); (ii) if we limit the sample to firms with at least two (rather than one) quarters of data in both the Pre and the Post period, the coefficient of $UE*POST*TREAT$ in Column (4) is 0.870 (p -value = 0.018); (iii) if we winsorize the continuous variables using the 1st and 99th percentile (instead of the 2nd and 98th percentile) in Column (4) the coefficient of $UE*POST*TREAT$ is 0.791 (p -value < 0.01); (iv) if we use mean, rather than median, of one-quarter ahead analyst forecasts to compute UE , the coefficient of $UE*POST*TREAT$ is 0.723 (p -value = 0.07); and (v) if we use the median of recent one-quarter ahead analyst forecasts (i.e. those issued within the last four weeks prior to the earnings announcements), the coefficient of $UE*POST*TREAT$ is 0.939 (p -value = 0.014).

5. Cross-sectional analyses

The theory underlying our study predicts a larger increase in ERC when the reduction in uncertainty about the manager's reporting objectives is greater, i.e. (in our setting) when the improvement in compensation disclosures is larger. Constructing a direct, firm-specific measure of such improvement is somewhat subjective and prohibitively costly in a large sample study. Thus, we use two sets of proxies, one capturing ex post observed improvements in compensation disclosures and one capturing ex ante expected improvements. The proxies are described below.

²⁰ If quarterly earnings innovations are assumed to be permanent only on an annual basis (i.e. a surprise of \$1 this quarter will only result in \$1 of additional earnings this year, and \$1 all subsequent years), then $r_{new} = 1/(1 + 0.925 - 1) = 9.16\%$, implying a decrease of 84 basis points in the cost of capital (relative to the 10% benchmark). If, instead, quarterly earnings innovations are assumed to be permanent on a quarterly basis (i.e. a surprise of \$1 this quarter will result in \$1 of additional earnings for each quarter of this year, and thus about \$4 each year for all subsequent years), then an ERC increase of 0.925 is equivalent to a decrease of 22 basis points in the cost of capital. This figure is obtained as follows: the 10% annual cost of capital used as benchmark implies a quarterly cost of equity capital of $r = (1 + 10\%)^{1/4} - 1 = 0.0241$ (2.41%). Thus, the benchmark quarterly ERC is: $(1/r) + 1 = (1/0.0241) + 1 = 42.4702$. Since we document an increase in ERC of 0.925, then $(1/r_{new}) + 1 = ERC + \text{Change in ERC} = 42.4702 + 0.925$, which implies a new quarterly cost of capital $r_{new} = 0.0236$. In turn, this implies an annual cost of capital of $(1 + 0.0236)^4 - 1 = 9.78\%$, i.e. a decline in the cost of capital of 22 basis points (from the 10% benchmark). Both assumptions are extreme. Empirically, quarterly ERCs tend to be larger than annual ERCs, suggesting that quarterly earnings innovations are not permanent only on annual basis (i.e. a surprise of \$1 this quarter tends to result in more than \$1 of additional earnings during the year and in all subsequent years). At the same time, they are unlikely to be permanent on a quarterly basis either, due to the presence of seasonal and transitory components (i.e. a surprise of \$1 this quarter tends to result in less than \$4 of additional earnings during the year and in all subsequent years). Hence, we can only state that in our setting the increase is equivalent to a decrease in the cost of capital ranging from 22 to 84 basis points.

Table 4

Change in ERC: Cross-sectional test based on Ex Post Measures of Improvement in Pay Disclosures

This table reports the results from the estimation of Eq. (1), modified to examine whether the change in ERC varies based on ex post measures of improvement in compensation disclosures. In Column (1) we only examine treatment firms (since control firms did not receive SEC comment letters) and partition them based on whether or not a firm received a SEC comment letter. In particular, we run the following robust regression: $CAR_{i,t} = \beta_0 + \beta_1 UE_{i,t} \times POST_{i,t} \times No_SEC_CL_{i,t} + \beta_2 UE_{i,t} + \beta_3 \times No_SEC_CL_{i,t} + \beta_4 POST_{i,t} + \beta_5 UE_{i,t} \times POST_{i,t} + \beta_6 UE_{i,t} \times No_SEC_CL_{i,t}$ defined

$$+ \beta_7 POST_{i,t} \times No_SEC_CL_{i,t} + \beta_m Controls + \beta_n UE_{i,t} \times Controls + \epsilon_{i,t}$$

as one if the firm does not receive a SEC Comment Letter for failure to comply with the 2006 compensation disclosures.

In Columns (2) and (3), we examine treatment and control firms separately and partition each subset based on whether or not a firm begins to disclose its CFO compensation after the CD&A rules. In particular, we run the following robust regression: $CAR_{i,t} = \beta_0 + \beta_1 UE_{i,t} \times POST_{i,t} \times CFO_Pay_New_{i,t} + \beta_2 UE_{i,t} + \beta_3 \times CFO_Pay_New_{i,t} + \beta_4 POST_{i,t} + \beta_5 UE_{i,t} \times POST_{i,t}$

$$+ \beta_6 UE_{i,t} \times CFO_Pay_New_{i,t} + \beta_7 POST_{i,t} \times CFO_Pay_New_{i,t} + \beta_m Controls + \beta_n UE_{i,t} \times Controls + \epsilon_{i,t}$$

CFO Pay_New is defined as one if the firm began to disclose information about CFO compensation after the CD&A rules. We present the results within treatment firms (Columns (2)) and within control firms (Column (3)), respectively. Also reported is the *p*-value of the difference in change in ERC between the two columns. For parsimony we only report the coefficient of interest ($UE \times POST \times No_SEC_CL$ and $UE \times POST \times CFO_Pay_New$). Note that the sample size in Columns (2) and (3) is smaller than in Table 3 because we have access to CFO pay data only for the S&P 1500 firms covered by the Execucomp dataset. *t*-statistics, based on robust standard errors clustered at the earnings announcement date level, are presented below the coefficient estimates. ***, **, * indicates significance level at 1%, 5%, and 10%, respectively.

VARIABLES	(1) No_SEC_CL = 1: Firm did not receive a SEC comment letter Within Treatment Firms	(2) CFO Pay_New = 1: Firm began to disclose CFO pay after CD&A rules Within Treatment Firms	(3) Within Control Firms
UE*POST*No_SEC_CL	0.812* (1.86)		
UE*POST* CFO Pay_New		2.819*** (3.48)	− 1.431 (− 0.74)
<i>p</i> -value for difference in coefficients		0.06	
Firm Characteristics, Firm Characteristics*UE	Yes	Yes	Yes
Fixed Effects	Year-Quarter, Industry	Year-Quarter, Industry	Year-Quarter, Industry
Treatment Indicators (POST, No_SEC_CL)	Yes		
Treatment Indicators (POST, CFO Pay_New)		Yes	Yes
Interaction Treatment Indicators	Yes	Yes	Yes
Treatment Indicators*UE, Fixed Effects*UE	Yes	Yes	Yes
Observations	12,765	6,603	1,061
Number of Obs. No_SEC_CL = 1 (0)	1,815 (10,950)		
Number of Obs. CFO Pay_New = 1 (0)		347 (6,256)	122 (939)
Cluster	Date	Date	Date
Adj. R-squared	0.139	0.156	0.233

5.1. Proxies for (ex post) observed improvement in compensation disclosures

Our first proxy takes advantage of the SEC review process. To evaluate (and induce) compliance with the CD&A rules, in April 2007 the SEC began reviewing proxy statements released by firms with a December FY end (thus, only treatment firms were reviewed). As a result of this process, the SEC issued a comment letter to 350 firms detailing specific inadequacies in the disclosures and urging the firm to remedy. Robinson et al. (2011) find that most of the defective disclosures relate to pay-for-performance issues (e.g. poor disclosures of performance targets/metrics).

Since firms receiving a SEC comment letter did not improve compensation disclosures to the extent required by the CD&A rules (according to the SEC), we expect them to exhibit a smaller increase in ERC relative to firms that did not receive comment letters. To test this conjecture, in Table 4, Column 1, we re-run regression Eq. (1) within the subset of treatment firms but replacing the *TREAT* indicator with an indicator for firms that did not receive a SEC comment letter (*No_SEC_CL*). The coefficient on *UE*POST*No_SEC_CL* is positive and significant (*p*-value = 0.064), consistent with a greater reduction in investors' uncertainty about managers' reporting incentives among treatment firms that most improved their disclosures (as noted above, control firms were not reviewed by the SEC).²¹

While the SEC comment letters proxy for overall improvement in disclosures, our second proxy aims to capture one specific, readily observable improvement. The CD&A rules require firms to disclose compensation information for the CEO, CFO and the three highest paid executives. In contrast, the 1992 rules covered the CEO and the four highest paid executives. Hence, before the CD&A rules, some firms did not include CFO pay data in the proxy filings (namely, firms where the CFO was not one of the four highest paid

²¹ The relatively low statistical significance is not surprising, considering that: (i) not all treated firms that did not receive SEC comment letters improved the quality of their disclosures (Robinson et al., 2011 documents incomplete or defective disclosures in a random sample of 50 firms that did not receive SEC comment letters); and (ii) some firms receiving a SEC comment letter may have improved the disclosures relevant to investors' ability to assess managers' reporting incentives (performance targets/metrics) and received the SEC comment letter for other reasons (e.g. poor disclosures of compensation setting process; Robinson et al., 2011).

Table 5

Change in ERC: cross-sectional test based on Ex Ante Measure of Expected Improvement in Pay Disclosures

This table reports the results from the estimation of Eq. (1) separately for treatment and control firms, with each subset partitioned based on the level of excess CEO pay, a proxy for expected improvement in compensation disclosures. We adopt the following robust regression: $CAR_{i,t} = \beta_0 + \beta_1 UE_{i,t} \times POST_{i,t} \times Excess Pay_{i,t} + \beta_2 UE_{i,t} + \beta_3 \times Excess Pay_{i,t} + \beta_4 POST_{i,t} + \beta_5 UE_{i,t} \times POST_{i,t} + \beta_6 UE_{i,t} \times Excess Pay_{i,t} + \beta_m Controls + \beta_n UE_{i,t} \times Controls + \varepsilon_{i,t}$. *Excess Pay* is equal to one for firms with excess CEO pay above the sample median. We run the analysis within treatment firms (Column (1)) and within control firms (Columns (2)) respectively. Also reported are *p*-values of the differences in change in ERC between treatment and control firms. In Column (3) and Column (4), we then partition our treatment sample based on whether the firm received a comment letter from the SEC. Also reported is the *p*-value of the difference in change in ERC between the two columns. For parsimony, we only report the coefficient of interest ($UE \times POST \times Excess Pay$). Note that the sample size is smaller than in Table 3 because we have access to executive compensation data only for the S&P 1500 firms covered by the Execucomp dataset, respectively. *t*-statistics, based on robust standard errors clustered at the earnings announcement date level, are presented below the coefficient estimates. ***, **, * indicates significance level at 1%, 5%, and 10%, respectively.

VARIABLES	(1) Excess Pay = 1: Excess pay > median	(2) Within Control Firms	VARIABLES	(3) Excess Pay = 1: Excess pay > median	(4) Within Treatment Firms w/ SEC comment letter
	Within Treatment Firms	Within Control Firms		Within Treatment Firms w/o SEC comment letter	Within Treatment Firms w/ SEC comment letter
UE*POST*Excess Pay	1.486** (2.53)	−0.032 (−0.02)	UE*POST*Excess Pay	3.595*** (4.93)	−1.252 (−1.33)
<i>p</i> -value for difference in coefficients	0.24		<i>p</i> -value for difference in coefficients	0.00	
Firm Characteristics, Firm Characteristics*UE	Yes	Yes	Firm Characteristics, Firm Characteristics*UE	Yes	Yes
Fixed Effects	Year-Quarter, Industry	Year-Quarter, Industry	Fixed Effects	Year-Quarter, Industry	Year-Quarter, Industry
Fixed Effects*UE	Yes	Yes	Fiscal Quarter*UE	Yes	Yes
Treatment Indicators (POST, Excess Pay)	Yes	Yes	Treatment Indicators (POST, Excess Pay)	Yes	Yes
POST*Excess Pay	Yes	Yes	POST*Excess Pay	Yes	Yes
UE*Treatment Indicators	Yes	Yes	UE*Treatment Indicators	Yes	Yes
Observations	4,277	854	Observations	1,378	3,164
N. Obs. Excess Pay = 1	2,369	334	N. Obs. Excess Pay = 1	784	1,482
N. Obs. Excess Pay = 0	1,908	520	N. Obs. Excess Pay = 0	594	1,682
Cluster	Date	Date	Cluster	Date	Date
Adj. R-squared	0.168	0.258	Adj. R-squared	0.210	0.156

officers and that did not voluntarily disclose CFO pay data). At the same time, a recent stream of studies highlights the role of CFOs' compensation incentives in financial reporting choices. For example, Jiang et al. (2010) find that CFO equity incentives are more associated with earnings management than CEO equity incentives, consistent with CFO's primary responsibility in preparing financial reports. To the extent that CFOs' incentives are a significant part of the “manager's” reporting objectives, we expect a more pronounced increase in ERC among treatment firms that began disclosing details of CFO pay after the CD&A rules.

To examine our prediction, in Panel B we partition our treatment sample between firms that began to disclose CFO pay information *after* the CD&A rules (captured by the indicator variable *CFO Pay_{New}*) and all other firms, and compare the change in ERC. As shown in Column (2), the coefficient on $UE*POST*CFO Pay_{New}$ is positive and significant (at the 1% level). In Column (3) we repeat the same test within control firms (as placebo). The coefficient is not significant and is statistically smaller than within treatment firms (*p*-value = 0.06). While these findings are consistent with our prediction, we caution that the sample size of firms that began disclosing information about the CFO pay plan after the CD&A rules is fairly small.

5.2. Proxy for (ex ante) expected improvement in compensation disclosures

As a proxy for expected improvement in compensation disclosures (and thus greater expected increase in ERC), we use excess CEO pay. Firms with higher excess CEO pay are subject to greater compensation-related activism (e.g. Ertimur et al., 2011, 2013) and thus arguably are under greater pressure to improve their disclosures (investors want to assess the strength of the pay-for-performance linkage and the appropriateness of performance targets).

Table 5, Column (1), compares changes in ERCs within the treatment sample for firms with excess CEO pay above- (*Excess Pay* = 1) and below- (*Excess Pay* = 0) sample median (measured in 2006). The coefficient on $UE*POST*Excess Pay$ is positive and significant (at the 5% level), suggesting greater increases in ERC for the subset of firms with higher excess CEO pay. In Column (2) we repeat the same test within control firms (as placebo) and find an insignificant coefficient. We note, though, that the difference between the coefficient in Columns 1 and 2 is not significant.

Our test views excess CEO pay firms as a proxy for investors' demand of better pay disclosures. However, excess CEO pay firms also proxy for lower supply of disclosures (i.e. managers more reluctant to provide details about their compensation). Thus, we repeat

our test, separately, within treatment firms receiving and not receiving a SEC comment letter (respectively, Column 4 and Column 3). If firms receiving a comment letter are, by construction, firms that *ex post* did not improve their disclosures, we would expect no effect of excess CEO pay on ERC within this group. Indeed, we find that $UE*POST*Excess$ is positive and significant only among firms *not* receiving comment letters (and the difference among coefficients in Column 3 and 4 is significant). Put it differently, on average, firms with excess CEO pay experienced an increase in ERC (Column 1), but the increase did not occur in the subset of more entrenched firms that resisted the pressure to improve disclosures (Column 4).

While admittedly imperfect, our cross-sectional tests suggest that the increase in ERC was more pronounced in firms that likely exhibited greater improvement in compensation disclosures.

6. Alternative explanations for the increase in ERC

In Fischer and Verrecchia (2000), the equilibrium equation for ERC is $\beta = \sigma_v^2 / (\sigma_v^2 + \sigma_n^2 + \beta^2 \sigma_x^2 / c^2)$. Thus, an increase in ERC can result not only from a decrease in σ_x^2 but also from an increase in σ_v^2 (greater prior uncertainty on firm value), an increase in c (higher marginal cost of adding bias to the earnings report), or a decrease in σ_n^2 (greater inherent earnings quality). As noted in the Introduction, a maintained assumption of our study is that the only effect of the CD&A rule was to improve the quality of compensation disclosures and, thus, reduce uncertainty about reporting objectives (σ_x^2). In this section we consider the implications of relaxing this (possibly strong) assumption by examining other potential consequences of the CD&A rule and their likely effect on those parameters and, thus, on ERC.²²

In particular, we focus on three potential consequences of the new disclosures. First, the biggest concern is that the CD&A rules may have led boards to modify compensation contracts, thereby altering managerial incentives and, through those, the economics of the firm.²³ We are especially concerned with changes that could explain an increase in ERC, such as a shift toward pay packages with greater risk-taking incentives, which in turn might lead to higher firm volatility, greater investors' uncertainty on firm value, σ_v^2 , and thus higher ERC. We note, however, that even if the CD&A rules affected compensation practices, it is not obvious that they differentially affected treatment and control firms. The reason is that the CD&A rules were finalized in August 2006. Hence, *both* our control and treatment firms started their FY 2007 with full knowledge of the final rules, and may have adjusted their compensation contracts already during FY 2007 (compensation contracts are typically finalized early in the fiscal year). That is, all firms were 'treated' at the same time and the firms properly identified as 'control' for the purpose of examining the change in ERC (because investors at these firms observe the new proxy disclosures only *after* FY 2007) are not proper 'control' firms for the purpose of analyzing compensation change (since those changes may already take place *during* FY 2007), making our difference-in-differences approach not suitable to capture CD&A-induced compensation changes. Under this scenario any effect of the CD&A rules on compensation could not explain the *differential* change in ERC we document for treatment and control firms.

In contrast, if the September–November FY-end firms began to adjust their compensation contracts only *after* their first proxy disclosures under the new regime (i.e. during FY 2008), then they would be a proper control sample and we can use our difference-in-difference research design to detect compensation changes induced by the CD&A rules. We examine this possibility in Table 6, Panel A, where we report difference-in-differences for a set of variables capturing the composition of annual total pay and various measures of incentives based on the equity portfolio (delta, vega and the dollar value of the equity portfolio).²⁴ It appears that both treatment and control firms experienced a shift from options to restricted stock (a well-known trend that began with the introduction of FAS123R; e.g. Hayes et al., 2012), but there is little evidence of any differential changes, except that the decrease in option-based pay is somewhat less pronounced for treatment firms. However, this does not affect any of the equity portfolio measures, nor the composition of the equity mix. Importantly, the difference-in-differences in vega (a proxy for risk-taking incentives) is not significant. The lack of differential effects is consistent with the CD&A rules either not significantly altering compensation practices, or affecting our treatment and control at the same time. Under both explanations, the effect of the CD&A rules on compensation contracts (or lack thereof) would not explain the *differential* changes in ERC we document.

It remains possible that the variables examined in Panel A do not fully capture changes in managerial incentives. Besides, managerial incentives may be altered by the CD&A rules even absent any change to compensation contracts. Hence, we also examine directly the potential real effects of changes in managerial incentives. In particular, in Panel B we provide a difference-in-difference analysis of the control variables from Table 3 (size, book-to-market, leverage, beta, analyst forecast dispersion, earnings persistence)

²² We especially focus on mechanisms by which the CD&A rules may have caused an increase in σ_v^2 or in c , since such increases would provide an alternative explanation for the documented increase in ERC. We have not identified a mechanism by which the CD&A could affect σ_n^2 , the inherent earnings quality observed by the manager.

²³ For example, expecting high public outrage under enhanced disclosures of performance-based pay, boards may have shifted pay packages toward more fixed pay and less variable pay. Alternatively, boards may have strengthened the pay-for-performance sensitivity in anticipation of investors' scrutiny of the pay-for-performance link.

²⁴ For each variable, we compute the mean across the top five executives. This analysis is limited to firms with compensation data on Execucomp. Execucomp has changed the reporting format after the introduction of the CD&A rules. As a result, while FY 2007 data follow the new reporting regime for both treatment and control firms, the FY 2006 data are reported under the new regime for treatment firms and the old one for control firms, making a difference-in-differences analysis potentially inaccurate. To address this problem, in Table 6 Panel A we define FY 2005 (old reporting regime for *both* treatment and control firms) as Pre period and FY 2007 (new reporting regime for *both* treatment and control firms) as Post period. While the Pre-Post comparison *within* treatment or control firms would be inaccurate, the difference-in-differences analysis is meaningful, as long as the change in reporting regime did not affect treatment and control firms in a systematically different way. In untabulated analyses, we find generally similar results using FY 2006 as Pre period.

Table 6

Change in compensation structure and firm characteristics

This table reports changes in executive compensation structure and firm characteristics for treatment and control firms from the Pre to the Post period. Panel A reports univariate difference-in-difference changes for different components of executives' annual pay, as well the delta, vega and dollar value of executives' stock and option portfolio. In Panel A, we define the Pre period as FY 2005 and the Post period as FY 2007, because reporting format changes resulting from the CD&A rules prevent comparability between compensation data in FY 2006 and FY 2007 for control firms. Panel B reports univariate difference-in-difference changes for our control variables from Table 3 (size, book-to-market, leverage, beta, analyst forecast dispersion, earning persistence), and for a set of additional variables capturing firm's real activities and risk profile, including: daily stock return volatility, Research & Development (R&D) expense, Selling, General and Administrative expenses (SG&A), and capital expenditures (CAPEX). In Panel B, we define the Pre period as FY 2006 and the Post period as FY 2007. All the variables are defined in detail in Appendix 2. ***, **, * indicates significance level at 1%, 5%, and 10%, respectively.

Panel A Changes in Compensation Structure from Pre to Post period					
	Treatment: Pre	Treatment: Post	Control: Pre	Control: Post	Difference-in-difference
Salary/Total Pay	0.315	0.331	0.317	0.361	−0.029
Bonus/Total Pay	0.223	0.218	0.218	0.229	−0.016
Option/Total Pay	0.228	0.165	0.284	0.174	0.046**
Restricted Stock/Total Pay	0.137	0.215	0.106	0.168	0.016
Option/Equity Pay	0.616	0.440	0.704	0.537	−0.009
Delta (\$000 s)	408.8	280.9	351.8	338.0	−114.1
Vega (\$000 s)	81.2	54.9	62.7	49.7	−13.3
Value of stock and option portfolio (\$000 s)	35,315.0	23,813.6	31,113.0	29,214.5	−9,602.9

Panel B Changes in Firm Characteristics from Pre to Post period					
	Treatment: Pre	Treatment: Post	Control: Pre	Control: Post	Difference-in-difference
Size (Million)	3,884	4,210	3,659	4,096	−111
Market-to-Book	3.231	3.212	2.889	2.966	−0.096
Beta	1.197	1.014	1.289	1.059	0.047
Leverage	0.791	0.953	0.607	0.647	0.122
Persistence	0.325	0.333	0.337	0.358	−0.013
Dispersion	0.006	0.007	0.004	0.007	−0.002
Stock Return Volatility	0.023	0.022	0.023	0.022	−0.001
R&D/Total Assets	0.049	0.047	0.050	0.044	0.004
SG&A/Total Assets	0.223	0.208	0.282	0.254	0.013
CAPEX/Total Assets	0.067	0.065	0.049	0.049	−0.002

as well as other variables capturing firm's investment activities and risk profile: return volatility, R&D, SG&A and capital expenditures. None of the difference-in-differences is significant. Overall, the evidence in Table 6 does not suggest differential changes in managerial incentives and firm's fundamentals between treatment and control firms around the introduction of the CD&A rules, at least over the short-window underlying our ERC analysis. In particular, we fail to detect a change in managerial risk-taking incentives or in the firms' risk profile that could explain an increase in σ_v^2 and thus in ERC.²⁵

The second potential consequence of the CD&A rules is that compensation disclosures may indirectly provide information about the firm's economics. For example, to the extent that the parameters of the incentive plan (performance metrics/weights/targets) 'reveal' the strategy of the firm (e.g. emphasis on growth versus profitability), greater compensation disclosures may help investors to better predict managers' real activities, and thus reduce investors' uncertainty on firm value. But such scenario would predict a reduction in σ_v^2 and, thus, in ERC, biasing against our hypothesis (thus, the documented increase in ERC might understate the effect of the reduction in σ_x^2). Besides, since investors have access to numerous information sources about the firm's fundamentals and strategy (financial disclosures, meetings with management, conference calls, industry analyses, etc.), it seems unlikely that compensation disclosures would provide a significant amount of *new* information.

Finally, a third potential consequence of the CD&A rules is that improved compensation disclosure might increase the marginal cost of adding bias, c . More precise information about compensation targets may have resulted in greater investors' scrutiny of reporting choices and/or stronger penalties for adding bias (e.g. reputation costs, turnover risk, litigation risk). However, in such scenario c would be a decreasing function of σ_x^2 (rather than a constant term, as in Fischer and Verrecchia (2000)), and thus an increase in c (as a result of a reduction in σ_x^2) would not be an *alternative* explanation for the increase in ERC. Rather, the reduction in σ_x^2 would affect the ERC both directly (through a reduction in investors' uncertainty about reporting objectives) and indirectly (through the increase in the cost of adding bias as a result of such reduction). Thus, the reduction in σ_x^2 would continue to be the explanation for the increase in ERC.²⁶

²⁵ We also note that a higher ex-ante uncertainty typically implies a bigger surprise ex post. Hence, an increase in σ_v^2 should manifest itself ex post in a larger absolute magnitude of *UE*. In contrast, in untabulated tests we find no significant change in the absolute magnitude of *UE* for treatment firms.

²⁶ The parameter c would be an alternative explanation for the increase in ERC if it increased around the CD&A rules for treatment firms relative to control firms for reasons unrelated to the improved quality of compensation disclosures. We are not aware of any such reasons. But even under this scenario, a necessary (but not sufficient) condition for c to be the *only* force driving the increase in ERC is a decrease in reporting bias (Fischer and Verrecchia, 2000). However, as noted in Section 7, we fail to find such evidence.

Notwithstanding the above arguments and tests, a caveat in interpreting our study is that we cannot fully rule out the possibility that the CD&A rules may have affected the ERC through channels other than a reduction of investors' uncertainty about reporting objectives.

7. Additional analyses

7.1. The bias prediction in the Fischer and Verrecchia (2000) model

In addition to the ERC, Fischer and Verrecchia (2000) make predictions about the equilibrium behavior of other variables (see Section 2.1). Especially relevant to our study, given the potential policy implications, is that their model predicts that a decrease in σ_x^2 , by causing an increase in β (ERC), will result in a higher amount of reporting bias. The intuition is that the expected benefit from adding bias will increase (because of the higher ERC), and thus managers will have greater incentives to add bias. To test this prediction, we examine the change in earnings management (our empirical translation of reporting bias) for treatment and control firms around the CD&A rules. As measures of earnings management, we use the signed values of discretionary accruals calculated based on four different models – Dechow and Dichev model (Dechow and Dichev 2002), modified Jones model (Dechow et al., 1995), performance-matched modified Jones model (Kothari et al., 2005), and growth-adjusted modified Jones model (Collins et al., 2017) (see Appendix 2 for details). Because firms can use either income-increasing or income-decreasing discretionary accruals to manipulate earnings, we also report the results using the absolute values of discretionary accruals.²⁷

Across the four measures, as shown in Table 7, we find no evidence of a change in the signed (Panel A) or absolute (Panel B) values of discretionary accruals between treatment and control firms from the Pre to the Post period (the difference-in-differences are not statistically significant). Next, we conduct a multivariate analysis where the dependent variables are the four discretionary accruals measures and the independent variables include *POST*, *TREAT*, the interaction *POST***TREAT* and the firm characteristics included in Table 3. Again, the coefficient of interest, *POST*_{*i,t*}**TREAT*_{*i,t*} is not statistically significant both when using signed values (Panel C) and absolute values (Panel D) of discretionary accruals. In unreported tests we also examine other measures (propensity to meet or beat earnings targets, discretionary accruals calculated based on the original Jones (1991) model, working capital accruals and total accruals), with similar findings.

There may be many reasons why we do not find the increase in bias predicted by the model.²⁸ We conjecture that the model's assumption of a constant cost of adding bias c , while generally reasonable, may not hold in our setting. As noted earlier, as compensation disclosures become more precise (and thus σ_x^2 declines), it is plausible that the cost of adding bias c increases, due to greater public scrutiny. Enhanced disclosures may allow investors to better infer ex post whether management opportunistically added bias (upward or downward) for compensation-related reasons. They may also allow regulators to target those firms whose managers have stronger incentives to manipulate earnings report, raising the legal cost of manipulating earnings. In brief, managers potentially face a higher cost of adding bias. If c , rather than being a constant, varies inversely with σ_x^2 , the effect of a decrease in σ_x^2 on bias ($b^* = \beta \frac{\mu_x}{c}$) is no longer obvious: any bias-increasing effect through higher β may be offset by a bias-decreasing effect due to higher c , with the net effect depending on the precise function linking c and σ_x^2 . In contrast, even when c varies inversely with σ_x^2 , a decrease in σ_x^2 (and the resulting increase in c) still leads to a higher ERC (as noted in Section 6).

Subject to the limitations of any attempt to translate a stylized model into empirical tests, a benign interpretation of our findings (and one of potential relevance to policy-makers) is that the net effect of the CD&A rules on financial reporting was to increase investors' response to earnings releases without a corresponding increase in earnings management, because the benefit from adding bias (via a higher ERC) may have been offset by an increase in its marginal cost.

7.2. Alternative settings: the 1992 SEC compensation disclosures

The first major reform of executive compensation disclosures by the SEC took place in 1992. Using a similar research design (i.e. exploiting the staggered introduction based on firms' FY ends) we re-examine our prediction around the introduction of the 1992 disclosures. In untabulated analyses we fail to find evidence of an increase in ERC. However, the adoption of the 1992 disclosures coincides with the introduction of Section 162(m) in the Revenue Reconciliation Act of 1993, which limits the tax deductibility of top 5 executive pay to \$1 million per individual, unless it meets the definition of "performance-based pay". Section 162(m) has been

²⁷ In Fischer and Verrecchia (2000), if the manager is more likely to inflate than deflate the stock price ($\mu_x > 0$) – the most common scenario – then expected bias is positive (i.e. income increasing) and will increase when uncertainty regarding the manager's objectives is reduced (this is because a decrease in σ_x increases ERC and, thus, the benefits from biasing the report upward are higher). However, if the manager is more likely to deflate than inflate the stock price ($\mu_x < 0$), as allowed by the model (e.g. incentives to induce a lower stock price before an option grant), then expected bias is negative (i.e. income decreasing) and will be even more negative when uncertainty regarding the manager's objectives is reduced (because a decrease in σ_x increases ERC and thus the benefits from biasing the report downward are higher). In either case, the magnitude of bias (i.e., the absolute value of bias) is predicted to increase following a decrease in σ_x . Thus, to allow for both possibilities (income-increasing and income-decreasing accruals), we also report the results using absolute values of accruals.

²⁸ Available measures of discretionary accruals may do a poor job at capturing "reporting bias", a long-standing challenge in accounting research. Also, the change in ERC may be not sufficient to have a detectable effect on earnings management. However, we continue to find no change in earnings management when we repeat the analysis in Table 7 for subsets of firms where the increase in ERC is highest (profit firms and firms without a SEC comment letters; untabulated).

Table 7

Change in earnings management around the CD&A Rules

This table compares proxies for earnings management between treatment and control firms in the Pre and Post periods as well as their change from the Pre to the Post period. Panel A and Panel B report univariate differences in, respectively, the levels of *signed* and *absolute* values of discretionary accruals calculated based on Dechow and Dichev (2002), modified Jones model (Dechow et al., 1995), performance-adjusted modified Jones model (Kothari et al., 2005), and growth-adjusted modified Jones model (Collins et al., 2017). Panel C and Panel D report the results from multivariate analyses regressing proxies for earnings management on treatment indicators (POST, TREAT), their interaction (POST*TREAT, the coefficient of interest) and the firm characteristics in Table 3. Columns (1) – (4) of Panel C and Panel D report results from OLS regressions where the dependent variables are the *signed* and *absolute* values of discretionary accruals calculated based on each of the four models, respectively. The coefficients for the intercepts are untabulated. *t*-statistics, based on robust standard errors clustered at the firm level, are presented below the coefficient estimates. All the variables are defined in detail in Appendix 2. ***, **, * indicates significance level at 1%, 5%, and 10%, respectively.

Panel A Changes in Signed Discretionary Accruals					
	Treatment: Pre	Treatment: Post	Control: Pre	Control: Post	Difference-in-difference
Dechow-Dichev (2002)	0.004	−0.001	0.005	−0.002	0.002
Modified Jones (Dechow et al., 1995)	0.011	0.005	0.010	0.005	− 0.001
Performance-adjusted Modified Jones (Kothari et al., 2005)	0.000	−0.002	0.000	−0.003	0.001
Growth-adjusted Modified Jones (Collins et al., 2017)	−0.002	−0.005	−0.003	−0.004	− 0.002

Panel B Changes in Absolute Discretionary Accruals					
	Treatment: Pre	Treatment: Post	Control: Pre	Control: Post	Difference-in-difference
Dechow-Dichev (2002)	0.019	0.018	0.023	0.021	0.001
Modified Jones (Dechow et al., 1995)	0.019	0.017	0.023	0.020	0.001
Performance-adjusted Modified Jones (Kothari et al., 2005)	0.030	0.028	0.034	0.033	− 0.001
Growth-adjusted Modified Jones (Collins et al., 2017)	0.020	0.019	0.023	0.021	0.001

Panel C Regression Analysis: Signed Discretionary Accruals				
VARIABLES	(1) Dechow-Dichev	(2) Modified-Jones	(3) Performance-matched	(4) Growth-adjusted
POST*TREAT	0.002 (0.60)	−0.001 (−0.39)	0.000 (0.09)	−0.002 (−0.65)
TREAT	−0.000 (−0.02)	0.001 (0.41)	0.000 (0.09)	0.000 (0.22)
Size	−0.002*** (−4.36)	−0.001** (−1.98)	−0.001*** (−3.14)	−0.002*** (−5.23)
Market-to-book	0.000 (0.80)	0.000 (1.46)	−0.000 (−0.05)	−0.001*** (−2.63)
Beta	0.003** (2.38)	0.002** (2.50)	0.003*** (3.00)	0.002** (2.56)
Leverage	0.000 (0.08)	−0.000 (−0.86)	0.001* (1.68)	0.000 (0.84)
Earn_Persistence	−0.004 (−1.44)	−0.004* (−1.79)	−0.005** (−2.18)	−0.004** (−2.35)
Loss	−0.003 (−1.09)	−0.003* (−1.85)	−0.004 (−1.63)	0.004** (2.43)
Dispersion	−0.007 (−0.31)	0.024 (1.30)	0.022 (0.79)	0.024 (1.42)
Observations	13,724	13,196	13,053	13,315
Fixed Effects	Year-Quarter, Industry	Year-Quarter, Industry	Year-Quarter, Industry	Year-Quarter, Industry
Cluster	Firm	Firm	Firm	Firm
Adj. R-squared	0.014	0.032	0.004	0.020

Panel D Regression Analysis: Absolute Discretionary Accruals				
VARIABLES	(1) Dechow-Dichev	(2) Modified-Jones	(3) Performance-matched	(4) Growth-adjusted
POST*TREAT	0.002 (1.51)	0.001 (0.51)	−0.000 (−0.25)	0.001 (0.46)
TREAT	−0.003*** (−3.29)	−0.003*** (−3.17)	−0.002** (−2.10)	−0.002** (−2.40)
Size	−0.001*** (−6.75)	−0.001*** (−6.26)	−0.001*** (−3.97)	−0.001*** (−4.25)
Market-to-book	0.001*** (6.83)	0.001*** (4.72)	0.001*** (6.61)	0.001*** (5.75)
Beta	0.001** (2.57)	0.002*** (4.90)	0.001* (1.92)	0.001* (1.95)
Leverage	−0.002*** (−7.10)	−0.002*** (−5.47)	−0.002*** (−5.64)	−0.001*** (−4.16)
Earn_Persistence	0.000	0.000	0.001	0.001

(continued on next page)

Table 7 (continued)

Panel D Regression Analysis: Absolute Discretionary Accruals				
	(0.42)	(0.06)	(0.54)	(0.56)
Loss	0.004***	0.002**	0.006***	0.004***
	(4.43)	(2.13)	(4.67)	(4.56)
Dispersion	0.001	0.001	0.025	0.006
	(0.21)	(0.11)	(1.11)	(0.94)
Observations	13,724	13,196	13,053	13,315
Fixed Effects	Year-Quarter, Industry	Year-Quarter, Industry	Year-Quarter, Industry	Year-Quarter, Industry
Cluster	Firm	Firm	Firm	Firm
Adj. R-squared	0.087	0.090	0.095	0.085

shown to significantly affect the composition of pay (e.g. [Perry and Zenner, 2001](#); [Rose and Walfren, 2000, 2002](#); [Balsam and Ryan, 2008](#)), with many observers attributing to Section 162(m) the explosion in the use of stock options in the mid-1990s ([Murphy, 2013](#)). In other words, the concern about confounding events and potential ‘real effects’ induced by concurrent changes in compensation contracts (see [Section 6](#)) is especially pronounced around the 1992 disclosures.

Also, we believe that the 2006 rules might have had a larger effect on reducing investors’ uncertainty about reporting objectives because of their emphasis on the disclosure of compensation-based performance targets. While far reaching in scope, with regard to performance-based pay the 1992 rules mostly consolidated in tabular format information already disclosed in proxy filings.²⁹ Tellingly, [Fischer and Verrecchia \(2000\)](#) use existing compensation disclosures as an example of uncertainty about reporting objectives, implicitly suggesting that the 1992 rules in place at the time were not viewed as providing precise information about compensation-driven manager’s reporting incentives.

Future work may re-examine the [Fischer and Verrecchia \(2000\)](#) predictions using new regulatory changes or changes that might have taken place in other jurisdictions.

8. Conclusion

We empirically examine the hypothesis that the information content of the earnings report, as captured by the earnings response coefficient (ERC), increases when investors’ uncertainty about the manager’s reporting objectives decreases, as predicted by [Fischer and Verrecchia \(2000\)](#). To do so, we use the executive compensation disclosures released by the Securities and Exchange Commission in 2006 as an instrument to the change in investors’ uncertainty about the manager’s reporting objectives, based on the empirical evidence that managers’ compensation incentives are an important driver of their reporting objectives. Since these disclosures apply to all publicly traded firms, our identification strategy exploits the staggered nature of their adoption based on the firm’s fiscal year end.

Consistent with our hypothesis, we find a statistically and economically significant increase in ERC for treatment firms relative to control firms. The result holds across various specifications, including allowing for nonlinearities in ERC, and is more pronounced for firms with a greater improvement in the quality of compensation disclosures (as proxied by firms that did not receive a SEC comment letter criticizing their disclosures, firms disclosing CFO pay information for the first time and firms with higher excess CEO pay). In addition to contributing to the research on ERC and the literature on compensation disclosures, our study suggests that policy makers can increase the information content of financial reports not only by improving their credibility and quality of accounting information, but also by reducing investors’ uncertainty about managers’ reporting objectives via better disclosures of compensation-related incentives.

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²⁹ The 1992 rules mandated the inclusion of the number of options granted in the new Summary Compensation Table as well as a new table with details about the option grants (e.g. expiration date), including an estimate of their value. But, as noted by [Murphy \(2013\)](#), details about option grants were already disclosed in (another part of) the proxy filing prior to 1992, together with salary and cash bonuses. In other words, with regard to annual pay, the 1992 rules mostly consolidated in tabular format information already disclosed in proxy filings, while expanding other parts of compensation disclosures less relevant to our research question (perks, severance, pensions, etc.). The only new item with respect to equity incentives was the disclosure of aggregate option holdings (though even prior to 1992 investors could already figure out option holdings, at least in part, from past option grants).

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Appendix 1. Evidence of institutional investors' interest in the effect of compensation incentives on reporting quality

The excerpts reported below are from firm-specific reports issued by a forensic accounting firm to its clients. The firm sells the reports to its clients on a subscription basis. The objective of these reports is to identify concerns with the firm's accounting quality.³⁰ We provide these excerpts to support the notion that sophisticated institutional investors care about the potential effect of compensation-driven incentives on reported numbers.

"In the proxy filing...the Company disclosed that a free cash flow goal has been added as a metric for executive incentive compensation. In prior years these targets included only various revenue and earnings metrics. We caution that when a company makes changes to its incentive compensation plan, this can sometimes lead to management manipulation of the new metrics"

"Accelerated recognition of redemption revenue seems to have accounted for most of the company's growth in revenue, EBITDA and Cash EPS...Management cash incentive compensation plans are based on revenue and EBITDA targets."

"We raise concern that the financial performance goal in the company's annual cash bonus plan for executives is based on GAAP EPS given that the non-cash amortization of acquired contract liabilities results in an inflated EPS"

"The Company used debt to finance pension contributions during the year. The decline in pension expense (above the line) was replaced with increased interest expense (below the line) which by definition increases EBITDA margin, all else equal. EBITDA is a key metric for determining long-term incentive compensation."

"We also note that with regard to compensation under the company's Performance Plan, EBIT and EPS targets are adjusted for restructuring and other charges. Given the Company's frequent incurrence of restructuring charges, we raise additional concern regarding earnings quality given the incentive to increase restructuring charges in order to keep costs out of EBIT for purposes of compensation."

Appendix 2. Variable Definitions

Variable Definitions		
Variable	Variable Name	Description
Treatment Variables		
Treatment firms	TREAT	<ul style="list-style-type: none"> • An indicator variable that equals 1 if a firm has December FY-end, and 0 a firm's FY year ends in September through November. More precisely, given the convention followed by Compustat in coding the month of the FY-end, TREAT equals 1 if a firm has a FY-end between December 15 (when the CD&A rules became effective) and December 31, and 0 if a firm's FY-end falls between September 15 and December 14.
Post treatment	POST	<ul style="list-style-type: none"> • An indicator variable that equals 1 for a quarter in fiscal year 2007, and 0 for a quarter in fiscal year 2006.
Variables of Interest		
Market response	CAR	<ul style="list-style-type: none"> • A firm's 3-day stock return around the quarterly earnings announcement date, minus the CRSP market return over the same period. • Data are obtained from CRSP.
Earnings surprise	UE	<ul style="list-style-type: none"> • I/B/E/S actual quarterly EPS minus the most recent median forecast of quarterly EPS published by I/B/E/S prior to the earnings announcement, scaled by the CRSP price from 2 days prior to the earnings announcement. The median forecast is based on all one-quarter ahead individuals forecasts (i.e. all individual forecasts for a given quarter issued after the previous earnings announcement date). • Both the actual and forecast EPS are from I/B/E/S.
Control Variables		

³⁰ A confidentiality agreement does not allow us to disclose the identity of the forensic accounting firms nor the identity of the firms covered by the reports.

Firm Size	Size	<ul style="list-style-type: none"> • Market cap of the firm measured at the end of the fiscal year. The market cap equals the share price * the number of shares outstanding as reported by Compustat. • We use the natural log of firm size in all of our regressions.
Market to Book Ratio	Market-to-Book	<ul style="list-style-type: none"> • Ratio of market value of equity to the book value of equity, both measured at the end of the fiscal year, from Compustat.
Market beta	Beta	<ul style="list-style-type: none"> • The regression coefficient from regressing excess daily returns for a firm on excess market returns over one calendar year, ending on the fiscal quarter-end date. • The market return and risk-free rate are obtained from Ken French's data library. • The return data are obtained from CRSP.
Financial Leverage	Leverage	<ul style="list-style-type: none"> • Ratio of total debt (debt in current liabilities + long term debt) to the book value of equity, as reported by Compustat.
Earnings persistence	Persistence	<ul style="list-style-type: none"> • The regression coefficient from regressing quarterly EPS on past quarter's EPS using up to 10 years data. This is calculated at the end of each quarter. • The data are obtained from Compustat.
Accounting loss indicator	Loss	<ul style="list-style-type: none"> • An indicator variable that equals 1 if the basic earnings per share excluding extraordinary items is less than 0, and 0 otherwise.
Analysts' forecast dispersion	Dispersion	<ul style="list-style-type: none"> • The difference between highest and lowest analyst forecasts, scaled by the CRSP price from 2 days prior to the earnings announcement. • Analyst forecast data are obtained from I/B/E/S.
Nonlinearity	Nonlinear	<ul style="list-style-type: none"> • Unexpected earnings multiplied by the absolute value of the unexpected earnings (i.e. $UE * UE$).
Firms not receiving a SEC Comment Letter	No_SEC_CL	<ul style="list-style-type: none"> • An indicator variable equal to 1 if the firm did not receive a SEC Comment Letter for failure to comply with the 2006 compensation disclosures.
Firms that began to disclose CFO Pay information after CD&A rules	CFO Pay_New	<ul style="list-style-type: none"> • Indicator variable equal to one if the firm began to disclose information about CFO compensation after the CD&A rules (i.e. in the 2007 proxy statement for fiscal year 2006). The data are obtained from Execucomp.
Excess CEO pay	Excess Pay	<ul style="list-style-type: none"> • An indicator variable equal to 1 if a CEO receives abnormal compensation higher than the median of the sample during 2006. • Abnormal compensation is defined as CEO total pay net of CEO predicted pay, where the predicted pay is defined as the predicted value from a regression of the natural logarithm of total CEO compensation on proxies for economic determinants of CEO compensation, including, CEO tenure, sales, indicator for S&P 500 index, book-to-market ratio, stock return, and ROA (Ertimur et al., 2011).
Base annual salary	Salary/Total Pay	<ul style="list-style-type: none"> • This variable is calculated as SALARY scaled by TDC1, where TDC1 is the total annual compensation (including salary, bonus, restricted stocks, options and other annual pay). • The data are obtained from Execucomp.
Annual bonus	Bonus/Total Pay	<ul style="list-style-type: none"> • We use BONUS in the Pre period, and BONUS plus NONEQ_INCENT in the Post period, scaled by TDC1. • The data are obtained from Execucomp.
Stock Options	Option/Total Pay	<ul style="list-style-type: none"> • We use OPTION_AWARDS_BLK_VALUE in the Pre period, and OPTION_AWARDS_FV in the Post period, scaled by TDC1. • The data are obtained from Execucomp.
Restricted stock	Restricted Stock/Total Pay	<ul style="list-style-type: none"> • We use RSTKGRNT in the Pre period, and STOCK_AWARDS_FV in the Post period, scaled by TDC1. • The data are obtained from Execucomp.
Equity Mix	Option/Equity Pay	<ul style="list-style-type: none"> • Defined as option grants scaled by total equity pay, where total equity pay is defined as the sum of option awards and restricted stocks granted during the fiscal year. • The data are obtained from Execucomp.

Compensation delta	Delta	<ul style="list-style-type: none"> • Dollar change in wealth associated with 1% change in the firm's stock price (in \$000 s). • The data are obtained from Professor Lalitha Naveen's website, based on the methodology in Core and Guay (2002).
Compensation vega	Vega	<ul style="list-style-type: none"> • Dollar change in wealth associated with a 0.01 change in the standard deviation of the firm's returns (in \$000 s). • The data are obtained from Professor Lalitha Naveen's website, based on the methodology in Core and Guay (2002).
Value of the executive's stock and option portfolio	Value of stock and option portfolio	<ul style="list-style-type: none"> • Dollar value of the stock and option portfolio held by the executive at the end of the previous fiscal year.
Daily stock return volatility	Stock return volatility	<ul style="list-style-type: none"> • The data are obtained from Professor Lalitha Naveen's website. • Daily stock return volatility during a fiscal year.
R&D expense	R&D/Total Assets	<ul style="list-style-type: none"> • The data are obtained from CRSP. • R&D expenditures scaled by beginning-year total assets, set to zero if missing.
SG&A expense	SG&A/Total Assets	<ul style="list-style-type: none"> • The data are obtained from Compustat. • Selling, General, and Administrative Expenses scaled by beginning-year total assets, set to zero if missing. • The data are obtained from Compustat.
Capital expenditure	CAPEX/Total Assets	<ul style="list-style-type: none"> • Capital expenditures scaled by beginning-year total assets, set to zero if missing. • The data are obtained from Compustat.
Discretionary accruals calculated based on Dechow and Dichev approach (2002)	Dechow-Dichev	<ul style="list-style-type: none"> • Calculated the same way as Dechow and Dichev (2002), modified at quarterly level. • We calculate the residuals from the industry-specific (defined by 2-digit SIC code) regression of change in working capital accruals (defined as below) on cash flows from operations in past one quarter, current quarter and one quarter ahead. • Working capital accruals are calculated as $\Delta AR + \Delta Inventory - \Delta AP - \Delta TP + \Delta Other Assets (net)$, where AR is accounts receivable, AP is accounts payable, and TP is taxes payable. • Instead of using the standard deviation of the residuals for a firm, we use the absolute value of the estimated residual for each firm-quarter (as suggested in footnote 6 in Dechow and Dichev (2002)).
Discretionary accruals calculated based on modified Jones model (Dechow et al., 1995)	Modified-Jones	<ul style="list-style-type: none"> • All the variables are obtained from Compustat. • Calculated the same way as Dechow et al. (1995), modified at quarterly level as suggested by Collins et al. (2017). • We calculate the residuals from the following regression by industry and year level: $ACC_{i,t} = \beta_0 + \beta_1 Q_{1,i,t} + \beta_2 Q_{2,i,t} + \beta_3 Q_{3,i,t} + \beta_4 Q_{4,i,t} + \beta_5 (\Delta SALES_{i,t} - \Delta AR_{i,t}) + \beta_6 ACC_{i,t-4} + \epsilon_{i,t}$, where $ACC_{i,t}$ is accruals calculated from the cash flow statement as the total of changes in accounts receivables, inventories, accounts payable, tax payable, and other accounts that affect accruals (Hribar and Collins, 2002). $Q_{1,i,t}$, $Q_{2,i,t}$, $Q_{3,i,t}$, $Q_{4,i,t}$ are fiscal quarter dummies. • We use the absolute value of the estimated residual for each firm-quarter.
Performance-matched discretionary accruals (Kothari et al., 2005)	Performance-matched	<ul style="list-style-type: none"> • All the variables are obtained from Compustat. • Match each firm-quarter observation with another from the same industry and quarter with the closest beginning-quarter ROA. • Calculated as the difference in discretionary accruals between a firm-quarter and the matched firm-quarter. We use the absolute value of the difference for each firm-quarter. • Discretionary accruals are from the modified Jones model.

Growth-adjusted discretionary accruals (Collins et al., 2017)

Growth-adjusted

- Calculated based on modified Jones model, adjusted for firm growth as suggested by Collins et al. (2017).
- In the estimation equation of modified Jones model, we add ROA, seasonally differenced sales growth, and beginning-quarter market-to-book ratio as additional control variables. We follow Collins et al. (2017) to use the quintile dummies of these variables instead of the raw values of these variables to control for the non-linear relation between accruals and growth.
- We use the absolute value of the estimated residual for each firm-quarter.
- All the variables are obtained from Compustat.

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