The Effect of Option-based Compensation on Payout Policy: Evidence from FAS123R

Fabrizio Ferri[*]
Columbia University
ff2270@gsb.columbia.edu

Nan Li
Columbia University
Nli18@gsb.columbia.edu

[*] Corresponding Author: Columbia Business School, Columbia University, Uris Hall 618, 3022 Broadway, New York, NY 10027, phone: (212) 854-0425.
We thank Chris Armstrong (discussant), David Becher, Thomas Bourveau, Vivian Fang, Wei Jiang, Christian Leuz, Greg Nini, Shiva Rajgopal, Jesus Salas, Joanna Wu and workshop participants at Columbia University, Drexel University, Fordham University, Temple University, University of North Carolina, the Stanford Accounting Summer Camp and the 2016 Accounting Conference at Temple University for their comments. We thank Jennifer Blouin for sharing her data on firms repatriating funds under the American Jobs Creation Act of 2004; Jack Ciesielski of *The Analyst's Accounting Observer* for his data on accelerated vesting; Wei Jiang for her data on hedge funds' activism and Lalitha Naveen for her data on co-opted directors. The study has been accepted for presentation at the 2017 American Finance Association Annual Meeting.

The Effect of Option-based Compensation on Payout Policy: Evidence from FAS123R

Abstract:

Does option-based compensation have a causal influence on payout policy? To address this question we examine the adoption of mandatory expensing of stock options (via accounting standard FAS123R), a plausible exogenous shock to the use of option-based compensation. As FAS123R applies to all firms, our identification strategy exploits the fact that the reduction in option-based compensation in response to the accounting standard varies with the firm-specific expected accounting impact, as measured by the option expense disclosed in the footnotes prior to FAS123R. Using a difference-in-difference research design we do not find that (accounting-driven) reductions in option-based pay cause dividends to increase, repurchases to decrease or the payout composition to change, except for some increase in dividends among dividend-paying small firms. Our results contrast with the widely held belief that option-based pay has a significant causal influence on payout policy and cast doubts on its role in the shift from dividends to repurchases in recent decades.

JEL Classification: G35, M41, M48, M52
Keywords: dividends, repurchases, executive stock options, FAS123R, payout policy

1. Introduction

In this study, we examine whether option-based executive compensation has a causal influence on payout policy. The motivation for the study is two-fold. The first is to better understand the economic consequences of the use of stock options to compensate executives. While the incentive properties of stock options are well studied, evidence on whether and how much these incentives affect firm outcomes is still tentative, with recent studies raising the question of whether these effects are significant (e.g. Hayes, Lemmon and Qiu 2012, failing to detect a casual effect of option-based pay on risk taking). The second motivation is to shed light on the determinants of payout policy.[1] As noted in a recent paper (Farre-Mensa, Michaely, and Schmalz, 2014), traditional theories of payout policy (e.g., agency, signaling, clientele and tax-based theories) help to explain cross-sectional variation in corporate payouts but have limited power in explaining why repurchases have replaced dividends as the primary payout vehicle over the last 30 years (Skinner 2008). In contrast, a number of studies suggest that this trend may reflect the simultaneous increase in the use of stock options for executive pay.

Previous research has identified two ways option-based pay may affect payout policy. The first is the dividend-protection channel (Lambert, Lanen and Larcker 1989). Because the value of a call option decreases with dividends and executive stock options are generally not dividend-protected (due to an unfavorable accounting treatment),[2] option grants give managers the incentive

[1] Payout policy plays a key role in corporate finance (Miller and Modigliani 1961; Jensen 1986; Fama and French 2001). How much cash to return to shareholders and how to do it (i.e. dividends or repurchases) has implications for the firm's investment and capital structure policy, signals the quality of future prospects, affects investors' taxes and, ultimately, impacts firm value.

[2] Zhang (2013) finds that between 2000 and 2009 less than 1% of S&P 500 firms provide dividend protection, confirming evidence for earlier periods (Murphy 1999; Weisbenner 2000; Cuny, Martin and Puthenpurackal 2009). Researchers have attributed this low frequency to the unfavorable accounting treatment (Fenn and Liang 2001). Dividend protection is effected through adjustments to an option's exercise price. Under Accounting Principles Board Opinion No.25 (APB 25), dividend-protected options were considered variable plan options because the exercise price was not "fixed" but contingent upon future events (e.g., a dividend payment). As such, their cost had to be reported in

to avoid/reduce dividends and (if there is a target payout amount) to replace them with repurchases. The second is the dilution channel, that is, the incentive to use repurchases to offset the dilutive effect from the exercise of employee stock options (ESO) (Khale 2002). A variation of this argument further notes that, even before the exercise of ESO, managers have an incentive to use repurchases to manage the dilutive effect of in-the-money unexercised options on diluted earnings-per-share (EPS) (Bens, Nagar, Skinner, and Wong 2003).[3] Under these dilution-related arguments, firms with more ESO have an incentive to engage in larger repurchases and, if they have a target payout amount, to replace dividends (or future dividend increases) with repurchases.

In sum, both the dividend-protection and the dilution channels predict that managers of firms with a large amount of option-based compensation will favor repurchases over dividends. A number of studies have provided empirical evidence consistent with this prediction.[4]

However, there are three reasons to re-examine this question. First, if boards anticipate the consequences of the lack of dividend protection, they can increase management compensation to offset the loss caused by the dividend payments. Second, changes to payout policy are visible and highly scrutizined. Given the relevance of payout choices to boards, analysts and institutional

---

the income statement at each measurement date (each quarter) based on their intrinsic value (difference between stock price and exercise price). In contrast, options with "fixed" terms required a compensation expense based on intrinsic value at the grant date (typically zero, since fixed options were granted with an exercise price equal to the stock price).

[3] Generally Accepted Accounting Principles (GAAP) require firms to report basic EPS and diluted EPS. The denominator of basic EPS is generally the number of common shares outstanding and thus it is affected by ESO only when they are exercised. In contrast, the denominator of diluted EPS takes into account ''potentially dilutive'' securities, such as ESO, using the so-called treasury stock method. Under this method, the denominator of diluted EPS increases as outstanding unexercised ESOs move into the money. Note that firms have incentives to use repurchases to manage EPS regardless of whether they use ESO (Hribar, Jenkins and Johnson, 2006).

[4] Lambert et al. (1989) finds that dividends decrease in the five-year period following the adoption of executive stock option plans. Jolls (1998) documents that firms relying more on option compensation are more likely to buy back shares. Weisbenner (2000) documents a positive association between exercises of ESO and repurchases. Fenn and Liang (2001) show that option compensation is negatively associated with dividends and positively associated with repurchases. Kahle (2002) finds that the number of exercisable employee stock options predicts the occurrence and amount of repurchases (consistent with the dilution channel) and that the number of executive stock options predicts the repurchase decision (consistent with the dividend-protection channel). Chetty and Saez (2005) finds that firms whose executives held more options were less likely to increase dividends in response to the dividend tax reduction in 2003. Zhang (2013), Burns, McTier and Minnick (2015) and Minnick and Rosenthal (2015) document that firms with a greater proportion of dividend-protected equity pay have higher dividend payouts. Bens et al. (2003) and Cuny et al. (2009) show that repurchases are positively correlated with the dilutive effect of ESO on diluted EPS.

investors, it would be difficult for managers to modify payout policy to maximize personal wealth without facing serious scrutiny. Third, the associations documented in earlier studies may reflect the endogenous nature of executive pay. A compensation contract is designed to mitigate agency problems that manifest in various corporate policies. This feature makes treatment assignment (e.g. stock option grants) prone to factors that could affect both executive pay and payout policy, some of which may be unobservable and cannot be easily controlled for (the omitted variable bias problem). For example, a firm with a positive innovation opportunity shock may both choose a low dividend level to preserve internal capital, and grant executive stock options to induce risk taking. Furthermore, executive pay may be the result of anticipated payout policy (the reverse causality problem). For example, if a board wants to raise dividends, it may give the CEO a pay package with fewer options and more (dividend-paying) restricted stocks (Aboody and Kasznik 2008). Different causal channels linking executive pay and firm payout possibly coexist and are difficult to disentangle empirically. As emphasized by Edmans and Gabaix (2015), the effects of executive compensation in general have not been satisfactorily identified, largely due to the scarcity of valid instrumental variables.

To address these problems we utilize the 2005 adoption of Financial Accounting Standard 123R (hereinafter FAS123R) as an exogenous shock to the use of option compensation.[5] Previously, firms were required to expense ESO's intrinsic value at the grant date while disclosing the fair value amount in footnotes (this amount is referred to as 'implied option expense'). As most ESO were granted at the money and, thus, with zero intrinsic value (Hall and Murphy, 2002), firms reported no expense in their income statements. FAS123R required firms to expense ESO in the

[5] FAS123R was introduced in response to the Enron-type accounting scandals of 2001-2002. At that time, many observers argued that overly favorable reporting treatment had led to an excessive use of option-based compensation (Hall and Murphy 2003) that ''perversely created incentives to artificially inflate reported earnings in order to keep stock prices high and rising'' (Greenspan 2002). Thus, it is reasonable to assume that FAS123R was exogenous with respect to trends in payout policy.

income statement at fair value. Consistent with FAS123R increasing the perceived cost of stock options (Murphy 2013), various studies have documented a large decrease in the use of stock options subsequent to its introduction (Carter, Lynch, and Tuna 2007). Hayes, Lemmon and Qiu (2012) report a decline in the median ratio of the value of option grants to total pay from 39.7% to 13.9% following FAS123R's implementation. Such a large, sudden change in executive pay composition is rare (Frydman and Jenter 2010) and thus provides a unique opportunity to investigate its effect on payout policy.

An additional benefit of this setting is that the decrease in ESO predicts an *increase* in dividends. Settings that predict a *decrease* in dividends are arguably less powerful as firms are notoriously reluctant to cut dividends given the evidence of a negative investors' reaction (Brav, Graham, Harvey and Michaely 2005).[6] Another advantage is that the post-FAS123R period witnessed a general increase in corporate payouts (Floyd, Li and Skinner 2015). These factors all combine to produce a powerful setting to detect a compensation-induced increase in dividends.

While FAS123R created an exogenous shock to the use of option-based compensation, a major challenge to the research design remains. The accounting standard is applied to all public firms, making it difficult to construct control groups. To tackle this challenge, our empirical strategy exploits the idea that the impact of FAS123R likely varies across different types of firms. We conjecture that firms facing a larger hit on their reported earnings (as measured by a higher implied option expense in 2002, *prior to* FAS123R) will reduce option grants significantly more than other firms. Indeed, in our sample, option compensation as percentage of total executive pay drops from 50.7% to 32.5% for firms in the top quartile of implied option expense versus 20.1% to 16.7% for firms in the bottom quartile. Using this cross sectional variation, our strategy is a

[6] Indeed, association studies reporting a negative association between option-based pay and dividends frame the issue in terms of *lower dividend increases* (i.e. repurchases substituting for future dividend increases in option-heavy firms) rather than in terms of *dividend decreases*.

difference-in-difference design where the first difference is the adoption of FAS123R and the second is the pre-event implied option expense, effectively our proxy for the expected accounting-induced reduction in option-based compensation. The key identification assumption is that the *level* of implied option expense (prior to the announcement of FAS123R) is not correlated with the potential future *change* in payout variables in the absence of treatment (i.e. FAS123R). We discuss this assumption further in Section 2.

We perform our empirical tests using a sample of S&P 1500 firms between 2003 and 2007, a five-year event window surrounding the adoption of FAS123R. In particular, we estimate the association between implied option expense, measured in 2002 and thus prior to the event window, and the change in the average payout variables from the "pre" (2003-2004) to the "post" (2005-2007) period, after controlling for known determinants of payout policy. Across various measures of dividend changes, we do not find that firms with higher implied option expense increased their dividends relative to firms with lower implied option expense, both in the full sample and in a sub-sample of firms that paid dividends prior to FAS123R. Also, among firms that did not pay dividends prior to FAS123R, those with higher implied option expense did not experience a relative increase in the rate of dividend initiation. This lack of an effect on dividend payouts is remarkable given that the accounting-induced decline in option-based pay was accompanied by a corresponding increase in restricted stock awards, which generally pay dividends. Further, we find no association between implied option expense and changes in the level of repurchases or changes in the relative weights of dividends and repurchases.

Our results are robust to using alternative measures of dividends and repurchases, employing a panel regression (rather than a first-difference regression), and controlling for the potential effects of concurrent events (tax-driven repatriation of foreign earnings, rise in hedge

fund activism). They are also robust to excluding firms that may have taken actions in anticipation of the expected adoption of FAS123R (e.g. voluntarily expensing options) and to alternative ways to address the lack of a control group (Bakke, Mahmudi, Fernando and Salas 2015). Finally, we examine sub-samples where we argue the opportunity and incentives to modify payout policy are presumably higher (e.g. greater CEO influence on the board, lower monitoring, higher ownership by outside directors), but find little evidence of an effect.

Overall, the evidence is not consistent with the prediction that option-based compensation has a first-order causal impact on payout policy and suggests that the previously documented associations perhaps reflect endogeneity rather than a causal influence. Our findings should not be viewed as denying that stock options provide an *incentive* to favor repurchases over dividends. Rather, they suggest this incentive does not imply an unconstrained *ability* to change payout policy in a self-serving way, likely because of the visibility of payout decisions and their relevance to boards and institutional investors.[7] In partial support for this conjecture, in additional analyses we find an increase in dividend payouts among small firms not included in the S&P 1500 index, presumably subject to less monitoring and scrutiny. An additional explanation for our results is that boards compensate executives for the lack of dividend-protection in other ways, effectively neutralizing those incentives. Importantly, our findings are consistent with managers' statements that lack of dividend-protection does not affect payout decisions (Brav et al. (2005), who highlight this stark contrast between their survey evidence and the empirical associations in the literature).[8]

[7] Note that our results are not inconsistent with evidence that managers take actions to increase personal wealth in settings where the level of scrutiny is lower (e.g. timing of option grants, backdating; Heron and Lie 2007) or where it is harder for outsiders to detect personal motives (e.g. risk-taking activities that increase the value of options; Bakke et al. 2015).

[8] Only 10.6% of surveyed executives agree that lack of dividend protection may cause managers to favor repurchases over dividends – the lowest level of support among all the questions in the survey. Of course, it is possible that managers would not admit to the effect on their behavior but similar surveys have elicited candid responses. For example, a large percentage of managers surveyed by Graham, Harvey and Rajgopal (2005) admit that they would sacrifice economic value to smooth earnings or to hit an earnings target.

Our results also provide another explanation for the persistent lack of dividend protection in executives' options: boards are not concerned with its effect because they can mitigate it by directly monitoring payout choices and/or adjusting compensation packages. Indeed, recent surveys suggest that boards are well aware of the potential impact of executive pay on payout policies (Tapestry Networks, 2016).

Our study contributes to a recent stream of research that uses exogenous shocks, including FAS123R, to understand the causal effect of option-based pay on economic outcomes. This research has focused on the impact on risk-taking (Low 2009; Chava and Purnanandam 2010; Hayes et al. 2012; Gormley, Matsa and Milbourn 2013; Bakke et al. 2015; Chu and Ma 2015; Shue and Townsend 2016). In contrast, we examine a different yet important economic outcome: payout policy. Also, our metholodogy differs from prior studies using FAS123R in that (i) it explicitly incorporates cross-sectional variation in how firms were affected by FAS123R and (ii) with some plausible assumptions, it can be applied to all firms in the Compustat universe without requiring executive pay data. Both features can benefit future research in this area.

Our study also contributes to a vast literature on the determinants of payout policy. First, while many studies document an association between option-based pay and payout policy, limited attempts have been made to examine its causal nature. Addressing this question will enhance our understanding of temporal changes in payout policy, such as the emergence of repurchases as primary payout vehicle (Farre-Mensa et al. 2014) and the recently observed 'resilience' of dividends (Floyd et al. 2015). Using the plausible exogenous variation in ESO induced by FAS123R, our evidence calls into question the notion that option-based compensation is a first-order driver of payout policy and challenges future research to consider alternative explanations for the trends in corporate payouts. Second, we extend the research that examines how

institutional changes affect payout policy. Most of these studies examine changes in dividend tax rates (e.g. Chetty and Saez 2005; Brown, Liang and Wesibenner 2007; Aboody and Kasznik 2008; Blouin, Raedy and Shackelford 2011; Hanlon and Hoopes 2014) or in the information environment (Hail, Tahoun and Wang 2014), while we examine a new accounting standard that affected the composition of executive pay.

Finally, we extend the literature on how accounting rules impact a firm's economic behavior (e.g. Carter and Lynch 2003; Carter et al. 2007; Bens and Mohanan 2008; Choudhary, Rajgopal and Venkatachalam 2009; Graham, Hanlon and Shevlin 2011) as well as the literature on the accounting treatment for stock options (Aboody 1996; Dechow, Hutton and Sloan 1996; Aboody, Barth and Kasznik 2004; Ferri and Sandino 2009).

2. Research Design

2.1 Empirical Strategy

It is commonly recognized that identifying the causal effects of executive compensation in empirical studies is challenging (Edmans and Gabaix 2015). FAS123R provides a unique opportunity to address this problem. Aside from significantly reducing the use of option compensation, the accounting change should not, in itself, correlate with the outcomes of interest (i.e. change in payout policy).[9] However, utilizing FAS123R is not easy because it affected all U.S. firms at the same time, making it difficult to measure a counterfactual change around the event based on a control group. A simple before-after comparison of the outcome variable around the adoption of FAS123R would assume that any change is driven entirely by the event of interest (i.e. it would assume away any conterfactual trend in the outcome variable), a questionable

[9] Hayes et al. (2012) describes FAS123R as an exogenous change in the accounting benefits of stock options without a change to the economic costs and benefits of stock options, while Bakke et al. (2015) describes it as causing a change in executives', but not shareholders', incentives.

assumption. An alternative strategy is to use the change in option-based compensation around FAS123R, effectively assuming that such change is "exogenous" (e.g. Chava and Purnanandam 2010; Anantharaman and Lee 2014). The problem with this approach is that FAS123R per se did not generate *exogenous cross-sectional* variation in option compensation. Even if the introduction of FAS123R is exogenous, a firm's response cannot be assumed to be entirely exogenous.[10]

To identify the exogenous portion of firms' response to the new standard, we make use of evidence that FAS123R's impact on the use of option compensation varies with the firm-specific expected accounting impact of the new rule, as measured by the implied option expense disclosed in the footnotes prior to FAS123R (Hayes et al. 2012). The reduction in option usage is highest in firms with the largest implied option expense. Thus, we can use the differential accounting impact of FAS123R to overcome the problem of lacking control group.

To validate our approach, we estimate the following first-differencing regression with ordinary lease square (OLS):

$$\Delta Option_i = \beta_0 + \beta_1 \, Accounting\ Impact_i + \Delta Control_i + \varepsilon_i \qquad (1)$$

where $\Delta Option_i$ is the change in the average value of executive stock options as percentage of total pay from the pre- to the post-FAS123R period. $Accounting\ Impact_i$ is the implied option expense measured prior to FAS123R (more details in Section 2.2). We expect β_1 to be negative, that is, a more pronounced reduction in option-based pay for firms with higher expected accounting impact.

Next, after validating the differential impact of FAS123R, we examine the central question of this study by estimating the following OLS regression:

[10] A universally assigned *encouragement* to receive treatment does not make the *realization* of treatment assignment free from endogeneity concerns. Consider the following example: after the adoption of FAS123R, firm A reduces the weight of option-based pay by 20% and firm B by 40%. A finding that firm B increases dividends compared to firm A cannot be interpreted as evidence of a causal influence of option compensation on dividend policy. This is because the *additional* reduction in option compensation by firm B relative to firm A may not be driven by FAS123R. It may, instead, reflect an attempt to align executives' incentives with anticipated changes in dividend policy (reverse causality). Without a way to incorporate the heterogeneous impact of FAS123R in the research design, the *cross sectional* variation in changes in option-based compensation is still subject to the usual endogeneity concerns.

$$\Delta Payout_i = \gamma_0 + \gamma_1 \, Accounting\ Impact_i + \Gamma Control_i + u_i \qquad (2)$$

where $\Delta Payout_i$ represents the change in the average value of one of the payout variables of interest (described in Section 2.3) from the the pre- to the post-FAS123R period.

Eq. (2) is essentially a difference-in-difference approach to estimate the effect of option-based pay on payout policy using the differential accounting impact of FAS123R to capture the exogenous (accounting-induced) portion of the change in option-based pay. If option-based pay has a detectable causal effect on dividends (repurchases) in the direction predicted by the literature, γ_1 should be positive (negative).

As noted by Bertrand, Duflo, and Mullainathan (2004), in difference-in-difference studies with panel data, OLS may yield inconsistent standard error estimates because the dependent variable and the main independent variable tend to be serially correlated within firm. To alleviate this concern, following Bertrand et al. (2004), in both Eqs. (1) and (2) we collapse the panel to a single "Pre" (2003-2004) and a single "Post" (2005-2007) period by averaging across years and then measuring the changes from Pre to Post.[11] We choose 2003 as the starting period in order to capture a period subsequent to the 2003 tax cut on individual dividend income. While the evidence on the effect of the tax cut on payout policy is mixed (Chetty and Saez 2005; Edgerton 2013; Floyd et al. 2015), starting with 2003 allows the pre-FAS123R levels of the key variables to reflect any effect of the tax cut.[12] We end our analysis in 2007 to avoid the potentially confounding effect of the 2008 financial crisis which had a significant impact on payouts (Floyd et

[11] In the collapsed two-period panel, a regression of changes is equivalent to a regression of levels with firm-fixed effects and an indicator for the Post period. Both approaches obviate unobserved time-invariant firm heterogeneity. We choose first differencing equation for a more intuitive interpretation.

[12] The Jobs and Growth Tax Relief Reconciliation Act of 2003 was signed into law on May 28, 2003 but had been proposed on January 7, 2003 and was made retroactive to the beginning of 2003. Chetty and Saez (2005) provide some evidence that the tax change affected dividend policy in 2003.

al. 2015).[13] Following Hayes et al. (2012) and Bakke et al. (2015), we include 2005 in the Post period because FAS123R, while effective in June 2005, was released in December 2004.[14] Alternatively, in untabulated tests, we exclude 2005 from the analysis and obtain similar results.

Another benefit of using a single Pre and Post period is that it is better suited than panel data to the analysis of repurchases. As noted by Skinner (2008), firms engaging in repurchases tend to do so every other year; thus the relation between repurchases and economic determinants is stronger over two- or three-year periods than one-year periods. Factors explaining the timing of repurchases differ from those explaining their level and the year-to-year variation in repurchases is not necessarily informative of changes in payout policy. As we are not interested in the timing of repurchases, our approach seems to better capture "long-run" changes in payout policy. However, a disadvantage of collapsing the panel is the potential loss of statistical power due to ignoring a portion of time-series information in the data. To cope with the concern of low power, we also present the results using a panel regression with firm- and year-fixed effects for the analysis of dividends (which, are generally paid every year and thus not subject to the above problem). When doing so, to alleviate the problem of serial correlation we cluster the standard errors by firm.

2.2 Accounting Impact

As noted earlier, all firms were affected by FAS123R, making it difficult to identify a control sample. To capture variation in the cross-sectional impact of FAS123R, in Eqs. (1) and (2) we use *Accounting Impact*$_i$, defined as the implied option expense disclosed in the footnote of the income statement for the 2002 fiscal year, scaled by total assets (we obtain similar findings when

[13] In November 2006, the Securities and Exchange Commission required more details about the terms of performance-based incentive plans. We are not aware of any evidence that these rules affected the weight of option-based pay (Gipper 2015) or had any effect on payout policy. However, we exclude 2007 from the Post period in robustness tests. Our inferences are unchanged.

[14] FAS123R was released in December 2004, effective as of the beginning of the first interim or annual reporting period beginning after June 15, 2005. Subsequently, the SEC allowed a six-month deferral, making the standard effective for fiscal years beginning after December 15, 2005.

scaling it by revenues). We measure this variable in 2002 (i.e. *before* the Pre period) rather than in 2003 or 2004 to avoid any confounding effect from the expected adoption of FAS123R (see discussion in Section 3.3). As this variable is highly skewed (see Table 1), we define it based on its quartile rank. We also presents results with separate indicators for the top three quartiles in order to better isolate the difference between top quartile (the most 'treated' group) and bottom quartile (the least 'treated' group, and thus, closer to the notion of a control group).

To visualize the differential impacts of FAS 123R on firms with different expected accounting impact, in Figure 1 we plot the trends of option-based compensation for each accounting impact quartile. Compared with firms in quartile 1 (lowest accounting impact), firms in quartile 4 (highest accounting impact) had much sharper decrease in option-based pay in 2005 and 2006. Furthermore, the differential trends of option-based pay are not visible from 2002 to 2004, suggesting firms in different quartiles would have similar trends of option-based pay in the absense of FAS 123R.

An alternative way to address the issue of a lack of control group is suggested by Bakke et al. (2015). They argue that two sets of firms were likely 'unaffected' by FAS123R (i) firms that did not grant options to the CEO in 2003 and 2004, as in these firms FAS123R could not affect CEO option pay and thus her incentives to make policy changes; (ii) firms that were already voluntarily expensing stock options as of 2002 (i.e. before FASB began discussions of the new standard), as these firms would not face any additional accounting charge under FAS123R.

Our approach offers a number of advantages. First, instead of collapsing all 'non-control' firms in a single 'treated' group, we exploit cross-sectional variation among treatment firms in terms of the likely impact of FAS123R, effectively using the most and least affected firms (respectively, top and bottom quartile of implied option expense) as treatment and control group.

As shown in Table 1, the implied option expense is highly skewed (i.e. only a relatively small number of firms was heavily affected by FAS123R). Capturing this variation is likely to increase the power of the test. Second, in Bakke et al. (2015) what matters is that the treatment definition captures an accounting-driven change in CEO option grants as their focus is on CEO risk-taking incentives. In our setting, we want to capture an accounting-driven change both in CEO option grants (predicted to affect incentives to use dividends) and in the overall use of stock options (predicted to affect the use of repurchases). As noted in Section 3.1 (see footnote 20), many firms without CEO option grants in 2003 and 2004 were issuing options to non-CEO executives and other employees and thus facing a potential accounting charge under FAS123R. Third, about half of the firms without CEO option grants in 2003 and 2004 issued options to the CEO in the Post-period. Hence, it is not clear that the control firms were truly unaffected by FAS123R. Finally, the implied option expense in 2002 (our measure of the expected accounting impact) is a function of option grants made in prior years, when firms could not anticipate the adoption of FAS123R. In contrast, option grant decisions in 2003 and 2004 may be affected by its anticipated adoption as FASB added accounting for stock-based compensation to its agenda in March 2003.

Notwithstanding the above arguments, throughout our study we also present the results using the definition of treatment and control in Bakke et al. (2015). In particular, we modify Eqs. (1) and (2) by replacing *Accounting Impact* with *Treated*, an indicator equal to zero for the two groups of firms identified as 'control' in Bakke et al. (2015) and one for all other firms.

2.3 Payout Variables and Control Variables

Following Brown et al. (2007), our baseline measure for changes in dividends is the change in the *Dividend/Assets* ratio—computed as the regular cash dividends paid during the year scaled

by total assets at the beginning of the year—from the Pre to the Post period.[15] However, the dividend-to-asset ratio is not what managers focus on when making dividend policy decisions. The stability of dividend per share (DPS) among dividend-paying firms (hereinafter payers) suggests that managers do not target a specific dividend-to-assets ratio, but rather a DPS level (Baker and Wurgler 2016). Survey evidence confirms that executives make dividend policy decisions in terms of changes to the current level of DPS (Brav et al. 2005). Therefore, we also examine the *percentage change* in the average DPS from the Pre to the Post period. However, this measure can only be computed for the subset of payers (defined as firms with non-zero DPS in at least one of the years of the Pre period). To provide insights into FAS123R's effect on dividend policy for non-payers, we examine the relation between our *Accounting Impact* variable and the rate of dividend initations in the Pre and Post periods.

Our main measure for repurchases is the change in *Repurchase/Assets*—that is the aggregate amount of common stock repurchases made during the year scaled by total assets at the beginning of the year—from the Pre to the Post period. In the empirical tests, we examine variations suggested in the literature.

Finally, as an alternative to examining dividends and repurchases separately, we also look at a measure of the payout composition, the change in the *Dividends/Total Payout* ratio from the Pre to the Post period (*Total Payout* is the sum of dividends and repurchases), effectively capturing any substitution effect between dividends and repurchases.[16] Examining the causal effect of the

[15] An alternative choice would be to scale dividends by the market value of equity, essentially computing a dividend yield. However, a drawback of using the dividend yield in our setting is that, after controlling for time-invariant firm characteristics through fixed effects or first differencing, the variation of the ratio comes mainly from the denominator, the stock price (Baker and Wurgler 2004). Thus we choose the more stable book value of total assets as denominator.

[16] Fama and French (2001) suggest that repurchases are often complement to dividends, while Grullon and Michaely (2002) document that the increase in share repurchases over the past decades has come at the expense of a lower increase in dividends, consistent with a substitution hypothesis. Brown et al. (2007) also provide evidence of a substitution effect in response to the 2003 dividend tax cut. Survey evidence in Brav et al. (2005) suggests an asymmetric substitution effect, with managers more willing to replace dividends with repurchases than the opposite.

change in option-based pay on payout composition is especially relevant if managers first target a total payout and then choose the relative weight of dividends and repurchases. In practice, it is unclear to what extent managers act this way (Brav et al. 2005). Nonetheless, for completeness we also examine this variable. We focus on the subsample of firms with both dividends and repurchases in both Pre and Post period so as to capture changes in payout mix by firms routinely using both vehicles to return cash to shareholders.

As for the control variables, in both Eqs. (1) and (2), $Control_i$ is a vector of firm characteristics identified in existent literature as determinants of payout policy (e.g. Fenn and Liang 2001; Brown et al. 2007; Skinner 2008; Cuny et al. 2009): firm size, book-to-market, leverage, asset tangibility, free cash flows, cash holdings, volatility in operating income, institutional ownership, sales growth, past stock returns and an indicator for loss firms. For all of these variables, we include both changes from Pre to Post period *and* levels in the Pre period, to allow for the possibility that levels of these variables affect subsequent changes in payout policy.[17] Finally, we include industry-fixed effects (Fama-French 48 industry indicators) to control for industry-level trends in payout policies and option-based pay. In unreported tests, we replace free cash flows with two separate variables: operating profit and capital expenditures. We also replace volatility in operating income with volatility in stock returns. Our results are unaffected. More detailed definitions of each variable are in the Appendix 1.

2.4 Identification Assumption

The key identifying assumption underlying the difference-in-difference estimation technique is that the parallel trends assumption is satisfied. In the absence of treatment, both treated and control firms should experience parallel trends in the outcome variable. Our approach

[17] In untabulated analyses, we run our tests after excluding changes in the control variables (i.e. including only their pre-treatment levels), since in difference-in-difference studies controlling variables measured after the shock may reintroduce selection bias. Our inferences remain unchanged.

assumes that, in the absence of FAS123R, the *change* in dividends or repurchases would be the same for high and low accounting impact firms. Note that the well-documented contemporaneous correlation between *levels* of option compensation (a key driver of the implied option expense and, thus, the accounting impact) and *levels* of payout variables does not violate our identification assumption. This is because our assumption pertains to the relation between the *level* of option compensation and future *changes* in payout variables.

A specific concern is that options may be granted in anticipation of expected future changes in payout policy (reverse causality problem). Relative to simple association tests, this concern is significantly reduced in our setting because the accounting impact is measured three years prior to the arrival of treatment and is the result of option grants made over the prior four or five years, depending on the vesting schedule.[18] For the reverse causality to be plausible, the implied option expense in 2002 (the result of option grants made approximately between 1998 and 2002) needs to reflect the anticipated change in payout policy in 2005-2007. This seems unlikely.

While the parallel trend assumption is not directly testable, similar to other difference-in-differences studies we examine the trends in dividends and repurchases prior to the event of interest. Figure 2, Panel A and B, suggests that high and low accounting impact firms (respectively, top and bottom quartile of implied option expense) exhibit similar trends in payout variables during the Pre period (2003 and 2004). They also exhibit fairly similar trends between 2001 and 2004, except for the change between 2002 and 2003. This is consistent with the 2003 dividend tax cut representing a shock to payout policy (Chetty and Saez 2005), with a differential impact based on firm characteristics potentially correlated with the use of stock options (and thus with our proxy for accounting impact), and provides support for our choice to include in the Pre

[18] The accounting treatment for stock options requires their fair value at the grant date to be recognized over the service period (usually the vesting period). Hence, the implied option expense in a given year includes a fraction of the expense associated with past option grants.

period only 2003 and 2004 (post-dividend tax cut years), as in Bakke et al. (2015). Figure 2, Panel C and Panel D, suggests similar (and perhaps even more parallel) trends in payout variables also when using the treatment and control definition of Bakke et al. (2015).

More formally, to examine the correlation between *Accounting Impact* and trends in the payout variables during the Pre period, we estimate the following regression only with observations in 2003 and 2004:

$$Payout_i = \gamma_0 + \gamma_1 Accounting\ Impact_i * FY2004 + \gamma_2 FY2004 + \Gamma Control_i + Firm\ FE + u_i \quad (3)$$

where $Payout_i$ is either *Dividend/Assets* or *Repurchase/Assets*; *FY2004* is an indicator of fiscal year 2004; *Firm FE* indicates firm-fixed effects. If firms with varying degrees of accounting impact have different payout trends *before* the adoption of FAS123R, γ_1 should be different from zero. However, for both payout variables, across our proxies for accounting impact, we find that the coefficient of interest is close to zero and not statistically significant at 10% level (see Appendix 2). Overall, the parallel trends assumption seems reasonable in our setting.

3. Empirical analyses

3.1 Sample Selection and Summary Statistics

Our sample is the S&P 1500 firms with available data on implied option expense (from Compustat) and on executive compensation (from Execucomp) for at least one year in the Pre period and one year in the Post period, to ensure consistency in the sample across periods. Similar to prior studies on payout policy (Fama and French 2001; Skinner 2008), we exclude financial firms (SIC code between 6000 and 6999) and utilities (SIC between code 4900 and 4999) as well as firms not incorporated in the U.S. Our final sample includes 1,176 firms.[19] We obtain dividend

[19] The results presented in the study are similar when: (i) we restrict the sample to firms with available data for the entire (2003-2007) sample period (1009 firms) and (ii) we exclude firms with CEO turnover in the Post period

and stock returns data from CRSP, stock repurchases and dividend per share data from COMPUSTAT, and institutional ownership data from Thomson Reuters 13F database. Other control variables are from CRSP and COMPUSTAT (see Appendix 1 for details). To deal with outliers, we winsorize all continuous variables at 1% and 99% by fiscal year.

Table 1 Panel A presents summary statistics of the relevant variables for the pooled sample of 5,653 firm-year observations. Implied option expense in 2002 (our proxy for *Accounting Impact*) is, on average, 1.73% of total assets, with the median at 0.52%. This suggests that the accounting impact is very large for a small number of firms. Over the entire sample period, on average dividends represent 0.93% of total assets while repurchases represent 3.94%. Untabulated analyses indicate that in 16% of the firm-year observations only dividends are paid out, in 25% only repurchases, in 32% both and in 27% neither. Within the subset of firm-years with a payout, dividends represent 38.74% of total payout. These data confirm prior evidence that repurchases have assumed the primary role as a vehicle to return cash to shareholders (e.g. Floyd et al. 2015). The mean (median) total executive pay (average across the top 5 executives) is $2.5 million ($1.6 million). On average, option grants and restricted stock grants represent, respectively, 28.3% and 14.1% of total pay, while salary and bonus comprise 29.6% and 19.7%, respectively. The composition of CEO pay (untabulated) is similar. As for the control variables, the figures are generally consistent with other studies examining S&P 1500 firms over the same period.

Panel B reports the pairwise Pearson correlations among the relevant variables. The correlations are in line with the associations documented in prior studies. For example, larger firms and firms with higher profitability (free cash flows) have higher payouts, loss firms have lower payouts, greater volatility in operating income is associated with lower dividends.

(reducing the sample size to 780 firms) as a new CEO may have a different compensation package and/or adopt a different payout policy.

More relevant to our analysis, Panel C presents the mean of each variable in the Pre and Post period for the full sample. For each variable, we first compute the firm-level mean over the Pre and the Post Period, and then compute the mean of the resulting figure across all firms. There is a striking increase in repurchases, which double from 2.45% to 4.90% of total assets, causing a similar increase in total payout. The increase in dividends is much smaller, from 0.83% to 0.99% of total assets. As a result, for firms with positive payout, dividends become a smaller fraction of total payout in the Post period (27.90% versus 39.14% in the Pre period). This trend is consistent with Floyd et al. (2015), who document a dramatic increase in repurchases in the years leading to the 2008 financial crisis. Consistent with previous studies, around FAS123R there is a significant reduction in option grants as a fraction of total executive pay (from 34.3% to 24.0%) and a corresponding increase in the use of restricted stock (from 8.7% to 17.6%), with little variation in the weight of salary and bonus (restricted stock, salary and bonus were expensed both before and after FAS123R). Similar patterns hold for CEO pay (untabulated). Compared to changes in the mix of executive pay, changes in the levels of control variables are generally less pronounced.

Panel D reports the change in the key compensation and payout variables from the Pre to the Post period by quartiles of implied option expense. Confirming the skewness suggested by Panel A, the mean implied option expense is 5.51% for firms in the top quartile, versus only 0.93%, 0.35% and 0.12% for firms in the second, third and bottom quartile, respectively. As expected, firms with higher implied option expense have greater use of option-based pay (e.g., in the Pre-period option pay represents 50.7% of executive pay at firms in the top quartile versus 20.1% in the bottom quartile). More importantly for our research design, the use of option pay changes significantly from the Pre to the Post period as a function of the accounting impact. For firms in the top quartile, the percentage of total pay represented by stock options drops by 18.2% (from

50.7% to 32.5%), versus 11.3%, 8.0% and 3.4% for firms in the second, third and bottom quartile, respectively. The decline in option usage across quartiles is accompanied by an increase in restricted stock grants, while the weight of salary and bonus shows little change. It is important to emphasize the substitution of stock options with restricted stocks because restricted stocks typically pay dividends (Zhang 2013), thus providing further reason to expect a dividend increase in response to the FAS123R-induced decline in option pay.

As for the payout variables, Panel D suggests that heavy option users (firms with higher implied option expense) generally use repurchases more than dividends, consistent with the associations documented in earlier studies. In the Pre period repurchases (dividends) represent 3.51% (0.30%) of total assets for the top quartile versus 1.13% (1.12%) for the bottom quartile. Panel D also indicates that the increase in repurchases documented in Panel C takes place across all quartiles. As for the (much smaller) change in dividends, the absolute increase is similar across the four quartiles, though the relative increase (the percentage change) appears to be slightly higher in the top quartile. Overall, these figures do not suggest a shift toward dividends and away from repurchases by firms with a large (accounting-induced) decline in the use of option pay.

Panel E presents the same data as Panel D but based on the definition of 'treatment' and 'control' in Bakke et al. (2015). Under this definition, 997 firms are classified as treatment group and 177 as control (171 firms are classified as 'control' because they did not grant options to the CEO in the Pre period and 6 because they were voluntarily expensing options before 2003). Interestingly, 'treated' firms are equally distributed across the quartiles, suggesting significant variation in the degree of treatment. As discussed earlier, one benefit of our approach is that it exploits cross-sectional variation among treatment firms in terms of the likely impact of FAS123R. Also, 40% of the control firms (71 out of 177) are in the lowest quartile while 35% ((37+25)/177)

are in the top two quartiles, reflecting the different focus of the two approaches in terms of identifying firms less affected by FAS123R.[20]

As for payout and compensation variables, Panel E shows that the treated group experiences a larger increase in repurchases than the control group, while the (less pronounced) increase in dividends is similar across treated and control firms. Treated firms experience a 12.6% decrease in option pay (from 38.7% to 26.1%), with a corresponding 9.4% increase in the weight of restricted stock grants (from 8.0% to 17.4%). The figures are similar at the CEO level (because the classification is based on options grants to the CEO, it is useful to report pay figures at both the top 5 executives and the CEO level). Interestingly, the control group experiences a pronounced *increase* in the weight of option grants in total CEO pay, from 1.0% to 12.7%, as almost half of the control firms issued options to their CEOs in the Post period (for these firms, options as percentage of total CEO pay increased from 0% to 25.4%; untabulated analysis). While this increased use of stock options suggests (ex post) that these firms were not concerned with the impact of FAS123R, another possibility is that the lack of use of option compensation for CEOs in 2003 and 2004 was temporary, rather than the effect of a permanent policy. If these firms were planning to issue options to the CEO in the Post period, the assumption that they were unaffected by FAS123R (and thus their use as control sample) is questionable.[21]

3.2 Differential Impact of FAS123R on the Use of Option-Based Compensation

[20] In untabulated analysis we find that 55% of the 171 firms issuing no options to the CEO in the Pre period granted options to the other top executives (and perhaps to other non-executive employees), thus explaining the relatively high implied option expense for the control group (1.0% versus 1.9% for the treatment group – see Panel E).

[21] Panel E also shows an increase in the use of restricted stock in CEO pay packages (from 12.8% to 18.2%; almost comparable to the increase among treated firms). The concurrent increase in options and restricted stock suggests these firms would have increased option pay significantly more had it not been for FAS123R, indicating further caution in classifying them as control group of unaffected firms.

A key feature of our approach is the presumed differential impact of FAS123R on option-based pay as a function of pre-existing implied option expense.[22] While Table 1, Panel D preliminarily supports the validity of this assumption, we formally examine it by estimating Eq. (1).

As reported in Table 2, Panel A, we find a significant negative association between *Accounting Impact* and the change in option-based pay around the adoption of FAS123R. In column (1), where we use the quartile rank of implied option expense, the -0.044 coefficient implies that around FAS123R, option grants over total pay decline by 4.4% as we move from the highest quartile to the next. Thus, the reduction in top quartile firms is 13.2% larger than in bottom quartile firms. In column 2, we replace the quartile rank with three indicators for the highest quartiles (with the bottom quartile collapsed in the intercept), so as to be able to compare directly top and bottom quartile, without imposing the same effect across quartiles. The negative and significant coefficient on the top quartile indicator, at -0.140, suggests that the reduction in option grants over total pay is 14.0% larger for top quartile firms relative to bottom quartile firms. Finally, in column 3, we use the indicator for treated firms following Bakke et al. (2015). The negative and significant coefficient, at -0.132, suggests that treated firms experience a 13.2% larger drop in the weight of option pay relative to control firms.

In Panel B, we re-estimate Eq. (1) using a panel regression with firm- and year-fixed effects (5,653 firm-year observations) where the dependent variable is the the ratio of option grants to total executive pay. The results are virtually identical in terms of significance and magnitude of the coefficients of interest (the interaction terms between, respectively, *Accounting impact* and *Treated*, and an indicator for the Post period).

[22] This is akin to the *relevance assumption* of instrumental variable if we interpret the implied option expense as an instrument to the change in option-based pay.

Overall, Table 2 suggests that high implied option expense and the introduction of FAS123R together strongly *encouraged* firms to cut back option-based compensation for executives. It also supports the general validity of the approach in Bakke et al. (2015) in identifying firms most affected by FAS123R, notwithstanding the concerns outlined earlier.

Our test of Eq. (1) focuses on executive stock options. For our dividend-protection hypothesis, it is important to establish that the level of implied option expense predicts a change in executive stock options (rather than only a change in the use of non-executive employee stock options) as we want to capture the effect of a change in executives' incentives on payout policy. At the same time, our dilution channel hypothesis assumes a change in the use of all (executive and non-executive employee) stock options. Thus, in untabulated analyses, we repeat the test in Eq. (1) by replacing the dependent variable with the change in the number of options granted to all employees (scaled by the total number of outstanding shares; note: for this analysis we can only use data from 2004 to 2007 because Compustat began reporting data on ESO in 2004). The coefficient on *Accounting Impact* and *Treated* continues to be negative and significant, suggesting that the differential reduction in the use of option compensation as a function of accounting impact occurred for both executive and non-executive employee stock options. Indeed, Brown and Lee (2011) report a similar decrease in executive *and* employee stock options around FAS123R.

3.3 Option-Based Pay and Dividend Payouts

In this section, we examine the effect of an exogenous (accounting-induced) change in option compensation on dividend policy by estimating Equation (2).

Table 3, Panel A, presents the results using the change in dividends from Pre to Post as dependent variable. We find no significant association between *Accounting Impac*t (column 1 and 2) or *Treated* (column 3) and the change in dividends around the adoption of FAS123R. This is in

stark contrast with the negative effect of option-based pay on dividends predicted in the literature and documented in various association studies. Panel B presents similar results using a panel regression (5,653 firm-year observations) with firm- and year-fixed effects and interaction terms between *Accounting Impact* (or *Treated*) and an indicator for the Post period. These tests complement the results in Panel A by offering potentially stronger statistical power.

One concern with this lack of effect is that perhaps our setting is not powerful enough to detect it (though Bakke et al. (2015) do find an effect on risk-taking in the oil & gas industry using the same setting and similar methodology). One way to examine this possibility is the following. The coefficient on *Accounting Impact rank=4* in column 2 of Panel A is 0.001 and the standard error (SE) is 0.0009. Assuming the estimated standard error is correct, then for a two-tailed t test, the coefficient needs to be greater than 0.0015 (=SE*1.645), or 0.15%, to reach the 10% percent significant level. In other words, if moving from the bottom to the top quartile (which predicts a 14% differential decline in option-based pay, as per Table 2, Panel A) resulted in a 0.15% differential increase of the dividend/asset ratio, our test would be able to detect a causal effect of option-based pay on dividends. Since firms in the bottom quartile increased the dividend/assets ratio by 0.12% (from 1.12% to 1.24%; see Table 1 Panel D), a differential increase of 0.15% would require firms in the top quartile to increase their dividend/assets ratio by 0.27%, that is, from 0.30% to 0.57%. This is not implausible, considering that: firms in the top quartile (i) increased the dividend/assets ratio by 0.19% (from 0.30% to 0.49%); and (ii) at the same time, they almost doubled the repurchase/assets ratio (from 3.51% to 6.87%), suggesting that they had enough cash available to increase dividend without significantly altering their repurchase plans. We do a similar analysis for the coefficient on *Treated* in column 3 of Panel A, *Acc. impact rank 4 x Post* in column 2 of Panel B and *Treated x Post* in column 3 of Panel B (panel regression), with

similar insights (the differential increase for the high accounting impact group would need to be even lower, at approximately 0.12%-0.13%).

Because the above analyses are based on the full sample (both dividend-paying and non-dividend-paying firms), they implicity treat as equal two potentially different decisions: to initiate dividends and to increase dividends.[23] Thus, following Chetty and Saez (2005), we separately examine how FAS123R impacts the intensity of dividend changes (for the subset of payers) and the likelihood of dividend initiations (for the subset of non-payers).

3.3.1. Option-Based Pay and Dividend Payouts: Intensity of Dividend Changes

In the first row of Panel C, we repeat the analysis in Panel A for the sub-sample of 572 payers, defined as firms with positive DPS in 2003 and/or 2004. Note that we report only the coefficients of interest for parsimony. In the second row, we replace the dependent variable with the percentage change in DPS. DPS is the choice variable for managers (Brav et al. 2005; Baker and Wurgler 2016) and, thus, our preferred measure to capture changes in dividend policy. To account for skewness in this variable (Khale 2002), in the third, fourth and fifth rows, we use indicator variables to denote whether the change in DPS surpasses a threshold: respectively, 20%, 40% and 60%. The proportion of firms that surpass these thresholds are 59%, 44% and 33%, respectively.[24] Across all of these specifications, we find no association between *Accounting Impact* or *Treated*

[23] For example, an increase in the dividend-to-asset ratio from 0.5% to 0.6% will be treated the same as an increase from 0.0% to 0.1%. However, the latter represents a critical dividend initiation decision given the pre-commitment value that investors attach to this decision and the strong negative market reaction when firms eliminate or reduce dividends (Brav et al. 2005). For executives trying to maximize the value of their own stock options (dividend-protection argument) the ability to influence a dividend initiation decision is likely lower than the ability to influence a dividend increase decision.

[24] Although in the third, fourth and fifth rows our dependent variable is a binary variable, we use a linear model (OLS) instead of a probit or logit nonlinear model. The reason is that estimating fixed effects in binary response models with panel data can cause the incidental parameters problem. The coefficient estimates can be biased when the panel data has a large number of observations but a short time series. Examples of studies making a similar choice are Puri, Rocholl, and Steffen (2011) and Chu and Ma (2015).

and the intensity of dividend changes. Thus, it does not appear that an exogenous reduction in option-based pay caused dividend-paying firms to increase dividends.

In Panel D, we perform four robustness tests. For parsimony, we show only the coefficients of interest and only when the dependent variable is an indicator denoting a greater than 40% increase in DPS. The results are similar for the other four "intensity" dependent variables in Panel C.

In the first row, we exclude firms that began to voluntarily expense options in 2003 or 2004 (84 firms, of which 68 are dividend payers). FASB added accounting for stock-based compensation to its agenda in March 2003 and issued an Exposure Draft proposing the expensing of stock options at fair value in March 2004. In anticipation, a number of firms began to voluntarily expense stock options in 2003 and 2004 (Aboody et al. 2004). If these firms reduced option grants and, *as a result*, increased dividends during the Pre period, then their inclusion may reduce the power of our tests and thus our ability to detect the hypothesized relation.[25]

In a similar spirit, in the second row we exclude firms in the top decile in terms of reduction in the ratio of option grants to total pay for the top 5 executives between 2002 and 2004 (i.e. during the Pre-period). This encompasses 113 firms (55 dividend payers). If these firms reduced option grants in anticipation of FAS123R and, *as a result*, increased dividends during the Pre period, their inclusion may reduce our ability to detect the hypothesized relation.

In the third row, we exclude firms that accelerated the vesting of options before FAS123R became effective in order to avoid recognizing an expense for the unvested portion of these options (Choudhary et al. 2009; Balsam, Reitenga and Yin 2008). This encompasses 196 firms of

[25] This is unlikely to be a serious concern, as previous studies suggest that voluntary expensers made limited use of stock options (Aboody et al. 2004). In fact, in our sample, 79% of these firms are in the bottom two quartiles of implied option expense (untabulated). Nonetheless, we present this test for robustness.

which 54 are dividend payers. If accelerating vesting was used to reduce the impact of FAS123R, the inclusion of these firms may reduce the power of our tests.[26]

In the fourth row, we deal with the concern that if firms target a fixed dividend to net income payout ratio (Lintner, 1956) and do so without adjusting for changes in accounting standards, then by decreasing net income (via option expense) the accounting change would mechanically cause a reduction in dividends. This would potentially offset and confound the increase predicted under the dividend-protection and dilution hypotheses. This seems unlikely for two reasons. First, firms are notoriously reluctant to cut dividends (Brav et al. 2005), especially in response to a decline in net income with no economic or cash flow implications (i.e. re-classification of an expense from the footnotes to the income statement). Second, firms today set dividends in terms of dividend per share, rather than by targeting a net income-based payout ratio (Brav et al. 2005). As noted by Skinner (2008), over the last few decades, as earnings have become more variable, the relation between earnings and dividends has become weaker, with dividend payments increasing smoothly and largely independent of the variation in earnings. Nevertheless, in the fourth row we exclude 66 dividend payers with a similar payout ratio in 2003 and 2004 (a proxy for firms with a target net income-based payout ratio in the Pre period).[27]

As shown in Panel D, even after excluding these subsets of firms, we fail to find an association between *Accounting Impact* or *Treated* and changes in dividends for dividend-paying firms.

---

[26] These firms explicitly stated that they accelerated the vesting of stock options to avoid the accounting charge (Choudhary et al. 2009). In fact, 72% are classified in the top two quartiles of implied option expense (untabulated). Thus, we would have predicted these firms to experience a significant decline in option pay. To the extent that this decline did not materialize because they minimized the impact of FAS123R by accelerating vesting, the ability of *Accounting Impact* to predict the reduction in option pay and thus its usefulness in examining changes in dividends around FAS123R may be reduced. The list of firms accelerating vesting was kindly provided by Jack Ciesielsky of *The Analysts' Accounting Observer*.

[27] We define a "similar" payout to be an absolute change in payout ratio of less than 1% from 2003 to 2004. Among these 66 firms, only 39% continued to have a similar payout ratio in 2005. We also try an alternative definition, where a firm is classified as likely to be using a net income-based target payout ratio if the absolute percentage change in the ratio from 2003 to 2004 is less than 10% (100 firms). Our inferences are unchanged.

Another concern is that, by reducing net income and thus retained earnings, FAS123R may have pushed some firms closer to violating dividend covenants in debt contracts and thus prevented them from increasing dividends. This would bias our study against finding a causal effect of option-based pay on dividends. However, this does not appear to be a significant concern for three reasons. First, the recognition of an option expense does not imply a decline in net income. In fact, more than 70% (75%) of the full sample firms (dividend payers) experienced an increase in net income before extraordinary items from 2004 to 2005 (the percentages are similar across the four quartiles of implied option expense). Second, dividend covenants are less common among dividend-paying firms (Nikolaev, 2010). Third, most loan contracts in recent years give parties the option to "freeze" GAAP when facing a mandatory change in accounting principles (Christensen and Nikolaev 2009). Firms expecting an adverse impact of FAS123R on their dividend covenants would likely exercise this option (particularly given the non-cash nature of option-based expense).

Nonetheless, to alleviate this concern, we perform three additional analyses (untabulated): (i) we repeat the tests in Panel C after excluding firms with a drop in net income between 2004 and 2005 (i.e. firms that *perhaps* faced some covenant-related constrains to their payout policy as a result of FAS123R); (ii) we explicitly control for net income and retained earnings in the regression (as in Skinner 2008); (iii) we repeat the tests in Panel C for dividend payers with dividends/asset ratio above the sample median, since dividend covenants are less frequent among firms paying large dividends (Jung, Lee and Yang 2016). Our inferences are unchanged.

3.3.2. Option-Based Pay and Dividend Payouts: Dividend Initiations

In this section, we examine whether the accounting-driven change in option pay affected *dividend initiations* among non-payers. We cannot employ the difference-in-difference approach

adopted so far, since by definition the same firm cannot initiate dividends in both periods. However, we can provide suggestive evidence as follows. First, we take all firms that did not pay dividends in 2002 and group them into quartiles based on 2002 implied option expense. For each quartile, we find the number of firms initiating dividends in 2003 and 2004 (the Pre period). Next, we take all firms that did not pay dividends in 2004, group them into quartiles based on 2002 implied option expense and, for each quartile, find the number of firms initiating dividends in 2005 and 2006 (the Post period). We do not include 2007 so as to have the same number of years in both periods and make the cumulative initiation rates comparable.

The left portion of Panel E reports the results of this exercise. There is a general decrease in the dividend initiation rate from the Pre to the Post period. This holds both in the full sample of non-payers (from 12.9% to 7.9%) and across all quartiles. This decrease may reflect a temporary spike in 2003 and 2004 in response to the 2003 dividend tax cut documented in Chetty and Saez (2005). It is noteworthy that initiation rates, in both periods, are lower among firms with higher implied option expense. This is consistent with option-heavy firms being mostly high-growth firms less inclined to pay dividends. More relevant to our research question, if the lower use of option compensation increased the propensity to pay dividends, we would expect to see a relative increase (i.e. a lower decrease) in dividend initiations among firms with the highest accounting impact. Instead, the rate does not differ depending on the accounting impact. There is approximatley a 5% decline in initiation rate in each quartile. In the right portion of Panel E, we examine similar data for dividend terminations. While the results should be interpreted with caution due to their rarity, firms with higher accounting impact do not experience a greater decline in termination rates. Our inferences in Panel E are similar when we compare treated and control firms following Bakke et al. (2015) (untabulated).

Even after taking into account the different nature of decisions for dividend paying and non-dividend-paying firms, we fail to find evidence of a causal influence of option-based compensation on dividend policy. This finding is in contrast to the associations documented in earlier studies. It is even more remarkable as most firms replaced stock options with restricted stock, which are generally dividend-protected and should provide further incentive to increase dividends.[28]

3.3.3. Option-Based Pay and Dividend Payouts: Special Dividends

The previous analyses fail to detect a causal effect of option-based pay on *regular* dividends. Perhaps treated firms did not initiate or increase dividends, but resorted to a greater use of *special*, one-time dividends. While special dividends are increasingly rare and represent a fairly small portion of total payouts (DeAngelo, DeAngelo and Skinner 2000; Blouin et al. 2011), one advantage they have over regular dividends is that they do not commit the firm to an ongoing dividend payout level. Along these lines, Chetty and Saez (2005) document a greater frequency of special dividends in 2003, possibly in response to the 2003 dividend tax cut. Hanlon and Hoopes (2014) find a surge of special dividends in the final months of 2010 and 2012, right before an expected increase in individual-level dividend tax rates (the expiration of the 2003 tax cuts). In our setting, it is plausible that executives receiving less (dividend-protected) options and more (dividend-paying) restricted stock, but concerned with mantaining financial flexibility, chose to use special dividends rather than regular dividends.[29]

---

[28] In a sample of S&P 500 firms between 2000 and 2009, Zhang (2013) reports that 80% of the firms granting restricted stocks offer dividend protection. In that sense, the FAS123R-induced shift from stock options to restricted stock may be characterized as a "shock" to dividend protection in unearned equity pay. Accordingly, our results cast doubt on the causal interpretation of the positive association between dividend protection and dividend payouts documented in prior studies (Zhang 2013; Burns et al. 2015; Minnick and Rosenthal 2015).

[29] This argument assumes that ESO are not adjusted to offset the payment of special dividends. Anecdotal evidence suggests this assumption may be incorrect (e.g. see FY 2007 10-K for Cambrex Corp.: https://www.sec.gov/Archives/edgar/data/820081/000095012308002193/y50398e10vk.txt). ESOs are often dividend-protected in case of special dividends. Nonetheless, we present data on trends in special dividends around FAS123R.

Panel F presents data on frequency and magnitude of special dividends in the Pre and Post periods by quartile of implied option expense. While the frequency of firm-year observations with a special dividend increases from 0.7% to 1.1% for firms in the top quartile, the increase is similar for firms in the bottom quartile (from 2.6% to 3.0%). As for the magnitude, within (the few) firms paying special dividends (last column in Panel F), the size of special dividends increases from Pre to Post period across the board, though it is a bit more pronounced in firms in the top two quartiles. When we compare treated and control firms following Bakke et al. (2015) (untabulated), we find a slighty higher increase in frequency of special dividends for control firms (from 1.4% to 1.7%, versus a decrease from 1.3% to 1.1% for treated firms). Also, conditioned on being paid out, special dividends increased in magnitude for control firms more than for treated firms. Overall, it does not appear that the accounting-driven decrease in option-based pay caused a significant increase in the use of special dividends.

3.4 Option-Based Pay and Repurchases

In this section, we examine the effect of an exogenous (accounting-induced) change in option compensation on repurchases by estimating Equation (2).

Table 4, Panel A, presents the results using the change in repurchases (scaled by total assets) from Pre to Post as a dependent variable. If the use of option-based pay had a positive effect on firms' propensity to engage in repurchases, as posited by the dividend-protection and dilution hypotheses, we should find a *negative* association between *Accounting Impac*t or *Treated* and repurchases. Firms with a higher (accounting-driven) reduction in option-based pay should reduce the use of repurchases either because they are shifting toward a greater use of dividends and/or because they have a lower need to offset the dilutive effects of stock options. However, we find no significant association between *Accounting Impact* (columns 1 and 2) and the change in

repurchases around the adoption of FAS123R. When we use the *Treated* indicator (column 3), we find a significant *positive* association rather than a negative one. In Panel B, we repeat the analysis using *net* repurchases (i.e. repurchases after removing the effect of share issuances) following Fama and French (2001) and Skinner (2008). Our inferences are unchanged.[30]

One concern is that we may not detect a decline in repurchases because in the Post period firms need to offset the exercise of ESO that were granted during the Pre period. However, Bens et al. (2003) find that firms use repurchases to manage the effect of unexercised ESO on diluted EPS, rather than to offset the actual exercise of ESO. Nonetheless, to alleviate this concern, in Panel C we use a measure of *adjusted* repurchases, where we subtract our estimate of the maximum amount of repurchases that might have been undertaken to offset the actual exercise of ESO.[31] In doing so, we effectively test whether firms experiencing an accounting-induced decrease in option grants engage in less repurchases (net of exercise-driven repurchases) because their need to manage the effect of unexercised options on diluted EPS is lower.[32] The coefficient on the variables of interest is either insignificant or positive, inconsistent with the hypothesis.

Another concern is that concurrent events affecting payout policies may have differentially affected high accounting impact firms. For example, Blouin and Krull (2009) find that firms that repatriated funds under the American Jobs Creation Act of 2004 increased repurchases by $146 billion more than non-repatriating firms over the 2005-2007 period. Repatriating firms constitute

[30] In the case of repurchases, we do not examine separately "initiations" and "intensity", as we do for dividends, because initiation is not clearly defined for repurchases. Unlike dividends, the occurrence of a repurchase in a given year (or the lack thereof) cannot be used to infer a general policy, due to the more discretionary nature of the timing of repurchases.

[31] As upper bound estimate of exercise-driven repurchases, we multiply the number of ESO exercised during the year times the average stock price during the year. Since the number of ESO exercised is available in Compustat starting in 2004, Panel C is based on observations from 2004 to 2007.

[32] To test whether firms experiencing an accounting-induced decrease in option grants engage in less repurchases because their need to manage the effect of actual exercises of ESO is lower, we would need to extend the Post period beyond 2007 (when options granted during the Post period will be exercised). However, as noted by Floyd et al. (2015), repurchases declined dramatically after 2007 because of the financial crisis, making it difficult to detect the effect of any other factor.

26% of our sample (311 firms). If repatriating firms were mostly concentrated in the top quartile of implied option expense (or the *Treated* group), the inclusion of these firms might prevent us from detecting the predicted decline in repurchases for high accounting impact firms. However, in reported analyses we find that repatriating firms are approximately equally distributed across the four quartiles of implied option expense, as well as between treated and control firms. Also, the results in Table 4 are similar when we include an indicator to control for repatriating firms.[33]

Along the same lines, Brav, Jiang, Partnoy and Thomas (2008) find that firms targeted by hedge fund activists experience an increase in overall payout (dividends plus repurchases) in the year of the activist's intervention and the subsequent year. If this increase was mostly driven by repurchases and positively correlated with implied option expense (or with *Treated*), it may prevent us from detecting the predicted decline in repurchases for high accounting impact firms. However, targeted firms are approximately equally distributed across the four quartiles of implied option expense, as well as between treated and control firms. In untabulated tests we include an indicator for firms targeted by hedge fund activists in the Pre period (41 firms) and in the Post period (162 firms). Our inferences are unaffected.

A final caveat is that most firms replaced stock options with restricted stock (Table 2, Panel C). Unvested restricted stock is excluded from the denominator in the computation of basic EPS (once vested, it is included). However, it is included in the denominator of diluted EPS using the treasury stock method, similar to stock options. Given the complexity of this method, without access to all details of restricted stock grants and option grants, it is difficult to determine whether the switch from stock options to restricted stock decreased the need for dilution-driven repurchases as our test implicitly assumes. Hence, we may not find a decrease in repurchases because a reduced need to

[33] While Blouin and Krull (2009) do not find that repatriating firms altered their dividend policy, for completeness we also re-run our earlier dividend regressions adding an indicator for repatriating firms. Our inferences are unchanged.

offset the dilutive effect of stock options was neutralized by an increased need to offset the dilutive effect of restricted stock. We note, however, that the increase in the weight of restricted stock from the Pre to the Post period is similar across the four quartiles of implied options (Table 1 Panel D).

Overall, the evidence in Table 4 is not consistent with a positive causal relation between the use of option-based pay and repurchases. More precisely, it suggests that option-driven dilution concerns do not have a first-order effect on the *magnitude* of repurchases even though they may play a role in the repurchase decision. Hence, our result is not necessarily in conflict with the evidence in Brav et al. (2005) where two-thirds of the surveyed financial executives state that "offsetting the dilutionary effect of stock option plans or other stock programs" is an important factor to their repurchase decisions.

3.5 Option-Based Pay and Payout Composition

As an alternative to examining dividends and repurchases separately, we also look at the payout composition: the change in the *Dividends/Total Payout* ratio from the Pre to the Post period where *Total Payout* is the sum of dividends and repurchases. As noted earlier, we focus on the subsample of firms with both dividends and repurchases in both Pre and Post periods so as to capture the change in payout mix by firms routinely using both vehicles to return cash to shareholders. If the use of option-based pay causes firms to substitute repurchases for dividends, we should find a positive association between *Accounting Impac*t or *Treated* and the payout mix, defined as *Dividends/Total Payout* ratio. Firms with a higher accounting-driven reduction in option-based pay should have an increase (decrease) in the relative weight of dividends (repurchases) to the total payout. However, as reported in Table 5, Panel A, we fail to find a significant association between *Accounting Impac*t (column 1 and 2), or *Treated* (column 3) and

the change in payout mix. The results are similar when we use *Net Payout* (dividends plus *net* repurchases) instead of *Total Payout* in computing the payout mix (Panel B).

Overall, the combined evidence in Tables 3-5 does not support the hypothesis that option-based compensation has negative (positive) effect on dividends (repurchases) or that it causes firms to replace dividends with repurchases.[34]

4 Additional Analyses: the role of monitoring

Our analyses fail to detect an *average* effect of option-based pay on payout policy. However, such effect may take place in sub-samples where the *incentive* or *opportunity* to change payout policy is stronger. We conjecture that the incentives to modify payout policy, and especially increase dividends, will be higher when the CEO's shareholdings (past option holdings) are higher (lower), since these CEOs will benefit more from a dividend increase; when outside directors' shareholding are higher, since these directors will also benefit from increased dividends; when there is a higher percentage of individual shareholders (based on the evidence that individual investors prefer dividends; Jain 2007). We also conjecture that the opportunity to modify payout policy in response to the FAS123R shock to option-based pay will be higher in firms with greater CEO influence over the board and lower external monitoring. We use CEO-duality and the percentage of coopted directors (Coles, Daniel and Naveen 2014) as proxies for the former, and lower institutional ownership, lower block ownership and lack of blockowners as proxies for the latter. To examine these conjectures, we either repeat our tests for sub-samples based on above-below median value of the above variables, or interact such variables with our proxies for

[34] An important caveat, comon to similar studies, is that the effect we identify is a "local" average treatment effect for the compliers to the instrument (i.e. those who receive the treatment due to the encouragement of the instrument; in our setting, the firms that reduce ESO in response to large implied option expense and the implementation of FAS 123R). Therefore, this effect may not be representative of the "population" average treatment effect if compliers are not representative of the population.

accounting impact. With few exceptions,[35] we find little evidence supporting a causal influence of option-based pay on payout policy even in these subsamples (unabulated analyses).

One explanation for the lack of effect is that perhaps the level of monitoring and scrutiny for S&P 1,500 firms is sufficiently high to prevent compensation-driven incentives from influencing payout policy. To examine this possibility, in our last analysis, we expand the sample to all Compustat firms. We are able to use non-S&P 1500 firms because our payout policy regression (Equation 2) does not directly involve executive compensation data. However, since we do not have access to executive options' data for non-S&P 1500 firms, this analysis assumes that our proxies for accounting impact predict a decline in option-based pay around FAS123 for Compustat firms as well (i.e., it assumes that the results in Table 2 hold for the broader Compustat sample). We believe this assumption can be maintained as non-S&P 1500 firms should have similar incentive to avoid accounting charge for option-based pay. Also, we only examine implied option expense since we cannot classify Compustat firms as treated and control following Bakke et al. (2015) without access to executives' option data.

After applying the same criteria as in Section 3.1, we end up with 2,961firms (versus 1,176 S&P 1500 firms), with average total assets of $4.4 billion (versus $11.1 billion for our sample of S&P 1500 firms). Thus, we effectively add a significant number of smaller firms. Table 6 reports the results. Panel A examines the change in the dividends/assets ratio (Model 1 and 2) and the change in the repurchases/assets ratio (Model 3 and 4) around FAS123R for the full sample (payers and non-papers), essentially replicating Table 3, Panel A, and Table 4, Panel A. We find no significant association between *Accounting Impac*t and the change in dividends, and a significantly *positive* association with repurchases (contrary to what the dilution channel predicts).

[35] We find a positive and significant association between some of our proxies for accounting impact and changes in dividends for firms without a block institutional owner.

In Panel B we repeat the analysis for dividend payers (essentially replicating a subset of Table 3 Panel C). Interestingly, we find a positive and significant association between implied option expense and both the change in dividend/assets ratio and the probability of a greater than 40% increase in DPS (results are similar using a 20% or 60% threshold), in line with the prediction of the dividend-protection channel.

To sum up, we find no evidence of causal influence of option-based pay on payout policy among the S&P 1,500 firms (the same sample examined in earlier association studies), consistent with the notion that boards understand compensation-driven incentives related to payout policy (Tapestry Networks, 2016) and perhaps adjust executive pay packages accordingly. Our findings are also consistent with boards and investors closely monitoring payout choices and preventing those incentives from affecting payout policy. In support for this explanation, we find some evidence of a causal effect on dividend payouts in dividend-paying smaller firms outside the S&P 1500, i.e. in firms presumably subject to a lower degree of internal and external monitoring.

5 Conclusion

Prior literature has documented robust associations between option-based compensation and payout policy, yet it remains unclear whether this association reflcts a causal influence. We investigate the causal effect of option-based compensation on payout policy using the adoption of FAS123R, which resulted in a significant decrease in option grants, making it a plausible exogenous shock to the use of option-based pay. To overcome the challenge of no control group, we exploit the fact that firms responded differently depending on the firm-specific accounting impact as measured by the implied option expense disclosed in the footnotes prior to FAS123R. Firms with larger expected accounting impact curtailed option-based compensation much more aggressively than firms with lower impact. Using a sample of S&P 1500 firms, we find no

evidence that firms with a larger accounting-induced decline in option compensation increased dividends, reduced repurchases or changed their payout composition, contrary to the widely held belief that option-based pay has a first-order causal influence on payout policy. We do find evidence of an effect on dividend payouts among a sample of (non-S&P 1,500) small firms, consistent with the notion that managers' compensation incentives may affect payout decisions in firms subject to a lower level of monitoring and scrutiny.

Appendix 1: Variable Definitions and Data Sources

Accounting Impact Variables

Accounting impact: Implied option expense (Compustat: XINTOPT) divided by total assets (Compustat: AT). This variable is measured at fiscal year 2002 and is time-invariant.

Accounting impact rank: The quartile rank of Accounting impact (1=lowest, 4=highest).

Payout Variables

Regular dividend/Total assets: The numerator is total regular cash dividends in the fiscal year from the distribution dataset in CRSP. Regular cash dividends are identified by the distribution codes 1212, 1222, 1232, or 1242. The denominator is one-year lagged total assets (Compustat: AT).

Special dividend/Total assets: The numerator is total special cash dividends in the fiscal year from the distribution dataset in CRSP. Special dividends are identified by the distribution codes 1262 or 1272. The denominator is one-year lagged total assets (Compustat: AT).

DPS: Dividend per share (Compustat: DVPSX_F).

DPS pct. change: The percentage change of average DPS from Pre- to Post-FAS123R period.

DPS pct. change >X% indicator: A dummy variable that equals one if DPC pct. change is greater than X%, and zero otherwise.

Repurchase/Total assets: The numerator is the repurchase of common equity which equals total repurchase (Compustat: PRSTK) minus the change in preferred stock redemption value (Compustat: PSTKRV). The denominator is one-year lagged total assets (Compustat: AT).

Net repurchase/Total assets: Net repurchase removes the effect of share issuances. The construction follows Fama and French (2001) and Skinner (2008): if the firm uses the treasury stock method for repurchase, net repurchase equals the increase in common treasury stock (Compustat: TSTKC); if the firm uses the retirement method (when common treasury stock is zero in the current and prior year), net repurchase equals total repurchases minus stock issuance (Compustat: SSTK) and change in preferred stock redemption value (Compustat: PSTKRV). When the above measures give negative value, net repurchase is set to zero.

Adjusted repurchase/Total assets: Adjusted repurchase removes from total repurchase the potential effect of repurchasing exercised options. This variable equals total repurchase of common stocks minus the product between the number of options exercised (Compustat: OPTEXD) and average stock price during the year. When the above measure gives negative value, adjusted repurchase is

set to zero. Since the number of options exercised is not available before 2003, we drop observations in 2003 when we use this variable.

Total payout/Total assets: the sum of Regular dividend/Total assets and Repurchase/Total assets.

*Regular dividend/Total payout***:** regular cash dividend divided by the sum of regular cash dividend and repurchase from common equity. In the two-period analysis (Pre- vs. Post-FAS123R), we separately sum the numerator and the denominator for each firm-period and then calculate the ratio.

Regular dividend/Net payout: regular dividend divided by the sum of regular dividend and net repurchase. In the two-period analysis (Pre- vs. Post-FAS123R), we separately sum the numerator and the denominator for each firm-period and then calculate the ratio.

Compensation Variables

Salary_$: Average salary (Execucomp: SALARY) of the top 5 highest paid executives in $1000. *Salary_%*: Average salary (Execucomp: SALARY) divided by average total pay (Execucomp: TDC1) of the top 5 highest paid executives.

Bonus_$: Average bonus (Execucomp: BONUS) of the top 5 highest paid executives in $1000. Due to a data format change, we add non-equity plan-based awards (Execucomp: NON_EQ_TARG) to this measure after 2006.

Bonus_%: *Bonus_$* divided by average total pay (Execucomp: TDC1) of the top 5 highest paid executives.

Option grant_$: Average option grant value (Execucomp: OPTION_AWARDS_BLK_VALUE or OPTION_AWARDS_FV) of the top 5 highest paid executives in $1000. Due to a data format change, we use OPTION_AWARDS_FV for this measure after 2006.

Option grant_%: *Option grant_$* divided by average total pay (Execucomp: TDC1) of the top 5 highest paid executives.

Restricted stock grant_$: Average restricted stock grant value (Execucomp: RSTKGRNT or STOCK_AWARDS_FV) of the top 5 highest paid executives in $1000. Due to data format change, we use STOCK_AWARDS_FV for this measure after 2006.

Restricted stock grant_%: Restricted stock grant_$ divided by average total pay (Execucomp: TDC1) of the top 5 highest paid executives.

Total pay_$: Average total pay (Execucomp: TDC1) of the top 5 highest paid executives in $1000.

Other Characteristics

Size: The book value of total assets (Compustat: AT) minus the book value of common equity (Compustat: CEQ) plus the market value of common equity from CRSP in $M.

Log(size): the natural logarithm of size.

Book-to-market: the book value of common equity dividend by its market value.

Leverage: long term debt plus short term debt (Compustat: DLTT + DLC) divided by total assets.

Asset tangibility: tangible assets (Compustat: AT - INTAN) divided by total assets.

Free cash flow: Operating cash flow (Compustat: OANCF) minus capital expenditure (Compustat: CAPX) then divided by lagged total assets.

Cash holding: Cash and short-term investments (Compustat: CHE) divided by total assets.

ROA volatility: the standard deviation of returns on assets (operating income before tax divided by lagged total assets) in the past five fiscal years.

Institutional ownership: the number of shares held by institutional investors divided by the number of shares outstanding from Thomson Reuters Institutional Holdings database.

Sales growth: the growth rate of sales (Compustat: SALE) from the prior year.

Past stock return: annual stock return from the prior year.

Loss indicator: a dummy variable taking value of one if the current net income is negative, and zero otherwise.

Appendix 2: Pre-trend Regressions

This table investigates whether Accounting Impact rank or Treated indicator is associated with the trend of dividend or repurchase from 2003 to 2004. The sample contains firm-year observations in 2003 and 2004. Panel A reports the results of estimating three fixed effects models. In all three specifications, the dependent variable is Regular dividend/Total assets. The main independent variables are (1) the interactions of fiscal year 2004 indicator with Accounting Impact rank, (2) Accounting impact rank indicators and (3) Treated, respectively. In Model (1), Accounting impact rank is the quartile rank of implied option expense/total assets measured in 2002 (1=lowest, 4=highest). In Model (2), Accounting impact rank=2 (3, 4) is an indicator that takes value of one if the firm's Accounting impact rank equals 2 (3, 4). In Model (3) we use an alternative treatment construction following Bakke et al. (2015), where Treated is an indicator that takes value of zero if the firm granted zero option to its CEO in both 2003 and 2004, or the firm began to voluntarily expense option-based pay at fair value in 2002 or earlier, and one otherwise. In Panel B we estimate the same three models using Total repurchase/Total assets as the dependent variable. All models in Penal A and B have firm fixed effects and year fixed effects, and the standard errors are clustered at firm level. All variables are defined in Appendix. The notation *, ** and *** indicates statistically significant at 10%, 5% and 1% level, respectively.

Panel A: Dividend pre-trends

Dependent variable: Regular dividend/Total assets

	Model (1)		Model (2)		Model (3)	
	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Acc. impact rank x FY04	0.000	0.06				
Acc. impact rank 2 x FY04			0.000	-0.01		
Acc. impact rank 3 x FY04			0.000	0.20		
Acc. impact rank 4 x FY04			0.000	-0.03		
Treated x FY04					0.000	-0.64
Control variables	Yes		Yes		Yes	
Fixed effects	Firm, Year		Firm, Year		Firm, Year	
Standard error	Clustered by firm		Clustered by firm		Clustered by firm	
Adj. R-squared	0.931		0.931		0.931	
Observations	2344		2344		2344	

Panel B: Repurchase pre-trends

Dependent variable: Total repurchase/Total assets

	Model (1)		Model (2)		Model (3)	
	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Acc. impact rank x FY04	0.002	0.89				
Acc. impact rank 2 x FY04			-0.001	-0.22		
Acc. impact rank 3 x FY04			0.002	0.42		
Acc. impact rank 4 x FY04			0.005	0.78		
Treated x FY04					0.006	1.22
Control variables	Yes		Yes		Yes	
Fixed effects	Firm, Year		Firm, Year		Firm, Year	
Standard error	Clustered by firm		Clustered by firm		Clustered by firm	
Adj. R-squared	0.537		0.536		0.537	
Observations	2344		2344		2344	

References

Aboody, David, 1996. Market Valuation of Employee Stock Options, *Journal of Accounting and Economics* 22, 357-391.

Aboody, David, Barth Mary E, and Ron Kasznik, 2004. Firms' Voluntary Recognition of Stock-Based Compensation Expense. *Journal of Accounting Research* 42, 123–150.

Aboody, David, and Ron Kasznik, 2008. Executive stock-based compensation and firms' cash payout: the role of shareholders' tax-related payout preferences. *Review of Accounting Studies* 13, 216–251.

Anantharaman, Divya, and Yong Gyu Lee, 2014. Managerial risk taking incentives and corporate pension policy. *Journal of Financial Economics* 111, 328–351.

Baker, Malcolm and Jeffrey Wurgler, 2004. A catering theory of dividends. *Journal of Finance* 59, 1125-1165.

Baker, Malcolm and Jeffrey Wurgler, 2016. Dividends as reference points: a behavioral signaling approach. *Review of Financial Studies* 29, 697-738.

Bakke, Tor-Erik, Hamed Mahmudi, Chitru Fernando and Jesus Salas, 2015. The causal effect of option pay on corporate risk management. *Journal of Financial Economics*, forthcoming.

Balsam Steven, Austin Reitenga and Jennifer Yin, 2008. Option acceleration in response to SFAS No. 123 (R). *Accounting Horizons* 22, 23-45.

Bens, Daniel and Steven Monahan, 2008. Altering Investment Decisions to Manage Financial Reporting Outcomes: Asset-Backed Commercial Paper Conduits and FIN 46. *Journal of Accounting Research* 46, 1017-1055.

Bens, Daniel A, Venky Nagar, Douglas J Skinner, and MH Franco Wong, 2003. Employee stock options, eps dilution, and stock repurchases. *Journal of Accounting and Economics* 36, 51–90.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan, 2004. How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics* 119, 249–275.

Blouin, Jennifer and L. Krull, 2009. Bringing It Home: A Study of the Incentives Surrounding the Repatriation of Foreign Earnings Under the American Jobs Creation Act of 2004. *Journal of Accounting Research* 47, 1027-1059.

Blouin, Jennifer L, Jana S Raedy and Douglas A Shackelford, 2011. Dividends, Share Repurchases, and Tax Clienteles: Evidence from the 2003 Reductions in Shareholder Taxes. *The Accounting Review* 86, 887-914.

Brav, Alon, John R Graham, Campbell R Harvey, and Roni Michaely, 2005. Payout policy in the 21st century. *Journal of Financial Economics* 77, 483–527.

Brav, Alon, Wei Jiang, Frank Partnoy, and Randall Thomas, 2008. Hedge Fund Activism, Corporate Governance and Firm Performance. *Journal of Finance* 63, 1729-1775.

Brown, Lawrence D. and Yen-Jung Lee, 2011. Changes in Option-Based Compensation Around the Issuance of SFAS123R. *Journal of Business Finance & Accounting* 38, 1053–109.

Brown, Jeffrey R, Nellie Liang, and Scott Weisbenner, 2007. Executive financial incentives and payout policy: Firm responses to the 2003 dividend tax cut. *Journal of Finance* 62, 1935–1965.

Burns, Natasha, Brian McTier and Kristina Minnick, 2015. Equity-incentive compensation and payout policy in Europe. *Journal of Corporate Finance* 30, 85-97.

Carter, Mary Ellen and Luann J Lynch, 2003. The Consequences of the FASB's 1998 Proposal on Accounting for Stock Option Repricing. *Journal of Accounting and Economics* 35, 51-72.

Carter, Mary Ellen, Luann J Lynch, and Irem Tuna, 2007. The role of accounting in the design of CEO equity compensation. *The Accounting Review* 82, 327–357.

Chava, Sudheer, and Amiyatosh Purnanandam, 2010. CEOs versus CFOs: Incentives and corporate policies. *Journal of Financial Economics* 97, 263–278.

Chetty, Raj, and Emmanuel Saez, 2005. Dividend taxes and corporate behavior: Evidence from the 2003 dividend tax cut. *The Quarterly Journal of Economics* 120, 791–833.

Choudhary, Preeti, Shivaram Rajgopal, and Mohan Venkatachalam, 2009. Accelerated vesting of employee stock options in anticipation of FAS123R. *Journal of Accounting Research* 47, 105–146.

Christensen, Hans B. and Valeri Nikolaev, 2009. Contracting on Mandatory Changes to GAAP: New Practice and Its Determinants. Working Paper, University of Chicago.

Chu, Yongqiang, and Tao Ma, 2015. How does management compensation affect bank risk taking: evidence from FAS 123R. University of South Carolina, Working Paper.

Coles, Jeffrey, Naveen Daniel and Lalitha Naveen, 2014. Co-opted Boards. *Review of Financial Studies* 27, 1751-1796.

Cuny, Charles J, Gerald S Martin, and John J Puthenpurackal, 2009. Stock options and total payout. *Journal of Financial and Quantitative Analysis* 44, 391–410.

DeAngelo Harry, Linda DeAngelo and Douglas J Skinner, 2000. Special Dividends and the Evolution of Dividend Signaling. *Journal of Financial Economics* 57, 309-354.

Dechow, Patricia M, Amy P Hutton, and Richard G. Sloan. 1996. Economic consequences of accounting for stock-based compensation. *Journal of Accounting Research* 34, 1–20.

Edgerton, J. 2013. Four facts about dividend payouts and the 2003 tax cut. *International Tax and Public Finance* 20, 769-784.

Edmans, Alex, and Xavier Gabaix, 2015. Executive compensation: a modern primer. *Journal of Economic Literature*, forthcoming.

Fama, Eugene F, and Kenneth R French, 2001. Disappearing dividends: changing firm characteristics or lower propensity to pay? *Journal of Financial Economics* 60, 3–43.

Farre-Mensa, Joan, Roni Michaely, and Martin C Schmalz, 2014. Payout policy. *Annual Review of Financial Economics* 6, 75–134.

Fenn, George W, and Nellie Liang, 2001. Corporate payout policy and managerial stock incentives. *Journal of Financial Economics* 60, 45–72.

Ferri, Fabrizio, and Tatiana Sandino, 2009. The impact of shareholder activism on financial reporting and compensation: the case of employee stock options expensing. *The Accounting Review* 84, 433–466.

Floyd Eric, Nan Li and Douglas J Skinner, 2015. Payout policy through the financial crisis: the growth of repurchases and the resilience of dividends. *Journal of Financial Economics* 118, 299-316.

Frydman, Carola and Dirk Jenter, 2010. CEO compensation. *Annual Review of Financial Economics* 2, 75-102.

Gipper, Brandon, 2015. Assessing the Effects of Disclosing Management Compensation. Working Paper, University of Chicago.

Gormley, Todd, David Matsa, and Todd Milbourn, 2013. CEO compensation and corporate risk: evidence from a natural experiment. *Journal of Accounting and Economics* 56, 79-101.

Graham, John, Michelle Hanlon and Terry Shevlin, 2011. Real effects of accounting rules: evidence from multinational firms' investment locations and profit repatriation decisions. *Journal of Accounting Research* 49, 137-185.

Graham, John, Campbell Harvey and Shiva Rajgopal, 2005. The Economic implications of corporate financial reporting. *Journal of Accounting and Economics* 40, 3-73.

Greenspan, Alan, 2002. Testimony of Alan Greenspan before the Committee on Banking, Housing, and Urban Affairs, U.S. Senate, July 16. Washington, D.C.: Government Printing Office.

Grullon Gustavo, and Roni Michaely, 2002. Dividends, Share Repurchases, and the Substitution Hypothesis. *Journal of Finance*, 57, 1649-1684.

Hail, Luzi, Ahmed Tahoun and Clare Wang, 2014. Dividend Payouts and Information Shocks. *Journal of Accounting Research* 52, 403-456.

Hall Brian J, and Kevin J Murphy, 2002. Stock options for undiversified executives. *Journal of Accounting & Economics* 33, 3-42.

Hall Brian J, and Kevin J Murphy, 2003. The trouble with stock options. *Journal of Economic Perspectives* 17, 49–70.

Hanlon, Michelle, and Jeffrey L Hoopes, 2014. What do firms do when dividend tax rates change? An examination of alternative payout responses. *Journal of Financial Economics* 114, 105-124.

Hayes, Rachel M, Michael Lemmon, and Mingming Qiu, 2012. Stock options and managerial incentives for risk taking: Evidence from FAS123R. *Journal of Financial Economics* 105, 174–190.

Heron Randall A, Erik Lie, 2007. Does backdating explain the stock price pattern around executive stock option grants? *Journal of Financial Economics* 83, 271-295.

Hribar, Paul, Nicole Thorne Jenkins, and W Bruce Johnson, 2006. Stock repurchases as an earnings management device. *Journal of Accounting and Economics* 41, 3–27.

Jain Ravi, 2007. Institutional and individual investor preferences for dividends and share repurchases. *Journal of Economics and Business* 59, 406-429.

Jensen, Michael C, 1986. Agency cost of free cash flow, corporate finance, and takeovers. *American Economic Review* 76, 323-329.

Jolls, Christine, 1998. Stock repurchases and incentive compensation. NBER Working Paper No. w6467.

Jung, Boochun, Woo-Jong Lee and Yanhua Sunny Yang, 2016. The Impact of Dividend Covenants on Investment and Operating Performance. *Journal of Business Finance & Accounting*, 43, 414–447.

Kahle, Kathleen M, 2002. When a buyback isn't a buyback: open market repurchases and employee options. *Journal of Financial Economics* 63, 235–261.

Lambert, Richard A, William N Lanen, and David F Larcker, 1989. Executive stock option plans and corporate dividend policy. *Journal of Financial and Quantitative Analysis* 24, 409–425.

Lintner, John, 1956. Distribution of incomes of corporations among dividends, retained earnings, and taxes. *The American Economic Review* 46, 97-113.

Low, Angie, 2009. Managerial risk-taking behavior and equity-based compensation. *Journal of Financial Economics* 92, 470-490.

Miller, Merton H, and Franco Modigliani, 1961. Dividend policy, growth, and the valuation of shares. *The Journal of Business* 34, 411–433.

Minnick, Kristina, and Leonard Rosenthal, 2015. Stealth compensation: Do CEOs increase their pay by influencing dividend policy? *Journal of Corporate Finance* 25, 435-454.

Murphy, Kevin J, 1999. Executive compensation. In O. Ashenfelter and D. Card (eds.), North Holland Handbook of Labor Economics. Amsterdam: Elsevier Science BV.

Murphy, Kevin J, 2013, Executive compensation: Where we are, and how we got there. Handbook of the Economics of Finance. Elsevier Science North Holland, 211-356.

Nikolaev, Valeri, 2010. Debt Covenants and Accounting Conservatism. *Journal of Accounting Research* 48, 143-176.

Puri, Manju, Jörg Rocholl, and Sascha Steffen, 2011. Global retail lending in the aftermath of the US financial crisis: Distinguishing between supply and demand effects. *Journal of Financial Economics* 100, 556-578.

Shue, Kelly, and Richard R Townsend, 2016. How do quasi-random option grants affect CEO risk-taking? *Journal of Finance*, forthcoming.

Skinner, Douglas J, 2008. The evolving relation between earnings, dividends, and stock repurchases. *Journal of Financial Economics* 87, 582-609.

Tapestry Networks, 2016. Buybacks and the Board, at: http://irrcinstitute.org/wp-content/uploads/2016/08/FINAL-Buybacks-Report-Aug-22-2016.pdf

Weisbenner, Scott J, 2000. Corporate share repurchases in the 1990s: What role do stock options play? FEDS Working Paper No. 2000-29.

Zhang, Dan, 2013. CEO dividend protection. Working Paper. BI Norwegian Business School.

Figure 1. Trends of option-based compensation from 2001 to 2007. This figure plots the average option grant (fair value) divided by total pay of the top 5 executives of firms in each quartile of Accounting Impact measured in 2002.

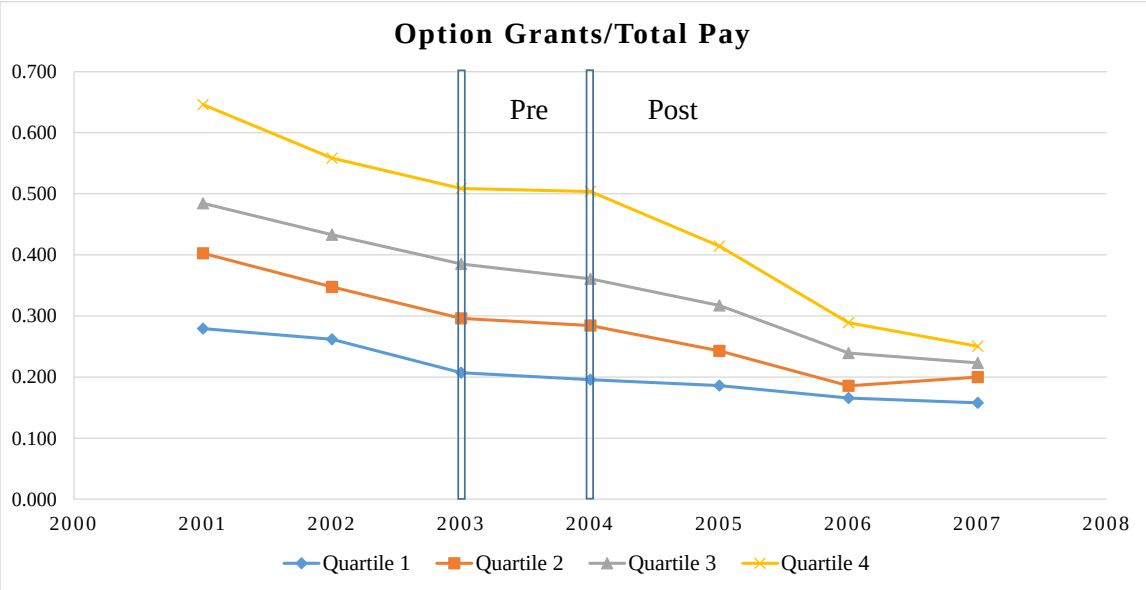
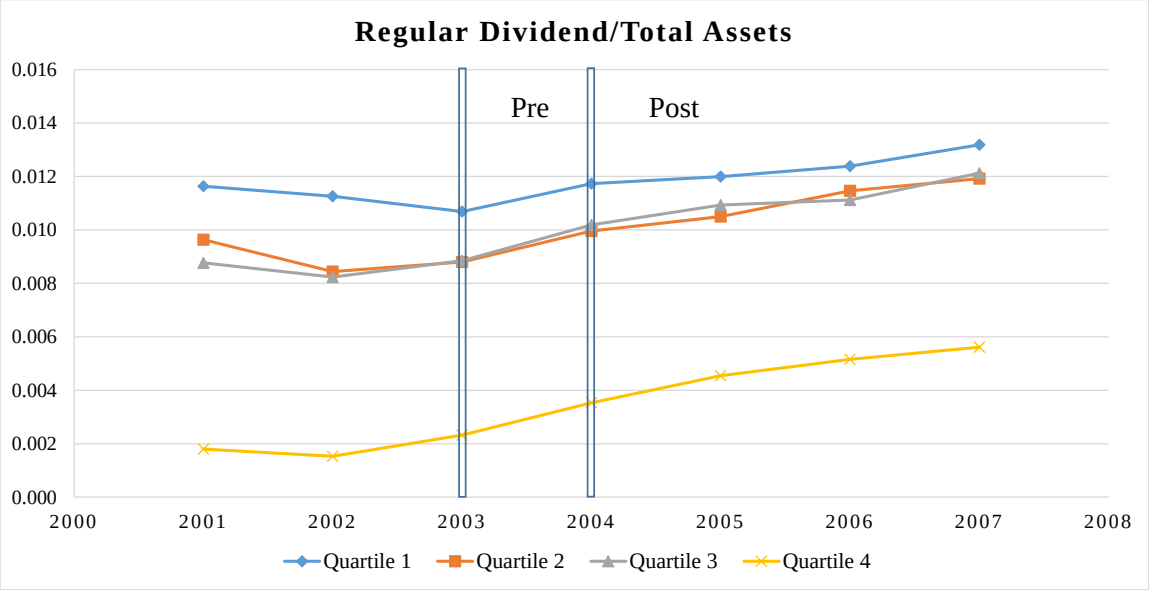
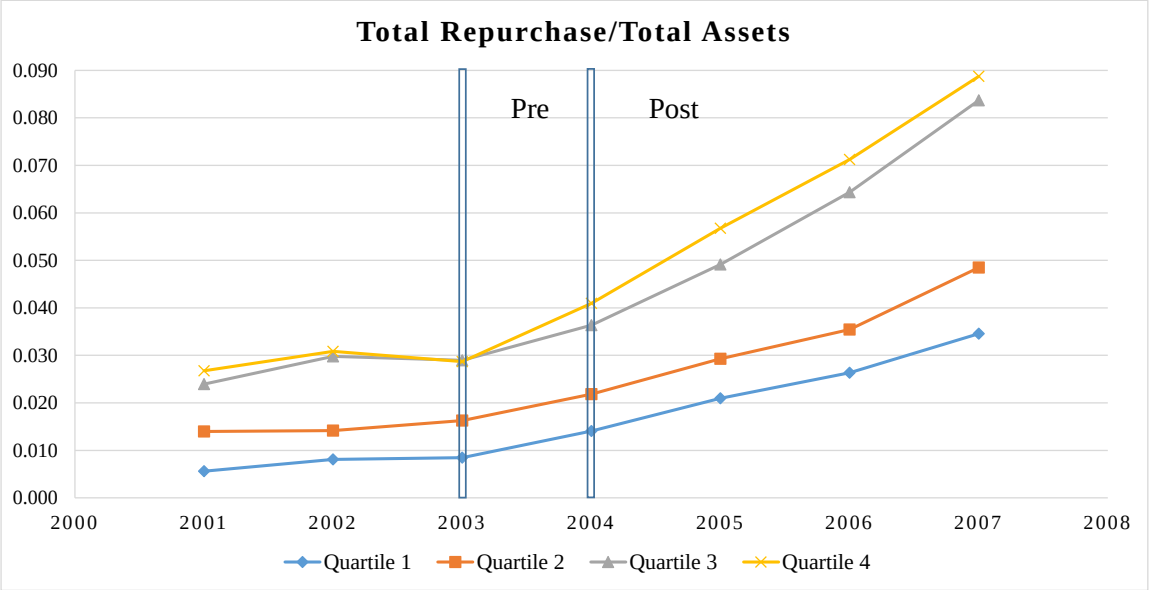


Figure 2. Trends of dividends and repurchases from 2001 to 2007. Panel A plots the average Regular Dividend/Total Assets of firms in each quartile of Accounting Impact measured in 2002. Panel B plots the average Total Repurchase/Total Assets of firms in each quartile of Accounting Impact measured in 2002. Panel C shows the average Regular Dividend/Total Assets of firms in treated group and control group as defined in Bakke et al. 2015. Panel D shows the average Total Repurchase/Total Assets of firms in treated group and control group as defined in Bakke et al. 2015.

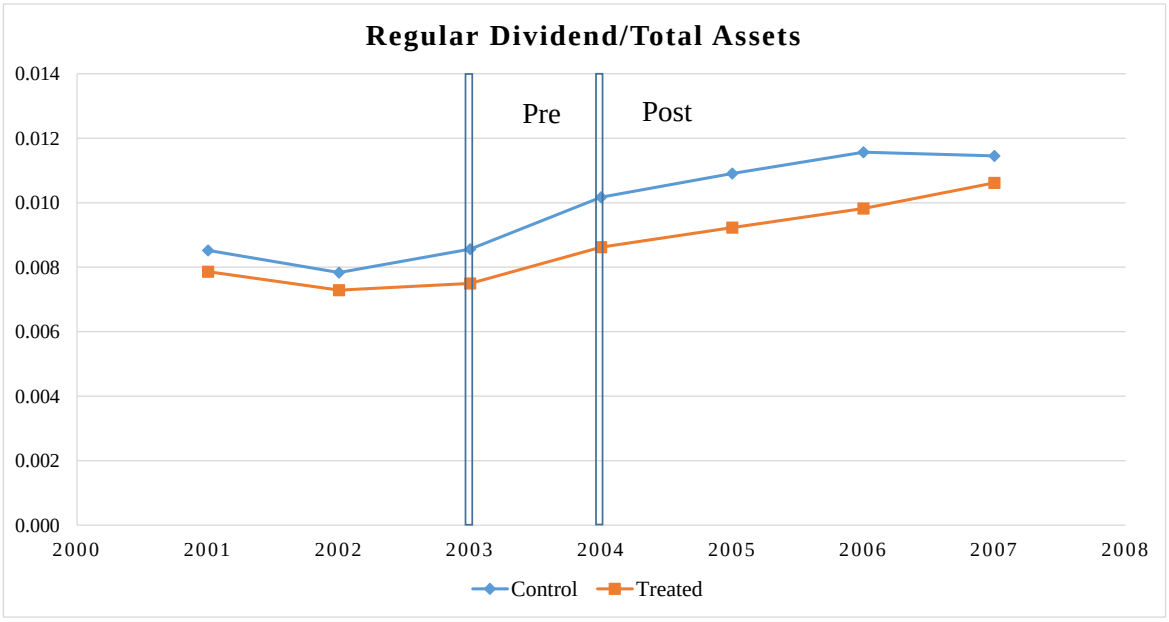
Panel A: Trends of regular dividends by Accounting Impact



Panel B: Trends of total repurchases by Accounting Impact



Panel C: Trends of regular dividends by treated and control



Panel D: Trends of total repurchases by treated and control

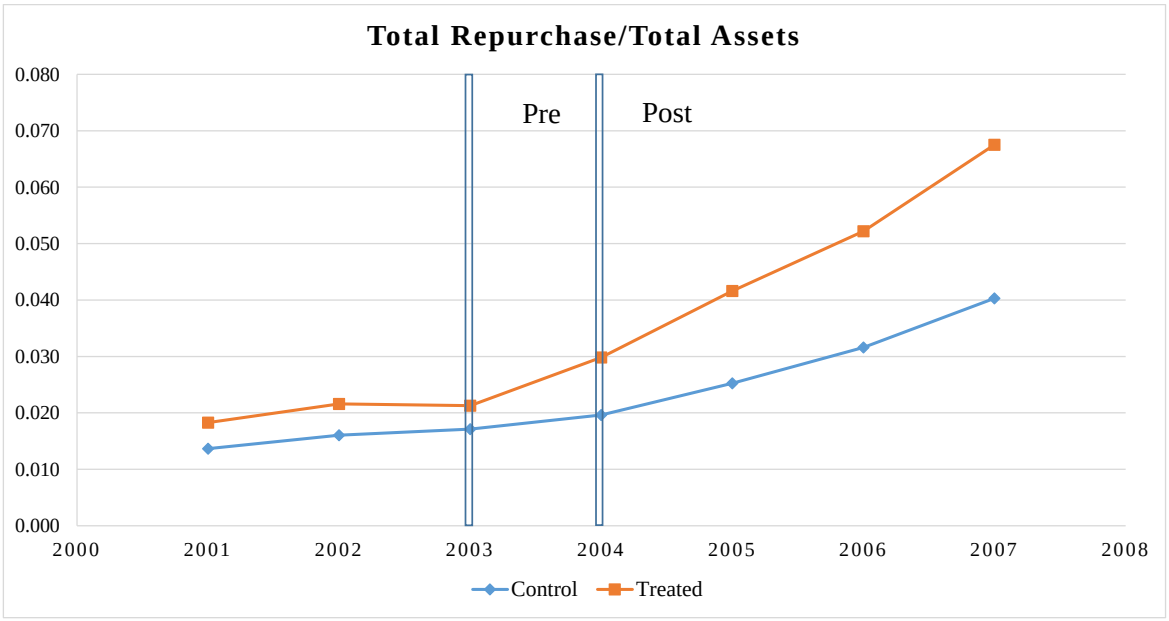


Table 1

Summary Statistics

This table presents summary statistics. Panel A shows the descriptive statistics of payout variables, compensation variables and other firm characteristics for the full sample. Accounting impact is the implied option expense measured at fiscal year 2002. All other variables are measured at firm-year level from fiscal year 2003 to 2007, resulting in 5,653 firm-year observations (except for Regular dividend/Total Payout, which can be measured only for the 4,901 firm-year observations with Total payout greater than zero). Panel B shows the pairwise correlations between main variables at firm-year level. Panel C compares payout variables, compensation variables and other firm characteristics before and after FAS123R. The Pre-FAS123R period consists of fiscal year 2003 and 2004; the Post-FAS123R period covers fiscal year 2005-2007. Panel D compares the trends of payout and compensation variables around FAS123R across four quartile groups ranked by 2002 accounting impact. Panel E compares the trends of payout and compensation variables around FAS123R between the treated and control group under the alternative treatment definition in Bakke et al. (2015) (see Section 2.2). All variables are defined in Appendix. The notation *, ** and *** in Panel C indicates statistically significant at 10%, 5% and 1% level in T-tests for equal means, respectively.

Panel A: Firm characteristics - full sample

	N	Mean	Std. dev.	25 pctl.	Median	75 pctl.
Accounting impact (2002)	1176	1.73%	3.66%	0.22%	0.52%	1.63%
Payout variables						
Regular dividend/Total assets	5653	0.93%	1.48%	0.00%	0.00%	1.42%
Repurchase/Total assets	5653	3.94%	7.10%	0.00%	0.35%	5.00%
Total payout/Total assets	5653	4.88%	7.48%	0.00%	1.75%	6.66%
Regular dividend/Total payout	4091	38.74%	40.57%	0.00%	22.37%	88.33%
Level of executive compensation (in $1000)						
Salary_$	5653	450	190	314	409	544
Bonus_$	5653	476	616	122	295	586
Option grant_$	5653	791	1137	62	384	1002
Restricted stock grant_$	5653	458	866	0	58	518
Total pay_$	5653	2481	2475	890	1647	3074
Percentage of executive compensation						
Salary_%	5653	29.6%	18.3%	16.5%	25.1%	38.2%
Bonus_%	5653	19.7%	14.8%	9.7%	17.7%	26.8%
Option grant_%	5653	28.3%	23.5%	6.7%	25.6%	44.5%
Restricted stock grant_%	5653	14.1%	18.0%	0.0%	4.1%	24.9%
Control variables						
Size ($M)	5653	11086	27294	1017	2459	8117
Log(size)	5653	7.985	1.565	6.925	7.808	9.002
Book-to-market	5653	0.445	0.287	0.251	0.388	0.578
Leverage	5653	0.202	0.172	0.038	0.187	0.309
Asset tangibility	5653	0.806	0.184	0.691	0.853	0.965
Free cash flow	5653	0.059	0.091	0.013	0.059	0.108
Cash holding	5653	0.166	0.172	0.034	0.100	0.247
ROA volatility	5653	0.056	0.056	0.021	0.038	0.070
Institutional ownership	5653	0.753	0.202	0.639	0.795	0.910
Sales growth	5653	0.121	0.189	0.028	0.096	0.183
Past stock return	5653	0.094	0.183	-0.012	0.094	0.199
Loss indicator	5653	0.164	0.370	0.000	0.000	0.000

Panel B: Pearson correlations

	Reg. div.	Repurchase	Total payout	Salary_%	Bonus_%	Option_%	RS_%	Log(size)	B/M	Leverage	Tangibility	FCF	Cash holding	ROA vol.	Inst. own.	Sales growth	Past return	Loss
Reg. div.	1.00																	
Repurchase	0.16	1.00																
Total payout	0.35	0.98	1.00															
Salary_%	-0.10	-0.18	-0.19	1.00														
Bonus_%	0.07	0.01	0.03	-0.01	1.00													
Option_%	-0.10	0.09	0.06	-0.41	-0.35	1.00												
RS_%	0.10	0.09	0.10	-0.32	-0.11	-0.40	1.00											
Log(size)	0.31	0.18	0.23	-0.56	0.11	0.12	0.22	1.00										
B/M	-0.21	-0.28	-0.31	0.30	-0.03	-0.20	0.00	-0.33	1.00									
Leverage	-0.01	-0.08	-0.08	-0.05	0.06	-0.11	0.08	0.19	-0.03	1.00								
Tangibility	0.02	0.01	0.01	0.12	0.01	-0.05	-0.06	-0.12	-0.04	-0.18	1.00							
FCF	0.22	0.37	0.39	-0.20	0.09	0.11	-0.01	0.24	-0.32	-0.21	-0.16	1.00						
Cash holding	-0.13	0.10	0.07	0.02	-0.14	0.22	-0.10	-0.23	-0.16	-0.35	0.29	0.12	1.00					
ROA vol.	-0.15	-0.04	-0.07	0.04	-0.11	0.19	-0.11	-0.23	-0.08	-0.11	0.12	-0.02	0.39	1.00				
Inst. own.	-0.09	0.14	0.11	-0.17	0.02	0.07	0.12	0.07	-0.01	0.03	-0.08	0.14	-0.06	-0.11	1.00			
Sales growth	-0.08	-0.04	-0.06	-0.15	0.05	0.10	-0.02	0.07	-0.16	-0.03	-0.08	0.13	0.02	0.14	0.04	1.00		
Past return	0.09	0.13	0.14	-0.18	0.15	-0.01	0.03	0.16	-0.29	-0.10	0.01	0.27	-0.14	-0.10	0.16	0.27	1.00	
Loss	-0.20	-0.19	-0.22	0.18	-0.18	0.04	-0.05	-0.26	0.19	0.13	0.06	-0.41	0.17	0.23	-0.16	-0.18	-0.40	1.00

Panel C: Firm characteristics - before and after FAS123R

	Pre FAS 123R	Post FAS 123R	Difference	
Payout variables				
Regular dividend/Total assets	0.83%	0.99%	0.16%	***
Repurchase/Total assets	2.45%	4.90%	2.45%	***
Total payout/Total assets	3.28%	5.89%	2.61%	***
Regular dividend/Total payout	39.14%	27.90%	-11.24%	***
Level of executive compensation (in $1000)				
Salary_$	426	463	37	***
Bonus_$	418	505	87	***
Option grant_$	896	707	-191	***
Restricted stock grant_$	269	574	305	***
Total pay_$	2263	2584	321	
Percentage of executive compensation				
Salary_%	30.5%	29.3%	-1.2%	*
Bonus_%	18.6%	20.4%	1.8%	***
Option grant_%	34.3%	24.0%	-10.3%	***
Restricted stock grant_%	8.7%	17.6%	8.9%	***
Control variables				
Size ($M)	9671	11631	1960	*
Log(size)	7.860	8.035	0.175	***
Book-to-market	0.437	0.453	0.016	
Leverage	0.201	0.203	0.002	
Asset tangibility	0.822	0.793	-0.029	***
Free cash flow	0.061	0.057	-0.004	
Cash holding	0.175	0.159	-0.016	**
ROA volatility	0.067	0.048	-0.019	***
Institutional ownership	0.712	0.781	0.069	***
Sales growth	0.137	0.108	-0.029	***
Past stock return	0.080	0.101	0.021	***
Loss indicator	0.173	0.159	-0.014	

Panel D: Firm characteristics by quartile of accounting impact - before and after FAS123R

	1st Quartile		2nd Quartile		3rd Quartile		4th Quartile	
Accounting impact (2002)	0.12%		0.35%		0.93%		5.51%	
Number of Firms	294		294		294		294	
	Pre	Post	Pre	Post	Pre	Post	Pre	Post
Payout variables								
Regular dividend/Total assets	1.12%	1.24%	0.94%	1.11%	0.95%	1.13%	0.30%	0.49%
Repurchase/Total assets	1.13%	2.68%	1.92%	3.73%	3.27%	6.31%	3.51%	6.87%
Total payout/Total assets	2.24%	3.91%	2.85%	4.84%	4.22%	7.44%	3.81%	7.36%
Regular dividend/Total payout	64.37%	46.98%	47.40%	34.11%	28.45%	19.54%	8.25%	7.45%
Percentage of executive compensation								
Salary_%	33.7%	29.5%	31.3%	28.7%	29.6%	28.9%	27.6%	30.0%
Bonus_%	22.3%	22.6%	21.4%	22.8%	17.2%	19.2%	13.4%	16.9%
Option grant_%	20.1%	16.7%	29.0%	21.0%	37.3%	26.0%	50.7%	32.5%
Restricted stock grant_%	11.6%	19.4%	9.8%	18.0%	8.9%	17.6%	4.3%	15.5%

Panel E: Firm characteristics by alternative treatment definition - before and after FAS123R

	Control		Treated	
Accounting impact	1.0%		1.9%	
Number of Firms	177		997	
By accounting impact quartile				
1	71		223	
2	44		250	
3	37		256	
4	25		268	
	Pre	Post	Pre	Post
Payout variables				
Regular dividend/Total assets	0.9%	1.1%	0.8%	1.0%
Repurchase/Total assets	1.8%	3.1%	2.6%	5.2%
Total payout/Total assets	2.8%	4.3%	3.4%	6.2%
Regular dividend/Total payout	42.8%	35.7%	38.6%	26.5%
Percentage of executive compensation				
Salary_%	43.5%	36.4%	28.2%	28.0%
Bonus_%	22.6%	21.4%	17.9%	20.2%
Option grant_%	9.2%	12.5%	38.7%	26.1%
Restricted stock grant_%	12.5%	18.8%	8.0%	17.4%
Percentage of CEO compensation				
Salary_%(CEO)	48.5%	36.4%	26.0%	25.9%
Bonus_%(CEO)	24.9%	22.8%	18.5%	21.9%
Option grant_%(CEO)	1.0%	12.7%	40.6%	27.5%
Restricted stock grant_%(CEO)	12.8%	18.2%	8.2%	18.3%

Table 2

Differential effect of FAS123R on the use of option-based compensation

This table investigates whether the effect of FAS123R on the use of option-based pay varies with its potential accounting impact. Panel A reports the results of estimating three two-period first-differencing models, where for each firm we average its characteristics in Pre- and Post-FAS123R period. The Pre-FAS123R period consists of fiscal year 2003 and 2004; the Post-FAS123R period covers fiscal year 2005-2007. In all three specifications, the dependent variable is the change of average Option grant/Total pay from Pre- to Post-FAS123R period. The main independent variable varies by model: in Model (1), the Accounting impact rank is the quartile rank of implied option expense/total assets measured in 2002 (1=lowest, 4=highest). In Model (2), Accounting impact rank=2 (3, 4) is an indicator that takes value of one if the firm's Accounting impact rank equals 2 (3, 4). In Model (3) we use an alternative treatment construction following Bakke et al. (2015), where Treated is an indicator that takes value of zero if the firm granted zero option to its CEO in both 2003 and 2004, or the firm began to voluntarily expense option-based pay at fair value in 2002 or earlier, and one otherwise. For each control variable, we include both its level in the Pre-FAS123R period and its change across the two periods. Panel B shows the results of estimating fixed effect models with firm-year observations. The dependent variable is the firm-year level Option grant/Total pay. The main independent variables are the interactions of Post-FAS123R indicator (Post) with Accounting Impact rank, Accounting impact rank indicators and Treated, respectively. All models in Penal A have industry fixed effects, and the standard errors are Huber-White heteroscedasticity-consistent standard errors. All models in Penal B have firm fixed effects and year fixed effects, and the standard errors are clustered at firm level. All variables are defined in Appendix. The notation *, ** and *** indicates statistically significant at 10%, 5% and 1% level, respectively.

Panel A: Two-period first-difference regression

Dependent variable: ΔAvg. Option grant/Total pay

		Model (1)		Model (2)		Model (3)	
	Prediction	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Accounting impact rank	-	-0.044	-6.28 ***				
Accounting impact rank=2	-			-0.044	-3.27 ***		
Accounting impact rank=3	-			-0.075	-4.68 ***		
Accounting impact rank=4	-			-0.140	-6.13 ***		
Treated	-					-0.132	-8.75 ***
ΔLog(size)		0.061	2.02 **	0.061	2.00 **	0.063	2.14 **
ΔBook-to-market		0.032	0.91	0.032	0.92	0.051	1.43
ΔLeverage		-0.157	-2.13 **	-0.160	-2.18 **	-0.111	-1.53
ΔAsset tangibility		-0.012	-0.14	-0.020	-0.23	-0.023	-0.26
ΔFree cash flow		-0.106	-0.95	-0.108	-0.97	-0.128	-1.17
ΔCash holding		-0.197	-2.11 **	-0.193	-2.06 **	-0.180	-1.94 **
ΔFive-year ROA volatility		-0.065	-0.31	-0.056	-0.26	-0.098	-0.47
ΔInstitutional ownership		-0.044	-0.91	-0.043	-0.88	-0.044	-0.92
ΔSales growth		-0.064	-1.02	-0.065	-1.04	-0.090	-1.53
ΔPast one-year stock return		0.051	0.78	0.052	0.80	0.091	1.38
ΔLoss frequency		0.056	2.07 **	0.057	2.07 **	0.047	1.79 *
Log(size)		-0.009	-2.10 **	-0.009	-2.07 **	-0.002	-0.35
Book-to-market		-0.015	-0.56	-0.013	-0.49	0.018	0.72
Leverage		0.022	0.46	0.023	0.48	0.068	1.50
Asset tangibility		0.033	0.77	0.033	0.78	0.053	1.28
Free cash flow		0.227	2.02 **	0.220	1.95 **	0.127	1.17
Cash holding		-0.002	-0.04	0.012	0.19	-0.078	-1.41
Five-year ROA volatility		-0.451	-2.40 ***	-0.430	-2.28 ***	-0.509	-2.83 ***
Institutional ownership		-0.123	-3.60 ***	-0.122	-3.59 ***	-0.100	-2.92 ***
Sales growth		-0.041	-0.56	-0.040	-0.55	-0.089	-1.25
Past one-year stock return		-0.009	-0.15	-0.006	-0.10	0.078	1.25
Loss frequency		0.055	1.74 *	0.056	1.78 *	0.054	1.73 *
Fixed effect		Industry		Industry		Industry	
Standard error		Huber-White		Huber-White		Huber-White	
Adj. R-squared		0.124		0.123		0.156	
Observations		1171		1171		1171	

Panel B: Panel fixed effects regression

Dependent variable: Option grant/Total pay

		Model (1)		Model (2)		Model (3)	
	Prediction	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Acc. impact rank x Post	-	-0.044	-8.07 ***				
Acc. impact rank 2 x Post	-			-0.046	-3.33 ***		
Acc. impact rank 3 x Post	-			-0.077	-5.26 ***		
Acc. impact rank 4 x Post	-			-0.137	-7.76 ***		
Treated x Post	-					-0.145	-9.70 ***
Log(size)		0.034	2.21 **	0.034	2.17 **	0.036	2.29 **
Book-to-market		0.018	0.84	0.017	0.83	0.015	0.70
Leverage		-0.004	-0.09	-0.006	-0.13	-0.007	-0.15
Asset tangibility		0.007	0.12	0.004	0.06	0.011	0.18
Free cash flow		-0.074	-1.51	-0.072	-1.46	-0.078	-1.58
Cash holding		-0.048	-0.86	-0.050	-0.90	-0.006	-0.11
Five-year ROA volatility		0.250	2.31 **	0.239	2.19 **	0.437	4.14 ***
Institutional ownership		0.043	1.59	0.044	1.61	0.041	1.50
Sales growth		-0.033	-1.68 *	-0.034	-1.71 *	-0.031	-1.58
Past one-year stock return		0.067	2.21 **	0.068	2.24 **	0.063	2.04 **
Loss indicator		0.023	2.09 **	0.022	2.07 **	0.024	2.22 **
Fixed effects		Firm, Year		Firm, Year		Firm, Year	
Standard error		Clustered by firm		Clustered by firm		Clustered by firm	
Adj. R-squared		0.523		0.523		0.526	
Observations		5653		5653		5653	

Table 3

Effect of option-based compensation on dividend policy

This table investigates the causal effect of option-based pay on dividend policy by examining whether changes in dividend policy around FAS123R vary with its potential accounting impact. Panel A reports the results of estimating three two-period first-differencing models, where for each firm we average its characteristics in Pre- and Post-FAS123R period. In all three specifications, the dependent variable is the change of average Regular dividend/Total assets from Pre- to Post-FAS123R period. The main independent variable varies by model: in Model (1), the Accounting impact rank is the quartile rank of implied option expense/total assets measured in 2002 (1=lowest, 4=highest). In Model (2), Accounting impact rank=2 (3, 4) is an indicator that takes value of one if the firm's Accounting impact rank equals 2 (3, 4). In Model (3) we use an alternative treatment construction following Bakke et al. (2015), where Treated is an indicator that takes value of zero if the firm granted zero option to its CEO in both 2003 and 2004, or the firm began to voluntarily expense option-based pay at fair value in 2002 or earlier, and one otherwise. For each control variable, we include both its level in the Pre-FAS123R period and its change across the two periods. Panel B shows the results of estimating fixed effect models with firm-year observations. The dependent variable is the firm-year level Regular dividend/Total assets. The main independent variables are the interactions of Post-FAS123R indicator (Post) with Accounting Impact rank, Accounting impact rank indicators and Treated, respectively. Panel C examines the intensity of dividend changes among firms with nonzero dividend per share in 2003 or 2004. In particular, we estimates the same three models as in Panel A(with the same independent variables) but for five dependent variables: the change of average Regular dividend/Total assets from Pre- to Post-FAS123R period (as in Panel A), the percentage change of DPS (dividend per share) from Pre- to Post-FAS123R period, and three indicators denoting, respectively, whether the percentage change of DPS was greater than 20%, 40% and 60%. Panel D reports four robustness tests of the dividend intensity regression. In each of them a set of possible confounding observations is removed (see Section 3.3 for details). Panel E compares the trends of dividend initiation and termination around FAS123R across the four quartile groups ranked by accounting impact. Panel F shows the statistics for special dividends across the four quartile groups. All models in Panel A, C and D have industry fixed effects, and the standard errors are Huber-White heteroscedasticity-consistent standard errors. All models in Penal B have firm fixed effects and year fixed effects, and the standard errors are clustered at firm level. All variables are defined in Appendix. The notation *, ** and *** indicates statistically significant at 10%, 5% and 1% level, respectively.

Panel A: Two-period first-difference regression

Dependent variable: ΔAvg. Regular dividend/Total assets

		Model (1)		Model (2)		Model (3)	
	Prediction	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Accounting impact rank	+	0.000	0.77				
Accounting impact rank=2	+			0.001	0.91		
Accounting impact rank=3	+			0.000	0.44		
Accounting impact rank=4	+			0.001	0.92		
Treated	+					0.000	-0.26
ΔLog(size)		0.000	-0.30	0.000	-0.30	0.000	-0.26
ΔBook-to-market		-0.002	-1.90 *	-0.002	-1.93 *	-0.002	-1.95 *
ΔLeverage		-0.002	-0.59	-0.002	-0.57	-0.002	-0.59
ΔAsset tangibility		0.016	4.31 ***	0.016	4.29 ***	0.016	4.30 ***
ΔFree cash flow		0.017	3.73 ***	0.017	3.73 ***	0.017	3.75 ***
ΔCash holding		-0.011	-2.88 ***	-0.011	-2.85 ***	-0.010	-2.82 ***
ΔFive-year ROA volatility		0.003	0.34	0.002	0.32	0.003	0.35
ΔInstitutional ownership		-0.002	-0.73	-0.002	-0.73	-0.002	-0.70
ΔSales growth		-0.001	-0.40	-0.001	-0.37	-0.001	-0.38
ΔPast one-year stock return		0.002	0.77	0.002	0.76	0.002	0.70
ΔLoss frequency		0.000	-0.48	0.000	-0.45	0.000	-0.46
Log(size)		0.000	1.83 *	0.000	1.82 *	0.000	1.82 *
Book-to-market		-0.002	-2.27 **	-0.002	-2.25 **	-0.002	-2.84 **
Leverage		-0.001	-0.37	-0.001	-0.39	-0.001	-0.48
Asset tangibility		0.003	1.79 *	0.003	1.80 *	0.003	1.76 *
Free cash flow		0.017	2.91 ***	0.017	2.91 ***	0.017	3.00 ***
Cash holding		0.002	0.80	0.002	0.77	0.002	0.98
Five-year ROA volatility		0.003	0.35	0.002	0.33	0.003	0.44
Institutional ownership		-0.001	-0.68	-0.001	-0.68	-0.001	-0.58
Sales growth		-0.004	-1.77 *	-0.004	-1.75	-0.004	-1.72 *
Past one-year stock return		0.004	1.69 *	0.004	1.67 *	0.004	1.65 *
Loss frequency		-0.001	-1.11	-0.001	-1.09	-0.001	-1.02
Fixed effect		Industry		Industry		Industry	
Standard error		Huber-White		Huber-White		Huber-White	
Adj. R-squared		0.105		0.104		0.105	
Observations		1171		1171		1171	

Panel B: Panel fixed effects regression

Dependent variable: Regular dividend/Total assets

		Model (1)		Model (2)		Model (3)	
	Prediction	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Acc. impact rank x Post	+	0.000	1.22				
Acc. impact rank 2 x Post	+			0.001	1.10		
Acc. impact rank 3 x Post	+			0.001	0.94		
Acc. impact rank 4 x Post	+			0.001	1.29		
Treated x Post	+					0.000	-0.12
Log(size)		0.001	1.79 *	0.001	1.79 *	0.001	1.81 *
Book-to-market		0.000	0.25	0.000	0.24	0.000	0.31
Leverage		0.001	0.62	0.001	0.64	0.001	0.69
Asset tangibility		0.009	3.60 ***	0.009	3.58 ***	0.009	3.63 ***
Free cash flow		0.002	0.90	0.002	0.93	0.002	0.92
Cash holding		-0.007	-2.95 ***	-0.007	-2.98 ***	-0.007	-2.98 ***
Five-year ROA volatility		0.001	0.41	0.001	0.38	0.000	-0.06
Institutional ownership		0.000	-0.27	0.000	-0.26	0.000	-0.27
Sales growth		0.000	0.53	0.000	0.51	0.000	0.44
Past one-year stock return		0.001	0.89	0.001	0.88	0.001	0.78
Loss indicator		0.000	-0.60	0.000	-0.57	0.000	-0.76
Fixed effects		Firm, Year		Firm, Year		Firm, Year	
Standard error		Clustered by firm		Clustered by firm		Clustered by firm	
Adj. R-squared		0.877		0.877		0.877	
Observations		5653		5653		5653	

Panel C: Intensity of dividend changes around FAS123R

		Model (1)		Model (2)		Model (3)	
	Prediction	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Dependent var: ΔAvg. Regular dividend/Assets							
Accounting impact rank	+	0.000	0.65				
Accounting impact rank=2	+			0.000	0.48		
Accounting impact rank=3	+			0.000	0.26		
Accounting impact rank=4	+			0.002	0.80		
Treated	+					0.001	0.63
Dependent var: DPS pct. change							
Accounting impact rank	+	-0.145	-0.72				
Accounting impact rank=2	+			0.018	0.06		
Accounting impact rank=3	+			-0.177	-0.45		
Accounting impact rank=4	+			-0.746	-1.00		
Treated	+					-0.407	-1.19
Dependent var: DPS pct. change >20% indicator							
Accounting impact rank	+	0.029	0.95				
Accounting impact rank=2	+			0.040	0.74		
Accounting impact rank=3	+			0.092	1.39		
Accounting impact rank=4	+			0.008	0.07		
Treated	+					-0.007	-0.12
Dependent var: DPS pct. change >40% indicator							
Accounting impact rank	+	0.024	0.76				
Accounting impact rank=2	+			0.047	0.88		
Accounting impact rank=3	+			0.079	1.14		
Accounting impact rank=4	+			-0.003	-0.02		
Treated	+					0.030	0.52
Dependent var: DPS pct. change >60% indicator							
Accounting impact rank	+	0.030	0.98				
Accounting impact rank=2	+			0.078	1.55		
Accounting impact rank=3	+			0.085	1.30		
Accounting impact rank=4	+			0.014	0.12		
Treated	+					-0.006	-0.11
Control variables		Yes		Yes		Yes	
Fixed effect		Industry		Industry		Industry	
Standard error		Huber-White		Huber-White		Huber-White	
Observations		572		572		572	

Panel D: Robustness tests for intensity of dividend changes around FAS123R

Dependent variable: DPS pct. change >40% indicator

		Model (1)		Model (2)		Model (3)	
	Prediction	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Exclude voluntary expensers							
Accounting impact rank	+	0.006	0.17				
Accounting impact rank=2	+			0.067	1.18		
Accounting impact rank=3	+			0.041	0.57		
Accounting impact rank=4	+			-0.061	-0.49		
Treated	+					0.022	0.35
Observations		504		504		504	
Exclude firms in the top decile of option reduction (02-04)							
Accounting impact rank	+	0.009	0.27				
Accounting impact rank=2	+			0.058	1.06		
Accounting impact rank=3	+			0.032	0.43		
Accounting impact rank=4	+			-0.032	-0.24		
Treated	+					0.018	0.28
Observations		517		517		517	
Exclude firms that accelerated option vesting							
Accounting impact rank	+	0.030	0.89				
Accounting impact rank=2	+			0.039	0.71		
Accounting impact rank=3	+			0.122	1.68 *		
Accounting impact rank=4	+			-0.063	-0.52		
Treated	+					0.075	1.32
Observations		519		519		519	
Exclude firms with stable payout ratio from 2003 to 2004							
Accounting impact rank	+	0.008	0.23				
Accounting impact rank=2	+			0.023	0.42		
Accounting impact rank=3	+			0.048	0.64		
Accounting impact rank=4	+			-0.042	-0.33		
Treated	+					-0.010	-0.16
Observations		506		506		506	
Control variables		Yes		Yes		Yes	
Fixed effect		Industry		Industry		Industry	
Standard error		Huber-White		Huber-White		Huber-White	

Panel E: Dividend initiation and termination rates around FAS123R

Initiation

Pre FAS 123R	Quartile	# firms	# without dividend in 2002	# initiation in 2003	# initiation in 2004	# 03-04 cumulative	% 03-04 cumulative
	1	280	75	7	6	13	17.3%
	2	284	135	18	9	27	20.0%
	3	286	181	11	14	25	13.8%
	4	282	260	9	10	19	7.3%
	Total	1132	651	45	39	84	12.9%

Post FAS 123R	Quartile	# firms	# without dividend in 2004	# initiation in 2005	# initiation in 2006	# 05-06 cumulative	% 05-06 cumulative
	1	275	63	4	4	8	12.7%
	2	280	116	9	9	18	15.5%
	3	276	152	9	3	12	7.9%
	4	263	223	5	1	6	2.7%
	Total	1094	554	27	17	44	7.9%

Termination

Pre FAS 123R	Quartile	# firms	# with dividend in 2002	# termination in 2003	# termination in 2004	# 03-04 cumulative	% 03-04 cumulative
	1	280	205	2	0	2	1.0%
	2	284	149	7	1	8	5.4%
	3	286	105	0	0	0	0.0%
	4	282	22	0	0	0	0.0%
	Total	1132	481	9	1	10	2.1%

Post FAS 123R	Quartile	# firms	# with dividend in 2004	# termination in 2005	# termination in 2006	# 05-06 cumulative	% 05-06 cumulative
	1	275	212	3	6	9	4.2%
	2	280	164	1	2	3	1.8%
	3	276	124	1	1	2	1.6%
	4	263	40	0	3	3	7.5%
	Total	1094	540	5	12	17	3.1%

Panel F: Special dividends around FAS123R

Pre FAS 123R	Accounting impact rank	Number of firm-years	Firm-years with special dividend (#)	Firm-years with special dividend (%)	Special dividend/Total assets	Special dividend/Total assets (payers)
	1	585	15	2.6%	0.0014	0.0549
	2	586	8	1.4%	0.0012	0.0846
	3	588	5	0.9%	0.0002	0.0251
	4	585	4	0.7%	0.0003	0.0432

Post FAS 123R	Accounting impact rank	Number of firm-years	Firm-years with special dividend (#)	Firm-years with special dividend (%)	Special dividend/Total assets	Special dividend/Total assets (payers)
	1	833	25	3.0%	0.0026	0.0864
	2	840	6	0.7%	0.0014	0.1999
	3	821	11	1.3%	0.0019	0.1390
	4	815	9	1.1%	0.0015	0.1370

Table 4

Effect of option-based compensation on repurchases

This table investigates the causal effect of option-based pay on payout policy by examining whether the changes of repurchase around FAS123R vary with its potential accounting impact. Panel A reports the results of estimating three two-period first-differencing models, where for each firm we average its characteristics in Pre- and Post-FAS123R period. In all three specifications, the dependent variable is the change of average Repurchase/Total assets from Pre- to Post-FAS123R period. The main independent variable varies by model: in Model (1), the Accounting impact rank is the quartile rank of implied option expense/total assets measured in 2002 (1=lowest, 4=highest). In Model (2), Accounting impact rank=2 (3, 4) is an indicator that takes value of one if the firm's Accounting impact rank equals 2 (3, 4). In Model (3) we use an alternative treatment construction following Bakke et al. (2015), where Treated is an indicator that takes value of zero if the firm granted zero option to its CEO in both 2003 and 2004, or the firm began to voluntarily expense option-based pay at fair value in 2002 or earlier, and one otherwise. Panel B reports the regressions with the change of Net repurchase/Total assets as the dependent variable and the same independent variable as in Panel A. Panel C reports the regressions with the change of Adjusted repurchase/Total assets as the dependent variable and the same independent variable as in Panel A. All models in Panel A, B and C have industry fixed effects, and the standard errors are Huber-White heteroscedasticity-consistent standard errors. All variables are defined in Appendix. The notation *, ** and *** indicates statistically significant at 10%, 5% and 1% level, respectively.

Panel A: Two-period first-difference regression with total repurchase

Dependent variable: ΔAvg. Repurchase/Total assets

		Model (1)		Model (2)		Model (3)	
	Prediction	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Accounting impact rank	-	0.002	1.22				
Accounting impact rank=2	-			0.002	0.71		
Accounting impact rank=3	-			0.005	1.23		
Accounting impact rank=4	-			0.007	1.02		
Treated	-					0.014	3.65 ***
Control variables		Yes		Yes		Yes	
Fixed effect		Industry		Industry		Industry	
Standard error		Huber-White		Huber-White		Huber-White	
Adj. R-squared		0.238		0.237		0.245	
Observations		1171		1171		1171	

Panel B: Two-period first-difference regression with net repurchase

Dependent variable: ΔAvg. Net repurchase/Total assets

		Model (1)		Model (2)		Model (3)	
	Prediction	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Accounting impact rank	-	0.003	1.66 *				
Accounting impact rank=2	-			0.002	0.51		
Accounting impact rank=3	-			0.006	1.65 *		
Accounting impact rank=4	-			0.009	1.37		
Treated	-					0.010	3.00 ***
Control variables		Yes		Yes		Yes	
Fixed effect		Industry		Industry		Industry	
Standard error		Huber-White		Huber-White		Huber-White	
Adj. R-squared		0.229		0.229		0.233	
Observations		1171		1171		1171	

Panel C: Two-period first-difference regression with repurchase adjusted for exercised options

Dependent variable: ΔAvg. Adjusted repurchase/Total assets

		Model (1)		Model (2)		Model (3)	
	Prediction	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Accounting impact rank	-	0.001	0.56				
Accounting impact rank=2	-			0.002	0.62		
Accounting impact rank=3	-			0.007	1.72 *		
Accounting impact rank=4	-			-0.001	-0.08		
Treated	-					0.006	1.97 **
Control variables		Yes		Yes		Yes	
Fixed effect		Industry		Industry		Industry	
Standard error		Huber-White		Huber-White		Huber-White	
Adj. R-squared		0.111		0.112		0.114	
Observations		1168		1168		1168	

Table 5

Effect of option-based compensation on payout mix

This table investigates the causal effect of option-based pay on payout mix for firms using both dividend and repurchase by examining whether the changes of payout mix around FAS123R vary with its potential accounting impact. Panel A reports the results of estimating three two-period first-differencing models, where for each firm we average its characteristics in Pre- and Post-FAS123R period. In all three specifications, the dependent variable is the change of average Regular dividend/Total payout from Pre- to Post-FAS123R period. The main independent variable varies by model: in Model (1), the Accounting impact rank is the quartile rank of implied option expense/total assets measured in 2002 (1=lowest, 4=highest). In Model (2), Accounting impact rank=2 (3, 4) is an indicator that takes value of one if the firm's Accounting impact rank equals 2 (3, 4). In Model (3) we use an alternative treatment construction following Bakke et al. (2015), where Treated is an indicator that takes value of zero if the firm granted zero option to its CEO in both 2003 and 2004, or the firm began to voluntarily expense option-based pay at fair value in 2002 or earlier, and one otherwise. Panel B reports the regressions with the change of Regular dividend/Net payout as the dependent variable and the same independent variables as in Panel A. Panel A (Panel B) is based on observations with nonzero regular dividend and repurchase (net repurchase) in both Pre- and Post-FAS123R period. All models in Panel A and B have industry fixed effects, and the standard errors are Huber-White heteroscedasticity-consistent standard errors. All variables are defined in Appendix. The notation *, ** and *** indicates statistically significant at 10%, 5% and 1% level, respectively.

Panel A: Two-period first-difference regression with total payout mix

Dependent variable: ΔAvg. Regular dividend/Total payout

		Model (1)		Model (2)		Model (3)	
	Prediction	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Accounting impact rank	+	0.006	0.28				
Accounting impact rank=2	+			0.018	0.45		
Accounting impact rank=3	+			-0.001	-0.01		
Accounting impact rank=4	+			0.043	0.52		
Treated	+					-0.035	-0.94
Control variables		Yes		Yes		Yes	
Fixed effect		Industry		Industry		Industry	
Standard error		Huber-White		Huber-White		Huber-White	
Adj. R-squared		0.154		0.151		0.156	
Observations		362		362		362	

Panel B: Two-period first-difference regression with net payout mix

Dependent variable: ΔAvg. Regular dividend/Net payout

		Model (1)		Model (2)		Model (3)	
	Prediction	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Accounting impact rank	+	-0.002	-0.08				
Accounting impact rank=2	+			0.008	0.16		
Accounting impact rank=3	+			0.018	0.34		
Accounting impact rank=4	+			-0.059	-0.55		
Treated	+					-0.050	-1.01
Control variables		Yes		Yes		Yes	
Fixed effect		Industry		Industry		Industry	
Standard error		Huber-White		Huber-White		Huber-White	
Adj. R-squared		0.049		0.043		0.054	
Observations		257		257		257	

Table 6

Expand the sample to include Compustat firms without compensation data

This table examines the effect of option-based pay on dividend and repurchase by extending the baseline research design to all firms in Compustat with financial and stock data. Panel A reports the results of estimating three two-period first-differencing models, where for each firm we average its characteristics in Pre- and Post-FAS 123R period. In the first two specifications, the dependent variable is the change of average Regular dividend/Total assets from Pre- to Post-FAS 123 period. The main independent variable varies by model: in Model (1), the Accounting impact rank is the quartile rank of implied option expense/total assets measured in 2002 (1=lowest, 4=highest). In Model (2), Accounting impact rank=2 (3, 4) is an indicator that takes value of one if the firm's Accounting impact rank equals 2 (3, 4). In Model (3) and (4), we replace the dependent variable with the change of average Total repurchase/Total assets, and use the same independent variables as in the first two models. Panel B examines the intensity of dividend changes among firms in the extended sample with nonzero dividend per share in 2003 or 2004. In Model (1) and (2), we estimate the same first two models as in Panel A. In Model (3) and (4), we replace the dependent variable with an indicator denoting whether the percentage change of DPS was greater than 40%, and use the same independent variables as in Model (1) and (2). All models have industry fixed effects, and the standard errors are Huber-White heteroscedasticity-consistent standard errors. All variables are defined in Appendix 1. The notation *, ** and *** indicates statistically significant at 10%, 5% and 1% level, respectively.

Panel A: Dividend and repurchase changes with extended sample

	ΔAvg. Regular dividend/Total assets				ΔAvg. Repurchase/Total assets			
	Model (1)		Model (2)		Model (3)		Model (4)	
	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Accounting impact rank	0.000	-0.26			0.004	4.19 ***		
Accounting impact rank=2			0.001	1.58			0.003	1.62
Accounting impact rank=3			0.000	0.23			0.009	3.67 ***
Accounting impact rank=4			0.000	-0.31			0.011	3.78 ***
Control variables	Yes		Yes		Yes		Yes	
Fixed effect	Industry		Industry		Industry		Industry	
Standard error	Huber-White		Huber-White		Huber-White		Huber-White	
Observations	2961		2961		2961		2961	

Panel B: Intensity of dividend changes with extended sample

	ΔAvg. Regular dividend/Total assets				DPS pct. change >40% indicator			
	Model (1)		Model (2)		Model (3)		Model (4)	
	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Accounting impact rank	0.001	2.39 **			0.062	2.31 **		
Accounting impact rank=2			0.001	1.42			0.109	2.76 ***
Accounting impact rank=3			0.002	1.69 *			0.121	2.01 **
Accounting impact rank=4			0.005	1.67 *			0.117	1.09
Control variables	Yes		Yes		Yes		Yes	
Fixed effect	Industry		Industry		Industry		Industry	
Standard error	Huber-White		Huber-White		Huber-White		Huber-White	
Observations	873		873		873		873	