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Does Option-Based Compensation Affect Payout Policy? Evidence from FAS 123R

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Abstract

Does option-based compensation affect payout policy? To address this question, we examine the adoption of mandatory expensing of stock options. Our identification strategy exploits the fact that the reduction in option-based compensation after the accounting change varies with the firm-specific expected accounting impact. Across a battery of tests, we do not find that (accounting-driven) reductions in option-based pay cause dividends to increase or repurchases to decrease. 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.

I. Introduction

We examine whether option-based executive compensation has a causal influence on payout policy. The motivation for the study is twofold. The first is to better understand the economic consequences of the use of stock options to compensate executives. Although the incentive properties of stock options are well studied, evidence of their causal effects on firm outcomes is limited, largely because of well-known identification challenges (Edmans and Gabaix (2016)). The second motivation is to shed light on the determinants of payout policy, a key element of corporate finance (Miller and Modigliani (1961), Jensen (1986), and Fama and French (2001)). As noted in a recent paper (Farre-Mensa, Michaely, and Schmalz (2014)), traditional theories of payout policy (e.g., agency,

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signaling, clientele, and tax-based theories) help explain cross-sectional variation in corporate payouts but not the shift from dividends to repurchases as the primary payout vehicle over the last 30 years (Skinner (2008)). In contrast, several studies suggest that this trend occurred because using stock options for executive pay encourages firms to favor repurchases over dividends.

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,[1] option grants give managers the incentive to avoid/reduce dividends and (if there is a target payout amount) to replace them with repurchases. That is, as option-based pay increases (decreases), managers bear higher (lower) personal costs in issuing dividends, which may translate into a payout mix with lower (higher) dividends. 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) (Kahle (2002)). A variation of this argument 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)).[2] 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 generally consistent with this prediction, largely based on data from the 1990s (e.g., Jolls (1998), Weisbenner (2000), Fenn and Liang (2001), Kahle (2002), Bens et al. (2003), and Cuny et al. (2009)).

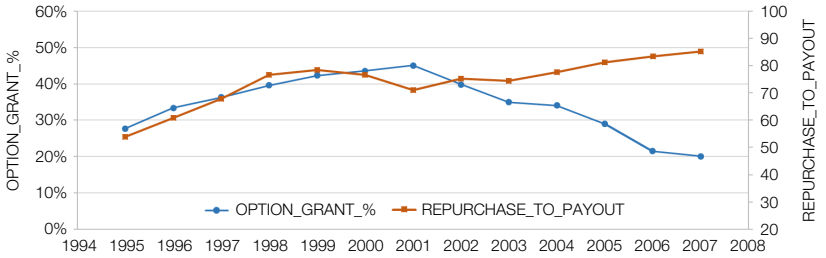
However, as shown in Figure 1, during the 1990s option-based compensation and the use of repurchases as a payout vehicle were increasing in tandem. In contrast, in the subsequent decade the two trends started to diverge, with the use of option-based pay trending downward while repurchases continued to account for an increasing fraction of total payout. Aside from this (admittedly casual) observation, there are three reasons to question a causal interpretation of the association documented in the literature. First, if boards anticipate the consequences of a

[1]Zhang (2018) finds that between 2000 and 2009, less than 1% of Standard & Poor's (S&P) 500 firms provide dividend protection, confirming evidence for earlier periods (Murphy (1999), Weisbenner (2000), and Cuny, Martin, and Puthenpurackal (2009)). For a discussion of the institutional reasons for lack of dividend protection, see Fenn and Liang (2001).

[2]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 ESO move into the money. Note that firms have incentives to use repurchases to manage EPS regardless of whether they use ESO (e.g., Hribar, Thorne Jenkins, and Johnson (2006), Young and Yang (2011), Cheng, Harford, and Zhang (2015), and Kim and Ng (2018)).

FIGURE 1
Trends of Option-Based Compensation and Payout Mix from 1995 to 2007

Figure 1 plots the average ratio of option grant (fair value) to total pay among the top 5 executives and the ratio of repurchases to total payout (repurchases plus dividends) for all firms in ExecuComp with nonmissing data.



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 scrutinized. Given the relevance of payout choices to boards, analysts, and institutional 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 (Edmans and Gabaix (2016)). 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 cannot be easily controlled for (the omitted variable bias problem). Furthermore, executive pay may be the result of anticipated payout policy (the reverse causality problem; e.g., Aboody and Kasznik (2008)).

To address these problems, we employ the 2005 adoption of Financial Accounting Standard 123R (FAS 123R) as an exogenous shock to the use of option compensation. Previously, firms were required to expense ESO's intrinsic value at the grant date and disclose 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. FAS 123R required firms to expense ESO in the income statement at fair value. Consistent with FAS 123R increasing the perceived cost of stock options (Murphy (2013)), studies document a large decrease in their use after 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 FAS 123R's implementation. Such a large, sudden change in executive pay composition is rare (Frydman and Jenter (2010)) and 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

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Michaely (2005)).[3] Another advantage is that the post–FAS 123R period witnessed a large increase in corporate payouts (Floyd, Li, and Skinner (2015)). That is, there were large amounts of cash available for distribution to shareholders, and thus firms had the opportunity to shift payouts toward more dividends. These factors combine to produce a powerful setting to detect an increase in dividends, if the hypothesized causal effect took place.

Because FAS 123R applies to all public firms, it is difficult to construct control groups. Our empirical strategy exploits the idea that the impact of FAS 123R likely varies across different types of firms. We conjecture and verify that firms facing a larger hit on their reported earnings (as measured by a higher implied option expense in 2002, 3 years before FAS 123R) reduce option grants significantly more than other firms. 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 difference-in-differences design where the first difference is the adoption of FAS 123R and the second is the pre-event implied option expense, our proxy for the expected accounting-induced reduction in option-based pay. The key identification assumption is that the level of implied option expense (before the announcement of FAS 123R) is not correlated with the potential future change in payout variables in the absence of treatment (i.e., FAS 123R). We discuss this assumption in Section II.

We perform our empirical tests using a sample of S&P 1500 firms between 2003 and 2007, a 5-year event window surrounding the adoption of FAS 123R. In particular, we estimate the association between implied option expense, measured in 2002 and thus before the event window, and the change in the average payout variables from the "pre" period (2003–2004) to the "post" period (2005–2007), 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 subsample of firms that paid dividends before FAS 123R. Also, among firms that did not pay dividends before FAS 123R, those with higher implied option expense did not experience a relative increase in the rate of dividend initiation. Furthermore, we find no association between implied option expense and changes in the level of repurchases or 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 FAS 123R (e.g., voluntarily expensing options) and to alternative ways to address the lack of a control group (Bakke, Mahmudi, Chitru, and Salas (2016)). Finally,

[3]Indeed, studies (e.g. Fenn and Liang (2001)) 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.

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additional analyses suggest that the lack of an effect does not reflect limited power of the FAS 123R setting.

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. Thus, 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. (2016), Mao and Zhang (2018)). An additional explanation for our results is that boards compensate executives for the lack of dividend protection in other ways, effectively neutralizing those incentives. Our findings are consistent with the managers' statement that lack of dividend protection does not affect payout decisions (Brav et al. (2005), who highlight the stark contrast between their survey evidence and the empirical associations in the literature). Our results provide another explanation for the persistent lack of dividend protection in executives' options: Boards are not concerned with the effect of executive pay on payout policy 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 FAS 123R, to understand the causal effect of option-based pay on economic outcomes. This research focuses on the impact on risk-taking and innovation activities (Low (2009), Chava and Purnanandam (2010), Hayes et al. (2012), Gormley, Matsa, and Milbourn (2013), and Shue and Townsend (2017)). For example, Bakke et al. (2016) find that an accounting-induced reduction in options is accompanied by a large increase in hedging intensity in the oil and gas sector. Mao and Zhang (2018) examine a broad sample of firms and document a decrease in innovation activities. Indirectly, both studies imply that FAS 123R is a powerful setting in which to detect a causal effect of a shock to option-based pay on firms' policies. In contrast, we examine a different yet important economic outcome: payout policy. Also, our methodology differs from most studies using FAS 123R in that it explicitly incorporates cross-sectional variation in how firms were affected by FAS 123R.

Our study contributes to a vast literature on the determinants of payout policy (for a review, see Farre-Mensa et al. (2014)). First, although many studies document an association between option-based pay and payout policy, few attempts have been made to examine its causal nature. Second, we extend the research that examines how payout policy is affected by institutional changes, such as changes in dividend tax rates (e.g., Chetty and Saez (2005), Brown, Liang, and Weisbenner (2007), Aboody and Kasznik (2008), Blouin, Raedy, and Shackelford (2011), and Hanlon and Hoopes (2014)) or in the information environment (Hail, Tahoun, and

Wang (2014), Koo, Ramalingegowda, and Yu (2017)), or other economic shocks (e.g., unionization; He, Tian, and Yang (2016)).

II. Research Design and Sample Selection

A. Empirical Strategy

As emphasized by Edmans and Gabaix (2016), identifying the causal effects of executive compensation is challenging, largely because of the scarcity of valid instrumental variables. FAS 123R provides a unique opportunity to address this problem. FAS 123R was introduced after the Enron-type accounting scandals of 2001–2002, partly in response to the perception that an excessive use of option-based pay (due to the favorable reporting treatment) had played a role in those scandals (Greenspan (2002), Hall and Murphy (2003)). Numerous studies document a decrease in option-based pay as a result of the new standard. However, the accounting change should not, in itself, correlate with changes in payout policy. In other words, it is reasonable to assume that FAS 123R was exogenous with respect to trends in payout policy (see Hayes et al. (2012), Mao and Zhang (2018), for a similar assumption with respect to investment policy).

A challenge in using FAS 123R is that 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. One possibility is to use the change in option-based compensation around FAS 123R, effectively assuming that such change is "exogenous" (e.g., Chava and Purnanandam (2010), Anantharaman and Lee (2014)). However, FAS 123R per se did not generate exogenous cross-sectional variation in option compensation. That is, even if the introduction of FAS 123R is exogenous, a firm's response cannot be assumed to be entirely exogenous. To identify the exogenous portion of a firm's response to the new standard, we use evidence that FAS 123R'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 before FAS 123R (Hayes et al. (2012)). The reduction in option use is highest in firms with the largest implied option expense. Thus, we can use the differential accounting impact of FAS 123R to overcome the lack of a control group.

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

$$(1)\quad \Delta\text{OPTION}_i = \beta_0 + \beta_1 \text{ACCOUNTING_IMPACT}_i + \Lambda \text{CONTROL}_i + \varepsilon_i,$$

where ΔOPTION_i is the change in the average value of executive stock options as a percentage of total pay from the pre- to the post-FAS 123R period. $\text{ACCOUNTING_IMPACT}_i$ is the implied option expense measured before FAS 123R (more details in Section II.B). 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 FAS 123R, we examine the central question of this study by estimating the following OLS regression:

$$(2)\quad \Delta\text{PAYOUT}_i = \gamma_0 + \gamma_1 \text{ACCOUNTING_IMPACT}_i + \Gamma \text{CONTROL}_i + \varepsilon_i,$$

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where $\Delta PAYOUT_i$ represents the change in the average value of one of the payout variables of interest (described in Section II.C) from the pre- to the post-FAS 123R period.

Equation (2) is essentially a difference-in-differences approach to estimate the effect of option-based pay on payout policy using the differential accounting impact of FAS 123R 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-differences 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 a firm. To alleviate this concern, following Bertrand et al., in both equations (1) and (2) we collapse the panel to a single "pre" period (2003–2004) and a single "post" period (2005–2007) by averaging across years and then measuring the changes from pre to post. We choose 2003 as the starting year to capture a period after the 2003 tax cut on individual dividend income, which may have affected payout policy (Chetty and Saez (2005), Edgerton (2013), and Floyd et al. (2015)). We end our analysis in 2007 to avoid any confounding effect of the 2008 financial crisis, which had a significant impact on payouts (Floyd et al. (2015), Kahle and Williams (2016)). Following Hayes et al. (2012), we include 2005 in the post period because FAS 123R, though effective in June 2005, was released in Dec. 2004.

Another benefit of using a single pre period and a single post period is that this is better suited than panel data to analyzing 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 2- or 3-year periods than 1-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 are not subject to the previous problem). To alleviate the problem of serial correlation when using a panel regression, we cluster the standard errors by firm.

B. Accounting Impact

As noted earlier, all firms were affected by FAS 123R, making it difficult to identify a control sample. To capture variation in the cross-sectional impact of FAS 123R, in equations (1) and (2) we use ACCOUNTING_IMPACT, 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 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

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expected adoption of FAS 123R (see discussion in Section IV.A). As this variable is highly skewed (see Section II.D), we define it based on its quartile rank. We also present results with separate indicators for the top 3 quartiles to better isolate the difference between the top quartile (the most "treated" group) and bottom quartile (the least "treated" group, and thus, closer to the notion of a control group).

An alternative way to address the lack of a control group is suggested by Bakke et al. (2016). They argue that 2 sets of firms were likely unaffected by FAS 123R: i) firms that did not grant options to the chief executive officer (CEO) in 2003 and 2004, as in these firms FAS 123R could not affect CEO option pay and thus her incentives to make policy changes, and ii) firms that were already voluntarily expensing stock options as of 2002 (i.e., before the Financial Accounting Standards Board (FASB) began discussions of the new standard), as these firms would not face any additional accounting charge under FAS 123R.

An advantage of our approach is that instead of collapsing all noncontrol firms in a single treated group, we exploit cross-sectional variation among treatment firms in terms of the likely impact of FAS 123R. Because the implied option expense is highly skewed (i.e., only a relatively small number of firms were heavily affected by FAS 123R), capturing this variation is likely to increase the power of the test. Also, in Bakke et al. (2016) 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). More than half of the firms without CEO option grants in 2003 and 2004 were issuing options to non-CEO executives and other employees and thus faced a potential accounting charge under FAS 123R (untabulated analysis), making their use as control firms questionable in our setting.[4] Nonetheless, we also present the results using the definition of treatment and control in Bakke et al. In particular, we modify equations (1) and (2) by replacing ACCOUNTING_IMPACT with TREATED, an indicator equal to 0 for the 2 groups of firms identified as control firms in Bakke et al., and 1 for all other firms.

C. Payout Variables and Control Variables

Following Brown et al. (2007), our baseline measure for changes in dividends is the change in the DIVIDEND_TO_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. It is well known that managers do not target a specific dividend-to-assets ratio but rather make dividend policy decisions in terms of changes to the current level of dividend per share (DPS) (Brav et al. (2005), Baker and Wurgler (2016)). Therefore, we also examine the percentage

[4] Another concern with the approach in Bakke et al. (2016) is that about half of the firms without CEO option grants in 2003 and 2004 did issue options to the CEO in the post period. Hence, it is not clear that the control firms were truly unaffected by FAS 123R. Also, 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 FAS 123R. In contrast, option grant decisions in 2003 and 2004 may be affected by the anticipated adoption as the FASB added accounting for stock-based compensation to its agenda in Mar. 2003.

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change in the average DPS from the pre to the post period, which, however, by definition can only be computed for the subset of dividend-paying firms (defined as firms with nonzero DPS in at least 1 year in the pre period).

Our main measure for repurchases is the change in REPURCHASE_TO_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.

Finally, we look at a measure of the payout composition, the change in the DIVIDEND_TO_PAYOUT ratio from the pre to the post period (PAYOUT is the sum of dividends and repurchases), to capture any substitution effect between dividends and repurchases (Grullon and Michaely (2002), Brown et al. (2007)). Examining 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 the pre and post periods to capture changes in payout mix by firms routinely using both vehicles to return cash to shareholders.

As for the control variables, in both equations (1) and (2), $CONTROL_i$ is a vector of firm characteristics identified in the literature as determinants of payout policy (e.g., Fenn and Liang (2001), Brown et al. (2007), Skinner (2008), and Cuny et al. (2009)): FIRM_SIZE, BOOK_TO_MARKET, LEVERAGE, ASSET_TANGIBILITY, FREE_CASH_FLOWS, CASH_HOLDINGS, ROA_VOLATILITY, INSTITUTIONAL_OWNERSHIP, SALES_GROWTH, PAST_STOCK_RETURN, and LOSS_FIRM indicator (see Appendix A for detailed definitions). For all of these variables, we include both changes from the pre to the post period and levels in the pre period to allow for the possibility that levels of these variables affect subsequent changes in payout policy.[5] Finally, we include industry fixed effects (Fama–French (1997) 48 industry indicators) to control for industry-level trends in payout policies and option-based pay.

D. Sample Selection and Summary Statistics

Our sample comprises S&P 1500 firms with available data on implied option expense (from Compustat) and on executive compensation (from ExecuComp) for at least 1 year in the pre period and 1 year in the post period to ensure consistency in the sample across periods. Similar to studies on payout policy (Fama and French (2001), Skinner (2008)), we exclude financial firms and utilities (Standard Industrial Classification (SIC) codes, respectively, between 6000–6999 and 4900–4999), and firms not incorporated in the United States. Our final sample includes 1,176 firms. Data sources are detailed in Appendix A. To deal with outliers, we winsorize all continuous variables at 1% and 99% by fiscal year.

[5]In untabulated analyses, we run our tests after excluding changes in the control variables (i.e., including only their pretreatment levels), as in difference-in-differences studies controlling variables measured after the shock may reintroduce selection bias. Also, we replace free cash flows with two separate variables, operating profit and capital expenditures, and replace volatility in operating income with volatility in stock returns. Our inferences remain unchanged.

Panel A of Table 1 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 whereas 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, and salary and bonus represent

TABLE 1
Summary Statistics

Table 1 presents summary statistics. Panel A provides descriptive statistics for 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 levels 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 0). Panel B compares payout variables, compensation variables, and other firm characteristics before and after Financial Accounting Standard 123R (FAS 123R). The pre-FAS 123R period covers fiscal years 2003 and 2004; the post-FAS 123R period covers fiscal years 2005–2007. Panel C compares the trends of payout, compensation, and other firm characteristics around FAS 123R across 4 quartile groups ranked by 2002 ACCOUNTING_IMPACT. All variables are defined in Appendix A. *, **, and *** indicate significance at the 10%, 5%, and 1% levels in *t*-tests for equal means, respectively.

Panel A. Firm Characteristics: Full Sample

Variable	*N*	Mean	Std. Dev.	P25	Median	P75
ACCOUNTING_IMPACT (2002)	1,176	1.73%	3.66%	0.22%	0.52%	1.63%
Payout Variables						
DIVIDEND_TO_ASSETS	5,653	0.93%	1.48%	0.00%	0.00%	1.42%
REPURCHASE_TO_ASSETS	5,653	3.94%	7.10%	0.00%	0.35%	5.00%
PAYOUT_TO_ASSETS	5,653	4.88%	7.48%	0.00%	1.75%	6.66%
DIVIDEND_TO_PAYOUT	4,091	38.74%	40.57%	0.00%	22.37%	88.33%
Level of Executive Compensation ($thousands)						
SALARY_$	5,653	450	190	314	409	544
BONUS_$	5,653	476	616	122	295	586
OPTION_GRANT_$	5,653	791	1,137	62	384	1,002
RESTRICTED_STOCK_GRANT_$	5,653	458	866	0	58	518
TOTAL_PAY_$	5,653	2,481	2,475	890	1,647	3,074
Percentage of Executive Compensation						
SALARY_%	5,653	29.6%	18.3%	16.5%	25.1%	38.2%
BONUS_%	5,653	19.7%	14.8%	9.7%	17.7%	26.8%
OPTION_GRANT_%	5,653	28.3%	23.5%	6.7%	25.6%	44.5%
RESTRICTED_STOCK_GRANT_%	5,653	14.1%	18.0%	0.0%	4.1%	24.9%
Control Variables						
FIRM_SIZE ($millions)	5,653	11,086	27,294	1,017	2,459	8,117
ln(FIRM_SIZE)	5,653	7.985	1.565	6.925	7.808	9.002
BOOK_TO_MARKET	5,653	0.445	0.287	0.251	0.388	0.578
LEVERAGE	5,653	0.202	0.172	0.038	0.187	0.309
ASSET_TANGIBILITY	5,653	0.806	0.184	0.691	0.853	0.965
FREE_CASH_FLOWS	5,653	0.059	0.091	0.013	0.059	0.108
CASH_HOLDINGS	5,653	0.166	0.172	0.034	0.100	0.247
ROA_VOLATILITY	5,653	0.056	0.056	0.021	0.038	0.070
INSTITUTIONAL_OWNERSHIP	5,653	0.753	0.202	0.639	0.795	0.910
SALES_GROWTH	5,653	0.121	0.189	0.028	0.096	0.183
PAST_STOCK_RETURN	5,653	0.094	0.184	−0.012	0.094	0.199
LOSS_FIRM	5,653	0.164	0.370	0.000	0.000	0.000

(continued on next page)

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TABLE 1 (continued)
Summary Statistics

Panel B. Firm Characteristics: Before and After FAS 123R

Variable	Pre-FAS 123R	Post-FAS 123R	Difference
Payout Variables			
DIVIDEND_TO_ASSETS	0.83%	0.99%	0.16%***
REPURCHASE_TO_ASSETS	2.45%	4.90%	2.45%***
PAYOUT_TO_ASSETS	3.28%	5.89%	2.61%***
DIVIDEND_TO_PAYOUT	39.14%	27.90%	−11.24%***
Level of Executive Compensation ($thousands)			
SALARY_$	426	463	37***
BONUS_$	418	505	87***
OPTION_GRANT_$	896	707	−191***
RESTRICTED_STOCK_GRANT_$	269	574	305***
TOTAL_PAY_$	2,263	2,584	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			
FIRM_SIZE ($millions)	9,671	11,631	1,960*
ln(FIRM_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_FLOWS	0.061	0.057	−0.004
CASH_HOLDINGS	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_FIRM	0.173	0.159	−0.014

Panel C. Firm Characteristics by Quartile of Accounting Impact: Before and After FAS 123R

	Quartile 1		Quartile 2		Quartile 3		Quartile 4	
ACCOUNTING_IMPACT (2002)	0.12%		0.35%		0.93%		5.51%	
No. of firms	294		294		294		294	
Variable	Pre	Post	Pre	Post	Pre	Post	Pre	Post
Payout Variables								
DIVIDEND_TO_ASSETS	1.12%	1.24%	0.94%	1.11%	0.95%	1.13%	0.30%	0.49%
REPURCHASE_TO_ASSETS	1.13%	2.68%	1.92%	3.73%	3.27%	6.31%	3.51%	6.87%
PAYOUT_TO_ASSETS	2.24%	3.91%	2.85%	4.84%	4.22%	7.44%	3.81%	7.36%
DIVIDEND_TO_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%
Control Variables								
FIRM_SIZE ($millions)	12,812	15,946	8,259	9,707	10,943	12,749	6,470	7,589
ln(FIRM_SIZE)	8.225	8.428	7.993	8.192	7.907	8.088	7.314	7.431
BOOK_TO_MARKET	0.545	0.527	0.460	0.479	0.387	0.408	0.357	0.397
LEVERAGE	0.266	0.255	0.252	0.246	0.169	0.185	0.117	0.125
ASSET_TANGIBILITY	0.843	0.829	0.792	0.766	0.801	0.779	0.853	0.798
FREE_CASH_FLOWS	0.039	0.041	0.054	0.048	0.083	0.070	0.067	0.069
CASH_HOLDINGS	0.090	0.088	0.086	0.076	0.155	0.145	0.369	0.327
ROA_VOLATILITY	0.041	0.039	0.043	0.037	0.060	0.045	0.126	0.072
INSTITUTIONAL_OWNERSHIP	0.688	0.765	0.720	0.785	0.739	0.802	0.701	0.770
SALES_GROWTH	0.125	0.095	0.116	0.100	0.134	0.110	0.173	0.129
PAST_STOCK_RETURN	0.098	0.144	0.094	0.123	0.100	0.099	0.029	0.036
LOSS_FIRM	0.122	0.145	0.122	0.105	0.121	0.120	0.328	0.265

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.

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Panel B of Table 1 presents the mean of each variable in the pre and post periods for the full sample. For each variable, we first compute the firm-level mean over the pre and post periods, 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% vs. 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 up to the 2008 financial crisis (see also Figure 1). Consistent with previous studies, around FAS 123R 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 FAS 123R). 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 C of Table 1 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, mean ACCOUNTING_IMPACT 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 quartiles, 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 vs. 20.1% in the bottom quartile). More important 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 quartiles, respectively. The decline in option usage across quartiles is accompanied by an increase in restricted stock grants (which typically pay dividends; Zhang (2018)), whereas the weight of salary and bonus shows little change.

As for the payout variables, Panel C of Table 1 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 C also indicates that the increase in repurchases documented in Panel B takes place across all quartiles. As for the (much smaller) change in dividends, the absolute increase is similar across the 4 quartiles, though the relative increase (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 C of Table 1 also shows that firms in different ACCOUNTING_IMPACT ranks differ in many other characteristics. For example, compared to firms in the lowest quartile, firms in the top quartile are smaller and have higher market-to-book and sales growth, more losses, lower leverage, and more volatile

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ROA. This is not surprising as these characteristics are associated with higher use of options, which in turn is a key driver of implied option expense. Because these differences could confound our analysis, we control for these characteristics in our main regressions. In addition, as discussed in Section V.A, we use both propensity score matching (PSM) and entropy balancing to identify firms that differ in terms of accounting impact but are comparable in terms of other dimensions. The results from the balanced samples are similar to what we find from the full sample.

E. Identification Assumption

The key identifying assumption underlying the difference-in-differences design 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. That is, in the absence of FAS 123R, the change in dividends or repurchases should 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 the 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 3 years before the treatment. Also, because the accounting rules require the fair value of stock options at the grant date to be recognized over the service period (usually the vesting period), the accounting impact in a given year (implied option expense) includes a fraction of the expense associated with grants made over the prior 4 or 5 years, depending on the vesting schedule. For 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.

Although the parallel trend assumption is not directly testable, similar to other difference-in-differences studies we examine the trends in dividends and repurchases before the event. Graphs A and B of Figure 2 suggest 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)) and provides support for our choice to include in the pre period only 2003 and 2004 (post-dividend tax cut years), as in Bakke et al. (2016). Graphs C and D suggest similar (and perhaps even more parallel) trends in payout variables also when using the treatment and control definition of Bakke et al.

More formally, to examine the correlation between ACCOUNTING_IMPACT and trends in the payout variables during the pre period, we estimate

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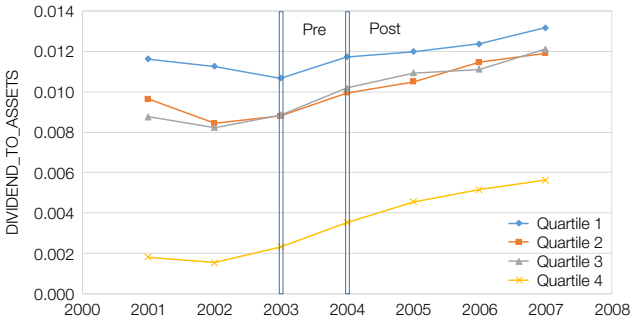
the following regression for 2003 and 2004 only:

$$\text{PAYOUT}_i = \gamma_0 + \gamma_1 \text{ACCOUNTING_IMPACT}_i \times \text{FY2004} + \gamma_2 \text{FY2004} + \Gamma \text{CONTROL}_i + \text{FIRM_FE} + \varepsilon_i, \quad (3)$$

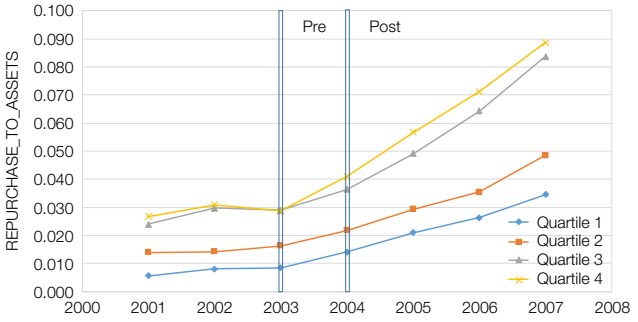
FIGURE 2
Trends of Dividends and Repurchases from 2001 to 2007

Graph A of Figure 2 plots the average DIVIDEND_TO_ASSETS of firms in each quartile of ACCOUNTING_IMPACT measured in 2002. Graph B plots the average total REPURCHASE_TO_ASSETS of firms in each quartile of ACCOUNTING_IMPACT measured in 2002. Graph C shows the average DIVIDEND_TO_ASSETS of firms in the treated and control groups as defined in Bakke et al. (2016). Graph D shows the average REPURCHASE_TO_ASSETS of firms in treated group and control group as defined in Bakke et al. Variables are defined in Appendix A.

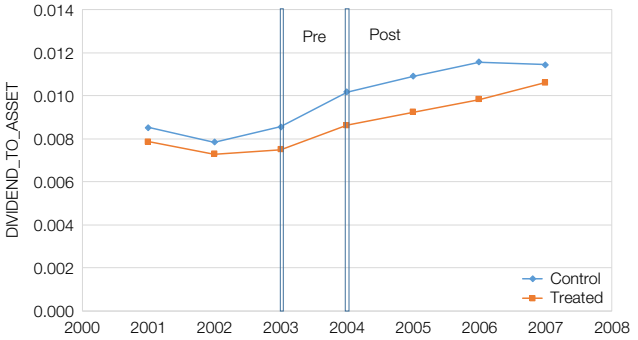
Graph A. Trends of Regular Dividends by ACCOUNTING_IMPACT



Graph B. Trends of Total Repurchases by ACCOUNTING_IMPACT



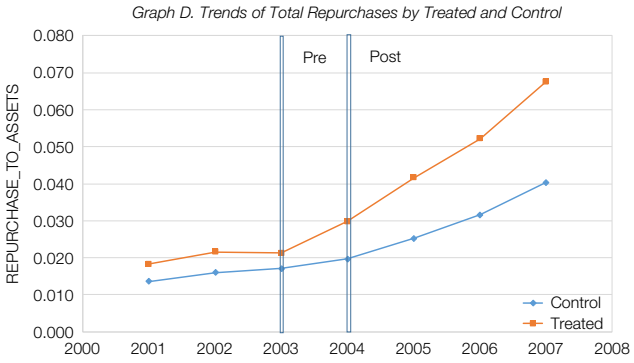
Graph C. Trends of Regular Dividends by Treated and Control



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FIGURE 2 (continued)
Trends of Dividends and Repurchases from 2001 to 2007



where $PAYOUT_i$ is either DIVIDEND_TO_ASSETS or REPURCHASE_TO_ASSETS, FY2004 is an indicator of fiscal year 2004, and FIRM_FE indicates firm fixed effects. If firms with varying degrees of accounting impact have different payout trends before the adoption of FAS 123R, γ_1 should be different from 0. However, for both payout variables, across our proxies for accounting impact, we find that the coefficient of interest is close to 0 and not statistically significant at the 10% level (see Appendix B). Overall, the parallel trends assumption seems reasonable in our setting.

III. Empirical Analyses

A. Differential Impact of FAS 123R on the Use of Option-Based Compensation

A key feature of our approach is the presumed differential impact of FAS 123R on option-based pay as a function of preexisting implied option expense. Panel C of Table 1 preliminarily supports the validity of this assumption. Also, Figure 3 plots the trends of option-based compensation for each ACCOUNTING_IMPACT quartile and shows that, compared with firms in quartile 1 (lowest ACCOUNTING_IMPACT), firms in quartile 4 (highest ACCOUNTING_IMPACT) had a much sharper decrease in option-based pay in 2005 and 2006.[6] Nonetheless, we formally examine the assumption by estimating equation (1).

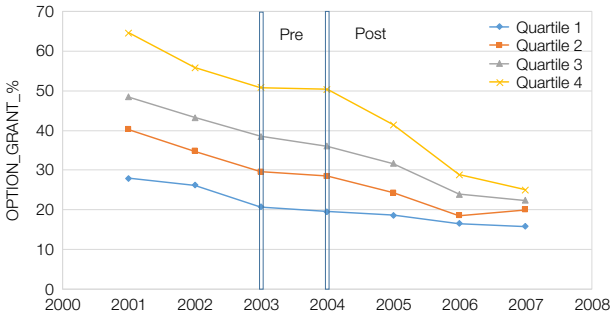
As reported in Panel A of Table 2, we find a significant negative association between ACCOUNTING_IMPACT and the change in option-based pay around the adoption of FAS 123R. In column 1, where we use the quartile rank of implied option expense, the −0.044 coefficient implies that around FAS 123R, option grants over total pay decline by 4.4% when moving from the highest quartile to the next highest. 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 3 indicators

[6]Furthermore, the trends were similar between 2002 and 2004, suggesting that firms in different quartiles would have similar trends of option-based pay after 2004 in the absence of FAS 123R.

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FIGURE 3
Trends of Option-Based Compensation from 2001 to 2007

Figure 3 plots the average ratio of option grant (fair value) to total pay among the top 5 executives of firms in each quartile of ACCOUNTING_IMPACT measured in 2002. Variables are defined in Appendix A.



for the highest quartiles (with the bottom quartile collapsed in the intercept) to compare directly the top and bottom quartiles 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. (2016). The negative and significant coefficient, at −0.132, suggests that treated firms experience a 13.2% larger decline in option pay. In Panel B, we reestimate equation (1) using a panel regression with firm and year fixed effects, where the dependent variable is 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.

Overall, Table 2 suggests that high implied option expense and the introduction of FAS 123R together strongly encouraged firms to cut back option-based compensation for executives.[7]

B. 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).

Panel A of Table 3 presents the results using the change in dividends from the pre to the post period as the dependent variable. We find no significant association between ACCOUNTING_IMPACT (columns 1 and 2) or TREATED (column 3) and the change in dividends around the adoption of FAS 123R. This is in stark contrast to the negative effect of option-based pay on dividends predicted in the

[7]Our test of equation (1) focuses on executives' stock options. However, our dilution channel hypothesis assumes a change in the use of all (executive and nonexecutive employee) stock options. Brown and Lee (2011) report a similar decrease in executive and employee stock options around FAS 123R. Nonetheless, in untabulated analyses, we repeat the test in equation (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 (for this analysis we can use data only 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.

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literature and documented in previous studies. Panel B presents similar results using a panel regression with firm and year fixed effects and interaction terms between ACCOUNTING_IMPACT (or TREATED) and an indicator for the post period.

Perhaps the insignificant effect is because our setting is not powerful enough to detect the effect. One way to examine this possibility is the following. The coefficient on ACCOUNTING_IMPACT_RANK_4 in column 2 of Panel A of Table 3 is 0.001 and the standard error is 0.0009. Assuming the estimated standard error is correct, for a two-tailed t-test, the coefficient needs to be greater than 0.0015

TABLE 2

Differential Effect of FAS 123R on the Use of Option-Based Compensation

Table 2 investigates whether the effect of Financial Accounting Standard 123R (FAS 123R) on the use of option-based pay varies with its potential accounting impact. Panel A reports the results of estimating 3 two-period first-differencing models, where for each firm we average its characteristics in the pre- and post-FAS 123R periods. The pre-FAS 123R period covers fiscal years 2003 and 2004; the post-FAS 123R period covers fiscal years 2005–2007. In all 3 specifications, the dependent variable is the change in average option grant/total pay from the pre- to the post-FAS 123R period. The main independent variable varies by model: 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 equals 1 if the firm's ACCOUNTING_IMPACT_RANK is 2 (3, 4); and in model 3, we use an alternative treatment construction following Bakke et al. (2016), where TREATED is an indicator that equals 0 if the firm granted 0 options to its chief executive officer (CEO) in both 2003 and 2004, or the firm began to voluntarily expense option-based pay at fair value in 2002 or earlier, and 1 otherwise. For each control variable, we include both its levels in the pre-FAS 123R period and its change across the 2 periods. Panel B reports the results of estimating fixed-effects 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 the post-FAS 123R indicator (post) with ACCOUNTING_IMPACT_RANK, ACCOUNTING IMPACT_RANK indicators, and TREATED, respectively. All models in Panel A have industry fixed effects, and the standard errors are Huber–White heteroscedasticity-consistent standard errors. All models in Panel B have firm and year fixed effects, and the standard errors are clustered at the firm level. All variables are defined in Appendix A. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

		Model 1		Model 2		Model 3	
Variable	Prediction	Coeff.	t-Stat.	Coeff.	t-Stat.	Coeff.	t-Stat.
Panel A. Two-Period First-Difference Regression (Dependent Variable: ΔOPTION_GRANT_%)							
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***
ΔIn(FIRM_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_FLOWS		−0.106	−0.95	−0.108	−0.97	−0.128	−1.17
ΔCASH_HOLDINGS		−0.197	−2.11**	−0.193	−2.06**	−0.180	−1.94**
ΔROA_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_STOCK_RETURN		0.051	0.78	0.052	0.80	0.091	1.38
ΔLOSS_FIRM		0.056	2.07**	0.057	2.07**	0.047	1.79*
In(FIRM_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	1.5
ASSET_TANGIBILITY		0.033	0.77	0.033	0.78	0.053	1.28
FREE_CASH_FLOWS		0.227	2.02**	0.220	1.95**	0.127	1.17
CASH_HOLDINGS		−0.002	−0.04	0.012	0.19	−0.078	−1.41
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_STOCK_RETURN		−0.009	−0.15	−0.006	−0.10	0.078	1.25
LOSS_FIRM		0.055	1.74*	0.056	1.78*	0.054	1.73*
Industry fixed effects		Yes		Yes		Yes	
Standard error		Huber–White		Huber–White		Huber–White	
Adj. R^2		0.124		0.123		0.156	
No. of obs.		1,171		1,171		1,171	

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TABLE 2 (continued)
Differential Effect of FAS 123R on the Use of Option-Based Compensation

Variable	Prediction	Model 1 Coeff.	Model 1 *t*-Stat.	Model 2 Coeff.	Model 2 *t*-Stat.	Model 3 Coeff.	Model 3 *t*-Stat.
Panel B. Panel Fixed Effects Regression (Dependent Variable: OPTION_GRANT_%)							
ACCOUNTING_IMPACT_RANK × POST	–	−0.044	−8.07***				
ACCOUNTING_IMPACT_RANK_2 × POST	–			−0.046	−3.33***		
ACCOUNTING_IMPACT_RANK_3 × POST	–			−0.077	−5.26***		
ACCOUNTING_IMPACT_RANK_4 × POST	–			−0.137	−7.76***		
TREATED × POST	–					−0.145	−9.70***
ln(FIRM_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_FLOWS		−0.074	−1.51	−0.072	−1.46	−0.078	−1.58
CASH_HOLDINGS		−0.048	−0.86	−0.050	−0.90	−0.006	−0.11
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_STOCK_RETURN		0.067	2.21**	0.068	2.24**	0.063	2.04**
LOSS_FIRM		0.023	2.09**	0.022	2.07**	0.024	2.22**
Firm fixed effects		Yes		Yes		Yes	
Year fixed effects		Yes		Yes		Yes	
Standard error		Clustered by firm		Clustered by firm		Clustered by firm	
Adj. R^2		0.523		0.523		0.526	
No. of obs.		5,653		5,653		5,653	

(= standard error × 1.645), or 0.15%, to reach the 10% significance level. In other words, if moving from the bottom to the top quartile (which predicts a 14% differential decline in option-based pay, per Panel A of Table 2) resulted in a 0.15% differential increase of DIVIDEND_TO_ASSETS, our test would be able to detect a causal effect of option-based pay on dividends. Because firms in the bottom quartile increased DIVIDEND_TO_ASSETS by 0.12% (from 1.12% to 1.24%; see Panel C of Table 1), a differential increase of 0.15% would require firms in the top quartile to increase their DIVIDEND_TO_ASSETS 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_TO_ASSETS by 0.19% (from 0.30% to 0.49%) and ii) at the same time almost doubled REPURCHASE_TO_ASSETS (from 3.51% to 6.87%), suggesting they had enough cash available to increase dividends without significantly altering their repurchase plans. We do a similar analysis for the coefficient on TREATED in column 3 of Panel A, ACCOUNTING_IMPACT_RANK_4 × POST in column 2 of Panel B, and TREATED × 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, approximately 0.12%–0.13%). Overall, it does not seem that our setting is not powerful enough to detect the hypothesized effect. Besides, it is worth noting that it has been successfully used to detect a causal effect of option-based pay on other firms' outcomes (risk taking, innovation activities) using a similar research design (Bakke et al. (2016), Chu and Ma (2015), and especially Mao and Zhang (2018)).

C. Option-Based Pay and Repurchases

Panel A of Table 4 presents the results of estimating equation (2) using the change in repurchases (scaled by total assets) from the pre to the post period as a dependent variable. If the use of option-based pay had a

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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_IMPACT 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 FAS 123R. When we use TREATED (column 3), we find a significant positive

TABLE 3

Effect of Option-Based Compensation on Dividend Policy

Table 3 investigates the causal effect of option-based pay on dividend policy by examining whether changes in dividend policy around Financial Accounting Standard 123R (FAS 123R) vary with its potential accounting impact. Panel A reports the results of estimating 3 two-period first-differencing models, where for each firm we average its characteristics in the pre- and post-FAS 123R periods. In all 3 specifications, the dependent variable is the change in average regular dividend/total assets from the pre- to the post-FAS 123R period. The main independent variable varies by model: 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 equals 1 if the firm's ACCOUNTING_IMPACT_RANK is 2 (3, 4); and in model 3, we use an alternative treatment construction following Bakke et al. (2016), where TREATED is an indicator that equals 0 if the firm granted 0 options to its chief executive officer (CEO) in both 2003 and 2004, or the firm began to voluntarily expense option-based pay at fair value in 2002 or earlier, and 1 otherwise. For each control variable, we include both its level in the pre-FAS 123R period and its change across the 2 periods. Panel B reports the results of estimating fixed-effects 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 the post-FAS 123R indicator (post) with ACCOUNTING_IMPACT_RANK, ACCOUNTING_IMPACT_RANK indicators, and TREATED, respectively. All models in Panel A have industry fixed effects, and the standard errors are Huber–White heteroscedasticity-consistent standard errors. All models in Panel B have firm and year fixed effects, and the standard errors are clustered at the firm level. All variables are defined in Appendix A. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

		Model 1		Model 2		Model 3	
Variable	Prediction	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.
Panel A. Two-Period First-Difference Regression (Dependent Variable: ΔDIVIDEND_TO_ASSETS)							
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
ΔIn(FIRM_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_FLOWS		0.017	3.73***	0.017	3.73***	0.017	3.75***
ΔCASH_HOLDINGS		−0.011	−2.88***	−0.011	−2.85***	−0.010	−2.82***
ΔROA_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_STOCK_RETURN		0.002	0.77	0.002	0.76	0.002	0.70
ΔLOSS_FIRM		0.000	−0.48	0.000	−0.45	0.000	−0.46
ln(FIRM_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_FLOWS		0.017	2.91***	0.017	2.91***	0.017	3.00***
CASH_HOLDINGS		0.002	0.80	0.002	0.77	0.002	0.98
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_STOCK_RETURN		0.004	1.69*	0.004	1.67*	0.004	1.65*
LOSS_FIRM		−0.001	−1.11	−0.001	−1.09	−0.001	−1.02
Industry fixed effects		Yes		Yes		Yes	
Standard error		Huber–White		Huber–White		Huber–White	
Adj. R^2		0.105		0.104		0.105	
No. of obs.		1,171		1,171		1,171	

(continued on next page)

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TABLE 3 (continued)
Effect of Option-Based Compensation on Dividend Policy

Variable	Prediction	Model 1 Coeff.	Model 1 *t*-Stat.	Model 2 Coeff.	Model 2 *t*-Stat.	Model 3 Coeff.	Model 3 *t*-Stat.
Panel B. Panel Fixed-Effects Regression (Dependent Variable: DIVIDEND_TO_ASSETS)							
ACCOUNTING_IMPACT_RANK × POST	+	0.000	1.22				
ACCOUNTING_IMPACT_RANK_2 × POST	+			0.001	1.10		
ACCOUNTING_IMPACT_RANK_3 × POST	+			0.001	0.94		
ACCOUNTING_IMPACT_RANK_4 × POST	+			0.001	1.29		
TREATED × POST	+					0.000	−0.12
ln(FIRM_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_FLOWS		0.002	0.90	0.002	0.93	0.002	0.92
CASH_HOLDINGS		−0.007	−2.95***	−0.007	−2.98***	−0.007	−2.98***
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_STOCK_RETURN		0.001	0.89	0.001	0.88	0.001	0.78
LOSS_FIRM		0.000	−0.60	0.000	−0.57	0.000	−0.76
Firm fixed effects		Yes		Yes		Yes	
Year fixed effects		Yes		Yes		Yes	
Standard error		Clustered by firm		Clustered by firm		Clustered by firm	
Adj. R^2		0.877		0.877		0.877	
No. of obs.		5,653		5,653		5,653	

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.

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 of Table 4 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.[8] In doing so, we effectively test whether firms experiencing an accounting-induced decrease in option grants engage in fewer repurchases (net of exercise-driven repurchases) because their need to manage the effect of unexercised options on diluted EPS is lower.[9] The coefficients on the variables of interest are 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

[8] As an 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. Because the number of ESO exercised is available in Compustat starting in 2004, Panel C of Table 4 is based on observations from 2004 to 2007.

[9] To test whether firms experiencing an accounting-induced decrease in option grants engage in fewer 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.

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TABLE 4

Effect of Option-Based Compensation on Repurchases

Table 4 investigates the causal effect of option-based pay on payout policy by examining whether the changes in repurchase around Financial Accounting Standard 123R (FAS 123R) vary with its potential accounting impact. Panel A reports the results of estimating 3 two-period first-differencing models, where for each firm we average its characteristics in the pre- and post-FAS 123R periods. In all 3 specifications, the dependent variable is the change in average repurchase/total assets from the pre- to the post-FAS 123R period. The main independent variable varies by model: 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 equals 1 if the firm's ACCOUNTING_IMPACT_RANK is 2 (3, 4); and in model 3, we use an alternative treatment construction following Bakke et al. (2016), where TREATED is an indicator that equals 0 if the firm granted 0 options to its chief executive officer (CEO) in both 2003 and 2004, or the firm began to voluntarily expense option-based pay at fair value in 2002 or earlier, and 1 otherwise. Panel B reports the regressions with the change in 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 in adjusted repurchase/total assets as the dependent variable and the same independent variable as in Panel A. All models in Panels 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 A. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

		Model 1		Model 2		Model 3	
Variable	Prediction	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.
Panel A. Two-Period First-Difference Regression with Total Repurchase (Dependent Variable: ΔREPURCHASE_TO_ASSETS)							
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	
Industry fixed effects		Yes		Yes		Yes	
Standard error		Huber–White		Huber–White		Huber–White	
Adj. R^2		0.238		0.237		0.245	
No. of obs.		1,171		1,171		1,171	
Panel B. Two-Period First-Difference Regression with Net Repurchase (Dependent Variable: ΔNET_REPURCHASE_TO_ASSETS)							
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	
Industry fixed effects		Yes		Yes		Yes	
Standard error		Huber–White		Huber–White		Huber–White	
Adj. R^2		0.229		0.229		0.233	
No. of obs.		1,171		1,171		1,171	
Panel C. Two-Period First-Difference Regression with Repurchase Adjusted for Exercised Options (Dependent Variable: ΔADJUSTED_REPURCHASE_TO_ASSETS)							
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	
Industry fixed effects		Yes		Yes		Yes	
Standard error		Huber–White		Huber–White		Huber–White	
Adj. R^2		0.111		0.112		0.114	
No. of obs.		1,168		1,168		1,168	

Creation Act of 2004 increased repurchases by $146 billion more than nonrepatriating firms over 2005–2007. Repatriating firms constitute 26% of our sample (311 firms). If these firms were mostly concentrated in the top quartile of implied option expense (or in the TREATED group), their inclusion might prevent us from detecting the predicted decline in repurchases for high ACCOUNTING_IMPACT

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firms. 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 TREATED), it may prevent us from detecting the predicted decline in repurchases for high ACCOUNTING_IMPACT firms. However, unreported analyses indicate that both repatriating firms and firms targeted by hedge fund activists are approximately equally distributed across the 4 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 for repatriating firms or an indicator for firms targeted by activists in the pre period (41 firms) and the post period (162 firms).

A final caveat is that most firms replaced stock options with restricted stock (Panel C of Table 2). 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 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 4 quartiles of implied options (Panel C of Table 1).

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 executives state that "offsetting the dilutionary effect of stock option plans or other stock programs" is an important factor in their repurchase decisions.

D. Option-Based Pay and Payout Composition

As an alternative to examining dividends and repurchases separately, we look at payout composition: the change in the DIVIDEND_TO_PAYOUT ratio from the pre to the post period, where 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 the pre and post periods 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_IMPACT or TREATED and DIVIDEND_TO_PAYOUT. However, as reported in Panel A of Table 5, we fail to find a significant association. The results are similar when we use DIVIDEND_TO_NET_PAYOUT (dividends plus net repurchases) instead of DIVIDEND_TO_PAYOUT (Panel B).

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TABLE 5

Effect of Option-Based Compensation on Payout Mix

Table 5 investigates the causal effect of option-based pay on payout mix for firms using both dividend and repurchase by examining whether the changes in payout mix around Financial Accounting Standard 123R (FAS 123R) vary with its potential accounting impact. Panel A reports the results of estimating 3 two-period first-differencing models, where for each firm we average its characteristics in the pre- and post-FAS 123R periods. In all 3 specifications, the dependent variable is the change in average regular dividend/total payout from the pre- to the post-FAS 123R period. The main independent variable varies by model: 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 equals 1 if the firm's ACCOUNTING_IMPACT_RANK is 2 (3, 4), and in model 3, we use an alternative treatment construction following Bakke et al. (2016), where TREATED is an indicator equals 0 if the firm granted 0 options to its chief executive officer (CEO) in both 2003 and 2004, or the firm began to voluntarily expense option-based pay at fair value in 2002 or earlier, and 1 otherwise. Panel B reports the regressions with the change in 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 the pre- and post-FAS 123R periods. All models in Panels A and B have industry fixed effects, and the standard errors are Huber–White heteroscedasticity-consistent standard errors. All variables are defined in Appendix A. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively; the lack of *, **, or *** indicates that the coefficient is insignificant.

		Model 1		Model 2		Model 3	
Variable	Prediction	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.
Panel A. Two-Period First-Difference Regression with Total Payout Mix (Dependent Variable: ΔDIVIDEND_TO_PAYOUT)							
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	
Industry fixed effects		Yes		Yes		Yes	
Standard error		Huber–White		Huber–White		Huber–White	
Adj. R^2		0.154		0.151		0.156	
No. of obs.		362		362		362	
Panel B. Two-Period First-Difference Regression with Net Payout Mix (Dependent Variable: ΔDIVIDEND_TO_NET_PAYOUT)							
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	
Industry fixed effects		Yes		Yes		Yes	
Standard error		Huber–White		Huber–White		Huber–White	
Adj. R^2		0.049		0.043		0.054	
No. of obs.		257		257		257	

Overall, the combined evidence in Tables 3–5 does not support the hypothesis that option-based compensation has a negative (positive) effect on dividends (repurchases) or that it causes firms to replace dividends with repurchases.[10]

IV. Intensity of Dividend Changes, Dividend Initiations, and Special Dividends

The dividend analysis in Table 3 includes both dividend-paying and non-dividend-paying firms (hereafter, payers and nonpayers, respectively). As such,

[10]The results in Tables 3–5 are similar (untabulated) when: i) we exclude fiscal year 2005 to account for the fact that FAS 123R was effective in June 2005 and its effect may not be fully reflected in the 2005 data, ii) we use only firms with data for the entire 2003–2007 period (1,009 firms) rather than firms with at least 1 year of data in the pre period and 1 year in the post period (1,176 firms), and iii) we exclude firms with CEO turnover in the post period, as a new CEO may have a different compensation package and/or introduce a different payout policy.

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it implicitly treats as equal a dividend increase and a dividend initiation. For example, an increase in the dividend-to-asset ratio from 0.5% to 0.6% is treated the same as an increase from 0.0% to 0.1%. However, the latter represents a critical decision given the precommitment value that investors attach to it 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 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. To capture these differences, following Chetty and Saez (2005), we separately examine how FAS 123R affects the intensity of dividend changes (for the subset of payers) and the likelihood of dividend initiations (for the subset of nonpayers).

A. Intensity of Dividend Changes

In the first row of Panel A of Table 6, we repeat the analysis in Panel A of Table 3 for the subsample of 572 payers, defined as firms with positive DPS in 2003 and/or 2004. For parsimony, we report only the coefficients of interest. 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 (Kahle (2002)), in the third, fourth, and fifth rows, we use indicator variables to denote whether the change in DPS surpasses a threshold: 20%, 40%, and 60%, respectively. The proportions of firms that surpass these thresholds are 59%, 44%, and 33%, respectively.[11] Across all of these specifications, we find no association between ACCOUNTING_IMPACT or TREATED 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 B of Table 6 we perform two robustness tests (for parsimony, we show only the case where the dependent variable is an indicator denoting a greater than 40% increase in DPS; the results are similar for the other 4 intensity dependent variables in Panel A).

The first robustness test (reported in rows 1–3 of Table 6) aims to account for actions possibly taken in anticipation of FAS 123R. In particular, in the first row we exclude firms that began to voluntarily expense options in 2003 or 2004 (84 firms, 68 of which are dividend payers), as the FASB was deliberating on the new standard (Aboody, Barth, and Kasznik (2004)). If these firms reduced option grants and, as a result, increased dividends during the pre period, their inclusion may reduce the power of our tests and thus our ability to detect the hypothesized relation. 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

[11] Although in rows 3–5 of Table 6 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 have a large number of observations but short time series. Examples of studies making a similar choice are Puri, Rocholl, and Steffen (2011) and Chu and Ma (2015).

fact, in our sample, 79% of these firms are in the bottom 2 quartiles of implied option expense (untabulated). Nonetheless, we present this test for robustness.

In a similar spirit, in the second row of Table 6 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 (or entirely replaced them with restricted stock, as Microsoft and other firms did) in anticipation of FAS 123R and, as a result, increased dividends during the pre period, their inclusion may reduce our ability to detect the hypothesized relation.

TABLE 6

Dividend Intensity, Dividend Initiations, and Special Dividends

Table 6 reports analyses of the intensity of dividend changes, dividend initiations, and special dividends. Panel A examines the intensity of dividend changes among firms with nonzero dividend per share in 2003 or 2004. In particular, we estimate the same 3 models as in Panel A of Table 3 (with the same independent variables) but for 5 dependent variables: the change in average regular dividend/total assets from the pre- to the post-Financial Accounting Standard 123R (FAS 123R) period, the percentage change in dividend per share (DPS) from the pre- to the post-FAS 123R period, and 3 indicators denoting whether the percentage change in DPS is greater than 20%, 40%, and 60%, respectively. Panel B reports 4 robustness tests of the dividend intensity regression. In each of them, a set of possible confounding observations is removed (see Section IV.A for details). Panel C compares the trends in dividend initiation and termination around FAS 123R across 4 quartile groups ranked by accounting impact. Panel D reports statistics for special dividends across the 4 quartile groups. All models in Panels A and B have industry fixed effects, and the standard errors are Huber–White heteroscedasticity-consistent standard errors. All variables are defined in Appendix A. * indicates significance at the 10% level.

		Model 1		Model 2		Model 3	
Variable	Prediction	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.
Panel A. Intensity of Dividend Changes around FAS 123R							
Dependent Variable: ΔDIVIDEND_TO_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 Variable: 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 Variable: DPS_PCT_CHANGE_20%							
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 Variable: DPS_PCT_CHANGE_40%							
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 Variable: DPS_PCT_CHANGE_60%							
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	
Industry fixed effects		Yes		Yes		Yes	
Standard error		Huber–White		Huber–White		Huber–White	
No. of obs.		572		572		572	

(continued on next page)

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TABLE 6 (continued)
Dividend Intensity, Dividend Initiations, and Special Dividends

Variable	Prediction	Model 1 Coeff.	Model 1 *t*-Stat.	Model 2 Coeff.	Model 2 *t*-Stat.	Model 3 Coeff.	Model 3 *t*-Stat.
Panel B. Robustness Tests for Intensity of Dividend Changes around FAS 123R (Dependent Variable: DPS_PCT_CHANGE_40%)							
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	+			0.006	−0.49		
TREATED	+					0.022	0.35
No. of obs.		504		504		504	
Exclude Firms in the Top Decile of Option Reduction (2002–2004)							
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
No. of obs.		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
No. of obs.		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
No. of obs.		506		506		506	
Control variables		Yes		Yes		Yes	
Industry fixed effects		Yes		Yes		Yes	
Standard error		Huber–White		Huber–White		Huber–White	

Panel C. Dividend Initiation and Termination Rates around FAS 123R

Panel C1. Initiation
Pre-FAS 123R

Quartile	No. of Firms	No. of Firms without Dividend in 2002	No. of Initiations in 2003	No. of Initiations in 2004	No. of Initiations 2003–2004 Cumulative	% of Initiations 2003–2004 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	1,132	651	45	39	84	12.9%

Post-FAS 123R

Quartile	No. of Firms	No. of Firms without Dividend in 2004	No. of Initiations in 2005	No. of Initiations in 2006	No. of Initiations 2005–2006 Cumulative	% of Initiations 2005–2006 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	1,094	554	27	17	44	7.9%

(continued on next page)

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TABLE 6 (continued)
Dividend Intensity, Dividend Initiations, and Special Dividends

Panel C2. Termination
Pre-FAS 123R

Quartile	No. of Firms	No. of Firms with Dividend in 2002	No. of Terminations in 2003	No. of Terminations in 2004	No. of Terminations 2003–2004 Cumulative	% of Terminations 2003–2004 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	1,132	481	9	1	10	2.1%

Post-FAS 123R

Quartile	No. of Firms	No. of Firms with Dividend in 2004	No. of Terminations in 2005	No. of Terminations in 2006	No. of Terminations 2005–2006 Cumulative	% of Terminations 2005–2006 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	1,094	540	5	12	17	3.1%

Panel D. Special Dividends around FAS 123R

ACCOUNTING_IMPACT_RANK	No. of Firm-Years	No. of Firm-Years with Special Dividend	% of Firm-Years with Special Dividend	SPECIAL_DIVIDEND_TO_ASSETS	SPECIAL_DIVIDEND_TO_ASSETS (payers)
Pre-FAS 123R					
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					
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

In the third row of Table 6, we exclude 196 firms (including 54 dividend payers) that accelerated the vesting of options before FAS 123R became effective to avoid recognizing an expense for the unvested portion of these options (Choudhary, Rajgopal, and Venkatachalam (2009), Balsam, Reitenga, and Yin (2008)). Seventy-two percent of these firms are classified in the top 2 quartiles of implied option expense (untabulated) and thus would have been predicted to experience a significant decline in option pay. To the extent that this decline did not materialize because they minimized the effect of FAS 123R by accelerating vesting, the inclusion of these firms may reduce the power of our tests. Across all 3 rows, though, our results and inferences remain unchanged.

The second robustness test deals with potential countervailing effects on dividends as a result of FAS 123R. In particular, we address 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

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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., reclassification 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 of Panel B of Table 6, we exclude 66 dividend payers with similar payout ratios in 2003 and 2004 (defined as an absolute change in payout ratio of less than 1%), a proxy for firms with a target net-income-based payout ratio in the pre period. Again, we fail to find an association between ACCOUNTING_IMPACT or TREATED and changes in dividends among dividend-paying firms.

Another potential countervailing effect of FAS 123R on dividends is that by reducing net income and thus retained earnings, FAS 123R may have pushed some firms closer to violating dividend covenants in debt contracts and thus prevented them from increasing dividends. This would bias against finding an effect of option 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. 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 4 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 (2017)). Firms expecting an adverse impact of FAS 123R on their dividend covenants would likely exercise this option (particularly given the noncash nature of option expense). Nonetheless, to alleviate this concern, we perform three additional analyses (untabulated): i) we repeat the tests in Panel A of Table 6 after excluding firms with a drop in net income between 2004 and 2005 (i.e., firms that perhaps faced some covenant-related constraints to their payout policy as a result of FAS 123R), ii) we explicitly control for net income and retained earnings in the regression (as in Skinner (2008)), and iii) we repeat the tests in Panel A for dividend payers with DIVIDEND_TO_ASSETS above the sample median, as dividend covenants are less frequent among firms paying large dividends (Jung, Lee, and Yang (2016)). Our inferences are unchanged.

B. Dividend Initiations

In this section, we examine whether the accounting-driven change in option pay affected dividend initiations among nonpayers. We cannot employ the difference-in-differences approach because, by definition, the same firm cannot initiate dividends in both periods. However, we can provide suggestive evidence as

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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 C of Table 6 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 nonpayers (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., 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 approximately a 5% decline in initiation rate in each quartile. In the right portion of Panel C, we examine similar data for dividend terminations. Although the results should be interpreted with caution (dividend terminations are rare), firms with higher accounting impact do not experience a greater decline in termination rates. Our inferences in Panel C are similar when we compare treated and control firms following Bakke et al. (2016) (untabulated). Hence, 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.

C. Special Dividends

The previous analyses fail to detect a causal effect of option-based pay on regular dividends. Perhaps treated firms resorted to a greater use of special, one-time dividends. Although 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 in special dividends in the final months of 2010 and 2012, right before an expected increase in individual-level dividend tax rates.

Panel D of Table 6 presents data on frequency and magnitude of special dividends in the pre and post periods by quartile of implied option expense. Although 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 D), the size of special dividends

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increases from the pre to the post period across the board, though it is a bit more pronounced in firms in the top 2 quartiles. When we compare treated and control firms following Bakke et al. (2016) (untabulated), we find a slightly higher increase in the frequency of special dividends for control firms (from 1.4% to 1.7% vs. 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.

V. Additional Analyses

A. PSM and Entropy Balancing

In our tests we use the pretreatment level of implied option expense scaled by total assets (ACCOUNTING_IMPACT) to create a treatment group and a control group. This approach does not entirely solve the endogeneity issue because these 2 groups have different characteristics, even though we control for those characteristics in the regression. For example, as shown in Panel C of Table 1, implied option expense is higher in smaller firms, which tend to use more option-based compensation than larger firms. To address this issue, we repeat our tests using PSM to identify control firms. Because PSM requires a binary treatment/control classification, we first divide sample firms into those in the highest quartile of ACCOUNTING_IMPACT (which we call M-treated) and those in the lower 3 quartiles of ACCOUNTING_IMPACT (which we call M-control). Then, we estimate the propensity score by regressing the top quartile indicator (M_TREATED) on the set of control variables from Table 1 in a probit model. The predicted value for each firm is taken as the propensity score. We conduct one-to-one matching (without replacement) based on the nearest propensity score between M-treated firms and M-control firms. Panel A of Table 7 reports the univariate difference between the treatment sample and the PSM control sample. Then in columns 1 and 2 of Panel B of Table 7 we replicate the analyses, respectively, from Panel A of Table 3 (regular dividends) and Panel A of Table 4 (total repurchases) using the PSM control sample. We continue to find no effect on dividends and repurchases.

A shortcoming of PSM is that the method matches only on propensity score and does not directly aim to balance firm characteristics. As a result, there is relatively poor balance in some characteristics, such as cash holding and past stock returns (see Panel A of Table 7). To address this problem, we use the entropy balancing technique, a quasi-matching approach that reweights each control observation so that postweighting distributional properties of matched variables of treatment and control observations are virtually identical, thereby ensuring covariate balance (Hainmueller (2012)). As a first step, entropy balancing requires selecting the distributional properties of interest (we focus on mean and variance) and the matching variables (we use the control variables in Table 1). Next, the algorithm proceeds by assigning possible weights (above or below 1) to control observations and then testing whether the balance conditions have been met, that is, whether the chosen distributional properties of treatment and postweighted control observations for the matched variables are identical.

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TABLE 7

Effect of Option-Based Compensation on Payout Policy: PSM and Entropy Balancing

Table 7 examines the robustness of the main results using PSM (Panels A and B) and entropy balancing (Panels C and D). We divide the sample into firms in the highest quartile of ACCOUNTING_IMPACT (called M-treated) and firms in the lower 3 quartiles of ACCOUNTING_IMPACT (called M-control). For PSM, we first estimate the propensity score by regressing M-treated on the pretreatment firm characteristics (those reported as control variables in Table 1) for the full sample using a probit regression (untabulated). Then, for each M-treated firm, we conduct one-to-one matching (without replacement) to identify the M-control firm with the nearest propensity score. Panel A reports the covariate balance of the resulting matched sample. Panel B reports the regression results for the matched sample. Column 1 replicates column 1 in Panel A of Table 3, with the M_TREATED indicator (i.e., top quartile of ACCOUNTING_IMPACT) replacing the variable ACCOUNTING_IMPACT_RANK; column 2 replicates column 1 in Panel A of Table 4, with the M_TREATED indicator replacing the variable ACCOUNTING_IMPACT_RANK. For entropy balancing, we first obtain entropy weights for each control observation by targeting the pretreatment balance of the control variables in Table 1 in terms of mean and variance. Panel C reports the covariate balance using entropy weights. The difference in weighted mean is statistically insignificant for all covariates. The entropy weights assigned to each control observation are then used in the regression analysis in Panel D, which replicates the main results for dividends (column 1 in Panel A of Table 3) and repurchases (column 1 in Panel A of Table 4). All models in Panels B and D have industry fixed effects, and the standard errors are Huber–White heteroscedasticity-consistent standard errors. All variables are defined in Appendix A. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A. PSM Covariance Balance

	M-Treated		M-Control (Matched)		Difference
Variable	*N*	Mean	*N*	Mean	Mean
ln(FIRM_SIZE)	292	7.318	292	7.641	−0.323***
BOOK_TO_MARKET	292	0.358	292	0.382	−0.025
LEVERAGE	292	0.115	292	0.147	−0.032**
ASSET_TANGIBILITY	292	0.853	292	0.822	0.031**
FREE_CASH_FLOWS	292	0.067	292	0.079	−0.012
CASH_HOLDINGS	292	0.370	292	0.216	0.153***
ROA_VOLATILITY	292	0.126	292	0.074	0.052***
INSTITUTIONAL_OWNERSHIP	292	0.700	292	0.731	−0.031*
SALES_GROWTH	292	0.173	292	0.144	0.029*
PAST_STOCK_RETURN	292	0.026	292	0.078	−0.051***
LOSS_FIRM	292	0.327	292	0.214	0.113***

Panel B. PSM Regressions

	Dependent Variable			
	ΔDIVIDEND_TO_ASSETS		ΔREPURCHASE_TO_ASSETS	
	1		2	
Variable	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.
M_TREATED	0.001	0.74	0.000	0.06
Δln(FIRM_SIZE)	0.000	−0.11	−0.004	−0.34
ΔBOOK_TO_MARKET	−0.006	−2.52**	−0.062	−3.55***
ΔLEVERAGE	−0.005	−0.91	0.003	0.09
ΔASSET_TANGIBILITY	0.016	3.11**	0.087	2.72***
ΔFREE_CASH_FLOWS	0.013	2.12**	0.240	4.78***
ΔCASH_HOLDINGS	−0.011	−2.23**	−0.079	−2.16**
ΔROA_VOLATILITY	0.001	0.11	−0.064	−0.90
ΔINSTITUTIONAL_OWNERSHIP	−0.001	−0.42	0.038	1.87*
ΔSALES_GROWTH	0.000	0.05	−0.053	−1.97**
ΔPAST_STOCK_RETURN	0.001	0.24	0.007	0.27
ΔLOSS_FIRM	0.000	0.04	−0.012	−1.23
ln(FIRM_SIZE)	0.001	1.83*	0.005	2.20**
BOOK_TO_MARKET	−0.001	−0.91	−0.039	−2.97***
LEVERAGE	−0.001	−0.53	−0.072	−3.76***
ASSET_TANGIBILITY	0.005	1.93**	0.031	1.66
FREE_CASH_FLOWS	0.012	1.75*	0.252	4.80***
CASH_HOLDINGS	0.002	0.72	0.013	0.69
ROA_VOLATILITY	0.002	0.21	0.090	1.48
INSTITUTIONAL_OWNERSHIP	0.000	−0.26	0.038	2.61***
SALES_GROWTH	−0.002	−0.56	−0.040	−1.31
PAST_STOCK_RETURN	0.002	0.73	−0.023	−0.87
LOSS_FIRM	−0.002	−1.89*	0.004	0.37
Industry fixed effects	Yes		Yes	
Standard error	Huber–White		Huber–White	
Adj. R^2	0.120		0.268	
No. of obs.	584		584	

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TABLE 7 (continued)

Effect of Option-Based Compensation on Payout Policy: PSM and Entropy Balancing

Panel C. Entropy Balancing Covariance Balance

	M-Treated			M-Control (Weighted)			Difference	
Variable	*N*	Mean	Variance	*N*	Mean	Variance	Mean	Variance
ln(FIRM_SIZE)	292	7.318	2.370	879	7.319	2.370	−0.001	0.000
BOOK_TO_MARKET	292	0.358	0.054	879	0.358	0.054	0.000	0.000
LEVERAGE	292	0.115	0.026	879	0.115	0.026	0.000	0.000
ASSET_TANGIBILITY	292	0.853	0.020	879	0.853	0.020	0.000	0.000
FREE_CASH_FLOWS	292	0.067	0.015	879	0.067	0.015	0.000	0.000
CASH_HOLDINGS	292	0.370	0.031	879	0.370	0.031	0.000	0.000
ROA_VOLATILITY	292	0.126	0.008	879	0.126	0.008	0.000	0.000
INSTITUTIONAL_OWNERSHIP	292	0.700	0.043	879	0.700	0.043	0.000	0.000
SALES_GROWTH	292	0.173	0.046	879	0.173	0.046	0.000	0.000
PAST_STOCK_RETURN	292	0.026	0.047	879	0.026	0.047	0.000	0.000
LOSS_FIRM	292	0.327	0.160	879	0.327	0.160	0.000	0.000

Panel D. Entropy Balancing Weighted Regressions

	Dependent Variable			
	ΔDIVIDEND_TO_ASSETS		ΔREPURCHASE_TO_ASSETS	
	1		2	
Variable	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.
M_TREATED	0.001	1.48	−0.002	−0.36
Δln(FIRM_SIZE)	0.000	−0.34	−0.004	−0.31
ΔBOOK_TO_MARKET	−0.006	−3.10***	−0.068	−3.80***
ΔLEVERAGE	−0.008	−1.96**	0.036	1.25
ΔASSET_TANGIBILITY	0.013	2.33**	0.089	2.51**
ΔFREE_CASH_FLOWS	0.017	2.85***	0.260	5.16***
ΔCASH_HOLDINGS	−0.012	−2.44**	−0.064	−1.86*
ΔROA_VOLATILITY	0.001	0.17	−0.035	−0.46
ΔINSTITUTIONAL_OWNERSHIP	0.002	0.71	0.041	1.80*
ΔSALES_GROWTH	−0.002	−0.80	−0.049	−1.89*
ΔPAST_STOCK_RETURN	−0.002	−0.52	0.013	0.51
ΔLOSS_FIRM	0.001	0.63	−0.008	−0.87
ln(FIRM_SIZE)	0.001	1.91*	0.003	1.33
BOOK_TO_MARKET	−0.001	−0.64	−0.035	−2.66***
LEVERAGE	−0.002	−0.77	−0.067	−3.23***
ASSET_TANGIBILITY	0.003	1.12	0.029	1.47
FREE_CASH_FLOWS	0.014	2.06**	0.281	5.20***
CASH_HOLDINGS	0.001	0.52	0.027	1.54
ROA_VOLATILITY	0.002	0.25	0.119	1.94**
INSTITUTIONAL_OWNERSHIP	−0.001	−0.35	0.045	2.74***
SALES_GROWTH	−0.003	−1.06	−0.038	−1.26
PAST_STOCK_RETURN	−0.001	−0.31	−0.034	−1.29
LOSS_FIRM	−0.001	−1.46	−0.002	−0.22
Industry fixed effects	Yes		Yes	
Standard error	Huber–White		Huber–White	
Adj. R^2	0.157		0.438	
No. of obs.	1,171		1,171	

The algorithm repeats this process over multiple iterations until a set of weights for control observations are found such that the balance conditions are met. Panel C of Table 7 shows that, indeed, after this process there is no difference in mean and variance between treatment and (reweighted) control firms. The weights assigned to each control observation at the end of this procedure are then used in the regression analysis.

Panel D of Table 7 presents the results of reestimating the regressions in Panel A of Table 3 (DIVIDEND_TO_ASSETS) and Panel A of Table 4 (REPURCHASE_TO_ASSETS) using the entropy weights. Similar to our

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baseline test and PSM test, we find no evidence of a change in dividends or repurchases for treatment firms, providing further support for our inferences.[12]

B. Placebo Test

To further support the validity of our research design, we perform a placebo test by replicating our analysis in a sample period without an accounting-induced exogenous shock to option-based pay. In doing so, one constraint is that Compustat has limited coverage for the variable implied option expense before fiscal year 1999. Hence, we focus on the 2000–2004 period. In particular, we measure accounting impact using the implied option expense in 2000. Then, similar to our baseline design (where the accounting impact is measured 1 year before the window of interest), we define 2001–2002 as the pseudo pre period and 2003–2004 as the pseudo post period. A shown in Table 8, the placebo test indicates that firms with different implied option expense do not have significantly differential trends in payout variables around the pseudo event.

TABLE 8

Placebo Test Using the Period from 2001 to 2004

Table 8 reports the results of a placebo test for the period 2001–2004. We define 2001–2002 as the pseudo "pre period" and 2003–2004 as pseudo "post period." Panel A replicates the main results with dividends (column 1 in Panel A of Table 3) and repurchases (column 1 in Panel A of Table 4) with the pseudo event date. Panel B replicates the dividend intensity results (column 1 in Panel A of Table 6) with the pseudo event date. All models have industry fixed effects, and the standard errors are Huber–White heteroscedasticity-consistent standard errors. All variables are defined in Appendix A. * indicates significance at the 10% level.

Panel A. Dividend and Repurchases over Assets

	Dependent Variable			
	ΔDIVIDEND_TO_ASSETS		ΔREPURCHASE_TO_ASSETS	
	1		2	
Variable	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.
ACCOUNTING_IMPACT_RANK	0.000	−0.22	0.000	0.06
Control variables	Yes		Yes	
Industry fixed effects	Yes		Yes	
Standard error	Huber–White		Huber–White	
Adj. R^2	0.066		0.065	
No. of obs.	1,174		1,174	

Panel B. Dividend Intensity

	Dependent Variable									
	ΔDIVIDEND_TO_ASSETS		DPS_PCT_CHANGE		DPS_PCT_CHANGE_20%		DPS_PCT_CHANGE_40%		DPS_PCT_CHANGE_60%	
	1		2		3		4		5	
Variable	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.
ACCOUNTING_IMPACT_RANK	0.000	−0.27	0.021	0.20	0.055	1.92*	0.035	1.34	0.010	0.45
Control variables	Yes		Yes		Yes		Yes		Yes	
Industry fixed effects	Yes		Yes		Yes		Yes		Yes	
Standard error	Huber–White		Huber–White		Huber–White		Huber–White		Huber–White	
Adj. R^2	0.183		0.334		0.187		0.167		0.191	
No. of obs.	504		504		504		504		504	

[12] In untabulated tests, we also examine the intensity of dividend changes using, alternatively, PSM and entropy balancing. The results are qualitatively similar to the baselines results in Panel C of Table 3.

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VI. Conclusion

The literature has documented robust associations between option-based compensation and payout policy, yet it remains unclear whether this association reflects a causal influence. We investigate the causal effect of option-based compensation on payout policy using the adoption of FAS 123R, 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 before FAS 123R. 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, across a number of tests 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 effect on payout policy. An important caveat is that we study S&P 1500 firms and thus cannot generalize our results to other firms. However, this is the same sample used in studies documenting the association between option-based pay and payout policy. Our findings contribute to the literature on the determinants of payout policy as well as to the research on the economic effects of option-based compensation.

Appendix A. 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 in fiscal year 2002 and is time invariant.

ACCOUNTING_IMPACT_RANK: Quartile rank of ACCOUNTING_IMPACT (1 = lowest, 4 = highest).

ACCOUNTING_IMPACT_RANK_X: A dummy variable that equals 1 if ACCOUNTING_IMPACT_RANK is X, and 0 otherwise.

TREATED: An indicator that equals 0 if the firm granted 0 options 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 1 otherwise.

Payout Variables

DIVIDEND_TO_ASSETS: The numerator is total regular cash dividends in the fiscal year from the distribution data set in the Center for Research in Security Prices (CRSP). Regular cash dividends are identified by the distribution codes 1212, 1222, 1232, or 1242. The denominator is 1-year lagged total assets (Compustat: AT).

SPECIAL_DIVIDEND_TO_ASSETS: The numerator is total special cash dividends in the fiscal year from the distribution data set in CRSP. Special dividends are identified by the distribution codes 1262 or 1272. The denominator is 1-year lagged total assets (Compustat: AT).

DPS: Dividend per share (Compustat: DVPSX_F).

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DPS_PCT_CHANGE: Percentage change in average DPS from the pre- to the post-FAS 123R period.

DPS_PCT_CHANGE_X%: Dummy variable that equals 1 if DPS_PCT_CHANGE is greater than X%, and 0 otherwise.

REPURCHASE_TO_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 1-year lagged total assets (Compustat: AT).

NET_REPURCHASE_TO_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 0 in the current and prior years), net repurchase equals total repurchases minus stock issuance (Compustat: SSTK) and change in preferred stock redemption value (Compustat: PSTKRV). When the above measures have a negative value, net repurchase is set to 0.

ADJUSTED_REPURCHASE_TO_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 measure has a negative value, adjusted repurchase is set to 0. Because the number of options exercised is not available before 2004, we drop observations in 2003 when we use this variable.

PAYOUT_TO_ASSETS: Sum of DIVIDEND_TO_ASSETS and REPURCHASE_TO_ASSETS.

DIVIDEND_TO_PAYOUT: Regular cash dividend divided by the sum of regular cash dividend and repurchase from common equity. In the 2-period analysis (pre- vs. post-FAS 123R), we separately sum the numerator and the denominator for each firm-period and then calculate the ratio.

DIVIDEND_TO_NET_PAYOUT: Regular dividend divided by the sum of regular dividend and net repurchase. In the 2-period analysis (pre- vs. post-FAS 123R), we separately sum the numerator and the denominator for each firm-period and then calculate the ratio.

REPURCHASE_TO_PAYOUT: Repurchase of common equity divided by the sum of regular cash dividend and repurchase from common equity. In the 2-period analysis (pre- vs. post-FAS 123R), 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 $thousand.

SALARY_%: 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 $thousand. Because of a data format change, we add nonequity 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

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$thousand. Because of 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 $thousand. Because of a 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 $thousand.

Other Characteristics

FIRM_SIZE: Book value of total assets (Compustat: AT) minus book value of common equity (Compustat: CEQ) plus market value of common equity from CRSP in $million.

ln(FIRM_SIZE): Natural logarithm of size.

BOOK_TO_MARKET: 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_FLOWS: Operating cash flow (Compustat: OANCF) minus capital expenditures (Compustat: CAPX) divided by lagged total assets.

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

ROA_VOLATILITY: Standard deviation of returns on assets (operating income before tax divided by lagged total assets) in the past 5 fiscal years.

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

SALES_GROWTH: Growth rate of sales (Compustat: SALE) from the prior year.

PAST_STOCK_RETURN: Annual stock return from the prior year (computed from CRSP).

LOSS_FIRM: Indicator variable that equals 1 if the current net income is negative, and 0 otherwise.

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Appendix B. Pre-Trend Regressions

TABLE B1

Placebo Test Using the Period from 2001 to 2004

Table B1 investigates whether ACCOUNTING_IMPACT_RANK or TREATED 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 3 fixed-effects models. In all 3 specifications, the dependent variable is regular dividend/total assets. The main independent variables are i) the interactions of fiscal year 2004 indicator (FY04) with ACCOUNTING_IMPACT_RANK, ii) the interactions of FY2004 with ACCOUNTING_IMPACT_RANK_2 (3, 4), and iii) the interactions of FY04 with 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 equals 1 if the firm's ACCOUNTING_IMPACT_RANK is 2 (3, 4). In model 3, we use an alternative treatment construction following Bakke et al. (2016), where TREATED is an indicator that equals 0 if the firm granted 0 options 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 1 otherwise. In Panel B, we estimate the same 3 models using total repurchase/total assets as the dependent variable. All models in Panels A and B have firm fixed effects and year fixed effects, and the standard errors are clustered at the firm level. All variables are defined in Appendix A. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively; the lack of *, **, or *** indicates that coefficient is insignificant.

	Model 1		Model 2		Model 3	
Variable	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.	Coeff.	*t*-Stat.
Panel A. Dividend Pre-Trends (Dependent Variable: DIVIDEND_TO_ASSETS)						
ACCOUNTING_IMPACT_RANK × FY04	0.000	0.06				
ACCOUNTING_IMPACT_RANK_2 × FY04			0.000	−0.01		
ACCOUNTING_IMPACT_RANK_3 × FY04			0.000	0.20		
ACCOUNTING_IMPACT_RANK_4 × FY04			0.000	−0.03		
TREATED × FY04					0.000	−0.64
Control variables	Yes		Yes		Yes	
Firm fixed effects	Yes		Yes		Yes	
Year fixed effects	Yes		Yes		Yes	
Standard error	Clustered by firm		Clustered by firm		Clustered by firm	
Adj. R^2	0.931		0.931		0.931	
No. of obs.	2,344		2,344		2,344	
Panel B. Repurchase Pre-Trends (Dependent Variable: REPURCHASE_TO_ASSETS)						
ACCOUNTING_IMPACT_RANK × FY04	0.002	0.89				
ACCOUNTING_IMPACT_RANK_2 × FY04			−0.001	−0.22		
ACCOUNTING_IMPACT_RANK_3 × FY04			0.002	0.42		
ACCOUNTING_IMPACT_RANK_4 × FY04			0.005	0.78		
TREATED × FY04					0.006	1.22
Control variables	Yes		Yes		Yes	
Firm fixed effects	Yes		Yes		Yes	
Year fixed effects	Yes		Yes		Yes	
Standard error	Clustered by firm		Clustered by firm		Clustered by firm	
Adj. R^2	0.537		0.536		0.537	
No. of obs.	2,344		2,344		2,344	

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