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Suspect CEOs, unethical culture, and corporate misbehavior ☆

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ABSTRACT

We show that firms with Chief Executive Officers (CEOs) who personally benefit from options backdating are more likely to engage in other corporate misbehaviors, suggestive of an unethical corporate culture. These firms are more likely to commit financial fraud to overstate earnings. They acquire more private companies, which could perpetuate their frauds, and their acquisitions are met with lower market responses. These misbehaviors are concentrated in firms with externally hired suspect CEOs, consistent with outside CEOs having greater discretion to shape firm culture. The costs of these misbehaviors are reflected in larger stock price declines during a market correction and increased CEO replacement.

"Finally, what we have learned from stock options backdating — and from every other scandal in the financial markets in recent years — is that character matters. Corporate character matters — and employees take their cues from the top. In our experience, the character of the CEO and other top officers is generally reflected in the character of the entire company. If a CEO is known for his integrity, integrity becomes the corporate norm. If, on the other hand, a company's top executives are more interested in personal enrichment at the expense of the shareholders, our backdating investigations demonstrate yet again that other employees will follow suit."

–Linda Chatman Thomsen, Director, Division of Enforcement, Securities and Exchange Commission

1. Introduction

Scandals at firms such as Enron, WorldCom, Tyco, and HealthSouth exposed numerous corporate executives who were complicit in perpetuating fraudulent activities that ultimately resulted in billions of dollars in shareholder losses. As a result, the topic of business ethics is receiving a dramatic increase in attention from the U.S. legislature, regulatory bodies such as the Securities and Exchange Commission (SEC), the popular press, and business schools around the world. Of particular importance in the current dialog is an understanding of (and potential means to

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mitigate) the forces that drive firms to mislead investors and cause the misallocation and destruction of scarce societal resources.

Anecdotal evidence suggests that fraudulent firms are often characterized by an unethical culture that permeates a nexus of employees, whose cooperation is necessary to perpetrate extensive corporate malfeasance (Langevoort, 2006). For instance, approximately 30 employees at HealthSouth and Peregrine Systems were convicted or pled guilty to charges related to financial statement fraud. But where does an unethical culture originate? The above quotation by Linda Thomsen, a former head of the Division of Enforcement at the SEC, represents a seasoned insiders' view that an unethical culture emanates from the actions and attitudes of those at the very top level of corporate leadership, in particular, the CEO. Her top-down perspective is echoed in the influential academic "upper echelons theory" of corporate behavior (Chatterjee and Hambrick, 2007; Hambrick, 2007). While numerous prior studies provide support for the upper echelons theory by establishing a relationship between certain executive characteristics and the economic outcomes of the firms that they manage (e.g., Bertrand and Schoar, 2003),[1] there is a clear deficiency of empirical work focused on the ethical dimension of a corporate culture.

The dearth of empirical work in this area could stem from the fact that the ethical values of corporate executives are difficult to empirically quantify. In this paper, we attempt to test whether executives with questionable ethics lead their firms to engage in broader corporate malfeasance. We propose a novel way to identify an unethical pattern of behavior, based on systematic participation in options backdating. In Section 2 we review employee stock option backdating practices at many U.S. firms in the 1990s and early 2000s. We submit that many of these cases are reasonably characterized as stealth activities undertaken by executives for their own personal gain and at an economic cost to other parties. However, we also recognize that there are disparate views regarding the ethics of backdating in general, and acknowledge that specific actions in some individual cases were not obviously inappropriate. On balance, we offer that it is reasonable to test our hypotheses using systematic participation in options backdating to identify executives with questionable ethics; although we concede that one could interpret our results with caution if they disagree with this characterization.

In our first set of analyses, we find that firms with CEOs who benefit directly from options backdating (hereafter 'suspect' firms) are also more likely to engage in fraud. Additional analyses provide evidence that suspect firms overstate their profitability and engage in suboptimal investment strategies. Furthermore, these misbehaviors increase following a suspect CEOs arrival and are concentrated in firms that hire their suspect CEOs from the outside. Finally, we explore some of the consequences of these behavior patterns and find that suspect CEOs are more likely to be fired and their firms are more likely to experience large losses during a market correction.

Overall, our results are consistent with the upper echelons theory in understanding how an unethical culture among corporate executives prevails and contribute to a broader literature on organizational culture (Kreps, 1990; Hodgson, 1996). Our findings support claims in the managerial discretion literature (Hambrick and Finkelstein, 1987) that outside CEOs enjoy greater decision-making discretion, and are also related to a literature that investigates the economic consequences of corporate fraud (e.g., Karpoff and Lott, 1993; Alexander, 1999; Murphy, Shrieves, and Tibbs, 2009). For example, Bernile and Jarrell (2009) show that the negative market reaction associated with firms implicated in backdating is much larger than the direct costs of the backdating activity. Our results provide support for their proposition that this response likely reflects the market's expectations about other suspect activities also present at backdating firms.

We use a data-driven approach to identify systematic options backdating. To be classified as a systematic backdater, at least 30% of an individuals' options activity (grants and/or exercises) must be characterized as "likely backdated." Using data from 1992 to 2009, we identify 249 backdating CEOs and augment this list with 12 additional CEOs who did not meet our identification criteria, but who are specifically named in an enforcement action or backdating settlement. We match our sample of suspect firms to a corresponding sample of non-suspect firms based on industry and firm size to control for other determinants of corporate malfeasance in our empirical analyses.

Univariate and multivariate analyses indicate a strong association between suspect firms and other forms of corporate misbehavior. We find that 8.82% of our suspect firms are implicated in a financial fraud compared to 2.94% for control firms. Given that instances of fraud are likely to engender class-action lawsuits by shareholders, it is not surprising that we also find the incidence of lawsuits is significantly greater for our sample of suspect firms when compared to their control group (26.3% versus 13.9%). Tests of earnings manipulation provide corroborating evidence of misbehavior at suspect firms. Firms with backdating CEOs are 14.55% more likely than control firms to narrowly meet or beat analysts' quarterly earnings forecasts, a tendency previous researchers point to as evidence of accounting manipulations aimed at bolstering stock prices (Hayn, 1995; Degeorge et al., 1999). Consistent with this interpretation, we find that suspect firms use significantly more positive discretionary accruals in the quarters when they narrowly attain these thresholds. These results hold in a multivariate setting that controls for firm characteristics, firm governance, prior financial performance, auditor identity, and the ownership and option compensation of the CEO.

We extend our empirical analyses by investigating the investment activities of suspect firms. Suspect firms make significantly more acquisitions and their acquisition announcements are met with a lower market response. Excessive acquisition activity could be motivated by either selfish empire building (Jensen, 1986; Lang, Stulz, and Walkling, 1991; Morck, Shleifer, and Vishny, 1990) or to

[1] Prior academic research focuses on executive characteristics such as overconfidence (Malmendier and Tate, 2008), political affiliation (Hutton, Jiang, and Kumar, forthcoming), gender (Huang and Kisgen, 2013), narcissism (Chatterjee and Hambrick, 2007), personal risk taking (Cain and McKeon, forthcoming), and personal tax aggressiveness (Chyz, 2013).

facilitate earnings management.[2] Consistent with the latter interpretation, results are stronger when considering the acquisition of private targets, whose opaque assets could be easier to manipulate in order to manage earnings. A number of the questionable corporate actions that we show are concentrated in firms that hire their suspect CEOs from the outside, which is consistent with externally hired CEOs being able to shape the culture of top decision makers along an ethical dimension.

The results discussed thus far establish a correlation between CEOs that engage in options backdating and other suspect corporate actions. However, correlations do not necessarily indicate a causal link. Even if one agrees that our identification metric accurately classifies unethical CEOs, it is certainly possible that firms with an existing unethical culture are more likely to attract executives with an unethical character. To help disentangle the causal relation, we contrast the actions of suspect firms to those of control firms around the arrival of new CEOs. Using difference-in-differences tests we demonstrate that there are significant increases in earnings management and acquisition activity after suspect CEOs arrive at their new firms. We also continue to find that these results are concentrated in firms whose suspect CEOs are outside hires.

As mentioned previously, although it seems reasonable to classify options backdating as unethical in many cases, it is also conceivable that some firms engage in the practice for economically justifiable reasons that are consistent with shareholders' interests. For instance, it is possible that cash-constrained firms modified compensation packages by backdating options in order to attract and retain employees in an increasingly competitive labor market. If these more innocent motivations prevail in our sample of suspect firms, it is possible that the relationships between backdating and other corporate misbehaviors are jointly driven by an omitted or unobservable variable (e.g., weak internal controls).

To address such concerns, we offer contrasting analyses that explore whether similar relationships hold when firms appear to backdate options only for lower-level employees. Our tests are motivated by the observation that if firms backdate options on behalf of top executives, there is an additional unscrupulous dimension to the practice. Specifically, it is misleading to investors regarding the amount and structure of compensation paid to the most important "named" executives, for whom extensive compensation disclosures are required in SEC filings. To allow for a more benign form of option backdating, we conduct all of our main tests on a sample of 172 firms in which backdating was apparent, but only for non-C-suite executives. Interestingly, we do not find any association between this variant of backdating and other corporate misbehaviors, which makes it less likely that the relations between CEO backdating and corporate misbehaviors are driven by an omitted factor.

[2] Schilit and Perler (2010) describes how the accounting rules for dealing with acquisitions allow firms to manage earnings upward by booking revenue from sales of the target's inventory without having to account for the expenses associated with producing the goods.

To conclude our empirical analyses, we consider whether suspect CEOs and/or their firms experience adverse consequences as a result of their actions. Our tests indicate that firms with suspect CEOs hired from the outside are not treated differently by the market during the run-up of the late 1990s and early 2000s. However, during the ensuing market correction, suspect firms are 24.5% more likely to experience severe stock price declines of at least −40%. In addition, outside-hire suspect CEOs are significantly more likely to be fired during the post-bubble period.

To illustrate anecdotally the patterns of behavior we uncover, it is instructive to consider the case of HealthSouth and its former CEO, Richard Scrushy. The data strongly suggest Scrushy was involved in backdating: of the ten option grants to Scrushy before the Sarbanes–Oxley (SOX) Act was implemented, improbably six of them occur on the most favorable day of the month. HealthSouth's story is now familiar; the company grew rapidly throughout the 1990s fueled, in part, by a spree of acquisitions. During this time, HealthSouth displayed a remarkable ability to meet analysts' earnings expectations as highlighted in the company's 2001 annual report to shareholders, where Scrushy stated, "we…celebrated another year of fulfilling Wall Street expectations, maintaining our record as the second-longest streak for meeting or exceeding analysts' expectations." However, in 2002, it became apparent that the success of HealthSouth was largely fictional and built upon one of the largest financial statement frauds in history. In a surprisingly candid book, HealthSouth's original Chief Financial Officer (CFO), Aaron Beam, paints a vivid picture of a dominant and influential leader (Scrushy) whose unethical character permeated others within the organization as well as the corporate actions of HealthSouth. The details that emerged in the aftermath of HealthSouth's fraud suggest Scrushy broke rules to increase his direct compensation at the expense of shareholders, broke reporting rules on behalf of the company to mislead investors, and used corporate resources to further his own interests. HealthSouth's earnings were falsely inflated by a total of $1.4B over the period 1997–2002. On a single day in March 2003 when the SEC charges against HealthSouth were revealed, the stock went from $20 to $0.45 per share, and, needless to say, Scrushy was relieved of his duties as CEO.

The remainder of the paper proceeds as follows. The next section discusses our identification and characterization of CEO backdating. Section 3 discusses the data and our sample. Section 4 investigates the association between suspect firms and other corporate misbehaviors and examines potential consequences for suspect firms. Finally, Section 5 concludes.

2. Identification

We consider firms to be suspect if they systematically engage in options backdating on behalf of their CEO. In Section 2.1 we review the mechanics and characteristics of options backdating. We examine the economic damages of backdating in Section 2.2 and consider an ethical characterization of backdating in Section 2.3. Section 2.4 examines potential alternate characterizations of backdating practices, and Section 2.5 explores expected differences between inside- and outside-hire suspect CEOs.

2.1. Discussion of options backdating

Stock options have rapidly become one of the most important components of executives' pay packages, often accounting for more than half of a CEO's total compensation (Murphy, 2003; Walker, 2007). In theory, boards of directors grant options to executives at "arm's length" to secure a compensation structure that is both consistent with shareholder-approved compensation plans and best serves shareholders' interests (Fried, 2008). The value of executive stock options depends critically on the options' exercise (or strike) price and the vast majority of executive options are reported to be issued at-the-money (Bebchuk and Fried, 2004).

A series of articles published by the *Wall Street Journal* in 2006 illuminated a practice whereby firms were secretly backdating option grants to effectively lower the strike price of executive options.[3] The reporting flexibility afforded to firms prior to SOX gave them up to 45 days after their fiscal year-end to report option grants, thus providing firms (and their executives) with ample opportunity to select grant dates with low prices on an ex post basis. Academic papers provide evidence that many firms used the practice of backdating option grants to increase executives' compensation (Heron and Lie, 2009; Narayanan and Seyhun, 2008).

Several more recent studies uncover a similar pattern around executive stock option exercises (Dhaliwal, Erickson, and Heitzman, 2009; Cicero, 2009). With exercises, executives' private incentives depend on when they dispose of the underlying shares. When executives exercise options and hold the shares, they have a personal tax incentive to do so when prices are low. Alternatively, executives who immediately sell the shares have a straightforward incentive to exercise their options when prices are high. Both Dhaliwal, Erickson, and Heitzman (2009) and Cicero (2009) find evidence consistent with option exercise backdating both when executives exercise their options and hold the underlying shares and when they exercise their options and return shares back to their companies.[4]

The public revelation of options backdating raised numerous questions regarding which parties were complicit in this practice. If all corporate laws and appropriate protocols are followed, backdating should require the knowledge and/or coordinated efforts of shareholders, the board of directors, and numerous executives. Throughout this period, companies were required under state and federal law as well as exchange listing standards to have the terms of option plans approved by shareholders. If companies followed the letter of the law, then any observed options backdating should have served shareholders' interests.

However, our review of several SEC litigation releases suggests a different reality. A common view expressed by the SEC is that the option granting process was often co-opted by top executives and their subordinates (CFOs, General Counsels, Directors of Human Resources, etc.), and that they hid backdating practices from other directors and shareholders. One example in which SEC lawyers clearly drew this conclusion is in the action against Take-Two Interactive Software, where they argue that the CEO "controlled and dominated the process," and the board "abdicated its option granting responsibilities." The mechanism often claimed to facilitate these stealth maneuvers by executives was the use of "unanimous written consent" orders, which were at times presented to directors for their approval after option grant dates had already been backdated. These actions were apparently carried out in violation of shareholder-adopted stock option plans that explicitly prohibited the granting of in-the-money options (e.g., the case of Mercury Interactive).[5] To the extent that these fact patterns are representative, it appears that influential executives were often the instigators for backdating options and that they did not necessarily have board or shareholder approval.

2.2. The economics of option backdating

Stock option grant backdating allows firms to give executives valuable in-the-money options while claiming publicly to provide them with less valuable at-the-money options. From the perspective of executives and the firm, this type of backdating allows firms to (1) report a lower dollar value for executives' compensation in required filings (Regulation S-K) and in the firm's annual report, and (2) obscure the performance-insensitivity of executive pay (Fried, 2008). In addition, firms did not have to recognize a compensation expense for at-the-money options prior to 2005, whereas in-the-money grants trigger an accounting expense against reported income.

From shareholders' perspective there are also important economic implications for both option grant and exercise backdating. For option grants, the magnitude of stealth executive compensation (the difference between the true value of backdated options that were actually granted "in-the-money" and the reported "at-the-money" fair value of options disclosed) represents a direct expense to shareholders. We estimate the economic magnitude of this expense to shareholders for each of the 483 backdated option grants in our sample and find that, on average, CEOs received $642,000 of stealth compensation for each backdated option grant.[6] The aggregate value of shareholder loss

[3] Credit for suggesting the widespread backdating of stock option grants is normally attributed to Lie (2005), and the *Wall Street Journal* was apparently motivated to begin their investigation of backdating by this paper and Heron and Lie (2007).

[4] Prior to the enactment of SOX, executives had up to ten days after the month of option exercise to report the event. Similar to the reporting change in option grants, this reporting requirement was changed to two business days following the exercise in the post-SOX period.

[5] This discussion is generalized based on facts alleged in SEC complaints and litigation releases available on their website against companies including Black Box Corp, Brocade, Comverse Technology, Inc., Engineered Support Systems, The Hain Celestial Group, KB Homes, Maxim Integrated Products, Mercury Interactive, Quest Software, Take-Two Interactive Software, Trident Microsystems, and Ulticom, Inc. http://www.sec.gov/spotlight/optionsbackdating.htm.

[6] To calculate the Black-Scholes "true" value of backdated option grants that were actually issued in-the-money, we use volatility, risk-free rate, and dividend yield inputs as specified by Execucomp and the options' strike price as reported by the company. Because these stock options were backdated to the lowest stock price of the month, the "true" stock price as of the measurement date is incorrectly reported by the company. To estimate the "true" stock price at the time of the grant, we

due to option grant backdating of suspect CEOs in our sample is $310 million and represents 24.1% of their reported option compensation.

Option exercise backdating also benefits executives at an expense to shareholders. Executives that backdate option exercises to a high price and deliver shares back to their companies in effect sell their shares to the company at inflated prices. In cases where an executive backdates an option exercise to a low price and holds the underlying shares, any reduction in the executives' tax liability is likely to increase the firm's tax liability.[7] We do not directly estimate the economic consequences from option exercise backdating here, but instead rely on estimates from existing literature. Cicero (2009) and Dhaliwal et al. (2009) estimate that the cost to shareholders for backdated exercises in which the executive held the underlying shares is between $85,326 and $95,361 per option exercise event. Cicero (2009) also suggests that the shareholder cost of option exercise backdating in which shares were sold back to the firm is $19,265 per event. Thus, both options grant and exercise backdating allow executives to increase their wealth at a significant expense to shareholders.

2.3. The ethics of options backdating

For our research design to be valid, there must be sufficient evidence that backdating options for top executives is reasonably characterized as inappropriate behavior. Our review of court documents suggests that at least some option grant backdating violated both the spirit and letter of accounting and disclosure rules. One important element of the ethical characterization of backdating is that firms did not appear to disclose to investors and legal authorities that they were using this practice as a means of enhancing compensation.[8] To the extent that grant backdating was not properly disclosed and expensed (and there is no evidence that we know of that it ever was), it violates antifraud rules, securities laws, tax laws, and constitutes false statements to the SEC.[9] In a similar manner, option exercise backdating involves misrepresentations in a firm's SEC filings and is likely to violate securities laws, tax laws, and corporate laws (Fried, 2008).

Backdating, as it was commonly practiced, exposes shareholders to additional risks from possible litigation, and loss of reputation and executives. In our view, backdating is reasonably characterized as being at odds with the definition of integrity recently introduced by Erhard, Jensen, and Zaffron (2009). Erhard et al. (2009) assert that integrity is a factor of production for individuals, groups, and societies, and that integrity can be defined as "honoring your word." To summarize very briefly, one's "word" is what one indicates they will or will not do, and to "honor" that word is to follow through on the expectations that it creates. Because backdating was not disclosed and often violated shareholder-approved options plans,[10] it could fall short of a standard that requires firms and executives to honor their word to shareholders and legal authorities.

An informed view of the appropriateness of options backdating should also take into account the extent to which the practice is consistent with the regulation of executive compensation. Firms are required under Regulation S-K to report detailed information about executive compensation on an annual basis, and recently, firms have been required to hold precatory shareholder votes to approve their executive compensation practices. Backdating options for top executive officers distorts both the structure and level of executive compensation by making executives' pay packages appear less valuable and more performance sensitive than they actually are. Options exercise backdating similarly causes the eventual payouts to exceed their ex ante expected value and also reduces uncertainty in the payout for the executive since they can choose exercise dates over a range of previous realized stock prices. The ability to mislead investors about these important dimensions of corporate strategy, and the fact that these effects are to the clear benefit of top executives (who are often under fire for their outsized pay packages and lack of pay-for-performance requirements), weighs against a favorable view of backdating at the C-suite level.

On the other hand, the practice of backdating options for lower-level employees does not carry this additional concern. The ethical implications for both top executives and others in the firm could be less acute in these cases. For example, it is possible that backdating to lower-level employees did not require CEO direction or involvement. Even if the CEO was involved, it is perhaps revealing that a backdating scheme was not implemented in a way that would mislead investors about the parameters of named executive compensation, even though top executives would gain from doing so. Recognizing this distinction, we divide the overall sample of backdating firms according to the locus of backdating activity. We expect that backdating on behalf of the CEO indicates unethical tendencies at the top and, under the upper echelons theory, predicts an association with other corporate misbehaviors. For comparison purposes, we also test these relationships

(*footnote continued*)
use the mean closing price during the calendar month in which the options were granted. We also assume that the number of options granted to an executive remains constant (see Walker, 2007).

[7] For non-qualified stock options, a tax deduction accrues to the company on the exercise day equal to the difference between the market and exercise prices. If the exercise is backdated to occur at a low price, the company forgoes a portion of this deduction.

[8] Holding all else equal, backdating by construction results in increased compensation for executives. Bebchuk, Grinstein, and Peyer (2010) show evidence that in actuality the incidence of backdating is correlated with higher overall levels of compensation, so that it does not appear to serve as a substitute for other forms of remuneration. In unreported empirical analyses we confirm that suspect CEOs in our sample receive higher overall levels of compensation.

[9] In legal complaints the SEC alleges that grant backdating violated the Exchange Act's antifraud provisions (Section 10(b)), false or misleading proxy statements (Section 14(a)), Sections 17(a)(1), (a)(2), and (a)(3) of the Securities Act, and Rule 10b-5. See Fried (2008) for a complete description of the legality of options backdating.

[10] We review proxy filings for several (randomly selected) options backdating events in our sample and find evidence that backdating or granting in-the-money options directly violates shareholder-approved plans.

among firms that backdate for non-executive employees only. If these backdating firms are not more likely to engage in other clearly defined corporate misbehaviors, it suggests that this form of backdating was not nefarious in intent, and emphasizes the importance of top executive behaviors for determining firm culture. Alternatively, if we find that these firms are also more likely to engage in other corporate misbehaviors, it would cast doubt on whether our results should be asserted to support the upper echelons theory.

2.4. Alternative characterizations of options backdating

Several accused litigants, academic researchers, and members of the press take issue with a characterization of options backdating as either unethical or illegal. To the extent that these arguments are valid, we do not expect to find an association between backdating and other forms of corporate misbehavior. In this section, we discuss these arguments and point out some patterns associated with backdating as it was practiced that cause us to question this perspective.

Many of the dissenting views expressed in both political circles and the popular press focus on a semantic difference in the interpretation of the word "backdating." As expressed by Senator Jim Bunning (Republican – KY), "Interestingly, even Chairman Cox acknowledges that backdating, in some circumstances, is perfectly legal".[11] In fact, firms do generally have discretion when setting stock option exercise prices.[12] As long as option practices are properly disclosed and accounted for, firms are free to choose an exercise price that, for example, is equal to the lowest closing price for that stock over the prior month. However, there is one primary reason why we believe this view does not properly characterize events that our methodology identifies as "secretive backdating." If a firm properly discloses and accounts for a backdated stock option grant (i.e., above-board backdating), we should observe an exercise price that differs from the closing market price on an accurately reported grant date. It is important to note that our methodology explicitly excludes observations that fall into this category, as they would appear to be granted in-the-money.

A second perspective expressed by at least one accused litigant is that some firms perceived backdating practices as acceptable under U.S. accounting standards. The accused litigant (Bill Ruehle at Broadcom) defended his actions by arguing for a more flexible interpretation of Accounting Principles Board (APB) Opinion No. 25. The central issue of interpretation regards the measurement date that applies to a particular option grant. As per APB 25, the measurement date is one on which the following are known with finality: the individual receiving the grant, the exact number of shares that the individual is entitled to receive, and the option exercise price. According to the SEC the meaning of APB 25 was clear and the only allowable exception, where grants might appear to look as if they were backdated, involves "(short) unimportant delays in the completion of administrative procedures to document the grant that did not involve misrepresentation of the option granting actions." This legal form of backdating does not include any type of look-back provision and therefore should be uncorrelated with grants that occur on the most favorable day of a calendar month (i.e., our identification metric).

The legality of option exercise backdating appears clear cut. As pointed out by Cicero (2009) and Dhaliwal, Erickson, and Heitzman (2009), the main motivation for backdating an option exercise is to avoid paying personal income taxes that are legally owed. Cicero (2009) states that "Concealed backdating for the purpose of reducing a tax burden is likely actionable under the antifraud provisions of the Internal Revenue Code (Sections 7201, 7206, 7207), and can be deemed a felony and garner penalties of up to $500,000." Approximately 33% of our sample of suspect CEOs appears to have backdated their option exercises.

Another alternative perspective on options backdating expressed in the popular press by Holman Jenkins is that it often amounted to nothing more than a "fairly meaningless violation of accounting rules" (*Wall Street Journal* editorial, November 17, 2010). Jenkins (2010) argues that the widespread use of backdating suggests it was an accepted business practice, and that the small number and nature of resultant criminal convictions fails to indicate widespread destructive behavior. Although this perspective is not wholly unreasonable, it should be tempered by acknowledgment that being convicted of an egregious act and having committed one are very different, and consideration must be given to the resource constraints of the Department of Justice and the difficulty and high standards of proving white collar crime in a U.S. court of law.

At least one academic study argues that option grant backdating could serve as an efficient means of substitute compensation by cash-constrained firms or with risk-averse managers (Gao and Mahmudi, 2011). Empirical evidence is generally unsupportive of these arguments. For example, Bebchuk, Grinstein, and Peyer (2010) show that backdating is correlated with higher overall levels of compensation. Additionally, in untabulated results, we find that the existence of systematic backdating is *positively* correlated with firm cash levels.

Finally, we note that if options backdating is properly disclosed and accounted for, it should legally be treated in an identical manner to in-the-money option grants. However, doing so effectively eliminates any benefit of engaging in this type of activity. In fact, not revealing backdating to authorities is a necessary condition for reaping many of the benefits, including understating the expenses associated with options grants and avoiding the personal taxes associated with option exercises. Giving consideration to all of these perspectives, we submit that it is reasonable, on balance, to interpret options backdating as a questionable

[11] "Dismissed with prejudice," *Directorship Magazine*, December 7, 2012.

[12] A firm's ability to set exercise prices in-the-money is limited to non-qualified options. For incentive stock options (ISO) the firm is required to set the exercise price as "not less than the fair market value of the stock at the time such option is granted." (Internal Revenue Code (IRC) 422(b) (4)).

secretive business practice that firms engaged in, for executives' personal benefit, and at the expense of other firm stakeholders. To the extent that this view is accurate, it supports the use of participation in systematic backdating to identify suspect CEOs.

2.5. Inside- versus outside-hire suspect CEOs

We submit that one might expect a stronger association between systematic backdating to the CEO and other forms of corporate misbehavior when the CEO is hired from outside the firm. The logic underpinning our assertion relies on the expectation that outside CEOs are likely to enjoy a greater level of discretion in corporate decision making than their counterparts who are promoted from within. According to Finkelstein, Hambrick, and Cannella (2009):

> *"…discretion serves to attenuate the relationship between executive characteristics (values, experiences and so on) and organizational outcomes. Namely, if high discretion exists, executive orientations become reflected in organizational outcomes; if low discretion exists, they do not".*

Hambrick and Finkelstein (1987) conjecture that outside CEOs are likely to enjoy a greater amount of autonomy/discretion in their decisions. This assumption is generally supported by extant literature. For example, Parrino (1997) discusses how outsiders are often hired with implicit or explicit mandates to change the direction of the firm. Other studies provide evidence that outside CEOs are more likely to influence corporate investment behavior and induce strategic changes (Huson, Malatesta, and Parrino, 2004; Bailey and Helfat, 2003). Prior literature also suggests that CEO discretion is influenced by the extent of dissention among other executive decision makers (Adams, Almeida, and Ferreira, 2005). Kesner and Dalton (1994) and Charan (2005) find that outside CEO succession is associated with higher levels of managerial turnover, consistent with outside CEOs altering the composition of their executive team to include a greater fraction of like-minded and/or acquiescent individuals.

We confirm these findings for our sample of suspect CEOs. As discussed in greater detail in Appendix A, we find that CFO replacement occurs in 46.3% of outside suspect CEO transitions versus only 31.0% for inside suspect CEO transitions (p-value for difference = 0.039). In addition, outside CEOs are significantly more likely to replace a member of the board (73.1% versus 58.4%). We also provide evidence that executives and board members replaced by outside suspect CEOs are significantly more likely to be systematic backdaters (i.e., like-minded). Specifically, we investigate the association between suspect CEOs and a newly appointed (within the first three years of the CEO's tenure) suspect CFO, director, or member of the C-suite. Following an outside CEO (inside CEO) transition, 22.2% (5.6%) of firms have a new CFO who is independently classified as suspect (p-value for difference < 0.001). The results for other new directors and executives are similar.

In addition to our analysis of the overall sample of suspect CEOs, we also conduct all empirical tests separately for CEOs that are hired from outside the firm and those that are internal promotions. We do so to explore whether the relationship between the CEO's personal ethics and other questionable corporate actions is stronger when the CEO was an outside hire who could take advantage of the opportunity to shape the culture around her.[13]

3. Data

We collect data on stock and option transactions from the Thomson Financial Network Insider Filing Data Feed (IFDF), which is designed to capture all U.S. insider activity as reported on Forms 3, 4, 5, and 144. For option grants, we investigate the sample period from January 1, 1992 to December 31, 2009, and for option exercises, we investigate the sample period from August 15, 1996 to December 31, 2009.[14] We treat multiple option grants to the same individual on the same day as a single observation and exclude all regularly scheduled grants as well as those that occur on an ex-dividend day, at the time of an annual meeting, or that are not issued at-the-money.[15] We also limit our sample to those with appropriate cleanse codes as identified by Bebchuk, Grinstein, and Peyer (2010).[16] We then classify the 144,456 option grants that meet these criteria as 'likely backdated' if they occur on the most favorable (i.e., lowest stock price) day of the calendar month (see Cicero, 2009; Bebchuk, Grinstein, and Peyer, 2010). In addition, we require that all likely backdated grants in the post-SOX period be reported at least 14 days after the SEC-required reporting date. This final requirement is consistent with Cicero (2009) and rules out grants without reasonable look-back periods. Using this

[13] A supporting theory for why outside-hire CEOs could differ from inside-hire CEOs is modeled by Akerlof and Kranton (2005), who envision corporate "insiders" as more loyal to their firms and therefore expect them to "act in the interests of the firm." Corporate "outsiders" are expected to ardently serve their own interests, which don't perfectly align with interests of the firm and shareholders.

[14] The beginning of our option grant sample period corresponds with Lie (2005) who states, "Since 1992, the SEC has required firms to disclose certain information in proxy statements about stock option grants to top executives during the fiscal year." The beginning of our option exercise sample period corresponds with the date when data regarding the sale of underlying option shares are first available (Cicero, 2009).

[15] Additional filters are consistent with those imposed by Bebchuk, Grinstein, and Peyer (2010). Scheduled grants include those that occur within in a three-day window around the one-year anniversary of a previous grant to the same individual. Ex-dividend grants include those that occur during the same day that a stock has an ex-dividend date. Annual meeting grants include any grant that occurs within one trading day of a firm's annual meeting date. Grants not issued at-the-money include any grant in which the strike price differs by more than 1% from the closest Center for Research in Security Prices (CRSP) closing price in the three-day window around the option grant date.

[16] As in Bebchuk et al. (2010), our sample of executive option grants is limited to those with cleanse codes that equal 'R' ("data verified through the cleansing process"), 'H' ("cleansed with a very high level of confidence"), or 'C' ("a record added to nonderivative table or derivative table to correspond with a record on the opposing table"). All other observations in the Thomson data set have issues of integrity and verifiability.

procedure, we identify 18,815 option grants to 16,312 individuals at 3,434 firms as likely backdated.

We next examine option exercises and identify a total of 185,660 individual option exercise days after limiting the sample to those with appropriate derivative codes, transcodes, and cleanse codes.[17] Following Cicero (2009), we partition option exercises into three mutually exclusive categories: (i) exercise-and-hold, (ii) exercise-and-sell transactions with a disposition of shares to the company, and (iii) exercise-and-sell transactions with an open market sale of shares. Identification for each exercise into one of these three categories is obtained by merging the options data with stock sales during the $[-1, +1]$ trading day window around the option exercise date. The exercise-and-sell open market transactions are excluded from the pool of potentially backdated options because it is unlikely that such counterparties would accept higher than market prices.

After applying these data filters, there are 33,206 option exercises that can be classified as 'likely backdated' if they occur on the most favorable day of a calendar month. For exercise-and-hold transactions, the most favorable date corresponds to the lowest stock price of the month. Out of 24,923 potential exercise-and-hold transactions, we classify 2,862 as likely backdated. For exercise-and-sell company disposition transactions, the most favorable day of the month is the highest stock price day. We classify 855 exercise-and-sell transactions as likely backdated out of the sample of 8,283 possible observations.

An individual is characterized as 'suspect' if we reasonably determine that she benefitted from systematic backdating. Assuming that grant and exercise dates are randomly distributed across time, approximately 5% should fall on the most favorable day of the month. We face a tradeoff between the accuracy of our classification and the number of suspect individuals in our sample (i.e., power of our tests). To help optimize the signal-to-noise ratio in our identification scheme, we require an individual to have at least two likely backdated option events and at least 30% of their option activity to be classified as likely backdated in order for that individual to be classified as suspect.[18] Of those individuals identified, we uncover 249 CEOs from 248 unique firms that appear to have benefitted directly from backdating (one firm in our sample had co-CEOs who were both identified as suspect). We augment our sample with 12 CEOs who did not meet our identification criteria, but who were specifically named in an enforcement action or participated in a settlement that is disclosed on the SEC's spotlight on options backdating website. After merging our sample with external data sources necessary to conduct our empirical tests, our final sample includes 258 suspect firms in which the CEO (and often other top managers) benefitted directly from options backdating. Of these CEOs, 178 (69%) were internal promotions and 80 (31%) were hired externally. In addition, throughout our analyses we contrast our results to those for 172 suspect firms in which it appears that backdating benefitted lower-level officers, but not the top executives (CEO, CFO, President, Chairman).

Given our data-driven approach, it is possible that our identification scheme will classify some firms as 'suspect' by random chance even though the firm did not engage in option backdating. The extent of this issue is a function of both the number of reporting individuals and the number of option events at a firm. To determine how many firms would be classified as 'suspect' by chance, we implement the following experiment: We randomly re-assign option events to event dates in the same calendar month that they occur, and then use the stock price on these pseudo-event dates to determine whether they are 'likely backdated.' 'Pseudo-suspect' firms are then selected using the same criteria discussed above. We repeat this procedure one hundred times to generate the distribution of possible samples that would be found randomly using our identification method. Using data for all insiders that are covered in the Thomson data, this procedure results in a distribution of randomly chosen pseudo-suspect CEO firms with a mean of 50 and a standard deviation of 6.9. It appears that a large fraction of the 249 suspect CEO firms actually identified by our methodology are accurately classified as backdaters.

Empirical tests for suspect firms in which the CEO directly benefitted from backdating focus on firm-years during the suspect CEOs' tenure. We obtain CEO tenure using annual reports and proxy statements filed through Electronic Data Gathering, Analysis, and Retrieval (EDGAR) on the SEC website. In addition, CEOs are classified as either outside hires or internal promotions based on their previous work experience with the firm. As in Cremers and Grinstein (forthcoming), executives that have been with the firm for less than one year prior to promotion to CEO are classified as outsiders and executives that have been with the firm for greater than one year are classified as insiders. Additionally, CEOs described as founders in the biographic information found in SEC filings are classified as insiders.

Data from a number of sources are necessary for this study. Compustat data is used to determine discretionary accruals and to obtain firm characteristics including market-to-book, leverage, return on assets, and auditor identity. Returns data are taken from the CRSP files. Earnings and analysts' forecasts are obtained from Institutional Brokers' Estimate System (IBES). Institutional ownership is obtained from the Thomson Financial 13F database, and board of director characteristics and executive stock ownership are taken from Compact Disclosure. Information regarding financial fraud is obtained from the Federal Securities Regulation (FSR) Database (Karpoff, Koester, Lee, and Martin, 2013) and information for shareholder class-action lawsuits is obtained from the Stanford Class Action Clearinghouse ("SCAC"). Finally, we obtain merger and acquisition data from the Securities Data Company (SDC) database.

In all of our analyses, we compare the activities of suspect firms to those of similar control firms. The control

[17] Our sample is limited to those with derivative code equal to Incentive Stock Option (ISO), Employee Stock Option (EMPO), or Non-Qualified Options (NONQ). Transcodes are limited to 'M,' 'X,' or 'J.'

[18] In unreported tests we confirm that our results are robust to implementing classification schemes for suspect that require at least three backdated option events or at least 50% of option events to be classified as 'likely backdated'.

Table 1
Summary statistics.

Panel A reports descriptive statistics for the time period from January 1990 to December 2009 for our sample of "suspect" firms as well as a sample of control firms. Data for both option grants and exercises are collected from the Thomson Financial Insiders trading database. Reporting insiders are classified as suspect if at least 30% of their option activity (and at least two option transactions) are classified as 'likely' backdated or they were highlighted in the SEC's Spotlight on Backdating. The CEO sample restricts the sample to the firm-years with a suspect CEO in power. The Non-mgmt sample consists of the firms in which at least one suspect insider is present that does not report under the following role codes: CEO, CFO, President, and Chairman. Matching between suspect and control samples is based on year, industry (three-digit SIC code), and market value of equity; up to five control firms are selected for each suspect firm on an annual basis. A single control observation is calculated for each suspect firm-year by averaging the values of the appropriate control observations. Market-to-book ratio, total assets, leverage, return on assets, and Tobin's Q are obtained from Compustat, and market value of equity is obtained from CRSP. Panel B reports the distribution of suspect firms and all Compustat firms by industry, using 12 industry groupings as presented by Fama and French. Numbers presented in parentheses are *p*-values calculated using standard errors clustered at the firm level.

Panel A: Firm characteristics

	Suspect – CEO sample			Suspect – Non-mgmt sample		
	Suspect	Control	Diff.	Suspect	Control	Diff.
MVE (*$ millions*)	1,875	1,737	138.4 (0.662)	1,875	1,671	203.5 (0.511)
Market-to-book	3.364	3.395	−0.032 (0.852)	3.587	3.684	−0.096 (0.642)
Assets (*$ millions*)	1,860	2,371	−510.9 (0.195)	2,087	2,040	47.32 (0.923)
Leverage	0.178	0.193	−0.015 (0.188)	0.168	0.185	−0.017 (0.215)
Tobin's Q	2.102	2.161	−0.059 (0.605)	2.275	2.310	−0.035 (0.800)
Return on assets	−0.0327	−0.0497	0.017 (0.122)	-0.040	-0.062	0.022 (0.129)
Firm-years	2,298	2,298		1,924	1,924	
Unique firms	258			172		

Panel B: Distribution by industry

Industry	Suspect CEO sample versus Compustat			CEO sample versus Non-mgmt sample		
	CEO suspect	Compustat firms	Difference	CEO suspect	Non-mgmt suspect	Difference
Consumer non-durables	3.10%	4.77%	−1.67%	3.10%	5.23%	−2.13%
Consumer durables	1.55%	2.22%	−0.67%	1.55%	2.91%	−1.36%
Manufacturing	8.91%	8.33%	0.58%	8.91%	6.40%	2.51%
Oil, gas, and coal extraction	3.49%	3.98%	−0.49%	3.49%	1.16%	2.33%
Chemicals and allied products	0.39%	1.76%	−1.37%	0.39%	1.16%	−0.77%
Business equipment — computers, etc.	37.98%	19.24%	18.74%	37.98%	34.88%	3.10%
Telephone and television transmission	2.33%	3.87%	−1.54%	2.33%	1.74%	0.59%
Utilities	0.78%	1.71%	−0.93%	0.78%	0.58%	0.20%
Wholesale, retail, and some services	7.75%	9.10%	−1.35%	7.75%	8.14%	−0.39%
Healthcare	13.18%	9.96%	3.22%	13.18%	16.86%	−3.68%
Finance	12.40%	20.57%	−8.17%	12.40%	9.30%	3.10%
Other	8.14%	14.48%	−6.34%	8.14%	11.63%	−3.49%
Unique firms	258	16,059		258	172	

observations are obtained by matching each suspect firm-quarter (or firm-year) to all firms without a suspect individual in the same quarter (or year) and SIC3 industry group. We retain up to five matched observations that are closest in market value of equity to our suspect observation and further require that differences in market capitalization between suspect and control observations not exceed 50%.

Summary statistics for our sample of suspect and control firms are presented in Table 1. To evaluate a characteristic of our control sample, we first take the average of that characteristic across the (up to five) control firms selected for each suspect firm. The characteristic averages for the control firm groups are then averaged and compared to the sample of suspect firm characteristics.[19] Not surprisingly given our matching method, we find that the samples of suspect and control firms have very similar characteristics. For the suspect CEO sample, we find

[19] This procedure minimizes the difficulty of comparing the overall averages for the suspect sample and controls, which can be biased if certain suspect firms systematically match with fewer appropriate control firms. For example, there are more appropriate matches on average for small firms than large firms, and without this adjustment we would report negatively biased average control firm size. To be confident that an imbalance of the number of control firms does not combine with possible nonlinearities across firm size to bias our results, we confirm that our inferences all continue to hold when using decile and sub-decile dummies to control for size in the multivariate regressions.

sample (control) firm-years have an average market value of equity of $1.88 billion ($1.74 billion), market-to-book value of 3.36 (3.40), and assets of $1.86 billion ($2.37 billion), all of which are insignificantly different. The average leverage of sample firms is 0.18 compared to 0.19 for control firms. Similar patterns hold when comparing the sample of firms that backdated only for lower-level employees (Non-mgmt sample) and their control firms.

Panel B of Table 1 presents a breakdown of our suspect firms across 12 Fama-French industry groups. As would be expected given the focus of the backdating investigations, the group that is most represented is "Business equipment – computers, software." However, we also find that the unconditional industry representation for this group is high across all Compustat firms. Although 38% of our suspect sample comes from the technology industry group, so too does 19% of Compustat firms. Other notable differences are that backdating appears somewhat more prevalent among healthcare companies (13% of our suspects versus 10% of the Compustat firms) and less prevalent in finance (12% versus 21%). Firms with lower-level officer backdating are similarly concentrated in the business equipment industry.

4. Empirical results

Our main hypothesis is that suspect CEOs will lead their firms to engage in suspect corporate activities. In this section we compare fraud, the quality of financial reporting, and investment activities for our sample of suspect firms to those of similar control firms.

4.1. Financial reporting fraud

To the extent that systematic participation in options backdating represents unethical behavior of the CEO and/or her firm, we posit that suspect firms are more likely to engage in other forms of financial reporting fraud. To test our conjecture, we obtain data from the Federal Securities Regulation Database (see Karpoff, Koester, Lee, and Martin, 2013). The FSR database contains detailed information pertaining to 1,099 cases of financial misrepresentation from 1978 to 2011. The database contains the beginning date, ending date, and revelation date of each fraud event. In untabulated univariate analyses, we find that 8.82% of our suspect firms are caught in a financial fraud compared to 2.94% of control firms (p-value for difference = 0.007). The higher rate of fraud is present in both the outside suspect CEO sample (9.37% versus 2.70%) and inside suspect CEO sample (8.57% versus 3.05%). We find similar patterns in the fraction of years that a fraud was perpetuated while a CEO is at his firm (*Fraction of fraud years*) and the total number of years there was reporting fraud (*Number of fraud years*). These univariate results suggest that firms with a suspect CEO at the helm are significantly more likely to engage in financial fraud.

We also evaluate the incidence of fraud in a multivariate framework. Using the FSR database we construct a panel data set that contains firm-year information for each of our suspect firms during years that the suspect CEO is leading her firm and their corresponding control firms. The dependent variable, *Fraud year*, is an indicator that is equal to one if the firm-year overlaps with a fraud event reported in the FSR database. We require frauds to be announced prior to December 31, 2005 to eliminate the confounding effect of options backdating-related frauds. We follow Burns and Kedia (2006) and control for standard firm characteristics that could be related to the incidence of fraud including the market value of equity (*MVE*), the market-to-book value of equity (*MTB*), *Leverage*, and return on assets (*ROA*). We also control for the level of *Institutional ownership* (Burns, Kedia, and Lipson, 2010) and corporate governance characteristics including the total number of directors (*Ln board size*) and the percentage of independent directors (*Board independence*) (Beasley, 1996). We include *Firm age*, which is constructed as the number of months since a firm first appeared in CRSP, and we control for CEO incentives by including the percentage of a firm owned by the CEO (*CEO ownership*).[20] These regressions also include year and industry (SIC3) fixed effects. The variable of interest is *Suspect*, an indicator equal to one if the firm has a suspect CEO. In subsequent regression specifications we partition the *Suspect* indicator into *Suspect CEO (outside)* and *Suspect CEO* (*inside*).

The fraud analysis is presented in Table 2. Consistent with Burns and Kedia (2006), we find positive and significant coefficients on *MVE* and *Leverage* indicating that fraud is more likely to be associated with larger firms and firms with greater leverage. Higher levels of fraud are also associated with less independent boards, lower levels of institutional ownership, younger firms, firms with lower market-to-book ratios, and lower levels of CEO ownership. In the first regression specification we find the coefficient on *Suspect CEO* is 0.0202 (p-value < 0.001), indicating that suspect firms are almost twice as likely to engage in fraud as control sample firms.[21] In the second regression specification, we partition the variable *Suspect* according to whether the suspect CEO is an external hire or internal promotion. The coefficient estimate for *Suspect CEO (outside)* is 0.0296 (p-value < 0.001) and for *Suspect CEO (inside)* is 0.0171 (p-value < 0.001), indicating an increase in the likelihood of fraud in each group of suspects. In the third regression, we redefine the dependent variable *Fraud year* to include only fraud years in which the fraudulent activity begins during a suspect CEO's tenure. The coefficient on *Suspect CEO (outside)* remains a positive and significant 0.0321 (p-value = 0.004) and the coefficient on *Suspect CEO (inside)* is an insignificant 0.0055 (p-value = 0.261). Altogether, these results suggest that suspect CEOs who are promoted from within are more likely to inherit and continue a financial fraud at their firms, whereas suspect CEOs hired from the outside are more likely to initiate a fraud.

Suspect firms that commit financial fraud or otherwise destroy shareholder value should be exposed to a greater incidence of shareholder class-action lawsuits. We collect data for shareholder lawsuits from the Stanford Class Action Clearinghouse ("SCAC"); this database contains information on more than 3,600 class-action lawsuits filed between 1996 and 2014. We match each suspect CEO firm with a single control firm by industry and market value of

[20] All independent variables in the regression, excluding *Suspect*, are lagged values.

[21] The unconditional probability of a firm-year with fraudulent reporting in our sample of 6,498 firm-years is 2.23%.

Table 2
Fraudulent reporting.

This table presents a multivariate panel regression analysis investigating the propensity of firms to perpetuate financial reporting fraud. The sample consists of all firm-years for suspect firms during the 1990–2005 sample period when the suspect CEO is leading her firm. We select up to five control firms for each suspect firm each year based on year, industry (three-digit SIC code), and market value of equity (the difference in MVE between the suspect firm and control firm cannot exceed 50% of the MVE of the suspect firm). Information concerning firm frauds (and years over which those frauds were perpetuated) is collected from the Federal Securities Regulation Database (Karpoff, Koester, Lee, and Martin, 2013). We require frauds to be announced prior to December 31, 2005 to eliminate the confounding effect of options backdating-related frauds. The dependent variable, *Fraud year*, is an indicator that is equal to one if the firm-year overlaps with a fraud event reported in the FSR database. The first three specifications are limited to the firms with suspect CEOs and the final specification investigates non-management suspect firms. In the third specification, the fraud indicator only indicates frauds at suspect firms that began after the suspect CEO was hired or promoted. Table 1 provides sample descriptive characteristics. Numbers presented in parentheses are *p*-values calculated using standard errors clustered at the firm level.

	Dependent variable = Fraud year			
	Suspect CEO	Suspect CEO	Suspect CEO	Non-mgmt
Independent variables				
Constant	−0.0553***	−0.0551***	−0.0503***	−0.00372
	(0.007)	(0.007)	(< 0.001)	(0.871)
Ln(MVE)	0.0136***	0.0137***	0.0130***	0.0106***
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)
Ln(MTB)	−0.00760***	−0.00771***	−0.00686***	−0.0100***
	(0.003)	(0.002)	(0.0099)	(0.0006)
Leverage	0.0222*	0.0221*	0.0152	0.0373***
	(0.059)	(0.059)	(0.148)	(0.006)
ROA	−0.00924	−0.00940	−0.0103	−0.00431
	(0.148)	(0.141)	(0.247)	(0.637)
Firm age	−0.00055***	−0.00054***	−0.00045**	−0.00052**
	(0.005)	(0.006)	(0.023)	(0.014)
Board size	0.000471	0.000499	0.00008	−0.00165
	(0.629)	(0.609)	(0.942)	(0.190)
Ln(board independence)	−0.0409**	−0.0415***	−0.0270*	0.0183
	(0.011)	(0.0099)	(0.022)	(0.238)
Institutional ownership	−0.0174**	−0.0179**	−0.0300***	−0.0328***
	(0.024)	(0.020)	(< 0.001)	(0.004)
CEO Ownership	−0.0401*	−0.0388*	−0.0366***	−0.0195
	(0.068)	(0.078)	(0.005)	(0.317)
Suspect CEO	0.0202***			
	(< 0.001)			
Suspect CEO (outside)		0.0296***	0.0321***	
		(< 0.001)	(0.004)	
Suspect CEO (inside)		0.0171***	0.00555	
		(< 0.001)	(0.261)	
Non-mgmt suspect				−0.00728*
				(0.055)
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Observations	6,498	6,498	6,498	5,088
R-squared	0.058	0.058	0.056	0.059

*Denotes significance at the 10% level,
**denotes significance at the 5% level,
***denotes significance at the 1% level.

equity, and only firm-years in which a suspect CEO is leading her firm are included in the sample.[22] In untabulated univariate tests, we find that the probability of being named in a class-action suit is 26.3% for our suspect firms compared to 13.9% for control firms (*p*-value for difference = 0.001). For the outside suspect CEO sample, 27.4% of suspect firms have a derivate lawsuit versus 13.5% for the control sample (*p*-value for difference = 0.043). For the inside suspect CEO sample, 25.7% of suspect firms have a shareholder lawsuit versus 14% for the control sample (*p*-value for difference = 0.01). Overall, our analyses suggest that suspect firms are more likely to commit fraud and to be exposed to class-action lawsuits.

As discussed previously, we also present all of our results for the sample of firms in which backdating was evident, but only for lower-level officers (i.e., Non-mgmt sample). These results are presented in the fourth specification of Table 2 and they suggest that the non-management sample is actually associated with a reduced instance of fraud (−0.0073; *p*-value = 0.055). The relationship between non-management suspect firms and lawsuits is also negative but is insignificant.

[22] Consistent with our fraud analysis, we require class-action lawsuits to be filed on or before December 31, 2005 to eliminate the confounding effect of options backdating-related lawsuits.

This contrast highlights the importance of CEO involvement in questionable options backdating. In the following subsections we provide more detailed analyses of the financial reporting and investment activities of firms led by suspect CEOs.

4.2. Just meeting or beating earnings expectations

Executives have direct private incentives to meet or exceed the earnings expectations of analysts, since executive compensation is largely comprised of equity-based components and stock prices are sensitive to meeting analysts' forecasts (Murphy, 2003; Bartov, Givoly, and Hayn, 2002). It is also common for executives to receive bonus compensation for meeting analysts' forecasts (Matsunaga and Park, 2001). Prior research finds that a disproportionately large number of firms just meet or beat analysts' forecasts (Hayn, 1995; Degeorge, Patel, and Zeckhauser, 1999) and commonly interpret this as evidence that executives opportunistically manage earnings to attain these thresholds.

Existing literature also highlights the fact that not all earnings management is nefarious. For example, several studies provide evidence that firms that use discretionary accruals to "smooth" their earnings have lower borrowing costs, increased equity values, and more efficiently impound earnings information into stock prices (Trueman and Titman, 1988; Subramanyam, 1996; Tucker and Zarowin, 2006; among others). Although some forms of earnings management could benefit firms and their shareholders, earnings management that is solely designed to meet or beat earnings expectations gives investors an incomplete view of a firm's latent fundamentals and is therefore a questionable practice.

We test whether suspect firms are more likely to engage in this particular form of earnings management. Our tests compare firms' earnings to analysts' forecasts reported in the IBES unadjusted summary files (Kasznik and McNichols, 2002; McVay, Nagar, and Tang, 2006). Graham, Harvey, and Rajgopal (2005) survey CEOs and find that meeting analysts' forecasts is an important earnings threshold. We take the last analyst consensus mean or median earnings forecast (prior to the earnings announcement) to benchmark earnings expectations.[23] Our measure of earnings surprise is the actual earnings announced minus the mean or median analyst forecast from IBES.

Table 3 shows univariate comparisons of quarterly earnings surprises. To conduct our tests, we follow the methodology of McVay, Nagar, and Tang (2006) and Burgstahler and Dichev (1997) and classify our samples of earnings announcements into those that just beat (by zero, one, or two cents) or just miss (by one or two cents) analysts' quarterly earnings forecasts.[24] Specifically, we construct an indicator variable *BEAT* if the quarterly earnings surprise is 0¢, 1¢, or 2¢, and define a similar indicator variable *MISS* for earnings surprises that equal −1¢ or −2¢. The univariate comparisons provide evidence that suspect firms just meet or beat earnings estimates significantly more often than control firms (43.3% versus 37.8%). The difference of 5.5% (p-value = 0.002) demonstrates that suspect firms meet or narrowly beat their earnings expectation approximately 15% more often than similar firms. Suspect firms are also less likely to just miss earnings forecasts, suggesting these "just miss" observations are pushed above the threshold to meet expectations. Relative to the mean forecast, suspect firms just miss by one or two cents 10.3% of the time, compared to 11.4% for control firms (p-value of the difference = 0.055).

We further investigate differences in earnings surprise patterns depending on whether or not a suspect CEO is an outside hire. Firms with outside-hire suspect CEOs just exceed the mean analyst forecast 47.7% of the time versus a mark of 39.3% for control firms (an increase of 21.4%; p-value = 0.008). In contrast, the difference across the inside-hire suspect CEOs and their peers is largely attenuated (41.8% versus 37.8%; p-value = 0.057). Consistent with results from our fraud analysis, we investigate the Non-mgmt sample and find that these firms were not more likely than similar firms to narrowly meet earnings expectations.

We pool suspect and control firm-quarters and conduct logit regressions predicting a quarterly earnings surprise in the narrow classification *BEAT*. The regressions include controls for standard firm characteristics as in Table 2 including size (*MVE*), growth opportunities (*MTB*), *Leverage*, profitability (*ROA*), *Firm age*, the total number of directors (*Ln board size*), the percentage of independent directors (*Board independence*), and *Institutional ownership*. We also control for CEO compensation incentives by aggregating the total number of options granted to the CEO in the prior year (*Prior year option grants*), and CEO stock ownership by including *CEO ownership* and *CEO ownership*^{2}. In addition, all regression specifications include industry (SIC3), auditor, and individual quarter fixed effects. Consistent with previous regressions, the variable of interest in our regression is *Suspect*, an indicator equal to one if the firm has a suspect CEO.

The earnings surprise regressions are presented in Table 4. We find positive and significant coefficients on *MVE* and *ROA* indicating that larger and more profitable firms are more likely to meet or narrowly beat their earnings targets. Just meeting or beating forecasts is also positively correlated with market-to-book and negatively correlated with firm age, suggesting that it is more common for growth firms and younger firms. Firms are more likely to meet these thresholds when the CEO is granted more options in the previous year, suggesting that compensation incentives are an important determinant of performance or earnings management. There does not appear to be a stable relationship between board characteristics and meeting earnings thresholds, nor do we find an important role for CEO stock ownership in our sample.

The results from our univariate analysis are confirmed in this multivariate setting. The coefficient on *Suspect* is positive and significant at the 10% level in the regression

[23] Our analysis is robust to two alternate measures of analyst expectations. First, we construct a mean and median analyst forecast using the most recent forecast from each analyst in the 90 days prior to an earnings announcement. Second, we use only the last analyst forecast prior to the earnings announcement day [see Ayers, Jiang, and Yeung (2006) for an analysis of which benchmark is the most appropriate for earnings targets].

[24] We consider alternate measures of just meet [0¢, 1¢] or just miss [−1¢] and also investigate actions in only the fourth quarter. All results are robust to these alternate measurements.

Table 3
Univariate statistics for earnings surprises.

This table reports the univariate statistics for earnings surprises during the sample period from January 1990 to December 2009 for our sample of suspect firms as well as a sample of control firms. Data for both option grants and exercises are collected from the Thomson Financial Insiders trading database. Reporting insiders are classified as suspect if at least 30% of their option activity (and at least two option transactions) is classified as 'likely' backdated or they were highlighted in the SEC's Spotlight on Backdating. The CEO sample restricts the sample to the firm-years with a suspect CEO in power. The Non-mgmt sample consists of the firms in which at least one suspect insider is present that does not report under the following rolecodes: CEO, CFO, President, and Chairman. Matching between suspect and control samples is based on year, industry (three-digit SIC code), and market value of equity; up to five control firms are selected for each suspect firm on a quarterly basis. For both the suspect and control samples we obtain earnings announcements and analysts' forecasts from the IBES unadjusted summary files and define earnings surprise as the actual earnings announced minus the mean or median analyst forecast from IBES. We construct an indicator variable *BEAT* if the earnings surprise for a firm quarter is 0¢, 1¢, or 2¢, and define a similar indicator variable *MISS* for earnings surprises that equal −1¢ or −2¢. We present univariate differences between suspect firm-quarters and the matched sample. The table also separately presents statistics for suspect CEOs that were outside hires and those that were internal hires. Suspect CEOs are classified as outside or inside hires based on the biographical information found in SEC filings. Executives who have been with the firm for longer than one year at the time they are promoted to CEO are classified as inside hires and those with less than one year of service prior to promotion are classified as outside hires. Numbers presented in parentheses are *p*-values calculated using standard errors clustered at the firm level.

	Meet/Beat [0¢ to 2¢]		Just miss [−2¢ to −1¢]		# Firm-years	# Unique firms
	versus Mean forecast	versus Median forecast	versus Mean forecast	versus Median forecast		
Suspect CEO sample						
Suspect	0.433	0.444	0.103	0.096	6,101	226
Control	0.378	0.386	0.114	0.112	20,615	
Difference	0.055***	0.058***	−0.011*	−0.016***		
	(0.002)	(0.001)	(0.055)	(0.004)		
Outside CEO sample						
Suspect	0.477	0.493	0.102	0.089	1,517	72
Control	0.393	0.403	0.115	0.114	6,262	
Difference	0.084***	0.090***	−0.013	−0.025**		
	(0.008)	(0.004)	(0.272)	(0.022)		
Inside CEO sample						
Suspect	0.418	0.4278	0.103	0.099	4,584	154
Control	0.378	0.385	0.114	0.113	15,794	
Difference	0.040*	0.043**	−0.011*	−0.014**		
	(0.057)	(0.042)	(0.086)	(0.023)		
Non-mgmt sample						
Suspect	0.412	0.419	0.115	0.113	4,931	161
Control	0.404	0.412	0.112	0.110	16,101	
Difference	0.008	0.007	0.003	0.003		
	(0.667)	(0.717)	(0.682)	(0.686)		

*Denotes significance at the 10% level,
**denotes significance at the 5% level,
***denotes significance at the 1% level.

with all backdating CEO firms. In the regression with CEOs hired externally, the coefficient on *Suspect* is 0.243 and significant at the 5% level, whereas in the regression for CEOs hired internally it is a much smaller 0.073 and is statistically insignificant. In terms of marginal effects, suspect firms in the full sample are 2.6% more likely to just meet or beat expectations in a particular quarter. For outside-hire CEO backdating firms the marginal effect is 5.9%, and for inside-hire CEOs the marginal effect is only 1.7%. As before, the result does not hold when the backdating is limited to lower-level employees.

4.3. Accruals-based earnings management

One of the primary ways that senior executives can meet or beat earnings targets is by managing their discretionary accruals. We therefore examine whether the higher incidence of just meeting or beating analyst forecasts is associated with increased discretionary accruals in those quarters. We calculate quarterly discretionary accruals consistent with previous literature, as detailed in Appendix B. We investigate discretionary accrual use in a multivariate setting by pooling suspect and matched firm-quarters (in a manner identical to regressions presented in Table 4) and running the following regression:

$$\begin{aligned} Disc.\ accrual_{i,t} = {} & \alpha_0 + \alpha_1 MVE_{i,t-1} + \alpha_2 MTB_{i,t-1} \\ & + \alpha_3 Leverage_{i,t-1} + \alpha_4 ROA_{i,t-1} \\ & + \alpha_5 Firm\ age_{i,t-1} + \alpha_6 IO_{i,t-1} + \alpha_7 Board\ ind_{i,t-1} \\ & + \alpha_8 Board\ size_{i,t-1} + \alpha_9 PYGrant_{i,t-1} \\ & + \alpha_{10} CEOOwn_{i,t-1} + \alpha_{11} CEOOwn^2_{i,t-1} + \alpha_{12} BEAT_{i,t} \\ & + \alpha_{13} Suspect{*}BEAT_{i,t} + X_i + \varepsilon_{i,t}, \end{aligned} \quad (1)$$

where *i* and *t* index the firm and quarter. The dependent variable, *Disc. accrual*, is the signed level of discretionary accruals obtained using the modified version of the Jones (1991) model as implemented by Yu (2008). Independent control variables are defined as before, and *X* is a vector of firm, quarter, and auditor fixed effects. *BEAT* is an indicator variable that equals one if the earnings surprise is equal to 0¢, +1¢, or +2¢. Our primary variable of interest is the interaction of *BEAT* with *Suspect*

Table 4
Multivariate analysis of earnings surprises.

This table reports the coefficient estimates of a logit regression of *BEAT* on independent variables that control for firm and CEO characteristics. The sample of observations includes earnings announcements from January 1990 to December 2009 in firm-quarters for our sample of suspect firms as well as a sample of control firms. The sample selection and matching procedures are consistent with those outlined in Table 3. For both the suspect and matched samples we obtain earnings announcements and analysts' forecasts from the IBES unadjusted summary files and define earnings surprise as the actual earnings announced minus the mean analyst forecast from IBES. We construct an indicator variable *BEAT* if the earnings surprise for a firm quarter is 0¢, 1¢, or 2¢. Independent variables *MVE* and *Firm age* are obtained from CRSP; *Market-to-book*, *Leverage*, and *Return on assets* are obtained from Compustat; *Institutional ownership* is obtained from Thomson Financial 13F filings; and *Board independence*, *Board size*, and *CEO ownership* are obtained from Compact Disclosure. *Prior year grants* is the number of options granted by the firm to the CEO as reported in Thomson. *Suspect* is an indicator variable that equals one if the firm has been classified as suspect. All regressions include industry, auditor, and individual quarter fixed effects. The table also separately presents statistics for suspect CEOs that were outside hires and those that were internal hires. Numbers presented in parentheses are *p*-values calculated using standard errors clustered at the firm level.

	CEO sample	CEO sample		Non-mgmt sample
		Outside CEOs	Inside CEOs	
Independent variables				
Constant	−2.366**	−3.176***	−2.678**	−0.798*
	(0.030)	(< 0.001)	(0.022)	(0.071)
Ln(MVE)	0.126***	0.140***	0.100***	0.091***
	(< 0.001)	(< 0.001)	(0.00104)	(< 0.001)
Ln(market-to-book)	0.145***	0.130**	0.162***	0.183***
	(< 0.001)	(0.017)	(< 0.001)	(< 0.001)
Leverage	−0.225	−0.121	−0.196	−0.223
	(0.117)	(0.597)	(0.247)	(0.183)
Return on assets	3.055***	4.020***	2.637***	2.397***
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)
Firm age	−0.120***	−0.092**	−0.131***	−0.114***
	(< 0.001)	(0.021)	(< 0.001)	(< 0.001)
Ln(inst. ownership)	0.184	0.186	0.167	0.207
	(0.134)	(0.328)	(0.240)	(0.137)
Ln(board independence)	0.171	−0.386	0.328	−0.298
	(0.459)	(0.264)	(0.220)	(0.272)
Ln(board size)	−0.052	0.045	−0.084	0.029
	(0.518)	(0.720)	(0.357)	(0.752)
Prior year grants	0.0014***	0.0007*	0.0015***	0.0009**
	(0.003)	(0.059)	(0.008)	(0.042)
CEO ownership	0.464	0.710	0.064	0.447
	(0.390)	(0.432)	(0.915)	(0.489)
CEO ownership^2	−0.545	−1.347	0.226	−0.056
	(0.650)	(0.464)	(0.861)	(0.962)
Suspect	0.117*	0.243**	0.073	0.042
	(0.067)	(0.028)	(0.345)	(0.595)
Industry fixed effects	Yes	Yes	Yes	Yes
Auditor fixed effects	Yes	Yes	Yes	Yes
Year/Qtr. fixed effects	Yes	Yes	Yes	Yes
# Observations	20,669	6,229	15,622	16,483
Psuedo-R^2	0.0629	0.0668	0.0669	0.0495

*Denotes significance at the 10% level,
**denotes significance at the 5% level,
***denotes significance at the 1% level.

(*BEAT***Suspect*), where *Suspect* is an indicator variable that equals one for firms with a suspect CEO. Because our regression specification includes firm fixed effects, we are unable to include *Suspect* as a stand-alone independent variable.

These regressions are presented in Table 5. For the control variables, we find consistently positive coefficients on market-to-book and return on assets, and consistently negative coefficients on MVE and leverage. Discretionary accruals are also positively related to the number of option grants in the previous years, consistent with a positive relationship between incentives and earnings management.

The coefficient of interest is the one on the interaction term, *Suspect***BEAT*. This coefficient is positive but just shy of being significant in the regression with all backdating CEOs (coeff.=0.0029; *p*-value=0.140). When only analyzing suspect firms with outside-hire CEOs, the coefficient is positive, large in magnitude compared to the other specifications, and highly significant (coeff.=0.0090; *p*-value=0.005). The magnitude of this coefficient estimate indicates that outside-hire CEO firms use discretionary accruals that are incrementally larger, by just under 1% of the value of firm assets, in the quarters that they narrowly meet or beat analysts' expectations. Our result is highly significant both in statistical and economic terms. In contrast, the interaction variable of interest is essentially zero in the regressions with either inside-hire backdating CEOs or Non-mgmt suspect firms.

Another interesting pattern in these regressions concerns the coefficient on the stand-alone variable *BEAT*. In all specifications other than the one focusing on outside-hire

CEOs, this coefficient is significant and positive, indicating that firms use positive discretionary accruals in the quarters when they just attain earnings expectations. However, the coefficient estimate is essentially zero in the regression that only includes outside-hire CEO suspect firms and their closely matched control firms. Although we cannot make a strong assertion here, this is a pattern we would expect if all bad-actor CEOs at these firms were outside hires, and all bad actors during this period also engaged in backdating.

4.4. Acquisitions by suspect firms

Thus far we provide evidence that firms with suspect CEOs are more likely to engage in financial frauds and have lower quality financial reporting. In this section we consider whether these firms are more likely to engage in real corporate activities from which executives may benefit, but that could be inconsistent with shareholders' interests. In particular, we analyze their acquisition activity, which is one of the largest and most easily observed forms of corporate investment.[25] Not only can excessive acquisition activity reflect a manager's selfish empire-building (Jensen, 1986), but it could also facilitate earnings manipulations that contribute to a broader financial fraud (Schilit and Perler, 2010).

We analyze completed acquisitions of both public and private targets of greater than $5 million that result in the acquirer owning 100% of the target. Similar to our previous analyses, we compare the acquisition activities of suspect firms to that of up to five other firms in the same SIC3 that are closest in size (within 50%). Our tests in this section focus on the frequency of acquisition activity as well as the three-day cumulative abnormal returns (CARs) surrounding the acquisition announcement dates.[26]

The univariate results for acquisition activity and abnormal returns surrounding their announcement are presented in Table 6. Firms with backdating CEOs are more likely to acquire other firms. Suspect CEO firms have an 18.8% chance of completing at least one acquisition in a given firm-year versus 15.0% for control firms (p-value of difference = 0.004.). Interestingly, these suspect firms are more likely to acquire a private company (14.9% versus 11.1%; p-value of difference = 0.001), but are not more likely to acquire a public company (5.3% versus 5.0%). This contrast is strongest when considering firms led by CEOs hired externally, where suspect firms had a 17.9% chance of acquiring a private company in a given year compared to an 11.1% chance for a control firm. For the suspect firms led by CEOs hired internally, these percentages are 13.9% versus 11.3% (p-value of difference = 0.054). Similar patterns continue to hold in unreported multivariate analyses of the probability of conducting an acquisition that also incorporate all of the control variables discussed previously.

The contrasting pattern across public and private firm acquisitions is interesting. Due to the opaque nature of private firms, executives could focus on private targets for reasons that are inconsistent with shareholders' interests. For example, executives who intend to use target assets for their private benefits could accomplish this by targeting companies where investors do not have a good understanding of the nature and quality of firm assets. In addition, if an acquisition is at least partially motivated by earnings management flexibility, it would certainly be easier to manipulate the earnings of a firm that has not yet made their financial statements publicly available.[27]

We also provide a comparison of the three-day bidder CARs surrounding the announcement of acquisitions in Table 6. When acquisitions of private and public companies are pooled, there is some evidence of lower announcement returns by firms with backdating CEOs. Consistent with previous research, acquisitions of public targets are met with negative average CARs in every sample. The CARs are more negative for acquisitions by backdating CEO firms when compared to control firms (−0.52% diff.) and this difference is exacerbated in the outside CEO sample (−3.5% diff.). Although these differences are large in economic terms, they are not statistically significant. The lack of statistical significance is likely a function of the small number of public acquisition events, particularly for the outside CEO sample.

Interesting patterns also emerge when looking at private acquisitions. Prior literature shows that bidder CARs are positive, on average, among private acquisition announcements (Fuller, Netter, and Stegemoller, 2002). This pattern holds for control firms but not for suspect CEO firms, where the average CARs are insignificantly different from zero. Announcement CARs are −1.36% lower for suspect CEO firms when compared to control firms (p-value = 0.007) and again this difference becomes economically larger for the outside CEO sample (−2.14%, p-value = 0.049). The magnitude of this difference is more than twice as large as that found in the inside-hire CEO sample (−0.99% diff., p-value = 0.062) and results for the Non-mgmt sample show no differences between the CARs of suspect and control firms.

In Table 7 we evaluate these merger announcement returns in a multivariate framework. Our regressions include standard controls for firm characteristics as well as industry and year fixed effects. In addition, we control for method of payment, relative deal size, and whether the target is a public or private firm (Fuller, Netter, and Stegemoller, 2002). As in previous regressions, our primary variable of interest is the indicator variable *Suspect*. Our results highlight the differential CARs associated with acquisitions by suspect firms with an outside-hire CEO. Acquisitions by outside-hire suspects are associated with a −3.41% lower CAR than acquisitions by control firms

[25] Numerous authors identify reasons why executives could engage in empire-building mergers that are not value-maximizing for shareholders (Jensen, 1986; Lang, Stulz, and Walkling, 1991; Morck, Shleifer, and Vishny, 1990). More recent studies find that monitoring by outside blockholders and stronger shareholder rights can mitigate the agency costs associated with acquisition decisions (Chen, Harford, and Li, 2007; Masulis, Wang, and Xie, 2007).

[26] The cumulative abnormal return for each acquisition announcement is calculated by adding the daily difference between the acquirer return and the return of the CRSP size decile for the three days centered on the acquisition announcement.

[27] According to Aaron Beam, the original CFO of HealthSouth, that company's acquisitions were at least in part motivated by these considerations during their fraud period.

Table 5
Multivariate analysis of discretionary accruals.

This table reports the coefficient estimates of a multivariate regression of the value of discretionary accruals on independent variables that control for firm and executive characteristics. The sample of observations includes earnings announcements from January 1990 to December 2009 in firm-quarters for our sample of suspect firms as well as a sample of control firms. The sample selection and matching procedures are consistent with those outlined in Table 3. For both suspect and matched sample firm-quarters, we calculate discretionary accruals using a modified version of the Jones (1991) model. For both the suspect and matched samples we obtain earnings announcements and analysts' forecasts from the IBES unadjusted summary files and define earnings surprise as the actual earnings announced minus the mean analyst forecast from IBES. Independent variables *MVE* and *Firm age* are obtained from CRSP; *Market-to-book*, *Leverage*, and *Return on assets* are obtained from Compustat; *Institutional ownership* is obtained from Thomson Financial 13F filings; and *Board independence*, *Board size*, and *CEO ownership* are obtained from Compact Disclosure. *Prior year grants* is the number of options granted by the firm to the CEO as reported in Thomson. *Suspect* is an indicator variable that equals one if the firm has been classified as suspect, *BEAT* is an indicator variable that equals one if the earnings surprise for a firm quarter is 0¢, 1¢, or 2¢, and *Suspect*BEAT* is an interaction variable between *Suspect* and *BEAT*. All regressions include firm, auditor, and individual quarter fixed effects. The table also separately presents statistics for suspect CEOs that were outside hires and those that were internal hires. Numbers presented in parentheses are *p*-values calculated using standard errors clustered at the firm level.

	CEO sample	CEO sample		Non-mgmt sample
		Outside CEOs	Inside CEOs	
Independent variables				
Constant	−0.0212**	−0.0076	−0.0258**	0.0669***
	(0.033)	(0.778)	(0.019)	(<0.001)
Ln(MVE)	−0.0043***	−0.0030	−0.0045***	−0.0038***
	(<0.001)	(0.237)	(0.001)	(0.003)
Ln(market-to-book)	0.0124***	0.0096***	0.0143***	0.0118***
	(<0.001)	(0.001)	(<0.001)	(<0.001)
Leverage	−0.0232***	−0.0274**	−0.0242***	−0.0291***
	(<0.001)	(0.012)	(<0.001)	(<0.001)
Return on assets	0.0213**	0.0383*	0.0146	0.0195**
	(0.011)	(0.075)	(0.164)	(0.016)
Firm age	0.0011	0.0003	0.0010	0.0070***
	(0.513)	(0.921)	(0.600)	(<0.001)
Ln(inst. ownership)	−0.0002	0.0016	0.0004	−0.0126**
	(0.971)	(0.840)	(0.936)	(0.015)
Ln(board independence)	0.0029	0.0075	0.00007	−0.0049
	(0.635)	(0.585)	(0.992)	(0.517)
Ln(board size)	0.0012	0.0024	0.0015	0.0052*
	(0.526)	(0.584)	(0.492)	(0.068)
Prior year grants	0.00002**	0.00001***	0.00002*	0.000008
	(0.018)	(0.008)	(0.092)	(0.605)
CEO ownership	0.0071	0.0183	0.0105	0.0025
	(0.704)	(0.600)	(0.604)	(0.913)
CEO ownership^2	−0.0312	−0.0121	−0.0476	0.0139
	(0.474)	(0.844)	(0.333)	(0.780)
BEAT	0.0041***	−0.0008	0.0053***	0.0048***
	(<0.001)	(0.676)	(<0.001)	(<0.001)
*Suspect*BEAT*	0.0029	0.0090***	0.0014	0.00001
	(0.140)	(0.005)	(0.557)	(0.994)
Firm fixed effects	Yes	Yes	Yes	Yes
Auditor fixed effects	Yes	Yes	Yes	Yes
Year/Qtr. fixed effects	Yes	Yes	Yes	Yes
# Observations	20,692	6,242	15,643	16,502
Psuedo-R^2	0.026	0.032	0.034	0.030
# Unique firms	2,508	1,419	2,086	1,978

*Denotes significance at the 10% level,
**denotes significance at the 5% level,
***denotes significance at the 1% level.

(*p*-value=0.015). In contrast, the coefficient on *Suspect* is an insignificant −0.53% in the regression with only inside-hire suspect CEOs. The final regression involving non-management suspect firms provides continued evidence of an insignificant difference between the announcement CARs of these firms and control firms (−0.07%, *p*-value=0.92).

4.5. Difference-in-differences analyses

Our findings thus far indicate that firms with CEOs that benefit directly from options backdating are associated with other corporate misbehaviors. In addition, these abnormal activities are more prevalent in the sample of firms that hire suspect CEOs from the outside. However, such correlations do not necessarily indicate a causal link. It is possible that firms engaging in these questionable practices are more likely to attract executives with a suspect character, or that existing firm culture influences executive actions.

We provide preliminary evidence of a causal relationship in Table 2, when we find that firms that hire suspect CEOs from the outside are more likely to initiate a financial fraud. Since the FSR database of financial frauds contains only ten firms in which more than one fraud event is present, our evidence is suggestive of a change in corporate behavior after the suspect CEO gets to her firm. To more rigorously test the causal nature of these changes for financial reporting and investment activities, we employ

Table 6
Univariate analysis of acquisitions.

This table presents a univariate analysis of acquisition activity during the period from January 1990 to December 2009 for our sample of firms classified as suspect and a control sample of firms without evidence of option backdating activity. Sample selection is consistent with the method reported in Table 3. Matching between suspect and non-suspect samples is based on year, industry (three-digit SIC code), and market value of equity (the difference in MVE between the suspect firm and control firm cannot exceed 50% of the MVE of the suspect firm); up to five control firms are selected for each suspect firm on an annual basis. For both the suspect and matched samples we create an indicator variable that equals one when a firm announces an acquisition during the fiscal year that is later completed with a deal size greater than $5 million. We obtain acquisition announcements from SDC Platinum. The cumulative abnormal return for each acquisition announcement is calculated by adding the daily difference between the acquirer return and the return of the CRSP size decile for the three days centered on the acquisition announcement. We present univariate differences between suspect firms and the matched sample for the proportion of firm-years with acquisitions and the CARs. The table also separately presents statistics for CEOs that were outside hires and those that were internal hires and for private and public targets. Numbers presented in parentheses are *p*-values calculated using standard errors clustered at the firm level.

	CEO sample			CEO sample						Non-management sample		
				Outside CEO sample			Inside CEO sample					
	Suspect	Control	Diff.	Suspect	Control	Diff.	Suspect	Control	Diff.	Suspect	Control	Diff.
Bidder firms												
Compl. acq. during year	0.188	0.150	0.0383***	0.202	0.152	0.050**	0.183	0.152	0.031**	0.162	0.156	0.006
			(0.004)			(0.040)			(0.048)			(0.683)
Comp. private acq. during year	0.149	0.111	0.0383***	0.179	0.111	0.068***	0.139	0.113	0.026*	0.126	0.117	0.008
			(0.001)			(0.002)			(0.054)			(0.462)
Comp. public acq. during year	0.053	0.050	0.003	0.043	0.052	−0.009	0.056	0.0498	0.006	0.049	0.052	−0.002
			(0.649)			(0.444)			(0.459)			(0.783)
Avg. # of firms acq. per year	0.267	0.212	0.055**	0.287	0.211	0.076*	0.260	0.216	0.044*	0.242	0.223	0.019
			(0.015)			(0.071)			(0.092)			(0.492)
CARs (Bidder firms)												
All acquisitions	−0.552	0.360	−0.912**	−1.670*	0.033	−1.703	−0.147	0.453	−0.600	0.213	0.090	0.123
	(0.126)	(0.155)	(0.038)	(0.080)	(0.948)	(0.108)	(0.664)	(0.106)	(0.170)	(0.603)	(0.747)	(0.803)
# Of acq.	534	1,555		142	452		392	1,211		407	1,316	
Public targets	−2.516***	−1.992***	−0.524	−6.184*	−2.685***	−3.500	−1.704***	−1.729***	0.025	−2.952***	−1.978***	−0.974
	(0.002)	(< 0.001)	(0.545)	(0.061)	(< 0.001)	(0.261)	(0.009)	(< 0.001)	(0.974)	(< 0.001)	(< 0.001)	(0.284)
# Of acq.	127	483		23	147		104	367		117	407	
Private targets	0.061	1.419***	−1.358***	−0.798	1.342**	−2.140**	0.415	1.402***	−0.987*	1.490***	1.016***	0.474
	(0.875)	(< 0.001)	(0.007)	(0.379)	(0.030)	(0.049)	(0.295)	(< 0.001)	(0.0622)	(0.002)	(0.004)	(0.419)
# Of acq.	407	1072		119	305		288	1,132		290	909	

*Denotes significance at the 10% level,
**denotes significance at the 5% level,
***denotes significance at the 1% level.

Table 7
Multivariate analysis of acquisition CARs.

This table presents multivariate regressions of the announcement returns to acquisitions on independent variables that control for firm, executive, and deal characteristics. Acquisition announcements are limited to those between January 1990 to December 2009 where the acquisition was completed and the reported value was greater than $5 million. Sample selection is consistent with the method reported in Table 3. Matching between suspect and non-suspect samples is based on year, industry (three-digit SIC code), and market value of equity; up to five control firms are selected for each suspect firm on an annual basis. The dependent variable is the three-day cumulative abnormal return of the bidder firm around the announcement of an acquisition. The cumulative abnormal return for each acquisition announcement is calculated by adding the daily difference between the acquirer return and the return of the CRSP size decile for the three days centered on the acquisition announcement. Independent variables *MVE* and *Firm age* are obtained from CRSP; *Market-to-book*, *Leverage*, and *Return on assets* are obtained from Compustat; *Institutional ownership* is obtained from Thomson Financial 13F filings; and *Board independence* and *Board size* are obtained from Compact Disclosure. *All cash* is an indicator variable that equals one if 100% of the purchase price was paid in cash. *All stock* is an indicator variable that equals one if 100% of the purchase price was paid with stock. *Suspect* is an indicator variable that equals one if the firm has been classified as suspect. All regressions include industry and year fixed effects. The table separately presents statistics for suspect CEOs that were outside hires and those that were internal hires. Numbers presented in parentheses are *p*-values calculated using standard errors clustered at the firm level.

	CEO sample	CEO sample		Non-mgmt sample
		Outside CEOs	Inside CEOs	
Independent variables				
Constant	−0.0126	−0.0867*	0.0292	0.1020
	(0.760)	(0.065)	(0.581)	(0.238)
Ln(MVE)	−0.0054**	−0.0074	−0.0063**	0.0018
	(0.048)	(0.255)	(0.042)	(0.631)
Ln(market-to-book)	−0.0078	0.0041	−0.0084	−0.0207***
	(0.191)	(0.726)	(0.182)	(0.002)
Leverage	−0.0200	−0.0153	−0.0416*	0.0068
	(0.249)	(0.695)	(0.064)	(0.775)
Return on assets	−0.0096	−0.0267	−0.00006	−0.0190
	(0.564)	(0.497)	(0.997)	(0.359)
Firm age	0.0033	0.0082	0.0015	−0.0044
	(0.391)	(0.248)	(0.722)	(0.382)
Ln(inst. ownership)	−0.0195	−0.0419	−0.0165	−0.0290*
	(0.159)	(0.123)	(0.322)	(0.069)
Ln(board ind.)	−0.0315	0.0433	−0.0374	0.0097
	(0.369)	(0.495)	(0.355)	(0.723)
Ln(board size)	0.0081	−0.0018	0.0090	0.0071
	(0.405)	(0.931)	(0.400)	(0.450)
All cash	0.0178***	0.0204	0.0168**	0.0082
	(0.009)	(0.187)	(0.022)	(0.247)
All stock	0.0064	−0.0034	0.0083	−0.0226**
	(0.390)	(0.816)	(0.333)	(0.026)
Ln(relative deal size)	−0.0328*	−0.0670*	−0.0191	0.0361
	(0.055)	(0.092)	(0.330)	(0.462)
Private acquisition	0.0294***	0.0439***	0.0252***	0.0392***
	(<0.001)	(0.009)	(0.002)	(<0.001)
Suspect	−0.0124**	−0.0341**	−0.0053	−0.0007
	(0.044)	(0.015)	(0.436)	(0.915)
Industry fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
# Observations	1,385	412	1,053	1,061
Psuedo-R^2	0.106	0.158	0.117	0.171

*Denotes significance at the 10% level,
**denotes significance at the 5% level,
***denotes significance at the 1% level.

difference-in-differences tests around the hiring of suspect CEOs.

We begin by constructing a sample of suspect CEO transitions, where the transition year is the first year that a suspect CEO appears in that role with her firm. We collect firm characteristics from Compustat and CRSP for five years before and after the transition year and require a firm to have available data for at least two years before and after the transition year to be included in the sample. We collect all suspect CEO transitions that meet the above criteria for transition years between 1992 and 2002 and collect non-suspect CEO transitions in an identical manner for (non-suspect CEO) Standard and Poor's (S&P) 1500 firms in the same three-digit SIC industries as our sample of suspect CEO transitions.[28] We identify 65 suspect CEO transitions (21 outside hires and 44 inside hires) and 539 non-suspect CEO transitions at other S&P 1500 firms that meet our data criteria during the period.

[28] The 1992–2002 sample period that we use to collect suspect CEO transitions is determined by the availability of option backdating data that we use to classify our suspect CEO sample.

4.5.1. Difference-in-differences in earnings surprises

Our first set of difference-in-differences tests focus on earnings management practices. We analyze changes in earnings surprises (*BEAT*) from the five years before a suspect CEO arrives at the firm (years $t-5$ to $t-1$) to five years after a suspect CEO arrives (years $t+1$ to $t+5$). We exclude the transition year (year t) because of anomalies often associated with CEO succession (Huang and Kisgen, 2013; Chatterjee and Hambrick, 2007). We proceed by running the following logit regression:

$$\begin{aligned} BEAT_{i,t} = {} & \alpha_0 + \alpha_1 MVE_{i,t-1} + \alpha_2 MTB_{i,t-1} + \alpha_3 Leverage_{i,t-1} \\ & + \alpha_4 ROA_{i,t-1} + \alpha_5 PYGrant_{i,t-1} + \alpha_6 CEOOwn_{i,t-1} \\ & + \alpha_7 CEOOwn^2_{i,t-1} + \alpha_8 Suspect_{i,t} + \alpha_9 Post_{i,t} \\ & + \alpha_{10} Suspect_{i,t} * \alpha_9 Post_{i,t} + X_i + \varepsilon_{i,t}, \end{aligned} \quad (2)$$

where i and t index the firm and quarter of observation. Independent variables *MVE*, *MTB*, *Leverage*, *ROA*, prior year option grants (*PYGrant*), and CEO ownership (*CEOOwn*) are defined as before, and X is a vector of industry and individual quarter fixed effects. Our difference-in-differences regression also includes the variables *Suspect*, *Post*, and *Suspect*Post*. *Post* is an indicator variable that equals one for quarters following a new CEO's arrival at the firm. Our primary variable of interest is the interaction *Suspect*Post*, where *Suspect* is a time-invariant indicator variable that equals one for firms with a suspect CEO transition.[29] If the arrival of a suspect CEO is associated with more earnings management of this type, we would expect a positive and significant coefficient on *Suspect*Post*.

The results are presented in Table 8 for the full sample of suspect CEO transitions as well as for outside hires and inside hires separately. Since firms that hire their CEOs from the outside are likely to differ systematically from those that elevate their new CEOs from within (Weisbach, 1995; Huson, Malatesta, and Parrino, 2004; Ang and Nagel, 2011), we pool outside-hire suspect CEO transitions only with control CEO transitions in which the CEO was also identified as an outside hire (we construct an analogous sample for inside-hire regressions). Pooling outside and inside hires separately alleviates concerns that any observed differences are due to systematic differences in firms' unobserved motivations for CEO replacement. We find that the coefficient on *Suspect*Post* is positive but insignificant in the full CEO sample (0.136; p-value = 0.477). However, it is positive and significant in the outside-hire CEO sample (0.527; p-value = 0.020), and insignificant in the inside-hire sample (-0.0271; p-value = 0.917). The marginal effect associated with *Suspect*Post* in the outside-hire sample regression indicates an 11% increase in the odds of just meeting analysts' earnings expectations in a given quarter. Our difference-in-differences tests are consistent with the contemporaneous earnings management tests in Tables 3 and 4.

[29] In alternate specifications we include firm fixed effects (and exclude *Suspect*, a time-invariant firm control, in the regression). All results are quantitatively similar.

4.5.2. Difference-in-differences in acquisition activity

We next provide evidence on the direction of causality between suspect CEOs and costly acquisitions. Table 9 presents difference-in-differences logit regressions identifying the change in probability of completing acquisitions from the five-year period before a suspect CEO arrives at the firm to the five-year period after suspect CEOs are hired. The suspect samples are limited to those for which we have Compustat data for at least two years before and after the suspect CEO transition, which includes 50 inside hires and 23 outside hires.[30] Control CEO transitions are constructed in an identical manner as those in Table 8, and consistent with Table 8 regressions we pool outside- (inside-) hire suspect CEO transitions only with control CEO transitions that are also outside (inside) hires. In addition, we run separate regressions for public and private acquisition activity. Our results indicate that there is an increase in the probability of completing an acquisition for the full sample of CEO transitions and that this effect is concentrated in the outside-hire suspect CEO sample. However, this increased propensity is only evident when the target is private (coefficient of 0.733 on *Suspect*PostYear* in the outside-hire/private target regression; p-value = 0.035). This translates to a marginal increase in the probability of an acquisition of 1.7% in any given quarter, which is a 62% increase relative to the unconditional odds of a private acquisition of 2.73%.

These limited tests are consistent with the conclusion that hiring an external CEO with low character can lead firms to adopt aggressive acquisition strategies, possibly for purposes other than shareholder value-maximization. We point out, however, that inference for both of our difference-in-differences tests is limited to the small number of CEO transitions that we are able to analyze.

4.6. Additional evidence on the consequences of hiring suspect CEOs

Given the evidence of corporate misbehaviors at firms in which the CEO benefits from backdating, we next examine whether these actions culminate in costly consequences. If suspect firms engage in financial frauds and/or make value-destroying acquisitions, then they are likely to eventually suffer large losses when their true quality becomes apparent. Consistent with our conjecture, in Section 4.1 we show that suspect firms are more likely to be named in a shareholder lawsuit. In addition, if these managers are indeed bad actors, then they could be more likely to lose their jobs when their type is eventually revealed.

4.6.1. Large shareholder losses

It has been asserted that economic booms encourage and conceal corporate fraud, which are subsequently revealed during ensuing bust periods (Galbraith, 1954; Gokcekus and Suzuki, 2011). We explore whether suspect firms are more likely to experience severe losses in market capitalization during the economic slowdown that followed

[30] The difference in sample size between the sample of suspect CEO transitions analyzed in Table 8 and that analyzed in Table 9 is driven by the requirement in Table 8 that suspect CEO firms have available information in IBES.

Table 8
Multivariate analysis of earnings surprises: Difference-in-differences.

This table reports the coefficient estimates of a logit regression of *BEAT* on independent variables that control for firm and executive characteristics. The sample of observations includes earnings announcements in firm-quarters around suspect CEO transitions and a matched sample of firm-quarters around CEO transitions identified in Execucomp. CEO transitions during the period from January 1992 to December 2002 are included. Firm-quarters are included in the sample if they fall into a five-year period before a CEO arrived at the firm or during a five-year period following their arrival (we exclude quarters from the year the CEO arrived). Firms without data available in at least the two years closest to the transition year in both pre- and post-CEO arrival periods are excluded. Suspect CEOs are identified using likely backdated option grants and exercises as described in Table 3. Control firms are limited to those with CEO turnovers from the same industries (SIC3) as represented by our suspect turnovers. Suspect executive transitions are classified as outside or inside hires based on the biographical information found in SEC filings and executive transitions from Execucomp are classified based on the date of the earliest transaction reported in Thomson. Executives who have been with the firm for longer than one year at the time they are promoted are classified as inside hires and those with less than one year of service prior to promotion are classified as outside hires. For both the suspect and matched samples we obtain earnings announcements and analysts' forecasts from the IBES unadjusted summary files and define earnings surprise as the actual earnings announced minus the mean analyst forecast from IBES. We construct an indicator variable *BEAT* if the earnings surprise for a firm year is 0¢, 1¢, or 2¢. Independent variables *MVE* are obtained from CRSP; *Market-to-book*, *Leverage*, and *Return on assets* are obtained from Compustat. *Suspect* is a time-invariant indicator variable that equals one for firms with a suspect CEO transition. *Post* is an indicator variable that equals one if the firm-quarter occurs in the five years after a CEO transition. *Suspect*Post* is an interaction variable of *Suspect* and *Post*. All regressions include industry and individual quarter fixed effects. Numbers presented in parentheses are *p*-values calculated using standard errors clustered at the firm level.

	Dependent variable = 1 if just meet or beat [0¢ to 2¢]		
	CEO sample	Outside CEOs	Inside CEOs
Independent variables			
Constant	−2.849***	−2.796**	−2.978***
	(<0.001)	(0.036)	(<0.001)
Ln(MVE)	0.0743***	−0.00474	0.0943***
	(0.002)	(0.904)	(0.001)
Ln(market-to-book)	0.190***	0.234***	0.193***
	(<0.001)	(0.004)	(0.002)
Leverage	−0.310	−0.583*	−0.0739
	(0.173)	(0.0953)	(0.804)
Return on assets	5.107***	5.974***	4.264***
	(<0.001)	(<0.001)	(<0.001)
Prior year grants	0.0023**	0.0033***	0.0014*
	(0.017)	(<0.001)	(0.056)
CEO ownership	1.903	2.450	2.710*
	(0.133)	(0.357)	(0.092)
CEO ownership^2	−6.384***	−7.791*	−7.491*
	(0.010)	(0.071)	(0.060)
Suspect	0.159	−0.303	0.249
	(0.379)	(0.163)	(0.287)
Post	−0.0269	−0.189	0.0170
	(0.699)	(0.182)	(0.834)
*Suspect*Post*	0.136	0.527**	−0.0271
	(0.477)	(0.020)	(0.917)
Industry fixed effects	Yes	Yes	Yes
Year/Quarter fixed effects	Yes	Yes	Yes
# Observations	20,260	5,901	14,336
Pseudo-R^2	0.0996	0.119	0.112

*Denotes significance at the 10% level,
**denotes significance at the 5% level,
***denotes significance at the 1% level.

the stock market "bubble" of the late 1990s. To test our prediction we conduct logit regressions predicting a severe stock price decline during the market correction of 2001–2002. The dependent variable is *Large loss*, an indicator that equals one if a firm experienced a large stock market loss (greater than either −40% or −50%). The suspect sample is limited to firms with a backdating CEO in office during the year 2000, and control firms are selected as before based on size and industry at the end of 2000. Control variables include firm characteristics that are incorporated in earlier analyses, measured as of the firm's fiscal year-end during the calendar year 2000, as well as additional controls for the level of past returns (returns during 1999 and 2000), the standard deviation of past returns, the level of analyst coverage, abnormal stock turnover, and stock liquidity. Our primary independent variables of interest are *Suspect*, an indicator variable that equals one for our suspect firms, and *Suspect CEO (outside)*, an incremental indicator variable that equals one if a suspect CEO was hired from the outside.

In untabulated results we find that the coefficient on *Suspect* is negative and insignificant and the coefficient on *Suspect CEO (outside)* is positive and significant at the 5% level. In the regression predicting a 40% loss, the coefficient on *Suspect CEO (outside)* is 0.982 (p-value = 0.044) indicating a marginal increase in the probability of a large loss of 24.5% at suspect firms with an outside-hire CEO. It appears that the shareholders' probability of experiencing large negative wealth shocks is greatly increased for firms that hire a bad-actor CEO from the outside.

4.6.2. Suspect CEO replacement

Our final test examines whether CEOs who personally benefit from backdating are more likely to be fired. We focus on CEO replacements during the period from 1996 to 2005, so that we identify decisions made before options backdating practices were brought to light by academic research and media attention. We again implement logit regressions predicting that a CEO is fired in a given year. A CEO is considered to have been fired if he is less than 65 years old at departure and there is evidence consistent with termination in the firm SEC filings and/or popular press. Control observations for these regressions are taken from Execucomp because it is possible to identify CEO transitions in this data set, which covers approximately the S&P 1500 firms each year. The late 1990s was a period when there was an elevated level of corporate fraud, and these frauds were more likely to come to light in the early 2000s. We control for these different time periods to determine whether CEOs who are bad actors were successful at keeping their jobs during the bubble period, and whether they were more likely to then be fired in the ensuing years. In addition to the standard control variables we use throughout this paper, we also include the length of a CEO's tenure and the current and lagged year stock return in these regressions. Our primary independent variables of interest are again *Suspect* and *Suspect CEO* (*outside*).

Our tests indicate that suspect CEOs who are hired from the outside have a 1.3% greater probability of being fired

Table 9
Multivariate analysis of acquisitions: difference-in-differences.

This table reports the coefficient estimates of a logit regression of acquisition activity on independent variables that control for firm and executive characteristics. The sample of observations includes quarters around suspect CEO transitions and a matched sample of firm-quarters around CEO transitions identified in Execucomp. CEO transitions during the period from January 1992 to December 2002 are included. Observations are included in the sample if they fall into a five-year period before a CEO arrived at the firm or during a five-year period following their arrival (we exclude the year of CEO transition). Firms without data available in at least the two years closest to the transition year in both pre- and post-CEO arrival periods are excluded. Suspect CEOs are identified using likely backdated option grants and exercises as described in Table 3. Control firms are limited to those with CEO turnovers from the same industries (SIC3) as represented by our suspect turnovers. Suspect executive transitions are classified as outside or inside hires based on the biographical information found in SEC filings and executive transitions from Execucomp are classified based on the date of the earliest transaction reported in Thomson. Executives who have been with the firm for longer than one year at the time they are promoted are classified as inside hires and those with less than one year of service prior to promotion are classified as outside hires. For both the suspect and matched samples we obtain acquisition announcements from SDC Platinum. We construct two dummy variables for each firm in the sample to indicate the presence of an acquisition: *Public* is an indicator that equals one when a firm completes an acquisition of a publicly traded firm with a deal size greater than $5 million in a given year and *Private* is an indicator that equals one when a firm completes an acquisition of a private firm with a deal size greater than $5 million in a given year. Independent variables *MVE* are obtained from CRSP; *Market-to-book*, *Leverage*, and *Return on assets* are obtained from Compustat. *Suspect* is a time-invariant indicator variable that equals one for firms with a suspect CEO transition. *Post* is an indicator variable that equals one if the quarter occurs after the CEO transition. *Suspect*∗*Post* is an interaction variable of *Suspect* and *Post*. All regressions include industry and year fixed effects. Numbers presented in parentheses are *p*-values calculated using standard errors clustered at the firm level.

	CEO sample		Outside CEO sample		Inside CEO sample	
	Private	Public	Private	Public	Private	Public
VARIABLES						
Constant	−3.044***	−7.328***	−2.701**	−4.967***	−4.088***	−7.997***
	(<0.001)	(<0.001)	(0.026)	(<0.001)	(<0.001)	(<0.001)
Ln(MVE)	0.100**	0.485***	0.080	0.421***	0.122**	0.518***
	(0.016)	(<0.001)	(0.306)	(<0.001)	(0.014)	(<0.001)
Ln(MTB)	0.061	−0.338***	0.240	−0.112	−0.045	−0.396***
	(0.498)	(0.001)	(0.125)	(0.516)	(0.689)	(0.005)
Leverage	−0.177	−0.535	−0.695	−1.659*	−0.033	0.367
	(0.679)	(0.328)	(0.435)	(0.071)	(0.947)	(0.576)
ROA	2.423	−6.675***	2.463	−6.648**	1.215	−6.499**
	(0.114)	(<0.001)	(0.269)	(0.010)	(0.566)	(0.012)
Post	−0.131	−0.163	−0.172	−0.084	−0.227	−0.176
	(0.365)	(0.244)	(0.571)	(0.761)	(0.163)	(0.299)
Suspect	−0.123	0.071	−0.303	−0.410	−0.025	0.188
	(0.634)	(0.823)	(0.489)	(0.418)	(0.936)	(0.628)
Suspect∗*Post*	0.548*	−0.273	0.733**	−0.257	0.487	−0.337
	(0.062)	(0.471)	(0.035)	(0.737)	(0.210)	(0.463)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19,335	19,110	5,277	4,836	13,009	12,432
R^2	0.068	0.137	0.104	0.174	0.064	0.127

*Denotes significance at the 10% level,
**denotes significance at the 5% level,
***denotes significance at the 1% level.

during the full sample period. Subperiod analyses indicate that the increased probability of outside suspect CEOs being fired is concentrated in the 2002–2005 period. During this period, the coefficient on *Suspect CEO (outside)* is 2.128 (p-value < 0.001), indicating a 1.6% increase in the odds of being fired in the years following the stock market bubble period. This is economically significant as the unconditional probability of being fired during the sample period is 1.55%. Our results suggest that bad-actor CEOs are able to extend their tenures during boom periods, and that it takes a significant economic slowdown to catalyze their replacement. The extent of mismanagement of corporate resources could therefore be magnified during these periods of stock market strength.

5. Conclusion

This paper explores the culture of top executives in a firm, with a focus on determining whether it is the CEO who drives a firm to engage in corporate misbehavior. We identify a group of suspect CEOs with questionable ethics as those that systematically backdate their option grants and/or exercises, and provide evidence that the firms they lead engage in other questionable activities. We show that the firms managed by suspect CEOs are more likely to perpetrate a financial reporting fraud and provide additional evidence that their financial reporting schemes enable them to manipulate reported earnings to meet analysts' expectations. The questionable nature of the actions taken by these firms also shows up in their investment policies. Their acquisitions of other companies are met by lower market responses, and they make more acquisitions of private companies, which could facilitate earnings manipulations. These questionable corporate practices increase after suspect CEOs are hired, suggesting a causal relationship between CEO ethics and firm outcomes.

An interesting pattern that we uncover is that the relationship between a suspect CEO and other forms of corporate malfeasance is most pronounced when the CEO

is an external hire. Our results suggest that externally hired CEOs are more effective in altering the culture of high-level decision makers and effectively changing corporate activities. These findings are consistent with the managerial discretion literature which predicts that externally hired CEOs are likely to enjoy greater discretion in shaping the culture around them.

This work provides evidence that the ethics of corporate leaders is an important determinant of the culture of the firms they manage, consistent with the "upper echelons theory" of corporate behavior first proposed by Hambrick and Mason (1984). Given our findings, we propose some questions for future work. Are some firms at greater risk of hiring a questionable CEO than others? Are there mechanisms available to mitigate this risk? For example, is there a role for executive search firms in helping firms identify executives of high integrity, or can appropriate compensation or employment contract design reduce the losses associated with a poor hiring decision? By gaining additional insights into these questions, we could perhaps help firms make better decisions in the future and help insure against the possibility of further large losses of corporate value.

Appendix A

This appendix investigates officer and director turnover following suspect CEO start dates. We begin our analysis by investigating all suspect CEO transitions that occur after 1990. We then assess whether a CFO, chief operating officer (COO), or director appears for the first time in SEC filings in the year of or year following a suspect CEO start date. Our results are reported below.

	Outside CEO	Inside CEO	*p*-Value of diff
New CFO	46.3%	31.0%	0.0398
New COO	11.9%	9.7%	0.6437
New director	73.1%	58.4%	0.0471
Number of new directors	2.66	1.81	0.0328

Our findings show that a new CFO appears in 46.3% of outside suspect CEO transitions versus 31.0% of inside transitions. Similarly, outside suspect CEOs appoint at least one new director in 73.1% of cases versus 58.4% for inside suspect CEOs.

We further investigate whether new team members hired after CEO replacement are suspect (i.e., systematic backdaters). Specifically, we investigate the association between suspect CEOs and a newly appointed (within the first three years of a suspect CEO's tenure) suspect CFO, director, or member of the C-suite. Our results are presented in the table below.

	Suspect CFO	Suspect director	Suspect in C-suite	Suspect C-suite or director
Inside suspect	0.0559	0.0615	0.0726	0.1229
Outside suspect	0.2222	0.1852	0.2593	0.358
Difference	−0.1664***	−0.1273***	−0.1866***	−0.2351***
p-Value	(<0.0001)	(0.002)	(<0.0001)	(<0.0001)

***Denotes significance at the 1% level.

Following a suspect outside CEO (inside CEO) transition, 22.2% (5.6%) of firms have a new CFO who is independently classified as suspect (*p*-value for difference < 0.001). The results are similar for directors and other members of the C-suite. Following an outside (inside) suspect CEO transition, 18.5% (6.2%) of firms have a new director who is independently classified as suspect, and 25.9% (7.3%) of firms have a member of the C-suite who is independently classified as suspect.

Appendix B

This appendix explains how we calculate discretionary accruals. We calculate total accruals and its subsequent decomposition into discretionary and non-discretionary components using the modified version of the Jones (1991) model as implemented by Yu (2008).

Our analysis is based on discretionary accruals measured at the quarterly level. First, total accruals for a given firm-quarter are defined as earnings before extraordinary items and discontinued operations less operating cash flows. To determine discretionary accruals for each firm and quarter, we first run the following cross-sectional ordinary least squares (OLS) regression in each quarter for all firms in the same industry (i.e., two-digit SIC code) to obtain coefficient estimates for α_1, α_2, and α_3. Such an approach adjusts for changing industry-wide economic conditions that might influence non-discretionary accruals.

$$\frac{TA_{i,t}}{Assets_{i,t-1}} = \hat{\alpha}_1 \frac{1}{Assets_{i,t-1}} + \hat{\alpha}_2 \frac{\Delta Sales_{i,t}}{Assets_{i,t-1}} + \hat{\alpha}_3 \frac{Net\ PPE_{i,t}}{Assets_{i,t-1}} + \varepsilon_{i,t}, \tag{B1}$$

where i and t index the firm and quarter, respectively, *TA* equals the total accruals, *Assets* are the total assets, $\Delta Sales$ is the quarterly change in sales, ΔAR is the change in accounts receivable from the prior quarter, *PPE* is the property, plant, and equipment, and ε is the error term.

We then use the coefficient estimates $\hat{\alpha}_1, \hat{\alpha}_2$ and $\hat{\alpha}_3$ from Eq. (B1) to calculate non-discretionary accruals for each firm-quarter in our sample:

$$NDA_{i,t} = \hat{\alpha}_1 \frac{1}{Assets_{i,t-1}} + \hat{\alpha}_2 \frac{\Delta Sales_{i,t} - \Delta AR_{i,t}}{Assets_{i,t-1}} + \hat{\alpha}_3 \frac{Net\ PPE_{i,t}}{Assets_{i,t-1}}, \tag{B2}$$

where $NDA_{i,t}$ are the non-discretionary accruals for firm i in quarter t, and all other variables are as described earlier. As such, non-discretionary accruals (*NDA*) represent the portion of total accruals that are driven by firm fundamentals and therefore unlikely to be attributed to managerial control. Finally, we obtain our measure of discretionary accruals (*DA*) by deducting *NDA* from total accruals (*TA*): $DA_{i,t} = (TA_{i,t}/Assets_{i,t-1}) - NDA_{i,t}$.

In robustness tests we employ three alternate measures of discretionary accruals as presented by Ecker, Francis, Olsson, and Schipper (2013). In the interest of brevity and because all measures yield similar conclusions, we choose not to tabulate any of the alternative measures.

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