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UNETHICAL CULTURE, SUSPECT CEOS AND CORPORATE MISBEHAVIOR

Lee Biggerstaff
David C. Cicero
Andy Puckett

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ABSTRACT

We show that firms with CEOs who personally benefitted from options backdating were more likely to engage in other forms of corporate misbehavior, suggestive of an unethical corporate culture. These firms were more likely to overstate firm profitability and to engage in less profitable acquisition strategies. The increased level of corporate misbehaviors is concentrated in firms with suspect CEOs who were outside hires, consistent with adverse selection in the market for chief executives. Difference-in-differences tests confirm that the propensity to engage in these activities is significantly increased following the arrival of an outside-hire 'suspect' CEO, suggesting that causation flows from the top executives to the firm. Finally, while these suspect CEOs appear to have avoided market discipline when the market was optimistic, they were more likely to lose their jobs and their firms were more likely to experience dramatic declines in value during the ensuing market correction.

Lee Biggerstaff
Stokely Management Center
Department of Finance
University of Tennessee
Knoxville, TN 37996
lbiggers@utk.edu

Andy Puckett
Stokely Management Center
Department of Finance
University of Tennessee
Knoxville, TN 37996
pucketta@utk.edu

David C. Cicero
University of Alabama
Culverhouse College of Commerce
Economics, Finance & Legal Studies
Tuscaloosa, AL 35487
dccicero@cba.ua.edu

"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

I. 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 has received 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.[1] Of particular importance in the current dialogue is an understanding of (and potential means to 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 thirty employees at Heathsouth 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

[1] The Aspen Institute: Center for Business Education reports that the number of MBA programs that require a course dedicated to societal and/or ethical issues has increased from 34% in 2001 to 79% in 2011.

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., Betrand and Schaor, 2003),[2] 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 may stem from the fact that the ethical values of executives and corporations are difficult to empirically quantify. In this paper, we propose a novel way to identify an unethical pattern of behavior, based on systematic participation in options backdating. We investigate the link between firms with CEOs who benefitted directly from options backdating (hereafter 'suspect' firms) and other corporate misbehaviors and find that suspect firms are more likely to overstate firm profitability and to engage in sub-optimal investment strategies. We also contribute to an understanding of the origins of an unethical culture by showing that these corporate misbehaviors increase following a suspect CEO's arrival at her firm and are concentrated in firms that hired their suspect CEOs from the outside. Our findings suggest that asymmetric information may at times lead to the hiring of a "lemon" CEO through adverse selection and that the damage to firms resulting from such a strategic mistake can be significant. Finally, we explore the consequences of unethical behavior and find, consistent with theory (Povel, Singh, and Winton, 2007), 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 provide support for the upper echelons theory in understanding how an unethical culture prevails and contribute to a broader literature on organizational culture (Kreps, 1990; Hodgson, 1996). Our findings are also related to a literature that investigates the economic

[2] Prior academic research has focused on executive characteristics such as overconfidence (Malmendier and Tate, 2008), political affiliation (Hutton, Jiang, and Kumar, 2010), gender (Huang and Kisgen, 2012), narcissism (Chaterjee and Hambrick,, 2007), personal risk taking (Cain and McKeon, 2011), and personal tax aggressiveness (Chyz, 2011).

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 what can be attributed directly to the costs of the backdating activity. Our results provide more direct support for their proposition that this market response likely reflects the market's expectations about other suspect activities also present at backdating firms.

Our analyses are predicated on the identification of 'suspect' firms as those that have systematically engaged in options backdating for the benefit of their CEO. Options backdating refers to the manipulation of stock option grant or exercise dates (and therefore grant or exercise prices) in order to maximize an individual's eventual payout, without reflecting the magnitude of the compensation on firm financial statements or to the I.R.S. As discussed in greater detail in Section II, options backdating for top executives likely implicates stealth (nefarious) activity undertaken for personal gain and to the detriment of others.[3] As such, it is consistent with selfish moral reasoning (Kohlberg, 1981, 1984) and violates the principal of integrity proposed by Erhard, Jensen and Zaffron (2009, 2010). Inclusion in this group serves as a reasonable indicator of unethical behavior on behalf of the chief officer and allows us to test whether this behavior is associated with an unethical culture.

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 classified as "likely backdated," meaning that they occurred on the most favorable day of the calendar month (Bebchuck, Grinstein, and Peyer, 2010). Using data from 1992 to 2009, we identify 249 backdating CEOs and augment this list with 12 additional CEOs who did not meet our

[3] We present a detailed account of this argument in Section II. In particular, there are dimensions of dishonesty, misrepresentation and personal enrichment associated with options backdating for top executives that may not necessarily be implicated by options backdating for the benefit of non-executives.

identification criteria, but who are specifically named in an enforcement action or backdating settlement. We match our sample of backdating CEO firms (i.e., suspect firms) to a corresponding sample of non-suspect firms based on industry (SIC3) 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. 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 have pointed 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. Our results are robust to several alternate measures of analysts' earnings expectations and continue to 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. Prior studies (Jensen, 1986; Lang, Stultz, and Walkling, 1991; and Morck, Shleifer, and Vishny, 1990) provide evidence that excessive acquisitions (i.e., "empire building") provide numerous pecuniary benefits for bidder firm executives but often damage the welfare of shareholders. We find that suspect firms make significantly more acquisitions and that their acquisition announcements are met with a significantly lower market response. We posit that these acquisitions could be motivated by either selfish empire building or, when the targets are privately held, attempts to acquire opaque assets, the value of which can be manipulated to facilitate earnings management. Interestingly, the excessive acquisitions and earnings management activities that we document are concentrated in

firms that hired their suspect CEOs from the outside, which is consistent with evidence of adverse selection problems in the market for executives.[4]

The results discussed thus far establish a correlation between CEOs that engage in questionable behaviors to enhance their own compensation and other suspect corporate actions. However, correlations do not necessarily indicate a causal link and it is 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, relative to that observed around control-firm CEO transitions. We also continue to find that these results are concentrated in firms whose suspect CEOs were outside hires.

Although it is reasonable to classify options backdating as unethical, it is also conceivable that some firms engaged 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 attract and retain employees in an increasingly competitive labor market. Regardless of the possible co-existence of these motivations, if firms backdated options on behalf of top executives, there was an additional unscrupulous dimension to the practice: it was necessarily 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 annual reports. To allow for a more benign form of option backdating we conduct all of our main tests on a sample of 178 firms where 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

[4] Akerlof (1970) formally modeled the problem of adverse selection in the product and labor markets. Zajac (1990) addresses the adverse selection problem specifically in the market for CEOs and hypothesizes that poor hires are more likely to come from outside the firm.

misbehaviors. This suggests that it was in the practice of backdating *on behalf of* top executives (which, as discussed in Section II, was typically also *at the direction of* those very same individuals) that the unethical intentions that shaped the corporate culture were revealed.[5]

To conclude our empirical analyses, we consider whether suspect CEOs and/or their firms experienced adverse consequences as a result of their actions. As predicted by the theory of Povel, Singh, Winton (2007), our tests indicate that firms with suspect CEOs who are hired from the outside were 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 were 25.4% more likely to experience severe stock price declines of at least -40%. In addition, outside-hire suspect CEOs were significantly more likely to be fired during the post-bubble period. Our results demonstrate the extent of damage that can accompany an ill-conceived executive search.

To illustrate anecdotally the patterns of behavior we uncover, it is instructive to consider the case of HealthSouth and its former CEO, Richard Scrushy.[6] The data strongly suggests Scrushy was involved in backdating: of the ten option grants to Scrushy before the Sarbanes-Oxley Act was implemented, improbably six of them occurred 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

[5] In unreported analyses discussed in Section II, we consider whether the proposed justifiable economic reasons for backdating were major determinants of the decision to engage in backdating for top executives. We fail to find strong evidence of this. We also show that our primary results are unaffected in regressions that allow for these possible explanations for backdating to modify the association between backdating and other corporate misbehaviors.

[6] All of the results in this paper are robust to omitting HealthSouth from the sample.

upon one of the largest financial statement frauds in history. In a surprisingly candid book Healthsouth's original 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 Heathsouth's fraud suggest Scrushy broke the 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 to 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.[7]

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 reveals the empirical results of our study, and Section 5 concludes.

II. Identification

We identify suspect firms as those that systematically engaged in options backdating on behalf of their CEO. In Section II.a. we review the mechanics and characteristics of options backdating, we consider an ethical characterization of backdating in Section II.b., Section II.c. examines potential alternate characterizations of backdating practices, and Section II.d. explores expected differences between inside- and outside-hire suspect CEOs.

II.a. Discussion of Options Backdating

[7] Scrushy was later convicted on criminal charges of bribery, conspiracy and mail fraud in connection with payment of over $500,000 to then Governor Siegelman, allegedly in exchange for a seat on the state's Certificates of Need Review Board for hospitals. He was also found liable in a civil case in 2009 to shareholders of HealthSouth for damages of $2.87B for orchestrating the HealthSouth fraud.

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" in order 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 had been secretly "backdating" option grants in order to effectively lower the strike price of executive options.[8] 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. Heron and Lie (2009) estimate that over 2,000 firms backdated executive option grants during the 1996 to 2002 period. Backdating effectively allowed these firms to disguise in-the-money option grants as at-the-money grants. While Heron and Lee (2007) find that patterns consistent with backdating are significantly attenuated following the stricter reporting requirements that accompanied SOX, other studies find that up to 20% of firms continued their backdating practices by simply disregarding the new reporting requirements (Narayanan and Seyhun, 2008).

Several more recent studies have uncovered 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.

[8] 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).

Alternatively, executives who will immediately sell the shares have a straightforward incentive to exercise their options when prices are high. However, it is unlikely that executives have the ability to backdate option exercises when the underlying shares are sold in the open market since the counterparty would purchase at an artificially elevated price. It is more likely that executives have the ability to backdate option exercises when the underlying shares are sold back to the executive's own company (see Cicero, 2009). 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. As with grant backdating, option exercise backdating activity is reduced in the post-SOX period.[9]

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.[10] 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 theme 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 the backdating practices from other directors and shareholders. One example where SEC lawyers clearly drew this

[9] Prior to the enactment of Sarbanes Oxley, executives had up to 10 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.

[10] For an excellent summary of the legal requirements for adopting equity-based compensation plans, see Capital Markets and Securities Bulletin, Vedder, Price, Kaufman & Kammholz, P.C., July 2003.

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 that often facilitated 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).[11] To the extent that these fact patterns are representative, it appears that influential executives were often the instigators for backdating options and that they didn't necessarily have board or shareholder approval.

II.b. Option Backdating as an Indicator of Unethical Character

Secretive stock option grant backdating allowed firms to give executives valuable in-the-money options while claiming to provide them with less valuable at-the-money options. From the perspective of executives and the firm, this type of backdating allowed 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 would trigger an accounting expense against reported income. Thus, backdating stock option grants allowed firms to report higher earnings.

From the shareholders' perspective there are also important implications for both option grant and exercise backdating. For option grants, any gain that accrues to an executive as a result of backdating comes at the direct expense of shareholders. Furthermore, executives that backdate option

[11] 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

exercises and deliver shares back to their companies create an additional company cash outflow that is pocketed by the executive. 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.[12] Thus, both options grant and exercise backdating allowed executives to increase their wealth at the expense of shareholders.

Court documents reveal that option grant backdating violated both the spirit and letter of accounting and disclosure rules. A key element of the ethical characterization of backdating is that firms did not disclose to investors and legal authorities that they were using it as a means of enhancing compensation.[13] 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 anti-fraud rules, securities laws, tax laws, and constitutes false statements to the SEC.[14] 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, exposed shareholders to additional risks from possible litigation, and loss of reputation and executives. Backdating is also at odds with the definition of integrity recently introduced by Erhard, Jensen, and Zaffron.[15] 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

[12] 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.

[13] 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 was correlated with higher overall levels of compensation, so that it does not appear to have served as a substitute for other forms of remuneration.

[14] 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.

[15] For a systematic development of the integrity model see Erhard, Jensen, and Zaffron (2009). For an abridged version that outlines the key arguments see Erhard, Jensen and Zaffron (2010).

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 it often violated shareholder-approved options plans,[16] it seems reasonably characterized as a failure by firms and executives to honor their word to shareholders and legal authorities.

As discussed previously, 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 were.[17] 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 the top executives (who are often under fire for their outsized pay packages and lack of pay-for-performance requirements), adds to the ethical implications of backdating on 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 may be less acute in these cases. For example, it is possible that backdating to lower-level employees did not even require CEO direction or involvement. Even if the CEO was involved, it is perhaps

[16] 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 violated shareholder approved plans.

[17] Reported compensation is lower than actual because firms report option values as of the reported grant dates, and the apparently at-the-money grants are less valuable than the actual backdated in-the-money grants. The incentives are understated because the options appear to have been granted at-the-money and with a particular vesting schedule based on the backdated grant date; in reality the options were in-the-money and the vesting period is shortened by the amount of the look-back period.

revealing that a backdating scheme was not implemented in a way that would mislead investors about the parameters of named executive compensation, even though the top executives would have gained 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 among firms that backdated 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.

II.c. Alternative Characterizations of Options Backdating

A number of parties – including accused litigants, other academic researchers, and members of the press – have taken 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."[18] In fact, firms do generally have complete discretion over

[18] "Dismissed with Prejudice", Directorship Magazine, December 7, 2012.

setting stock option exercise prices.[19] 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.

Another, albeit less obvious, potential concern is that a firm might falsely report a grant date where the market price corresponds with the option grant exercise price. Concerning this possible exception, we asked Denny Beresford (former chairman of the Financial Accounting Standards Board from 1987-1997 and director on more than six publicly-traded firm boards) whether such a practice would be appropriate. He responded "Absolutely not. Backdating (reporting a false date on option grant documents) is illegal."

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 citing a more flexible interpretation of Accounting Principles Board (APB) Opinion No. 25 in his defense. 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 was clear and the only allowable exception – where grants might

[19] 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 a the time such option is granted." (I.R.C. 422(b)(4)).

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."[20] 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 Dahliwal, 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."[21] 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 argues that the widespread use of backdating suggests it was an accepted business practice,[22] and that the small number and nature of resultant criminal convictions fails to indicate widespread destructive behavior.[23] Although this perspective is not wholly unreasonable, it should be tempered by

[20] This delay does not include final approval (by the board of directors or compensation committee) since the absence of such approvals represent a situation where option terms have not been determined with finality. Letter from SEC Chief Accountant Lawrence Salva; September 19, 2006.

[21] The SEC (Litigation Release 18734) alleged that Symbol Technologies engaged in option exercise backdating. The lack of other enforcement actions by the SEC for exercise backdating was due to the expiration of the 5 year statute of limitations for securities fraud.

[22] Heron and Lie (2009) estimate that approximately 30% of firms engaged in backdating over the period 1996 to 2005. The government's prosecution record was mixed, and only twelve executives have been convicted criminally.

[23] Of note is the conviction of Bruce Karatz of KB Homes, who is otherwise very highly esteemed as a businessman and philanthropist. The sentencing judge rejected prosecutor's request for a 6.5 year prison sentence and instead required Karatz to serve five years of probation. The judge noted that there was no evidence of damage to KB

acknowledgement 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 may 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, Bebchuck, Grinstein and Peyer (2010) show that backdating was 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 was 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 to interpret options backdating as a questionable secretive business practice that firms engaged in, for executives' personal benefit, and at the expense of other firm stakeholders.[24] It is thus arguably unethical, and clearly suspect.

II.d Inside versus Outside Hire Suspect CEOs

Extant literature supports the conjecture that firms are more likely to end up with a CEO of suspect character when they do not have a history of observing that person in the workplace. Akerlof (1970) formally modeled the problem of adverse selection in the product and labor markets, and

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Homes or its shareholders and called the government's sentencing memorandum "mean spirited and beneath this office." (Pfeifer, Los Angeles Times, 2010).

[24] To make this discussion more concrete, a review of one high profile backdating case, that of Kobi Alexander at Comverse, is presented in Appendix I.

Zajac (1990) addresses the adverse selection problem specifically in the market for CEOs and hypothesizes that outside hires are more likely to be bad hires. In this spirit, we conduct all empirical tests separately for CEOs that were hired from outside the firm and those that were internal promotions.

We submit two plausible explanations for why one might expect a weaker association between systematic backdating to the CEO and other forms of corporate misbehavior when the CEO was promoted from within the firm. The first possible explanation is that internally promoted CEOs bear a greater responsibility for the firm's past success and thus a higher proportion of their backdated options may be directed by a board looking to reward the CEO for past performance (e.g., Steve Jobs at Apple). The second possible alternative, as modeled by Akerlof and Kranton (2005), is that internally promoted CEOs have a greater loyalty to the firm.

The model of Akerlof and Kranton (2005) envisions corporate "insiders" as more loyal to their firms and expects 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 interest of the firm and shareholders. In support of these arguments, Taylor, Audia and Gupta (1996) provide evidence that a managers' level of commitment to an organization is a function of the length of their tenure with that organization, and Cappelli and Hamori (2006) show that executives who pursue outside opportunities are less loyal. According to Cappelli and Hamori (2006), executives who move firms (stay put) may have revealed their lack of (high degree of) loyalty to the firm. Based on these arguments, it is reasonable that executives hired from the outside will be less loyal to the organizations they are hired into than those who are elevated internally. As such, the relationship between systematic backdating and larger forms of corporate malfeasance may be mitigated by insiders' greater loyalty.

III. Data

We collect data on stock and option transactions from the Thompson 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.[25] For option grants, we investigate the sample period from January 1, 1992 to December 31, 2009,[26] and for option exercises, we investigate the sample period from August 15, 1996 to December 31, 2009.[27] Our classification scheme identifies backdating firms as those with sufficiently suspicious option event patterns for at least one reporting officer or director. We then separate these suspect firms into more narrow classifications based on whether or not there is evidence of backdating that directly benefitted the CEO.

We begin with the option grant data, which is drawn from Table 1 of the SEC reporting forms. We treat multiple grants to the same individual on the same day as a single observation. Before classifying option grants, we 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.[28] We also limit our sample to those with appropriate cleanse codes as identified by Bebchuck, Grinstein and Peyer (2010).[29] 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

[25] Insiders are required to file Form 3 to report initial beneficial ownership of shares, Form 4 to report changes in beneficial holdings, Form 5 to report annual changes in beneficial ownership, and Form 144 to declare their intention to sell restricted shares.

[26] The beginning of our 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."*

[27] The beginning of our sample period corresponds with the date when data regarding the sale of underlying option shares are first available (Cicero, 2009).

[28] Additional filters are consistent with those imposed by Bebchuk, Grinstein, and Peyer (2010). Scheduled grants include those that occur within in a 3-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 where the strike price differs by more than 1% from the closest CRSP closing price in the 3-day window around the option grant date.

[29] As in Bebchuk, et al (2010) our sample of executive option grants is limited to those with cleanse codes that equal 'R', 'H', or 'C'.

look-back periods. Using this procedure, we identify 18,815 option grants to 16,312 individuals at 3,434 firms as likely backdated.

We next examine option exercises, which are summarized in Table 2 of the SEC reporting forms. We identify a total of 185,660 individual option exercise days after limiting the sample to those with appropriate *derivative codes*, *transcodes*, and *cleanse codes*.[30] 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 from Table 1 of the Thompson Financial Insider Filers Database. 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.

A firm is characterized as 'suspect' if we reasonably determine that at least 1 reporting individual benefitted from systematic backdating. Assuming that grant and exercise dates are

[30] Our sample is limited to those with derivative code equal to ISO (Incentive Stock Option), EMPO (Employee Stock Option), or NONQ (Non-Qualified Options). Transcodes are limited to 'M', 'X', or 'J'. We delete observations with cleanse codes equal to 'S' or 'A'.

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.[31] 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, Dixon Ticonderoga, 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 where the CEO (and often other top managers) benefitted directly from options backdating schemes. Of these CEOs, 178 (69%) were internal promotions, and 80 (31%) were hired externally. In addition, throughout our analysis we contrast our results to those for 172 suspect firms where 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 occurred, 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

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

same criteria discussed above. We repeat this procedure 100 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 Thompson data, this procedure resulted in a distribution of randomly chosen pseudo-suspect CEO firms with a mean of 50 and a standard deviation of 6.9 suspect firms. It appears to be clear that a large fraction of the 249 suspect CEO firms actually identified by our methodology are accurately classified as backdaters.[32]

Empirical tests for the sample of suspect firms where the CEO directly benefited from backdating focus on firm-years during the suspect CEOs' tenure. We obtain CEO tenure using annual reports and proxy statements filed through 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 (2011), 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.

Data from a number of public sources were necessary for this study. Compustat data is used to determine discretionary accruals and to obtain firm characteristics including market-to-book, leverage, and return on assets. Returns data are taken from the CRSP files. Earnings and analysts' forecasts are obtained from IBES. Institutional ownership is obtained from the Thompson Financial 13F database, and board of director characteristics and executive stock ownership are taken from Compact Disclosure. Finally, we obtain merger and acquisition data from the Securities Data Company (SDC) database.

[32] If a groups of CEOs have systematically different numbers of option events (grants and exercises), it is possible that our classification scheme would result in a greater rate of misclassification across the groups. To make sure the rate of misclassification of suspect CEOs should not differ across inside versus outside hire CEOs, we confirm using the Execucomp data that there is not a meaningful difference in the mean or median number of option events across these two groups.

In all of our analyses, we compare the activities of suspect firms to those of similar control firms. The control observations are obtained by matching each suspect firm-quarter to all firms without a suspect individual in the same quarter and SIC-3 industry group. We retain up to five matched observations that are closest in market value of equity to our suspect firm-quarter observation and further require that differences in market capitalization between suspect and control observations not exceed 50 percent.

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.[33] Not surprisingly given our matching method, we find that the sample of suspect and control firms have very similar characteristics. For the suspect CEO sample, we find 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. Interestingly, the average return on assets for our sample is higher than that of the control firms (-3.3% versus -5.0%) even though the level of statistical significance falls just outside conventional levels. It is possible that this reflects the increase in reported accounting earnings that are obtained from backdating employee stock options. It is also interesting that ROA is negative on average for these firms. This is likely driven by the fact that there are a large number of technology firms in the samples which realized low accounting returns while investing in growth options. Similar patterns hold when

[33] This procedure minimizes the difficulty of comparing the overall averages for the samples 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.

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 twelve 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.

IV. Empirical Results

In this section we compare the financial reporting and investment activities of our sample of suspect firms to those of similar control firms. We also investigate the potential negative consequences of suspect behavior from the perspective of both the CEO and shareholders.

IV.a. 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 et al, 1999) and commonly interpret this as evidence that executives opportunistically manage earnings in order 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 may 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. Graham, Harvey, and Rajgoptal (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.[34] Our measure of earnings surprise is the actual earnings announced minus the mean or median analyst forecast from IBES. We focus on unadjusted earnings surprises as in Kaznik and McNichols (2002) and McVay et al (2006).

Figure 1, Panel A plots the distributions of earnings surprises for suspect CEO firms relative to a matched control sample. It is evident that the frequency of just meeting or beating earnings estimates is higher for our suspect sample. Suspect firms also have lower frequencies of just missing earnings targets by one to three cents, suggesting these "just miss" observations have been pushed above the threshold to meet expectations. To determine if differences are significant, 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

[34] 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).

(by one or two cents) analysts' quarterly earnings forecasts.[35] 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¢.

Table 2 shows univariate comparisons of quarterly earnings surprises. We find that the frequency of *BEAT* surprises is significantly higher when CEOs benefitted from backdating (43.3% for suspect firms relative to the mean forecast versus 37.8% for control firms). 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 more likely to just miss earnings forecasts, although the economic significance is reduced. 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 was an outside hire. The patterns that emerge are striking (both here, and across the remainder of our analyses) and are illustrated in Figure 1, Panel B. Firms with suspect CEOs are much more likely to engage in other forms of corporate misbehavior if the CEO was hired from the outside. These firms 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).

As discussed previously, we also present all of our results for the sample of firms where backdating was evident, but only for lower-level officers (i.e. Non-Mgmt Sample). We find that these firms were not more likely than similar firms to narrowly meet earnings expectations. This contrast is to be expected if options backdating for top-level executives reflects a dishonest management practice, and supports the upper echelons theory of corporate behavior.

[35] 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.

We next examine earnings surprises in a multivariate setting. We pool suspect and control firm quarters and run logit regressions predicting a quarterly earnings surprise in the narrow classification *BEAT*. The regressions include controls for standard firm characteristics including size (*MVE*), growth opportunities (*MTB*), *Leverage*, and profitability (*ROA*). We control for the level of *Institutional ownership* as in Davis, Soo, and Trompeter (2009). *Firm age* is constructed as the number of months since a firm first appeared in CRSP.[36] The regressions also include controls for corporate governance characteristics including the total number of directors (*Ln Board Size*) and the percentage of independent directors (*Board Independence*). We 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. The variable of interest in our regression is *Suspect*, an indicator equal to 1 if the firm has a suspect CEO.

These regressions are presented in Table 3. 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 growth firms and younger firms are more likely to narrowly beat earnings thresholds. Firms are also more likely to meet these thresholds when the CEO has been 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.

[36] All independent variables in the regression, excluding *suspect*, are lagged values.

The results from our univariate analysis are confirmed in this multivariate setting. The coefficient on *Suspect* is positive and significant at the ten percent level in the regression with all backdating CEO firms. In the regression with CEOs hired externally the coefficient on *Suspect* is 0.243 and significant at the five percent 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 was limited to lower level employees. Overall, these multivariate results continue to support the upper echelons theory for understanding a corporate culture of malfeasance.[37]

IV.b. Accrual-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 II.[38] 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 3) and running the following regression:

[37] In untabulated multivariate tests, we repeat the regression analysis presented in Table 3 with *MISS* as the dependent variable. The coefficient on *suspect* is negative but insignificantly different from zero, suggesting that suspect CEO firms are not significantly less likely to narrowly miss their earnings targets.

[38] In robustness tests we employ four alternate measures of discretionary accruals as calculated by Ecker, Francis, Olsson, and Schipper (2011). Our results are qualitatively and quantitatively similar regardless of the model employed.

$$
\begin{aligned}
Disc.\ Accrual_{i,t} = {} & \alpha_0 + \alpha_1 MVE_{i,t-1} + \alpha_2 MTB_{i,t-1} + \alpha_3 Leverage_{i,t-1} + \alpha_4 ROA_{i,t-1} + \\
& \alpha_5 Firm\,age_{i,t-1} + \alpha_6 IO_{i,t-1} + \alpha_7 Board\ Ind_{i,t-1} + \alpha_8 Board\ Size_{i,t-1} + \\
& \alpha_9 PYGrant_{i,t-1} + \alpha_{10} CEOOwn_{i,t-1} + \alpha_{11} CEOOwn^2{}_{i,t-1} + \alpha_{12} BEAT_{i,t} + \\
& \alpha_{13} Suspect * BEAT_{i,t} + \mathrm{X}_i + \varepsilon_{i,t}
\end{aligned} \tag{1}
$$

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* (*BEAT*Suspect*), where *Suspect* is an indicator variable that equals 1 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 Table 4. 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 can't 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.[39]

IV.c. Acquisitions by Suspect Firms

Thus far we have provided evidence that firms with suspect CEOs hired from the outside are more likely to manipulate earnings data provided to the marketplace. In this section we consider whether these firms are more likely to engage in real corporate activities from which executives may benefit, but that are 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.[40]

We analyze completed acquisitions of both public and private targets of greater than $5 million that result in the acquirer owning 100 percent of the target. Similar to our previous analyses,

[39] In untabulated results we find that when firms hire a suspect CEO from outside of their firm, they are significantly more likely than matched firms to restate financials because of accounting irregularities (i.e., restatements of intentionally misleading financials) during or immediately following the suspect CEO's tenure (we are careful to exclude from this analysis any restatements directly related to the backdating activity). However, due to the extremely small number of restatements due to accounting irregularities, our results and inference regarding this finding are extremely sensitive to the inclusion/exclusion of a small number of observations.

[40] Numerous authors have identified reasons that executives may 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 have found that monitoring by outside blockholders and stronger shareholder rights can mitigate the agency costs associated with acquisition decisions (Li, Harford and Chen, 2007; Masulis, Wang and Xie, 2007).

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 3-day cumulative abnormal returns (CARs) surrounding the acquisition announcement dates, following Masulis, Wang and Xie (2007).[41] Consistent with empirical tests in the first part of this paper, we present results for these analyses separately for outside- and inside-hire suspect CEO firms as well as for suspect firms without top executive involvement (Non-Mgmt Sample).

The univariate results for acquisition activity and abnormal returns surrounding their announcement are presented in Table 5. Firms with backdating CEOs were more likely to acquire other firms. Suspect CEO firms had 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 were more likely to acquire a private company (14.9% vs. 11.1%; p-value of difference=0.001), but were not more likely to acquire a public company (5.3% vs. 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 may focus on private targets for reasons that are inconsistent with shareholders' interests. For example, executives who intend to use target assets for

[41] As in Masulis, Wang and Xie (2007), CARs are calculated relative to daily expected returns generated by a market model using the value-weighted market index estimated over the 200 trading day period ending 10 days before the acquisition announcement date.

their private benefits may 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.[42]

We also provide a comparison of the 3-day bidder CARs surrounding the announcement of acquisitions in Table 5. 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 the private acquisitions. Prior literature shows that bidder CARs are positive, on average, among private acquisition announcements.[43] 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

[42] 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.

[43] Fuller, Netter and Stegemoller (2002) show that returns to private acquisitions are positive on average and argue that the gains flow from better pricing due to the illiquidity of the acquired shares, the new possibility for outside monitoring of previously closely-held corporations and tax benefits to private company shareholders.

results for the Non-Mgmt sample show no differences between the CARs of suspect and control firms.

In Table 6 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 (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).

IV.d. Difference-in-Differences Analyses

Our findings thus far indicate that firms with CEOs that benefitted directly from options backdating are associated with other corporate misbehaviors, including earnings manipulations and elevated levels of lower quality acquisitions. In addition, these abnormal activities are concentrated 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. To help determine whether the corporate culture is impacted by the executives that firms hire, we employ 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 and 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) S&P 1500 firms in the same 3-digit SIC industries as our sample of suspect CEO transitions.[44] 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.

IV.d.i Difference-in-Differences in Earnings Surprises

Our first set of difference-in-differences tests focuses 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, 2012; Chaterjee and Hambrick, 2007). We proceed by running the following logit regression:

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

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-of-differences regression also includes the variables *Suspect, Post* and *Suspect*Post*. *Post* is an indicator variable that equals one for quarters following a new CEOs arrival at the firm. Our primary variable of interest is the interaction *Suspect*Post*, where *Suspect* is a time-invariant indicator variable that equals 1 for firms

[44] The 1992 to 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.

with a suspect CEO transition.[45] 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 7 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 et al, 2004; Ang and Nagel, 2010), we pool outside-hire suspect CEO transitions only with control CEO transitions where 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 it remains 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-of-differences tests are consistent with the contemporaneous earnings management tests in Tables 2 and 3 and suggest that firms at times hire outside CEOs that are character "lemons" who have a detrimental impact on firm financial reporting.

IV.d.ii Difference-in-Differences in Acquisition Activity

We next provide evidence on the direction of causality between suspect CEOs and costly acquisitions. Table 8 presents difference-in-differences logit regressions identifying the change in probability of completing acquisitions from the five year periods 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 2 years before and after the suspect CEO transition, which

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

includes 50 inside hires and 23 outside hires.[46] Control CEO transitions are constructed in an identical manner as those in Table 7, and consistent with Table 7 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 a 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.

IV.e The Consequences of Suspect Behavior

Given our evidence thus far on the corporate misbehaviors at firms where the CEO benefitted from backdating, we next examine whether these actions culminate in costly consequences. If suspect firms overstated their quality to the market and/or made value-destroying acquisitions, then they are likely to eventually suffer large losses when their true quality becomes apparent. In addition, if these managers are indeed "lemons", then they may be more likely to lose their jobs when their type is eventually revealed.

We begin by analyzing whether suspect firms are more likely to experience large declines in value when overall economic activity declines. Povel, Singh and Winton (2007) provide an economic model explaining the common belief that economic booms encourage and conceal corporate fraud,

[46] The difference in sample size between the sample of suspect CEO transitions analyzed in Table 7 and that analyzed in Table 8 is driven by the requirement in Table 7 that suspect CEO firms have available information in IBES.

which are subsequently revealed during ensuing bust periods. Based on this intuition, we explore whether suspect firms were more likely to experience severe losses in market capitalization during the economic slowdown that followed the stock market "bubble" of the late 1990s.

Logit regressions predicting severe stock price declines are presented in Table 9. 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%) in either 2001 or 2002. 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 were 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.[47] The regressions include an indicator for suspect firms with backdating CEOs (*Suspect*) and another incremental indicator for firms with backdating CEOs that were hired externally (*Outside Suspect*). *Suspect* is negative and insignificant in both regressions, and *Outside Suspect* is associated with a much larger positive and significant coefficient. For example, the coefficient is 0.982 (p-value=0.044) in the regression predicting a 40% stock market decline. This indicates a marginal increase in the probability of a large loss of 24.5% at suspect firms with an outside-hire CEO. In unreported specifications we include indicator variables for *Inside Suspect* and *Outside Suspect,* separately, and confirm that the coefficient on *Outside Suspect* is also significantly greater than zero. 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.

Our final test examines whether CEOs who personally benefitted from backdating were more likely to be fired. We focus on CEO replacements during the period from 1996 to 2005, so that we

[47] Refer to the Table 9 header for an exact description of how these variables are constructed.

identify decisions made before options backdating practices were brought to light by academic research and media attention. Table 10 presents logit regressions predicting that a CEO is fired in a given year. A CEO is considered to have been fired if he was 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 easy to identify CEO transitions in this dataset, 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 variable we have used throughout this paper, we also include the length of a CEO's tenure and the current and lagged year stock return in these regressions. The first specification includes observations for the full time period and shows that suspect CEOs hired from the outside were much more likely to be fired during this period. The coefficient on *Suspect Outside* of 1.336 (p-value = 0.004) indicates a 1.3% greater probability of being fired. The second specification only includes firm-years from 2002 to 2005, and the coefficient on *Suspect Outside* increases to 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. The final specification only includes outside hire suspect CEOs and control firms. The insignificant coefficient of -0.442 on *Suspect* indicates that these CEOs were not more likely to be fired during the bubble period, and the coefficient on *Suspect*Post2002* of 2.735 (p-value <0.001) confirms their greater odds of being fired after the bubble period ended. These results suggest that "lemon" 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 may therefore be magnified during these periods of stock market strength.

V. Conclusion

This paper explores corporate culture, with a focus on determining whether the ethics of top executive officers drive firms to have unethical cultures. We identify a group of suspect CEOs with questionable ethics as those that systematically backdated their option grants and/or exercises, and provide evidence that the firms they lead engaged in other questionable activities too. Firms managed by suspect CEOs are more likely to just meet or narrowly beat analyst earnings expectations and they use more earnings-increasing discretionary accruals in the quarters when they meet these thresholds. The acquisitions of these firms are met by lower market responses, and they make more acquisitions of private companies, which may facilitate earnings manipulations. These questionable corporate practices increase after suspect CEOs are hired, suggesting a causal relationship between CEO character and firm outcomes.

The results we present in this paper are concentrated in those firms who bring in new CEOs from the outside. The link between outside-hire CEOs' personal ethics and other negative corporate actions is suggestive of a "lemons" problem in the market for CEOs that can lead to negative consequences for shareholders. These CEOs appear to avoid negative consequences during a period of stock market excess, but they are more likely to be fired and their firms are more likely to suffer large losses during a market correction.

This work provides evidence that the ethics of corporate leaders is an important determinant of the ethical 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 low character CEO than others? Are there mechanisms 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?

Appendix I

This appendix reviews the case of Jacob ("Kobi") Alexander, a NYU MBA graduate who was the CEO of Comverse Technology, to illustrate the suspect character of backdating executives. In 2006, Alexander was accused of criminal securities fraud and falsification of records or accounts in relation to the backdating of stock option grants. He subsequently fled the country to reside in Namibia, a country without an extradition treaty with the U.S., and transferred $40 million from his U.S. bank account to his personal account in Israel. In 2011, he settled civil fraud charges with the S.E.C., and handed over $46 million to settle shareholder lawsuits against Comverse. At this time he remains a fugitive in the criminal case against him.

The S.E.C. litigation release (No. 21753, Nov. 23, 2010) discussing Mr. Alexander's settlement alleged the following misconduct:

"Alexander and two other former Comverse senior executives engaged in a decade-long fraudulent scheme to grant in-the-money options to themselves and to others by backdating stock option grants to coincide with historically low closing prices of Comverse common stock. ... Alexander created a slush fund of backdated options by causing options to be granted to fictitious employees and, later used these options, some of which were made immediately exercisable, to recruit and retain key personnel. As part of the scheme, the former executives made material misrepresentations to Comverse investors regarding Comverse's stock option grants, including representing that the options had been granted at exercise prices equal to fair market value on the grant dates and concealing that Comverse was required to but had not recorded compensation expenses relating to such grants. As a result, Comverse materially overstated its net income and earnings per share for more than a decade."

Mr. Alexander entered a settlement agreement without admitting or denying these allegations, but it seems unlikely that he would take such a drastic course of action to avoid prosecution without a great deal of truth to these statements. Given his graduate business education at a prestigious U.S. business school, it seems unlikely that he would have been unaware of the ethical and legal consequences of these actions. The fact that he has avoided facing the consequences of his actions also suggests an appreciation of the gravity of the offense.

Appendix II

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. In order to determine discretionary accruals for each firm and quarter, we first run the following cross-sectional OLS regression in each quarter for all firms in the same industry (i.e., two-digit SIC code) in order 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.

$$\frac{TA_{i,t}}{Assets_{i,t-1}} = \hat{\alpha}_1 \frac{1}{Assets_{i,t-1}} + \hat{\alpha}_2 \frac{\Delta Sales_{i,t}}{Assets_{i,t-1}} + \hat{\alpha}_3 \frac{Net\ PPE_{i,t}}{Assets_{i,t-1}} + \varepsilon_{i,t} \qquad \text{(A1)}$$

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 equation A1 to calculate non-discretionary accruals for each firm-quarter in our sample:

$$NDA_{i,t} = \hat{\alpha}_1 \frac{1}{Assets_{i,t-1}} + \hat{\alpha}_2 \frac{\Delta Sales_{i,t} - \Delta AR_{i,t}}{Assets_{i,t-1}} + \hat{\alpha}_3 \frac{Net\ PPE_{i,t}}{Assets_{i,t-1}} \qquad \text{(A2)}$$

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*): $DA_{i,t} = (TA_{i,t} \div Assets_{i,t-1}) - NDA_{i,t}$.

In robustness tests we employ four alternate measures of discretionary accruals as presented by Ecker, Francis, Olsson, and Schipper (2011). 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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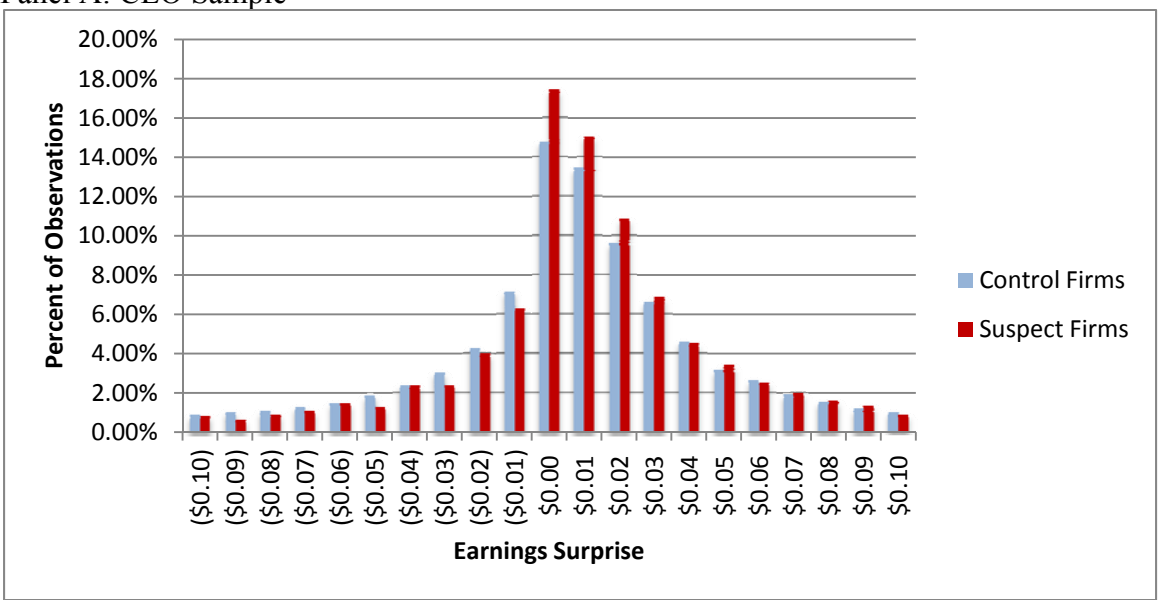
Figure 1– Frequency of Earnings Surprises

These figures present the frequency of earnings surprises from -10¢ to +10¢ for quarterly earnings announcements during the 1990 to 2009 sample period. Earnings surprise is measured as actual earnings minus the most recent mean analyst forecast. Reported earnings and analysts forecast earnings are obtained from the Institutional Brokers Estimate System (IBES) unadjusted summary files. We present results for both our sample of firm-quarters where a "suspect" CEO is present and for a matched sample of non-suspect firm quarters. Suspect CEOs are defined as those where at least 30% of their option activity (and at least two option transactions) are classified as 'likely' backdated or were highlighted in the SEC's Spotlight on Backdating. Matched firm-quarter observations are those in which there are no reporting insiders classified as "Suspect" and are matched to the suspect sample on quarter, 3-digit SIC code, and market value of equity. Panel A presents data for all suspect CEO firms and Panel B presents data for suspect CEO firms where the CEO was hired from the outside.

Panel A: CEO Sample



Panel B: Outside CEO Sample

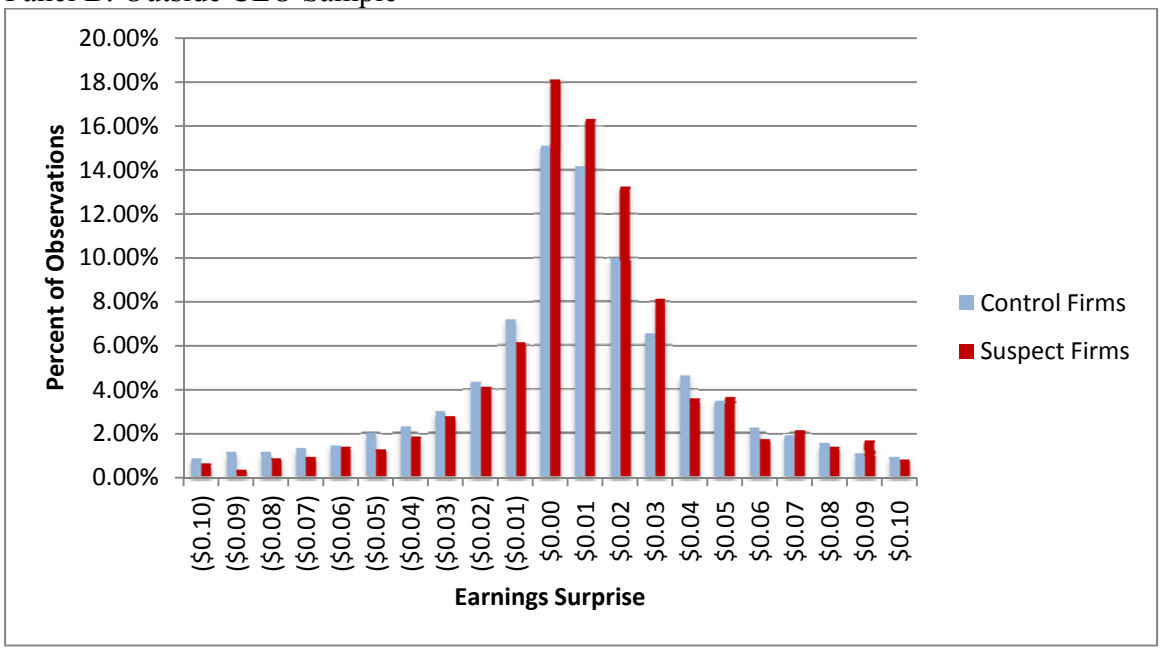


Table 1 – Summary Statistics

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 is collected from the Thompson 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 where 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 (3-digit SIC code), and market value of equity; up 5 control firms are selected for each suspect firm on a quarterly 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.
Market Value of Equity	1,875	1,737	138.4	1,875	1,671	203.5
			(0.662)			(0.511)
Market-to-Book	3.364	3.395	-0.032	3.587	3.684	-0.096
			(0.852)			(0.642)
Assets	1,860	2,371	-510.9	2,087	2,040	47.32
			(0.195)			(0.923)
Leverage	0.178	0.193	-0.015	0.168	0.185	-0.017
			(0.188)			(0.215)
Tobin's Q	2.102	2.161	-0.059	2.275	2.310	-0.035
			(0.605)			(0.800)
Return on Assets	-0.0327	-0.0497	0.017	-0.040	-0.062	0.022
			(0.122)			(0.129)
Firm-Years	2,298	2,298		1,924	1,924	
Unique Firms	258			172		

Panel B: Distribution by Industry

	Suspect CEO Sample vs. Compustat			CEO Sample vs. Non-Mgmt Sample		
Industry	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	

Table 2 – 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 is collected from the Thompson 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 where 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 (3-digit SIC code), and market value of equity; up 5 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 1 year at the time they are promoted to CEO are classified as inside hires and those with less than 1 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¢]			
	v. Mean forecast	v. Median forecast	v. Mean forecast	v. Median forecast	# Firm-Years	# Unique Firms
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

Table 3 – 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 2. 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 Thompson 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 Thompson. *Suspect* is an indicator variable that equals 1 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	Outside CEOs	Inside CEOs	Non-Mgmt Sample
Independent Variables				
Constant	-2.366**	-3.176***	-2.678**	-0.798*
	(0.030)	(<0.001)	(0.022)	(0.071)
MVE	0.126***	0.140***	0.100***	0.091***
	(<0.001)	(<0.001)	(0.00104)	(<.001)
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)
Institutional Ownership	0.184	0.186	0.167	0.207
	(0.134)	(0.328)	(0.240)	(0.137)
Board Independence	0.171	-0.386	0.328	-0.298
	(0.459)	(0.264)	(0.220)	(0.272)
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

Table 4 – 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 2. 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 Thompson 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 Thompson. *Suspect* is an indicator variable that equals 1 if the firm has been classified as suspect, *BEAT* is an indicator variable that equals 1 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	Outside CEOs	Inside CEOs	Non-Mgmt Sample
Independent Variables				
Constant	-0.0212**	-0.0076	-0.0258**	0.0669***
	(0.033)	(0.778)	(0.019)	(<0.001)
MVE	-0.0043***	-0.0030	-0.0045***	-0.0038***
	(<0.001)	(0.237)	(0.001)	(0.003)
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)
Institutional Ownership	-0.0002	0.0016	0.0004	-0.0126**
	(0.971)	(0.840)	(0.936)	(0.015)
Board Independence	0.0029	0.0075	0.00007	-0.0049
	(0.635)	(0.585)	(0.992)	(0.517)
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.

Table 5 – Univariate Analysis of Acquisitions

This table presents a univariate analysis of acquisition activity during the sample 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 as the method reported in Table 2. Matching between suspect and non-suspect samples is based on year, industry (3-digit SIC code), and market value of equity; and up to 5 control firms are selected for each suspect firm on a yearly basis. For both the suspect and matched samples we create an indicator variable that equals 1 when a firm announces an acquisition during the fiscal year that is later completed with a deal size greater than $5 million. The 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 3 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								
	CEO Sample			Outside CEO Sample			Inside CEO Sample			Non-Management 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)	(<.001)	(0.545)	(0.061)	(<.001)	(0.261)	(0.009)	(<.001)	(0.974)	(<.001)	(<.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)	(<.001)	(0.007)	(0.379)	(0.030)	(0.049)	(0.295)	(<.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 6 – Multivariate Analysis of Acquisition CARs

This table presents multivariate regressions of the market to acquisitions of greater than $5 million on independent variables that control for firm and executive characteristics. 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 as the method reported in Table 2. Matching between suspect and non-suspect samples is based on year, industry (3-digit SIC code), and market value of equity; up to 5 control firms are selected for each suspect firm on an annual basis. The Dependent variable is the 3-day cumulative abnormal return of the bidder firm around the announcement of an acquisition with a deal size greater than $5 million. 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 3 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 Thompson Financial 13F filings; and *Board Independence* and *Board Size* are obtained from Compact Disclosure. *All Cash* is an indicator variable that equals 1 if 100% of the purchase price was paid in cash. *All Stock* is an indicator variable that equals 1 if 100% of the purchase price was paid with stock. *Suspect* is an indicator variable that equals 1 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)
MVE	-0.0054**	-0.0074	-0.0063**	0.0018
	(0.048)	(0.255)	(0.042)	(0.631)
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)
Institutional Ownership	-0.0195	-0.0419	-0.0165	-0.0290*
	(0.159)	(0.123)	(0.322)	(0.069)
Board Independence	-0.0315	0.0433	-0.0374	0.0097
	(0.369)	(0.495)	(0.355)	(0.723)
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)
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.

Table 7 – Multivariate Analysis of Earnings Surprises: Difference-of-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 with data available in at least the two closest years to the transition year in both pre- and post-CEO arrival periods are included. Suspect CEOs are identified using likely backdated option grants and exercises as described in Table 2. 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 Thompson. Executives who have been with the firm for longer than 1 year at the time they are promoted are classified as insider hires and those with less than 1 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 1 for firms with a suspect CEO transition. *Post* is an indicator variable that equals 1 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)
MVE	0.0743***	-0.00474	0.0943***
	(0.002)	(0.904)	(0.001)
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
Psuedo-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.

Table 8 – Multivariate Analysis of Acquisitions: Difference-of-Differences

This table reports the coefficient estimates of a logit regressions of *Acquisition, Public, and Private* 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 with data available in at least the two years closest to the transition year in both pre- and post-CEO arrival periods are included. Suspect CEOs are identified using likely backdated option grants and exercises as described in Table 2. 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 Thompson. Executives who have been with the firm for longer than 1 year at the time they are promoted are classified as insider hires and those with less than 1 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 1 when a firm completes an acquisition of a publically traded firm with a deal size greater than $5 million in a given year and *Private* is an indicator that equals 1 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 1 for firms with a suspect CEO transition. *Post* is an indicator variable that equals 1 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)
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)
Market-to-Book	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)
PostYear	-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*PostYear*	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.

Table 9 – Large Losses

This table reports the coefficient estimates of logit regressions of the propensity to experience large losses in market value during 2001 or 2002 on independent variables that control for firm and executive characteristics. The sample of observations includes firms trading at the end of 2000 for our sample of firms classified as suspect and a control sample of firms without evidence of option backdating activity. Suspect firms are matched to control firms based on industry (3-digit SIC code) and market value of equity at the end of 2000; up to 5 control firms are selected for each suspect firm. The Dependent variable: *Large Loss* is an indicator that equals 1 if a firm has a total return of -40% or lower in 2000 or 2001. Independent variables *MVE, Firm Age, Firm Return 1999, Firm Return 2000, Ret. Std. Deviation, Abn. Turnover, and ILLIQ* are obtained from CRSP; *Market-to-book* and *Leverage*, are obtained from Compustat; *Institutional Ownership* is obtained from Thompson Financial 13F filings; *and CEO Ownership* is obtained from Compact Disclosure. *Prior Year Grants* is obtained from Thompson IFDF and *Analyst Coverage* is collected from IBES. *Abn. Turnover* is calculated as the change in average monthly turnover from 1999 to 2000, *ILLIQ* is calculated as the daily average of the absolute value of the ratio of returns to dollar trading volume during 2000, *Ret. Std. Deviation* is the standard deviation of monthly returns for the firm during 2000, and *Analyst Coverage* is the number of analyst reporting in IBES for the fiscal year ending in 2000. *Suspect* is an indicator variable that equals 1 if the firm has been classified as suspect, *Outside Suspect* is an indicator variable that equals 1 for firms with a suspect CEO who was hired from outside the firm. All regressions include industry fixed effects. Numbers presented in parentheses are p-values calculated using robust standard errors.

	Dep Var = 1 if Ret <= -40%		Dep Var = 1 if Ret <= -50%	
Independent				
Constant	-1.922	-1.798	-2.387*	-2.310*
	(0.179)	(0.212)	(0.083)	(0.098)
MVE	-0.110	-0.129	-0.083	-0.098
	(0.233)	(0.171)	(0.377)	(0.310)
Market-to-Book	0.351***	0.366***	0.275**	0.284**
	(0.009)	(0.007)	(0.029)	(0.026)
Leverage	0.521	0.516	1.125*	1.149*
	(0.442)	(0.452)	(0.093)	(0.090)
Firm Return 1999	0.252***	0.255***	0.206***	0.210***
	(0.002)	(0.002)	(0.002)	(0.002)
Firm Return 2000	-0.0716	-0.061	-0.110	-0.103
	(0.581)	(0.644)	(0.413)	(0.445)
Firm Age	-0.354***	-0.360***	-0.356***	-0.359***
	(0.006)	(0.005)	(0.005)	(0.005)
Institutional Own.	-1.237**	-1.226**	-1.273**	-1.246**
	(0.023)	(0.026)	(0.021)	(0.024)
Ret. Std. Deviation	5.545***	5.602***	5.720***	5.783***
	(<0.001)	(<0.001)	(<0.001)	(<0.001)
Analyst Coverage	0.289*	0.295**	0.290*	0.301*
	(0.053)	(0.048)	(0.058)	(0.050)
Abn. Turnover	-0.001	0.0002	-0.0344	-0.0331
	(0.968)	(0.996)	(0.365)	(0.372)
ILLIQ	-0.043	-0.0432	-0.0422	-0.0436
	(0.156)	(0.149)	(0.146)	(0.141)
Prior Year Grants		0.0037		0.0039
		(0.297)		(0.286)
CEO Ownership		-1.819		0.074
		(0.515)		(0.978)
CEO Ownership^2		5.439		1.609
		(0.426)		(0.793)
Suspect	-0.156	-0.183	-0.194	-0.225
	(0.522)	(0.455)	(0.419)	(0.352)
Outside Suspect	1.017**	0.982**	0.850*	0.833*
	(0.037)	(0.044)	(0.074)	(0.079)
Industry Fixed Eff.	Yes	Yes	Yes	Yes
# Observations	637	637	605	605
Psuedo-R^2	0.235	0.236	0.209	0.210

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

Table 10 – CEO Forced Turnover

This table reports the coefficient estimates of logit regressions of the propensity to have a forced CEO turnover event in a given year on independent variables that control for firm and executive characteristics. The sample of observations includes firm years between January 1996 to December 2005 for our sample of firms classified as suspect and control firms selected from Execucomp. The Dependent variable: *Forced Turnover* is an indicator that equals 1 if a firm has a CEO turnover classified as forced (see Parrino, 1997). Independent variables *MVE, Firm Age, and Prior Returns* are obtained from CRSP; *Market-to-book*, *Leverage*, and *ROA* are obtained from Compustat; *Institutional Ownership* is obtained from Thompson Financial 13F filings; *CEO Tenure* is determine from Execucomp and SEC filings; *Suspect* is an indicator variable that equals 1 if the firm has been classified as suspect; *Outside* is an indicator that equals 1 if the CEO was hired from outside the firm; *Suspect Outside* is the intersection of suspect and outside. All regressions include industry and year fixed effects. *Post 2002* is an indicator that equals 1 in 2002 and later; *Suspect * Post 2002* is the intersection of *Suspect* and *Post 2002*. Numbers presented in parentheses are p-values calculated using standard errors clustered at the industry level.

	CEO Sample		Outside CEOs
	1996-2005	2002-2005	1996-2005
Independent			
Constant	-4.379***	-3.093***	-4.373***
	(<0.001)	(<0.001)	(<0.001)
MVE	0.077	-0.021	-0.049
	(0.124)	(0.806)	(0.563)
Market-to-Book	-0.039	-0.063	0.214
	(0.826)	(0.790)	(0.400)
Leverage	0.401	1.203**	0.758
	(0.311)	(0.042)	(0.282)
ROA	-0.020	0.457	0.452
	(0.970)	(0.583)	(0.572)
Insti. Ownership	-0.152	-0.335	-0.686*
	(0.518)	(0.436)	(0.071)
CEO Tenure	-0.015	0.098	-0.139
	(0.859)	(0.580)	(0.299)
Stock Returns (t-1)	-1.242***	-1.202***	-0.777***
	(<0.001)	(<0.001)	(0.002)
Stock Returns (t)	-1.578***	-2.323***	-1.569***
	(<0.001)	(<0.001)	(<0.001)
Suspect	-0.503*	0.433	-0.442
	(0.076)	(0.176)	(0.470)
*Suspect * Outside*	1.336***	2.128***	
	(0.004)	(<0.001)	
Outside	-0.120	-0.531	
	(0.534)	(0.158)	
*Suspect * Post 2002*			2.735***
			(<0.001)
Post 2002			-0.382
			(0.686)
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Observations	10,644	3,800	3,889
Psuedo-R^2	0.114	0.175	0.123