Career Concerns for Revealing Misreporting

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Abstract

We examine whether revealing misreporting affects the careers of executives and independent directors. To isolate the effects of revealing misreporting from the underlying malfeasance, we analyze executives and directors who joined firms after stock option backdating ceased, but who were in place to determine how the firm would respond to the unfolding backdating crisis. Overall, these new executives and directors faced career penalties at firms that issued a backdating restatement relative to those at firms that remained silent despite strong evidence of backdating having occurred. We conduct a variety of tests to rule out alternative explanations, and conclude that new executives and directors face career penalties after firms reveal misreporting.

Key words: Corporate governance, executive turnover, board of directors, director labor market, shareholder voting, financial restatements

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I. INTRODUCTION

A wide literature uses financial restatements as a proxy for governance failures and accounting malfeasance (see Dechow, Ge and Schrand, 2010 for a review). Consistent with the view that restatements are an adverse event, several studies find that restatements lead to negative outcomes for the firm and its management.[1] However, in one of the earliest studies, Srinivasan (2005) questions whether a restatement announcement signifies a governance success or failure as it arises when a firm both misreports (a failure) as well as detects and corrects the error (a success). Whether restatements could reflect positively on executives and directors (hereafter collectively called "managers") is largely unexplored in the literature. We shed light on this question by examining whether managers face career penalties for revealing misreporting or, conversely, for failing to respond to the strong inference that misreporting has occurred.

The challenges in addressing this topic lie in identifying managers that publicly reveal misreporting (but were not responsible for the misreporting) as well as managers that do not respond to the strong inference of misreporting having occurred. We utilize the stock options backdating setting as we can identify both groups. First, because misreporting occurred several years before its discovery (see appendix 1 for a timeline of events), we can identify "new" managers who joined the firm after backdating ceased, but before the scandal broke. Since these individuals were not present when the backdating occurred, they were not directly responsible for failing to prevent it. Second, we can identify "suspect" firms that appeared to materially backdate (according to common methods used in the literature), but did not issue a restatement,

[1] For example, studies examine consequences to executives and directors associated with firms that restate (Arthaud-Day, Certo, Dalton and Dalton, 2006; Cheng and Farber, 2008; Desai, Hogan and Wilkins, 2006; Hennes, Leone and Miller, 2008; Leone and Liu, 2010), and consequences to firms that restate (Badertscher, Hribar and Jenkins, 2011; Palmrose and Scholz, 2004; Palmrose, Richardson and Scholz, 2004; Files, Swanson and Tse, 2009).

thus violating SEC guidance issued at the time clarifying that companies with material backdating errors must issue a restatement.[2]

As an illustration of the research question we explore, consider the case of Charles "Lanny" Baker who joined the online job portal Monster Worldwide, Inc. ("Monster") as the CFO in March of 2005. Roughly one year later (June of 2006), Monster conducted an internal review of its option granting practices, and restated prior period financial statements. Soon thereafter several executives departed, including the founder, Chairman and CEO (Andrew McKelvey), the subsequent CEO (William Pastore), the Senior Vice President, General Counsel and Secretary (Myron Olesnyckyj), as well as Lanny Baker.

Since Baker joined Monster approximately two years after the backdated grants, he bore no responsibility for the underlying backdating.[3] While we do not definitively know why Baker left Monster, the circumstances of his departure suggest that he was enveloped in the backdating scandal and paid a career penalty. At the time, he was too young to retire (40 years old), served as CFO only two years, did not obtain subsequent employment for 18 months, took a substantial pay cut upon departure, and left shortly after Monster issued the backdating restatement.[4]

Our empirical tests are designed to determine whether Mr. Baker's experience was an aberration or reflects a general tendency for managers to face career penalties when governance issues that predate their tenures come to light. Our primary empirical tests compare the rates of

[2] The SEC reiterated that material errors from backdating require restatements. See http://www.sec.gov/info/accountants/staffletters/fei_aicpa091906.htm (accessed 8/2017).
[3] Monster issued the backdated option grants from 1997 through March 31, 2003. See the 2005 10-K issued on December 13, 2006.
[4] Baker became CFO of ZipRealty in December 2008. He earned over $2 million in 2006 and 2007 with Monster, but only $425,843 and $445,885 with ZipRealty in 2008 and 2009. His earnings at would not return to 2007 levels until he became CFO of Yelp.com more than a decade later (2018), when he earned $3.3 million. Further, press reports speculated that backdating played a role and noted "the management shake-up comes after Monster filed restated financials in December to include charges related to an investigation into its stock-option granting (Associated Press, 2007)."

director and executive turnover and director votes withheld among new managers at restatement firms relative to new managers at suspect firms.

If the issuance of a restatement is viewed as a governance success (in detecting and remediating accounting issues), new managers at firms that appear to materially backdate but do not issue a restatement might face greater career penalties than new managers at restating firms. Alternatively, perhaps restatements represent a failure among managers in handling the remediation of accounting issues, in which case new managers at restatement firms could face greater penalties than new managers at suspect firms. Similarly, firing a new manager could reflect a need to obtain a manager skilled at repairing the firm's tarnished reputation (Chakravarthy, deHaan, & Rajgopal, 2014), or to avoid the costs of future governance problems (as discussed in detail in section 2). At the individual firm level, dismissing new managers at restatement firms could represent a rational response. Yet, from a regulatory perspective, facing negative consequences for issuing a restatement could chill managerial incentives to reveal misbehavior, to the extent that managers can foresee the penalties and avoid them by remaining silent.

Our first tests examine whether new directors at restating firms faced higher votes withheld after the backdating scandal unfolded compared to new directors at suspect firms. Univariate and multivariate models suggest that the amount of votes withheld are 5.4% and 5.9% higher at restating firms, respectively, which is around 40% of the sample mean.

While these tests indicate that shareholders held new directors accountable when the firm issued a restatement, it is possible that proxy advisor recommendations drove these results. Since the prior models do not control for proxy advisor influence, shareholders could have followed proxy advisor recommendations, which disciplined new managers at restating firms to a greater

extent than their peers at suspect firms. To test the role of proxy advisors, we replicate the voting model while controlling for whether the Institutional Shareholder Services group (ISS) recommended withholding votes in general, and because of backdating, specifically. When controlling for all ISS recommendations, we still find a statistically different level of votes withheld comparing new directors at restating firms to new directors at suspect firms (T-stat = 2.46). However, when we limit the recommendations to those that relate specifically to backdating, we no longer find a statistically different level of votes withheld between the two groups (T-stat = 1.47).

Extending our analysis into proxy advisor behavior, we next test whether ISS examined option grant data and recommended voting against directors at firms that appear to materially backdate, but do not issue a restatement. Among firms that did not issue a restatement, we find no statistical relation between the likelihood the firm backdated and the likelihood of receiving a vote withheld recommendation from ISS. Overall, these results suggest that ISS recommendations were the channel through which new independent directors at restating firms faced a voting penalty.

We next examine whether new directors at restatement firms also faced elevated levels of turnover. Examining all instances of turnover, univariate and multivariate models indicate that turnover rates are 6.3% and 6.9% higher at restating firms, respectively, which is around 31% of the sample means.[5] Broadly, this evidence is consistent with restatements serving as a definitive board failure, even among directors who were not responsible for the misreporting, and even though explicit SEC guidance required restatements for backdating issues.

[5] Prior studies (Bereskin & Smith, 2014; Ertimur, Ferri, & Maber, 2012) do not find higher turnover rates among new directors. The difference is attributable to the identification of backdating firms. Prior studies examine all firms implicated in backdating, such as those facing SEC investigations. As noted in section 4, if we include all firms implicated in backdating, we no longer find a statistically higher rate of turnover among new independent directors.

To examine CEO and CFO career penalties, we first match new executives at restatement firms to new executives at suspect firms based on the propensity to issue a restatement. Our design appears to match firms relatively well, as we find no statistically significant difference in 9 of 13 (11 of 13) firm-level dimensions for the CEO (CFO) sample. Turning to our main results, we find that new CEOs at restating firms turnover at more than twice the rate of new CEOs at control firms (42.5% versus 15.0%, Z-Stat = 2.72). We then examine the individual circumstances of each turnover to determine whether it appears forced.[6] Results are similar in magnitude (22.5% for new restating CEOs versus 10.0% for new control CEOs), but the difference is no longer statistically significant (Z-Stat = 1.52). In regards to CFOs, we find that new CFOs at restating firms, relative to new CFOs at control firms, face statistically higher levels of both all turnover (66.0% versus 35.9%, Z-Stat = 3.11) and forced turnover (24.5% versus 1.9%, Z-Stat = 3.44). Therefore, it appears that new executives faced penalties if the firm issued a restatement relative to new executives at firms that appear to materially backdate, but do not issue a restatement.

While our empirical tests suggest that managers face career penalties when the firm issues a restatement, we note four limitations to our setting. First, we cannot observe internal actions, such as changing policies and strengthening internal controls to prevent backdating from occurring. However, neither could proxy advisors, shareholders, journalists, or others who exerted pressure on firms and affected managerial careers. Second, we cannot publicly observe who uncovered backdating or forced the restatement. It is possible that other actors catalyzed the restatement and managers faced penalties for their tepid response. However, we find no penalties

[6] In particular, we examine 1) the language of the press release 2) the age of the executive 3) how long it took the executive to obtain subsequent employment and 4) whether the subsequent position and salary is comparable forced turnover, similar to prior studies (Efendi, Files, Ouyang, & Swanson, 2013; Huson, Parrino, & Starks, 2001).

at firms that appear to backdate based on statistical properties of options grants, and those firms likely faced similar pressures to respond. Third, we examine only observable penalties, and managers could face other incentives (such as personal integrity) to reveal misbehavior. Fourth, while the backdating setting is unique in providing an opportunity to examine penalties for revealing misreporting, it also limits the generalizability of our results.

We are not alone in suggesting that managers not involved in financial misconduct still face career penalties. Groysberg et al. (2017) find that even alumni employees who were not present during the financial misconduct still received lower future compensation after the misconduct was revealed. And, Choi and Gipper (2019) find that rank and file employees at fraud firms suffer long lasting stigma that affects future career consequences.

Similarly, other studies suggest that the disclosure of adverse news can lead to detrimental effects. At the executive level, Solomon and Soltes (2019) compare CEO turnover between firms that do and do not voluntarily disclose ongoing SEC investigations. They find that CEO turnover is 14% higher among firms that disclose the investigation relative to CEOs at firms that are also undergoing an SEC investigation, but do not reveal it. At the firm level, Files et al. (2009) show that abnormal returns are lower and litigation is more likely as the firm discloses restatements more transparently. Our results are consistent with both groups of studies in that we find that after firms reveal misreporting, firms are motivated to remove even managers who were not present when the misreporting occurred.

Other studies suggests that many companies that backdated did not correct the errors by restating. Heron and Lie (2009) estimate that over 2,000 firms backdated, but other studies find that under 300 were actually implicated in backdating (Ertimur, Ferri, & Maber, 2012; Efendi, Files, Ouyang, & Swanson, 2013). Curtis et al. (2018) estimate that 15% of firms that materially

backdated issued a restatement. Dechow and Tan (2019) find that law firms associated with corporations that issue a backdating restatement are also associated with suspect companies (suggesting that these companies likely backdated but did not issue a restatement). We complement these studies by identifying incentives for managers to avoid issuing backdating restatements (i.e. to avoid career penalties).

Finally, in building our models and interpreting our results, we leverage prior studies that examine consequences to executives and directors implicated in backdating (Bereskin & Smith, 2014; Efendi, Files, Ouyang, & Swanson, 2013; Ertimur, Ferri, & Maber, 2012). Our contribution to this literature is in examining consequences (or lack thereof) to managers at suspect firms and analyzing new managers who were not present during the backdating period.

II. BACKGROUND AND HYPOTHESES DEVELOPMENT

Background on stock option backdating

Stock option backdating refers to the process of granting "in the money" stock options (i.e. when the stock price exceeds the strike price), but avoiding the appropriate compensation expense by moving the measurement date to a day when the stock price was lower. Backdating generally took place prior to 2002 because the SEC did not require firms to provide immediate notice of an option grant. Instead, the SEC granted 45 calendar days to report stock option grants, which provided a wide window to set the measurement at a prior date when the stock price was lower. In addition, accounting standards in effect at the time required companies to record compensation expense equivalent to the intrinsic value of the grant (i.e. the difference between the strike price and the price on the measurement date multiplied by the number of options granted). As such, if the strike price were set equivalent to the stock price as of the measurement date, no compensation expense was required. Companies that set the measurement date at an earlier date when the stock price was lower effectively granted an in-the-money option

while avoiding compensation expense.

The investing public was generally unaware of the practice of stock option backdating until the Wall Street Journal exposed the practice through a series of articles, which ultimately received a Pulitzer Prize award. The first, published on November 11, 2005 highlighted research demonstrating that stock prices fell (on average) prior to the issuance of grants, and rose subsequently thereafter, creating a "V" shape with the grant falling at a low price so frequently that the observable patterns could not be attributed to randomness (Maremont, 2005). The second article, published on March 18, 2006 and entitled "Perfect Payday", documented the astronomical odds (e.g. one in three hundred billion), that specific grants at several companies were issued on the day of such a low stock price.

Hypotheses

To determine whether revealing misreporting affects the careers of directors and executives, we examine career consequences to the public revelation of the backdating scandal. We compare new managers at restatement firms with new managers at suspect firms as both groups show evidence of backdating, but differ in their revelation of backdating. We focus on new managers as they were involved in the firm's revelation of backdating, but not the underlying backdating.

Shareholder Voting in Director Elections

The first career consequence we examine is the proportion of votes that shareholders withhold against individual directors. This continuous variable captures subtle shifts in shareholder dissatisfaction, which illuminates the pressures and incentives facing directors. Further, votes withheld can lead to career consequences for directors, even when the director remains on the board after the election (Aggarwal, Dahiya, & Prabhala, 2019).

New directors at restatement firms might receive more votes withheld after the backdating scandal than new directors at suspect firms. Ertimur et al. (2012) find that new directors at firms implicated in backdating received more votes withheld than new directors at all other firms. This suggests shareholders interpret a backdating restatement as a governance failure. Further, some shareholders might actually prefer that firms do not reveal backdating, and instead avoid the crisis, entirely. After all, this might prevent the firm from facing costs associated with implication in the backdating scandal. Curtis et al. (2018) show that the number of backdating firms greatly exceeds the number of firms that revealed backdating suggesting that at least some firms perceived silence as the most appropriate strategy.

Alternatively, new directors at restatement firms might receive fewer votes withheld after the backdating scandal than new directors at suspect firms. Shareholders could have interpreted a restatement as evidence of vigilance and transparency. After all, restatement firms often disclosed extensive efforts to remediate the control deficiencies discovered, which may reduce the costs associated with future misreporting. Further, admitting to wrongdoing (by issuing a restatement) often reduces the expected consequences to misreporting, such as regulatory penalties (Files, 2012). Finally, since the board was ultimately responsible for firms' responses to backdating, directors at suspect firms could have faced penalties for failing to respond more proactively to these pressures relative to directors at restating firms. In short, relative to suspect firms, restating firms demonstrated responsiveness and transparency, which shareholders might reward.

Given the lack of a clear ex ante prediction, we state the first hypothesis non-directionally:

H1: The proportion of votes withheld following the backdating scandal is no different

between new directors at firms that issue a backdating restatement and new directors at suspect firms.

Role of Proxy Advisors

We next examine whether proxy advisors responded to the unfolding backdating scandal by issuing adverse recommendations against (suspect) firms that appeared to backdate, but did not issue a restatement. The role proxy advisors play in influencing shareholder voting (and corporate governance more broadly) is timely given that their recommendations have a large effect on shareholder votes (Malenko & Shen, 2016; Cai, Garner, & Walkling, 2009), and the SEC has recently proposed new rules affecting proxy advisor regulation.[7] Further, it is unclear how proxy advisors would have responded to suspect firms that show strong evidence of having backdated without a restatement.

On one hand, proxy advisors may have avoided issuing adverse recommendations when firms exhibited statistical evidence of backdating absent a restatement. Without an admission of wronging from the firm, proxy advisors might have faced legal consequences. In fact, proxy advisors have faced legal challenges based on adverse recommendations in the past.[8] Therefore, a recommendation against a director based on the statistical inference of backdating might have created legal risk that ISS could readily avoid by staying silent.

On the other hand, proxy advisors could have identified suspect firms and recommended against directors at these firms for failing to address the potential GAAP violation or the internal

[7] See SEC release number 34-87457 "Amendments to Exemptions from the Proxy Rules for Proxy Voting Advice" issued on November 5, 2019.

[8]For example, in 2003, an executive director sued ISS alleging that statements in a report were defamatory when ISS sided against him in a contested director election (Vitale v. ISS, 8:03-cv-01737-PMJ, Dist. of Maryland, July 12, 2003.) More recently, former SEC commission Paul Atkins, who served during the post-period of our study, encouraged firms to challenge adverse ISS recommendations through litigation when based on incorrect information (Orol, 2014).

control weaknesses. This could be the case as proxy advisors promote their expertise in governance and analysis. In fact, on its webpage, ISS touts its ability to provide "high-quality data, analytics, and insight".[9] Proxy advisors had the ability to analyze option grants and could have viewed the backdating scandal as an opportunity to showcase this expertise.

Given the lack of a clear ex ante prediction, we state the second hypothesis non-directionally:

> **H2**: Among firms that did not issue a backdating restatement, the likelihood of a proxy advisor issuing an adverse recommendation for a director is no different between firms that show evidence of backdating and firms that do not.

Managerial Turnover

We next examine whether turnover rates for new directors and executives ("managers") differs between restatement and suspect firms. As a labor market incentive, turnover reflects potentially the sharpest career penalty. However, as a binary outcome, it may not capture more subtle shifts in shareholder dissatisfaction. Also, independent directors in our sample may turnover voluntarily. To some extent, our empirical design mitigates the concern that directors in our sample depart voluntarily.[10] And, as described below, we also examined forced turnover, which is less susceptible to this criticism. Therefore, we view the turnover analyses as providing evidence corroborating the results of the director voting tests.

On one hand, firms might rationally turn over new managers at restatement firms at a higher rate than new managers at suspect firms. First, new managers at restating firms could turn over simply because the operating environment has changed and the firm needs executives and

[9] See https://www.issgovernance.com/about/about-iss/#1570776311994-db534a1e-7bb2 (accessed 1/2020).
[10] Dou (2017) demonstrates that directors who leave immediately after a negative event (such as those in our sample) are often most accountable. Therefore, director departures captured in our sample are less likely to be voluntarily initiated by a non involved party than the typical director departure.

directors with a different skillset who could repair the firm's reputation. Studies show that managerial turnover following a restatement does enhance financial reporting credibility (Chakravarthy, deHaan, & Rajgopal, 2014; Wilson, 2008; Chen, Cheng, & Lo, 2014). Second, other studies show that misreporting damaged the careers of employees not involved in the misreporting such as alumni (Groysberg, Lin, & Serafeim, 2017) and rank and file personnel (Choi & Gipper, 2019). These results are consistent with experiments in social psychology documenting a stigma that could lead to penalties even absent any culpability (Goldstein & Johnson, 1997; Pryor, Reeder, & Monroe, 2012; Hebl & Mannix, 2003). Third, organizations such as Glass Lewis and NERA Economic Consulting tracked firms associated with backdating and placed any firm that issued a restatement on a public list. Firms on the list faced media scrutiny and shareholder distrust, and likely felt pressured to "do something," which might include replacing managers, regardless of their tenure.

On the other hand, new managers at suspect firms might face higher turnover rates than new managers at restatement firms. As noted above, restatement firms demonstrated vigilance and often revealed efforts to improve internal controls and possibly reduce the likelihood of future accounting errors. To the extent that the labor market interprets restatements as governance successes and views suspect firms as having failed in this respect, managers at suspect firms could see penalties in excess of managers at restatement firms.

Finally, since the new managers were not present when the backdating occurred, the turnover rates may be no different, regardless of whether the firm issued a restatement or not. Terminating a manager absent definitive evidence of backdating, or for governance issues that predate their tenure could potentially violate employment agreements, or lead to wrongful termination lawsuits.

In terms of directors, although prior literature examines turnover rates among new directors at backdating firms, results are not consistent. Bereskin and Smith (2014) find that new directors at firms implicated in backdating were more likely to lose reelection than early directors, whereas Ertimur et al. (2012) do not find higher rates of turnover among new directors at firms implicated in backdating. Further, these studies do not examine whether new directors at suspect firms faced career penalties.

Similarly, in terms of executives, prior literature does not inform our hypothesis. Efendi et al. (2013) find higher turnover rates of CEOs and CFOs of firms associated with backdating relative to a propensity matched sample. However, Efendi et al. (2013) does not identify suspect firms or examine new executives.

Given the lack of a clear ex ante predictions, we the final hypotheses non-directionally:

H3: Turnover rates are no different between new directors at firms that issue a backdating restatement and new directors at suspect firms.

H4: Turnover rates are no different between new executives at firms that issue a backdating restatement and new executives at suspect firms.

III. EMPIRICAL DESIGN

Sample

To create our samples of executives and independent directors, we gather accounting data from Compustat, executive data from Execucomp, director data from Boardex, returns from CRSP, restatement data from Audit Analytics, stock granting data from Thomson Reuters, institutional ownership data from Factset, and director voting results from ISS voting analytics.[11]

[11] We focus on independent directors as they played an important role in determining how the firm would respond to inferences of backdating. New regulations gave independent directors a stronger voice in corporate governance just as the backdating scandal began unfolding. The Sarbanes Oxley Act of 2002 requires that all members of the audit committee are independent, and the NYSE and Nasdaq adopted new rules requiring that a majority of directors on the board are independent.

We also collect director and executive identities by searching proxy statements and annual reports, when missing in these datasets. Appendix 3 documents a description of all variables as well as the data sources.

Table 1 documents the executive turnover, director turnover and director voting samples. We start with 4,352 firms with coverage in CRSP and Compustat for the entire backdating revelation period (roughly 2005-2007). Of these, 3,496 issued stock option grants from 1996-2002 (the backdating period). Director data is available in Boardex for 2,240 firms, we can identify all requisite control variables for 2,114 firms. Of these, 31 firms disclosed an internal investigation that did not uncover backdating. Since these firms were not clouded in the suspicion of backdating, we drop this group. This leads to a director turnover sample of 336 firms composed of 78 firms that issued a backdating restatement, and 258 firms that demonstrate strong evidence of having backdated, but do not issue a restatement (using the methodology described in appendix 2).[12]

While the number of restatements may seem low compared to other studies examining backdating, we isolate backdating restatements (as opposed to identifying firms implicated in backdating) and impose stricter data requirements.[13] Panel B documents a similar evolution of the sample with the added data restrictions of ISS recommendations leading to a sample of 184

[12] Audit Analytics categorizes three types of restatements related to stock options: "deferred, stock-based options backdating only", "deferred, stock-based and/or executive comp issues", and "deferred, stock-based SFAS 123 only". We reviewed the disclosures of all three types issued between November of 2005 and December of 2007 and included them in our sample if the description suggested evidence of backdating (as opposed to other issues such as incorrectly estimating the Black Scholes model). We also reviewed any restatements in the Glass Lewis list that were not captured by this search and include them if they indicate that the firm backdated.

[13] For example, Efendi et al. (2013) find 141 backdating firms, Ertimur et al. (2012) find 186 and Bernile and Jarrell (2009) find 129. Our study differs from these in that we capture only firms that issued a restatement as opposed to disclosing an investigation, or admitting to non-material backdating, and we impose additional data restrictions. Our sample would include 171 backdating firms if we were to eliminate these data restrictions and use a more inclusive definition of backdating consistent with these studies.

firms and 672 independent directors. As discussed below, we identify new managers as those who joined the firm after August of 2002, when the SEC changed reporting requirements and backdating largely ceased (Heron & Lie, 2007).[14]

Director Voting Model

To test hypothesis 1, we capture voting results disclosed within a two-year window pre-backdating (November 2003) and post-backdating (June 2007) to reduce the likelihood that non-backdating events affect votes. Therefore, at most we can capture two voting cycles. For classified boards (roughly half the firms), we will not capture voting data for one class of directors (or roughly one-third of directors). However, we often miss more than one class because the timing of the annual meeting does not correspond to our specific window. We measure votes withheld in the post period as of the first meeting the director stood for election after the backdating scandal broke (June 2007), and control for votes withheld prior to the backdating scandal (during the latest voting meeting prior to November of 2005).

We cannot use the actual restatement dates because suspect firms in our sample do not have a specific date upon which backdating was publicly acknowledged, precluding specific "pre" and "post" periods for these firms. See Appendix 1 for a timeline of backdating events and our estimation windows.

To test hypothesis 1, we estimate the following director-level, cross-sectional OLS model:

$$\begin{aligned} \textit{Votes Withheld} = {} & \beta_0 + \beta_1 \textit{Restatement Indicator} + \beta_2 \textit{Early Director} \\ & \textit{Indicator} + \beta_3 \textit{Restatement Indicator x Early Director Indicator} + \\ & \beta_i \textstyle\sum_i \textit{Director Controls} + \beta_j \sum_j \textit{Firm Controls} + \varepsilon \end{aligned} \quad (1)$$

A statistically significant coefficient for β_1 would indicate that new directors at restating

[14] Although the rule change took effect in August we remove all directors who joined the board in 2002 as the proxy statement often documents the year the director joined and not the specific date.

firms face higher votes withheld relative to new directors at suspect firms, which would support rejecting hypothesis 1. A statistically significant coefficient for β_3 would indicate that early directors at restatement firms face higher levels of votes withheld, relative to new directors at restatement firms.

Similar to Ertimur et al. (2012), we control for a variety of director- and firm-level variables such as the proportion of director votes withheld in the prior election, ROA, size, abnormal return, the dollar amount of common stock holdings, gender, age, and whether it was the director's first year on the board, as these are all associated with director penalties for reasons unrelated to stock option backdating.

Proxy Advisor Recommendation Model

We next examine whether directors at firms that show strong evidence of having backdated are more likely to receive a vote-withheld recommendation from a proxy advisor. To do so, we obtain proxy recommendations from the Institutional Shareholders Services group (ISS), which is one of the largest proxy advisor firms covering 44,000 shareholder meetings in 115 countries.[15] We then merge these recommendations with our director voting sample and eliminate any firms that issue a restatement or announce a backdating investigation as these firms were not under the suspicion of having backdated. We then test whether the likelihood of having backdated correlates with the likelihood of receiving a vote withheld recommendation from ISS. We do so by estimating the following firm-level, cross-sectional OLS models:[16]

$$ISS\ Withholding\ recommendation = \beta_0 + \beta_i Backdating\ Likelihood + \beta_j \sum_i Director\ Controls + \beta_k \sum_j Firm\ Controls + \varepsilon \quad (2)$$

[15] See ISS's website at: https://www.issgovernance.com/about/about-iss/#1570776311994-db534a1e-7bb2 (accessed 1/2020).

[16] Although the dependent variable is binary, we use OLS models because our model includes an interaction, and interpreting interaction coefficients in non-linear models is problematic (Ai & Norton, 2003; Greene, 2010).

We use three variables to identify the likelihood the firm backdated: an indicator whether the firm is a suspect firm in the prior test, the likelihood the firm issued backdating grants, and the estimated materiality of backdating. We use the same director and firm controls as in equation 1. A statistically significant coefficient for β_i would indicate that ISS incorporated backdating inferences in its voting recommendations.

Director Turnover Model

We next model director turnover, as this powerful incentive likely affects behavior. We include all directors on the board as of the latest voting meeting prior to November of 2005, just before the Wall Street Journal broke the backdating scandal in late 2005. Similarly, in June 2007, Glass Lewis published a report identifying all firms publicly implicated in BD (Glass Lewis & Co., 2007). We measure turnover in the post period as of the first meeting after the Glass Lewis report where the director would have stood for election.

To test hypothesis 3, we estimate the following OLS model similar to equation 1:

$$\begin{aligned} &\textit{Director Turnover} = \beta_0 + \beta_1 \textit{Restatement Indicator} + \beta_2 \textit{Early Director} \\ &\textit{Indicator} + \beta_3 \textit{Restatement Indicator x Early Director Indicator} + \\ &\beta_i \textstyle\sum_i \textit{Director Controls} + \beta_j \sum_j \textit{Firm Controls} + \varepsilon \end{aligned} \quad (3)$$

A statistically significant coefficient for β_1 would indicate that new directors at restating firms face a greater likelihood of turnover relative to new directors at suspect firms, which would support rejecting hypothesis 3. A statistically significant coefficient for β_3 would indicate that early directors at restatement firms face higher levels of turnover, relative to new directors at restatement firms.

Similar to model 1, we control for a variety of director- and firm-level variables. The only difference is that we control for the prior turnover rate of directors as this might affect the current turnover rate independent from backdating. And, we do not control for the proportion of votes withheld in the prior election as this restricts the sample and is not necessary since we are

not examining the change in votes driven by backdating.

Executive Turnover Model

Finally, we identify the CEO and CFO who was in place as of 2005, and also measure control variables as of 2005.[17] We initially identify an executive as having turned over if he or she was no longer present at the firm (in any executive or director capacity) in 2008. We use a roughly two-year window (2006-2008) as this is the period during which firms were responding to backdating allegations (94% of the backdating restatements occurred in this window).

As turnover may be voluntary, we then identify cases in which turnover appears to be forced, similar to the process employed by Huson et al. (2001) and Efendi et al. (2013).[18] We start by matching each executive at a restating firm with an executive at a suspect firm. We limit the support because characterization of turnover involves an examination, on a case-by-case basis, of press releases, biographical databases, LinkedIn, and other sources and websites to track executives' careers. To match restatement and suspect executives, we employ a propensity matched regression which predicts the likelihood of a restatement using log assets and abnormal return. We then match the executive of the restatement firm with the suspect executive who has the closest propensity from the probit model, and is in the same classification (i.e. matching new restatement CEOs to new suspect CEOs). Because we want to ensure that the matched firm

[17] November of 2005 was the publication date of the first article in the Wall Street Journal (also covered by other mainstream publication outlets such as CFO.com) documenting the backdating scandal. Arguably, the more famous Wall Street Journal article ("Perfect Payday") triggered the backdating scandal four months later in March, 2006 (Forelle, 2006). However, we chose the earlier date to capture any penalties that might be attributable to backdating. See Appendix 1 for a timeline.

[18] To ensure that our methodology for identifying forced turnover is consistent with prior research, we obtained executive turnover data from one of the authors of Efendi et al. (2013) and compared our assessment with theirs. Our assessment was the same for 30 of the 37 CEOs and 33 of the 44 CFOs that overlapped both samples. Of the 7 CEO differences, 2 were attributable to the fact that we identify turnover over a longer horizon. Of the remaining 5 differences, 2 weakened the inferences we draw (i.e. we do not identify restating executives as having forced turned over whereas Efendi et al. (2013) do). Of the 11 CFO differences, 8 were attributable to a different horizon and all 3 remaining differences weakened the inferences we draw.

appears to backdate to the same degree as the restatement firm, we limit the suspect firms to those with a materiality ratio in excess of 5%.[19] While this test does not incorporate the effect of unobservable characteristics in our tests, propensity-matching tests are closer to random assignment and produce results with minimal bias (Heckman, Ichimura, & Todd, 1997). We also test for similarities across a number of dimensions not included in the model.

After obtaining matches, we examine all cases of turnover and classify the turnover as forced if (1) the announcement clearly indicated that the turnover was forced (2) the announcement does not mention the health of the executive, he or she does not remain at the firm in any capacity, and he or she is less than 60 years old or (3) the announcement indicates retirement but the individual leaves within 6 months of the announcement. If (2) or (3) occurs, we track the career of the executive. If he or she receives a comparable job title or higher compensation within 6 months, or does not obtain future employment (except a board seat) indicating that the individual retired we reclassify the turnover as non-forced.

To test hypothesis 4, we then compare forced turnover rates between new executives at restatement and suspect firms. As the restatement and suspect firms are propensity matched, we draw inferences from these comparisons of mean turnover rates between the samples.

IV. EMPIRICAL RESULTS

Tests of Director Votes Withheld and Turnover

Table 2 documents univariate differences in backdating likelihood, materiality ratio, votes withheld and turnover between restatement and suspect firms. Suspect firms have a higher

[19] Alternatively, we could include the backdating likelihood and materiality ratio in the propensity matching model. However, we find that the coefficient on backdating likelihood variable in this model is statistically significant, but negative. Therefore, including this variable biases the model toward selecting matching firms that are less likely to have backdated. Further, the materiality ratio in this model is not statistically significant. If we do not limit suspect firms to those with a higher materiality ratio, we find that the materiality ratio of the matched suspect firms is much lower than that of the restating firms.

backdating likelihood, and lower materiality ratio, although the latter is not statistically different. Importantly, the director turnover rate and level of votes withheld is higher for directors at restatement firms relative to suspect firms, regardless of when the director joined the board. New directors at restating firms have 5.4% more votes withheld and a 6.3% higher rate of turnover than new directors at suspect firms. These rates are very similar to those reported in Ertimur et al. (2012) although we detect higher rates for restatement firms.[20] Economically, these differences reflect an increase of 88% and 48% of the vote withheld and turnover rate for directors at suspect firms.

Panel A of Table 3 documents multivariate OLS models of director votes withheld and turnover. Similar to univariate tests, new directors at restating firms faced higher levels of votes withheld and turnover than new directors at suspect firms. Model 1 documents tests of votes withheld and hypothesis 1. The restatement indicator is positive and statistically significant (T-stat = 2.62) indicating that new directors at restating firms received 5.86% more votes withheld than new directors at suspect firms, which is 44% of the sample mean. Model 2 indicates that new directors at firms that issue a restatement are 6.90% more likely to turnover, a statistically significant level relative to new directors at suspect firms (T-stat = 1.87).[21] Economically, this is 31.4% of the sample mean. These results suggest rejecting hypotheses 1 and 3 by concluding that new directors at restating firms faced career penalties relative to new directors at suspect firms.

[20] This is likely because restating firms are the most serious cases of backdating whereas the backdating sample in Ertimur et al. (2012) include all firms associated with backdating.

[21] Ertimur et al. (2012) and Bereskin and Smith (2014) do not find a statistically higher rate of turnover among new directors at firms implicated in backdating. The difference is attributable to the fact that we examine the most serious cases of backdating (that involved a restatement) whereas these studies examine all firms implicated in backdating, and also because our control sample includes suspect firms. In untabulated results, if we estimate equation 2 and examine all firms implicated in backdating, we find that the coefficient on the implicated indicator is 5.91, but did not statistically significant (T-stat = 1.62).

Role of ISS in Votes Withheld

Given that new directors at restating firms faced higher votes withheld than new directors at suspect firms, we next examine the role of proxy advisor recommendations in influencing these votes withheld. Since ISS recommendations have an economically large effect on director voting (Cai, Garner, & Walkling, 2009; Malenko & Shen, 2016), it is possible that restating directors face a voting penalty because the ISS recommends withholding votes against new restating directors. In fact, during the backdating scandal, ISS had an explicit policy to "recommend shareholders withhold votes from…current compensation committee members who fail to respond to the issue proactively (Institutional Shareholder Services, 2007)." See appendix 4 for an illustration of ISS's recommendation to withhold votes against two new directors of Black Box Corporation in 2007.

We obtain data on ISS voting recommendations and replicate the votes withheld model while controlling for the ISS recommendation. Results, presented in Panel B of Table 3, indicate that the voting penalty levied on new directors at restating firms falls to 3.53, but is still statistically significant at conventional levels (T-stat = 2.46).

Since ISS recommends withholding votes for a variety of non-backdating reasons, we next obtain the voting report for the restatement firms and identify those for which ISS recommends withholding votes for backdating reasons. We read these reports, code a new variable for recommendations influenced by backdating, and replicate the voting model, while controlling for this new variable. Results, presented in Panel B of Table 3, indicate that the voting penalty levied on new directors at restating firms falls to 2.41, and is no longer statistically significant at conventional levels (T-stat = 1.47). This suggests that ISS recommendations are a channel whereby new restating directors receive higher votes withheld.

We then test hypothesis 2 and examine whether the likelihood that the firm backdated is associated with an ISS vote withheld recommendation, among firms that did not issue a backdating restatement. Results, presented in Table 4, indicate that the likelihood of ISS issuing a vote withheld recommendation is statistically unrelated to the likelihood that the firm backdated. These results complement the prior analyses, and indicate that new independent directors at firms that appear to backdate avoided a vote withheld recommendation from ISS, as long as the firm did not issue a restatement. Perhaps ISS did not identify backdating firms out of concern that recommending against a director without definitive proof of wrongdoing could lead to legal troubles.

In summary, directors at firms that issued a backdating restatement faced the risk that ISS would disagree with their remediation approach and recommend against the director, leading to a voting penalty. On the other hand, directors at firms that appear to backdate but did not issue a restatement avoided ISS scrutiny. This suggest that ISS is the channel through which new independent directors at restating firms faced higher votes withheld. Without the ISS recommendation related to backdating, new directors at restating firms did not face statistically higher levels of votes withheld.

Tests of Executive Turnover

Panel A of Table 5 documents the results of the matching process of the CEO sample. The restatement sample is similar to the matched sample along most of the covariate dimensions. The main differences are that the matched sample has a higher likelihood of backdating (0.98 versus 0.93), leverage (0.20 versus 0.13), and lower tenure (4.85 versus 6.70), ROA (0.01 versus 0.09), institutional ownership (0.62 versus 0.79) and is less likely to operate in the tech sector (0.41 versus 0.58). Finally, although the turnover window in this study is longer than that in

Efendi et al. (2013), the rates of all and forced turnover for the CEO and CFO samples are similar.[22]

Panels B and C of Table 5 document propensity matched tests of CEO turnover. Panel B indicates that new CEOs at restating firms turned over at a rate more than double that of the new CEOs at suspect firms (42.5% versus 15.0%, Z-Stat = 2.72). Similarly, examining forced turnover, Panel C indicates that new CEOs at restating firms also faced forced turnover rates much higher than new CFOs at suspect firms, although the difference is not statistically significant at conventional levels (22.5% versus 10.0%, Z-Stat = 1.52).

Table 6 document the same tests for the CFO sample. The matching for the CFO sample, see Panel A, documents an even closer match than the CEO sample. The only statistically significant differences between the restatement and matched sample are that the matched sample has a higher backdating likelihood (0.97 versus 0.93), leverage (0.25 versus 0.13), and is less likely to operate in the tech sector (0.44 versus 0.57). Turning to the hypothesis testing, Panels B and C indicate that new CFOs at restating firms turned over at a rate roughly double that of the new CFOs at suspect firms (66.0% versus 35.9%, Z-Stat = 3.11). Forced turnover comparisons are even more striking as Panel C indicates that new CFOs at restating firms also faced forced turnover rates much higher than new CFOs at suspect firms (24.5% versus 1.9%, Z-Stat = 3.44).[23]

In summary, evidence is consistent with rejecting hypothesis 4 and finding higher levels of turnover among new executives at restatement firms relative to new executives at suspect

[22] The turnover (forced turnover) rates for CEOs reported in Efendi et al. (2013) for backdating and control firms is 29.8% (24.1%) and 6.4% (5.0%) respectively. The same rates for the CFO sample are 34.8% (24.8%) and 19.9% (7.8%).

[23] The forced turnover rate of new CEOs at restatement firms is similar to the rates reported by Efendi et al. (2013) of backdating firms (24.1%), although they do not examine new executives separately.

firms. Overall, univariate, multivariate and matched sample tests indicate that, when it comes to new managers' careers, disclosing a restatement is an adverse event.

Additional Tests

It is possible that, although the executive was not in the CEO or CFO position during the backdating period, he or she could have been employed by the backdating firm in a different position. To determine whether this was the case, we examine each new restating executive and partition the sample according to whether he or she was internally or externally promoted. We find no difference in turnover rates across these groups. We do note that, although several new restating CFOs were internally hired, none were with the firm during the backdating period.

We then test whether new CEOs at restating firms face a changing compensation structure. This provides some evidence that backdating restatements led to fundamental changes within these firms, and not just the departure of new executives. In untabulated tests, we find that restating firms with new CEOs and CFOs issued around 0.01% fewer option grants to the CEO and CFO after backdating (T-stat = 1.67 and 1.78, respectively). While we also find that the proportion of total salary consisting of option grants declined for new CEOs and CFOs at restatement firms, the difference is not statistically significant (T-stat = 1.47 and 1.21, respectively). Efendi et al. (2013) also find that the proportion of CEO compensation consisting of option grants declined for backdating firms.[24] This provides some evidence that the compensation structure for new executives changed after the firm issued a backdating restatement, although the changes may have been modest.

Since the hiring of independent directors is not random, unobservable characteristics that

[24] In particular, Efendi et al. (2013) find a 22.2% decline in the decline in the proportion of CEO compensation consisting of option grants post backdating, and a 0.1% decline in the change in the number of grants issued to backdating CEOs. The first result is higher than our estimate of 8.9%, perhaps attributable to a different sample composition, while the second result is the same.

affect the decision to hire independent directors could also affect the penalties shareholders levy on the director after the backdating scandal materializes. Therefore, we follow prior studies (Armstrong, Core, & Guay, 2014; Duchin, Matsusaka, & Ozbas, 2010) and utilize the new requirements stock exchanges imposed on firms as a shock to the hiring of independent directors. If unobservable firm characteristics drive the hiring of independent directors and the penalties detected, we would expect to see greater penalties levied on directors that were at firms whose choice of hiring were unconstrained by the regulations (i.e. the firms already in compliance). We estimate prior models of director turnover and votes withheld and partition the sample of new directors at restating firms according to whether they joined the board of a firm that was already in compliance with the new requirements (and were not required to hire additional independent directors). Untabulated tests indicate no difference in penalties between new directors at restating firms regardless of whether the firm complied with the new independence requirement.

Thus far, empirical tests focus on penalties levied on directors at firms associated with backdating. In reality, a director weighs these penalties against opportunities to sit on boards outside the backdating firm. For instance, developing a reputation as a diligent monitor by revealing backdating behavior could lead to gains in additional board seats, which more than offset the loss of the board seat at a backdating firm. Similarly, directors at firms that do not respond to the inference of backdating could face lower external opportunities. That said, Ertimur et al. (2012) find no change in outside board seats among directors associated with backdating.

To test this, we measure the change in total board seats held by directors in 2010 relative to 2005. This count excludes any seats on a firm associated with backdating (either by restating, investigating without a restatement, or in the suspect backdating sample), and includes seats on

all firms-public and private. We also examine whether the director increased or decreased total board seats over this period, and conduct multivariate tests controlling for a variety of characteristics of the individual director (gender, age) and the firm (ROA, Tenure and whether the director served on the audit committee). The variables of interest, similar to models 1 and 2, are the interactions between restatement and early director. We estimate these models at the director level, and capture the maximum value of the firm-level characteristics among the director's portfolio of non-backdating board seats. In untabulated analysis, we find that none of the coefficients on any interaction terms in any models are statistically different from zero. This suggests that gains in board seats at additional firms do not offset the penalties previously documented at firms that restate. Similarly, directors at suspect firms suffer no observable consequences in the external labor market.

V. CONCLUSION

We examine whether directors and executives face career penalties when firms correct misreporting that occurred prior to their tenure. Our primary samples include managers at firms that either issued a backdating restatement, or showed strong evidence of backdating, but did not respond to the unfolding backdating crisis. We find that directors and executives who joined firms after backdating ceased face career penalties if the firm issued a restatement, relative to managers at firms that appear to backdate, but remain silent. We conduct a variety of tests to rule out alternative explanations for these penalties such as new managers leaving voluntarily, having culpability in backdating, being internally promoted, or the endogenous assignment of directors to firms. Ultimately, we find no evidence to support these explanations. However, we do find evidence suggesting that ISS recommendations to vote against new directors at restating firms is the primary channel of the director voting results. After removing the effect of ISS voting

recommendations because of backdating, we detect no observable difference in voting results between new directors at restating and suspect firms.

Overall, results suggest that executives and directors associated with backdating restatements face career penalties, even if they were not present at the time the backdating occurred. We note that these penalties could be rational as the evolving needs of the firm could require managers skilled at repairing the firm's reputation. Although replacing new managers could be justified for any individual firm, collectively levying penalties in this manner may create an ex ante disincentive to reveal misreporting to the extent that managers foresee these penalties.

Our conclusions are subject to certain caveats. First, while the research design includes several elements designed to limit the culpability of new executives and directors, it is possible that some were involved in backdating. Second, the firms we identify as suspect backdating may not have issued backdated stock option grants. This concern is somewhat limited in that our research examines whether directors at firms that appear to have issued a material error face penalties for failing to act on the inference of wrongdoing. Therefore, it is not the actual incidence of backdating that we attempt to capture, but the appearance of it. Third, we cannot possibly analyze all factors that affect executive or director behavior. Therefore, managers could still face unobservable incentives to respond to the inference of misbehavior. Overall, while we attempt to mitigate the likelihood of these risks, we cannot eliminate these concerns.

By examining the relation between career concerns and the revelation of misreporting, we provide some response to longstanding questions that have eluded prior research. Srinivasan (2005) implies that restatements might be perceived as a governance success as they arise only when firms uncover and reveal misreporting. However, we find no evidence that executives or

directors benefit from issuing backdating restatements. Further, we see our study in response to calls for research investigating how career concerns affect corporate disclosures (Beyer, Cohen, Lys, & Walther, 2010; Healy & Palepu, 2001). From a practical standpoint, given that managers face career penalties for the revelation of adverse news, our results highlight the importance of leniency programs (e.g. Files, 2012). Finally, our examination of the role ISS played in the director voting patterns is timely given that the SEC has recently proposed new rules affecting proxy advisor regulation.

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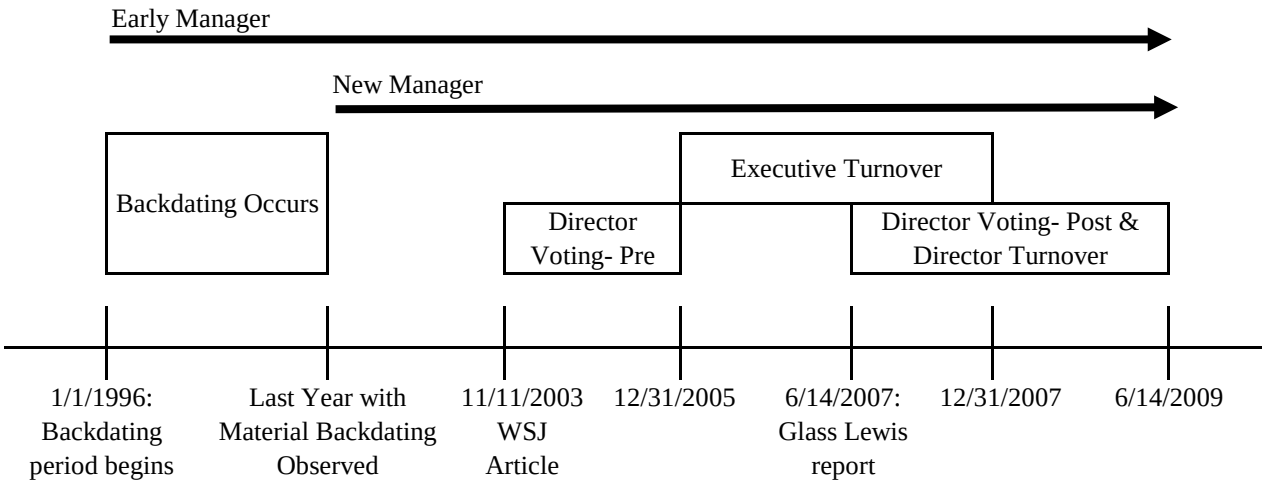
Appendix 1: Timeline of Events (Not Drawn to Scale)



Appendix 2: Methodology for Identifying Suspect Backdating Firms

To determine the likelihood that a particular option grant was backdated, we compute the return on the underlying stock in the 20 trading days before and after the grant, and estimate the return "reversal" as the post-grant change in price minus the pre-grant change. [25] For each firm, we compute the firm's decile of monthly stock price volatility relative to the sample, and compute the reversal for 1,000,000 hypothetical grant dates of a firm in the same volatility decile by randomly drawing from the CRSP Daily Stock Price database over the same period as our sample of option grants. We match the grant to the randomly generated sample of hypothetical grant dates with the same volatility decile and compute the proportion of hypothetical grants with lower reversal. This gives a volatility-controlled estimate of the likelihood that a grant on a randomly chosen date would have reversal at least as large as the observed reversal of the grant.

We then compute a firm-year estimate of the likelihood of having backdated by aggregating the grant-level estimates to the firm-year level. This aggregation is important given variation in grants across firms. We select the grant with the highest probability of backdating and correct for the family-wise error rate, which accounts for the fact that firms issuing many grants are more likely to randomly issue one with a high probability of backdating (Sidak, 1967). To do so, we calculate the firm likelihood of backdating as follows:

$$FirmYearProb = GrantProb^{NGrants}$$

Where FirmYearProb is the firm-year probability of backdating, GrantProb is the highest grant-level probability the firm issued during any year as estimated as described above and NGrants are the number of grants issued by the firm for that year. While other estimates are

[25] This window length approximates the 45 calendar days allotted to firms to file the Form 4 with the SEC after granting stock options. Further, while some firms did issue backdating restatements for periods beyond this date, Heron and Lie (2007) show that option returns patterns indicative of backdating dramatically declined after August of 2002.

possible (such as aggregating the firm-level estimate across all grants with a high probability of backdating), this is a conservative estimate, as it only accounts for the grant with the highest probability and assumes the rest are not backdated. We identify a firm to be suspect backdating if the firm-level probability exceeds 95% in any year.

In addition to estimating the likelihood of backdating, we also account for the materiality of the backdating in our tests to capture a sample of firms that appear to be the most egregious backdaters.[26] Consistent with practitioner discussions (Vorhies, 2005) and empirical evidence on restatement thresholds (Acito, Burks, & Johnson, 2009), we consider a grant to be materially backdated if the difference between the strike price on the grant date and the strike price on our estimated measurement date multiplied by the number of grants exceeds 5% of annual income.

To operationalize this measure, we compare the stock price on the date the grant was issued with the median stock price over a 60 calendar-day window centered on the grant date. The difference between these stock prices reflects the compensation expense the firm should have recognized over the vesting period for one grant if the stock price on the actual measurement date were the median price over the window. We then multiply this by the number of grants issued, and compare it to the benchmark threshold of 5% of yearly income less special items or non-operating items, following Vorhies (2005) and consistent with the finding that net income is the appropriate materiality benchmark. If the expense exceeds the threshold, we identify the grant as material. We create a ratio of yearly materiality (aggregating the materiality of all backdated grants) scaled by annual income less special items or non-operating items, and

[26] Even minor backdating errors could be seen as material because they indicate "particularly egregious circumstances such as self-dealing or misappropriation by management" which are material regardless of error size (ASC 250-10-S99). Consistent with this, Bernile and Jarrell (2009) document significant abnormal negative returns upon the announcement of backdating.

select the highest value to use as a control in firm-level regressions.[27]

To ensure that our results are not dependent on this method of identifying suspect backdating firms, we also follow Bebchuk et al. (2010) and identify backdating firms using a more easily implementable measure: whether firms issued a grant at the lowest price of the month. Results are consistent with prior tests. New directors at firms that issued grants in this fashion (but did not restate) faced lower penalties than new directors at restating firms.

While this methodology captures the essence of the possible error's size relative to the firm's operations, it is subject to the following three limitations. First, this method assumes the median stock price over the 60-day window reflects the stock price on the measurement date. Our estimation of materiality will vary from the actual error to the degree that these prices diverge. Second, we do not account for the income tax effects, although these are likely immaterial. Bernile and Jarrell (2009) examine backdating restatements and find that one-third report higher taxes while the median restatement reduced taxes by a negligible amount (0.09% of market value). Third, APB 25 requires amortizing the expense over the vesting period while our methodology compares the expense to one year of income. We do so because, although the accounting errors arose upon the issuance of the grants from 1996-2002, the errors were not disclosed until the 2005-2007 period. Thus, the magnitude of the error to incorporate into the current period financials would have reflected the aggregate amount of the error that should have already been expensed over the entire period between the grant date and the current period.

[27] We test alternative thresholds for the likelihood of backdating (90% and 99%) and materiality (10% and 15% of net income), and find very similar results.

Appendix 3: Variable Definitions

Samples	Description	Source
Restatement	Indicator variable that takes a value of one if the firm issued a backdating restatement and zero otherwise. Backdating restatements are those labeled by Audit Analytics as "Deferred, stock-based options backdating only". We also reviewed the restatement text for all other stock option restatements issued during the backdating period. See section 3 for more detail.	Audit Analytics
Suspect Backdater	Indicator variable that takes a value of one if the firm has a backdating likelihood above 0.95 and materiality above 0.05 and zero otherwise. See appendix 1 for detail on how we estimate backdating likelihood and materiality.	Thomson Financials Insider Filing database and CRSP
Outcomes		
CEO Turnover	Indicator variable that takes a value of one if the CEO was replaced between 1/1/2006 and 12/31/2007 and did not hold any other position in the firm, and zero otherwise.	Execucomp, hand collection
CFO Turnover	Indicator variable that takes a value of one if the CFO was replaced between 1/1/2006 and 12/31/2007 and did not hold any other position in the firm, and zero otherwise.	Execucomp, hand collection
Votes Withheld - Post	The proportion of votes withheld against the director in the first election after the backdating scandal unfolded (6/14/2007 to 6/14/2009) measured as: [(votes withheld + votes against)/(votes for + votes against + votes abstain + votes withheld)] multiplied by 100.	ISS Voting Analytics
Director Turnover	Indicator variable that takes a value of 0.01 if the director was not on the board in the first year after he or she would have stood for election after the backdating scandal unfolded (between 6/14/2007 and 6/14/2009) and 0 otherwise;	Boardex
Controls		
Abn Return	Annualized monthly raw return less the value weighted return compounded from March to June of 2006.	CRSP
Board Independence	The percentage of independent board members in the meeting immediately preceding the beginning of the backdating scandal (11/11/2005).	Boardex
Board Size	The number of board members in the meeting immediately preceding the beginning of the backdating scandal (11/11/2005).	Boardex
Director Age>65	Indicator variable that takes a value of one if the director is older than 65 years of age as of the meeting immediately preceding the beginning of the backdating scandal (11/11/2005), and zero otherwise.	Boardex
Director Stock Holdings	The market value of the firm's shares held by the director when the backdating scandal began to unfold. Measured as shares held in the latest form 4 multiplied by share price as of 11/1/2005.	Thomson Financials Insider Filing database and CRSP
Early Executive/Director	Indicator variable that takes a value of one if the executive or director held the position prior to August 29, 2002 (when most backdating ceased) or prior to the last materially backdated grant, in the case that the firm backdated past 2002, and zero otherwise.	Thomson Financials Insider Filing database, CRSP, Boardex, Hand collection
Executive Director	Indicator variable that takes a value of one if the executive was also a director and zero otherwise.	Thomson Financials Insider Filing database, CRSP, Boardex, Hand collection

Continued on next page

Appendix 3: Variable Definitions (Continued)

Executive Tenure	The number of years the CEO or CFO served in the current position as of 2005.	Execucomp, Hand collection
Female	Indicator variable that takes a value of one if the director is female and zero otherwise.	Boardex
First Year Indicator	Indicator variable that takes a value of one if the director is serving his/her first year on the Board and zero otherwise as of 2005.	Boardex
Institutional Ownership	the percentage of equity holdings held by independent institutions as of 2005, and set to 0 when missing, following Ferreira et al. (2008).	Factset
ISS Withhold Recomm.	Indicator variable that takes a value of one if ISS recommended a vote against or vote withheld for the director in the first election after the backdating scandal unfolded (6/14/2007 to 6/14/2009) and zero otherwise.	ISS Voting Analytics
Leverage	The ratio of total liabilities (LT) to total assets (AT) as of 2005.	
Network Size	The size of the directors network (director network size in Boardex) as	Boardex
Prior Turnover Rate	The rate of annual turnover of the executive or director at the firm from 2003 to 2005 calculated as: (the number of unique executives or directors -1)/(the number of years with data in Execucomp between 2003-2005).	Boardex, Execucomp, Hand Collection
ROA	Return on assets defined as income before extraordinary items (IB) divided by total assets (AT) as of the beginning of 2005.	Compustat
Size	Log of market value of equity as of 2005.	Compustat
Std. Returns	Standard deviation of monthly raw returns over the backdating period (2006-2007).	CRSP
Tech Indicator	Indicator variable that takes a value of one if the firm operates in a high-tech sector (SIC from 2833-2836, 8731-8734, 3570-3577, 7370-7374, or 3600-3674), and zero otherwise.	Compustat
Tenure	The number of years the director served on the board as of 2005. The first year the director appears on the proxy tenure is set to zero.	Boardex, Hand collection
Votes Withheld - Pre	The proportion of votes withheld against the director in the last election before the backdating scandal came to light (11/11/2005 to 11/11/2003) measured as: [(votes withheld + votes against)/(votes for + votes against + votes abstain + votes withheld)] multiplied by 100.	ISS Voting Analytics

Appendix 4: Illustration of ISS Withholding Recommendation

To illustrate why ISS recommended withholding votes against new directors, we note the case of Thomas Golonski and Richard Crouch, who ran for election as directors of Black Box Corporation in 2007.

In providing a recommendation for this election, ISS explains its policy of evaluating companies involved in backdating:

> *In cases where a company has practiced options backdating or is facing backdating allegations, ISS may recommend shareholders withhold votes from the Compensation Committee members who oversaw the questionable options grant practices or from current compensation committee members who fail to respond to the issue proactively. In determining our vote recommendation, ISS will analyze, on a case-by-case basis, the severity of the practices and the subsequent corrective actions taken by the company. Our analysis will be based on several factors, including, but not limited to: (i) Reason and motive for the options backdating issue, such as inadvertent vs. deliberate grant date changes; (ii) Length of time of options backdating; (iii) Size of restatement or adjustment due to options backdating; (iv) Corrective actions taken by the board or compensation committee, such as canceling or repricing backdated options, or recoupment of option gains on backdated grants; and (v) Adoption of a grant policy that prohibits backdating, and creation of a fixed grant schedule or window period for equity grants going forward* (Institutional Shareholder Services, 2007, p. 11).

ISS also notes that the board took a variety of corrective measures:

> *With regard to corrective actions taken by the board, we note that Messrs. Andrews, Golonski, McAndrew, and Greig agreed voluntarily to reprice their outstanding misdated*

options. Additionally, the company's former CEO's outstanding options were cancelled upon his resignation. However, there have been no broad-based repricing, cancellation, or recoupment initiatives instituted by the board. The company discloses that it has implemented additional procedures to the process for approving stock option grants that are focused on formalized documentation of appropriate approvals and determination of grant terms to employees and directors. However, the actual procedures have not been specified in the company's public disclosures. It is unclear if the new procedures provide for a fixed grant schedule, window period for future grants, or express prohibition of backdating (page 11).

Finally, ISS notes that Thomas Golonski was a member of the board since 2003 and Richard Crouch since 2004. ISS also notes "the most egregrious [backdating] period was from 1994 to 2001", indicating that neither Golonski nor Crouch were present during the egregious backdating period. ISS points out that other directors (Andrews and Greig) "were both members of the compensation committee during the most egregious backdating period (page 12)". Despite the actions the board took, and despite the fact that Golonski and Crouch were not present when the egregious backdating occurred, ISS recommends against voting for Golonski and Crouch for "failure to adequately remediate the option grant issues at the company" (page 12)." Perhaps as a result of this recommendation, the votes withheld for Golonski and Crouch rose 20% from the prior election.

Table 1
Sample Composition

Panel A: Director Turnover Sample	Firms	Directors
Firms with data in Compustat and CRSP 2005-2008	4,352	
Firms issuing grants from 1996-2002	3,496	20,903
Director data available from Boardex pre and post backdating scandal	2,240	19,647
Include only independent directors	2,240	16,082
All control variables available	2,114	15,285
Firms that did not issue a non-material backdating restatement ("charge")	2,079	15,033
Firms that did not reveal a "clean" backdating investigation	2,048	14,825
Firms that restate or are suspect	336	2,040
Composition:		
Restatement	78	512
Suspect Backdating	258	1,528

Panel B: Director Voting Sample	Firms	Directors
Directors that do not turnover	2,044	11,701
ISS provided recommendation	1,375	5,277
Directors standing for election during the pre and post windows	1,326	5,082
Composition:		
(A) Restatement	54	231
(B) Suspect Backdating	130	441
(C) Neither suspect nor restating	1,142	4,410
Director Voting Sample (A+B)	184	672

Panel C: Executive Turnover Sample	Firms
Firms with data in Compustat and CRSP 2005-2008	4,352
Firms issuing grants from 1996-2002	3,496
All control variables available	2,590
CFO is identified through sample period	2,285
Firms that disclosed non-material backdating	2,241
Firms that did not reveal a "clean" backdating investigation	2,228
Firms that restate or are suspect	423
Composition:	
Restatement	107
Suspect Backdating	316

Table 2

Univariate Tests of Differences between the Restating Firms and Suspect Firms in the Director Sample

This table documents z-tests of differences in proportions of Director Turnover and t-tests of differences in p-value, materiality ratio, and director votes withheld between firms that restate and firms that show strong evidence of having backdated, but remained silent (suspect firms). ***,** represents significance at a $p<0.01$, $p<0.05$ level, respectively using two-sided tests. Variables are defined in appendix 3.

		Directors at Restatement Firms (N=512)	Directors at Suspect Firms (N=1,528)	Difference in Means
Backdating Likelihood		93.4%	97.9%	-4.5% ***
				(7.52)
Materiality Ratio		18.9%	17.5%	1.4%
				(0.41)
Votes Withheld:	All Directors	13.3%	7.0%	6.3% ***
				(6.97)
	New Directors	11.6%	6.2%	5.4% ***
				(3.78)
Turnover Rate:	All Directors	22.0%	14.1%	7.9% ***
				(4.20)
	New Directors	19.5%	13.2%	6.3% **
				(2.12)

Table 3
Multivariate Tests of Director Voting and Turnover

This table documents OLS tests of turnover (column 1) and votes withheld (column 2) among independent directors at firms that issued a backdating restatement (restatement indicator), and whether the director was present at least one year that the firm issued a material misstatement related to backdating (early director). ***,**,* represents significance at a p<0.01, p<0.05, p<0.1 level, respectively using two-sided tests and t-statistics clustered at the firm level. Variables are defined in appendix 3. In this table, the dependent variables are multiplied by 100, and the first year indicator is multiplied by 1,000.

Panel A: Baseline Models

	(1) Votes Withheld	(2) Turnover
Restatement	5.86***	6.90*
	(2.62)	(1.87)
Early Director	0.36	-0.61
	(0.33)	(-0.27)
Restatement x Early Director	1.52	-0.86
	(0.59)	(-0.18)
Votes Withheld - Pre	0.19**	
	(2.10)	
Prior Turnover Rate		3.83
		(1.10)
Lose External Seat	-0.90	22.10***
	(-0.84)	(9.66)
Busy Director	1.81*	-1.73
	(1.90)	(-0.98)
Director Stock Holdings	0.01	0.05
	(0.10)	(0.27)
Board Size	-0.19	0.89*
	(-0.69)	(1.78)
Board Independence	6.57	-14.48
	(0.79)	(-1.37)
ROA	-0.14	1.26
	(-0.04)	(0.22)
Size	-0.64	-0.39
	(-0.88)	(-0.58)
Abn Return	-0.48	-4.87***
	(-0.45)	(-3.86)
Leverage	-0.03	-0.10
	(-0.19)	(-0.37)
Std Returns	-28.32	-2.67
	(-1.59)	(-0.14)
Tech	1.23	3.96**
	(0.86)	(2.09)
Institutional Ownership	0.98	-2.52
	(0.42)	(-0.69)
Tenure	7.43	27.25
	(0.70)	(1.59)
Female	-2.22	-2.20
	(-1.65)	(-0.81)
Director Age>65	-1.27	-1.38
	(-1.13)	(-0.85)
First Year Indicator	1.60	2.73
	(1.03)	(1.04)
Constant	6.30	14.33
	(0.90)	(1.52)
Observations	672	2,040
R-Squared	0.18	0.10
Cluster	Firm	Firm

Table 3 (Continued)
Multivariate Tests of Director Voting and Turnover

Panel B: Voting Results Incorporating ISS Recommenations

	(1) ISS withholding recommendation for any reason	(2) ISS withholding recommendation related to backdating
Restatement	3.53**	2.41
	(2.46)	(1.47)
Early Director	-0.73	0.43
	(-0.94)	(0.39)
Restatement x Early Director	2.23*	5.10**
	(1.71)	(2.35)
ISS Withhold Recomm.	22.13***	
	(10.21)	
ISS Withhold Recomm.-BD		27.25***
		(6.73)
Votes Withheld - Pre	0.08*	0.15*
	(1.75)	(1.77)
Lose External Seat	-0.29	-0.90
	(-0.36)	(-0.87)
Busy Director	1.88**	1.65*
	(2.47)	(1.79)
Director Stock Holdings	-0.04	0.02
	(-0.38)	(0.17)
Board Size	-0.36	-0.26
	(-1.33)	(-1.02)
Board Independence	-0.28	8.30
	(-0.04)	(1.04)
ROA	-0.16	-0.26
	(-0.06)	(-0.07)
Size	-0.44	-0.38
	(-0.73)	(-0.56)
Abn Return	-0.56	-0.38
	(-0.61)	(-0.38)
Leverage	-0.14	-0.05
	(-0.93)	(-0.31)
Std Returns	-39.60***	-25.71
	(-2.72)	(-1.54)
Tech	1.68	1.36
	(1.63)	(1.02)
Institutional Ownership	3.88**	1.08
	(2.17)	(0.52)
Tenure	0.76	9.18
	(0.11)	(0.86)
Female	-1.96**	-1.78
	(-2.02)	(-1.41)
Director Age>65	-0.51	-1.21
	(-0.78)	(-1.13)
First Year Indicator	0.43	1.76
	(0.42)	(1.03)
Constant	10.31**	3.39
	(2.08)	(0.53)
Observations	672	672
R-Squared	0.60	0.25
Cluster	Firm	Firm

Table 4

Multivariate Tests of Whether ISS Recommended Withholding Votes Against Directors at Firms that Show Evidence of Backdating

This table documents OLS tests of whether ISS recommended withholding votes against the director among independent directors at firms that did not issue a backdating restatement. ***,**,* represents significance at a p<0.01, p<0.05, p<0.1 level, respectively using two-sided tests and t-statistics clustered at the firm level. Variables are defined in appendix 3. In this table, the controls are the same as in the previous table.

	(1)	(2)
	Dependent Variable: Whether ISS recommended a withholding vote Sample: Directors at firms that did not issue a backdating restatement	
Indicator for a suspect firm	-0.00	
	(-0.10)	
P Value (likelihood of backdating)		0.03
		(0.95)
Materiality Ratio (magnitude of backdating)		-0.00
		(-0.80)
Controls	YES	YES
Observations	4,846	4,846
R-Squared	0.11	0.11
Cluster	Firm	Firm

Table 5

Propensity Score Matching Tests of CEO Turnover

This table reports propensity score matching tests of all CEO and forced CEO turnover. Panel A reports pairwise comparisons of the variables between restating and suspect firms after matching. T-statistics are tabulated in parenthesis based on z-tests of differences in sample proportions for binary variables (Turnover, Forced Turnover, Director, Big N and High Tech), and t-tests of differences in sample means for continuous variables (remaining variables). Panel B (C) reports z-tests of differences in sample proportions between all (forced) CEO turnover. ***,**,* represents significance at a p<0.01, p<0.05, p<0.1 level, respectively using two-sided tests.

Panel A: Univariate Sample Comparisons

	Restatement Sample	Matched Suspect Sample	Difference
Backdating Likelihood	0.93	0.98	-0.04***
			(3.23)
Materiality Ratio	0.38	0.43	-0.04
			(0.19)
Turnover	0.43	0.23	0.20***
			(-3.05)
Forced Turnover	0.25	0.06	0.19***
			(-3.97)
Abnormal Return	-0.09	-0.13	0.04
			(-1.55)
Prior Turnover Rate	0.09	0.10	-0.01
			(0.28)
Director	0.94	0.92	0.03
			(-0.80)
Tenure	6.70	4.85	1.85**
			(-2.02)
Big N	0.94	0.91	0.04
			(-1.04)
Leverage	0.13	0.20	-0.07**
			(2.24)
ROA	0.09	0.01	0.08**
			(-2.01)
Size	6.79	6.48	0.31
			(-1.60)
Tech Indicator	0.58	0.41	0.17***
			(-2.46)
Institutional Ownership	0.79	0.62	0.16***
			(-4.13)
Std Returns	0.11	0.11	0.00
			(-0.49)
Board Size	7.79	7.91	-0.11

Table 5 (Continued)

Propensity Score Matching Tests of CEO Turnover

Panel B: CEO Turnover

		Early CEO = 0	Early CEO = 1	
Restating Firm	0	15.00% [40 obs.] (A)	28.36% [67 obs.] (B)	(A) - (B) Difference: -13.36% Z Stat: 1.58
	1	42.50% [40 obs.] (C)	43.28% [67 obs.] (D)	(C) - (D) Difference: -0.78% Z Stat: 0.94

	(C) - (A)	(D) - (B)
Difference:	27.50%	14.93%
Z Stat:	2.72	1.80

Panel C: Forced CEO Turnover

		Early CEO = 0	Early CEO = 1	
Restating Firm	0	10.00% [40 obs.] (A)	2.99% [67 obs.] (B)	(A) - (B) Difference: 7.01% Z Stat: 1.53
	1	22.50% [40 obs.] (C)	26.87% [67 obs.] (D)	(C) - (D) Difference: -4.37% Z Stat: 0.50

	(C) - (A)	(D) - (B)
Difference:	12.50%	23.88%
Z Stat:	1.52	3.88

Table 6

Propensity Score Matching Tests of CFO Turnover

This table reports propensity score matching tests of all CFO and forced CFO turnover. Panel A reports pairwise comparisons of the variables between restating and suspect firms after matching. T-statistics are tabulated in parenthesis based on z-tests of differences in sample proportions for binary variables (Turnover, Forced Turnover, Director, Big N and High Tech), and t-tests of differences in sample means for continuous variables (remaining variables). Panel B (C) reports z-tests of differences in sample proportions between all (forced) CFO turnover. ***,**,* represents significance at a p<0.01, p<0.05, p<0.1 level, respectively using two-sided tests.

Panel A: Univariate Sample Comparisons

	Restatement Sample	Matched Suspect Sample	Difference
Backdating Likelihood	0.93	0.97	-0.04***
			(2.93)
Materiality Ratio	0.39	0.34	0.05
			(-0.21)
Turnover	0.68	0.24	0.44***
			(-6.37)
Forced Turnover	0.29	0.01	0.28***
			(-5.60)
Abnormal Return	-0.09	-0.09	0.00
			(-0.03)
Prior Turnover Rate	0.12	0.11	0.01
			(-0.34)
Director	0.05	0.06	-0.01
			(0.31)
Tenure	2.59	2.85	-0.26
			(0.71)
Big N	0.94	0.89	0.06
			(-1.48)
Leverage	0.13	0.25	-0.13***
			(3.50)
ROA	0.10	0.07	0.03
			(-0.93)
Size	7.13	6.92	0.21
			(-1.05)
Tech Indicator	0.57	0.44	0.13*
			(-1.93)
Institutional Ownership	0.79	0.73	0.05
			(-1.44)
Std Returns	0.11	0.11	0.00
			(-0.71)
Board Size	7.80	8.09	-0.29

Table 6 (Continued)

Propensity Score Matching Tests of CFO Turnover

Panel B: CFO Turnover

		Early CFO = 0	Early CFO = 1	
Restating Firm	0	35.85% [53 obs.] (A)	11.54% [52 obs.] (B)	(A) - (B) Difference: 24.31% Z Stat: 2.92
	1	66.04% [53 obs.] (C)	69.23% [52 obs.] (D)	(C) - (D) Difference: -3.19% Z Stat: 0.35

	(C) - (A)	(D) - (B)
Difference:	30.19%	57.69%
Z Stat:	3.11	6.00

Panel C: Forced CFO Turnover

		Early CFO = 0	Early CFO = 1	
Restating Firm	0	1.89% [53 obs.] (A)	0.00% [52 obs.] (B)	(A) - (B) Difference: 1.89% Z Stat: 1.00
	1	24.53% [53 obs.] (C)	32.69% [52 obs.] (D)	(C) - (D) Difference: -8.16% Z Stat: 0.93

	(C) - (A)	(D) - (B)
Difference:	22.64%	32.69%
Z Stat:	3.44	4.51