

Director Penalties and Incentives to Restate: Evidence from Stock Option Backdating

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PRELIMINARY AND INCOMPLETE, CURRENTLY UNDER REVISION

February 2016

Key words: Board of directors, Restatements, Corporate governance, Shareholder voting

JEL Codes: K22

Abstract

We analyze stock option grants and identify 250 firms that likely backdated materially but did not restate. After the backdating scandal came to light, director turnover, votes withheld, and the likelihood of a withholding recommendation by ISS was 7%, 5%, and 9% lower for directors at these firms relative to firms that issued a backdating restatement, but no different from firms that exhibit no evidence of backdating. Since we focus on directors who joined the board after the backdating occurred, these results suggest that penalties created disincentives for independent directors to encourage restating.

The authors can be reached at qcurtis@virginia.edu and hopkins@virginia.edu. We thank Matt Cain, Rich Evans, Marc Lipson, Elena Loutskina, Pedro Matos and workshop participants at the Securities and Exchange Commission, Mason School of Business at the College of William and Mary, and the Darden Graduate School of Business at the University of Virginia for helpful comments. We gratefully acknowledge research support provided by the Darden Graduate School of Business and the Law School at the University of Virginia. All errors are our own.

1. Introduction

We examine whether director penalties create incentives to reveal misbehavior. Given that independent directors play a key role in monitoring managers (Adams, Hermalin, & Weisbach, 2010) and promoting transparency (Armstrong, Core, & Guay, 2014) one might expect that director penalties would create incentives for independent directors to reveal misbehavior. However, recent studies find that independent directors face penalties when misbehavior is revealed (Ertimur, Ferri, & Maber, 2012; Srinivasan, 2005), which may create disincentives for forthright disclosure. In short, if penalties are levied only when the firm reveals misbehavior, then penalties create disincentives to diligently investigate possible issues. However, if directors incur penalties if the firm fails to report misbehavior when the inference of misbehavior is high, then directors have greater incentives to monitor managers and reveal misbehavior.

The challenge in answering this question is to identify firms that publicly appear to misbehave but do not disclose the misbehavior. We utilize the setting of stock option backdating as it allows us to analyze publicly available grant data to generate the likelihood that any grant-issuing firm reported a material misstatement by backdating, while separately identifying firms that remedied the error by issuing a restatement. Further, Heron and Lie (2009) estimate that over 2,000 firms backdated grants, but prior studies identify only 271 firms that were publicly associated with backdating (e.g. Ertimur et. al. 2012). This suggests that backdating is a good setting to identify concealed misbehavior as many more firms engaged in the practice than have been publicly identified.

We follow a process similar to Bizjak et al. (2009) and analyze option grants during the period of the backdating scandal (1996-2002) to identify three groups of firms. *Restating firms* issued a material misstatement during the backdating period and issued a restatement. *Likely*

backdating firms appear to have issued a material misstatement due to backdating, but never restated.¹ *Clean firms* have an estimated likelihood of backdating below the median. Our goal is to evaluate the relative career consequences for independent directors at likely backdating firms relative to the other two groups.

To do this, we construct a panel of “new” independent directors that sat on these boards when the backdating scandal came to light (November 2005) but joined the board after backdating largely ceased (August 2002). We do so to ensure that the penalties levied on these directors is attributable only to the restatement as these directors have no direct connection to the underlying backdating behavior. We then examine director penalties after the backdating scandal came to light (post June 2007). Within this sample we compare turnover, votes withheld, and the likelihood of ISS recommending withholding votes between the likely backdating directors and those in the other groups (restating and clean). We control for the severity of backdating (likelihood of backdating and materiality) as well as votes withheld and the ISS rating before the backdating scandal (pre November 2005).

We find that new independent directors at likely backdating firms fared significantly better than independent directors at restating firms, and no worse than independent directors at clean firms, holding the likelihood and amount of options backdating constant. In fact, new independent directors at firms that issued a backdating restatement were 7% more likely to lose their board seats after the backdating scandal became public, and received 5% more votes withheld relative to directors at likely backdating firms. These penalties are quite large given a mean turnover rate of 12% and vote withheld rate of 5% across the whole sample.

¹ In order to analyze firms that appear to be the most egregious backdaters, we set high thresholds for the likelihood that a grant was backdated (at least 95%) and that the backdated grants were material to the firm’s operations (at least 5% of income). See section 3 for more detail.

We next examine the likelihood of ISS issuing a recommendation to withhold votes. As a provider of services to institutional investors, ISS might be more likely to account for forthcoming disclosures and not penalize independent directors for encouraging the issuance of the restatement. On the other hand, ISS explicitly stated that it would consider the “size of the restatement due to options backdating” in establishing voting guidelines which would suggest that restatements would be positively associated with a recommendation to withhold votes. Our results indicate that directors at restating firms face a likelihood of receiving a recommendation to withhold votes 10% above the unconditional rate of 9%.

While this evidence suggests that penalties created disincentives for directors to encourage firms to issue a restatement, these results may be attributable to two alternative explanations. First, it may be unrealistic to assume that penalties could have been levied in accordance with our method of identifying backdating firms which requires large-sample data gathering and analysis. Therefore, we also use other measures to identify firms and directors that are more closely associated with the underlying backdating behavior than our sample of new directors. In particular, we examine firms that admitted to non-material backdating and issued a charge in lieu of restating prior period financial statements. We also identify directors that sat on the compensation committee during the backdating era, and those that likely received a backdated grant. Results suggest that new directors at restating firms face penalties at least equal to, and often greater than, other groups of directors more closely associated with backdating that did not issue a restatement.

Second, restatements could be associated with more egregious backdating along some unobservable dimension, or simply confirm with certainty what would otherwise be a probabilistic estimate of backdating behavior. However, we find that director consequences among likely

backdating firms are no different than clean firms which suggests that shareholders disregarded public evidence of backdating and relied instead on firms to self-report before levying penalties, even when the statistical likelihood of backdating was high. Since we find steep penalties levied on firms that restate, consistent with Ertimur et al. (2012), shareholders clearly sought to punish the practice. Further, we control for the severity of firms' backdating activity in several ways.² Nevertheless, by not issuing a restatement, independent directors at firms that exhibit clear probabilistic evidence of material backdating were able to avoid penalties relative to directors at restating firms. To the extent that directors were able to anticipate this dynamic, it created disincentives for directors to encourage firms to issue a restatement.

Since we separately examine penalties levied on directors for restatements and the underlying material misstatement, this study is related to those that examine director penalties for governance failures (Brochet & Srinivasan, 2014; Fich & Shivdasani, 2007; Srinivasan, 2005). Most directly, Ertimur et al. (2012) examine a large sample of grant-issuing and non-grant-issuing firms and find higher reputational penalties for independent directors at backdating firms relative to non-backdating firms. Whereas Ertimur et al. (2012) examine director penalties for firms publicly associated with backdating, we focus on penalties levied for the issuance of a restatement, and compare these penalties to firms that likely backdated but were never publicly implicated.

2. Related literature and background

Options backdating refers to the practice of granting stock options at a price below the current share price by falsifying the grant date. Accounting standards in existence at the time (APB

² In addition to controlling for the likelihood and severity of the backdated grant, we control for the number of backdated grants, aggregate materiality of all backdated grants, and the number of years in which the firm issued a backdated grant. Second, to ensure that our results are not sensitive to our model of identifying backdating firms, we repeat our tests using alternative measures of backdating: the number of grants issued on the lowest-priced day of the month (Bebchuk et al., 2010) and whether the firm appeared on a list of backdating companies compiled by Glass Lewis. In all tests, results are confirmed.

23), required companies to record compensation expense equivalent to this difference multiplied by the number of options granted. However, companies dated the grants as of a past date where the strike price equaled the share price, effectively avoiding the recognition of compensation expense. This practice largely discontinued when the Sarbanes-Oxley Act required reporting stock option grants within two business days.³

Existing studies have explored the implications for firms, directors and shareholders for involvement in backdating. Bernile and Jarrell (2009) demonstrate abnormal negative returns, and higher rates of litigation and executive turnover following a backdating restatement. Further, Ertimur et al. (2012) examine votes withheld and turnover among independent directors serving on firms publicly linked with backdating compared with independent directors serving on non-backdating firms. They analyze votes withheld and turnover after the firm is publicly associated with backdating and compare penalties across a variety of dimensions including current and prior board committee membership. Ertimur et al. (2012) find that votes withheld are highest for directors that serve on the compensation committee during the backdating period while turnover is greater for directors serving on the compensation committee of firms implicated in backdating relative to firms not implicated in backdating.

We explore the effects of restating on director penalties for backdating in order to gain a deeper understanding of the incentives directors face when deciding whether to encourage the firm to issue a restatement. The decision to issue a restatement is a complex process involving multiple parties including executives, directors, and auditors. While independent directors are not the sole decision makers in determining whether the firm issues a restatement, research suggests that they play an important role in establishing firms' disclosure policies. Armstrong et al. (2014) show that

³ See the Securities Exchange Act of 1934 § 16(a)(2)(C), 15 U.S.C. § 78p(a)(2)(c).

an exogenous increase in the number of independent directors sitting on the board led to a decline in the information asymmetry component of the bid-ask spread suggesting that independent directors play a role in shaping the firm's information environment. Similarly, studies show that the proportion of independent directors is associated with financial reporting outcomes such as the likelihood of fraud (Beasley, 1996; Dechow, Sloan, & Sweeney, 1996) and earnings informativeness (Petra, 2007).

Prior studies also demonstrate that independent directors serving on firms that issue a restatement face penalties. Srinivasan (2005) examines 409 restatements from 1997-2001 and finds an increase in turnover among independent directors sitting on these boards. Similarly, Ertimur et al. (2012) find greater turnover and votes withheld among independent directors serving on firms that issue a high backdating restatement relative to directors serving on boards of firms that issue a low backdating restatement. To the extent that independent directors are expected to oversee the financial reporting process and prevent such misstatements from occurring, these penalties seem quite natural.

That said, while these studies establish a link between restatements and penalties, it is not clear whether the documented penalties are triggered by the failure to prevent the misstatement, or for the issuance of the restatement. Intuitively, penalties ought to attach for failure to prevent a misstatement, but in many contexts, it is not possible to identify firms that issued a misstatement without the restatement. Thus, restatements may provide shareholders, nomination committees, and other actors with a simple heuristic for governance problems in the absence of an independent channel to identify restatements. Such an effect would be consistent with Jenter and Kanaan (2015) who find that CEO turnover is greater with declines in peer firms' performance even though the CEO is not responsible for broader industry declines. Jenter and Kanaan (2015) find some evidence

that boards use salient benchmarks and mistakenly attribute industry performance to CEOs.

A unique feature of the backdating setting is that external parties could examine publicly available data on options to measure the likely culpability of a given firm independent of a restatement. We gain insight into whether director penalties are levied in this manner by separately examining the penalties for the underlying misstatement and the restatement in two ways. First, we examine stock option data to identify firms that likely backdated but did not issue a restatement. Penalties levied on directors serving at these firms must be associated only with the underlying backdating behavior as there was no associated restatement. Second, since several years transpired between the end of the backdating behavior (on August, 2002) and the revelation period (November 2005), we can identify a relatively large sample of directors whose tenure on the board began after the backdating behavior largely ceased. This allows us to test penalties levied on the opposite group: directors who have no direct link with the underlying backdating behavior, but who serve at firms that issue a backdating restatement. Therefore the restatement itself, and not the underlying backdating behavior, likely drives penalties levied on these directors.

3. Data and methods

3.1 Identification of Firms that Backdate

Our goal in identifying backdating firms is to mirror the potential actions of a market participant (i.e. a shareholder or ISS) who attempted to discern whether a given firm backdated absent a restatement. Even if we had private information about whether a firm actually did backdate, we are interested in only firms that could be identified as backdaters by analyzing publicly available grant patterns. Accordingly, we follow Bizjak et al., (2009) and compute the probability that each option grant was awarded on an abnormally favorable date. Using the Thomson Financials Insider Filing database, we identify option grants to officers and directors between January 1, 1996, and August 29, 2002, when changes to reporting rule made backdating

much more difficult.⁴ To determine whether a particular option grant was backdated, we compute the return on the underlying stock in the 20 trading days before and after the grant.⁵

We next estimate the return “reversal” as the post-grant change in price minus the pre-grant change. Intuitively, this estimates the “V” shape where steeper sides create greater reversals. For each firm, we also compute the firm’s decile of monthly stock price volatility relative to the sample. We then compute the reversal for 1,000,000 random draws of firms in the same volatility decile over the same time period as our sample of option grants. We then estimate the likelihood that the grant was backdated by measuring the portion of the random draws with lower reversals. This creates 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.

Although it may seem unreasonable to expect that those who impose penalties on directors would follow such a methodology, this technique is very similar to that used in contemporaneous litigation. For example, In *DeSimone v. Barrows* the plaintiff filed a derivative lawsuit on June 9, 2006 against the board of directors and officers of Sycamore Networks Inc. alleging stock option backdating.⁶ As evidence of backdating, the complaint shows that the stock price of Sycamore increased by 52.5% and 45.3% in the 20 trading days after two illustrated grants were issued. This increase in the stock price following the grant is the effect our measure captures.⁷ In fact, Sycamore Networks has a 99.76% likelihood of backdating according to our measure.

⁴ In particular, in August 2002 the SEC shortened the filing deadline for Form 4 from 30 days to 2 days after the grant, substantially shortening the window over which grants can be backdated.

⁵ This window length approximates the 30 calendar days allotted to firms to file the Form 4 with the SEC after granting stock options.

⁶ For example, see Derivative Action Complaint at 36, *Desimone v. Barrows*, 2006 WL 1971843 (Del. Ch. 2006) (No. 2210-N).

⁷ We also estimate the return prior to the grant because evidence suggests that in addition to back dating, firms could have also “forward dated” options. This refers to the practice of waiting for the stock price to fall and dating the grant at a future date when the price is lower.

We then compute a firm-year level 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 granting activity 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 level 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 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 backdating firms if the firm-level probability exceeds 95% in any year. We test robustness of inferences to this threshold below.

3.2 Estimating the materiality of the backdating

Per U.S. GAAP, errors that would influence “the judgment of a reasonable person relying upon the report” require a restatement (FASB, 1980). We thus account for materiality in our tests.⁸ U.S. GAAP does not explicitly quantify materiality. ASC 250-10-S99 does provide one such threshold, as it suggests that “the use of a percentage as a numerical threshold, such as 5%, may provide the basis for a preliminary assumption that - without considering all relevant circumstances - a deviation of less than the specified percentage with respect to a particular item on the registrant's

⁸ It could be argued that even minor backdating errors are material because they signal a breakdown in corporate governance. Consistent with this, Bernile and Jarrell (2009) document significant abnormal negative returns upon the announcement of backdating. Further, ASC 250-10-S99 alludes to the fact that “particularly egregious circumstances such as self-dealing or misappropriation by management” are material regardless of error size.

financial statements is unlikely to be material.” This 5% threshold is consistent with other sources providing guidance to practitioners (Vorhies, 2005). Consistent with this, we consider a grant to be materially backdated if the difference between the strike price on the grant date and the strike price on the date the grant was actually issued multiplied by the number of grants exceeds 5% of yearly income.

As an illustration, consider a hypothetical firm that issued 100 grants to its CEO on May 5 when the stock price was \$50. However, the stock price was \$40 just 3 days earlier on May 2. To provide greater compensation to the CEO, the firm set the grant date on May 2 and a strike price of \$40 (although the grant was actually awarded on May 5). In this case, APB 25 required the firm to recognize \$1,000 in compensation expense over the vesting period of the grant.⁹

To operationalize this measure, we compare the stock price on the date the grant was issued with the median stock price over a 40-day trading window centered on the grant date, consistent with the window used to estimate the likelihood of backdating. The difference between these stock prices reflects the amount of compensation expense the firm should have recognized over the vesting period for one grant. 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 (Acito, Burks, & Johnson, 2009). If the expense exceeds the threshold, we consider the grant to be material. In order to create a continuous materiality variable to use as a control in multivariate specifications, we create a ratio of yearly materiality (aggregating the materiality of all backdated grants) scaled by yearly income less special items or non-operating items. We select the highest value to use in firm-level regressions.

⁹ The total compensation expense is the difference between the stock price on the date the grant was actually issued (\$50) and the strike price (\$40) multiplied by the number of options (100).

While this methodology captures the essence of the possible error's size relative to the firm's operations, it is subject to the following assumptions. First, this method assumes the median stock price over the 40 trading day window reflects the stock price on the actual grant date. Our estimation of materiality will vary from the actual error to the degree that these prices diverge. Second, although we do not account for the income tax effects, these are likely immaterial.¹⁰ 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).¹¹ Third, APB 25 requires amortizing the expense over the vesting period while our methodology compares the expense to one year of income.¹² We do so because, although the accounting errors arose upon the issuance of the grants from 1996-2002, they were not disclosed until the 2005-2007 period. Therefore, when the firm was considering whether to restate (from 2005-2007), many years had already passed (perhaps even the entire vesting period of the lucky grant). 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.¹³

¹⁰ The income tax effects of the restatement are unclear. Reclassifying options as in-the-money could increase or decrease taxes depending on how the options were classified (qualified or non-qualified), the rank of the employee (executive versus employee), the pattern and timing of past and expected future exercises and any penalties or interest firm may owe or reimburse employees for past taxes and failure to withhold taxes.

¹¹ Our analysis assumes that firms applied APB 25. However, firms could have opted for SFAS 123. If this were the case, our methodology may overstate the materiality of the error as the firm would have already expensed the fair value of the grant as of the grant date. However, most firms did not voluntarily apply SFAS 123 during the sample period. Further, the firm would still have under-accrued the compensation expense as the fair value actually expensed would have reflected the fair value of the grant while the stock was trading at a relatively low level. Had the firm estimated the fair value when the grant was actually awarded (when the stock price was higher), the fair value of the grant would have been higher, as well (Bernile and Jarrell, 2009).

¹² In 1995, FASB issued SFAS 123, which provided the option of amortizing the fair value of the options on the grant date. However, firms could still use the intrinsic value method of APB 23 as long as the firm disclosed this information. The vast majority of firms elected the intrinsic value method until FASB required the fair value method in 2004 (Bernile and Jarrell, 2009). Thus, for our sample period, 1996-2002, most firms applied the intrinsic value method.

¹³ Although backdating primarily affected financial statements for the current period, the errors would remain on the balance sheet until corrected. Restatements corrected the overstated income during the backdating period as well as

Despite these challenges in estimating materiality, we find that our estimate is highly correlated with actual restated amounts. In fact, in untabulated tests, the explanatory power of a model where the amount actually restated is regressed on only our estimate of materiality is 37.45%. The coefficient on the estimated amount is over 13, consistent with our methodology capturing a very conservative estimate of materiality, and statistically significant (T-stat=2.04).

Although our method of identifying firms provides only indirect evidence of intentional backdating, it is notable that courts have ruled that evidence drawn from statistical analyses (of the type we conduct) “‘emphatically suggests’ that grant dates were manipulated” (Ryan v. Gifford, 918 A.2d 341,345 (Del. Ch. 2007)). Similarly, other courts have found that this evidence is sufficient to “constitute ‘some’ evidence of corporate wrongdoing” (Louisiana Mun. Police Employees’ Retirement Sys. v. Countrywide Financial Corp., 2007 WL 2896540 at 11 (Del. Ch. 2007)). Nonetheless, we conduct a variety of robustness checks to ensure that our results are not driven by small changes in the research design. Furthermore, what we aim to capture is not whether the firm *actually* backdated, but whether the firm *appeared* to have engaged in backdating. Therefore, even if we had access to private information about backdating activities, our tests aim to discern whether parties used publicly available information to penalize directors.

3.3 Data sources and sample

To create our primary sample of independent directors we gather accounting data from Compustat, data on the composition of boards from Boardex, returns from CRSP, restatement data from Audit Analytics, and director voting results from ISS. Panel A of Table 1 documents the evolution of the samples used to examine independent director turnover. We start with 4,352 firms

the current equity accounts. In untabulated tests, we find that backdating behavior increased over time (consistent with Lie 2009). To ensure that our results are not affected by backdating that only occurred in the early backdating period, we include only firms that backdated after 2000 and find results similar to those tabulated.

represented in CRSP and Compustat for the entire backdating period (roughly 2005-2007). Of these, 2,910 issued grants from 1996-2002 (the backdating period). Boardex includes data on 2,172 of these firms corresponding with 15,649 total independent directors. Finally, we drop all independent directors serving on the board during the backdating period to minimize the likely culpability of our sample, reducing the sample to 4,817 independent directors serving on 1,874 boards.

We identify four, non-overlapping subgroups within this sample. First, 70 (179) “restating firms (directors)” have issued a restatement related to backdating. Second, 250 (636) “likely backdating firms (directors)” appear to have materially backdated but did not issue a restatement. We then split the remaining firms on the median likelihood of having backdated. We label the 939 (2,002) “clean firms (directors)” as those that have a probability of backdating below the median. The remaining firms are those that do not fit into any group.

Panel B documents the voting sample evolution. Three main differences arise between this sample and the prior. First, we cannot capture voting data on directors that turnover. This reduces the sample by 803 directors (to 4,014). Second, we only capture voting results disclosed within a two year window pre and post backdating. Therefore, at most we can capture two voting cycles. For classified boards (of which are roughly half the firms) we will not capture voting data for one class of directors (or roughly one-third of directors).¹⁴ However, we often miss more than one class. This reduces the sample by another 1,020 directors (to 2,994 directors). We also do not have ISS recommendations for 1,313 directors and are not able to obtain vote totals for 63 directors. This could happen if the firm did not hold director elections within the pre or post windows, if the firm classifies its board (and is not identified as such in our data). This leads to a final voting and

¹⁴ Classified boards are those that divide directors into classes and alternate which class is up for election in any year. Often, boards are divided into three classes. Therefore, each director stands for election once every three years.

ISS recommendation sample of 935 firms and 1,618 directors. Of this sample, 41 restate, 98 show evidence of backdating but do not restate (likely backdate), 364 show no signs of backdating (clean), and 432 do not fit into any group.

Descriptive statistics of these samples are illustrated in table 2. The unconditional turnover rate is around 12% for the full sample while 5.45% of votes are withheld and ISS issues a withholding recommendation in 7.79% of the director elections, on average. The average firm shows a backdating likelihood of 86.24%, and this is material in about 6% of the cases.¹⁵

4. Empirical Results

4.1 Univariate tests of differences between likely backdating and restating firms

Table 3 documents T-tests of differences between directors at restating, likely backdating and clean firms. Relative to likely backdating firms, directors at restating firms are roughly 50% more likely to turnover (19.55% versus 13.05%), have nearly twice as many votes withheld (11.21% versus 5.72%), and are nearly twice as likely to receive a withholding recommendation from ISS (17.59% versus 8.76%).

Also, we note that evidence does not uniformly suggest that restating firms backdated more egregiously. On one hand, the likelihood of having backdated is greater larger for likely backdating firms (98.06 versus 94.00). This suggests that the grant patterns of likely backdating firms is more suspicious than at firms that eventually restated. However, we do note that materiality is higher for the restating firms (21.13 versus 14.53, T-stat=1.85). Broadly, this suggests that the “V” shape is more pronounced for likely backdating firms than restating firms, but restating firms issued more

¹⁵ Our estimate identifies 38% of firms as having a backdating likelihood in excess of 95%. This is roughly consistent with Heron and Lie (2009) who find that 30.1% of firms backdated during this time period. The difference between our estimate and that of Heron and Lie (2009) is attributable to data requirements imposed in our study (i.e. Boardex coverage for new directors). Our sample includes just 1,874 firms relative to 6,868 firms in Heron and Lie (2009). We apply our identification methodology to a broader sample of 8,441 grant issuing firms without imposing data restrictions, and find that 27.2% backdate (untabulated).

grants on suspicious days.

We also observe some differences across samples in other firm characteristics. However, these differences would generally lead to directors at restating firms facing lower penalties. For example, board size, log assets, and ROA are all greater for restating firms while restating firms have fewer directors above retirement age. These characteristics are generally associated with *lower* turnover. Overall, this table provides preliminary evidence that shareholders and the ISS levied greater penalties on directors serving on backdating firms that issue restatements relative to those serving on likely backdating firms.

4.2 Univariate tests of differences between likely backdating and clean firms

The prior tests suggest that issuing a restatement is costly for directors at backdating firms. The next comparison tests whether shareholders levy penalties on likely backdating firms relative to firms that exhibit no discernable signs of having backdated. To do so, we compare the sample of likely backdating and clean firms and note no observable differences in penalties levied after the backdating scandal. Directors turn over at the nearly the same rate (13.05% versus 12.34%), shareholders withhold votes at nearly the same rate (5.72% versus 5.84%), and ISS issues a withholding recommendation at nearly the same rate (8.76% versus 8.65%).

Similar to the prior tests, we observe some differences in other firm characteristics among the group of likely backdating and clean firms. Relative to clean firms, likely backdating firms have fewer votes withheld prior to the backdating scandal period and have smaller and less independent boards, lower ROA, but greater abnormal returns), are smaller, and have fewer females on the board. Given these differences among samples (some of which are associated with greater penalties while others are associated with less) we now test for differences among the groups in a multivariate setting.

4.3 Multivariate tests of differences in director penalties among restating, likely backdating and clean firms

To analyze the penalties levied on directors for restating, we estimate the following director-level cross-sectional OLS and probit models:

$$\text{Turnover} = \beta_0 + \beta_1 \text{Restate Indicator} + \beta_2 \text{Likely Backdate Indicator} + \beta_3 \text{Clean Indicator} + \beta_4 \text{Backdating Likelihood} + \beta_5 \text{Materiality} + \beta_j \sum_j \text{Director and Firm Controls} + \varepsilon \quad (1)$$

$$\text{Votes Withheld Post} = \beta_0 + \beta_1 \text{Votes Withheld Pre} + \beta_2 \text{Restate Indicator} + \beta_3 \text{Likely Backdate Indicator} + \beta_4 \text{Clean Indicator} + \beta_5 \text{Backdating Likelihood} + \beta_6 \text{Materiality} + \beta_j \sum_j \text{Director and Firm Controls} + \varepsilon \quad (2)$$

$$\text{ISS Withhold Recommendation Post} = \beta_0 + \beta_1 \text{ISS Withhold Recommendation Pre} + \beta_2 \text{Restate Indicator} + \beta_3 \text{Likely Backdate Indicator} + \beta_4 \text{Clean Indicator} + \beta_5 \text{Backdating Likelihood} + \beta_6 \text{Materiality} + \beta_j \sum_j \text{Director and Firm Controls} + \varepsilon \quad (3)$$

In model 1 we estimate a probit model as the dependent variable is an indicator for whether the director turns over after the backdating scandal came to light for each director-company observation. The advantage to this outcome is that it is a clear indicator of a sharp penalty levied on independent directors. The disadvantage is that, as a binary outcome, it may not capture subtle shifts in investor dissatisfaction. For this reason, we complement these tests with voting results (in model 2), a more refined measure.

To estimate turnover we include all directors on the board as of the latest voting meeting prior to November of 2005, which is the date the Wall Street Journal broke the backdating scandal.¹⁶ Similarly, in June 2007, Glass Lewis published a report identifying all firms publicly implicated in backdating. Therefore, we measure turnover in the post period as of the first meeting

¹⁶ We use November of 2005 because that 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. However, we chose the earlier date so as to not capture any voting penalties that might be attributable to backdating occurring between November 2005 and March 2006. See Appendix 2 for a timeline.

after the Glass Lewis report where the director stands for election.¹⁷ We use these dates because many firms in our sample were never publicly implicated in backdating, and do not have a fixed revelation date to evaluate “pre” and “post” periods. See Appendix 2 for a timeline of backdating events and our estimation windows.

The variables of interest are the indicators for each of the three groups: (restating, likely backdating and clean). Since the sample includes all grant-issuing firms, the coefficients reflect the difference in the dependent variables between the sample of interest and the omitted sample (i.e. firms that do not fit into any group) conditional on the covariates. As we are interested in comparing differences between the likely backdating firms and the other groups (restating and clean) our main tests will be a comparison of the magnitude of the coefficients on the group indicators. If shareholders and the ISS levied penalties on directors for issuing a restatement, we would observe higher turnover, votes withheld, and a vote withheld recommendation for restating firms relative to likely backdating firms ($\beta_1 > \beta_2$). Alternatively, if independent directors are rewarded when firms resolve the accounting error by issuing a restatement, restating firms should show lower penalties relative to likely backdating firms ($\beta_1 < \beta_2$). Finally, if director penalties do not discriminate between firms that show evidence of backdating (likely backdating) and those that show no evidence of backdating (clean), the respective coefficients should not differ (i.e. $\beta_2 = \beta_3$).

In terms of control variables, we follow prior literature and include a variety of director and firm characteristics (e.g. Cai, Garner, & Walkling, 2009). We control for the following firm

¹⁷ We delay the beginning of the post period as it likely took time for shareholders to recognize the extent of backdating and how it could be detected. Also, for firms with a staggered board, we capture shares withheld within two years of either the pre or post measurement date. For example, we capture votes withheld in the pre period for all directors who faced an election between November 2003 and November 2005. If the director faced two elections in that period we use the latest election results. Similarly, we capture votes withheld in the post period for all directors who faced an election between June 2007 and June 2009. If the director faced two elections in that period we use the earliest election results.

characteristics: the number of directors (Board Size), the percentage of independent directors (Board Independence), ROA, log of assets, and the contemporaneous abnormal return. In terms of director characteristics, we control for director tenure, gender, age, and whether it was the directors first year on the board. We estimate these characteristics as of the fiscal year end in 2007, which is around the time backdating would affect shareholder votes.

In equation 2, we examine the continuous measure of votes withheld using OLS. Also, we control for votes withheld prior to the BD scandal rendering this a “changes” analysis. Consistent with the turnover tests, we measure votes withheld in the pre period as of the latest voting meeting prior to November of 2005, and post votes as of the first meeting the director stood for election after June 2007.

Structurally, model 3 is similar to 1 in that the dependent variable is binary and is estimated using probit. In this model, however, we control for whether ISS issued a withholding recommendation in the pre period to capture changes in ISS recommendations occurring contemporaneously with the revelation of the backdating scandal. Conceptually, however, this model is different from the prior models in that we are modeling the behavior of an independent actor (ISS) as opposed to shareholders.

Given that ISS is an institution dedicated to providing governance recommendations, we expect that it is more likely than the average shareholder to understand the intricacies of backdating and potentially be able to identify backdating firms absent disclosures (as we do). Consistent with this expertise, proxy advisory firms have significant impact on shareholder voting outcomes (Malenko & Shen, 2015; Bethel & Stuart, 2002; Cai, Garner, & Walkling, 2009; Morgan, Poulsen, & Wolf, 2006). That said, ISS’s official policy regarding backdating is to “examine case-by-case [examples of backdating] for distinctions to be made between ‘sloppy’ plan administration versus

deliberate action or fraud.” Further, one of the factors it explicitly considers is the “size of the restatement due to options backdating”.¹⁸ Given this policy, it appears that ISS would rely to some degree on firms to self-report backdating in deciding whether to issue a withhold recommendation. If that is the case, we would expect that restatements would increase the likelihood of an ISS withholding recommendation.

Table 4 documents results of these tests. Broadly, directors at restating firms, relative to those at likely backdating firms, are more likely to turnover, face an ISS withholding recommendation as well as have more votes withheld, consistent with univariate tests. However, directors at likely backdating firms face no different penalties relative to those at clean firms. These results hold after controlling for the severity of the backdating (backdating likelihood and materiality) as well as a variety of firm and director characteristics.

Economically, the penalties levied on restating directors are large. The marginal effect of restating on turnover (an ISS withholding recommendation), holding all covariates at their means, is 6% (9%) higher, which compares to unconditional rates of 12% (8%). The sharp increase in the likelihood of an ISS withholding recommendation is not surprising given the explicit policy of ISS to withhold votes for directors at firms implicated in backdating. Further, directors at restating firms see 6.7% more votes withheld which compares with 5% of mean votes withheld in the full sample. That shareholders levy such seemingly severe penalties for restating is consistent with Bernile and Jarrell (2009) who find that each dollar restated because of backdating led to a dollar loss in valuation. Since stock compensation is non cash, they attribute this erosion in value to an increase in information risk. Broadly, these models suggest that directors at restating firms face statistically and economically larger penalties than those at likely backdating firms. However,

¹⁸ See ISS’s official policy on its website (accessed 10/2015): <https://www.issgovernance.com/file/policy/2015-us-summary-voting-guidelines-updated.pdf>

among firms that do not issue a restatement, director penalties are uniformly applied regardless of the likelihood the firm engaged in backdating.

These results are not dependent on our method of identifying backdating firms. Even if shareholders, the ISS or others were unable to apply statistical methods to grant patterns to identify backdaters, they could have applied penalties to any firm publicly associated with backdating by the media, SEC, etc. For example, Glass Lewis compiled a list of firms publicly implicated in backdating through either a restatement, SEC investigation, class action lawsuit or otherwise. In unreported regressions, we find that directors serving at firms on this list incur significantly higher penalties than directors serving on firms not on the list. However, when we separate the list between firms that restate and firms that do not restate, we find penalties levied only on the restating firms. In fact, directors at firms publicly associated with backdating that do not restate suffer penalties no greater than firms not on the list. Finally, we also follow Bebchuk et al. (2010) and identify backdating firms using an easily implementable measure: whether firms issued a grant at the lowest price of the month. Again, we find no evidence of penalties for directors serving on boards that backdated by this measure. These results again suggest that penalties were levied according to a rather simple heuristic (i.e. a restatement), even if the firm was publicly associated with backdating.

4.4 Subsample analysis

Our prior tests suggest that directors faced consequences only when their firm issued a backdating restatement. Such an effect would create an incentive to avoid issuing a restatement. One alternative possibility, though, is that the directors at restating firms were genuinely more culpable, perhaps because they or their firm behaved more egregiously along some unobservable dimension. To address this possibility we next examine whether director penalties vary across

several dimensions related to the likely culpability of the independent director in the backdating scandal.

Prior tests examine one form of disclosure through which firms could admit to the accounting error (i.e. the restatement). However, in the context of backdating, several firms disclosed nonmaterial backdating errors, and issued a charge. In effect, these firms admitted to backdating, but did not restate prior periods. We also explore other dimensions related to the director's association with backdating including: whether the director was actually on the board during the backdating period (early versus new), whether the director sat on the compensation or audit committee, whether we statistically identify the firm as involved in backdating using the method explained in section 3.1, and whether the director personally received a grant that was likely backdated.

Table 5 documents these tests. To examine penalties levied on directors at firms that issue a restatement, we compile a sample of independent directors at restating firms that are the least likely to have been involved: those who were not on the board during the backdating period (new directors). We then compare penalties for this group relative to five other groups of firms that do not issue a restatement, but whose directors were most likely to have been involved in backdating: early directors on firms that issue a charge (B), directors that sat on the compensation committee during the backdating period on firms that issued a charge (C), early directors on firms that are likely to have backdated (D), directors that sat on the compensation committee during the backdating period at firms that were likely to have backdated (E), and directors that likely received a backdated grant (F). Broadly, univariate and multivariate results indicate that new directors at firms that restated faced penalties at least as severe as each of the groups analyzed, and often greater.

These tests suggest that independent directors at restating firms whose service on the board post-dates the backdating era fared no better, and in some cases, worse, than independent directors who not only served on the board of firms that are likely to have engaged in backdating during the backdating era, but also: served at firms that publicly acknowledged backdating issues ((A) and (B)) or even received a suspicious grant (F). Since all of the comparison groups, (B) through (F), represent higher complicity in backdating relative to a director who was not even on the board during the backdating era, these results undercut the view that the differential penalties between restating and non-restating firms simply represents differential complicity in the scandal.

4.5 Propensity Score Tests

The prior tables compare penalties levied on directors at restating firms relative to penalties levied on directors at either firms that show evidence of backdating, or penalties levied on firms that issue a charge. We now examine a third comparison group: directors at firms that have a similar propensity to (but do not) issue a backdating restatement. To conduct this test we estimate equations 1-3 and model the likelihood of a firm issuing a backdating restatement. We then match with replacement directors at firms that issued a backdating restatement to 1, 3 and 5 directors at firms with the nearest likelihood of issuing a restatement within the same Fama-French 22 industry designation. We then conduct a T-test of differences between the penalties levied on the director at the restating firm and the directors at the non-restating firms.

Panel A of Table 6 demonstrates that prior to the matching, restating firms have a greater likelihood of having backdated (94% versus 86%), a higher materiality ratio (21.13% versus 5.43%), smaller boards (8.77 versus 9.67) and lower ROA (-2.67% versus 0.11%). After matching, these differences are no longer statistically different except that restating directors sit at firms with larger boards (8.77 versus 8.07).

Panel B tabulates the propensity score model used to create the matched sample (pre-match column). Consistent with our statistical measures properly identifying backdating firms, the likelihood of issuing a restatement is positively related to backdating likelihood (coefficient=1.71, T-stat=5.32) and materiality (coefficient=0.49, T-stat=5.46). In untabulated tests, we find that a one standard deviation increase in the likelihood of backdating (materiality ratio) increases the likelihood of issuing a restatement by 2.2% (0.9%) relative to an unconditional likelihood of 3.7%. In fact, the chi-squared statistic and pseudo r-squared of a model with only these two variables is 83.67 and 0.05, which is more than half that of the full model tabulated in Panel B. That said, the full model has significant explanatory power with a pseudo r-squared of 0.09 and chi-squared test 134.86, which rejects the null of no joint significance at a $P < .01$ level. However, when the same model is estimated over the matched sample, statistical significance fades for nearly all variables, and the chi-squared test and pseudo r-squared sharply declines. This indicates that the loss of statistical differences between the groups noted in Panel A is not attributable to a decline in degrees of freedom, but a reduction in meaningful differences across the treatment and control groups.

Table 7 tabulates results of the propensity score tests. The figures tabulated within the table are the mean level of penalties within a given group. We tabulate tests altering the number of control firms matched to a treatment firm (one, three and five) to ensure that inferences are not sensitive to this dimension of the model. For example, the first line indicates that 19.55% of directors at firms that restated turned over (consistent with table 2). However, the turnover rate for the control group composed by a one-to-one match between the control and treatment group was only 9.5%. The difference between the change in the treatment and control group (of 10.05%) is statistically significant at a 0.05 level (T-Statistic of 2.08). The differences reported in this table are always positive, and statistically significant at conventional levels in eight of nine models

indicating greater penalties for the directors at restating firms relative to matched directors at non-restating firms.

4.6 Additional Tests

To further ensure that restating firms are not simply more severe cases of backdating we control for a variety of other estimates of the severity of the backdating.¹⁹ First, we count the number of backdated grants issued by the firm (i.e. all grants with a backdating likelihood in excess of 0.95). Second, we scale this count by the number of grants issued by the firm over the granting period. Third, we aggregate the materiality of all backdated grants issued by the firm. Fourth, we count the number of years the firm issued a materially backdated grant. Finally, to ensure that results are not sensitive to the thresholds we set for the likelihood of backdating or materiality, we test alternative thresholds for the likelihood of backdating (90 and 99%) as well as materiality (10 and 15% of net income). In untabulated analyses, we replicate Table 4 controlling for these measures and find very similar results to those documented in this paper.

Finally, to examine whether the restatement heuristic triggers director penalties in a broader context, we analyze penalties associated with restatements that arise for any reason. To do so, we merge restatement data from Audit Analytics with director data from Risk Metrics and identify directors that joined the board prior to a restatement disclosure, but after the restatement period ended.²⁰ In this way, these directors were not on the board when the firm issued financial statements with material errors, but were on the board when the error were revealed. Preliminary results indicate that these new directors turned over at a mean rate statistically higher than the base rate.

¹⁹ To clarify, we do control for the severity of backdating with variables estimating the likelihood of backdating and the materiality of the backdated grant in tables 4 and 5.

²⁰ We do not use Boardex data as the exact date of the annual meeting is not identified.

5. Conclusion

In this paper we examine whether director penalties create incentives for directors to monitor executives and encourage firms to timely report evidence of misbehavior. We find that directors serving on boards of firms that issue a backdating restatement relative to directors serving on firms that likely backdated but did not issue a restatement are more likely to turnover, have more votes withheld and are more likely to receive a withholding recommendation from ISS. Further, these results hold among directors that joined the board after backdating largely ceased. Also, among directors serving at firms that do not issue a restatement, these outcomes are no different between a group of firms that are highly likely to have backdated, and a group that exhibits no evidence of backdating. Broadly, the evidence in this paper suggests that director penalties follow a simple heuristic and rely on the presence of a restatement even in a setting where it is possible to gauge the likelihood of misbehavior independent from the firm's admission. Such penalties likely create perverse disincentives for independent directors to push for greater transparency as it relates to accounting errors.

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Appendix 1
Variable Definitions

Penalties

- Votes Withheld - Post = the proportion of votes withheld against the director in the first election after 6/14/2007, but not after 6/14/2009 measured as: $[(\text{votes withheld} + \text{votes against}) / (\text{votes for} + \text{votes against} + \text{votes abstain} + \text{votes withheld})]$;
- Turnover = indicator variable that takes a value of (1) if the director was not on the board in the first year after he or she would have stood for election and (0) otherwise;
- ISS Withhold - Post = indicator variable that takes a value of (1) if ISS recommended a vote against or vote withheld for the director in the first election after 6/14/2007, but not after 6/14/2009 and (0) otherwise;

Severity

- Backdating Likelihood = the likelihood that the firm backdated estimated following the methodology outlined in section 3.1;
- Materiality = the materiality of backdated grants which is a ratio of the estimated amount of backdating (described in section 3.2) aggregated across all grants issued in the fiscal year and scaled by yearly income less special items or non-operating items. We select the highest yearly ratio to use in firm-level regressions;

Controls

- Votes Withheld - Pre = the proportion of votes withheld against the director in the last election before 11/11/2005, but not before 11/11/2003 measured as: $[(\text{votes withheld} + \text{votes against}) / (\text{votes for} + \text{votes against} + \text{votes abstain} + \text{votes withheld})]$;
- ISS Withhold - Pre = indicator variable that takes a value of (1) if ISS recommended a vote against or vote withheld for the director in the last election before 11/11/2005, but not before 11/11/2003 and (0) otherwise;
- Abnormal Return = firm raw return less the value weighted return compounded over the contemporaneous fiscal year;
- Backdated Grant = indicator variable that takes a value of (1) if the director received a backdated grant from the firm and (0) otherwise;
- Board Independence = the percentage of board members who are independent;
- Board Size = the number of board members;
- Director Age>65 = indicator variable that takes a value of (1) if the director is older than 65 years of age and (0) otherwise;
- Female = indicator variable that takes a value of (1) if the director is female and (0) otherwise;
- First Year Indicator = indicator variable that takes a value of (1) if the director is serving his/her first year on the Board and (0) otherwise;
- Industry = defined as the Fama-French 22 industry designation;
- Log Assets = log of total assets
- Network Size = the size of the directors network, obtained from Boardex;
- ROA = return on assets defined as income Pre extraordinary items (IBQ) divided by assets as the beginning of the quarter (ATQq-1);
- Tenure = the number of years the director served on the board;

Appendix 2: Timeline of Events

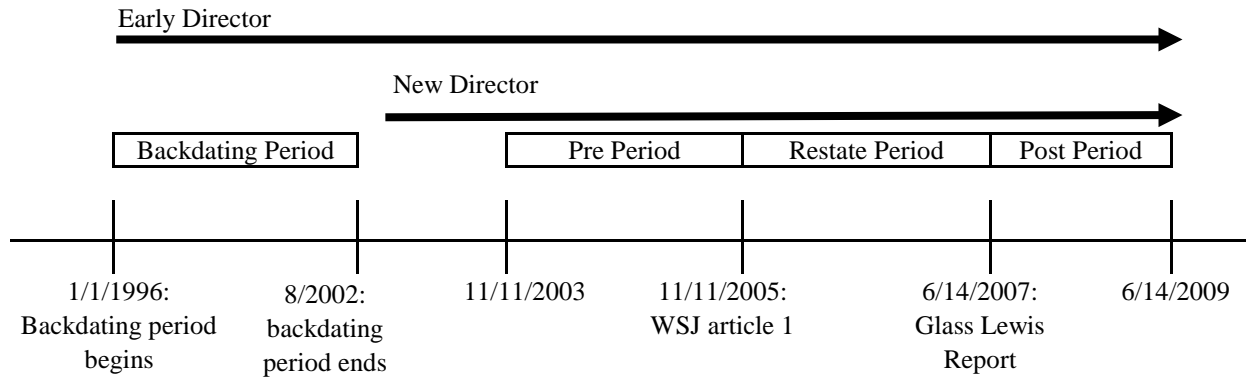


Table 1
Sample Attrition

	<u>Firms</u>	<u>Directors</u>
Panel A: Turnover Sample		
Firms with data in Compustat, Execucomp and CRSP pre and post backdating scandal	4,352	
Firms issuing grants from 1996-2002	2,910	
Independent director data available from Boardex pre and post backdating scandal	2,172	15,649
Independent directors not on board during backdating	<u>1,874</u>	<u>4,817</u>
Composition:		
Restating firm (high likelihood of backdating, restatement)	70	179
Non restating firm (high likelihood of backdating, no restatement)	250	636
Clean firm (low likelihood of backdating, no restatement)	939	2,002
Remaining control firm	913	2,000
Panel B: Voting and ISS Recommendation Sample		
Turnover Sample	1,874	4,817
Directors that do not turnover	1,782	4,014
Directors standing for election during the pre and post windows	1,501	2,994
ISS recommendation available	946	1,681
Voting data available	<u>935</u>	<u>1,618</u>
Composition:		
Restating firm (high likelihood of backdating, restatement)	41	78
Non restating firm (high likelihood of backdating, no restatement)	98	170
Clean firm (low likelihood of backdating, no restatement)	364	685
Remaining control firm	<u>432</u>	<u>685</u>

Table 2
Descriptive Statistics

This table provides descriptive statistics for the main samples used in this paper. In this table, backdating likelihood, turnover, votes withheld, ISS withholding recommendation, materiality, indep. percentage, ROA, Female, Director Age>65 and First Year Indicator are multiplied by 100. Variables are defined in Appendix 1.

	N	Mean	Std	P25	P50	P75
<u>Penalties</u>						
Turnover	4,817	12.14	32.67	0.00	0.00	0.00
Votes Withheld Post	1,618	5.45	8.64	0.01	0.02	0.06
ISS Withhold Recomm. Post	1,618	7.79	26.81	0.00	0.00	0.00
<u>Severity</u>						
Backdating Likelihood	4,817	86.24	17.12	82.68	92.35	97.39
Materiality	4,817	6.01	24.77	0.09	0.48	2.14
<u>Controls</u>						
Votes Withheld Pre	1,618	5.34	7.26	1.41	2.86	6.17
ISS Withhold Recomm. Pre	1,618	5.62	23.05	0.00	0.00	0.00
Board Size	4,817	9.63	2.78	8.00	9.00	11.00
Indep. Percentage	4,817	83.25	8.91	77.78	85.71	90.00
ROA	4,817	0.01	17.88	0.59	3.37	7.49
Log Assets	4,817	7.28	2.12	5.87	7.32	8.68
Abn Return	4,817	-0.06	0.39	-0.30	-0.12	0.11
Tenure	4,817	1.15	0.93	0.00	1.00	2.00
Female	4,817	13.58	34.26	0.00	0.00	0.00
Director Age>65	4,817	12.71	34.47	0.00	0.00	0.00
First Year Indicator	4,817	31.24	46.35	0.00	0.00	100.00

Table 3

Univariate Tests of Penalties and Characteristics across Firm Groups

This table provides T-tests of differences in means across whether the firm restated ***, **, * represents significance at a $p < 0.01$, $p < 0.05$, $p < 0.1$ level, respectively using two-sided tests. In this table, backdating likelihood is multiplied by 100. Variables are defined in Appendix 1. In this table, all variables are multiplied by 100 except board size, log assets, and tenure.

	Sample: Restate? Likelihood of Backdating?	(A)	(B)	(C)	Difference in Means	
		Likely Backdate	Restate Yes	Clean No	(A)-(B)	(A)-(C)
		No High	High	Low		
Penalties						
Turnover		13.05 (636 Obs.)	19.55 (179 Obs.)	12.34 (2002 Obs.)	-6.50 ** (-2)	0.71 (0.47)
Votes Withheld Post		5.72 (276 Obs.)	11.21 (108 Obs.)	5.84 (1064 Obs.)	-5.49 *** (-4.11)	-0.12 (-0.21)
ISS Withhold Recomm. Post		8.76 (274 Obs.)	17.59 (108 Obs.)	8.65 (1063 Obs.)	-8.83 ** (-2.18)	0.11 (0.05)
Severity						
Backdating Likelihood		98.06	94.00	72.58	4.06 *** (3.7)	25.48 *** (59.92)
Materiality		14.53	21.13	4.04	-6.60 * (-1.85)	10.49 *** (6.41)
Controls						
Votes Withheld Pre		4.21	9.41	5.20	-5.20 *** (-4.84)	-0.99 ** (-2.31)
ISS Withhold Recomm. Pre		7.31	10.19	6.81	-2.88 (-0.86)	0.50 (0.28)
Board Size		8.21	8.77	9.67	-0.56 *** (-2.98)	-1.46 *** (-13.8)
Indep. Percentage		81.91	82.82	83.40	-0.91 (-1.18)	-1.49 *** (-3.46)
ROA		-5.71	-2.67	-0.81	-3.04 * (-1.76)	-4.90 *** (-4.67)
Log Assets		6.12	7.44	7.09	-1.32 *** (-9.16)	-0.97 *** (-11.11)
Abn Return		-1.49	-4.53	-4.97	3.04 (0.95)	3.48 * (1.65)
Tenure		1.19	1.17	1.14	0.02 (0.22)	0.05 (1.04)
Female		9.43	9.50	13.19	-0.07 (-0.03)	-3.76 *** (-2.71)
Director Age>65		15.25	9.50	12.94	5.75 ** (2.2)	2.31 (1.44)
First Year Indicator		33.33	32.96	31.27	0.37 (0.09)	2.06 (0.97)

Table 4
Multivariate tests of director penalties

This table documents multinomial probit (columns 1 and 3) and OLS (column 2) tests of the relation between a restatement and director penalties (where the dependent variable is tabulated in the column heading). The sample is all firms issuing stock-option grants from 1996-2002. ***, **, * 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 the appendix. In this table, tenure is scaled by 100 and the first year indicator by 1,000.

	(1) Probit: DV= Turnover	(2) OLS: DV= Votes Withheld	(3) Probit: DV= ISS Withhold Recomm.
<u>Sample</u>			
Likely Backdate (High likelihood of backdating, No restatement)	0.04 (0.48)	-0.27 (-0.31)	-0.24 (-1.13)
Restate (Backdating restatement)	0.32** (2.53)	6.50*** (3.31)	0.69*** (2.62)
Clean (Low likelihood of backdating, No restatement)	0.05 (0.69)	0.48 (0.63)	-0.11 (-0.61)
Likely Backdate - Restate	-0.28**	-6.77***	-0.93***
Pvalue	0.04	< 0.01	< 0.01
Likely Backdate - Clean	-0.01	-0.75	-0.13
Pvalue	0.92	0.44	0.59
<u>Severity</u>			
Backdating Likelihood	-0.01 (-0.07)	1.47 (0.76)	0.39 (0.83)
Materiality	-0.01 (-0.16)	-1.58*** (-2.60)	-0.70** (-2.33)
<u>Controls</u>			
Votes Withheld - Pre		0.23*** (3.93)	
ISS Withhold Recomm. - Pre			1.07*** (6.15)
Board Size	0.04*** (3.31)	-0.12 (-1.05)	-0.00 (-0.11)
Board Independence	-0.02 (-0.06)	-3.97 (-1.16)	-0.21 (-0.29)
ROA	-0.54*** (-3.43)	-6.98** (-2.47)	-1.16** (-1.98)
Log Assets	-3.69** (-2.03)	-1.75 (-0.08)	-4.26 (-0.84)
Abn Return	-0.10 (-1.35)	0.07 (0.07)	0.30* (1.77)
Tenure	1.76 (0.42)	0.83 (0.02)	11.10 (1.42)

Continued on next page

Table 4 (Continued)

Multivariate tests of director penalties

	(1) Probit: DV= Turnover	(2) OLS: DV= Votes Withheld	(3) Probit: DV= ISS Withhold Recomm.
Female	-0.08 (-1.09)	-0.07 (-0.12)	0.08 (0.65)
Director Age>65	0.20*** (2.96)	-1.07* (-1.77)	-0.19 (-1.13)
First Year Indicator	-54.86 (-0.65)	80.07 (0.10)	162.53 (0.92)
Constant	-1.17*** (-3.10)	6.27 (1.64)	-5.57*** (-7.54)
Observations	4,817	1,618	1,618
R-Squared		0.10	
Pseudo R-Squared	0.03		0.12
Cluster	Firm	Firm	Firm
Fixed Effects	Industry	Industry	Industry

Table 5

Tests of Penalties and Characteristics for New Independent Directors at Firms that Issue a Restatement Relative to Early Directors at Firms that did not Issue a Restatement

This table examines penalties for directors that joined boards after backdating largely ceased but who were on the board of a firm that issued a backdating restatement. Panel A provides T-tests of differences in means while panel B documents multivariate tests. In panel B, the sample includes the benchmark sample (A) as well as the test sample examined and the tabulated figure is the parameter estimate and statistical significance of an indicator for the benchmark sample (A). Otherwise, these models replicate equations 1-3 with all control variables, industry-fixed effects and standard errors clustered at the firm level. We tabulate either the R-squared (votes withheld models) or pseudo R-squared of these models in the "Fit" field. ***, **, * represents significance at a $p < 0.01$, $p < 0.05$, $p < 0.1$ level, respectively using two-sided tests and assuming unequal variances of the population in panel A. Variables are defined in Appendix 1.

	Mean Penalty (Observations)			Panel A: T Tests of Difference in Mean Penalties			Panel B: Multivariate Tests of Penalties between Benchmark and Test Samples			
	Turnover	Votes	ISS Withhold	Turnover	Votes	ISS Withhold	Turnover	Votes	ISS Withhold	
		Withheld	Recomm.		Withheld	Recomm.		Withheld	Recomm.	
Benchmark Sample										
(A) New restating directors	0.20 (179)	0.11 (108)	0.18 (108)							
Test Sample										
(B) Early directors that issue a charge	0.12 (163)	0.06 (96)	0.08 (78)	(A)-(B)	0.08* (1.85)	0.05*** (3.12)	0.10** (2.07)	-0.15 (-0.52)	8.54*** (2.76)	0.77 (1.21)
								Obs	295	155
								Fit	0.10	0.49
(C) Early directors on the comp comm that issue a charge	0.06 (65)	0.06 (40)	0.10 (40)	(A)-(C)	0.14*** (3.17)	0.05*** (2.76)	0.08 (1.25)	2.00*** (3.22)	7.65 (1.48)	-0.18 (-0.23)
								Obs	214	110
								Fit	0.20	0.51
(D) Early directors at firms with a high likelihood of backdating	0.14 (1081)	0.07 (453)	0.15 (451)	(A)-(D)	0.06* (1.77)	0.04*** (2.84)	0.03 (0.68)	0.25 (1.35)	5.98*** (2.75)	1.04*** (2.98)
								Obs	1,248	417
								Fit	0.05	0.26
(E) Early directors on the comp comm at firms with a high likelihood of backdating	0.14 (284)	0.06 (158)	0.10 (158)	(A)-(E)	0.06 (1.51)	0.05*** (3.34)	0.08* (1.70)	0.58** (2.35)	7.39*** (2.92)	2.44*** (3.06)
								Obs	436	210
								Fit	0.09	0.43
(F) Early directors with a high likelihood of receiving a backdated grant	0.14 (498)	0.07 (217)	0.10 (217)	(A)-(F)	0.06 (1.64)	0.04*** (2.92)	0.08* (1.77)	0.22 (1.01)	4.42* (1.96)	0.54 (1.44)
								Obs	650	245
								Fit	0.09	0.37

Table 6
Propensity Score Matching: Diagnostic Tests

This table reports diagnostics from the propensity score matching tests presented in the following table. Panel A reports pairwise comparisons of the variables on which matching is performed. Tabulated differences are between new directors at firms that issue a restatement related to backdating and new directors at firms that do not issue a backdating restatement, both pre- and post-matching. T-statistics are tabulated in parenthesis. Panel B reports the propensity score model on both the full sample (pre-match), and using the matching sample (post-match). The expected value from the pre-match model generates the propensity scores for matching. The dependent variable equals (1) if the director serves at a firm that issues a restatement and (0) otherwise. The probit model is estimated using the first model in the following table with a one-to-one match. Accordingly, the 179 treatment directors (those serving at firms that issue a backdating restatement) are matched to one control firm in the same industry with the closest propensity score, derived from the matching model. ***,**,* represents significance at a $p < 0.01$, $p < 0.05$, $p < 0.1$ level, respectively using two-sided tests. In Panel A the tabulated statistics for backdating likelihood, materiality, board independence, ROA, Log Assets, abnormal return and tenure are multiplied by 100 and first year indicator is multiplied by 1,000.

	Panel A					Panel B	
	Comparing Sample Characteristics					Probit Regressions- Propensity Model	
	Pre-Match			Post-Match		Pre- Match	Post-Match
	Restate=1	Restate=0	Diff.	Restate=0	Diff.		
Backdating Likelihood	94.00	85.95	8.06*** (6.20)	93.14	0.87 (0.64)	1.71*** (5.32)	0.28 (0.50)
Materiality	21.13	5.43	15.70*** (8.38)	15.70	5.43 (0.91)	0.49*** (5.46)	0.20 (1.32)
Board Size	8.77	9.67	-0.90*** (-4.25)	8.07	0.70*** (2.72)	-0.12*** (-5.87)	0.11** (2.53)
Board Independence	82.82	83.26	-0.44 (-0.65)	80.45	2.38* (1.90)	-0.03 (-0.07)	1.33 (1.64)
ROA	-2.67	0.11	-2.78** (-2.04)	-0.49	-2.18 (-0.92)	-0.59*** (-2.96)	-0.06 (-0.14)
Log Assets	7.44	7.27	0.17 (1.04)	7.18	0.26 (1.05)	13.12*** (5.40)	-2.76 (-0.57)
Abn Return	-4.53	-5.61	1.08 (0.37)	-0.10	-4.43 (-0.75)	0.08 (0.88)	-0.11 (-0.60)
Tenure	1.17	1.15	0.02 (0.38)	1.26	-0.08 (-0.64)	6.26 (1.02)	-14.57 (-1.25)
Female	0.10	0.14	-0.04 (-0.04)	0.06	0.04 (1.03)	-0.19 (-1.60)	0.30 (1.13)
Director Age>65	0.10	0.13	-0.03 (-1.27)	0.12	-0.03 (-0.62)	-0.13 (-1.20)	-0.15 (-0.69)
First Year Indicator	0.33	0.31	0.02 (0.51)	0.31	0.02 (0.27)	136.72 (1.10)	-249.53 (-1.03)
Constant						-3.32*** (-7.43)	-1.85** (-2.38)
Observations						4,817	358
New Restate Directors						179	179
Chi-squared						134.86	20.27
P-Value						< 0.01	0.04
Pseudo R-squared						0.09	0.04

Table 7**Propensity Score Matching: Testing the Change in Director Penalties across the Presence of a Restatement**

This table reports estimations from a nearest neighbors matching model testing the change in the mean outcome (turnover, votes withheld and the likelihood of an ISS withholding recommendation) across treatment (directors at firms that issue a restatement) and control firms. Treatment firms are matched to the given number of nearest neighbors according to the propensity scoring model following equation 1-3 (as illustrated in table 4). Matching occurs within industry-year groups to one, three and five firms with the nearest propensity of having issued a backdating restatement. Reported statistics are the mean level of turnover, votes withheld and the likelihood of an ISS withholding recommendation. T-statistics are tabulated in parenthesis. ***, **, * represents significance at a $p < 0.01$, $p < 0.05$, $p < 0.1$ level, respectively using two-sided tests. In this table, all outcomes are scaled by 100.

<u>Outcome</u>	<u>Nearest Neighbors</u>	<u>Restate=1 (Obs.)</u>	<u>Restate=0 (Obs.)</u>	<u>Difference (T-Stat)</u>
Turnover	One		9.50 (179 Obs.)	10.05** (2.08)
	Three	19.55 (179 Obs.)	13.04 (358 Obs.)	6.51* (1.67)
	Five		13.74 (537 Obs.)	5.81 (1.60)
Votes Withheld	One		5.38 (78 Obs.)	6.75*** (3.07)
	Three	12.13 (78 Obs.)	6.18 (156 Obs.)	5.95*** (3.29)
	Five		5.72 (234 Obs.)	6.41*** (3.70)
ISS Withholding Recommendation	One		1.28 (78 Obs.)	16.67*** (3.45)
	Three	17.95 (78 Obs.)	3.85 (156 Obs.)	14.1*** (2.95)
	Five		4.36 (234 Obs.)	13.59*** (2.86)