Tournament incentives and corporate fraud

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ABSTRACT

This paper identifies a new incentive for managers to engage in corporate fraud stemming from the relative performance evaluation feature of CEO promotion tournaments. We document higher propensities to engage in fraud for firms with strong tournament incentives (as proxied for by the CEO pay gap). We posit that the relative performance evaluation feature of CEO promotion tournaments creates incentives to manipulate performance, while the option-like character can motivate managers to engage in risky activities. We thereby extend previous corporate fraud literature that focuses mainly on equity-based incentives and reports mixed findings. Our results are robust to using different fraud samples, and controlling for other known determinants of fraud as well as manager skills.

JEL Classification: J33; G30; M53
Keywords: Corporate fraud, tournament incentives, CEO pay gap

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ABSTRACT

This paper identifies a new incentive for managers to engage in corporate fraud stemming from the relative performance evaluation feature of CEO promotion tournaments. We document higher propensities to engage in fraud for firms with strong tournament incentives (as proxied for by the CEO pay gap). We posit that the relative performance evaluation feature of CEO promotion tournaments creates incentives to manipulate performance, while the option-like character can motivate managers to engage in risky activities. We thereby extend previous corporate fraud literature that focuses mainly on equity-based incentives and reports mixed findings. Our results are robust to using different fraud samples, and controlling for other known determinants of fraud as well as manager skills.

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1. Introduction

Corporate fraud can have huge costs for shareholders, as evidenced by drops in market value of as high as 38% for firms accused of such behavior (Karpoff, Lee, and Martin, 2008a). To mitigate those costs, it is important to explore the incentives that lead to fraudulent behavior in corporations. Prior literature has primarily investigated how equity-based incentives of CEOs impact the propensity to commit fraud. While CEO behavior will respond exclusively to performance-based incentives, lower ranked executives (e.g., VPs) will respond both to performance-based incentives and to incentives stemming from the opportunity to get promoted to the CEO position and receive increased compensation, that is, promotion-based tournament incentives (e.g., Green and Stokey, 1983; Baker, Jensen, and Murphy1988).

Rank-order tournaments are schemes of relative performance evaluation because the best relative performer wins and will receive the tournament prize. Tournament theory evolved as a way to explain the large pay gaps between the CEO and lower ranked executives commonly observed in practice and receiving considerable media attention. Lazear and Rosen (1981) have derived analytically that tournaments are optimal labor contracts, and they demonstrate a positive relationship between the effort made by agents and the magnitude of the tournament prize. Additionally, pay gaps provide a solution to the agency problems associated with monitoring difficulties that can prevent linking executive compensation to marginal product, such as, managerial shirking (Henderson and Frederickson, 2001). Consistent with this finding, Kale, Reis, and Venkateswaran (2009) find that tournament incentives are positively associated with firm performance. This literature commonly proxies for tournament incentives (the promotion prize) using the pay gap between the CEO and lower ranked executives (VPs).

Cheng (2011) presents analytical and empirical evidence that schemes of relative performance evaluation can lead to financial misreporting, because executives try to manipulate the learning process about their abilities due to career concerns. Furthermore, Goel and Thakor (2008) show analytically that executives faced with tournament incentives tend to take on greater risks to increase their promotion probability. Greater risk-taking incentives may translate into more efficient operating and financial policies, but Armstrong et al. (2013) find that such incentives may also increase the incidence of financial misreporting activities.

Finally, experimental studies document that stronger tournament incentives lead to more sabotage (Harbring and Irlenbusch, 2011) and cheating, e.g., in the form of dishonest performance reporting (Conrads et al., 2014). Accordingly, this paper tests whether tournament incentives are positively associated with a propensity to engage in fraudulent behavior.

We investigate this question by using a sample of reported fraud cases in large U.S. companies between 1994 and 2004, as identified in Dyck, Morse, and Zingales (2010). In order to measure tournament incentives, we follow prior literature and use the pay gap between the CEO and the median VP. We expect to find larger pay gaps for fraud firms than for non-fraud firms.

We find that fraud firms, on average, have significantly larger pay gaps than non-fraud firms. Multivariate tests confirm our prediction of a positive association between tournament incentives and the propensity to engage in fraudulent behavior. Our results also suggest an economically meaningful effect of tournament incentives relative to other fraud determinants.

These results are robust to using different matching methods, including industry fixed effects (Erickson, Hanlon, and Maydew, 2006) and random effects (Lennox and Pittman, 2010). We also document the robustness of our findings to using alternative pay gap measures, controlling for VP ability and to the inclusion of variables related to corporate governance quality and CEO power (e.g., Chen et al., 2006; Bebchuk, Cremers, and Peyer, 2011). We further confirm the external validity of our findings based on the Dyck, Morse, and Zingales (2010) fraud sample by replicating a positive association between tournament incentives and the propensity to engage in fraud using firms named in SEC Accounting and Auditing Enforcement Releases (AAERs). To address the limitation that our primary measure of fraud captures the joint event of a firm engaging in fraud and being caught, we replicate our findings using the likelihood of misreporting as the dependent variable (Dechow et al., 2011).

Taken together, our results suggest that one potential drawback of providing tournament incentives is an increased propensity to engage in fraud. This finding contributes to the literature that investigates dysfunctional consequences as a response to increased incentives (Holmström and Milgrom, 1991; Baker, 1992; Jacob and Levitt, 2003). Specifically, our results align closely with other papers that find a positive association between tournament incentives and excessive risk-taking (e.g., Knoeber and Thurman, 1994), sabotage, and cheating behavior. Our results also complement recent evidence that the use of relative performance evaluation schemes (across firms) can encourage fraud (Wang and Winton, 2012). Our results suggest that tournament incentives significantly influence VP behavior as firms with larger tournament incentives are more likely to engage in fraudulent activities.

Our study also contributes to the literature that investigates the determinants of corporate fraud, and, more specifically, compensation. Most of that work has focused on performance-based incentives of the CEO and other executives (e.g., Armstrong et al., 2013). We document the importance of tournament incentives in explaining the observed variation in fraudulent behavior across firms omitted by prior literature.

Our findings are subject to four primary limitations. First, we caution against a causal interpretation of our findings. Although we aim to address omitted variables bias and results remain robust, e.g., via different forms of matching, random effects estimation and controlling for CEO compensation, CEO power, and corporate governance, ultimately, we cannot rule out that some unobservable variable is correlated with the pay gap and a propensity to engage in fraud.[1]

Second, we use the pay gap to proxy for tournament incentives. This is in line with labor economics (Bognanno, 2001) and other papers in the finance literature that study the effect of tournament incentives on performance (Kale, Reis, and Venkateswaran, 2009) and risk-taking (Kini and Williams, 2012). However, other papers have used variants of the measure to capture different phenomena such as CEO power (Bebchuk, Cremers, and Peyer, 2011) or CEO skill (Masulis and Zhang, 2012). Although we control for these alternative explanations, we cannot rule out that our proxy may suffer from measurement error.

Third, our results indicate that larger pay gaps are associated with dysfunctional consequences, but we provide no evidence of whether the benefits of tournament incentives, documented by previous studies (e.g., Kale, Reis, and Venkateswaran, 2009; Faleye, Reis, and Venkateswaran, 2010), outweigh the costs. Our results suggest considering both the benefits and costs of tournament incentives when designing compensation contracts.

Finally, our results primarily refer to a sample of detected fraud cases. These cases are a function of (i) conducting fraud and (ii) the probability of being detected. To the extent that the probability of being detected is correlated with the pay gap, this would alter our inferences. Our results using the probability of misreporting as the dependent variable, however, document that this alternative explanation is less likely.

The remainder of this paper is structured as follows. In section 2, we develop a testable hypothesis for the association between the likelihood of fraud and the CEO pay gap. Section 3 discusses our sample selection procedure and our research design, while section 4 reports our empirical results. In section 5, we conduct a number of robustness tests, and consider alternative explanations. Section 6 summarizes and concludes.

[1] For example, Bereskin, Campbell, and Kedia (2014) and Cumming, Leung, and Rui (2015) show that corporate culture and board diversity may be associated with fraud.

2. Literature and hypothesis development

2.1 Fraud and incentives

Both fraudulent behavior and managerial compensation have been topics of great interest and debate in practice and academia. The outsize number of detected fraud cases in recent years has led to regulatory changes, as well as to growing interest in the determinants and prevention of fraud due to the high costs to market participants (e.g., Ball, 2009; Karpoff and Lou, 2010; Kedia and Philippon, 2009).

Theory holds that individuals will only engage in fraudulent behavior if the benefits outweigh the costs. Previous literature has identified performance-based compensation, more specifically equity-based compensation, as such a benefit[2]. The traditional view is that equity-based compensation plans align the incentives of management and shareholders (Jensen and Meckling, 1976; Alexander and Cohen, 1999). However, prior research has argued that a manager whose wealth is more sensitive to changes in the firm's stock price has a greater incentive to engage in fraudulent activities such as financial misreporting.

For example, Goldman and Slezak (2006) develop an agency model in which stock-based compensation is a double-edged sword, inducing managers to work harder in some cases, but to engage in fraud in others. Bergstresser and Philippon (2006) find a positive relationship between the sensitivity of a CEO's equity portfolio to changes in stock price (delta) and earnings management; Burns and Kedia (2006) and Efendi, Srivastava, and Swanson (2007) find a positive relationship between a CEO's portfolio delta and earnings restatements. Erickson, Hanlon, and Maydew (2006) and Armstrong, Jagolinzer, and Larcker (2010) fail to find a positive relationship for all VPs or the CEO; Jiang, Petroni, and Wang (2010) find a relationship only for CFOs, not for CEOs. Finally, Bhattacharya and Marshall

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[2] The costs to manangers include penalties, criminal charges, job loss, restrictions on future employment and fines (Karpoff, Lee, and Martin, 2008a; Ahorny, Liu, and Yawson, 2015).

(2012) find that compensation and wealth is positively linked to the probability of being indicted as an illegal insider trader.

These mixed findings may arise because different measures of misreporting and research design were used (Armstrong, Jagolinzer, and Larcker, 2010), or because prior papers did not sufficiently control for detection mechanisms that mitigate the effect of equity incentives on misreporting (e.g., Jayaraman and Milbourn, 2014). Armstrong et al. (2013) summarize this research by considering portfolio vegas as an additional factor and find strong evidence of a positive relation between vega and misreporting. Overall, they document that equity portfolios can provide management with incentives to misreport because they make managers less averse to equity risk.

2.2 Tournament incentives, the CEO pay gap, and fraud

The literature discussed above primarily focuses on the role of CEOs and their performance-based incentives in fraud but usually does not take other VPs' tournament incentives into account. More recent evidence, however, indicates that VPs are also likely to be involved in financial misconduct (Jiang, Petroni, and Wang, 2010; Feng et al., 2011). Karpoff, Lee, and Martin (2008a, 2008b) document that 1,433 of 2,206 culpable employees in fraud cases were VPs, of which only a third (515) were CEOs. Hence, our primary objective is to examine whether tournament incentives contribute to explain VPs' engagement in fraudulent behavior.

Tournament theory offers an explanation for the large gaps between CEO pay and the pay of lower ranked executives observed empirically. These gaps are inconsistent with pay being linked to executives' marginal product. Instead, pay gaps[3] provide a solution to the agency problems, such as managerial shirking (Henderson and Fredrickson, 2001), that can

[3] We define the CEO pay gap as the natural logarithm of the difference between CEO total compensation and median VP total compensation. One of the reasons we do not focus on the second highest paid executive is to avoid confounding incentives resulting from mutual monitoring, as documented by Li (2014).

arise from monitoring difficulties. Similarly to equity-based compensation, they aim at aligning the goals and interests of management and shareholders (Lazear and Rosen, 1981).

While the proportion of equity incentives offered to VPs is growing (see Fuller and Jensen, 2002), VPs also face increasing promotion-based tournament incentives over time.[4] In fact, a significant likelihood of insider succession[5] exists in listed firms indicating that assuming a tournament scenario for the CEO position among VPs is plausible: Cremers and Grinstein (2014) document a 71% probability of insider succession between 1993 and 2005 in US firms with only modest variation across industries.

Principals often set up rank-based pay, where pay gaps increase with each rank: The best relative performer is promoted to the next level in the hierarchy, while the others are passed over. These pay gaps are aimed at a better alignment of the interests of principals and agents: In such a tournament scheme, agents have strong incentives to perform well and expend greater efforts because this increases their chances of promotion. At the same time, this behavior increases a firm's output. Moreover, given that the pay gap increases with rank and the largest gap occurs between VPs and the CEO, promoted agents will still have incentives to perform well. Consequently, all VPs compete in a tournament to become the new CEO.

A large body of literature tests the analytical prediction by Lazear and Rosen (1981) that larger tournament prizes (i.e., higher pay gaps) lead to more effort. For example, Kale, Reis, and Venkateswaran (2009) and Chen, Ezzamel, and Cai (2011) find that tournament incentives are associated with better firm performance, while Bebchuk, Cremers, and Peyer (2011) find the opposite result. Mobbs and Raheja (2012) add to these mixed results by

[4] In our sample, the CEO pay gap increases on average by 4% per year over the 1993-2004 period.

[5] The predictions of tournament theory continue to hold even though the pool of contestants for CEO promotion is not restricted to managers inside the firm. In general, non-CEO managers in the firm compete for the CEO position not only with other non-CEO managers inside the firm, but also with outsiders. Tournament theory predicts contestants will compete more strongly if the "prize" for winning the tournament is higher. Whether contestants are from inside the firm or from outside is irrelevant, as long as at least one insider competes in the tournament.

showing that tournament incentives are important for firms where firm-specific human capital is not critical.

This paper tests whether increasing tournament incentives lead to dysfunctional responses, a prediction largely supported analytically and empirically in other contexts (Holmström and Milgrom, 1991; Baker, 1992; Jacob and Levitt, 2003). The specific dysfunctional consequences of employing a tournament may range from excessive risk-taking (e.g., Prendergast, 1999; and Knoeber and Thurman, 1994) and cheating (see Berentsen, 2002; and Cheng, 2011) to sabotage of other competitors (Lazear, 1989).[6] Prior literature has documented that these activities are more likely to occur for higher tournament prizes. While Drago and Garvey (1998) document a decrease in helping effort, Garicano and Palacios-Huerta (2006)[7] and Harbring and Irlenbusch (2011) document greater effort and more sabotage activities for higher tournament prizes.[8]

Cheng (2011)[9] presents analytical and empirical evidence that managers inflate earnings to manipulate how the market learns about and judges their abilities - a situation comparable to that in a rank-order-tournament. Wang and Winton (2012) find that the use of relative performance evaluation in managerial retention decisions can encourage fraud. Experimental findings by Conrads et al. (2014) document a negative association between the level of honesty, as captured by participants' reporting of their own performance, and higher tournament prizes.

Finally, a strand of related literature focuses on tournament incentives and risk-taking. Nalebuff and Stiglitz (1983) and Ehrenberg and Bognanno (1990) argue that tournaments can

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[6] While sabotage aims at diminishing competing managers' performance, cheating aims at increasing one's own performance relative to others' performance.

[7] Garicano and Palacios-Huerta (2006) study an exogenous change in the reward for a win in a soccer game from two to three points, and find more effort and more sabotage activities (as proxied for by the number of defenders and bookings).

[8] See Chowdhury and Gürtler (2013) and Dechenaux, Kovenock, and Sheremeta (2014) for a survey of studies on sabotage behavior in tournaments.

[9] Other analytical papers that illustrate a relationship between tournament incentives and cheating or doping are, for example, Berentsen (2002), Berentsen and Lengwiler (2004) and Kräkel (2007).

potentially alter an individual's adoption of risky strategies. For example, in the mutual fund industry, managers are compensated against the market. Brown, Harlow, and Starks (1996) and Kempf and Ruenzi (2008) find these managers will reallocate their holdings to relatively risky assets depending on their interim positions in the tournament.

In an experimental design, Andersson et al. (2013) document that agents respond strongly to tournament incentives by increasing the principals' risk exposure. This result complements analytical work by Goel and Thakor (2008) who illustrate that the risk level of projects chosen by managers (CEOs and VPs) tends to increase with the promotion prize. Testing this prediction, Kini and Williams (2012) document a significantly positive relationship between firm risk and tournament incentives.

Overall, this literature concludes that tournament incentives can lead to higher risk-taking. Armstrong et al. (2013) document that managers with greater risk-taking incentives are more likely to misreport because they are less averse to the increased equity risk that accompanies misreporting.

In summary, tournaments are based on relative performance evaluation and with increasing tournament incentives, dysfunctional responses, such as manipulating performance, become more likely. Because tournament incentives can also encourage higher risk-taking, managers will be less averse to the increases in equity risk induced by fraudulent behavior. Hence, we predict that:

H: Tournament incentives are positively associated with the likelihood of fraud.

3. Sample selection and research design

3.1 Sample selection

We use data from Dyck, Morse, and Zingales (2010) (hereinafter, DMZ) to determine our fraud firm sample. The DMZ dataset uses the Stanford Securities Class Action Clearinghouse collection to identify fraud cases, and consists of U.S. firms against which a securities class action lawsuit has been filed under the provisions of the Federal 1933/1934 Exchange Act for the 1994-2004 period. After applying several filters to ensure the cases considered are not frivolous, they obtain 216 fraudulent firms.

Untabulated results reveal that the fraud alleged in 75% of these cases is accounting-based. Hence, our study is closely related to prior literature on (accounting) fraud and equity compensation in restatement and enforcement action samples (e.g., Armstrong et al., 2013). In robustness tests presented later, we replicate our findings in other samples, and find our results are unchanged. We exclude multiple fraud occurrences per firm because we are only interested in the initial fraud year, which leaves us with 205 firms. After eliminating firms not covered in ExecuComp, we have 190 firms, and we have sufficient data from Compustat and ExecuComp to employ our research design for 111 firms.

Table 1 outlines our sample selection procedure. We use the beginning of the class action period, the year in which the firm allegedly began engaging in fraudulent behavior, to proxy for the fraud year.

[Table 1 about here]

Table 1 provides further information on our comparison sample. We select all available firm-year observations in ExecuComp not included in our fraud sample. For fraud firms, we delete observations after the fraud year.[10] After further deleting observations with missing values on the control variables or test variables, we obtain 16,052 observations (111 fraud firm-years and 15,941 non-fraud firm-years). These observations relate to 2,309 unique non-fraud firms and 111 fraud firms, indicating a fraud probability, conditional on detection, of 4.6%.

[10] Results are robust to including observations after the fraud year, and to assigning a value of 1 for each fraud year rather than assigning a 1 to the first fraud year only.

Table 2 gives a breakdown of the fraud firms by year and industry.[11] Almost half of our sample observations come from the period 1999-2001 corresponding to the dot-com bubble and subsequent market collapse but results are robust to including year fixed effects and to including a dot-com indicator variable. With respect to industry distribution, we find no obvious clustering by industry.

[Table 2 about here]

3.2 Research design

To examine the association between tournament incentives and the likelihood of fraud, we estimate a probit model following Armstrong et al. (2013).[12] The dependent variable is a binary variable equal to 1 if the firm is classified as a fraud firm in the DMZ database and if it allegedly began fraudulent behavior in the respective year, and 0 otherwise.[13] Following prior literature, all fraud determinants are measured one year prior to the measure of fraud.[14]

We measure the strength of a firm's tournament incentives as the difference between total CEO compensation and total median VP compensation (Kale, Reis, and Venkateswaran, 2009; Kini and Williams, 2012). More specifically,

CEO pay gap = Ln(total CEO compensation

– median value of total VP compensation).

[11] Note that all of our results hold when we exclude financial firms or observations prior to 1996. The Private Securities Litigation Reform Act (PSLRA) of 1995 established several provisions to increase the effectiveness of monitoring by institutional owners.

[12] Following Gow, Ormazabal, and Taylor (2010) and Thompson (2011), we use robust standard errors clustered at the firm and year level. We do note that these papers deal with OLS regression and not maximum likelihood estimation but our inferences are unchanged when using different clustering approaches.

[13] Probit regressions are based on maximum likelihood techniques which assume no serial correlation. Yet clustering is used to take care of non-independence of observations. Under certain conditions, this can lead to artificially low standard errors. We therefore rerun our main model without clustering and by using OLS with two-way clustering. Our results do not alter and we continue to find a positive significant coefficient for the CEO pay gap.

[14] Apart from age and binary variables, we winsorize all of the continuous control variables at the 1st and 99th percentiles.

Our main analysis includes the top five VPs reported in ExecuComp for a given firm-year, but results are robust to the inclusion of all non-CEO executives.

We control for equity incentives of CEOs by including option intensity (Denis, Hanouna, and Sarin, 2006) in our analysis.[15] We also include several other control variables related to the likelihood of fraud and litigation risk (as summarized by Kim and Skinner, 2012). First, to control for market-related incentives and strike suits, we include cumulative stock returns, return skewness, and turnover as controls (Johnson, Kasznik, and Nelson, 2000). We also include return volatility to control for firms operating in uncertain environments (Erickson, Hanlon, and Maydew, 2006). Second, we control for firm size and age by including the natural logarithm of total assets (e.g., Denis, Hanouna, and Sarin, 2006), and the number of years the firm appeared in Compustat (e.g., Armstrong et al., 2013). Third, we include sales growth, because firms with higher growth opportunities have greater financing needs (Wang, 2004). Fourth, we include return on assets and the book to market ratio to control for firm performance, because poorly performing firms may engage in fraud to cover up poor results (Erickson, Hanlon, and Maydew, 2006). Fifth, we include leverage, receivables, inventory, and intangibles (Erickson, Hanlon, and Maydew, 2006; Firth, Rui, and Wu, 2011; Armstrong et al., 2013). Sixth, we control for the need for external financing by including the amount of money raised through stock and debt (Armstrong et al., 2013). Seventh, we control for capital intensity by including net plant, property, and equipment scaled by total assets. Finally, we include an acquisition dummy equal to 1 if more than one-fifth of sales in a year are related to an acquisition, and 0 otherwise. Further details on variable definitions are presented in Appendices 1 and 2.

Our primary estimation compares the sample of fraud firms against all other available non-fraud observations over the same time period. We also follow prior literature and

[15] See Appendix 1 for a complete description of and the formula for our option intensity measure.

compare fraud firms against a sample of non-fraud firms matched by size, industry, and year (e.g., Erickson, Hanlon, and Maydew, 2006).

4. The relationship between tournament incentives and fraud

4.1 Univariate comparison of tournament incentives

Table 3 reports differences in CEO pay gaps for our sample of fraud firms, matched non-fraud firms, and the full sample. In panel A of Table 3, we compare the tournament incentives between fraud firms and all as well as non-fraud firms. In line with our prediction, we find a significant difference between tournament incentives for fraud relative to all as well as matched non-fraud firms. The average tournament incentives, measured as the natural logarithm of the CEO/median VP pay gap, are 8.10 ($3.29 million) for fraud firms and 7.10 ($1.21 million) for all non-fraud firms and 7.70 ($2.21 million) for matched non-fraud firms. This difference is significant at the 1% and 5% level and also represents economically significant stronger tournament incentives for fraud firms.

[Table 3 about here]

Table 3 also compares the values of our control variables over the three samples (fraud firms, non-fraud firms, and matched non-fraud firms). For each variable, Table 3 shows the mean, median, standard deviation, and both upper and lower quartiles.

Comparing fraud and unmatched non-fraud firms reveals that fraud firms, on average, are larger, older, have higher leverage, higher sales growth rates, more receivables, and higher financing needs. Non-fraud firms have higher book to market ratios, return skewness, returns, turnover, return volatility, and more capital expenditures. We find no evidence that option intensity differs among fraud and non-fraud firms, nor is there a difference in return on assets, acquisitions made, intangible assets, or inventory level.

Comparing the matched non-fraud and fraud firms, we only detect a difference for four of the seventeen variables. Fraud firms have a higher sales growth rate, higher financing needs, and a lower book-to-market ratio. We do note that apart from financing, the difference in averages is reduced significantly after matching. Contrary to the full sample, we find that fraud firms have higher option intensity than the matched firms. For these reasons, we include all control variables in our regressions for the matched sample.

4.2 The relationship between tournament incentives and fraud

The univariate results suggest a positive association between tournament incentives and the likelihood of engaging in fraudulent behavior. In this section, we estimate probit models in which our dependent variable is equal to 1 if the company started to engage in fraud, and 0 otherwise. Results are presented in Table 4.

Model 1 shows the results for a comparison of the fraud and non-fraud firms, excluding controls. We find a significantly positive association between the pay gap and the likelihood of fraud (significant at the 1% level).

In order to test our hypothesis, we include controls for size, desire for external financing, performance, return volatility, and other factors in Model 2; as described earlier. We find a significantly positive relationship between the CEO pay gap and the likelihood of fraud at the 1% level.

This result is both statistically and economically significant. The marginal effect of the pay gap is 0.14 percentage points. Accordingly, a 100% increase in the pay gap measure corresponds to a 0.14 percentage point increase in the probability of being fraudulent for the average firm, which is an increase of 37% relative to the mean predicted probability of engaging in fraud in our sample of 0.38%. Put differently, an increase in the pay gap at its mean from 1.2 million by 100% to 2.4 million increases the probability of engaging in fraud in a given year from 0.38% to 0.52%.

With respect to the control variables, we find that sales growth is significantly positively associated with the propensity to engage in fraud in line with Armstrong et al. (2013). We also find that the coefficient on size is significantly positive. This result is consistent with Armstrong et al. (2013), Lennox and Pittman (2010), and Burns and Kedia (2006). Similarly to Lennox and Pittman (2010), we find that book to market is significantly negatively associated with the propensity to engage in fraud. Finally, we find that the coefficient on financing is significantly positive, which is in line with Erickson, Hanlon, and Maydew (2006) and Lennox and Pittman (2010). Moreover firms with lower return skewness are more likely to engage in fraud. For the other determinants, we find these are not significantly different from zero, which is overall consistent with the previous literature.

In Model 3, we compare the fraud firm sample against a sample of non-fraud firms matched by industry, size, and year. Our results hold: We find a significantly positive association between the CEO pay gap and the likelihood of fraud (significant at the 5% level). Further, we note that none of the control variables is significant, confirming that our matching performs reasonably well in reducing observable differences between fraud and non-fraud firms.

[Table 4 about here]

5. Robustness tests

In this section, we test the robustness of our primary findings to alternative research designs, alternative measures of the CEO pay gap, non-CEO skills, alternative measures for equity-based incentives, and alternative samples.

5.1 Alternative research design

Table 5 follows different research designs proposed by prior literature to test the robustness of our results. Model 1 controls for unobservable firm characteristics, and shows the results of a random effects probit regression (Lennox and Pittman, 2010). Again, we find a significantly positive coefficient of the pay gap (1% level). Model 2 is a logistic regression following Erickson, Hanlon, and Maydew (2006) that includes industry fixed effects to control for industry-related effects.[16] Our results continue to hold (1% level) after controlling for industry. Model 3 checks the robustness of our results against different types of matching. In this model, we use Lennox and Pittman's (2010) propensity score matching approach to control for differences between fraud and non-fraud firms in the year prior to engaging in fraud. To obtain a matched sample, we first estimate a fraud prediction model where the dependent variable indicates whether the company is about to commit a fraud, excluding our experimental variable (CEO pay gap). We perform a nearest neighbor propensity score match, and explore the influence of promotion-based tournament incentives on the likelihood of fraud across otherwise identical firms with equal probabilities of engaging in fraud. Our results again hold, and we find a significantly positive coefficient (10% level).

All subsequent analyses are robust to the different research designs outlined above (Model 1-3), unless stated otherwise. Overall, these results render it less likely that the pay gap is capturing some omitted firm-specific characteristic that can explain fraud in the cross-section.

[Table 5 about here]

5.2 Pay gap as a measure of tournament incentives

[16] Results are robust to using the penalized likelihood method to reduce any small-sample bias in maximum likelihood estimation (Firth, 1993).

Table 6 uses alternative measures for tournament incentives and controls for VP skills since differences in pay may be related to differences in VP ability (Masulis and Zhang, 2012). Our use of the median VP pay could overestimate tournament incentives if only one or two VPs have significantly higher pay and higher chances of obtaining promotions than the remaining VPs (Masulis and Zhang, 2012). To rule out this measurement error in our tournament incentive proxy, we use the natural logarithm of the difference in pay between the CEO and the mean VP instead. We also use the difference between CEO pay and the highest paid VP.

Our results remain unchanged as we continue to find significant results for the gap between the CEO and the highest paid VP (at the 1% level in the full (Model 1a) and at the 5% level in the matched (Model 2a) model), and for the mean pay gap (at the 5% level in both the full (Model 1b) and matched (Model 2b) model).

Finally, we control for non-CEO skills in Table 6, following Masulis and Zhang (2012). We include average team age, average team salary growth (%), and average team salary growth. The results in Table 6 show that the pay gap remains significantly positive at the 1% level for the full Models 1c and 1d, and at the 5% level for the remaining models when we include these additional control variables.

[Table 6 about here]

5.3. Alternative measures for equity-based incentives

In Table 7, we proxy for equity-based compensation by using the sensitivity of the CEO's (VP's) equity portfolio to changes in stock price (delta) and equity risk (vega). Specifically, the CEO (VP) delta is the natural logarithm of 1 plus the CEO portfolio delta (the average VP portfolio delta). Similarly, the CEO (VP) vega is the natural logarithm of 1 plus the CEO portfolio vega (the average VP portfolio vega).

Following Core and Guay (2002), we calculate a portfolio delta (vega) as the dollar change in a VP's equity portfolio for a 1% change in stock price (firm stock return volatility). Given the low number of observations for which we have CFO compensation data, we only estimate the full model when additionally controlling for CFO delta and vega. We further note that untabulated results reveal no significant differences for the interaction between the CEO pay gap and our measures of equity-based compensation and, hence, do not report these in Table 7.

In Models 1a and 2a, we include both the CEO delta (vega) and the VP delta (vega), with largely unchanged inferences. We find that our results hold for the full sample (1% level) and are marginally significant for the matched sample (p-value of 0.13). In Model 1b, we include both the CEO delta (vega) and the CFO delta (vega). Due to missing annual titles, we only retain fifty-three fraud firms with CFO data. However, our results remain robust (at the 5% level). Overall, we do not find strong evidence that our results are driven by CEO or CFO equity-based incentives.

[Table 7 about here]

5.4. Corporate governance and CEO power

Our measure of tournament incentives, the CEO pay gap, will likely be related to corporate governance mechanisms at the firm level, and corporate governance mechanisms themselves may be related to corporate fraud (e.g. Chen et al., 2006; Lo, Wong, and Firth, 2010). In order to account for this potentially correlated omitted variable, we include several control variables related to corporate governance quality in Table 8.

First, we include the G-index following Gompers, Ishii, and Metrick (2003). Second, we include a binary variable to control for a classified board, which is equal to 1 if the directors are elected to staggered rather than annual terms, and 0 otherwise. Third, we control for board independence by including the number of independent directors over the total number of board members. These variables are obtained from Risk Metrics. We exclude firms with missing values on the governance variable, which further reduces our sample to sixty-eight fraud firms.

Table 8 presents the results. Our result of a positive association of tournament incentives with the likelihood of engaging in fraudulent behavior is robust to controlling for corporate governance measures in the full sample (Model 1a). We note that none of the corporate governance measures are significant. Given the low number of observations for which corporate governance data is available in our sample, we have low power in the matched sample and fail to find significant results (Model 2a). As an alternative to our matching approach based on size, industry, and year, we estimate Model 1a for the full sample including year and industry fixed effects and controlling for size to overcome this issue. Untabulated results of this alternative approach confirm a significantly positive coefficient of the pay gap (at the 1% level).

Bebchuk, Cremers, and Peyer (2011) argue that the pay gap captures CEO power or importance. Moreover, VPs may engage in fraud because they are succumbing to pressure from the CEO, not because they are seeking immediate personal financial benefits from their equity incentives (Feng et al., 2011).

We investigate this alternative explanation by including two CEO power measures: CEO-chairman duality and founder status (Feng et al., 2011). Duality is a binary variable that takes the value of 1 if the CEO is also the chairman of the board; founder is a binary variable that takes the value of 1 if the CEO is also the founder of the company. In Model 1b, we again find a significantly positive coefficient for the pay gap (5% level). These results continue to hold in the matched sample (5% level). Of note, we fail to find any association between founder status and duality with the likelihood of engaging in fraudulent behavior (in line with Feng et al., 2011 and Kryzanowski and Zhang, 2013).[17]

[Table 8 about here]

5.5. Alternative fraud samples

Karpoff et al. (2014) explore the advantages and disadvantages of the various databases used in fraud research. To test the robustness of our results, we examine whether the CEO pay gap is positively associated with the propensity to engage in fraudulent behavior in two alternative samples.

First, we use SEC Accounting and Auditing Enforcement Releases (AAERs), which are issued by the SEC during or at the conclusion of an investigation for alleged accounting and/or auditing misconduct. Hence, we set our dependent variable equal to 1 if the SEC published an AAER for the firm during the year, and 0 otherwise (Dechow et al., 2011).

Second, we use the likelihood of manipulation (F-score), obtained from Dechow et al. (2011). Our dependent variable takes a value of 0 if the F-score for a given observation is lower than 1 (indicating normal or low risk), a value of 1 if the F-score is greater than 1 but below 1.85 (indicating an above normal risk), a value of 2 if the F-score is greater than1.85 but below 2.45 (indicating a substantial risk), and a value of 3 for an F-score greater than 2.45 (indicating a high risk) (Dechow et al., 2011; Jia, van Lent, and Zeng, 2014).

Our results with unchanged inferences are presented in Table 9. We find a significantly positive coefficient for both the F-score in the full sample (Model 1a), and for

[17] Feng et al. (2011) also include the CEO pay slice. Because this variable captures both tournament incentives and CEO power (CEO pay gap and slice are strongly correlated), we restrict our tabulated results to pure power measures (duality and founder status). However, our results are robust to including both the CEO pay slice and the CEO pay gap. We note that variance inflation factors are at conventionally low levels when including both.

the AAERs in the full (Model 1b) and matched sample (Model 2b) at the 1 and 5% level, respectively.

[Table 9 about here]

6. Summary and implications

Prior literature focused primarily on whether options and equity compensation provide incentives to commit fraud. This is of particular interest in the wake of the accounting scandals over the past decade. Given that fraud cases frequently involve non-CEO VPs and that prior literature documents these VPs respond to both equity-based and tournament incentives, we investigate a previously overlooked source of incentives potentially contributing to explain fraudulent behavior: tournament incentives.

Our results indicate that firms with larger tournament incentives, e.g., larger gaps between CEO and median VP pay, are more likely to engage in fraudulent activities. Therefore, similarly to equity-based compensation, the predicted alignment between shareholder and VP interests is not fully achieved. This result is robust to other research designs, other fraud samples, different measures of the pay gap, different measures of equity-based compensation, and controlling for corporate governance and CEO power

Our results are illustrative only for one aspect of tournament incentives, fraud, but they are consistent with other dysfunctional consequences of tournament incentives, such as sabotage and cheating, documented by prior literature. Other papers, however, have documented that tournament incentives can also lead to better cooperation and higher firm value (Kale, Reis, and Venkateswaran, 2009), and they can influence risk-taking (Kini and Williams, 2012). Hence, our results do not indicate whether the costs of tournament incentives generally outweigh the benefits, and they should be viewed as an indication that both equity-based and tournament incentives need to be considered (rather than being viewed strictly as evidence of the need to restrict CEO pay).

Our results have direct implications for the public debate on the structure of top executive compensation. Our findings can provide boards with an additional factor to take into account when assessing compensation programs and their incentive alignments. When large tournament incentives are present, corporate governance mechanisms aimed at monitoring could mitigate fraud by increasing the likelihood of detection. Accordingly, studying how the interaction of tournament incentives and monitoring mechanisms is associated with fraud is an interesting avenue for future research. Moreover, our study adds an extra dimension to the debate over outsize CEO compensation. While previous research focused on the effects of equity compensation with respect to fraud and the impact of tournament incentives on firm value, our findings indicate a dark side of tournament incentives. Finally, our findings have implications for the literature on fraud determinants by demonstrating the importance of the entire management team's pay, not just the CEO's pay.

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Table 1
Sample selection: This table summarizes our sample selection procedure. Our sample consists of 111 fraud firms where compensation data on ExecuComp were available. The initial sample consisted of 216 firms. The ExecuComp universe over the 1993-2003 period consists of 20,835 firm-years.

Fraud firms		
Cases in DMZ (2010)		216
Multiple occurrences in the same year	-11	205
Firm not in ExecuComp	-15	190
Missing control variables	-79	111
Non-fraud firms		
ExecuComp universe		20835
Missing ExecuComp data	-4894	15941
Full sample		16052

Table 2

Sample distribution by year and industry: Frequency of fraud by year and industry. Our sample consists of 111 fraud firms where compensation data on ExecuComp was available.

Year	% of fraud firms
1994	2.70%
1995	2.70%
1996	5.41%
1997	16.22%
1998	12.61%
1999	19.82%
2000	16.22%
2001	13.51%
2002	8.11%
2003	1.80%
2004	0.90%
	100%

Fama-French industry codes	% of fraud firms
Food products	2.70%
Recreation	0.90%
Apparel	0.90%
Healthcare	3.60%
Medical equipment	0.90%
Pharmaceutical products	5.41%
Chemicals	0.90%
Textiles	0.90%
Construction	1.80%
Steel works	0.90%
Machinery	1.80%
Electrical equipment	0.90%
Aircraft	1.80%
Defense	0.90%
Precious metals	0.90%
Industrial metal mining	0.90%
Petroleum and natural gas	1.80%
Utilities	9.91%
Communication	4.50%
Personal services	2.70%
Business services	9.91%
Computers	6.31%
Electronic equipment	4.50%
Measuring and control equipment	0.90%
Transportation	2.70%
Wholesale	5.41%
Retail	8.11%
Banking	9.01%
Insurance	6.31%
Other	1.80%
	100%

Table 3
Univariate comparison of the CEO pay gap and control variables among samples: Mean, median, standard deviation, upper and lower quartile for firm and compensation characteristics for the total sample, fraud and non-fraud firms. Our sample consists of 111 fraud firms that had compensation data on ExecuComp available. The non-fraud sample consists of the ExecuComp universe (fraud firms excluded). The matched sample consists of 111 non-fraud firms matched by size, industry, and year. Variables in dollar amounts are in thousands, except for total assets, which is in millions. All variables are winsorized at the 1% and 99% levels over the distribution of the full sample plus fraud firms, except for variables truncated at zero, which are winsorized at the 99% level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively. Variables are as defined in Appendices 1 and 2.

Panel A

	N	Mean	t-test	Q1	Median	Q3	Std. Dev.
Fraud firms	111	8.10		7.31	8.05	9.02	1.29
Non-fraud firm years	15941	7.10	-8.09***	6.26	7.07	7.96	1.29
Matched non-fraud firms	111	7.70	-2.369**	6.86	7.77	8.37	1.18
Total sample	16052	7.10		6.26	7.08	7.96	1.29

Panel B

Variable	N	Mean	Q1	Median	Q3	Std Dev	Mean fraud firms	Mean non-fraud firms	t-test	Mean matched non-fraud firms	t-test
Option intensity	16052	22.12	6.75	16.10	31.87	19.85	18.29	21.26	1.60	14.34	1.93*
Sales growth	16052	0.14	0.002	0.09	0.21	0.29	0.26	0.13	3.79***	0.17	2.32**
Volatility	16052	0.12	0.07	0.10	0.15	0.07	0.11	0.12	1.80*	0.11	0.60
Receivables	16052	0.18	0.08	0.15	0.24	0.16	0.22	0.18	2.14**	0.19	1.22
Intangibles	16052	0.05	0	0.01	0.05	0.12	0.04	0.05	1.56	0.04	0.00
Size	16052	7.35	6.07	7.17	8.48	1.72	8.65	7.34	8.00***	8.56	0.41
Book to market	16052	0.51	0.27	0.43	0.65	0.37	0.36	0.51	5.86***	0.44	2.11**
Leverage	16052	0.23	0.08	0.23	0.35	0.18	0.28	0.23	2.47***	0.24	1.49
Return on assets	16052	0.03	0.01	0.04	0.08	0.10	0.04	0.03	1.14	0.04	0.07
Capital	16052	0.30	0.11	0.25	0.46	0.24	0.27	0.31	1.72*	0.27	0.07
Financing	16052	0.10	0.01	0.04	0.12	0.16	0.13	0.11	1.94*	0.09	2.03**
Acquisition	16052	0.04	0	0	0	0.20	0.08	0.04	1.46	0.06	0.52
Age	16052	23.97	9.00	21.00	38.00	15.58	27.11	23.95	1.96*	29.32	1.01
Inventory	16052	0.11	0.01	0.08	0.17	0.13	0.10	0.11	1.40	0.10	0.32
Turnover	16052	1.49	0.61	1.00	1.78	1.42	1.29	1.49	1.94*	1.25	0.30
Skewness	16052	0.002	-0.003	0.001	0.001	0.01	-0.0003	0.002	2.48**	0.0007	0.89
Return	16052	-0.01	-0.11	-0.02	0.08	0.18	-0.04	-0.01	1.97*	-0.03	0.73

Table 4

Tournament incentives and corporate fraud. We perform a probit regression with a dummy variable equal to 1 for the first year the firm allegedly engaged in a fraud, and 0 otherwise. For Models 1 and 2, our sample consists of 111 fraud firms and all non-fraud firms in the ExecuComp universe. Standard errors are clustered by firm and year. For Model 3, our sample consists of 111 fraud firms and 111 non-fraud firms matched by size, industry, and year. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively. Robust standard errors are reported in parentheses below the coefficients. Variables are as defined in Appendices 1 and 2.

	Model 1	Model 2	Model 3
CEO pay gap	0.2211***	0.1238***	0.1962**
	(0.0464)	(0.0442)	(0.0929)
Option intensity		0.0018	0.0079
		(0.0021)	(0.0065)
Sales growth		0.3876***	0.4100
		(0.1008)	(0.3336)
Volatility		0.2810	-2.7337
		(1.0624)	(2.2699)
Receivables		0.1551	0.8619
		(0.2115)	(0.6514)
Intangibles		-0.4220	-1.0163
		(0.8013)	(1.3835)
Size		0.1158***	-0.0067
		(0.0307)	(0.0890)
Book to market		-0.5093***	-0.2804
		(0.1621)	(0.3174)
Leverage		-0.0008	0.3385
		(0.2174)	(0.5817)
Return on assets		-0.2544	-0.5588
		(0.6606)	(1.3115)
Capital		-0.0122	0.3738
		(0.1237)	(0.5447)
Financing		0.4948***	1.0591
		(0.1409)	(0.7195)
Acquisition		-0.0245	0.0597
		(0.2118)	(0.3785)
Age		0.0004	-0.0024
		(0.0019)	(0.0066)
Inventory		0.1734	0.5276
		(0.2072)	(0.7817)
Turnover		-0.0657	0.0246
		(0.0556)	(0.1095)
Skewness		-9.6284**	-6.3868
		(4.8217)	(11.6179)
Return		-0.2753	-0.1989
		(0.1931)	(0.5794)
Constant	-4.1302***	-4.2303***	-1.7384*
	(0.3191)	(0.3695)	(0.9117)
N	16052	16052	222
Log-Likelihood	-629.9650	-597.2417	-141.4497

Table 5

Robustness tests: Alternative research designs. We perform a probit regression with a dummy variable equal to 1 for the first year the firm allegedly engaged in a fraud, and 0 otherwise. For Models 1 and 2, our sample consists of 111 fraud firms and all non-fraud firms in the ExecuComp universe. Model 1 is a probit regression including random effects (Lennox and Pittman, 2010). Model 2 is a logistic regression including industry fixed effects (Erickson, Hanlon, and Maydew, 2006). For Model 3, our sample consists of 111 fraud firms and 111 non-fraud firms matched by propensity score of being accused of fraud (Lennox and Pittman, 2010). Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively. Standard errors are reported in parentheses below the coefficients. Variables are as defined in Appendices 1 and 2.

	Model 1	Model 2	Model 3
CEO pay gap	0.1238***	0.2892***	0.1771*
	(0.0394)	(0.1072)	(0.0947)
Option intensity	0.0018	0.0091	-0.0021
	(0.0025)	(0.0071)	(0.0065)
Sales growth	0.3876***	0.9021***	0.3827
	(0.1185)	(0.3038)	(0.3248)
Volatility	0.2810	-0.2888	-2.9658
	(0.8395)	(2.3368)	(2.2505)
Receivables	0.1551	2.2633**	-0.0525
	(0.2577)	(0.9379)	(0.6530)
Intangibles	-0.4220	-1.2779	0.3135
	(0.4924)	(1.7671)	(1.5148)
Size	0.1158***	0.4422***	-0.0621
	(0.0349)	(0.1059)	(0.0889)
Book to market	-0.5093***	-1.6777***	0.2957
	(0.1515)	(0.4627)	(0.4003)
Leverage	-0.0008	0.5070	0.6875
	(0.2359)	(0.6947)	(0.6121)
Return on assets	-0.2544	-0.6609	-1.3521
	(0.5239)	(1.4309)	(1.6294)
Capital	-0.0122	0.0205	-0.2789
	(0.2096)	(0.8100)	(0.5934)
Financing	0.4948**	0.9179	0.3712
	(0.2302)	(0.6326)	(0.6722)
Acquisition	-0.0245	-0.0113	-0.4906
	(0.1582)	(0.4071)	(0.3282)
Age	0.0004	-0.0060	0.0000
	(0.0027)	(0.0085)	(0.0067)
Inventory	0.1734	-0.0578	-0.0557
	(0.3252)	(1.2676)	(0.7361)
Turnover	-0.0657	-0.2264*	0.0111
	(0.0412)	(0.1184)	(0.1034)
Skewness	-9.6283*	-26.7660*	3.1273
	(5.0400)	(13.7753)	(11.9736)
Return	-0.2753	-0.9716	-0.1795
	(0.2311)	(0.6359)	(0.5493)
Constant	-4.2303***	-13.0568***	-0.7670
	(0.3471)	(1.2842)	(0.9380)
N	16052	16052	222
Log-Likelihood	-597.2417	-561.2964	-148.1308

Table 6
Robustness tests: Pay gap as a measure of tournament incentives. We perform a probit regression with a dummy variable equal to 1 for the first year the firm allegedly engaged in a fraud and 0 otherwise. For Model 1a, our sample consists of 90 fraud firms and all non-fraud firms in the ExecuComp universe. For Model 1b, our sample consists of 104 fraud firms and all non-fraud firms in the ExecuComp universe. For Models 1c-e, our sample consists of 106 fraud firms and all non-fraud firms in the ExecuComp universe. Robust standard errors are clustered by firm and by year. For Model 2a, our sample consists of 90 fraud firms and 90 non-fraud firms matched by size, industry, and year. For Model 2b, our sample consists of 104 fraud firms and 104 non-fraud firms matched by size, industry, and year. For Models 2c-e, our sample consists of 106 fraud firms and 106 non-fraud firms matched by size, industry, and year. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively. Standard errors are reported in parentheses below the coefficients. Variables are as defined in Appendices 1 and 2.

	Model 1a	Model 1b	Model 1c	Model 1d	Model 1e	Model 2a	Model 2b	Model 2c	Model 2d	Model 2e
CEO pay gap			0.1308***	0.1341***	0.1068**			0.2468**	0.2204**	0.2109**
			(0.0480)	(0.0470)	(0.0477)			(0.0976)	(0.1100)	(0.1028)
CEO pay gap (max VP)	0.0936***					0.1979**				
	(0.0292)					(0.0791)				
CEO pay gap (mean VP)		0.1062**					0.1759**			
		(0.0520)					(0.0794)			
Average age			-0.0293***					-0.0543**		
			(0.0054)					(0.0229)		
Pay growth (%)				0.0000					0.0000	
				(0.0000)					(0.0000)	
Pay growth					0.0000***					0.0006
					(0.0000)					(0.0007)
Option intensity	0.0033	0.0034	0.0021	0.0024	0.0024	0.0098	0.0148**	0.0095	0.0101	0.0105
	(0.0025)	(0.0025)	(0.0021)	(0.0022)	(0.0021)	(0.0064)	(0.0070)	(0.0067)	(0.0066)	(0.0066)
Sales growth	0.3877***	0.3281***	0.3456***	0.3724***	0.3416***	0.7140	0.4221	0.3559	0.2826	0.2608
	(0.0863)	(0.0738)	(0.1020)	(0.0919)	(0.0793)	(0.4506)	(0.3844)	(0.3606)	(0.3834)	(0.3770)
Volatility	1.0898	0.3877	-0.2585	-0.1210	-0.3180	-2.8159	-3.9032	-2.6023	-2.8746	-2.9842
	(1.0928)	(1.0655)	(1.0944)	(1.0858)	(1.0675)	(2.4254)	(2.3747)	(2.3773)	(2.3581)	(2.3835)
Receivables	0.0567	0.2164	0.1872	0.1544	0.1581	0.7473	1.0725	0.8823	0.9086	0.9838
	(0.2789)	(0.2205)	(0.1991)	(0.2034)	(0.1969)	(0.7207)	(0.6754)	(0.6717)	(0.6703)	(0.6772)
Intangibles	-1.3956	-0.3201	-0.3159	-0.3342	-0.3367	-2.1446	-0.0271	-1.1719	-1.0120	-0.9190
	(0.9738)	(0.8404)	(0.8324)	(0.8373)	(0.8497)	(2.3987)	(1.5980)	(1.4246)	(1.4083)	(1.4052)

Table 6 (continued)

Size	0.1374***	0.1379***	0.1203***	0.1060***	0.1051***	-0.0078	0.0363	0.0309	-0.0054	-0.0003
	(0.0341)	(0.0414)	(0.0320)	(0.0323)	(0.0325)	(0.0983)	(0.0861)	(0.0944)	(0.0922)	(0.0924)
Book to market	-0.5452***	-0.4992***	-0.4997***	-0.4784***	-0.4707***	-0.3623	-0.2325	-0.3878	-0.2222	-0.2147
	(0.1957)	(0.1845)	(0.1521)	(0.1558)	(0.1563)	(0.3220)	(0.3187)	(0.3339)	(0.3215)	(0.3234)
Leverage	-0.2201	-0.0609	-0.0321	0.0218	0.0368	0.0821	0.0724	0.2485	0.3384	0.3804
	(0.2086)	(0.2178)	(0.2215)	(0.2284)	(0.2303)	(0.6685)	(0.6028)	(0.6046)	(0.5995)	(0.6048)
Return on assets	-0.0050	-0.1802	-0.2871	-0.3157	-0.3615	-0.4197	-0.2844	-0.5923	-0.6090	-0.4885
	(0.6576)	(0.5973)	(0.6373)	(0.6388)	(0.6358)	(1.4810)	(1.4214)	(1.3522)	(1.3540)	(1.3643)
Capital	-0.1068	0.0228	0.0114	-0.0269	-0.0214	0.0008	0.4270	0.5273	0.4888	0.5376
	(0.1615)	(0.1502)	(0.1317)	(0.1351)	(0.1354)	(0.6160)	(0.5625)	(0.5642)	(0.5579)	(0.5625)
Financing	0.4439***	0.4473***	0.4337**	0.4423**	0.4428**	0.5111	0.8204	0.8013	0.8439	0.7205
	(0.1625)	(0.1251)	(0.1775)	(0.1754)	(0.1794)	(0.7822)	(0.7678)	(0.7797)	(0.7738)	(0.8098)
Acquisition	-0.0134	0.0651	0.0205	0.0065	0.0001	-0.4061	0.0539	0.2101	0.2101	0.2577
	(0.2000)	(0.2009)	(0.2107)	(0.2133)	(0.2170)	(0.4187)	(0.3890)	(0.3987)	(0.3985)	(0.4004)
Age	0.0015	0.0020	0.0014	0.0004	0.0005	-0.0002	-0.0002	-0.0015	-0.0032	-0.0035
	(0.0028)	(0.0022)	(0.0020)	(0.0021)	(0.0021)	(0.0075)	(0.0069)	(0.0069)	(0.0069)	(0.0069)
Inventory	0.4249**	0.2789	0.2626	0.1914	0.2117	0.5376	0.7263	0.4941	0.5404	0.6397
	(0.1987)	(0.1868)	(0.1932)	(0.2015)	(0.1997)	(0.8455)	(0.8242)	(0.8075)	(0.8022)	(0.8151)
Turnover	-0.0703	-0.0618	-0.0792	-0.0663	-0.0642	0.0188	-0.0064	-0.0123	0.0439	0.0353
	(0.0631)	(0.0627)	(0.0604)	(0.0590)	(0.0578)	(0.1201)	(0.1149)	(0.1235)	(0.1209)	(0.1218)
Skewness	-10.0532*	-8.4421*	-7.6870*	-7.5325*	-7.0625	-0.1250	6.4229	-8.7884	-5.2146	-6.4001
	(5.7495)	(4.9250)	(4.5070)	(4.3753)	(4.5724)	(13.4487)	(12.4429)	(12.4673)	(12.2279)	(12.2696)
Return	-0.3164	-0.4074*	-0.2306	-0.2260	-0.2598	-0.0955	-0.6252	0.1156	-0.0611	-0.1241
	(0.2183)	(0.2236)	(0.1936)	(0.1885)	(0.1904)	(0.6388)	(0.6071)	(0.6195)	(0.6161)	(0.6407)
Constant	-4.1842***	-4.4057***	-2.8119***	-4.1993***	-3.9878***	-1.4537	-2.0082**	0.3212	-1.9997**	-2.0187**
	(0.3673)	(0.3656)	(0.4664)	(0.4066)	(0.4096)	(0.9706)	(0.9654)	1.3805	(0.9664)	(0.9292)
N	13473	15624	14881	14881	14881	180	208	212	212	212
Log-Likelihood	-487.0062	-561.8550	-564.2927	-569.4333	-566.2362	-112.8513	-130.5900	-130.8637	-133.6445	-133.2810

Table 7
Robustness tests: Alternative measures for equity-based incentives. We perform a probit regression with a dummy variable equal to 1 for the first year the firm allegedly engaged in a fraud, and 0 otherwise. For Model 1a, our sample consists of 104 fraud firms and all non-fraud firms in the ExecuComp universe. Standard errors are clustered by firm and by year. For Model 1b, our sample consists of 53 fraud firms and all non-fraud firms in the ExecuComp universe. Robust standard errors are clustered by firm and by year. For Model 2a, our sample consists of 98 fraud firms and 98 non-fraud firms matched by size, industry, and, year. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively. Standard errors are reported in parentheses below the coefficients. Variables are as defined in Appendices 1 and 2.

	Model 1a	Model 2a	Model 1b
CEO pay gap	0.1101***	0.1647	0.0927**
	(0.0415)	(0.1091)	(0.0394)
Delta CEO	0.0117	0.2202	-0.0145
	(0.0449)	(0.1541)	(0.0445)
Vega CEO	-0.0272	0.0048	0.2422***
	(0.0765)	(0.1511)	(0.0822)
Delta VPs	0.0505	-0.2410	
	(0.0784)	(0.2442)	
Vega VPs	0.0873	0.1467	
	(0.1064)	(0.2277)	
Delta CFO			0.1662***
			(0.0310)
Vega CFO			-0.1141
			(0.1064)
Sales growth	0.3287***	0.6306	0.3336*
	(0.0928)	(0.4293)	(0.1710)
Volatility	0.3995	-2.4083	-0.2255
	(1.1799)	(2.4468)	(1.6865)
Receivables	0.3543*	0.7081	0.3655**
	(0.1969)	(0.7037)	(0.1835)
Intangibles	-0.5898	-1.4930	0.4316
	(0.8225)	(1.4237)	(0.8651)
Size	0.0532	-0.0869	0.0340
	(0.0371)	(0.0999)	(0.0419)
Book to market	-0.4127*	-0.1541	-0.4482
	(0.2177)	(0.3735)	(0.3166)
Leverage	0.0702	0.3752	0.7399***
	(0.2743)	(0.6332)	(0.2262)
Return on assets	-0.6270	-1.2020	-0.2321
	(0.6581)	(1.4068)	(0.4073)
Capital	0.0464	-0.0182	0.1122
	(0.1330)	(0.5759)	(0.2342)
Financing	0.4884***	1.4864*	0.1203
	(0.1547)	(0.8174)	(0.3900)
Acquisition	0.0219	-0.0224	-0.2307
	(0.2021)	(0.4085)	(0.2300)
Age	0.0007	0.0002	0.0006
	(0.0025)	(0.0074)	(0.0023)
Inventory	0.1411	0.2777	0.4304
	(0.2417)	(0.8661)	(0.4231)

Table 7 (continued)

Turnover	-0.0885	-0.0707	-0.1208***
	(0.0543)	(0.1438)	(0.0461)
Skewness	-10.5061*	-10.7458	-12.8446**
	(5.4968)	(12.6296)	(5.3574)
Return	-0.2513	-0.3584	-0.5278
	(0.2301)	(0.6248)	(0.4083)
Constant	-4.1145***	-1.2829	-4.6396***
	(0.3999)	(1.0226)	(0.5167)
N	15089	196	5386
Log-Likelihood	-557.1443	-122.3508	-245.6425

Table 8
Robustness tests: Corporate governance and CEO power. We perform a probit regression with a dummy variable equal to 1 for the first year the firm allegedly engaged in a fraud, and 0 otherwise. For Model 1a, our sample consists of 68 fraud firms and all non-fraud firms in the ExecuComp universe. Standard errors are clustered by firm and by year. For Model 2a, our sample consists of 68 fraud firms and 68 non-fraud firms matched by size, industry and year. For Models 1b and 1c, our sample consists of 110 fraud firms and all non-fraud firms in the ExecuComp universe. For Model 1d, our sample consists of 111 fraud firms and all non-fraud firms in the ExecuComp universe. Robust standard errors are clustered by firm and by year. For Model 2b, our sample consists of 110 fraud firms and 110 non-fraud firms matched by size, industry, and year. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively. Standard errors are reported in parentheses below the coefficients. Variables are as defined in Appendices 1 and 2.

	Model 1a	Model 2a	Model 1b	Model 1c	Model 1d	Model 2b
CEO pay gap	0.1367**	0.1662	0.1171**	0.1159**	0.1259***	0.2251**
	(0.0569)	(0.1402)	(0.0469)	(0.0473)	(0.0430)	(0.0952)
Independence	0.2873	1.0212				
	(0.2077)	(0.8010)				
Classified board	0.0058	0.1777				
	(0.0775)	(0.2862)				
G index	0.0047	-0.0860				
	(0.0272)	(0.0616)				
Founder			0.0599		0.0420	0.1319
			(0.1333)		(0.1255)	(0.2579)
Duality			0.0972	0.0916		-0.2496
			(0.3928)	(0.3963)		(0.2175)
Option intensity	0.0040	0.0135	0.0019	0.0020	0.0014	0.0088
	(0.0029)	(0.0099)	(0.0024)	(0.0024)	(0.0023)	(0.0065)
Sales growth	0.5535***	0.6278	0.3735***	0.3733***	0.3878***	0.3415
	(0.1553)	(0.4391)	(0.0969)	(0.0977)	(0.1001)	(0.3394)
Volatility	0.7719	-4.9743	0.6561	0.6529	0.2980	-3.1220
	(1.4944)	(3.0870)	(1.1460)	(1.1521)	(1.0503)	(2.3002)
Receivables	0.2965	0.7097	0.2099	0.2073	0.1544	0.8205
	(0.3280)	(0.8509)	(0.2218)	(0.2200)	(0.2117)	(0.6539)
Intangibles	0.1944	0.1235	-0.6507	-0.6456	-0.4284	-1.1472
	(1.0430)	(2.3075)	(0.8039)	(0.8036)	(0.7995)	(1.4063)
Size	0.1453***	0.0255	0.1161***	0.1178***	0.1123***	-0.0059
	(0.0288)	(0.1350)	(0.0331)	(0.0327)	(0.0310)	(0.0894)
Book to market	-0.6738***	-0.5346	-0.6903***	-0.6930***	-0.5077***	-0.3107
	(0.1775)	(0.4714)	(0.2190)	(0.2182)	(0.1623)	(0.3227)
Leverage	0.0987	-0.1837	0.0021	-0.0023	0.0092	0.4239
	(0.3483)	(0.8365)	(0.2914)	(0.2891)	(0.2223)	(0.5883)
Return on assets	-0.3925	-0.6833	-0.7211	-0.7006	-0.2714	-0.6388
	(1.0706)	(1.6340)	(0.6905)	(0.7051)	(0.6419)	(1.3243)
Capital	0.1816	0.5703	0.0242	0.0261	-0.0197	0.4668
	(0.2525)	(0.7821)	(0.1265)	(0.1268)	(0.1230)	(0.5499)
Financing	0.2439	1.5536	0.5337***	0.5313***	0.4972***	0.9751
	(0.2591)	(1.1809)	(0.1977)	(0.1996)	(0.1402)	(0.7295)
Acquisition	-0.2524	-0.4434	-0.0250	-0.0234	-0.0262	0.1024
	(0.2350)	(0.6668)	(0.2165)	(0.2212)	(0.2074)	(0.3817)
Age	0.0010	-0.0025	0.0007	0.0000	0.0007	-0.0008
	(0.0024)	(0.0104)	(0.0030)	(0.0024)	(0.0023)	(0.0074)
Inventory	0.7368**	0.9322	0.2199	0.2231	0.1668	0.5354
	(0.3202)	(1.1424)	(0.2479)	(0.2520)	(0.2057)	(0.8112)

Table 8 (continued)

Turnover	-0.0585	0.0854	-0.0747	-0.0743	-0.0658	0.0653
	(0.0719)	(0.1634)	(0.0601)	(0.0607)	(0.0550)	(0.1151)
Skewness	-17.3170**	-16.6464	-11.0720*	-11.0195*	-9.6376**	-9.9718
	(8.3493)	(15.1777)	(5.7032)	(5.7301)	(4.7853)	(13.1107)
Return	-0.3251	-0.1586	-0.2696	-0.2693	-0.2761	-0.0371
	(0.2806)	(0.7766)	(0.2208)	(0.2207)	(0.1931)	(0.6298)
Constant	-5.1945***	-1.5967	-4.1456***	-4.1220***	-4.2296***	-4.3890***
	(0.6619)	(1.6314)	(0.3808)	(0.3874)	(0.3572)	(1.0880)
N	8210	138	14627	14627	16051	220
Log-Likelihood	-343.8786	-83.4185	-552.2309	-552.4092	-597.2297	-139.0648

Table 9

Robustness tests: Alternative fraud samples. We perform a probit regression with a dummy variable equal to 1 for the first year the firm engaged in a fraud, and 0 otherwise. For Model 1a, we use the F-score developed by Dechow et al. (2011). For Model 1b, our sample consists of 351 AAER firms and all non-fraud firms in the ExecuComp universe. Robust standard errors are clustered by firm and by year. For Model 2b, our sample consists of 334 AAER firms and 334 non-AAER firms matched by size, industry, and year. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively. Standard errors are reported in parentheses below the coefficients. Variables are as defined in Appendices 1 and 2.

	Model 1a	Model 1b	Model 2b
CEO pay gap	0.0814***	0.1266***	0.1181**
	(0.0198)	(0.0300)	(0.0499)
Option intensity	-0.0033**	0.0046**	0.0087***
	(0.0014)	(0.0021)	(0.0033)
Sales growth	0.5524***	0.1876**	0.2244
	(0.0655)	(0.0740)	(0.1671)
Volatility	-0.1298	0.8451	-2.0290**
	(0.3315)	(0.6149)	(1.0067)
Receivables	-0.5532***	-0.2593	0.1390
	(0.1696)	(0.2669)	(0.3996)
Intangibles	-1.0213***	-0.4084	-1.0716*
	(0.2430)	(0.3865)	(0.6394)
Size	-0.1537***	0.0966***	0.0010
	(0.0240)	(0.0247)	(0.0493)
Book to market	0.0529	-0.0784	0.1046
	(0.0737)	(0.1109)	(0.1551)
Leverage	1.3603***	0.2977	0.7535**
	(0.1563)	(0.2470)	(0.3420)
Return on assets	1.4589***	0.2330	-0.4386
	(0.1714)	(0.3223)	(0.6473)
Capital	-2.8274***	-0.5902***	-0.3275
	(0.1412)	(0.2134)	(0.3154)
Financing	1.5452***	-0.0904	-0.1473
	(0.1632)	(0.1882)	(0.3955)
Acquisition	0.4286***	0.1911***	0.1599
	(0.0720)	(0.0653)	(0.2108)
Age	0.0119***	-0.0046	-0.0061
	(0.0019)	(0.0030)	(0.0041)
Inventory	1.6176***	-0.3212	0.6283
	(0.2219)	(0.3017)	(0.4452)
Turnover	-0.0929***	0.0487*	0.0891**
	(0.0175)	(0.0269)	(0.0420)
Skewness	-6.0486***	0.1883	0.8757
	(1.7064)	(3.9889)	(7.3208)
Return	-0.0003	-0.1237	0.1657
	(0.0881)	(0.1176)	(0.2836)
Constant	-0.3918**	-3.7107***	-1.0941**
	(0.1802)	(0.2516)	(0.4458)
N	16051	16051	668
Log-Likelihood	-6343.9038	-1543.7620	-436.1722

Appendix 1. Calculation of option intensity

We estimate option intensity as the sum of the intensity of options granted in the current year, the previously granted and unexercisable options, and the previously granted exercisable options (Core and Guay, 2002). For each type, we use the Black-Scholes (1973) formula for valuing European call options, while also accounting for dividends (Merton, 1973).

$$\text{Option intensity } = e^{-dt} * N(Z) * \frac{OPTS}{NOSH} * \$1,000 ,$$

where $Z = \dfrac{\ln\dfrac{S}{X} + T\left(r - d + \dfrac{\sigma^2}{2}\right)}{S\sqrt{T}}$; N is the cumulative probability function for the normal distribution; *OPTS* is the number of options granted; *NOSH* is the number of shares outstanding; S is the price of the underlying stock; *X* is the exercise price of the option; σ is the expected stock return volatility over the life of the option; *r* is the natural logarithm of the risk-free interest rate; *T* is the time to maturity of the option in years; and *d* is the natural logarithm of the expected dividend yield over the life of the option. We aggregate values over executives, including the CEO.

We obtain the risk-free interest rate from CRSP and T (EXDATE), X (EXPRIC), and S (PRCCF) from ExecuComp. For previously granted options, we calculate the exercise price by using the realizable value. We divide the unexercisable (excluding new grants) and exercisable values (OPT_UNEX_UNEXER_EST_VAL and OPT_UNEX_EXER_EST_VAL) by the number of unexercisable and exercisable options held by the executive (OPT_UNEX_UNEXER_NUM and OPT_UNEX_EXER_NUM). We then subtract these numbers from the firm’s stock price as a proxy for the exercise prices (Core and Guay, 2002). The maturity for previously granted options is set equal to the average maturity of newly granted options, -1 for exercisable and -4 for unexercisable options. If the average maturity is not available, we set these equal to 6 and 9, respectively.

Finally, σ and *d* are available from ExecuComp prior to 2006 (BS_VOLATILITY and BS_YIELD).

In sensitivity tests, we also use the delta and vega for both the CEO and the remaining VPs, defined as follows:

$$Delta = \frac{e^{-dt} * N(Z) * S}{100},$$

$$Vega = e^{-dt} * N(Z) * S\sqrt{T} * 0.01,$$

where *N* is the normal density function, and all other variables are as defined above.

Appendix 2. Variable definitions

Test variables	
CEO pay gap	Natural logarithm of total CEO compensation (ExecuComp item TDC1) – the median of all other VPs' total compensation (ExecuComp item TDC1), provided in ExecuComp for a given firm year.
CEO pay gap (max VP)	Natural logarithm of the difference between total CEO compensation (ExecuComp item TDC1) –total compensation of the highest paid VP (excluding the CEO).
CEO pay gap (mean VP)	Natural logarithm of total CEO compensation (ExecuComp item TDC1) – the mean of all other VPs' total compensation (ExecuComp item TDC1), provided in ExecuComp for a given firm year.
Control variables	
Option intensity	Option intensity = $e^{-dt} * N(Z) * \frac{OPTS}{NOSH} * \$1,000;$ aggregated over all executives. More details in Appendix 1.
Sales growth	The percentage change in sales (Compustat item #12) from the prior year to the current year.
Volatility	Standard deviation of the firm's past 12-month return from CRSP.
Receivables	Accounts receivable (Compustat #2) item scaled by total assets (Compustat item #6).
Intangibles	Ratio of research and development (Compustat item #48) and advertising expense (Compustat item #45) to sales (Compustat item #12).
Size	Natural logarithm of the firm's total assets (Compustat item #6). For fraud firms, total assets are measured as of the year preceding the fraud year.
Book to market	The book value of shareholders' equity (Compustat item #216), divided by the market value of equity (Compustat item #199*#25).
Leverage	Total debt (Compustat items #34 + #9) scaled by total assets (Compustat item #6).
Return on assets	Net income (Compustat item #172) divided by year-end asses (Compustat item #6).
Capital	Net plant, property, and equipment (Compustat item #8) scaled by total assets (Compustat item #6).
Financing	Financing during the year by debt and stock (Compustat items #111 + #108) scaled by total assets (Compustat item #6).
Acquisition	Binary variable equal to 1 if acquisitions (Compustat item #249) account for more than 20% of sales, and 0 otherwise.
Age	Number of years the firm appears in Compustat.
Inventory	Inventory (Compustat item #3) scaled by total assets (Compustat item #6).
Turnover	Trading volume accumulated over the past 12-month period scaled by beginning of the year shares outstanding.
Skewness	Skewness of the firm's past 12-month return from CRSP.
Return	12-month stock return from CRSP.
Delta CEO	The natural logarithm of 1 plus the CEO portfolio delta. More details in Appendix 1.
Delta VPs	The natural logarithm of 1 plus the portfolio delta aggregated over all VPs. More details in Appendix 1.
Vega CEO	The natural logarithm of 1 plus the CEO portfolio vega. More details in Appendix 1.
Vega VPs	The natural logarithm of 1 plus the portfolio vega aggregated over all VPs.

	More details in Appendix 1.
Delta CFO	The natural logarithm of 1 plus the CFO portfolio delta. More details in Appendix 1.
Vega CFO	The natural logarithm of 1 plus the CFO portfolio vega. More details in Appendix 1.
G index	The number of anti-takeover-provision (ATP) measures in a firm's charter and in the legal code of the state where the firm is incorporated (Gompers, Ishii, and Metrick, 2003).
Classified board	A binary variable equal to 1 if the board of directors are elected to staggered terms instead of annual terms, and 0 otherwise (Risk Metrics).
CEO duality	A binary variable equal to 1 if the roles of chairman and CEO are not separated, and 0 otherwise (ExecuComp).
Independence	The number of independent outside directors divided by board size (Risk Metrics).
CEO pay slice	CEO total compensation over total VP compensation (ExecuComp item TDC1).
Founder	Binary variable equal to 1 if the CEO is also the founder of the company (Feng et al., 2011).