Executive Compensation and Corporate Fraud

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April 16, 2003

Abstract

We examine the relation between executive compensation and corporate fraud. Executives at fraud firms have significantly larger equity-based compensation and greater financial incentives to commit fraud than do executives at industry- and size-matched control firms. Executives at fraud firms also earn significantly more total compensation by exercising significantly larger fractions of their vested options than the control executives during the fraud years. Operating and stock performance measures suggest executives who commit corporate fraud attempt to offset declines in performance that would otherwise occur. Our results imply that optimal governance measures depend on the strength of executives’ financial incentives.

Keywords: Corporate fraud; Incentives; Stock options; Executive compensation; Governance

JEL codes: G30, K00, M40, M52

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Acknowledgements: We thank Hao Li, Stephen Smith, and Brooke Stanley for excellent research assistance, and Bill Lane, Kelley Pace, Jeff Pontiff, and Cliff Stephens for helpful comments. Johnson and Tian thank the Social Sciences and Humanity Research Council of Canada for financial support.
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1. Introduction

In light of the recent and often spectacular cases of corporate fraud, we ask a basic empirical question: Do the executives who choose to commit corporate fraud face greater financial incentives to do so than those who choose not to commit fraud? The question is motivated by Becker’s (1968) economic theory of crime framework, in which agents choose to commit crime because the expected utility of the payoff is high enough to outweigh the expected disutility of getting caught and prosecuted. Analyzing corporate fraud in this framework is particularly interesting because the fraction of executive compensation tied to stock performance has increased significantly in past years (Hall and Murphy, 2002). If the probability of getting caught and the consequent punishment remain constant, an increase in equity-based compensation creates greater incentives for executives to produce fraudulent financial statements or take other actions to mislead analysts and investors about the value of their firms’ stocks.¹ This combination of factors could explain the increased frequency of earnings restatements over past years (U.S. General Accounting Office, 2002).

Most corporate executives have equity-based compensation contracts and most (presumably) do not commit fraud. This observation suggests that the presence of equity-based compensation is not a sufficient condition for fraud. An open question, however, is whether the size or strength of the incentives provided by equity-based compensation affects the likelihood of

¹ An individual executive’s ethics and morals can be such that he would not commit fraud regardless of the level of the incentives. It is more useful to think about a group of executives, each with a different proclivity to commit fraud, i.e., each with a different threshold of the size of financial payoff that would induce him to commit fraud. Increasing the size of the incentives that each faces should result in greater numbers of executives who cross that threshold and choose to commit fraud.
choosing to commit fraud. To shed light on this question, we use a sample of firms that are the subjects of Securities and Exchange Commission’s (SEC) Accounting and Auditing Enforcement Releases (AAERs), hereafter, fraud firms. For the executives at each firm, we compare the potential dollar payoff of equity-based compensation, standardized for dollar and percentage increases in share price, to similar metrics for executives at industry- and size-matched control firms that are not subjects of AAERs.

We find that executives at fraud firms face significantly greater stock and option payoffs from share price increases compared to executives at matched “innocent” firms. The median executive at a fraud firm has financial incentives that are 51% greater than the median executive at an innocent firm. In dollar terms, a one percent increase in firm value increases executive compensation at fraud firms by approximately $58,844 more than at control firms. The stronger financial incentives stem from significantly larger stock and option holdings by fraud executives, approximately $4.1 million more at the median. During fraud years, executives at the fraud firms also exercise significantly larger fractions of their vested options than do executives at the control firms. Consequently, executives at the fraud firms earn significantly greater total compensation than do the executives at the control firms. The median fraud executive earns approximately $614,000 more per year during the fraud than the median executive at the matched control firms. The median fraud event in our sample lasts two years, which implies a fraudulent marginal gain to the median executive of approximately $1.1 million in present value at a 5% discount rate. To put this gain in perspective, executives at the matched innocent firms earn median total compensation of $1.0 million per year.
We also compare stock and operating performance measures for fraud and control firms. Stock returns over the years of the fraud do not differ significantly across fraud and control firms. Assuming that the frauds had positive effects on fraud firms’ market values during the fraud period, this result implies that but for the frauds, the fraud firms’ would have significantly underperformed their matched control firms. Both the fraud firms and the control firms have significantly negative market-adjusted returns during the fraud periods. This result suggests that the industries in which the fraud firms compete did not perform well relative to the overall stock market during the fraud period. Growth in earnings per share in the year before the frauds begin is significantly lower than in prior years for fraud firms, but not for control firms. This finding implies that the executives committed fraud to offset a decline in operating performance. Collectively, the stock and operating performance results suggest that executives in our sample committed fraud to avoid underperformance relative to their peers.

The industry distribution of fraud firms differs significantly from the ExecuComp population from which we draw our sample. We find that prepackaged computer software and catalog and mail order retail represent the two largest industry concentrations of frauds in our sample. These industries have greater than average growth opportunities, which are more difficult to monitor than assets in place. Since it is more difficult to monitor managers’ actions, firms with these characteristics make greater use of incentive compensation in the form of equity-based compensation (Demsetz and Lehn, 1985; Smith and Watts, 1992; Gaver and Gaver, 1993; Mehran, 1995). This compensation choice creates an interesting tradeoff: Shareholders and boards of directors who cannot easily monitor the actions of their executives should use stronger incentives to align the executives’ incentives, but the same inability to monitor coupled
with the stronger incentives create opportunities and incentives for the executives to commit fraud.

Our analysis cannot answer the question of whether firms’ use of equity-based compensation provides efficient levels of incentive compensation. Ex post we could conclude that the incentives for our fraud firms were too strong, but since those fraud firms represent less than two percent of the population of firms from which our sample is drawn, another conclusion is that most firms have incentives that are not too strong. Without answering the efficiency question, our analysis still highlights the importance of considering the increased potential for fraud and its associated costs when choosing increased levels of incentive compensation.

If executives rationally commit fraud because they face incentives that make the expected benefits of fraud exceed the expected costs, then firms, investors, auditors, regulators, and others should pay more attention to the expected costs. Even absent civil and criminal proceedings against fraud executives, an efficient labor market should punish executives who commit fraud through reductions in the value of their human capital (Fama, 1980). Extant empirical evidence, however, generally suggests small expected costs of committing fraud. Beneish (1999) finds no differences in employment losses across firms that do and do not manipulate earnings. He also finds that the SEC is unlikely to impose trading sanctions on executives unless executives sell their own shares as part of a security offering. Agrawal et al. (1999) find that firms suspected of fraud or charged with fraud do not have significantly higher turnover of senior managers or directors. Agrawal and Chadha (2002) find that having several key governance mechanisms in place does not reduce the likelihood of restatements. In contrast, Dechow et al. (1996) find that fraud firms are more likely to have boards dominated by management, CEOs as chairmen of the
board, and founder CEOs, and less likely to have audit committees and outside blockholders. Dechow et al.’s results imply that fraud firms have a suboptimal level of corporate governance.

Extant research has examined other unintended outcomes associated with equity-based compensation contracts. Yermack (1997) and Aboody and Kasznik (2000) find evidence that suggests that firms time stock option grants around news events to benefit executives. Bettis et al. (2001) find that some executives use zero-cost collars and equity swaps to hedge out their risk exposure created by option and stock holdings. Such actions weaken the incentives provided by equity-based compensation. To the extent that shareholders or boards of directors do not adjust for unintended outcomes, the resulting compensation contracts are likely inefficient. Our results suggest another unintended outcome that could create inefficiencies.

Our focus on equity-based compensation differs from studies of earnings manipulation in the accounting literature, most of which focus on bonus contracts (see e.g., Healy, 1985; Dechow et al., 1996). This different focus is important since options have grown as a fraction of total executive compensation over past years, and since differences in executive pay sensitivity are driven primarily by differences in the sizes of stock and option holdings (see Hall and Liebman, 1998; Hall and Murphy, 2002). The difference is also important because bonus payoffs are often capped, which likely limits the potential gain to fraud compared to stock and option payoffs.

The next section of the paper describes our data and methods. Section 3 contains analysis comparing fraud firms to control and other firms on incentives and several other dimensions. Section 4 concludes.
2. Data and Methods

To generate a sample of fraud firms, we begin with the set of firms in Standard and Poor’s (S&P) ExecuComp database. The ExecuComp database covers the period 1992 through 2001, and contains executive compensation data for firms in the S&P 1500 index, which comprises the S&P 500 index, the S&P 600 midcap index, and the S&P 400 small cap index. If a firm is deleted from the S&P 1500 index and replaced with a new firm, ExecuComp retains the deleted firm’s historical information and adds data for the new firm. In total, the ExecuComp database contains data for 2,504 firms.

We next search the Securities and Exchange Commission’s (SEC) set of Accounting and Auditing Enforcement Releases (AAERs) for the 2,504 ExecuComp firms. AAERs represent cases where the SEC believes that there is sufficient evidence of accounting or auditing fraud to prosecute a case against a firm or its executives. We omit cases in which the SEC charges a firm with having inadequate controls in place to prevent embezzlement by non-executive employees. We also omit cases in which the SEC charges non-executive employees at foreign subsidiaries with bribery of foreign officials or foreign customers. We view both of these types of cases to be inappropriate for studying the relation between executive compensation and corporate fraud.

The time span between a fraud event and the filing of an AAER can range from zero to several years. Also, a particular fraud event can occur over several years. For example, a firm could produce fraudulent financial statements over a period of five years. Since ExecuComp has data from 1992 to 2001, we require that the fraud event occur in that time interval, regardless of the filing date of the AAER. We include firms with fraud events that occur from 1992 to 2001, even if the event began in 1990 or 1991, if we can backfill executive compensation data from
proxy statements. We omit several multiple-year fraud events that began before 1990 because we are unable to backfill the data. Our final sample comprises 43 unique fraud events, represented by 102 fraud-years.

Table 1 presents the distribution of fraud events over our sample period and data on the length of the frauds for each year of our sample period. In contrast to what might be expected from recent coverage of fraud events in the financial and popular press, there are 25 fraud events in the first half of the sample period (1992-1996) compared to 18 in the second half of the sample period (1997-2001). The lower incidence of fraud in the latter part of the sample could reflect an actual decrease in the commission of frauds over time. Alternatively, it could reflect the possibility that the SEC has not yet have discovered and charged firms that committed frauds in the later part of our sample period. The increase in (not necessarily fraudulent) earnings restatements over the 1990s gives some support for the latter possibility.

[Table 1 about here]

Table 2 presents the industry distribution of our fraud firms compared to the industry distribution of ExecuComp firms. The single largest industry represented in the fraud sample, SIC code 7372 (prepackaged computer software) represents 11.63% of the fraud sample, but only 3.96% of the ExecuComp set of firms. We also find concentrations in SIC codes 5961 (catalog & mail order retail), 4953 (refuse and disposal systems), 5172 (petroleum and other fuels), and 6021 (national commercial banks). A continuity-adjusted chi-square test, significant at the 0.01 level, indicates that the industry distributions differ across fraud and ExecuComp firms.
Firms that derive more value from intangible assets that produce future growth opportunities are more difficult to monitor than firms that derive more value from assets in place. Thus, such firms should optimally use more incentive compensation to counter the monitoring difficulties (Demsetz and Lehn, 1985; Smith and Watts, 1992; Gaver and Gaver, 1993; Mehran, 1995). The inability to monitor, however, also makes these firms more susceptible to fraud. To examine this premise, we compare growth opportunities for the higher-than-expected concentration fraud industries (SIC codes 7372, 5961, 4953, and 5172) to growth opportunities for all other industries. We use the market-to-book-asset ratio as a proxy for growth opportunities. Unreported results show that the two industries with the greatest concentration in the fraud sample—7372 and 5961—have significantly higher mean market-to-book ratios than other industries.

For each fraud firm in each crime year, we identify an industry- and size-matched control firm in the ExecuComp database that is not the subject of an AAER at any time during our sample period. We match on industry and size because board of director compensation committees typically use benchmarks that depend on these two factors when setting executive compensation packages. If we do not find a four-digit-SIC code match that is within 30% of the revenues of our fraud firm, we look for a three-digit-SIC code match. For 49 of the 102 fraud years, we find a match within 30% of the fraud firm. Later we investigate potential problems with using matches that are outside the 30% bounds. We find qualitatively similar results to those reported if we use only matches that are inside the 30% bounds.
It is well known that many executives manage or manipulate their firms’ earnings legally within Generally Accepted Accounting Principals (GAAP). It is possible that our control firms also manage or manipulate earnings within GAAP. By using AAERs to identify fraud firms, we focus on cases in which executives cross the threshold of GAAP to engage in illegal earnings manipulation. Since there are no criminal or civil penalties for earnings management within GAAP, and potentially significant penalties for managing or manipulating earnings in violation of GAAP, we assume that crossing this threshold is significant. If crossing the threshold does not represent a significant executive decision, then our approach of using AAERs to define fraud firms has a bias against finding any differences in incentives across fraud firms and control firms. It is also possible that some of our control firms commit fraud, but do not get caught. Such cases should also create a bias against finding any differences in incentives across fraud firms and control firms.

We calculate two incentive measures for each executive at each fraud and control firm. First, we calculate a dollars-on-dollars incentive measure as the dollar change in an executive’s stock and option portfolio for a $1,000 dollar change in firm value. Baker and Hall (2002) argue that researchers should use a dollars-on-dollars measure to compare incentives across firms when executive actions affect dollar returns, as in perquisite consumption. We also use a dollars-on-percentage incentive measure, defined as the dollar change in an executive’s equity-based portfolio (stock and options) for a one percent change in firm value. Baker and Hall argue that this dollars-on-percentage measure is more appropriate when executive actions have a similar percentage impact on the firms in the comparison. Most of the fraud events in our sample involve overstating revenues or understating expenses in apparent attempts to inflate stock prices. Although these actions can produce very different changes in the dollar value of the firm,
they likely produce similar percentage changes. Thus, we focus our analyses on the dollars-on-
percentage incentive measure.

To calculate the two incentive measures, we need an option pricing model and several
input parameters. Consistent with previous research (e.g., Marquardt, 2002), we adopt a modified
version of the Black-Scholes model. Since executives typically exercise their options before
maturity (Hemmer et al. 1996; Huddart and Lang, 1996; Heath et al., 1999), we reduce the
contractual option maturity by 30%. We use the average yield on U.S. treasury securities that
most closely match the option’s (reduced) maturity to approximate the risk-free rate. We use the
standard deviation of stock returns over the prior 60 months to estimate the stock return
volatility. We use the average dividend yield over the prior three years as a proxy for the future
dividend yield. For newly granted options, the strike price and the maturity come directly from
ExecuComp. ExecuComp does not report terms and numbers of individual grants for previously
granted options. We use Core and Guay’s (2002) one-year approximation method to estimate the
strike price and the maturity of previously granted options.

We define our two incentive measures as:

\[
\text{Dollars-on-dollars} = \frac{\text{Change in executive's equity portfolio}}{\text{Change in firm value}} \times 1000,
\]

\[
\text{Dollars-on-percentage} = \frac{\text{Change in executive's equity portfolio}}{\% \text{Change in firm value}} \times 0.01.
\]

These measures are linked to the equity portfolio delta, which reflects the change in portfolio
value as stock price changes. We use the modified Black-Scholes model to calculate the delta of
the executive’s equity portfolio, which we use to calculate the two incentive measures. The Executive’s equity portfolio includes options and restricted and unrestricted stock holdings.

Anecdotal descriptions of fraud firms suggest that some firms have a culture among top management that encourages fraud. Other descriptions imply that one influential individual exerts pressure on others to engage in fraud. This dichotomy leads us to study both group incentives and individual incentives. To study group incentives, we would prefer to sum the incentive measures across executives at each firm to measure how large of a “prize” the top managers would split if they could cause share price to increase. A large potential prize could promote a culture that encouraged fraud as a means of increasing share price. Unfortunately, fraud firms and control firms do not necessarily have the same numbers of executives for whom they report compensation data. Thus, such an analysis could involve, for example, comparing the sum of incentives of five executives to the sum of incentives for three executives. We doubt that such comparisons would be valid. The differing numbers of executives means that we also cannot match each individual executive at fraud firms with a corresponding individual executive at a control firm to focus on each individual’s incentives. Executives have different combinations of titles (e.g., president and COO) across fraud and control firms, which also prevents us from matching up fraud and control executives individually.

Given the different numbers of executives across firms, we use two comparisons of incentives across fraud and control firms. First, we compute the mean incentive for executives at each firm to measure group incentives. We can compare these means across fraud and control firms even if they have differing numbers of executives. Second, we compute the maximum incentive among executives at each firm. Efficient contracting implies that the executive with
the strongest incentives is the most influential. Our approach attempts to capture the fact that such a person might exert pressure on others to engage in fraud.

3. Results

3.1. Executive compensation and incentives for fraud versus control firms

In Table 3, we present statistics to compare group (i.e., mean) incentive measures for executives at fraud and control firms. Even though there are 102 fraud-years, the means are significantly skewed by extreme values. Thus, we present and discuss results for medians. We use signed-rank tests to test if the median of the paired differences is significantly different from zero. Since the measures occur over time, we use the consumer price index to express all values in constant 2002 dollars.

Panel A contains the results for group incentives. We use the average across all executives for each firm as our proxy for group incentives. Our primary incentive measure, the dollars-on-percentage measure, is $174,501 for fraud firms and $115,657 for control firms. A median paired-difference test has a \( p \)-value less than 0.01. Thus, the fraud firms provide significantly stronger financial incentives than the control firms provide. The magnitudes indicate that for each one-percent increase in share price, the fraud executive’s stock and option portfolio increases by $58,844, or 51%, more than the increase in the portfolio of the control firm’s executive. The dollars-on-dollars measure (i.e., the dollar increase in the executive’s stock and option portfolio for a $1,000 increase in firm value) is also significantly larger for the fraud firms than for the control firms: $6.52 for fraud firms compared to $3.17 for control firms.
The median measure for fraud firms is more than twice the median measure for the control firms. The paired-difference test for this measure has a \( p \)-value less than 0.01. These results support the hypothesis that executives of fraud firms face larger financial incentives, which creates incentives to commit fraud. The results imply that the financial incentives could be large enough to outweigh the expected disutility of getting caught.

In Panel A of Table 3, we also compare the incentive measures for fraud firms to those for ExecuComp firms in general. Unlike the control-firm approach, this non-matched analysis does not control for industry or firm size. This approach reveals, however, approximately where in the overall distribution of ExecuComp firms the incentives for fraud firms fall. The median dollars-on-percentage incentive measure for fraud firms is approximately 2.46 times the median for ExecuComp firms. The fraud firm median and the ExecuComp median differ significantly at the 0.01 level. Thus, executives at fraud firms receive stronger incentives on a relative basis (compared to size and industry matched controls) and on an absolute basis (compared to all other ExecuComp firms). In contrast to the dollars-on-percentage measure, the dollars-on-dollars measure does not differ significantly across fraud firms and ExecuComp firms.

[Table 3 about here]

The greater incentives shown by our comparison of the two incentive measures reflect the larger stock and option portfolios held by the executives at fraud firms. As shown in Panel A of Table 3, the median fraud executive holds stocks and options worth $12,638,700. This value compares to the median for control executives of $8,551,042. The median paired difference is significantly greater than zero at the 0.01 level. These differences in total portfolio values reflect larger holdings of both stock and options. The median paired differences for stock holdings and
for option holdings indicate that fraud executives hold significantly larger portfolios of equity-based compensation.

The results described above raise an interesting question: Do fraud firms’ executives hold larger option and stock portfolios because their firms substitute options and stock for cash salary and bonus in their compensation packages? If fraud firms are relatively more cash constrained, such a substitution could lead to a greater fraction of equity-based compensation for fraud firms. To address this question, we also present in Table 3 measures of compensation and compensation structure that reflect the fractions of compensation represented by salary and bonus. The median salary is slightly lower for fraud firms ($406,957) than for control firms ($421,874) with a $p$-value of 0.10. Similarly, the ratio of salary to total compensation is lower for fraud firms (45.46%) than for control firms (47.55%), with a $p$-value of 0.08. The median bonus and the ratio of salary and bonus to total compensation do not differ significantly for fraud firms and control firms. The results for bonus are somewhat surprising since many bonuses are linked to accounting figures. The results for salary are only marginally statistically significant and are probably economically insignificant. In general, these results suggest that fraud firms do not substitute stock options for salary and bonus to create the larger incentives discussed earlier.

Panel B of Table 3 presents results comparable to those in Panel A, except that we use the maximum incentive per firm measure rather than the average measure as our proxy. We use the maximum incentive measure to capture the incentives of the most influential executive at each firm. The results for the dollars-on-percentage measure and the dollars-on-dollars measure are similar to those in Panel A—both incentive measures indicate that fraud executives faced greater financial incentives to commit fraud. Fraud firms have a median dollars-on-percentage measure
of $511,323, which is approximately 87% greater than the value of $273,907 for control firms. These greater incentives reflect significantly larger stock and option holdings: $45,733,900 for fraud firms versus $19,754,630 for control firms. The differences across fraud and control firms in individual incentives are much larger than the differences in the group incentives.

We next examine whether fraud executives cashed in on their frauds during the fraud period by exercising options at greater rates than control executives. In Table 4 we compare measures of the exercise behavior by fraud and control firm executives. We calculate four ratios: (1) (number of options exercised) / (an executive’s total options outstanding); (2) (number of options exercised) / (an executive’s total number of vested options); (3) (dollar payoffs from option exercises) / (total cash compensation); and (4) (dollar payoffs from option exercises) / (total cash and non-cash compensation). The median fraud-firm executive exercises 4.94% of his total options outstanding and 12.81% of his vested options during the fraud years. In contrast, the median control-firm executive exercises 1.62% and 3.11%, respectively. These medians differ significantly across fraud firms and control firms for both measures. These results suggest that fraud-firm executives exercise their options much more aggressively during fraud years than their counterparts at control firms. The option exercise results contrast with Dechow et al.’s (1996) finding that insider sales of stock are not proportionately larger at fraud firms than control firms.

[Table 4 about here]

The more aggressive exercise behavior provides payoffs that represent larger fractions of cash compensation and total compensation. The median fraud-firm executive has gains from option exercise equal to 31.42% of his cash compensation and 12.48% of his total (cash and non-
cash) compensation. In contrast, control-firm executives receive payoffs equal to 11.58% of cash compensation and 6.76% of total compensation. Both measures differ significantly across fraud and control firms. Fraud firm executives exercise stock options more aggressively during fraud periods. During these periods, they obtain much larger fractions of their compensation from these exercises.

The results in Table 3 indicate economically small and statistically weak differences in salary and bonus across fraud and control executives. The results in Table 4 indicate greater dollar payoffs from option exercises for fraud executives. The latter result implies that these executives receive larger total compensation during their fraud years. To shed additional light on this issue, we present in Table 4 the medians for total compensation. The median fraud executive receives total compensation from all sources of $1.623 million, which compares to $1.008 million for control executives.

Although not related to the incentive question per se, it is interesting to ask what these option exercises cost shareholders of the fraud firms. To shed light on this question, we compute the gains from option exercises as a percentage of firm market capitalization. Exercise payoffs at fraud firms represent 5.61% of firm market capitalization compared to 2.60% for payoffs at control firms. The percentages differ at the 0.07 level. Thus, the evidence suggests that fraud-firm shareholders give up larger fractions of firm value to their executives than do shareholders at control firms.
3.2. Stock and operating performance measures for fraud versus control firms

We next compare stock returns across fraud and control firms during the fraud periods. Because the AAERs almost always use fiscal years instead of calendar days to define the fraud period, we compute returns from the beginning of the first fiscal year of the fraud period through the end of the last fiscal year of the fraud period. Alternatively, we measure stock returns beginning one year before the fraud started. This alternative approach accounts for the possibility that executives made fraudulent statements to analysts or investors about expected revenues, expenses, and earnings in the months preceding the first fiscal year in which they produced fraudulent financial statements. Following our method outlined in the previous section we present medians and corresponding $p$-values from paired-differences tests.

[Table 5 about here]

Panel A of Table 5 contains median annualized raw returns during the fraud period. We present data for both fraud firms and control firms. The median fraud firm generates an annual return of 2.04% during their fraud years, which compares to 4.91% for control firms. The $p$-value from the paired-difference test is 0.42, which indicates that the medians do not differ significantly. We find similar results when we begin our test period one year before the fraud begins. We also subtract the return from the value-weighted CRSP index to adjust the raw returns for market performance. Panel B of Table 5 contains the market-adjusted returns. Fraud firms have a median annual market-adjusted return of $-9.55\%$ during the fraud period, which is significantly different from zero at the 0.01 level. Control firms have a median annual market-adjusted return of $-9.46\%$, which is also significantly different from zero at the 0.01 level. Like
the unadjusted returns, the market-adjusted returns do not differ significantly across fraud firms and control firms.

The results show that both the fraud and the control firms significantly underperform the overall stock market. Additionally, the median raw and market-adjusted returns do not significantly differ across fraud firms and control firms. Thus, although fraud firms provide fraudulent information to financial markets during the fraud periods, they significantly underperform the overall stock market. Also, they do not perform better than their control firms. Assuming that frauds such as overstated revenues or understated expenses positively impact the fraud firms’ stock values during the fraud periods, our results imply that fraud firms would have significantly underperformed the matched control firms had the frauds not occurred. Thus, it appears that the frauds in our sample are motivated by a desire to avoid underperformance relative to peer firms rather than a desire to outperform peer firms.

Anecdotal evidence suggests that analysts, investors, and financial market commentators frequently focus on firms’ abilities to consistently increase earnings per share (EPS). Firms that fail to maintain EPS growth suffer via lower stock prices. Since such stock-price penalties reduce the value of executives’ holdings of stock and stock options, executives have the incentives to avoid negative EPS growth and possibly even reductions in EPS growth. These incentives could motivate an executive to commit fraud. To examine this premise, we compare EPS growth across fraud firms and control firms in the years preceding the frauds.

We compute an arithmetic average of EPS growth from years minus five to minus two. We use an arithmetic average to allow calculation of average annual growth rates from a positive to negative value for earnings per share. Since we cannot calculate a growth rate from a
negative to a positive EPS, we substitute the EPS for year minus four if the earnings figure in year minus five is negative and the minus two EPS is positive. If the year minus four EPS is also negative, we omit the observation. We also omit observations with inadequate data on Research Insight to calculate any of these growth rates. Because we conduct matched-pair tests, we have the same data requirements across fraud and control firms. Collectively, these restrictions limit this analysis to 31 pairs of fraud and control firms. For fraud firms and control firms, we compare the average annual growth rate in EPS from years minus five to minus two to the annual growth rate from years minus two to minus one, where year zero is the first fraud year.

As shown in Table 6, fraud firms have a median annual growth in EPS from years minus five to minus two of 18.24%. For the year immediately preceding the frauds (i.e., from years minus two to minus one), the median annual growth rate is only 5.00%. The slowdown in growth is statistically significant at the 0.01 level. By contrast, control firms have median annual growth in EPS of 13.45% from years minus five to minus two. This growth rate does not differ significantly from the growth rate of 13.66% from years minus two to minus one ($p$-value = 0.46). The EPS growth rates do not differ significantly across fraud and control firms in the minus-five to minus-two period. However, the median EPS growth rate for fraud firms is significantly less than the median for control firms in the year immediately preceding the fraud (minus two to minus one). Thus, fraud firms display a significant slowdown in EPS growth and have significantly lower EPS growth than control firms in the year before the fraud.

In summary, the stock-return and operating-performance results suggest that the desire to avoid underperformance motivates most of the frauds in our sample. Although our primary
incentive measure indicates the change in portfolio value for a one percent increase in firm value, the magnitude of the measure is nearly symmetric but opposite in sign for a one percent decrease in firm value. Thus, we can view the equity-based compensation for fraud executives as providing stronger incentives to increase firm value or stronger incentives to avoid decreases in firm value, both of which create greater incentives to commit fraud. Given our findings of poor stock- and operating-performance, we conclude that fraud executives in our sample committed fraud because they had stronger incentives to avoid decreases in firm value.

3.3. Sensitivity and robustness checks

Rising stock prices increase the moneyness of stock options. Ceteris paribus, the increase in the moneyness increases the option deltas and the strength of the associated incentives. Thus, the greater incentives we find for fraud firms, in particular in years beyond the first year of the fraud, could be created by the fraud itself if it leads to higher stock prices. If executives commit frauds that increase stock price and the moneyness of their options, stronger measured incentives could be induced by the rising stock prices. If so, our measured incentives would not reflect the differences in the compensation that was initially awarded to the executives.

To check whether such a phenomena drives our results, we repeat the comparison of fraud versus control firms using only the first year of data for each firm. In unreported results, we find that the two incentive measures, dollars-on-percentage and dollars-on-dollars, are still significantly larger for fraud firms than for control firms at least at the 0.05 level. We conduct a similar analysis over the 31 observations for which we have incentive data for the year preceding the first fraud year. The two incentive measures are still significantly larger for fraud firms than for control firms, albeit at a lower significance level (p-value = 0.08) for the dollars-on-dollars
measure. Thus, our findings of stronger financial incentives for fraud firms do not appear to be driven by the increases in stock price and option moneyness that result from the frauds themselves.

In the data discussion we noted that the control firm’s revenue differs from the fraud firm’s revenue by more than 30% for a significant number of fraud-control pairs. Since compensation is related to size, our results could be sensitive to inclusion of mismatched fraud-control pairs, where mismatched means their revenues are not within 30% of each other. Thus, we repeat our analysis excluding the mismatched paired observations. Unreported results show that our two incentive measures remain significantly greater for fraud firms than for control firms at the 0.01 level. Thus, our main results are not sensitive to inclusion of mismatched fraud-control pairs.

The results in Table 3 reveal much greater differences between fraud and control firms for the maximum incentive measures per firm than the mean incentive measures per firm. Extremely high maximum values could skew the means for some firms upward, which could lead to incorrect inferences about the importance of group versus individual incentives. To avoid the effects of possibly extreme values for the most highly compensated executive at each firm, we repeat our analysis using median incentives per firm instead of mean incentives per firm. Unreported results show that the two incentives remain significantly greater for fraud firms than for control firms. Thus, both group and individual incentives are significantly higher for fraud firms.

Our analysis of fraud-years, where some fraud events extend to more than one fraud year, means that there is no strict independence among all of the observations. The dependence
among fraud years for the same fraud event could lead to overstated test statistics and significance levels. Although this is likely to be a trivial problem since the maximum fraud length for any single firm is six years, we also conducted an analysis designed to avoid such problems. Specifically, we average the incentive measures across years for each fraud event to produce one observation for each unique fraud event. This approach results in a sample that is independent across fraud events. Unreported results show that the two incentive measures, the dollars-on-percentage measure and the dollars-on-dollars measure, are still both significantly larger for fraud firms than for control firms at the 0.01 level.

4. Conclusions

We find that executives who commit corporate fraud face greater financial incentives to do so. These incentives stem from significantly larger stock and option holdings. Executives who commit fraud also exercise significantly larger fractions of their stock options. By committing fraud, these executives receive significantly larger total compensation during the fraud period than they would have otherwise received. Fraud firms have significant slowdowns in earnings-per-share growth in the year before the beginning of their frauds. They do not provide superior stock returns over the fraud periods. Instead, our results suggest that executives commit frauds to avoid underperformance as a result of significant slowdowns in their earnings growth. Frauds appear disproportionately in certain industries. We also find that these industries have relatively low stock returns during the fraud periods, which suggests that executives are more likely to commit frauds during industry downturns.

Our results have several implications for optimal incentive structures and for optimal expenditures on anti-fraud measures by firms, investors, analysts, and regulators. First, the
strength of costly corporate governance measures need not be constant across firms. Instead, the optimal level of anti-fraud measures should depend in part on the strength of the incentives to commit fraud. For firms that should optimally provide very strong incentives to executives via equity-based compensation (e.g., firms with high growth opportunities), the optimal level of anti-fraud measures should be higher. This implication is a potentially difficult prescription because it is more difficult to monitor, and thus prevent fraud, by executives of firms with higher growth opportunities. Second, the concentration of fraud in certain industries where it intuitively seems easier to commit fraud suggests that the optimal level of anti-fraud mechanisms varies across industry and firm characteristics. Third, the level of equity-based compensation trended upward in recent years. Managers receive a greater proportion of their compensation from equity-based incentives than they did in the past. Anti-fraud measures—both at the firm level and at the auditor, investor, and regulator level—should increase commensurately. It is not obvious that such measures did increase commensurately with the levels of equity-based compensation, which could explain the apparent increased incidence of corporate fraud in recent years.

Research on equity-based compensation suggests that it has substantial benefits. However, the timing issue in Yermack (1997) Aboody and Kasznik (2000) and the hedging issue in Bettis et al. (2001), together with our findings on fraud incentives, suggest that at least some compensation contracts do not maximize shareholder wealth ex post. Future theoretical and empirical work on executive compensation should incorporate potential indirect or unintended costs of various compensation structures and levels.

We note that corporate fraud imposes costs on society in a number of ways. First, corporate fraud leads to a misallocation of resources, inducing greater capital flows to firms that
do not actually provide the highest risk-adjusted expected returns. Second, corporate fraud can reduce investor confidence in all or many public corporations, leading to increases in the costs of capital for non-fraud firms (U.S. General Accounting Office, 2002). Third, an increased likelihood of corporate fraud increases monitoring and enforcement costs of investors, boards of directors, auditors, bankers, and regulators. Our results cannot speak to the question of whether the current monitoring and enforcement expenditures of these parties or the level of equity-based compensation are optimal in a societal welfare sense. Our results do imply, however, that researchers who consider that question must also consider the nature and size of executive incentives provided by equity-based compensation.
References


Agrawal, A., Chadha, S., 2002, Corporate governance and accounting scandals, University of Alabama working paper.


Table 1
Distribution of frauds and lengths of frauds through time
This table presents data on the number of fraud events over time and on the length of these frauds. We define fraud events as SEC Accounting and Auditing Enforcement Releases filed between 1992 and 2002 against firms in the ExecuComp database. We classify the frauds by the first year of the fraud event.

<table>
<thead>
<tr>
<th>Year</th>
<th>Number of fraud events</th>
<th>Median fraud length in years</th>
<th>Minimum fraud length in years</th>
<th>Maximum fraud length in years</th>
</tr>
</thead>
<tbody>
<tr>
<td>1992</td>
<td>6</td>
<td>2.5</td>
<td>1</td>
<td>6</td>
</tr>
<tr>
<td>1993</td>
<td>6</td>
<td>1.5</td>
<td>1</td>
<td>4</td>
</tr>
<tr>
<td>1994</td>
<td>3</td>
<td>3</td>
<td>2</td>
<td>3</td>
</tr>
<tr>
<td>1995</td>
<td>5</td>
<td>2</td>
<td>2</td>
<td>3</td>
</tr>
<tr>
<td>1996</td>
<td>5</td>
<td>2</td>
<td>1</td>
<td>7</td>
</tr>
<tr>
<td>1997</td>
<td>8</td>
<td>2.5</td>
<td>1</td>
<td>4</td>
</tr>
<tr>
<td>1998</td>
<td>4</td>
<td>3</td>
<td>1</td>
<td>3</td>
</tr>
<tr>
<td>1999</td>
<td>3</td>
<td>1</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>2000</td>
<td>1</td>
<td>3</td>
<td>3</td>
<td>3</td>
</tr>
<tr>
<td>2001</td>
<td>2</td>
<td>1</td>
<td>1</td>
<td>1</td>
</tr>
</tbody>
</table>
Table 2
Industry distribution of fraud firms versus ExecuComp firms
This table compares the industry distribution of fraud firms with the industry distribution of ExecuComp firms. A continuity-corrected chi-square test indicates that the distributions differ from each other at the 0.01 level.

<table>
<thead>
<tr>
<th>Industry (SIC code)</th>
<th>Percentage of Fraud Firms Industry Represents</th>
<th>Percentage of ExecuComp Firms Industry Represents</th>
</tr>
</thead>
<tbody>
<tr>
<td>7372: Prepackaged computer software</td>
<td>11.63%(^a)</td>
<td>3.96%</td>
</tr>
<tr>
<td>5961: Catalog &amp; mail order retail</td>
<td>6.98(^a)</td>
<td>0.64</td>
</tr>
<tr>
<td>4953: Refuse and disposal systems</td>
<td>4.65(^a)</td>
<td>0.52</td>
</tr>
<tr>
<td>5172: Petroleum and other fuels</td>
<td>4.65(^a)</td>
<td>0.12</td>
</tr>
<tr>
<td>6021: National commercial banks</td>
<td>4.65</td>
<td>3.00</td>
</tr>
<tr>
<td>All others</td>
<td>67.44(^a)</td>
<td>91.77</td>
</tr>
</tbody>
</table>

\(^a\)Indicates the fraud and ExecuComp proportions are significantly different from each other at the 0.05 level.
Table 3
Incentive measures for fraud firms versus matched control firms and ExecuComp firms
This table presents incentive and compensation measures for fraud firms and control firms for 102 fraud-years. We calculate the dollars-on-percentage incentive measure as the dollar change in stock and option holdings for a one percent increase in firm value. We calculate the dollars-on-dollars incentive measure as the dollar change in executive stock and option holdings for a $1,000 increase in firm value. We use Black-Scholes values in all measures that contain option values.

Panel A: Observations are means per firm

<table>
<thead>
<tr>
<th>Measure</th>
<th>Fraud Firms</th>
<th>Matched Firms</th>
<th>p-value of $H_0$: Median paired difference = 0</th>
<th>ExecuComp Firms</th>
<th>p-value of $H_0$: Fraud median = ExecuComp median</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dollars-on-percentage incentive measure ($)</td>
<td>174,501</td>
<td>115,657</td>
<td>&lt;0.01</td>
<td>71,008</td>
<td>&lt;0.01</td>
</tr>
<tr>
<td>Dollars-on-dollars incentive measure ($)</td>
<td>6.52</td>
<td>3.17</td>
<td>&lt;0.01</td>
<td>6.91</td>
<td>0.86</td>
</tr>
<tr>
<td>Value of options &amp; stock</td>
<td>12,638,700</td>
<td>8,551,042</td>
<td>&lt;0.01</td>
<td>5,703,920</td>
<td>&lt;0.01</td>
</tr>
<tr>
<td>Salary</td>
<td>406,957</td>
<td>421,874</td>
<td>0.10</td>
<td>318,972</td>
<td>&lt;0.01</td>
</tr>
<tr>
<td>Bonus</td>
<td>198,192</td>
<td>195,616</td>
<td>0.22</td>
<td>163,304</td>
<td>0.10</td>
</tr>
<tr>
<td>Salary / Total compensation</td>
<td>45.46%</td>
<td>47.55%</td>
<td>0.08</td>
<td>50.16%</td>
<td>&lt;0.01</td>
</tr>
<tr>
<td>(Salary + Bonus) / Total Compensation</td>
<td>69.05%</td>
<td>72.30%</td>
<td>0.14</td>
<td>77.77%</td>
<td>&lt;0.01</td>
</tr>
<tr>
<td>New Options Granted / Total Compensation</td>
<td>42%</td>
<td>36%</td>
<td>0.04</td>
<td>38%</td>
<td>0.05</td>
</tr>
</tbody>
</table>

Panel B: Observations are maximums per firm

<table>
<thead>
<tr>
<th>Measure</th>
<th>Fraud Firms</th>
<th>Matched Firms</th>
<th>p-value of $H_0$: Median paired difference = 0</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dollars-on-percentage incentive measure ($)</td>
<td>511,323</td>
<td>273,907</td>
<td>&lt;0.01</td>
</tr>
<tr>
<td>Dollars-on-dollars incentive measure ($)</td>
<td>22.59</td>
<td>9.01</td>
<td>&lt;0.01</td>
</tr>
<tr>
<td>Value of options and stock</td>
<td>45,733,900</td>
<td>19,754,630</td>
<td>&lt;0.01</td>
</tr>
<tr>
<td>Salary</td>
<td>786,867</td>
<td>790,253</td>
<td>0.82</td>
</tr>
<tr>
<td>Bonus</td>
<td>580,900</td>
<td>319,162</td>
<td>0.13</td>
</tr>
<tr>
<td>Salary / Total compensation</td>
<td>74.46%</td>
<td>66.88%</td>
<td>0.18</td>
</tr>
<tr>
<td>(Salary + Bonus) / Total Compensation</td>
<td>98.00%</td>
<td>96.33%</td>
<td>&lt;0.08</td>
</tr>
<tr>
<td>New Options Granted / Total Compensation</td>
<td>100.00%</td>
<td>100.00%</td>
<td>0.16</td>
</tr>
</tbody>
</table>
Table 4  
Exercise behavior for fraud firms versus control firms  
This table presents data on the number of options exercised during the fraud period standardized by several compensation and firm measures. Total options outstanding includes both vested and unvested options. Vested options comprise all options currently exercisable by the executives. Cash compensation includes both salary and cash bonus, and non-cash compensation comprises both stock awards and stock option grants. We calculate the firm’s market capitalization as the product of share price and shares outstanding in each fraud year.

<table>
<thead>
<tr>
<th>Measure</th>
<th>Fraud Firms</th>
<th>Matched Firms</th>
<th>p-value of $H_0$: Median paired difference = 0</th>
</tr>
</thead>
<tbody>
<tr>
<td>No. of exercised options / total options outstanding</td>
<td>4.94%</td>
<td>1.62%</td>
<td>0.05</td>
</tr>
<tr>
<td>No. of exercised options / total vested options</td>
<td>12.81%</td>
<td>3.11%</td>
<td>&lt;0.01</td>
</tr>
<tr>
<td>Value of option exercises / total cash compensation</td>
<td>31.42%</td>
<td>11.58%</td>
<td>&lt;0.01</td>
</tr>
<tr>
<td>Value of option exercises / total cash and non-cash compensation</td>
<td>12.48%</td>
<td>6.76%</td>
<td>0.02</td>
</tr>
<tr>
<td>(Value of option exercises / firm market capitalization) * 10,000</td>
<td>5.61</td>
<td>2.60</td>
<td>0.07</td>
</tr>
<tr>
<td>Total compensation, including option exercises</td>
<td>$1,622,766</td>
<td>$1,008,836</td>
<td>&lt;0.01</td>
</tr>
</tbody>
</table>
Table 5
Fraud and control firm stock returns during fraud periods
This table presents annualized raw and market-adjusted returns for 43 pairs of fraud firms and control firms during fraud periods. We calculate market-adjusted returns as firm returns less value-weighted CRSP market returns.

<table>
<thead>
<tr>
<th>Measure</th>
<th>Fraud Firms</th>
<th>Matched Firms</th>
<th>p-value of $H_0$: Median paired difference = 0</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: Raw Returns</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Annualized stock return over fraud period</td>
<td>2.04%</td>
<td>4.91%</td>
<td>0.42</td>
</tr>
<tr>
<td>Annualized stock return from one year before fraud to</td>
<td>4.20%</td>
<td>6.95%</td>
<td>0.84</td>
</tr>
<tr>
<td>end of fraud</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Panel B: Market-Adjusted Returns</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Annualized stock return over fraud period</td>
<td>-9.55$^a$</td>
<td>-9.46$^a$</td>
<td>0.42</td>
</tr>
<tr>
<td>Annualized stock return from one year before fraud to</td>
<td>-13.03$^a$</td>
<td>-9.13$^a$</td>
<td>0.79</td>
</tr>
<tr>
<td>end of fraud</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

$^a$ Indicates the media market-adjusted return is significantly different from zero at the 0.05 level or better.
Table 6
Fraud and control firm earnings per share growth
This table presents and compares growth rates in earnings per share (EPS) from –5 to –2 years before the fraud to –2 to –1 years before the fraud for 29 matched fraud firm-control firm pairs.

<table>
<thead>
<tr>
<th>Measure</th>
<th>Fraud Firms</th>
<th>Matched Firms</th>
<th>p-value of $H_0$: Median paired difference = 0</th>
</tr>
</thead>
<tbody>
<tr>
<td>Annual EPS growth rate ($t = -2, -1$)</td>
<td>5.00%</td>
<td>13.66%</td>
<td>0.05</td>
</tr>
<tr>
<td>Annual EPS growth rate ($t = -5, -2$)</td>
<td>18.24%</td>
<td>13.45%</td>
<td>0.22</td>
</tr>
<tr>
<td>p-value of $H_0$: Median paired change from growth rate ($t = -5, -2$) to growth rate ($t = -2, -1$)</td>
<td>&lt;0.01</td>
<td>0.46</td>
<td></td>
</tr>
</tbody>
</table>