Managerial Incentives and Corporate Fraud: The Sources of Incentives Matter

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ABSTRACT

Operating and stock return results imply that managers that commit fraud likely anticipate large stock price declines if they do not misreport earnings. Stock price declines cause greater losses for managerial stockholdings than for option holdings because of differences in payoff convexity. Fraud firms have significantly greater incentives from unrestricted stockholdings than control firms do, and unrestricted stockholdings are the largest source of incentives at fraud firms. Collectively, these results emphasize the importance of the shape and vesting status of managerial incentive payoffs in providing incentives to commit fraud. Fraud firms also have characteristics that suggest a lower likelihood of fraud detection, which implies lower expected costs of fraud. Overall, the results are consistent with the economic theory of crime.

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ABSTRACT

Operating and stock return results imply that managers that commit fraud likely anticipate large stock price declines if they do not misreport earnings. Stock price declines cause greater losses for stockholdings than for option holdings because of differences in payoff convexity. Fraud firms have significantly greater incentives from unrestricted stockholdings than control firms do, and unrestricted stockholdings are the largest source of incentives at fraud firms. Collectively, these results emphasize the importance of the shape and vesting status of managerial incentive payoffs in providing incentives to commit fraud. Fraud firms also have characteristics that suggest a lower likelihood of fraud detection, which implies lower expected costs of fraud. Overall, the results are consistent with the economic theory of crime.

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1. Introduction

Equity-based incentives are a powerful tool that can mitigate the agency problems inherent in the separation of ownership and control. Aligning managers' interests with those of shareholders encourages effort, efficient investment policies, and a long-term perspective. Equity-based incentive plans, however, can also produce unintended consequences in certain states of the world (e.g., Yermack (1997), Bertrand and Mullainathan (2001)). We analyze corporate fraud as an undesirable outcome associated with managerial incentives and ask a basic empirical question: Do the executives who commit fraud face greater financial incentives to do so? The question is motivated by Becker's (1968) economic theory of crime framework, in which agents commit crime because the expected utility of the payoff exceeds the expected disutility of getting caught and punished.

We focus on two key dimensions of managerial incentives that may relate to the incentives to commit fraud. First, incentives differ in the shape of the relation between payoffs and stock price. Payoffs from restricted and unrestricted stock are linear in stock price, whereas payoffs from vested and unvested options are convex. Although the benefits from stock appreciation are similar for both types of incentives, managers with primarily linear incentives incur larger losses from sharp share price declines than managers with primarily convex incentives do. Thus, managers with larger linear incentives may be more likely to commit fraud in an attempt to avoid severe price declines. This is important because, as we report later, fraud firms experience statistically zero fraud-period raw stock returns and large share price declines when the frauds are revealed. Second, incentives differ with regard to vesting restrictions. Assuming that frauds will eventually be reversed via future accounting transactions or will be revealed, managers only benefit from fraud to the extent that they can exercise options and sell stock before the fraud is reversed or revealed. Thus, incentives from vested or unrestricted sources, which offer short-term opportunities for profit, should be more important in providing incentives to commit fraud than incentives from unvested or restricted sources.

Our sample consists of firms that are subjects of Securities and Exchange Commission's (SEC) Accounting and Auditing Enforcement Releases (AAERs) from 1991-2005. We focus on AAERs in which the SEC believes that there is sufficient evidence of accounting fraud to bring a case against a firm or its executives. For ease of exposition, hereafter we refer to these firms as fraud firms. For the executives at each fraud firm, we compare the financial incentives from stock and option holdings to similar metrics for executives at industry- and size-matched control firms that are not subjects of AAERs.

We first compare stock and operating performance measures for fraud and control firms. Growth in earnings per share decelerates in the year before the frauds begin and is significantly lower than in prior years for both fraud and control firms, which suggests that they face negative shocks to operating performance. Fraud firms earn statistically zero raw stock returns over the fraud period, and stock price declines an average of 23 percent for fraud firms around the first disclosure of potential fraud. Both the operating performance and stock return results suggest that executives who commit fraud are likely motivated by a desire to avoid large share price declines.

We next examine incentives and find that fraud executives have larger incentives that are linear in stock price, which would experience greater losses from large stock price declines. Consistent with the hypothesis that linear incentives and vesting status influence the likelihood of fraud, we find that incentives from unrestricted stock for the median fraud executive are 54% greater than those of the median control executive; at the 75th percentile, they are 84% greater. Relative differences across fraud and control firms in incentives from other sources are smaller in magnitude. Unrestricted stock is also the largest incentive source at fraud firms, whereas vested options are the largest source at control firms. Controlling for corporate governance, firm, and CEO characteristics, the likelihood of fraud is positively related to incentives from unrestricted stock, and is unrelated to incentives from vested options, unvested options, and restricted stock. Thus, executives at fraud firms face significantly greater financial incentives to prop up share prices compared to executives at control firms, and these incentives are linear

in stock price and unrestricted (or vested). Comparing a direct convexity measure confirms that fraud executives' incentives are less convex than control executives' incentives.

Unlike the incentive measures, which are direct measures of the potential benefits of fraud, we do not have direct measures of the expected costs. We do, however, find differences in two firm characteristics that suggest that executives at fraud firms face lower expected costs of committing fraud. Ceteris paribus, expected costs are lower if the probability of getting caught is lower. We find that fraud firms have significantly greater pre-fraud sales growth, which suggests that more of their value stems from growth opportunities. It is more difficult to monitor firms with greater growth opportunities (Demsetz and Lehn (1985), Smith and Watts (1992)), which may reduce the likelihood of getting caught and thus reduce the expected costs of committing fraud. We also find that fraud firms have a greater fraction of insiders on the audit committee. If insiders' greater representation increases their influence on the audit committee, it should reduce the likelihood that the committee and the auditors that they hire discover fraud events. These results suggest that executives who choose to commit fraud face a lower probability of getting caught.

Since our evidence suggests that executives at fraud firms face greater expected benefits and lower expected costs of committing fraud, we also examine whether these executives profit from the frauds. During fraud years, the median fraud executive sells approximately $3.8 million of stock, which is significantly greater than the $2.3 million figure for control executives; the respective figures for CEOs are $6.5 million and $2.8 million. This result, coupled with the greater incentives from unrestricted stock, supports Bar-Gill and Bebchuk's (2003a, 2003b) claim that the ability to sell stock provides an incentive to commit fraud, and complements Dittman and Maug (2007) who show that the optimal incentive contract should consist of salary and *restricted* stock. Our findings imply that firms who use large amounts of restricted stock should consider longer vesting periods and greater fraud prevention measures as restricted shares vest.

Collectively, the results tell a consistent story. Operating and stock return results imply that executives at fraud firms likely anticipate large stock price declines if they do not fraudulently prop up earnings. Executives with large stockholdings suffer greater financial losses from stock price declines than executives with large option holdings because of the linearity (convexity) of stock (option) payoffs. Our results show that fraud firms have significantly greater incentives from unrestricted stockholdings than control firms do, and that unrestricted stockholdings are the largest source of incentives at fraud firms. Working in conjunction with the stronger incentives, fraud firms have characteristics that suggest a relatively low likelihood of getting caught committing fraud. Finally, fraud executives profit from their frauds (and potentially their knowledge of future negative returns when the accounting irregularities are reversed) by selling significantly more unrestricted stock in the fraud period than control executives do.

Our results are consistent with Becker's (1968) economic theory of crime, in which people commit crime because the expected utility of the payoff exceeds the expected disutility of getting caught and punished. Our financial incentive results imply greater ex ante estimates of the monetary income from the fraud and our sales growth and audit committee results imply lower ex ante estimates of the probability of getting caught. Although we do not find any differences in other governance characteristics, the audit committee has the responsibility to review accounting procedures and is likely the governance mechanism that would most influence the probability that the firm would catch and penalize an executive who commits fraud. We cannot measure any psychic income from the fraud, the probability of being convicted once caught, or the monetary equivalent of punishment. Recent high profile executives convicted of corporate fraud suffered large losses of wealth, position, and social status and received lengthy prison terms.[1] These outcomes suggest that the potential costs of punishment are high. Thus, it stands to reason that managers who commit fraud likely believe that they will not get caught or will avoid punishment if they were caught. Our results also support several recent theoretical papers that develop models that predict a relation between equity-based compensation and incentives to

[1] See, for instance, "Ebbers is Sentenced to 25 Years for $11 Billion Worldcom Fraud," *Wall Street Journal*, July 14, 2005 or "Skilling Gets 24 Years in Prison ," *Wall Street Journal*, October 24, 2006.

commit fraud (e.g., Bar-Gill and Bebchuk (2003a, 2003b), Bebchuk and Fried (2003), Goldman and Slezak (2006), Robison and Santore (2004), Chesney and Gibson-Asner (2004)).

Our results contrast with the results in Erickson, et al. (2006), who conclude that there are no significant differences in equity-based incentives across fraud and other firms. Our research design and econometric analysis differ from theirs on two key dimensions. First, they sum incentive measures across executives per firm, which is likely problematic in cases for which fraud firms and control firms have incentive compensation data for different numbers of executives. The numbers of executives differ across fraud and control firms for 38% of the fraud-firm years in our sample, so we compute a *mean* incentive measure per firm to address this problem. Second, Erickson, et al. estimate *unconditional* logistic regressions using their matched-pairs sample, which produces inconsistent and asymptotically biased parameter estimates (Manski and Lerman (1977), Palepu (1986)). In contrast, we estimate conditional logistic regressions that explicitly recognize the conditional nature of the matched-pairs sample. Ignoring the summing problem or the logistic regression specification problem (or ignoring both simultaneously) yields results similar to those in Erickson, et al.

Our focus on cases of misreporting that the SEC chose to prosecute complements studies that use broader samples of restating firms (Burns and Kedia (2006)) and securities class action lawsuits (Peng and Roell (2007)), the overwhelming majority of which the SEC did not prosecute. Using broader samples, these studies find significantly greater stock option incentives for misreporting or lawsuit-target firms compared to "innocent" control firms, but not significantly greater unrestricted stock incentives as we find for the AAER sample. The combined results in our study and these studies imply that both options and unrestricted stock can provide incentives to misreport. Importantly, our results suggest that replacing option grants with stock grants will not necessarily reduce or eliminate incentives to misreport. Gao and Shrieves (2002), Bergstresser and Philippon (2006), and Effendi, et al. (2007) also study the relation between earnings management or misreporting and CEO incentives, but we cannot compare our results to theirs because they do not examine the effect of unrestricted stock separately.

The next section of the paper describes our data and methods. Section 3 contains the main results, followed by the conclusion in Section 4.

2. Sample and Methods

2.1. SAMPLE

To generate a sample of fraud firms, we begin with the set of firms in Standard and Poor's (S&P) ExecuComp database. We search the Securities and Exchange Commission's (SEC) set of Accounting and Auditing Enforcement Releases (AAERs) for the ExecuComp firms. AAERs represent cases where the SEC believes that there is sufficient evidence of accounting or auditing problems to bring a case against a firm or its executives. We focus on AAERs in which the SEC believes that there is sufficient evidence of accounting fraud to bring a case against a firm or its executives. For example, we omit cases in which the SEC charges a firm with having inadequate controls in place to prevent embezzlement by non-executive employees, and cases in which the SEC charges non-executive employees at foreign subsidiaries with bribery of foreign officials or foreign customers. We view these types of cases to be inappropriate for studying the relation between executive compensation and corporate fraud.

A particular fraud event can occur over several years, and the time span between a fraud event and the filing of an AAER can range from zero to several years. We require that the fraud event occur between 1992 and 2005 regardless of the filing date of the AAER. We include fraud events that occur from 1992 to 2005 even if the event began in 1990 or 1991, if we can backfill required data from proxy statements. We omit several multiple-year fraud events that began before 1990 because we cannot backfill the data. Committing fraud over multiple years involves a decision to commit fraud at each point in time that requires the filing of a financial statement. The decisions at each point in a multi-year fraud are likely not independent, so treating each fraud year as an independent observation would likely overstate test statistics. Thus, we average the incentive measures across years for each fraud event to produce one observation per fraud. Our final sample includes 87 unique fraud events, some of which extend to multiple fiscal years.

We obtain accounting data for all Execucomp firms from the S&P Compustat database. For each fraud firm, 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.[2] A fraud firm cannot serve as another fraud firm's control, regardless of the year of the fraud. 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 68% of the pairs, we find a match by size within 30% of the fraud firm. In regressions later we explicitly control for differences in firm size to account for any imperfections in the matches. Our matching approach allows us to compare the fraud firm's compensation level and structure to that for a firm in the same industry that is approximately the same size. Most firms set their executives' compensation levels based on the levels for same-size firms in the same industry. We examine paired differences between the fraud and control firms. The matched pair differencing approach implicitly controls for unobservable factors that are similar across same-size firms in the same industry and also should be relatively insensitive to potential nonlinearities in any relations with such unobservable factors.

Although we choose matches based only on sales and industry, Table I shows that the fraud firms and control firms also match closely on total assets, market capitalization of equity, and market-to-book ratio. None of these firm characteristics differs significantly across fraud and control firms. Pre-fraud three-year sales growth is significantly greater for fraud firms than for control firms. To the extent that rapidly growing firms derive more value from intangible growth opportunities, which are more difficult for outsiders to monitor than assets in place (Demsetz and Lehn (1985), Smith and Watts (1992)), it may be easier to commit fraud without getting caught in rapidly growing firms. The sales growth result is consistent with this view. Later, we also examine and control for differences in firms' governance

[2] Our requirement that both fraud and matched firms be in ExecuComp is an important difference in research design from Erickson, et al. (2006). Cadman, et al. (2006) find significant differences across ExecuComp and non-ExecuComp firms in return on equity, return on assets, market to book, market value of equity, total assets, leverage, average stock returns, and stock return volatility. Thus, comparing ExecuComp firms with non-ExecuComp firms risks making comparisons of firms that differ significantly on many important firm characteristics that are likely related to optimal types and levels of incentive compensation.

characteristics that may also affect the probability that an executive is caught and penalized for committing fraud.

[Table I about here]

It is well known that many executives manage or manipulate their firms' earnings legally within Generally Accepted Accounting Principals (GAAP), so our control firms may also manage or manipulate earnings within GAAP. By using AAERs to identify fraud firms, we focus on cases in which executives cross the GAAP threshold to provide fraudulent information to investors. This distinction is important because investors cannot use accounting rules to infer true economic earnings from false information as they could if executives merely manipulated earnings within a known set of rules. Additionally, there are no criminal or civil penalties for earnings management within GAAP, but there are potentially significant penalties stemming from SEC enforcement actions. Thus, we believe that crossing this threshold is significant. Only 18% of the restatement sample in Burns and Kedia (2006) were under SEC investigation. Karpoff, Lee, and Martin (2006) report that 41% of restatements result in enforcement actions. If crossing the threshold is not viewed as significant by executives, 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 or have not yet gotten caught. Such cases should also create a bias against finding any differences in incentives across fraud firms and control firms.

2.2. INCENTIVE MEASURES

Our primary hypotheses focus on differences in vesting status and differences in the convexity of payoffs from different incentive sources, so our incentive measures should directly incorporate these differences. Thus, we compute incentives separately for restricted stock, unrestricted stock, vested options, and unvested options. To measure incentives, it is common to compute dollar changes in executive stock or option values for a one-percent increase in stock price (see, e.g., Core and Guay (2002)). Computing changes in values for a one-percent increase in stock price, however, will not capture

the effects of differences in convexity across stock and option payoffs very effectively. Furthermore, to measure incentives to commit fraud, the measure should quantify the difference between a manager's wealth levels if he commits fraud versus if he does not. It seems unlikely that a manager who chooses to commit fraud expects only a one-percent difference in stock price from doing so.

One way to think about the incentive measurement problem is as follows. Assume that a firm's manager privately observes the firm's true economic performance before it is reported publicly, and then must decide whether to report it truthfully or cheat by reporting an inflated value. He knows the distributions of stock price conditional on the reported economic performance and also the values of his stock and option holdings at each stock price. His financial incentive to cheat should be the difference between the stock and option values at the stock price conditional on cheating and their values at the stock price conditional on reporting truthfully. Unfortunately, we cannot observe the manager's private views of the conditional distributions of stock price, so we cannot compute the incentives exactly. To simplify things, assume that the conditional distributions of stock price degenerate, so that the manager knows the stock price as a single point given a reported level of economic performance.[3] After the manager reports the inflated (untrue) economic performance, but before the market learns that he has cheated, the stock price should reflect the inflated performance. Assuming that the stock price before the market learns of the fraud reflects misreported performance and that the post-fraud-revelation stock price reflects the true performance, we can estimate the difference between them by examining the effect on stock price of the revelation of the fraud to the market. We can then map this difference into the differences in the manager's stock and option values that result from cheating. To conduct such an analysis, we need to

[3] To compute the manager's financial incentives without assuming that the conditional distribution of stock price degenerates, one would integrate the option and stock values across the distributions of stock price conditional on misreporting economic performance and reporting true economic performance, and then compute the incentives from those values. Unfortunately, there is no obvious way to estimate what the conditional distributions look like for each fraud firm. However, if our results hold for any (reasonable) assumed change in stock price, then they should hold for any (reasonable) distribution of stock price changes. We implement our procedure of using a single hypothetical stock price change for various changes ranging from −50% to +50% and obtain results similar to those reported in the paper.

examine the stock return performance during fraud period and when the fraud is revealed to the market. We also examine operating performance to better understand the motives and incentives to commit fraud.

We first compare stock returns across fraud and control firms both before and during the fraud periods. AAERs typically specify fiscal years instead of calendar days to define the fraud period, so we compute returns based on the fiscal years of the fraud firms. We measure fraud-period 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 or the public revelation of the fraud, whichever is first. We also compute pre-fraud returns beginning three years before the fraud until the year before the fraud begins, and separately for the year immediately preceding the start of the fraud. As a robustness check, we examine returns beginning in year −5 and −4 and find similar results to those reported for the window beginning in year −3. For firms that have return data for a partial year (n days), we annualize returns to a 250-trading-day year by taking the n^{th} root of one plus the holding period return and compounding by 250 days. The results are not sensitive to including firms that do not have complete full-year return data.

Panel A of Table II contains median annualized raw and market-adjusted returns. We focus discussion on the raw returns because payoffs for executive stock and options are rarely adjusted for market returns. Prior to the beginning of the fraud, the median fraud firm realizes an annual stock return of 29.6% from year −3 to −1 compared to 21.2% for the median matched firm. In the year immediately preceding the start of the fraud, the median fraud firm's stock has a 21.1% return compared to 14.1% for the median matched firm. These returns do not differ significantly across the two groups. The median fraud firm generates an annual raw return of −3.5% during their fraud years, which is not significantly different from zero. The median control firm generates an annual return of 6.4% during the fraud years, which is significantly positive at the 0.10 level. The median returns during the fraud periods differ significantly across fraud and control firms at the 0.01 level.

[Table II about here]

We subtract the return on the value-weighted CRSP index to adjust the raw returns for market performance. From year −3 to −1, the median fraud firm generates a significantly positive (at the 0.10 level) annual excess return of 8.8%, whereas the median control firm generates a statistically insignificant excess return of 3.4%. In the year immediately prior to the start of the fraud, the median fraud firm continues to generate a significantly positive market-adjusted return (9.0%), while the median matched firm continues to earn a statistically insignificant market-adjusted return (1.2%). Fraud firms have a median annual market-adjusted return during the fraud period of −9.5%, which is significantly negative at the 0.05 level. Control firms have a median annual market-adjusted return of –2.1%, which is statistically insignificant.

We use the Lexis/Nexis system to search newswire articles for the first public disclosure of information that suggests a potential fraud and use the event study methodology of Patell (1976) to estimate the stock price reaction. Panel B presents the results of this analysis. We find a mean cumulative raw return from one day before the announcement to one day after of −14.9%, which is significantly different from zero at the 0.01 level. Additionally, 80% of the firms experience a negative reaction upon the announcement of information that suggests a potential fraud. News about these events frequently does not occur on a single day, so we also compute a cumulative return from day −5 to +30 around the initial announcement. The mean cumulative raw return over this period is −22.7%, which is also significant at the 0.01 level. Results based on market-adjusted returns are close in magnitude to the respective raw returns.[4]

[4] The hypothesis that executives commit fraud in part to try to avoid losses on equity-based compensation relies on the implicit assumption that fraud props up stock prices. If investors are sufficiently informed to know that financial statement information is incorrect and value stocks based on correct but unreported information, then they should not revalue stocks when restatements are announced. In addition to our finding of significantly negative returns in reaction to news about potential frauds, Anderson and Yohn (2002) and Palmrose, et al. (2001) report large negative excess returns to restatement announcements stemming from fraud. The magnitudes of these downward revisions in price when investors receive corrected financial statement information imply that pre-restatement stock prices were based on faulty information and were artificially high. An executive who sold shares before the restatement occurred would have gained from his fraudulent behavior.

Given the poor stock return performance over the fraud periods, we next examine whether fraud executives would have had any advance warning of the low returns. 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. Such stock price penalties reduce the value of executives' holdings of stock and stock options, so executives have incentives to avoid reductions in EPS growth.

In Panel C of Table II we compare earnings per share growth across fraud firms and control firms in the years preceding the frauds. Fraud firms have a median annual growth in EPS from years –5 to –2 of 6.0%. The median annual growth rate falls to −12.7% for the year immediately preceding the frauds (from years –2 to –1). The reduction in EPS growth rates is statistically significant at the 0.01 level. Control firms also see a significant deceleration in median annual growth in EPS, falling from 7.6% from years –5 to –2 to −3.9% from years –2 to –1 (p-value = 0.09). The EPS growth rates do not differ significantly across the fraud and control firms in either time period.

The results above show that managers at fraud firms see a negative shock to pre-fraud operating performance and imply they likely expect large stock price declines if they do not commit fraud. Incentive measures should reflect this expectation, incorporating both the inflated stock price that reflects misreported economic performance and the stock price that reflects true economic performance. We can use the stock price reaction to the revelation of the fraud to measure the difference between the stock price conditional on the inflated performance and the stock price conditional on the true performance, and then use this difference to compute the differences in the manager's stock and option values that result from cheating. Although optimally we could pair each firm's own event study return with its stock price to compute the differences, limitations in dating the first revelation of frauds and confounding events for some firms make this impractical. Thus, we use the mean event study raw return, which we round to −23%, in combination with each firm's stock price to generate a proxy for the stock price conditional on true economic performance. We then estimate the financial incentives to cheat as the difference between

the stock and option values at the stock price conditional on inflated economic performance and the values at the proxy for the stock price conditional on true economic performance.

Specifically, we compute the fraud-related changes in values for executives' restricted and unrestricted stock holdings as the values at the then-current stock price minus the values at that stock price times (1 − 0.23). We compute the fraud-related change in the values of the vested and unvested stock option holdings as the option values at the then-current stock price minus the option values at that stock price times (1 − 0.23). To improve comparability with other studies, we rescale these measures to be comparable to a one-percent change by dividing by 23 and also report them as positive figures. The figures presented in the paper are based on these standardized incentive measures and are comparable to those in other studies, except that they explicitly incorporate the convexity in payoffs. We compute alternative incentive measures based on the more common 1% *increase* in stock price and find results qualitatively similar to those reported in the paper.

Consistent with previous research (e.g., Marquardt (2002)), we adopt a modified version of the Black-Scholes model to compute the option values. Executives typically exercise their options before maturity, so 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 past three years as a proxy for the future dividend yield. For newly granted options, strike price and 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 maturity of previously granted options.

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. These two factors are not mutually exclusive. Because either or both factors could encourage fraud, we study both group incentives and individual CEO incentives.

To measure group incentives, it is important to understand how ExecuComp reports executive compensation data. SEC regulations require that firms disclose compensation information for the top five executives. ExecuComp can report data for more or fewer than five executives for a particular firm for a particular year for several reasons. First, a firm can report more than five executives in the summary compensation table of its proxy statement if two or more executives are tied on compensation rank for the top five places. Alternatively, some firms report data for fewer than five executive officers because they have fewer than five. Second, if a new CEO takes over during the fiscal year ExecuComp reports data on both the new and former CEOs. Third, the summary compensation tables in proxies report compensation data for the current and two prior years. ExecuComp uses data reported in a particular proxy year to backfill other years for executives who are in the top five for a given year, but were not necessarily top five in the backfilled years. This procedure can result in substantially more than five executives per firm-year in ExecuComp.

To avoid erroneously including non-top-five executives in our analysis, we hand collect the identities of the true top-five executives for each year from the fraud and control firms' proxy statements for the fraud periods. We then use this data to exclude those executives who show up in a particular year solely because ExecuComp backfilled their data. Researchers can use some variables on ExecuComp to identify top-five executives correctly for *some* firms, but we could not find a single variable or combination of variables that produced accurate classifications for all firms in our sample. To our knowledge, our study is the only one that controls for this data problem. Given that it is infeasible to hand collect the true top-five executives for all ExecuComp firms for all sample years, this also means that we cannot be confident in a comparison of fraud firms to all other ExecuComp firms.

Erickson, et al. (2006) base their main incentive measure on sums. Comparing summed incentives across the fraud and control firms seems invalid, however, for cases in which there are

different numbers of executives at a fraud firm and its matched control firm because the sums are over different numbers of executives. For 38% of the fraud firm-years in our sample, the fraud and control firms report data for different numbers of executives. To avoid comparison problems created by summing incentives across different numbers of executives, we compute the *mean* incentive for executives at each firm to measure group incentives. We can compare means across fraud and control firms even if they have differing numbers of executives. We also study the individual CEO's incentives. The CEO is typically the most powerful executive at a firm, and can potentially pressure others to engage in fraud.

2.3. EXPECTED COST MEASURES AND CONTROL VARIABLES

In addition to incentive differences, executives at fraud firms may also face different expected costs in committing fraud, which should be important in the decision of whether to commit fraud according to Becker's (1968) framework. In contrast to the incentive measures that directly measure potential benefits, we do not have direct measures of the probability of getting caught and convicted or the monetary-equivalent costs of punishment. Firms with weaker governance, however, may be less likely to detect fraud and penalize managers who commit fraud, so we test whether fraud firms have weaker governance. We discuss later in the paper the details of the governance measures and other firm and CEO characteristics we examine.

3. Results

3.1. MANAGERIAL INCENTIVES FOR FRAUD AND CONTROL FIRMS

Table III presents statistics that compare incentive measures for the top five executives (Panel A) and CEOs (Panel B) at fraud and control firms. Extreme values skew the means significantly, so we present and discuss results for medians. We use signed-rank tests to test whether the median of the paired differences is significantly different from zero. We measure incentives at different points in time, so we use the consumer price index to express all values in 2005 dollars.

Three main findings emerge from the analysis in Table III.[5] First, Panel A of Table III shows that executives at fraud firms have significantly larger incentives from unrestricted stock holdings (*p*-value = 0.02) and from unvested options (*p*-value = 0.08). The *p*-values for restricted stock (*p*-value = 0.26) and vested options (*p*-value = 0.44) imply no statistical differences in incentives from these sources across fraud and control firms. The results in Panel B for CEOs are similar except that the *p*-value for unvested options is 0.13.

[Table III about here]

Second, the relative magnitudes of the incentives vary across the different sources. The largest incentives for fraud firms stem from holdings of unrestricted stock. Specifically, at the median, incentives for fraud executives from unrestricted stock are $51,813, from restricted stock are $24, from vested options are $35,503, and from unvested options are $34,122. Moreover, incentives for fraud executives from unrestricted stock are significantly greater than their incentives from vested options (*p*-values are 0.03 for top five executives and 0.02 for CEOs based on own-firm paired differences between the incentive sources). By contrast, incentives from vested options are the largest source for control executives and there are no significant differences in the incentives from unrestricted stock and vested options (*p*-values are 0.51 for top five executives and 0.49 for CEOs based on own-firm paired differences).

Third, the largest differences across fraud and control firm executive incentives stem from holdings of unrestricted stock. At the medians, the incentives from unrestricted stock holdings are 54% greater for fraud firms than for control firms. The relative differences are even greater at the 25th (98%) and 75th (84%) percentiles. By contrast, the relative difference between fraud and control firms at the median is 8% for unvested options, the other incentive source that differs across fraud and control firms. The pattern of the results in Panel B for CEOs is similar.

[5] The results are qualitatively similar when we compute the incentive measures based on hypothetical stock price changes ranging from −50% to +50%. Thus, the results do not depend on our use of the mean event sturdy return of −23% to compute incentives.

The results that the incentive differences stem primarily from unrestricted stockholdings instead of vested options and that unrestricted stockholdings represent the largest source of incentives for fraud firms are somewhat surprising given the implication of stock option compensation in recent corporate frauds by the popular and financial press. The result is, however, consistent with the view that greater incentives from linear sources (e.g., stockholdings) coupled with the ability to sell shares provides managers greater incentives to commit fraud if they otherwise face potentially large share price declines.

The quartile values in Table III for the fraud and control firms, respectively, are not necessarily matched pairs. Untabulated results show that differences across the groups are similar to those reported in Table III when we compute quartiles based on paired differences. Specifically, the largest median paired difference among the incentives sources is for unrestricted stock incentives—the median paired difference in unrestricted stock incentives is 2.9 times the median paired difference in incentives from unvested options and 13.8 times the median paired difference in incentives from vested options (the median paired difference for restricted stock is zero, so we cannot compute a corresponding multiple).

As reported in the previous section, stock return and operating performance results suggest that the frauds in our sample follow negative shocks to operating performance and that the revelation of fraud results in large stock price declines. Control firms experience negative operating shocks similar to those of fraud firms, but only executives at fraud firms, with their significantly larger incentives from unrestricted stocks, choose to commit fraud. In this section, the univariate analysis of incentives shows that executives at fraud firms face significantly greater financial incentives to commit fraud than their counterparts at control firms, and that these differences are driven primarily by incentives from unrestricted stockholdings. Furthermore, unrestricted stock holdings represent the largest incentive source for executives at fraud firms. Bar-Gill and Bebchuk (2003a, 2003b) argue that the ability to sell stock provides an executive with the incentive to commit fraud to hide bad news. Although we cannot know what executives were thinking when they chose to commit fraud, avoiding declines in stock price would be consistent with our findings that the primary differences in incentives stem from unrestricted

stock holdings. The linear payoff structure of stock holdings means that a given decline in stock price will cause greater losses on stock holdings than on options that are not very deep in the money.

3.2. OTHER FACTORS POTENTIALLY ASSOCIATED WITH FRAUD

The analysis thus far focuses on whether fraud executives face greater financial incentives to commit fraud. The commission of a fraud also requires opportunity and an assessment by the executive that the expected costs of committing the fraud are less than the expected benefits generated by the financial payoffs. Evidence suggests that governance can mitigate unintended consequences associated with incentive compensation, for instance rewarding CEOs for luck (Bertrand and Mullainathan (2001)). In principle, strong governance mechanisms could increase the likelihood that an executive who commits fraud is caught and/or penalized, which should increase the expected costs the executive considers when deciding whether to commit fraud.

The governance literature suggests various proxies for the strength of a firm's governance mechanisms. Lipton and Lorsch (1992) and Jensen (1993) argue that larger boards are less effective. Yermack (1996) and Eisenberg, et al. (1998) report evidence consistent with that argument. Weisbach (1988) finds a positive relation between CEO turnover following poor performance and the fraction of outside directors. Rosenstein and Wyatt (1990, 1997) find a positive stock price reaction to the election of outside directors, and a negative (positive) reaction to the election of inside directors when their stock ownership is low (high). These two studies suggest that the fraction of outsiders on a board is important. Another potential indicator of governance strength is whether the CEO holds the chair of the board position (Hermalin and Weisbach (1998)). Dahya, et al. (2002) find higher CEO turnover following poor firm performance for firms with separate CEOs and board chairs and at least three outside directors. These firms also exhibit improved operating and stock price performance. Brickley, et al. (1997) find that dual CEO-board chairs hold significantly more stock than non-chair-CEOs. Deli and Gillan (2000) provide evidence on the role of audit committee independence and the number of audit committee meetings. Vafeas (1999) finds that the number of board meetings is an indicator of governance strength.

Gompers, et al. (2003) and Bebchuk, et al. (2004) also provide indexes designed to measure the strength of shareholder rights, which should be directly related to the strength of a firm's governance.

Based on the studies discussed above, we test for differences in the board size, the percentage of insiders serving on the board, the number of board meetings, the percentage of shares held by outside blockholders, the size of the audit committee, the percentage of insiders serving on the audit committee, and the number of audit committee meetings. We also test whether fraud firms are more likely to have the CEO hold the chairman of the board position. We collect these data from firms' proxy statements.

The results are in Table IV. Boards of directors at fraud firms meet slightly more often than those at control firms (7.5 times per year versus 6.0, *p*-value = 0.07), and fraud firms have a greater percentage of insiders on the audit committee than control firms do (*p*-value = 0.10). There are no other significant differences in governance measures across fraud and control firms at conventional significance levels. Vafeas (1999) finds that boards meet more frequently following declines in operating performance. As we show later, the EPS growth is negative in the year preceding a fraud. The more frequent meetings by boards at fraud firms are likely a response to the slowdown in EPS growth. Although overall board characteristics (e.g., board independence) indicate that the boards of fraud firms and matched firms have similar attributes in general, the higher incidence of insiders on the audit committee suggests that fraud executives face lower expected costs in committing fraud than control firm executives do. The audit committee is charged with the oversight of accounting procedures (see Deli and Gillan, 2000) and would seem to be the most direct governance measure related to fraud detection. The lack of differences in the other governance mechanisms indicates that those mechanisms do not create differences in the likelihood of getting caught committing fraud across fraud and control firms.

[Table IV about here]

The need for additional financing and the desire to obtain it on terms more favorable than terms that reflect the true risk and performance of the firm potentially provides another motivation to commit fraud. Alternatively, managers might commit fraud to avoid financial distress and/or default under debt

covenants based on accounting measures. Following Dechow, et al. (1996), we construct the following measure to indicate the need for external financing: (cash from operations – mean capital expenditures) / current assets, where capital expenditures are averaged over the prior three years. The less this measure, the more likely the firm will need external financing. As a rough proxy for the likelihood of financial distress and/or violating debt covenants, we construct a leverage measure defined as the sum of short and long-term debt divided by total assets. Results of median paired-difference tests for these measures are also in Table IV. Consistent with the view that an external financing need can motivate fraud, the median financing need measure is 0.02 for fraud firms, which is significantly less (p-value < 0.01) than the median of 0.13 for control firms. The median leverage for fraud firms is 0.21, which does not differ significantly from the median of 0.19 for control firms.

In Table IV we also examine differences in CEO characteristics across fraud and control firms. CEOs with a longer tenure may have a greater ability to influence other executives and employees to help commit fraud. A CEO closer to retirement age or who serves on fewer boards of directors may feel he has less to lose in reputation damage if caught committing fraud, and thus be more likely to commit fraud.[6] Anecdotal accounts in the popular press suggest that firm founders have high levels of risk tolerance and aggressiveness, which arguably could increase the likelihood of committing fraud. We find no significant differences in any of these CEO characteristics.

3.3. CONDITIONAL LOGISTIC REGRESSIONS

Given that we find some differences in non-incentive characteristics across fraud and control firms, we next estimate a logistic regression that models the probability of a fraud firm as a function of incentive measures and governance, firm, and CEO characteristics presented in Table V. We use the matched pairs sample in this analysis instead of the full ExecuComp population. Using a matched-pairs sample produces more efficient parameter estimates than would using the full ExecuComp population, given the very small fraction of fraud firms in the population (Manski and Lerman (1977), Manski and

[6] Fitch and Shivdasani (2006) find that outside directors lose 50% of their directorships, on average, following shareholder class action lawsuits charging violations of financial fraud in violation of SEC rule 10(b)-5.

McFadden (1981), Palepu (1986)). Using matched pairs implicitly controls for industry, firm size, and industry growth opportunities, but because the matches are imperfect, we include explicit controls for firm size and growth opportunities. For growth opportunities, we include the pre-fraud three-year sales growth measure. We include net sales as the firm size proxy, but reported results are similar using total assets or market value of common equity as size measures.

The matched-pair nature of our data dictates that we estimate a conditional logistic regression that explicitly recognizes the conditional nature of the probabilities that a matched-pairs sample creates (i.e., it models the probability of being a fraud firm conditional on the fact that one of the firms in each pair is a fraud firm). Using an unconditional logistic regression on a matched-pairs sample produces inconsistent and asymptotically biased parameter estimates (Maski and Lerman (1977), Palepu (1986)). Given that we use a conditional logistic regression, our results are not comparable to the unconditional logistic regression results in Erickson, et al. (2006). When we ignore the conditional nature of the probabilities created by the use of a matched pairs sample and estimate an unconditional logit regression, we find results similar to those in Erickson, et al. (2006), i.e., none of the coefficients on the incentive measures is statistically significant.

The results of the logistic regressions that model the probability of fraud are in Table V.[7] In the first column of figures we include all test and control variables except the Gompers, Ishii, and Metrick (2003) governance index because that index is not available for many pairs of firms.[8] When we attempt to estimate the full regression specification over the subsample of firms for which the governance index is available, the maximum likelihood estimation does not converge. Thus, in the second column of figures, we estimate a parsimonious model that contains the incentive measures, the governance index, and only those control variables that are statistically significant in the full model. The regression in the third column also includes the governance index, but omits the incentive measures that are not statistically

[7] The parameter estimates do not include an intercept because it is impossible to identify (uniquely) an intercept in a matched pairs conditional logistic regression.
[8] We also use the entrenchment index proposed by Bebchuk, Cohen, and Ferrell (2004) and obtain similar results.

significant in the first regression. Given that many of the control variables are CEO specific, we report results based on regressions using incentives for CEOs only; unreported results using incentives for top five executives are similar to those reported.

[Table V about here]

The most striking finding from the conditional logistic regressions is that only incentives from unrestricted stock are positively related to the likelihood of fraud after controlling for other potential determinants of fraud. The *p*-value for the coefficient on unrestricted stock incentives is 0.03 in the full model, 0.06 in the parsimonious model including the governance index and all incentive measures, and 0.01 in the parsimonious model that includes only variables that are statistically significant in the first regression. In contrast, the coefficient on the incentives from vested options has *p*-values of 0.62 and 0.31 in the regressions. This result contrasts with the negative implications associated with stock options in the financial and popular press following the recent scandals. It supports our conjecture, however, that linear incentives and the ability to profit in the near term provide stronger incentives to commit fraud. The fact that the probability of fraud is unrelated to incentives from restricted stock, which also has linear payoffs, suggests that the unrestricted nature of unrestricted stock is an important element of the incentives it provides to commit fraud.

The three-year prior sales growth has a significantly positive coefficient, which is consistent with the view that there is a lower likelihood of getting caught committing fraud in more rapidly growing firms. The percentage of insiders on the audit committee has significantly positive coefficients at the 0.10 level or better in the three regressions. Thus, greater insider participation on the audit committee increases the likelihood of committing fraud, which would be expected if their participation reduces the probability of getting caught. The sales growth and audit committee results are consistent with the hypothesis that executives at fraud firms face a lower probability of getting caught committing fraud. The insignificant coefficients on most other governance measures are similar to Agrawal and Chadha's (2005) findings that having in place several key governance mechanisms does not reduce the likelihood of

restatements. The (inverse) financing need variable, the leverage measure, the CEO characteristics, and the Gompers, Ishii, and Metrick (2003) governance index have insignificant coefficients. It is interesting that audit committee composition appears to influence the likelihood of fraud but that the governance index, which measures anti-takeover and shareholder rights provisions, does not.

In summary, the positive effect of incentives from unrestricted stock on the probability of fraud is robust to controlling for various firm, governance, and CEO characteristics. After controlling for these characteristics, the probability of fraud is unrelated to managerial incentives from other sources. The results are consistent with the view that greater unrestricted linear incentives increase the likelihood of fraud. The probability of committing fraud is positively related to a firm's sales growth and the fraction of insiders on the audit committee, both of which should reduce the likelihood of getting caught committing fraud and thus reduce the expected costs of committing fraud. Collectively, these results are consistent with viewing corporate fraud in the context of Becker's (1968) economic theory of crime framework.

3.4. ADDITIONAL EVIDENCE ON DIFFERENCES IN PAYOFF CONVEXITY

The fact that unrestricted stock incentives, which are linear in stock price, are the only incentive source related to the probability of fraud suggests that the convexity of incentives matters in providing incentives to commit fraud. The incentive measures in the previous sections incorporate convexity explicitly, but they also reflect differences in the scale or magnitude of the incentives. Thus, the incentive measures do not reveal whether fraud firms have incentives that are less convex than respective control firms, holding constant the scale or magnitude effect.[9] We next examine more direct evidence on the convexity effect per se.

[9] To understand the joint convexity and scale issue in our main measure, suppose that executives at fraud firms and control firms only have stock options with identical parameter values across the firms, but that fraud firms have twice as many options, i.e., the scale of the incentives is twice as large. On a per option basis, the convexity is equal across the firms because the option parameters are identical. Our convexity-adjusted incentive measures (e.g., the main incentive measures), however, will be greater for fraud firms than for control firms because they sum the dollar value of the incentives across the options. To consider convexity per se, we need to remove the scale effect.

Gamma, the second derivative of option price with respective to stock price, is a standard measure of convexity in option pricing. To form an aggregate gamma measure similar to our main incentive measures, we compute and then sum individual gammas across all the individual stock and option holdings in an executive's portfolio. Stock holdings have gammas of zero, so the summed gamma measure actually reflects only the convexity created by an executive's option holdings. This summed gamma measure also contains scale or magnitude effects. To hold constant scale effects, we divide this summed gamma measure by the corresponding deltas summed across the executive's stock and option holdings. This ratio then captures the convexity per dollar of incentives and can be compared across fraud and control executives with incentives of different magnitudes.

If fraud executives have less convex incentives, the convexity per dollar of incentives ratio should be smaller than the corresponding measure for control executives. We compute paired (fraud minus control) differences in the convexity measure, and test whether the median paired difference equals zero. Consistent with the hypothesis that fraud executives face less convex incentives than control executives, untabulated results show that the median paired difference is negative for top five executives and for CEOs with respective *p*-values of 0.10 and 0.11 for two-tailed tests. With the caveat that the results are weak statistically, they are consistent with the view that fraud executives have less convex incentives independent of scale effects, and that given a potential large negative stock return, have greater financial incentive to commit fraud.

Another way to illustrate the differences in convexity across fraud and control firms is to examine incentive measures across a wide range of stock prices. As described in footnote 3 earlier as a robustness check, we recompute the incentive measures across a range of hypothetical stock price changes from −50% to +50%. In Figure 1, we plot the respective median total incentives for fraud firms and control firms from that analysis. The figure shows that control firms' incentives fall markedly over the range of hypothetical stock price changes from 0% to −50%, whereas fraud firms' incentives are relatively flat over the entire range of price changes. Given the less convex incentives in fraud firms stemming from

their large unrestricted stockholdings, the differences between fraud firms and matched control firms widen markedly as the drop in the share price becomes larger.

[Figure 1 about here]

3.5. COMPENSATION AND PAYOFFS FOR FRAUD AND CONTROL EXECUTIVES

The prior sections show empirically that fraud executives face greater ex ante financial incentives from unrestricted stock, a lower probability of getting caught committing fraud, and apparently attempt to avoid losses created by large share price declines. In this section we examine whether fraud executives "cash in" on their frauds by generating greater payoffs and compensation than control executives during the fraud period. Table VI contains statistics to compare payoffs and compensation figures for the top executives at fraud and control firms. As with the earlier analysis, we present and discuss results for medians and use the consumer price index to express all values in constant 2005 dollars.

Using data from Thomson Financial's database of insider transactions, we collect buys and sales of stock by fraud and control executives over the period from the beginning of fraud extending through the earlier of either the end of the fraud or the first mention of the fraud in the press. We assume that most executives would avoid selling shares after the fraud became public. According to Thomson Financial, many of the forms from which they collect data contain errors, so they apply an extensive cleansing process to the data. Using cleansed data only, we collect insider transactions for 59 matched pairs of fraud and control firms for the top five executives, and 34 matched pairs for CEO-only data. For each executive we compute the difference between the dollar value of shares sold and dollar value of shares purchased. As before, for the top five measures we compute an average for the executives at each firm.

[Table VI about here]

As shown in Panel A of Table VI, the median top five fraud executive sells $3.8 million in stock over the fraud period, which is significantly greater than the $2.3 million that the median control executive sells (p-value = 0.04).[10] The comparable figures for CEOs in Panel B are $6.5 million for fraud CEOs and $2.8 million for control CEOs (p-value = 0.11). Thus, results in previous sections show that fraud executives face greater ex ante financial incentives from unrestricted stock, and the results here show that they take advantage of the fraud by selling more unrestricted stock during the fraud periods.

The value of option exercises over the fraud period for the median fraud executive is $0.8 million, which is significantly greater than the corresponding value of $0.5 million for the median control executive. Thus, although our previous results indicate no significant differences in incentives from vested options across fraud and control executives, fraud executives do extract more gains from option exercises than control executives do. The gains from option exercises are relatively small compared to stock sales during the fraud period and the pattern of option gains does not hold for CEOs. There are no reliable differences across fraud and control executives in the ratio of the number of options exercised to total vested options or the number of vested options to total options. Top five fraud executives do have a significantly greater level of moneyness than control executives (1.70 versus 1.43, p-value = 0.01), but the difference for CEOs has a p-value of only 0.15. Median salary does not differ significantly across fraud firms and control firms. Median bonus is greater for top five fraud executives than control executives (p-value = 0.08), but the corresponding difference for CEOs is not significant.

In sum, fraud executives generate greater payoffs than control executives during the fraud periods by selling stock and exercising options, with the overwhelming majority of the payoffs coming from stock sales. The evidence of greater stock sales is consistent with similar evidence for firms that restate earnings in Bergstresser and Phillipon (2006) and Burns and Kedia (2006). There are generally no economically meaningful differences in salary or bonus across fraud and control executives.

[10] Based on the *option sell indicator* in Thomson Financial's database of insider trades, we find that approximately 20% of the stock sales for the median fraud top-five executive are associated with option exercises; the corresponding median for fraud CEOs is 0%. Thus, the overwhelming majority of stock sales are not associated with option exercises.

4. Conclusions

We find that executives who commit corporate fraud face greater financial incentives to do so. Controlling for firm, governance, and CEO characteristics, the likelihood of corporate fraud is positively related to incentives from unrestricted stockholdings and is unrelated to incentives from restricted stock and unvested and vested options. Moreover, unrestricted stockholdings are the largest source of managerial incentives at fraud firms, whereas vested options are the largest source at control firms. Frauds begin following declines in operating performance, fraud firms earn statistically zero raw stock returns over fraud periods, and stock prices fall significantly disclosure of potential fraud, which suggest that frauds are attempts to avoid stock price declines. Fraud firms are also high growth firms and have proportionately more insiders on their audit committees, both of which imply a lower likelihood of fraud detection and thus lower expected costs of committing fraud. The results are broadly consistent with Becker's (1968) economic theory of crime.

Research on equity-based compensation suggests that it has substantial benefits. The timing issues in Yermack (1997) and Aboody and Kasznik (2000) and the hedging issue in Bettis, et al. (2001), together with our findings on fraud incentives, however, suggest that at least some compensation contracts can produce unexpected outcomes that likely do not maximize shareholder wealth. Several recent theoretical models also predict that in certain states of the world equity-based compensation can provide perverse incentives. Given that firms grant options and stock to provide long-term value-maximizing incentives, it is interesting to ask why for some executives the incentives to commit fraud in the short term outweigh the incentives to maximize value in the long term. The observed large negative share price reactions to fraud revelations imply that fraud does not maximize value in the long term. One potential answer is that some executives pursue fraud because they think they cannot legitimately increase value in the long term. It is difficult to envision how one could test this explanation, however, because it is difficult to see how one could measure the extent to which an executive believes he cannot legitimately increase value in the long term. Another potential answer is that fraud executives could believe that there

is a very low probability of getting caught so that pursuing a short-term fraud strategy is not inconsistent with long-term value maximization. Again, it is not obvious how one could directly test this explanation without knowing changes in the probability of getting caught, but the characteristics of fraud firms suggest that frauds are more likely to occur in firms in which there is a lower likelihood of the fraud being discovered. Both explanations suggest directions for future research.

Our results have 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 is a potentially difficult prescription because it is more difficult to monitor executives of firms with higher growth opportunities. Second, given that the primary incentive to commit fraud comes from unrestricted stock holdings, firms should consider imposing additional limits on the ability of managers to sell stock when other factors exacerbate the risk of fraud.

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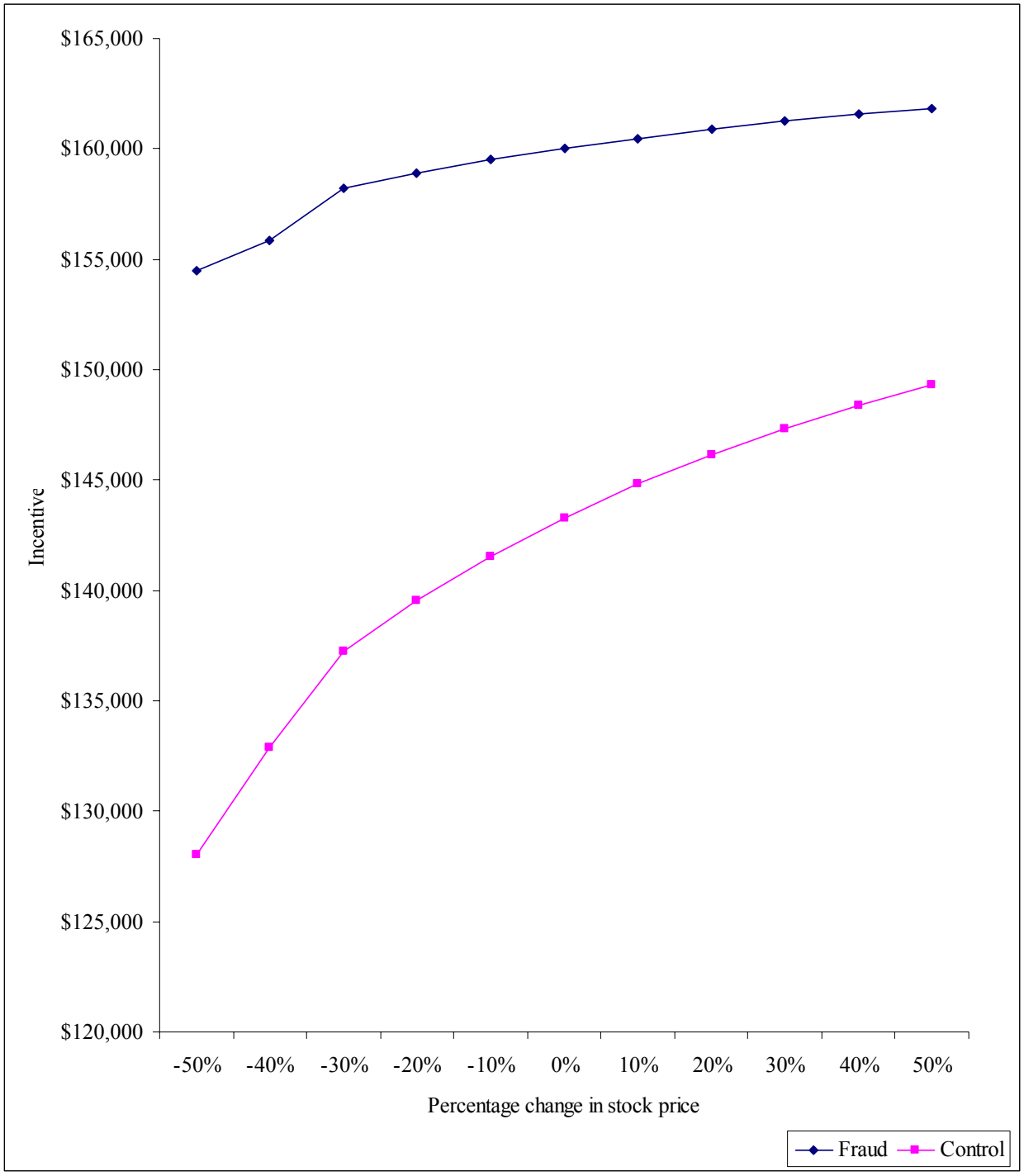

Figure 1. Median total incentives at various changes in stock price for fraud and matched firms. Incentives plotted in this figure are comparable to the main incentive measures presented in the paper, except that we compute them here over a broad range of hypothetical stock price changes.

Table I. Firm Characteristics of Fraud Firms and Control Firms

Each fraud firm has a control firm chosen to match as closely as possible on sales and industry. Each pair of firms' figures is measured the year before the fraud begins. The market to book ratio is defined as market value of assets divided by book value of assets, with market value of assets defined as book value of assets plus the market value of common stock minus the book value of common stock and deferred taxes. The sample size is 87 frauds except for sales growth which is 71 matched pairs due to unavailable data.

Measure	Median for Fraud Firms	Median for Matched Firms	p-value of H_0: Median paired difference = 0
Sales (billions)	$1.32	$1.39	0.35
Total Assets (billions)	$1.37	$1.25	0.86
Market Capitalization of Equity (billions)	$1.55	$1.78	0.96
Market to Book Ratio	1.50	1.59	0.99
Three-year sales growth	13.57%	8.25%	0.00

Table II. Stock Returns and Operating Performance during Fraud Periods

This table presents median annualized raw and market-adjusted returns for 87 pairs of fraud firms and control firms and an event study analysis of the first public disclosure of potential fraud. Market-adjusted (in brackets) returns are firm returns less value-weighted CRSP index returns. Panel B presents cumulative raw and abnormal returns surrounding initial news announcements that there were potential frauds. Abnormal returns (in brackets) are returns in excess of those predicted by the market model. Panel C compares median growth rates in earnings per share (EPS) from –5 to –2 years before the fraud to –2 to –1 years before the fraud for 41 matched fraud firm-control firm pairs [a]The sample size is 67 matched pairs due to missing data for either fraud or matched firms. [b]The sample size is 74 matched pairs due to missing data for either fraud or matched firms. [c]The sample size is 49 matched pairs due to missing data for either fraud or matched firms. [d]The sample size is 57 matched pairs due to missing data or problems with calculating growth rates caused by negative EPS for either fraud or matched firms. * Significantly different from zero at the 0.10 level; ** Significantly different from zero at the 0.05 level; *** Significantly different from zero at the 0.01 level.

Panel A: Median Raw [Market-adjusted] Stock Returns

Measure	Fraud Firms	Matched Firms	*p*-value of H_0: Median paired difference = 0
Annualized stock return from three years before the fraud to one year before the fraud[a]	29.6%*** [8.8%]*	21.2%*** [3.4%]	0.35
Stock return for the year before the fraud begins[b]	21.1*** [9.00%]*	14.1%*** [1.2%]	0.75
Annualized stock return over fraud period[c]	−3.5 [−9.5%]**	6.4%* [−2.1%]	0.01

Panel B: Event Study Analysis of First Public Disclosure of Potential Fraud[c]

Days −1, +1		Days −5, +30	
Cumulative Raw [Abnormal] Return	Percent Negative Raw [Abnormal} Returns	Cumulative Raw [Abnormal] Return	Percent Negative Raw [Abnormal] Returns
−14.9%*** [−14.7%]***	80% [85%]	−22.7%*** [−23.8%]***	70% [77%]

Panel C: Median Earnings Per Share (EPS) Growth Rates[d]

Measure	Fraud Firms	Matched Firms	*p*-value of H_0: Median paired difference = 0
Annual EPS growth rate (−5 to −2)	6.02***	7.63**	0.62
Annual EPS growth rate (−2 to −1)	−12.68	−3.85	0.35
p-value of H_0: Median paired change from growth rate (−5 to −2) to growth rate (−2 to −1) = 0	0.00	0.09	0.68

Table III. Distribution of Incentives by Source for Fraud Firms and Control Firms

Top-five executive measures in Panel A are averaged across each firms top-five executives for each year and then averaged across years for each fraud. CEO-only measures in Panel B are averaged across years for each fraud. Each incentive measure is defined as the dollar change in the respective stock or option holdings for a one-percent increase in firm value and incorporate convexity. The sample size is 87 frauds. *p*-values are for the hypotheses that the median paired difference in incentive measures equal across fraud and control firms. [a]The median incentive from unrestricted stock is significantly greater than the median incentive from vested options for fraud firms (*p*-values = 0.03 for top five executives and 0.02 for CEOs). There are no significant differences in the corresponding medians for control firms (*p*-values = 0.51 and 0.49).

	25th percentile	*50th percentile*	*75th percentile*
Panel A: Incentives for Top Five Executives			
Includes only restricted stock holdings (*p*-value = 0.26)			
Fraud firms	$0	$24	$10,744
Control firms	$0	$0	$5,062
Includes only unrestricted stock holdings (*p*-value = 0.02)			
Fraud firms	$19,690	$51,813[a]	$268,319
Control firms	$9,922	$33,692	$145,800
Includes only vested options (*p*-value = 0.44)			
Fraud firms	$10,278	$35,503[a]	$127,236
Control firms	$10,871	$35,984	$86,487
Includes only unvested options (*p*-value = 0.08)			
Fraud firms	$11,917	$34,122	$133,757
Control firms	$13,226	$31,635	$67,331
Panel B: CEO Incentives Only			
Includes only restricted stock holdings (*p*-value = 0.48)			
Fraud firms	$0	$0	$21,172
Control firms	$0	$0	$9,508
Includes only unrestricted stock holdings (*p*-value = 0.02)			
Fraud firms	$42,460	$111,631[a]	$592,754
Control firms	$20,065	$90,769	$357,911
Includes only vested options (*p*-value = 0.99)			
Fraud firms	$13,519	$48,907[a]	$251,452
Control firms	$16,429	$64,885	$185,571
Includes only unvested options (*p*-value = 0.13)			
Fraud firms	$16,474	$43,764	$206,437
Control firms	$16,223	$41,642	$129,163

Table IV. Governance, Firm, and CEO Characteristics for Fraud Firms and Control Firms

Governance, firm, and CEO characteristics for fraud and control firms and corresponding median paired difference tests. The measures are for the first fraud year for each observation. The sample size is 82 pairs, except for the financing need variable for which data are meaningful for only 78 pairs and the governance and entrenchment indexes, for which we have data on only 49 matched pairs. The financing need measure is cash flow from operations minus the prior three-year average of capital expenditures divided by current assets. The leverage measure is short and long-term debt divided by total assets. [a]*p*-value is for hypothesis that proportions equal for this measure.

Measure	Median for Fraud Firms	Median for Matched Firms	*p*-value of H_0: Median paired difference = 0
Governance measures			
Number of directors on board	8.5	9	0.27
% Insiders on board	25.0	25.0	0.76
No. of board meetings	7.5	6	0.07
CEO holds chairman of board position[a]	1	1	0.59
% Shares held by outside blockholders	15.4	15.5	0.91
Number of directors on audit committee	3	3	0.62
% Insiders on audit committee	0	0	0.10
No. of audit committee meetings	4	3	0.16
Governance index	9	9	0.30
Entrenchment index	2	2	0.50
Firm characteristics			
Financing need	1.45%	12.85%	0.00
Leverage	19.34%	20.77%	0.88
CEO characteristics			
Tenure (years)	6	5.5	0.91
Age (years)	52.5	55	0.36
CEO is founder	0	0	0.59
Number of other boards on which CEO serves	1	1	0.62

Table V. Matched-Pair Conditional Logistic Regressions Modeling the Probability of Fraud.

Matched-pair conditional logistic regressions modeling the probability of a fraud firm. All measures are measured in the first fraud year for each observation. The incentive measures are the dollar changes (divided by $1,000,000) in the respective stock or option holdings for a standardized percentage change in firm value for the firms' CEOs. The measures are for the first fraud year for each observation. The financing need measure is cash flow from operations minus the prior three-year average of capital expenditures divided by current assets. The leverage measure is short and long-term debt divided by total assets. The sample size is 136 for the full model and 90 for the model with the governance index. p-values based on robust standard errors are in parentheses. . * Significantly different from zero at the 0.10 level; ** Significantly different from zero at the 0.05 level; *** Significantly different from zero at the 0.01 level.

Incentives from restricted stock	-0.0107	-3.5264	
	(1.00)	(0.84)	
Incentives from unrestricted stock	0.8967**	1.6783*	1.8817***
	(0.03)	(0.06)	(0.01)
Incentives from unvested options	-0.2162	0.4512	
	(0.89)	(0.78)	
Incentives from vested options	0.8424	1.5340	
	(0.62)	(0.31)	
Log (Sales)	-1.0937		
	(0.26)		
Pre-fraud three-year sales growth	5.0374**	0.8946	1.8353
	(0.03)	(0.74)	(0.41)
Log (1+ number of directors on board)	-0.6013		
	(0.66)		
% Insiders on board	2.0307		
	(0.38)		
Log(1+number. of board meetings)	1.9865*	1.7989*	1.7600**
	(0.09)	(0.06)	(0.06)
CEO is chairman of board (0/1)	0.8499		
	(0.14)		
% Shares held by outside blockholders	-1.4950		
	(0.65)		
Log (1+Number of directors on audit committee)	2.1497		
	(0.13)		
% Insiders on audit committee	5.4694**	23.7291*	26.0031**
	(0.05)	(0.08)	(0.02)
Log (1+ number of audit committee meetings)	0.3464		
	(0.55)		
Financing need	-3.0532		
	(0.37)		
Leverage	2.3620		
	(0.47)		
Log (1+CEO tenure) (years)	0.4293		
	(0.36)		
Log (1+Age) (years)	-1.5726		
	(0.36)		
CEO is founder (0/1)	-0.6992		
	(0.33)		
Number of other boards on which CEO serves	0.2226		
	(0.40)		
Governance Index		0.1367	0.1202
		(0.35)	(0.40)
Pseudo R^2	0.39	0.36	0.34

Table VI. Payoffs and Compensation for Fraud Executives and Control Executives

Top-five executive measures in Panel A are averaged across each firm's top-five executives for each year and then averaged or summed across years for each fraud. CEO-only measures in Panel B are averaged or summed across years for each fraud. The value of shares sold minus shares bought and the value of option exercises are summed over the years in the fraud periods; all other measures are averaged per year. Salary and bonus are in constant 2005 dollars. The sample size is 87, except the stock sales sample size is 59 in Panel A and 34 in Panel B.

Measure	Median for Fraud Firms	Median for Matched Firms	*p*-value of H_0: Median paired difference = 0
Panel A: Top Five Executives			
Value of shares sold minus shares bought	$3,821,599	$2,293,539	0.04
Value of option exercises	$801,211	$466,527	0.02
No. of exercised options / total vested options	12.4%	7.3%	0.14
Vested options / total options	43.6%	46.5%	0.14
Stock price / average strike price of vested options	1.70	1.43	<0.01
Salary	$413,000	$429,000	0.98
Bonus	$239,315	$197,900	0.08
Panel B: CEOs Only			
Value of shares sold minus shares bought	$6,489,385	$2,812,252	0.11
Value of option exercises	$79,200	$275,000	0.21
No. of exercised options / total vested options	4.4%	5.6%	0.38
Vested options / total options	56.3%	55.7%	0.24
Stock price / average strike price of vested options	1.63	1.46	0.15
Salary	$696,200	$580,287	0.41
Bonus	$300,400	$291,000	0.21