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16 13

Information Production, Misconduct Effort, and the Duration of Corporate Fraud

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We develop and test a model linking the duration of financial fraud to information produced by auditors and analysts and efforts by managers to conceal the fraud. Our empirical results suggest fraud termination is more likely in the quarter following the release of audited financial statements, especially when reports contain explanatory language, indicating auditors' observable signals reduce fraud duration. Analyst attention increases the likelihood of fraud termination, but the marginal effect beyond the first analyst is negative, possibly due to free riding and herding behavior impairing analysts' ability to illuminate misconduct. Finally, evidence suggests managerial concealment significantly increases fraud duration.

JEL classification: G34; G38; K22; K42; L51; M41.

Keywords: Fraud duration; Information production; Fraud effort; Auditor reports; Hazard models.

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Why do some corporate financial frauds persist for longer than others? In this paper, we analyze this question by developing and testing a simple model where fraud duration is a function of the signals detected by information producers, such as auditors and analysts, as well as managerial effort to hide the fraud. Analyzing fraud duration is important for at least three reasons. First, long-lasting frauds affect more accounting reports making distortions caused by the firm's fraudulent financial information more costly. Indeed, research has shown that longer accounting misconduct spells are associated with larger penalties in formal SEC enforcement actions (see Files (2012)).[1] Second, if we want to understand what measures would prevent financial statement fraud, it is instructive to investigate what factors cut short or prolong frauds already in place. After all, managers would be discouraged from engaging in fraud if they believed they would be caught quickly. Third, from a more technical perspective, by focusing on frauds already in place, we can mitigate the problem of empirically confounding factors related to fraud commitment and fraud detection (see Wang (2013) for a discussion of this problem).

In our model, information producers periodically scrutinize firms. As a result of their scrutiny, information producers detect signals indicating that either there is no need for concern (good signal) or there is something unusual going on (bad signal). Fraudulent firms – called Manipulators – are the only ones that generate a bad signal with positive probability. Based on the observed signals, monitors (e.g., institutional investors, the SEC, board members, etc.) decide to intervene in a firm or not. Because intervention is costly, monitors want to minimize the chance of a superfluous action. For this reason, they intervene only if a bad signal is revealed.

Our model shows that the expected duration of a fraud depends on the likelihood of a bad signal being disclosed. Specifically, it predicts that the fraud termination hazard rate is increasing in the number of information producers and that the size of this effect is positively related to the ability of the information producers to detect a fraud signal. However, this positive effect is attenuated if the information producers' signals are not independent.

We extend the model to take into account the firm's decision to commit and continue a fraud as well as the decision to exercise effort to conceal it. We show that only firms that highly benefit from fraud will manipulate their statements. Even more, if the probability of a bad signal increases over time – e.g., information producers become more effective as the fraud progresses or it becomes

[1] Table 7 of Files (2012) shows that longer misconduct spells (measured as number of days from start to end of the misstatement period) are positively and significantly related to both individual and firm penalties in SEC enforcement actions, thus indicating that longer misconduct is more serious on average.

harder to hide manipulation as time passes – the frauds not stopped voluntarily by management are those with the greatest benefits to manipulation. Thus if the firm's benefits to fraud are positively correlated with the cost of the fraud to society, and active, ongoing frauds are the ones most likely to be caught, it follows that the intervened firms are the ones with the costliest frauds for markets and investors.

Finally, we model how the optimal effort to conceal fraud evolves as the probability of a bad signal increases. Specifically, we show that if effort becomes less effective in slowing down the increase in the probability of detection, firms optimally reduce their efforts over time. On the other hand, as long as there exists a learning curve to hiding accounting misconduct, meaning that the concealing effort becomes more effective as time passes, firms optimally increase their efforts over time. Based on these theoretical dynamics, we can evaluate whether concealing efforts become more prevalent over time, a "slippery slope" pattern of fraudulent behavior, or frauds tend to be planned ahead and are already more complex from the start.

For our empirical analysis of financial statement fraud we use a sample of SEC Accounting and Auditing Enforcement Releases (AAERs) drawn from an updated version of the data set used in Dechow, Ge, Larson, and Sloan (2011). It is important to note that while the AAERs in our sample involve financial misstatements, they are often not associated with a formal admission of guilt or legal ruling of fraud. Thus, the term fraud used in this paper is meant to have a colloquial, rather than legal, meaning.[2] The updated data from Dechow et al. (2011) provides the full misstatement periods (including which 10Q and 10K statements were affected by the fraud) for 926 instances of accounting misconduct between 1982 and 2012. The availability of clear beginning and end dates of fraud-related misstatements allows us to estimate a discrete time duration model without issues of left or right truncation, which could otherwise complicate and potentially bias our analysis.[3]

We test our model's predictions about the impact of information producers on the expected length of a fraud spell using two different sets of scrutinizers: auditors and financial analysts. We find strong evidence that auditors are important information producers, significantly shortening fraud duration. Through their professional task, auditors have access to a firm's internal, non-public

[2]Note that we also use the word 'fraud' interchangeably with terms such as 'accounting misconduct', and 'financial misstatement' throughout the paper.

[3]An expected drawback of our analysis is that, by focusing on accounting frauds already in place, we can only observe frauds ultimately revealed to the world. On the other hand, as long as, conditional on the covariates, the probability of fraud termination at any point in time is the same across detected and undetected frauds, our estimates are consistent.

accounting information. Consequently, auditors should be able to detect signals about fraudulent activities that other information producers cannot. Because auditors only audit the annual financial statements, every fourth fiscal quarter there is an additional potential fraud signal from a high ability information producer. Consistent with this, we document that fraud termination hazard rates significantly spike following the fourth fiscal quarter. On the other hand, there is no evidence that the perceived quality of the auditor (proxied by whether the auditor is from a Big N auditing firm or not) or the auditor's experience auditing the firm matters for this effect.

To better understand whether the fourth fiscal quarter effect is directly due to signals from the auditor or reflects that annual reports attract more general scrutiny than interim reports, we examine the use of explanatory language in otherwise unqualified audit reports. These "audit explanations" provide additional information to investors (e.g. highlighting changes in accounting standards by the firm) without any implications about the auditor's view on the quality of the report. The rationale for this test is based on recent research by Czerney, Schmidt, and Thompson (2014) that shows audit explanations predict future restatements of financial reports. Thus, although seemingly innocuous, the explanatory language appears to contain valuable signals. Further supporting this view, Beasley, Carcello, and Hermanson (2010) find evidence that these additional explanations are more likely to appear in the financial statements of fraudulent firms than in a control group of firms in the same industry and with similar size and profitability. In our empirical study, we find that the marginal impact of the fourth fiscal quarter on the fraud termination hazard rate mainly stems from explanatory language in the auditor report. Therefore, it appears that it is the actual information produced by the auditors that matters the most.

Analyst following also has an impact on estimated fraud termination hazard rates. Following Gilson, Healy, Noe, and Palepu (2001) we divide analysts into industry specialists and non-specialists based on the idea that analysts with industry experience have a greater ability to detect reporting anomalies and accounting misconduct. We find that fraud spells are shorter if the company is followed by at least one industry specialist analyst. However, the impact of specialist analyst following declines in the number of specialists that cover a firm, implying that coverage by too many analysts may be counter-productive at the margin. This result is consistent with analysts free riding on the screening efforts of their peers, which reduces the overall scrutiny of the firm. It may also be related to analysts' search for conformity, which may drive herding among financial analysts' forecasts and recommendations (see evidence on analyst herding behavior by Welch (2000), Clement and Tse (2005), and Jegadeesh and Kim (2010)). In contrast to industry specialists, non-specialist analysts have no significant effect on the fraud termination hazard.

Next, we consider analyst forecast error as an explanatory variable in the analysis. We calculate analyst forecast error as the absolute difference between the mean analyst forecast of annual earnings per share (EPS) just before the earning announcement and the actual reported EPS in a given year, scaled by the corresponding end-of-fiscal year stock price. Greater error suggests that either analysts produced poorer quality forecasts or that there has been a greater earnings surprise. An earnings surprise may raise a red flag for monitors because disagreement between forecasts and actual results could come from accounting misconduct. Concordantly, we observe that a greater forecast error is associated with a shorter misconduct spell. Therefore, our result is consistent with greater earnings surprises generating more scrutiny of the firm.

Our model allows for management to exert effort to conceal fraud from information producers, thereby prolonging the misconduct. The first proxy for managerial effort we consider is an indicator for whether the fraud started in the first fiscal quarter or not. Managers who choose to start frauds in the first fiscal quarter have more time to design the accounting fraud before the statements are audited. Thus a first fiscal quarter start suggests a higher degree of premeditation compared to other frauds, which is likely to correlate both with a higher fraud benefit and a greater level of effort to conceal fraud. As hypothesized, we find that estimated fraud termination hazard rates are significantly lower (both in economic and statistical terms) for frauds started in the first fiscal quarter.[4]

The second proxy for fraud effort we consider is the number of areas of the financial statements that are affected by the misconduct. More areas affected should indicate a more complex fraud, as well as an increased effort to conceal the fraud by making sure all financial statement accounts agree with each other. Similar to the first fiscal quarter indicator, this measure is significantly negatively related to the fraud termination hazard rate.

The third, and last, measure of managerial fraud effort we consider is the level of total accruals. Higher total accruals is likely to indicate more aggressive accounting, which could be indicative of efforts to conceal the fraud. A benefit of this measure compared to the other two proxies for effort is that it is time-varying, capturing changes in effort over time. Our results reveal that this measure is also significantly associated with lower fraud termination hazard rates. Furthermore, the individual effects of all three effort variables on fraud termination hazards are concurrently

[4]This result is not mechanically obtained. Frauds started in the first quarter are not only more than 5 quarters longer than frauds started in other quarters, they also have other distinct characteristics.

present, suggesting that they capture different aspects of management's effort to conceal fraud. Overall, our evidence is compatible with managers optimally choosing to exert effort to hide and prolong frauds.

Our paper is related to the literature on fraud prediction. For example, Dechow et al. (2011) document that misstating firms that have been issued AAERs tend to have a greater average market capitalization than the overall Compustat universe. Estimating a formal prediction model of a firm's propensity to have misstated financial statements in any given year, Dechow et al. (2011) obtain that accruals are unusually high during the misstatement years for the AAER firms relative to other Compustat firms. AAER firms also tend to have a higher fraction of "soft assets" (i.e., all assets other than cash and property, plant, and equipment) during their misstatement years. Moreover, return on assets and the number of employees appear to be declining during the misstatement years, whereas cash sales are increasing.

One problem with fraud prediction approaches such as that in Dechow et al. (2011) is highlighted by Wang (2013), who points out that the only frauds that can be observed are detected frauds. For this reason, any fraud database combines two latent processes: fraud commitment and fraud detection. Consequently, the standard binary response models applied in the fraud literature are measuring the probability of detected frauds instead of the actual probability of committing them. This implies such models may lead to incorrect assessment of the efficacy of public policies designed to combat fraud occurrence. In order to address this issue, Wang (2013) proposes a bivariate probit model with partial observability of fraud. In this approach, both processes – occurrence and detection – are explicitly modeled, allowing the researcher to infer the actual probability of fraud commission.

Another related stream of research studies the characteristics of whistle-blowers in fraud cases (e.g. Bowen, Call, and Rajgopal (2010), Dyck, Morse, and Zingales (2010), and Miller (2006)). In terms of who discovers the fraud, the characteristics of the whistle-blowers appear quite broad. Studying a sample of 216 cases of alleged corporate frauds, Dyck et al. (2010) find that six types of players account for at least 10% of detection each, while none is responsible for more than 17%. Together, these classes account for 82% of all cases. Specifically, these classes of players are: employees (17%), media (13%), industry regulators (13%), auditors (10.5%), short sellers (14.5%) and analysts (13.5%). So, in the authors' words, it "takes a village" to detect fraud in the U.S.

Our paper complements both the fraud prediction literature as well as the whistle-blower literature by taking an alternative approach. While our analysis is conditional on fraud detection, we focus on the elements that affect misconduct duration. For concreteness, we investigate the characteristics of fraudulent firms, frauds, information producers, and managerial concealing efforts that may allow frauds to persist longer and, consequently, produce more damage. Importantly, we extend the whistle blowers' literature by showing that intervention by monitors and whistle blowers does not happen in a vacuum. Instead, information producers that raise red flags are crucial in inducing other agents to act. Hence, even though auditors and analysts are not themselves whistle blowers in many cases, the reports that they issue are essential to trigger fraud termination.

Finally, our paper is related to Karpoff and Liu (2010), who study the relation between short selling and financial misconduct. In particular, they find that short selling activity is positively associated with a quicker time to fraud revelation. The authors interpret this finding as short sellers being important for uncovering fraud, which would make them another potential class of information producers in our model. Although we do not explicitly consider short selling activity in our empirical analysis, we indirectly capture its effect by including firms' contemporaneous quarterly stock returns as a control variable. We observe a significant negative relation between this variable and fraud termination hazard rates, which is consistent with Karpoff and Liu's (2010) results to the extent that lower stock returns are driven by short selling activity.

I. Model

In this section, we develop a stylized model of information production and fraud duration that guides our empirical analysis. A detailed presentation of the proofs, which are quite straightforward and do not add to the results' economic intuition, is presented in Internet Appendix I.A.

A. *Basic Model*

Consider that there are two types of risk-neutral, long-lived firms: Manipulators (*M*) and Non-Manipulators (*NM*). We assume that *NM*s never misrepresent their financial statements. Differently, *M*s regularly manipulate their financial statements in their own benefit. Even though later we endogenize the firm's choice of becoming a manipulator, let's initially denote the probability that any given firm is a manipulator by $\xi \in (0,1)$.

Every time a financial statement is issued, a group of information producers and intermediaries scrutinize the accounting data. These are auditors, analysts, institutional investors, among others. In this basic model, we assume a unique information producer – we generalize the results for multiple information producers in the next subsection. The signals detected by the information producers can be good ($s = G$) or bad ($s = B$). The probability that a manipulator generates a bad signal is given by $\Pr(B|M) = p$. On the other hand, non-manipulators always generate a good signal, i.e., $\Pr(B|NM) = 0$. Signals across different financial statements are assumed i.i.d. in this section – we relax this assumption later.

Risk neutral monitors – comprised of regulators, institutional investors, and board members – observe the signals detected by the information producers and decide if they intervene in the firm or not, i.e. a monitor's action space is $A = \{I, NI\}$, where I and NI represent intervention and non-intervention, respectively. In order to intervene in a firm and scrutinize it for accounting misbehavior, monitors must incur a cost $\mathscr{C} > 0$. Whenever a manipulator is caught, intervening monitors obtain a gain of $P > \mathscr{C}$. However, if they intervene in a non-manipulator, their return is normalized to zero. Both $\mathscr{P} > 0$ and $\mathscr{C} > 0$ may be monitor-specific, but for ease of notation and because our results do not depend on such heterogeneity, we assume $\mathscr{P}$ and $\mathscr{C}$ common for all monitors. Accordingly, in period t, a monitor's instantaneous expected utility is given by:

$$u(a_t, \mathscr{H}_t) = \begin{cases} \Pr(M|\mathscr{H}_t) \times P - \mathscr{C}, & \text{if } a_t = I, \\ 0, & \text{if } a_t = NI. \end{cases} \tag{1}$$

where the probability of a manipulator conditional on the history of signals is

$$\Pr(M|\mathscr{H}_t) = \begin{cases} 1, & \text{if} \quad h_i = B, \text{ for some } h_i \in \mathscr{H}_t \\ \frac{\xi(1-p)^t}{(1-\xi)+\xi(1-p)^t}, & \text{otherwise.} \end{cases} \tag{2}$$

Based on the instantaneous utility function, the value function for monitors is given by

$$V(\mathscr{H}_t) = \max_{a_t \in A} \{\Pr(M|\mathscr{H}_t) \times P - \mathscr{C}, \delta E_t[V(\mathscr{H}_{t+1})]\}, \tag{3}$$

where $\delta \in (0,1)$ is the discount rate.[5] Now, we can show a few results, but let's first define $\mathscr{H}_t(B) = \{\mathscr{H}_t \text{ s.t. } \exists\ h_i = B \in \mathscr{H}_t\}$ as the set of histories in which a bad signal was observed at some point and denote the history at the beginning of the firm by $\mathscr{H}_\emptyset = \emptyset$.

LEMMA 1: *If $\mathscr{H}_t \in \mathscr{H}_t(B)$, monitors should intervene, i.e., $V(\mathscr{H}_t) = P - \mathscr{C}$.*

[5]From equation (3), it is clear that $\mathscr{P}$ includes the discounted difference between the value obtained by the monitor from correct intervention and from superfluous intervention, whereas $\mathscr{C}$ includes his/her discounted value of needless intervention.

Then, the following conclusion is a straightforward consequence:

COROLLARY 1: *Monitors should immediately intervene if they observe a bad signal.*

We can now state the main proposition in the Monitor's problem.

PROPOSITION 1: *If $\xi P < \mathscr{C}$, then monitors only intervene if they observe a bad signal.*

Therefore, based on Proposition 1, if it is not optimal to immediately intervene in a firm – even before observing any signal – it is never optimal to intervene before observing a bad signal. From this point on, we keep the assumption $\xi P < \mathscr{C}$, so monitors only intervene once they observe a bad signal.[6] Consequently, the length of a fraud is described by a geometric distribution, which leads to the following proposition:

PROPOSITION 2: *The expected length of a fraud is given by $E[N] = \frac{1}{p}$.*

As a result, the better the information producers are at spotting frauds, by detecting bad signals, the lower the life expectancy of a fraud. Before we move to the extensions, keep in mind that the hazard rate function, i.e. the probability that a fraud is detected in period t conditional on having survived until period $t-1$, is given by p, a constant, as the geometric distribution is memoryless. In the extensions, we consider cases in which the hazard rate is time dependent, due to the fact that longer frauds may become easier to catch.

B. Extensions

B.1. Multiple information producers

Independent signals

Let $\mathscr{I} := \{1, ..., \mathbf{I}\}$ be the set of information producers. In order to study the case in which they are the most efficient, assume that they detect signals independently from each other. As before, assume that *NM* firms never generate a bad signal. Differently, we assume that information provider i detects a bad signal for a type M firm with probability p_i. Then, the probability that at least one information provider detects a bad signal is given by:

$$\Pr(B|M) = 1 - \prod_{i \in \mathscr{I}} (1 - p_i), \tag{4}$$

[6]Due to the fact that monitors intervene whenever they observe a bad signal, it is also not optimal for firms that plan to engage in fraudulent behavior to build up reputation by delaying the fraud start.

and, the expected duration of a fraud is given by:

$$E[N] = \frac{1}{1-\prod_{i\in\mathscr{I}}(1-p_i)}. \tag{5}$$

As before, the better information providers are spotting a fraud – i.e., the higher p_i for at least some $i \in \mathscr{I}$, the shorter the fraud. Likewise, the introduction of an additional information producer increases the probability of fraud detection and reduces its expected length.

PROPOSITION 3: *The introduction of a new information producer at a given period increases the likelihood of a bad signal detection, shortening the fraud's length. The better the new information producer is catching frauds – i.e., the higher his/her p – the larger the effect.*

Correlated signals

In this case, since signal detection is not independent across information producers, we take into account the interactions among detected signals through their joint p.d.f.. Therefore, we have that the probability that at least one information producer detects a bad signal is $\Pr(B|M) = 1-\Pr(s_1 = G, s_2 = G, ..., s_{\mathbf{I}} = G)$, and the expected fraud duration is given by:

$$E[N] = \frac{1}{1-\Pr(s_1 = G, s_2 = G, ..., s_{\mathbf{I}} = G)}. \tag{6}$$

As expected, as long as the signals are not perfectly correlated, in the sense that $\Pr(s_i = G|s_1 = G, s_2 = G, ..., s_{i-1} = G, s_{i+1} = G, ..., s_I = G) < 1, \forall i \in \mathscr{I}$, all previous results are qualitatively the same, even though they are quantitatively weaker.

Due to the fact that notation becomes cumbersome in the case of correlated signals across information producers, we focus on the case with independent signals. However, the reader should keep in mind that all results are preserved once we allow for partial correlation.

We can also consider the incentives for firms to exert effort to make frauds harder to detect. Before we discuss that, let's consider the case in which the probability of detection varies over time.

B.2. Time-varying probability of a bad signal

As we mentioned previously, in the basic model the hazard rate is constant over time. This lack of memory is a feature of the geometric distribution that may not be particularly suited to our case. In this sense, we may consider that the probability of producing a bad signal may change over time, i.e.:

$$\Pr(B|M,t) = p(t). \tag{7}$$

A natural assumption would be $p'(t) > 0$, i.e., as time passes, the probability of obtaining a bad signal increases. For example, a longer fraud means that more financial statements are affected by the fraud and it may be easier to spot inconsistencies. We also assume that $p(t) < 1, \forall t \in \mathbb{N}$ and $\lim_{t\to\infty} p(t) = 1$, i.e., the probability of getting a bad signal increases but it is never 1 at a finite time. Then, the expected duration of the fraud is now:

$$E[N] = \sum_{t=1}^{\infty} t\, p(t) \prod_{t'=1}^{t-1} (1 - p(t')). \tag{8}$$

While the hazard rate is now $h(t) = p(t)$.[7]

Moreover, even though we imagine that the probability of being detected has an upward trend, the actual probability may vary around the trend. In particular, we may expect that market and firm time-varying characteristics may affect the detection probability, pushing it above or below the long-term trend. For example, good or bad performance in the stock market may increase or decrease incentives to scrutiny, making it easier or harder for information producers to detect signs of manipulation. A similar argument can be made about the firm's own operational and stock market performance.

B.3. Firm-specific factors and the probability of a bad signal

Observable firm characteristics may influence the likelihood that a information producer may detect a bad signal. For example, firm size may be related to the duration of accounting misconduct in a few ways. Large firms have relatively richer information environments than small firms. A richer information environment should make the marginal cost of issuing an additional fraud signal lower for information producers and thus reduce the duration of accounting misconduct. Conversely, large firms also tend to have a wider scope of operations than small firms, which may make it easier for a manager to conceal misconduct. In this sense, we expect that the probability that a IP issues a bad signal for a manipulator i is given by $p(\mathbf{x}_{i,t}, t)$ where $\mathbf{x}_{i,t}$ is a vector of firm i

[7] In the Internet Appendix we present a simple example in which $p(t)$ is an increasing and concave function.

characteristics at time t that make it easier or harder for IPs to spot a bad signal.[8]

C. *Firm's decision on fraud commission and fraud-hiding efforts*

C.1. Firm's decision to commit fraud

Up to now, we consider the decision of committing fraud or not as exogenous, representing the firm's type. In this section, we consider the firm's decision of committing fraud.

We assume that firms differ in their benefit of committing fraud or not, i.e. the firm's benefit of committing fraud $\mathscr{B}$ is a draw in the distribution $F(.)$ with support $(0,\overline{\mathscr{B}})$. We also assume that if the firm is caught, it incurs in a loss of $L \equiv \overline{\mathscr{B}}$, independent of its type. Finally, a firm decides each moment if it continues to commit fraud or if it decides to stop. For simplicity, we assume that only ongoing frauds can be discovered. In this sense, the firm can decide if it commits (or continues) a fraud period by period.

Then, the period t expected benefit (or loss) of committing a fraud that has been ongoing for t periods for a type $\mathscr{B}$ firm is given by:

$$\textbf{Profit}(\mathscr{B},\textbf{t}) = (1-p(t))\mathscr{B} + p(t)(-L). \tag{9}$$

Even though firms live forever and the decision to start or continue a fraud is a dynamic problem, proposition 4 below shows that the decision ultimately depends only on the current period expected benefit or loss. Therefore, a firm decides to start or continue an ongoing fraud if **Profit($\mathscr{B}$,t)**> 0.

PROPOSITION 4: *In an economy in which firms choose optimally to commit fraud and frauds do not become harder to spot over time - i.e. $p'(t) \geq 0$ - the following is true:*

1. *Non-Manipulation is the optimal policy for all firms with $\mathscr{B} \leq \mathscr{B}^*$, where $\mathscr{B}^*$ is given by:*

$$(1-p(1))\mathscr{B}^* + p(1)(-L) = 0. \tag{10}$$

2. *If $p(t) \equiv p, \forall t$ then if a firm decides to commit fraud it will never stop until it gets caught.*
3. *If $p'(t) > 0$ and $\lim_{t\to\infty} p(t) = 1$, for every $\mathscr{B} > \mathscr{B}^*$ there is a $T(\mathscr{B}) < \infty$ in which if the firm has not been caught up to that point, management decides that it is not profitable to continue*

[8] We allow $\mathbf{x}_{i,t}$ to depend on t since several important firm characteristics – such as size, leverage, fraction of soft assets, among others – vary over time.

the fraud anymore. $T(\mathscr{B})$ is defined by:

$$(1-p(T(\mathscr{B})))\mathscr{B}+p(T(\mathscr{B}))(-L)=0. \tag{11}$$

From implicit function theorem, notice that

$$\frac{dT(\mathscr{B})}{d\mathscr{B}}=\frac{(1-p(T(\mathscr{B}))}{p'(T(\mathscr{B}))(\mathscr{B}+L)}>0. \tag{12}$$

Since $p'(T)>0, \forall T$. Based on this result, we have the following corollary:

COROLLARY 2: *Firms that benefit the most out of a fraud are more likely to get caught instead of stopping the fraud by themselves*

Finally, based on the proof of proposition 4, we can also easily conclude that all results presented here are still true for time varying benefit of fraud and loss due to detection – $\mathscr{B}(t)$ and $L(t)$ – as long as $(1-p(t))\mathscr{B}(t)+p(t)L(t)$ decreases over time. In this sense, as long as $\mathscr{B}(t)$ does not increase faster than $L(t)$ over time, our results are still valid.

C.2. Fraudster's effort

Consider that the fraudster can exert an effort $e_M>0$ in order to make harder for information producers to spot irregularities. In order to simplify notation, let's initially assume that the probability of a bad signal does not change over time. Therefore, we assume that $\frac{\partial p_i(e_M)}{\partial e_M}<0$, i.e., by exercising effort, the manipulator reduces the likelihood of a bad signal for any information provider $i \in \mathscr{I}$. We also assume that the cost of effort is given by a convex, strictly increasing function $C(e_M)$, while $\lim_{e_M \to e_M^*} C(e_M)=\infty$, where $p_i(e_M^*)=0$, $\forall i \in \mathscr{I}$. In other words, it would be prohibitively expensive to completely eliminate the risk of getting caught.

Then, it is easy to see that the expected duration of the fraud is given by:

$$E[N|e_M]=\frac{1}{1-\prod_{i\in\mathscr{I}}(1-p_i(e_M))}. \tag{13}$$

Therefore, as expected $\frac{\partial E[N|e_M]}{\partial e_M}>0$.

C.3. Optimal choice of effort

Now, let's consider that the firm committing fraud can optimally choose its effort to hide an ongoing fraud. As in the previous section, we consider that the firm not only chooses if it starts or continues an ongoing fraud every period[9] but also its efforts hiding the fraud, paying a flow cost $C(e_M) > 0$. Then, if the firm decides to commit a fraud, the optimal choice of effort in period t is given by:

$$\max_{e_M}(1 - p(t, e_M))\mathscr{B} + p(t, e_M)(-L) - C(e_M). \tag{14}$$

Then, from the first order condition (F.O.C), we have

$$-\frac{\partial p(t, e_M)}{\partial e_M}(\mathscr{B} + L) - C'(e_M) = 0. \tag{15}$$

where $\frac{\partial p(t,e_M)}{\partial e_M} < 0$. From the second order condition, we have:

$$-\frac{\partial^2 p(t, e_M)}{\partial e_M^2}(\mathscr{B} + L) - C''(e_M). \tag{16}$$

So, as long as $\frac{\partial^2 p(t,e_M)}{\partial e_M^2} > 0$, the problem is strictly concave and there is a unique optimal effort $e^*(t, \mathscr{B})$ pinned down by the F.O.C.

Notice that the firm's choice of committing or continuing a fraud is now given by:

$$(1 - p(t, e^*(t, \mathscr{B})))\mathscr{B} + p(t, e^*(t, \mathscr{B}))(-L) - C(e^*(t, \mathscr{B})) > 0. \tag{17}$$

where $e^*(t, \mathscr{B})$ is pinned down by the F.O.C.

Finally, from F.O.C., we also obtain the following results:

PROPOSITION 5: *Based on a manipulator's optimal effort decision* $e^*(t, \mathscr{B})$*, the following is true:*

1. $\frac{\partial e^*(t,\mathscr{B})}{\partial \mathscr{B}} > 0$*, i.e., the firms that benefit the most incurring in fraud are also the ones that put more effort to hide it;*
2. $\frac{\partial e^*(t,\mathscr{B})}{\partial t}$ *depends on* $\frac{\partial^2 p(t,e_M)}{\partial e_M \partial t}$*. In particular, if* $\frac{\partial^2 p(t,e_M)}{\partial e_M \partial t} > 0$ *the effect of the fraudster's efforts*

[9] We assume here that only ongoing frauds can be detected in order to simplify our expressions. Results are still true if we assume that stopped frauds see a significant decrease in their likelihood of detection.

concealing the misconduct decreases over time, so $\frac{\partial e^*(t,\mathscr{B})}{\partial t} < 0$.

D. Summary of Model Implications

The model delivers several empirical implications. In this subsection, we summarize them and discuss their intuition.

Implication 1: *The introduction of a new information producer increases the likelihood of detection, reducing the expected duration of the fraud. Moreover, the better the new information producer is at spotting a fraud – i.e., the higher the probability of spotting a fraud – the larger the effect.*

Implication 1 comes directly from Proposition 3 of the model and justifies testing the impact of both the number and quality of information producers on fraud termination hazard rates. When the signals detected by different information producers can be correlated, the model yields the following related implication:

Implication 2: *The effect of a new information producer on the fraud detection hazard rate is smaller the more correlated the signal detected by the the new information producer is to those identified by the existing information producers analyzing the firm.*

In terms of the time-varying probability of a bad signal, as we mentioned before, the assumption that the hazard rate is constant over time is quite strong. For example, we would expect that inconsistencies of financial statements due to the fraud grow and become easier to catch over time. Implication 3 encapsulates this idea.

Implication 3: *If bad signals are more likely to occur the longer the fraud, the hazard rate is increasing over time.*

Implication 3 validates the use of an empirical model of fraud duration that allows for time dependence in the hazard rate, such as the one we use in our empirical analysis (see the description in the next section).

In terms of the decision to commit fraud, the model predicts that the higher the likelihood of a bad signal, the higher the threshold $\mathscr{B}^*$ for the firm's fraud benefit. Similarly, if Implication 3 holds and the likelihood of a bad signal goes up over time, frauds become less profitable in expected terms as time passes. Consequently, firms engaging in them are more likely to terminate the misconduct before detection, especially for frauds that were not particularly profitable from the beginning. In this sense, only frauds that are very profitable for fraudsters are likely to last long, since they tend to go on until they are detected due to a bad signal from information producers. Implication 4 summarizes these results:

Implication 4: *In terms of fraud incidence, the model delivers the following results:*

- *The better information producers are at detecting bad signals, the higher the expected benefit of fraud among firms that decide to engage in fraudulent behavior*;
- *If the hazard rate of detection increases over the duration a fraud, the firms with high benefit of fraud are more likely to be caught, while firms with low fraud benefit are more likely to terminate the fraud spontaneously.*

If the benefit that firms obtain by engaging in fraud is positively correlated to the cost of the fraud for society, Implication 4 renders two important conclusions. First, if measures are taken to make financial statements more transparent and thereby increase the likelihood of bad signals issuance, we should expect that the average fraud that is ultimately incurred and then later caught is more damaging. Second, even though this result implies that our sample may be biased, it also says that the sample is biased towards the most harmful frauds, and consequently, more economically relevant.

In terms of how much effort fraudulent firms incur trying to avoid or delay detection, our model shows not only that effort is positively correlated with the firm's benefit of fraud, but also that it has a non-trivial connection to how the hazard rate evolves through time and how effective the effort is to slow down the increase of the hazard rate over the fraud's duration. These results are captured by Implication 5:

Implication 5: *In terms of the effort to avoid or delay detection, the model delivers the following results:*

- *Managerial effort to hide a fraud is in general associated with lower fraud termination hazard rates;*
- *Managers of firms with the largest benefit of fraud incur the highest effort to hide it;*
- *If the hazard rate increases over time, effort only increases if it also becomes more effective slowing down the increase in the likelihood of a bad signal being generated.*

As before, if there is a positive correlation between a firm's fraud benefit and the cost of fraud for investors, Implication 5 entails that the most costly frauds are also the ones in which a fraudulent firm spends the most effort to conceal.

Regarding how the effort to cover up a fraud changes over time, Implication 5 says that if efforts to conceal become less effective over time, the firm progressively reduces its efforts in trying to hide an ongoing fraud. Hence, if we believe that premeditated frauds are the ones in which the firm puts a lot of effort in fraud design, then additional attempts to hide the these frauds over time are arguably less effective than the follow-up effort to conceal unpremeditated frauds.[10] If this reasoning is correct, then, by looking at changes in proxies for effort as frauds evolve, Implication 5 allows us to distinguish between "premeditated" vs. "slippery-slope" misconducts.

Finally, even though we do not directly consider it in the model, if there is any decision of effort allocation by information producers, a straightforward extension would indicate that in case some types of frauds are more important than others for investors, information producers rationally respond by putting more efforts in these areas (for example, more focus on income statement). Consequently, frauds affecting such areas should have lower life expectancy. Implication 6 highlights this result:

Implication 6: *Frauds affecting areas that are of higher concern for investors are expected to be shorter due to more scrutiny.*

II. Empirical Method and Hypotheses

A. *Hazard Model*

In the previous section, we developed a model of fraud termination hazard rates as a function of information production and managerial fraud effort. In this section we give a brief description of the discrete time hazard models we use to estimate fraud termination hazard rates. A more detailed discussion is provided in Internet Appendix I.B.

We estimate the following discrete hazard rate: the probability of transition out of the initial state (active fraud) in period j conditional on having survived up until period $j-1$ and on a vector of independent variables. Denoting the survival time by T:

$$h_j(\mathbf{x}) := \Pr(t_{j-1} < T \leq t_j | T > t_{j-1}, \mathbf{x}). \tag{18}$$

[10]Unpremeditated frauds are frauds occurring in response to an unexpected need or opportunity, for example.

Assuming a proportional hazard form and discrete time intervals of equal length (quarterly periods in our case), we can estimate $h_j(\mathbf{x})$ using the following complementary log transformation (cloglog):

$$\log\left(-\log\left[1-h_j\left(\mathbf{x}\right)\right]\right)=\beta'\mathbf{x}+\gamma_j, \tag{19}$$

where γ_j represents the baseline hazard at period j, i.e. the functional form of γ_j captures the pattern of duration dependence. We use two commonly used specifications of γ_j: one parametric (Weibull) and one semi-parametric (Cox). If survival time follows a Weibull distribution, γ_j is captured by $\log(j)$ as an additional new variable along the vector of covariates ($\mathbf{x}$):

$$\log\left(-\log\left[1-h_j\left(\mathbf{x}\right)\right]\right)=\beta'\mathbf{x}+\log(j). \tag{20}$$

Alternatively, following Cox (1972) we can choose to not impose a specific functional form on γ_j and instead include individual duration period dummies together with $\mathbf{x}$ (which cannot contain an intercept) That is we estimate the following semi-parametric cloglog model:

$$\log\left(-\log\left[1-h_j\left(\mathbf{x}\right)\right]\right)=\beta'\mathbf{x}+\gamma_1 D_1+\gamma_2 D_2+...+\gamma_j D_j. \tag{21}$$

All estimates of fraud termination hazard rates reported in the papers are based on the Weibull specification, but the results are all robust to instead using the above Cox specification (these estimation results are available upon request).

It can be important to consider unobserved firm heterogeneity in duration models. Our estimations take such unobserved firm heterogeneity into account in a manner similar to dealing with random firm effects in a linear regression setting (see Internet Appendix I.B for a more thorough discussion).

B. Empirical Hypotheses

In this section, we develop empirical hypotheses for determinants of the hazard rate of fraud termination based on the model implications. The focus of both the model and our empirical analysis is on the direct effect of information producers (such as auditors and analysts) and managerial effort on fraud termination hazard rates. However, as we discussed in sections I.B.2 and I.B.3, firm and market characteristics and their changes over time may impact on the difficulty information producers face in interpreting the signals issued from the firms. Thus, in order to net out spurious correlations resulting from their omission, it is important to control for these characteristics before we test our main hypotheses. Full empirical definitions of the variables discussed below are provided in the Appendix.

B.1. Firm Characteristics and Market Factors

Fundamental firm characteristics can be related to both the amount of scrutiny a firm receives by information producers as well as the efficacy of managerial effort to conceal the fraud. The characteristics we consider are: firm size, profitability, leverage, and variables that capture the nature of the firm's assets and operations, such as the fraction of 'soft' assets, and the market-to-book ratio. We also control for industry fixed effects.

Firm size may be related to the duration of accounting misconduct in a few ways. Large firms have relatively richer information environments than small firms. A richer information environment should make the marginal cost of issuing an additional fraud signal lower for information producers and thus reduce the duration of accounting misconduct. Conversely, large firms also tend to have a wider scope of operations than small firms, which may make it easier for a manager to conceal misconduct. As a result, the effect of firm size on fraud termination hazard rates is ex-ante ambiguous.

When a firm is not performing well, managers seek ways to improve performance. One way of reaching this goal may be accounting fraud. Hence, poor firm performance may provide motivation for managers to start and prolong a fraud (Harris and Bromiley (2007)). However, poor firm performance itself may induce more scrutiny from outsiders. Thus, the effect of performance on misconduct duration is unclear, ex-ante.[11] We use both accounting-based (return on equity) as well as market-based (stock return) firm performance measures in our analysis.

A firm's capital structure can be linked to the duration of fraud spells. On the one hand, a very high level of leverage indicates the firm is near financial distress, increasing managerial incentives to conduct and maintain fraud. However, excessive leverage is also likely to increase scrutiny by creditors as well as other stakeholders such as shareholders, employees, customers, suppliers, business media, etc. Thus, the predicted effect of leverage on the fraud termination hazard is undetermined.

The nature of a firm's assets and operations may influence fraud duration. Managers of firms

[11] Note that we use the restated accounting data as reported by Compustat in our empirical analysis, so our accounting performance measure captures the actual performance during the fraud spell.

with more intangible assets or other assets without well-established replacement or market values have more discretionary power over financial reports. Consequently, such accounts may be easier to manipulate over a longer time period. Growth opportunities may also impact the length of misconduct spells. For example, managers of firms with few growth opportunities are likely to have greater incentives to conduct and maintain accounting fraud in order to appear more valuable than they actually are. On the other hand, firms with many growth opportunities may be harder to evaluate, having an easier time perpetrating fraud. Of course, there could be other characteristics of a firms' assets and operations that affect the cost of information production and the ease of conducting fraud. However, to the extent that these other characteristics are industry-based, we control for their influence in our estimations by including industry fixed effects for broad industry groups.

Finally, the extent and quality of monitoring by market actors may vary with the state of the market. For example, Povel, Singh, and Winton (2007) develop a model where investors' beliefs about business conditions affect their monitoring intensity, resulting in more monitoring in perceived good times than in bad. As a consequence, more frauds are started when market conditions are relatively good and detected when market conditions turn for the worse. To capture investors beliefs about the market we control for overall stock market return. To control for broader time variation in the market related to the intensity of monitoring or the quality of fraud signals, we use calendar period indicators.

B.2. Information Producers: Auditors

The auditing process gives auditors periodic access to internal firm information that is generally not accessible to outside monitors. Thus, auditors are key information producers that in a one-on-one comparison are likely to be more effective at identifying signals of accounting fraud than other information producers such as financial analysts. At the same time, because auditors are tasked with issuing statements regarding the firms' annual accounts, they only produce potential signals of fraud following the 4^{th} fiscal quarter of the year. Based on Implication 1 from our model, these facts lead to the immediate empirical prediction that fraud termination hazard rates increase following each 4^{th} fiscal quarter. That is due to the combination of both mechanically adding an information producer every 4^{th} quarter and the presumption that the average quality of the signal issued by this information producer is high. Because our data is on a higher than annual frequency (i.e. quarterly), we can directly test this prediction in our hazard rate estimations by including a dummy variable indicating whether the last fraud quarter was the 4^{th} fiscal quarter.

The quality of the auditor may of course also affect the likelihood of detecting accounting irregularities. Big N auditors are generally considered to be of higher quality compared to other auditing firms.[12] If that is true, we would expect any positive effect of auditing on fraud termination hazard rates to be greater for firms with Big N auditors.[13] We test for this hypothesis in our empirical analysis by interacting the 4^{th} fiscal quarter dummy with an indicator for whether that particular year's statement was audited by a Big N auditor.

Auditor tenure may impact the likelihood of a fraud signal as well. Most studies find that longer auditor tenure with a firm increases auditing quality (see DeFond and Zhang (2013) for a review of this evidence). On the other hand, if agents acknowledge this positive effect of auditor tenure on auditing quality, a recent change in auditor may itself raise suspicion that the firm is trying to hide something and induce closer inspection by other stakeholders. Thus, the effect of auditor tenure on the fraud termination hazard could be either negative or positive. We test for the effect of auditor tenure by including an interaction between the 4^{th} fiscal quarter dummy and an indicator for whether that particular year's annual statement was audited by a new auditor company.

Finally, we test the effect of an actual observable signal auditors issue by adding explanatory language in otherwise unqualified auditor reports. In principle, we should not expect any impact of these additional explanations, since they usually reveal innocuous information related to the firm's accounting or the audit procedures. For concreteness, the most frequent comments are related to changes in accounting standards (i.e. new FASB pronouncements), explanations that audits may have happened in different days, and indications that some accounts or subsidiaries may have been audited by a different company. However, Czerney et al. (2014) empirically analyze the impact of audit explanations on the likelihood of future restatements of financial reports and find a significant positive association. Although Czerney et al. (2014) look at restatements rather than fraud, support for audit explanations also signaling outright accounting misconduct can be found in Beasley et. al. (2010). The later employ a sample of 347 fraud firms matched to similar peers and find that in 56% of the fraud firms, auditors gave an unqualified opinion that included an explanatory paragraph.

[12] See the review of auditor quality by DeFond and Zhang (2013) for in-depth arguments and evidence in favor of this view.

[13] However, there exist arguments and evidence in support of non-Big N auditors being at least equally as good as Big N auditors. For example, Lawrence, Minutti-Meza, and Zhang (2011) use a propensity score matching method and find evidence that suggests that higher auditing quality among Big N firms disappear once differences in client characteristics are controlled for.

Conversely, only 36% of matched non-fraud companies received the same explanatory paragraphs. These previous results suggest that explanatory language can signal accounting fraud. We test for this possibility by including an interaction between the 4^{th} fiscal quarter dummy and an indicator for whether that particular year's unqualified auditor report contained explanatory language.

B.3. Information Producers: Financial Analysts

Like auditors, financial analysts are important information producers that facilitate monitoring of firms. For example, Yu (2008) shows that firms followed by more analysts manage their earnings less and that the effect is greater for more experienced analysts. Given this importance, Implication 1 of the model directly suggests that the fraud termination hazard rate is increasing in the number of analysts following a firm. However, unlike for the case of auditors, more than one analyst can generate fraud signals for the same firm at any given time. Thus, the introduction of additional analysts may raise the concern of correlated signals weakening the marginal impact on the fraud termination hazard as presented by Implication 2 of the model. For instance, to the extent that analysts have similar skill sets, they may be prone to commit the same mistakes or interpret information in the same way (Hong and Page (2001) and Eeckhout and Pinheiro (2014)). Alternatively, herding may lead to correlated signals (Welch (2000), Clement and Tse (2005), and Jegadeesh and Kim (2010)).

We test the prediction that increased analyst following increases the fraud termination hazard rate but at a decreasing rate by including a dummy variable for having at least one analyst following the firm as well as the log of (1 + the number analysts). Based on Implications 1 and 2 of the model, we expect both variables to be positively associated with fraud termination hazard rates. The analyst indicator variable captures the effect on the hazard rate of having at least one analyst vs. no analyst coverage, while log of (1 + number of analysts) allows us to estimate the additional marginal effect from adding more analysts.

Like for auditors, some analysts may be better than others at issuing positive fraud signals. Thus, following Implication 1 of the model, we expect the effect of analyst following on fraud termination hazard rates to be larger for more capable analysts. We use industry specialization as our proxy for analyst quality. For example, analysts following several firms within the same industry may develop industry-specific expertise that makes it easier for them to spot any accounting irregularities (see, e.g., Gilson et al. (2001) for evidence on the relative importance of industry specialization among analysts). We test the hypothesis that analyst quality matters for fraud termination hazard rates by including two separate sets of the analyst variables described above in our estimations, one set for each type of analysts: specialist and non-specialist.

B.4. Managerial Fraud Effort

We consider three different proxies for managerial efforts to make a fraud harder to detect. Specifically, we consider (i) whether a fraud starts in the 1^{st} fiscal quarter, (ii) number of areas of the financial statement affected by the fraud, and (iii) the magnitude of total accruals. All these variables are at least partly under control of the management, which qualifies them as effort measures. We discuss each one of these variables below.

As we discussed in the previous section, auditors only thoroughly scrutinize the annual financial statements. Thus, if a firm starts a fraud in the 1^{st} fiscal quarter, the firm has more time to adjust the fraud details before the additional scrutiny of auditors. This suggests that if a manager can optimally choose when to start a fraud, the 1^{st} fiscal quarter of any given fiscal year is a likely candidate. In this sense, given that starting in the 1^{st} fiscal quarter indicates management's effort in designing a more complex and harder to detect fraud, we would expect that these frauds are associated with a higher fraud benefit. If that is true, the model – and in particular Implications 4 and 5 – then predicts that these frauds are likely to be longer as well as less likely to be stopped by management, and consequently they are expected to continue until a bad signal is issued. Moreover, if starting a fraud in the 1^{st} fiscal quarter indicates premeditation, we would expect effort to decrease over time as the marginal benefit of effort is likely lower in this case.

Similarly, the fact that a fraud affects more areas of the financial statement would indicate a more complex fraud, as well as an effort to conceal the fraud by making sure all financial statement accounts agree with each other and consequently no "red flags" appear due to inconsistencies across accounts. Hence, as with the 1^{st} fiscal quarter, we would expect frauds that affect multiple areas to be longer. By observing the evolution of this variable over time, it is possible to extend the analysis further by distinguishing between "slippery slope" and "premeditated" frauds. Unfortunately, we are not yet able to do so because our data do not identify when a particular account was manipulated along the duration of the misconduct.

Our final proxy for managerial effort is total accruals. In order to paint a more accurate picture of the current financial condition of a firm, accountants accrue for differences in the timing of economic actions (e.g., earning revenues and incurring expenses) and the exchange of cash associated with those actions. Typically, these accruals require some estimation which is subject to managerial discretion. As a result, prior research argues that management's incentives can drive how they adjust their accruals (e.g., Jones (1991) and Dechow et al. (1995)).

If the manager of a firm wishes to manipulate its accounting by making an accrual that should not be made (e.g., recording revenue and an account receivable for a contract that does not exist), its accrual-based income statement will deviate from reported cash flows. Further, because accruals reverse at the end of the period, a manager who wishes to maintain a fraud must continue to make that accrual each year after the fictitious accrual is made (e.g., the account receivable must be maintained on the books). If a firm's financial condition deteriorates and more accrual manipulation is required, it becomes more costly to the firm (and therefore requires more effort by the manager) because it needs to report the new fictitious accrual as well as all prior ones. Thus, large amounts of accruals suggest that the manager is exerting extra effort to maintain the fraud.

A benefit of the accruals measure as an effort proxy compared to 1^{st} fiscal quarter and total number of financial statement areas affected by the fraud is that its dynamics is observable. For this reason, we can see how changes in total accruals and, consequently, changes in the fraud concealing effort may impact changes in the hazard rate over time. A drawback on this measure is that increasing accruals may also generate a red flag to information producers, so managers' discretionary power over accruals is constrained. In particular, there is a large literature that indicates that investors see large accruals as an indication of aggressive accounting, making the financial statements less representative of the true economic health of the company (see Beneish, Lee, and Nichols (2012)).

B.5. Gross Earnings Related Fraud and Information Production

If information producers have to choose how they spend their effort, Implication 6 suggests that they choose to scrutinize the areas of financial statements that are of most interest to monitors. Because investors are mostly concerned about firms' profitability and cash flows, it is reasonable that earnings tend to be the main focus of analysts' and investors' attention. In particular, we believe a firm's gross earnings (revenues minus cost of goods sold) would be a key component of interest to investors, as gross earnings directly reflect the core profitability of firms' business operations. Thus, we hypothesize that misconduct directly affecting gross earnings related accounts are associated with higher fraud termination hazard rates. We test for this by including a dummy variable indicating the accounting misconduct identified in the AAERs is related to gross earnings in the income statement.

III. Data and Sample

A. *Accounting and Auditing Enforcement Releases (AAERs): Data and Sample Selection*

The SEC regularly reviews companies for violations of securities laws pertaining to financial statements. Reviews can be triggered by media attention, anonymous tips, or by something within an SEC filing itself, such as a restatement that brings attention to a company. Moreover, the SEC reviews about one third of public companies annually and check them for compliance with GAAP (Dechow et al. (2011)). If, as a result of the review, the SEC believes that the company, an officer, or auditor has been engaged in accounting or auditing misconduct, an enforcement action may be taken that results in restatements, lawsuits or some other remedy. These enforcement actions are summarized in the AAERs issued by SEC (AAERs from October 18, 1999 and onward are readily available at http://www.sec.gov/divisions/enforce/friactions.shtml). The AAERs have been used extensively in accounting and finance research as a sample of financial accounting frauds. Our initial dataset is composed of quarterly AAER data from the Center for Financial Reporting and Management at the University of California at Berkeley. This dataset includes detail about the misstatement periods for all AAERs issued by the SEC between May $17^{th.}$ 1982 and August 31^{st} 2012. The initial sample includes 706 unique AAER firms and 926 primary AAERs that cover 7,702 AAER-quarters. For a detailed description of this dataset please see Dechow et al. (2011).

We adjust this dataset by removing AAER firms without adequate data for our duration analysis. Table A.2 explains how we arrived at our final sample. We drop those AAERs without both start and end dates, those that target more than one company, and those related to banks and other financial institutions (SIC 6000-6999) due to their unique regulatory environment. We also drop companies with multiple AAERs occurring at the same time because it is unclear which AAER duration to use. We remove AAERs related to backdating options because of the apparent increase in enforcement proceedings regarding this behavior by the SEC following the widespread attention backdating attracted in the mid-2000s (see Choi, Pritchard, and Wiechman (2013)). The increased focus by SEC resulted in several backdating episodes that had been ongoing for a long time being discovered and issued AAERs. Because of this background of the backdating AAERs and the fact that backdating is different in nature from the regular accounting misconduct we have in mind for our analysis, we think it is prudent to not include them in our sample.[14] We also remove AAERs that start prior to 1982 or after 2006 to address sample selection issues. Specifically, we are concerned that misconduct starting after 2006 are yet to be caught and misconduct that occurred prior to 1982 may have been caught before the inception of the AAER program.[15] In the end, our sample includes 300 unique AAER-firm pairs that cover 2,254 misconduct-quarters.

We can confidently say that the set of AAERs includes intentional misstatements of financial reports. However, the set of AAERs does not include firms with intentionally misstated earnings that were not identified by the SEC (Type II error). As a result, our findings might be due, in part, to correlations between fraud duration and SEC procedures for identifying misstating firms. While this may be the case for small scale intentional misstatements, we are confident that AAERs capture most, if not all, known large scale intentional misstatements. Identification of large scale misstatements requires very little discretion by the SEC because they are generally reported on by the media or ultimately show up in SEC filings (e.g. restatements).

We also believe that our sample is preferable to other samples of fraud related misstatement. Potential alternatives would be lawsuit datasets such as the Stanford Securities Class Action Clearinghouse (SSCAC). While these datasets may do a better job of capturing the entire set of potential frauds relative to AAER datasets, they still suffer from Type II error. They are still unable to capture alleged fraud that has not yet been brought to court. Additionally, these datasets may include frivolous cases (Type I error), a concern that we do not have with AAERs.

Panel A in Table I shows the fraction of AAERs that affect certain areas of the financial statements. The areas of misstatement are from Dechow et al (2011). In particular, we can see that about 65% of the misstatements concern gross earnings related accounts (revenue or cost of goods sold). Moreover, it is clear that many different areas of accounting are affected by misstatement in a non-trivial fraction of times.

Panel B in Table I shows the distribution of our final sample of AAERs by start and end year respectively, as well as the the average fraud duration in terms of consecutive fiscal quarters affected. Considering the whole sample period from 1982 to 2006, the average length of an accounting fraud spell in our sample is 7.5 quarters.

Panel C in Table I displays the cumulative frequency of the fraud duration. Although 50%

[14] However, note that our results are robust to including these backdating related AAERs in the sample.

[15] The SEC began initiating AAERs after congress passed the Foreign and Corrupt Practices Act of 1977. The first AAERs were initiated in 1978. According to Karpoff Lee and Martin (2008), only 20 AAERs were issued prior to the beginning of our sample period (1982). For a review of the 1977 law and its provisions see Maher (1981).

of the fraud spells last at most six fiscal quarters, some continue for almost 8 years (max sample duration is 31 quarters).

B. Other Data

We also include quarterly financial accounting, auditor, stock price, analyst, and institutional blockholder data in our analysis. Auditor and financial accounting data comes from Compustat's Quarterly and Annual databases. Stock return data comes from the Center for Research in Securities Prices (CRSP) database. Analyst and institutional blockholder data comes from Thomson Reuters' I/B/E/S and 13-F databases, respectively. For inclusion in our sample, we require non-missing quarterly data on stock returns and core firm characteristics (RoE, Total Assets, Market-to-Book, Leverage, and Soft Assets).

C. Summary Statistics

Table II shows descriptive statistics for the main variables used in this study. Exact definitions of all variables are provided in the Appendix. In order to provide a clear perspective of the time-varying characteristics, while avoiding double counting of firm observations, we look at the summary statistics at two particular moments within the fraud duration - at the fraud's first and last quarters. In this sense, we are able to identify characteristics that may show significant differences across these two important points in a fraud's life. For variables that are constant over the course of the fraud, we only present their summary statistics at the fraud's first quarter columns.

In terms of the overall magnitudes, it is interesting to highlight that there is at least one analyst present at the onset of the fraud in about 70% of the cases. More important, specialist analysts are present in about 54% of frauds' first quarters, whereas non-specialists are represented in 62% of the cases. Moreover, as we can see from the summary statistics at the frauds' last quarter, not only the presence of analysts seems common place among fraudulent firms, we do not observe a significant change in coverage over the life span of the fraud. In fact the presence, number, and quality of analyst coverage is mostly unchanged once we compare the summary statistics for the 1^{st} and last quarters of the fraud. In fact, none of the analysts' measures shows a difference across the two periods that is statistically significant. In this sense, none of the analysts' results we present in the next section seem to be due to a change in coverage over time, with analysts "jumping the ship" as they see signs of fraudulent behavior.

Regarding the characteristics for which we see changes in magnitude as the fraud progresses, we observe that firms at the fraud termination quarter are bigger, less profitable, and have more soft assets than at the fraud's onset. Moreover, we observe that abnormal stock returns are significantly higher at the fraud's first quarter. In terms of the characteristics of the period when frauds tend to start and to be terminated, we observe that the stock market return – measured by the CRSP Value-Weighted Index – is on average significantly higher at the frauds' first quarter. Even more, considering the auditing cycle, we observe that only 17% of frauds in our sample start in a 4^{th} quarter, while 37% of frauds are terminated after a 4^{th} quarter. In a similar manner, 57.3% of frauds start at a 1^{st} quarter, the furthest away from an auditing episode. This initial evidence points toward an effort to avoid the presence of auditors at the fraud's start, as well as delaying the scrutiny of auditors as long as possible during the fraud's early quarters, potentially in order to better design and structure the fraudulent scheme. Our results in the following sections will present further evidence corroborating this conjecture.

IV. Results

A. *Baseline Results on Fraud Termination Hazard Rates*

As discussed in section II.A, we estimate fraud termination hazard rates using a discrete time cloglog model based on the Weibull distribution of duration dependence, where we also allow for firm heterogeneity in the form of random firm effects. In this section, we present the results for a baseline model using the firm characteristics and market factors discussed in section II.B.1 as determinants. Our measures of firm characteristics are mostly based on accounting information from the quarterly statements. Because the quarterly statements are reported with a time lag, we lag the accounting information one quarter relative to the end-of-fraud indicator (=1 if the misstatement quarter is the last quarter of the fraud spell; =0 otherwise) to ensure that we use measures that not only managers would be aware of but also all interested parties outside the firm.[16]

As a measure for firm size we use the log of book value of total assets adjusted for inflation (log of Total Assets). We measure firm performance both on an accounting and a stock market basis,

[16] It should be noted that we use the standard Compustat Quarterly dataset, which includes restated financial statement values, since Compustat's Unrestated Quarterly data is not available before the year 1987. Thus, not all accounting information we use is necessarily what outside parties were actually observing at the time. However, our results are robust to excluding AAERs for frauds initiated prior to 1987.

where return on equity (RoE) is the accounting measure and the concurrent quarter abnormal firm stock return (i.e. quarterly stock return minus the corresponding CRSP value-weighted (VW) index return) is the market measure.[17] The firm's capital structure is captured by a book value based leverage measure (Leverage). The nature of the firm's assets-in-place is captured by the ratio of soft assets to total assets (Soft Assets), where soft assets are the assets that a manager has relatively more accounting discretion over. These include all assets besides cash, cash equivalents, property, plant, and equipment (see Dechow et al. (2011)). We proxy for the value of growth opportunities by the Market-to-Book ratio (Market-to-Book). Finally, we always include industry dummies for Fama-French 17 industry groups in our estimations to take into account differences in industry characteristics that may affect fraud termination hazard rates but are not captured by the other firm characteristics.

As a measure for concurrent market conditions, we use the corresponding quarterly CRSP VW market index return for each fraud quarter. To capture more slow-moving market conditions that may be related to overall monitoring and enforcement activity we use dummies indicating six different sub-periods (of approximately equal length) of the total sample time period. These time periods are: 1982-1986, 1987-1991, 1992-1996, 1997-2001, 2002-2006, and 2007-2010. The results are robust if we instead use individual calendar year dummies.

Table III presents the estimation results. Model 1 of Table III shows the effect of duration dependence without controlling for any other covariates. Consistent with our model's Implication 3 that the fraud termination hazard rate is naturally increasing over time, the coefficient on log(Period) is positive and significant.

Model 2 of Table III shows the estimation results including the core set of firm characteristics and market factors discussed above. Industry and time period fixed effects are included, but not reported. In any case, their coefficients are all insignificant.

Regarding firm characteristics, Model 2 shows that the log of total assets enters significantly (at the 1%-level) with a negative sign. That is, larger firms are associated within longer spells of accounting fraud. To illustrate the magnitude of the size effect, we estimate hazard rates of fraud termination across the range of durations in our sample (1-31 quarters) for firms at the 25^{th} and 75^{th} percentiles of the firm size distribution, respectively, while holding all other variables fixed at their median values. Figure 1A shows the results of this exercise. We see that the hazard rates are

[17] We use return on equity rather than return on assets since not all firms report operating income in their quarterly statements. However, because we also control for firm leverage, any bias inherent in this should be mitigated.

substantially larger for firms at the 25^{th} percentile of total assets (around \$57 million in year 2000 values) than for firms at the 75^{th} percentile (around \$2.3 billion in assets). For example, assuming firms are at the 6^{th} quarter of a fraud spell (which is the median spell length), the hazard rate of the misconduct ending the next quarter for a firm at the 25^{th} percentile of size is 13.8% whereas the same hazard rate for a firm at the 75^{th} percentile is 10.2%. Thus, although large firms are likely to be scrutinized by more actors, they can maintain false accounting statements for a longer time. One possible explanation for this could be that the scale and scope of a large firm's activities make it easier to hide accounting misconduct.

Besides firm size, firm performance, as measured by both return on equity and firm specific stock returns, is significantly related to the end of accounting fraud. There is a strongly significant negative relation between both RoE and Abnormal Stock Return and the probability of a misconduct spell termination. However, when estimating the marginal effects on the hazard rate of RoE and Abnormal Stock Return for different spell lengths, only the effect of Abnormal Stock Return appears to be important in economic magnitude. Figure 1B shows hazards of ending a misconduct spell for firms with RoE values at the 25^{th} and 75^{th} percentiles, keeping all other variables at their median values. As it can be seen, the estimated hazards are relatively close across the whole range of time, indicating a moderate economic magnitude of the effect. For example, at the 6^{th} quarter of duration, the difference in hazard rates is only 0.8%-points. Therefore, only extremely poor profitability would have a material effect on the hazard rates. The marginal effect of Abnormal Stock Return is illustrated in Figure 1C by showing estimated hazards for firms at the 25^{th} and 75^{th} percentile values of this independent variable. Unlike for RoE, the economic magnitude of the effect appears important. At a spell length of 6 quarters, a firm at the 25^{th} percentile value of Abnormal Stock Return ($= -0.181$) has an estimated hazard that is 2.6%-points higher than a firm at the 75^{th} percentile value ($= 0.139$). This result likely reflects that firms doing well in the stock market attract less critical scrutiny by market actors.

Leverage, the Market-to-Book ratio, and Soft Assets all appear unrelated to the duration of misconduct.

Finally, the concurrent overall stock market return, as measured by the CRSP value-weighted market index return, is significantly negatively related to fraud termination hazard rate. Figure 1D illustrates the magnitude of the effect by showing estimated hazards for firms at the 25^{th} and 75^{th} percentile sample values of the CRSP VW index return. At a spell length of 6 quarters, a firm at the 25^{th} percentile value of CRSP VW index return ($= -0.041$) has an estimated hazard that is

1.5%-points higher than a firm at the 75^{th} percentile value ($= 0.103$). This result is consistent with the notion that monitoring intensity is greater in bad times than in good times.

Overall the results from the baseline model stress the importance of controlling for various core firm and market characteristics when estimating fraud termination hazard rates. Accordingly, we include the full set of variables used in Model 2 of Table III in all subsequent estimations.

B. The Effect of Auditors on Fraud Duration

We next analyze the effect of auditors on fraud termination hazard rates. As discussed in section II.B.2, we recognize that auditors only actively audit firms at the conclusion of the fiscal year. Thus, firms will feel monitoring pressure due to the signals generated by auditors the most following the fourth fiscal quarter. If auditing is effective in curbing accounting misconduct, we would therefore expect hazard rates of fraud termination to be especially high following the fourth fiscal quarter. As outlined before, we estimate this effect by including a dummy variable (4^{th} Quarter) that is equal to one when the concurrent fiscal quarter is the fourth and is zero for the other three fiscal quarters. We also include the full set of variables included in Model 2 of Table III, although we do not report those results due to space considerations.[18] Consistent with auditors being important information producers about corporate fraud, Model 1 of Table IV shows a strong significantly positive effect on the fraud termination hazard rate immediately following the fourth fiscal quarter. The coefficient on the 4^{th} Quarter dummy is positive (0.762) and strongly significant (p-value < 0.01). To further analyze if this regular spike in fraud termination hazard rate is related to the quality of the auditor, we next include the interaction between the 4^{th} Quarter dummy and an indicator for whether the firm's auditor is from a Big N auditing firm. In our sample, Big N firms were responsible for 82% of all audited financial statements. Model 2 of Table IV shows the results from adding this interaction. The coefficient on the interaction is insignificant and the coefficient on the 4^{th} Quarter dummy itself barely changes. Hence, there is no evidence that Big N auditors are better at issuing signals of fraud compared to other auditors.

As an alternative measure for audit quality, we also use an indicator for whether the annual financial statements were audited for the first time by a new auditor, with the presumption that the new auditor would have a harder time issuing signals of misconduct. New auditors were responsible for around 36% of the audited annual statements in our sample. Model 3 of Table IV shows the effect from adding the interaction between 4^{th} Quarter dummy and an indicator for whether the

[18]Full results are available from the authors upon request.

firm's auditor is new. Similarly to the results for Big N auditors, there is no significant effect from this interaction. Thus, whether the auditor is new or not does not seem to affect the signal issued by auditing.

The fourth quarter effect may not necessarily be due to the signals issued directly by the auditors. It could also be that the release of audited annual reports serves as an information focal point that triggers heightened scrutiny by other information producers and monitors such as investors, analysts, media, and regulators. To test if the fourth quarter effect is directly related to the actual information production by auditors, we use an indicator for an easily observed signal issued by them: whether the audit report contains explanatory language or not. Model 4 of Table IV shows the effect from adding an interaction between 4^{th} Quarter dummy and an indicator for the auditor report containing explanatory language. As we can see from the estimation results in Model 4 of Table IV, there is a huge effect on the fraud termination hazard rate if the auditing report contains such comments. The coefficient on the interaction is positive and significant, and more than two and a half times as large as the coefficient on the fourth quarter dummy itself (0.967 vs. 0.346). The latter coefficient is still significant, indicating that there is a positive effect on fraud termination hazard rates following the fourth fiscal quarter even when there is no explanatory language in the auditor report, but the decline in the magnitude between the 4^{th} Quarter dummy coefficient in Model 3 and Model 4 is more than 50%.

Figure 2 illustrates the magnitude of the effects. At a spell length of 6 quarters, the estimated fraud termination hazard rate if the quarter is not a fourth fiscal quarter is 10.3%. If the quarter is the fourth fiscal quarter but the auditor report contained no explanatory language, the corresponding hazard rate is 14.2%. Finally, when the quarter is the fourth fiscal quarter and the auditor report contains explanatory language, the hazard rate jumps to 33.1%. The strong impact of explanatory language in the audit report is consistent with the recent finding by Czerney et al. (2014) that explanatory language is related to restatement risk of the audited financial statements.

Our results in this section directly support our model's implication for the value of adding a marginal, high ability information producer if we want to stop frauds short. Auditors appear to be important information producers in terms of generating credible fraud signals. In fact, our results together with the results in Czerney et al. (2014) suggest that auditors can use rather subtle ways to signal that there is financial statement risk in firms, even if they cannot (or choose not to) communicate direct evidence of misconduct. Thus, although auditors rarely are direct whistle-blowers in fraud cases, as documented by, for example, Dyck et al (2010), they seem to be important information intermediaries that facilitate fraud detection and intervention by others.

C. The Effect of Analysts on Fraud Duration

As discussed in section II.B.3, the second group of information producers we consider are financial analysts. We gather data on analyst following from I/B/E/S. As shown in Table II, around 70% of firms in our sample are followed by at least one analyst. Conditional on analyst following in a firm-quarter, the mean (median) number of analysts in our sample is around 12 (10).

To test the hypothesis that analyst following is associated with fraud detection, we first include a dummy indicating that at least one analyst is following the firm in our estimation of the fraud termination hazard rate. We also include the full set of variables used in Model 4 of Table IV. Model 1 of Table V shows that there is no significant effect of having at least one analyst follow the firm. We next also include log(1 + number of analysts) as an independent variable in our estimations as our model predicts that the effect of analyst following should increase with the number of analysts (Implication 1), but possibly at a decreasing rate due to correlated signals (Implication 2). The results from adding this variable are reported in Model 2 of Table IV. We see that, in contrast to Model 1, now there is a strong positive and significant (at the 5%-level) effect of the analyst presence dummy. That is, a firm with at least one analyst covering it has a significantly higher fraud termination hazard than a firm with no analyst following. Somewhat surprisingly, we find a negative and significant (at the 1%-level) coefficient on log(1 + number of analysts), implying that the marginal effect of having more analysts after the first is declining rather than increasing. In other words, this result suggests that once a firm has more than one analyst covering it, the value of information produced for fraud detection is immediately declining. Herding and free-riding incentives in combination could possibly explain such a direct negative marginal effect of having more than one analyst covering the firm.

However, not all analysts are identical. Some have more experience or expertise in a particular industry that potentially allows them to analyze companies in that industry better. In order to explore this heterogeneity, we divide the analysts in our sample between industry specialists and non-specialists, following Gilson et al. (2001). We consider an analyst an industry specialist if he/she covers at least 5 other firms in the same Fama-French 49 industry in the period.[19] Results are presented in Model 3 of Table V. The benefit of adding analyst coverage appears to solely

[19] We did robustness checks varying the number of firms covered by the analyst from 5 to 10 firms with no significant changes in our results.

come from the introduction of a specialist coverage. We note that, while the specialist dummy is positive and significant at the 1% level across models, the dummy for non-specialist is not significant. Therefore, the introduction of a non-specialist analyst does not significantly change the hazard of ending the misconduct compared to the original no-analyst state. In terms of the marginal effect of adding additional analysts, our results indicate that also in this case this result is driven by industry specialists: adding a specialist decreases the fraud termination hazard rate at the margin (significant at the 1%-level), indicating that there may exist free-riding among specialists. By contrast, non-specialist analyst following does not seem important for fraud termination at all.

Figure 3 illustrates the economic magnitudes of the estimated effects of specialist analyst following by showing the estimated hazards of end of misconduct for firms with: (i) no specialist analyst following (true for slightly more than 40% of the sample), (ii) one specialist analyst following (the 41th percentile), (iii) two specialist analysts following (the sample median), and (iv) for 8 specialist analysts following (the 75^{th} percentile in the sample). The estimates keep all other variables constant at their median values. It is clear that having one specialist analyst following the firm substantially increases the hazard of the misconduct ending compared to having no analyst at all. However, it is also evident that the negative marginal effect of adding more analysts is economically meaningful. In fact, when the firm is followed by 8 specialist analysts, the marginal hazard rate is somewhat lower than for firms having no analyst following at all. Thus, in terms of affecting the duration of accounting fraud, some specialist analyst coverage is good, but too much coverage becomes outright counter-productive.

Finally, given that analyst presence seems to matter for fraud duration, we also include analyst earnings forecast error as a variable capturing the nature of information generated by the analysts. We include this variable since a large earnings forecast error may motivate analysts to scrutinize the firm's financial and operations more carefully in order to figure out why their forecasts were wrong. Also, other interested parties such as investors and business journalists may be induced to scrutinize a firm more the greater an earnings surprise is. We measure analyst forecast error as the absolute difference between the mean analyst forecast of the annual earnings per share (EPS) prior to the earning announcement and the actual reported EPS in a given year, scaled by the corresponding end-of-fiscal year stock price. The variable takes the value of zero if there are no analyst following the firm, and thus the coefficient needs to be interpreted conditional on at least one analyst following the firm.

Model 4 of Table V shows the results from including analyst forecast error along the other analyst following variables. We find that greater forecast error is significantly associated with shorter accounting fraud spells. This is consistent with the view that a greater forecast error attracts greater scrutiny of the firm, which shortens the fraud. It is important to realize that the forecast variable is still heavily skewed towards zero, even conditional on the firm being followed by analysts. Thus, the results are driven by observations in the far right tail of the distribution of forecast error, implying that only extremely large deviations seem to generate more scrutiny of the firm. For most firms the forecast error is too small to materially alter the estimated hazards of fraud termination.

Given that managers manipulate earnings and our sample is comprised of ongoing accounting frauds, a large forecast error may alternatively indicate that the benefit of exerting concealing effort has become marginally negative and the firm decided to stop trying to hide the fraud.

D. The Effect of Managerial Effort on Fraud Duration

We next turn to the impact of managers' efforts in designing and concealing the fraud on the termination hazard. As outlined in section II.B.4, we use three different proxy variables for managerial effort: a dummy indicating that the fraud starts in the first fiscal quarter, (ii) the log of the number of accounting areas being misstated, and (iii) the magnitude of total accruals.

In our sample of 300 AAERs, 57% started their accounting misconduct in the first fiscal quarter. That is a significantly larger fraction than the 25% we would expect if the fiscal quarter in which a firm starts its fraud was totally random. Combined with the large fourth quarter effect on fraud termination hazard we documented above, this lends credence to the idea that frauds started in the first fiscal quarter are likely to be pre-mediated and therefore involve more managerial effort. Managers who intend to engage in accounting misconduct, anticipating the effectiveness of auditing, choose to start the fraud as far from the auditing event as possible in order to perfect the fraud over time, if necessary. If starting a fraud in the first fiscal quarter captures managerial fraud effort, we expect such a fraud to have a significantly longer duration based on our model's implications. Model 1 of Table VI shows the results from adding the 1^{st} fiscal quarter dummy to the set of variables used in the estimation of Model 3 in Table V. As it can be seen, the coefficient on the 1^{st} fiscal quarter dummy is negative with a large magnitude and statistically significant at the 1%-level. This result is consistent with greater managerial effort significantly prolonging fraud duration.

In Model 2 of Table VI we instead add our second proxy for managerial effort, the log of number of areas contaminated by the fraud, to our estimation of the fraud termination hazard rate.

The idea is that maintaining a fraud that is broader in scope takes more effort, while making it harder for information producers to spot inconsistencies. Our results show that this proxy for managerial fraud effort is also related to the fraud termination hazard in the predict way: the coefficient on the log of number of areas is negative and significant at the 1%-level. Thus, the more accounting areas that the fraud affects, the lower the hazard rate of fraud termination.

It is possible that these two proxies for fraud effort capture different aspects of fraud effort. For example, starting the fraud in the first fiscal quarter may indicate more effort in terms of planning whereas the number of areas affected may indicate more effort in terms of the execution of the fraud. To allow for this possibility, we include both fraud proxies in the specification whose results are reported in Model 3 of Table VI. As it can be seen, although the magnitude of the coefficients of both variables decrease somewhat, they are both still significantly negatively related to the fraud termination hazards. Thus, they may indeed capture complementary aspects of fraud effort.

Figure 4A illustrates the economic impact of starting the fraud in the first fiscal quarter on the fraud termination hazard rate based on the estimates in Model 3 of Table VI, while holding all other variables constant at their median values. We can see that there is a very substantial negative effect. For concreteness, for a fraud spell that has reached 6 quarters of duration, the probability of fraud termination the next quarter is about 16%-points lower if the firm started its misconduct in the first fiscal quarter relative to firms that did not start the misconduct in the first fiscal quarter. Figure 4B illustrates the corresponding economic magnitude for the number of areas affected. At a fraud spell duration of 6 quarters, the marginal effect on the fraud termination hazard of going from the 25^{th} percentile value of areas affected (one area) to the 75^{th} percentile (three areas) is a reduction of 3.4%-points.

The two proxies for managerial effort we considered so far are not time-varying. As discussed in section II.B.4, we also study a third proxy for managerial effort, whose time path we are able to track: total accruals. This is defined as the difference between net income and operating cash flows scaled by the average of total assets over the period. We observe this variable on an annual basis (we get too many missing observations if we instead use quarterly data). Model 4 of Table VI shows the estimation results from adding this variable alongside the other two proxies for managerial fraud effort. Consistent with our predictions, we find a significantly negative impact on fraud hazard rates also from the total accruals measure. Figure 4C illustrates the economic impact. Holding all other variables constant at the median values and considering a fraud spell in its 6^{th} quarter, moving total accruals from the 25^{th} percentile sample value to the 75^{th} percentile value decreases the fraud termination hazard by 1.3%-points.

E. The Effect of Gross Earnings Related Misstatements on Fraud Duration

As discussed in section II.B.5, a straightforward extension to our model yields the prediction that frauds that affect areas of the accounting statements that information producers scrutinize harder are more likely to be shorter. We hypothesized that information producers (and monitors) care especially about the accuracy of reported gross earnings, which would then make gross earnings (i.e, revenue and costs of goods sold) related fraud harder to maintain than fraud affecting other financial accounts.

In Model 1 of Table VII we add a dummy variable (Gross Earnings Related) indicating whether the fraud affected reported revenues or operating costs (or both) alongside the full set of variables included in Model 3 of Table VI. These results show that frauds affecting gross earnings tend to be shorter. The coefficient on the Gross Earnings Related dummy is positive and significant at the 1%-level. Figure 5 shows the magnitude of the estimated effect. For example, if a misconduct spell is in its 6^{th} quarter and the misstatement is gross earnings-related, the hazard of ending the fraud next quarter is 13.5% versus 9.7% if the misstatement is not gross earnings-related.

F. The Effect of Institutional Blockholders on Fraud Duration

So far we have assumed that all firms are facing more or less homogeneous monitors that passively wait for fraud signals generated by separate information producers before intervening in a firm. Clearly that is an oversimplification of reality. In this section we allow for heterogeneity in monitors by considering the effect of the presence of blockholders on fraud termination hazard duration. By virtue of the size of their ownership stakes, blockholders are more likely to have incentives to incur the costs of both generating information on their own as well as actually intervening when there is a red flag (see, e.g., Shleifer and Vishny (1986)). We focus on institutional blockholders rather than private individual blockholders as the former are more likely to be independent monitors concerned about shareholder value alone. We gather data on blockholdings by financial institutions from Thomson Reuter's database of 13-F filings. We focus on three variables: (i) a blockholder dummy indicating the presence of at least one 5% institutional owner; (ii) the fraction ownership held by all institutional blockholders; and (iii) the fraction ownership by the largest institutional blockholder.

In Model 1 of Table VIII, we extend the specification in Table VII by including the blockholder dummy. In Model 2 we replace the blockholder dummy with the fraction of ownership by blockholders. Finally, in Model 3 of Table VIII, we instead use the fraction ownership by the largest institutional blockholder. The latter two variables are included to capture the relative size of the blockholders' economic incentives. The blockholder variables are insignificant in all three alternative specifications. Thus, this particular form of outside monitoring does not seem to have an incremental impact on the duration of accounting misconduct. Moreover, all of the previous results are robust to the inclusion of these variables.

V. Robustness Checks

In this section, we present some robustness tests showing that our results do not rely on outliers. In particular, we replicate our previous results for relevant subsamples that divide the overall dataset in terms of fraud duration as well as firm size at the time of the fraud onset. First, we limit our sample to frauds that last at least three quarters. This exercise allows us to see if there is anything intrinsically different about short frauds that may bias our results. Second, we consider a sample trimmed at the 10^{th} and 90^{th} size percentiles, calculated based on the log(Total Assets) at the last quarter before the fraud starts. Reestimating our specifications using this subsample allows us to test if the previously reported results are driven by very small or very large firms. Finally, we further investigate how sensitive our results are to the fraudulent firm's size, by dividing our total sample in two subsamples: Small firms' subsample, comprised of firms that are below the median of log(Total Assets) in period 0 and the large firms' subsample, with firms above the median. Results are presented in Table IX following the same specification presented in Table VII. In Internet Appendix C, we provide results for other specifications. Since results are qualitatively similar to the ones presented in Table IX, we omit them here.

Overall, results in Table IX are qualitatively the same as the ones presented for the overall sample in Table VII. As presented in Model 1 of Table IX, restricting our sample to frauds that last 3 quarters or more not only do not change our results qualitatively, but it actually makes our results quantitatively stronger. Similarly, the results for the trimmed sample, presented in Model 2 of Table IX, are quite similar to the ones presented in Table VII. The only distinction is that the level dummy indicating 4^{th} Quarter is not statistically significant, while in Table VII this coefficient is significant at the 10% level. In any case, the coefficient for the interaction between the 4^{th} Quarter dummy and the presence of explanatory language is still highly significant, corroborating the importance of auditors' oversight. Finally, the results for the subsamples of large and small firms, presented in Models 3 and 4 of Table IX, respectively, are also fairly consistent with the ones obtained for the overall sample. The few minor differences observed are likely due to the disparity in the richness of information environment across the two subsamples. In particular, the results for 4^{th} quarter and analysts indicate that the presence of analysts, as well as the auditing process itself, may generate more new information for small firms, while large firms may demand an auditor signal in order to generate further scrutiny.

In summary, the results that presented here, as well as the ones presented in the Internet Appendix, show that our findings are robust to focusing on frauds that last more than 2 quarters as well as focusing on fraudulent firms with different initial sizes.

VI. Conclusion

In this paper, we investigate the impact of information producers – in particular auditors and financial analysts – and managerial effort to conceal accounting misconduct on the duration of financial statement fraud. We build a simple model that shows how accounting fraud duration is related to the presence and quality of information providers as well as the firm's efforts to hide the fraud. In order to test the model implications, we gather a database of 300 unique AAER-firm pairs that cover 2,254 firm-quarters - with start dates from 1982 until 2006.

Overall, our empirical results corroborate the implications of the model. In terms of the presence of information producers, our results show that the fact that auditors scrutinize the yearly financial statements significantly increases the likelihood of detection, notably if explanatory language has been added to the auditor report. Moreover, this effect is independent of whether the auditing firm is a Big N firm or not, as well as independent of whether the auditing firm has previously audited the firm's statements or not. In terms of analyst coverage, we show that being followed by a specialist analyst significantly increases the likelihood of fraud termination, although the inclusion of additional specialists appears to generate herding and free-riding and consequently has a negative effect at the margin. The inclusion of non-specialists has no effect on fraud termination.

Regarding the efforts engaged by management to conceal a fraud, we show that starting a fraud in the first fiscal quarter, and consequently having time before financial statements are properly audited, significantly increase fraud duration. Moreover, frauds that affect more areas of the financial statements are also significantly longer, indicating that more complex frauds (which are likely higher effort frauds) are also harder to spot. Finally, firms that have higher total accruals, an indication of more aggressive accounting and consequently less informative statements, also have longer frauds on average. In summary, we show that managerial effort can significantly prolong the expected duration of financial statement fraud.

Lastly, we show that frauds that affect areas that investors may care the most, for example the income statement, tend to be more short-lived than the frauds that do not affect them. This result indicates that information producers more carefully scrutinize financial statement areas that investors care most about. Alternatively, investors pay more attention to signals for such areas.

By focusing on the determinants of the duration of accounting fraud, our paper provides a complementary approach to past studies that have focused on fraud prediction or the role of different types of whistle blowers for fraud detection.

Appendix

Table A.1
Variable Definitions

Variable	Description
End of Misconduct	An indicator variable equal to 1 for the final quarter misstated and 0 otherwise
log(Period)	The natural log of the count of quarters misstated at time t (count continues until fraud is caught; i.e. failure =1)
log(Total Assets)	The natural log of total assets (Compustat Quarterly atq) adjusted for inflation
RoE	Income before extraordinary items / average total equity (Compustat Quarterly ibt/(teqt - teqt-4))
Market-to-Book	Market value of assets to book value of assets (Compustat Quarterly (atq-ceqq+cshoq*prccq)/atq))
Leverage	Debt to assets ratio (Compustat Quarterly (dlcq + dlttq)/atq)
Soft Assets	Percentage of assets with accounting flexibility from Dechow et al. (2011) (Compustat Quarterly (atq-ppentq-cheq)/atq)
CRSP Value-Weighted Index	CRSP value-weighted index quarterly return
Abnormal Stock Return	Firm quarterly stock return - CRSP value-weighted index quarterly return
4^{th} Quarter	An indicator variable equal to 1 if the quarter is the fourth fiscal quarter

Start 1^{st} Quarter	An indicator variable equal to 1 if the first misconduct quarter is the first fiscal quarter
Big N Auditor	An indicator variable equal to 1 if the auditor is KPMG, Ernst & Young, PricewaterhouseCoopers, Deloitte & Touche, Arthur Anderson or their precursors (=1 if Compustat Quarterly AU = 1,2,3,4,5,6,7 or 8) and 0 otherwise
Audit Explanation	An indicator variable equal to 1 if Compustat variable auop is different from 1 (unqualified opinion with no explanatory language) and 0 otherwise
New Auditor	An indicator variable equal to 1 if the financial statments are audited by a new auditor and 0 otherwise
log (1+ Number of Analysts)	The natural log of one plus the number of analysts issuing year end forecasts in the I/B/E/S detail dataset
Specialist dummy	An indicator variable equal to 1 if the analyst covers 10 or more firms in the same Fama-French 48 industries in the same period
log (1+ Number of Specialists)	The natural log of one plus the number of specialist analysts issuing year end forecasts in the I/B/E/S detail dataset
abs(Mean Forecast Error)	The absolute value of the average analyst forecast error for EPS in fiscal year t scaled by the stock price at the end of fiscal year *t*
log(number of areas)	The natural log of the total number of areas misstated by the company (including revenue, receivables, cogs, inventory, reserves, debt, mkt securities, assets, pay, and liabilities)
Gross Earnings-Related Areas	An indicator variable equal to 1 if the misstatement affected gross earnings related areas in the income statement and 0 otherwise
Total Accruals	(Net income - Operating Cash Flows) / Average Total Assets (Compustat Annual)

Table A.2

Sample

Description	AAER Firms	AAERs
Total Sample from Dechow Ge Larson & Sloan 2011 Quarterly Database	706	926
Drop AAERs without start and end dates, AAERs that sued more than 1 company, AAERs where the reason is unclear & companies with multiple AAERs	(177)	(397)
Drop Banks and Financial institutions (SIC 6000-6999) and missing industry information	(98)	(98)
Drop option backdating AAERs	(14)	(14)
Drop case dismissed by court	(1)	(1)
Drop AAERs that start prior to 1980 or after 2007	(12)	(12)
Drop firms with missing stock price data in CRSP or missing financial statement data in Compustat Quarterly	(104)	(104)
Sample for initial regressions	**300**	**300**

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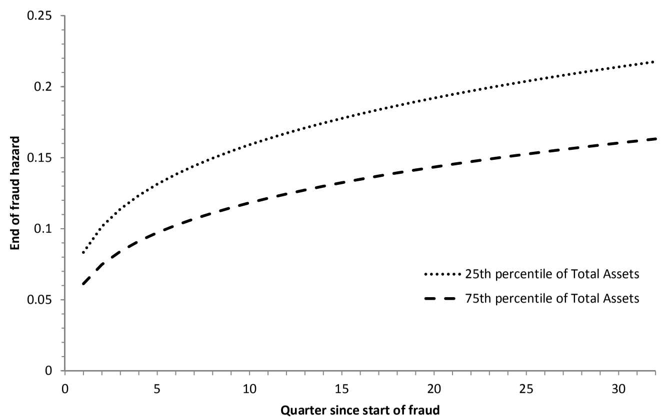


Figure 1A. Firm size and end of fraud hazards. The figure shows the estimated hazards of end of fraud as a function of quarters elapsed since the start of the misconduct. The hazards are estimated at the 25th percentile and 75th percentile sample values of book value of total assets, holding all other variables constant at their median sample values. The hazard estimates are based on Model 2 of Table III.

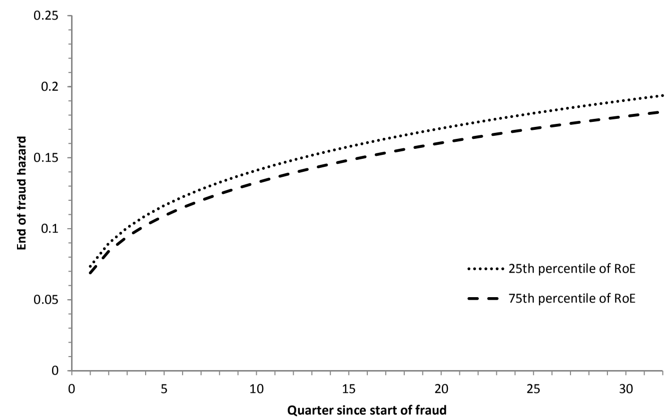


Figure 1B. Profitability and end of fraud hazards. The figure shows the estimated hazards of end of fraud as a function of quarters elapsed since the start of the fraud. The hazards are estimated at the 25th percentile and 75th percentile sample values for return on equity (RoE), holding all other variables constant at their median sample values . The hazard estimates are based on Model 2 of Table III.

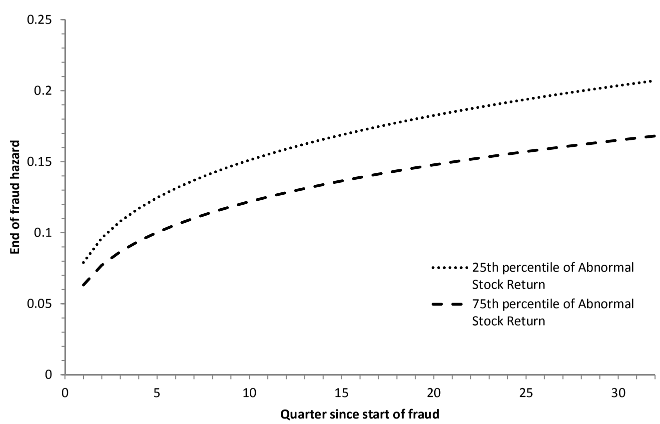


Figure 1C. Firm stock performance and end of fraud hazards. The figure shows the estimated hazards of end of frau as a function of quarters elapsed since the start of the fraud. The hazards are estimated at the 25th percentile and 75th percentile sample values for the firms quarterly abnormal stock return, holding all other variables constant at their median sample values. The hazard estimates are based on Model 2 of Table III.

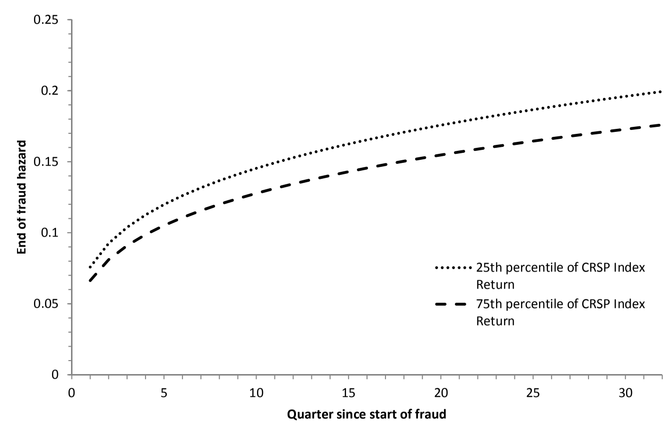


Figure 1D. Stock market stock performance and end of fraud hazards. The figure shows the estimated hazards of end of fraud as a function of quarters elapsed since the start of the fraud. The hazards are estimated at the 25th percentile and 75th percentile sample values for the firms quarterly stock return (Stock Return), holding all other variables constant at their median sample values. The hazard estimates are based on Model 2 of Table III.

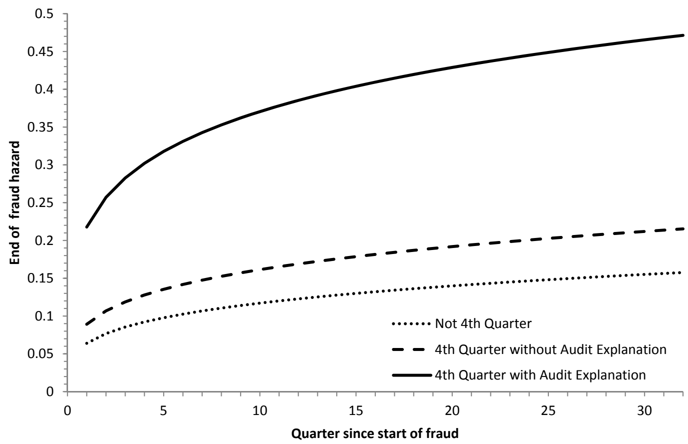


Figure 2. Fourth fiscal quarter and end of misconduct hazards. The figure shows the estimated hazards of end of fraud as a function of quarters elapsed since the start of the fraud. The hazards are estimated based the quarter not being the 4th fiscal quarter, being the 4th fiscal quarter without an audit explanation, and being the 4th fiscal quarter with an audit explanation. The hazard estimates are based on Model 4 of Table IV.

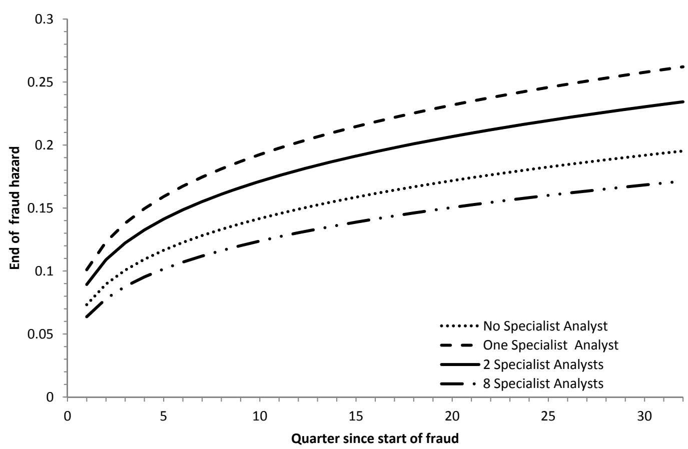


Figure 3. Specialist Analyst following and end of misconduct hazards. The figure shows the estimated hazards of end of fraud as a function of quarters elapsed since the start of the fraud. The hazards are estimated for firms with: no analyst following, with one specialist analyst following (the 41st percentile), with two specialist analysts following (the median), and with 8 analysts following (the 75th percentile); holding all other variables constant at their median values. The hazard estimates are based on Model 3 of Table V.

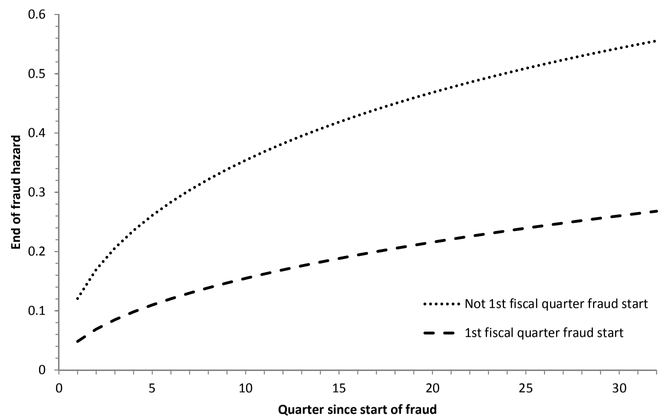


Figure 4A. First fiscal quarter fraud start and end of fraud hazards. The figure shows the estimated hazards of end of fraud as a function of quarters elapsed since start of the fraud. The hazards are estimated for firms that started their fraud in the first fiscal quarter and firms that started their fraud any other fiscal quarter, holding all other variables constant at their median sample values. The hazard estimates are based on Model 3 of Table VI.

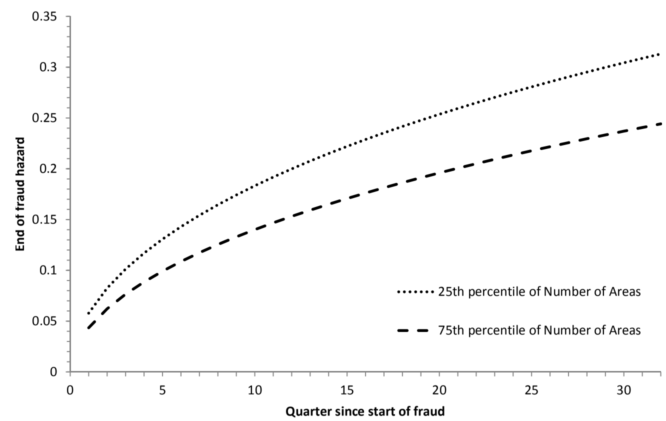


Figure 4B. Number of affected accounting areas and end of fraud hazards. The figure shows the estimated hazards of end of fraud as a function of quarters elapsed since the start of the fraud. The hazards are estimated at the 25th percentile and 75th percentile sample values of affected accounting areas (one and three areas, respectively), holding all other variables constant at their median sample values. The hazard estimates are based on Model 3 of Table VI.

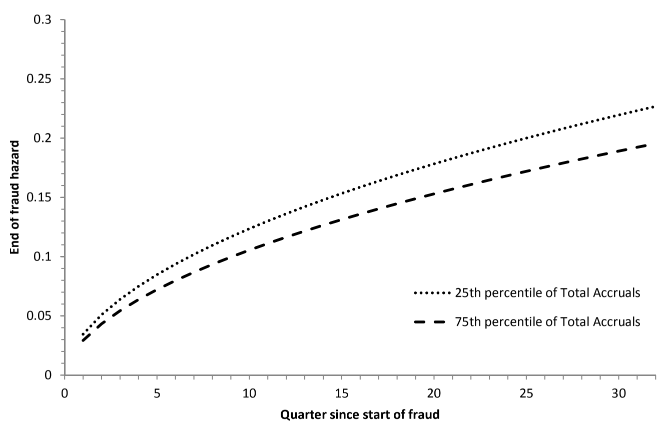


Figure 4C. Total accruals and end of fraud hazards. The figure shows the estimated hazards of end of fraud as a function of quarters elapsed since the start of the fraud. The hazards are estimated at the 25th percentile and 75th percentile sample values of total accruals, holding all other variables constant at their median sample values. The hazard estimates are based on Model 3 of Table VI.

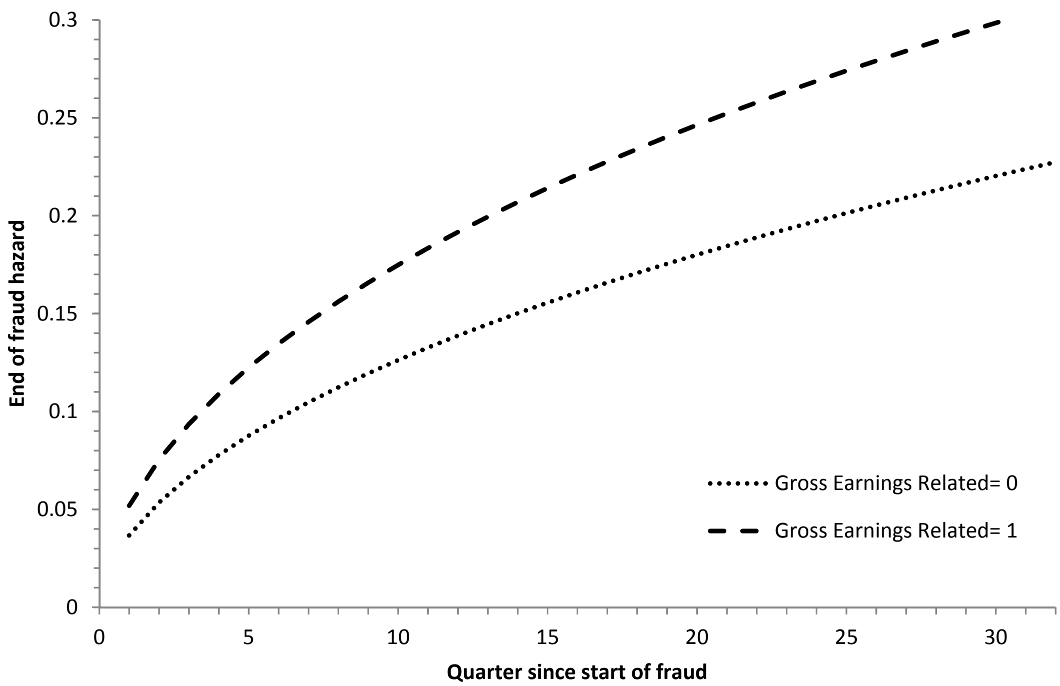


Figure 5. Gross earnings related fraud and end of misconduct hazards. The figure shows the estimated hazards of end of fraud as a function of whether the accounting misstatement is directly related to gross earnings or not. The hazards are estimated for firms that had an earnings related misstatement (Gross Earnings Related=1) as well as for firms that did not (Gross Earnings Related=0), holding all other variables constant at their median sample values. The hazard estimates are based on Model 1 of Table VII.

Table I

Description of Fraud Sample

The table reports key characteristics of a sample of 300 SEC AAERs over the 1982 to 2010 period. Panel A shows how these frauds are divided in terms of the areas of the financial statements affected by the misconduct. Panel B shows how these frauds are distributed across time, both in terms of time of origination as well as termination. Finally, Panel C shows how the sample frauds are distributed in terms of duration (in quarters).

Panel A: Misconduct per Area

Type of Misconduct	Fraction
Revenue	64.3%
Costs of goods sold (cogs)	13.3%
Gross earnings-related (revenue or cogs)	64.60%
Other expense/shareholder equity account	35.7%
Accounts receivable	24.3%
Inventory	19.7%
Capitalized costs as assets	18.3%
Reserve account	10.0%
Liabilities	9.3%
Payables	5.3%
Allowance for bad debt	3.7%
Marketable securities	1.3%

Table I (cont.)

Description of Fraud Sample

Panel B: Frequency of Misconducts per year

	Start year of fraud		End year of fraud	
Year	**Frequency**	**Avg. Fraud Duration** (in quarters)	**Frequency**	**Avg. Fraud Duration** (in quarters)
1982	6	6.2	1	2.0
1983	3	7.0	3	5.3
1984	5	5.2	5	4.0
1985	7	5.6	7	5.1
1986	6	7.3	3	4.7
1987	5	2.2	6	5.0
1988	2	8.5	6	7.8
1989	8	3.6	6	3.8
1990	7	3.9	9	5.0
1991	11	4.6	7	3.9
1992	14	6.3	11	4.4
1993	9	3.9	10	4.1
1994	7	5.1	8	4.4
1995	6	9.2	5	6.8
1996	13	8.7	7	5.9
1997	14	10.6	6	6.8
1998	23	9.3	13	6.1
1999	37	8.0	23	4.8
2000	39	8.6	30	5.6
2001	29	7.5	34	7.6
2002	16	10.8	26	9.0
2003	13	6.2	20	9.7
2004	9	10.3	18	13.3
2005	7	5.6	17	12.1
2006	4	7.0	6	8.2
2007			6	15.5
2008			4	16.5
2009			2	18.0
2010			1	23.0
Total	**300**	**7.5**	**300**	**7.5**

Table I (cont.)

Description of Fraud Sample

Panel C: Cumulative Frequency of Fraud Duration

Fraud Duration (in quarters)	**Freq.**	**Percent**	**Cum.**
1	35	11.67	11.67
2	29	9.67	21.33
3	19	6.33	27.67
4	36	12	39.67
5	20	6.67	46.33
6	27	9	55.33
7	19	6.33	61.67
8	24	8	69.67
9	8	2.67	72.33
10	6	2	74.33
11	10	3.33	77.67
12	18	6	83.67
13	7	2.33	86
14	2	0.67	86.67
15	5	1.67	88.33
16	5	1.67	90
17	2	0.67	90.67
18	2	0.67	91.33
19	6	2	93.33
20	7	2.33	95.67
21	1	0.33	96
22	1	0.33	96.33
23	3	1	97.33
24	6	2	99.33
30	1	0.33	99.67
31	1	0.33	100

Table II
Descriptives

The table reports summary statistics for a sample of 300 SEC AAERs over the 1982 to 2010 period gathered from an updated version of the sample in Dechow et al. (2011). The definitions of all variables are presented in Table A.1. Differences of means are used in two-tail t-tests. *, **, and *** represent significance levels at 10, 5, and 1%, respectively.

	Fraud's First Quarter				Fraud's Last Quarter				
Variable	**Mean**	**Median**	**Std. Dev.**	**N**	**Mean**	**Median**	**Std. Dev.**	**N**	**Diff. means**
log(Total Assets)	5.276	5.062	2.374	300	5.571	5.285	2.336	300	0.295^{*}
RoE	0.103	0.027	1.675	300	-0.145	0.005	1.081	300	-0.248^{**}
Market-to-Book	3.010	1.817	3.163	300	2.699	1.675	3.808	300	-0.310
Leverage	0.244	0.228	0.210	300	0.249	0.233	0.195	300	0.005
Soft Assets	0.626	0.677	0.222	300	0.653	0.699	0.215	300	0.027^{*}
Abnormal Stock Return	0.172	0.055	0.642	300	0.029	-0.017	0.405	300	-0.143^{***}
CRSP Value - Weighted Index	0.030	0.037	0.088	300	0.009	0.022	0.090	300	-0.021^{***}
$4^{th.}$ Quarter	0.170	0.000	0.376	300	0.370	0.000	0.484	300	0.200^{***}
Analyst Dummy	0.703	1.000	0.458	300	0.720	1.000	0.450	300	0.017
log(1+ Number of Analysts)	1.483	1.609	1.174	300	1.545	1.609	1.194	300	0.062
Specialist Dummy	0.537	1.000	0.499	300	0.547	1.000	0.499	300	0.010
log(1+ Number of Specialists)	0.958	0.693	1.073	300	0.978	0.693	1.096	300	0.020
Non-Specialist Dummy	0.620	1.000	0.486	300	0.643	1.000	0.480	300	0.023
log(1+ Number of Non-Specialists)	0.971	0.693	0.931	300	1.039	1.099	0.956	300	0.068
Mean Forecast Error	0.026	0.000	0.265	296	0.023	0.000	0.127	300	-0.003
Cash Flow Accruals	0.055	0.031	0.183	236	0.054	0.036	0.167	242	-0.001
$1^{st.}$ Quarter Fraud Start	0.573	1.000	0.495	300	-	-	-	-	-
Log(Number of Fraud Areas)	0.562	0.693	0.551	300	-	-	-	-	-
Gross Earnings Related	0.697	1.000	0.460	300	-	-	-	-	-

Table III

Baseline Model

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1) **End of fraud**	(2) **End of fraud**
log(Period)	0.168** (0.067)	0.299*** (0.080)
log(Total Assets)		-0.086*** (0.030)
RoE		-1.264*** (0.300)
Market-to-Book		0.003 (0.030)
Leverage		0.412 (0.349)
Soft Assets		0.288 (0.302)
Abnormal Stock Return		-0.723*** (0.203)
CRSP Value-Weighted Index		-1.330** (0.646)
Constant	-2.215*** (0.125)	
Industry Dummies	NO	YES
Time Period Dummies	NO	YES
N	2,254	2,254

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table IV

The role of auditors

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table III is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1) **End of fraud**	(2) **End of fraud**	(3) **End of fraud**	(4) **End of fraud**
4^{th} Quarter	0.762***	0.733***	0.726***	0.346**
	(0.123)	(0.226)	(0.129)	(0.163)
4^{th} Quarter x Big N		0.034		
		(0.245)		
4^{th} Quarter x New Auditor			0.262	
			(0.270)	
4^{th} Quarter x Audit Explanation				0.967***
				(0.201)
log(Period)	0.276***	0.276***	0.277***	0.275***
	(0.080)	(0.080)	(0.080)	(0.080)
Control Variables	YES	YES	YES	YES
Industry Dummies	YES	YES	YES	YES
Time Period Dummies	YES	YES	YES	YES
N	2,254	2,250	2,254	2,254

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table V

The role of analysts

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table III is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)	(2)	(3)	(4)
	End of fraud	**End of fraud**	**End of fraud**	**End of fraud**
Analyst Dummy	0.107	0.598**		
	(0.182)	(0.253)		
log(1+ Number of Analysts)		-0.335***		
		(0.124)		
Specialist Dummy			0.557**	0.503**
			(0.223)	(0.224)
log(1+ Number of Specialists)			-0.320***	-0.292**
			(0.116)	(0.116)
Non-Specialist Dummy			0.169	0.118
			(0.237)	(0.238)
log(1+ Number of Non-Specialists)			-0.200	-0.187
			(0.129)	(0.129)
Mean Forecast Error				5.244***
				(1.695)
4^{th} Quarter	0.343**	0.335**	0.344**	0.355**
	(0.163)	(0.163)	(0.163)	(0.163)
4^{th} Quarter x Audit Explanation	0.970***	0.997***	0.974***	0.959***
	(0.201)	(0.201)	(0.201)	(0.201)
log(Period)	0.279***	0.293***	0.303***	0.314***
	(0.080)	(0.080)	(0.081)	(0.082)
Control Variables	YES	YES	YES	YES
Industry Dummies	YES	YES	YES	YES
Time Period Dummies	YES	YES	YES	YES
N	2,254	2,254	2,254	2,246

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table VI
The role of managerial effort

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table III is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)	(2)	(3)	(4)
	End of fraud	**End of fraud**	**End of fraud**	**End of fraud**
1^{st} Quarter	-0.998***		-0.955***	-0.956***
	(0.139)		(0.140)	(0.154)
log(Number of Areas)		-0.366***	-0.267**	-0.227*
		(0.117)	(0.120)	(0.132)
Total Accruals				-1.711***
				(0.386)
4^{th} Quarter	0.267	0.333**	0.263	0.318*
	(0.163)	(0.163)	(0.163)	(0.178)
4^{th} Quarter x Audit Explanation	0.998***	0.995***	1.007***	1.069***
	(0.201)	(0.201)	(0.201)	(0.217)
Specialist Dummy	0.662***	0.544**	0.642***	0.844***
	(0.225)	(0.224)	(0.226)	(0.244)
log(1+ Number of Specialists)	-0.341***	-0.328***	-0.352***	-0.389***
	(0.114)	(0.116)	(0.115)	(0.119)
Non-Specialist Dummy	0.074	0.127	0.048	0.218
	(0.241)	(0.238)	(0.242)	(0.260)
log(1+ Number of Non-Specialists)	-0.132	-0.190	-0.122	-0.067
	(0.128)	(0.128)	(0.128)	(0.132)
log(Period)	0.522***	0.323***	0.531***	0.574***
	(0.090)	(0.081)	(0.090)	(0.099)
Control Variables	YES	YES	YES	YES
Industry Dummies	YES	YES	YES	YES
Time Period Dummies	YES	YES	YES	YES
N	2,254	2,254	2,254	2,046

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table VII

Frauds affecting gross earnings

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table III is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)
	End of fraud
Gross Earnings Related Dummy	0.354**
	(0.144)
1^{st} Quarter	-0.938***
	(0.140)
log(Number of Areas)	-0.334***
	(0.123)
4^{th} Quarter	0.270*
	(0.163)
4^{th} Quarter x Audit Explanation	1.006***
	(0.201)
Specialist Dummy	0.625***
	(0.225)
log(1+ Number of Specialists)	-0.348***
	(0.114)
Non-Specialist Dummy	0.082
	(0.245)
log(1+ Number of Non-Specialists)	-0.166
	(0.130)
log(Period)	0.557***
	(0.091)
Control Variables	YES
Industry Dummies	YES
Time Period Dummies	YES
N	2,254

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table VIII

Institutional Blockholders

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table III is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)	(2)	(3)
	End of fraud	**End of fraud**	**End of fraud**
Blockholder Dummy	0.112		
	(0.147)		
Fraction Ownership by All Blockholders		0.385	
		(0.498)	
Fraction Ownership by Largest Blockholder			1.779
			(1.363)
Gross Earnings Related Dummy	0.355**	0.355**	0.348**
	(0.144)	(0.144)	(0.144)
1^{st} Quarter	-0.941***	-0.941***	-0.941***
	(0.140)	(0.140)	(0.140)
log(number of areas)	-0.333***	-0.328***	-0.331***
	(0.123)	(0.123)	(0.123)
4^{th} Quarter	0.272*	0.269*	0.268
	(0.163)	(0.163)	(0.163)
4^{th} Quarter x Audit Explanation	0.995***	1.002***	0.999***
	(0.202)	(0.202)	(0.202)
Specialist Dummy	0.606***	0.617***	0.597***
	(0.227)	(0.226)	(0.227)
log(1+ number of specialists)	-0.340***	-0.344***	-0.341***
	(0.115)	(0.114)	(0.115)
Non-Specialist Dummy	0.039	0.053	0.025
	(0.251)	(0.247)	(0.248)
log(1+ number of non-specialists)	-0.160	-0.160	-0.155
	(0.130)	(0.130)	(0.130)
log(Period)	0.559***	0.554***	0.558***
	(0.091)	(0.091)	(0.091)
Control Variables	YES	YES	YES
Industry Dummies	YES	YES	YES
Time Period Dummies	YES	YES	YES
N	2,254	2,254	2,254

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table IX

Robustness Tests

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using different subsamples based on our sample of SEC AAERs over the 1982 to 2010 period. Model 1 restricts the sample to the subsample of frauds that last longer than 2 quarters. Model 2 restricts the sample to a trimmed subsample in which we eliminate both firms at the $1^{st.}$ and $9^{th.}$ deciles in terms of log(Total Assets) at the fraud's onset. Finally, Models 3 and 4 restrict the sample to the subsamples of fraudulent firms below and above the median log(Total Assets) at the fraud's onset, respectively. The full set of variables used in Model 2 of Table III is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)	(2)	(3)	(4)
	End of fraud	**End of fraud**	**End of fraud**	**End of fraud**
Gross Earnings Related Dummy	0.798**	0.356**	0.617***	0.168
	(0.381)	(0.165)	(0.225)	(0.225)
1^{st} Quarter	-1.323***	-0.997***	-1.147***	-0.998***
	(0.401)	(0.159)	(0.205)	(0.217)
log(Number of Areas)	-0.534*	-0.395***	-0.374*	-0.304*
	(0.281)	(0.136)	(0.192)	(0.179)
4^{th} Quarter	0.367*	0.084	0.468**	0.060
	(0.199)	(0.189)	(0.216)	(0.256)
4^{th} Quarter x Audit Explanation	1.144***	1.011***	0.550*	1.330***
	(0.251)	(0.243)	(0.306)	(0.297)
Specialist Dummy	0.851**	0.771***	0.773**	0.227
	(0.410)	(0.247)	(0.382)	(0.374)
log(1+ Number of Specialists)	-0.573**	-0.485***	-0.456	-0.139
	(0.254)	(0.142)	(0.288)	(0.160)
Non-Specialist Dummy	0.217	-0.072	0.244	-0.402
	(0.370)	(0.282)	(0.391)	(0.433)
log(1+ Number of Non-Specialists)	-0.314	-0.160	-0.540*	0.110
	(0.219)	(0.159)	(0.277)	(0.171)
log(Period)	2.195**	0.549***	0.751***	0.613***
	(1.037)	(0.101)	(0.142)	(0.138)
Control Variables	YES	YES	YES	YES
Industry Dummies	YES	YES	YES	YES
Time Period Dummies	YES	YES	YES	YES
N	2,155	1,711	912	1,336

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Internet Appendix to

"Information Production, Misconduct Effort, and the Duration of Corporate Fraud"

Example of time-varying hazard function

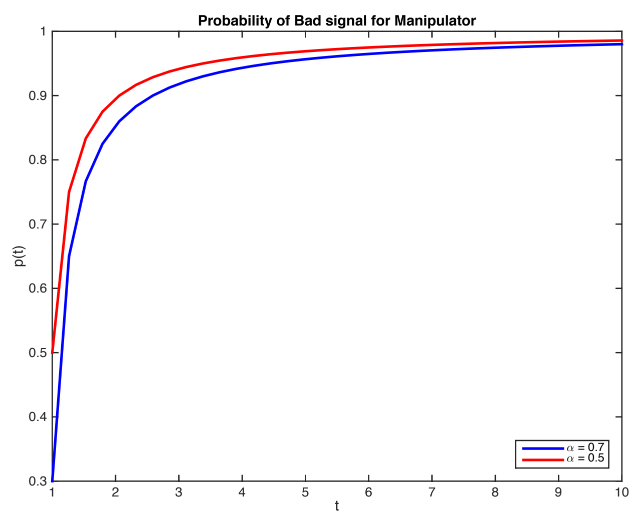
Assume that the probability of a bad signal for a manipulator that has an ongoing fraud for t periods is given by:

$$p(t) = 1 - \frac{\alpha}{t}. \tag{A.1}$$

Naturally

$$\frac{\partial p(t)}{\partial \alpha} = -\frac{1}{t} < 0 \quad \text{and} \quad \frac{\partial^2 p(t)}{\partial \alpha \partial t} = \frac{1}{t^2} > 0. \tag{A.2}$$

The figure below presents a couple of examples for $p(t)$ as we vary α



Notice also that $(1 - p(t)) = \frac{\alpha}{t}$. In this case, the expected duration of the fraud is given by

$$E[N] = \sum_{t=1}^{\infty} t \left(1 - \frac{\alpha}{t}\right) \prod_{t'=1}^{t-1} \frac{\alpha}{t'}.$$

Rearranging it, we have:

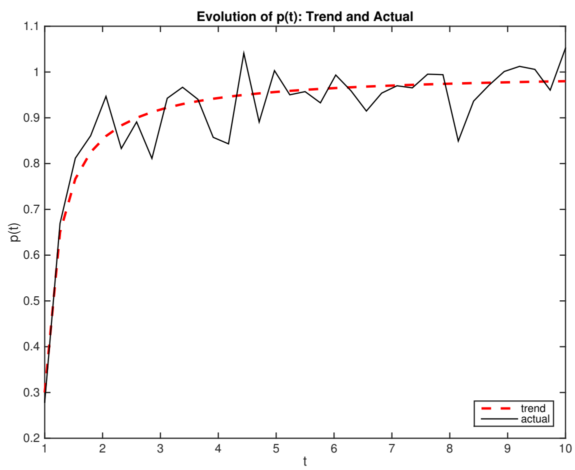
$$E[N] = \sum_{t=0}^{\infty} (t+1) \frac{\alpha^t}{t!} - \alpha \sum_{t=0}^{\infty} \frac{\alpha^t}{t!}. \tag{A.3}$$

Solving it, we obtain:

$$E[N] = (1+\alpha)e^{\alpha} - \alpha e^{\alpha} = e^{\alpha}. \tag{A.4}$$

Therefore, the higher α, the longer the duration of the fraud.

Moreover, even though we imagine that the probability of being detected has an upward trend, the actual probability may vary around the trend. In particular, we may expect that market and firm time-varying characteristics may affect the detection probability, pushing it above or below the long-term trend. For example, good or bad performance in the stock market may increase or decrease incentives to scrutiny, making it easier or harder for information producers to detect signs of manipulation. A similar argument can be made about the firm's own operational and stock market performance. Back to the example presented above, we would have that the graph for $p(t)$ over time would look more like the one in the figure below:



Appendix I.A: Proofs

Proof of Lemma 1: If $\mathscr{H}_t \in \mathscr{H}_t(B)$, we have that $\Pr(M|\mathscr{H}_t) = 1$. But then, it is not optimal to wait to intervene in the company, since $\delta < 1$ and $\mathscr{H}_{t+1} \in \mathscr{H}_t(B)$. $\square$

Proof of Proposition 1: If $\xi P < \mathscr{C}$, we have that at $\mathscr{H}_\emptyset = \emptyset$ it's optimal to wait for a signal instead of immediately intervening to the firm. But then at $t = 1$, if monitors observe a bad signal, as seen in *Corollary 1*, they should intervene to the firm, since $\Pr(M|\mathscr{H}_1) = 1$. On the other hand, if $s_1 = G$, then $\Pr(M|\mathscr{H}_1) = \frac{(1-p)\xi}{(1-\xi)+(1-p)\xi} < \xi$. More generally, we have that, $\forall \mathscr{H}_t \notin \mathscr{H}_t(B), \Pr(M|\mathscr{H}_t) = \frac{(1-p)^t\xi}{(1-\xi)+(1-p)^t\xi} < \xi$. Therefore, $\Pr(M|\mathscr{H}_t)P - \mathscr{C} < 0$, $\forall \mathscr{H}_t \notin \mathscr{H}_t(B)$. Since $\delta E_t[V(\mathscr{H}_{t+1}) \geq 0$, it is not optimal to intervene until a bad signal is observed. $\square$

Proof of Proposition 2:

$$E[N] = \sum_{n=1}^{\infty} np(1-p)^{n-1} = p\sum_{n=1}^{\infty} \frac{d}{d(1-p)}(1-p)^n$$
$$= p\frac{d}{d(1-p)}\sum_{n=1}^{\infty}(1-p)^n = p\frac{d}{d(1-p)}\left[\frac{1-p}{1-(1-p)}\right] = \frac{1}{p}.$$

□

Proof of Proposition 3: Consider that the current number of information providers is **I**. Then, the probability of a bad signal for a manipulator is

$$\Pr(B|M) = 1 - \prod_{i=1}^{\mathbf{I}}(1-p_i).$$

Now let's introduce an additional information provider, then, the probability of a bad signal becomes:

$$\Pr(B|M) = 1 - \prod_{i=1}^{\mathbf{I+1}}(1-p_i).$$

Therefore, the likelihood of a bad signal increases by:

$$1-(1-p_{\mathbf{I+1}}) = p_{\mathbf{I+1}}.$$

Therefore, the better the new information producer, the higher the likelihood of a bad signal for a manipulator.

Similarly, the new expected duration of a fraud is given by

$$E[N] = \frac{1}{1-\prod_{i\in\mathscr{I}+1}(1-p_i)}.$$

While the expected length of a fraud has been reduced by

$$\frac{1}{1-\prod_{i\in\mathscr{I}+1}(1-p_i)} - \frac{1}{1-\prod_{i\in\mathscr{I}}(1-p_i)} =$$
$$= \frac{[1-\prod_{i\in\mathscr{I}}(1-p_i)]-[1-\prod_{i\in\mathscr{I}+1}(1-p_i)]}{[1-\prod_{i\in\mathscr{I}+1}(1-p_i)]\times[1-\prod_{i\in\mathscr{I}}(1-p_i)]}$$
$$= \frac{-p_{\mathbf{I+1}}\prod_{i\in\mathscr{I}}(1-p_i)}{[1-\prod_{i\in\mathscr{I}+1}(1-p_i)]\times[1-\prod_{i\in\mathscr{I}}(1-p_i)]}.$$

As before, the better the new information provider spotting a fraud, the shorter the expected length of the fraud. $\square$

Proof of Proposition 4: We initially present the proofs for items 1 and 3.

Proof of 1. and 3.:

The optimal decision of starting/continuing a fraud at period $t \in \{1,2,...\}$ is given by:

$$\Pi(\mathscr{B},t) = \max\{0+\delta\Pi(\mathscr{B},t),(1-p(t))[\mathscr{B}+\delta\Pi(\mathscr{B},t+1)]+p(t)(-L)\}.$$

If $0+\delta\Pi(\mathscr{B},t) > (1-p(t))[\mathscr{B}+\delta\Pi(\mathscr{B},t+1)]+p(t)(-L)$, then, we have that:

$$\Pi(\mathscr{B},t) = 0+\delta\Pi(\mathscr{B},t).$$

Rearranging it, we have:

$$\Pi(\mathscr{B},t) = \frac{0}{1-\delta} = 0.$$

Therefore, $\Pi(\mathscr{B},t) > 0$ implies that the fraud is started or continued. Consequently:

$$(1-p(t))[\mathscr{B}+\delta\Pi(\mathscr{B},t+1)]+p(t)(-L) > 0.$$

Rearranging it, we have:

$$(1-p(t))\mathscr{B}+p(t)(-L) > -\delta\Pi(\mathscr{B},t+1).$$

By definition $\Pi(\mathscr{B},t+1) \geq 0$. If $\Pi(\mathscr{B},t+1) = 0$, the above expression becomes $(1-p(t))\mathscr{B}+p(t)(-L) > 0$, which concludes the proof. On the other hand, imagine that $(1-p(t))\mathscr{B}+p(t)(-L) < 0$ but $(1-p(t))\mathscr{B}+p(t)(-L) > -\delta\Pi(\mathscr{B},t+1)$. Notice that $\Pi(\mathscr{B},t+1)$ is given by

$$\Pi(\mathscr{B},t+1) = (1-p(t+1))\mathscr{B}+p(t+1)(-L)+\sum_{j=1}^{T-t-1}[(1-p(t+1+j))\mathscr{B}+p(t+1+j)(-L)]\delta^j\prod_{i=0}^{j-1}(1-p(t+1+i)).$$

where T is the optimal time to stop the fraud (if there is no optimal time to stop the fraud, then we can take $T \to \infty$ without changing the argument).

Since $p(.)$ is strictly increasing in its argument, we would have that $\Pi(\mathscr{B},t+1) < 0$, since all its arguments would be negative. As a result, we have a contradiction.

Once we have this result, it is easy to see that as t increases $(1-p(t))\mathscr{B}+p(t)(-L)$ decreases and eventually crosses the zero threshold.

Proof of 2.:

Now we have $p(t) \equiv p$. In this case the problem becomes stationary. Then $\Pi(\mathscr{B},t) \equiv \Pi(\mathscr{B})$

$$\Pi(\mathscr{B}) = \max\{0 + \delta\Pi(\mathscr{B}), (1-p)[\mathscr{B} + \delta\Pi(\mathscr{B})] + p(-L)\}.$$

in which we assume that if the fraud is discontinued, the firm still have the right to continue with the fraud next period, but the duration of the fraud is considered frozen at period t. As we will see, our result is independent of this particular assumption.

So, if the first term in the max operator is the highest, we can easily see that $\Pi(\mathscr{B}) = 0$. Similarly, if starting the fraud is optimal, we have that $\Pi(\mathscr{B}) = \frac{(1-p)\mathscr{B}+p(-L)}{1-\delta}$ which is positive if $1-p)\mathscr{B} + p(-L) > 0$. But once the problem is stationary, the value of continuing the fraud the next period is still the same, so it will be optimal to continue the fraud. So the fraud will continue until the firm is caught. □

Proof of Proposition 5: Both items are proved applying implicit function theorem (IFT) to FOC. For item 1., we have:

$$\frac{\partial e^*(t,\mathscr{B})}{\partial \mathscr{B}} = \frac{-\frac{\partial p(t,e_M)}{\partial e_M}}{\frac{\partial^2 p(t,e_M)}{\partial e_M^2} + C''(e_M)} > 0.$$

While, for item 2, applying IFT we have:

$$\frac{\partial e^*(t,\mathscr{B})}{\partial t} = \frac{-\frac{\partial^2 p(t,e_M)}{\partial e_M \partial t}(\mathscr{B}+L)}{\frac{\partial^2 p(t,e_M)}{\partial e_M^2}(\mathscr{B}+L) + C''(e_M)}.$$

Therefore, the sign of $\frac{\partial e^*(t,\mathscr{B})}{\partial t}$ depends on $\frac{\partial^2 p(t,e_M)}{\partial e_M \partial t}$, i.e., if $\frac{\partial^2 p(t,e_M)}{\partial e_M \partial t} > 0$ we must have $\frac{\partial e^*(t,\mathscr{B})}{\partial t} < 0$. Similarly, if $\frac{\partial^2 p(t,e_M)}{\partial e_M \partial t} < 0$ we must have $\frac{\partial e^*(t,\mathscr{B})}{\partial t} > 0$. □

Appendix I.B: Description of Hazard Model

In this section, we provide a more detailed review of the econometric methodology we use to estimate the determinants of the duration of an accounting misconduct spell. However, before proceeding, it should be pointed out that the literature on duration analysis is quite extensive and that, for this reason, we do not mean to be exhaustive on the subject. Instead, our purpose is to define the basic concepts and to provide the intuition as well as justification for the discrete time duration methods we employ in this paper.[1]*survivor function*. The probability that a fraud is ended within period j is $\Pr(t_{j-1} < T \leq t_j) = F(t_j) - F(t_{j-1}) = S(t_{j-1}) - S(t_j)$. The (discrete) *hazard rate*, h_j, which gives the probability of transition from the initial state in period j conditional on having survived up until period $j-1$, is defined as $h_j := \Pr(t_{j-1} < T \leq t_j | T > t_{j-1})$. The central purpose of this paper is to estimate the (discrete) hazard rate as a function of j and of a vector of covariates $\mathbf{x}$, $h_j(\mathbf{x})$ while allowing for influence of individual heterogeneity.

It is important to note that, from the series of hazard rates over time periods, it is possible to recover the value of the survivor function at the end of period, $S_j := S(t_j)$. Because the probability of survival until the end of period j is equal to the probability of surviving up until period $j-1$ times the probability of not experiencing a transition out of the initial state in period j conditional on not having failed up until period $j-1$, it follows that:

$$S_j = \prod_{k=1}^{j}(1-h_k). \tag{B.1}$$

Equation (B.1) naturally suggests a way to estimate the survivor function nonparametrically. Let R_k be the number of observations at risk of failing at period k, i.e. the ones that have neither transitioned out of the initial state until t_{k-1}. Let M_k be the number of individuals who left the initial state in period k. A consistent estimator of $\Pr(T > t_k | T > t_{k-1}) = 1 - h_k$ is given by $(R_k - M_k)/R_k$. Therefore, a consistent estimator of the survivor function at t_j is given by:

$$\hat{S}_j = \prod_{k=1}^{j}\frac{R_k - M_k}{R_k}. \tag{B.2}$$

[1] More thorough discussions on duration analysis can be found in, e.g., Lancaster (1990) and Wooldridge (2002).

To begin, we note that although time evolves continuously, duration data, notably in social sciences, is often grouped in time intervals: $[t_0, t_1], (t_1, t_2], ..., (t_{K-1}, t_K]$. For ease of exposition, let's assume that all intervals are of equal length and, whenever there is no ambiguity, refer to period $(t_{j-1}, t_j]$ simply as period j. In our particular case the data is recorded at a quarterly frequency and each period j thus represents a three-month interval.

Duration data may be generated in a number of different ways. In our case, data is derived from outflow sampling as we trace back accounting misconduct events from the moment they ended. Thus, we observe the whole misconduct spells. This fact is important, because it implies that we are free of censoring concerns, which are otherwise very prevalent in survival analysis. Hence, since our data is not censored and we aim for concision, we ignore censoring issues in this section.

Let $T > 0$ be the time spent in a certain initial state. In our case, T is the time that a fraud remains active. The probability that a fraud is terminated before or at period j is $F(t_j)$ and the probability that it does not end until period j is $S(t_j) = 1 - F(t_j)$, which is referred to as the

This is the Kaplan-Meier estimator. In addition to it there exists a variety of non-parametric estimators in duration analysis. A prominent one is Nelson-Aalen, which is defined as:

$$\hat{H}_j = \sum_{k=1}^{j} \frac{M_k}{R_k}, \tag{B.3}$$

which is the sum of empirical hazard rates. Combining equation (B.1) with equation (B.3), it is possible to estimate the survivor function as $\hat{S}_j = \exp(-\hat{H}_j)$, which is sometimes called the Fleming-Harrington estimator. Although Kaplan-Meier and Nelson-Aalen estimators have different small sample properties, they are asymptotically equivalent. Obtaining a non-parametric characterization of the survivor function is informative first for its own sake as it provides a visual pattern of $S(t_j)$. Moreover, one can compare survival behavior for different categories of a qualitative variable, such as industry, without imposing any distribution for failure time. Lastly, the examination of the non-parametric estimates may prove helpful in imposing constraints on the parametric models.

In order to estimate the latter, first define a binary response variable y_{ij} taking on value one in case cross section unit i is out of the initial state in period j and value zero otherwise. Reorganize data into a balanced panel format, so that each cross section observation consists of a $(M \times 1)$ vector of binary responses, $\mathbf{y}_i$, and a $(M \times Q)$ matrix of covariates, $\mathbf{x}_i$[2], where M is the lengthiest duration. Since the interest lays on the interval in which $y_{ij} = 1$ for the first time, the model can be expressed in terms of $\Pr(y_{ij} = 1 | y_{ij-s} = 0 \text{ for all } s > 0, \mathbf{x}_i) = \Pr(y_{ij} = 1 | y_{ij-1} = 0, \mathbf{x}_i) = h_j(\mathbf{x}_i)$, where $\mathbf{x}$ may include time-constant as well as time-varying covariates. Once a functional form for $h_j(\mathbf{x}_i)$ is specified, the model is estimated by maximum likelihood.

Now, suppose that the hazard function can be expressed in the *proportional hazard* form, $\theta(t, \mathbf{x}) = \theta_0(t)\lambda$, where $\lambda = \exp(\beta\mathbf{x})$. In this case, from equation (B.1), it follows that $S(t_j, \mathbf{x}) = \exp(-\lambda H_j)$, where $H_j = \int_0^{t_j} \theta_0(u)du$. Now, because $h_j(\mathbf{x}) = [S(t_{j-1}, \mathbf{x}) - S(t_j, \mathbf{x})] / S(t_{j-1}, \mathbf{x})$, we obtain that $h_j(\mathbf{x}) = 1 - \exp[\lambda (H_{j-1} - H_j)]$. Taking logs and rearranging, we find that:

$$\log(-\log[1 - h_j(\mathbf{x})]) = \beta\mathbf{x} + \log(H_j - H_{j-1}). \tag{B.4}$$

The proportional hazard specification is commonly referred to as cloglog model for the transformation $\log(-\log(\cdot))$ is known as complementary log transformation. While it is impossible to identify the within interval variation $\gamma_j := \log(H_j - H_{j-1})$ without further assumptions, the cloglog model allows one to remain agnostic about γ_j as long as $\mathbf{x}$ does not contain an intercept, as proposed by Cox (1972). In this paper we follow the two approaches - we fit both parametric models that impose a pattern for duration dependence γ_j

[2] Were our data subject to censoring, in addition to $\mathbf{y}_i$ and $\mathbf{x}_i$, we would also create another vector, $\mathbf{c}_i$, where $c_{ij} = 1$ from the interval that duration of cross section unit i is censored thereafter and $c_{ij} = 0$ before that. By convention, if $c_{ij} = 1$, we would set $y_{ij} = 1$.

and Cox semi-parametric models that place no restrictions on γ_j. We explain how these models are estimated in turn.

First, consider the parametric approach, in which case the behavior of γ_j is specified. Accordingly, vector $\mathbf{x}$, in addition to time-constant and time-varying regressors, also includes a description of the duration dependence. For instance, if survival time follows a Weibull distribution, then duration dependence is captured by $\log(j)$ as a new variable to the vector of covariates.

Next, consider the Cox model. One of the reasons why it is attractive is that a researcher may get around imposing an arbitrary duration dependence shape, so that the model stays nonparametric relatively to time, while it remains parametric with respect to the covariates $\mathbf{x}$. Hence, in the absence of any theoretical argument for a particular duration dependence form, this semi-parametric approach has the advantage of avoiding inconsistency in the covariate coefficients estimates due to misspecification of the baseline hazard function. On the other hand, to estimate it, one needs to add a (possibly long) series of dummy variables to $\mathbf{x}$, which consumes more degrees of freedom than the estimation of a parametric model, such as Weibull. Therefore, when the case for parsimony is strong, the parametric approach may be preferable to the Cox model. In this paper, because the number of complete spells in our data set is relatively modest, we focus attention on the parametric model with Weibull duration dependence and delegate the Cox model as a robustness test.

It is possible to incorporate unobserved heterogeneity into duration models. The way this is usually done is by entering the individual idiosyncratic term, $v > 0$, multiplicatively in the hazard function: $\theta(t, \mathbf{x}|v) = v\theta(t, \mathbf{x})$, where it is also often assumed that v is independent of $\mathbf{x}$ and that the distribution of v is known up to a finite number of parameters with mean normalized to one, for identification reasons, and finite variance σ_v^2. Hence, models of this kind are are essentially random effects models in a duration setting. Two popular choices for the distribution of v are gamma and normal. We assume the latter and estimate the cloglog model with unobserved heterogeneity using the **xtcloglog** program in Stata.[3]

Controlling for unobserved heterogeneity may be important, even when it is assumed independent of the observed variables, for (at least) three reasons. First, to the extent that units with higher v tend to transition out of the initial state more quickly, as the number of periods advances, the fraction of survivors with low v becomes disproportionately higher, implying a hazard that decreases too fast. Thus, when individual heterogeneity is ignored, duration dependence is downward biased (negative duration dependence is overestimated and positive duration dependence is underestimated). This spurious duration dependence is a selection effect. A similar weeding out effect reasoning applies to the impact of an observed regressor at any point of time. When individual heterogeneity is ignored, the proportionate impact of a regressor on the hazard is not constant and independent of time. Moreover, its impact is attenuated.[4]

[3] See http://www.stata.com/manuals13/xtxtcloglog.pdf for a further description of this program.

[4] Lancaster (1979) shows these results under the assumption that v follows a gamma distribution, though they hold more generally.

Appendix I.C: Robustness Checks

A. Excluding short frauds (1-2 quarters long)

Table A.1. End of misconduct hazard: Baseline dropped short frauds (1 or 2 quarters)

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1) **End of fraud**	(2) **End of fraud**
log(Period)	6.315***	3.655***
	(1.038)	(0.873)
log(Total Assets)		-0.387***
		(0.115)
RoE		-1.075**
		(0.483)
Market-to-Book		-0.316***
		(0.084)
Leverage		-0.365
		(0.873)
Soft Assets		-2.556***
		(0.744)
Abnormal Stock Return		-1.178***
		(0.278)
CRSP Value-Weighted Index		-1.094
		(0.840)
Constant	-13.412***	
	(2.011)	
$\log(\sigma_u^2)$	2.870***	1.853***
	(0.323)	(0.458)
Industry Dummies	NO	YES
Time Period Dummies	NO	YES
N	2,161	2,161

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A.2. The role of auditors - Dropped short frauds

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1) **End of fraud**	(2) **End of fraud**	(3) **End of fraud**	(4) **End of fraud**
4^{th} Quarter	1.126***	1.291***	1.115***	0.454**
	(0.187)	(0.373)	(0.202)	(0.219)
4^{th} Quarter x Big N		-0.222		
		(0.398)		
4^{th} Quarter x New Auditor			0.135	
			(0.447)	
4^{th} Quarter x Audit Explanation				1.110***
				(0.280)
log(Period)	5.953***	5.736***	6.679***	3.711***
	(1.235)	(1.300)	(1.259)	(1.136)
$\log(\sigma_u^2)$	2.574***	2.502***	2.784***	1.637***
	(0.410)	(0.447)	(0.375)	(0.619)
N	2,161	2,157	2,124	2,155
Control Variables	YES	YES	YES	YES
Industry Dummies	YES	YES	YES	YES
Time Period Dummies	YES	YES	YES	YES

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A.3. The role of analysts - Dropped Short Frauds

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)	(2)	(3)	(4)
	End of fraud	**End of fraud**	**End of fraud**	**End of fraud**
Analyst Dummy	-0.104	1.180*		
	(0.439)	(0.672)		
log(1+ Number of Analysts)		-0.858**		
		(0.350)		
Specialist Dummy			1.190**	1.161**
			(0.525)	(0.518)
log(1+ Number of Specialists)			-0.807**	-0.778**
			(0.317)	(0.311)
Non-Specialist Dummy			0.230	0.189
			(0.539)	(0.539)
log(1+ Number of Non-Specialists)			-0.492*	-0.475
			(0.296)	(0.296)
Mean Forecast Error				12.036***
				(4.327)
4^{th} Quarter	0.460**	0.496**	0.469**	0.489**
	(0.221)	(0.229)	(0.225)	(0.226)
4^{th} Quarter x Audit Explanation	1.110***	1.134***	1.155***	1.158***
	(0.281)	(0.290)	(0.286)	(0.288)
log(Period)	3.794***	4.221***	4.021***	3.988***
	(1.169)	(1.232)	(1.223)	(1.158)
$\log(\sigma_u^2)$	1.684***	1.904***	1.791***	1.767***
	(0.622)	(0.588)	(0.619)	(0.594)
Control Variables	YES	YES	YES	YES
Industry Dummies	YES	YES	YES	YES
Time Period Dummies	YES	YES	YES	YES
N	2,155	2,155	2,155	2,150

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A.4. The role of managerial effort - Dropped Short Frauds

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)	(2)	(3)	(4)
	End of fraud	**End of fraud**	**End of fraud**	**End of fraud**
1^{st} Quarter	-1.250***		-1.305***	-1.116***
	(0.340)		(0.355)	(0.183)
log(Number of Areas)		-1.010**	-0.381	-0.215
		(0.416)	(0.237)	(0.155)
Total Accruals				-1.763***
				(0.471)
4^{th} Quarter	0.356*	0.461**	0.356*	0.485**
	(0.192)	(0.226)	(0.196)	(0.201)
4^{th} Quarter x Audit Explanation	1.103***	1.178***	1.139***	1.131***
	(0.241)	(0.287)	(0.246)	(0.236)
Specialist Dummy	0.792**	1.162**	0.829**	0.886***
	(0.342)	(0.525)	(0.369)	(0.282)
log(1+ Number of Specialists)	-0.491**	-0.826***	-0.548**	-0.458***
	(0.203)	(0.318)	(0.214)	(0.134)
Non-Specialist Dummy	0.249	0.182	0.173	0.375
	(0.335)	(0.541)	(0.362)	(0.304)
log(1+ Number of Non-Specialists)	-0.215	-0.494*	-0.227	-0.097
	(0.184)	(0.296)	(0.201)	(0.153)
log(Period)	1.690**	4.066***	1.985**	1.361***
	(0.685)	(1.198)	(0.801)	(0.144)
$\log(\sigma_u^2)$	-1.006	1.779***	-0.343	-12.634
	(2.064)	(0.603)	(1.456)	(19.110)
Control Variables	YES	YES	YES	YES
Industry Dummies	YES	YES	YES	YES
Time Period Dummies	YES	YES	YES	YES
N	2,155	2,155	2,155	1,966

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A.5. Frauds affecting earnings - Dropped Short Frauds

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)
	End of fraud
Earnings Related Dummy	0.798**
	(0.381)
1^{st} Quarter	-1.323***
	(0.401)
log(Number of Areas)	-0.534*
	(0.281)
4^{th} Quarter	0.367*
	(0.199)
4^{th} Quarter x Audit Explanation	1.144***
	(0.251)
Specialist Dummy	0.851**
	(0.410)
log(1+ Number of Specialists)	-0.573**
	(0.254)
Non-Specialist Dummy	0.217
	(0.370)
log(1+ Number of Non-Specialists)	-0.314
	(0.219)
log(Period)	2.195**
	(1.037)
$\log(\sigma_u^2)$	-0.103
	(1.582)
Control Variables	YES
Industry Dummies	YES
Time Period Dummies	YES
N	2,155

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A.6. Institutional Blockholders - Dropped short frauds

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)	(2)	(3)
	End of fraud	**End of fraud**	**End of fraud**
Blockholder Dummy	0.374*		
	(0.207)		
Fraction Ownership by All Blockholders		0.775	
		(0.738)	
Fraction Ownership by Largest Blockholder			2.835
			(1.884)
Earnings Related Dummy	0.742**	0.794**	0.713**
	(0.328)	(0.373)	(0.322)
1^{st} Quarter	-1.275***	-1.323***	-1.244***
	(0.346)	(0.393)	(0.346)
log(Number of Areas)	-0.493**	-0.525*	-0.483*
	(0.249)	(0.278)	(0.249)
4^{th} Quarter	0.376*	0.369*	0.358*
	(0.196)	(0.200)	(0.195)
4^{th} Quarter x Audit Explanation	1.105***	1.137***	1.122***
	(0.248)	(0.251)	(0.246)
Specialist Dummy	0.764**	0.835**	0.746**
	(0.380)	(0.409)	(0.376)
log(1+ Number of Specialists)	-0.517**	-0.566**	-0.513**
	(0.236)	(0.255)	(0.235)
Non-Specialist Dummy	0.106	0.166	0.144
	(0.356)	(0.372)	(0.348)
log(1+ Number of Non-Specialists)	-0.275	-0.300	-0.264
	(0.204)	(0.217)	(0.205)
log(Period)	2.015**	2.182**	1.933**
	(0.864)	(1.019)	(0.871)
$\log(\sigma_u^2)$	-0.449	-0.113	-0.590
	(1.679)	(1.570)	(1.882)
Control Variables	YES	YES	YES
Industry Dummies	YES	YES	YES
Time Period Dummies	YES	YES	YES
N	2,155	2,155	2,155

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

B. Splitting between Small and Large Firms

Table B.1.1. End of misconduct hazard: Baseline Small firms

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1) **End of fraud**	(2) **End of fraud**
log(Period)	0.243 (0.214)	0.247 (0.268)
log(Total Assets)		-0.081 (0.079)
RoE		-1.134*** (0.387)
Market-to-Book		-0.080** (0.033)
Leverage		-0.380 (0.508)
Soft Assets		-1.104*** (0.383)
Abnormal Stock Return		-0.511** (0.237)
CRSP Value-Weighted Index		-2.812*** (0.933)
Constant	-2.063*** (0.258)	
$\log(\sigma_u^2)$	-4.974 (26.345)	-2.171 (2.529)
Industry Dummies	NO	YES
Time Period Dummies	NO	YES
N	915	915

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B.1.2. End of misconduct hazard: Baseline Large firms

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1) **End of fraud**	(2) **End of fraud**
log(Period)	0.213**	0.394***
	(0.095)	(0.122)
log(Total Assets)		-0.175***
		(0.063)
RoE		-0.919
		(0.580)
Market-to-Book		-0.024
		(0.062)
Leverage		0.931
		(0.578)
Soft Assets		0.350
		(0.494)
Abnormal Stock Return		-1.102***
		(0.344)
CRSP Value-Weighted Index		-0.237
		(0.900)
Constant	-2.504***	
	(0.193)	
$\log(\sigma_u^2)$	-13.146	-13.475
	(15.067)	(27.352)
Industry Dummies	NO	YES
Time Period Dummies	NO	YES
N	1,339	1,339

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B.2.1. The role of auditors - Small Firms

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)	(2)	(3)	(4)
	End of fraud	**End of fraud**	**End of fraud**	**End of fraud**
4^{th} Quarter	0.722***	0.802***	0.694***	0.481**
	(0.175)	(0.237)	(0.190)	(0.216)
4^{th} Quarter x Big N		-0.142		
		(0.289)		
4^{th} Quarter x New Auditor			0.031	
			(0.412)	
4^{th} Quarter x Audit Explanation				0.575*
				(0.304)
log(Period)	0.350***	0.345***	0.361***	0.366***
	(0.118)	(0.119)	(0.123)	(0.119)
$\log(\sigma_u^2)$	-14.122	-14.131	-14.175	-14.143
	(29.706)	(30.162)	(15.544)	(15.312)
N	915	914	884	912
Control Variables	YES	YES	YES	YES
Industry Dummies	YES	YES	YES	YES
Time Period Dummies	YES	YES	YES	YES

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B.2.2. The role of auditors - Large Firms

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1) **End of fraud**	(2) **End of fraud**	(3) **End of fraud**	(4) **End of fraud**
4^{th} Quarter	0.793***	1.451	0.729***	0.160
	(0.175)	(1.049)	(0.185)	(0.254)
4^{th} Quarter x Big N		-0.672		
		(1.055)		
4^{th} Quarter x New Auditor			0.418	
			(0.374)	
4^{th} Quarter x Audit Explanation				1.261***
				(0.295)
log(Period)	0.358***	0.365***	0.355***	0.370***
	(0.123)	(0.124)	(0.123)	(0.124)
$\log(\sigma_u^2)$	-14.456	-13.173	-13.328	-13.432
	(24.996)	(26.528)	(15.189)	(15.098)
N	1,339	1,336	1,328	1,336
Control Variables	YES	YES	YES	YES
Industry Dummies	YES	YES	YES	YES
Time Period Dummies	YES	YES	YES	YES

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B.3.1. The role of analysts - Small Firms

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)	(2)	(3)	(4)
	End of fraud	**End of fraud**	**End of fraud**	**End of fraud**
Analyst Dummy	0.100	0.578		
	(0.237)	(0.357)		
log(1+ Number of Analysts)		-0.381*		
		(0.221)		
Specialist Dummy			0.877**	0.835**
			(0.368)	(0.370)
log(1+ Number of Specialists)			-0.574**	-0.554**
			(0.281)	(0.280)
Non-Specialist Dummy			0.164	0.167
			(0.383)	(0.381)
log(1+ Number of Non-Specialists)			-0.257	-0.283
			(0.265)	(0.266)
Mean Forecast Error				2.265
				(2.389)
4^{th} Quarter	0.478**	0.477**	0.488**	0.511**
	(0.216)	(0.216)	(0.216)	(0.217)
4^{th} Quarter x Audit Explanation	0.582*	0.605**	0.592*	0.552*
	(0.305)	(0.305)	(0.305)	(0.306)
log(period)	0.371***	0.388***	0.386***	0.387***
	(0.120)	(0.121)	(0.121)	(0.122)
$\log(\sigma_u^2)$	-14.151	-14.636	-13.119	-12.789
	(15.315)	(283.265)	(26.535)	(23.072)
Control Variables	YES	YES	YES	YES
Industry Dummies	YES	YES	YES	YES
Time Period Dummies	YES	YES	YES	YES
N	912	912	912	907

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B.3.2. The role of analysts - Large Firms

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)	(2)	(3)	(4)
	End of fraud	**End of fraud**	**End of fraud**	**End of fraud**
Analyst Dummy	-0.986**	-1.002		
	(0.383)	(0.660)		
log(1+ Number of Analysts)		0.006		
		(0.201)		
Specialist Dummy			0.061	-0.026
			(0.370)	(0.369)
log(1+ Number of Specialists)			-0.078	-0.031
			(0.162)	(0.161)
Non-Specialist Dummy			-0.419	-0.467
			(0.427)	(0.438)
log(1+ Number of Non-Specialists)			0.013	0.074
			(0.169)	(0.171)
Mean Forecast Error				12.098***
				(2.896)
4^{th} Quarter	0.157	0.157	0.164	0.203
	(0.253)	(0.254)	(0.254)	(0.256)
4^{th} Quarter x Audit Explanation	1.252***	1.251***	1.251***	1.277***
	(0.294)	(0.295)	(0.295)	(0.297)
log(period)	0.407***	0.407***	0.389***	0.398***
	(0.126)	(0.126)	(0.126)	(0.127)
$\log(\sigma_u^2)$	-13.626	-13.687	-13.104	-13.103
	(27.511)	(24.489)	(27.219)	(27.118)
Control Variables	YES	YES	YES	YES
Industry Dummies	YES	YES	YES	YES
Time Period Dummies	YES	YES	YES	YES
N	1,336	1,336	1,336	1,335

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B.4.1. The role of managerial effort - Small Firms

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)	(2)	(3)	(4)
	End of fraud	**End of fraud**	**End of fraud**	**End of fraud**
1^{st} Quarter	-1.202***		-1.170***	-1.266***
	(0.203)		(0.204)	(0.239)
log(Number of Areas)		-0.375**	-0.260	-0.142
		(0.182)	(0.187)	(0.229)
Total Accruals				-1.988***
				(0.560)
4^{th} Quarter	0.451**	0.489**	0.456**	0.576**
	(0.216)	(0.216)	(0.216)	(0.255)
4^{th} Quarter x Audit Explanation	0.509*	0.597**	0.512*	0.554
	(0.307)	(0.304)	(0.306)	(0.353)
Specialist Dummy	0.840**	0.818**	0.813**	1.099**
	(0.374)	(0.370)	(0.376)	(0.463)
log(1+ Number of Specialists)	-0.449	-0.535*	-0.430	-0.718**
	(0.280)	(0.283)	(0.283)	(0.361)
Non-Specialist Dummy	0.104	0.231	0.168	0.353
	(0.385)	(0.386)	(0.389)	(0.435)
log(1+ Number of Non-Specialists)	-0.363	-0.351	-0.430	-0.415
	(0.267)	(0.269)	(0.271)	(0.318)
log(period)	0.679***	0.401***	0.679***	0.704***
	(0.137)	(0.120)	(0.136)	(0.161)
$\log(\sigma_u^2)$	-13.392	-14.900	-12.086	-14.253
	(23.830)	(29.997)	(24.901)	(31.442)
Control Variables	YES	YES	YES	YES
Industry Dummies	YES	YES	YES	YES
Time Period Dummies	YES	YES	YES	YES
N	912	912	912	721

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B.4.2. The role of managerial effort - Large Firms

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)	(2)	(3)	(4)
	End of fraud	**End of fraud**	**End of fraud**	**End of fraud**
1^{st} Quarter	-1.033***		-1.001***	-0.899***
	(0.216)		(0.217)	(0.226)
log(Number of Areas)		-0.348**	-0.277	-0.291
		(0.173)	(0.175)	(0.180)
Total Accruals				-2.204***
				(0.619)
4^{th} Quarter	0.076	0.140	0.059	0.104
	(0.256)	(0.255)	(0.256)	(0.259)
4^{th} Quarter x Audit Explanation	1.316***	1.281***	1.335***	1.366***
	(0.297)	(0.296)	(0.297)	(0.300)
Specialist Dummy	0.214	0.076	0.205	0.505
	(0.375)	(0.371)	(0.374)	(0.386)
log(1+ Number of Specialists)	-0.122	-0.084	-0.132	-0.187
	(0.161)	(0.162)	(0.161)	(0.160)
Non-Specialist Dummy	-0.421	-0.442	-0.439	-0.121
	(0.429)	(0.426)	(0.428)	(0.460)
log(1+ Number of Non-Specialists)	0.116	0.029	0.130	0.122
	(0.170)	(0.169)	(0.169)	(0.169)
log(period)	0.594***	0.394***	0.603***	0.618***
	(0.137)	(0.125)	(0.137)	(0.141)
$\log(\sigma_u^2)$	-14.670	-12.674	-12.821	-14.002
	(287.219)	(22.867)	(22.494)	(15.396)
Control Variables	YES	YES	YES	YES
Industry Dummies	YES	YES	YES	YES
Time Period Dummies	YES	YES	YES	YES
N	1,336	1,336	1,336	1,323

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B.5.1. Frauds affecting earnings - Small Firms

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)
	End of fraud
log(period)	0.751***
	(0.142)
Earnings Related Dummy	0.617***
	(0.225)
1^{st} Quarter	-1.147***
	(0.205)
log(Number of Areas)	-0.374*
	(0.192)
4^{th} Quarter	0.468**
	(0.216)
4^{th} Quarter x Audit Explanation	0.550*
	(0.306)
Specialist Dummy	0.773**
	(0.382)
log(1+ Number of Specialists)	-0.456
	(0.288)
Non-Specialist Dummy	0.244
	(0.391)
log(1+ Number of Non-Specialists)	-0.540*
	(0.277)
log(period)	0.751***
	(0.142)
$\log(\sigma_u^2)$	-13.538
	(27.353)
Control Variables	YES
Industry Dummies	YES
Time Period Dummies	YES
N	912

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B.5.2. Frauds affecting earnings - Large Firms

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)
	End of fraud
Earnings Related Dummy	0.168
	(0.225)
1^{st} Quarter	-0.998***
	(0.217)
log(Number of Areas)	-0.304*
	(0.179)
4^{th} Quarter	0.060
	(0.256)
4^{th} Quarter x Audit Explanation	1.330***
	(0.297)
Specialist Dummy	0.227
	(0.374)
log(1+ Number of Specialists)	-0.139
	(0.160)
Non-Specialist Dummy	-0.402
	(0.433)
log(1+ Number of Non-Specialists)	0.110
	(0.171)
log(period)	0.613***
	(0.138)
$\log(\sigma_u^2)$	-14.186
	(15.297)
Control Variables	YES
Industry Dummies	YES
Time Period Dummies	YES
N	1,336

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B.6.1. Institutional Blockholders - Small Firms

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)	(2)	(3)
	End of fraud	**End of fraud**	**End of fraud**
Blockholder Dummy	0.106		
	(0.210)		
Fraction Ownership by All Blockholders		1.336	
		(0.934)	
Fraction Ownership by Largest Blockholder			1.885
			(2.024)
Earnings Related Dummy	0.621***	0.602***	0.605***
	(0.225)	(0.226)	(0.225)
1^{st} Quarter	-1.152***	-1.154***	-1.156***
	(0.206)	(0.206)	(0.206)
log(Number of Areas)	-0.379**	-0.384**	-0.386**
	(0.192)	(0.191)	(0.192)
4^{th} Quarter	0.472**	0.468**	0.467**
	(0.216)	(0.216)	(0.216)
4^{th} Quarter x Audit Explanation	0.538*	0.538*	0.534*
	(0.306)	(0.306)	(0.306)
Specialist Dummy	0.766**	0.859**	0.761**
	(0.382)	(0.390)	(0.382)
log(1+ Number of Specialists)	-0.459	-0.566*	-0.465
	(0.288)	(0.302)	(0.288)
Non-Specialist Dummy	0.219	0.232	0.214
	(0.394)	(0.389)	(0.392)
log(1+ Number of Non-Specialists)	-0.538*	-0.559**	-0.533*
	(0.278)	(0.278)	(0.278)
log(period)	0.755***	0.767***	0.767***
	(0.143)	(0.143)	(0.144)
$\log(\sigma_u^2)$	-13.264	-13.450	-14.472
	(26.794)	(15.096)	(24.971)
Control Variables	YES	YES	YES
Industry Dummies	YES	YES	YES
Time Period Dummies	YES	YES	YES
N	912	912	912

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B.6.2. Institutional Blockholders - Large Firms

The table reports the results of implementing a random effects panel complementary log-log regression of the quarterly fraud termination hazard rate using a sample of SEC AAERs over the 1982 to 2010 period. The full set of variables used in Model 2 of Table 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Table A.1. Standard errors are reported in parentheses.

	(1)	(2)	(3)
	End of fraud	**End of fraud**	**End of fraud**
Blockholder Dummy	0.076		
	(0.238)		
Fraction Ownership by All Blockholders		0.173	
		(0.746)	
Fraction Ownership by Largest Blockholder			1.585
			(2.279)
Earnings Related Dummy	0.170	0.176	0.182
	(0.224)	(0.226)	(0.225)
1^{st} Quarter	-1.001***	-1.006***	-1.004***
	(0.217)	(0.219)	(0.217)
log(Number of Areas)	-0.305*	-0.304*	-0.311*
	(0.179)	(0.179)	(0.180)
4^{th} Quarter	0.059	0.059	0.055
	(0.256)	(0.256)	(0.256)
4^{th} Quarter x Audit Explanation	1.327***	1.331***	1.338***
	(0.297)	(0.297)	(0.298)
Specialist Dummy	0.227	0.230	0.220
	(0.374)	(0.374)	(0.374)
log(1+ Number of Specialists)	-0.137	-0.136	-0.136
	(0.160)	(0.161)	(0.160)
Non-Specialist Dummy	-0.409	-0.401	-0.391
	(0.433)	(0.433)	(0.433)
log(1+ Number of Non-Specialists)	0.111	0.113	0.112
	(0.171)	(0.172)	(0.171)
log(Period)	0.611***	0.609***	0.599***
	(0.138)	(0.140)	(0.140)
$\log(\sigma_u^2)$	-14.681	-14.191	-14.190
	(287.567)	(15.297)	(15.298)
Control Variables	YES	YES	YES
Industry Dummies	YES	YES	YES
Time Period Dummies	YES	YES	YES
N	1,336	1,336	1,336

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

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