Information Production and the Duration of Accounting Fraud*

Jonathan Black Mattias Nilsson

Roberto Pinheiro Maximiliano da Silva

March 13th., 2015

Abstract

In this paper, we develop and test a model that connects information production by auditors and analysts to the expected duration of accounting fraud. Using a sample of AAERs issued by the SEC, we find that the likelihood of fraud detection is significantly greater in the quarter following the issuance of audited financial statements. Furthermore, the presence of explanatory language in the audit report significantly strengthens the result, indicating that the content of the reports is important for reducing fraud duration. We find no evidence that Big N auditors matter for this relationship. In terms of the role of analysts, we find that the presence of industry specialist analysts reduces the expected fraud duration, consistent with our model. However, there is a direct negative marginal effect of adding more specialists suggesting that free riding and herding among analysts impair their ability to illuminate fraud. We find no evidence that the addition of non-specialist analysts affects the length of the fraud. Lastly, we explore the effect of management effort to conceal information on fraud duration. We find that frauds starting in the first fiscal quarter – the furthest from the auditing episode and therefore most likely to be premeditated – tend to be longer. Similarly, complex frauds that affect more areas of the financial statements or have high total accruals also tend to be longer.

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

JEL Code: G34; G38; K22; K42; L51; M41

*Black, Nilsson, and Pinheiro: University of Colorado, Leeds School; Silva: University of São Paulo. A previous version was circulated under the title "What Determines the Duration of Accounting Misconduct?". We thank Henrik Cronqvist, Robert Davidson, Henry L. Friedman, Irena Hutton, Yrjo Koskinen, R. Mark Reesor, Rodrigo Verdi, and audiences at the AAA Western Doctoral Student Interchange, the AAA 2015 FARS Midyear Meeting, the CFA-JCF-Schulich Conference on Financial Market Misconduct, the FMA 2014 Annual Meeting, University of Colorado, and University of São Paulo. All remaining errors are our own. Jonathan Black also thanks the Accounting Doctoral Scholars program for funding.

1 Introduction

Why do some financial statement frauds persist for longer than others? The question is important for at least three reasons. First, more accounting reports are affected by long lasting frauds, therefore distortions caused by the fraudulent firm's financial information are potentially more costly. Indeed, the SEC considers that longer accounting frauds are more serious and requires that firms and managers associated with these frauds face larger penalties following a formal enforcement action (Files 2012). Second, if we want to understand what measures would prevent financial statement fraud, it is instructive to analyze what factors cut short frauds already in place. After all, dishonest managers would not engage in fraud if they did not believe they could maintain it for some period of time. Third, from a more technical perspective, by focusing on frauds already in place, we can to some extent avoid the problem of empirically confounding factors related to commission of fraud with factors that are associated with fraud detection (see Wang (2013) for a discussion of this problem).

In this paper, we focus on the role of information production on accounting fraud duration. By doing so, we complement a stream of research that studies the characteristics of whistle-blowers in fraud cases (e.g. Bowen, Call, and Rajgopal (2010), Dyck, Morse, and Zingales (2010), and Miller (2006)). Our approach is distinct from that literature in that we take an alternative view by considering fraud detection as a function of the signals conveyed by information producers (such as auditors and analysts) as well as the managerial effort to hide the fraud.

We construct a simple model that generates testable implications for our empirical analysis. In our model, information producers periodically scrutinize firms. From information producers' activities, signals are generated indicating that either there is no need for concern (good or G signal) or there is something unusual going on (bad or B signal). Fraudulent firms – called Manipulators – are the only ones that induce information producers to generate a bad signal with positive probability. Based on the observed signals, monitors – institutional investors, the SEC, and board members, among others – decide if they intervene in a firm or not. Because intervention is costly, monitors want to minimize the chances of superfluous actions. Consequently, we show that monitors intervene only if a bad signal is revealed by at least one of the information producers.

Our model shows that the expected duration of a fraud depends on how likely a bad signal can be generated. Moreover, the change in the likelihood of detection in any given period of an ongoing undetected fraud – i.e. the hazard rate – is increasing in the number of information producers. The magnitude of the impact of an additional information producer depends not only on how good the new information producer is at generating a bad signal, but also on how independent his/her signal is from the other information producers scrutinizing the firm.

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 misconduct. We show that only the firms that highly benefit from fraud manipulate their statements. Even more, if the probability of a bad signal increases over time – i.e., information producers become more effective as the fraud progresses or it is harder to hide manipulation as time passes – the frauds not voluntarily stopped by the firm's management are the ones with the greatest benefits to manipulation. If we believe that the firm's manipulation benefits are positively correlated with the market's cost of fraud and ongoing frauds are the ones most likely to be caught, we conclude 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 changes over time as the probability of a bad signal increases. In particular, 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, we show that if there is a learning curve to hiding accounting misconduct and effort becomes more effective as time passes, firms optimally increase their efforts over time. In this sense, 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 we use a sample of financial statement frauds[1] drawn from an updated version of the data set in Dechow, Ge, Larson, and Sloan (2011) containing descriptions of SEC Accounting and Auditing Enforcement Releases (AAERs). This data 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 employ a discrete time hazard rate estimation method to model the distribution of fraud durations without issues of left or right truncation, which could otherwise complicate and potentially bias our analysis.[2] In particular, we measure the duration of fraud as the number of consecutive fiscal quarters affected by the fraud.

Our model predicts that the fraud termination hazard rate increases as we add more information producers. The size of this effect is positively related to the ability of the information producers to generate a signal of fraud. However, this positive effect is attenuated if the information producers' signals are correlated. We test these predictions using two different sets of information producers: auditors and financial analysts.

We find strong evidence that auditors are important information producers, shortening fraud dura-

[1] We use the term 'fraud' interchangeably with terms such as 'accounting misconduct' or 'financial misstatements' throughout the paper. However, it is important to note that the AAERs that constitute our sample of financial statement frauds 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] Of course, 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.

tion significantly. Through their professional task, auditors have access to more of a firm's accounting information than other outside information producers. Consequently, auditors should be able to generate signals about fraudulent activities that other information producers could not. Because auditors only audit the annual financial statement, every fourth fiscal quarter there is an additional potential signal about fraud from a high ability information producer. Consistent with this, fraud termination hazard rates significantly spike following every fourth fiscal quarter. There is no evidence that the perceived quality of the auditor (proxied by 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 rather 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 of the quality of the report. Recent research by Czerney, Schmidt, and Thompson (2014) show that 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 et al. (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. We find that the marginal impact of the fourth fiscal quarter on the fraud termination hazard rate mainly comes from fourth quarters with explanatory language in the auditor report. Thus, it appears that it is the actual signals produced by the auditors that matter the most.

Analyst following has also some impact on estimated fraud termination hazard rates. Following Gilson, Healy, Noe, and Palepu (2001) we divide analysts between industry specialists and non-specialists based on the idea that analysts with industry experience may have a greater ability to detect 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 is strictly declining in the number of specialists following the firm, implying that coverage by too many analysts may be counter-productive at the margin. This result is consistent with analysts and other outside monitors trying to free ride 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), among others). In contrast to industry specialists, non-specialist analysts have no significant effect on the fraud termination hazard.

We also include 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 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. A greater error can be framed as worse quality forecasts or alternatively as bigger earnings surprises. These earnings surprises can be seen by monitors as a red flag since the disagreement between forecasts and actual results may come from accounting misconduct. We find 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 its activities from information producers, thereby prolonging the fraud. The first proxy for managerial effort we consider is an indicator for whether the fraud was started in the first fiscal quarter or not. Managers starting frauds in the first fiscal quarter have more time to optimally 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 in turn is likely to correlate both with a higher fraud benefit and a greater level of effort to conceal fraud. 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[3], which is consistent with the hypothesized impact of effort on fraud duration.

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 effort to conceal the fraud by making sure all financial statement accounts agree with each other. Similar to first fiscal quarter indicator, this measure is significantly negatively related to the fraud termination hazard rate.

Finally we consider the level of total accruals as a measure of managerial fraud effort. Higher total accruals is likely to indicate more aggressive accounting, which in our sample of fraud firms could be a direct indicator of efforts to conceal the fraud. A benefit of this measure compared with the other two proxies for effort is that it is time-varying, and can therefore capture changes in effort over time. Our results indicate that this measure of effort is also significantly associated with lower fraud termination hazard rates. Furthermore, the individual effects of all three effort variables on fraud termination hazards are present at the same time, suggesting that they capture different aspects of fraud effort. Overall, our evidence are consistent with managers optimally choosing to exert effort to hide and prolong frauds.

Our paper is related to both the literature on fraud prediction and the literature on whistle blowers. In terms of fraud prediction, Dechow, Ge, Larson, and Sloan (2011) document that misstating firms that have been issued AAERs tend to have a greater market capitalization than the corresponding 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

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

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. In this sense, any fraud database combines two latent processes: fraud commitment and fraud detection. Consequently, the standard binary response models applied in the fraud literature are ultimately measuring the probability of detected frauds instead of the actual probability of committing them. This subtle difference 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 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.

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, Morse, and Zingales (2010) find that there are six types of players that account for at least 10% of detection, while none is responsible for more than 17%. Together these classes account for 82% of all cases. In particular, 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 U.S.

Our paper complements these literatures by taking an alternative approach. While our analysis is conditional on fraud detection, we focus on the elements that trigger detection sooner than later. In this sense, we investigate the characteristics of fraudulent firms, frauds, information producers, and managerial concealing efforts that may allow frauds to continue for a longer time and consequently produce more damage. Moreover, compared to the whistle blowers' literature, we show that the intervention by monitors and whistle blowers does not happen in a vacuum. In particular, the presence of information producers that generate red flags that induce other agents to act seems essential. In this sense, even though auditors and analysts are not themselves whistle blowers in many cases, the reports that they issue are essential to trigger fraud termination.

In the next section, we develop our model of information production and fraud duration. Section 3 describe the discrete time hazard model we use for our estimations and also develop empirical hypotheses based on the testable implications of our model. Section 4 describes our data. Section 5 presents our results, and section 6 concludes the paper.

2 Model

In this section, we develop a simple model of information production and fraud duration that guides our empirical analysis. Please note that all proofs can be found in Appendix A.

2.1 Basic Model

Consider that there are two types of risk-neutral, long-lived firms: Manipulators (M) and Non-Manipulators (NM). We assume that NMs never misrepresent their financial statements. Differently, Ms regularly manipulate their financial statements in their own benefit. Even though later we may 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 will generalize the results for multiple information producers in the next section. The signals detected by the information producers can be good (G) or bad (B). The probability that a manipulator generates a bad signal is given by $\Pr(B|M) = p$, where p may depend on firm predetermined characteristics, such as size, industry, and so on. On the other hand, non-manipulators always generate a good signal, i.e., $\Pr(B|NM) = 0$. Signals across different financial statements are i.i.d. in this section.

Risk Neutral monitors – comprised of regulators, institutional investors, and board members – observe the signals and decide if they intervene in the firm or not. In order to intervene in a firm and scrutinize it for accounting misbehavior, monitors must incur in a cost $\mathcal{C} > 0$. If they catch a manipulator, monitors obtain a gain of $P > \mathcal{C}$. However, if they intervene in a non-manipulator, the return is normalized to zero. Therefore, in period t, the expected gain of intervention after a history of signals $\mathcal{H}_t$ is given by:

$$u(I, \mathcal{H}_t) = \Pr(M|\mathcal{H}_t) \times P - \mathcal{C}, \tag{1}$$

where I represents intervention and

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

Therefore, we consider that at any time t, a monitor can decide to intervene in the firm or not, based on the history of signals $\mathcal{H}_t$. We represent this by taking the action $a_t \in A = \{I, NI\}$. Hence, we have that the instantaneous expected utility for the monitor is given by:

$$u(a_t, \mathcal{H}_t) = \begin{cases} \Pr(M|\mathcal{H}_t) \times P - \mathcal{C}, & \text{if } a_t = I, \\ 0, & \text{if } a_t = NI. \end{cases} \tag{3}$$

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

$$V(\mathcal{H}_t) = \max_{a_t \in A} \{\Pr(M|\mathcal{H}_t) \times P - \mathcal{C}, \delta E_t[V(\mathcal{H}_{t+1})]\}, \tag{4}$$

where $\delta \in (0,1)$ is the discount rate. Then, we can show a few results. First, let's define $\mathcal{H}_t(B) = \{\mathcal{H}_t \text{ s.t. } \exists\ h_i = B \in \mathcal{H}_t\}$ as the set of histories in which a bad signal was observed at some point. Similarly, define $\mathcal{H}_\emptyset = \emptyset$ as the history at the beginning of the firm.

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

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 $\xi P < \mathcal{C}$, 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 $\xi P < \mathcal{C}$, so monitors only intervene once they observe a bad signal.[4] Based on this result, the length of a fraud is described by a geometric distribution. Consequently, we have the following proposition:

Proposition 2. *The expected length of a fraud is given by*

$$E[N] = \frac{1}{p}. \tag{5}$$

As a result, the better the information producers are spotting frauds, by detecting bad signals, the faster a fraud ends. Before we move to the extensions, keep in mind that the hazard rate function is given by:

$$h(t) = \frac{f(t)}{1 - F(t)} = \frac{p(1-p)^{t-1}}{(1-p)^{t-1}} = p. \tag{6}$$

Notice that the hazard rate is constant, since the geometric distribution is memoryless. In the extensions, we will consider cases in which the hazard rate is time dependent, due to the fact that longer frauds may become easier to catch.

[4]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.

2.2 Extensions

2.2.1 Multiple information producers

Independent signals

Consider in this case that we have $\mathcal{I}$ information producers. In order to study the case in which they are the most efficient, assume that their signals are independent. As before, assume that NM firms never generate a bad signal. Differently, we assume that information provider i generates 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:

$$\Pr(B|M) = 1 - \prod_{i \in \mathcal{I}} (1 - p_i). \tag{7}$$

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

$$E[N] = \frac{1}{1 - \prod_{i \in \mathcal{I}} (1 - p_i)}. \tag{8}$$

As before, the better information providers are spotting a fraud – i.e., the higher p_i for at least some $i \in \mathcal{I}$, the shorter the fraud.

The next proposition consider the introduction of an additional information producer.

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 signals are not independent, we must work with the joint p.d.f. of the signals. Therefore, we have that at least one information producer detects a bad signal is:

$$\Pr(B|M) = 1 - \Pr(s_1 = G, s_2 = G, ..., s_{\mathbf{I}} = G), \tag{9}$$

while the expected fraud duration is given by:

$$E[N] = \frac{1}{1 - \Pr(s_1 = G, s_2 = G, ..., s_{\mathbf{I}} = G)}. \tag{10}$$

As expected, as long as the signals are not perfectly correlated, in the sense that $\Pr(s_i = G|s_1 = G, s_2 = G, ..., s_{i-1} = G, s_{i+1} = G, ..., s_I = G) < 1, \forall i \in \mathcal{I}$, 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 providers, 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.

2.2.2 Fraudster's effort

In this case, consider that the fraudster can exert an effort $e_M > 0$ in order to make harder for information producers to spot irregularities. 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 $i \in \mathcal{I}$. 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 $p_i(e_M^*) = 0$, $\forall i \in \mathcal{I}$. 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:

$$E[N|e_M] = \frac{1}{1 - \prod_{i \in \mathcal{I}}(1 - p_i(e_M))}. \tag{11}$$

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

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.

2.2.3 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.:

$$\Pr(B|M, t) = p(t). \tag{12}$$

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:

$$E[N] = \sum_{t=1}^{\infty} tp(t) \prod_{t'=1}^{t-1}(1 - p(t')). \tag{13}$$

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

Let's now consider a particular example:

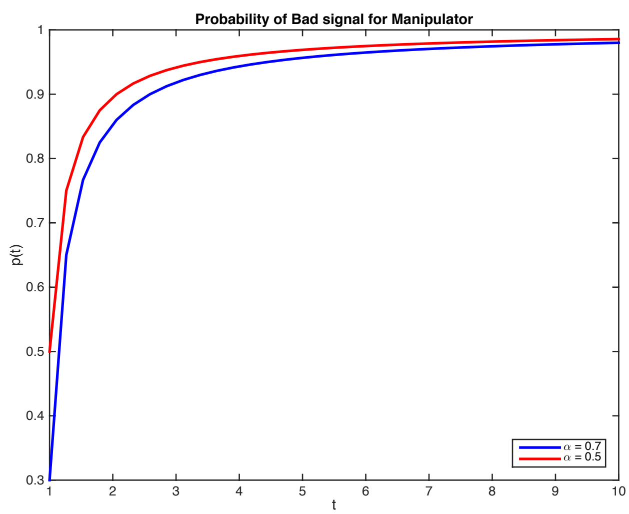
Example 1. *Assume that the probability of a bad signal for a manipulator that has an ongoing fraud for t periods is given by:*

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

Naturally

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

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:

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

Solving it, we obtain:

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

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

2.2.4 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 $\mathcal{B}$ is a draw in the distribution $F(.)$ with support $(0, \overline{\mathcal{B}})$. We also assume that if the firm is caught, it incurs in a loss of $L \equiv \overline{\mathcal{B}}$, 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 $\mathcal{B}$ firm is given by:

$$\mathbf{Profit}(\mathcal{B}, \mathbf{t}) = (1 - p(t))\mathcal{B} + p(t)(-L). \tag{14}$$

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 $\mathbf{Profit}(\mathcal{B},\mathbf{t}) > 0$.

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 $\mathcal{B} \leq \mathcal{B}^*$, where $\mathcal{B}^*$ is given by*

$$(1 - p(1))\mathcal{B}^* + p(1)(-L) = 0. \tag{15}$$

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 $\mathcal{B} > \mathcal{B}^*$ there is a $T(\mathcal{B}) < \infty$ in which if the firm has not been caught up to that point, management decides that it is not profitable to continue the fraud anymore. $T(\mathcal{B})$ is defined by*

$$(1 - p(T(\mathcal{B})))\mathcal{B} + p(T(\mathcal{B}))(-L) = 0. \tag{16}$$

From implicit function theorem, notice that

$$\frac{dT(\mathcal{B})}{d\mathcal{B}} = \frac{(1 - p(T(\mathcal{B}))}{p'(T(\mathcal{B}))(\mathcal{B} + L)} > 0. \tag{17}$$

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 – $\mathcal{B}(t)$ and $L(t)$ – as long as $(1-p(t))\mathcal{B}(t) + p(t)L(t)$ decreases over time. In this sense, as long as $\mathcal{B}(t)$ does not increase faster than $L(t)$ over time, our results are still valid.

2.2.5 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[5] 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

$$\max_{e_M} (1 - p(t, e_M))\mathcal{B} + p(t, e_M)(-L) - C(e_M). \tag{18}$$

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

$$-\frac{\partial p(t, e_M)}{\partial e_M}(\mathcal{B} + L) - C'(e_M) = 0. \tag{19}$$

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

$$-\frac{\partial^2 p(t, e_M)}{\partial e_M^2}(\mathcal{B} + L) - C''(e_M). \tag{20}$$

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 $e^*(t, \mathcal{B})$ pinned down by the FOC.

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

$$(1 - p(t, e^*(t, \mathcal{B})))\mathcal{B} + p(t, e^*(t, \mathcal{B}))(-L) - C(e^*(t, \mathcal{B})) > 0. \tag{21}$$

where $e^*(t, \mathcal{B})$ 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 $e^*(t, \mathcal{B})$, the following is true:*

[5] and only ongoing frauds can be detected

1. $\frac{\partial e^*(t,\mathcal{B})}{\partial \mathcal{B}} > 0$, *i.e., the firms that benefit the most incurring in fraud are also the ones that put more effort to hide it;*

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

2.3 Summary of Model Implications

The model delivers several important empirical implications:

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 generated by different information producers can be correlated, the model yields the following related implication:

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

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

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 termination hazard rates 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 $\mathcal{B}^*$ 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 producer at generating bad signals, the higher the expected benefit of fraud among firms that decide to engage in fraudulent behavior*;
- *If the hazard rate increases over the duration of the fraud, the firms with high benefit of fraud are more likely to be caught, while firms with lower fraud benefit are more likely to stop the fraud by themselves.*

If there is a correlation between the benefit that firms obtain by engaging in fraud and the cost of the fraud for investors and the market as a whole, Implication 4 tells us two important points. First, if measures are taken to make financial statements more transparent and thereby increases the likelihood of bad signals, we should expect that the average fraud that is ultimately incurred and then later caught is a worse fraud. Second, even though this result ultimately says that our sample may be biased, it also says that the sample is biased towards the worse 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.

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.*

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

In terms of how the effort to conceal changes over time, Implication 5 says that if efforts to conceal become less effective over time, the firm puts less and less effort in trying to hide the fraud the longer the fraud. In this sense, if we believe that premeditated frauds are the ones in which the firm puts a lot of effort to design the fraud, it is likely that additional efforts to hide the fraud over time are less effective than the follow-up efforts to conceal frauds that were started without premeditation, due to an unexpected need or opportunity. In this sense, by looking at changes in proxies for effort as the fraud progresses, Implication 5 allows us to distinguish between "premeditated" vs. "slippery-slope" frauds.

Finally, even though we do not directly consider it in the model. if there is any decision of effort allocation by information providers, a straightforward extension would indicate that if some types of frauds are more important than others for investors, information producers may optimally put more efforts in these areas (for example, more focus on income statement). Based on this, the following implication is a natural conclusion:

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

3 Empirical Method and Hypotheses

3.1 Empirical Method

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 Appendix 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:

$$h_j(\mathbf{x}) := \Pr(t_{j-1} < T \leq t_j | T > t_{j-1}, \mathbf{x}). \tag{22}$$

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):

$$\log\left(-\log\left[1 - h_j\left(\mathbf{x}\right)\right]\right) = \beta'\mathbf{x} + \gamma_j, \tag{23}$$

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 ($\mathbf{x}$):

$$\log\left(-\log\left[1-h_j\left(\mathbf{x}\right)\right]\right)=\beta'\mathbf{x}+\log(j). \tag{24}$$

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:

$$\log\left(-\log\left[1-h_j\left(\mathbf{x}\right)\right]\right)=\beta'\mathbf{x}+\gamma_1 D_1+\gamma_2 D_2+...+\gamma_j D_j. \tag{25}$$

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 Appendix B for a more thorough discussion).

3.2 Empirical Hypotheses

In this section we develop empirical hypotheses for determinants of the hazard rate of fraud termination based on the model implications. Although 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, we first discuss the importance of controlling for firm fundamentals and stock market factors that are not explicitly considered in the model, but may affect the probability of a fraud signal or monitors' incentives to intervene given a positive fraud signal. Full empirical definitions of the variables discussed below are provided in Appendix C.

3.2.1 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 capturing the nature of the firm's assets and operations (fraction of 'soft' assets, the market-to-book ratio, 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 generating an additional fraud signal lower for information producers and thus reduce the duration of accounting misconduct. However, 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 of these ways 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.[6] We use both accounting-based (return on equity) as well as market-based (stock return) 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 (shareholders, employees, customers, suppliers, business media, etc.). Thus the predicted effect of leverage on the fraud termination hazard is ambiguous.

The nature of a firm's assets and operations may influence fraud duration. Managers of firms with more intangible assets or other assets without well-established replacement or market values have more opportunity to exercise discretion in 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 to appear more valuable than they 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. To the extent such other characteristics are industry-based, we control for this in our estimations by including industry fixed effects for broad industry groups.

Finally, although we do not consider it in the presented version of the model, a straightforward extension of the model shows that 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 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 and the quality of fraud signals we use calendar period

[6]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 to the extent financial statements were restated after the misconduct ended

indicators.

3.2.2 Information Producers: Auditors

The auditing process give 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 leads to the direct 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 generated 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.[7] 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.[8] We test for auditor quality 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 also have an effect on the likelihood of a fraud signal. 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 this is true, a recent change in auditor may itself be a signal that the firm is trying to hide something and induce closer inspection by monitors. 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.

Finally we test the effect of an actual observable signal auditors generate: the presence of 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

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

[8]However, there is also arguments and evidence that suggests that non-Big N auditors may be 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.

or the audit procedures. For example, 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, Schmidt, and Thompson (2014) empirically analyze the impact of audit explanations on the likelihood of future restatements of financial statements and find a significant positive association. Although Czerney et al. look at restatements rather than fraud, support for audit explanations also signaling outright accounting misconduct can be found in Beasley et. al. (2010). They 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. 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.

3.2.3 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 example, 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 (see, e.g., Welch (2000), Clement and Tse (2005), and Jegadeesh and Kim (2010), among others)..

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 marginal 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 hypothesized decreasing marginal effect from adding more analysts.

Like for auditors, some analysts may be better than others at generating 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, Healy, Noe, and Palepu (2001) for evidence on the relative importance of industry specialization among analysts). We then test the hypothesis that analyst quality matters for fraud termination hazard rates by including two separate sets of the two analyst variables described above in our estimations, one set for each type of analysts.

3.2.4 Managerial Fraud Effort

We consider three different proxies for managerial efforts to make a fraud harder to detect. In particular, we consider (i) whether a fraud starts in the $1^{st.}$ fiscal quarter, (ii) whether the fraud affects more areas of the financial statement, and (iii) the magnitude of total accruals as proxies for managerial effort in order to conceal a fraud. 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 will continue until detected through a bad signal. Moreover, if starting a fraud in the $1^{st.}$ fiscal quarter is a clear indication of 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. In this sense, as with the $1^{st.}$ fiscal quarter, we would expect frauds that affect multiple areas to be longer and to be detected due to bad signals instead of being terminated by management. This variable would also be an ideal test for the evolution of effort over time – i.e. a way to distinguish between "slippery slope" vs. "premeditated frauds". Unfortunately, we are not yet able to identify when a particular account was manipulated in the duration of the fraud.

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 use discretion in their accruals (e.g., Jones 1991, 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 fictitious accruals. 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 this is a time-varying indicator. In this sense, 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 it is that increasing accruals may also generate a red flag to information producers, so there is a limit in how much managers can manipulate accruals. 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 1999; Beneish, Lee, and Nichols 2012).

3.2.5 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. We hypothesize that misconduct directly affecting 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 operating earnings in the income statement.

4 Data and Sample

4.1 Accounting and Auditing Enforcement Releases (AAERs) Data and Sample Selection

The SEC regularly reviews companies for violations of securities laws. Reviews can be brought on by the media, anonymous tips or by something within an SEC filing, such as a restatement that brings attention to a company. In all, the SEC states that they review about one third of public companies and check them for compliance with GAAP (Dechow, Ge, Larson, and Sloan (2011)). If, as a result of the review, the SEC believes that the company has violated securities laws they will further their investigation and may take enforcement action resulting in restatement, lawsuits or some other remedy. SEC enforcement action related to accounting and auditing is summarized in AAERs which are available on the SEC website (http://www.sec.gov/divisions/enforce/friactions.shtml) and 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 17th 1982 and August 31th 2012. The initial sample includes 706 unique AAER firms and 926 primary AAERs that cover 7,702 AAER-quarters. For a detailed description of the initial sample please see Dechow, Ge, Larson and Sloan (2011).

We adjust this dataset by removing AAER firms without adequate data for our duration analysis. Table 1.A explains how we arrived at our final sample. We drop those AAERs without both start and end dates, those which 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 make additional adjustments to address some selection issues. We remove AAERs related to backdating options because of widespread confusion about whether backdating option actually constituted fraud until the mid-2000s. We also remove AAERs that start prior to 1982 or after 2006 to address sample selection issues around those observations at the beginning of the SEC's AAER program.[9] Namely, we are concerned that frauds starting after 2006 are yet to be caught and frauds that occurred prior to 1982 may have been caught before the inception of the AAER program. Our sample includes 300 unique AAER-firm pairs that cover 2,254 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

[9] 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).

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 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 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.

Table 1.B shows the fraction of AAERs that affect certain areas of the financial statements accounting. In particular, we can see that in about 65% of the fraudulent books a misconduct is introduced through the revenue channel. Moreover, it is clear that many areas of accounting are affected by fraud in a non-trivial fraction of times.

Table 1.C shows the distribution of our final sample of AAERs by start and end year respectively, as well as the the average fraud duration. Notice that, considering the whole period, from 1982 to 2006, the average length of an accounting fraud spell is 7.7 quarters.

Table 1.D displays the cumulative frequency of the fraud duration. As we can see, the empirical distribution of fraud duration is positively skewed as over 50% of the events last at most six quarters, although there exist some quite long instances.

4.2 Other Data

We also include stock price, analyst, auditor, and quarterly financial accounting data in our analysis. Auditor and financial accounting data comes from COMPUSTAT Quarterly and stock return data comes from the Center for Research in Securities Prices (CRSP) database. For inclusion in our sample, we require stock price data and non-missing data for core firm characteristics (RoE, Total Assets, Market-to-Book, Leverage and Soft Assets). Analyst data comes from I/B/E/S.

4.3 Summary Statistics

Table 2.A shows descriptive statistics for the main variables used in this study. Exact definitions of all variables used in this study are provided in Appendix C. Inasmuch as this paper is concerned with analysts it is interesting to note that there is at least one analyst present in about 77% of fraud-quarters. Specialist analysts are present in about 59% of fraud quarters whereas non-specialists are represented

in 70.5% of fraud quarters. Table 2.B presents the correlation matrix for the duration of misconduct in terms of number of quarters and the set of firm characteristics that we use in our analysis. Here, it should be noted that there exists significant partial correlation between most pairs of variables. Hence, our rich set of covariates should help us mitigate the possibility of spurious correlation between the explanatory variables and fraud duration.

5 Results

5.1 Baseline Results on Fraud Termination Hazard Rates

As discussed in Section 3.1, 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 3.2.1 as determinants. Our measures of firm characteristics are 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 to ensure that we use measures that not only managers would be aware of but also outside monitors.

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, where return on equity (RoE) is the accounting measure[10] and the concurrent quarter abnormal firm stock return (= quarterly stock return minus corresponding CRSP VW index return) is the market measure. The firm's capital structure is captured by a book value based leverage measure (Leverage). The nature of a firms 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, Ge, Larson, and Sloan (2011)). We proxy for the value of growth opportunities by the Market-to-Book ratio. 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 value-weighted market index return for each fraud quarter. To capture more slow-moving market conditions that may be related to overall monitoring 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-

[10] 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.

1991, 1992-1996, 1997 -2001, 2002-2006, 2007-2010. The results are robust if we instead use individual calendar year dummies.

Table 3 presents the estimation results. Model 1 of Table 3 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. Figure 1 plots the actual empirical survivor probabilities against the estimated Weibull survivor function. As can be seen the fit is extraordinary. Clearly, the Weibull distribution assumption seems appropriate for the duration of our sample frauds.

Model 2 of Table 3 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. However they 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 engage in longer spells of accounting fraud. To illustrate the magnitude of the size effect, we have estimated 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 firm size, respectively, while holding all other variables fixed at their median values. Figure 2A shows the results of this exercise. We see that the hazard rates are 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 nevertheless can maintain false accounting statements for a longer time. It 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 2B shows hazards of ending a misconduct spell for firms with RoE values at the $25^{th.}$ percentile and $75^{th.}$ percentile, respectively, 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. Thus, only extremely poor profitability would have a material effect on the hazard rates. The marginal effect of Abnormal Stock Return is illustrated in Figure 2C by showing estimated hazards for firms at the $25^{th.}$ and $75^{th.}$ percentile values of Abnormal Stock Return, respectively. 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 lower 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 duration. Figure 2D illustrates the magnitude of the effect by showing estimated hazards for firms at the $25^{th.}$ and $75^{th.}$ percentile sample values, respectively, 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 lower 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 above show the importance of controlling for various core firm and market characteristics when estimating fraud termination hazard rates. We therefore include the full set of variables used in Model 2 of Table 3 in all subsequent estimations.

5.2 The Effect of Auditors on Fraud Duration

We next analyze the effect of auditors on fraud termination hazard rates. As discussed in section 3.2.2, 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 3, although we do not report those results due to space considerations.[11] Consistent with auditors being important information producers about corporate fraud, Model 1 of Table 4 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.

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

Model 2 of Table 4 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. Thus, there is no evidence that Big N auditors are better at generating 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 presumptions that the new auditor would have a harder time generating signals of misconduct. New auditors were responsible for around 36% of the audited annual statements in our sample. Model 3 of Table 4 shows the effect from adding the interaction between ($4^{th.}$ Quarter) dummy and an indicator for whether the 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 generated by auditing.

The fourth quarter effect may not necessarily be due to the the signals generated 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 generated by the auditors: whether the audit report contains explanatory language or not. Model 4 of Table 4 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, there is a huge positive effect on the fraud termination hazard rate if the auditing report contains explanatory language. 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 difference in magnitude is substantial. Figure 3 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 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 substantial 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 nevertheless seem to be important information intermediaries that facilitate fraud detection and intervention by others.

5.3 The Effect of Analysts on Fraud Duration

As discussed in section 3.2.3, 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 2.A, around 77% of firm-quarters 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 4. Model 1 of Table 5 shows that there is no significant effect of having at least one analyst follow the firm. We next also include the log of (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 is reported in Model 2 of Table 4. We see that there is a strong positive and significant (at the 5%-level) effect of the analyst presence dummy. That is, a firm with one analyst covering it have 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 the log of (1 + number of analysts). Thus, the marginal effect of having more analysts after the first is declining rather than increasing. This results suggest 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 this sense, we divide the analysts in our sample between industry specialists and non-specialists, following Gilson, Healy, Noe, and Palepu (2001). In particular, 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.[12] Results are presented in Model 3 of Table 5. The benefit of adding analyst coverage appears to solely come from the introduction of a specialist coverage. In particular, 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

[12] 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.

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 is free-riding among specialists. By contrast, non-specialist analyst following appear does not seem important for fraud termination at all.

Figure 4 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 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 clear 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 to some degree, 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 greater earnings forecast error may make analysts scrutinize the firm's financial and operations more carefully 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 5 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. Thus, 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 it 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.

5.4 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 3.2.4, 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 a firm starts its fraud in is 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. 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 6 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 5. 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 6 we instead add our second proxy for managerial effort, the log of number of (fraud) areas, 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 ares affected may indicate more effort in terms of the execution of the fraud. To allow for this possibility, we include both fraud proxies at the same time in our estimations. The results from this exercise are reported in Model 3 of Table 6. 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 5A 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 6, while holding all other variables constant at their median values. As it can be seen, there is a very substantial negative effect. For example, 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 originally started its misconduct in the first fiscal quarter relative to firms that did not start the misconduct in the first fiscal quarter. Figure 5B 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 3.2.4, we also consider a more time varying proxy for managerial fraud effort: total accruals, defined as the differences 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 6 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 5C show 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 will decrease the fraud termination hazard by 1.3%-points.

Overall, this section shows that frauds that are likely to be premeditated and involve a lot managerial effort carry on for substantially longer durations than other frauds, consistent with our model's implications.

5.5 The Effect of Earnings Related Fraud on Fraud Duration

As discussed in Section 3.2.5, 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 most about the accuracy of reported earnings, which would then make earnings- (i.e, income statement) related fraud harder to maintain than fraud affecting other financial accounts.

In Model 1 of Table 7 we add a dummy variable (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 6. These results show that frauds affecting earnings tend to be shorter. The coefficient on the Earnings Related dummy is positive and significant at the 1%-level. Figure 6 shows the magnitude of the estimated effect. For example, if a misconduct spell is in its $6^{th.}$ quarter and the misstatement is earnings-related, the hazard of ending the fraud next quarter is 13.5% versus 9.7% if the misstatement is not directly earnings-related.

6 Conclusion

In this paper, we investigate the impact of information producers – in particular auditors and financial analysts – as well as 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 conceal 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 increase the likelihood of detection, in particular if non-standard explanatory language has been added to the auditor report. Moreover, this effect is independent of whether the auditing firm is a 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.

In terms of 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 your 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 are also harder to be spotted. 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.

Finally, we show that frauds that affects areas that investors may care the most, for example the income statement, are more likely to be caught sooner than later. This result indicates that information producers scrutinize more carefully financial statement areas that investors care more about.

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 A: Proofs

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

Proof of Proposition 1: If $\xi P < \mathcal{C}$, we have that at $\mathcal{H}_\emptyset = \emptyset$ 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 $\Pr(M|\mathcal{H}_1) = 1$. On the other hand, if $s_1 = G$, then $\Pr(M|\mathcal{H}_1) = \frac{(1-p)\xi}{(1-\xi)+(1-p)\xi} < \xi$. More generally, we have that, $\forall \mathcal{H}_t \notin \mathcal{H}_t(B), \Pr(M|\mathcal{H}_t) = \frac{(1-p)^t\xi}{(1-\xi)+(1-p)^t\xi} < \xi$. Therefore, $\Pr(M|\mathcal{H}_t)P - \mathcal{C} < 0,\ \forall \mathcal{H}_t \notin \mathcal{H}_t(B)$. Since $\delta E_t[V(\mathcal{H}_{t+1}) \geq 0$, it is not optimal to intervene until a bad signal is observed. $\square$

Proof of Proposition 2:

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

$\square$

Proof of Proposition 3: Consider that the current number of information providers is $\mathbf{I}$. 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

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

While the expected length of a fraud has been reduced by

$$\begin{aligned}
&\frac{1}{1 - \prod_{i \in \mathcal{I}+1}(1-p_i)} - \frac{1}{1 - \prod_{i \in \mathcal{I}}(1-p_i)} = \\
&= \frac{\left[1 - \prod_{i \in \mathcal{I}}(1-p_i)\right] - \left[1 - \prod_{i \in \mathcal{I}+1}(1-p_i)\right]}{\left[1 - \prod_{i \in \mathcal{I}+1}(1-p_i)\right] \times \left[1 - \prod_{i \in \mathcal{I}}(1-p_i)\right]} \\
&= \frac{-p_{\mathbf{I+1}} \prod_{i \in \mathcal{I}}(1-p_i)}{\left[1 - \prod_{i \in \mathcal{I}+1}(1-p_i)\right] \times \left[1 - \prod_{i \in \mathcal{I}}(1-p_i)\right]}.
\end{aligned}$$

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

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:

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

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

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

Rearranging it, we have:

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

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

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

Rearranging it, we have:

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

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

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

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 $\Pi(\mathcal{B}, t+1) < 0$, 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 $(1-p(t))\mathcal{B}+p(t)(-L)$ 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 $\Pi(\mathcal{B}, t) \equiv \Pi(\mathcal{B})$

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

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 $\Pi(\mathcal{B}) = 0$. Similarly, if starting the fraud is optimal, we have that $\Pi(\mathcal{B}) = \frac{(1-p)\mathcal{B}+p(-L)}{1-\delta}$ which is positive if $1-p)\mathcal{B}+p(-L) > 0$. 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:

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

While, for item 2, applying IFT we have:

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

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

Appendix 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.[13]

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.

To begin, 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 *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:

$$S_j = \prod_{k=1}^{j} (1 - h_k). \tag{26}$$

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

Equation (26) naturally suggests a way to estimate the survivor function non-parametrically. 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:

$$\hat{S}_j = \prod_{k=1}^{j} \frac{R_k - M_k}{R_k}. \tag{27}$$

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:

$$\hat{H}_j = \sum_{k=1}^{j} \frac{M_k}{R_k}, \tag{28}$$

which is the sum of empirical hazard rates. Combining equation (26) with equation (28), 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, $\mathbf{x}_i$[14], 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 (26), it follows that $S(t_j, \mathbf{x}) = \exp(-\lambda H_j)$,

[14] 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 $y_{ij} = 1$.

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:

$$\log(-\log[1 - h_j(\mathbf{x})]) = \beta\mathbf{x} + \log(H_j - H_{j-1}). \tag{29}$$

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 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.[15]

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

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.[16]

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

Appendix C: 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)
Earnings-Related Areas	An indicator variable equal to 1 if the misstatement affected earnings related areas in the income statement and 0 otherwise
Total Accruals	(Net income - Operating Cash Flows) / Average Total Assets (COMPUSTAT Annual)

References

[1] BAER, Mirian. Linkage and the Deterrence of Corporate Fraud. **Virginia Law Review**, v. 94, pp. 1295-1365, Oct. 2008.

[2] BAKER, Malcolm, and STEIN, Jeremy. Market Liquidity as a Sentiment Indicator. **Journal of Financial Market**, v. 7, pp. 271-299, 2004.

[3] BAKER, Malcolm, and WURGLER, Jeffrey. Investor Sentiment and the Cross-Section of Stock Returns. **Journal of Finance**, v. LXI, pp. 1645-1680, Aug. 2006.

[4] BEASLEY, Mark S., CARCELLO, Joseph V., and HERMANSON, Dana R. Fraudulent Financial Reporting: 1998-2007 An Analysis of U.S. Public Companies. New York: COSO, 2010.

[5] BENEISH, Messod D. The detection of earnings manipulation. **Financial Analysts Journal**, v. 55, n.5, pp. 24-36, 1999.

[6] BENEISH, Messod D., LEE, M. C., and NICHOLS, D. Craig. Fraud detection and expected returns, mimeo, 2012.

[7] BRENNAN, Michael, and SUBRAHMANYAM, Avanidhar. Investment analysis and price formation in securities market. **Journal of Financial Economics**, v. 38, pp. 361-381, 1995.

[8] BOWEN, Robert M., CALL, Andrew C., and RAJGOPAL, Shiva. Whistle-Blowing: Target Firm Characteristics and Economic Consequences. **The Accounting Review** v. 85, pp. 1239-1271, 2010.

[9] CHEN, Tao, HARFORD, Jarrad, and LIN, Chen. Do Financial Analysts Play a Monitoring Role? Evidence from Natural Experiments, mimeo, 2012.

[10] CLEMENT, Michael and TSE, Senyo. Financial Analyst Characteristics and Herding Behavior in Forecasting. **Journal of Finance**, v. LX, pp. 307-341, 2005.

[11] COTTER, Julie, and YOUNG, Susan M. Do Analysts Anticipate Accounting Fraud?, mimeo, 2007.

[12] COX, David R.. Regression Models of Life Tables. **Journal of the Royal Statistical Society,** series B, v. 34, n. 2, pp.187-220, 1972.

[13] CZERNEY, Keith, SCHMIDT, Jaime, and THOMPSON, Anne. Does Auditor Explanatory Language in Unqualified Audit Reports Indicate Increased Financial Misstatement Risk? **The Accounting Review**, v. 89, n. 6, pp. 2115-2149, 2014.

[14] DECHOW, Patricia, GE, Weili, LARSON, Chad R., and SLOAN, Richard G. Predicting Material Accounting Misstatements. **Contemporary Accounting Research**, v. 28, n. 1, pp. 17-82, Spring 2011.

[15] DECHOW, Patricia, SLOAN, Richard G., and SWEENEY, Amy P. Detecting earnings management. **The Accounting Review**, v. 70, n.2, pp. 193-225, 1995.

[16] DEFOND, Mark L., and ZHANG, Jieying. A Review of Archival Auditing Research, mimeo, Aug. 2013.

[17] DYCK, Alexander, MORSE, Adair, and ZINGALES, Luigi. Who Blows the Whistle on Corporate Fraud? **Journal of Finance**, v. LXV, pp. 2213-2253, 2010.

[18] EECKHOUT, Jan, and PINHEIRO, Roberto. Diverse Organizations and the Competition for Talent. **International Economic Review**, v. 55, pp. 625-664, 2014.

[19] ELLUL, Andrew, and PANAYIDES, Marios. Do Financial Analysts Restrain Insiders' Informational Advantage? mimeo, 2009.

[20] FILES, Rebecca. SEC enforcement: Does forthright disclosure and cooperation really matter? **Journal of Accounting and Economics**, v. 53, pp. 353-374, 2012.

[21] GILSON, Stuart, HEALY, Paul, NOE, Christopher, and PALEPU, Krishna. Analyst Specialization and Conglomerate Stock Breakups. **Journal of Accounting Research**, v. 39, n. 3 , pp. 565-582, 2001.

[22] GRAHAM, John, CAMPBELL, Harvey, and RAJGOPAL, Shiva. The economics implications of corporate financial reporting. **Journal of Accounting and Economics**, 40, pp. 3-73, 2005.

[23] HARRIS,Jared, and BROMILEY, Philip. Incentives to cheat: The influence of executive compensation and firm performance on financial misrepresentation.**Organization Science** v.18, pp. 350-367, 2007.

[24] HONG, Lu, and PAGE, Scott. Problem Solving by Heterogeneous Agents. **Journal of Economic Theory**, v. 97, pp. 123-163, 2001.

[25] JEGADEESH, Narasimhan, and KIM, Woojin. Do Analysts Herd? An Analysis of Recommendations and Market Reactions. **Review of Financial Studies**, v. 23, n. 2, pp. 901-937, Feb. 2010.

[26] JONES, Jennifer J. Earnings management during import relief investigations. **Journal of Accounting Research**, v. 29, n. 2, pp. 193-228, 1991.

[27] KARPOFF, Jonathan; LEE, D. Scott, and MARTIN, Gerald S. The Consequences to Managers for Financial Misrepresentation. **Journal of Finance**, v. 88, pp. 193-215, May 2008.

[28] KARPOFF, Jonathan; LEE, D. Scott, and MARTIN, Gerald S. The Cost of Firms of Cooking the Books. **Journal of Financial and Quantitative Analysis**, v. 43, pp. 581-612, Sep. 2008.

[29] KELLY, Bryan, and LJUNGQVIST, Alexander. Testing Asymmetric-Information Asset Pricing Models. **Reveiw of Financial Studies**, v. 25, pp. 1366-1413, 2012.

[30] LANCASTER, Tony. Econometric Methods for the Duration of Unemployment, **Econometrica**, v. 47, n. 4, pp.939-656, Jul. 1979.

[31] LANCASTER, Tony. 1990. **The Econometric Analysis of Transition Data**. Cambridge: Cambridge University Press.

[32] LAWRENCE, A., MINUTTI-MEZA, M., and ZHANG, P. Can Big 4 versus non-Big 4 differences in audit-quality proxies be attributed to client characteristics? **The Accounting Review**, v. 86, n. 1, pp. 259-286, 2011.

[33] LEE, Thomas A., INGRAM, Robert W., and HOWARD, Thomas P.. The Difference between Earnings and Operating Cash Flow as an Indicator of Financial Reporting Fraud. **Contemporary Accounting Research** v.16, pp. 749-786, 1999.

[34] MAHER, M. W. The Impact of Regulation on Controls: Firms' Response to the Foreign Corrupt Practices Act. **The Accounting Review**, v. 56, pp. 751-770, 1981.

[35] MILLER, Gregory S. The Press as a Watch Dog for Accounting Fraud. **The Journal of Accounting Research**, v. 44, pp. 1001-1033, 2006.

[36] POVEL, Paul, SINGH, Rajdeep, and WINTON, Andrew. Boom, Busts, and Fraud. **Review of Financial Studies**, v. 20, n. 4, pp. 1219-1254, 2007.

[37] PAN, Yihui, WANG, Tracy Y., and WEISBACH, Michael S. Learning about CEO Ability and STock Return Volatility, mimeo, 2013.

[38] SHLEIFER, A., and VISHNY R. Large Shareholders and Corporate Control. **Journal of Political Economy**, v. 94, pp. 461-488, 1986.

[39] WANG, Tracy Y. Corporate Securities Fraud: Insights from a New Empirical Framework. **The Journal of Law, Economics, and Organization**, v. 29, n. 3, pp. 535-568, 2013.

[40] WELCH, Ivo. Herding among security analysts. **Journal of Financial Economics**, 58, pp. 369-396, 2000.

[41] WOOLDRIDGE, Jeffrey M. 2002. **Econometric Analysis of Cross Section and Panel Data**. Cambridge, MA: The MIT Press.

[42] YU, Fang. Analyst coverage and earnings management. **Journal of Financial Economics**, v. 88, n. 2, pp. 245-271, 2008.

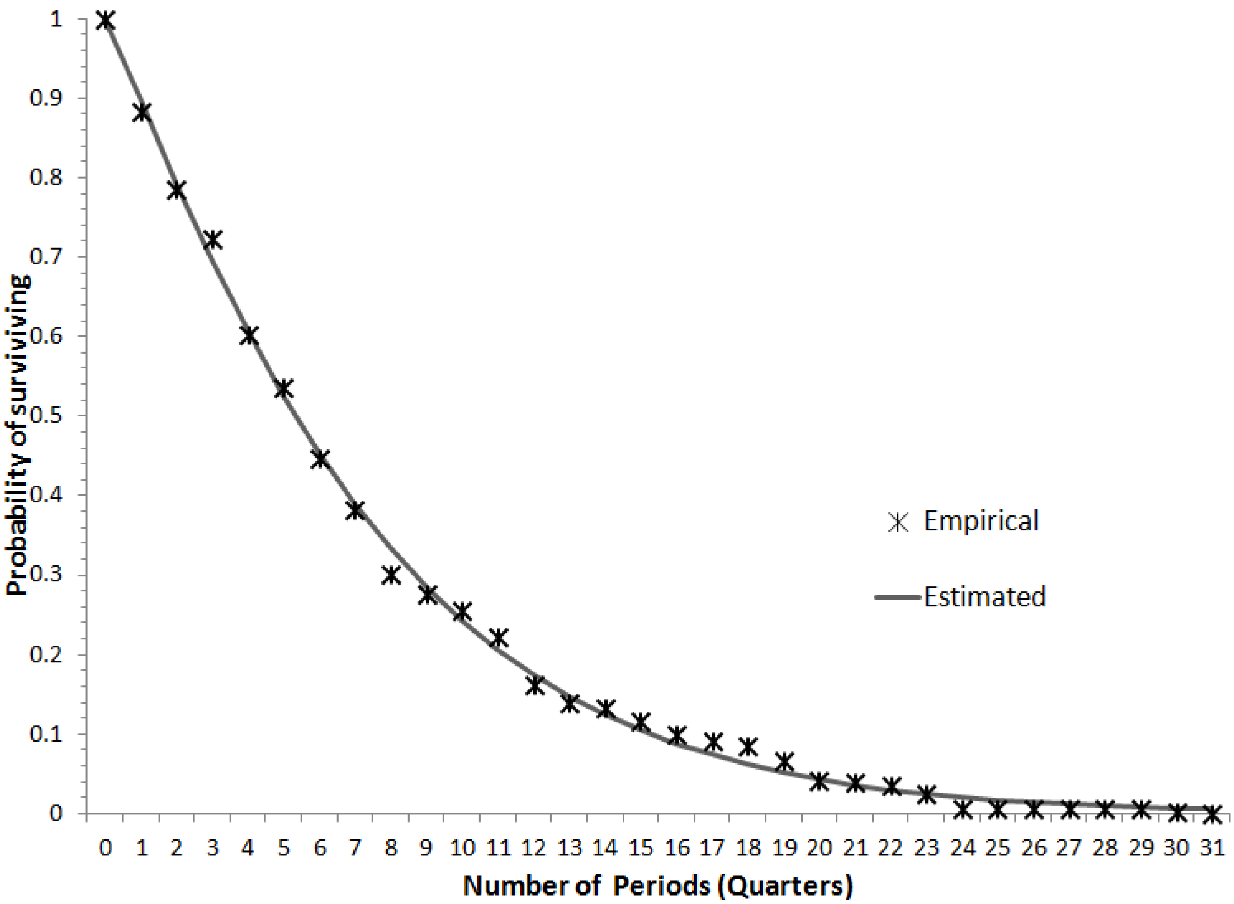


Figure 1. The figure plots the empirical survivor function for our sample of AAERs (N=300) versus the estimated Weibull survivor function from Model 1 in Table 3.

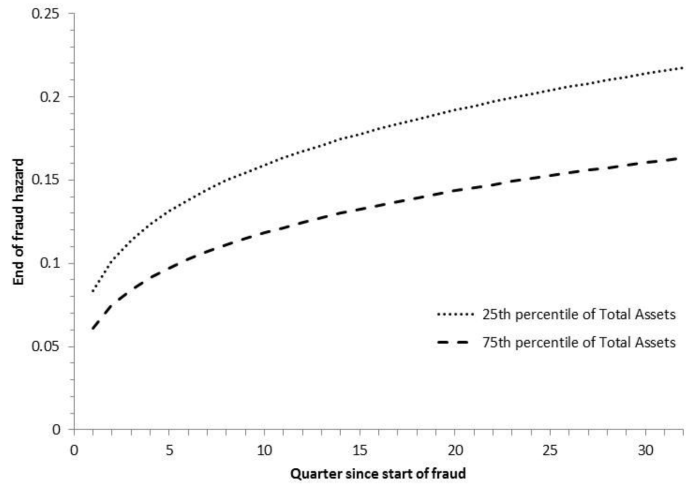


Figure 2A. Firm size and end of fraud hazards. The figure shows the estimated hazards of end of fraud as a function of quarters elapsed since the start of the misconduct. The hazards are estimated at the 25th percentile and 75th percentile sample values of book value of total assets, holding all other variables constant at their median sample values. The hazard estimates are based on Model 2 of Table 3.

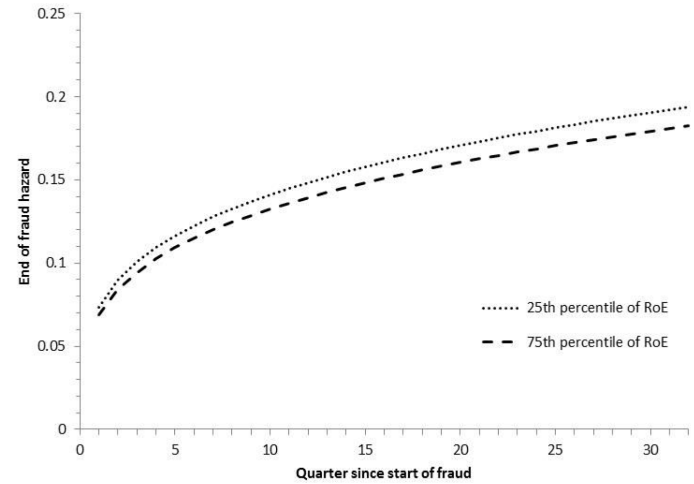


Figure 2B. Profitability and end of fraud hazards. The figure shows the estimated hazards of end of fraud as a function of quarters elapsed since the start of the fraud. The hazards are estimated at the 25th percentile and 75th percentile sample values for return on equity (RoE), holding all other variables constant at their median sample values . The hazard estimates are based on Model 2 of Table 3.

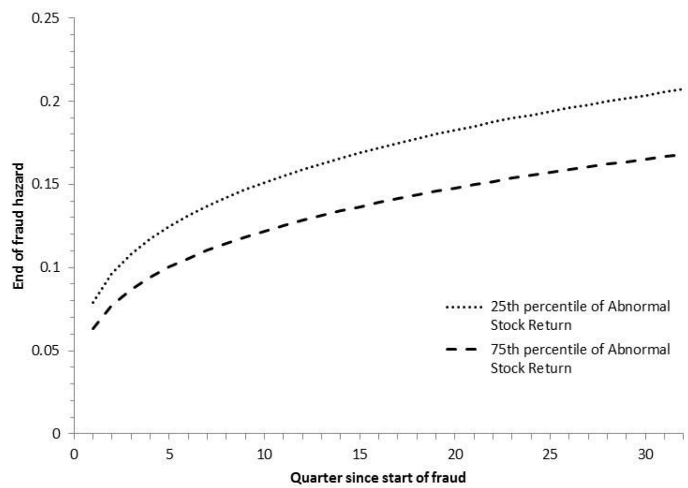


Figure 2C. Firm stock performance and end of fraud hazards. The figure shows the estimated hazards of end of frau as a function of quarters elapsed since the start of the fraud. The hazards are estimated at the 25th percentile and 75th percentile sample values for the firm's quarterly abnormal stock return, holding all other variables constant at their median sample values. The hazard estimates are based on Model 2 of Table 3.

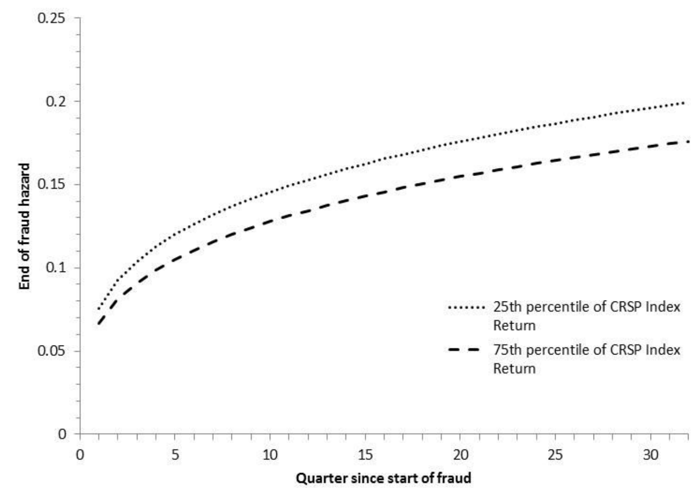


Figure 2D. Stock market stock performance and end of fraud hazards. The figure shows the estimated hazards of end of fraud as a function of quarters elapsed since the start of the fraud. The hazards are estimated at the 25th percentile and 75th percentile sample values for the firm's quarterly stock return (Stock Return), holding all other variables constant at their median sample values. The hazard estimates are based on Model 2 of Table 3

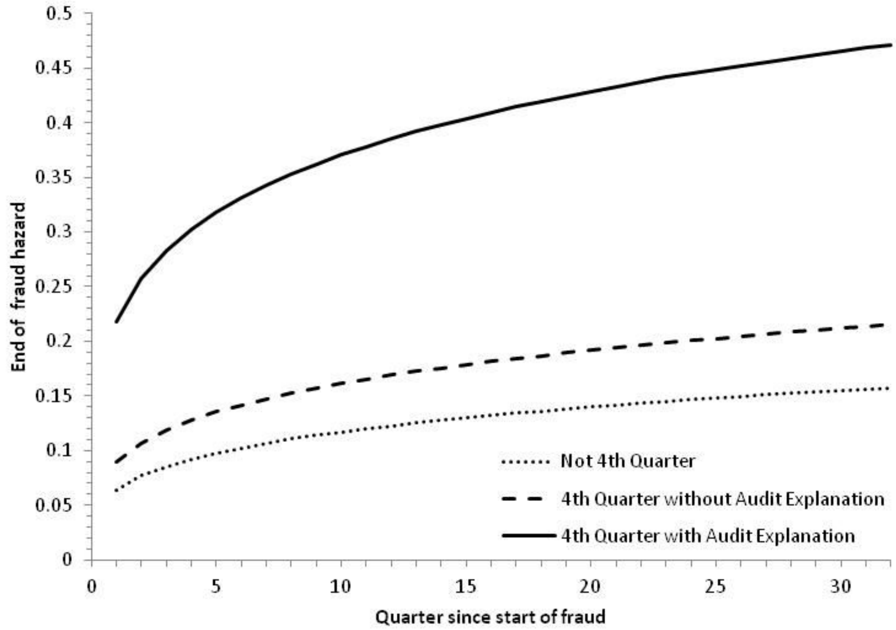


Figure 3. Fourth fiscal quarter and end of misconduct hazards. The figure shows the estimated hazards of end of fraud as a function of quarters elapsed since the start of the fraud. The hazards are estimated based the quarter not being the 4th fiscal quarter, being the 4th fiscal quarter without an audit explanation, and being the 4th fiscal quarter with an audit explanation. The hazard estimates are based on Model 4 of Table 4.

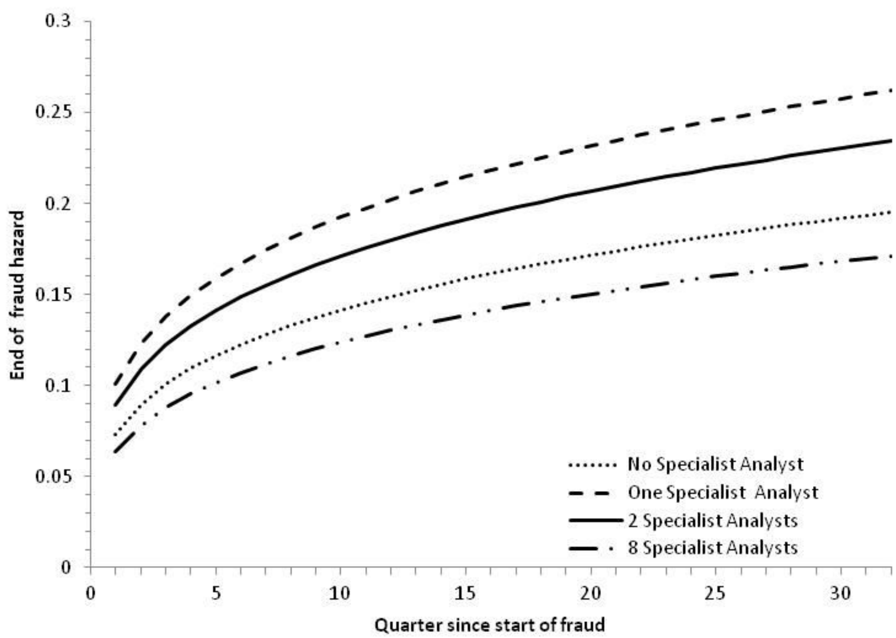


Figure 4. Specialist Analyst following and end of misconduct hazards. The figure shows the estimated hazards of end of fraud as a function of quarters elapsed since the start of the fraud. The hazards are estimated for firms with: no analyst following, with one specialist analyst following (the 41st 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 5.

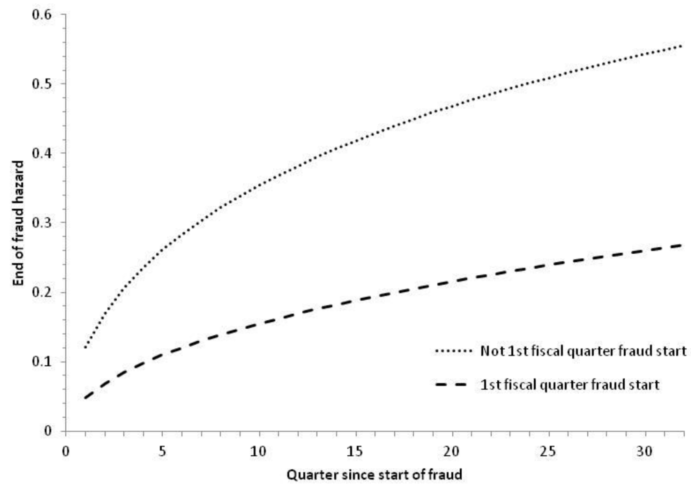


Figure 5A. First fiscal quarter fraud start and end of fraud hazards. The figure shows the estimated hazards of end of fraud as a function of quarters elapsed since start of the fraud. The hazards are estimated for firms that started their fraud in the first fiscal quarter and firms that started their 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 6

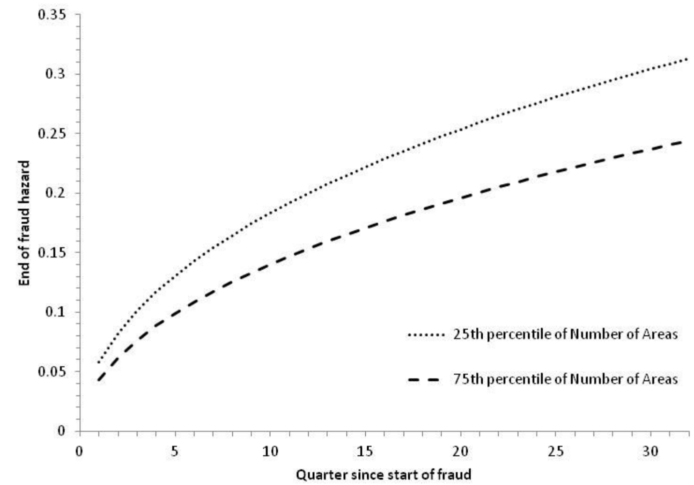


Figure 5B. Number of affected accounting areas and end of fraud hazards. The figure shows the estimated hazards of end of fraud as a function of quarters elapsed since the start of the fraud. The hazards are estimated at the 25th percentile and 75th percentile sample values of affected accounting areas (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 6.

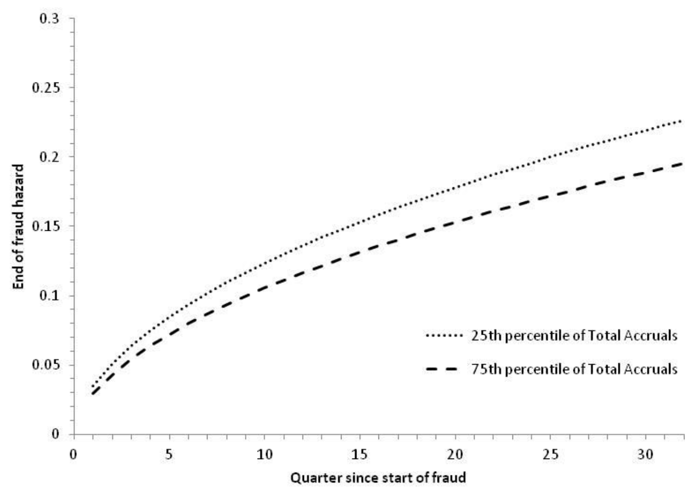


Figure 5C. Total accruals and end of fraud hazards. The figure shows the estimated hazards of end of fraud as a function of quarters elapsed since the start of the fraud. The hazards are estimated at the 25th percentile and 75th percentile sample values of total accruals, holding all other variables constant at their median sample values. The hazard estimates are based on Model 3 of Table 6.

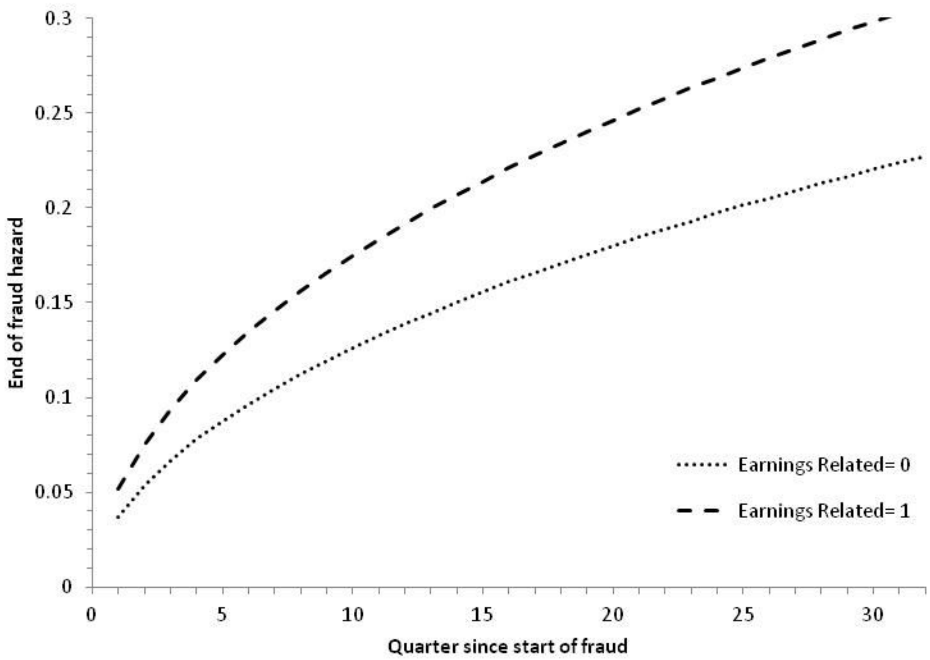


Figure 6. Earnings related fraud and end of misconduct hazards. The figure shows the estimated hazards of end of fraud as a function of whether the accounting misstatement is directly related to earnings or not. The hazards are estimated for firms that had an earnings related misstatement (Earnings Related=1) as well as for firms that did not (Earnings Related=0), holding all other variables constant at their median sample values. The hazard estimates are based on Model 1 of Table 7.

.

Table 1.A: 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**

Table 1.B: Misconduct per Area

Type of Misconduct	**Fraction**
revenue	64.3%
cogs	13.3%
Earnings-Related (revenue or cogs)	64.60%
receivables	24.3%
inventory	19.7%
reserves	10.0%
debt	3.7%
mkt securities	1.3%
asset	18.3%
payables	5.3%
liabilities	9.3%
other	35.7%

Table 1.C: 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 1.D: 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 2.A: Descriptives

Variable	Mean	Median	Std. Dev.	N
log(Total Assets)	6.042	5.981	2.511	2254
RoE	-0.001	0.021	0.147	2254
Market-to-Book	2.437	1.630	2.213	2254
Leverage	0.243	0.228	0.192	2254
Soft Assets	0.645	0.691	0.215	2254
Abnormal Stock Return	0.012	-0.027	0.335	2254
CRSP Value-Weighted Index	0.018	0.023	0.091	2254
$4^{th.}$ Quarter	0.233	0.000	0.423	2254
Analyst Dummy	0.771	1.000	0.420	2254
log(1+ Number of Analysts)	1.792	2.079	1.202	2254
Specialist Dummy	0.593	1.000	0.491	2254
log(1+ Number of Specialists)	1.178	1.099	1.177	2254
Non-Specialist Dummy	0.705	1.000	0.456	2254
log(1+ Number of Non-Specialists)	1.183	1.099	0.971	2254
Mean Forecast Error	0.007	0.000	0.025	2246
$1^{st.}$ Quarter Fraud Start	0.742	1.000	0.438	2254
Log(Number of Fraud Areas)	0.667	0.693	0.573	2254
Total Accruals	-0.056	-0.039	0.160	2046
Earnings Related	0.646	1.000	0.478	2254

Table 2.B: Cross-correlation table

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Quarters	..																			
log(TA)	0.361	..																		
RoE .01	0.117	0.113	..																	
Market-to-book	-0.149	-0.222	-0.026	..																
Leverage	-0.056	0.255	0.022	-0.279	..															
Soft Assets	0.004	0.057	0.071	-0.178	0.149	..														
Stock Return	0.027	-0.067	0.049	-0.014	-0.050	-0.014	..													
CRSP V.W. Index	-0.030	-0.092	0.005	-0.021	-0.002	0.025	0.115	..												
$4^{th.}$ Quarter	0.007	0.009	-0.005	-0.020	0.017	0.014	-0.008	0.192	..											
analyst dummy	0.189	0.561	0.091	-0.093	-0.074	-0.031	-0.017	-0.057	0.018	..										
log(1+no. of analysts)	0.290	0.801	0.105	-0.054	0.015	-0.070	-0.045	-0.081	0.014	0.812	..									
Specialist Dummy	0.203	0.559	0.069	-0.039	0.000	-0.037	-0.039	-0.062	0.006	0.657	0.735	..								
log(1 +no. of specialists)	0.275	0.660	0.069	0.008	0.013	-0.117	-0.041	-0.073	0.002	0.545	0.800	0.830	..							
Nonspecialist Dummy	0.153	0.536	0.081	-0.080	-0.037	0.005	-0.029	-0.046	0.022	0.843	0.758	0.486	0.437	..						
ln_number_nonspecialist,	0.179	0.632	0.085	-0.074	0.032	0.044	-0.037	-0.058	0.021	0.664	0.783	0.390	0.325	0.787	..					
abs(mean forecast error)	-0.075	-0.065	-0.135	-0.097	-0.057	0.009	0.042	-0.015	-0.023	0.053	-0.045	0.002	-0.045	0.005	-0.052	..				
Start 1^{st} Q	0.416	0.176	0.070	-0.058	-0.004	0.078	0.020	-0.007	-0.061	0.132	0.149	0.130	0.113	0.112	0.155	-0.076	..			
log(no. of areas)	0.176	0.170	0.069	-0.145	0.073	0.136	-0.022	-0.009	-0.000	-0.009	0.045	0.022	0.039	-0.024	0.037	-0.092	0.144	..		
Total Accruals	0.023	0.014	0.406	-0.001	0.105	0.203	0.033	0.060	0.007	0.098	0.009	0.018	-0.055	0.073	0.037	-0.100	0.092	0.133	..	
Earnings Related fraud	-0.217	-0.087	-0.030	0.157	-0.026	0.209	-0.013	-0.016	-0.006	-0.057	-0.072	-0.067	-0.103	-0.048	0.017	0.024	-0.071	0.243	0.021	..

Table 3: End of misconduct hazard: 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 Appendix C. 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 4: Hazard rate estimates : 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 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Appendix C. 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*** (0.123)	0.733*** (0.226)	0.726*** (0.129)	0.346** (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*** (0.080)	0.276*** (0.080)	0.277*** (0.080)	0.275*** (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 5: Hazard rate estimates : 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 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Appendix C. 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-Specialists 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 6: Hazard rate estimates : 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 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Appendix C. 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-Specialists 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 7: Hazard rate estimates : Frauds affecting 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 3 is included but not reported. Full estimation results are available from the authors upon request. The definitions of all variables are presented in Appendix C. Standard errors are reported in parentheses.

	(1)
	End of fraud
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-Specialists 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

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