Fraud discovery in the credit default swaps market[*]

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August 2016

[*] We appreciate valuable comments from seminar and conference participants at McGill University; the University of Macau; Fudan University; the 2014 Six-University (CityU–NanjingU–NUS–ReminU–SNU–XiamenU) Winter Research Camp at Suzhou, China; the 2015 World Accounting Frontiers Series in Macau, China; the 2015 CAAA Annual Conference in Toronto, Canada; the 2015 EAA Annual Conference in Glasgow, Scotland; and the 2015 AAA Annual Meeting in Chicago. We acknowledge partial financial support from the City University of Hong Kong, Fonds de Recherche du Québec – Société et Culture, and McGill University.

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ABSTRACT

This study investigates the behavior of credit default swap (CDS) spreads surrounding the discovery of financial reporting fraud. We find that CDS spreads increase in the months before the public discovery of fraud and then spike on the discovery date, implying some CDS investors are better able to detect fraud. We next show that the increase in CDS spreads prior to the public discovery of fraud is more pronounced for firms with larger bank loans and more lead banks in a loan syndicate. We also find that CDS spreads before the public discovery increase more significantly for fraud firms with higher credit risk, less effective governance, and greater information asymmetry between corporate insiders and outside investors. Overall, our results suggest that CDS investors who have higher incentives to monitor the credit risk of the reference entity tend to possess superior information about suspected fraudulent activities, and thus, are better able to detect financial reporting fraud, prior to the public disclosure of fraud.

***JEL classification*:** M41; G12; G13; G34

***Keywords*:** Financial fraud; Fraud discovery; Credit default swap; Credit spread; Credit risk; Private information gathering

1. Introduction

This study examines the behavior of credit default swap (CDS) spreads surrounding the discovery of financial reporting fraud. A CDS is an over-the-counter financial derivative contract that is designed to protect investors from credit risk. A typical CDS contract requires the protection seller to compensate the protection buyer when credit event of a specific company occurs. Credit events in a CDS contract typically include failure to pay or default, restructuring, and bankruptcy. In return, the protection seller charges a fixed premium, known as the spread, to the protection buyer. This spread or premium is quoted in basis points of the contract's notional principal. While a CDS contract is written on a specific company, known as the reference entity, the company is not a party to the contract. Generally, an investor may buy CDSs to hedge the credit risks it bears for a position in the reference entity's bond, loan, or other debt instruments. Investors have been increasingly purchasing CDSs without owning any debt of the reference entity, with the sole purpose of speculating on the specific company's creditworthiness (Kopecki and Harrington 2009). Since they are traded in an over-the-counter market and not in organized exchanges, CDS transactions are subject to minimal regulation. For instance, CDS contracts are largely exempt from the regulations of the U.S. Securities and Exchange Commission (SEC) and the Commodities and Futures Trading Commission (CFTC) with regard to information dissemination (e.g., SEC Rule 10b-5). As a result, the CDS market is not as informationally transparent as the organized stock or bond markets and is commonly criticized for the prevalence of informed trading (*The Economist* 2003; *The Financial Times* 2005).

The CDS market has grown substantially since its introduction in the early 1990s. By 2012, the CDS market was estimated to be worth about US$25.5 trillion.[1] Participants of the CDS market are largely financial institutions such as banks, securities firms, hedge funds, and insurance companies; banks generally account for a large portion of buyers while insurance companies account for a large proportion of sellers (Longstaff et al. 2005). Prior research shows ample evidence that these institutional investors are more diligent and more sophisticated and have superior ability to analyze financial information (e.g., Boehmer and Kelly 2009). Prior literature also argues that, because many CDS market participants are secured creditors or financiers of the reference entities, these parties may have access to critical private information about the specific company not known to the public (e.g., Acharya and Johnson 2007; Simkovic and Kaminetzky

[1] Source: International Swaps and Derivatives Association.

2011; Qiu and Yu 2012). One can therefore expect that CDS market participants are likely to react to a firm's fraud-committing activities prior to their public discovery.

Extant literature has looked into the issue of fraud detection in the pre-discovery or fraudcommitting period by examining the firm-level determinants of fraud (e.g., Dechow et al. 1996;

Beneish 1997, 1999; Dechow et al. 2011) and the behaviors of insiders (Summers and Sweeney 1998; Agrawal and Cooper 2007), boards of directors (Fahlenbrach et al. 2013; Bar-Hava et al. 2013; Gao et al. 2015), and employees (Dyck et al. 2010), prior to the public disclosure of fraud.[2] A few studies provide evidence on whether outside stakeholders, particularly equity market participants, can identify fraud firms and foresee financial reporting irregularities. This line of research (e.g., Efendi et al. 2006; Desai et al. 2006; Karpoff and Lou 2010) focuses mostly on short sellers and finds that short sellers increase their positions prior to earnings restatements, suggesting that they are aware of the forthcoming restatements. However, Bardos et al. (2011) show that investors are usually misled by a firm's erroneous earnings, and Griffin (2003) finds that most equity analysts are unable to anticipate the prospective bad news in advance of a corrective disclosure event.

Financial reporting fraud signifies serious *downside* risk that credit investors are mainly concerned about.[3] Surprisingly, however, prior research on fraud has paid little attention to the

[2] Though less related to our current study, there also exists an extensive literature focusing on the consequences and repercussions of the revelation of financial reporting fraud (e.g., Agrawal et al. 1999; Agrawal and Chadha 2005; Farber 2005; Fich and Shivdasani 2007).

[3] For instance, Karpoff et al. (2008) show that firms subject to financial reporting fraud litigation suffer enormous valuation loss. The negative effect amounts to an average one-day abnormal return of -25.24% on trigger event dates, -7% on class action lawsuits, and -14.4% following a company announcement of investigation events. Cumulatively, the loss related to financial fraud has an average return of -41%. For debt investors, Graham et al. (2008) show that non-fraudulently related restatements lead to an average 42.6% increase in loan spread, while a fraudulent restatement has an average effect of 68.9%.

extent to which debt market participants anticipate or detect financial reporting fraud prior to its public discovery. As a result, little is known about whether and how debt market participants respond to a firm's fraudulent activities. In this study, we therefore aim to provide large-sample, systematic evidence on how debt market participants discover financial reporting fraud and incorporate it into debt pricing before and after its public discovery. Credit investors in both the CDS and bond markets have definite concerns about credit risk of a firm in which they invest, and would have incentives to discover any fraudulent reporting activities before they are revealed to the public. However, our analysis focuses on the CDS market, not on the bond market, for the following reason. First, CDS spreads are known to be a better proxy for a firm's credit risk than bond spreads (Lok and Richardson 2011; Griffin 2014).[4] Second, CDS investors consist mostly of large banks[5] and represent some of the most sophisticated investors in the capital market. It is therefore likely that due diligence is prevalent in the CDS market and CDS investors should possess relevant knowledge and experience to identify any irregular financial reporting activities committed by the reference entities. The non-transparency in the CDS market associated with the lack of public disclosure requirements may also motivate CDS investors to engage in private information gathering on firms' suspicious fraudulent activities before they are revealed to the public. Specifically, our study has two objectives. First, we investigate the behavior of credit investors in the CDS market in the pre-discovery periods leading up to the public discovery of fraud. Our objective here is to examine whether credit investors have access to information about a reference entity's fraud-committing activities *prior to* the public discovery of fraud. To this end,

[4] Lok and Richardson (2011) and Griffin (2014) argue that CDS spreads are a clean measure of credit risk because, unlike bond spreads, CDS spreads do not reflect any price-relevant features such as covenants and guarantees and are more invariant to short-term changes in cash flows or earnings than both bond and equity measures are. In addition, liquidity in the secondary loan market is historically low (Alexander et al. 1998), hence changes in credit risk is less timely reflected in bond spreads.
[5] Source: Bank of International Settlements (BIS).

our analysis focuses on the intertemporal changes in CDS spreads during this pre-discovery period. Second, we also examine the reaction of credit investors *upon* the public discovery of fraud. If credit investors do not have private information and learn about fraud mainly through public channels, we expect to observe a significant market reaction in the CDS market on the event date of fraud discovery.

Using the Audit Analytics Corporate + Legal database, we construct a sample of fraud firms that became the subject of shareholder class action lawsuits during 1997–2013. We identify specific trigger event dates with regard to the public disclosure of financial reporting fraud through SEC's litigation releases.[6] We then look for any abnormal changes in CDS spreads in the period from six months before to six months after these trigger event dates. We find that the CDS spreads of fraud firms begin to increase six months before the public discovery of fraud and then spike upon public discovery. Our multivariate analysis compares the CDS spreads of fraud firms with those of matched control firms and show that CDS spread changes are significantly higher for fraud firms in the six-month pre-discovery period and also upon public discovery on the event dates. The results are interesting because they imply that *some* credit investors have superior private information about suspected fraudulent reporting activities months in advance of the public disclosure of fraud and that their responses are reflected in the CDS pricing during the prediscovery period. However, our results also imply that *not all* CDS investors possess such private

[6] Karpoff et al. (2008) provide a detailed overview of the SEC's enforcement process. Their study shows that indications of fraud surface on the trigger event dates, usually a firm's public disclosure of a serious event (e.g., restatement, auditor firing) that implies financial reporting irregularities.

information, since many CDS investors react concomitantly with the rest of the capital market upon the public disclosure of fraud.[7]

The interpretation of our findings thus far indicates that *some* credit investors are better than others in detecting financial fraud: Some CDS investors are more sophisticated, experienced investors than others and are more likely to engage in private information gathering prior to the public discovery of fraud. Hence, these sophisticated investors are more alert to any financial reporting red flags than other investors. As such these sophisticated investors are more likely to incorporate this private information about fraud in a timelier manner in the pre-discovery period, compared to other investors. Stated another way, their concern about increased credit risk associated with financial reporting fraud is reflected in the pricing of CDSs before the public discovery of fraud.

We further examine what factors facilitate CDS investors looking into a firm's financial reporting irregularities before fraud is publicly revealed. We conjecture that the incentives of CDS investors to monitor credit risk of the reference entities do matter because CDS investors with stronger monitoring incentives are likely to engage more intensely in gathering private information about the reference entities, enabling them to detect any financial reporting irregularities in a timelier manner. We expect that banks, who are the dominant and among the most sophisticated players in the CDS market, have higher monitoring incentives, particularly when they have lending relationships with the reference entities. Banks with lending relationships should also have more privileged access to private information about any financial misconduct within the reference

[7] An alternative interpretation of the findings is that while CDS investors may have some private information, they do not react completely when they suspect but do not have confirmed evidence of company wrongdoings before the public discovery of fraud and only respond fully to concrete information upon the public disclosure of fraud. Nonetheless, this interpretation implies that CDS investors possess an information advantage over other market participants.

entities (e.g., Boot 2000). Hence, we predict that the increase in CDS spreads in the pre-discovery period should be more pronounced for firms with larger bank loans and more lead banks in a loan syndicate, because such firms are subject to more intense bank monitoring.

On the contrary, the availability of CDS contracts that are traded in the CDS market could have reduced these lending banks' incentives to monitor the reference entities. This is because banks with extensive lending activities would use the CDS contracts as a means to transfer their credit risks to other credit investors by purchasing CDS contracts. For instance, Ashcraft and Santos (2007) highlight the uniqueness of the CDS setting in that it reduces the incentives of lead banks to serve as a monitor. Our empirical results show a significant increase in CDS spreads for fraud firms with extensive lending activities in the pre-discovery period and for fraud firms with more lead banks in a loan syndicate. The findings suggest that CDS investors with higher monitoring incentives are better able to discover financial reporting fraud prior to its public discovery.

We further analyze whether firms that require more monitoring would affect the ability of CDS investors to discover fraud. We conjecture that credit investors should be more concerned with firms that are closer to default and have lower transparency and greater information asymmetry between corporate insiders and outside investors. Credit investors would exert more monitoring efforts on these entities, allowing them to better able to detect any financial reporting irregularities. We first argue that firms with higher financial constraints and higher default risk tend to have greater credit risk and are thus more likely to experience a credit event in the future. Hence, CDS investors are likely to exercise more monitoring effort in these firms. The need for heightened monitoring encourages CDS investors to gather more private information, and incentivizes them to obtain information about these firms' gloomy prospects. We find confirming evidences that CDS spreads increase significantly prior to the public discovery of fraud and spike upon discovery *only* for firms with ex ante low credit ratings and high default or bankruptcy likelihood (reflected in a low Z-score).

Second, we also find that CDS spreads increase significantly prior to the public disclosure of fraud *only* for firms with poor governance proxied by a high anti-takeover index and low institutional shareholding. These findings suggest that CDS investors are more concerned about firms with weak governance mechanisms that cannot rectify financial reporting irregularities and, therefore, CDS investors tend to monitor these firms closely and engage more in private information gathering about these poorly governed firms.

Third, we predict and test that credit investors have greater monitoring incentives for gathering private information on firms with higher information asymmetry. Using the number of business segments and accrual quality as proxies of information asymmetry, we find that CDS spreads increase, to a greater degree, prior to the public discovery of fraud for firms with higher information asymmetry. This finding is also consistent with the view that credit investors consider a firm's information risk in their pricing (e.g., Wittenberg-Moerman 2008).

We conduct a variety of robustness checks in an effort to strengthen our main findings. We find that our main results are robust to the use of (i) alternative definitions of fraud, (ii) an alternative sample constructed using propensity score matching (PSM), (iii) an alternative definition of the pre-discovery period, and (iv) CDS contracts with a one-year (instead of a fiveyear) maturity. Overall, our study shows that at least some debt market participants, in this case CDS investors, are aware of financial reporting irregularities of their reference entities. However, their ability to detect financial reporting fraud varies and it depends critically on their private information gathering activities with respect to the reference entities. We argue the CDS investors' lending relationships with the reference entities could enhance the efficacy of external monitoring by CDS investors, enabling them to obtain privileged information on the reference entities' fraudulent activities. We also show that the discovery of financial reporting fraud in the CDS market is more apparent when the reference entities require more monitoring or face more severe information uncertainty, as our evidences show that CDS spreads before the public discovery increase more significantly for fraud firms with higher default risk, less effective governance, and greater information asymmetry between corporate insiders and outside investors.

Our paper contributes to the literature in several important ways. To the best of our knowledge, our paper is the first to study the behavior of debt market participants before and upon public discovery of fraud. Our findings complement prior research that examines the behavior of short sellers surrounding the fraud discovery (e.g., Karpoff and Lou 2010), and indicate that credit investors in the CDS market are also able to detect financial reporting irregularities of reference entities before public discovery and, accordingly, they adjust CDS spreads to properly reflect the increase in credit risk.

More importantly, we document that CDS investors are not a homogeneous group (i.e., they differ in their ability to detect financial reporting fraud) and that their monitoring incentives matter. The CDS market offers a unique and interesting setting for several reasons: First, it is an over-the-counter market where information disclosure is less regulated. Overall, information in this market is not very transparent; for example, CDS investors do not observe the price signals of other similar CDS contracts.[8] Credit investors in this market thus have greater incentives to gather information relevant for the pricing of CDS contracts, because the benefits from private

[8] We interviewed an investment banker from BNP Paribas to verify our claim. In her view, very similar CDS contracts on the same reference entities do not have the same CDS prices because CDS investors actively seek an information advantage over others and this private information is reflected in differences in CDS pricing.

information gathering is greater to them in the CDS market than in other debt markets such as public bond market.

Second, the CDS market creates a conflicting incentive for lending banks to exert monitoring effort on their reference entities. While it is more critical for banks with extensive lending relationships to monitor the reference entities closely, the CDS market offers these banks the option to hedge their credit exposure by taking a long position on the CDS contracts. We show that the CDS market does not take away the monitoring function of credit investors, as CDS investors continue to have differing abilities to detect financial reporting fraud, depending critically on their monitoring incentives and their motivation on private information gathering. Our study provides novel evidence suggesting that CDS investors play an important role in monitoring the credit quality of reference entities, particularly in relation to fraud discovery. Some CDS investors who have *"more to lose"* exercise a heightened degree of oversight on their reference entities, and the monitoring incentives are also higher for entities with more serious credit risk exposure. The current findings could also provide some insights into reconciling the results of prior studies regarding why some equity market participants (e.g., short sellers) could detect financial reporting fraud while the others (e.g., financial analysts) do not: a plausible reason is that short sellers would also have "more to lose", and thus be more motivated to engage in private information gathering.

Our research also offers practical implications for the capital market. While most prior research focuses on the negative price consequence of fraud in the equity market, we examine the impact of fraud in the credit market. Prior research in the equity market provides some evidence showing that short sellers trade abnormally before the discovery of financial fraud, aiding in the price discovery of the adverse event (e.g., Efendi et al. 2006). Thus far, however, little is known about how the incidence of fraud can affect the pricing of credit instruments. While the equity and debt markets are arguably intertwined (e.g., Berndt and Ostrovnaya 2014), we believe that our finding that CDS market participants anticipate financial reporting fraud and incorporate it into CDS spread prior to its public discovery provides *credible* and *timely* signals to other outside stakeholders about firms' future (particularly negative) prospects. As mentioned earlier, the CDS market is dominated by a group of large banks. These banks are probably some of the most sophisticated and reputed investors in the capital market and, hence, CDS pricing could offer more credible signals on fraud to outside stakeholders in the capital market, compared to shorting transactions by short sellers. Prior research has predominantly shown that price discovery takes place sooner in the CDS market than in other markets, suggesting that CDS spreads reflect information in a timelier manner.[9]

The remainder of the paper is organized as follows: Section 2 reviews the related literature. Section 3 develops our hypotheses. Section 4 describes the sample selection process and explains the descriptive statistics. Section 5 presents our main empirical findings. Section 6 discusses additional and robustness analyses. The final section summarizes and provides concluding remarks.

2. Literature review

This study is related to both the literature on fraud and on CDS. Regarding fraud, it fits in the literature focusing on the ex ante detection of financial reporting fraud (e.g., searching for red flags

[9] Daniels and Jensen (2005) show the CDS market leads the bond market, indicating that more price discovery occurs for CDS investors than for bond investors. Blanco et al. (2005) shows bond market correction occurs first through changes in CDS spreads. Berndt and Ostrovnaya (2014) show the flow of information travels mostly from the CDS market to the stock and option markets and this flow is especially stronger for bad news events such as accounting scandals or negative earnings surprises. Hull et al. (2004) and Norden and Weber (2004) both show that CDS spreads significantly increase in the event of credit rating changes. However, some studies challenge the above findings and argue the stock market reflects more informed trades than the CDS market does (e.g., Griffin et al. 2013; Hilscher et al. 2014).

or signals that indicate financial misstatements). Early works in this area typically focus on the firm-level determinants of fraud. For example, Dechow et al. (1996) study a sample of firms subject to SEC accounting enforcement actions and indicate that these firms have a greater need to attract external financing. Moreover, these firms are less likely to have an audit committee and an external blockholder, more likely to have a company founder as chief executive officer, a chief executive officer who serves as chair of the board, and a corporate board dominated by insiders. Beasley (1996) shows that the presence of outside members on a firm's board of directors significantly reduces the likelihood of fraud. Beneish (1997) shows that fraud firms subject to SEC enforcement actions are distinctively different from the control sample of firms with merely high discretionary accruals, which the author terms aggressive accruers. The author shows that fraud firms differ in their accruals, day's sales in receivables, and prior performance. Beneish (1999) shows that days' sales in receivables, gross margins, sales growth, asset quality, and accruals are important determinants of fraud firms. Abbott et al. (2000) show that audit committee independence is inversely related to the incidence of fraud. Dunn (2004) finds that fraud is more likely to occur when the firm is controlled by insiders. Dechow et al. (2011) examine the characteristics of misstating firms and find that fraud firms in their misstating years have unusually high accruals, a declining return on assets (ROA), more operating leases, and relatively less property, plant, and equipment. These misstating firms also face greater market pressures (i.e., new financing, higher market-to-book ratios, and stronger prior stock price performance).

Our study is also related to the scant literature that focuses on stakeholder behavior before the public discovery of fraud. Summers and Sweeney (1998) show that company insiders significantly reduce their net position through high levels of stock sale activities before the revelation of fraud. Agrawal and Cooper (2007), on the contrary, show that managers are less likely to trade before accounting scandals; the authors argue that the sales by managers may increase investor scrutiny and the likelihood of the manipulation being revealed. Dyck et al. (2010) find that employees, non-financial market regulators, and the media are important players in fraud discovery and these players have a much higher probability of detecting fraud when they have access to private information. Recently, Fahlenbrach et al. (2013) show that outside directors have incentives to resign right before a firm discloses bad news. Bar-Hava et al. (2013) investigate reasons for outside directors' resignations and find that, while their resignations are associated with poor subsequent firm performance and future litigation, the information about their reasons for resignation has no incremental information content. Gao et al. (2015) show that outside director turnover is abnormally high during the alleged fraud committing period, indicating that the board of directors may have had knowledge of financial reporting irregularities and chose to disassociate themselves from the firm.

Limited research has examined the behavior of outside stakeholders prior to the public disclosure of fraud. Griffin (2003) finds that the largest analyst revisions on firms subject to SEC allegations of fraud occur in the month of corrective disclosure, suggesting that financial analysts tend to react to a corrective disclosure rather than anticipate it prior to public disclosure. Desai et al. (2006) and Efendi et al. (2006) show that short sellers increase their positions before a restatement and decrease them thereafter. Karpoff and Lou (2010) show that short sellers increase their positions before financial misconduct is publicly revealed, particularly when the misconduct is severe. Bardos et al. (2011) show that abnormal share returns are negative up to one month prior to a restatement announcement, but investors are still misled upon the initial announcement of erroneous earnings.

The finance literature has proposed three models to explain the spread in credit derivatives: (i) a structural model (Merton 1974; Longstaff and Schwartz 1995; Duffie 1999), (ii) a reducedform model (Das 1995; Das and Sundaram 2000; Hull and White 2000, 2001), and (iii) a hybrid model (Duffie and Lando 2001). Subsequent research on CDSs mostly adopts a long-window regression approach in which CDS spreads are regressed on their cross-sectional determinants (e.g., Collin-Dufresne et al. 2001; Benkert 2004; Longstaff et al. 2005; Callen et al. 2009; Das et al. 2009; Ericsson et al. 2009; Batta 2011; Kim et al. 2013). Recent studies examine the change in CDS spreads over a specific event window, such as Shivakumar et al. (2011) on the announcement of management earnings forecasts, Zhang and Zhang (2013) on earnings surprises, Bhat et al. (2013, 2014) on the adoption of International Financial Reporting Standards, and Griffin et al.

(2014) on the multi-phased XBRL adoption.

3. Hypothesis development

Duffie and Lando (2001) postulate that imperfect information in the credit derivative market can lead to different predictions of credit spreads by different investors. As a result, credit pricing could be a function of the quality of information that individual investors possess or gain access to. This reasoning forms the basis of our empirical prediction. In the over-the-counter CDS market, information transfer is imperfect and individual CDS investors can only determine CDS spreads based on available public information unless they invest time and effort to acquire private information and/or improve the quality of the information they possess.

Financial reporting fraud is a serious credit event that entails a significant, negative impact on a firm’s credit risk. In the event that CDS reference entities are involved in financial reporting fraud, investors in the CDS market face large downside risks on their CDS investments. CDS investors are thus likely to devote more time and effort to gathering private information about the reference entities and monitoring their credit risk, as they perceive the likelihood of fraud to be higher. This means that investors in the CDS market are likely to possess an information advantage about a firm's engagement in financial reporting fraud even before its public disclosure, compared with other investors. To the extent that CDS investors have private information about a firm's engagement in fraudulent activities in advance, one can expect that CDS spread increases prior to the public discovery of fraud, as a reflection of the increase in credit risk perceived by these investors. Alternatively, if credit investors do not possess an information advantage, they will react concomitantly with the rest of the capital market upon the public disclosure of fraud. In such a case, one would observe no significant reaction prior to public disclosure, while there would only be a significant reaction at the time of public discovery.

In reality, however, it is reasonable to assume that some credit investors are better informed of financial fraud while others are less informed or uninformed (i.e., informed only through public disclosure) due to imperfect information and differing levels of private information gathering activities among different investors in the CDS market. The above discussions lead us to predict a substantial increase in CDS spreads prior to the public discovery of fraud (due to well-informed investors), as well as upon its public discovery (due to less-informed or uninformed investors). To provide large-sample, systematic evidence on the prediction, we test the following two hypotheses in alternative form:

> **$\mathbf{H_{1A}}$:** *Prior to the public discovery of financial reporting fraud, CDS spread changes are larger for fraud firms than for non-fraud firms.*
>
> **$\mathbf{H_{1B}}$:** *Upon the discovery of financial reporting fraud, CDS spread changes are larger for fraud firms than for non-fraud firms.*

While the above two hypotheses address intertemporal changes in CDS spread surrounding the public discovery of fraud, our next hypothesis is concerned with cross-sectional variations in the fraud-CDS spread relation. We argue that CDS investors, who have privileged access to private information about the reference entities or spend more time and effort to gather private information about their credit risk, are better able to detect financial reporting fraud prior to its public discovery. More specifically, we conjecture that banks are more likely to detect financial reporting fraud, because banks have a privileged access to borrowers' inside information via their lending activities and ex post monitoring and thus have a significant information advantage over other investors in the CDS market.

We hypothesize that banks with extensive lending relationships with the reference entities would have *"more to lose"* in the credit event such as loan default. Hence, they have incentives to exert more monitoring effort and, as a result, possess superior information. To the extent that effective bank monitoring, along with the privileged access to borrowers' inside information, facilitates the ability of banks to discover financial reporting irregularities, we predict that banks utilize such information for CDS pricing. As a result, CDS spread will be higher for such reference entities that are subject to a heightened level of bank monitoring. On the contrary, Ashcraft and Santos (2007) show CDS trading could reduce banks' incentives to monitor borrowers' credit quality, and thus, the effectiveness of bank monitoring. This is because banks can purchase CDS contracts as a means to hedge a bank's credit exposure rather to engage in costly monitoring. In such a case, there would no significant difference in changes of CDS spreads for firms with or without extensive bank lending activities. Given the conflicting predictions above and the scarcity of empirical evidence on the issue, we propose and test our second hypothesis as follows.

> **H_2:** *Prior to the public discovery of financial reporting fraud, CDS spread changes are significantly greater for fraud firms with larger bank loans and more intensive bank monitoring.*

While the second hypothesis focuses on the monitoring role of banks, an important investor in the CDS market, our third set of hypotheses focus on whether and how the level of credit risk facing the reference entity influences the relation between CDS spread changes and the incidence of fraud prior to its public discovery. We conjecture that, as the default or governance risk of the reference entities (associated with fraud) increases and the information asymmetry between corporate insiders and outside investors grows, CDS investors would become more concerned about these reference entities, and thus exert more time and effort to monitor them closely. Enhanced monitoring by CDS investors would enable them to obtain private information about credit risk. In this process, investors in the CDS market are likely to gain access to private information about suspicious fraudulent financial reporting activities in advance. Hence, we expect CDS spreads to increase more prior to the public discovery of fraud for firms with higher inherent default risk or information risk.

We explore reference entity-specific factors that influence firms' inherent default risk and the level of information asymmetry between corporate insiders and outside investors in the CDS market. Specifically, firms with higher ex ante default risk, as measured by levels of financial constraint and its closeness to default, are more likely to experience credit events specified in CDS contracts, such as loan default, restructuring and bankruptcy. Stated differently, should financial reporting irregularities occur, these firms could have a higher chance of failing to meet their ongoing financial obligations and, in the worst cases, heading into restructuring and bankruptcy. These firms would be more likely to receive attention from CDS investors and/or receive financial reporting red flags, because CDS investors monitor these high-risk companies more closely. We therefore expect CDS investors to increase the CDS spreads for these firms prior to the public discovery of fraud. To provide systematic evidence on this untested issue, we hypothesize the following in alternative form.

H_{3A}: *Prior to the public discovery of financial reporting fraud, CDS spread changes are larger for fraud firms with higher ex ante default risk as proxied by higher levels of financial constraint and its closeness to default.*

Next, we also predict that the CDS market reacts more intensely to the fraudulent financial reporting activities of a firm with a weaker governance structure. When the corporate governance structure of a fraud firm is weaker and, thus, stakeholder protection is also weaker, CDS investors are likely to engage more intensely in private information gathering to better monitor their credit exposure. We therefore expect CDS investors to adjust CDS spreads upward more for firms with a weak governance structure prior to the public discovery of fraud. Given the scarcity of empirical evidence on the issue, we test the following hypothesis in alternative form.

H_{3B}: *Prior to the public discovery of financial reporting fraud, CDS spread changes are larger for fraud firms with a weaker corporate governance mechanism.*

Finally, the information asymmetry between corporate insiders and outside investors is an important source of information risk in the debt market (e.g., Wittenberg-Moerman 2008), or what Duffie and Lando (2001) call the transparency component of credit spread. In an environment of high information asymmetry, credit investors can gain greater benefits from acquiring and processing private information, to the extent that newly acquired information can reduce information risk. In this environment, one can expect CDS investors to be more likely to engage intensely in private information gathering activities, enabling them to detect financial reporting irregularities in a timelier manner. In this study, we posit that CDS investors face higher information asymmetry when the reference entities have greater operational complexity and lower accrual quality (or larger discretionary accruals). On the other hand, one may argue that higher information asymmetry makes it more difficult for CDS investors to assess the credit quality of the reference entities. This could, in turn, make CDS investors less capable of detecting any suspicious reporting irregularities. Given the lack of evidence on the issue, we test the following hypothesis in alternative form.

> **H_{3C}:** *Prior to the public discovery of financial reporting fraud, CDS spread changes are larger for fraud firms with greater information asymmetry between corporate insiders and outside investors.*

4. Data and descriptive statistics

4.1. Sample and data sources

We obtain our sample of fraud firms from the Audit Analytics Corporate + Legal database for the period 1997–2013.[10] We extract 6,739 class action litigation cases. Of these cases, we identify 4,497 litigation cases based on securities laws. We then delete (i) cases with a lead defendant not matched to any Compustat firm (1,602 cases), (ii) cases lasting less than two weeks (169 cases), and (iii) cases that are less than four years after previous cases or less than one year before subsequent cases (841 cases). [11] This preliminary filtering leaves 1,885 fraud cases remaining in the sample period. Of these fraud firms, we find that 334 firms are covered by the

Markit CDS database, with a total of 345,396 monthly CDS observations over the sample period. Following Callen et al. (2009), we eliminate CDSs denominated in a non-US currency (211,065), CDSs with modified-modified (MM) restructuring clauses (15,362), and subordinated CDSs (11,512). Since we require CDS observations around our fraud events (i.e., from twelve months before to six months after the trigger event dates), we exclude 105 firms (96,863 CDS observations) with no CDS information available around fraud event dates and 54 firms (652 observations) because of infrequent CDS transactions (i.e., fewer than 25 observations in the

[10] Our period for empirical analysis starts in 2001, since this is the start year for the Markit CDS database.
[11] This is to avoid contamination from recent cases for the same firm.

sample period). Finally, we exclude ten firms (1,095 CDS observations) because of missing values in either Compustat or the Center for Research in Security Prices (CRSP) and 26 firms (1,666 CDS observations) because of missing values in control firms. Hence, our test sample consists of 139 fraud firms with 7,181 CDS observations. Table 1 sequentially describes our sample selection process.

[Insert Table 1]

We create a control sample by matching each of the 139 fraud firms to non-fraud firms with available CDS information from the Markit CDS database. We follow a procedure similar to that of Feng et al. (2011) and match each fraud firm to multiple control firms. For each fraud firm, we rank, based on firm size, all firms in the same two-digit industry with CDS information available around the fraud event dates. We select a maximum of four control firms closest to each fraud firm. Using this procedure, we match 68 fraud firms with four control firms, twelve fraud firms with three control firms, eight fraud firms with two control firms, and 51 fraud firms with one control firm. Hence, our control sample consists of 375 firms with 18,672 CDS observations.[12] Hence, our total sample has 514 firms, with 25,853 CDS observations.

4.2. Descriptive statistics and univariate tests

Table 2, Panel A, provides a univariate comparison of the mean CDS spread and its changes between fraud firms and matched control firms during the 18-month period around fraud event dates. The fraud event date is defined as the trigger event date, which represents the first date of public disclosure indicating possible financial reporting irregularities, such as a restatement date, an SEC investigation date, or an auditor resignation date. The first row shows that in the

[12] In robustness analysis, we also utilize a different control sample based on a PSM approach. We discuss the details of the PSM results in a subsequent section.

Benchmark period, which we define as month -12 to month -7 before the public discovery of fraud, there is no significant difference in the mean spread and spread change between the two groups of firms. However, in the *Before* period (i.e., month -6 to month -1 before fraud event dates), the mean spread and spread change both become significantly higher for fraud firms at the 1% level. The trend amplifies in the *After* period (i.e., month 0 relative to fraud event dates), when the mean CDS spread peaks for fraud firms over the sample period and the mean spread difference between fraud and control firms peaks upon public discovery of the fraud. It is interesting to note that, in the *After_1M* period (i.e., month 1 to month 6 after fraud event dates), the mean CDS spread remains higher for fraud firms than for comparable control firms, while the mean spread change is lower for fraud firms than for control firms.

[Insert Table 2]

Panel B of Table 2 presents descriptive statistics for the variables in our main regression analysis. The mean spread is 1.489 and the mean spread change is 0.037. By construction, CDS observations from fraud firms constitute nearly 28% of our sample (7,181 out of 25,853). As for the control variables, we find that changes in firm size, leverage, and return volatility all have a positive mean over the sample period, while changes in credit rating, ROA, and spot rate all have a negative mean over the same period.

Finally, Panel C of Table 2 presents descriptive statistics for the same variables, separately, for the test sample of fraud firms and the control sample of firms matched to fraud firms. The pvalues reported in the third and six columns represent the levels of significance for t-tests for the mean difference between fraud firms and control firms and the Wilcoxon signed rank test for the median difference between the two, respectively. We find that, as expected, fraud firms have significantly higher spread levels and larger spread changes compared with non-fraud matched control firms.

5. Empirical procedures and results

Our first objective is to examine whether fraud firms have positively significant spread changes around fraud event dates. To test our first hypothesis H_1, we specify the following change regression model:

$$\Delta Spread = \beta_0 + \beta_{1A}\, Before*Fraud + \beta_{1B}\, After*Fraud + \beta_{1C}\, After\_1M*Fraud$$
$$+ \beta_2\, Fraud + \beta_3\, Before + \beta_4\, After + \beta_5\, After\_1M + \beta_6\, \Delta Controls \qquad (1)$$

where Δ*Spread* is the monthly change in a five-year CDS spread[13] and *Fraud* is a dummy variable equal to one for fraud firms and zero for non-fraud control firms. We define three sub-period dummies to partition the time period around fraud event dates into four sub-periods: (1) the *Benchmark* period (month -12 to month -7 before the fraud event dates), (2) *Before* (i.e., takes the value of one if CDS observations are within month -6 to month -1 relative to the fraud event dates and zero otherwise), (3) *After* (i.e., takes the value of one if CDS observations are within month 0 relative to the fraud event dates and zero otherwise), and (4) *After_1M* (i.e., takes the value of one if CDS observations are within month 1 to month 6 relative to the fraud event dates and zero otherwise).

In Eq. (1), our key variables of interest are the interaction variables of *Fraud* with the three sub-period dummies. A positive and significant coefficient for β_{1A} would indicate that CDS spreads increase before the public discovery of fraud (i.e., the fraud event date), implying that credit investors have superior knowledge about a firm's suspected fraudulent activities and thus adjust their CDS spreads to reflect the increase in credit risk accordingly. A positive and significant coefficient for β_{1B} and β_{1C} would indicate that CDS spreads change concomitantly upon the public discovery of fraud, implying that credit investors do not have superior knowledge and react

[13] We focus on five-year CDS spreads because these are the most common and liquid CDS market (Taksler 2006). Nonetheless, our additional analysis considers CDS contracts of different maturities.

concurrently with the rest of the capital market. We partition the post-fraud period (after fraud events) into two sub-periods, *After* and *After_1M*, to separate the immediate from the long-term reactions to the public discovery of fraud.

To control for other characteristics that can affect CDS spread, we include a vector of firmlevel controls and macroeconomic factors, all in a change form, that are known to affect CDS spread in the prior literature (e.g., Callen et al. 2009; Das et al. 2009; Ericsson et al. 2009; Griffin et al. 2014): Δ*Size* (change in firm size), Δ*Leverage* (change in leverage), Δ*Ret_Vol* (change in return volatility), Δ*Rating* (change in credit rating), Δ*ROA* (change in ROA), Δ*Spot* (change in the market spot interest rate), and a set of industry dummies. The variable Δ*Size* acts as a proxy for the completeness of accounting information because larger firms are expected to provide more complete and transparent information, resulting in a lower credit spread (Duffie and Lando 2001). We expect that the higher the leverage, the more volatile the firm return (Δ*Ret_Vol)*, and the lower the firm return *(ROA),* the higher the probability of nonpayment of maturing debts and thus the higher the CDS spread (Callen et al. 2009). We include credit rating since higher-rated firms have a better access to the capital market and are less likely to experience credit events (e.g., Callen et al. 2009). We also expect that the higher the risk-free interest rate, the lower the CDS spread (e.g., Callen et al. 2009). The Appendix provides detailed empirical definitions of these variables.

5.1. Test of H_1: Baseline regression results

Table 3 presents the results of our baseline regression in Eq. (1), using ordinary least squares (OLS). Reported t-values are based on robust and CDS clustered standard errors throughout the paper. Column 1 shows results without the inclusion of the control variables. We find that the interaction term of *Before*Fraud* is positive and significant at the 1% level (coefficient = 0.07), indicating that the CDS spread increased prior to the public discovery of fraud. This finding is in line with H_{1A}, suggesting that some credit investors have gained superior knowledge about a firm's suspected wrongdoings before public discovery and that they have reflected their perceived changes in credit risk in the pricing of the CDSs.

We find that the interaction term of *After*Fraud* is also positive and significant at the 1% level (coefficient = 0.193), with β_{1B} being significantly larger in magnitude than β_{1A} (p-value = 0.000). This finding is consistent with the prediction in H_{1B}. The above findings suggest that many credit investors, *but not all*, do not have superior knowledge about a firm's engagement in fraudulent activities in advance, and these investors react concurrently with the rest of the capital market upon the public discovery of fraud. Our finding corresponds to the perception of the credit derivatives market in which information is imperfect (Duffie and Lando 2001) and supports the view that some credit investors have privileged access to private information about a firm's fraudcommitting behavior prior to its public discovery, while others do not.

Lastly, we find that the coefficient of *After_1M*Fraud*, that is, β_{1C}, is not significant. This finding indicates that credit investors' reactions to the public discovery of fraud tend to be relatively immediate. We obtain very similar results when firm-level factors are controlled for in column 2 of Table 3 and when additional macroeconomic and industrial factors (i.e., Δ*Spot* and industry dummies) are included in column 3. We find that all the control variables are significant determinants of CDS spread, with the same expected signs as reported in the prior literature.[14]

[Insert Table 3]

5.2. Test of H_2: Do banks' monitoring incentives matter for CDS pricing?

[14] Some studies (e.g., Packer and Zhu 2005; Berndt et al. 2006) indicate that restructuring clauses are important determinants of CDS pricing. However, in their empirical study, Callen et al. (2009) do not find restructuring clauses to be a significant factor in CDS spreads. Alternatively, we include two additional variables in the regression model to control for restructuring clauses. The variable *XR* is a dummy equal to one (zero otherwise) when the CDS contains an ex-restructuring clause and *CR* is a dummy variable equal to one (zero otherwise) when the CDS contains a cumrestructuring clause. Though not tabulated here for brevity, we find that the inclusion of *XR* and *CR* does not alter our main results. We also find that neither of the two restructuring variables has any significant impact on CDS pricing.

Next, we investigate whether and how the relation between CDS spread and financial fraud is conditioned upon banks' incentives to monitor the reference entity's credit risk. In so doing, we assume that banks have greater incentives to monitor the reference entity when banks have larger stakes at the reference entity in terms of the lending relationship between the two parties. Specifically, to test H_2, we first construct two proxies for the level of bank monitoring, using bank loan data obtained from Thomson Reuters LPC's DealScan database. First, we obtain from DealScan information about the amount of outstanding bank loans from different banks and identify the largest outstanding loan (relative to total assets) for each firm in a given month. We then construct our first variable, *Has Large Loans*, to represent the level of bank lending activities. Our assumption is that banks with more extensive lending activities would have *"more to lose"* if the credit events happen, and hence would exert more effort to monitor the reference entity's credit risk. This indicator variable, *Has Large Loan*, equals one if a firm's largest outstanding loan (relative to total assets) in a given month is above the sample median.[15] Second, we construct a variable to capture banks' incentives to monitor the reference entities based on loan syndicate structure. In a syndicated loan, ex post monitoring is typically delegated to the lead bank(s). Prior studies (e.g., Sufi 2007; Bharath et al. 2009) show that the monitoring incentive of banks is captured by the fraction of loans held by lead banks. Hence, to measure the monitoring incentive of lead banks, we construct the *#10% Bank* variable which is defined as the total number of lead banks with at least a 10% fraction of a loan over all of a firm's outstanding loans (e.g., Petersen and Rajan 1994). The underlying assumption here is that such lead banks have strong incentive to monitor the reference entity and that the greater is the number of such lead banks in a loan

[15] We do not consider whether a firm has outstanding bank loans, as do a number of studies (e.g., James and Wier 1990; Datta et al. 1999; Dahiya et al. 2003), because the majority of our sample firms (88.7%) have bank loans outstanding.

syndicate, the stronger is the incentives for monitoring credit quality of syndicated loans to the reference entity.

We then partition our sample into two subsamples based on the median levels of the aforementioned bank lending activities and lead bank monitoring, that is, *Has Large Loans* and *#10% Banks*. We then estimate Eq. (1), separately, for each subsample, and report the results in Table 4. Columns 1 and 2 of Table 4 present the regression results for the subsamples of firms with *Has Large Loans* = 0 and *Has Large Loans* = 1, respectively. We find that the coefficient of our key variable of interest, *Before*Fraud*, is positive and highly significant at the 1% level (in column 2) for the subsample of firms with high loan amounts, but is insignificant (in column 1) for the subsample of firms with low loan amounts. The coefficients of *Before*Fraud* in columns 1 and 2 of Table 4 are 0.005 and 0.119, respectively. Tests of the equality of the regression coefficients between the two samples indicate that the difference in magnitude between these two coefficients is highly significant, with $p = 0.005$, as shown in the second row from the bottom of Table 4. The finding is in line with the prediction in H_2, suggesting that, compared to firms with small bank loans, firms with large bank loans are subject to more monitoring and their fraud is more likely to be detected.

The coefficient of *After*Fraud* in column 2 is also highly significant (0.292, $t = 3.869$), while it is insignificant in column 1. The result of a formal test of the equality of these two coefficients reveals that the difference is highly significant ($p < 0.000$), as shown in the bottom row of the table, suggesting that CDS spreads spike more on the public discovery of fraud (within one month) for firms with relatively high bank loans.

In columns 3 and 4 of Table 4, we partition the sample into two subsamples based on the median value of *#10% Banks* to examine whether and how the fraud–CDS spread relation before and after public discovery is differentially influenced by lead banks' monitoring incentives. As shown in columns 3 and 4, we find that the coefficient of *Before*Fraud* is positive and highly significant *only* for the sample of firms with high monitoring incentives (column 4), which is consistent with the prediction in H_2. Finally, we also find that the coefficient of *After*Fraud* is positive and significant at the 5% level for firms with high monitoring incentives. This finding is again consistent with H_2, suggesting that CDS investors engage more intensely in monitoring reference entities when they have larger credit exposure.

[Insert Table 4]

5.3. Tests of H_3: Does the reference entity's credit risk matter for CDS pricing?

While the second hypothesis, H_2, focuses on the monitoring role of banks, our third set of hypotheses, H_{3A} to H_{3C}, is concerned with whether CDS spread changes prior to the public discovery of fraud are more pronounced for the reference entities with higher credit risk. To test these hypotheses, we construct three proxies for the reference entity's credit risk: (i) ex ante default risk (H_{3A}), (ii) corporate governance structure (H_{3B}), and (iii) information asymmetry (H_{3C}).

5.3.1. Test of H_{3A}: The impact of default risk

Hypothesis H_{3A} is based on the notion that firms with higher ex ante default risk tend to have a higher likelihood of credit events. To test this hypothesis, we first measure ex ante default risk using two proxies, that is: (i) the financial constraint; and (ii) its closeness to default of a reference entity. We partition our sample into two subsamples based on the median values of financial constraint and closeness to default, and then, estimate Eq. (1) separately for each subsample. In so doing, we use a firm's credit rating as our proxy for financial constraint, because a firm with a low credit rating encounters greater difficulties in securing additional financing to sustain its operations in times of financial constraint, thereby increasing credit risk. We use

Altman's Z-score to measure a firm's closeness to default. Here we assume that credit investors are more concerned about CDS contracts written on the reference entities with a low Z-score (i.e., high default risk).

Table 5 presents the regression results of CDS spread changes on fraud for the subsamples partitioned by levels of financial constraint (captured by credit rating) and default risk. As shown in columns 1 and 2, we find that credit investors' reactions are concentrated in firms with high financial constraints, as reflected in their low credit rating. The coefficients of both *Before*Fraud* and *After*Fraud* are positive and significant at the 1% level for the subsample of firms with a low credit rating (column 1) but insignificant for the subsample of firms with a high credit rating (column 2). The differences in the coefficients of both *Before*Fraud* and *After*Fraud* between the two subsamples are significant at less than the 1% level, as shown in the second last and last rows, respectively, of the table. The above findings are consistent with the prediction in H_{3A}.

[Insert Table 5]

We also partition the total sample into two subsamples based the median value of Altman's Z-score. In columns 3 and 4 of Table 5, we present the results of regressions for each of the two subsamples with high and low default risk proxied by low and high Altman's Z-score, respectively. As shown in column 3, we find that the coefficients of both *Before*Fraud* and *After*Fraud* are positive and highly significant at less than the 1% level for the subsample of firms with high default risk (i.e., low Z-score). In contrast, as shown in column 4, we find that both coefficients are insignificant, albeit positive, for the subsample of firms with low default risk (i.e., high Z-score).

In summary, the results reported in Table 5 clearly show that the coefficients of both *Before*Fraud* and *After*Fraud* are positive and significant *only* for the subsample of firms with high credit risk. The above results support the view that credit investors engage more in private information gathering and monitoring of the reference entities with higher default risk and are more aware of any financial reporting fraud by these firms.

5.3.2. Test of H_{3B}: The impact of corporate governance

We now examine the impact of corporate governance on CDS spread changes for fraud firms (H_{3B}). We use two alternative proxies to measure the strength of corporate governance: (i) the anti-takeover index, or simply the GIM index developed by Gompers, Ishii, and Metrick (2003), and (ii) the percentage of outstanding shares owned by institutional investors, or simply institutional ownership.

Gompers et al. (2003) show that anti-takeover provisions represent an important aspect in corporate governance for equity investors, affecting firm value and stock return.[16] We partition our total sample into two subsamples of firms based on the GIM index. Given that the anti-takeover index is available only in alternate years, we extrapolate the values for the in-between years, as commonly done in prior related research (e.g., Gompers et al. 2003; Bebchuk et al. 2009). We maintain that higher levels of anti-takeover and managerial entrenchment represent a lower quality of corporate governance. Under this assumption, creditors are more concerned about firms with poor corporate governance. Credit investors are therefore likely to engage more in information gathering for firms with poor governance and to be more aware of any financial reporting irregularities by these reference entities.

[16] In unreported robustness analysis, we also use the entrenchment index developed by Bebchuk et al. (2009) and find similar results.

We also use institutional ownership of 1% to 5% of total shares outstanding to proxy for the quality of corporate governance.[17] Prior studies have specifically examined the role of institutional investors in corrective disclosures that lead to securities litigation (Griffin 2003) and in accounting restatements (Hribar et al. 2009; Burns et al. 2010). Given that institutional investors play a monitoring role, we expect credit investors to engage more in private information gathering and monitoring for the reference entities with low institutional ownership (i.e., poor governance).

Table 6 presents regression results regarding the role of corporate governance in CDS pricing at fraud firms. Due to missing values in our governance proxies, the total number of observations is notably smaller than that used in our main tests.[18] As shown in columns 1 and 2, we find that the coefficients of both *Before*Fraud* and *After*Fraud* are positive and significant, respectively, at less than the 1% and 5% levels for the subsample of firms with poor governance proxied by a high GIM index (column 1), but insignificant for the subsample of firms with a low GIM index (column 2). The results of tests of the equality of the two coefficients between the two different regressions reveal that the differences in these two coefficients between the two subsamples are significant at the 1% and 5% levels, as shown in the last two rows from the bottom of the table, respectively.

In columns 3 and 4 of Table 6, we report the regression results for the subsamples of firms with low and high institutional ownership, respectively. We find that while the coefficient of *Before*Fraud* is positive and significant (at the 1% level) in column 3, it is not significant in column 4. As shown in the second to last row from the bottom of the table, the difference in their magnitudes is highly significant. In addition, the coefficient of *After*Fraud* is positive and

[17] We follow Ali et al. (2008), since they show that institutions with medium stakeholdings are better monitors and better informed.

[18] Note that the number of observations is larger in columns (2) and (4), since we partition sample firms into terciles for both variables and denote the firms in the last (first) GIM index (institutional ownership) tercile as high GIM index (low institutional ownership) firms.

significant in both columns, but its magnitude is larger in column 3, albeit the difference is statistically insignificant. Collectively, the results in Table 6 suggest that credit investors are more concerned with CDS offerings in reference to firms with lower-quality governance. Hence, some creditors may devote more time and effort to gather private information about these firms with relatively poor governance and to monitor such firms. They are therefore better able to discover financial irregularities by these poorly governed firms before the public disclosure of fraud.

[Insert Table 6]

5.3.3. Test of H_{3C}: The impact of information asymmetry

Hypothesis H_{3C} is concerned with whether and how the information asymmetry between corporate insiders and outside investors influences the fraud discovery–CDS spread relation. To test H_{3C}, we measure the information asymmetry using a firm's operational complexity and accrual quality. We assume that the information asymmetry is higher for firms with higher operational complexity and low accrual quality. We argue that private information gathering and monitoring activities are of more value for firms with higher information asymmetry. We measure operational complexity by the number of business segments (e.g., Cohen and Lou 2012).[19]

Columns 1 and 2 of Table 7 report the results of regressions for the subsamples of firms of high and low operational complexity.[20] As shown in columns 1 and 2, we find that the coefficients of *Before*Fraud* are positive and highly significant for both subsamples of firms. We find, however, that the magnitudes of these two coefficients are about three times greater for the subsample of firms of high operational complexity (0.155 in column 1) than for the subsample of firms of low

[19] Alternatively, we define a business segment as a major business division that comprises at least 1% of total sales. We find that our results remain qualitatively similar.

[20] Note the number of observations is larger in columns (2) and (4), since we partition sample firms in terciles for both variables and denote firms in the last # Segments and SD_DA terciles as High # Segments and SD_DA firms, respectively.

operational complexity (0.057 in column 2) and the difference in magnitude is also statistically significant, as shown in the second to last row of the table. We also find that the coefficients of *After*Fraud* are positive and significant for both subsamples. Moreover, we find that the magnitudes of these two coefficients are about six times greater for the subsample of firms of high operational complexity (0.471 in column 1) than for the subsample of firms of low operational complexity (0.085 in column 2). As shown at the bottom of the table, this difference in magnitude is statistically significant as well.

[Insert Table 7]

In columns 3 and 4 of Table 7, we partition the total sample into two subsamples, firms of high and low accrual quality, and then estimate our baseline regression in Eq. (1) separately for each subsample. Dechow et al. (2011) show that accrual quality is a significant predictor of accounting misstatements. We argue creditors can also utilize accrual quality measures when assessing information risk associated with their CDS pricing. We measure accrual quality by the standard deviation of discretionary accruals over the last five years, where discretionary accruals are estimated using the residuals from the Dechow–Dichev (2002) model.[21] Columns 3 and 4 of Table 7 present the regression results for the subsamples of firms with low and high accrual quality, respectively (i.e., high and low standard deviations of discretionary accruals, respectively). We find that the coefficient of *Before*Fraud* is highly significant at less than the 1% level for the subsample of firms with low accrual quality (column 3), but insignificant in column 4. We also note that the same coefficient is about seven times larger in column 3 than in column 4 and the difference in its magnitude is highly significant, as shown in the bottom part of the table. The

[21] We estimate the abnormal accruals using the Dechow–Dichev (2002) model for each two-digit Standard Industrial Classification industry in each year with at least 20 observations.

coefficient of *After*Fraud* is highly significant in column 4, but insignificant in column 3. However, the difference in coefficients between the two columns is not statistically significant, as shown in the last row of the table (p-value = 0.636).

Collectively, the above results are consistent with H_{3C}, suggesting that high information asymmetry, as reflected by high operational complexity and low accrual quality, motivates some credit investors to devote more time and effort in monitoring to gathering private information about these firms before the public discovery of fraud. Therefore, these credit investors are better able to detect suspicious fraudulent activities in the pre-discovery period. Overall, the results reported in Table 7, taken as a whole, provide strong and reliable evidence that some credit investors do consider both a firm's operational complexity and accrual quality when determining CDS pricing.

6. Additional analysis and robustness check

6.1. The seriousness of fraud

Fich and Shivdasani (2007) and Brochet and Srinivasan (2014) show that the seriousness of fraud matters for firms and directors subject to securities litigation. We presume that credit investors are concerned about credit risk associated with fraud and that the more serious the fraud, the higher the credit risk associated therewith. Hence, we expect a positive correlation between the seriousness of fraud and CDS spread changes upon fraud discovery. Moreover, the suspicion of more serious financial fraud could drive credit investors to devote more time and effort in monitoring to gathering information about a reference entity. More serious fraud could also simply be easier to detect before its public discovery (Gao et al. 2015). We therefore predict that CDS spread changes increase with the seriousness of fraud in the pre-discovery period. We test this prediction by introducing an alternative variable in lieu of *Fraud* in the regression model, *Fraud*

Length, defined as the logged value of the fraud period length (the number of months from the exposure start date to the exposure end date). We use this variable to proxy for the seriousness of fraud, since more serious fraud typically takes longer to commit (Fich and Shivdasani 2007; Gao et al. 2015).

Table 8, column 1, presents the results with which we test the seriousness of fraud on CDS spread changes. We find the coefficient of *Before* Fraud Length* is positive and significant at the 1% level, indicating that the longer the fraud committing period, the higher the likelihood that some credit investors are able to detect fraudulent financial reporting irregularities before the public disclosure of fraud and these investors increase CDS spreads accordingly.

[Insert Table 8]

6.2. Potential endogeneity and PSM design

One can argue that reverse causality may exist in our empirical analysis if the increase in CDS spreads constitutes a credible negative market signal to firms and thus prompts managers to engage in more aggressive reporting practices that subsequently lead to litigation. Kim et al. (2014) also shows that increases in CDS spread can compel managers to disclose news faster than they would otherwise. To alleviate such an endogeneity concern, we apply a PSM approach and construct a control sample of the closest four firms by matching the fraud firms with non-fraud firms based on the predicted likelihood, or propensity score, of fraud. Specifically, we use the estimated coefficients of the accounting misstatement model from Dechow et al. (2011) to compute the predicted likelihood. We then follow the same matching procedure as Gao et al. (2015) do.

Table 8, column 2, presents the results using the PSM approach to construct the control sample. We find that the coefficients of both *Before*Fraud* and *After*Fraud* are positive and highly significant at less than the 1% level. The findings suggest that our main results are unlikely to be driven by possible endogeneity with respect to the relation between fraud and CDS spreads.

6.3. Alternative definitions of the before and after periods

In our main analysis, we use the trigger event dates as the dates of the fraud events, since they represent the time a firm first attracted the public's attention, as documented in the litigation release. While trigger events such as restatements and the firing of an auditor can happen in a single day, other events, such as insider trading, can happen over a short period of time before it catches the public's attention. To see if our main results are sensitive to the time period we use to define the *Before* and *After* periods, we alternatively define *Before* as the time period from month -6 to month -2 relative to the fraud event dates and *After* as the time period from month -1 to month 0 relative to the fraud event dates. Table 8, column 3, shows that the coefficients of *Before*Fraud* and *After*Fraud* are positive and highly significant at less than the 1% level, suggesting that some credit investors have private information about fraud activities at least two months before the public discovery of fraud, while other credit investors react, along with the rest of the market participants, starting a month before the public discovery of fraud.

6.4. Does the maturity of a CDS contract matter?

Our main analysis uses CDS contracts with a five-year maturity, because these are the most popular and thus the most liquid. As part of our sensitivity tests, we also consider the impact of fraud on one-year CDSs because these credit investors have the shortest investment horizon and may behave differently from other longer-term credit investors. On one hand, investors in these shorter-term CDS may not be as concerned with credit risk because their investment time horizon is short. On the other hand, these investors may opt for shorter-term CDSs for some reference entities because their inherent credit risk may already be too high.[22] Table 8, column 4, reveals that the use of CDS contracts with a one-year maturity does not alter any statistical inferences on our test variables. More specifically, we find that the coefficients of *Before*Fraud* and *After*Fraud* are both positive and highly significant.

6.5. Other robustness checks

In un-tabulated tests, we conduct several additional analyses to ensure the robustness of our findings. First, we define spread change as the weekly, instead of monthly, change in CDS spreads. Note that, in our main analysis, we use monthly spread changes because CDS spreads do not always change on a weekly basis for less liquid reference entities. Second, in our main analysis, we include repeat fraud firms (i.e., firms that get caught more than once over the sample period) as long as the fraud event periods are non-overlapping. We repeat our analysis excluding these repeat offenders. Third, we augment the regression model by including additional firm-specific controls, including changes in the market-to-book ratio (MTB) and changes in cash flows from operations. Fourth, we augment the regression model by including not only the changes but also the levels for the firm fundamental variables. We find that our results are robust to the use of these alternative samples and model specifications.

7. Summary and concluding remarks

This paper presents novel evidence from the CDS market on the effect of financial reporting fraud on credit spread changes. Our findings show a substantial increase in CDS spread changes for fraud firms during the pre-discovery months leading up to the trigger event dates. We also

[22] As an example, investors may be more willing to buy and sell five-year CDSs on the sovereign bonds of United States and Canada but they might choose to trade only one-year CDSs on the sovereign bonds of countries such as the Ukraine and Greece.

observe a large increase in CDS spread changes upon fraud discovery on the trigger event dates. The results suggest that some credit investors are able to anticipate financial reporting fraud prior to its public discovery. The results also suggest that other credit investors, who do not possess private information, react concurrently with the rest of the capital market at the time of public discovery.

We show that banks' monitoring incentives play an important role in the detection of financial reporting fraud for CDS investors. We show banks have an information advantage via their lending activities and ex post monitoring associated therewith, since the pre-discovery CDS spread changes are more pronounced for fraud firms with more intensive bank monitoring. We also identify the types of firms that pose more serious credit concerns for CDS investors and require more monitoring. For these reference entities, credit investors are motivated to exert more time and effort to monitor their credit risks and engage in more extensive information gathering, enabling them to better assess any financial reporting irregularities. We show that CDS investors are more likely to increase their spreads before the public discovery of fraud for reference entities with higher levels of financial constraint and default risk, weaker corporate governance structure, higher operational complexity, and a lower quality of accounting information.

Overall, the evidence reported in our study provide strong and reliable evidence that the discovery of financial reporting fraud or irregularities is perceived as an unfavorable event that increases downside risk in general and credit risk in particular. And, because monitoring matters, some CDS market participants who exert monitoring effort could correctly anticipate the public discovery of fraud in advance, while others could not.

Previous research on fraud has paid relatively little attention to the pre- and post-discovery impacts of fraudulent financial reporting activities on credit risk. Given the evidence that the public discovery of fraud increases credit risk, a natural question for further research is whether such discovery increases credit risk via increasing default risk or information risk or both. Knowing that CDS market participants are predominantly sophisticated institutional investors, such as banks, insurance companies, and hedge funds, and thus the CDS market leads the bond market or the equity market in price discovery, it would also be interesting to examine whether and how private information, if any, gathered by investors in the (less regulated or unregulated) CDS market during the pre-discovery period is transmitted to other markets, such as (more regulated) equity and bond markets or option markets. Given the scarcity of empirical evidence on the above issues, further research in this direction seems warranted.

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Appendix: Variable definitions

ΔSpread	Monthly change in 5-year CDS spread
Fraud	A dummy variable with the value 1 for fraud firms and 0 otherwise
Before	A dummy variable that takes the value 1 if the sample period is from month -6 to month -1 relative to the fraud event date (trigger event date)
After	A dummy variable that takes the value 1 if the sample period is month 0 relative to the fraud event date (trigger event date)
After_1M	A dummy variable that takes the value 1 if the sample period is from month 1 to month 6 relative to the fraud event date (trigger event date)
Size	Logged quarterly total assets (item 44)
Leverage	Long-term debt at quarter-end (item 51) divided by quarterly total assets (item 44)
Ret_Vol	Standard deviation of daily stock returns in the fiscal quarter
Ratings	Score of Standard & Poor's credit ratings, with AAA equal to 18 and ratings at or below CCC+ with value 2
ROA	Income before extraordinary items in the quarter (item 8) divided by quarterly total assets (item 44)
Spot	One-year T-bill rate
Has Large Loans	A dummy variable with the value 1 if a firm's largest outstanding loan (normalized by total assets) in a month is above the sample median and 0 otherwise
#10% Banks	Number of lead banks with at least a 10% share of the loan
Credit Rating	A dummy variable with the value 1 if a firm has long-term credit ratings assigned by Standard & Poor's and 0 otherwise
Z-Score	Computed as the sum of 1.2*working capital (item 179), 1.4*retained earnings (item 36), 3.3*pretax income (item 170), and 0.999*sales (item12) divided by total assets (item 6)
GIM Index	Corporate anti-takeover index from Gompers, Ishii, and Metrick (2003)
Institutional Ownership	Fraction of shares owned by institutions with medium stockholdings
# Segments	Number of business segments
SD_DA	Standard deviation of discretionary accruals over the last 5 years, where discretionary accruals are estimated using the Dechow–Dichev (2002) model and current accruals are defined as changes in current assets minus changes in current liability and cash plus changes in current debt divided by total assets
Fraud Length	The logged value of the length of the fraud period (number of months from the exposure start date to the exposure end date)
ΔSpread_1Yr	Monthly change in 1-year CDS spread

Table 1: Sample Selection

	Firm-cases	CDS
Total class action litigation lawsuits	6,739	
Less non-securities class action lawsuits	(2,242)	
Total securities class action litigation lawsuits	4,497	
Less		
Cases with lead defendant a non-Compustat firm	(1,602)	
Cases with fraud duration less than 2 weeks	(169)	
Cases with start date less than 4 years after previous cases or 1 year before subsequent cases	(841)	
Remaining fraud firms	1,885	
Fraud firms with monthly observations in CDS data with nonmissing maturity and spread	334	345,396
Less		
CDS denominated in a non-US dollar currency		(211,065)
CDS with MM restructuring clauses		(15,362)
Subordinated CDSs		(11,512)
Firms without CDS observations around trigger event dates	(105)	(96,863)
Firms with infrequent CDSs	(54)	(652)
Firms with missing data from Compustat and the CRSP	(10)	(1,095)
Firms with missing values for control firms	(26)	(1,666)
Fraud firms with CDS data around trigger event dates	139	7,181
Control firms with CDS data around trigger event dates	375	18,672
Final sample	514	25,853

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Table 2 Summary Statistics

Panel A: Comparison of the mean CDS spread between fraud and control firms						
	Spread			*ΔSpread*		
Sub-periods	Fraud firms	Control firms	Mean equality test p-value	Fraud firms	Control firms	Mean equality test p-value
Benchmark (-12, -7)	1.398	1.364	0.421	0.021	0.032	0.259
Before (-6, -1)	1.632	1.434	0.000	0.090	0.031	0.000
After (0, 0)	1.922	1.461	0.000	0.205	0.023	0.000
After_1M (+1, +6)	1.916	1.465	0.000	0.007	0.038	0.007

Panel B: Summary statistics of main variables					
	Mean	SD	Q1	Median	Q3
Spread	1.489	1.796	0.400	0.777	1.839
ΔSpread	0.037	0.435	-0.050	0.000	0.059
Fraud	0.278	0.448	0.000	0.000	1.000
ΔSize	0.046	0.170	-0.026	0.049	0.115
ΔLeverage	0.010	0.062	-0.016	0.000	0.032
ΔRet_Vol	0.001	0.011	-0.004	-0.000	0.003
ΔRating	-0.011	0.207	0.000	0.000	0.000
ΔROA	-0.010	0.079	-0.019	-0.001	0.011
ΔSpot	-0.015	0.201	-0.070	0.000	0.090

Panel C: Mean and median comparison of main variables						
	Mean			Median		
	Fraud	Control	p-Value	Fraud	Control	p-Value
Spread	1.660	1.424	0.000	0.826	0.764	0.001
ΔSpread	0.048	0.033	0.011	0.000	0.000	0.073
ΔSize	0.050	0.045	0.038	0.045	0.049	0.000
ΔLeverage	0.012	0.004	0.000	0.002	-0.002	0.000
ΔRet_Vol	0.001	0.000	0.040	-0.000	-0.000	0.441
ΔRating	-0.027	-0.004	0.000	0.000	0.000	0.676
ΔROA	-0.000	-0.004	0.000	-0.001	-0.000	0.000
ΔSpot	-0.021	-0.013	0.006	-0.010	0.000	0.001

Table 3 Regression of CDS Spread Change on Fraud

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This table presents the regression results of monthly CDS spread change on fraud. The dependent variable is *ΔSpread*, the monthly change in a five-year CDS spread. All other variables are as defined in the Appendix. Robust and CDS clustered t-statistics are reported in parentheses. The superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
Before×Fraud	0.070***	0.054***	0.055***
	(3.638)	(3.046)	(3.151)
After× Fraud	0.193***	0.157***	0.162***
	(4.438)	(3.794)	(3.929)
After_1M×Fraud	-0.020	-0.011	-0.013
	(-1.020)	(-0.633)	(-0.741)
Fraud	-0.011	-0.011	-0.013
	(-0.838)	(-0.876)	(-1.132)
Before	-0.001	0.005	0.006
	(-0.100)	(0.570)	(0.784)
After	-0.009	-0.007	-0.008
	(-0.507)	(-0.408)	(-0.485)
After_1M	0.006	0.004	0.007
	(0.677)	(0.468)	(0.922)
ΔSize		-0.109***	-0.094***
		(-4.056)	(-3.842)
ΔLeverage		0.117**	0.095*
		(2.522)	(1.864)
ΔRet_Vol		8.638***	7.432***
		(15.788)	(14.060)
ΔRating		-0.128***	-0.129***
		(-5.067)	(-5.275)

: Regression of CDS Spread Change on Fraud

ΔROA		-0.546***	-0.480***
		(-4.866)	(-4.440)
ΔSpot			-0.190***
			(-11.387)
Constant	0.032***	0.028***	-0.027***
	(5.347)	(4.784)	(-5.017)
Industry dummies	NO	NO	YES
Observations	25853	25853	25853
Adjusted R^2	0.004	0.069	0.085

Table 4 – Bank Lending Activities and Monitoring

This table presents the regression results of monthly CDS spread change on fraud for subsamples partitioned by bank loan variables. The dependent variable is *ΔSpread*, the monthly change in the five-year CDS spread. All other variables are as defined in the Appendix. All columns include industry dummies. Robust and CDS clustered t-statistics are reported in parentheses. The superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Has Large Loans		*#10% Banks*	
	Low	High	Low	High
Before×Fraud	0.005	0.119***	-0.042	0.139***
	(0.322)	(3.189)	(-1.313)	(3.302)
After× Fraud	-0.021	0.292***	-0.006	0.196**
	(-0.461)	(3.869)	(-0.084)	(2.300)
× *After_1M Fraud*	-0.046**	0.014	-0.063	0.015
	(-2.187)	(0.443)	(-1.364)	(0.409)
Fraud	0.031**	-0.071***	0.039*	-0.030
	(2.484)	(-3.106)	(1.901)	(-1.235)
Before	0.010	0.007	0.019	-0.025*
	(0.989)	(0.532)	(0.955)	(-1.708)
	0.005		0.075*	0.055**

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After		0.012		
	(0.214)	(0.548)	(1.794)	(1.989)
After_1M	0.012	0.025**	0.028	0.028*
	(1.345)	(2.182)	(1.624)	(1.896)
ΔSize	-0.149***	-0.052	-0.269***	-0.044
	(-4.327)	(-1.457)	(-4.167)	(-1.347)
ΔLeverage	-0.097	0.175***	0.269*	0.124
	(-0.890)	(2.753)	(1.902)	(1.276)
ΔRet_Vol	8.291***	6.496***	6.089***	6.212***
	(8.366)	(9.463)	(7.313)	(5.078)
ΔRating	-0.125**	-0.109***	-0.111	-0.073**
	(-2.192)	(-4.621)	(-1.594)	(-2.434)
ΔROA	-0.875***	-0.497***	-0.953***	-1.140***
	(-3.579)	(-3.500)	(-4.638)	(-6.816)
ΔSpot	-0.164***	-0.244***	-0.186***	-0.192***
	(-7.779)	(-8.428)	(-6.039)	(-4.945)
Constant	0.020***	0.029**	0.999***	-0.024***
	(3.176)	(2.091)	(25.196)	(-2.798)
Observations	11498	11466	6183	5801
Adjusted R^2	0.107	0.089	0.118	0.108
p-Value: Before×Fraud	0.005		0.001	
p-Value: After×Fraud	0.000		0.076	

Table 5: Regression of CDS Spread Change on Fraud, by Financial Constraint and Default Risk

This table presents the regression results of monthly CDS spread change on fraud for subsamples partitioned by financial constraint and default risk. The dependent variable is *ΔSpread*, the monthly change in the five-year CDS spread. All other variables are as defined in the Appendix. All columns include industry dummies. Robust and CDS clustered t-statistics are reported in parentheses. The superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Credit Rating		*Z-Score*	
	Low	High	Low	High
Before×Fraud	0.107***	-0.009	0.115***	0.009
	(3.404)	(-0.738)	(3.026)	(0.604)
After×Fraud	0.282***	0.039	0.292***	0.046
	(3.835)	(0.922)	(3.276)	(1.552)
After_1M×Fraud	0.012	-0.047***	0.011	-0.032*
	(0.368)	(-3.927)	(0.275)	(-1.689)
Fraud	-0.030	0.012*	-0.052*	-0.001
	(-1.355)	(1.947)	(-1.704)	(-0.066)
Before	-0.004	0.029***	-0.026	-0.003
	(-0.396)	(3.392)	(-1.541)	(-0.397)
After	-0.015	0.010	-0.019	-0.014
	(-0.737)	(0.328)	(-0.711)	(-0.750)
After_1M	-0.001	0.025***	-0.027*	0.014
	(-0.086)	(2.599)	(-1.742)	(1.552)
ΔSize	-0.064**	-0.208***	-0.065**	-0.085***
	(-2.547)	(-3.060)	(-2.381)	(-4.036)

: Regression of CDS Spread Change on Fraud, by

ΔLeverage	0.222***	-0.182***	0.255***	-0.094***
	(3.212)	(-2.770)	(2.831)	(-3.362)
ΔRet_Vol	7.412***	6.971***	7.354***	7.052***
	(11.954)	(7.404)	(9.043)	(14.403)
ΔRating	-0.139***	-0.054	-0.133***	-0.079**
	(-4.982)	(-1.574)	(-3.788)	(-2.521)
ΔROA	-0.482***	-0.728***	-0.400***	-0.178*
	(-3.879)	(-2.723)	(-2.736)	(-1.704)
ΔSpot	-0.231***	-0.143***	-0.328***	-0.053***
	(-9.541)	(-6.455)	(-9.640)	(-3.167)
Constant	-0.076**	0.007	-0.276***	-0.094***
	(-2.437)	(0.700)	(-8.281)	(-2.958)
Observations	15825	10028	9852	9752
Adjusted R^2	0.082	0.125	0.084	0.081
p-Value: Before×Fraud	0.001		0.009	
p-Value: After×Fraud	0.004		0.009	

Table 6 Corporate Governance

This table presents the regression results of monthly CDS spread change on fraud for subsamples partitioned by the level of corporate governance. The dependent variable is *ΔSpread*, the monthly change in the five-year CDS spread. All other variables are as defined in the Appendix. All columns include industry dummies. Robust and CDS clustered

: Regression of CDS Spread Change on Fraud, by

t-statistics are reported in parentheses. The superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	GIM Index		*Institutional Ownership*	
	High	Low	Low	High
Before×Fraud	0.091***	-0.019	0.134***	0.018
	(3.545)	(-0.870)	(3.352)	(1.308)
After×Fraud	0.266**	-0.073	0.228***	0.136***
	(2.127)	(-1.432)	(3.036)	(2.767)
After_1M×Fraud	0.026	-0.050	0.026	-0.033*
	(0.972)	(-1.506)	(0.840)	(-1.664)
Fraud	-0.030*	0.080***	-0.078***	0.017
	(-1.944)	(3.655)	(-2.977)	(1.569)
Before	-0.013	0.012	0.019	-0.000
	(-1.118)	(1.500)	(0.967)	(-0.045)
After	-0.031**	0.041***	-0.018	-0.006
	(-2.558)	(4.417)	(-0.462)	(-0.418)
After_1M	-0.003	0.006	0.009	0.004
	(-0.304)	(0.795)	(0.591)	(0.452)
ΔSize	0.034	-0.142***	-0.125***	-0.103***
	(1.352)	(-3.553)	(-3.146)	(-5.361)
ΔLeverage	-0.099	0.129*	0.152*	0.014

: Regression of CDS Spread Change on Fraud, by

	(-1.192)	(1.875)	(1.833)	(0.226)
ΔRet_Vol	7.470***	8.680***	8.074***	7.089***
	(4.702)	(7.046)	(8.823)	(11.959)
ΔRating	-0.167***	0.006	-0.039	-0.174***
	(-2.903)	(0.096)	(-1.367)	(-5.170)
ΔROA	-0.419*	-1.360***	-0.861***	-0.189
	(-1.868)	(-4.021)	(-6.218)	(-1.509)
ΔSpot	-0.040	-0.102***	-0.316***	-0.122***
	(-1.247)	(-3.725)	(-9.384)	(-6.819)
Constant	0.005	-0.183***	-0.125***	-0.036***
	(0.372)	(-9.833)	(-3.220)	(-6.850)

Observations 2595 5977 8631 17222 Adjusted R^2 0.083 0.082 0.089 0.104 p-Value: Before×Fraud 0.001 0.006

p-Value: After×Fraud 0.012

0.304

Table 7 Operational Complexity and Accrual Quality

This table presents the regression results of monthly CDS spread change on fraud for subsamples partitioned by the level of operational complexity or accrual quality. The dependent variable is *ΔSpread*, the monthly change in the fiveyear CDS spread. All other variables are as defined in the Appendix. All columns include industry dummies. Robust and CDS clustered t-statistics are reported in parentheses. The superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	# Segments		*Dechow-Dichev Model*	
	High	Low	High *SD_DA*	Low *SD_DA*

: Regression of CDS Spread Change on Fraud, by

Before×Fraud	0.155***	0.057**	0.170***	0.024
	(3.598)	(2.203)	(3.785)	(1.103)
After× Fraud	0.471***	0.085*	0.145	0.196***
	(4.154)	(1.780)	(1.525)	(3.677)
× *After_1M Fraud*	0.091**	-0.021	0.071*	-0.026
	(2.001)	(-0.890)	(1.789)	(-1.009)
Fraud	-0.047*	-0.013	-0.077***	-0.008
	(-1.778)	(-0.830)	(-2.884)	(-0.428)
Before	0.011	0.007	-0.026*	-0.012
	(0.800)	(0.605)	(-1.660)	(-1.001)
After	-0.035	-0.020	-0.013	-0.025
	(-1.588)	(-0.765)	(-0.449)	(-1.170)
After_1M	-0.019	0.015	-0.025	-0.001
	(-1.589)	(1.147)	(-1.596)	(-0.122)
ΔSize	0.032	-0.111***	-0.059	-0.073**
	(0.696)	(-3.361)	(-1.146)	(-2.504)
ΔLeverage	-0.106	0.068	0.176*	0.143**
	(-0.412)	(1.074)	(1.704)	(2.228)
ΔRet_Vol	8.728***	6.904***	6.499***	7.828***
	(7.340)	(9.849)	(5.635)	(11.923)
ΔRating	-0.135***	-0.153***	-0.111*	-0.118***
	(-2.744)	(-4.640)	(-1.887)	(-4.869)
ΔROA	-0.503**	-0.475***	-0.272**	-0.416***
	(-2.415)	(-3.736)	(-2.263)	(-2.689)

: Regression of CDS Spread Change on Fraud, by

ΔSpot	-0.142***	-0.230***	-0.202***	-0.180***
	(-3.822)	(-10.003)	(-4.885)	(-7.875)
Constant	0.008	0.058***	0.023	-0.016
	(0.648)	(3.660)	(1.455)	(-0.768)
Observations	5113	13854	6440	12930
Adjusted R^2	0.131	0.085	0.056	0.088
p-Value: Before×Fraud	0.051		0.004	
p-Value: After×Fraud	0.002		0.636	

Table 8: The Regression of CDS Spread Change on Fraud– Additional Analysis

This table presents the regression results of monthly CDS spread change on fraud in additional analysis. Column 1 replaces the independent variable *Fraud* with *Fraud Length.* Column 2 replicates the main analysis using the PSM control sample. In column 3, *Before* is a dummy variable with value one if the period is between month -6 and month -2 relative to the fraud event date and zero otherwise and *After* is a dummy variable with value one if the period is between month -1 and month 0 relative to the fraud event date and zero otherwise. Column 4 replaces the dependent variable *ΔSpread* with *ΔSpread_1Yr,* the monthly change in the one-year CDS spread. All other variables are as defined in the Appendix. Robust and CDS clustered t-statistics are reported in parentheses. The superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	(1) *Fraud Length*	(2) *PSM Control Sample*	(3) *Alt. Before & After*	(4) *ΔSpread_1Yr*
Before×Fraud (Fraud Length)	0.017***	0.053***	0.048***	0.053**
	(2.719)	(3.088)	(2.730)	(2.149)
After× Fraud (Fraud Length)	0.050***	0.137***	0.126***	0.179***
	(3.394)	(3.314)	(4.620)	(2.927)
After_1M× Fraud (Fraud Length)	-0.008	-0.022	-0.013	-0.034
	(-1.253)	(-1.281)	(-0.740)	(-0.980)
Fraud (Fraud Length)	-0.003	-0.004	-0.013	0.002
	(-0.610)	(-0.311)	(-1.135)	(0.107)
Before	0.008	0.008	0.002	-0.004
	(1.011)	(1.116)	(0.277)	(-0.254)
After	-0.005	0.019	0.009	-0.044*
	(-0.291)	(1.249)	(0.800)	(-1.673)
After_1M	0.007	0.018**	0.007	0.021
	(0.910)	(2.354)	(0.928)	(1.090)
ΔSize	-0.093***	-0.071***	-0.093***	-0.177**
	(-3.697)	(-3.293)	(-3.815)	(-2.416)
ΔLeverage	0.095*	0.134***	0.094*	0.106
	(1.865)	(3.200)	(1.856)	(0.524)
ΔRet_Vol	7.557***	6.836***	7.418***	8.327***
	(14.482)	(11.213)	(14.052)	(5.543)
ΔRating	-0.132***	-0.108***	-0.128***	-0.195***
	(-5.308)	(-4.931)	(-5.222)	(-3.964)
				-0.709***

ΔROA	-0.482***	-0.871***	-0.478***	
	(-4.425)	(-6.651)	(-4.442)	(-3.615)
ΔSpot	-0.193***	-0.172***	-0.191***	-0.388***
	(-11.487)	(-10.586)	(-11.415)	(-5.738)
Constant	-0.027***	-0.034***	-0.027***	0.747
	(-5.293)	(-6.796)	(-5.020)	(1.417)
Observations	25179	25756	25853	21870
Adjusted R^2	0.083	0.080	0.085	0.043