BANKS' ACQUISITION OF PRIVATE INFORMATION ABOUT FINANCIAL MISREPORTING

BY

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DISSERTATION

Submitted in partial fulfillment of the requirements
for the degree of Doctor of Philosophy in Accountancy
in the Graduate College of the
University of Illinois at Urbana-Champaign, 2012

Urbana, Illinois

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ABSTRACT

This study investigates whether banks acquire information about the borrowing firms' subsequent financial restatements during the misreporting period, a period in which firms issue misstated financial reports. Finance theory suggests that banks have superior ability in gathering and processing information. In this paper, I test whether banks respond to financial misreporting by their client firms before this malpractice becomes known to the public.

I find that bank loans initiated to restating firms during the misreporting period have significantly higher interest spreads, more restrictive financial covenants, and shorter loan maturities than loans made to non-restating firms. This finding suggests that banks are aware of and responsive to borrowers' ongoing financial misreporting. In contrast, equity holders do not respond differently to earnings announcements of restating firms than to those of non-restating firms. I also do not find an increase in analyst forecast dispersion for restating firms during the misreporting period. In addition, bondholders do not price new bond issues differently for restating and non-restating firms.

Taken together, these empirical findings suggest that banks possess private information about ongoing financial misreporting by borrowers, which allows them to adapt their decisions more quickly than equity investors, financial analysts, and public debtholders.

ACKNOWLEDGEMENTS

I would like to express profound gratitude to my advisor and the chair of my dissertation committee, Professor A. Rashad Abdel-Khalik, for his continuous guidance and support along the way. I have benefited tremendously from his thought-provoking remarks and insightful comments on my dissertation. He has also inspired me with his passion for the profession, dedication to achieving high standards of academic work, and commitment to helping students. It has been a great pleasure and an honor to work with and learn from him through all these years.

I would like to thank my wife, Yen-Lin, for being supportive and caring during the course of my Ph.D. program. Her company has brought me happiness, inner peace, and momentum for hard working. I also want to give special thanks to my parents for their constant encouragement and care about me studying abroad for these many years. I would not have been able to achieve this milestone of my life without their teaching and support.

I greatly appreciate the guidance and support from Professor Ganapathi S. Narayanamoorthy, my dissertation committee member and coauthor of a working paper. He always provided brilliant suggestions to help improve my work and gave me confidence in pursuing high quality research. Thanks also go to two of my dissertation committee members, Professor Heitor Almeida and Professor Kevin Jackson, for their generous support and helpful comments on my paper.

Finally, I would like to thank my peers in the doctoral program at the University of Illinois for making the Ph.D. study a memorable and rewarding lifetime experience.

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CHAPTER 1

INTRODUCTION

Decision relevance of financial statements is a key objective in financial reporting, and reliability is a prerequisite for rendering information as relevant. As a result, any actions or events that undermine the reliability of financial statements threaten investors' and creditors' confidence in the information. Financial restatements are one of those events as they reduce the credibility of firms' financial statements. Revealing the inaccuracy of previously reported financial numbers can lead to revised beliefs about the firm's expected levels and risk of future cash flows. Prior literature has documented various adverse consequences of announcing restatements[1]; however, little is known about information use when the events that constitute misreporting were taking place (i.e., when firms issued misstated financial statements). Before firms announce the restatement, information about financial misreporting remains unknown to the public. In this study, I investigate whether banks access and use this private information during the *misreporting period*, identified as the time between the starting and ending dates of each restatement. Specifically, I use the pattern and behavior of the terms of bank loan contracts as the proxy for information acquisition to test whether banks' decisions are consistent with possessing signals about the ongoing financial misreporting by borrowers. This test should be viewed as a joint test of banks' *acquisition* and *use* of information in the lending process.

Bank loans are a major source of corporate financing for most U.S. companies, and banks are considered to have superior access to client information (Fama 1985, Sharpe 1990, Diamond 1991a). Loan contracts provide the mechanism for banks to closely monitor their clients and

[1] Prior studies document that restating firms are faced with negative stock market return (Palmrose et al. 2004, Anderson and Yohn 2002), increased cost of equity capital (Hribar and Jenkins 2004), higher information risk (Kravet and Shevlin 2010), and increased management turnover (Desai et al. 2006) after the announcement of restatement.

have continuous access to borrower information. Fama (1985) posits that banks, as inside debt holders, have access to information about borrowers that is not publicly available otherwise. If they do have a superior ability to gather and process information, banks should be able to identify signals diagnostic of financial misreporting earlier than other outside capital providers. Early identification of these signals allows banks to price the added risk and tailor contractual terms to safeguard their loans in a timely manner.[2] The counterargument is that misreporting firms[3] will try to prevent related information or signals of misconduct from being detected. Therefore, it is unclear a priori whether banks do in fact acquire information diagnostic of their client firms' financial misreporting *during* the misreporting period. While much research has examined consequences of restatement announcements, there is no empirical evidence that addresses this important research question.

Using a sample of restatement firms collected from Audit Analytics, this study identifies the misreporting period for each restatement and examines bank loans initiated during that period. To enhance the internal validity of this study, I adopt a matched-pair design and use non-restating firms as a control group for comparison against matched restating firms. Each matched pair contains two firms of comparable size and in the same two-digit SIC (standard industrial classification) industry. For the final sample of 294 matched pairs, a difference-in-difference approach is implemented in the multivariate analysis to test the association between the incidence of financial misreporting and terms of loan contracts, after controlling for other micro and macro pricing factors.

[2] This reasoning remains as a proposition and a hypothesis because there is no sure way of using secondary data to categorically know that banks do in fact acquire such knowledge.
[3] I use the terms "misreporting firms" and "restating firms" interchangeably throughout this paper as they both refer to firms that issue misreported financial statements and subsequently restate them at a later period.

The empirical results are consistent with the hypothesis that bank loans made to restating firms during the misreporting period have higher loan spreads than loans made to control firms. The increase in loan spreads ranges from 16 to 22 basis points and is both statistically and economically significant.[4] In addition, during the misreporting period, loans made to restating firms have more restrictive financial covenants and shorter maturity than loans made to non-restating borrowers. These findings are consistent with the proposition that banks acquire relevant information correlated with financial misreporting by their clients and tailor their loan terms accordingly.

To verify that changes in loan terms are not made in response to publicly available information, I conduct three additional tests. First, I test equity holders' reactions to earnings announcements during the test period. The results show no significant difference in the short-window earnings announcement returns between restating and non-restating firms during the misreporting period. This finding suggests that equity holders do not respond differently to restating firms' earnings announcements than to those of the control group, which implies that before public announcements investors are not aware of firms' ongoing financial misreporting.

Second, I examine whether financial analysts react to financial misreporting by firms they follow but do not find a significant change in analyst forecast dispersion for restating firms during the misreporting period. Third, additional analysis indicates that during the misreporting period bondholders also do not price new bonds differently for restating firms and non-restating firms. These findings consistently show that other agents who do not have access to private

[4] The results are based on controlling for other determinants of loan pricing and any existing difference in loan spreads between these two groups in the pre-misreporting period. In terms of magnitude, the estimated loan spread increase accounts for 10 percent or more of the average loan spreads of 176 basis points (or 1.76%) for the test sample.

information like banks do not make decisions consistent with having access to information about borrowing firms' ongoing financial misreporting.

This study makes three contributions. First, it enhances our understanding of the impact of events and actions preceding the public restatement announcements. In developing this understanding, I provide empirical evidence on banks' acquisition of private information related to financial reporting misconduct during the misreporting period. As a result, this paper complements the findings of other studies that focus on the consequences of restatement announcement. In particular, I show that despite the opaque nature of financial misreporting, debt holders who have access to private information act as if they acquire relevant information about the ongoing irregularities. Second, by showing that banks are capable of acquiring relevant information in such a situation, this paper provides new evidence for the finance literature regarding banks' ability to gather information from their borrowers. Finally, my findings can have practical implications as they demonstrate that informed stakeholders could bring about the economic consequences of financial misreporting for misreporting firms at an early stage of the restatement process.

The remainder of this paper proceeds as follows. Chapter 2 provides an overview of related literature and develops the hypotheses. Chapter 3 describes the research design, and Chapter 4 provides information about the sample and descriptive statistics. Chapter 5 and Chapter 6 present the main results and further analysis, respectively. Chapter 7 provides a summary discussion of the empirical results and conclusions.

CHAPTER 2

RELATED LITERATURE AND HYPOTHESIS DEVELOPMENT

Financial misreporting

Financial misreporting impairs the credibility of a firm's financial statements. It leads users to revise their beliefs about the levels and risk of firm's future cash flows because restatement is an admission of having made inaccurate financial reporting. As a result, restatement announcements often generate adverse consequences. Palmrose et al. (2004) document a 9 percent negative stock return over the two-day restatement announcement window. They also provide empirical evidence of higher analyst forecast dispersion and bid-ask spreads after the restatement announcement. Anderson and Yohn (2002) report similar findings on equity investors' adverse reactions to the restatement as well as an increase in bid-ask spreads surrounding restatement announcements. These findings show a heightened level of uncertainty about the financial information of restating firms. Similarly, Hribar and Jenkins (2004) find an increase in the cost of equity capital for firms facing accounting restatements; Shi and Zhang (2008) document negative bond market reactions to the restatement news and an increase in risk premiums for new bonds issued subsequent to restatement announcements. More recently, Kravet and Shevlin (2010) find an increase in restating firms' information risk, proxied by accrual quality, following the restatement announcements.[5]

Although prior literature mostly addresses the reaction of public capital providers to restatement announcements, two recent studies investigate the effect of financial restatements on private debt holders (i.e., banks). Graham et al. (2008) study the effect of restatements on subsequent bank loan contracting and find that bank loans that are initiated after the restatement

[5] In addition to the negative impact of restatements on capital markets, Palmrose and Scholz (2004) find a positive relationship between restatements and the incidents of litigation. Desai et al. (2006) also document that restating firms usually experience higher management turnover in the years after restatement announcements.

announcements have higher interest spreads, shorter maturities, a higher likelihood of being secured, and more covenant restrictions compared to loans initiated before the restatement announcement. Files and Gurun (2011) test the contagion effect of restatements on bank loans by examining whether restatements announced by peer firms, suppliers, or customers of a firm have an impact on the interest rates that bank lenders charge to that firm. While both studies examine the relationship between restatements and bank loans, they focus on the reaction of banks to *public* information of restatement announcements. In contrast, this study examines banks' potential acquisition of *private* information about financial misreporting by concentrating on the misreporting period rather than on the period after restatement announcements.

The misreporting period when a firm commits errors has received much less attention compared to restatement announcements, on which there is extensive evidence. One exception is Bardos et al. (2011), who examine whether investors see through misstated earnings and anticipate earnings restatements. They find that for firms that restate at least one annual report, investors were misled by mistakes in reported earnings at the time of initial earnings announcements. The authors show that investors reacted positively to the component of favorable earnings surprise that a firm subsequently restates and attached the same valuation to it as to the true earnings surprise.[6] Their findings suggest that investors are misled by mistakes in reported earnings during the misreporting period, which further motivates this study to investigate banks' ability in acquiring information about ongoing financial misreporting that is not readily observable or accessible to outside capital providers.[7]

[6] Their further analysis suggests that investors anticipate the subsequent downward restatements and start marking stock prices down three months before a restatement announcement.

[7] In additional analysis, I also examine equity investors' responses to earnings announcements during the misreporting period as a validation test. The results are consistent with Bardos et al. (2011). In another related study, Karpoff and Lou (2010) look at short sellers' behavior prior to the announcement of financial reporting misconduct (i.e., AAERs). They find that short sellers accumulate positions in those firms before the public release of news. These findings suggest that short sellers anticipate the eventual discovery of financial misreporting. Because AAERs

Banks' information gathering and processing

Bank loans are different from other types of financing because banks monitor their clients closely throughout the loan-lending process and the continuous verification of borrowers' adherence to debt covenants. Consequently, banks can obtain more information from their clients, giving them an information advantage over other capital providers. As the lending relationship evolves, banks learn more about client characteristics such that those borrowing firms are, in essence, "informationally captured" by their lending banks (Sharpe 1990).

Fama (1985) posits that banks are inside debt holders who have access to information about a borrower that is not publicly available otherwise; such access to nonpublic information distinguishes bank lending from other "arm's length" funding arrangements (Rajan 1992). The private information obtained by banks then generates lender-client specificity (Ariccia and Marquez 2004) that reduces information asymmetry between banks and borrowing firms. In summary, it is commonly accepted in the finance literature that banks are uniquely positioned to have more information about their borrowers than other capital market participants.

Financial reporting and bank loan contracting

In spite of the banks' access to private information about borrowing firms, a significant component of the information on which they rely is made public in the form of financial statements. Financial statements play a significant role because a large number of debt covenants are based on accounting numbers or financial ratios.

Prior literature provides evidence that shows how important financial reporting quality is in loan contracting. Ball et al. (2008) investigate the effect of debt-contracting value on syndicated loans. In their study, debt-contracting value is proxied by the use of publicly reported

usually represent the most egregious cases of financial misreporting, it is unclear whether these findings would hold in a general sample of restatements, as used in this study. However, the findings of both studies have implications for this paper, and further investigation may be necessary.

accounting data for the timely prediction of the deterioration in a borrower's credit quality. They document that the lead arranger holds a smaller portion of new loan deals when the borrowing firm's accounting information has a higher debt-contracting value. According to the authors, this finding implies less information asymmetry between the lead arranger and other syndicate participants. Subsequently, Minnis (2011) examines the value of financial statement verification in debt financing for private U.S. firms and finds that audited private firms have a significantly lower cost of debt and that lenders more heavily weight audited financial information when setting the interest rate of loan contracts. In another study, Demerjian (2011) investigates the effect of change in accounting standards on the use of accounting-based covenants in bank loan contracts and finds that a policy shift towards a balance-sheet approach is associated with reductions in balance-sheet covenants used in private debt contracts.

The above discussion suggests that banks have strong incentives to monitor the financial reporting quality of borrowers because it is highly relevant in assessing borrowers' performance and credit risk.

I am interested in whether the unencumbered access to information, coupled with ample time and the ability to process information, allows banks to locate signals of financial misreporting by borrowing firms at an early stage. Because information acquisition and processing by bank lenders is not directly observable, the lending decision outcome is used as evidence from which inferences can be made about the use of information regarding the financial misreporting of borrowers.

If the discovery of a reporting error increases the perception of client risk, this updated risk assessment will be reflected in the terms of bank loans initiated or renegotiated after this

finding.[8] For instance, banks may charge higher interest rates or impose more restrictive covenants on the loans to compensate for the increased risk of the borrowing firms. As a result, I anticipate that if banks acquire relevant information with respect to their clients' financial misreporting, they will use the information to set the terms of new loans. In a sense, therefore, testing hypothesis using decision outcome is a joint test of banks' *acquisition* and *use* of information.

My first test hypothesis examines the difference between interest rate spreads on the loans made to the treatment (restating) and control (non-restating) groups. These loans originated during the misreporting period, and the test controls for two main factors: (a) the loan-spread differences during the pre-misreporting period and (b) other determinants of interest rate spreads, including borrower-specific variables, loan-specific variables, and macroeconomic factors. The hypothesis stated in an alternative form is as follows:

H1: During the misreporting period, interest rate spreads on bank loans are higher for restating firms than for non-restating firms, after controlling for other determinants and pre-misreporting differences.

In addition to interest rates, the direct cost of borrowing, loan contracts include non-price terms. Lenders could use both price and non-price terms to mitigate the information problems they face when designing loan contracts (Graham et al. 2008, Kim et al. 2011). Non-price terms are also used to manage potential conflicts between lenders and borrowers (Vasvari 2008). Chava and Roberts (2008) describe the presence of loan covenants as motivated and rationalized by their ability to mitigate agency problems and to assist in obtaining financing through the

[8] Because the data on loan renegotiation is not available, this paper focuses on newly initiated bank loans.

pledge of state-contingent control rights. Prior literature on debt covenants indicates that lenders could improve *ex post* monitoring of borrowers by their judicial use of loan covenants that allow lenders to monitor the borrower's credit quality subsequent to loan initiation (Rajan and Winston 1995). It is therefore reasonable to expect loans issued to borrowers with information problems to contain more restrictive covenants. This is my second test hypothesis stated in an alternative form:

H2: During the misreporting period, more restrictive financial covenants are used in bank loans for restating firms as compared to non-restating firms, after controlling for other determinants and pre-misreporting differences.

The third main indicator of loan terms is debt maturity, which is a function of the borrower's risk level (Diamond 1991b). Lenders can adjust the loan maturity according to borrower risk and performance. By issuing short-term loans to borrowers with information problems, lenders can periodically evaluate borrower performance and maintain a stronger bargaining position through the near-term debt renewal process. Therefore, high-risk firms, which have a high probability of default, may be denied long-term loans (Graham et al. 2008). As financial misreporting heightens the perceived risk of a firm, I predict that bank loans made to restating firms have shorter maturity than loan contracts for non-restating firms during the misreporting period. This leads to my third hypothesis:

H3: During the misreporting period, bank loans initiated to restating firms have shorter maturity as compared to bank loans initiated to non-restating firms, after controlling for other determinants and pre-misreporting differences.

It should be noted, however, that banks' acquisition of information regarding borrower financial misreporting is by no means guaranteed despite their superior information access and processing capability. In fact, it is unclear, *ex ante*, whether banks are able to obtain any private information related to the ongoing misreporting of borrowing firms. If firms intentionally misreport their financial information in order to cover poor performance or to avoid debt-covenant violations, they will try to hide the information from all parties, including lenders. Therefore, banks' acquisition of relevant information with respect to the ongoing misreporting by borrowing firms is an empirical question that needs to be examined.

CHAPTER 3

RESEARCH DESIGN

The research design is structured around the timeline of financial misreporting. As illustrated in Figure 1, the period between the starting and ending dates of misreporting is defined as the "Misreporting Period" (T_1) during which firms issue misstated financial statements. This is also the test period for examining the terms of bank loans issued to restating firms. On the other hand, the "Pre-Misreporting Period" (T_0) represents the period before the inception of misreporting. Loan contracts initiated during this period are used as controls in the empirical tests. To further highlight the different focus of time period in this study from others, the "Post-Restatement Period" (T_2) is also marked on the timeline referring to the period after the public announcement of restatement.[9,10]

Table 1 outlines how this study attempts to examine the terms of bank loans during the test period. To enhance the internal validity of the test, I use a matched-pair sample design to compare selected terms of bank loans made between restating firms (X_1) and non-restating firms (C_1) during the misreporting period (T_1) and examine the difference (i.e., $X_{1,i} - C_{1,i}$, where i = loan interest rate spread, number of financial covenants, and loan maturity). Each restating firm is matched with a control firm, based on industry and firm size[11], which did not have restatement at any time. To isolate the effect of financial misreporting from other factors, a comparison is made between the terms of bank loans made to restating firms (X_0) and those made to non-

[9] Nevertheless, in unreported analysis I compare interest rate spreads between loans made to restating and non-restating firms in the post-restatement period to examine whether there is any incremental effect of restatement announcement on direct borrowing cost upon its public revelation. I do not find a significant increase in interest rate spread during this period, as compared to the misreporting period.

[10] The period between the ending date of misreporting and the restatement announcement date is the error discovery period. Because information leakage could occur during this period and the length of this period varies significantly across cases of restatement, this error discovery period is not included in the analysis.

[11] In further analysis, I add accrual quality as the third matching variable to address the possibility that banks, instead of having access to private information, simply adjust loan terms to reflect their prediction of (or concern about) potential earnings management. See Section IV for details.

restating firms (C_0) during the pre-misreporting period (T_0), and the difference (i.e., $X_{0,i} - C_{0,i}$) is used as a benchmark. This "difference-in-difference" approach essentially tests X_1-C_1 conditioning on X_0-$C_{0;}$ therefore, it provides a cleaner examination of the association between financial misreporting and the terms of bank loans than an unconditional comparison.

Given the above design, this study uses a regression analysis to empirically test the three hypotheses by examining the relationship between financial misreporting and the three variables of loan contracting (e.g., interest rate spread, number of financial covenants, and loan maturity). The following regression model implements the difference-in-difference approach to examine the effect of financial misreporting on loan interest rate spread:[12]

$$AIS = \alpha + \beta_1 RES + \beta_2 POSTMIS + \beta_3 POSTMIS\_RES + \beta_4 Borrower\_specific\ Control + \beta_5 Loan\_specific\ Control + \beta_6 Macroeconomy\_wide\ Control + \varepsilon \quad (1)$$

AIS refers interest rate spread, the direct borrowing cost of a bank loan. It is measured by the all-in spread drawn (plus the annual fee, if any) in basis points over the London Inter-Bank Offer Rate (LIBOR),[13] divided by 100.[14] *RES* is an indicator variable that equals 1 if the borrower is the firm that had restated previous financial statements; otherwise, it equals 0. *POSTMIS* is an indicator variable that equals 1 if the bank loan was originated during the misreporting period and 0 if the bank loan was originated before the inception of misreporting. The test variable of interest is the interaction term, *POSTMIS_RES*, which is the product of *RES* and *POSTMIS*. The coefficient β_3 on *POSTMIS_RES* captures the difference in loan interest spread that restating firms need to pay during the misreporting period compared to non-restating firms. Therefore, H1 implies $\alpha_3 > 0$, which suggests that banks charge higher interest rates on

[12] Firm and time denotations are suppressed in the equation for ease of presentation.
[13] All-in spread represents a composite way to report the pricing of bank loans such that a comparison can be made across multiple loans regardless of the underlying fee and spread structure.
[14] 1 basis point = 0.01%. Therefore, dividing basis points by 100 creates a measure of interest spread in percentage (e.g., 1.50 indicates 1.50%).

loans issued to restating firms compared with control firms during the misreporting period. This test is conditional on the difference in borrowing cost between these two groups of firms over the pre-misreporting period.

The analysis controls for three sets of factors that impact loan interest cost: (a) borrower-specific determinants, (b) loan-specific determinants, and (c) macroeconomic factors, following prior literature (e.g., Graham et al. 2008, Kim et al. 2011, Bharath et al. 2011).

(a) Borrower-specific determinants:

The firm-specific variables that determine a borrower's credit risk are used as control variables. *LEVERAGE* is measured as the ratio of total debt to total assets and is an indicator of financial and credit risk. Therefore, higher leverage is predicted to be associated with higher interest rate spreads. *PROFITABILITY* is measured as return on assets; more profitable firms are likely to have lower borrowing cost. The third variable is sales growth, *GROWTH*, which is expected to positively correlate with loan spreads as growing firms are often faced with high risk. Controlling for these three firm-level variables is particularly relevant in light of previous literature that examines the predictability of accounting manipulation using public financial variables (e.g., Dechow et al. 1996, Richardson et al. 2002, Dechow et al. 2011). Because this study investigates the possibility that banks have superior access to private information about financial misreporting, I attempt to control for the effect of other publicly available predicting variables on the contractual terms of bank loans.

Several other firm-level determinants of loan interest rates are also included. *SIZE* is the natural logarithm of total assets; larger firms are expected to have smaller interest rate spreads. *MTB* is the market-to-book ratio measured as market value of equity divided by the book value of equity. This variable is predicted to be negatively associated with borrowing cost to the extent

that it represents the additional value over book assets that debt holders can access in the event of default. *TANGIBILITY* is the ratio of property, plant, and equipment to total assets. Firms with a higher proportion of tangible assets are expected to have lower borrowing costs because lenders can recover those tangible assets in the event of loan default. *CFVOLATILITY,* the cash flow volatility, is measured as the standard deviation of quarterly cash flows from operations over the eight fiscal quarters prior to loan initiation, deflated by total assets. Similar to Graham et al. (2008), I use *CFVOLATILITY* as a proxy for earnings risk and expect this variable to be positively associated with the cost of debt. The Altman's Z-score, *ALTMANZ*, is also included to capture the estimated default risk and should negatively correlate with the interest rate spreads as a higher score indicates a lower default risk. Finally, the variable *RETURN*, the buy-and-hold return over 90 days before the initiation of a loan facility, is included to capture banks' potential use of information from the stock market in their lending decisions.

(b) Loan-specific determinants:

The extant literature shows that loan-specific characteristics are related to the interest cost of borrowing (Strahan 1999, Bharath et al. 2008, Graham et al. 2008, Kim et al. 2011). To control for these factors, I include the following variables in Equation (1): *LMATURITY, LLOANSIZE, LNLENDER, PPRICING, and SYNDICATION*. *LMATURITY* is the natural logarithm of loan maturity in months. Previous research finds that lenders charge lower interest rates for loans with shorter maturity (Graham et al. 2008, Bharath et al. 2008, Kim et al. 2011). Therefore, a positive relationship is predicted between *LMATURITY* and interest rate spreads. *LLOANSIZE* is the natural logarithm of each loan facility in dollars, and a negative coefficient is expected for *LLOANSIZE* as larger loan facilities are usually charged with lower interest rates. *LNLENDER* is the natural logarithm of the number of lenders (i.e., banks) in the loan deal.

PPRICING is an indicator variable equal to 1 if the loan contract contains performance pricing provisions and equal to 0 otherwise. Loan contracts that involve higher numbers of lenders and performance pricing provisions are expected to have lower interest rates. *SYNDICATION* is an indicator variable that equals 1 if the loan facility is syndicated and equals 0 otherwise. This variable captures any difference in the interest rate charged for syndicated versus non-syndicated loans.

(c) Macroeconomic factors:

I include two variables to control for the potential effects of macroeconomic conditions on loan contracting (Graham et al. 2008, Kim et al. 2011, Files and Gurun 2011). *CSPREAD*, capturing the credit spread, is the difference between the yields of BAA- and AAA-rated corporate bonds, and *TSPREAD*, the proxy for term spread, is measured as the difference in yield between ten-year and two-year U.S. Treasury bonds. Prior research suggests that credit spread and term spread are good indicators of macroeconomic conditions and that they help to explain stock and bond returns (Chen et al. 1986, Fama and French 1993, Graham et al. 2008). Because investors require more compensation for increased default risk in bad economic conditions, Collin-Dufresne et al. (2001) argue that credit spreads tend to increase in recessions and decrease in expansions. Therefore, *CSPREAD* is expected to be positively associated with loan spreads if individual loan contracts reflect the economy-wide default risk.

To examine the second hypothesis, the following empirical model, similar to Equation (1), is adopted:

$$FINCOV = \alpha + \beta_1 RES + \beta_2 POSTMIS + \beta_3 POSTMIS\_RES + \beta_4 Borrower\_specific\ Control + \beta_5 Loan\_specific\ Control + \beta_6 Macroeconomy\_wide\ Control + \varepsilon \quad (2)$$

FINCOV is the number of financial covenants in a loan contract,[15] and the other variables are the same as in Equation (1). Following Kim et al. (2011), I use the Poisson regression to test the effect of financial misreporting on the use of financial covenants in bank loans because the dependent variable here is the total number of financial covenants included in the loan agreement. The main test variable in Equation (2) is *POSTMIS_RES*, the interaction term of the restatement and misreporting period indicators. H2 predicts a positive coefficient α_3 on *POSTMIS_RES*, which suggests that banks impose more restrictive financial covenants on loans issued to restating firms during the misreporting period than on loans issued to non-restating firms. Similar to Equation (1), this model also controls for any difference in covenants imposed on loans issued to these two groups of firms in the pre-misreporting period.

In addition to the main test variables, this regression includes several borrower-specific, loan-specific, and macroeconomic factors that are potential determinants of covenant decisions. Following Bradley and Roberts (2004), I expect the use of restrictive financial covenants to be positively associated with leverage and negatively correlated with firm size and tangibility. With respect to loan characteristics, loans with longer maturity are expected to have more covenants. Finally, the credit spread, a proxy for the general economic risk, is predicted to correlate positively with the inclusion of covenants.

Next, an empirical test on the association between loan maturity and financial misreporting uses the following model:

$$LMATURITY = \alpha + \beta_1 RES + \beta_2 POSTMIS + \beta_3 POSTMIS\_RES + \beta_4 Borrower\_specific\ Control + \beta_5 Loan\_specific\ Control + \beta_6 Macroeconomy\_wide\ Control + \varepsilon \quad (3)$$

[15] Because the financial covenants are set for each package (i.e., deal) of bank loans, I select the facility with the largest loan amount in each loan package for the analysis to avoid the spurious relationship that may arise from the inclusion of multiple facilities of the same loan package.

LMATURITY is the natural logarithm of loan maturity in the bank loan contract, and the other variables are the same as in Equation (1).[16] H3 predicts a negative coefficient α_3 on *POSTMIS_RES*, suggesting that bank loans made to restating firms during the misreporting period have shorter maturities than loans given to non-restating firms after controlling for any difference in the pre-misreporting period.

[16] Except that *LMATURITY,* by construct, is no longer included as a loan-specific control variable on the right-hand side of the regression.

CHAPTER 4

SAMPLE AND DESCRIPTIVE STATISTICS

Restatement Data

The initial restatement sample is selected from the Audit Analytics database that contains information about restatements filed by all SEC registrants beginning January 1, 2000. This database includes 10,815 restatements filed by approximately 6,900 firms from 2000 to 2010. Audit Analytics uses software to search all Edgar filings, which allows more efficient identification of restatements filed without announcement in Form 8-K or in a press release, particularly restatements noted only in Form 10-K or Form 10-Q (Scholz 2008). The inclusion of these so-called "stealth" restatements is suitable for the setting in this study because the main purpose here is to examine whether banks gather any information with respect to the borrowing firms' ongoing misreporting, regardless of the form of eventual restatement disclosures.[17] For each restatement, Audit Analytics identifies the filing date of the restatement as well as the starting and ending dates of the restated period. Because this study focuses on the misreporting period, I keep only the first restatement for firms that restate their financial statements more than once in order to avoid any confounding effects.[18] This filtering process and a further exclusion of financial institutions leave 5,872 unique restatements in the sample. The sample selection procedure is presented in Table 2, Panel A.

Loan Data

Data on bank loans come from the Loan Pricing Corporation's (LPC) DealScan database, which contains detailed loan information about commercial loans made to U.S. and foreign

[17] Scholz (2008) compares different restatement databases for the overlapping periods and finds that Audit Analytics includes nearly all restatements captured in the GAO reports and Lexis-Nexis searches and some that are not identified through these methods.

[18] Such a confounding effect may arise from the fact that the misreporting period of the first restatement could overlap with the pre-misreporting period of the second restatement.

corporations. In DealScan, loan data are complied for each deal (also referred as *package*), which is a loan contract made between a borrower and lender(s) at a specific date. Each package will have only one *facility* (the basic unit of a loan) or several facilities with varying price and non-price terms. Following prior literature (Graham et al. 2008, Kim et al. 2011, Files and Gurun 2011), I consider each facility as a separate unit of loan observation because loan spreads and loan characteristics vary across facilities.

Loan data are merged with accounting data from Compustat Fundamentals Quarterly for the most recent quarter prior to the initiation of a loan facility and with return data from CRSP Daily Stock. This dataset is then combined with unique restatements described in the previous section, which creates a sample of 1,521 restating firms with data. To facilitate the inter-period comparison, I further require that each firm have data available in both misreporting and pre-misreporting periods. This reduces the sample to 379 restating firms with a total number of 2,569 loan facilities.

Selecting Matched-pair Control Sample

For the construction of a control sample, each of the remaining treatment (restating) firms is matched with a control (non-restating) firm using the same two-digit SIC industry and the smallest difference in firm size.[19] These criteria identify 294 matched non-restating firms with available data on bank loans and other variables for the test period. In total, the final sample contains 588 firms with a total number of 3,142 loan facilities spanning the period from 1989 to 2009, of which 1,694 loan facilities come from restating firms and 1,448 loan facilities from those matched non-restating firms.

[19] Total assets at the fiscal quarter ending prior to the beginning date of misreporting period are used here for the matching, and I require that total assets of a control firm fall within the range of 50%-150% of the restating firm's.

Table 2, Panel B reports the distribution of restating firms in the final sample by year. The number of unique restatements[20] increases from 2000 to 2005 and starts to decrease in 2006, a pattern consistent with what is observed in the full sample reported by Audit Analytics (Cheffers et al. 2009).

Table 2, Panel C lists the restating firms sorted by Fama-French 17 industries. Panel D of Table 2 reports the distribution of loan facilities by misreporting period. For the 294 restating firms, 1,042 loans were initiated before the misreporting period and 652 loans during the misreporting period. For non-restating firms, 906 loans were initiated before the misreporting period and 542 loans during the misreporting period.

Descriptive Statistics

Table 3 reports the descriptive statistics for the full sample. Panel A presents the descriptive statistics of the loan-specific variables, while Panel B those of the test variables and the borrower-specific variables considered in this study. The mean and median drawn all-in spreads over LIBOR (i.e., AIS) are approximately 1.76% and 1.50%, respectively, with a standard deviation of approximately 1.36%. The mean (median) loan maturity is approximately 46 (48) months, while the mean (median) loan facility size is $287 ($145) million. On average, 51.9 percent of the loan facilities have a performance pricing provision. Most of the loan facilities are syndicated loans with, on average, nine lenders.

As shown in Panel B of Table 3, restating firms comprise approximately 53.9 percent of the loan facilities. Approximately 38 percent of loans were initiated during the misreporting period, while 62 percent were issued before the misreporting. For firm-specific variables, *SIZE* has a mean (median) of 6.80 (6.76) and market-to-book ratio (*MTB*) has a mean (median) of 3.28

[20] Because of the research design, each restating firm has a unique restatement. Thus, the by-year distribution of restating firms corresponds to that of unique restatements.

(2.17). In addition, total debt, EBITDA, and tangible assets on average account for 31 percent, 3.4 percent, and 33 percent of total assets, respectively.

Univariate Comparisons

The full sample is partitioned into the misreporting period and pre-misreporting period. For each subsample, loan features and borrower characteristics are compared between (1) borrowers engaged in financial misreporting (i.e., restating firms) and (2) borrowers not engaged in financial misreporting (i.e., control firms). Table 4, Panel A presents the mean and median of loan features and borrower-specific characteristics of these two subsamples for the misreporting period, while Panel B of Table 4 reports the statistics for the same characteristics of the two subsamples for the pre-misreporting period. As shown in Table 4, Panel A, the mean (median) *AIS* is approximately 2.07% (1.75%) for restating firms and 1.77% (1.50%) for non-restating firms. The mean and median differences are both significant at 1%, suggesting that lenders charge higher interest rate spreads to restating firms as compared to non-restating firms during the misreporting period.

In terms of loan maturity, the mean (median) maturity is approximately 49 (48) months for loans to restating firms in the misreporting period, and the mean (median) maturity for loans to non-restating firms is approximately 45 (48) months. Other loan features have no significant differences between loans awarded to test and control firms during the misreporting period. However, restating firms on average have larger size, higher leverage, higher default risk, lower cash flow volatility, and a higher proportion of tangible assets.

As shown in Table 4, Panel B, the mean (median) *AIS* for loans to restating firms is approximately 1.72% (1.50%) in the pre-misreporting period. For loans made to non-restating firms before the misreporting period, the mean and median *AIS* are 1.58% and 1.25%,

respectively. While the mean difference in *AIS* between restating and non-restating firms is also significant at 5%, the magnitude of the difference during the pre-misreporting period (0.14%) is less than one half of that during the misreporting period (0.30%).

As to the comparison of loan maturity, the mean (median) loan maturity of loans to restating firms is 48 (50) months, which is significantly higher than the maturity of loans to non-restating firms (mean [median] is 42 [36]) during the pre-misreporting period.

Regarding other borrower-specific characteristics, restating firms have higher leverage, higher growth, and higher likelihood of default than non-restating firms. This finding is consistent with prior literature that suggests that firms may have incentives to misreport their financial results, which include maintaining high growth and avoiding default or covenant violations.

Correlation Matrix

Table 5 presents the Pearson correlation coefficients between selected loan and borrower variables. The loan spread, *AIS*, is positively correlated with *POSTMIS_RES*, the main variable of interest, at 1% (correlation coefficient = 0.116), suggesting that banks charge higher interest rates to restating firms during the misreporting period than to non-restating borrowers. Correlations between borrower-specific variables and *AIS* generally behave in the predicted direction. For example, *AIS* is negatively correlated with *SIZE*, *PROFITABILITY*, and *ALTMANZ*, but it is positively correlated with *LEVERAGE*, *GROWTH*, *CFVOLATILITY*, and *RETURN*. With regard to other loan features, *AIS* is positively correlated with loan maturity (*LMATURITY*) but negatively correlated with loan size (*LLOANSIZE*), provision of performance pricing (*PPRICING*), and number of lenders (*LNLENDER*).

CHAPTER 5

REGRESSION RESULTS

Misreporting and Loan Spread: Test of H1

Table 6 reports the results of estimating the regression model, Equation (1), using *AIS* as the dependent variable. In Column 1 of Table 6, *AIS* is regressed on the main test variable, *POSTMIS_RES*, and the two lower-level variables, *RES* and *POSTMIS* (including year and industry fixed effects).[21] The control variables are added incrementally to this basic regression model, and the estimated results are reported in Column 2 through Column 5. All of these models include controlling for various combinations of fixed effects. Column 2 adds borrower-specific control variables; Column 3 controls for loan-specific variables and fixed effects of loan purpose and loan type. Column 4 includes macroeconomic variables, *CSPREAD* and *TSPREAD*, and Column 5 has the full model with adjustment of the standard errors for firm- and year-level clustering.

All results in Table 6 show a statistically significant positive coefficient on *POSTMIS_RES*, consistent with the prediction of H1. The level of significance varies by the scope of explanatory variables. The coefficient is significantly positive at 10% in Column 1 (t=1.94), and similar results hold as I incrementally add various sets of control variables to the regression model. As reported in Columns 2 and 3, the coefficients on *POSTMIS_RES* continue to be significantly positive after borrower-specific and loan-specific variables are controlled for (t-statistic equals 2.63 [2.19] in Column 2 [3], significant at 1% [5%]). Finally, the addition of macroeconomic factors as controls, shown in Columns 4 and 5 of Table 6, does not change the main effect of misreporting on bank loan spread (t = 2.16 [1.83] in Column 4 [5]). The adjusted

[21] I use the Fama-French 17 industry definition to construct industry dummy variables. The results remain the same if the two-digit SIC code is used instead.

R-square is approximately 52 percent in Column 5, a level that is comparable to those of prior studies on loan contracting (e.g., Graham et al. 2008, Kim et al. 2011, Files and Gurun 2011).[22]

The estimated coefficients on the control variables, as shown in Table 6, are generally consistent with predictions, which provide further validity of the results. Loan spread is negatively associated with profitability, borrower size, loan facility size, and the presence of performance pricing provisions. Additionally, loan spread is positively associated with leverage, growth, default risk, and cash flow volatility. While not the subject of a test hypothesis, loan spread is also positively associated with stock return; this suggests that banks may use the stock performance of borrowers as a proxy for potential investment opportunities and thus charge higher interest rate spreads when expecting higher growth. As for macroeconomic variables, loan spread is negatively associated with credit spread while the coefficient is not significant.[23]

The above finding shows that banks charge higher interest rates for loans issued to restating firms during the misreporting period than for those issued to non-restating firms. This result obtains after controlling for any existing difference between the borrowing costs of these two groups of firms in the pre-misreporting period and other determinants of loan interest rate spread. Restating firms, on average, pay 0.16% to 0.22% more for the interest on bank loans during the misreporting period than non-restating borrowers. This suggests that the increase in bank loan spread due to financial misreporting is both statistically and economically significant.[24]

Misreporting and Financial Covenants: Test of H2

[22] The adjusted R-square also increases from 7 percent in Column 1 to 52 percent in Column 5, suggesting improvement in the model specification.

[23] In an untabulated analysis, I find a significantly positive coefficient on credit spread when the year dummies are not included in the regression model, suggesting that the effect of credit spread could be subsumed by the inclusion of time indicators. This is reasonable given that time indicators, to some extent, also capture the macroeconomic conditions.

[24] The average interest spread for the full sample is 1.76% (see Table 3), thus an increase of 0.16%–0.22% represents about, or more than, 10 percent of the borrowing cost.

To assess the impact of financial misreporting on lenders' use of restrictive financial covenants, Equation (2) is estimated with *FINCOV* (the number of financial covenants included in each loan package) as the dependent variable in the context of Poisson regression, following prior literature (Graham et al. 2008, Kim et al. 2011). Because financial covenants are set at the package (or deal) level, I select the loan facility with the largest amount of borrowings from each loan package.[25] As a result, the number of observations is smaller than that in the previous test of loan interest spread. Table 7 reports the results from five specifications of the regression model (Columns 1–5), similar to Table 6. The goodness-of-fit chi-squared statistic in every specification has a *p*-value near 1.00; this finding indicates that Poisson regression is suitable because the null hypothesis that the dependent variable is Poisson distributed cannot be rejected.

In Table 7, Column 1, the number of financial covenants is regressed on *RES*, *POSTMIS*, and the interaction term, *POSTMIS_RES,* without controlling for other determinants of loan covenants. The estimated coefficient on *POSTMIS_RES* is positive and significant at 10%. The coefficient remains positive and statistically significant ($p < 0.05$ in Columns 2–5) as I incrementally add different sets of control variables to the regression model. This indicates that the effect of misreporting on the increase in the number of restrictive financial covenants in bank loans holds after controlling for other known determinants of the use of financial covenants. This result is also robust to any omitted industry- or time-specific characteristics that could influence the use of financial covenants in loan contracts.

With respect to control variables, the results suggest that lenders tend to impose fewer financial covenants on larger borrowers, larger loans, loans with more lenders, and borrowers with a higher percentage of tangible assets. On the other hand, the borrower's leverage is positively associated with the intensity of financial covenants in the loan contract. These results

[25] See footnote 4.

are generally consistent with those of prior studies (Bradley and Roberts 2004, Graham et al. 2008, Kim et al. 2011).[26]

The H2 test results suggest that lenders write more restrictive financial covenants on loans to restating borrowers than on loans to non-restating clients during the misreporting period.

Misreporting and Loan Maturity: Test of H3

To assess the impact of financial misreporting on loan maturity, I estimate Equation (3), with *LMATURITY* (the natural logarithm of maturity) as the dependent variable, and incrementally add various control variables to the regression specification. As reported in in Column 3 of Table 8, the coefficient on *POSTMIS_RES* becomes significantly negative at 5% (*t* = -2.46) after controlling for the loan-specific variables and fixed effects of loan purpose and loan type. The same results hold after I include macroeconomic variables in Columns 4 and 5.[27]

This finding, consistent with the prediction of H3, indicates that loans initiated to restating firms during the misreporting period are associated with a shorter maturity than loans made to non-restating borrowers. Similar to Graham et al. (2008), loan maturity is positively associated with other control variables such as borrower's profitability, size of the loan facility, and provision of performance pricing. In addition, loans with more lenders tend to have longer maturity.

In summary, the results obtained from all tests confirm the three hypotheses. Consistent with H1, the findings show that banks charge higher interest rate spreads for loans issued to restating firms during the misreporting period than loans made to non-restating firms during the same period. As H2 predicts, banks also impose more restrictive financial covenants on loans

[26] The adjusted R-square of the full regression model (Table 6, Column 5) is equal to 0.04, similar to that reported by Graham et al. (2008).

[27] With the adjusted R-square of approximately 70 percent in Column 5, as compared to only 9 percent in Column 3, the full model has significantly improved the model specification.

made to restating firms as compared to loans made to control firms during the misreporting period. Finally, the maturity of loans issued to restating firms during the misreporting period is shorter than that of loans issued to non-restating firms after controlling for other explanatory variables; these results directly support the prediction of H3. Taken together, these findings suggest that banks acquire information about ongoing financial misreporting by borrowers, and such information influences their lending decisions in the price and nonprice contractual terms during the misreporting period.

CHAPTER 6

FURTHER ANALYSIS

Earnings Announcement Returns and Financial Misreporting

The cross-sectional test results confirm the hypothesis that banks are aware of ongoing misreporting by borrowers and tailor price as well as non-price terms of newly issued loans to reflect the added risks and information problems. Banks appear to process borrower financial misreporting before the restatement announcement.

If the information to which banks have access pertains to private information, it would be relevant to show that external users without access to the same information do not reflect that information in their decisions. To conduct this validation test, I first examine the impact of earnings announcements by misreporting firms on equity prices. Specifically, this analysis compares market returns around earnings announcements between restating firms and non-restating firms during the misreporting period. A negative effect of financial misreporting on short-window returns around the earnings announcement period would indicate that investors are able to detect ongoing financial reporting misconduct. On the contrary, if external investors remain unaware of ongoing financial misreporting, there would be no significant difference in earnings announcement returns between two groups of firms.

I run a regression model similar to that of Burks (2011) for the empirical analysis:

$$CRET = \alpha + \beta_1 RES + \beta_2 POSTMIS + \beta_3 POSTMIS\_RES + \beta_4 SIZE + \beta_5 MBE + \beta_6 ESURPRISE + \beta_7 VOLATILITY\_INDEX + \varepsilon \quad (4)$$

CRET is the size-adjusted, buy-and-hold stock return of a firm *i* measured over the three-day window (-1, +1) around the quarterly earnings announcement. *RES* is an indicator variable that equals 1 if the earnings announcement is made by a firm that has subsequently restated financial statements and 0 otherwise. *POSTMIS* is an indicator variable that equals 1 if the

earnings announcement is made during the misreporting period and 0 if made in the pre-misreporting period. Definitions of misreporting and pre-misreporting period are the same as previously described. The main test variable is *POSTMIS_RES*, the interaction term of *RES* and *POSTMIS*. Observing a negative coefficient β_3 on *POSTMIS_RES* implies that investors have detected the problem during the misreporting period. Contrarily, if investors are not aware of ongoing misreporting, β_3 will not be different from zero.

In this model, I also include several control variables following prior literature. *SIZE* is the firm size measured as natural logarithm of total assets. *MBE*, which captures whether the reported earnings meet or beat the analyst forecast, is an indicator variable equal to 1 if the actual earnings are equal to or higher than the most recent mean analyst forecast and equal to 0 otherwise. *ESURPRISE*, a proxy for earnings surprise, is measured as the difference between the actual earnings and the most recent mean analyst forecast, scaled by stock price. Finally, *VOLATILITY_INDEX* is the Chicago Board Options Exchange's volatility index on the day before the earnings announcement.

To implement this analysis, additional data need to be collected for several variables mentioned above. Analyst forecast data are obtained from IBES Summary Statistics, and data on volatility index come from CBOE Indexes. Out of the 294 matched pairs of firms in the loan test sample, the required data are complete for 246 pairs. In total, there are 17,259 firm quarter observations used in this regression analysis with the earnings announcement period ranging from 1989 to 2010.

Table 9 reports the results of this estimation. As shown in Table 9, Column 1, the coefficient on *POSTMIS_RES* is not significantly different from zero (β_3= -0.003 and *t* = -0.871); this suggests that equity prices of test and control firms do not respond differently to

earnings announcements during the misreporting period. The result does not change when the length of misreporting period is aligned with that of pre-misreporting period, which reduces the sample size to 8,216 observations. β_3 remains not significantly different from zero, as reported in Column 2 of Table 9. This lends further evidence that, unlike bank lenders, equity holders are unaware of ongoing financial reporting misconduct during the misreporting period.

Analyst Forecast Dispersion and Financial Misreporting

The first validation test yields results consistent with banks acquiring and using information that is not available to equity investors during the misreporting period. To further examine the validity of the private information argument, I consider the issue of whether analysts are aware of ongoing financial misreporting by firms they follow. This is relevant because financial analysts play an important role as information intermediary to investors and it is possible that analysts use in their forecasts some information that investors fail to incorporate as quickly in equity pricing. If analysts do acquire information about ongoing misreporting, their use of such information would be reflected in forecast dispersion due to enhanced information uncertainty associated with the revelation of poor financial reporting quality (Palmrose et al. 2004).

The empirical model for this analysis is as follows:

$$DISP = \alpha + \beta_1 RES + \beta_2 POSTMIS + \beta_3 POSTMIS\_RES + \beta_4 ROA + \beta_5 SIZE + \beta_6 MTB + \beta_7 GROWTH + \varepsilon \quad (5)$$

DISP is analyst forecast dispersion measured as the standard deviation of one-quarter-ahead earnings forecast, scaled by prior quarter-end stock price. *RES, POSTMIS*, and *POSTMIS_RES* are the same variables as previously defined. Similar to previous tests, *POSTMIS_RES* is the main variable of interest and I expect to observe a significantly positive

(insignificant) β_3 if analysts (do not) acquire and use information about ongoing reporting irregularities during the misreporting period. This model also includes several other control variables as discussed in the prior literature (e.g., Bhushan 1989; Lang and Lundholm 1996): firm size (*SIZE*), return on assets (*ROA*), market-to-book ratio (*MTB*), and sales growth (*GROWTH*).

Data required for this analysis come from same sources discussed in previous sections. The final test sample contains 203 pairs of firms and a total number of 32,972 observations of analyst forecast dispersion in one-quarter-ahead earnings forecast from 1984 through 2010.

As shown in Table 10, Column 1, the coefficient on *POSTMIS_RES* is not significantly different from zero. This indicates that the level of information uncertainty perceived by analysts does not change because of ongoing misreporting by firms they follow. Same result is obtained, as reported in Table 10, Column 2, when I further align the length of pre-misreporting period and that of misreporting period. This finding suggests that analysts do not appear to be aware of firms' ongoing financial misreporting, which lends further evidence consistent with the private information argument about the response of bank lenders to financial misreporting by borrowers.

Bond Interest Spread and Financial Misreporting

While the results presented above are aligned with the notion that signals of ongoing misreporting are not available to equity investors or analysts, they provide no direct evidence to an alternative explanation that these signals could be available to debtholders only. Because of their asymmetric payoff function, debtholders are more concerned about downside risk than upside potential of a firm (Watts 2003). This could lead debtholders, as compared to equity investors, to pay closer attention to negative signals about a firm. As a result, information acquired by banks may not be available to shareholders or even financial analysts. To test this

alternative hypothesis, I examine the response of bondholders to new bonds issued by restating firms during the misreporting period. If bondholders also access and use the same signals acquired by banks about ongoing misreporting, then those signals are more likely to be public than private ones. On the other hand, the signals pertain to private information if they are not available to either equity investors or public debtholders.

To conduct the analysis, I compare interest spreads on new bonds issued by restating and non-restating firms during the misreporting period. Observing significant (no significant) differences in bond interest spreads for these two groups would suggest that the information is available (not available) to bondholders and thus support the alternative (private information) hypothesis.

The empirical model for the test of bond interest spreads is as follows:

$$BONDSPREAD = \alpha + \beta_1 RES + \beta_2 POSTMIS + \beta_3 POSTMIS\_RES + \beta_4 LEVERAGE + \beta_5 SIZE + \beta_6 PROFITABILITY + \beta_7 MTB + \beta_8 GROWTH + \beta_9 ALTMANZ + \beta_{10} TANGIBILITY + \beta_{11} BONDMATURITY + \beta_{12} BONDSIZE + \beta_{13} SECURED + \varepsilon \quad (6)$$

BONDSPREAD is the interest spread in basis points on the public bond over the interest rate on a treasury of similar maturity, divided by 100. *RES*, *POSTMIS*, and *POSTMIS_RES* are the same variables as previously defined. The main variable of interest here is the interaction term, *POSTMIS_RES*, which captures the difference-in-difference effect of financial misreporting on bond interest spreads. A positive and significant β_3 would suggest that bondholders acquire and use signals about the borrowing firm's financial misreporting in bond pricing. Contrarily, an insignificant β_3 indicates that bondholders do not price public debts differently between bonds issued by restating firms and non-restating firms during the misreporting period. *BONDMATURITY* is the natural logarithm of bond maturity measured in

months. *BONDSIZE* is the natural logarithm of bond amount. *SECURED* is an indicator variable that equals one if the bond is secured with collateral and zero otherwise. Other firm-specific variables are the same as defined in Section III. This empirical model follows Bharath et al. (2008) and Shi and Zhang (2008), with some modifications.

Bond data are collected from the Fixed Income Securities Database (FISD); other variables come from databases discussed in previous sections. The final test sample contains a total number of 892 new bond issues offered from 1981 to 2006 for 144 pairs of treatment and control firms.

Table 11 reports the estimation results, and the estimated coefficients on *POSTMIS_RES* are insignificant in both model specifications with and without bond-specific control variables (*t* statistics are -0.301 and -0.356, respectively). This suggests that bondholders, unlike banks, do not possess or use signals about financial reporting misconduct by borrowing firms during the misreporting period, a finding in line with banks acquiring private information related to the borrower's ongoing financial misreporting.

Audit Fees and Financial Misreporting

The above analyses yield consistent results indicating banks' acquisition of non-public signals related to the borrower's ongoing misreporting. An indirect yet interesting question is whether auditors are also picking up some signals along the misreporting process. With their expertise in auditing financial statements, auditors are expected to detect the occurrence of financial misreporting in a timely manner. To shed some light on this question, I conduct an additional analysis and compare audit fees between restating and non-restating firms during the misreporting period using the difference-in-difference approach, as in previous tests.

The empirical model is as follows:

$$LAF = \alpha + \beta_1 RES + \beta_2 POSTMIS + \beta_3 POSTMIS\_RES + \beta_4 SIZE + \beta_5 LSEG + \beta_6 CATA + \beta_7 QUICK + \beta_8 DE + \beta_9 ROI + \beta_{10} YE + \beta_{11} OPINION + \beta_{12} BIG4 + \varepsilon \quad (7)$$

LAF is the natural logarithm of audit fees. *RES*, *POSTMIS*, and *POSTMIS_RES* are the same as defined in previous sections. *POSTMIS_RES* is the main variable of interest as it captures the difference-in-difference effect of financial misreporting on audit fees. A positive and significant β_3 would suggest that auditors charge higher fees for restating firms relative to non-restating firms during the misreporting period. On the contrary, an insignificant β_3 would suggest that auditors do not price audit services differently between restating firms and non-restating firms during this period.

The selection of control variables follows prior literature on audit fees (e.g., Francis et al. 2005). *SIZE* is the natural logarithm of total assets. *LSEG* is the natural logarithm of the number of business segments. *CATA* is the current ratio measured as current assets divided by total assets. *QUICK* is the quick ratio calculated as quick assets divided by total assets. *DE* is the debt-to-equity ratio measured as long-term debt divided by shareholders' equity. *ROI* is earnings before interest and tax deflated by total assets. *YE* is an indicator variable that equals 1 if the fiscal year end is not December and 0 otherwise. *OPINION* is an indicator variable that equals 1 if the firm receives a non-standard audit opinion. *BIG4* is an indicator variable that equals 1 if the auditor is one of the big 4 audit firms.

Audit fee data come from Audit Analytics; other variables are from databases discussed in early sections. The final sample contains 1,843 firm-year observations for the period from 2000 to 2009.

As reported in Table 12, the estimated coefficient on *POSTMIS_RES* is positive but insignificant (β_3 = 0.073 and t = 1.173), suggesting that auditors do not seem to possess

information about auditees' ongoing financial misreporting[28]. While this result is somewhat surprising, there are two possible explanations for it. First, it is likely that auditors access client information through different channels from banks, which contributes to their acquisition of different information sets. Second, banks and auditors could have different materiality thresholds in their judgment and decision-making. This could explain why banks and auditors may respond to same signals differently (if both parties possess the same information).

Characteristics of Eventual Restatement and Loan Pricing during the Misreporting Period

The results that have been discussed so far indicate that banks respond to financial misreporting by borrowing firms at an early stage that precedes the eventual announcement of restatement. One related issue is whether banks are aware of the financial misreporting in a "general sense" (i.e., the lender observes some signals about the borrower's information problems in financial reporting but does not have access to specific details) or in a "specific manner" (i.e., the lender has access to some information with specific details about ongoing financial misreporting).

To examine this question, I use *ex post* information about the reasons of restatement and partition the sample into two categories: core and non-core restatements. More specifically, restatements associated with revenue-recognition or expense-recording issues are categorized as core restatements because these restatements usually affect components of pre-tax operating income. Restatements associated with other issues are categorized as non-core restatements.[29]

As prior studies suggest, core restatements are viewed as more severe to financial statement users than non-core restatements (Palmrose et al. 2004, Scholz 2008, Burks 2011).

[28] In an untabulated analysis, I use change in audit fees as the dependent variable and get similar results.
[29] A restatement usually involves multiple issues (Scholz, 2008). On average, each restatement in my sample is associated with about three different issues. To be inclusive, I categorize the restatement as a core-restatement as long as revenue-recognition or expense recording is one of the issues associated with that restatement. However, this procedure is biased against finding results if a restatement includes both core and non-core issues.

Therefore, I test whether banks respond to these two subsamples differently during the misreporting period, which can shed light on the extent to which banks are aware of the financial misreporting by their clients.

For this analysis, Equation (1) is estimated for the two sub-samples of core and non-core restatements. As reported in Table 13, the coefficients on *POSTMIS_RES* in both sub-samples are significantly positive, which is consistent with previous results that banks charge higher interest rate spreads for restating firms as compared to control firms. In terms the estimated coefficient's magnitude, the coefficient on *POSTMIS_RES* in the core restatement sub-sample ($\beta_3 = 0.246$) is slightly larger than the coefficient on *POSTMIS_RES* in the non-core restatement sub-sample ($\beta_3 = 0.200$). However, the difference between these two estimated coefficients is not statistically significant ($p = 0.84$). Therefore, there is insufficient empirical evidence to make inferences on the level of information to which banks have access with respect to borrowers' ongoing financial misreporting.

Accrual Quality-based Matching

As discussed in previous sections about research design and sample selection, each restating firm is matched to a non-restating control firm of similar size in the same two-digit SIC industry. While this matching procedure follows prior literature on restatements (Graham et al. 2008, Desai et al. 2006), the use of an additional matching variable needs to be considered in light of an alternative explanation that banks could in fact use accrual quality, which is publicly available information, as a signal of potential misreporting and adjust loan contractual terms accordingly.

To address this issue, I add an accrual-based variable as the third matching criterion. Under this additional requirement, matched pairs of restating and non-restating firms will have a

similar level of accrual quality in addition to the matched size and industry. If significant differences in loan pricing for this new matched sample continue to exist during the misreporting period, I can attribute the results to banks using private information rather than reacting to public signals such as accrual quality.

For this analysis, I use working capital accruals as the accrual measure to construct a new match sample. This particular accrual measure is adopted for two reasons. First, Dechow et al. (2011) use working capital accruals as one of their accrual quality measures and find significant differences in this variable between misstatement and non-misstatement firm years. Second, this measure is not subject to the same degree of estimation error commonly associated with other discretionary accrual measures. According to Allen et al. (2009) and Dechow et al. (2011), working capital accruals are calculated as follows:

WACC = Change in current assets (ACTQ) – change in cash (CHEQ) – change in current liabilities (LCTQ) + change in debt in current liabilities (DLCQ) + change in income taxes payable (TXPQ), scaled by average total assets.

After the variable is constructed, each restating firm is matched to a control firm in the same two-digit SIC industry with the smallest difference in firm size and working capital accruals at the fiscal quarter end prior to the beginning of misreporting period.[30]

This matching procedure creates a new sample of 2,093 loan facilities for 248 matched pairs of firms, and Table 14, Column 1 presents test results on the association between loan pricing and misreporting (i.e., Equation [1]) using this sample. The estimated coefficient on

[30] Because there are two continuous matching variables (i.e., firm size and accruals), I follow the method in Huang and Stoll (1996) and Desai et al. (2006) to create a deviation score for each potential matching firm. The deviation score is calculated as follows:

Deviation Score = $[\frac{AT^t - AT^m}{(AT^t + AT^m)/2}]^2 + [\frac{WACC^t - WACC^m}{(WACC^t + WACC^m)/2}]^2$

where t and m represent treatment and matching firm values. For each treatment firm, I then sort the potential matching firms by the deviation score and keep the best match as the control firm.

POSTMIS_RES is positive (β_3=0.198) and significant at 5% (t = 2.054). As shown in Table 14, Column 2, same results hold for a modification of the variable construction, which uses the accrual measure at the quarter end *after* the inception of misreporting as the matching variable.[31] These findings suggest that banks respond to signals beyond what is contained in publicly available information such as accrual quality; this further supports the argument that banks appear to acquire and use private information about ongoing financial misreporting by borrowers in making lending decisions.

The Effect of Continuous Lending Relationships

When banks develop close relationships with borrowers over time, the proximity between the bank and the borrower should facilitate monitoring and screening and reduce the information asymmetry problem (Boot 2000). Prior transactions would have allowed lenders to gather proprietary inside information about borrowers (Bharath et al. 2008). Accordingly, if banks have access to private information about their clients, this capacity is likely to increase as the lending relationship between banks and borrowers continues.

To investigate whether past lending relationships play a significant role in banks' acquisition and use of information related to financial misreporting, I segregate bank loans made to restating firms into relationship loans and non-relationship loans and examine the association between lending relationships and loan pricing during the misreporting period. Following Dahya et al. (2003) and Bharath et al. (2008), I identify the lead bank(s) on each loan facility and classify a bank loan as a relationship loan if the borrower has another bank loan issued by the same lead bank(s) during the past five years.[32] Otherwise, a loan facility is classified as a non-

[31] This alternative matching is used to match firms based on their accrual quality *during* the misreporting period.
[32] As in Bharath et al. (2008), I classify a bank as a lead bank in a loan if it (1) is accorded the role of lead arranger credit or (2) is accorded one of the following four roles and retains a significant share of the loan (> 25%): agent,

relationship loan.

With the identification of relationship loans, the following empirical model is employed:

$$AIS = \alpha + \beta_1 REL + \beta_2 POSTMIS + \beta_3 POSTMIS\_REL + \beta_4 Borrower\_specific\ Control + \beta_5 Loan\_specific\ Control + \beta_6 Macroeconomy\_wide\ Control + \varepsilon \quad (8)$$

REL is an indicator variable that equals 1 if the loan facility is a relationship loan and 0 if it is a non-relationship loan. *POSTMIS_REL*, the interaction term of *POSTMIS* and *REL*, is the main test variable as it captures the difference-in-difference effect of past lending relationships on loan pricing during the misreporting period. All other variables are the same as previously defined.

Table 15 reports the estimation results. Column 1 shows the results using the standard size- and industry-matched sample, and Column 2 presents the results using the size-, industry-, and accrual quality-matched sample. The significant and positive β_3 in both regressions suggest that banks having past lending relationships with restating firms charge higher interest rates on loans issued to those firms during the misreporting period as compared to lenders with no prior relationships with restating firms. This finding implies that lending relationships seem to facilitate banks' acquisition of private information related to financial misreporting, which is consistent with the argument that relationship lending mitigates the information asymmetry between banks and borrowers.

Other Robustness Tests

To further validate the main results of this study, several additional tests are conducted to check robustness. First, a system of equations is estimated using the seemingly unrelated regression (SUR) procedure to address the concern that price and nonprice terms of loan

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administrative agent, arranger, lead bank. Because loan prices are compared between pre-misreporting and misreporting period, I also require that the start date of misreporting be within the five-year horizon.

contracts, to some extent, could be jointly determined during the loan process.[33] As reported in Table 16, the estimated coefficients on *POSTMIS_RES* from all three models are statistically significant in directions consistent with testing hypotheses. Second, untabulated results reveal that the effect of misreporting on loan interest spread is not affected by the exclusion of two potential endogenous variables, loan facility size and loan maturity, from the regression models.

Third, the loan-pricing regression is re-estimated at the deal level by selecting the largest loan facility in each loan package. Results (unreported) obtained from this deal-level estimation are similar to those of the facility-level regressions. Finally, same results hold for the loan-pricing test if the length of the pre-misreporting period and the misreporting period are aligned (i.e., it is required that loans be issued within two years prior to the inception of misreporting if the financial misreporting spans two years).

[33] As Kim et al. (2011) point out, however, prior literature on syndicated loans suggests that nonprice terms of loans are usually determined before the settlement of loan interest rate during the process (e.g., Dennis and Mullineaux 2000, Bharath et al. 2009).

CHAPTER 7

DISCUSSION AND CONCLUSIONS

This study investigates whether banks, as inside debt holders, are able to acquire and process information about ongoing financial reporting misconduct by borrowing firms and reflect this information in their lending decisions during the misreporting period. For this analysis, the misreporting period refers to the pre-disclosure period when the errors were made. For a sample of 3,142 loan facilities initiated in both the pre-misreporting and misreporting periods, the price and nonprice terms of loan contracts are compared between restating firms and non-restating firms. Three sets of control variables are used: loan-specific, borrower-specific, and economy-wide factors, which are known determinants of contractual terms in bank loans.

The results show that loans issued to restating firms during the misreporting period are associated with a higher interest cost than loans made to non-restating firms during the same period. These results are obtained after controlling for other determinants of borrowing cost. With respect to nonprice terms of the loans, banks impose more restrictive financial covenants on loans issued to restating firms than on those made to non-restating firms during the misreporting period. Loans issued to restating firms during the misreporting period also have shorter maturities as compared to loans made to non-restating firms. These findings suggest that banks appear to acquire signals about ongoing financial misreporting by borrowers and tailor terms of new loan contracts accordingly to reflect the added risk(s) caused by such information problems.

To validate the main test results, several additional analyses are conducted. These analyses show that (1) there is no significant difference in equity investors' response to earnings announcements between restating and non-restating firms during the misreporting period, (2) there is no significant change in analyst forecast dispersion during the misreporting period, and

(3) other public debtholders, i.e., bondholders, also do not price new bond issues differently for restating and non-restating firms. Together, these findings suggest that the signals banks acquire about borrowers' ongoing financial misreporting are not available to either equity-market participants or public debtholders during the misreporting period. Overall, the results of this study indicate that banks appear to be aware of and responsive to financial reporting misconduct by borrowers in a timelier manner than investors, equity analysts, and bondholders. These results are also robust to different model specifications, additional matching requirements, and several other robustness checks.

This paper adds to the literature on restatements by examining banks' acquisition and use of private signals about ongoing misreporting by borrowers. The use of information by bank lenders in setting terms of new loan contracts during the misreporting period extends our understanding of the impact of events preceding restatement announcements. This study also complements finance literature on banks' superior access to borrower information by providing evidence on the ability of banks in acquiring and processing information in a unique setting. Financial misreporting provides an interesting setting because borrowers have incentives to hide information from their lenders when issuing misstated financial statements. Furthermore, a practical implication offered by this study is that restating firms could incur the cost of financial misreporting well before they announce the restatement publicly.

There are several limitations of this study. First, this paper uses *restated* accounting numbers in the analysis because originally reported numbers are not available from the data sources. The discrepancy between restated numbers and original values might affect estimation results because of measurement errors in some explanatory variables.[34] Second, the earnings

[34] However, it is unknown how the difference in restated and original numbers would affect my results. Besides, if banks are aware of ongoing financial misreporting, it is reasonable to assume that they will also adjust their use of

impact of financial misreporting in each period is not available. Third, this paper uses a restatement sample from Audit Analytics for the period 2000 to 2010, even though other sources of restatement information (e.g., GAO reports) exist. Therefore, further replication is desired in order to generalize the empirical results documented here to other restatement samples or to other periods. Lastly, there could be other factors, in addition to what this study has attempted to control for, that lead to the differences between bank lenders' and other capital providers' response to ongoing misreporting by a firm. For example, Hirst et al. (2003) suggest that investors have prior expectations about opportunistic reporting of a firm and these expectations are priced before the confirmation of opportunistic reporting. As a result, equity investors may also receive information about ongoing misreporting but do not respond simply because they have already factored the "expected misreporting" into their decisions. To my knowledge, there is no empirical evidence on prior knowledge of banks about borrowers' opportunistic reporting. This topic would present a good opportunity for future research.

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reported accounting numbers in loan contracting. Therefore, my conjecture is that the results should not be affected if the original numbers are used in the analysis.

FIGURES AND TABLES

Figure 1 Timeline of Financial Misreporting

This figure illustrates the timeline around financial misreporting. The period between starting date and ending date of misreporting is defined as the "Misreporting Period" (T_1) during which firms issue misstated financial statements. The period prior to the beginning of misreporting is the "Pre-Misreporting Period" (T_0), while the period after the public announcement of restatement is defined as the "Post-Restatement Period" (T_2).

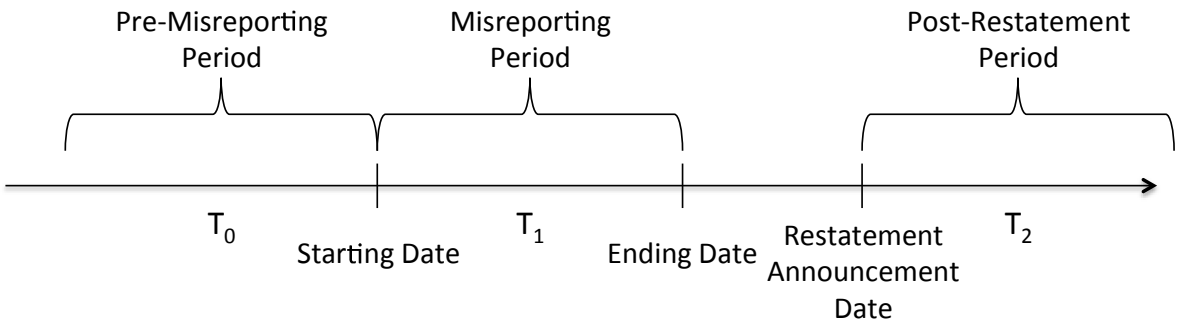

Table 1 Research Design

This table shows the structure of my research design, which is a matched-pair comparison of bank loan terms across misreporting and pre-misreporting period.

	Pre-Misreporting Period T_0	Misreporting Period T_1
Restating Firm (Experiment Group)	$X_{0,i}$	$X_{1,i}$
Non-restating Firm (Control Group)	$C_{0,i}$	$C_{1,i}$

Restating firm: the firm that subsequently announces restatement of previously issued financial statements.

Non-restating firm: the firm that has no restatement announcement and is matched to the restating firm based on (a) same two-digit SIC industry code, and (b) firm size.

The main test is to compare terms of bank loans made to restating firms (X_1) and non-restating firms (C_1) during the misreporting period (T_1) and examine the difference (i.e. $X_{1,i} - C_{1,i}$).

The three loan terms, denoted as i, are:
1. Loan interest rate spread,
2. Number of financial covenants,
3. Loan maturity.

To isolate the effect of financial misreporting from other factors that could also lead to differences in loan terms between restating firms, I also compare the terms of bank loans made to restating firms (X_0) and non-restating firms (C_0) during the pre-misreporting period (T_0) and use the difference (i.e. $X_{0,i} - C_{0,i}$) as the control.

Table 2 Sample Selection and Distribution

This table describes the selection process of my sample with restatements filed in the period 2000-2010, the characteristics of the final 294 restating firms, and the distribution of a total number of 3,142 loan facilities in the final sample of 588 firms (294 matched-pair of restating and non-restating firms).

Panel A: Sample Selection

	Number of Restating Firms	Number of Loan Facilities
Audit Analytics restatement sample	10,815	
Less: multiple restatements	(3,871)	
Less: financial companies	(1,072)	
Less: firms without DealScan loan data, Compustat data, or CRSP data	(4,351)	
Less: firms without data in both pre-misreporting and misreporting period*	(1,142)	
Less: firms without matched pair	(85)	
Final sample of restating firms	294	1,694
Final sample of restating and non-restating firms	588	3,142

* Similar to Graham et al. (2008), I require that each restating firm have loan observations in both the pre-misreporting and during-misreporting period in order to make fair comparison between debt contracts between these two periods.

Panel B: Distribution of Restating Firms by Filing Year

Year	Number of restating firms	Percentage
2000	16	5.4%
2001	15	5.1%
2002	19	6.5%
2003	35	11.9%
2004	39	13.3%
2005	77	26.2%
2006	37	12.6%
2007	24	8.2%
2008	16	5.4%
2009	10	3.4%
2010	6	2.0%
	294	100.0%

Table 2 (cont.)

Panel C: Distribution of Restating Firms by Industry

Fama-French 17 Industry	Number of restating firms	Percentage
Food	14	4.8%
Mining and Minerals	1	0.3%
Oil and Petroleum Products	26	8.8%
Textiles, Apparel & Footware	6	2.0%
Consumer Durables	8	2.7%
Chemicals	8	2.7%
Drugs, Soap, Perfumes, Tobacco	12	4.1%
Construction and Construction Materials	5	1.7%
Steel Works	7	2.4%
Fabricated Products	4	1.4%
Machinery and Business Equipment	32	10.9%
Automobiles	5	1.7%
Transportation	7	2.4%
Utilities	7	2.4%
Retail Stores	42	14.3%
Other	110	37.4%
	294	100.0%

Panel D: Distribution of Loan Facilities by Restating and Non-Restating Firms across Pre-Misreporting and Misreporting Period

	Number of Loan Facilities		
	Pre-Misreporting Period	Misreporting Period	Total
Restating Firms	1,042	652	1,694
Non-Restating Firms	906	542	1,448
	1,948	1,194	3,142

Table 3 Descriptive Statistics

This table presents the descriptive statistics on main variables, including loan-specific characteristics, borrower-specific characteristics, and macroeconomic factors, for the 3,142 loan facilities.

Panel A: Loan Facility Characteristics

Variables	N	Mean	S.D.	P25	P50	P75
AIS (%)	3,142	1.763	1.365	0.750	1.500	2.500
MATURITY (months)	3,142	45.688	27.126	24.000	48.000	60.000
LMATURITY	3,142	3.599	0.749	3.178	3.871	4.094
LOANSIZE (millions)	3,142	286.935	634.789	50.000	145.443	300.000
LLOANSIZE	3,142	18.575	1.466	17.728	18.795	19.519
PPRICING	3,142	0.519	0.500	0.000	1.000	1.000
NLENDER	3,142	8.883	10.559	2.000	6.000	12.000
SYNDICATION	3,083	0.944	0.230	1.000	1.000	1.000

Panel B: Firm Characteristics

Variables	N	Mean	S.D.	P25	P50	P75
RES	3,142	0.539	0.499	0.000	1.000	1.000
POSTMIS	3,142	0.380	0.485	0.000	0.000	1.000
SIZE	3,142	6.799	1.639	5.621	6.756	7.913
LEVERAGE	3,142	0.313	0.172	0.191	0.303	0.425
MTB	3,142	3.283	4.409	1.404	2.170	3.496
PROFITABILITY	3,142	0.034	0.032	0.021	0.033	0.048
GROWTH	3,142	0.190	0.473	-0.002	0.091	0.226
ALTMANZ	3,142	3.480	2.427	1.812	4.430	4.743
CFVOLATILITY	3,142	0.029	0.030	0.012	0.020	0.033
TANGIBILITY	3,142	0.329	0.235	0.151	0.259	0.457
RETURN	3,142	0.062	0.271	-0.094	0.036	0.171

Panel C: Macroeconomic Factors

Variables	N	Mean	S.D.	P25	P50	P75
CSPREAD	3,142	0.838	0.227	0.680	0.790	0.920
TSPREAD	3,142	0.951	0.887	0.200	0.610	1.870

See Appendix A for variable definitions.

Table 4 Univariate Comparisons of Loan and Firm Characteristics

With the total number of 3,142 loan facilities divided into two periods: misreporting (1,194 loans) and pre-misreporting (1,948 loans), this table reports the comparison of main variables on loan facilities between restating and non-restating firms in these two periods. Panel A compares the loans made during the misreporting period, and Panel B compares the loans in the pre-misreporting period.

Panel A: Non-Restating Firms versus Restating Firms (misreporting period)

	(1) Non-Restating Firms			(2) Restating Firms			(2) - (1)	
Variables	**N**	**Mean**	**Median**	**N**	**Mean**	**Median**	**t**	**z**
AIS (%)	542	1.767	1.500	652	2.071	1.750	3.54***	3.85***
LEVERAGE	542	0.279	0.275	652	0.330	0.319	4.97***	4.35***
PROFITABILITY	542	0.032	0.034	652	0.030	0.029	-1.08	-3.25***
SIZE	542	6.966	6.956	652	7.206	7.225	2.61***	2.71***
GROWTH	542	0.115	0.071	652	0.148	0.069	1.45	0.25
MTB	542	3.278	2.107	652	3.052	2.189	-0.90	-0.22
ALTMANZ	542	3.597	4.423	652	3.179	4.343	-2.77***	-2.96***
CFVOLATILITY	542	0.030	0.021	652	0.026	0.018	-2.49**	-3.06***
TANGIBILITY	542	0.299	0.224	652	0.327	0.254	2.05**	1.99**
RETURN	542	0.058	0.041	652	0.058	0.020	0.03	-0.79
MATURITY (months)	542	44.605	48.000	652	48.538	48.000	2.68***	1.68*
LMATURITY	542	3.598	3.871	652	3.700	3.871	2.54**	1.68*
LOANSIZE (millions)	542	343.769	150.000	652	330.859	175.000	-0.26	0.03
LLOANSIZE	542	18.751	18.826	652	18.762	18.980	0.14	0.03
PPRICING	542	0.579	1.000	652	0.600	1.000	0.71	0.71
NLENDER	542	10.009	7.000	652	9.479	7.000	-0.74	0.51

Panel B: Non-Restating Firms versus Restating Firms (pre-misreporting period)

	(1) Non-Restating Firms			(2) Restating Firms			(2) - (1)	
Variables	**N**	**Mean**	**Median**	**N**	**Mean**	**Median**	**t**	**z**
AIS (%)	906	1.584	1.250	1,042	1.723	1.500	2.41**	2.57***
LEVERAGE	906	0.304	0.293	1,042	0.327	0.317	2.91***	2.94***
PROFITABILITY	906	0.034	0.033	1,042	0.036	0.037	1.24	1.97**
SIZE	906	6.631	6.567	1,042	6.604	6.487	-0.37	-0.63
GROWTH	906	0.184	0.093	1,042	0.259	0.110	3.19***	3.52***
MTB	906	3.313	2.166	1,042	3.403	2.212	0.45	0.76
ALTMANZ	906	3.705	4.496	1,042	3.412	4.435	-2.80***	-4.26***
CFVOLATILITY	906	0.032	0.020	1,042	0.027	0.019	-3.76***	-3.30***
TANGIBILITY	906	0.316	0.247	1,042	0.355	0.298	3.73***	3.48***
RETURN	906	0.048	0.025	1,042	0.077	0.046	2.36**	1.89**
MATURITY (months)	906	41.737	36.000	1,042	47.904	50.000	4.85***	4.45***
LMATURITY	906	3.482	3.584	1,042	3.639	3.912	4.46***	4.45***
LOANSIZE (millions)	906	246.187	116.178	1,042	265.318	125.000	0.96	0.70
LLOANSIZE	906	18.440	18.571	1,042	18.484	18.644	0.64	0.70
PPRICING	906	0.451	0.000	1,042	0.496	0.000	1.97**	1.97**
NLENDER	906	8.089	5.000	1,042	8.615	6.000	1.25	0.54

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, in a two-tailed test.

Table 5
Pearson Correlation Table

(obs=3,083)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
(1) *AIS*	1.000																			
(2) *RES*	**0.072**	1.000																		
(3) *POSTMIS*	**0.099**	0.007	1.000																	
(4) *POSTMIS_RES*	**0.118**	**0.475**	**0.653**	1.000																
(5) *LEVERAGE*	**0.239**	**0.093**	-0.033	**0.048**	1.000															
(6) *PROFITABILITY*	**-0.182**	0.004	**-0.062**	**-0.057**	**-0.132**	1.000														
(7) *SIZE*	**-0.388**	0.019	**0.140**	**0.125**	**0.113**	-0.023	1.000													
(8) *GROWTH*	**0.058**	**0.060**	**-0.093**	*-0.045*	**0.051**	**0.128**	**-0.047**	1.000												
(9) *MTB*	-0.010	-0.005	-0.025	-0.029	**0.194**	**0.127**	**0.099**	**0.049**	1.000											
(10) *ALTMANZ*	**-0.257**	**-0.068**	-0.033	**-0.063**	**-0.404**	**0.188**	**-0.071**	-0.028	**-0.069**	1.000										
(11) *CFVOLATILITY*	**0.080**	**-0.072**	-0.029	**-0.050**	**-0.184**	**-0.061**	**-0.227**	**-0.101**	-0.006	**0.077**	1.000									
(12) *TANGIBILITY*	0.021	**0.073**	*-0.045*	-0.002	**0.159**	**0.098**	*0.046*	0.013	-0.012	**-0.252**	**-0.146**	1.000								
(13) *RETURN*	**0.146**	0.033	-0.011	-0.008	*0.043*	**0.065**	**-0.080**	*0.043*	**0.074**	*-0.036*	*0.041*	-0.033	1.000							
(14) *LMATURITY*	**0.128**	**0.090**	**0.059**	**0.069**	**0.152**	0.026	**-0.051**	0.013	*0.043*	**-0.052**	**-0.091**	*0.039*	**0.082**	1.000						
(15) *LLOANSIZE*	**-0.421**	0.001	**0.095**	**0.061**	**0.087**	**0.076**	**0.762**	-0.025	**0.111**	-0.004	**-0.153**	0.039	-0.040	**0.072**	1.000					
(16) *PPRICING*	**-0.136**	0.032	**0.101**	**0.075**	*-0.036*	*0.038*	0.006	-0.030	0.003	*0.042*	*-0.045*	**-0.053**	-0.013	**0.168**	**0.131**	1.000				
(17) *NLENDER*	**-0.193**	0.002	**0.054**	0.023	**0.113**	0.003	**0.458**	0.023	**0.066**	*-0.036*	**-0.090**	-0.021	0.001	**0.094**	**0.499**	**0.144**	1.000			
(18) *SYNDICATION*	**-0.140**	0.000	**0.092**	**0.074**	**0.082**	**0.070**	**0.301**	0.011	**0.053**	-0.013	**-0.099**	0.001	0.018	**0.134**	**0.417**	**0.167**	**0.179**	1.000		
(19) *CSPREAD*	**0.148**	-0.033	**0.299**	**0.192**	-0.010	**-0.056**	**0.086**	**-0.123**	-0.020	0.005	0.005	*-0.035*	0.017	**-0.066**	0.006	*0.038*	-0.022	0.024	1.000	
(20) *TSPREAD*	**0.114**	-0.023	**0.205**	**0.140**	**-0.092**	**-0.090**	**0.066**	**-0.136**	**-0.077**	-0.023	0.031	0.013	*0.038*	**-0.061**	-0.033	-0.025	0.020	*-0.045*	**0.542**	1.000

Bold, *italic*, and underlined fonts indicate significance level at 1 percent, 5 percent, and 10 percent, respectively. See Appendix for variable definitions.

Table 6 Loan Spread and Misreporting

	OLS Regression with Dependent Variable = *AIS*				
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
RES	0.111**	0.043	0.030	0.028	0.028
	(1.964)	(0.937)	(0.731)	(0.691)	(0.371)
POSTMIS	-0.046	-0.032	-0.013	-0.013	-0.013
	(-0.561)	(-0.504)	(-0.231)	(-0.225)	(-0.175)
POSTMIS_RES	0.195*	0.217***	0.162**	0.160**	0.160*
	(1.940)	(2.627)	(2.192)	(2.160)	(1.832)
LEVERAGE		1.680***	1.159***	1.174***	1.174***
		(11.473)	(8.716)	(8.872)	(5.085)
PROFITABILITY		-5.772***	-4.558***	-4.589***	-4.589***
		(-5.785)	(-5.132)	(-5.207)	(-3.015)
SIZE		-0.379***	-0.153***	-0.153***	-0.153***
		(-28.316)	(-6.077)	(-6.075)	(-3.496)
GROWTH		0.204***	0.151***	0.155***	0.155***
		(4.626)	(3.969)	(4.084)	(4.131)
MTB		-0.005	-0.003	-0.003	-0.003
		(-0.795)	(-0.532)	(-0.533)	(-0.316)
ALTMANZ		-0.097***	-0.088***	-0.087***	-0.087***
		(-6.713)	(-7.634)	(-7.548)	(-5.872)
CFVOLATILITY		2.591***	3.586***	3.578***	3.578***
		(2.938)	(4.929)	(4.922)	(4.120)
TANGIBILITY		0.077	0.063	0.059	0.059
		(0.665)	(0.608)	(0.565)	(0.289)
RETURN		0.438***	0.345***	0.348***	0.348**
		(4.073)	(3.749)	(3.771)	(2.457)
LMATURITY			-0.100*	-0.097*	-0.097
			(-1.939)	(-1.887)	(-1.363)
LLOANSIZE			-0.187***	-0.188***	-0.188***
			(-6.443)	(-6.470)	(-4.720)
PPRICING			-0.313***	-0.311***	-0.311***
			(-7.389)	(-7.366)	(-5.240)
LNLENDER			-0.015	-0.016	-0.016
			(-0.609)	(-0.621)	(-0.443)
SYNDICATION			0.010	0.006	0.006
			(0.081)	(0.048)	(0.046)
CSPREAD				-0.274	-0.274
				(-1.456)	(-1.480)
TSPREAD				0.141**	0.141
				(2.119)	(1.600)
Intercept	1.185***	2.263***	4.616***	4.890***	4.890***
	(11.497)	(12.494)	(9.827)	(9.353)	(8.728)
Year fixed effect	yes	yes	yes	yes	yes
Industry fixed effect	yes	yes	yes	yes	yes
Loan Purpose and Type fixed effect	no	no	yes	yes	yes
Firm and year clustering adjusted standard error	no	no	no	no	yes
Number of observations	3,142	3,142	3,083	3,083	3,083
Adjusted R^2	0.068	0.375	0.521	0.522	0.522

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, in a two-tailed test. The t-statistics in parentheses are based on standard errors corrected for heteroskedasticity. See Appendix A for variable definitions.

Table 7 Financial Covenants and Misreporting

	Poisson Regression with Dependent Variable = *FINCOV*				
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
RES	0.014	-0.014	-0.018	-0.019	-0.019
	(0.488)	(-0.538)	(-0.730)	(-0.745)	(-0.640)
POSTMIS	-0.029	-0.019	-0.037	-0.035	-0.035
	(-0.808)	(-0.557)	(-1.156)	(-1.123)	(-1.073)
POSTMIS_RES	0.082*	0.098**	0.100***	0.099***	0.099**
	(1.889)	(2.400)	(2.606)	(2.587)	(2.480)
LEVERAGE		0.449***	0.234***	0.235***	0.235***
		(6.469)	(3.309)	(3.335)	(2.736)
PROFITABILITY		0.168	0.036	0.035	0.035
		(0.599)	(0.130)	(0.124)	(0.116)
SIZE		-0.091***	-0.053***	-0.054***	-0.054***
		(-11.793)	(-4.081)	(-4.129)	(-3.385)
GROWTH		0.015	0.014	0.016	0.016
		(0.529)	(0.524)	(0.592)	(0.578)
MTB		0.001	0.001	0.001	0.001
		(0.387)	(0.437)	(0.429)	(0.407)
ALTMANZ		-0.008*	-0.010**	-0.010**	-0.010*
		(-1.645)	(-2.030)	(-2.010)	(-1.806)
CFVOLATILITY		-1.102***	-0.873**	-0.884**	-0.884**
		(-2.875)	(-2.302)	(-2.330)	(-2.002)
TANGIBILITY		-0.097*	-0.104*	-0.105*	-0.105
		(-1.693)	(-1.933)	(-1.955)	(-1.358)
RETURN		0.057	0.018	0.018	0.018
		(1.577)	(0.507)	(0.499)	(0.519)
LMATURITY			0.025	0.026	0.026
			(0.911)	(0.937)	(0.888)
LLOANSIZE			-0.062***	-0.062***	-0.062***
			(-3.134)	(-3.147)	(-2.850)
PPRICING			0.042	0.042	0.042
			(1.601)	(1.598)	(1.426)
LNLENDER			0.034**	0.034**	0.034**
			(2.000)	(2.000)	(1.969)
SYNDICATION			0.102*	0.101*	0.101*
			(1.928)	(1.905)	(1.734)
CSPREAD				-0.126	-0.126
				(-1.122)	(-1.121)
TSPREAD				0.040	0.040
				(1.079)	(1.032)
Intercept	0.626*	1.332***	1.940***	2.145***	2.145***
	(1.774)	(2.900)	(3.632)	(4.120)	(3.917)
Year fixed effect	yes	yes	yes	yes	yes
Industry fixed effect	yes	yes	yes	yes	yes
Loan Purpose and Type fixed effect	no	no	yes	yes	yes
Firm and year clustering adjusted standard error	no	no	no	no	yes
Number of observations	1,511	1,511	1,511	1,511	1,511
Adjusted R^2	0.011	0.026	0.041	0.041	0.041

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, in a two-tailed test. The t-statistics in parentheses are based on standard errors corrected for heteroskedasticity. See Appendix A for variable definitions.

Table 8 Loan Maturity and Misreporting

OLS Regression with Dependent Variable = *LMATURITY*

Variable	(1)	(2)	(3)	(4)	(5)
RES	0.136***	0.111***	0.057***	0.056***	0.056*
	(3.861)	(3.166)	(2.699)	(2.656)	(1.822)
POSTMIS	0.089*	0.099**	0.065**	0.064**	0.064*
	(1.949)	(2.230)	(2.512)	(2.489)	(1.810)
POSTMIS_RES	-0.043	-0.046	-0.075**	-0.074**	-0.074*
	(-0.805)	(-0.878)	(-2.459)	(-2.405)	(-1.857)
LEVERAGE		0.710***	0.056	0.052	0.052
		(8.145)	(0.975)	(0.912)	(0.711)
PROFITABILITY		0.881**	0.864***	0.845***	0.845***
		(2.057)	(3.183)	(3.098)	(2.645)
SIZE		-0.035***	-0.011	-0.011	-0.011
		(-3.815)	(-1.037)	(-1.034)	(-0.842)
GROWTH		-0.012	-0.035	-0.036	-0.036*
		(-0.240)	(-1.351)	(-1.388)	(-1.664)
MTB		-0.001	0.000	-0.000	-0.000
		(-0.354)	(0.012)	(-0.016)	(-0.015)
ALTMANZ		0.002	0.003	0.003	0.003
		(0.335)	(0.581)	(0.583)	(0.471)
CFVOLATILITY		-1.492***	-0.756**	-0.750**	-0.750
		(-2.839)	(-2.421)	(-2.414)	(-1.544)
TANGIBILITY		0.139*	0.025	0.026	0.026
		(1.828)	(0.539)	(0.551)	(0.451)
RETURN		0.172***	0.045	0.045	0.045***
		(3.520)	(1.228)	(1.229)	(3.474)
LLOANSIZE			0.074***	0.073***	0.073***
			(6.321)	(6.283)	(5.547)
PPRICING			0.082***	0.082***	0.082***
			(4.568)	(4.553)	(3.915)
LNLENDER			0.033***	0.033***	0.033**
			(2.725)	(2.795)	(2.059)
SYNDICATION			0.083	0.084	0.084
			(1.326)	(1.337)	(1.425)
CSPREAD				-0.039	-0.039
				(-0.466)	(-0.426)
TSPREAD				-0.056*	-0.056**
				(-1.814)	(-2.107)
Intercept	3.591***	3.360***	1.203***	1.237***	1.237***
	(48.033)	(30.265)	(6.462)	(5.897)	(6.758)
Year fixed effect	yes	yes	yes	yes	yes
Industry fixed effect	yes	yes	yes	yes	yes
Loan Purpose and Type fixed effect	no	no	yes	yes	yes
Firm and year clustering adjusted standard error	no	no	no	no	yes
Number of observations	3,142	3,142	3,083	3,083	3,083
Adjusted R^2	0.053	0.088	0.690	0.690	0.690

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, in a two-tailed test. The t-statistics in parentheses are based on standard errors corrected for heteroskedasticity. See Appendix A for variable definitions.

Table 9 Earnings Announcement Returns during Misreporting Period

	OLS with Dependent Variable=*CRET*	
Variable	**(1)**	**(2)**
RES	-0.000	-0.003
	(-0.356)	(-1.634)
POSTMIS	-0.000	-0.002
	(-0.185)	(-0.778)
POSTMIS_RES	-0.003	-0.001
	(-0.871)	(-0.268)
SIZE	-0.001***	-0.002***
	(-3.497)	(-3.586)
MBE	0.027***	0.031***
	(14.176)	(11.084)
ESURPRISE	0.415**	0.208
	(2.541)	(1.170)
VOLATILITY_INDEX	-0.001***	-0.000***
	(-4.347)	(-3.351)
Intercept	0.001	-0.024***
	(0.090)	(-2.932)
Year fixed effect	yes	yes
Industry fixed effect	yes	yes
Number of observations	17,259	8,216
Adjusted R^2	0.039	0.035

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, in a two-tailed test. The t-statistics in parentheses are based on standard errors adjusted for firm and year clustering. *CRET* is the size-adjusted, buy-and-hold stock return of a firm *i* measured over the three-day window (-1, +1) around the quarterly earnings announcement. *RES* is an indicator variable equal to 1 if the earnings announcement is made by a restating firm, and 0 if made by a non-restating firm. *POSTMIS* is an indicator variable equal to 1 if the earnings announcement is made during the misreporting period, and 0 if made in the pre-misreporting period. *POSTMIS_RES* is the interaction term. *SIZE* is the firm size measured as natural logarithm of total assets. *MBE* is an indicator variable equal to 1 if the actual earnings are equal to or higher than the most recent mean analyst forecast, and 0 otherwise. *ESURPRISE* is measured as the difference between the actual earnings and the most recent mean analyst forecast, scaled by stock price. *VOLATILITY_INDEX* is the Chicago Board Options Exchange's volatility index on the day before earnings announcement.

Table 10 Analyst Forecast Dispersion during Misreporting Period

	OLS with Dependent Variable=*DISP*	
RES	-0.00012	0.00008
	(-0.749)	(0.356)
POSTMIS	-0.00008	0.00009
	(-0.375)	(0.642)
POSTMIS_RES	0.00022	0.00002
	(0.879)	(0.082)
ROA	-0.01879***	-0.01937***
	(-9.331)	(-9.439)
SIZE	-0.00013***	-0.00014***
	(-2.793)	(-2.845)
MTB	-0.00003	-0.00003**
	(-1.591)	(-2.569)
GROWTH	-0.00044***	-0.00043**
	(-3.199)	(-2.171)
Intercept	0.00255***	0.00401***
	(6.297)	(8.447)
Year fixed effect	yes	yes
Industry fixed effect	yes	yes
Number of observations	32,320	14,576
Adjusted R2	0.114	0.127

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, in a two-tailed test. The t-statistics in parentheses are based on standard errors adjusted for firm and year level clustering. *DISP* is analyst forecast dispersion measured as the standard deviation of one-quarter-ahead earnings forecast, scaled by prior quarter-end stock price. *RES* is an indicator variable that equals to 1 if the analyst earnings forecasts are made for a restating firm and equals to 0 if made for a non-restating firm. *POSTMIS* is an indicator variable that equals to 1 if the analyst earnings forecasts are made during the misreporting period and equals 0 if made in the pre-misreporting period. *POSTMIS_RES* is the interaction term. *ROA* is EBITDA/total assets. *SIZE* is the natural logarithm of total assets. *MTB* is market value of equity/book value of common shareholders' equity. *GROWTH* is the growth of total sales measured in percentage.

Table 11 Bond Interest Spread during Misreporting Period

	OLS with Dependent Variable=*BONDSPREAD*	
RES	0.072	0.078
	(0.572)	(0.665)
POSTMIS	0.051	0.046
	(0.232)	(0.201)
POSTMIS_RES	-0.080	-0.101
	(-0.301)	(-0.356)
LEVERAGE	0.812	0.831*
	(1.628)	(1.818)
SIZE	-0.304***	-0.345***
	(-6.848)	(-3.781)
PROFITABILITY	-5.071***	-5.046***
	(-2.747)	(-3.536)
MTB	-0.066***	-0.069**
	(-2.724)	(-2.024)
GROWTH	0.274**	0.255
	(2.051)	(1.606)
ALTMANZ	-0.145***	-0.142**
	(-2.938)	(-2.365)
TANGIBILITY	0.358	0.388
	(1.164)	(1.443)
BONDMATURITY		0.011
		(0.243)
BONDSIZE		0.130*
		(1.858)
SECURED		-0.042
		(-0.153)
Intercept	2.418***	1.297
	(3.957)	(0.985)
Year fixed effect	yes	yes
Industry fixed effect	yes	yes
Number of observations	892	892
Adjusted R2	0.537	0.537

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, in a two-tailed test. The t-statistics in parentheses are based on standard errors adjusted for firm and year level clustering. *BONDSPREAD* is the interest spread in basis points on the public bond over the interest rate on a treasury of similar maturity, divided by 100. *BONDMATURITY* is the natural logarithm of bond maturity measured in months. *BONDSIZE* is the natural logarithm of bond amount. *SECURED* is an indicator variable that equals 1 if the bond is secured with collateral and 0 otherwise. Other variables are the same as defined in Appendix A.

Table 12 Audit Fees during Misreporting Period

	OLS with Dependent Variable=*LAF*
RES	-0.068
	(-1.155)
POSTMIS	-0.083
	(-1.614)
POSTMIS_RES	0.073
	(1.173)
LTA	0.568***
	(32.904)
CATA	1.009***
	(7.200)
QUICK	-0.074***
	(-3.815)
DE	0.049
	(0.380)
ROI	-0.620***
	(-3.401)
YE	-0.225***
	(-4.720)
OPINION	0.021
	(0.630)
BIG4	-0.122
	(-0.937)
LSEG	0.125***
	(3.807)
Intercept	9.049***
	(44.974)
Year fixed effect	yes
Industry fixed effect	yes
Number of observations	1,843
Adjusted R2	0.796

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, in a two-tailed test. The t-statistics in parentheses are based on standard errors adjusted for firm and year level clustering. *LAF* is the natural logarithm of audit fees. *LTA* is the natural logarithm of total assets. *CATA* is current assets/total assets. *QUICK* is (current assets-inventory)/total assets. *DE* is debt-to-equity ratio. *ROI* is EBIT/total assets. *YE* is an indicator variable that equals 1 if the fiscal year end month is not December. *OPINION* is an indicator variable that equals 1 if the client receives a non-standard audit opinion and 0 otherwise. *BIG4* is an indicator variable that equals 1 if the auditor is one of the big 4 firms and 0 otherwise. *LSEG* is the natural logarithm of number of business segments. Other variables are the same as defined in Appendix A.

Table 13 Loan Spread and Type of Restatement

	OLS Regression with Dependent Variable = *AIS*	
Variable	**Core Restatement**	**Non-Core Restatement**
RES	-0.036	0.046
	(-0.355)	(0.805)
POSTMIS	-0.256**	0.010
	(-2.069)	(0.130)
POSTMIS_RES	0.246*	0.200**
	(1.700)	(2.144)
LEVERAGE	1.121***	1.472***
	(4.256)	(8.804)
PROFITABILITY	-6.992***	-4.266***
	(-5.287)	(-5.576)
SIZE	-0.217***	-0.102***
	(-5.411)	(-3.959)
GROWTH	0.094	0.206***
	(1.196)	(4.212)
MTB	-0.009	0.000
	(-1.007)	(0.010)
ALTMANZ	-0.074***	-0.127***
	(-4.873)	(-8.977)
CFVOLATILITY	1.390	4.511***
	(1.194)	(4.326)
TANGIBILITY	0.230	-0.281
	(0.954)	(-0.227)
RETURN	0.323***	0.367***
	(2.599)	(4.234)
LMATURITY	-0.002	0.147***
	(-0.038)	(4.279)
LLOANSIZE	-0.259***	-0.232***
	(-5.557)	(-8.168)
PPRICING	-0.442***	-0.433***
	(-5.266)	(-8.612)
LNLENDER	0.174***	-0.096***
	(3.336)	(-2.911)
SYNDICATION	-0.386	0.652
	(-0.403)	(0.896)
CSPREAD	0.033	-0.427
	(0.063)	(-1.564)
TSPREAD	0.002	0.262***
	(0.015)	(2.895)
Intercept	7.044***	7.099***
	(4.203)	(6.232)
Year fixed effect	yes	yes
Industry fixed effect	yes	yes
Loan purpose fixed effect	yes	yes
Loan type fixed effect	yes	yes
Number of observations	745	2,157
Adjusted R2	0.555	0.454

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, in a two-tailed test. The t-statistics in parentheses are based on standard errors corrected for heteroskedasticity. See Appendix A for variable definitions.

Table 14 Loan Spread and Misreporting - Accrual Quality-Based Matching

	OLS Regression with Dependent Variable = *AIS*	
Variable	**ACC1**	**ACC2**
RES	0.021	0.072
	(0.379)	(1.441)
POSTMIS	-0.112	-0.151**
	(-1.350)	(-2.430)
POSTMIS_RES	0.198**	0.249***
	(2.054)	(2.974)
LEVERAGE	1.285***	1.336***
	(7.467)	(9.025)
PROFITABILITY	-2.381**	-3.750***
	(-2.428)	(-3.583)
SIZE	-0.124***	-0.173***
	(-4.143)	(-6.173)
GROWTH	0.182***	0.104*
	(3.486)	(1.854)
MTB	0.005	-0.005
	(0.627)	(-0.727)
ALTMANZ	-0.091***	-0.070***
	(-4.814)	(-4.463)
CFVOLATILITY	3.890***	3.101***
	(3.948)	(3.614)
TANGIBILITY	-0.042	-0.039
	(-0.322)	(-0.296)
RETURN	0.337***	0.143
	(2.976)	(1.326)
LMATURITY	-0.005	-0.076
	(-0.073)	(-1.241)
LLOANSIZE	-0.176***	-0.201***
	(-5.087)	(-6.512)
PPRICING	-0.378***	-0.314***
	(-6.704)	(-6.602)
LNLENDER	-0.085***	-0.037
	(-2.620)	(-1.339)
SYNDICATION	-0.289**	-0.130
	(-2.055)	(-1.073)
CSPREAD	0.175	-0.185
	(0.717)	(-0.787)
TSPREAD	0.217**	0.172**
	(2.401)	(2.111)
Intercept	3.944***	4.967***
	(6.381)	(8.747)
Year fixed effect	yes	yes
Industry fixed effect	yes	yes
Loan purpose fixed effect	yes	yes
Loan type fixed effect	yes	yes
Number of observations	2,093	2,455
Adjusted R2	0.498	0.518

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, in a two-tailed test. The t-statistics in parentheses are based on standard errors corrected for heteroskedasticity. See Appendix A for variable definitions.

Table 15 Loan Spread and Misreporting – The Effect of Lending Relationships

	OLS Regression with Dependent Variable = *AIS*	
Variable	**Model 1**	**Model 2**
REL	-0.216***	-0.269***
	(-3.121)	(-3.094)
POSTMIS	-0.050	-0.108
	(-0.625)	(-1.009)
POSTMIS_REL	0.200*	0.294**
	(1.777)	(2.115)
LEVERAGE	1.043***	1.418***
	(6.107)	(6.571)
PROFITABILITY	-1.551	-1.804
	(-1.396)	(-1.501)
SIZE	-0.114***	-0.135***
	(-3.144)	(-3.463)
GROWTH	0.240***	0.176***
	(4.492)	(2.843)
MTB	-0.002	0.011
	(-0.218)	(0.865)
ALTMANZ	-0.086***	-0.088***
	(-5.256)	(-3.400)
CFVOLATILITY	3.806***	4.445***
	(3.286)	(3.293)
TANGIBILITY	0.143	-0.033
	(0.992)	(-0.196)
RETURN	0.344***	0.381***
	(2.631)	(2.642)
LLOANSIZE	-0.174***	-0.156***
	(-4.512)	(-3.613)
PPRICING	-0.401***	-0.409***
	(-6.544)	(-5.632)
LNLENDER	-0.081**	-0.085**
	(-2.469)	(-2.106)
SYNDICATION	-0.092	-0.208
	(-0.576)	(-1.250)
CSPREAD	0.411	0.692*
	(1.372)	(1.892)
TSPREAD	0.244**	0.262**
	(2.525)	(2.227)
Intercept	4.945***	4.325***
	(6.874)	(4.735)
Year fixed effect	yes	yes
Industry fixed effect	yes	yes
Loan purpose fixed effect	yes	yes
Loan type fixed effect	yes	yes
Number of observations	1,674	1,375
Adjusted R2	0.540	0.487

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, in a two-tailed test. The t-statistics in parentheses are based on standard errors corrected for heteroskedasticity. *REL* is an indicator variable that equals 1 if the loan facility is a relationship loan and 0 otherwise. *POSTMIS_REL* is the interaction term of *REL* and *POSTMIS*. See Appendix A for all other variable definitions.

Table 16 Seemingly Unrelated Regressions

This table reports the estimated results from the SUR (seemingly unrelated regression) procedure including loan pricing, financial covenants, and loan maturity as dependent variables.

Variable	*AIS*	*FINCOV*	*LMATURITY*
RES	0.023	-0.011	0.056***
	(0.517)	(-0.204)	(2.864)
POSTMIS	-0.019	0.005	0.064**
	(-0.329)	(0.074)	(2.515)
POSTMIS_RES	0.167**	0.157**	-0.074**
	(2.376)	(2.004)	(-2.370)
LEVERAGE	1.168***	0.811***	0.052
	(9.529)	(6.233)	(0.958)
PROFITABILITY	-4.670***	0.687	0.845***
	(-7.970)	(1.127)	(3.259)
SIZE	-0.152***	-0.134***	-0.011
	(-7.680)	(-5.331)	(-1.313)
GROWTH	0.158***	0.019	-0.036**
	(4.205)	(0.461)	(-2.172)
MTB	-0.003	0.001	-0.000
	(-0.737)	(0.187)	(-0.013)
ALTMANZ	-0.088***	-0.021**	0.003
	(-9.687)	(-2.262)	(0.694)
CFVOLATILITY	3.651***	-2.311***	-0.750**
	(5.505)	(-3.212)	(-2.557)
TANGIBILITY	0.056	-0.172	0.026
	(0.575)	(-1.584)	(0.590)
RETURN	0.343***	0.167**	0.045
	(5.315)	(2.541)	(1.592)
LLOANSIZE	-0.195***	-0.118***	0.073***
	(-8.635)	(-4.313)	(7.349)
PPRICING	-0.319***	0.085*	0.082***
	(-8.074)	(1.711)	(4.675)
LNLENDER	-0.019	0.057*	0.033***
	(-0.759)	(1.926)	(3.051)
SYNDICATION	-0.003	0.249**	0.084**
	(-0.029)	(2.468)	(2.167)
CSPREAD	-0.270	-0.454**	-0.039
	(-1.343)	(-2.048)	(-0.434)
TSPREAD	0.147**	0.120	-0.056*
	(2.123)	(1.533)	(-1.831)
Intercept	4.771***	4.471***	1.237***
	(4.688)	(4.414)	(2.747)
Year fixed effect	yes	yes	yes
Industry fixed effect	yes	yes	yes
Loan purpose fixed effect	yes	yes	yes
Loan type fixed effect	yes	yes	yes

*, **, *** Denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively, in a two-tailed test. The t-statistics in parentheses are based on standard errors corrected for heteroskedasticity. See Appendix A for variable definitions.

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APPENDIX A: VARIABLE DEFINITIONS

Misreporting Variables	
RES	An indicator variable that equals 1 if the borrower is a firm that subsequently restates previous financial statements and 0 otherwise.
POSTMIS	An indicator variable that equals 1 if the bank loan is initiated during the misreporting period and 0 if the bank loan is initiated before the inception of misreporting.
POSTMIS_RES	The interaction term of *RES* and *POSTMIS*.
Borrower-Specific Variables	
LEVERAGE	(Long-term debt + debt in current liabilities)/total assets.
PROFITABILITY	EBITDA/total assets.
SIZE	The natural logarithm of total assets.
GROWTH	The growth of total sales measured in percentage.
MTB	Market value of equity/book value of common shareholders' equity.
ALTMANZ	Altman's Z-score, calculated as 4.34+0.08×working capital/total assets-0.04×retained earnings/total assets+0.1×earnings before interest and taxes/total assets+0.22×market value of equity/book value of total liabilities-0.06×Sales/total assets for manufacturing firms following Hillegeist et al. (2004), and 6.56×working capital/total assets+ 3.26×retained earnings/total assets+6.72×earnings before interest and taxes /total assets+ 1.05×book value of equity/book value of total liabilities for non-manufacturing firms following Altman(2000).
CFVOLATILITY	Standard deviation of quarterly cash flows from operations over the eight fiscal quarters prior to the loan initiation quarter scaled by total assets.
TANGIBILITY	Net property, plant and equipment/total assets.
RETURN	Buy-and-hold stock return over the ninety days prior to the loan initiation date.
Industry Indicators	A series of indicator variables for the Fama and French's 17 industry definitions.
Loan-Specific Variables	
AIS	Loan spread measured as all-in-spread drawn in the Dealscan database, divided by 100. All-in-spread drawn is defined as the amount the borrower pays in basis points over LIBOR or LIBOR equivalent for each dollar drawn down. (For loans not based on LIBOR, LPC converts the spread into LIBOR terms by adding or subtracting a differential that is adjusted periodically.) This measure adds the borrowing spread of the loan over LIBOR with any annual fee paid to the bank group.

FINCOV	Financial covenant index constructed by counting the number of financial covenants included in a loan contract (at the deal, or package, level).
MATURITY	The maturity of the loan measured in months.
LMATURITY	The natural logarithm of *MATURITY*.
LOANSIZE	The amount of the loan facility measured in millions of dollars.
LLOANSIZE	The natural logarithm of *LOANSIZE*.
PPRICING	An indicator variable equal to 1 if the loan contract includes performance pricing provisions, and 0 otherwise.
NLENDER	The total number of banks in the loan contract.
LNLENDER	The natural logarithm of *NLENDER*.
SYNDICATION	An indicator variable equal to 1 if the loan is syndicated, and 0 otherwise.
Loan Purpose Indicators	A series of the indicator variables for the purposes of loan facilities in DealScan, including corporate purposes, debt repayment, working capital, capital expenditures, takeover, stock buybacks, etc.
Loan Type Indicators	A series of the indicator variables for the types of loan facilities in DealScan, including term loan, revolvers, and 364-day facilities.
Macroeconomic Variables	
CSPREAD	Difference in the yield between BAA and AAA corporate bonds measured one month before the loan initiation, obtained from Federal Reserve Board of Governors.
TSPREAD	Difference in the yield between ten-year and two-year U.S. Treasury bonds measured one month before the loan initiation, obtained from Federal Reserve Board of Governors.