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Three essays on earnings management, financial irregularities, and capital structure

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THREE ESSAYS ON EARNINGS MANAGEMENT, FINANCIAL IRREGULARITIES, AND CAPITAL STRUCTURE

by

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An Abstract

Of a thesis submitted in partial fulfillment
of the requirements for the
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Thesis Supervisor: Professor Anand M. Vijh

ABSTRACT

This thesis comprises of three essays. The first essay is titled ‘Do Acquiring Firms Manage Earnings?’ and is co-authored with Professor Anand M. Vijh. The second essay is titled ‘Do Firms Have a Target Leverage? Evidence from Credit Markets’ and is joint work with Professors Anand M. Vijh and Redouane Elkamhi. The third is essay is single authored and titled ‘Bondholder Wealth Effects of Fraudulent Reporting.’

In the first essay, we investigate possible earnings management by inflating discretionary accruals in a sample of 1,719 cash acquirers and 895 stock acquirers during 1989-2005. Following previous literature, we document higher ROA-matched discretionary accruals for stock acquirers than for cash acquirers. However, simulation evidence with quarterly data shows that ROA-matched discretionary accruals are misspecified for both high-growth and low-growth firms. This is relevant to the current investigation because the median sales growth rate equals 12.1% for cash acquirers and 38.5% for stock acquirers (besides similar differences in other growth measures). We propose a new discretionary accrual measure that controls for both ROA and sales growth. This measure is well-specified and powerful in detecting earnings management in stratified random samples, and it leads to an insignificant difference between discretionary accruals of cash and stock acquirers. Other tests of acquirer incentives to manage earnings, market reaction to earnings management, and time delay between earnings announcement and merger announcement strengthen the evidence against earnings management attributed to stock acquisitions.

In the second essay, we propose credit market based test of whether firms have a target leverage. The static tradeoff theory of capital structure hypothesizes that firms have a target leverage which optimizes firm value in the presence of benefits and costs of leverage (such as taxes and bankruptcy costs). If firms adjust their actual leverage toward this target leverage over time, then rational investors should consider both current and target leverage in pricing contracts whose value depends on the firm’s default risk. Using a large sample of corporate bonds and credit default swap (CDS) contracts during 2000 to 2007, we document evidence consistent with this prediction. In particular, target leverage is both an economically and statistically significant determinant of bond and CDS spreads, and its role increases with contract maturity. Credit ratings also reflect the effect of target leverage, which suggests that the credit rating agencies rate firms as if their capital structure decisions are consistent with the tradeoff theory.

In the third and final essay, I examine how the disclosure of fraudulent reporting affects bondholder wealth, credit ratings, and contract features of new bond issues. I find that fraud announcements trigger swift, sharp, and long lasting credit rating downgrades and are associated with significant declines in bondholder wealth. An examination of new bond issues confirms a significant increase in both the yield spread and the gross spread charged by the investment bank compared to pre-fraud levels. Moreover, a significant proportion of bonds issued after a fraud contain call provisions that are more expensive in the short run but may be potentially value maximizing in the long run if credit conditions improve. Thus, I argue that managers are optimistic that the increase in the cost of debt induced by the fraud is temporary. However, contrary to managers' optimistic beliefs, I find that corporate credit ratings, once decreased, remain significantly depressed for at least three years following the fraud announcement.

Abstract Approved: ______________________
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Graduate College
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CERTIFICATE OF APPROVAL

PH.D. THESIS

This is to certify that the Ph.D. thesis of

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has been approved by the Examining Committee
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To my family

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CHAPTER 1

DO ACQUIRING FIRMS MANAGE EARNINGS?

1. Introduction

Mergers and acquisitions are important events associated with the creation, destruction, and redistribution of shareholder wealth. Previous literature documents negative announcement and long-term returns earned by acquirer firms that use stock payment.[1] This suggests the potential overvaluation of acquirer stock which is not fully corrected on the announcement date. The overvaluation may stem from an irrational assessment of the firm's future prospects by investors, as modeled by Shleifer and Vishny (2003) and empirically documented by Savor and Lu (2008), or an intentional misleading of investors by the firm's managers in the form of earnings management. Erickson and Wang (1999), Louis (2004), Baik, Kang, and Morton (2007), Botsari and Meeks (2008), and Gong, Louis, and Sun (2008b) (henceforth, the previous literature) examine the discretionary accruals of acquiring firms and find them to be significantly higher in stock acquisitions (where there is an incentive to inflate stock prices) than in cash acquisitions (where there is no such incentive). Thus, they conclude that stock acquirers manage earnings ahead of their planned acquisitions.

This paper re-examines the evidence in favor of earnings management from previous literature with a sample of 1,719 cash and 895 stock acquisitions announced during 1989 to 2005. We find that their results can be explained by an incomplete specification of the quarterly discretionary accruals model that does not adjust for the cross-sectional differences in growth characteristics of acquirer firms. We propose a new measure of discretionary accruals that adjusts for both return on assets (ROA) and sales

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[1] See Andrade, Mitchell, and Stafford (2001) for evidence on announcement-period returns, and Loughran and Vijh (1997), Rau and Vermaelen (1998), and Agrawal and Jaffe (2000) for evidence on long-term returns.

growth and show that it leads to well-specified and powerful tests in simulation experiments with quarterly data. Using this measure we find insignificant differences between the discretionary accruals of cash and stock acquirers. Supplemented by other tests that use accrual and non-accrual measures, we conclude that there is little evidence of earnings management attributed to stock acquisitions.

There are numerous reports of earnings management (and accounting fraud) in different contexts in both the popular press and the academic literature, so the existence of earnings management per se is not disputable.[2] An incentive to report higher earnings and sustain higher stock prices can arise from the link between executive pay and performance, or from equity issues that can transfer wealth from new shareholders to old shareholders. Thus, academic researchers have documented evidence of earnings management before initial public offerings (IPOs), seasoned equity offerings (SEOs), and stock repurchases (which can be thought of as negative equity issues), besides stock acquisitions.[3]

Stock acquirers differ from the firms issuing IPOs and SEOs in one important respect which raises questions about their ability to manipulate earnings. While in IPOs and SEOs the new shares are issued to the general public, in stock acquisitions the new shares are issued to the existing shareholders of a target firm. The target shareholders have their managers to protect their interests, and the managers typically hire investment bankers to render an opinion on the fair value of the stock offer. Thus, the previous literature either assumes that the target managers and investment bankers cannot

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[2] Earnings management refers to inflating or deflating accruals and charges within generally accepted accounting principles (GAAP), whereas accounting fraud refers to misreporting accounting numbers outside GAAP.

[3] Teoh, Welch, and Wong (1998) document that firms manage accruals to increase their earnings before SEOs, in an apparent effort to boost their stock price. Gong, Louis, and Sun (2008a) document that firms manage accruals to decrease their earnings before stock repurchases, in an apparent effort to suppress their stock price.

detect earnings management, or that they can be influenced into making a recommendation that is not in the interests of their shareholders. However, if academic researchers can detect earnings management in broad samples, then it is not entirely clear why target managers and investment bankers with greater access to firm-specific and industry-specific information cannot do the same. Further, it is possible that managers of public targets can be influenced by the payment of merger bonuses (Hartzell, Ofek, and Yermack, 2004) or cashing out their stock and option holdings (Cai and Vijh, 2007). But there is no such influence in the case of private targets where presumably the interests of shareholders and managers are aligned. Yet, Baik, Kang, and Morton (2007) report stronger evidence of earnings management in the subsample with private targets than in the subsample with public targets.

A few other reasons motivate our re-examination of the evidence on earnings management in the previous literature. First, while these studies document significant evidence of earnings management by acquiring firms, they do not document parallel evidence for target firms. Erickson and Wang (1999) examine the target firms, but find insignificant evidence of earnings management by them. This seems odd because the target firms have as much control over the acquisition process and as much incentive to inflate their stock price. Second, and more important, we argue that the discretionary accrual models used to establish earnings management in the previous literature suffers from an omitted variable problem.

This omitted variable is explained as follows. First, using traditional measures such as book-to-market and prior returns, a substantial finance literature documents that stock acquirers have stronger growth characteristics than cash acquirers (Faccio and Masulis, 2005; Dong et al., 2006). Second, a substantial accounting literature documents that Jones-type accrual models used in the previous literature on earnings management systematically over-estimate discretionary accruals for growth firms, and thus over-reject the null hypothesis of earnings management. In particular, McNichols (2000, 2002) argues that strong prior sales growth as well as expected future sales growth influence management's working capital decisions, and therefore the firm's current accruals. This growth variable is missing from the discretionary accrual models of previous literature on earnings management by acquiring firms.

Dechow, Kothari, and Watts (1998) and Zhang (2007) argue that similar to investments in fixed assets increases in working capital are a necessary investment to sustain a firm's sales growth. Desai, Rajgopal, and Venkatachalam (2004) discuss the importance of this relation and suggest that the long-term poor performance of firms with high discretionary accruals can be explained as a glamour (or growth) stock phenomenon in disguise. Kothari, Leone, and Wasley (2005) further document that raw accruals are higher for high sales growth firms than for low sales growth firms. Finally, in the context of IPOs, Ball and Shivakumar (2008) find that after controlling for the growth characteristics and the use of proceeds the reporting quality of issuing firms in U.K. is actually conservative. The combined literature thus raises the question of whether the evidence on earnings management by stock acquirers relative to cash acquirers can be explained by the differences in their growth characteristics.

We start by replicating the results of previous literature on earnings management with our sample of cash and stock acquirers. All through this paper we present two sets of results with discretionary accruals. The first set starts with the raw current accruals retrieved from the quarterly balance sheet, and the second set starts with the quarterly cash flow statement. We apply the modified Jones model to the raw current accruals and calculate the discretionary accruals as the difference between the model residuals for sample firms and ROA-matched same-industry firms (similar to Gong, Louis, and Sun, 2008b). We find that the discretionary accruals calculated using the balance sheet method are significantly positive for stock acquirers over each of the quarters t-2, t-1, and t, where t-1 is the quarter of earnings announcement immediately preceding the acquisition announcement. In addition, the discretionary accruals for stock acquirers are significantly higher than for cash acquirers. The results are weakened when the discretionary accruals are calculated using the cash flow method, but remain significant over quarter *t*-1.

We next show that stock acquirers exhibit stronger growth characteristics than cash acquirers. The median sales growth from quarter *t*-5 to *t*-1 equals 12.1% for cash acquirers and 38.5% for stock acquirers. There are similar differences in employee growth (5.7% and 18.8%) and book-to-market (0.24 and 0.39). This raises the concern that not accounting for growth characteristics results in a misspecified discretionary accrual model that overstates the differences between cash and stock acquirers.

In order to address the model specification issues we conduct the simulation experiment outlined in Kothari, Leone, and Wasley (2005), but with quarterly data. We use stratified random samples picked from the aggregate sample of Compustat firm-quarters and test the null hypothesis of zero discretionary accruals versus the alternate hypothesis of positive discretionary accruals. We document that the ROA-adjusted discretionary accruals calculated using the balance sheet method result in significantly higher rejection rates than predicted in all high-growth partitions formed by sales growth, employee growth, or book-to-market. The evidence is particularly acute in the high sales growth partition, with an observed rejection rate of 41% compared to a predicted rejection rate of 5%. While the rejection rates are moderated when the balance sheet method is replaced by the cash flow method, they remain significantly higher than predicted. In summary, we find that ROA-adjusted discretionary accruals used in the prior literature are misspecified in high-growth partitions where the stock acquirers are frequently found.

Following previous discussion and simulation results, we extend the Kothari, Leone, and Wasley (2005) matching-firm procedure to incorporate the effects of both ROA and sales growth. Instead of adjusting the Jones-model residuals by only the median accrual from the matching ROA quintile, we adjust by the median accrual from the intersection of the matching ROA quintile and sales growth tercile. We find that the resulting discretionary accrual measure is well-specified in all three high-growth partitions using the cash flow method and two out of three high-growth partitions using the balance sheet method. Further, tested on the aggregate sample of Compustat firm-quarters, the new measure and the old measure have comparable power in rejecting the null hypothesis of no earnings management when quarterly discretionary accruals of randomly picked firms are artificially deflated or inflated by amounts ranging between -1.0% and 1.0% of total assets.

Based on existing theory and simulation results we argue that one should use ROA and sales growth adjusted discretionary accruals, especially when the underlying partition is correlated with growth. Using this measure, we find no evidence to suggest that the discretionary accruals for stock acquirers are either significantly positive or significantly greater than the discretionary accruals for cash acquirers. In other words, we find no evidence to suggest that stock acquirers manage earnings. However, it may be argued that matching on sales growth can wash away some of the discretionary accruals if a part of sales growth is managed (i.e., throw the baby out with the bathwater). This concern arises from Dechow and Schrand (2004) who document that overstated revenue is a frequent mechanism of accounting fraud. In addition, Shleifer and Vishny (2003) and Jensen (2005) argue that growth firms sometimes become overvalued. While the overvaluation stems from market irrationality, it gives the firms an incentive to make stock acquisitions as well as to maintain their overvaluation by earnings management.

We address this concern in two ways. First, we note that this is similar to a concern that ROA matching may wash away some of the discretionary accruals, which is addressed by Kothari, Leone, and Wasley (2005). We apply and extend their argument to our context. Specifically, any experiment to measure event-specific earnings management should capture the effect related to the event that is beyond what may be attributed to firm characteristics such as ROA and sales growth. Thus, a minimum interpretation of our results would be that there is no evidence of earnings management which can be directly attributed to stock acquisitions.

Second, we report a few additional tests of earnings management whose results do not depend on which of the two models of discretionary accruals is used. These tests focus on the correlation between accrual measures and other variables of interest within partitions of stock acquirers. Given high correlation of 0.90 between ROA adjusted and ROA and sales growth adjusted discretionary accruals, the results hold with both measures.[4] The first test examines the correlation between acquirer incentives to manage earnings and discretionary accruals. The most obvious proxy for acquirer incentive is the relative size of the target to that of the acquirer firm, so we arrange stock acquirers into relative size deciles. We find no significant correlation between acquirer incentives and discretionary accruals.

The second additional test goes back to the time of presumed earnings management. If acquiring firms have the ability to mislead investors, then there should be a positive correlation between discretionary accruals and excess returns around the immediately preceding earnings announcement date. However, using several different-length windows around the earnings announcement and merger announcement dates, we find no significant correlation between discretionary accrual deciles and acquirer stock

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4 This is explained in Section 4.3 and Table 1.5. We report a regression of ROA adjusted discretionary accrual on the following independent variables: ROA and sales growth adjusted discretionary accrual, a dummy variable that equals one if the firm belongs to high-growth partition, and a dummy variable that equals one if the firm belongs to low-growth partition. We find that the high-growth dummy is significantly positive, the low-growth dummy is significantly negative, and the R^2 is very high (arising from correlation between accrual measures). When examining the difference between discretionary accruals of cash and stock acquirers, the dummies come into the picture and the results are sensitive to the measure employed. However, when examining discretionary accruals across partitions of stock acquirers that are predominantly high-growth firms, the dummies do not come into the picture and the results are not sensitive to the measure employed.

returns. It may be that the market understands and at least partially discounts the accrual part of earnings as pointed out by DeFond and Park (2001).

The third additional test examines the correlation between discretionary accruals of stock acquirers and the time lag between the earnings announcement date and the merger announcement and completion dates. Given the temporary nature of discretionary accruals, stock acquirers that indulge in greater earnings management should be in a greater hurry to announce and complete their acquisitions. Once again, we find no significant correlation between discretionary accrual deciles and the time taken to merger announcement and completion. Further, we find that as a group stock acquirers take significantly longer to announce and complete their acquisitions than cash acquirers.

Finally, we examine the role of acquisitions in a sample of firms that were identified by the Securities and Exchanges Commission (SEC) as having committed accounting fraud in *Accounting and Auditing Enforcement Releases* (AAERs) during 1989 to 2003. Less than one percent of our acquiring firms appear in this sample, and these are about evenly split between cash and stock acquirers. We report a logit analysis with a dummy variable that takes the value one for fraud firm-years and zero for other Compustat firm-years.[5] Whereas we find some evidence that fraud firm-years are associated with greater acquisition activity, we find no evidence that fraud firm-years are incrementally associated with greater stock acquisition activity.

The cumulative evidence in this paper is inconsistent with the conclusions of Erickson and Wang (1999), Louis (2004), Baik, Kang, and Morton (2007), Botsari and Meeks (2008), and Gong, Louis, and Sun (2008b). While we cannot eliminate the possibility that earnings management occurs in individual cases, there is no evidence of systematic earnings management before stock acquisitions designed to expropriate wealth

[5] A fraud firm-year (or fraud year) is one for which the firm had manipulated earnings according to the AAER.

from the target shareholders to the acquirer shareholders. As a caveat, we cannot rule out the possibility that there may be some earnings management by stock acquirers common to other same-industry firms with similar ROA and sales growth characteristics over a longer period of time, which is not captured by our discretionary accrual model. However, any secular pattern in earnings management of this nature cannot be attributed to the stock acquisition decision.

2. Measures of earnings management

Current accruals consist of short-term working capital accruals while total accruals include both the short-term and long-term accruals. The biggest long-term accruals are depreciation and amortization. The choice between current accruals and total accruals thus depends on whether depreciation and amortization are managed in a given context. Generally speaking, firms are less likely to manage earnings by manipulating depreciation because disclosure requirements make changes in depreciation policy highly visible (Beneish, 1998; Botsari and Meeks, 2008). Further, Louis (2004) argues that acquiring firms are more likely to manipulate current accruals as investment bankers rely on earnings before interest, taxes, depreciation, and amortization (EBITDA) in their valuation of mergers and acquisitions.

Most studies that examine current accruals estimate them by using the balance sheet method. According to this method, current accruals are estimated from successive balance sheets as follows:

$$Current\ ACC_{bs} = \Delta CA - \Delta CL - \Delta Cash + \Delta STDEBT \tag{1}$$

Here, ΔCA is the change in current assets (Compustat quarterly item 40), ΔCL is the change in current liabilities (item 49), $\Delta Cash$ is the change in cash and cash equivalents (item 36), and $\Delta STDEBT$ is the change in current maturities of long-term debt and other short-term debt included in current liabilities during the quarter under consideration (item 45).

Hribar and Collins (2002) show that measuring accruals using the balance sheet method results in a significantly biased estimate in the presence of prior non-operating events such as mergers and acquisitions, divestitures, and foreign currency translations. These events affect the balance sheet with no corresponding impact on earnings as changes in current assets and liabilities do not flow through the income statement. Thus, Hribar and Collins recommend computing accruals directly from the cash flow statement as it does not suffer from the above bias.

We compute current accruals using the cash flow method as described in Ashbaugh-Skaife, Collins, Kinney, and LaFond (2008) and Botsari and Meeks (2008). We define current accruals as earnings before extraordinary items and discontinued operations (Compustat quarterly item 76) minus operating cash flows (item 108 – item 78) plus depreciation and amortization (item 77).[6]

$$Current\ ACC_{cf} = EBXI - CFO_{cf} + DEP_{cf} \tag{2}$$

In order to maintain comparability and contrast our results with prior literature, we report current accruals computed using both the balance sheet and cash flow methods.

[6] Alternately, one could measure current accruals directly from the cash flow statement as – *(ΔAR + ΔINV + ΔAP + ΔTAX + ΔOTHER)*. However, quarterly data on these individual items are missing in the majority of cases. We overcome this missing items issue and obtain a close approximation of current accruals from the cash flow statement by using this definition.

2.1. Separating accruals into discretionary and nondiscretionary components: The modified Jones model with quarterly data

Following Erickson and Wang (1999), Louis (2004), Baik, Kang, and Morton (2007), and Gong, Louis, and Sun (2008b), we separate current accruals into discretionary and nondiscretionary components using a modified Jones (1991) model applied to quarterly data. Specifically, we estimate the following cross-sectional regression for all Compustat firms i belonging to the same two-digit SIC (standard industry classification) code and calendar quarter τ:

$$\begin{aligned} &Current\ ACC_{i,\tau} = \\ &b_1 Q_{1,i,\tau} + b_2 Q_{2,i,\tau} + b_3 Q_{3,i,\tau} + b_4 Q_{4,i,\tau} + b_5\left(\Delta SALES_{i,\tau} - \Delta AR_{i,\tau}\right) \\ &+ b_6 Current\ ACC_{i,\tau-4} + \varepsilon_{i\tau} \end{aligned} \tag{3}$$

Here, $Q_{1,i} - Q_{4,i}$ are fiscal quarter dummies included to account for seasonality, $\Delta SALES_i$ is the change in sales since last quarter (item 2), ΔAR_i is the change in accounts receivables since the last quarter (item 37), and $Current\ ACC_{i,\tau-4}$ is the current accrual from the same quarter in the previous fiscal year. All variables are scaled by lagged total assets. As the Jones model is designed to explain the level of non-discretionary accruals, discretionary accruals are measured as the residuals from this regression.[7]

Finally, following Louis (2004), Kothari, Leone, and Wasley (2005), Botsari and Meeks (2008), and Gong, Louis, and Sun (2008a, 2008b), we adjust discretionary accruals for performance. This involves computing the difference between discretionary accruals for the sample firm and the median discretionary accruals for the quintile of same-industry firms matched by ROA from the same quarter of previous year. We refer

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[7] We require at least 20 observations for each separate regression by calendar quarter and 2-digit SIC industry code. We delete the following variables at the one percent level: current accruals, lagged current accruals, change in sales, change in accounts receivable. We also discard untenable observations where the absolute value of accruals scaled by total assets is greater than one.

to this final statistic as ROA adjusted discretionary accruals. It is the starting (or traditional) measure of earnings management in our study.

ROA adjusted discretionary accruals have an expected median value close to zero in absence of earnings management, by construction. Thus, we report the median values calculated within various subsamples using the balance sheet method and the cash flow method and the corresponding *p*-values using the Wilcoxon sign-rank test.

3. Data sources

3.1. Sample of acquisitions

Our comprehensive sample includes all acquisitions from the Securities Data Company (SDC) database that meet the following criteria: 1. The acquisition is announced during January 1989 to December 2005. 2. The target is a public, private, or subsidiary firm domiciled in the U.S. 3. The acquirer is a public firm included in the Center for Research in Security Prices (CRSP) and Compustat databases with ordinary common shares outstanding (CRSP share code 10 or 11, which excludes American Depository Receipts (ADRs), Real Estate Investment Trusts (REITs), units, certificates, and trusts). 4. The acquirer is not a financial firm. 5. The transaction value exceeds $10 million. 6. The transaction form is categorized as deal type 1 (disclosed value mergers and acquisitions). 7. The acquirer owns less than 50% of target shares before announcement and 100% of target shares after completion. 8. The form of payment is all cash or all stock (thus, mixed payment cases are excluded). 9. The acquirer has relevant data on Compustat quarterly file to calculate the performance-adjusted discretionary accruals using both the balance sheet and cash flow methods (as described in Section 2).

We obtain the transaction value from the SDC and the acquirer total assets and market value from Compustat as of the last quarter-end. We calculate relative size of the deal as the transaction value divided by the acquirer market value, the acquirer book-to-market value as the book value divided by the market value of equity, and the acquirer return on assets as the net income divided by total assets. The prior-year market-adjusted excess returns are calculated by subtracting the cumulative market returns from the cumulative stock returns over a 254-day period ending on MAD-2 (where MAD denotes the merger announcement date). Sales growth rate is defined as the change in sales from quarter *t*-5 to quarter *t*-1 divided by sales in quarter *t*-5, where *t*-1 is the quarter with the earnings announcement date immediately preceding the merger announcement date.

3.2. Sample description

Table 1.1 presents the sample distribution. Panel A shows that the final sample includes acquisitions of 717 public targets, 1,106 private targets, and 791 subsidiary targets, for a total of 2,614 deals. Of these, 1,719 are cash acquisitions and 895 are stock acquisitions. To our knowledge, this is the most comprehensive sample used to study earnings management around mergers and acquisitions. Panel B shows a calendar pattern in the relative numbers of stock and cash acquisitions. While the numbers are fairly comparable during 1995-2000, a period characterized by rich stock valuations, there are few stock acquisitions during 2002-2005, a period characterized by poor stock valuations.

Table 1.2 presents the summary statistics for the aggregate sample and the subsets formed by the payment method and the target type. The table legend defines many variables that are not defined in the text. We note a few patterns. First, not surprisingly, private targets are smaller than public targets, and are also sought by smaller acquirers. Second, for any target type, cash acquisitions are made by bigger firms than stock acquisitions. Third, as we later show, relative size is an important variable as it determines the acquirer incentives for manipulation in stock acquisitions. Relative size is smaller for private targets than for public targets.

The remaining rows of Table 1.2 present acquirer characteristics associated with overvaluation and accruals. Consistent with the overvaluation hypothesis, prior-year market-adjusted excess return is higher for stock acquirers than for cash acquirers. Next, we note that ROA is similar across the two categories. In contrast, the growth measures are higher for stock acquirers than for cash acquirers in the aggregate sample as well as the subsets formed by target type. In the aggregate sample, the median sales growth rates for cash and stock acquirers equal 12.1% and 38.5%. Recall that sales growth is associated with higher working capital need in an extensive accounting literature mentioned in Section 1. Employee growth and book-to-market do not have the same theoretical or empirical backing, but serve as alternate measures of firm growth. Employee growth is a non-financial variable that is expensive to manipulate. We note that the median employee growth rates for cash and stock acquirers equal 5.7% and 18.8%. However, as employee information is only available from the Compustat annual file, employee growth is calculated over a one-year period ending the year before the merger announcement. Finally, the median book-to-market values for cash and stock acquirers equal 0.39 and 0.24.

Figure 1 shows the distribution of cash and stock acquirers across growth deciles. These deciles are formed using the Fama-French procedure. For example, we compute sales growth rates for all NYSE-listed firms during each calendar quarter with available data to ascertain the sales growth decile cutoffs. Using these cutoffs we assign a sales growth decile rank to all firms in our sample. We find that approximately half of all stock acquirers belong to the highest sales growth decile, and seven out of ten belong to the top three deciles. In comparison, cash acquirers are more evenly distributed, although there is some tendency for them also to belong to the higher sales growth deciles. Similar differences between the distributions of cash and stock acquirers across growth deciles are found when we use the employee growth and book-to-market criteria. Finally, for comparison, the distributions of both cash and stock acquirers are quite flat across the market value deciles. In summary, Table 1.2 and Figure 1.2 show that the cash and stock acquirers differ considerably in their growth characteristics.

4. Main results

4.1. Traditional measures of earnings management preceding cash and stock acquisitions

Previous literature on earnings management examines whether stock acquirers have significantly positive discretionary accruals in the quarter preceding the merger announcement (Erickson and Wang, 1999; Louis, 2004; Botsari and Meeks, 2008). This literature also examines whether discretionary accruals are higher for stock acquirers than for cash acquirers. Clearly, if increasing bottomline earnings through accrual management sustains or increases acquirer stock price, then an acquisition financed by a stock swap would result in an expropriation of wealth from the target shareholders to the acquirer shareholders. Thus, these studies argue that a strategic intent to manage earnings by inflating accruals exists in the case of stock acquisitions but not in the case of cash acquisitions. However, in order for this intent to manifest into action, certain additional conditions would have to be met. First, it must be assumed that the acquirer managers know as of last quarter (if not earlier) with a high probability they would be able to acquire a target firm, and that the target shareholders would accept stock payment. Second, it must be assumed that the target managers and investment bankers (besides shareholders) would not detect earnings management, or that if they do detect earnings management, then they somehow would be influenced into making a recommendation that is not in the interests of shareholders. Neither of these conditions is clearly feasible or clearly infeasible, so earnings management becomes an empirical issue.

Table 1.3 lists performance-adjusted discretionary accruals for acquirer firms as of quarters *t*-2, *t*-1, and *t*, where quarter *t*-1 is the fiscal quarter with the last earnings announcement date (EAD) preceding the merger announcement date (MAD). The previous literature finds strong evidence of earnings management during quarter *t*-1. In addition, the examination of quarter *t*-2 is necessary since there must be some uncertainty about when a merger would be announced. Further, Erickson and Wang (1999) suggest that the incentive to manage earnings does not disappear during the following quarters with earnings announcement dates preceding the merger completion date. In fact, it may be argued that the acquirers would be even more motivated to manage earnings during the following quarters in order to support stock prices after having made a stock offer. We therefore examine the discretionary accruals during quarter *t* for the cases where the merger is not completed by the corresponding earnings announcement date.

Panel A of Table 1.3 presents results based on current accruals calculated using the balance sheet method. These results are broadly consistent with earnings management as in the previous literature. First, in the aggregate sample of all acquisitions, the stock acquirers have median discretionary current accruals of 0.397% during quarter *t*-1, significant at 1% level, compared to an insignificant 0.067% for cash acquirers. The difference between medians equals 0.330%, also significant at 1% level. To understand the economic significance of these numbers, note that the median quarterly ROA is the order of 1.70% for all acquirers. The results are also highly significant for quarter *t*, which cumulatively paint a picture of strong earnings management in the aggregate sample of acquirers. Second, looking across subsets, there is even stronger evidence of earnings management by stock acquirers of private targets, while there is weaker but still significant evidence for stock acquirers of public targets. These results are surprising because the median relative size in Table 1.2 is much smaller for private targets than for public targets (hence a lower incentive), and because there is little or no divergence between manager and shareholder interests of private targets (hence a lower ability). Third, given the relatively low numbers of stock acquisitions of subsidiary targets, the evidence is in the direction of earnings management by stock acquirers, but only significant at 10% level during quarter *t*-1.

Panel B of Table 1.3 shows that the evidence in favor of earnings management is moderated if current accruals are computed using the cash flow method. Stock acquirers still have significantly larger discretionary accruals than cash acquirers for the aggregate sample and for the subset of public targets in the quarter immediately preceding the acquisition announcement. However, in the subsamples of private and subsidiary targets, discretionary accruals for stock acquirers are insignificantly different from zero.

In summary, while current accruals computed using the balance sheet method with ROA adjustment provide strong evidence in favor of earnings management, the evidence is moderate but significant when current accruals are computed using the cash flow method with ROA adjustment. The biases induced by using the balance sheet method relative to the cash flow method with annual data have been illustrated by Hribar and Collins (2002). Below we argue that discretionary accruals derived from both methods are misspecified with quarterly data unless one additionally controls for the cross-sectional differences in sales growth.

4.2. A new ROA and sales growth adjusted measure of discretionary accrual

Any test of earnings management that uses discretionary accruals is also a joint test of the discretionary accrual model. As a result, the specification of the discretionary accrual model assumes critical importance in making unbiased inferences. Dechow, Sloan, and Sweeney (1995) show that Jones-type models over-reject the null hypothesis of no earnings management in firms with extreme financial performance. For such reasons, Kothari, Leone, and Wasley (2005) conduct a simulation experiment to show that performance matching on ROA improves the reliability of earnings management tests. However, McNichols (2000, 2002) finds that both ROA and growth are highly correlated with discretionary accruals in the presence of each other. Her evidence suggests that matching only on ROA may not be adequate. Combined with the theory model of Dechow, Kothari, and Watts (1998) linking sales growth to accruals and the stark difference between sales growth rates of cash and stock acquirers, there is a serious concern that Table 1.3 results may be biased.

An investigation of the role played by sales growth on quarterly discretionary accruals starts with a measure of this variable. Notice that the modified Jones model applied to quarterly data as described by Equation (3) and used in previous literature includes changes in quarterly sales as an explanatory variable. However, this short-term growth measure may be noisy and is not likely to capture the trend in sales growth. In addition, quarterly sales changes may be confounded with the effect of seasonality, which is quite distinct from growth. We have therefore measured the sales growth rate as the growth in sales from the same quarter in the previous fiscal year to the current quarter as described in Section 3.

One method to adjust for the effect of sales growth would be to add this measure to the modified Jones model in Equation (3). However, we lack a clear theory that suggests a linear relation between sales growth and quarterly accruals. Further, there may be interaction effects between ROA and sales growth. In a parallel context, Kothari, Leone, and Wasley (205) adjust for nonlinearity in the relation between discretionary accruals and ROA by using a matched firm approach. Using extensive simulations, they demonstrate that the matched firm approach works better than the linear regression approach. The matched firm approach does not require knowing the functional relationship of accruals on one side and ROA and sales growth on the other side.

As both ROA and sales growth can be thought of as performance characteristics, we propose a simple extension to the Kothari, Leone, and Wasley (2005) matching procedure to mitigate their joint effect. The previous matching procedure creates five portfolios by sorting observations into quintiles based on their ROA in the same quarter in the previous year (Gong, Louis, and Sun, 2008b). The performance matched discretionary accrual is then computed as the difference between the discretionary accrual for the sample firm and the median discretionary accrual of the matched portfolio (excluding the sample firm). We enhance this procedure by creating terciles based on the sales growth rates of the same industry-quarter matching firms. We then require that firms in the matched portfolio be both in the same ROA quintile and in the same sales growth tercile as the sample firm.[8] This is our new ROA and sales growth adjusted measure of discretionary accruals.

4.3. A simulation experiment to compare the specification and power of tests using ROA adjusted discretionary accruals versus ROA and sales growth adjusted discretionary accruals

Simulations represent a controlled experiment in which we can assess the specification and power of discretionary accrual measures by testing on samples for which we know the precise extent of earnings management. While Kothari, Leone, and Wasley (2005) report a simulation experiment to assess the measures with annual data, we report a parallel experiment to assess the measures with quarterly data. The added importance of analyzing quarterly accruals data beyond the context of this study is highlighted by an increasing literature on earnings management around corporate events. Typically, in such cases the effect is localized over a few quarters bracketing the event. To the best of our knowledge, there is no published study that documents such results with quarterly data.

4.3.1. Simulation sample and methodology

We begin with all firm-quarter observations from the Compustat quarterly file during 1989 to 2005. Quarter 1989-1 includes all firms with fiscal quarter-end in February, March, and April of 1989, and so on. We exclude observations for which we

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8 The construction of terciles based on sales growth rather than quartiles or quintiles is an empirical choice in view of the sample size restrictions.

cannot calculate ROA adjusted or ROA and sales growth adjusted discretionary accruals as described in Sections 2 and 4.2. We further require that there is enough data to calculate sales growth, book-to-market, and market value. (The employee growth data is collected from the Compustat annual file and may be missing in some cases.) Collectively, these requirements yield a sample of around 132,000 observations.

Following Kothari, Leone, and Wasley (2005), we report simulation results based on 250 samples of 100 firms each. We draw samples without replacement from the full dataset of firm-quarters or from stratified subsets. The subsets represent the high and low quartile partitions by sales growth, employee growth, book-to-market, and market value. We identify these partitions using the Fama-French procedure. Specifically, we arrange all NYSE-listed firms with available data into quartiles each calendar quarter. We use the cutoffs corresponding to the top and bottom quartiles to identify the high and low partitions from the full dataset of Compustat firm-quarters.

Following Kothari, Leone, and Wasley (2005), for each of the 250 randomly selected samples we assess the significance of the mean discretionary accrual using a one-tailed *t*-test. The *t*-test is defined as the equally-weighted sample mean divided by the sample standard deviation of discretionary accruals. We count the percent cases in which the *t*-test rejects the null hypothesis of zero discretionary accruals (i.e., no earnings management) in favor of the alternate hypothesis of positive (or negative) discretionary accruals (i.e., earnings management) at the 5% significance level. This count should lie between 2% and 8% with 95% probability in case there is no earnings management.

4.3.2. Simulation results

Table 1.4 shows the simulation results. Panels A and B use the balance sheet method, and Panels C and D use the cash flow method. Panels A and C test the null hypothesis of zero discretionary accruals versus the alternate hypothesis of negative discretionary accruals, and Panels B and D test the same null hypothesis versus the alternate hypothesis of positive discretionary accruals. Within each panel, the first subpanel uses ROA adjusted discretionary accruals, and the second subpanel uses ROA and sales growth adjusted discretionary accruals. Each subpanel has three rows that show rejection rates when the raw accruals from Compustat are artificially increased by 0.0%, 0.5%, and 1.0% (alternately, 0.0%, -0.5%, and -1.0%) of lagged assets. The 0.0% case in each subpanel is the basic scenario where the randomly picked samples emulate the situation of all firms within a given partition. Here a well-specified model that measures discretionary accruals of sample firms by filtering out the accruals common to all firms should have rejection rates in the range of 2% to 8%. To facilitate interpretation of results, rejection rates that are lower than 2% appear in bold italic, and rejection rates that are higher than 8% appear in bold.

Table 1.4 shows that the ROA adjusted discretionary accrual model is considerably misspecified in all high and low sales growth partitions. The observed rejection rates are often quite large, and more so with the balance sheet method than with the cash flow method. For example, in the high sales growth partition, the old ROA adjusted model finds significantly positive discretionary accruals in 40.8% and 26.4% of samples with the two methods when no earnings management is present. In the low sales growth partition, this model finds significantly negative accruals even more often, in 64.8% and 57.2% of all samples. These high rejection rates appear surprising at first, but can be related to previous literature that shows a strong link between sales growth and accruals. For example, using annual data, Kothari, Leone, and Wasley (2005) show that the mean total accruals in high and low sales growth partitions equal 1.31% and -7.68%, both very different from -3.03% in the aggregate sample (see their Table 1). In fact, sales growth produces more polarized results than any other partitioning variable in their sample.

Table 1.4 also shows that the new ROA and sales growth adjusted discretionary accrual model is well-specified in all sales growth partitions. We next examine the evidence within high and low partitions formed by employee growth. The ROA adjusted model continues to give too high or too low rejection rates in all employee growth partitions, although the rejection rates are moderated to a maximum of 32.8%. In all cases the ROA and sales growth adjusted model gives better results, with a maximum rejection rate of 10.4%. Thus, even this model shows a mild degree of misspecification. The differences between ROA adjusted and ROA and sales growth adjusted model misspecifications continue to a lesser extent in book-to-market partitions. However, neither model shows a remarkable level of misspecification in market value partitions.

We next examine the model misspecification issues applicable to our sample. From Table 1.2 we know that stock acquirers tend to be high growth firms, and the important question is whether their discretionary accruals are positive. Table 1.4 shades in gray the regions where the stock acquirers are usually found. Briefly speaking, the above evidence on model misspecification carries over just the same to our sample of acquirers. The ROA adjusted discretionary accrual model is considerably misspecified, and more so with the balance sheet method than with the cash flow method. In comparison, the ROA and sales growth adjusted model does very well, especially with the cash flow method.

We next address the statistical power of the two models. In the shaded regions relevant to our sample the ROA adjusted model overwhelmingly rejects the null hypothesis of zero discretionary accruals in favor of the alternate hypothesis of positive discretionary accruals when a positive seed discretionary accrual is added to each observation in the sample. However, this is mainly due to model misspecification, not model power. Looking instead at samples drawn from the collection of all firms next to the shaded regions, the two models have comparable rejection rates. Taking into consideration the entire evidence, it is fair to say that the ROA and sales growth adjusted discretionary accrual model is well-specified and has comparable power to the ROA adjusted model (which, however, is not well-specified).

Table 1.5 presents the quantitative differences between the two models. Using the aggregate sample of 132,000 firm-quarters with available data, we regress the ROA adjusted discretionary accrual on the following independent variables: ROA and sales growth adjusted discretionary accrual, a dummy variable that equals one if the firm belongs to the high-growth partition, and a dummy variable that equals one if the firm belongs to the low-growth partition. The intercept takes the role of the medium-growth partition spanning the middle two quartiles. All three measures of firm growth are reported, as are both the balance sheet method and the cash flow method of calculating accruals, for a total of six regressions.

All six regressions in Table 1.5 have adjusted-R^2 values ranging between 0.82 and 0.85. This is not surprising since the correlation between the two discretionary accrual measures equals 0.91. In view of the large number of observations, most *t*-statistics are highly significant, so we look at the economic magnitudes. The intercept is always close to zero, but the high-growth and low-growth dummies are quite large. We focus on the cash flow method for the interpretation of dummy coefficients. Regression (4) shows that if one compares two samples, one belonging to the medium sales growth partition (e.g., cash acquirers) and the other belonging to the high sales growth partition (e.g., stock acquirers), then the ROA adjusted discretionary accrual model will capture an effect that is roughly 0.369% higher than with the ROA and sales growth adjusted discretionary accrual model. This is our quantitative estimate of the model misspecification in Table 1.4. Regressions (5) and (6) show that this misspecification is reduced but remains economically significant when the comparison samples are clustered in different growth partitions based on employee growth and book-to-market. Finally, the balance sheet method exacerbates all results.

4.4. Do acquiring firms manage earnings? Evidence based on an improved measure of discretionary accruals

Table 1.6 reports ROA and sales-growth adjusted discretionary accruals for acquiring firms as of quarter *t*-2, *t*-1, and *t*. We find no evidence that stock acquirers engage in accrual manipulation prior to an acquisition. These results are in sharp contrast with those presented by Erickson and Wang (1999), Louis (2004), and Botsari and Meeks (2008), which are replicated in our Table 1.3. Ten out of 12 median estimates of discretionary accruals for stock acquirers are insignificant while two are significant in the opposite direction at the minimum 10% level. In addition, 11 out of 12 differences between cash and stock acquirers presented in the right-most column of this table are insignificant, while one is significant in the opposite direction at 5% level. This is roughly what one would expect by chance. Although not shown in the table, the results for private and subsidiary targets are also insignificant with both methods. In summary, after correcting for the cross-sectional differences in firm characteristics of ROA and sales growth, there is no evidence of earnings management by stock acquirers.[9]

5. Additional tests of earnings management

So far our attention has been focused on tests of earnings management using accrual models. These tests have examined whether the discretionary accruals of cash acquirers and stock acquirers are significantly different from zero and each other. We now report a few tests that examine the correlation between accruals and other

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9 As an alternative to sales growth, one may consider the analyst long term earnings growth forecasts from the Institutional Brokers Estimate System (IBES). McNichols (2000) finds that the median earnings growth forecasts are positively correlated with discretionary accruals. However, Chan, Karceski, and Lakonishok (2003) find that IBES earnings growth forecasts are overly optimistic and add little predictive power, while there is some level of persistence and predictability in realized sales growth. Nevertheless, we repeat our entire experiment using the analyst earnings growth forecasts from IBES. This reduces our sample size as well as the set of matching firms, but we find that our results are qualitatively and inferentially similar.

variables of interest within partitions of stock acquirers (alternately, within partitions of cash acquirers). Besides supplementing the evidence, these tests offer one distinct advantage over the previous tests, that their results are valid with both the new ROA and sales growth adjusted model and the old ROA adjusted model. This follows from Table 1.5 which shows that model misspecification is a bigger concern when one compares a sample concentrated within a high or a low growth partition with a benchmark of zero or another sample in a different partition. It would be a lesser concern if we examine partitions of stock acquirers that are concentrated within the high-growth quartile. For a parsimonious presentation, we report the following results only with the ROA and sales growth adjusted model (so we drop the repeated mention of this matching procedure for expositional reasons).[10]

5.1. Are relatively large acquisitions preceded by higher discretionary accruals as predicted by the incentives theory of earnings management?

It is argued that stock acquirers manage earnings to boost stock price and transfer wealth from target shareholders to acquirer shareholders. It should follow that the greater this incentive, the greater the extent of earnings management. Intuitively, the most natural proxy for acquirer incentive should be the relative size of the deal. We formalize this intuition in Appendix 1 and show that the wealth transfer equals $\frac{f \times Relative\ \ Size}{1+Relative\ \ Size}$, where f is the percent overvaluation of the acquirer stock caused by earnings management. Thus, for a fixed percent overvaluation, acquisitions with a larger relative size offer a greater incentive for earnings management.

[10] The results with the ROA adjusted model are available from the authors on request.

Previous literature documents mixed results on the relation between acquirer incentives and earnings management. Using samples of acquisitions of public targets, Erickson and Wang (1999) show that earnings management increases with relative size, but Louis (2004) finds no such relation. In contrast, using a sample of acquisitions of private targets, Baik, Kang, and Morton (2007) find that earnings management is more pronounced for deals with a smaller relative size. They argue that a smaller private target poses a greater information asymmetry problem to an acquirer, which in turn takes pre-emptive action of boosting its stock price by inflating the accruals still further. Their evidence goes against the theory model presented in Appendix A and the evidence presented in Erickson and Wang with a different sample. A possible resolution to this contrast may emerge when we examine the magnitude of potential wealth expropriation from target to acquirer shareholders as a result of earnings management. This specific variable has not been examined in the previous literature.

We arrange all stock acquisitions into relative size deciles in Table 1.7. Panel A presents the evidence for all targets, and Panel B presents the evidence for public targets. Each panel reports the median relative size, the median wealth transfer to acquirer shareholders assuming a fixed f = 15% overvaluation of the acquirer stock due to earnings management, and the median discretionary accruals computed using both the balance sheet method and the cash flow method. The choice of 15% overvaluation is somewhat arbitrary, but it may be a generous estimate of the average price increase resulting from earnings management in view of our previous results. (We specifically examine the magnitude of this overvaluation in the next table.)

In the broad cross-section of acquisitions, we find that many targets are very small relative to acquirers. In deciles 1 to 5, the median relative size equals 0.027 for the entire sample and 0.036 when the sample is restricted to public targets. Even assuming an overvaluation of 15%, the median wealth transfer equals 0.4% of the acquirer stock price for all targets and 0.5% for public targets. Oddly, however, the median discretionary accruals are somewhat higher for deciles 1 to 5 compared to deciles 6 to 10 (although both are insignificant). Alternately, the rank correlation between discretionary accruals and relative size decile is negative but insignificant in both panels.

In summary, earnings management intended to expropriate wealth from target shareholders to acquirer shareholders is not related to the most obvious proxy for the underlying incentive effects, which is the relative size of target to acquirer firm. Alternately, there is no evidence of earnings management even in the subset of data where the incentives are the strongest. This casts further doubt on the existence of earnings management in the anticipation of mergers and acquisitions.

5.2. Do acquirers expedite merger announcement and completion after presumed earnings management?

Any stock price increase caused by within-GAAP accrual manipulation must be temporary, because the firm merely borrows earnings from future periods. Young (2008) therefore argues that acquiring firms which manage earnings would announce the merger agreement quickly after the earnings announcement. This would enable them to capitalize on the temporarily inflated stock price and avoid the inevitable price deflation with the arrival of subsequent adverse information. We test this prediction by sorting the merger sample into deciles based on discretionary current accruals and payment method and examining the mean number of days from the earnings release date to the merger announcement date.

The results in Figure 2-A do not support this prediction. It appears that stock acquirers are in no hurry to announce their acquisitions following the earnings announcement date. The mean number of days between earnings announcement and merger announcement is a little over 45 for both cash and stock deals. In addition, this duration is not related to the discretionary accrual decile rank in either case.

Of course, the game does not end with the merger announcement. The acquiring firms must sustain their earnings management until completion of the deal. Thus, if discretionary accruals are employed to manage earnings, we should expect that they would be inversely related to the number of days between announcement and completion, especially for stock acquisitions. Figure 2-B tests this prediction. We find that in general it takes significantly longer to complete a stock acquisition than a cash acquisition, possibly because of an extended due diligence and shareholder approval process. However, once again we find no relation between discretionary accruals and the time to completion. The results of Figures 2-A and 2-B suggest that acquiring firms do not expedite merger announcements or rush to complete their stock deals to cash in on presumed earnings management that would only temporarily increase their stock prices.

5.3. Does earnings manipulation via discretionary accruals have a positive stock price effect?

A critical assumption made by earnings management studies is that such accrual management increases stock prices. However, this has not been tested in the contexts of mergers and acquisitions. Below we test this assumption directly by examining the correlation between discretionary accruals and excess returns to acquirer stocks over several windows bracketing the earnings announcement and merger announcement dates. In an efficient market the correlation would be insignificant as the market would discount the impact of discretionary accruals. In contrast, the very existence of earnings management depends on an assumed positive correlation.

Empirical evidence regarding this issue is mixed in prior studies. In the context of the accrual anomaly, Xie (2001) finds that the market overestimates the persistence of discretionary accruals and thus overprices them. However, DeFond and Park (2001) show that the earnings response coefficient (ERC) is higher (lower) when discretionary accruals suppress (accentuate) the magnitude of a positive earnings surprise. This suggests that the market partially adjusts for the possibility of accruals management on the earnings announcement date. Baber, Chen, and Kang (2006) confirm the DeFond and Park results and document that the ability of market participants to detect earnings management is enhanced when firms elect to provide supplementary balance sheet and cash flow disclosure at earnings announcement.

Table 1.8 arranges our sample of cash and stock acquirers into deciles sorted on discretionary current accruals computed using the balance sheet method in Panel A and cash flow method in Panel B. Both panels employ the ROA and sales growth adjusted matching procedure. We examine the market-adjusted excess returns to cash and stock acquirers over several windows, EAD-1 to EAD+1, QEND-11 to MAD-2, and MAD-1 to MAD+1, where EAD is the earnings announcement date, MAD is the merger announcement date, and QEND is the fiscal quarter end date.

We find no evidence that large income-increasing discretionary accruals increase the acquirer stock price over any of these three windows. On the contrary, the rank correlation between discretionary accrual deciles and market adjusted excess returns is negative over EAD-1 to EAD+1, the three-day window around earnings announcement. This result is consistent with prior literature which documents that the market partially discounts earnings numbers associated with income increasing discretionary accruals (DeFond and Park, 2001; Baber, Chen, and Kang, 2006).

We next focus on the window that starts eleven days prior to the fiscal quarter end date and stops two days prior to the merger announcement (QEND-11 to MAD-2). This window should capture a large part of the effect of earnings announcement and earnings management. Skinner and Sloan (2002) argue that 75% of the earnings preannouncements occur within two weeks on either side of the fiscal quarter end, so this window should capture a large part of the associated price effects. As a limitation, however, it is also a rather long window (average 55 trading days). In both panels the mean excess return to stock acquirers over QEND-11 to MAD-2 is greater than the mean excess return to cash acquirers in all 10 cases. However, the rank correlation between discretionary accrual decile and the mean excess return is insignificant for both cash and stock acquirers. Finally, following an extensive literature in finance, we document that the mean excess returns to cash deals is greater than the mean excess returns to stock deals over MAD-1 to MAD+1 in seven out of ten cases in Panel A and eight out of ten cases in Panel B.

The evidence in Table 1.8 has two simultaneous interpretations. First, larger income increasing discretionary accruals do not increase stock prices, at least preceding mergers and acquisitions. Thus, the overvaluation *f* stemming from earnings management in Section 5.1 may be indistinguishable from zero. Second, higher stock returns preceding merger announcement are associated with stock acquisitions, regardless of the reasons for such returns. In other words, the evidence in this section is simultaneously consistent with the Shleifer and Vishny (2003) hypothesis, that premium stock valuations lead managers to make stock acquisitions, and inconsistent with the earnings management hypothesis, that earnings management causes the premium stock valuations.

6. Fraudulent financial reporting and acquisitions

Earnings management by strategically inflating accruals does not necessarily entail a violation of GAAP. On the other hand, fraudulent financial reporting is a clear violation of GAAP. While tests that utilize discretionary accrual models attempt to isolate firms that are more likely to have managed earnings, tests that utilize firms that have SEC enforcement actions against them are known to have committed fraud. They represent the extreme tail of the earnings management distribution.

Beasley, Carcello, and Hermanson (1999) (the COSO report) identify over 200 cases of financial statement fraud during 1987 to 1997 using AAERs.[11] They find that

[11] COSO stands for Committee of the Sponsoring Organizations of the Treadway Commission. As defined before, AAER stands for Accounting and Auditing Enforcement Release.

relative to the average public firm, most companies committing financial fraud are small with total assets less than $100 million. Moreover, these companies are experiencing net losses or barely breaking even and the fraud is motivated by financial distress (i.e., when their survival is at stake). They also find that there are severe consequences for the firm and the individuals involved with the fraud. The discovery of the fraud often leads to bankruptcy, significant changes in ownership, delisting from exchanges, and financial penalties for the firm. In addition, the senior executives involved with the fraud may be terminated or forced to resign and face personal penalties, class action lawsuits, and reduced external employment opportunities. Thus, the incentives of acquiring firms and managers to inflate earnings to affect the company's stock price should be considered against the high costs that they would bear if the fraud is revealed.

We investigate whether firms that were identified by the SEC as having committed accounting fraud are more likely to have made stock acquisitions. We start with the comprehensive sample of AAERs during 1989 to 2003 analyzed by Jones, Krishnan, and Melendrez (2008). We include all cases from their sample which satisfy the following criteria: 1. Firm can be identified on the CRSP and Compustat databases. 2. Enough information exists to identify the period of fraud in the AAERs, so that we can construct a sample of fraud-years. We next merge this sample of fraud-years with the aggregate sample of firm-years from the CRSP and Compustat databases. We require information on ROA, total assets, whether the firm had a Big 4 auditor, leverage, proportion of tangible assets, and current-year and lagged-year market returns. These variables are defined in the legend to Table 1.9. The net result is a sample of 261 fraud-years in an aggregate sample of 59,552 Compustat firm-years.

We report three tests with the sample of fraud-years. First, we merge the sample of fraud-years with our primary sample of acquisitions used in all previous tests. We find a small intersection. Only seven cash acquisitions made by five different firms and 11 stock acquisitions made by seven different firms in our sample occur during fraud years.

However, one limitation of this test is that our primary sample requires enough accounting information to calculate discretionary accruals of acquiring firms. Thus, many mergers and acquisitions made by fraud firms may not appear in our primary sample as their accounting information may be missing.

To overcome this limitation, we obtain a fresh list of announced acquisitions that were subsequently completed from the SDC database. As elsewhere in this paper, we only retain all-cash and all-stock acquisitions. The acquisition dummy equals one for a particular firm-year if it is associated with at least one acquisition, and zero otherwise. Similarly, the stock acquisition dummy equals one for a particular firm-year if the firm is associated with at least one stock acquisition in that year, and zero otherwise. The corresponding relative size variables are defined as the transaction value of acquisitions normalized by the market value of acquirer.

The second test compares the frequency of cash and stock acquisitions made by the 261 fraud-years and the 59,552 Compustat firm-years. Panel A of Table 1.9 shows that there is a time trend in the frequency of fraud years, with number of cases peaking during 1997 to 2000. Not surprisingly, these years were characterized by high stock valuations, especially of technology firms with a lower proportion of tangible assets. These were also the years characterized by a higher than average frequency of stock acquisitions relative to cash acquisitions. We next compare the observed frequency of acquisitions made during fraud-years with the implied frequency calculated from the aggregate sample of all firm-years.[12] The observed frequency is 24 cash and 25 stock acquisitions announced during 261 fraud years, and the implied frequency is 16.6 cash

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[12] The implied frequency is calculated each year as follows. During 1998, there were 413 cash and 303 stock acquisitions made by 4,723 firm years. Given a frequency of 31 frauds during 1998, the implied frequency of cash and stock acquisitions made by fraud firms equals 31×413/4,723 = 2.7 and 31×303/4,723 = 2.0, reported in the last two columns of Table 9.

and 13.3 stock acquisitions. It appears that fraud firms have higher than average frequency of both types of acquisitions. As a limitation, this is a univariate test, and its statistical significance is unknown.

The third test is a logistic regression to overcome this limitation. The dependent variable in Panel B of Table 1.9 takes the value of one for fraud-years, and zero otherwise. The control variables include all variables used by Jones, Krishnan, and Melendrez (2008) in a similar context, plus the proportion of tangible assets (to capture the technology stock effect) and current-year and lagged-year market returns (to capture market-wide valuations). We find that all control variables are significant (except current-year market return). However, our focus is on the acquisition variables. In Model (7) the acquisition dummy is significant at 5% level, but the stock acquisition dummy is insignificant. In Model (8) both of the associated relative size variables are insignificant.

Overall, the tests of this section provide some evidence that firms identified as having committed accounting fraud had higher than average acquisition activity. However, there is no significant evidence to suggest that this acquisition activity was more in the form of stock acquisition activity. The evidence is weak, and it is based on a small number of fraud-years. Moreover, it does not hold with the relative size specification that underlies our analysis of acquirer incentives to manipulate earnings.

6. Conclusions

This paper re-examines the evidence in favor of earnings management by stock acquirers as documented by Erickson and Wang (1999), Louis (2004), Baik, Kang, and Morton (2007), Botsari and Meeks (2008), and Gong, Louis, and Sun (2008b). Using a sample of 2,614 cash and stock acquirers during 1989 to 2005, we show that their results are driven by an incomplete specification of the discretionary accrual model. In particular, these studies do not adjust for the effect of sales growth on accruals. However, a substantial literature argues that growth firms require an extra investment in working capital, so not adjusting for sales growth would result in fictitiously high discretionary accruals for stock acquirers relative to cash acquirers. This occurs because, on average, stock acquirers have a higher sales growth rate than cash acquirers as well as the aggregate sample of Compustat firms.

We employ a simulation framework similar to Kothari, Leone, and Wasley (2005) to test the specification and power of two discretionary accrual models using quarterly data. The first model calculates ROA adjusted discretionary accruals similar to previous literature, and the second model calculates ROA and sales growth adjusted discretionary accruals proposed by us. Simulation results show that the first model is considerably misspecified, especially when combined with a balance sheet based accrual measure. It gives higher than expected rejection rates in both high-growth and low-growth partitions formed by sales growth, employee growth, and book-to-market criteria. In comparison, the second model is both well-specified and powerful. We recommend using this model to detect earnings management around corporate events, combined with a cash flow statement based accrual measure. This is especially important when the partitioning variable is correlated with firm growth.

Using the new ROA and sales growth adjusted discretionary accrual model, we find no evidence of earnings management by stock acquirers. Several additional tests support this conclusion. First, we find an insignificant correlation between the acquirer incentives to manipulate earnings and discretionary accruals of stock acquirers. Even in the partition characterized by stronger incentives, discretionary accruals remain insignificant. Second, we find no evidence that higher discretionary accruals lead to a higher acquirer stock price as measured by the market reaction surrounding the earnings announcement preceding the acquisition announcement. Third, we find that stock acquirers are in no hurry to acquire following the earnings announcement, regardless of their discretionary accruals, and take longer than cash acquirers to complete the transaction.

In departure from the above tests, our final test shows some evidence that firms identified as having committed accounting fraud by the SEC have higher than average acquisition activity. However, this higher activity consists of both cash and stock acquisitions, and given a small sample the results are weak. Overall, we conclude that there is no evidence of earnings management that can be attributed to the stock acquisition decision.

We offer several reasons to explain why stock acquirers may not indulge in earnings management. First, it is not entirely clear that earnings management by acquirer firms would go unnoticed by managers and investment bankers of target firms, especially when it is on a large scale. Second, using reasonable parameter values we estimate that the acquirer incentives to manipulate earnings are low. Third, the incentives of acquiring firms and managers to inflate earnings in order to manipulate the stock price must be weighed against the high costs that they would bear if the manipulation is revealed. Karpoff, Lee, and Martin (2008a) document that fully 93% of individuals responsible for fraud in the SEC and Department of Justice enforcement actions lose their jobs. In addition, 28% of them face criminal charges and penalties including jail sentences that average 4.3 years. Karpoff, Lee, and Martin (2008b) further document that the firms involved in such enforcement actions lose market value that is several times higher than the price increase occurring at the time of manipulation. These penalties may have a deterrent effect. In summary, while there is no denying that cases of earnings management and fraud come up from time to time, the problem is not as wide-spread as documented in the previous literature on mergers and acquisitions.

Table 1.1 Sample distribution

Panel A: Sample distribution by target status and form of payment

Target type	All targets	Cash	Stock
All Targets	2614	1719	895
Public	717	347	370
Private	1106	651	455
Subsidiary	791	721	70

Panel B: Sample distribution over time

	1989	1990	1991	1992	1993	1994	1995	1996	1997
Cash	1	18	22	28	53	70	73	99	131
Stock	0	9	18	30	20	50	84	98	84
Total	1	27	40	58	73	120	157	197	215
	1998	1999	2000	2001	2002	2003	2004	2005	Total
Cash	151	130	126	104	140	155	211	207	1719
Stock	104	125	121	58	22	30	22	20	895
Total	255	255	247	162	162	185	233	227	2614

Note: Our sample includes all acquisitions from the SDC database that meet the following criteria: 1. The acquisition is announced during 1989-2005. 2. The target is a public, private, or subsidiary firm domiciled in the U.S. 3. The acquirer is a public firm included in the CRSP and Compustat databases with ordinary common shares outstanding (CRSP share code 10 or 11, which excludes ADRs, REITs, units, certificates, and trusts). 4. The acquirer is not a financial firm. 5. The transaction value exceeds $10 million. 6. The transaction form is categorized as deal type 1 (disclosed value mergers and acquisitions). 7. The acquirer owns less than 50% of target shares before announcement and 100% of target shares after completion. 8. The form of payment is all cash or all stock (thus, mixed payment cases are excluded). 9. The acquirer has relevant data on Compustat quarterly file to calculate performance and sales growth adjusted abnormal accruals using the balance sheet and cash flow methods. 10. We delete the top and bottom one percentiles of current accruals, lagged current accruals, change in sales, and change in accounts receivables to mitigate the influence of outliers. (These variables enter the modified Jones model, which is described in Table 1.3.) In addition, we delete the top and bottom one percentiles of estimated discretionary accruals after adjusting them for performance.

Table 1.2 Summary statistics of data

	All targets			Public		Private		Subsidiary	
	Cash	Stock	All	Cash	Stock	Cash	Stock	Cash	Stock
Acquirer total assets ($ mil)	909	469	725	2314	800	576	300	993	309
Acquirer market value ($ mil)	1227	1174	1208	2831	1491	783	1038	1236	653
Transaction value ($ mil)	65	84	70	183	219	45	47	65	47
Relative size	0.068	0.079	0.072	0.071	0.210	0.057	0.042	0.075	0.081
Acquirer prior-year market-adjusted excess returns (%)	12.4	67.6	31.3	5.4	45.3	16.0	89.4	12.5	43.6
Acquirer return on assets (%)	1.63	1.91	1.70	1.77	1.69	1.74	2.24	1.47	1.28
Acquirer book-to-market	0.39	0.24	0.34	0.34	0.26	0.39	0.20	0.40	0.33
Acquirer sales growth (%)	12.1	38.5	17.0	9.4	30.4	15.5	46.0	11.5	30.2
Acquirer employee growth (%)	5.7	18.8	8.9	3.8	13.0	7.2	25.0	6.1	11.2
N	1719	895	2614	347	370	651	455	721	70

Note: Table 1.1 describes the sample of acquisitions. We obtain the acquirer total assets and market value from Compustat as of the end of quarter *t*-1 (defined as the last quarter for which the earnings announcement date precedes the merger [or acquisition] announcement date MAD). We obtain the transaction value from SDC and calculate the relative size as the transaction value divided by the acquirer market value. We calculate the acquirer return on assets (ROA) as the net income divided by total assets for quarter *t*-1, the book-to-market as the book value divided by the market value of equity as of the end of quarter *t*-1, and the sales growth as the percent change in sales from quarter *t*-5 to *t*-1. However, since the number of employees is available only from annual file, the percent employee growth is calculated as the percent change in the number of employees over a one-year period ending with the last fiscal year ending before the acquisition announcement date. The reported values for all of these variables are sample medians. The prior year excess return is calculated by subtracting the cumulative market return from the cumulative stock return over a 254-day period ending on MAD-2. The reported values for prior-year excess returns only are sample means.

Table 1.3 Traditional measures of discretionary accruals calculated using ROA-adjusted benchmarks for the sample of acquirers

Quarter	Cash acquirers N	Cash acquirers Median accruals [1] (*p*-value) [2]	Stock acquirers N	Stock acquirers Median accruals (*p*-value) [3]	Difference between medians (*p*-value) [4]
Panel A: ROA-adjusted discretionary accruals calculated using the balance sheet method					
A.1. All targets					
t-2	1544	-0.032 (0.562)	721	0.234 (0.018)**	0.266 (0.102)
t-1	1719	0.067 (0.435)	895	0.397 (0.000)***	0.330 (0.000)***
t	741	0.006 (0.876)	512	0.355 (0.000)***	0.349 (0.003)***
A.2. Public targets					
t-2	313	-0.140 (0.160)	306	-0.063 (0.561)	0.077 (0.342)
t-1	347	-0.160 (0.189)	370	0.273 (0.060)*	0.433 (0.029)**
t	237	0.060 (0.826)	288	0.267 (0.018)**	0.207 (0.052)**
A.3. Private targets					
t-2	577	-0.090 (0.761)	357	0.369 (0.022)**	0.459 (0.075)*
t-1	651	0.105 (0.158)	455	0.571 (0.000)***	0.466 (0.014)**
t	182	-0.090 (0.460)	187	0.586 (0.013)**	0.676 (0.017)**
A.4 Subsidiary targets					
t-2	654	0.032 (0.131)	58	0.374 (0.206)	0.342 (0.384)
t-1	721	0.080 (0.487)	70	0.523 (0.065)*	0.443 (0.107)
t	322	0.028 (0.315)	37	0.311 (0.353)	0.283 (0.600)
Panel B: ROA-adjusted discretionary accruals calculated using the cash flow method					
B.1. All targets					
t-2	1544	0.052 (0.237)	721	-0.016 (0.665)	-0.068 (0.769)
t-1	1719	0.103 (0.066)*	895	0.243 (0.007)***	0.140 (0.099)*
t	741	0.067 (0.549)	512	0.096 (0.517)	0.029 (0.814)
B.2. Public targets					
t-2	313	0.012 (0.495)	306	-0.227 (0.413)	-0.239 (0.714)
t-1	347	0.003 (0.610)	370	0.221 (0.050)**	0.218 (0.067)*
t	237	0.102 (0.962)	288	0.176 (0.204)	0.074 (0.373)
B.3. Private targets					
t-2	577	0.038 (0.446)	357	0.206 (0.282)	0.168 (0.571)
t-1	651	0.145 (0.128)	455	0.231 (0.183)	0.086 (0.664)
t	182	-0.036 (0.280)	187	-0.088 (0.802)	-0.052 (0.604)
B.4 Subsidiary targets					
t-2	654	0.104 (0.115)	58	0.259 (0.633)	0.155 (0.978)
t-1	721	0.124 (0.079)*	70	0.416 (0.106)	0.292 (0.157)
t	322	0.111 (0.096)*	37	-0.240 (0.519)	-0.351 (0.257)

Note: Table 1.1 describes the sample of acquisitions. The calculation of performance-adjusted discretionary accruals follows a three-step process. First, we compute current accruals from the balance sheet as *ΔCA-ΔCL-ΔCASH+ΔSTDEBT*, where *ΔCA* is the change in current assets during quarter *t* (Compustat quarterly item 40), *ΔCL* is the change in current liabilities (item 49), *ΔCASH* in the change in cash and cash equivalents (item 36), and *ΔSTDEBT* is the current maturities of long-term debt and other short-term

Table 1.3 Continued

debt included in current liabilities (item 45). In addition, we compute current accruals from the cash flow statement as $EBXI - CFO_{cf} + DEP_{cf}$, where $EBXI$ is earnings before extraordinary items and discontinued operations (item 76), CFO_{cf} is the operating cash flow taken directly from the cash flow statement (item 108) adjusted for the cash portion of discontinued operations and extraordinary items (item 78), and DEP_{cf} is depreciation and amortization (item 77). Second, we estimate discretionary current accruals using both the balance sheet and the cash flow statement methods as the residual from a modified Jones model applied to cross-sectional quarterly data for all firms belonging to the same two-digit SIC code and calendar quarter:

$$Current\ ACC_{i,\tau} = \beta_1 Q_{1,i,\tau} + \beta_2 Q_{2,i,\tau} + \beta_3 Q_{3,i,\tau} + \beta_4 Q_{4,i,\tau} + \beta_5 (\Delta SALES_{i,\tau} - \Delta AR_{i,\tau}) + \beta_6 Current\ ACC_{i,\tau-4} + \varepsilon_{i\tau}\ .$$

In this expression, subscript i denotes the firm and τ denotes the quarter. $Q_{1,i} - Q_{4,i}$ are the fiscal quarter dummies, $\Delta SALES_i$ is the quarterly change in sales, ΔAR_i is the quarterly change in accounts receivables, and $Current\ ACC_{i,\tau-4}$ is the current accrual from the same quarter in the preceding year. All variables are scaled by lagged total assets. Third, we calculate the ROA-adjusted discretionary accruals using both the balance sheet and the cash flow statement methods. These equal the difference between discretionary accruals for the sample firm and the median discretionary accruals for the quintile of same-industry firms matched by ROA during the fourth quarter before the current quarter. Quarter t-1 in the first column of this table is the quarter with an earnings announcement date immediately preceding the merger announcement date and it includes the full sample. However, the quarter t sample includes only acquisitions that were announced but not completed before the next earnings announcement date. The notations [*], [**], and [***] denote significance at the 10%, 5%, and 1% levels.

[1] All accruals are reported in percent form. Thus, an accrual of 0.123 means 0.123% of lagged assets (and not 0.123 times lagged assets).

[2,3] The p-value is based on Wilcoxon sign-rank test and examines whether the median discretionary accrual is significantly different from zero.

[4] The p-value is based on Wilcoxon rank-sum test and examines whether the median discretionary accrual for stock acquirers is significantly different from the median discretionary accrual for cash acquirers.

Table 1.4 A comparison of the specification and power of the tests using ROA adjusted discretionary accruals versus ROA and sales growth adjusted discretionary accruals

Seed discretionary accrual (%)	All firms	Sales growth		Employee growth		Book-to-market		Market value	
		High	Low	High	Low	High	Low	High	Low
Median value of partitioning variable		36.3%	-12.1%	23.9%	-11.9%	1.03	0.20	$7.6b	$65m
Panel A: Test H_0: Discretionary accruals = 0 versus H_{A1}: Discretionary accruals < 0, Balance sheet method									
A.1. Rejection rates for ROA-adjusted discretionary accruals									
0.0	6.4	***0.0***	**64.8**	***0.0***	**32.8**	**19.2**	***0.8***	**8.4**	5.2
-0.5	42.8	4.4	98.0	7.6	85.6	67.2	18.4	70.4	42.0
-1.0	89.6	33.6	100.0	53.2	99.6	94.8	70.8	99.6	86.0
A.2. Rejection rates for ROA and sales growth adjusted discretionary accruals									
0.0	**8.4**	4.8	5.6	2.8	**10.0**	**10.4**	3.6	**9.2**	4.0
-0.5	38.8	37.6	44.4	25.6	55.2	46.0	33.2	70.0	31.6
-1.0	83.6	82.8	86.4	73.2	93.6	88.0	82.4	98.4	78.8
Panel B: Test H_0: Discretionary accruals = 0 versus H_{A2}: Discretionary accruals > 0, Balance sheet method									
B.1. Rejection rates for ROA-adjusted discretionary accruals									
0.0	**8.4**	**40.8**	***0.0***	**29.6**	***0.0***	2.0	**14.4**	4.8	2.8
0.5	38.0	99.2	0.4	79.6	4.0	17.2	62.8	55.6	29.2
1.0	86.8	99.2	18.8	98.8	44.0	62.4	98.0	98.8	78.0
B.2. Rejection rates for ROA and sales growth adjusted discretionary accruals									
0.0	6.4	5.6	2.4	**10.0**	***0.8***	4.0	5.6	2.0	5.6
0.5	36.0	30.0	30.4	48.8	19.2	27.6	41.6	43.6	35.2
1.0	82.8	82.0	79.6	91.2	68.0	75.6	86.4	93.6	79.2
Panel C: Test H_0: Discretionary accruals = 0 versus H_{A1}: Discretionary accruals < 0, Cash flow method									
C.1. Rejection rates for ROA-adjusted discretionary accruals									
0.0	7.2	***0.4***	**57.2**	***0.4***	**28.8**	**20.0**	3.2	7.6	**8.8**
-0.5	48.4	8.8	95.6	14.4	78.8	68.8	38.0	78.4	47.6
-1.0	91.2	50.0	100.0	71.2	99.6	98.4	84.4	100.0	92.4
C.2. Rejection rates for ROA and sales growth adjusted discretionary accruals									
0.0	5.2	6.0	8.0	***1.2***	**10.4**	6.0	7.2	**10.0**	6.4
-0.5	38.0	40.4	44.0	31.6	47.2	47.2	48.4	72.4	34.0
-1.0	87.2	84.0	86.0	84.4	91.6	90.4	88.0	99.6	84.0
Panel D: Test H_0: Discretionary accruals = 0 versus H_{A2}: Discretionary accruals > 0, Cash flow method									
D.1. Rejection rates for ROA-adjusted discretionary accruals									
0.0	5.2	**26.4**	***0.0***	**16.8**	***0.0***	***0.0***	7.2	3.6	2.4
0.5	38.4	89.6	0.4	66.0	6.0	15.2	42.8	54.8	25.6
1.0	85.6	98.4	20.8	98.4	49.6	62.4	89.6	98.4	76.4
D.2. Rejection rates for ROA and sales growth adjusted discretionary accruals									
0.0	4.0	3.6	3.2	8.0	***0.8***	***1.6***	3.6	2.4	2.4
0.5	37.2	32.4	29.2	36.4	23.2	25.2	26.8	47.6	26.8
1.0	83.2	77.6	73.2	87.6	72.8	73.2	76.4	95.2	79.6

Note: This table follows the methodology outlined in Kothari, Leone, and Wasley (2005). For each sample the indicated seed level (as percent of total assets) is added to current accruals before estimating the respective discretionary accrual model. The table reports the percent of 250 samples of 100 firms each where the null hypothesis of zero discretionary accruals is rejected at the 5% level (upper and lower one-tailed tests). For the case of zero seed, numbers higher than 8.0 or lower than 2.0 represent a rejection rate significantly different from 5% and represent Type I error (shown in bold). The significance of the mean discretionary accrual of each sample is based on a cross-sectional *t*-test. The samples are drawn from the population of all Compustat firm-quarters during 1989-2005 for which both the ROA-adjusted and ROA and sales growth adjusted accruals can be calculated as described in Sections 2 and 4.2. High and low partitions based on sales growth, employee growth, book-to-market, and market value are

Table 1.4 Continued

identified following the Fama-French procedure. Specifically, we arrange all NYSE-listed firms with available data into quartiles each calendar quarter. We use the cutoffs corresponding to top and bottom quartiles to identify high and low partitions from the population of Compustat firm-quarters. Regions where stock acquirers are likely to be found are shaded in gray. Section 3 gives the definitions of partitioning variables.

Table 1.5 Relation between ROA adjusted and ROA and sales growth adjusted discretionary accruals

Independent variables	Balance sheet method			Cash flow method		
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	-0.016	-0.022	0.023	-0.007	-0.025	0.005
	(-2.77)[c]	(-3.75)[c]	(4.03)[c]	(-1.17)	(-4.35)[c]	(0.88)
ROA and sales growth adjusted discretionary accruals	0.853	0.852	0.855	0.859	0.858	0.861
	(816.94)[c]	(765.82)[c]	(784.86)[c]	(852.03)[c]	(805.77)[c]	(825.84)[c]
High sales growth dummy	0.495			0.369		
	(58.83)[c]			(45.60)[c]		
Low sales growth dummy	-0.531			-0.497		
	(-59.60)[c]			(-58.16)[c]		
High employee growth dummy		0.265			0.192	
		(29.81)[c]			(22.69)[c]	
Low employee growth dummy		-0.225			-0.214	
		(-23.65)[c]			(-23.67)[c]	
High book-to-market dummy			-0.158			-0.143
			(-18.15)[c]			(-17.23)[c]
Low book-to-market dummy			0.128			0.084
			(13.66)[c]			(9.46)[c]
N	131,983	125,739	131,983	131,983	125,739	131,983
Adjusted R^2	0.838	0.825	0.824	0.849	0.839	0.838

Note: The sample includes all Compustat firm-quarters during 1989-2005 for which both the ROA adjusted and ROA and sales growth adjusted discretionary accruals can be calculated. The dependent variable in all six regressions is the ROA adjusted discretionary accrual. The independent variables include the ROA and sales growth adjusted accrual and several partition dummies that take the value of one in the indicated high-growth or low-growth partition, and zero otherwise. (The intercept plays the role of medium-growth partition.) High-growth and low-growth partitions based on sales growth, employee growth, book-to-market, and market value are identified following the Fama-French procedure. Specifically, we arrange all NYSE-listed firms with available data into quartiles each calendar quarter. We use the cutoffs corresponding to the top and bottom quartiles to identify the high and low partitions from the population of Compustat firm-quarters. Sections 2 and 4.2 describe the calculation of discretionary accrual measures and Section 3 gives the definitions of partitioning variables. The figures in parentheses represent the *t*-statistics. The notations [a], [b], and [c] denote significance at the 10%, 5%, and 1% levels.

[1] All accruals are transformed to percent form before regression. Thus, a coefficient of 0.495 for the high sales growth dummy in Regression (1) represents 0.495% of lagged assets (and not 0.495 times lagged assets).

Table 1.6 Improved measures of discretionary accruals calculated using ROA and sales growth adjusted benchmarks for the sample of acquirers

	Cash acquirers		Stock acquirers		
Quarter	N	Median accruals [1] (p-value) [2]	N	Median accruals (p-value) [3]	Difference between medians (p-value) [4]
Panel A: ROA and sales growth adjusted discretionary current accruals calculated using the balance sheet method					
A.1. All targets					
t-2	1510	-0.100 (0.917)	717	-0.099 (0.397)	0.001 (0.326)
t-1	1719	-0.023 (0.859)	895	0.166 (0.182)	0.189 (0.190)
t	722	0.057 (0.862)	507	0.172 (0.235)	0.115 (0.360)
A.2. Public targets					
t-2	302	-0.162 (0.339)	303	-0.258 (0.314)	-0.096 (0.787)
t-1	347	-0.241 (0.059)*	370	-0.102 (0.963)	0.139 (0.324)
t	230	0.063 (0.969)	287	0.150 (0.377)	0.087 (0.556)
Panel B: ROA and sales growth adjusted discretionary current accruals calculated using the cash flow method					
B.1. All targets					
t-2	1510	-0.008 (0.351)	717	-0.305 (0.051)*	-0.297 (0.016)**
t-1	1719	0.057 (0.360)	895	0.072 (0.712)	0.015 (0.918)
t	722	0.015 (0.486)	507	-0.109 (0.357)	-0.124 (0.232)
B.2. Public targets					
t-2	302	-0.077 (0.530)	303	-0.344 (0.064)*	-0.267 (0.219)
t-1	347	-0.033 (0.372)	370	0.040 (0.544)	0.073 (0.340)
t	230	0.102 (0.825)	287	-0.032 (0.908)	-0.134 (0.814)

Note: This table tests for the presence of earnings management in the sample of acquirers using ROA and sales growth adjusted discretionary accruals. (It contrasts with Table 1.3 that does the same with only ROA adjusted discretionary accruals.) We compute the discretionary accruals for sample firms using the first two steps of the procedure described in Table 1.3. We then compute annual sales growth rate for each firm-quarter observation from four quarters before to the current quarter. The ROA and sales growth adjusted discretionary accruals equal the difference between discretionary accruals for the sample firm and the median discretionary accruals for the matched portfolio. The matched portfolio includes all same-industry firms that belong to the same ROA quintile during the fourth quarter before the current quarter as a sample firm, and the same annual sales growth tercile. Table 1.1 describes the sample of acquisitions. The notations *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table 1.7 Are relatively large stock acquisitions preceded by higher discretionary accruals as predicted by the incentives theory of earnings management?

Relative size decile	N	Median relative size	Median wealth transfer to acquirer shareholders assuming $f = 0.15$	Median ROA and sales growth adjusted discretionary accruals: Balance sheet method[1]	Median ROA and sales growth adjusted discretionary accruals: Cash flow method
Panel A: Stock acquisitions of all targets					
1	88	0.002	0.000	-0.440	-0.726
2	78	0.010	0.001	-0.020	-0.245
3	90	0.023	0.003	0.847	0.743
4	85	0.036	0.005	0.787	0.689
5	86	0.057	0.008	0.793	0.230
6	68	0.087	0.012	-0.178	0.288
7	79	0.129	0.017	0.075	0.445
8	89	0.208	0.026	-0.388	-0.109
9	101	0.365	0.040	-0.105	-0.193
10	131	0.898	0.071	0.158	-0.321
Rank correlation with relative size decile				-0.030	-0.067
Deciles 1-5	495	0.027	0.004	0.312	0.163
Deciles 6-10	400	0.319	0.036	-0.100	-0.025
Difference				-0.412	-0.188
Panel B: Stock acquisitions of public targets					
1	20	0.003	0.000	0.636	0.220
2	22	0.011	0.002	-0.177	0.084
3	16	0.020	0.003	0.141	-0.769
4	21	0.039	0.006	1.443	1.442
5	21	0.056	0.008	-0.388	0.203
6	31	0.086	0.012	-0.141	0.437
7	27	0.129	0.017	-0.385	-0.020
8	52	0.207	0.026	-0.410	0.126
9	69	0.366	0.040	-0.118	0.007
10	91	0.848	0.069	0.013	-0.412
Rank correlation with relative size decile				-0.345	-0.333
Deciles 1-5	131	0.036	0.005	0.001	0.301
Deciles 6-10	239	0.395	0.042	-0.169	-0.193
Difference				-0.170	-0.494

Note: Table 1.1 describes the sample of acquisitions, and Tables 1.3 and 1.6 describe the methodology used to compute ROA and sales growth adjusted discretionary accruals. This table tests whether discretionary accruals (our proxy for earnings management) are larger in acquisitions which have a higher relative size (transaction value divided by market value of the acquirer) as predicted by the incentives theory of earnings management. Following the model described in Appendix 1, the wealth transfer from target to acquirer shareholders is given by $\frac{f \times Relative\ Size}{1+Relative\ Size}$ where f is the percentage overvaluation of the acquirer stock as a result of earnings management. We choose one

Table 1.7 Continued

scenario of f = 15% for an illustration of wealth transfer. Panel A consider the aggregate sample of all targets, and Panel B considers the subset of public targets. Both panels report the median ROA and sales growth adjusted discretionary current accruals using both the balance sheet method and the cash flow method. The Spearman rank correlation coefficient is used to test whether higher relative size deciles are associated with larger accruals. The notations *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table 1.8 Measuring *f* : The likely stock price increase caused by discretionary accruals for the sample of acquirers

Discretionary accrual decile	N		Market-adjusted excess returns					
			EAD-1 to EAD+1		QEND-11 to MAD-2		MAD-1 to MAD+1	
	Cash	Stock	Cash	Stock	Cash	Stock	Cash	Stock
Panel A: Current accruals using balance sheet method								
1	152	109	1.52	1.56	4.03	10.94	2.09	0.38
2	167	94	0.56	2.22	2.35	12.51	0.09	1.90
3	179	83	0.41	1.07	2.25	5.38	1.30	-0.76
4	184	77	0.07	0.82	3.48	13.36	1.12	-0.46
5	195	67	0.51	3.49	2.93	28.26	2.55	-0.23
6	179	82	0.02	0.78	2.11	15.44	0.65	0.00
7	193	69	1.47	1.13	1.78	2.55	1.46	-1.17
8	165	97	0.00	1.66	2.23	14.74	0.85	0.97
9	166	95	0.29	0.34	1.75	13.45	1.69	3.21
10	139	122	0.70	0.80	3.13	16.53	2.52	-0.64
All	1719	895	0.55	1.34	2.58	13.30	1.42	0.39
Rank correlation with accrual decile			-0.27	-0.48	-0.50	0.48	0.24	-0.04
Panel B: Current accruals using cash flow method								
1	134	127	0.68	3.22	1.69	15.10	1.63	0.76
2	167	94	0.47	0.74	2.53	7.63	1.63	1.50
3	182	80	0.64	2.10	2.81	9.73	0.96	-1.97
4	188	74	0.42	2.26	2.25	16.32	0.54	1.95
5	191	70	1.47	0.27	3.96	11.78	1.36	0.54
6	193	68	1.12	1.69	5.14	14.98	1.59	1.68
7	178	84	0.31	0.21	1.25	10.04	1.50	1.07
8	179	82	0.29	0.31	3.19	18.28	1.75	-0.47
9	159	103	0.43	0.87	1.01	14.99	1.64	-0.99
10	148	113	-0.66	1.02	1.00	13.95	1.80	0.21
All	1719	895	0.55	1.34	2.58	13.30	1.42	0.39
Rank correlation with accrual decile			-0.60*	-0.44	-0.33	0.24	0.53	-0.33

Note: Table 1.1 describes the sample of acquisitions, and Tables 1.3 and 1.6 describe the methodology used to compute ROA and sales growth adjusted discretionary accruals. Excess returns for the acquirer firms are calculated by subtracting cumulative market returns from cumulative stock returns over various windows. Market returns are measured by the CRSP value-weighted index returns. Equally-weighted mean excess returns are reported in each test. EAD refers to the earnings announcement date, MAD to the merger announcement date, and QEND to the fiscal quarter-end date. The short windows EAD-1 to EAD+1 and MAD-1 to MAD+1 measure the announcement returns, and the long window QEND-11 to MAD-2 measures the cumulative effect of earnings management. The Spearman rank correlation coefficient is used to test whether higher discretionary accrual deciles are associated with larger market adjusted price increase over various windows. The notations *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table 1.9 Fraudulent financial reporting and acquisitions

Panel A: Acquisition activity in aggregate sample and the sample of fraudulent firms

	Aggregate sample			Fraudulent firms subject to SEC enforcement action				
Year	Firm years	Cash deals	Stock deals	Fraud years	Observed cash deals	Observed stock deals	Implied cash deals	Implied stock deals
1989	3204	108	54	12	0	0	0.4	0.2
1990	3173	99	50	13	0	0	0.4	0.2
1991	3241	93	73	14	0	1	0.4	0.3
1992	3426	123	99	17	2	1	0.6	0.5
1993	3746	204	116	16	2	0	0.9	0.5
1994	4035	229	175	20	2	1	1.1	0.9
1995	4259	249	244	13	0	4	0.8	0.7
1996	4611	307	313	20	2	2	1.3	1.4
1997	4845	400	339	25	3	5	2.1	1.7
1998	4723	413	303	31	7	2	2.7	2.0
1999	4535	341	359	26	4	1	2.0	2.1
2000	4412	298	346	25	1	7	1.7	2.0
2001	4053	254	141	17	1	1	1.1	0.6
2002	3758	347	67	5	0	0	0.5	0.1
2003	3531	314	63	7	0	0	0.6	0.1
All years	59,552	2,742	3,779	261	24	25	16.6	13.3

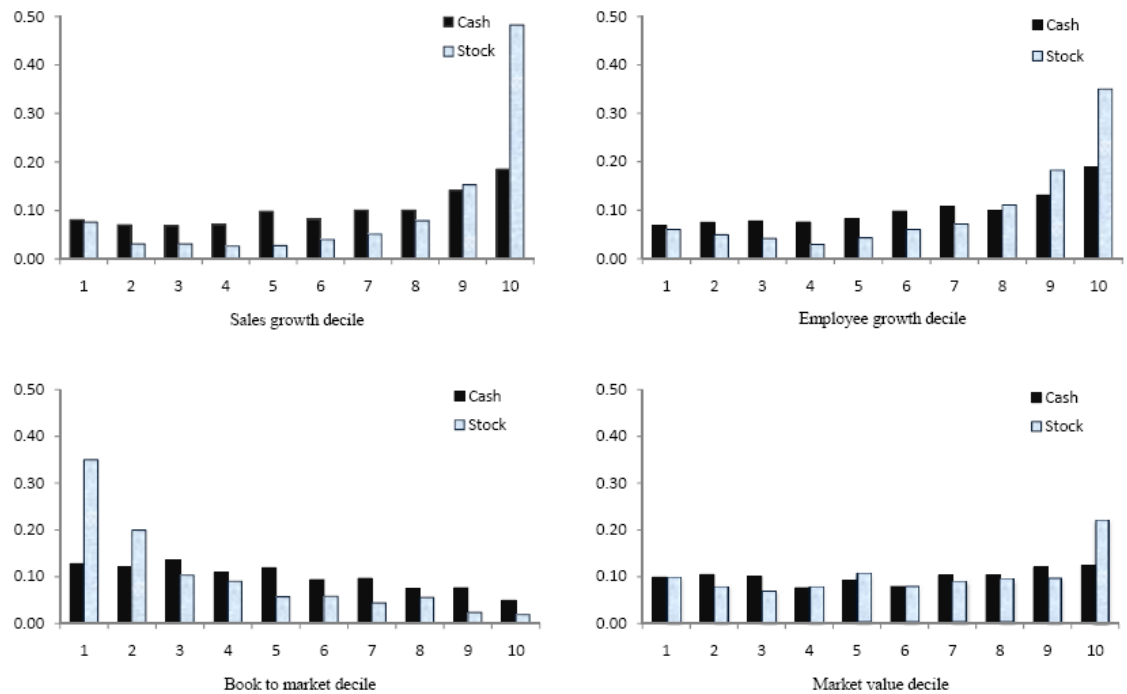
Panel B: Logit model analysis of fraud-year dummy

Variable	(7)	(8)
Intercept	-5.54 (22.02)***	-5.59 (22.27)***
Acquisition dummy	0.49 (2.02)**	
Stock acquisition dummy	0.42 (1.38)	
Relative size of acquisitions		-0.38 (0.54)
Relative size of stock acquisitions		1.20 (1.59)
Return on assets	-1.42 (6.76)***	-1.43 (6.81)***
Log of total assets	0.15 (4.01)***	0.18 (4.83)***
Big 4 auditor	-0.68 (3.70)***	-0.67 (3.67)***
Leverage	0.78 (2.91)***	0.73 (2.75)***
Proportion tangible assets	-1.82 (7.86)***	-1.90 (8.19)***
Current year market return	0.91 (1.41)	0.94 (1.45)
Lagged year market return	1.45 (2.26)**	1.52 (2.37)**
Number of firm-years	59,552	59,552
Number of fraud years	261	261

Note: This table investigates the role of acquisitions in a sample of firms that were identified by the SEC as having committed accounting fraud in *Accounting and Auditing Enforcement Releases* (AAERs) during 1989 to 2003. The initial sample comes from Jones, Krishnan, and Melendrez (2008). We include all cases from their aggregate sample which satisfy the following criteria: 1. The firm can be identified on the CRSP and

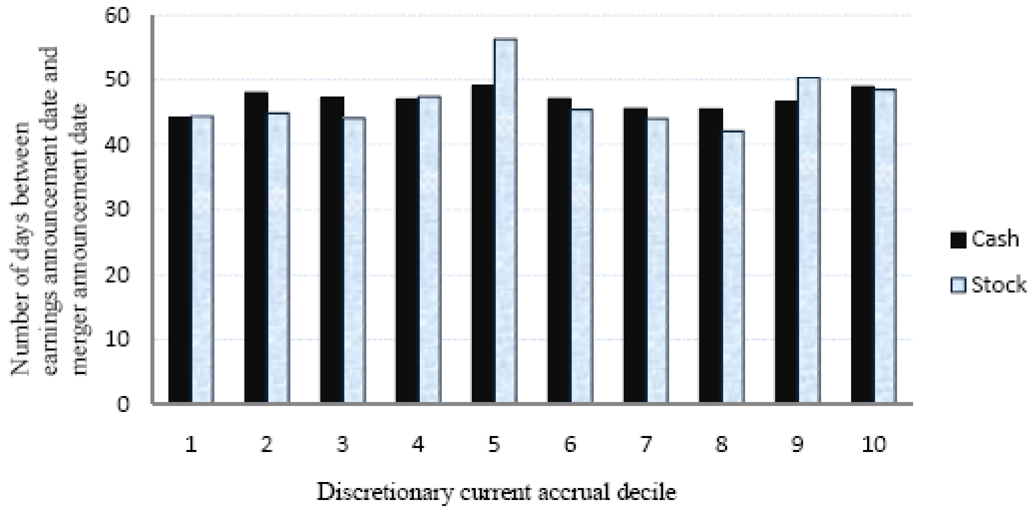
Table 1.9 Continued

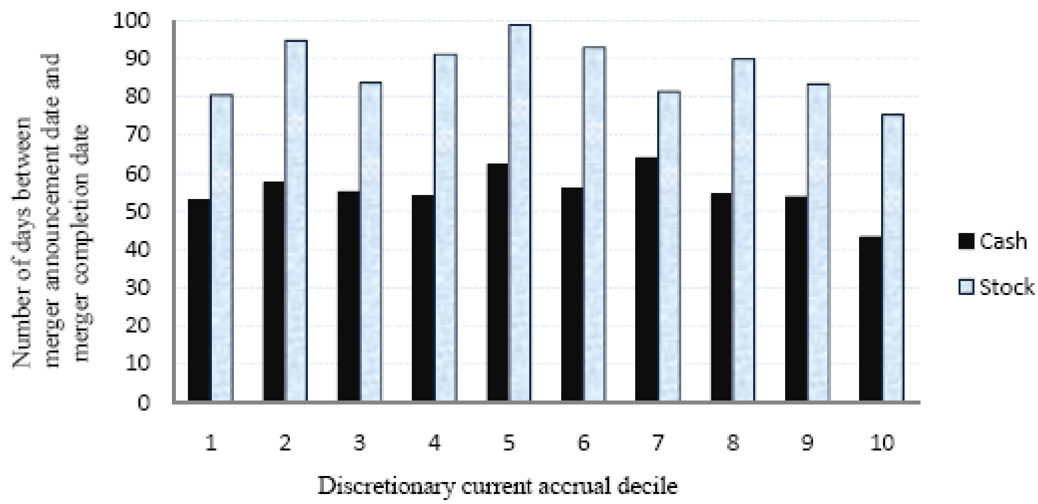
Compustat databases. 2. Enough information exists to identify the period of fraud, so we can construct a sample of fraud-years. We next merge this sample of fraud-years with the aggregate sample of firm-years from the CRSP and Compustat databases. The final sample additionally requires information on the following accounting variables. The return on assets (ROA) equals the net income divided by total assets of the acquirer firm, and the leverage equals total liabilities divided by total assets. The proportional tangible assets equal the property, plant, and equipment divided by total assets, and the Big 4 auditor takes the value one or zero as appropriate. All accounting variables are as of the last year. The current year market return is the cumulative value-weighted market return from the CRSP database over a one-year period ending on June 30 of the current year, and the lagged year market return is over the preceding one-year period. Finally, for each firm-year, we search the SDC database to obtain information on announced acquisitions that were subsequently completed. As elsewhere in this paper, we only retain all-cash and all-stock acquisitions. The acquisition dummy equals one if a firm-year is associated with at least one acquisition, and zero otherwise. The stock acquisition dummy similarly equals one if a firm-year is associated with at least one stock acquisition, and zero otherwise. The relative size variables are defined as the transaction value of acquisitions normalized by the market value of acquirer as of last year-end. Panel A presents frequency of acquisition activity by the year, and Panel B presents a logit analysis of a dummy variable that takes the value one for fraud-years, and zero for all other firm-years. The implied numbers of cash deals and stock deals for the sample of fraud-years in Panel A are calculated by applying the percent frequencies for all firm-years during the corresponding calendar year. The figures in parentheses represent the *t*-statistics. The notations *, **, and *** in Panel B denote significance at the 10%, 5%, and 1% levels.



Note: Table 1.1 describes the sample of acquisitions, and Table 2 defines sales growth, employee growth, book-to-market, and market value. This figure shows the distribution of cash and stock acquirers across deciles formed by these variables. For each variable, we arrange all NYSE-listed firms into deciles during each calendar quarter. We use the resulting cutoffs to identify the NYSE decile rank of each acquirer firm as of the quarter with earnings announcement date preceding the merger announcement date.

Figure 1.1. Distribution of cash and stock acquirers across NYSE deciles.





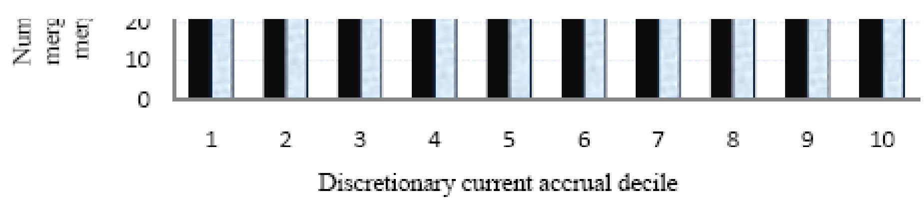


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Figure 1.2. Do acquirers expedite merger announcement and completion after presumed earnings management to capitalize on the temporarily inflated stock price?

CHAPTER 2

DO FIRMS HAVE A TARGET LEVERAGE? EVIDENCE FROM CREDIT MARKETS

1. Introduction

Few issues have attracted as much attention in the finance literature as the choice of capital structure by firms. Ever since Modigliani and Miller (1958) assumed perfect and frictionless capital markets to prove the irrelevance of capital structure, attention has been focused on the role of market imperfections in determining a relevant capital structure. The static tradeoff theory focuses on the benefits and costs of issuing debt and predicts that there is an optimal target debt ratio (or leverage) which maximizes the firm value. The primary benefits arise from the tax deductibility of interest payments to the firm (despite the existence of personal taxes [Miller, 1977; Graham, 2000] and non-debt tax shields [DeAngelo and Masulis, 1980]). The primary costs arise from an increasing probability of financial distress (Modigliani and Miller, 1963) and conflicts between shareholders and debtholders (Jensen and Meckling, 1976). The marginal benefits decrease and the marginal costs increase with increasing debt, which leads to an interior optimum target leverage.[13]

As an implication of the above insights, the static tradeoff theory says that firms with leverage below the target leverage increase their debt level over time to move toward the target leverage. Similarly, firms with leverage above the target leverage decrease their debt level to move toward the target leverage. However, a variety of market frictions make this a slow process.

Given the importance of understanding the capital structure decision, many empirical studies have examined the static tradeoff theory. Earlier studies focus on the

[13] Stiglitz (1969), Kraus and Litzenberger (1973), DeAngelo and Masulis (1988), Bradley, Jarrell, and Kim (1984), Harris and Raviv (1991), Barclay and Smith (1999), Myers (2002), and many others further discuss the theoretical underpinnings of the tradeoff theory.

relation between firm leverage and firm characteristics such as size, growth opportunities, tax rates, profitability, liquidation value, and ownership structure (Bradley, Jarrell, and Kim, 1984; Long and Malitz, 1985; Titman and Wessels, 1988; Rajan and Zingales, 1995; Chittenden, Hall, and Hutchinson, 1996). More recent studies use these established firm characteristics to determine the target leverage and examine whether the firms adjust their debt level toward the target leverage (Fama and French, 2002; Leary and Roberts, 2005; Flannery and Rangan, 2006; Kayhan and Titman, 2007; Harford, Klasa, and Walcott, 2009).

The tradeoff theory is not without its detractors. First, many authors argue that the marginal tax savings from incremental leverage seem to be much larger than the marginal bankruptcy costs for most firms and question why the firms continue to be under-leveraged (Miller, 1977; Warner, 1977; Graham and Harvey, 2001; Almeida and Philippon, 2008; Elkamhi, Ericsson, and Parsons, 2009). Second, the static tradeoff theory predicts that more profitable firms should have more debt since they have more profits that could be shielded from taxes without incurring an undue cost of bankruptcy. However, the empirical evidence is the opposite (Myers, 1984; Titman and Wessels, 1988; Fama and French, 2002). Third, some authors such as Shyam-Sunder and Myers (1999) formally test the existence of a target leverage and find evidence to the contrary (although Frank and Goyal [2003] argue that their tests lack the statistical power to overturn the theory). Fourth, some authors such as Chang and Dasgupta (2008) raise econometric issues that question the evidence of many studies of target leverage.

Given the ongoing debate about the existence of target leverage, we follow an alternate approach to test an implication of the subject theory. We reason that if firms adjust their capital structure toward a target leverage, and if the credit markets rationally price all information, then the bond and CDS spreads should be related to target leverage in addition to current leverage. One may think of target leverage as a long-term leverage factor and current leverage as a short-term leverage factor.

Using a large sample of bonds during 2002 to 2007 and CDS contracts during 2000 to 2007, we document several pieces of evidence consistent with the static tradeoff theory. Credit markets price bond and CDS spreads as if firms move toward a long-term target leverage. The effect is economically and statistically significant after including the relevant control variables from extant literature. In addition, we base all our inferences on *t*-statistics that correct for clustering at both the firm level and in calendar time (given the panel nature of our data that samples bonds and CDS contracts at quarterly intervals following an earnings announcement). We employ several additional tests using firm fixed effects and Fama-Macbeth regressions, but the evidence remains unchanged.

We next arrange bonds and CDS contracts into three subsamples depending on time to maturity. We find that the effect of target leverage increases with maturity. This evidence is consistent with a delayed adjustment of leverage due to significant transaction costs in the process. We also find that the effect is stronger when firms are above target leverage or carry a speculative grade bond rating. This evidence is consistent with lower transaction costs in downward adjustment of leverage, especially compared to the higher bankruptcy costs of persistently high leverage.

To measure the magnitude of target leverage effect, we report a matched-firm experiment. We arrange the sample of bonds into four quartiles by target leverage. For each bond in the bottom quartile we pick a bond from the top quartile which is matched on other major determinants of default risk (namely, distance to default, credit rating, and leverage). Firms in the bottom and top quartiles have median target leverage of 22.5 and 36.5 percent. This difference is associated with a median 22 basis points increase in bond spread, which compares with a median spread of 84 basis points for the aggregate bond sample. Thus, the target leverage effect is quite significant in economic terms.

If investors can foresee the changing firm risk in future with changing leverage, a natural question arises as to whether the credit rating agencies can foresee the same. In the following tests we regress credit ratings on target leverage in addition to known control variables from extant literature. Once again, target leverage is a significant determinant of credit ratings. However, it appears that the credit rating agencies do not take into consideration the entire effect. Thus, when we re-run our tests of bond and CDS spreads by including both credit ratings and target leverage, the latter's effect is reduced, but remains marginally significant.

Finally, we examine whether our tests of the static tradeoff theory of capital structure are robust in the presence of two other theories. First, the pecking order theory of capital structure predicts that firms prefer to finance their deficits by debt rather than by equity. This theory predicts that leverage will increase over time, and that this effect will be stronger for firms with higher deficits. We therefore include financing deficit as an additional variable. Second, the market timing theory of capital structure predicts that firms are less likely to finance with debt and more likely to finance with equity if a firm has had a recent price run-up. We therefore include the excess stock return of the firm over a 180-day period ending before the earnings announcement as an additional control variable. Briefly speaking, we find that the market timing effect is significant but the pecking order effect is insignificant. We also find that the target leverage effect is relatively unchanged.

In addition to target leverage, we propose and include a new variable, the leverage trend, in all our tests. This is a momentum type of variable that captures in how many of the last eight quarters there was an increase or decrease in actual leverage. If investors extrapolate leverage trend from recent quarters, then this variable should be positively related to the bond and CDS spreads. We find some evidence that this is so.

In summary, our combined evidence on bond spreads and credit ratings provides strong support for the target leverage implication of the static tradeoff theory of capital structure. Our evidence differs from the evidence of Flannery, Nikolova, and Oztekin (2009). Using bond spread data during 1986 to 1998, they find statistically significant but economically insignificant evidence in favor of the target leverage hypothesis. Their evidence is based on a changes model of bond spreads and relies on a time-series analysis. Specifically, they regress quarterly changes in bond spread on quarterly changes in target leverage and other variables. We argue that a changes model is unnecessary in view of the stationarity of quarterly bond and CDS spreads. We further show that it suffers from a significant errors-in-variables problem as the quarterly changes in target leverage are smaller than the measurement errors.

The remaining paper proceeds as follows. Section 2 discusses the data, and Section 3 discusses the methodology. Section 4 presents the primary tests of the target leverage effect, and Sections 5 and 6 present the additional tests. Section 7 concludes.

2. Data sources

2.1. Bond sample: Transaction Reporting And Compliance Engine (TRACE)

The SEC mandated the use of the TRACE system first in July 2002 for large, investment grade bonds. Under the system, bond dealers were required to report their transactions to the National Association of Security Dealers (NASD) which would then be made available to the public. Prior to the introduction of TRACE, corporate bonds traded in an opaque environment where bond quotes were available only by telephone and bond prices were not made public (Bessembinder and Maxwell, 2008).

The TRACE bond transaction database was introduced in three phases. In the first phase, beginning July 1, 2002, NASD required only investment grade bonds with an issue size of at least $1 billion to be part of the database. In the second phase, beginning March 3, 2003, the database was expanded to include bonds with an issue size of at least $100 million and with a rating of at least A3/A-. On April 14, 2003, the sample was expanded again to include trades in 120 BBB bonds. In the final expansion phase, beginning October 1, 2004, the system was expanded to all corporate bond trades in the secondary over-the-counter market (Dick-Nielsen, Feldhutter, and Lando, 2009).

TRACE data cannot be used directly as it contains several data errors. We follow the TRACE cleaning procedures outlined in Dick-Nielsen (2009). First, duplicate trades based on intraday unique message sequence numbers are identified and deleted. Second, if a trade is canceled in the same day both the correction and the original are deleted. Third, if a trade is reversed in the same day, only the original trade is deleted. Fourth, all trades marked as reversals after the same day are deleted along with their corresponding original trades. Fifth, all commission trades are deleted. Sixth, all trades of size less than $100,000 are removed as these tend to be non-institutional trades and may not reflect the true underlying price of the bond.

Next, we merge the cleaned TRACE data with FISD in order to create a working sample of regular bonds. Specifically, bonds which are convertible, putable, callable, asset backed, have floating coupons or credit enhancements, are not issued in US dollars, or are not domiciled in the US are deleted. In addition, bonds that are not senior unsecured obligations of the issuing firm are deleted. Following Duffee (1999), bonds with less than a year to maturity are not included in the sample. While these restrictions reduce sample size, they are necessary to ensure that bond yields under consideration are comparable and not driven by idiosyncratic bond characteristics.

The TRACE database is constructed by aggregating bond transactions. As some bonds trade multiple times a day while others do not trade for several days, it is important to note that the TRACE database does not contain bond prices at regular intervals. We convert the cleaned transactions data into end-of-day bond yields by trade-weighting yields associated with individual transactions during the day. Finally, bond spreads are computed by subtracting the US constant maturity Treasury yield interpolated to match for maturity.

We then match this cleaned bond spread data to the quarterly Compustat database, restricted only to ensure that variables used to estimate target leverage, and associated control variables are available. If a bond trades within fifteen days after the release date of quarterly earnings (RDQ), the bond spread on the date closest to the earnings release date is included in the sample. Thus, bond spreads are sampled quarterly. If there is no bond transaction in the fifteen days following the earnings release date, the quarterly firm observation is dropped. If a firm has multiple bonds, all available bonds are included.[14] The final bond sample contains 6,580 bond quarters, 1,581 distinct bonds, and 331 distinct firms.

2.2. Credit default swap sample: Markit

A CDS spread can be defined as the premium paid to insure the loss of value on the underlying debt obligation against pre-specified credit events. The value of the CDS contract reflects default risk and is a function of the probability of default and the loss-given-default of the underlying obligation (Longstaff, Mitthal, and Neis, 2005; Ericsson, Reneby, and Wang, 2005). This is in contrast with the yield spread of corporate bonds, which reflect not only default risk but also the risk-free benchmark yield and the differential tax treatment and liquidity of corporate and Treasury bonds (Cao, Yu, and Zhong, 2009). Unlike bonds that age over time, CDS spreads are quoted daily for a fixed maturity. Moreover, unlike corporate bond spreads that are available from TRACE only if the underlying bond trades, CDS quotes are available on a daily basis for a large number of firms. Thus, the CDS spread can be considered as a cleaner and a more timely measure of default risk for a given maturity.

We therefore perform our analysis on both CDS and bond spreads. We collect single-name CDS spreads from a comprehensive database compiled by Markit. Daily CDS spreads reflect the average quote contributed by major market participants. The data is cleaned to remove outliers and stale quotes. In addition, we require two or more banks to contribute their spread quotes (Cao, Yu, and Zhong, 2009).

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[14] Notice that our empirical analysis explicitly controls for clustering across firms and quarters.

We use the spread of US dollar-denominated five-year CDS contracts written on senior unsecured debt of US firms. While CDS contracts come in a range of maturities from six months to thirty years, the five year CDS contract is commonly used in the US and has the greatest liquidity. The five year CDS contract is also used in almost all previous academic studies that use the CDS spread as an alternative for the bond yield spread. In addition to the five year CDS spread, we also use quotes on the one and ten year CDS contract for a specific proposition that tests the maturity effect.

The range of restructurings that qualifies as credit event varies across CDS contracts from no restructuring (XR) to unrestricted restructuring (CR). Modified restructuring (MR) contracts that limit the range of maturities of debt instruments that can be delivered in case of a credit event are the most popular in the United States. Thus, we use US dollar-denominated contracts on senior unsecured obligations with modified restructuring (MR) which represent the most liquid CDS contracts in the US market (Duarte, Young, and Yu, 2007).

While a firm may have multiple bonds at any given time, the above restrictions ensure that each firm is associated with only one five year US dollar denominated MR-CDS contract.[15] We use the average CDS spread for 5 trading days after the earnings announcement date. The final CDS sample contains 6,530 firm quarters and 447 distinct firms.

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[15] We also use 1 year and 10 year CDS contracts in one experiment.

3. Methodology

3.1. The choice of levels model versus changes model of credit spreads

Structural models of credit risk imply that credit spreads (which include both bond and CDS spreads) are a function of leverage, volatility, and the riskfree rate. In addition, liquidity, taxes, and macroeconomic conditions are also known to affect credit spreads (Elton et al., 2001; Campbell and Taksler, 2002; Longstaff, Mitthal, and Neis, 2005; Pan et al., 2008). More generally,

$$Credit\ Spread = f(Issuer\ Controls, Issue\ Controls, Macroeconomic\ Controls) \quad (1)$$

The economic relationship between credit spreads and its determinants can be examined in a levels framework or a changes framework. Both approaches present advantages and disadvantages.

By differencing both the dependent and the independent variables, the changes model can control for unobserved time invariant heterogeneity by removing the issuer or issue specific unobserved fixed effect. More importantly, the changes model is necessary if the underlying time series is nonstationary. Several studies have shown that interest rates are nonstationary (Fama, 1976; Fama, 1977; Rose, 1988). However, unlike interest rates, there is economic and econometric reason to believe that spreads are stationary. Theoretical valuation models assume a stationary process for credit spreads (Das and Tufano, 1996; Jarrow, Lando, and Turnbull, 1997). Also, as noted by Cremers et al. (2008), credit spreads are not likely to have a unit root as they are expected return differentials.

There is further little econometric evidence for nonstationarity of credit spreads when they are sampled at a low frequency, such as quarterly, as used in this paper.

Nevertheless, in order to be certain that credit spreads do not contain a unit root, we take the monthly BAA and AAA Moody's corporate bond yield series from the St. Louis FRED database for the 1955-2007 period and sample it on a quarterly basis. We subtract the corresponding constant maturity Treasury rate from these yields to compute bond spreads. We reject the null hypothesis of nonstationarity using both the Phillips-Perron and the Adjusted Dickey-Fuller test.[16]

In this study we mainly employ the levels model in view of the evidence that the credit spreads are stationary. The introduction of noise in the estimation is a major concern with first differencing a stationary series, which is why we do not employ the changes model for the most part (Cremers et al., 2008). Later in Section 3 we present an innovative analysis based on first-order autocorrelations of how first differencing the key independent variable of this study, the target leverage, can introduce unacceptable noise in the estimation procedure.

Finally, while the changes model focuses only on the time series variation, the levels model allows us to study both the time series and cross sectional variation in credit spreads. We therefore argue that the levels framework is better suited for our analysis as our primary hypothesis relates credit spreads to cross-sectional variation in target leverage. The levels model is also employed by Campbell and Taksler (2003), Yu (2003), Cremers, Driessen, Maenhout, and Weinbaum (2008), Yu (2008), and Zhang, Zhou, and Zhu (2009), among others.

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[16] We use quarterly observations during 1955 to 2007. For the difference between AAA corporate bond yield and 10-year constant maturity Treasury bond yield, the Phillips-Perron test rejects nonstationarity with a *p*-value of 0.024 and the Adjusted Dickey-Fuller test rejects with a *p*-value of 0.019. The corresponding *p*-values for the difference between BAA corporate bond yield and 10-year constant maturity Treasury bond yield are 0.007 and 0.008.

3.2. The choice of book leverage versus market leverage

While the relationship between credit spreads and target leverage is central to this study, the definition of leverage is not immediately obvious. As in prior studies, there exists a choice between book leverage and market leverage. We report our primary results using book leverage for several reasons. First, Myers (1977) reasons that leverage is supported by assets in place rather than growth opportunities. Unlike book leverage, market leverage also includes the present value of growth opportunities. Second, according to the survey evidence of Graham and Harvey (2001), managers track book leverage more closely than market leverage. This may be because managers can control the extent of book leverage by issuing and retiring debt or issuing and repurchasing equity. This may not be possible with market leverage which depends on volatile market prices beyond the managers' control. Third, the majority of debt covenants are based on the book value and not market value of equity. Once again managers can take concrete steps toward the realization of a set book value based target, which may not be possible with a market value based target. Fourth, Titman and Wessels (1988) argue that the use of variable market leverage to normalize leverage as well as target leverage and other control variables can induce a spurious correlation. For these reasons, we use book leverage in our reported analysis. However, we verify that our results are qualitatively similar with the use of market leverage (not reported in tables). We measure book leverage as the sum of long-term and short-term debt scaled by total assets. We also verify that our results are qualitatively similar if we define it as total liabilities scaled by total assets.

3.3. Estimation of target leverage

Many cross sectional studies of capital structure estimate target leverage by a regression of leverage in period *t+1* on firm characteristics that proxy for the tradeoff between costs and benefits of leverage in period t (Parsons and Titman, 2009). The typical specification is as follows:

$$Leverage_{i,t+1} = X_{i,t}\beta + \epsilon_{it} \tag{2}$$

where X_{it} is a vector of firm characteristics. We follow Kayhan and Titman (2007) and Harford, Klasa, and Walcott (2009) and include the following firm characteristics as shown in Appendix B1: Log total assets, earnings before interest and taxes, market to book ratio, depreciation, fixed assets, an R&D dummy, R&D expense, selling expense, a rated dummy, industry dummies, and year dummies. All our variables are defined in Appendix 2. Parsons and Titman (2009) describe the motivation behind the inclusion of each of these variables in the target leverage regression in considerable detail.

Panel A of Appendix B1 presents summary statistics for variables that enter the firm-year regression used to compute target leverage. Panel B presents the main target leverage regression for both book and market leverage. All covariates have expected signs corresponding to the literature (Kayhan and Titman, 2007; and Harford, Klasa, and Walcott, 2009).

3.4. A new variable in credit spread models: The leverage trend

In addition to target leverage, we introduce a new behavioral variable in our analysis of bond and credit spreads. This variable is the leverage trend during the last eight quarters. This is a runs type of variable, similar to the momentum factor in asset returns. We start with a value of zero, and each quarter if the leverage increases from the last quarter we add one, and if the leverage decreases from last quarter we subtract one. Thus, the leverage trend variable takes values in the range of -7 to +7. If investors extrapolate leverage trend from recent quarters or years, then this variable should be positively related to the bond and CDS spreads.

3.5. The choice of control variables in the credit spread models

Based on an extensive literature and the hypothesis of this paper, we specify the base model of corporate bond spreads as follows:

Bond spread = b_0 + b_1 *Leverage* + b_2 *Target leverage* + b_3 *Leverage trend during last eight quarters* + b_4 *Historical volatility of firm* + b_5 *Log total assets* + b_6 *Coupon* + b_7 *Years to maturity* + b_8 *Bond age* + b_9 *10-year Treasury bond yield* + b_{10} *Term spread* + b_{11} *Default spread* + b_{12} *Market volatility (VIX)* + b_{13} *Market return* + *Error term* . (3)

The target leverage and the leverage trend during last eight quarters are our focus variables and are defined above. The remaining variables are defined in Appendix B2. Below we explain the reasons for including these different control variables.

Leverage: Every model of bond spread has leverage, which hardly needs an explanation. In the Merton (1974) model leverage serves as the strike price of the put option that the bondholders sell to the equityholders. Higher leverage is the most visible reason for default risk and leads to higher bond spreads. Its coefficient is thus predicted to be positive.

Historical volatility of firm: In the Merton (1974) model volatility of assets of the firm determines the probability of exercise of the put option the bondholders sell to the equityholders. Campbell and Taksler (2003) show that equity volatility can serve as a proxy for asset volatility. They also show that increases in credit spread over the 1990s can be attributed to an increase in idiosyncratic volatility of equity. Its coefficient is predicted to be positive.

Log total assets: Larger firms have higher name recognition, less information asymmetry, and hence greater access to capital markets. This reduces their default risk, so its coefficient is predicted to be negative. Larger firms also have more actively traded bonds, and liquidity increases bond prices.

Coupon: Elton et al. (2001) argue that higher coupon bonds are less desirable than lower coupon bonds as they incur higher taxes throughout their life. Its coefficient is predicted to be positive. To balance the picture, Campbell and Taksler (2003) argue that higher coupons shorten the bond duration, which could reduce the default risk and hence the bond spread.

Years to maturity: The higher the bond life the greater the default risk, so its coefficient is predicted to be positive.

Bond age: Recently issued corporate bonds are more liquid than older bonds, which is sometimes known as on-the-run versus off-the-run phenomenon in the case of Treasury bonds. Its coefficient is predicted to be positive.

10-year Treasury bond yield: In structural models the riskfree interest rate increases the drift term of the stochastic process for asset value and reduces the probability of default. Duffee (1998) empirically shows that the 10-year Treasury bond yield acts as a proxy for the riskfree interest rate. Its coefficient is predicted to be negative.

Term spread: Collin-Dufresne, Goldstein, and Martin (2001) argue that a higher term spread is an indicator of improving economy as well as increasing future riskfree rates. On both counts the default risk decreases, so its coefficient is expected to be negative.

Default spread: This is a measure of the default risk as well as default risk premium in the bond market, and its coefficient is predicted to be positive.

Market volatility: In absence of implied volatilities for individual stocks, we use the market volatility as measured by the VIX index as another proxy for default risk of the firm and in the economy. Its coefficient is predicted to be positive.

Market return: The 180-day market return captures the effect of an improving economy that lowers default risk, so its coefficient is predicted to be negative. However, Campbell and Taksler (2003) motivate their paper by documenting that over the 1990s there was an increase in both the market level and idiosyncratic volatility. Thus, a higher market return may also capture increasing asset volatility, which would predict a positive coefficient.

Finally, when analyzing CDS spreads, we use all the control variables used for bond spreads listed in the Economic model (3) except the bond-specific parameters. The revised model is specified as:

CDS spread = c_0 + c_1 *Leverage* + c_2 *Target leverage* + c_3 *Leverage trend during last eight quarters* + c_4 *Historical volatility of firm* + c_5 *Log assets* + c_6 *10-year Treasury bond yield* + c_7 *Term spread* + c_8 *Default spread* + c_9 *Market volatility (VIX)* + c_{10} *Market return* + *Error term* . (4)

3.6. Computation of significance levels

The panel nature of the samples used in studies of credit spreads introduces a downward bias in standard errors in the presence of firm and time dependence, and therefore an upward bias in OLS *t*-statistics. Economically, we expect credit spreads for individual firms to be dependent across time thus introducing a firm effect. We also expect credit spreads, in the economy as a whole, to covary across time thus introducing a time effect (Peterson, 2009).

However, there is no set convention on the methodology used to compute significance in prior studies that have used a panel dataset of bond or CDS spreads. For example, Avramov, Jostova, and Philipov (2007), Collins-Dufresne, Goldstein, and Martin (2001), and Campbell and Taksler (2003) use OLS *t*-statistics, Cremers et al. (2008) use *t*-statistics corrected for heteroscedasticity, autocorrelation, and crosscorrelations, and Zhang, Zhou, and Zhu (2009) employ standard errors clustered at the firm level.

We recognize the firm and time dependence in our panel dataset and present two way clustered *t*-statistics whenever applicable. We note that while robust *t*-statistics account for heteroscedasticity, they do not address the issue of dependence. However, consistent with some of the prior literature, we also present robust *t*-statistics. The large difference in magnitude between robust and two-way clustered *t*-statistics highlights that residuals in the panel dataset are correlated across firms and time.

An alternate way of accounting for firm dependence includes adding firm fixed effects in the regression. Similarly, a Fama-Macbeth approach can account for time dependence (Peterson, 2009). We document that our results are robust to these approaches.

4. Does target leverage have an economically significant effect on bond and CDS spreads?

4.1. The changes model of bond spreads

We start our analysis with the changes model of bond spreads similar to Flannery, Nikolova, and Oztekin (2009). The changes model is obtained by differencing the variables included in the Economic Model (3) at quarterly intervals. Time invariant firm-specific and bond-specific variables drop out by differencing, so we test the reduced specification shown in Table 2.2. The sample size decreases to 2,967 observations for which changes in dependent and independent variables are available during consecutive quarters.

Panel A.2 of Appendix 2 shows that most changes in variables have close to zero mean and a low standard deviation. Model (2.1) in Table 2.2 further shows that the change in target leverage has an insignificant two-way clustered *t*-statistic in a univariate analysis of the determinants of the change in bond spread. The regression also has an insignificant R^2 of 0.004. Models (2.2) and (2.3) further show that the R^2 equals 0.187 with only the control variables and 0.190 with both the control variables and target leverage. Overall, one is tempted to conclude from these results that target leverage has an insignificant effect on bond spreads. However, we point out that the coefficients of change in leverage and change in historic volatility are also insignificant. In fact, the changes in bond spread in Models (2.2) and (2.3) are driven to a large extent by the changes in macroeconomic variables, namely, the Treasury bond yield, term spread, market volatility, and market return. Given the well-documented role of firm-specific variables such as leverage and volatility in determining bond spreads, this raises the possibility that the insignificant results in our Table 2.2 and in Flannery, Nikolova, and Oztekin arise from measurement errors that are large relative to the true but unobserved changes in some of the variables.

We propose a simple analytical model to estimate the measurement errors in the key independent variable, which is the change in target leverage. This model is inspired by Roll (1984) who examines the autocorrelations in stock returns to estimate another type of measurement error, namely, the bid-ask spread that causes observed trade prices to differ from the true middle values. Denote the true target leverage for a given firm at time *t* by TL_t , the measurement error by η_t , and the observed target leverage by $\widehat{TL}_t = TL_t + \eta_t$. (The firm subscript is dropped for easier exposition.) Denote further the changes in variables as $\Delta TL_t = TL_t - TL_{t-1}$, etc. It follows that

$$Cov\left(\Delta\widehat{TL}_t \,, \Delta\widehat{TL}_{t-1}\right) = Cov(\Delta TL_t + \Delta\eta_t \,, \Delta TL_{t-1} + \Delta\eta_{t-1})$$

$$= Cov(\Delta TL_t \,, \Delta TL_{t-1}) + \; Cov(\Delta\eta_t \,, \Delta\eta_{t-1}) \,,$$

which assumes that the measurement errors are white noise, thus uncorrelated with the true target leverage. For simplicity, we further assume that the changes in true target leverage have a zero autocorrelation, so that

$$Cov(\Delta\widehat{TL}_t, \Delta\widehat{TL}_{t-1}) = Cov(\Delta\eta_t, \Delta\eta_{t-1}) = -Var(\eta_{t-1}) = -Var(\Delta\eta_t)/2 =$$

$$-u^2/2 \ ,$$

$$Var(\Delta\widehat{TL}_t) = Var(\Delta TL_t + \Delta\eta_t) = Var(\Delta TL_t) + Var(\Delta\eta_t) = v^2 + u^2 \ ,$$

$$\rho_1 = Correl(\Delta\widehat{TL}_t, \Delta\widehat{TL}_{t-1}) = \frac{Cov(\Delta\widehat{TL}_t, \Delta\widehat{TL}_{t-1})}{Var(\Delta\widehat{TL}_t)} = \frac{-u^2}{2(v^2+u^2)},$$

Alternately, $\frac{u^2}{v^2}$

$$= -\frac{2\rho}{1+2\rho} \ . \qquad (5)$$

Here u^2 is the variance of measured changes in target leverage induced by measurement errors, and v^2 is the variance induced by true changes. A few simple observations follow. First, if $u^2 \ll v^2$, then the autocorrelation will be close to zero. Second, if $u^2 \gg v^2$, then the autocorrelation will be close to –0.50. Third, if $u^2 \approx v^2$, then the autocorrelation will be close to –0.25.

We measure the autocorrelations in target leverage for each stock listed on the CRSP and Compustat databases over the entire length of time for which the target leverage can be calculated during 1962 to 2005. Differencing over annual periods and averaging across stocks, the autocorrelation in measured changes in target leverage equals –0.145. From Equation (5) we therefore calculate that $u^2/v^2 = 0.408$. In turn, this implies that differencing over quarterly intervals the ratio of error variance to true variance should approximately equal 4×0.408 = 1.632, which further implies an autocorrelation of –0.310. This is because over quarterly intervals the error variance, u^2, remains unchanged while the true variance, v^2, becomes one-fourth of the annual variance. Interestingly, using the same dataset, we find that the autocorrelation in measured changes in target leverage over quarterly periods equals –0.270. The close correspondence between the observed autocorrelation over quarterly intervals and its predicted value based on autocorrelation over annual intervals provides strong support for our model and its assumptions. Finally, using the observed autocorrelation of −0.270, we calculate that over quarterly intervals $u^2/v^2 = 1.174$.

Whether extrapolated from autocorrelations over annual periods or inferred directly over quarterly intervals, the inescapable conclusion from the above analysis is that over quarterly intervals the error variance exceeds the true variance of changes in target leverage. In our opinion this constitutes a serious limitation of the changes model. The insignificant relation between changes in bond spread and changes in target leverage in Table 2.2 thus cannot be interpreted as evidence against the joint hypothesis that firms adjust their capital structure toward a target leverage and that the credit market participants rationally price these adjustments into bond spreads.

4.2. Does the bond market price target leverage? Analysis based on a levels model

Given the limitations of the changes model arising from measurement errors in target leverage, we next examine the levels model specification of the Economic Model (3). This examination is made possible by our earlier discussion that the bond spreads and CDS spreads are stationary at the quarterly level. Table 2.3 shows five different model specifications. The sample includes 6,580 bond-quarters for which the relevant data are available. Given the pooled time-series and cross-sectional nature of this sample, we base our inferences on two-way clustered *t*-statistics. Model (3.1) shows that previously known and tested firm-specific, bond-specific, and macroeconomic control variables give an R^2 of 0.510. The coefficients of most variables are in line with previous literature as explained in Section 3.5.

Model (3.2) shows that by itself and without control variables the target leverage is a highly significant determinant of bond spread with an R^2 of 0.081. Model (3.3) next shows that by itself the leverage trend is also significant and has an R^2 of 0.019. Apparently, the naïve extrapolation of leverage increases and decreases from the last eight quarters has some explanatory power, but not as much as the movement toward an optimum target leverage. Model (3.4) shows that when both the target leverage and leverage trend are included besides the control variables, their statistical significance level rises, possibly due to the lower error variance. The R^2 value rises to 0.541 compared to 0.510 without the new variables proposed in this study. In addition, the coefficient of leverage decreases from 1.48 to 0.93 with the addition of target leverage and leverage trend.

Finally, Model (3.5) tests the explanatory power of the new variables after fully adjusting for the explanatory power of old variables. This regression uses the residual spread from Model (3.1) that includes the old variables. Target leverage still remains significant while the leverage trend becomes insignificant. Together the new variables have an incremental R^2 of 0.053 in explaining the residual spreads. The combined results of Table 2.3 suggest that target leverage is a significant factor in the pricing of bond spreads. This bond market evidence is consistent with the tradeoff theory of capital structure which suggests that value maximizing firms move toward a target leverage that optimizes between the costs and benefits of debt versus equity.

4.3. Does the CDS market price target leverage? Analysis based on a levels model

As discussed before, the CDS contracts offer an alternative to corporate bonds for understanding the determinants of default risk. These contracts offer some advantages as they can be thought of as pure-plays on the firm's default risk. In fact, it can be argued that the CDS spread should equal the default probability multiplied by the loss in the event of default to bondholders. In addition, the CDS contracts analyzed in this paper are written on the firm's default risk, which relates to the firm's aggregate portfolio of bonds rather than an individual bond.

Table 2.4 presents five models similar to Table 2.3, except that the bond-specific factors such as coupon, years to maturity, and bond age are no longer relevant and are omitted. By sheer coincidence the sample sizes in the two tables are very close at 6,580 and 6,530. Overall, the results of Table 2.4 are similar to the results of Table 2.3, but arguably more favorable to our target leverage hypothesis. Now the target leverage has an R^2 of 0.102 in a univariate setting while the leverage trend has an R^2 of 0.041. Both variables are highly significant in all regressions. Model (4.1) shows that even after accounting for all firm-specific and macroeconomic control variables the residual spread regressed on the target leverage and leverage trend has an incremental R^2 of 0.099.

4.4. Robustness tests of statistical significance of the base models of bond and CDS spreads

The combined results of the levels models in Tables 2.3 and 2.4 are inconsistent with the results of the changes model in Table 2.2. These later results suggest that credit market participants price bonds and CDS contracts as if they expect firms to move toward a target leverage. We now present a few robustness tests of the statistical significance of these results. The main concern is that the sample of 6,580 bonds and 6,530 CDS contracts includes fewer distinct bonds and distinct firms. Notice for this reason we based our inferences in Tables 2.3 and 2.4 on two-way clustered *t*-statistics. However, Table 2.5 presents some additional evidence on the statistical significance of our results.

Models (5.1) and (5.2) replicate the results of Model (3.4) for bond spreads using the same linear specification of variables but alternative procedures of inferring their statistical significance. First, Model (5.1) includes firm fixed effects as an alternative to the clustered *t*-statistic approach. As typical, the firm fixed effects approach increases the R^2, from 0.541 to 0.818, but reduces the coefficients of all but two variables.

Interestingly, one of these two variables is the target leverage, whose coefficient increases from 2.52 to 3.04 (and the other is the years to maturity, whose coefficient increases from 1.69 to 2.01). Using White's heteroscedasticity-adjusted *t*-statistics we find that both the target leverage and leverage trend remain highly significant in this regression. Second, Model (5.2) reports the results of a Fama-Macbeth approach. The reported coefficients and *t*-statistics in this case are calculated from the distribution of coefficients for 27 quarterly regressions, which naturally corrects for clustering at the firm level to a large extent. The macroeconomic variables are dropped in the Fama-Macbeth regressions as they would take the same value for all observations in a calendar quarter, thus leading to extreme collinearity. We find that the average R^2 across the 27 quarterly regressions is 0.604 compared to 0.541 for the pooled regression, and the average coefficient of target leverage is 4.77 compared to 2.52.

Models (5.3) and (5.4) replicate the results of Model (4.4) for CDS spreads using the firm fixed effects approach and the Fama-Macbeth approach. The results are very similar to those for bond spreads. In particular, the coefficient of every cross-sectional variable is lower in Model (5.3) with firm fixed effects compared to Model (4.4) without firm fixed effects, except target leverage, which equals 4.55 in the former case and 3.01 in the latter case.

Overall, the robustness tests of Table 2.5 uphold and enhance the evidence in favor of the target leverage hypothesis or the tradeoff theory of capital structure. Finally, we carry out a few tests to ascertain that the coefficient of target leverage in Models (3.4) and (4.4) are not driven simply by a large sample size (not reported in a table). First, for each bond and CDS spread we pick a target leverage at random from a normal distribution with the same mean and standard deviation as the empirical distribution of target leverage for the aggregate samples of bonds and CDS contracts. Second, instead of a normal distribution, we pick values at random (with replacement) from the empirical distribution of target leverage values itself. We run the models with these random but plausible values of target leverage for the sample firms. The coefficients of target leverage are insignificant in all cases.

4.5. The magnitude issue: How large is the target leverage effect on bond and CDS spreads?

Our regression tests so far suggest that target leverage is a statistically significant factor in determining bond and CDS spreads. This is presumably because firms adjust their leverage toward a target leverage over time, and both current and future leverage determine the default risk of the firm as pointed out by Collin-Dufresne and Goldstein (2001). We have also presented evidence on the importance of this new factor in bond spreads by discussing the incremental R^2 upon adding it to the regression analysis. While this approach is quite common and useful, it has the limitation that the incremental R^2 of any variable depends on the order in which it is added relative to the other variables. An additional limitation concerns the linear specification of the underlying structural model. It is quite likely that the effect of many variables such as current leverage and volatility enters the bond spread in a nonlinear fashion as in any option pricing model.

Table 2.6 reports a matched-bond experiment to answer both the magnitude issue and the nonlinearity concern. Perhaps the most intuitive measure of magnitude is the incremental bond spread. To understand the matching procedure, consider Panel A. We partition the aggregate sample into four quartiles based on target leverage. The dispersion in target leverage values across these subsamples is quite significant, with a mean value of 36.9 percent in the top quartile and 21.8 percent in the bottom quartile. Next, for each bond in the bottom quartile, we select a bond of a different firm in the top quartile that is the closest match on leverage. This procedure gives 1,810 matched pairs of bonds with similar leverage but considerable difference between target leverage. For a proper examination, we also report the distance to default and credit ratings, the other two determinants of default risk.

Panel A shows that the bond spread averages 81.2 basis points for the low target leverage quartile and 155.2 basis points for the high leverage quartile (medians 66.0 and 121.9 basis points). The difference of 74.0 basis points between the average values and 55.9 basis points between the median values of bond spread is highly significant in economic terms, especially compared to the average spread of 116 basis points for all bonds. Part of the reason may be that matching on leverage alone does not match well on the other two determinants of default risk.

While leverage is a principal source of default risk, structural credit risk models suggest that volatility also has a material impact on bond spreads. Thus, in order to correctly account for default risk, both volatility and leverage should be considered together. Bharath and Shumway (2008) use the distance to default measure that captures both factors in an option pricing framework. They show that this measure captures the default risk in bonds. Panel B of Table 2.6 matches firms on distance to default, which implicitly matches them reasonably on leverage and credit rating as well. However, presumably due to the difference between the target leverage of the two subsamples, their bond spreads continue to differ substantially. The difference between average bond spreads equals 21.8 basis points and the difference between median bond spreads equals 23.1 basis points.

Panel C does a similar matching by credit rating. Given the discrete nature of credit ratings, the matching is very close in this case. Yet the difference between average and median bond spreads in the top and bottom quartiles of target leverage equals 26.6 and 31.3 basis points. Finally, Panel D explicitly matches by both distance to default and credit rating, which implicitly matches by leverage and volatility as well. Despite this close matching on all three of these previously known determinants of default risk, the differences between bond spreads equal 21.9 and 23.3 basis points, not much different from Panels B and C. This is likely to be the best estimate of the incremental effect of target leverage. An inter-quartile spread of 35.4 – 22.3 = 13.1 percent in target leverage is associated with an incremental bond spread of 22 or 23 basis points. Given a median time to maturity of six years from Table 2.1, which may translate into a bond duration of five years, this difference in spreads corresponds to a difference in prices of the order of 1.10 percent. The combined results of Tables 2.3, 2.4, 2.5, and 2.6 provide strong evidence that higher target leverage is associated with higher default risk of corporate bonds, consistent with the tradeoff theory of capital structure which suggests that the target leverage is the likely long-term leverage value.

5. Target leverage, adjustment costs, and bond and CDS spreads

5.1. Does target leverage have a greater effect on longer-term bond and CDS spreads?

Several papers suggest that transaction costs slow down the adjustment process of current leverage toward the target leverage. Thus, in aggregate bond samples, Fama and French (2002) estimate a speed of adjustment toward a target leverage in the range of 7 to 18 percent per year. Thus, at the average rate of 12.5 percent per year, a firm would cover $1.00\text{-}(1\text{-}0.125)^5$ = 49 percent of the gap between leverage and target leverage in five years and 74 percent of the gap in ten years (assuming that target leverage remains unchanged over the period). Other authors, such as Flannery and Rangan (2006), suggest that the firms cover as much as one-third of the gap in one year, which implies an adjustment of 87 percent in five years and 98 percent in ten years. Regardless of the adjustment speed, it is obvious that the longer the term to maturity the greater the effect of target leverage on bond and CDS spreads.

Table 2.7 sorts the aggregate bond sample into three subsamples with one to three years, three to six years, and greater than six years to maturity. We test the same specification of bond spreads as given by the base Model (3.4). The first three regressions in Table 2.7 show that the coefficient of target leverage equals 1.48 for short-maturity bonds, 2.40 for medium-maturity bonds, and 2.56 for long-maturity bonds. The monotonic increase in the coefficient of target leverage with bond maturity is unlike the coefficient of leverage, which equals 0.85, 0.69, and 1.10 for the three subsamples.

The next three regressions in Table 2.7 present the evidence for CDS contracts of one year, five years, and ten years to maturity. Once again, the coefficient of target leverage equals 1.61, 3.01, and 3.56 for the three subsamples, showing a monotonic increase with maturity. This is unlike the coefficient of leverage, which equals 1.52, 2.65, and 2.56 in the three subsamples. The combined evidence of Table 2.7 suggests that the effect of target leverage on bond and CDS spreads increases with contract maturity as implied by a partial adjustment model of firm leverage.

5.2. Is the target leverage effect asymmetric depending on current leverage or credit rating?

The preceding evidence is consistent with significant transaction costs in the adjustment process. This further raises the question of whether these transaction costs are asymmetric depending on current leverage or credit rating of the firm. The first two regressions in Panel A of Table 2.8 bifurcate the bond sample based on whether the current leverage is lower or higher than target leverage. The coefficient of target leverage equals 1.87 in the former case and 3.93 in the latter case. One interpretation of this evidence is that firms are quicker at adjusting deviations from target leverage when the current leverage is higher than target leverage than when it is lower than target leverage. This could be because the transaction costs of downward adjustment of leverage are lower, especially compared to the economic cost of a looming bankruptcy when the current leverage of the firm is very high.[17]

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[17] As a caveat, we also point out that the average spreads for investment grade and speculative grade bonds equal 89 and 319 basis points as shown in Table 1. Thus, the coefficients of many other variables are also higher for above target leverage (or speculative grade) firms in Table 8.

The next two regressions in Panel A of Table 2.8 test the model across subsamples formed by the firm's credit rating to take another look at the asymmetry of transaction costs. Once again, the coefficient of target leverage for speculative grade bonds is higher than for investment grade bonds, and the above explanation applies. Panel B of Table 2.8 reports the corresponding tests for CDS spreads. The coefficient of target leverage in this panel continues to be higher when the current leverage is higher than target leverage, but the evidence is flat across subsamples formed by the firm's credit rating. Overall, this table provides some evidence of asymmetry in transaction costs that impede the rapid adjustment of current leverage toward a target leverage.

6. Additional evidence on target leverage

6.1. Does target leverage affect credit ratings?

Our tests so far suggest that investors in corporate bond and CDS markets price contracts to account for the future changes in a firm's leverage toward its target leverage. This raises the question of whether the credit rating agencies also take this factor into consideration in arriving at their recommendations. We are not aware of any previous evidence on this issue.

The Compustat database provides Standard & Poor's credit ratings for firms starting with the first quarter of 1992. Thus, to ensure that we have the largest possible sample, we analyze credit ratings over the aggregate period from 1992 to 2007. The expanded sample contains a wider range of firms and more speculative grade bonds. Compustat assigns numerical values to the letter credit ratings by Standard & Poor's, with higher numbers representing lower credit ratings. Our regression model includes target leverage besides several control variables used by Ashbaugh-Skaife, Collins, and LaFond (2006). However, we drop their corporate governance variables which would reduce the sample size.

Panel A of Table 2.9 provides the summary statistics of variables used to analyze credit ratings, and Panel B provides the cross-sectional results. Model (9.1) reports a pooled regression with 37,030 observations and Model (9.2) reports a Fama-Macbeth regression over 64 quarters. Most control variables are significant and in the same direction as documented by Ashbaugh-Skaife, Collins, and LaFond. More importantly, target leverage is highly significant in both models. We infer that rating agencies such as Standard & Poor's Corporation rate firms as if their capital structure decisions are consistent with the tradeoff theory.

At this stage we know that bond and CDS spreads reflect target leverage. We also know that credit ratings reflect target leverage. This raises the next critical question as to whether target leverage is priced in bond and CDS spreads after controlling for credit ratings. We bring back Model (3.4) for bond spreads and Model (4.4) for CDS spreads and add the observed credit rating as an independent variable in Panel C of Table 2.9. Model (9.3) shows that, as expected, credit rating by itself explains a large part of the cross-sectional variation in bond spreads, with an R^2 of 0.425. Model (9.4) next shows that in the multivariate framework the inclusion of credit rating weakens the coefficient of target leverage, but this coefficient remains marginally significant at the 10% level (using two-way clustered *t*-statistics). Models (9.5) and (9.6) next show similar evidence for CDS spreads. Overall, Table 2.9 suggests that target leverage affects bond and CDS spreads after controlling for its effect built into the credit ratings.

6.2. Are tests of target leverage based on tradeoff theory robust in the presence of controls for pecking order theory and market timing theory?

The capital structure puzzle has many explanations in the finance literature. We now test whether the target leverage implications of the static tradeoff theory considered in this paper are robust in the presence of two alternate theories. First, the pecking order theory predicts that firms prefer to finance their deficits by debt than by equity. It follows that the greater the expected financing deficit in future years the greater the debt to equity ratio. We use the financing deficit over the last fiscal year ending before the observation date to control for the pecking order effect on bond and CDS spreads. Its coefficient is predicted to be positive. Second, the market timing theory of Baker and Wurgler (2002) predicts that firms prefer to issue equity when their equity is attractively priced and debt is unattractively priced. We measure equity valuation by the prior year return, and predict that its coefficient should be negative.

Model (10.1) in Table 2.10 extends the base specification of Model (3.4) for bond spreads by adding financing deficit and prior year return. Contrary to the predictions of pecking order theory, the coefficient of financing deficit is negative and mildly significant. The coefficient of prior year return is also negative, but highly significant and consistent with the predictions of the market timing theory. The coefficient of target leverage is only slightly reduced, from 2.52 to 2.42, and it remains highly significant. Similar evidence is obtained for CDS spreads in Model (10.2). Overall, the evidence on the role of target leverage in determining bond and CDS spreads as predicted by the tradeoff theory is unaffected by the inclusion of additional controls motivated by the pecking order theory and the market timing theory of capital structure.

7. Conclusions

The static tradeoff theory offers an explanation of how firms choose their capital structure. It suggests that there is an interior optimum leverage which balances the tax benefits against the bankruptcy and agency costs of debt. This becomes the target leverage toward which the firms move over time. We reason that rational investors predict this movement and build it into bond and CDS spreads. Consistent with this hypothesis, we show statistically and economically significant evidence that target leverage acts as an additional long-term leverage factor in credit spreads. The statistical inferences are based on a levels model and corrections to *t*-statistics for clustering at the firm level and in calendar time. In economic terms, a matched-pairs experiment in our paper analyzes the magnitude of the target leverage effect after controlling for other major determinants of default risk (namely, distance to default, credit rating, and leverage). For each bond from the bottom quartile of target leverage, we pick a matching bond of a different firm from the top quartile. The median target leverage equals 22.5 and 36.5 basis points for the two groups, and this is associated with a median difference of 22 basis points in their bond spreads.

Several other tests strengthen the evidence in favor of the tradeoff theory and the target leverage hypothesis. First, we show that the longer the maturity of the bond or CDS contract the greater the target leverage effect. Second, we show that the target leverage is a more significant determinant of credit spreads when the current leverage of the firm is higher than its target leverage. This is consistent with an asymmetric transaction argument. Apparently, firms bring down higher than optimal leverage (which increases bankruptcy cost) more quickly than they bring up lower than optimal leverage (which decreases tax benefits). Third, we show that bond ratings are related to target leverage in addition to other known determinants. This suggests that credit rating agencies also rate firms as if their capital structure decisions are consistent with the tradeoff theory. However, we find that target leverage continues to have a marginally significant effect on credit spreads after adjusting for the effect of credit ratings. Fourth, we show that the evidence in favor of the tradeoff theory and the target leverage hypothesis is relatively unaffected in the presence of controls for the pecking order hypothesis and the market timing hypothesis.

Table 2.1 Summary statistics

Variable	Mean	Q1	Median	Q3
Panel A.1: Bond sample – levels model – 6,580 observations				
Bond spread – all (basis points)	116	56	84	135
Bond spread – investment grade – 5,815 cases	89	52	77	114
Bond spread – speculative grade – 765 cases	319	190	281	410
Leverage (percent)	29.9	20.4	27.1	37.2
Target leverage (percent)	28.5	24.4	27.5	32.0
Historical volatility – annualized (percent)	25.3	10.2	23.2	29.7
Leverage trend last 8 quarters (number)	-1.6	-3.0	-1.0	1.0
Assets ($ billions)	45.9	7.4	18.4	38.7
Coupon (percent)	6.2	5.3	6.3	7.1
Years to maturity (years)	8.9	3.3	6.0	9.5
Bond age (years)	4.2	2.0	4.0	6.0
10-year Treasury bond yield (basis points)	449	418	446	473
Term spread (basis points)	75	2	31	152
Default spread (basis points)	89	81	91	94
Market volatility (percent)	14.0	12.0	14.6	16.9
Market return (percent)	14.0	9.4	14.7	19.0
Panel A.2: Bond sample – changes model – 2,967 observations				
Change in bond spread (basis points)	0.2	-13.3	-0.1	13.4
Change in leverage (percent)	-0.3	-1.3	-0.4	0.6
Change in target leverage (percent)	-0.0	-0.6	-0.1	0.6
Change in historical volatility (percent)	0.7	-0.8	0.4	1.9
Change in 10-year T bond yield (basis points)	2.1	-15.0	3.0	18.0
Change in term spread (basis points)	-9.2	-29.0	-11.0	8.0
Change in default spread (basis points)	-0.8	-8.0	0.0	6.0
Change in market volatility (percent)	-0.1	-2.3	-0.5	2.5
Market return (percent)	14.6	9.8	14.9	19.0
Panel B: CDS sample – 6,530 observations				
5-year CDS spread (basis points)	113	30	56	141
5-year CDS spread – investment grade – 4,925 cases	60	25	41	69
5-year CDS spread – speculative grade – 1,605 cases	275	153	228	361
Leverage (percent)	28.9	18.7	26.4	36.6
Target leverage (percent)	28.2	23.9	27.6	32.4
Leverage trend last 8 quarters (number)	-1.8	-3.0	-1.0	1.0
Historical volatility (percent)	30.2	22.0	28.1	36.0
Assets ($ billions)	12.9	3.1	6.1	14.4
10-year Treasury bond yield (basis points)	444	415	445	472
Term spread (basis points)	101	10	69	203
Default spread (basis points)	94	82	91	103
Market volatility (percent)	17.3	12.8	15.4	20.2
Market return (percent)	10.9	5.6	14.0	18.5
1-year CDS spread – 5,684 cases (basis points)	62	9	20	60
10-year CDS spread – 5,632 cases (basis points)	129	44	76	160

Note: The first sample of 6,580 observations in Panel A.1 includes bonds with all maturities and credit ratings for which the required quarterly data are available during 2002-Q2 to 2007-Q3. The second sample of 2,967 observation in Panel A.2 includes quarterly changes in bond yields and other variables during the same period. This sample is smaller than in Panel A.1 because it requires nonmissing data for all variables during consecutive quarters. The third sample of 6,530 observations in Panel B includes MR-

Table 2.1 Continued

class CDS during 2000-Q4 to 2007-Q4. Appendix B1 describes the model used to estimate the target leverage, and Appendix B2 defines the variables analyzed.

Table 2.2 The changes model of bond spread and target leverage

	Dependent variable is quarterly change in bond spread in basis points		
Independent variables	(2.1)	(2.2)	(2.3)
Change in leverage		0.41 (1.26), [1.18]	0.31 (0.94), [0.82]
Change in target leverage	1.84 (2.66), [1.32]		1.71 (2.57), [1.63]
Change in historical volatility of firm		0.11 (0.23), [0.19]	0.12 (0.25), [0.21]
Change in 10-year Treasury bond yield		-0.10 (-4.13), [-2.38]	-0.11 (-4.35), [-2.37]
Change in term spread		0.11 (3.24), [1.70]	0.10 (3.00), [1.56]
Change in default spread		0.75 (9.77), [4.62]	0.75 (9.84), [4.68]
Change in market volatility (VIX)		3.85 (17.41), [6.65]	3.86 (17.48), [6.56]
Market return		0.17 (1.75), [1.12]	0.18 (1.87), [1.20]
Constant	0.22 (0.29), [0.05]	0.07 (0.04), [0.02]	-0.19 (-0.12), [-0.07]
Adjusted R-square	0.004	0.187	0.190
Observations	2,967	2,967	2,967

Note: This table examines the effect of target leverage on bond spread using the changes model with quarterly data. The sample period spans 2002-Q2 (second quarter of 2002) to 2007-Q3. The sample includes bonds with all maturities and credit ratings for which the required quarterly data are available. Appendix B1 describes the model used to estimate the target leverage, and Appendix B2 defines the variables analyzed. White's heteroscedasticity-adjusted *t*-statistics are shown in round parentheses, and two-way clustered *t*-statistics that adjust for clustering at the firm and the calendar quarter levels are shown in square brackets.

Table 2.3 Does the bond market price target leverage? Analysis based on a levels model

	Dependent variable: bond spread				Residual spread from Model (3.1)
Independent variables	(3.1)	(3.2)	(3.3)	(3.4)	(3.5)
Leverage	1.48 (17.11), [4.05]			0.93 (11.10), [2.93]	
Target leverage		4.88 (20.17), [3.87]		2.52 (16.06), [4.06]	2.74 (17.00), [2.49]
Leverage trend during last eight quarters			4.75 (9.07), [1.96]	4.49 (12.35), [3.83]	1.01 (3.05), [0.65]
Historical volatility of firm	5.20 (20.97), [7.87]			4.90 (21.71), [8.57]	
Log total assets	-10.33 (-8.83), [-2.54]			-15.27 (-15.96), [-5.18]	
Coupon	17.27 (16.00), [5.13]			14.05 (13.12), [5.12]	
Years to maturity	1.63 (15.83), [6.55]			1.69 (17.66), [7.01]	
Bond age	0.77 (0.79), [0.28]			1.38 (2.71), [1.09]	
10-year Treasury bond yield	-0.14 (3.74), [2.64]			-0.16 (-4.18), [-1.33]	
Term spread	-0.49 (-22.10), [-6.46]			-0.45 (-23.04), [-6.09]	
Default spread	-1.03 (-11.56), [-4.83]			-1.10 (-12.98), [-4.39]	
Market volatility (VIX)	6.65 (13.25), [5.20]			6.27 (15.84), [5.52]	
Market return	1.17 (8.20), [3.04]			1.07 (9.90), [3.03]	
Constant	-11.93 (-4.06), [-2.66]	-23.49 (-3.71), [-0.72]	123.21 (66.64), [11.68]	34.68 (1.75), [0.69]	39.23 (8.76), [1.26]
Adjusted R-square	0.510	0.081	0.019	0.541	0.053
Observations	6,580	6,580	6,580	6,580	6,580

Note: This table examines the effect of target leverage on bond spread using the levels model with quarterly data. The sample period spans 2002-Q2 (second quarter of 2002) to 2007-Q3. The sample includes bonds with all maturities and credit ratings for which the required quarterly data are available. Models (3.1) to (3.4) use the bond spread as the dependent variable, and Model (3.5) uses residuals from Model (3.1) as the dependent variable. Appendix B1 describes the model used to estimate the target leverage, and Appendix B2 defines the variables analyzed. White's heteroscedasticity-adjusted *t*-statistics are shown in round parentheses, and two-way clustered *t*-statistics that adjust for clustering at the firm and the calendar quarter levels are shown in square brackets.

Table 2.4 Does the CDS market price target leverage? Analysis based on a levels model

	Dependent variable: CDS spread				Residual spread from Model (4.1)
Independent variables	(4.1)	(4.2)	(4.3)	(4.4)	(4.5)
Leverage	3.21 (29.03), [10.50]			2.65 (24.22), [9.19]	
Target leverage		6.58 (23.32), [7.50]		3.01 (15.99), [5.60]	3.50 (18.29), [6.45]
Leverage trend during last eight quarters			9.82 (14.12), [5.41]	2.95 (6.05), [2.41]	5.53 (12.45), [4.93]
Historical volatility of firm	6.89 (37.18), [14.46]			6.57 (36.43), [14.63]	
Log total assets	-9.70 (-7.67), [-2.65]			-12.47 (-9.91), [-3.48]	
10-year Treasury bond yield	-0.08 (-1.99), [-0.56]			-0.10 (-2.52), [-0.73]	
Term spread	-0.27 (-14.30), [-4.94]			-0.25 (-13.65), [-4.80]	
Default spread	-0.50 (-4.90), [-1.46]			-0.59 (-5.80), [-1.80]	
Market volatility (VIX)	2.51 (6.98), [3.40]			2.31 (6.58), [3.24]	
Market return	0.87 (7.01), [2.61]			0.79 (6.50), [2.46]	
Constant	-44.36 (-2.00), [-0.82]	-72.37 (-9.83), [-3.30]	131.04 (53.86), [16.85]	-54.41 (-2.43), [-1.03]	24.56 (4.50), [1.59]
Adjusted R-square	0.481	0.102	0.041	0.504	0.099
Observations	6,530	6,530	6,530	6,530	6,530

Note: This table examines the effect of target leverage on CDS spread using the levels model with quarterly data. The sample includes MR-class CDS with constant five years to maturity for which the required quarterly data are available. The sample period spans 2000-Q4 to 2007-Q4. Models (4.1) to (4.4) use the CDS spread as the dependent variable, and Model (4.5) uses residuals from Model (4.1) as the dependent variable. Appendix B1 describes the model used to estimate the target leverage, and Appendix B2 defines the variables analyzed. White's heteroscedasticity-adjusted *t*-statistics are shown in round parentheses, and two-way clustered *t*-statistics that adjust for clustering at the firm and the calendar quarter levels are shown in square brackets.

Table 2.5 Robustness checks of target leverage effect for aggregate samples of bonds and CDS

	Robustness tests of Model (3.4) for bond spreads		Robustness tests of Model (4.4) for CDS spreads	
	Firm fixed effects	Fama-Macbeth	Firm fixed effects	Fama-Macbeth
Independent variables	(5.1)	(5.2)	(5.3)	(5.4)
Leverage	0.81	2.94	1.63	2.58
	(3.68)	(1.42)	(8.58)	(19.83)
Target leverage	3.04	4.77	4.55	2.75
	(5.60)	(2.44)	(8.35)	(10.69)
Leverage trend during last eight quarters	1.48	-7.29	0.18	3.46
	(4.33)	(-0.70)	(0.45)	(5.83)
Historical volatility of firm	2.12	7.65	2.98	7.07
	(7.42)	(3.08)	(13.23)	(23.54)
Log total assets	-2.69	5.65	-7.49	-12.30
	(-0.48)	(0.24)	(-1.35)	(-5.49)
Coupon	2.92	12.94		
	(3.55)	(4.85)		
Years to maturity	2.01	1.35		
	(23.55)	(6.17)		
Bond age	0.31	2.01		
	(0.92)	(1.55)		
10-year Treasury bond yield	-0.10		-0.08	
	(-4.26)		(-3.83)	
Term spread	-0.24		-0.08	
	(-13.77)		(-4.69)	
Default spread	-0.28		0.04	
	(-4.08)		(0.60)	
Market volatility (VIX)	4.98		1.67	
	(19.23)		(7.19)	
Market return	0.65		0.17	
	(8.91)		(2.14)	
Constant	-53.03	-454.8	-74.85	-145.96
	(-0.97)	(-1.10)	(-1.39)	(-6.34)
Adjusted R-square	0.818	0.604	0.809	0.572
Observations	6,580	6,580	6,530	6,530

Note: This table reports robustness tests of Model (3.4) for bonds from Table 2.3 and Model (4.4) for CDS from Table 2.4. Thus, all sample details below correspond to those models. The firm fixed effects model uses White's heteroscedasticity-adjusted *t*-statistics, since the firm dummy substitutes for clustering and the clustered *t*-statistics are no longer feasible. The Fama-Macbeth model reports simple averages and ordinary *t*-statistics of coefficient estimates from quarterly regressions. The adjusted R-square values in this case are the averages of corresponding values for quarterly regressions. There are 23 quarterly regressions for bonds and 27 quarterly regressions for CDS. The macro variables are omitted in Fama-Macbeth tests since they take the same value for all observations within a quarter, thus leading to extreme collinearity.

Table 2.6 The magnitude issue: How large is the target leverage effect on bond and CDS spreads after controlling for other important determinants of default risk?

Variables	Low target leverage quartile		High target leverage quartile		Difference in means	Difference in medians
	Mean	Median	Mean	Median		
Panel A: Matched by leverage (N=1,810)						
Target leverage	21.8	22.3	36.9	35.4		
Leverage	26.3	23.6	26.6	23.8		
Distance to default	11.9	11.8	6.5	6.6		
Credit rating	7.9	8.0	10.9	11.0		
Bond spread	81.2	66.0	155.2	121.9	74.0***	55.9***
Panel B: Matched by distance to default (N=1,810)						
Target leverage	21.8	22.3	36.1	35.1		
Distance to default	11.9	11.8	10.8	11.2		
Leverage	26.3	23.6	23.3	21.9		
Credit rating	7.9	8.0	9.8	10.0		
Bond spread	81.2	66.0	103.0	89.1	21.8***	23.1***
Panel C: Matched by credit rating (N=1,469)						
Target leverage	22.0	22.5	38.6	38.6		
Credit rating	8.6	8.0	8.6	8.0		
Leverage	26.2	22.9	27.6	26.5		
Distance to default	11.8	11.7	7.7	8.2		
Bond spread	86.2	72.1	112.8	103.4	26.6***	31.3***
Panel D: Matched by distance to default and credit rating (N=1,469)						
Target leverage	22.0	22.5	38.1	36.5		
Distance to default	11.8	11.7	9.2	9.1		
Credit rating	8.6	8.0	8.6	8.0		
Leverage	26.2	22.9	24.7	23.7		
Bond spread	86.2	72.1	108.1	95.4	21.9***	23.3***

Note: We measure the effect of target leverage on bond spread after controlling for other variables known to be strongly associated with default (or bankruptcy) risk. These variables are book leverage, distance to default, and credit ratings. We do this by controlling for one variable at a time in a matched-bond experiment. Each quarter we arrange all bonds with relevant information into quartiles formed by target leverage. For each bond in the bottom quartile of target leverage, we then select a bond of a different firm in the top quartile of target leverage that is the closest match on leverage in Panel A, distance to default in Panel B, credit rating in Panel C, and both distance-to-default and credit rating in Panel D. All variables are defined in Appendix B2. The significance levels of differences in means and medians are based on a t-test and a sign rank test. Differences significantly different from zero at the one-percent level are shown by ***.

Table 2.7 Does target leverage have a greater effect on longer-term bond and CDS spreads?

	Dependent variable: bond spread			Dependent variable: CDS spread		
	Maturity 1 to 3 years	Maturity 3 to 6 years	Maturity 6 years and more	Maturity 1 year	Maturity 5 years	Maturity 10 years
Independent variables	(7.1)	(7.2)	(7.3)	(7.4)	(7.5)	(7.6)
Leverage	0.85 (5.90), [2.74]	0.69 (4.80), [2.06]	1.10 (8.20), [2.87]	1.52 (15.08), [6.44]	2.65 (24.22), [9.19]	2.56 (21.16), [7.90]
Target leverage	1.48 (5.10), [2.52]	2.40 (8.01), [3.19]	2.56 (12.48), [4.34]	1.61 (9.59), [3.65]	3.01 (15.99), [5.60]	3.56 (16.66), [6.08]
Leverage trend during last eight quarters	2.77 (3.98), [2.61]	4.21 (6.71), [3.29]	5.58 (10.65), [4.18]	2.91 (6.91), [3.81]	2.95 (6.05), [2.41]	2.81 (5.21), [2.04]
Historical volatility of firm	5.05 (10.97), [7.10]	5.76 (14.31), [8.91]	4.19 (14.17), [6.78]	5.14 (25.81), [11.79]	6.57 (36.43), [14.63]	6.76 (32.35), [12.49]
Log total assets	-12.07 (-7.49), [-4.05]	-12.04 (-7.48), [-3.78]	-17.56 (-12.32), [-4.46]	-6.09 (-5.39), [-2.14]	-12.47 (-9.91), [-3.48]	-12.88 (-9.32), [-3.38]
Coupon	6.78 (3.67), [2.26]	10.70 (5.09), [2.97]	21.81 (11.37), [4.68]			
Years to maturity	9.88 (3.83), [3.62]	11.19 (4.69), [3.76]	0.45 (2.91), [1.28]			
Bond age	0.99 (0.95), [0.65]	2.44 (1.80), [1.03]	2.32 (3.48), [1.77]			
10-year Treasury bond yield	-0.28 (-3.65), [-2.54]	-0.24 (-3.12), [-1.69]	-0.06 (-1.13), [-0.48]	-0.02 (-0.61), [-0.40]	-0.10 (-2.52), [-0.73]	-0.17 (-19.59), [-1.22]
Term spread	-0.42 (-9.04), [-5.43]	-0.42 (-11.71), [-4.99]	-0.48 (-18.01), [-5.97]	0.04 (2.62), [1.26]	-0.25 (-13.65), [-4.80]	-0.41 (-19.59), [-7.58]
Default spread	-1.45 (-7.21), [-5.86]	-0.95 (-5.55), [-2.83]	-0.92 (-8.23), [-3.40]	-0.38 (-3.92), [-2.15]	-0.59 (-5.80), [-1.80]	-0.72 (-6.51), [-2.05]
Market volatility (VIX)	5.56 (6.08), [5.08]	7.36 (9.74), [5.59]	5.81 (11.49), [4.34]	1.08 (3.29), [2.15]	2.31 (6.58), [3.24]	3.39 (8.48), [4.58]
Market return	0.70 (2.74), [1.83]	1.19 (6.02), [2.88]	1.17 (8.17), [2.91]	0.06 (0.53), [0.29]	0.79 (6.50), [2.46]	1.06 (8.02), [3.46]
Constant	138.60 (3.55), [2.60]	-37.70 (-1.06), [-0.80]	-12.32 (-0.45), [-0.20]	-98.20 (-4.83), [-2.56]	-54.41 (-2.43), [-1.03]	-14.64 (-0.61), [-0.26]
Adjusted R-square	0.528	0.579	0.540	0.455	0.504	0.495
Observations	1,406	1,862	3,312	5,837	6,530	5,791
Mean spread	81	105	137	62	113	129

Note: The first sample includes bonds with all maturities and credit ratings for which the required quarterly data are available during 2002-Q2 to 2007-Q3. The second sample includes MR-class CDS during 2000-Q4 to 2007-Q4. The bonds and the CDS are categorized by maturity as shown in the panel headings. Appendix B1 describes the model used to estimate the target leverage, and Appendix B2 defines the variables analyzed. White's heteroscedasticity-adjusted *t*-statistics are shown in round parentheses, and two-way clustered *t*-statistics that adjust for clustering at the firm and the calendar quarter levels are shown in square brackets.

Table 2.8 Does target leverage have a greater effect on bond and CDS prices for investment grade firms relative to speculative grade firms? Does it have a greater effect when leverage is lower than target leverage compared to when it is higher than target leverage?

Panel A: Dependent variable is bond spread

Independent variables	Leverage < Target leverage (8.1)	Leverage > Target leverage (8.2)	Investment grade (8.3)	Speculative grade (8.4)
Leverage	1.31	0.41	0.17	1.46
	(6.40), [2.37]	(2.75), [0.84]	(4.49), [1.20]	(3.91), [1.62]
Target leverage	1.87	3.93	1.79	5.35
	(10.08), [3.09]	(9.46), [2.41]	(20.00), [5.73]	(6.05), [2.75]
Historical volatility of firm	3.52	5.81	1.52	4.63
	(11.36), [5.09]	(16.25), [8.24]	(17.25), [5.17]	(7.37), [4.75]
Log total assets	-11.33	-12.73	-9.35	-5.75
	(-9.48), [-3.45]	(-9.03), [-3.04]	(-21.44), [-5.99]	(-0.97), [-0.39]
Coupon	10.31	15.30	8.34	19.70
	(8.60), [4.19]	(9.32), [4.07]	(15.48), [6.89]	(3.99), [1.95]
Years to maturity	1.81	1.62	1.94	2.48
	(14.92), [6.96]	(11.26), [4.92]	(26.41), [11.38]	(5.75), [3.46]
Bond age	0.35	2.12	-1.43	3.40
	(0.69), [0.36]	(2.47), [1.03]	(-6.42), [-2.99]	(2.27), [1.19]
10-year Treasury bond yield	-0.03	-0.22	-0.00	-0.42
	(-0.78), [-0.37]	(-3.47), [-1.33]	(-0.05), [-0.02]	(-2.29), [-3.88]
Term spread	-0.33	-0.57	-0.20	-0.42
	(-16.25), [-5.94]	(-17.70), [-5.79]	(-20.32), [-4.63]	(-2.84), [-1.86]
Default spread	-0.66	-1.20	-0.19	0.74
	(-6.34), [-2.74]	(-9.20), [-4.69]	(-4.42), [-1.35]	(1.13), [0.90]
Market volatility (VIX)	5.67	7.28	3.44	13.04
	(12.97), [7.26]	(10.90), [4.75]	(18.77), [4.09]	(7.09), [8.79]
Market return	0.79	1.32	0.40	0.50
	(5.89), [3.45]	(8.06), [2.95]	(7.21), [2.22]	(0.61), [0.52]
Constant	-39.54	-13.11	3.71	-256.63
	(-1.51), [-0.67]	(-0.45), [-0.20]	(0.35), [0.12]	(-2.05), [-1.37]
Adjusted R-square	0.420	0.601	0.464	0.411
Observations	3,443	3,137	5,815	765

Table 2.8 Continued

Panel B: Dependent variable is CDS spread				
	Leverage < Target leverage	Leverage > Target leverage	Investment grade	Speculative grade
Independent variables	(8.5)	(8.6)	(8.7)	(8.8)
Leverage	1.91	2.35	0.84	2.86
	(7.80), [3.92]	(11.99), [5.51]	(10.11), [4.09]	(13.30), [6.15]
Target leverage	2.49	4.30	1.59	1.45
	(10.75), [4.14]	(11.92), [5.08]	(10.06), [3.81]	(3.08), [1.41]
Historical volatility of firm	5.04	7.65	2.49	6.03
	(19.06), [9.09]	(27.73), [12.64]	(16.74), [7.04]	(16.68), [7.95]
Log total assets	-9.49	-16.64	-8.58	2.94
	(-6.33), [-2.84]	(-7.78), [-2.80]	(-11.90), [-4.42]	(0.60), [0.26]
10-year Treasury bond yield	-0.06	-0.10	0.02	0.12
	(-1.32), [-0.46]	(-1.52), [-0.70]	(0.69), [0.35]	(0.98), [0.81]
Term spread	-0.16	-0.33	-0.04	-0.04
	(-7.04), [-3.03]	(-10.92), [-5.23]	(-3.01), [-1.33]	(-0.68), [-0.38]
Default spread	-0.40	-0.53	0.08	0.52
	(-3.47), [-1.31]	(-3.23), [-1.48]	(1.31), [0.71]	(1.76), [1.80]
Market volatility (VIX)	2.08	2.70	0.80	3.94
	(5.06), [2.78]	(4.72), [3.63]	(3.41), [2.37]	(3.95), [2.74]
Market return	0.61	0.94	-0.05	-0.13
	(4.17), [2.17]	(4.87), [2.57]	(-0.63), [-0.31]	(-0.34), [-0.26]
Constant	-52.29	-81.93	-21.94	-292.63
	(-2.01), [-1.02]	(-2.30), [-1.18]	(-1.49), [-0.68]	(-3.79), [-2.21]
Adjusted R-square	0.342	0.556	0.277	0.392
Observations	3,562	2,968	4,925	1,605

Note: The first sample in Panel A includes bonds with all maturities and credit ratings for which the required quarterly data are available during 2002-Q2 to 2007-Q3. This sample is bifurcated based on whether leverage is lower or higher than target leverage and bond rating. The second sample in Panel B includes MR-class CDS during 2000-Q4 to 2007-Q4. This sample is similarly bifurcated. Appendix 1 describes the model used to estimate the target leverage, and Appendix 2 defines the variables analyzed. The leverage trend variable is dropped from this table as it is correlated with the bifurcating variable of leverage relative to target leverage. White's heteroscedasticity-adjusted *t*-statistics are shown in round parentheses, and two-way clustered *t*-statistics that adjust for clustering at the firm and the calendar quarter levels are shown in square brackets.

Table 2.9 Does target leverage affect credit ratings? Does target leverage affect bond and CDS spreads after adjusting for its effect on credit ratings?

Panel A: Summary statistics of variables that affect credit rating (N=37,030)

Variable	Mean	Q1	Median	Q3
Rating	12.1	10.0	12.0	15.0
Book leverage (percent)	34.5	21.8	31.8	44.0
Target leverage (percent)	31.6	26.6	31.2	36.3
Historical volatility of firm (percent)	40.0	27.0	35.6	48.2
Total assets ($ billions)	5.05	0.88	1.99	4.86
Return on assets	1.01	0.27	1.05	1.92
Interest coverage ratio	8.8	2.3	5.1	10.3
Capital intensity	50.3	13.2	45.1	79.0
Loss dummy	0.04	0.00	0.00	0.00
Term spread (basis points)	91	18	55	179
Default spread (basis points)	83	68	79	92

Panel B: Dependent variable is credit rating

	Pooled regression	Fama-Macbeth regression
Independent variables	(9.1)	(9.2)
Leverage	0.03	0.03
	(39.57), [9.78]	(19.28)
Target leverage	0.04	0.08
	(18.51), [3.86]	(27.69)
Historical volatility of firm	0.04	0.08
	(48.44), [8.04]	(28.09)
Log total assets	-1.22	-1.16
	(-110.48), [-20.12]	(-74.46)
Return on assets	-0.24	-0.17
	(-27.19), [-8.10]	(-13.98)
Interest coverage	-0.02	-0.03
	(-19.09), [-4.74]	(-16.98)
Capital intensity	-0.01	-0.01
	(-24.00), [-5.20]	(-9.96)
Loss dummy	0.79	0.19
	(11.51), [4.22]	(2.33)
Term spread	-0.00	
	(-21.29), [-1.61]	
Default spread	0.02	
	(31.63), [2.73]	
Constant	16.80	15.00
	(130.91), [22.09]	(89.29)
Adjusted R-square	0.555	0.677
Number of time periods	64	64
Observations	37,030	37,030

Table 2.9 Continued

Panel C: Dependent variable is credit spread				
	Effect of rating on bond spreads		Effect of rating on CDS spreads	
Independent variables	(9.3)	(9.4)	(9.5)	(9.6)
Leverage		0.47 (5.74), [1.45]		1.70 (17.31), [7.45]
Credit rating	21.52 (38.75), [8.12]	14.54 (24.93), [7.05]	30.94 (51.78), [17.11]	22.47 (36.75), [13.10]
Target leverage		1.14 (7.68), [1.88]		0.70 (3.92), [1.65]
Leverage trend during last eight quarters		6.41 (18.24), [5.80]		4.77 (11.10), [4.59]
Historical volatility of firm		3.15 (14.28), [5.91]		3.96 (22.52), [11.34]
Log total assets		-0.88 (-0.82), [-0.25]		6.75 (5.60), [2.05]
Coupon		5.40 (5.07), [2.21]		
Years to maturity		1.79 (21.02), [8.68]		
Bond age		1.81 (3.91), [1.67]		
10-year Treasury bond yield		-0.12 (-3.53), [-1.25]		-0.03 (-0.81), [-0.35]
Term spread		-0.29 (-16.93), [-5.43]		-0.05 (-1.31), [-0.61]
Default spread		-0.61 (-7.66), [-2.97]		-0.12 (-1.31), [-0.61]
Market volatility (VIX)		5.54 (16.19), [6.17]		1.50 (4.95), [2.68]
Market return		0.76 (7.92), [2.79]		0.07 (0.62), [0.29]
Constant	-81.09 (-15.90), [-3.35]	-145.85 (-7.75), [-3.02]	-220.14 (-37.57), [-11.49]	-366.31 (-16.63), [-6.56]
Adjusted R-square	0.425	0.610	0.458	0.619
Observations	6,575	6,575	6,462	6,462

Note: The cross-sectional model for determinants of bond credit ratings is based on Ashbaugh-Skaife, Collins, and LaFond (2006). The credit ratings are available from 1992-Q1 to 2007-Q4, so we include all 37,030 firm-quarters during this period for which the information on relevant firm characteristics is available. In Panel A we report the firm characteristics, which are further defined in Appendix 2. In Panel B we report the cross-sectional model of credit ratings. Note that higher credit rating numbers reflect higher default risk (AAA=2, AA+=4, AA=5, …, CC=23, etc.). The pooled regression reports White's heteroscedasticity-adjusted *t*-statistics in round parentheses and two-way clustered *t*-statistics that adjust for clustering at the firm and the calendar quarter levels in square brackets. The Fama-Macbeth regression reports averages and *t*-statistics of coefficients obtained from 64 quarterly regressions. In Panel C we expand Model (3.4) for bond spreads and Model (4.4) for CDS spreads by including the credit ratings.

Table 2.10 Are tests of leverage based on the tradeoff theory robust in the presence of controls for pecking order theory and market timing theory?

Independent variables	Dependent variable: bond spreads (10.1)	Dependent variable: CDS spreads (10.2)
Leverage	0.92 (10.88), [3.01]	2.65 (24.17), [9.14]
Target leverage	2.42 (15.85), [4.15]	3.10 (16.17), [5.81]
Leverage trend during last eight quarters	3.52 (10.26), [3.53]	2.32 (4.79), [1.97]
Financing deficit	-0.55 (-3.14), [-1.83]	-0.24 (-1.56), [-0.73]
Prior year excess return	-0.46 (-8.54), [-3.57]	-0.28 (-5.63), [-3.22]
Historical volatility of firm	5.16 (23.24), [8.53]	6.80 (36.51), [14.08]
Log total assets	-14.04 (-15.24), [-5.09]	-12.46 (-9.93), [-3.53]
Coupon	14.17 (13.37), [5.05]	
Years to maturity	1.69 (17.65), [6.87]	
Bond age	1.12 (2.20), [0.90]	
10-year Treasury bond yield	-0.13 (-3.50), [-1.15]	-0.07 (-1.74), [-0.49]
Term spread	-0.44 (-23.31), [-6.14]	-0.24 (-12.87), [-4.34]
Default spread	-1.18 (-13.69), [-4.38]	-0.65 (-6.44), [-1.87]
Market volatility (VIX)	6.28 (16.14), [5.92]	2.37 (6.69), [3.17]
Market return	1.00 (9.50), [2.87]	0.75 (6.24), [2.20]
Constant	11.35 (0.58), [0.22]	-72.90 (-3.23), [-1.39]
Adjusted R-square	0.545	0.511
Observations	6,513	6,464

Note: Models (9.1) and (9.2) test the results of Models (3.4) and (4.4) by including two additional control variables. The first variable is financing deficit and it controls for the pecking order theory: firms with greater financing deficit are more likely to raise financing, and debt financing is preferred to equity financing. The second variable is prior year excess return and it controls for the market timing theory: firms which have had a recent runup in stock price are less likely to issue debt than equity. Both variables are defined in Appendix 2. For comparison, the coefficients of target leverage equal 2.52

Table 2.10 Continued

and 3.01 in Models (3.4) and (4.4) (with two-way clustered *t*-statistics of 4.06 and 5.60), the coefficients of leverage trend during last eight quarters equal 4.49 and 2.95 (with two-way clustered *t*-statistics of 3.83 and 2.41), and the adjusted R-square values equal 0.541 and 0.504.

CHAPTER 3

BONDHOLDER WEALTH EFFECTS OF FRAUDULENT REPORTING

1. Introduction

Much of the academic research focusing on the consequences of fraudulent reporting has been restricted to two stakeholders: shareholders and managers. However, financial statements are also the primary mechanism of monitoring and contracting for bondholders and any disclosure of fraudulent reporting is likely to have significant consequences for them. In addition to representing a decline in previously reported earnings, frauds represent a direct shock that tarnishes the credibility of financial statements and highlights creditors to shortcomings in the firm's disclosure process (Graham, Li, and Qiu, 2008). Corporate bonds are an important source of external financing for many U.S. corporations. By 2006, U.S. corporations had over $5.3 trillion in bonds outstanding (Bessembinder and Maxwell, 2008). Given the relevance of the corporate bond market, and the large scale expected but undocumented ramifications of fraud to bondholders, this study seeks to fill the gap in the literature by examining how the specific nature of the fraud, various firm attributes, and the governance environment can affect changes in bondholder wealth, credit ratings, and contract features of new bond issues upon disclosure of fraudulent reporting.

I find that fraud announcements trigger swift, sharp, and long lasting declines in credit ratings in light of increased uncertainty. Within just two months following the fraud, the average bond in the sample moves from being rated investment grade (Baa) to speculative grade (Ba), representing a fivefold jump in five year default probability from 2.06% to 10.57%.

While the cross section of bond returns varies widely, the average bondholder loses around 7% at fraud announcement. Fraud disclosure varies widely, and announcements with uncertain outcomes that do not provide the magnitude of the fraud at the time of announcement are associated with significantly more negative bondholder returns in the short term. Without detailed information, bondholders assume the worst, and are likely right given that long term credit rating changes measured over two years after the announcement are also significantly negatively related to the uncertainty at announcement. Moreover, consistent with Chava, Kumar, and Warga (2010), bondholder wealth effects at fraud announcement and credit rating downgrades that follow are related to the firm governance, and are significantly negative in the protection offered to managers and directors.

The revelation of financial fraud significantly changes the risk profile of the firm, as fraud increases the yield spread on new debt issues by 67% on average. Managers react to this increased cost of debt by modifying bond features. Results indicate that managers are significantly more likely to issue callable bonds in the post fraud regime. The increased incidence of callable bonds seems to suggest that managers are optimistic that the increase in the cost of debt is temporary and that the additional premium paid to issue callable bonds maximizes firm value in the long run. However, contrary to managers' optimistic beliefs, I find that fraud induced credit rating downgrades are long lasting. Once decreased, credit ratings stay significantly depressed for at least three years.

This paper adds to work in both the accounting and finance literatures on corporate fraud and accounting irregularities. Specifically, Karpoff, Lee, and Martin (2008a, 2008b) document significant market penalties for firms and managers involved in fraud. Palmrose, Richardson, and Scholtz (2004) document shareholder wealth effects while Richardson, Tuna, and Wu (2002) document characteristics of firms in involved in fraud. This paper is also related to recent work by Chava, Kumar, and Warga (2010) who document how bond covenants respond to the ex-ante risk of fraud.

Tests of new bond issues in this paper are also closely related to Graham, Li, and Qiu's (2008) study on corporate misreporting and the contracting of bank loans which was the first study to examine the impact of restatements on the debt market. While this study focuses solely on cases of fraudulent reporting, the sample of restatements in Graham, Li, and Qiu consist of both intentional irregularities and unintentional errors. Nevertheless, Graham, Li, and Qiu document that bank loans initiated after restatements are significantly costlier in terms of spread and are accompanied with tighter loan contract terms compared to loans initiated before restatements. However, the public bond market differs from the bank loan market in four important ways. First, it is much larger than the bank loan market. Second, a more precise isolation of the event is possible in the case of public bonds because they trade in a transparent, secondary market. In other words, as bonds are a traded asset they allow for the direct computation of wealth effects. Third, bonds may have protective covenants that when triggered allow the bondholders to put the bond to the firm. Thus, the impact of the restatement on bondholder wealth could be either positive or negative conditional on the bondholder protections embedded in the bond indenture. Fourth, and more importantly, unlike banks, public bondholders do not have direct access to the firm and thus depend more heavily on public disclosure and financial statements. Thus, the public bond market is characterized by a more severe asymmetric information problem compared to the bank loan market.

The rest of the paper is structured as follows. Section 2 presents a literature review, section 3 describes the data including choices regarding sample selection and variable construction, section 4 presents results, and section 5 concludes.

2. Literature Review

Research on financial restatements and fraudulent reporting received a boost after the public availability of a comprehensive sample of restatements collected by the U.S.

Government Accountability Office (GAO). The GAO study was motivated by a spate of large accounting scandals such as Enron, Worldcom, and Tyco that threatened to undermine the U.S. capital markets. The situation led Harvey Pitt, the Chairman of the SEC in 2002, to remark that he Sarbanes-Oxley Act was motivated in large part by the rise in restatements and the broader damage to investor confidence. A similar sentiment was echoed by several analysts at the time and by the GAO's 2002 report on financial restatements (GAO, 2002; Wilson, 2008).[18]

In addition to anecdotal remarks, academic studies have highlighted the negative stock reactions to restatement announcements. With the exception of Palmrose, Richardson, and Scholz (2004) and Hennes, Leone, and Miller (2008) most existing studies do not separately control for restatements resulting from unintentional errors and those resulting intentional misstatements or fraud. Palmrose, Richardson, and Scholz (2004) examine a sample of 403 restatements from 1995 to 1999 and document an average abnormal stock return of -9% over a two day announcement window. They show that the market penalizes restatements that result from fraud more heavily than those that are a result of simple errors. Hribar and Jenkins (2004) also document abnormal stock returns of -9% while Wu (2002) documents an abnormal stock return of -11.2%.

Given the importance of restatement announcements, studies have also tried to determine the characteristics of restating firms and the motivations of firms to engage in willful manipulation that may result in a future restatement. There is some disagreement about the characteristics of restating firms in the literature. Early studies document that restating firms are likely to be smaller, less profitable, slower growing, and more

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[18] The following comments highlight concern amongst analysts about the credibility of financial statements following restatements. A senior strategist from Morgan Stanley wrote in 2002 '… investors don't have confidence that the corporate numbers game will end soon. We need a restoration of trust from every entity.' An analyst at Merrill Lynch remarked the following in response to a restatement announced by McKesson HBOC, '…this clearly raises the question about whether any number can be trusted at HBOC' (Wilson, 2008).

leveraged than control firms (Kinney and McDaniel, 1989; DeFond and Jimbalvo, 1991). However, a more recent study by Richardson, Tuna, and Wu (2002) shows that restating firms have high levels of outstanding debt and higher market expectations for future earnings growth. They argue that capital market pressures to raise cheap capital is a primary motivation for firms to engage in earnings manipulation. A broad literature on earnings management has documented a comprehensive list of incentives to manipulate earnings from a desire to beat earnings benchmarks to managerial incentives that arise from excessive option based compensation (Burgstahler and Dichev, 1997; Burns and Kedia, 2006). It is likely that incentives to manage earnings are correlated with frauds announced in the future.

Given the broad based negative impact of fraud announcements, it is fair to ask whether managers face any penalties as a result. Desai, Hogan, and Wilkins (2006) study the reputational penalties to managers and document that 60 percent of restating firms experience a turnover of at least one top manager within 24 months of the restatement. They show that subsequent employment prospects of displaced managers are poorer than those of displaced managers of control firms. Similarly, Karpoff, Lee, and Martin (2008a, 2008b) document significant market penalties for firms and managers that are targeted by SEC enforcement actions. Even if managers are not displaced, Cheng and Farber (2008) show that their option based compensation declines significantly in the two years following restatement. This decrease is accompanied with a decrease in the riskiness of investments as proxied by a lower stock return volatility and subsequent improvements in operating performance.

3. Data

3.1. Data sources

Table 3.1 describes the sample selection methodology. In 2002, the GAO collected a sample of 919 restatements that occurred between January 1997 and June 2002. In 2006, the sample was updated to include 1,390 restatements from 2002 through 2006[19]. The GAO database includes restatements due to both intentional misstatements, or accounting irregularities, and unintentional misstatements, or errors[20]. Thus, after removing duplicate restatement announcements that refer to the same underlying issue, I drop cases that are classified as unintentional errors by Hennes, Leone, and Miller (2008)[21]. This allows the study to focus solely on cases of fraudulent reporting. Since only restatements due to frauds remain, the announcement date of the restatement is taken to be the fraud revelation or the fraud announcement date.

I further remove firms that are not classified as share code 10 or 11 on CRSP and those that do not have a long term credit rating and basic firm data used in the study on Compustat. I then intersect this data with FISD's bond issue database to ensure that an event firm has a bond outstanding at the time of announcement. The FISD issue sample is restricted to standard US corporate bonds. Specifically, bonds which are convertible, asset backed, have floating coupons or credit enhancements, are not issued in US dollars, or are not domiciled in the US are deleted. In addition, bonds that are not senior unsecured obligations of the issuing firm are deleted. While these restrictions reduce sample size, they are necessary to ensure that inferences are related to the event under consideration and are not driven by idiosyncratic bond characteristics. Bond prices are then hand collected from monthly Moody's/Mergent bond records. A total of nine

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[19] The GAO database does not include a number of smaller public companies that have restated in the 2002-2005 period. As bond-issuing firms are typically larger in terms of market value than the average public firm, this apparent limitation of the GAO database is unlikely to have an impact on the final sample.

[20] Technically the sample of frauds is based on accounting irregularities. However, as Hennes, Leone, and Miller (2008) note, the distinction between fraud and irregularities has diminished over time and auditing guidelines (SAS No. 82, AICPA 1997) use ‘fraud’ to refer to all intentional misstatements.

[21] I thank the authors for generously making this classification publicly available.

observations are dropped because of insufficient data on bond prices. The final sample used to study bondholder wealth effects consists of 77 firms and 290 bonds.

A further six observations are dropped because of missing bond specific credit rating data on FISD. Thus, the sample used to study credit rating changes is slightly smaller and consists of 69 firms and 280 bonds.

In addition to studying outstanding bonds, this paper also examines the impact of fraudulent reporting on new bond issues. I download data on new corporate bond issues from 1990-2008 from SDC's global new issues database. In order to be consistent with tests on bondholder wealth effects, I exclude bonds that are convertible, asset-backed, have floating coupons or credit enhancement features. As some tests in this section require callable bonds, I retain them in the sample and report tests with and without callable bonds or by controlling for their presence. In addition to the issue date, I download bond specific data such as the coupon, offering yield to maturity, offering yield spread, net proceeds from issue, and gross spread from SDC. This sample is then merged with the base sample of firms as identified in Table 3.1, with an additional restriction to only include issues of firms that have issued bonds both before and after the fraud announcement. Macroeconomic characteristics, such as the default spread and term spread are computed from daily data series available in the St. Louis Federal Reserve's FRED database.

3.2. Variable definitions

Firm characteristics are calculated using data from the last available annual report on the fraud announcement date. Total assets is Computstat #6. Book to market is Compustat #60/(#25*#199). Book leverage is Compustat (#9 + #34)/#6. Return on assets is Compustat (#178/#6).

The prior year return is computed using daily return data on CRSP and is defined as the firm's market adjusted return over a one year period ending two days prior to the announcement date. Annualized volatility is computed using daily returns over the same horizon.

The number of years restated was hand collected from the press releases around announcement sourced from Factiva and is equal to the number of years of financial statements affected by the fraud if mentioned, and one otherwise. The pre SOX dummy is equal to one if the fraud occurred in 2002 or prior, and zero otherwise. In a significant number of cases, the firm’s disclosure on the announcement date does not include details regarding the magnitude and severity of the fraud if internal or external investigations are still in progress. This lack of disclosure results in a significant uncertainty with regards to pricing of the firm’s securities.

For example, on March 12, 1997 Unison Healthcare’s corporate bonds lost value after S&P placed it on a negative credit watch with an accompanying note stating that ‘*the lack of clarity with respect to the nature of the write-off and whether it represents any ongoing fundamental change in Unison's business is of issue* (DJ Newsires, 1997).’

On the other hand, a firm may resolve pending uncertainty about its disclosure and be positively rewarded by the market. For example, Bally’s bonds sharply rallied on clearer accounting on 12 March, 2004:

> Bally's 9.875% senior subordinated notes due 2007, rated B3 by Moody's Investors Service and single-B minus by Standard & Poor's, were quoted at 84 cents on the dollar bid. The company's 10.5% senior notes due 2011 and rated B2 by Moody's and single-B by S&P were at 95. Both of those issues have gained about five points, or cents on the dollar, since the earnings were released on Thursday evening. The gains, however, didn't come as a result of strong results from the gym and health club operator. Rather, the market reacted to the company's new, clearer accounting system, which reports revenues when they are collected and eliminates the confusion that has plagued Bally's watchers for years and has driven the bonds lower. "This accounting has always been really

> confusing," said Phelps Hoyt, a high-yield analyst at KDP Advisor, a fixed income investment advisory firm. "If you don't know what's going on, you trade at a discount (Covel, 2004)."

In order to proxy for the effects of complete or incomplete disclosure at the time of announcement, the uncertain outcome dummy is equal to one if the firm's disclosure does not mention the source and quantify the magnitude of the irregularity, or includes wording to suggest that an internal or external investigation is under way and additional information would be forthcoming.

The CEO protection index and the Director protection index are based on protection risk factors identified in Gompers, Ishii, and Metrick (2003) and available in the Riskmetrics governance database. Specifically, the CEO protection index is the sum of the following three indicator variables scaled by three: golden parachute, compensation plan, and executive severance agreement. Similarly, the director protection index is the sum of the following three indicator variables scaled by three: director liability protection, director indemnification, and director indemnification contracts.

4. Results

4.1. Summary statistics

Table 3.2 classifies the sample used to study bondholder wealth effects by restatement characteristics. As a firm may have multiple bonds, the sample includes 77 firms and 290 bonds issued by those firms. Panel A lists the number of bonds and firms by the primary account used for misreporting. Panel B lists the number of bonds and firms by the original prompter of the fraud as classified by the GAO. It is important to note that while restatements attributed to the SEC or the auditor were likely initiated by them; restatements attributed to the company may not necessarily be due to an internal review (Hribar and Jenkins, 2004). If data required for classification across dimensions in Panel

A or B was missing in the GAO sample, an attempt was made to update it through searches on EDGAR and Factiva.

Panel A shows that the majority of the frauds in the sample occur due to misreported revenue. Core restatements, defined as the ones affecting core revenue, cost, or expense accounts, affect a total of 132 bonds and 34 firms in the sample. Panel B shows that externally forced restatements, defined as those prompted by the SEC or the external auditor, affect 86 bonds and 18 firms in the sample.

Table 3.3 presents summary statistics of firms in the sample. The mean (median) firm in the sample has total assets of $53.3 ($11.0) billion. Thus, the representative firm in the sample is economically significant in size and much larger than the average restating firm in the GAO sample. The sum of total assets of all firms in the sample is around $4.1 trillion. The mean (median) firm has a book leverage of 37.5% (31.5%), a book to market ratio of 0.59 (0.42), and return on assets of 6.1% (5.1%). The mean firm exhibits a slight drop in its stock price in the year preceding the fraud announcement as seen by the mean market adjusted return of -5.9%. The annualized stock return volatility over the same horizon is around 47.5%.

Table 3.3 also presents additional summary statistics related to the extent of the actual fraud. If the number of years restated is disclosed at the time of announcement, it is coded as presented. If the number of years is not disclosed, however, it is coded as one. While this measure is likely biased downward, it presents a fair understanding of the duration of the fraud. The mean (median) number of years restated due thus coded is 2.7 (2). In addition, the uncertain outcome dummy has a mean value of 0.52 implying that slightly more than half of the sample firms do not disclose complete details related to the magnitude or extent of the fraud at or close to the announcement date.

The CEO protection index and the director protection index are each constructed using three governance indicators and scaled by three. They can thus take the value of 0, 0.33, 0.67, or 1 with higher numbers indicating increased protection offered to the CEO or the director concerned. The mean (median) CEO protection index is equal to 0.54 (0.67) and the mean (median) director protection index is equal to 0.25 (0.33).

The mean bond in the sample has a coupon rate of 7.2% and a face value of $363 million. The sum of face values of all bonds in the sample equals $105.3 billion.

4.2. Bondholder wealth effects of fraud

4.2.1 Measuring abnormal bond returns

Bond prices for each bond in the sample for two months preceding and two months after the fraud announcement were hand collected from the Moody's/Mergent bond record. The bond record is issued at a monthly frequency and lists the best price available at the end of month. These prices allows for the computation of monthly bond returns. Next, I follow Billett, King, and Mauer (2004) and compute abnormal bond returns as the bond's monthly total return minus the monthly total return on a bond index. The primary benchmark used in the study is a set of total return indexes sourced from Barclays Capital (previously, Lehman Brothers) for investment grade and speculative grade US corporate bonds.

An alternative to computing monthly abnormal returns would be to use daily bond prices to isolate bondholder wealth effects in a shorter window around announcement. However, bond prices were not publicly available at a higher frequency before the TRACE system was introduced in 2002 thus severely restricting the sample to the post TRACE (incidentally, also post-SOX) regime. Thus, requiring TRACE data would mean the exclusion of several high profile accounting frauds at Enron, Tyco, and Worldcom from the sample that led to the passage of SOX.

Moreover, while bond prices based on bond transactions are now available at a greater frequency, TRACE does not address event study concerns of illiquidity and stale prices. A monthly window also addresses concerns of pre event information leakage as exhibited by significantly negative stock and bond returns in the month prior to the announcement[22].

4.2.2. Interpreting abnormal bond returns

The economic magnitude at which returns are considered abnormal in the bond market differs substantially from the stock market. Bond prices are typically much less volatile than stock prices. Bessembinder, Kahle, Maxwell, and Xu (2009) argue that as a typical bond earns a risk premium of 100 to 150 basis points per year, a 15 to 25 basis point move may be considered abnormal as it represents one-sixth of the historical premium.

As abnormal bond returns are highly skewed around most corporate events with means far exceeding median values, I examine both parametric and non parametric test statistics. While mean tests reflect the aggregate experience of investors, non-parametric median tests are more powerful at detecting events that affect all bonds similarly (Bessembinder, Kahle, Maxwell, and Xu, 2009). Table 3.4 thus presents a series of distributional statistics designed to give a comprehensive overview of bondholder wealth effects. These include the mean, the median, the 1st and 3rd quartiles of the return distribution, and the percentage of returns that are positive and negative.

While Table 3.4 lists wealth effects with a specific emphasis on bondholders, Panel A presents firm level abnormal stock returns for comparison. The mean (median) abnormal stock return in the 60 day window around the fraud announcement is -12.24% (-11.45%). This number is similar to the stockholder wealth effects of fraud related restatements documented in the prior literature (Palmrose, Richardson, and Scholz , 2004; Hennes, Leone, and Miller, 2008).

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[22] Nevertheless, abnormal bond returns for short announcement windows around the fraud announcement computed using TRACE are available upon request.

Panel B1 documents that the mean abnormal bond return in the (-1, 1) month window is -6.62%, while the median abnormal bond return is -0.51%. This exhibits the skewness in bond returns with some bonds (firms) being affected to a much greater degree than others as a result of the fraud. The ratio of positive to negative bond returns stands at 33:67 and the first quartile of the abnormal bond return is equal to -3.27%. Thus, the revelation of accounting irregularities results in significantly negative bond returns for the average investor. Abnormal returns computed over a longer (-2, 2) month window are slightly larger in magnitude with a mean (median) of -7.55% (-0.75%). Pre event abnormal bond returns designed to capture information leakage in the (-2, -1) month prior to the announcement are significantly negative as well with mean (median) values of -2.24% (-0.50%). However, post event returns for the window (+1, +2) are not statistically significant showing that most of the information related to the fraud announcement is captured in the pre event and event window.

In Panel B1, bond returns were computed at the bond level. However, as firms may have multiple bonds, I additionally present abnormal bond returns at the firm level in Panel B2 by constructing a weighted average bond return for each firm. While the mean abnormal bond returns at the firm level are somewhat lower than those computed at the bond level, they remain large in magnitude and statistically significant. Non parametric statistics such as the ratio of positive to negative returns are very similar at both the bond and firm level.

4.2.3. Determinants of abnormal bond returns

Next, I study the determinants of stock and bondholder wealth effects and present the results in Table 3.5. Regression (1) presents the determinants of the abnormal stock return computed over the three day announcement window. The regression indicates that a firm's financial condition, past history of restatements, and uncertainty about outcomes at announcement significantly affects stockholder wealth effects. The first restatement dummy is equal to one if the announced irregularity represents the first time the firm enters the comprehensive GAO sample and is a noisy proxy for prior restatements by the firm. As the market may have already discounted prior infractions in accounting quality at previous restatements, I hypothesize that the first restatement represents a separating equilibrium where a firm moves from the set of firms having a clean record into a set of firms that are discounted for poor disclosure. This dummy is negative and significant at the 10% level.

More importantly, fraud announcements with incomplete disclosure and uncertain outcomes at the time of announcement are associated with significantly negative stock returns. This does not necessarily imply that the firm is hiding such information, as the quantification of the irregularities maybe under internal or external investigation. Nevertheless, this creates uncertainty for investors as they are made aware of the presence of a fraud, but not of its extent. It is also possible that the larger, more expansive frauds require substantially more time and expertise to quantify and the firm while obligated to release that investigations are underway is not able to release the magnitude of the fraud at the same time. Thus, in addition to proxying for the uncertainty, or variance effect, the dummy may also proxy for the magnitude, or mean, effect.

Regressions (2) – (4) study the determinants of bondholder wealth effects. These regressions are run at the issue level with heteroskedasticity robust clustered t-statistics at the firm level.

Regression (2) presents the determinants of bondholder wealth effects in the (-2, -1) month pre announcement window. Results indicate that pre announcement returns are negative and significant for large firms. This is likely because large firms face greater scrutiny, have more coverage by analysts, and attract the attention of a larger number of investors.

Regressions (3) and (4) present the determinants of the bondholder wealth effects in the announcement window. Regression (3) uses all available observations of 290 bonds across 77 firms whereas regression (4) is restricted to the observations where governance data from Riskmetrics is available. This restriction reduces the sample to 144 bonds across 33 firms. Book to market is positive and statistically significant in both regressions signaling that fraud announcements negatively affect bonds of growth firms substantially more than value firms. Core account is positive and significant in regression (4). This result is inconsistent with prior studies of restatements that have found that restatements involving the core revenue, expense, or cost accounts result in larger penalties (Palmrose, Richardson, and Scholtz, 2004; Hribar and Jenkins, 2004). However, these studies focus on restatements at large and not on the subsample of restatements involving intentional misreporting. Several instances of large frauds in the sample involve off balance sheet accounts, related party transactions, and restructurings that may not necessarily involve core accounts. Consistent with prior studies, frauds revealed by the external auditor or the SEC are associated with significantly more negative bond returns. In addition, similar to the regression on abnormal stock returns, both the uncertain outcome dummy and the first restatement dummy are negative and significant.

Any disclosure of accounting improprieties also calls into question the role and oversight of a variety of agents including the management and board of directors. Core (1997) argues that the availability and extent of Directors' liability insurance proxies for a firm's litigation and distress risk. While it is possible that directors at these high risk firms demand insurance and indemnification (and that firms supply them), it is also probable that these risk reduction measures may reduce the quality of governance in the firm (Gompers, Ishii, Metrick, 2003). Results indicate that firms that have greater protections for their CEO and board of directors are associated with significantly negative bondholder returns. This result suggests a link between firm governance attributes and the extent of bondholder loss.

4.3. Credit ratings changes

The previous section documents that frauds adversely affects outstanding bonds and reduces bondholder wealth. However, does the disclosure of accounting irregularities affect a firm's cost of issuing corporate bonds in the long run? Indeed, any material reassessment of a firm's financial position will change the market's estimate of the firm's future profitability and tarnish the credibility of the firm's financial disclosure. Credit rating agencies are likely to react by lowering ratings as the firm's probability of default is revised in light of new information and greater uncertainty. Clearly, any downgrade in the firm's credit rating will increase the cost of future borrowing for the firm and reduce the number of positive NPV projects that the firm can undertake.

In some cases a credit rating downgrade may have a snowball effect on the firm's finances regardless of whether the firm decides to issue any new debt. For instance, the New York Times reported the following on October 11, 2005 after Refco's disclosure that financial statements were materially misstated:

> 'After Refco's disclosure yesterday, Standard & Poor's lowered its credit rating to B+ from BB- and placed it on credit watch with negative implications, indicating a further downgrade was possible. Both ratings are below investment grade.
>
> Tom Foley, an analyst with the credit-rating agency, said bondholders and banks that had lent Refco money could use the latest disclosure to force it to repay the debt sooner than scheduled, putting Refco under financial pressure. Also, he said, "this indicates a possibility that there could be other accounting control issues (Norris and Anderson, 1995).'

Thus, if the bond indenture includes covenants that require the firm to maintain a particular credit rating, bondholders may have an option to put their bonds before maturity. While not all bonds have an explicit credit rating covenant, prepayment pressure to a firm's finances may still arise if a restatement requires the firm to delay filing a report to the bond trustee and triggers a timely reporting covenant.

In this section, I empirically document the impact of the fraud announcement on credit ratings as they imply longer term changes in a firm's cost of debt. As shown in Appendix A, I code Moody's bond ratings from 20 to 1 where 20 refers to a Aaa rating and 1 refers to a Ca-C rating. Ratings 20 to 11 are investment grade and ratings 10 and below are speculative grade.

Table 3.6 presents the average credit rating of bonds in the sample at two year and two month windows around the announcement date. The average credit rating for the 280 bonds in the sample two months before announcement is 11.31 (Moody's rating 11=Baa3). This drops to 9.86 two months after the announcement (Moody's rating 9 = Ba2). While this represents just two notches on the scale of ratings, it represents an economically significant change in categories from investment grade to speculative grade (Appendix A). Appendix B indicates that the average 5 year default rate for US corporate bonds rated Ba at 2.06% is approximately five times the default rate of bonds rated Baa at 10.57%. In other words, on average, the revelation of fraud increases default risk as measured by five year default rates by a factor of five. Difference t-tests presented in Table 3.6 indicate that the decline in credit ratings as seen in the (-2, +2) month and (-24, +24) month event windows are statistically and economically significant. In addition to the results for the overall sample, Table 3.6 also presents credit rating changes in the pre SOX and post SOX regimes, credit rating changes for the subsample of firms that have limited disclosure regarding the irregularity at announcement (uncertain outcome dummy=1), and for bonds that were on negative credit watch at the time of the irregularity announcement.

Figure 3.1 shows the evolution of bond credit ratings in the (-36, +36) month window around the fraud announcement. Credit ratings are sampled each month and the last updated credit rating is assigned to the bond. Note that as higher numbers reflect more sound credit ratings, a drop in the line reflects a credit rating downgrade. The shaded region represents credit ratings that are investment grade. The figure shows that credit ratings stabilize at the downgraded level and do not revert back to pre fraud announcement levels even three years after the announcement. Thus, credit rating downgrades induced by the disclosure of intentional misreporting are likely to be long lasting.

Figure 3.2 shows the percentage of bonds in the sample that are under a negative credit watch in event time. The percentage of bonds under negative credit watch significantly increases from six months prior to the announcement and keeps on increasing until six months after the announcement. The increase in percentage of bonds under negative credit watch prior to the fraud announcement indicates that the bond rating agency suspects deteriorating credit quality either due to faltering fundamentals, or suspicion about the firms accounting quality. This increased incidence of negative credit watch is consistent with negative pre announcement bond returns presented in Table 3.4.

Regressions (5)-(9) in Table 3.7 are designed to explain the short term change in credit rating from two months before the irregularity announcement to a two months after the irregularity announcement; while regression (10) is designed to explain the long term change in credit ratings from two months before the announcement to two years after the announcement.

Regression (5) explains the change in credit rating using the abnormal bond return computed in the (-1, 1) month window. Abnormal bond returns can explain around 75% of the variability in credit ratings. This does not necessarily suggest that information in bond returns is being captured by the credit rating agency; as the causality can just as likely be in the other direction where changes in credit ratings result in changes in bond prices, and thus bond returns. Regression (6) repeats the regression with abnormal stock returns. While stock returns can also significantly explain the short term change in credit ratings, they do not explain as much of the variability in ratings as bond returns. Regression (7) shows a regression that includes both stock and bond returns as covariates and shows that bond returns are significantly more tied to the changes in credit ratings than stock returns, as expected.

Regressions (8) – (10) substitute both abnormal stock returns and abnormal bond returns and regress the change in credit rating on firm and fraud specific variables. Results are broadly consistent with regressions on bondholder wealth effects. In other words, factors that explain bond returns also significantly explain changes in credit ratings.

Regression (9) indicates that credit rating downgrades in the (-2, 2) month announcement window are much more likely for large, growth firms with low ROA. In addition, frauds that have uncertain outcomes at the time of announcement are likely to be resolved to the detriment of the firm as seen by the economically and statistically significant negative coefficient on the uncertain outcome dummy. CEO and director protection are significantly related to short window credit rating downgrades. However, their influence declines but remains significant in regression (10) that explains the long window change in credit ratings.

Thus, announcements of intentional misreporting increase the cost of accessing public debt markets as proxied by the change in the firm's credit ratings.

4.4. Does accounting fraud increase the cost of new bond issues?

In this section, I study new issues of corporate bonds by firms that have announced accounting frauds in the pre announcement and post announcement period. Table 3.8 lists summary statistics for the sample of new bond issues obtained from SDC between 1990 and 2008.

Table 3.9 is inspired by Table 3 in Graham, Li, and Qiu (2008). It lists regressions of the log bond spread at the time of issue on firm specific, bond specific, and macroeconomic control variables. The regressions include an after fraud dummy that is set to one if the new bond was issued after the fraud announcement, and zero otherwise. The coefficient on the after fraud dummy highlights the actual impact of the fraud announcement on new bond issues. The regressions include a variety of firm specific factors such as leverage, volatility, firm size, book-to-market, and return on assets current as of the time of bond issue. They also include bond specific variables such as years to maturity, bond coupon, and a dummy that identifies whether a bond is callable. Finally, macroeconomic factors such as the default spread and the term spread at the time of issue are also included.

Regressions (11) and (12) include both callable and non callable bonds, while regressions (13) and (14) are restricted to only non callable bonds. After controlling for these known factors that affect spreads, results indicate that a the bond yield spread rises by 38% to 67% as exhibited by the coefficient on the after fraud dummy in regressions (12) and (14). This result is consistent with the downgraded credit ratings exhibited in Figure 1 and is also consistent in magnitude with findings in Graham, Li, and Qiu (2008) concerning the impact of restatements on private bank loan debt.

4.5. Are contract terms for new debt issues affected by the revelation of accounting fraud?

The sections above have documented a fairly significant decline in bondholder wealth, drop in credit ratings, and increase in the cost of new debt issues due to revelation of accounting fraud. Finally, I explore if accounting frauds alter additional parameters related to new bond issues such as the total amount of proceeds, the gross spread charged for bond issuance, and option features. Table 3.10 presents these results in regressions (15) through (20).

Regression (15) explains bond issue proceeds using the after fraud dummy with firm and market specific controls. The after fraud dummy is insignificant. Thus, while results in this paper show that the revelation of fraud affect the cost of new bond issues, they do not seem to significantly affect the size of the new bond issue.

Firms tend to issue callable bonds when they have a pending need for capital but deem their current borrowing rate to be higher than their borrowing rate in the long term. I test whether firms are more likely to issue callable bonds after fraud announcement using a logistic specification in regressions (17) and (18). As the firm faces material increases in the cost of debt financing, an increase in the incidence of callable bonds in the period after fraud would suggest that firm managers optimistically believe, first, that the increase in the cost of debt as a result of the fraud is temporary and, second, that the additional premium paid to issue callable bonds maximizes firm value in the long run. The results are consistent with managerial optimism and indicate that firms are significantly more likely to issue callable bonds after fraud even though Figure 1 suggests that corporate bond ratings sharply decline at announcement and remain significantly depressed at least over the following three years.

In unreported tests, I find that there is no significant difference in the maturity of bonds issued after fraud. At first glance this result may seem inconsistent with the Graham, Li, and Qiu (2008) result documenting shorter loan maturity periods for loans issued after a restatement. However it is easy to see that an embedded call option can effectively reduce the maturity of the bond without explicitly reducing the promised bond maturity in the bond indenture.

Finally, in regressions (19) and (20), I test whether the gross spread charged by the investment bank to issue corporate bonds increases after fraud announcement. The after fraud dummy has a coefficient equal to 0.27% indicating an approximately quarter percent increase in fee in the post fraud period. For the median bond in the sample with a face value of $250 million, a 0.27% increase amounts to a non trivial increase of $675,000 from prior levels. I suggest two reasons for the increased fee. First, an incidence of intentional misreporting is a negative signal and it is more difficult to find investors for bonds of firms with a history of questionable reporting. Second, investment banks may have to conduct additional due diligence to vet the issuer's financial statements.

The results in this section indicate that while a firm that seeks to access the public debt market following a fraud is generally not limited in the amount of debt it can raise, it faces both an increased yield spread and an increased gross spread to compensate investors for their increased risk.

5. Conclusions

In summary, I show that the revelation of accounting fraud significantly affects creditors and the credit environment of a firm. I document economically and econometrically significant declines in bondholder wealth with the average bond losing approximately 7.6% in the two month window around the announcement. I also find that rating agencies react to frauds by significantly downgrading credit ratings where the average bond moves from being investment grade to speculative grade signaling a fivefold increase in default probabilities. The extent of bondholder wealth loss and credit rating decline depends on the type of firm and the extent to which a firm protects its management and directors. Bondholder wealth loss is also significantly related to firm's disclosure at the time of fraud announcement, with announcements with incomplete disclosure regarding fraud magnitudes being systematically worse for bondholders over the long run.

Tests of new bond issues show a 38 to 67% increase in the yield spread and a 0.27% increase in the gross spread compared to pre fraud levels. Moreover, a significant proportion of bonds issued after a fraud contain call provisions that command a higher yield at issue but may be potentially value maximizing for the firm if credit conditions improve in the long run. Thus, I argue that managers are optimistic that the increase in the cost of debt induced by the fraud is temporary. However, contrary to managers' optimistic beliefs, I find that corporate credit ratings, once decreased, remain significantly depressed for at least three years following the fraud.

The revelation of accounting fraud is a result of excessive earnings management in prior periods. Richardson, Tuna, and Wu (2002) argue that capital market pressures to raise cheap capital are a primary motivation for firms to engage in earnings manipulation. If enforcement is reasonably efficient, earnings manipulation outside the purview of GAAP is likely to result in a costly restatement. The results in this paper are consistent with significant penalties to firms that announce frauds and specifically indicate that a firm's cost of debt significantly increases after a fraud is revealed. Thus, managers who yield to short term capital market pressures and aggressively manipulate earnings to lower the cost of capital are likely to significantly destroy firm value in the long run.

Table 3.1 Sample selection

	Sample size (Number of firms)
Number of restatements in the Hennes, Leone, Miller (2008) dataset	2,705
Less duplicates	(208)
Less restatements that are not irregularities	(1,922)
Number of accounting irregularities	575
Less cases not available on CRSP as sharecode 10 or 11, and not on Compustat	(104)
Less cases without a firm credit rating, or those without an outstanding bond in FISD	(385)
Base sample	86
Less cases where bond prices are not available	(9)
Sample for computing bondholder wealth effects	77
Less cases without bond ratings in FISD	(6)
Sample for computing rating changes	69

Note: The sample of frauds was generated as follows. I start with the sample of restatements from 1997-2006 collected by the GAO, and coded into irregularities (intentional) and errors (unintentional) by Hennes, Leone, and Miller (2008). After removing duplicates, I drop observations that are classified as errors to focus on announcements of intentional misstatements. I further remove firms that are not classified as share code 10 or 11 on CRSP, and those that do not have a long term credit rating on Compustat. I then intersect this data with FISD's bond issue database to ensure that an event firm has a bond outstanding at the time of announcement. This forms the base sample used in the study. From this sample, nine observations are dropped because of insufficient data on bond prices. The final sample used to study bondholder wealth effects consists of 77 firms and 290 bonds. A further six observations are dropped because of missing bond specific credit rating data on FISD. Thus, the final sample used to study rating changes is slightly smaller and consists of 69 firms and 280 bonds.

Table 3.2 Restatement characteristics

Panel A: Primary reason for restatement		
	Number of bonds	Number of firms
Cost or expense	42	14
Revenue	90	20
Securities related	13	4
Reserves	21	5
Restructuring, assets, or inventory	58	15
Related party transactions	22	3
Other	44	16
Total	290	77

Panel B: Prompter of restatement		
	Number of bonds	Number of firms
Company	172	54
Auditor	20	3
SEC	66	15
Other	32	5
Total	290	77

Note: Panel A lists the number of bonds and restatement firms in the final sample by the primary reason for the restatement. Panel B lists the same by the prompter of the restatement. The number of bonds affected is greater than the number of firms as individual firms may have multiple bonds in the sample. The sample selection methodology is described in Table 3.1 and consists of frauds from 1997 to 2006.

Table 3.3 Summary statistics

	Number of firms	Number of bonds	Mean	Standard deviation	Q1	Median	Q3
Firm characteristics							
Total assets (in $ billions)	77	290	53.3	145.6	2.5	11.0	27.8
Book to market	77	290	0.59	0.54	0.25	0.42	0.86
Book leverage (%)	77	290	37.5	22.5	23.1	31.5	50.1
Return on assets (%)	77	290	6.1	6.2	2.3	5.1	9.7
Prior year return (%)	77	290	-5.9	48.4	-38.8	-10.3	9.6
Annualized volatility (%)	77	290	47.5	25.6	29.4	43.0	61.5
Number of years restated	77	290	2.7	1.6	1	2	4
Pre SOX dummy	77	290	0.45	0.50	0	0	1
Uncertain outcome dummy	77	290	0.52	0.50	0	1	1
CEO protection index	34	142	0.54	0.20	0.33	0.67	0.67
Director protection index	34	142	0.25	0.26	0	0.33	0.33
Bond characteristics							
Coupon (%)	77	290	7.20	1.32	6.50	7.13	7.75
Bond face value (in $ mill.)	77	290	363	427	180	250	450

Note: This table presents firm and bond specific summary statistics. Firm characteristics are calculated using data from the last available annual report at the announcement of the intentional misstatement. Total assets is Computstat #6. Book to market is Compustat #60/(#25*#199). Book leverage is Compustat (#9 + #34)/#6. Return on assets is Compustat (#178/#6). The prior year return is defined as the firm's market adjusted return over a one year period ending two days prior to the irregularity announcement date. Annualized volatility is computed using daily returns over the same horizon. The number of years restated was hand collected from Factiva and is equal to the number of years of financial statements affected by the irregularity. The pre SOX dummy is equal to one if the restatement occurred in 2002 or prior, and zero otherwise. The uncertain outcome dummy is equal to one if the firm's disclosure does not mention the source and magnitude of the irregularity, or includes wording to suggest that an investigation is under way and additional information would be forthcoming. The CEO protection index and the Director protection index are based on protection risk factors identified in Gompers, Ishii, and Metrick (2003) and available in the Riskmetrics governance database. Specifically, the CEO protection index is the sum of the following three indicator variables scaled by three: golden parachute, compensation plan, and executive severance agreement. Similarly, the director protection index is the sum of the following three indicator variables scaled by three: director liability protection, director indemnification, and director indemnification contracts. Bond characteristics such as the coupon rate and the face value are coded using Moody's/Mergent bond records.

Table 3.4 Bondholder wealth effects

	N	Mean	Q1	Median	Q3	% positive: % negative
Panel A. Stock returns						
Abnormal stock return (-1,1) days	77	-3.738**	-6.359	-2.048***	1.739	30:70
Abnormal stock return (-1, 1) month	77	-12.242***	-23.282	-11.447***	1.650	27:73
Abnormal stock return (-2, 2) months	77	-11.761***	-33.725	-11.857***	3.024	30:70
Panel B. Bond returns						
B1. Bond level						
Raw bond return (-1, 1) months	290	-6.870***	-3.444	-0.753***	0.220	29:71
Abnormal bond return (-1, 1) month	290	-6.618***	-3.268	-0.513***	0.318	33:67
Abnormal bond return (-2, 2) months	290	-7.550***	-5.962	-0.753***	0.662	36:64
Pre event return Abnormal bond return (-2, -1) month	290	-2.239***	-3.210	-0.504***	0.674	40:60
Post event return Abnormal bond return (1, 2) month	290	0.729	-0.944	-0.080	1.126	48:52
B2. Aggregated to 'one bond' per firm, weighted by face value of each bond						
Raw bond return (-1, 1) month	77	-3.123**	-2.469	-0.702***	0.382	34:66
Abnormal bond return (-1, 1) month	77	-3.139**	-2.728	-0.402***	0.286	34:67
Abnormal bond return (-2, 2) month	77	-4.071***	-4.961	-0.931***	0.379	36:64
Pre event return Abnormal bond return (-2, -1) month	77	-1.096*	-1.578	-0.437**	0.640	39:61
Post event return Abnormal bond return (1, 2) month	77	0.105	-1.128	-0.208	0.648	43:57

Note: This table documents stock and bondholder wealth effects around the fraud announcement date. Panel A lists abnormal stock returns while Panel B lists abnormal bond returns. Abnormal stock returns are computed using the market adjusted method, and are listed for various windows. The (-1, 1) days window refers to the three day window, the (-1, 1) month window refers to the (-30, 30) day window, and the (-2, 2) month window refers to the (-60, 60) day window around the irregularity announcement. Bond returns are computed using end of month bond prices published by Moody's/Mergent and are also presented over various windows. Abnormal bond returns are computed using corresponding Barclay's bond indices. Panel B1 presents bond

Table 3.4 Continued

abnormal returns at the issue level, whereas Panel B2 presents abnormal bond returns aggregated to the firm level as the weighted average of multiple issues. Significance levels for the mean t-test and the median sign rank test are indicated by ***, **, and * at the 1%, 5%, and 10% levels respectively. In addition, Q1 and Q3 refer to the quartiles of the return distribution, while the final column documents the percentage of returns within each row that are positive and negative.

Table 3.5 Determinants of bondholder wealth effects

	Abnormal stock return	Abnormal bond return (-2, 1)	Abnormal bond return (-1, 1)	Abnormal bond return (-1, 1)
	(1)	(2)	(3)	(4)
Intercept	-10.40 (-1.07)	25.74 (3.31)***	46.07 (1.68)*	130.00 (4.40)***
Log total assets	0.50 (0.63)	-1.81 (-2.81)***	-2.77 (-1.27)	-7.45 (-3.96)***
Leverage	-0.06 (-1.17)	0.05 (1.09)	0.14 (1.03)	-0.17 (-1.23)
Equity volatility	0.07 (1.50)	-0.06 (-1.07)	-0.25 (-1.67)*	0.17 (1.10)
Book to market	2.71 (0.86)	2.60 (0.95)	15.88 (2.01)**	28.50 (3.58)***
Return on assets	0.69 (1.99)**	0.12 (0.67)	0.45 (1.16)	1.65 (1.35)
Core account	2.79 (1.14)	0.63 (0.34)	3.38 (0.73)	13.54 (2.43)**
SEC/Auditor initiated	2.18 (0.83)	1.19 (0.75)	0.57 (0.11)	-23.68 (-4.25)***
First restatement	-3.56 (-1.87)*	0.62 (0.34)	-6.16 (-1.41)	-19.14 (-3.88)***
Uncertain outcome dummy	-8.43 (-3.45)***	0.40 (0.28)	-4.32 (-1.28)	-21.58 (-4.63)***
Number of years restated	1.04 (1.49)	-0.31 (-0.52)	-2.07 (-1.19)	-0.56 (-0.35)
Pre SOX dummy	-5.97 (-1.93)*	-3.29 (-1.09)	-0.38 (-0.07)	1.73 (0.29)
Director protection index				-37.25 (-4.88)***
CEO protection index				-100.43 (-8.12)***
Coupon rate		-1.46 (-3.16)***	-3.00 (-2.54)**	-0.44 (-0.47)
Bond face value x 10^{-2}		0.10 (1.08)	0.44 (1.33)	0.37 (1.26)
Adjusted R-square	0.17	0.18	0.37	0.85
N	77	290	290	144

Table 3.5 Continued

Note: This table presents the determinants of the abnormal stock return computed over the three day announcement window in regression (1). Regression (2) presents the determinants for the abnormal bond return in the (-2, -1) month pre announcement window. Regressions (3) and (4) present the determinants for the abnormal bond return in the announcement window. Regression (3) uses all available observations of 290 bonds across 77 firms whereas regression (4) is restricted to the observations where Riskmetrics data is available. This restriction reduces the sample to 144 bonds across 33 firms. Core account is coded as one if the restatement involves a revenue or a cost/ expense account, and zero otherwise. SEC/Auditor initiated is coded as one if the announcement was prompted by the SEC or the external auditor, and zero otherwise. First restatement is coded as one if the restatement announcement represents the first restatement of the firm in the comprehensive GAO database, subsequent announcements are coded as zero. Other variables are as defined in the table 3.3 legend. Bond regressions are run at the issue level and heteroskedasticity robust t-statistics clustered at the firm level are presented in parentheses. Significance levels at the 1%, 5%, and 10% levels are indicated by ***, **, and *.

Table 3.6 Changes in credit rating

Panel A : Univariate tests of the change in credit rating	N	Credit rating 2 years *prior to* restatement	Credit rating 2 months *prior to* restatement	Credit rating 2 months *after* restatement	Credit rating 2 years *after* restatement	*Change in* credit rating (-2, 2) months	*Change in* credit rating (-2, 2) years
All cases	280	12.31	11.31	9.86	8.79	-1.49***	-3.53***
Pre SOX	112	13.01	12.58	10.15	9.09	-2.43***	-3.93***
Post SOX	168	11.84	10.46	9.66	8.58	-0.80***	-3.26***
Uncertain outcome dummy	184	11.69	10.59	8.63	7.23	-1.96***	-4.46***
Negative credit watch	87	12.79	10.89	9.41	7.84	-1.47***	-4.95***

Note: This table presents the evolution of bond credit ratings around the fraud announcement. Moody's credit ratings are sourced from FISD and converted into numerical codes (given in Appendix A). Panel A presents the average credit rating change in the four month, and four year event windows. These tests are replicated for: (1) the pre SOX and post SOX sub periods, (2) observations where the uncertain outcome dummy is equal to one, and (3) observations where the bond was on credit watch at the time of the irregularity announcement. Appendix B presents cumulative default rates for US corporate bonds by credit rating. Note that as higher numbers reflect more sound credit ratings, a negative change reflects a credit rating downgrade. Significance levels at the 1%, 5%, and 10% levels are indicated by ***, **, and *.

Table 3.7 Determinants of changes in credit rating

Rating change window (in months)	(-2, 2)	(-2, 2)	(-2, 2)	(-2, 2)	(-2, 2)	(-2, 24)
	(5)	(6)	(7)	(8)	(9)	(10)
Intercept	-0.58 (-2.18)**	-0.14 (-0.42)	-0.32 (-1.20)	3.38 (1.63)	13.55 (2.88)***	4.25 (0.94)
Abnormal bond return	0.13 (6.42)***	0.07 (3.20)***	0.10 (3.79)***			
Abnormal stock return			0.02 (1.90)*			
Log total assets				-0.64 (-2.73)***	-0.68 (-2.69)**	-0.33 (-1.21)
Leverage				0.04 (1.53)	-0.03 (-1.11)	-0.01 (-0.65)
Equity volatility				-0.02 (-1.00)	0.04 (1.86)*	0.04 (3.39)***
Book to market				1.49 (1.53)	2.46 (2.24)**	2.05 (2.51)**
Return on assets				0.15 (1.86)*	0.41 (2.33)**	0.57 (4.23)***
SEC/Auditor initiated				0.18 (0.25)	-1.81 (-1.87)*	-2.91 (-2.48)**
First restatement				-0.27 (-0.52)	-1.50 (-1.80)*	-1.89 (-2.25)**
Uncertain outcome				-0.94 (-1.60)	-3.15 (-4.46)***	-4.70 (-6.10)***
Number of years restated				-0.41 (-1.81)*	-0.38 (-1.51)	0.06 (0.16)
Pre SOX dummy				-1.99 (-1.88)*	-3.16 (-2.96)***	-4.06 (-3.51)***
CEO protection index					-10.49 (-4.59)***	-4.52 (-1.81)*
Director protection index					-5.33 (-3.29)***	-4.69 (-2.54)**
Adjusted R-square	0.75	0.59	0.78	0.48	0.81	0.75
N	280	280	280	280	152	152

Note: This table presents the determinants of changes in bond credit ratings around the fraud announcement. Moody's credit ratings are sourced from FISD and converted into numerical codes (given in Appendix A). Panel A presents the average credit rating change in the four month, and four year event windows. The dependent variable in regressions (5)-(9) is the difference in credit rating between two months after the announcement and two months before the announcement, while the dependent variable in regression (10) is the difference in credit rating between twenty four months after the announcement and two months before the announcement. Abnormal bond and stock returns in regressions

Table 3.7 Continued

(5)-(7) are computed over the (-1, 1) month window. Other variables are as defined in the legends to tables 3.3 and 3.5. Significance levels at the 1%, 5%, and 10% levels are indicated by ***, **, and *.

Table 3.8 Summary statistics (new bond issues)

	N	Mean	Standard deviation	Q1	Median	Q3
Firm characteristics						
Total assets (in $ billions)	542	51.1	95.6	6.9	15.5	31.1
Book to market ratio	542	0.60	0.45	0.27	0.48	0.92
Book leverage (%)	542	32.88	15.63	21.34	30.82	43.96
Annualized volatility (%)	542	30.09	10.70	23.14	27.74	35.83
Return on assets (%)	542	3.04	3.73	1.14	2.30	4.75
Bond characteristics						
Spread (basis points)	542	132	94	70	99	167
Coupon (%)	542	6.92	1.35	6.25	6.95	7.75
Bond proceeds (in millions)	542	308	358	100	250	450
Gross spread (%)	462	0.76	0.42	0.60	0.65	0.88
Years to maturity	542	9.97	7.45	5.00	10.00	10.00
Callable dummy	542	0.28	0.45	0	0	1
Macroeconomic characteristics						
Default spread (basis points)	542	81	20	66	77	91
Term spread (basis points)	542	84	86	18	54	161

Note: This table presents summary statistics for the sample of new corporate bond issues downloaded from SDC. The comprehensive new issue sample includes bonds issued from 1990 to 2008. This sample is then merged with the base sample of firms with announced frauds as defined in Table 3.1, with an additional restriction to only include issues of firms that have issued bonds both before and after the irregularity announcement. Firm characteristics are computed using annual data available at the time of the bond issue. Bond characteristics are computed using variables directly available from SDC and macroeconomic data on the default and term spread are obtained from the St. Louis Fed's FRED database. The default spread is defined as the difference between the yields of BAA and AAA corporate bonds. The term spread is defined as the difference between the 10 year and the 2 year constant maturity treasury security.

Table 3.9 Fraud and the cost of corporate bond debt

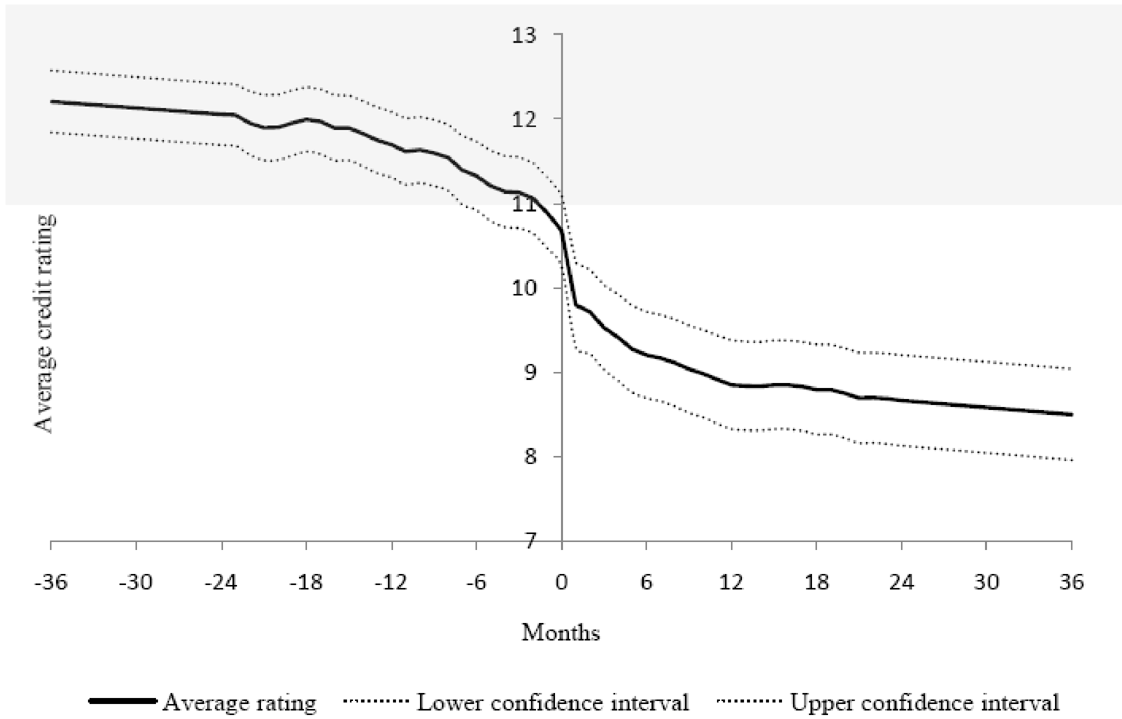
Dependent variable:	Log (spread)	Log (spread)	Log (spread)	Log (spread)
	(11)	(12)	(13)	(14)
Intercept	4.61 (132.24)***	2.66 (10.57)	4.47 (111.13)	2.98 (9.52)
After fraud	0.23 (3.05)***	0.38 (6.29)***	0.26 (2.32)**	0.67 (7.38)***
Log total assets		-0.06 (-3.50)***		-0.08 (-3.69)***
Leverage x 10^{-2}		0.72 (4.16)***		0.86 (4.05)***
Volatility x 10^{-2}		1.05 (4.16)***		0.81 (2.17)**
Book to market		0.17 (1.72)*		0.20 (1.54)
Return on assets x 10^{-2}		-2.56 (-3.44)***		-2.47 (-2.55)**
Years to maturity x 10^{-2}		0.30 (1.04)		0.71 (1.84)*
Coupon		0.20 (8.39)***		0.20 (6.96)***
Callable dummy		0.49 (9.85)***		
Default spread x 10^{-2}		0.40 (3.53)***		0.30 (1.99)*
Term spread x 10^{-2}		0.03 (1.22)		-0.01 (-0.40)
Adjusted R-square	0.01	0.45	0.01	0.37
N	542	542	388	388

Note: This table is inspired by table 3 in Graham, Li, and Qiu (2008). The dependent variable is the log yield spread to treasury (matched for maturity) at time of issue. The after fraud dummy variable takes the value of one if the bond was issued after fraud announcement, and a value of zero otherwise. Regressions (11) and (12) include both callable and non callable bonds, while regressions (13) and (14) include only non-callable bonds. Measurement details on firm, bond, and macroeconomic factors included as covariates are described in the legends to tables 3, 5, and 8. Parentheses reflect heteroskedasticity-adjusted robust t-statistics. Significance levels at the 1%, 5%, and 10% levels are indicated by ***, **, and *.

Table 3.10 Frauds and bond issue terms

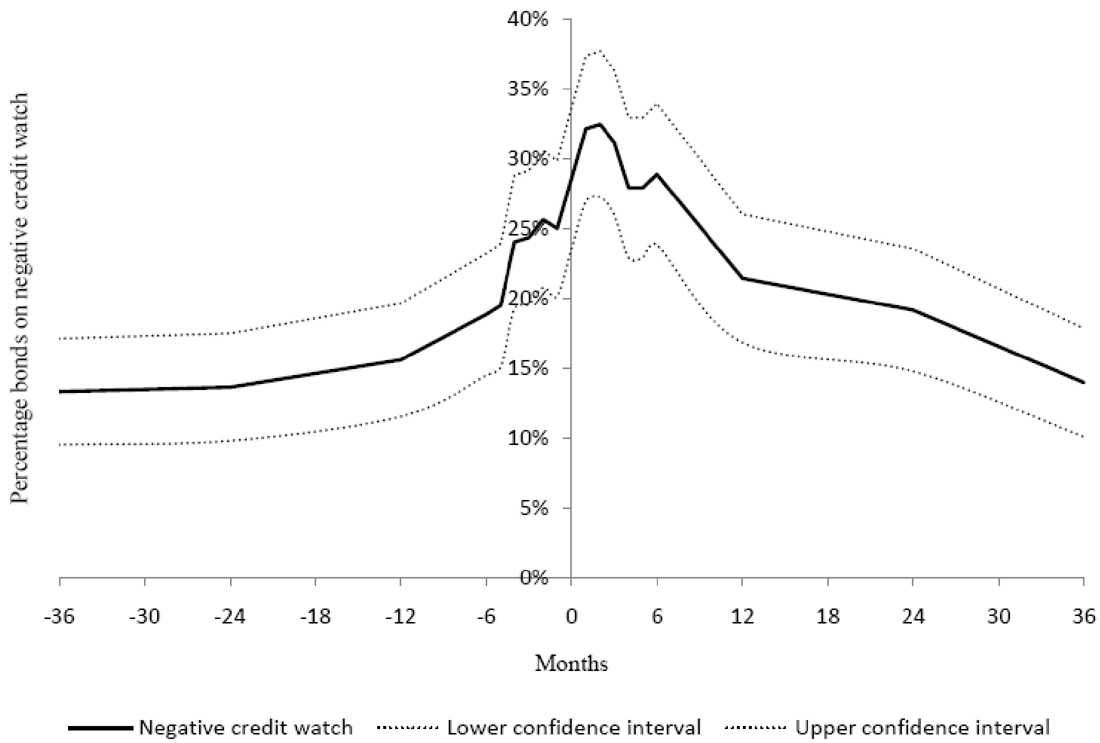
Dependent variable:	Proceeds	Proceeds	Callable dummy	Callable dummy	Gross spread	Gross spread
	(15)	(16)	(17)	(18)	(19)	(20)
Intercept	279.19 (16.05)***	-1076.53 (-5.34)***	-1.15 (-10.45)***	0.52 (0.39)	0.73 (36.48)***	0.73 (4.68)***
After fraud	175.27 (5.78)***	51.84 (1.01)	1.17 (4.90)***	1.23 (4.16)***	0.15 (2.34)**	0.27 (4.90)***
Log total assets		98.07 (5.83)***		-0.26 (-2.88)***		-0.07 (-6.92)***
Leverage		-0.07 (-0.06)		-0.02 (-2.35)*		0.01 (7.10)***
Volatility		7.90 (6.31)***		0.07 (5.60)***		0.00 (0.16)
Book to market		-21.94 (-0.41)		0.69 (1.40)		-0.06 (-1.01)
Return on assets		28.68 (3.12)***		0.01 (0.15)		-0.01 (-3.32)***
Years to maturity		4.50 (2.62)***		0.06 (3.87)***		0.01 (4.60)***
Coupon		-15.78 (-1.14)		-0.39 (-3.81)***		0.05 (3.40) ***
Default spread x 10^{-2}		178.65 (2.33)**		1.68 (2.67)***		-0.12 (-1.27)
Term spread x 10^{-2}		37.43 (2.02)**		-0.49 (-3.42)***		0.04 (2.33)
Adjusted R-square	0.03	0.373	0.04	0.17	0.01	0.449
N	542	542	542	542	462	462

Note: This table presents the effect of a fraud on new bond issue proceeds, the likelihood of having a call feature, and the gross spread as a percentage of proceeds. The after fraud dummy variable takes the value of one if the bond was issued after the fraud announcement, and a value of zero otherwise. Measurement details on firm, bond, and macroeconomic factors included as covariates are described in the legends to tables 3, 5, and 8. Regressions (15) and (16) explain the dollar proceeds raised by the bond issue. Logit regressions (17) and (18) explains the proportion of new bond issues that contain a call feature. Finally, regression (19) and (20) explain the gross spread as a percentage of proceeds, better known as the issue management fee, charged by the investment bank at the time of issue. Parentheses reflect heteroskedasticity-adjusted robust t-statistics. Significance levels at the 1%, 5%, and 10% levels are indicated by ***, **, and *.



Note: This figure presents the evolution of bond credit ratings in the six year window around the fraud announcement. Credit ratings are sampled each month and the last available credit rating is assigned to the bond. Moody's credit ratings are obtained from FISD and converted into numerical codes (given in Appendix A). Appendix B presents cumulative default rates for US corporate bonds by credit rating. Note that as higher numbers reflect more sound credit ratings, a drop in the line reflects a credit rating downgrade. The shaded region represents credit ratings that are investment grade. A credit rating code of 12 refers to a Baa2 rating with an approximate 5 year default rate of 2.06%, while a credit rating code of 9 refers to a Ba2 credit rating with an approximate 5 year default rate of 10.57 %. Dotted lines represent the upper and lower 95% confidence intervals.

Figure 3.1 Credit rating changes



Note: This figure presents the percentage of bonds in the sample that are under a negative credit watch in the six year window around the announcement of the intentional misstatement. Credit ratings are sampled each month and the last available credit rating watch information is assigned to the bond. Dotted lines represent the upper and lower 95% confidence intervals.

Figure 3.2 Negative credit watch

APPENDIX A

INCENTIVE EFFECTS OF EARNINGS MANAGEMENT

Suppose an acquirer has an observed market value of AV and it makes an acquisition with a transaction value of TV. Now suppose that prior to the acquisition the acquirer boosted its stock price by managing earnings by a factor f, from $\frac{AV}{1+f}$ to AV.

Notice, if there were no earnings management, the target shareholders would get a proportion of the combined firm equal to $\frac{TV}{TV+\frac{AV}{1+f}}$. However, with earnings management, they only receive a fraction of the combined firm equal to $\frac{TV}{TV+AV}$. Given that the true value of combined firm equals $\left(TV+\frac{AV}{1+f}\right)$, the dollar expropriation from target to acquirer shareholders as a result of earnings management is given by

$$\text{True Value of Combined Firm} \times (\text{Payout to Target Shareholders without EM} - \text{Payout to Target Shareholders with EM})$$

$$= \left(TV+\frac{AV}{1+f}\right) \times \left(\frac{TV}{TV+\frac{AV}{1+f}} - \frac{TV}{TV+AV}\right) = \frac{f \times TV}{(1+f)\left(\frac{TV}{AV}+1\right)}$$

The percent expropriation can be obtained by dividing the dollar expropriation by the true value of acquirer equity, which is $\frac{AV}{1+f}$, as follows:

$$\frac{\frac{f \times TV}{(1+f)\left(\frac{TV}{AV}+1\right)}}{\frac{AV}{1+f}} = \frac{f \times \frac{TV}{AV}}{\left(\frac{TV}{AV}+1\right)} = \frac{f \times Relative\ Size}{(1+Relative\ Size)}$$

Where *Relative Size* is the ratio of transaction value to observed and possibly inflated acquirer value. For very small *Relative Size*, the expression can be simplified to $f \times Relative\ Size$.

The above expression can also be inverted to give a minimum value of *Relative Size* that would produce a certain percentage benefit *p* for a given price boost *f*. This minimum value equals $\frac{p}{f-p}$. Thus, assuming $f = 0.15$, the *Relative Size* would have to exceed $\frac{1}{14} = 0.071$ to create a 1% benefit, or exceed $\frac{1}{2} = 0.50$ to create a 5% benefit for the acquirer shareholders.

APPENDIX B

TARGET LEVERAGE

Table B1. Estimating target leverage

Panel A: Summary statistics of variables analyzed in this table				
Variable	Mean	Q1	Median	Q3
Book leverage (percent)	23.8	8.4	22.0	35.2
Market leverage (percent)	25.9	5.6	20.5	41.2
Assets (or TA) - $ millions	1,157	30	101	418
EBIT / TA	0.083	0.044	0.100	0.155
Market to book	1.479	0.737	1.051	1.679
Depreciation / TA	0.044	0.025	0.038	0.055
Fixed assets / TA	0.305	0.148	0.264	0.418
R&D dummy	0.432	0.000	0.000	1.000
R&D expense / TA	0.028	0.000	0.000	0.032
Selling expense / Sales	0.261	0.125	0.207	0.318

Panel B: Cross-sectional models of target leverage		
Variable	Book Leverage	Market Leverage
Log total assets	0.002	0.008
	(5.40)	(16.99)
EBIT / TA	-0.304	-0.443
	(-57.44)	(-74.86)
Market to book	-0.010	-0.044
	(-18.51)	(-69.96)
Depreciation / TA	-0.446	-0.734
	(-16.45)	(-24.22)
Fixed assets / TA	0.214	0.190
	(48.39)	(38.63)
R&D dummy	0.012	0.015
	(7.77)	(9.01)
R&D expense / TA	-0.445	-0.458
	(-25.88)	(-23.69)
Selling expenses / Sales	-0.066	-0.125
	(-16.78)	(-28.44)
Rated dummy	0.106	0.077
	(49.95)	(32.63)
Fama-French industry fixed effects	Yes	Yes
Year fixed effects	Yes	Yes

Note: We estimate the following model of target leverage: $Lev_{i,t+1} = X_{i,t}\,\beta + \epsilon_{i,t}$, where $Lev_{i,t+1}$ denotes the book leverage of firm i during period t+1 (next year), $X_{i,t}$ is a vector of firm characteristics during period t (current year), β is the vector of coefficients, and $\epsilon_{i,t}$ is the error term. In Panel A we report the firm characteristics, which are defined

Table B1. Continued

in Appendix 2. In Panel B we estimate the Tobit regression model that constrains the dependent variable to lie between 0 and 1. The firm characteristics include Fama-French industry fixed effects as well as year fixed effects (thus no constant term). The sample includes 100,945 firm-years during 1967 to 2007 with available data on CRSP and Compustat.

Table B2. Variable definitions

Variable	Definition
Bond spread	Corporate bond yield minus corresponding maturity interpolated constant maturity treasury yield
Book leverage	(Total long term debt + Total debt in current liabilities) / Total assets (DLTT+DLC)/AT
Capital intensity	Gross property, plant, and equipment / Total assets (PPEGTQ/ATQ)
Coupon	Annual coupon expressed as a percent of par value
Credit rating	The most recent S&P credit rating prior to the earnings announcement date, lower numbers represent lower default risk (AAA=2, AA+=4, AA=5, …, CC=23, etc.)
Default spread	Difference between yield of BAA rated corporate bonds and AAA rated corporate bonds as of the earnings announcement date
Depreciation / Total assets	Depreciation and amortization / Total assets (DP/AT)
Distance to default	Distance to default is computed using the KMV-Merton model following Bharath and Shumway (2004)
EBIT / Total assets	(Interest expense + Income taxes + Income before extraordinary items)/ Total assets (XINT+TXT+IB)/AT
Financing deficit	Annual financing deficit is computed following Frank and Goyal (2003)
Fixed assets / Total assets	Total Property, Plant, and Equipment / Total assets (PPENT/AT)
Historical volatility	Volatility of daily returns is computed using 254 day period ending one day prior to the earnings announcement date, expressed in percent, and annualized
Interest coverage ratio	Operating income before depreciation / Interest expense (OIBDPQ/XINTQ)
Leverage trend during last eight quarters	Plus (minus) 1 if leverage increased (decreased) from the prior quarter. Count over 8 quarters.
Loss dummy	Equals 1 if net income before extraordinary items (IBQ) is less than 0 for the last 5 consecutive quarters, 0 otherwise.
Market leverage	(Total long term debt + Total debt in current liabilities) / (Total long term debt + Total debt in current liabilities + Market value of equity) (DLTT+DLC)/(DLTT+DLC+PRCC_F*CSHO)
Market return	One year value weighted market return (CRSP VWRETD) computed until one day before the earnings announcement date
Market to Book	(Total long term debt + Total debt in current liabilities + Preferred Stock + Market value of equity)/ Total assets (DLTT+DLC+PSTKL+PRCC_F*CSHO)/AT
Market volatility	CBOE VIX level as of the earnings announcement date

Table B2. Continued

Prior year excess return	Cumulative stock return over a 254 day period ending one day prior to the earnings announcement date minus the cumulative market return, expressed in percent
Rated dummy	Equals 1 if firm is rated by S&P, 0 otherwise
R&D dummy	Equals 1 if R&D expense is missing, 0 otherwise
R&D expense / Total assets	Research and development expense / Total assets (XRD/AT)

APPENDIX C

CREDIT RATINGS

Table C1. Numerical conversion table for credit ratings

Moody's credit rating	Numerical code
Investment Grade	
Aaa	20
Aa1	19
Aa2	18
Aa3	17
A1	16
A2	15
A3	14
Baa1	13
Baa2	12
Baa3	11
Speculative Grade	
Ba1	10
Ba2	9
Ba3	8
B1	7
B2	6
B3	5
Caa1	4
Caa2	3
Caa3	2
Ca-C	1

Note: This table translates Moody's corporate credit ratings into numerical codes for use in empirical analysis. Smaller numbers represent bonds with higher credit risk. Corporate bond ratings are sourced from the Fixed Income Securities Database.

Table C2. Default rates by letter rating

Credit rating	Numerical code range	Average one year cumulative default rate (%)	Average five years cumulative default rate (%)	Average ten years cumulative default rate (%)
Investment Grade				
Aaa	20	0.00	0.11	0.56
Aa	17-19	0.01	0.19	0.58
A	14-16	0.02	0.51	1.42
Baa	11-13	0.18	2.06	4.89
Speculative Grade				
Ba	8-10	1.23	10.57	19.86
B	5-7	5.65	29.06	46.12
Caa-C	1-4	21.12	56.52	74.72

Note: This table lists average withdrawal-adjusted cumulative default rates for US corporate bonds for one-year, five-year, and ten-year horizons calculated over 1970 to 2005 by whole letter rating (Hamilton and Cantor, 2006).

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