BEATING EARNINGS BENCHMARKS AND THE COST OF DEBT

by

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(Under the Direction of Benjamin C. Ayers)

ABSTRACT

Prior research documents that firms tend to beat three earnings benchmarks: zero earnings, last year's earnings, and analyst's forecasted earnings, and that there are both equity market and compensation-related benefits associated with beating these benchmarks. This study investigates whether and under what conditions the bond market rewards firms for beating these three earnings benchmarks. I use two proxies for a firm's cost of debt: credit ratings and initial bond yield spread. Results suggest that firms beating earnings benchmarks have better one-year ahead credit ratings and a smaller initial bond yield spread. Additional analyses indicate that (i) the benefits of beating earnings benchmarks are much more pronounced for firms with high default risk (i.e., firms for which earnings are more informative about bondholders' future payoffs) and (ii) beating the zero earnings benchmark (i.e., the "profit" benchmark) provides the biggest reward in terms of a lower cost of debt. In sum, results suggest that bond investors, similar to equity investors, reward firms for beating earnings benchmarks, but the relative importance of specific benchmarks differs across the equity and bond markets.

INDEX WORDS: Earnings benchmarks; Cost of debt; Credit ratings; Yield spread

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DEDICATION

To my wife Yanyan: *for endless love and support*

AND

To my advisor Ben: *for continuous guidance and encouragement*

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CHAPTER 1

INTRODUCTION

Recent research documents that a disproportionate number of firms *barely* report profits, earnings increases over last year's earnings, and positive earnings surprises relative to analysts' expectations (Burgstahler and Dichev 1997; Degeorge et al. 1999; Hayn 1995; Burgstahler and Eames 2002). Burgstahler and Dichev (1997) and Degeorge et al. (1999) postulate that firms try to beat earnings benchmarks because firms' stakeholders use earnings benchmarks to evaluate firms' performance. They suggest that at least *some* firm stakeholders rely on heuristics, such as earnings benchmarks, to reduce information processing costs when assessing firms' performance. In addition, prospect theory predicts that decision-makers are less sensitive to *gains* but more sensitive to *losses* relative to a reference point (Kahneman and Tversky 1979). Firms, therefore, have incentives to beat earnings benchmarks to avoid the disproportionate adverse reactions to missing the benchmarks. Indeed, former SEC chairman Arthur Levitt in a 1998 speech lamented how "unforgiving" the stock market was to firms that missed analysts' earnings forecasts (Levitt 1998).

Prior research has focused on how beating earnings benchmarks affects the equity market and CEO compensation. In particular, previous studies find that firms that beat earnings benchmarks have both higher equity valuations and CEO compensation (Barth et al. 1999; Bartov et al. 2002; Brown and Caylor 2005; Kasznik and McNichols 2002; Lopez and Rees 2002; Matsunaga and Park 2001; Skinner and Sloan 2002; Myers and Skinner 2002). In contrast to the substantial evidence regarding the equity market effects of beating earnings benchmarks, there is little evidence regarding whether and how other financial statement users use earnings benchmarks in evaluating firm performance. Holthausen and Watts (2001, 26) point out that "it is not apparent that the relevance of a given number would be the same for equity investors and lenders," and call for more research into how accounting information is utilized by financial statement users other than equity investors.[1] This study answers this call by investigating whether and under what conditions bond investors and credit rating agencies reward firms for beating earnings benchmarks.

Investigating the effects of earnings benchmarks within the context of the bond market is important for at least three reasons. First, firms are increasingly relying on debt financing. For example, in 2000, corporate bond issuances exceeded $1.2 trillion, whereas firms issued less than $200 billion in new equity (Beller 2003). Accordingly, small changes in bond prices represent large changes in capital allocation, so factors that influence bond prices are of great economic importance. Second, the effect of beating earnings benchmarks is likely to be different in the bond market than in the equity market because of the rich information environment specific to the bond market. Approximately ninety-five percent of bondholders consist of institutional investors who are arguably more sophisticated and have access to more firm-specific information.[2] Similarly, credit rating agencies have access to firms' nonpublic information such as minutes of board meetings, profit breakdown by product, budgets and forecasts, and internal capital allocation data (Ederington and Yawitz 1987; SEC 2003). Given their access to potentially more informative data regarding a firm's prospects, bond investors and rating agencies may place less reliance on earnings benchmarks in evaluating firm performance.

[1] Similarly, Pettit et al. (2004, 1) argue that "credit ratings and rating agencies are mentioned only in passing in most business schools and remain one of the most *understudied* aspects of modern corporate finance." This study enhances our studying of how ratings agencies use accounting information.
[2] Prior research uses high institutional ownership to proxy for investor sophistication and richer firm information environments (e.g., Ayers and Freeman 2003; Bartov et al. 2000; Battalio and Mendenhall 2003; Collins et al. 2003; Hand 1990; Jiambalvo et al. 2002; and Walther 1997).

Third, even if bondholders and rating agencies use earnings benchmarks in evaluating firm performance, the relative importance of the three earnings benchmarks likely differs between the bond and equity markets. In the equity market, Brown and Caylor (2005) find that either beating analysts' earnings forecasts or reporting earnings increases leads to the highest three day abnormal returns around the quarterly earnings announcement date after controlling for the level of unexpected earnings surprise.[3] In contrast to equity investors, bondholders have a fixed claim against the firm's value. They bear the firm's downside risk but do not share in the firm's upside growth potential (Fischer and Verrecchia 1997; Plummer and Tse 1999). The most salient benchmark in assuring bondholders that the firm will survive and satisfy its financial obligations is reporting positive income. In contrast, beating last year's earnings indicates earnings growth, whereas beating analysts' forecasts suggests superior performance relative to analysts' expectations. *Ceteris paribus*, these latter two benchmarks may be less informative to bond investors because they do not share in a firm's upside potential.

To address whether beating earnings benchmarks influences a firm's cost of debt, I construct annual earnings benchmarks based on earnings per share, changes in earnings per share, and the single most recent analyst's forecasted earnings per share. I use firm credit ratings (Ahmed et al. 2002; Minton and Schrand 1999; Francis et al. 2005) and initial bond yield spread (Sengupta 1998; Shi 2003) to proxy for a firm's cost of debt. I examine the relation between credit ratings and yield spread and three separate dichotomous variables that denote whether firms beat the three annual earnings benchmarks. Both ratings and spread models control for the magnitude of earnings, earnings changes and earnings surprises and other factors that prior research suggests influence firm credit ratings and yield spread. Absent controls for the

[3] Brown and Caylor (2005) find that in recent years beating analysts' earnings forecast received the largest market reward. They conjecture that their finding is likely attributable to increased media attention to analysts' earnings forecast.

magnitude of earnings, earnings changes and earnings surprises, the significant association between beating earnings benchmarks and cost of debt could be simply attributed to an association between superior earnings performance and the cost of debt. Including these controls allows me to test whether bond investors and credit rating agencies reward firms specifically for beating earnings benchmarks.

Consistent with bond investors using earnings benchmarks to evaluate firm performance, I find that beating earnings benchmarks is associated with a lower cost of debt (i.e., better credit ratings and smaller yield spread). I also find that the effects of beating earnings benchmarks are much more pronounced for firms with high default risk. This finding suggests that the importance of earnings benchmarks increases for firms whose earnings are more informative about bondholders' future payoffs. Finally, I find that the reduction in a firm's cost of debt associated with reporting profits (i.e., beating the "profit" benchmark) equals or exceeds the effects associated with reporting an earnings increase and beating analysts' forecasts.

This study makes three contributions to the literature. First, this study enhances our understanding of firms' incentives to beat earnings benchmarks. In particular, it provides new insight into firms' potential motivation for beating earnings benchmarks by identifying an additional economically significant benefit of beating benchmarks. Second, my findings confirm Burgstahler and Dichev (1997) and Degeorge et al. (1999)'s conjecture that creditors use earnings benchmarks to make decisions. Even though bondholders are primarily institutional investors, and rating agencies have access to private firm information, my results suggest that both groups rely on earnings benchmarks to evaluate firm performance. The results are consistent with Bhojraj and Swaminathan (2004), who find that bond investors are *no more* sophisticated than equity investors in pricing accruals.[4] Third, I quantify the benefits of beating earnings benchmarks in the bond market and provide evidence that the relative importance of earnings benchmarks differs across equity and bond markets. Among other implications, this study provides evidence that the importance of a specific earnings benchmark may differ based upon the firm's financing alternatives (equity versus debt).

This study proceeds as follows. Chapter 2 reviews prior studies that investigate earnings benchmarks and develops testable hypotheses. Chapter 3 describes the research design. Chapter 4 reports the sample selection and descriptive statistics. Chapters 5 and 6 present the main results and sensitivity analyses, respectively. Chapter 7 concludes.

[4] The results are also consistent with Burgstahler (1997) who finds that the likelihood of equity and bond ratings upgrade increases in the vicinity of zero earnings changes and zero levels of earnings. Burgstahler (1997), however, does not perform statistical tests or control for other factors such as firm size that explain debt ratings.

CHAPTER 2

LITERATURE REVIEW AND HYPOTHESIS DEVELOPMENT

2.1. Prior Studies on Beating Earnings Benchmarks

Recent research documents that a disproportionate number of firms *barely* report profits, earnings increases over past period earnings, and positive earnings surprises relative to analysts' expectations. The discontinuity around these three earnings numbers in the earnings distribution suggests that these earnings numbers represent "benchmarks" that firms try to beat. The discontinuity has also been viewed as *de facto* evidence of earnings management.

Hayn (1995) first documents that there is a discontinuity in the annual earnings distribution around zero earnings: too few firms report small losses and too many firms report small profits. She conjectures that "firms whose earnings are expected to fall just below the zero earnings point engage in earnings manipulations to help them cross the 'red line' for the year" (1995, 132). Likewise, Burgstahler and Dichev (1997) find that the frequency of small earnings increases (decreases) over last year's earnings is much higher (lower) than expected. In the quarterly earnings distributions, Degeorge et al. (1999) find that a disproportionate number of firms report a small profit, a small earnings increase over the same quarter of last year, and a zero or small positive earnings surprise over analysts' consensus expectations.

If there are no special incentives related to beating earnings benchmarks and managers do not take additional actions to beat these benchmarks, then the earnings, earnings change, and earnings surprise distributions should be relatively smooth around the benchmarks. Based upon these assumptions, Hayn (1995), Burgstahler and Dichev (1997) and Degeorge et al. (1999) conclude that the "kink" is due to managers' earnings management. These papers are viewed as providing "compelling evidence that earnings are managed to achieve earnings targets" (McNichols 2000, 314) due to the vivid "kink" graph and "strongly significant results" (Dechow and Skinner, 2000, 243). However, as McNichols (2000) points out, the early earnings benchmark papers did not investigate 1) the incentives for managers to beat specific benchmarks; and 2) the actions that managers take to beat earnings benchmarks. These questions are the focus of recent literature.

Recent papers in the earnings benchmark literature may be classified into three groups. The first group of papers investigates whether the discontinuity in the distribution of earnings, earnings changes, and earnings surprises around the respective benchmarks can be explained by a data generating process other than managers' discretion. Beaver, McNichols and Nelson (2003) contend that the distribution of earnings levels and earnings changes will exhibit a discontinuity even without earnings management. They argue that in the earnings level distribution, the greater frequency of transitory items in loss firms results in a lower density of observations just below zero earnings and the greater effective tax rate for profit firms results in a higher density of observations just above zero. They conclude that two-thirds of the discontinuity around the zero earnings benchmark can be attributed to the effects of transitory items and income tax. Durtschi and Easton (2004) argue that both the deflator (i.e., market price) and the sample selection criteria contribute to the discontinuity in the earnings and earnings changes distributions. They also contend that systematic bias in analysts' forecasts rather than firms' earnings management causes the discontinuity in the distribution of earnings forecast errors. In sum, to date there is no consensus as to the factors that explain the kink in the distributions of earnings, earnings changes, and earnings surprises.

A second group of papers investigates how firms beat the three earnings benchmarks. Burgstahler and Dichev (1997) find that cash flows from operations are much higher for small profit firms than for small loss firms. Roychowdhury (2004) finds that firms with small profits and small earnings increases, on average, have lower unexpected cash flows from operations and higher production cost than other firms, which is consistent with firms offering price discounts and reducing reported cost of goods sold to beat benchmarks. Both studies suggest that firms may take real actions to beat the profit and earnings increase benchmarks. This conclusion is consistent with survey results in Graham et al. (2005), which indicate that managers are willing to sacrifice economic value to beat earnings benchmarks.

With respect to accruals management, Dechow et al. (2004) use univariate analyses to compare discretionary accruals for small profit firms relative to other firms. They find that small profit firms, on average, have higher discretionary accruals than other firms but not relative to small loss firms. Dechow et al. (2004) conclude that small profit firms engage in accruals management to beat the profit benchmark, but that accrual management measured by Jones-type models does not explain the discontinuity in the earnings distribution. Ayers et al. (2005) and Phillips et al. (2003) use probit analyses to investigate the association between a firm's discretionary accruals and its propensity to beat earnings benchmarks. Both studies include a firm's cash flow from operations in probit regressions to control for the firm's need to use discretionary accruals to beat earnings benchmarks. Results in both studies suggest that various accruals measures (total accruals, modified Jones model accruals, forward-looking accruals, and deferred tax expense) are positively associated with a firm's propensity to beat the profit and earnings increase benchmarks. In addition, Ayers et al. (2005), using a more timely measure of analysts' forecasted earnings (i.e., the single most recent analyst forecast) find a positive association between abnormal accruals and a firm's propensity to beat the analysts' forecast benchmark.

With respect to specific accruals, Dhaliwal et al. (2005) find that firms reduce their fourth quarter effective tax rates to beat analysts' earnings forecast. Beatty et al. (2002) find that public banks are more likely than private banks to use the *loan loss provision* and *security gain realizations* to avoid small earnings decreases. Similarly, Beaver et al. (2003) find that property-casualty insurers understate *loss reserves* to report a small profit.

Recent research also suggests that managers may use earnings guidance to beat the analysts' forecast benchmark. In particular, based upon the assumption that analysts' forecasts are endogenous and subject to manager manipulation, Matsumoto (2002) investigates whether firms guide analysts' forecasts downward to avoid negative earnings surprises. She finds that firms with positive earnings surprises have more positive abnormal accruals and more downward earnings guidance.

The third group of earnings benchmark research, including this study, analyzes firms' incentives to beat earnings benchmarks. Burgstahler and Dichev (1997) and Degeorge et al. (1999) suggest that firms' stakeholders, such as the board of directors, investors, and creditors use earnings benchmarks to evaluate firms' performance. Consistent with this conjecture, Matsunaga and Park (2001) find that CEOs' cash bonuses are significantly lower when firms miss analysts' forecasts or experience earnings decreases, even after controlling for firm performance. Their findings suggest that boards of directors rely on earnings benchmarks to evaluate CEO performance.

Several studies investigate the equity market effects of beating earnings benchmarks. Barth et al. (1999) find that firms that consistently exceed previous years' earnings have a higher price-earnings multiple, which disappears if the pattern is broken. Bartov et al. (2002), Kasznik and McNichols (2002), and Lopez and Rees (2002) find that firms meeting or beating analysts' forecasted earnings have higher abnormal returns and higher earnings response coefficients than firms missing analysts' forecasted earnings. Brown and Caylor (2005) find positive abnormal returns around quarterly earnings announcements for firms reporting a profit, earnings increase, or beating analysts' forecasted earnings even after controlling for the level of unexpected earnings surprise. Finally, Skinner and Sloan (2002, 300) find that growth firms that miss analysts' forecasted earnings by a small amount experience "disproportionately large stock price declines."

In contrast to the evidence on the equity market effects and incentives associated with beating earnings benchmarks, there is little evidence regarding whether and how other financial statement users use earnings benchmarks in evaluating firm performance. Holthausen and Watts (2001, 26) point out that "it is not apparent that the relevance of a given number would be the same for equity investors and lenders," and call for more research into how accounting information is utilized by financial statement users other than equity investors. This study answers this call by investigating whether and under what conditions bond investors and credit rating agencies reward firms for beating earnings benchmarks.

2.2 Hypothesis Development

This study investigates whether bondholders and credit rating agencies use earnings benchmarks to assess firm performance. Prior research finds that earnings information in general is useful to both bondholders and rating agencies.[5] Given the substantial differences between the

[5] For example, Ziebart and Reiter (1992) find that high return on assets (ROA) is associated with low bond yields and high bond ratings. Khurana and Raman (2003) find that fundamental scores that they construct to predict future earnings can explain cross-sectional differences in the initial bond yields. Datta and Dhillon (1993) find that

bond and equity markets, it is not obvious that beating earnings benchmarks will have valuation effects in the bond market. For example, ninety-five percent of bondholders are institutional investors who are arguably more sophisticated and/or have informational advantages. Thus, they may be less likely to rely on earnings benchmarks in evaluating firm performance. Likewise, rating agencies have access to firms' nonpublic information such as minutes of board meetings, profit breakdown by product, budgets and forecasts, and internal capital allocation data (Ederington and Yawitz 1987; SEC 2003). With access to potentially more informative data regarding a firm's prospects, rating agencies may not rely on earnings benchmarks to assign ratings, a key determinant of bond yield. Accordingly, bond ratings and new bond yield may not be sensitive to beating earnings benchmarks.

Other evidence, however, questions the sophistication of institutional investors. For example, Ali et al. (2000) find that high institutional ownership *exacerbates* rather than mitigates the mispricing of accruals. Bhojraj and Swaminathan (2004) find that the bond market misprices accruals similarly to the equity market. Other evidence questions the information advantage of credit rating agencies relative to equity investors. For example, Holthausen and Leftwich (1986) find that rating changes tend to occur *after* abnormal stock returns. Thus, it is an empirical question whether bondholders and ratings agencies use earnings benchmarks in pricing bonds and assigning ratings.

Because bondholders' claim on a firm's income is fixed, earnings are less informative for bondholders of firms with low default risk. Consistent with this proposition, prior research finds cross-sectional differences in the usefulness of earnings to bondholders as a function of a firm's default risk. For example, Plummer and Tse (1999) find that earnings changes become less

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quarterly earnings surprises are positively associated with daily bond returns. Similarly, Plummer and Tse (1999) find that *annual* earnings changes are positively associated with annual bond returns.

closely associated with bond returns as bond ratings improve. To the extent that beating earnings benchmarks lowers the cost of debt, I expect that such effects will increase with firms' default risk. Specifically, I hypothesize that beating earnings benchmarks is associated with a lower cost of debt, and the association is more pronounced for high default risk firms than for low default risk firms. In alternative form, my first two hypotheses are:

H1: *Ceteris paribus,* beating earnings benchmarks lowers a firm's cost of debt.

H2: Beating earnings benchmarks has more pronounced effects on the cost of debt for firms with high default risk than for firms with low default risk.

The relative importance of each earnings benchmark is subject to debate. Degeorge et al. (1999) conclude that there is a hierarchy among the three earnings benchmarks: the profit benchmark is the most important; the earnings increase benchmark is second in importance; and beating analysts' forecasts is the least important. In contrast, Graham, Harvey and Rajgopal (2004) survey 312 financial executives from publicly traded firms and report that the majority of their respondents indicate that reporting a quarterly earnings increase is most important, followed by beating analysts' forecasts and reporting profits. Brown and Caylor (2005) find that from 1985 to 2002, either reporting quarterly earnings increases or beating analysts' forecasts receives the largest equity market reward (i.e., three day abnormal returns around earnings announcements) and reporting profits receives the least reward after controlling for the level of unexpected earnings surprise.

I anticipate that the relative benefits of beating each earnings benchmark differ between the bond market and the equity market. Bondholders have a fixed claim against a firm's value. They bear the firm's downside risk but do not share in the firm's upside growth potential

(Fischer and Verrecchia 1997; Plummer and Tse 1999). Reporting profits is likely the most salient benchmark in assuring bondholders that the firm will survive and satisfy its financial obligations. In contrast, beating last year's earnings indicates earnings growth, whereas beating analysts' forecasts suggests superior performance relative to analysts' expectations. *Ceteris paribus*, these latter two benchmarks may be less informative to bond investors who do not share in the firm's upside potential. Accordingly, I test the following hypothesis (in alternative form):

> **H3**: Reporting a profit has a more pronounced effect on a firm's cost of debt than reporting an earnings increase or beating analysts' forecasted earnings.

CHAPTER 3

RESEARCH DESIGN

3.1 Model Specifications

I investigate whether firms beating earnings benchmarks have a lower cost of debt. I use firm credit ratings and initial bond yield spread to proxy for a firm's cost of debt. Prior research has used both credit ratings and initial bond yield spread to proxy for cost of debt (Ahmed et al. 2002; Minton and Schrand 1999; Francis et al. 2005; Sengputa 1998; Shi 2003). Credit ratings represent the rating agencies' assessment of a firm's credit worthiness and can affect a firm's access to bank loans, bonds and commercial paper markets. Ratings research suggests that downward rating changes affect both bond and stock prices (Dichev and Piotroski 2001; Hand, Holthausen, and Leftwich 1992; and Holthausen and Leftwich 1986) and can trigger accelerated payment of existing debt (Reason 2002). Initial bond yield spread (i.e., corporate bond yield at issuance minus the Treasury bond yield with comparable maturity) represents the risk premium that firms must pay to borrow money in the bond market and is a direct measure of a firm's incremental cost of debt (Sengupta 1998; Shi 2003).

I construct the earnings benchmark dichotomous variables using earnings per share, changes in earnings per share, and the single most recent analyst's forecasted earnings per share. Consistent with prior research, I use earnings per share instead of undeflated earnings to control for the possibility that firms may beat earnings benchmarks by manipulating shares outstanding (Bens et al. 2003; Myers and Skinner 2002). I use the single most recent analyst forecast from the I/B/E/S detail history file as the analyst forecast benchmark instead of the consensus forecast from the I/B/E/S summary file to avoid misclassifications due to rounding problems (Baber and Kang 2002; Payne and Thomas 2003) and to mitigate the effects of stale analysts' forecasts (Brown 1991; Brown and Kim 1991; Ayers et al. 2005). I measure $Spread_{t+1}$ and $Rating_{t+1}$ after year *t*'s earnings announcement. Specifically, I calculate $Spread_{t+1}$ using new bonds issued within 360 days following year *t*'s earnings announcement and measure $Rating_{t+1}$ at the end of year *t+1*.

I use the following regression models to evaluate the associations between beating the three earnings benchmarks and (1) firm credit ratings and (2) bond yield spread:

$$Rating_{it+1} = \alpha_0 + \alpha_1 Benchmark_{it} + \beta_1 EPS_{it} + \beta_2 \Delta EPS_{it} + \beta_3 UE\_EPS_{it} + \beta_4 Rating_{it} + \beta_5 Salegro_{it} + \beta_6 Beta_{it} + \beta_7 StdRet_{it} + \beta_8 BM_{it} + \beta_9 Size_{it} + \beta_{10} Lev_{it} + \textstyle\sum_t \beta_t Year_{it} + Error_{it} \quad (1)$$

$$Spread_{it+1} = \alpha_0 + \alpha_1 Benchmark_{it} + \beta_1 EPS_{it} + \beta_2 \Delta EPS_{it} + \beta_3 UE\_EPS_{it} + \beta_4 Rating_{it} + \beta_5 Salegro_{it} + \beta_6 Beta_{it} + \beta_7 StdRet_{it} + \beta_8 BM_{it} + \beta_9 Size_{it} + \beta_{10} Lev_{it} + \beta_{11} Senior_{it} + \beta_{12} IssueSize_{it} + \beta_{13} Call_{it} + \textstyle\sum_t \beta_t TimePrd_{it} + Error_{it} \quad (2)$$

where:

$Rating_{it+1}$ = firm *i*'s Standard & Poor's senior debt rating in year *t+1*. Standard & Poor's rates a firm's debt from AAA (indicating a strong capacity to pay interest and repay principal) to D (indicating actual default). I translate ratings letters into ratings numbers, with a smaller number indicating a better rating. Table 3.1 provides a complete conversion table.

$Spread_{it+1}$ = the yield to maturity at the issuance date for the largest bond firm *i* issued in year *t+1*, minus the Treasury bond yield with similar maturity. I measure $Spread_{it+1}$ as a percentage.

$Benchmark_{it}$ takes one of the three following three specifications:[6]

[6] Defining the benchmarks excluding "meeting" observations (i.e., those in which performance equals the benchmark) yields similar inferences.

$Profit_{it}$ = one if firm *i*'s basic earnings per share before extraordinary items is greater than or equal to zero in year *t*, and zero otherwise.

$Incr_{it}$ = one if firm *i*'s earnings per share before extraordinary items in year *t* is greater than or equal to that of year *t-1*, and zero otherwise.

$Surp_{it}$ = one if firm *i*'s earnings per share equals or exceeds the single most recent analyst forecast in year *t*, and zero otherwise.

The control variables include:

EPS_{it} = firm *i*'s earnings per share before extraordinary items in year *t* divided by its stock price at the end of year *t-1*.

ΔEPS_{it} = firm *i*'s earnings per share before extraordinary items in year *t* minus its earnings per share in year *t-1,* divided by its stock price at the end of year *t-1*.

UE_EPS_{it} = firm *i*'s actual earnings per share minus the single most recent analyst forecast for year *t*, divided by its stock price at the end of year *t-1*. I collect both the actual earnings per share and the most recent analyst earnings forecasts from the I/B/E/S detail history file.

$Rating_{it}$ = in model (1), $Rating_{it}$ is firm *i*'s Standard & Poor's senior credit rating at the end of year *t*. In model (2), $Rating_{it}$ is bond *i*'s Standard & Poor's rating at the issue date. I translate ratings letters into ratings numbers, with a smaller number indicating a better rating. Table 3.1 provides a complete conversion table.

$Salegro_{it}$ = the annual percentage change in firm *i*'s sales from year *t-1* to year *t*.

$Beta_{it}$ = the beta coefficient calculated from the value-weighted CAPM for firm *i* using monthly returns during the 5 years preceding year *t*.

$StdRet_{it}$ = the standard deviation of firm *i*'s daily stock returns during year *t*.

BM_{it} = the log of firm *i*'s book value of equity divided by its market value of equity, both measured at the end of year *t*.

$Size_{it}$ = the natural logarithm of firm *i*'s total assets at the end of year *t*.

Lev_{it} = firm *i*'s long-term debt divided by total assets at the end of year *t*.

$Senior_{it}$ = one for senior bonds and zero for subordinated bonds.

$IssueSize_{it}$ = the natural logarithm of the offering amount of the bond (in millions of dollars).

$Call_{it}$ = the ratio of the number of years to first call divided by the number of years to maturity. $Call_{it}$ takes the value of one if there is no call provision and zero if it is callable from the date of issuance.

$Year_{it}$ = one if observation *i* is in year *t*; and zero otherwise. For the *Rating* model, *t* includes years from 1986 to 2001.

$TimePrd_{it}$ = one if observation i is in "time-period" t, and zero otherwise. Due to limited sample observations per year in the *Spread* model, I separate the initial bond issue sample chronologically into five groups of approximately equal size and include indicator variables for four "time-period" groups.

I use ordinary least squares to estimate both models. To test H1, I estimate each model including one benchmark specification ($Profit_{it}$, $Incr_{it}$, and $Surp_{it}$) at a time. The coefficient for each benchmark indicator represents the average effect of beating the earnings benchmark on the cost of debt, after controlling for firm performance and other determinants of credit ratings and bond yields. If beating earnings benchmarks is associated with a lower cost of debt, I expect the benchmark coefficients to be negative in the *Spread* and *Rating* models.

To test H2 (i.e., whether the impact of beating an earnings benchmark is more pronounced for firms with high default risk), I partition the sample into high and low default risk subsamples. For the credit ratings (new bond issue) sample, I classify firms (bonds) rated as BBB or below into the high default risk subsample and firms (bonds) rated as BBB+ or above into the low default risk subsample.[7] I estimate models (1) and (2) for the high and low default risk subsamples separately. If the effects of beating earnings benchmarks are more pronounced for high default risk firms than for low default risk firms, I expect the coefficients of benchmarks to be larger in magnitude (i.e., more negative) for high default risk firms than for low default risk firms.

To test H3, I estimate models (1) and (2) including all three benchmarks to assess the relative importance and the corresponding incentives associated with beating each benchmark. The coefficient for each benchmark represents the incremental effect of meeting or beating that

[7] Classifying firms (bonds) into high and low default risk subsamples based on Ohlson's O-score (1980) yields identical inferences. Using the investment grade/non-investment grade ratings (i.e., BBB-/BB+) to separate the high and low default risk subsamples yields similar inferences for credit ratings sample but weaker inferences for new bond issue sample due to limited bonds with non-investment grade ratings.

benchmark after controlling for the effects of the other two benchmarks. If reporting profits yields the largest reduction in firms' cost of debt, I expect the coefficient of $Profit_{it}$ to be significantly more negative than the coefficients for $Incr_{it}$ and $Surp_{it}$.

I include in each model the magnitude of earnings, earnings changes and earnings surprises to control for the "normal" effect of earnings or profitability on the cost of debt.[8] Furthermore, I include sales growth ($Salegro_{it}$) to control for performance not captured by the three earnings measures. I expect higher earnings and growth to be associated with better ratings and smaller yield spread. Given these controls, the coefficients on the earnings benchmarks reflect the *incremental* effect of meeting or beating the benchmarks *after* controlling for firm performance. Thus, the coefficients on the benchmarks cannot simply be attributed to the general association between firm performance and the cost of debt.

3.2 Additional Control Variables in the Credit Ratings Model

I use firms' one-year ahead credit ratings ($Rating_{it+1}$) as the dependent variable in the rating model. Ratings are relatively stable over time and are highly correlated across years. For example, 78 percent of sample firms have the same ratings as the previous year. I include the firms' current year credit ratings ($Rating_{it}$) to control for the effects of positive autocorrelation among error terms as well as potentially correlated omitted variables that may influence credit ratings (i.e., "the historical factors that cause current *differences* in the dependent variable that are difficult to account for in other ways" (Wooldridge 2000, 289)).

Prior literature finds that more profitable, larger, and less risky firms have better credit ratings (Ahmed et al. 2002; Kaplan and Urwitz 1979; Sengputa 1998). I control for these factors

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[8] I include all three earnings variables to control for different aspects of firm performance. The correlation among the three earnings variables, however, reduces the precision of their coefficients estimates. When I only include one earnings variable at a time, inferences for the benchmark indicators remain the same except that $Incr_{it}$ in the *Spread* model becomes significant in both the aggregate sample and the high default risk subsample.

by including a firm's systematic equity risk ($Beta_{it}$), equity volatility ($StdRet_{it}$), book-to-market ratio (BM_{it}), size ($Size_{it}$), and leverage (Lev_{it}). Blume, Lim and Mackinlay (1998) suggest that there is a downward time trend in the ratings assignment process. I include year indicators ($Year_{it}$) to control for this trend.

3.3 Additional Control Variables in the Initial Bond Yield Spread Model:

Prior research documents that factors affecting initial bond yields include macroeconomic conditions, firm characteristics, and features of the bond (Bhojraj and Sengupta 2003; Fisher 1959; Khurana and Raman 2003; Sengupta 1998; Shi 2003; Ziebart and Reiter 1992). To control for macroeconomic conditions, I calculate $Spread_{it+1}$ as the corporate bond yield minus the Treasury bond yield with the closest maturity date to the corporate bond. Following Elton et al. (2001), Campbell and Taksler (2003) and Sengupta (1998), I control for firm characteristics by including a firm's systematic equity risk ($Beta_{it}$), equity volatility ($StdRet_{it}$), book-to-market ratio (BM_{it}), size ($Size_{it}$), and leverage (Lev_{it}). I expect firms with higher beta, higher equity volatility, higher book-to-market ratio, smaller size, and higher leverage to have a larger bond yield spread.

To control for bond features associated with the yield spread, I include the bond's credit rating ($Rating_{it}$), seniority status ($Senior_{it}$), issue size ($IssueSize_{it}$), and call provisions ($Call_{it}$). I transform the Standard & Poor's bond ratings into integers such that lower rating numbers indicate lower default risk. I expect a positive relation between bond ratings and yield spread. Holders of senior bonds have more protection than subordinated bondholders if bond issuers default. Thus, I expect senior bonds to have a smaller yield spread. $IssueSize_{it}$ reflects a bond's liquidity (i.e., larger size, higher liquidity), which is generally negatively associated with yield spread (Sengupta 1998). On the other hand, $IssueSize_{it}$ also reflects a firm's overall debt burden, which is generally positively associated with bond yield spread (Shi 2003). Thus, I have no prediction regarding $IssueSize_{it}$. A bond's call provision exposes bondholders to interest risk (i.e., a lower call ratio implies higher risk exposure for bondholders). Accordingly, I expect call ratio to be negatively associated with yield spread. Finally, due to the few bond issues per year in the sample, I separate the sample chronologically into five groups of approximately equal size. I include in model (2) indicator variables for four "time-period" groups to control for any time-related effects on yield spread.

Table 3.1
The Transformation of Ratings Letters and Sample Observations across Ratings

S&P Credit Rating Letter	$Rating_{it}$ Variable	Credit Ratings Sample[a]		New Bond Issues Sample[b]	
		Obs.	Percentage	Obs.	Percentage
AAA	1	237	2.27%	39	2.21%
AA+	2	95	0.91%	13	0.74%
AA	3	406	3.89%	114	6.47%
AA-	4	413	3.96%	93	5.28%
A+	5	679	6.51%	194	11.00%
A	6	1,060	10.16%	284	16.11%
A-	7	799	7.66%	178	10.10%
BBB+	8	910	8.72%	214	12.14%
BBB	9	1,141	10.94%	218	12.37%
BBB-	10	849	8.14%	138	7.83%
BB+	11	642	6.16%	42	2.38%
BB	12	797	7.64%	33	1.87%
BB-	13	935	8.96%	49	2.78%
B+	14	953	9.14%	63	3.57%
B	15	307	2.94%	45	2.55%
B-	16	133	1.28%	36	2.04%
CCC+[c]	17	42	0.40%	10	0.57%
CCC or CC	18	18	0.17%		
CCC- or C	19	7	0.07%		
D or SD	20	7	0.07%		

[a] The credit ratings sample consists of 10,430 firm year observations from 1985 to 2002.
[b] The new bond issue sample consists of 1,763 nonconvertible and fixed rate corporate bonds issued from 1983 to 2002.
[c] In the new bond issue sample, I combine all ratings below CCC+ with CCC+ into one category because of the limited number of observations under CCC+. For the credit ratings sample, combining observations rated below CCC+ into one category yields similar inferences.

Table 3.2
Variable Definitions

$Rating_{it+1}$	= firm *i*'s Standard & Poor's senior debt rating in year *t+1*. Standard & Poor's rates a firm's debt from AAA (indicating a strong capacity to pay interest and repay principal) to D (indicating actual default). I translate ratings letters into ratings numbers, with a smaller number indicating a better rating. Table 3.1 provides a complete conversion table.
$Spread_{it+1}$	= the yield to maturity at issuance date for the largest bond issued in year *t+1* for firm *i* minus the Treasury bond yield with similar maturity. I measure $Spread_{it+1}$ as a percentage.
$Profit_{it}$	= one if firm *i*'s basic earnings per share before extraordinary items is greater than or equal to zero in year *t*, and zero otherwise;
$Incr_{it}$	= one if firm *i*'s earnings per share before extraordinary items in year *t* is greater than or equal to that of year *t-1*, and zero otherwise;
$Surp_{it}$	= one if firm *i*'s earnings per share equals or exceeds the single most recent analyst forecast in year *t*, and zero otherwise;
EPS_{it}	= firm *i*'s earnings per share excluding extraordinary items in year *t* divided by its stock price at the end of year *t-1*.
ΔEPS_{it}	= firm *i*'s earnings per share before extraordinary items in year *t* minus its earnings per share in year *t-1* and divided by its stock price at the end of year *t-1*.
UE_EPS_{it}	= firm *i*'s actual earnings per share minus the single most recent analyst forecast for year *t*, divided by its stock price at the end of year *t-1*. I collect both the actual earnings per share and the most recent analyst earnings forecast from the I/B/E/S detail history file.
$Rating_{it}$	= in model (1), $Rating_{it}$ is firm *i*'s Standard & Poor's senior credit rating at the end of year *t*; in model (2), $Rating_{it}$ is bond *i*'s Standard & Poor's rating at the issue date. I translate ratings letters into ratings numbers, with a smaller number indicating a better rating. Table 3.1 provides a complete conversion table.
$Salegro_{it}$	= the annual percentage change in sales for firm *i* from year *t-1* to year *t*.
$Beta_{it}$	= the beta coefficient calculated from the value-weighted CAPM model for firm *i* using monthly returns during the 5 years preceding year *t*.
$StdRet_{it}$	= the standard deviation of daily stock returns during year *t* for firm *i*.
BM_{it}	= the log of firm *i*'s book value of equity divided by its market value of equity, both measured at the end of year *t*.
$Size_{it}$	= the natural logarithm of firm *i*'s total assets at the end of year *t*.
Lev_{it}	= firm *i*'s long-term debt divided by total assets at the end of year *t*.
$Senior_{it}$	= one for senior bonds and zero for subordinated bonds.

$IssueSize_{it}$	= the natural logarithm of the offering amount of the bond (in millions of dollars).
$Call_{it}$	= the ratio of the number of years to first call divided by the number of years to maturity. $Call_{it}$ takes the value of one if there is no call provision and zero if it is callable from the date of issuance.
$Year_{it}$	= one if observation *i* is in year *t*, and zero otherwise. For the *Rating* model, *t* includes years from 1986 to 2001.
$TimePrd_{it}$	= one if observation *i* is in "time-period" *t*, and zero otherwise. Due to limited sample observations per year in the *Spread* model, I separate the initial bond issue sample chronologically into five groups of approximately equal size and include indicator variables for four "time-period" groups.

CHAPTER 4

SAMPLE SELECTIONS AND DESCRIPTIVE STATISTICS

I use the I/B/E/S detail history file to construct the earnings surprise indicator ($Surp_{it}$) and unexpected earnings (UE_EPS_{it}). I calculate all other firm-specific variables using data from COMPUSTAT and CRSP. I exclude public utilities (two-digit SIC code 49) and financial service firms (two-digit SIC code 67) because these industries have different operating characteristics and different debt financing activities than industrial firms.[9] I exclude observations with prior year stock price less than three dollars to avoid extreme values for those variables divided by prior year stock price.

4.1 The Credit Ratings Sample

Standard & Poor's (S&P) usually assigns each company a long-term "issuer" rating intended to measure a company's ability to meet its senior obligations. S&P also assigns specific ratings for each debt issuance according to the debt contract. Senior debt ratings are usually the same as the issuer rating (S&P 2003). I collect firms' senior debt ratings from the annual COMPUSTAT file (data item 280) available since 1985. I exclude firms that change ratings dramatically in adjacent years (i.e., $|Rating_{it+1} - Rating_{it}| > 3$). Large rating changes could be coding errors or due to significant events such as a merger or acquisition. I exclude such observations because model (1) does not account for such factors. I classify firms that are rated as BBB+ or above in year *t* into the low default risk subsample and firms that are rated as

[9] For example, Smith (1986) points out that utility firms access the debt market more extensively than industrial firms.

BBB or below in year *t* into the high default risk subsample. The final sample includes 10,430 firm-year observations from 1985 to 2002: 5,831 observations in the high default risk subsample and 4,599 observations in the low default risk subsample. Table 3.1 reports the distribution of observations across the ratings categories.

Table 4.1 presents descriptive statistics for the aggregate sample, as well as subsamples partitioned by high and low default risk. In the aggregate sample the average S&P rating is BBB. Approximately 82 percent of firm-years report profits, whereas 60 percent report earnings increases and 62 percent beat the most recent analyst earnings forecast. Univariate comparisons indicate that low default risk firms beat the three earnings benchmarks more frequently than high default risk firms. Low default risk firms also have significantly higher earnings per share, lower beta, lower stock return volatility, lower book-to-market ratio, larger size, and lower leverage than high default risk firms. Nonetheless, high default risk firms have higher sales growth rates. Differences in earnings changes (ΔEPS_{it}) and earnings surprises (UE_EPS_{it}) between the two subsamples are insignificant.

Table 4.2 presents univariate correlations among all variables for the aggregate sample. I report Spearman correlations above the diagonal and Pearson correlations below the diagonal. To simplify the presentation, I only report correlations that are significant at 10 percent significance levels (two-tailed test). Consistent with H1, all three earnings benchmark indicators are negatively associated with $Rating_{it+1}$, indicating that beating any of the earnings benchmarks in year *t* is associated with better credit ratings in *t+1*. Consistent with H3, the Pearson correlation between $Profit_{it}$ and $Rating_{it+1}$ (ρ= -0.400) is stronger than the correlation between $Incr_{it}$ and $Rating_{it+1}$ (ρ= -0.149) and the correlation between $Surp_{it}$ and $Rating_{it+1}$ (ρ= -0.060).

All control variables are significantly correlated with $Rating_{it+1}$ with the expected sign except for $Salegro_{it}$.

4.2 The New Bond Issue Sample

I collect nonconvertible, fixed-rate bonds issued by U.S. firms from 1983 to 2002 from Securities Data Company's Global New Issues database. I exclude bonds with asset-backed or credit-enhancement features because the spreads of these bonds reflect the creditworthiness of the collateral rather than the creditworthiness of the firm (Campbell and Taksler 2003). For firms with multiple issuances in a given year, I only include the issue with the largest offering amount (Khurana and Raman 2003). Consistent with the credit ratings sample, I classify bonds rated as BBB+ or above at the issuance date into the low default risk subsample and those rated as BBB or below at the issuance date into the high default risk subsample. The final sample includes 1,763 bond issues: 1,129 low default risk bonds issued by 313 firms and 634 high default risk bonds issued by 322 firms. Table 3.1 reports the distribution of observations across the ratings categories.

Table 4.3 presents descriptive statistics for the aggregate new bond issue sample as well as subsamples partitioned by default risk. The mean yield spread equals 1.53 percent. As expected, the yield spread of high default risk bonds is much higher (2.55 percent) than that of low default risk bonds (0.96 percent). Approximately 92 percent of sample firms report profits, whereas 64 percent report earnings increases or beat analyst's earnings forecasts. Compared to the credit ratings sample, the new bond issue sample has a larger proportion of firms beating all three earnings benchmarks. This evidence is consistent with better-performing firms, *ceteris paribus*, being more likely to issue public bonds (Denis and Mihov 2003). Also as expected, low default risk firms are more likely to report profits.[10] However, the incidence of earnings increases and beating analysts' forecasts does not differ significantly across the two subsamples. Univariate comparisons in Table 4.3 also indicate that high default risk firms have higher beta, more volatile stock returns, higher book-to-market ratio, smaller size, and higher leverage than low default risk firms. On the other hand, high default risk firms have, on average, larger earnings increases, larger earnings surprises and higher sales growth than low default risk firms.

Table 4.4 presents the correlations among all variables for the aggregate new bond issue sample. I report Spearman correlations above the diagonal and Pearson correlations below the diagonal. I only report correlations that are significant at 10 percent significance levels (two-tailed test). Consistent with H1, all three earnings benchmark indicators are negatively associated with $Spread_{it+1}$, indicating that beating any of the earnings benchmarks in year t is associated with a smaller yield spread in year $t+1$.[11] Consistent with H3, the Pearson correlation between $Profit_{it}$ and $Spread_{it+1}$ is stronger (ρ= -0.255) than the correlation between $Incr_{it}$ and $Spread_{it+1}$ (ρ= -0.098) and the correlation between $Surp_{it}$ and $Spread_{it+1}$ (ρ= -0.045). Pearson correlations indicate that most control variables are significantly correlated with $Spread_{it+1}$ with the expected sign except for ΔEPS_{it} and $Salegro_{it}$.

[10] In the new bond issue sample, the default risk measure ($Rating_{it}$) is bond specific. For senior bonds, the bond rating is usually the firm credit rating. In my sample, 90 percent of bonds are senior bonds. For simplicity, I refer to "high default risk firms" instead of "firms that issue high default risk bonds."
[11] $Surp_{it}$ is only significantly associated with $Spread_{it+1}$ using Pearson correlations.

Table 4.1
Descriptive Statistics for Credit Ratings Sample[a]

Variable	Significant Difference[b]	Mean	Median	Std Dev	Lower Quartile	Upper Quartile
$Rating_{it+1}$						
Aggregate		9	9	4	6	12
High Default	HD>LD	12	12	2	10	14
Low Default		6	6	2	5	7
Profit						
Aggregate		0.8240	1.0000	0.3809	1.0000	1.0000
High Default	HD<LD	0.7306	1.0000	0.4437	0.0000	1.0000
Low Default		0.9424	1.0000	0.2331	1.0000	1.0000
Incr						
Aggregate		0.5981	1.0000	0.4903	0.0000	1.0000
High Default	HD<LD	0.5598	1.0000	0.4965	0.0000	1.0000
Low Default		0.6467	1.0000	0.4781	0.0000	1.0000
Surp						
Aggregate		0.6207	1.0000	0.4852	0.0000	1.0000
High Default	HD<LD	0.6097	1.0000	0.4879	0.0000	1.0000
Low Default		0.6347	1.0000	0.4816	0.0000	1.0000
EPS						
Aggregate		0.0344	0.0539	0.1331	0.0201	0.0809
High Default	HD<LD	0.0160	0.0459	0.1709	-0.0092	0.0846
Low Default		0.0577	0.0588	0.0464	0.0383	0.0782
ΔEPS						
Aggregate		0.0054	0.0059	0.2302	-0.0215	0.0242
High Default	HD ≈LD	0.0073	0.0063	0.3042	-0.0378	0.0378
Low Default		0.0030	0.0057	0.0529	-0.0092	0.0160
UE_EPS						
Aggregate		-0.0023	0.0003	0.0468	-0.0017	0.0026
High Default	HD ≈LD	-0.0040	0.0004	0.0606	-0.0031	0.0036
Low Default		-0.0002	0.0003	0.0170	-0.0009	0.0018
$Rating_{it}$						
Aggregate		9	9	4	6	12
High Default	HD>LD	12	12	2	10	14
Low Default		6	6	2	4	7
Salegro						
Aggregate		0.1525	0.0797	0.6749	0.0007	0.1844
High Default	HD>LD	0.1993	0.0914	0.8659	-0.0067	0.2416
Low Default		0.0932	0.0710	0.2759	0.0083	0.1406
Beta						
Aggregate		1.0935	1.0484	0.5388	0.7629	1.3462
High Default	HD>LD	1.1844	1.1274	0.6288	0.7860	1.5016
Low Default		0.9783	0.9853	0.3653	0.7414	1.2064
StdRet						
Aggregate		0.0260	0.0229	0.0125	0.0174	0.0312
High Default	HD>LD	0.0308	0.0280	0.0138	0.0214	0.0367
Low Default		0.0198	0.0183	0.0069	0.0149	0.0232

Table 4.1 (Continued)
Descriptive Statistics for Credit Ratings Sample[a]

Variable	Significant Difference[b]	Mean	Median	Std Dev	Lower Quartile	Upper Quartile
BM						
Aggregate		-0.7759	-0.7173	0.7898	-1.1828	-0.2793
High Default	HD>LD	-0.5953	-0.5294	0.8188	-1.0004	-0.0919
Low Default		-1.0050	-0.9212	0.6862	-1.3428	-0.5440
Size						
Aggregate		7.7150	7.5776	1.4425	6.7064	8.6061
High Default	HD<LD	7.1020	6.9838	1.2279	6.2649	7.8211
Low Default		8.4923	8.3325	1.3141	7.5186	9.3630
Lev						
Aggregate		0.2804	0.2578	0.1591	0.1648	0.3748
High Default	HD>LD	0.3488	0.3349	0.1616	0.2342	0.4552
Low Default		0.1937	0.1867	0.1041	0.1192	0.2594

[a] The aggregate sample consists of 10,430 firm year observations from 1985 to 2002. The high default risk subsample consists of 5,831 firm year observations whose S&P senior debt ratings are BBB or below. The low default risk subsample consists of 4,599 firm year observations whose S&P senior debt ratings are BBB+ or above. Table 3.2 provides variable definitions.

[b] Significant differences between the high default risk and low default risk subsamples are based on z-statistics of Wilcoxon Rank Sum Tests of medians ($p < 0.10$).

Table 4.2
Significant Correlations for the Aggregate Credit Ratings Sample

	$Rating_{it+1}$	Profit	Incr	Surp	EPS	ΔEPS	UE_EPS	$Rating_{it}$	Salegro	Beta	StdRet	BM	Size	Lev
$Rating_{it+1}$		-0.392	-0.145	-0.058	-0.238	-0.042	-0.017	0.983	0.078	0.223	0.626	0.340	-0.593	0.583
Profit	-0.400		0.369	0.151	0.660	0.391	0.135	-0.367	0.143	-0.145	-0.333	-0.186	0.146	-0.225
Incr	-0.149	0.369		0.111	0.475	0.849	0.102	-0.113	0.240	-0.037	-0.141	-0.200	0.031	-0.121
Surp	-0.060	0.151	0.111		0.143	0.122	0.841	-0.047	0.046	-0.047		-0.080	0.056	-0.047
EPS	-0.274	0.608	0.334	0.133		0.531	0.185	-0.206	0.155	-0.078	-0.304		0.052	-0.143
ΔEPS		0.194	0.292	0.054	0.386		0.146		0.220	-0.020	-0.067	-0.118		-0.061
UE_EPS	-0.084	0.152	0.082	0.286	0.228	0.128			0.029	-0.017	-0.018		0.025	-0.018
$Rating_{it}$	0.983	-0.371	-0.115	-0.048	-0.246		-0.071		0.104	0.228	0.614	0.311	-0.602	0.578
Salegro	0.103	-0.021	0.037			0.041		0.114		0.096	0.092	-0.169	-0.103	0.089
Beta	0.264	-0.179	-0.052	-0.041	-0.104			0.265	0.101		0.230	0.100	-0.172	0.024
StdRet	0.595	-0.389	-0.146	-0.030	-0.334		-0.066	0.579	0.099	0.342		0.178	-0.324	0.306
BM	0.305	-0.166	-0.177	-0.072	-0.086	-0.047	-0.051	0.279	-0.052	0.055	0.152			0.063
Size	-0.586	0.136	0.024	0.052	0.099		0.070	-0.597	-0.066	-0.174	-0.284	0.023		-0.305
Lev	0.580	-0.252	-0.126	-0.057	-0.162	-0.025	-0.055	0.575	0.099	0.053	0.292	-0.035	-0.307	

The aggregate credit ratings sample consists of 10,430 firm year observations from 1985 to 2002.
Spearman (Pearson) correlations are above (below) the diagonal.
All correlations in the table are significant at $p < .10$ (two-tailed) significance levels.
Table 3.2 provides variable definitions.

Table 4.3
Descriptive Statistics for the New Bond Issue Sample[a]

Variable	Significant Difference[b]	Mean	Median	Std Dev	Lower Quartile	Upper Quartile
Spread						
Aggregate		1.5304	1.1000	1.2405	0.7200	1.8100
High Default	HD>LD	2.5508	2.1950	1.4688	1.3500	3.6300
Low Default		0.9575	0.8500	0.5284	0.6000	1.2000
Profit						
Aggregate		0.9189	1.0000	0.2731	1.0000	1.0000
High Default	HD<LD	0.8517	1.0000	0.3556	1.0000	1.0000
Low Default		0.9566	1.0000	0.2038	1.0000	1.0000
Incr						
Aggregate		0.6449	1.0000	0.4787	0.0000	1.0000
High Default	HD ≈LD	0.6230	1.0000	0.4850	0.0000	1.0000
Low Default		0.6572	1.0000	0.4748	0.0000	1.0000
Surp						
Aggregate		0.6398	1.0000	0.4802	0.0000	1.0000
High Default	HD ≈LD	0.6467	1.0000	0.4784	0.0000	1.0000
Low Default		0.6360	1.0000	0.4814	0.0000	1.0000
EPS						
Aggregate		0.0621	0.0646	0.0741	0.0395	0.0903
High Default	HD ≈LD	0.0560	0.0621	0.1084	0.0315	0.1018
Low Default		0.0656	0.0650	0.0443	0.0439	0.0866
ΔEPS						
Aggregate		0.0081	0.0063	0.1001	-0.0111	0.0196
High Default	HD>LD	0.0172	0.0087	0.1565	-0.0181	0.0336
Low Default		0.0031	0.0059	0.0427	-0.0084	0.0151
UE_EPS						
Aggregate		0.0000	0.0004	0.0239	-0.0010	0.0026
High Default	HD>LD	0.0003	0.0006	0.0362	-0.0013	0.0043
Low Default		-0.0002	0.0003	0.0125	-0.0008	0.0018
Rating						
Aggregate		8	7	3	5	9
High Default	HD>LD	11	10	2	9	13
Low Default		6	6	2	5	7
Salegro						
Aggregate		0.1205	0.0771	0.2344	0.0191	0.1574
High Default	HD>LD	0.1706	0.0928	0.3207	0.0131	0.2358
Low Default		0.0924	0.0746	0.1608	0.0216	0.1349
Beta						
Aggregate		1.0271	1.0267	0.4141	0.7699	1.2644
High Default	HD>LD	1.0890	1.0812	0.4962	0.7714	1.3792
Low Default		0.9924	1.0058	0.3555	0.7699	1.2083
StdRet						
Aggregate		2.0588	1.9106	0.6904	1.5613	2.4258
High Default	HD>LD	2.3659	2.2117	0.7729	1.8368	2.8284
Low Default		1.8863	1.7521	0.5714	1.4822	2.1418

Table 4.3 (Continued)
Descriptive Statistics of the New Bond Issue Sample[a]

Variable	Significant Difference[b]	Mean	Median	Std Dev	Lower Quartile	Upper Quartile
BM						
Aggregate		-0.8568	-0.7770	0.7050	-1.2349	-0.3770
High Default	HD>LD	-0.6399	-0.5629	0.6776	-0.9891	-0.1722
Low Default		-0.9785	-0.8870	0.6910	-1.3236	-0.5087
Size						
Aggregate		8.3273	8.3291	1.4277	7.3783	9.3104
High Default	HD<LD	7.6023	7.4995	1.4166	6.5445	8.7169
Low Default		8.7345	8.6311	1.2635	7.8540	9.5396
Lev						
Aggregate		0.2516	0.2366	0.1337	0.1548	0.3345
High Default	HD>LD	0.3292	0.3236	0.1423	0.2332	0.4105
Low Default		0.2080	0.1975	0.1061	0.1323	0.2765
Senior						
Aggregate		0.9132	1.0000	0.2816	1.0000	1.0000
High Default	HD<LD	0.7618	1.0000	0.4263	1.0000	1.0000
Low Default		0.9982	1.0000	0.0421	1.0000	1.0000
IssueSize						
Aggregate		5.1641	5.2862	0.8689	4.6052	5.6994
High Default	HD<LD	5.0839	5.0106	0.7988	4.6042	5.5607
Low Default		5.2091	5.2938	0.9032	4.6052	5.7014
Call						
Aggregate		0.6264	0.7886	0.4162	0.2821	1.0000
High Default	HD<LD	0.5512	0.4936	0.3922	0.2467	1.0000
Low Default		0.6687	1.0000	0.4235	0.3283	1.0000

[a] The aggregate sample consists of 1,763 nonconvertible and fixed rate bonds issued from 1983 to 2002. The high default risk subsample consists of 634 bonds whose S&P ratings are BBB or below. The low default risk subsample consists of 1,129 bonds whose S&P ratings are BBB+ or above. Table 3.2 provides variable definitions.

[b] Significant differences between the high default risk and low default risk subsamples are based on *z*-statistics of Wilcoxon Rank Sum Tests of medians ($p < 0.10$).

Table 4.4
Significant Correlations for the Aggregate New Bond Issue Sample

	$Rating_{it+1}$	Profit	Incr	Surp	EPS	ΔEPS	UE_EPS	$Rating_{it}$	Salegro	Beta	StdRet	BM	Size	Lev
$Rating_{it+1}$		-0.392	-0.145	-0.058	-0.238	-0.042	-0.017	0.983	0.078	0.223	0.626	0.340	-0.593	0.583
Profit	-0.400		0.369	0.151	0.660	0.391	0.135	-0.367	0.143	-0.145	-0.333	-0.186	0.146	-0.225
Incr	-0.149	0.369		0.111	0.475	0.849	0.102	-0.113	0.240	-0.037	-0.141	-0.200	0.031	-0.121
Surp	-0.060	0.151	0.111		0.143	0.122	0.841	-0.047	0.046	-0.047		-0.080	0.056	-0.047
EPS	-0.274	0.608	0.334	0.133		0.531	0.185	-0.206	0.155	-0.078	-0.304		0.052	-0.143
ΔEPS		0.194	0.292	0.054	0.386		0.146		0.220	-0.020	-0.067	-0.118		-0.061
UE_EPS	-0.084	0.152	0.082	0.286	0.228	0.128			0.029	-0.017	-0.018		0.025	-0.018
$Rating_{it}$	0.983	-0.371	-0.115	-0.048	-0.246		-0.071		0.104	0.228	0.614	0.311	-0.602	0.578
Salegro	0.103	-0.021	0.037			0.041		0.114		0.096	0.092	-0.169	-0.103	0.089
Beta	0.264	-0.179	-0.052	-0.041	-0.104			0.265	0.101		0.230	0.100	-0.172	0.024
StdRet	0.595	-0.389	-0.146	-0.030	-0.334		-0.066	0.579	0.099	0.342		0.178	-0.324	0.306
BM	0.305	-0.166	-0.177	-0.072	-0.086	-0.047	-0.051	0.279	-0.052	0.055	0.152			0.063
Size	-0.586	0.136	0.024	0.052	0.099		0.070	-0.597	-0.066	-0.174	-0.284	0.023		-0.305
Lev	0.580	-0.252	-0.126	-0.057	-0.162	-0.025	-0.055	0.575	0.099	0.053	0.292	-0.035	-0.307	

The aggregate sample consists of 1,763 nonconvertible and fixed rate bonds issued from 1983 to 2002.
Spearman (Pearson) correlations are above (below) the diagonal.
All correlations in the table are significant at $p < .10$ (two-tailed) significance levels.
Table 3.2 provides variable definitions.

CHAPTER 5

MAIN RESULTS

I estimate models (1) and (2) using ordinary least squares. I remove the effects of influential observations identified by the Belsley et al.'s (1980) procedure (i.e., absolute R-student value exceeding two). Inferences are identical when estimating both models using all observations. To control for heteroscedasticity, I report *p*-values based on robust standard errors (White 1980).

Table 5.1, Panel A and Table 5.4, Panel A report the results of estimating models (1) and (2) respectively. Columns (1)-(3) of each panel report results including only one benchmark indicator per model. The coefficient of each benchmark indicator represents the *average* effect of beating that benchmark on next year's credit ratings or on the bond's initial yield spread. Columns (4)-(6) report results including all three benchmark indicators for the aggregate sample, the high default risk subsample, and the low default risk subsample, respectively. In columns (4)-(6), the coefficient for each benchmark indicates the *incremental* effect of beating the benchmark after controlling for the effects of the other two benchmarks. Column (7) compares the coefficient estimates between the high default risk and low default risk subsamples. To compare the coefficients differences between high and low default risk firms, I estimate models (1) and (2) using the aggregate sample allowing observations from the high and low default risk subsamples to have different coefficients. Column (7) reports the coefficient differences and related *p* values from *t* statistics testing whether the coefficients for the high and low default risk subsamples are equal.

Table 5.1, Panel B and Table 5.4, Panel B report the relative importance of beating each benchmark in the credit ratings sample and in the new bond issue sample, respectively. Because I expect that beating earnings benchmarks has little effect on credit ratings or yield spread for low default risk firms, I calculate the differences in the coefficients between any two benchmark indicators only for the aggregate and high default risk samples.

5.1 The Credit Ratings Sample

Panel A of Table 5.1 presents the results of estimating model (1) for the credit ratings sample. The results in columns (1)-(3) suggest that consistent with H1, beating any of the three earnings benchmarks is associated with improved one-year ahead credit ratings ($p \leq 0.001$, one-tailed test). Column (4) indicates that for the aggregate sample, beating any of the three earnings benchmarks is associated with better one-year ahead ratings even after controlling for the effect of the other two benchmarks ($p \leq 0.008$, one-tailed test). Columns (5)-(7) of Panel A test whether the effect of beating earnings benchmarks is stronger for high default risk firms than for low default risk firms. Column (5) indicates that beating any of the three earnings benchmarks is incrementally associated with an improved one-year ahead rating for high default risk firms ($p \leq 0.005$, one-tailed test). For low default risk firms, column (6) shows that only reporting an earnings increase is incrementally associated with a better one-year-ahead rating ($p < 0.001$, one-tailed test). Column (7) reports the difference between the coefficient estimates for high default risk firms and low default risk firms. The results indicate that beating any of the three earnings benchmarks has a bigger impact on credit ratings for high default risk firms than for low default risk firms ($p \leq 0.08$, one tailed test). Overall, the results in columns (5)-(7) support H2.

Panel B of Table 5.1 compares the relative importance of each of the three benchmarks in the aggregate and high default risk samples. In both samples, the effect of reporting profits is significantly stronger than the effect of beating analysts' earnings forecasts ($p = 0.001$, one-tailed test). The difference between reporting profits and reporting an earnings increase is in the expected direction for the aggregate sample but not statistically significant ($p = 0.224$, one-tailed test). For the high default risk sample, the difference is in the expected direction and statistically significant ($p = 0.088$, one-tailed test). Panel B also shows that reporting an earnings increase has a significantly stronger effect than beating analysts' earnings forecasts ($p \leq 0.023$, two-tailed test). Overall, the results suggest that the relative importance of beating the various earnings benchmarks differs in the debt market than in the equity market. In contrast to the equity market, both reporting profits and reporting earnings increases have stronger impacts on firms' credit ratings than beating analysts' earnings forecasts.

Table 5.1, Panel A indicates that the coefficient estimates on most control variables are significant in the expected directions. In particular, the coefficients for $Rating_{it}$, $Beta_{it}$, $StdRet_{it}$, BM_{it}, and Lev_{it} are positive and significant. Likewise, the coefficients for $Salegro_{it}$ and $Size_{it}$ are negative and significant. In general, the coefficients for the continuous earnings variables (EPS_{it}, ΔEPS_{it} and UE_EPS_{it}) are negative and significant as expected. The few cases of insignificant coefficients for the continuous earnings variables could be due to collinearity among the independent variables.[12] To test whether the results in Panel A are sensitive to the collinearity arising from including all three continuous earnings variables, EPS_{it}, ΔEPS_{it}, or UE_EPS_{it}, I repeat the analysis including only one of them at a time as a control variable in model (1). Table 5.2 indicates that the magnitudes of the three earnings benchmark indicator variables remain similar regardless of which continuous earnings variable I include.

[12] Both the Variance Inflation Factor (VIF) and the condition index indicate the existence of multicollinearity. For the model estimated in the aggregate sample with all three benchmark variables included (column 4 of Panel A), the average VIF is 1.66 and the condition number is 44, suggesting multicollinearity is somewhat problematic (Belsey, Kuh, and Welsch 1980; Kennedy 1998).

Model (1) assumes that the continuous earnings variables (EPS_{it}, ΔEPS_{it}, and UE_EPS_{it}) have the same effects on $Ratings_{it+1}$ independent of whether the firm beats or misses the earnings benchmarks. To ensure that the benchmark indicators do not spuriously reflect a possible asymmetric effect between EPS_{it}, ΔEPS_{it}, *or* UE_EPS_{it} and $Ratings_{it+1}$, I add interactions between each benchmark indicator and its corresponding continuous earnings variable. Table 5.3 presents the results from this analysis and indicates that inferences regarding the benchmark indicators remain the same.

5.2 The New Bond Issue Sample

Table 5.4, Panel A presents the results of estimating model (2) for the new bond issue sample. The results in columns (1) and (3) indicate that consistent with H1, beating the profit benchmark and the most recent analyst's earnings forecast is associated with a smaller yield spread. In particular, firms reporting profits have a bond yield spread that is 25 basis points smaller than firms reporting losses ($p < 0.001$, one-tailed test). Firms beating the most recent analyst earnings forecast have a bond yield spread that is seven basis points smaller than firms missing the analyst's earnings forecasts ($p = 0.003$, one-tailed test). Column (2) indicates that firms reporting earnings increases have a bond yield spread that is three basis points smaller than firms reporting earnings decreases, but the difference is not significant at conventional levels ($p = 0.169$, one-tailed test).

Column (4) shows that in the aggregate sample, both reporting profits and beating the analyst's earnings forecast are incrementally associated with a smaller initial bond yield spread after controlling for the other benchmarks ($p \leq 0.005$, one-tailed test). The incremental effect of reporting an earnings increase on the yield spread is negative but not significant at conventional levels ($p = 0.225$, one-tailed test).

Columns (5) and (6) suggest that consistent with H2, the effect of beating earnings benchmarks is stronger for high default risk firms than for low default risk firms. For example, for the high default risk firms, both reporting profits and beating the analyst's forecasted earnings are associated with a smaller initial bond yield spread ($p \leq 0.060$, one-tailed test). In contrast, for the low default risk subsample, no benchmark is incrementally associated with a smaller yield spread – i.e., there is no evidence that beating earnings benchmarks is associated with a smaller yield spread for the low default risk sample. Column (7) shows that the effect of reporting profits is stronger in the high default risk subsample than in the low default risk subsample ($p \leq 0.048$, one-tailed test). The yield spread of beating the most recent analyst earnings forecasts for high default risk firms is six basis points smaller than that for low default risk firms, but the difference is not significant at conventional levels ($p = 0.140$, one-tailed test). Overall, the results in columns (5)-(7) provide some evidence that the effects of beatings earnings benchmarks are stronger for high default risk firms than for low default risk firms.

Panel B of Table 5.4 compares the relative importance of beating each benchmark in the new bond issue sample. Consistent with H3, reporting profits has the largest effect on the initial bond yield spread for both the aggregate sample and the high default risk subsample ($p \leq 0.086$, one-tailed test). In contrast to the credit ratings sample, reporting an earnings increase does not have a larger effect on the initial bond yield spread than beating analysts' earnings forecasts.

The majority of the control variables are significant with expected signs. In particular, the coefficients for $Rating_{it}$, $StdRet_{it}$, BM_{it} and Lev_{it} are positive and significant. Likewise, the coefficients for $Salegro_{it}$, $Size_{it}$, $Senior_{it}$, and $Call_{it}$ are negative and significant. The coefficient of $IssueSize_{it}$ is positive and significant in most cases, indicating that in my sample the size of the bond issue reflects the overall debt burden more than the bond's liquidity. The coefficients for the continuous earnings variables (EPS_{it},, ΔEPS_{it} and UE_EPS_{it}) are in general negative and significant as expected.

I have included all three continuous earnings variables (EPS_{it}, ΔEPS_{it}, and UE_EPS_{it}) in model (2). The collinearity among the independent variables will lead to less precise coefficient estimates. To test whether the results in Table 5.4 are sensitive to the collinearity arising from including all three continuous earnings variables, EPS_{it}, ΔEPS_{it}, or UE_EPS_{it}, I repeat the analysis including only one of them at a time as a control variable in model (2). Table 5.5 indicates that the inferences regarding the *incremental* effects of beating each earnings benchmark remain the same (i.e., the coefficients for *Profit* and *Surp* continue to be negative and statistically significant while the coefficient for *Incr* remains insignificant). Table 5.6 reports the results of testing the *average* effects of each benchmark including only one benchmark (e.g., *Profit*) and its corresponding continuous earnings variable (e.g., EPS_{it}) per regression. The coefficients for *Profit* and *Surp* continue to be negative and statistically significant as before. In contrast to previous regressions, $Incr_{it}$ becomes significant in both the aggregate and the high default risk samples ($p= 0.094$ and $p=0.051$ respectively, one tailed test). All other inference remains the same.

To assess whether the benchmark indicators reflect a possible asymmetric effect between EPS_{it}, ΔEPS_{it}, *or* UE_EPS_{it} and $Spread_{it+1}$, I add interactions between each benchmark indicator and its corresponding continuous earnings variable. The results in Table 5.7 indicate that the inferences regarding $Profit_{\mathrm{it}}$ and $Surp_{it}$ remain the same while the coefficient for $Incr_{\mathrm{it}}$ is insignificant.

Table 5.1
Regression of One-year-ahead Ratings on the Three Earnings Benchmarks

$$Rating_{it+1} = \alpha_0 + \alpha_1\, Profit_{it} + \alpha_2\, Incr_{it} + \alpha_3\, Surp_{it} + \beta_1 EPS_{it} + \beta_2 \Delta EPS_{it} + \beta_3 UE\_EPS_{it} + \beta_4\, Rating_{it} + \beta_5\, Salegro_{it} + \beta_6\, Beta_{it} + \beta_7\, StdRet_{it} + \beta_8\, BM_{it} + \beta_9\, Size_{it} + \beta_{10}\, Lev_{it} + \textstyle\sum_t \beta_t\, Year_{it} + Error_{it}$$

Panel A: Regression Results

Variable	Predicted Sign	(1) Profit	(2) Incr	(3) Surp	(4) Agg	(5) High	(6) Low	(7)[a] High-Low
Profit	-	-0.130			-0.098	-0.132	0.009	-0.141
		(0.000)			(0.000)	(0.000)	(0.591)	(0.001)
Incr	-		-0.093		-0.080	-0.092	-0.060	-0.032
			(0.000)		(0.000)	(0.000)	(0.000)	(0.084)
Surp	-			-0.033	-0.026	-0.043	-0.002	-0.041
				(0.001)	(0.008)	(0.005)	(0.449)	(0.026)
EPS	-	-0.244	-0.380	-0.461	-0.228	-0.208	-0.841	0.633
		(0.001)	(0.000)	(0.000)	(0.000)	(0.002)	(0.001)	(0.018)
ΔEPS	-	-0.091	-0.026	-0.061	-0.040	-0.039	0.058	-0.097
		(0.034)	(0.118)	(0.004)	(0.019)	(0.020)	(0.585)	(0.719)
UE_EPS	-	-0.225	-0.272	-0.188	-0.092	-0.115	-0.189	0.074
		(0.064)	(0.038)	(0.117)	(0.269)	(0.245)	(0.232)	(0.810)
Rating	+	0.937	0.940	0.939	0.938	0.918	0.955	-0.036
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Salegro	-	-0.047	-0.045	-0.046	-0.045	-0.025	-0.064	0.039
		(0.000)	(0.000)	(0.000)	(0.000)	(0.054)	(0.025)	(0.285)
Beta	+	0.024	0.025	0.031	0.023	0.024	0.029	-0.005
		(0.016)	(0.012)	(0.003)	(0.018)	(0.034)	(0.067)	(0.828)
StdRet	+	4.290	4.540	4.547	4.212	5.841	0.792	5.049
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.281)	(0.002)
BM	+	0.140	0.131	0.141	0.132	0.135	0.139	-0.005
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.757)
Size	-	-0.039	-0.036	-0.035	-0.04	-0.057	-0.021	-0.036
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Lev	+	0.557	0.553	0.580	0.537	0.546	0.537	0.008
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.922)
Observations		9952	9949	9950	9952	5578	4387	9965
Adjusted R-squared		0.9831	0.9832	0.9831	0.9832	0.9386	0.9608	0.9830

Robust *p*-values are in parentheses. I calculate *p*-values using one-tailed tests unless noted otherwise. The sample sizes vary across the models because I eliminate potentially influential observations in each regression using Belsley

et al.'s (1980) diagnostics. The estimated coefficients for the constant term and year dummy variables are not reported. Table 3.2 provides variable definitions.
[a] Column (7) reports the difference in coefficients between the high default risk subsample and the low default risk subsample. The *p-values* are based on one-tailed tests for *Profit, Incr,* and *Surp* and two-tailed tests for all other variables.

Table 5.1 (Continued)
Panel B: Testing the Relative Importance of Beating Each Benchmark

	H0: *Profit* ≥ *Incr* *Difference** *(p-value)*	H0: *Profit* ≥ *Surp* *Difference** *(p-value)*	H0: *Incr* = *Surp* *Difference** *(p-value)*
Aggregate Sample	-0.017	-0.072	-0.055
	(0.224)	(0.001)	(0.000)
High Default Sample	-0.040	-0.089	-0.049
	(0.088)	(0.001)	(0.023)

*Difference in the earnings benchmark coefficients from columns (4) or (5) of Panel A. I calculate *p-values* using a one-tailed test when comparing *Profit* with *Incr* and *Surp*. I use a two-tailed test when comparing *Incr* and *Surp*.

Table 5.2
Regression of One-year-ahead Ratings on the Three Earnings Benchmarks with One Continuous Earnings Variable

$$Rating_{it+1} = \alpha_0 + \alpha_1\, Profit_{it} + \alpha_2\, Incr_{it} + \alpha_3\, Surp_{it} + \beta_1 X_{it} + \beta_2\, Rating_{it} + \beta_3\, Salegro_{it} + \beta_4\, Beta_{it} + \beta_5\, StdRet_{it} + \beta_6\, BM_{it} + \beta_7\, Size_{it} + \beta_8\, Lev_{it} + \sum_t \beta_t\, Year_{it} + Error_{it}$$

X= EPS_{it} or ΔEPS_{it} or UE_EPS_{it}.

Variable	Predicted Sign	(1) Agg	(2) High	(3) Low	(4)[a] High-Low	(5) Agg	(6) High	(7) Low	(8)[a] High-Low	(9) Agg	(10) High	(11) Low	(12)[a] High-Low
Profit	-	-0.098	-0.131	0.009	-0.140	-0.137	-0.169	-0.062	-0.106	-0.142	-0.172	-0.076	-0.096
		(0.000)	(0.000)	(0.593)	(0.001)	(0.000)	(0.000)	(0.038)	(0.004)	(0.000)	(0.000)	(0.009)	(0.067)
Incr	-	-0.084	-0.096	-0.057	-0.039	-0.085	-0.096	-0.066	-0.030	-0.093	-0.108	-0.077	-0.031
		(0.000)	(0.000)	(0.000)	(0.034)	(0.000)	(0.000)	(0.000)	(0.100)	(0.000)	(0.000)	(0.000)	(0.037)
Surp	-	-0.028	-0.045	-0.004	-0.041	-0.029	-0.047	-0.006	-0.041	-0.025	-0.041	-0.003	-0.038
		(0.004)	(0.003)	(0.384)	(0.022)	(0.003)	(0.002)	(0.328)	(0.021)	(0.010)	(0.007)	(0.395)	(0.000)
EPS	-	-0.254	-0.248	-0.814	0.566								
		(0.000)	(0.000)	(0.000)	(0.016)								
ΔEPS	-					-0.075	-0.07	-0.219	0.149				
						(0.002)	(0.003)	(0.169)	(0.517)				
UE_EPS	-									-0.197	-0.214	-0.26	0.045
										(0.090)	(0.097)	(0.156)	(0.882)
Observations		9952	5578	4387	9965	9952	5576	4389	9965	9955	5577	4389	9966
Adjusted R-squared		0.9832	0.9385	0.9608	0.9830	0.9832	0.9386	0.9607	0.9830	0.9831	0.9385	0.9606	0.9830

*Robust *p*-values are in parentheses. I calculate *p*-values using one-tailed tests unless noted otherwise. The sample sizes vary across the models because I eliminate potentially influential observations in each regression using Belsley et al.'s (1980) diagnostics. The estimated coefficients for most control variables are not reported. Table 3.2 provides variable definitions.

[a] Column (4), (8) and (12) report the difference in coefficients between the high default risk subsample and the low default risk subsample. I calculate *p-values* using one-tailed tests for *Profit, Incr,* and *Surp* and two-tailed tests for all other variables.

Table 5.3
Regression of One-year-ahead Ratings on the Three Earnings Benchmarks with Interactions Terms

$Rating_{it+1} = \alpha_0 + \alpha_1\ Benchmark_{it} + \alpha_2\ EarningsVar_{it} + \alpha_3\ Benchmark_{it}{*}EarningsVar_{it} + \beta_1\ Rating_{it} + \beta_2\ Salegro_{it} + \beta_3\ Beta_{it} + \beta_4\ StdRet_{it} + \beta_5\ BM_{it} + \beta_6\ Size_{it} + \beta_7\ Lev_{it} + \sum_t \beta_t\ Year_{it} + Error_{it}$

$Benchmark_{it}$ is $Profit_{it}$, $Incr_{it}$, or $Surp_{it}$ and $EarningsVar_{it}$ is the related continuous earnings variable EPS_{it}, ΔEPS_{it}, or UE_EPS_{it}

Variable	Predicted Sign	(1) Profit	(2) Profit	(3) Incr	(4) Incr	(5) Surp	(6) Surp	(7) Agg	(8) High	(9) LOW	(10) High_low
Profit	-	-0.129	-0.095					-0.070	-0.097	0.021	-0.119
		(0.000)	(0.000)					(0.000)	(0.000)	(0.690)	(0.007)
EPS	-	-0.322	-0.095					0.089	0.073	0.049	0.024
		(0.000)	(0.088)					(0.849)	(0.776)	(0.535)	(0.517)
Profit* EPS	?		-1.002					-1.011	-0.997	-1.004	0.008
			(0.000)					(0.000)	(0.000)	(0.107)	(0.991)
Incr	-			-0.106	-0.104			-0.056	-0.062	-0.047	-0.015
				(0.000)	(0.000)			(0.000)	(0.000)	(0.003)	(0.271)
ΔEPS	-			-0.165	-0.315			-0.201	-0.189	-0.712	0.522
				(0.000)	(0.000)			(0.026)	(0.050)	(0.023)	(0.918)
Incr*ΔEPS	?				0.250			0.155	0.141	1.118	-0.977
					(0.004)			(0.141)	(0.228)	(0.038)	(0.077)
Surp	-					-0.040	-0.039	-0.022	-0.039	-0.001	-0.038
						(0.000)	(0.000)	(0.037)	(0.018)	(0.958)	(0.068)
UE_EPS	-					-0.435	-0.838	-0.549	-0.708	-0.347	-0.361
						(0.001)	(0.000)	(0.002)	(0.002)	(0.136)	(0.183)
Surp*UE_EPS	?						1.068	0.995	1.221	0.455	0.766
							(0.000)	(0.000)	(0.000)	(0.454)	(0.264)
Observations		9953	9947	9943	9946	9942	9945	9949	5578	4389	9967
Adjusted R-squared		0.9831	0.9833	0.9831	0.9831	0.9829	0.9829	0.9833	0.9389	0.9608	0.9831

*Robust *p*-values are in parentheses. I calculate *p*-values using one-tailed tests for variables with predicted sign and two-tailed tests otherwise. The sample sizes vary across the models because I eliminate potentially influential observations in each regression using Belsley et al.'s (1980) diagnostics. The estimated coefficients for most control variables are not reported. Table 3.2 provides variable definitions.

Table 5.4
Regression of Initial Bond Yield spread on the Three Earnings Benchmarks

$$Spread_{it+1} = \alpha_0 + \alpha_1\, Profit_{it} + \alpha_2\, Incr_{it} + \alpha_3\, Surp_{it} + \beta_1 EPS_{it} + \beta_2 \Delta EPS_{it} + \beta_3 UE\_EPS_{it} + \beta_4\, Rating_{it} + \beta_5\, Salegro_{it} + \beta_6\, Beta_{it} + \beta_7\, StdRet_{it} + \beta_8\, BM_{it} + \beta_9\, Size_{it} + \beta_{10}\, Lev_{it} + \beta_{11}\, Senior_{it} + \beta_{12}\, IssueSize_{it} + \beta_{13}\, Call_{it} + \sum_t \beta_t\, TimePrd_{it} + Error_{it}$$

Panel A: Regression Results

Variable	Predicted Sign	(1) Profit	(2) Incr	(3) Surp	(4) Agg	(5) High	(6) Low	(7)[a] High-Low
Profit	-	-0.250			-0.241	-0.260	-0.053	-0.207
		(0.000)			(0.001)	(0.009)	(0.191)	(0.048)
Incr	-		-0.031		-0.025	-0.035	0.001	-0.036
			(0.169)		(0.225)	(0.296)	(0.517)	(0.303)
Surp	-			-0.074	-0.071	-0.090	-0.024	-0.066
				(0.003)	(0.005)	(0.060)	(0.133)	(0.140)
EPS	-	-0.073	-0.528	-0.592	-0.023	0.010	0.838	-0.828
		(0.414)	(0.029)	(0.012)	(0.474)	(0.509)	(0.991)	(0.144)
ΔEPS	-	-0.000	-0.005	-0.042	0.066	-0.109	-1.038	0.929
		(0.500)	(0.492)	(0.427)	(0.606)	(0.351)	(0.000)	(0.028)
UE_EPS	-	-1.349	-1.201	-0.831	-0.947	-1.074	-0.203	(0.872)
		(0.016)	(0.025)	(0.087)	(0.066)	(0.115)	(0.400)	(0.467)
Rating	+	0.159	0.161	0.162	0.160	0.413	0.074	0.339
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Salegro	-	-0.132	-0.130	-0.107	-0.118	-0.221	-0.073	(0.148)
		(0.026)	(0.020)	(0.053)	(0.043)	(0.005)	(0.122)	(0.163)
Beta	+	-0.013	-0.007	-0.006	-0.009	0.059	-0.049	0.108
		(0.629)	(0.567)	(0.559)	(0.589)	(0.173)	(0.927)	(0.129)
StdRet	+	0.258	0.262	0.254	0.253	0.294	0.121	0.174
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)
BM	+	0.107	0.114	0.118	0.105	0.213	0.119	0.093
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.037)
Size	-	-0.069	-0.066	-0.065	-0.069	-0.108	-0.029	-0.079
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.006)
Lev	+	0.233	0.292	0.299	0.241	0.047	0.247	(0.200)
		(0.035)	(0.011)	(0.009)	(0.030)	(0.413)	(0.006)	(0.398)
Senior	-	-0.814	-0.807	-0.808	-0.810	0.165	-0.028	0.193
		(0.000)	(0.000)	(0.000)	(0.000)	(0.947)	(0.437)	(0.345)
IssueSize	?	0.057	0.054	0.058	0.058	0.104	0.011	0.093
		(0.002)	(0.003)	(0.002)	(0.001)	(0.016)	(0.427)	(0.040)
Call	-	-0.200	-0.212	-0.200	-0.198	-0.136	-0.206	0.070
		(0.000)	(0.000)	(0.000)	(0.000)	(0.033)	(0.000)	(0.376)
Observations		1694	1694	1695	1696	609	1094	1703
Adjusted R-squared		0.7628	0.7607	0.7611	0.7626	0.7891	0.5189	0.8494

Robust *p*-values are in parentheses. I calculate *p*-values using one-tailed tests unless noted otherwise. The sample sizes vary across the models because I eliminate potentially influential observations in each regression using Belsley

et al.'s (1980) diagnostics. The estimated coefficients for the constant term and year dummy variables are not reported. Table 3.2 provides variable definitions.
[a] Column (7) reports the difference in coefficients between the high default risk subsample and the low default risk subsample. I calculate *p-values* using one-tailed tests for *Profit, Incr,* and *Surp* and two-tailed tests for all other variables.

Table 5.4 (Continued)
Panel B: Testing the Relative Importance of Earnings Benchmarks in the New Bond Issue Sample

	H0: *Profit* ≥ *Incr* *Difference** *(p-value)*	H0: *Profit* ≥ *Surp* *Difference** *(p-value)*	H0: *Incr* = *Surp* *Difference** *(p-value)*
Aggregate Sample	-0.217	-0.170	0.047
	(0.003)	(0.017)	(0.138)
High Default Sample	-0.225	-0.170	0.055
	(0.049)	(0.086)	(0.274)

*Difference in the earnings benchmark coefficients from columns (4) or (5) of Panel A. I calculate *p-values* using a one-tailed test when comparing *Profit* with *Incr* and *Surp*. I use a two-tailed test when comparing *Incr* and *Surp*.

Table 5.5
Regression of One-year-ahead Ratings on the Three Earnings Benchmarks with One Continuous Earnings Variable

$$Spread_{it+1} = \alpha_0 + \alpha_1\, Profit_{it} + \alpha_2\, Incr_{it} + \alpha_3\, Surp_{it} + \beta_1 X_{it} + \beta_2\, Rating_{it} + \beta_3\, Salegro_{it} + \beta_4\, Beta_{it} + \beta_5\, StdRet_{it} + \beta_6\, BM_{it} + \beta_7\, Size_{it} + \beta_8\, Lev_{it} + \beta_9\, Senior_{it} + \beta_{10}\, IssueSize_{it} + \beta_{11}\, Call_{it} + \sum_t \beta_t\, TimePrd_{it} + Error_{it}$$

X= EPS_{it} or ΔEPS_{it} or UE_EPS_{it}

Variable	Predicted Sign	(1) Agg	(2) High	(3) Low	(4)[a] High-Low	(5) Agg	(6) High	(7) Low	(8)[a] High-Low	(9) Agg	(10) High	(11) Low	(12)[a] High-Low
Profit	-	-0.246	-0.262	-0.074	-0.188	-0.236	-0.268	0.011	-0.279	-0.223	-0.299	-0.032	-0.267
		(0.001)	(0.009)	(0.113)	(0.068)	(0.000)	(0.002)	(0.583)	(0.005)	(0.000)	(0.001)	(0.259)	(0.007)
Incr	-	-0.02	-0.033	-0.044	0.010	-0.028	-0.032	0.000	-0.032	-0.017	-0.039	-0.031	-0.008
		(0.240)	(0.302)	(0.032)	(0.559)	(0.181)	(0.311)	(0.499)	(0.323)	(0.262)	(0.262)	(0.085)	(0.453)
Surp	-	-0.083	-0.101	-0.03	-0.071	-0.081	-0.114	-0.023	-0.091	-0.073	-0.081	-0.029	-0.052
		(0.001)	(0.040)	(0.065)	(0.121)	(0.001)	(0.021)	(0.123)	(0.062)	(0.004)	(0.080)	(0.093)	(0.196)
EPS	-	0	-0.265	0.416	-0.681								
		(0.500)	(0.268)	(0.086)	(0.097)								
ΔEPS	-					0.102	-0.166	-0.705	0.539				
						(0.330)	(0.275)	(0.005)	(0.083)				
UE_EPS	-									-1.094	-0.959	-0.054	-0.905
										(0.043)	(0.138)	(0.473)	(0.224)
Observations		1697	611	1090	1701	1696	609	1091	1700	1697	611	1092	1703
Adjusted R-squared		0.7626	0.7878	0.5151	0.8498	0.7631	0.7891	0.5168	0.8500	0.7624	0.7881	0.5130	0.8493

*Robust *p*-values are in parentheses. I calculate *p*-values using one-tailed tests unless noted otherwise. The sample sizes vary across the models because I eliminate potentially influential observations in each regression using Belsley et al.'s (1980) diagnostics. The estimated coefficients for most control variables are not reported. Table 3.2 provides variable definitions.

[a] Column (4), (8) and (12) report the difference in coefficients between the high default risk subsample and the low default risk subsample. I calculate *p-values* using one-tailed tests for *Profit, Incr,* and *Surp* and two-tailed tests for all other variables.

Table 5.6
Regression of One-year-ahead Ratings on the Three Earnings Benchmarks with One Continuous Earnings Variable

$$Spread_{it+1} = \alpha_0 + \alpha_1\, Benchmark_{it} + \beta_1 EarningsVar_{it} + \beta_2\, Rating_{it} + \beta_3\, Salegro_{it} + \beta_4\, Beta_{it} + \beta_5\, StdRet_{it} + \beta_6\, BM_{it} + \beta_7\, Size_{it} + \beta_8\, Lev_{it} + \beta_9\, Senior_{it} + \beta_{10}\, IssueSize_{it} + \beta_{11}\, Call_{it} + \textstyle\sum_t \beta_t\, TimePrd_{it} + Error_{it}$$

$Benchmark_{it}$ is $Profit_{it}$, $Incr_{it}$, or $Surp_{it}$ and $EarningsVar_{it}$ is the related continuous earnings variable EPS_{it}, ΔEPS_{it}, or UE_EPS_{it}.

Variable	Predicted Sign	(1) Agg	(2) High	(3) Low	(4)[a] High-Low	(5) Agg	(6) High	(7) Low	(8)[a] High-Low	(9) Agg	(10) High	(11) Low	(12)[a] High-Low
Profit	-	-0.252	-0.288	-0.074	-0.214								
		(0.000)	(0.004)	(0.116)	(0.043)								
Incr	-					-0.040	-0.102	0.005	-0.107				
						(0.094)	(0.051)	(0.578)	(0.056)				
Surp	-									-0.083	-0.109	-0.029	-0.080
										(0.002)	(0.029)	(0.087)	(0.097)
EPS	-	-0.092	-0.316	0.202	-0.517								
		(0.398)	(0.221)	(0.760)	(0.301)								
ΔEPS	-					-0.296	-0.219	-0.727	0.509				
						(0.103)	(0.215)	(0.002)	(0.177)				
UE_EPS	-									-1.251	-1.549	-0.123	-1.425
										(0.032)	(0.030)	(0.440)	(0.217)
Observations		1696	610	1092	1702	1695	610	1092	1702	1696	608	1093	1701
Adjusted R-squared		0.7625	0.7864	0.5104	0.8481	0.7610	0.7804	0.5140	0.8454	0.7610	0.7853	0.5097	0.8468

*Robust *p*-values are in parentheses. I calculate *p*-values using one-tailed tests unless noted otherwise. The sample sizes vary across the models because I eliminate potentially influential observations in each regression using Belsley et al.'s (1980) diagnostics. The estimated coefficients for most control variables are not reported. Table 3.2 provides variable definitions.

[a] Column (4), (8) and (12) report the difference in coefficients between the high default risk subsample and the low default risk subsample. I calculate *p-values* using one-tailed tests for *Profit, Incr,* and *Surp* and two-tailed tests for all other variables.

Table 5.7
Regression of One-year-ahead Ratings on the Three Earnings Benchmarks with Interactions Terms

$$Spread_{it+1} = \alpha_0 + \alpha_1\ Benchmark_{it} + \alpha_2\ EarningsVar_{it} + \alpha_3\ Benchmark_{it}*EarningsVar_{it} + \beta_1 Rating_{it} + \beta_2\ Salegro_{it} + \beta_3\ Beta_{it} + \beta_4\ StdRet_{it} + \beta_5\ BM_{it} + \beta_6\ Size_{it} + \beta_7\ Lev_{it} + \beta_8\ Senior_{it} + \beta_9\ IssueSize_{it} + \beta_{10}\ Call_{it} + \sum_t \beta_t\ TimePrd_{it} + Error_{it}$$

$Benchmark_{it}$ is $Profit_{it}$, $Incr_{it}$, or $Surp_{it}$ and $EarningsVar_{it}$ is the related continuous earnings variable EPS_{it}, ΔEPS_{it}, or UE_EPS_{it}

Variable	Predicted Sign	(1) Profit	(2) Profit	(3) Incr	(4) Incr	(5) Surp	(6) Surp	(7) Agg	(8) High	(9) LOW	(10) High_low
Profit	-	-0.252	-0.199					-0.048	-0.193	-0.008	-0.185
		(0.000)	(0.003)					(0.241)	(0.039)	(0.449)	(0.073)
EPS	-	-0.092	-2.233					-1.88	0.377	1.185	-0.808
		(0.398)	(0.000)					(0.000)	(0.238)	(0.063)	(0.195)
Profit* EPS	?		3.437					3.328	-0.078	-0.476	0.398
			(0.000)					(0.000)	(0.919)	(0.566)	(0.725)
Incr	-			-0.040	0.023			-0.025	0.01	0.013	-0.003
				(0.094)	(0.217)			(0.202)	(0.561)	(0.679)	(0.483)
ΔEPS	-			-0.296	-2.611			-2.085	-2.048	-1.866	-0.182
				(0.103)	(0.000)			(0.000)	(0.000)	(0.001)	(0.404)
Incr*ΔEPS	?				3.225			2.556	2.213	1.391	0.822
					(0.000)			(0.000)	(0.000)	(0.051)	(0.343)
Surp	-					-0.083	-0.080	-0.081	-0.094	-0.026	-0.068
						(0.002)	(0.002)	(0.002)	(0.052)	(0.110)	(0.134)
UE_EPS	-					-1.251	-2.137	0.763	-0.276	-1.207	0.931
						(0.032)	(0.030)	(0.249)	(0.432)	(0.158)	(0.321)
Surp*UE_EPS	?						2.744	-1.84	-1.717	2.729	-4.446
							(0.079)	(0.167)	(0.392)	(0.070)	(0.075)
Observations		1696	1696	1695	1697	1696	1695	1699	610	1093	1703
Adjusted R-squared		0.7625	0.7691	0.7610	0.7693	0.7610	0.7627	0.7745	0.7921	0.5161	0.8515

*Robust *p*-values are in parentheses. I calculate *p*-values using one-tailed tests for variables with predicted sign and two-tailed tests otherwise. The sample sizes vary across the models because I eliminate potentially influential observations in each regression using Belsley et al.'s (1980) diagnostics. The estimated coefficients for most control variables are not reported. Table 3.2 provides variable definitions.

CHAPTER 6

ALTERNATIVE SPECIFICATION FOR CREDIT RATINGS

In this chapter, I present four sets of analyses to assess the robustness of my findings that use credit ratings to represent a firm's cost of debt. First, I estimate model (1) using an ordered logit model instead of OLS because the dependent variable, $Rating_{it+1}$, is an ordinal rather than a continuous measure. Second, I estimate model (1) (without year dummies) for each year from 1985-2002 and test the statistical significance using the mean and time-series standard errors of the 18 sets of coefficient estimates (Fama and MacBeth 1973) to mitigate concerns about cross-sectional dependences of credit ratings. This analysis also allows me to examine whether the effect of beating earnings benchmarks has changed over time. Third, I use Ohlson's O-score (1980) to separate firms into high and low default risks firms rather than using credit ratings to avoid any possible selection bias induced by partitioning the sample by current year credit ratings, which is highly correlated with the dependent variable (i.e., one-year-ahead credit rating). Fourth, I estimate a credit ratings change model to control for omitted correlated variables and autocorrelation in the errors terms. Each of these additional analyses confirms the inferences from the main results presented in Chapter 5.

6.1 Ordered Logit Model

Panel A of Table 6.1 presents the results of estimating model (1) using an ordered logit model. The inferences are the same when compared to the OLS results in Panel A of Table 5.1 except that the effect of reporting an earnings increase is no longer larger for high default risk firms than for low default firms.

Columns (1)-(3) indicate that consistent with H1, beating any of the three earnings benchmarks is associated with improved one-year-ahead credit ratings ($p < 0.001$, one-tailed test). Columns (4) and (5) indicate that for both the aggregate and the high default risk samples, beating any of the three earnings benchmarks is associated with better one-year ahead ratings even after controlling for the effect of the other two benchmarks ($p \leq 0.001$, one-tailed test). Column (6) suggests for the low default risk sample, only reporting an earnings increase is incrementally associated with a better one-year-ahead rating ($p < 0.001$, one-tailed test). Column (7) indicates that both reporting a profit and a positive earnings surprise have a bigger impact on credit ratings for high default risk firms than for low default risk firms ($p \leq 0.032$, one-tailed test). The coefficients of reporting earnings increase are similar for high default risk firms and for low default risk firms (p=0.621, one-tailed test).

Panel B of Table 6.1 compares the relative importance of each of the three benchmarks in the aggregate and high default risk samples. Results are similar to the OLS results in Panel B of Table 5.1 except that reporting a profit has a stronger effect than reporting an earnings increase in the aggregate sample ($p = 0.017$, one-tailed test). Similarly to the OLS estimates, both reporting profits and reporting earnings increases have a stronger effect on credit ratings than reporting earnings surprise.

6.2 Fama-MacBeth (1973) Estimate

To mitigate the concern over cross-sectional dependencies among credit ratings, I estimate model (1) (without year dummies) for each year from 1985 to 2002 and estimate the statistical significance using the mean and time-series standard errors of the 18 sets of coefficient estimates (Fama and MacBeth 1973). Panel A of Table 6.2 reports the coefficients estimates and tests of H2. Results are similar to those in the pooled analysis in Table 5.1. Beating any of the three earnings benchmarks is associated with an improved credit rating in both the aggregate and high default risk samples after controlling for the other two benchmarks ($p \leq 0.001$, one-tailed test). In the low default risk firms, only reporting an earnings increase is incrementally associated with a better one-year-ahead rating ($p = 0.065$, one-tailed test). Consistent with H2, the impact of beatings earnings benchmarks is stronger in the high default risk firms than in the low default risk firms ($p \leq 0.041$, one-tailed test). Most control variables are in expected signs and statistically significant.

Panel B of Table 6.2 reports the test of H3. Reporting a profit has a stronger impact than reporting an earnings increase in the aggregate sample but not in the high default risk sample ($p = 0.093$ and $p = 0.178$, respectively, one-tailed test). Other inferences are the same as drawn from the pooled results in Table 5.1.

Brown and Caylor (2005) find that the equity effects of beating the three earnings benchmarks change over time. Specifically, they find that in recent years beating analysts' forecasted earnings becomes the most important benchmark in the equity market. I plot the coefficient estimates of the three earnings benchmarks from the annual regression of model (1) in Figure 6.1. In recent years, the effect of reporting earnings increase becomes very similar to that of beating analysts' forecasted earnings. Nonetheless, cross-temporally, reporting a profit continues to be the most important benchmark for credit ratings. In sum, Figure 6.1 indicates that the relative importance of the three earnings benchmarks in the debt market does not follow the same time pattern as in the equity market.

6.3 Using O-Score to Separate the High and Low Default Risk Sample

To test H2, I separate firm year observations into high and low default risk firms by current year credit ratings in both the new bond yield and credit ratings samples. For the credit ratings model, the dependent variable, $Ratings_{it+1}$, is highly associated with current year ratings. To mitigate the selection bias that might be induced by the partition process, I divide the credit ratings by an alternative default risk measure, the O-score from the Ohlson (1980) model 1. Begley, Ming and Watts (1997) find that the original O-score has better prediction power than Altman's Z-score in predicting bankruptcy in recent years and both Dichev (1998) and Griffin and Lemmon (2002) utilize the O-score to proxy for default risk.

I calculate the O-score following Dichev (1998, 1133):

O_{it} = -1.32-0.407 log (total assets_{it}) + 6.03 (total liability_{it}/total assets_{it}) -1.43 (working capital_{it}/total assets_{it}) + 0.076 (current liabilities_{it}/current assets_{it}) - 1.72 (1 if total liabilities_{it} > total assets_{it}, 0 else) - 2.37 (net income_{it}/total assets_{it}) -1.83 (funds from operations_{it}/total liabilities_{it}) + 0.285 (1 if net loss for last two years, else 0) - 0.521 (net income_{it} – net income_{it-1})/(|net income_{it}| + |net income_{it-1}|).

A higher O-score indicates a high probability of bankruptcy. I classify firm-year observations with higher than average sample O-scores as high default risk observations and firm-year observations with lower than average sample O-scores as low default risk observations. Using the alternative default risk measure only affects the test of H2 and H3. However, the calculation of O-score in year *t* requires data in *t-1* and *t-2*, which reduces the aggregate credit ratings sample. To facilitate comparisons with earlier analyses, I present model estimations of the high and low default risk subsamples as well as the aggregate sample.

Table 6.3 presents the results of estimating model (1) for observations with available O-scores. Results are similar to those reported in Table 5.1. Beating any of the three earnings benchmarks alone or after controlling for the other two benchmarks is associated with better one-year-ahead credit ratings in both the aggregate and high default risk samples. The effects of beating earnings benchmarks are stronger for high default risk firms and reporting a profit is the most important benchmark for credit ratings. In sum, results are robust to alternative default risk measures.

6.4 Estimation of Credit Ratings Change Model

In model (1), I include lagged credit ratings as an independent variable to control for omitted correlated variables and autocorrelation of the error term. As an alternative method to mitigate the effects of omitted correlated variables and autocorrelation of bond ratings, I estimate the following changes specification of the ratings model:

$$\begin{aligned} ChgRating_{it} = {} & \alpha_0 + \alpha_1\, Profit_{it} + \alpha_2\, Incr_{it} + \alpha_3\, Surp_{it} + \beta_1 EPS_{it} + \beta_2 \Delta EPS_{it} + \beta_3 UE\_EPS_{it} + \\ & \beta_4\, ChgSalegro_{it} + \beta_5\, ChgBeta_{it} + \beta_6\, ChgStdRet_{it} + \beta_7\, ChgBM_{it} + \beta_8 \\ & ChgSize_{it} + \beta_9\, ChgLev_{it} + \textstyle\sum_t \beta_t\, Year_{it} + \varepsilon_{it} \end{aligned} \tag{3}$$

In model (3), the dependent variable, $ChgRating_{it,}$, equals the difference between $Rating_{it+1}$ and $Rating_{it}$. Each control variable is calculated as a first difference (e.g., $ChgSalegro_{it} = Salegro_{it} - Salegro_{it-1}$) except the three continuous earnings variables. Because I include these three variables to control for the underlying performance constructs associated with each earnings benchmark, I do not alter these variables in the changes specification. If an omitted correlated variable does not change from year *t-1* to *t*, the variable will be cancelled out in the change specification of model (3). If the error term $Error_{it}$ of model (1) is highly correlated with $Error_{it-1}$, then their difference (i.e., the new error term in the change specification) will more likely be independent.

After taking the difference, there are 9,553 firm year observations available for the change analysis: 5,143 observations in the high default risk subsample and 4,599 observations in the low default risk subsample. Table 6.4 presents descriptive statistics for the aggregate sample, as well as subsamples partitioned by default risk. In the aggregate sample, the average credit rating change per year is 0.09. As expected, this data suggests that ratings for most sample firms do not change year to year. The median ratings change of low default risk firms is larger than that of high default risk firms. Consistent with the larger ratings changes, low default risk firms have larger changes in sales growth, size and leverage than high default risk firms. Although low default risk firms have lower beta and smaller standard deviation of returns than high default risk firms, the changes of *Beta* and *StdRet* are similar in both groups.

Table 6.5 presents univariate correlations among all variables for the aggregate sample. I report Spearman correlations above the diagonal and Pearson correlations below the diagonal. I only report correlations that are significant at 10 percent significance levels (two-tailed test). Consistent with H1, all three earnings benchmark indicators are negatively associated with $ChgRating_{it}$, indicating that beating any of the earnings benchmarks in year t is associated with a rating improvement from t to $t+1$. All control variables are significantly correlated with $ChgRating_{it}$ with the expected sign.

Panel A of Table 6.6 presents the results of estimating model (3) using ordinary least squares. Comparing to the "level" specification with lagged credit ratings, results are similar except that the effects of reporting a profit and earnings increase are no longer stronger in the high default risk sample than in the low default risk sample. The results in columns (1)-(3) suggest that consistent with H1, beating any of the three earnings benchmarks is associated with a credit rating improvement in next year ($p \leq 0.012$, one-tailed test). Column (4) indicates that for the aggregate sample, beating any of the three earnings benchmarks is associated with an ratings improvement after controlling for the effect of the other two benchmarks ($p \leq 0.062$, one-tailed test). Columns (5)-(7) of Panel A test whether the effect of beating earnings benchmarks is stronger for high default risk firms than for low default risk firms. Column (5) indicates that beating any of the three earnings benchmarks is incrementally associated with a ratings improvement for high default risk firms ($p \leq 0.025$, one-tailed test). For low default risk firms, column (6) shows that only reporting an earnings increase is incrementally associated with a ratings improvement ($p < 0.001$, one-tailed test). Column (7) reports the difference between the coefficient estimates for high default risk firms and low default risk firms. The results indicate that beating any of the three earnings benchmarks has a bigger impact on credit ratings changes for high default risk firms than for low default risk firms, but only the impact of beating analysts' forecast is statistically significant ($p = 0.087$, one tailed test).

Panel B of Table 6.6 compares the relative importance of each of the three benchmarks in the aggregate and high default risk samples. In contrast to the "level" specification with lagged credit ratings, reporting profit only has a stronger impact than beating analysts' forecast in the high default risk sample ($p = 0.001$, one-tailed test). In both aggregate and high default risk samples, reporting an earnings increase has a stronger effect than beating analysts' earnings forecasts ($p \leq 0.012$, two-tailed test).

The change specification of model (3) mitigates concerns regarding omitted correlated variable that are firm-specific and time-invariant. The change specification, however, does not rule out the possibility that there are still unspecified omitted correlated variables. For example, if the credit ratings of individual firms follow a time trend (note that the overall time trend of credit ratings has been controlled by year indicator variables), then the change specification can still have an unspecified firm-specific variable g_i omitted from model (3) (Wooldridge, 2002, 315-317). To address this possibility, I estimate the following model that includes firm fixed effects:

$$ChgRating_{it} = \alpha_0 + \alpha_1\, Profit_{it} + \alpha_2\, Incr_{it} + \alpha_3\, Surp_{it} + \beta_1 EPS_{it} + \beta_2 \Delta EPS_{it} + \beta_3 UE\_EPS_{it} + \beta_4\, ChgSalegro_{it} + \beta_5\, ChgBeta_{it} + \beta_6\, ChgStdRet_{it} + \beta_7\, ChgBM_{it} + \beta_8\, ChgSize_{it} + \beta_9\, ChgLev_{it} + \sum_t \beta_t\, Year_{it} + g_i + \varepsilon_{it} \quad (4)$$

Table 6.7 reports the fixed effect estimation of model (4). Comparing to Table 6.6 and Table 5.1, columns (1)-(4) indicate that the effect of beating any of the earnings benchmarks is stronger after controlling for firm fixed effects. For example, the coefficient of $Profit_{it}$ is -0.182 in Table 6.7 versus -0.076 when estimating the change specification without controlling for firm fixed effects (Table 6.6) and -0.130 when estimating the "level" specification with lagged credit ratings (Table 5.1). Similarly, the coefficient of $Incr_{it}$ is -0.125 in Table 6.7, comparing to -0.092 in Table 6.6 and -0.093 in Table 5.1.

To compare the effects of beating earnings benchmarks for high and low default risk firms, I estimate model (4) allowing firms in high and low default risks to have separate coefficients. Column (5) and (6) reports the coefficient estimates for high default risk and low default risk firms, respectively. All three benchmark indicators have the expected signs for both groups. For high default risk firms, all three benchmark indicators are significant and for low default risk firms only $Incr_{it}$ is significant ($p \leq 0.008$, one-tailed test). Column (7) compares the coefficients difference between high and low default risk firms. Reporting a profit and beating analysts' earnings forecast have a stronger effect for high default risk than for low default risk firms ($p \leq 0.044$, one-tailed test).

Panel B of Table 6.7 compares the relative importance of each benchmark in the aggregate and high default risk samples. Inferences from this fixed effect estimation are the same as to those from the "level" specification with lagged ratings. Reporting profit has a larger effect than both reporting an earnings increase and beating analysts' forecasts in the high default risk sample.

Overall, the results in Table 6.6 and Table 6.7 indicate that my findings are robust to using a changes or levels specification.

Table 6.1
Ordered Logit Model Estimation for Credit Ratings

$$Rating_{it+1} = \alpha_0 + \alpha_1\, Profit_{it} + \alpha_2\, Incr_{it} + \alpha_3\, Surp_{it} + \beta_1 EPS_{it} + \beta_2 \Delta EPS_{it} + \beta_3 UE\_EPS_{it} + \beta_4\, Rating_{it} + \beta_5\, Salegro_{it} + \beta_6\, Beta_{it} + \beta_7\, StdRet_{it} + \beta_8\, BM_{it} + \beta_9\, Size_{it} + \beta_{10}\, Lev_{it} + \sum_t \beta_t\, Year_{it} + Error_{it}$$

Panel A: Regression Results

Variable	Predicted Sign	(1) Profit	(2) Incr	(3) Surp	(4) Agg	(5) High	(6) Low	(7)[a] High-Low
Profit	-	-0.654 (0.000)			-0.537 (0.000)	-0.572 (0.000)	-0.163 (0.198)	-0.395 (0.032)
Incr	-		-0.414 (0.000)		-0.342 (0.000)	-0.313 (0.000)	-0.338 (0.000)	0.029 (0.621)
Surp	-			-0.169 (0.000)	-0.122 (0.001)	-0.194 (0.000)	-0.030 (0.322)	-0.161 (0.027)
EPS	-	-1.041 (0.001)	-1.69 (0.000)	-2.072 (0.000)	-0.873 (0.003)	-0.677 (0.011)	-4.530 (0.000)	3.824 (0.002)
ΔEPS	-	-0.242 (0.002)	-0.072 (0.196)	-0.238 (0.001)	-0.108 (0.076)	-0.098 (0.074)	0.521 (0.319)	-0.566 (0.604)
UE_EPS	-	-1.321 (0.023)	-1.385 (0.023)	-0.864 (0.104)	-0.982 (0.063)	-0.790 (0.103)	-0.687 (0.324)	-0.175 (0.915)
Rating	+	3.176 (0.000)	3.197 (0.000)	3.178 (0.000)	3.196 (0.000)	2.833 (0.000)	3.839 (0.000)	-0.984 (0.000)
Salegro	-	-0.122 (0.084)	-0.116 (0.113)	-0.131 (0.077)	-0.114 (0.112)	-0.070 (0.184)	-0.419 (0.002)	0.339 (0.035)
Beta	+	0.059 (0.088)	0.063 (0.075)	0.074 (0.043)	0.055 (0.104)	0.067 (0.079)	0.029 (0.380)	0.033 (0.752)
StdRet	+	22.607 (0.000)	24.293 (0.000)	25.04 (0.000)	22.275 (0.000)	24.708 (0.000)	19.27 (0.003)	5.307 (0.492)
BM	+	0.510 (0.000)	0.487 (0.000)	0.524 (0.000)	0.480 (0.000)	0.410 (0.000)	0.669 (0.000)	-0.257 (0.000)
Size	-	-0.103 (0.000)	-0.087 (0.000)	-0.084 (0.000)	-0.104 (0.000)	-0.153 (0.000)	-0.065 (0.012)	-0.087 (0.012)
Lev	+	1.856 (0.000)	1.809 (0.000)	1.894 (0.000)	1.767 (0.000)	1.565 (0.000)	2.51 (0.000)	-0.898 (0.006)
Observations		10430	10430	10430	10430	5831	4599	10430

Robust *p*-values are in parentheses. I calculate *p*-values using one-tailed tests unless noted otherwise. The estimated coefficients for the constant term and year dummy variables are not reported. Table 3.2 provides variable definitions.

[a] Column (7) reports the difference in coefficients between the high default risk subsample and the low default risk subsample. The *p-values* are based on one-tailed tests for *Profit, Incr,* and *Surp* and two-tailed tests for all other variables.

Table 6.1 (Continued)
Panel B: Testing the Relative Importance of Beating Each Benchmark

	H0: *Profit* ≥ *Incr* *Difference** *(p-value)*	H0: *Profit* ≥ *Surp* *Difference** *(p-value)*	H0: *Incr* = *Surp* *Difference** *(p-value)*
Aggregate Sample	-0.195	-0.416	-0.221
	(0.017)	(0.000)	(0.000)
High Default Sample	-0.259	-0.378	-0.119
	(0.006)	(0.000)	(0.071)

*Difference in the earnings benchmark coefficients from columns (4) or (5) of Panel A. I calculate *p-values* using a one-tailed test when comparing *Profit* with *Incr* and *Surp*. I use a two-tailed test when comparing *Incr* and *Surp*.

Table 6.2
Estimation of the Credit Ratings Model Using Fama-MacBeth Method

$$Rating_{it+1} = \alpha_0 + \alpha_1\, Profit_{it} + \alpha_2\, Incr_{it} + \alpha_3\, Surp_{it} + \beta_1 EPS_{it} + \beta_2 \Delta EPS_{it} + \beta_3 UE\_EPS_{it} + \beta_4\, Rating_{it} + \beta_5\, Salegro_{it} + \beta_6\, Beta_{it} + \beta_7\, StdRet_{it} + \beta_8\, BM_{it} + \beta_9\, Size_{it} + \beta_{10}\, Lev_{it} + Error_{it}$$

Panel A: Regression Results

Variable	Predicted Sign	*Aggregate* [a]		*High Default* [a]		*Low Default* [a]		*High-Low* [b]	
		Coef	*p*-Value	Coef	*p*-Value	Coef	*p*-Value	Coef	*p*-Value
Profit	-	-0.180	0.000	-0.195	0.001	-0.033	0.335	-0.162	0.041
Incr	-	-0.114	0.000	-0.136	0.000	-0.055	0.065	-0.081	0.032
Surp	-	-0.040	0.001	-0.070	0.000	-0.012	0.299	-0.059	0.014
EPS	-	-0.381	0.005	-0.406	0.005	-1.241	0.006	0.835	0.083
ΔEPS	-	-0.037	0.356	0.014	0.440	-0.066	0.432	0.080	0.839
UE_EPS	-	-0.219	0.202	-0.141	0.308	-0.573	0.322	0.432	0.732
Rating	+	0.919	0.000	0.892	0.000	0.941	0.000	-0.050	0.000
Salegro	-	-0.040	0.027	-0.026	0.185	-0.150	0.037	0.124	0.149
Beta	+	0.011	0.317	-0.011	0.365	0.007	0.443	-0.018	0.749
StdRet	+	8.552	0.000	11.029	0.000	8.353	0.004	2.676	0.409
BM	+	0.182	0.000	0.158	0.000	0.211	0.000	-0.053	0.106
Size	-	-0.044	0.000	-0.059	0.000	-0.026	0.003	-0.032	0.047
Lev	+	0.735	0.000	0.721	0.000	0.720	0.000	0.001	0.996
R-Square		0.971	0.000	0.909	0.000	0.923	0.000	-0.014	0.071

[a] I estimate model (1) each year (without year dummies) from 1985 to 2002. The "Coef" column reports the mean coefficient estimates from these 18 sets annual regressions. The "*p*-Value" column reports the *p*-value for the t-statistics of the mean coefficient estimates. With the exception of R-Square, all *p*-values are one-tailed.

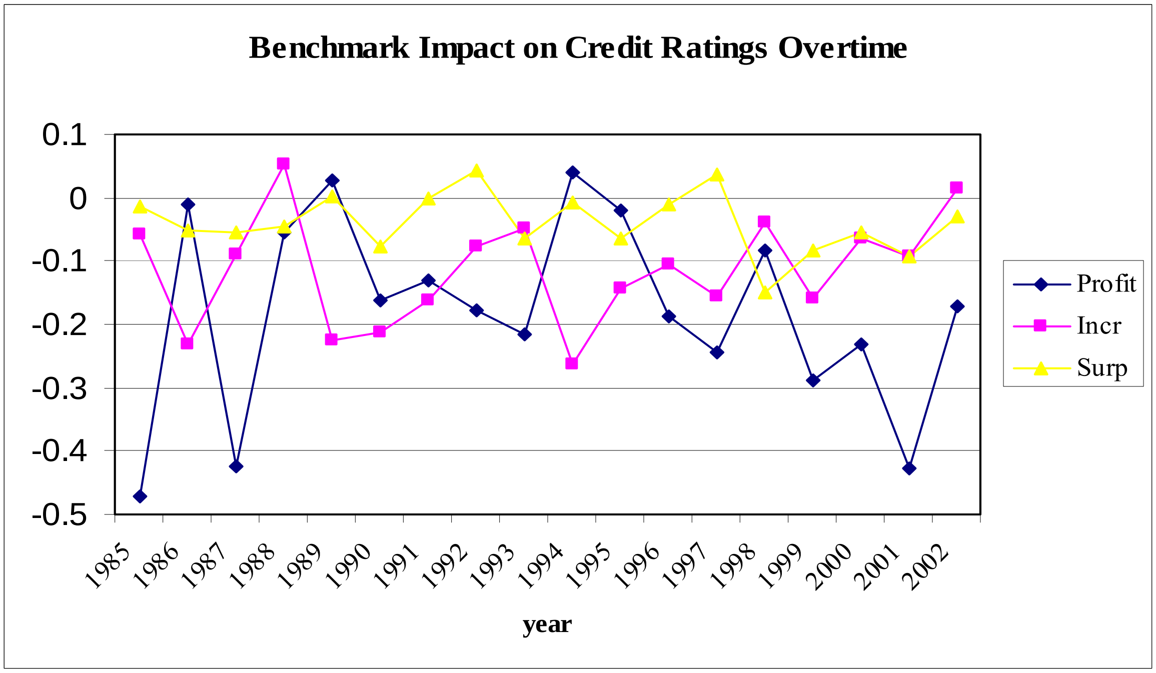
[b] "Coef" represents the differences in the mean values of the coefficients estimates in high and low default risks samples. The *p-values* are based on one-tailed tests for *Profit, Incr,* and *Surp* and two-tailed tests for all other variables.

Panel B: Testing the Relative Importance of Beating Each Benchmark

	H0: *Profit ≥ Incr* *Difference** *(p-value)*	H0: *Profit ≥ Surp* *Difference** *(p-value)*	H0: *Incr = Surp* *Difference** *(p-value)*
Aggregate Sample	-0.065	-0.140	-0.075
	(0.093)	(0.001)	(0.009)
High Default Sample	-0.059	-0.124	-0.065
	(0.178)	(0.013)	(0.034)

*Difference in the earnings benchmark coefficients from the "Aggregate" and "High Default" columns from Panel A. I calculate *p-values* using a one-tailed test when comparing *Profit* with *Incr* and *Surp*. I use a two-tailed test when comparing *Incr* and *Surp*.

Figure 6.1



The graph is based on the coefficient estimates of benchmark indicators from model (1) (without year dummies) from 1985 to 2002 for the aggregate sample. The Y-axis represents the coefficient estimates and the X-axis represents the year indicator.

Table 6.3
Partitioning High and Low Default Risk Firms by the O-Score

$$Rating_{it+1} = \alpha_0 + \alpha_1\, Profit_{it} + \alpha_2\, Incr_{it} + \alpha_3\, Surp_{it} + \beta_1 EPS_{it} + \beta_2 \Delta EPS_{it} + \beta_3 UE\_EPS_{it} + \beta_4\, Rating_{it} + \beta_5\, Salegro_{it} + \beta_6\, Beta_{it} + \beta_7\, StdRet_{it} + \beta_8\, BM_{it} + \beta_9\, Size_{it} + \beta_{10}\, Lev_{it} + \sum_t \beta_t\, Year_{it} + Error_{it}$$

Panel A: Regression Results

Variable	Predicted Sign	(1) Profit	(2) Incr	(3) Surp	(4) Agg	(5)[a] High	(6)[a] Low	(7)[b] High-Low
Profit	-	-0.131			-0.100	-0.113	-0.043	-0.070
		(0.000)			(0.000)	(0.000)	(0.141)	(0.069)
Incr	-		-0.093		-0.080	-0.072	-0.040	-0.031
			(0.000)		(0.000)	(0.000)	(0.005)	(0.083)
Surp	-			-0.034	-0.027	-0.039	-0.009	-0.030
				(0.001)	(0.007)	(0.011)	(0.277)	(0.088)
EPS	-	-0.270	-0.388	-0.465	-0.227	-0.256	-0.625	0.369
		(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.083)
ΔEPS	-	-0.054	-0.022	-0.055	-0.036	-0.027	-0.211	0.183
		(0.007)	(0.147)	(0.004)	(0.031)	(0.099)	(0.052)	(0.163)
UE_EPS	-	-0.393	-0.532	-0.245	-0.168	-0.221	-0.151	-0.070
		(0.005)	(0.000)	(0.061)	(0.124)	(0.138)	(0.290)	(0.836)
Rating	+	0.935	0.938	0.938	0.936	0.927	0.925	0.002
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.740)
Salegro	-	-0.051	-0.049	-0.049	-0.048	-0.027	-0.028	0.000
		(0.000)	(0.000)	(0.000)	(0.000)	(0.057)	(0.138)	(0.989)
Beta	+	0.029	0.030	0.036	0.027	0.031	0.026	0.005
		(0.006)	(0.006)	(0.001)	(0.011)	(0.028)	(0.066)	(0.839)
StdRet	+	4.028	4.357	4.398	4.056	4.203	7.150	-2.948
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.052)
BM	+	0.144	0.134	0.144	0.134	0.123	0.187	-0.063
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Size	-	-0.043	-0.042	-0.040	-0.044	-0.038	-0.056	0.018
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.111)
Lev	+	0.583	0.573	0.601	0.552	0.423	0.507	-0.084
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.391)
Observations		9292	9286	9288	9289	4606	4697	9303
Adjusted R-squared		0.9832	0.9833	0.9832	0.9833	0.9726	0.9824	0.9831

Robust *p*-values are in parentheses. I calculate *p*-values using one-tailed tests unless noted otherwise. The sample sizes vary across the models because I eliminate potentially influential observations in each regression using Belsley et al.'s (1980) diagnostics. The estimated coefficients for the constant term and year dummy variables are not reported. Table 3.2 provides variable definitions.

[a] I group firm-year observations with higher than sample average O-scores into high default risk sample and firm-year observations with lower than sample average O-score into low default risk sample. The calculation of O-score follows Dichev (1998, 1133).
[b] Column (7) reports the difference in coefficients between the high default risk subsample and the low default risk subsample. The *p-values* are based on one-tailed tests for *Profit, Incr,* and *Surp* and two-tailed tests for all other variables.

Table 6.3 (Continued)
Panel B: Testing the Relative Importance of Beating Each Benchmark

	H0: *Profit* ≥ *Incr* *Difference** *(p-value)*	H0: *Profit* ≥ *Surp* *Difference** *(p-value)*	H0: *Incr* = *Surp* *Difference** *(p-value)*
Aggregate Sample	-0.017	-0.072	-0.055
	(0.224)	(0.001)	(0.000)
High Default Sample	-0.040	-0.089	-0.049
	(0.088)	(0.001)	(0.023)

*Difference in the earnings benchmark coefficients from columns (4) or (5) of Panel A. I calculate *p-values* using a one-tailed test when comparing *Profit* with *Incr* and *Surp*. I use a two-tailed test when comparing *Incr* and *Surp*.

Table 6.4
Descriptive Statistics of the Credit Ratings Change Sample[a]

Variable	Significant Difference[b]	Mean	Median	Std Dev	Lower Quartile	Upper Quartile
ChgRating						
Aggregate		0.0880	0.0000	0.6823	0.0000	0.0000
High Default	HD < LD	0.0533	0.0000	0.7439	0.0000	0.0000
Low Default		0.1286	0.0000	0.6002	0.0000	0.0000
Profit						
Aggregate		0.8323	1.0000	0.3736	1.0000	1.0000
High Default	HD < LD	0.7373	1.0000	0.4401	0.0000	1.0000
Low Default		0.9431	1.0000	0.2317	1.0000	1.0000
Incr						
Aggregate		0.6017	1.0000	0.4896	0.0000	1.0000
High Default	HD < LD	0.5604	1.0000	0.4964	0.0000	1.0000
Low Default		0.6499	1.0000	0.4771	0.0000	1.0000
Surp						
Aggregate		0.6234	1.0000	0.4846	0.0000	1.0000
High Default	HD < LD	0.6131	1.0000	0.4871	0.0000	1.0000
Low Default		0.6354	1.0000	0.4814	0.0000	1.0000
EPS						
Aggregate		0.0361	0.0544	0.1282	0.0222	0.0806
High Default	HD < LD	0.0175	0.0463	0.1672	-0.0066	0.0836
Low Default		0.0579	0.0592	0.0465	0.0388	0.0783
ΔEPS						
Aggregate		0.0026	0.0060	0.1490	-0.0206	0.0236
High Default	HD ≈ LD	0.0023	0.0064	0.1971	-0.0380	0.0374
Low Default		0.0030	0.0058	0.0529	-0.0088	0.0159
UE_EPS						
Aggregate		-0.0021	0.0003	0.0430	-0.0016	0.0025
High Default	HD ≈ LD	-0.0038	0.0004	0.0564	-0.0029	0.0036
Low Default		-0.0002	0.0003	0.0170	-0.0008	0.0017
ChgSalegro						
Aggregate		-0.1021	-0.0127	5.9711	-0.1228	0.0792
High Default	HD < LD	-0.1820	-0.0206	8.1319	-0.1753	0.0970
Low Default		-0.0088	-0.0080	0.3272	-0.0843	0.0635
ChgBeta						
Aggregate		-0.0194	-0.0115	0.2599	-0.1271	0.0885
High Default	HD ≈ LD	-0.0217	-0.0156	0.3133	-0.1639	0.1157
Low Default		-0.0166	-0.0079	0.1786	-0.0954	0.0673
ChgStdRet						
Aggregate		0.0008	0.0004	0.0081	-0.0032	0.0043
High Default	HD ≈ LD	0.0011	0.0004	0.0097	-0.0040	0.0052
Low Default		0.0005	0.0004	0.0055	-0.0026	0.0036
ChgBM						
Aggregate		0.0391	0.0129	0.4693	-0.1988	0.2499
High Default	HD > LD	0.0818	0.0516	0.5539	-0.2072	0.3408
Low Default		-0.0106	-0.0167	0.3389	-0.1913	0.1561

Table 6.4 (Continued)
Descriptive Statistics of the Credit Ratings Change Sample[a]

Variable	Significant Difference*	Mean	Median	Std Dev	Lower Quartile	Upper Quartile
ChgSize						
Aggregate		0.1053	0.0655	0.2363	-0.0057	0.1616
High Default	HD < LD	0.1149	0.0584	0.2816	-0.0223	0.1875
Low Default		0.0941	0.0706	0.1680	0.0134	0.1460
ChgLev						
Aggregate		0.0082	-0.0024	0.0824	-0.0285	0.0335
High Default	HD < LD	0.0100	-0.0029	0.0996	-0.0366	0.0441
Low Default		0.0062	-0.0020	0.0562	-0.0205	0.0266

[a] The aggregate sample consists of 9,553 firm year observations from 1985 to 2002. The high default risk subsample consists of 5,143 firm year observations whose S&P senior debt ratings are BBB or below. The low default risk subsample consists of 4,410 firm year observations whose S&P senior debt ratings are BBB+ or above.

[b] Significant differences between the high default risk and low default risk subsamples are based on z-statistics of Wilcoxon Rank Sum Tests of medians (p< 0.10).

$ChgRating_{it}$ = firm *i*'s S&P senior debt ratings from year *t* to *t+1*. Standard & Poor rates a firm's debt from AAA (indicating a strong capacity to pay interest and repay principal) to D (indicating actual default). I translate the ratings letters into ratings numbers, with a smaller number indicating a higher rating. Table 3.1 provides a complete conversion table.

$Profit_{it}$ = one if firm *i*'s basic earnings per share before extraordinary items is greater than or equal to zero in year t, and zero otherwise;

$Incr_{it}$ = one if firm *i*'s earnings per share before extraordinary items in year t is greater than or equal to that of year t-1, and zero otherwise;

$Surp_{it}$ = one if firm *i*'s earnings per share equals or exceeds the single most recent analyst forecast in year t, and zero otherwise;

EPS_{it} = firm *i*'s earnings per share excluding extraordinary items in year t divided by its stock price at the end of year t-1.

ΔEPS_{it} = firm *i*'s earnings per share before extraordinary items in year t minus its earnings per share in year t-1 and divided by its stock price at the end of year t-1.

UE_EPS_{it} = firm *i*'s actual earnings per share minus the single most recent analyst forecast for year t, divided by its stock price at the end of year t-1. I collect both the actual earnings per share and the most recent analyst earnings forecast from the I/B/E/S detail history file.

$ChgSalegro_{it}$ = the change of *$Salegro_{it}$* from year *t* to *t+1*. *Salegro is the* annual percentage change in sales for firm *i*.

$ChgBeta_{it}$ = the change of *Beta* from year *t* to *t+1*. *Beta* is the beta coefficient calculated from the value-weighted CAPM model for firm *i* using monthly returns during the 5 years preceding year *t*.

$ChgStdRet_{it}$ = the change of *StdRet* from year *t* to *t+1*. *StdRet* is the standard deviation of daily stock return during year *t* or *t+1* for firm *i*.

$ChgBM_{it}$, = the change of *BM* from year *t* to *t+1*. *BM* is the log of firm *i*'s book value of equity divided by its market value of equity.

$ChgSize_{it}$ = the change of *Size* from year *t* to *t+1*. *Size* is the natural log of firm *i*'s total assets at the end of year *t*.

$ChgLev_{it}$ = the change of *Lev* from year *t* to *t+1*. *Lev* firm *i*'s long-term debt divided by total assets.

Table 6.5
Significant Correlations for the Credit Ratings Change Sample

	ChgRating	Profit	Incr	Surp	EPS	ΔEPS	UE_EPS	ChgSalegro	Chgbeta	ChgStdRet	ChgBM	ChgSize	ChgLev
ChgRating		-0.172	-0.197	-0.067	-0.199	-0.203	-0.073	-0.078	0.032	0.100	0.141	-0.101	0.142
Profit	-0.177		0.363	0.149	0.647	0.385	0.134	0.112	-0.055	-0.103	-0.005	0.245	-0.139
Incr	-0.190	0.363		0.110	0.474	0.848	0.100	0.225	-0.025	-0.067	-0.108	0.151	-0.187
Surp	-0.069	0.149	0.110		0.139	0.122	0.839	0.042	-0.025	-0.024	-0.017	0.056	-0.038
EPS	-0.153	0.608	0.333	0.135		0.530	0.185	0.125	-0.045	-0.125	-0.139	0.209	-0.167
ΔEPS	-0.131	0.291	0.423	0.076	0.569		0.148	0.247	-0.030	-0.087	-0.114	0.111	-0.221
UE_EPS	-0.070	0.154	0.081	0.288	0.224	0.107		0.052		-0.026	-0.039	0.032	-0.050
ChgSalegro											-0.099	0.238	
ChgBeta	0.023	-0.060	-0.026	-0.023	-0.037	-0.014	0.016			0.059	0.063	0.030	0.021
ChgStdRet	0.109	-0.141	-0.080	-0.035	-0.167	-0.098	-0.060		0.080		0.113	0.022	0.044
ChgBM	0.139	-0.019	-0.097		0.041		0.027		0.073	0.171		0.102	-0.035
ChgSize	-0.064	0.148	0.080	0.057	0.189	0.060	0.060	-0.032	0.040	0.022	0.148		0.167
ChgLev	0.100	-0.091	-0.138		-0.099	-0.107		-0.026	0.042		-0.088	0.305	

The change of credit rating sample consists of 9,553 firm year observations from 1985 to 2002.
Spearman (Pearson) correlations are above (below) the diagonal.
All correlations in the table are significant at $p < .10$ (two-tailed) significance levels.
Table 6.4 provides variable definitions.

Table 6.6
Regression of Credit Ratings Change on the Three Earnings Benchmarks

$$ChgRating_{it} = \alpha_0 + \alpha_1 Profit_{it} + \alpha_2 Incr_{it} + \alpha_3 Surp_{it} + \beta_1 EPS_{it} + \beta_2 \Delta EPS_{it} + \beta_3 UE\_EPS_{it} + \beta_4 ChgSalegro_{it} + \beta_5 ChgBeta_{it} + \beta_6 ChgStdRet_{it} + \beta_7 ChgBM_{it} + \beta_8 ChgSize_{it} + \beta_9 ChgLev_{it} + \sum_t \beta_t Year_{it} + \varepsilon_{it}$$

Panel A: Regression Results

Variable	Predicted Sign	(1) Profit	(2) Incr	(3) Surp	(4) Agg	(5) High	(6) Low	(7)[a] High-Low
Profit	-	-0.076			-0.045	-0.107	-0.036	-0.071
		(0.000)			(0.008)	(0.000)	(0.194)	(0.066)
Incr	-		-0.092		-0.085	-0.100	-0.076	-0.024
			(0.000)		(0.000)	(0.000)	(0.000)	(0.181)
Surp	-			-0.026	-0.018	-0.036	0.003	-0.039
				(0.012)	(0.062)	(0.025)	(0.586)	(0.043)
EPS	-	-0.073	-0.165	-0.207	-0.085	-0.117	-0.279	0.163
		(0.162)	(0.005)	(0.001)	(0.121)	(0.067)	(0.155)	(0.569)
ΔEPS	-	-0.204	-0.088	-0.194	-0.112	-0.035	-0.16	0.125
		(0.001)	(0.084)	(0.001)	(0.040)	(0.298)	(0.300)	(0.687)
UE_EPS	-	-0.355	-0.371	-0.276	-0.293	-0.31	-0.287	-0.023
		(0.006)	(0.005)	(0.028)	(0.021)	(0.035)	(0.132)	(0.941)
ChgSalegro	-	-0.000	-0.000	-0.000	-0.000	-0.000	0.020	-0.020
		(0.074)	(0.070)	(0.054)	(0.066)	(0.012)	(0.126)	(0.244)
ChgBeta	+	0.015	0.023	0.021	0.017	-0.01	0.069	-0.078
		(0.245)	(0.136)	(0.165)	(0.217)	(0.653)	(0.039)	(0.089)
ChgStdRet	+	2.53	2.663	2.384	2.59	2.869	0.153	2.717
		(0.002)	(0.001)	(0.003)	(0.002)	(0.003)	(0.461)	(0.157)
ChgBM	+	0.154	0.139	0.15	0.142	0.163	0.137	0.026
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.351)
ChgSize	-	-0.240	-0.225	-0.24	-0.226	-0.189	-0.224	0.035
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.546)
ChgLev	+	0.913	0.850	0.911	0.847	0.944	0.559	0.385
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.030)
Observations		9129	9123	9123	9129	4918	4212	9130
Adjusted R-squared		0.0680	0.0703	0.0648	0.0729	0.1098	0.0413	0.0949

Robust *p*-values are in parentheses. I calculate *p*-values using one-tailed tests unless noted otherwise. The sample sizes vary across the models because I eliminate potentially influential observations in each regression using Belsley et al.'s (1980) diagnostics. The estimated coefficients for the constant term and year dummy variables are not reported.

[a] Column (7) reports the difference in coefficients between the high default risk subsample and the low default risk subsample. The *p-values* are based on one-tailed tests for *Profit, Incr,* and *Surp* and two-tailed tests for all other variables.

Table 6.6 (Continued)
Panel B: Testing the Relative Importance of Beating Each Benchmark

	H0: *Profit* ≥ *Incr* *Difference** *(p-value)*	H0: *Profit* ≥ *Surp* *Difference** *(p-value)*	H0: *Incr* = *Surp* *Difference** *(p-value)*
Aggregate Sample	0.039	-0.027	-0.067
	(0.951)	(0.111)	(0.000)
High Default Sample	-0.007	-0.071	-0.064
	(0.409)	(0.008)	(0.012)

*Difference in the earnings benchmark coefficients from columns (4) or (5) of Panel A. I calculate *p-values* using a one-tailed test when comparing *Profit* with *Incr* and *Surp*. I use a two-tailed test when comparing *Incr* and *Surp*.

Table 6.7
Regression of Credit Ratings Change on the Three Earnings Benchmarks after Controlling for Fixed Effect

$$ChgRating_{it} = \alpha_0 + \alpha_1\,Profit_{it} + \alpha_2\,Incr_{it} + \alpha_3\,Surp_{it} + \beta_1 EPS_{it} + \beta_2 \Delta EPS_{it} + \beta_3 UE\_EPS_{it} + \beta_4\,ChgSalegro_{it} + \beta_5\,ChgBeta_{it} + \beta_6\,ChgStdRet_{it} + \beta_7\,ChgBM_{it} + \beta_8\,ChgSize_{it} + \beta_9\,ChgLev_{it} + \sum_t \beta_t\,Year_{it} + g_i + \varepsilon_{it}$$

Panel A: Results

Variable	Predicted Sign	(1) Profit	(2) Incr	(3) Surp	(4) Agg	(5) High	(6) Low	(7)[a] High-Low
Profit	-	-0.182			-0.144	-0.189	-0.030	-0.159
		(0.000)			(0.000)	(0.000)	(0.302)	(0.008)
Incr	-		-0.125		-0.106	-0.107	-0.070	-0.037
			(0.000)		(0.000)	(0.000)	(0.004)	(0.147)
Surp	-			-0.038	-0.029	-0.054	-0.000	-0.054
				(0.008)	(0.035)	(0.008)	(0.494)	(0.044)
EPS	-	-0.228	-0.455	-0.485	-0.246	-0.197	-1.513	1.316
		(0.009)	(0.000)	(0.000)	(0.006)	(0.025)	(0.000)	(0.001)
ΔEPS	-	-0.185	-0.039	-0.178	-0.066	-0.039	0.148	-0.187
		(0.002)	(0.273)	(0.002)	(0.155)	(0.282)	(0.706)	(0.507)
UE_EPS	-	-0.547	-0.541	-0.461	-0.437	-0.373	-0.462	0.089
		(0.002)	(0.002)	(0.008)	(0.011)	(0.030)	(0.233)	(0.893)
ChgSalegro	-	-0.000	-0.000	-0.000	-0.001	-0.000	0.004	-0.004
		(0.385)	(0.415)	(0.426)	(0.368)	(0.371)	(0.548)	(0.891)
ChgBeta	+	-0.032	-0.031	-0.034	-0.031	-0.043	0.044	-0.087
		(0.143)	(0.153)	(0.128)	(0.152)	(0.102)	(0.769)	(0.205)
ChgStdRet	+	2.936	3.169	3.289	2.847	2.730	3.173	-0.442
		(0.003)	(0.001)	(0.001)	(0.004)	(0.011)	(0.078)	(0.861)
ChgBM	+	0.225	0.210	0.225	0.210	0.205	0.185	0.020
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.603)
ChgSize	-	-0.105	-0.109	-0.127	-0.090	-0.077	-0.099	0.021
		(0.002)	(0.002)	(0.000)	(0.008)	(0.040)	(0.078)	(0.795)
ChgLev	+	0.697	0.635	0.726	0.624	0.721	0.305	0.415
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.051)	(0.057)
Observations		9553	9553	9553	9553		9553	
Unique Firms		1626	1626	1626	1626		1626	
Adjusted R-squared		0.0716	0.0731	0.0672	0.0766		0.1064	

Robust *p*-values are in parentheses. I calculate *p*-values using one-tailed tests unless noted otherwise. The estimated coefficients for the constant term and year dummy variables are not reported.
[a] Column (7) reports the difference in coefficient estimates in Column (5) and Column (6). The *p-values* are based on one-tailed tests for *Profit, Incr,* and *Surp* and two-tailed tests for all other variables.

Table 6.7 (Continued)
Panel B: Testing the Relative Importance of Beating Each Benchmark

	H0: *Profit* ≥ *Incr* *Difference** *(p-value)*	H0: *Profit* ≥ *Surp* *Difference** *(p-value)*	H0: *Incr* = *Surp* *Difference** *(p-value)*
Aggregate Sample	-0.038	-0.115	-0.077
	(0.143)	(0.000)	(0.001)
High Default Sample	-0.082	-0.134	-0.053
	(0.037)	(0.001)	(0.061)

*Difference in the earnings benchmark coefficients from columns (4) or (5) of Panel A. I calculate *p-values* using a one-tailed test when comparing *Profit* with *Incr* and *Surp*. I use a two-tailed test when comparing *Incr* and *Surp*.

CHAPTER 7

CONCLUSIONS

This study investigates whether beating earnings benchmarks reduces a firm's cost of debt. I measure a firm's cost of debt using firm credit ratings and initial bond yield spread. I find that firms that beat earnings benchmarks have better one-year-ahead credit ratings and smaller initial bond yield spread. In addition, the effect of beating earnings benchmarks generally is much stronger for firms with high default risk than for firms with low default risk. Finally, I find that the relative importance of the three earnings benchmarks is different in the debt market than in the equity market. Unlike the equity market, beating the profit benchmark generally has the largest impact on a firm's cost of debt.

This study makes three contributions to the literature. First, this study provides new insights into firms' potential motivation for beating earnings benchmarks by identifying an additional economically significant benefit of beating benchmarks. Second, my findings confirm Burgstahler and Dichev (1997) and Degeorge et al. (1999)'s conjecture that creditors may use earnings benchmarks to make decisions. Although bondholders primarily consist of institutional investors and credit rating agencies arguably have access to private firm information, my evidence suggests that both groups rely on earnings benchmarks to evaluate firm performance. The results are consistent with Bhojraj and Swaminathan (2004), who find that bond investors are *no more* sophisticated than equity investors in pricing accruals.

Third, I quantify the benefits of beating earnings benchmarks in the bond market and provide evidence that the relative importance of earnings benchmarks differs across equity and bond markets. Among other implications, this study provides evidence that the importance of a specific earnings benchmark may differ based upon the firm’s financing alternatives (equity vs. debt).

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