Do Security Analysts Discipline Credit Rating Agencies?

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Abstract

Credit ratings of corporations are biased but the forces driving this bias are unclear. We argue it would be difficult for rating agencies to issue high grades for a firm's debt when there is lots of objective equity analyst reports about the firm's earnings which are informative about a firm's default. We find that an exogenous drop in analyst coverage leads to greater optimism-bias in ratings, especially for firms with little bond analyst coverage and firms that are close to default. This coverage-induced shock leads to less informative ratings about future defaults and downgrades, and more subsequent bond security mispricings.

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Unbiased and accurate credit ratings contribute to efficient corporate debt markets. However, there is considerable evidence that credit ratings issued by analysts at the rating agencies are biased for a variety of reasons. Some of these reasons include revolving doors, that is, credit analysts who go on to work for the companies they rate (Cornaggia, Cornaggia, and Xia 2016), home bias (Cornaggia, Cornaggia, and Israelsen 2020), and political beliefs of the credit analysts (Kempf and Tsoutsoura 2021). The subjectivity of the rating analyst has been shown to affect debt prices (Fracassi, Petry, and Tate 2016).

But credit ratings bias does not occur in a vacuum. Theories suggest that there are likely to be many forces that mediate ratings inflation.[1] For instance, it is generally thought that limited competition among the three credit rating agencies (S&P 500, Moody's, and Fitch) or the rating-shopping system might be one important reason. But the evidence for this rationale is mixed (Becker and Milbourn 2011; Doherty, Kartasheva, and Phillips 2012; Xia 2014).

In this paper, we propose a novel reason having to do with information spillovers from equity markets to credit ratings and debt markets. Corporate debt pricing theory (Merton 1974) tells us that the price of both the debt and the equity claims of a company should be based on the same underlying fundamental asset value. In practice, equity market prices even often lead credit ratings and are crucial to the determination of credit spreads (Ederington and Goh 1998).

Our hypothesis is that it would be difficult for credit rating agencies to issue high grades for a firm's debt when there are lots of objective equity analyst reports about the firm's earnings which would be informative about a firm's distance to default. As a matter of fact,

[1] The competition forces among credit rating agencies and their impact on credit rating optimism are studied theoretically in Bar-Isaac and Shapiro (2011), Bolton, Freixas, and Shapiro (2012), Manso (2013), Mathis, McAndrews, and Rochet (2009), and Skreta and Veldkamp (2011), among others.

the consensus earnings forecast for a firm's equity enters directly into many credit ratings of firm debt, such as the Morningstar credit rating model. If the consensus earnings forecast for a stock is less optimistic or more objective because of greater analyst coverage (as has been shown, for instance, in Hong and Kacperczyk 2010), it is then difficult for credit rating agencies to issue too optimistic forecasts and still be credible. Interestingly then, even though analysts are not directly competing with credit rating agencies, the fact that there is lots of competitive equity analyst coverage, which leads to more objective consensus earnings forecasts, disciplines the credit rating agencies.[2]

We provide evidence for this disciplining spillover of analyst coverage on credit rating agency bias. After all, around one thousand publicly traded firms issue both debt and equity in a given year during our sample period of 1985-2005. Hence, there is substantial scope to compare credit rating outcomes for firms treated with different levels of analyst coverage and disciplining spillover. We expect that the more analyst coverage there is, the more difficult it will be for the credit rating agencies to be optimistically biased.

Our measure of optimism bias is a residual from a regression model of credit ratings notches (ranging from one for highest or AAA rated to 24 for D rated) on a host of standard variables explaining credit spreads of debt such as distance to default: The more positive this residual credit rating is, the more optimistically biased are the ratings. We find in OLS regressions that competition in the form of more analyst coverage is associated with less optimistically biased ratings. These OLS regressions are, of course, severely biased due to omitted variables.

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[2] The disciplining logic echoes the studies of media and news suppression by Gentzkow and Shapiro (2006) and Besley and Prat (2006). There can be multiple channels through which this disciplining spill-over works (see Section II and IV.B.6).

To this end, we use Hong and Kacperczyk (2010)'s quasi-experiment for analyst coverage and calculate a difference-in-differences estimate for a firm's change in credit ratings bias when the treatment firm that is part of a brokerage house merger experiences a decline of one analyst covering that firm. The control firms are those matched firms, which were not covered by both merging brokerage houses. The usual identifying assumption is that the treatment and control groups have the same selection biases when it comes to credit ratings or in terms of being affected by generalized excitement about a firm's prospects.[3]

Our baseline difference-in-differences estimate from this quasi-experiment implies a statistically significant and economically sizeable effect of coverage, or competition, on credit rating bias. For instance, treatment firms experiencing a one-analyst reduction in coverage relative to control firms experience a favorable increase in residual credit ratings in the subsequent year of around 0.422 relative to control firms, which is around 10% of a standard deviation of credit ratings in our sample. This diff-in-diff estimate is thus ten times larger than our OLS estimate.

Our hypothesis also predicts, and which we verify, that this disciplining effect is stronger for firms that have low credit ratings and are near default, when a firm's junk debt trades like equity. To identify that our effects are due to strategic considerations, our hypothesis also suggests that this disciplining effect ought to be stronger for firms without a lot of competition in the credit sector to begin with. Since there are only three major credit rating agencies, we consider bond analyst coverage as a mediating factor where bond analyst coverage plays the role of providing direct disciplining to credit rating agencies. We expect and

[3] The idea of using mergers as an instrument seems superior to the one of using brokerage house closures that has been advocated elsewhere. Brokerage house closure is likely correlated with an overall macroeconomic effect; say, the underperformance of stock market and subsequent closure of the house. The subsequent optimism can be explained by analysts mistakenly expecting the reversal in the market.

find stock coverage to matter much more where there is not a lot of initial bond analyst coverage.

We then show that this coverage-induced shock to credit ratings has real economic effects on two related fronts. The first is that it leads to a rating error. We measure this rating error with the informativeness of credit ratings for downgrades and the default probability of firm debt. Again, if ratings are more optimistically biased, they should have less explanatory power for the default probability of a firm's debt. The latter measure is prominently featured in the work of Becker and Milbourn (2011) on collusion of credit rating agencies and ratings bias. Using our quasi-experiment, we show that informativeness of credit ratings significantly deteriorates for the treated firms compared to the control group.

The second real consequence of this coverage-induced rating shock is that the treated firms experience a decrease in their yields (or increase in price) compared to the treated group. Since this relative decrease is not due to fundamentals, we can construct a trading strategy to short the relatively over-priced bonds of the treated firms and buy the relatively cheap bonds of the control group. We show that this trading strategy yields a cumulative return of around one percent in the subsequent four quarters after the coverage-induced shock to credit ratings. These findings suggest then that many investors are overreacting to the credit rating error and are unable to see through the credit rating biases.

Our paper contributes to the literature on the determinants and consequences of corporate debt credit rating bias literature which we cited above. We identify a novel mechanism whereby credit ratings bias of a firm's debt is disciplined by security analyst coverage of the firm's equity. Our last finding on the mispricing of corporate debt echoes the findings of Kisgen and Strahan (2010) and Ellul, Jotikasthira, and Lundblad (2011) on how credit ratings influence firm cost of capital.

Our paper also offers insights into the ratings bias of overly optimistic ratings of the mortgage subprime credit (CDOs) for contributing greatly to the credit bubble of 2003-2007 and the system's near-collapse in 2008 (Coval, Jurek, and Stafford 2009; Griffin and Tang 2012). Consistent with our findings, there was little disciplining by security analysts for these CDOs since they were all structured finance products. In mortgage CDOs, for instance, the natural competitive force would be security analysts covering housing stocks. Unfortunately, these housing stocks are typically not very large in market capitalization and hence draw little analyst coverage, thereby mitigating any potential spillovers associated with equity market coverage.

Finally, we contribute to the literature that uses exogenous variation in analyst coverage to understand various economic outcomes. In particular, Derrien, Kecskes, and Mansi (2015) study the importance of asymmetric information for debt prices based on the pecking order theory of Myers and Majluf (1984). They find that a drop in analyst coverage leads to an increase in cost of debt and default rates. Less directly related, Derrien and Kecskes (2013) show that the exogenous drop in analyst coverage leads to a decrease in corporate investments and financing. Similarly, Irani and Oesch (2013) show that the exogenous shock to analyst coverage leads to worse financial reporting quality.

Our paper proceeds as follows. We describe the potential mechanisms behind our hypothesis in Section 1. We describe the data in Section 2. We report our results in Section 3. We conclude in Section 4.

1. How Security Analysts' Earnings Forecasts Can Discipline Credit Ratings

We begin with a simple study of Morningstar's credit scoring model to illustrate the channels through which security analysts' earnings forecasts can influence credit ratings. We focus on Morningstar because it is a quantitative model and is transparent in its methodology in contrast to other ratings agencies such as Moody's. But anecdotes from the media suggest that there are not large differences in terms of what these different agencies focus on. Moreover, the quantitative aspect of Morningstar will allow us to consider a calibration exercise below to see if our empirical findings are plausible.

The Morningstar Credit Score is constructed as follows. The higher is the score the worse is the credit rating. Let *DD* denote the distance to default score of a firm, which is roughly its stock price volatility times its leverage ratio. A score of 1 is best and 10 is worst. Let *SS* denote the solvency score of a firm, which is higher with liabilities to assets, higher with interest expense to EBITDA, lower with return on invested capital, and lower with the quick ratio. Again, 1 is best and 10 is worst. Let *BR* denote a firm's business risk score, which loads positively on firm size, negatively on concentration customer base, negatively on management, negatively on dependence on capital markets and cyclicality. *CC* is the cash-flow cushion score, which is cash and free cash flow projected out over debt. The final credit score is constructed with the following formula: $3.5*DD + 3.5*SS + 8*BR + Max(DD,SS,BR)*CC$.

Morningstar notes "…However, the Cash Flow Cushion™ is also subject to analysts' forecasts, which can contain modeling errors." More optimistic projections by security analysts allow for more optimistic projections from credit ratings. We view credit ratings agencies having more wiggle room to inflate ratings when they are covering a stock with more inflated earnings forecasts.[4]

Indeed, there is good reason to favor the strategic bias interpretation given what we know from the work of Hong and Kacperczyk (2010) about how competition among security analysts leads—via the independence rationale and competition—to more objective and less biased earnings forecasts. This competition and independence rationale or whistleblower effect is more likely in stocks with more coverage since with more numbers it becomes harder and harder to bribe everyone and suppress news.

Meredith Whitney, who was an unknown analyst at a lower-tier brokerage house name Oppenheimer, on Citibank on October 31st, 2007 is a case in point. Whitney argued in the report that Citibank might go bankrupt as a result of their subprime mortgage holdings, thus challenging ratings of agencies. Her report is now widely acknowledged as forcing the release of the pent-up bad news regarding financial firms, which had been unreported by credit rating agencies and other analysts.

2. Data

Our data on credit ratings, yields, and bond returns are obtained from two data sources. For the periods 1985-1996 and 2003-2005 the data come from Lehman Brothers Bond Database (LBBD), whereas we use Merrill Lynch Bond Database (MLBD) for the intermediate period of 1997-2002. The combined database provides month-end security-specific information on the universe of bonds for the period of 1985-2005. Our focus is on ratings of publicly traded

[4] Indeed, we have found that adding forecasted earnings scaled by firm book equity to the standard regressions of ratings on distance-to-default significantly improves the R-squared. For instance, a univariate regression of ratings on distance-to-default yields an R-square of 11.07%. Adding forecasted earnings to this regression increase the R-squared to 14.07%.

companies—a subset of issuers included in the bond database—because it is for these companies that we observe analyst coverage and other firm characteristics. In total, our sample includes 2908 unique firms. It is important to note that our sample includes only a subset of public companies because: (1) only some companies issue debt; (2) some firms issue debt, which is not rated by credit rating agencies. Hence, our results may suffer from a potential selection bias. We explore the severity of this bias by inspecting the time-series and cross-sectional variation of inclusion in our sample.

In Table 1, we provide year-by-year summary of the coverage of firms in our restricted sample, relative to the universe of companies in CRSP/COMPUSTAT data. On average, our sample includes about 1000 firms in each year relative to the universe of about 7000 firms. Notably, our sample of firms matches closely the time-series pattern of the number of firms in the universe, which makes us believe that our sample selection is unlikely to be driven by systematic differences in reporting in the data. In fact, to the best of our knowledge, our bond data providers cover the universe of bond issues.

We further explore cross-sectional differences between rated and non-rated firms. To this end, we compare rated and non-rated firms (over time) along several firm characteristics that can potentially interact with the rating quality: asset size, book-to-market ratio, whether the company is part of S&P 500 index, market leverage, volatility, and distance to default, measured as a product of volatility and market leverage. Table 2 presents the results. Rated firms are distinctly different from non-rated firms: On average, they are larger, take more leverage, have lower volatility, and are more likely to be included in S&P 500 index. Hence, it is possible that any standard linear regression model relating credit ratings to analyst coverage would suffer from a potential selection bias. This worry is one of the strong motivating reasons behind our use of the quasi-experiment.

Our data on defaults have been generously provided to us by Edward Altman and include the comprehensive list of any bond-level defaults that occurred during our sample period.

Our data on security analysts come from the Institutional Brokers Estimates System (IBES) database. In our study, we focus on annual earnings forecasts since these types of forecasts are most commonly issued. For each year, we take the most recent forecast of the annual earnings. As a result, for each year, we have one forecast issued by each analyst covering a stock.

Our data on characteristics of U.S. firms come from the Center for Research in Security Prices (CRSP) and COMPUSTAT. From CRSP, we obtain monthly closing stock prices, monthly shares outstanding, daily and monthly stock returns for NYSE, AMEX, and NASDAQ stocks. From COMPUSTAT, we obtain annual information on book value of equity, book value of assets, debt outstanding, cash, and asset tangibility during the same period. To be included in our sample, a firm must have the requisite financial data from both CRSP and COMPUSTAT. We follow other studies in focusing on companies' ordinary shares, that is, companies with CRSP share codes of 10 or 11.

Overall, our data set is a result of a matching process of LBBD, MLBD, Altman's default data, IBES, and CRSP/COMPUSTAT data. This process has taken multiple steps, beginning with a mechanical matching along ticker and gvkey dimensions and ending with manual matches based on company names. External validity with other research studies gives us comfort that the matching has been accurate.

Our main dependent variable is credit rating. In the LBBD/MLBD data, ratings are provided for different bond issues by three rating agencies: Standard and Poor's, Moody's, and Fitch. To obtain an aggregate rating, we first convert each individual rating into a numeric score, ranging from one for the highest rating provided by a given agency to 24 for the lowest rating. As an example, for Standard & Poor's, AAA-rating would be coded as one and D-rating as 24. Since our analysis is conducted at the stock level and ratings are provided at the issue level, we further aggregate each agency's rating in a given year into one individual rating using weights that depend on face values of each individual issue. As a result, for each firm in a given year, we have three individual ratings. In a final step, we obtain one aggregate rating, for each year *t* and firm *i* by calculating the mean rating across the three rating agencies, which we denote by $RATING_{it}$. This is our main dependent variable of interest. In addition, in some tests we consider four other dependent variables, $YIELD_{it}$, $DEFAULT_{it}$, $UPGRADE_{it}$, and $DOWNGRADE_{it}$. Yield is obtained for the bond issue corresponding to the rating and is aggregated in the same way as rating. Default is an indicator variable equal one if any of the bond issues of company *i* defaulted in year *t* and zero, otherwise. Upgrade (Downgrade) is an indicator variable equal one if any of the bond issues of company *i* got upgraded (downgraded) in year *t* and zero, otherwise.

Our main independent variable is $COVERAGE_{it}$, measured by the number of analysts covering stock *i* in year *t*. As in earlier studies, stocks that do not appear in IBES are assumed to have no analyst estimates. We also utilize a number of other independent variables. $ASSETS_t$ is the firm *i*'s book value of assets at the end of year *t*. BM_{it} is firm *i*'s book value divided by its market cap at the end of year *t*. $MOMENTUM_{it}$ is the average monthly return on stock *i* in year *t*. $LEVERAGE_{it}$ is firm *i*'s market value of debt over total assets. $TANGIBILITY_{it}$ is tangible assets over total assets. $VOLATILITY_{it}$ is the variance of daily (simple, raw) returns of stock *i* during year *t*. DD_{it} is the distance to default calculated as a product of leverage and volatility. $CASH_{i,t}$ is the value of cash position in firm *i* at time *t*.

SP500$_{it}$ is an indicator variable equal to one if the stock is included in S&P 500 index and zero otherwise.

The summary statistics for these regressions (time-series averages of cross-sectional means, medians, and standard deviations) are reported in Table 3. The cross-sectional mean (median) analyst coverage of these stocks is about 17.9 (16) analysts and the standard deviation across stocks is about 10.5 analysts. The cross-sectional mean (median) credit rating is 11.9 (12.3) with a standard deviation of around 4.2. The equivalent numbers for yield are 8.54 (8.03) and 4.22, while those for default are 2.09 (0) and 16.78.

3. Results

3.1 OLS regressions of optimism-bias in ratings on analyst coverage

We begin by estimating a pooled OLS regression model of *RATING* on lagged values of *COVERAGE* and a set of standard control variables, which include *LNASSETS, LNBM, MOMENTUM, LEVERAGE, VOLATILITY, DD, CASH,* and *SP500*. We additionally include year and industry-fixed effects, where industry is classified by a 2-digit SIC code. Standard errors are clustered at the firm and year groupings.

The regression results are presented in Table 4. We first present the results for the model without fixed effects: in column 1 with *LNASSETS*, *LNBM*, *MOMENTUM*, *LEVERAGE*, and *TANGIBILITY* as control variables, and additionally with *DD*, *VOLATILITY*, *CASH*, and *SP500*, in column 2. In column 1, the coefficient of *COVERAGE* is 0.048 and is statistically significant at the 1% level of significance. In column 2, the coefficient is 0.050 and it is still statistically significant at the 1% level of significance. So, depending on the controls we use, we find that a decrease in coverage by one analyst leads to a decrease in *RATING* (better credit rating) of anywhere from 0.048 to 0.050. The rating score for a typical stock is about 11.9 with a standard deviation across stocks of about 4.2.

Hence, these estimates obtained from cross-section regressions suggest only a small decrease in rating score of about 1.1 to 1.2 percent as a fraction of the cross-sectional standard deviation of credit rating as we decrease coverage by one analyst, though some are very precisely measured. In column 3, we include industry-fixed effects and in column 4 additionally year-fixed effects. The results using the extended model are reported. Again, there is little difference in the coefficient of *COVERAGE* in terms of its sign and statistical significance, though the coefficients are now significant at the 5% level, but the economic magnitude of the effect drops to about 0.45-0.79 percent drop of the cross-sectional standard deviation of *RATING*.

The other control variables also come in significant in these regressions. Credit rating improves with firms' assets, tangibility, cash, and for firms in the S&P500 index, and it is lower for firms with high leverage and high volatility. The sign on book-to-market ratio and distance to default is ambiguous depending on whether fixed effects are included.

However, as we explained it in the Introduction, all these OLS regressions are difficult to interpret due to omitted variables. For instance, there is the problem of selecting on the left-hand-side variable since we can only estimate an OLS regression model for the subsample of firms which have credit ratings to begin with. If this selection bias is in the direction of us never seeing firms with poor credit ratings since these are so poor that they cannot even get a rating, then these firms might not be covered by analysts either. Hence, low coverage might actually be associated with poor ratings rather than good ratings if we had all firms rather than a selected sample. In this scenario, the OLS regression might be overstating the causal effect of coverage on credit ratings. At the same time, our OLS results might actually be understating this effect if analyst coverage and credit ratings are correlated with generalized excitement about a firm's fundamental prospects which would make firms with coverage also have higher ratings, rather than less as we find.

In the remainder of our analysis, we rely on a quasi-experiment to sort out these endogeneity issues. We use mergers of brokerage houses as our experiment on the premise that mergers typically lead to a reduction in analyst coverage on the stocks that were covered by both the bidder and target firms pre-merger. If a stock is covered by both firms before the merger, they will get rid of at least one of the analysts, usually the target analyst. It is to this experiment that we now turn.

3.2 Evidence from mergers quasi-experiment

Our analysis of the effect of competition on credit ratings utilizes a quasi-experiment involving brokerage house mergers. The outcome of such a process is the reduction in the number of analysts employed in the combined entity compared to the total number of analysts employed in bidder and target entities prior to merger. As a result, the number of analysts covering a stock that was covered by both houses before the merger (our treatment sample) should drop as one of the redundant analysts is let go or reallocated to another stock (or maybe even both are let go) and thus the competition in the treatment sample decreases. The questions then are whether there is a decrease in competition among analysts around merger events and whether this decrease is associated with an economically significant effect on average credit rating.

Our empirical methodology requires that we specify a representative window around the merger events. In choosing the proper estimation window we face a trade-off unlike most other event studies that would have us focus on a very narrow window. As is the case with most event studies, choosing a window which is too long may incorporate information which is not really relevant for the event in consideration. But in our case, choosing too short of a window means we may lose observations since analysts may not issue forecasts on the same date or with the same frequency. We want to keep a long enough window to look at the change in the performance of all analysts before and after the merger.

To this effect, we use a two-year window, with one year of data selected for each pre- and post-event period. Most analysts will typically issue at least one forecast within a twelve-month window. Given that in each of the two windows one analyst could issue more than one forecast we retain only the forecast which has the shortest possible time distance from the merger date. As a result, for every stock we note only two observations–one in each window of the event.[5]

Having chosen this one-year before and one-year after the merger event, one then has to factor in the fact that coverage and the average credit rating may vary from one year to the next one. In other words, to identify how the merger affected coverage in the stocks covered by both houses pre-merger and how the credit ratings in these companies then also changed, one needs to account for the fact that there may be natural changes from year to year in coverage and credit rating for these companies.[6]

A standard approach to deal with these time trends is based on the difference-in-differences (DID) methodology. In this approach, the sample of firms is divided into treatment and control groups. In the context of our paper, the treatment group includes all

[5] In our merger experiment, it is possible that some brokerage houses might terminate their coverage prior to the merger date, in an action that is potentially independent of the merger event itself. Consequently, we would erroneously assign a drop in coverage as one that is due to merger. Since such events are generally close to the merger date it is difficult to establish their pure independence with respect to the merger itself. Hence, we decided to stay with our broader definition of treatment. But we have also examined the sample in which all these cases are excluded, thus erring on the extreme side of conservatism. Our results are qualitatively very similar, and the magnitudes are generally a bit larger (the results are available on request). Hence, one can treat our estimates as a lower bound of the true competitive effect of analyst coverage.

[6] For example, Bolton, Freixas, and Shapiro (2012) and Bar-Isaac and Shapiro (2013) argue theoretically that the ratings optimism is highly pro-cyclical, largely driven by a greater presence of unsophisticated investors and lower reputation costs.

firms that were covered by both brokerage houses before the merger. The control group includes all the remaining companies.

Table 5 presents summary statistics for the treatment sample in the two-year window around the merger. The characteristics of the treatment sample are similar to those reported in Table 3 for the OLS sample. For instance, the coverage is about 15 analysts for the typical stock. The mean rating is 10.56 with a standard deviation of around 4.25. These numbers, along with those of the control variables, are fairly similar across these two samples. This provides comfort that we can then relate the economic effect of competition obtained from our treatment sample to the OLS estimates presented in Table 4.

To capture the effect of change in credit rating due to merger that we consider is to estimate the following regression model:

$$C_i = a + \beta_1 After_i + \beta_2 Affected_i + \beta_3 After_i \times Affected_i + \beta_4 Controls_i + \varepsilon_i \qquad (1)$$

where C is the characteristic which may be subject to merger; in our context it is either *COVERAGE* or *RATING*. *After* is an indicator variable, equal to one for observations after the merger, and zero, otherwise; *Affected* is an indicator variable equal to one if stock i is affected by the merger, and zero, otherwise; *Controls* is a vector of stock-specific covariates affecting C. In this specification, the coefficient of primary interest is β_3, which captures the partial effect to change due to merger. By including additional controls, we account for any systematic differences in stocks, which may affect the partial effect to change due to merger.

We estimate our regression model using a pooled (panel) regression and calculating standard errors by clustering at the merger grouping. This approach addresses the concern that the errors, conditional on the independent variables, are correlated within firm and time groupings (e.g., Moulton 1986). One reason why this may occur is that the rating bias occurring in one company may also naturally arise in another company covered by the same house because they are part of the same merger event with similar bias pressures.[7]

3.2.1 Validity of mergers quasi-experiment in our setting. Notably, in the description of our results we side-step some of the more nuanced aspects of the experiment's validation by drawing upon the study by Hong and Kacperczyk (2010), who examine the validity of this instrument in greater detail in their study of equity analyst forecast bias. We briefly show that the treatment and control groups are not very different in terms of important characteristics, and we do not actually capture the ex-ante differences in various observables. To this end, we report similar DID estimators for other response variables–*LNASSETS*, *LNBM*, *MOMENTUM*, *LEVERAGE*, and *DD*. The results in Table 6 show that none of the important observables are significantly affected by the merger event. These results are comforting as they confirm the validity of our matching procedure. They also confirm that our results are not an outcome of a differential response of ratings to changing firm-level characteristics.

3.2.2 Coverage and credit ratings. The results for the effect on rating using a regression approach outlined in equation (1) are presented in Panel A of Table 7. The first two columns show the results for *COVERAGE* as a dependent variable. In the first column, we report the results with a set of basic controls: *LNASSETS*, *LNBM*, *MOMENTUM*, *LEVERAGE*, *TANGIBILITY*, which in the table are defined as *Controls1*. We also include merger, industry, and year-fixed effects. The coefficient of *AFTER×AFFECTED* is -0.884, which is significant at the 1% level. In column 2, we additionally include *VOLATILITY*, *SP500*, *CASH*, and *DD*, which we shortly define as *Controls2*. The coefficient of interest is -0.938 and the statistical

[7] We have also experimented with clustering along many other dimensions, including firm/year, merger/year and firm. The results we report here produced the most conservative standard errors.

significance level is 1%. The results confirm the premise of our instrument, that is, mergers reduce analyst coverage by approximately one analyst.

In columns 3 and 4, we analyze the effect of the change in competitive pressure for credit ratings. The empirical specifications mirror those in columns (1) and (2) except that we also include *COVERAGE* as an additional control variable.[8] The coefficient of the interaction term in this baseline specification equals -0.355 and it is statistically significant at the 10% level of significance. The coefficient increases slightly, to -0.422, for the extended specification and improves its statistical significance to 5%. The results are also economically significant: The increase in rating optimism resulting from a drop of one analyst is approximately equal to 11 percent of the cross-sectional standard deviation of *RATING* in our sample. So, this means that the estimate of the competitive effect from our natural experiment is approximately ten times as large as that from the OLS estimates. This is a sizeable difference and suggests that the OLS estimates are severely biased downwards.[9]

In sum, Table 6 establishes that nothing changed about the treated firm as measured by fundamentals like leverage or assets. Table 7 then shows that the ratings of the treated firms increased. The conjunction of Tables 6 and 7 then verify that it is not an issue of stale ratings and changing fundamentals. Rather, it is non-changing fundamentals and changing ratings.

3.2.3 Non-parametric analysis. A potential concern with the above estimator is the possibility that the treatment and control groups may be significantly different from each other and thus the partial effect may additionally capture the differences in characteristics of the

[8] In the regression model with rating as dependent variable, coverage is part of *Controls1*.

[9] We have also estimated the same regression model for five largest mergers in our sample and for each of the mergers separately. In both cases, we find results that are broadly consistent with our base-case estimates. In four out of five cases, the coefficient of *RATING* is negative and statistically significant. In the last case, the coefficient is also negative but statistically insignificant. Detailed results are available upon request.

different groups. For example, the average stocks in both groups may differ in terms of their market capitalizations, value characteristics, or past return characteristics. For instance, it might be that companies with good recent returns lead analysts to cover their stocks and to be more optimistic about them. Hence, we want to make sure that past returns of the stocks in the treatment and control samples are similar. We are also worried that higher analyst coverage stocks may simply be different than lower analyst coverage stocks for reasons unrelated to our competition effect. So, we will also want to keep the pre-merger coverage characteristics of our treatment sample similar to those of our control sample.

Our regression model in Table 7 aims to account for such differences by explicitly including the relevant controls in the regression model. But since the controls only account for average differences between treatment and control groups along one individual dimension it is still possible that we do not capture all the nonlinearities in the data.

To account for such systematic differences across the two samples we use the matching technique similar to that used in the context of IPO event studies or characteristic-based asset pricing. In particular, each firm in the treatment sample is matched with its own benchmark portfolio obtained using the sample of firms in the control group. We expect our controls to typically do a better job at capturing our true effect by netting out unobserved heterogeneity.

To construct the benchmark, we first sort companies into tercile portfolios according to their market capitalizations. Next, we sort companies within each size portfolio according to their book-to-market ratios. This sort results in nine different benchmark portfolios. Finally, we sort companies in each of the nine portfolios into tercile portfolios according to their past returns, which results in 27 different benchmark portfolios. Overall, our benchmark includes 27 portfolios.

Using the above benchmark specification, we then construct the benchmark-adjusted *DID* estimator (*BDID*). In particular, for each stock *i* in the treatment sample the partial effect to change due to merger is calculated as the difference between two components:

$$BDID^{i} = (C^{i}_{T,2} - C^{i}_{T,1}) - (BC^{i}_{C,2} - BC^{i}_{C,1}), \tag{2}$$

where the first component is the difference in characteristics of stock *i* in the treatment sample moving from the pre-merger to post-merger period. The second component is the difference in the average characteristics of the benchmark portfolios that are matched to stock *i* along the size/value/momentum dimensions. In general, the results are comparable if we use benchmarks matched along any subset of the characteristics. To assess the average effect for all stocks in the treatment sample, one can then take the average of all individual *BDID*s.

We first verify the premise of our natural experiment by measuring the change in analyst coverage for the treatment sample from the year before the merger to the year after. We expect these stocks to experience a decrease in coverage.

Panel A of Table 8 (column 1) reports the results of this analysis. We present the DID estimator for coverage using our benchmarking technique–size, book-to-market, and momentum matched. We observe a discernible drop in coverage due to merger of around 1.13 analysts using the DID estimator and the level of the drop of between one and two analysts is in line with our expectations. This effect is significant at the 1% level of significance.

We next look at how the credit rating optimism changes for the treatment sample across the mergers. We present the findings in column 2. Using the DID estimator, we find an increase in credit rating optimism of 0.234, the effect that is significant at the 5% level. The effect for rating, though slightly smaller than that we estimated using the regression model, is consistent with our premise that the drop in analyst coverage results in an increase of credit rating optimism. In terms of its economic significance, the effect is approximately six times as large as that obtained from the OLS specification.

Second, we further validate our auxiliary prediction on the degree of competitive pressure by performing a similar nonparametric analysis, this time conditioning on the initial analyst coverage. The results are presented in Panel B of Table 8. We find that the effect is economically and statistically large for cases in which analyst coverage was low or medium to begin with and it is miniscule for the cases where competitive pressure was strong to begin with. The effect is a sizable 1.118 increase in rating optimism for lowest coverage group, a moderate 0.387 for the medium-coverage group, and a negligible 0.084 for the highly covered companies.

Notice that we still get ratings increases that are significant even for treated stocks with between 5 and 20 initial analysts covering. Only for very large initial coverage in excess of 20 do we get negligible effects but even then the coefficient is negative. As such, it appears we have some competition effects even for stocks with coverage less than 20. While our hypothesis cannot really pin down at what level of initial coverage would losing one analyst matter, we can at least look at a percentage loss of analysts as a guide. Clearly, with 5 initial analysts, losing one or 20% is a lot. But even with around 20 analysts covering, losing one or around 5% would still seem to be meaningful. Since most of the stocks in our sample have less than 20 analysts covering, these findings then explain why we get an economically meaningful effect on average.

Overall, we conclude that our results are unlikely to be driven by potential differences between the companies in the treatment and control groups. Nevertheless, we further perform a number of additional tests that provide additional robustness to our experiment.

3.2.4 Addressing selection biases. The nature of our experiment requires that the same company be covered by two merging houses. To ensure that our effects are not merely due to the fact that the selection of the companies to brokerage houses is not random, we re-examine our evidence by focusing on stocks that are covered by one of the merging houses, but not by both. We show in Panel A of Table 9 that the average stock coverage does not change significantly on the event date across these treatment and control groups and the change in the bias is statistically not different from zero. We further apply this setting to validate the quality of our control group. Specifically, in Panel B of Table 9, we show that using stocks covered by only one of the two merging houses as a control group does not change the nature of our results. In fact, the results become slightly stronger than those in our baseline specification.

3.2.5 Conditioning on speculative grade cut. We then explore whether the competition effect is stronger for firms that are closer to default. Given that near default a firm's junk debt trades like equity, we should expect the effect to be more likely to bind for such firms. In other words, near default, both equity analysts and credit rating agencies focus on the same, left-tail distribution of the firms' cash flows. We then explore whether the competition effect is stronger for firms that are closer to default.

We test this hypothesis using the same DID framework as before with a full set of controls, *Controls1*, *Controls2*, and merger, industry, and year-fixed effects. Formally, we split our sample into high-default and low-default observations. We use three measures of distance to default: credit ratings (investment grade vs. junk bonds), naïve distance to default measure of Bharath and Shumway (2008) (below vs. above median), and our previously used measure, that is, the product of firm leverage and its equity return volatility (below 25th percentile vs. above median).

Our results, presented in Table 10, confirm that the competition effect is much stronger for firms that are close to default. The estimates of interest vary a bit with specification: In terms of economic magnitudes, they are the strongest for the *DD* measure of distance to default and the weakest for the samples conditioned on credit ratings. Nonetheless, they are typically at least twice as large for the sample of firms with high-default probability and thus offer a strong support to our baseline hypothesis.

3.2.6 Disciplining mechanisms. There are a few different ways in which it would be difficult for credit rating agencies to issue high grades for a firm's debt when there is lots of objective equity analyst reports about the firm's earnings.

First, even if credit rating analysts take these earnings forecasts at face value and use quantitative credit default models such as in Section II, these forecasts directly go into credit default models and hence credit ratings. Second, more likely, objective external earnings forecasts would make it more difficult for credit rating analysts to exaggerate their ratings along the lines of competition and information suppression models (Gentzkow and Shapiro 2006; Besley and Prat 2006). Third, credit rating analysts might need to rely on access to management to obtain accurate information and hence trade-off bias to boost the precision of their ratings (Lim 2000). To the extent there are more plentiful sources of equity information about firm defaults, this trade-off is less important.

We provide some evidence along the lines of the second mechanism, that our effects are due to competition and that this disciplining effect ought to be stronger for firms without a lot of competition in the credit sector to begin with. Since there are only three major credit rating agencies, we consider bond analyst coverage as a mediating factor where bond analyst coverage plays the role of providing direct disciplining to credit rating agencies. We first verify that this premise is indeed true in an OLS framework. In contrast to stock analyst coverage, there are far fewer bond analysts and data are hard to come by. However, our primary interest is to use bond analyst coverage as a conditioning variable to indicate whether or not there are already enough outlets to keep the rating agencies disciplined. We expect then stock coverage to matter much more where there is not a lot of initial bond analyst coverage, which is indeed what we find.

To this end, we collect reports about U.S. corporate firms from ThomsonOne Fixed Income research report database. The data are for the limited period of 2002–2006. We exclude reports about REITs, financial institutions, such as banks or insurance companies, companies domiciled in non-U.S. countries, macroeconomic variables, and industry indices.

The bond analyst report list includes report number, data, pages, contributor (brokerage firm), analyst (team), subtitle, and title. Unfortunately, the list does not have information about company ticker or CRSP permno, and the company name is embedded in the title and or the subtitle. Hence, we have to extract manually the company names from the title and match them to CRSP permno, which is our main company identifier. In the process of matching, we have cleaned up contributor names and analyst names to make sure different entities are not due to spelling or reporting (lead or team, full name or initial) differences.

In sum, we gather information for about 1000 different companies in our sample. The average bond coverage in the data is approximately 1.7 with a standard deviation of 1.16, as opposed to an average of about 18 for analyst coverage. Ideally, we would like to perform a similar mergers quasi-experiment as before to see if the same results hold up; unfortunately, the universe of bond analysts is small compared to equity analysts and we are unable to calculate our difference-in-differences estimate. Hence, we resort to OLS estimation, similar to our analysis in Table 4.

In additional results, we first verify that bond analyst coverage has some disciplining effect on credit ratings to begin with. We present the results from the estimation of *RATING* on *BOND COVERAGE* and a set of similar controls as in Table 4. The coefficient of *BOND COVERAGE* is positive and statistically significant at the 1% of significance across all the same specification as in Table 4. Overall, we find that an increase in bond analyst coverage is associated with a decrease in credit rating optimism. Although the findings are potentially subject to endogeneity concerns and are obtained for considerably smaller sample, they are suggestive that the competitive effect we document for equity analysts also holds in other information markets.

We then move to our main estimation interest: We expect our disciplining effect using stock analyst coverage to be stronger for firms without a lot of initial bond analyst coverage to begin with. We test this hypothesis using the same DID framework as before with a full set of controls, *Controls1*, *Controls2*, and merger, industry, and year-fixed effects. Formally, we split our sample into high bond analyst coverage (with more than two bond analysts) and low bond analyst coverage (with at most two bond analysts) observations and estimate our DID framework separately for these two sub-samples.

Our results, presented in Table 11, confirm that the disciplining effect from stock analyst coverage is much stronger for firms with few bond analysts' coverage. Indeed, all the disciplining effects from stock analyst coverage are coming from low bond analyst coverage. There are no effects for high bond analyst coverage.

3.3 Alternative explanations

In this section, we provide additional discussion of possible explanations of our findings. First, we discuss the possibility that ratings reflect changes in firms' activities. Second, we explore the possibility of the rating changes driven by corporate managers' responses to an increase in asymmetric information.

3.3.1 Changes in firms' activities. Another way to explain our findings could be through agency costs involving corporate decisions and the viability of debt claims on the firm. Given that prior work has argued that the reduction in analyst coverage leads to a drop in risky R&D investing or repurchasing less shares, both known to lead to risk shifting, these changes should increase the rating agencies' outlook for the firm since these corporate activities have less benefits for debt claimants. While we think this mechanism could explain the increase in credit rating it is less clear why this would make the rating more biased. In particular, contrary to our mechanism, this story would not be able to explain the result that credit ratings become less informative of bond yields upon the drop in analyst coverage.

3.3.2 Changes in information asymmetry. The reduction in analyst coverage leads to an increase in information asymmetry. Based on this argument, one could argue that firm managers could respond to a drop in analyst coverage by providing more optimistic disclosures to the market or privately to credit rating agencies. This communication could persuade agencies to look more favorably. Measuring such communication channel is empirically challenging, which makes it difficult to test this hypothesis directly. However, it seems natural to think that the role of the managers would be rather to reduce any information asymmetry but not to bias information in one direction. Hence, we find this possibility less convincing.

3.4 Economic significance of coverage-induced ratings shock: Informativeness of ratings for defaults and ratings changes

Having established that there is indeed an exogenous coverage-induced shock to credit ratings, we now show that this shock has real economic effects on the informativeness of credit ratings for the default probability of firm debt and credit rating changes more generally. The crux of our analysis thus far is that this coverage-induced shock to credit ratings leads to a rating error. That is, the treated firms after the merger that lost analysts experience a negative shock to coverage and a positive shock to optimism bias in earnings and ratings that is unrelated to the fundamentals of the company. We measure this rating error with the informativeness of credit ratings for downgrades and the default probability of firm debt. Again, if ratings are more optimistically biased, they should have less explanatory power for the default probability of a firm's debt.

But before we consider our quasi-experiment, it is useful to start by estimating the linear probability model with *DEFAULT* as a dependent variable in column 1 of Table 12, and *RATING* as the main independent variable. This provides us a benchmark to consider the quasi-experiment. The coefficient in front of *RATING* is .003 and it is statistically significant. Higher *RATING* rank, which means a more risky firm, has a higher propensity to default as expected. In column 2, we use *UPGRADE* as the dependent variable of interest. Even though the coefficient is going the right way, meaning a higher risky rank means a lower chance of upgrades, it is not statistically significant. This is perhaps the result of there being an asymmetry since firm credit scores are bounded above by AAA of which there are very few in contrast to lower rated firms. Indeed, in column 3, we see that *RATING* does explain downgrades. Higher *RATING* rank means lower quality firms are more likely to be downgraded. These coefficients serve as a baseline for the informativeness of ratings for future defaults or credit rating changes. The bigger are these coefficients the more informative are ratings for future fundamentals.

Since we are interested in the marginal effect of change in coverage we estimate the rating effect conditional on coverage, which is equivalent to including the interaction term

*RATING*COVERAGE* in each of these three columns. The results from the OLS estimation, presented in Table 12, indicate a very weak effect of disciplinary forces in explaining default by ratings. In three out of four specifications, we find the effect that is very small in magnitude and statistically insignificant. But as we explained it in the Introduction, all these OLS regressions are difficult to interpret due to omitted variables.

We then use our quasi-experiment to re-estimate this relationship and show that informativeness of credit ratings significantly deteriorates for the treated firms compared to the control group. Empirically, to assess this effect we can use a similar framework to the one in Table 9. One difference, however, is that we want to observe the power of ratings to explain both dependent variables before and after the merger event. This approach is equivalent to estimating the model of triple differences, in which the coefficient of interest is the one of the product *Rating*Affected*After*.

In Table 13, we present the results from this estimation for *Default* as a dependent variable. In line with our hypothesis, we expect the coefficient of the triple interaction term to be negative, that is, the ability of ratings to explain default should decrease once the stock coverage is exogenously decreased by approximately one analyst. In columns 1-5, we present results from various specifications in which we iteratively introduce additional control variables and several fixed effects. In all the specifications, we consistently observe that the effect is indeed negative and statistically significant.

To get a sense of the economic magnitudes, consider the coefficient in front of *Rating*After* from column 1 is .011, which provides a baseline measure of how informative ratings are for defaults after the merger event. The coefficient in front of the triple interaction is -.008, which means that the informativeness of ratings in the sample after the merger is entirely coming from the control group. The ratings of the treated group have very little information about defaults. The economic magnitudes are quite robust and consistent across all the specifications.

In a similar fashion, we estimate in Table 14 the same model for *Downgrades* in Panel A as the dependent variable and *Upgrades* in Panel B. Again, the prediction is that the drop in coverage should reduce the ability of credit ratings to predict future credit rating changes. We present the results from the estimation in Table 14 Panel A using a similar sequence of controls as before. We find that the coefficient of the triple interaction term is negative and statistically significant for all five different regression models. There are, however, no effects when it comes to *Upgrades* in Panel B. One reason is that there is not much informativeness of ratings for upgrades to begin with from the earlier OLS results. Overall, the results in this section confirm our earlier findings from Table 9 that the reduction in analyst coverage makes credit ratings more optimistic and also less informative. As a robustness check, we present logit regression analogs to Tables 13 and 14 in the Appendix and the results are very similar.

3.5 Economic significance of coverage-induced ratings shock: Trading strategy to exploit resulting bond market inefficiency

The second real consequence of this coverage-induced rating shock is that, assuming investors respond naively to the shock, the treated firms experience a decrease in their yield spreads (or increase in price) compared to the control group. But since this relative decrease is not due to fundamentals, we can construct a trading strategy to short the relatively over-priced bonds of the treated firms and buy the relatively cheap bonds of the control group.

To provide the additional robustness of this fact, we look at the effect on the treated firms relative to controls firms over the same event window. Our sample of control firms includes companies closest to the treated firm in terms of their average bond duration in period of event time zero. We plot average yields for both the treated and control firms in Figure 1.

The results show two effects: First, the control sample exhibits yields which are very close to the yields of treated firms prior to the merger shock. This provides comfort to our analysis. Second, we see a sizable difference in yields following the merger event between the treated and control firms. While the yields on the treated fall immediately following the merger event the response of the control group is visibly different. The difference between the two samples yields about 20-30 basis points difference in average yields over the subsequent course of six-nine months. The gap between the two samples seems to close at the end of month 9.

The results in Figure 1 suggest an investment opportunity for investors in the bond market. Starting from month 1, one could buy bonds of control firms which have lower prices and short bonds of treated firms, which have abnormally high prices. The important aspect of this strategy is that it does not require any prior knowledge as the shock happens before the trading begins. We evaluate the average performance of this strategy using additional data on monthly bond returns for the variety of different horizons: 1 to 4 quarters out. In Table 15, we show the cumulative quarterly returns of the long-short portfolio controlling for differences in bond duration, company size, and industry-fixed effects.

The results indicate economically and statistically significant returns of the proposed strategy. The first quarter return of the strategy yields about 14 basis points. As expected, the results get stronger, statistically and economically, over longer horizons. The cumulative first 2-quarter returns are 70 basis points. The number rises to 96 basis points for the 3-quarter returns, and finally levels off at around 103 basis points for the 4-quarter returns. These figures are quite substantial for corporate bond expected return studies. In contrast to equity returns, even 1% per annum represents a significant premium for corporate bonds, which typically have durations of around 7 years. In sum, we conclude that the coverage shock created significant mispricings in the treated bonds.

4. Conclusion

Credit ratings of companies are biased and they contribute to inefficient corporate debt markets. We show that one important force that mediate this bias has to do with information spillovers from equity markets to credit ratings. Credit rating agencies find it harder to issue high grades for a firm's debt when there is lots of objective equity analyst reports about the firm's earnings which are informative about a firm's distance-to-default. We find that an exogenous drop in one analyst covering increases the subsequent ratings of a firm by around a half-rating notch, an economically sizeable and statistically significant effect. This coverage-induced shock also leads to less informative ratings for future credit events and credit mispricings which are exploitable.

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Table 1: Coverage of Rated Firms Relative to the Universe

We report the distribution of companies over time in a full sample of companies available from CRSP/COMPUSTAT, and in a restricted sample of firms for which we have information on credit ratings and analyst coverage. The sample covers the period 1985—2005.

Year	Full Sample	Restricted Sample
1985	5694	792
1986	6090	852
1987	6461	861
1988	6397	826
1989	6336	798
1990	6344	747
1991	6544	770
1992	6935	878
1993	7695	960
1994	8158	963
1995	8348	1005
1996	8815	1125
1997	8842	1249
1998	8549	1080
1999	8703	1116
2000	8518	1029
2001	8046	1046
2002	7722	1113
2003	7414	1159
2004	7098	1049
2005	6995	997

Table 2: Summary Statistics: Rated vs. Non-Rated Firms

We report summary statistics for two sets of firms: those without credit rating (in Panel A), and those with credit rating (in Panel B). $ASSETS_t$ is the firm i's book value of assets at the end of year t. BM_{it} is firm i's book value divided by its market cap at the end of year t. $SP500_{it}$ is an indicator variable equal to one if the stock is included in S&P 500 index and zero otherwise. $LEVERAGE_{it}$ is firm i's book value of debt over total assets. $VOLATILITY_{it}$ is the variance of daily (simple, raw) returns of stock i during year t. DD_{it} measures distance to default, defined as $LEVERAGE \times VOLATILITY$. The sample covers the period 1985—2005.

A: Non-Rated Firms

Year	Assets	BM	SP500	Leverage	Volatility	DD
1985	309.87	0.979	0.033	0.257	0.476	0.127
1986	353.26	0.717	0.031	0.245	0.512	0.135
1987	385.53	0.824	0.028	0.267	0.542	0.151
1988	428.18	0.933	0.029	0.273	0.676	0.198
1989	558.75	0.826	0.030	0.276	0.526	0.161
1990	565.65	1.200	0.031	0.315	0.564	0.197
1991	637.11	0.964	0.032	0.267	0.712	0.220
1992	614.55	0.804	0.030	0.229	0.667	0.172
1993	633.35	0.682	0.027	0.207	0.720	0.169
1994	602.19	0.725	0.026	0.226	0.637	0.153
1995	822.64	0.656	0.025	0.216	0.624	0.144
1996	980.21	0.608	0.023	0.201	0.623	0.130
1997	1197.44	0.557	0.022	0.195	0.624	0.127
1998	1404.04	0.784	0.024	0.248	0.602	0.147
1999	1961.73	2.644	0.021	0.257	0.779	0.193
2000	2039.68	1.083	0.023	0.273	0.827	0.216
2001	1963.75	0.964	0.023	0.261	0.851	0.209
2002	1861.36	1.089	0.021	0.268	0.692	0.179
2003	2506.98	0.699	0.019	0.224	0.688	0.153
2004	2441.35	0.554	0.022	0.193	0.682	0.127
2005	2312.52	0.535	0.025	0.194	0.106	0.018
Total	1190.50	0.903	0.026	0.241	0.634	0.159

B: Rated Firms

Year	Assets	BM	SP500	Leverage	Volatility	DD
1985	3164.09	0.913	0.208	0.456	0.294	0.139
1986	3455.79	0.859	0.217	0.452	0.327	0.152
1987	3601.32	1.027	0.230	0.486	0.360	0.181
1988	4466.57	2.799	0.243	0.490	0.489	0.244
1989	5426.26	0.857	0.257	0.476	0.295	0.147
1990	5562.08	1.237	0.274	0.523	0.333	0.188
1991	6448.08	0.933	0.278	0.464	0.464	0.247
1992	6635.13	0.746	0.264	0.439	0.408	0.201
1993	6707.63	0.645	0.254	0.409	0.359	0.157
1994	7452.05	0.730	0.258	0.441	0.329	0.152
1995	8186.07	0.654	0.262	0.417	0.315	0.147
1996	8524.27	0.627	0.250	0.405	0.334	0.150
1997	9853.20	0.541	0.239	0.385	0.360	0.156
1998	13847.05	0.649	0.272	0.415	0.362	0.162
1999	17288.72	0.750	0.286	0.427	0.516	0.227
2000	18337.68	1.155	0.303	0.459	0.554	0.267
2001	18645.18	45.512	0.316	0.451	0.560	0.265
2002	15348.40	1.060	0.318	0.473	0.473	0.242
2003	17826.48	5.935	0.322	0.395	0.518	0.230
2004	21674.91	0.564	0.346	0.353	0.536	0.192
2005	23830.74	0.568	0.348	0.353	0.080	0.030
Total	11344.89	3.340	0.276	0.432	0.397	0.185

Table 3: Summary Statistics (OLS)

We consider a sample of stocks covered by IBES during the period 1985-2005 with valid annual earnings forecast records. *RATING* is an average rating, represented as a numeric score from 1 (best) to 24 (worst), provided by Standard & Poor's, Moody's, and Fitch agency for company *i* in year *t*. $YIELD_{it}$ is the value-weighted yield of the bond issues for company *i* and year *t*. *DEFAULT* equals one if the company *i* goes bankrupt in year *t* and is zero otherwise. $COVERAGE_{it}$ is a measure of analyst coverage, defined as the number of analysts covering firm *i* at the end of year *t*. $LNASSETS_{it}$ is the natural logarithm of firm *i*'s market capitalization (price times shares outstanding) at the end of year *t*. $LNBM_{it}$ is the natural logarithm of firm *i*'s book value divided by its market cap at the end of year *t*. $MOMENTUM_{it}$ is the average monthly return on stock *i* in year *t*. $LEVERAGE_{it}$ is firm *i*'s book value of debt over total assets. $TANGIBILITY_{it}$ is tangible assets over total assets. DD_{it} measures distance to default, defined as *LEVERAGE×VOLATILITY*. $VOLATILITY_{it}$ is the variance of daily (simple, raw) returns of stock *i* in year *t*. $SP500_{it}$ is an indicator variable equal to one if stock *i* is included in the S&P500 index in year *t*. $CASH_{i,t}$ is the value of cash position in firm *i* at time *t*.

	Mean	Median	St. dev.
Rating	11.93	12.33	4.19
Yield	8.54	8.03	4.22
Default	2.09	0	16.78
Coverage	17.93	16.00	10.48
Ln(Assets)	8.38	8.27	1.48
Ln(BM)	-0.55	-0.46	0.73
Momentum	0.01	0.01	0.04
SP500	0.39	0	0.49
Leverage	0.40	0.39	0.23
Volatility	0.34	0.31	0.16
DD	0.14	0.11	0.12
Tangibility	0.39	0.36	0.28
Cash	1.81	0.09	7.42
Bond Coverage	1.70	1	1.16

Table 4: Credit Rating and Coverage (OLS)

The dependent variable is *RATING*$_{it}$. Independent variables include *COVERAGE*$_{it}$, *LNASSETS*$_{i,t}$, *LNBM*$_{i,t}$, *MOMENTUM*$_{i,t}$, *LEVERAGE*$_{i,t}$, *TANGIBILITY*$_{i,t}$, *DD*$_{i,t}$, *VOLATILITY*$_{it}$, *SP500*$_{it}$, and *CASH*$_{i,t}$. Results in columns (3) and (4) include industry-fixed effects (defined as the 2-digit SIC), and in column (4) year-fixed effects. Standard errors (in parentheses) are clustered at the firm and year groupings. ***, **, * denotes 1%, 5%, and 10% statistical significance.

	(1)	(2)	(3)	(4)
VARIABLES	Rating	Rating	Rating	Rating
Coverage	0.048***	0.050***	0.033**	0.019**
	(0.015)	(0.015)	(0.017)	(0.008)
Ln(Assets)	-1.765***	-1.542***	-1.468***	-0.895***
	(0.092)	(0.091)	(0.113)	(0.088)
Ln(BM)	0.035	0.131	0.283	0.017
	(0.184)	(0.193)	(0.177)	(0.084)
Momentum	3.618	4.603	3.891	-0.705
	(4.069)	(3.713)	(3.842)	(1.559)
Leverage	5.688***	4.027***	6.159***	6.694***
	(0.510)	(1.059)	(1.035)	(0.667)
Tangibility	-3.567***	-3.162***	-1.750***	-1.428***
	(0.546)	(0.507)	(0.414)	(0.386)
DD		2.512	-0.578	-3.599**
		(2.172)	(2.058)	(1.412)
Volatility		3.184*	2.416	9.752***
		(1.890)	(2.039)	(0.929)
Cash		-0.020***	-0.009	-0.023***
		(0.008)	(0.009)	(0.008)
SP500		-0.648***	-0.476**	-0.285*
		(0.209)	(0.187)	(0.165)
Constant	11.931***	11.931***	10.732***	13.800***
	(0.366)	(0.400)	(0.250)	(0.236)
Industry-Fixed Effects	No	No	Yes	Yes
Year-Fixed Effects	No	No	No	Yes
Observations	11,870	11,870	11,870	11,870
R-squared	0.326	0.356	0.427	0.616

Table 5: Summary Statistics (IV)

We consider all stocks covered by two merging brokerage houses around the one-year merger event window. *RATING* is an average rating, represented as a numeric score from 1 (best) to 24 (worst), provided by Standard & Poor's, Moody's, and Fitch agency for company *i* in year *t*. $YIELD_{it}$ is the value-weighted yield of the bond issues for company *i* and year *t*. *DEFAULT* equals one if the company *i* goes bankrupt in year *t* and is zero otherwise. *DOWNGRADE* (*UPGRADE*) equals one if the company *i* gets downgraded (upgraded) in year *t* and is zero otherwise. $COVERAGE_{it}$ is a measure of analyst coverage, defined as the number of analysts covering firm *i* at the end of year *t*. $LNASSETS_{it}$ is the natural logarithm of firm *i*'s market capitalization (price times shares outstanding) at the end of year *t*. $LNBM_{it}$ is the natural logarithm of firm *i*'s book value divided by its market cap at the end of year *t*. $MOMENTUM_{it}$ is the average monthly return on stock *i* during year *t*. $LEVERAGE_{it}$ is firm *i*'s book value of debt over total assets. $TANGIBILITY_{it}$ is tangible assets over total assets. $VOLATILITY_t$ is the variance of daily (simple, raw) returns of stock *i* during year *t*. DD_{it} measures distance to default, defined as *LEVERAGE*×*VOLATILITY*. $SP500_{it}$ is an indicator variable equal to one if stock *i* is included in the S&P500 index. $CASH_{i,t}$ is the value of cash position in firm *i* at time *t*.

	Mean	Median	St. dev.
Rating	10.56	10.00	4.25
Yield	8.05	7.61	2.27
Default	2.21	0	14.69
Downgrade	10.32	0	30.42
Upgrade	8.86	0	28.42
Coverage	15.02	14.00	8.81
Ln(Assets)	8.54	8.45	1.50
Ln(BM)	-0.74	-0.67	0.80
Momentum	0.01	0.01	0.03
SP500	0.43	0.00	0.50
Leverage	0.38	0.36	0.23
Volatility	0.37	0.34	0.17
DD	0.14	0.11	0.12
Tangibility	0.37	0.33	0.27
Cash	2.41	0.09	38.72

Table 6: Validity of Experiment

We provide the DID estimator for various corporate characteristics, including Ln(Assets), Ln(BM), Momentum, Leverage, and Distance to Default (DD). Standard errors (in parentheses) are clustered at the merger groupings. ***, **, * denotes 1%, 5%, and 10% statistical significance.

N=844

Stock Characteristic	SIZE/BM/MOM-Matched
Ln(Assets)	-0.002
	(0.035)
Ln(BM)	0.026
	(0.024)
Momentum (in %)	-0.099
	(0.199)
Leverage (in %)	0.861
	(0.608)
DD	0.002
	(0.003)

Table 7: The Effect on Coverage and Ratings

In Panel A, the dependent variable is credit rating (*RATING*). For each merger, we consider a one-year window prior to merger (pre-event window) and a one-year window after the merger (post-event window). We construct an indicator variable (*AFTER*) equal to one for the post-event period and zero for the pre-event period. For each merger window, we assign an indicator variable (*AFFECTED*) equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. Each variable is measured separately for the window before and after the merger event. *LNASSETS* is a natural logarithm of the market cap of the stock; *MOMENTUM* is annual return on the stock; *LNBM* is a natural logarithm of the book to market ratio; *COVERAGE* denotes the number of analysts tracking the stock; $LEVERAGE_{it}$ is firm *i*'s book value of debt over total assets. $TANGIBILITY_{it}$ is tangible assets over total assets. DD_{it} measures distance to default, defined as $LEVERAGE \times VOLATILITY$; $VOLATILITY_{it}$ is the variance of daily (simple, raw) returns of stock *i* during period *t*; $CASH_{i,t}$ is the value of cash position in firm *i* at time *t*; *SP500* is an indicator variable equal to one if a stock is included in the S&P500 index. *Controls1* includes assets, book-to-market ratio, momentum, leverage, and tangibility. *Controls2* additionally includes volatility, distance to default, cash, and an indicator variable for S&P 500 index inclusion. Standard errors (in parentheses) are clustered at the merger grouping. ***, **, * denotes 1%, 5%, and 10% statistical significance.

Panel A: Basecase Results

VARIABLES	(1) Coverage	(2) Coverage	(3) Rating	(4) Rating
After	1.128***	0.852***	-0.781***	-1.056***
	(0.152)	(0.161)	(0.024)	(0.032)
Affected	4.449***	4.321***	0.387*	0.428**
	(0.501)	(0.486)	(0.186)	(0.181)
After*Affected	-0.884***	-0.938***	-0.355*	-0.422**
	(0.283)	(0.292)	(0.186)	(0.164)
Controls1	Yes	Yes	Yes	Yes
Controls2	No	Yes	No	Yes
Constant	-22.844***	-27.879***	20.438***	14.181***
	(1.199)	(1.847)	(0.291)	(0.686)
Merger-Fixed Effects	Yes	Yes	Yes	Yes
Industry-Fixed Effects	Yes	Yes	Yes	Yes
Year-Fixed Effects	Yes	Yes	Yes	Yes
Observations	15,631	15,631	15,631	15,631
R-squared	0.623	0.641	0.652	0.692

Table 8: Nonparametric Evidence

We measure analyst coverage as the number of analysts covering firm *i* at the end of year *t*. For all mergers, we split the sample of stocks into those covered by both merging brokerage houses (treatment sample) and those not covered by both houses (control sample). We also divide stocks into pre-merger period and post-merger period (one-year window for each period). For each period we further construct benchmark portfolios using the control sample based on stocks' assets (*SIZE*), book-to-market ratio (*BM*), and average past year's returns (*RET*). Our benchmark assignment involves three portfolios in each category. Each stock in the treatment sample is then assigned to its own benchmark *SIZE/BM/RET*-matched). Next, for each period, we calculate the cross-sectional average of the differences in analyst stock coverage and credit rating across all stocks in the treatment sample and their respective benchmarks. Finally, we calculate the difference in differences between post-event period and pre-event period (DID Estimator). Panel A presents the results for the unconditional sample. Panel B presents our results by cuts on initial coverage. There are three groups: lowest coverage (<5), medium coverage (>=5 and <20) and highest coverage (>=20). Standard errors (in parentheses) are clustered at the merger groupings. ***, **, * denotes 1%, 5%, and 10% statistical significance.

Panel A: Coverage and Credit Rating Optimism

N=844

	(1)	(2)
	Coverage	Rating
SIZE/BM/MOM-Matched	-1.130***	-0.234**
	(0.230)	(0.108)

Panel B: Change in Rating: Conditioning on Initial Coverage

	Rating
SIZE/BM/MOM-Matched (Coverage <5)	-1.118*
	(0.684)
SIZE/BM/MOM-Matched	-0.387***
(Coverage>=5 & <20)	(0.167)
SIZE/BM/MOM-Matched (Coverage>=20)	-0.084
	(0.138)

Table 9: Selection Biases

In Panel A, the treatment sample is constructed based on the stocks that are covered by one but not both merging houses. **In Panel B**, the control sample is constructed using the stocks which are covered by one but not both merging houses. Standard errors (in parentheses) are clustered at the merger groupings. ***, **, * denotes 1%, 5%, and 10% statistical significance.

Panel A: Change in Rating for Non-overlapping Stocks

	(1)	(2)
	Coverage	Rating
SIZE/BM/MOM-Matched	-0.093	-0.001
	(0.163)	(0.076)

Panel B: Change in Rating for Non-overlapping Stocks as a Control

N=844

	(1)	(2)
	Coverage	Rating
SIZE/BM/MOM-Matched	-1.245***	-0.404***
	(0.265)	(0.118)

Table 10: Conditioning on Speculative Grade Debt

The dependent variable is credit rating (*RATING*). For each merger, we consider a one-year window prior to merger (pre-event window) and a one-year window after the merger (post-event window). The table presents our results by cuts on different measures of probability of default: Investment Grade vs. Speculative Grade; below and above median of naïve distance to default of Bharath and Shumway (2008); below 25% of DD and above median DD. We construct an indicator variable (*AFTER*) equal to one for the post-event period and zero for the pre-event period. For each merger window, we assign an indicator variable (*AFFECTED*) equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. *LNASSETS* is a natural logarithm of the market cap of the stock; *MOMENTUM* is annual return on the stock; *LNBM* is a natural logarithm of the book to market ratio; *COVERAGE* denotes the number of analysts tracking the stock; $LEVERAGE_{it}$ is firm *i*'s book value of debt over total assets. $TANGIBILITY_{it}$ is tangible assets over total assets. DD_{it} measures distance to default, defined as *LEVERAGE×VOLATILITY;* $VOLATILITY_{it}$ is the variance of daily (simple, raw) returns of stock *i* during year *t*; $CASH_{i,t}$ is the value of cash position in firm *i* at time *t*; *SP500* is an indicator variable equal to one if a stock is included in the S&P500 index. *Controls1* includes assets, book-to-market ratio, momentum, leverage, and tangibility. *Controls2* additionally includes volatility, distance to default, cash, and an indicator variable for S&P 500 index inclusion. All regressions include merger-fixed effects, industry-fixed effects, and year-fixed effects. Standard errors (in parentheses) are clustered at the merger grouping. ***, **, * denotes 1%, 5%, and 10% statistical significance.

VARIABLES	(1) Investment Grade	(2) Speculative Grade	(3) Naïve DD<5	(4) Naïve DD>5	(5) DD<0.05	(6) DD>0.11
After	-0.348***	-0.672***	-0.895***	-1.173***	-1.366***	-0.908***
	(0.024)	(0.051)	(0.035)	(0.043)	(0.043)	(0.023)
Affected	0.302	0.380**	0.237*	0.404**	0.323	0.418**
	(0.242)	(0.131)	(0.132)	(0.170)	(0.187)	(0.166)
After*Affected	-0.261	-0.476**	-0.148	-0.410**	-0.151	-0.425**
	(0.165)	(0.184)	(0.150)	(0.165)	(0.209)	(0.170)
Constant	12.166***	17.829***	17.668***	20.757***	14.871***	17.562***
	(0.457)	(0.480)	(0.702)	(0.684)	(0.561)	(0.933)
Controls1	Yes	Yes	Yes	Yes	Yes	Yes
Controls2	Yes	Yes	Yes	Yes	Yes	Yes
Merger-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6565	9066	6383	9248	3394	7907
R-squared	0.394	0.718	0.658	0.691	0.748	0.675

Table 11: Conditioning on Bond Analyst Coverage (DID)

The dependent variable is credit rating (*RATING*). For each merger, we consider a one-year window prior to merger (pre-event window) and a one-year window after the merger (post-event window). We construct an indicator variable (*AFTER*) equal to one for the post-event period and zero for the pre-event period. For each merger window, we assign an indicator variable (*AFFECTED*) equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. Each variable is measured separately for the window before and after the merger event. In column (1) we estimate the regression on a sample with low bond analyst coverage (fewer than two analysts), and in column (2) on a sample with high bond analyst coverage (more than two analysts). *BOND COVERAGE*$_{it}$ is defined as the number of bond analysts covering firm *i* at the end of year *t*. *Controls1* includes assets, book-to-market ratio, momentum, leverage, and tangibility. *Controls2* additionally includes volatility, distance to default, cash, and an indicator variable for S&P 500 index inclusion. Standard errors (in parentheses) are clustered at the merger grouping. ***, **, * denotes 1%, 5%, and 10% statistical significance.

VARIABLES	(1) Low Bond Analyst Coverage	(2) High Bond Analyst Coverage
After	-1.060***	-0.956***
	(0.032)	(0.190)
Affected	0.421**	-0.671
	(0.183)	(1.094)
After*Affected	-0.425**	0.158
	(0.165)	(0.729)
Constant	14.198***	21.914***
	(0.701)	(1.303)
Controls1	Yes	Yes
Controls2	Yes	Yes
Merger-Fixed Effects	Yes	Yes
Industry-Fixed Effects	Yes	Yes
Year-Fixed Effects	Yes	Yes
Observations	1,323	308
R-squared	0.694	0.753

Table 12: Ratings and Default (OLS)

The dependent variables are $DEFAULT_{it}$, $UPGRADE_{it}$, and $DOWNGRADE_{it}$. Independent variables include $RATING_{it}$, $COVERAGE_{it}$, $RATING_{it}*COVERAGE_{it}$, $LNASSETS_{i,t}$, $LNBM_{i,t}$, $MOMENTUM_{i,t}$, $LEVERAGE_{i,t}$, $TANGIBILITY_{i,t}$, $DD_{i,t}$, $VOLATILITY_{it}$, $SP500_{it}$, and $CASH_{i,t}$. The results include industry-fixed effects (defined as the 2-digit SIC) and year-fixed effects. Standard errors (in parentheses) are clustered at the firm and year groupings. ***, **, * denotes 1%, 5%, and 10% statistical significance.

VARIABLES	(1) Default	(2) Upgrade	(3) Downgrade
Rating	0.003***	-0.001	0.003**
	(0.001)	(0.004)	(0.002)
Coverage	0.000	-0.000	0.002**
	(0.001)	(0.001)	(0.001)
Rating*Coverage	-0.000	-0.000	0.000***
	(0.000)	(0.000)	(0.000)
Ln(Assets)	0.001	0.022***	0.026***
	(0.004)	(0.006)	(0.007)
Ln(BM)	0.004	-0.007	0.023***
	(0.006)	(0.007)	(0.009)
Momentum	-1.497***	-0.098	0.233**
	(0.167)	(0.112)	(0.115)
Leverage	-0.041	0.013	0.197***
	(0.032)	(0.041)	(0.066)
Tangibility	0.038**	0.008	0.048**
	(0.017)	(0.025)	(0.024)
DD	0.314***	-0.216**	0.049
	(0.080)	(0.108)	(0.106)
Volatility	0.062	0.153*	0.111
	(0.052)	(0.079)	(0.104)
Cash	-0.000	0.000	-0.001*
	(0.000)	(0.000)	(0.001)
SP500	-0.007	-0.008	-0.027*
	(0.005)	(0.014)	(0.014)
Constant	0.048	-0.098***	0.848***
	(0.006)	(0.012)	(0.024)
Industry-Fixed Effects	Yes	Yes	Yes
Year-Fixed Effects	Yes	Yes	Yes
Observations	11,897	11,897	11,897
R-squared	0.223	0.202	0.124

Table 13: Ratings and Default (DID)

The dependent variable is the instance of default (*DEFAULT*). For each merger, we consider a one-year window prior to merger (pre-event window) and a one-year window after the merger (post-event window). We construct an indicator variable (*AFTER*) equal to one for the post-event period and zero for the pre-event period. For each merger window, we assign an indicator variable (*AFFECTED*) equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. Each variable is measured separately for the window before and after the merger event. *RATING* is an average credit rating. *LNASSETS* is a natural logarithm of the market cap of the stock; *MOMENTUM* is annual return on the stock; *LNBM* is a natural logarithm of the book to market ratio; *COVERAGE* denotes the number of analysts tracking the stock; $LEVERAGE_{it}$ is firm *i*'s book value of debt over total assets. $TANGIBILITY_{it}$ is tangible assets over total assets. DD_{it} measures distance to default, defined as *LEVERAGE×VOLATILITY;* $VOLATILITY_{it}$ is the variance of daily (simple, raw) returns of stock *i* during period *t*; $CASH_{i,t}$ is the value of cash position in firm *i* at time *t*; *SP500* is an indicator variable equal to one if a stock is included in the S&P500 index. *Controls1* includes assets, book-to-market ratio, momentum, leverage, and tangibility. *Controls2* additionally includes volatility, distance to default, cash, and an indicator variable for S&P 500 index inclusion.

	(1)	(2)	(3)	(4)	(5)
VARIABLES			Default		
Rating	0.002**	0.001	-0.002**	-0.003***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Rating*After	0.011***	0.010***	0.010***	0.009***	0.012***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Rating*Affected	0.003**	0.002**	0.002**	0.002**	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Affected*After	0.047***	0.046**	0.046**	0.046**	0.031
	(0.015)	(0.017)	(0.017)	(0.018)	(0.025)
Rating*Affected*After	-0.008***	-0.007***	-0.007***	-0.007***	-0.004**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
After	-0.061***	-0.056***	-0.063***	-0.054***	-0.084***
	(0.013)	(0.015)	(0.016)	(0.015)	(0.022)
Affected	-0.015	-0.007	-0.008	-0.010	-0.004
	(0.013)	(0.012)	(0.010)	(0.010)	(0.009)
Controls 1	No	Yes	Yes	Yes	Yes
Controls 2	No	No	Yes	Yes	Yes
Merger-fixed effects	Yes	Yes	Yes	Yes	Yes
Industry-fixed effects	Yes	Yes	Yes	Yes	Yes
Stock-fixed effects	No	No	No	Yes	Yes
Year-fixed effects	No	No	No	No	Yes
Constant	-0.057***	-0.071*	-0.074*	0.045	0.031
	(0.010)	(0.035)	(0.040)	(0.061)	(0.061)
Observations	18,272	17,720	17,530	17,530	17,530
R-squared	0.108	0.143	0.171	0.514	0.523

Table 14: Ratings and Downgrades/Upgrades (DID)

The dependent variable is the instance of upgrade/downgrade (*UPGRADE/DOWNGRADE*). For each merger, we consider a one-year window prior to merger (pre-event window) and a one-year window after the merger (post-event window). We construct an indicator variable (*AFTER*) equal to one for the post-event period and zero for the pre-event period. For each merger window, we assign an indicator variable (*AFFECTED*) equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. Each variable is measured separately for the window before and after the merger event. *RATING* is an average credit rating. *LNASSETS* is a natural logarithm of the market cap of the stock; *MOMENTUM* is annual return on the stock; *LNBM* is a natural logarithm of the book to market ratio; *COVERAGE* denotes the number of analysts tracking the stock; $LEVERAGE_{it}$ is firm *i*'s book value of debt over total assets. $TANGIBILITY_{it}$ is tangible assets over total assets. DD_{it} measures distance to default, defined as *LEVERAGE×VOLATILITY;* $VOLATILITY_{it}$ is the variance of daily (simple, raw) returns of stock *i* during period *t*; $CASH_{i,t}$ is the value of cash position in firm *i* at time *t*; *SP500* is an indicator variable equal to one if a stock is included in the S&P500 index. *Controls1* includes assets, book-to-market ratio, momentum, leverage, and tangibility. *Controls2* additionally includes volatility, distance to default, cash, and an indicator variable for S&P 500 index inclusion.

Panel A: Downgrades

	(1)	(2)	(3)	(4)	(5)
VARIABLES			Downgrade		
Rating	-0.010***	-0.010**	-0.010***	-0.028***	-0.017**
	(0.003)	(0.003)	(0.003)	(0.005)	(0.008)
Rating*After	0.005	0.005	0.005	0.004	0.005*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Rating*Affected	0.009***	0.009***	0.010***	0.009***	0.004*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Affected*After	0.021	0.019	0.026	0.052	0.045
	(0.065)	(0.064)	(0.062)	(0.053)	(0.036)
Rating*Affected*After	-0.010***	-0.010***	-0.011***	-0.011***	-0.006**
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
After	0.007	0.012	0.011	-0.008	-0.038
	(0.056)	(0.055)	(0.053)	(0.044)	(0.029)
Affected	-0.066**	-0.060*	-0.063*	-0.058*	-0.037
	(0.030)	(0.029)	(0.030)	(0.032)	(0.026)
Controls 1	No	Yes	Yes	Yes	Yes
Controls 2	No	No	Yes	Yes	Yes
Merger-fixed effects	Yes	Yes	Yes	Yes	Yes
Industry-fixed effects	Yes	Yes	Yes	Yes	Yes
Stock-fixed effects	No	No	No	Yes	Yes
Year-fixed effects	No	No	No	No	Yes
Constant	0.278***	0.294***	0.308***	0.510***	0.233*
	(0.039)	(0.060)	(0.073)	(0.120)	(0.125)
Observations	18,272	17,720	17,530	17,530	17,530
R-squared	0.066	0.066	0.069	0.173	0.199

Panel B: Upgrades

	(1)	(2)	(3)	(4)	(5)
VARIABLES			Upgrade		
Rating	0.004**	0.005**	0.003**	0.019***	0.023***
	(0.002)	(0.002)	(0.001)	(0.005)	(0.006)
Rating*After	-0.003	-0.004	-0.004*	-0.004*	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Rating*Affected	0.002	0.001	0.000	-0.002	-0.003
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Affected*After	-0.024	-0.034	-0.040	-0.069	-0.032
	(0.056)	(0.057)	(0.059)	(0.064)	(0.053)
Rating*Affected*After	-0.001	0.000	0.000	0.001	0.002
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
After	0.026	0.028	0.023	0.040	0.047**
	(0.030)	(0.029)	(0.032)	(0.034)	(0.021)
Affected	0.016	0.023	0.024	0.064	0.049
	(0.058)	(0.056)	(0.057)	(0.063)	(0.059)
Controls 1	No	Yes	Yes	Yes	Yes
Controls 2	No	No	Yes	Yes	Yes
Merger-fixed effects	Yes	Yes	Yes	Yes	Yes
Industry-fixed effects	Yes	Yes	Yes	Yes	Yes
Stock-fixed effects	No	No	No	Yes	Yes
Year-fixed effects	No	No	No	No	Yes
Constant	0.199***	0.008	-0.045	-0.771**	-0.777**
	(0.027)	(0.052)	(0.086)	(0.270)	(0.315)
Observations	18,272	17,720	17,530	17,530	17,530
R-squared	0.115	0.128	0.142	0.275	0.301

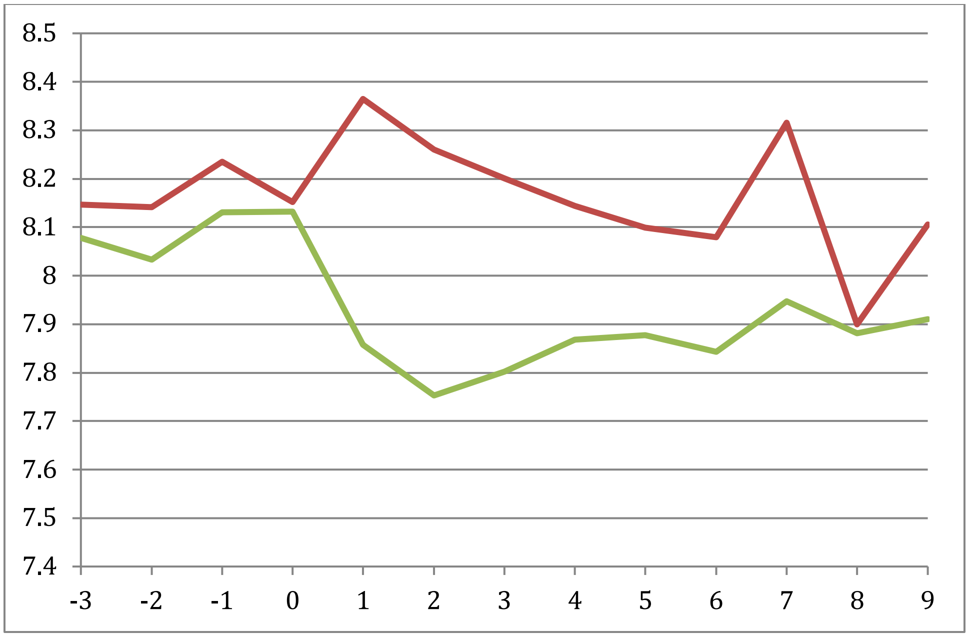
Table 15: Cumulative Post-Event Returns of the Long-Short Strategy

The dependent variable is the cumulative bond return. *Strategy* is an indicator variable equal to one for companies being part of the Control group and zero for companies being part of the Treatment group. Treatment group includes all companies being affected by the merger shock. The control group is matched individually based on the closest match in terms of average duration of the bond issues. Controls include natural logarithm of asset size and average duration of the company. We also include industry-fixed effects. Standard errors are clustered at the year dimension. ***, **, * denotes 1%, 5%, and 10% statistical significance.

	(1)	(2)	(3)	(4)
VARIABLES	1-quarter	2-quarters	3-quarters	4-quarters
Strategy	0.0014	0.0070*	0.0096**	0.0103**
	(0.0053)	(0.0042)	(0.0044)	(0.0048)
Controls	Yes	Yes	Yes	Yes
Industry F.E.	Yes	Yes	Yes	Yes

Figure 1: Yields of the Treatment and Control Samples around Event Date

The lines show average bond yields (in %) for the treatment firms (bottom line) and the control firms (top line) around the merger events (event date equal to 1). Treatment group includes all companies being affected by the merger shock. The control group is matched individually based on the closest match in terms of average duration of the bond issues.



Internet Appendix Supplementary Tables

Table A.I: Ratings and Default: Logit Specification (DID)

The dependent variable is the instance of default (*DEFAULT*). For each merger, we consider a one-year window prior to merger (pre-event window) and a one-year window after the merger (post-event window). We construct an indicator variable (*AFTER*) equal to one for the post-event period and zero for the pre-event period. For each merger window, we assign an indicator variable (*AFFECTED*) equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. Each variable is measured separately for the window before and after the merger event. *RATING* is an average credit rating. *LNASSETS* is a natural logarithm of the market cap of the stock; *MOMENTUM* is annual return on the stock; *LNBM* is a natural logarithm of the book to market ratio; *COVERAGE* denotes the number of analysts tracking the stock; $LEVERAGE_{it}$ is firm *i*'s book value of debt over total assets. DD_{it} measures distance to default, defined as $LEVERAGE \times VOLATILITY$; $VOLATILITY_{it}$ is the variance of daily (simple, raw) returns of stock *i* during period *t*; $CASH_{i,t}$ is the value of cash position in firm *i* at time *t*; *SP500* is an indicator variable equal to one if a stock is included in the S&P500 index. *Controls1* includes assets, book-to-market ratio, momentum, and leverage. *Controls2* additionally includes volatility, distance to default, cash, and an indicator variable for S&P 500 index inclusion. Standard errors are obtained using Ai and Norton correction. ***, **, * denotes 1%, 5%, and 10% statistical significance.

	(1)	(2)	(3)	(4)	(5)
VARIABLES			Default		
Rating	0.253***	0.236***	0.104*	0.015	-0.008
	(0.068)	(0.068)	(0.061)	(0.032)	(0.045)
Rating*After	0.066	0.036	0.082	0.049	0.044
	(0.069)	(0.068)	(0.061)	(0.036)	(0.055)
Rating*Affected	0.198**	0.160*	0.204***	0.011	0.022
	(0.091)	(0.082)	(0.073)	(0.059)	(0.060)
Affected*After	2.288	2.069	2.745	0.036	-0.018
	(2.248)	(2.143)	(2.114)	(0.089)	(0.088)
Rating*Affected*After	-0.213**	-0.181**	-0.248***	-0.146**	-0.138**
	(0.102)	(0.092)	(0.093)	(0.076)	(0.072)
After	2.967***	3.101***	2.514***	-0.002	-0.177
	(0.984)	(1.020)	(0.972)	(0.054)	(0.365)
Affected	-2.572	-1.903	-2.279	-0.027	-0.025
	(1.966)	(1.874)	(1.715)	(0.243)	(0.242)
Controls 1	No	Yes	Yes	Yes	Yes
Controls 2	No	No	Yes	Yes	Yes
Merger-fixed effects	Yes	Yes	Yes	Yes	Yes
Industry-fixed effects	Yes	Yes	Yes	Yes	Yes
Stock-fixed effects	No	No	No	Yes	Yes
Year-fixed effects	No	No	No	No	Yes
Constant	-23.715***	-24.366***	-25.272***	-15.093***	-15.152***
	(1.724)	(1.841)	(2.136)	(1.033)	(0.953)
Observations	17,054	16,909	16,337	13,967	13,967
Pseudo R-squared	0.341	0.410	0.452	0.523	0.524

Table A.II: Ratings and Upgrades: Logit Specification (DID)

The dependent variable is the instance of default (*DEFAULT*). For each merger, we consider a one-year window prior to merger (pre-event window) and a one-year window after the merger (post-event window). We construct an indicator variable (*AFTER*) equal to one for the post-event period and zero for the pre-event period. For each merger window, we assign an indicator variable (*AFFECTED*) equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. Each variable is measured separately for the window before and after the merger event. *RATING* is an average credit rating. *LNASSETS* is a natural logarithm of the market cap of the stock; *MOMENTUM* is annual return on the stock; *LNBM* is a natural logarithm of the book to market ratio; *COVERAGE* denotes the number of analysts tracking the stock; $LEVERAGE_{it}$ is firm *i*'s book value of debt over total assets. DD_{it} measures distance to default, defined as *LEVERAGE×VOLATILITY;* $VOLATILITY_{it}$ is the variance of daily (simple, raw) returns of stock *i* during period *t*; $CASH_{i,t}$ is the value of cash position in firm *i* at time *t*; *SP500* is an indicator variable equal to one if a stock is included in the S&P500 index. *Controls1* includes assets, book-to-market ratio, momentum, and leverage. *Controls2* additionally includes volatility, distance to default, cash, and an indicator variable for S&P 500 index inclusion. Standard errors are obtained using Ai and Norton correction. ***, **, * denotes 1%, 5%, and 10% statistical significance.

	(1)	(2)	(3)	(4)	(5)
VARIABLES			Upgrade		
Rating	-0.031	-0.037	-0.040	-0.006	-0.028
	(0.029)	(0.031)	(0.032)	(0.028)	(0.037)
Rating*After	0.594	0.646	0.752*	-0.038	-0.029
	(0.498)	(0.481)	(0.435)	(0.265)	(0.260)
Rating*Affected	-0.023	-0.024	-0.014	0.092**	0.075**
	(0.053)	(0.055)	(0.054)	(0.039)	(0.032)
Affected*After	-0.011	-0.029	-0.034	0.167*	0.159*
	(0.026)	(0.025)	(0.024)	(0.095)	(0.092)
Rating*Affected*After	-0.024	-0.012	-0.265	-0.081	-0.066
	(0.833)	(0.860)	(0.792)	(0.090)	(0.073)
After	0.056**	0.070***	0.049**	0.053	0.068
	(0.025)	(0.024)	(0.023)	(0.050)	(0.059)
Affected	0.126	0.149	0.295	0.052	1.049
	(0.728)	(0.749)	(0.693)	(0.056)	(0.841)
Controls 1	No	Yes	Yes	Yes	Yes
Controls 2	No	No	Yes	Yes	Yes
Merger-fixed effects	Yes	Yes	Yes	Yes	Yes
Industry-fixed effects	Yes	Yes	Yes	Yes	Yes
Stock-fixed effects	No	No	No	Yes	Yes
Year-fixed effects	No	No	No	No	Yes
Constant	-14.169***	-16.140***	-16.291***	-15.388***	-15.904***
	(1.263)	(1.266)	(1.405)	(0.722)	(0.489)
Observations	18,217	18,064	17,440	15,516	15,516
Pseudo R-squared	0.158	0.177	0.190	0.087	0.090

Table A.III: Ratings and Downgrades: Logit Specification (DID)

The dependent variable is the instance of default (*DEFAULT*). For each merger, we consider a one-year window prior to merger (pre-event window) and a one-year window after the merger (post-event window). We construct an indicator variable (*AFTER*) equal to one for the post-event period and zero for the pre-event period. For each merger window, we assign an indicator variable (*AFFECTED*) equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. Each variable is measured separately for the window before and after the merger event. *RATING* is an average credit rating. *LNASSETS* is a natural logarithm of the market cap of the stock; *MOMENTUM* is annual return on the stock; *LNBM* is a natural logarithm of the book to market ratio; *COVERAGE* denotes the number of analysts tracking the stock; $LEVERAGE_{it}$ is firm *i*'s book value of debt over total assets. DD_{it} measures distance to default, defined as *LEVERAGE×VOLATILITY;* $VOLATILITY_{it}$ is the variance of daily (simple, raw) returns of stock *i* during period *t*; $CASH_{i,t}$ is the value of cash position in firm *i* at time *t*; *SP500* is an indicator variable equal to one if a stock is included in the S&P500 index. *Controls1* includes assets, book-to-market ratio, momentum, and leverage. *Controls2* additionally includes volatility, distance to default, cash, and an indicator variable for S&P 500 index inclusion. Standard errors are obtained using Ai and Norton correction. ***, **, * denotes 1%, 5%, and 10% statistical significance.

	(1)	(2)	(3)	(4)	(5)
VARIABLES			Downgrade		
Rating	-0.118***	-0.115***	-0.111***	0.052	0.058
	(0.031)	(0.035)	(0.035)	(0.082)	(0.077)
Rating*After	0.084**	0.082**	0.077**	-0.016	-0.179***
	(0.035)	(0.035)	(0.034)	(0.036)	(0.064)
Rating*Affected	0.154***	0.150***	0.142***	0.177	0.183
	(0.038)	(0.038)	(0.040)	(0.166)	(0.158)
Affected*After	1.460*	1.481	1.294	-0.077	-0.330**
	(0.870)	(0.902)	(0.952)	(0.083)	(0.142)
Rating*Affected*After	-0.209***	-0.212***	-0.197***	-0.138**	-0.117**
	(0.065)	(0.068)	(0.071)	(0.067)	(0.052)
After	-0.352	-0.348	-0.313	0.023	2.011
	(0.557)	(0.571)	(0.560)	(0.097)	(1.583)
Affected	-1.375***	-1.325***	-1.218**	-0.032	-0.016
	(0.480)	(0.472)	(0.530)	(0.478)	(0.468)
Controls 1	No	Yes	Yes	Yes	Yes
Controls 2	No	No	Yes	Yes	Yes
Merger-fixed effects	Yes	Yes	Yes	Yes	Yes
Industry-fixed effects	Yes	Yes	Yes	Yes	Yes
Stock-fixed effects	No	No	No	Yes	Yes
Year-fixed effects	No	No	No	No	Yes
Constant	1.178***	1.050*	1.070	0.200	-0.612
	(0.456)	(0.617)	(0.855)	(0.220)	(1.031)
Observations	18,228	18,075	17,488	15,511	15,511
Pseudo R-squared	0.098	0.098	0.101	0.112	0.128