Do Security Analysts Discipline Credit Rating Agencies?

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Abstract

We test the hypothesis that security analysts discipline credit rating agencies. After all, analyst reports about a firm's equity would no doubt be informative about its debt default probability and calibrate its credit ratings. We follow Hong and Kacperczyk (2010) in using brokerage house mergers, which eliminate redundant analysts, to shock analyst following so as to identify the causal effect of coverage on credit ratings. We find that a drop in one analyst covering increases the subsequent ratings of a company by around a significant half-rating notch. This effect is coming largely from firms with low initial analyst coverage and hence where the loss of one analyst is a sizeable percentage drop in market discipline. Our effect is stronger for firms close to default and hence where firm debt trades more like equity. The higher ratings due to fewer analysts following also result in lower bond yields.

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Introduction

The Financial Crisis of 2008 revealed the over-reliance of our financial system on the ratings of the credit rating agencies (CRAs), particularly on those of the big three of Standard and Poor's, Moody's, and Fitch. Many point to their 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. U.S. issuance of these credits grew 10-fold in under a decade, with the peak during the first half of 2007 seeing $200 billion. As Coval, Jurek, and Stafford (2010) point out, this expansion would have been difficult without the blessing of the CRAs. During this period, 60% of all global structured products were AAA-rated, while less than 1% of corporate issues were. These striking figures have reignited long-held suspicions of biased credit ratings due to conflicts of interest associated with issuers of credits paying the rating agencies.

Most importantly, the dominance of market share by the three credit rating agencies has appropriately raised the question of whether limited competition led to biased CRAs' recommendations. The theory of reporting bias yields ambiguous answers when it comes to the disciplining role of competition. But there is recent compelling evidence from other settings in favor of competitive pressure moderating the suppression of unfavorable news by firms due to Gentzkow and Shapiro (2006) and Besley and Prat (2006). Intuitively, the more suppliers of information covering the firm or issuer of credit in our context, the more costly it will be for the issuer to keep unfavorable news suppressed. Detailed case studies of media markets point to competition helping to subvert attempted suppression of news. As an example, Gentzkow and Shapiro (2008) and Gentzkow, Glaeser, and Goldin (2006) show that cheap newsprint engendered greater competition which increased the rewards to objective reporting in the late nineteenth century.

Most germane to our study is Hong and Kacperczyk (2010)'s study of competition and reporting bias among security analysts. They find that stocks with more competition as measured by analyst coverage have lower optimism bias, measured by the difference between the consensus forecasts versus actual earnings of the companies. They instrument for analyst coverage using brokerage house mergers which eliminated redundant coverage among the merging firms. Stocks covered by the merging firms observe a decline in one analyst covering it on average and experience a substantial increase in the optimism bias of the other analysts following that firm. This competition effect is pronounced for firms with low analyst coverage, defined as less than five, to begin with.

In this paper, we use this work to investigate credit rating agency bias by examining whether disciplining competition for credit rating agencies can come from security analyst covering the issuing firm's stock. Our insight is that one can indirectly answer the question of whether credit ratings are biased and whether competition influences them by looking to potential market discipline provided by security analysts. 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 hence competitive pressure. This stands in contrast to studying competitive effects using only three credit rating agencies that dominate the credit ratings market and which might be subject to collusion effects that complicate the inference of competitive pressure.

Indeed, there are good reasons for us to expect that security analysts might exert considerable disciplining pressure on credit rating agencies. Financial economic theory 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. Credit ratings which measure whether a firm is likely to pay back its debt naturally depend on equity prices and information generated about the earnings prospects of the firm by security analysts. Moreover, there is a substantial literature that shows that equity market prices lead debt market prices and ratings and are crucial to the determination of credit spreads (Merton (1974)). There is also evidence that news in analyst forecasts impact credit ratings (e.g., Ederington and Goh (1998)).

Hence, even though analysts are not direct competitors with credit rating agencies since they deal with different assets and clients, there is likely to be market discipline spillovers from equity analysts' reports to credit ratings. It would be difficult for credit rating agencies to issue high grades for a firm's debt when there are poor reports about the firm's stock since a firm's distance-to-default, defined as the product of the volatility of the firm's stock price times the firm's leverage ratio, would no doubt be elevated due to these poor reports. The disciplining logic is similar to studies of media and news suppression described above.

There are two mediating factors to this disciplining effect. The first is that the degree of discipline depends of course on how many analysts are covering the stock. The more analysts, the less the optimism bias among them and the greater the disciplining effect on credit ratings. The second is that it is more likely to bind for firms that have low credit ratings and are near default, when a firm's junk debt trades like equity.

Hence, we expect that the more analyst coverage there is, the more difficult it will be for the credit rating agencies to be optimistically biased, where optimism bias can be measured as a residual from a regression 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, defined as the product of a firm's leverage ratio and its stock price volatility. An OLS regression of a firm's credit ratings on coverage plus these standard credit spread predictors yields a positive and statistically significant coefficient, suggesting that competition in the form of more security analyst coverage is associated with less optimistic ratings. But the economic effect is small, on the order of a 1% move of the cross-sectional variation in the left-hand-side variable for an increase of one analyst covering.

This OLS regression is severely biased due to omitted variables. For instance, there is the problem of selection on the left-hand-side variable since we can only estimate an OLS regression model on the subsample of firms with 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 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 or competition for 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 greater coverage also have higher ratings, rather than less as we find.

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 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 group 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.[1]

Our baseline difference-in-differences estimate from this quasi-experiment implies a statistically significant and economically sizeable effect of coverage, or competition, on credit ratings. Treatment firms experiencing a one-analyst reduction in coverage relative to control firms experience a favorable increase in credit ratings in the subsequent year of around 0.463 relative to control firms, which is around 11% of a standard deviation of credit ratings in our sample. This diff-in-diff estimate is thus ten times larger than our OLS estimate.

When we condition on initial coverage, we find that the bulk of this effect is coming from low initial coverage firms with less than five analysts. For these firms, the coefficient from this exercise is around -1.4, which is around 33% of the standard deviation of credit ratings. We also confirm that our effects are much stronger for firms that are close to default, where this closeness is measured using a cut on investment grade (a rating of BBB or better) versus speculative grade (below BBB) or different measures of distance to default. The estimates of interest vary a bit with specification but are typically twice as large. We then consider a number of robustness exercises and draw similar conclusions.

Next, instead of using bond ratings, we perform the same set of exercises using bond yields instead of ratings for a firm on the idea that higher ratings are translated into lower bond yields. Our exercise is also a test to see whether investors can see through the optimistic ratings due to lack of market disciplining forces. We find that bond yields are lower by around 23bps for treated firms. The economic effect is similar to what one gets from moving up a half-rating notch for a firm with a rating of BBB. This is potentially one metric of the economic effect of rating bias from the point of view of bond investors.

[1] The idea of the experiment is also similar in spirit to that in Kelly and Ljungqvist (2012) who determine an exogenous change of coverage based on terminations of coverage by brokerage houses.

Finally, we provide additional analyses on potentially different versions of the disciplining effect. In doing so, we look at the effect of equity analyst forecast bias, accuracy, and timing in driving the rating optimism and find no material difference in our baseline results. Also, we hand-collect bond analyst coverage data to perform an out-of-sample test 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. However, we are able to estimate an OLS regression model of credit rating on bond analyst coverage and find very similar results to those obtained from the OLS regressions for equity analysts.

In the aftermath of the financial crisis, there has understandably been renewed interest in understanding the bias of credit ratings. These papers, many of which are theory oriented, point out that conflicts of interest might lead CRAs to issue biased recommendations.[2] The consideration of competition is among competing CRAs and the results point to a nuanced role of competitive effects of bias in line with the general literature on the economics of reporting bias. Notably, more competing firms might issue more favorable ratings in competition for business rather than less. Indeed, on the empirical front, Becker and Milbourn (2011) find evidence that the entry of Fitch lead to better ratings. The opposite results are reported in Doherty et al. (2012) in their analysis of entry into insurance market by A.M. Best. The collusion issue of just a few competitors might also attenuate competitive effects as suggested by Gentzkow and Shapiro (2006) and Besley and Prat (2006). As such, our identification strategy of disciplining effects from security

[2] The competition forces among credit rating agencies and their impact on credit rating optimism are studied theoretically in Bar-Isaac and Shapiro (2011), Camanho et al. (2010), Manso (2011), Mathis et al. (2009), and Skreta and Veldkamp (2011), among others.

analysts emerges as novel and important in pointing toward a plausibly exogenous mechanism for how competition might affect CRAs.

Indeed, one piece of striking evidence in support of our findings is that the CDOs had little competitive effects from security analysts since there are few analysts covering housing stocks. This stands in contrast to the better performance of CRAs in corporate bonds.

Our paper proceeds as follows. We describe our data in Section II. We report our results in Section III. We conclude in Section IV. Here, we draw some lessons of our analysis for the recent financial crisis and potential regulatory reforms.

II. Data

Our data on credit ratings come from Lehman Brothers Bond Database (LBBD), which 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 companies—a subset of issuers included in the LBBD 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, LBBD covers 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 rating 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 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, 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, 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 fairly accurate.

Our main dependent variable is credit rating. In the LBBD 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.

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 book 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. $SP500_{it}$ is an indicator variable equal to one if the stock is included in S&P 500 index and zero otherwise.

III. Results

A. OLS Regressions

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,* and *SP500*. We additionally include year-fixed effects. Standard errors are clustered at the firm and year groupings.

The summary statistics for these regressions (time-series averages of cross-sectional means, medians, and standard deviations) are reported in Panel A of 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 regression results are presented in Panel B of Table 3. We first present the results for the model without year-fixed effects: in column (1) with *LNASSETS*, *LNBM*, *MOMENTUM*, *LEVERAGE*, and *TANGIBILITY* as control variables, and additionally with *DD*, *VOLATILITY*, 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 0.051 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 0.051. 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 columns (3), and (4), we additionally include 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, but the economic magnitude of the effect drops to about 0.76-0.88 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, 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 year-fixed effects are included.

Of course, as we explained in the Introduction, 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 on the subsample of firms with 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 result 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. Notably, in the description of our results we side-step some of the more nuanced aspects of the experiment's validation 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. For any missing details, the reader is encouraged to consult the referenced study.

B. 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.[3]

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.

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

[3] 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.

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

Table 4 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 10.57 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 3.

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.[4]

The results for the effect on rating using a regression approach outlined in equation (1) are presented in Panel A of Table 5. 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 and year-fixed effects. The coefficient of *AFTER×AFFECTED* is -0.826, which is significant at the 5% level. In column (2), we additionally include *VOLATILITY*, *SP500*, and *DD*, which we shortly define as *Controls2*. The coefficient of interest is -0.890 and the statistical 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.[5] The coefficient of the interaction term in this baseline specification equals -0.392 and it is statistically significant at the 10% level of significance. The coefficient increases slightly, to -0.463, 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

[4] We have also experimented with clustering along many other dimension, including firm/year, merger/year and firm. The results we report here produced the most conservative standard errors.
[5] In the regression model with rating as dependent variable, coverage is part of *Controls1*.

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.

C. Cut by Analyst Coverage

We next test a key auxiliary prediction that will further buttress our identification strategy. We check whether the competition effect is more pronounced for companies with smaller initial analyst coverage. The idea is that the more analysts cover a stock, the less the loss of an additional analyst matters, akin to the Cournot view of competition. For instance, in the independence rationale of Gentzkow and Shapiro, when there are already many analysts, losing one would not change much the likelihood of drawing an independent analyst. In contrast, when there are a few analysts to begin with, losing one analyst could really affect the likelihood of getting an independent supplier of information.

However, note that if collusion is possible, then we might expect a nonlinear relationship between bias and coverage. Suppose that collusion is easier when there are only a few analysts. Under this scenario, going from one to two analysts may not have an effect because the two can collude. And we might find more of an effect when going from five to six analysts if the sixth analyst does not collude. With collusion, it might be that we expect the biggest effect for stocks covered by a moderate number of analysts–that is, an inverted u-shape with the effect being the biggest for medium coverage stocks.

We examine this issue in Panel B of Table 5 using the same DID framework as before with a full set of controls, *Controls2* and merger and year-fixed effects. We divide initial coverage into three groups: less than 5 analysts, between 5 and 19 analysts, and greater than or equal 20 analysts. We expect and find that the effect is significantly smaller when there are a lot of analysts covering. The effect is greatest for the first group (less than 5 analysts). Here, the mean credit rating increases by 1.366 and the effect is significant at the

10% level of significance. The next largest effect is for the second group (greater than or equal 5 and less than 20): The mean credit rating increases by 0.430. The result is significant at the 1% level of significance. Finally, the effect is much smaller for the highest-coverage group: The mean credit rating increases by 0.165 and the estimate is statistically insignificant. In sum, the evidence is remarkably comforting as it conforms well to our priors on competition being more important when there are fewer analysts around. This result reassures us that our estimation is a sensible one.[6][7]

D. Cut by Distance to Default

In our second auxiliary test, we 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 test this hypothesis using the same DID framework as before with a full set of controls, *Controls1*, *Controls2*, and merger 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).

[6] The results are not affected by a particular cut-off level for the number of analysts. The results are generally declining in a nonlinear way with an increase of coverage.

[7] As a robustness test, we have looked at the effect of stock market capitalization and S&P 500 index participation on our results. To the extent that small stocks and stocks that are not part of the index are less covered by markets, we should expect the effect of change in analyst coverage to be stronger among such firms because the change in coverage is a relatively larger change in the aggregate amount of information and the disciplinary power. This is indeed what we find: The effect for smaller stocks is about twice as large as the effect for small stocks, though both are statistically significant. The results are available upon request.

Our results, presented in Table 6, 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.

E. Robustness Checks

In this section, we provide a number of additional robustness tests that jointly assure that our results are not spurious.

First, 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 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 5 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 7 (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 7. Like before, 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. 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.

Third, another way in which we can ensure the validity of our experiment is to 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 Panel A of Table 8 show that none of the important observables is significantly affected by the merger event. These results are comforting as they confirm the validity of our matching procedure.

Fourth, 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 reexamine our evidence by focusing on stocks that are covered by one of the merging houses, but not by both. We show in Panel B of Table 8 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 C of Table 8, 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.

F. Yields

Our results so far indicate the average increase of about 0.5 rating notch as a result of a decrease of one analyst in stock level coverage as a result of a merger. To provide the more precise magnitude of the effect for the bond investor we want to look at bond yields associated with the ratings on the idea that higher ratings are associated with lower bond yields. Our exercise is also a test to see whether investors can see through the optimistic ratings due to lack of market disciplining forces.

We again invoke the same DID framework as before with a full set of controls, *Controls1*, *Controls2*, and merger and year-fixed effects. Our dependent variable now is *YIELD*, which is the average bond yield of a given stock in a one-year period. The results are presented in column (1) of Table 9. We find that bond yields are lower by around 23bps for treated firms. The economic effect is similar to what one gets from moving up a half-rating notch for a firm with a rating of BBB. We can interpret this result as measuring the economic effect of rating bias from the point of view of bond investors.

We also examine the robustness of the yield result when we condition our sample on the initial analyst coverage. The results are presented in columns (2)-(4). As for credit ratings, we find the effect is strongest for the low-coverage sample and similar and smaller for medium and high-coverage groups. The observed effect for the low-coverage sample is an economically significant 71 basis point reduction in yield, though this result is only statistically significant at the 10% level.

In sum, the causal effect of analyst coverage on ratings has its counterpart in yield effect. It appears that bond investors by and large incorporate the rating effect in their bond pricing.

G. Further Analyses

Controlling for Equity Analysts' Attributes

Hong and Kacperczyk (2010) show that competition among security analysts reduces bias. To the extent that these biases carry over to bond markets, we would expect a herding-like effect manifested in the behavior of credit rating agencies. To address this possibility, we control for average stock bias coming from equity analysts in our rating regressions in Table 5. *Bias* is defined as the difference between analyst's earnings forecast and the actual earnings. We aggregate the bias across all analysts covering a given stock and scale it by the past stock price.

The results, presented in column (1) of Table 10, suggest that the bias effect cannot explain the direct effect of reduced analyst coverage. While it is true that equity analysts' optimism translates into greater optimism among agencies, the coefficient of the interaction term between merger and event indicators remains negative and statistically significant.

Similarly, we test whether accuracy of equity analysts and the timing of their forecasts relative to the earnings date can explain our coverage effects. *Accuracy* is defined as an absolute value of the analyst forecast error, aggregated across all analysts covering a given stock. *Timing* is the distance in days between analyst forecast and the release of the earnings, aggregated at the stock level. The results in columns (2) and (3) indicate the two variables cannot explain the rating optimism. Finally, when included jointly in the regression all three variables do not take away much off our baseline result.

Bond Analyst Coverage

In another test, we analyze the impact of bond analyst coverage on credit ratings. To the extent that bond analysts cover the same companies, we should expect a similar competitive pressure to take place as well. To this end, we hand-collect reports about U.S. corporate firms with bond securities for which bond trading data from the Trade Reporting and Compliance Engine (TRACE) database are available. 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 3.

In Table 11, we present the results from the estimation of *RATING* on *BOND COVERAGE* and a set of similar controls as in Table 3. In columns (1) and (2), we present the results for the model without year-fixed effects. The coefficient of *BOND COVERAGE* is positive and statistically significant at the 1% of significance. Similarly, in columns (3) and (4), we present the results for the model with year-fixed effects. The results are qualitatively the same, but their magnitudes are slightly smaller.

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.

Monitoring Mechanism

Finally, one could try to explain the role of competition in credit ratings in terms of monitoring mechanism. In particular, analyst coverage could be interpreted as a source of monitoring force. In this case, the increase in coverage would lead to better monitoring, which would mean better firm governance or less uncertainty about firm prospects. To the extent that better governance or less uncertainty is positive news about a company's prospects we should expect the positive revision of the rating. But, we find that the increase in coverage actually leads to a decrease in rating, which is inconsistent with the monitoring story, but is consistent with our disciplining force story.

In sum, while we cannot rule out the possibility of the monitoring mechanism being at play, we can say the effect due to the disciplining force dominates the effect due to monitoring.

IV. Conclusion

The study of how conflicts of interest influence credit rating agency bias has never been more important than in the aftermath of the Financial Crisis of 2008. The crisis revealed the importance of these credit ratings in the functioning of financial markets and called into question their role, especially in light of regulatory reforms. One particular worry is whether the lack of competitive effects associated with the ratings game being dominated by three firms exacerbated issues. But this question is a challenging one since there is little variation in competitive effects to speak of when looking at the ratings industry.

To this end, our basic idea is to broaden the scope of competitive pressure on CRAs by asking whether security analysts might serve such a disciplining role in the context of the ratings of corporate bonds. Our hypothesis is that competition from security analysts disciplines the credit rating agencies. We use the Hong and Kacperczyk (2010)'s brokerage house mergers quasi-experiment to shock analyst coverage so as to identify the causal effect of analyst coverage or competition on credit ratings. We find that a 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 effect is coming largely from firms with low initial analyst coverage to begin with and hence where the loss of one analyst is a sizeable percentage drop in disciplining competitors.

Our study might have also implications for how to think about the failure of the CRAs in CDOs. Consistent with our findings, there was little disciplining of security analysts for these CDOs since they were all structured finance products. The natural competitive force would be security analyst 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. Our study suggests that regulatory reforms of CRAs take into account and encourage these potential spillovers.

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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). $ASSETS_i$ 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: Credit Rating and Coverage (OLS)

In Panel A, 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*. $COVERAGE_{it}$ 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*. **In Panel B**, the dependent variable is *RATING*. Independent variables include $COVERAGE_{i,t}$, $LNASSETS_{i,t}$, $LNBM_{i,t}$, $MOMENTUM_{i,t}$, $LEVERAGE_{i,t}$, $TANGIBILITY_{i,t}$, $DD_{i,t}$, $VOLATILITY_{it}$, and $SP500_{it}$. Regressions in columns (3) and (4) include year-fixed effects. Standard errors (in parentheses) are clustered at the firm and year groupings. ***, **, * denotes 1%, 5%, and 10% statistical significance.

Panel A: Summary Statistics

	Mean	Median	St. dev.
Rating	11.93	12.33	4.19
Coverage	17.91	16.00	10.48
Ln(Assets)	8.39	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	0.41	0.39	0.23
Volatility	0.34	0.31	0.16
DD	0.14	0.11	0.12
Tangibility	0.39	0.35	0.28
Bond Coverage	1.70	1	1.16

Panel B: Regression Evidence

VARIABLES	(1) Rating	(2) Rating	(3) Rating	(4) Rating
Coverage	0.048***	0.051***	0.037***	0.032***
	(0.015)	(0.015)	(0.010)	(0.009)
Ln(Assets)	-1.760***	-1.560***	-1.680***	-1.065***
	(0.092)	(0.089)	(0.093)	(0.107)
Ln(BM)	0.023	0.122	-0.192	-0.072
	(0.184)	(0.195)	(0.128)	(0.092)
Momentum	3.636	4.535	-1.639	-0.360
	(4.064)	(3.683)	(3.005)	(1.514)
Leverage	5.741***	4.001***	6.023***	4.262***
	(0.515)	(1.065)	(0.486)	(0.778)
Tangibility	-3.578***	-3.045***	-3.520***	-2.117***
	(0.543)	(0.494)	(0.559)	(0.452)
DD		2.436		-0.688
		(2.171)		(1.596)
Volatility		3.188*		10.924***
		(1.882)		(0.984)
SP500		-0.637***		-0.362**
		(0.209)		(0.183)
Constant	11.935***	11.935***	11.470***	12.691***
	(0.367)	(0.400)	(0.124)	(0.173)
Year-Fixed Effects	No	No	Yes	Yes
Observations	11,901	11,901	11,901	11,901
R-squared	0.325	0.353	0.459	0.549

Table 4: Summary Statistics (IV)

We consider all stocks covered by two merging brokerage houses around the one-year merger event window. $COVERAGE_{it}$ 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 $LEVERAGE \times VOLATILITY$. $SP500_{it}$ is an indicator variable equal to one if stock *i* is included in the S&P500 index.

	Mean	Median	St. dev.
Rating	10.57	10.00	4.25
Coverage	15.01	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

Table 5: 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. *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 year *t*; *SP500* is an indicator variable equal to one if a stock is included in the S&P500 index. *Controls1* includes a set of indicator variables derived from assigning each observation along assets, book-to-market ratio, momentum, leverage, and tangibility into decile portfolios. *Controls2* additionally includes similar indictor variables for volatility and distance to default, and an indicator variable for S&P 500 index inclusion. **Panel B** presents our results by cuts on initial coverage. There are three groups: low coverage (<5), medium coverage (>=5 and <20) and high coverage (>=20). All regressions include *Controls1* and Controls2, as well as merger-fixed effects and year-fixed effects. 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	1.223***	0.868***	-0.722***	-1.090***
	(0.158)	(0.169)	(0.023)	(0.032)
Affected	4.984***	4.818***	0.340	0.445**
	(0.464)	(0.445)	(0.200)	(0.152)
After*Affected	-0.826**	-0.890***	-0.392*	-0.463**
	(0.281)	(0.295)	(0.205)	(0.166)
Controls1	Yes	Yes	Yes	Yes
Controls2	No	Yes	No	Yes
Constant	-15.643***	-20.335***	25.791***	19.773***
	(1.274)	(1.726)	(0.518)	(0.712)
Merger-Fixed Effects	Yes	Yes	Yes	Yes
Year-Fixed Effects	Yes	Yes	Yes	Yes
Observations	15,658	15,658	15,658	15,658
R-squared	0.567	0.593	0.567	0.648

Panel B: Conditioning on Initial Coverage

VARIABLES	(1) Low Coverage	(2) Medium Coverage	(3) High Coverage
After	-0.834***	-1.118***	-1.222***
	(0.059)	(0.030)	(0.048)
Affected	1.006	0.353***	0.140
	(1.368)	(0.080)	(0.210)
After*Affected	-1.366*	-0.430***	-0.165
	(0.706)	(0.142)	(0.162)
Constant	18.748***	19.152***	16.695***
	(0.958)	(0.724)	(1.082)
Controls1	Yes	Yes	Yes
Controls2	Yes	Yes	Yes
Merger-Fixed Effects	Yes	Yes	Yes
Year-Fixed Effects	Yes	Yes	Yes
Observations	1654	9508	4496
R-squared	0.559	0.613	0.605

Table 6: Conditioning on Probability of Default

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 $LEVERAGE \times VOLATILITY$; $VOLATILITY_{it}$ is the variance of daily (simple, raw) returns of stock *i* during year *t*; *SP500* is an indicator variable equal to one if a stock is included in the S&P500 index. *Controls1* includes a set of indicator variables derived from assigning each observation along assets, book-to-market ratio, momentum, leverage, and tangibility into decile portfolios. *Controls2* additionally includes similar indictor variables for volatility and distance to default, and an indicator variable for S&P 500 index inclusion. All regressions include merger-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	-0.326***	-0.684***	-0.943***	-1.193***	-1.387***	-0.962***
	(0.021)	(0.053)	(0.031)	(0.048)	(0.039)	(0.026)
Affected	0.418*	0.294**	0.237	0.455***	0.283	0.446***
	(0.208)	(0.102)	(0.141)	(0.143)	(0.161)	(0.135)
After*Affected	-0.313*	-0.519**	-0.183	-0.456**	-0.113	-0.468**
	(0.165)	(0.178)	(0.178)	(0.167)	(0.209)	(0.168)
Constant	11.560***	18.438***	20.400***	19.564***	14.833***	19.318***
	(0.414)	(0.542)	(0.944)	(0.755)	(1.097)	(0.804)
Controls1	Yes	Yes	Yes	Yes	Yes	Yes
Controls2	Yes	Yes	Yes	Yes	Yes	Yes
Merger-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6573	9085	6397	9261	3402	7922
R-squared	0.326	0.682	0.603	0.643	0.707	0.626

Table 7: 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 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 8: Validity of Experiment

In Panel A, we provide the DID estimator for various corporate characteristics, including Ln(Assets), Ln(BM), Momentum, Leverage, and Distance to Default (DD). **In Panel B**, the treatment sample is constructed based on the stocks that are covered by one but not both merging houses. **In Panel C**, 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 Firm Characteristics

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)

Panel B: Change in Rating for Non-overlapping Stocks

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

Panel C: 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 9: The Effect on Bond Yields

The dependent variable is the average firm-level bond yield (*YIELD*) calculated as the simple average of average monthly bond yields. 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. *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 year *t*; *SP500* is an indicator variable equal to one if a stock is included in the S&P500 index. *Controls1* includes a set of indicator variables derived from assigning each observation along assets, book-to-market ratio, momentum, leverage, and tangibility into decile portfolios. *Controls2* additionally includes similar indictor variables for volatility and distance to default, and an indicator variable for S&P 500 index inclusion. Columns (2)-(4) present our results by cuts on initial coverage. There are three groups: low coverage (<5), medium coverage (>=5 and <20) and high coverage (>=20). All regressions include *Controls1* and Controls2, as well as merger-fixed effects, and year-fixed effects. Standard errors (in parentheses) are clustered at the merger grouping. ***, **, * denotes 1%, 5%, and 10% statistical significance.

	(1)	(2)	(3)	(4)
VARIABLES	Yield	Yield	Yield	Yield
	All	Low Coverage	Medium Coverage	High Coverage
After	-0.049	0.616***	-0.117***	-0.160***
	(0.033)	(0.067)	(0.034)	(0.049)
Affected	0.129**	0.132	0.061	0.007
	(0.055)	(0.261)	(0.054)	(0.104)
After*Affected	-0.232**	-0.712*	-0.093	-0.120
	(0.106)	(0.405)	(0.067)	(0.089)
Controls1	Yes	Yes	Yes	Yes
Controls2	Yes	Yes	Yes	Yes
Constant	9.800***	11.828***	9.778***	7.574***
	(0.211)	(0.971)	(0.228)	(0.269)
Merger-Fixed Effects	Yes	Yes	Yes	Yes
Year-Fixed Effects	Yes	Yes	Yes	Yes
Observations	16,251	1811	9908	4532
R-squared	0.530	0.524	0.539	0.416

Table 10: Controlling for Equity Analysts' Accuracy, Bias, and Timing

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. *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 year *t*; *SP500* is an indicator variable equal to one if a stock is included in the S&P500 index. *Controls1* includes a set of indicator variables derived from assigning each observation along assets, book-to-market ratio, momentum, leverage, and tangibility into decile portfolios. *Controls2* additionally includes similar indictor variables for volatility and distance to default, and an indicator variable for S&P 500 index inclusion. In columns (1)-(3) we iteratively include measures of stock-level equity analysts' bias, accuracy, and timing of their forecasts. All regressions include merger-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) Rating	(2) Rating	(3) Rating	(4) Rating
After	-1.085***	-1.089***	-1.090***	-1.070***
	(0.034)	(0.031)	(0.032)	(0.033)
Affected	0.439**	0.445**	0.445**	0.422**
	(0.153)	(0.151)	(0.153)	(0.153)
After*Affected	-0.459**	-0.457**	-0.462**	-0.433**
	(0.168)	(0.168)	(0.165)	(0.181)
Bias	-3.075**			-11.143***
	(1.262)			(1.239)
Accuracy		4.683***		12.801***
		(1.066)		(0.820)
Timing			0.000	0.000
			(0.000)	(0.000)
Constant	19.938***	19.616***	19.768***	19.934***
	(0.745)	(0.757)	(0.746)	(0.806)
Controls1	Yes	Yes	Yes	Yes
Controls2	Yes	Yes	Yes	Yes
Merger-Fixed Effects	Yes	Yes	Yes	Yes
Year-Fixed Effects	Yes	Yes	Yes	Yes
Observations	15,658	15,658	15,658	15,658
R-squared	0.649	0.649	0.648	0.654

Table 11: Ratings and Bond Coverage

The dependent variable is *RATING*—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*. *BOND COVERAGE$_{it}$* is a measure of bond analyst coverage, defined as the number of bond 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*. Regressions in columns (3) and (4) include year-fixed effects. Standard errors (in parentheses) are clustered at the merger grouping. ***, **, * denotes 1%, 5%, and 10% statistical significance.

VARIABLES	(1) Rating	(2) Rating	(3) Rating	(4) Rating
Bond Coverage	0.522***	0.398***	0.323***	0.202**
	(0.091)	(0.067)	(0.090)	(0.096)
Ln(Assets)	-1.917***	-1.516***	-1.910***	-1.434***
	(0.139)	(0.142)	(0.135)	(0.131)
Ln(BM)	-0.196	-0.115	-0.257	-0.109
	(0.188)	(0.173)	(0.211)	(0.150)
Momentum	0.287	1.802	-2.151	-1.515
	(5.318)	(3.687)	(5.295)	(3.001)
Leverage	4.973***	4.551***	5.368***	4.288***
	(0.554)	(1.096)	(0.500)	(0.915)
Tangibility	-0.603	-0.181	-0.434	0.200
	(0.591)	(0.544)	(0.549)	(0.442)
DD		-1.401		-0.838
		(1.451)		(1.807)
Volatility		4.776***		6.437***
		(1.541)		(1.113)
SP500		-1.141***		-1.063***
		(0.440)		(0.401)
Constant	12.649***	12.037***	13.444***	13.472***
	(0.428)	(0.468)	(0.200)	(0.294)
Year-Fixed Effects	No	No	Yes	Yes
Observations	1315	1315	1315	1315
R-squared	0.485	0.527	0.572	0.631