Do Credit Analysts Matter? The Effect of Analysts on Ratings, Prices, and Corporate Decisions[*,#]

Cesare Fracassi
University of Texas – Austin

Stefan Petry
University of Melbourne

Geoffrey Tate
University of North Carolina – Chapel Hill

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Abstract

We find evidence of systematic optimism and pessimism among credit analysts, comparing contemporaneous ratings of the same firm across rating agencies. These biases carry through to debt prices and negatively predict future changes in credit spreads, consistent with mispricing. Moreover, they affect corporate policies: firms covered by more pessimistic analysts issue less debt, use more equity financing, and experience slower revenue growth. We find that MBAs provide higher quality ratings; however, optimism increases and accuracy decreases with tenure covering the firm. Our analysis uncovers a novel mechanism through which debt prices become distorted and demonstrates its effect on corporate decisions.

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The recent financial crisis raised concerns about the efficiency of pricing in securities markets, and particularly about the information content of credit ratings. Ratings play an important role in shaping investors' expectations. Not only do they ostensibly provide public signals of credit quality, they also directly affect the clientele for debt instruments.[1] Yet, little is known about the effect of the credit analysts covering a firm on the ratings of its debt securities. Investor sentiment appears to affect pricing in equity markets (Baker and Wurgler, 2006 and 2007). If ratings reflect the individual biases of the analysts who produce them, then the widespread use of ratings by market participants can be a similar source of distortions in debt prices. These distortions in turn can affect the incentives of firms seeking external financing.

We construct a novel dataset that links long-term corporate issuer ratings from all three major rating agencies to the individual analysts responsible for each rating. We find evidence of significant analyst-specific effects on firms' long-term credit ratings that cannot be explained by firm, time, or agency effects. These biases carry through to the cost of debt capital, significantly affecting not only the choice between debt and equity, but also real growth rates.

Though rating agencies' objective is to measure issuers' creditworthiness, the rating process provides opportunities for the discretion of individual analysts to affect ratings. Upon receiving a request for a rating from a corporate issuer, the rating agency assigns a small team of analysts who work in the sector to cover the firm. Since individual analysts typically cover between 10 and 20 companies across 2 to 3 sectors, there is substantial variation across companies in the composition of these teams. After a pre-evaluation of the firm, the analysts meet with the firm's management to review relevant information. They then evaluate the information and propose a rating to a rating committee, which votes on the rating. Before issuing a press release announcing the rating, the agency notifies the firm of the rating and provides a rationale.[2] Thus, analysts not only have substantial discretion in the evaluation of the firm and

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[1] Basel II uses ratings to calculate minimum capital standards for bank investment assets. Moreover, pension funds are typically prohibited from investing in assets that do not carry investment-grade ratings.

[2] See, e.g., https://www.spratings.com/about/about-credit-ratings/ratings-process.html for a description of the process at Standard and Poor's.

power over the process, but also multiple opportunities for direct communication with management. A firm may be assigned analysts who tend to be pessimistic or optimistic. In addition, repeated interactions with management can create the potential for conflicts of interest or bias arising from familiarity with the rated firm.[3] The resulting differences in ratings can affect the cost of debt capital if there are limits to arbitrage in debt markets. Moreover, changes in the cost of debt can affect the mix of financing obtained by the firm or even investment and growth if there are constraints on the firm's ability to substitute equity or cash for debt capital.

We test this hypothesis in two main steps. First, we measure the fixed effects of individual analysts on long-term credit ratings. To correct for nonrandom matching of analysts to the firms they cover, we include fixed effects for each firm-quarter in our regressions. Thus, we compare each analyst's rating only to peers who rate the same company at the same time and average across the firm-quarters in which we see each analyst. As a result, our estimates of analyst effects are orthogonal to differences in observed firm fundamentals. We also separate the effect of individual analysts from the effect of different agencies for which they work by including fixed effects for each of the three major rating agencies. Alternatively, we allow for quarter-by-quarter differences in how each agency rates different sectors or for fixed agency effects on the rating of each sample firm. In all cases, we find significant analyst-specific effects on ratings. The estimates are also economically meaningful: analyst fixed effects explain 26.81% to 30.24% of the contemporaneous variation in ratings across agencies covering the same firm, an order of magnitude larger than the explanatory power of agency fixed effects. Moreover, they are difficult to explain by differences in the quality of private information available to analysts covering the same firms, since private information is likely to be good for some firms covered by a given analyst, but bad for others. Instead, the fixed effects capture a systematic tendency for analysts to be relatively more optimistic or pessimistic than peers across the firms that they rate.

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[3] Rating agencies were exempted from the provisions of Regulation FD prohibiting disclosure of private information to select individuals or groups, recognizing the exchange of information between agencies and issuers. Though, this exemption ended with the passage of Dodd-Frank (Purda, 2011), the practical effect on the relationships between agencies and rated firms remains unclear.

Second, we measure the degree to which these analyst effects carry through to firms' costs of capital, financial policies, and growth rates. To avoid the possibility of reverse causality, we reestimate each analyst's fixed effect on ratings quarter-by-quarter on a backward-looking sample using the specification described above. We then decompose the firm's observed credit rating into the portion determined by the fixed effects of the analysts covering the firm in that quarter and the residual, "de-biased" rating. We find that both portions of the credit rating significantly predict spreads on the firm's outstanding debt. In our baseline specification, a one notch increment to de-biased ratings changes spreads by 49 basis points while a one notch increment to the analyst-driven portion of ratings changes spreads by 35 basis points. The difference is statistically significant, suggesting that the market understands that persistent analyst-driven biases in ratings are less informative about credit quality than the remainder of ratings. Consistent with this interpretation, we find that the market fully adjusts for analyst-driven biases in ratings when pricing highly-rated bonds (the estimate on analyst effects is zero), but makes no significant adjustment among lower quality bonds, for which arbitrage is likely to be difficult due to trading restrictions faced by institutional investors. Moreover, we find that systematic analyst optimism (pessimism) in ratings predicts an increase (decrease) in spreads over the following quarters, suggesting that the portion of analyst effects that is priced does not reflect information. We find similar pricing effects among new issues of public debt: a one notch increment to the analyst-driven portion of ratings changes the offering yield-to-maturity by 25 basis points, compared to 29 basis points for a one notch increment to de-biased ratings.

Given the effect of analyst-specific biases on debt prices, we test whether firms adjust their corporate policies in response. We find a significant effect of the analyst-driven portion of ratings on the odds of debt issuance, conditional on raising external finance: a one notch increase in relative analyst pessimism decreases the odds of debt issuance by 40%. Here, the response to the analyst-driven portion of ratings is more than three times as large as the response to the de-biased portion of ratings, suggesting that firms find debt markets particularly unattractive when unfavorable credit ratings – and the associated pricing effects – are driven by systematic analyst

biases. We find similar effects when we look at unconditional financing choices: analyst pessimism is associated with more frequent debt retirement and equity issuance as well as less frequent equity repurchases. Moreover, the influence of analysts is not contained to financial policies, but also has real effects: we estimate a significant one percentage point lower growth rate in sales for a one notch change in ratings due to analyst pessimism.

As a final step, we link differences in rating levels, rating dispersion, and rating accuracy to observable analyst traits. Using web sources, we gather demographic information for roughly two thirds of the analysts in our sample, including age, gender, and educational background. Mirroring our prior analysis, we test whether these characteristics predict differences in rating outcomes using a fixed effects model that compares analysts across agencies rating the same firm in the same quarter. We find that analysts with MBAs and with longer tenure in the rating agency provide less optimistic and more accurate ratings, consistent with higher skill or less bias. They also deviate more from other analysts in their assessments of covered firms. We also uncover a dark side to long-term matches between firms and credit analysts: ratings become more optimistic and less accurate as the analyst's tenure covering the firm increases. Thus, our results provide a potential mechanism for "sluggishness" in downward ratings adjustments, a feature of ratings that generated attention from policymakers in the wake of the Enron and Worldcom scandals and the recent Lehman Brothers bankruptcy (White, 2010). Finally, we find that the impact of analyst biases is strongest among firms that are likely to face financing constraints due to information frictions: small firms, young firms, diversified firms, firms with low analyst coverage, and firms with high dispersion in earnings forecasts. Given the negative effect of long tenure, our results suggest that appropriate regulation – for example mandatory analyst rotation – may improve ratings quality and, thereby, ease financing frictions.

Our analysis provides novel evidence on how firms respond to market inefficiencies. Corporate managers believe they can time markets when they issue new securities (Graham and Harvey, 2001). Consistent with this belief, stock prices predictably underperform following new issues (Loughran and Ritter, 1995). The few existing studies that test for timing in debt markets

typically exploit time series variation in aggregate issuance (e.g., Baker, Greenwood, and Wurgler, 2003). We take a different approach, exploiting cross-sectional variation in ratings due to analyst coverage and showing that firms respond to predictable variation in debt prices that is plausibly unrelated to firm fundamentals. Thus, we provide a novel explanation for previous resulting linking credit ratings with financing and investment choices (Baghai, Servaes, and Tamayo, forthcoming; Chernenko and Sunderam, 2012; Kisgen, 2006). Our approach is conceptually similar to DellaVigna and Pollet (2013) who measure managerial responses to mispricing of predictable demographic shifts in equity markets.

Our analysis also parallels a large literature that studies the impact of sell-side equity analysts on recommendations, forecasts, and firm value. Prior work identifies several analyst characteristics that correlate with recommendation quality, including experience and attention (Clement, 1999), past accuracy (Clement and Tse, 2005), gender (Kumar, 2010), and "all-star status" (Clarke et al, 2007; Fang and Yasuda, 2009). Other studies identify effects of competition (Hong and Kacperczyk, 2010) or conflicts of interest (Lin and McNichols, 1998; Michaely and Womack, 1999) on the quality of equity analyst recommendations. Though our results complement the findings in these papers, ratings analysts have different objectives from sell-side equity analysts. Ratings analysts assess the creditworthiness of corporate borrowers; sell-side equity analysts, instead, provide portfolio recommendations to equity investors. Thus, the recommendations of the latter group are less likely to affect credit markets. There has been considerably less research on ratings analysts, even though the channels through which ratings analysts can influence real corporate decisions appear more direct than the corresponding channels for sell-side equity analysts.[4] For example, firms typically solicit input from the rating agencies on how the financing of major projects like acquisitions will impact their credit ratings.

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[4] A recent exception is Cornaggia, Cornaggia and Xia (2014) who show that analysts who leave a rating agency to work for a firm they previously covered tend to issue more favorable ratings about their future employer prior to the transition. Their analysis exploits a recent law change that requires the disclosure of such relationships and, as a result, cannot address the effect of the larger set of analysts who do not join covered firms.

The remainder of the paper is organized as follows. In Section I, we describe our credit analyst data and the construction of the samples used in our empirical analysis. In Section II, we measure the systematic effects of individual analysts on ratings. In Section III, we link analyst biases to debt prices and corporate financing policies. In Section IV, we explore the mechanisms through which analysts affect ratings. Finally, Section V concludes.

I.   Data

The core of our dataset is credit rating information from all three major ratings agencies – Fitch, Moody's, and Standard and Poor's – which we obtain from Thomson CreditViews. The data provide announcements of all rating upgrades, downgrades and affirmations as well as changes in outlooks and watches for all U.S. issuers and long- and short-term issues. Because data are sparse prior to 2000, we restrict our sample to announcements between 2000 and 2011. To measure differences in firms' abilities to access additional debt capital, we focus on long-term issuer ratings, which ostensibly measure the ability to honor senior unsecured financial obligations. We also restrict the sample to firms with available cusips that we can match to Compustat (for quarterly accounting data) and CRSP (for stock price data). We match each announcement to a ratings report that includes the name(s) of the analyst(s) covering the firm using the Moody's and Fitch websites and Standard and Poor's Global Credit Portal.[5] Our final sample consists of 44,829 announcements on 1,721 firms, of which 571 belonged to the S&P 500 index at some point during the sample period.[6]

From this data, we construct a quarterly panel dataset of long-term issuer ratings from each of the three rating agencies by taking the rating and analyst names from the most recent report at the end of each firm-quarter. To minimize measurement error in the identity of the analysts covering the firm, we do not assign analysts to quarters that end after the date of the final report in which we observe the analyst covering the firm. We also use Standard and Poor's

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[5] We are able to find the report corresponding to the announcement in roughly 73% of cases.
[6] See the Appendix for additional details on the announcements including breakouts by type and agency.

long-term issuer ratings retrieved from Compustat to verify the accuracy of our data.[7] We find that the ratings agree in roughly 96.5% of cases. Moreover, in the small number of cases in which they disagree, it is often due to differences in when a rating change is recognized. We use the exact date of the announcement (relative to the end date of the quarter) to determine the timing of changes. We also use S&P data from Compustat to measure the frequency of unsolicited ratings among our sample firms. Though we do not directly observe this information in CreditViews, unsolicited issuer ratings are generally rare in the United States: we find only 2 unsolicited S&P long-term issuer ratings out of 27,342 quarterly observations. In Panel A of Table I, we report summary statistics of the data at the firm-quarter-agency level. The median issuer rating in our sample is BB+, translating all ratings to the S&P rating scale. There are some cross-sectional differences across agencies: the median Fitch rating is BBB, the median S&P rating BB+, and the median Moody's rating BB-.

Our analysis relies on comparisons of ratings across agencies: we observe ratings by multiple agencies in 42% of firm-quarters and, among those observations, we observe split ratings 51% of the time (or in 7,916 distinct firm-quarters). In Appendix Table A-II, we present the distribution of ratings for the subsamples of firm-quarters with and without split ratings. On the split ratings sample, we present separate distributions of the minimum and maximum rating by firm-quarter. Overall, the distributions of ratings are similar for firms with and without split ratings, though firms with split ratings appear slightly worse on average than firms about which the agencies agree. In the event of a split rating, the average difference in ratings across agencies is 1.28 notches. In the Online Appendix, we also provide a breakout of the summary statistics from Table I for firms with and without split ratings. Generally, the firms look quite similar. For example the mean natural logarithm of sales is 6.68 and 6.67 in the two samples while mean leverage is 0.382 and 0.342. Nevertheless, existing research emphasizes the opacity of the assets as a determinant of split ratings (Livingston, Naranjo, and Zhou, 2007; Morgan, 2002). If there is

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[7] It is impossible to do a similar exercise for Fitch and Moody's ratings since we do not have an independent source of ratings information against which to compare our dataset.

less room for analyst discretion in firms without split ratings, then our results may extend less readily beyond the set of split-rated firms. We address this possibility explicitly in Section III.

We use our data to measure a number of analyst traits. We use first names (and, in ambiguous cases, additional web searches) to infer analyst gender, and we construct measures of analyst tenure in the agency and covering each individual firm. We also supplement the data with hand-collected demographic information from web searches, most commonly from public LinkedIn profiles. Of the 1,072 unique analysts in our data, we are able to retrieve data for 798. We extract biographical information on age as well as the professional and educational background of the analysts. Educational background (school, degree, and degree date) is available for 638 analysts, of whom 65% have an MBA. To construct the age variable, we estimate the birth year by taking the minimum between the first year of employment minus 22 years and the first year of college minus 18 years. Finally, we construct a number of variables intended to capture variation in ratings across analysts. We measure analyst (relative) optimism by computing the difference in each firm-quarter between the analyst's rating of the firm and the average rating of the analysts in other agencies covering the firm.[8] We use our measure of optimism to construct a measure of relative rating accuracy. In firm quarter t, we measure accuracy by multiplying -1 by relative optimism by the forward change in credit spreads measured starting at time t and continuing for three years.[9] The change in credit spreads captures realized changes in the issuer's credit quality over time, while the optimism measure captures the analyst's prediction. Thus, an analyst who was more optimistic about the firm than her peers preceding a decrease in the firm's credit spread would be coded as relatively accurate (i.e., the accuracy score would be greater than 0) and the magnitude of the accuracy score would increase in the number of notches more optimistic she was ex ante as well as the decrease in the credit

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[8] We follow convention in translating ratings to a numerical scale (see, e.g., Bongaerts, Cremers, and Goetzmann, 2012). We provide the full translation in Appendix Table A-II. We negate the difference between the analyst's rating and the average when computing optimism so that higher values of the difference correspond to more favorable relative rankings. Our measure of optimism is similar to the one employed by Hong and Kubik (2003) for equity analysts.

[9] Changes in credit spreads are measured as a value-weighted average across all the firm's outstanding bond issues. See the Appendix for more details on this computation.

spread. An alternative would be to ask how well analysts predict default (i.e., accurate analysts are the ones whose ratings were relatively pessimistic preceding default). Since default is a rare event, our measure provides a natural generalization of this approach.

To link analyst biases with corporate financial policies, we use accounting and financial data from Compustat, CRSP, and SDC. We follow the approach of Leary and Roberts (2005) and Hovakimian, Opler and Titman (2001) to measure external financing episodes. We classify a firm as making a debt issue (retirement) if total debt scaled by beginning-of-quarter assets increases (decreases) by 5% in a given quarter. Similarly, equity issuance occurs if net equity issuance (sale of common and preferred stock minus purchase of common and preferred stock) scaled by assets exceeds 5%. Following Leary and Roberts (2005), we classify a 1.25% increase in net equity to assets as an equity repurchase.[10] We also obtain the yield-to-maturity for new public debt issues from the SDC database. Cash reserves are cash and short term investments scaled by assets and sales growth is the quarter over quarter percentage change in sales. Both variables are winsorized at the 1% and 99% levels to remove extreme outliers. For our analyses of credit spreads and security issuance, we construct a battery of controls, following Blume, Lim, and MacKinlay (1998) and Baghai, Servaes, and Tamayo (forthcoming). We provide complete variable definitions in Appendix Table A-I.

We also use accounting information from Compustat and equity analyst information from I/B/E/S to measure the sensitivity of firms to information frictions in our analysis of analyst traits. We measure firm size using total assets at the end of the fiscal quarter and firm age as the number of years since the firm first appeared in Compustat. We also use segment data to measure firm diversification, counting the number of segments operating in distinct Fama-French 49 industry groups. We use I/B/E/S data to gather the number of equity analysts following each firm and the dispersion in annual earnings forecasts, measured six months prior to the date of the annual earnings announcement. We measure dispersion in earnings forecasts as the standard

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[10] They motivate this choice by the observation that smaller-scale repurchase programs that would fall between the 1.25% and 5% thresholds are common in practice.

deviation of the earnings forecasts divided by their mean. In Panel A of Table I, we also provide summary statistics of the data for the subsample on which the analyst traits are available.[11] In a given firm-quarter, the average analyst is 39.5 years old and has worked for her agency for 7 years, covering the industry for 3.5 years and the firm for 2 years. The average covered firm is 29 years old, has roughly $37 billion in assets, and is covered by 11 equity analysts. Panel C presents selected pairwise correlations of the variables.

II.   Do Analysts Matter for Credit Ratings?

II.A. Empirical Specification and Identification Strategy

Our first step is to ask whether the identity of the analyst(s) covering a firm influences its credit rating after accounting for fundamentals. To answer this question, we follow an approach similar to the one used by Bertrand and Schoar (2003) to identify the effect of corporate managers on firm policies separately from firm effects. Our baseline regression specification is the following:

$$Rating_{ijt} = \alpha_{jt} + \beta_i + \boldsymbol{\gamma}_{analyst} + \epsilon_{ijt} \qquad (1)$$

$Rating_{ijt}$ is the long-term issuer rating for firm j in quarter t by rating agency i. α_{jt} is a firm-quarter fixed effect and β_i is a rating agency fixed effect. $\boldsymbol{\gamma}_{analyst}$ includes the explanatory variables of interest: dummy variables for each sample analyst that take the value 1 if the analyst covered firm j in quarter t for agency i and zero otherwise. To have sufficient variation to estimate reasonably precise effects for each analyst, we include dummies only for analysts who cover at least 5 sample firms. Even with this restriction, we retain 99% of firm-quarter-agency observations.[12]

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[11] In addition to losing observations due to analysts who are not in LinkedIn, the optimism measure requires that we observe ratings from at least two agencies in a given firm-quarter to be defined. The accuracy measures are defined on a smaller subsample due mainly to missing information on credit spreads due to bond illiquidity (see the Appendix) and attrition from measuring spreads 3 years into the future.

[12] This choice does not affect any conclusions in the paper. In a prior version of the paper, we reported all included regressions without this sample restriction.

Because we observe multiple agencies rating the same firm at the same time, our setting has identification advantages relative to the setting studied by Bertrand and Schoar (2003). In their setting, including a firm fixed effect absorbs the between firm variation and, thus, the specification relies on time-series variation within firms to identify manager effects. To control for time-varying firm effects that might confound the estimates, it is necessary to specify and define appropriate time-varying controls. In our setting, by contrast, including a firm fixed effect leaves two sources of variation: (1) time-series variation within firms and (2) cross-sectional variation across agencies covering the same firm. Instead of relying on the first source of variation for identification, we use firm-quarter fixed effects to absorb it, leaving only the variation across agencies (analysts) covering the same firm at the same point in time. This approach makes it unnecessary to specify or include any time-varying controls for firm fundamentals (e.g., leverage ratios or cash holdings), since they cannot be identified independently from the fixed effects. It also mitigates selection concerns. The matching of analysts to firms is unlikely to be random; for example, analyst teams are often organized by sector. However, the interpretation of our results is not affected by this type of matching because we identify analyst effects by comparing analysts who cover the same firm at the same time.

We identify the effect of analysts on ratings separately from the effects of their agencies in several ways. Equation (1) includes a fixed effect for each rating agency so that our estimates of $\gamma_{analyst}$ are not confounded by differences in the average ratings conferred by the three agencies. We also estimate three more stringent variations of the model. First, we allow the agency fixed effects to vary by sector s, defined using 2-digit Global Industry Classification (GIC) codes, replacing β_i with β_{is}. This specification allows for differences within and across agencies in average ratings by sector. Since we identify the analyst effects using only variation within each agency-sector pair, they are unaffected by differences across agencies in how analysts are assigned to sectors. Second, we allow the differences in how the agencies assess each sector to vary over time by including interactions of the agency-sector effects with quarter fixed effects, replacing β_i with β_{ist}. Thus, our estimates are robust to differences in the matching

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of analysts to sectors across agencies and time. Finally, we change the unit of observation from the sector to the firm, including fixed effects for each agency-firm combination, replacing β_i with β_{ij}. In this specification, we allow each agency to have a different average rating for each sample firm and identify the analyst effects using only firms that are covered by multiple analysts for the same agency at different points in time. Because we compare only analysts who cover the same firm at different times for the same agency, our estimates are unaffected by differences across agencies in how analysts are matched to firms they cover.

Though our specifications address the most compelling sources of nonrandom sorting, it is impossible to rule out with additional fixed effects the possibility that sorting is nonrandom and differs across agency-firm-quarter groupings. For example, agencies could reassign analysts within a sector to cover different firms over time depending on the performance of their ratings or current firm conditions (i.e., not randomly) and differently across the agencies. However, this kind of sorting does not appear to be a practical concern – agencies do not systematically measure and track the accuracy of ratings by analysts. Moreover, analyst-firm matches appear to be quite stable over time, perhaps because agencies perceive a cost from sacrificing match-specific expertise. [13]

Our null hypothesis is that the coefficients on the individual analyst effects are jointly equal to zero. That is, credit ratings are fully explained by the macroeconomic, firm, and agency factors captured by the firm-quarter and agency fixed effects (or, each individual analyst is unbiased). Recent research raises concerns about inferences from standard Wald tests in this type of specification (Fee, Hadlock, and Pierce, 2011). In particular, the dependent variable in our regression is highly persistent over time. Thus, analyst fixed effects, because they are also quite persistent, may appear significant in our regression even if the null is satisfied. Moreover, such a test requires an assumption that the idiosyncratic errors are normally distributed (Wooldridge,

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[13] To assess the importance of this potential sorting mechanism, we spoke with credit analysts and executives from two of the major agencies who provided information on the process by which analysts are assigned to cover firms and how they are evaluated over time. Within a sector, the most common factor that determines the assignment of a new firm to an analyst appears to be available "bandwidth" of the analyst. Thus, it is reasonable to consider the matching of analysts to firms to be essentially random within agency-sector pairs.

2002).[14] To address these econometric concerns, we assess statistical significance using a resampling approach to test our hypotheses. Since our interest is in the F-statistic for a joint test of the significance of the analyst fixed effects, we use a block bootstrap procedure to construct the empirical distribution of the F-statistic and to assess its significance.[15] First, we identify each analyst-firm spell in the data. For example, if Analyst 1 covers GE for five consecutive quarters, this represents a single analyst-firm spell. Under our null hypothesis, the labels on these analyst spells are exchangeable. Thus, we randomly reassign sample analysts to the analyst-firm spells, requiring that each analyst still be assigned to the same number of spells as in the actual data. Notice by construction that the resulting dataset preserves the same persistence structure as the original data since the spells themselves do not vary and the dependent variable is the same. We hold the number of spells assigned to each analyst constant, but vary only the identity of those spells. Suppose, for example, that Analyst 1 simultaneously covers IBM and Microsoft in addition to GE. In the scrambled data, these three spells may be assigned separately to three different analysts. Analyst 1 will still be assigned to cover three spells, but likely in firms other than GE, IBM, and Microsoft. To perform our hypothesis test, we make 1,000 such reassignments. We then estimate equation (1) separately on each sample and compute the F-statistic for a test that the analyst dummy variables are jointly significant. Finally, we compare the F-statistic on the actual sample to these 1,000 placebo samples. We compute a *p*-value for the null hypothesis that the actual analyst effects equal 0 as the fraction of F-statistics in the placebo samples that exceed the actual F-statistic. Though it would be possible to put further restrictions on the assignment of analysts to spells, it is important not to include any restrictions based on analyst-level variation since the resampling would then subsume a portion of the effect of

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[14] One possible way to bypass these issues might be to cluster standard errors; however, such an approach would require strong assumptions about the nature of the correlation in the data. In particular, we would need to identify groups within which observations are correlated, but across which they are independent. In our data, firms, analysts, agencies, and time are all potential sources of dependence across observations and the interactions among the groups are unclear. Moreover, clustering errors would not address small sample biases or address the need to make distributional assumptions. Thus, our approach provides a higher hurdle for significance.

[15] It is also possible to use a block bootstrap to construct standard errors for each analyst dummy in a LSDV implementation of the fixed effects model; however, using these standard errors to perform the joint significance test would require additional distributional assumptions, partially defeating the purpose of the bootstrap.

interest. For example, it would not be appropriate to reshuffle analysts only among spells of the same length.

The analyst effects in Equation (1) capture a systematic tendency to rate firms either higher or lower than other analysts covering the same firms at the same time, orthogonally to fundamentals. Agencies claim to rarely obtain private information about firms they rate, suggesting that analyst effects capture systematic differences in how analysts interpret the same information.[16] Even if the information available to analysts does differ, better information does not predict a systematic bias in the mean of the forecast since the information can be either good or bad. Thus, analyst fixed effects provide a credible measure of analyst biases.

II.B. Analyst Effects on Long-term Issuer Ratings

In Column 1 of Table II, we present the results from estimating equation (1) using long-term issuer ratings as the dependent variable and testing the joint significance of the analyst effects as described above. Our regressions confirm that there are significant differences across agencies in mean ratings, even after washing out all firm-level variation: Fitch ratings are the most lenient (though they are not statistically different on average from S&P ratings) and Moody's ratings are significantly lower on average than the other two agencies. Turning to the analyst effects, we find an F-statistic of 10.59 for the test that the analyst effects jointly equal 0. The *p*-value for a traditional Wald test is less than 0.001. 41% of the individual analyst effects are statistically significant at the 5% level. Applying our resampling procedure, we find that the true F-statistic is larger than all 1,000 F-statistics computed on the placebo samples. Thus, we compute a *p*-value of 0.001 for our null hypothesis.

To gauge the economic significance of the analyst effects, we first ask how much of the within variation they are able to explain (relative to the agency fixed effects). In our estimate of equation (1), the adjusted within R^2 is 0.3057. To provide a lower bound on how much of this

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[16] Following Dodd-Frank, rating agencies were no longer exempted from the provisions of Regulation FD prohibiting the disclosure of private information to select individuals or groups.

explanatory power comes from the analyst effects, we reestimate equation (1), but excluding the analyst effects. We find an adjusted within R^2 of 0.0376. Thus, the agency fixed effects explain at most 3.76% of the variation, implying that the analyst effects account for at least 26.81%. We also compute an upper bound by re-estimating equation (1), but excluding the agency fixed effects. The adjusted within R^2 is 0.3024, implying that the analyst effects explain at most 30.24% of the within variation in ratings. Another way to assess the economic importance of the variation in ratings due to analyst effects is to compare it to other known drivers of ratings. For example, Becker and Milbourn (2011) find that a one standard deviation change in competition among agencies changes ratings by 0.19 notches. By comparison, a one standard deviation change in ratings due to analyst biases is 0.46 notches, suggesting that the economic importance of analysts is relatively large. In Section III, we further demonstrate the economic significance of analyst biases by establishing a link to debt prices, corporate issuance activity, and firm growth.

Next, we estimate the three variations of equation (1) described in Section II.A that allow for more flexible differences in long-term ratings across agencies. First, we allow the agency effect to differ by sector. Equation (1) uses only variation within agencies to identify analyst effects; here we further restrict our attention to variation within agencies and sectors. As in the baseline specification we include firm-quarter fixed effects so that we compare each analyst only to other analysts simultaneously covering the same firm. We present the results in Column 2 of Table II. The F-statistic to test the joint significance of the analyst fixed effects is 9.00, again yielding a p-value less than 0.001 for a traditional Wald test. Using our block bootstrap procedure, we find that the F-statistic of 9.00 is higher than the F-statistic from 982 of 1,000 regressions on placebo samples, implying a p-value of 0.018.

Second, we allow the agency-sector effect to vary by quarter. Thus, we identify the analyst effects using only variation across analysts working in the same sector for the same agency in the same quarter. We present the results in Column 3. Using a traditional Wald test, the analyst effects are again significant with a p-value of less than 0.001. Moreover, the

estimated F-statistic of 8.66 is larger than the F-statistic in 942 of 1,000 placebo samples, implying a *p*-value of 0.058 using our block bootstrap procedure.

Third, we allow the agency fixed effects to vary firm-by-firm. In this case, we use only variation among analysts who cover the same firm for the same agency at different points in time to identify the analyst fixed effects. Thus, our estimates of analyst effects are robust to any time-invariant differences across agencies in how they treat specific firms, including how they select the analysts who cover them. The cost is that the analyst effects are likely to be measured with less precision since for each analyst we can only use the subset of covered firms in which we observe turnover in the analyst team for his or her agency to identify the effect. Nevertheless, we obtain similar results (Column 4). The F-statistic for a test of the joint significance of the analyst fixed effects is 5.54, implying a *p*-value for a traditional Wald test of less than 0.001. Using our block bootstrap procedure, we find a *p*-value of 0.063. Thus, using all three alternative specifications we find that analysts exert a significant influence on long-term issuer ratings.

A potential alternative explanation of our results is that analyst fixed effects capture short-term differences in the timing of ratings announcements. Recall, however, that ratings are split in over half of the cases in which we observe multiple agencies covering the same firm (Section I). Thus, the data do not support a story in which split ratings simply reflect differences in the timing of changes to the same consensus rating. Moreover, a simple tendency to update ratings more quickly would not generate a bias towards relative optimism or pessimism.

Overall, we uncover significant analyst effects on long-term issuer ratings. These effects provide credible measures of systematic relative optimism or pessimism at the analyst level. In Figure 1, we graph the distribution of the estimated analyst effects (Panel A). We also plot the distribution of the F-statistics from the 1,000 placebo samples created by our block bootstrap procedure, indicating the placement of the true F-statistic in the distribution with a dotted line. For brevity, we present only the specification with agency-sector-quarter fixed effects, which we adapt and apply in the remainder of the paper. In the Online Appendix, we show that analysts also have significant fixed effects on the short-term watches that agencies place on issuer ratings.

III. Real Effects of Systematic Analyst Optimism or Pessimism

III.A. Analyst Effects on Credit Spreads

Having established that analysts significantly affect credit ratings, we now ask whether the resulting biases in ratings have real effects on the rated firms. First, we ask whether the identities of the analysts covering the firm translate to differences in the prices of the firm's debt. If an efficient market recognizes that a portion of a firm's credit rating derives from biases of the particular analysts covering the firm, then it should adjust for those biases, determining prices using only the real information contained in the rating. Thus, our null hypothesis is that the portion of ratings determined by analyst effects does not predict credit spreads on the firm's debt.

To test this hypothesis, we reestimate the analyst fixed effects, but using only information available to market participants at the time prices are set. Though it would be possible to use exactly the fixed effects we estimated in Section II.B, constructing a backward-looking measure of analyst effects allows us to avoid the potential for reverse causality. Thus, for each sample quarter, we estimate equation (1) using only sample observations from prior quarters. We also include agency-sector-quarter fixed effects in lieu of the agency effects in equation (1). An advantage of this specification is that the comparison groups for each analyst in a particular quarter do not change as we add additional quarters to the regression – each analyst continues to be compared only to other analysts simultaneously covering the same firm and to other analysts simultaneously covering the same sector within his/her agency. We update the analyst effects when we add a new quarter to the regression only due to changes in how the analyst behaved relative to other analysts in that quarter. Though we report only this specification, we find similar results if we instead include either agency or agency-firm fixed effects in equation (1).

Next, we aggregate the estimated fixed effects of the analysts covering each sample firm in a given quarter. First, we sum the estimated fixed effects for the analysts covering the firm for each agency. This computation yields the portion of each agency's rating in each quarter that is due to the systematic optimism or pessimism of the analysts covering the firm (*Aggregate*

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Analyst Effects). We then subtract the aggregate analyst effects from the observed credit rating, yielding a "de-biased" rating (*Adjusted Credit Rating*). This decomposition isolates the portion of the observed rating driven by the biases of the analysts covering the firm from the portion of ratings driven by all other factors (creditworthiness, etc.).

Though we measure the relative optimism or pessimism of analysts using the difference in ratings between analysts covering the same firm at the same time, the aggregate analyst effects for each given firm-quarter are almost always different from zero. This is because the analyst fixed effect is the systematic relative optimism of an analyst averaged across different firms over time. Moreover, we can apply our measure of analyst biases to all sample firms even though we construct it using the subsample of split-rated firms. Our economic hypothesis is that analysts have fixed biases that apply across the set of firms they rate; split-rated firms merely provide a setting in which we can observe those relative biases. Recall from Section I that split-rated firms do not appear to differ meaningfully from other sample firms in their fundamentals.

Because the dependent variable does not vary by agency, we average the aggregate analyst effect and adjusted credit rating across agencies for each firm quarter. An alternative approach would be to run the regression at the firm-quarter-agency level and then to adjust the standard errors for the repetition of firm-quarters. Because the panel is unbalanced (i.e., the number of agencies providing a rating differs across firm-quarters) the two approaches are not equivalent. We prefer to average observations to avoid overweighting observations with greater agency coverage in the regressions.[17]

In Column 1 of Table III, we present estimates of our baseline regression of credit spreads – measured as the value-weighted credit spread across the firm's outstanding bond issues at the end of a given quarter – on decomposed long-term credit ratings. Note that the coefficient estimate on *Adjusted Credit Rating* will be identical to the coefficient we would estimate on the observed credit rating if we instead included *Aggregate Analyst Effects* and the observed rating as regressors. In that case, the coefficient on *Aggregate Analyst Effects* would measure the

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[17] We follow this approach throughout the remainder of the paper. Our conclusions are never sensitive to this choice.

difference between the effect of the observed rating and the analyst effects on spreads instead of the direct effect of analyst effects. We include controls for the value-weighted averages of the duration, callability, and age of the firm's outstanding bonds. We also include the time since the last date on which the firm's bonds traded as a measure of bond liquidity. Finally, we include fixed effects for each quarter to adjust for market-wide trends in yields. Because we observe persistent sets of bonds within a firm over time and because spreads are likely to move together with the market across firms, we cluster standard errors on two dimensions, firm and quarter, using the method from Thompson (2011).

We find that firms with callable bonds and bonds with longer duration face significantly lower credit spreads. On the other hand, firms with older and less liquid bond issues face higher spreads. Turning to the effects of interest, we find that a one notch improvement in the firm's adjusted credit rating is associated with a 49 basis point decrease in credit spreads, consistent with ratings conveying valuable information to market participants. Recall that our estimates of analyst effects are orthogonal to firm fundamentals by construction, since equation (1) contains firm-quarter fixed effects. Yet, the market reacts significantly to the portion of ratings driven by analyst effects: a one notch improvement in ratings due to aggregate analyst effects decreases spreads by 35 basis points.[18] We do uncover evidence of significant adjustment to the source of the rating information: the estimates on the aggregate analyst effect and the adjusted credit rating are significantly different (*p*-value = 0.001). However, we still observe a substantial and highly significant response to the portion of ratings driven by analyst identity, equal to roughly 71% of the effect of observed ratings on spreads. Thus, the assignment of analysts – and therefore a particular set of systematic biases – to firms affects the prices at which the firms' debt trades in the marketplace.

In Column 2, we add a number of additional controls to the regression. First, we include a battery of firm-level controls for cash-flow- and capital-structure-relevant variables, measured at

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[18] A one standard deviation change in the *Aggregate Analyst Effect*s in our sample is roughly 0.601 notches. Note however that it is not possible to change a rating by less than one notch, making a one notch change an appropriate unit of analysis.

the beginning of the quarter: long-term leverage, profit margin, market-to-book, the natural logarithm of sales, tangibility, the utilization of tax shields and carryforwards, and the ratio of R&D expenditures to sales.[19] We also include variables from the set of controls in the credit rating model of Blume, Lim, and MacKinlay (1998) that are not already part of the specification: total leverage, interest coverage divided into four splines, the natural logarithm of the market value of equity, equity beta, and equity volatility. Likewise, we include additional controls from Baghai, Servaes, and Tamayo (forthcoming), who estimate a similar regression of credit spreads on differences between observed and model-predicted credit ratings: the natural logarithm of the annual stock return and the expected default frequency.[20] The addition of the controls (and resulting reduction in sample size) has some impact on the estimates of interest. Nevertheless, our conclusions are unchanged: the market significantly adjusts for the portion of ratings driven by aggregate analyst effects, but leaves roughly 80% of the effect in place.

In Columns 3 and 4, we estimate regressions on two subsamples of the data designed to capture the mass of potential arbitrageurs in the market. In Column 3, we consider the subsample of firms with ratings of A- or higher. The bonds in this subsample fall comfortably in the investment grade universe. In Column 4, we consider firms with ratings between BBB and BB+. These bonds have ratings just around the investment grade cutoff. Since large institutions that might be well positioned to make arbitrage trades are likely to either face restrictions when trading in non-investment grade debt or be prohibited from holding such debt altogether, we expect the pressure to correct mispricing to be smaller in the latter sample.[21] For example, such institutions would not be able to buy a bond with a BB+ rating, even if they believe the bond should rightfully be rated BBB. We continue to include the full set of controls from the Column 2 specification. We find no evidence that the market prices relative analyst biases in the subsample of firms with ratings of A- or better. A one notch increment to the adjusted rating

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[19] For brevity, we do not tabulate the coefficient estimates for the firm-level controls. See the Online Appendix for a full version of the table including all coefficient estimates.

[20] We also estimate separate specifications that mirror the Blume, Lim, and MacKinlay (1998) and Baghai, Servaes, and Tamayo (forthcoming) regressions, finding similar results. See the Online Appendix for tables.

[21] See, e.g., Kisgen and Strahan (2010) for a detailed description of these regulatory constraints.

changes spreads by 9.26 basis points; however, there is no significant effect of aggregate analyst effects. There are two differences among firms around the investment grade cutoff: first, a one notch increment to adjusted ratings has a larger effect on spreads (50 basis points) and, second, the market prices the aggregate analyst effects. In this subsample, a one notch increment to the measured relative analyst bias changes spreads by 51 basis points, meaning that the entire effect of the analyst on ratings feeds through to credit spreads.[22] This result provides both a confirmation of our interpretation of analyst effects as biases (they are not priced for A- to AAA-rated issues) and a mechanism by which those biases nevertheless feed into market prices (limits to arbitrage due to regulatory constraints).

In Columns 5 and 6, we repeat the regressions from Columns 3 and 4, but using only the subsample of firm-quarters for which we do not observe a split rating. By restricting to this subsample, we guarantee that the firm-quarters (and ratings) we use to measure the analyst biases are not themselves part of the estimation. Even though our measures of analyst biases are orthogonal to firm fundamentals by construction, this approach further limits the scope for endogeneity via omitted variables to cloud the interpretation of our analysis. The results are nearly identical: among highly rated bonds, ratings affect prices, but analyst biases do not. However, both ratings and analyst biases matter for the pricing of lower-quality bonds. In the Online Appendix, we further restrict the estimation sample to only firms for which we never observe a split rating in the sample, again finding similar results.

We perform a variety of robustness checks on the results. One concern is that measurement error in our estimate of analyst biases might cause us to overestimate its effect on spreads.[23] As a first step to assess the potential for measurement error to alter our conclusions,

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[22] The results are nearly identical if we include the full set of firms with ratings below investment grade instead of restricting to the subsample with ratings of at least BB+. Likewise, we can exclude BBB rated issuers from the estimation without affecting the results. Including the restriction keeps the sizes of the subsamples roughly equal and demonstrates that the results are not driven by stale pricing in near-default bonds.

[23] To see this, recall that the regression is equivalent to one in which we include the observed credit rating (measured without error) and our estimate of aggregate analyst effects. In this specification, the coefficient on aggregate analyst effects will capture the difference between the market's response to the rating and the portion of ratings driven by analyst effects. If there is measurement error, then there is an errors-in-variables problem that will attenuate the estimate of this coefficient.

we reestimate the regression in Column 1, but progressively dropping early sample years in which the fixed effects are measured less precisely (due to smaller backward-looking estimation samples). We find that the estimated difference between the effects of observed ratings and analyst biases on spreads initially increases (consistent with measurement error in early years), but then begins to decline after we drop the first four sample years. The largest estimated difference is 20.5, implying a significant estimate of 29.7 basis points for the effect of a one notch change in analyst biases on spreads. Thus, our conclusions are unaffected. As an alternative, we exploit the skewness of the distribution of analyst biases (see Figure 1) to account for measurement error following the approach of Erickson, Jiang, and Whited (forthcoming). This approach requires a large cross-section of data, making it inappropriate for our regressions on smaller data samples. Nevertheless, we find similar results when we reestimate the Column 1 specification using this approach and setting the maximum degree of the cumulant to 5.

Another possible concern is that the persistence of both credit spreads and credit ratings could generate spurious results. A related concern is that ratings themselves respond to credit spreads, even though agencies explicitly state that their ratings do not consider prices. In our regressions, we measure ratings (and analyst biases) prior to credit spreads, but, if spreads are persistent, a channel from spreads to ratings could cloud the interpretation of our results. As a first step to address these concerns, we reestimate the regressions in Table III including the lag of credit spreads as an additional explanatory variable. We again cluster standard errors both by firm and by quarter. We provide the estimates in the Online Appendix. The lag of spreads is significant in all specifications, but we continue to find a similar pattern to Table III: aggregate analyst effects have significant predictive power for spreads (though less than observed ratings), particularly among firms with lower issuer ratings. We also reestimate the regressions allowing for an unobserved time-invariant firm effect and clustering errors by firm and quarter. We again find similar results (see the Online Appendix). As a final step, we reestimate the regressions including firm fixed effects and also the lag of credit spreads to soak up any remaining persistence after the fixed effects transformation, again finding similar results.

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Overall, we conclude that analysts exert a significant influence not only on ratings themselves, but also on the credit spreads firms face in the marketplace, particularly in the presence of limits to arbitrage.

III.B. Analyst Effects and Predictability of Bond Returns

Systematic optimism or pessimism of the individual analysts covering a firm – independent of its fundamentals – affects the pricing of its debt securities in the market. Thus far, these pricing effects do not appear to be justified by information, since they appear exclusively in market segments in which it is difficult to perform arbitrage. To assess further whether the influence of analysts on spreads is an instance of mispricing, we measure the future returns to the firms' bonds. If analyst optimism (pessimism) leads to spreads that are too low (high), then we should find a negative relation between our measure of analyst biases and future returns, capturing the reversion of market prices to fundamental value.

We measure returns as the difference in the natural logarithms of future and current credit spreads. We consider future returns over one, two, three, and four quarters. We also consider returns over 8 and 12 quarters (2 and 3 years, respectively). To test our hypothesis, we regress future returns on the decomposed credit rating (*Aggregate Analyst Effects* and *Adjusted Credit Rating*). We also include the full set of controls from Column 2 of Table III. As in Table III, we cluster standard errors both by firm and quarter to account for cross-sectional and time series dependence in the errors.

We report the results in Table IV. We find that bond characteristics affect changes in spreads in the expected directions: older bonds, callable bonds, and bonds with higher duration have larger declines in spreads over time. Otherwise, we find few robust predictors of future changes in spreads, including observed credit ratings. However, we find that the portion of ratings driven by systematic analyst biases significantly predicts returns at all six horizons considered: firms covered by analysts who are systematically pessimistic significantly outperform firms covered by analysts who are systematically optimistic (i.e., credit spreads

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decline). Moreover, in this regression, measurement error in the aggregate analyst effects would imply that our results understate the true effect.[24] Likewise, adjusted credit ratings are more persistent than the aggregate analyst effects, suggesting that the results are not likely to be an artifact of including a persistent regressor.

Overall, the evidence suggests that the pricing of analyst effects on net does not improve the efficiency of bond markets. Instead, the debt of firms with systematically optimistic (pessimistic) analysts is priced too low (high). Not only are the effects of analyst biases most pronounced among bonds for which arbitrage is likely to be limited, but prices predictably move in the opposite direction of the analyst biases over the following 12 quarters. Thus, our evidence provides a rationale for companies to target debt ratings, as Hovakimian, Kayhan, and Titman (2009) and Kisgen (2009), among others, argue that they do.

III.C. Analyst Effects and Corporate Financial Policies

Having established a link from analyst biases to debt market prices, we next turn to the decisions made by corporate managers who must raise capital in those markets. Our results identify cross-sectional differences in the costs of raising debt. Firms with pessimistic (optimistic) analyst coverage are likely to face overpriced (underpriced) debt. We ask whether corporate managers respond to those incentives, shifting capital structure choices in the predicted direction. We also test whether there are spillovers from financial decisions to the real decisions of firms that face higher borrowing costs. These effects are not immediate given differences in the yields on outstanding bonds; if Modigliani-Miller holds, then prices properly reflect future cash flows (even if they differ across firms) and do not provide an incentive to shift the composition of the firm's capital structure. Our approach is similar in spirit to DellaVigna and Pollet (2007, 2013) who identify mispricing of predictable demographic information in equity

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[24] Since the estimate on *Aggregate Analyst Effects* is larger in magnitude than the estimate on *Adjusted Credit Rating* and it is the difference between the two that is attenuated if *Aggregate Analyst Effects* is measured with error, a larger difference implies a larger estimate on *Aggregate Analyst Effects.*

markets and its effects on corporate capital structure choices. We instead identify a friction in debt markets, which have received less attention in existing behavioral finance literature.

First, we test whether analyst-driven biases in ratings affect the relative likelihood of raising new debt capital and whether they affect the terms on which that capital can be raised. To avoid confounding the need for capital with the choice of financing instrument, we consider the choice between debt and equity, conditional on making an issue of (at least) one type during the quarter. We measure debt and equity issuance using the "financing spikes" approach of Leary and Roberts (2005) and Hovakimian, Opler, and Titman (2001), among others. The advantage of this approach relative to using SDC security issuance data is that it includes debt issuance through private sources and, thus, provides a relatively complete accounting of external financing episodes.[25] Moreover, it excludes debt issuance that simply rolls over existing debt without increasing debt outstanding and allows us to identify explicitly debt retirements. On the subsample of issuers, we estimate a logit regression using a binary indicator of debt issuance as the dependent variable.[26] We include the battery of firm-level controls from Column 2 of Table IV: long term leverage, profit margin, market-to-book, the natural logarithm of sales, tangibility, the utilization of tax shields and carryforwards, and the ratio of R&D expenditures to sales as well as industry and quarter fixed effects. We also include the aggregate analyst effect on credit ratings and the de-biased credit rating, constructed as in Section III.A. Note that our main variable of interest, the aggregated analyst effect, is plausibly exogenous since it comes from a backward-looking regression that includes fixed effects for firm-quarters. We again average observations for the same firm-quarter across agencies to obtain a firm-quarter panel and cluster standard errors at both the firm and quarter levels.

We report the estimates in the form of log odds ratios in Column 1 of Table V. Not surprisingly, we find that firms with higher leverage and larger firms are more likely to issue

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[25] A potential downside of including private debt issues in our analysis is that credit ratings may have less influence on the terms provided by private lenders, who may be more likely to do their own monitoring. To the extent that this is the case, it should attenuate our estimates.

[26] Here and throughout this Section, our results are robust to estimating instead a linear probability model.

debt, conditional on tapping external markets. Firms with weaker de-biased (or, observed) ratings are less likely to issue debt, suggesting that credit ratings correlate with some market friction that breaks the Modigliani-Miller result. Moreover, the portion of ratings driven by analyst biases is a strong negative predictor of debt issuance. The magnitude of the effect is three times the effect of de-biased ratings: a one notch increase in the analyst-driven portion of ratings would decrease the odds of debt issuance by 40%. This finding is consistent with the firm viewing worse ratings along this dimension as an undue friction.

In Column 2, we consider the prices at which new debt issues occur. For this analysis, we restrict our attention to the set of public debt issuances by sample firms available from the SDC database. We use the offering yield to maturity to measure debt terms. We regress the yield on the aggregate analyst effect, the de-biased rating, and the set of controls from Column 1. Here, the results mirror our results from Section III.A. The de-biased rating has a significant positive effect on yields: firms with worse ratings receive worse prices. The portion of ratings driven by analyst biases also has a significant effect on yields. Though the market partially adjusts (i.e., this portion of ratings affects yields less than the de-biased piece), roughly 80% of the effect remains. The result is nearly unchanged if we include an additional control for the size of the debt issue. Thus, firms that happen to have analysts who are systematically pessimistic do indeed experience higher costs of raising new debt capital.

In Panel B of Table V, we test whether analyst effects matter for the unconditional likelihoods of various financing choices: debt issuances, debt retirements, equity issuances, and share repurchases. In all cases, we estimate logit regressions on the full sample of firm-quarters and include the same set of controls as in Panel A. The evidence is broadly consistent with an (unduly) high cost of debt capital among firms with more generally pessimistic analysts. We find a strong negative effect of analyst pessimism on the likelihood of repurchasing shares. Though it is negative, the effect of analyst pessimism on the unconditional likelihood of issuing debt is not statistically significant. We also find strong evidence that such firms are more likely to retire

debt and to issue equity: the coefficients on the aggregate analyst effect are significant at the 1% level in both regressions.

Finally, we test whether these apparent financing frictions affect firms' real growth rates. In Column 7, we replicate the specification from Column 6, but using sales growth as the dependent variable. We find evidence that firms with a more pessimistic aggregate analyst effect grow significantly more slowly: the growth rate is a full percentage point lower for a one notch increase in the aggregate analyst effect. This effect is roughly half of the median growth rate in the sample and 5% of its standard deviation. Moreover, this portion of the rating is a stronger drag on real growth than the de-biased portion of the rating. Thus, the corporate impacts of analyst biases do not appear to be restricted to the liabilities side of the balance sheet: analyst pessimism affects not only how the firm is financed, but also its ability to grow.

As a robustness check, we reestimate all the specifications in Table V including firm fixed effects. The results are generally similar and, in some cases stronger. The dependent variables in Table V are also generally not persistent, further confirming that our estimates of analyst biases are unlikely to be significant spuriously due to their persistence. Moreover, the point estimates in Table V are generally larger on the aggregate analyst effects than on adjusted ratings, so that the presence of measurement error in the analyst effects would cause us to understate the estimate on the analyst effects.

We also estimate alternative specifications of the Table V regressions in which we use only the subsample of firm-quarters on which we do not observe split ratings. This approach again allows us to separate the measurement of analyst biases from the identification of their effect on corporate policies. The results are generally similar (See the Online Appendix). Not surprisingly, some of the marginal results are not robust on the smaller sample; however, our key results on the choice between debt versus equity and revenue growth remain strong economically and statistically.

We find that firms with optimistic analyst coverage at the beginning of a quarter tend to skew financing choices during the quarter towards debt. These differences in financing choices

may interact with the future predictability of spreads we identified in Section III.B. To separate the effect of issuing new debt with (potentially) higher yields from the effect of changes in market beliefs about future cash flows, we reestimate the return predictability regressions from Table IV, but lagging the aggregate analyst effect and adjusted credit rating by one quarter. Thus, we do not include the quarter immediately following the measurement of analyst biases – during which debt issues may occur – when calculating the change in future spreads. We do not lag the controls for bond characteristics, like age and duration, so that these variables will capture any changes to the nature of the debt outstanding between the measurement of analyst effects and the change in spreads. We find similar results. At a one (two) year horizon, the estimate on analyst effects is -0.023 (-0.041). Both effects are significant at the 5% level, yet the effect of observed ratings is roughly half the size and not reliably significant.

IV. Which Analysts Matter?

Thus far, we have shown that the biases of analysts matter for credit ratings, security prices, and corporate financing decisions. But, we have said little about which types of analysts have the greatest effects. As a final step, we link the optimistic/pessimistic biases of analysts to observable analyst traits to shed light on the sources of analyst biases and potential remediations.

To conduct this analysis, we supplement our data with information on analysts' backgrounds from web searches (see Section I for additional details). We then measure a number of different analyst traits: age, gender, education, tenure covering each firm, tenure covering each industry, tenure within the rating agency, and the number of firms covered. We adapt model (1) from Section II.A. to test whether differences in these traits can account for the observed differences in ratings across analysts. In place of $\gamma_{analyst}$, we include our measures of analyst traits. Because we often observe multiple analysts covering a particular firm-quarter for the same agency, we first average characteristics across analysts within each agency-firm-quarter before running our regressions. Thus our data retains the same panel structure as in Section II.B. An alternative would be to include each analyst within an agency-firm-quarter as a separate

observation (and then cluster standard errors within the group to correct for repetition). These options are not equivalent since we observe varying numbers of analysts covering each agency-firm-quarter. Thus, the group weightings using the two approaches would differ. For robustness, we conduct our analysis both ways, finding that no conclusions are altered by this choice.

We include a control variable for the number of years the agency has covered the firm, since prior research suggests that long relationships with rating agencies can lead to more favorable ratings (Mahlmann, 2011). We also continue to include firm-quarter fixed effects. Thus, we measure the effect of analyst traits after accounting for potentially non-random matching of analysts to firms – the estimates compare only analysts covering the same firm for different agencies at the same time. We also continue to include the agency fixed effects. We cluster standard errors at the firm-quarter level to account for repetition across agencies.

We consider several dependent variables. First, we construct a measure of analyst optimism by computing the difference between the analyst's rating in a given firm-quarter and the average of the ratings from other analysts.[27] Since worse ratings are associated with higher numbers on our numerical scale (see Table A-II in the Appendix), we negate the difference so that higher values of optimism correspond to relatively stronger ratings of the firm. It is important to note that this measure captures optimism of the analyst relative to other analysts contemporaneously following the same firm, but it does not allow us to measure absolute optimism or pessimism of the ratings. Because the measure is a relative comparison, we restrict the sample to firm-quarters in which at least two agencies offer ratings of the firm. We also consider the dispersion between the analyst's rating and the average of the ratings from other analysts in the same firm-quarter by taking the absolute value of the optimism measure. Finally, we consider a measure of relative forecast accuracy. The measure is the product of analyst relative optimism and the change in forward credit spreads over three years, negated so that a

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[27] We choose this approach, rather than simply using the long-term rating itself as the dependent variable so that the analyst's own rating is not included in computing the benchmark (or "consensus" rating). This distinction is important since we observe at most three distinct ratings per firm-quarter.

higher value corresponds to greater accuracy. Intuitively, an analyst is "right" if s/he is relatively more optimistic (pessimistic) and credit spreads fall (rise) over the given horizon.

We present the results of estimating the regression models in Table VI. We find relatively little evidence that agency tenure covering the firm affects ratings quality, after accounting for analyst effects. The exception is a relatively small, but significant decline in rating accuracy (Columns 5 and 6). However, we find evidence of two general patterns in the types of analyst who produce higher quality (or less biased) ratings. First, our results suggest that analyst skill or experience is an important factor in explaining differences in ratings. We see in Column 1 that analysts with an MBA tend to provide significantly less optimistic ratings than other analysts covering the same firm at the same time.[28] We also find in Column 3 that their ratings deviate more on average in either direction from other analysts contemporaneously covering the firm than their peers without MBAs. In Column 5, we find evidence that their ratings prove significantly more accurate over time. The increase in accuracy is roughly 30% of a standard deviation. The results are consistent with an MBA as a proxy for heightened expertise: analysts with an MBA are more likely to disagree with other analysts contemporaneously rating the same firm and are less likely to inflate ratings. Moreover, these ratings more often prove accurate predictors of future movements in credit spreads. To confirm this interpretation we also split the MBA proxy into MBAs received from top-5 and other business schools (Columns 2, 4, and 6), measured using the 2011 *Economist* rankings. We find that the results are stronger for all three dependent variables for analysts with MBAs from top-5 schools.

We find similar (though weaker) evidence looking at covariates that capture analyst experience. We find that analysts with longer tenure in the agency are more accurate as are analysts with longer tenure covering the industry, though the estimate is not statistically significant. An MBA provides an increase in accuracy equivalent to roughly 6 years covering the industry or 9.7 years in the agency. We also see that a higher number of covered firms is

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[28] Roughly 65% of analysts for whom we have educational information have an MBA. Roughly 12% of analysts have a master's degree in another field, 1% hold a PhD, and 2.5% are CFAs. The remainder have only a bachelor's degree. Thus, an MBA degree in our sample is a reasonable proxy for financial expertise.

associated with lower rating optimism and heightened accuracy. An MBA appears to have a similar effect on rating outcomes to coverage of 10 firms.

We also uncover a second pattern. We find that as analyst tenure covering a firm increases, relative optimism about the firm increases. Roughly 10 years covering a firm would increase relative optimism by a standard deviation; even a single year increases ratings by roughly 10% of a rating notch relative to peers evaluating the same firm contemporaneously. Moreover, as tenure covering the firm increases, ratings become a worse predictor of movements in credit spreads, an effect that is significant at the 5% level. After 3.3 years following a firm, the decline in rating accuracy would roughly offset the benefit provided by an MBA. Thus, rating quality appears to deteriorate with time spent covering a firm. One possible explanation is the deterioration of career concern incentives as analyst tenure covering the firm increases (Holmstrom, 1999), though in this case we might expect similar effects as analyst tenure in the agency or analyst tenure covering the industry increase and we do not find evidence of such effects. Since meetings between the agency and firm are frequent throughout the rating process (Purda, 2011), an alternative interpretation is that relationships between the analyst and the rated firm cloud the analyst's incentives. Recent work, for example, studies cases in which analysts move from rating agencies to the firms that they rate, finding that such analysts tend to inflate bond ratings (Cornaggia, Cornaggia, and Xia, 2014) or buy recommendations (Cohen, Frazzini, and Malloy, 2012) prior to being hired. Of course, relationships may be associated with greater leniency even in the absence of an explicit ulterior motive, like gaining employment at the rated firm. Moreover, longer relationships may create an "illusion of knowledge" (Oskamp, 1965), leading to a decline in rating quality, even for analysts without conflicts of interest.

Finally, we see some evidence that female analysts provide higher quality ratings. We find that ratings of female analysts are significantly lower on average than other analysts contemporaneously covering the same firms. Interestingly, the effect seems to be entirely in the level of ratings, as we see no difference in the (unsigned) deviation of ratings from the other analysts. And, over a 3-year horizon, we see that their forecasts are on average more accurate.

Economically, the effect is roughly as large as the effect of an MBA on forecast accuracy, though it is not statistically significant. This effect could represent either a selection or a style effect. Women who choose to become credit analysts, for example, may be higher skilled on average than men who make the same choice, though women are significantly less likely to have MBAs (Table I, Panel C). Alternatively, women may be less prone to certain behavioral biases that can lead to inflated ratings (Lundeberg, Fox, and Punccohar, 1994) or may have preferences that are better aligned with creditors' interests.

We also test whether the effects of analyst traits on ratings are more pronounced in some firms than in others. In particular, we consider five proxies for transparency or the ease with which companies can be evaluated: firm size, firm age, diversification, the number of equity analysts covering the firm, and the dispersion in analyst earnings forecasts. We split the sample at the median of each characteristic and reestimate our regression separately on each subsample. We report the results in Table VII. In the table, we focus on a single proxy for analyst skill (MBA) and a proxy for analyst bias (time covering the firm) due to space constraints; however, we provide complete estimates in the Online Appendix. The dependent variable is rating accuracy over a three-year horizon.[29] We find for every sample split that the increased accuracy of analysts with an MBA is most pronounced for firms that are likely to face higher information asymmetries with the market: smaller firms, younger firms, diversified firms, firms with a low degree of equity analyst coverage, and firms with high dispersion in analyst earnings forecasts. In all cases but one (number of equity analysts covering the firm), the differences are statistically significant at the 5% level. Thus, overall, the results suggest that the higher quality ratings provided by skilled analysts occur precisely among the firms that are the most difficult to evaluate. We see similar evidence when we focus on analysts with a long tenure covering the firm. In particular, we find that the decline in relative accuracy among such analysts is concentrated in the information-sensitive firms, though the results are statistically weaker. Our

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[29] We also consider optimism as a dependent variable, but find less consistent patterns across the sample splits.

analysis suggests that the lack of transparency in such firms allows for more analyst discretion or subjectivity in ratings, which can reveal both differences in skill and biases.

Overall, we find evidence of multiple channels through which analysts exert an effect on credit ratings. Analysts with greater expertise appear to issue higher quality, less biased ratings. Most interesting from a policy perspective, long-term relationships between analysts and the firms they cover appear to erode the quality of ratings. Moreover, these effects are most pronounced in firms likely to face constraints in accessing external capital, magnifying the real impact of analyst differences. A caveat to our results, however, is that there is likely to be a number of unobserved traits that also explain portions of the analyst effects we uncover in Section II, particularly given the limited set of measurable traits available for our analysis.

V.   Conclusion

We uncover evidence that significant variation in credit ratings can be explained by the biases of the analysts covering the firm. We use firm-quarter fixed effects to wash out all firm-level variation that might explain differences in credit ratings, finding that analyst fixed effects explain a significant portion of the contemporaneous variation in ratings of the same firm across agencies. The result holds correcting for differences in average ratings across agencies, sector-level differences in ratings across agencies, or sector-level differences in ratings across agencies that vary quarter-by-quarter. It also holds allowing for firm-specific agency fixed effects.

We find that these systematic analyst effects, though orthogonal to firm fundamentals, carry through to credit spreads on the firm's existing debt. They also affect the cost of raising new debt capital. Firms that are covered by analysts who are systematically more pessimistic than their peers have debt with higher spreads and obtain worse terms on debt issues. Moreover, systematic analyst optimism or pessimism in ratings predicts future movements of spreads in the opposite direction even though observed ratings themselves have no such predictive power, suggesting that the pricing effects are unwarranted. We find that firms adjust their capital structure polices accordingly. Managers facing more pessimistic analyst teams raise debt less

frequently, retire debt more frequently, and lean more heavily on equity financing. Finally, these financing constraints appear to affect real corporate outcomes: firms covered by analysts who are systematically pessimistic grow significantly more slowly than firms with optimistic analysts.

We also link individual analyst traits to the analyst's effect on ratings. We find evidence of at least two distinct patterns in the quality of ratings produced by different analysts: First, analysts with greater expertise or experience (measured by MBA degrees and longer tenure covering the industry) appear to provide higher quality ratings. We find evidence that analyst skill is associated with lower relative optimism in ratings and greater accuracy over a 3-year horizon. Second, we find evidence that ratings quality deteriorates as analyst tenure covering the firm increases. Ratings become relatively more optimistic and less accurate. The effects are the most pronounced precisely in the firms that are most likely to face frictions in raising external capital, thus magnifying their real impact.

Our results have important policy implications. On the one hand, our results suggest that some firms may face more frictions in raising capital simply because they are covered by less able credit analysts. Perhaps of more significance, our results suggest that long-term relationships between firms and the analysts who rate their debt issues can lead to inflated ratings and costs of capital that are too low. These inefficiencies could carry through to real investment choices by distorting NPV computations and, ultimately, could lead to value-destroying overinvestment. Thus, among other things, our results point to potential benefits from implementing formal analyst rotation schemes, as suggested by the SEC in the wake of the recent financial crisis (SEC, 2009) and as is mandatory among company auditors.

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Appendix

In this appendix, we provide some additional details on the construction of our dataset and on the variables we use in our analysis. First, we provide a breakout of the types of ratings announcements in the core data from Thomson CreditViews:

Announcement Type	Freq.	Percent
New Rating	1,616	3.60%
Rating Affirmed	12,686	28.30%
Rating Downgraded	5,124	11.43%
Rating Upgraded	2,833	6.32%
Rating Withdrawn	670	1.49%
Rating Off Watch	3,272	7.30%
Rating On Watch Developing	270	0.60%
Rating On Watch Down	3,210	7.16%
Rating On Watch Up	1,047	2.34%
Outlook Developing	153	0.34%
Outlook Negative	3,212	7.17%
Outlook Positive	1,600	3.57%
Outlook Stable	5,601	12.49%
Outlook Withdrawn	3,532	7.88%
Unknown	3	0.01%
Total	44,829	100%

Below, we provide a breakout of the announcements by agency:

Agency	Freq.	Percent
Fitch	7,189	16.04%
Moodys	12,353	27.56%
Standard and Poor's	25,287	56.41%
Total	44,829	100%

Note that Standard and Poor's is responsible for a greater proportion of the reports in our data than the other two agencies. Part of this effect is due to the increasing coverage by Fitch over time: in 2000, only 4% of reports originate with Fitch, but the percentage increases to 22% in 2010.

Next, we provide some additional details on how we compute the credit spreads necessary to construct the analyst accuracy measures we use in our analysis. In order to calculate

credit spreads, we merge cleaned TRACE data with the Mergent FISD issue and redemption file using the complete cusip.[30] From the Mergent file, we remove bonds with special characteristics, i.e. bonds that are exchangeable, putable, convertible, pay-in-kind, subordinated, secured, or guaranteed, and zero coupon bonds and bonds with a variable coupon. In addition, we drop observations with missing maturity dates.

To construct daily bond prices, we compute a daily trade-weighted average price, i.e. each trade price is weighted by its size.[31] We use these daily bond prices to calculate the yield to maturity and the duration of each bond. For each daily bond price, we calculate the credit spread as the difference between the bond's yield to maturity and a benchmark Treasury yield using the daily CRSP fixed term indexes for the periods 1, 2, 5, 7, 10, 20 and 30 years. We then use linear interpolation of the yields of the two government bonds that have the next lower and higher duration relative to the respective corporate bond. We delete observations with a duration of less than one year. For bonds with a duration of more than 30 years, we use the 30-year treasury yield. We delete a few observations that have missing or negative yields. The approach follows Campbell and Taksler (2003), Bongaerts, Cremers and Goetzmann (2012) and Bessembinder et al. (2012).

Should firms have multiple bonds outstanding, we follow Qiu and Yu (2009)'s value-weighted approach by using the amount outstanding of each bond as the weight to aggregate credit spreads to firm-level measures.

Finally, we present a list of the variables we use in our analysis, together with detailed definitions and information on the data source in Table A-I. And, we tabulate the correspondence between the numerical scale we use for long-term ratings and the letter ratings scales of the three agencies in Table A-II.

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[30] We follow the guide by Dick-Nielsen (2009) to remove erroneous entries from the TRACE data. In particular, we pay attention to cancelled and corrected trades, and whether they are as-of trades. We follow Bessembinder et al. (2012) and replace trades with indicators +$1MM and +$5MM with the numerical vales 1,000,000 and 5,000,000. In addition, we follow Bongaerts, Cremers and Goetzmann (2012) and delete trades that include a commission or have a settlement period of more than 5 days, and remove trades with a negative reported yield.

[31] Bessembinder et al. (2012) find that trade-weighted prices exhibit better statistical properties. This also helps to reduce the effect of any remaining data errors in the TRACE data.

Appendix Table A-I
Variable Definitions

Variable Name	Definition	Data Source
Accuracy	The product of -1 times Optimism and the forward change in credit spreads over a 3 year horizon, measured starting at the end of the quarter.	Thomson/Trace
Agency Tenure Covering the Firm	The number of years between the date the agency covers a firm for the first time and the date on which the quarter ends.	Thomson
Aggregate Analyst Effects	The sum of the dummy coefficients γ from the equation $Rating_{ijt} = \alpha_{jt} + \beta_{ist} + \boldsymbol{\gamma}_{analyst} + \epsilon_{ijt}$ for all analysts covering each firm j in sector s during quarter t for each agency i. To ensure that we measure the reaction only to information that was available to market participants at the time, we construct a backward-looking estimate of the fixed analyst effects on ratings by running the equation for each quarter including only the data up to that quarter.	Thomson
Analyst Age	The minimum of the first year of employment minus 22 years and the first year of college minus 18 years.	LinkedIn/S&P, Moody's, and Fitch websites
Analyst Tenure Covering the Firm	The number of years between the date an analyst covers a firm for the first time and the date on which the quarter ends.	Thomson
Analyst Tenure Covering the Industry	The number of years between the date an analyst covers a company in the industry in which the rated firm operates for the first time (Fama French 49 classification) and the date on which the quarter ends.	Thomson
Analyst Tenure in the Agency	The number of years between the date an analyst starts working for the rating agency and the date on which the quarter ends.	LinkedIn/S&P, Moody's, and Fitch websites
Bond Age	Firm-level volume-weighted average of the number of days since the debt issuance of all outstanding bonds issued by the firm, measured at the end of each given quarter.	Trace, Mergent FISD.
Bond Duration	Firm-level volume-weighted average of the duration of all outstanding bonds issued by the firm, measured at the end of each given quarter.	Trace, Mergent FISD.
Callable Bond	Firm-level volume-weighted average of the bond callable dummies, where each dummy is equal to one if the bond is callable, measured at the end of each given quarter.	Trace, Mergent FISD.
Carryforwards	Ratio between tax loss carry forwards and total assets, winsorized at the 1% and 99% level. The carry forward variable is set to 0 when missing in Compustat.	Compustat
Credit Rating	A number from 1 to 21 indicating the credit rating of a company at the end of the quarter. Table A-II shows the rating correspondence across agencies.	Thomson
Credit Rating (Adjusted)	The difference between the credit rating of a firm, and the aggregate analyst effect.	Thomson
Credit Spread	Firm-level volume-weighted average of the credit spreads of all outstanding bonds issued by the firm. Credit spreads for each issue are calculated by subtracting from the bond's yield to maturity the yield resulting from a linear interpolation of the CRSP treasury yields (among the periods 1, 2, 5, 7, 10, 20, and 30 years) that have the next lower and higher duration relative to the bond's duration. For bonds with a duration of more than 30 years, we use the 30-year treasury yield. The spread is measured in basis points at the end of each given quarter.	Trace, Mergent FISD.
Debt Retirement Spike	Dummy variable equal to 1 if total debt decreases during a given quarter by more than 5% of total assets at the beginning of the quarter.	Compustat
Debt Issuance Spike	Dummy variable equal to 1 if total debt increases during a given quarter by more than 5% of total assets at the beginning of the quarter.	Compustat
Expected Default Frequency	Expected default frequency estimated following Bharath and Shumway (2008): $EDF = \phi[-(\ln[(E+F)/F] + \mu - 0.5\sigma^2)/\sigma]$ where E is the market value of equity; F is the face value of debt (computed as short term debt plus one half long term debt); μ is the prior 12-month stock return; σ is asset volatility (estimated as $\sigma = (E/(E+F))\sigma_e + (F/(E+F))(0.05+0.25\sigma_e)$, where σ_e is the annualized volatility of daily stock returns over the prior 12 months); and ϕ is the standard normal cumulative distribution function.	Compustat, CRSP
Equity Analysts' Earnings Forecast Dispersion	Standard deviation of equity analysts' earnings forecasts covering the firm six months prior to the annual earnings announcement, standardized by the mean earnings forecast.	I/B/E/S
Equity Beta	Beta coefficient of daily stock returns relative to the value-weighted CRSP market portfolio for the previous fiscal year.	CRSP
Equity Volatility	Annualized average daily stock return volatility over the previous 12 months. A minimum of 21 trading days are required for volatility to be computed.	Compustat
Female	A dummy variable equal to 1 if the analyst's gender is female.	S&P, Moody's, and Fitch websites
Firm Age	Difference in years between the end of the fiscal quarter date and the first time the firm appears in Compustat.	Thomson, Compustat
Interest Coverage k1, k2, k3, k4	Spline variables based on the interest coverage ratio, contructed as in Blume, Lim, and MacKinlay (1998).	Compustat, CRSP
Long-Term Leverage	Long-term debt divided by total assets, winsorized at the 1% and 99% level.	Compustat

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Variable Name	Definition	Data Source
Market-to-Book	Ratio between the market value of assets and the book value of assets. The market value of assets is the total book value of assets plus the market value of equity (number of shares outstanding * stock price) minus the book value of equity. The ratio is winsorized at the 1% and 99% level.	Compustat
Market Value of Equity (log)	Natural log of 1 plus the product of the stock price and the number of shares outstanding.	Compustat
MBA	A dummy variable equal to 1 if the individual has a Master of Business Administration degree.	LinkedIn/S&P, Moody's, and Fitch websites
MBA Non-Top 5	A dummy variable equal to 1 if the individual has a Master of Business Administration degree not from one of the top 5 MBA programs according to the 2011 *Economist* ranking (University of Chicago, Tuck, Berkeley, University of Virginia, IESE)	LinkedIn/S&P, Moody's, and Fitch websites
MBA Top 5	A dummy variable equal to 1 if the individual has a Master of Business Administration degree from one of the top 5 MBA programs according to the 2011 *Economist* ranking (University of Chicago, Tuck, Berkeley, University of Virginia, IESE)	LinkedIn/S&P, Moody's, and Fitch websites
Net Equity Issuance Spike	Dummy variable equal to 1 if net equity issuance (sale of common and preferred stock minus purchase of common and preferred stock) in a given quarter is greater than 5% of total assets at the beginning of the quarter. Equity issued and equity repurchased are set to 0 when missing in Compsutat.	Compustat
Net Equity Repurchases Spike	Dummy variable equal to 1 if net equity repurchases (purchase of common and preferred stock minus sale of common and preferred stock) in a given quarter are greater than 1.25% of total assets at the beginning of the quarter. Equity issued and equity repurchased are set to 0 when missing in Compsutat.	Compustat
Number of Equity Analysts	Number of equity analysts covering the firm six months prior to the date of the annual earnings announcement.	I/B/E/S
Number of Firms Currently Covered	The number of companies covered by an analyst at the end of the quarter.	Thomson/S&P, Moody's, and Fitch websites
Number of Segments	Number of business segments operating in distinct Fama French 49 industry codes	Compustat Segments
Offering Yield to Maturity	Dollar-weighted average of the offering yield to maturity of all bonds issued in a quarter by a given firm.	SDC
Optimism	The difference in each firm-quarter between the analyst's rating of the firm and the average rating of the other analysts covering the firm, multiplied by -1.	Thomson
Outlook Negative	A dummy variable equal to 1 if the long-term outlook for the firm at the end of the fiscal quarter is negative.	Thomson
Outlook Positive	A dummy variable equal to 1 if the long-term outlook for the firm at the end of the fiscal quarter is positive.	Thomson
Outlook Stable	A dummy variable equal to 1 if the long-term outlook for the firm at the end of the fiscal quarter is stable.	Thomson
Profit Margin	Annualized quarterly profit divided by total assets, winsorized at the 1% and 99% level.	Compustat
R&D/Sales	Ratio between quarterly R&D expenditures and quarterly sales, winsorized at the 1% and 99% level. R&D is set to 0 when missing in Compustat.	Compustat
Rating Dispersion	The absolute value of the difference in each firm-quarter between the analyst's rating of the firm and the average rating of the other analysts covering the firm.	Thomson
Sales (log)	The natural log of 1 plus total quarterly sales.	Compustat
Sales Growth	Ratio of the change in sales during a given quarter and the sales at the beginning of the quarter. The measure is winsorized at the 1% and 99% level.	Compustat
Stock Return (log)	Natural log of 1 plus annualized average monthly returns for the previous 12 months.	
Tangibility	Ratio between PP&E and total assets, winsorized at the 1% and 99% level.	Compustat
Taxshields	Ratio of deferred taxes and investment tax credit to total assets, winsorized at the 1% and 99% level. Deferred taxes and investment tax credit are set to 0 when missing in Compsutat.	Compustat
Time Since Last Bond Trading Date	Firm-level volume-weighted average of the number of days since the date the bond was traded last, measured at the end of each given quarter.	Trace, Mergent FISD.
Time Since Last Rating Action	The number of days between the current and the last announcement of a rating upgrade, downgrade, or affirmation for the rated firm.	Thomson
Total Assets	Total assets (quarterly).	Compustat
Total Leverage	Total debt debt divided by total assets, winsorized at the 1% and 99% level.	Compustat
Watch Negative	A dummy variable equal to 1 if the firm has been put on a negative watch during the quarter, and zero otherwise.	Thomson
Watch Positive	A dummy variable equal to 1 if the firm has been put on a positive watch during the quarter, and zero otherwise.	Thomson
Watch Signed	A dummy variable equal to 1 if the firm has been put on a positive watch during the quarter, -1 if the firm has been put on a negative watch during the quarter, and zero otherwise.	Thomson

Appendix Table A-II
Credit Rating System and Letter Rating Conversion

The table shows the credit rating systems for Standard & Poor's, Moody's and Fitch ratings, and how ratings vary across agencies. The table also shows the percentage of firm-quarter observations with each numerical credit rating value. The Agreement Sample includes firm-quarters in which all agencies that rate the firm have the same numerical rating. The complement is the Split Rating Subsample. On the latter subsample, we present both the minimum and maximum rating for the firm-quarter.

Credit Rating	Letter Rating			Agreement Sample (N= 29,005)	Split Rating Subsample (N=7,916)	
	Standard & Poor's	Moody's	Fitch		Min. Rating	Max. Rating
1	AAA	Aaa	AAA	0.36	0.13	N/A
2	AA+	Aa1	AA+	0.09	0.43	0.13
3	AA	Aa2	AA	0.64	1.40	0.25
4	AA-	Aa3	AA-	1.46	1.52	1.12
5	A+	A1	A+	3.44	2.85	1.89
6	A	A2	A	6.67	3.39	2.11
7	A-	A3	A-	6.84	5.26	3.64
8	BBB+	Baa1	BBB+	8.21	7.95	4.03
9	BBB	Baa2	BBB	12.68	7.50	8.32
10	BBB-	Baa3	BBB-	8.47	8.34	6.61
11	BB+	Ba1	BB+	6.4	12.05	7.90
12	BB	Ba2	BB	8.34	13.05	11.56
13	BB-	Ba3	BB-	11.02	11.84	13.40
14	B+	B1	B+	10.62	10.69	11.67
15	B	B2	B	8.1	7.58	10.52
16	B-	B3	B-	3.81	3.60	8.97
17	CCC+	Caa1	CCC+	1.53	1.48	4.04
18	CCC	Caa2	CCC	0.76	0.58	2.02
19	CCC-	Caa3	CCC-	0.17	0.20	0.72
20	CC, C	Ca	CC, C	0.22	0.18	0.75
21	D	C	D, DD, DDD	0.18	N/A	0.34
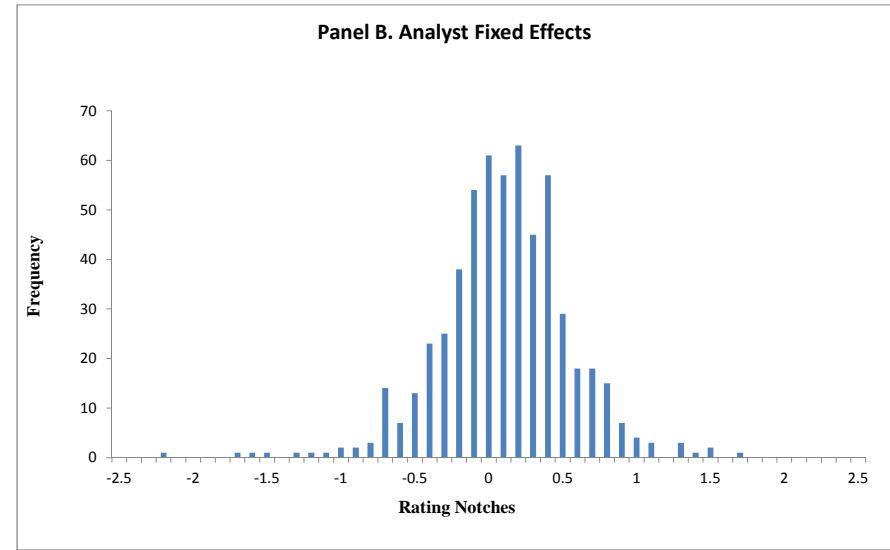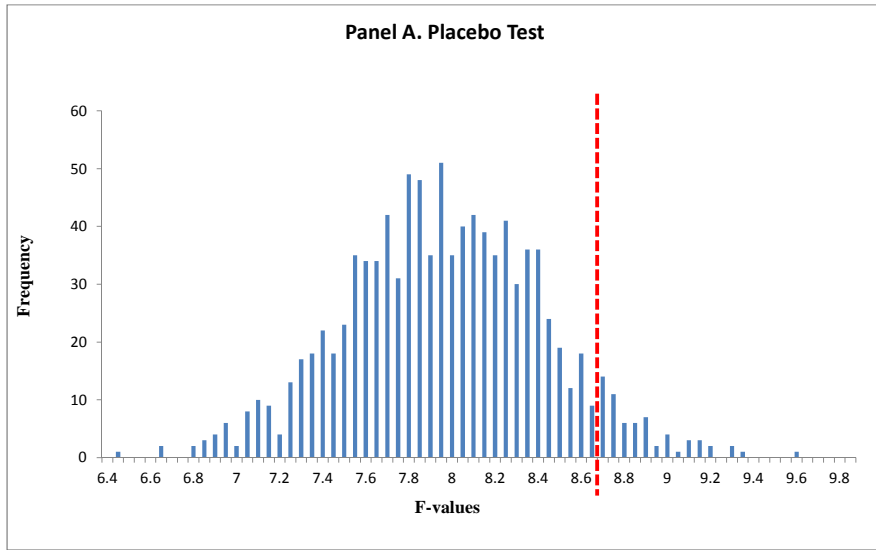
Figure 1. Histograms of placebo test results and analyst fixed effects. Panel A shows the histogram of F-statistics on 1,000 placebo runs in which we substitute the analyst name with the name of an analyst drawn randomly for each analyst-firm pair. The F-statistic is for a test of the joint significance of analyst fixed effects in an OLS regression of long-term credit ratings on analyst fixed effects, firm-quarter fixed effects, and agency-sector-quarter fixed effects. The vertical dashed line represents the F-statistic for a test of the joint significance of analyst fixed effects in the same regression specification on the real data. Panel B shows the histogram of the estimated coefficients of the analyst effects from an OLS regression of long-term credit ratings on analyst fixed effects, firm-quarter fixed effects, and agency-sector-quarter fixed effects.

Table I
Summary Statistics

This table provides summary statistics of the variables used in the paper. Panel A describes the credit rating variables used for the Wald tests as well as analyst and firm traits for each agency-firm-quarter. Panel B summarizes firm characteristics and ratings for each firm-quarter. Panel C shows the pairwise correlations of the analyst variables and ratings. All variables are defined in the Appendix.

Panel A: Agency-Firm Panel

	Obs.	Mean	Median	Std. Dev.	10th Perc.	90th Perc.
Rating Variables						
Credit Rating	53,184	11.058	11	3.453	6	15
Negative Outlook	59,674	0.181	0	0.385	0	1
Negative Watch	59,674	0.047	0	0.211	0	0
Positive Outlook	59,674	0.078	0	0.268	0	0
Positive Watch	59,674	0.014	0	0.119	0	0
Signed Watch	59,674	-0.032	0.000	0.245	0	0.000
Stable Outlook	59,674	0.413	0.000	0.492	0	1.000
Analyst and Firm Variables						
Accuracy	6,683	1.459	0	234.127	-219.230	222.050
Agency = Moody's	22,827	0.360	0	0.480	0	1
Agency = SP	22,827	0.396	0	0.489	0	1
Agency Tenure Covering the Firm	22,827	4.805	4.252	3.444	0.751	9.260
Analyst Age	22,827	39.500	39	7.689	30	49
Analyst Tenure Covering the Firm	22,827	2.086	1.751	1.729	0.249	4.381
Analyst Tenure Covering the Industry	22,827	3.475	3.127	2.267	0.832	6.510
Analyst Tenure in the Agency	22,827	6.973	5.921	4.771	2.127	12.756
Equity Analysts' Earnings Forecast Dispersion	18,773	0.015	0.026	1.231	-0.109	0.174
Female	22,827	0.257	0	0.375	0	1
Firm Age	22,827	28.776	22.764	18.539	7.501	56.038
MBA	22,827	0.735	1	0.420	0	1
MBA Non-Top 5	22,827	0.665	1	0.445	0	1
MBA Top 5	22,827	0.070	0	0.235	0	0
N. of Firms Currently Covered	22,827	13.557	11	9.663	4.5	27
N. of Equity Analysts	19,789	10.977	10.000	7.023	3	21
Number of Segments	18,324	1.606	1.000	0.885	1	3
Optimism	22,827	-0.036	0	0.955	-1	1
Rating Dispersion	22,827	0.654	0.500	0.697	0	1.500
Total Assets	22,684	36,647	4,830	166,658	787	43,312

Panel B: Firm Panel

	Obs.	Mean	Median	Std. Dev.	10th Perc.	90th Perc.
Aggregate Analyst Effects	23,386	0.053	0.067	0.601	-0.498	0.645
Bond Age (days)	15,349	1379	1,144	1075	281	2756
Bond Duration	15,349	5.456	5.139	2.488	2.567	8.712
Callable Bond Dummy	15,349	0.834	1	0.352	0	1
Carryforwards	23,386	0.052	0	0.13	0	0.162
Credit Rating	23,386	10.978	11	3.380	6	15
Credit Rating (Adjusted)	23,386	10.925	11.254	3.378	6.218	15.056
Credit Spread	15,349	324.513	254.475	239.142	81.795	693.727
Debt Issuance Spike	23,368	0.076	0	0.265	0	0
Debt Retirement Spike	23,368	0.052	0	0.223	0	0
Equity Beta	11,483	1.231	1.118	0.627	0.550	2.06
Equity Volatility	14,737	0.386	0.321	0.237	0.179	0.673
Expected Default Frequency	13,623	0.063	0	0.196	0	0.162
Change in Log Credit Spread $[t,t+1]$	8,358	0.015	0.003	0.342	-0.341	0.391
Change in Log Credit Spread $[t,t+2]$	7,791	0.031	0.025	0.434	-0.475	0.562

Continued on next page

Table I (Cont.)

Summary Statistics

Panel B: Firm Panel (Cont.)

	Obs.	Mean	Median	Std. Dev.	10th Perc.	90th Perc.
Change in Log Credit Spread $[t,t+3]$	7,287	0.048	0.043	0.517	-0.587	0.685
Change in Log Credit Spread $[t,t+4]$	6,826	0.070	0.039	0.584	-0.625	0.823
Change in Log Credit Spread $[t,t+8]$	5,312	0.204	0.159	0.776	-0.768	1.232
Change in Log Credit Spread $[t,t+12]$	4,087	0.459	0.499	0.756	-0.535	1.411
Interest Coverage k1	14,350	4.014	5	1.253	2.126	5
Interest Coverage k2	14,350	1.71	0.047	2.118	0	5
Interest Coverage k3	14,350	1.311	0	3.003	0	6.747
Interest Coverage k4	14,350	1.353	0	6.906	0	0
Long-Term Leverage	23,386	0.319	0.284	0.21	0.088	0.588
Market-to-Book	23,386	1.481	1.268	0.676	0.935	2.278
Market Value of Equity (log)	14,825	8.445	8.408	1.559	6.500	10.442
Net Equity Issuance Spike	23,368	0.016	0	0.124	0	0
Net Equity Repurchase Spike	23,368	0.097	0	0.296	0	0
Profit Margin	23,386	0.194	0.161	0.167	0.039	0.407
R&D/Sales	23,386	0.014	0	0.039	0	0.048
Sales (log)	23,386	6.675	6.602	1.428	4.916	8.535
Sales Growth	23,386	0.03	0.019	0.185	-0.15	0.205
Stock Return	14,578	0.051	0.091	0.432	-0.445	0.486
Tangibility	23,386	0.323	0.26	0.253	0.021	0.708
Taxshields	23,386	0.036	0.014	0.048	0	0.112
Time Since Last Bond Trading	15,349	5.974	1	14.275	0	17
Total Leverage	22,664	0.36	0.323	0.215	0.125	0.632

Panel C. Pairwise Correlations

	Optimism	Rating Dispersion	Accuracy	Credit Rating	MBA	Analyst Age	Female	Analyst Tenure Cov. Firm	Agency Tenure Cov. Firm	Analyst Tenure Cov. Ind.	Analyst Tenure in Agency	N. Firms Currently Covered
Optimism	1.000											
Rating Dispersion	0.001	1.000										
Accuracy	-0.361	0.002	1.000									
Credit Rating	-0.214	0.107	0.081	1.000								
MBA	-0.006	-0.033	0.011	0.041	1.000							
Analyst Age	-0.038	0.053	0.025	-0.123	-0.095	1.000						
Female	-0.050	0.026	0.040	-0.059	-0.243	0.066	1.000					
Analyst Tenure Covering Firm	0.072	-0.008	-0.038	-0.108	-0.005	0.280	-0.031	1.000				
Agency Tenure Covering Firm	0.061	0.018	-0.057	-0.090	-0.028	0.134	-0.032	0.305	1.000			
Analyst Tenure Covering Industry	0.076	0.021	0.004	-0.104	-0.045	0.349	-0.031	0.728	0.305	1.000		
Analyst Tenure in Agency	-0.010	0.064	0.023	-0.164	-0.210	0.555	0.164	0.363	0.159	0.479	1.000	
Number of Firms Currently Covered	-0.154	0.037	0.069	0.231	-0.028	0.238	-0.095	0.057	0.028	0.140	0.098	1.000

Table II
Wald Test and Placebo Simulation: Credit Ratings

The table reports the F-statistics to test the joint significance of the analyst fixed effects in an OLS regression of long-term credit ratings on analyst fixed effects, firm-quarter fixed effects, and either agency fixed effects (Column 1), agency-sector fixed effects (Column 2), agency-sector-quarter fixed effects (Column 3), or agency-firm fixed effects (Column 4). Sectors are measured using 2-digit GIC codes. The credit rating is a numeric variable ranging from 1 (AAA) to 21 (Default). The table also reports in the row Placebo Test the percentage of 1,000 runs in which the F-statistic to test the joint significance of analyst effects in the same regression specification on a placebo sample is greater than the F-statistic in the true data. In each placebo run, we substitute the analyst name with the name of an analyst drawn randomly for each analyst-firm pair. Significance for a traditional Wald test at the 1%, 5%, and 10% levels is denoted by ***, **, and *, respectively.

	(1)	(2)	(3)	(4)
F-Value Analyst FE	10.59 ***	9.00 ***	8.66 ***	5.54 ***
P-Value Analyst FE	<0.1%	<0.1%	<0.1%	<0.1%
Placebo Test P-Value Analyst FE	<0.1%	1.8%	5.8%	6.3%
Analyst FE	Yes	Yes	Yes	Yes
Firm-Quarter FE	Yes	Yes	Yes	Yes
Agency FE	Yes			
Agency-Sector FE		Yes		
Agency-Sector-Quarter FE			Yes	
Agency-Firm FE				Yes
N. Observations	53,184	52,763	52,763	53,184

Table III

Credit Spreads and Aggregate Analyst Effects

The table reports coefficient estimates from OLS regressions. The dependent variable is Credit Spread, the firm-level volume-weighted average of the credit spreads of all outstanding bonds issued by the firm. All variables are defined in the Appendix. Panel A includes all observations. Panel B includes only firm-quarter observations in which a firm does not have a split rating. Columns (3) and (5) include only observations in which the credit rating is between AAA and A-, and columns (4) and (6) include observations in which the rating is between BBB and BB+. Standard firm controls are long-term leverage, profit margin, market-to-book, sales (log), tangibility, tax shields, carry forwards, and R&D/Sales; coefficient estimates are reported in the Online Appendix. Robust t-statistics double-clustered at the firm and quarter levels are reported in parentheses below the coefficients. Constant included. Significance at the 1%, 5%, and 10% levels is denoted by ∗∗∗, ∗∗, and ∗, respectively.

	Panel A: Full Sample				Panel B: Only Firm-Quarters with No Split Ratings	
	All Ratings		AAA/A-	BBB/BB+	AAA/A-	BBB/BB+
	(1)	(2)	(3)	(4)	(5)	(6)
Adjusted Credit Rating	48.637 ***	31.834 ***	9.260 ***	50.224 ***	8.885 **	52.649 ***
	(26.53)	(14.30)	(2.90)	(9.33)	(2.33)	(9.91)
Aggregate Analyst Effects	35.324 ***	25.754 ***	0.001	50.850 ***	-0.134	59.927 ***
	(8.62)	(6.61)	(0.00)	(6.90)	(-0.02)	(7.90)
Bond Duration	-2.617 **	0.936	3.546 ***	2.142	3.012 **	2.918 **
	(-1.96)	(0.85)	(3.19)	(1.62)	(2.32)	(2.09)
Callable Bond Dummy	-39.291 ***	-0.111	26.688 *	4.101	24.647	11.408
	(-3.59)	(-0.01)	(1.90)	(0.40)	(1.49)	(1.01)
Bond Age	0.006 **	0.010 ***	0.019 ***	0.013 ***	0.022 ***	0.013 ***
	(2.18)	(3.54)	(4.73)	(3.41)	(4.53)	(3.92)
Time Since Last Trade	0.778 ***	-0.055	0.412	-0.031	0.434	-0.088
	(5.09)	(-0.36)	(1.02)	(-0.16)	(0.79)	(-0.52)
Interest Coverage k1		-8.280 **	-6.029	2.883	-3.285	-4.200
		(-2.37)	(-0.70)	(0.73)	(-0.39)	(-1.07)
Interest Coverage k2		0.426	0.240	-0.525	0.542	-0.743
		(0.26)	(0.09)	(-0.24)	(0.20)	(-0.27)
Interest Coverage k3		0.941	-1.481	2.723 *	-1.475	1.949
		(0.85)	(-1.55)	(1.72)	(-1.25)	(1.01)
Interest Coverage k4		0.885	-0.038	0.435	-0.123	-0.345
		(1.16)	(-0.12)	(0.45)	(-0.34)	(-0.48)
Total Leverage		13.981	-51.882	64.341	-58.191	39.550
		(0.40)	(-1.21)	(0.86)	(-1.22)	(0.49)
Mkt. Value of Equity (log)		-29.468 ***	-15.153 **	-19.118 **	-13.750 *	-21.227 ***
		(-6.29)	(-2.37)	(-2.50)	(-1.75)	(-3.28)
Equity Beta		-4.693	-22.575 ***	-3.383	-30.489 **	-12.655
		(-0.92)	(-2.69)	(-0.46)	(-2.43)	(-1.46)
Equity Volatility		233.308 ***	231.095 ***	213.748 ***	240.566 ***	246.825 ***
		(7.29)	(3.98)	(4.97)	(3.25)	(4.36)
Exp. Default Frequency		130.471 ***	96.795 ***	115.609 ***	83.379 ***	144.706 ***
		(6.35)	(3.34)	(3.69)	(2.94)	(3.26)
Stock Return (log)		-22.517 ***	-20.425	-21.018 *	-21.891	-10.915
		(-2.79)	(-1.58)	(-1.75)	(-1.49)	(-0.81)
Standard Firm Controls	No	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.734	0.813	0.686	0.777	0.673	0.800
Observations	15,349	9,259	2,050	3,067	1,449	1,833
p-value for t-test that Adj. Credit Rating = Aggr. Analyst Effects	<0.001	0.057	0.007	0.910	0.049	0.229

Table IV
Future Bond Returns and Aggregate Analyst Effects

The table reports coefficient estimates from OLS regressions. The dependent variable is the forward change in the natural logarithm of Credit Spread (the firm-level volume-weighted average of the credit spreads of all outstanding bonds issued by the firm), measured over the interval indicated in the column heading (in quarters). All variables are defined in the Appendix. Standard firm controls are long-term leverage, profit margin, market-to-book, sales (log), tangibility, tax shields, carry forwards, and R&D/Sales; coefficient estimates are reported in the Online Appendix. Robust t-statistics double-clustered at the firm and quarter levels are reported in parentheses below the coefficients. Constant included. Significance at the 1%, 5%, and 10% levels is denoted by ***, **, and *, respectively.

	$\ln(S_{t+1})-\ln(S_t)$	$\ln(S_{t+2})-\ln(S_t)$	$\ln(S_{t+3})-\ln(S_t)$	$\ln(S_{t+4})-\ln(S_t)$	$\ln(S_{t+8})-\ln(S_t)$	$\ln(S_{t+12})-\ln(S_t)$
	(1)	(2)	(3)	(4)	(5)	(6)
Adjusted Credit Rating	-0.004	-0.007	-0.008	-0.012 *	-0.016	-0.025 **
	(-1.54)	(-1.53)	(-1.48)	(-1.78)	(-1.59)	(-2.20)
Aggregate Analyst Effects	-0.009 ***	-0.016 **	-0.020 **	-0.025 **	-0.043 ***	-0.046 **
	(-5.95)	(-2.26)	(-2.21)	(-2.18)	(-2.60)	(-2.10)
Bond Duration	-0.011 ***	-0.014 ***	-0.016 ***	-0.018 ***	-0.030 ***	-0.040 ***
	(-3.61)	(-3.73)	(-3.67)	(-3.99)	(-4.29)	(-6.09)
Callable Bond Dummy	-0.029	-0.041 **	-0.041 **	-0.088 ***	-0.116 ***	-0.155 ***
	(-1.63)	(-2.37)	(-2.11)	(-3.83)	(-3.62)	(-3.47)
Bond Age	-0.000 ***	-0.000 ***	-0.000 ***	-0.000 ***	-0.000 ***	-0.000 ***
	(-3.96)	(-3.39)	(-4.04)	(-3.40)	(-3.79)	(-4.03)
Time Since Last Trade	0.000	-0.000	0.000	0.002	-0.000	-0.001
	(0.33)	(-0.20)	(0.55)	(1.57)	(-0.35)	(-1.16)
Interest Coverage k1	0.002	0.006	0.010	0.016 **	0.025 *	0.029
	(0.66)	(1.46)	(1.58)	(2.19)	(1.83)	(1.64)
Interest Coverage k2	0.001	-0.002	-0.002	-0.003	-0.007	-0.006
	(-0.37)	(-0.85)	(-0.70)	(-0.87)	(-0.94)	(-0.61)
Interest Coverage k3	-0.001	0.002	0.002	0.001	0.002	-0.002
	(-1.60)	(0.99)	(0.93)	(0.33)	(0.36)	(-0.30)
Interest Coverage k4	-0.000	-0.001	-0.002 **	-0.002 **	-0.002	-0.000
	(-0.92)	(-1.36)	(-2.13)	(-2.00)	(-1.15)	(-0.08)
Total Leverage	0.072	0.127 *	0.159 *	0.158	0.146	0.312
	(1.61)	(1.96)	(1.88)	(1.61)	(0.79)	(1.27)
Mkt. Value of Equity (log)	0.001	0.004	0.008	0.001	0.011	0.002
	(0.22)	(0.50)	(0.73)	(0.11)	(0.53)	(0.06)
Equity Beta	-0.002	0.010	0.009	0.005	0.021	0.081 **
	(-0.22)	(0.78)	(0.53)	(0.26)	(0.88)	(2.33)
Equity Volatility	-0.059	-0.110 *	-0.096	-0.052	-0.090	-0.431 **
	(-1.49)	(-1.89)	(-1.26)	(-0.64)	(-0.81)	(-1.97)
Exp. Default Frequency	0.023	0.049	0.042	0.041	0.064	-0.171
	(0.57)	(0.71)	(0.52)	(0.48)	(0.72)	(-1.48)
Stock Return (log)	0.010	0.012	-0.011	-0.023	-0.060	-0.125 **
	(0.87)	(0.58)	(-0.52)	(-0.96)	(-1.53)	(-2.33)
Standard Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.255	0.432	0.526	0.593	0.680	0.639
Observations	8,358	7,791	7,287	6,826	5,312	4,087

Table V

Corporate Policies and Aggregate Analyst Effects

The table reports coefficient estimates and the pseudo R-squared from logit regressions in columns (1), (3), (4), (5), and (6), and coefficient estimates and R-squared from OLS regressions in columns (2) and (7). The dependent variable is displayed at the top of each column. All variables are defined in the Appendix. Industries are measured using the Fama-French 49 classification. Columns in Panel A include only firm-quarter observations in which there is at least one debt or equity issuance. Columns in Panel B include all observations. Robust t-statistics double-clustered at the firm and quarter levels are reported in parentheses below the coefficients. Constant included. Significance at the 1%, 5%, and 10% levels is denoted by ∗∗∗, ∗∗, and ∗, respectively.

	Panel A: Conditioning on Issuing Equity or Debt		Panel B: Full Sample				
	Debt Issuance Spike Logit	Offering Yield-to-Maturity OLS	Debt Retirem. Spike Logit	Debt Issuance Spike Logit	Equity Rep. Spike Logit	Equity Iss. Spike Logit	Sales Growth OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Adjusted Credit Rating	-0.119 ***	0.294 ***	0.158 ***	0.035 ***	-0.160 ***	0.120 ***	-0.006 ***
	(-2.85)	(7.08)	(7.81)	(2.73)	(-5.89)	(3.48)	(-6.14)
Aggregate Analyst Effects	-0.516 ***	0.248 ***	0.187 **	-0.052	-0.267 ***	0.310 **	-0.011 ***
	(-2.83)	(2.98)	(2.36)	(-1.18)	(-3.18)	(2.20)	(-4.88)
Long-Term Leverage	-2.586 ***	0.206	0.457 **	-0.701 ***	-1.677 ***	0.922 ***	0.006
	(-6.27)	(0.36)	(2.42)	(-4.48)	(-5.17)	(3.14)	(0.69)
Profit Margin	0.256	-1.744 ***	0.412	0.593 **	0.773 *	0.449	-0.204 ***
	(0.55)	(-3.73)	(1.23)	(2.30)	(1.71)	(1.24)	(-7.38)
Market-to-Book	0.147	-0.474 ***	0.159 ***	0.276 ***	0.756 ***	0.278 ***	0.018 ***
	(0.95)	(-3.44)	(2.64)	(5.26)	(9.22)	(3.87)	(6.79)
Sales (log)	0.166 **	-0.468 ***	-0.006	-0.132 ***	0.064	-0.324 ***	-0.022 ***
	(2.15)	(-8.01)	(-0.20)	(-4.04)	(1.30)	(-4.03)	(-9.23)
Tangibility	-0.365	0.643 **	-0.781 ***	0.579 **	-1.045 ***	0.746 *	0.033 ***
	(-0.67)	(1.97)	(-3.23)	(2.39)	(-3.05)	(1.71)	(2.65)
Taxshields	0.222	0.515	-2.299 **	-2.178 **	2.261 *	-3.725 **	0.084 ***
	(0.11)	(0.38)	(-2.24)	(-2.31)	(1.71)	(-2.15)	(2.64)
Carryforwards	0.049	0.641	-0.004	-0.420	-0.064	-0.824	-0.020 **
	(0.08)	(1.40)	(-0.02)	(-1.52)	(-0.19)	(-1.62)	(-2.07)
R&D/Sales	-2.950	0.179	0.978	1.798 *	-0.262	2.515	0.099
	(-1.25)	(0.10)	(1.03)	(1.74)	(-0.16)	(1.42)	(1.37)
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2 / Pseudo R^2	0.164	0.589	0.062	0.044	0.217	0.126	0.076
Observations	1,718	3,064	23,368	23,290	23,278	22,547	23,386

Table VI
Optimism and Accuracy

The table reports coefficient estimates from OLS regressions. The dependent variable is displayed at the top of each column. Optimism is the product of -1 times the difference in each firm-quarter between the analyst's rating of the firm and the average rating of the other analysts covering the firm. Rating Dispersion is the absolute value of the difference in each firm-quarter between the analyst's rating of the firm and the average rating of the other analysts covering the firm. Accuracy is the product of -1 times Optimism and the forward change in credit spreads over a 3-year horizon, measured starting at the end of the quarter. All variables are defined in the Appendix. Robust t-statistics clustered at the firm-quarter level are reported in parentheses below the coefficients. Constant included. Significance at the 1%, 5%, and 10% levels is denoted by ***, **, and *, respectively.

	Optimism		Rating Dispersion		Accuracy	
	(1)	(2)	(3)	(4)	(5)	(6)
MBA	-0.149 ***		0.077 ***		43.708 **	
	(-3.21)		(6.44)		(2.10)	
Top-5 MBA		-0.344 ***		0.144 ***		128.380 ***
		(-4.03)		(6.49)		(2.96)
Non-Top 5 MBA		-0.131 ***		0.071 ***		38.55 *
		(-2.82)		(5.90)		(1.92)
Analyst Age	-0.003	-0.004	0.001	0.001	-0.781	-0.492
	(-1.13)	(-1.52)	(0.73)	(1.25)	(-0.55)	(-0.35)
Female	-0.320 ***	-0.315 ***	-0.010	-0.012	34.657	37.688
	(-5.98)	(-5.90)	(-0.83)	(-0.97)	(1.32)	(1.43)
Analyst Tenure Covering the Firm	0.108 ***	0.105 ***	0.004	0.005	-13.326 **	-12.388 **
	(7.32)	(7.11)	(1.19)	(1.45)	(-2.22)	(-2.08)
Agency Tenure Covering the Firm	0.007	0.006	-0.002	-0.002	-3.499 *	-3.611 *
	(1.09)	(1.00)	(-1.24)	(-1.13)	(-1.72)	(-1.78)
Analyst Tenure Covering the Industry	-0.001	0.004	-0.006 *	-0.008 **	7.377	8.096
	(-0.08)	(0.30)	(-1.96)	(-2.48)	(1.30)	(1.42)
Analyst Tenure in the Agency	-0.029 ***	-0.030 ***	0.004 ***	0.004 ***	4.459 **	3.875 *
	(-6.70)	(-6.84)	(3.14)	(3.31)	(2.29)	(1.96)
N. of Firms Currently Covered	-0.007 ***	-0.007 ***	-0.002 ***	-0.002 ***	4.097 ***	4.281 ***
	(-2.70)	(-2.69)	(-2.92)	(-2.95)	(3.81)	(3.97)
Agency = Moody's	-0.106 **	-0.115 ***	0.064 ***	0.067 ***	-45.394 **	-36.267 *
	(-2.41)	(-2.60)	(4.46)	(4.69)	(-2.52)	(-1.94)
Agency = SP	0.231 ***	0.213 ***	0.032 ***	0.038 ***	-17.963	-8.568
	(5.94)	(5.39)	(2.67)	(3.19)	(-1.07)	(-0.50)
Firm-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.069	0.071	0.015	0.018	0.020	0.023
Observations	22,827	22,827	22,827	22,827	6,683	6,683

Table VII
Accuracy: Cross-Sectional Analysis

The table reports coefficient estimates from OLS regressions splitting the sample at the median value of the variable reported at the top of the column. The dependent variable is Accuracy, the product of -1 times Optimism and the forward change in credit spreads over 3 years, measured starting at the end of the quarter. All variables are defined in the Appendix. All specifications include the same control variables as in Table VI; coefficient estimates for the full set of controls are reported in the Online Appendix. Robust t-statistics clustered at the firm-quarter level are reported in parentheses below the coefficients. For each split sample, we also report the two-tailed p-value of a two-sample t-test for equality of the coefficient estimates across the two subsamples. Constant included. Significance at the 1%, 5%, and 10% levels is denoted by ∗∗∗, ∗∗, and ∗, respectively.

	Total Assets		Firm Age		Number of Segments		Number of Equity Analysts		Equity Analysts' Earnings Forecast Dispersion	
	Low	High	Low	High	Low	High	Low	High	Low	High
MBA	109.603 ***	0.170	89.882 **	4.464	-22.836	68.911	94.446 **	40.541	-10.170	129.837 ***
	(2.83)	(0.01)	(2.26)	(0.18)	(-0.68)	(1.64)	(2.56)	(1.41)	(-0.37)	(3.60)
		0.011**		0.068*		0.088*		0.249		0.002***
Analyst Tenure Covering Firm	-12.791	-1.787	-26.701 ***	1.395	-0.369	-18.960 *	-16.415	-8.166	-1.011	-22.150 *
	(-1.07)	(-0.30)	(-2.61)	(0.20)	(-0.03)	(-1.94)	(-1.37)	(-1.43)	(-0.16)	(-1.85)
		0.412		0.022**		0.202		0.534		0.116
Firm-Quarter FE, Agency FE, and Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.054	0.025	0.040	0.037	0.035	0.051	0.040	0.051	0.032	0.058
Observations	3,336	3,334	3,342	3,341	2,673	2,575	3,162	3,148	3,056	3,066