Are Credit Ratings Subjective? The Role of Credit Analysts in Determining Ratings*

Cesare Fracassi
University of Texas – Austin

Stefan Petry
University of Melbourne

Geoffrey Tate
University of North Carolina – Chapel Hill

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Abstract

Credit ratings affect firms' access to capital and investment choices. We show that the identity of the credit analysts covering a firm significantly affects the firm's rating, comparing ratings for the same firm in the same quarter across agencies. Analyst effects account for 30% of the within variation in ratings and 70%-80% of the effect carries through to credit spreads. Analysts with MBAs provide less optimistic and more accurate ratings; however, optimism increases and accuracy decreases with tenure covering the firm, particularly among information-sensitive firms. The market responds more to downgrades from long-tenured analysts, consistent with increased leniency over time.

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Key words: Credit Analysts, Credit Ratings, Credit Spreads, Analyst Biases

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Credit ratings ostensibly provide information on the credit-worthiness of corporate borrowers. Market participants may use them as a way to gauge the probability of default in the event of a new debt issue. If so, they can have an effect both on firms' access to new capital and on the terms at which they can borrow. Moreover, ratings directly affect the clientele for debt instruments as they determine whether assets count toward banks' capital requirements and whether they are in the universe of assets in which pension funds are allowed to invest. But, how are corporate credit ratings determined?

We construct a novel dataset containing long-term corporate issuer ratings from all three major credit agencies and the identities of the individual analysts responsible for each rating. We gather additional demographic information on each analyst, including age, gender, education, and tenure covering the firm. We find that individual credit analysts exert a significant influence on firms' long-term credit ratings that cannot be explained by firm, time, or rating agency effects. Moreover, these effects carry through to credit spreads on the firms' outstanding debt. We identify both skill and biases as important factors explaining the analyst effects.

According to Standard and Poor's, their credit ratings "express forward-looking opinions about the creditworthiness of issuers and obligations. Issuers and obligations with the highest ratings are judged…to be more creditworthy than issuers and obligations with lower credit ratings." (Standard and Poor's, 2009). They identify likelihood of default as the primary rating factor and payment priority, projected recovery rates, and credit stability as secondary factors. Thus, ratings agencies endeavor to provide a sufficient statistic for the key inputs to the expected financial distress costs of rated firms. Given the visibility of ratings, they are likely to exert a significant influence on market participants' expectations of those costs. If so, ratings can affect not only the ease with which firms can access new debt capital, but also the cost of that capital. If these assessments are incorrect, then they may skew corporate capital structures suboptimally toward or against debt (depending on whether the ratings over- or understate default costs).

They may also affect the overall ability of the firm to raise capital on fair terms, resulting in an inefficient allocation of capital across projects in the economy.[1]

We study a relatively unexplored aspect of corporate credit ratings: the influence of individual credit analysts on the rating process. Though the rating agencies stress their focus on measuring the fundamentals of rated firms, the identity of the analyst covering the firm may matter if analysts gather different information before reaching a rating recommendation. Alternatively, different analysts may interpret the same information differently, even if the information gathering process is standardized within the agency. Moreover, analysts covering a firm develop long-term relationships with firm management – at least prior to the implementation of the Dodd-Frank Act in 2010 – creating the potential for conflicts of interest or bias arising from familiarity with the rated firms.[2]

We test whether individual analysts matter for the long-term credit ratings of U.S. firms using a novel dataset that links corporate ratings information from all three major ratings agencies with information on the analysts who generate each report. First, we demonstrate that the identity of the credit analyst can explain a significant portion of the variation in credit ratings in a regression model containing fixed effects for each firm-quarter and each of the three ratings agencies. By comparing only analysts covering the same firm at the same point in time for different rating agencies, we mitigate the effect of selection in the assignment of analysts to covered firms.

Since the dependent and independent variables are both persistent, we assess the statistical significance of the result using a resampling procedure in which we randomly reassign sample analysts to different observed firm-analyst spells in the data. We compare the F-statistics

[1] The recent financial crisis provides evidence that ratings may indeed be affected by systematic errors or biases. In January of 2011, the Financial Crisis Inquiry Commission reported that "the three credit rating agencies were key enablers of the financial meltdown" (FCIC, 2011) and, in February of 2013, the Department of Justice brought suit against S&P for fraudulently inflating ratings on mortgage-backed instruments prior to the financial crisis. Several recent papers address the issue of rating accuracy in this context (e.g., Griffin and Tang, forthcoming; Benmelech and Dlugosz, 2009). Our focus instead is on corporate issuer ratings and the link to the cost of debt capital.

[2] 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. However, this exemption ended with the passage of Dodd-Frank (Purda, 2011).

for the estimated analyst effects in the true data to the F-statistics for estimated analyst effects from the same regression on the data resulting from 1,000 reassignments. We find that analysts matter for credit ratings, considering only variation across analysts covering the same firm in the same quarter and correcting for systematic rating agency effects. The effects are also economically meaningful: analyst fixed effects explain 29.55% to 31.57% of the contemporaneous variation in ratings across agencies covering the same firm, an order of magnitude larger than the explanatory power provided by agency fixed effects.

We run a number of robustness checks, including an alternative specification in which we correct for an agency-firm fixed effect. Even when we allow for systematic biases across agencies with respect to particular firms, we continue to find that individual analysts explain significant variation in long-term credit ratings. Moreover, analyst identities matter for the short-term watches that agencies release about firms' issuer ratings.

We also measure the degree to which the effect of analysts on credit ratings translates to credit spreads. First, we calculate the implied change in spreads for a one standard deviation change in an analyst fixed effect (0.63 notches), assuming that the market does not distinguish between ratings driven by analyst effects and fundamentals.[3] For firms with ratings around the investment grade cutoff, we find that a one category increment in rating has a 75 basis point effect on credit spreads, implying that a 0.63 notch increment to ratings has a 47 basis point impact on spreads. Second, we estimate the impact of ratings on spreads, but allowing the market to respond to the estimated analyst effects. We find that the market undoes between 20% and 30% of the effect of analysts on ratings. For firms around the investment grade cutoff, a 0.63 notch change in ratings driven entirely by the identity of the analyst would translate to a 37 basis point increment in credit spreads, accounting for this adjustment.

We then investigate the mechanism by which analysts matter. We measure three channels through which analysts can have a systematic influence on ratings: differences in rating levels,

[3] This is also approximately equal to replacing an analyst from the 25th percentile of the distribution of estimated analyst effects with an analyst from the 75th percentile.

differences in rating dispersion, and differences in rating accuracy. Using LinkedIn, we gather demographic information for roughly two thirds of the analysts in our sample, including age, gender, and educational background. We then 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. Consistent with an effect of skill or expertise on rating quality, we find that analysts with MBAs and with longer tenure in the rating agency provide less optimistic ratings that are more accurate over a 2- or 3- year horizon. They are also more likely to deviate from other analysts in their assessments of covered firms. However, we also uncover a dark side to long-term matches between firms and credit analysts. We find that rating quality deteriorates with the length of time analysts have covered a particular firm: ratings become more optimistic, but also less accurate over a 3-year horizon. Thus our results provide a potential mechanism for "sluggishness" in downward ratings adjustments, a feature of ratings that generated considerable attention from policymakers in the wake of the Enron and Worldcom scandals as well as the recent Lehman Brothers bankruptcy (White, 2010).

We also ask whether the effects of analyst skill or biases differ depending on the nature of the rated firm, using five proxies for information sensitivity to partition the sample: firm size, firm age, diversification, the number of equity analysts following the firm, and the dispersion in earnings forecasts. We find mixed evidence with respect to analyst optimism. Where the differences across samples are statistically significant, they suggest that analysts with MBAs are particularly less optimistic than their peers among more informationally sensitive firms. Likewise, analysts with long tenure covering the firm are particularly more optimistic than their peers in such firms. The patterns in analyst accuracy are stronger. In almost all cases, we find that the enhanced accuracy of analysts with MBAs is particularly strong among informationally sensitive firms. We see a similar pattern using an alternative proxy for analyst skill: tenure covering the industry. We also find that the compromised accuracy of long-tenured analysts is particularly prevalent among such firms. Thus, our results suggest that the identity of analysts is likely to lead to the most variation in ratings (and, hence, credit spreads) precisely among firms that are the most likely to face financing constraints due to information frictions.

Finally, we test whether information about the analysts covering the firm affects the stock market reaction to announcements of ratings upgrades and downgrades. If ratings convey information to the market and investors recognize differences in the quality of information provided by different analysts, then we would expect larger reactions to ratings changes by better analysts, all else equal. We find significant positive (negative) reactions to market upgrades (downgrades). However, consistent with the results on credit spreads, we find that the market only partially responds to analyst effects. We find some evidence of analyst fixed effects on market reactions to downgrades, but we generally find little evidence that the market accounts for the variability in ratings quality depending on observable analyst traits. An exception is the reaction to ratings changes by analysts with a long tenure covering the firm. We find that the market reacts less positively to upgrades by longer tenured analysts (though the effect is not statistically significant), and significantly more negatively to ratings downgrades by long-tenured analysts. This pattern is consistent with the market recognizing that worse news is necessary to generate a downgrade from such analysts relative to shorter tenured peers.

Overall, we demonstrate that credit ratings differ across firms due to the skills or biases of the individual analysts rating the firms, even after accounting for differences in firm fundamentals. These differences carry through to the firms' costs of debt capital, potentially impacting investment and firm value. Our results also point to biases or conflicts of interest arising from long tenure covering a firm as a mechanism. To the extent that the relative optimism of such analysts lowers the cost of capital and generates overinvestment, some costs to claimholders may be mitigated by appropriate regulation of the credit rating agencies.

Our results contribute to the literature on corporate credit ratings. Recent papers find significant links between ratings and investment and corporate financing choices (Chernenko and Sunderam, 2012; Kisgen, 2006). We provide direct evidence of a channel from ratings to the cost of debt capital and show that the relation varies with the identity of the analysts responsible for the ratings. We also provide a new angle on the economics behind split bond ratings. While existing research emphasizes the opacity of the assets (Livingston, Naranjo, and Zhou, 2007; Morgan, 2002), we show that analyst biases can explain a significant fraction of such cases.

Our analysis parallels a large literature that studies the impact of sell-side equity analysts on recommendations, forecasts, and firm value. Prior work has identified a number of 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). Many studies also identify effects of conflicts of interest on the quality of equity analyst recommendations (Lin and McNichols, 1998; Michaely and Womack, 1999). Though our results complement the findings in these papers, it is important to note the differences in the objectives of ratings analysts and sell-side equity analysts, and therefore the differences in the constituencies for and likely effects of their output. 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 unlikely to tell us much about credit markets (or link as readily to costs of capital). There has been considerably less work focusing on ratings analysts. This oversight is surprising given that the channels through which ratings analysts can influence real corporate decisions appear more direct than the corresponding channels for sell-side equity analysts. For example, firms typically solicit input from the ratings agencies on how the financing of major projects like acquisitions will impact their credit ratings. A recent exception is Cornaggia, Cornaggia and Xia (2012) 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 takes advantage of a recent law change that requires such relationships to be disclosed and, as a result, cannot address the effect of the larger set of analysts who do not move to covered firms.

The remainder of the paper is organized as follows. In Section I, we describe the process by which we collect our credit analyst data and the construction of the samples used in our empirical analysis. Section II presents our main results demonstrating a significant effect of analysts on ratings outcomes, controlling for time-varying firm effects and agency effects. In Section III, we explore the mechanisms through which analysts affect ratings and in Section IV, we measure market reactions. 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 is sparse prior to 2000, we restrict our sample to announcements between 2000 and 2011. Our goal is to measure differences in the ability to access additional debt capital; so, we focus on long-term issuer ratings. 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.[4] Our final sample consists of 44,829 announcements on 1,721 firms, of which 571 belonged to the S&P500 index at some point during the sample period.[5]

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. Long-term issuer ratings measure the ability of firms to honor senior unsecured financial obligations. To minimize measurement error in the identity of the analysts covering the firm, we do not assign analysts to quarters beyond the date of the final report in which we observe the analyst covering the firm. We also use Standard and Poor's long-

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

term issuer ratings retrieved from Compustat to verify the accuracy of our data.[6] 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. 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 38% of firm-quarters and, among those observations, we observe split ratings 57% of the time (or in 8,075 distinct firm-quarters).

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 information from LinkedIn. Of the 1,072 unique analysts in our data, we are able to retrieve data for 798.[7] We extract biographical information on age as well as the professional and educational background of the analysts. Educational background (school, degree, and degree date) are 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

[6] 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.
[7] We require that the individual in LinkedIn have the exact same first and last name as the analyst from the rating report and have listed professional experience at one of the three rating agencies.

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 over the horizon h (where h is 1, 2, or 3 years) by multiplying -1 times relative optimism by the forward change in credit spreads over horizon h, measured starting at time t.[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 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.

We also supplement the data with accounting information from Compustat and equity analyst information from I/B/E/S to allow us to test for differences in the impact of analyst traits in different types of firms. 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 deviation of the earnings forecasts divided by their mean. In

[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.

Panel B of Table I, we provide summary statistics of the data for the subsample on which the analyst traits are available.[10] 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 $37.5 billion in assets, and is covered by 11 equity analysts. Panel D presents selected pairwise correlations of the variables.

Finally, we conduct an analysis of market reactions at the ratings announcement level. Panel C of Table I provides summary statistics of the sample of ratings announcements for which our measures of analyst traits are available. In our data, rating affirmations (5,336) are more common than upgrades (1,179) or downgrades (1,858). On average, the magnitude of the stock price decline in response to a downgrade (2.6% over a three day event window surrounding the announcement) is larger than the increase following an upgrade (0.7%), though both are statistically significant. This pattern, which mirrors the findings in Jorion, Zhu and Shi (2005), is consistent with market belief in an optimistic bias in ratings, rendering ratings downgrades more informative than upgrades. We do not observe a significant market response to affirmations.

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 main regression specification is the following:

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

[10] 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).

In our main tests, $Rating_{ijt}$ is the long-term issuer rating for firm *j* in quarter *t* by rating agency i. Later, we consider additional dependent variables related to ratings watches and long-term outlooks. α_{jt} is a firm-quarter fixed effect and β_i is a rating agency fixed effect. $\boldsymbol{\gamma}_{analyst}$ represents 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.

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.

Our approach also mitigates selection concerns. Analysts are typically assigned to cover firms based on their interests and expertise. Because we identify analyst effects by comparing only analysts who cover the same firm at the same time, the interpretation of our results is not clouded by this endogenous matching. A potential remaining concern is that agencies reassign analysts to cover different firms over time, depending on the performance of the ratings or firm (i.e., not randomly) and differently across agencies (so the sorting is not corrected by the firm-quarter fixed effects). However, this kind of reshuffling does not appear to be a practical concern: agencies rarely reassign analysts to cover different firms, perhaps because they perceive a cost from sacrificing match-specific expertise.[11]

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. 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, 2002). 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.[12] Instead, we use a resampling approach to gauge the significance of the analyst effects. 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. We then randomly reassign our 1,072 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

[11] To assess the importance of this potential sorting mechanism, we had extensive conversations with a credit analyst for one of the major agencies who provided information on the process by which analysts are initially assigned to cover firms and confirmed that this kind of analyst reshuffling over time is not common practice.

[12] 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.

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.

We also go a step further, imposing an even higher identification hurdle on our analysis. We modify equation (1) as follows, allowing for the rating agency effect to differ for each individual firm (β_{ij}):

$$Rating_{ijt} = \alpha_{jt} + \beta_{ij} + \boldsymbol{\gamma}_{analyst} + \epsilon_{ijt} \tag{2}$$

In this specification, we identify the analyst effects using only firms that are covered during the sample period by multiple analysts for the same agency at different points in time. Thus, our estimates are robust to the possibility that agencies favor individual firms independently from the analysts covering those firms and the firms' fundamentals. Again, we assess statistical significance using our resampling procedure.

Another possible way to generalize equation (1) would be to allow the agency fixed effect β_i to vary with time. The firm-quarter fixed effects in equations (1) and (2) absorb time-series variation at the level of the firm, but cannot absorb differences in the time series of ratings at the agency level. For example, there may be a sample year in which S&P changes its ratings methodology across the board in a way that makes all of its ratings systematically less optimistic relative to the other agencies. We estimate such a specification as a robustness check, finding results that are nearly identical to the results from estimating model (1). Thus, we focus on models (1) and (2) throughout our analysis.

II.B. Long-term Issuer Ratings

In Panel A 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 8.45 for the test that the analyst effects jointly equal 0 (Column 1). In Panel A of Figure 1, we present a histogram of the F-statistics from the placebo samples, indicating the F-statistic from the true sample with a red dotted line. The true F-statistic of 8.45 is larger than 948 out of 1,000 F-statistics computed on the placebo samples. Thus, we compute a *p*-value of 0.052 for our null.[13]

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 0.3192. To provide a lower bound on how much of this explanatory power comes from the analyst effects, we re-estimate equation (1), but excluding the analyst effects. We find an adjusted within R^2 of 0.0237. Thus, the agency fixed effects explain at most 2.37% of the variation, implying that the analyst fixed effects account for at least 29.55%. We also compute an upper bound by re-estimating equation (1), but excluding the agency fixed effects. The adjusted within R^2 is 0.3157, implying that the analyst effects explain at most 31.57% of the within variation in ratings. As an alternative approach, we also ask how big of a change in the cost of debt – measured by credit spreads – is implied by a reasonable change in an analyst fixed effect. In our data, a move from the 25th to the 75th percentile of the distribution of estimated analyst effects would increase ratings by roughly 0.58 notches. A standard deviation of the distribution is 0.63. We graph the full distribution of the estimated analyst effects in Panel A of Figure 2. To translate this effect into credit spreads, we regress the value weighted credit spread across each firm's outstanding bond issues on long-term issuer ratings. We run the regression separately on five quintiles of the ratings distribution to allow for

[13] Note that this result confirms that our test provides a higher hurdle than the Wald test itself, since the F-statistic of 8.45 implies a *p*-value for the null of a zero effect that is (far) less than 0.001.

a non-linear effect. We include controls for bond age, duration, callability, and liquidity. We also include quarter fixed effects to capture market conditions. We estimate the model with and without firm fixed effects, finding similar estimates for the effect of ratings on yield spreads. The median firm in the sample has a rating in the third quintile (which also includes the investment grade cutoff). Within that quintile, we estimate that a one notch increment in the long-term issuer rating significantly decreases the yield spread by roughly 75 basis points.[14] Thus, a one standard deviation change in an analyst fixed effect would change credit spreads by 0.63 * 75 or 47 basis points. We re-visit the link between analyst effects on ratings and credit spreads in Section II.D.

A potential concern for our analysis is analysts who cover relatively few firms. The analyst fixed effects are estimated with more precision the more firms the analysts cover. Moreover, fixed effects estimated from few observations could generate large outlier observations that distort our inferences. As a robustness check, we repeat our analysis, but progressively add stricter filters for inclusion in the sample. In our sample, the mean (median) number of firms covered by each analyst is 12.5 (6).[15] The 25th percentile of the distribution is 2 and the 75th percentile is 16. We begin by requiring that each analyst cover at least 5 sample firms, which is roughly equivalent to focusing on the 60% of sample analysts with the largest portfolios of covered firms. With this restriction, there remains enough variation to identify 572 distinct analyst effects. We present the results of estimating equation (1) on the restricted sample in Column 2 of Table II, Panel A. To assess significance, we again use our resampling procedure. In Panel B of Figure 1, we graph the distribution of F-statistics in 1,000 placebo samples. We find that the true F-statistic exceeds all 1,000 F-statistics from the placebo samples, implying the analyst effects are significant at a level less than 0.1%. We also consider a restricted sample that includes only analysts who cover at least 10 firms, which is equivalent to focusing on the top 40% of analysts by coverage. In this case, we are able to identify fixed effects for 405

[14] For tabulated estimates of the credit spread regressions, see the Online Appendix.
[15] Here we simply count the number of firms that each analyst covers within our sample period. Thus, the summary statistics differ from Table I, in which we report the average number of firms covered in a particular quarter.

analysts. Nevertheless, we find similar results: the F-statistic for the analyst effects in the true data is 11.91, greater than the F-statistics from 1,000 placebo samples created by reassigning analysts to random firm-analyst spells. We present a histogram of the F-statistics in Panel C of Figure 1. Thus, our full sample results appear to be conservative as a result of including infrequently observed analysts for whom we cannot estimate precise fixed effects.[16]

Next, we turn to the estimates of equation (2), which includes an interacted fixed effect for each rating agency-firm pair. In this context, we can only use cases in which we observe multiple analysts covering the same firm for the same rating agency at different points in time to achieve identification. Because of this, our assumption regarding the minimum number of firms an analyst must cover to be included in the sample proves particularly important. In Panel B of Table II, we report the results from estimating equation (2) on the full sample and imposing thresholds of 5 and 10 covered firms. We graph the distribution of the estimated analyst effects in the full sample in Panel B of Figure 2. In this sample, we find an F-statistic of 4.45 for a test that the analyst effects jointly equal 0. However, when we reshuffle the analysts to create placebo samples according to the procedure outlined above, we do not find that this result is statistically significant. Panel D of Figure 1 presents the distribution of the F-statistics in the placebo samples and indicates the placement of the true statistic (4.45) in the distribution. Similar to the estimates of equation (1), as we impose a higher hurdle for inclusion in the sample, the estimates of the analyst effects become more precise, yielding higher F-statistics. Moreover, the *p*-values from the hypothesis tests that the analyst effects jointly equal 0 decrease. When we impose the restriction that analysts must cover at least 5 firms to be included in the sample, we find an F-statistic of 5.54 with a *p*-value of 0.063.[17] Panel E of Figure 1 presents a histogram of the F-statistics in the placebo samples. Thus, we find evidence that the influence of analysts on

[16] In untabulated estimations, we repeat the same procedure, restricting the sample progressively to analysts who cover at least 2, 3, and 4 firms. We find a monotonic decline in the implied *p*-values for the analyst effects from the resampling procedure, consistent with the pattern we observe moving from the full sample to the samples restricted to analysts who cover 5 and 10 firms.

[17] Here again we estimate equation (2) on samples restricted progressively to analysts who cover 2, 3, and 4 firms (results untabulated). We find a monotonic decline in the implied *p*-values on the analyst effects. The reported result on the subsample of analysts who cover at least 5 firms is the first to cross the 10% hurdle for significance.

ratings persists even when we attribute time-invariant differences in the ratings of individual firms by different agencies to factors other than the analysts themselves. It is intuitive that the noise introduced by including rarely observed analysts with imprecisely measured individual effects would be of more consequence here since we compare the relatively small numbers of analysts within an agency covering a particular firm over time. Thus a single outlier can have a large influence on the results. Note, however, that we still observe a reasonable sample of firms in which we have multiple analysts covering at least 5 firms. Recall that the median analyst covers 6 firms. Our initial sample in Column 1 of Panel II (before imposing any restrictions on the number of firms each analyst covers) consists of 1,594 firms.[18] Of these firms, 1,377 (and 2,201 firm-agency pairs) are covered by at least two different analysts from the same agency at different points in time who cover at least 5 firms (i.e., 1,377 of 1,594 firms can be used for identification in the restricted sample). Moreover, we continue to find significant analyst effects if we further restrict the sample; Table II also reports the results from restricting the sample to analysts who cover at least 10 firms, finding a *p*-value of 0.061.

Overall, we conclude that analysts exert a significant influence on long-term issuer ratings, even controlling for unspecified time, firm, and agency effects. In Section III, we explore the specific channels through which this influence occurs.

II.C. Ratings Watches and Long-term Outlooks

We also use the methodology developed in Section II.A. to test whether individual analysts matter for agencies' decisions to place a short-term ratings watch on a firm or for the long-term outlooks they issue. Agencies use ratings watches to indicate that there is an increased likelihood that the current rating will change going forward. They also typically indicate the direction of the potential change. Watches are often driven by particular triggering events and, as such, are usually short term in nature (i.e., they can be resolved once the event itself has

[18] Note that not all 1,721 firms for which we observe announcements as described in Section II appear in this data. The reason is that not all announcements provide long-term issuer ratings, which are required for these regressions (e.g., we may only observe reports on short-term ratings, but not long-term ratings in the excluded firms).

resolved). We often observe that agencies both place a firm on a rating watch and resolve that watch within a particular firm-quarter. Thus, we construct a dependent variable that takes the value -1 if firm *j* is placed on a watch down by agency *i* at any point during the firm-quarter *t*, 1 if the firm is placed on a watch up, and 0 otherwise. We also consider separately watches up and down, defining an indicator that takes the value 1 if firm *j* is placed on a watch down by agency *i* at any point during the firm-quarter *t* and zero otherwise and a separate indicator that takes the value 1 if firm *j* is placed on a watch up by agency *i* at any point during the firm-quarter *t* and zero otherwise. We use these variables in place of $Rating_{ijt}$ in the estimation of equations (1) and (2). We use the resampling procedure described in Section II.A to assess the significance of the estimated analyst effects.

In Panel A of Table III, we present the results from estimating equation (1). In Columns 1 and 2, the dependent variables are the indicators for upward and downward watches, respectively. Though the dependent variables are binary, we estimate linear probability models to avoid the incidental parameters problems associated with fixed effects in logit and probit models (particularly since in our context the fixed effects are precisely the variables of interest). We calculate an F-statistic of 1.76 for the test that the analyst effects jointly equal 0 when the dependent variable indicates an upward watch and an F-statistic of 1.77 for downward watches. In both cases, the F-statistics exceed the F-statistics from all 1,000 random reassignments of analysts across firm-agency spells. In Column 3, we use as the dependent variable the tri-valued indicator that combines information on upward and downward watches. We find a similar result: the F-statistic of 1.76 has an implied *p*-value less than 0.001 since it exceeds all 1,000 F-statistics from the randomly reassigned placebo samples. In Panel B, we report the results from estimating model (2) using the watch indicators as dependent variables. We find similar results: analysts exert a significant influence on the likelihood that long-term ratings are placed on upward or downward watches. The resampling procedure confirms that the results are significant; in all cases the F-statistics on the true data exceed the F-statistics in all 1,000 placebo samples. Thus, analysts appear to exert a significant effect on the short term watches applied to firms, even comparing only analysts covering the same firm at the same time and allowing for agency-specific biases towards individual firms. This result is comforting given our prior result that analysts significantly affect the ratings themselves.

As we did for ratings, we also re-estimate the results on restricted samples in which we require that each analyst cover at least five or at least 10 firms. We find that the results are robust. The analyst effects are significant whether we include an agency fixed effect or an agency-firm interaction together with the firm-quarter fixed effects. We do see some evidence, particularly in the latter case, that analysts are more influential for the decision to place firms' long-term ratings on a watch for a downgrade. This result is interesting in light of the evidence in Table I, Panel C that the market reacts more strongly to ratings downgrades than to upgrades.

We conduct a similar exercise to examine the long-term ratings outlooks provided by the agencies. Outlooks are intended to provide information about the direction a rating is likely to take over a one to two year period. As such, the vast majority of outlooks are "stable," meaning no movement in either direction is anticipated. A positive or negative outlook does not imply a rating change is imminent or inevitable. We construct three dependent variables that capture the long-term outlook of each sample firm at the end of each fiscal quarter. First, we construct a dependent variable that takes the value -1 if firm *j* has a negative outlook from agency *i* at the end of firm-quarter *t*, 1 if the firm has a positive outlook, and 0 otherwise. Second, we consider separately positive and negative outlooks, defining an indicator that takes the value 1 if firm *j* has a negative outlook from agency *i* at the end of firm-quarter *t* and zero otherwise and a separate indicator that takes the value 1 if firm *j* has a positive outlook from agency *i* at the end of firm-quarter *t* and zero otherwise. We then estimate models (1) and (2) using the three outlook variables as dependent variables in place of $Rating_{ijt}$. Though we find F-statistics that are significant using conventional tests (e.g., the full sample F-statistics from model (1) for positive and negative outlooks are 3.67 and 3.37 respectively), we conclude that there are no significant effects based on our resampling procedure. Thus, analysts appear to exercise discretion in setting ratings and in making short-term projections about movements in those ratings, but they do not appear to influence long-term ratings outlooks. A possible explanation is that there is less variation across agencies in long-term outlooks for a single firm at a given point in time relative to short-term watches and ratings themselves.

II.D. Analyst Effects on Credit Spreads

Having established the effect of analysts on credit ratings, we revisit the link between ratings and credit spreads. In Section II.B, we calculated the implied impact of estimated analyst fixed effects on credit spreads assuming that the market does not distinguish the component of ratings driven by analyst effects from the portion driven by fundamentals or other factors. Here, we measure directly the impact of analyst effects on credit spreads. For each sample quarter, we construct a backward-looking estimate of the fixed analyst effect on ratings for each analyst covering a sample firm using equation (1). We take this approach instead of using the full sample estimates from Section II.B to ensure that we measure the reaction only to information that was available to market participants at the time we measure credit spreads. We sum the estimated fixed effects for all analysts covering each firm during the quarter for each agency and then average across agencies. We then regress the credit spread on the firm's credit rating and the aggregated fixed effect of analysts covering the firm during the quarter. We include additional controls for the value-weighted averages of the duration, callability, and age of the firm's outstanding bond issues. 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 quarterly fixed effects to adjust for market-wide trends in bond yields. We cluster standard errors by firm.

We present the results for the full sample of firm quarters for which credit spreads are available in Column 1 of Table IV. 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. Generally, a one notch improvement in the firm's credit rating is associated with a 49 basis point decrease in credit spreads. The market appears to react significantly less to an improvement that derives from a fixed analyst effect: a one notch improvement in rating would lead to a 10.5 basis point smaller decrease in spreads. However, we still observe a substantial and highly significant response to the portion of ratings driven entirely by analyst identity, equal to roughly 80% of the general effect of ratings on spreads. Recall that our estimates of analyst effects are orthogonal to firm fundamentals, since equation (1) contains firm-quarter fixed effects.

In Columns 2 through 6 of Table IV, we present the results of estimating the credit spread regression separately on quintiles of firms partitioned using credit ratings. In all five quintiles, we estimate a significant effect of credit ratings on spreads (more so as ratings decline). Though we have less power on the smaller subsamples, we generally observe negative coefficients on the aggregated analyst effects, suggesting that the market partially adjusts for ratings information driven by analysts and not fundamentals. However, the adjustment never exceeds 21% of the general effect of ratings on spreads. In quintile 3, which includes the investment grade cutoff, the 0.63 notch increment to credit ratings we considered in Section II.B would imply a 37 basis point change in credit spreads, after accounting for the market's discount on analyst-driven information.

We perform a number of robustness checks on this evidence. First, we re-estimate the regressions including firm fixed effects. When we estimate the extent to which the market adjusts for analyst fixed effects using only within-firm variation, we obtain small and insignificant estimates, suggesting that the pooled sample provides a conservative estimate of the degree to which analyst effects on ratings feed through to spreads. We also re-estimate the regressions in Table IV using only the subsamples of analysts who cover at least 5 and at least 10 firms. For such analysts, the market can obtain a more precise estimate of the fixed effect. Consistent with this hypothesis, we estimate somewhat stronger adjustment in these subsamples, though not substantially so. We estimate the largest adjustment in the 5-firm sample, finding a coefficient of roughly -17 basis points on the analyst effect in the full sample and -12 basis points in the third quintile (the estimated direct effect of ratings on spreads is virtually identical to Table IV). Finally, we re-estimate the regressions in Table IV, but progressively dropping early sample years to ensure that years in which the fixed effects are measured less precisely (due to smaller backward-looking estimation samples) do not dampen our estimates. The largest estimated adjustment occurs when we drop the first four sample years and still amounts to only 29% of the estimated impact of ratings on credit spreads.

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. Thus, the identity of the analysts covering the firm is likely to affect the cost of raising new debt capital. Evidence that companies target debt ratings (see, e.g., Hovakimian, Kayhan, and Titman, 2009, and Kisgen, 2009) is consistent with this view.

III. Which Analyst Traits Matter?

Having established that long-term issuer ratings (and spreads) differ depending on the identity of the analyst covering the firm, we turn our attention to the mechanism(s) through which this influence occurs. We supplement our data with information on analysts' backgrounds from LinkedIn (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 $\boldsymbol{\gamma}_{analyst}$, 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 will 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 potential 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.[19] Since worse ratings are associated with higher numbers on our numerical scale (see footnote 7), 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 measure 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 construct a measure of relative forecast accuracy over 1-, 2-, and 3-year horizons. The measure is the product of analyst relative optimism and the change in forward credit spreads over the horizon in question, negated so that a 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 V. We find relatively little evidence that agency tenure covering the firm affects ratings quality, after accounting for

[19] 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.

analyst effects. The exception is a relatively small, but significant decline in rating accuracy over a three-year horizon (Column 5). However, we find evidence of two economic mechanisms through which analysts appear to influence the level and quality of 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. We also find in Column 2 that their ratings deviate more on average in either direction from other analysts contemporaneously covering the firm than their peers without MBAs. When we look at the relative accuracy of their ratings in Columns 3 through 5, we find evidence that their ratings prove more accurate over time. Over a 1-year horizon, we do not see any significant difference between the accuracy of their ratings and the ratings of their peers. However, over a 2- and 3-year horizon, we find that their ratings are significantly more accurate, at the 5% and 1% levels, respectively. At a 2-year horizon, an MBA is associated with an increase of roughly 16% of a standard deviation in accuracy. At a 3-year horizon, the increase 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, particularly over longer horizons for which forecasting is likely to require greater skill. We find similar (though weaker) evidence looking at covariates that capture analyst experience. We find that analysts with longer tenure covering the industry provide ratings that are relatively more accurate over the 2- and 3-year horizons. An analyst with between 2.5 and 4 years covering the industry would have the same heightened accuracy as an analyst with an MBA. We also see that longer tenure in the rating agency and a higher number of covered firms are associated with lower rating optimism, though the effects are economically weaker and do not appear to be associated with gains in accuracy.

We also see evidence consistent with a second distinct mechanism. We find that as analyst tenure covering a firm increases, relative optimism about the firm increases. 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, long-term rating accuracy appears to decline with tenure covering the firm. We find a decline in accuracy over a 2-year horizon, but the effect is marginally insignificant. However, at a 3-year horizon, ratings become a worse predictor of movements in credit spreads, significant at the 1% level. After 4 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, 2012) 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, increased information from the rated firm over time may lead to an "illusion of knowledge" bias (Oskamp, 1965), leading to a decline in rating quality, even for analysts without any conscious 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. 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. 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 re-estimate our regression separately on each subsample. We report the results in Table VI. 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. In Panel A, the dependent variable is analyst relative optimism about the firm. We find that the effect of an MBA on analyst relative optimism is significantly more pronounced in firms with high dispersion in analyst earnings forecasts. We see a similar pattern comparing the estimated effects of an MBA on optimism across small and large firms (the effect is larger in magnitude among small firms), though the difference is not statistically significant. In Panel B, the dependent variable is rating accuracy over a three-year horizon. 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. Our results suggest 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. Skilled analysts appear to issue higher quality 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 likely to be most pronounced precisely in the set of firms likely to face the toughest 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.

IV. Analyst Traits and the Market Reaction to Rating Changes

As a final step, we investigate whether the equity market recognizes the differences in the informativeness of ratings across analysts and reacts differently to ratings announcements depending on the analysts who are responsible for the change. Though credit analysts provide direct information on the quality of firms' debt, this information should also affect the value of the residual claim held by shareholders. We use the announcement-level data summarized in Panel C of Table I. To begin, we estimate the following regression model:

$$CAR_j = \boldsymbol{\alpha}_{agency} + \boldsymbol{X_j}'\boldsymbol{\beta} + \boldsymbol{\delta}_{quarter} + \boldsymbol{\rho}_{ratingpair} + \boldsymbol{\gamma}_{analyst} + \epsilon_j \qquad (3)$$

CAR_j is the cumulative abnormal return around the ratings announcement, measured using the CRSP value-weighted index as a proxy for expected returns. *j* indexes the rating event. We consider two event windows, [-3, +3] and [-1, +1], where day 0 is the date of the report announcing the rating action. $\boldsymbol{\alpha}_{agency}$ are fixed effects for rating agencies, $\boldsymbol{\delta}_{quarter}$ are fixed effects for fiscal quarters, and $\boldsymbol{\rho}_{ratingpair}$ are fixed effects for ratings pairs (i.e., the ratings level before and after the announcement). The latter, for example, allow for differences in the average

market reactions to a change from a B rating to BBB than for a change from a B rating to BB or from an A rating to AAA. $\boldsymbol{X_j}$ is a vector of control variables; in our main specification we include the time since the last rating action and the time the agency has covered the firm as additional controls. Again, the quantities of interest are the estimates of $\boldsymbol{\gamma}_{analyst}$, the fixed effects for the analysts responsible for the ratings reports. Our null hypothesis is that the analyst effects are jointly equal to 0.

In Table VII, we report the results of estimating equation (3) using the cumulative abnormal return over a three day window [-1, +1]. We consider separately the subsamples of ratings upgrades, downgrades, and affirmations since these categories of events have different directional predictions for stock prices. We do not find a statistically significant effect of analysts on the market reactions to upgrades or affirmations. However, we do find that the market reacts differently to ratings downgrades depending on the analyst(s) responsible for the report. The F-statistic for a test of the hypothesis that the analyst effects on CARs around downgrades jointly equal zero is 1.30, which, using a conventional F-test, is significant at the 1% level. In Section II, we describe an alternative resampling procedure to assess the significance of analyst effects in a similar model. In that setting, conventional tests are potentially unreliable due in part to the strong persistence in the dependent variable (quarterly long-term issuer ratings). Here, persistence is less of a concern. The data is not a true panel, in that firms only appear in the data at the time of a ratings announcement and those announcements occur at different irregular rates across firms. Moreover, there are theoretical reasons not to expect strong persistence in abnormal returns. Nevertheless, we repeat our resampling test to provide a more conservative assessment of statistical significance. We find that the F-statistic on the true sample is greater than the F-statistics from all 1,000 placebo samples, confirming statistical significance. Recall from Table I that the market reacts more strongly to market downgrades than upgrades. Thus, our results are consistent with the market viewing downgrades as more informative than upgrades (or affirmations) and also recognizing variation in the informativeness of the signal depending on the analyst responsible for producing it.

It is likely that the market learns more about the tendencies of analysts who cover a larger set of firms. To test this conjecture, we again restrict our sample to analysts who cover at least 5 and 10 firms, respectively, re-estimating equation (3) on each subsample. The effect of analysts on the market reaction to downgrade announcements does not strengthen with the number of firms the analyst covers. On the 5-firm subsample, we find a smaller F-statistic than in the full sample that exceeds the F-statistics from resampled data in 974 out of 1,000 trials. On the 10-firm subsample, the estimated F-statistic of 1.24 exceeds the F-statistics from 988 of 1,000 placebo runs. Nevertheless, we confirm the robustness of our result across samples.

A caveat to our results is that we are not able to independently identify analyst and firm fixed effects in this context.[20] Thus, the evidence is weaker than the evidence in Section II. However, to provide some guidance on whether the estimated analyst effects are likely to simply reflect uncontrolled firm effects, we re-estimate equation (3) including firm fixed effects in place of the analyst effects. We cannot reject the null hypothesis that the firm fixed effects jointly equal zero in this specification. Thus the evidence points to analysts and not firms as significant predictors of the market's reaction to rating downgrades.

Mirroring our analysis of ratings levels, we next turn to the effect of specific analyst traits on the market reaction to ratings announcements. We re-estimate equation (3), but substituting the individual traits from Section III in the place of $\boldsymbol{\gamma}_{analyst}$. We also add firm fixed effects to the specification, so that we compare the reaction of the market to different rating events of the same firm, depending on the characteristics of the analysts following the firm at that time.[21] Also, as in Section III, we average analyst traits within the agency for each event before running the regression. We present the results in Table VIII. We report coefficient estimates in basis

[20] Instead of explaining ratings levels over time, which are a function of upgrades, downgrades, and affirmations, we must consider each type of change separately in this case because they have different implications for prices. This in turn drastically reduces the amount of within-firm variation in the regressions.

[21] We do not include firm effects when we estimate analyst fixed effects in equation (3). Here, having directional predictions for each individual trait (rather than estimating the net effect of all of the analyst's traits) allows us to achieve identification. Moreover, including a small number of traits as explanatory variables consumes far fewer degrees of freedom than attempting to estimate 609 analyst effects (in addition to more than 800 firm effects), given our sample of 2,871 observations in the downgrades specification.

points and cluster standard errors at the firm level. We find little evidence that the proxies for analyst skill or experience that we identified in Section III lead to meaningful differences in the market's reaction to analyst upgrades, downgrades, or affirmations over either a three day or seven day event window. In particular, MBAs do not appear to be associated with different market reactions to ratings announcements. We see some evidence that female analysts are associated with stronger reactions to ratings upgrades; however, these estimates are not as robust across specifications as other estimates in the paper. Most interestingly, we see evidence that the decline in stock prices around a rating downgrade is significantly larger as the analyst's tenure covering the firm increases. This result suggests that the market may recognize the upward bias in such analysts' ratings, as measured in Section III. That is, a rating downgrade from an analyst who is generally predisposed towards a favorable view presents a particularly strong negative signal about future cash flows. The effect is economically large (>1% for a 1 year increase in analyst tenure covering the firm) and is significant at the 1% level over both event windows.

Overall, we find mixed evidence on the question of whether the equity market responds to systematic differences across analysts in how they grant ratings. We do see some evidence of an analyst fixed effect. Moreover, the market seems to recognize that analysts with long tenure covering the same firm have an upward rating bias. However, we do not observe differences in ratings in response to other characteristics – like holding an MBA – that appear to affect both ratings levels and accuracy. This evidence is consistent with the partial adjustment of credit spreads to analyst effects that we measured in Section II and again suggests that differences in the analysts covering firms are likely to carry through to the firms' costs of capital.

V. Conclusion

We uncover evidence that significant variation in credit ratings can be explained by the identities 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. It also holds allowing for a firm-specific agency fixed effect, once we restrict attention to analysts who cover at least 5 firms. That is, our result cannot be explained by greater relative optimism or pessimism at particular agencies towards specific firms, but instead identifies changes in those sentiments over time as the analysts covering the firms change.

To conclude that the effects we identify are significant, we use a resampling procedure that compares the F-statistics in the true data to F-statistics computed on 1,000 placebo samples created by reshuffling analysts within the sample. Our approach preserves the same firm-analyst spells in the placebo samples that we observe in the true sample, changing only the identity of the analyst who serves each spell. Thus, the analyst effects have exactly the same persistence in the true data and the placebo samples, ensuring that we will not obtain spurious significance simply because both the dependent and explanatory variables are persistent. It also restricts each analyst to cover exactly the same number of firms in each placebo sample as in the true data. Thus, we obtain identification only from changing the particular groupings of spells that are served by each sample analyst. Comparing significance levels from this approach to traditional F-tests confirms that it provides a substantially tougher hurdle. Nevertheless, we conclude that analysts indeed have a significant effect on firms' credit ratings. We also find similar evidence of analyst effects on the likelihood firms' ratings are placed on short-term watches. Moreover, we find that these analyst effects, though orthogonal to firm fundamentals, carry through to credit spreads and, so, are likely to affect the cost of new debt capital.

In a second step, we consider individual analyst traits to shed light on the mechanisms by which analysts affect ratings. We find evidence of at least two distinct channels: 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 2- and 3-year horizons. Second, we find evidence that ratings quality deteriorates as analyst tenure covering the firm increases. Ratings become relatively more optimistic and less accurate over 3-year horizons. 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.

Finally, we ask whether the equity market recognizes these effects and reacts differently to ratings changes depending on the analysts who issue them. We find some evidence of an analyst fixed effect on the cumulative abnormal returns around ratings downgrades. When we consider individual analyst traits, we find no evidence of differences in reactions associated with our proxies for analyst expertise or experience. However, we do find evidence that the market reacts significantly more negatively when an analyst with long tenure covering a firm issues a downgrade, consistent with an understanding that these analysts have an upward bias on average. Our mixed results in this context suggest that the effects of analysts on ratings can carry through to the securities prices firms face in the market.

Our results have important policy implications. Credit ratings are significant determinants of credit spreads and, therefore, potential determinants of firms' overall costs of capital. 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, 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.[22] 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.[23] 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.

[22] 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.

[23] 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 (1-year, 2-year, 3-year)	The product of -1 times Optimism and the forward change in credit spreads over horizon *h* (where *h* is 1, 2, or 3 years), 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 from equation (1) for all analysts covering each firm during each quarter for each agency, averaged across agencies. 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 equation (1) 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 age of all outstanding bonds issued by the firm	Trace, Mergent FISD.
Bond Duration	Firm-level volume-weighted average of the duration of all outstanding bonds issued by the firm.	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.	Trace, Mergent FISD.
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 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.	Trace, Mergent FISD.
Cumulative Abnormal Return (CAR)	The cumulative abnormal returns relative to the value-weighted CRSP index for a window of [-1;+1] or [-3;+3] days centered around the announcement of a rating action (upgrade, downgrade, or affirmation).	Thomson/CRSP
Equity Analysts' Earning Forecast Dispersion	Standard deviation of the earning forecasts of equity analysts covering the firm six months prior to the annual earnings announcement, standardized by the mean earnings forecast	I/B/E/S
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
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
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 using the Fama French 49 industry classification code	Compustat Segments
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.	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
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
Time Since Last Bond Trading Date	Firm-level volume-weighted average of the number of days since the date the bond was traded last.	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
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 match across agencies.

Credit Rating	Letter Rating		
	Standard & Poor's	Moody's	Fitch
1	AAA	Aaa	AAA
2	AA+	Aa1	AA+
3	AA	Aa2	AA
4	AA-	Aa3	AA-
5	A+	A1	A+
6	A	A2	A
7	A-	A3	A-
8	BBB+	Baa1	BBB+
9	BBB	Baa2	BBB
10	BBB-	Baa3	BBB-
11	BB+	Ba1	BB+
12	BB	Ba2	BB
13	BB-	Ba3	BB-
14	B+	B1	B+
15	B	B2	B
16	B-	B3	B-
17	CCC+	Caa1	CCC+
18	CCC	Caa2	CCC
19	CCC-	Caa3	CCC-
20	CC, C	Ca	CC, C
21	D	C	D, DD, DDD

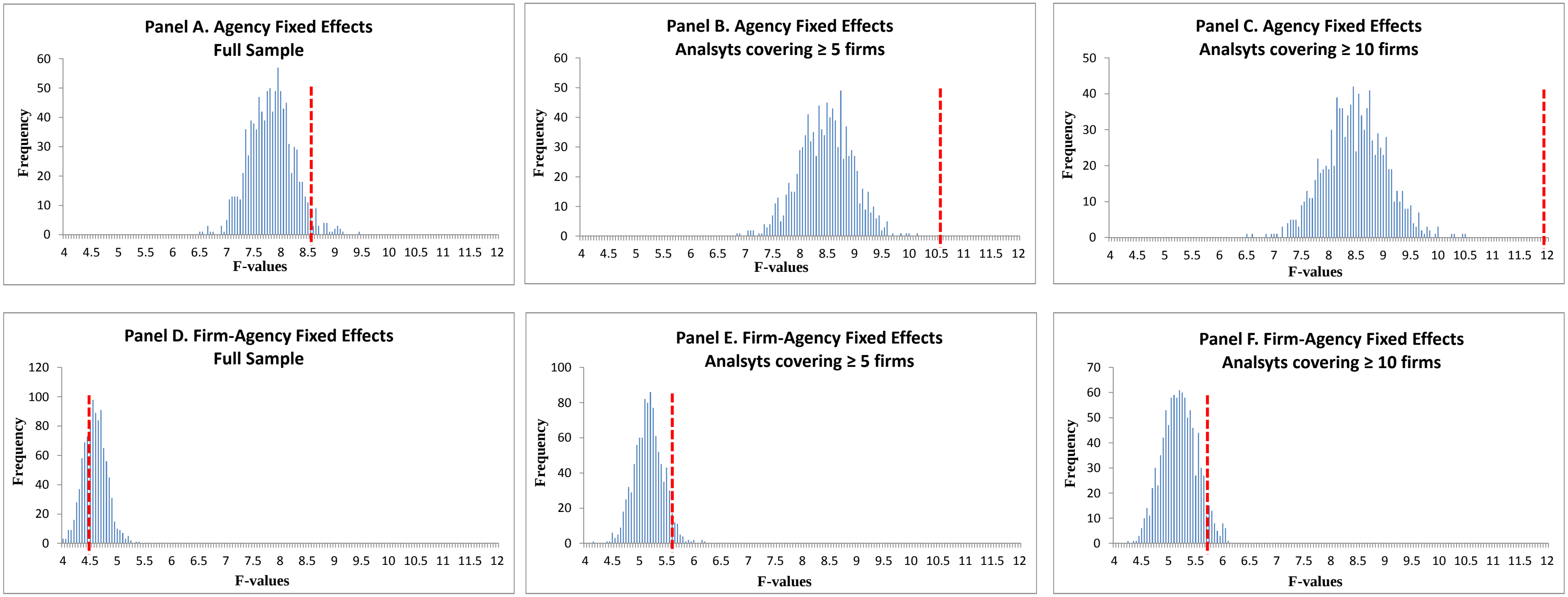
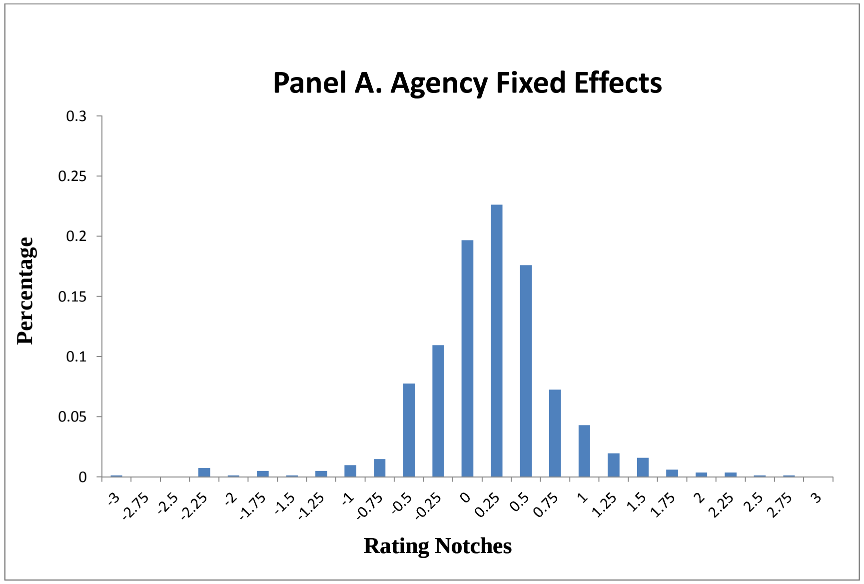


Figure 1. Histograms of placebo test results. This figure shows the histograms of F-statistics on 1,000 placebo runs where we substitute the analyst name with the name of an analyst drawn randomly for each analyst-firm pair. The F-statistic is for a test of the joint significance of analyst fixed effects in an OLS regression of long-term credit ratings on analyst fixed effects, firm-quarter fixed effects, and either agency fixed effects or firm-agency fixed effects. The top plots include agency fixed effects and the bottom plots include firm-agency fixed effects. The left plots include all analysts, the central plots restrict the sample only to analysts covering at least 5 firms, and the right plots restrict the sample only to analysts covering at least 10 firms. The vertical dashed lines represent the F-statistics for a test of the joint significance of analyst fixed effects in the same regression specification on the real data.



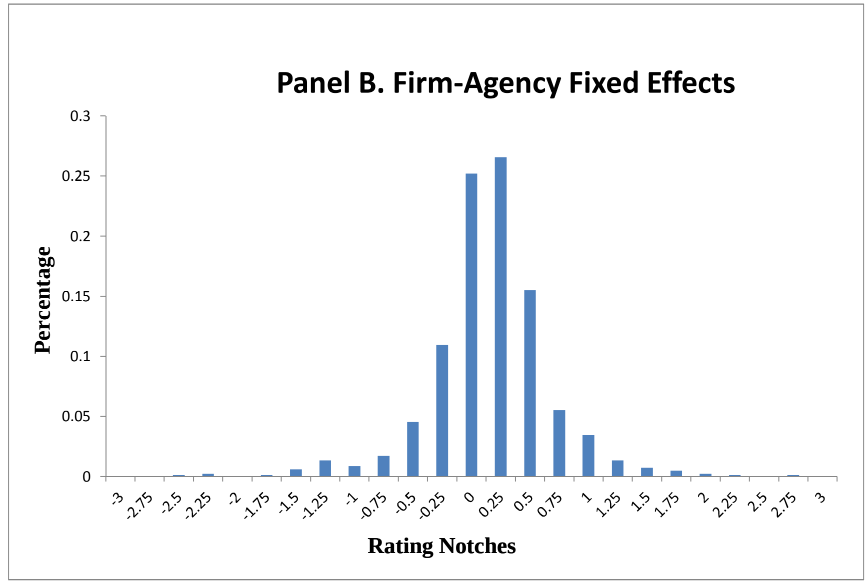


Figure 2. Histograms of analyst effects. This figure shows histograms of the estimated analyst effects from OLS regressions of long-term credit ratings on analyst fixed effects, firm-quarter fixed effects, and either agency fixed effects (Panel A) or firm-agency fixed effects (Panel B).

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. Panel B lists the analyst and firm characteristics and ratings for each firm-quarter. Panel C lists the analyst and firm characteristics for each rating announcement. Panel D shows the pairwise correlations of the analyst variables and ratings. All variables are defined in the Appendix.

Panel A: Quarterly Rating Panel

	Obs.	Mean	Median	Std. Dev.	10th Percentile	90th Percentile
Credit Rating	53,747	11.05	11	3.45	6	15
Negative Watch	60,296	0.05	0	0.21	0	0
Positive Watch	60,296	0.01	0	0.12	0	0
Positive Outlook	60,296	0.08	0	0.27	0	0
Negative Outlook	60,296	0.18	0	0.39	0	1
Stable Outlook	60,296	0.41	0	0.49	0	1
Credit Spread	15,499	324.85	255.36	239.07	81.87	693.38
Bond Duration	15,499	5.45	5.14	2.49	2.57	8.69
Callable Bond Dummy	15,499	0.83	1	0.35	0	1
Bond Age	15,499	1,378.16	1,142	1,075.27	280	2,757
Time Since Last Bond Trading Date	15,499	6.00	1	14.30	0	17

Panel B: Quarterly Rating Panel with Analyst Characteristics

	Obs.	Mean	Median	Std. Dev.	10th Percentile	90th Percentile
Firm Age	23,287	28.67	22.76	18.53	7.25	56.04
Total Assets	23,131	37,594.94	4,895.29	170,061.20	788.26	43,767
Number of Segments	18,644	1.60	1.00	0.89	1.00	3.00
Number of Equity Analysts	20,192	10.94	10.00	7.01	3.00	21.00
Equity Analysts' Earnings Forecast Dispersion	19,148	0.01	0.03	1.22	-0.11	0.17
Optimism	23,287	-0.04	0	0.96	-1	1
Rating Dispersion	23,287	0.66	0.50	0.70	0	1.50
1-Year Accuracy	10,534	0.01	0	175.35	-128.46	131.98
2-Year Accuracy	8,771	3.53	0	233.48	-184.59	194.07
3-Year Accuracy	6,822	3.12	0	235.73	-218.14	224.30
Credit Rating	23,287	11.33	12	3.54	6	15
MBA	23,287	0.73	1	0.42	0	1
Analyst Age	23,287	39.46	39	7.68	30	49
Female	23,287	0.26	0	0.37	0	1
Analyst Tenure Covering the Firm	23,287	2.07	1.75	1.72	0.25	4.38
Agency Tenure Covering the Firm	23,287	4.79	4.25	3.44	0.75	9.26
Analyst Tenure Covering the Industry	23,287	3.49	3.13	2.34	0.75	6.63
Analyst Tenure in the Agency	23,287	6.96	5.92	4.73	2.12	12.75
N. of Firms Currently Covered	23,287	13.34	11	9.68	4	27
Agency = Moody's	23,287	0.36	0	0.48	0	1
Agency = SP	23,287	0.40	0	0.49	0	1

Table I (Cont.)

Summary Statistics

Panel C. Credit Event Announcements

	Rating Upgrades			Rating Downgrades			Rating Affirmations		
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
CAR -1+1	1,179	66.23	551.12	1,858	-264.82	1,427.99	5,336	13.77	599.69
CAR -3+3	1,176	131.11	1,073.64	1,858	-381.19	1,914.79	5,335	26.13	843.85
Time Since Last Rating Action	1,179	222.50	230.13	1,858	159.95	208.54	5,336	229.27	257.23
Current Credit Rating	1,179	12.00	3.01	1,858	12.91	3.68	5,336	10.80	3.46
Previous Credit Rating	1,179	13.08	3.10	1,858	11.38	3.44	5,336	10.78	3.46
MBA	1,179	0.78	0.39	1,858	0.73	0.41	5,336	0.74	0.40
Analyst Age	1,179	39.15	8.19	1,858	39.56	7.52	5,336	38.84	7.72
Female	1,179	0.24	0.34	1,858	0.29	0.35	5,336	0.27	0.35
Analyst Tenure Cov. Firm	1,179	1.84	1.71	1,858	1.73	1.56	5,336	1.84	1.60
Agency Tenure Cov. Firm	1,179	5.66	3.31	1,858	4.98	3.43	5,336	5.00	3.26
Analyst Tenure Cov. Ind.	1,179	3.25	2.35	1,858	3.11	2.25	5,336	3.17	2.18
Analyst Tenure in the Agency	1,179	6.56	4.71	1,858	6.89	4.88	5,336	6.47	4.42
N. of Firms Currently Covered	1,179	14.01	10.52	1,858	13.55	9.44	5,336	13.03	9.47
Agency = Moody's	1,179	0.41	0.49	1,858	0.40	0.49	5,336	0.31	0.46
Agency = SP	1,179	0.44	0.50	1,858	0.42	0.49	5,336	0.40	0.49

Panel D. Pairwise Correlations

	Optimism	Rating Dis-persion	1-Year Accuracy	2-Year Accuracy	3-Year 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	-0.007	1.000												
1-Year Accuracy	-0.175	-0.023	1.000											
2-Year Accuracy	-0.247	0.007	0.631	1.000										
3-Year Accuracy	-0.362	0.013	0.379	0.687	1.000									
Credit Rating	-0.204	0.097	0.035	0.058	0.075	1.000								
MBA	-0.016	-0.022	-0.019	-0.010	0.019	0.035	1.000							
Analyst Age	-0.040	0.052	0.024	0.024	0.026	-0.122	-0.083	1.000						
Female	-0.062	0.033	0.005	0.024	0.044	-0.064	-0.235	0.055	1.000					
Analyst Tenure Cov. Firm	0.056	-0.004	-0.002	-0.010	-0.029	-0.116	-0.001	0.290	-0.027	1.000				
Agency Tenure Cov. Firm	0.058	0.015	-0.017	-0.036	-0.058	-0.087	-0.028	0.132	-0.030	0.307	1.000			
Analyst Tenure Cov. Ind.	0.036	0.025	0.008	0.018	0.018	-0.172	-0.104	0.381	0.018	0.690	0.316	1.000		
Analyst Tenure in Agency	-0.013	0.063	-0.004	0.018	0.028	-0.164	-0.201	0.550	0.159	0.371	0.155	0.536	1.000	
N. of Firms Currently Covered	-0.147	0.028	0.010	0.047	0.061	0.237	-0.030	0.235	-0.099	0.054	0.036	0.084	0.094	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 (Panel A) or firm-agency fixed effects (Panel B). The credit rating is a numeric variable ranging from 1 (AAA) to 21 (Default). Column (1) shows the results for the full sample of analysts. Columns (2) and (3) show the results only for a subset of analysts covering at least 5 and 10 firms, respectively. 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.

Panel A. Firm-Quarter and Agency Fixed Effects

	Full Sample	Analysts Covering ≥ 5 firms	Analysts Covering ≥ 10 firms
	(1)	(2)	(3)
F-Value	8.45 ***	10.59 ***	11.91 ***
Placebo Test	5.2%	<0.1%	<0.1%
N. Observations	53,747	53,184	51,616
N. Analysts	813	572	405

Panel B. Firm-Quarter and Firm-Agency Fixed Effects

	Full Sample	Analysts Covering ≥ 5 firms	Analysts Covering ≥ 10 firms
F-Value	4.45 ***	5.54 ***	5.67 ***
Placebo Test	69.5%	6.3%	6.1%
N. Observations	53,747	53,184	51,616
N. Analysts	813	572	405

Table III
Wald Test and Placebo Simulation: Credit Watches

The table reports the F-statistics to test the joint significance of the analyst fixed effects in an OLS regression of indicators for short-term watches on analyst fixed effects, firm-quarter fixed effects, and either agency fixed effects (Panel A) or firm-agency fixed effects (Panel B). The dependent variable in Columns (1), (4), and (7) is an indicator equal to 1 if the agency placed the firm on a watch for a rating increase during the quarter, and zero otherwise. The dependent variable in Columns (2), (5), and (8) is an indicator equal to 1 if the agency placed the firm on a watch for a rating decrease during the quarter, and zero otherwise. The dependent variable in Columns (3), (6), and (9) equals 1 if the credit rating agency assigned a positive watch during the quarter, -1 for a negative watch, and zero otherwise. Columns (1) to (3) present the results for the full sample of analysts. Columns (4) to (6) present the results only for a subset of analysts covering at least 5 firms. Columns (7) to (9) present the results only for a subset of analysts covering at least 10 firms. The table also reports in the row Placebo Test the percentage of 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.

Panel A. Firm-Quarter and Agency Fixed Effects

	Full Sample			Analysts Covering ≥ 5 firms			Analysts Covering ≥ 10 firms		
	Positive Watch	Negative Watch	Signed Watch	Positive Watch	Negative Watch	Signed Watch	Positive Watch	Negative Watch	Signed Watch
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
F-Value	1.76 ***	1.77 ***	1.76 ***	1.45 ***	1.72 ***	1.54 ***	1.31 ***	1.78 ***	1.55 ***
Placebo Test	<0.1%	<0.1%	<0.1%	<0.1%	<0.1%	<0.1%	0.3%	<0.1%	<0.1%
N. Observations	60,296	60,296	60,296	59,236	59,236	59,236	54,986	54,986	54,986
N. Analysts	852	852	852	577	577	577	405	405	405

Panel B. Firm-Quarter and Firm-Agency Fixed Effects

	Full Sample			Analysts Covering ≥ 5 firms			Analysts Covering ≥ 10 firms		
	Positive Watch	Negative Watch	Signed Watch	Positive Watch	Negative Watch	Signed Watch	Positive Watch	Negative Watch	Signed Watch
F-Value	1.69 ***	1.71 ***	1.71 ***	1.44 ***	1.65 ***	1.53 ***	1.23 ***	1.84 ***	1.63 ***
Placebo Test	<0.1%	<0.1%	<0.1%	2.7%	<0.1%	0.1%	49.6%	<0.1%	<0.1%
N. Observations	60,296	60,296	60,296	59,236	59,236	59,236	54,986	54,986	54,986
N. Analysts	852	852	852	577	577	577	405	405	405

Table IV
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. Column (1) includes all observations. Columns (2) to (6) include only observations in which the long-term rating is in the respective quintile. Robust t-statistics clustered at the firm level are reported in parentheses below the coefficients. Constant included. Significance at the 1%, 5%, and 10% levels is denoted by ∗∗∗, ∗∗, and ∗, respectively.

	Full Sample	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
	(1)	(2)	(3)	(4)	(5)	(6)
Credit Rating	48.924 ***	18.114 ***	34.514 ***	69.108 ***	59.370 ***	63.610 ***
	(50.62)	(8.73)	(5.04)	(14.50)	(7.30)	(10.53)
Aggregate Analyst Effects	-10.545 ***	-1.919	-3.665	-10.965	-12.216	3.883
	(-2.66)	(-0.56)	(-0.60)	(-1.46)	(-1.13)	(0.25)
Bond Duration	-2.443 **	3.140 ***	1.748	-1.840	-10.751 ***	-9.106 **
	(-2.51)	(3.13)	(1.50)	(-0.86)	(-3.53)	(-2.20)
Callable Bond Dummy	-39.548 ***	-30.770 ***	-37.655 ***	-12.557	-28.277	82.737 ***
	(-4.32)	(-3.59)	(-2.73)	(-1.02)	(-0.87)	(3.10)
Bond Age	0.006 **	0.012 ***	0.006	0.009 **	-0.005	0.013 *
	(2.55)	(4.37)	(1.43)	(2.39)	(-0.98)	(1.77)
Time Since Last Bond Trading Date	0.808 ***	0.571 *	0.409 **	0.562 ***	1.201 ***	1.652 ***
	(6.74)	(1.70)	(2.20)	(3.00)	(4.12)	(6.14)
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.737	0.591	0.606	0.696	0.659	0.532
Observations	15,499	3,591	3,663	3,670	2,541	2,034

Table V
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 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 horizon h (where h is 1, 2, or 3 years), measured starting at the end of the quarter. All variables are defined in the Appendix. All specifications include firm-quarter fixed effects and agency fixed effects. 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 (1)	Rating Dispersion (2)	1-yr Accuracy (3)	2-yr Accuracy (4)	3-yr Accuracy (5)
MBA	-0.218 ***	0.081 ***	-0.428	37.450 **	68.018 ***
	(-4.74)	(6.94)	(-0.04)	(2.12)	(3.41)
Analyst Age	-0.003	0.001	1.134	-0.313	-1.155
	(-0.91)	(0.86)	(1.37)	(-0.27)	(-0.81)
Female	-0.353 ***	-0.007	-2.940	7.839	57.540 **
	(-6.69)	(-0.61)	(-0.19)	(0.38)	(2.29)
Analyst Tenure Covering the Firm	0.088 ***	0.006	0.289	-8.174	-15.943 ***
	(6.31)	(1.41)	(0.08)	(-1.57)	(-2.81)
Agency Tenure Covering the Firm	0.009	-0.001	0.638	1.489	-4.744 **
	(1.54)	(-0.57)	(0.50)	(0.83)	(-2.37)
Analyst Tenure Covering the Industry	0.007	-0.008 **	2.336	14.544 ***	18.427 ***
	(0.51)	(-2.23)	(0.61)	(2.75)	(3.05)
Analyst Tenure in the Agency	-0.029 ***	0.004 ***	-0.229	2.311	2.538
	(-6.38)	(3.26)	(-0.18)	(1.30)	(1.32)
N. of Firms Currently Covered	-0.007 ***	-0.002 ***	-1.137 *	-0.009	2.895 ***
	(-2.63)	(-2.67)	(-1.72)	(-0.01)	(2.71)
Agency = Moody's	-0.162 ***	0.055 ***	-8.897	-25.766 *	-13.499
	(-3.74)	(3.99)	(-0.81)	(-1.67)	(-0.76)
Agency = SP	0.168 ***	0.026 **	-9.109	-36.345 **	-12.190
	(4.32)	(2.28)	(-0.89)	(-2.52)	(-0.72)
Firm-Quarter FE	Yes	Yes	Yes	Yes	Yes
R^2	0.062	0.016	0.003	0.010	0.024
Observations	23,287	23,287	10,534	8,771	6,822

Table VI
Optimism and Accuracy: Split Samples

The table reports coefficient estimates from OLS regressions splitting the sample at the median value for each splitting variable reported at the top of the column. In Panel A, the dependent variable is 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. In Panel B, the dependent variable is 3-Year 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 V. Robust t-statistics clustered at the firm-quarter level are reported in parentheses below the coefficients. For each split sample, we also report the 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.

Panel A. Optimism

	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	-0.299 ***	-0.175 ***	-0.205 ***	-0.206 ***	-0.278 ***	-0.330 ***	-0.189 ***	-0.182 ***	-0.097	-0.263 ***
	(-4.22)	(-2.90)	(-3.11)	(-3.14)	(-4.69)	(-3.52)	(-2.64)	(-2.70)	(-1.32)	(-4.11)
	0.183		0.991		0.644		0.949		0.088	
Analyst Tenure Covering Firm	0.084 ***	0.095 ***	0.114 ***	0.072 ***	0.103 ***	0.163 ***	0.059 **	0.084 ***	0.080 ***	0.073 ***
	(3.38)	(5.67)	(5.33)	(3.82)	(5.21)	(6.67)	(2.38)	(4.75)	(3.90)	(3.30)
	0.719		0.147		0.057		0.399		0.825	
Firm-Quarter FE, Agency FE, and Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.073	0.068	0.082	0.064	0.104	0.102	0.079	0.084	0.082	0.080
Observations	11,566	11,565	11,684	11,603	11,074	7,570	10,752	9,440	9,573	9,575

Panel B. 3-Year Accuracy

	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	126.629 ***	25.571	119.791 ***	20.695	-10.534	88.054 **	107.866 ***	75.038 **	11.528	154.685 ***
	(3.35)	(1.32)	(3.29)	(0.88)	(-0.34)	(2.36)	(3.26)	(2.51)	(0.40)	(4.75)
	0.017		0.022		0.042		0.462		0.001	
Analyst Tenure Covering Firm	-25.999 **	1.737	-29.185 ***	-7.020	-11.034	-25.651 ***	-22.447 *	-6.547	-3.343	-32.879 ***
	(-2.18)	(0.32)	(-2.92)	(-1.14)	(-1.07)	(-2.66)	(-1.95)	(-1.10)	(-0.51)	(-2.91)
	0.034		0.059		0.302		0.220		0.024	
Firm-Quarter FE, Agency FE, and Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.062	0.029	0.047	0.041	0.034	0.069	0.048	0.044	0.028	0.070
Observations	3,404	3,404	3,414	3,408	2,717	2,638	3,249	3,191	3,119	3,121

Table VII
Wald Test and Placebo Simulation: Cumulative Abnormal Returns

The table reports the F-statistics to test the joint significance of the analyst fixed effects in an OLS regression of the cumulative abnormal returns for a [-1;+1] window around the announcements of a rating upgrade (Column 1), downgrade (Column 2) or affirmation (Column 3) on analyst fixed effects, fiscal quarter fixed effects, fixed effects for all current-past rating pairs, agency fixed effects, and a control for the time since the last rating action. Panel A shows the results for the full sample of analysts. Panel B shows the results only for a subset of analysts covering at least 5 firms. Panel 10 shows the results only for a subset of analysts covering at least 10 firms. The table also reports the percentage of 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 F-test at the 1%, 5%, and 10% levels is denoted by ∗∗∗, ∗∗, and ∗, respectively.

Panel A. Full Sample

	Rating Upgrades	Rating Downgrades	Rating Affirmations
	(1)	(2)	(3)
F-Value	0.92	1.30 ***	0.98
Placebo Test	76.6%	<0.1%	86.9%
N. Observations	1,850	2,871	8,304
N. Analysts	525	609	745

Panel B. Analysts Covering ≥ 5 firms

	Rating Upgrades	Rating Downgrades	Rating Affirmations
	(1)	(2)	(3)
F-Value	0.94	1.13 **	0.93
Placebo Test	79.1%	2.6%	95.2%
N. Observations	1,785	2,794	8,078
N. Analysts	460	503	544

Panel C. Analysts Covering ≥ 10 firms

	Rating Upgrades	Rating Downgrades	Rating Affirmations
	(1)	(2)	(3)
F-Value	0.96	1.24 ***	1.02
Placebo Test	70.3%	1.2%	70.1%
N. Observations	1,618	2,554	7,210
N. Analysts	337	363	371

Table VIII
Cumulative Abnormal Returns

The table reports coefficient estimates from OLS regressions of the cumulative abnormal returns for the [-1;+1] and [-3;+3] windows around the announcements of a rating upgrade (Columns 1-2), downgrade (Columns 3-4), or affirmation (Columns 5-6) on analyst characteristics, firm and fiscal quarter fixed effects, fixed effects for all current-past rating pairs, and a control for the time since the last rating action. All variables are defined in the Appendix. Robust t-statistics clustered at the firm level are reported in parentheses below the coefficients. Constant included. Significance at the 1%, 5%, and 10% levels is denoted by ∗∗∗, ∗∗, and ∗, respectively.

	Rating Upgrades		Rating Downgrades		Rating Affirmations	
	CAR [-1;+1]	CAR [-3;+3]	CAR [-1;+1]	CAR [-3;+3]	CAR [-1;+1]	CAR [-3;+3]
	(1)	(2)	(3)	(4)	(5)	(6)
Time Since Last Rating Action	0.105	0.193	-0.167	-0.066	-0.071	-0.148 **
	(0.94)	(0.81)	(-0.81)	(-0.24)	(-1.61)	(-2.37)
MBA	-82.883	-199.391	81.482	163.576	11.764	6.652
	(-1.18)	(-1.28)	(0.70)	(1.04)	(0.37)	(0.15)
Age	1.251	2.415	-9.504	-22.727 **	1.920	-0.078
	(0.35)	(0.26)	(-1.29)	(-2.01)	(0.92)	(-0.02)
Female	91.515	339.372 **	66.921	86.603	18.000	20.103
	(1.19)	(2.07)	(0.47)	(0.45)	(0.45)	(0.35)
Analyst Tenure Cov. Firm	-13.123	3.823	-128.227 ***	-146.304 ***	6.065	0.245
	(-0.69)	(0.11)	(-3.06)	(-2.79)	(0.70)	(0.02)
Agency Tenure Cov. Firm	-14.032	-17.659	25.144	16.760	-7.494	-2.352
	(-0.94)	(-0.48)	(0.99)	(0.54)	(-1.44)	(-0.32)
Analyst Tenure in Agency	-0.892	-10.913	-9.157	16.645	-2.993	-2.291
	(-0.13)	(-0.73)	(-0.74)	(0.87)	(-0.77)	(-0.41)
Analyst Tenure Cov. Ind.	3.793	9.987	54.861	63.454	-3.561	7.522
	(0.21)	(0.28)	(1.46)	(1.30)	(-0.40)	(0.58)
N. of Firms Currently Covered	5.022	8.162	0.708	-7.454	-0.770	-2.296
	(1.29)	(1.09)	(0.08)	(-0.68)	(-0.45)	(-0.94)
Agency = Moody's	14.054	113.909	60.631	276.044	-1.485	-17.029
	(0.16)	(0.68)	(0.33)	(1.18)	(-0.04)	(-0.28)
Agency = SP	132.013	149.716	-147.739	-100.018	-1.957	-29.644
	(1.38)	(0.80)	(-1.05)	(-0.65)	(-0.06)	(-0.67)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Current and Past Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.417	0.379	0.285	0.246	0.069	0.090
Observations	1,179	1,176	1,858	1,858	5,336	5,335

Table OA-I
Credit Spread and Credit Ratings

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. Column (1) includes all observations. Columns (2) to (6) include only observations where the dependent variable is in the respective quintile. Robust t-statistics clustered at the firm level are reported in parentheses below the coefficients. Constant included. Significance at the 1%, 5%, and 10% levels is denoted by ***, **, and *, respectively.

	Full Sample (1)	Quintile 1 (2)	Quintile 2 (3)	Quintile 3 (4)	Quintile 4 (5)	Quintile 5 (6)
Credit Rating	49.886 ***	18.677 ***	30.703 ***	74.215 ***	62.153 ***	60.861 ***
	(56.70)	(8.74)	(4.92)	(16.59)	(8.48)	(10.97)
Bond Duration	-1.581 *	3.585 ***	1.850 *	-0.530	-10.705 ***	-11.515 ***
	(-1.80)	(4.05)	(1.83)	(-0.27)	(-3.74)	(-2.87)
Callable Bond Dummy	-41.051 ***	-24.127 ***	-39.149 ***	-20.029	-17.078	56.675 **
	(-4.75)	(-2.84)	(-3.13)	(-1.43)	(-0.58)	(2.12)
Bond Age	0.006 ***	0.013 ***	0.006 *	0.007 *	-0.005	0.007
	(2.76)	(5.16)	(1.82)	(1.91)	(-0.97)	(1.07)
Time Since Last Bond Trading Date	0.729 ***	0.445 *	0.326 **	0.564 ***	1.084 ***	1.668 ***
	(6.92)	(1.74)	(2.16)	(3.42)	(4.43)	(6.57)
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.735	0.551	0.587	0.677	0.658	0.540
Observations	20,567	4,993	5,120	4,677	3,187	2,590

Table OA-II
Optimism and Accuracy: Split Samples

The table reports coefficient estimates from OLS regressions splitting the sample at the median value for each splitting variable reported at the top of the column. In Panel A, the dependent variable is Optimism. In Panel B the dependent variable is the 3-Year Accuracy. All variables are defined in the Appendix. All specifications include firm-quarter fixed effects and agency fixed effects. 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.

Panel A. Optimism

	Total Assets		Firm Age		Number of Segments		Number of Equity Analysts		Equity Analysts Earning Forecast Dispersion	
	Low	High	Low	High	Low	High	Low	High	Low	High
MBA	-0.299 ***	-0.175 ***	-0.205 ***	-0.206 ***	-0.278 ***	-0.330 ***	-0.189 ***	-0.182 ***	-0.097	-0.263 ***
	(-4.22)	(-2.90)	(-3.11)	(-3.14)	(-4.69)	(-3.52)	(-2.64)	(-2.70)	(-1.32)	(-4.11)
Analyst Age	-0.009 **	0.008 **	-0.009 **	0.004	-0.012 ***	0.005	-0.004	-0.007 *	-0.002	-0.008 *
	(-2.23)	(1.98)	(-2.28)	(0.93)	(-2.83)	(0.99)	(-1.04)	(-1.84)	(-0.37)	(-1.96)
Female	-0.123	-0.488 ***	-0.106	-0.635 ***	-0.235 ***	-0.318 ***	-0.356 ***	-0.430 ***	-0.360 ***	-0.483 ***
	(-1.37)	(-7.31)	(-1.45)	(-8.33)	(-3.03)	(-3.70)	(-4.61)	(-5.63)	(-4.93)	(-5.83)
Analyst Tenure Covering Firm	0.084 ***	0.095 ***	0.114 ***	0.072 ***	0.103 ***	0.163 ***	0.059 **	0.084 ***	0.080 ***	0.073 ***
	(3.38)	(5.67)	(5.33)	(3.82)	(5.21)	(6.67)	(2.38)	(4.75)	(3.90)	(3.30)
Agency Tenure Covering Firm	0.006	0.010	0.012	0.009	-0.032 ***	0.024 ***	0.011	0.019 ***	0.021 ***	-0.001
	(0.55)	(1.42)	(1.25)	(1.25)	(-3.29)	(2.66)	(1.09)	(2.76)	(2.84)	(-0.10)
Analyst Tenure Cov. Industry	0.016	-0.005	0.016	-0.009	0.036 **	-0.068 ***	0.009	0.031 *	0.049 **	0.002
	(0.70)	(-0.33)	(0.84)	(-0.49)	(2.20)	(-2.81)	(0.39)	(1.88)	(2.13)	(0.10)
Analyst Tenure in the Agency	-0.028 ***	-0.030 ***	-0.032 ***	-0.023 ***	-0.030 ***	-0.019 **	0.004	-0.043 ***	-0.048 ***	-0.015 **
	(-3.56)	(-5.33)	(-4.86)	(-3.69)	(-4.51)	(-1.99)	(0.54)	(-7.29)	(-7.00)	(-2.31)
N. of Firms Currently Covered	0.000	-0.014 ***	-0.001	-0.012 ***	0.004	-0.004	-0.008 **	-0.003	0.001	-0.010 ***
	(0.02)	(-3.49)	(-0.28)	(-3.49)	(1.06)	(-1.05)	(-2.33)	(-0.84)	(0.33)	(-2.71)
Agency = Moody's	-0.496 ***	-0.117 **	-0.088	-0.195 ***	-0.417 ***	-0.239 ***	-0.155 **	-0.237 ***	-0.285 ***	-0.139 **
	(-5.65)	(-2.16)	(-1.45)	(-3.27)	(-6.72)	(-3.38)	(-2.12)	(-4.17)	(-4.44)	(-2.26)
Agency = SP	-0.120	0.219 ***	0.320 ***	0.035	0.093	0.277 ***	0.295 ***	0.037	0.071	0.216 ***
	(-1.42)	(4.55)	(5.76)	(0.64)	(1.54)	(4.23)	(4.25)	(0.74)	(1.24)	(3.63)
Firm-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.073	0.068	0.082	0.064	0.104	0.102	0.079	0.084	0.082	0.080
Observations	11,566	11,565	11,684	11,603	11,074	7,570	10,752	9,440	9,573	9,575

Table OA-II (Cont.)
Optimism and Accuracy: Split Samples

Panel B. 3-Year Accuracy

	Total Assets		Firm Age		Number of Segments		Number of Equity Analysts		Equity Analysts Earning Forecast Dispersion	
	Low	High	Low	High	Low	High	Low	High	Low	High
MBA	126.629 ***	25.571	119.791 ***	20.695	-10.534	88.054 **	107.866 ***	75.038 **	11.528	154.685 ***
	(3.35)	(1.32)	(3.29)	(0.88)	(-0.34)	(2.36)	(3.26)	(2.51)	(0.40)	(4.75)
Age	-2.503	1.017	-7.114 ***	5.556 ***	-5.037 **	2.093	1.663	-3.644 **	-2.267	1.562
	(-1.18)	(0.50)	(-3.29)	(3.04)	(-2.24)	(0.96)	(0.71)	(-2.23)	(-1.27)	(0.62)
Female	147.313 **	27.420	66.679	69.477 **	-78.858	91.133 **	64.751	59.595 **	61.867 **	83.642 **
	(2.51)	(1.13)	(1.64)	(2.45)	(-1.50)	(2.58)	(1.33)	(2.39)	(2.08)	(2.05)
Analyst Tenure Covering Firm	-25.999 **	1.737	-29.185 ***	-7.020	-11.034	-25.651 ***	-22.447 *	-6.547	-3.343	-32.879 ***
	(-2.18)	(0.32)	(-2.92)	(-1.14)	(-1.07)	(-2.66)	(-1.95)	(-1.10)	(-0.51)	(-2.91)
Agency Tenure Covering Firm	-6.128	-4.823 **	-9.946 **	-0.239	-3.702	-6.671 **	-6.714 *	0.395	0.489	-21.503 ***
	(-1.24)	(-2.53)	(-2.17)	(-0.12)	(-1.01)	(-2.17)	(-1.69)	(0.16)	(0.22)	(-4.16)
Analyst Tenure Cov. Industry	37.784 ***	-8.296	36.921 ***	4.159	18.171 *	33.850 ***	35.195 ***	-6.813	9.751	24.758 **
	(3.33)	(-1.50)	(4.07)	(0.60)	(1.83)	(3.14)	(3.11)	(-1.04)	(1.16)	(2.53)
Analyst Tenure in the Agency	2.085	2.802	7.805 ***	-4.792 **	6.111 **	1.091	-2.982	9.112 ***	3.814	0.015
	(0.54)	(1.22)	(2.66)	(-1.98)	(2.05)	(0.27)	(-0.75)	(3.85)	(1.22)	(0.00)
N. of Firms Currently Covered	3.918 **	-0.465	2.387	4.504 ***	4.891 ***	1.213	3.725 **	1.377	4.134 ***	3.316 **
	(2.27)	(-0.38)	(1.31)	(3.52)	(2.95)	(0.67)	(2.29)	(1.09)	(2.88)	(2.15)
Agency = Moody's	-43.613	-21.228	48.734	-63.616 ***	14.282	10.603	-38.160	-6.395	2.711	-24.833
	(-1.16)	(-1.18)	(1.56)	(-3.35)	(0.52)	(0.30)	(-1.33)	(-0.29)	(0.10)	(-0.97)
Agency = SP	-105.215 ***	56.375 ***	-23.078	4.247	1.213	-33.891	-59.094 *	15.572	11.807	-16.007
	(-3.02)	(2.84)	(-0.78)	(0.23)	(0.05)	(-1.13)	(-1.77)	(0.76)	(0.43)	(-0.54)
Firm-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.062	0.029	0.047	0.041	0.034	0.069	0.048	0.044	0.028	0.070
Observations	3,404	3,404	3,414	3,408	2,717	2,638	3,249	3,191	3,119	3,121