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Does rating analyst subjectivity affect corporate debt pricing?[☆]

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ABSTRACT

We find evidence of systematic optimism and pessimism among credit analysts, comparing contemporaneous ratings of the same firm across rating agencies. These differences in perspectives carry through to debt prices and negatively predict future changes in credit spreads, consistent with mispricing. Moreover, the pricing effects are the largest among firms that are the most opaque, likely exacerbating financing constraints. We find that masters of business administration (MBAs) provide higher quality ratings. However, optimism increases and accuracy decreases with tenure covering the firm. Our analysis demonstrates the role analysts play in shaping investor expectations and its effect on corporate debt markets.

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1. Introduction

Credit ratings provide a prominent public signal of credit quality. As a result, the analysts who generate those ratings can have an important influence on investors' expectations. We construct a novel data set that links long-term corporate issuer ratings from all three major rating agencies to the individual analysts responsible for each rating. We find evidence of significant analyst fixed effects on firms' long-term credit ratings that cannot be explained by firm, time, or agency effects. These fixed differences in perspectives carry through to the cost of debt capital, particularly among information-sensitive firms.

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In the presence of search or information frictions, ratings analysts can provide a valuable service to investors by aggregating and processing information. If no differences exist in how analysts perform this service, then the assignment of analysts to covered firms will not matter for ratings, even though ratings inform the market. But, if analysts have persistent differences in ability or perspectives, then the assignment of analysts to firms can lead to systematic and predictable differences in ratings. These differences in ratings, in turn, can lead to differences in debt prices if arbitrage is limited and market participants cannot filter them from information.

Individual analysts have several opportunities to affect ratings. When an issuer requests a rating, the rating agency assigns a small team of analysts to cover the firm. After a pre-evaluation, the analysts meet with the firm's management to review relevant information. They then propose a rating to a rating committee, which votes on the rating. Before issuing a press release announcing the rating, the agency notifies the firm of the rating and provides a rationale.[1] Thus, analysts have not only substantial discretion in the evaluation of the firm, but also multiple opportunities for direct communication with management. A firm can be assigned analysts who tend to be pessimistic or optimistic. In addition, repeated interactions with management can create the potential for conflicts of interest or bias arising from familiarity with the rated firm.[2]

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

Second, we measure the degree to which these analyst effects carry through to firms' costs of capital. To avoid the possibility of reverse causality, we reestimate each analyst's fixed effect on ratings quarter by quarter on a backward-looking sample. We then decompose the firm's observed credit rating into the portion determined by the fixed effects of the analysts covering the firm in that quarter and the residual rating. We find that both portions of the credit rating significantly predict spreads on the firm's outstanding debt. In our baseline specification, a one notch increment to residual ratings changes spreads by 49 basis points while a one notch increment to the portion of ratings due to differences in analysts' perspectives changes spreads by 35 basis points. The difference is statistically significant, suggesting that the market views the portion of ratings due to fixed differences in analyst perspectives to be less informative about credit quality than the remainder of ratings. We find similar pricing effects among new issues of public debt. A one notch increment to the analyst-driven portion of ratings changes the offering yield-to-maturity by 25 basis points, compared with 29 basis points for a one notch increment to residual ratings.

We identify several sources of cross-sectional variation in the extent to which the market prices analyst fixed effects into bond spreads. We find that the market fully adjusts for analyst effects in ratings when pricing highly rated bonds (the estimate on the analyst effects is zero) but makes no significant adjustment among lower quality bonds. This result could reflect trading restrictions faced by institutional investors that limit arbitrage pressure or the relative difficulty faced by market participants in filtering information from noise among low-rated firms. To test the second mechanism explicitly, we construct five firm-level measures of information opacity: firm size, firm age, firm scope, the breadth of equity analyst coverage, and the variation in analysts' earnings forecasts. Among opaque firms, we find that analyst fixed effects exert a stronger influence on bond prices. Moreover, the difference between the impact of analyst effects and the residual portion of ratings on prices is smaller. Finally, we consider variation across firms in the information produced by the rating agencies. Among firms covered by multiple agencies and for which, as a result, more reports are available, we find that the market prices significantly less of the analyst fixed effect.

We also consider the dynamics of debt prices. We find little evidence that the residual portion of ratings is significantly associated with future changes in credit spreads, even though it strongly predicts current spreads (more so than analyst fixed effects). This result suggests that analysts do inform the market. On the other hand, systematic analyst optimism (pessimism) in ratings predicts an increase (decrease) in spreads over the following quarters, suggesting that the pricing of analyst fixed effects does not reflect the incorporation of information into prices.

Given the significance of analyst perspectives to debt pricing, our final step is to investigate the extent to which

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

[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. Although this exemption ended with the passage of the 2010 Dodd-Frank Wall Street Reform and Consumer Protection Act (Purda, 2011), the practical effect on the relationships between agencies and rated firms remains unclear.

we can predict differences in analyst perspectives using observable analyst characteristics. We consider both fixed and time-varying characteristics. Using Web sources, we gather demographic information for roughly two-thirds of the analysts in our sample. Again comparing analysts who rate the same firm in the same quarter, we find that analysts with a master of business administration (MBA) degree, with more covered firms, and with longer tenure in the rating agency provide relatively less optimistic ratings. Analysts with a longer tenure covering the firm tend to provide relatively more optimistic ratings. We also measure differences in the accuracy of ratings across analysts. We use Moody's methodology to compute cumulative accuracy profiles (CAP curves) and accuracy ratios across subsamples of analysts defined using the analyst characteristics. We find that MBAs, analysts with more covered firms, and analysts with more experience in the agency provide more accurate ratings; analysts with longer tenure covering the firm are less accurate. We also estimate the results in a panel regression framework, using the correlation of relative optimism with future changes in credit spreads as an alternative measure of accuracy. We find similar results. Thus, while MBAs and experienced analysts appear to produce higher quality ratings, long tenure covering the firm appears to diminish ratings quality. Finally, using our five measures of opacity, we find again that analyst traits matter the most for the ratings of firms that are more opaque and therefore likely to face financing constraints due to information frictions.

Our analysis contributes to the literature on the determinants of credit ratings and their effects on corporate borrowers. Prior studies link ratings quality to features of the industrial organization of the CRAs, such as competition or the issuer-pay ratings model (e.g., Becker and Milbourn, 2011; Kisgen and Strahan, 2010; Xia, 2014; Cornaggia and Cornaggia, 2013). Griffin and Tang (2012) find evidence of subjectivity in the ratings of collateralized debt obligations prior to the 2008 financial crisis. Unlike these studies, we link subjectivity in corporate ratings to the identities of the analysts responsible for the ratings. Consistent with our analysis, Cornaggia, Cornaggia, and Xia (forthcoming) show that analysts who leave a rating agency to work for a firm they previously covered tend to issue more favorable ratings about that firm prior to the transition. We consider the full set of ratings analysts, finding evidence of persistent relative optimism among certain analysts, similar to the dispositional optimism identified in the psychology literature (Scheier and Carver, 1985; Scheier, Carver, and Bridges, 1994) and recent work in household finance (Puri and Robinson, 2007). By tracing the link from such optimism to ratings and then to debt prices, we also provide a novel explanation for previous results linking credit ratings with financing and investment choices (Baghai, Servaes, and Tamayo, 2014; Chernenko and Sunderam, 2012; Kisgen, 2006).

Our analysis parallels a large literature that studies the impact of sell-side equity analysts on recommendations, forecasts, and firm value. Prior work identifies several analyst characteristics that correlate with recommendation quality, including experience and attention (Clement, 1999), past accuracy (Clement and Tse, 2005), gender (Kumar, 2010), all-star status (Clarke, Khorana, Patel, and Rau, 2007; Fang and Yasuda, 2009), competition (Hong and Kacperczyk, 2010), and conflicts of interest (Lin and McNichols, 1998; Michaely and Womack, 1999). Though our results complement the findings in these papers, ratings analysts have different objectives from sell-side equity analysts. Ratings analysts assess the creditworthiness of corporate borrowers; sell-side equity analysts provide portfolio recommendations to equity investors. Thus, the channels through which ratings analysts can influence corporate decisions appear more direct than the corresponding channels for sell-side equity analysts.

Finally, our findings have implications for the behavioral finance literature on investor sentiment. A growing body of work links proxies for investor sentiment to asset prices (e.g., Baker and Wurgler, 2006, 2007; Baker, Wurgler, and Yuan, 2012; Stambaugh, Yu, and Yuan, 2012). However, this work remains largely silent on the determinants of investor sentiment. To the extent that the analyst effects on ratings and prices that we identify do not reflect information about fundamentals, our results suggest that analysts can play a role in generating correlated investor sentiment in financial markets.

2. Data

The core of our data set is credit rating information from all three major ratings agencies—Fitch, Moody's Investors Service, and Standard and Poor's (S&P)—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 US issuers and long- and short-term issues. Because data are sparse prior to 2000, we restrict our sample to announcements between 2000 and 2011. We also restrict the sample to firms with available CUSIPs that we can match to Compustat (for quarterly accounting data) and the Center for Research in Security Prices (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. We are able to find the report corresponding to the announcement in roughly 73% of cases. Our final sample consists of 44,829 announcements on 1,721 firms, of which 571 belonged to the S&P 500 index at some point during the sample period. In the Online Appendix, we provide additional details on the construction of our data sample and breakouts of the announcements by type and agency.

From these data, we construct a quarterly panel data set of long-term issuer ratings from each of the three rating agencies by taking the rating and analyst names from the most recent report at the end of each firm-quarter. To minimize measurement error in the identity of the analysts covering the firm, we do not assign analysts to quarters that end after the date of the final report in which we observe the analyst covering the firm. We measure differences in firms' abilities to access additional debt capital using long-term issuer ratings, which ostensibly measure the ability to honor senior unsecured financial obligations. An alternative would be to consider ratings of individual

Table 1
Credit rating system and letter rating conversion.
The table shows the credit rating systems for Standard & Poor's, Moody's Investors Service, and Fitch ratings and how ratings vary across agencies. The table also shows the percentage of firm-quarter observations with each numerical credit rating value. The agreement sample includes firm-quarters in which all agencies that rate the firm have the same numerical rating. The complement is the split rating subsample, with both the minimum and maximum rating for the firm-quarter.

Credit rating	Letter rating: Standard & Poor's	Letter rating: Moody's	Letter rating: Fitch	Agreement sample ($N = 29{,}005$)	Split rating subsample ($N = 7{,}916$): Minimum rating	Split rating subsample ($N = 7{,}916$): Maximum rating
1	AAA	Aaa	AAA	0.36	0.13	N/A
2	AA+	Aa1	AA+	0.09	0.43	0.13
3	AA	Aa2	AA	0.64	1.40	0.25
4	AA–	Aa3	AA–	1.46	1.52	1.12
5	A+	A1	A+	3.44	2.85	1.89
6	A	A2	A	6.67	3.39	2.11
7	A–	A3	A–	6.84	5.26	3.64
8	BBB+	Baa1	BBB+	8.21	7.95	4.03
9	BBB	Baa2	BBB	12.68	7.50	8.32
10	BBB–	Baa3	BBB–	8.47	8.34	6.61
11	BB+	Ba1	BB+	6.4	12.05	7.90
12	BB	Ba2	BB	8.34	13.05	11.56
13	BB–	Ba3	BB–	11.02	11.84	13.40
14	B+	B1	B+	10.62	10.69	11.67
15	B	B2	B	8.1	7.58	10.52
16	B–	B3	B–	3.81	3.60	8.97
17	CCC+	Caa1	CCC+	1.53	1.48	4.04
18	CCC	Caa2	CCC	0.76	0.58	2.02
19	CCC–	Caa3	CCC–	0.17	0.20	0.72
20	CC, C	Ca	CC, C	0.22	0.18	0.75
21	D	C	D, DD, DDD	0.18	N/A	0.34

bond issues. At the time of issuance, the ratings coincide. However, on an ongoing basis, the long-term issuer rating is a more direct measure of the marginal cost of additional debt capital. We use Standard and Poor's long-term issuer ratings retrieved from Compustat to verify the accuracy of our data. It is impossible to do a similar exercise for Fitch and Moody's ratings because we do not have an independent source of ratings information against which to compare our data set. We find that the ratings agree in roughly 96.5% of cases. Moreover, in the small number of cases in which they disagree, differences are often due to 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 two unsolicited S&P long-term issuer ratings out of 27,342 quarterly observations.

Our analysis relies on comparisons of ratings across agencies. We observe ratings by multiple agencies in 42% of firm-quarters and, among those observations, we observe split ratings 51% of the time (or in 7,916 distinct firm-quarters).[3] Conditional on observing multiple ratings, the frequency of split ratings in our sample appears similar to the frequencies reported in other studies. For example, Livingston and Zhou (2010) find that 49% of bond issues have split ratings between S&P and Moody's over a 1983–2008 sample period. Bongaerts, Cremers, and Goetzmann (2012) find a somewhat lower frequency of split ratings between S&P and Moody's (37%). The difference appears to come from looking at a different sample period and from the requirement that certain financial variables necessary for the analysis be available. In Table 1, we present the distribution of ratings for the subsamples of firm-quarters with and without split ratings. On the split ratings sample, we present separate distributions of the minimum and maximum rating by firm-quarter. Overall, the distributions of ratings are similar for firms with and without split ratings, though firms with split ratings appear slightly worse on average than firms about which the agencies agree. In the event of a split rating, the average difference in ratings across agencies is 1.28 notches. In Online Appendix Table OA2, we provide a breakout of the summary statistics from Table 2 for firms with and without split ratings. Generally, the firms look similar. For example, the mean natural logarithm of sales is 6.68 and 6.67 in the two samples, and mean leverage is 0.382 and 0.342. Nevertheless, existing research emphasizes the opacity of the assets as a determinant of split ratings (Livingston, Naranjo, and Zhou, 2007; Morgan, 2002). If less room exists for analyst discretion in firms without split ratings, then our results could extend less readily beyond the set of split-rated firms. We

[3] Missing ratings from agencies occur for two main reasons in our data set. First, we sometimes cannot assign an analyst to a firm-quarter because the quarter occurs after the last report from analysts who previously covered the firm, but before any reports from newly assigned analysts. In this scenario, we do not know which analyst(s) to assign. Second, we sometimes do not observe long-term issuer ratings from an agency, even in some cases for firms whose bond issues they do rate. To be sure that these restrictions do not impose bias, we reestimate our main regressions on the subsample of firms for which we (at a minimum) observe ratings from both Moody's and Standard and Poor's. Our results are unaffected (see Online Appendix Table OA3).

Table 2
Summary statistics.
This table provides summary statistics of the variables used in the paper. Panel A describes the credit rating variables used for the Wald tests as well as analyst and firm traits for each agency-firm-quarter. Panel B summarizes firm characteristics and ratings for each firm-quarter. Panel C shows the pairwise correlations of the analyst variables and ratings. All variables are defined in Table A1.

Panel A: Agency-firm panel

Variable	Number of observations	Mean	Median	Standard deviation	10th percentile	90th percentile
Rating variables						
Credit rating	53,184	11.058	11	3.453	6	15
Negative outlook	59,674	0.181	0	0.385	0	1
Negative watch	59,674	0.047	0	0.211	0	0
Positive outlook	59,674	0.078	0	0.268	0	0
Positive watch	59,674	0.014	0	0.119	0	0
Signed watch	59,674	−0.032	0.000	0.245	0	0.000
Stable outlook	59,674	0.413	0.000	0.492	0	1.000
Analyst and firm variables						
Accuracy	6,683	1.459	0	234.127	−219.230	222.050
Agency = Moody's	22,827	0.360	0	0.480	0	1
Agency = Standard & Poor's	22,827	0.396	0	0.489	0	1
Agency Tenure Covering the Firm	22,827	4.805	4.252	3.444	0.751	9.260
Analyst age	22,827	39.500	39	7.689	30	49
Analyst tenure covering the firm	22,827	2.086	1.751	1.729	0.249	4.381
Analyst tenure covering the industry	22,827	3.475	3.127	2.267	0.832	6.510
Analyst tenure in the agency	22,827	6.973	5.921	4.771	2.127	12.756
Equity analysts' earnings forecast dispersion	18,773	0.015	0.026	1.231	−0.109	0.174
Female	22,827	0.257	0	0.375	0	1
Firm age	22,827	28.776	22.764	18.539	7.501	56.038
MBA	22,827	0.735	1	0.420	0	1
MBA non top 5	22,827	0.665	1	0.445	0	1
MBA top 5	22,827	0.070	0	0.235	0	0
Number of firms currently covered	22,827	13.557	11	9.663	4.5	27
Number of equity analysts	19,789	10.977	10.000	7.023	3	21
Number of segments	18,324	1.606	1.000	0.885	1	3
Optimism	22,827	−0.036	0	0.955	−1	1
Rating dispersion	22,827	0.654	0.500	0.697	0	1.500
Total assets	22,684	36,647	4,830	166,658	787	43,312

Panel B: Firm panel

Variable	Number of observations	Mean	Median	Standard deviation	10th percentile	90th percentile
Aggregate analyst effects	23,386	0.053	0.067	0.601	−0.498	0.645
Bond age (days)	15,349	1,379	1,144	1,075	281	2,756
Bond duration	15,349	5.456	5.139	2.488	2.567	8.712
Callable bond dummy	15,349	0.834	1	0.352	0	1
Carryforwards	23,386	0.052	0	0.13	0	0.162
Credit rating	23,386	10.978	11	3.380	6	15
Credit rating (adjusted)	23,386	10.925	11.254	3.378	6.218	15.056
Credit spread	15,349	324.513	254.475	239.142	81.795	693.727
Equity beta	11,483	1.231	1.118	0.627	0.550	2.06
Equity volatility	14,737	0.386	0.321	0.237	0.179	0.673
Expected default frequency	13,623	0.063	0	0.196	0	0.162
Change in log credit spread [t,t+1]	8,358	0.015	0.003	0.342	−0.341	0.391
Change in log credit spread [t,t+4]	6,826	0.070	0.039	0.584	−0.625	0.823
Change in log credit spread [t,t+8]	5,312	0.204	0.159	0.776	−0.768	1.232
Change in log credit spread [t,t+12]	4,087	0.459	0.499	0.756	−0.535	1.411
Interest coverage k1	14,350	4.014	5	1.253	2.126	5
Interest coverage k2	14,350	1.71	0.047	2.118	0	5
Interest coverage k3	14,350	1.311	0	3.003	0	6.747
Interest coverage k4	14,350	1.353	0	6.906	0	0
Leverage decrease spikes	23,368	0.063	0	0.243	0	0
Leverage increase spikes	23,368	0.162	0	0.369	0	1
Long-term leverage	23,386	0.319	0.284	0.21	0.088	0.588
Market-to-book	23,386	1.481	1.268	0.676	0.935	2.278
Market value of equity (log)	14,825	8.445	8.408	1.559	6.500	10.442
Pessimism count	14,902	0.073	0.000	1.192	−1.000	2.000
Profit margin	23,386	0.194	0.161	0.167	0.039	0.407
R&D/sales	23,386	0.014	0	0.039	0	0.048
Sales (log)	23,386	6.675	6.602	1.428	4.916	8.535
Sales growth	23,386	0.03	0.019	0.185	−0.15	0.205
Stock return	14,578	0.051	0.091	0.432	−0.445	0.486
Tangibility	23,386	0.323	0.26	0.253	0.021	0.708
Taxshields	23,386	0.036	0.014	0.048	0	0.112
Time since last bond trading	15,349	5.974	1	14.275	0	17
Total leverage	22,664	0.36	0.323	0.215	0.125	0.632

(continued on next page)

Table 2 *(continued)*

Panel C: Pairwise correlations

Variable	Optimism	Rating dispersion	Accuracy	Credit rating	MBA	Analyst age	Female	Analyst tenure covering the firm	Agency tenure covering the firm	Analyst tenure covering the industry	Analyst tenure in agency	Number of firms currently covered
Optimism	1.000											
Rating dispersion	0.001	1.000										
Accuracy	−0.361	0.002	1.000									
Credit rating	−0.214	0.107	0.081	1.000								
MBA	−0.006	−0.033	0.011	0.041	1.000							
Analyst age	−0.038	0.053	0.025	−0.123	−0.095	1.000						
Female	−0.050	0.026	0.040	−0.059	−0.243	0.066	1.000					
Analyst tenure covering the firm	0.072	−0.008	−0.038	−0.108	−0.005	0.280	−0.031	1.000				
Agency tenure covering the firm	0.061	0.018	−0.057	−0.090	−0.028	0.134	−0.032	0.305	1.000			
Analyst tenure covering the industry	0.076	0.021	0.004	−0.104	−0.045	0.349	−0.031	0.728	0.305	1.000		
Analyst tenure in agency	−0.010	0.064	0.023	−0.164	−0.210	0.555	0.164	0.363	0.159	0.479	1.000	
Number of firms currently covered	−0.154	0.037	0.069	0.231	−0.028	0.238	−0.095	0.057	0.028	0.140	0.098	1.000

address this possibility explicitly in Section 4. Throughout our empirical analysis, we follow convention in translating ratings to a numerical scale (see, e.g., Bongaerts, Cremers, and Goetzmann, 2012). We provide the full translation in Table 1.

We supplement the analyst data with hand-collected demographic information from Web searches, most commonly from public LinkedIn profiles. Of the 1,072 unique analysts in our data, we are able to retrieve data for 798. We extract biographical information on age as well as the professional and educational background of the analysts. Educational background (school, degree, and degree date) is available for 638 analysts, of whom 65% have an MBA. To measure analyst age, 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. We use first names (and, in ambiguous cases, additional Web searches) to infer analyst gender.

We use Trade Reporting and Compliance Engine (TRACE) data and the Mergent Fixed Income Securities Database (FISD) issue and redemption file to measure credit spreads at the bond level following the approach of Campbell and Taksler (2003), Bongaerts, Cremers, and Goetzmann (2012), and Bessembinder, Kahle, Maxwell, and Xu (2009). For firms with multiple outstanding bond issues, we aggregate spreads to the firm level using the approach of Qiu and Yu (2009). See the Online Appendix for a description of the construction of credit spreads. We also obtain the offering yield to maturity for new public debt issues from the Securities Data Company (SDC) database.

Finally, we use accounting information from Compustat and equity analyst information from the Institutional Brokers' Estimate System (I/B/E/S) to measure the sensitivity of firms to information frictions. We measure firm size using total assets at the end of the 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 and 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. We provide complete variable definitions in Table A1.

In Panel A of Table 2, we report summary statistics of the data at the firm-quarter-agency level. The median issuer rating in our sample is BB+, translating all ratings to the S&P rating scale. Some cross-sectional differences appear across agencies. The median Fitch rating is BBB; the median S&P rating, BB+; and the median Moody's rating, BB−. We also provide summary statistics of the data for the subsample on which the analyst traits are available. In a given firm-quarter, the average analyst is 39.5 years old and has worked for her agency for seven years, covering the industry for 3.5 years and the firm for two years. The average covered firm is 29 years old, has roughly $37 billion in assets, and is covered by 11 equity analysts. In Panel B, we report summary statistics of the firm-level variables that we use in our analysis of credit spreads.

Panel C presents selected pairwise correlations of the variables.

3. Do analysts matter for credit ratings?

Our first step is to ask whether the identity of the analyst(s) covering a firm influences its credit rating after accounting for fundamentals.

3.1. Empirical specification and identification strategy

To identify systematic analyst effects on ratings, we follow an approach similar to the one used by Bertrand and Schoar (2003) to identify the effect of corporate managers on firm policies separately from firm effects. Our baseline regression specification is

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

$Rating_{ijt}$ is the long-term issuer rating for firm j in quarter t by rating agency i. α_{jt} is a firm-quarter fixed effect, and β_i is a rating agency fixed effect. $\boldsymbol{\gamma}_{analyst}$ includes the explanatory variables of interest: dummy variables for each sample analyst that take the value one if the analyst covered firm j in quarter t for agency i and zero otherwise. To have sufficient variation to estimate effects for each analyst, we include dummies only for analysts who cover at least five sample firms. Even with this restriction, we retain 99% of firm-quarter-agency observations.[4]

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 could confound the estimates, appropriate time-varying controls must be specified and defined. 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 time. This approach makes it unnecessary to specify or include any time-varying controls for firm fundamentals (e.g., leverage ratios or cash holdings), as they cannot be identified independently from the fixed effects. It also mitigates selection concerns. The matching of analysts to firms is unlikely to be random; for example, analyst teams are often organized by sector. However, the interpretation of our results is not affected by this type of matching because we identify analyst effects by comparing analysts who cover the same firm at the same time.

We identify the effect of analysts on ratings separately from the effects of their agencies in several ways. Eq. (1) includes a fixed effect for each rating agency so that our estimates of $\boldsymbol{\gamma}_{analyst}$ are not confounded by differences in the average ratings conferred by the three agencies. We also estimate three more stringent variations of the model. First, we allow the agency fixed effects to vary by sector s, defined using two-digit Global Industry Classification Standard (GICS) codes, replacing β_i with β_{is}. This specification allows for differences within and across agencies in average ratings by sector. Because we identify the analyst effects using only variation within each agency-sector pair, they are unaffected by differences across agencies in how analysts are assigned to sectors. Second, we allow the differences in how the agencies assess each sector to vary over time by including interactions of the agency-sector effects with quarter fixed effects, replacing β_i with β_{ist}. Thus, our estimates are robust to differences in the matching of analysts to sectors across agencies and time. Third, we change the unit of observation from the sector to the firm, including fixed effects for each agency-firm combination, replacing β_i with β_{ij}. In this specification, we allow each agency to have a different average rating for each sample firm and identify the analyst effects using only firms that are covered by multiple analysts for the same agency at different times. Because we compare only analysts who cover the same firm at different times for the same agency, our estimates are unaffected by differences across agencies in how analysts are matched to firms they cover.

Though our specifications address the most compelling sources of nonrandom sorting, it is impossible to rule out with additional fixed effects the possibility that sorting is nonrandom and differs across agency-firm-quarter groupings. For example, agencies could reassign analysts within a sector to cover different firms over time depending on the performance of their ratings or current firm conditions (i.e., not randomly) and differently across the agencies. However, this kind of sorting does not appear to be a practical concern. Standard measures of rating accuracy (such as CAP curves) are impractical to calculate for individual analysts, making it difficult to systematically measure and track the accuracy of ratings by individual analysts. For example, we confirmed in a conversation with a Moody's executive that the agency does not compute CAP curves for individual analysts. Moreover, analyst-firm matches appear to be stable over time, perhaps because agencies perceive a cost from sacrificing match-specific expertise.[5]

Our null hypothesis is that the coefficients on the individual analyst effects are jointly equal to zero. That is, credit ratings are fully explained by the macroeconomic, firm, and agency factors captured by the firm-quarter and agency fixed effects (or the individual dispositions of analysts are irrelevant to ratings). Recent research raises con-

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

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

cerns about inferences from standard Wald tests in this type of specification (Fee, Hadlock, and Pierce, 2013). In particular, the dependent variable in our regression is highly persistent over time. Thus, analyst fixed effects, because they are also persistent, can 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).[6] To address these econometric concerns, we assess statistical significance using a resampling approach to test our hypotheses. Because our interest is in the F-statistic for a joint test of the significance of the analyst fixed effects, we use a block bootstrap procedure to construct the empirical distribution of the F-statistic and to assess its significance.[7]

To execute the bootstrap, we identify each analyst-firm spell in the data. For example, if Analyst 1 covers GE for five consecutive quarters, this represents a single analyst-firm spell. Under our null hypothesis, the labels on these analyst spells are exchangeable. Thus, we randomly reassign sample analysts to the analyst-firm spells, requiring that each analyst still be assigned to the same number of spells as in the actual data. By construction, the resulting data set preserves the same persistence structure as the original data because 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 can be assigned separately to three different analysts. Analyst 1 is still assigned to cover three spells, but likely in firms other than GE, IBM, and Microsoft. To perform our hypothesis test, we make one thousand such reassignments. We then estimate Eq. (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 one thousand placebo samples. We compute a p-value for the null hypothesis that the actual analyst effects equal zero as the fraction of F-statistics in the placebo samples that exceed the actual F-statistic. Although putting further restrictions on the assignment of analysts to spells would be possible, it is important not to include any restrictions based on analyst-level variation because the resampling would then subsume a portion of the effect of interest. For example, reshuffling analysts only among spells of the same length would not be appropriate.

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

3.2. Analyst effects on long-term issuer ratings

To illustrate the sources of variation that we use to identify the analyst effects, we begin by constructing the network of analyst coverage across firms. In Panel A of Fig. 1, we illustrate the coverage networks within each of the three rating agencies in the fourth quarter of 2008 and, in Panel B, we illustrate the full network across agencies in the same quarter.[8] Each point in the diagrams represents an analyst and we define two analysts to be connected if they share coverage of at least one sample firm during the quarter. Because analysts work as teams to cover firms, the identification of distinct effects for individual analysts requires variation in the composition of teams across firms. The density of the connections in the network diagrams suggests that this condition is met. We find that the likelihood that two analysts in an agency who cover a firm together also cover another common firm is only 33% on average. We do see some clusters of analysts who are separated from the main mass of analysts, particularly in the S&P and Fitch networks. These subnetworks do not tend to be contained within a single sector; for example, the cluster at the top left of the Fitch diagram contains both big box retailers and consumer products companies (RadioShack, Best Buy, Home Depot, Whirlpool, CocaCola, PepsiCo, Dole Foods, etc.). Overall, the diagrams do not suggest rigid clustering of analysts by sector (either within or across agencies), which allows us to distinguish analyst fixed effects from agency-sector (-quarter) effects.

In Column 1 of Table 3, we present the results from estimating Eq. (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 significant differences exist across agencies in mean ratings, even after washing out all firm-level variation. Fitch ratings are the most lenient (though they are not statistically different on average from S&P ratings), and Moody's ratings are significantly lower on average than the other two agencies. Turning to the analyst effects, we find an F-statistic of 10.59 for the test that the analyst effects jointly equal zero. The p-value for a traditional Wald test is less than 0.001. Forty-one percent of the individual analyst effects are statistically significant at the 5% level. Applying

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

[7] Block bootstrap also can be used to construct standard errors for each analyst dummy in a least squares dummy variables implementation of the fixed effects model. However, using these standard errors to perform the joint significance test would require additional distributional assumptions, partially defeating the purpose of the bootstrap.

[8] We also constructed the coverage networks for other quarters, both earlier and later in the sample. Though we report only a single quarter, the basic features of the networks appear to be representative.

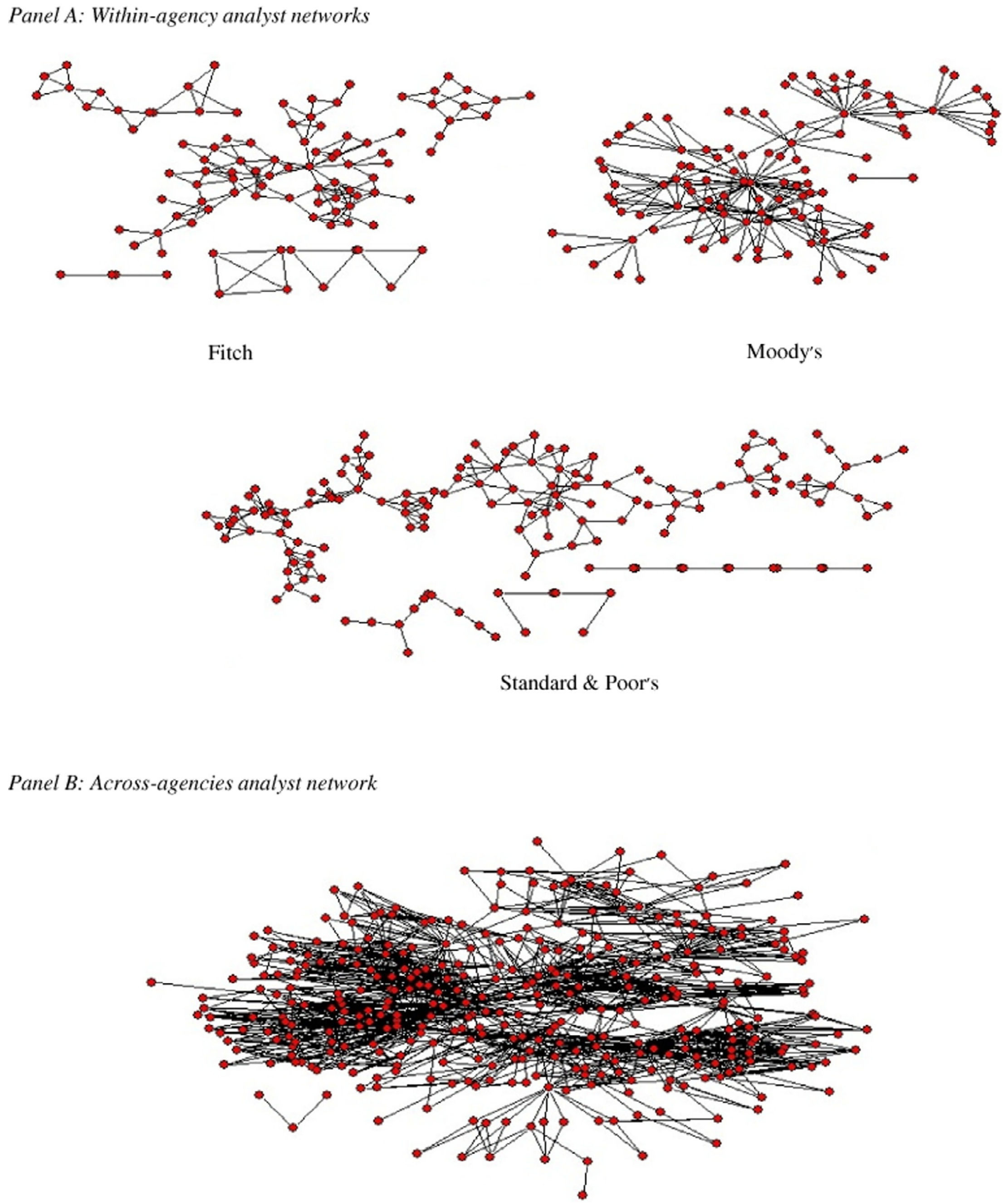

Fig. 1. Rating analyst networks. The figures show the networks of rating analysts as of the end of the fourth quarter of 2008. Analyst pairs are connected if there is an overlap in firm coverage between the two analysts. Panel A shows analyst connections within each agency. Panel B shows analyst connections across agencies. The network is plotted using the Kamada-Kawai energy algorithm. The figures include only analysts with at least one connection.

our resampling procedure, we find that the true F-statistic is larger than all one-thousand F-statistics computed on the placebo samples. Thus, we compute a p-value of 0.001 for our null hypothesis.

To gauge the economic significance of the analyst effects, we first ask how much of the within variation they are able to explain (relative to the agency fixed effects). In our estimate of Eq. (1), the adjusted within R^2 is 0.3057. To provide a lower bound on how much of this explanatory power comes from the analyst effects, we reestimate Eq. (1), excluding the analyst effects. We find an adjusted within R^2 of 0.0376. Thus, the agency fixed effects explain at most 3.76% of the variation, implying that the analyst effects account for at least 26.81%. We also compute an upper bound by reestimating Eq. (1), excluding the agency fixed effects. The adjusted within R^2 is 0.3024, implying

Table 3
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 ordinary least squares regression of long-term credit ratings on analyst fixed effects, firm-quarter fixed effects, and either agency fixed effects (Column 1), agency-sector fixed effects (Column 2), agency-sector-quarter fixed effects (Column 3), or agency-firm fixed effects (Column 4). Sectors are measured using two-digit Global Industry Classification Standard (GICS) codes. The credit rating is a numeric variable ranging from 1 (AAA) to 21 (default). The table also reports in the row "Placebo test" the percentage of one thousand 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% level is denoted by ***, **, and *, respectively.

Test statistic	(1)	(2)	(3)	(4)
F-statistic analyst fixed effects	10.59***	9.00***	8.66***	5.54***
p-value analyst fixed effects	<0.1%	<0.1%	<0.1%	<0.1%
Placebo test p-value analyst fixed effects	<0.1%	1.8%	5.8%	6.3%
Analyst fixed effects	Yes	Yes	Yes	Yes
Firm-quarter fixed effects	Yes	Yes	Yes	Yes
Agency fixed effects	Yes			
Agency-sector fixed effects		Yes		
Agency-sector-quarter fixed effects			Yes	
Agency-firm fixed effects				Yes
Number of observations	53,184	52,763	52,763	53,184

that analyst effects explain at most 30.24% of the within variation in ratings. However, it is important to note that fixed effects do not isolate all of the variation that is due to analysts. In particular, analysts' information production is likely to vary across firms and over time and is not captured fully by the fixed effects in Eq. (1).

Another way to assess the economic importance of the variation in ratings due to the analyst fixed effects is to compare it with other known drivers of ratings. For example, Becker and Milbourn (2011) find that a one standard deviation change in competition among agencies changes ratings by 0.19 notches. By comparison, a one standard deviation change in ratings due to analyst effects is 0.46 notches, suggesting that the economic importance of analysts is relatively large. In Section 4, we further demonstrate the economic significance of analyst effects by establishing a link to debt prices.

Next, we estimate the three variations of Eq. (1) described in Section 3.1 that allow for more flexible differences in long-term ratings across agencies. First, we allow the agency effect to differ by sector. Eq. (1) uses only variation within agencies to identify analyst effects. Here, we further restrict our attention to variation within agencies and sectors. As in the baseline specification, we include firm-quarter fixed effects so that we compare each analyst only with other analysts simultaneously covering the same firm. We present the results in Column 2 of Table 3. The F-statistic to test the joint significance of the analyst fixed effects is 9.00, again yielding a p-value less than 0.001 for a traditional Wald test. Using our block bootstrap procedure, we find that the F-statistic of 9.00 is higher than the F-statistic from 982 of one thousand regressions on placebo samples, implying a p-value of 0.018.

Second, we allow the agency-sector effect to vary by quarter. Thus, we identify the analyst effects using only variation across analysts working in the same sector for the same agency in the same quarter. We present the results in Column 3. Using a traditional Wald test, the analyst effects are again significant with a p-value of less than 0.001. Moreover, the estimated F-statistic of 8.66 is larger than the F-statistic in 942 of one thousand placebo samples, implying a p-value of 0.058 using our block bootstrap procedure.

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

A potential alternative explanation of our results is that analyst fixed effects capture short-term differences in the timing of ratings announcements. However, ratings are split in over half of the cases in which we observe multiple agencies covering the same firm (Section 2). Thus, the data do not support a story in which split ratings simply reflect differences in the timing of changes to the same consensus rating. Moreover, a simple tendency to update ratings more quickly would not generate a bias toward relative optimism or pessimism.

Overall, we uncover significant analyst effects on long-term issuer ratings. These effects provide credible measures of dispositional optimism at the analyst level. In Fig. 2, we graph the distribution of the estimated analyst effects (Panel B). We also plot the distribution of the F-statistics from the one thousand placebo samples created by our block bootstrap procedure, indicating the placement of the true F-statistic in the distribution with a dotted line (Panel A). For brevity, we present only the specification with agency-sector-quarter fixed effects, which we

Panel A. Placebo test *Panel B. Analyst fixed effects*

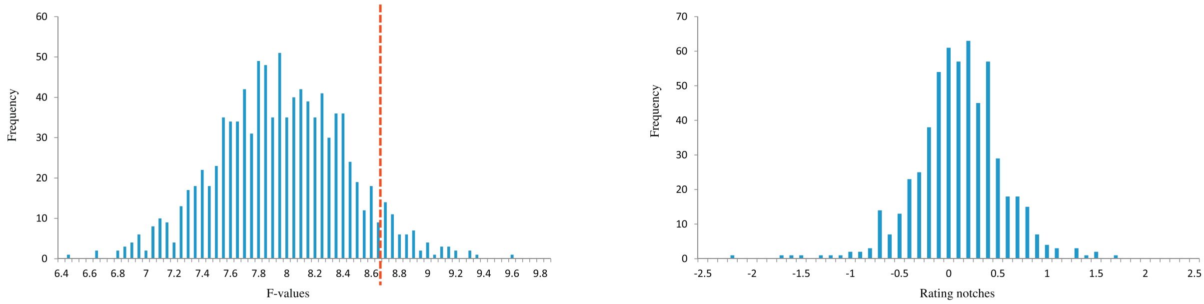

Fig. 2. Histograms of placebo test results and analyst fixed effects. Panel A shows the histogram of F-statistics on one thousand placebo runs in which we substitute the analyst name with the name of an analyst drawn randomly for each analyst-firm pair. The F-statistic is for a test of the joint significance of analyst fixed effects in an ordinary least squares (OLS) regression of long-term credit ratings on analyst fixed effects, firm-quarter fixed effects, and agency-sector-quarter fixed effects. The vertical dashed line represents the F-statistic for a test of the joint significance of analyst fixed effects in the same regression specification on the real data. Panel B shows the histogram of the estimated coefficients of the analyst effects from an OLS regression of long-term credit ratings on analyst fixed effects, firm-quarter fixed effects, and agency-sector-quarter fixed effects.

adapt and apply in the remainder of the paper. In Online Appendix Table OA4, we show that analysts also have significant fixed effects on the short-term watches that agencies place on issuer ratings.

4. Real effects of systematic analyst optimism or pessimism

Having established that analysts significantly affect credit ratings, we now ask whether the resulting fixed effects on ratings translate to differences in the prices of the firm's debt.

4.1. Analyst effects on credit spreads

If an efficient market recognizes that a portion of a firm's credit rating derives from systematic optimism or pessimism of the analysts covering the firm, then it should respond accordingly, determining prices using only the real information contained in the rating. Thus, our null hypothesis is that the portion of ratings determined by analyst effects does not predict credit spreads on the firm's debt.

To test this hypothesis, we reestimate the analyst fixed effects, but using only information available to market participants at the time prices are set. Although using exactly the fixed effects we estimated in Section 3.2 would be possible, constructing a backward-looking measure of analyst effects allows us to avoid the potential for reverse causality. Thus, for each sample quarter, we estimate Eq. (1) using only sample observations from prior quarters. We also include agency-sector-quarter fixed effects in lieu of the agency effects in Eq. (1). An advantage of this specification is that the comparison groups for each analyst in a particular quarter do not change as we add additional quarters to the regression. Each analyst continues to be compared only with other analysts simultaneously covering the same firm and with other analysts simultaneously covering the same sector within his or her agency. We update the analyst effects when we add a new quarter to the regression only due to changes in how the analyst behaved relative to other analysts in that quarter. Though we report only this specification, we find similar results if we instead include either agency or agency-firm fixed effects in Eq. (1).

Next, we aggregate the estimated fixed effects of the analysts covering each sample firm in a given quarter. We sum the estimated fixed effects for the analysts covering the firm for each agency. This computation yields the portion of each agency's rating in each quarter that is due to the systematic optimism or pessimism of the analysts covering the firm (*Aggregate Analyst Effects*). We then subtract the aggregate analyst effects from the observed credit rating, yielding a residual rating (*Adjusted Credit Rating*). This decomposition isolates the portion of the observed rating driven by the dispositional optimism of the analysts covering the firm from the portion of ratings driven by all other factors (creditworthiness, information, etc.).

Though we measure the relative optimism or pessimism of analysts using the difference in ratings between analysts covering the same firm at the same time, the aggregate analyst effects for each given firm-quarter are almost always different from zero. This is because the analyst fixed effect is the systematic relative optimism of an analyst averaged across different firms over time. Moreover, we can apply our measure of analyst fixed effects to all sample firms even though we construct it using the subsample of split-rated firms. Our economic hypothesis is that certain analysts are predisposed to relative optimism or pessimism across the set of firms they rate. Split-rated firms merely provide a setting in which we can observe those dispositions. Split-rated firms do not appear to differ meaningfully from other sample firms in their fundamentals (see Section 2).

Because the dependent variable, the firm's credit spread, does not vary by agency, we average the aggregate analyst effects and adjusted credit rating across agencies for each firm quarter. An alternative approach would be to run the regression at the firm-quarter-agency level and then to adjust the standard errors for the repetition of firm-quarters. Because the panel is unbalanced (i.e., the number of agencies providing a rating differs across firm-quarters), the two approaches are not equivalent.

Table 4
Credit spreads and aggregate analyst effects.

The table reports coefficient estimates from ordinary least squares 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 Table A1. Columns 1 and 2 contain all observations. Columns 3 and 4 contain only observations in which the credit rating is between AAA and A− and between BBB and BB+, respectively. Columns 5 and 6 contain only observations in which the firm is rated only by a single rating agency and by more than one agency, respectively. Standard firm controls are long-term leverage, profit margin, market-to-book, sales (log), tangibility, tax shields, carryforwards, and ratio of quarterly research and development expenditures to quarterly sales. Coefficient estimates are reported in the Online Appendix. Robust t-statistics double-clustered at the firm and quarter levels are reported in parentheses below the coefficients. Constant is included. Significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Variable	All ratings		AAA/A−	BBB/BB+	Single agency	Multiple agencies
	(1)	(2)	(3)	(4)	(5)	(6)
Adjusted credit rating	48.637***	31.834***	9.260***	50.224***	33.253***	30.873***
	(26.53)	(14.30)	(2.90)	(9.33)	(11.99)	(12.44)
Aggregate analyst effects	35.324***	25.754***	0.001	50.850***	32.924***	19.075***
	(8.62)	(6.61)	(0.00)	(6.90)	(7.01)	(3.17)
Bond duration	−2.617**	0.936	3.546***	2.142	1.401	0.384
	(−1.96)	(0.85)	(3.19)	(1.62)	(1.17)	(0.26)
Callable bond dummy	−39.291***	−0.111	26.688*	4.101	8.159	−1.248
	(−3.59)	(−0.01)	(1.90)	(0.40)	(0.87)	(−0.11)
Bond age	0.006**	0.010***	0.019***	0.013***	0.009**	0.011**
	(2.18)	(3.54)	(4.73)	(3.41)	(2.48)	(2.46)
Time since last trade	0.778***	−0.055	0.412	−0.031	−0.125	0.055
	(5.09)	(−0.36)	(1.02)	(−0.16)	(−0.53)	(0.17)
Interest coverage k1		−8.280**	−6.029	2.883	−15.016***	−3.672
		(−2.37)	(−0.70)	(0.73)	(−2.64)	(−1.02)
Interest coverage k2		0.426	0.240	−0.525	1.829	−0.976
		(0.26)	(0.09)	(−0.24)	(0.78)	(−0.47)
Interest coverage k3		0.941	−1.481	2.723*	0.088	2.399
		(0.85)	(−1.55)	(1.72)	(0.06)	(1.58)
Interest coverage k4		0.885	−0.038	0.435	−0.415	2.411**
		(1.16)	(−0.12)	(0.45)	(−0.97)	(2.34)
Total leverage		13.981	−51.882	64.341	−47.516	60.626
		(0.40)	(−1.21)	(0.86)	(−1.08)	(1.58)
Market value of equity (log)		−29.468***	−15.153**	−19.118**	−36.803***	−24.900***
		(−6.29)	(−2.37)	(−2.50)	(−6.27)	(−4.26)
Equity beta		−4.693	−22.575***	−3.383	−12.573*	0.293
		(−0.92)	(−2.69)	(−0.46)	(−1.80)	(0.04)
Equity volatility		233.308***	231.095***	213.748***	281.601***	203.759***
		(7.29)	(3.98)	(4.97)	(7.09)	(5.58)
Expected default frequency		130.471***	96.795***	115.609***	95.949***	153.592***
		(6.35)	(3.34)	(3.69)	(3.47)	(5.89)
Stock return (log)		−22.517***	−20.425	−21.018*	−30.664***	−13.203
		(−2.79)	(−1.58)	(−1.75)	(−2.87)	(−1.30)
Standard firm controls	No	Yes	Yes	Yes	Yes	Yes
Year-quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.734	0.813	0.686	0.777	0.796	0.830
Number of observations	15,349	9,259	2,050	3,067	4,051	5,208
p-value for t-test Group 1 *Aggregate analyst effects* = Group 2 *Aggregate analyst effects*			0.001		0.070	
p-value for t-test *Adjusted credit rating* = *Aggregate analyst effects*	<0.001	0.057	0.007	0.911	0.931	0.027

We prefer to average observations to avoid overweighting observations with greater agency coverage in the regressions.[9]

In Column 1 of Table 4, we present estimates of our baseline regression of credit spreads (measured as the value-weighted credit spread across the firm's outstanding bond issues at the end of a given quarter) on decomposed long-term credit ratings. The coefficient estimate on *Adjusted Credit Rating* is identical to the coefficient we would estimate on the observed credit rating if we instead included *Aggregate Analyst Effects* and the observed rating as regressors. In that case, the coefficient on *Aggregate Analyst Effects* would measure the difference between the effect of the observed rating and the analyst effects on spreads instead of the direct effect of analyst effects. We include controls for the value-weighted averages of the duration, callability, and age of the firm's outstanding bonds. We also include the time since the last date on which the firm's bonds traded as a measure of bond liquidity. Finally, we include fixed effects for each quarter to adjust for market-wide trends in yields. Because we observe persistent sets of bonds within a firm over time and because spreads are likely to move together with the market across firms, we cluster standard errors on two dimensions, firm and quarter, using the method from Thompson (2011).

We find that firms with callable bonds and bonds with longer duration face significantly lower credit spreads.

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

Firms with older and less liquid bond issues face higher spreads. Turning to the effects of interest, we find that a one notch improvement in the firm's adjusted credit rating is associated with a 49 basis point decrease in credit spreads, consistent with ratings conveying valuable information to market participants. Our estimates of analyst effects are orthogonal to firm fundamentals by construction, because Eq. (1) contains firm-quarter fixed effects. Yet, the market reacts significantly to the portion of ratings driven by analyst effects. A one notch improvement in ratings due to aggregate analyst effects decreases spreads by 35 basis points.[10] We do uncover evidence of significant adjustment to the source of the rating information. The estimates on the aggregate analyst effect and the adjusted credit rating are significantly different ($p\text{-value} = 0.001$). However, we still observe a substantial and highly significant response to the portion of ratings driven by analyst identity, equal to roughly 71% of the effect of observed ratings on spreads. Thus, the assignment of analysts to firms, and therefore the assignment of a particular set of tendencies toward optimism or pessimism, affects the prices at which the firms' debt trades in the marketplace.

In Column 2, we add a number of additional controls to the regression. We include a battery of firm-level controls for cash flow- and capital structure-relevant variables, measured at the beginning of the quarter: long-term leverage, profit margin, market-to-book, the natural logarithm of sales, tangibility, the utilization of tax shields and carryforwards, and the ratio of research and development (R&D) expenditures to sales.[11] We also include variables from the set of controls in the credit rating model of Blume, Lim, and MacKinlay (1998) that are not already part of the specification: total leverage, interest coverage divided into four splines, the natural logarithm of the market value of equity, equity beta, and equity volatility. Likewise, we include additional controls from Baghai, Servaes, and Tamayo (2014), who estimate a similar regression of credit spreads on differences between observed and model-predicted credit ratings: the natural logarithm of the annual stock return and the expected default frequency.[12] The addition of the controls (and resulting reduction in sample size) has some impact on the estimates of interest. Nevertheless, our conclusions are unchanged. The market significantly adjusts for the portion of ratings driven by aggregate analyst effects but leaves roughly 80% of the effect in place.

If the pricing of fixed analyst effects is not a result of the market incorporating new information about fundamentals, then prices should react more to those effects when arbitrage is limited. As a rough proxy for arbitrage pressure, we separate the sample of firms into those with ratings of A− or higher and those with ratings between BBB and BB+. The latter group of bonds has ratings just around the investment-grade cutoff. Many large institutions that could be well positioned to make arbitrage trades face either explicit restrictions or higher costs when trading non-investment-grade debt. Charters often limit the ability of pension funds or mutual funds to hold non-investment-grade debt. Moreover, Basel and the National Association of Insurance Commissioners (NAIC) impose higher capital requirements on banks and insurance companies that hold high-yield debt. Thus, we expect the pressure to correct mispricing to be smaller in this subsample.[13] For example, such institutions might not be willing or able to buy a bond with a BB+ rating, even if they believe the bond should rightfully be rated BBB.

In Columns 3 and 4 of Table 4, we report the results of estimating the specification from Column 2 separately on the two subsamples. We find no evidence that the market prices relative analyst optimism in the subsample of firms with ratings of A− or better. A one notch increment to the adjusted rating changes spreads by 9.26 basis points. However, aggregate analyst effects are not statistically or economically significant. Moreover, the difference between the two estimates (or the estimated adjustment by the market) is statistically significant at the 1% level. There are two differences among firms around the investment-grade cutoff. First, a one notch increment to adjusted ratings has a larger effect on spreads (50 basis points). This result is not surprising given that there is likely to be more uncertainty about the fundamentals of lower rated firms and, therefore, a greater scope for analysts to convey (time-varying firm-specific) information to the market through ratings. Second, and more interesting, the market prices the aggregate analyst effects. In this subsample, a one notch increment to the analyst effects changes spreads by 51 basis points. Moreover, no evidence exists of significant adjustment by the market to the nature of the rating information (the two coefficient estimates are not significantly different).[14] Although our results are consistent with the hypothesis that investment-grade bonds are subject to more intense arbitrage pressure, market participants could simply be better able to infer the fundamentals of investment-grade firms without reference to ratings. At least among analysts, however, disagreement still exists about the credit quality of A-rated firms. Split ratings are evident across agencies 29% of the time, compared with 37% in the BBB to BB+ subsample. We also continue to find no pricing of the analyst effects and a significant difference with the effect of the residual portion of ratings on prices if we eliminate firms rated AA or higher from the sample. Moreover, this potential difference in interpretation does not affect our conclusion that the market prices the portion of ratings

[10] A one standard deviation change in the *Aggregate Analyst Effects* in our sample is roughly 0.601 notches. Because it is not possible to change a rating by less than one notch, a one notch change is an appropriate unit of analysis.

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

[12] We also estimate separate specifications that mirror the Blume, Lim, and MacKinlay (1998) and Baghai, Servaes, and Tamayo (2014) regressions, finding similar results. See the Online Appendix for tables.

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

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

attributable to the dispositional optimism of the analysts rating the firm, at least among firms of lower credit quality. We consider the interaction of the pricing effects with the transparency of rated firms more directly in Section 4.3.

We also test whether variation in the amount of information produced by the CRAs affects the degree to which analyst effects influence debt prices. We split the sample of firm-quarters depending on whether the firm was covered by a single CRA or by multiple CRAs. Not surprisingly, the standard deviation of the aggregate analyst effects is smaller when we average across multiple agencies (the standard deviation is 0.70 when a firm is covered by a single agency, but only 0.50 when it is covered by multiple agencies). Because each agency produces ratings as well as detailed reports about the firm and any rating actions, there is also more information available to the market about firms covered by multiple agencies. Thus, we predict that the market, conditional on analyst fixed effects of a given size, should be able to better filter the portion of ratings that reflects real information about fundamentals from the portion driven by the dispositional optimism of the analysts. In Columns 5 and 6 of Table 4, we test this prediction by estimating the regression specification from Column 2 separately on the two subsamples. We find that the residual portion of ratings does not have a different effect on prices in the two samples, suggesting that they do not differ in their inherent transparency conditional on the controls. However, analyst effects are significantly more predictive of credit spreads among firms for which the CRAs produce less information. Moreover, there is a significant difference between how the market reacts to the residual portion of ratings and the portion determined by analyst optimism only among the firms covered by multiple agencies. Thus, our analysis suggests a novel avenue through which increasing competition among CRAs can improve the quality of ratings and, in turn, the efficiency of corporate debt markets.

4.2. Robustness

We perform a variety of additional tests to assess the robustness of the evidence. To begin, we replicate the regressions from Table 4, using only the subsample of firm-quarters in which we do not observe a split rating. Our measures of analyst effects are orthogonal to firm fundamentals by construction. However, this additional restriction allows us to further limit the possibility that the pricing of analyst fixed effects reflects some form of information about the firm's fundamentals because we measure the analysts' dispositional optimism fully out-of-sample. The results are nearly identical. For example, among highly rated bonds, ratings affect prices, but analyst effects do not and the difference is statistically significant. However, both ratings and analyst effects matter among lower quality bonds. We also further restrict the estimation sample to only firms for which we never observe a split rating in the sample, again finding similar results (see Online Appendix Tables OA7 and OA8).

One concern with our identification strategy is that measurement error in our estimates of analysts' relative optimism could cause us to overestimate its effect on spreads. To see this, recall that the regression is equivalent to one in which we include the observed credit rating (measured without error) and our estimate of aggregate analyst effects. In this specification, the coefficient on aggregate analyst effects captures the difference between the market's response to the rating and the portion of ratings driven by analyst effects. If there is measurement error, then there is an errors-in-variables problem that attenuates the estimate of this coefficient. As a first step to assess the potential for measurement error to alter our conclusions, we reestimate the regression in Column 1 of Table 4 but progressively drop early sample years in which the fixed effects are measured less precisely (due to smaller backward-looking estimation samples). We find that the estimated difference between the effects of observed ratings and analyst effects on spreads initially increases (consistent with measurement error in early years) but then plateaus after we drop the first four sample years. The largest estimated difference is 20.5, implying a significant estimate of 29.7 basis points for the effect of a one notch change in analyst effects on spreads. Thus, our conclusions are unaffected. As an alternative, we exploit the skewness of the distribution of analyst effects (see Fig. 2) to account for measurement error following the approach of Erickson, Jiang, and Whited (2014). This approach requires a large cross section of data, making it inappropriate for our regressions on smaller data samples. Nevertheless, we find similar results when we reestimate the Column 1 specification using this approach and setting the maximum degree of the cumulant to 5.

We also construct an alternative measure of analysts' tendencies toward relative optimism that does not rely directly on the point estimates from Eq. (1). Instead of aggregating the fixed effects of the analysts covering each firm in a sample quarter, we calculate the difference between the raw numbers of relative pessimists and optimists using the estimated fixed effects (*Pessimism Count*). We then replicate the regression specifications in Table 4 with this variable in lieu of the aggregate analyst effect. We again find similar results. Firms covered by a group of analysts with less dispositional optimism face higher credit spreads on their bonds (see Online Appendix Table OA9). An advantage of this approach is that it limits the effect of measurement error in the analyst fixed effects on our estimates, because we need accurately measure only the signs of the effects. On the other hand, we throw away information on the magnitude of the effects that can provide sharper distinctions among analysts. Moreover, the estimate of the effect of dispositional optimism is no longer measured in the same units as the effect of ratings, making the two estimates difficult to compare with each other.

Another possible concern is that the persistence of both credit spreads and credit ratings could generate spurious results. A related concern is that ratings themselves respond to credit spreads, even though agencies explicitly state that their ratings do not consider prices. In our regressions, we measure ratings (and analyst effects) prior to credit spreads, but, if spreads are persistent, a channel from spreads to ratings could cloud the interpretation of our results. We reestimate the regressions allowing for an

Table 5
Credit spread and aggregate analyst effects: cross-sectional splits.
The table reports coefficient estimates from ordinary least squares regressions, splitting the sample at the median value of the variable reported at the top of the column. 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 Table A1. Firm controls include interest coverage ratio k1, k2, k3, and k4, total leverage, market value of equity (log), equity beta, equity volatility, expected default frequency, stock return (log), long-term leverage, profit margin, market-to-book, sales (log), tangibility, tax shields, carryforwards, and ratio of quarterly research and development expenditures to quarterly sales. Robust *t*-statistics double-clustered at the firm and quarter levels are reported in parentheses below the coefficients. Constant is included. Significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Variable	*Total assets*		*Firm age*		*Number of segments*		*Number of equity analysts*		*Equity analysts' earnings forecast dispersion*	
	Low (1)	High (2)	Low (3)	High (4)	Low (5)	High (6)	Low (7)	High (8)	Low (9)	High (10)
Adjusted credit rating	34.945***	29.528***	31.863***	27.116***	32.003***	31.778***	34.184***	29.609***	30.208***	34.901***
	(11.83)	(11.61)	(11.96)	(9.88)	(13.24)	(10.54)	(13.86)	(10.63)	(11.81)	(12.91)
Aggregate analyst effects	35.084***	19.735***	25.198***	24.906***	22.895***	29.672***	31.966***	21.272***	23.390***	35.142***
	(5.74)	(4.86)	(5.00)	(5.20)	(5.32)	(4.42)	(5.34)	(4.67)	(4.77)	(6.28)
Bond duration	1.238	1.546	−0.713	2.067*	−0.399	3.429**	0.458	1.676	0.831	0.904
	(0.80)	(1.07)	(−0.36)	(1.83)	(−0.29)	(2.40)	(0.30)	(1.29)	(0.66)	(0.62)
Callable bond dummy	24.438**	−3.245	9.803	−9.803	11.897	−11.981	18.459*	−15.333	1.340	3.132
	(2.07)	(−0.28)	(0.91)	(−0.92)	(1.09)	(−1.05)	(1.86)	(−1.25)	(0.11)	(0.34)
Bond age	0.009**	0.012***	0.009	0.011***	0.012***	0.006	0.010***	0.010***	0.012***	0.007**
	(2.19)	(3.35)	(1.36)	(3.11)	(3.41)	(1.30)	(2.58)	(2.78)	(3.20)	(2.08)
Time since last trade	−0.130	0.293	0.017	−0.172	−0.025	−0.144	−0.064	−0.142	−0.030	−0.110
	(−0.68)	(0.57)	(0.08)	(−0.62)	(−0.13)	(−0.42)	(−0.29)	(−0.65)	(−0.13)	(−0.49)
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.817	0.800	0.818	0.802	0.818	0.814	0.816	0.797	0.821	0.809
Number of observations	4,630	4,629	4,634	4,625	6,018	3,241	4,671	4,588	4,405	4,854
p-value for *t*-test that Low *Aggregate analyst effects* = High *Aggregate analyst effects*	0.036		0.967		0.396		0.155		0.114	
p-value for *t*-test that *Adjusted credit rating* = *Aggregate analyst effects*	0.980	0.010	0.123	0.580	0.020	0.701	0.672	0.026	0.133	0.957

unobserved time-invariant firm effect and clustering errors by firm and quarter. We again find similar results (see Online Appendix Table OA10).

As a final robustness check, we replicate our estimation strategy but measure the effects of ratings and dispositional optimism on corporate debt prices using a different sample and dependent variable. We consider the yields firms receive on newly issued bonds. We regress the offering yield to maturity on the aggregate analyst effect among the analysts covering the firm at the time of the issuance, the residual rating, and a set of firm-level controls and fixed effects (see Online Appendix Table OA1). We again find results that mirror Table 4. Residual ratings have a significant positive effect on yields. Firms with worse ratings receive worse debt terms. The portion of ratings driven by analyst effects also has a significant effect on yields. Though the market partially adjusts (i.e., this portion of ratings affects yields less than the residual piece), roughly 80% of the effect remains. Thus, firms that happen to have analysts who are systematically pessimistic experience higher costs of raising new debt.

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.

4.3. Firm opacity and the effect of analyst perspectives on debt prices

In Section 4.1, we find evidence that the sensitivity of debt prices to analysts' dispositional optimism varies depending on the information about the firm provided to market participants by the CRAs. Next, we test directly whether the ease with which market participants can evaluate the firm's fundamentals independent of CRA output affects the degree to which the market prices into credit spreads the relative optimism or pessimism of the analysts covering the firm. We consider five measure of transparency: firm size, firm age, diversification, the number of equity analysts covering the firm, and the dispersion in analyst earnings forecasts. We partition the sample of firms into two groups at the median of each of the measures. We then replicate the credit spread regression from Column 2 of Table 4 on each subsample. If it is harder for market participants to evaluate less transparent firms, then we expect the aggregate analyst effects to have a larger impact on credit spreads in the opaque subsamples (small firms, young firms, diversified firms, firms with less equity analyst coverage, and firms with more dispersion in equity analysts' earnings forecasts). Moreover, we expect to see a larger difference between the effect of the residual rating and the aggregate analyst effects in the subsamples of transparent firms, for which market participants can more easily filter the information content in ratings from the dispositional optimism of the analysts.

We present the results in Table 5. Generally, we find that the residual portion of ratings has a larger effect on prices in the subsamples of firms we identify as opaque, providing some empirical confirmation for the classification rules (the exception is the sample split based on the number of business segments operated by the firm). Turning to the analyst effects, we find large differences in the coefficient estimate on the analyst effects across transparent and opaque firms for all measures but firm age. The differences are large, ranging from 7 to 15 basis points, though they are typically not statistically significant. Finally, a larger difference exists between the coefficient estimates on aggregate analyst effects and the residual portion of ratings among transparent firms using all of the measures except firm age (consistent with greater adjustment by the market to the determinants of the rating). Here, the estimated adjustment is statistically significant in three of the four remaining cases and marginally insignificant (p-value = 0.133) in the fourth. Overall, the sample splits paint a consistent picture. The dispositional optimism of the rating analysts covering the firm has a greater effect on how the market prices the firm's debt when the firm is more opaque and, therefore, it is more difficult for market participants to filter information from noise.

4.4. Analyst effects and predictability of bond returns

Both the portion of ratings determined by the systematic optimism of the analysts covering a firm and the residual rating affect the pricing of its debt securities in the market. In the latter case, the pricing of rating information is likely to reflect the (time-varying firm-specific) information about fundamentals that analysts and the CRAs bring to the market. In the former case, the information channel is less obvious. Our empirical model explicitly separates the dispositional optimism of analysts from time-varying information about fundamentals. The value created by CRAs is likely to be precisely in the provision of the latter kind of information. Moreover, much weaker evidence exists that the market incorporates the relative optimism of analysts into debt prices in environments in which information is more available, even though it continues to incorporate the information provided by the residual portion of ratings. Next, we measure the persistence of the pricing effects. If the pricing of either portion of ratings reflects the incorporation of information by a semi-strong from efficient market, we expect that the pricing effects will persist over time. That is, we should not be able to predict future changes in prices based only on analyst optimism or residual ratings from prior quarters.

We measure returns on firms' outstanding bonds as the difference in the natural logarithms of future and current credit spreads. We consider future returns over one quarter and over one, two, and three years. To test our hypothesis, we regress future returns on the decomposed credit rating (*Aggregate Analyst Effects* and *Adjusted Credit Rating*). We include the full set of controls from Column 2 of Table 4. As in Table 4, we cluster standard errors both by firm and quarter to account for cross-sectional and time series dependence in the errors.

We report the results in Table 6. [15] We find that bond characteristics affect changes in spreads in the expected directions. Older bonds, callable bonds, and bonds with higher duration have larger declines in spreads over time. Otherwise, we find few robust predictors of future changes in spreads. Notably, the residual portion of ratings does not reliably predict future changes in credit spreads, even though it is a strong predictor of current spreads. This result is consistent with the residual portion of ratings conveying information about fundamentals that the market accurately incorporates into prices. However, we find that the portion of ratings driven by systematic analyst optimism significantly predicts returns at all four horizons considered. Firms covered by analysts who are systematically pessimistic significantly outperform firms covered by analysts who are systematically optimistic (i.e., credit spreads decline). In this regression, measurement error in the aggregate analyst effects would imply that our results understate the true effect.[16] Moreover, residual ratings are more persistent than the aggregate analyst effects, suggesting that the results are not likely to be an artifact of including a persistent regressor.

A potential concern is that credit spreads exhibit mean reversion and dispositional optimism predicts future changes in spreads only because it also predicts higher current spreads. However, it is unclear why analyst effects would pick up this mean reversion and not the residual rating, even though residual ratings are an even stronger predictor of current spreads. Moreover, predictable mean reversion in prices is itself puzzling if markets are efficient. Thus, it would be economically interesting if relative analyst optimism partially explains why there is mean reversion in spreads. Nevertheless, we add a control for the difference between the natural logarithm of the lagged credit spread and the natural logarithm of the backward-looking three-year rolling average of spreads. We use the rolling average so that the regression remains predictive. We report the results in Columns 5–8 of Table 6. We do find some evidence that the credit spreads on firms' bonds revert toward the historical mean. However, we continue to find significant predictive power of systematic analyst optimism for changes in spreads.[17]

Overall, the evidence suggests that the pricing of adjusted ratings improves the efficiency of corporate debt markets though the pricing of analyst effects likely does not. Instead, the debt of firms with systematically optimistic analysts has spreads that are too low. Thus, our evidence provides a rationale for companies to target debt

[15] See Online Appendix Table OA11 for full estimates, including controls we omit from Table 6 for brevity.

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

[17] Because our independent variable of interest is a function of analyst fixed effects, it is inherently persistent. This creates a potential concern in interpreting the results in Columns 5–8. The analyst effects will not only affect current spreads, but also be correlated with the lag of spreads because they will be correlated with the lagged analyst effects. Thus, the control we include to capture mean reversion in spreads may be a “bad control” in the language of Angrist and Pischke (2009) even though it is predetermined. Including the difference between the lag of spreads and the rolling average of spreads mitigates this concern relative to including only the lag of spreads because analyst effects should be less likely to determine shocks to spreads.

Table 6
Future bond returns and aggregate analyst effects.

The table reports coefficient estimates from ordinary least squares regressions. The dependent variable is the forward change in the natural logarithm of *Credit spread* (the firm-level volume-weighted average of the credit spreads of all outstanding bonds issued by the firm), measured over the interval indicated in the column heading (in quarters). S_{av} is the firm's average credit spread from quarter $t-13$ to $t-2$. All variables are defined in Table A1. Standard firm controls are long-term leverage, profit margin, market-to-book, sales (log), tangibility, tax shields, carryforwards, and ratio of quarterly research and development expenditures to quarterly sales. Coefficient estimates are reported in the Online Appendix. Robust *t*-statistics double-clustered at the firm and quarter levels are reported in parentheses below the coefficients. Constant is included. Significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Variable	$\ln(S_{t+1})-\ln(S_t)$ (1)	$\ln(S_{t+4})-\ln(S_t)$ (2)	$\ln(S_{t+8})-\ln(S_t)$ (3)	$\ln(S_{t+12})-\ln(S_t)$ (4)	$\ln(S_{t+1})-\ln(S_t)$ (5)	$\ln(S_{t+4})-\ln(S_t)$ (6)	$\ln(S_{t+8})-\ln(S_t)$ (7)	$\ln(S_{t+12})-\ln(S_t)$ (8)
Adjusted credit rating	−0.004	−0.012*	−0.016	−0.025**	−0.004	−0.010	−0.014	−0.027**
	(−1.34)	(−1.78)	(−1.59)	(−2.20)	(−1.38)	(−1.51)	(−1.32)	(−2.31)
Aggregate analyst effects	−0.009**	−0.025**	−0.043***	−0.046**	−0.011**	−0.025**	−0.041**	−0.039
	(−2.09)	(−2.18)	(−2.60)	(−2.10)	(−2.46)	(−1.99)	(−2.46)	(−1.63)
$\ln(S_{t-1})-\ln(S_{av})$					−0.021	−0.066**	−0.172***	−0.130**
					(−0.92)	(−2.13)	(−3.43)	(−2.55)
Bond duration	−0.011***	−0.018***	−0.030***	−0.040***	−0.012***	−0.020***	−0.032***	−0.042***
	(−3.28)	(−3.99)	(−4.29)	(−6.09)	(−3.26)	(−3.94)	(−4.25)	(−5.32)
Callable bond dummy	−0.029	−0.088***	−0.116***	−0.155***	−0.021	−0.073***	−0.118***	−0.164***
	(−1.50)	(−3.83)	(−3.62)	(−3.47)	(−1.33)	(−3.24)	(−3.56)	(−3.43)
Bond age	−0.000***	−0.000***	−0.000***	−0.000***	−0.000***	−0.000**	−0.000**	−0.000***
	(−2.65)	(−3.40)	(−3.79)	(−4.03)	(−2.80)	(−2.32)	(−2.46)	(−3.34)
Time since last trade	0.000	0.002	−0.000	−0.001	0.001	0.002*	0.001	0.000
	(0.24)	(1.57)	(−0.35)	(−1.16)	(1.38)	(1.83)	(0.70)	(0.18)
Interest coverage k1	0.002	0.016**	0.025*	0.029	0.001	0.019**	0.032**	0.028
	(0.44)	(2.19)	(1.83)	(1.64)	(0.25)	(2.15)	(2.11)	(1.61)
Interest coverage k2	0.001	−0.003	−0.007	−0.006	−0.001	−0.003	−0.006	−0.006
	(0.33)	(−0.87)	(−0.94)	(−0.61)	(−0.28)	(−0.54)	(−0.65)	(−0.54)
Interest coverage k3	−0.001	0.001	0.002	−0.002	−0.000	0.000	0.001	−0.001
	(−0.60)	(0.33)	(0.36)	(−0.30)	(−0.06)	(0.08)	(0.22)	(−0.06)
Interest coverage k4	−0.000	−0.002**	−0.002	−0.000	−0.001	−0.002	−0.000	0.000
	(−0.30)	(−2.00)	(−1.15)	(−0.08)	(−0.51)	(−1.34)	(−0.18)	(0.02)
Total leverage	0.072	0.158	0.146	0.312	0.080*	0.151	0.219	0.315
	(1.60)	(1.61)	(0.79)	(1.27)	(1.95)	(1.48)	(1.13)	(1.37)
Market value of equity (log)	0.001	0.001	0.011	0.002	0.007	0.006	0.021	0.011
	(0.19)	(0.11)	(0.53)	(0.06)	(0.88)	(0.44)	(0.91)	(0.34)
Equity beta	−0.002	0.005	0.021	0.081**	0.002	0.001	0.021	0.049
	(−0.20)	(0.26)	(0.88)	(2.33)	(0.21)	(0.04)	(0.80)	(1.49)
Equity volatility	−0.059	−0.052	−0.090	−0.431**	−0.063*	0.004	−0.021	0.014
	(−1.46)	(−0.64)	(−0.81)	(−1.97)	(−1.73)	(0.04)	(−0.17)	(0.07)
Expected default frequency	0.023	0.041	0.064	−0.171	0.033	0.053	0.082	−0.199*
	(0.57)	(0.48)	(0.72)	(−1.48)	(0.78)	(0.62)	(0.95)	(−1.70)
Stock return (log)	0.010	−0.023	−0.060	−0.125**	0.007	−0.032	−0.075*	−0.131**
	(0.80)	(−0.96)	(−1.53)	(−2.33)	(0.52)	(−1.30)	(−1.80)	(−2.11)
Standard firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.255	0.593	0.680	0.639	0.271	0.617	0.704	0.646
Number of observations	8,358	6,826	5,312	4,087	7,327	5,914	4,538	3,419

ratings (as in Hovakimian, Kayhan, and Titman, 2009 and Kisgen, 2009). In Online Appendix Table OA1, we present complementary evidence that firms respond to these incentives: systematic analyst pessimism predicts less use of debt conditional on raising external finance and smaller capital expenditures and sales growth.

5. Which analysts are optimistic?

As a final step, we investigate whether dispositional optimism correlates with observable analyst characteristics. We test whether analyst optimism changes with time-varying analyst characteristics or the dynamics of analysts' relationships with their agencies and covered firms. Our goal is to provide as clear a picture as possible of the profile of the analysts who are likely to provide high-quality ratings.

To conduct this analysis, we supplement our data with information on analysts' backgrounds from Web searches (see Section 2). 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. To begin, we adapt Eq. (1) to test whether differences in these traits can account for differences in relative optimism across analysts. In place of $\boldsymbol{\gamma}_{analyst}$, we use our measures of analyst traits. We also include a control variable for the number of years the agency has covered the firm, as prior research suggests that long relationships with rating agencies can lead to more favorable ratings (Mahlmann, 2011). Because we often observe multiple analysts covering a particular

Table 7
Optimism and accuracy.
The table reports coefficient estimates from ordinary least squares regressions. The dependent variable is displayed at the top of each column. *Optimism* is the product of −1 times the difference in each firm-quarter between the analyst's rating of the firm and the average rating of the other analysts covering the firm. *Accuracy* is the product of −1 times *Optimism* and the forward change in credit spreads over a three-year horizon, measured starting at the end of the quarter. All variables are defined in Table A1. Robust *t*-statistics clustered at the firm-quarter level are reported in parentheses below the coefficients. Constant is included. Significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Variable	*Optimism*		*Accuracy*	
	(1)	(2)	(3)	(4)
MBA	−0.149***		43.708**	
	(−3.21)		(2.10)	
Top 5 MBA		−0.344***		128.380***
		(−4.03)		(2.96)
Non top 5 MBA		−0.131***		38.55*
		(−2.82)		(1.92)
Analyst age	−0.003	−0.004	−0.781	−0.492
	(−1.13)	(−1.52)	(−0.55)	(−0.35)
Female	−0.320***	−0.315***	34.657	37.688
	(−5.98)	(−5.90)	(1.32)	(1.43)
Analyst tenure covering the firm	0.108***	0.105***	−13.326**	−12.388**
	(7.32)	(7.11)	(−2.22)	(−2.08)
Agency tenure covering the firm	0.007	0.006	−3.499*	−3.611*
	(1.09)	(1.00)	(−1.72)	(−1.78)
Analyst tenure covering the industry	−0.001	0.004	7.377	8.096
	(−0.08)	(0.30)	(1.30)	(1.42)
Analyst tenure in the agency	−0.029***	−0.030***	4.459**	3.875*
	(−6.70)	(−6.84)	(2.29)	(1.96)
Number of firms currently covered	−0.007***	−0.007***	4.097***	4.281***
	(−2.70)	(−2.69)	(3.81)	(3.97)
Agency = Moody's	−0.106**	−0.115***	−45.394**	−36.267*
	(−2.41)	(−2.60)	(−2.52)	(−1.94)
Agency = Standard & Poor's	0.231***	0.213***	−17.963	−8.568
	(5.94)	(5.39)	(−1.07)	(−0.50)
Firm-quarter fixed effects	Yes	Yes	Yes	Yes
R^2	0.069	0.071	0.020	0.023
Number of observations	22,827	22,827	6,683	6,683

firm-quarter for the same agency, we average characteristics across analysts within each agency-firm-quarter before running our regressions. Thus, our data retain the same panel structure as in Section 3.2. 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 because we observe varying numbers of analysts covering each agency-firm-quarter. Thus, the group weightings using the two approaches would differ. For robustness, we conduct our analysis both ways, finding that no conclusions are altered by this choice. We continue to include firm-quarter fixed effects. Thus, we measure the effect of analyst traits after accounting for potentially nonrandom 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 agency fixed effects. We cluster standard errors at the firm-quarter level to account for repetition across agencies.

We measure 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. We choose this approach, instead of 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 because we observe at most three distinct ratings per firm-quarter. Because worse ratings are associated with higher numbers on the numerical scale, 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. 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. An alternative to measuring relative optimism quarter by quarter would be to use the model-estimated fixed effects from Section 3.2 as a dependent variable. Here, we observe analyst traits (and analyst-agency or analyst-firm traits) that vary at the agency-firm-quarter level. Thus, it is not necessary to consider fixed effects to separate the effects of analysts from time-varying firm fundamentals or agency effects. Using fixed effects as the dependent variable would also make it impossible to estimate the effect of changes in observable analyst traits on analyst beliefs.

We present the results in Column 1 of Table 7. We find little evidence that the agency's tenure covering the firm affects the relative optimism of its analysts. How-

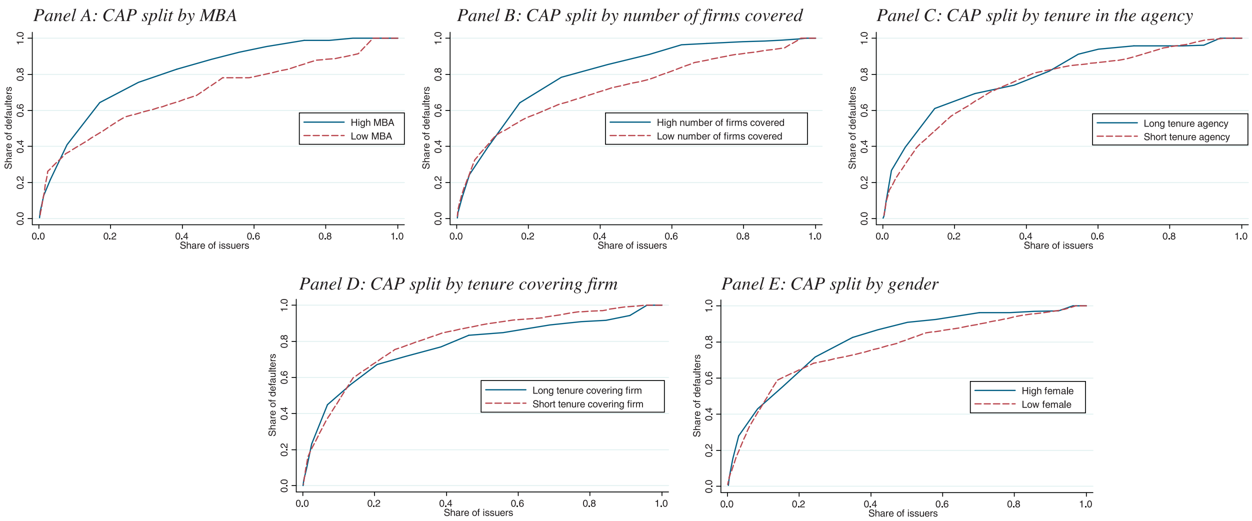

Fig. 3. Cumulative accuracy profiles (CAP). The plots show three-year cumulative accuracy profiles. The cumulative accuracy profile is constructed by plotting the percentage of defaults within a three-year horizon accounted for by firms with the same or lower rating, relative to the percentage of all firms with the same or lower rating. Quarterly cumulative accuracy profiles are averaged over time using the total number of defaults in each quarter as weights. Panel A shows the cumulative accuracy profile split between firms covered by analysts with and without a master of business administration (MBA) degree. Panel B splits the sample between firms covered by analysts with high (above median) and low (below median) numbers of firms covered. Panel C splits the sample between firms covered by analysts with long (above median) and short (below median) tenure in the agency. Panel D splits the sample between firms covered by analysts with long (top quartile) and short (bottom quartile) tenure covering the firm. Panel E splits the sample between firms covered by at least one female analyst and firms covered by all male rating analysts.

ever, we do find that several analyst traits significantly correlate with relative optimism. MBAs produce significantly less optimistic ratings than their peers who simultaneously cover the same firms. Also, analysts with a longer tenure in their agencies and analysts who cover more firms provide significantly less optimistic ratings than their peers. Together, these effects suggest that relative optimism decreases with analyst experience (or skill). To sharpen this interpretation of the MBA effect, we reestimate the model, splitting the MBA proxy into indicators for analysts who received MBAs from the top five business schools and from business schools outside the top five.[18] We present the results in Column 2. Consistent with the skill interpretation, we find that an MBA from a top five school has an even stronger negative correlation with relative optimism than an MBA from other business schools.

On the other hand, analysts with a longer tenure covering a firm produce more optimistic ratings than their peers covering the same firm. A single year covering the firm is associated with an increase in ratings of roughly 10% of a notch. Roughly 10 years would increase relative optimism by one standard deviation. One possible explanation is the deterioration of career concern incentives as analyst tenure covering the firm increases (Holmstrom, 1999), though in this case we could expect similar effects as analyst tenure in the agency or analyst tenure covering the industry increase (which we do not find). Because 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. Cornaggia, Cornaggia, and Xia (forthcoming) find, for example, that analysts who move from rating agencies to the firms that they rate tend to inflate bond ratings prior to the move. Similarly, equity analysts are more likely to issue buy recommendations prior to being hired by firms they cover (Cohen, Frazzini, and Malloy, 2012). Relationships can be associated with greater leniency even in the absence of an ulterior motive, such as gaining employment at a rated firm. Moreover, longer relationships can create an "illusion of knowledge" (Oskamp, 1965), leading to a decline in rating quality, even for analysts without conflicts of interest.

Thus far our discussion presupposes that relatively optimistic ratings are of lower quality. Next, we test whether the analysts who provide relatively optimistic ratings indeed prove to be less accurate over time. We replicate Moody's methodology to assess relative ratings accuracy by comparing the cumulative distribution of defaults over ratings across groups of analysts (e.g., analysts with an MBA and analysts without an MBA). Within each group and for each rated firm and quarter, we compute the fraction of firms with a worse rating. We then compute the fraction of observed defaults over the following n years that occur among firms with worse ratings, considering (separately) n ranging from 1 to 5. Each resulting pair is a point on the cumulative accuracy profile (CAP curve). Thus, we can think of the curve as a cumulative distribution function. By construction, it takes a value of zero when the fraction of firms with worse ratings is zero (i.e., for the lowest rated firm), and it equals one for the highest rated firm. Moody's deems a curve that increases to one more quickly to represent greater accuracy, as it indicates that defaults are concentrated among the firms with the worst ratings. The 45-

[18] We use the 2011 *Economist* rankings to identify top-five business schools.

degree line is the CAP associated with randomly assigned ratings. Thus, the closer the curve is to the 45-degree line, the less accurate ratings are. To summarize the curve in a single number, it defines the accuracy ratio as the ratio of the area between the CAP curve and the 45-degree line to the maximum possible area above the 45-degree line. As the CAP curve shifts to the left, the accuracy ratio also increases.[19]

In Fig. 3, we present the CAP curves over a three-year horizon for subsamples of analysts defined using the characteristics that have significant correlations with relative optimism in Table 7. For each characteristic, we split the sample of analysts into two groups and calculate separate CAP curves for each subsample. We average the CAP curves across quarters using the number of defaults as weights. In general, we find that groups of analysts who tend to provide more relatively optimistic ratings also provide less accurate ratings. Analysts who have an MBA, who cover more firms than the sample median, and who have longer tenure in their agency than the sample median appear to have CAP curves that are shifted to the left relative to the complementary sets of analysts. Analysts who have a longer tenure covering a firm have CAP curves that are shifted farther to the right compared with analysts with shorter tenures. In this case, we compare analysts in the top and bottom quartiles of the distribution. Although we find a similar pattern when we split the sample at the median, the reduction in accuracy is strongest among analysts in the highest quartile of the distribution. In Table 8, we present the corresponding accuracy ratios and test the significance of the cross-sample differences. We compute standard errors using the time series variation in the accuracy ratios within each subsample, again weighted by the number of defaults per quarter. These tests have relatively low power because each sample consists on average of 45 quarters. At a three-year horizon, we find that all the cross-sample differences are statistically significant except the difference between analysts with a long and short tenure in their agency.[20] We also present accuracy ratios and cross-sample tests for four different horizons: one year, two years, four years, and five years. The cross-group differences are similar in magnitude regardless of the chosen horizon. Statistically, the differences between analysts with and without MBAs and between analysts with high and low coverage levels are the most robust.

A drawback of the Moody's methodology is that CAP curves and accuracy ratios cannot be defined at the individual analyst level because individual analysts rarely produce enough ratings to observe a distribution with full support or a nontrivial number of defaults. To assess differences in accuracy in a multivariate regression framework, we construct an alternative measure of accuracy. In firm quarter t, we measure accuracy by multiplying -1 by relative optimism by the forward change in credit

[19] See Cantor and Mann (2003) for more details on how Moody's measures the performance of corporate bond ratings.

[20] Here we also consider quartiles of the distribution to see if the pattern mimics what we find for tenure covering the firm. However, we do not see bigger differences between groups if we look at the top and bottom quartiles.

Table 8
Accuracy ratio.
The table reports accuracy ratios at various horizons, ranging from one to five years. Following Cantor and Mann (2003), the accuracy ratio is defined as the ratio between the size of two areas. The first area (in the numerator) is the space between the cumulative accuracy profile and the uniform cumulative accuracy profile under random ratings. The second area (in the denominator) is the maximum possible area above the uniform cumulative accuracy profile under random ratings. The accuracy ratio is first computed for each quarter cohort and then averaged using as weights the number of defaults in the quarter. In Column 1 we present the accuracy ratio for the full sample. In Columns 2–5 we split the sample between firms covered by rating analysts with a master of business administration (MBA) degree or not. In Columns 6–9 we split the sample between firms followed by rating analysts covering a large number of firms (above median) and firms followed by rating analysts covering a small number of firms (below median). In Columns 10–13 we split the sample between firms covered by analysts with long tenure in the agency (above median) and firms covered by rating analysts with short tenure in the agency (below median). In Columns 14–17 we split the sample between firms covered by analysts with long tenure covering the firm (top quartile) and firms covered by rating analysts with short tenure covering the firm (bottom quartile). In Columns 18–21 we split the sample between firms covered by at least one female analyst and firms covered by all male rating analysts.

Accuracy ratio	Full	*MBA*				*Number of firms currently covered*				*Tenure in agency*				*Tenure covering firm*				*Female analyst*			
	sample (1)	Yes (2)	No (3)	Difference (4)	p-value (5)	High (6)	Low (7)	Difference (8)	p-value (9)	Long (10)	Short (11)	Difference (12)	p-value (13)	Long (14)	Short (15)	Difference (16)	p-value (17)	Yes (18)	No (19)	Difference (20)	p-value (21)
One-year horizon	0.616	0.655	0.434	0.221	0.000	0.624	0.539	0.085	0.100	0.609	0.561	0.048	0.413	0.522	0.660	-0.139	0.021	0.620	0.585	0.035	0.417
Two-year horizon	0.570	0.603	0.397	0.206	0.000	0.588	0.440	0.148	0.001	0.558	0.491	0.067	0.138	0.508	0.578	-0.070	0.118	0.574	0.512	0.062	0.098
Three-year horizon	0.545	0.570	0.371	0.199	0.000	0.555	0.406	0.149	0.000	0.519	0.474	0.045	0.197	0.482	0.556	-0.074	0.072	0.552	0.476	0.076	0.011
Four-year horizon	0.517	0.535	0.345	0.190	0.000	0.533	0.391	0.141	0.000	0.499	0.462	0.037	0.508	0.476	0.534	-0.058	0.136	0.545	0.454	0.092	0.000
Five-year horizon	0.496	0.510	0.318	0.191	0.000	0.506	0.384	0.122	0.000	0.461	0.449	0.012	0.689	0.465	0.509	-0.044	0.176	0.544	0.428	0.117	0.000

Table 9

Accuracy: cross-sectional analysis.

The table reports coefficient estimates from ordinary least squares regressions splitting the sample at the median value of the variable reported at the top of the column. The dependent variable is *Accuracy*, the product of −1 times *Optimism* and the forward change in credit spreads over three years, measured starting at the end of the quarter. All variables are defined in Table A1. Other controls include analyst age, female, agency tenure covering the firm, analyst tenure covering the industry, analyst tenure in the agency, and number of firms currently covered. Coefficient estimates for the full set of controls are reported in the Online Appendix. Robust t-statistics clustered at the firm-quarter level are reported in parentheses below the coefficients. For each split sample, we also report the two-tailed p-value of a two-sample t-test for equality of the coefficient estimates across the two subsamples. Constant is included. Significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Variable	*Total assets*		*Firm age*		*Number of segments*		*Number of equity analysts*		*Equity analysts' earnings forecast dispersion*	
	Low	High	Low	High	Low	High	Low	High	Low	High
MBA	109.603***	0.170	89.882**	4.464	−22.836	68.911	94.446**	40.541	−10.170	129.837***
	(2.83)	(0.01)	(2.26)	(0.18)	(−0.68)	(1.64)	(2.56)	(1.41)	(−0.37)	(3.60)
	0.011**		0.068*		0.088*		0.249		0.002***	
Analyst tenure covering firm	−12.791	−1.787	−26.701***	1.395	−0.369	−18.960*	−16.415	−8.166	−1.011	−22.150*
	(−1.07)	(−0.30)	(−2.61)	(0.20)	(−0.03)	(−1.94)	(−1.37)	(−1.43)	(−0.16)	(−1.85)
	0.412		0.022**		0.202		0.534		0.116	
Firm-quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agency fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.054	0.025	0.040	0.037	0.035	0.051	0.040	0.051	0.032	0.058
Number of observations	3,336	3,334	3,342	3,341	2,673	2,575	3,162	3,148	3,056	3,066

spreads measured starting at time t and continuing for three years.[21] The change in credit spreads captures realized changes in the issuer's credit quality over time, and the optimism measure captures the analyst's prediction. Here, increases in credit spreads substitute for defaults as a measure of negative credit events. But, the overall intuition is similar. We measure analysts who provide lower ratings to firms that subsequently experience bad shocks to be more accurate.

In Columns 3 and 4 of Table 7, we report the results from estimating the regressions in Columns 1 and 2 using our measure of accuracy as a dependent variable. The results confirm the patterns in Fig. 3 and Table 8. We again find that analysts who hold MBA degrees (and particularly MBAs from the top five schools), analysts who cover more firms, and analysts who have longer tenure in their agency produce more accurate ratings, even in a multivariate setting controlling for firm-quarter and agency fixed effects. Similarly, we find that analysts with a longer tenure covering a firm produce significantly less accurate ratings. The difference in accuracy between analysts with and without an MBA is roughly 30% of a standard deviation, similar to the effect of covering roughly 10.5 firms or spending 10 years in the agency. The decrease in accuracy associated with 3.3 years following a firm would roughly offset the benefit provided by an MBA.

In Tables 7 and 8, we also find evidence that female analysts provide higher quality ratings. We find evidence of less relative optimism and better ratings accuracy (particularly when measured using accuracy ratios). We devote less attention to this effect because it is trickier to interpret. Women who choose to become credit analysts, for example, could be higher skilled on average than men who make the same choice, although women are significantly less likely to have MBAs (Table 2, Panel C). Alternatively, women could be less prone to certain behavioral biases that can lead to inflated ratings (Lundeberg, Fox, and Puncohar, 1994), or they could have preferences that are better aligned with creditors' interests.

As a final step, we test whether the effects of analyst traits on ratings are more pronounced in some firms than in others. We consider the same five proxies for transparency that we used in our analysis of credit spreads. We split the sample at the median of each characteristic and reestimate the accuracy regression from Column 3 of Table 7 separately on each subsample.[22] We report the results in Table 9. In the table, we include a single proxy for analyst skill (MBA) and our measure of tenure covering the firm. (We provide complete estimates in the Online Appendix.) We find for every sample split that the increased accuracy of analysts with an MBA is most pronounced among opaque firms. 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. We find that the decline in relative accuracy among such analysts is concentrated in the information-sensitive firms, though the results are statistically weaker. Our analysis suggests that the lack of transparency in such firms allows for more analyst discretion or subjectivity in ratings.

Overall, we find evidence of multiple channels through which analysts exert an effect on credit ratings. Analysts with greater expertise appear to issue higher quality 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

[21] 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.

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

effects are most pronounced in firms likely to face constraints in accessing external capital, magnifying the real impact of analyst differences. A caveat to our results is that a number of unobserved traits likely also explain portions of the analyst effects we uncover in Section 3, particularly given the limited set of measurable traits available for our analysis.

6. Conclusion

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

We find that these systematic analyst effects, though orthogonal to firm fundamentals, carry through to credit spreads on the firm's existing debt. They also affect the cost of raising new debt capital. Firms that are covered by analysts who are systematically more pessimistic than their peers have debt with higher spreads and obtain worse terms on debt issues. The effects appear to be concentrated in firms that are difficult for market participants to evaluate independently from the output of the CRAs. Moreover, systematic analyst optimism in ratings predicts future increases in spreads even though observed ratings themselves do not predict future changes in spreads, suggesting that the pricing effects are unwarranted.

We also link individual analyst traits to the analyst's effect on ratings. We find evidence of at least two distinct patterns in the quality of ratings produced by different analysts. First, analysts with greater expertise or experience (measured by MBA degrees, greater breadth of coverage, and longer tenure covering the industry) appear to produce higher quality ratings. We find evidence that analyst skill is associated with lower relative optimism in ratings and greater accuracy over a three-year horizon. Second, ratings quality deteriorates as analyst tenure covering the firm in-

Table A1
Variable definitions.
This table provides detailed definitions of the variables we use in our analysis along with information on the source of each data item.

Variable name	Definition	Data source
Accuracy	Product of −1 times *Optimism* and the forward change in credit spreads over a three-year horizon, measured starting at the end of the quarter.	Thomson, Trade Reporting and Compliance Engine (TRACE)
Accuracy ratio	Ratio of the area between the *Cumulative accuracy profile* curve and the 45-degree line to the maximum possible area above the 45-degree line.	Moody's Investors Service, Thomson
Agency tenure covering the firm	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	Sum of the dummy coefficients γ from the equation for all analysts covering each firm j in sector s during quarter t for each agency i. To ensure that we measure the reaction only to information that was available to market participants at the time, we construct a backward-looking estimate of the fixed analyst effects on ratings by running the equation for each quarter including only the data up to that quarter.	Thomson
Analyst age	Minimum of the first year of employment minus 22 years and the first year of college minus 18 years.	LinkedIn, Standard & Poor's, Moody's, and Fitch websites
Analyst tenure covering the firm	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	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	Number of years between the date an analyst starts working for the rating agency and the date on which the quarter ends.	LinkedIn, Standard & Poor's, Moody's, and Fitch websites
Bond age	Firm-level volume-weighted average of the number of days since the debt issuance of all outstanding bonds issued by the firm, measured at the end of each given quarter.	TRACE, Mergent Fixed Income Securities Database (FISD)
Bond duration	Firm-level volume-weighted average of the duration of all outstanding bonds issued by the firm, measured at the end of each given quarter.	TRACE, Mergent FISD
Callable bond	Firm-level volume-weighted average of the bond callable dummies, in which each dummy is equal to one if the bond is callable, measured at the end of each given quarter.	TRACE, Mergent FISD
Carryforwards	Ratio between tax loss carryforwards and total assets, winsorized at the 1% and 99% level. The carryforward variable is set to zero when missing in Compustat.	Compustat
Credit rating	A number from 1 to 21 indicating the credit rating of a company at the end of the quarter. Table 1 shows the rating correspondence across agencies.	Thomson
Credit rating (adjusted)	Difference between the credit rating of a firm and the aggregate analyst effect.	Thomson

(*continued on next page*)

Table A1 (*continued*)

Variable name	Definition	Data source
Credit spread	Firm-level volume-weighted average of the credit spreads of all outstanding bonds issued by the firm. Credit spreads for each issue are calculated by subtracting from the bond's yield to maturity the yield resulting from a linear interpolation of the CRSP treasury yields (among the periods 1, 2, 5, 7, 10, 20, and 30 years) that have the next lower and higher duration relative to the bond's duration. For bonds with a duration of more than 30 years, we use the 30-year treasury yield. The spread is measured in basis points at the end of each given quarter.	TRACE, Mergent FISD
Cumulative accuracy profile	Constructed by plotting, for each rating category, the proportion of defaults accounted for by firms with the same or a lower rating against the proportion of all firms with the same or a lower rating.	Moody's, Thomson
Debt retirement spike	Dummy variable equal to one if total debt decreases during a given quarter by more than 5% of total assets at the beginning of the quarter.	Compustat
Debt issuance spike	Dummy variable equal to one if total debt increases during a given quarter by more than 5% of total assets at the beginning of the quarter.	Compustat
Expected default frequency	Expected default frequency estimated following Bharath and Shumway (2008): $EDF = \phi[-(\ln[(E+F)/F]+\mu-0.5\sigma^2)/\sigma]$, where E is the market value of equity; F is the face value of debt (computed as short term debt plus one half long term debt); μ is the prior 12-month stock return; σ is asset volatility (estimated as $\sigma = (E/(E+F))\sigma_e + (F/(E+F))(0.05+0.25\sigma_e)$, where σ_e is the annualized volatility of daily stock returns over the prior 12 months); and ϕ is the standard normal cumulative distribution function.	Compustat, CRSP
Equity analysts' earnings forecast dispersion	Standard deviation of the earnings forecasts of equity analysts who cover the firm six months prior to the annual earnings announcement, standardized by the mean earnings forecast.	Institutional Brokers' Estimate System (I/B/E/S)
Equity beta	Beta coefficient of daily stock returns relative to the value-weighted CRSP market portfolio for the previous fiscal year.	CRSP
Equity volatility	Annualized average daily stock return volatility over the previous 12 months. A minimum of 21 trading days are required for volatility to be computed.	Compustat
Female	A dummy variable equal to one if the analyst's gender is female.	Standard & Poor's, Moody's, and Fitch websites
Firm age	Difference in years between the end of the fiscal quarter date and the first time the firm appears in Compustat.	Thomson, Compustat
Interest coverage k1, k2, k3, k4	Spline variables based on the interest coverage ratio, constructed as in Blume, Lim, and MacKinlay (1998).	Compustat, CRSP
Leverage decrease spike	Dummy variable equal to one if *Debt retirement spike* = 1 or if *Net equity issuance spike* = 1.	Compustat
Leverage increase spike	Dummy variable equal to one if *Debt issuance spike* = 1 or if *Net equity repurchases spike* = 1.	Compustat
Long-term leverage	Long-term debt divided by total assets, winsorized at the 1% and 99% level.	Compustat
Market-to-book	Ratio between the market value of assets and the book value of assets. The market value of assets is the total book value of assets plus the market value of equity (number of shares outstanding × stock price) minus the book value of equity. The ratio is winsorized at the 1% and 99% level.	Compustat
Market value of equity (log)	Natural log of one plus the product of the stock price and the number of shares outstanding.	Compustat
MBA	Dummy variable equal to one if the individual has a master of business administration (MBA) degree.	LinkedIn, Standard & Poor's, Moody's, and Fitch websites
MBA non top 5	Dummy variable equal to one if the individual has a master of business administration (MBA) degree not from one of the top five MBA programs according to the 2011 *Economist* ranking (University of Chicago, Tuck School of Business, Haas School of Business, University of Virginia, and IESE Business School).	LinkedIn, Standard & Poor's, Moody's, and Fitch websites
MBA top 5	Dummy variable equal to one if the individual has a Master of Business Administration degree from one of the top five MBA programs according to the 2011 *Economist* ranking (University of Chicago, Tuck School of Business, Haas School of Business, University of Virginia, and IESE Business School).	LinkedIn, Standard & Poor's, Moody's, and Fitch websites
Net equity issuance spike	Dummy variable equal to one if net equity issuance (sale of common and preferred stock minus purchase of common and preferred stock) in a given quarter is greater than 5% of total assets at the beginning of the quarter. Equity issued and equity repurchased are set to zero when missing in Compustat.	Compustat
Net equity repurchases spike	Dummy variable equal to one if net equity repurchases (purchase of common and preferred stock minus sale of common and preferred stock) in a given quarter are greater than 1.25% of total assets at the beginning of the quarter. Equity issued and equity repurchased are set to zero when missing in Compustat.	Compustat

(*continued on next page*)

Table A1 (*continued*)

Variable name	Definition	Data source
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	Number of companies covered by an analyst at the end of the quarter.	Thomson, Standard & Poor's, Moody's, and Fitch websites
Number of segments	Number of business segments operating in distinct Fama-French 49 industry codes.	Compustat segments file
Offering yield to maturity	Dollar-weighted average of the offering yield to maturity of all bonds issued in a quarter by a given firm.	Securities Data Company
Optimism	Difference in each firm-quarter between the analyst's rating of the firm and the average rating of the other analysts covering the firm, multiplied by −1.	Thomson
Outlook negative	Dummy variable equal to one if the long-term outlook for the firm at the end of the fiscal quarter is negative.	Thomson
Outlook positive	Dummy variable equal to one if the long-term outlook for the firm at the end of the fiscal quarter is positive.	Thomson
Outlook stable	Dummy variable equal to one if the long-term outlook for the firm at the end of the fiscal quarter is stable.	Thomson
Pessimism count	Difference between the raw numbers of relative pessimistic and optimistic analysts using the estimated analyst fixed effects.	Thomson
Profit margin	Annualized quarterly profit divided by quarterly sales, winsorized at the 1% and 99% level.	Compustat
R&D/Sales	Ratio between quarterly research and development (R&D) expenditures and quarterly sales, winsorized at the 1% and 99% level. R&D is set to 0 when missing in Compustat.	Compustat
Rating dispersion	Absolute value of the difference in each firm-quarter between the analyst's rating of the firm and the average rating of the other analysts covering the firm.	Thomson
Sales (log)	Natural log of one plus total quarterly sales.	Compustat
Sales growth	Ratio of the change in sales during a given quarter and the sales at the beginning of the quarter. The measure is winsorized at the 1% and 99% level.	Compustat
Stock return (log)	Natural log of one plus annualized average monthly returns for the previous 12 months.	CRSP
Tangibility	Ratio between property, plant, and equipment and total assets, winsorized at the 1% and 99% level.	Compustat
Taxshields	Ratio of deferred taxes and investment tax credit to total assets, winsorized at the 1% and 99% level. Deferred taxes and investment tax credit are set to zero when missing in Compustat.	Compustat
Time since last bond trading date	Firm-level volume-weighted average of the number of days since the date the bond was traded last, measured at the end of each given quarter.	TRACE, Mergent FISD
Time since last rating action	Number of days between the current and the last announcement of a rating upgrade, downgrade, or affirmation for the rated firm.	Thomson
Total assets	Total assets (quarterly).	Compustat
Total leverage	Total debt divided by total assets, winsorized at the 1% and 99% level.	Compustat
Watch negative	Dummy variable equal to one if the firm has been put on a negative watch during the quarter and zero otherwise.	Thomson
Watch positive	Dummy variable equal to one if the firm has been put on a positive watch during the quarter and zero otherwise.	Thomson
Watch signed	Dummy variable equal to one 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

creases. Ratings become relatively more optimistic and less accurate. The effects are the most pronounced precisely in the firms that are most likely to face frictions in raising external capital, thus magnifying their real impact.

Our results suggest that some firms can 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 net present value computations and, ultimately, could lead to value-destroying overinvestment. Thus, our results suggest a potential benefit to broadening the policy debate to include a discussion of how CRA, industry, or market features could interact with the individual beliefs and perspectives of analysts.

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