Internet Appendix:

Cross-Extrapolative Beliefs: Evidence from Equity Analysts

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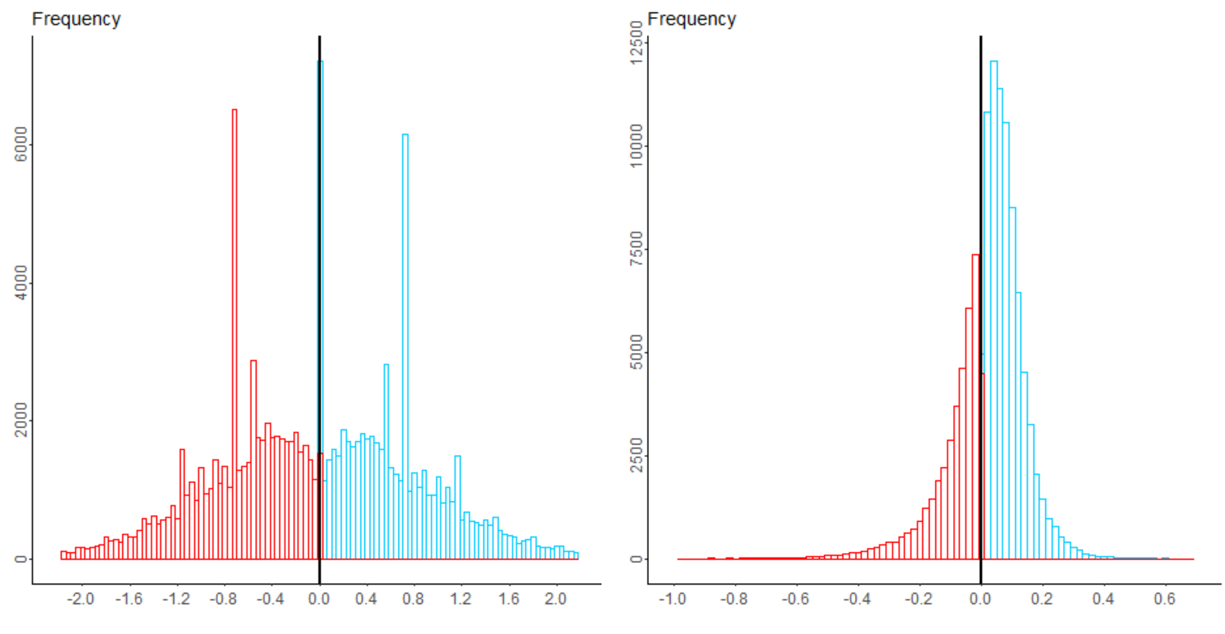
IA.1 Initial coverage and news from other firms

This online appendix provides evidence supporting our identification assumption that analysts' coverage decisions are not driven by bad news from other coverage firms. We first identify coverage initiated as when an analyst issues his or her first earnings forecast on a particular stock (since 1995), and we then examine the distribution of these initial forecasts adjusted within firm-quarters and the corresponding $OCFNews$ prior to those forecasts. If our results are driven by pessimistic analysts initiating coverage following bad $OCFNews$, we would observe that (1) analysts' initial forecasts are more pessimistic relative to their peers; and (2) the corresponding $OCFNews$ variable is more likely negative. Figure IA.1 plots the histogram of adjusted earnings forecasts and $OCFNews$ at initial coverage, respectively. As is shown, while there is no obvious bias in the initial forecasts, the $OCFNews$ variable is more likely positive. In fact, about 48.7% of the initial forecasts are relatively pessimistic, while only 34.2% of the $OCFNews$ variable is negative. This finding suggests that, even though analysts endogenously choose coverage, this selection is unlikely to contaminate our results.

Figure IA.1: Histogram of the adjusted earnings forecasts and $OCFNews$ at initial coverage



Panel A: Adjusted earnings forecast

Panel B: Value of $OCFNews$

IA.2 Additional explanatory power from cross-extrapolation

This online appendix investigates the extent to which cross-extrapolation helps improve the explanatory power of extrapolation-based models used to explain analyst forecasts. We start by estimating the following OLS regressions without including any control variables or fixed effects:

$$Forecast_{ijt} = \alpha + \sum_{s=0}^{n} \beta_s \times FNews_{ijt-s} + \varepsilon_{ijt},$$

with $n = 1, \ldots, 8$ lagged quarters. We record the adjusted R^2 from these regressions in the first row of Table IA.1.

Table IA.1: Explanatory power of cross-extrapolation

This table reports the adjusted R^2 in percentages (i.e., from 0 to 100) from regressing forecasts on lagged *Fnews* with and without *OCFNews*. The bottom row reports the relative improvement in adjusted R^2 by including n lags of *OCFNews* as additional predictors to n lags of *FNews*.

	Lags: n							
	1	2	3	4	5	6	7	8
W/O *OCFNews*	0.03	0.11	0.15	0.15	0.11	0.09	0.16	0.23
With *OCFNews*	0.10	0.21	0.29	0.30	0.25	0.24	0.30	0.35
Relative improvement	296%	86%	92%	101%	128%	178%	84%	50%

Next, we estimate

$$Forecast_{ijt} = \alpha + \sum_{s=0}^{n} \beta_s \times FNews_{ijt-s} + \sum_{s=0}^{n} \beta_s \times \gamma_s \times OCFNews_{ijt-s} + \varepsilon_{ijt},$$

with n lags of *OCFNews* as additional regressors. The adjusted R^2 values for this specification are reported in the second row of Table IA.1. By comparing the adjusted R^2 between the two specifications, we find that the adjusted R^2 typically doubles when we account for cross-extrapolation in addition to own-extrapolation: the median (mean) relative increase is 96% (127%).

IA.3 Robustness

This online appendix provides robustness tests for our main findings. We examine alternative constructions of the variable $OCFNews$, different industry classifications, various subperiods within our sample, and alternative dependent variables that span different earnings forecast horizons. The results are shown in Table IA.2.

First, we compute the $OCFNews$ variable by equal-weighting (instead of value-weighting) analysts' coverage firms in Equation (4). This weighting-scheme yields coefficient estimates of similar statistical significance and slightly larger magnitude. Next, we consider two alternative measures of firm-specific news. First, we use raw stock returns unadjusted for market and industry instead of adjusted returns (i.e., Ret_{ikt} instead of $\widehat{Ret}_{ikt}$) in Equation (4) to compute $OCFNews$. The regression estimates are greater than the baseline estimates in both magnitude and statistical significance. Second, we use standardized earnings surprises from the previous fiscal quarter to substitute for adjusted returns in Equation (4) to compute $OCFNews$. We again obtain qualitatively similar estimates with smaller magnitude, which is likely due to the relatively smaller variation in standardized earnings surprise.

In addition, we consider three alternative industry classifications: Fama-French 12 industries, the 11 Global Industry Classification Standard (GICS) sectors, and the Hoberg and Phillips (2016) 10-K text-based 50 industry classification (FIC-50). We adjust returns for industry performance using each of these alternative industry definitions when computing the $OCFNews$ variable. As is shown, the statistical significance and magnitude of the coefficient estimates are again similar to those of the baseline. Our results are therefore not driven by any particular industry (mis)classification.

Furthermore, we divide the main sample period into four subperiods: before and after Regulation Fair Disclosure (Reg FD), the financial crisis period, and the post-crisis period. Reg FD, which was ratified by the SEC in 2000, prohibited selective information disclosure by firms to a subset of analysts and thus could affect analysts' private information, for exanple due to their personal connections to management (e.g., Cohen et al., 2010). As is shown, the results of all sub-periods are similar to the baseline, suggesting that our main findings are persistent over time and not solely driven by extreme negative events such as the financial crisis.

Finally, we consider alternative dependent variables with earnings forecast horizons ranging

Table IA.2: Robustness tests

This table shows the robustness of our baseline results, exploring alternative constructions of the variable $OCFNews$, different industry classifications, various subperiods within our sample, and alternative dependent variables that span different earnings forecast horizons. For brevity, we only present coefficients of interest and suppress the control variables. The baseline estimates in the first row refer to columns (2)-(3), (5)-(6), and (8)-(9) of Table 2. We first reestimate the corresponding specifications by considering alternative ways of constructing the variable $OCFNews$. We compute $OCFNews$ by (i) equally weighting performance of other coverage firms in Equation (4); (ii) using raw stock returns (unadjusted for market and industry) or earnings surprises as measure of firm-specific news, and (iii) using three alternative industry classifications: Fama-French 12 industries, GICS-11 sectors, or the 10-K text-based 50 industry classification (FIC-50) of Hoberg and Phillips (2016). Furthermore, we present the coefficient estimates when restricting the sample to four different sub-periods. Finally, we report the results of using one- to five-year earnings forecasts as dependent variables, where we replace the stock × quarter fixed effects with the stock × horizon (1Y-5Y) fixed effects. The corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Forecast		PMAFE		Overpessimistic		
	OCFNews	D1	OCFNews	D1	OCFNews	D1	Obs.
Baseline	0.121***	-0.035***	-0.061***	0.014***	-0.064***	0.015***	1,403,038
	(7.031)	(-6.438)	(-4.988)	(3.024)	(-8.940)	(5.598)	
Alternative weights across coverage firms:							
Equally-weighted	0.183***	-0.052***	-0.113***	0.020***	0.020***	0.025***	1,403,038
	(8.236)	(-7.198)	(-6.155)	(3.770)	(3.770)	(8.124)	
Alternative measure for news:							
Unadjusted returns	0.171***	-0.057***	-0.212***	0.080***	-0.164***	0.058***	1,403,038
	(6.476)	(-5.678)	(-9.087)	(8.560)	(-11.898)	(9.841)	
Earnings surprises	0.016***	-0.019***	-0.003	0.003	-0.006***	0.007***	1,349,180
	(3.206)	(-3.426)	(-0.750)	(0.823)	(-2.973)	(2.866)	
Alternative industry classifications:							
Fama-French 12	0.118***	-0.034***	-0.087***	0.023***	-0.079***	0.021***	1,403,038
	(6.954)	(-5.467)	(-7.857)	(4.792)	(-10.324)	(6.565)	
GICS 11 sectors	0.131***	-0.041***	-0.060***	0.010*	-0.077***	0.020***	1,403,038
	(5.753)	(-5.107)	(-4.739)	(1.817)	(-8.373)	(5.817)	
Hoberg-Phillips 50	0.119***	-0.025***	-0.066***	0.017**	-0.073***	0.017***	755,349
	(5.180)	(-3.196)	(-4.241)	(2.390)	(-7.697)	(5.010)	
Subperiods:							
Pre-Reg FD: 1994-2000	0.139***	-0.047***	-0.054**	0.011	-0.066***	0.013***	261,420
	(4.668)	(-4.574)	(-2.324)	(1.094)	(-5.360)	(3.011)	
Post-Reg FD: 2001-2006	0.135***	-0.026**	-0.063**	0.017**	-0.064***	0.013**	344,654
	(4.086)	(-2.379)	(-2.603)	(2.206)	(-4.281)	(2.631)	
Crisis: 2007-2009	0.119***	-0.062***	-0.078**	0.023*	-0.072***	0.026***	223,218
	(2.814)	(-3.253)	(-2.326)	(1.934)	(-3.527)	(3.184)	
Post-crisis: 2010-2016	0.084***	-0.021***	-0.055***	0.004	-0.056***	0.011***	573,746
	(2.668)	(-3.568)	(-2.696)	(0.678)	(-4.843)	(3.465)	
Alternative EPS-Horizons:							
1Y	0.106***	-0.046***	-0.079***	0.033***	-0.087***	0.029***	516,779
	(4.990)	(-4.425)	(-4.587)	(4.490)	(-8.481)	(6.349)	
2Y	0.048**	-0.043***	-0.021**	0.019***	-0.002	0.008***	495,115
	(2.115)	(-3.980)	(-2.214)	(4.228)	(-0.352)	(3.655)	
3Y	0.065	-0.017	-0.071**	0.028***	-0.026	0.015***	196,983
	(1.452)	(-1.257)	(-2.296)	(3.221)	(-1.564)	(2.877)	
4Y	-0.023	-0.031	-0.015	0.051***	-0.003	0.019***	73,546
	(-1.389)	(-1.646)	(-1.299)	(4.669)	(-0.952)	(3.560)	
5Y	0.039	-0.053**	-0.049	0.034**	-0.009	0.015***	42,442
	(1.251)	(-2.308)	(-1.483)	(2.095)	(-0.870)	(2.684)	

from one year to five years. During estimation, we replace the stock $\times$ quarter fixed effects with the stock $\times$ horizon fixed effects, which allow us to exploit variations in forecasts made for the same firm and fiscal date over the same horizon (1Y to 5Y). The results remain qualitatively consistent with our baseline findings. This implies that cross-extrapolation affects not only short-term forecasts but also long-term forecasts. This finding is particularly noteworthy given the emerging consensus in the behavioral finance literature to consider long-term forecasts rather than short-term forecasts as the main driver of stock prices (e.g., Nagel and Xu, 2022; Bordalo et al., 2023).

IA.4 Alternative Explanations

This online appendix examines alternative (or additional) explanations for our main findings.

IA.4.1 Information spillovers

Our results indicate that bad news from other coverage firms (industries) makes analysts less accurate and overly pessimistic, which suggests that these analysts do not obtain valuable information spillovers that makes them do better than other analysts. An interesting possibility is that the information that analysts acquire takes time to realize and affects the focal firms more slowly. Analysts may trade off the risk of being too pessimistic this period with the benefit of being perceived as the first analyst to notice bad times ahead. In this case, even though their current forecasts are inaccurate, their future forecasts would be more accurate. To test this possibility, we estimate the effects of lagged OCFNews on analyst forecast and accuracy in Table IA.3. As is shown, we find no evidence that news from other firms improves analysts' accuracy in the future, as lagged $OCFNews$ has no significant effect on analyst forecast or accuracy.

In addition, we examine Long-Term Growth (LTG) forecasts to test whether news from other coverage firms affects analyst beliefs about the long-run prospects of focal firms.[2] In Panel A of Table IA.4, the dependent variable is the LTG forecast in columns (1) and (2), the LTG forecast error in absolute value in columns (3) and (4), and a dummy variable indicating overpessimistic LTG forecast in columns (5) and (6). To calculate the forecast errors, we compare the LTG forecast with the realized earnings growth from five years ahead, and we demean and scale the errors in the same way as PMAFE from Equation (2). The dummy variable indicating overpessimism is constructed as in Equation (3). We run the same regressions as in Table 2 and find that bad $OCFNews$ leads to lower and inaccurate LTG forecasts, suggesting that affected analysts become overpessimistic about focal firms' long-run prospects.

As a complementary analysis, we examine those LTG forecasts by using the empirical specification of Coibion and Gorodnichenko (2015) to test whether bad news from other coverage firms leads to departures from the rational expectations of full information. Specifically, we estimate

[2] We demean and scale long-term growth rate (LTG) forecast within each firm-calendar year group in a similar fashion as for earnings forecasts in Equation (1).

Table IA.3: Lagged $OCFNews$ and past forecast errors

This table reports the effects of lagged $OCFNews$ or past forecast errors on analyst forecast and accuracy. The dependent variable is forecast in columns (1-2), PMAFE in columns (3-4), and the dummy variable indicating overpessimism in columns (5-6). We estimate Equation (8) by additionally controlling for lagged $OCFNews$ variables up to four periods back in odd columns, and by controlling for lagged forecast erros up to four periods back in even columns. For brevity, we suppress all control variables and only report the coefficients on the $OCFNews$ variable and all lagged terms. All specifications control for the firm × fiscal year-quarter and analyst × firm fixed effects. Standard errors are double-clustered at the analyst and fiscal year-quarter level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Forecast		PMAFE		Overpessimistic	
	(1)	(2)	(3)	(4)	(5)	(6)
OCFNews	**0.136*** **	**0.135*** **	**-0.041** **	**-0.044*** **	**-0.062*** **	**-0.063*** **
	(6.354)	**(6.297)**	**(-2.551)**	**(-2.739)**	**(-6.203)**	**(-6.334)**
OCFNews (Lag1)	**0.043** **		**-0.001**		**-0.014***	
	(2.130)		**(-0.103)**		**(-1.796)**	
OCFNews (Lag2)	**0.002**		**0.018**		**0.006**	
	(0.115)		**(1.411)**		**(0.699)**	
OCFNews (Lag3)	**0.011**		**0.005**		**0.004**	
	(0.701)		**(0.394)**		**(0.509)**	
OCFNews (Lag4)	**-0.001**		**0.013**		**0.006**	
	(-0.077)		**(1.075)**		**(0.773)**	
Forecast error (Lag 1)		0.891		-1.209***		-0.201
		(1.412)		(-3.938)		(-1.016)
Forecast error (Lag 2)		-3.666***		-0.207		1.059***
		(-7.352)		(-0.897)		(7.773)
Forecast error (Lag 3)		-2.745***		-0.437**		0.757***
		(-7.047)		(-2.148)		(5.538)
Forecast error (Lag 4)		-1.925***		-0.351		0.439***
		(-6.060)		(-1.644)		(4.243)
Observations	866,643	866,643	866,643	866,643	866,643	866,643
R^2	0.266	0.266	0.246	0.246	0.416	0.416
	Firm × Quarter FE & Analyst × Firm FE & Controls					

variations of the following model

$$FE_{ijt}^{LTG} = \alpha_{jt} + \alpha_{ij} + \beta_1 \times FR_{ijt}^{LTG} + \beta_2 \times FR_{ijt}^{LTG} \times OCFNews_{ijt} + \gamma' X_{ijt} \varepsilon_{ijt}, \qquad (15)$$

where FE_{ijt}^{LTG} is the LTG forecast error made by analyst i for firm j in year t, and FR_{ijt}^{LTG} is the forecast revision relative to the analyst's previous LTG forecast. In Panel B of Table IA.4, columns (1)-(2) first report a negative and highly significant coefficient on forecast revisions, implying that analysts overreact to news. This is consistent with evidence from macroeconomic expectations (e.g., Bordalo et al., 2022b). When additionally interacting forecast revisions with $OCFNews$ in columns (3)-(4), we find a significantly positive estimate for β_2. Consistently, in columns (5)-(6), we separate good and bad news from other coverage firms and find a negative

Table IA.4: Long-term growth forecasts

This table shows the effects of bad news from other firms on analysts' long-term growth forecasts. In Panel A, the dependent variable is the long-term growth rate (LTG) forecast in columns (1) and (2), the forecast error in absolute value (compared with realized earnings growth over 5-years) in columns (3) and (4), and a dummy variable indicating overpessimitc LTG forecast in columns (5) and (6). All specifications include the same set of control variables from Table 2, firm × fiscal year-quarter fixed effects and analyst × firm fixed effects. The standard errors are double-clustered at the analyst and fiscal year-quarter level. In Panel B, we estimate the regression specification of Coibion and Gorodnichenko (2015) as in Equation (15). All specifications include firm × calendar year fixed effects and analyst × firm fixed effects. In the even numbered columns, we also include the same set of control variables from Table 2. The standard errors are double-clustered at the analyst and year level. In all specifications of both panels, we compute the $OCFNews$ variables over the window over the past 90 days before each LTG forecast. For brevity, we only present the coefficients of interest and suppress the other control variables. The t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

Panel A: LTG Forecast

	Forecast		Error		Overpessimistic	
	(1)	(2)	(3)	(4)	(5)	(6)
OCFNews	0.051**		-0.012*		-0.012	
	(2.362)		(-1.692)		(-1.380)	
D1		-0.034**		0.012**		0.022***
		(-2.014)		(2.338)		(2.852)
D10		0.027		0.000		0.009
		(1.432)		(0.037)		(1.123)
Observations	150,544	150,544	108,560	108,560	108,560	108,560
R^2	0.412	0.412	0.365	0.365	0.366	0.366
	Firm × Quarter FE & Analyst × Firm FE & Controls					

Panel B: Goibion-Gorodnichenko specification

	LTG Forecast Error					
	(1)	(2)	(3)	(4)	(5)	(6)
LTG Revision	-0.428***	-0.428***	-0.427***	-0.427***	-0.424***	-0.424***
	(-28.746)	(-28.582)	(-29.011)	(-28.827)	(-27.200)	(-27.046)
LTG Revision × OCFNews			0.112*	0.110*		
			(1.895)	(1.884)		
LTG Revision × D1					-0.107*	-0.107*
					(-2.023)	(-2.047)
LTG Revision × D10					0.069*	0.066*
					(1.786)	(1.751)
Observations	77,337	77,337	77,337	77,337	77,337	77,337
R^2	0.998	0.998	0.998	0.998	0.998	0.998
Controls	No	Yes	No	Yes	No	Yes
	Firm × Year FE & Analyst × Firm FE					

sign on the interaction with bad news and a smaller positive sign on the interaction with good news. These estimates imply that analysts overreact even more when there is bad news from other coverage firms. It is also reassuring to see that our main findings of cross-extrapolative beliefs remain robust to this alternative specification.

Furthermore, we exploit the relatedness among firms and industries to assess whether analysts process news from closely related coverage firms differently than that from distantly related firms. The hypothesis is that due to information spillovers, analysts may correctly incorporate news from close firms and only overreact to news from distant firms. To identify relatedness, we use the three-digit NAICS codes to classify industries and track firm- and industry-level supplier-customer relationships. More specifically, suppose an analyst covers firms k and k'. When computing the $OCFNews$ variable for this analyst with respect to firm k, we consider firm k' as *distantly* related to firm k if (1) k and k' operate respectively in industries I_1 and I_2 with two different three-digit NAICS codes; (2) k and k' have no customer-suppler relationships, and firm k has no large customer in industry I_2;[3] (3) I_1 and I_2 have no flows of goods and services with each other in U.S. Input-Output Table from the BEA;[4] (4) firm k has no product market rivals in I_2;[5] (5) firm k has no subsegment operating in industry I_2.[6] Otherwise, k' is defined as *closely* related to firm k. Under this classification, around 20% of all the analysts in our sample cover some distantly related firms.

Using this classification, we estimate analysts' response to news from closely and distantly related firms separately, using the specification from Table 2. We focus on those analysts who cover multiple industries and restrict the sample to the firms with at least one analyst who covers a distantly related firm. In addition, we restrict the period to 1996 - 2015 because we only have the TNIC-3 data for this subperiod.

As is shown in Table IA.5, the effects from close firms are larger in economic magnitude than the baseline effects, while the effects from distant firms are weaker in magnitude but remain statistically significant. When an analyst experienced a bad performance of -10% or more from closely related firms, the analyst's forecast for the focal firm is about 2.9% less accurate and 2.1%

[3]We detect firm-level customer-supplier links by using the network relationships constructed by Barrot and Sauvagnat (2016). They obtain the identity of large customers of all public US firms, which, under regulation Statement of Financial Accounting Standards (SFAS) No. 131, are obliged to report the identify of any customer representing more than 10% of total reported sales.

[4]The U.S. Input-Output Tables from the Bureau of Economic Analysis (BEA) are based on the NAICS codes and provide detailed information on the flows of the goods and services among industries. We use the tables of the commodities by industry valued at purchasers' prices under Use Tables/After Redefinitions/Purchaser Value (`https://www.bea.gov/industry/io_annual.htm`).

[5]We detect horizontal links in product markets by utilizing the 10-K text-based network industry classification (TNIC-3) data developed by Hoberg and Phillips (2016).

[6]We use Compustat Segment files to identify business and operating segments of conglomerate companies that have a different industry classification than the company's primary sector.

Table IA.5: News from closely and distantly related firms

This table reports the differential effects of news from closely and distantly related coverage firms. We estimate the same specifications from Table 2 by including the same set of control variables, firm × fiscal year-quarter fixed effects and analyst × firm fixed effects. The standard errors are double-clustered at the analyst and fiscal year-quarter level, and the corresponding t-statistics are reported in parentheses. For brevity, we only present the coefficients of interest and suppress the other control variables.

	Forecast		PMAFE		Overpessimistic	
	(1)	(2)	(3)	(4)	(5)	(6)
OCFNews (Close)	0.112***		-0.085***		-0.064***	
	(5.029)		(-5.423)		(-7.063)	
OCFNews (Distant)	0.030*		-0.032***		-0.023***	
	(1.877)		(-2.655)		(-3.253)	
D1 (Close)		-0.040***		0.029***		0.021***
		(-4.854)		(4.984)		(5.724)
D1 (Distant)		-0.019**		0.013**		0.010***
		(-2.428)		(2.454)		(2.936)
D10 (Close)		-0.001		0.005		0.001
		(-0.241)		(1.244)		(0.327)
D10 (Distant)		-0.004		0.001		-0.000
		(-0.653)		(0.227)		(-0.087)
Observations	765,897	765,897	765,897	765,897	765,897	765,897
R^2	0.229	0.229	0.250	0.250	0.410	0.410
	Firm × Quarter FE & Analyst × Firm FE & Controls					

more overpessimistic than that of peers. When such bad news comes from distantly related firms, analyst forecast for the focal firm is about 1.3% less accurate and 1% more overpessimistic than the forecasts of peers. As such, bad news from both closely and distantly related coverage firms makes analysts mistakenly pessimistic about the focal firms. The larger coefficient estimates on news from close firms imply that analysts are aware of the relatedness among firms and are more likely to incorrectly extrapolate signals from related firms.

IA.4.2 Overcorrecting past forecast errors

In this section, we study a channel in which analysts overreact to their past forecast errors. To illustrate the idea, recall the example from the introduction. If bad news from other coverage firms also disrupts the earnings of Medicine in the previous quarters (2005Q2-2006Q1), which surprises analyst A who is overoptimistic about Medicine (assuming analyst B is not surprised), analyst A might overreact to this individual signal and become subsequently overpessimistic about Medicine in 2006Q2. This overcorrection could for example happen if analyst A forms a diagnostic expectation (Bordalo et al., 2020; Bordalo et al., 2018). As a result, the analyst's forecast is negatively correlated with previous forecast errors, and our results may (partly) be due to this correlation.

To examine this possibility, we reestimate the effects of $OCFNews$ after controlling for analysts' past forecast errors in columns (2), (4), and (6) of Table IA.3. We control for lagged forecast errors up to four fiscal quarters back. The estimated coefficients on two- to four-quarter lagged forecast errors are significantly negative, implying that analysts make more pessimistic forecasts if their forecasts for the previous fiscal quarters were overoptimistic, and vice versa. This is consistent with the implications of diagnostic expectations that analysts overcorrect their past forecast errors. Nevertheless, even after controlling for analyst response to past forecast errors, the magnitude and statistical significance of the coefficients on $OCFNews$ are virtually the same as those in the baseline, implying that our main findings are not driven by analysts' overcorrection to past errors.

IA.4.3 Analyst distraction

News from other firms or industries could distract analysts' attention from focal firms. Potentially, distracted analysts issue relatively conservative earnings forecasts that turn out to be less accurate. To investigate the importance of analyst distraction in our setting, we examine analyst forecast revisions. If analysts are distracted by coverage firms with attention-grabbing news, they might allocate less effort to the other coverage firms and revise their forecasts less frequently than usual. In Table IA.6, we estimate Equation (9) in columns (1) and (2) with the total number of revisions as the dependent variable. The $OCFNews$ variable is computed here over the period from the earnings announcement date of fiscal quarter $t-1$ to that of fiscal quarter t. As is shown, neither $OCFNews$ nor the bottom- and top-decile dummies significantly affect analyst revisions. This evidence does not suggest that analysts spend less effort on focal firms or revise their forecasts less often.

Next, we estimate the effect of $OCFNews$ on the direction and magnitude of forecast revisions. The dependent variable is the standardized unexpected forecast (SUF) computed as in Stickel (1992) and Malloy (2005) to measure revision magnitude. The $OCFNews$ variables are computed over the window between the announcement date of an analyst's most recent forecast and the

Table IA.6: Forecast revisions and market reactions

This table reports the effect of *OCFNews* on analyst forecast revisions. The dependent variable is the number of forecast revisions issued by analyst i for firm j regarding fiscal year-quarter t in columns (1) and (2); the magnitude of forecast revision in columns (3) and (4), which is measured as the standardized unexpected forecast (SUF) computed as in Stickel (1992); the market-adjusted cumulative abnormal returns (CARs) measured over the event window (0, +1) around the forecast revision announcement dates in columns (5) and (6). The *OCFNews* variable is computed over the period from the earnings announcement date of fiscal year-quarter $t-1$ to that of fiscal year-quarter t in columns (1) and (2). In the remaining columns, the *OCFNews* variable is computed over the period between the announcement date of analyst i's most recent forecast for firm j in quarter t and the announcement date of the analyst's previous forecast for the same firm and fiscal quarter. All specifications include firm × fiscal year-quarter and analyst × firm fixed effects, and all of the control variables from Table 2. We additinally control for forecast revisions in columns (5) and (6). The standard errors are double-clustered at the analyst and fiscal year-quarter level, and the corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Number of Revisions		Forecast Revision		CAR(0, +1)	
	(1)	(2)	(3)	(4)	(5)	(6)
OCFNews	0.003		0.217***		0.003	
	(0.357)		(3.198)		(1.128)	
D1		0.000		-0.077***		-0.001
		(0.084)		(-2.727)		(-0.650)
D10		0.000		0.025*		0.001
		(0.204)		(1.901)		(1.018)
Observations	1,403,038	1,403,038	279,243	279,243	280,841	280,841
R^2	0.651	0.651	0.599	0.599	0.712	0.712
	Firm × Quarter FE & Analyst × Firm FE & Controls					

announcement date of the analyst's previous forecast for the same firm and fiscal quarter. Our sample contains 279,243 forecast revisions. The estimation results are shown in columns (3) and (4) of Table IA.6. The direction and magnitude of analyst forecast revisions are strongly associated with the performance of other coverage firms: analysts revise their forecasts downwards (upwards) significantly more when the other firms experience bad (good) news events. This finding does not point to analyst inattention, as distracted analysts would not incorporate other firms' news into their revisions.

In columns (5) and (6), we further test whether analysts bring valuable information to the market by examining the stock market reaction to forecast revisions associated with $OCFNews$. The dependent variable is the market-adjusted cumulative abnormal returns (CARs) measured over the event window (0, +1) around the announcement date of forecast revisions. If analysts optimally incorporate useful information from $OCFNews$, their forecast revisions would have a greater impact on stock prices. However, as is shown, conditional on the direction and magnitude of the revisions, news from other firms does not significantly affect market reactions. Moreover, the insignificant coefficients on the $OCFNews$ variables suggest that investors do not unravel the biases in analysts' forecasts resulting from cross-extrapolation.

While the findings in Table IA.6 confirm the asymmetric effect of negative and positive news, they suggest a less extreme version. Analysts also respond to positive news and revise forecasts upwards. However, given the significantly smaller revision magnitude, their final forecasts might not differ sufficiently from other analysts' forecasts for econometricians to detect any significant difference.

IA.4.4 Incentivized overweighting

As a final possibility we study whether analysts strategically overexaggerate bad news from other firms to signal their skills. Prior work suggests that analysts have incentives to overweight their private information. For example, Zitzewitz (2001) shows that exaggeration or anti-herding can help underrated analysts signal their skills to potential clients. However, this argument has difficulty explaining why analysts overweight negative rather than positive signals, as suggested by previous studies such as Chen and Jiang (2006).

If professional analysts intentionally overexaggerate bad news, it would be most likely due to their career concerns. To test whether analysts gain benefits from such overexaggeration, we study the impact of overweighting bad news from other firms on analysts' career outcomes. While we do not directly observe analysts' compensation in our data, we measure their implicit incentives associated with movements across brokerage houses. Among brokerage houses, there is a well-known hierarchy of prestige, and being at a high-status house is typically a better job (e.g., higher compensation and prestige) than being at a low-status house. Following Hong and Kubik (2003), we measure the status of a brokerage house by the number of analysts it employs and identify whether an analyst moves up (down) in a given year.

We aggregate data at the analyst-year level by averaging analyst characteristics across their coverage firms and over quarters. For any analyst i and any year t, we count the number of times this analyst has been overpessimistic following bad $OCFNews$ across all the analyst's coverage firms in year t, and create dummy variables indicating whether analyst i belongs to the top- or bottom-10% group with overweighting occurrences in year t. We then estimate the effect of overweighting on analyst job separations and report the results in Panel A of Table IA.7. In column (1) and (4), we first confirm that analyst forecast inaccuracy significantly increases (lowers) the probability of moving to a lower (higher) status brokerage house, reinforcing the fact that analysts benefit from being accurate. In column (2), we show that more overweighting significantly increases the probability of moving down to a lower status brokerage house. The top-10% overweighting analysts are 0.5 percentage point more likely to move down, which is 20% more likely relative to the average yearly moving-down probability of 2.4 percentage point. This effect remains significant after controlling for forecast accuracy in column (3). In addition, we find no effect of overweighting on the likelihood of moving up. Thus, there is no evidence that analysts benefit career-wise from overweighting bad news from other coverage firms.

In addition, we also consider learning by analysts. If analysts strategically exaggerate private information and intentionally make overpessimistic forecasts, they would not be affected by their past overweighting experiences. In contrast, if analysts unintentionally overextrapolate bad news from other firms and make overpessimistic forecasts, which hurt their accuracy and likely also their reputation, they should learn from such adverse experiences and try to overcome such overweighting in the future. In the even columns of Panel B of Table IA.7, we test whether analysts become less likely to extrapolate bad news from other firms when they have made more

Table IA.7: Overgeneralization versus incentivized overweighting

This table shows whether analysts are incentivized to overweight news from other firms. In Panel A, we aggregate data at the analyst-year level and examine how overweighting $OCFNews$ affects analyst job separations. The dependent variable is a dummy variable indicating whether an analyst moves from a high- to a low-status house (move down) in columns (1)-(3), or moves from a low- to a high-status house (move up) in columns (4)-(6). We include year fixed effects and cluster the standard errors at the analyst level. In Panel B, we reestimate our baseline specifications and interact $OCFNews$ and $D1$ with the number of past overweighting occurrences. We identify an overweighting experience from a past overpessimistic forecast for the same firms following bad $OCFNews$ (i.e., $OCFNews_{ijt} < 0$ and $\text{Overpessimistic}_{ijt} = 1$). The corresponding t-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively. For brevity, we only present the coefficients of interest and suppress the other control variables.

Panel A: Analyst job separations

	Move Down			Move Up		
	(1)	(2)	(3)	(4)	(5)	(6)
Inaccuracy	0.003*		0.003	-0.005**		-0.004**
	(1.714)		(1.408)	(-2.506)		(-2.323)
Top 10% Extrapolator		0.005*	0.004*		-0.001	-0.000
		(1.956)	(1.715)		(-0.409)	(-0.002)
Bottom 10% Extrapolator		0.001	0.001		0.002	0.002
		(0.693)	(0.821)		(1.012)	(0.803)
Observations	61,695	61,695	61,695	61,695	61,695	61,695
R-squared	0.004	0.004	0.004	0.010	0.009	0.010
	Year FE & Controls					

Panel B: Past experiences

	Forecast		PMAFE		Overpessimistic	
	(1)	(2)	(3)	(4)	(5)	(6)
OCFNews	0.152***		-0.083***		-0.085***	
	(7.840)		(-6.472)		(-10.306)	
OCFNews × Past Extrapolation	-0.029***		0.020***		0.019***	
	(-3.897)		(3.613)		(4.691)	
D1		-0.057***		-0.057***		-0.057***
		(-10.502)		(-10.502)		(-10.502)
D1 × Past Extrapolation		0.021***		0.021***		0.021***
		(9.419)		(9.419)		(9.419)
Observations	1,403,038	1,403,038	1,403,038	1,403,038	1,403,038	1,403,038
R^2	0.224	0.224	0.207	0.224	0.390	0.224
	Firm × Quarter FE & Analyst × Firm FE & Controls					

of such mistakes in the past.

Specifically, we track the same analyst covering the same firm over time and count the number of times they have made overpessimistic forecasts for the same firms following bad news from other coverage firms (i.e., when $OCFNews_{ijt} < 0$ and $\text{Overpessimistic}_{ijt} = 1$). Then, we reestimate our baseline specifications and interact the $OCFNews$ variable and $D1$ dummy with this number of past overweighting occurrences. As is shown, past overweighting experience significantly mitigates the impact of news from other firms on analyst forecasts. The estimates suggest that four overweighting occurrences in the past are enough to offset the impact of $OCFNews$ completely and prevent analysts from being overpessimistic. This pattern of learning suggests that analysts unintentionally overextrapolate bad news from other firms rather than intentionally exaggerate private information.

IA.5 Emotions

Our evidence on the relatively short-lived effects and the stronger effects to negative news might suggest that cross-extrapolation likely operates through emotions or "moods". Testing such a hypothesis directly is not straightforward in our setting. Still, to obtain some insights into whether emotions play a role, we follow Hirshleifer et al. (2020), who build on experimental, survey, and empirical research that shows that mood states vary per calendar month and weekdays. In particular, January, March, and Fridays can be seen as "high-mood" periods, whereas September, October, and Mondays correspond to "low-mood" periods. Our hypothesis is that, if emotions play a role, analysts would cross-extrapolate bad news more during low-mood periods than during high-mood periods.

Table IA.8: Cross-extrapolation and the role of emotions

This table shows results regarding the role of emotions in forming cross-extrapolative beliefs. We follow Hirshleifer et al. (2020) to classify January, March, and Friday as high-mood periods, and September, October, and Monday as low-mood periods, and construct two dummy variables indicating whether a forecast was made during a high- or low-mood period. We reestimate our main specifications from Table 2 by additionally including the mood dummies and interacting them with the *OCFNews* variable or *D*1 dummy. For brevity, we only present the coefficients of interest and suppress the other control variables. The corresponding *t*-statistics are reported in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%, respectively.

	Forecast		PMAFE		Overpessimistic	
	(1)	(2)	(3)	(4)	(5)	(6)
OCFNews	0.071***		-0.052***		-0.053***	
	(3.239)		(-4.304)		(-6.215)	
OCFNews × Low Mood	0.092***		-0.073**		-0.046**	
	(2.714)		(-2.530)		(-2.579)	
OCFNews × High Mood	0.019		-0.010		-0.003	
	(0.636)		(-0.418)		(-0.187)	
D1		-0.018***		0.018***		0.013***
		(-2.718)		(3.660)		(4.578)
D1 × Low Mood		-0.027**		0.011		0.012**
		(-2.246)		(1.366)		(2.125)
D1 × High Mood		-0.006		0.004		0.002
		(-0.620)		(0.617)		(0.578)
Low Mood	0.003	0.005	0.013**	0.012**	0.007	0.006
	(0.362)	(0.613)	(2.029)	(2.005)	(1.515)	(1.373)
High Mood	-0.006	-0.006	0.005	0.004	0.004**	0.004**
	(-1.458)	(-1.329)	(1.418)	(1.387)	(2.110)	(2.054)
Observations	1,403,038	1,403,038	1,403,038	1,403,038	1,403,038	1,403,038
R^2	0.226	0.226	0.214	0.214	0.391	0.391
	Firm × Quarter FE & Analyst × Firm FE & Controls					

We construct two dummy variables indicating whether a forecast was made during a high- or low-mood period, and reestimate our main specifications from Table 2 by additionally including the mood dummies and interacting them with the $OCFNews$ variable or $D1$ dummy. The results are shown in Table IA.8. Across all specifications, the coefficient estimates on $OCFNews$ or $D1$ alone are smaller compared to the baseline. The estimates of the interaction terms with the low-mood dummy are large and significant, whereas those of the interaction terms with the high-mood dummy are small and insignificant. These findings suggest that analysts overreact to bad news from other coverage firms about twice as much during low-mood periods than during other times, which provides suggestive evidence that emotions play a role in cross-extrapolation.

As analysts still overreact outside low-mood periods, cross-extrapolation likely also operates through other channels. In particular, we find in Section IA.4.1 that analysts overreact to bad news from closely-related firms more than distantly-related firms. This is consistent with the idea of similarity-based recall in the recent literature of non-domain-specific experience effects. As modeled in Bordalo et al. (2022a), when agents form beliefs about a task, different experiences may compete for recall, including non-task-specific ones. Recalled experiences are then used to simulate the task based on their similarity and relatedness. Thus, our evidence suggests that cross-extrapolation operates through similarity-based recall and has a selective memory root.

IA.6 Model

IA.6.1 Setup

This simple model follows the setups in Harris and Raviv (1993) and Kandel and Pearson (1995): (1) There is a risk-free asset with a zero rate of return and a risky security with an uncertain payoff R. (2) There are three time periods: at time 1, investors form prior beliefs about the value of the asset; at time 2, they update their beliefs according to analyst forecasts; at time 3, the value of the risky asset is realized and investors consume their wealth. (3) There is a continuum of investors with total mass equal to 1 who maximize mean-variance utility $\mathbb{E}_{i,t}[W_i] - \frac{\lambda}{2}\text{Var}_{i,t}[W_i]$, where λ is the coefficient of absolute risk aversion. (4) There are two types of investors indexed by $i = 1, 2$. They hold prior beliefs that the return R is normally distributed with mean R_i and precision $h_0 = \sigma_0^{-2}$. A proportion α of traders are of type 1; without loss of generality, they are assumed to initially be more optimistic, $R_1 > R_2$. (5) There are two analysts indexed by $i = 1, 2$ who make forecasts for the risky asset R. Their forecasts are given by $F_i = R + \varepsilon_i$, where $\varepsilon_i \sim \mathcal{N}(s_i, h_\varepsilon = \sigma_\varepsilon^{-2})$. The distribution of the noise term ε_i models that analyst i cross-extrapolates signal s_i from other coverage firms and makes a biased forecast for R. (6) At time 2, type i investors update their beliefs using analyst i's forecast F_i. However, they trust that analysts make an unbiased forecast for R. That is, they believe that $\varepsilon_i \sim \mathcal{N}(0, h_\varepsilon = \sigma_\varepsilon^{-2})$.

There are two key frictions in this model. The first is that type i investors update their beliefs in a naive Bayesian manner and only update beliefs with respect to analyst i's forecast, without taking into account the information sets and actions of others. In particular, at time 1, they do not take into account that at time 2, prices will be "incorrect" because the other agents are updating their beliefs and trading using different information based on the other analyst's forecast. This assumption is standard in the literature of speculative trading and information diffusion (see Kandel and Pearson (1995), Hong and Stein (1999), and Hirshleifer and Teoh (2003)). The second friction is that investors are unable to debias analyst research and are therefore misled by biased forecasts, which is supported by our results on the market reaction to analysts' revisions. Studies such as Jackson (2005) also provide empirical evidence supporting this assumption.

IA.6.2 Equilibrium prices and investor holdings

At time 1, each type i investor optimizes her mean-variance preference

$$\max_{q_{i,1}} \mathbb{E}_{i,1}\left[q_{i,1}(R-P_1)\right] - \frac{\lambda}{2}\mathrm{Var}_{i,t}\left[q_{i,1}(R-P_1)\right].$$

So each type i investor demands

$$q_{i,1}(P_1) = \frac{h_0}{\lambda}(R_i - P_1)$$

of the risky asset. The aggregate demands are $Q_{1,1}(P_1) = \alpha q_{1,1}(P_1)$ and $Q_{2,1}(P_1) = (1-\alpha)q_{2,1}(P_1)$. The market-clearing condition implies that the total demand from all investors must equal the zero net supply. Hence, the market-clearing equilibrium price is

$$P_1^* = \alpha R_1 + (1-\alpha)R_2 = \overline{R}, \tag{16}$$

and the equilibrium holdings are

$$q_{1,1}^* = \frac{h_0}{\lambda}(1-\alpha)\Delta R \quad \text{and} \quad q_{2,1}^* = -\frac{h_0}{\lambda}\alpha\Delta R, \tag{17}$$

where $\Delta R = R_1 - R_2$. By assumption, $\Delta R > 0$, and the aggregate supply of securities is zero. The second type holds a short position, $q_{2,1}^* < 0$.

After analysts 1 and 2 have issued forecasts F_1 and F_2 at time 2, investors update their beliefs and resume trading. The posterior beliefs of type i investors are given by a normal distribution with mean

$$\mathbb{E}_{i,2}[R|F_i] = \frac{h_0}{h_0+h_\varepsilon}R_i + \frac{h_\varepsilon}{h_0+h_\varepsilon}F_i.$$

Similar to period 1, optimizing investors' mean-variance preferences and using the market-clearing condition gives the equilibrium price at time 2:

$$P_2^* = \frac{h_0\overline{R} + h_\varepsilon\overline{F}}{h_0+h_\varepsilon}, \tag{18}$$

where $\overline{F} = \alpha F_1 + (1-\alpha)F_2$. Likewise, equilibrium holdings at time 2 are given by

$$q^*_{1,2} = \frac{(1-\alpha)}{\lambda}(h_0 \Delta R + h_\varepsilon \Delta F) \quad \text{and} \quad q^*_{2,2} = -\frac{\alpha}{\lambda}(h_0 \Delta R + h_\varepsilon \Delta F), \tag{19}$$

where $\Delta F = F_1 - F_2 = \varepsilon_1 - \varepsilon_2 = \Delta\varepsilon$ indicates the difference in analysts' opinions.

IA.6.3 Prediction regarding asset volatility and trading volume

When we compare the prices in (16) and (18), it is clear that the absolute price change between the two periods depends linearly on the new information of analyst forecasts and on the difference in analysts' opinions,

$$|\Delta P^*| = |P^*_2 - P^*_1| = \frac{h_\varepsilon}{h_0 + h_\varepsilon}|F_2 - P^*_1| + \frac{\alpha h_\varepsilon}{h_0 + h_\varepsilon}|\Delta\varepsilon|. \tag{20}$$

Because a larger fluctuation of the security price is equivalent to higher return volatility, a greater dispersion in analyst opinions could increase the return volatility of the underlying risky asset.

Furthermore, calculating the change in the equilibrium holdings in (17) and (19), we can show that it is also linearly related to the difference in analyst forecasts. Because the net supply of the risky security is assumed to be zero, the absolute value of the change in the aggregate holdings by type i investors represents the trading volume in period 2. Taking the absolute difference between (17) and (19) yields the trading volume

$$TV = |Q^*_{1,1} - Q^*_{1,2}| = \frac{\alpha(1-\alpha)h_\varepsilon}{\lambda}|\Delta\varepsilon|. \tag{21}$$

The trading volume is therefore also proportional to the difference in analysts' opinions.

Recall that analyst i's bias ε_i is assumed to depend on the signal s_i from the analyst's other coverage firms, namely, $\mathbb{E}[\varepsilon_i] = s_i$, which implies that $\mathbb{E}[|\Delta\varepsilon|] = |s_1 - s_2| = |\Delta s|$. This leads to an important empirical prediction regarding the impact of analyst cross-extrapolation on financial markets.

Prediction. *Dispersion in news from other coverage firms induces analyst disagreement and thereby increases trading volumes and return volatilities.*

IA.6.4 Prediction regarding asset returns

In this simple model, the expected price change depends on the new information in analysts' forecasts $F_{1,2}$, which are essentially determined by the signals $s_{1,2}$:

$$\mathbb{E}[P_2^* - P_1^*] = \frac{h_\varepsilon}{h_0 + h_\varepsilon}\mathbb{E}[\overline{F} - \overline{R}] = \frac{h_\varepsilon}{h_0 + h_\varepsilon}\overline{s}, \tag{22}$$

where $\overline{s} = \alpha s_1 + (1 - \alpha)s_2$. If an analyst extrapolates bad news from other coverage firms and becomes pessimistic about this risky asset such that $\overline{s} < 0$, the expected return on this asset would be negative.

Moreover, the expected difference between P_2^* and the asset's fundamental value R is given by

$$\mathbb{E}[P_2^* - R] = \frac{h_0}{h_0 + h_\varepsilon}(\overline{R} - \mathbb{E}[R]) + \frac{h_\varepsilon}{h_0 + h_\varepsilon}\overline{s}. \tag{23}$$

This expression implies that, if more analysts cross-extrapolate bad news and make overpessimistic forecasts (that is, $\overline{s} < 0$), the price will shift away from the asset's fundamental value. Connecting to our empirical findings above, because analysts lower their expectations by too much, their overpessimism would induce underpricing of the security. This underpricing would be more pronounced if more analysts are influenced by bad news from other firms. Of course, when the true information is revealed to the market (realization of R in the model or firms' announcement of actual earnings in practice), the price will reverse to the fundamental value.

Prediction. *When many analysts cross-extrapolate bad news from other firms, their overpessimism exerts downward price pressure.*

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