Living up to Analyst Expectations*

Edward Shore†

UNSW Business School

February 1, 2025

Abstract

I develop a new empirical approach to reexamine the relationship between analyst forecasts and earnings manipulation. Traditional methods relying on 'bunching' of earnings just above forecasts are limited by the endogenous nature of forecasts as benchmarks. To address this concern, I propose an instrumental variable design, leveraging brokerage mergers and the composition of analyst 'optimism', that generates exogenous forecast variation. Findings reveal a symmetric, one-to-one response of earnings to forecast changes. Reduced-form results align with a model framing manipulation as a systematic response to forecasts, driven by broader incentives than 'beating' the forecast.

---

*JEL Codes: G14, G24, G34, D82

†Email: e.shore@unsw.edu.au; I am very grateful to my advisers, Hassan Afrouzi, Harrison Hong, Matthieu Gomez, and Jennifer La'O, for all of their help and support throughout my PhD. I also want to thank Sandra Black, Matthias Breuer, Alwyn Clarke, Mark Dean, Andres Drenik, Lukas Fischer, Kingsley Fong, Bruno Furtado, Jarrad Harford, Alexandre Jeanneret, Brian Lee, Adam Lovett, Jing Lu, Ron Masulis, Andrew Olenski, Noémie Pinardon-Touati, Jose Scheinkman, Jianfeng Shen, Michael Woodford, the Cognition and Decision Lab at Columbia University, and seminar participants at the Trans-Atlantic Doctoral Conference for helpful suggestions and advice.

1 Introduction

I develop a new empirical design to address key concerns regarding the link between analyst forecasts and earnings manipulation. The dominant view in the literature emphasizes a pump and dump' strategy: managers inflate earnings to just exceed analyst forecasts, driving up the stock price, and then capitalize by selling their stock options at a profit (Bergstresser and Philippon (2006), Bhojraj et al. (2009), Almeida et al. (2016)). Central to this argument is the presence of less-informed, or 'naive', investors who misinterpret these marginally positive earnings surprises as signals of genuine firm performance. This position is typically supported by evidence of bunching in the 'earnings surprise' distribution (earnings minus the consensus forecast). The presence of bunching just above the forecast, and a lack of bunching just below, is taken to indicate that firms have engaged in earnings management, as the distribution deviates from the expected smooth shape (Burgstahler and Chuk (2015), Almeida et al. (2016), Terry (2023)).

While elegant and flexible, the bunching approach has limitations. Traditional applications of bunching rely on fixed, regulatory thresholds.[1] In contrast, analyst forecasts are endogenous, firm-specific, and time-varying, with managers directly influencing them via guidance.[2] As a result, these forecasts lack the exogenous, fixed thresholds required for traditional bunching identification (Kleven (2016)). Furthermore, the magnitude of the bunching is sensitive to how the underlying sample is constructed, and how the earnings variable is scaled.[3] This makes it difficult to construct robust measures of the strength of the relationship between realized earnings and analyst forecasts. Finally, by focusing exclusively on the local characteristics of the earnings surprise distribution, the bunching design

[1]Examples include taxation (Saez (2010), Chetty et al. (2011), Kleven and Waseem (2013)), minimum wages (Harasztosi and Lindner (2019)), and welfare programs (Yelowitz (1995)).

[2]Several papers document an earnings 'guidance game' where analysts issue optimistic forecasts initially, then reduce them to levels firms can beat (Hong and Kubik (2003), Richardson et al. (2004), Ottaviani and Sørensen (2006), Chiang et al. (2019)).

[3]Durtschi and Easton (2005, 2009) highlight the effects of deflating earnings metrics (e.g., dividing by beginning-of-period market capitalization), selection criteria that exclude more loss firms than profit firms, and differences in firm characteristics on either side of zero (e.g., differences in stock prices or analyst optimism) that distort the earnings surprise distribution. Durtschi and Easton (2009) argue that when these factors are removed—i.e., by analyzing raw rather than deflated earnings, ensuring consistent sample inclusion of profit and loss firms, and accounting for pricing differences—the discontinuity disappears. Burgstahler and Chuk (2015) contest the claim that bunching is purely a function of scaling/selection, though they acknowledge that the size of the discontinuity is reactive to such factors.

is restricted to tests of forecast-induced manipulation near zero. This approach implicitly assumes that the primary channel for manipulation is the opportunistic exploitation of naive investors, but this is not the only plausible mechanism.[4] Firms may have broader incentives to manage earnings in response to forecasts, including efforts to influence how the market assesses firm profitability or growth prospects. Stein (1989) shows that motivations for manipulation of this kind can persist even in efficient markets with fully rational investors. As a result, methods that only consider local bunching may miss these broader aspects of the forecast-earnings relationship.

This paper proposes a new methodology that addresses all of these concerns. The novelty lies in constructing an instrumental variable approach that combines brokerage mergers with changes in analyst 'optimism' composition that creates a source of exogenous variation in consensus analyst forecasts. Brokerage mergers are well known to induce analyst exits: when the merging brokerages employ analysts that cover the same stocks, the target firm analysts are typically fired (Hong and Kacperczyk (2010)). Several papers use these brokerage-merger-induced exits to consider how firms react to changes in the number of analysts in their coverage.[5] Where my approach differs is in emphasizing the substantial and measurable differences in the forecasting behavior of individual analysts. Losing an 'optimistic' analyst, who consistently publishes forecasts that exceed their peers, is likely to have very different implications for a firm than if the analyst is 'pessimistic'. Although in both cases, the change in total coverage is the same, the change induced on the firm's forecasting environment plausibly differs significantly.

The instrument is constructed in the following way. I first produce estimates of analyst optimism via a fixed effects design similar in spirit to Dobbie et al. (2018). This approach uses IBES analyst-level data and controls for firm-year fixed effects to isolate an analyst-specific 'optimism' fixed effect, a measure that captures how positive/negative the analyst is

[4] Incentives to manipulate to beat the forecast may also be a reaction to explicit compensation structures imposed by the board. These compensation structures need not be arbitrary: Terry (2023) shows that a discontinuous incentive for outperforming the analyst forecast can emerge as a means of resolving agency concerns surrounding overinvestment. Nonetheless, attention remains restricted to a narrow region of the earnings surprise distribution in these cases.

[5] He and Tian (2013) show that greater analyst coverage leads to weaker patent generation, both in volume and impact; Kelly and Ljungqvist (2012) show that prices and uninformed demand for stocks fall as information asymmetries increase after the loss of analyst coverage; and Chen et al. (2015) find that analyst coverage loss creates a number of governance concerns including higher excess compensation for the CEO, greater earnings management, and a higher propensity for value-destroying acquisitions.

compared to other analysts covering the same firm in the same year. I then calculate how the overall optimism changes post-merger when some analysts exit the sample by comparing the set of fixed effects of analysts in coverage before vs. after the merger. Consistent with the intuition above, the instrument captures significant and economically meaningful variation in the consensus earnings forecasts. The results indicate a clear and positive relationship between changes in the optimism composition and the change in the consensus forecast: when more 'negative' analysts leave a firm's coverage such that the optimism composition increases, the subsequent consensus earnings forecast is higher, and vice versa.[6]

The instrumental variable approach outlined in this paper provides three contributions. First, it addresses the problem of endogenous benchmarks, as the analyst exit is determined by the brokerage, not by the firms that the analyst covers. Second, the approach yields a continuous, firm-level measure of forecast variation that avoids the scaling and selection issues that distort bunching estimates. Third, the proposed instrument allows for tests of forecast-driven earnings manipulation over the entire 'earnings surprise' distribution.

The main result is as follows: not only do reported earnings respond directly to analyst forecasts, but the relationship is roughly one-to-one. After controlling for firm and year fixed effects, and a number of firm-level controls, I find evidence of a causal 0.98 standard deviation response in firm-level earnings to a 1 standard deviation consensus forecast shock, significant at the 1% level.[7] To the best of my knowledge, this paper is the first to quantify explicitly the causal relationship between realized earnings and analyst forecasts.

My approach generates three additional and significant results linking realized earnings to analyst forecasts. First, the relationship between forecasts and earnings operates across the earnings surprise distribution and is robust in magnitude across the sample. To illustrate,

[6] An identification concern is that analysts who exit after mergers could differ systematically from those who remain. For instance, if firms pressure brokerages to fire more optimistic analysts, then the instrument could simply capture a 'guidance game' on the extensive rather than the intensive margin. Fortunately, this is something that I can test. I find no evidence that analysts who exit after mergers are systematically different from analysts that remain, either in terms of their optimism, their average forecast error, or the number of firms they cover.

[7] Managers appear to achieve this earnings response by adjusting their accruals as opposed to adjusting real expenditures such as R&D. The result that managers principally use discretionary accruals to modify earnings is consistent with findings in Bergstresser and Philippon (2006), Cornett et al. (2008), Yu (2008), Hazarika et al. (2012), Armstrong et al. (2013), and Fang et al. (2016). Other papers that find evidence for manipulation of real expenditures typically focus on firms that are close to the cutoff of just beating/just missing the forecast (Kelly and Ljungqvist (2012), He and Tian (2013), Terry (2023)), where incentives to adjust earnings may be more acute.

even when omitting more than half of the observations around the 'just beat/just miss' cutoff, I still uncover a one-to-one relationship between forecasts and earnings. Second, the relationship is symmetric: positive (negative) forecast shocks lead to higher (lower) realized earnings. This symmetry is notable given the findings of Bhojraj et al. (2009), Almeida et al. (2016), and Terry (2023), which emphasize managerial actions to exceed forecasts: the symmetry here suggests that forecast-driven earnings manipulation may not be limited to opportunistic threshold-based behavior.[8] Third, the relationship is weaker for firms offering executive directors more long-term cash incentives unlinked to the share price. This suggests that managers' short-term incentives play a major role in determining the elasticity of earnings to forecasts.

The instrumental variable approach in this paper is flexible, and easily allows us to assess the impact of analyst forecast shocks on variables other than earnings. A natural question is whether forecast shocks affect stock returns.[9] Ex-ante, it is not clear which sign we should expect. On the one hand, ceteris paribus, positive forecast shocks should weakly improve beliefs about future earnings; weakly, because if the shock is understood by the market to be entirely arbitrary, we should expect the impact on returns to be close to zero. On the other hand, if the market understands that the shock induces manipulation, and takes the view that this manipulation is costly, then this should weakly deteriorate beliefs about future earnings. The overall impact then depends on the interaction of these two countervailing pressures. My results suggest that the latter effect dominates: stock returns respond negatively to the forecast shock. A typical positive forecast shock lowers subsequent average monthly returns by 29-37 bps, or 3.43-4.30% annually. This finding suggests that the market recognizes and prices in the costs associated with forecast-driven manipulation, consistent with the idea that earnings distortions are not value-neutral.

These empirical findings are reconcilable, and interpretable, using an adapted version of the model framework in Stein (1989). Specifically, two adaptations are required: one, the introduction of analyst forecasts as an additional signal observable to the market, and two,

[8]Reductions in earnings in response to lower forecasts can be reconciled with a threshold design that places constraints on the level of manipulation: when forecasts are low, managers 'save up' earnings to allow for greater capacity to manipulate in the future (Degeorge et al. (1999)). In the theoretical portion of the paper, I show an alternative justification for the phenomenon that does not rely on such restrictions.

[9]Earnings manipulation is often thought to be costly to the firm. For example, manipulation can lead to lawsuits (Karpoff et al. (2008b)), management turnover (Karpoff et al. (2008a)), and reputational damage (Seybert (2010)).

a relaxation of the assumption that the underlying permanent component of earnings affects realized earnings in a strictly linear fashion.[10] These two minor modifications are sufficient to generate earnings manipulation that co-moves with analyst forecasts without relying on a 'pump and dump' incentive. Symmetry in the earnings response emerges endogenously in the model due to a trade-off between the costs of earnings management and the perceived benefit of influencing market perceptions of future earnings. As in Stein (1989), the earnings response is weaker for firms that have executives with fewer short-term incentives.

The model can also generate negative price reactions to positive forecast shocks. This is because, in equilibrium, managers are trapped into manipulating, even though the market is not fooled by the earnings management.[11] Positive forecast shocks increase the pressure to report higher earnings, prompting managers to inflate through costly manipulation. Rational investors recognize this behavior and adjust their beliefs accordingly, attributing a smaller portion of the forecast increase to improvements in fundamentals and a larger portion to costly manipulation. The result is a decline in firm value as investors price in the future costs of the manipulation.

Finally, the model predicts that analysts magnify rather than reduce the problem of short-termism. Somewhat counterintuitively, this result comes directly from the improvement that forecasts provide to the information environment. Under the presence of analyst forecasts, the market has more information about the firm's performance, and hence more precise beliefs over the firm's quality. As a consequence, the contemporaneous earnings signal has a greater impact on beliefs about future earnings. This means that the marginal benefit of manipulation is higher, leading to higher overall manipulation. This theoretical finding offers a countervailing balance to the commonly held view that analysts act as external monitors of managers, helping to mitigate agency problems (Jensen and Meckling (1976); Yu (2008); Chen et al. (2016)).

A calibrated version of the model rationalizes the one-to-one relationship between earn-

[10] In the original model in Stein (1989), the relationship between the underlying permanent component and reported earnings is linear, meaning that a one-unit change maps directly to a proportional change in reported earnings. This leads to constant levels of manipulation. By relaxing this assumption, I allow the sensitivity and level of manipulation to vary. This adjustment enables the model to capture the responsiveness of earnings realizations to forecast shocks. An additional advantage of this modification is that it generates skewness in the earnings distribution, which more closely matches real-world data (Fama and French (2004), Denis and McKeon (2021)).

[11] This is the principal insight from the original Stein (1989) model.

ings management and forecast shocks, while also matching key features of the earnings and forecast distributions under reasonable parameterizations. This exercise shows that it is possible to generate significant co-movement of earnings and forecasts without the added friction of a 'pump and dump' premium for just beating the forecast. Counterfactual exercises suggest that the cost of short-termist earnings manipulation is significant. Taken together, these empirical and theoretical findings support a strong causal relationship between earnings and analyst forecasts, that may span a broader arena than just 'beating the forecast'.

Outline. Section 2 outlines the data used in the reduced form and calibration exercises. This data is a combination of readily available firm level data and some hand-collected evidence on brokerage mergers. Section 3 describes the identification strategy. Section 4 contains the reduced form findings. Section 5 describes the theoretical model, and Section 6 discusses the model calibration, outlining counterfactuals.

2 Data

I use publicly available data on forecasts, earnings, and other firm fundamentals. For the identification strategy, I also require data on brokerage mergers, which requires hand collection.

2.1 Forecast Data

I use the IBES database as the source for analyst forecasts. IBES is a standard database of analyst forecasts, with wide use across the literature. It also has the highest coverage across alternatives. For these reasons, I focus on IBES forecasts.

IBES Detail is a historical forecast database that collates analyst estimates on a number of forecast measures. The dataset offers comprehensive coverage of US publicly traded firms, from 1982 through to the 2020. I use the diluted, annual earnings-per-share (EPS) forecast as the measure of Wall Street earnings forecasts. Diluted EPS forecasts are the most well-populated in the IBES Detail dataset, and also the variable typically used when reporting earnings performance relative to forecast (So (2013), Kothari et al. (2016)).

Whilst EPS forecasts are available across a fairly long horizon, by far the most represented of these forecasts are the 'F1' and 'F2' forecasts —these are forecasts of annual earnings-per-share for the upcoming year and the year after, respectively. To maximize the number of observations, and hence the precision of the estimation, I use both 'F1' and 'F2' forecasts to estimate analyst fixed effects. In total, there are around 3.7 million 'F1' and 3.5 million 'F2' EPS forecasts, covering 16,521 unique firms for forecast period end-dates from 1989-2023. Using both 'F1' and 'F2' forecasts means I have, on average, 311.3 forecasts for each analyst. Using only 'F1' would drop that number to 167.1 forecasts.

I also make use of the IBES Summary dataset to collect the IBES consensus forecasts; this is the consensus forecast that is typically used for market tests (Brown (2001), Lim (2001), Bartov et al. (2002)). I use the most recent consensus forecast prior to the forecast period end-date as the measure that earnings performances are compared against; again, this is because market tests are typically performed relative to this measure. I use the change to the mean forecast (IBES Summary item 'MEANEST') in the main analysis, although the results are near identical if I instead use the change to the median forecast (IBES Summary item 'MEDEST').

2.2 Firm Fundamentals and Earnings

For firm fundamentals, I use the CRSP-Compustat merged database. CRSP-Compustat is a standard and comprehensive database containing annual fundamental financial and market information for US publicly traded firms across the same time period as IBES estimates.

For the measure of earnings, I use Compustat item 'EPS-FX' —that is diluted earnings per share excluding extraordinary items —and also subtract special items (Compustat item 'SPID'). Philbrick and Ricks (1991) show that IBES earnings data is often unreliable, which motivates the use of the Compustat data. I follow the example of Bradshaw and Sloan (2002) and So (2013), in excluding extraordinary and special items; IBES earnings and analyst forecasts often exclude non-recurring items that are included in Generally Accepted Accounting Principles (GAAP), and would therefore appear in the Compustat data under earnings. Excluding extraordinary or special items then ensures that earnings and earnings forecasts are directly comparable.

For controls, I follow the guidelines in So (2013). Specifically, I control for the one-year lagged values of: the log of total assets, the log of market-to-book ratio, the log of the end-of-year stock price, the dividends-per-share, the return on assets, and the leverage. I drop all firms with SIC codes in the Finance, Insurance, and Real Estate sectors, any utilities companies, non-operating establishments, and industrial conglomerates from the sample.

2.3 Brokerage Mergers

In the past, the process of identifying brokerage mergers was simple, as IBES tracked analyst and brokerage names. In 2018, these were anonymized. To meet this challenge, I follow the methodology in Gibbons et al. (2020). This method allows me to identify 27 brokerage mergers across the 1990-2020 sample period.

I first link the IBES Recommendations database to the Detail database to match analyst names to forecasts.[12] I then search for these analyst names on Bloomberg to identify the name of the brokerage firm the analyst was working for at the time of each reported forecast.

I then identify brokerage mergers by finding the date of the last forecast registered to that brokerage in the IBES dataset. I search Factiva for any news reports regarding brokerage mergers around this date.[13] If the brokerage was indeed subject to a merger, I note the date and include it in the dataset.

I identify analysts at the target firm who exit the sample due to brokerage mergers as those who posted their last forecast between four months before, and two months after the brokerage merger date. I restrict to only those exiting analysts that leave the sample permanently. In other words, the exiting analysts are those who do not join a new brokerage after the merger.[14]

This process results in 27 mergers. Table 1 offers more detail, including the number of analysts that exited following the merger, and the number of firms affected by these exits. Before merging and cleaning the sample, a total of 232 analyst exits can be attributed to brokerage mergers, affecting 1,419 unique firms.[15]

[12] IBES Recommendations is an accompanying database containing buy/hold/sell recommendations for IBES tracked companies that still includes analyst names

[13] Factiva is a business information and research platform that compiles content from newspapers, journals, magazines, newswires, and other printed media.

[14] This exclusion criterion is possible because an analyst's identifier in the IBES data is permanent, and so is preserved if the analyst moves between brokerages.

[15] Note that the sum of the firms affected by each individual merger does not sum to the total number of unique firms affected (it sums to 1,759). This is because some mergers affect the same firms.

Table 1: Identified Mergers

This table presents the 27 mergers that I identify in the data. I include the number of analysts that exit the sample after the merger, and the number of firms affected by these exits. Since 2018, brokerage names have been anonymised by the owners of the IBES dataset, Thomson Reuters. To get around this problem, I follow the strategy in Gibbons et al. (2020), using IBES Recommendations to identify analyst names, then matching analysts to brokerages using Bloomberg data. I validate the mergers using newspaper evidence from Factiva.

Target Brokerage	Merger Date	Acquiring Brokerage	No. Analysts Dropped	No. Firms Affected
Wertheim	July 7th 1994	Schroders plc.	37	151
Kidder Peabody	October 18th 1994	PaineWebber Group	16	142
Hamilton Investments Inc.	January 13th 1995	New York Bancorp	2	3
Kemper Securities	April 11th 1995	Zurich Insurance	5	26
Dean Witter Reynolds	February 6th 1997	Morgan Stanley	11	90
Alex Brown	April 8th 1997	Bankers Trust New York Corp	8	35
Equitable Securities	September 26th 1997	SunTrust Banks, Inc.	6	26
Natwest Equities	November 23rd 1997	Bankers Trust New York Corp	3	8
Volpe Brown	December 13th 1999	Prudential Securities	4	30
JC Bradford	May 1st 2000	PaineWebber Group	6	37
Donaldson, Lufkin and Jenrette	August 30th 2000	Credit Suisse	33	253
Dain Rauscher Wessels	September 28th 2000	RBC	18	132
Josephthal Lyon and Ross	September 18th 2001	Fahnestock Viner Holdings Inc.	1	21
Soundview Technology Group	December 3rd 2003	Charles Schwab	9	74
Parker/Hunter	February 23rd 2005	Janney Montgomery Scott	1	6
Advest	September 19th 2005	Merrill Lynch	2	11
Legg Mason	November 10th 2005	Citigroup	4	42
AG Edwards	May 31st 2007	Wachovia Securities	17	172
Ferris Baker Watts	February 14th 2008	RBC Dain Rauscher	11	92
Fox Pitt Kelton Cochran Caronia Waller	September 30th 2009	Macquarie Group	3	4
Thomas Weisel	April 26th 2010	Stifel Financial	5	28
Morgan Keegan	April 5th 2012	Raymond James Financial	5	69
Dahlman Rose	February 1st 2013	Cowen	6	91
ISI Group	October 31st 2014	Evercore	2	26
CRT Capital	March 22nd 2016	Cowen	11	114
Wunderlich Securities	May 18th 2017	B. Riley Financial	3	28
Sandler O'Neill	July 9th 2019	Piper Jaffray	3	20
Total			232	1,419

2.4 Merging the datasets

Merging IBES and Compustat data is non-trivial —no simple, one-to-one mapping of the two datasets exists. I implement Python code available on the Wharton Research Data Services (WRDS), known as 'IClink', that maps IBES 'Ticker' identifiers to CRSP 'PERMNO' identifiers. These linkages are scored from 0 to 6 based on their accuracy, with 0 being the most accurate and 6 the least. The scores are computed by comparing information on company name and Exchange Ticker symbol corresponding to linkages. Using this link, I can match IBES data with Compustat data using the CRSP/Compustat merged database. In the main analysis, I restrict to linkages that score the highest (score of 0).

Final Dataset. After merging and removing any rows with missing values for the main analysis, the final dataset consists of an unbalanced panel of 66,782 firm-year observations, for 8,055 unique firms, from 1990 to 2020, with 750 unique firms affected by brokerage mergers. In total, 920 firm-year observations are affected by 159 analyst exits. Table 2 shows summary statistics of firms who experience an analyst exit due to a brokerage merger, versus those that do not. Unsurprisingly, firms that experience an exit are larger, more valuable, have higher earnings, and pay out more total dividends. Given that these variables all correlate with analyst coverage, and given that the identification strategy harnesses a plausibly random change in coverage, we would expect to see such firms affected more often than their smaller counterparts.

3 Identification Strategy

Establishing a causal relationship between analyst forecasts and earnings is complicated by significant concerns of endogeneity: firm performance is linked to analyst forecasts by construction.

To get around this problem, I implement a new instrumental variable design that takes advantage of brokerage mergers as a source of exogenous variation in analyst coverage. The instrument I use captures changes in the 'optimism' of analyst coverage effected by analyst losses induced by these brokerage mergers.

When brokerage mergers occur, they do so for reasons plausibly exogenous to the firms that they cover (Hong and Kacperczyk (2010)). Mergers result in analyst job losses, as

Table 2: Summary Statistics of Firms with Exiting vs. No Exiting Analysts

In this table I show summary statistics of the firms that, at some point in the sample, experience an analyst exit from a brokerage merger, compared to those firms that do not experience an exit. Observations are at the firm-year level.

Analyst Exit?	No				Yes			
Variable	N	Mean	SD	Median	N	Mean	SD	Median
EPS ($)	53,659	0.7	1.6	0.6	13,102	1.5	1.7	1.4
Sales ($m)	53,658	2,875	11,493	513	13,102	11,136	31,906	2,363
Cost of Goods Sold ($m)	53,651	1,979	8,782	295	13,102	7,383	24,352	1,378
Net Income ($m)	53,659	134	1,017	15	13,102	748	3,156	102
Total Assets ($m)	53,644	4,257	16,637	611	13,100	13,385	34,862	2,884
Plant, Property, and Equipment (Net) ($m)	53,597	1,646	7,566	124	13,084	4,521	14,647	704
Market Value ($m)	42,598	2,998	13,856	593	10,555	17,532	56,903	2,892
Stock Price ($)	53,658	26	39	18	13,102	40	46	32
Number of Employees (#)	52,614	10	33	2.1	13,017	34	100	8.4
Total Dividends ($m)	53,511	76	515	0	13,084	339	1,209	14
Common Shares Outstanding (#)	53,596	118	323	39	13,097	375	921	95
Shareholder Equity ($m)	53,622	1,580	6,897	272	13,100	5,216	14,584	1,150

Statistical significance markers: * p<0.1; ** p<0.05; *** p<0.01

target firm analysts are fired if their coverage overlaps with analysts at the acquiring firm. This induces exogenous variation in the set of analysts that cover a firm.[16]

Changes in the set of analysts that cover a firm result in at least two important effects on the forecasts the firm is likely to face. First, after exit, the number of analysts is lower.[17] Second, the *composition* of analysts covering the firm also changes. To illustrate, suppose that each analyst's forecast is composed of an analyst fixed effect, a firm-year fixed effect, and some error term:

$$\mathbb{F}_{a,t-1}[EPS_{i,t}] = \alpha_a + \phi_{i,t} + u_{a,i,t} \tag{1}$$

The analyst fixed effect α_a, is a measure of how 'optimistic'/'pessimistic' that analyst is relative to their peers covering the same firms in the same years (captured by $\phi_{i,t}$). Then

[16] I identify an analyst as having exited if they worked at the target firm, and produced a forecast for the brokerage at least four months before the merger, and leave the IBES sample entirely within two months after the merger.

[17] This is the component of the brokerage merger first considered in Hong and Kacperczyk (2010), and applied in subsequent papers (for example, Kelly and Ljungqvist (2012), He and Tian (2013), Chen et al. (2015))

consider the following decomposition of the consensus forecast:

$$\mathbb{F}_{t-1}[EPS_{i,t}] = \frac{1}{|A_{i,t}|} \sum_{a \in A_{i,t}} [\alpha_a + \phi_{i,t} + u_{a,i,t}]$$
$$= \left[\frac{1}{|A_{i,t}|} \sum_{a \in A_{i,t}} \alpha_a \right] + \phi_{i,t} + U_{i,t} \tag{2}$$

where $A_{i,t}$ is the set of analysts covering firm i in year t. The term in square brackets is the average of the fixed effects of analysts in set $A_{i,t}$. When analysts exit, this component of the consensus forecast changes. I use changes in this 'optimism composition' induced by brokerage mergers as an instrument in an IV design.[18]

3.1 Estimating Analyst Fixed Effects

Key to identification is the estimation of analyst 'fixed effects'. An analyst fixed effect is a unique, time-invariant descriptor of that analyst's forecast behavior, which I estimate using standard techniques.[19]

I isolate the analyst-specific variation in forecasts by estimating the following regression using the IBES dataset of EPS forecasts:

$$\mathbb{F}_{a,t-1}[EPS_{i,t}] = \alpha_a + \psi_{FPI} + \lambda_{PDF} + \phi_{i,t} + u_{a,i,t} \tag{3}$$

Here, α_a is an analyst fixed effect, ψ_{FPI} is a Forecast Period Indicator that identifies whether the forecast is an 'F1' or 'F2' forecast (one year or two years ahead respectively), λ_{PDF} is an indicator for a basic vs. a diluted EPS forecast, and $\phi_{i,t}$ is a firm-time fixed effect designed to pick up any firm-time specific variation within forecasts.[20] I remove any observations in

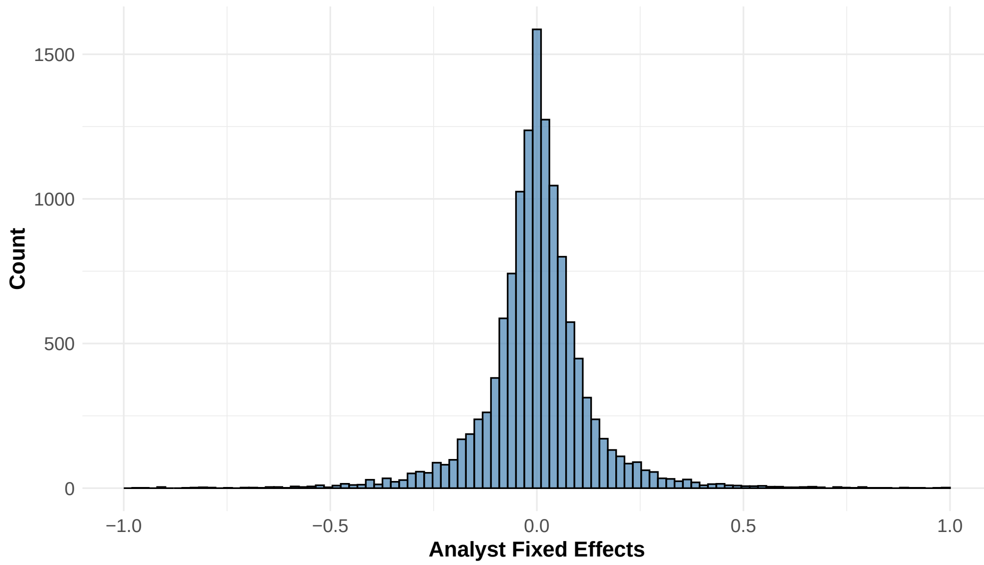
[18]Throughout the paper, I focus on changes in the forecast, rather than levels. This is because the identification strategy relies on changes in the composition of analyst fixed effects rather than levels. In the Online Appendix, I also include results for levels (Table O.3). The final conclusions are the same, though the first stage is slightly weaker.

[19]I focus on a holistic fixed effect that describes the analyst in general, rather than an analyst-firm fixed effect, as this increases the number of observations I can use in the estimation by a factor of 10. I also restrict to analysts with at least 30 forecasts to avoid extreme values of the fixed effects (Breuer and Schütt (2023)).

[20]As discussed in Section 2.1, I use both 'F1' and 'F2' forecasts as this roughly doubles the number of observations I can use to estimate analyst fixed effects (from an average of 160.1 forecasts per analyst to

Figure 1: Histogram of Analyst Fixed Effects

This is a histogram of the estimated analyst fixed effects (α_a), constructed using the specification outlined in Equation 4. As the fixed effects are calculated relative to an arbitrary analyst, the absolute magnitude is not interpretable. What matters is the relative size. To capture this idea, I rebalance the set of fixed effects to have mean zero.



the top/bottom 2.5% of the entire sample to avoid fixed effects being driven by outliers.[21]

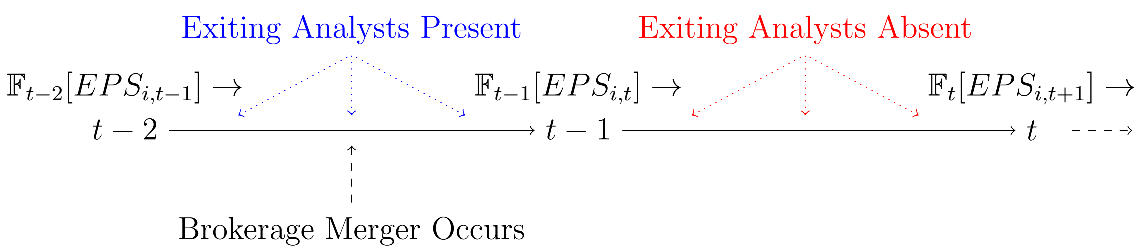
Figure 1 shows the histogram of the estimated analyst fixed effects (α_a) across the entire sample. The distribution displays a high degree of symmetry, with substantial variation across analysts. Note that the fixed effects are calculated relative to an arbitrarily selected analyst. As a consequence, the absolute magnitudes of the fixed effects are not directly interpretable. As the relative size of the fixed effect is all that matters for this analysis, this is not a limiting factor for the paper. For ease of exposition, I normalize the distribution to have mean zero.

311.3). I also include basic as well as diluted EPS forecasts for the same reason: this increases the sample size with which to estimate analyst fixed effects by roughly 13%. My results are robust to removing these forecasts.

[21] This is a significant problem in the IBES dataset. In some cases, forecasts are as high as $972,000,000 per share, and as low as -$97,902,000 per share. After trimming, the max/min is $22.40/-$3.19 per share.

Figure 2: Timeline for Identification

This figure shows the timeline that governs the identification strategy for a given firm. If a brokerage merger that leads to analyst loss occurs between years $(t-2)$ and $(t-1)$, then it is possible that the exiting analyst's forecast remains part of that firm's consensus forecast, $\mathbb{F}_{i,t-2}[EPS_{i,t-1}]$. However, it is certain that the analyst is not contributing to the consensus forecast in the subsequent year, $\mathbb{F}_{i,t-1}[EPS_{i,t}]$. I therefore compare how the forecast changes from the financial year that contains the exiting analyst, to the year when the analyst is absent.



3.2 Timing of the Shock

When a brokerage merger occurs, and analysts exit the sample, their forecasts may remain part of the consensus forecast for some time before becoming 'stale' and being removed (Jegadeesh and Kim (2010)). As such, the forecast could remain unchanged in the year that a brokerage merger occurs even after analyst loss. To avoid this problem, I compare the consensus forecast in the financial year in which the analyst was definitively present, to the following year when the analyst is definitively not part of the firm's coverage. See Figure 2 for an overview of the timeline.

3.3 Constructing the instrument

I use the difference in the average fixed effects of all the analysts that cover a firm to those who exit due to a brokerage merger as the instrument. In plain English, this measure tells us how optimistic the exiting analysts were compared to non-exiting analysts. I label this instrument $\partial AFE_{i,t-1}$. Let $A^{exit}_{i,t-1}$ be the set of analysts that no longer cover firm i at time $t-1$, due to their exit during the year $(t-2)$ due to a brokerage merger. Then the instrument I use is defined by the following expression:

$$\partial AFE_{i,t-1} = \frac{1}{|A_{i,t-1}|} \sum_{a \in A_{i,t-1}} \alpha_a - \frac{1}{|A^{exit}_{i,t-1}|} \sum_{a \in A^{exit}_{i,t-1}} \alpha_a \tag{4}$$

I use this comparison of averages rather than a measure that considers the total number of analysts to avoid using an instrument that correlates with firm fundamentals related to earnings, e.g. size.[22] Note that for the majority of the observations, the value of the instrument, $\partial AFE_{i,t-1}$, is equal to zero, as most observations do not contain an analyst exit.

The identification argument rests on two claims: (i) that the term $\partial AFE_{i,t-1}$ is correlated with a firm's consensus forecast, and (ii) that $\partial AFE_{i,t-1}$ is orthogonal to firm earnings. This identification argument can be summarized as follows:

$$cov(\partial AFE_{i,t-1}, \mathbb{F}_{t-1}[EPS_{i,t}]) \neq 0 \tag{5}$$

$$cov(\partial AFE_{i,t-1}, \nu_{i,t}) = 0 \tag{6}$$

where Equation 5 is the standard relevance condition, and Equation 6 is the exogeneity condition, where $\nu_{i,t}$ is the error in the second stage regression.

3.3.1 Relevance of the Instrument —Equation 5.

To demonstrate instrumental relevance, I estimate a local linear regression of the standardized changes in the consensus forecast on the constructed instrument, using only observations that involve an analyst exit effected by a brokerage merger. This illustrates visually the relationship between the instrument and the consensus forecast, and the localized structure avoids results that are driven purely by outliers. Figure 3 shows the result of this exercise, overlaid on a histogram of the instrument.

Figure 3 shows a clear and economically meaningful relationship between the change in the analyst fixed effects and the change in the consensus forecast. When optimistic analysts exit, i.e. when $\partial AFE_{i,t-1}$ is negative, then the change to the consensus forecast in the subsequent period is negative. Note further that when the instrument takes a value of zero, then the change in the consensus forecast ought also to be zero, which is confirmed by the local linear regression.

I also present results from the standard linear first stage in Table 3. The specification

[22] For example, if the instrument is scaled by the proportion of analysts that exit, then this will necessarily correlate with greater levels of coverage, which in turn correlates with larger size.

Figure 3: Local Linear Regression of Change in Analyst Fixed Effects on Change in Consensus Analyst Forecast

This is a plot of a local linear estimation of the effect of changes in the optimism composition of the analyst set that covers a given firm in the wake of a brokerage merger on the subsequent change to their consensus earnings forecast. When the value in the x-axis is large, this means that the optimism of the coverage increased after analyst exits. I overlay this plot on a histogram of the distribution of those analyst fixed effect changes. Confidence intervals are shown at the 95% level. The upward slope indicates that when more 'pessimistic' analysts leave the set due to brokerage mergers, the subsequent consensus forecast is higher, and vice versa.

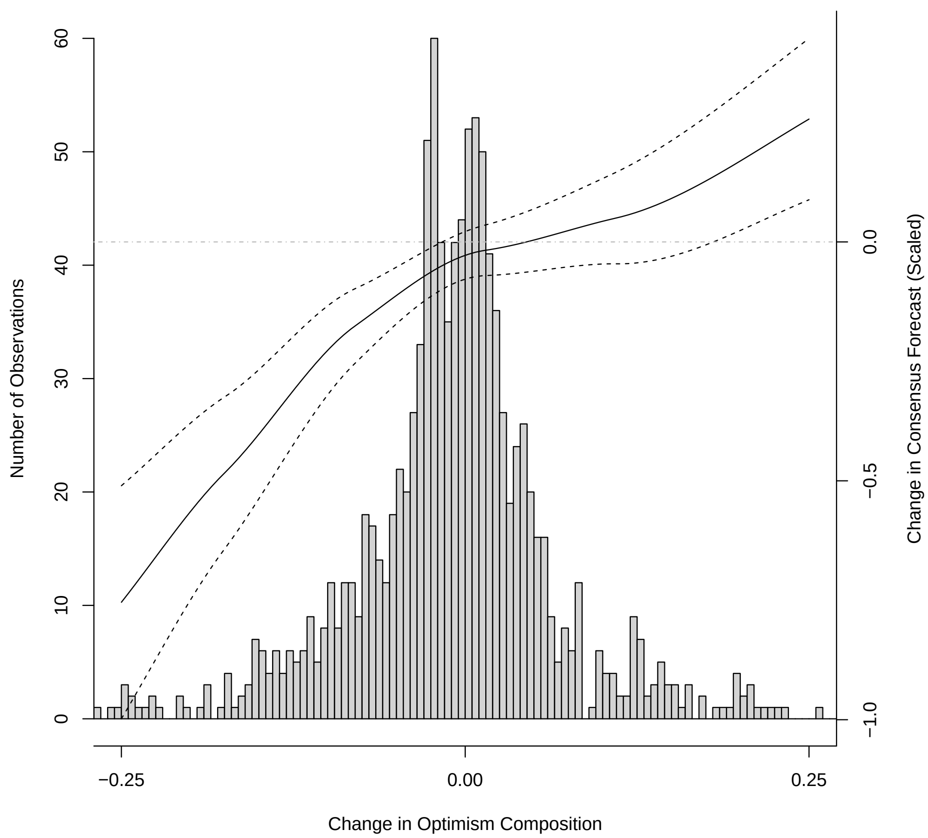


Table 3: First Stage Results of the Change in the Consensus Forecast on the Instrument, $\partial AFE_{i,t-1}$

This table presents the first stage regression of the IV approach. Regression outputs come from the following specification: $\Delta\mathbb{F}_{t-1}[EPS_{i,t}] = \phi_i + \tau_t + \beta\partial AFE_{i,t-1} + \Gamma X_{i,t} + u_{i,t}$, where $\partial AFE_{i,t-1}$ is the constructed instrument that roughly captures how the overall optimism of the analyst set changes after the brokerage merger induced exits. The controls are all lagged variables from the following set: the log of total assets (Compustat: AT), the log of the market-to-book ratio ($MTB = (PRCC\_F \times CSHO)/CEQ$), the log of the stock price ($Price = PRCC\_F$), dividends-per-share ($DVPS = DVT/CSHO$), return on assets ($ROA = NI/AT$), and leverage ($Leverage = (DLTT + DLC)/AT$). The change to the consensus forecast is scaled by the standard deviation of the firm's earnings. Standard errors are clustered at the 'Firm' level. Consistent with the economic intuition, the positive estimate for the coefficient on $\partial AFE_{i,t}$ suggests that when pessimistic analysts cease coverage due to a brokerage merger, the subsequent consensus forecast is higher.

Dependent Variable:			$\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$			
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
$\partial AFE_{i,t-1}$	1.70***	1.76***	1.76***	1.75***	1.75***	1.76***
	(0.39)	(0.39)	(0.39)	(0.39)	(0.39)	(0.39)
$\log(AT_{i,t-1})$	-0.18***	-0.15***	-0.11***	-0.11***	-0.11***	-0.12***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$\log(MTB_{i,t-1})$		0.13***	0.19***	0.19***	0.19***	0.18***
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$\log(Price_{i,t-1})$			-0.09***	-0.09***	-0.09***	-0.08***
			(0.01)	(0.01)	(0.01)	(0.01)
$\text{DVPS}_{i,t-1}$				-0.03***	-0.03***	-0.03***
				(0.01)	(0.01)	(0.01)
$\text{ROA}_{i,t-1}$					0.02	0.02
					(0.02)	(0.02)
$\text{Leverage}_{i,t-1}$						0.05
						(0.04)
Fixed-effects						
Firm	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	69,609	67,278	67,278	67,047	67,039	66,782
R^2	0.15	0.16	0.16	0.16	0.16	0.16
F-test (1st stage)	29.92	32.16	32.23	32.07	32.04	32.16

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

for that regression is shown in Equation 7:

$$\Delta\mathbb{F}_{t-1}[\text{EPS}_{i,t}] = \phi_i + \tau_t + \theta\partial AFE_{i,t-1} + \Delta X_{i,t} + \epsilon_{i,t} \tag{7}$$

where θ is the coefficient of interest, and $X_{i,t}$ is a vector of firm-year controls. The controls are all lagged variables from the following set: the log of total assets (Compustat: AT), the log of the market-to-book ratio ($(PRCC\_F \times CSHO)/CEQ$), the log of the stock price ($PRCC\_F$), dividends-per-share ($DVT/CSHO$), return on assets (NI/AT), and leverage ($(DLTT + DLC)/AT$). The F-test statistic for weak instruments is well above the standard threshold of 10 outlined in Stock and Yogo (2002). The same basic intuition emerges: when more 'optimistic' analysts leave the set, the subsequent consensus forecast is lower.

While the coefficient on the instrument ($\partial AFE_{i,t}$) is positive and highly significant, its magnitude does not have a simple interpretation. This is because the instrument measures the difference between two averages: the mean analyst optimism of those who remain after the merger and the mean optimism of those who exit. Since the number of analysts in each group can differ, the difference in averages does not correspond to a one-to-one shift in the firm's consensus forecast. This design avoids issues that would arise from directly scaling the instrument by the number of analysts, as analyst count correlates with firm size, potentially introducing endogeneity. Instead, $\partial AFE_{i,t}$ is designed to capture pure changes in the composition of optimism—not changes in total coverage or coverage intensity—ensuring that firm characteristics like size do not drive the instrument.

A second reason for the lack of a one-to-one mapping is that analyst forecasts are interdependent. Forecasts are subject to herding and other forms of cross-contamination, where analysts respond to each other's forecasts (Hong and Kubik (2003), Jegadeesh and Kim (2010)). Due to interdependence, remaining analysts may revise their forecasts in response to the exit of a peer, leading to consensus changes that are not mechanically proportional to the shift in analyst composition. The instrument remains relevant as long as shifts in analyst composition systematically affect the consensus forecast, which is confirmed by the first-stage results.[23]

[23] Importantly, interdependence does not pose a threat to exogeneity. To violate the exclusion restriction, firm-specific earnings shocks would have to affect the instrument ($\partial AFE_{i,t}$) directly. While it is true that remaining analysts may revise their forecasts in response to analyst exits, this occurs for reasons that are unrelated to firm-level earnings shocks, but are instead driven by the exogenous change in the analyst set.

Robustness. I conduct three robustness checks to validate this result. The first is a placebo test designed to demonstrate that the instrument behaves as expected when applied to a setting where no analyst exits occur. Instead of using the change in optimism due to analysts exiting, I construct a 'pseudo-change' based on the same set of analysts but during a period when they remain active. Specifically, rather than comparing the period before and after an analyst exits (as in the main specification, where I compare $t-1$ to t for analysts present in $t-1$ but absent in t), I instead compare the change in optimism between $t-2$ and $t-1$ for the same analysts, who remain active contributors to the forecast in both periods. Since these analysts are still contributing to the consensus forecast, we should expect the sign of the first-stage coefficient to be inverted relative to the main result. Consistent with this logic, I find a negative coefficient. Full details of this placebo test are provided in Table O.1 in the Online Appendix.

In the second robustness test, I perform a bootstrapping exercise. I randomly assign 'exits' across the whole sample, calculate the first stage coefficient, and then repeat the process 10,000 times. I assign 159 analyst exits to each iteration to ensure a fair comparison between the pseudo sample and the main dataset. Across 10,000 iterations, I find that 99.3% of coefficients are below the 1.76 reported in Table 3. See Figure O.1 in the Online Appendix for the distribution of estimates of the first stage coefficient.

The third robustness test assesses whether the instrument simply picks up changes in guidance directly from the firms. As one of the central purported advantages of this approach is that it is robust to guidance, this is an important test. I use data from the IBES Guidance dataset to construct two dependent variables. The first is the change in the average annual guidance level for earnings-per-share, $\Delta\mathbb{G}_{t-1}[EPS_{i,t}]$. The second is the change in the range of the maximum and the minimum guidance value within each year, $\Delta\left(\mathbb{G}^{max}_{t-1}[EPS_{i,t}] - \mathbb{G}^{min}_{t-1}[EPS_{i,t}]\right)$. I then test whether the instrument is related to these two measures using a similar specification to Equation 7, but replacing the dependent variable with those described above. I use the same controls as before. I find no evidence for a relationship between the instrument and either variable. Details can be found in the Online Appendix in Table O.2.

Thus, while interdependence may affect how strongly the instrument moves the consensus forecast, it does not create a pathway by which firm-specific shocks influence the instrument itself.

Table 4: Comparison of Exiting and Non-Exiting Analysts

This table presents summary statistics of analysts working at brokerages that experience mergers, split into those who subsequently exit the sample and those that do not. I fail to find statistically significant differences in the average error, average squared error, estimated analyst fixed effect (AFE), estimated analyst fixed effect using only pre-merger forecasts (AFE Pre-Merger), or the number of firms that the analyst covers. If I restrict to only the last two years of forecasts, I also fail to find statistically significant differences in mean or squared errors.

Treatment	Non-Exiting				Exiting				
Variable	N	Mean	SD	Median	N	Mean	SD	Median	Test
Mean Error	1,540	-0.46	1.8	-0.19	159	-0.36	1.1	-0.21	F= 0.498
Squared Error	1,540	10	44	2.2	159	9.1	22	3.1	F= 0.12
Mean Error (Final 2 Years)	986	-0.51	1.8	-0.21	159	-0.39	1.2	-0.25	F= 0.539
Squared Error (Final 2 Years)	986	10	45	2	159	9.3	23	2.7	F= 0.054
AFE	1,540	0.00	0.08	0.00	159	0.01	0.11	0.00	F= 1.568
AFE Pre-Merger	1,540	0.00	0.13	0.00	159	-0.02	0.13	-0.01	F= 1.985
Number of Unique Firms Covered	1,540	12	11	10	159	13	9.5	12	F= 0.978

Statistical significance markers: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

3.3.2 Exogeneity Condition—Equation 6.

For the instrument to be valid, it must generate forecast shocks that are uncorrelated with firm fundamentals. This requires that the analysts who exit after brokerage mergers are close to a random sample from the brokerage's workforce. If certain types of analysts-—such as more optimistic or more pessimistic analysts-—are systematically more likely to exit, then the resulting changes in forecasts would be correlated with firm fundamentals, violating exogeneity.

Previous work has shown that analysts who exit due to mergers are not systematically different from those who keep their jobs (Hong and Kacperczyk (2010)). This is because the firing decision is driven by overlapping coverage, which is essentially random, rather than analyst-level performance. To confirm this finding, I compare the distributions of exiting analysts with those of all non-exiting analysts at the target brokerage. I consider several variables: the average error, the average squared error, the estimated fixed effect of the analyst, and the number of unique firms that the analyst covers. For the error terms and the number of unique firms, I restrict to data before the merger. For the analyst fixed effects, I also re-estimate for all non-exiting analysts at the target brokerages, using only data pre-merger.[24] This avoids the problem that non-exiting analysts continue to produce

[24]In practice, some analysts work for several brokerages throughout the sample. This can be an issue if

forecasts after the merger, whereas the exiting analysts do not. Recent poor performance might explain job loss post-merger, so I also compare the average error and average squared error using only data from the two years prior to the merger.

Table 4 shows the average values for each of these variables across the two groups, alongside an F-test of group differences. I fail to reject the null that the analyst fixed effects of exiting analysts are systematically different from those of non-exiting analysts. I also fail to reject the null that the two groups are not statistically different along all other dimensions.[25]

Since analyst exits are close to a random sample, the resulting forecast shocks are not only idiosyncratic but also robust to common macroeconomic fluctuations. Put simply, a merger event results in positive shocks for some firms and negative shocks for others, even in the presence of a common macroeconomic event. This feature is crucial for ensuring that the forecast shocks are acyclical and uncorrelated with aggregate conditions, allowing for a cleaner causal interpretation.

This approach therefore has a key advantage over prior studies that focus on changes in total analyst coverage. Since brokerage mergers mechanically reduce coverage, and since mergers are typically cyclical (Harford (2005); Erel et al. (2012); Bonaime et al. (2018)), coverage-based instruments risk confounding forecast shocks with broader economic trends. In contrast, by focusing on the composition of analyst optimism rather than the total number of analysts, I isolate changes in forecasts that are driven by analyst departures rather than macroeconomic conditions. While total coverage may fall during pro-cyclical merger events, changes in the optimism composition depend on the characteristics of the exiting analysts. As long as the optimism of the exiting analyst (relative to their peers) is plausibly random, the resulting forecast shocks are idiosyncratic and uncorrelated with aggregate conditions. This approach generates heterogeneous shocks-—positive for some firms and negative for others—-that are acyclical, breaking the link between the forecast shock and the business cycle.

---

the analyst worked for two or more of the identified brokerages, as it is not obvious how to select which forecasts to omit. To get around this problem, I take a conservative approach and remove all forecasts before the earliest merger data in that analyst's history.

[25] I also present histograms of the distributions of estimated analyst fixed effects, mean errors, and mean squared errors in Figure O.2 in the Online Appendix. Again, these distributions appear consistent with exiting analysts being close to a random sample.

4 Reduced-Form Results

4.1 Earnings Reaction to Arbitrary Forecast Shocks

Using the instrument described in Section 3, I estimate a two stage least squares model. The first stage explores the relationship between the instrumental variable, $\partial AFE_{i,t-1}$, and the change in the consensus earnings forecast, $\Delta\mathbb{F}_{t-1}[\text{EPS}_{i,t}]$. I specify this relationship as:

$$\Delta\mathbb{F}_{t-1}[\text{EPS}_{i,t}] = \phi_i + \tau_t + \theta\partial AFE_{i,t-1} + \Phi X_{i,t} + \epsilon_{i,t} \tag{8}$$

where $\Delta\mathbb{F}_{t-1}[\text{EPS}_{i,t}]$ is the change in the consensus earnings forecast; ϕ_i represents firm-specific fixed effects. τ_t is the time-specific fixed effect; $\partial AFE_{i,t-1}$ is our instrumental variable; $\Delta X_{i,t}$ is a vector of control variables; and $\epsilon_{i,t}$ is the error term.

In the second stage I use the predicted values generated by estimation of Equation 8, $\Delta\widehat{\mathbb{F}_{t-1}[\text{EPS}}_{i,t}]$ to examine the causal impact of $\Delta\mathbb{F}_{t-1}[\text{EPS}_{i,t}]$ on ΔEPS. The model is:

$$\Delta\text{EPS}_{i,t} = \phi_i + \tau_t + \beta\Delta\widehat{\mathbb{F}_{t-1}[\text{EPS}}_{i,t}] + \Gamma X_{i,t} + \nu_{i,t} \tag{9}$$

I winsorize the consensus forecast ($\mathbb{F}_{t-1}[\text{EPS}_{i,t}]$) and the earnings-per-share ($\text{EPS}_{i,t}$) at the 5% level. To account for problems of scale, I standardize earnings and the consensus forecast.[26] I find that firm-level earnings respond roughly one-to-one to plausibly exogenous variation in analyst forecasts. As Table 5 shows, the coefficient on the change in the consensus forecast is roughly invariant to the inclusion of several firm-level controls.

In a robustness test, I also run the estimation using unadjusted dollar values for earnings and forecasts. The same result emerges: for a $1 increase in the consensus forecast, earnings increase by $1.49, though the coefficient is not statistically distinguishable from 1. See Table O.4 in the Online Appendix for more details.

[26] I subtract the firm-level mean and divide by the standard deviation for each variable respectively. Therefore coefficients should be interpreted as standard deviation changes. I take this approach as opposed to running a log regression, because both earnings and forecasts are systematically negative, which log regressions cannot interpret.

Table 5: Earnings Response to Consensus Forecast Shock

This table presents the second stage regression of the IV estimation to establish the causal impact of forecast changes on changes in realized earnings. Regression outputs come from estimating Equation 9 using the instrument outlined in Section 3. Details of the first stage can be found in Table 3. The included controls are all lagged variables from the following set: the log of total assets (Compustat: AT), the log of the market-to-book ratio ($MTB = (PRCC\_F \times CSHO)/CEQ$), the log of the stock price ($Price = PRCC\_F$), dividends-per-share ($DVPS = DVT/CSHO$), return on assets ($ROA = NI/AT$), and leverage ($Leverage = (DLTT + DLC)/AT$). The changes to the consensus forecast and earnings are standardized. Standard errors are clustered at the 'Firm' level.

Dependent Variable:	$\Delta EPS_{i,t}$					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
$\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	1.01***	1.04***	1.04***	1.04***	1.04***	1.05***
	(0.26)	(0.26)	(0.26)	(0.26)	(0.26)	(0.26)
$\log(AT_{i,t-1})$	-0.05	-0.07	0.08***	0.08***	0.09***	0.07**
	(0.05)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
$\log(MTB_{i,t-1})$		-0.09***	0.11**	0.11**	0.10**	0.08
		(0.03)	(0.05)	(0.05)	(0.05)	(0.05)
$\log(Price_{i,t-1})$			-0.31***	-0.31***	-0.30***	-0.28***
			(0.03)	(0.03)	(0.03)	(0.03)
$\text{DVPS}_{i,t-1}$				0.02**	0.02**	0.02**
				(0.01)	(0.01)	(0.01)
$\text{ROA}_{i,t-1}$					-0.10*	-0.10*
					(0.06)	(0.06)
$\text{Leverage}_{i,t-1}$						0.24***
						(0.05)
Fixed-effects						
Firm	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	69,609	67,278	67,278	67,047	67,039	66,782
R^2	0.15	0.14	0.15	0.15	0.15	0.15
F-test (1st stage), $\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	29.92	32.16	32.23	32.07	32.04	32.16
Wu-Hausman, p-value	0.03	0.02	0.01	0.01	0.01	0.01

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

4.1.1 Mechanism of Earnings Response

By an accounting identity, earnings are equal to the sum of cash flows and accruals.[27] Therefore, at least one of these channels must be responsible for the earnings reaction. Note that:

$$NI \equiv \text{Cash Flow} + \text{Accruals} \tag{10}$$

where NI is net income, cash flow is the flow of cash in and out of the business, and accruals are defined as in Larson et al. (2018):

$$\text{Accruals} \equiv \Delta CEQ - \Delta CHE \tag{11}$$

where CEQ is common equity, and CHE is cash balances. The main variable of interest is diluted earnings-per-share, which is simply net income, adjusted for extraordinary items, over the current number of shares outstanding, plus adjustments that take into account all the securities that can be converted into shares, and thereby dilute the earnings per existing share:

$$EPS \equiv \frac{\text{Accruals}}{Shares} + \frac{\text{Cash Flow}}{Shares} - \frac{\text{Extraordinary Items}}{Shares} \tag{12}$$

To increase earnings-per-share, it is therefore necessary to either increase cash flows, increase accruals, or decrease the number of shares outstanding.

I test whether the per-share values of cash flows or accruals increase in response to the consensus forecast shock by performing a decomposition of the earnings result. To do this, I take the accounting identity in Equation 10 and express all terms as per-share variables using the appropriate diluted share number (Compustat item: CSHFD). I scale all variables by the firm-level standard deviation of earnings. As in the case of earnings and forecasts, I winsorize the accruals variable at the 5% level. Note that I include the extraordinary items in the decomposition of the earnings result to ensure consistency with the accounting identity. I use the same controls as in the main reduced form exercise.

[27] Accruals constitute a set of judgments managers make when recording expenses and earnings. These include adjusting the timing of transactions, the estimation of uncertain amounts (i.e. bad debt provision, warranty provision, etc.), and determining whether financial details warrant noting (known as 'materiality'). Whilst tightly regulated by GAAP and IFRS, there is still substantial discretion on the part of managers as to how they construct accruals. As a consequence, accrual adjustments have long been considered a prime candidate for earnings management (Jones (1991), Roychowdhury (2006), Bergstresser and Philippon (2006)).

Table 6: Decomposition of Earnings into Cash Flow and Accruals

This table presents the findings from an IV estimation exercise of the impact of a consensus forecast shock on a decomposition of the earnings result. The earnings variable is Net Income (NI) less any extraordinary items, where (NI) is equal to the sum of cash flows (CF) and accruals (ACC). I define accruals according to the definition in Larson et al. (2018), that $ACC = \Delta CEQ - \Delta CHE$, where CEQ is common equity, and CHE is cash balances. In column (1) I showcase the earnings result. In column (2) I show the result for changes in accruals, in column (3) for changes in cashflows, and in column (4) for changes in extraordinary items. Note that the coefficients in columns (2)-(4) sum to the coefficient in column (1).

Dependent Variables:	$\Delta EPS_{i,t}$	$\Delta ACC_{i,t}$	$\Delta CF_{i,t}$	$\Delta EXTRA_{i,t}$
Model:	(1)	(2)	(3)	(4)
Variables				
$\Delta \mathbb{F}_{t-1}[EPS_{i,t}]$	1.07***	1.50**	-0.35	-0.08
	(0.27)	(0.70)	(0.64)	(0.18)
Controls	Yes	Yes	Yes	Yes
Fixed-effects				
Firm	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	54,552	54,552	54,552	54,552
R^2	0.15	0.11	0.13	0.07
F-test (1st stage), sc$_{D}ave$	37.40	37.40	37.40	37.40
Wu-Hausman, p-value	0.01	0.28	0.57	0.86

Clustered (Firm) standard-errors in parentheses

Findings are reported in Table 6. I find that accruals dominate the earnings-per-share result, accounting for essentially the entire earnings response. The coefficients on cash flows and extraordinary items are not statistically different from zero.[28]

In the Online Appendix, I assess whether discretionary or non-discretionary accruals drive the accruals response using data from Breuer and Schütt (2023). I find that the entire response comes from discretionary accruals—details can be found in Table O.5. I also test for a real activities-based response using typical variables from the literature (R&D, Selling and General Expenses, and Provision for Bad Debt) as the dependent variable. I find no evidence to support these channels as major contributors to the overall earnings response—details in Table O.6. Finally, I also test for the presence of share repurchasing as a channel for earnings management, as found in Almeida et al. (2016). Again, I find no evidence that share repurchases drive the earnings response. Details can be found in Table O.7 in the Online Appendix.

The finding that accruals are the primary channel for forecast-induced earnings manipulation does not necessarily contradict prior evidence of real activities manipulation. Existing studies that identify real activities manipulation often rely on bunching-based designs, which focus exclusively on firms near the earnings forecast threshold (Bhojraj et al. (2009); Almeida et al. (2016); Terry (2023)). It is possible that real activities manipulation plays a more prominent role for firms narrowly attempting to 'just beat' the forecast, whereas the accruals-based response identified here could reflect a broader, more generalized response to forecast fluctuations.

4.2 Symmetry of Response and Distance from Cutoff

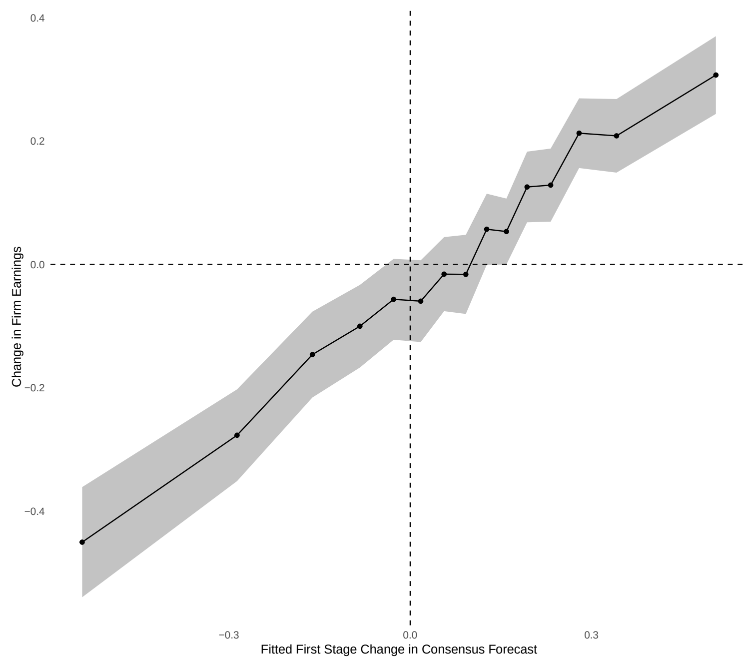
Symmetry of Response: To establish whether the effect is symmetric in sign, I plot a quantile bin scatter of the change in firm earnings on the fitted values of the change in the consensus earnings forecast.

I first estimate the first stage as in Equation 7, then construct quantile bins of the fitted values for the consensus forecast. I form fifteen equally sized bins, and for each bin I

[28] One feature of accruals is that they typically revert in the subsequent year. I test whether this is the case by running the same estimation on earnings as in Section 4.1, but on the subsequent year change rather than the current. Consistent with the accruals response reverting, I find a negative coefficient on the forecast. Details can be found in Table O.8 in the Online Appendix.

Figure 4: Quantile Bin Scatter Plot of Second Stage

In this figure, I plot a quantile bin scatter of the change in firm earnings on the fitted change in consensus earnings forecasts. I use fifteen bins with an equal number of observations in each bin. I show the average value for each bin with 95% confidence intervals. I produce the fitted change using the specification outlined in Equation 8.



calculate the average value of the fitted consensus forecast and the standardized change in firm earnings. To construct standard errors, I take the standard deviation of the change in firm earnings and divide by the square root of the number of observations.

Figure 4 shows the details. The points represent the joint averages of the fitted consensus forecast and the change in firm earnings. The confidence intervals are constructed at the 95% level using the calculated standard errors. I find a positive, consistent, and symmetric relationship between the forecast shock and the earnings response.

A drawback of quantile bin scatters is the difficulty in accounting for control variables and/or fixed effects. To address this problem, I also estimate a modified version of the model in Equation 9 where I split positive and negative forecast revisions and estimate the impact on earnings of each respectively. The regression model takes the following form:

$$\begin{aligned}\Delta\text{EPS}_{i,t} =& \phi_i + \tau_t + \beta_0 \Delta\mathbb{F}_{t-1}[\text{EPS}_{i,t}] \\ & + \beta_1 \Delta\mathbb{F}_{t-1}[\text{EPS}_{i,t}] \times \mathbb{I}\{\Delta\mathbb{F}_{t-1} \geq 0\} + \Gamma X_{i,t} + \nu_{i,t}\end{aligned} \tag{13}$$

where $\mathbb{I}\{\Delta\mathbb{F}_{t-1} \geq 0\}$ is an indicator that takes a value of one if the forecast change is positive. I instrument the changes in forecasts using $\partial AFE_{i,t-1}$ and $\partial AFE_{i,t-1} \times \mathbb{I}\{\Delta\mathbb{F}_{t-1} \geq 0\}$. The coefficient β_0 captures the impact of negative changes in the consensus forecast on earnings, and the coefficient β_1 captures the differential impact of positive changes. The test for weak symmetry is that both $\beta_0 > 0$ and $\beta_1 \geq 0$. In this case, the sign of the effect is the same for both positive and negative shocks. The test for strong symmetry is that β_1 is precisely zero.

Results can be found in Table 7. Consistent with a weakly symmetric response, the β_0 coefficient is significantly positive, and the β_1 coefficient is also positive, though it is not statistically significant. Therefore, even in the presence of controls, the argument for symmetry holds.

Distance from Cutoff: To test whether distance to the cutoff matters, we would ideally know the counterfactual forecast and earnings had the shock not occurred, and then restrict attention to firms where the distance between these two objects is large. However, these objects are not observable.

Fortunately, the reduced form coefficient outlined in Table 5 suggests that the relationship between earnings and forecast shocks is roughly one-to-one. Therefore, realized earnings and

Table 7: Testing for Asymmetry in Earnings Response to Forecast Shocks

This table presents the results of estimating Equation 13 to test for a symmetric response of earnings to positive and negative forecast shocks. I include the first stages and second stage estimates (Columns (1)-(2) and Column (3) respectively) for both the forecast change ($\mathbb{F}_{t-1}[EPS_{i,t}]$) and the forecast change interacted with an indicator that takes a value of one if the forecast change is positive ($\mathbb{F}_{t-1}[EPS_{i,t}] \times \mathbb{I}\{\Delta\mathbb{F}_{t-1} \geq 0\}$). I include the same controls as in the main estimation design. Specifically, the lagged value of: the log of total assets, the log of the market-to-book ratio, the log of the stock price, dividends-per-share, return on assets, and leverage respectively.

Dependent Variables:	$\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	$\Delta\mathbb{F}_{t-1}[EPS_{i,t}] \times \mathbb{I}\{\Delta\mathbb{F}_{t-1} \geq 0\}$	$\Delta EPS_{i,t}$
IV stages	First		Second
Model:	(1)	(2)	(3)
Variables			
$\partial AFE_{i,t-1}$	2.15***	0.17	
	(0.64)	(0.21)	
$\partial AFE_{i,t-1} \times$ I$\{\Delta\mathbb{F}_{t-1} \geq 0\}$	-0.73	0.85**	
	(0.83)	(0.43)	
$\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$			0.82**
			(0.37)
$\Delta\mathbb{F}_{t-1}[EPS_{i,t}] \times \mathbb{I}\{\Delta\mathbb{F}_{t-1} \geq 0\}$			0.64
			(0.98)
Controls	Yes	Yes	Yes
Fixed-effects			
Firm	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	66,782	66,782	66,782
R^2	0.16	0.19	0.09
F-test (1st stage)	16.79	9.25	
F-test (1st stage), $\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$			16.79
F-test (1st stage), $\Delta\mathbb{F}_{t-1}[EPS_{i,t}] \times \mathbb{I}\{\Delta\mathbb{F}_{t-1} \geq 0\}$			9.25
Wu-Hausman, p-value			0.04

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 8: Restricted Earnings Response—'Just Beat/Just Miss' Cutoff Restrictions

This table presents the results of estimating restricted samples of the data where observations close to the 'just beat/just miss' cutoff are removed. I follow the specification outlined in Equations 8 and 9. In column (1) I show the result for the entire sample. In columns (2)-(5) I remove any observations within 5 cents, 10 cents, 25 cents, and 50 cents of the 'just beat/just miss' cutoff. I include the same controls as in the main estimation design. Specifically, the lagged value of: the log of total assets, the log of the market-to-book ratio, the log of the stock price, dividends-per-share, return on assets, and leverage respectively.

Dependent Variable:			$\Delta EPS_{i,t}$		
Model:	(1)	(2)	(3)	(4)	(5)
Cutoff Distance:	0	0.05	0.1	0.25	0.5
Variables					
$\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	1.05^{***}	1.08^{***}	0.99^{***}	0.98^{**}	1.12^{**}
	(0.28)	(0.33)	(0.35)	(0.38)	(0.48)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed-effects					
Firm	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	66,782	52,316	46,185	35,881	26,204
R^2	0.15	0.13	0.18	0.21	0.19
F-test (1st stage), $\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	32.16	28.76	26.39	23.53	20.78
Wu-Hausman, p-value	0.01	0.01	0.04	0.07	0.03

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

forecasts given the shock should have roughly the same distance as if the shock had been absent.

To test whether results are driven entirely by observations close to the 'just beat/just miss' cutoff, I restrict the sample to only include data outside this region. I remove any observations where the earnings minus the consensus forecast was within: (i) 5 cents, (ii) 10 cents, (iii) 25 cents, or (iv) 50 cents of zero. These windows are broad: in case (iv), I lose more than 60% of total observations. Results can be found in Table 8. I also include the baseline estimate using the whole sample (Column (1)). In all four cases (Columns (2)-(5)), I find a virtually identical second stage coefficient.

4.3 Significance of Short-Termism as Motivator

I conduct an additional exercise to highlight that short-termism is the driving factor. I use data from BoardEx to measure the average value of long-term cash incentives for executive directors in each of the firms in the sample. Crucially, these incentives reward managers for improving long-term performance, and are not linked to the share price.

As the overlap of coverage between Boardex and Compustat is limited, especially over time, I do a simple time-series average to construct the proxy. I take the median cash value of long-term incentive plans for all the executive directors of the firm (BoardEx item: $CASHVALHELD$) and divide this by the median market value (Compustat item $CSHO$ multiplied by item $PRCC_F$), using all available observations at the firm level, to get $LTIP_i$.[29] When $LTIP_i$ is high, then the cash value of long-term incentive plans for executive directors is large relative to the market capitalization of the firm.

I then run three estimations using the same setup as in Section 4.1: (i) I restrict to firms at or below the median value of $LTIP_i$, m^{LTIP} (ii) I restrict to firms above the median, and (iii) I estimate across the whole sample, including an indicator that takes a value of one if the firm is above the median ($\mathbb{I}\{LTIP_i > m^{LTIP}\}$) interacted with the change in the forecast. In case (iii) I also include the instrument interacted with $\mathbb{I}\{LTIP_i > m^{LTIP}\}$ so that the number of instruments matches the number of endogenous covariates.

Results can be found in Tables O.9 and O.10 in the Online Appendix. While there are concerns of weak instruments due to the restricted sample, the first stage is significant and consistent with the main findings in all three specifications.

In the second stage, I find a positive and statistically significant coefficient (equal to one) for the causal relationship between forecasts and earnings for firms below the median value of $LTIP$, and an insignificant coefficient close to zero for firms above the median value. In the differential regression, I again find evidence that the earnings of firms below the median respond to forecasts, while the coefficient on the interaction term for firms above the median is negative, though with limited statistical significance. These results are supportive of short-termism being the motivating factor behind the manipulation.

[29] If I calculate the average for each firm-year, the merge results in 10,000 firm-year observations with only 406 containing treatment. This is not enough to achieve identification in the first stage.

4.4 Market Reaction to Forecast Shock

The preceding results indicate that earnings respond causally to arbitrary fluctuations in forecasts, and that this response is driven by discretionary accruals, i.e. manipulation. An extensive literature documents that earnings manipulation can be costly to a firm's future earnings.[30] To see if this cost is acknowledged by market participants, I turn to stock returns data.

To see how stock returns react, I run a regression taking average monthly returns as the dependent variable and performing the same instrumental variable approach to establish the consensus forecast shock as in Section 4.1.

To construct average monthly variables ($\bar{r}_{i,t}$), I take the mean return for each year for each firm. I construct two measures of returns: excess and abnormal. For excess returns, I take the average firm level return minus the risk-free rate ($\bar{r}_{i,t}^{Excess}$). For abnormal returns I adjust using a four-factor Carhart (1997) model ($\bar{r}_{i,t}^{Abnormal}$).[31] I then run the following two-stage regression:

$$\Delta\mathbb{F}_{t-1}[\text{EPS}_{i,t}] = \phi_i + \tau_t + \theta\partial AFE_{i,t-1} + \Phi X_{i,t} + \epsilon_{i,t} \tag{14}$$

$$r_{i,t} = \phi_i + \tau_t + \beta\widehat{\Delta\mathbb{F}_{t-1}[\text{EPS}_{i,t}]} + \Gamma S_{i,t} + \nu_{i,t} \tag{15}$$

where $S_{i,t}$ is a vector of firm-year level controls. I control for the log of the market-to-book ratio, the return-on-assets, the leverage, the annual volatility, the log of the lagged price, and the lag of dividends-per-share. I restrict to larger firms (total assets exceeding \$200m) to avoid liquidity concerns, and remove 2008 data from the Great Recession. I winsorize returns at the 5% level.

Results can be found in columns (1) and (2) in Table 9. For both excess and abnormal returns, I find a negative coefficient, and in the case of excess returns, a statistically significant coefficient. In both cases, the Wu-Hausman p-value is less than 5%, indicating a significant difference between the instrumented coefficient and the OLS result (shown in columns (3) and (4)).

[30] For example, manipulation can lead to lawsuits (Karpoff et al. (2008b)), management turnover (Karpoff et al. (2008a)), and reputational damage (Seybert (2010)).

[31] I use a 60 month rolling window to calculate β's for each of the four factors: the market risk premium (MKT), the size factor (SMB), the value factor (HML) and the momentum factor (MOM).

Table 9: Impact of Forecast Shock on Stock Returns

This table presents the results of estimating the impact of the forecast shock on stock returns, both 'Abnormal' and 'Excess'. 'Abnormal' returns are constructed using a four-factor Carhart (1997) model, and excess returns are simply reported returns minus the risk-free rate. In Columns (1) and (2) I use the IV design to identify the causal response of returns to forecast shocks. In Columns (3) and (4) I show OLS results. The return variables are the mean monthly return in the year of observation. I control for the log of the market-to-book ratio, the return-on-assets, the leverage, the annual volatility, the log of the lagged price, and the lag of dividends-per-share.

Dependent Variables:	$\bar{r}_{i,t}^{Abnormal}$	$\bar{r}_{i,t}^{Excess}$	$\bar{r}_{i,t}^{Abnormal}$	$\bar{r}_{i,t}^{Excess}$
Design:	IV		OLS	
Model:	(1)	(2)	(3)	(4)
Variables				
$\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	-0.0171	-0.0215*	0.0026***	0.0028***
	(0.0117)	(0.0129)	(0.0002)	(0.0002)
Fixed-effects				
Firm	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	42,159	42,159	42,159	42,159
R^2	-0.05	0.05	0.31	0.44
F-test (1st stage), $\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	10.58	10.28		
Wu-Hausman, p-value	0.03	0.01		

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The effect is also economically significant. The average forecast shock in the data is 0.17 standard deviations.[32] So for a typical shock, average monthly returns drop by 29bps (3.43% annually) for abnormal returns, and by 37bps (4.30% annually) for excess returns. These findings are consistent with the market recognizing that forecast shocks induce earnings manipulation, and that those manipulations are costly.

5 Model of forecast-dependent earnings manipulation

To show that the reduced form results are reconcilable with economic theory, I construct a model based on the framework in Stein (1989). Managers face pressure to care about the contemporaneous share price. They can engage in manipulation to modify their earnings in the hope of fooling the market. Their incentives to engage in manipulation vary with the forecasts that they face.

5.1 Setting up the model

There are three players: the manager, the analyst, and the market. The manager makes choices about how to allocate resources intertemporally. The analyst produces forecasts, though acts as a degenerate player that does not react strategically. The market forms expectations about the firm's future earnings based on their observations of past earnings and past forecasts. The manager's optimal response depends on market expectations, as these determine the stock price.[33]

Throughout the model I assume that earnings are represented as earnings-per-share, that the firm pays out all earnings as dividends, and that prices are constructed using the discounted expected value of future dividends-per-share, i.e. earnings-per-share. For the sake of brevity, whenever I refer to 'earnings', I mean to refer to 'earnings-per-share'.

[32] I calculate this value by taking the standard deviation of the instrument ($\partial AFE_{i,t-1}$) for all observations that contain an analyst exit (roughly 0.09), and then multiply this by the coefficient on $\partial AFE_{i,t-1}$ for the first stage (roughly 1.78). This gives a final value of 0.17.

[33] In Stein (1989), there are no analysts. Their inclusion in this model is one of two substantive changes I make to the basic framework. The other I discuss below relates to the relationship between underlying firm performance and observable earnings.

The Manager's Problem. Managers can influence earnings by inefficiently reallocating earnings intertemporally. Observable earnings (e_t) that are known to all players are composed of the sum of three components:

$$e_t = e_t^n + b_t - c(b_{t-1}) \tag{16}$$

where e_t^n is an exogenous process labelled 'natural earnings' that is outside of the manager's control, b_t is the amount that managers reallocate from future earnings to today, and $c(.)$ is a cost function that captures the inefficiency of reallocation.[34] As in Stein (1989), I assume the following features for the cost function:

$$c(0) = 0, c'(0) = (1 + r), c'' > 0 \tag{17}$$

The process that governs natural earnings is a potentially non-linear state space model of the following form:

$$\alpha_t = \alpha_{t-1} + \eta_t, \eta_t \sim N(0, \sigma_\eta^2) \tag{18}$$
$$e_t^n = h(\alpha_t) + \epsilon_t, \epsilon_t \sim N(0, \sigma_\epsilon^2) \tag{19}$$

Here α_t is some underlying state that evolves according to a random walk, and enters into the process of natural earnings through the generic function $h(.)$. The introduction of this function is one of two key differences to the framework in Stein (1989), where $h(.)$ is assumed to be the linear identity function.

There exists a wedge that separates the interests of managers from those of their firm: a short-term pressure to maximize the contemporaneous stock price.[35] The manager's per-period utility is given by:

$$U_t = e_t + \pi P_t + (1 - \pi)\frac{e_{t+1}}{1 + r} \tag{20}$$

where e_t are firm earnings, r is the discount rate, π is the measure of short-termist pressure,

[34]Natural earnings (e_t^n) can be thought of as the earnings of the firm post-optimization.

[35]In Stein (1989) the main intuition for this assumption is that managers are unable to fully insure against takeovers, which force them to tender their shares at the market price. As such, they face a pressure to maximize the contemporaneous stock price as a form of self-insurance. Other intuitions are also discussed, though I abstract from a lengthy discussion of the source of the short-termist pressure as the purpose of the model is not to establish what causes short-termism, but rather to assess its consequences.

and P_t is the stock price, defined as the market's expectation of discounted future earnings.[36] The stock price is defined as:

$$P_t = \mathbb{E}_t \left[\sum_{j=1} \frac{e_{t+j}}{(1+r)^j} \right] \tag{21}$$

The manager does not observe e_t^n, and, crucially, both b_t *and* e_t^n are unobservable to the market.

The Analyst's Problem. Analysts are degenerate players. I assume that analyst forecasts are generated by a process that is near identical to earnings, save for an independent and arbitrary shock, labelled ξ_t. Analysts do not behave strategically, but instead produce a single forecast in a mechanical fashion, which is isomorphic to the consensus forecast. Let ϕ_t be the consensus forecast. Then the process that generates this forecast is given by the following expression:

$$\phi_t^n = h(\alpha_t) + \xi_t, \xi_t \sim N(0, \sigma_\xi^2); \xi_t \perp\!\!\!\perp \epsilon_t \tag{22}$$

$$\phi_t = \phi_t^n + b_t - c(b_{t-1}) \tag{23}$$

I abstract from how this shock might be generated for simplicity, and assume an i.i.d. shock unrelated to any aspects of the actual business.[37]

The Market's Problem. The market does not observe e_t^n, ϕ_t^n, or b_t, only e_t and ϕ_t. On the basis of these observations, the market forms expectations about future earnings, $\{e_{t+j}\}$. Without knowledge of b_t or e_t^n, it is necessary for the market to make some conjecture.[38] Suppose this conjecture is over the path of borrowing, which I will label $\{\hat{b}_t\}$. The goal of the market is to choose a conjecture that minimizes the gap between their conjecture and

[36] Although the problem is not restricted to two periods, from the manager's perspective it is: given that the borrowing term, b_t, is a jump variable and only relevant to period t and $(t+1)$, the manager can act as though they face a series of two-period problems.

[37] Although I identify this shock in the reduced form by looking at changes in individual analysts, the ultimate goal there is to construct a plausibly random shock to the *consensus* forecast, akin to ξ_t.

[38] Note that a conjecture of b_t is sufficient to establish a conjecture over e_t^n, as they are jointly determined by Equation 16

actual borrowing:

$$\min_{\{\hat{b}_t\}} \mathbb{E}_t \left[\sum_{j=0}^{\infty} (\hat{b}_j - b_j)^2 \right] \tag{24}$$

For a given conjecture, the market's beliefs over future earnings is determined by the following non-linear state space model:

$$\alpha_t = \alpha_{t-1} + \eta_t, \eta_t \sim \mathbf{N}(0, \sigma_\eta^2) \tag{25}$$

$$\begin{bmatrix} \hat{e}_t^n \\ \hat{\phi}_t^n \end{bmatrix} = \mathbf{h}(\alpha_t) + \begin{bmatrix} \epsilon_t \\ \xi_t \end{bmatrix}, \begin{bmatrix} \epsilon_t \\ \xi_t \end{bmatrix} \sim \mathbf{N} \left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \sigma_\epsilon^2 & 0 \\ 0 & \sigma_\xi^2 \end{bmatrix} \right) \tag{26}$$

where $\hat{e}_t^n = e_t^n + \hat{b}_t - c(\hat{b}_{t-1})$, and $\hat{\phi}_t^n = \phi_t^n + \hat{b}_t - c(\hat{b}_{t-1})$.

5.2 Outline of Solution

The solution mechanism is a 'signal-jamming equilibrium'. The basic idea is as follows: we suppose that the 'market' has some conjecture over borrowing, $\{\hat{b}_t\}$. On the basis of this conjecture, the market constructs observations of natural earnings/forecasts by backing out conjectured borrowings from observed actual earnings/forecasts. The manager treats the market's conjecture over borrowing as fixed, and concludes that changes in observable earnings are interpreted as changes to natural earnings. The manager derives their best response under this assumption.

In equilibrium, the market's optimal conjecture over borrowing coincides with actual borrowing. This is because the market understands the manager's problem, and there is no asymmetric information regarding the underlying state. In equilibrium, b_t is known to all, and there is no asymmetric information whatsoever; both manager and market have identical beliefs over the hidden state, α_t.[39]

Defining Equilibrium. Formally, we define an equilibrium in the following way:

Definition 1. *For a given exogenous sequence of natural earnings/forecasts, $\{e_t^n, \phi_t^n\}$, and exogenous parameters $\{\pi, r\}$, an equilibrium of the model is a path for borrowing, $\{b_t\}$, a conjecture over borrowing, $\{\hat{b}_t\}$, and observable earnings/forecasts, $\{e_t, \phi_t\}$, such that:*

[39] This is one of the central features of the model in Stein (1989).

- *The choice of $\{b_t\}$ is an optimal response to a given path of earnings, $\{e_t\}$, and conjectures over borrowing, $\{\hat{b}_t\}$; i.e.* $b_t = \arg\max e_t + \pi P_t + (1-\pi)\frac{e_{t+1}}{1+r}$*, subject to borrowing conjecture* $\{\hat{b}_t\}$.

- *The conjecture over borrowing is equal to actual borrowing;* $b_t = \hat{b}_t$, $\forall t$.

Finding Equilibrium. We begin by finding the optimal borrowing conditional on the manager's conjecture. First note the first order condition of their problem:

$$c'(b_t^*) = \frac{1+r}{1-\pi}\left(1 + \pi\frac{\partial P_t}{\partial b_t}\right) \tag{27}$$

The only unknown component in this expression is $\partial P_t/\partial b_t$. In Stein (1989), this derivative comes from application of a Kalman filter to the linear state space model. Introducing non-linearities creates a fundamental problem, as the Kalman filter is the optimal estimator only for linear system models. To deal with this problem, I implement an Extended Kalman filter framework that can handle non-linearities.

The Extended Kalman filter is a *near*-optimal estimator, that can perform state estimation of nonlinear dynamic systems by implementing a local linearization of the non-linearities.[40] Note that the introduction of $h(.)$ does not change the first order condition of the manager, though it will change $\frac{\partial P_t}{\partial b_t}$. Application of the Extended Kalman Filter leads to the following result:

Theorem 1. *When natural earnings/forecasts, $\{e_t^n, \phi_t^n\}$, are determined by Equations 25 and 26, then, up to a first order approximation:*

$$\frac{\partial P_t}{\partial b_t} = \frac{K_t \boldsymbol{h}'(\mathbb{E}_t[\alpha_t])}{r} \tag{28}$$

where K_t is the Kalman gain in the Extended Kalman filter. Consequently, optimal borrowing is given by:

$$c'(b_t^*) = \frac{1+r}{1-\pi}\left(1 + \frac{\pi}{r}K_t \boldsymbol{h}'(\mathbb{E}_t[\alpha_t])\right) \tag{29}$$

[40] The Extended Kalman Filter was developed by NASA Ames in their attempts to apply filtering techniques to nonlinear systems (Smith et al. (1962), McElhoe (1966)). It is the 'de facto' standard in nonlinear state estimation, and is implemented in, for example, navigation systems and GPS (Wan (2006)).

Proof. See Appendix A □

Optimal Borrowing and Market Beliefs. Given the result in Theorem 1, optimal borrowing is given by Equation 29. For a given pair of exogenous sequences of $\{e_t^n, \phi_t^n\}$, and parameters $\{\pi, r\}$, then:

- $b_t^* = c'^{-1}\left(\frac{1+r}{1-\pi}\left(1 + \frac{\pi}{r}K_t\mathbf{h}'(\mathbb{E}_t[\alpha_t])\right)\right), \forall t$
- $\hat{b}_t = b_t^*, \forall t$

which gives us the equilibrium of the model. Note that the borrowing is directly proportional to the slope of the function $h(\mathbb{E}_t[\alpha_t])$. When the slope is high, borrowing will be high, and vice versa. Intuitively, when the slope of $h(.)$ is large, it requires a significantly greater change in earnings to move market beliefs about the underlying state, α_t. As the manager wants to increase the value of that belief today due to short-term pressure to maximize the share price, they will be forced into borrowing more to achieve this response.

5.3 Theoretical Consequences of Solution

State-Dependence of Borrowing. Note that optimal borrowing is time-varying, and depends on beliefs about the state, α_t. This contrasts with Stein (1989), where borrowing is fixed and state independent.

State dependence emerges because the slope of the response of the stock price to borrowing is no longer constant. In the linear case, $\partial P_t/\partial b_t$ is fixed because the impact of earnings on beliefs about the state (α_t) is linear. By contrast, once changes in earnings have non-linear effects on beliefs over the state, the slope of $\partial P_t/\partial b_t$ is no longer fixed. To understand how a change in earnings would influence a change in beliefs about the state, it is necessary to know where on that non-linear function you are, as the gradient is changing across the domain. Hence, $\partial P_t/\partial b_t$ in Equation 28 is no longer constant, but varies with beliefs over the current value of the state, α_t.[41]

[41] Note that the Kalman gain term, K_t, which is a one-by-two vector, is also time-varying, unlike in Stein (1989). This lack of convergence is a direct feature of the linearizations involved in the Extended Kalman Filter (EKF) procedure, and is neither an economically nor quantitatively meaningful source of state dependence.

The impact of forecast shocks on earnings management. When natural earnings/-forecasts, $\{e_t^n, \phi_t^n\}$, are determined by Equations 26 and 25, then we arrive at the following result:

Theorem 2. *If, and only if, $h(.)$ is convex, then:*

$$\frac{\partial b_t}{\partial \xi_t} > 0$$

i.e. optimal borrowing is increasing in arbitrary variation in analyst forecasts.

Proof. We know that

$$b_t^* = c'^{-1}\left(\frac{1+r}{1-\pi}\left(1 + \frac{\pi}{r} K_t \mathbf{h}'(\mathbb{E}_t[\alpha_t])\right)\right)$$

Given $c'(.) > 0, c''(0) > 0$, it follows that c'^{-1} is an increasing function. Thus it is enough to show that $\frac{\partial K_t \mathbf{h}'(\mathbb{E}_t[\alpha_t])}{\partial \xi_t} > 0$. Let $K_{2,t}$ be the component of K_t that corresponds to ϕ_t. Then note that:

$$\frac{\partial K_t \mathbf{h}'(\mathbb{E}_t[\alpha_t])}{\partial \xi_t} = h''(\mathbb{E}_t[\alpha_t])(K_{2,t})^2$$

which is positive iff $h''(.) > 0$ □

This result arises from the manager's fundamental trade-off: while aiming to influence the stock price, the manager also values long-term earnings and thus seeks to avoid excessive costly manipulation. If the function linking the state variable to earnings, $h(\alpha_t)$, is convex, a positive forecast shock creates a strong incentive for the manager to confirm this signal. Higher beliefs about α_t lead to significantly higher expected future earnings, given the increasing slope of $h(\alpha_t)$, making the marginal benefit of aligning with the forecast substantial. Conversely, if the forecast shock is negative, the change in beliefs about α_t is likely to be limited due to the flattening slope, so the manager can reduce manipulation rather than signaling a costly and ineffective rejection of the forecast.

In a concave setting, the opposite intuition applies. A positive forecast shock may increase beliefs about α_t, but under a concave $h(\alpha_t)$, higher values of α_t translate to only modest gains in earnings, due to the function's diminishing slope. Here, the manager is less motivated to confirm the positive signal through additional manipulation, as the potential benefits are limited. Instead, they may take this opportunity to reduce manipulation, letting the forecast shock alone influence expectations. However, if the forecast shock is negative, the stakes increase: the manager has a strong incentive to counteract this signal, as a decline in beliefs about α_t would have increasingly adverse effects on expected earnings. In essence, the opposing results under concavity vs. convexity are determined by whether it pays to confirm good news (convex case) or to dispute bad news (concave case.)

Convexity in the function $h(\alpha_t)$ delivers an additional advantage alongside producing manipulation behavior consistent with the reduced form evidence. Convexity ensures that the skewness of the distribution of earnings-per-share in the model matches the skewness of real-world data. A convex function that loads on the symmetrically distributed state results in positive skew by amplifying the value of positive states.

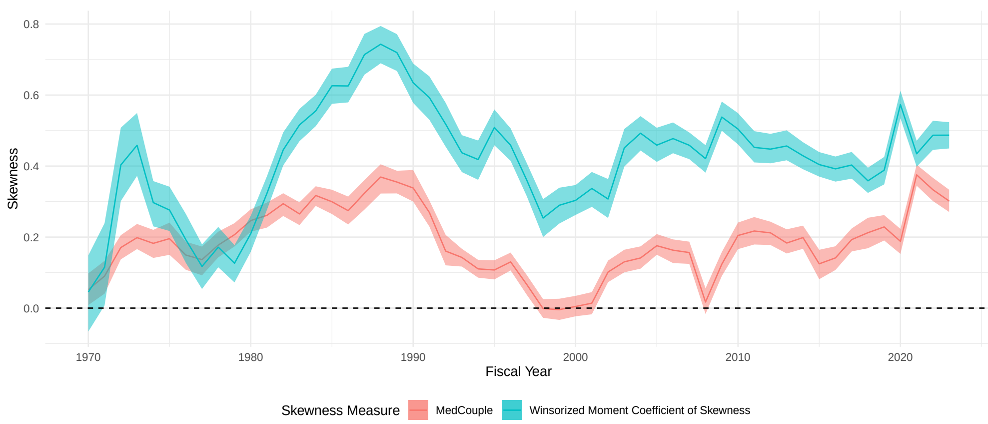
In Figure 5, I show a time series of two measures of skewness in the cross-sectional earnings-per-share distribution, using data from CRSP-Compustat. I use both the moment coefficient of skewness, and the 'MedCouple' robust skewness estimator (Brys et al. (2004)). For the former, I winsorize at the 5% level to avoid estimates being driven by outliers. The 'MedCouple' approach is robust to outliers as it is driven by median and quartile values, so this series is constructed using the entire within-year sample. I construct confidence intervals at the 95% level using bootstrapping. For both measures, the cross-sectional distribution of earnings-per-share displays strong and consistently positive skew over time.

Symmetry in earnings response. The response of earnings to forecast shocks is symmetric in sign (negative forecasts imply lower earnings). Under a modelling framework that incorporates a discontinuity in payoffs for 'just beating' the forecast, such behavior would require additional features like a borrowing constraint on manipulation that induces precautionary 'saving' during positive earnings shocks. By contrast, this symmetric relationship emerges automatically from the setup above. Note that while the effect is symmetric in *sign*, the linearity of the response will depend on the functional form of the cost function, $c(b_t)$, and of the function $h(\alpha_t)$.

The role of analysts. Under the model framework, analysts serve to amplify the incentives to manipulate. Optimal borrowing is driven by the implied effect of borrowing on price, which in turn is simply a function of how much borrowing moves beliefs over the underlying state. With two informative signals, borrowing moves beliefs over the state more than with

Figure 5: Skewness of the Earnings-per-share Distribution.

In this figure, I plot a time series of two measures of skewness in cross-sectional earnings-per-share distributions from CRSP-Compustat data. I construct earnings-per-share using Compustat item 'EPS-FX' —that is diluted earnings per share excluding extraordinary items —minus Compustat item 'SPID' (special items). In red I show the time series for the moment coefficient of skewness, and in blue for the 'MedCouple' robust skewness estimator. For the moment coefficient of skewness, I winsorize the data at the 5% level to avoid estimates being driven by outliers. I perform no winsorization for the 'MedCouple' time series as this estimator is robust to outliers. I calculate 95% confidence intervals using bootstrapping.



a single earnings signal, increasing the perceived 'returns to manipulation'.

This can be seen from inspection of Equation 28, where the price response is the sum of the Kalman gain terms multiplied by $h'(\mathbb{E}_t[\alpha_t])$, divided by the discount rate, r. Under a single signal, there is only a single Kalman gain term. The sum of the Kalman gain terms in the two-signal case must be at least as large as the value in the one-signal case by the Conditional Entropy Inequality ('more information cannot hurt') and the Data Processing Inequality ('post-processing cannot increase information'). This is because the Kalman gain quantifies the information extracted from the signals, and due to the properties of data processing, it is not possible to extract more information from a single signal than from multiple signals.

A common view is that analysts act as external monitors of managers, helping to mitigate agency problems (Jensen and Meckling (1976); Yu (2008); Chen et al. (2016)). However, this theoretical result suggests that analysts may also play an antagonistic role by incentivizing managers to engage in earnings management.

Impact on Stock Prices. Recall that stock prices are the sum of expected discounted future earnings. We can decompose these earnings in the following way:

$$\begin{aligned} P_t &= \sum_{j=1}^{\infty}(1+r)^{-j}\mathbb{E}_t[e_{t+j}] \\ &= \underbrace{\sum_{j=1}^{\infty}(1+r)^{-j}\mathbb{E}_t[h(\alpha_{t+j})]}_{(i)} + \underbrace{\sum_{j=1}^{\infty}(1+r)^{-j}\mathbb{E}_t[b_{t+j}]}_{(ii)} - \underbrace{\sum_{j=1}^{\infty}(1+r)^{-j}\mathbb{E}_t[c(b_{t+j-1})]}_{(iii)} \end{aligned} \tag{30}$$

We want to establish the value of $dP_t/d\xi_t$ in equilibrium, i.e. the total derivative. As the forecast shock affects both beliefs and borrowing, we can decompose this total derivative into the sum of two components, the direct effect through changes in beliefs about the state, and the indirect effect through increased borrowing:

$$\frac{dP_t}{d\xi_t} = \frac{\partial P_t}{\partial \xi_t} + \frac{\partial P_t}{\partial b_t}\frac{\partial b_t}{\partial \xi_t} \tag{31}$$

The first term, $\partial P_t / \partial \xi_t$ that represents the direct impact of a forecast shock on prices, should be weakly positive: ceteris paribus, a positive forecast shock should weakly increase the value of term (i) in Equation 30. This is because the forecast shock is never fully revealed: if market participants were able to identify the shock, then this term would be zero.

The second term, which captures the indirect effect of the forecast, will be weakly negative: ceteris paribus, the forecast shock encourages manipulation that weakly deteriorate earnings, and hence lowers the firm's stock price, i.e. the term (ii) is larger than (iii) in Equation 30. The size of this effect will be closely linked to the costliness of manipulation, as this determines the distance between (ii) and (iii). Which effect dominates, the direct or the indirect, will depend on the specific parameterization of the model. The easiest way to see this is noting that the degree of short-termism, π, increases the responsiveness of borrowing to forecasts (Equation 29), but is independent of beliefs about α_t.[42]

6 Model Calibration

The evidence in Section 4 is consistent with the theoretical conclusions of the model I outline in Section 5. I now show that the model is also capable of matching the size of the earnings response, while generating earnings and forecast distributions that are in line with the data. This illustrates that even without the additional friction of a 'pump and dump' incentive, it is possible to generate forecast-dependent earnings manipulation that matches the magnitudes identified in the data.

6.1 Functional Form Assumptions

The theoretical model contains two generic functions that require a form assumption: $c(b_t)$, and $h(\alpha_t)$. I assume the following functional forms:

$$c(b_t) = (1+r)b_t + \frac{\chi}{2}b_t^2 \tag{32}$$

[42]It is difficult to establish an analytical understanding of the general parameter conditions that would achieve an overall negative response (as in the reduced form evidence), due to the complications introduced to the belief formation process through the inclusion of non-linearities: the expected value of earnings in the future depends on the history of shocks that the firm will draw going forward.

$$h(\alpha_t) = \alpha_t + \psi e^{\alpha_t} \tag{33}$$

Equation 32, which represents the cost of borrowing function, assumes a simple quadratic cost, where the parameter χ is a measure of the degree of convexity. This functional form captures the three key theoretical features required of the cost function as outlined in Section 5, namely: (i) $c(0) = 0$, (ii) $c'(0) = (1 + r)$, and (iii) $c''(.) > 0$ for all $\chi > 0$. Further, because borrowing is never negative for any $\pi \geq 0$, this function displays monotonicity across the support of feasible b_t.[43]

Equation 33, which represents the non-linear function mapping the state variable to earnings/forecasts, includes the standard linear case, plus an exponent term scaled by ψ that captures the degree of non-linearity. This functional form has the advantage of both nesting the linear case ($\psi = 0$), and displaying monotonicity in the value of the state, α_t, with no upper or lower bound to the function output. In this latter respect, the assumption of an exponent is preferable to a quadratic assumption that would introduce a lower bound to the function.

Together, these functional form assumptions imply the following form for the key optimal borrowing condition:

$$b_t^* = \frac{1}{\chi}\left(\frac{1+r}{1-\pi}\left(\pi + \frac{\pi}{r}K_t\left[\begin{matrix}1+\psi e^{\mathbb{E}_t[\alpha_t]}\\ 1+\psi e^{\mathbb{E}_t[\alpha_t]}\end{matrix}\right]\right)\right) \tag{34}$$

As well as these two functional form assumptions, I also relax the assumption that transitory shocks to earnings/forecasts (ϵ_t/ξ_t) are mean zero. I allow these two shocks to have differing means, which I label μ_ϵ and μ_ξ respectively.

6.2 Parameters and Moments

I calibrate seven parameters using seven moments. Details of the parameters and moments involved in the estimation can be found in Table 10.

Parameters. The model contains nine parameters in total: π, the degree of short-termism; χ, the convex cost of borrowing; ψ, the degree of non-linearity in the state loading function

[43] See Equation 29: the functional form of the function $h(\alpha_t)$ implies that $h'(.)$ is strictly positive, and the Kalman gain term, K_t is also non-negative. Hence, for any $\pi \geq 0$, $c'(b_t^*) \geq 0$, which implies that $b_t \geq 0$.

$h(\alpha_t)$; σ_α, the standard deviation of the shocks to the underlying state, α_t; $\{\mu_\epsilon, \sigma_\epsilon\}$, the mean and standard deviation of the transitory shocks to earnings, ϵ_t; $\{\mu_\xi, \sigma_\xi\}$, the mean and standard deviation of the transitory shocks to forecasts, ξ_t; and r, the discount rate.

Of these nine parameters, I calibrate all but σ_ξ and r. For σ_ξ, I assume the value of the standard deviation of the forecast shocks identified in the reduced form exercise, which is 0.17. For r, I assume an 8% rate, roughly in line with the equity premium produced by the average stock return (Fama and French (2002)).

Moments. To calibrate the seven parameters, I select seven moments to match: the mean, the standard deviation, and the skewness of earnings, the mean and skewness of forecasts, the correlation between earnings and forecasts, and the responsiveness of earnings through borrowing to a forecast shock.[44]

The final moment is lifted from the reduced form analysis in Section 4. In the model, this moment is the slope of borrowing with respect to the forecast shock, ξ_t. Note that this slope is equivalent to that of the slope of earnings with respect to the forecast shock, as borrowing enters into earnings linearly. Under the functional form assumptions listed above, the slope takes the following form:

$$\frac{\partial e_t}{\partial \xi_t} = \frac{1}{\chi}\left(\frac{\pi(1+r)}{r(1-\pi)}\psi e^{\mathbb{E}_t[\alpha_t]} K_{2,t}^2\right) \tag{35}$$

To construct the data moments, I use the same sample as in the reduced form exercise. I first take the average of earnings and the consensus forecast across the entire sample. I then remove an industry-year fixed effect from both variables, before re-adding the previously calculated means.

6.3 Final Calibration

The details of the calibrated parameters, data moments, and model moments can be found in Table 10. I match the moments very closely, including the responsiveness of earnings to arbitrary forecast shocks.

[44] Ideally, I would also match to the price response documented in Section 4.4. However, due to the non-linearities in the model, beliefs evolve in a sophisticated and non-linear fashion, making this task challenging: figuring out the expected value for earnings in the future depends on the history of shocks that the firm will draw going forward.

Table 10: Calibrated Baseline Model

This table presents the calibrated parameters and moments from the baseline model, including data moments as reference. Panel B's data moments are constructed using the reduced-form sample, covering a Compustat-IBES panel of 12,432 unique firms for 63,773 firm-year observations, from 1990 to 2020. Model moments use twenty samples of 40-year simulated panels of 1,000 firms.

Panel A: Estimated Parameters	Notation	Calibrated Value
Short-Termism	π	0.0164
Borrowing Cost	χ	0.0235
Non-linearity	ψ	0.6875
Standard Deviation of State Shocks	σ_α	0.1110
Standard Deviation of Earnings Shocks	σ_ϵ	1.1350
Mean of Earnings Shocks	μ_ϵ	1.1050
Mean of Forecast Shocks	μ_ξ	1.2600
Panel B: Assumed Parameters	**Notation**	**Value**
Discount rate	r	0.08
Standard Deviation of Forecast Shocks	σ_ξ	0.17
Panel C: Moments	**Data**	**Model**
Mean of Earnings	0.741	0.737
Standard Deviation of Earnings	1.452	1.443
Skewness of Earnings	0.254	0.248
Correlation of Earnings and Forecasts	0.640	0.606
Mean of Forecasts	0.895	0.893
Skewness of Forecasts	1.172	1.028
Slope of Borrowing	1.000	0.951

Table 11: Counterfactual Results

This table presents the simulated moments from the counterfactual exercises. I repeat the data and baseline model moments as reference. The 'No Short-Termism' column corresponds to a counterfactual where I set $\pi = 0$, and the 'No Analyst Forecasts' column corresponds to a counterfactual where analyst forecasts are entirely uninformative (the shock to the forecast has arbitrarily high variance ($\sigma_\xi > c$, where c is some arbitrarily large number). I use twenty simulated 40-year panels of 2,000 firms to construct these moments.

	Data	Model	No Short-Termism	No Analyst Forecasts
Mean of Earnings	0.741	0.737	1.903	1.649
Standard Deviation of Earnings	1.452	1.443	1.504	1.476
Skewness of Earnings	0.254	0.248	0.311	0.272
Correlation of Earnings and Forecasts	0.634	0.606	0.646	—
Mean of Forecasts	0.895	0.895	2.059	—
Skewness of Forecasts	1.172	1.028	1.074	—
Slope of Borrowing	1.000	0.951	0	—

The short-termism parameter, π, is calibrated to 0.0164.[45] It is therefore possible to generate sizable reactions to forecast shocks in the model without unrealistically high levels of short-termism; a π value of 0.0164 suggests managers are close to acting as though they do not face pressure to maximize the contemporaneous stock price.

The borrowing cost parameter, χ, is calibrated at 0.0235. This parameter does not have a real-world analogue, but the parameterization suggests that the cost of borrowing function can be relatively close to the costless case, yet still generate sensible distributions of earnings/forecasts.

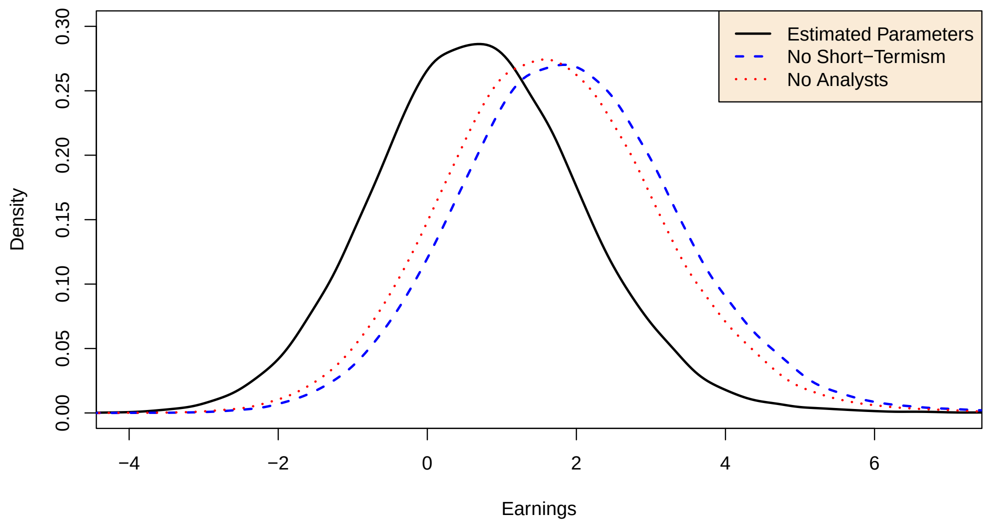
I calibrate the non-linearity parameter, ψ, at 0.6875. It is necessary for ψ to be greater than zero to generate skewness in the data, and to achieve responsiveness of earnings to forecast shocks. I find that a relatively high value for this parameter generates moments that match this data well.

The remaining calibrated parameters are chosen naturally to fit the associated moments, i.e. μ_ϵ to fit the mean of earnings, μ_ξ to fit the mean of forecasts, etc. All of these parameters take reasonable values that roughly coincide with the data moment analogues.

[45] One interpretation of short-termism in the model is as self-insurance against the risk of takeover. The empirical frequency of takeovers typically ranges from about 3-8% annually (Edmans et al. (2017)), so the calibrated value here is roughly in line with those estimates.

Figure 6: Counterfactual Exercises

In this figure, I plot the earnings distributions from simulated 40-year panels of 2,000 firms under the baseline, the 'No Short-Termism' counterfactual, and the 'No Analyst Forecasts' counterfactual. The baseline case is represented by a solid line, the 'No Short-Termism' by the dashed line, an the 'No Analyst Forecasts' by the dotted line.



Removing Short-Termism. The first counterfactual exercise is to set the short-termism parameter, π, to zero. This means managers do not face a short-term pressure to maximize the contemporaneous stock price. Table 11 shows the simulated moments: I find that the mean of earnings and forecasts increase, and the slope of borrowing falls to zero. Specifically, when $\pi = 0$, earnings increase by 0.80 standard deviations, compared to the baseline model. This increase in earnings/forecasts is a result of no inefficient borrowing when $\pi = 0$; $b_t = 0$ for all t, as can be seen from Equation 34. Other moments are mostly unchanged.

In Figure 6 I plot simulated distributions of earnings from the baseline model (solid black line) and of earnings from the counterfactual case where $\pi = 0$ (dashed blue line). Removing short-termism leads to a positive shift in earnings with a slight spread in the overall distribution. These findings indicate that short-termism results in a non-trivial cost in the form of lower earnings.

Removing Analyst Forecasts. In the model, analyst forecasts increase the incentive to manipulate. To gauge how significant this additional pressure is, I conduct a counterfactual exercise where I remove the analyst signal. I keep all other parameters the same as in the baseline model. Table 11 shows the simulated moments under this framework. I also plot the simulated distribution under this framework in Figure 6 as the dotted black line. Both the Table and the Figure confirm that the earnings distribution is closer to the counterfactual of no short-termism than it is to the baseline when analyst forecasts are removed.

7 Conclusion

This paper outlines a novel empirical approach that addresses key limitations in existing earnings manipulation research by leveraging brokerage mergers and analyst optimism to create exogenous variation in analyst forecasts. Unlike traditional 'bunching' methods reliant on endogenous thresholds, this instrumental variable design allows for precise causal estimation of forecast impacts on earnings across the distribution, beyond the 'just beat' region. The findings reveal a one-to-one, symmetric response of earnings to forecast changes, indicating that earnings manipulation aligns systematically with analyst expectations, not just in the case of proximity to forecast cutoffs. This relationship matches model predictions, suggesting that earnings responses emerge symmetrically from incentives to manage perceptions. This methodological contribution offers a robust framework for examining responses to forecast dynamics with broader applications in financial reporting analysis.

References

Almeida, H., Fos, V., and Kronlund, M. (2016). The real effects of share repurchases. *Journal of Financial Economics*, 119(1):168–185.

Armstrong, C. S., Larcker, D. F., Ormazabal, G., and Taylor, D. J. (2013). The relation between equity incentives and misreporting: The role of risk-taking incentives. *Journal of Financial Economics*, 109(2):327–350.

Bartov, E., Givoly, D., and Hayn, C. (2002). The rewards to meeting or beating earnings expectations. *Journal of Accounting and Economics*, 33(2):173–204.

Bergstresser, D. and Philippon, T. (2006). CEO incentives and earnings management. *Journal of Financial Economics*, 80(3).

Bhojraj, S., Hribar, P., Picconi, M., and McINNIS, J. (2009). Making sense of cents: An examination of firms that marginally miss or beat analyst forecasts. *The Journal of Finance*, 64(5):2361–2388.

Bonaime, A., Gulen, H., and Ion, M. (2018). Does policy uncertainty affect mergers and acquisitions? *Journal of Financial Economics*, 129(3):531–558.

Bradshaw, M. T. and Sloan, R. G. (2002). GAAP versus The Street: An empirical assessment of two alternative definitions of earnings. *Journal of Accounting Research*, 40(1):41–66.

Breuer, M. and Schütt, H. H. (2023). Accounting for uncertainty: An application of Bayesian methods to accruals models. *Review of Accounting Studies*, 28(2):726–768.

Brown, L. D. (2001). A temporal analysis of earnings surprises: Profits versus losses. *Journal of Accounting Research*, 39(2):221–241.

Brys, G., Hubert, M., and Struyf, A. (2004). A robust measure of skewness. *Journal of Computational and Graphical Statistics*, 13(4):996–1017.

Burgstahler, D. and Chuk, E. (2015). Do scaling and selection explain earnings discontinuities? *Journal of Accounting and Economics*, 60(1):168–186.

Carhart, M. M. (1997). On persistence in mutual fund performance. *The Journal of Finance*, 52(1):57–82.

Chen, J., Cumming, D., Hou, W., and Lee, E. (2016). Does the external monitoring effect of financial analysts deter corporate fraud in china? *Journal of Business Ethics*, 134:727–742.

Chen, T., Harford, J., and Lin, C. (2015). Do analysts matter for governance? Evidence from natural experiments. *Journal of Financial Economics*, 115(2):383–410.

Chetty, R., Friedman, J. N., Olsen, T., and Pistaferri, L. (2011). Adjustment costs, firm responses, and micro vs. macro labor supply elasticities: Evidence from Danish tax records. *The Quarterly Journal of Economics*, 126(2):749–804.

Chiang, C.-h., Dai, W., Fan, J., Hong, H., and Tu, J. (2019). Robust measures of earnings surprises. *The Journal of Finance*, 74(2):943–983.

Cornett, M. M., Marcus, A. J., and Tehranian, H. (2008). Corporate governance and pay-for-performance: The impact of earnings management. *Journal of Financial Economics*, 87(2):357–373.

Degeorge, F., Patel, J., and Zeckhauser, R. (1999). Earnings management to exceed thresholds. *The Journal of Business*, 72(1):1–33. Publisher: The University of Chicago Press.

Denis, D. J. and McKeon, S. B. (2021). Persistent negative cash flows, staged financing, and the stockpiling of cash balances. *Journal of Financial Economics*, 142(1):293–313.

Dobbie, W., Goldin, J., and Yang, C. S. (2018). The effects of pretrial detention on conviction, future crime, and employment: Evidence from randomly assigned judges. *American Economic Review*, 108(2):201–240.

Durtschi, C. and Easton, P. (2005). Earnings management? The shapes of the frequency distributions of earnings metrics are not evidence ipso facto. *Journal of Accounting Research*, 43(4):557–592.

Durtschi, C. and Easton, P. (2009). Earnings management? Erroneous inferences based on earnings frequency distributions. *Journal of Accounting Research*, 47(5):1249–1281.

Edmans, A., Fang, V. W., and Lewellen, K. A. (2017). Equity vesting and investment. *The Review of Financial Studies*, 30(7):2229–2271.

Erel, I., Liao, R. C., and Weisbach, M. S. (2012). Determinants of cross-border mergers and acquisitions. *The Journal of Finance*, 67(3):1045–1082.

Fama, E. F. and French, K. R. (2002). The equity premium. *The Journal of Finance*, 57(2):637–659.

Fama, E. F. and French, K. R. (2004). New lists: Fundamentals and survival rates. *Journal of Financial Economics*, 73(2):229–269.

Fang, V. W., Huang, A. H., and Karpoff, J. M. (2016). Short selling and earnings management: A controlled experiment. *The Journal of Finance*, 71(3):1251–1294.

Gibbons, B., Iliev, P., and Kalodimos, J. (2020). Analyst information acquisition via EDGAR. *Management Science*, 67(2):769–793.

Harasztosi, P. and Lindner, A. (2019). Who pays for the minimum wage? *American Economic Review*, 109(8):2693–2727.

Harford, J. (2005). What drives merger waves? *Journal of Financial Economics*, 77(3):529–560.

Hazarika, S., Karpoff, J. M., and Nahata, R. (2012). Internal corporate governance, CEO turnover, and earnings management. *Journal of Financial Economics*, 104(1):44–69.

He, J. J. and Tian, X. (2013). The dark side of analyst coverage: The case of innovation. *Journal of Financial Economics*, 109(3):856–878.

Hong, H. and Kacperczyk, M. (2010). Competition and Bias. *The Quarterly Journal of Economics*, 125(4):1683–1725.

Hong, H. and Kubik, J. D. (2003). Analyzing the analysts: Career concerns and biased earnings forecasts. *The Journal of Finance*, 58(1):313–351.

Jegadeesh, N. and Kim, W. (2010). Do analysts herd? An analysis of recommendations and market reactions. *The Review of Financial Studies*, 23(2):901–937.

Jensen, M. C. and Meckling, W. H. (1976). Theory of the firm: Managerial behavior, agency costs and ownership structure. *Journal of Financial Economics*, 3(4):305–360.

Jones, J. J. (1991). Earnings management during import relief investigations. *Journal of Accounting Research*, 29(2):193–228.

Karpoff, J., Lee, S., and Martin, G. (2008a). The consequences to managers for financial misrepresentation. *Journal of Financial Economics*, 88(2):193–215.

Karpoff, J., Lee, S., and Martin, G. (2008b). The cost to firms of cooking the books. *Journal of Financial and Quantitative Analysis*, 43(3):581–611.

Kelly, B. and Ljungqvist, A. (2012). Testing asymmetric-information asset pricing models. *The Review of Financial Studies*, 25(5):1366–1413.

Kleven, H. (2016). Bunching. *Annual Review of Economics*, 8(1):435–464.

Kleven, H. J. and Waseem, M. (2013). Using notches to uncover optimization frictions and structural elasticities: Theory and evidence from Pakistan. *The Quarterly Journal of Economics*, 128(2):669–723.

Kothari, S. P., So, E. C., and Verdi, R. S. (2016). Analysts' forecasts and asset pricing: A survey. SSRN Scholarly Paper.

Larson, C. R., Sloan, R., and Zha Giedt, J. (2018). Defining, measuring, and modeling accruals: A guide for researchers. *Review of Accounting Studies*, 23:827–871.

Lim, T. (2001). Rationality and analysts' forecast bias. *The Journal of Finance*, 56(1):369–385.

McElhoe, B. A. (1966). An assessment of the navigation and course corrections for a manned flyby of Mars or Venus. *IEEE Transactions on Aerospace and Electronic Systems*, (4):613–623.

Ottaviani, M. and Sørensen, P. N. (2006). The strategy of professional forecasting. *Journal of Financial Economics*, 81(2):441–466.

Philbrick, D. R. and Ricks, W. E. (1991). Using Value Line and IBES analyst forecasts in accounting research. *Journal of Accounting Research*, 29(2):397–417.

Richardson, S., Teoh, S. H., and Wysocki, P. D. (2004). The walk-down to beatable analyst forecasts: The role of equity issuance and insider trading incentives. *Contemporary Accounting Research*, 21(4):885–924.

Roychowdhury, S. (2006). Earnings management through real activities manipulation. *Journal of Accounting and Economics*, 42(3):335–370.

Saez, E. (2010). Do taxpayers bunch at kink points? *American Economic Journal: Economic Policy*, 2(3):180–212.

Seybert, N. (2010). R&D capitalization and reputation-driven real earnings management (Partially Retracted). *The Accounting Review*, 85(2):671–693.

Smith, G. L., Schmidt, S. F., and McGee, L. A. (1962). *Application of statistical filter theory to the optimal estimation of position and velocity on board a circumlunar vehicle*, volume 135. National Aeronautics and Space Administration.

So, E. C. (2013). A new approach to predicting analyst forecast errors: Do investors overweight analyst forecasts? *Journal of Financial Economics*, 108(3):615–640.

Stein, J. C. (1989). Efficient capital markets, inefficient firms: A model of myopic corporate behavior. *The Quarterly Journal of Economics*, 104(4):655–669.

Stock, J. H. and Yogo, M. (2002). Testing for weak instruments in linear IV regression. Working Paper 284, National Bureau of Economic Research. Series: Technical Working Paper Series.

Terry, S. J. (2023). The macro impact of short-termism. *Econometrica*, 91(5):1881–1912.

Wan, E. (2006). Sigma-point filters: An overview with applications to integrated navigation and vision assisted control. In *2006 IEEE Nonlinear Statistical Signal Processing Workshop*, pages 201–202. IEEE.

Yelowitz, A. S. (1995). The Medicaid notch, labor supply, and welfare participation: Evidence from eligibility expansions. *The Quarterly Journal of Economics*, 110(4):909–939.

Yu, F. F. (2008). Analyst coverage and earnings management. *Journal of Financial Economics*, 88(2):245–271.

A Proof of Theorem 1

Note that:

$$P_t = \sum_{j=1}^{\infty} (1+r)^{-j} \mathbb{E}_t[h(\alpha_{t+j})] + \text{ borrowing conjecture} \tag{A1}$$

Up to a first-order approximation,

$$\mathbb{E}_t[h(\alpha_{t+j})] = h(\mathbb{E}_t[\alpha_{t+j}])$$

We know that $\mathbb{E}_t[\alpha_{t+j}] = \mathbb{E}_t[\alpha_t], \forall j$ by Equation 25. Therefore:

$$P_t = \frac{1}{r} h(\mathbb{E}_t[\alpha_t]) + \text{borrowing conjecture}$$

$$\begin{aligned} \implies \frac{\partial P_t}{\partial b_t} &= \frac{1}{r} \frac{\partial h(\mathbb{E}_t[\alpha_t])}{\partial b_t} \\ &= \frac{1}{r} h'(\mathbb{E}_t[\alpha_t]) \frac{\partial \mathbb{E}_t[\alpha_t]}{\partial b_t} \end{aligned}$$

Application of the Extended Kalman Filter shows that:

$$\mathbb{E}_t[\alpha_t] = \mathbb{E}_{t-1}[\alpha_t] + K_t \left(\begin{bmatrix} e_t^n \\ \phi_t^n \end{bmatrix} - \mathbf{h}(\mathbb{E}_{t-1}[\alpha_t]) \right)$$

where, in general:

$$\underbrace{K_t}_{1\times 2} = P_{t|t-1} H_t^T S_t^{-1}$$

$$\underbrace{S_t}_{2\times 2} = H_t P_{t|t-1} H_t^T + R$$

$$\underbrace{H_t}_{2\times 1} = \left. \frac{\partial \mathbf{h}}{\partial \alpha} \right|_{\mathbb{E}_{t-1}[\alpha_t]}$$

$$\underbrace{R}_{2\times 2} = \begin{bmatrix} \sigma_\epsilon^2 & 0 \\ 0 & \sigma_\xi^2 \end{bmatrix}$$

$$\underbrace{P_{t|t-1}}_{1\times 1} = P_{t-1|t-1} + \sigma_\eta^2$$

$$\underbrace{P_{t|t}}_{1\times 1} = (1 - K_t H_t) P_{t|t-1}$$

Note that K_t becomes a constant only if H_t is constant. This occurs only when $h(.)$ is a linear function. It follows from the above that:

$$\frac{\partial \mathbb{E}_t[\alpha_t]}{\partial b_t} = K_{1,t} + K_{2,t}$$

So it follows that:

$$\begin{aligned}\frac{\partial P_t}{\partial b_t} &= \frac{1}{r} h'(\mathbb{E}_t[\alpha_t]) \left(K_{1,t} + K_{2,t}\right) \\ &= \frac{1}{r} K_t \mathbf{h}'(\mathbb{E}_t[\alpha_t])\end{aligned}$$

By the FOC of the manager's problem:

$$\begin{aligned}c'(b^*) &= \frac{1+r}{1-\pi}\left(1 + \pi \frac{\partial P_t}{\partial b_t}\right) \\ &= \frac{1+r}{1-\pi}\left(1 + \frac{\pi}{r} K_t \mathbf{h}'(\mathbb{E}_t[\alpha_t])\right)\end{aligned}$$

Supplementary Appendix for Online Publication Only

The following supplementary tables and figures are for Shore, "Living up to Analyst Expectations".

Table O.1: Placebo Test —First Stage Of Non-Exiting Analysts

This table shows results of estimating the first stage of the main analysis (Equation 8 in Section 4.1) using a placebo instrument. Rather than comparing the period before and after an analyst exits (as in the main specification, where I compare $t-1$ to t for analysts present in $t-1$ but absent in t), captured by $\partial AFE_{i,t}$, I instead compare the change in optimism between $t-2$ and $t-1$ for the same analysts, who remain active contributors to the forecast in both periods, $\partial AFE_{i,t}^{placebo}$.

Dependent Variable:	$\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
$\partial AFE_{i,t-1}^{placebo}$	-0.83**	-0.87**	-0.85**	-0.86**	-0.86**	-0.86**
	(0.38)	(0.39)	(0.39)	(0.39)	(0.39)	(0.39)
$\log(AT_{i,t-1})$	-0.18***	-0.16***	-0.11***	-0.11***	-0.11***	-0.12***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$\log(MTB_{i,t-1})$		0.12***	0.18***	0.18***	0.18***	0.18***
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$\log(Price_{i,t-1})$			-0.09***	-0.09***	-0.09***	-0.08***
			(0.01)	(0.01)	(0.01)	(0.01)
$\text{DVPS}_{i,t-1}$				-0.03***	-0.03***	-0.03***
				(0.01)	(0.01)	(0.01)
$\text{ROA}_{i,t-1}$					0.02	0.02
					(0.02)	(0.02)
$\text{Leverage}_{i,t-1}$						0.07
						(0.04)
Fixed-effects						
Firm	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	61,461	59,405	59,405	59,211	59,206	58,978
R^2	0.14	0.15	0.16	0.16	0.16	0.16
F-test (1st stage)	6.87	7.57	7.27	7.43	7.44	7.45

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table O.2: Relationship between Instrument and Firm Guidance

This table shows results of estimating the relationship between the constructed instrument, $\partial AFE_{i,t}$, on the change in firm-level guidance, $\Delta\mathbb{G}_{t-1}[EPS_{i,t}]$ (Column (1)), and the change in the range between the maximum and the minimum firm level guidance within a year, $\Delta\left(\mathbb{G}^{max}_{t-1}[EPS_{i,t}] - \mathbb{G}^{min}_{t-1}[EPS_{i,t}]\right)$. The instrument captures changes in the optimism composition of the set of analysts covering the firm.

Dependent Variables:	$\Delta\mathbb{G}_{t-1}[EPS_{i,t}]$	$\Delta\left(\mathbb{G}^{max}_{t-1}[EPS_{i,t}] - \mathbb{G}^{min}_{t-1}[EPS_{i,t}]\right)$
Model:	(1)	(2)
Variables		
$\partial AFE_{i,t}$	0.17	0.15
	(0.23)	(0.18)
$\log(AT_{i,t-1})$	-0.04***	0.00
	(0.01)	(0.00)
$\log(MTB_{i,t-1})$	0.03***	0.00
	(0.01)	(0.00)
$\log(Price_{i,t-1})$	0.02**	0.00
	(0.01)	(0.00)
$\text{DVPS}_{i,t-1}$	-0.01***	-0.00***
	(0.00)	(0.00)
$\text{ROA}_{i,t-1}$	-0.15***	0.00
	(0.04)	(0.01)
$\text{Leverage}_{i,t-1}$	-0.00	0.00
	(0.02)	(0.01)
Fixed-effects		
Firm	Yes	Yes
Year	Yes	Yes
Fit statistics		
Observations	64,814	52,757
R^2	0.10	0.99

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table O.3: Earnings Response in Levels

This table shows results from the first and second stage regression of the IV estimation using levels rather than changes of the forecast and earnings. Regression outputs come from estimating Equations 7 and 9 but taking the level rather than the change in the observation. I use the instrument that I describe in Section 3, $\partial AFE_{i,t}$, that captures the change in the optimism of analyst coverage. The levels of the consensus forecast and earnings are standardized.

Dependent Variables:	$\mathrm{F}_{t-1}[EPS_{i,t}]$	$\mathrm{EPS}_{i,t}$
IV stages	First	Second
Model:	(1)	(2)
Variables		
$\partial AFE_{i,t-1}$	1.08^{***}	
	(0.34)	
$\mathrm{F}_{t-1}[EPS_{i,t}]$		1.42^{***}
		(0.42)
Controls	Yes	Yes
Fixed-effects		
Firm	Yes	Yes
Year	Yes	Yes
Fit statistics		
Observations	66,782	66,782
R^2	0.32	0.00
F-test (1st stage)	13.45	
F-test (1st stage), $\mathrm{F}_{t-1}[EPS_{i,t}]$		13.45
Wu-Hausman, p-value		0.00

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table O.4: Earnings Response in Dollar Amounts

This table shows results from the second stage regression of the IV estimation using the dollar value of forecasts and earnings rather than scaled values. Regression outputs come from estimating Equations 7 and 9. I use the instrument that I describe in Section 3.

Dependent Variable:			$\Delta EPS^{\$}_{i,t}$			
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
$\Delta\mathbb{F}_{t-1}[EPS^{\$}_{i,t}]$	1.39***	1.44***	1.44***	1.44***	1.44***	1.44***
	(0.44)	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)
$\log(AT_{i,t-1})$	-0.06	-0.07	0.10***	0.09***	0.10***	0.07*
	(0.06)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)
$\log(MTB_{i,t-1})$		-0.08	0.14*	0.14*	0.13*	0.11
		(0.05)	(0.07)	(0.07)	(0.07)	(0.07)
$\log(Price_{i,t-1})$			-0.33***	-0.34***	-0.33***	-0.30***
			(0.03)	(0.03)	(0.03)	(0.03)
$\text{DVPS}_{i,t-1}$				0.03*	0.03*	0.03
				(0.02)	(0.02)	(0.02)
$\text{ROA}_{i,t-1}$					-0.08	-0.08
					(0.06)	(0.06)
$\text{Leverage}_{i,t-1}$						0.27***
						(0.06)
Fixed-effects						
Firm	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	70,309	67,930	67,930	67,697	67,687	67,421
R^2	0.07	0.05	0.06	0.06	0.07	0.07
F-test (1st stage), $\Delta\mathbb{F}_{t-1}[EPS^{\$}_{i,t}]$	24.36	27.16	27.21	26.92	26.91	27.11
Wu-Hausman, p-value	0.02	0.01	0.01	0.01	0.01	0.01

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table O.5: Tests for Discretionary Accruals

This table presents my findings from an IV estimation exercise of the impact of a consensus forecast shock on discretionary accruals ($ACC_{i,t}^{disc}$). I use the non-discretionary accruals estimated in Breuer and Schütt (2023), $ACC_{i,t}^{non-disc}$, to back out discretionary accruals from total accruals, $ACC_{i,t}$. These are constructed using a Bayesian estimation method that incorporates parameter and model uncertainty into the estimation of normal accruals. I express all variables in per-share terms, and scale by the firm-level standard deviation of earnings. I control for a firm and year fixed effect, plus the same set of controls that I include in my estimation of the Earnings effect in Table 5.

Dependent Variables:	$\Delta ACC_{i,t}$	$\Delta ACC_{i,t}^{disc}$	$\Delta ACC_{i,t}^{non-disc}$
Model:	(1)	(2)	(3)
Variables			
$\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	1.71**	1.50**	0.21
	(0.77)	(0.76)	(0.22)
Controls	Yes	Yes	Yes
Fixed-effects			
Firm	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	28,175	28,175	28,175
R^2	0.07	0.06	0.14
F-test (1st stage), $\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	30.29	30.29	30.29
Wu-Hausman, p-value	0.12	0.14	0.89

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table O.6: Tests for Real Activities-Based Earnings Management

This table presents findings from an additional set of tests of real activities earnings management that are prominent in the literature, using the main IV approach. Here $XSGA$ is selling and general expenses, XRD is research and development expense, and $RECD$ is the provision of bad debt. For the provision of bad debt, I take a double difference to ensure that the measure captures the change in the flow rather than stock. I also include the main earnings result in column (1).

Dependent Variables:	$\Delta EPS_{i,t}$	$\Delta XRD_{i,t}$	$\Delta XSGA_{i,t}$	$\Delta RECD_{i,t}$
Model:	(1)	(2)	(3)	(4)
Variables				
$\Delta \mathbb{F}_{t-1}[EPS_{i,t}]$	1.07***	0.05	0.16	-0.08
	(0.27)	(0.09)	(0.19)	(0.06)
Controls	Yes	Yes	Yes	Yes
Fixed-effects				
Firm	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	54,552	32,587	48,138	34,108
R^2	0.15	0.10	0.26	0.01
F-test (1st stage), $\Delta \mathbb{F}_{t-1}[EPS_{i,t}]$	37.40	24.50	29.34	22.20
Wu-Hausman, p-value	0.01	0.79	0.64	0.25

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table O.7: Tests for Share Repurchasing Earnings Management

This table presents findings from an additional test of share-repurchasing as earnings management, as identified in Almeida et al. (2016), using the main IV approach. Here $PRSTKC$ is the Compustat item for the purchasing of common stock, and $CSHO$ is the Compustat item for the number of common shares outstanding. I also include the main earnings result in column (1).

Dependent Variables:	$\Delta EPS_{i,t}$	$\Delta PRSTKC_{i,t}$	$\Delta\Delta CSHO_{i,t}$
Model:	(1)	(2)	(3)
Variables			
$\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	1.07***	0.15	-0.09
	(0.27)	(0.19)	(0.09)
Controls	Yes	Yes	Yes
Fixed-effects			
Firm	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	54,552	49,361	48,069
R^2	0.15	0.31	0.13
F-test (1st stage), $\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	37.40	37.02	34.03
Wu-Hausman, p-value	0.01	0.73	0.47

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table O.8: Reversion of Earnings Response

This table shows findings on the reversion of the earnings response the year after the forecast shock ($\Delta EPS_{i,t+1}$). As the response is driven by accruals, and accruals typically revert, we should see a negative coefficient the year after the shock. I use the same controls for both specifications, but adjust controls forward a year in Column (2) to align in year with the dependent variable.

Dependent Variables:	$\Delta EPS_{i,t}$	$\Delta EPS_{i,t+1}$
Model:	(1)	(2)
Variables		
$\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	1.05***	-0.55**
	(0.28)	(0.22)
Fixed-effects		
Firm	Yes	Yes
Year	Yes	Yes
Fit statistics		
Observations	66,782	58,978
R^2	0.15	0.08
F-test (1st stage), $\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	32.16	33.04
Wu-Hausman, p-value	0.01	0.14

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table O.9: Earnings Response and 'Short-Termism'—First Stage

This table presents the results of the first stage of the exercise in Section 4.3, which tests whether the earnings response is driven by short-termism. The variable $LTIP_i$ is the average cash value of long-term incentive plans for executive directors at firm i, scaled by the average market value of firm i. In Column (1) I show the first stage for the sample of firms whose $LTIP_i$ is below the median value (m^{LTIP}), and in Column (2) I show the first stage for firms above the median value. In Columns marked (3), I show the two first stage components for the analysis on the entire sample when I include an indicator term for firms that are above the median ($\mathbb{I}\{LTIP_i > m^{LTIP}\}$).

Dependent Variables:	$\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$		$\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	$\Delta\mathbb{F}_{t-1}[EPS_{i,t}] : \mathbb{I}\{LTIP_i > m^{LTIP}\}$
Model:	(1)	(2)	(3)	(3)
Variables				
$\partial AFE_{i,t-1}$	1.40***	1.24*	1.36***	-0.09
	(0.53)	(0.70)	(0.53)	(0.09)
$\partial AFE_{i,t-1} \times$ I{LTIP$_i > m^{LTIP}$}			-0.07	1.55**
			(0.88)	(0.73)
Fixed-effects				
Firm	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	19,061	15,770	34,831	34,831
R^2	0.12	0.11	0.11	0.08
F-test (1st stage)	12.38	3.15	7.02	4.65

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table O.10: Earnings Response and 'Short-Termism'

This table presents the results of the second stage of the exercise in Section 4.3, which tests whether the earnings response is driven by short-termism. The variable $LTIP_i$ is the average cash value of long-term incentive plans for executive directors at firm i, scaled by the average market value of firm i. In Panel A, I include summary statistics of firms that are both below (Column 'No') and above (Column 'Yes') the median value of $LTIP_i$, labelled m^{LTIP}. In Panel B, I present the second stage results: in Column (1) I show the second stage for the sample of firms whose $LTIP_i$ is below the median value (m^{LTIP}), and in Column (2) I show the second stage for firms above the median value. In Column (3) I show the second stage for the analysis on the entire sample, including an indicator term for firms that are above the median ($\mathbb{I}\{LTIP_i > m^{LTIP}\}$).

Panel A: Summary Statistics

Above Median LTIP	No			Yes			
Variable	N	Mean	SD	N	Mean	SD	Test
$\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	19,690	0.02	0.98	16,448	0.01	1.00	F= 2.32
Total Assets ($m)	20,761	24,451.00	87,032.00	17,471	4,074.00	19,357.00	F= 919.43***
Market Value ($m)	20,755	16,518.00	46,192.00	17,468	2,384.00	5,207.00	F= 1617.98***

Statistical significance markers: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Panel B:

Dependent Variable:		$\Delta EPS_{i,t}$	
Model:	(1)	(2)	(3)
Variables			
$\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	1.11**	0.31	1.05**
	(0.48)	(0.73)	(0.46)
$\Delta\mathbb{F}_{t-1}[EPS_{i,t}] : \mathbb{I}\{LTIP_i > m^{LTIP}\}$			-0.57
			(0.72)
Controls	Yes	Yes	Yes
Fixed-effects			
Firm	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	19,061	15,770	34,831
R^2	0.14	0.27	0.22
F-test (1st stage), $\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$	12.38	3.15	7.02
F-test (1st stage), $\Delta\mathbb{F}_{t-1}[EPS_{i,t}] : \mathbb{I}\{LTIP_i > m^{LTIP}\}$			4.65
Wu-Hausman, p-value	0.19	0.71	0.47

Clustered (Firm) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Figure O.1: Distribution of First Stage Coefficients in Random Treatment Assignment

This figure shows the distribution of first-stage coefficients of the relationship between the instrument, $\partial AFE_{i,t}$, and the change in the consensus forecast, $\Delta\mathbb{F}_{t-1}[EPS_{i,t}]$, when the analyst exit is randomly assigned instead of assigned via brokerage induced analyst exits. I construct 10,000 samples, where I randomly assign exits to analysts, rather than identifying exits using brokerage mergers. I then estimate the relationship between the change in the optimism of analyst coverage induced by 'pseudo' exits and the change in the consensus forecast. I include a dashed, vertical, red line to indicate the coefficient in the main analysis when brokerage merger induced exits are used to construct the instrument (1.76).

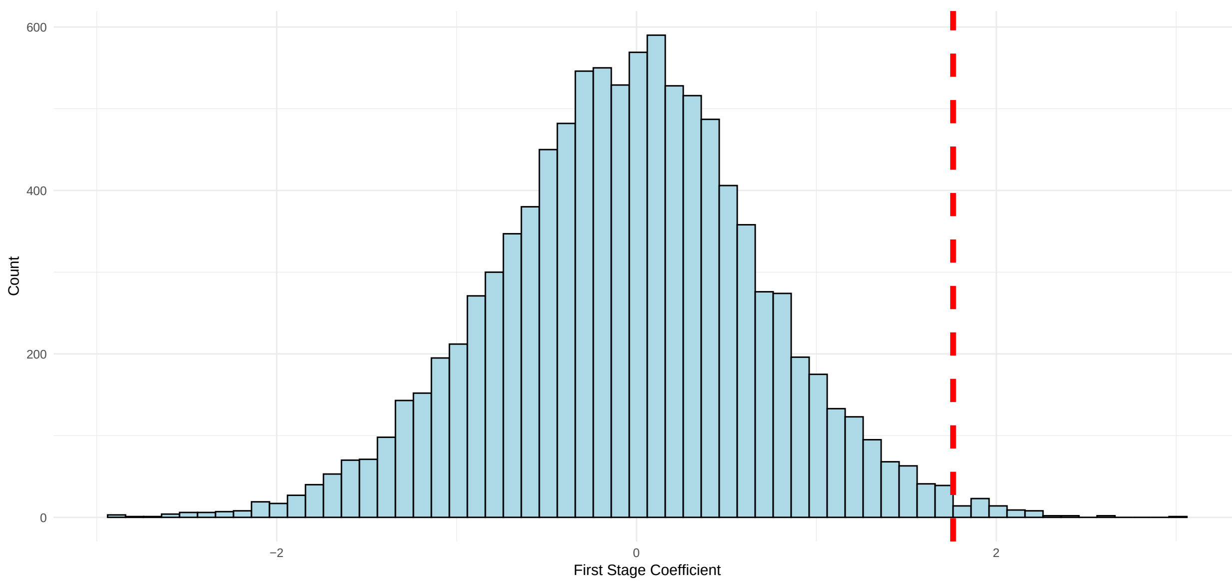


Figure O.2: Histograms of Distributions of Exiting v.s Non-Exiting Analysts

In this figure, I plot histograms of the mean squared error, mean error, and analyst fixed effects (AFE) of analysts working at brokerages that experience a merger. I plot two separate histograms for analysts who exit in the wake of the merger (blue), and those that do not (red).