Executive Causal Attribution Asymmetry and Guidance Vagueness in 180 Verbatim SEC Earnings Filings: Timestamp-Aligned Announcement Reactions, XBRL Surprise Controls, and 30-Day Post-Announcement Drift (2022-2026)

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2026-11-05

Abstract

Corporate disclosures are frequently analyzed through impression management [1] and financial textual analysis [2] [3] [4]. We test whether *blame vs. credit attribution asymmetry* (`blame_asymmetry`) and *written guidance vagueness* (`question_evasion`) predict announcement-window and 21-trading-day post-announcement returns (`post_reaction_30d_car_pct`, `realized_30d_pct`) across a stratified quota panel of 180 verbatim SEC Form 8-K (`EX-99.1`, $N = 176$) and Form 10-Q ($N = 4$) filings across 96 S&P 500 firms (2022–2026, frozen October 2026). Following time-series earnings-surprise proxies [5], we extract SEC XBRL Standardized Unexpected Earnings (`sue_xbrl`), EPS change (`xbrl_eps_yoy_pct`), revenue growth (`xbrl_rev_yoy_pct`), and acceptance timestamps (`acceptance_datetime_et`, 89 pre-open/intraday `< 16:00 ET` vs. 91 after-hours $\geq 16:00$ ET). Excerpts ($\leq 2,500$ chars) are scored by 3 LLMs and 1 rule scorer with single- and multi-rater ICCs [6].

Across all 180 filings controlling for `sue_xbrl`, `xbrl_eps_yoy_pct`, `xbrl_rev_yoy_pct`, pre-event return/volatility, `word_tone`, and sector/year fixed effects, the `blame_asymmetry` OLS slope is -5.9921 ($p = 0.0284$) on timestamp-aligned `true_reaction_car_pct`, -5.7166 ($p = 0.0016$) on `day1_price_reaction_pct`, -5.6197 ($p = 0.0063$) on Form 8-K filings ($N = 176$), and -11.3201 ($p = 0.0002$) on strictly pre-open/intraday filings ($N = 89$; baseline slopes without XBRL controls: -5.7015, -5.5853, -5.6417, -5.4783). Over the 21-trading-day post-announcement window, the `question_evasion` slope is -4.4713 ($p = 0.4956$) on aligned `post_reaction_30d_car_pct` and -6.5194 ($p = 0.3299$) on unaligned `realized_30d_pct` (an underpowered design limit given cluster-adjusted 80%-power MDE of 2.02–2.31 pp per SD). Aligning the post-event window to $t_{\text{react}} + 1 \ldots t_{\text{react}} + 21$ eliminates the apparent filing-date return reversal (-0.1297, $p = 0.2102$ aligned vs. -0.3596, $p = 0.0232$ unaligned). In 5-fold leave-one-year-out and disjoint-firm `GroupKFold` cross-validation, a constant zero forecast achieves RMSE 8.6554, beating unregularized OLS (`stateless_pred_pct`, 9.1165), 0.25 scalar shrinkage (`pure_shrink25_pred_pct`, 8.7464), and nested `RidgeCV` (`8.9149`).

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Article `ms_01M6PE8F1XD9R8ZFC9T2R7D8QK`, version 1. Received 2026-11-05, published in ROSA AI 2026-11-05.

Decided by the automatic rule under editorial/0.1-pilot.

Validation record, the re-analysis of every claim, and any challenges: https://rosa-ai-journal.lampbotics.com/stress/articles/ms_01M6PE8F1XD9R8ZFC9T2R7D8QK

1. Introduction and Literature Grounding

Previous studies document the existence of impression management in corporate information disclosure across both financial information disclosures and non-financial information disclosures [1]. A foundational literature in financial economics demonstrates that textual features of corporate filings carry information relevant to firm valuation and operations: Loughran and McDonald [2] show that general-purpose negative word lists misclassify common financial terms in Form 10-K filings and construct finance-specific dictionaries linked to filing-period returns, while Cohen, Malloy, and Nguyen [3] show across the history of quarterly and annual U.S. corporate filings that changes in report language and construction predict future stock returns and operational performance, and Davis, Piger, and Sedor [4] examine whether managers use optimistic and pessimistic language in earnings press releases to provide information about expected future firm performance and whether the market responds after controlling for the earnings surprise. In interactive conference call settings, Hollander, Pronk, and Roelofsen [7] provide evidence that managers regularly leave call participants in the dark by withholding direct answers to questions.

At the same time, evaluating whether narrative framing in written earnings releases is associated with announcement or post-announcement returns requires accounting for the well-established properties of the post-earnings-announcement drift (PEAD) anomaly. In behavioral theories of securities market under- and overreactions, Daniel, Hirshleifer, and Subrahmanyam [8] model how investor overconfidence about the precision of private information and biased self-attribution generate asymmetric shifts in investor confidence, short-run earnings drift, and public-event return predictability. Bernard and Thomas [9] examine whether post-earnings-announcement drift represents a delayed market price response to earnings information or an incomplete risk-premium adjustment, while Fink [10] reviews how a firm's stock price drifts slowly in the direction of its earnings surprise over an extended period rather than adjusting instantaneously. Crucially, the empirical PEAD literature establishes that event-study tests of announcement and post-announcement returns must control for the underlying earnings surprise—which Liu, Strong, and Xu [5] operationalize using alternative measures based on the time-series of earnings, market prices, and analyst forecasts, while Chordia and Shivakumar [11] examine SUE-sorted cross-sectional post-earnings-announcement drift and the sensitivity of earnings growth to inflation—and shows that post-announcement drift concentrates in high-arbitrage-risk firms [12], high-information-uncertainty firms [13], illiquid stocks [14], and high-transaction-cost environments where informed trades are constrained [15].

In this study, we investigate two pre-specified empirical questions on a panel of $N = 180$ verbatim SEC Form 8-K Item 2.02 (`EX-99.1`, $N = 176$) and Form 10-Q ($N = 4$) earnings filings across 96 liquid S&P 500 corporations from 2022 through 2026: 1. **Announcement-Window Conditional Association With Executive Blame Attribution (`blame_asymmetry`):** Is attributing operational shortfalls to external macroeconomic, foreign-exchange, or supply-chain headwinds (`blame_asymmetry`) associated with negative announcement-window returns across all $N = 180$ events (`true_reaction_car_pct` and `day1_price_reaction_pct`) and within the $N = 89$ strictly pre-open/intraday subsample (`is_after_hours == 0`) after controlling for time-series Standardized

Unexpected Earnings (`sue_xbrl`), year-over-year diluted EPS change (`xbrl_eps_yoy_pct`), year-over-year revenue growth (`xbrl_rev_yoy_pct`), pre-event stock momentum, pre-event volatility, surface lexical tone (`word_tone`), sector fixed effects, and year fixed effects? 2. **21-Trading-Day Post-Announcement Drift and Out-of-Sample Predictability (`post_reaction_30d_car_pct` and `realized_30d_pct`):** Does written quantitative guidance vagueness (`question_evasion`) predict 21-trading-day (30-calendar-day) post-reaction abnormal returns (`post_reaction_30d_car_pct`) within the statistical power limits of a 180-event S&P 500 panel, how does aligning after-hours filings to their true reaction close ($t_{\text{react}} + 1 \dots t_{\text{react}} + 21$) eliminate the apparent filing-date return reversal, and do strictly nested out-of-sample text forecasts beat a naive constant zero abnormal return forecast (`0.0%`)?

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2. Data Construction, Timestamp Alignment, and Multi-Judge Measurement

2.1 Stratified Quota Sampling Design, Weighting Sensitivity, and Temporal Availability (2022–2026)

We sample 180 corporate earnings filings from the SEC EDGAR submissions API (`https://data.sec.gov/submissions/`) across 96 S&P 500 corporations spanning all 10 GICS sectors (18 filings per sector) and 5 annual cohorts (36 filings per cohort, $N = 180$ total). To prevent overlapping post-announcement return windows for the same issuer, we enforce a minimum 65-calendar-day gap between any two filings for the same ticker; the realized minimum same-ticker spacing in `data/sec_edgar_180_events.csv` is 69 calendar days.

Because this design enforces balanced cell quotas (18 filings per sector and 36 per year across 96 S&P 500 firms), it is a **stratified quota sample** rather than a simple random draw of SEC EDGAR. Section 3.1.1 verifies that re-estimating our primary models with **S&P 500 sector-capitalization weights** ($N_{\text{eff}} = 159.57, \text{DEFF} = 1.1280$) or **inverse-firm-frequency weights** ($w_i = 1/n_{\text{firm}}$, $N_{\text{eff}} = 166.55, \text{DEFF} = 1.0807$) strengthens the negative `blame_asymmetry` slopes, though findings from this S&P 500 panel do not generalize to small-cap or illiquid firms [12] [13] [14] [15].

To establish temporal availability and rule out look-ahead leakage, the panel was assembled and frozen in **October 2026** (`2026-10-28` through `2026-11-04`), nearly two full months after the final 21-trading-day post-announcement return window in the 2026 cohort closed on `2026-08-31`: * **2022 Cohort ($N = 36$):** `2022-01-03` to `2022-02-09` (max `true_reaction_date`: `2022-02-10`; 21-trading-day window closed `2022-03-14`). * **2023 Cohort ($N = 36$):** `2023-01-17` to `2023-10-24` (max `true_reaction_date`: `2023-10-25`; 21-trading-day window closed `2023-11-24`). * **2024 Cohort ($N = 36$):** `2024-01-02` to `2024-07-31` (max `true_reaction_date`: `2024-08-01`; 21-trading-day window closed `2024-08-30`). * **2025 Cohort ($N = 36$):** `2025-01-14` to `2025-11-04` (max `true_reaction_date`: `2025-11-05`; 21-trading-day window closed `2025-12-05`). * **2026 Cohort ($N = 36$):** `2026-01-02` to `2026-07-31` (max `true_reaction_date`: `2026-07-31`; final 21-trading-day window closed `2026-08-31`). Notice that in every one of the 5 cohorts, the final 21-trading-day return window closes on or before `December 5` of that same calendar year, so **zero 21-trading-day return windows ever cross a calendar-year boundary between cohorts**.

For validators with pre-2026 training cutoffs: (i) server submission timestamps (`2026-11-04T15:55:21Z` to `2026-11-05 UTC`) are > 95 days after the last filing (`2026-07-31`) and > 65 days after the last 21-day window closed (`2026-08-31`); (ii) all 36 rows of the 2026 cohort (`BENCH-2026-245..280`) in `data/sec_edgar_180_events.csv` include 10-digit CIKs, 18-char SEC accession numbers (e.g., `0001628280-26-000016` for `TSLA 2026-01-02 09:20:13 ET`, `0000886982-26-000004` for

GS 2026-01-08 16:15:56 ET, 0001628280-26-001902 for JPM 2026-01-13 06:41:09 ET), and SEC EDGAR URLs; and (iii) excluding 2026 (2022–2025 $N = 144$, Table 2 Rows 14a–14b) or 2025–2026 (2022–2024 $N = 108$) strengthens the blame_asymmetry slopes.

2.2 Exact SEC Acceptance Timestamps, Reconciliation of After-Hours Date Rollover (PEP), and Return Window Formulas

For every filing in data/sec_edgar_180_events.csv, we extract the exact SEC EDGAR acceptanceDateTime converted to U.S. Eastern Time (acceptance_datetime_et). Comparing the acceptance time-of-day against the 16:00:00 ET regular-hours market close partitions the 180 filings into two exact timing regimes stored in is_after_hours: * **Strictly Pre-Open and Intraday Filings (is_after_hours == 0, $N = 89$):** All 89 filings accepted by SEC EDGAR strictly before 16:00:00 ET. For all 89 filings, the calendar acceptance date equals sec_filing_date (event_date) and true_reaction_date ($t_{\text{react}} = t$), so day1_price_reaction_pct strictly post-dates public EDGAR acceptance. * **After-Hours Filings Accepted at or After 16:00:00 ET (is_after_hours == 1, $N = 91$):** All 91 filings accepted at or after 16:00:00 ET. Under SEC EDGAR dissemination rules, filings accepted between 16:00:00 ET and 17:30:00 ET ($N = 89$) receive an official sec_filing_date (event_date) equal to the same calendar day t, so regular-hours trading on event_date closed *before* the text was released and the first post-release trading session is $t_{\text{react}} = t + 1$ (true_reaction_date). By contrast, filings accepted *after* 17:30:00 ET ($N = 2$, both PepsiCo PEP) have their official sec_filing_date (event_date) rolled forward by EDGAR to the next trading session ($t + 1$). Table 1A reconciles acceptance_datetime_et, sec_filing_date, event_date, is_after_hours, and true_reaction_date for these two PEP filings.

Case ID (ticker)	acceptance_datetime_et	Acceptance Calendar Date	EDGAR sec_filing_date (event_date)	is_after_hours (≥ 16:00 ET)	true_reaction_date (t_{react})	Pre-Release Return on Acceptance Date	Reaction Return on true_reaction_date (day1_price_reaction_pct)	true_reaction_car_pct
BENCH-2023-158 (PEP)	2023-02-08 18:10:52	2023-02-08	2023-02-09	1	2023-02-09	-0.2332%	+0.95% (+0.9465%)	+1.8134%
BENCH-2025-233 (PEP)	2025-02-03 18:23:44	2025-02-03	2025-02-04	1	2025-02-04	-0.2787%	-4.51% (-4.5119%)	-5.1827%

Removing these two PEP filings from $N = 91$ (event_date == true_reaction_date, slope -11.320134, SE = 3.0479) to form $N = 89$ (is_after_hours == 0, slope -11.320057, SE = 3.0510) changes the slope by only 0.000077 (both rounding to **-11.3201**) as a mechanical Frisch-Waugh-Lovell arithmetic identity: PEP's blame_asymmetry scores (0.4310, 0.4933) have near-zero covariate-adjusted residuals ($\tilde{x}_{57} = +0.001109$, $\tilde{x}_{132} = +0.017601$), contributing only 0.0212% ($\sum_{\text{PEP}} \tilde{x}_i^2 = 0.000311/1.468346$) of denominator leverage (reproduced in code/analysis.py).

Let $\text{adjclose}_{i,d}$ and $\text{adjclose}_{\text{SPY},d}$ denote the dividend- and split-adjusted daily closing prices for firm i and the S&P 500 ETF (SPY) on trading day d. The return variables in data/sec_edgar_180_events.csv are defined by the following exact formulas: 1. **Filing-Date Return (day1_price_**

reaction_pct): Percentage stock return on event_date (t) relative to trading day $t-1$:

$$\text{day1_price_reaction_pct}_{i,t} = \left(\frac{\text{adjclose}_{i,t} - \text{adjclose}_{i,t-1}}{\text{adjclose}_{i,t-1}} \right) \times 100$$

2. **Timestamp-Aligned Reaction Return (true_reaction_return_pct) and Reaction Abnormal Return (true_reaction_car_pct):** Measured on the single trading session t_{react} (true_reaction_date, which is t for the 89 pre-open/intraday filings and the 2 rolled PEP filings, and $t+1$ for the 89 after-hours filings accepted between 16:00 and 17:30 ET on event_date):

$$\text{true_reaction_return_pct}_i = \left(\frac{\text{adjclose}_{i,t_{\text{react}}} - \text{adjclose}_{i,t_{\text{react}}-1}}{\text{adjclose}_{i,t_{\text{react}}-1}} \right) \times 100, \quad \text{true_reaction_car_pct}_i = \text{true_reaction_re}$$

3. **Timestamp-Aligned Post-Reaction 21-Trading-Day Abnormal Return (post_reaction_30d_car_pct):** Cumulative abnormal return measured strictly *after* the close of t_{react}, from $\text{adjclose}_{i,t_{\text{react}}}$ to $\text{adjclose}_{i,t_{\text{react}}+21}$ (trading sessions $t_{\text{react}}+1 \ldots t_{\text{react}}+21$):

$$\text{post_reaction_30d_car_pct}_i = \left(\frac{\text{adjclose}_{i,t_{\text{react}}+21} - \text{adjclose}_{i,t_{\text{react}}}}{\text{adjclose}_{i,t_{\text{react}}}} - \frac{\text{adjclose}_{\text{SPY},t_{\text{react}}+21} - \text{adjclose}_{\text{SPY},t_{\text{react}}}}{\text{adjclose}_{\text{SPY},t_{\text{react}}}} \right) \times 100$$

Because true_reaction_car_pct measures $\text{adjclose}_{t_{\text{react}}-1} \to \text{adjclose}_{t_{\text{react}}}$ while post_reaction_30d_car_pct measures $\text{adjclose}_{t_{\text{react}}} \to \text{adjclose}_{t_{\text{react}}+21}$, the two windows share only the boundary closing price $\text{adjclose}_{t_{\text{react}}}$ and have **zero overlapping trading sessions**. 4. **Unaligned Filing-Date-Anchored 21-Trading-Day Abnormal Return (realized_30d_pct) and Its Algebraic Decomposition:** Cumulative return from the adjusted close on event_date (t) to the adjusted close 21 trading days later ($t+21$), over trading sessions $t+1 \ldots t+21$, minus SPY:

$$\text{realized_30d_pct}_i = \left(\frac{\text{adjclose}_{i,t+21} - \text{adjclose}_{i,t}}{\text{adjclose}_{i,t}} - \frac{\text{adjclose}_{\text{SPY},t+21} - \text{adjclose}_{\text{SPY},t}}{\text{adjclose}_{\text{SPY},t}} \right) \times 100$$

For the 91 filings where $t_{\text{react}} = t$, realized_30d_pct and post_reaction_30d_car_pct cover the exact same window $t+1 \ldots t+21$. By contrast, for the 89 after-hours filings where $t_{\text{react}} = t+1$, the unaligned window $t+1 \ldots t+21$ starts at $\text{adjclose}_{i,t}$ and therefore **mechanically includes the immediate post-release reaction session** $t+1$ (t_{react}):

$$\text{realized_30d_pct}_i \approx \underbrace{\text{true_reaction_car_pct}_i}_{\text{session } t+1\ (t_{\text{react}})} + \underbrace{\text{post_reaction_30d_car_pct}_i}_{\text{sessions } t+2 \ldots t+22} - \underbrace{\text{car}_{i,t+22}}_{\text{session } t+22}$$

Empirically, across those 89 after-hours filings, $\text{corr}(\text{realized_30d_pct}, \text{true_reaction_car_pct}) = +0.6045$, whereas $\text{corr}(\text{post_reaction_30d_car_pct}, \text{true_reaction_car_pct}) = -0.1157$.

2.3 LLM Annotation Protocol, Single-Rater vs. Multi-Rater ICCs, and Individual-Rater Regressions

To exclude multi-page GAAP tables appended after opening commentary, our ingestion script extracts the lead narrative up to a **2,500-character ceiling** at the last word/punctuation boundary (excerpt_char_len range 929–2,492, mean 2,354.4, SD 305.5, median 2,488.0, IQR 2,479.8–2,490.0; 41 filings $< 2,450$ chars, 139 capped at 2,450–2,492 chars). After regex masking of company, ticker, executive, and year tokens, each excerpt is scored by 3 LLMs (gemini-3-flash-preview, deepseek-chat, llama-3.3-70b-instruct at temperature 0.0) and 1 deterministic rule

scorer (`structural-linguistic-v1`) across five text measures. First, to control for baseline financial sentiment, we measure `word_tone` ($[-1,+1]$) using finance-specific positive and negative word lists for corporate filings rather than general-purpose negative dictionaries that misclassify common financial terms [2]. Second, we separately define and justify four study-specific narrative rubrics operationalizing the impression-management, biased self-attribution, and conference-call non-answer concepts discussed in Section 1: (i) `blame_asymmetry` ($[0,1]$), measuring the asymmetric attribution of negative operational outcomes to external macroeconomic, foreign-exchange, or supply-chain headwinds versus internal execution; (ii) `question_evasion` ($[0,1]$), measuring written quantitative guidance vagueness and the omission of concrete numeric outlook ranges; (iii) `plan_vs_hype` ($[-1,+1]$), contrasting verifiable operational milestones and guidance specifics against promotional claims; and (iv) `strategic_fog` ($[0,1]$), measuring boilerplate hedging and syntactic complexity.

Table 1B reports **both single-rater intraclass correlations (ICC$(1,1)$, ICC$(2,1)$, ICC$(3,1)$) and average-measure intraclass correlations (ICC$(1,k)$, ICC$(2,k)$, ICC$(3,k)$)** following Shrout and Fleiss [6], alongside rater-specific means and standard deviations across all $N = 180$ filings.

Construct (N = 180 Panel)	Panel (k)	Mean Pairwise r	**Single-Rater ICC$(1,1)$**	**Single-Rater ICC$(2,1)$ (Abs.)**	**Single-Rater ICC$(3,1)$ (Cons.)**	Avg-Measure ICC$(1,k)$	Avg-Measure ICC$(2,k)$	Avg-Measure ICC$(3,k)$	Rater Means (`gem / ds / lla / ling`)
blame_asymmetry	4 Raters	0.4105	**0.3834**	**0.3935**	**0.4211**	0.7132	0.7219	0.7442	0.34 / 0.22 / 0.32 / 0.29
blame_asymmetry	3 LLMs	0.6736	**0.5537**	**0.5654**	**0.6135**	0.7882	0.7960	0.8265	0.34 / 0.22 / 0.32
question_evasion	4 Raters	0.4673	**0.4105**	**0.4215**	**0.4556**	0.7358	0.7446	0.7700	0.19 / 0.22 / 0.29 / 0.24
question_evasion	3 LLMs	0.4554	**0.3617**	**0.3871**	**0.4397**	0.6296	0.6546	0.7019	0.19 / 0.22 / 0.29
plan_vs_hype	4 Raters	0.5571	**0.3832**	**0.4145**	**0.5200**	0.7131	0.7390	0.8125	0.71 / 0.66 / 0.52 / 0.39
plan_vs_hype	3 LLMs	0.6120	**0.5011**	**0.5225**	**0.5994**	0.7508	0.7665	0.8178	0.71 / 0.66 / 0.52
strategic_fog	4 Raters	0.4708	**0.1823**	**0.2666**	**0.4537**	0.4714	0.5925	0.7686	0.22 / 0.22 / 0.42 / 0.17
strategic_fog	3 LLMs	0.5511	**0.2188**	**0.3208**	**0.5276**	0.4566	0.5863	0.7701	0.22 / 0.22 / 0.42
word_tone	4 Raters	0.7424	**0.5939**	**0.6062**	**0.6898**	0.8540	0.8603	0.8989	0.49 / 0.58 / 0.64 / 0.36
word_tone	3 LLMs	0.8558	**0.7982**	**0.8023**	**0.8550**	0.9223	0.9241	0.9465	0.49 / 0.58 / 0.64

Single-rater reliability on `blame_asymmetry` is modest across all 4 raters (ICC$(2,1) = 0.3935$, ICC$(3,1) = 0.4211$) and moderate across the 3 LLMs (ICC$(2,1) = 0.5654$, ICC$(3,1) = 0.6135$, $r = 0.6736$) because the deterministic rule scorer (`blame_ling`, SD $= 0.0969$ vs. $0.2083, 0.1584, 0.3002$ for the LLMs) misses context-dependent attributions. Under classical errors-in-variables, imperfect reliability (ICC$(3,4) = 0.7442$) attenuates OLS slopes toward zero (disattenuated slope $-5.7166/0.7442 = -7.6815$). Because the study lacks human-expert coding, we report individual-rater and rater-pair regressions in Table 2A.

3. Empirical Results

3.1 Announcement-Window Reaction to Blame Attribution Asymmetry: Pre-Specified Hierarchy, Parsimonious Models, and Weighting / SUE Sensitivity

To avoid selective emphasis on subsample specifications, we organize our announcement-window analysis around a strict hierarchy: * **Co-Primary Full-Sample Specifications ($N = 180$, Table 2 Rows 1–2):** Our headline tests evaluate `blame_asymmetry` across **all** $N = 180$ **filings** using (1) the timestamp-aligned reaction abnormal return `true_reaction_car_pct` (`-5.9921`, firm-clustered SE $= 2.7337, t = -2.19, p = 0.0284$) and (2) the filing-date return `day1_price_reaction_pct` (`-5.7166`, firm-clustered SE $= 1.8164, t = -3.15, p = 0.0016$), both controlling for XBRL earnings and revenue surprises (`sue_xbrl`, `xbrl_eps_yoy_pct`, `xbrl_rev_yoy_pct`), pre-event return and volatility, `word_tone`, sector fixed effects, and year fixed effects. * **Homogeneous Form 8-K and Strictly Pre-Open/Intraday Subsamples (Table 2 Rows 3–4):** We then report the homogeneous Form 8-K subsample ($N = 176$, `-5.6197`, $p = 0.0063$) and the strictly pre-open/intraday subsample (`is_after_hours == 0`, $N = 89$, `-11.3201`, $p = 0.0002$) as timing-regime decomposition checks explaining why mixing pre-release day-t trading for after-hours filers into `day1_price_reaction_pct` attenuates the filing-date slope relative to pre-open/intraday releases, whereas on the timestamp-aligned `true_reaction_car_pct` the formal interaction `blame_asymmetry:is_after_hours` is statistically indistinguishable from zero ($+5.0039$, SE $= 5.4649, t = +0.92, p = 0.3622$). Across all 8 announcement-window `blame_asymmetry` claims in `claims.json`, the Benjamini-Hochberg false discovery rate (FDR) q-values are ≤ 0.0308 (with Holm-Bonferroni family-wise adjusted $p \leq 0.0308$ across the 4 effect claims). Every primary, baseline, and supplementary specification in Table 2 (Rows 1–14b) and Table 4A is deterministically computed and verified via explicit `assert` statements in `code/analysis.py` during `recipe_replay`.

Specification	Sample (N, Clusters M, df_{resid})	Dependent Variable	`blame_asymmetry` Coef	Firm-Clustered SE	Clustered t-stat	Clustered p-value (BH FDR q)	R^2 (Adj. R^2)
Part A: Primary SUE-Controlled Specifications (`kind: effect` in `claims.json`)							
1. Co-Primary Full Panel: Timestamp-Aligned Reaction CAR (`t_react`) + SUE + FE	$N = 180, M = 96, df = 159$	`true_reaction_car_pct`	**`-5.9921`**	`2.7337`	`-2.19`	`0.0284` ($q = 0.0284$)	`0.1627` (`0.0574`)
2. Co-Primary Full Panel: Filing-Day Return + XBRL SUE/EPS/Rev + Two-Way FE	$N = 180, M = 96, df = 159$	`day1_price_reaction_pct`	**`-5.7166`**	`1.8164`	`-3.15`	`0.0016` ($q = 0.0032$)	`0.1808` (`0.0778`)

Specification	Sample (N, Clusters M, df_{resid})	Dependent Variable	blame_asymmetry Coef	Firm-Clustered SE	Clustered t-stat	Clustered p-value (BH FDR q)	R^2 (Adj. R^2)
3. Homogeneous Form 8-K Only (sec_form == '8-K') + Vagueness + SUE + FE	$N = 176, M = 96, df = 154$	day1_price_reaction_pct	**-5.6197**	2.0557	-2.73	0.0063 ($q = 0.0084$)	0.1811 (0.0695)
4a. Strictly Pre-Open / Intraday (is_after_hours == 0, < 16:00 ET) + SUE + FE	$N = 89, M = 52, df = 69$	day1_price_reaction_pct	**-11.3201** (-11.320057)	3.0510	-3.71	0.0002 ($q = 0.0008$)	0.3612 (0.1853)
4b. All Reaction-on-Filing-Date (event_date == true_reaction_date) + SUE + FE	$N = 91, M = 53, df = 71$	day1_price_reaction_pct	-11.320134	3.0479	-3.71	0.0002 ($q = 0.0008$)	0.3646 (0.1946)
Part B: Baseline Claims (kind: descriptive in claims.json)							
5. Baseline Two-Way FE (No XBRL Controls)	$N = 180, M = 96, df = 162$	day1_price_reaction_pct	**-5.7015**	1.7983	-3.17	0.0015 ($q = 0.0044$)	0.1801 (0.0940)
6. Baseline Two-Way FE + question_evasion Control	$N = 180, M = 96, df = 161$	day1_price_reaction_pct	**-5.5853**	1.8562	-3.01	0.0026 ($q = 0.0055$)	0.1803 (0.0886)
7. Unadjusted Bivariate OLS (day1_price_reaction_pct ~ blame_asymmetry)	$N = 180, M = 96, df = 178$	day1_price_reaction_pct	**-5.6417**	1.7207	-3.28	0.0010 ($q = 0.0044$)	0.0541 (0.0488)
8. Baseline Two-Way FE + All 4 Communication Constructs Jointly	$N = 180, M = 96, df = 159$	day1_price_reaction_pct	**-5.4783**	1.9425	-2.82	0.0048 ($q = 0.0073$)	0.1816 (0.0786)

Specification	Sample (N, Clusters M, df_{resid})	Dependent Variable	blame_asymmetry Coef	Firm-Clustered SE	Clustered t-stat	Clustered p-value (BH FDR q)	R^2 (Adj. R^2)
Part C: Targeted Methodological Checks for Quota Weighting, Ultra-Parsimonious / Zero-Singleton $N = 89$**, and Guidance / SUE Sensitivity**							
9a. S&P 500 Sector-Cap Weighted WLS ($N_{eff} =$ 159.57, **DEFF** = 1.1280) **on true_reaction_car_pct**	$N = 180, M =$ 96, df = 159	true_reaction_car_pct	-7.3231	3.1786	-2.30	0.0234	0.1795 (0.0763)
9b. Inverse-Firm-Frequency Weighted WLS ($N_{eff} =$ 166.55, **DEFF** = 1.0807) **on day1_price_reaction_pct**	$N = 180, M =$ 96, df = 159	day1_price_reaction_pct	-6.1091	1.7948	-3.40	0.0010	0.1932 (0.0917)
10a. Overfitting Check Only: Bivariate OLS on Pre-Open $N = 89$ ($k = 2$**, No Controls/FE**)	$N = 89, M =$ 52, df = 87	day1_price_reaction_pct	**-9.3315**	2.9927 (HC3: 3.0752)	-3.12	0.0030 (HC3: 0.0032)	0.0886 (0.0781)
10b. Overfitting Check Only: 3-Param SUE on Pre-Open $N = 89$ (**blame + sue_xbrl,** $k = 3$**, No FE**)	$N = 89, M =$ 52, df = 86	day1_price_reaction_pct	**-9.3475**	2.9703 (HC3: 3.1301)	-3.15	0.0028 (HC3: 0.0037)	0.0890 (0.0678)

Specification	Sample (N, Clusters M, df_{resid})	Dependent Variable	blame_asymmetry Coef	Firm-Clustered SE	Clustered t-stat	Clustered p-value (BH FDR q)	R^2 (Adj. R^2)
10c. All Continuous Financial & Tone Controls on Pre-Open $N = 89$ ($k = 8$, **No FE** / $k = 12$ **Year FE Only**)	$N = 89, M = 52$, df = 81 / 77	day1_price_reaction_pct	**-10.0194** (Year FE: **-9.9227**)	3.5379 (3.6565)	-2.83 (-2.71)	0.0066 (0.0067)	0.1942 (0.1538)
10d. Zero-Singleton Pre-Open Subset (≥ 2 **Filings/Firm,** 0 **Singletons, Full Controls + Two-Way / Year FE**)	$N = 74, M = 37$, df = 54 / 62	day1_price_reaction_pct	**-11.0867** (Year FE: **-11.5215**)	3.1379 (4.1073)	-3.53 (-2.81)	0.0011 (0.0050)	0.4095 (0.1818)
11. Pooled Regime Interaction (blame_asymmetry * is_after_hours) + SUE + FE	$N = 180, M = 96$, df = 157	day1_price_reaction_pct	-9.6191 / Int: **+7.7500**	2.7852 / 3.4384	-3.45 / +2.25	0.0008 / 0.0265	0.2143 (0.1042)
12. Controlling for Guidance Specificity (plan_vs_hype), Numeric Density & Net Revision	$N = 180, M = 96$, df = 156	day1_price_reaction_pct	-5.7262	1.9322	-2.96	0.0038	0.1897 (0.0702)
13a. Controlling for Excerpt Length (excerpt_char_len) on Aligned CAR (true_reaction_car_pct)	$N = 180, M = 96$, df = 158	true_reaction_car_pct	**-5.9133**	2.8575	-2.07	0.0412	0.1629 (0.0517)

Specification	Sample (N, Clusters M, df_{resid})	Dependent Variable	blame_asymmetry Coef	Firm-Clustered SE	Clustered t-stat	Clustered p-value (BH FDR q)	R^2 (Adj. R^2)
13b. Controlling for Excerpt Length (excerpt_char_len) on Filing-Day Return (day1_price_reaction_pct)	$N = 180, M = 96$, df = 158	day1_price_reaction_pct	**-5.1732**	1.8678	-2.77	0.0067	0.1972 (0.0905)
13c. Pooled Uncapped Subsample (excerpt_char_len < 2491, $T_1 \cup T_2$**, Pre-Open** $N = 70$**) + SUE + FE**	$N = 70, M = 47$, df = 50	day1_price_reaction_pct	**-8.5258**	3.9185	-2.18	0.0296	0.3601 (0.1169)
13d. Pooled Uncapped Subsample (excerpt_char_len < 2491, $T_1 \cup T_2$**, All** $N = 141$**, Parsimonious** $k = 8$**)**	$N = 141, M = 86$, df = 133	day1_price_reaction_pct	**-4.5224**	2.2905	-1.97	0.0483	0.0985 (0.0510)
14a. Pre-2026 Subsample (year <= 2025, Excluding All 2026 Filings) + SUE + FE	$N = 144, M = 94$, df = 124	day1_price_reaction_pct	**-6.9591**	1.9733	-3.53	0.0004	0.2145 (0.0941)
14b. Pre-2026 Strictly Pre-Open Subsample (year <= 2025 & is_after_hours == 0) + SUE + FE	$N = 67, M = 47$, df = 48	day1_price_reaction_pct	**-13.9562**	3.5383	-3.94	0.0001	0.4278 (0.2132)

3.1.1 Detailed Verification of Weighting, Ultra-Parsimonious / Zero-Singleton Subsample Inference, and Individual-Rater Regressions

1. **Sector-Cap and Inverse-Firm-Frequency WLS:** Under S&P 500 sector-cap WLS (Row 9a; normalized weight range 0.6000–1.8000, median 0.9500, IQR 0.6000–1.2000; Kish $N_{\text{eff}} = 159.57$, DEFF $= 1.1280$ on $N = 180$; $N_{\text{eff}} = 82.40$, DEFF $= 1.0801$ on $N = 89$), the blame_asymmetry slope is **-7.3231** (**SE** $= 3.1786$, $t = -2.30$, $p = 0.0234$) on true_reaction_car_pct,

-6.1037 (**SE** $= 1.7848, t = -3.42, p = 0.0009$) on `day1_price_reaction_pct`, and **-13.0924** (**SE** $= 3.1574, t = -4.15, p = 0.0001$) on $N = 89$. Under inverse-firm-frequency WLS ($w_i = 1/n_{\text{firm}}$, Row 9b; range `0.6250–1.8750`, median/IQR `0.9375`; $N_{\text{eff}} = 166.55$, DEFF $= 1.0807$ on $N = 180$; $N_{\text{eff}} = 83.15$, DEFF $= 1.0703$ on $N = 89$), the slopes are **-6.2558** ($p = 0.0278$), **-6.1091** ($p = 0.0010$), and **-11.3590** ($p = 0.0006$).

2. **Which Specifications Carry the Conditional Observational (Causal-Adjacent) Interpretation vs. Overfitting Sensitivity Checks on $N = 89$:** We state explicitly that **only the full-control specifications**—Co-Primary Specifications 1 and 2 on $N = 180$ ($\text{df}_{\text{resid}} = 159$, Table 2 Rows 1–2), the full 20-parameter SUE-controlled specification on $N = 89$ (Row 4a, `-11.3201`), and the zero-singleton multi-event pre-open specification with full continuous controls and two-way fixed effects on $N = 74, M = 37$ firms (Row 10d, `-11.0867`)—carry the conditional observational (confounder-adjusted) interpretation. By contrast, the small $k = 2$ bivariate (Row 10a) and $k = 3$ SUE-only (Row 10b) models omit pre-event momentum, volatility, lexical tone, and sector/year fixed effects deemed essential in the primary model; **they are reported strictly as robustness checks against degrees-of-freedom overfitting, not against omitted-variable bias**:
   - **Overfitting-Only Checks ($k = 2$ Row 10a and $k = 3$ Row 10b):** Bivariate OLS ($k = 2, \text{df}_{\text{resid}} = 87$) yields **-9.3315** (**firm-clustered SE** $= 2.9927, t = -3.12, p = 0.0030$; **finite-sample MacKinnon-White HC3 SE** $= 3.0752, p = 0.0032$), and 3-parameter SUE-only OLS (`blame_asymmetry + sue_xbrl`, $k = 3, \text{df}_{\text{resid}} = 86$) yields **-9.3475** (**firm-clustered SE** $= 2.9703, t = -3.15, p = 0.0028$; **HC3 SE** $= 3.1301, p = 0.0037$);
   - **All Continuous Financial and Lexical Controls With Reduced Fixed Effects ($k = 12$ Year FE Only or $k = 17$ Sector FE Only) or Without Fixed Effects ($k = 8, \mathbf{df_{resid}} = 81$, Row 10c):** To maintain both confounder adjustment and a healthy observation-to-parameter ratio on $N = 89$, retaining all continuous controls (`sue_xbrl`, `xbrl_eps_yoy_pct`, `xbrl_rev_yoy_pct`, `pre_20d_return_pct`, `pre_20d_volatility_pct`, `word_tone`) plus **Year FE only (`C(year)`,** $k = 12, \mathbf{df_{resid}} = 77$, 7.4 **observations per parameter**) yields **-9.9227** (**firm-clustered SE** $= 3.6565, t = -2.71, p = 0.0067, R^2 = 0.1538$), retaining **Sector FE only (`C(sector)`,** $k = 17, \mathbf{df_{resid}} = 72$) yields **-11.0505** (**firm-clustered SE** $= 2.8690, t = -3.85, p = 0.0001$), and retaining all continuous controls without fixed effects ($k = 8, \text{df}_{\text{resid}} = 81$, > 11 observations per parameter) yields **-10.0194** (**firm-clustered SE** $= 3.5379, t = -2.83, p = 0.0046$; **HC3 SE** $= 3.8262, t = -2.62, p = 0.0105$) on `day1_price_reaction_pct` and **-9.0872** (**firm-clustered SE** $= 3.4115, t = -2.66, p = 0.0077$) on `true_reaction_car_pct`;
   - **Zero-Singleton Multi-Event Subset With Full Controls and Two-Way or Reduced Fixed Effects ($N = 74, M = 37$ firms, 0 singletons, Row 10d):** Dropping all 15 singleton firms while retaining all continuous controls yields **-11.0867** (**firm-clustered SE** $= 3.1379, t = -3.53, p = 0.0011$; **HC3 SE** $= 4.1410, p = 0.0098$) with full two-way fixed effects ($\text{df}_{\text{resid}} = 54$), **-11.5215** (**firm-clustered SE** $= 4.1073, t = -2.81, p = 0.0050$) with **Year FE only** ($k = 12, \mathbf{df_{resid}} = 62$, 6.2 **observations per parameter**), and **-11.7566** (**firm-clustered SE** $= 3.9111, p = 0.0048$; **HC3 SE** $= 4.2908, p = 0.0079$) without fixed effects ($k = 8, \text{df}_{\text{resid}} = 66$).
   - **Reconciling Timing-Regime Heterogeneity (`day1_price_reaction_pct` vs. `true_reaction_car_pct`):** For the 91 after-hours filings (`is_after_hours == 1`, $\geq 16:00$ ET), regular trading on `event_date` closes before public release (except the 2 rolled `PEP` rows), attenuating the after-hours slope on pre-release `day1_price_reaction_pct` to `-2.7291` (SE $= 1.6717, t = -1.63, p = 0.1026$) and producing a significant interaction on `day1_price_reaction_pct` ($+7.7500$, SE $= 3.4384, t = +2.25, p = 0.0265$, Row 11). Map-

ping after-hours filings to t_{react} shifts the after-hours slope to `-3.7017` (SE = 5.2788) on `true_reaction_return_pct` and `-3.5286` (SE = 4.9532) on `true_reaction_car_pct`, rendering `blame_asymmetry:is_after_hours` insignificant on `true_reaction_car_pct` ($+5.0039, \text{SE} = 5.4649, t = +0.92, p = 0.3622$), while 1.76× higher overnight return dispersion (SD = 4.97% vs. 2.83%) from concurrent after-hours calls and overnight analyst revisions inflates overnight standard errors.

3. **Individual-Rater and Rater-Pair Regressions:** Table 2A estimates the co-primary and pre-open specifications separately for each individual judge and for two-rater sub-composites.

Rater / Score Column	Full Panel Filing-Day (`day1_price_reaction_pct`, $N = 180$)	Strictly Pre-Open / Intraday (`is_after_hours == 0`, $N = 89$)	Timestamp-Aligned Reaction CAR (`true_reaction_car_pct`, $N = 180$)
4-Rater Composite (`blame_asymmetry`)	**`-5.7166`** (SE = 1.8164, $t = -3.15, p = 0.0022$)	**`-11.3201`** (SE = 3.0510, $t = -3.71, p = 0.0005$)	**`-5.9921`** (SE = 2.7337, $t = -2.19, p = 0.0308$)
3-LLM Composite (`blame_llm3`)	**`-4.5647`** (SE = 1.4094, $t = -3.24, p = 0.0017$)	**`-9.2197`** (SE = 2.4202, $t = -3.81, p = 0.0004$)	**`-4.5378`** (SE = 2.1149, $t = -2.15, p = 0.0345$)
Judge 1 Alone: `blame_gemini` (`gemini-3-flash-preview`)	**`-4.2273`** (SE = 1.3217, $t = -3.20, p = 0.0019$)	**`-9.9008`** (SE = 2.3523, $t = -4.21, p = 0.0001$)	`-3.1836` (SE = 2.0904, $t = -1.52, p = 0.1311$)
Judge 2 Alone: `blame_llama` (`llama-3.3-70b-instruct`)	**`-2.6393`** (SE = 0.9635, $t = -2.74, p = 0.0074$)	**`-5.2158`** (SE = 1.5417, $t = -3.38, p = 0.0014$)	**`-3.0346`** (SE = 1.3788, $t = -2.20, p = 0.0302$)
Judge 3 Alone: `blame_deepseek` (`deepseek-chat`)	`-3.6143` (SE = 1.9427, $t = -1.86, p = 0.0659$)	`-5.8223` (SE = 3.5060, $t = -1.66, p = 0.1029$)	`-3.8468` (SE = 2.9713, $t = -1.29, p = 0.1986$)
Judge 4 Alone: `blame_ling` (`structural-linguistic-v1`)	`-0.7239` (SE = 2.3287, $t = -0.31, p = 0.7566$)	`-0.1617` (SE = 5.7171, $t = -0.03, p = 0.9776$)	`-3.5528` (SE = 6.3039, $t = -0.56, p = 0.5744$)
2-Rater Pair A (`0.5*(blame_gemini + blame_llama)`)	**`-3.8597`** (SE = 1.1892, $t = -3.25, p = 0.0012$)	**`-8.1491`** (SE = 1.9653, $t = -4.15, p < 0.0001$)	**`-3.7824`** (SE = 1.7421, $t = -2.17, p = 0.0299$)
2-Rater Pair B (`0.5*(blame_deepseek + blame_ling)`)	`-4.5577` (SE = 2.9720, $t = -1.53, p = 0.1251$)	`-7.4174` (SE = 5.4276, $t = -1.37, p = 0.1717$)	`-6.1935` (SE = 4.9767, $t = -1.24, p = 0.2133$)

All three LLMs show negative slopes (`-2.64` to `-4.23` on $N = 180$; `-5.22` to `-9.90` on $N = 89$), and Pair B (`0.5*(blame_deepseek + blame_ling)`) yields negative point estimates (`-4.5577`, `-7.4174`, `-6.1935`) of comparable or larger magnitude than Pair A (`-3.8597`, `-8.1491`, `-3.7824`), with 2.5× larger SE (`2.9720` vs. `1.1892`) because `blame_deepseek` (SD = 0.1584) and `blame_ling` (SD = 0.0969) have compressed variance relative to `blame_gemini` (0.2083) and `blame_llama` (0.3002).

4. **2,500-Character Excerpt Truncation, Uncapped Subsample ($N = 141$), and Tercile Heterogeneity (Table 2 Rows 13a–13d):** `excerpt_char_len` correlates weakly with `blame_asymmetry` ($r = 0.1305$) and `question_evasion` ($r = -0.1119$). Controlling for `excerpt_char_len` yields **`-5.9133`** (**SE** = 2.8575, $t = -2.07, p = 0.0412$) on `true_reaction_car_pct` (Row 13a; length slope $-0.0002, p = 0.8540$), **`-5.1732`** (**SE** = 1.8678, $t = -2.77, p = 0.0067$) on `day1_price_reaction_pct` (Row 13b; length slope $-0.0017, p = 0.0541$), and **`-10.1739`** (**SE** = 3.1731, $t = -3.21, p = 0.0023$) on $N = 89$ (length slope $-0.0022, p = 0.0932$). Interactions with standardized length (`+0.5938, \text{SE} = 2.5224, p = 0.8144` on CAR,

main effect -5.9866, p = 0.0332; -2.7671, \text{SE} = 1.7018, p = 0.1073 on Day-1, main effect -4.8313, p = 0.0117) and with the cap indicator excerpt_char_len >= 2450 ($+3.6959, \text{SE} = 6.9080, p = 0.5939$ on CAR; $-4.8349, \text{SE} = 4.0023, p = 0.2300$ on Day-1) are insignificant. Across terciles (T1: 929–2,484, $N = 61, M = 47$; T2: 2,485–2,490, $N = 80, M = 62$; T3: 2,491–2,492, $N = 39, M = 34$), slopes are negative in every tercile on day1_price_reaction_pct (T1 = -3.5749, \text{SE} = 3.5340, p = 0.3170; T2 = -3.5951, \text{SE} = 2.9344, p = 0.2252; T3 = -11.0348, \text{SE} = 3.4488, p = 0.0030) and true_reaction_car_pct (T1 = -2.4144, \text{SE} = 4.4575, p = 0.5907; T2 = -0.2174, \text{SE} = 4.0975, p = 0.9579; T3 = -23.0405, \text{SE} = 5.6792, p = 0.0003). Pooling T1 and T2 (excerpt_char_len < 2491, $N = 141, M = 86$, Rows 13c–13d) yields **-8.5258** (**SE** $= 3.9185, t = -2.18, p = 0.0296$) in the uncapped pre-open subsample ($N = 70, M = 47$, Row 13c) and marginal significance in the uncapped full sample ($N = 141$: parsimonious $k = 8$ slope **-4.5224**, SE $= 2.2905, t = -1.97, p = 0.0483$, Row 13d; with full two-way FE $\text{df}_{\text{resid}} = 120$: -4.1318, SE $= 2.4033, t = -1.72, p = 0.0856$ with question_evasion or -4.2943, SE $= 2.4631, p = 0.0856$ on day1_price_reaction_pct and -7.2836, SE $= 4.9547, p = 0.1456$ on true_reaction_car_pct). Because T3 exhibits $\sim 2.5\times$ larger effects (-11.0348 and -23.0405) while pooled uncapped full-FE significance is marginal ($p = 0.0856$), **excerpt-length heterogeneity is a substantive empirical boundary condition** (Limitation 5).

5. **Pre-2026 Cohort Sensitivity (2022–2025 $N = 144$ and 2022–2024 $N = 108$, Table 2 Rows 14a–14b):** Excluding all 36 filings from the 2026 cohort leaves the blame_asymmetry penalty stronger than in the full panel: across 2022–2025 ($N = 144, M = 94$ firms, Row 14a), the SUE- and FE-controlled slope on day1_price_reaction_pct is **-6.9591** (**firm-clustered SE** $= 1.9733, t = -3.53, p = 0.0004$), and in the 2022–2025 pre-open subsample ($N = 67, M = 47$ firms, Row 14b) it is **-13.9562** (**SE** $= 3.5383, t = -3.94, p = 0.0001$) on day1_price_reaction_pct and **-11.9515** (**SE** $= 3.5632, t = -3.35, p = 0.0008$) on true_reaction_car_pct. Restricting further to 2022–2024 ($N = 108, M = 90$ firms) yields **-6.2866** (**SE** $= 2.4667, t = -2.55, p = 0.0108$) on the full pre-2025 sample and **-12.0422** (**SE** $= 3.8607, t = -3.12, p = 0.0018$) on day1_price_reaction_pct (**-9.8042, SE** $= 4.0761, t = -2.41, p = 0.0162$ on true_reaction_car_pct) in the 2022–2024 pre-open subsample ($N = 51, M = 45$ firms).

3.2 21-Trading-Day Post-Announcement Abnormal Returns: Leading With the Timestamp-Aligned Result, Underpowered Design Limit, and HAC(21) Inference

In Table 3, we **lead with the properly timestamp-aligned post-reaction 21-trading-day abnormal return (post_reaction_30d_car_pct over trading sessions $t_{\text{react}} + 1 \dots t_{\text{react}} + 21$, Row 1)** before presenting the unaligned filing-date window (realized_30d_pct over $t + 1 \dots t + 21$, Row 2) and the split-sample decomposition by is_after_hours == 0 vs. 1 (Rows 3–4). **We designate firm-clustered standard errors ($M = 96$ firms) as our single pre-specified primary inference in Table 3.** Because setting Newey-West maxlags = 21 on $N = 180$ observations ($\approx 11.7\%$ of the sample size) can yield unstable long-run variance estimates in finite samples, HAC(21) and calendar year-quarter clustered SEs ($G = 14$)—alongside shorter-bandwidth 5-lag Newey-West HAC(5) (SE $= 6.2699, p = 0.4769$ for question_evasion and SE $= 0.1044, p = 0.2156$ for true_reaction_return_pct in Row 1)—are reported strictly as secondary sensitivity checks confirming that the 21-day post-reaction results remain null under every variance estimator.

Specification / Subsample	Dependent Variable	question_evasion Coef (Firm-Clustered SE, p | HAC(21) SE, p)	Reaction-Day Return Regressor Coef (Firm-Clustered SE, p | HAC(21) SE, p)
1. Primary Aligned Full Panel ($N = 180$, **t_react+1..t_react+21**)	post_reaction_30d_car_pct	**-4.4713** (Clust SE = 6.5613, $p = 0.4956$ | HAC(21) SE = 6.3400, $p = 0.4817$)	true_reaction_return_pct: **-0.1297** (Clust SE = 0.1035, $t = -1.25$, $p = 0.2102$ | HAC(21) SE = 0.1027, $p = 0.2083$)
2. Unaligned Filing-Date Full Panel ($N = 180$, **t+1..t+21**)	realized_30d_pct	**-6.5194** (Clust SE = 6.6918, $p = 0.3299$ | HAC(21) SE = 7.1769, $p = 0.3650$)	day1_price_reaction_pct: **-0.3596** (Clust SE = 0.1583, $p = 0.0232$ | HAC(21) SE = 0.1108, $p = 0.0014$)
3a. Split: Strictly Pre-Open / Intraday (is_after_hours == 0, $N = 89$) — Aligned	post_reaction_30d_car_pct	-10.7113 (Clust SE = 9.5985, $p = 0.2697$)	true_reaction_return_pct: -0.3783 (Clust SE = 0.1606, $p = 0.0224$)
3b. Split: Strictly Pre-Open / Intraday (is_after_hours == 0, $N = 89$) — Unaligned	realized_30d_pct	-10.7068 (Clust SE = 9.5993, $p = 0.2699$)	day1_price_reaction_pct: -0.3781 (Clust SE = 0.1606, $p = 0.0225$)
4a. Split: After-Hours (is_after_hours == 1, $N = 91$) — Aligned (t_react+1..t_react+21)	post_reaction_30d_car_pct	-0.0208 (Clust SE = 8.4212, $p = 0.9980$)	true_reaction_return_pct: **+0.0278** (Clust SE = 0.1347, $p = 0.8373$)
4b. Split: After-Hours (is_after_hours == 1, $N = 91$) — Unaligned (t+1 inside dep. var.)	realized_30d_pct	-3.3733 (Clust SE = 10.1443, $p = 0.7408$)	day1_price_reaction_pct: **-0.8739** (Clust SE = 0.5293, $p = 0.1047$)
5. Full Panel CAPM Beta-Adjusted ($N = 180$)	capm_30d_car_pct	-5.1962 (Clust SE = 6.3079, $p = 0.4101$)	day1_price_reaction_pct: -0.2593 (Clust SE = 0.1502, $p = 0.0874$)

Table 3 establishes two core conclusions: 1. **Underpowered Design Limit of the 21-Day Drift Test (Analytic i.i.d. and Cluster/Overlap-Adjusted MDE):** The question_evasion slope is -4.4713 (Clust SE $= 6.5613, p = 0.4956$; HAC(21) SE $= 6.3400, p = 0.4817$) on post_reaction_30d_car_pct and -6.5194 (Clust SE $= 6.6918, p = 0.3299$; HAC(21) SE $= 7.1769, p = 0.3650$) on realized_30d_pct. From $\sigma_Y = 8.6795\%$, $\sigma_X = 0.1145$ (0.1326), $N = 180$, and $\text{VIF}_{X|Z,\text{FE}} = 1.4151$ ($\text{df}_{\text{resid}} = 160$), the i.i.d. analytic SE = 5.6835 (7.1311 FE-adjusted) gives an 80%-power MDE_{SD} of **1.82–2.30 pp** ($0.210\sigma_Y$–$0.264\sigma_Y$). Accounting for within-firm clustering and 21-day overlap on post_reaction_30d_car_pct raises $\text{SE}(\hat{\beta}_E)$ from 5.9447% ($\text{MDE}_{\text{SD}} = 1.91$ pp) to 6.3063% under HAC(21) (DEFF $= 1.1254, \text{MDE}_{\text{unit}} = 17.67\%, \text{MDE}_{\text{SD}} = 2.02$ pp), 6.5613% under 96-firm clustering (DEFF $= 1.2182, \text{MDE}_{\text{unit}} = 18.38\%, \text{MDE}_{\text{SD}} = 2.10$ pp; power 7.9%, 15.8%, 28.5% for 0.5, 1.0, 1.5 pp per SD), and 7.1141% under 19-year-quarter clustering (DEFF $= 1.4322, \text{MDE}_{\text{unit}} = 19.93\%, \text{MDE}_{\text{SD}} = 2.28$ pp; power 7.3%, 13.9%, 24.6%; on realized_30d_pct: $\text{MDE}_{\text{SD}} = 2.15$ pp with SE $= 6.6918\%$, DEFF $= 1.0087$ firm-clustered; 2.29 pp with SE $= 7.1514\%$, DEFF $= 1.1520$ under HAC(21); 2.31 pp with SE $= 7.2031\%$, DEFF $= 1.1687$ year-quarter clustered). **Thus, our 21-day drift tests are underpowered for $<$ 2.10–2.31 pp effects and cannot rule out PEAD in illiquid or high-uncertainty populations** [12] [13] [14] [15]. 2. **Leading With the Timestamp-**

Aligned Post-Reaction Return Eliminates the Apparent Reversal: In our primary timestamp-aligned specification (`post_reaction_30d_car_pct` over $t_{\text{react}}+1 \dots t_{\text{react}}+21$, Row 1), the OLS coefficient on `true_reaction_return_pct` is -0.1297 (firm-clustered SE $= 0.1035, t = -1.25, p = 0.2102$; HAC(21) SE $= 0.1027, p = 0.2083$; year-quarter clustered SE $= 0.1070, p = 0.2564$)—a statistical null. By contrast, the unaligned specification (`realized_30d_pct` over $t+1 \dots t+21$, Row 2) yields a negative slope on `day1_price_reaction_pct` of -0.3596 (firm-clustered SE $= 0.1583, p = 0.0232$) only because, for the 91 after-hours filings (`is_after_hours == 1`, Rows 4a–4b), `realized_30d_pct` mechanically includes the immediate reaction on $t+1$ (`-0.8739` unaligned vs. **`+0.0278`**, $p = 0.8373$ **aligned**). Thus, we do not interpret `-0.3596` as post-announcement mean reversion; rather, properly aligning the post-event window to $t_{\text{react}}+1 \dots t_{\text{react}}+21$ shows no statistically significant reversal, consistent with PEAD theory [9] [10].

3.3 Out-of-Sample Forecast Comparison: Disjoint-Firm vs. Leave-One-Year-Out CV, Nested Regularization, and `HAC(21)` Inference

To evaluate out-of-sample 21-trading-day return predictability (`realized_30d_pct`) without tuning leakage or cross-fold firm/window dependence, Table 4 reports our strictly out-of-sample forecasting pipelines across the 5 leave-one-year-out folds (`2022–2026`, $N = 36$ per fold). We address both structural questions raised regarding the cross-validation design: 1. **Zero Cross-Year Window Overlap and Disjoint-Firm `GroupKFold` CV:** Because every cohort's latest `true_reaction_date` is on or before `November 5`, every 21-trading-day window closes on or before `December 5` of the same calendar year (**zero cross-year window overlap**). To eliminate cross-fold firm recurrence as well, we run a **5-fold Disjoint-Firm `GroupKFold` CV** partitioning the 96 sorted tickers modulo 5 into 5 disjoint firm groups (`0` shared firms across folds; Table 4A). Because a constant zero forecast ($\hat{y}_i \equiv 0$) estimates zero parameters, its pooled RMSE across any partition $F_1 \cup \cdots \cup F_5 = \{1, \dots, 180\}$ is $\sqrt{\frac{1}{180}\sum_{i=1}^{180} y_i^2} = 8.6554\%$ (`8.655369%`), whereas trained models differ across partitions: under Disjoint-Firm `GroupKFold`, Constant Zero (`8.6554%`) beats pure 0.25 shrinkage (`8.7534%`), nested `RidgeCV` (`8.7659%`), and unregularized OLS (`9.4133%`). Table 4 also reports Newey-West HAC(21) Diebold-Mariano t-statistics alongside firm-clustered DM t, unclustered DM t, and Wilcoxon signed-rank p-values. 2. **Exact Closed-Form NumPy Hat-Matrix LOOCV Specification (`_closed_form_ridge_loocv_predict`) and Equivalence to `sklearn.linear_model.RidgeCV`:** Because the deterministic replay container provides `numpy`, `pandas`, `scipy`, and `statsmodels` without `scikit-learn`, `code/analysis.py` implements nested `RidgeCV` in pure `numpy` via `_closed_form_ridge_loocv_predict` over the 61-point log-spaced grid $\alpha \in$ np.logspace$(-2, 4, 61)$ (10^{-2} to 10^{4}). Inside each training fold (N_{train} rows, $p = 14$ standardized text/tone/sector predictors X_{tr}), both predictors and target are centered by their training-fold means ($\tilde{X} = X_{\text{tr}} - \bar{X}_{\text{tr}}$, $\tilde{y} = y_{\text{tr}} - \bar{y}_{\text{tr}}$) so the intercept $\hat{\beta}_0(\alpha) = \bar{y}_{\text{tr}} - \bar{X}_{\text{tr}}\hat{\beta}(\alpha)$ is unpenalized. For each candidate α, the ridge slope is $\hat{\beta}(\alpha) = (\tilde{X}^\top\tilde{X} + \alpha I_p)^{-1}\tilde{X}^\top\tilde{y}$, the exact leverage diagonal of the hat matrix (including the unpenalized intercept) is $H_{ii}(\alpha) = \tilde{X}_i(\tilde{X}^\top\tilde{X} + \alpha I_p)^{-1}\tilde{X}_i^\top + \frac{1}{N_{\text{train}}}$, and the exact Allen (1974) PRESS leave-one-out cross-validation MSE is $\text{LOOCV}(\alpha) = \frac{1}{N_{\text{train}}}\sum_{i=1}^{N_{\text{train}}}\left(\frac{\tilde{y}_i - \tilde{X}_i\hat{\beta}(\alpha)}{1 - H_{ii}(\alpha)}\right)^2$. Because `sklearn.linear_model.RidgeCV(alphas=np.logspace(-2, 4, 61), scoring='neg_mean_squared_error')` evaluates this identical hat-matrix identity, `_closed_form_ridge_loocv_predict` selects the exact same α^* in every fold (`10000.00, 10000.00, 1258.93, 1995.26, 10000.00` across Disjoint-Firm Folds 0–4, yielding fold RMSEs `10.4208 / 9.2841 / 8.3868 / 8.4562 / 6.9982` and pooled RMSE **`8.7659%`**; and `10000.00, 10000.00, 10000.00, 316.23, 10000.00` under 4-fold inner-year CV in LOYO, yielding fold RMSEs `9.3358 / 6.2588 / 9.9710 / 10.1818 / 8.2434` and pooled RMSE **`8.9149%`**)

with a maximum absolute prediction difference across all 180 events of 6.11×10^{-16}. In Table 4, we separate these strictly out-of-sample models (Rows 1–8) from `intuition_pred_pct` (Row 9, `8.6459`, $p = 0.8424$), which is removed from `claims.json` and reported solely as an exploratory non-nested cross-edition heuristic.

Forecasting Pipeline ($N = 180$)	Pooled RMSE	Fold RMSEs (2022 / 2023 / 2024 / 2025 / 2026)	Firm-Clustered DM t vs. Zero (p)	Unclustered DM t vs. Zero (p)	Newey-West HAC(21) DM t vs. Zero (p)	Wilcoxon Signed-Rank p vs. Zero
Part A: Strictly Out-of-Sample Benchmarks & Nested Regularized Models						
1. Constant Zero Forecast (`0.0%`, Benchmark)	**`8.6554`**	`9.27 / 6.23 / 9.97 / 9.09 / 8.24` (SD `1.44`)	—	—	—	—
2. Training-Fold Historical Mean (`hist_mean_pred_pct`)	`8.7039`	`9.34 / 6.26 / 9.97 / 9.22 / 8.24`	`-1.89` (`0.0611`)	`-2.08` (`0.0394`)	`-2.35` (`0.0190`)	`0.0306`
3. Pure 0.25 Scalar Shrinkage (`pure_shrink25_pred_pct`)	**`8.7464`**	`9.36 / 6.18 / 9.96 / 9.40 / 8.31`	`-2.77` (`0.0068`)	`-3.01` (`0.0030`)	`-1.70` (`0.0891`)	`0.0694`
4. Nested Within-Fold Scalar-Shrunk OLS (c_{opt} via inner CV)	`8.7591`	`9.27 / 6.23 / 9.97 / 9.57 / 8.24`	`-2.98` (`0.0036`)	`-2.92` (`0.0040`)	`-1.81` (`0.0706`)	`0.0017`
5. Nested Within-Fold `RidgeCV` (α via inner LOYO CV)	`8.9149`	`9.34 / 6.26 / 9.97 / 10.18 / 8.24`	`-3.31` (`0.0013`)	`-3.38` (`0.0009`)	`-1.52` (`0.1285`)	`0.0071`
6. Nested Within-Fold `LassoCV` (α via inner LOYO CV)	`8.9229`	`9.34 / 6.26 / 9.97 / 10.21 / 8.24`	`-3.35` (`0.0012`)	`-3.39` (`0.0009`)	`-1.50` (`0.1342`)	`0.0051`
7. Fixed Out-of-Sample LASSO (`lasso_pred_pct`, $\alpha = 0.5$)	`8.9262`	`9.33 / 6.19 / 9.96 / 10.26 / 8.28`	`-3.17` (`0.0020`)	`-3.24` (`0.0014`)	`-1.47` (`0.1408`)	`0.0116`
8. Unregularized Text OLS (`stateless_pred_pct`)	**`9.1165`**	`9.69 / 6.08 / 9.97 / 10.54 / 8.61`	`-3.25` (`0.0016`)	`-3.52` (`0.0005`)	`-1.90` (`0.0574`)	`0.0098`
Part B: Exploratory Non-Nested Heuristic Baseline (Not a Valid Strictly OOS Comparator)						

Forecasting Pipeline ($N = 180$)	Pooled RMSE	Fold RMSEs (2022 / 2023 / 2024 / 2025 / 2026)	Firm-Clustered DM t vs. Zero (p)	Unclustered DM t vs. Zero (p)	Newey-West HAC(21) DM t vs. Zero (p)	Wilcoxon Signed-Rank p vs. Zero
9. Exploratory Cross-Edition Tuned Heuristic (`intuition_pred_pct`)	**8.6459**	9.14 / 6.20 / 9.91 / 9.31 / 8.18	+0.20 (0.8424)	+0.21 (0.8362)	+0.17 (0.8658)	0.3403

Table 4A: Per-Fold Sample Sizes (N_k, M_k), Exact Firm Allocations, and Per-Fold Out-of-Sample RMSEs Under 5-Fold LOYO and 5-Fold Disjoint-Firm `GroupKFold`

CV Scheme & Fold	Test Events (N_k)	Test Firms (M_k)	Cohort / Disjoint-Firm Ticker Allocation (`sorted(ticker)` modulo 5)	Fold Constant Zero RMSE (%)	Fold Unshrunk OLS RMSE (%)	Fold 0.25-Shrunk OLS RMSE (%)	Fold Nested `RidgeCV` RMSE (%)
A. LOYO Fold 1 (`2022`)	36	36	All 2022 cohort filings (`year == 2022`)	9.2695	9.6904	9.3596	9.3400
A. LOYO Fold 2 (`2023`)	36	36	All 2023 cohort filings (`year == 2023`)	6.2340	6.0782	6.1832	6.2600
A. LOYO Fold 3 (`2024`)	36	36	All 2024 cohort filings (`year == 2024`)	9.9673	9.9681	9.9628	9.9700
A. LOYO Fold 4 (`2025`)	36	36	All 2025 cohort filings (`year == 2025`)	9.0877	10.5422	9.3993	10.1800
A. LOYO Fold 5 (`2026`)	36	36	All 2026 cohort filings (`year == 2026`)	8.2376	8.6140	8.3102	8.2400
Pooled LOYO ($N = 180$)	**180**	**96**	$\sqrt{\frac{1}{180}\sum_{k=1}^{5} N_k \mathrm{RMSE}_k^2}$	**8.6554**	**9.1165**	**8.7464**	**8.9149**
B. Disjoint-Firm Fold 0	35	20	`ABT, AMGN, BAC, COF, CVX, ECL, F, GIS, HON, JBHT, LLY, MDLZ, MU, NVDA, PFE, RTX, SWK, TMUS, UNP, XOM`	10.2348	11.0223	10.3392	10.4208
B. Disjoint-Firm Fold 1	38	19	`ADBE, AMZN, BKNG, COP, DE, EL, FCX, GM, IBM, JNJ, LOW, META, NEM, NWL, PG, SBUX, SYK, TSLA, UPS`	9.2712	9.0877	9.1766	9.2841

CV Scheme & Fold	Test Events (N_k)	Test Firms (M_k)	Cohort / Disjoint-Firm Ticker Allocation (`sorted(tickers)` modulo 5)	Fold Constant Zero RMSE (%)	Fold Unshrunk OLS RMSE (%)	Fold 0.25-Shrunk OLS RMSE (%)	Fold Nested `RidgeCV` RMSE (%)
B. Disjoint-Firm Fold 2	34	19	ADI, APD, BLK, COST, DG, ENPH, FDX, GOOGL, INTC, JPM, LRCX, MHK, NFLX, ORCL, PINS, SCHW, T, TWLO, V	8.1489	9.5309	8.3633	8.3868
B. Disjoint-Firm Fold 3	35	19	AMAT, AVGO, CAT, CRM, DIS, EOG, FSLR, GS, INTU, KHC, MAS, MS, NKE, PANW, PYPL, SLB, TGT, TXN, VZ	8.3804	9.0511	8.4467	8.4562
B. Disjoint-Firm Fold 4	38	19	AMD, AXP, CL, CVS, DOW, ETN, GE, HD, ISRG, KO, MCD, MSFT, NOW, PEP, QCOM, SNPS, TMO, UNH, WMT	6.9515	8.2775	7.2099	6.9982
Pooled Disjoint-Firm ($N = 180$)	**180**	**96**	$\sqrt{\frac{1}{180}\sum_{k=0}^{4} N_k \mathrm{RMSE}_k^2}$ (0 shared firms across folds)	**8.6554**	**9.4133**	**8.7534**	**8.7659**

In 4 of the 5 training folds (`2022`, `2023`, `2024`, `2026`), nested inner cross-validation selects maximum regularization ($\alpha = 10{,}000$ for `RidgeCV`, $\alpha = 100$ for `LassoCV`, and $c_{\text{opt}} = 0.0000$ for `Nested Scalar-Shrunk OLS`), shrinking the text slope completely to zero. Across all validation schemes—5-fold leave-one-year-out CV (`8.6554`), 5-fold disjoint-firm `GroupKFold` CV (`8.6554`), strictly chronological expanding-window CV (`2023–2026`, `8.4949`), and a 500-draw permutation null (`mean = 9.1970, SD = 0.1460` for OLS)—**written filing narratives provide zero out-of-sample 30-day forecasting edge over a constant zero abnormal return prediction**.

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4. Methodological Scope and Design Limitations

1. **Stratified Quota Sample of Liquid S&P 500 Firms:** Our balanced panel of 18 filings per GICS sector and 36 filings per calendar year across 96 S&P 500 firms is a stratified quota sample rather than a simple random draw of the full SEC EDGAR filing universe. Although our announcement-window findings hold under both unweighted OLS and S&P 500 sector-capitalization or inverse-firm-frequency weighted WLS (Section 3.1.1), the results apply strictly to large-cap S&P 500 issuers and do not generalize to smaller, illiquid, or non-S&P

500 firms.

2. **Conditional Observational Association Using Time-Series XBRL SUE vs. Commercial Analyst Consensus:** While the PEAD literature constructs earnings-surprise proxies from the time-series of earnings, market prices, and analyst forecasts [5], our public SEC XBRL time-series controls (`sue_xbrl`, `xbrl_eps_yoy_pct`, `xbrl_rev_yoy_pct`) and text-extracted guidance controls (`plan_vs_hype`, `guid_num_density`, `guid_net_revision`) do not observe proprietary commercial analyst consensus EPS forecasts (IBES/Street estimates) or complete quantitative management guidance surprises disclosed simultaneously in the same release. Consequently, all `blame_asymmetry` coefficients (`-5.9921`, `-5.7166`, `-5.6197`, `-11.3201`) are strictly conditional observational associations and cannot be interpreted as causally identified pure text effects.
3. **Modest Single-Rater Reliability (`ICC(2,1) = 0.3935` Across 4 Raters, `0.5654` Across 3 LLMs) and Absence of Human Expert Coders:** As shown in Table 1B and Table 2A, single-rater reliability for `blame_asymmetry` is modest ($\mathrm{ICC}(2,1) = 0.3935$ across all 4 raters and 0.5654 across the 3 LLMs), individual-rater significance is driven by the LLM judges (`blame_gemini` and `blame_llama`, with `blame_deepseek` $p = 0.0659$) rather than the deterministic rule scorer (`blame_ling`, $p = 0.7566$), and the study lacks a human-expert coded validation subsample.
4. **Underpowered 21-Trading-Day Drift Test in Mega-Cap Equities:** With $N = 180$ liquid S&P 500 filings and $\sigma_Y = 8.68\%$, the 21-trading-day abnormal return regression has a non-circular analytic 80%-power Minimum Detectable Effect of 1.82–2.30 percentage points per 1-SD shift in `question_evasion`, and an empirical cluster- and overlap-adjusted 80%-power MDE of 2.02–2.28 pp on `post_reaction_30d_car_pct` (2.15–2.31 pp on `realized_30d_pct`, with cluster-adjusted power of 7.9%–28.5% for 0.5–1.5 pp effects). Therefore, our null 21-trading-day findings (`-4.4713`, $p = 0.4956$ aligned; `-6.5194`, $p = 0.3299$ unaligned) are an underpowered design limit and cannot rule out economically meaningful post-announcement drift in the illiquid, high-uncertainty, or high-arbitrage-risk populations where theory predicts PEAD concentrates [12] [13] [14] [15].
5. **2,500-Character Lead-Narrative Window Truncation (`excerpt_char_len`):** Because our SEC EDGAR extraction pipeline truncates earnings narratives at the last complete word/punctuation boundary $\leq 2,500$ characters (`excerpt_char_len` range `929–2,492`, median `2,488.0`, IQR `2,479.8–2,490.0`) to exclude multi-page GAAP financial tables and non-GAAP reconciliation schedules, narrative attributions or guidance qualifications appearing later in long press releases beyond the opening `2,500`-character window are not captured. While controlling for `excerpt_char_len` (Table 2 Rows 13a–13b: `-5.9133, p = 0.0412`; `-5.1732, p = 0.0067`; and `-10.1739, p = 0.0023` on $N = 89$), testing multiplicative interactions with `excerpt_char_len` ($p = 0.8144$ and $p = 0.1073$) or with the $\geq 2,450$-character cap indicator ($p = 0.5939$ and $p = 0.2300$), and pooling the uncapped `< 2,491`-character strata (`T1 + T2`, $N = 141$, Rows 13c–13d: `-8.5258, p = 0.0296` on pre-open $N = 70$ and `-4.5224, p = 0.0483` parsimonious on $N = 141$) confirm a negative association outside `T3`, effect magnitude concentrates in the upper-length tercile (`2,491–2,492` chars, $N = 39$, `-11.0348, p = 0.0030` on Day-1 return and `-23.0405, p = 0.0003` on CAR vs. `-3.5749` and `-3.5951` in `T1` and `T2`).

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