Testing AI Washing Versus Verifiable Financial Receipts on 180 SEC Form 8-K Earnings Releases: Pre-Registered Nulls, Sector Benchmarks, and Exploratory Limits (2023-2025)

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Abstract

We test whether corporate "AI Washing"—promotional artificial intelligence framing and guidance vagueness unbacked by verifiable quantitative financial receipts, combined in a zero-clipped composite index ($W = \text{clip}(0.45H + 0.55U - 0.65V, 0, 1)$) and a centered continuous index ($W_{\text{centered}} = \text{clip}(0.50+0.45H+0.55U-0.65V, 0, 1)$)—has a descriptive observational association with announcement reaction-day abnormal returns and strictly post-reaction 21-trading-day (30-calendar-day) cumulative abnormal returns across 180 non-overlapping SEC Form 8-K Item 2.02 (Exhibit 99.1) quarterly earnings releases filed by 45 publicly traded U.S. technology firms between January 24, 2023 and March 20, 2025. Because the 45-firm universe was selected with hindsight of 2023–2025 technology prominence (on November 1, 2026) and our 300-word excerpts represent 7.3% of full release length with 5.42 times artificial intelligence term density enrichment (3.57 times early, 6.26 times late), all estimates are descriptive selected-excerpt associations that do not establish full-filing construct validity or broader market generalizability. Four raters (three large language models and one deterministic lexical analyzer) score each filing excerpt, achieving three-model Shrout-Fleiss average-measures intraclass correlation 0.9156 (0.916 rounded to three decimal places, single-rater 0.783; four-rater average-measures 0.8611, single-rater 0.608) [1]. Both pre-registered confirmatory hypotheses are null in both the pre-registered primary specifications (which control for reaction-day returns on t_{react} to estimate the direct post-reaction drift association conditional on the initial reaction shock) and a documented post-hoc protocol deviation omitting reaction-day returns (which estimates the total post-reaction drift association using pre-event covariates and XBRL revenue growth only, avoiding post-treatment collider conditioning): in the late cohort ($N = 90$ across 45 firms), the controlled ordinary least squares slope on the composite index (Hypothesis H1) is 6.6555 (+6.66%, firm-clustered standard error 5.5659, $t = +1.20$, $p = 0.2318$ against the S&P 500; documented pre-event-only deviation 6.9292, standard error 5.7439, $p = 0.2277$; and 6.6356, +6.64%, standard error 5.0568, $t = +1.31$, $p = 0.1894$ against the technology sector exchange-traded fund XLK; documented pre-event-only deviation 6.8850, standard error 5.2451, $p = 0.1893$), while across the full panel ($N = 180$), the continuous regime interaction (Hypothesis H2) is -10.3424 (-10.34%, standard error 7.0713, $t = -1.46$, $p = 0.1436$ against the S&P 500; documented pre-event-only deviation -9.6973, standard error 7.1670, $p = 0.1760$; and -7.2827, -7.28%, standard error 7.1107, $t = -1.02$, $p = 0.3057$ against XLK; documented pre-event-only deviation -6.7471, standard error 7.2212, $p = 0.3501$). Post-hoc 30-day period shifts C1 (-3.0520, -3.052, -3.05%, standard error 1.4272, $p = 0.0325$ against the S&P 500) and C2 on the elevated centered-index subgroup (-4.6882, -4.69%, standard error 1.8647, $p = 0.0119$ against the S&P 500, $N = 114$) are retired as technology-sector rotation artifacts: benchmarking against XLK reduces C1 to -0.0668 (-0.07%, standard error 1.2917, $p = 0.9588$) and C2 to -1.8304 (-1.83%, standard error 1.6966, $p = 0.2807$), and leaving out the first quarter of 2025 ($N = 135$) collapses both (-1.21%,

$p = 0.4135$; -1.77%, $p = 0.3553$). Within the first quarter of 2025 ($N = 45$ singleton firms, residual degrees of freedom $df = 35$), the controlled slope on the composite index is positive at 15.4446 (+15.44%, HC1 standard error 6.4847, asymptotic normal-z $p = 0.0172$, finite-sample $t(35)$ $p = 0.0228$, four-season Bonferroni $p = 0.0912$; HC3 standard error 7.5734, $t(35)$ $p = 0.0490$, Bonferroni $p = 0.1961$; bivariate +8.05%, $p = 0.2715$), providing no evidence of a within-season AI Washing penalty. Two post-hoc exploratory patterns—the reaction-day shift C3 on filings with promotional intensity at least 0.25 (-5.2690, -5.269, -5.27%, standard error 2.2766, unadjusted $p = 0.0206$, $N = 89$) and the early-cohort composite slope (15.2371, +15.24%, standard error 5.9443, $p = 0.0104$, $N = 90$)—fail robustness checks: C3 fails seven-threshold Bonferroni correction ($\min p = 0.1442 > 0.05$, dropping to -3.23%, $p = 0.2346$ at $H \geq 0.30$), collapses to -2.57% ($p = 0.3423$) when the first quarter of 2025 is excluded, has only 7 filings in the first quarter of 2023, lacks street consensus earnings-per-share surprise controls, and attenuates under 45 firm fixed effects to -5.0771 (-5.08%, HC1 $p = 0.1417$, firm-clustered $p = 0.1744$; -5.91%, $p = 0.0566$ on the $N = 89$ subgroup; within-firm paired difference -3.94%, $p = 0.0528$), while the early-cohort slope comes solely from the first quarter of 2023 (+26.6041, HC1 standard error 9.7958, $t = +2.72$, asymptotic normal-z $p = 0.0066$, finite-sample $t(35)$ $p = 0.0102$, Bonferroni $p = 0.0408$; HC3 standard error 12.7177, $t(35)$ $p = 0.0438$, Bonferroni $p = 0.1751$ versus +5.4076, normal-z $p = 0.5115$, $t(35)$ $p = 0.5158$ in the third quarter of 2023) where the composite index is zero-clipped for 32 of 45 filings (71.1%, and 59 of 90 or 65.6% across the early cohort) and attenuates to +5.94% ($p = 0.0545$) on the unclipped index. Finally, a 22-constant text-based heuristic forecast (mean absolute error 5.8317%) underperforms a text-free regime constant (mean absolute error 5.7783%) by 0.0534 percentage points ($p = 0.2840$).

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Introduction

A central question in accounting and financial economics is how corporate disclosure language, market frictions, and impression management relate to stock returns and post-earnings-announcement drift. Foundational work on post-earnings-announcement drift examines whether post-announcement abnormal returns reflect a delayed price response or an incomplete risk adjustment [2], and subsequent reviews summarize how stock prices continue to drift in the direction of an earnings surprise over an extended period rather than adjusting instantaneously [3]. Examining cross-sectional determinants of post-earnings-announcement drift, Mendenhall [4] shows that the magnitude of post-earnings-announcement drift is strongly related to the arbitrage risk faced by arbitrageurs who view the anomaly as a trading opportunity, while Francis, LaFond, Olsson, and Schipper [5] examine whether rational investor responses to information uncertainty explain the properties of and returns to post-earnings-announcement drift, finding more muted initial market reactions for unexpected earnings signals characterized by greater information uncertainty. Similarly, Ng, Rusticus, and Verdi [6] show that transaction costs constrain the informed trades neces-

sary to incorporate earnings information into price—implying weaker return responses at the time of the earnings announcement and higher subsequent drift—and Chordia, Goyal, Sadka, Sadka, and Shivakumar [7] document that post-earnings-announcement drift occurs mainly in highly illiquid stocks with high trading costs and market impact. Related work examines how institutional ownership composition relates to market reactions to negative earnings announcements [8] and how firm-specific Twitter investor sentiment affects stock returns around negative and positive earnings surprises and post-announcement return reversals [9].

In parallel, research on corporate disclosure language examines whether optimistic and pessimistic language in earnings press releases provides information about future firm performance and triggers market responses after controlling for earnings surprises [10], while literature reviews of corporate reporting examine impression management across financial and non-financial disclosures [11]. At the same time, methodological studies in financial textual analysis demonstrate that general-purpose negative word lists misclassify financial words in 10-K filings and develop finance-specific word lists linked to 10-K filing returns [12], survey textual analysis methods and implementation tripwires in accounting and finance [13], review the construction and use of lexicons in financial textual analysis [14], organize natural language processing methods in a unified framework for automated measurement of disclosure sentiment and readability in accounting [15], and compare machine learning techniques for analyzing unstructured text in finance and accounting [16].

The 2023–2025 enterprise artificial intelligence cycle offers a natural setting to examine these methodological issues. Between early 2023 and early 2025, public technology companies rapidly expanded references to generative artificial intelligence, large language models, and accelerated computing in their quarterly Form 8-K earnings releases. A plausible narrative hypothesis—which motivated our pre-registration—was that equity investors initially rewarded broad artificial intelligence promotional framing during the 2023 boom but subsequently penalized "AI Washing" (high promotional buzzwords and vague forward guidance unbacked by verifiable dollar revenue or margin receipts) from mid-2024 onward.

Yet testing whether corporate artificial intelligence language has a descriptive association with announcement-day or 30-day post-earnings stock returns across an observational panel of technology filings faces four severe econometric challenges. First, pre-registered continuous hypotheses must be strictly separated from post-hoc threshold splits and regime indicators; if confirmatory continuous slopes and regime interactions are statistically null, post-hoc subgroup contrasts cannot be substituted as confirmatory findings. Second, when a panel of corporate filings clusters into only four discrete quarterly earnings seasons, comparing average S&P 500 market-adjusted returns across time periods conflates text-specific effects with technology-sector rotation relative to the broader equity index. Third, because reaction-day returns occur contemporaneously with the release of the earnings filing, conditioning on reaction-day returns when predicting subsequent 21-trading-day drift conditions on a post-treatment variable; regressions must therefore document pre-event-covariates-only specifications (measuring the total post-reaction drift association) alongside the pre-registered primary specifications that control for the initial price shock, and all models must be interpreted strictly as descriptive associations rather than causal effects. Fourth, within-firm variation must be separated from between-firm compositional differences using firm fixed effects, and multi-judge language-model annotations must be audited for single-rater reliability, measurement-error attenuation, zero-clipping floor effects, and excerpt-selection enrichment.

In this paper, we conduct a comprehensive methodological and empirical reckoning on 180 verbatim SEC Form 8-K Item 2.02 (Exhibit 99.1) quarterly earnings releases filed by 45 U.S. technology

companies across four earnings seasons between January 2023 and March 2025. We establish four principal conclusions:

1. **Both pre-registered confirmatory hypotheses (H1 and H2) are null:** In the late cohort (90 filings across 45 firms in the third quarter of 2024 and the first quarter of 2025), the controlled slope of 21-trading-day (30-calendar-day) post-reaction cumulative abnormal returns on the composite AI Washing Index (Hypothesis H1, predicted to be negative) is positive and statistically indistinguishable from zero at 6.6555 (+6.66%, firm-clustered standard error 5.5659, $t = +1.20$, $p = 0.2318$ against the S&P 500 in the pre-registered primary specification; 6.9292, standard error 5.7439, $p = 0.2277$ in the documented pre-event-only deviation omitting post-treatment reaction-day returns; and 6.6356, +6.64%, standard error 5.0568, $t = +1.31$, $p = 0.1894$ against the technology sector fund XLK; 6.8850, standard error 5.2451, $p = 0.1893$ in the pre-event-only deviation). Across the full 180-filing panel, the pre-registered continuous regime interaction between the composite index and the late-regime indicator (Hypothesis H2) is likewise statistically null at -10.3424 (-10.34%, standard error 7.0713, $t = -1.46$, $p = 0.1436$ against the S&P 500; -9.6973, standard error 7.1670, $p = 0.1760$ in the pre-event-only deviation; and -7.2827, -7.28%, standard error 7.1107, $t = -1.02$, $p = 0.3057$ against XLK; -6.7471, standard error 7.2212, $p = 0.3501$ in the pre-event-only deviation). These nulls hold across every individual language-model rater, under 1,000-replication joint firm-cluster bootstrap reliability-scaling bounds, and under mean-centered, Gram-Schmidt orthogonal, and standardized (z-score) quadratic polynomial tests.
2. **Retirement of post-hoc 30-day period shifts (C1 and C2) under technology sector benchmarks and leave-one-season-out diagnostics:** In earlier versions of this study, we highlighted a post-hoc full-panel period shift in 30-day S&P 500 abnormal returns (C1: -3.0520, -3.052, -3.05%, standard error 1.4272, $t = -2.14$, $p = 0.0325$) and a post-hoc subgroup shift among 114 filings with centered index at least 0.38 (C2: -4.6882, -4.69%, standard error 1.8647, $t = -2.51$, $p = 0.0119$). We retire both claims. Although adjusting for pre-event S&P 500 market beta (mean 1.650) leaves C1 (-2.63%, $p = 0.0459$) and C2 (-4.37%, $p = 0.0134$) significant only because the S&P 500 rose +3.12% during the third quarter of 2024, benchmarking technology stocks against the technology sector (XLK or QQQ) eliminates both completely: against XLK, C1 vanishes to -0.0668 (-0.07%, standard error 1.2917, $t = -0.05$, $p = 0.9588$; -0.39%, $p = 0.7718$ against QQQ) and C2 attenuates to -1.8304 (-1.83%, standard error 1.6966, $t = -1.08$, $p = 0.2807$; -2.17%, $p = 0.2227$ against QQQ). Moreover, dropping the first quarter of 2025 ($N = 135$) collapses C1 (-1.21%, $p = 0.4135$) and C2 (-1.77%, $p = 0.3553$) to noise, and within the first quarter of 2025 itself ($N = 45$ singleton firms, $df = 35$), the controlled slope on the composite index is positive at 15.4446 (+15.44%, HC1 standard error 6.4847, normal-z $p = 0.0172$, finite-sample $t(35)$ $p = 0.0228$, four-season Bonferroni $p = 0.0912$; HC3 standard error 7.5734, $t(35)$ $p = 0.0490$, Bonferroni $p = 0.1961$; bivariate +8.05%, $p = 0.2715$), providing no evidence of a within-season penalty.
3. **Strict structural boundaries on exploratory patterns (C3 and Early Boom W):** Across the 89 filings with promotional intensity at least 0.25 across 36 firms, the post-hoc reaction-day abnormal return shift (C3) is -5.2690 (-5.269, -5.27%, standard error 2.2766, $t = -2.31$, unadjusted $p = 0.0206$ against the S&P 500; -5.35%, $p = 0.0158$ against XLK). However, replacing sector indicators with 45 firm fixed effects across all 180 filings attenuates the pooled differential interaction to -5.0771 (-5.08%, HC1 standard error 3.46%, $t = -1.47$, $p = 0.1417$; firm-clustered standard error 3.74%, $t = -1.36$, $p = 0.1744$; -5.91%, $p = 0.0566$ on the $N = 89$ subgroup; within-firm paired difference -3.94%, $p = 0.0528$), dropping the first quarter of 2025 collapses C3 to -2.57% ($p = 0.3423$), the first-quarter 2023 subgroup cell contains only 7

filings, C3 drops to -3.23% ($p = 0.2346$) at $H \geq 0.30$ and none of the 7 threshold cut-offs survives a 7-test Bonferroni correction ($\min p = 0.1442$), and SEC XBRL revenue growth does not control for street consensus earnings-per-share surprises. Similarly, the exploratory early-cohort slope on the composite index (15.2371, +15.24%, standard error 5.9443, $t = +2.56$, $p = 0.0104$ against the S&P 500; +13.29%, $p = 0.0220$ against XLK) comes entirely from the first quarter of 2023 (+26.6041, HC1 standard error 9.7958, $t = +2.72$, asymptotic normal-z $p = 0.0066$, finite-sample $t(35)$ $p = 0.0102$, Bonferroni $p = 0.0408$; HC3 standard error 12.7177, $t(35)$ $p = 0.0438$, four-season HC3 Bonferroni $p = 0.1751$) where 32 of 45 filings (71.1%) are clipped at zero, and attenuates to +5.94% ($p = 0.0545$) on the unclipped index.

4. **Zero forecasting alpha over text-free baselines:** A 22-constant heuristic rule combining our text scores (mean absolute error 5.8317% across all 180 events) has a higher (worse) mean absolute error than a text-free regime constant (mean absolute error 5.7783%) by 0.0534 percentage points (paired $t = +1.07$, $p = 0.2840$) and also underperforms a constant -1.0% rule (mean absolute error 5.822%).

Data and methods

Corporate universe, four-season calendar structure, and exact timestamp alignment

We examine 45 publicly traded U.S. technology companies with SEC Central Index Keys and continuous 2022–2025 equity price histories across four sectors: cloud data, cybersecurity, and networking (12 firms, 48 filings; reference category); enterprise software and software-as-a-service (13 firms, 52 filings); internet and cloud platforms (9 firms, 36 filings); and semiconductors and artificial intelligence hardware (11 firms, 44 filings). To maintain a homogeneous primary-source document corpus, we restrict sampling exclusively to official SEC EDGAR Form 8-K Item 2.02 ("Results of Operations and Financial Condition") filings containing Exhibit 99.1 quarterly earnings press releases (excluding all Form 10-Q cover pages).

For each of the 45 firms, our ingestion pipeline queries the SEC EDGAR submissions history and selects the earliest two eligible Form 8-K Item 2.02 filings between January 1, 2023 and June 30, 2024 (the early cohort, $N = 90$) and the earliest two eligible filings after July 1, 2024 (the late cohort, $N = 90$), enforcing a minimum spacing of at least 75 calendar days between any two filings of the same firm (actual minimum gap = 161 calendar days). Because U.S. technology companies report quarterly earnings on synchronized fiscal calendars, picking the earliest two filings after January 1, 2023 and the earliest two after July 1, 2024 produces four discrete ~8-week earnings seasons of exactly 45 filings each (one filing per firm in each season): the first quarter of 2023 (January 24 to March 28, 2023), the third quarter of 2023 (July 19 to September 27, 2023), the third quarter of 2024 (July 22 to September 25, 2024), and the first quarter of 2025 (January 23 to March 20, 2025). Because the 45-firm universe was selected with hindsight of prominent 2023–2025 AI-cycle technology companies on November 1, 2026 (replacing earlier synthetic pilot samples) and conditioned on continuous 2022–2025 listing history, the firm sample is non-random and survivorship-conditioned; outcome-blind sampling applies only to selecting filing dates within each firm, limiting generalizability beyond large-cap U.S. technology firms.

To align market reaction windows accurately, we extract the exact SEC EDGAR acceptance timestamp in Eastern Time for every filing. Filings accepted before 16:00:00 Eastern Time on a trading day (12 of 180 filings, 6.7%) are assigned reaction day t_{react} equal to the filing date; filings accepted at or after 16:00:00 Eastern Time (168 of 180 filings, 93.3%, with 0 of 180 accepted at or after 17:30 Eastern Time) are assigned reaction day t_{react} equal to the next trading session. Denoting a close-

to-close return from closing price P_a on trading day a to closing price P_b on trading day b as having close-to-close boundary pair $[a, b]$ and spanning the half-open active trading-session interval $(a, b]$ (which aggregates daily returns across sessions $a+1, \ldots, b$), we define: 1. **Reaction-day return and abnormal return on t_{react}:** The raw reaction-day return is $(P_{t_{\text{react}}}/P_{t_{\text{react}}-1} - 1) \times 100$, spanning the single active session $(t_{\text{react}}-1, t_{\text{react}}]$ (close-to-close boundary pair $[t_{\text{react}}-1, t_{\text{react}}]$). Subtracting the concurrent S&P 500 return on session t_{react} yields the reaction-day abnormal return (mean +0.65%, standard deviation 10.52%, range $[-25.16\%, +32.05\%]$), with analogous reaction-day abnormal returns computed against XLK, QQQ, and pre-event CAPM beta. 2. **Strictly non-overlapping 21-trading-day (30-calendar-day) post-reaction cumulative abnormal returns:** Post-reaction drift is measured strictly forward from the reaction-day close $P_{t_{\text{react}}}$ to the closing price 21 trading days later, $P_{t_{\text{react}}+21}$:

$$\text{CAR}_{(t_{\text{react}}, t_{\text{react}}+21]} = \left(\frac{P_{t_{\text{react}}+21}}{P_{t_{\text{react}}}} - 1 \right) - \left(\frac{B_{t_{\text{react}}+21}}{B_{t_{\text{react}}}} - 1 \right),$$

spanning the 21 active trading sessions $(t_{\text{react}}, t_{\text{react}} + 21]$ (sessions $t_{\text{react}} + 1$ through $t_{\text{react}} + 21$, close-to-close pair $[t_{\text{react}}, t_{\text{react}} + 21]$), where benchmark B is the S&P 500 ETF SPY (mean -1.40%, standard deviation 7.80%, range $[-27.96\%, +22.96\%]$), the technology sector ETF XLK (mean -1.63%, standard deviation 7.32%, range $[-25.02\%, +24.96\%]$), the Nasdaq-100 ETF QQQ, or the pre-event CAPM market model ($\hat{\beta}_i \cdot R_{\text{SPY}}$, with $\hat{\beta}_i$ estimated over trading days $[t_{\text{react}}-280, t_{\text{react}}-30]$, mean 1.650, median 1.593). Because $(t_{\text{react}} - 1, t_{\text{react}}]$ and $(t_{\text{react}}, t_{\text{react}} + 21]$ share zero active trading sessions, the bivariate ordinary least squares slope of the 30-day abnormal return on the reaction-day return across all 180 events is near zero at -0.0225 (firm-clustered standard error 0.0659, $p = 0.7326$, $R^2 = 0.0010$; +0.0474, $p = 0.5882$ in the early cohort; -0.1019, $p = 0.2221$ in the late cohort).

Multi-judge text scoring, excerpt enrichment, and reliability diagnostics

From each Form 8-K Exhibit 99.1 press release, our extraction script selects narrative paragraphs up to roughly 1,200–2,400 characters (actual mean 1,987.3 characters, range 1,069 to 4,878; mean 298.4 words, median 288.5, range 159 to 807) by scoring paragraphs for artificial intelligence and executive-commentary terms while filtering out tabular financial lines. To quantify how this excerpt selection alters term density relative to complete press releases, we downloaded and compared 30 full Form 8-K Exhibit 99.1 filings (mean 4,414.9 words) against their extracted excerpts: the excerpts represent 7.3% of full filing character length (7.1% in the early cohort, 1,964 of 28,990 characters; 7.4% in the late cohort, 1,981 of 28,600 characters) and enrich artificial intelligence term density by 5.42 times overall (3.57 times in the early cohort, 2.00 versus 0.56 terms per 1,000 words; 6.26 times in the late cohort, 7.76 versus 1.24 terms per 1,000 words). Thus, our text scores measure keyword-enriched narrative excerpts rather than entire filings, and do not establish construct validity for "AI Washing" across the full filing text.

Each excerpt is scored on $[0, 1]$ by three independent large language models (Google Gemini 3 Flash Preview, DeepSeek V3 Chat, and Meta Llama 3.3 70B Instruct at temperature 0.0) and one deterministic lexical analyzer across three primary dimensions, plus lexical tone sentiment on $[-1, +1]$. In our panel dataset, the primary dimension scores H, V, and U are the equal-weight **four-rater** arithmetic means across the three language models and the lexical analyzer (while separate columns record the three-language-model-only averages and each individual rater's scores): - **Promotional artificial intelligence intensity ($H \in [0, 1]$, four-rater mean 0.294, standard deviation 0.225):** Prominence and density of generative artificial intelligence, large language model, agentic, neural, and transformation framing. - **Verifiable financial receipt specificity ($V \in [0, 1]$,**

four-rater mean 0.597, standard deviation 0.207): Density of auditable quantitative financial disclosures (explicit dollar revenue, annual recurring revenue, basis-point margin changes, remaining performance obligations, and numerical guidance ranges). - **Guidance vagueness and unbacked framing ($U \in [0,1]$, four-rater mean 0.401, standard deviation 0.144):** Extent to which qualitative promotional superlatives outweigh concrete quantitative guidance in the written press release. - **Composite AI Washing Index ($W \in [0,1]$) and centered continuous index ($W_{\text{centered}} \in [0,1]$):**

$$W_i = \text{clip}(0.45H_i + 0.55U_i - 0.65V_i, 0.0, 1.0), \quad W_{\text{centered},i} = \text{clip}(0.50 + 0.45H_i + 0.55U_i - 0.65V_i, 0.0, 1.0).$$

Across all 180 filings, W_i (computed from the four-rater means H_i, U_i, V_i) has mean 0.082 (standard deviation 0.148) and is clipped at zero for 109 of 180 filings (60.6%: 32 of 45 or 71.1% in the first quarter of 2023, 27 of 45 or 60.0% in the third quarter of 2023, 27 of 45 or 60.0% in the third quarter of 2024, and 23 of 45 or 51.1% in the first quarter of 2025), whereas $W_{\text{centered},i}$ retains continuous variation above zero (mean 0.463, standard deviation 0.241).

Table 1 assesses rater reliability across all 180 filings using Shrout and Fleiss intraclass correlation formulas [1].

Construct	Three-model average-measures ICC(2,3)	Three-model ICC(1,3) / ICC(3,3) / single-rater ICC(2,1)	Four-rater average-measures ICC(2,4)	Four-rater ICC(1,4) / ICC(3,4) / single-rater ICC(2,1)
Promotional AI intensity (H)	0.9418 (0.942)	0.941 / 0.963 / 0.844	0.9400 (0.940)	0.939 / 0.953 / 0.797
Verifiable financial receipts (V)	0.9330 (0.933)	0.932 / 0.946 / 0.823	0.8390 (0.839)	0.824 / 0.915 / 0.566
Guidance vagueness (U)	0.8227 (0.823)	0.813 / 0.868 / 0.607	0.7810 (0.781)	0.762 / 0.847 / 0.471
Baseline word tone sentiment	0.9420 (0.942)	0.941 / 0.953 / 0.843	0.8800 (0.880)	0.880 / 0.885 / 0.648
Composite AI Washing Index (W)	0.9156 (0.916)	0.915 / 0.918 / 0.783	0.8611 (0.861)	0.857 / 0.884 / 0.608

Across the three language models, average-measures absolute-agreement reliability ICC(2,3) is 0.9418 (0.942) for H, 0.9330 (0.933) for V, 0.8227 (0.823) for U, and 0.9156 (0.916, single-rater ICC(2,1) = 0.783) for the three-model index. Including the deterministic lexical analyzer yields four-rater average-measures ICC(2,4) = 0.8611 (0.861, single-rater ICC(2,1) = 0.608), matching the four-rater composition of W. We note explicitly that high inter-LLM agreement reflects shared prompt instructions and overlapping pre-training corpora across the three language models rather than validation against human-expert or full-filing ground truth, and that the lexical analyzer correlates moderately with the three-model average on W ($r = 0.5811$, versus $r = 0.8243$ on H) because deterministic regex counts do not capture contextual phrasing; we therefore report individual-rater regressions alongside 1,000-replication joint firm-cluster bootstrap reliability-scaling bounds.

Econometric specifications, pre-registered hierarchy vs pre-event deviation, and cluster-robust inference

In our multivariate regressions predicting 21-trading-day post-reaction cumulative abnormal returns, we control for audited year-over-year quarterly revenue growth extracted from SEC EDGAR

XBRL company facts for the reported quarter, pre-announcement 20-trading-day stock return momentum over $[t_{\text{react}} - 21, t_{\text{react}} - 1]$, pre-announcement 20-trading-day annualized realized volatility, baseline word tone sentiment, and sector fixed effects (with cloud data, cybersecurity, and networking as the reference sector). Our pre-registration specified the models controlling for the reaction-day return on t_{react} as the primary confirmatory specifications (measuring direct post-reaction drift conditional on the initial price shock). Because the reaction-day return on t_{react} is realized contemporaneously with the earnings release, conditioning on it in a post-reaction drift regression conditions on a post-treatment variable and can induce collider bias when estimating the total post-reaction drift association; we therefore retain the reaction-day-controlled models as the pre-registered primary specifications and report pre-event-covariates-only models omitting the reaction-day return alongside them as a documented post-hoc protocol deviation, interpreting all estimates strictly as descriptive conditional associations rather than causal effects. To account for within-cluster error correlation across repeated observations per firm, we compute cluster-robust standard errors grouped by firm across the $G = 45$ corporate clusters [17]. Because the panel spans only $G_2 = 4$ discrete earnings seasons—making two-way firm-by-time clustering infeasible—we assess temporal stability via season fixed effects and leave-one-season-out sensitivity, and in single-season regressions ($N = 45$ singleton firm clusters, residual degrees of freedom $df = 35$) we report finite-sample $t(35)$ p-values under both HC1 and leverage-adjusted HC3 standard errors alongside four-season Bonferroni adjustments.

Results

Confirmatory hypotheses (H1 and H2) and retirement of period shifts (C1 and C2)

Table 2 presents our confirmatory regressions for Hypothesis H1 (late cohort, $N = 90$) and Hypothesis H2 (full panel, $N = 180$) alongside the retired post-hoc period-shift model C1 ($N = 180$), evaluated against both the S&P 500 (SPY) and the technology sector (XLK), both in the pre-registered primary specifications (with the reaction-day return control) and in the documented post-hoc pre-event-only deviation specifications omitting the post-treatment reaction-day return.

Independent variable	H1 late cohort (SPY, pre-reg primary)	H1 late cohort (SPY, pre-event deviation)	H1 late cohort (XLK, pre-reg primary)	H2 full panel (SPY, pre-reg primary)	H2 full panel (SPY, pre-event deviation)	H2 full panel (XLK, pre-reg primary)	C1 period shift (SPY, with reaction return)	C1 period shift (XLK, with reaction return)
Composite AI Washing Index (W) [confirmatory in H1; non-confirmatory Post=0 baseline in H2]	6.6555 (5.5659) [$p = 0.2318$]	6.9292 (5.7439) [$p = 0.2277$]	6.6356 (5.0568) [$p = 0.1894$]	15.7831 (5.1586) [$p = 0.0022$, non-confirmatory Post=0 baseline]	15.2391 (5.1481) [$p = 0.0031$, non-confirmatory Post=0 baseline]	13.4223 (5.2189) [$p = 0.0101$, non-confirmatory Post=0 baseline]	—	—
Late regime indicator (Post)	—	—	—	-2.1306 (1.4938) [$p = 0.1538$]	-2.0981 (1.5040) [$p = 0.1630$]	0.6075 (1.3607) [$p = 0.6552$]	-3.0520 (1.4272) [$p = 0.0325$]	-0.0668 (1.2917) [$p = 0.9588$]
Interaction (Post × W) [confirmatory H2 interaction]	—	—	—	-10.3424 (7.0713) [$p = 0.1436$]	-9.6973 (7.1670) [$p = 0.1760$]	-7.2827 (7.1107) [$p = 0.3057$]	—	—

Independent variable	H1 late cohort (SPY, pre-reg primary)	H1 late cohort (SPY, pre-event deviation)	H1 late cohort (XLK, pre-reg primary)	H2 full panel (SPY, pre-reg primary)	H2 full panel (SPY, pre-event deviation)	H2 full panel (XLK, pre-reg primary)	C1 period shift (SPY, with reaction return)	C1 period shift (XLK, with reaction return)
Reaction-day return (%) [post-treatment]	-0.0936 (0.0772)	— [omitted]	-0.0853 (0.0689)	-0.0447 (0.0616)	— [omitted]	-0.0371 (0.0566)	-0.0269 (0.0621)	-0.0223 (0.0568)
XBRL YoY revenue growth (%)	-0.0305 (0.0694)	-0.0411 (0.0671)	-0.0401 (0.0610)	0.0205 (0.0403)	0.0200 (0.0395)	-0.0017 (0.0353)	0.0061 (0.0418)	-0.0127 (0.0363)
Pre-event 20-day momentum (%)	-0.1020 (0.1105)	-0.1109 (0.1123)	-0.1420 (0.0975)	-0.0678 (0.0775)	-0.0690 (0.0775)	-0.1088 (0.0715)	-0.0895 (0.0783)	-0.1300 (0.0736)
Pre-event 20-day volatility (%)	-0.2087 (0.0490)	-0.2058 (0.0503)	-0.2030 (0.0457)	-0.1132 (0.0526)	-0.1145 (0.0520)	-0.1237 (0.0480)	-0.0882 (0.0629)	-0.1021 (0.0558)
Baseline word tone sentiment	-0.8657 (3.0164)	-0.2247 (3.1910)	-1.4356 (2.9015)	0.6154 (2.6243)	0.7727 (2.7007)	1.1371 (2.5822)	-0.0122 (2.4731)	0.4785 (2.4283)
Enterprise software sector	0.4323 (1.8689)	0.4819 (1.8805)	-0.7965 (1.7421)	1.1311 (1.3150)	1.1072 (1.2997)	0.5321 (1.2743)	0.9880 (1.3976)	0.4341 (1.3226)
Internet and cloud platform sector	-0.3874 (2.9557)	-0.3910 (2.9412)	-0.9723 (2.8329)	-0.5350 (1.6330)	-0.5404 (1.5840)	-0.2259 (1.6338)	-0.4291 (1.5185)	-0.1388 (1.5281)
Semiconductor and AI hardware sector	3.2512 (2.1552)	3.6897 (2.2937)	2.3585 (1.9728)	0.1489 (1.5798)	0.3079 (1.6575)	0.1528 (1.4859)	0.1032 (1.5770)	0.1147 (1.4717)
Intercept	5.0801 (2.8680)	4.6300 (2.9570)	7.4427 (2.6681)	2.4635 (3.0912)	2.3769 (2.9848)	1.5846 (3.0066)	3.4973 (3.2522)	2.5141 (3.1183)
Observations / firm clusters	90 / 45	90 / 45	90 / 45	180 / 45	180 / 45	180 / 45	180 / 45	180 / 45
R^2	0.1531	0.1389	0.1733	0.1283	0.1247	0.1409	0.0800	0.0986

As shown in Table 2, both pre-registered confirmatory hypotheses are refuted. In the late cohort ($N = 90$), the pre-registered primary controlled slope on the composite AI Washing Index (Hypothesis H1, predicted to be negative) is positive at 6.6555 (+6.66%, firm-clustered standard error 5.5659, $t = +1.20$, $p = 0.2318$ against SPY; documented pre-event-only deviation 6.9292, standard error 5.7439, $p = 0.2277$ omitting the reaction-day return; bivariate slope 5.4170, standard error 5.2555, $p = 0.3026$) and 6.6356 (+6.64%, standard error 5.0568, $t = +1.31$, $p = 0.1894$ against XLK; documented pre-event-only deviation 6.8850, standard error 5.2451, $p = 0.1893$ omitting the reaction-day return). Across the full 180-event panel, the pre-registered primary confirmatory parameter for Hypothesis H2 is solely the continuous regime interaction (Post × W), which is null at -10.3424 (-10.34%, standard error 7.0713, $t = -1.46$, $p = 0.1436$ against SPY; documented pre-event-only deviation -9.6973, standard error 7.1670, $p = 0.1760$ omitting the reaction-day return) and -7.2827 (-7.28%, standard error 7.1107, $t = -1.02$, $p = 0.3057$ against XLK; documented pre-event-only deviation -6.7471, standard error 7.2212, $p = 0.3501$ omitting the reaction-day return). We emphasize that the coefficient on W in the H2 interaction columns of Table 2 (15.7831, standard error 5.1586, $p = 0.0022$ against SPY; 15.2391, $p = 0.0031$ pre-event-only; 13.4223, standard error 5.2189, $p = 0.0101$ against XLK) is **not** a confirmatory full-panel main effect; in an uncentered multiplicative interaction model, that parameter represents only the conditional baseline slope in the

Post = 0 reference period, which is non-confirmatory and reflects the zero-clipped first-quarter 2023 floor effect examined below.

These confirmatory null results are robust across three additional econometric checks: 1. **Individual-rater regressions:** Estimating H1 ($N = 90$) and H2 ($N = 180$) separately across each rater yields null H1 slopes for every rater: +6.5949 (standard error 6.6002, $p = 0.3177$; H2 interaction -10.4624, $p = 0.1819$) for the three-language-model average, +8.7415 (standard error 7.6494, $p = 0.2531$; H2 -10.5802, $p = 0.2544$) for Gemini 3 Flash, +5.8005 (standard error 4.5829, $p = 0.2056$; H2 -9.2561, $p = 0.1253$) for DeepSeek V3, +2.9101 (standard error 6.3694, $p = 0.6478$; H2 -8.5280, $p = 0.2334$) for Llama 3.3 70B, and +1.0821 (standard error 3.7526, $p = 0.7731$; H2 -11.4106, standard error 4.9607, $p = 0.0214$) for the lexical analyzer. 2. **Heuristic reliability-scaling bounds and limits of classical $1/$ICC deattenuation:** In our panel dataset, the composite index W is constructed from the four-rater means H, V, U (whose four-rater average-measures reliability is $\text{ICC}(2,4) = 0.8611$ and single-rater $\text{ICC}(2,1) = 0.6077$), whereas the three-language-model average has reliability $\text{ICC}(2,3) = 0.9156$ (single-rater $\text{ICC}(2,1) = 0.7833$). We emphasize that dividing a multivariate ordinary least squares slope by an intraclass correlation is **not** a valid structural errors-in-variables estimator here: classical $1/\text{ICC}$ deattenuation requires additive, mutually independent measurement error orthogonal to all covariates and an uncensored linear predictor, whereas all four raters evaluate the same excerpt text ($r = 0.5811$ between the three-model average and the lexical analyzer on W), covariates are correlated, and W is nonlinearly clipped at zero for 60.6% of observations. Purely as a heuristic upper-bound sensitivity check using a 1,000-replication non-parametric firm-cluster bootstrap (seed 20261111) that jointly resamples the 45 firm clusters and re-estimates both the regression slope and the intraclass correlation on every draw, scaling H1 (6.6555) and H2 (-10.3424) by three-model $\text{ICC}(2,3) = 0.9156$ yields $6.6555/0.9156 = +7.2690$ (joint cluster-bootstrap standard error 7.2658, $p = 0.3171$; or $6.5949/0.9156 = +7.2028$, $p = 0.3180$ on the three-model average directly) and $-10.3424/0.9156 = -11.2958$ (standard error 8.2870, $p = 0.1729$); scaling by four-rater $\text{ICC}(2,4) = 0.8611$ yields +7.7294 (standard error 7.4612, $p = 0.3002$) and -12.0113 (standard error 8.5054, $p = 0.1579$); and scaling by single-rater $\text{ICC}(2,1) = 0.6077$ yields +10.9513 (standard error 10.9746, $p = 0.3183$) and -17.0179 (standard error 12.6851, $p = 0.1797$), remaining null under every ratio. 3. **Mean-centered, orthogonal, and standardized (z-score) polynomial tests:** Because W in the late cohort ($N = 90$) has a standard deviation of 0.1409 ($\text{SD}^2 = 0.0199$, which scales raw squared coefficients and standard errors by $1/\text{SD}^2 = 50.34$) and the centered index W_{centered} has a standard deviation of 0.2240 ($\text{SD}^2 = 0.0502$, scaling factor 19.92), we test for quadratic curvature using unit-variance standardized z-scores, mean-centered polynomials, and Gram-Schmidt orthogonal polynomials: in the late cohort ($N = 90$), the standardized z-score linear and quadratic coefficients on W are -1.0574 (standard error 1.9631, $p = 0.5901$) and +1.0445 (standard error 1.0378, $p = 0.3142$; mean-centered linear -7.5025, standard error 13.9277, orthogonal basis linear +6.7919, standard error 5.6173, raw quadratic +52.5773, standard error 52.2408), and on W_{centered} are -0.0165 (standard error 0.7976, $p = 0.9835$) and +0.9522 (standard error 0.5619, $p = 0.0901$; mean-centered linear -0.0738, standard error 3.5599, orthogonal basis linear +1.8491, standard error 3.4595, raw quadratic +18.9701, standard error 11.1943). We note that Gram-Schmidt orthogonal linear coefficients are expressed in unit-norm orthogonal basis units and are not directly comparable in magnitude to raw linear slopes, and that null quadratic curvature tests on W do not rescue ordinary least squares linearity under severe zero-clipping (55.6% zero-clipped in the late cohort), which is why we also evaluate the unclipped index W_{centered}.

Four-season decomposition and retirement of C1 and C2 under technology sector benchmarks

Table 3 decomposes the 180-event panel across its four earnings seasons ($N = 45$ per season), comparing 30-day abnormal returns across SPY, XLK, QQQ, and pre-event CAPM beta ($\hat{\beta}_i \cdot R_{\text{SPY}}$, mean pre-event beta 1.650).

Earnings season ($N =$ 45 per season)	Filing date range	SPY 30d mean (%)	XLK 30d mean (%)	QQQ 30d mean (%)	30d CAR vs SPY (%)	30d CAR vs XLK (%)	30d CAR vs QQQ (%)	30d CAR vs CAPM beta (%)	Day-1 CAR vs SPY (%)	Within-season controlled W slope vs SPY [HC1 / HC3 $t(35)$ p & 4-season Bonferroni]
First quarter 2023	2023-01-24..2023-03-28	-1.05	+2.44	+1.86	-0.37	-3.85	-3.27	+0.34	+3.01	+26.6041 (HC1 SE 9.7958, $p_z =$ 0.0066, $t(35)$ $p =$ 0.0102, Bonf $p =$ 0.0408; HC3 SE 12.7177, $p =$ 0.0438, Bonf $p =$ 0.1751)
Third quarter 2023	2023-07-19..2023-09-27	-1.99	-1.77	-1.55	-0.04	-0.26	-0.48	+1.01	+0.53	+5.4076 (HC1 SE 8.2369, $p_z =$ 0.5115, $t(35)$ $p =$ 0.5158, Bonf $p = 1.00$; HC3 SE 11.7596, $p =$ 0.6485)
Third quarter 2024	2024-07-22..2024-09-25	+3.12	+2.99	+2.89	-0.91	-0.77	-0.68	-3.23	+0.69	+2.2743 (HC1 SE 9.6987, $p_z =$ 0.8146, $t(35)$ $p =$ 0.8160, Bonf $p = 1.00$; HC3 SE 12.4365, $p =$ 0.8560)

Earnings season (N = 45 per season)	Filing date range	SPY 30d mean (%)	XLK 30d mean (%)	QQQ 30d mean (%)	30d CAR vs SPY (%)	30d CAR vs XLK (%)	30d CAR vs QQQ (%)	30d CAR vs CAPM beta (%)	Day-1 CAR vs SPY (%)	Within-season controlled W slope vs SPY [HC1 / HC3 $t(35)$ p & 4-season Bonferroni]
First quarter 2025	2025-01-23..2025-03-20	-5.93	-8.60	-7.78	-4.30	-1.62	-2.44	+0.27	-1.61	15.4446 (+15.44%, HC1 SE 6.4847, p_z = 0.0172, $t(35)$ p = 0.0228, Bonf p = 0.0912; HC3 SE 7.5734, p = 0.0490, Bonf p = 0.1961)

Table 3 explains why the post-hoc 30-day period shifts C1 and C2 are retired: 1. **Concentration in a single macro-shock season (first quarter of 2025):** Mean 30-day abnormal returns against SPY are flat across the first three seasons (-0.37% in the first quarter of 2023, -0.04% in the third quarter of 2023, and -0.91% in the third quarter of 2024, which spanned the Federal Reserve's September 18, 2024 interest rate cut) before dropping to -4.30% in the first quarter of 2025 during the late-January 2025 DeepSeek-R1 artificial intelligence capital-expenditure repricing and the February–March 2025 technology tariff and growth selloff. Leaving out the first quarter of 2025 ($N = 135$) collapses C1 (-1.21%, $t = -0.82$, $p = 0.4135$), C2 (-1.77%, $t = -0.92$, $p = 0.3553$), and C3 (-2.57%, $t = -0.95$, $p = 0.3423$) to statistical noise. 2. **Elimination under technology sector benchmarks (XLK and QQQ):** In the first quarter of 2023, the technology sector outperformed the S&P 500 by +3.48 percentage points (XLK +2.44% versus SPY -1.05%), whereas in the first quarter of 2025, the technology sector fell -2.67 percentage points harder than the S&P 500 (XLK -8.60% versus SPY -5.93%). When abnormal returns are benchmarked against XLK rather than SPY, the full-panel period shift C1 vanishes from -3.0520 (-3.052, -3.05%, standard error 1.4272, $t = -2.14$, $p = 0.0325$; -3.0000 omitting the reaction-day return) to -0.0668 (-0.07%, standard error 1.2917, $t = -0.05$, $p = 0.9588$; -0.0237 omitting the reaction-day return; -0.39%, $p = 0.7718$ against QQQ). Similarly, the subgroup shift C2 ($W_{\text{centered}} \geq 0.38$, $N = 114$ across 40 firms) attenuates from -4.6882 (-4.69%, standard error 1.8647, $t = -2.51$, $p = 0.0119$ against SPY) to -1.8304 (-1.83%, standard error 1.6966, $t = -1.08$, $p = 0.2807$ against XLK; -2.17%, $p = 0.2227$ against QQQ). Across all seven threshold cut-offs from 0.25 to 0.50, C2 against XLK is statistically indistinguishable from zero (-0.11% to -3.11%, all unadjusted $p \geq 0.1285$, seven-test Bonferroni $p = 1.00$), and the pooled differential interaction across all 180 filings is null (-2.78%, $p = 0.3066$ against SPY; -2.72%, $p = 0.2624$ against XLK). 3. **No evidence of a within-season penalty on W in the first quarter of 2025:** Within the first quarter of 2025 ($N = 45$ singleton firm clusters with 9 covariates and an intercept, leaving residual degrees of freedom $df = 35$ and no within-firm variation), there is no evidence of a penalty on W: the bivariate slope is statistically null at +8.05% (HC1 standard error 7.32%, $t = +1.10$, $p = 0.2715$; the 22 filings with $W > 0$ averaged -4.52% versus -4.08% for the 23 filings with $W =$

0), and conditional on the 9 controls the exploratory slope is positive at 15.4446 (+15.44%, HC1 standard error 6.4847, asymptotic normal-z $p = 0.0172$, finite-sample $t(35)$ $p = 0.0228$, four-season Bonferroni $p = 0.0912$; leverage-adjusted HC3 standard error 7.5734, $t(35)$ $p = 0.0490$, four-season Bonferroni $p = 0.1961$; +14.69%, $p_z = 0.0193$ against XLK; +17.44%, $p_z = 0.0067$ omitting the reaction-day return; and attenuating to +12.68%, $p_z = 0.0163$ after dropping the three highest-Cook's-D observations, $N = 42$). Because this single-season regression fits 9 covariates on 45 singleton firms ($df = 35$) and is statistically null in bivariate form (+8.05%, $p = 0.2715$) and after four-season Bonferroni adjustment ($p = 0.0912$ HC1 / $p = 0.1961$ HC3), we interpret it strictly as an exploratory diagnostic providing no evidence of a within-season AI Washing penalty during the first-quarter 2025 drawdown. The largest 30-day post-announcement drawdowns in the first quarter of 2025 occurred among high-momentum software and platform firms with zero or near-zero W that had surged +18% to +23% on reaction day in early February and then reversed sharply during the late-February and March 2025 momentum selloff (such as AppLovin with $W = 0.097$, reaction-day +22.96%, 30-day -27.96%; Cloudflare with $W = 0.010$, reaction-day +18.68%, 30-day -20.49%; Palantir with $W = 0.000$, reaction-day +23.32%, 30-day -17.68%; and Pinterest with $W = 0.000$, reaction-day +20.00%, 30-day -14.06%).

Exploratory findings (C3 and Early Boom W), structural limits, and forecast evaluation

All post-hoc specifications (C1, C2, C3, Early Boom W, within-season regressions, and threshold scans) are non-confirmatory exploratory analyses conducted without global family-wise error control: 1. **Exploratory reaction-day shift C3 ($H \geq 0.25$, $N = 89$ across 36 firms):** Within the 89 filings with promotional intensity at least 0.25, the controlled reaction-day shift on the late-regime indicator is -5.2690 (-5.269, -5.27%, firm-clustered standard error 2.2766, $t = -2.31$, unadjusted $p = 0.0206$ against SPY; -5.35%, $t = -2.41$, $p = 0.0158$ against XLK; -5.23%, $t = -2.34$, $p = 0.0195$ against QQQ), and the pooled differential interaction across all 180 filings is -6.39% ($p = 0.0407$ against SPY), -6.90% ($p = 0.0254$ against XLK), and -7.65% (clustered standard error 3.28%, $t = -2.33$, $p = 0.0197$) with 16 sector-by-season fixed effects. However, when sector fixed effects are replaced by 45 firm fixed effects across all 180 filings, the pooled differential interaction attenuates to -5.0771 (-5.08%, HC1 standard error 3.46%, $t = -1.47$, $p = 0.1417$; firm-clustered standard error 3.74%, $t = -1.36$, $p = 0.1744$; on the $N = 89$ subgroup with 36 firm fixed effects, -5.91%, clustered standard error 3.10%, $t = -1.91$, $p = 0.0566$; within-firm paired difference across 31 firms = -3.94%, $t = -2.02$, $p = 0.0528$). Furthermore, dropping the first quarter of 2025 collapses C3 to -2.57% ($p = 0.3423$), the first-quarter 2023 high-promotional cell contains only 7 filings (15.6% of the season, mean +6.24%, driven by C3.ai +32.05% and NVIDIA +13.49%), C3 drops to -3.23% ($p = 0.2346$) at the adjacent threshold $H \geq 0.30$ and fails a seven-threshold Bonferroni correction ($\min p = 0.1442 > 0.05$), and XBRL year-over-year revenue growth does not control for street consensus earnings-per-share surprises. Thus, C3 is an exploratory between-firm association concentrated in the first quarter of 2025 rather than a within-firm language treatment effect. 2. **Exploratory early-cohort slope on W ($N = 90$ across 45 firms):** In the early cohort (the first and third quarters of 2023), the controlled slope of 30-day abnormal returns on W is 15.2371 (+15.24%, firm-clustered standard error 5.9443, $t = +2.56$, $p = 0.0104$ against SPY; +13.29%, $p = 0.0220$ against XLK; +13.90%, $p = 0.0130$ against QQQ; +16.70%, $p = 0.0062$ against CAPM beta). However, this post-hoc slope comes entirely from the first quarter of 2023 (+26.6041, HC1 standard error 9.7958, $t = +2.72$, asymptotic normal-z $p = 0.0066$, finite-sample $t(35)$ $p = 0.0102$, four-season Bonferroni $p = 0.0408$; HC3 standard error 12.7177, $t(35)$ $p = 0.0438$, four-season Bonferroni $p = 0.1751$) rather than the third quarter of 2023 (+5.4076, HC1 standard error 8.2369, normal-z $p = 0.5115$, $t(35)$ $p = 0.5158$), and W is clipped at zero for 32 of 45 filings (71.1%) in the first quarter of 2023 (59 of

90 or 65.6% across the early cohort and 109 of 180 or 60.6% overall). Because severe zero-clipping creates a floor effect that violates ordinary least squares linearity and concentrates leverage in the 13 non-zero observations of the first quarter of 2023, removing zero-clipping via the unclipped index W_{centered} attenuates the early-cohort slope to +5.94% ($t = +1.92$, $p = 0.0545$). 3. **Inferior forecast accuracy relative to text-free baselines:** Across all 180 events, a 22-constant heuristic forecast rule combining our text scores (tuned in-sample on commit `62d9a7d`) achieves a mean absolute error of 5.8317% (5.832% rounded to three decimal places, 103 of 180 directional hits), which is higher (worse) than a text-free regime constant (0.0% in the early cohort and -1.65% in the late cohort; mean absolute error 5.7783%, 5.778%, 101 of 180 hits) by 0.0534 percentage points (paired $t = +1.07$, $p = 0.2840$) and also worse than a constant -1.0% forecast (mean absolute error 5.822%, 109 of 180 hits, paired $t = +0.14$, $p = 0.8852$). In the late cohort ($N = 90$), the text-based rule (mean absolute error 6.355%) similarly trails both the text-free regime constant (6.221%, paired $t = +1.53$, $p = 0.1269$) and the constant -1.0% rule (6.320%). We therefore retire all claims of out-of-sample forecasting value for the text scores.

Discussion

Our study illustrates why benchmark selection, leave-one-season-out sensitivity, within-firm fixed effects, pre-event covariate separation, and pre-registration discipline are critical in financial textual analysis. When we test our pre-registered continuous hypotheses across 180 verbatim SEC Form 8-K Item 2.02 earnings releases from 45 technology firms, neither the late-cohort slope on the AI Washing Index (6.6555 against SPY in the pre-registered primary model; 6.9292 in the documented pre-event-only deviation; 6.6356 against XLK) nor the full-panel regime interaction (-10.3424 against SPY; -9.6973 in the pre-event-only deviation; -7.2827 against XLK) is statistically different from zero. Equally important, two post-hoc 30-day period shifts that appeared statistically significant against the S&P 500 (C1: -3.0520; C2: -4.6882) vanish completely when benchmarked against the technology sector ETF XLK (C1: -0.0668, $p = 0.9588$; C2: -1.8304, $p = 0.2807$) or when the single macro-shock season of the first quarter of 2025 is omitted. Inside the first quarter of 2025 itself ($N = 45$ singleton firms, $df = 35$), the exploratory controlled slope on the composite index is positive (15.4446, HC1 $t(35)$ $p = 0.0228$, four-season Bonferroni $p = 0.0912$; HC3 $t(35)$ $p = 0.0490$, Bonferroni $p = 0.1961$; bivariate +8.05%, $p = 0.2715$), providing no evidence that investors imposed a post-earnings drift penalty on artificial intelligence promotional language during that quarter. Finally, because our 45-firm universe was selected with hindsight of 2023–2025 technology prominence, our 300-word excerpts represent a 5.42-times AI-term-enriched narrative proxy (7.3% of full filing length), our composite index is zero-clipped for 60.6% of filings, and SEC XBRL revenue growth does not measure street consensus earnings-per-share surprises, future research should evaluate full-filing disclosures across broader, ex-ante index-constituent universes with analyst consensus surprise controls.

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