Labor Market Information and Product Market Spillovers: Evidence from Employee Reviews

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Abstract

We study whether labor-market information spills over into the product market and influences consumer purchasing behavior. Because job seekers frequently consult Glassdoor to evaluate potential employers and are also consumers themselves, employee-generated information encountered during job search may subsequently affect their purchasing decisions. We exploit peer firms' earnings announcements as shocks to job-search activity, using variation in peer announcement timing to identify periods when employee-generated information is more likely to be salient to local consumers. We find that negative employee reviews reduce product sales more strongly during periods of heightened exposure to peer firms' earnings announcements, whereas positive reviews have no comparable effect. The effect is stronger when peer firms have more active employee review activity on Glassdoor and when local job-posting intensity is higher, consistent with greater job-search attention amplifying the impact of employee-generated information. Our findings identify a novel informational channel connecting labor and product markets.

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I. INTRODUCTION

Traditionally, information about workplace conditions was largely confined to employees and job seekers through word-of-mouth communication. The rise of online employment platforms such as Glassdoor has fundamentally changed the accessibility of such information by enabling employees to publicly share their experiences regarding workplace culture, management quality, compensation practices, and working conditions. With more than 60 million monthly users worldwide, Glassdoor has become one of the largest repositories of employee-generated information about firms. A growing literature shows that employee reviews contain economically meaningful information about firms and affect the decisions of employees, managers, investors, and regulators. Prior work establishes that employee reviews contain economically meaningful information about firms and are value-relevant to capital-market participants (Briscoe-Tran, 2025; Dube and Zhu, 2021; Gadgil and Sockin, 2026; Sockin and Sojourner, 2023; Teoh et al., 2023). While these studies focus on capital-market stakeholders, whether employees' disclosures also reach and affect consumers remains unexplored. Unlike investors, who actively acquire and process publicly available information, consumers face greater attention constraints and may not systematically incorporate labor-market information into their purchasing decisions. As a result, whether employee-generated information spills over from the labor market to the product market and influences consumer purchasing decisions remains an open empirical question.

When a firm treats its workers poorly, does it also lose customers? Whether such spillovers arise in practice depends on whether employee-generated information can overcome at least two important frictions. The first is an informational friction between the two markets. The labor market and the product market are largely segmented, and information that circulates among employ-

ees and job seekers need not reach consumers at the point of purchase. Acquiring and processing firm-level information is costly, and consumers rationally acquire it only until its expected marginal benefit equals its marginal cost, remaining partially uninformed in equilibrium (Stigler, 1961). Even investors, who have strong incentives to monitor firms, underreact to value-relevant information when their attention is constrained (Barber and Odean, 2008; DellaVigna and Pollet, 2009; Hirshleifer et al., 2009), and such information diffuses only gradually across economically linked yet segmented markets (Cohen and Frazzini, 2008; Hong et al., 2007; Menzly and Ozbas, 2010). Consumers, who have weaker incentives than investors to monitor employer-review platforms, are accordingly less likely to incorporate employee-generated information into their purchasing decisions. Consistent with this friction, Asay et al. (2024) document no consumer purchase response to negative news regarding corporate tax avoidance, notwithstanding survey evidence that consumers disapprove of such practices.

The second friction is that the relevance of information may not transfer across the two markets. Even absent the transmission friction, information about a firm *as an employer*, such as management quality, organizational culture, and the treatment of workers, pertains to attributes that need not be directly informative about the price, quality, and functional characteristics that conventionally determine product choice. Translating disapproval of a firm's workplace conduct into a purchasing response thus requires consumers to treat employer conduct as a product attribute and to act on that preference at the point of sale. Yet a substantial literature documents a persistent gap between consumers' stated social concerns and their realized purchasing behavior: survey-based measures of ethical purchase intentions overstate willingness to pay (Auger and Devinney, 2007), and even ethically minded consumers rarely act on those intentions (Carrington et al., 2010). Learning that a firm is a poor employer may therefore alter consumers' attitudes without altering

their purchasing decisions.

Notwithstanding these frictions, the transmission friction is unlikely to be uniform over time. The individuals who purchase a firm's products also participate in the labor market, and their attention to labor market information rises when they search for jobs. A key institutional feature of Glassdoor's platform design makes this attention partially observable and links it to focal-firm review exposure. Glassdoor's "give-to-get" policy requires users to submit an anonymous review of their current employer before gaining unrestricted access to the platform's full database of employer ratings and compensation data.[1] employees who wish to read reviews of potential prospective employers must therefore first disclose information about their own employer. As deHaan et al. (2023) show, earnings announcements prompt employees of the announcing firm to reassess their employment situation and engage in precisely this type of Glassdoor-mediated job-search activity, a wave of review submissions that simultaneously reveals the announcing firms' workplace conditions and unlocks the broader database for the submitting employees. We extend this single-firm insight to the local labor market: when multiple peer firms headquartered in the same state announce earnings in a given week, employees across those announcing firms simultaneously seek labor market information, browse reviews of nearby firms, and because job search is geographically bounded (Kline and Moretti, 2014; Manning and Petrongolo, 2017; Schmutz and Sidibé, 2019), encounter the focal firm's employee reviews.

With respect to relevance, recent evidence confirms that firm-level information reaches consumers once it becomes salient (Noh et al., 2025), and that consumers respond to non-product

[1]Glassdoor's "give-to-get" policy requires users to submit an anonymous workplace review, salary report, or interview insight before gaining unrestricted access to the platform's database of employer ratings, salary benchmarks, and reviews. This design means that seeking employer information on Glassdoor necessarily involves first disclosing information about one's current employer. Review volume therefore serves as a direct behavioral proxy for the intensity of job-search information-gathering activity on the platform (deHaan et al., 2023).

firm attributes when awareness is high (Bozos et al., 2024; Meier et al., 2023).[2] Consumers may come to regard employer conduct as relevant to their purchasing decisions for several reasons. One possibility is that workplace information serves as a signal of expected product or service quality. Poor workplace conditions, low employee morale, or ineffective management may plausibly translate into poorer customer service, weaker operational execution, or lower quality control, so that employee reviews provide indirect information about the products a firm sells, consistent with evidence that labor-related frictions can affect product quality and customer outcomes (Bao, 2026; Krueger and Mas, 2004; Mas, 2008). Another possibility is that consumers derive utility directly from firms' treatment of employees, even absent any implication for product quality. In this view, workplace practices become a firm attribute that consumers value in much the same way as CEO pay ratios or political positions once these characteristics become salient (Bartling et al., 2015; Hainmueller et al., 2015; Servaes and Tamayo, 2013). Our empirical setting does not require consumers to distinguish between these mechanisms. Whether workplace information is interpreted as a signal of expected product quality or as evidence of firms' employment practices, both channels predict that greater exposure to unfavorable workplace information can reduce demand for the firm's products. Accordingly, we remain agnostic about the dominant mechanism and instead test whether increased exposure to workplace information is reflected in product sales.

Because attention is the binding constraint in both settings, employee-generated information is most likely to influence purchasing precisely when job search renders it salient. This reasoning suggests a novel channel through which labor-market information spills over into the product mar-

[2]Whether heightened salience during job search is also sufficient to overcome the attitude-action gap, the second friction identified above, is a question our design cannot directly resolve. Our evidence is consistent with the view that consumers who hold latent concerns about a firm's workplace conduct are more likely to act on those concerns when employer-related information is particularly salient, but we cannot rule out that other mechanisms contribute to the observed sales response.

ket: consumers who encounter employee disclosures during job search may subsequently revise their purchasing decisions in response to what they read.

Empirically identifying the effect of employee-generated information on consumer purchases presents several challenges. Consumers are unlikely to continuously monitor employee reviews, making it difficult to observe when such information becomes salient. To address this challenge, we exploit peer firms' earnings announcements as shocks to job-search activity. Compared with a focal firm's own earnings announcement, which may directly affect consumer attention, product demand, and other firm outcomes, peer firms' earnings announcements provide a cleaner source of variation in job-search activity in the local labor market. They motivate employees of the announcing firms to engage in Glassdoor-mediated job-search behavior, through the give-to-get mechanism described above, without mechanically shifting the focal firm's product demand. We identify peer firms by headquarters location in the same state as the focal firm's establishment. Because headquarters employees are more likely to pay attention to firm financial reporting (Ham et al., 2026), this construction targets the employees whose job-search responses are most plausibly activated by an earnings announcement. The geographic boundedness of job search then links these headquarters-based shocks to the focal firm: employees of announcing firms, having accessed Glassdoor's database, browse employers in their local area and encounter the focal firm's reviews. Because peer firms are identified by headquarters location rather than product-market proximity, their announcements convey information about local labor-market conditions without generally carrying direct demand signals for the focal firm's specific products. Any residual state-level correlation between peer announcement volume and focal-firm sales is absorbed by the state $\times$ year-quarter fixed effects in the baseline specification. To examine this prediction, we combine Glassdoor employee reviews, Nielsen retail scanner data, and establishment information from Ref-

erenceUSA. We then test whether employee reviews have a stronger effect on product sales during periods with greater exposure to peer firms' earnings announcements.

We find that negative employee reviews have a stronger adverse effect on product sales during periods with greater exposure to peer firms' earnings announcements. Specifically, the interaction between negative employee review intensity and peer firms' earnings announcements is negative and statistically significant even after controlling for firm $\times$ year-quarter and state $\times$ year-quarter fixed effects, which absorb time-varying firm fundamentals and state-level characteristics. These results suggest that consumers become more responsive to negative employee disclosures when attention to employee-generated information increases. The economic magnitude is meaningful: when *Peer EA* increases from zero to its 80th percentile, product sales decline by approximately 1.67% for a firm with average negative review intensity. In contrast, we find no comparable effect for positive employee reviews. This asymmetric response is consistent with a large literature documenting that individuals place greater weight on negative information than on positive information (Baumeister et al., 2001; Chevalier and Mayzlin, 2006; Groening and Kanuri, 2018).

We further validate the proposed labor-market-information channel using two placebo tests: the effect disappears both when the focal firm has no local establishment in the state, consistent with the absence of a local employment relationship, and when *Peer EA* is randomly shuffled across the sample, consistent with the effect depending on the genuine timing and firm-state-specific incidence of peer earnings announcements rather than some fixed but unobserved firm-state characteristic correlated with *Peer EA*. Overall, the evidence suggests that employee-generated information can spill over from the labor market to the product market through the dual role of job seekers and employees.

We then examine whether the results are consistent with the proposed job-search mechanism. If

the pattern of results is consistent with peer firms' earnings announcements amplifying consumer responsiveness to negative reviews through heightened job-search activity, the effect should be stronger in locations with a larger pool of potential job seekers. To test this prediction, we use two complementary proxies: peer-firm employee review activity on Glassdoor and local job-posting intensity from the Revelio database. Across both measures, we find that the negative effect of employee reviews following peer firms' earnings announcements is significantly stronger when the potential job-seeker pool is larger. The consistency of the findings across these distinct measures provides support for the proposed mechanism. Overall, the evidence suggests that peer firms' earnings announcements affect consumer purchases by increasing attention to employee-generated information among potential job seekers.

We also explore additional sources of heterogeneity in local demographic characteristics and product prices. The interaction between negative employee reviews and peer earnings announcements is stronger in states with a larger active working-age population and higher consumer spending, consistent with the effect being more pronounced where a larger share of the local population is likely to encounter employee reviews and has greater purchasing power. We also find that the effect is concentrated among lower-priced products, with no comparable effect for higher-priced items whose purchase decisions may instead be driven by product-specific characteristics.

Finally, our results are robust to a battery of additional tests. The findings hold after controlling for ESG incidents, the content of early employee reviews, and the cumulative abnormal return associated with peer earnings announcements. The results are also robust to alternative samples, including excluding observations in which peer and focal earnings announcements coincide, as well as to alternative specifications, including using Poisson regression.

The remainder of the paper proceeds as follows. Section II reviews the related literature and

discusses our contributions. Section III describes the data, variable construction, and sample characteristics. Section IV presents the main results, as well as placebo tests and robustness checks. Section V provides additional evidence on the proposed job-search mechanism through cross-sectional tests, and further examines heterogeneity by local demographic characteristics and product prices. Section VI concludes.

II. RELATED LITERATURE

Our study relates to and contributes to three streams of literature. First, we contribute to the literature on employee information and employee reviews. Prior studies show that employees possess valuable information about firms that is often unavailable to external stakeholders, with insights into firms' ESG performance, workplace practices, corporate misconduct, and organizational culture (Briscoe-Tran, 2025; Dube and Zhu, 2021; Gadgil and Sockin, 2026; Sockin and Sojourner, 2023; Teoh et al., 2023). A substantial body of this work establishes that such information is value-relevant to capital-market participants: employee satisfaction and crowdsourced employer ratings predict firms' future earnings and stock returns (Edmans, 2011; Green et al., 2019). While this literature demonstrates that employee disclosures influence employees, managers, investors, and regulators, their consequences for consumers remain largely unexplored. We extend it by showing that employee-generated information affects consumer purchasing behavior, identifying consumers as an important stakeholder group that responds to employee disclosures.

Second, our study contributes to the literature examining consumer responses to corporate information. Prior research shows that consumers react to a range of information sources, including product reviews, earnings announcements, ESG disclosures, ESG incidents, and CEO compensation disclosures (Bozos et al., 2024; Chevalier and Mayzlin, 2006; Duan et al., 2023; Meier et al.,

2023; Noh et al., 2025). Whether consumers respond to non-product, firm-level information of this kind is far from settled. On one hand, the gap between consumers' stated social concerns and their realized purchasing behavior is well documented (Auger and Devinney, 2007; Carrington et al., 2010), and consumer purchase responses to firm-level disclosures are often muted (Asay et al., 2024). On the other hand, consumers demonstrably act on non-product attributes once those attributes are made salient (Bartling et al., 2015; Hainmueller et al., 2015; Servaes and Tamayo, 2013). We contribute two distinct insights to this stream. First, we extend the set of information sources that affect consumer demand: existing studies examine firm-generated disclosures (earnings announcements, ESG reports, CEO pay) or third-party product information (consumer reviews). We show that employee-generated workplace information, produced by workers for job seekers, not by firms for consumers, can also shape purchasing decisions. Second, and more broadly, we identify job-search salience as a novel mechanism through which any labor-market information can reach consumers: periods of heightened job-search activity serve as a bridge between otherwise segmented labor and product markets, converting employer-review platforms into a channel for product-market spillovers.

Third, our study contributes to the literature on spillover across markets. A central theme of this literature is that value-relevant information diffuses only gradually across segmented markets, because investors specialize and attend primarily to their own markets (Cohen and Frazzini, 2008; Hong et al., 2007; Menzly and Ozbas, 2010); a CEO's political statements, for instance, can trigger boycott and buycott movements on social media that move product sales (Liaukonyte et al., 2023). Most closely related to our study is the spillover of labor-market conditions into product markets. The evidence to date operates through the producer channel, whereby labor strife degrades the product itself: tires produced during a contentious strike exhibited higher defect rates (Krueger

and Mas, 2004), construction equipment manufactured during labor unrest received worse appraisal reports (Mas, 2008), and firms facing higher mandated social-insurance contributions committed more product-quality violations as labor costs rose (Bao, 2026). A related strand links the two markets through consumers directly: Li and Vansteenkiste (2025) show, using barcode-level scanner data, that firms sell more and charge higher prices in counties where they employ workers, an effect operating through local employment provision rather than information about workplace conditions. We identify a distinct, informational channel that holds the product fixed: through the dual role of individuals as both consumers and job seekers, employee-generated information encountered during job search enters purchasing decisions. The institutional mechanism enabling this channel is Glassdoor's give-to-get policy, which requires users to submit a review of their current employer before accessing reviews of other firms. When earnings announcements motivate employees of peer firms to seek labor-market information, the give-to-get constraint means they must first review their own employers, making review volume a behavioral signal of job-search activity, and then, having gained platform access, they browse reviews of nearby firms in the local labor market, including the focal firm.

III. DATA, VARIABLE CONSTRUCTION, AND SAMPLE DESCRIPTION

A. Data and Sample

Our analysis combines product market, labor market, and capital market data. The primary data sources are the Nielsen Retail Scanner Database and Glassdoor employee reviews. Nielsen provides weekly sales information for consumer products across retail stores in the United States, allowing us to observe local consumer purchasing behavior. To identify the publicly listed firm associated with each product, we first extract the manufacturer prefix (the first 6–11 digits) from

each product's Universal Product Code (UPC) and match it to the GS1 Global Company Prefix database, which provides the corresponding manufacturer name. We then standardize manufacturer names and match them to Compustat firms, with manual verification of ambiguous matches, to obtain the corresponding *GVKEY* identifiers. Glassdoor provides employee-generated evaluations of workplace experiences, including both positive and negative aspects of employment. We further obtain earnings announcement dates and firm characteristics from Compustat, which are used to construct measures of peer earnings announcement exposure and firm-level control variables, respectively. ESG incident data are obtained from the RepRisk database. Combining these datasets enables us to jointly observe local consumer purchases, employee-generated workplace information, and peer firms' information releases within a common geographic setting, allowing us to examine how labor-market information spills over to product-market outcomes.

Table 1 reports the sample construction procedure. We begin with firms covered by the Nielsen Retail Scanner Database that can be matched to publicly listed firms through GS1 identifiers and Compustat. We require stores to have valid geographic information and aggregate product sales to the firm-state-week level. The state is the finest geographic unit at which we can consistently link product sales with firms' establishment information. We further require firms to have establishment (including headquarters) information to ensure that the focal firm maintains a local employment presence in a state, making it plausible that local job seekers may encounter the firm's Glassdoor reviews when searching for employment opportunities. In addition, firms must have available Glassdoor reviews, which constitute the primary source of employee-generated information examined in this study. After imposing these requirements, excluding observations with missing variables, and dropping singleton observations after absorbing the firm$\times$year-quarter and state$\times$year-quarter fixed effects used in the baseline specification, the final sample consists of 198

firms and 823,566 firm-state-week observations from 2008 to 2023. We use the week as the unit of time because it is the finest temporal frequency available in the Nielsen data and is well suited for capturing the immediate effects of peer firms' earnings announcements on job-search activity and subsequent consumer purchasing behavior.

Table 2 provides descriptive information on the composition of our sample and compares sample firms with the broader Compustat universe. Panel A reports the number of unique firms by calendar year. The number of firms increases steadily over time in both the Pre-Glassdoor and final samples. The time-series patterns are broadly similar across the two samples, suggesting that the Glassdoor coverage requirement does not materially alter the temporal composition of the sample. Panel B reports the industry distribution based on the Fama–French 12-industry classification. Both samples are concentrated in consumer-facing industries, particularly Consumer Non-Durables, Manufacturing, and Wholesale & Retail, which is expected given the retail-sales focus of the Nielsen database.

Panel C compares firm characteristics across the Pre-Glassdoor sample, the final sample, and the Compustat benchmarks. The final sample closely resembles the Pre-Glassdoor sample in terms of assets, employment, leverage, market value, and market-to-book ratios, suggesting that the Glassdoor availability requirement primarily affects sample coverage rather than firm characteristics. Relative to the broader Compustat universe, firms in our sample tend to be larger, employ more workers, and have higher market values. These differences are expected because Nielsen primarily covers firms with substantial consumer-product operations and broad retail distribution networks. Overall, the evidence suggests that our sample is representative of large consumer-facing firms for which employee-generated information is likely to be economically meaningful for both potential employees and consumers.

B. Variable Construction

Our analysis focuses on two key variables: employee review disclosure and peer earnings announcement exposure. This section describes the construction of these variables. Definitions of other variables are provided in Appendix Table A.1 and discussed in the relevant sections of the paper.

Employees voluntarily provide assessments of workplace experiences on Glassdoor through separate free-text fields describing positive aspects (*Pros*) and negative aspects (*Cons*) of employment. To measure the intensity of employee review disclosure, we first identify whether each review contains substantive content that is consistent with the intended review category. Specifically, we use a large language model (GPT-4o) to label a random sample of 10,000 reviews for whether their *Pros* and *Cons* fields report genuine workplace content, excluding responses that do not provide meaningful positive or negative information, including blank entries, generic placeholders (e.g., none" or N/A"), and responses whose content is inconsistent with the designated review field (e.g., positive statements appearing in the *Cons* field). Because applying the LLM to all 6.69 million reviews individually is prohibitively costly and would not be replicable across model versions, we use these labels to fine-tune a lightweight classifier, which we then apply to classify every review in the full sample.[3] We then count the number of words contained in each review field that is classified as informative and aggregate reviews to the firm-week level. We construct *Cons* (*Pros*) as the review-count-weighted average number of words in the negative (positive) review field over the previous 51 weeks. These measures capture the intensity of employee-generated workplace information available over the recent year.

[3] Online Appendix OA.2 describes the labeling procedure, classifier training and validation, and full-sample inference in detail.

To capture variation in job-search activity across the local labor market, we construct a measure of peer earnings announcement exposure. Following the mechanism described in the introduction, Peer EA captures the density of peer-firm earnings announcements in the local labor market during week $t-1$. Formally, *Peer EA* is defined as the number of peer firms headquartered in the same state as the focal firm's establishment that announce earnings during week $t-1$, divided by the total number of such peer firms.[4]

We extend the single-firm insight of deHaan et al. (2023) to a local-market setting by summing this mechanism across announcing firms. In a given week, *Peer EA*, the fraction of peer firms headquartered in the same state as the focal firm's establishment that announce earnings, captures the breadth of this job-search activation across the local labor market. A higher value of *Peer EA* means that more labor market participants in the same geographic area are simultaneously motivated to seek labor market information, submit reviews of their own employers, and browse the focal firm's employee reviews. *Peer EA* therefore measures the density of the job-search wave passing through the local labor market in a given week, which determines how many potential consumers are exposed to the focal firm's Glassdoor page. We identify earnings announcement dates using Compustat and define *Peer EA* formally as the number of peer firms with an earnings announcement during week $t-1$ divided by the total number of peer firms headquartered in the corresponding state.

Figure 1 summarizes the construction of the key variables and the empirical setting used

[4]Importantly, our measure of peer earnings announcement exposure rests on the premise that earnings announcements by firms in the local labor market raise the general salience of career-related information. It could also activate job-search behavior among a *broad* pool of local workers, not only employees of the announcing firms. They could also browse information as long as they have left reviews in the same year. These reviewers may not be fully captured by (deHaan et al., 2023). When multiple firms headquartered in the same state announce earnings in a short window, local workers are more likely to reassess their labor-market options and to consult employer-review platforms such as Glassdoor.

throughout the paper. Panel 1a illustrates the timing of variable measurement. Employee review disclosure is accumulated over the previous 51 weeks to capture the relevant stock of workplace information available when workers search for jobs. Peer earnings announcements are measured during week $t-1$, while store-state sales are measured in week t, ensuring that labor-market information precedes consumer purchasing behavior. Panel 1b illustrates how peer firms are constructed. For each focal firm-state observation, peer firms are defined as firms whose headquarters are located in the same state as the focal firm's establishment, thereby capturing labor-market interactions within the same local geographic area. We define peer firms based on geographic proximity because an important step of the mechanism, browse local employers, requires job search to be spatially constrained. Spatial mobility is more limited than industry or occupational mobility, and workers typically search for alternative employment within their local labor market before considering opportunities elsewhere (Kline and Moretti, 2014; Manning and Petrongolo, 2017; Schmutz and Sidibé, 2019). Geographically proximate firms therefore constitute the relevant choice set for job seekers in the local area, making them the most plausible source of focal-firm review exposure. This headquarters-based geographic definition also serves an identification purpose: peer firms in the same state span many industries and consumer markets, so their announcements convey information about local labor-market conditions without generally carrying product-demand signals for the focal firm's specific goods. In Panel 1b, State A and State B illustrate the baseline sample, where the focal firm has both an establishment and retail sales. State C is excluded because retail sales are unavailable. State D represents observations where the focal firm has retail sales but no local establishment and therefore provides no local employment opportunities. These observations are excluded from the baseline sample but used for one placebo test.

C. Summary Statistics

Table 3 reports summary statistics for the key variables used in the analysis. Panel A presents overall summary statistics. On average, *Cons* and *Pros* contain 31 and 20 words, respectively, indicating that employees provide substantial workplace-related information through Glassdoor reviews. The mean value of *Peer EA* is 0.07. Given that the average focal firm has approximately 129 peer firms, this corresponds to roughly nine peer firms announcing earnings in a typical week. Panel B reports annual averages of the main variables. Both *Cons* and *Pros* exhibit a gradual decline over the sample period, whereas *Peer EA* remains relatively stable over time. These patterns suggest that employee review disclosure evolves over the sample period, while the frequency of peer firms' earnings announcements remains relatively constant.

Panel C reports industry-level averages based on the Fama–French 12-industry classification. Across the nine largest industries, cross-industry variation in *Pros* is compressed, ranging from 18.4 words (Manufacturing) to 21.1 words (Business Equipment), a spread of roughly three words. *Cons* exhibits wider dispersion, ranging from 28.8 words (Healthcare) to 35.9 words (Other), a spread of around seven words, and is highest in the Other, Wholesale & Retail, and Consumer Durables industries. Telecommunications is a notable exception: despite accounting for only 0.1% of sample observations, it is the only industry where *Pros* (35.9) and *Cons* (36.2) are nearly equal, suggesting that the small number of firms in this category have unusually voluminous reviews in both valences. The gap between *Cons* and *Pros*, which captures the relative emphasis on negative over positive content, also varies markedly across sectors. The gap is largest in the Other (15.4 words) and Wholesale & Retail (14.7 words) industries, where negative review content substantially outweighs positive, and smallest among Business Equipment (8.8 words) and Healthcare (9.1

words) firms. This pattern is consistent with prior work documenting lower reported job satisfaction in labor-intensive, customer-facing industries relative to technology and professional-services sectors.

Panel D reports state-level averages. California accounts for the largest number of firm-state-week observations. Nevertheless, the levels of *Cons*, *Pros*, and *Peer EA* are broadly similar across states, indicating that no single geographic region disproportionately drives the variation in the key variables.

IV. MAIN RESULTS

A. Baseline Results

Our central hypothesis is that employee reviews become more influential when potential job seekers are more likely to search for employer-related information. Following the give-to-get mechanism described above, periods with more peer-firm earnings announcements expose the focal firm's reviews to a larger pool of potential consumers. We test this prediction using the baseline specification:

$$\begin{aligned} \textit{Sales (log)}_{i,s,t} = {} & \beta_1 \textit{Reviews}_{i,t-1} + \beta_2 \textit{Peer EA}_{i,s,t-1} \\ & + \beta_3 \textit{Reviews}_{i,t-1} \times \textit{Peer EA}_{i,s,t-1} + \text{FEs} + \varepsilon_{i,s,t}, \end{aligned} \tag{1}$$

where $\textit{Reviews}_{i,t-1}$ represents $\textit{Cons}_{i,t-1}$ and $\textit{Pros}_{i,t-1}$, is the mean word count of negative and positive employee review content lagged 1 to 51 weeks, respectively. $\textit{Peer EA}_{i,s,t-1}$ is the lagged sum of scaled peer earnings announcement store-state exposure.

Table 4 reports the baseline estimates of the relation between employee reviews, peer firms' earnings announcements, and product sales under progressively more demanding fixed-effect spec-

ifications. Column (1) presents a parsimonious specification without fixed effects. Column (2) introduces firm, state, and year-quarter fixed effects to account for time-invariant differences across firms and local markets, as well as common shocks affecting all firms and states in a given quarter. Column (3) further absorbs Firm $\times$ Year-Quarter and State $\times$ Year-Quarter fixed effects, thereby controlling for time-varying firm fundamentals and local economic conditions. These fixed effects help rule out the possibility that changes in firm performance simultaneously affect employee reviews and product sales, or that state-level shocks jointly drive review activity and consumer demand.

Across all specifications, the coefficient on *Cons* $\times$ *Peer EA* remains negative and statistically significant. In the most strict specification reported in Column (3), the estimated coefficient implies that increasing *Peer EA* from zero to its 80th percentile value (0.172), evaluated at the sample mean of *Cons* (31.2 words), is associated with a 1.67% decline in product sales.[5] These findings provide support for the hypothesis that negative employee reviews become more influential when peer firms' earnings announcements increase attention to employer-related information.

In contrast, the coefficient on *Pros* $\times$ *Peer EA* is economically small and statistically insignificant. This finding suggests that the documented effect is not driven by employee reviews in general. Rather, consumers appear to respond primarily to negative workplace disclosures, which may be perceived as more diagnostic and credible. When peer firms' earnings announcements increase attention to employer-related information, negative reviews become more salient and are therefore more likely to influence purchasing decisions. The asymmetric response to negative and

[5]Evaluated at the sample mean of *Cons* (31.2 words), increasing *Peer EA* from zero to its 80th percentile value (0.1724) yields a change in log sales of $-0.0031 \times 31.2 \times 0.1724 = -0.0158$, corresponding to a 1.67% decline in sales ($).

positive employee disclosures is consistent with a large literature documenting that stakeholders place greater weight on negative information than on positive information and view negative disclosures as more diagnostic and informative (Baumeister et al., 2001; Chevalier and Mayzlin, 2006; Groening and Kanuri, 2018).

B. Placebo Tests

To further validate the proposed labor-market-information channel, we conduct two placebo tests. The first exploits cross-sectional variation in the focal firm's local employment presence, while the second exploits the time-series structure of *Peer EA*.

The mechanism underlying our study relies on local job seekers becoming more attentive to labor market information following peer firms' earnings announcements and subsequently encountering employee reviews of the focal firm. When a firm has no establishments in a state, however, it is unlikely to be viewed as a potential employer by local workers. Consequently, peer earnings announcements should be less likely to direct local attention toward the firm's workplace information, weakening the transmission of labor market information into the product market. To test this prediction, Column (1) of Table 5 restricts the sample to firm-state pairs with sales but without firm establishments in the state.

As a second placebo test, we construct a pseudo *Peer EA* variable by randomly shuffling *Peer EA* across the entire regression sample, thereby breaking both the original time-series ordering of *Peer EA* within each firm-state and its original cross-sectional assignment across firm-state units. If our baseline results are driven by the genuine timing and firm-state-specific incidence of peer earnings announcements rather than by some other confound correlated with *Peer EA*, the effect of *Cons* $\times \times$ *Peer EA* should disappear once *Peer EA* is randomized in this way. Column (2) of

Table 5 reports the results using this pseudo *Peer EA* variable.

Consistent with both predictions, the coefficient on *Cons* $\times$ *Peer EA* becomes economically small and statistically insignificant in both columns. The results in Column (1) suggest that the baseline effect is concentrated in locations where the focal firm participates in the local labor market and can therefore attract the attention of local job seekers. The results in Column (2) indicate that the baseline effect depends on the genuine time-series and firm-state-specific incidence of peer earnings announcements, rather than on some fixed but unobserved firm-state characteristic correlated with the overall level of *Peer EA*. Overall, the evidence from both placebo tests is consistent with our interpretation that peer earnings announcements drive the spillover of employee review information into the product market. The effect depends on both local employment presence and the salience of peer earnings announcements. These patterns mitigate concerns about both cross-sectional and time-series confounds.

C. Robustness Tests

Appendix Table A.2 reports a battery of robustness checks addressing alternative explanations and specification choices.

Panel A augments the baseline specification with additional control variables. A potential concern is that our baseline measure of negative employee reviews simply proxies for broader corporate misconduct, rather than capturing workplace-specific information disclosed by employees. To address this concern, Column (1) controls for ESG incidents obtained from the RepRisk database, measured over the same window as *Cons* (lag 1–51 weeks). We first include the level of ESG incidents to control for its direct effect on product sales, and further include its interaction with *Peer EA* to rule out the possibility that the baseline interaction effect is driven by heightened attention

to ESG controversies rather than to employee-generated workplace information. Column (2) controls for early employee reviews, measured more than one year prior to the *Cons* window (*Cons Early*, lag 52+ weeks), together with its interaction with *Peer EA*, thereby distinguishing the effect of the most recent and relevant workplace information from that of outdated, long-standing firm reputation. Column (3) controls for the cumulative abnormal return associated with peer earnings announcements, *CAR (Peer EA)*, constructed over the $[-1, 1]$ event window around the peer firm's earnings announcement date, addressing the concern that our results simply capture the information content of peer firms' earnings news rather than its effect on local job-search activity. Column (4) further splits the *Cons* $\times$ *Peer EA* interaction by whether *CAR (Peer EA)* is above (High) or below (Low) its sample median. We find that the interaction effect remains negative and statistically significant in both subsamples, suggesting that our results are not concentrated among peer announcements with positive or negative earnings surprises but instead reflect a broad-based job-search response to earnings announcements.

Panel B re-estimates the baseline specification on alternative samples to address concerns regarding sample composition. Column (1) excludes firm-state-weeks in which a peer earnings announcement (*Peer EA* > 0) overlaps with the focal firm's own earnings announcement, ensuring that the documented effect is not driven by information released by the focal firm itself. Column (2) excludes firm-state-weeks corresponding to a single headquarters establishment, as such firms represent a unique organizational structure with an unusually concentrated labor-market presence. Column (3) excludes California (*store_state* = CA), given its disproportionate weight in the Nielsen sample, to verify that the baseline results are not driven by this single state.

Panel C re-estimates the baseline specification under alternative model specifications. Column (1) applies the inverse hyperbolic sine (IHS) transformation to sales, $\text{asinh}(\textit{Sales})$, which

accommodates zero sales values while approximating a log transformation. Column (2) uses the square root of sales as the dependent variable as an additional transformation robust to zeros. Column (3) estimates a Poisson pseudo-maximum-likelihood (PPML) regression with sales as the dependent variable, which is well suited for non-negative outcome variables.

Across all panels and specifications, the coefficient on *Cons* $\times$ *Peer EA* remains negative and statistically significant, indicating that our baseline findings are robust to alternative controls, sample restrictions, and empirical specifications.

V. CROSS-SECTIONAL HETEROGENEITY

A. Cross-sectional Tests on Job Seeker Attention

Our main finding is based on the interpretation that peer firms' earnings announcements increase attention to employee reviews by stimulating job-search activities. Under this mechanism, the effect should be stronger in locations with a larger pool of potential job seekers, as a larger pool of job seekers implies a greater number of individuals whose attention to employer-related information is activated by peer earnings announcements. To capture variation in the size of the potential job-seeker pool, we employ two complementary measures.

The first measure is based on peer-firm employee review activity on Glassdoor. Because Glassdoor encourages users to contribute reviews in exchange for access to employer information and reviews posted by others, review-writing activity provides a direct measure of engagement with the platform among local workers. A larger volume of peer-firm reviews indicates a larger base of users who actively interact with the platform in a given location, and is therefore likely to be associated with a larger pool of potential job seekers who may also encounter the focal firm's employee reviews. We construct this measure as the number of reviews posted for peer firms in the

focal firm's state during week $t-1$, the same week used to measure *Peer EA*, so that the measure captures the contemporaneous platform activity. We define *High* as an indicator equal to one if this measure is above the corresponding state $\times$ year-quarter median, thereby comparing job-seeker activity within the same state and quarter.

The second measure uses local job-posting intensity from the Revelio database, which captures the availability of alternative employment opportunities in the local labor market. Job-posting activity reflects current labor demand and the extent to which workers in a given location have realistic alternative employment options to consider. Locations with greater job-posting intensity should accordingly feature a larger and more active pool of job seekers, amplifying the extent to which peer earnings announcements direct local attention toward employer-related information, including the focal firm's employee reviews. Because individual job postings typically remain active for approximately one month, we construct this measure as the number of job postings in the focal firm's state during weeks $t-1$ to $t-3$, capturing the stock of job postings still relevant to job seekers during the period in which *Peer EA* is measured. We similarly define *High* as an indicator equal to one if this measure exceeds the corresponding state $\times$ year-quarter median.

Table 6 reports the results. Across both specifications, the coefficient on *Cons* $\times$ *Peer EA* $\times$ *High* is negative and statistically significant. These results indicate that the adverse effect of negative employee reviews following peer firms' earnings announcements is amplified when the local labor market contains a larger pool of potential job seekers, relative to other weeks within the same state and quarter. The consistency of these findings across measures constructed from different data sources mitigates the concern that our results are driven by a single, idiosyncratic measure of job-seeker attention. Taken together, these findings provide supporting evidence that peer firms' earnings announcements increase the salience of employee-generated information when there is

a larger pool of workers who may be actively seeking employment, consistent with a job-search mechanism.

B. Heterogeneity by Local Demographic Characteristics or Product Prices

In this subsection, we examine additional sources of heterogeneity in local demographic, economic, and political characteristics, as well as in product price tiers.

We first examine heterogeneity across local demographic and economic characteristics. Table 7 splits the firm-state-week sample into High and Low subsamples based on the within-year median across states for each characteristic. Column (1) splits on active working adult population, defined as the combined population aged 20–44 in the state, obtained from the Census Bureau's American Community Survey 5-year DP05 demographic estimates. Column (2) splits on the share of the state population aged 25 and older with a bachelor's degree or higher, obtained from the corresponding DP02 social characteristics estimates. Column (3) splits on the Democratic two-party vote share in the most recent presidential election prior to each sample year, obtained from the MIT Election Data and Science Lab (MIT Election Data and Science Lab, 2017). Column (4) splits on mean household income and Column (5) splits on the unemployment rate, both obtained from the DP03 economic characteristics estimates. Because the relevant ACS 5-year estimates are first available starting in 2010, the sample underlying these specifications is correspondingly smaller than in our baseline tests. Column (6) splits on state-level personal consumption expenditures, obtained from the Bureau of Economic Analysis.

The coefficient on *Cons* $\times$ *Peer EA* is negative and statistically significant in the High subsample across all six characteristics, while it is consistently smaller in the corresponding Low subsample. The High–Low difference is statistically significant for the active working adult population

(Column 1) and consumer spending (Column 6), indicating that the baseline effect is stronger where a larger share of the population is likely to use Glassdoor and where consumers have greater purchasing power. These findings are consistent with employee-generated information exerting a larger impact when more individuals are likely to encounter employee reviews during job search and subsequently translate that information into purchasing decisions. Although the High–Low differences are not statistically significant for other columns, the estimated coefficients remain larger in the High subsamples. This pattern is also consistent with our proposed mechanism, as individuals with higher educational attainment and those in more Democratic-leaning states may be more attentive to labor-market information, while higher unemployment may increase job-search intensity, although these differences are not estimated precisely enough to distinguish the High and Low groups statistically.

We next examine whether the baseline effect varies across product price tiers. For each firm-state-week observation, we use Nielsen's UPC-level transaction data to split total sales into two components based on the unit price (price divided by the multi-unit price multiplier) of the underlying transactions: sales of products priced below the sample-wide median unit price, and sales of products priced at or above the median. Table 8 reports the results separately for each price tier. The coefficient on *Cons* $\times$ *Peer EA* is negative and statistically significant for sales of below-median-priced products (Column 1), but economically smaller and statistically insignificant for above-median-priced products (Column 2). This pattern suggests that the baseline effect is concentrated among lower-priced products, consistent with the interpretation that purchases of relatively inexpensive, frequently replenished items are more sensitive to shifts in consumer attention than purchases of higher-priced products, which may instead be driven primarily by product-specific characteristics.

VI. CONCLUSION

This study examines whether employee-generated information spills over from the labor market to the product market and influences consumer purchasing decisions. Using peer firms' earnings announcements as shocks to local job-search activity, we find that employee reviews have a stronger effect on product sales during periods of heightened peer announcement exposure, an effect concentrated in negative reviews, with no comparable pattern for positive reviews.

Our study makes three contributions. First, we identify consumers as a novel stakeholder group affected by employee disclosures. We show that employee-generated information, produced by workers for job seekers, not by firms for consumers, can shape consumer purchasing decisions. Second, we identify job-search salience as a mechanism through which labor market information crosses into the product market: periods of heightened job-search activity convert employer-review platforms into a channel for product market spillovers. Third, more broadly, our findings suggest that online employment platforms are not merely labor-market intermediaries; their reach extends into product markets in ways that managers, consumers, and researchers are only beginning to understand. As employee-generated information becomes increasingly accessible, understanding its broader economic consequences remains an important avenue for future research.

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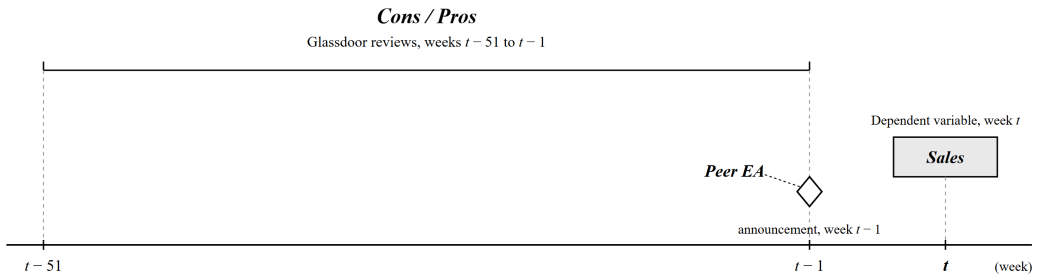
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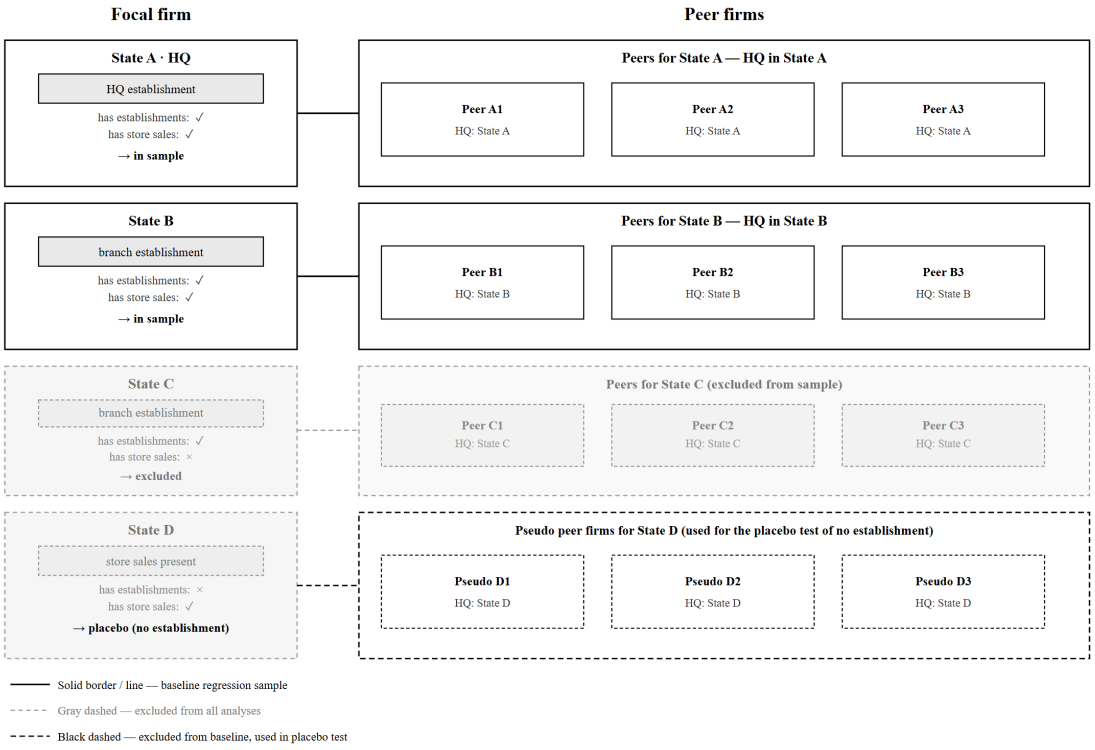
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(a) Timeline of Measurement of Cons/Pros, Peer EA, and Sales



(b) Construction of Peer Firms

Figure 1. Timeline of Variable Measurement and Construction of Peer Firms

This figure illustrates the measurement timing and construction of the key variables used in the analysis. Panel A presents the timeline of variable measurement relative to the observation week t. *Cons* (*Pros*) measures the average number of words in employee review content identified as negative (positive), aggregated over the prior 51 weeks (weeks $t-51$ to $t-1$). For each review, a large language model is used to identify genuine negative and positive content from the review's "Cons" and "Pros" sections, and the variable is constructed as the average word count of the identified content per review among those posted during this window. *Peer EA* is the scaled number of peer earnings announcements during the prior week (week $t-1$), where peers are firms whose headquarters are located in the same state as a focal firm establishment; the measure is normalized by the number of peer firms in the state. *Sales* is the focal firm's state-level sales, measured in week t, and serves as the dependent variable. Panel B illustrates how peer firms are constructed for each focal firm-state observation. A focal firm enters the baseline regression sample only in states where it has both an establishment (including its headquarters) and retail sales (States A and B). For each such establishment state, peer firms are defined as firms whose headquarters are located in the same state, and *Peer EA* is calculated as the number of peer earnings announcements scaled by the total number of peer firms in that state. State C represents a state in which the focal firm has an establishment but no retail sales and is therefore excluded from the sample. State D represents a state in which the focal firm has retail sales but no establishment. Such observations are excluded from the baseline sample but are used for the placebo test.

Table 1. Sampling Procedure

Step	Description	# Firms	# Firm-Years	# Firm-State-Years	# Firm-State-Weeks
1	Nielsen UPCs matched to public firms via GS1 and Compustat	483	—	—	—
2	With valid physical store location in Nielsen	354	3,375	107,365	5,126,722
3	With establishments or headquarters in store state	274	2,047	25,464	1,214,622
4	With Glassdoor employee reviews	213	1,510	18,411	843,989
5	With non-missing independent variables and after singleton drop	198	1,413	18,069	823,566

This table presents the sample construction procedure. We start with Nielsen UPCs matched to public firms via the GS1 and Compustat databases. We then restrict the sample to firms with valid physical store locations in Nielsen and require firms to have either an establishment presence or headquarters located in the store state. Next, we retain only firms with Glassdoor employee reviews. The final regression sample requires non-missing independent variables and excludes singleton observations. The unit of observation is the firm-state-week.

Table 2. Sample Distribution

Panel A: Sample Distribution by Year

Year	Compustat Universe	Pre-Glassdoor	Final Sample
2006	8,535	89	—
2007	8,311	90	—
2008	7,900	101	46
2009	7,726	111	62
2010	7,751	105	68
2011	7,722	101	73
2012	8,123	98	71
2013	8,112	106	76
2014	7,887	104	82
2015	7,518	103	87
2016	7,310	107	86
2017	7,229	107	81
2018	7,089	121	96
2019	7,037	120	98
2020	7,194	134	116
2021	7,386	157	135
2022	7,215	155	122
2023	6,983	138	114

Panel B: Industry Distribution (FF-12)

FF-12 Industry	Compustat Universe (%)	Pre-Glassdoor (%)	Final Sample (%)
Consumer NonDurables	3.6	29.2	32.8
Consumer Durables	2.0	2.9	3.0
Manufacturing	6.4	12.4	14.1
Chemicals	1.9	6.2	6.6
Business Equipment	17.0	8.0	7.6
Telecom	2.3	1.5	1.5
Wholesale & Retail	6.8	20.1	18.7
Healthcare	16.7	14.6	12.1
Other	17.0	4.0	3.5
Oil, Gas & Coal	4.8	0.7	0.0
Utilities	2.3	0.0	0.0
Finance	19.2	0.4	0.0

Panel C: Average Firm Characteristics

Variable	Compustat Universe	Pre-Glassdoor	Final Sample
Assets	8,601.817	16,893.203	15,512.194
Leverage	0.160	0.232	0.248
Employees	8.894	27.615	24.750
Market Value	4,646.448	18,472.296	17,508.496
MTB	4.106	5.468	5.628

This table compares our sample with the Compustat universe. The Compustat universe includes all firms meeting standard data filters (industrial format, standardized, consolidated) with non-missing total assets and book equity. The Pre-Glassdoor sample includes Nielsen firms with establishment or headquarters presence in a given state. The final sample further requires non-missing Glassdoor employee review data, non-missing values for the independent variables, and excludes singleton observations after fixed-effects absorption. Panel A reports the number of unique firms by calendar year. Panel B reports the industry distribution based on the Fama–French 12-industry classification. Panel C compares average firm characteristics, including total assets (*Assets*, in millions of dollars), number of employees (*Employees*, in thousands), financial leverage (*Leverage*), market value of equity (*Market Value*, in millions of dollars), and the market-to-book ratio (*MTB*). All firm characteristics are winsorized annually at the 1st and 99th percentiles.

Table 3. Summary Statistics

Panel A: Overall Summary Statistics

Variable	*N*	Mean	Std	P25	Median	P75	Within Std
Sales ($)	823,566	63,503.044	153,801.475	107.433	4,032.960	43,231.355	90,180.042
Sales (log)	823,566	7.425	4.057	4.686	8.303	10.674	1.178
Cons	823,566	31.160	17.171	19.047	28.262	37.976	3.375
Pros	823,566	19.554	11.217	11.837	17.500	23.189	1.931
Peer EA	823,566	0.070	0.093	0.000	0.028	0.100	0.092

Panel B: Mean of Key Variables by Year

Year	*N*	*Sales($)*	*Sales (log)*	*Cons*	*Pros*	*Peer EA*
2008	14,393	63,796.607	8.277	61.369	47.480	0.067
2009	31,996	53,134.419	7.387	59.874	43.160	0.070
2010	31,512	53,908.476	7.101	46.768	28.998	0.070
2011	40,753	46,125.297	6.776	39.977	22.626	0.069
2012	44,379	45,562.735	6.548	39.560	22.677	0.069
2013	46,542	49,058.612	6.992	37.191	21.947	0.068
2014	52,720	43,337.539	6.654	34.985	20.320	0.070
2015	52,395	49,721.712	6.926	34.948	23.683	0.071
2016	61,439	51,650.749	7.509	32.375	19.975	0.069
2017	60,169	55,072.701	7.847	29.913	19.513	0.069
2018	60,086	74,688.812	7.867	30.071	19.068	0.070
2019	61,290	74,466.750	8.000	28.482	16.813	0.068
2020	62,111	81,604.654	7.840	24.443	15.352	0.068
2021	68,508	95,845.523	7.835	16.532	11.222	0.071
2022	68,514	59,839.367	7.255	19.917	11.790	0.071
2023	66,759	84,385.072	7.678	18.603	11.333	0.074

Panel C: Mean of Key Variables by FF-12 Industry

Industry	*N*	*Sales($)*	*Sales (log)*	*Cons*	*Pros*	*Peer EA*
Consumer Non-Durables	306,086	72,397.337	8.743	30.568	19.861	0.070
Wholesale & Retail	161,159	50,417.051	6.958	33.531	18.877	0.069
Chemicals	117,106	119,673.787	6.340	30.631	19.146	0.070
Manufacturing	74,593	4,008.859	5.182	30.037	18.403	0.070
Business Equipment	54,752	68,949.856	8.809	29.861	21.085	0.070
Healthcare	49,541	43,237.562	5.421	28.782	19.683	0.070
Consumer Durables	46,614	36,562.105	8.877	32.647	20.215	0.070
Other	12,663	6,228.405	1.716	35.865	20.419	0.071
Telecom	1,052	69.510	2.215	36.202	35.904	0.070

Panel D: Mean of Key Variables by State

State	*N*	*Sales($)*	*Sales (log)*	*Cons*	*Pros*	*Peer EA*
CA	40,158	180,826.714	8.663	31.243	20.355	0.066
NY	31,979	115,250.842	7.869	31.272	19.874	0.069
IL	29,486	94,790.771	7.673	31.630	19.642	0.070
TX	28,541	134,944.386	8.641	30.224	19.350	0.071
PA	28,200	84,800.168	8.044	32.667	20.044	0.070
OH	26,943	99,025.401	8.076	32.593	20.090	0.074

NJ	26,498	64,543.677	7.681	31.340	19.237	0.071
FL	26,491	96,871.504	7.813	31.529	19.962	0.070
GA	24,957	58,945.299	7.396	31.838	19.336	0.071
MA	23,278	62,896.984	7.737	31.249	20.503	0.064
NC	22,488	68,533.201	7.317	31.805	19.007	0.070
WA	22,476	80,198.892	8.200	31.711	20.251	0.070
MN	22,364	56,363.166	7.645	32.853	20.127	0.073
VA	21,623	69,701.312	7.406	30.354	19.018	0.072
TN	21,318	59,115.796	7.615	32.174	19.717	0.072
CO	21,198	84,261.159	8.018	31.597	20.159	0.071
MO	20,445	30,468.384	7.035	31.351	19.148	0.072
AR	20,373	13,442.869	7.011	31.588	19.608	0.075
MD	20,168	55,456.903	7.367	31.742	20.592	0.069
WI	19,275	43,038.145	7.037	30.929	19.160	0.074
IN	19,010	44,751.340	7.083	31.169	19.486	0.073
AZ	17,905	89,467.022	7.890	30.961	19.923	0.072
MI	17,697	90,351.194	8.276	31.546	19.851	0.072
KS	16,213	23,014.811	6.877	31.120	19.608	0.072
OR	15,224	53,395.305	7.884	32.298	21.143	0.072
IA	15,111	32,010.590	6.900	31.968	20.364	0.074
KY	14,916	45,098.445	7.467	29.312	18.609	0.070
CT	14,774	32,216.901	6.848	30.399	19.036	0.071
LA	14,193	30,975.725	6.931	31.503	19.216	0.069
UT	14,076	21,258.863	6.722	29.631	19.133	0.068
SC	14,069	22,054.406	6.277	29.399	17.553	0.071
AL	13,634	15,440.643	6.429	29.372	17.462	0.071
ID	12,540	10,330.020	6.214	31.124	20.090	0.070
NV	11,460	39,884.674	7.237	29.866	18.344	0.067
NE	11,152	18,553.887	6.497	30.791	19.337	0.076
OK	10,299	18,913.379	5.993	29.638	19.056	0.070
NM	9,745	17,117.880	6.621	30.302	19.055	0.065
MS	9,616	11,407.358	6.048	28.954	18.261	0.065
DE	9,513	15,966.822	6.199	29.659	18.486	0.067
NH	9,124	24,184.005	6.879	29.016	19.118	0.063
RI	8,225	7,944.051	6.006	30.237	18.403	0.074
ME	7,782	23,261.529	6.235	31.020	19.271	0.063
DC	7,316	10,802.863	6.814	32.026	21.816	0.064
WV	6,449	13,433.989	6.652	30.266	18.345	0.062
MT	5,908	13,725.009	7.228	31.132	19.155	0.063
ND	5,747	7,909.761	6.986	29.479	18.719	0.061
WY	4,711	11,093.463	6.946	31.891	19.667	0.027
VT	4,565	6,480.921	5.565	26.723	17.269	0.061
SD	4,024	10,941.032	6.848	31.286	17.568	0.069
HI	206	124.096	4.681	26.243	32.545	0.062
AK	103	341.094	5.804	27.844	15.539	0.039

This table reports summary statistics for the final regression sample. Panel A presents the overall distribution of the key variables, reporting the number of observations, mean, standard deviation, the within standard deviation after absorbing firm-by-year-quarter and state-by-year-quarter fixed effects, and the 25th, 50th, and 75th percentiles for each variable. Panel B reports variable means by year. Panel C reports variable means by FF-12 industry. Panel D reports variable means by store state, ordered by the number of observations. All continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are provided in Appendix Table A.1.

Table 4. Baseline Regression: Attention to Employee Reviews and Sales

Dep Var	*Sales (log)*			
	(1)	(2)	(3)	(4)
Cons × *Peer EA*	-0.0066*	-0.0036**	-0.0031**	
	(0.0034)	(0.0018)	(0.0013)	
Pros × *Peer EA*				-0.0018
				(0.0013)
Cons	-0.0132	-0.0170**	-0.0026***	
	(0.0090)	(0.0075)	(0.0009)	
Pros				-0.0017
				(0.0011)
Peer EA	0.2227*	0.1578**	0.1243***	0.0648*
	(0.1181)	(0.0622)	(0.0445)	(0.0355)
Firm FE	No	Yes	No	No
State FE	No	Yes	No	No
Firm × Year-Quarter FE	No	No	Yes	Yes
State × Year-Quarter FE	No	No	Yes	Yes
N	823,566	823,566	823,566	823,566
Adj. R^2	0.003	0.770	0.915	0.915

This table reports OLS estimates of how employee reviews affect product sales following peer firms' earnings announcements. The regressions are estimated at the firm-week-state level. The dependent variable is the logarithm of one plus weekly state-level sales, $\log(1 + \textit{Sales(\$)})$. *Cons* (*Pros*) measures the average number of words in employee review content identified as negative (positive) over the prior 51 weeks. For each review, a large language model is used to identify genuine negative and positive content from the review's "Cons" and "Pros" sections, and the variable is constructed as the average word count of the identified negative and positive content across reviews. *Peer EA* is the scaled number of peer earnings announcements during the prior week, where peers are firms whose headquarters are located in the same state as the focal firm's establishment. The measure is normalized by the number of peer firms in the state. The interaction terms *Cons* × *Peer EA* and *Pros* × *Peer EA* capture whether the effect of employee reviews on sales is amplified following peer earnings announcements. Columns (1)–(3) use negative reviews (*Cons*), while Column (4) uses positive reviews (*Pros*). All continuous variables are winsorized at the 1st and 99th percentiles. The standard errors reported in parentheses are clustered by firm. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5. Placebo Tests

Dep Var	*Sales (log)*	
	(1) Pseudo Focal (No Focal-Firm Establishment)	(2) Pseudo *Peer EA*
Cons × *Peer EA*	-0.0010	-0.0002
	(0.0011)	(0.0007)
Cons	-0.0013	-0.0028***
	(0.0011)	(0.0009)
Peer EA	0.0044	-0.0287
	(0.0440)	(0.0254)
Firm × YQ FE	Yes	Yes
State × YQ FE	Yes	Yes
N	1,240,369	823,566
Adj. R^2	0.862	0.915

This table presents placebo tests for the baseline results. Column (1) restricts the sample to firm-state pairs with sales but without firm establishments in the state. Column (2) uses a pseudo *Peer EA* variable constructed by randomly shuffling *Peer EA* across the sample. All continuous variables are winsorized at the 1st and 99th percentiles. All specifications include Firm×Year-Quarter and State×Year-Quarter fixed effects. The standard errors reported in parentheses are clustered by firm. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6. Cross-Sectional Heterogeneity by Potential Job-Seeker Pool

Dep Var	*Sales (log)*	
	(1) Peer Reviews	(2) Job Postings
Cons × *Peer EA* × *High*	-0.0162**	-0.0063**
	(0.0069)	(0.0026)
Cons × *Peer EA*	-0.0024*	-0.0026*
	(0.0014)	(0.0013)
Cons	-0.0026***	-0.0029***
	(0.0009)	(0.0009)
Cons × *High*	0.0016	0.0018***
	(0.0030)	(0.0006)
Peer EA	0.0767	0.1427***
	(0.0582)	(0.0520)
Peer EA × *High*	0.5939**	0.0540
	(0.2328)	(0.0746)
High	0.0332	-0.0403**
	(0.1268)	(0.0159)
Firm × Year-Quarter FE	Yes	Yes
State × Year-Quarter FE	Yes	Yes
N	823,529	823,529
Adj. R^2	0.915	0.915

This table reports cross-sectional tests examining whether the effect of employee reviews on product sales following peer firms' earnings announcements varies with the size of the potential job-seeker pool, proxied separately by peer review activity and local job posting activity. The regressions are estimated at the firm-week-state level. The dependent variable is the logarithm of one plus weekly state-level sales, $\log(1 + \textit{Sales(\$)})$. *Cons* measures the average number of words in employee review content identified as negative over the prior 51 weeks. *Peer EA* is the scaled number of peer earnings announcements during the prior week. In Column (1), *High* is an indicator equal to one if the number of peer firm Glassdoor reviews in the state during the peer EA week is above the state-quarter median. In Column (2), *High* is an indicator equal to one if the number of job postings in the state is above the state-quarter median. All continuous variables are winsorized at the 1st and 99th percentiles. All specifications include Firm×Year-Quarter and State×Year-Quarter fixed effects. The standard errors reported in parentheses are clustered by firm. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7. Heterogeneity by Local Demographic Characteristics

Dep Var	*Sales (log)*					
	(1) Active Working Adults	(2) BA or Higher Share	(3) Democratic Vote Share	(4) HH Income	(5) Unemployment	(6) Consumer Spending
Cons × *Peer EA* × *High*	-0.0049***	-0.0051**	-0.0037**	-0.0056**	-0.0037***	-0.0044**
	(0.0017)	(0.0024)	(0.0018)	(0.0022)	(0.0013)	(0.0017)
Cons × *Peer EA* × *Low*	-0.0013	-0.0028**	-0.0024*	-0.0029**	-0.0021	-0.0015
	(0.0015)	(0.0013)	(0.0013)	(0.0012)	(0.0019)	(0.0012)
Cons	-0.0023**	-0.0023**	-0.0026***	-0.0023**	-0.0023**	-0.0026***
	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0009)
Peer EA	0.1313***	0.1461***	0.1225***	0.1554***	0.1188***	0.1256***
	(0.0423)	(0.0474)	(0.0434)	(0.0490)	(0.0456)	(0.0450)
Difference (**High* − **Low*)	-0.0036	-0.0023	-0.0013	-0.0027	-0.0016	-0.0029
p-value	0.0750*	0.3407	0.5259	0.1276	0.2922	0.0923*
Firm × Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
N	777,177	777,177	823,566	777,177	777,177	823,566
Adj. R^2	0.915	0.915	0.915	0.915	0.915	0.915

This table examines whether the effect of Cons × Peer EA on store-state sales varies with local demographic and economic characteristics. For each characteristic, the firm-state-week sample is split into High and Low subsamples based on the within-year median across states. States above the yearly median are assigned to the High group, while the remaining states form the Low group. Column (1) splits on the size of active working adult population, defined as the combined population aged 20–44. Column (2) splits on the share of the state population aged 25 and older with a bachelor's degree or higher. Column (3) splits on the Democratic two-party vote share in the most recent presidential election prior to each sample year. Column (4) splits on mean household income. Column (5) splits on the unemployment rate. Column (6) splits on personal consumption expenditures. All demographic and economic variables are measured at the state-year level. The dependent variable is the logarithm of one plus weekly state-level sales, $\log(1 + \textit{Sales(\$)})$. *Cons* measures the average number of words in employee review content identified as negative over the prior 51 weeks. *Peer EA* is the scaled number of peer earnings announcements during the prior week. All continuous variables are winsorized at the 1st and 99th percentiles. All specifications include Firm×Year-Quarter and State×Year-Quarter fixed effects. The standard errors reported in parentheses are clustered by firm. The row labeled "Difference (**High* − **Low*)" reports the difference between the High and Low coefficients on Cons × Peer EA, and the *p*-value below it is from a two-sided Wald test of the null hypothesis that the High and Low coefficients are equal. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 8. Heterogeneity by Product Price

Dep Var	*Sales (log)*	
	(1) Below Median Price	(2) Above Median Price
Cons × *Peer EA*	-0.0049***	-0.0020
	(0.0019)	(0.0013)
Cons	-0.0012	-0.0023**
	(0.0009)	(0.0009)
Peer EA	0.1573***	0.0919**
	(0.0563)	(0.0453)
Firm × Year-Quarter FE	Yes	Yes
State × Year-Quarter FE	Yes	Yes
N	823,566	823,566
Adj. R^2	0.925	0.913

This table presents the heterogeneity by product price tier. For each firm-week-state observation, total sales are split into two components based on the unit price (price divided by the multi-unit price multiplier, `price/prmult`) of the underlying UPC-level transactions: sales of products priced below the sample-wide median unit price (*Below Median Price*, Column 1) and sales of products priced at or above the median (*Above Median Price*, Column 2). The dependent variable is the logarithm of one plus weekly state-level sales, $\log(1 + \textit{Sales(\$)})$. *Cons* measures the average number of words in employee review content identified as negative over the prior 51 weeks. *Peer EA* is the scaled number of peer earnings announcements during the prior week. All continuous variables are winsorized at the 1st and 99th percentiles. All specifications include Firm×Year-Quarter and State×Year-Quarter fixed effects. The standard errors reported in parentheses are clustered by firm. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A.1. Variable Definitions

Variable	Definition	Data Source
Above Median Price	The natural logarithm of one plus weekly state-level sales of products priced at or above the sample-wide median unit price, where unit price is calculated as `price/prmult`.	Nielsen
Active Working Adults	The combined population aged 20–44 in the state.	Census
Assets	Total book value of assets, measured in millions of dollars.	Compustat
BA or Higher Share	The share of the state population aged 25 and older with a bachelor's degree or higher.	Census
Below Median Price	The natural logarithm of one plus weekly state-level sales of products priced below the sample-wide median unit price, where unit price is calculated as `price/prmult`.	Nielsen
CAR (Peer EA)	The cumulative abnormal stock return around peer firms' earnings announcements, aggregated at the state-week level.	CRSP/Compustat
Cons	The average number of words identified as negative content in employee reviews posted during the previous 51 weeks. Negative content is extracted from the "Cons" section of employee reviews using a large language model.	Revelio Labs
Cons Early	The average negative employee review content from reviews posted more than one year earlier, i.e., reviews dated at least 52 weeks before the focal week.	Revelio Labs
Consumer Spending	State-level personal consumption expenditures.	BEA
Democratic Vote Share	The Democratic two-party vote share in the most recent presidential election prior to each sample year.	MIT Election Data and Science Lab
Employees	Total number of employees, measured in thousands.	Compustat
ESG Incident	A measure of firm-level ESG controversy exposure obtained from RepRisk and aggregated over the previous 51 weeks.	RepRisk
HH Income	Mean household income at the state level.	Census
Job Posting	The number of job postings in the focal firm's state during weeks $t-1$ to $t-3$, capturing the stock of job postings still active given that individual postings typically remain open for approximately one month.	Revelio Labs
Leverage	Financial leverage, calculated as total debt divided by total assets.	Compustat
Market Value	Market value of equity, calculated as share price multiplied by shares outstanding, measured in millions of dollars.	Compustat
MTB	Market-to-book ratio, calculated as market value of equity divided by book value of equity.	Compustat

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Table A.1 Continued from previous page

Variable	Definition	Data Source
Own EA	An indicator equal to one if the focal firm announces earnings during the same week as the peer firms' earnings announcements, and zero otherwise.	Compustat
Peer EA	The scaled number of earnings announcements made by peer firms during the previous week. Peer firms are defined as firms whose headquarters are located in the same state as the focal firm's establishment. The measure is normalized by the number of peer firms in the state.	Compustat
Peer Review Activity	The number of reviews posted for peer firms in the focal firm's state during week $t-1$, the same week used to measure *Peer EA*.	Revelio Labs
Pros	The average number of words identified as positive content in employee reviews posted during the previous 51 weeks. Positive content is extracted from the "Pros" section of employee reviews using a large language model.	Revelio Labs
Reviews	It represents *Cons* or *Pros* in the analysis.	Revelio Labs
Sales (log)	The natural logarithm of one plus weekly state-level retail sales of a focal firm, *Sales ($)*.	Nielsen
Unemployment	The state-level unemployment rate.	Census

Table A.2. Robustness Tests

Panel A: Additional Controls

Dep Var = *Sales (log)*	(1) *ESG Incident*	(2) *Cons Early*	(3) *CAR (Peer EA)*	(4) *High/Low CAR (Peer EA)*
Cons × *Peer EA*	−0.0027**	−0.0043**	−0.0034***	
	(0.0013)	(0.0021)	(0.0013)	
Cons × *Peer EA* × *High CAR*				−0.0039***
				(0.0014)
Cons × *Peer EA* × *Low CAR*				−0.0030**
				(0.0012)
Cons	−0.0026***	−0.0027**	−0.0027***	−0.0027***
	(0.0009)	(0.0012)	(0.0009)	(0.0009)
Peer EA	0.0758	0.1538***	0.1286***	0.1282***
	(0.0523)	(0.0508)	(0.0458)	(0.0457)
ESG Incident	−0.0023			
	(0.0036)			
ESG Incident × *Peer EA*	0.0050*			
	(0.0026)			
Cons Early		0.0007		
		(0.0021)		
Cons Early × *Peer EA*		0.0001		
		(0.0021)		
CAR (Peer EA)			−0.0220	
			(0.0163)	
Firm × YQ FE	Yes	Yes	Yes	Yes
State × YQ FE	Yes	Yes	Yes	Yes
N	823,566	779,910	782,682	782,682
Adj. R^2	0.9149	0.9146	0.9149	0.9149

Panel B: Alternative Samples

Dep Var = *Sales (log)*	(1) Exclude Overlap of Peer EA & Own EA	(2) Exclude Single HQ-Establishment	(3) Exclude CA
Cons × *Peer EA*	−0.0037***	−0.0031**	−0.0031**
	(0.0013)	(0.0013)	(0.0013)
Cons	−0.0026***	−0.0027***	−0.0025***
	(0.0009)	(0.0009)	(0.0009)
Peer EA	0.1558***	0.1241***	0.1292***
	(0.0470)	(0.0446)	(0.0458)
Firm × YQ FE	Yes	Yes	Yes
State × YQ FE	Yes	Yes	Yes
N	774,204	820,719	783,404
Adj. R^2	0.9148	0.9149	0.9144

Panel C: Alternative Specifications

Dep Var = *Sales**	(1) IHS	(2) Square Root	(3) Poisson
Cons × *Peer EA*	−0.0033**	−0.2309***	−0.0034***
	(0.0014)	(0.0732)	(0.0008)
Cons	−0.0029***	−0.0611*	−0.0010*

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(Table A.2 continued)

	(0.0010)	(0.0320)	(0.0006)
Peer EA	0.1289***	9.4854**	0.1308***
	(0.0484)	(3.7246)	(0.0403)
Firm × YQ FE	Yes	Yes	Yes
State × YQ FE	Yes	Yes	Yes
N	823,566	823,566	823,566
Adj. R^2	0.9075	0.8371	—

This table presents a battery of robustness checks. Panel A adds additional control variables. Column (1) controls for ESG incidents measured over the same window as *Cons* (lag 1–51 weeks) and its interaction with *Peer EA*. Column (2) controls for early employee reviews, measured more than one year prior to the *Cons* window (*Cons Early*, lag 52+ weeks), and its interaction with *Peer EA*. Column (3) controls for the cumulative abnormal return associated with peer earnings announcements, *CAR (Peer EA)*. Column (4) splits the *Cons* × *Peer EA* interaction by whether *CAR (Peer EA)* is above (High) or below (Low) its sample median. Panel B re-estimates the baseline specification on alternative samples. Column (1) excludes the subset of firm-state-weeks in which a peer earnings announcement (*Peer EA* > 0) overlaps with a focal (own) earnings announcement event. Column (2) excludes firm-state-weeks corresponding to a single headquarters establishment. Column (3) excludes California (*store_state* = CA). Panel C re-estimates the baseline specification under alternative specifications. Column (1) applies the inverse hyperbolic sine (IHS) transformation to sales, asinh(*Sales ($)*). Column (2) uses the square root of sales as the dependent variable. Column (3) estimates a Poisson pseudo-maximum-likelihood regression with *Sales ($)* as the dependent variable. All continuous variables are winsorized at the 1st and 99th percentiles. All specifications include Firm × Year-Quarter and State × Year-Quarter fixed effects. The standard errors reported in parentheses are clustered by firm. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Online Appendix for Labor Market Information and Product Market Spillovers: Evidence from Employee Reviews

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July 31, 2026

OA.1. MOTIVATION: WHY RAW WORD COUNTS DO NOT WORK

This appendix describes how we construct *Cons* and *Pros* and reports evidence on their validity.

Glassdoor requires reviewers to complete two separate free-response fields, *Pros* and *Cons*. Because both fields are mandatory, reviewers must enter text even when they have nothing to report, and a non-trivial fraction of fields therefore contains no information of the type the field is designed to elicit. Reading a random sample of reviews shows that such text is not random noise. Beyond boilerplate, it consists predominantly of content that belongs to the opposite field.

The *Cons* field frequently contains praise for the employer:

- "there is not any cons" (5 words)
- "It's difficult to come up with any cons of working for this company, however Stryker seems to be great at keeping teams lean while growing at a rapid pace. We accomplish a lot (and very quickly) with fewer resources. For the ambitions, talented people who work here, the fast-paced environment and full work load really feels like more of a pro than a con most of the time." (70 words)
- "Recruitment is a challenging career, and every day brings its own set of obstacles. However, at Robert Half, it's the exceptional training and supportive management team that help you overcome these hurdles. . . ." (104 words)

The *Pros* field frequently contains criticism of the employer:

- "No pros, dumb environment" (4 words)
- "I didn't see any pros here. This was by far the worst job I have ever worked out. They monitor the team you spend on a ticket and micromanage every little thing you do." (35 words)
- "There are none. I can not think of one thing that republic does well or a positive result for their hard working employees. . . ." (44 words)

A raw word count assigns the 44-word criticism reported above to *Pros* and the 104-word passage of praise to *Cons*. Each firm's *Pros* is therefore inflated by negative text and its *Cons* by

positive text, which moves the two raw measures toward each other and understates the difference between them. Because our main tests turn on the difference in how consumers respond to *Cons* and to *Pros*, raw word counts bias the estimated asymmetry toward zero.

We therefore screen the text before counting words. We classify a review field as **informative** when it reports at least one workplace advantage (in *Pros*) or at least one workplace complaint (in *Cons*), and as **uninformative** otherwise. Informativeness is a question of presence rather than of quantity or intensity: a *Cons* field consisting of the single word "stressful," or of the phrase "no job security," is informative, because a job seeker reading it learns of a specific workplace shortcoming. A field is uninformative only when it reports no such content at all, which occurs in three cases: the field is empty or contains a placeholder; the field denies that any advantage or complaint exists ("no cons," "can't think of any"); or the field reports content belonging to the opposite tone, as in the examples above.

Cons and *Pros* are then constructed in two steps. At the review-field level, the word count of a field enters the measure when the field is informative and is set to zero otherwise. At the firm-week level, these values are averaged over the preceding 51 weeks. The measures are thus intended to capture how much negative (positive) workplace information a job seeker reads on the focal firm's page, and not how satisfied its employees are; Section OA.4 shows that the two differ in the data. Section OA.2 describes the sample, the classification procedure, and the aggregation.

OA.2. SAMPLE AND MEASURE CONSTRUCTION

OA.2.1 Review Sample

Employee reviews are drawn from Revelio Labs, which compiles individual employee reviews reported on employer-review platforms. We link reviews to Compustat firms using Revelio's com-

pany identifier (*rcid*) and its mapping to *gvkey*, and retain firms that appear in Compustat quarterly with industrial-format (INDL) records and non-missing, non-zero total assets. We further restrict the sample to English-language reviews, because the classifiers described below are trained on English text and because job seekers in the U.S. local labor markets we study read reviews in English. The resulting sample comprises 6,690,854 reviews of U.S. publicly listed firms posted between 2008 and 2024.[1] All statistics reported in this appendix are computed on this sample. We measure the length of a review field as the number of alphanumeric tokens it contains, treating punctuation and line breaks as delimiters rather than as tokens.

OA.2.2 LLM Labeling and Human Validation

We draw a random sample of 10,000 reviews and label them for two tasks: Task 1, whether the *Pros* field is informative; and Task 2, whether the *Cons* field is informative. Labeling is performed by GPT-4o with the generation temperature set to zero to ensure deterministic output, in batches of fifteen reviews per query with structured output. Labels for the held-out reviews are finalized after human validation. Section OA.8 reports the complete prompts for both tasks.

OA.2.3 Model Distillation

The full sample comprises 6.69 million reviews, for which review-by-review LLM queries are prohibitively costly. In addition, API responses are not stable across model versions, which limits replicability. We therefore use the labels obtained in the first step as training labels for a smaller classification model, which we then apply to the full sample. This procedure applies a single fixed decision rule to all reviews and allows the resulting classifications to be replicated.

[1] The review sample extends through 2024, whereas the estimation sample in the main text ends in 2023 because Nielsen retail scanner coverage ends in that year. We report review statistics through 2024 to document the full time-series behavior of the measures.

Specifically, we fine-tune one DeBERTa-v3-base binary classifier for each of the two tasks. The labeled sample is randomly partitioned into training, validation, and test sets in approximately 70/15/15 proportions. The three subsets are deduplicated against one another on text content to prevent label leakage, and the decision threshold is selected using the validation set alone, with the test set excluded from model selection and hyperparameter tuning.

The choice of DeBERTa reflects the nature of the classification task. Correct classification depends on sentence structure rather than on word choice. The phrase "no cons" and the phrase "the cons are" contain the same content word, and a *Cons* field that begins with "no cons" may still report a genuine complaint, as in "No cons. Sometimes late night meetings," which is informative. Conversely, the *Pros* field "I didn't see any pros here" contains a positive term but reports no advantage. Classification therefore requires the model to determine what a negation applies to and how a clause relates to the clauses around it, rather than to count positive or negative words. DeBERTa employs a disentangled attention mechanism that represents the content and the position of a token separately, and therefore captures relations of this kind more effectively than the absolute positional encoding used in BERT. The v3 variant additionally adopts a replaced-token-detection pre-training objective and attains stronger language-understanding performance than BERT at comparable parameter scale.

We do not employ FinBERT. FinBERT is pre-trained further on financial text, including financial reports, analyst reports, and financial news, and its comparative advantage lies in identifying sentiment in financial contexts. Employee reviews are informal assessments of workplace conditions whose vocabulary and syntax differ substantially from financial text, which limits the benefit of this domain adaptation. More fundamentally, our task is not to classify sentiment polarity but to determine whether a field reports an advantage or a complaint, an objective that is not aligned with

FinBERT's pre-training.

OA.2.4 Full-Sample Inference and Variable Construction

We apply the fine-tuned classifiers to all 6,690,854 reviews and obtain, for each review, an indicator for whether its *Pros* and *Cons* fields are informative.

We measure the amount of workplace information using word counts rather than the informativeness indicator itself, because the indicator records only whether such information is present and not how much of it a job seeker encounters. An extended account of understaffing and turnover conveys more to a reader than the single word "stressful," although both fields are informative. The role of the indicator is to exclude fields that report no workplace content of the relevant tone; the role of the word count is to measure how much such content the field provides.

Let j index reviews posted for firm i between weeks $t-51$ and $t-1$, let wc_j^{cons} denote the word count of the *Cons* field of review j, and let $\mathbb{1}_j^{\text{cons}}$ equal one when that field is classified as informative and zero otherwise. The measure is

$$\text{Cons}_{i,t} = \frac{1}{N_{i,t}} \sum_{j \in (t-51,\, t-1)} wc_j^{\text{cons}} \cdot \mathbb{1}_j^{\text{cons}} \tag{OA.1}$$

$\text{Pros}_{i,t}$ is constructed analogously using wc_j^{pros} and $\mathbb{1}_j^{\text{pros}}$.

Reviews whose fields are classified as uninformative enter the sum with a value of zero but remain in the denominator: $N_{i,t}$ is the total number of reviews for firm i within the window rather than the number of informative reviews. As a result, $\text{Cons}_{i,t}$ reflects two dimensions simultaneously, namely the proportion of reviews within the window that report a complaint and the length of the complaints reported.

Screening changes the level of the measures only modestly. In the review sample, 15.1% of

Cons fields and 4.4% of *Pros* fields are classified as uninformative, and screening lowers the mean of *Cons* from 21.71 to 20.42 words (−6.0%) and the mean of *Pros* from 14.24 to 13.79 words (−3.2%).[2] The measures are therefore a refinement of the raw text rather than a product of the classification procedure.

OA.3. INTERNAL VALIDITY OF THE CLASSIFIERS

Table OA.1 reports the performance of the two classifiers on the test set. As described in Section OA.2.3, the 10,000 labeled reviews are partitioned into training, validation, and test sets in approximately 70/15/15 proportions. The test set is excluded from training and threshold selection, and its labels are human-validated. We define "the field is informative" as the positive class, because a review contributes to *Cons* or *Pros* only when the corresponding field is classified as informative.

The two classifiers perform comparably on the positive class, with precision of approximately 0.99 in both tasks and recall of 0.990 and 0.979, respectively. This evidence mitigates the concern that the absence of a significant effect for *Pros* in the main tests reflects greater measurement error in *Pros* than in *Cons*. Because the two measures are constructed with comparable precision, the asymmetry between *Cons* and *Pros* is unlikely to be attributable to differences in measurement quality.

[2] These means, and those reported in Table OA.3, are computed at the review level over the review sample described in Section OA.2.1. The means reported in Panel B of Table 3 of the main text are computed at the firm-week level over the estimation sample, in which each observation is a 51-week average across the reviews of a given firm. The two sets of means are therefore not directly comparable in level.

OA.4. EXTERNAL VALIDITY: DOES THE SCREEN EXCLUDE THE INTENDED FIELDS?

The tests reported in the preceding section benchmark the classifiers against human-validated labels and are therefore internal. This section instead uses Glassdoor star ratings as an external benchmark. Ratings are reported by reviewers themselves and are native to the platform, and the classifiers are not exposed to rating information at any stage of training or inference.

The prediction is straightforward: the word count of *Cons* should be negatively correlated with the rating, because more satisfied reviewers write shorter complaints, and the word count of *Pros* should be positively correlated with the rating. Panel A of Table OA.2 reports that both measures exhibit the predicted sign, and that their correlations with the rating strengthen after the screen is applied.

This improvement could, however, be mechanical, because setting the word counts of any subset of reviews to zero alters the correlation coefficient. To isolate this possibility, we construct a placebo screen. Within each word-count bin, we randomly select the same number of reviews that the classifiers label as uninformative and set their word counts to zero. By construction, the placebo removes text with the same count and the same length distribution as the classifier-based screen, and the two screens differ only in which reviews are selected. If the improvement associated with the classifier-based screen arises because the screen disproportionately removes shorter text, the placebo should reproduce that improvement.

The placebo screen reproduces the correlations obtained from raw word counts almost exactly (*Cons*: -0.2917 versus -0.2924; *Pros*: $+0.0427$ versus $+0.0433$), and the distribution of values across the 1,000 draws is narrow. Holding the number and the length distribution of the

excluded text constant, setting the word counts of randomly selected reviews to zero therefore has no discernible effect on the correlations, which indicates that the screening operation does not mechanically improve them.

The improvement from raw word counts to the classifier-based screen is accordingly attributable to which reviews the classifier excludes: among reviews of comparable length, the classifier excludes those that report no workplace content of the tone the field is designed to elicit. This finding corroborates the pattern documented in Section OA.1. A classifier that observes only text, and never observes ratings, produces classifications that align with this independent signal to a significantly greater degree than any random alternative.

OA.5. TIME TRENDS

Panel B of Table 3 in the main text reports that *Cons* declines from approximately 72 words to approximately 19 words over the sample period. A natural concern is whether this decline reflects inconsistent classifier performance across years.

Table OA.3 indicates that it does not. Raw and screened word counts decline by nearly identical magnitudes: raw *Cons* declines from 56.1 words in 2008 to 16.1 words in 2024 (-71%), and screened *Cons* declines from 55.5 words to 14.9 words (-73%). The decline therefore reflects a change in platform usage rather than a property of the classifier.

Overall ratings increase over the same period, from 3.21 to 3.63. Measured by the level of employer evaluations, ratings indicate improving conditions, whereas the amount of information available to job seekers declines by more than 70%. The opposing time-series patterns provide further evidence that ratings and our text-based measures capture distinct quantities.

OA.6. REPRESENTATIVE REVIEWS

Table OA.4 presents representative reviews drawn from the human-validated test set, including fields classified as informative for comparison with the examples reported in Section OA.1.

Panels A and C show that field length varies widely among informative fields, from five words to more than one hundred, which motivates measuring the amount of information with word counts rather than with the informativeness indicator alone. Panels B and D show the measurement error that raw word counts introduce: these fields would contribute up to 104 words to *Cons* and 44 words to *Pros*, although a job seeker reading them would learn of no complaint and no advantage, respectively.

OA.7. LIMITATIONS

Content that is reported under the wrong tone is excluded by the screen and is not reassigned to the opposite measure. A criticism reported in the *Pros* field is set to zero in *Pros* and does not enter *Cons*. Our measure of *Cons* therefore does not capture negative information that appears in the *Pros* field.

We adopt this approach because job seekers acquire review information field by field. A reader seeking negative information about an employer examines primarily the *Cons* field and need not read the *Pros* field in full or notice criticism reported within it, whereas a reader seeking positive information does not examine the *Cons* field. Constructing the measures at the field level corresponds to this pattern of use.

To the extent that some readers nonetheless read reviews in their entirety, *Cons* understates the amount of negative information to which those readers are exposed. The direction of this bias

is conservative for our inferences, because it attenuates rather than inflates the estimated effect of *Cons*. In terms of magnitude, 4.4% of *Pros* fields are classified as uninformative, and only a subset of these reports criticism rather than boilerplate. We leave the construction of a measure that reassigns such content to the corresponding field to future research.

OA.8. CLASSIFICATION PROMPTS

Table OA.5 reports the prompts used in the labeling stage described in Section OA.2.2. Panel A reports the prompt for Task 1, which classifies the *Pros* field, and Panel B reports the prompt for Task 2, which classifies the *Cons* field. Both prompts instruct the model to return a binary response and provide calibration guidance and worked examples. Two features of the instructions correspond to the definition of informativeness in Section OA.1. First, brevity does not disqualify a field: the prompts direct the model to recognize short statements such as "work/life balance" and "stressful" as reporting a genuine advantage and complaint, respectively. Second, a field that opens with a denial but subsequently reports a genuine advantage or complaint is classified as informative, as in the example "No cons. Sometimes late night meetings."

Table OA.1. Classifier Performance on the Test Set

	Task 1 (*Pros*)	Task 2 (*Cons*)
Test-set observations	1,500	1,500
Proportion classified as informative	97.1%	87.3%
Accuracy	0.985	0.971
Precision	0.994	0.988
Recall	0.990	0.979
F1	0.992	0.983

This table reports the out-of-sample performance of the two DeBERTa-v3-base classifiers described in Section OA.2.3. The 10,000 labeled reviews are partitioned into training, validation, and test sets in approximately 70/15/15 proportions; the test set is excluded from training and from threshold selection, and its labels are human-validated. Task 1 classifies the *Pros* field and Task 2 classifies the *Cons* field. The positive class is "the field is informative," that is, the field reports at least one workplace advantage (*Pros*) or at least one workplace complaint (*Cons*).

Table OA.2. Placebo Test of the Screen and Rating Correlations

	Cons	*Pros*
Panel A: Correlations with the Overall Rating		
Screened word count	−0.3118	+0.0721
Raw word count	−0.2924	+0.0433
Difference (Screened − Raw)	-0.0193^{***}	$+0.0288^{***}$
Panel B: Placebo Screen		
Screened word count	−0.3118	+0.0721
Placebo screen	−0.2917	+0.0427
Difference (Screened − Placebo)	-0.0200^{***}	$+0.0294^{***}$
Reviews set to zero	1,012,857	297,285
as a share of all fields	15.1%	4.4%

This table reports Pearson correlations between review-field word counts and the reviewer's overall Glassdoor star rating, computed over the review sample described in Section OA.2.1. The screened word count sets to zero the word count of fields that the classifiers label as uninformative. The placebo screen sets to zero the word counts of an equal number of randomly selected reviews within each word-count bin, so that the count and the length distribution of the excluded text are identical to those under the classifier-based screen. In both panels, a more negative difference for *Cons* and a more positive difference for *Pros* indicate a stronger correlation in the predicted direction. Significance in Panel A is based on a block bootstrap clustered by firm (1,000 resamples), which accommodates correlation among reviews of the same firm. Significance in Panel B is based on randomization inference under the null hypothesis that, conditional on text length, the reviews set to zero are selected at random; none of the 1,000 draws attains the magnitude obtained under the classifier-based screen. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table OA.3. Annual Means of Key Variables (Selected Years)

	Cons		*Pros*		Informative	Overall
Year	Raw	Screened	Raw	Screened	*Cons* fields	rating
2008	56.13	55.54	44.90	44.24	0.970	3.211
2012	35.52	34.77	21.14	20.79	0.940	3.250
2016	29.66	28.45	19.62	19.11	0.899	3.384
2020	17.51	16.03	12.27	11.88	0.816	3.697
2024	16.12	14.87	10.73	10.28	0.835	3.632

This table reports annual means for selected years over the review sample described in Section OA.2.1. *Raw* is the word count of the review field before screening; *Screened* sets to zero the word count of fields that the classifiers label as uninformative. "Informative *Cons* fields" is the proportion of *Cons* fields classified as informative in the year. "Overall rating" is the mean of the reviewer's Glassdoor star rating. All means are computed at the review level; see footnote 2 for the relation to the firm-week means reported in Panel B of Table 3 of the main text.

Table OA.4. Representative Reviews

Panel A: *Cons* Field — Classified as Informative

Words	Review text
5	Pay rate is pretty low.
6	No job security in the organization
20	You are constantly on alert for being let go; Mitel makes it extremely hard for partners to work with them
26	no place for growth / no place for re-hire. I interned during summer. When applied for fulltime, wasnt given a chance for face to face interview
105	Management makes the same mistakes over and over in order to optimize the next quarterly financial report / fiscal year close. / A large number of VPs that are useless. / The 'old guard' that has been around forever and is still there because of who they know and not what they contribute is absolutely killing the place. We've been forced to let go of high performing employees in favor of people that are friends with the old guard. These people get paid more and are largely useless. They were high performers once a time, but the world has changed and EA refuses to recognize and adapt.

Panel B: *Cons* Field — Classified as Uninformative

Words	Review text	Basis for classification
5	there is not any cons	Denies that any complaint exists
23	None. I enjoyed my experience. I can't come up with any negatives during my tenure with company. Some people enjoy the work.	Denies that any complaint exists, then reports praise
70	It's difficult to come up with any cons of working for this company, however Stryker seems to be great at keeping teams lean while growing at a rapid pace. We accomplish a lot (and very quickly) with fewer resources. For the ambitions, talented people who work here, the fast-paced environment and full work load really feels like more of a pro than a con most of the time.	Reports no complaint; content belongs to the *Pros* field
104	Recruitment is a challenging career, and every day brings its own set of obstacles. However, at Robert Half, it's the exceptional training and supportive management team that help you overcome these hurdles. The training is thorough and ongoing, ensuring you're always prepared to tackle whatever comes your way. Any challenges that arise in the recruitment process are easily managed thanks to the strong support from both leadership and teammates. The resources and guidance provided here truly transform potential setbacks into opportunities for growth. If you're pursuing a career in recruitment, Robert Half offers the tools and environment you need to succeed.	Reports no complaint; content belongs to the *Pros* field

Panel C: *Pros* Field — Classified as Informative

Words	Review text
5	autonomy, good work great manager
5	Good salary, fun environment, magical

92	I love working at Udemy! For the onboarding process, the whole staff makes you feel welcome and people went out of their way to tell me about their departments. My manager is very thoughtful about knowledgeable, and maintains open dialogue regarding architectural choices and work division. I feel supported and inspired to succeed, and receive constant feedback about my progress. The culture is similarly open and there is a team that plans events and parties (often), and everyone sits together in the kitchen for lunch. I feel lucky to be a Udemy!	

Panel D: *Pros* Field — Classified as Uninformative

Words	Review text	Basis for classification
4	No pros, dumb environment	Denies that any advantage exists
34	Not many pros about working here, Atco has decided to make me a travelling construction crew lineman. I was hired as a service man now on a travelling crew. Will be giving my notice.	Reports no advantage; content belongs to the *Cons* field
35	I didn't see any pros here. This was by far the worst job I have ever worked out. They monitor the team you spend on a ticket and micromanage every little thing you do.	Reports no advantage; content belongs to the *Cons* field
44	There are none. I can not think of one thing that republic does well or a positive result for their hard working employees. Trust me, after one month here I was looking to leave to a new opportunity that was not a lateral move.	Denies that any advantage exists

This table presents representative review fields drawn from the human-validated test set described in Section OA.2.3. Panels A and C report fields classified as informative; Panels B and D report fields classified as uninformative. "Words" is the raw word count of the field, that is, the value the field would contribute to *Cons* or *Pros* in the absence of the screen. Review text is reproduced verbatim and without correction; "/" denotes a line break in the original text.

Table OA.5. Classification Prompts

Panel A: Task 1 — Classification of the *Pros* Field

You are an experienced workplace analyst specializing in employee review analysis. Given an employee review's positive section (pros/advantages), determine whether it contains genuine positive content.

Core Principles:

- **Balanced accuracy approach** — Recognize authentic positive content even if briefly stated or conditionally mentioned
- **Context-aware classification** — Consider the overall meaning, not just keywords
- **Keywords are illustrative, not exhaustive** — Include any expressions that convey similar positive meaning
- **Apply intelligent synonym recognition** — Interpret equivalent phrases and synonymous terms
- **Contradiction detection** — Watch for positive words followed by obvious negation

Specific Requirements:

1. **Should be classified as containing advantages:** Explicit praise, satisfaction expressions, benefit mentions, favorable comparisons, positive aspects of work environment, compensation, culture, or opportunities. **Include conditional positives** (e.g., "decent benefits, but…") and **brief positive statements** (e.g., "work/life balance").
2. **Should NOT be classified as containing advantages:** Explicit statements like "no advantages," "can't think of any," "nothing good," empty/meaningless content, or contradictory statements where positive words are immediately negated.
3. **Key positive indicators (examples, not exhaustive):** good, great, excellent, benefits, opportunities, growth, supportive, flexible, competitive, love, enjoy, satisfied, amazing, best, decent, okay, pleasant

Important Calibration Guidelines:

- **Conditional positives count:** "The only pro is X" means X is still a genuine advantage
- **Brief statements matter:** Single positive terms or short phrases like "pleasant people" should be recognized
- **Minimal positives are still positives:** "decent benefits" or "pay is okay" contain authentic positive elements
- **When in doubt on borderline cases:** Lean toward positive if ANY genuine workplace advantage is mentioned

Response Format: [True/False]

Example 1. Input: "Great people, good benefits, flexible work environment" — The content contains multiple positive indicators including "great people," "good benefits," and "flexible work environment," all expressing clear advantages of working at the company. Output: True

Example 2. Input: "Nothing comes to mind, can't think of any advantages" — The content explicitly states inability to identify advantages with phrases like "nothing comes to mind" and "can't think of any advantages," indicating no positive content. Output: False

Example 3. Input: "The only pro is a paycheck." — While "only" suggests limitations, "paycheck" represents a genuine workplace advantage (compensation). The statement acknowledges at least one positive aspect. Output: True

Example 4. Input: "decent benefits, pays well but only sometimes" — Contains clear positive indicators "decent benefits" and "pays well." The conditional "but only sometimes" doesn't negate these genuine advantages. Output: True

Example 5. Input: "Work / life balance ." — Brief but clear positive statement about a significant workplace advantage. Output: True

Now, directly answer the question based on the input.

Panel B: Task 2 — Classification of the *Cons* Field

You are an experienced workplace analyst specializing in employee review analysis. Given an employee review's negative section (cons/complaints), determine whether it contains genuine complaint content.

Core Principles:

- **Balanced accuracy approach** — Recognize authentic negative content even if briefly stated or conditionally mentioned
- **Context-aware classification** — Consider the overall meaning, not just keywords
- **Keywords are illustrative, not exhaustive** — Include any expressions that convey similar negative meaning
- **Apply intelligent synonym recognition** — Interpret equivalent phrases and synonymous terms
- **Contradiction detection** — Watch for negative words followed by obvious positives

Specific Requirements:

1. **Should be classified as containing complaints:** Explicit complaints, dissatisfaction, criticism, problems, topic category for improvement, negative review about any work aspect. **Include conditional negatives** (e.g., "good company, but long hours") and **brief negative statements** (e.g., "stressful", "fast pace").
2. **Should NOT be classified as containing complaints:** Explicit statements like "no cons," "nothing negative," "no disadvantages," obviously positive content mistakenly placed in complaints section, or contradictory statements where genuine "no complaints" expressions are clearly stated.
3. **Key negative indicators (examples, not exhaustive):** poor, bad, terrible, lack of, insufficient, low, limited, difficult, stressful, unfair, micromanagement, understaffed, long hours, busy, expectations

Important Calibration Guidelines:

- **Conditional negatives count:** "No major issues, but sometimes overtime" means overtime is still a genuine complaint
- **Brief statements matter:** Single negative terms or short phrases like "stressful environment" should be recognized
- **Minimal negatives are still negatives:** "can be busy" or "fast pace sometimes" contain authentic negative elements
- **Mixed content with actual issues:** Even if starting with "no cons," any subsequent genuine workplace issue should be classified as negative
- **When in doubt on borderline cases:** If ANY genuine workplace complaint or concern is mentioned, lean toward negative

Response Format: [True/False]

Example 1. Input: "Poor management, long hours, and low pay" — The content contains clear complaints including "poor management," "long hours," and "low pay," all expressing dissatisfaction with work conditions. Output: True

Example 2. Input: "No cons, everything is great here" — The content explicitly states "no cons" and expresses positive sentiment, indicating no genuine complaints despite being in the negative section. Output: False

Example 3. Input: "No cons \ Sometimes late night meetings" — While starting with "no cons," the content mentions "late night meetings," which represents a genuine workplace concern. Output: True

Example 4. Input: "Can be a fast pace sometimes" — Brief but clear negative statement about work intensity, representing an authentic workplace challenge. Output: True

Example 5. Input: "Nothing to mention sometimes works more then shift hours" — Despite the dismissive opening, "works more then shift hours" describes a genuine workplace issue (overtime/long hours). Output: True

Now, directly answer the question based on the input.

This table reproduces the prompts used in the labeling stage described in Section OA.2.2. Panel A reports the prompt for Task 1, which classifies the *Pros* field, and Panel B reports the prompt for Task 2, which classifies the *Cons* field. Prompts are reproduced verbatim as submitted to the model, including the original emphasis and punctuation. Both tasks are run with GPT-4o at a generation temperature of zero, in batches of fifteen reviews per query with structured output.