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RESEARCH

Assessing the Impact of Price-Matching Guarantees and Price Fluctuations on Consumer Feedback: Insights from the Online Consumer Electronics Market

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Abstract
This study examines how price-matching guarantees (PMGs) influence consumer behaviour in online retail. Using a Difference-in-Differences design (DiD), the paper tracks how product ratings react to within-product price changes during PMG periods. PMGs alone have no effect, but price increases under an active PMG reduce ratings, with impacts intensifying over time and concentrating on cheaper, less visible items. To probe mechanisms, the paper links review texts to the product–platform panel and builds transparent dictionaries for price/value language and sentiment. When prices rise under PMG, references to price/value spike, while placebo topics (quality, service, delivery) remain flat; sentiment shifts little. Event-study tests show flat pre-trends, supporting identification. Overall, PMGs act as market signals whose disciplinary bite emerges when prices move, especially where buyers are highly price sensitive or visibility is low. Antitrust and policy implications include safeguarding pricing transparency and consumer protection while balancing informational benefits against risks of anti-competitive conduct and consumer harm.

Keywords Price-matching guarantees · Online retailing · Online consumer reviews · Difference-in-Differences · Signalling theory · Retail tactics and pricing

JEL Classification C23 · C81 · D12 · D83 · H83 · K21 · L15 · L40 · L51 · L81 · M31

Highlights

- **PMG framing conditions the price–ratings link:** PMGs do not affect ratings in levels, but *price increases during an active PMG* are associated with *lower* ratings, consistent with consumer "*discipline*" when a low-price signal is violated.

- **Disciplining effects are heterogeneous:** The negative price-under-PMG response is stronger for *cheaper* and *less visible* products, and the perceived effect of PMGs becomes more favourable as exposure persists.

- **Mechanism evidence from review text:** When prices *move* under PMGs, price/value language in reviews *spikes*, while placebo topics (quality/service/delivery) do not respond—supporting a value/reference-point channel rather than generic sentiment shifts.

Extended author information available on the last page of the article

Introduction

In today's competitive e-commerce markets, retailers compete not only on product features and quality but also on pricing cues. A prominent instrument is the price-matching guarantee (PMG), typically advertised as a promise to match or beat rivals' prices, used to shape price image, signal competitive pricing, and build trust. Unlike classic promotions, PMGs do not post a lower price ex ante—being something like "*if you see a lower price for any model we stock, we will be happy to refund you the difference*"; rather, they offer a refund conditional on consumers incurring the search and proof costs of a claim.

Theoretical and empirical work offers three main views. First, PMGs may soften competition and sustain higher prices (e.g., Belton, 1987; Hay, 1981; Salop, 1986). Secondly, they may facilitate price discrimination when firms can screen heterogeneous consumers (Corts, 1997; Nalca et al., 2010; Png & Hirshleifer, 1987, among others). Thirdly, in a signalling framework, consumers infer that PMG adopters are low-price sellers, so the policy shifts price perceptions and purchase intent (Dutta et al., 2007; Jain & Srivastava, 2000; Mamadehussene, 2019; Moorthy & Winter, 2006; Srivastava, 1999, 2001, 2004). Because PMG adoption is typically more persistent than day-to-day prices, firms can advertise PMGs more effectively, and consumers may be more aware of guarantees than of posted prices; in such settings, PMGs act as signals to price-uninformed shoppers (see Moorthy & Winter, 2006). Indeed, despite high online transparency, many consumers are not fully informed about current prices and rely on sellers' price/trust "image" (Simester, 1995). PMGs are one of several trust-building devices; retailers often bundle them with warranties or loyalty perks to reinforce value claims. These policies thus serve as tangible assurances that consumers are receiving good value.

A PMG, however, does not ensure the lowest price ex post: consumers can search and claim refunds. Understanding when guarantees credibly signal low prices therefore hinges on frictions, policy design, and the information environment. Prior work has examined individual drivers (time costs, search costs, policy terms, and price cues) (e.g., Biswas et al., 2002; Kukar-Kinney & Walters, 2003; Srivastava & Lurie, 2001), but market-level discipline remains less explored. In particular, whether consumers' collective behaviour deters false signalling is crucial: withholding repeat purchases, negative word-of-mouth, and calls for redress can discipline sellers (Rao et al., 1999; Wernerfelt, 1988) and reduce incentives to mislead (Feick & Price, 1987; Wernerfelt, 1988), with implications for PMGs' effectiveness and for policy oversight (Bloom, 1989; Edlin, 1997; Ippolito, 1990). This mechanism hinges on how consumers interpret guarantees in light of market conditions and observed conduct (Stiglitz, 1989). Seminal work by Srivastava and Lurie (2004) argues that market factors shape beliefs about others' search and, in turn, the effectiveness of PMGs as signals (see also Hamilton & Chernev, 2013; Pee et al., 2018; Bergel et al., 2019).

We study online consumer electronics, where prices are volatile and consumers rely heavily on online consumer reviews (OCRs). We investigate whether PMGs operate as a market-discipline device visible in ratings and review text, not only in posted prices. Our question is whether the effectiveness of PMGs in signalling low prices depends on how consumers evaluate sellers in OCRs, given their shopping experience and the prices paid.

We implement a Difference-in-Differences (DiD) design in which products are "*treated*" when subject to PMGs. We collect daily prices and OCRs for PMG-targeted items on NewEgg, a leading US online platform, and match identical items sold on Amazon US, which do not offer PMGs, to form a comparison set.[1] In particular, NewEgg's items constitute the treatment group, while Amazon's ones are controls. The quasi-experimental design exploits within-product timing and cross-retailer contrasts to isolate the effect of PMGs on the review landscape. This allows us to track whether price changes under PMGs are followed by shifts in ratings and sentiment, and to study the dynamics of reactions during policy activation, thus possibly informing the efficacy of pricing signals in online retail and offering practical guidance to retailers on communicating value.

Two bench cases organize interpretation:

- Hypothesis 1 (H1): *If prices rise during PMG validity and ratings fall, OCRs act as a disciplinary mechanism, punishing deviations from the "low-price" frame. Negative feedback can erode credibility and induce sellers to realign pricing with consumers' value expectations.*
- Hypothesis 2 (H2): *If ratings do not fall when prices increase, other factors—quality, brand, or service—may dominate perceptions; consumers may be more forgiving within a PMG frame, or trust may neutralize modest price changes relative to rivals.*

Static estimates show that prices and PMG status, separately, do not affect ratings, but their interaction is negative and significant $\left(\mathrm{logPrices}_{\mathrm{i,r,t}} \cdot \mathrm{PMG}_{\mathrm{i,r,t}} = -0.00796\right)$: when a PMG is active, higher prices receive lower ratings. Allowing effects to vary with policy duration suggests that ratings improve as the PMG persists, consistent with evolving perceptions. Weighting by a product-popularity index yields similar results. Heterogeneity indicates stronger discipline for cheaper and less visible items: for these, price increases during PMGs draw sharper rating penalties, and effects accumulate with policy duration. Dynamic ATTs suggest instead that PMGs exert a positive effect on products' ratings, starting 6 days after the treatment and lasting up to the ninth day. Overall, these results broadly align with Hypothesis 1.

We also bring text-as-data evidence. We merge Amazon/NewEgg review texts to the product–provider–week panel (by ASIN/item code) and construct dictionary flags for (*i*) any *price/valu*e language and its (*ii*) explicitly positive and (*iii*) explicitly negative variants. Leveraging a two-way fixed-effects linear-probability model, we find that the likelihood that reviews mention price/value rises sharply when prices move under PMGs (coefficient 2.341, s.e. 0.759, $p=.002$); explicitly positive wording increases marginally (coefficient 0.855, s.e. 0.448, $p<.10$), whereas explicitly negative wording is rare and imprecise (coefficient 0.458, s.e. 0.426). Event-study checks show flat pre-trends also in this analysis, and placebo topics (quality, service, delivery) do not respond, indicating a topic-specific, not generic, shift.

Finally, we situate the mechanism within two lenses. In the signalling view (Srivastava & Lurie, 2004), consumers discipline high-price mimickers, so PMGs operate as reference-point devices rather than mechanical price cuts (Moorthy & Winter, 2006). Alternatively, PMGs can enable screening or tacit coordination when frictions and heterogeneity allow firms to target inelastic buyers; Robbiano's (2025) model for data-driven markets formalizes these conditions and shows how auto-refunds could neutralize discriminatory rents. Our evidence is most consistent with signalling in this setting: the PMG label does not move ratings when prices are flat; when prices rise under PMGs, price/value salience

[1] NewEgg Price Matching Guarantee (policy page): https://kb.NewEgg.com/knowledge-base/price-match-guarantee/.

jumps and ratings fall; and average daily price movements are small. Explicit PMG mentions in reviews are rare, suggesting the policy acts as a background frame.

Our paper draws on three strands of literature. First, signalling theory shows how observable cues resolve information asymmetries (e.g., Nelson, 1970): PMGs can credibly signal low prices when buyers search and claim, reputational bonding is strong, and hassle costs screen types (Dugar, 2007; Dugar & Sorensen, 2006; Ippolito, 1990; Jain & Srivastava, 2000; Kirmani & Rao, 2000; Mamadehussene, 2019; Moorthy & Winter, 2006; Moorthy & Zhang, 2006; Srivastava & Lurie, 2004; Yuan & Krishna, 2011). Secondly, OCRs shape demand and trust and interact with pricing: Higher prices typically depress perceived value and ratings, though effects are context-dependent (Bar-Isaac & Tadelis, 2008; Cabral & Li, 2015; Carnehl et al., 2022, 2024; Chen & Xie, 2008; Chevalier & Mayzlin, 2006; Dellarocas, 2003; Filieri & McLeay, 2014; Ghose & Yang, 2009; Jin et al., 2016; Li & Hitt, 2008; Luca & Reshef, 2021; Sorokin, 2021; Zegners, 2019; Zhu & Zhang, 2010). Thirdly, quasi-experimental evidence on PMGs suggests short-lived, adopter-side price increases rather than broad market-wide collusion, with heterogeneity by visibility and price tier (Bottasso et al., 2025; Zhuo, 2017). In particular, in a companion paper that relies on the same dataset of our work, Bottasso et al. (2025) show that PMG activation does not lower posted prices: the adopter's price increases by about 4.7% during the validity window (and shortly after), while the main non-adopting competitor is not affected, with effects driven by highly-rated, visible, and expensive products. Our contribution is to bring consumer outcomes—ratings and review text—into this debate, using repeated on/off exposure to identify the mechanism whereby a PMG frame sharpens value expectations and conditions the rating response to price moves.

In particular, this paper contributes to the literature in three ways. First, we provide evidence on whether PMGs operate as a market-discipline device in online retail by linking policy exposure and posted prices to consumer ratings. Secondly, we test the signalling interpretation in Srivastava and Lurie (2004) by examining whether the relationship between prices and ratings differs when the low-price frame is active. Thirdly, we study heterogeneity by product characteristics—price level and online visibility—to assess where the disciplinary response is strongest. Together, these results inform how retailers communicate value in digital markets and how PMGs should be interpreted from a competition and consumer-protection perspective.

To conclude, the study has different policy implications from both antitrust and policy-makers' perspectives. On the one hand, from an antitrust standpoint, our findings provide insights into the competitive dynamics of online retail markets. By demonstrating the impact of pricing policies such as PMGs on consumer behaviour and product ratings, the research can inform antitrust authorities about the potential anti-competitive effects of such strategies. Specifically, if retailers use PMGs to signal low prices and gain a competitive advantage, this could raise concerns about collusion or exclusionary practices that harm consumer welfare or stifle competition. Antitrust regulators may need to assess the use of pricing policies like PMGs in the context of market power and competition enforcement, ensuring that they do not lead to anti-competitive behaviour or market distortions. On the other hand, from a policy-maker's perspective, our study highlights the importance of consumer protection and transparency in online markets. If PMGs influence consumer perceptions and purchasing decisions, policy-makers may need to consider regulatory measures to ensure that pricing signals are accurate, truthful, and beneficial for consumers. This could involve guidelines or regulations governing the use of pricing policies in online retailing, requiring retailers to provide clear and accurate information about their pricing strategies and commitments to competitive pricing. Additionally, policy-makers may explore measures to enhance consumer awareness and education about pricing practices in online markets, empowering consumers to make informed choices and protect their interests. More broadly, this echoes reference-price regulation in sale advertising: one could condition the marketing of a "low-price guarantee" on demonstrable enforceability—e.g., a minimum share of successful refunds or mandatory disclosure of redemption/refund rates—so that the PMG cannot function as a costless "low-price" badge.[2]

The rest of the paper presents data (section "Data"), outlines identification (section "Identification Approach"), reports results and robustness (section "Results"), and concludes (section "Conclusions").

Data

We study the consumer electronics market, a sector frequently targeted by PMGs and well suited to observing pricing signals and online consumer responses. Electronic goods are comparatively less seasonal, yielding cleaner price signals over our window (May–October 2018), which excludes major holidays. Although many sellers compete (from local stores to national chains), we focus on online retailing to exploit observables unique to this channel—online consumer reviews (OCRs)—and because digital markets reduce the cost and time of evaluating search goods, potentially sharpening responses to PMG policies.

Within online sellers, we centre on NewEgg, a major US electronics retailer that implements PMGs. NewEgg displays a badge on product pages when the guarantee applies; eligible purchases carry a 14-day window for claiming a refund upon observing a lower advertised price at listed rivals.[3] We identified all NewEgg electronics with PMGs in May 2018 and tracked these items from May to October, recording prices, PMG status, and OCRs. Importantly, our price series captures posted prices shown on product pages; we do not observe PMG claims/refunds or transaction prices net of reimbursement. For comparison, we matched the same products on Amazon US, which does not offer PMGs. The final sample comprises 58 products across 19 sub-categories (e.g., components, tablets, phones, printers, speakers). We restrict attention to items sold directly by NewEgg and Amazon (excluding marketplace/third-party sellers, to which PMGs do not apply), thereby abstracting from platform intermediation issues. While this narrows the sample, it aligns with our design and with the platforms' review guidelines, which help sustain recommender-system integrity.

Because no comprehensive repository exists for daily prices, we built a Python scraper with change-detection alerts and deployed it on AWS virtual servers. This approach delivers high-frequency, high-quality product-page data in real time, avoiding the limitations of price trackers or scraped price-history plots. Importantly, our price series captures posted prices shown on product pages; we do not observe PMG claims/refunds or transaction prices net of reimbursement. We therefore cannot provide descriptive evidence on redemption frequencies or the average refund cost of the policy. As an external benchmark, evidence from retail settings reports redemption rates ranging from roughly 1% to 25% and suggests that rates above about 10% are compatible with the redemption assumptions used in several PMG models (e.g., Moorthy & Winter, 2006).

[2] See the FTC Guides Against Deceptive Pricing (16 C.F.R. Part 233)—official eCFR: https://www.ecfr.gov/current/title-16/chapter-I/subchapter-B/part-233.

[3] See https://promotions.NewEgg.com/nepro/16-2624/index.html.

Alongside prices and PMG labels, we collect OCRs, namely star ratings and written reviews, from which we construct online-exclusive product characteristics. OCRs are a valuable source of information for consumers and retailers alike: for example, Timoshenko and Hauser (2019) show that insights gleaned from OCRs can approximate those from interviews and focus groups. In practice, online platforms surface successful or popular products and facilitate information sharing among users, generating network effects that make OCRs central to platforms' information-gathering strategies (Belleflamme & Peitz, 2018). Platforms also tailor recommendations using past interactions, a process visible in OCR patterns. In particular, Belleflamme and Peitz (2018) contend that these mechanisms are indispensable for the operational success of digital retail platforms. They assert that prospective consumers face an opportunity cost when assessing the quality of items and their suitability to personal preferences. Consequently, buyers place considerable value on ratings, reviews, and recommendations as they facilitate more informed decision-making. Furthermore, in environments where there is a diverse array of consumer tastes, ratings and reviews play a crucial role in evaluating product quality, providing valuable guidance to customers in their selection process. Moreover, OCRs are often perceived as less influenced by commercial interests compared to product information provided directly by sellers. This lack of bias makes them a valuable resource for consumers when making consumption decisions (Chen & Xie, 2008).

OCRs also underpin market discipline. Buyers' actions—negative word-of-mouth and low ratings—penalize misleading claims and can raise the cost of false price signals (e.g., Rao et al., 1999; Wernerfelt, 1988). Publicly displayed reviews hold sellers to account, strengthen consumer voice, and create feedback loops that motivate service improvements. Positive feedback builds credibility and trust, crucial in online transactions, while negative reviews operate as quasi-regulation, deterring poor conduct.

For each product, we record the star rating (0–5), the count of written reviews, and an index of relative popularity: the ratio of a product's review count to the maximum within its sub-category. We also measure product "age" (days since first observed availability) and treatment duration (days under PMG). Finally, we proxy search frictions with a Google search-rank measure. For each item–retailer pair, we query ("product name" AND "retailer name") and record the rank of the specific product page up to the third results page (1–30). Because Google can also return similar products or category pages, we retain only the exact product page.

It is worth noting that we also collect and link both Amazon US and NewEgg review texts to the product–provider–week panel via platform identifiers (ASIN/item code) and implement a text-as-data module that constructs transparent dictionary flags for (*i*) any price/value language and its (*ii*) explicitly positive and (*iii*) explicitly negative variants, whose results will be discussed in section "Other Threats to Identification."

Table 1 summarizes key variables. The panel contains 9028 daily observations for 58 products over 175 days on NewEgg and Amazon US. Ratings average 4.15 stars overall (SD = 0.68); within NewEgg, the mean is 4.14 (SD = 0.83), and Amazon exhibits similar means with lower dispersion (SD = 0.48). Prices are dispersed (mean \$253.48; SD = \$307.53). Popularity, age, and search rank also vary substantially (means 0.26, 865.59, and 7.55; SDs 0.30, 759.07, and 9.08, respectively). PMG spells on NewEgg average 14 days (SD = 15.44; range 1–74). Our time span predates COVID-19, avoiding pandemic-era distortions in demand, availability, and online shopping behaviour.

Table 1 Summary statistics

	Product ratings [0,5]	Provider prices ($)	Product popularity [0,1]	Search rank [1,30]	Product age (days)	Treatment duration (days)
Full sample	4.15	253.48	0.26	7.55	865.59	
	(0.68)	(307.53)	(0.30)	(9.08)	(759.07)	
NewEgg	4.14	253.15	0.20	10.74	910.20	
	(0.83)	(302.39)	(0.23)	(10.89)	(853.06)	
NewEgg and $PMG_{i,r,t} = 0$	4.13	253.67	0.19	11.97	786.75	
	(0.86)	(277.81)	(0.23)	(11.48)	(752.62)	
NewEgg and $PMG_{i,r,t} = 1$	4.19	251.01	0.25	5.68	1417.84	14.47
	(0.70)	(387.59)	(0.23)	(5.77)	(1035.15)	(15.44)
Amazon US	4.16	253.81	0.32	4.36	820.98	
	(0.48)	(312.63)	(0.34)	(5.08)	(648.69)	

Notes: PMGs adopted by NewEgg are the treatment of interest, so that NewEgg's products are the treated sample; an alternative sample has been built by recovering products' information for the same titles targeted by NewEgg's PMGs but sold on a different retailing website (Amazon US). The sample period includes 175 days, from May 2018 until October 2018

Identification Approach

We estimate the causal effect of NewEgg's PMG policy on targeted products' ratings using a Difference-in-Differences (DiD) design. Treated items are those under an active PMG; controls are the same products sold on Amazon US (which does not offer PMGs). We compare before–after changes in ratings for treated products with contemporaneous changes for controls and estimate the following baseline specification:

$$\begin{aligned} Ratings_{i,r,t} = \alpha + \beta logPrices_{i,r,t} + \phi PMG_{i,r,t} + \gamma(logPrices_{i,r,t} \times PMG_{i,r,t}) \\ + \iota X_{i,r,t} + \mu_i + \tau_t + \psi_{i,r,t} \end{aligned} \tag{1}$$

In Eq. (1), $\text{Ratings}_{\text{i,r,t}}$ is the number of stars for product i on retailer r at day t, $\text{Prices}_{\text{i,r,t}}$ is the (natural log) posted price, and $\text{PMG}_{\text{i,r,t}}$ is a treatment indicator equal to one when a PMG applies. We include day fixed effects (τ_{t}) to absorb shocks common to all goods and product fixed effects (μ_{i}) to control for time-invariant heterogeneity at the product–retailer level. The vector $X_{\text{i,r,t}}$ gathers time-varying product characteristics (reviews, popularity, and age) that may co-vary with ratings. All specifications are estimated by OLS with Huber–White heteroscedasticity-robust standard errors.

The parameter of primary interest is ϕ, the DiD estimate of the average level effect of PMGs on ratings. The coefficient γ on $\left(\text{logPrices}_{\text{i,r,t}} \times \text{PMG}_{\text{i,r,t}}\right)$ captures whether the price–ratings slope differs under a PMG: a significant γ indicates that the effect of prices on ratings depends on policy status. In particular, a negative γ implies that when prices rise during PMG validity, ratings fall more than they otherwise would, consistent with market discipline operating through OCRs. Indeed, negative ratings can undermine a seller's reputation and credibility, potentially leading to a loss of trust and consumer confidence in the seller's pricing policies.

Because a PMG guarantees a refund rather than announcing an immediate price drop, its impact may build over time as consumers learn, experience the policy, and adjust expectations. We therefore allow the effect of PMGs to vary with policy duration:

$$\begin{aligned} Ratings_{i,r,t} = \alpha + \beta logPrices_{i,r,t} + \phi PMG_{i,r,t} + \gamma(logPrices_{i,r,t} \times PMG_{i,r,t}) \\ + \lambda(PMG_{i,r,t} \times Treatment\_Duration_{i,r,t}) + \iota X_{i,r,t} + \mu_i + \tau_t + \psi_{i,r,t}. \end{aligned} \tag{2}$$

Here, the term $\text{Treatment\_Duration}_{\text{i,r,t}}$ counts the days since PMGs activation for product i (e.g., zero before activation; one on the first day). Given Eq. 2, the time-varying PMGs effect is therefore $\phi + \lambda \cdot \text{Treatment\_Duration}_{\text{i,r,t}}$, allowing us to trace the policy's profile as exposure lengthens.

We also examine possible impact heterogeneity. First, we re-estimate Eq. (1) on subsamples split by initial price: cheaper (expensive) products have baseline prices below (above) the sample mean at the start of the period. This captures that price sensitivity and perceived value may differ across tiers. Secondly, in the spirit of models that emphasize search frictions (Dugar & Sorensen, 2006; Hviid & Shaffer, 1999), we test whether effects vary by online visibility. Using the search-rank measure described in section "Data," we classify products as easily found if the initial rank is less than three and hardly found otherwise (the rationale behind this threshold relates to the median of the distribution of the visibility index). If search costs are higher for less visible items, PMGs may operate as a stronger disciplinary device in that segment, with false signals more likely to be penalized through OCRs.

We also corroborate identification with a complete suite of robustness checks reported in both section "Results" and Appendix B, including event-study pre-trends, placebo timings, and random-allocation tests. Results are stable across these diagnostics.

Finally, we link review texts from both providers to the product–provider–week panel using platform identifiers (ASIN/item code). For each product–provider–week cell, we construct transparent, pre-specified dictionary flags that indicate (*i*) whether any review mentions price or value-for-money and (*ii*) whether such mentions are explicitly positive or explicitly negative. The outcomes are therefore cell-level indicators (and, in robustness, rates) that capture the salience and valence of price/value language in that week's reviews. Identification comes from within-product variation over time. We compare weeks when a product's price moves under a period with the guarantee active to weeks when it does not, while absorbing all time-invariant product heterogeneity with product fixed effects and economy-wide shocks with calendar-week fixed effects. In practice, this means we attribute any shift in price/value talk specifically to the coincidence of two conditions for the same product: a within-week price change and exposure to the guarantee in that week. Weeks with no guarantee at the same product serve as its own control; Amazon observations, for which the guarantee is never active, provide additional cross-retailer contrast under the same calendar-week shocks. To reduce small-sample noise and reflect information content, estimation is weighted by the number of reviews in each cell, with standard errors clustered at the product level. We include a parsimonious set of time-varying controls (e.g., review volume and product age) to account for changes in review intensity. The key identifying assumption is that, conditional on product, week fixed effects, and these controls, week-to-week price movements under the guarantee are not systematically driven by unobserved factors that also make reviewers more (or less) likely to talk about price/value.

Results

Table 2 provides the DiD estimates. In particular, column (1) first shows estimates of the static version of our model, as in Eq. 1, without including control variables, while column (2) displays the replication of the analysis after including controls.

Estimates indicate that changes in product prices, as reflected in the logarithm of the provider's retailing price, do not have a significant direct impact on product ratings. Moreover, on average, the presence of the PMG policy alone does not have a statistically significant impact on our variable of interest. This means that the overall effect of prices and the PMG on ratings, independently of each other, is not statistically discernible from zero. However, it should be noted that the negative and statistically significant coefficient on the interaction term, namely $\left(\mathrm{logPrices}_{i,r,t} * \mathrm{PMG}_{i,r,t}\right)$, indicates that the relationship between prices and ratings differs depending on whether the PMG is present or not; specifically, when the PMG policy is present $\left(\mathrm{PMG}_{i,r,t} = 1\right)$, the negative coefficient (−0.00821) suggests that an increase in prices is associated with a decrease in ratings, statistically significant at the 5% level. Very similar effects are obtained when repeating the analysis with the inclusion of control variables (Table 2, col. 2).

To explore whether PMGs' duration matters in determining the level of products' ratings, we estimate Eq. 2, which allows for a differential impact of PMGs according to the number of days elapsing since the application of the policy. Results of this analysis, reported in column (3) of Table 2, indicate that one additional day of the policy being in place

Table 2 Baseline results

Dep. Var.: products' ratings	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{logPrices}_{i,r,t}$	−0.00373	0.00726	−0.00390	0.00691	−0.01008	0.00436	−0.01043	0.00382
	(0.01543)	(0.01614)	(0.01543)	(0.01610)	(0.01619)	(0.01701)	(0.01618)	(0.01695)
$\text{PMG}_{i,r,t}$	0.01790	0.01391	−0.00086	−0.00255	0.00813	0.00858	−0.01384	−0.01011
	(0.01619)	(0.01640)	(0.02094)	(0.02108)	(0.02054)	(0.02055)	(0.02656)	(0.02613)
$\text{logPrices}_{i,r,t} * \text{PMG}_{i,r,t}$	−0.00821**	−0.00796**	−0.00833**	−0.00807**	−0.00667*	−0.00714*	−0.00678*	−0.00723*
	(0.00373)	(0.00367)	(0.00384)	(0.00377)	(0.00375)	(0.00381)	(0.00388)	(0.00392)
$\text{PMG}_{i,r,t} * \text{Treatment\_Duration}_{i,r,t}$			0.00158***	0.00140**			0.00188***	0.00160***
			(0.00059)	(0.00055)			(0.00067)	(0.00062)
Observations	9028	9028	9028	9028	9028	9028	9028	9028
Controls	×	✓	×	✓	×	✓	×	✓
Product FE	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓
R^2	.87206	.87271	.87216	.87278	.86413	.86519	.86426	.86528

Notes: This table presents DiD estimates of Eq. 1 (columns 1, 2, 5, 6) and Eq. 2 (columns 3, 4, 7, 8) on the effect of PMGs on products' ratings. $\text{Ratings}_{i,r,t}$ is the number of stars gained by product *i* on retailing website *r* at time *t*. $\text{Prices}_{i,r,t}$ represents product *i*'s price (natural logarithm) in day *t* for retailer *r*. $\text{PMG}_{i,r,t}$ is a dummy variable equal to 1 if items are subject to the PMG in day *t*. $\text{Treatment\_Duration}_{i,r,t}$ captures the number of days since the application of the PMG. All specifications include product and time fixed effects. Controls include the absolute and the relative number of reviews, as well as products' age. Regressions in columns (5), (6), (7), and (8) use observations weighted according to the product popularity index, as described in section "Data," at the beginning of the sample period. All specifications are estimated by OLS. Huber–White heteroscedasticity-robust standard errors are shown in parentheses: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively significantly increases products' ratings $\left(\text{PMG}_{i,r,t} * \text{Treatment\_Duration}_{i,r,t} = 0.00158\right)$, at the 1% level of statistical significance. This suggests that the effect of the PMG on product ratings varies depending on the duration since the application of the policy. Specifically, the positive coefficient indicates that, as the number of days since the application of the PMG increases, the impact on product ratings becomes more positive. This could imply that consumers' perceptions of the PMG become more favourable over time, leading to higher ratings for products subject to the policy.

Once again, a very similar estimated coefficient is obtained when we repeat the analysis with the inclusion of control variables (col. 4). It is worth noting that, also in this case, even though the main effect of PMGs is not different from zero, the significant estimated coefficient associated with the $\left(\text{logPrices}_{i,r,t} * \text{PMG}_{i,r,t}\right)$ term suggests that the impact of prices on ratings varies depending on whether the policy is present or not. This highlights the importance of considering the interaction effects and not solely relying on the main effects when interpreting regression results, being again consistent with the expectation that consumers may perceive price increases during the PMG period as a deviation from the expected pricing strategy implied by the policy, leading to lower ratings.

Finally, in columns (5), (6), (7), and (8) of Table 2, we replicate the previous regressions by weighting observations according to the product popularity index, as described in section "Data," at the beginning of the sample period; reassuringly, results do not change. Indeed, the coefficient of both $\text{logPrices}_{i,r,t}$ and $\text{PMG}_{i,r,t}$ are not statistically significant both in the static specification without control variables (col. 5) and in the one including controls (col. 6), while estimated point estimates on the $\left(\text{logPrices}_{i,r,t} * \text{PMG}_{i,r,t}\right)$ term remain negative and statistically significant; this finding further corroborates the idea that consumers are likely to exert an effective market disciplinary mechanism by engaging in bad ratings when firms use PMG to falsely signal low prices. It can be noticed that neither the inclusion of the $\text{PMG}_{i,r,t} * \text{Treatment\_Duration}_{i,r,t}$ interaction term without controls, nor the specification including the latter, change the direction and the magnitude of the treatment effect; this result from the weighting procedure suggests that products may not appear to significantly differ in terms of observables, and that the treatment effect is not driven by any particular item in our sample.

Thus far, our results have shown that PMG policies are associated with an increase in products' ratings, but only after a given period since the application of the latter. To better understand the dynamics of the effectiveness of the policy, we have disentangled the aggregate effect into daily effects. Specifically, for any day after the application of the PMG, we use the estimated coefficient of Table 2 (columns 4 and 8) to compute the following linear combination: $\phi + \lambda \cdot \text{Treatment\_Duration}_{i,r,t}$. The results are reported in Figure 1A, from which it is interesting to observe a statistically significant increase in products' ratings since the seventeenth day after the application of the policy: indeed, the effect statistically significantly increases from about 0.026 (S.E. = 0.02) to 0.12 (S.E. = 0.03). Similar results are observed after weighting observations according to the product popularity index (Fig. 1B), with the estimated coefficients being slightly smaller in magnitude.

These findings align with a signalling interpretation of PMGs (Srivastava & Lurie, 2004). PMGs act as observable claims about a retailer's pricing stance; when market discipline is strong, false signals are costly, so credible guarantees shape expectations. In our data, the negative coefficient on $\text{logPrices}_{i,r,t} * \text{PMG}_{i,r,t}$ indicates that consumers penalize price increases under a PMG, using ratings as a disciplining device. This is consistent with OCRs providing transparent, reputational feedback that rewards perceived value and punishes deviations from the "low-price" frame. Moreover, the

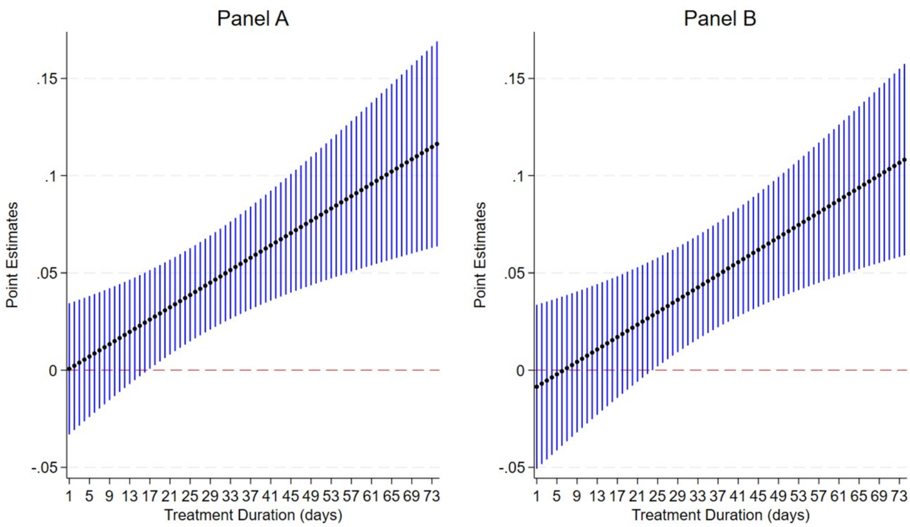

Fig. 1 Effect of PMGs on products' ratings according to the days since the application of the policy. Notes. This figure presents DiD estimates of the effect of the PMG policy on the level of products' ratings according to the number of days since the application of the policy. Estimates in **A** and **B** are based on the estimated coefficients of columns (4) and (8) of Table 2, respectively. The following linear combination, $\phi + \lambda \cdot \mathrm{Treatment\_Duration}_{\mathrm{i,r,t}}$, is applied. Ninety percent confidence intervals are reported. Huber–White heteroscedasticity-robust standard errors are computed

positive $\mathrm{PMG}_{\mathrm{i,r,t}} * \mathrm{Treatment\_Duration}_{\mathrm{i,r,t}}$ term suggests that perceptions become more favourable as the policy persists, consistent with trust accumulation. Taken together (Table 2, cols. 3–4), prices, PMGs, and duration interact in a manner predicted by signalling theory and broadly consistent with our Hypothesis 1: PMGs serve as credible price cues; when observed prices move against that cue, ratings fall, whereas sustained exposure strengthens the policy's credibility.

It is worth noting that one potential concern when estimating Eq. 2 is related to the linearity assumption, i.e., we have assumed that the effect of prices and PMGs is a linear function of the time span since the policy is applied. Hence, we relax this assumption by leveraging a more general specification which includes leads and lags à la Autor (2003) and that takes the following form:

$$Ratings_{i,r,t} = \sum_{\xi=-K}^{-1} \Gamma_{\xi}(PMG_{i,r,t+\xi}) + \sum_{\xi=0}^{M} \delta_{\xi}(PMG_{i,r,t+\xi}) + X_{i,r,t}^{T}\beta + \mu_{i,r} + \tau_{t} + \epsilon_{i,r,t} \tag{3}$$

Differently from Eq. 2, in the latter one, the K leads of the treatment indicator span from 5 to 1 day before the treatment and the Γ's represent pre-trends coefficients. The M lags of treatment variable span from the treatment day ($t=0$) to 15 days after the treatment and onwards; the δ's represent the DiD coefficients of interest, reflecting dynamic average treatment effects, i.e., this specification allows PMGs to have a differential effect over time. Moreover, fundamental for the validity of a DiD research design, this specification allows us to test the existence of a parallel trend between treated and control groups, since it contains different leads of the treatment. Indeed, a statistically significant coefficient for leads terms might be indicative of possible anticipatory effects, namely that the parallel trend assumption might be violated.

Figure 2 provides event study estimates of Eq. 3 and suggests that the parallel trend assumption required by DiD research designs is satisfied. In fact, pre-trends coefficients of the treatment K leads, spanning from 5 to 1 day before the application of the PMG, are individually and jointly not statistically different from zero, thus confirming the absence of any anticipatory effect and the validity of the research design. We also conduct an F test of leads' joint significance for both the specification without controls $[F(5;8791) = 1.26 -$ Prob $> F = 0.2775]$, and the one including controls $[F(5;8789) = 1.29 -$ Prob $> F = 0.2649]$, that highlight the absence of any anticipatory effects. Moreover, the estimated specification allows us to investigate the possibility that the effect of the treatment may speed up, stabilize, or mean revert over time, since it includes M lags of the treatment, ranging from the implementation day ($t = 0$) until 15 days later and onward. Estimated coefficients, graphically shown in Figure 2, suggest that PMGs exert a positive effect on products' ratings, starting 6 days after the treatment and lasting up to the ninth day, after which they become non-statistically different from zero. Comfortingly, if we replicate the above analysis after including controls, we find very similar results. Taken together, these findings confirm the results from the linear specification in Eq. 2: this suggests that imposing a linear structure does not lead to biased results.

In order to further investigate the studied issue, we perform additional analyses to verify if the impact of PMG and pricing policies is heterogeneous depending on products' prices at the beginning of the sample period. Indeed, the effectiveness of PMGs as a market disciplinary mechanism may be stronger for cheaper items compared to expensive ones for several reasons. Firstly, consumers tend to be more price-sensitive when purchasing cheaper items, making them more responsive to price signals such as PMGs. Additionally, cheaper

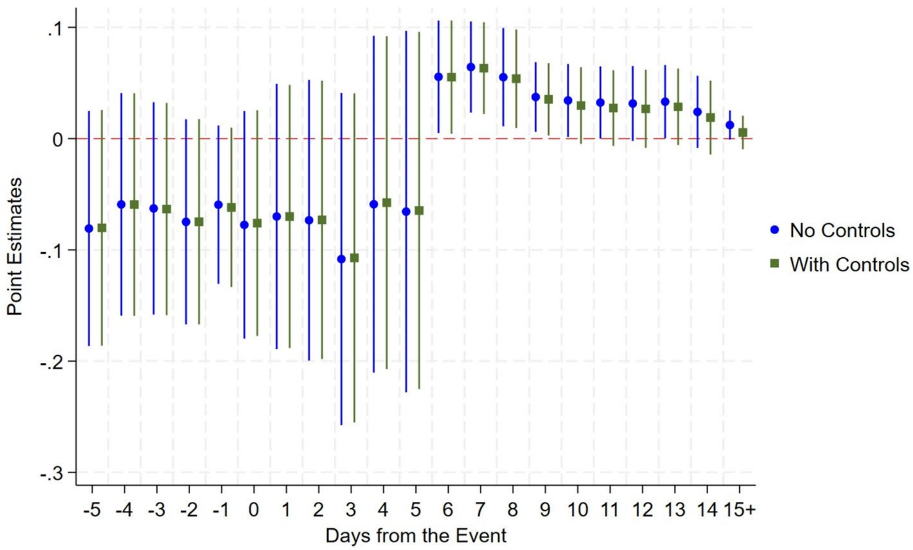

Fig. 2 Event study representation of the effect of PMGs on products' ratings. Notes. Event study estimates of Eq. 3. All specifications include product and time fixed effects. Controls include the absolute and the relative number of reviews, as well as products' age. Ninety percent confidence intervals are reported. Huber–White heteroscedasticity-robust standard errors are computed

items often represent smaller financial investments for consumers, leading them to prioritize obtaining the best value for their money. In highly competitive markets, sellers may use PMGs to differentiate themselves and attract customers, particularly for cheaper items facing greater competition. Moreover, the higher sales volume associated with cheaper items means that even small improvements in pricing strategies, such as the implementation of PMGs, can have a significant impact on overall revenue. Lastly, consumers may perceive the risk associated with purchasing cheaper items differently, being more inclined to take a chance on a cheaper item if they believe they are getting a good deal with a PMG.

Table 3 presents the regression results for an analysis investigating those possible heterogeneous effects. Across all specifications, the estimated coefficients associated with products' prices are not different from zero, suggesting the absence of a direct impact of pricing policies on ratings. Conversely, and differently from results in Table 2, PMGs have a positive and statistically significant impact on product ratings, with estimated coefficients ranging from 0.28 (S.E. = 0.09) to 0.037 (S.E. = 0.02) with statistical significance between 1% and 5%. The $\left(\mathrm{logPrices}_{i,r,t} * \mathrm{PMG}_{i,r,t}\right)$ interaction term, once again, assesses whether the impact of prices on ratings differs between products subject to the PMG policy and those that are not. Interestingly, it is worth noting that, when the PMG policy is applied $\left(\mathrm{PMG}_{i,r,t} = 1\right)$, negative and statistically significant coefficients are associated with cheaper products, suggesting that an increase in prices is likely to generate a decrease in cheaper products' ratings; moreover, estimated coefficients are slightly higher in magnitude with respect to those in Table 1 and statistically significant at the 1% level. It should be noted that expensive items are not affected by neither pricing policies nor the PMG policies: moreover, also, $\left(\mathrm{logPrices}_{i,r,t} * \mathrm{PMG}_{i,r,t}\right)$ and $\mathrm{PMG}_{i,r,t} * \mathrm{Treatment\_Duration}_{i,r,t}$ interaction terms' coefficients are statistically not different from zero. Lastly, we examine whether the duration of the PMG policy implementation affects the relationship between prices and ratings. Again, statistically significant positive coefficients are associated to cheaper products, while the expensive ones are not affected; this suggests that the effect of PMGs on ratings is amplified over time for cheaper products subject to the policy, differently from expensive items. Specifically, when we include the interaction term, $\mathrm{PMG}_{i,r,t} * \mathrm{Treatment\_Duration}_{i,r,t}$, the estimated coefficient related to cheaper products turns out to be positive and statistically significant, both in the specification without controls and in that including controls (0.00219, columns 7 and 8, respectively).

In a similar vein, we also explore whether the impact of PMGs and pricing is different on products that require different search efforts at the beginning of the sample period. Indeed, if PMGs serve as signals of low store prices (conveying valuable information about a retailer's pricing strategy and commitment to providing competitive prices), when applied to products that are relegated to secondary pages of search engines, those requiring higher search effort, these guarantees can become even more potent as market discipline mechanisms for several reasons. Firstly, products featured on secondary pages typically receive less visibility and traffic compared to those on primary pages. Consumers navigating to secondary pages may exhibit higher price sensitivity, actively seeking cost-effective options. In this scenario, the presence of a PMG policy signals to consumers that despite the product's placement on a less prominent page, the retailer is committed to offering competitive prices. Secondly, consumers exploring secondary pages may be more inclined to conduct price comparisons across multiple retailers. The presence of a PMG encourages consumers to perceive the retailer as transparent and confident in its pricing competitiveness. This perception fosters trust and loyalty among consumers, even for products with lower visibility. Thirdly, consumers navigating to secondary pages may be more discerning and deliberate in their purchasing decisions, often motivated by specific product attributes

Table 3 Heterogeneous results: cheaper/expensive products

Dep. Var.: products' ratings	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Expensive products				Cheaper products			
$logPrices_{i,r,t}$	−0.02610	0.00706	−0.02591	−0.02591	−0.01496	−0.00427	−0.01579	−0.01579
	(0.02923)	(0.02852)	(0.02925)	(0.02925)	(0.01881)	(0.01959)	(0.01876)	(0.01876)
$PMG_{i,r,t}$	0.03176**	0.02963**	0.03700**	0.03700**	0.30957***	0.29211***	0.28043***	0.28043***
	(0.01447)	(0.01447)	(0.01652)	(0.01652)	(0.10394)	(0.09647)	(0.09418)	(0.09418)
$logPrices_{i,r,t} * PMG_{i,r,t}$	0.00018	−0.00182	−0.00041	−0.00041	−0.07693***	−0.07291***	−0.07593***	−0.07593***
	(0.00203)	(0.00207)	(0.00223)	(0.00223)	(0.02853)	(0.02683)	(0.02814)	(0.02814)
$PMG_{i,r,t} * Treatment\_Duration_{i,r,t}$			−0.00011	−0.00011			0.00219**	0.00219**
			(0.00010)	(0.00010)			(0.00085)	(0.00085)
Observations	2606	2606	2606	2606	6422	6422	6422	6422
Controls	×	✓	×	✓	×	✓	×	✓
Product FE	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓		✓
R^2	.99220	.99295	.99220	.99220	.81804	.81920	.81820	.81820

Notes: This table presents DiD estimates of Eq. 1 (columns 1, 2, 5, 6) and Eq. 2 (columns 3, 4, 7, 8) on the heterogeneous effect of PMGs on products' ratings, based on products' initial prices. $Ratings_{i,r,t}$ is the number of stars gained by product *i* on retailing website *r* at time *t*. $Prices_{i,r,t}$ represents product *i*'s price (natural logarithm) in day *t* for retailer *r*. $PMG_{i,r,t}$ is a dummy variable equal to 1 if items are subject to the PMG in day *t*. $Treatment\_Duration_{i,r,t}$ captures the number of days since the application of the PMG. All specifications include product and time fixed effects. Controls include the absolute and the relative number of reviews, as well as products' age. Cheaper (expensive) products are those with initial prices below (above) the average value. All specifications are estimated by OLS. Huber–White heteroscedasticity-robust standard errors are shown in parentheses: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively or price considerations. The presence of a PMG on these pages reassures consumers that they are making informed choices and receiving the best possible value for their money. Moreover, in the competitive landscape of e-commerce, where consumers have access to a plethora of options and information, retailers must differentiate themselves to stand out. By offering PMGs, especially for products on secondary pages, retailers signal their commitment to customer satisfaction and competitiveness, thereby enhancing their brand reputation and attracting discerning consumers.

Table 4 shows the results of aforementioned analysis. PMGs policies are confirmed to exert a positive and statistically significant impact on products' ratings across almost all specifications; however, the effect is noticeably greater for less visible items, with point estimates in the range 0.17–0.26, depending on whether we consider the specification without or with controls (columns 5 and 6, respectively). It is worth noting that when we include the duration interaction term (point estimates of Eq. 2 in columns 3, 4, 7, 8), i.e., $\mathrm{PMG}_{i,r,t} * \mathrm{Treatment\_Duration}_{i,r,t}$, the PMG policy has a smaller effect on product ratings as the duration increases for more visible products, whereas the opposite holds true for less visible ones. This pattern suggests that the visibility of products plays a crucial role in shaping consumer responses to pricing policies like PMGs. More visible products may already enjoy a higher level of consumer awareness and trust, making them less sensitive to changes in pricing policies over time. In contrast, less visible products may rely more heavily on pricing signals, such as PMGs, to attract consumer attention and compete effectively. Therefore, the diminishing effect of the PMG policy on ratings for more visible products could be attributed to factors such as consumer familiarity, brand reputation, and trust, which mitigate the influence of pricing policies over time. Conversely, the increasing effect of the policy on ratings for less visible products suggests that pricing signals become more influential as consumers actively seek cost-effective options, especially for products with lower visibility.

A further aspect to be considered in the heterogeneity analysis based on online visibility provided in Table 4 refers to the fact that the estimates of the $\left(\mathrm{logPrices}_{i,r,t} * \mathrm{PMG}_{i,r,t}\right)$ term are negative and statistically significant in all specifications pertaining to the sample of less visible products, as opposed to those referring to the more visible products one. In particular, the coefficient on the interaction term is negative and often highly statistically significant across all specifications (columns 5–8), indicating that the PMG policy amplifies the negative effect of price increases on ratings for less visible items. Indeed, if we assume that the presence of a PMG policy can signal to consumers the retailer's commitment to providing competitive prices, the effectiveness of this signal may vary depending on the visibility of the product in online search results. For more visible products, consumers may already have higher expectations regarding pricing competitiveness due to their prominence in search results. Thus, the presence of a PMG policy may serve as reinforcement of the retailer's commitment to competitive pricing, mitigating the negative impact of higher prices on consumer ratings. Conversely, for less visible items, consumers may have lower initial expectations regarding pricing competitiveness. In this case, the presence of the policy may be a more salient signal of the retailer's pricing strategy, amplifying the negative impact of increasing prices on consumer ratings. This aligns with the findings of our analysis, where the interaction between prices and the PMG policy has a more pronounced effect on ratings for less visible products.

In addition, we subject the main results to a comprehensive battery of robustness checks, whose detailed results are shown in Appendix B. First, we estimate the more extended specifications of our models over both the full sample and the sub-samples of cheaper products and less visible ones, after introducing *fake* treatment timings; comfortingly, as

Table 4 Heterogeneous results: more/less visible products

Dep. Var.: products' ratings	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	More visible products				Less visible products			
$\text{logPrices}_{i,r,t}$	−0.01211	−0.00529	−0.01349	−0.01349	−0.01370	0.02348	−0.01191	−0.01191
	(0.03383)	(0.03333)	(0.03392)	(0.03392)	(0.01450)	(0.01998)	(0.01476)	(0.01476)
$\text{PMG}_{i,r,t}$	0.04860*	0.04076	0.07418**	0.07418**	0.17310***	0.26235*	0.17483**	0.17483**
	(0.02857)	(0.02872)	(0.03423)	(0.03423)	(0.06390)	(0.15758)	(0.06805)	(0.06805)
$\text{logPrices}_{i,r,t} * \text{PMG}_{i,r,t}$	−0.00325	0.00260	−0.00496	−0.00496	−0.06257***	−0.06311*	−0.07583***	−0.07583***
	(0.00412)	(0.00413)	(0.00444)	(0.00444)	(0.02077)	(0.03490)	(0.02412)	(0.02412)
$\text{PMG}_{i,r,t} * \text{Treatment\_Duration}_{i,r,t}$			−0.00130***	−0.00130***			0.00529***	0.00529***
			(0.00036)	(0.00036)			(0.00135)	(0.00135)
Observations	4405	4405	4405	4405	4623	4623	4623	4623
Controls	×	✓	×	✓	×	✓	×	✓
Product FE	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓
R^2	.86941	.86978	.86961	.86961	.86817	.89716	.86879	.86879

Notes: This table presents DiD estimates of Eq. 1 (columns 1, 2, 5, 6) and Eq. 2 (columns 3, 4, 7, 8) on the heterogeneous effect of PMGs on products' ratings, based on products' initial online visibility. $\text{Ratings}_{i,r,t}$ is the number of stars gained by product i on retailing website r at time t. $\text{Prices}_{i,r,t}$ represents product i's price (natural logarithm) in day t for retailer r. $\text{PMG}_{i,r,t}$ is a dummy variable equal to 1 if items are subject to the PMG in day t. $\text{Treatment\_Duration}_{i,r,t}$ captures the number of days since the application of the PMG. All specifications include product and time fixed effects. Controls include the absolute and the relative number of reviews, as well as products' age. More (less) visible products are those with an initial search rank below (above) the median value. All specifications are estimated by OLS. Huber–White heteroscedasticity-robust standard errors are shown in parentheses: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively one expects, we do not observe any significant effect of the latter on products prices. Second, we re-estimate both Eqs. 1 and 2 after substituting the dependent variable with a *fake* one, in which products' *fake* ratings are drawn from product specific random distributions resembling sampling ones (same mean and variance). Once again, we do not observe any significant effect of NewEgg's PMGs on *fake* products' ratings. Third, we provide a random allocation test, in which we build a placebo treatment variable taking on value 1 if random numbers drawn from a uniform distribution [0,1] are greater than the sampling treatment probability. We then estimate the most extended specification of Eq. 1, leveraging on this new fake treatment indicator, and iterating such procedure a thousand times, in order to obtain a distribution of placebo average treatment effects to compare with the estimated average values observed in column (2) of Table 2, namely $\mathrm{PMG}_{i,r,t} = 0.01391(\mathrm{SE} = 0.02)$ and $\mathrm{logPrices}_{i,r,t} * \mathrm{PMG}_{i,r,t} = -0.00796(\mathrm{S.E.} = 0.004)$. Indeed, a statistically significant average treatment effect should be different with respect to those obtained with placebo estimates; comfortingly, this is the case.

Last but not least, as a complementary outcome, we test robustness using the number of "good reviews," defined as total reviews multiplied by the share of 4–5-star ratings. While star ratings provide an immediate, granular measure of satisfaction, the "good reviews" metric conflates quantity and positivity, adjusts more slowly to recent sentiment, and can be dominated by high-volume products. Using a DiD design analogous to the baseline, three results emerge (detailed estimates and discussion are provided in Appendix B). First, higher prices are associated with fewer good reviews (large, precise negative effects). Secondly, PMG exposure increases good reviews. Thirdly, the price–PMG interaction is negative and highly significant, indicating that price rises under a PMG depress positive review volume. These patterns are robust to controls and popularity weighting. Unlike star ratings, however, the PMG–duration interaction is small and imprecise, suggesting that ratings better capture the gradual accumulation of perceived value during longer PMG spells.

Other Threats to Identification

A potential concern with relying exclusively on star ratings is that they compress rich consumer reactions into a single number and may therefore obscure the behavioural channel through which pricing—particularly under a low-price framing—affects feedback. To illuminate the pathway, we complement the ratings-based estimates with a systematic analysis of the *text* of consumer reviews from our study sample. We collect 712 review texts from 2018 (Amazon: 370; NewEgg: 342) and construct interpretable indicators of price salience and price-related valence.

We pre-processed each review by lower-casing, removing obvious boilerplate and display artefacts (URLs, HTML remnants, repeated punctuation), and normalizing whitespaces. Using a curated dictionary tailored to consumer-electronics discourse, we flagged the following: (*i*) *price/value salience* (tokens and collocations such as "price," "pricing," "price point," "value," "worth"), (*ii*) *positive price/value valence* (e.g., "great/good/fair price," "worth the price," "value for money"), (*iii*) *negative price/value valence* (e.g., "overpriced," "too expensive," "not worth the money," "price gouging"), (*iv*) *explicit low-price policy language* ("price match," "price matching," "price guarantee," "refund the difference"), and (*v*) *direct price-increase phr*asing ("price went up/jacked up/raised/hiked").[4] It is worth noting that our approach is deliberately conservative: the lexicon emphasizes

[4] For transparency, the full dictionary can be provided upon request.

high-precision phrases commonly observed in retail contexts. This minimizes false positives at the cost of potentially understating diffuse, metaphorical complaints. For interpretability, we also recorded whether a review that mentions price/value simultaneously references other dissatisfaction categories, notably *customer service* (e.g., support, RMA, return, refund, “seller responsiveness”) and *product quality* (e.g., defective, failed, broke, flimsy). Finally, we examined frequent local collocations around “price” (e.g., bigrams such as “great price,” “not worth”) to gauge the typical semantic framing.

Two robust facts emerge, as shown in Table 5. First, price/value is salient: 30.9% of all reviews contain price/value language (Amazon: 27.8%; NewEgg: 34.2%). Within the full corpus, 12.4% of reviews express positive price/value judgements and 3.4% express negative price/value judgements. Positive price/value signals are more frequent on NewEgg (17.0%) than Amazon (8.1%), while negative price/value is similar across platforms (about 3–3.5%). Secondly, direct policy language is virtually absent: only one review (0.1%) explicitly mentions a price guarantee or price matching, and we observe essentially no instances of seller-directed “price hike” phrasing. Among the subset of reviews that criticize price/value, co-mentions frequently involve other sources of dissatisfaction: approximately 42% also reference customer service issues and 38% mention product-quality concerns. This pattern is consistent with multi-attribute disappointment rather than isolated “price policing” or technical enforcement of guarantee terms. Frequent collocations around “price” include “great price,” “good price,” “price point,” and “worth the price,” indicating that reviewers typically frame their assessment in terms of perceived deal quality.

These textual regularities dovetail with the core econometric result that the interaction between the low-price framing and price increases (PMG $\times\ \Delta logp$) depresses ratings. The review texts indicate that consumers routinely evaluate purchases through a value lens (“worth the price”) but rarely invoke PMG policy terms verbatim. This is exactly what one would expect if the policy primarily shifts expectations about the appropriate price level—i.e., it establishes a “lowest-price” reference—so that a subsequent increase in price deteriorates perceived deal quality. Ratings, in turn, embed that deterioration even when reviewers do not “name the policy.” The co-mention structure reinforces this interpretation: when value is judged to be poor, consumers often also emphasize service or quality frictions, suggesting that a disappointing price/value proposition can amplify other negatives, leading to a broader penalty in the overall rating. In short, the mechanism appears to be a value-perception channel rather than explicit textual enforcement of PMG terms.

Table 5 Textual evidence on price/value and PMG mentions in 2018 reviews

	All	Amazon	NewEgg
Number of reviews	712	370	342
Price/value mentioned (%)	30.90	27.80	34.20
Positive price/value (%)	12.40	8.10	17.00
Negative price/value (%)	3.40	3.20	3.50
Explicit PMG mention (%)	0.10	0.00	0.30

Notes: Shares computed over the full set of review texts (2018). “Price/value mentioned” flags any occurrence of price/value terms; “Positive” and “Negative” subsets reflect unambiguous valence phrases. “Explicit PMG mention” includes “price match/matching” and “price guarantee” terms. See text for co-mention patterns with customer service and product quality

The textual evidence also alleviates two natural concerns. First, if the ratings effect were driven by unobserved product shocks unrelated to pricing (e.g., sudden quality defects), we would not expect to see systematic price/value language accompanying negative assessments. Instead, we do observe coherent value-related phrasing when sentiment is negative. Secondly, if the effect hinged on reviewers explicitly "policing" policy conditions, we would expect references to guarantees or "price hikes" to feature more prominently; they do not. Rather than undermining the causal interpretation, this rarity is informative: the policy works through expectation setting and reference points, not rule enforcement in prose.

To further align the text with the identification design, one can recast the textual flags as outcomes in the same DiD framework used for ratings. In particular, we link review texts to the panel at the product–week level. For Amazon, we extract the ASIN from the product URL in the panel (*/dp/ASIN*) and match it to the review file's *id_product*. For NewEgg, we extract the item code from the panel URL (*Item = N82E…*) and match it to the review file's Product ID. Review dates are parsed and mapped to ISO weeks (2018, weeks 19–44). For each product–provider–week, we construct the following binary flags (equal to one if any review in that cell contains the relevant language): (*i*) *price/value salience* (*price_any*: mentions of "price," "value," "worth," "deal"), (*ii*) *negative price/value* (*price_neg*: "overpriced," "too expensive," "not worth the money," "rip-off," "poor value"), and (*iii*) *positive price/value* (*price_pos*: "great/good/fair price," "worth the price," "value for money"). We also flag explicit "price guarantee/matching" phrases and "price hike" wording; as in the descriptive evidence, these are exceedingly rare and not used as outcomes. We restrict the analysis sample to product–week cells with at least one review to ensure the outcomes are observed by construction. The final sample comprises 148 product–week cells spanning 19 distinct products and 26 weeks; variation in the policy indicator occurs on NewEgg in this window, while the Amazon policy flag is zero throughout.

Let $\mathrm{price\_flag}^{(k)}_{\mathrm{i,w,r}}$ denote one of the text indicators ($k \in \{\mathrm{any}, \mathrm{neg}, \mathrm{pos}\}$) for product i in week w for retailer r. We estimate the following TWFE model:

$$\begin{aligned} price\_flag^{(k)}_{i,w,r} = \alpha_i + \tau_w + \beta_k (PMG\_share_{i,w,r} \times \Delta logp_{i,w,r}) \\ + \gamma_k PMG\_share_{i,w,r} + \delta_k \Delta logp_{i,w,r} + \varepsilon_{i,w,r} \end{aligned} \tag{4}$$

where $\mathrm{PMG\_share}_{\mathrm{i,w,r}}$ is the fraction of days in week w when the policy is active for product i and $\Delta logp_{i,w,r}$ is the net within-week log price change (sum of daily first differences, i.e., $\log p_{\mathrm{i,w,r,end}} - \log p_{\mathrm{i,w,r,start}}$). We also include product fixed effects (α_{i}) and week fixed effects $\left(\tau_{\mathrm{w}}\right)$. This mirrors the main design but uses text-as-data outcomes.

Table 6 examines whether the low-price framing amplifies price/value discourse in review texts when prices move. Three findings stand out.

First, column (1) shows a precisely estimated and economically large effect on salience: the interaction coefficient on $\left(PMG\_share_{i,w,r} \times \Delta logp_{i,w,r}\right)$ is positive and statistically significant at the 1% level. Quantitatively, when the policy is active throughout the week $\left(\mathrm{PMG}_{\mathrm{share}_{\mathrm{i,w,r}}} = 1\right)$, a 5% (10%) price change is associated with an increase of roughly $2.341 \times 0.05 \approx 11.7$ p.p. ($\approx$23.4 p.p.) in the probability that any review mentions price or value. This result dovetails with the idea that the policy raises the salience of the "deal" dimension: reviews are more likely to discuss value precisely when prices move under the low-price framing. Secondly, column (3) indicates a marginally significant ($p < .10$) rise in positive price/value language under the same interaction

Table 6 Text-as-data outcomes: price/value discourse in review texts (TWFE)

Dependent variables	(1) Price/value mentioned	(2) Price/value negative	(3) Price/value positive
$\Delta logPrice_{i,w,r}$	−0.487	0.064	−0.372*
	(0.547)	(0.088)	(0.227)
$PMG\_share_{i,w,r}$	−0.162	0.024	0.096
	(0.254)	(0.045)	(0.139)
$\Delta logPrice_{i,w,r} \times PMG\_share_{i,w,r}$	2.341***	0.458	0.855*
	(0.759)	(0.426)	(0.448)
Observations	148	148	148
Controls	×	×	×
Product FE	✓	✓	✓
Time (week) FE	✓	✓	✓
Weighted by # reviews	✓		✓

Notes: This table presents estimated results of Eq. 4. The dependent variables are binary indicators equal to 1 if any review in product i and week w contains the relevant language: (1) any price/value mention (*price_any*); (2) explicitly negative price/value phrasing (*price_neg*); (3) explicitly positive price/value phrasing (*price_pos*). $\Delta logPrice_{i,w,r}$ is the net within-week log price change (end-of-week minus start-of-week). $\text{PMG\_share}_{i,w,r}$ is the fraction of days in week w with the low-price policy active. Estimates are weighted by the number of reviews observed in each (i,w) cell; the sample is restricted to cells with at least one review. All specifications are estimated by OLS. Huber–White heteroscedasticity-robust standard errors are shown in parentheses: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively

(coefficient 0.855). The main effect of $\Delta logp_{i,w,r}$ is negative and weakly significant, implying that, absent the policy, price increases reduce positive price talk. The positive interaction therefore suggests that the framing attenuates (and may even overturn) this negative slope, consistent with the policy re-setting expectations and prompting reviewers to frame contemporaneous price movements in "good deal/fair price" terms. Thirdly, column (2) shows that explicitly negative price/value phrasing (overpriced/not worth, etc.) is rare at the product–week frequency, and the corresponding interaction is imprecisely estimated, which is in line with the descriptive evidence that overtly negative price language is uncommon in consumer electronics reviews.

Two implications follow. On mechanism, the strong salience response in column (1) provides direct textual corroboration for the value-perception channel underpinning the ratings results: when prices change under a low-price frame, reviewers are substantially more likely to talk about price/value, even if they do not explicitly name the policy. On interpretation, the suggestive pattern in column (3) is consistent with compositional effects within PMG weeks (e.g., promotional dynamics) and with expectation management: the policy softens the otherwise negative association between price rises and positive price talk, indicating that value judgements are made relative to a policy-induced reference point. Overall, the text-as-data outcomes reinforce the interpretation that the policy operates by shifting and sharpening consumers' value expectations; when observed prices depart from those expectations, the language of reviews pivots towards price/value, and this linguistic shift is mirrored by the numerical rating penalties documented elsewhere in the paper.

However, some caveats merit emphasis. First, dictionary methods privilege transparency and replicability but necessarily undercount nuance and irony. In practice, the estimated shares should be interpreted as lower bounds on price/value discourse. Second, textual mentions of "price" are not one-to-one with instantaneous price changes; rather, they capture consumers' perceived deal quality at the time of writing, which is precisely the construct our interpretation invokes. These caveats notwithstanding, the consistency between the language of value in reviews and the sign and heterogeneity of our ratings estimates (stronger effects for cheaper/less-visible items) is difficult to reconcile with alternative stories centred solely on unobserved quality shocks or transient platform quirks.

Overall, the review texts show that consumers frequently talk about value but rarely cite guarantee terms or literal price hikes. This empirical pattern is precisely what a reference-point account predicts: the low-price framing sets expectations; when observed prices move against that frame, perceived value deteriorates and overall ratings fall. The textual evidence therefore complements the causal estimates by identifying a concrete behavioural channel—value-perception—through which $PMG\_share_{i,w,r} \times \Delta logp_{i,w,r}$ translates into lower star ratings.

Another natural—and "first-order"—question is whether prices are actually lower when a PMG is in place. Leveraging the same our dataset, Bottasso et al. (2025) show that PMG activation does not coincide with lower posted prices in this market; if anything, the adopter's posted prices rise during the validity window (and briefly after), while the main non-adopting competitor is essentially unchanged, with effects concentrated among highly-rated, visible, and expensive products. In this paper, we therefore focus on the "second-order" margin: how consumers react, in ratings and text, when a low-price frame is present and prices move. To connect these two margins, we additionally examine whether PMG episodes are associated with large day-to-day price jumps or instead with small adjustments that can nevertheless matter for perceived value and review behaviour. Addressing this clarifies how pricing behaviour under the policy relates to our ratings-based effects and helps reconcile those results with the text-as-data evidence that price/value discourse becomes more salient precisely when prices move under PMG (Table 6). If daily price changes are small on average—especially for cheaper or hardly found items—then the ratings penalty we estimate under $PMG\_share_{i,w,r} \times \Delta logp_{i,w,r}$ should be interpreted primarily through expectation and value-perception channels rather than large mechanical price hikes.

To this end, for each product i sold by provider r on day t, we compute the within-product–provider daily change in log price, i.e., $\Delta logp_{i,r,t} \equiv logp_{i,r,t} - logp_{i,r,t-1}$. Product types, as in Section "Identification Approach," are defined pre-treatment using initial conditions at the start of the sample: cheaper (expensive) items have an initial price below (above) the cross-product mean; easily found (hardly found) ones have an initial search rank strictly below 3 (respectively, ≥ 3). We then estimate a TWFE DiD regression of daily $\Delta logp_{i,r,t}$ on PMG interacted with the group indicators, including product fixed effects and calendar-week fixed effects, with Huber–White standard errors clustered at the product level.

Three conclusions emerge from results in Table 7. First, on average, the policy does not generate systematic day-to-day price increases: the coefficient on PMG is small and imprecise in both columns. Secondly, we do not detect a distinct day-to-day price escalation in the cheaper segment: the interaction PMG × Cheaper is positive but statistically indistinguishable from zero (column 1). Thirdly, there is weak evidence that easily found items experience slightly larger positive daily movements under the policy (interaction PMG × Easily Found $\approx$ 0.010, significant at the 10% level; column 2), but magnitudes are economically small in absolute terms.

These pricing patterns are consistent with, and help interpret, the rest of the paper. The ratings-based estimates show that $PMG \times \Delta logprices$ depresses consumer evaluations; the text-as-data results further indicate that, when prices move under PMG, review

Table 7 Policy and daily price changes: cost and visibility heterogeneity

Dependent variable: daily $\Delta logp$	(1) Cost heterogeneity	(2) Visibility heterogeneity
$PMG_{i,r,t}$	0.0004	0.0028
	(0.0023)	(0.0038)
$PMG_{i,r,t} * Cheaper_i$	0.0056	
	(0.0042)	
$PMG_{i,r,t} * Easily\ Found_i$		0.0103*
		(0.0058)
Product FE	✓	✓
Week FE	✓	✓
Observations	8970	8970

Notes: In this table, for each product i sold by provider r on day t, we compute the within-product–provider daily change in log price, i.e., $\Delta logp_{i,r,t} \equiv logp_{i,r,t} - logp_{i,r,t-1}$. Product types, as in Section "Identification Approach," are defined pre-treatment using initial conditions at the start of the sample: cheaper (expensive) items have an initial price below (above) the cross-product mean; easily found (hardly found) ones have an initial search rank strictly below 3 (respectively, ≥ 3). We then estimate a TWFE DiD regression of daily $\Delta logp$ on PMG interacted with the group indicators, including product fixed effects and calendar week fixed effects, with Huber–White standard errors clustered at the product level: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively

language pivots strongly towards price/value salience (Table 6), with explicit negative price wording being rare. The daily pricing evidence here shows that any mechanical price increases under the policy are small on average—especially for cheaper or hardly found items. Taken together, the results support a reference-point/value-perception mechanism: the policy sets a low-price expectation, and even modest price movements against that frame increase the salience of value in review texts and translate into lower star ratings, without requiring large systematic price hikes in the relevant segments.

It is worth noting that, if claiming frictions ("hassle costs") associated with a low-price guarantee are salient to consumers, the perceived net value of the policy should diminish as those frictions rise. In that case, the adverse ratings response we document for price increases under a PMG could be stronger precisely when hassle is salient. To test this idea, we extend the text-as-data pipeline by building a transparent dictionary of *hassle complaints* (e.g., "price match refused/denied," "not honour the price match," "fine print," "paperwork," "proof/documentation," "claim process/form/denied," "refund not issued," "no response," "customer service wouldn't help"). Reviews are mapped to product–provider–week cells via platform identifiers (ASIN for Amazon; item code for NewEgg) and aggregated to construct (*i*) a binary indicator that any review in the cell mentions hassle and (*ii*) the share of reviews with such terms. We take the binary indicator as our primary proxy for high observed hassle salience. We then estimate a two-way fixed-effects model (product and calendar-week) for two outcomes: (a) whether any review in the cell contains price/value language (salience) and (b) the weekly average star rating. Specifications include the main effects of the PMG exposure share, within-week price change, and hassle salience, their baseline interaction between PMG exposure and price change, and an additional term that allows this interaction to differ in cells flagged as high hassle.

The added moderation term, namely the "triple interaction" capturing whether the PMG–price-change effect is stronger when hassle is salient, is statistically indistinguishable from zero in both the salience and the ratings models. By contrast, the PMG–price-change interaction remains economically meaningful and precisely estimated for the text salience outcome and retains the same sign and order of magnitude for ratings as in the main design. Taken together, this implies that, within our product–provider–week cells, the marginal impact of price movements under a PMG does not vary systematically with overt hassle language in reviews. This supports the interpretation that the primary channel is value-perception and reference-point setting under a low-price frame, rather than explicit documentation of claiming frictions in review text. We note, however, that the rarity of explicit hassle wording and possible measurement error in the dictionary proxy can attenuate moderation effects; still, the confidence intervals do not suggest large amplifications.

As additional scope checks, we perform the following: (*i*) a dynamic event study around the first PMG activation shows flat pre-trends for both ratings and price/value salience and post-activation paths consistent with the main estimates; (*ii*) placebo models for non-price topics (quality, customer service, delivery) show no systematic response to the PMG–price-change term, indicating topic specificity rather than a generic sentiment shift; (*iii*) the ratings penalty is larger when the cross-retailer "Refund Gap" (revealed price differential) is high, aligning with a reference-point/value mechanism. Results are robust to splitting by platform (Amazon vs. NewEgg), employing two-way clustered standard errors, and winsorising price changes.[5]

Conclusions

In our study, we explore the dynamics surrounding price-matching guarantees (PMGs) and consumer behaviour within the realm of online retail. Through our empirical analysis, we provide insights into the efficacy of PMGs as market signals and their implications for consumer welfare. To do so, we focus on the consumer electronics retail market and we rely on a Difference-in-Differences (DiD) research design in which products sold by NewEgg, a leading US online player for this market, are considered treated when targeted by its PMG policy, while the same titles sold on Amazon US, which never offers price-matching guarantees, act as the control group.

Our findings reveal that, while these policies alone may not exert a direct impact on consumer ratings, they wield significant influence over consumer responses to price fluctuations during PMG periods. Of particular note is our observation that price increases during PMG periods precipitate declining ratings over time, indicative of consumer perceptions of pricing deviations during such periods as misleading or unfavourable. Moreover, our study illuminates the differential impact of PMGs across various product categories, emphasizing their heightened effectiveness for cheaper and less visible items. This observation suggests that PMGs may serve as potent signalling mechanisms, especially in markets characterized by heightened price sensitivity or products facing visibility challenges, carrying profound

[5] Full coefficient tables and robustness details are available from the authors upon request.

implications for market participants and highlighting the need for tailored pricing strategies that account for product characteristics and market dynamics.

In addition, we merge Amazon and NewEgg review texts to the product–provider–week panel using platform identifiers (ASIN/item code) and construct transparent dictionary flags for (*i*) any price/value language and its (*ii*) explicitly positive and (*iii*) explicitly negative variants. We estimate a model in which the key regressor is the interaction between the within-week log price change and the share of days the PMG is active. The likelihood that reviews mention price/value rises sharply when prices move under PMG; explicitly positive price/value wording increases marginally, whereas explicitly negative price/value wording is rare and imprecisely estimated. Event-study diagnostics show flat pre-trends, and placebo topics (quality, service, delivery) indicate that the effect is topic-specific rather than a generic shift in sentiment. Overall, the text evidence supports an expectations/reference-point mechanism: under a "low-price" frame, even modest price increases heighten value-for-money salience and coincide with lower ratings.

The ratings-based results and the text-as-data analysis are mutually consistent. When prices rise under an active PMG, star evaluations decline while references to price/value in reviews increase markedly; by contrast, placebo topics (quality, service, delivery) remain flat. This concordance supports a disciplining-signal mechanism: PMGs do little in levels, but once prices move, consumers perceive a value shortfall and sanction the seller reputationally. Effects concentrate on cheaper and less visible products, where price sensitivity is higher and informational salience lower. Small average shifts in sentiment polarity are compatible with reviews that discuss price/value in a relatively neutral tone yet still translate into lower ratings.

Therefore, our findings align closely with the signalling interpretation of PMGs, as stated in Hypothesis 1 described in the "Introduction": The policy acts as a low-price frame, so even modest price movements under a PMG shift review discourse towards price/value and depress ratings, while average daily price changes remain small (consistent with Moorthy & Winter, 2006). At the same time, PMGs need not be purely benign. Robbiano's (2025) model for contemporary, algorithmic markets clarifies how frictions and heterogeneity can turn PMGs into a screening device, with implications for distribution and competition, and how process remedies could mitigate such outcomes. Taken together, the results indicate that in our setting the expectations/reference-point channel dominates, whereas overt discrimination or coordination mechanisms are less salient in the observed period. We view this theoretical integration as complementary to the empirical contribution: it sharpens why PMGs are interpreted as credible signals here, while highlighting contexts—greater frictions, stronger heterogeneity, or different competitive conditions—where alternative mechanisms may prevail.

From an antitrust perspective, our findings raise different considerations regarding the competitive implications of PMGs in digital markets. While PMGs can foster pricing transparency and consumer welfare, there is a pressing need to guard against potential anticompetitive behaviour or consumer harm arising from misleading pricing signals. Policymakers face the formidable task of designing regulatory frameworks that strike a delicate balance between fostering market competition, safeguarding consumer interests, and promoting innovation in digital marketplaces.

A further consumer-protection angle parallels reference-price ("sale") regulation. In that context, regulators often require that a reference price be genuine (e.g., a meaningful share of sales at the reference price) to prevent artificially inflated anchors. A similar logic could apply to low-price guarantees: a policy that is advertised as a "lowest-price" commitment may be more credible—and less likely to mislead—if it is accompanied by minimum disclosure standards. In practice, one could imagine requiring firms to disclose redemption statistics (e.g., fraction of transactions with refunds; average refund amounts) or to meet a minimum refund share to continue advertising a PMG label. Although we cannot observe redemptions in our data, existing evidence suggests that redemption can be economically non-trivial, so disclosure-based rules could be practically meaningful. For instance, reported redemption rates in retail settings span a wide range (roughly 1–25%), with rates above about 10% often viewed as consistent with the assumptions underpinning several PMG models (e.g., Moorthy & Winter, 2006). More broadly, our findings imply that consumer-facing transparency (via OCRs) can discipline deviations from the low-price framing; disclosure requirements would complement—rather than replace—the reputational discipline channel we document through OCRs.

Overall, the evidence contributes to the literature on online pricing policies and reputation systems by documenting how a low-price guarantee can shape consumers' reference points and their evaluation of subsequent price movements. In a competitive and transparent e-commerce environment, these results suggest that PMGs may influence feedback even when average price effects are small, because they affect expectations and the salience of value-for-money considerations. More broadly, the analysis motivates further work on how pricing frames interact with platform reputation mechanisms in digital marketplaces.

Last but not least, two caveats should be made at this point. From the one hand, while our identification uses product- and time-fixed effects at the product–provider level and the mechanism tests are stable across specifications, we cannot exclude heterogeneity along dimensions we do not observe directly. In particular, the effect of PMGs may vary by product category, consumer type (repeat vs. new buyers), and platform-specific trust norms or review cultures. Our coverage within categories is incomplete; we do not observe buyer identities or histories, nor do we observe PMG redemptions or refund amounts, so we cannot directly quantify the policy's average cost to the retailer, and platform norms at the time of data collection may differ from those prevailing today. Therefore, cross-platform differences (e.g., Amazon vs. NewEgg) should be interpreted cautiously despite our research design. This specific caveat does not overturn the central mechanism—price/value salience rises and ratings fall when prices move under a low-price frame—but it does bound external validity. We see value in future work that combines balanced category sampling with consumer-level panels and exogenous changes in platform policies to recover heterogeneity by category, buyer type, and trust environment more sharply.

On the other hand, our evidence is based on 58 products across 19 sub-categories observed from May to October 2018 (175 days) on two major e-commerce platforms (Amazon and NewEgg). This window is intentionally tight to support credible identification, but it also limits direct extrapolation to today's e-commerce environment, to other geographies, or to marketplaces with different competitive intensity. Platform practices, consumer search technologies, and price-match policies have evolved since our sample period, and the composition of categories on major platforms may have shifted. While the internal validity of our estimates is preserved by product and time fixed effects and by the mechanism checks, the external validity is consequently bounded in time, platform, and market structure. We therefore view our results as characterizing the behaviour of PMGs in the studied market and period. Extending the analysis to longer and more recent panels, to additional retailers and geographies, and to policy changes documented after 2018 is a valuable direction for future work.

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