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Price Matching in Online Retail

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We analyze a sample of consumer-electronics products sold by the US NewEgg online-retailer to study the impact of Price Matching Guarantees (PMGs) policies on prices. By applying a Difference-in-Differences approach, we find that prices of the policy-adopting retailer increase by 4.7% during the policy validity period and up to five days after the treatment, while those of the major non-adopting competitor are not affected. Results are mainly driven by highly-rated, visible and expensive products, while the policy does not affect low-rated, less visible and cheaper ones. Overall findings are consistent with the hypothesis that PMGs act as price discrimination tools.

KEYWORDS
Price Matching Guarantees, Online Retailing, User Generated Contents, Difference-in-Differences, Price Discrimination, Collusion, Signalling

JEL CLASSIFICATION
L11, L13, L15, L81

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1 | INTRODUCTION

Online retailers have recently gained increasing importance in a number of markets, since they can provide personalized services, more convenient delivery schedules and can reach a very high number of consumers. Moreover, they provide recommender systems, based on reviews and ratings, that represent an important source of information for both consumers and sellers. In addition, online retailers claim to warrant lower prices with respect to traditional stores through the provision of special offers, promotions, down prices and other price discounting policies. Among these options, they often implement Price Matching Guarantees (PMGs) policies, i.e. the promise to match lower prices offered by competitors, that are often designed in a more complex way with respect to brick-and-mortar stores commercial policies.

The announcement to tie prices to those of competitors is appealing for customers, since they usually interpret the policy as a guarantee of low prices that increases consumer confidence and brand fidelity. However, the theoretical literature analysing PMGs along various directions has stressed how these policies can reduce competition and sustain high prices in certain markets, thus harming consumers' welfare under some conditions. In particular, the most relevant anti-competitive theories consider PMGs as tools for implementing collusive practices [see, among others, Hay, 1981, Salop, 1986, Belton, 1987] or, alternatively, price discrimination. The first strand of literature suggests that, under certain hypothesis, PMGs can sustain collusion in oligopoly models and highlights that such clauses might be considered as threats to punishment for firms that lower cartel prices, thus reducing firms' incentives to deviate from agreements.[1] Alternatively, some other authors argue that sellers can use PMGs policies as a price discrimination tool. Png and Hirshleifer [1987], Belton [1987], Corts [1997] and Nalca et al. [2010] propose theoretical models where firms rely on PMGs to discriminate across groups of consumers that can be either uninformed (e.g. characterized by high search costs and inelastic demand) or informed (e.g. with low search costs and elastic demand). In such models the PMG-adopting firm raises posted products' prices to target uninformed buyers (which do not shop around) and leverages on the existence of competitor's lower listed prices to ensure a lower selling price to informed buyers (which are more price sensitive, are able to shop around and claim PMGs refunds).[2] Another strand of literature has proposed the hypothesis that PMGs can be used as signals of unobservable prices, leading to a market equilibrium where only low-price sellers adopt PMGs, under certain hypothesis [Jain and Srivastava, 2000, Moorthy and Winter, 2006, Moorthy and Zhang, 2006, Mamadehussene, 2019]. In particular, Moorthy and Winter [2006] argue that PMGs can act as signals of unobservable prices for uninformed consumers, thus providing incentives to stop further searching, so that the adopting seller can charge relatively higher prices. Moreover, Srivastava and Lurie [2004] suggest that PMGs are likely to be credible signals when market disciplinary mechanisms are strong, that is when buyers have a high willingness to search and claim refunds.[3]

Since the theoretical literature suggests that PMGs policies can have different effects on prices, according to markets conditions and consumers characteristics, it becomes interesting to analyze such an issue from an empirical point of view. However, applied studies designed to test the effects of these pricing policies are scarce and often inconclusive [Mago and Pate, 2009].[4] Excluding previous analyses of small products' groups in narrow local markets,

[1] Several other papers support the pro-collusive argument by extending the basic oligopolistic setting or applying the Hotelling model [e.g. Logan and Lutter, 1989, Baye and Kovenock, 1994, Lu and Wright, 2010, Hviid and Shaffer, 2010, Pollak, 2017, Constantinou and Bernhardt, 2018, Cabral et al., 2018]. However, Hviid and Shaffer [1999] highlight that the presence of hassle costs (costs for activating the guarantee) might undermine possible anti-competitive effects of PMGs.

[2] See also Edlin [1997].

[3] Examples of these mechanisms are buyers' actions, like withholding repeat purchases, engaging in negative word-of-mouth and calling for regulatory actions, that might act as punishments against false price signals.

[4] See Section 2 for a short survey of the applied literature.

relying on products catalogues and newspaper advertisements, the only study that investigates the impact of PMGs on prices by observing a wide range of goods in a national (online) market is that of Zhuo [2017]. The author finds that these policies can generate significantly higher market prices and interprets its results as consistent with both collusion and price discrimination theories of PMGs. Therefore, we believe it is worthwhile to further investigate this topic. In particular, we test the following predictions of aforementioned theoretical models in terms of price patterns associated to the adoption of PMGs policies.

Hypothesis 1 (H1): *Price Matching Guarantees as collusive devices.*

Collusion may arise when firms rely on PMGs as punishment threats; moreover, such polices may favour cartel stability. By reducing firms' incentives to cut prices and deviate from agreements, PMGs generate higher equilibrium market prices for both adopting and non-adopting retailers.[5]

Hypothesis 2 (H2): *Price Matching Guarantees as price discrimination tools.*

Price discrimination practices associated to PMGs policies require the existence of consumers with different willingness to search and pay. Within this framework, theoretical models predict higher equilibrium posted prices for the policy-adopting retailer, conditionally on the existence of a lower price listed by at least one competitor.[6]

Hypothesis 3 (H3): *Price Matching Guarantees as signals of low prices.*

If PMGs act as signals of actual low prices, consumers are more likely to stop further search, so that relatively higher prices for the adopting retailer should be observed. Moreover, PMG-adopting firms should be those associated with the lowest prices in the market.

In order to empirically test the aforementioned hypotheses, we rely on a Difference-in-Differences (DiD) identification strategy where products are considered as treated during the PMGs policies' validity period. In particular, we collect prices of PMG-targeted products on a sample of daily consumer electronics observed on the US NewEgg online retailer, which exclusively sells this type of products and applies PMGs to different items on different days, sometimes repeatedly for the same goods.[7] Moreover, we recover price information for the same (treated) products but sold on different online retailers' websites, namely Amazon UK and Amazon US (which never offer PMGs) during the sample period.[8]

Given the complexity of the treatment design, we apply the DiD estimator proposed by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022], that controls for treatment heterogeneity and dynamic effects and is well suited to our case, characterized by units repeatedly going in and out of treatment status. It is worth pointing out that our identification strategy is based on the analysis of price changes for both the adopting and the non-adopting retailer and complements the approach of Zhuo [2017], who only focuses on price changes for the non-adopting one, before and after the implementation of PMGs by competitors.

[5] It is worth noting that assumptions underlying theoretical collusive models of PMGs have been proposed for brick-and-mortar stores and do not account for online markets' peculiarities, like the presence of relatively lower hassle costs.

[6] It should be noted that these models require a significant percentage of buyers invoking the guarantee.

[7] NewEgg's PMG policy states that "if you purchase an item from Newegg.com which is carrying the Price Match Guarantee badge at the time of purchase, then find the exact same item at a lower price by Newegg or a major retailer, just let us know, and we'll send you a Newegg Customer Care Card to cover the difference". See `https://kb.newegg.com/knowledge-base/price-match-guarantee/`.

[8] They will be alternately used to test the different hypotheses.

First, we compare NewEgg's average price changes, before and after the PMGs policy validity period, with those of Amazon UK over the same period.[9] Empirical results provide evidence in favour of a positive average treatment effect of about 4.7%, whereby NewEgg's prices increase during the PMGs validity period; in addition, estimates suggest that this positive impact of PMGs on prices vanishes five days after the treatment. These findings are consistent with all the above-mentioned hypotheses, predicting higher posted prices for the PMG-adopting retailer. However, when we analyze, with a similar research design, price patterns for the same PMGs-targeted products, but sold by one of the major NewEgg's competitors (Amazon US), we find that they are not affected by the policy, contrary to the predictions of collusive models.[10] Moreover, by investigating if PMGs have different effects on products characterized by different initial prices, we find that more expensive products exhibit 6% higher prices during the policy validity period, while PMGs barely affect initially lower-priced items. These findings seem to be not fully consistent with signalling models, predicting higher prices for initially lower product prices.

Overall results lead us to interpret the evidence of higher posted prices for the adopting retailer as consistent with the hypothesis that PMGs can act as price discriminating tools in our sample, since another assumption of these models is supported by the data, namely the existence of lower listed prices by competing retailers for targeted products. In order to strengthen this interpretation, we analyze possible heterogeneous PMGs treatment effects. In particular, since price discrimination theories assume the existence of consumers with different search costs and willingness to pay, we believe it is worth analyzing if the impact of PMGs policies on prices is different according to the easiness of finding products on the Internet. Specifically, we infer products' visibility and popularity by analyzing some of the User Generated Contents (UGCs), which represent a great added value associated to the use of online data and are important tools for reducing asymmetric information between customers and retailers.[11]

Estimates conducted on specific sub-samples suggest that highly rated (popular) products experience price increases of about 6,8% during the PMGs validity period, while for low rated (unpopular) ones the average treatment effect of the policy is not statistically different from zero. Similar results are found when we separately analyze goods according to their online visibility, whereby highly visible items exhibit higher prices during the treatment of about 9%.[12] The heterogeneity analysis highlights that our findings obtained over the full sample are mainly driven by highly rated (popular) and highly visible products and we believe that these results are consistent with predictions associated to theoretical models related to the hypothesis H2. We argue that, for these products, uninformed buyers (that are supposed not to shop around) can be charged higher posted prices during the policy validity period, while informed

[9] NewEgg's PMGs only apply to purchases within the United States, so that it is less likely that products sold by Amazon UK are affected by NewEgg's commercial policies (`https://promotions.newegg.com/nepro/16-2624/index.html`). Moreover, differentials in size and market relevance between Amazon UK and NewEgg strongly alleviate concerns about possible strategic interactions across these retailers; therefore, we argue that Amazon UK can be considered as an appropriate counterfactual sample, mimicking what would have happened to prices of treated products in the absence of PMGs.

[10] Indeed, collusion models would imply higher prices also for non-adopting firms. It might be argued that analyzing only two retailers is not sufficient to detect possible collusive behaviors, since prices may or may not have also increased for other relevant competitors; however, given the size and relevance of Amazon US, the fact that its prices are not affected by NewEgg's PMGs policies provides substantial evidence in favour of the absence of collusive strategies.

[11] As we explain in detail in the Data Section, we proxy products' popularity by means of ratings (# stars) or, alternatively, by the number of reviews received by each product, while products' visibility is measured by the Google search rank, that proxies for the time spent online to find the product.

[12] Moreover, since products' ratings are often correlated to products' online visibility, we replicate our analysis after distinguishing goods on the basis of both characteristics jointly considered. Such an analysis suggests that NewEgg's highly rated-visible items experience a price increase of about 9% during the policy validity days. Again, prices of low rated-visible products are not affected by the policy; indeed, such sub-sample has a really small size and these products are less targeted by PMGs policies, so that coefficients are more likely to be poorly estimated.

ones (which are more likely to shop around) may be retained on the retailer's website by guaranteeing a lower selling price thanks to PMGs refunds (and a lower listed price elsewhere). Indeed, the effectiveness and profitability of the price discrimination policy for popular products relies on the existence of a sufficient number of uninformed consumers buying products at higher prices, as suggested by Moorthy and Winter [2006]. As far as less popular products are concerned, we argue that a relatively lower number of uninformed buyers would find them, given that they have high search costs, so that PMGs might not be effective price discrimination tools for this class of products.[13]

The paper is organised as follows. Section 2 discusses previous empirical literature and our article's contribution; Section 3 describes the data extraction process and provides summary statistics; Section 4 explains our identification strategy; Section 5 discusses estimates results and robustness analysis. Section 6 concludes.

2 | RELATED EMPIRICAL LITERATURE

The empirical literature that studies the effects of PMGs policies on prices focuses on specific markets (tyre, gasoline) and/or on retailing prices from supermarkets and chain stores; just few studies analyses online markets.

Arbatskaya et al. [2000] recover daily price quotes from the tyre industry's advertisements from 61 US Sunday newspapers, observed for three months in 1996. Authors find weak evidence of anti-competitive effects of PMGs and show that an increase in the number of firms implementing the policy leads to a 10% increase in prices.[14] The same sector has also been analyzed by Mamadehussene [2021], who proposes a structural framework to study the effect of PMGs on market competition through a counterfactual analysis, and finding that prices are maintained by PMGs at a 1–8% higher level than they would be without the policy. The author also shows that PMGs have the largest effect on the most price-sensitive consumers segment, that pays up to 10% higher prices in the presence of PMGs. Cabral et al. [2018] focus instead on daily pricing policies adopted by the Shell network of gas stations in Germany in 2015. Leveraging on gas stations localization and consumers demographics as sources of identification, they suggest that PMGs can be a collusion enacting policy.[15] Gas station prices have been analyzed also by Byrne and De Roos [2019] for Australia by means of a detailed 15 years time series dataset. Authors argue that the majority of gas stations prices follow a weekly cycle and that dominant firms can use PMGs to coordinate market prices and reduce price competition.[16]

Pro-collusive results can also be found in Chilet [2018], who analyses pricing policies of three big retail pharmacy chains in Chile, observed over the period 2006-2008. The author follows an identification strategy based on the estimation of a demand model, in which quantity sold is a function of price differentials with respect to competitors, over the period where collusive price increases occurred. Differently, Chen and Liu [2011] scrutinize prices for 55 consumer electronics products offered by a group of large electronics retailers, including retail chains, department stores and Internet based retailers, in order to analyse the combined effect of "Most-Favored Customer" (MFC) clauses and PMGs introduced by the Best Buy platform, on strategic price linkages across retailers.[17] The authors find that Best

[13] The validity of our research design is confirmed by an extended set of statistical analysis, including event study regressions and falsification/placebo tests.

[14] The same authors, in Arbatskaya et al. [2006], confirm their results by analysing the same data with a different approach.

[15] See also Atkinson et al. [2009] and Wilhelm [2019].

[16] See also Cavallo [2017].

[17] MFCs policies are commercial practices very close to those of PMGs, whereby sellers promise to match own future prices over a certain period of time after a buyer's purchase. MFCs commercial policies may seem pro-competitive, because they offer past customers the same attractive terms as brand-new customers. However, MFCs policies can be employed as tools to maintain higher prices, since they decrease sellers' incentives to cut prices over time [see among others, Schnitzer, 1994].

Buy lowers its average prices by 1.6% following the policy change, and the same result is observed for its competitors.[18] Hess and Gerstner [1991] analyse instead the effect of PMGs on prices by collecting weekly data of 114 goods sold in several US supermarkets and grocery stores, from 1984 to 1986, and provide evidence in favour of higher prices (about 1-2%) when the guarantee is introduced. Different results are provided by Moorthy and Winter [2006], who analyse prices of several products sold by 46 Canadian retailers in 2002 and assume the existence of informed and uninformed consumers. Authors argue that the adoption of PMGs policies might be interpreted as a way to signal lower prices to uninformed customers and suggest that PMGs are mainly adopted by low cost/low service chain stores.[19] Similar results can be found in Chung and Kim [2016] for three leading hypermarkets in Korea. Finally, Zhuo [2017] focuses on online retailers and collect US price data from online price trackers for 150 products offered on Amazon in 2012. The author observes prices during and after the implementation of PMGs policies by two big-box stores (Target and Best Buy) targeted specifically on matching Amazon prices; by applying DiD and RDD methods, the author suggests that prices of the non-adopting retailer increase by about six percentage points during the period of validity of the policy. Moreover, the analysis highlights an heterogeneous impact of PMGs, with larger price increases for initially lower-priced goods. The author interprets these results as consistent with both collusive and price discrimination theories on PMGs.[20]

It is worth noting that also the experimental research has analyzed the effects of PMGs using laboratory experiments and computer simulated agents. Yuan and Krishna [2011] investigate the impact of PMGs on buyers' search and sellers' pricing behavior by analysing their strategic interaction. Authors suggest that, when searchers' demand is more elastic than non-searchers' one, PMGs can result in more intense price competition, even when sellers are symmetric and highlight that "price matching symmetric sellers are more likely to use PMGs as devices to price discriminate than to collude, that is, they will randomize their prices rather than collude at a high price level". Among others, experiments conducted by Jain and Srivastava [2000] suggest that PMGs lead to a perception of lower prices in PMG stores, while Srivastava and Lurie [2001] find evidence that consumers are less likely to search after encountering a price-matching guarantee.[21] Furthermore, we point out Deck and Wilson [2003], who find that PMGs generate significantly higher prices relative to other pricing algorithms, and Deck and Wilson [2006], suggesting that sellers price discriminate by tracking customers' search activity and finding that "buyers who searched more receive lower prices and buyers who search less receive higher prices". Moreover, Fatas and Mañez [2007] report that markets converge to the collusive result and almost all sellers choose to adopt PMGs. In works by Dugar and Sorensen [2006] and Dugar [2007] the presence of buyers with positive hassle costs acts as a market disciplining device that reduce the incentive to collude. The authors suggest that firms adopt PMGs as a device to screen buyers in terms of their willingness to pay the lowest price for the product. Overall, also the experimental literature does not provide conclusive evidence on the impact of PMGs on prices.

[18] They explain their results by arguing that consumers can be distinguished according to their level of hassle costs, so that when a firm adopts the MFC clause it can discriminate across them, charging lower prices to low hassle costs buyers but regular prices to high hassle costs ones. However, such policy induces rivals to lower prices in order to retain their low hassle costs customers. Moreover, if rivals' prices are sufficiently low, firms might start competing also for attracting high hassle cost consumers. As a result, regular prices decline for all competitors. However, the authors do not provide a formal theoretical model.

[19] Authors suggest that firms offering higher prices do not find convenient to apply PMGs as it would imply devolving their pricing decisions to low price competitors [Moorthy and Winter, 2006].

[20] Some authors [Arbatskaya et al., 2006, Corts, 1997] analyse the impact of price-beating guarantees, that are less widespread policies with similar terms as price matching ones (in price beating guarantees refund exceeds the price difference). Studies that refer to these policies argue that, with respect to price matching guarantees, they might be serving different purposes in practice and likely be effective in enhancing competition.

[21] See also Srivastava and Lurie [2004] and Mago and Pate [2009].

Our study enriches the literature on the price effects of PMGs along different lines. First, it tests some predictions of theoretical models on the impact of PMGs on prices using online retailing data, that have been insufficiently analyzed so far since most of previous literature on this issue focused on brick-and-mortar stores. Second, we apply a novel research design by also analyzing the impact of PMGs on prices of the PMG-adopting firm, while the only previous study relative to online markets focused on prices of the non-adopting one. Third, we analyze a treatment design that is quite complex and different from the standard practices previously analyzed. Fourth, by considering detailed real-time daily online data obtained with a scraping program specifically coded for the analysis, the study relies on information that is characterized by a significantly higher level of quality with respect to previously used one, based on history charts built from price tracking websites, or recovered with online tools that extracts data from plots and images. Finally, the analysis exploits the high information content made available by online retailers and generated by consumers, namely the UGCs. Indeed, products characteristics based on information on UGCs are employed for the first time to empirically test the results of some theoretical models on PMGs.

3 | DATA

3.1 | Data Extraction

In order to study the impact of PMGs on prices, we focus on the consumer electronics retail market, that is often the target of such pricing policies. Moreover, electronic goods are barely affected by seasonal effects, so that prices signals are more stable over time.[22] Even if such a sector is characterized by the presence of several competitors, ranging from small local stores to chain stores, we have decided to focus on the online retailing. This choice is related to the possibility of exploiting products information, other than price, only available for online markets, namely User Generated Contents (UGCs); moreover, the advent of online markets has made the search goods' (like electronic products) evaluation process potentially cheaper and faster and has probably changed the working of PMGs policies, whose outcome depends, among other factors, on consumers willingness to search.

Among different online sellers, we choose to focus on NewEgg, a leading US retailer of consumer electronics products that implements PMGs policies on selected items in different periods, often switching the policy on and off several times for the same goods.[23] In particular, NewEgg communicates the period of validity of the price guarantee by means of a label that appears on the specific product's online page; the customer who purchases an eligible item and discovers the PMG badge has 14 calendar days of time to find the same "title" at a lower advertised price from major retailers belonging to a declared list of US competitors.[24] The list includes amazon.com (US), bestbuy.com, cdw.com, crutchfield.com, dell.com, frys.com, gamestop.com, kmart.com, officedepot.com, officemax.com, sears.com, staples.com, target.com, and walmart.com; moreover, the term "major retailer" excludes third-party sellers whose products are sold through a major retailer's online marketplace.

With the aim of building our sample, we have identified all NewEgg electronics products subject to PMGs on May 2018 and we have observed such products (100) from May 2018 to October 2018, in order to recover their prices, PMGs information and other UGCs.[25] As mentioned in the Introduction, we also consider alternative samples by

[22] In particular, our sample covers the period May - October 2018 and does not include important dates like Thanksgiving or Christmas.

[23] Given that our identification strategy is based on the comparison of prices before and after the policy validity period, we do not consider online retailers that apply PMGs to wide groups of products continuously over time (i.e. Target, among others).

[24] With "title" one refers to a product with the same brand and model number. NewEgg, after checking the validity of the claim, sends a Customer Care Card to refund the price difference (Source: `https://promotions.newegg.com/nepro/16-2624/index.html`).

[25] The average number of treatments occurred in our sample is about 2.8, thus suggesting that, on average, the policy is applied more than twice to each product during the sample period.

selecting the same titles but sold on other retailing websites, i.e. Amazon US and UK, that never offer PMGs policies; for these items we have collected prices and UGCs information. This approach has led to a reduction in the number of observations, so that the final sample includes 87 products belonging to 19 sub-categories (computer hardware, tablet and computers, mobile phones, printers and scanners, PC accessories, speakers for domotics, screens and audio devices).[26] It is important to point out that we consider products directly sold from NewEgg and Amazon (UK and US) and we do not consider third-party sellers products, since PMGs do not apply to those ones; this implies that we can neglect considerations associated to the two-sided nature of online retailing platforms. However, we believe that it is worth focusing on a relevant online retailing website since it interesting to analyze a complex treatment design like the one implemented by NewEgg; moreover, as we want to enrich our analysis with information stemming from UGCs, we argue that such contents provided by relevant online retailers are sufficiently reliable, because both NewEgg and Amazon provide guidelines for customer reviews and ratings, and take the reliability of the recommender system very seriously. In particular, Newegg declares that "the platform reads all reviews before posting them and reserves the right to deny any review", while Amazon states that "if we determine that you have attempted to manipulate reviews or violated our guidelines in any other manner, we may immediately suspend or terminate your Amazon privileges, remove reviews, and delist related products".[27]

The retrieving of sample data has been a challenging task. Given the absence of ready-made and easy-to-use repositories on price data, we have developed an ad-hoc scraping program (in Python language) able to protect the scraping process from unpredictable changes of the web page and capable to recover the data without stressing the website, thus limiting the risk of interruptions due to firewalls. In particular, the scraping process has been supported by several alert tools signalling periodical changes of the internal page structure, since retailing platforms frequently change the deep structure of the page, in a not visible way by the human reader but in a way that affects the program code and the scraping process. The process of data collection has required the subscription to the Amazon Web Service (AWS) cloud in order to use virtual servers where installing and launching the daily loop process. The scraping code allowed us to navigate among product pages, select the field tags, get the data and save on a server disk. Each scraping session run about 20 minutes every day. It is worth noting that this approach allows us to collect real-time high-quality information and overcomes some limits associated to the use of price monitoring websites, that only track prices of relatively popular products, or to the extraction of data from price history graphs (e.g. WebPlotDigitizer) that do not provide continuous price changes [see Zhuo, 2017].

In addition to information on product prices and PMGs, we also have collected some products characteristics exclusively available online and based on customers' rates and comments. Indeed, UGCs like ratings and reviews left by previous customers can be a significant source of knowledge for both consumers and retailers.[28] In particular, the online retailer enables buyers to assess various offerings that have been successful or well-liked by others and it coordinates the sharing of information among users while guaranteeing their trustworthiness. In addition, UGCs can generate significant network effects. Overall, as suggested by Belleflamme and Peitz [2018], "since buyers actively provide and access the information, we may consider ratings and reviews as part of a platform's information-pull strategy".[29] Moreover, online retailers use an information-push strategy by targeting buyers with recommendations

[26] In the Appendix A we provide a detailed list of selected products (Tables A.1 and A.2).

[27] See `https://www.amazon.com/gp/help/customer/display.html?nodeId=G8UYX7LALQC8V9KA` and `https://kb.newegg.com/knowledge-base/newegg-product-review-guidelines/`.

[28] Timoshenko and Hauser [2019], by leveraging on machine-learning techniques, compare a database on customer needs based on interviews and focus groups with information based on UGCs, showing that the latter are at least as valuable as a source of information.

[29] UGCs are considered to be less biased with respect to products information provided by sellers, since they are supposed to be free of commercial interests, so that they represent a valuable source of information for consumption choices [Chen and Xie, 2008].

of specific products based on their traits and observed behavior, as also revealed through UGCs.[30]

Specifically, we have recovered products' ratings and the number of reviews received by each item. Products' ratings given by consumers are measured as the number of stars gained by each good, ranging from zero to five, while the absolute number of reviews is based on written texts about products, services or experiences. Starting from this latter information, we have built a normalized index as the ratio between the number of reviews obtained by a product and the amount of reviews received from the most reviewed item in the same sub-category. All these information are well suited to proxy for products' popularity; however, we prefer to consider products' ratings as a proxy for their popularity since such information is found to be more stable over time. Finally, we have built a product specific Google search rank as a proxy of the time spent on the search engine to discover the specific web-page of a certain product. It is reasonable to assume that this index is inversely correlated to the search effort needed to find it. In particular, for each item we have launched, at the beginning of the sample period, the Google query composed by the sentence ("*product name*" AND "*retailer name*") and we have recovered its ranking position.[31] Such position has been normalized in order to interpret the search index as the probability to find the product in first ranked positions of Google. It is worth noting that, although products analysed are sold by Amazon UK and NewEgg in different countries, information on some of considered UGCs maintain their consistency across countries. This property is typical of consumer electronics goods that have a standardized nature; however, we adopt a country-specific search index by launching the Google search engine with specific country settings.[32]

3.2 | Descriptive Statistics

The analyzed sample includes 13,542 daily price observations for 87 products observed from May 2018 until October 2018 (175 days) on different retailing websites (NewEgg, Amazon UK and Amazon US). Table 1 shows summary statistics on prices and selected products' characteristics for these samples. Prices show a large variability, being the average for the overall sample \$268.32 and the standard deviation \$320.77. Average prices for Amazon UK are noticeably higher than those of both NewEgg and Amazon US (about 253\$). It is worth noting that such patterns do not represent an issue for our identification strategy as long as the parallel trend assumption is satisfied (see Section 4).

In order to have a first glance of the pattern of prices before and after PMGs policy validity period, we estimate the following equation for the sample of products observed on NewEgg:

$$\log Price_{i,t} = \beta_0 + \theta PMG_{i,t} + X_{i,t}^T \beta + \mu_i + \tau_t + \psi_{i,t} \tag{1}$$

where $Price_{i,t}$ is the (log) price of NewEgg's product i at time t and $PMG_{i,t}$ is a binary variable equal to one if the policy is applied to product i at time t and zero otherwise; Equation 1 also includes a set of UGCs controls ($X_{i,t}$), as well as product and day fixed effects, μ_i and τ_t respectively, while $\psi_{i,t}$ is an error term.

[30] Belleflamme and Peitz [2018] argue that these systems are essential for the functionality of digital retailing platforms. Indeed, potential consumers may incur an opportunity cost in evaluating how items fare in terms of quality and how the latter fit their tastes; as a result, buyers value ratings, reviews, and recommendations since they enable them to make more informed choices. Moreover, ratings and reviews are helpful in the products quality evaluation process in presence of a wide range of tastes, since these suggestions guide customers in their product selection.

[31] It is worth noting that such query can provide not only the specific product page, but also a similar product page or a bucket of products that includes the specific object of the search. We rank only the product's specific web page.

[32] Amazon UK prices have been converted into dollars at the daily exchange rate.

TABLE 1 Summary Statistics.

Variables	Full Sample	NewEgg Sample	Amazon UK Sample	Amazon US Sample
Provider Price ($)	268.32	253.15	298.00	253.81
	(320.77)	(302.39)	(343.85)	(312.63)
Normalized Reviews (0-1)	0.26	0.20	0.26	0.32
	(0.30)	(0.23)	(0.30)	(0.34)
Search Rank (0-1)	0.78	0.64	0.85	0.85
	(0.27)	(0.36)	(0.17)	(0.17)
Rating (0-5 stars)	4.15	4.15	4.14	4.16
	(0.62)	(0.83)	(0.48)	(0.48)
Treatment Duration (days)		30.87		
		(28.78)		
Treatment Freq. (#)		2.80		
		(1.86)		
Observations (#)	13,542	4,514	4,514	4,514

Notes. The treatment of interest is the PMG commercial policy adopted by NewEgg. Alternative samples has been built by recovering price data for the same titles targeted by NewEgg's PMGs but sold on different retailing websites (Amazon UK and Amazon US). The sample period includes 175 days, from May 2018 until October 2018.

TABLE 2 Average Change in NewEgg Product Prices Before and After PMGs Validity Periods.

NewEgg's Products Prices (log)	(1)	(2)
$PMG_{i,t}$	0.060*	0.062*
	(0.034)	(0.032)
Controls	✗	✓
Product FE	✓	✓
Day FE	✓	✓
Observations	4,514	4,514
R-squared	0.980	0.980
F Test (p-value)	0.084	0.053

Notes. This analysis leverages solely on the NewEgg's treated sample. All specifications include time and product fixed effects and are estimated by OLS. Controls include the absolute and the relative number of reviews as well as ratings. Standard errors, clustered at product-title level, are shown in parentheses; ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Table 2 reports estimates of the average change in NewEgg's product prices when a product becomes treated, i.e. when the PMG is applied. Results shown in columns (1) and (2) suggest that prices are about 6% higher during the policy validity days, and such pattern is confirmed by including controls. These findings provide evidence in favour of a strong correlation between the adoption of PMGs policies and NewEgg's products average price changes.

Finally, another important issue is related to the representativeness of our sample. Appendix A provides a detailed analysis of such an issue.

4 | IDENTIFICATION STRATEGY

In order to test hypotheses H1, H2 and H3 discussed in the Introduction, we estimate the causal effect of PMGs policies on targeted products' prices by relying on a DiD research design; this amounts to compare the average price change, before and after the policy validity period, for treated products to the average price change for the control group, over the same sample period. As discussed in Section 3, the empirical setting that we observe is quite complex, since different units are treated at different points in time and some of them are repeatedly subject to PMGs over the sample period. Most of previous studies that have adopted a DiD approach to settings characterized by complex treatment designs have applied Two Way Fixed Effects (TWFE) regressions; however, De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] suggest that "under a parallel trends assumption, TWFE regressions may produce biased estimates of the treatment's instantaneous and dynamic effects, if effects are heterogeneous across groups and over time" and propose a different DiD estimator able to estimate these effects in the presence of such kind of treatment designs.[33] We leverage on such a new estimator that not only allows us to address the complexity of our treatment design, but also to compute "placebo" estimators that can be used to test the non-anticipation (pre-trend), strict exogeneity or parallel trends assumptions underlying the DiD estimator. Therefore, our empirical strategy is based on estimating different specifications of the following event study Equation:

$$\log Price_{i,r,t} = \sum_{\xi=-K}^{-1} \gamma_\xi \left(PMG_{i,r,t+\xi}\right) + \sum_{\xi=0}^{M} \delta_\xi \left(PMG_{i,r,t+\xi}\right) + X_{i,r,t}^{T}\beta + \mu_{i,r} + \tau_t + \lambda_{r,m} + \epsilon_{i,r,t} \tag{2}$$

The dependent variable, $Price_{i,r,t}$, represents the price (natural logarithm) of product i at time t for retailer r and $PMG_{i,r,t}$ denotes a binary variable equal to 1 if goods are targeted by the price guarantee ξ days before day t. The K leads of the treatment indicator span from 6 to to 1 days 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 6 days after the treatment and the δ's represent the DiD coefficients of interest, reflecting dynamic average treatment effects.[34] Positive (negative) δ's would suggest higher (lower) prices for treated products. Finally, the model includes a full set of day time dummies, τ_t, accounting for unobserved time-varying price determinants that are common to all goods.[35] Products fixed effects, $\mu_{i,r}$, control for any time invariant unobserved heterogeneity at the product and retailer level that could be correlated with the included regressors and that could also drive prices, while $\lambda_{r,m}$ are retailer-by-month fixed effects

[33] The authors provide a Stata command (*did_multiplegt*) to implement their estimator. We refer to De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] for methodological details.

[34] Our main analysis relies on 6 leads and lags; in Appendix B we show different event study specifications with both 3 and 8 leads and lags, respectively.

[35] As noted in Section 3, the sample does not include important dates, like Thanksgiving or Christmas; moreover day fixed effects allow to control for possible time effects associated to particular periods like "back to school" days.

that account for possible retailer/country time varying specific shocks that might affect the results.[36] Equation (2) also contains a set of covariates, $X_{i,r,t}$, controlling for products' characteristics (products' ratings and reviews) derived from UGCs that might affect the outcome of PMGs. All specifications are estimated with the DiD estimator proposed by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] with bootstrapped standard errors, clustered at product-title level.[37]

In the first part of our empirical strategy we test the aforementioned hypotheses by analyzing the impact of PMGs on NewEgg's price patterns; in particular, we estimate the DiD model in Equation (2) relying on Amazon UK as the control sample. The latter choice has been taken since NewEgg's PMGs policies only apply to purchases made within the United States; moreover, we argue that NewEgg and Amazon UK are not direct competitors in the US market, so that prices of identical products sold overseas by Amazon UK are less likely to be related to NewEgg's prices. Such arguments, together with the observation that Amazon UK and NewEgg have a significant different size and relevance, strongly alleviate concerns about possible strategic interactions and price tracking practices taking place across these retailers; hence, we believe that Amazon UK can be used as an appropriate counterfactual sample, mimicking what would have happened to prices of NewEgg's products in the absence of PMGs. With the aim to strengthen our analysis, we also replicate our baseline estimates by alternatively relying on Amazon US as the control sample.

Secondly, in order to provide some evidence on the hypothesis that PMGs can sustain collusion, we examine price changes, before and after PMGs announcements, for the same items targeted by PMGs, but sold on Amazon US, one of the main NewEgg's market competitors. In particular, we estimate Equation 2 by considering prices of NewEgg-PMGs-targeted goods sold on Amazon US as the treated sample, while Amazon UK's product prices act again as the control one. This analysis allows us to highlight if Amazon US's prices are affected by NewEgg's PMGs. Indeed, as mentioned in the Introduction, the observation of only two US competitors is not sufficient for making inference on possible collusive strategies in the consumer electronics market; however the magnitude and importance of the observed players might provide some good hints on such an issue.

Furthermore, in the light of the predictions of some theoretical models that consider PMGs as signals of low prices, we test if the policy has different effects on products that result to be cheaper at the beginning of the sample period. As suggested by Moorthy and Winter [2006], "if price-matching firms are associated to the lowest prices in the market rather than with the highest prices, then it would be hard to argue that price matching is being motivated by price discrimination as opposed to signalling". Indeed, splitting the sample according to initial prices might raise some concerns about selecting on the outcome; however, as suggested by Zhuo [2017], the inclusion of product fixed effects in the model leads us to only rely on within-product price variations instead of the between-product variation, thus making safe grouping goods trough the initial price for each item. Once the panel has been balanced, we therefore estimate Equation 2 after splitting products into two groups based on their average prices at the beginning of the sample period (above/below the median).

In order to provide further evidence on the investigated issue, we explore the existence of possible heterogeneous effects of PMGs on NewEgg's prices by estimating Equation 2 after splitting the sample according to products

[36]It is worth noting that the US and UK online electronics sectors may be affected, during the sample period, by different macroeconomic or sector specific time varying shocks that have the potential to affect the results. For this reason, we include a set of retailer-by-month fixed effects that should control for such possibility. Furthermore, it should be noted that the replication of our estimates relying on Amazon US products' prices as a control sample can provide evidence alleviating concerns on such an issue. Note that including a full set of products-by-day fixed effects is unfeasible since, in this case, our sample size is limited relative to the number of requested fixed effects.

[37]The implementation of the estimator proposed by De Chaisemartin and d'Haultfoeuille [2020], De Chaisemartin and d'Haultfoeuille [2022] requires a reasonable number of replications in order to get reliable standard error estimates. We run 100 bootstrap replications (50 is the default number set by *did_multiplegt* Stata command) and we found that a higher number of replications did not improve the goodness of fit, while significantly increasing the running time. Moreover, standard errors are clustered at product-title level, as defined in footnote 24.

features that allows us to further investigate testable predictions associated to theoretical models on PMGs. In these models consumers may have different willingness to search, so that we believe it is worthwhile to separately analyze the impact of PMGs on prices for products characterized by high (low) popularity and high (low) visibility, that are inversely associated to search efforts.[38] In particular, we measure products' popularity and visibility by exploiting the informative content of UGCs. We consider the rating (# stars) obtained by each product as a proxy for its popularity and we classify products as highly (low) popular if their rating is greater (lower) than 3.8. As far as products' visibility is concerned, we consider the normalized Google search rank indicator discussed in the Section 3 and we classify products as highly (low) visible if their normalized rank is greater (lower) than 0.7. Finally, given that products' popularity and products' visibility are highly correlated in our sample, we further split the sample according to both characteristics jointly considered.

Finally, it should be pointed out that, in order to verify the robustness of our results, we perform a full set of falsification and placebo tests, whose results are discussed in Section 5.1. Moreover, the validity of our research design is supported by an in-depth analysis of the strict exogeneity of the treatment and of the parallel trends assumption, that are fundamental for the validity of a DiD approach. Such analysis is performed by estimating, for all models, the event study specification of Equation 2 and testing if leads coefficients are statistically significant. If the parallel trend assumption holds, such coefficients should be individually and jointly equal to zero. Moreover, we further investigate this issue following a very strictly related approach suggested by Wooldridge [2010], based on estimating a fixed effects regression where NewEgg's prices at time t are estimated as a function of the treatment at time t and of a subset of treatment leads from time $(t+1)$ to $(t+6)$. Under the null hypothesis of strict exogeneity, the coefficients of such leads should be equal to zero and a Wald test, robust to arbitrary serial correlation and heteroskedasticity, can be constructed to test their joint significance. In our setting, this test allows us to verify if the current outcome does have predictive power on future treatments; in other words, if the treatment assignment is exogenous one should not logically expect any correlation between lagged outcomes and current treatment status. This indeed what is required by the parallel trend assumption. We include in Appendix B results of the strict exogeneity test that, comfortingly, allows us to exclude that PMGs policies decision depends on product prices, i.e. there is no selection into treatment in our sample, thus validating our research design.[39]

5 | EMPIRICAL RESULTS

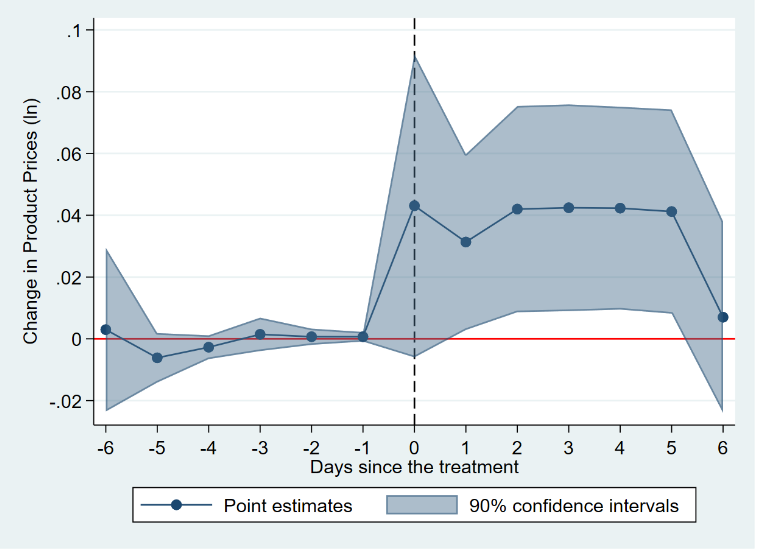
In this Section we provide empirical results obtained by estimating the event study Equation 2 with the DiD estimator proposed by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022], that allows us to test the parallel trend assumption and to give a causal interpretation to the estimated effects of NewEgg's PMGs on prices.

As discussed in the previous Section, we first investigate the impact of PMGs on price patterns observed on NewEgg, the policy-adopting retailer. Empirical results are shown in Tables 3 and 4, as well as in Figure 1. The latter graphically shows estimated event study coefficients of Equation 2 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

[38] Indeed, as suggested, among others, by Branco and Brossard-Ruffey [2017] and Townley et al. [2017], the availability of information released by users on internet can enable online retailers to segment the market. Moreover, Deck and Wilson [2006] argue how the ability to track consumers' search can enable retailers to offer different prices for different customers.

[39] Indeed, we cannot completely rule out potential treatment endogeneity, since the retailer might target those products that are more likely to make PMGs effective, thus generating a possible upward bias in coefficients estimates. However, the analysis on parallel trends and the rich robustness and falsification analysis that we provide lead us to trust our results as not affected by major and systematic bias problems.

FIGURE 1 Event Study Representation of the Effect of PMGs on NewEgg's Prices. Control Sample Amazon UK.



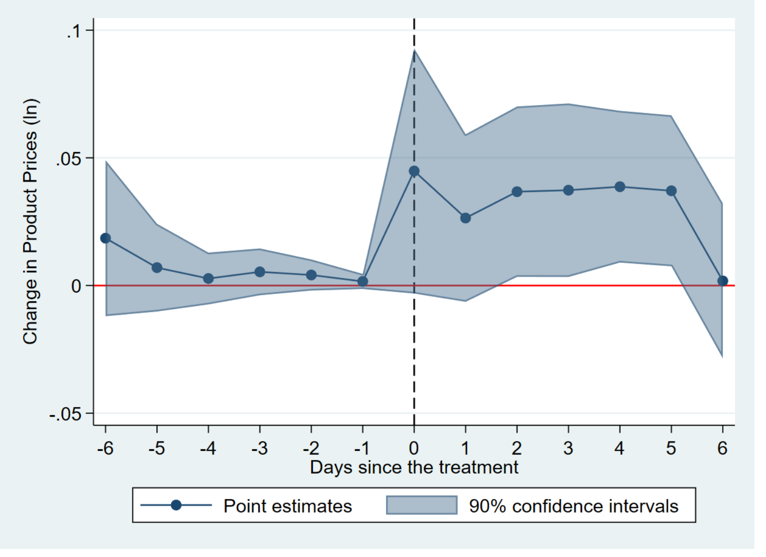
Notes. Estimates of Equation 2 on the full sample (Table 4, column 1), with NewEgg as the treated sample and Amazon UK as the control one. The specification includes controls (absolute and the relative number of reviews, ratings), as well as day, product and retailer$x$month fixed effects. The DiD estimator by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] is applied. The blue area represents the 90 percent confidence intervals around leads and lags. Standard errors, clustered at product-title level, are based on 100 bootstrap replications.

6 to 1 days before the treatment, are individually and jointly not statistically different from zero, thus confirming the absence of any anticipatory effect and the validity of the research design.[40] Estimates shown in column (1) of Table 3 provide evidence in favour of an average price increase of about 4.7 percentage points during the policy validity period for treated products and this result is confirmed when we include control variables in the model (Table 4, column 1). Furthermore, 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 6 days later. Estimated coefficients, graphically shown in Figure 1, suggest that PMGs exert a positive effect on prices, starting the day after the treatment and lasting up to five days, remaining stable in magnitude over time. Comfortingly, if we replicate the above analysis by alternatively relying on Amazon US price data as the control sample, we find similar results; in particular, during the policy validity period, NewEgg's treated products prices increase 4.2% more with respect to control sample ones (see Figure 2).

Evidence of higher posted prices associated to PMGs for the policy-adopting retailer is consistent with the predictions of collusive (H1), price discrimination (H2) and signalling (H3) models of PMGs. In particular, under the collusion theory, PMGs are perceived as signals of willingness to collude which reduce incentives for both policy-adopting firms and non-adopting ones (if any) to cut prices, so that equilibrium prices are relatively high. Moreover, such policies may act as credible punishment threats that can sustain collusive agreements by reducing the likelihood to deviate. Alternatively, under the price discrimination theory, PMGs allow adopting firms to segment the market by charging higher posted prices to uninformed consumers, characterized by higher search costs (less price sensitive) and that do not shop around, while informed customers (those more price sensitive and able to shop around) can benefit from PMGs

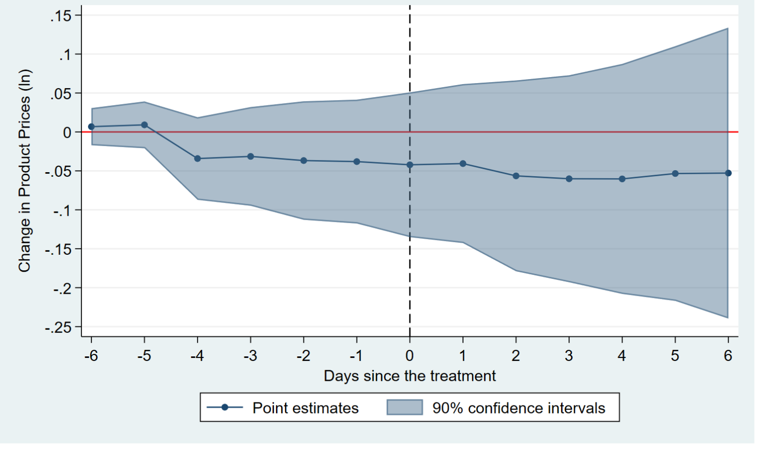
[40] In both Tables 3 and 4 is reported a parallel trends test computing p-value of a statistics testing that all lead coefficients are jointly equal to 0.

FIGURE 2 Event Study Representation of the Effect of PMGs on NewEgg's Prices. Control Sample Amazon US.



Notes. Estimates of Equation 2 on the full sample, with NewEgg as the treated sample and Amazon US as the control one. The specification includes controls (absolute and the relative number of reviews, ratings), as well as day, product and retailer$x$month fixed effects. The DiD estimator by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] is applied. The blue area represents the 90 percent confidence intervals around leads and lags. Standard errors, clustered at product-title level, are based on 100 bootstrap replications.

FIGURE 3 Event Study Representation of the Effect of NewEgg's PMGs on Amazon US Prices.



Notes. Estimates of Equation 2 with policy-targeted goods sold on Amazon US during the NewEgg's PMGs validity period as the treated sample and Amazon UK as the control one. The specification includes controls (absolute and the relative number of reviews, ratings), as well as day, product and retailer$x$month fixed effects. The DiD estimator by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] is applied. The blue area represents the 90 percent confidence intervals around leads and lags. Standard errors, clustered at product-title level, are based on 100 bootstrap replications.

TABLE 3 DiD Estimates of the Impact of PMGs on NewEgg's Prices. No Controls.

Product Prices (*log*)	(1) FULL SAMPLE	(2) L-RATING	(3) H-RATING	(4) L-VISIB	(5) H-VISIB	(6) LR-LV	(7) HR-HV
$PMG_{i,l,t-6}$	0.003	0.000	0.012	0.022	0.016	-0.011	0.016
	(0.016)	(0.009)	(0.024)	(0.034)	(0.026)	(0.007)	(0.024)
$PMG_{i,l,t-5}$	-0.006	-0.059	-0.002	0.008	-0.004	-0.002	-0.004
	(0.005)	(0.047)	(0.004)	(0.030)	(0.006)	(0.001)	(0.006)
$PMG_{i,l,t-4}$	-0.003	-0.050	-0.002	0.014	-0.003	-0.002	-0.003
	(0.002)	(0.040)	(0.003)	(0.023)	(0.004)	(0.001)	(0.004)
$PMG_{i,l,t-3}$	0.001	0.006	-0.001	0.092	-0.001	-0.001	-0.002
	(0.003)	(0.007)	(0.003)	(0.142)	(0.003)	(0.001)	(0.003)
$PMG_{i,l,t-2}$	0.001	0.011*	-0.001	0.001	0.001	0.001	0.000
	(0.002)	(0.006)	(0.003)	(0.001)	(0.003)	(0.001)	(0.003)
$PMG_{i,l,t-1}$	0.001	0.007	0.000	-0.005	0.001	0.001	0.000
	(0.001)	(0.006)	(0.002)	(0.009)	(0.002)	(0.000)	(0.002)
$PMG_{i,l,t}$	0.043	-0.111	0.054	-0.040	0.075*	-0.150	0.084*
	(0.030)	(0.083)	(0.033)	(0.050)	(0.042)	(0.097)	(0.047)
$PMG_{i,l,t+1}$	0.031*	-0.065	0.049**	-0.040	0.057**	-0.150	0.063**
	(0.017)	(0.069)	(0.020)	(0.050)	(0.027)	(0.097)	(0.031)
$PMG_{i,l,t+2}$	0.042**	-0.030	0.055**	-0.020	0.073**	-0.056	0.073*
	(0.020)	(0.059)	(0.022)	(0.041)	(0.033)	(0.088)	(0.038)
$PMG_{i,l,t+3}$	0.043**	-0.016	0.055**	-0.017	0.074**	-0.040	0.073*
	(0.020)	(0.059)	(0.023)	(0.040)	(0.033)	(0.085)	(0.038)
$PMG_{i,l,t+4}$	0.043**	-0.011	0.056**	-0.021	0.075**	-0.055	0.073*
	(0.019)	(0.055)	(0.023)	(0.036)	(0.032)	(0.073)	(0.038)
$PMG_{i,l,t+5}$	0.042**	-0.009	0.055**	-0.017	0.074**	-0.053	0.070*
	(0.020)	(0.054)	(0.024)	(0.043)	(0.032)	(0.072)	(0.039)
$PMG_{i,l,t+6}$	0.008	-0.035	0.020	-0.039	0.031	-0.067	0.022
	(0.018)	(0.060)	(0.021)	(0.051)	(0.023)	(0.075)	(0.023)
ATE	0.047**	-0.051	0.068**	-0.029	0.091**	-0.104	0.091**
	(0.023)	(0.074)	(0.027)	(0.045)	(0.037)	(0.106)	(0.040)
Product FE	✓	✓	✓	✓	✓	✓	✓
Day FE	✓	✓	✓	✓	✓	✓	✓
Retailer*x*Month FE	✓	✓	✓	✓	✓	✓	✓
Observations	9,028	1,803	7,225	2,011	7,017	894	6,108
Parallel Trends Test	0.834	0.020	0.943	0.656	0.643	0.855	0.930

Notes. Estimates of Equation 2 with NewEgg as the treated sample and Amazon UK as the control one. All specifications include day, product, retailer*x*month fixed effects. Highly (low) popular products have ratings higher (lower) than 3.8. Highly (low) visible products have a normalized search index higher (lower) than 0.7. LR-LV are low rated-visible products, HR-HV are highly rated-visible products. The DiD estimator by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] is applied. The parallel trends test computes p-values of the joint test that all lead coefficients are equal to 0. Bootstrapped standard errors, clustered at product-title level, are shown in parentheses; ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

by demonstrating that a cheaper price is available elsewhere, thus being eligible for refunds and lower selling prices. Finally, higher prices during PMGs policy validity periods might also be consistent with a mechanism of endogenous search, whereby a significant fraction of consumers mistakenly assume that PMGs are signals for low prices and stop searching, so that prices are expected to be higher, as suggested by some signalling models.

To shed further light on the interpretation of our results, we analyze the impact of PMGs on prices of the non-adopting retailer. Figure 3 shows event study DiD estimates of the impact of NewEgg's PMGs on Amazon US's prices for the same targeted products. These estimates allow us to understand how does non-adopting retailer's prices react to such policies. In particular, if prices of the non-adopting firm would move in the same direction with respect

TABLE 4 DiD Estimates of the Impact of PMGs on NewEgg's Prices. With Controls.

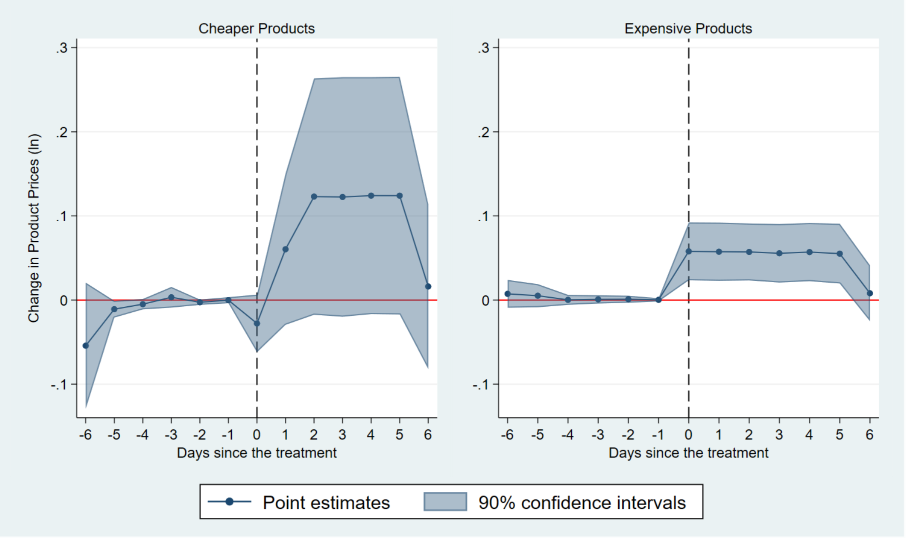
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Product Prices (*log*)	FULL SAMPLE	L-RATING	H-RATING	L-VISIB	H-VISIB	LR-LV	HR-HV
$PMG_{i,l,t-6}$	0.003	0.000	0.012	0.022	0.016	-0.011	0.016
	(0.016)	(0.009)	(0.024)	(0.034)	(0.026)	(0.007)	(0.025)
$PMG_{i,l,t-5}$	-0.006	-0.059	-0.003	0.008	-0.005	-0.002	-0.004
	(0.005)	(0.047)	(0.004)	(0.030)	(0.006)	(0.001)	(0.006)
$PMG_{i,l,t-4}$	-0.003	-0.051	-0.002	0.014	-0.003	-0.002	-0.003
	(0.002)	(0.040)	(0.003)	(0.023)	(0.004)	(0.001)	(0.004)
$PMG_{i,l,t-3}$	0.001	0.006	-0.001	0.007	-0.001	-0.001	-0.002
	(0.003)	(0.007)	(0.003)	(0.011)	(0.003)	(0.001)	(0.004)
$PMG_{i,l,t-2}$	0.001	0.011*	-0.001	0.001	0.001	0.001	0.000
	(0.002)	(0.006)	(0.003)	(0.001)	(0.003)	(0.001)	(0.003)
$PMG_{i,l,t-1}$	0.001	0.007	0.000	-0.005	0.000	0.001	0.000
	(0.001)	(0.006)	(0.002)	(0.009)	(0.002)	(0.000)	(0.002)
$PMG_{i,l,t}$	0.043	-0.111	0.054	-0.040	0.075*	-0.150	0.084*
	(0.030)	(0.083)	(0.034)	(0.050)	(0.042)	(0.097)	(0.047)
$PMG_{i,l,t+1}$	0.031*	-0.065	0.049**	-0.040	0.057**	-0.150	0.063**
	(0.017)	(0.069)	(0.020)	(0.050)	(0.027)	(0.097)	(0.031)
$PMG_{i,l,t+2}$	0.042**	-0.030	0.055**	-0.020	0.073**	-0.056	0.073*
	(0.020)	(0.059)	(0.023)	(0.041)	(0.033)	(0.088)	(0.038)
$PMG_{i,l,t+3}$	0.042**	-0.016	0.055**	-0.017	0.074**	-0.040	0.073*
	(0.020)	(0.060)	(0.023)	(0.040)	(0.033)	(0.085)	(0.038)
$PMG_{i,l,t+4}$	0.042**	-0.011	0.056**	-0.021	0.075**	-0.055	0.073*
	(0.020)	(0.057)	(0.023)	(0.036)	(0.032)	(0.073)	(0.038)
$PMG_{i,l,t+5}$	0.041**	-0.008	0.054**	-0.017	0.074**	-0.053	0.070*
	(0.020)	(0.057)	(0.025)	(0.043)	(0.032)	(0.072)	(0.039)
$PMG_{i,l,t+6}$	0.007	-0.040	0.020	0.020	0.031	-0.055	0.022
	(0.019)	(0.214)	(0.022)	(0.141)	(0.023)	(0.079)	(0.023)
ATE	0.047**	-0.051	0.068**	-0.021	0.091**	-0.102	0.091**
	(0.023)	(0.074)	(0.028)	(0.048)	(0.037)	(0.106)	(0.040)
Product FE	✓	✓	✓	✓	✓	✓	✓
Day FE	✓	✓	✓	✓	✓	✓	✓
Retailer*x*Month FE	✓	✓	✓	✓	✓	✓	✓
Observations	9,028	1,803	7,225	2,011	7,017	894	6,108
Parallel Trends Test	0.835	0.000	0.957	0.798	0.713	0.855	0.936

Notes. Estimates of Equation 2 with NewEgg as the treated sample and Amazon UK as the control one. All specifications include controls (absolute and relative number of reviews, ratings), as well as day, product, retailer*x*month fixed effects. Highly (low) popular products have ratings higher (lower) than 3.8. Highly (low) visible products have a normalized search index higher (lower) than 0.7. LR-LV are low rated-visible products, HR-HV are highly rated-visible products. The DiD estimator by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] is applied. The parallel trends test computes p-values of the joint test that all lead coefficients are equal to 0. Bootstrapped standard errors, clustered at product-title level, are shown in parentheses; ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

to those of the adopting one, we might interpret results as potential evidence in favour of collusive strategies (H1). Estimates confirm the existence of parallel trends between treated and control groups and show that price levels of products sold on Amazon US before and after NewEgg's PMGs validity periods seem not to be affected by the policy. This result is not consistent with collusive models of PMGs, since one should expect higher prices for both adopting and non-adopting firms, while the existence of lower prices posted by at least one of the competitors (Amazon US) is a fundamental pre-requisite for price discrimination models based on PMGs. Indeed, while we are aware that the observation of a single competitor cannot allow us to draw exhaustive conclusions on possible collusive strategies, we argue that the size and relevance of Amazon US as a major player in the online consumer electronics market may suggest some interesting insights on this issue. Furthermore, as suggested by Moorthy and Winter [2006], even in the presence of facilitating devices like PMGs, cartel pricing is less likely in industries where competitors span from small local stores to chain stores and Internet-based providers.

Another perspective of analysis that is worth investigating is related to the assumptions of some signalling models, that predict higher prices associated to PMGs for initially lower product prices; hence, we explore the possibility that PMGs may heterogeneously affect prices of goods characterized by different prices at the beginning of the observational period [see Zhuo, 2017].[41] When we estimate our baseline model after splitting the sample according to initial prices, we obtain results shown in Figure 4. Estimates suggest that prices of more expensive products are 6% higher during the policy validity period, while the policy barely affects initially lower-priced products. This result seems to be not fully consistent with theoretical predictions of signalling models (H3).

FIGURE 4 Event Study Representation of the Effect of PMGs on NewEgg's Prices. Cheaper/Expensive Products.



Notes. Estimates from Equation 2 on sub-samples based on product prices at the beginning of the sample period, with NewEgg as the treated sample and Amazon UK as the control one (balanced sample). Cheaper (expensive) products are those with initial prices below (above) the median of the distribution. All specifications include controls (absolute and the relative number of reviews, ratings), as well as day, product and retailer*x*month fixed effects. The DiD estimator by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] is applied. The blue area represents the 90 percent confidence intervals around leads and lags. Standard errors, clustered at product-title level, are based on 100 bootstrap replications.

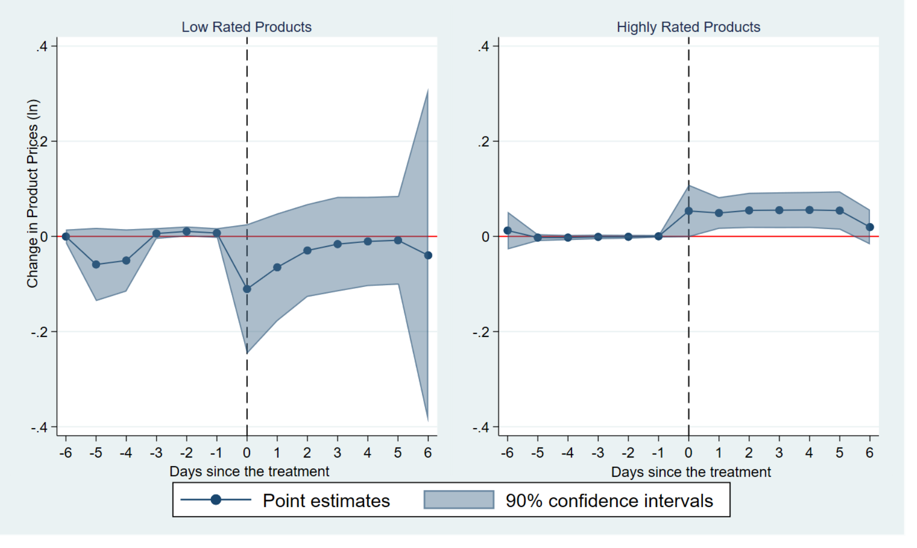
The analysis conducted so far seems to suggest that our findings of higher prices for products targeted by PMGs can be interpreted as consistent with the hypothesis H2, whereby such commercial policies act as price discrimination devices. Tentative evidence on price dynamics of one of the NewEgg's main competitors leads us to rule out the hypothesis that PMGs act as facilitating tools to sustain collusion in our sample, while results from the heterogeneity analysis based on initial product prices differentials is not consistent with the predictions of signalling models. Our main findings can be compared with those obtained by Zhuo [2017] on a large sample of products observed on

[41] After balancing the panel, we therefore define the cheapest products as those whose initial price is below the median of the corresponding price distribution, while the most expensive products are those whose initial price lies above it.

Amazon in 2012. We argue that our results complement these findings, since the author focuses on the non-adopting retailer, while we mainly focus on prices of the adopting one. However, Zhuo [2017] interprets its results as evidence in favour of both collusive and price discrimination theories, while we tend to exclude the interpretation of PMGs as collusive facilitating devices for our sample.[42]

In order to shed further light on the interpretation of our main results, we extend the analysis by studying possible heterogeneous treatment effects. As explained in Section 4, we think it is worthwhile to investigate whether the impact of PMGs on prices varies according to how easily products can be found online, since price discriminating theories of PMGs assume that consumers are characterized by different degrees of search costs; we therefore estimate Equation (2) on different sub-samples built according to products' ratings and visibility. We first distinguish goods on the basis of their ratings, as a proxy of products' popularity. Results reported in columns (2) and (3) of Table 3 suggest that, when PMGs are implemented, for highly rated (popular) items prices increase of about 6.8% (and such effect vanishes after five days from the treatment), while low rated (unpopular) products prices are not affected by the policy. Similar patterns also arise from estimates of Equation2 which include controls, as shown in columns (2) and (3) of Table 4 and in Figure 5.

FIGURE 5 Event Study Representation of the Effect of PMGs on NewEgg's Prices. Low/Highly Rated Products.



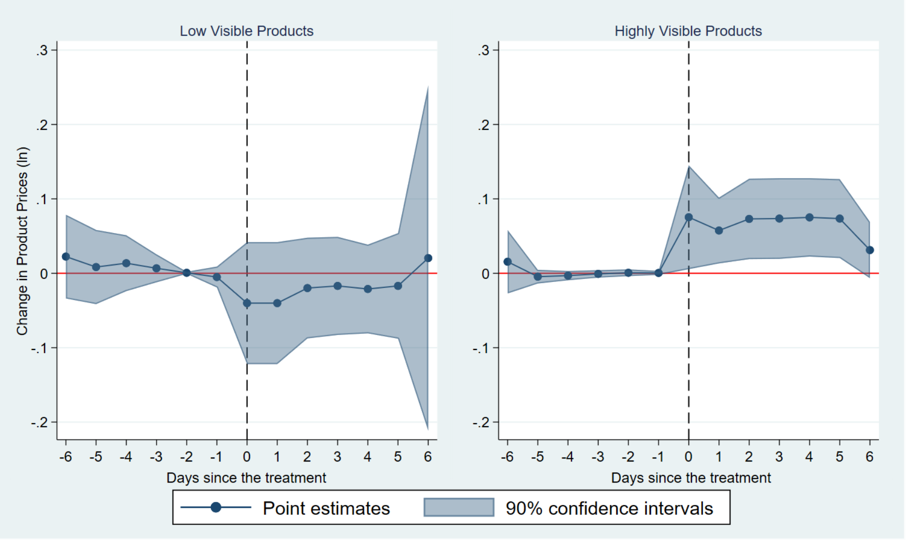
Notes. Estimates from Equation 2 on sub-samples based on products' ratings (Table 4, columns 2 and 3), with NewEgg as the treated sample and Amazon UK as the control one. Highly (low) popular products have ratings higher (lower) than 3.8. All specifications include controls (absolute and the relative number of reviews, ratings), as well as day, product and retailer*x*month fixed effects. The DiD estimator by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] is applied. The blue area represents the 90 percent confidence intervals around leads and lags. Standard errors, clustered at product-title level, are based on 100 bootstrap replications.

Second, we split the sample according to products' visibility, as proxied by the Google search rank described in Section 3. Results reported in columns (4) and (5) of Tables 3 and 4 suggest that highly visible goods experience price increases of about 9% when PMGs are in place, while low visible ones are not affected by the policy. Again, as shown in Figure 6,

[42]The consumer electronic sector has also been investigated by Chen and Liu [2011], that analyze the impact of Most Favoured Customer Clauses (MFCs) policies jointly applied with PMGs. The authors find lower prices for treated products for all competitors; however, they do not provide a formal model to support such results.

estimates confirm the presence of dynamic treatment effects starting from the implementation day up to 5 days later. Third, since products' ratings and visibility are correlated in our sample, we estimate Equation (2) after splitting the sample according to both product characteristics jointly considered. Results shown in columns (6) and (7) of Tables 3 and 4 suggest that, when PMGs are implemented, NewEgg's prices for highly rated-visible items significantly increase of about 9%, while we do not find any effects for low rated-visible products. Once again, estimates shown in Figure 7 provide evidence in favour of significant dynamic treatment effects, lasting for 5 days after the treatment.[43]

FIGURE 6 Event Study Representation of the Effect of PMGs on NewEgg's Prices. Low/Highly Visible Products.



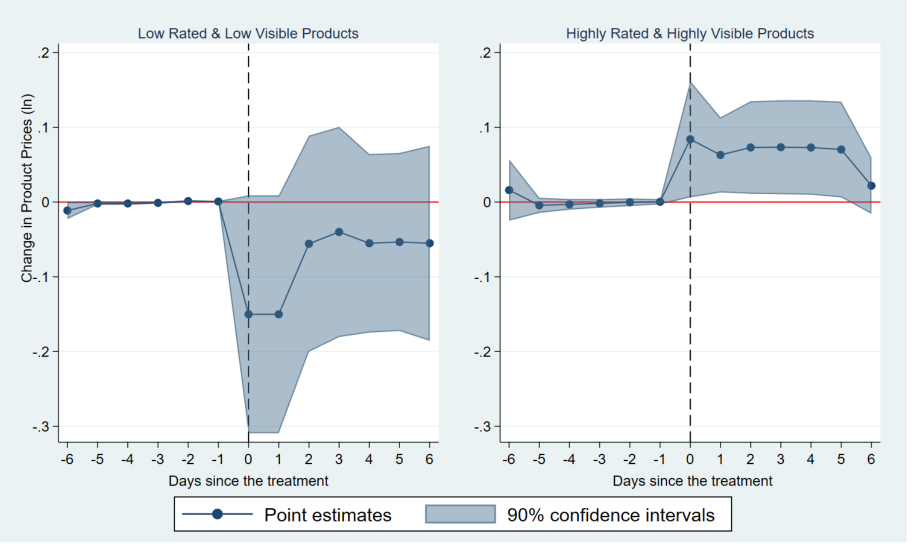
Notes. Estimates from Equation 2 on sub-samples based on products' visibility (Table 4, columns 4 and 5), with NewEgg as the treated sample and Amazon UK as the control one. Highly (low) visible products have a normalized search index higher (lower) than 0.7. All specifications include controls (absolute and the relative number of reviews, ratings), as well as day, product and retailer*x*month fixed effects. The DiD estimator by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] is applied. The blue area represents the 90 percent confidence intervals around leads and lags. Standard errors, clustered at product-title level, are based on 100 bootstrap replications.

The heterogeneity analysis highlights that results found for the full sample are mainly driven by highly rated (popular) and visible products. From the one hand, our findings suggest that PMGs do not have a statistically significant impact on pricing of low popular and low visible items; we argue that such kind of products are more likely to be discovered mainly by informed (low search cost) consumers, that usually shop around and are more price sensitive and more prone to invoke the guarantees, thus making price discrimination ineffective.[44] From the other hand, empirical results for highly rated and visible products provide further evidence in favour of the hypothesis of PMGs acting as price discrimination tools (H2). These products can be easily found by all type of consumers and uninformed buyers

[43] It is worth noting that the parallel trends assumption is also fulfilled for the different sub-samples analyzed, as shown by estimated coefficients associated to event study leads, that turn out to be not statistically significant in all regressions. The parallel trends test that computes *p*-value of the joint test that all leads are equal to 0, reported at the bottom of Tables 3 and 4, reveals the absence of any anticipatory effect.

[44] It should be noted that point coefficients for low rated/visible products are poorly estimated; indeed, in our sample, this class of products is not only less represented, but also less likely to be targeted by PMGs (on average, 1 run of treatment that lasts 6 days), while the latter are applied more frequently to highly rated, popular and visible products (3 treatment's runs lasting 41 days on average). This might explain the lack of goodness of fit.

FIGURE 7 Event Study Representation of the Effect of PMGs on NewEgg's Prices. Low/Highly Rated and Visible Products.



Notes. Estimates from Equation 2 on sub-samples based on products' ratings and visibility (Table 4, columns 6 and 7), with NewEgg as the treated sample and Amazon UK as the control one. Highly (low) popular products have ratings higher (lower) than 3.8. Highly (low) visible products have a normalized search index higher (lower) than 0.7. All specifications include controls (absolute and the relative number of reviews, ratings), as well as day, product and retailer*x*month fixed effects. The DiD estimator by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] is applied. The blue area represents the 90 percent confidence intervals around leads and lags. Standard errors, clustered at product-title level, are based on 100 bootstrap replications.

(who do not to shop around) might be charged higher posted prices throughout the policy validity period, whereas informed ones (who are more likely to search around) could obtain a reduced selling price by invoking the guarantee. Indeed, as highlighted by Moorthy and Winter [2006], the success and profitability of price discrimination policy (for popular products) is dependent on the presence of a sufficient number of uninformed consumers purchasing products at higher costs.

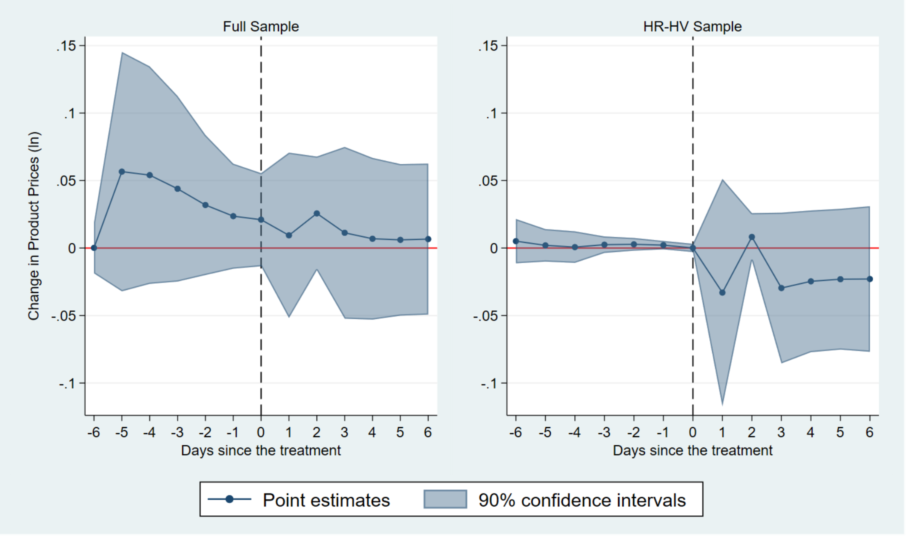
Before turning to discuss the robustness analysis, some caveats are worth making on the interpretation of our results. First, the price discrimination PMGs theoretical models require a significant percentage of customers invoking the guarantee; however, we do not have data on this issue. Comfortingly, Moorthy and Winter [2006] observe redemption rates ranging between 1% and 25% on a sample of 46 Canadian retailers, and suggest that percentages above 10% are compatible with the aforementioned hypothesis. We believe that it is reasonable to expect similar (or higher) redemption rates for online markets. Second, these theoretical models have been proposed for physical markets, that differ from online ones along different dimensions. For example, information is more easily available in online markets and the process of evaluation of search goods is potentially cheaper and faster thanks to UGCs availability; moreover, menu costs are lower, so that commercial policies can be more easily implemented. Indeed, it would be interesting to develop a theoretical model that analyzes PMGs by properly taking into account peculiarities of online markets.

As a final remark, we note that our analysis might be related to the strand of literature that explains the source of price stickiness in online markets. Gorodnichenko and Talavera [2017] analyze prices for a broad set of products recovered on a leading price comparator website over a period of five years, and find that the magnitude of pricing fluctuations in online shops is significantly lower with respect to physical retailers and that price changes are more frequent in online stores. The authors argue that, despite online prices are more flexible, it is still possible to observe some rigidity related to adjustment costs that can be more complicated than just physical menu expenditures, since they can include costs for information collection and processing. Along these lines, Ellison et al. [2018] build a dynamic model of boundedly rational managers, and ascribe price stickiness to frictions in managerial behavior. Such arguments might explain our findings of dynamic treatment effects lasting about five days from the treatment. However, such issue would deserve further analysis, also based on a wider sample observed over a longer period.

5.1 | Robustness Analysis

In this Section we discuss empirical results obtained by conducting an in-depth robustness analysis. We first estimate the more extended specification of Equation 2, including control variables, for both the full sample and the sub-sample of highly rated-visible products (HR-HV), after introducing *fake* treatment timings. The latter are drawn from Bernoulli distributions, with parameters p (probability of success) derived from the $PMG_{i,r,t}$ sampling distribution. In this setting, we should not observe any significant effect of *fake* PMGs on NewEgg's prices: comfortingly, results reported in Figure 8 confirm this prediction.

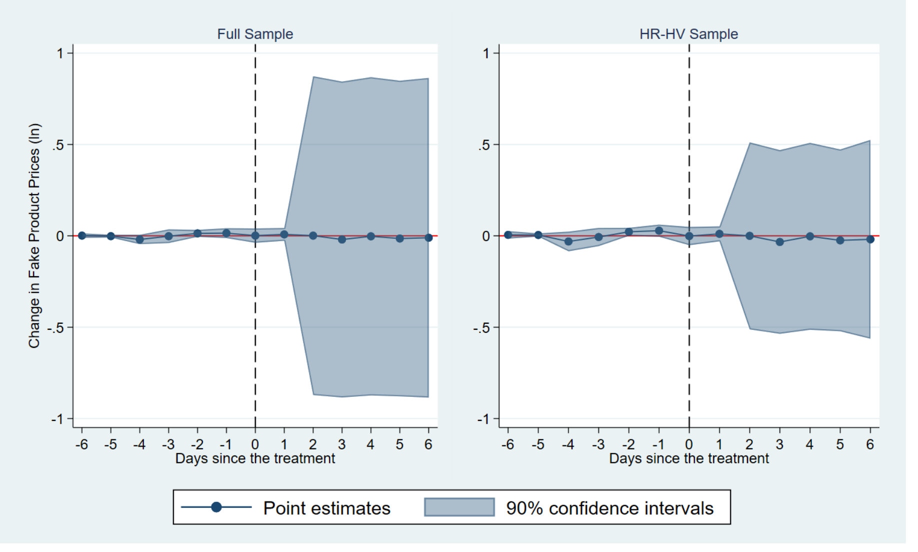
FIGURE 8 Event Study Representation of the Placebo Effect of *Fake* Treatments on NewEgg's Prices.



Notes. Placebo estimates from Equation 2 on the full sample and for highly rated/visible products, with NewEgg as the treated sample and Amazon UK as the control one. *Fake* treatments are drawn from Bernoulli distributions with parameters p (probability of success) derived from the sample distributions of $PMG_{i,r,t}$. Highly (low) popular products have ratings higher (lower) than 3.8. Highly (low) visible products have a normalized search index higher (lower) than 0.7. All specifications include controls (absolute and the relative number of reviews, ratings), as well as day, product and retailer$x$month fixed effects. The DiD estimator by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] is applied. The blue area represents the 90 percent confidence intervals around leads and lags. Standard errors, clustered at product-title level, are based on 100 bootstrap replications.

Second, our results are confirmed when we estimate Equation 2 after substituting the dependent variable with a *fake* outcome, where *fake* product prices are drawn from product specific random distributions resembling sampling ones (same mean and variance). Also in this case we should not observe any significant effect of NewEgg's PMGs on

FIGURE 9 Event Study Representation of the Effect of the Treatment on *Fake* Prices.



Notes. Placebo estimates from Equation 2 on the full sample and for highly rated/visible products, with NewEgg as the treated sample and Amazon UK as the control one. *Fake* prices are drawn from random price distributions resembling sample ones (same mean and variance). Highly (low) popular products have ratings higher (lower) than 3.8. Highly (low) visible products have a normalized search index higher (lower) than 0.7. All specifications include controls (absolute and the relative number of reviews, ratings), as well as day, product and retailer*x*month fixed effects. The DiD estimator by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] is applied. The blue area represents the 90 percent confidence intervals around leads and lags. Standard errors, clustered at product-title level, are based on 100 bootstrap replications.

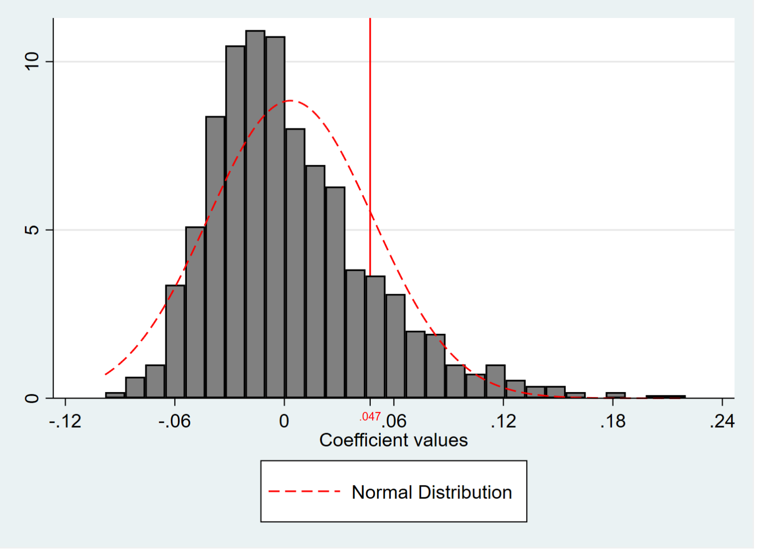
fake prices. Results shown in Figure 9 confirm the absence of any impact of NewEgg's PMGs on the *fake* outcome.

Third, we provide a random allocation test by building a placebo treatment variable that takes on value 1 if random numbers drawn from a uniform distribution [0,1] are greater than the sampling treatment probability. We then estimate Equation (2) relying on the new *fake* treatment indicator and we iterate such procedure 1,000 times, in order to obtain a distribution of placebo average treatment effects to compare with the estimated average value observed in column (1) of Table 4 ($ATE = 0.047$). The rationale is that a statistically significant average treatment effect should be different with respect to those obtained with placebo estimates. In Figure 10 dark bars represent the distribution of estimated placebo coefficients obtained with such iterative method, while the vertical red line shows the estimated average treatment effect based on actual treatments. It is worth noting that placebo coefficients are almost normally distributed, with estimates centered at zero, thus highlighting no treatment effects under the hypothesis of *fake* treatment assignments. However, in the various simulations it can happen that, by chance, some placebo estimates are very similar to the "true" one, making the "true" effect fall back inside the range. Overall, this random allocation test supports the robustness of our main findings.

As a further robustness check, we analyze if our main findings are driven by the inclusion of a particular product and we estimate Equation (2) after dropping one product at a time. Estimates, available upon request, show that this is not the case. Moreover, we explore different specifications of the event study regression in Equation 2; in particular, we consider specifications with three and eight leads and lags around the treatment period, respectively. Comfortingly, results reported in Appendix B are unchanged. Finally, it is worth noting that overall results are confirmed when we replicate our analysis by relying on Amazon US as an alternative control sample, as reported in Appendix B.[45]

[45]We have also replicated the heterogeneity analysis after using the number of products' reviews as an alternative measure of products' popu-

FIGURE 10 Placebo Plot Test. Random Treatment Allocation.



Notes. In this test we randomly assign treatments timings by generating simulated values for the dummy $PMG_{i,r,t}$. In particular, we build a *fake* $PMG_{i,r,t}$ equal to 1 if random numbers drawn from a uniform distribution [0,1] are greater than the sample treatment probability. We then estimate Equation (2) after including the new treatment indicator, $PMG^{fake}_{i,r,t}$ and we iterate such procedure 1,000 times in order to obtain a distribution of placebo coefficients to compare with the average estimated value shown in column (1) of Table 4. Dark bars represent the distribution of estimated placebo coefficients. The vertical solid red line represents the estimated average treatment effect ($ATE = 0.047$).

6 | CONCLUSIONS

In this study we empirically investigate the effects of Price Matching Guarantees (PMGs) commercial policies on daily prices of a representative sample of consumer electronics products observed on the US NewEgg retailing website, between May and October 2018. In order to perform the causal analysis we also recover price data for the same treated products but sold on another retailing websites, namely Amazon UK and Amazon US, which never offer such policies. In the light of the theoretical literature on PMGs, we test the hypothesis that these commercial policies may alternatively act as collusive devices (H1), price discriminating tools (H2) or signals of low prices (H3). Our identification strategy is based on a Difference-in-Differences (DiD) research design where the application of the commercial policy represents the treatment of interest. Since the observed treatment design is quite complex, whereby products repeatedly go in and out of treatment status, we apply the DiD estimator proposed by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022], that properly takes into account for heterogeneous and dynamic treatment effects. Moreover, such approach provides estimates of event study DiD regressions that allow us to test for the parallel trend assumption and, therefore, the validity of our research design.

Estimates conducted over the full sample provide evidence in favour of a positive treatment effect on NewEgg prices of about 4.7% during the policy validity period and suggest that such effect persists for five days after the treatment; conversely, NewEgg's PMGs do not seem to affect prices of its main competitor, namely Amazon US. Further estimates suggest that price of products with relatively higher initial prices increase of about 6%, while cheapest ones seem to be not affected by the policy. The heterogeneity analysis shows that full sample results are mainly driven

larity and all results have been confirmed.

by high popular and highly visible products, that experience a price increase ranging between 7.8-9% when targeted by the PMG policy, while for low popular and low visible products the average treatment effect is not statistically different from zero.

We believe that results obtained over the full sample, together with those stemming from the heterogeneity analysis, provide evidence consistent with the hypothesis that PMGs may act as a price discrimination device, thus allowing sellers to segment the market on the basis of customers heterogeneity in terms of willingness to search and price sensitiveness. Relying on this literature, we argue that informed buyers (with low search costs and high willingness to shop around), may be retained on the retailer website by the possibility to pay lower selling prices by invoking the guarantee, thanks to a lower price posted elsewhere, while uninformed consumers, who have high search costs and that do not shop around, are charged higher posted prices. We argue that popular products are observed by a sufficiently high proportion of uniformed buyers, so that market segmentation is feasible, as suggested by Moorthy and Winter [2006]. Conversely, our results are not fully consistent with alternative theories of PMGs, i.e. the collusive and the signalling ones. On the one hand, we tend to rule out the collusion hypothesis given that we do not find any impact of NewEgg's policies on prices of one of its major competitor, namely Amazon US, despite such issue would deserve further analysis, possibly observing a larger number of competitors. On the other hand, overall results are not fully consistent with the hypothesis that PMGs act as signal of low prices, since prices of product with lower initial prices are not affected by the policy.

Before concluding, it is worth pointing out that it would be interesting evaluating welfare effects of PMGs applied in online markets and characterized by complex implementation designs. It would also be interesting developing a specific theoretical framework that examines PMGs by correctly taking into account the peculiarities of online markets.

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Appendix A

An important issue is related to the representativeness of our sample. In this Appendix we provide a detailed analysis of such an issue. In particular, we first report a list of analyzed products, as shown in Tables A.1 and A.2. Moreover, the top panel of Figure A.1 provides the distribution of products by price classes (10). The histogram highlights that 76% of the products belong to the first two price deciles, with prices ranging between 14$ and 309$, and 15% to the third and the fourth decile (prices between 317$ and 523$); the remaining products are ranked from the sixth to the tenth decile, with price ranging between 834$ and 1,574$. This pattern matches typical price distributions observed in several markets [Coad, 2009], often characterized by a large amount of low cost accessories and few luxury goods. Furthermore, calculating the log-price distribution (bottom panel of Figure A.1) and mapping the integer part of this value on the x-axis, we obtain a septile-partition. By plotting the distribution of products by log-price classes we obtain a distribution that resembles a Normal one. This finding is confirmed by implementing a Shapiro-Wilk W test on the null hypotesis of normality (z stat = 0.536, p-value = 0.296).

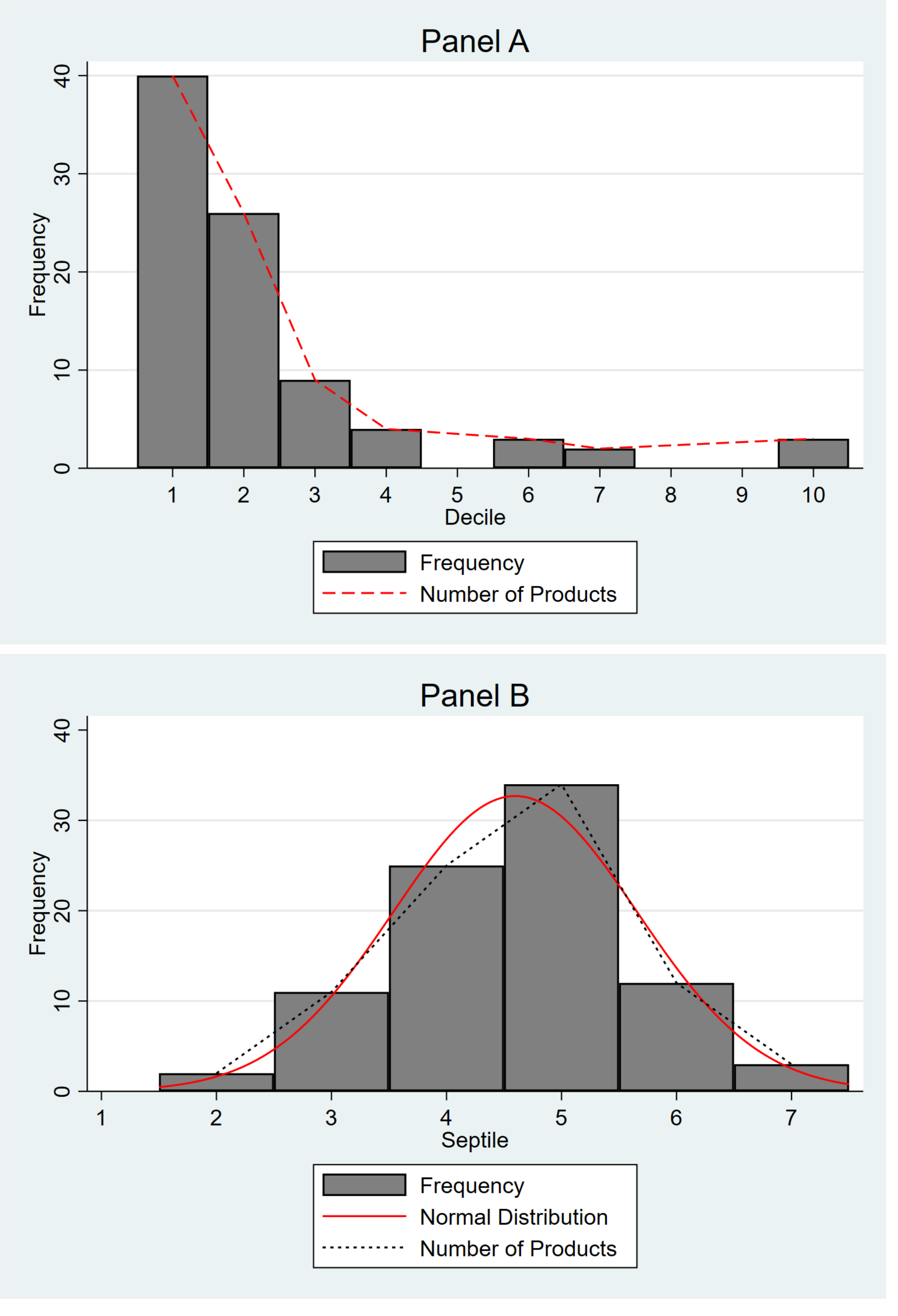
TABLE A.1 Sub-Categories List.

Sub - Categories	Total Products	Treated Products NewEgg	Control Products Amazon UK	Alternative Control Products Amazon US
CPU Processor	9	3	3	3
Computer Case	6	2	2	2
Mobile Phone	3	1	1	1
Scanner	6	2	2	2
Speaker	6	2	2	2
Motherboard	3	1	1	1
Monitor	9	3	3	3
Headset	3	1	1	1
USB Flash	3	1	1	1
CPU Cooler	3	1	1	1
Speaker for Domotic	3	1	1	1
Desktop PC	3	1	1	1
Tablet	3	1	1	1
Desktop PC	3	1	1	1
Laptop PC	3	1	1	1
Power Supply	3	1	1	1
Printer	6	2	2	2
Memory Card	6	2	2	2
Hard Disk	3	1	1	1
Smart Thing Domotic	6	2	2	2
TOTAL	87	29	29	29

TABLE A.2 Treated Products Titles.

Treated Products Titles
AMD Ryzen 5 1500X Processor
Corsair Crystal Series 570X RGB - Tempered Glass; Premium ATX Mid-Tower Case
BlackBerry PRIV (32GB) Verizon Factory Unlocked Phone
Fujitsu fi-7160 Color Duplex Document Scanner
Fujitsu ScanSnap S1300i Instant PDF Multi Sheet-Fed Scanner
Philips BT50B/37 Wireless Portable Bluetooth Speaker
Asus ROG MAXIMUS VIII FORMULA DDR4 ATX Motherboards
ASUS VS247H-P 23.6 Full HD 1920x1080 2ms HDMI DVI VGA Monitor
Samsung Hmd Odyssey Windows Mixed Reality Headset
Samsung 128GB BAR (METAL) USB 3.0 Flash Drive
Corsair CW-9060025-WW Hydro Series Liquid CPU Cooler
Echo Dot (2nd Generation) - Smart speaker with Alexa - Black
ASUS VivoMini Mini PC
Dell XF9PJ Latitude 7490 Notebook
Intel Core i7-8700 Desktop Processor 6 Cores
AMD Ryzen 7 2700X Processor Wraith Prism LED Cooler
Corsair RMx Series RM850 x 80 PLUS Gold Fully Modular ATX Power Supply
ASUS 24-inch Full HD FreeSync Gaming Monitor
Brother Monochrome Laser Printer; Compact All-in One Printer
Team 64GB microSDXC UHS-I/U1 Class 10 Memory Card with Adapter
LG Electronics 21.5 Screen LED-Lit Monitor
HP LaserJet Pro M227fdw All-in-One Wireless Laser Printer
Logitech Z313 Speaker System + Logitech Bluetooth Audio Adapter Bundle
PNY CS900 960GB 2.5 Sata III Internal Solid State Drive (SSD)
Samsung SmartThings ADT Wireless Home Security Starter Kit
Samsung SmartThings Smart Home Hub
Rosewill 2U Server Chassis Server Case (RSV-2600)
Corsair Apple Certified 16GB (2 x 8GB) DDR3 1333 MHz (PC3 10600) Laptop Memory
Acer Iconia One 10 NT.LDPAA.003 10.1-Inch Tablet

FIGURE A.1 Products Distribution by Price Classes and Log-Price Classes.



Notes. The top panel provides the distribution of products by price classes (10). The figure in the bottom panel is obtained by calculating the log-price distribution and mapping the integer part of this value on the x-axis. A septile-partition is shown.

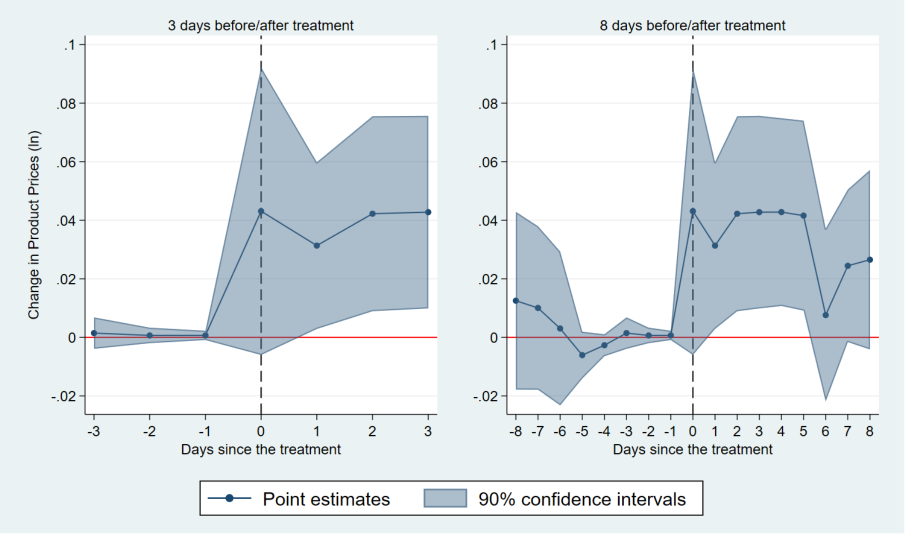
Appendix B

In this Section we present additional empirical results aimed at validating our research design. First, we investigate the issue of treatment strict exogeneity, following the approach suggested by Wooldridge [2010] and based on an augmented fixed effects regression where NewEgg's prices at time t are estimated as a function of the treatment at time t and of a subset of treatment leads from time $(t+1)$ to $(t+6)$:

$$\log Price_{i,r,t} = \gamma_t PMG_{i,r,t} + \sum_{j=1}^{6} \gamma_{t+j} \left(PMG_{i,l,t+j}\right) + X_{i,r,t}^T \beta + \mu_{i,l} + \tau_t + \lambda_{l,m} + \epsilon_{i,r,t} \tag{3}$$

Under the null hypothesis of treatment strict exogeneity, the coefficients γ_{t+j} of the lead terms should be equal to zero. This amounts to test if the current outcome does have predictive power on future treatments, that is indeed what is required by the parallel trend assumption. Estimates of Equation 3 conducted using Amazon UK as control sample lead us to accept the null hypothesis of treatment strict exogeneity, based on a Wald test for the joint significance of leads coefficients (Wald stat. 1.06 with p-value 0.397); identical results are found when we use products sold on Amazon US as alternative counterfactual (Wald stat. 1.29 with p-value 0.277).[1] Comfortingly, the above analysis allows us to exclude that PMGs policies decisions depend on product prices, i.e. there isn't selection into treatment in our sample, thus validating our research design.

FIGURE B.1 Event Study Representation of the Effect of PMGs on Prices. Different Event Study Specifications.

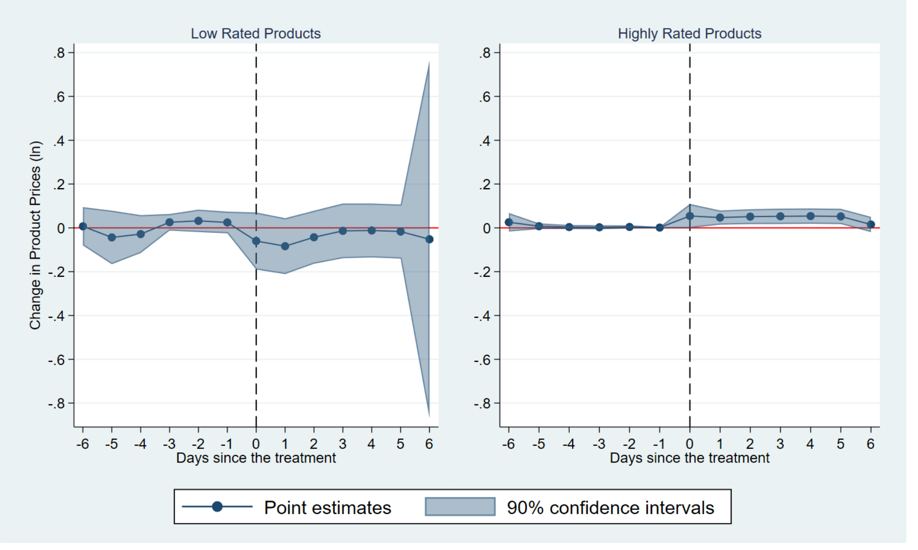


Notes. Estimates of Equation 2 on the full sample, with NewEgg as the treated sample and Amazon UK as the control one. We consider different event study specifications, namely with 3 and 8 leads and lags, respectively. The specifications include controls (absolute and the relative number of reviews, ratings), as well as day, product and retailer$x$month fixed effects. The DiD estimator by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] is applied. The blue area represents the 90 percent confidence intervals around leads and lags. Standard errors, clustered at product-title level, are based on 100 bootstrap replications.

Second, since the effect of NewEgg's PMGs on prices vanishes after five days in our results, as a robustness check

[1] Regression results are available upon request.

FIGURE B.2 Event Study Representation of the Effect of PMGs on NewEgg's Prices. Low/Highly Rated Products. Control Sample Amazon US.



Notes. Estimates from Equation 2 on sub-samples based on ratings, with NewEgg as the treated sample and Amazon US as the control one. Highly (low) popular products have ratings higher (lower) than 3.8. All specifications include controls (absolute and the relative number of reviews, ratings), day and product fixed effects. The DiD estimator by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] is applied. The blue area represents the 90 percent confidence intervals around leads and lags. Standard errors, clustered at product-title level, are based on 100 bootstrap replications.

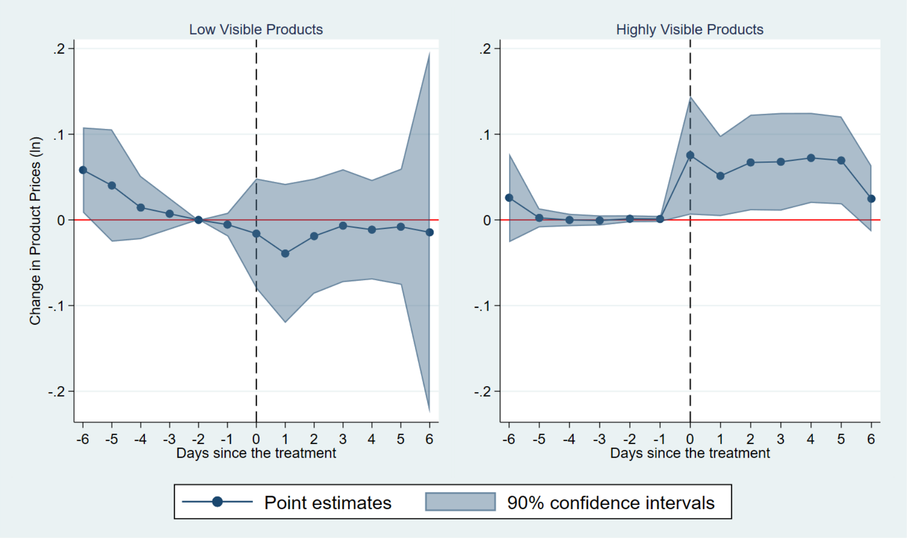
we estimate Equation 2 after including both 3 and 8 periods before and after the treatment, respectively. Figure B.1 provides point estimates that confirm our previous results shown in Figure 1 and Table 4, in the main text.

Third, after having presented in the main text event study plots obtained by estimating Equation 2 over the full sample, with the DiD estimator proposed by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] and using products sold on Amazon US as the control sample (Figure 2), we also estimate Equation 2 on sub-samples of products based on their ratings and product visibility intensities.[2] The analysis over different sub-samples provides support to an heterogeneous impact of the policy, i.e. that prices of low popular and low visible products are not affected by PMGs policies, while the ones of highly popular and highly visible products are significantly higher. In particular, highly popular (highly visible) products experience, on average, 6.5% (8.5%) higher prices during PMGs validity periods. Moreover, if we split the sample according to both features jointly considered, again, we find 8.7% higher prices for highly popular and visible products. It is worth noting that dynamic treatment effects are consistent with those estimated in our main analysis in Section 5, highlighting the robustness of our main findings.[3]

[2] Estimates, not reported, are available upon request.

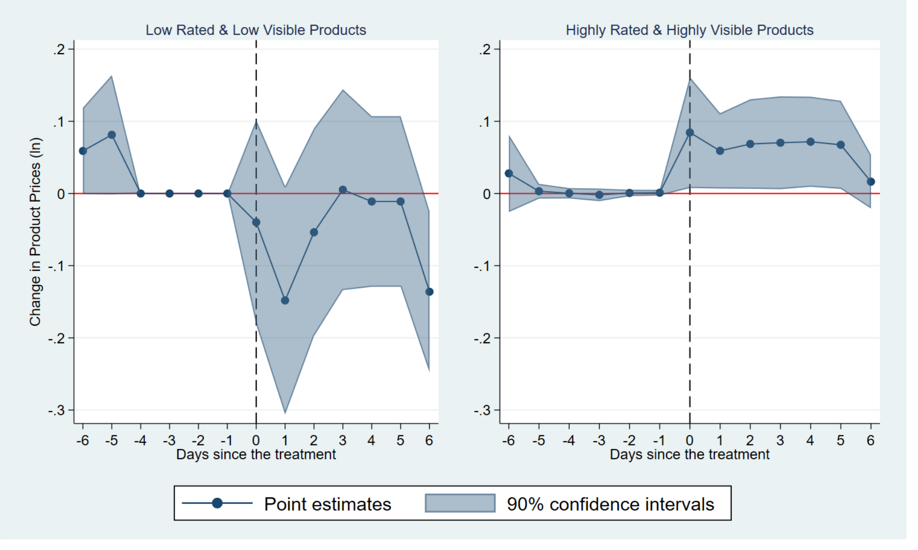
[3] Also in this additional empirical analysis, parallel trends assumptions is fulfilled. Moreover, robustness tests as those discussed in Section 5.1 confirm results obtained by using Amazon US as alternative control sample. Results are available upon request.

FIGURE B.3 Event Study Representation of the Effect of PMGs on NewEgg's Prices. Low/Highly Visible Products. Control Sample Amazon US.



Notes. Estimates from Equation 2 on sub-samples based on products visibility, with NewEgg as the treated sample and Amazon US as the control one. Highly (low) visible products have a normalized search index higher (lower) than 0.7. All specifications include controls (absolute and the relative number of reviews, ratings), day and product fixed effects. The DiD estimator by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] is applied. The blue area represents the 90 percent confidence intervals around leads and lags. Standard errors, clustered at product-title level, are based on 100 bootstrap replications.

FIGURE B.4 Event Study Representation of the Effect of PMGs on NewEgg's Prices. Low/Highly Rated and Visible Products. Control Sample Amazon US.



Notes. Estimates from Equation 2 on sub-samples based on products rating and visibility, with NewEgg as the treated sample and Amazon US as the control one. Highly (low) popular products have ratings higher (lower) than 3.8. Highly (low) visible products have a normalized search index higher (lower) than 0.7. All specifications include controls (absolute and the relative number of reviews, ratings), day and product fixed effects. The DiD estimator by De Chaisemartin and d'Haultfoeuille [2020] and De Chaisemartin and d'Haultfoeuille [2022] is applied. The blue area represents the 90 percent confidence intervals around leads and lags. Standard errors, clustered at product-title level, are based on 100 bootstrap replications.