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Price Matching and Platform Pricing*

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Abstract

In this study we investigate the effects of Price Matching Guarantees (PMGs) commercial policies on U.S. online consumer electronics daily prices. By applying a Diff-in-Diff identification strategy we find evidence in favor of price reductions occurring after the PMG policy is repealed.

We further investigate if such effect is heterogeneous according to products characteristics, by exploiting User Generated Contents (UGCs, as products popularity and quality) and online search visibility measures (Google Search Rank). Estimates suggest that for high quality (visibility) products PMGs policies harms competition by keeping prices high, while for low quality (visibility) products, prices decrease during the policy validity period.

Keywords. Price Matching Guarantees; Online Sales Platforms; Price Discrimination; User Generated Contents; Customers Heterogeneity; Counterfactual Impact Evaluation.

JEL Classification. L11; L15; L20; L81.

1 Introduction

Online sales platforms have recently gained increasing importance in both retail and wholesale markets.[1] Such markets are characterized by the supply of personalized services, more convenient delivery schedules and the ability to reach a very high number of consumers. In addition, platforms claim to warrant lower prices with respect to traditional stores through the provision of offers, promotions,

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[1] The term "online platform" identifies a range of digital services that facilitates interactions between two or more distinct but interdependent sets of users (whether firms or individuals) who interact through the service via the Internet (OECD, 2019). Online sales platforms can operate as online retailers, as a marketplace for third-party sellers or they can offer both services.

down prices and other price discounting policies. Among these options, online sales platforms often implement Price Matching Guarantees (PMGs) policies, that is the promise to reimburse price differences when competitors offer a lower prices.[2]

PMGs policies are surely appealing for customers and can increase consumer confidence and brand fidelity. However, the announcement to tie prices to those of competitors can have anti-competitive effects and sustain high prices, thus harming consumers' welfare.

Most of theoretical literature agrees on the fact that PMGs reduce firms' incentive to compete on prices and lower the motivation for consumers to search better sale conditions (Hay, 1981; Salop, 1986). However, in some models, PMGs are considered as tools for price discriminating or as real discounting policies (Png and Hirshleifer, 1987; Belton, 1987). Therefore, empirical analyses become particularly relevant in order to understand under what conditions such pricing policies reduce consumers' welfare. Indeed, the applied literature analyzing this issue is scant and does not provide conclusive results (Mago and Pate, 2009; Zhuo, 2017).

Our work adds to this literature by providing empirical evidence on the effect of platforms' PMGs policies on daily consumer electronics prices observed on US online market. We have focused on the NewEgg platform that exclusively sells consumer electronics products and implements PMGs policies that turn on and off over time (blinking PMGs). Given that our identification strategy is based on a comparison of price levels before and after the policy shutdown, we excluded platforms that never stop offering PMGs (like Target).

In particular, we apply a Diff-in-Diff (DiD) approach where we consider as the treated sample the pool of NewEgg products interested by PMGs policies. Differently from standard practices in studies adopting a DiD approach, we build the control sample with price data for the same products observed on a different platform, namely Amazon, that never offers PMGs to customers. Furthermore, in order to ensure that our counterfactual sample is less likely to be influenced by the PMG policy adopted by NewEgg, we have considered data from the Amazon UK platform, instead of Amazon US. Indeed, price observed on Amazon US might not be completely independent from the policy under scrutiny, because of price tracking practices frequently adopted by platforms.

Estimates provide evidence in favor of an average price reduction of about 3.9% after the interruption of the PMG policy. However, in order to have a more detailed picture of the issue, we investigate if such effect is heterogeneous, depending on products characteristics. In particular, we focus on products features that might affect the outcome of PMGs policies and that can be recovered exclusively on online markets. Platform data allow us to obtain information from User Generated Contents (UGCs), like products' popularity and quality, as well as online search visibility (Google Search Rank); indeed, we believe that these product characteristics might provide indirect information on consumers heterogeneity. Estimates conducted on specific sub-samples show that when the PMG is interrupted, low quality (and low search rank) products experience a price increase of about 3.4%, while for high quality (and high visibility) products a price reduction of about 3.7% is observed. These findings are in line with the lack of unambiguous predictions of the theoretical literature.

The anti-competitive effects of PMGs observed for high quality (visibility) products has been predicted by theoretical models where such polices make collusion more likely (Hay, 1981; Salop, 1986; Cabral et al., 2018). These findings can be also explained by the theoretical predictions of a class of models, like Corts (1997) and Nalca et al. (2010), where PMGs are tools for discriminating customers according to their sensitiveness to price and products quality. These models also explain our results obtained for products characterized by low visibility (quality). Indeed, the willingness to engage in

[2]For example, 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/`.

search activity could indirectly identify those customers whose demand is more rigid, as argued by the search literature (Ellison and Ellison, 2009).

The analysis conducted in this study enriches the literature on price effects of PMGs by using very detailed platform daily price data for a specific market (consumer electronics) where such policies are very common. First, our work overcomes previous research (see Zhuo, 2017) by using real-time data instead of historical information. This makes the use of price tracking websites and algorithms that extract data from price charts outdated. Second, the DiD identification strategy adopted is based on the construction of a control group with a novel approach; finally, products characteristics based on Users Generated Contents (UGCs) are employed for the first time in order to study possible heterogeneous effects of such policies.

The rest of the paper is organised as follows. In the next section we briefly discuss related literature and in Section 3 we accurately describe the data extraction process and the variables used in the empirical analysis. Section 4 explains our identification strategy and in Section 5 we discuss estimates results and robustness analysis. Section 6 concludes.

2 Literature Review

The theoretical literature has analysed possible impacts of PMGs on different market outcomes, since such commercial policies might affect the behaviour of firms and consumers in different ways.
The most common prediction of the theoretical models is that PMGs hamper competition by keeping prices high and sustaining collusive practices; moreover, some authors suggest that they might be tools for realising price discrimination or signalling cheap prices.

Hay (1981), Salop (1986) and Belton (1987) have first suggested that PMGs can sustain collusion in oligopoly models; they highlight that such clauses might be considered as threats to punishment for firms that lower cartel prices, thus reducing firms' incentive to deviate from the agreement. They argue that, if all competitors in the market adopt a PMG policy, none of them has the incentive to lower its price and the latter tends to the monopolistic level. Moreover, they agree on the fact that the adoption of such policies increase the stability of the cartel, as any price cut must be refunded to the consumers, so that the policy generates a credible penalty system.
Several other papers support the pro-collusive argument by extending the basic oligopolistic setting (see, among others, Doyle, 1988; Logan and Lutter, 1989; Baye and Kovenock, 1994), while other authors explore the impact of PMGs extending the analysis in dynamic, multi-stage and Hotelling frameworks (see, among others, Chen, 1995; Lu and Wright, 2010; Hviid and Shaffer, 2010; Pollak et al., 2017; Constantinou and Bernhardt, 2018). Cabral et al. (2018) suggest that a PMG can be a collusion enacting practice. In the model two firms alternate over time in setting prices; given that starting a collusion process implies several risks, like for example antitrust penalties, firms include collusion costs in their decisions. The main prediction of the model is that the probability of tacit collusion rises when the policy is in place.
In studies reviewed so far, it is implicitly assumed that customers automatically claim the price guarantee whenever they find a price differential: indeed, this is not always the case, because of lack of information or because there are small costs for the customer to activate a guarantee, the so called "hassle costs". Hviid and Shaffer (1999) highlight that the presence of hassle costs undermines possible anti-competitive effects of PMGs, but do not completely cancel them. Precisely, with symmetric firms PMGs are unable to support any price increase in presence of hassle costs. Indeed, each firm will be interested in lowering price levels by an amount that is marginally smaller than these costs, so that buyers are attracted from cheaper firms and do not activate the guarantee. Otherwise, with asymmetric firms, a rise in prices might be supported, but not at the monopolistic level. Moreover, their model can explain why universal adoption is not a realistic assumption of previous studies.

Some other models explore the possibility that sellers use PMGs policies as a price discrimination tool. If customers are different in terms of some subjective characteristic, like information on prices and guarantee terms, degree of loyalty to a specific retailer or level of hassle costs in requesting the refund, firms could use the price guarantee to discriminate between different groups of consumers.
Png and Hirshleifer (1987), Belton (1987) and Corts (1997) first suggested duopoly models where firms discriminate between different consumers groups, namely "unsophisticated" customers and "sophisticated" ones. Consumer segmentation and PMGs allow firms to set higher prices for unsophisticated consumers, while sophisticated ones benefit from the lowest price guaranteed by the policy. The main intuition from this strand of literature is that price discrimination might at least benefit some customers with actually lower prices.[3]
Finally, Moorthy and Winter (2006) suggest that PMGs might be a credible signal of low prices, if low cost firms adopt the policy and (high cost) competitors can not match the policy. Similarly, Jain and Srivastava (2000) develop a theoretical model that identifies the conditions under which PMGs might lead to lower market prices.[4] In the presence of informed and uninformed consumers (about prices and store characteristics) and of different kind of stores (in terms of size, service quality and so on), only stores with low prices offer PMGs policies.[5]

Despite the theoretical literature is rich and analyses several aspects of PMGs policies, the empirical evidence is scant and does not provide conclusive results. Some studies focus on specific markets, like tyre or gasoline, while others analyze retailing prices from supermarkets, grocery stores or online platform markets.
Analysing daily price quotes from the tyre industry advertisements, on 61 US Sunday newspapers observed for three months in 1996, Arbatskaya et al. (2000), through a Feasible Generalised Least Square approach, 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.[6] Cabral et al. (2018), focus instead on daily pricing policies adopted by the Shell network of gas stations in Germany in 2015.[7] Leveraging on gas stations localization and consumers demographics as sources of identification, they suggest that PMGs can be a collusion enacting policy. 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. Similar results can 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 the differences between own prices with the competitors ones, around the time period where collusive price increases occurred.
Hess and Gerstner (1991) analyse 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. Authors, by means of a time series analysis, provide evidence in favor of higher prices of about 1-2% when the guarantee is introduced. Moorthy and Winter (2006) argue instead that the adoption or non-adoption of the PMG might be interpreted as a way to signal the seller service–price profile.[8] Authors analyse data for several product categories from 46 Canadian retailers observed in 2002. They assume the existence

[3] Similar results can be found also in Edlin (1997) and Nalca et al. (2010).

[4] Authors have realized two experiments to analyze the effects of PMGs policies on prices' consumer perceptions and have shown that consumers expect lower prices from PMGs.

[5] Similar results can be found also in Moorthy and Zhang (2006).

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

[7] See also Wilhelm (2016).

[8] The authors refer to the retailer service-price profile as to any sellers characteristic that might induce customers to choose one seller over another one, like better sales assistance and customer care, a clear Web site, personalised delivery and selling services.

of informed and uninformed consumers and show that PMGs might be a tool to signal low prices to uninformed consumers. In particular, they find that PMGs are mainly adopted by low cost/low service chain stores. Similar results can be found in Chung et al. (2016) for three leading hypermarkets in Korea. Finally, Zhuo (2017) focuses on online platforms 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 Amazon prices; by applying DiD and RDD methods, the author suggests that prices increase by about six percentage points during the period of validity of the policy.[9] Moreover, the analysis highlights an heterogeneous impact of PMGs, with larger price increases for initially lower-priced goods.[10]

3 Data

3.1 Data Extraction

In order to study the impact of PMGs on prices, we focus on the online consumer electronics market, since it is one of the most widespread sector on online retailing and is often interested by such pricing policies. In particular, electronic products are search goods, whose quality can be evaluated before the purchase: the advent of online markets has made this process much cheaper and faster and is most likely to affect the impact of such policies, whose outcome depends, among other factors, by the level of search and hassle costs. Moreover, electronic goods are barely affected by seasonal effects, so that prices signals are more stable over time and show low price differentials across countries (Gorodnichenko and Talavera, 2017; Stallkamp and Schotter, 2019). These characteristics allow us to improve the identification strategy through the construction of a more refined control group (see Section 4 for a rationale.).

Among different online retailing platforms, we choose to focus on NewEgg, a leading online US retailer of consumer electronics products, that implements a so-called "blinking" PMG, i.e. a price guarantee that turns on and off over time on selected items. Given that our identification strategy is based on the comparison of prices before and after a policy shutdown, we do not consider platforms that apply PMGs to wide groups of products continuously over time (i.e. Target, among others). In particular, NewEgg communicates the period of validity of the price guarantee by means of a label that appears on the specific product online page; the customer who discovers the PMG badge has 14 calendar days of time to find the same title at a lower price from US competitors belonging to a declared list.[11] PMGs policies are often repeated over time on the same products without any notice, so that consumers looking for deals have to exert an higher effort in the search process.

In order to build the sample we have identified 100 NewEgg products interested by PMGs on May 10^{th} 2018. For such products we have collected price data and the presence of the NewEgg's PMG badge until 31^{st} October 2018. We identify as the treatment of interest the interruption of the PMG policy, so that prices observed on the NewEgg platform represent the treated sample. The control sample has been built by recovering price data for the same products observed on NewEgg but sold on the Amazon

[9]Similar results can be found also in Wu et al. (2015), Haruvy and Leszczyc (2016).

[10]Some other authors 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. Experimental literature also focuses on the effect of price matching and price-beating guarantees: however, experimental results lack the complexity of real interactions between sellers and consumers.

[11]With title we refer 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://kb.newegg.com/knowledge-base/price-match-guarantee/).

UK platform, that never offers PMGs policies.[12] This reduces the number of observed products, so that the final sample includes 29 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). In the Appendix B we provide a detailed list of selected products (Tables B1 and B2). It is worth noting that both Amazon and NewEgg operate either as online retailers or markeplaces for third-party sellers who pay fees and royalties to access to the customer base. In such marketplace, online platforms often act only as a payment intermediary and goods are kept in the third-party sellers inventory. Thus, in order to build a valid control group, we have excluded data on products sold by third-party sellers on both Amazon and NewEgg platforms.

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 page and capable to recover the data without stressing the site, thus limiting the risk of interruptions due to firewalls.[13] In particular, the scraping process has been supported by several alert tools signalling periodical changes of the internal page structure.[14]

The process of data collection has required the daily implementation of these main steps:

i Sign up for subscription to Amazon Web Service (AWS) cloud, in order to use virtual servers in which to install and launch the program;[15]

ii Accept the norms and terms of use of the platform site, in order to be compliant to the server navigation policies;

iii Launch the daily loop process, in order to navigate among product pages, select the field tags, get the data and save on a server disk. Each scraping session runs about 20 minutes every day.

In addition to products daily prices retrieved on both platforms, we also collect several product characteristics available exclusively on online sales platforms. In particular, we recover some User Generated Contents (UGCs), like the absolute number of reviews received by the specific product under consideration and the most popular one in the same subcategory, as the product rating; moreover, from Google we perform and collect a product's search rank.
The absolute number of reviews is a dynamic information which represents a sort of popularity index, since it is proportional to the product market diffusion.[16] We also calculate the relative number of reviews as the ratio between the number of reviews of each product and the amount of reviews received from the most popular product in the same subcategory.[17] This normalized index, that ranges from zero to one, shows the relative popularity of the product with respect to the other items of the same sub-category. We also collect data on product ratings (stars) provided by consumers. We consider the number of stars gained by each product, ranging from zero (low quality) to five (high quality), as a proxy of product quality. Finally, we develop a search index as a proxy of the time spent on search engines to discover the page of a certain product. More precisely, the search index represents the probability to find the product in first ranked positions of Google results.[18]

[12]See Section 4 for a rationale on this choice.

[13]A typical problem is to intercept daily changes of web pages not only about prices, but also concerning other dynamic contents, such as the number of customer reviews, the average rating and so forth. Code available from authors upon request.

[14]Indeed, 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.

[15]AWS is a comprehensive, evolving cloud computing platform provided by Amazon.

[16]In online commerce, product reviews are used by retailing platforms to give consumers an opportunity to comment on products they have purchased, right on the product page.

[17]See Table B1 in Appendix for details.

[18]The ranking position of an item is retrieved launching the Google query composed by the sentence:

It is worth noting that, although the products analysed are sold by Amazon and NewEgg in different countries, information on some of the considered UGCs (e.g. rating) maintain their consistency. This property is typical of consumer electronics goods that have a standardized nature. Concerning the search index, we adopt a country specific value by launching the Google search engine with specific country settings (UK and US).

3.2 Descriptive Statistics

Our sample consists of 9028 daily price observations (174 days) for 29 products observed on NewEgg and Amazon UK platforms, from 10 May 2018 until 31 October 2018. Table 1 shows the mean and the standard deviations of prices and selected product characteristics for the overall sample and for the treated and control group ones. Prices show a large variability, being the average for the overall sample $240.43 and the standard deviation $283.53. By comparing average values observed over the two platforms, it emerges that both prices and UGCs display similar values, thus confirming what has been observed by the previous literature on the low dispersion of consumer electronics prices across countries (Stallkamp and Schotter, 2019); moreover, such similarities support our approach for the choice of the control sample. As Table 2 shows, in the case of the treated sample (NewEgg) the average price during the policy validity period (before treatment) is about $18 higher with respect to the post implementation period.[19]

Table 1: Summary Statistics. Treated and Control Samples.

Variables	Full Sample	Amazon UK	NewEgg
Provider Price ($)	240.43	227.72	253.15
	(283.53)	(262.74)	(302.39)
Product Popularity (0-1)	0.23	0.26	0.20
	(0.27)	(0.30)	(0.23)
Search Rank (0-1)	0.75	0.85	0.64
	(0.30)	(0.17)	(0.36)
Rating (0-5 stars)	4.14	4.14	4.15
	(0.68)	(0.48)	(0.83)

Table 2: Summary Statistics. NewEgg. Pre and Post Treatment.

Variables	Pre Treatment	Post Treatment
Provider Price ($)	231.22	209.63
	(329.01)	(235.21)
Product Popularity (0-1)	0.22	0.19
	(0.25)	(0.23)
Search Rank (0-1)	0.77	0.71
	(0.26)	(0.31)
Rating (0-5 stars)	4.16	4.06
	(0.98)	(1.05)

Notes: The pre-treatment period is the policy implementation period.

"the name of product" AND "the name of platform". The resulting position is then normalized, mapping in the probability range [0,1].

[19]We remember that our treatment is the policy shutdown.

Table 3: Summary Statistics. Sub-Samples.

Variables	Low Quality Low Search Rank		High Quality High Search Rank	
	NewEgg	Amazon.uk	NewEgg	Amazon.uk
Provider Price ($)	206.02	95.81	221.06	161.10
	(143.92)	(35.11)	(354.69)	(163.90)
Product Popularity (0-1)	0.01	0.01	0.28	0.29
	(0.01)	(0.01)	(0.25)	(0.33)
Search Rank (0-1)	0.19	0.38	0.90	0.91
	(0.27)	(0.38)	(0.04)	(0.05)
Rating (0-5 stars)	2.93	3.19	4.40	4.45
	(0.71)	(0.32)	(0.49)	(0.20)

Notes: For high quality products we mean those with ratings higher than 4. For high visibility products we mean those with a normalized search index higher than 0,8.

In order to investigate the issue of heterogeneity in the effect of PMGs policies on prices, we distinguish products according to products characteristics recovered from UGCs. In particular we classify products depending on their quality and visibility, measured trough UGCs as explained in Section 3.1. Given that quality assessment by consumers is highly correlated to products visibility, in Table 3 we show some descriptive statistics for products classified according to such characteristics.[20] Again, data show that products characteristics stemming from UGCs are quite similar across countries/platforms.

As far as the PMG policy is concerned, NewEgg adopts a blinking strategy, so that the policy is applied in a non continuous way, often to the same products. Table 4 shows the total number of days of treatment (absence of PMGs) and the average number of treatments occurred in each sample. This latter information suggests that, on average, the policy is applied to each product twice during the sample period (174 days) and such frequency does not seem to be correlated to products quality and visibility. Indeed, since prices are highly correlated to quality, we can reasonably assume that there is not selection into treatment associated to products price or quality (visibility), so that the assumption of random assignment required by the identification strategy seems reasonably fulfilled. On the other side, it seems that, for low quality (and visibility) products, the policy implementation period is longer.

Table 4: Summary Statistics. PMGs.

Variables	Full Sample	Low Quality Low Search Rank	High Quality High Search Rank
Treatment Duration (# days)	38.25	58.13	29.30
	(39.94)	(43.37)	(33.54)
Number of Treatments	2.38	1.81	2.78
	(1.75)	(0.82)	(2.06)

Notes: Treatment duration is the average number of days without PMGs. The sample period includes 174 days.

Another important issue is related to the representativeness of our sample. Figure 1 represents the distribution of products by price classes (10). The graph shows that 22 products out of 29 belong to the first two price deciles, with price ranging between 0$ and 240$. This picture closely matches a typical distribution observed in consumer electronics (Coad, 2009), often characterized by a large amount of

[20] High quality products are those characterized by a rating higher than 4/5, while high visibility ones are those endowed of a search rank index greater than 0.8.

low cost accessories and few luxury goods. Furthermore, calculating the log-price distribution (Figure 2) 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 the Normal one. Such result is in line with those obtained by Coad (2009).

Figure 1: Products Distribution by Price Classes.

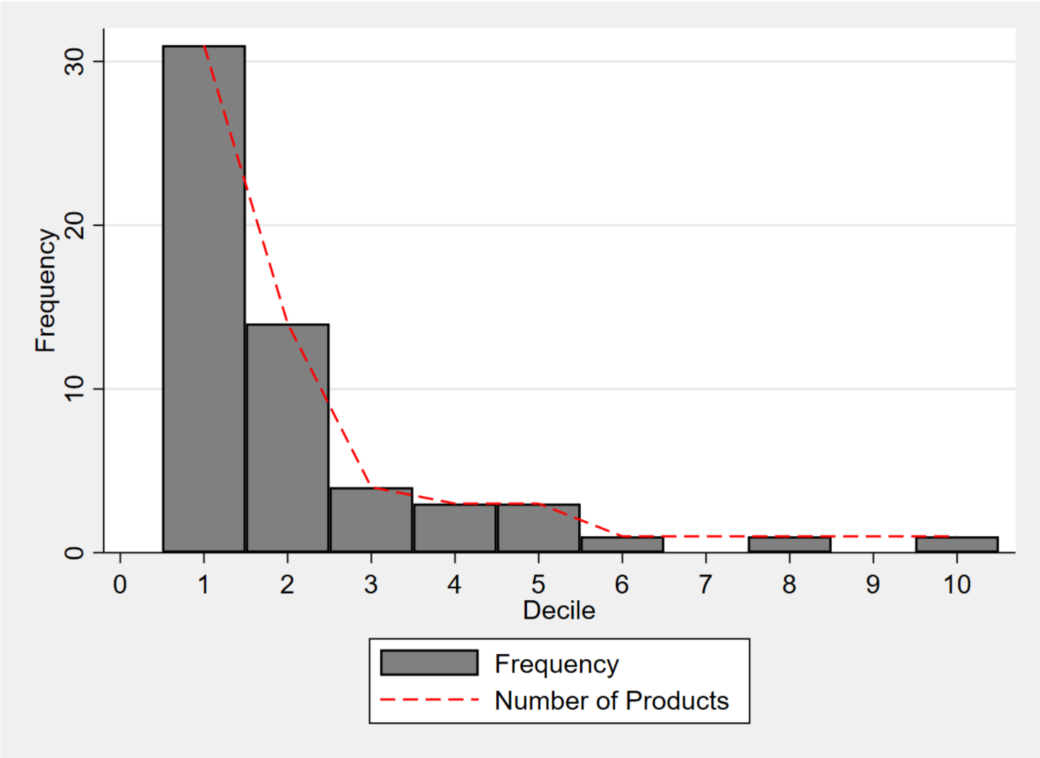
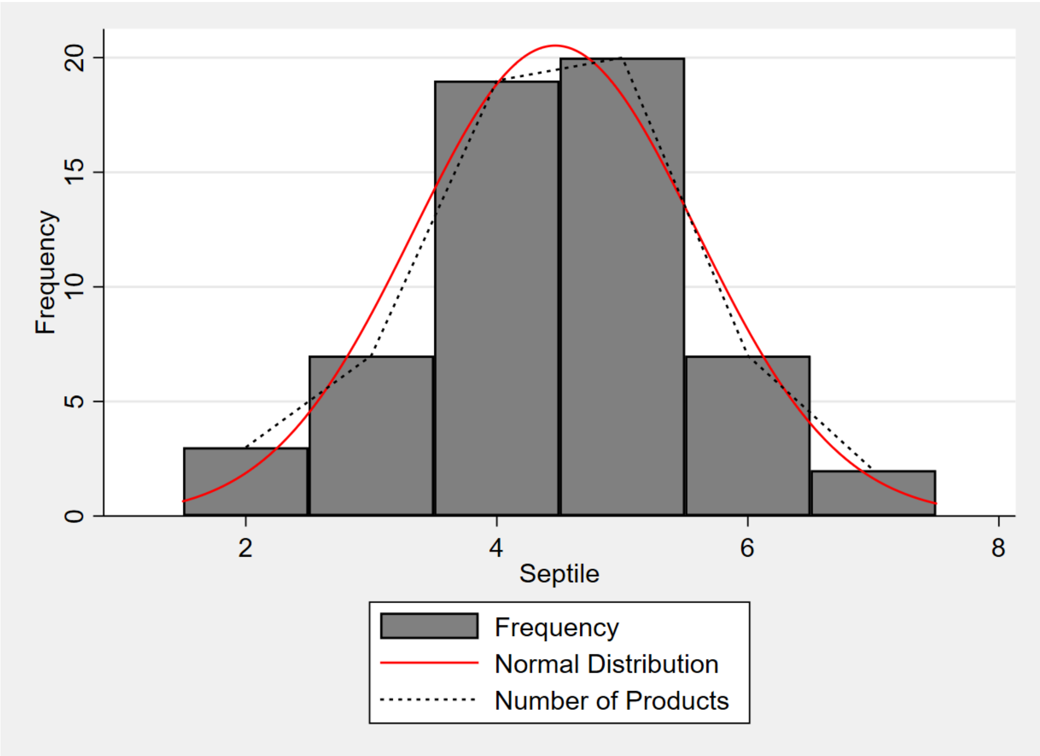


Figure 2: Products Distribution by Log-Price Classes.



4 Identification Strategy

We identify the causal effect of PMGs on price levels, by comparing prices before and after the policy shutdown for a sample of products sold by NewEgg (the treatment group), to the prices average change for the same products sold by Amazon UK (the control group). Indeed, and crucially for our identification strategy, PMGs implemented by NewEgg only affects products that are sold in US, thereby naturally creating a treatment and a control group; the same products sold by Amazon UK (that never offers price warranties) are less likely to be affected by the policy and well represents a counterfactual sample mimicking what would have happened to prices of treated products in the absence of PMGs. This framework provides a quasi-natural experiment that allows us to study the causal impact of PMGs on prices through a Diff-in-Diff research design.

This identification approach requires the estimation of the following panel FE model:[21]

$$\log Price_{i,l,t} = \alpha_{i,l} + \gamma\left(T_{i,l,t} * P_{i,t}\right) + X^T_{i,l,t}\beta + \mu_{i,l} + \tau_t + \epsilon_{i,l,t} \tag{1}$$

The dependent variable, $Price_{i,l,t}$ represents the price (natural logarithm) of good i, on platform l, at time t; $T_{i,l,t}$ denotes a binary variable equal to 1 for treated goods; $P_{i,t}$ is a binary variable that is equal to 1 for any day since the policy shutdown and zero otherwise and $\epsilon_{i,l,t}$ is an error term. The model includes a full set of daily time dummies, τ_t, accounting for unobserved time-varying determinants of prices that are common to all goods. Product fixed effects, $\mu_{i,l}$, control for any time invariant unobserved heterogeneity at the product and platform level, that could be correlated with the included regressors and that could also drive prices. Moreover, the presence of individual (product) fixed effects in the Diff-in-Diff research design rises the degree of comparability of treatment and control groups.

We include a set of covariates, $X_{i,l,t}$ in Equation (1), in order to control for products characteristics derived by UGCs that might affect the outcome of the PMGs policies.

The γ coefficient associated to the interaction term ($T_{i,l,t} * P_{i,t}$) represents the DiD estimate of the effect of PMGs shutdown on treated products prices and it measures the average price differential between the treated and the control group.

We also explore the issue of heterogeneity in the effect of PMGs policies on prices. Indeed, as discussed in Section 2, most of the predictions of theoretical models on the price effects of PMGs policies rely on assumptions related to the presence of heterogeneous consumers. By distinguishing products according to consumers' quality assessment, we indirectly assume that consumers are heterogeneous in terms of their preferences towards quality and their availability to pay a price premium for that. Indeed, for high quality goods the price elasticity of demand is usually assumed to be lower than the price elasticity for low profile goods. We further classify products according to their visibility, as measured by the search index described above. We believe that the time spent for finding a product indirectly selects consumers according to their willingness to engage in search activity and that such availability is directly correlated to their price sensitiveness.

Based on the above reasoning, we estimate Equation (1) on different sub-samples built according to product quality and visibility indices. In particular, we analyse separately high (low) quality products, namely products characterized by rating greater (lower) than 4/5, and products characterised by high (low) visibility in terms of Google search rank, namely products whose search index is greater (lower) than 0,8. Moreover, given that products quality and visibility resulted to be highly correlated, we split the sample according to both characteristics. As discussed in Section 3.2, such products characteristics do not affect the probability of being treated.

[21] In a Diff-in-Diff context, a classic model would be built like $Y = \alpha + \beta_1(Treated) + \beta_2(Post) + \beta_3(Treated * Post) + \varepsilon$. In all models we exclude $Treated$ and $Post$ terms, since they are multicollinear with time and product fixed effects.

The heterogeneity issue is also investigated with a different approach by estimating a Triple Difference regression (DDD) on the full sample. In particular, we estimate the following model:

$$\log Price_{i,l,t} = \alpha_{i,l} + \varphi\left(T_{i,l,t} * P_{i,t} * HRHV_{i,l,t}\right) + X^{T}_{i,l,t}\beta + \mu_{i,l} + \tau_t + \omega_{i,l,t} \qquad (2)$$

Equation (2) includes an additional component in the interaction term, $HRHV_{i,l,t}$, i.e. a dummy variable assuming value 1 for high quality and high visibility products. The coefficient φ of the triple interaction measures the average treatment effect of PMGs on prices for high quality (visibility) products.

All specifications are estimated by OLS and Standard Errors are robustly estimated. Moreover, following Cameron and Miller (2015), we compute bootstrapped standard errors with a cluster structure (at product level) and all results are confirmed. Finally, we conduct an extensive robustness analysis through different falsification and placebo tests (see Section 5.2).

5 Empirical Results

5.1 Main Results

In Table 5 and 6 we show DiD estimates; in particular, we first report results obtained by estimating Equation (1) without including control variables (Table 5, column 1), while Table 6 (column 1) provides estimates obtained after including all control variables.

Table 5: DiD Estimates of the Impact of PMGs on Prices.

Products Prices (log)	(1) FULL SAMPLE	(2) L. RATING	(3) H. RATING	(4) L. VISIBILITY	(5) H. VISIBILITY	(6) LR-LV	(7) HR-HV
$T_{i,l,t} * P_{i,t}$	-0.0401***	-0.0064	-0.0250***	0.0242***	-0.0543***	0.0331**	-0.0381***
	(0.00628)	(0.01300)	(0.00688)	(0.00769)	(0.00795)	(0.01370)	(0.00864)
Observations	9,028	2,896	6,132	2,295	6,733	994	4,864
R-squared	0.986	0.985	0.986	0.990	0.984	0.983	0.983
Controls	NO	NO	NO	NO	NO	NO	NO
Product Dummies	YES	YES	YES	YES	YES	YES	YES
Time Dummies	YES	YES	YES	YES	YES	YES	YES
F Test (p-value)	0.000	0.623	0.000	0.002	0.000	0.016	0.000

Notes: All specifications include time and product fixed effects. Controls include product popularity, number of reviews and rating. High quality products have ratings higher than 4. High visibility products have a normalized search index higher than 0,8. LR-LV are low rating and low search index products, HR-HV are high rating and high search rank products. Robust Standard Errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 6: DiD Estimates of the Impact of PMGs on Prices.

Products Prices (log)	(1) FULL SAMPLE	(2) L. RATING	(3) H. RATING	(4) L. VISIBILITY	(5) H. VISIBILITY	(6) LR-LV	(7) HR-HV
$T_{i,l,t} * P_{i,t}$	-0.0424***	-0.0108	-0.0270***	0.0322***	-0.0577***	0.0532***	-0.0398***
	(0.00629)	(0.01510)	(0.00693)	(0.00786)	(0.00799)	(0.01310)	(0.00879)
Observations	9,028	2,896	6,132	2,295	6,733	994	4,864
R-squared	0.986	0.986	0.986	0.992	0.985	0.989	0.983
Controls	YES	YES	YES	YES	YES	YES	YES
Product Dummies	YES	YES	YES	YES	YES	YES	YES
Time Dummies	YES	YES	YES	YES	YES	YES	YES
F Test (p-value)	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: All specifications include time and product fixed effects. Controls include product popularity, number of reviews and rating. High quality products have ratings higher than 4. High visibility products have a normalized search index higher than 0,8. LR-LV are low rating and low search index products, HR-HV are high rating and high search rank products. Robust Standard Errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

DiD estimates suggest that the PMG shutdown triggers a significant reduction of price levels of about 3.9%. Rather comfortingly, the inclusion of control variables into Model (1) does not significantly affect the result. These findings suggest that, on average, the adoption of PMGs has an anti-competitive effect on prices since, after the policy validity period, they show a substantial reduction. These results are consistent with those obtained by Zhuo (2017) on a large sample of products observed on the Amazon platform in 2012. However, we follow a rather different identification strategy. While Zhuo (2017) focuses on price changes observed on the non-adopting platform, before and after the implementation of PMGs by competitors, we focus on price changes observed on the adopting platform. Moreover, we innovatively build the control sample with platform price data for the same treated products but observed in another country (UK).

To explore whether product properties affect the impact of PMGs on prices, we split the sample according to different classes of product quality and visibility and we re-estimate Equation (1). Columns from (2) to (5) in Tables 5 and 6 show results of this disaggregated analysis. Estimates indicate that a policy repeal produces a price reduction for both low and high quality products; however, the estimated coefficients for the low quality sample are not statistically different from zero, while those for high quality products indicate a statistically significant price reduction of about 2.5%. When we split the sample according to values assumed by the search index, results suggest that, when the PMG is interrupted, products characterised by a low search rank experience a price increase of roughly 2,4%, while for high visibility products prices decreases of about 5,3%. These findings support the hypothesis that, in online consumer electronics market, PMGs policies harm competition for high visible products by keeping prices high, while for low visible products, such policies have a pro competitive effect on prices.

Finally, as highlighted in Section 3.2, quality and visibility are highly correlated in our sample. Hence, we estimate Equation (1) after splitting the sample according to both product properties.

Results shown in column (6) and (7) of Table 5 suggest that the PMG shutdown triggers a reduction of prices for high quality and high visibility products (3,7%), while prices of low quality and low visibility ones raise of about 3,4%.

These findings are confirmed when we include control variables into the model (Table 6, columns 6 and 7) and when we analyse heterogeneous effects of PMGs by means of a Triple Difference regression approach (model 2), as shown in columns (1) and (2) of Table 7.

Table 7: DDD Estimates of the Impact of PMGs on Prices.

Products Prices (log)	(1) DDD	(2) DDD
$T_{i,l,t} * P_{i,t} * HRHV_{i,l,t}$	-0.0537*** (0.00808)	-0.0556*** (0.00810)
Observations	9,028	9,028
R-squared	0.986	0.986
Controls	NO	YES
Product Dummies	YES	YES
Time Dummies	YES	YES
F Test (p-value)	0.000	0.000

Notes: All specifications include time and product fixed effects. Controls include product popularity, number of reviews and rating. $HRHV_{i,l,t}$ is a dummy equal to 1 for high quality and high visibility products. Robust Standard Errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Another important issue that we tackle is the possibility that the effects of the treatment speed up, stabilize, or mean revert over time. In order to explore this topic, we estimate a specification of Equation (1) that includes lags à la Autor (2003) and takes on the following form:

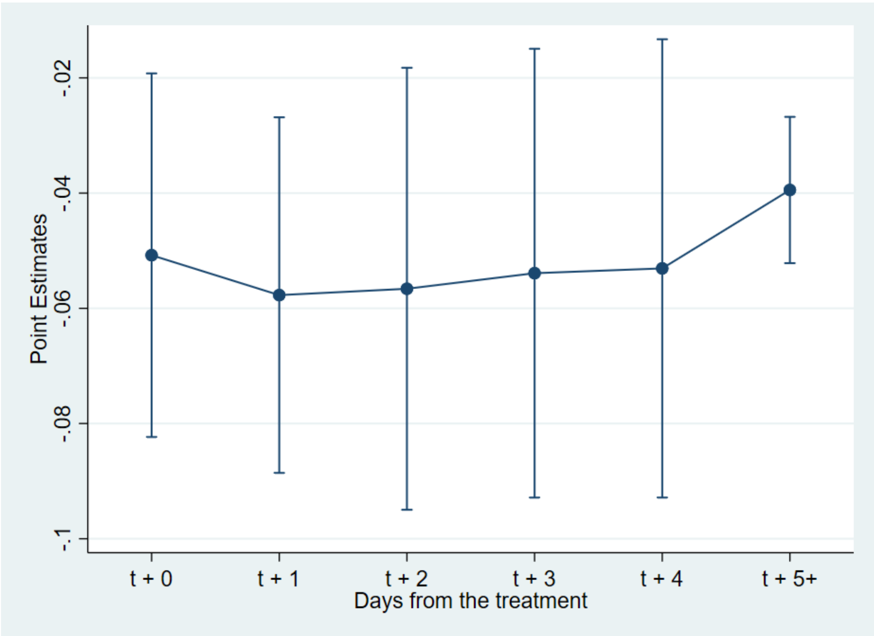
$$\log Price_{i,l,t} = \alpha_{i,l} + \sum_{j=0}^{5+} \gamma_j (T_{i,l,t} * P_{i,t+j}) + X_{i,l,t}^T \beta + \mu_{i,l} + \tau_t + \epsilon_{i,l,t} \tag{3}$$

Table 8: DiD Estimates of the Impact of PMGs on Prices with lags à la Autor (2003).

Products Prices (log)	(1) DiD	(2) DiD
$T_{i,l,t} * P_{i,t+0}$	-0.0500***	-0.0508**
	(0.01610)	(0.01610)
$T_{i,l,t} * P_{i,t+1}$	-0.0566***	-0.0577**
	(0.01580)	(0.01570)
$T_{i,l,t} * P_{i,t+2}$	-0.0558***	-0.0566**
	(0.01960)	(0.01960)
$T_{i,l,t} * P_{i,t+3}$	-0.0530***	-0.0539**
	(0.01990)	(0.01990)
$T_{i,l,t} * P_{i,t+4}$	-0.0529***	-0.0531**
	(0.02030)	(0.02030)
$T_{i,l,t} * P_{i,t+5+}$	-0.0368***	-0.0395**
	(0.00646)	(0.00648)
Observations	9,028	9,028
R-squared	0.986	0.986
Controls	NO	YES
Product Dummies	YES	YES
Time Dummies	YES	YES
F Test (p-value)	0.000	0.000

Notes: All specifications include time and product fixed effects. Controls include product popularity, number of reviews and rating. Robust Standard Errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Figure 3: DiD Estimates of the Impact of PMGs on Prices with lags à la Autor (2003).



Notes: The Figure refers to point estimates in column (2) of Table 8 (Full Sample, with Controls).

Specification (3), where $P_{i,t+j}$ assumes the value of 1 in day $t+j$, and 0 otherwise, allows the PMGs repeals to generate different effects over time. In order to lower the number of parameters of the model, we estimate the effect of a PMG shutdown from the implementation day ($j=0$) until five days later and onward.

According to results shown in Table 8, coefficients related to lagged variables are always negative and statistically significant for the full sample. However, point estimates suggest that the impact of the treatment reaches its maximum after one day and starts decreasing afterwards. Figure 3 graphically shows parameter estimates patterns.

Our empirical findings can be explained by the main predictions of theoretical models analysing the impact of PMGs on prices and competition.

The anti-competitive effect of PMGs observed for high quality (visibility) products has been predicted by theoretical models where such polices make collusion more likely (Hay, 1981; Salop, 1986; Cabral et al., 2018). These findings can be also explained by the theoretical predictions of a class of models, like Corts (1997) and Nalca et al. (2010), where a PMG is a tool for discriminating customers according to their sensitiveness to price and products quality. These models also explain our results obtained for products characterized by low visibility (quality).

Indeed, most of the predictions of theoretical models on the price effects of PMGs policies rely on assumptions related to the presence of heterogeneous consumers. By classifying products on the base of consumers' quality assessment, we indirectly assume that consumers are heterogeneous in terms of their preferences towards quality and their availability to pay a price premium for that. Similarly, the time spent for finding a product can indirectly select consumers according to their willingness to engage in search activity and it is reasonable to argue that such availability is directly correlated to price sensitiveness.

5.2 Robustness Analysis

In this Section, we discuss empirical results obtained by conducting an in-depth robustness analysis of our results.

The first concern we tackle is an important issue in a DiD research design, i.e. the presence of pre-treatment common trends for treated and control samples. This assumption is indeed fundamental for the validity of the counterfactual policy evaluation analysis.

In order to explore this issue, in Figure 4 we show point estimates values and relative confidence intervals of the difference in the level of prices between treated and control products from five days before the treatment to the day of the policy shutdown.[22] Plotted point estimates suggest that price levels for the treated platform do not seem to be significantly different from prices of the control one before the treatment. This result provides evidence in favor of the validity of parallel trends assumption for our samples.

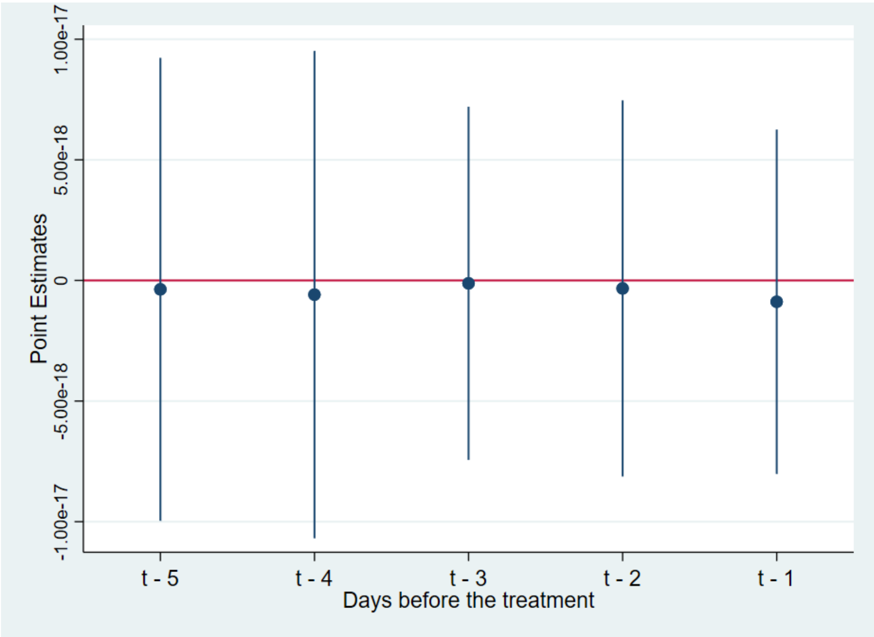
Aimed to further analyse this topic, we follow Autor (2003) and we estimate Equation (3) after including some leads of the treatment interaction variable:

$$\log Price_{i,l,t} = \alpha_{i,l} + \sum_{j=-1}^{-5} \gamma_j (T_{i,l,t} * P_{i,t+j}) + X_{i,l,t}^T \beta + \mu_{i,l} + \tau_t + \epsilon_{i,l,t} \tag{4}$$

If leads coefficients turn out to be statistically significant, there may be anticipatory effects and a failure in the parallel trend assumption. According to Table 9 and Figure 5, estimated coefficients of the anticipatory effects are not statistically significant, thus providing further evidence in favor of the existence of a parallel trend between treatment and control sample.

[22] In order to obtain these values, we estimate a panel model where we regress average daily price differences between the two samples on lead terms for five days before the treatment. We control for product fixed effects and daily fixed effects.

Figure 4: Price Differentials Between Treated and Control Groups Before PMGs Shutdown.



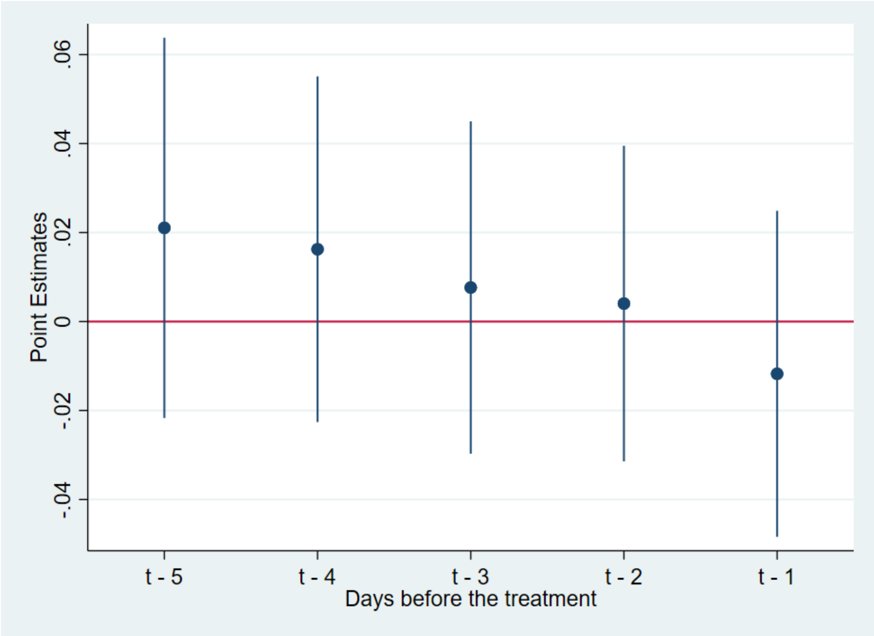
Notes: The Figure shows point estimates values and relative confidence intervals of the difference in the level of prices between treated and control products from five days before the treatment to the day of the policy shutdown. In order to obtain these values, we estimate a panel model where we regress average daily price differences between the two samples on lead terms for five days before the treatment. We control for product fixed effects and daily fixed effects.

Table 9: DiD Estimates of the Impact of PMGs on Prices with leads à la Autor (2003).

	(1)	(2)
Products Prices (log)	DiD	DiD
$T_{i,l,t} * P_{i,t-1}$	-0.0121	-0.0117
	(0.0187)	(0.0187)
$T_{i,l,t} * P_{i,t-2}$	0.0030	0.0040
	(0.0181)	(0.0181)
$T_{i,l,t} * P_{i,t-3}$	0.0067	0.0076
	(0.0191)	(0.0191)
$T_{i,l,t} * P_{i,t-4}$	0.0155	0.0162
	(0.0199)	(0.0198)
$T_{i,l,t} * P_{i,t-5}$	0.0205	0.0211
	(0.0219)	(0.0218)
Observations	9,028	9,028
R-squared	0.986	0.986
Controls	NO	YES
Product Dummies	YES	YES
Time Dummies	YES	YES
F Test (p-value)	0.842	0.000

Notes: All specifications include time and product fixed effects. Controls include product popularity, number of reviews and rating. Robust Standard Errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Figure 5: DiD Estimates of the Impact of PMGs on Prices with leads à la Autor (2003).



Notes: The Figure refers to point estimates in column (2) of Table 9 (Full Sample, with Controls).

In order to extend our robustness analysis, we implement a complete set of placebo tests. First, we build 1000 simulated datasets in which treatments timings are randomly reordered in each one by drawing from uniform distributions [0,1], while prices, treated products and UGCs are preserved.[23] For every simulated dataset, we then re-estimate baseline Model (1); therefore, each iteration of the placebo test allows to obtain a distribution of γ coefficient values under the null hypothesis of no treatment effect, against which to compare the effective value obtained in Table 6. The rationale is that a statistically significant effect of the treatment should be unusually large relative to the distribution of placebo effects. In the top panel of Figure 6 dark bars represent the distribution of $Treated_{i,l,t} * Post_{i,t}$ coefficient values estimated with such iterative method, while the vertical red line shows the effective γ coefficient inferred from our baseline specification. It is worth noting that the latter lies outside the density distribution of coefficient estimates from simulated datasets, thus being statistically significantly larger in absolute value than those obtained using randomly ordered treatments. This result is likely to corroborate our main findings.

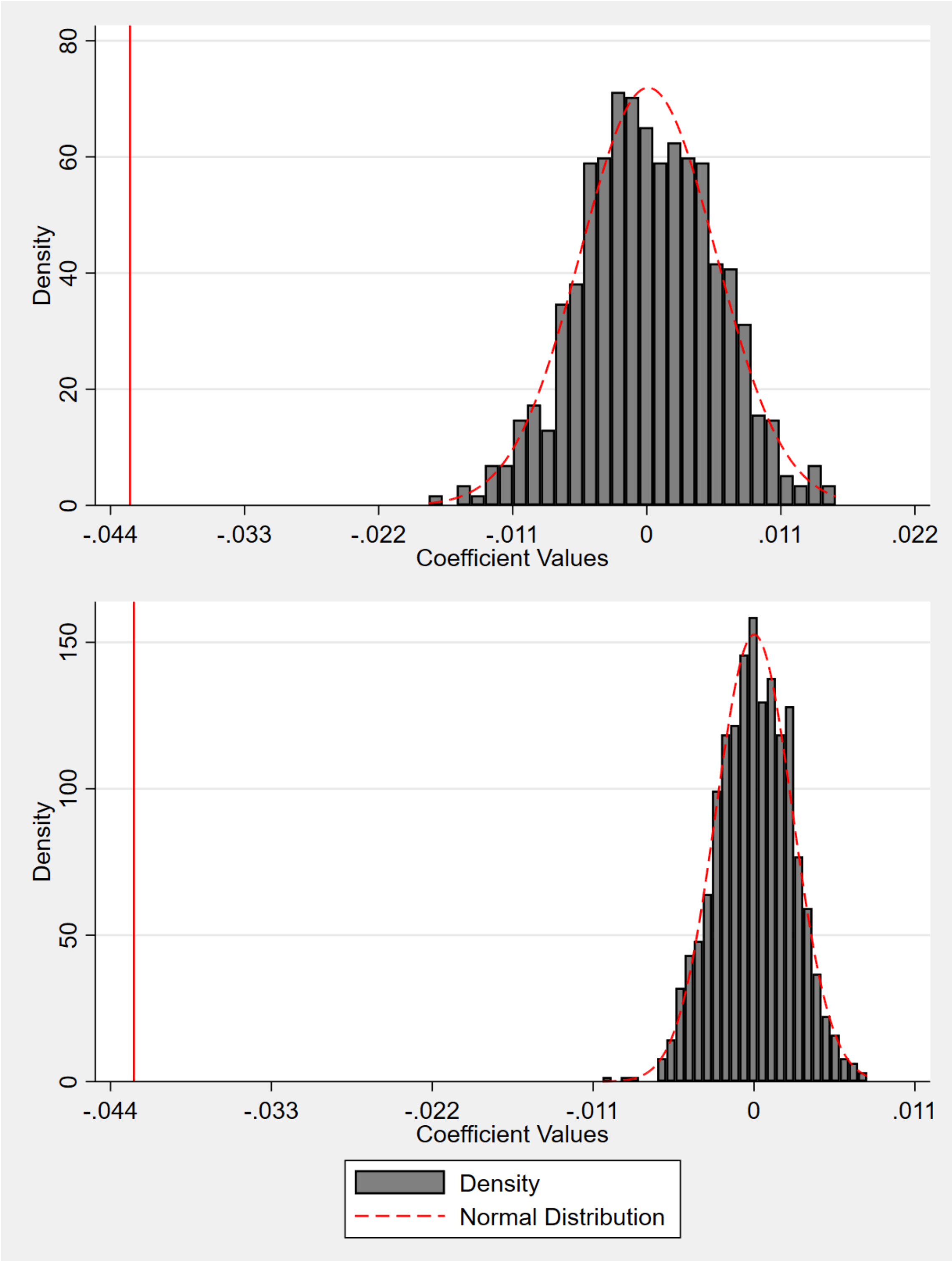
Second, we follow the same procedure to build up further 1000 simulated datasets characterised by artificially timed treatments as well as artificially treated subjects. The bottom panel of Figure 6 shows density estimates for such placebo test: once again, the coefficient inferred from our main analysis is abnormally large with respect to the distribution of coefficient values from the placebo study, confirming the robustness of our previous results.[24]

It is worth noting that our results are robust both when we re-estimate our baseline and DDD specifications by relying on a simulated dataset in which subjects and treatments *fake* assignments are randomly drawn from two Bernoulli distributions and when we estimate our models after substituting

[23]Placebo $Post_{i,t}$ is therefore built like a binary variable equal to 1 if random numbers drawn from the uniform distribution are greater than the probability of being treated derived from the original sample.

[24]It is worth noting that the distribution of coefficient values from placebo studies in Figure 6 resembles a Normal one with zero mean, thus highlighting no treatment effects under the hypothesis of subjects and/or treatments *fake* assignments. Results are confirmed also drawing from Bernoulli distributions instead of Uniform ones, as Figure A1, in Appendix A, clearly shows.

Figure 6: Placebo Plot Test. Density Estimates.



Notes: The figure shows results from iterative placebo tests. In the top panel, Model (1) is estimated relying on 1000 simulated datasets in which treatments are randomly reshuffled by drawing from an uniform distribution [0,1] in each iteration. The bottom panel relies on the same iterative method but with artificially timed treatments and artificially treated subjects. Vertical red lines represent the effective coefficient of $Treated_{i,l,t} * Post_{i,t}$ ($\gamma = -0.0424$) in Table 6. Dark bars show the distribution of γ coefficient values from placebo tests.

the dependent variable with a placebo outcome that should not be affected by PMGs shutdown.[25]

Lastly, in order to analyze if our main results are robust to the exclusion of a particular product we re-estimate the baseline Model (1) after dropping one product at a time. Results suggest that this is not the case and confirm all previous findings.[26] In the same spirit, we estimate equation (1) after balancing the panel dataset and all results are confirmed.[27] Finally, it is worth noting that results do not change if we compute bootstrapped standard errors at product level.

6 Conclusions

In this work we empirically investigate the effects of Price Matching Guarantees (PMGs) commercial policies on U.S. online consumer electronics prices by applying a Difference-in-Difference research design.

Estimates conducted over a sample of product prices, observed on the NewEgg platform between May and October 2018, provide evidence in favor of an average price reduction of about 3.9% after the interruption of the PMG policy. In order to have a more detailed picture of the issue, we investigate if such effect is heterogeneous across products. In particular, we focus on products features that might affect the outcome of PMGs policies and that can be recovered exclusively on online markets. Platform data allow us to obtain information from User Generated Contents (UGCs), like product popularity and product quality, as well as online search visibility (Google Search Rank); indeed, we believe that these product characteristics might provide indirect information on consumers heterogeneity. Estimates conducted on specific sub-samples show that when PMGs are interrupted, low quality (low search rank) products experience a price increase of about 3.4%, while for high quality (high visibility) products a price reduction of about 3.7% is observed.

These findings are in line with the lack of unambiguous predictions of the theoretical literature and are consistent with models predicting anti-competitive effects of PMGs policies and with those interpreting such policies as a price discriminating device. Theoretical models predicting anti-competitive effects of PMGs, suggest that such policies might induce higher prices in oligopoly markets (as the online consumer electronics) by sustaining collusion. In particular, online retailing platforms can easily monitor competitors prices trough price-tracking systems and can react faster to price signals, if compared to brick and mortar retailers. This possibility might sustain collusion by decreasing information asymmetries among competitors and reducing detection lags. On the other side, buyers' sensitivity to product quality and the willingness to engage in search activity can indirectly identify those customers whose demand is more rigid, thus allowing price discrimination practices. Indeed, e-commerce allows platforms to easily recover information on buyers, thanks also to UGCs, thus favoring discrimination policies.

Models that predict anti-competitive effects of PMGs on prices are well suited to explain the results for high quality and visible products. The demand of such products is high and stable and consumers are likely to be available to pay a price premium. Such features, together with easily detectable price signals, make collusion more sustainable. Thus, PMGs policies might be an invitation to collude that can be quickly and easily captured by competitors. However, it is worth noting that our analysis does not allows us to support such theoretical interpretation of the results since we do not analyse NewEggs

[25]In particular, in the first case we draw from Bernoulli distributions with parameters p (probability of success) derived from the sample distributions of $Treated_{i,l,t}$ and $Post_{i,t}$ respectively. In the second case, we generate *fake* product prices drawn by random distributions resembling sample ones (same mean and variance). Results are reported in Appendix A.

[26]Results, not reported, are available from the authors upon request.

[27]Precisely, we drop first 34 days in which we observe only some products; results are available upon request.

competitors' behavior.

Our empirical results are also consistent with theoretical models arguing that PMGs act as price discrimination tools. Indeed, such theoretical explanation requires a significant percentage of consumers invoking PMGs rights; unfortunately, we do not have data on PMGs redemption frequency. However, Moorthy and Winter (2006) find redemption rates ranging between 5% and 25% on a sample of 46 retailers operating in the United States and in Canada. It is reasonable to assume that online markets redemption rates can be similar to physical ones, thus providing support to the price discrimination interpretation of PMGs policies.

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Appendix A

Additional Robustness Test

In order to further verify our results, we perform a full set of additional robustness tests.

First, we re-estimate our baseline and DDD specifications by relying on a simulated dataset in which subjects and treatments *fake* assignments are drawn from two Bernoulli distributions with parameters p (probability of success) derived from the sample distributions of $Treated_{i,l,t}$ and $Post_{i,t}$ respectively.
Within this setting, we should not observe any significant effect of PMGs repeals on prices. Comfortingly, results reported in Table A1 confirm this prediction.

Second, Figure A1 provides results from a placebo test that follows the method described for Figure 6, in the main text, but relies on a Bernoulli distribution instead of an uniform one. Rather comfortingly, results in Figure A1 confirm the robustness of our main findings.

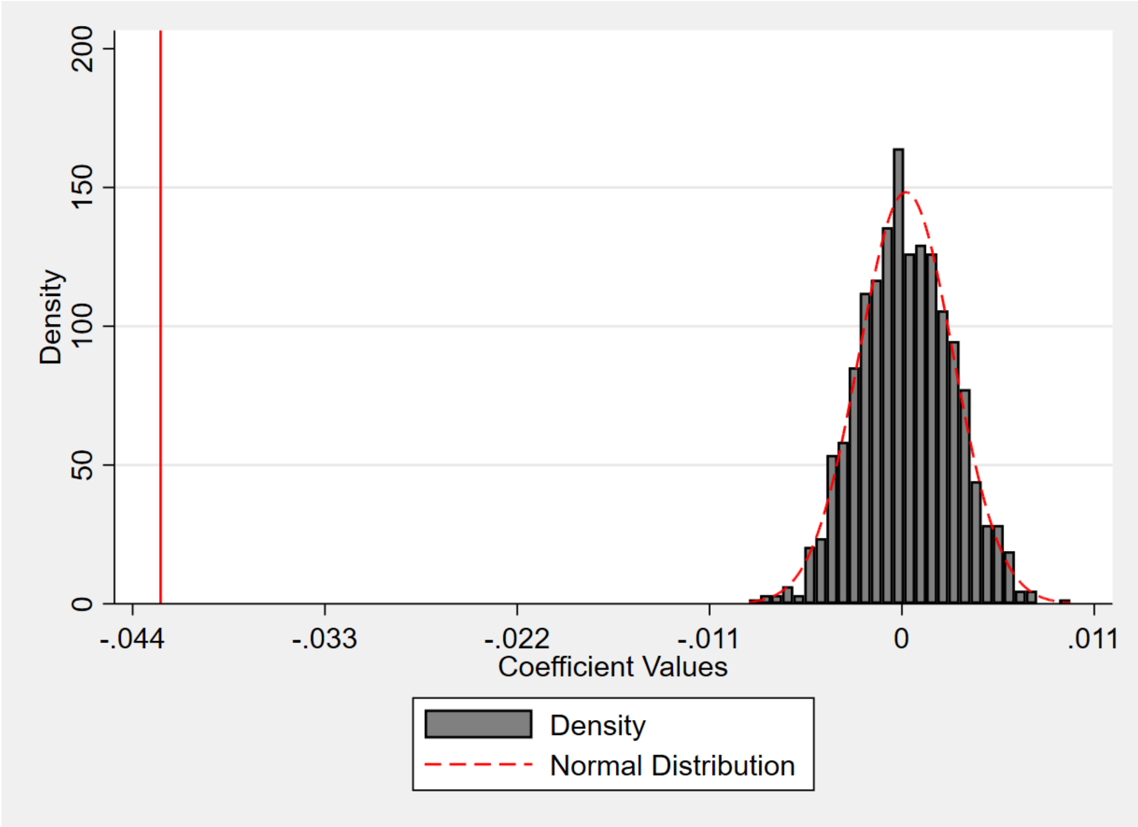
Finally, we conduct another falsification test by estimating our models after substituting the dependent variable with a placebo outcome that should not be affected by PMGs shutdown. In particular, we generate *fake* product prices drawn by random distributions resembling sample ones (same mean and variance). Results shown in Table A2 confirm the absence of any impact of PMGs repeals on *fake* outcome.

Table A1: DiD and DDD Estimates of the Impact of *Fake* Implementation Period on Prices for *Fake* Treated/Control Samples.

	(1)	(2)	(3)	(4)
Products Prices (log)	DiD	DiD	DDD	DDD
$T_{i,l,t} * P_{i,t}(Fake)$	0.0018	0.0019		
	(0.00262)	(0.00261)		
$T_{i,l,t} * P_{i,t} * HRHV_{i,l,t}(Fake)$			0.0033	0.0033
			(0.00403)	(0.00403)
Observations	9,028	9,028	9,028	9,028
R-squared	0.986	0.986	0.986	0.986
Controls	NO	YES	NO	YES
Product Dummies	YES	YES	YES	YES
Time Dummies	YES	YES	YES	YES
F Test (p-value)	0.502	0.000	0.418	0.000

Notes: All specifications include time and product fixed effects. Controls include product popularity, number of reviews and rating. Robust Standard Errors in in parentheses *** p<0.01, ** p<0.05, * p<0.1

Figure A1: Placebo Plot Test. Density Estimates. Bernoulli.



Notes: The figure shows the result from a iterative placebo test with artificially timed treatments and artificially treated subjects.. Model (1) is estimated relying on 1000 simulated datasets in which treatments are randomly reshuffled in each iteration by drawing from a Bernoulli distribution, with parameters p (probability of success) derived from the sample distributions of $Treated_{i,l,t}$ and $Post_{i,t}$ respectively. The vertical red line represents the effective coefficient of $Treated_{i,l,t} * Post_{i,t}$ ($\gamma = -0.0424$) in Table 6. Dark bars show the distribution of γ coefficient values from placebo tests.

Table A2: DiD and DDD Estimates of the Impact of PMGs on *Fake* Prices.

Fake Products Prices (log)	(1) DiD	(2) DiD	(3) DDD	(4) DDD
$T_{i,l,t} * P_{i,t}$	-0.0011	-0.0011		
	(0.00154)	(0.00154)		
$T_{i,l,t} * P_{i,t} * HRHV_{i,l,t}$			-0.0013	-0.0013
			(0.00203)	(0.00204)
Observations	9,028	9,028	9,028	9,028
R-squared	0.999	0.999	0.999	0.999
Controls	NO	YES	NO	YES
Product Dummies	YES	YES	YES	YES
Time Dummies	YES	YES	YES	YES
F Test (p-value)	0.466	0.722	0.516	0.746

Notes: All specifications include time and product fixed effects. Controls include product popularity, number of reviews and rating. Robust Standard Errors in in parentheses *** p<0.01, ** p<0.05, * p<0.1

Appendix B

List of Analysed Products

Table B1: Sub-Categories List.

Sub - Categories	# products
CPU Processor	3
Computer Case	2
Mobile Phone	1
Scanner	2
Speaker	2
Motherboard	1
Monitor	3
Headset	1
USB Flash	1
CPU Cooler	1
Speaker for Domotic	1
Tablet	1
Desktop PC	1
Laptop PC	1
Power Supply	1
Printer	2
Memory Card	2
Hard Disk	1
Smart Thing Domotic	2

Table B2: Products List.

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