Never Knowingly Undersold – Do Low Price Guarantees Result in Lower Prices?

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24th July, 2025

Abstract

Low Price Guarantees promise consumers that a retailer will match prices with eligible competitors. These policies are frequently used by retailers across the economy. On the face of it, such policies sound good for consumers, who ought to receive lower prices. However, economic theory is not conclusive on the actual effect. Theories of collusion and price discrimination predict that such guarantees could result in price rises, to the detriment of consumers. On the other hand, low price signalling theories would predict that LPGs are good for consumers. In this paper, I test the empirical effects of the removal of a low price guarantee at a leading UK retailer, and find evidence that prices rise post-removal of the policy. This implies that low price guarantees have a pro-competitive effect on retail prices and provides evidence supportive of pro-competitive arguments. Interestingly, I find that there is no effect at the retailer who removed the promise but instead that its competitors were the ones to raise prices post-removal of the guarantee.

JEL classifications: D22, D43, K21, L11, L81, C23

Keywords: low price guarantees, competition policy, retail pricing

1 Introduction

Low Price Guarantees (LPGs) are widely used policies by retailers promising consumers the lowest price relative to (eligible) competitors. Such guarantees have been used for decades to entice consumers to a retailer or prevent customers from switching. Whilst there is no precise data on the prevalence of these guarantees, nor their economic value, their impact can be seen from the large number of retailers employing them – a search of major UK retailers at the start of 2025 revealed 23 retailers, with over £108bn in sales, were operating some form of LPG.[1] Whilst this figure is non-exhaustive, and many other retailers have employed such policies in the past and/or will do so in the future, it highlights that a large number of consumer purchases are covered by LPGs and may be influenced by such policies. It is therefore vital for policymakers to understand the implications for such guarantees on the prices of retailers who adopt them, as well as the prices of their rivals, to examine the impact on competitors across retail markets.

At face value, these promises sound good for consumers, who are supposedly guaranteed to get the lowest price, potentially without the need to shop around. However, collusive and price discrimination theories predict that such guarantees may actually be anti-competitive and result in higher prices for consumers. The collusive theory posits that by offering consumers the same price as competitors, the incumbent prevents customers switching to rivals (Hay 1981, Moorthy & Winter 2006, Salop 1986). This therefore lowers the incentive of competitors to offer lower prices because they would not gain any market share. Consequently, these guarantees are predicted to soften price competition between retailers, leading to gradually higher prices for all consumers. Furthermore, low price guarantees can provide more stable and visible prices, with price deviations easily detected (by consumers reporting non-compliance through activation of the LPG), making collusion between retailers easier to sustain. Incorporating consumer

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The author gratefully acknowledges comments and feedback from Chris Doyle, Christos Genakos, Marta Grzeskiewicz, Edgar Liberis, Blair Lyu, Paul Kattuman, Kamiar Mohaddes, Michael Pollitt, Imke Reimers, Jacob Seifert, and participants at the University of Cambridge IO Day 2025 and Network of Industrial Economics Symposium 2025.

[1] As of January 2025, in the UK, the following major retailers had some form of LPG: AO, Booking.com, Boots, Currys, Deichmann, Dell, eBay, Euronics, Furniture Village, Goldsmiths Jewellers, Halfords, H.Samuel, Hughes Electrical, Jessops, Morrisons, Richer Sounds, Runners Need, Sainsbury's, Samsung, Sony, Tesco, The Fragrance Shop, and Wickes. Sales figures are based on UK sales reported in financial statements for the most recent year available. Not all sales would have been covered by the LPG. Arbatskaya et al. (2004) study newspaper adverts across the US and find 515 low price guarantees offered by firms across 50 different product categories and applying to both high price and low price goods as well as high-search cost and low-search cost goods.

information asymmetry, price discrimination theories argue that a price guarantee would increase prices for unsophisticated customers who do not shop around, whilst maintaining competitive prices for sophisticated customers who can take advantage of the LPG (Png & Hirshleifer 1987).[2] It is unclear from this theory whether aggregate prices rise or fall but such a policy is detrimental to unsophisticated customers. On the other hand, signalling theory argues that low cost retailers have an incentive to signal their low cost, and hence low price, to customers by using an LPG (Moorthy & Winter 2006). According to this theory such a policy is therefore welfare enhancing, as consumers switch to the lowest-cost retailer, rather than mistakenly shop at higher-cost retailers, and hence pay a lower price. This theory would predict that LPGs can be beneficial to consumers, in contrast to the collusive and price discrimination theories. The lack of clear theoretical predictions on the effects of LPGs clearly leaves room for empirical research to clarify what effects are observed in practice.[3]

Whether LPGs are pro- or anti-competitive is of great relevance to policymakers given their prevalence across the economy. Despite the importance and prevalence of such policies, there is limited empirical evidence evaluating the impact of LPGs and understanding their effects on consumers. This likely stems from the empirical difficulties in evaluating their impact. To ameliorate this lack of evidence and provide guidance for policymakers in whether to be concerned with these promises, this paper addresses the price effects of low price guarantees by exploiting a natural experiment. This paper is the first to empirically study the effects of an LPG removal and is also the first paper to study the pricing effects of LPGs across a large number of products and retailers.

I study the removal of an LPG at a leading retailer in the UK. The LPG was a pro-active price-matching promise.[4] The reasons for the removal of the guarantee, outlined in detail below, make the policy removal the ideal setting for a natural experiment to empirically test whether the LPG policy was pro- or anti-competitive. In particular, if the anti-competitive predictions of economic theory are correct, it would be expected that prices would fall after the removal of the retailer's LPG. Of interest is the aggregate price effect across all retailers and the disaggregated price effects across retailer groups depending on their exposure to the low price guarantee. In particular, I distinguish between price effects of the focal retailer, John Lewis (JL), who had a low price guarantee which was removed; inside retailers, who were covered by the LPG and therefore directly affected by the removal; and an outside retailer (Amazon), which was not covered by the LPG and was not directly affected by the removal other than through the normal competitive process.

I utilise a difference-in-difference framework to test the post-treatment change in prices at the focal retailer, alongside leading retail competitors. The counterfactual group is products that are sold exclusively by the focal retailer and therefore are unaffected by the low price guarantee. Overall, the results show that the removal of the low price guarantee causes insider competitor prices to rise by around 2.2%, although I find no statistically significant change in prices at the focal retailer nor the outsider. Back-of-the-envelope calculations suggest that UK consumers would have saved at least a cumulative £7.5 million in the 2021-22 financial year, had the LPG not been removed. In terms of treatment timing effects, prices do not rise immediately but take a couple of months. Focusing on treatment effects two months post-treatment I find that insiders increase prices by around 2.7%.

This provides evidence in favour of pro-competitive arguments for low price guarantees, contrary to the majority of existing theoretical and empirical evidence. Interestingly, however, the results do not necessarily suggest that the low-cost signalling theory is relevant in this context, as the focal retailer (John Lewis) does not have the lowest cost in the retail sector. Instead, I propose an alternative signalling mechanism to explain the findings. On the firm-side, JL's removal of the LPG may have signalled to its competitors that it would compete less fiercely with them, motivating them to put upward pressure on prices. On the consumer-side, the signal may have suggested that JL would increase prices (despite this not actually being observed). In response, consumers may have reduced their search intensity or

[2] An alternative explanation to unsophisticated customers who do not shop around (and therefore do not know competing retailer prices), is that such consumers have high hassle costs and thus do not activate the low price guarantee. However, when consumers have even a trivial but non-zero hassle cost, firms are only able to support a small increase in equilibrium prices through their use of a low price guarantee (Hviid & Shaffer 1999).

[3] Other theoretical evidence considers the impact of LPGs on entry deterrence (Arbatskaya 2001) and on the incentives of firms to offer LPGs given strategic interactions among competitors (Constantinou & Bernhardt 2018). Such theories are less relevant for discussion in the empirical context studied here.

[4] Some LPGs are not pro-active but require customer activation, whilst other LPGs are price-beating. Existing evidence suggests that price-matching LPGs are the most prevalent form offered by firms (Arbatskaya et al. 2004).

switched their demand to competitors, allowing competitors to increase prices. Whilst I do not have data on consumer/firm perceptions to directly test this explanation, I am able to provide evidence that post-treatment price increases are greater for products with higher pre-treatment price dispersion. One interpretation of this finding is that prices rise more on products that witness lower search behaviour by consumers, consistent with the explanation provided here.

Investigating further, I find that certain categories of products (cameras, cookers and ovens, fridges and freezers, microwaves, and washing machines and tumble dryers; jointly accounting for 57% of sampled products) are more affected by this policy than others. There is some evidence that the treatment effect is stronger the greater the number of competitors selling a product, although this is partially explained by a product composition effect. There is also evidence that the treatment effect is stronger for more expensive products and for products with higher (pre-treatment) price dispersion. Results are robust to a battery of robustness tests.

Turning to the existing empirical literature, there is a limited pool of evidence, with existing studies being limited in their approach due to data availability (Manez 2006, Zhuo 2017, Hess & Gerstner 1991, Cabral et al. 2021). The lack of empirical evidence is unlikely to be because of a lack of interest in the topic but more likely reflects the difficulties in empirically testing the effects of LPGs. The primary difficulties lie in (i) finding a natural experiment, where an LPG is introduced or removed, (ii) collecting or obtaining the necessary pricing data, and (iii) finding a suitable counterfactual. I overcome issue (i) by identifying a natural experiment where John Lewis removes its LPG, which is pre-announced, allowing the collection of pricing data through a web scraper (solving issue (ii)), in advance of the policy being removed. I collect data for a large number of products (over 1,800 products) across 12 UK retailers. This in contrast to the existing literature which typically studies far fewer products (between 46 and 150). I use products that were exclusive to the focal retailer, and therefore ineligible for the LPG, as the counterfactual group to overcome issue (iii). Additionally, as a robustness check, I use Amazon products as the counterfactual group (given that no effect was found on Amazon prices following the LPG removal) and find supportive evidence for the pro-competitive conclusion.

Hess & Gerstner (1991) and Manez (2006)[5] study the introduction of LPGs operated by supermarkets (dealing with issue i), in the US and the UK, respectively, albeit with a very limited dataset and only looking at a narrow geographic area. Manez (2006) manually collects pricing data by hand from supermarkets, which limits the ability to obtain data across a large number of products (highlighting problem ii). Both authors use products sold by the supermarkets but not contained within the LPG policy as their counterfactual (to satisfy issue iii). They find different results, with Hess & Gerstner (1991) concluding that their evidence supports the anti-competitive collusive theory, whilst Manez (2006) finds evidence in support of the pro-competitive signalling theory. If these results are accurate, then this may simply reflect the fact that LPGs were used for different purposes, rather than one theory being universally correct. Zhuo (2017)[6] studies the introduction of an LPG (dealing with issue i) in the context of electronic stores in the US and finds supportive evidence for anti-competitive theories. Whilst this author uses a larger sample (150 products) of pricing data - cleverly collected, retrospectively, through online prices posted on a comparison website (solving issue ii) - there are difficulties around the reliability and accuracy of the treatment and control group. Moreover, the author is only able to study the impact of prices at a competing retailer (Amazon) rather than the focal retailers which introduced the LPG policy. Finally, Cabral et al. (2021) study the introduction of an LPG in the German petrol market – although the LPG only applied to customers that held a loyalty card - and find that it was used to help facilitate tacit collusion; again, suggesting higher prices as a result of LPGs. To satisfy condition (ii) they utilise posted prices reported to the regulator by firms, and prices are compared with distant retailers who would likely be less affected by the LPG and thus unlikely to adjust their prices in response (satisfying condition iii).

This paper contributes to the existing literature by studying the removal (as opposed to introduction) of an LPG, across a large number of products, and across multiple retailers (capturing the largest players), and in an economically significant market (the UK electronics/white goods market). Importantly, I focus on a low price guarantee that was pro-active and, in a setting, where the focal retailer is not the lowest-cost retailer. Such a setting poses a strong candidate for exhibiting collusive effects, yet I find evidence

[5]Manez studies a price-beating guarantee.

[6]The LPG studied applies only to prices at a single retailer (Amazon) and is not pro-active: customers were required to activate the policy.

that the LPG used in this setting was actually pro-competitive. Whilst I only study a single example of an LPG – and do not conclude that LPGs necessarily exhibit universal pro-competitive effects as a result – I find evidence contrary to the bulk of existing literature, highlighting that LPGs are not universally anti-competitive in their consumer effects.

The rest of this paper proceeds as follows. Section 2 details the LPG retailer which forms the basis of the natural experiment, the leading UK department store John Lewis, and the competitive landscape this retailer faces. Section 3 outlines the constructed dataset. In Section 4, I provide descriptive statistics, and the empirical strategy is explained in Section 5. Section 6 provides the results and discussion around this whilst Section 7 conducts a series of robustness checks. I conclude in Section 8.

2 Institutional Setting

John Lewis is a leading retailer and department store operating in the UK since 1864. In the 2022-23 financial year, John Lewis (JL) had 34 stores, revenue of £3.8 billion, operating profit of £676 million, 21,000 employees and 11.7 million customers (John Lewis 2023). 60% of JL's business was derived from online sales, where its website received more than half a billion visits, and the remaining 40% came from sales in physical shops, which received a footfall of around 100 million (John Lewis 2023).

The company consists of three main departments, each accounting for roughly a third of sales: electronics (relatively low profit margins), home (relatively high margins), and beauty and fashion (margins in between electronics and home).[7] Around 60% of products in the home section were JL-exclusive and so the LPG would not have applied heavily in this category. Theoretically, profits could be recouped in this product category if the LPG attracted consumers to stores to purchase electronics and who also bought products from the home section. John Lewis is owned by the John Lewis Partnership, an employee-owned trust, which also includes the supermarket chain Waitrose, data for which is excluded in the analysis.[8]

The company was well known for its 97-year-old "Never Knowingly Undersold" (NKU) pledge, which promised to match the price of any High Street retailer offering the (exact) same product. As the competing retailer had to have multiple bricks-and-mortar shops, the promise did not apply to online-only retailers. Details of the NKU pledge's terms and conditions are displayed in Annex A.1. Discussions with a former JL executive suggested that the motivation for this LPG was to advertise to consumers that the firm would match low prices seen elsewhere, despite the high-brand-value associated with the company, which may otherwise have portrayed the company as being high-priced. This assertion would suggest a pro-competitive signalling motivation for the LPG, although the company was not actually the lowest-cost retailer in the market.

As well as matching lower prices that were reported by customers, John Lewis actively monitored the prices of competitors on a daily basis and reduced prices automatically, ameliorating the need for customers to make a price match claim. In fact, in 2021, less than 1% of customers actively made price match requests.[9] Moreover, in Section 4, I show that 79% of LPG-eligible products at John Lewis had the lowest price relative to competitors, meaning that there are few instances where the LPG would have been required to be activated directly by consumers.[10] Consequently, this demonstrates that the LPG studied is a case of an active low price guarantee, rather than one which only targets price-savvy consumers and so rules out the price discrimination theory of low price guarantees in this context.

Active monitoring and price reductions were made by a dedicated team of around 10 staff, who would search prices of competing retailers and update JL prices if they were not the cheapest on like-for-like products. Furthermore, customer reports of price differences were passed on to the NKU team and incorporated into JL prices, alongside reports from JL staff who were incentivised to report lower prices

[7]Figures come from discussion with a former JL executive.

[8]Note that the Never Knowingly Undersold pledge only applied to the John Lewis department store and not the Waitrose supermarket chain.

[9]https://www.thegrocer.co.uk/waitrose/john-lewis-ditches-never-knowingly-undersold-pledge-as-it-invests-500m-in-prices/664935.article [accessed on 09/02/2025]

[10]The reason that this figure is not 100% may be due to data collection errors, inaccuracies by the JL NKU team, or because the product was not stocked at JL/competitor and therefore the LPG did not apply.

they spotted elsewhere.[11]

This long-standing price match was removed on 23rd August 2022, by a relatively new outside hire CEO, with the retailer claiming that it was outdated and, as it did not cover competing online retailers (i.e. Amazon), was no longer benefiting customers who were increasingly shopping online. Furthermore, consumers were often unaware that the LPG did not apply to online retailers and so became frustrated and disillusioned when John Lewis was unable to match the prices for online competitors. John Lewis believed that such frustration could affect its brand image, if consumers felt like they were being deceived by the low price guarantee. I therefore argue that this policy can be treated as exogenous (to pricing decisions) and is thus suitable to form the basis of a natural experiment for empirical investigation into the effects of low price guarantees.

In a narrow-sense, John Lewis is a department store which competed with other department stores in the UK. These include, Debenhams, Fortnum and Mason, Harrods, Harvey Nichols, House of Fraser and Selfridges. The largest of these competitors, Debenhams, which had 120 stores, closed in May 2021. In a wider-sense, John Lewis competed with a much broader pool of retailers who sell only a subset of the product categories stocked at JL. In the electronics and electrical appliances category – the focus of this study – these firms would include Amazon, AO, Argos[12], Currys, Homebase, Hughes, and Wickes.[13] I focus on electronics and electrical appliances because I would expect these products to be the most comparable across retailers and to be the least subject to seasonal changes in variety.

Some of these competing retailers also had low price guarantees at the same time as John Lewis, including AO, Currys, and Hughes. These policies have remained in place throughout the sample period and cover all UK-based bricks-and-mortar retailers. This is interesting because, as found in Section 6, AO and Currys increase their prices post-treatment, despite no change in John Lewis' prices. One potential explanation is because the LPGs at AO, Currys, and Hughes all required the consumer to activate the guarantee, either through an online form or speaking to a store assistant, in contrast to John Lewis' guarantee which is pro-active. Therefore, retailers with guarantees which required consumer activation may have had more scope to adjust prices – relative to guarantees which are pro-active – particularly in a context where activation costs differ between consumers, or consumers have different awareness of the LPG's existence.[14]

Against its competitors, John Lewis is typically perceived as having higher customer service, a higher quality range of products stocked, and offers generous extras such as a minimum 2-year guarantee on all electricals (5 years for TVs) at no extra cost. In July 2024, the UK Customer Satisfaction Index found John Lewis to be the highest rated non-food retailer for customer service (Institute of Customer Service 2024). Coupled with a low price guarantee, there is clearly an incentive for consumers to switch retailers to John Lewis to take advantage of the higher service provision and product guarantees, highlighting the potential profitability of utilising a low price guarantee.

3 Data

Price data from John Lewis, Amazon[15] and 17 other leading UK retailers that compete with John Lewis, including AO, Argos, and Currys, was obtained through the daily web-scraping of online prices between 19th April 2022 until 30th April 2023.[16] In addition to collecting pricing data at John Lewis and competing retailers, I also collected weekly data on the number and score of customer reviews left at John

[11] According to discussions with a former JL executive.

[12] Which has been owned by supermarket group Sainsbury's since 2016.

[13] Department stores at the time of this study were not active in the sale of electronics, with the exception of House of Fraser which was included in the sample.

[14] Hviid & Shaffer (1999) note that if firm asymmetry is large enough then positive hassle costs from consumer activation can allow higher prices to be supported.

[15] Amazon prices were obtained from the Buy Box pricing figures and may have therefore included third party sellers.

[16] The full list of retailers is Amazon, AO, Argos, B&Q, Boots, Costco, Currys, Decathlon, Dunelm, Homebase, House of Fraser, Hughes, John Lewis, Richer Sounds, Robert Dyas, Tesco, TradePoint, Travis Perkins, and Wickes. This list of retailers was chosen to capture the main competitors of John Lewis in this product category (electricals/white goods). In particular, retailers that had multiple physical stores (across the country) were chosen, as the LPG would apply to them and they would count as relevant inside retailers.

Lewis for sampled products which is merged into the main pricing dataset. The treatment – removal of the low price guarantee – occurred on 23rd August 2022, so there are 4 months of pre-treatment data and 8 months of post-treatment data. Data was collected for a period of a year to ensure that I had a long enough sample to explore long-run effects whilst balancing the costs of continued data collection.

Whilst I collected pricing data from online sources, for the eligibility of the JL LPG to be valid requires that online prices matched in-store prices and also that nationwide pricing occurs. To check this was the case, I contacted each of the 19 retailers studied, with 12 confirming that online prices were the same as in-store. The remaining 7 retailers couldn't guarantee that all online prices were the same as in-store, particularly in the case of discounts and promotion periods, and so were conservatively removed from the sample.[17] As a robustness check, I re-estimate the empirical specification including these removed retailers and find similar results. The final list of retailers included in the study is: AO, Amazon, Argos, Boots, Currys, Dunelm, Homebase, Hughes, John Lewis, Tesco, TradePoint, and Wickes.

The products chosen to be included in the dataset were focused on white good categories to ensure that products would remain in sample throughout the year of the study. The product categories sampled include cameras, cookers & ovens, cooking appliances, dishwashers, electricals, electronics, fridges and freezers, heating, home telephones, ironing, microwaves, mobile phones, smart home, smart tech, and washing machines and tumble dryers. Despite the intention, a moderately high level of product atrophy is witnessed over the year, with 34.4% of sampled products being discontinued by the end of the sample (see Section 4). To create the list of products, I constructed a catalogue of white goods products sold at John Lewis, the focal retailer, on 9th April 2022. I then matched each JL product in the sample to identical products sold at alternative retailers.[18] The reason for selecting identical products was because JL's LPG was restricted to identically matched products at eligible retailers.

One limitation of constructing the sample in this manner is that I can only study products which were selected to be sold at John Lewis, and do not have data on products sold at competing retailers if they were not also stocked at JL. Whilst this limits the ability to evaluate aggregate price effects it was a necessary restriction in order to collect the data in a timely fashion. It would be expected that products directly affected by the LPG would have the largest price effects, and that any spillover effects on to non-JL-sold products at competing retailers are likely to be smaller in magnitude. Therefore, the dataset constructed allows me to focus on the products where one would expect to see the largest impact post-removal of the LPG. Another implication of this approach is that whilst a given retailer may appear to have a small number of products in the sample, this may not reflect their actual retail size, simply the fact that they only stock a few of the same products as John Lewis.

Table 1 below shows the total number of price observations which were collected for each retailer alongside the maximum number of unique products sampled at a given retailer. Note that on a given day, a product may not have been available due to a stock-out and hence the pricing data may not have been collected. In total there are 1,510,448 price observations across the retailers over the 365 days in the sample.[19] Section 4 provides summary statistics on the cleaned dataset.

The counterfactual chosen for analysis is John Lewis exclusive products, where exclusivity is defined by no sampled competing retailer stocking the product. This counterfactual is constructed considering all 18 nationally competing retailers.[20]

Finally, to study whether there was a change in service provision over the sample period, I collected weekly data on the number and average value of customer ratings left on John Lewis product pages. 218 (out of 1,820) products had zero reviews left and I was unable to collect review data for 1 product. Product ratings range between 1 (lowest) and 5 (highest). I do not have data on the review comments

[17] The following retailers informed me that that their online price may differ due to online promotions: B&Q, Costco, Decathlon, House of Fraser, Richer Sounds, Robert Dyas and Travis Perkins.

[18] This process was conducted both manually and with the aid of a further webscraper which identified matching products at alternative retailers which were then manually checked for accuracy.

[19] Whilst the sample runs over a calendar period of 377 days, technical issues prevented a webscrape for 12 days which are thus missing. These missing observations occurred throughout the time period, rather than in a concentrated period.

[20] In Section 7, a robustness check is conducted, constructing the counterfactual using only the 11 competing retailers in the analysis. I find that results are unaffected by this decision.

[21] Note that TradePoint is owned by B&Q (which could not guarantee the same online price as instore). There were only 23 instances where B&Q and TradePoint both sold the same product and didn't have the same price (out of 9,767 joint observations). However, there were 9,140 instances where they didn't sell the same product at the same time.

Table 1: Number of pricing observations and unique (sampled) products at each retailer

Retailer	Number of Observations	Number of Unique Products
Amazon	332,166 (63%)	1,209 (66%)
AO	263,387 (50%)	905 (50%)
Argos	135,197 (26%)	438 (24%)
Boots	6,941 (1%)	21 (1%)
Currys	154,018 (29%)	524 (29%)
Dunelm	655 (0%)	2 (0%)
Homebase	15,946 (3%)	52 (3%)
Hughes	45,698 (9%)	167 (9%)
John Lewis	523,587 (100%)	1,820 (100%)
Tesco	255 (0%)	1 (0%)
TradePoint[21]	16,429 (3%)	49 (3%)
Wickes	16,169 (3%)	47 (3%)

Note: the number of unique products requires only that at least one price was observed over the sample. In brackets I show the percentage of observations/products at a given retailer relative to the total number at John Lewis, thereby demonstrating which retailers have high overlap / can be considered near competitors.

left but note that commenters' reviews evaluated both the quality of the product, the price / value for money, and the service provision provided by John Lewis.

4 Descriptive Statistics

After cleaning[22], the final sample of dataset consists of 1,820 products across 1,486,749 price observations. Table 2 provides summary statistics for each retailer, showing the mean, standard deviation, maximum and minimum values for prices, alongside the number of observations and the number of unique products.[23] Out of the 1,820 products in the sample, 414 products have zero competitors in the final sample, an additional 447 are sold by one other retailer, 379 by 2 retailers, 298 by 3 retailers, 206 by 4 retailers, 64 by 5 retailers, 6 by 6 retailers, and 6 by 7 retailers.[24]

The lowest price in the sample is £4 whilst the highest price is £7,500. Comparing the average price at Amazon vs John Lewis, Amazon has a lower average price. However, this does not directly indicate that Amazon has lower prices but may simply reflect that it only stocks the cheaper varieties of products that John Lewis offers.

In terms of quality, the mean product rating was 4.38 (with a standard deviation of 0.59), and the mean number of reviews was 112 (with a standard deviation of 495). Over the period of study, the number of ratings grew by 38% (median) whilst there is a 0% (median) change in rating.[25]

I test the difference before/after in minimum and maximum of log price, price dispersion (max log price – min log price), mean log price, median log price, and standard deviation of log price. I exclude

[22] A parsimonious approach is taken to removing outliers, as the extent of manual checks leads me to believe that the dataset is of a high accuracy. Firstly, I remove any observation which is less than half the value of the 1st quartile or more than twice the value of the 3rd quartile. This results in the removal of 7,894 observations. Secondly, I create an average price across all retailers and remove any retailer-product pair where the average price is more or less than 60% compared with the John Lewis average. This results in the removal of 24,557 observations or 77 retailer-product pairs.

[23] Any differences with Table 1 come from the removal of observations during the cleaning stage.

[24] There are a total of 12 retailers in the sample (including JL). This means that there are no products stocked by all retailers in this sample. Furthermore, the JL-only counterfactual consists of 382 products whilst 414 products have zero competitors. The reason for this is that 32 products are sold at the 7 retailers that were dropped from the sample.

[25] Taking the first rating as either the start of the sample or the date a first review was left. Note that the mean growth rate of ratings was 165%.

Table 2: Summary Statistics for Retailer Price (£)

Retailer	Mean	Std Dev	Min	Max	#Obs	#UniqueProducts
Amazon	561.92	600.55	4.99	7,499.00	320,484	1,159
AO	788.21	754.66	11.99	7,499.00	261,567	896
Argos	429.55	452.55	7.99	3,550.00	132,413	430
Boots	95.21	43.22	30.00	219.99	6,863	21
Currys	596.51	598.04	6.98	7,499.00	148,572	502
Dunelm	76.74	22.00	55.00	99.00	655	2
Homebase	584.67	253.155	59.00	1,169.00	15,749	52
Hughes	510.35	333.78	16.95	1,539.00	44,881	163
John Lewis	823.74	915.27	4.00	7,500.00	522,726	1,820
Tesco	40.00	0.00	40.00	40.00	255	1
TradePoint	698.10	433.85	22.00	2,218.00	16,415	48
Wickes	993.46	661.17	200.00	4,699.00	16,169	47

Note: the number of unique products requires only that at least one price was observed over the sample.

Amazon (focusing on the focal and inside retailers) from the calculation of these variables and include product fixed effects. I find that the minimum and maximum (log) prices increases by 1.8% (p=0.000) and 1.1% (p=0.000), respectively, following treatment; the mean and median (log) price both increase by roughly the same amount: 1.5% (p=0.000); and price dispersion is reduced, with the max-min (log) price falling by 0.7% post-treatment (p=0.000); the standard deviation of (log) price is small and statistically insignificant.

Next, I investigate the effectiveness of John Lewis' price matching team given the pro-active nature of the LPG. It would be expected that if John Lewis were truly offering the lowest price (pre-treatment) that they would have the lowest price, compared with bricks-and-mortar stores, on a daily basis.[26] Out of 149,988 observations when JL had the product in stock, John Lewis has the (joint) lowest price on 107,765 observations.[27] This indicates that for 72% of observations when John Lewis stocks the item, it has the lowest price. It seems odd that it is not higher given the supposed price-matching activities of the John Lewis NKU team. It might be the case that there are small discrepancies in finding the lowest price, and also that it takes a few days for price changes to take effect. I therefore include a 5-day lag and ignore price differences less than or equal to £5. Now I find that for 119,158 (79%) observations, John Lewis has the lowest price. Clearly this is below 100% for which there are three possible explanations: (i) there are mistakes in the collected pricing data which have not been removed during the cleaning process, (ii) products which have been assigned as perfect matches across retailers are not in fact perfect matches and therefore the LPG doesn't apply to them, or (iii) prices in the dataset are accurate and JL is not perfectly price matching. The latter explanation could be as a result of zero customers reporting a price difference in a given time period, potentially due to low sales of a particular product or during periods of stockout. Without further data – such as sales data – it is difficult to conclude which explanation is driving this finding. Nonetheless, I note that for 79% of observations in this sample, JL is correctly price-matching.

4.1 Product Atrophy

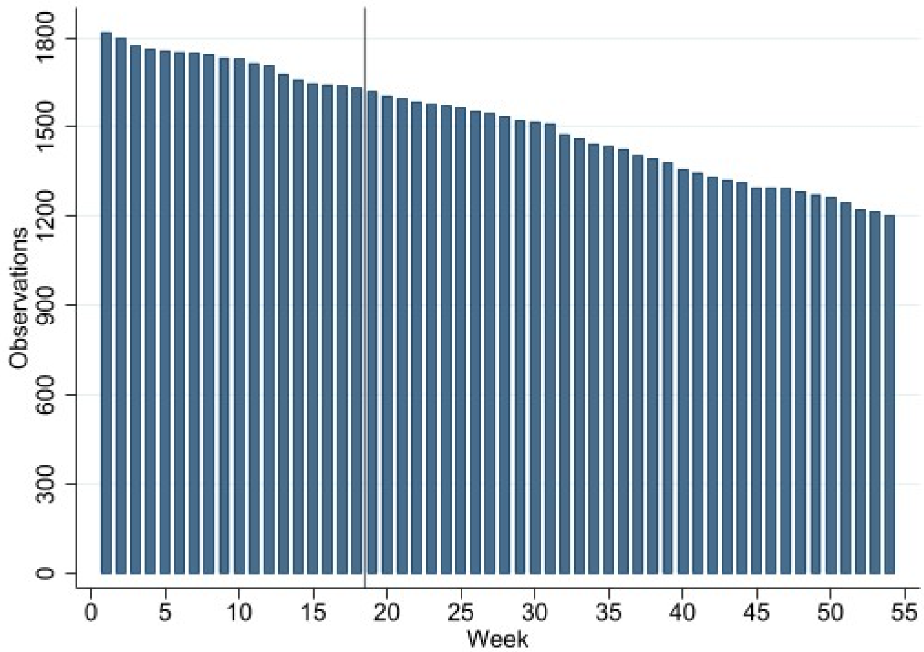
A high rate of product atrophy is observed, with only 65.6% of observations being present across the whole sample. This is particularly surprising given that the sample comprises white goods, which would be expected to have the lowest discontinuation rate of John Lewis products. Nonetheless, this finding is

[26] I exclude Amazon (outsider) prices from these calculations as I am comparing with bricks-and-mortar retailers for whom the LPG applies (insiders).

[27] I exclude Amazon, John Lewis exclusive products and product-days when JL does not stock an item. I am only looking at the pre-treatment period when JL was price matching.

consistent with Ashenfelter et al. (2013) who find that white-good products have a shelf life of around a year. As shown in Figure 1, we can see that there are 1,820 products at the start of the sample, which reduces to 1,631 products by the week of treatment, and finishes at 1,203 products. This means that 11% of products are discontinued before treatment.

Figure 1: Number of Products Sold at John Lewis During Sample Period



Note: week 1 begins on 19th April 2022 whilst the final week begins on 24th April 2023. Grey line at week 18 represents the date of treatment. There may be fewer price observations in certain weeks than indicated in the graph if the product is temporarily out of stock.

Looking at other retailers, the product discontinuation rate is lower than that of John Lewis: for AO the rate is 24.4%, for Amazon 13.5%, Argos 16.0% and Currys 12.6%. The higher product discontinuation rate at John Lewis could reflect the fact that JL puts more effort in to curating their range of stocked products, relative to the other retailers in the sample. In the case of Amazon, whilst it has a lower product discontinuation rate than John Lewis, it has a significantly higher stock-out / unavailability rate.

4.2 John Lewis Exclusive Products (counterfactual)

382 of the sampled products are sold exclusively by John Lewis and form the basis for the counterfactual group in the empirical analysis. In other words, around 21% of products stocked at John Lewis in the electrical/white-good category are JL-exclusive and do not fall under the NKU pledge. Since April 2021, John Lewis has an own-brand product range called ANYDAY. Of the 382 JL-only products in the control group, 69 (18%) are own-brand whilst the remaining 313 (82%) are produced by branded manufacturers.

JL-only products are generally similar in category to the non-JL-only products, with the exception that JL-only products do not contain those in the product category "electricals". 73.6% of JL-only observations come from the *home appliances* product category, 25.0% from *mobiles*, *cameras* and *smart tech* and 1.4% from *small appliances* and *vacuums*. This compares with 66.5% of non-JL-only observations coming from *home appliances*, 28.0% from *mobiles*, *cameras* and *smart tech*, 3.5% from *small appliances* and *vacuums*, and 2.0% from *electricals*. Focusing on the sub-sub category, JL-only products span 31 out of the 45 product sub-sub categories (see Table 10 in Annex A.2). In general, the distribution of observations in these sub-sub categories is rather consistent, with the exception of *vacuums*, of which there are no JL-only products, whilst 6.8% of the remaining sample consists of *vacuums*.

Comparing average (JL) prices across JL-only products and non-JL-only products, it can be seen that JL-only products have a higher average price than non-JL-only products (average price of £732.25 for non-JL-only products versus £1,165.80 for JL-only products). It is not clear whether this is because they have no LPG applied (and no competition) or because JL stocks higher-end products, relative to its competitors. Nonetheless, this supports the earlier assertion that John Lewis might be able to recoup margins on sales of JL-exclusive products, if the LPG helps to attract customers that would otherwise shop elsewhere.

Finally, the average quality rating between JL-only and non-JL-only products is compared and there is no significant difference in the quality rating (non-JL-only products have an average quality rating of 4.38 against a rating of 4.36 for JL-only products). However, JL-only products have much fewer reviews (34 versus non-JL-only average of 133), suggesting that JL-only products may be less popular, or newer.

5 Empirical Strategy

In order to evaluate the impact on prices at John Lewis and competing retailers following the removal of the LPG, the primary empirical strategy is to use a difference-in-difference (DiD) approach comparing the treatment group with a control group. This allows capturing any retail-specific shocks which would cause prices to change for reasons other than the removal of the LPG.

The following difference-in-difference regression equation is estimated:

$$ln(Price_{irt}) = \alpha_i + \delta_r + \gamma_t + \beta_1 Post_t + \beta_2 Treatment_r + \beta_3 Post_t * Treatment_r + \epsilon_{irt} \quad (1)$$

Where i is a product sold at retailer r at time t. In the primary specifications, the time period is given at the daily frequency. Product, α_i, retailer δ_r, and time fixed effects, γ_t, are included, where the latter captures common seasonal trends amongst retailers. The error term ϵ_{irt} is a random shock with $E(\epsilon_{irt} \mid Post_t, Treatment_r) = 0$. The coefficient of interest is β_3, the coefficient on the DiD interaction term, which gives the average treatment effect for the treated (ATT). The variable $Post_t$ refers to an observation with a time period after treatment (i.e. after 23rd August 2022). The variable $Treatment_r$ captures treated retailers versus the control group. The control group used is John Lewis exclusive products - defined as products which were not stocked at competing sampled retailers - which were not subject to the LPG because the product had no competitor. This choice of counterfactual is favoured in the (limited) empirical LPG literature, perhaps due to a lack of suitable alternative (Zhuo (2017); Manez (2006)). I cluster standard errors at the product-level, where it is expected that pricing shocks are product-specific and following Zhuo (2017) and Ashenfelter et al. (2013).[28]

Additional regressions look at the response at individual retailers or retailer groups (focal/insiders/outsider) by including interaction terms. More specifically the term attached to coefficient β_3 becomes $Post_t * RetailerDummy$ where the Retailer Dummy takes a value of 1 for a given retailer. A similar dummy and interaction term is created for the focal/insiders/outsider retailer groups. Heterogeneous effects are explored by including a full triple interaction between post, treatment, and the heterogeneity (product sub-category, number of competitors, whether Amazon is the lowest price, pre-treatment average price, level of product dispersion, and average product quality).

The DiD set-up taken compares the treatment of the removal of an LPG policy with a group who were once subject to the LPG but no longer are, compared with a control group that never faced the LPG. Such a set-up differs slightly to the canonical DiD design and is known as a reversal design, a popular approach widely used in the literature (e.g. Bleemer & Mehta 2022, Cao & Chen 2022, Grönqvist et al. 2020, Slattery et al. 2023).

In addition to the static difference-in-difference set-up provided in equation (1) I also estimate a dynamic version, as shown below in equation (2). The set of Day_q denotes indicator variables for each day within

[28] In Section 7, as a robustness check, I also cluster at the product*retailer level.

the sample period. T_1 represents the number of days before treatment (118) whilst T_2 represents the number of days on and after treatment (247). The day before treatment, $t = -1$, is used as the reference group. By testing whether the 118 coefficients (β_q) on the leads are jointly and individually insignificant it is possible to see whether there is any violation of the parallel trends assumption.

$$ln(Price_{irt}) = \alpha_i + \delta_r + \gamma_t + \sum_{q=-2}^{T_1=-118} \beta_q * Day_q * Treatment_r + \sum_{q=0}^{T_2=247} \beta_q * Day_q * Treatment_r + \epsilon_{irt} \quad (2)$$

Whilst the LPG was not removed until 23rd August 2022, the policy was announced in the media on 25th February 2022.[29] There is no indication that John Lewis adjusted prices in anticipation of removal and discussions with a former executive confirmed that consumers were able to continue to price match – and the activities of the NKU price match team continued – until removal on 23rd August. Nonetheless, I test the data for whether any anticipatory effects were observed.

5.1 SUTVA

There may be concerns of SUTVA violation given that John Lewis is a multi-product profit maximising retailer which ought to adjust prices across its entire product range in response to a change in competitive environment, potentially invalidating the claim that non-LPG-eligible products ought to be immune to the effects of the LPG removal (treatment). To argue that this is not the case, I show that the control group is not affected by the treatment.

I evaluate what happens to the treatment group post-treatment by estimating a simple regression of the dependent variable (natural logarithm of prices) on a post-treatment dummy variable (which equals 1 if the date is after 23rd August 2022), and a product fixed effect.[30] Looking at the treatment group in aggregate, I find that prices rise by 2.1% (p=0.000) post-treatment. In comparison, there is no statistically significant increase in prices (0.6% increase; p=0.479) for control products. This suggests that the treatment does cause an effect for the treated group whilst it does not cause an effect on the control group, supportive that there is not a SUTVA violation.

As discussed in Section 4, the control group consists of JL-branded products (18%) and national brands (82%). It might be suspected that John Lewis has greater control of pricing decisions on JL-branded goods relative to national brands which might mean there is greater scope for SUTVA violation on such products. Alternatively, national brands might be more substitutable with treatment products than JL-branded products, implying that SUTVA violations may be greater on such products. To alleviate these concerns, I repeat the before/after comparison for the control group again but separate the control group into JL-branded products and national brands. I find that neither group exhibits a statistically significant increase in prices (JL-branded: coefficient = -0.9%, p=0.255; national brands: coefficient = 0.9%, p=0.365). Additionally, I estimate a difference-in-difference regression, including product and time fixed effects, comparing the control group of JL-branded products against a placebo treatment of national brands and find a statistically insignificant treatment coefficient (coefficient = 1.7%, p=0.167). These results emphasise that the control group is not affected by treatment, even when we may suspect that certain types of products (JL-branded / national brands) may be especially susceptible to SUTVA violation concerns.

Finally, it is noted that if I am wrong, and there is a SUTVA violation - because John Lewis is profit-maximising across products, and so the counterfactual group is contaminated by treatment - then the estimated results would be attenuated, such that the findings represent a lower bound on the true effect.[31]

[29] For instance, see https://www.bbc.co.uk/news/business-60522421 [accessed on 25th February 2022] or https://www.theguardian.com/business/2022/feb/25/john-lewis-drops-never-knowingly-undersold-pledge [accessed on 29th May 2024].

[30] In this simple analysis, I am just focusing on the treatment effect, without comparison with a control group, so cannot control for industry demand or supply shocks which may be driving these observed price increases. The baseline specification controls for these factors.

[31] To see this, consider that products in the control group (JL-only products) would be positively correlated with treatment product prices.

5.2 Policy Exogeneity

Another necessary assumption for the validity of the empirical strategy is that the treatment policy is exogenous to the dependent variable. This means that the reasons for JL deciding to remove the LPG need to be independent of any factors affecting product prices. As explained in Section 2, John Lewis claimed that the policy was removed because consumers were confused and sometimes angered that the policy did not apply to online-only retailers, such as Amazon. In its public announcement about the removal of the LPG the retailer explained that "Never Knowingly Undersold is no longer enough to assure trust because it applies to fewer and fewer sales as shopping moves increasingly online and isn't applicable to online-only retailers. So, we're replacing it with a new approach, which all of our customers can trust because it applies to however and wherever you shop - in store or online".[32] This suggests that the decision to remove the LPG was not primarily because of increasing costs or declining profits at John Lewis.

At the same time, this policy was removed by a relatively new, outside hire, CEO. Discussions with a former executive suggest that the decision was unpopular with other insiders at the time. Pippa Wickes became Executive Director (CEO) of John Lewis in June 2020 and left abruptly and unexpectedly in February 2023. John Lewis announced that the interim CEO would lead the "delivery of the 2023 plan" suggesting there wouldn't be immediate changes following Wickes' departure.[33] This continuity in plans suggests that there should not be a change in firm behaviour around the time of departure. Nonetheless, as a robustness check I repeat the analysis but cut the dataset at the date of departure. Media reports suggest the reason for departure was a result of a culture clash between her and other JL managers (consistent with the comments made by a former executive).[34] Alongside the removal of the LPG, the CEO was also responsible for the launch of the ANYDAY product range (JL's own-brand) in April 2021 (prior to the start of this study). The author is not aware of any other significant changes made during this period. These facts support the exogeneity requirement of the treatment policy, given the timing of the CEOs arrival and the lack of agreement around the policy change.

Consequently, the timing and decision to axe the LPG is independent of market prices and can thus be considered exogenous, allowing this policy to be treated as a natural experiment. Nonetheless, for unconvinced readers, I point out that if the decision were endogenous, then it would be expected for JL to remove the LPG due to increased competition and lower prices, which was making the guarantee unaffordable. If this were the case, then one would likely expect prices to rise at John Lewis following the removal, a result that is not found. I argue that this reinforces the argument that the treatment policy is exogenous.

5.3 Parallel Trends

A well-designed natural experiment is based on the premise that the treatment would have followed the same trends as the control group absent treatment. Clearly, it is impossible to prove this to be the case, however, I provide several pieces of evidence to support this assumption of parallel trends. Firstly, I remind the reader of the discussion in Section 4 of the similarity between the treatment and control group in terms of product category and quality. There are some differences in prices, which motivates the use of fixed effects.

Secondly, to further illustrate the similarity between the treatment and control groups I show this graphically. I plot the residuals from a product-FE regression over time, comparing the treatment and control group. The reason for plotting the residual – rather than the absolute log price – is that there are strong differences in log price across products which demeaning helps to remove and show a cleaner picture. Furthermore, this better controls for compositional changes, whereby some products atrophy from the sample over time. Figure 2 shows that pre-treatment there is a similar trend between treatment and control, although, the mean residual log price is higher in the control group than the treatment group.

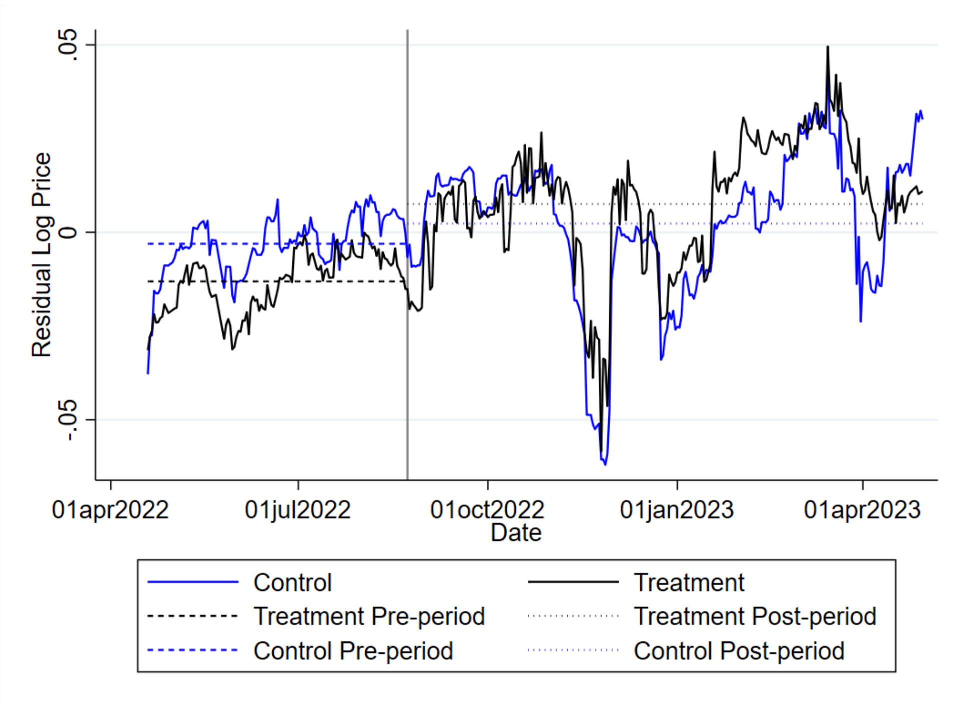
[32] See Annex A.1 for details.

[33] https://www.johnlewispartnership.media/news/waitrose/27022023/the-john-lewis-partnership-is-today-announcing-changes-to-the-executive-team [accessed on 15/07/2025].

[34] https://www.retail-week.com/people/john-lewis-wicks-departure-comes-amid-culture-clash/7043300.article [accessed on 15/07/2025].

After treatment, there is an increase in mean residual log price in the treatment group, which eventually overtakes the control group after several months. We can clearly see strong sales periods in this figure, particularly around the Christmas period where prices in both the treatment and counterfactual group fall.

Figure 2: Residual Log Price, Treatment vs Control



Note: Residuals from a product fixed effects regression are estimated and averaged across treatment and control group over time. The solid blue line shows the mean residual log price for the control group whilst the solid black line shows the mean residual log price for the treatment group. The grey line shows the time of treatment whilst the (long) dash blue and black line show pre-period averages for control and treatment (respectively) and the (short) dash line shows post-period averages.

Thirdly, in support that parallel trends are not violated, I estimate a dynamic difference-in-difference equation, which can be seen in Figures 3 and 4 in Section 6.1. I find few pre-treatment coefficients to be statistically significant, supporting the assumption of parallel trends.

Finally, in Section 7, I run a series of placebo tests, introducing fake treatment dates and testing whether there are statistically significant effects on the pre-treatment dataset. I do not find any evidence that there is, again, supporting the assumption of parallel trends.

6 Results and Discussion

In Table 3 equation (1) is estimated as outlined above. In column 1, one can see that there is a positive price effect following treatment, in that price rise by around 1.6% following removal of the LPG (p=0.054). This result is driven solely by the inside retailers: disaggregating this result by retailer group, it can be seen (column 2) that there is no price effect post-treatment at the focal retailer (John Lewis) nor the outside retailer (Amazon) but inside competitors respond sharply by increasing their prices by around 2.2%. Turning to the individual retailer disaggregation (column 3), it can be seen that 5 out of the 10 inside retailers see a positive price increase, ranging from 1.7% to 6.8%.[35] This includes the retailers AO and Currys, with significant overlap of JL product coverage, but there is a statistically insignificant

[35] The coefficient for the retailer Currys is only statistically significant at the 6.1% significance level.

effect at Argos. These findings support economic theories that predict a pro-competitive argument for LPGs.

Table 3: Impact of Removal of LPG on Prices

	(1) Aggregate	(2) Insiders vs Outsider	(3) Retailers
Aggregate	0.0161^{*}	–	–
	(0.00833)		
Focal Retailer	–	0.0106	–
		(0.00842)	
Inside Retailers	–	0.0216^{**}	–
		(0.00851)	
Outside Retailer	–	0.0124	–
		(0.00905)	
AO	–	–	0.0296^{***}
			(0.00863)
Amazon	–	–	0.0123
			(0.00905)
Argos	–	–	0.00653
			(0.0110)
Boots	–	–	-0.0231
			(0.0266)
Currys	–	–	0.0165^{*}
			(0.00879)
Dunelm	–	–	-0.00807
			(0.00796)
Homebase	–	–	-0.00172
			(0.0155)
Hughes	–	–	0.0242^{**}
			(0.0108)
John Lewis	–	–	0.0106
			(0.00842)
Tesco	–	–	-0.0115
			(0.00812)
TradePoint	–	–	0.0516^{***}
			(0.0120)
Wickes	–	–	0.0660^{***}
			(0.0122)
Observations	1,486,749	1,486,749	1,486,749
Adjusted R^2	0.986	0.986	0.986

Note: Table shows the difference-in-difference estimator for the treatment effect (removal of LPG) relative to the control group (John Lewis exclusive products). The coefficients can be interpreted as percentage changes by applying the following formula: $(EXP(coefficient) - 1) * 100$. The dependent variable is the logarithm of prices for product i, at retailer r, at time t. Product, date, and retailer fixed effects are included. Robust standard errors are shown in parentheses below the coefficients and are clustered at the product level: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

It may be surprising that there is a price increase at both AO and Currys, as they both promoted their own low price guarantee throughout the period of study. One potential explanation for this price increase, despite their LPG, is that AO and Currys' LPG required activation – unlike John Lewis' pledge

– and if customer activation is low then it would be profitable for AO and Currys to increase prices despite their guarantees. There might have been even stronger price effects post-LPG-removal had these two companies' LPG not existed.

With respect to the finding that there is no pricing effect at Amazon (outside retailer), I note the existing puzzle from the empirical literature, with some evidence that Amazon is a consumer-focused firm that deviates from pure profit-maximising (Reimers & Waldfogel 2017) against other papers which find that Amazon still responds to competitive changes in the marketplace, despite being the lowest-priced retailer (He et al. 2022). The results support the former literature, with Amazon not responding to increasing prices at competitors, at least within 8-months post-policy.

The mean pre-treatment price, across all treatment retailers, is £630, so this result suggests that prices are about £10 higher, following the removal of the LPG, a not inconsequential increase in prices for consumers. Back-of-the-envelope calculations suggest that UK consumers would have saved at least a cumulative £7.5 million in the 2021-22 financial year, on products considered in the sample alone, as a result of the pro-competitive effects of the low price guarantee that John Lewis offered.[36] This figure conservatively assumes no spillover effect to products not sampled and is weighted based on the number of products stocked by each retailer, relative to the number of sampled products. Given that we sample relatively popular products, we would expect the true figure to be higher than the back-of-the-envelope calculation suggests.

To summarise the aggregate results: there are positive price effects driven by inside retailers, with no effect on the focal or outside retailers. I now outline a potential treatment mechanism which may explain these findings, before addressing potential concerns regarding non-price factors affecting competition which may impact upon the study.

It may seem surprising that inside retailers increased their price in response to JL's removal of the LPG, despite John Lewis not changing their prices. However, I suggest that the LPG removal acted as a signal to JL's competitors that JL would act less aggressively with respect to price competition and that the competitors therefore responded to this weaker price competition by increasing their prices. On the consumer side, the removal of the LPG may have been perceived as signalling that JL would increase prices (despite this not actually being observed).[37] In response, consumers may have reduced their search intensity or switched their demand to competitors, allowing competitors to increase prices. Both mechanisms rely on a signalling argument and hence depends upon the underlying beliefs of competitors and consumers. I do not have sufficient data on consumer/retailer perceptions to be able to empirically test the exact underlying mechanism. However, in Section 6.2, I provide evidence that post-treatment price increases are greater for products with higher pre-treatment price dispersion. One interpretation of this finding is that prices rise more on products that witness lower search behaviour by consumers, consistent with the explanation provided here.

It is noteworthy that whilst a pro-competitive effect of LPGs is observed, the low-cost signalling argument does not seem to be consistent with the evidence in this setting. This is particularly the case given that John Lewis, the focal retailer, has higher costs than its retail competitors, with strong warranties and guarantees, and high levels of service provision. Nonetheless, other aspects of signalling may be important in this context, as outlined above. Furthermore, the majority of the theoretical literature (both pro- and anti-competitive) relies on the framework of a single-product retailer. In the context I study, all retailers are multi-product retailers, who might benefit from cross-product sales from products where the LPG applies to non-eligible products. This may explain why John Lewis was able to offer a pro-active low price guarantee for nearly 100 years, if it was able to recoup low margins on LPG-eligible sales with higher margins for other products that consumers may buy after being attracted to the retailer.

Whilst the primary focus of this study is on the pricing impact of LPGs, service provision, product quality, and product range are other important aspects of the competitive process. These factors are unlikely to change within the year period of study and so are likely to be accounted for in the product

[36] I multiply the regression coefficients (where statistically significant at the 10% level) estimated in Table 3 by individual firms' total UK revenues in the 2021-22 financial year, sourced from their financial reports. For AO, Currys, and Wickes I am able to locate product-specific revenues (i.e. excluding service revenues). I adjust this figure to account for the share of products included in this study (i.e. Table 2) relative to the share of total products sold by the retailer, based on catalogue figures from June 2025 or company statements (it is not possible to weight this by sales quantity or by revenue).

[37] The removal of the LPG was well highlighted in the UK press, particularly given the historical nature of the promise.

and retailer fixed effects. Furthermore, there is no evidence to suggest that product quality or service provision were affected by treatment. This can be seen by testing whether there is a change in reported quality ratings before versus after the LPG was removed. A regression analysis of weekly log quality ratings on treatment, controlling for product fixed effects, yields a statistically insignificant coefficient (coefficient = -0.0031; p=0.516). This highlights that the factors associated with product reviews – often encompassing product quality and service provision – are unaffected by treatment.

The reader may also be concerned about whether the high level of product atrophy, observed in Figure 1, comes from a strong motive to replace existing products with exclusive products during the sample when the LPG existed relative to the post-treatment period when there was no LPG. If this were the case, then product range would be affected by treatment, which may affect competition other than through the pricing mechanism. To elaborate, since the LPG only applies to exact same products, there is an inherent incentive for a retailer to stock own-brand/exclusive products or to ask manufacturers to make minor product adjustments which would make the LPG redundant. On the other hand, if consumers realise that few products are actually covered by the LPG then they won't believe it and the signalling effect is lost, such that there is no competitive effect from using LPGs. This clearly illustrates the trade-off that retailers employing LPGs face in deciding their product range. This trade-off is especially true if it is possible to replace the discontinued product with an exclusive product for which the LPG does not then apply.

Examining the level of product discontinuities in Figure 1, there does not appear to be a series break in week 18 – when the LGP was removed – suggesting that this incentive did not motivate product discontinuation at John Lewis. To test this hypothesis more rigorously, I generate a dependent variable which is the weekly difference in number of product observations (i.e. weekly number of product discontinuations) and test to see if there is an increase in the number of discontinuations post-treatment (after week 18). The coefficient on such a regression is indeed statistically insignificant (coefficient = -0.29; p=0.898).

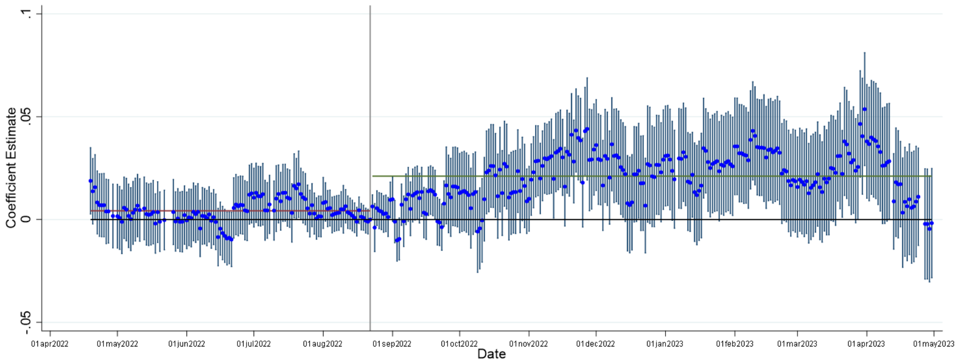
To further test this hypothesis, I can also compare the rate of discontinuation between JL-only products and non-JL-only products, where I would expect the aforementioned incentive to be stronger on non-exclusive products than JL-only products. At the start of the sample there are 382 JL-only products and 1,438 non-JL-only products, whilst at the end, there are 254 JL-only products and 949 non-JL-only products. I therefore see an attrition rate over the sample period of 33.5% for JL-only products and 34.0% for non-JL-only products. In summary, there is no evidence that John Lewis discontinues products faster when it has a direct incentive from LPG-related savings than when it does not have such an incentive.

Next, I explore whether there is evidence of a treatment effect with respect to demand. Whilst I do not have sales information for any retailer, I do have information on the rating and number of product reviews left on the John Lewis website. An existing literature uses the number of customer reviews to proxy for demand (e.g. Cabral & Hortacsu 2010). Replacing the dependent variable in Equation (1) with the log number of reviews, I re-estimate this equation and find no statistically significant coefficient attached to the DiD coefficient (coefficient = 0.0304; p-value = 0.190). To the extent that this proxy is useful – and bear in mind, it only captures reviews left on the online platform for one of the retailers in this study – it demonstrates that there was no change in demand at the focal retailer following treatment, which is unsurprising in the context that there were no treatment price effects found at the focal retailer. Nonetheless, it may illustrate that increasing prices post-treatment at other retailers, such as AO and Currys, did not seem to drive demand towards John Lewis. Whilst the evidence is weak, this is supportive of our proposed treatment mechanism, which assumes that consumers' perception of John Lewis is that it is increasing prices post-treatment.

6.1 Dynamic Effects

Next, the dynamic effects are investigated through estimation of equation (2). Figure 3 shows the dynamic effects of the treatment, in aggregate across all retailers. Prior to the LPG removal, only 4 leads are statistically significant at the 5% confidence level, providing evidence that the parallel trends assumption is not violated (Autor 2003). There is also no visible anticipation effect to the pre-announced removal.

Figure 3: Dynamic difference-in-difference estimation



Note: Figure shows the coefficient estimates at the daily frequency for the aggregate group of all retailers. The vertical grey line on 22nd August shows the date the day before treatment, the horizontal red line shows the average of pre-treatment coefficients whilst the horizontal green line shows the average of post-treatment coefficients. Vertical lines around coefficient estimates are confidence intervals at the 5% level of significance. Gaps indicate missing data.

Disaggregation of the dynamic effects by retailer group (focal, insiders, and outsider) in Figure 4 shows that, for the focal retailer (John Lewis), prices do not rise on average but there are clearly periods where prices are high, post-treatment, particularly in February and March. The use of day fixed effects captures any jointly observed variation in prices around particular holidays or sales period.[38] This might suggest that John Lewis was reluctant to increase prices immediately but may have done so in the longer-term. For the inside retailers, the treatment effect takes around a month to materialise but is relatively stable thereafter. However, there is a noticeable drop in treatment effect in the final month of the sample. Lastly, there are no noticeable treatment effects for the outside retailer (Amazon). Prior to treatment, the focal retailer had 0 (out of 118) leads that are statistically significant at the 5% confidence level, the insiders 22, and the outsider 12. These seem sufficiently small to suggest that the parallel trends assumption is not violated.

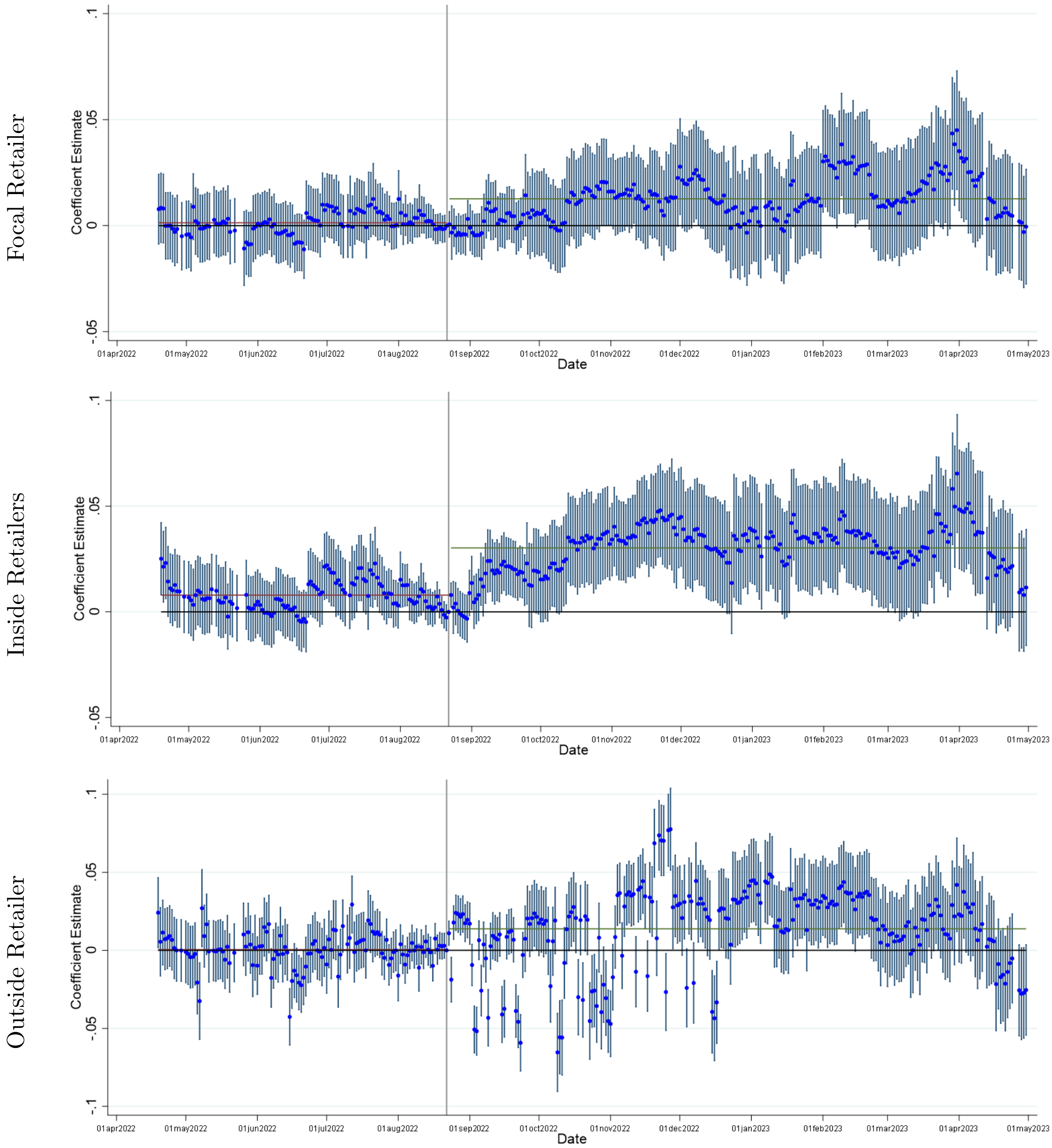
The dynamic effects for individual retailers are shown in Figure 5 (in Annex A.3).[39]

To further demonstrate the dynamic nature of the treatment effect, I re-estimate the static difference-in-difference specification but replace the post variable with a variable which equals zero prior to treatment, 1 in the two months following treatment (i.e. until 23rd October) and 2 in the months following this. As shown in Table 4, there are insignificant price effects in the short-run (first 2 months) but strong treatment effects in the long-run (with even the aggregate variable exhibiting a statistically significant coefficient). Compared to the overall treatment effect of a 2.2% price rise at inside competitors, in the long-run prices rise by a higher 2.7%, demonstrating the lag taken to adjust prices / respond to John Lewis' LPG removal.

[38] It is not possible to add specific dummy variables capturing sales/holiday periods as controls when using day fixed effects.

[39] In terms of the pre-trends test for other retailers, the following number of statistical leads are found: 14 for AO, 12 for Amazon, 62 for Argos, 3 for Boots, 9 for Currys, 22 for Dunelm, 37 for Homebase, 17 for Hughes 0 for John Lewis, 0 for Tesco, 11 for Tradepoint and 4 for Wickes. The very high number of statistically significant leads for Argos suggests that parallel trends does not hold for this retailer, and one should be cautious about interpreting the results for this retailer (which are statistically insignificant anyhow).

Figure 4: Dynamic difference-in-difference estimation by retailer group



Note: Figure shows the coefficient estimates at the daily frequency for the aggregate group of all retailers. The vertical grey line on 22nd August shows the date the day before treatment, the horizontal red line shows the average of pre-treatment coefficients whilst the horizontal green line shows the average of post-treatment coefficients. Vertical lines around coefficient estimates are confidence intervals at the 5% level of significance. Gaps indicate missing data.

Table 4: Impact of Removal of LPG on Prices, short-run vs long-run

	(1) Aggregate SR	(2) Aggregate LR	(3) Insiders vs Outsider SR	(4) Insiders vs Outsider LR	(5) Retailers SR	(6) Retailers LR
Aggregate	0.00526	0.0208^{**}	–	–	–	–
	(0.00521)	(0.00997)				
Focal Retailer	–	–	0.00267	0.0141	–	–
			(0.00537)	(0.0101)		
Inside Retailers	–	–	0.0106^{*}	0.0263^{***}	–	–
			(0.00542)	(0.0102)		
Outside Retailer	–	–	-0.00191	0.0182^{*}	–	–
			(0.00604)	(0.0107)		
AO	–	–	–	–	0.0180^{***}	0.0345^{***}
					(0.00561)	(0.0103)
Amazon	–	–	–	–	-0.00192	0.0182^{*}
					(0.00604)	(0.0107)
Argos	–	–	–	–	0.00164	0.00908
					(0.00796)	(0.0128)
Boots	–	–	–	–	-0.0117	-0.0260
					(0.0241)	(0.0287)
Currys	–	–	–	–	0.00738	0.0206^{**}
					(0.00605)	(0.0104)
Dunelm	–	–	–	–	-0.0118^{**}	-0.00632
					(0.00476)	(0.00958)
Homebase	–	–	–	–	-0.000920	-0.00101
					(0.0128)	(0.0173)

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	(1) Aggregate SR	(2) Aggregate LR	(3) Insiders vs Outsider SR	(4) Insiders vs Outsider LR	(5) Retailers SR	(6) Retailers LR
Hughes	–	–	–	–	0.0151*	0.0283**
					(0.00863)	(0.0126)
John Lewis	–	–	–	–	0.00268	0.0141
					(0.00537)	(0.0101)
Tesco	–	–	–	–	-0.0271***	-0.00675
					(0.00527)	(0.00961)
TradePoint	–	–	–	–	0.00719	0.0676***
					(0.00791)	(0.0144)
Wickes	–	–	–	–	0.00524	0.0886***
					(0.00810)	(0.0147)
Observations	1,486,749	1,486,749	1,486,749	1,486,749	1,486,749	1,486,749
Adjusted R^2	0.986	0.986	0.986	0.986	0.986	0.986

Note: Table shows the difference-in-difference estimator for the treatment effect (removal of LPG) relative to the control group (John Lewis exclusive products). The dependent variable is the logarithm of prices for product i, at retailer r, at time t. Product, date, and retailer fixed effects are included. Robust standard errors are shown in parentheses below the coefficients and are clustered at the product level: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

6.2 Heterogeneous Effects

Next, I explore whether there are any heterogeneities in the findings, in terms of product sub-categories, the product pre-treatment average price, the number of competitors and competition from Amazon, the level of product price dispersion, and the average product quality.

Firstly, I test whether there are heterogeneities across product sub-categories (Table 5). It is found that the results are driven by five product sub-categories, jointly accounting for 57% of sampled products: cameras, cookers and ovens, fridges and freezers, microwaves, and washing machines and tumble dryers (there are statistically weak effects also for dishwashers, electricals, and smart tech). These products are high price items where we might expect consumers to shop around for these items.[40] It is somewhat surprising that retailers increased prices more – relative to lower-priced products – on these products post-treatment, given that we might expect consumers to search around more for such products. This might indicate that the LPG was displaying strong pro-competitive effects in these important high-priced products.

Turning to heterogeneous responses in price, I calculate the average pre-treatment price across all retailers and take the log of this value. As shown in Table 6, there is a positive coefficient attached to the triple interaction term for average pre-treatment price, although it is only statistically significant at the 10% significance level. To further explore this, I create 5 price bins by taking the average pre-treatment price across all retailers depending on the 1/5th percentile. It can be seen that, relative to the lowest priced bin, products priced at between the 20th and 80th percentile see higher price increases, with there being no statistically significant effect for the most expensive products. This is consistent with the evidence for the product subcategory heterogeneity, where more expensive categories had stronger treatment effects.

Next, I test whether there are heterogeneities across the number of competitors. One might expect the observed upward price effects, post-LPG-removal, to be stronger for products which face less competition, as retailers have more scope to increase prices on such products, without losing customers (Bresnahan & Reiss 1991, Genakos & Pagliero 2022). In other words, competition is softer when there are fewer competitors. I actually find some evidence for the opposite effect (Table 7): prices rise by more, post-LPG-removal, when there is more competition than less competition.

Whilst this finding is surprising, there is some evidence that this is driven by a composition effect – particularly at the upper end of the competition spectrum - rather than reflecting a true competition effect. 67% of products which have 6 competitors are in the product sub-category cookers and ovens, whilst 100% of products which have 7 competitors are in this same category.[41]

In addition to directly testing whether the level of competition alters the treatment effect, I can also test whether competition from Amazon plays a role. To do this, I create a variable which captures the difference in pre-treatment average product price at Amazon, and at all retailers except Amazon.[42] I find that there is a (small but) negative coefficient on the triple interaction term, which is interpreted as showing that the treatment effect is weaker when Amazon sells at a lower price than other retailers (Table 8). I also create two dummy variables, one for when Amazon has a lower average pre-treatment price than other retailers (55% of observations), and one for when Amazon has a higher average pre-treatment price than other retailers (22%).[43] Consistently, it is found that when Amazon has, on average, a cheaper price than other retailers, the treatment effect is stronger; or, conversely, when Amazon has, on average, a more expensive price than other retailers, the treatment effect is weaker. Hence, this evidence tells us that the treatment effect is stronger when Amazon is the cheapest retailer. One explanation for this might be that for such products, non-Amazon retailers don't try to compete with Amazon and hence have greater freedom to increase prices, ignoring the competitive constraint from Amazon in these cases.[44] It

[40] The average pre-treatment prices for these 5 categories were £894, £1,160, £1,090, £392, and £651 compared with an average pre-treatment price of £281 across all other product categories.

[41] The remaining shares are 17% from electricals and 17% from washing machines and tumble dryers (the latter category was also driving the composition results above).

[42] This value is missing for control observations but is explicitly set to zero to allow for regression analysis. Additionally, I repeat this exercise but only calculate the non-Amazon retailers average price using the major non-Amazon retailers (Argos, AO, Currys, John Lewis) and the results are very similar.

[43] The remaining 23% of observations either do not have an Amazon pre-treatment price observation, or the difference is zero.

[44] Whilst there are differences in the product sub-categories for which Amazon is, on average, cheaper, these differences

Product Subcategory	Treatment Effect
Post*Treat Interaction	-0.0986^{*}
	(0.0528)
Cameras	0.108^{**}
	(0.0540)
Computing and Gaming	NA
Cookers & Ovens	0.123^{**}
	(0.0531)
Cooking Appliances	NA
Dishwashers	0.103^{*}
	(0.0572)
Electricals	0.111^{*}
	(0.0578)
Electronics	0.313
	(0.280)
Fridges and Freezers	0.154^{***}
	(0.0544)
Home Telephones	0.0744
	(0.0572)
Ironing	0.0608
	(0.0727)
Microwaves	0.195^{***}
	(0.0663)
Mobile Phones	NA
Smart Home	0.0909
	(0.0564)
Smart Tech	0.0912^{*}
	(0.0547)
Washing Machines and Tumble Dryers	0.114^{**}
	(0.0534)

Table 5: Heterogeneity for Product Sub-Category

Note: Table shows the coefficient on the triple interaction term ($Treatment_r * Post_t * SubCategory_i$). There are $1,486,749$ observations and adjusted $R^2 = 0.986$. The baseline product subcategory is *Heating*. NA refers to omitted product sub-categories where the product sub-category does not exist in the control group and can therefore not be estimated. The dependent variable is the logarithm of prices for product i, at retailer r, at time t. Product, date, and retailer fixed effects are included. Robust standard errors are shown in parentheses below the coefficients and are clustered at the product level: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

might be the case that for such products, consumers consider non-price factors as being important.

One mechanism to explain the headline result is that John Lewis' removal of the LPG signals to consumers that John Lewis will compete less aggressively. Consequently, consumers might reduce their search intensity. One indication of consumer search behaviour comes from price dispersion: if price dispersion is high, then it would suggest that consumers are not searching, for whatever reason. I therefore test whether there is a heterogeneity in the treatment effect depending on the pre-treatment price dispersion of products. To do so, I take the highest and lowest price for every product on every pre-treatment day before taking the average of the highest and lowest prices across products. I then interact this with the post and treatment variables and find a positive coefficient on the interaction term (Table 9). Whilst the

do not appear to be driving the results.

Table 6: Heterogeneity for Pre-Treatment Price

Average Pre-Treatment Price	$Post * TreatInteraction$	$Post * Treat * PriceInteraction$
Continuous Measure	-0.0492	0.0108*
	(0.0413)	(0.00618)
Bin 2 (£161 – £368)		0.0573**
		(0.0256)
Bin 3 (£369 – £631)	-0.0209	0.0534**
		(0.0209)
Bin 4 (£632 – £1037)		0.0728**
		(0.0360)
Bin 5 (£1038 – £6713)		0.0235
		(0.0199)
Observations	1,486,554	1,486,554
Adjusted R^2	0.986	0.986

Note: Table shows the coefficient on the triple interaction term ($Treatment_r * Post_t * AveragePreTreatmentPrice_i$). The baseline product subcategory is Bin 1 (£0 - £160). The dependent variable is the logarithm of prices for product i, at retailer r, at time t. Product, date, and retailer fixed effects are included. The difference of 195 observations compared to other regressions is a result of one product not being stocked pre-treatment. Robust standard errors are shown in parentheses below the coefficients and are clustered at the product level: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

coefficient is very small in magnitude, it is interpreted as a £1 increase in the average difference between the highest and lowest price for a product on a given day leads to a 0.01% increase in the treatment effect. The median level of product price dispersion is £33. I interpret this finding as suggesting that firms increased prices more, post-treatment, on products where consumers searched less.[45] This makes intuitive economic sense.

Finally, I tested whether there existed any differences in post-treatment response depending on the average quality of the product, and did not find any evidence to support this.[46] This may stem from the fact that that the vast majority of observations had a quality rating greater than 4, limiting variation in this measure.

7 Robustness Checks

A battery of robustness checks are conducted and it is found that the results are robust to (1) the definition of counterfactual, (2) the exclusion of certain retailers, (3) incorrect treatment date placebo tests, (4) the product category composition of the treatment vs counterfactual, (5) product composition over time, (6) to the nature of clustering, (7) dropping smaller retailers, and (8) restricting the sample to end on 27th February. Additionally, I present evidence from using Amazon as the counterfactual group, and do not find any evidence of anti-competitive effects of LPGs.

Firstly, the counterfactual group of JL-only products is constructed by considering products that were not sold at any of the competing retailers.[47] This can be constructed either by considering (i) all 18 competing retailers for which data was collected, or (ii) only for the 11 retailers used in the analysis

[45] Note that this implies that search intensities are complements.

[46] Average quality per product was calculated and tests were conducted on the rounded integer value, as well as binned values, and no differential effect was found.

[47] See also the discussion in footnote 24.

Table 7: Heterogeneity for Product-Level Competition

Number of Competitors	$Post * TreatInteraction$	$Post * Treat * CompetitionInteraction$
Continuous Measure	-0.00398	0.00702***
	(0.00976)	(0.00193)
1		0.0435
		(0.0327)
2		0.0599*
		(0.0324)
3		0.0655**
		(0.0324)
4	-0.0440	0.0612*
		(0.0327)
5		0.0670**
		(0.0339)
6		0.114***
		(0.0355)
7		0.116***
		(0.0348)
Observations	1,486,749	1,486,749
Adjusted R^2	0.986	0.986

Note: Table shows the coefficient on the triple interaction term ($Treatment_r * Post_t * Num_Competitors_i$). The baseline number of competitors is zero and the control group only contains zero competitors. The dependent variable is the logarithm of prices for product i, at retailer r, at time t. Product, date, and retailer fixed effects are included. Robust standard errors are shown in parentheses below the coefficients and are clustered at the product level: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

(remember that 7 retailers were dropped from the analysis due to the uncertainty around online versus in-store pricing). In the baseline estimations, we take approach (i), which generates 382 JL-only products. Restricting the counterfactual to (ii) results in 416 JL-only products. Equation (1) is re-estimated using the 416 products with zero-competitors and it is found (Table 11, Annex A.4) that the results are very similar, albeit with smaller standard errors and higher treatment effects.

Secondly, I re-estimate regressions including all 18 nationwide retailers for which data was collected and find very similar results, albeit with higher standard errors (Table 12, Annex A.4). Interestingly, we observe that two retailers, Decathlon and Robert Dyas, (both of which have very small overlap with JL products), exhibit statistically significant negative treatment effects. Overall, however, the key results do not hinge on the exclusion of the 7 excluded retailers that couldn't confirm in-store prices were the same as online.

Thirdly, I conduct a placebo test, where I choose several random incorrect dates for treatment and see whether I find any significant price effects. To elaborate, any post-treatment observations are dropped, and I focus only on the pre-treatment sample. I then assign a treatment value by randomly choosing 5 false treatment date and re-estimating the DiD equations from Section 6.[48] There are no statistically significant coefficients when using these placebo dates, supporting the robustness of these results and finding no evidence that the parallel trends assumption is violated.

[48] The following dates were arbitrarily selected: 20th May, 12th June, 22nd June, 5th July, 1st August.

Table 8: Heterogeneity for whether Amazon has the Lowest Price

Competition from Amazon	$Post * TreatInteraction$	$Post * Treat * AmazonInteraction$
Average Amazon Price - Average Price	0.0142*	-0.0000665***
	(0.00841)	(0.0000252)
Amazon Cheap	0.00383	0.0163***
	(0.00959)	(0.00621)
Amazon Expensive	0.0199**	-0.0159**
	(0.00852)	(0.00628)
Observations	1,279,481	1,279,481
Adjusted R^2	0.986	0.986

Note: Table shows the coefficient on the triple interaction term ($Treatment_r * Post_t * Amazon_Competition_i$). The drop in the number of observations reflects that some products never observe a pre-treatment price at Amazon, and are therefore excluded from the regressions. The control group has a missing value for the Amazon variable (as the product is not sold at Amazon). This is instead set to zero. The dependent variable is the logarithm of prices for product i, at retailer r, at time t. Product, date, and retailer fixed effects are included. Robust standard errors are shown in parentheses below the coefficients and are clustered at the product level: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 9: Heterogeneity for Pre-Treatment Price Dispersion

Price Dispersion	$Post * TreatInteraction$	$Post * Treat * DispersionInteraction$
Average (Product-Day Highest Price) – Average (Product-Day Lowest Price)	0.0120	0.0000503***
	(0.00847)	(0.0000165)
Observations	1,486,554	1,486,554
Adjusted R^2	0.986	0.986

Note: Table shows the coefficient on the triple interaction term ($Treatment_r * Post_t * Amazon_Competition_i$). The control group has zero price dispersion by definition. The dependent variable is the logarithm of prices for product i, at retailer r, at time t. Product, date, and retailer fixed effects are included. Robust standard errors are shown in parentheses below the coefficients and are clustered at the product level: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Fourthly, to ensure that the results aren't driven by differences in product sub-sub category between the treatment and control group, I repeat the analysis but first exclude any products which is in a sub-sub category that is not present in the control group.[49] In Table 13 (Annex A.4), we can see that the conclusions are robust to this exercise.

Fifthly, to ensure that the results are not sensitive to product atrophy over time, I re-estimate regressions on a sample which (1) only includes retailer-product pairs that are contained in the sample both pre- and post-treatment, and (2) only includes products, sold at John Lewis, that are contained in the sample until the final date of observation. In Tables 14 and 15 (Annex A.4) it can be seen that, relative to the benchmark, the results are similar and the conclusions unchanged. This is reassuring given that it might be expected for prices to fall as it is discontinued (clearance sales).

Sixthly, the results are robust to changing the level of clustering to the $retailer * product$ level (Table 16, Annex A.4). In fact, the statistical significance improves with such clustering.

Seventhly, given the statistically significant impact of coefficients attached to some retailers with small

[49] Products in the following sub-sub categories which are not present in the control group are removed in this robustness exercise: Bread Makers, Camcorders, Condenser Tumble Dryers, Grills, Humidifiers, Integrated Washing Machines, Laptops and Macbooks, Men's Clippers Hair Trimmers & Groomers, Mobile Phones, Shirt and Trouser Presses, Soup Makers, Steam Irons Steam Generators and Steam Brushes, Tablets, and Vacuums.

overlap between John Lewis (i.e. Hughes, TradePoint and Wickes), I re-estimate regressions after dropping smaller retailers from the sample. In particular, I only keep Amazon, John Lewis, Argos, AO, and Currys in the sample. As shown in Table 17 (Annex A.4), the conclusions are unchanged.

Eighthly, as mentioned in Section 5, John Lewis' CEO unexpectedly departed the company on 27th February 2023. Whilst the company reported that the interim CEO would continue with the plans laid out by the departing CEO, there may still be a concern that JL's firm behaviour changed after 27th February 2023. I therefore repeat the empirical exercise but cut the sample on 27th February 2023. I find that the overall conclusions are maintained (Table 18, Annex A.4). Whilst we continue to see no effect at John Lewis, there is a marginally significant coefficient attached to Amazon, where prices increase by 1.6% (p=0.057). Given that there is no statistically significant response from Amazon in the full sample, it suggests this price increase was short-lived.

In addition, I explore the use of Amazon as a counterfactual, instead of using JL-exclusive products. On the one hand, Amazon is not included in John Lewis' LPG and so is not directly affected by it. On the other hand, Amazon is likely to be indirectly affected through the change in competitor's prices. The reader is reminded that whilst there is no treatment effect found for Amazon in the full sample when using the JL-exclusive counterfactual, Amazon prices do increase post-treatment in a simple regression analysis. Nonetheless, repeating the estimation with Amazon as the counterfactual, I find no evidence of anti-competitive effects from the low price guarantee (Table 19, Annex A.4).[50] I find that there are no statistically significant treatment effects on aggregate (although the coefficient is positive), there are statistically significant results for inside retailers (prices increase by around 0.9%, p=0.037) under the retail group disaggregation.[51] Given the potential for SUTVA violation (which would act to ameliorate the results), this counterfactual is not presented as a benchmark but is instead presented to highlight that the pro-competitive finding of LPGs is robust to use of a different counterfactual group.

8 Concluding Remarks

I conclude that the evidence from the removal of a low price guarantee which applied at a major UK retailer, over a long period of time and covering thousands of products, demonstrates pro-competitive effects of low price guarantees, in contrast to much of the theoretical predictions which posit that LPGs have negative effects for consumers. I show that post-treatment, prices rise by around 2.2% at John Lewis' competitors, who were directly affected by the LPG, although there is no effect on prices at John Lewis itself nor at a directly unaffected competitor (Amazon). Prices do not rise immediately and are stronger over time. Focusing on treatment effects 2 months post-treatment I see that prices of insiders rise by 2.7%.

This suggests pro-competitive effects of the LPG – contrary to much of the existing theoretical and empirical evidence – suggesting that policymakers do not need to take action, as there seems to be little economic harm from such guarantees. Back-of-the-envelope calculations suggest that UK consumers would have saved at least a cumulative £7.5 million in the 2021-22 financial year as a result of the pro-competitive effects of the low price guarantee that John Lewis offered.[52]

These results support the pro-competitive theories, although not the low-cost signalling theory, as in this context the focal retail is not the lowest-cost retailer. Whilst it could still be profitable for the non-lowest-cost retailer to utilise an LPG – when cost differences with competitors are not too high and profits can be recouped across a product range where the LPG increases revenue – I instead focus on an alternative explanation, using a simpler signalling theory. Under this argument, the removal of the LPG by John Lewis may have signalled to competitors and consumers that John Lewis would no longer be competing aggressively on price. Such a signal may have led consumers to reduce their search intensity – if they expected higher prices at John Lewis (contrary to actual, observed, prices) - which may have allowed competing firms to increase their prices in response. Whilst I do not have data on consumer/firm

[50] JL-exclusive products are dropped but similar results are found if they are included.

[51] These results are primarily driven by the treatment response at AO, where prices rose by around 1.7%. The only other statistically significant responses were from Dunelm, Tesco (both had negative coefficients), TradePoint and Wickes (both had positive coefficients). The reader is reminded that these 4 retailers have a very limited representation in the sample.

[52] See footnote 36.

perceptions to directly test this explanation, I am able to provide evidence that post-treatment price increases are greater for products with higher pre-treatment price dispersion. One interpretation of this finding is that prices rise more on products that witness lower search behaviour by consumers, consistent with the explanation provided here.

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A Annex

A.1 Never Knowingly Undersold pledge Terms-and-Conditions

Screenshots on the John Lewis website terms and condition page were taken on 4th April 2022. These were transcribed using chatGPT model 4o. The terms and conditions are outlined below.

Are you removing your Never Knowingly Undersold Policy?

Yes, we're retiring our Never Knowingly Undersold policy this summer.

John Lewis is the brand you can trust for Quality & Value. Our Never Knowingly Undersold pledge has given customers reassurance for nearly a century.

However, Never Knowingly Undersold is no longer enough to assure trust because it applies to fewer and fewer sales as shopping moves increasingly online, and isn't applicable to online-only retailers. So, we're replacing it with a new approach, which all of our customers can trust because it applies to however and wherever you shop – in store or online.

We have pledged a £500 million investment so that we can bring you John Lewis quality at great value prices. That's 25% more than we invested in prices last year.

Retiring Never Knowingly Undersold in favour of everyday Quality & Value means that all John Lewis customers will benefit from great prices every day – without having to shop around. We will more proactively lead on great value, rather than reacting to other retailers' price changes. The £500m investment in value is 25% higher than the amount we spent on keeping prices affordable last year.

Our founding philosophy of treating customers fairly remains. Shoppers can be reassured that we'll continue to monitor other retailers' prices – especially on products that matter most to them – from mattresses to microwaves and TVs to toys – regardless of whether they're buying online or in store. We will contact customers in the summer to confirm the exact date of policy removal – until then our policy remains unchanged and in place.

How do we set fair prices for our own brand products?

We're proud of all the products we sell under the John Lewis & Partners name, because each one is developed to bring you the best quality and value. And to ensure we offer the most competitive prices, we regularly benchmark all these products against others in the market.

How do we ensure prices set for branded products are always competitive?

Our dedicated price-monitoring team proactively checks the prices of branded products at our national high street competitors, including online and during sales.

If we find that they are selling the same individual product, sold with the same service conditions, at a lower price, we'll meet that price in our shops and at johnlewis.com. So we don't expect you to find a lower price elsewhere. But if you do, you can easily make a price match request.

We apply the same national price to products in our shops and online. But if a local high street competitor within eight miles of one of our shops has a lower price, we may drop our price even further in that shop to meet that of the competitor.

How do we compare prices?

First, we need to be sure that the product is available through a high street competitor which has a national presence, rather than, for example, an online-only company. You can find out more about what we mean by high street competitors in the 'small print' section.

Our team also needs to be satisfied that the product is the same make, model, size and colour, and comes with the same conditions of sale, delivery times and service conditions we offer. To find out how we compare products and services, please see the 'small print' section.

What if you do find a lower price elsewhere?

If you do find the same product for sale at a lower price at a UK mainland high street competitor, either in store or online, and sold with the same service conditions, you can make a price match request. It's really easy – just let us know in one of the following ways:

- Make a claim using `this form`
- Talk to a Partner in one of our shops
- Call our Customer Services team on `03456 049 049` (UK) and `+44 (0) 1698 54 54 54` (from abroad)

The Small Print

What do we mean by a high street competitor?

Our high street competitors trade on the same basis as us. They have multiple premises on high streets or comparable shopping destinations, nationwide, which are freely open to the public on the UK mainland. They stock a reasonable range of goods for customers to buy and take away, with clearly displayed prices. Their websites trade under the same brand and on the same basis as their high street shops.

Businesses that trade on a different basis to John Lewis & Partners are not deemed comparable high street competitors. These include: outlets that operate only on the internet or through mail order, showrooms attached to internet-only companies, pop-up or temporary shops, collection points, auction sites, factory outlets, membership clubs, duty-free shops, market stalls or home shopping channels.

We also don't match competitors who are in administration or closing down.

Which prices do we match?

The competitor's prices need to be publicly available to all customers. For us to verify these prices, they need to be displayed in the competitor's own shop or on their website, or be the first price quoted to our price monitoring team over the telephone. The price must apply to a new, identical individual product – that's the same make, model, size and colour.

We're happy to consider an individual price match for a competitor which is further than 8 miles from one of our shops. Should we agree to your request, if they don't have a nationwide presence, we're unlikely to adjust our national or local selling price.

What if a competitor is offering a bundle of products?

We will match on a case-by-case basis the total price for the bundle of products, if the price of the bundle is publicly available to all customers and the products in the competitor's bundle are identical to products we sell, including any free items. If bundled products offered at a promotional rate by a competitor can also be bought individually at full price, we will generally not lower our price for the bundle itself in store or online.

How do we compare service conditions?

We always aim to offer you the best possible service, so we have a range of delivery options, as well as fitting and installation services if you require them. Our price-monitoring team look carefully at how the competitor sells the product, to ensure that it's comparable to our offer in the following ways:

Stock availability: We carry a wide range of products, and for us to match the price, our competitor must have the product in stock (rather than available only to order). For products that we deliver, we ask that the competitor is able to deliver in equivalent timescales to us.

Delivery or Click & Collect charges: For products that require delivery or are available for Click & Collect only, we will take any associated charges into account when comparing the total price you will pay.

Fitting or installation services: We carefully select our fitting and installation providers to ensure that they match the quality of the products we sell. We will consider lowering the combined price of the product and the service, but only if a competitor's installation service is comparable in terms of quality. We compare things like the scope of the work, the quality of the materials used, the timescale for completing the work and the accreditation of the fitters.

What about promotions?

We match price-based promotions given at the point of purchase, but we don't consider service or reward-based promotions, such as trade-ins, cashbacks, extended warranties or express delivery. If a competitor is running a very short-term promotion, we may not have a chance to lower our price, but we'll still consider refunding you the difference.

What about temporary or exclusive discounts?

We do consider temporarily discounted prices, including any publicly-available voucher codes that are published alongside the product at the point of purchase, or on the competitor's website. We don't consider special prices that are only available to certain customers – for instance negotiated prices or prices for groups such as account holders, reward card or club members. We don't consider prices that are only available through third-party websites, or with voucher codes issued to individual customers or groups.

What about special and export orders?

We will consider matching the price on special order lines that aren't part of our normal stocked range. We'll also consider matching prices for orders destined for overseas export placed using our specialist export services. But we reserve the right to refuse to match prices on all such orders where it would be uneconomical for us to do so. We'll let you know if we're able to match a price at the point of confirming your order, as after this point we are unable to consider further price match requests.

What about products in our foodhalls?

Our Foodhalls are stocked by Waitrose & Partners, so products in our Foodhalls are not covered by John Lewis & Partners pricing policy.

What about third-party in-store concessions?

Our third-party concessions, such as Kuoni, are operated by the third party, not John Lewis & Partners, so products and services in them are not covered by our pricing policy.

What about beauty consultations or spa treatments?

We will match the price for identical individual products sold with the same service conditions as set out above. But due to the individual nature of the treatments and services we offer, we do not consider matching the prices of these.

A.2 Additional Descriptive Statistics

Table 10: Differences in product sub-sub categories between the treatment sample (non-JL-only) and the control sample (JL-only)

Product Sub-Sub Category	Non-JL-only sample	JL-only sample
Air Conditioners	0.15%	0.81%
Air Purifiers	1.17%	0.54%
American Fridge-Freezers	3.60%	1.08%
Bread Makers	0.29%	0.00%
Built-in Ovens	4.81%	4.14%
Camcorders	0.29%	0.00%
Camera Lenses	4.63%	5.15%
Cameras	6.98%	3.53%
Coffee Machines	3.76%	6.51%
Condenser Tumble Dryers	0.73%	0.00%
Cookers	8.32%	17.43%
Dehumidifiers	0.22%	0.81%
Dishwashers	3.38%	4.37%
Electric Hobs	7.12%	14.10%
Fans	0.15%	4.88%
Freestanding Washer Dryers	1.54%	1.14%
Freestanding Washing Machines	3.40%	2.50%
Grills	0.95%	0.00%
Heat Pump Tumble Dryer	1.57%	2.44%
Heaters	0.15%	1.90%
Humidifiers	0.07%	0.00%
Integrated Freezers	0.29%	0.54%
Integrated Fridges	0.51%	0.54%
Integrated Washer Dryers	0.44%	0.27%
Integrated Washing Machines	0.73%	0.00%
Kettles	2.50%	1.36%
Landline Phones	1.47%	0.81%
Laptops and Macbooks	1.41%	0.00%
Men's Clippers, Hair Trimmers & Groomers	0.66%	0.00%
Microwaves	3.27%	1.90%
Mobile Phones	0.74%	0.00%
Shirt and Trouser Presses	0.37%	0.00%
Smart Home Security	5.37%	8.13%
Smart Lighting	2.21%	3.25%
Smart Speakers	1.84%	0.54%
Smart Wearables	4.44%	3.52%
Soup Makers	0.29%	0.00%
Standard Fridge Freezers	4.95%	3.33%
Steam Irons	2.86%	1.41%
Steam Irons, Steam Generators and Brushes	0.29%	0.00%
Tablets	0.60%	0.00%
Toasters	2.22%	1.63%
Upright Freezers	1.19%	0.81%
Upright Fridges	1.41%	0.60%
Vacuums	6.64%	0.00%

A.3 Dynamic difference-in-difference figures for individual retailers

Figure 5: Dynamic difference-in-difference estimation of log price at individual retailers

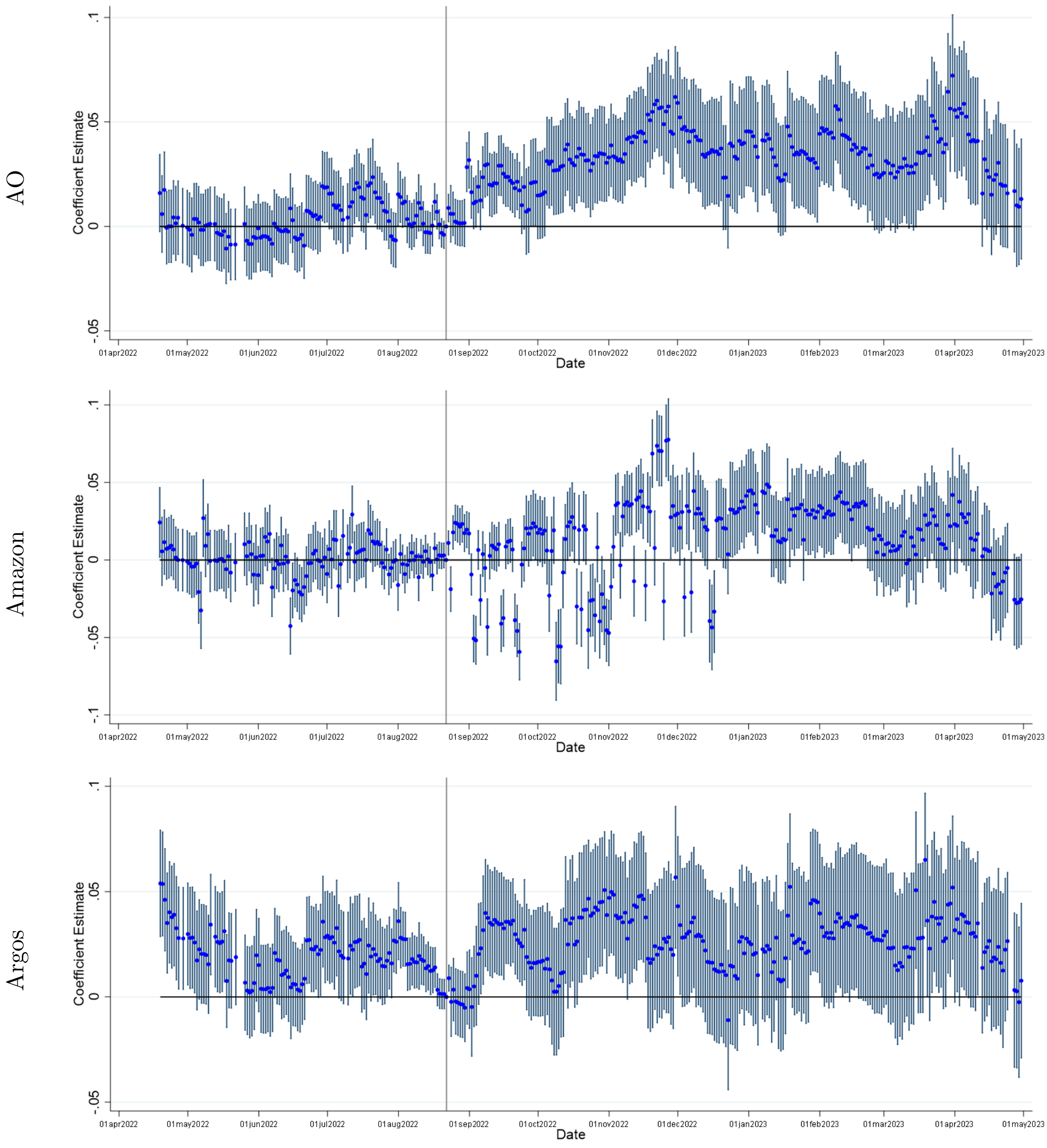
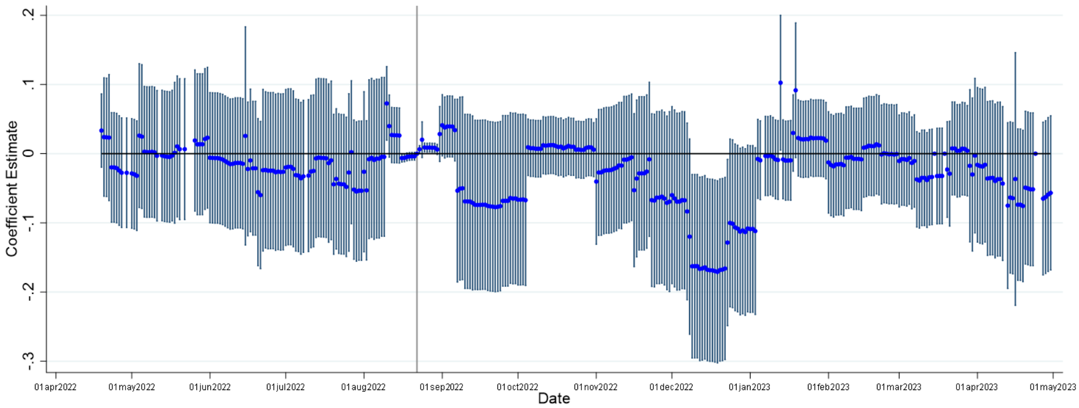
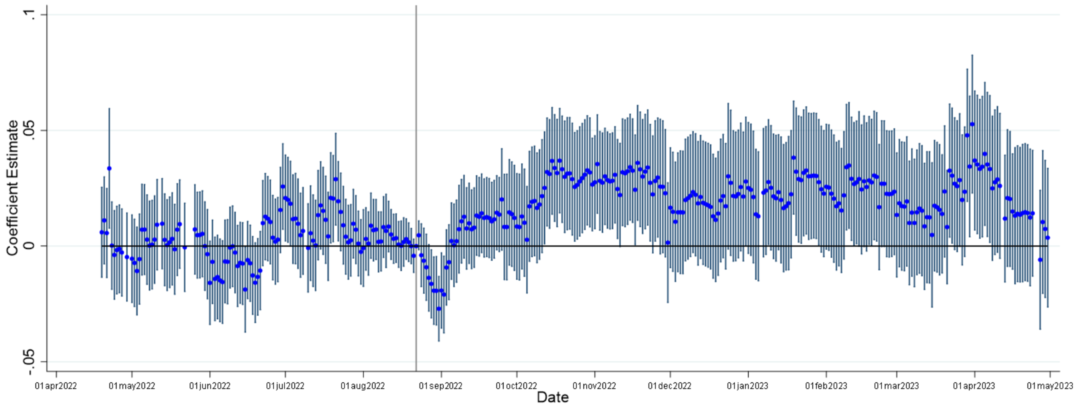


Figure 5 (continued)

Boots



Currys



Dunelm

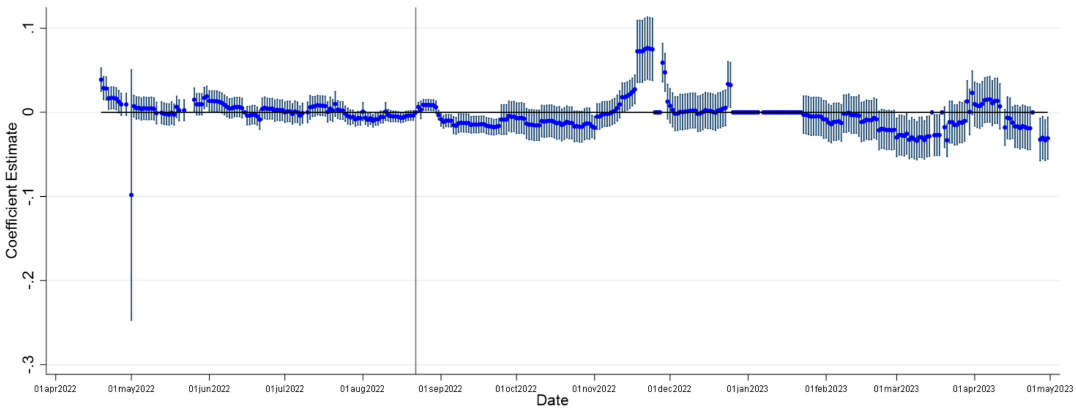
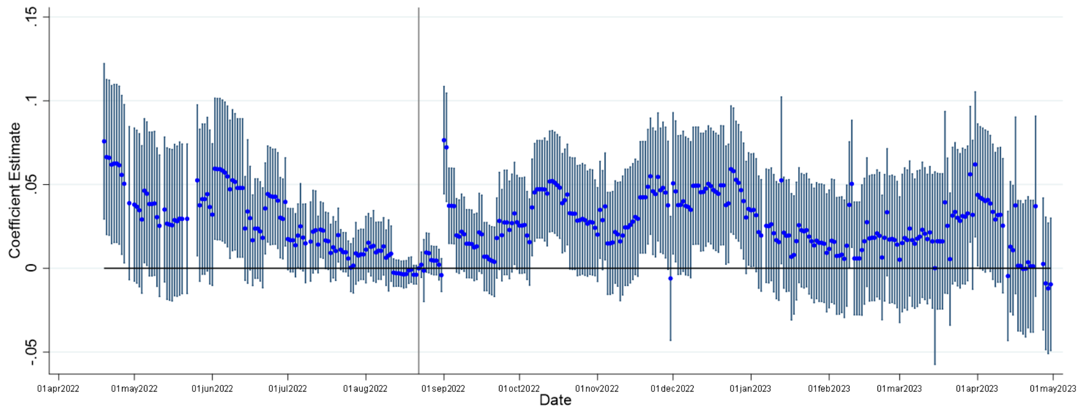
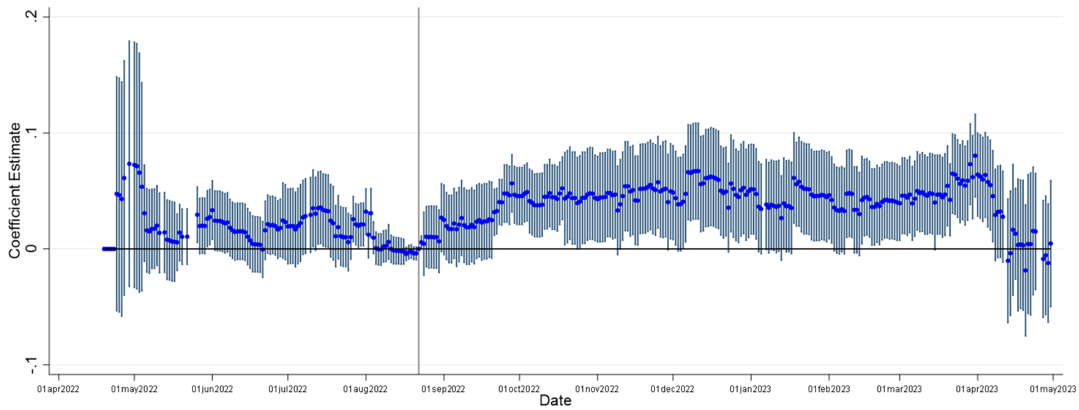


Figure 5 (continued)

Homebase



Hughes



John Lewis

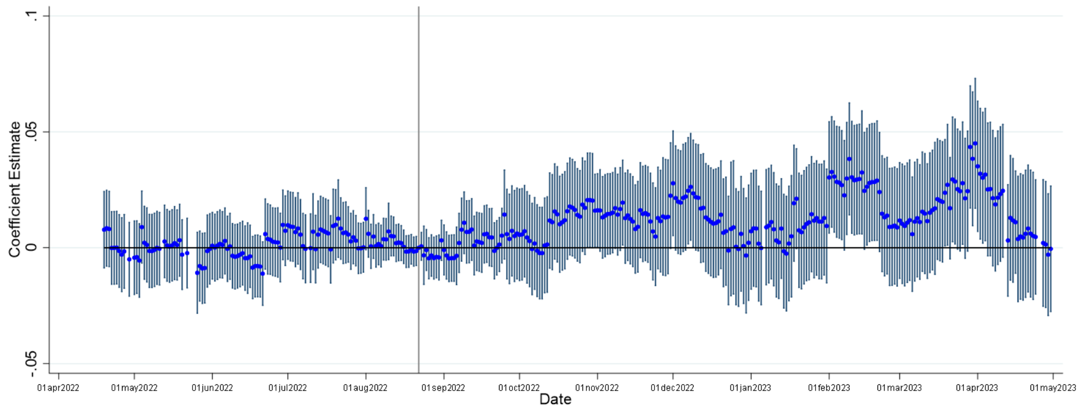
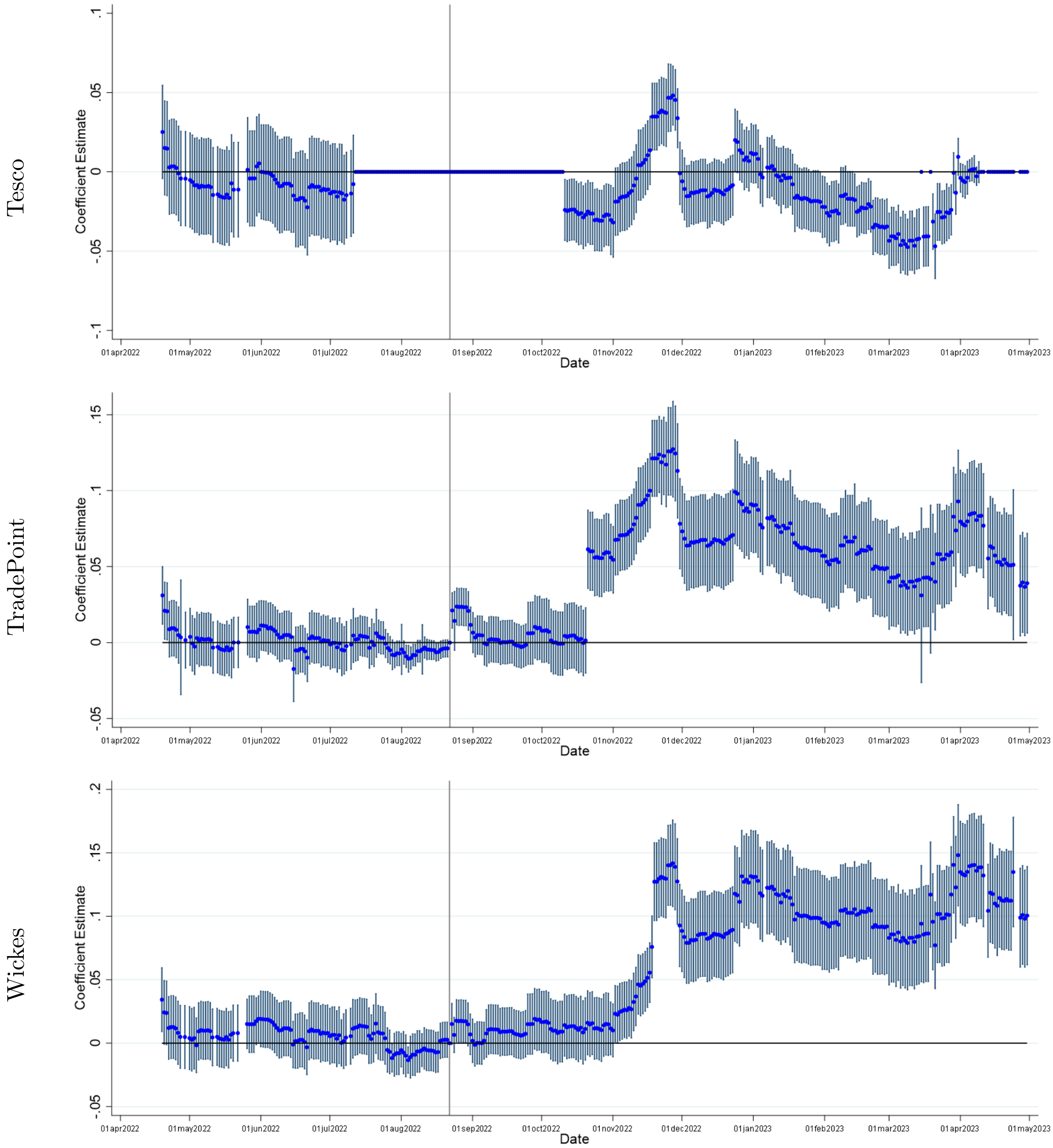


Figure 5 (continued)



Note: daily frequency, the vertical grey line on 22nd August shows the date the day before treatment. Vertical lines around coefficient estimates are confidence intervals at the 5% level of significance.

A.4 Robustness Results

Table 12: Impact of Removal of LPG on Prices, sample includes all 18 retailers

	(1) Aggregate	(2) Insiders vs Outsider	(3) Retailers
Aggregate	0.0156*	–	–
	(0.00833)		
Focal Retailer	–	0.0106	–
		(0.00842)	
Inside Retailers	–	0.0201**	–
		(0.00850)	
Outside Retailer	–	0.0125	–
		(0.00904)	
AO	–	–	0.0296***
			(0.00863)
Amazon	–	–	0.0124
			(0.00904)
Argos	–	–	0.00654
			(0.0110)
B&Q	–	–	0.0521***
			(0.0150)
Boots	–	–	-0.0232
			(0.0266)
Costco	–	–	-0.0119
			(0.0143)
Currys	–	–	0.0165*
			(0.00879)
Decathlon	–	–	-0.0855***
			(0.0320)
Dunelm	–	–	-0.00789
			(0.00796)
Homebase	–	–	-0.00164
			(0.0155)
House of Fraser	–	–	-0.00908
			(0.00796)
Hughes	–	–	0.0241**
			(0.0108)
John Lewis	–	–	0.0106
			(0.00842)
RicherSounds	–	–	-0.000122
			(0.0203)
Robert Dyas	–	–	-0.0404**
			(0.0159)
Tesco	–	–	-0.0114

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	(1) Aggregate	(2) Insiders vs Outsider	(3) Retailers
			(0.00812)
TradePoint	–	–	0.0514***
			(0.0120)
Travis Perkins	–	–	0.0495***
			(0.0112)
Wickes	–	–	0.0660***
			(0.0122)
Observations	1,531,321	1,531,321	1,531,321
Adjusted R^2	0.986	0.986	0.986

Note: Table shows the difference-in-difference estimator for the treatment effect (removal of LPG) relative to the control group (John Lewis exclusive products). The dependent variable is the logarithm of prices for product i, at retailer r, at time t. Product, date, and retailer fixed effects are included. Robust standard errors are shown in parentheses below the coefficients and are clustered at the product level: $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table 11: Impact of Removal of LPG on Prices, robustness for counterfactual creation

	(1) Aggregate	(2) Insiders vs Outsider	(3) Retailers
Aggregate	0.0193^{**} (0.00812)	–	–
Focal Retailer	–	0.0144^{*} (0.00820)	–
Inside Retailers	–	0.0245^{***} (0.00830)	–
Outside Retailer	–	0.0153^{*} (0.00885)	–
AO	–	–	0.0325^{***} (0.00843)
Amazon	–	–	0.0152^{*} (0.00885)
Argos	–	–	0.00943 (0.0108)
Boots	–	–	-0.0202 (0.0266)
Currys	–	–	0.0194^{**} (0.00859)
Dunelm	–	–	-0.00518 (0.00773)
Homebase	–	–	0.00118 (0.0154)
Hughes	–	–	0.0271^{**} (0.0106)
John Lewis	–	–	0.0145^{*} (0.00820)
Tesco	–	–	-0.00864 (0.00791)
TradePoint	–	–	0.0545^{***} (0.0118)
Wickes	–	–	0.0689^{***} (0.0120)
Observations	1,486,749	1,486,749	1,486,749
Adjusted R^2	0.986	0.986	0.986

Note: Table shows the difference-in-difference estimator for the treatment effect (removal of LPG) relative to the control group (John Lewis exclusive products, covering all products with zero competitors). The dependent variable is the logarithm of prices for product i, at retailer r, at time t. Product, date, and retailer fixed effects are included. Robust standard errors are shown in parentheses below the coefficients and are clustered at the product level: $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table 13: Impact of Removal of LPG on Prices, sample restricted to products with sub-sub categories contained in both treatment and control groups

	(1) Aggregate	(2) Insiders vs Outsider	(3) Retailers
Aggregate	0.0168^{**}	–	–
	(0.00835)		
Focal Retailer	–	0.0108	–
		(0.00844)	
Inside Retailers	–	0.0235^{***}	–
		(0.00849)	
Outside Retailer	–	0.0116	–
		(0.00931)	
AO	–	–	0.0305^{***}
			(0.00856)
Amazon	–	–	0.0116
			(0.00932)
Argos	–	–	0.00947
			(0.00978)
Boots	–	–	-0.0280
			(0.0423)
Currys	–	–	0.0153^{*}
			(0.00888)
Dunelm	–	–	-0.00815
			(0.00797)
Homebase	–	–	0.0185
			(0.0129)
Hughes	–	–	0.0263^{**}
			(0.0116)
John Lewis	–	–	0.0108
			(0.00844)
Tesco	–	–	-0.0115
			(0.00815)
TradePoint	–	–	0.0527^{***}
			(0.0121)
Wickes	–	–	0.0647^{***}
			(0.0124)
Observations	1,280,781	1,280,781	1,280,781
Adjusted R^2	0.988	0.988	0.988

Note: Table shows the difference-in-difference estimator for the treatment effect (removal of LPG) relative to the control group (John Lewis exclusive products). The dependent variable is the logarithm of prices for product i, at retailer r, at time t. The sample has been restricted to include only products which have a sub-sub category present in both treatment and control groups. More specifically, we remove products with the following sub-sub categories, which are not present in the control group: Bread Makers, Camcorders, Condenser Tumble Dryers, Grills, Humidifiers, Integrated Washing Machines, Laptops and Macbooks, Men's Clippers Hair Trimmers & Groomers, Mobile Phones, Shirt and Trouser Presses, Soup Makers, Steam Irons Steam Generators and Steam Brushes, Tablets, and Vacuums. Product, date, and retailer fixed effects are included. Robust standard errors are shown in parentheses below the coefficients and are clustered at the product level: $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table 14: Sample only includes retailer-product pairs observed in both the pre- and post-sample

	(1) Aggregate	(2) Insiders vs Outsider	(3) Retailers
Aggregate	0.0161*	–	–
	(0.00833)		
Focal Retailer	–	0.00992	–
		(0.00842)	
Inside Retailers	–	0.0219**	–
		(0.00851)	
Outside Retailer	–	0.0125	–
		(0.00906)	
AO	–	–	0.0301***
			(0.00864)
Amazon	–	–	0.0125
			(0.00906)
Argos	–	–	0.00691
			(0.0110)
Boots	–	–	-0.0232
			(0.0267)
Currys	–	–	0.0165*
			(0.00879)
Dunelm	–	–	-0.00808
			(0.00796)
Homebase	–	–	0.000790
			(0.0150)
Hughes	–	–	0.0245**
			(0.0108)
John Lewis	–	–	0.00993
			(0.00842)
Tesco	–	–	-0.0115
			(0.00812)
TradePoint	–	–	0.0511***
			(0.0119)
Wickes	–	–	0.0666***
			(0.0121)
Observations	1,476,406	1,476,406	1,476,406
Adjusted R^2	0.986	0.986	0.986

Note: Table shows the difference-in-difference estimator for the treatment effect (removal of LPG) relative to the control group (John Lewis exclusive products). The dependent variable is the logarithm of prices for product i, at retailer r, at time t. The sample has been restricted to only include products that are observed in the sample in both pre- and post-treatment periods. Product, date, and retailer fixed effects are included. Robust standard errors are shown in parentheses below the coefficients and are clustered at the product level: $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table 15: Sample only includes products sold at John Lewis on the final day of the sample

	(1) Aggregate	(2) Insiders vs Outsider	(3) Retailers
Aggregate	0.0179*	–	–
	(0.00989)		
Focal Retailer	–	0.0143	–
		(0.00997)	
Inside Retailers	–	0.0237**	–
		(0.0100)	
Outside Retailer	–	0.0104	–
		(0.0103)	
AO	–	–	0.0268***
			(0.0100)
Amazon	–	–	0.0104
			(0.0103)
Argos	–	–	0.00895
			(0.0114)
Boots	–	–	-0.0476*
			(0.0256)
Currys	–	–	0.0222**
			(0.0104)
Dunelm	–	–	-0.0118
			(0.00955)
Homebase	–	–	0.0249*
			(0.0141)
Hughes	–	–	0.0301**
			(0.0118)
John Lewis	–	–	0.0143
			(0.00997)
Tesco	–	–	–
TradePoint	–	–	0.0525***
			(0.0130)
Wickes	–	–	0.0682***
			(0.0136)
Observations	1,133,375	1,133,375	1,133,375
Adjusted R^2	0.988	0.988	0.988

Note: Table shows the difference-in-difference estimator for the treatment effect (removal of LPG) relative to the control group (John Lewis exclusive products). The sample has been restricted to only include products that are sold at John Lewis until the final date of observation. Tesco does not contain a product sold until the end of the sample. The dependent variable is the logarithm of prices for product i, at retailer r, at time t. Product, date, and retailer fixed effects are included. Robust standard errors are shown in parentheses below the coefficients and are clustered at the product level: $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table 16: Impact of Removal of LPG on Prices, retailer-product level clustering

	(1) Aggregate	(2) Insiders vs Outsider	(3) Retailers
Aggregate	0.0161^{**}	–	–
	(0.00813)		
Focal Retailer	–	0.0106	–
		(0.00839)	
Inside Retailers	–	0.0216^{***}	–
		(0.00830)	
Outside Retailer	–	0.0124	–
		(0.00903)	
AO	–	–	0.0296^{***}
			(0.00863)
Amazon	–	–	0.0123
			(0.00903)
Argos	–	–	0.00653
			(0.0109)
Boots	–	–	-0.0231
			(0.0265)
Currys	–	–	0.0165^{*}
			(0.00878)
Dunelm	–	–	-0.00807
			(0.00797)
Homebase	–	–	-0.00172
			(0.0145)
Hughes	–	–	0.0242^{**}
			(0.0108)
John Lewis	–	–	0.0106
			(0.00839)
Tesco	–	–	-0.0115
			(0.00811)
TradePoint	–	–	0.0516^{***}
			(0.0120)
Wickes	–	–	0.0660^{***}
			(0.0122)
Observations	1,486,749	1,486,749	1,486,749
Adjusted R^2	0.986	0.986	0.986

Note: Table shows the difference-in-difference estimator for the treatment effect (removal of LPG) relative to the control group (John Lewis exclusive products). The dependent variable is the logarithm of prices for product i, at retailer r, at time t. Product, date, and retailer fixed effects are included. Robust standard errors are shown in parentheses below the coefficients and are clustered at the retailer-product-level: $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table 17: Sample only includes Amazon, AO, Argos, Currys, and John Lewis

	(1) Aggregate	(2) Insiders vs Outsider	(3) Retailers
Aggregate	0.0152^{*}	–	–
	(0.00834)		
Focal Retailer	–	0.0107	–
		(0.00842)	
Inside Retailers	–	0.0204^{**}	–
		(0.00856)	
Outside Retailer	–	0.0124	–
		(0.00904)	
AO	–	–	0.0296^{***}
			(0.00863)
Amazon	–	–	0.0124
			(0.00904)
Argos	–	–	0.00705
			(0.0110)
Boots	–	–	–
Currys	–	–	0.0161^{*}
			(0.00880)
Dunelm	–	–	–
Homebase	–	–	–
Hughes	–	–	–
John Lewis	–	–	0.0108
			(0.00842)
Tesco	–	–	–
TradePoint	–	–	–
Wickes	–	–	–
Observations	1,385,762	1,385,762	1,385,762
Adjusted R^2	0.986	0.986	0.986

Note: Table shows the difference-in-difference estimator for the treatment effect (removal of LPG) relative to the control group (John Lewis exclusive products). The dependent variable is the logarithm of prices for product i, at retailer r, at time t. The sample has been restricted to only include products sold at Amazon, AO, Argos, Currys, and John Lewis. Product, date, and retailer fixed effects are included. Robust standard errors are shown in parentheses below the coefficients and are clustered at the product level: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 18: Sample cut on 27th February 2023 when CEO departs

	(1) Aggregate	(2) Insiders vs Outsider	(3) Retailers
Aggregate	0.0175^{**}	–	–
	(0.00731)		
Focal Retailer	–	0.0110	–
		(0.00741)	
Inside Retailers	–	0.0229^{***}	–
		(0.00754)	
Outside Retailer	–	0.0154^{*}	–
		(0.00806)	
AO	–	–	0.0310^{***}
			(0.00765)
Amazon	–	–	0.0153^{*}
			(0.00806)
Argos	–	–	0.00810
			(0.0105)
Boots	–	–	-0.0230
			(0.0277)
Currys	–	–	0.0181^{**}
			(0.00790)
Dunelm	–	–	-0.00313
			(0.00687)
Homebase	–	–	0.00162
			(0.0147)
Hughes	–	–	0.0263^{**}
			(0.0105)
John Lewis	–	–	0.0110
			(0.00741)
Tesco	–	–	-0.00594
			(0.00706)
TradePoint	–	–	0.0523^{***}
			(0.0109)
Wickes	–	–	0.0569^{***}
			(0.0107)
Observations	1,273,161	1,273,161	1,273,161
Adjusted R^2	0.986	0.986	0.986

Note: Table shows the difference-in-difference estimator for the treatment effect (removal of LPG) relative to the control group (John Lewis exclusive products). The dependent variable is the logarithm of prices for product i, at retailer r, at time t. The sample has been restricted and is cut off after 27th February 2023 (the date of JL CEO's departure). Product, date, and retailer fixed effects are included. Robust standard errors are shown in parentheses below the coefficients and are clustered at the product level: $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table 19: Impact of Removal of LPG on Prices, Amazon counterfactual

	(1) Aggregate	(2) Insiders vs Outsider	(3) Retailers
Aggregate	0.00486	–	–
	(0.00430)		
Focal Retailer	–	-0.00176	–
		(0.00467)	
Inside Retailers	–	0.00922**	–
		(0.00441)	
Outside Retailer	–	–	–
AO	–	–	0.0172***
			(0.00478)
Amazon	–	–	–
Argos	–	–	-0.00581
			(0.00799)
Boots	–	–	-0.0355
			(0.0255)
Currys	–	–	0.00417
			(0.00500)
Dunelm	–	–	-0.0204***
			(0.00429)
Homebase	–	–	-0.0141
			(0.0138)
Hughes	–	–	0.0119
			(0.00785)
John Lewis	–	–	-0.00174
			(0.00467)
Tesco	–	–	-0.0243***
			(0.00468)
TradePoint	–	–	0.0392***
			(0.00999)
Wickes	–	–	0.0536***
			(0.0101)
Observations	1,376,443	1,376,443	1,376,443
Adjusted R^2	0.985	0.985	0.985

Note: Table shows the difference-in-difference estimator for the treatment effect (removal of LPG) relative to the control group (Amazon products). John Lewis exclusive products have been dropped. The dependent variable is the logarithm of prices for product i, at retailer r, at time t. Product, date, and retailer fixed effects are included. Robust standard errors are shown in parentheses below the coefficients and are clustered at the product level: