INSTITUTE
OF ECONOMIC STUDIES
Faculty of Social Sciences
Charles University

HIGH-FREQUENCY GROCERIES PRICES: EVIDENCE FROM CZECHIA

Anna Pavlovova

IES Working Paper 31/2023

Institute of Economic Studies,
Faculty of Social Sciences,
Charles University in Prague

[UK FSV – IES]

Opletalova 26
CZ-110 00, Prague
E-mail : ies@fsv.cuni.cz
http://ies.fsv.cuni.cz

Institut ekonomických studií
Fakulta sociálních věd
Univerzita Karlova v Praze

Opletalova 26
110 00 Praha 1

E-mail : ies@fsv.cuni.cz
http://ies.fsv.cuni.cz

Bibliographic information:
Pavlovova A. (2023): "High-Frequency Groceries Prices: Evidence from Czechia" IES Working Papers 31/2023. IES FSV. Charles University.

High-Frequency Groceries Prices: Evidence from Czechia

Anna Pavlovova

Charles University, Institute of Economic Studies, Faculty of Social Sciences, Prague, Czech Republic
E-mail: anna.pavlovova@fsv.cuni.cz

October 2023

Abstract:
How often do online retailers change prices? Are there any differences in their price rigity? I collected and analysed more than 4 million daily prices of online grocery retailers from Czechia during the unprecedented period between January 2020 and April 2021. There are substantial differences in pricing among the four retailers. The mean number of all price changes ranges among the retailers between 3.10 and almost 11 per year. Most of the price changes are temporary. Retailers change prices permanently on average between 0.68 to 4.04 times per year. The differences in pricing persist even after the disaggregation of the products into individual categories and even in the estimation of the within-between model of the probability of price change. An in-depth analysis of temporary price adjustments is crucial to robustly assess pricing and price rigidity. It is likely to explain part of the discrepancy in pricing found across the retailers.

JEL: E30, D22, L11, L81, M21
Keywords: price setting, price exibility, scraped prices, temporary price changes

Acknowledgement: This project is supported by Charles University project GA UK No. 182723 and is part of the project GEOCEP that has received funding from the European Union's Horizon 2020 research and innovation program under the Marie Sklodowska-Curie grant agreement No. 870245.

An online appendix is available at https://sites.google.com/view/pavlovova-research.

1 Introduction

Online prices serve as a valuable data source, offering a helpful complement to more traditional price indices in economic research and practice (Cavallo, 2017; Gorodnichenko & Talavera, 2017). Data manipulation during the construction of price statistics commonly used for economic research can introduce significant biases in results regarding the frequency and magnitude of price changes (Cavallo & Rigobon, 2016; Cavallo, 2018b). By directly collecting prices from retailers' web pages, researcher have control over handling missing values and temporary price fluctuations, such as sales. Online data provide an opportunity for more in-depth price analysis due to their availability at the retailer and product levels and the possibility of collecting them at a much higher frequency. Additionally, the potential differences in costs and incentives faced by online and offline retailers can help us assess the reliability of various economic theories (Cavallo, 2017; Gorodnichenko & Talavera, 2017). As the share of online shopping in overall consumption continues to grow steadily, it becomes increasingly crucial to gain a deeper understanding of this segment of the economy.

In this paper, I analyse price-setting behaviour and price rigidity of four retailers selling groceries online in the Czech Republic. I collected an unbalanced panel of over 4 million daily online prices for nearly 11,800 products between the beginning of 2020 and the first quarter of 2021. The paper contributes to the existing literature in several ways. Firstly, I start by analysing the frequency of price adjustments. I extend the current knowledge by using a more up-to-date and more extensive dataset, focusing on a more specific segment of online retail and examining potential differences in pricing behaviour among four retailers from the same country.

Secondly, the issue of price rigidity often involves the exclusion of temporary price adjustments.Despite sales accounting for the majority of price changes (more than 75% in this paper's case), limited attention is typically given to the analysis of such price adjustments and the impact of their different definitions on the implied price rigidity. To fill this gap and to underscore the importance of such analysis, I compare results using several specifications of temporary price developments. Next, I look at the magnitude of price changes. Finally, I construct a binary correlated random effect model, also called the within-between model or the hybrid model, to examine how various factors and product characteristics influence the probability of price change.

I partially build upon Nakamura & Steinsson (2008), who analysed different specifications of temporary price offers and their impact on price rigidity in the offline world. Many researchers, including those dealing with online prices, refer to their paper when excluding temporary price adjustments to assess price rigidity. However, since Nakamura & Steinsson (2008) conducted the analysis on offline data from 1988 to 2005, it is in place to address this topic in the online world and on newer data.

Even though the physical costs of changing price tags are presumed to be relatively low in the online sector, I find online grocery prices to be relatively rigid. Gorodnichenko *et al.* (2018b) suggest that menu costs are significantly less important in price rigidity than traditionally believed. However, the distribution of the analysed price changes aligns with this theory. Hence, the source of the rigidity might not be the direct expenses associated with altering prices but rather the indirect costs, such as the potential loss of customer favour (Paraschiv *et al.*, 2023).

I find substantial differences among the four retailers' pricing, and these differences persist even after disaggregating the results into product categories. When temporary price adjustments are not excluded, prices change on average from 3.10 to almost 11 times per year, depending on the retailer. That results in a substantially longer average duration of prices compared to Hillen & Fedoseeva (2021) who analyse online grocery prices for the US. The relative proportion of temporary price changes, such as sales, is also heterogeneous among retailers. The definition of temporary price adjustments has a substantial impact on implied rigidity, with effects varying across retailers. The average count of permanent price changes ranges from 0.68 to 4.04 per year. The models describing the probability of price change provide additional evidence of the differences in pricing across the retailers.

As in Nakamura & Steinsson (2008) and Klenow & Malin (2010), the magnitude of price changes is relatively pronounced (compared to the prevailing level of inflation). Contrary to Hillen & Fedoseeva (2021), the distributions of price change magnitude are not bell-shaped. There are slightly more price increases than price decreases, and in many cases, price rises do not come from the same distribution as price falls (in absolute terms). In agreement with the existing literature, permanent price adjustments are, on average, of lower magnitude than temporary ones (Nakamura & Steinsson, 2008; Klenow & Malin, 2010; Hillen & Fedoseeva, 2021). As in the previous cases, there are substantial differences in price change magnitude across the retailers.

Additionally to the existing literature, I examine the factors influencing the likelihood of price change for all price changes and separately for permanent price changes and sales. I find significant differences between these three types of price changes and the four retailers. In line with Hillen (2021a) and Gorodnichenko *et al.* (2018b), the prices do not uniformly and significantly react to major macroeconomic events or presumed changes in their individual demand caused by the introductions of lockdowns during the days directly following such events. The effects of the introduction of lockdowns during the coronavirus pandemic and the changes in interest rates on price change probability differ in sign and significance across the retailers and assumed duration of the effect.

Overall, the strong dependence of price rigidity on the examined retailer and assumptions imposed on temporary price changes highlights the importance of a more thorough analysis of temporary price adjustments. While the literature often lacks robustness checks using different definitions of temporary price changes, such checks are likely to substantially enhance the credibility of the presented results regarding price rigidity. In line with Cavallo (2017), I find that understanding temporary price offers may help us explain part of the price dispersion across retailers.

2 Related Literature

Assumptions about firms' behaviour and pricing are often imposed in macroeconomic models to introduce pricing frictions and short-term real effect of monetary policy. As online and traditional offline retailers face different incentives, online prices bring valuable insights into economic theories regarding pricing behaviour (Cavallo, 2017; Gorodnichenko & Talavera, 2017). For example, the actual costs of changing a price (so-called menu costs) might be lower in online retail. However, the potential impact of price increases on the given shop's reputation and customers' decision to buy the goods elsewhere is likely to be more important in online shopping. This is because shopping around in the online world is associated with only minimal additional burden (Gorodnichenko *et al.*, 2018a; Gorodnichenko & Talavera, 2017).

Gorodnichenko & Talavera (2017) and Gorodnichenko *et al.* (2018a) find online prices to change more frequently than prices from traditional brick-and-mortar shops. Cavallo (2017) compares prices of multi-channel retailers (i.e. retailers selling both online and offline) and concludes that even though most price changes do not occur simultaneously online and offline, the frequency and magnitude of price changes online and offline are similar. However, the similarity between online and offline prices differs across retailers, sectors and countries. Price changes are usually reflected sooner in the online prices than in the officially reported consumer price index (Breton *et al.*, 2016; Cavallo & Rigobon, 2016; Aparicio & Bertolotto, 2020).

On the other hand, Cavallo (2018b) finds online prices to be more sticky compared to literature based on offline prices. Some of the small price changes in more traditional offline data, that contradict the theory of costly price adjustment, may be artificial. They are likely to result from the imputation of missing values and time-averaging during the indices construction. This creates more frequent price jumps of smaller magnitude (Cavallo & Rigobon, 2016; Cavallo, 2018b). Based on online prices where imputation or time-averaging is not used, price changes are less frequent and of higher magnitude. The resulting distribution is then in line with the widely excepted theory of menu costs (Cavallo, 2018b).

Even though online retailers can be perceived as facing a significantly lower cost of price change, neither online prices nor online shopping activity reacts to unforeseen changes in macroeconomic activity within the two subsequent weeks. This might suggest that the online sector is more likely to react to changes in demand for particular products than to changes in the overall aggregate macroeconomic conditions. The important conclusion is that even though the distribution of online price changes might be in line with the theory of menu costs, they may play a significantly less important role in price rigidity than it is traditionally believed (Gorodnichenko *et al.*, 2018b).

Similarly to the offline sector (Alvarez *et al.*, 2006; Dhyne *et al.*, 2006; Klenow & Malin, 2010), there are substantial differences in pricing across product categories in online retail (Cavallo, 2018a). Hillen & Fedoseeva (2021) finds prices of groceries sold by Amazon in California to be relatively flexible and the incidence of relatively small price changes to be quite high. The authors cannot confirm that Amazon uses dynamic pricing, deploying some form of an algorithm. However, the fact that prices frequently change, especially for products with shorter shelf-life, and that the median and average price change is small, it is possible that Amazon deploys some form of dynamic pricing to manage its stock efficiently. The possibility of dynamic pricing in other Amazon product categories was previously indicated by Cavallo (2018a).

Several studies utilized online prices (combined with expenditure data from payment cards) to assess the impact of lockdowns during the COVID-19 pandemic on price levels. Cavallo (2020) and Alvarez & Lein (2020) combine online prices and expenditure data from payment cards to create a novel price index reflecting the change in consumption patterns during this period. They arrive to inflation higher than the official CPI. However, despite the unprecedented popularity of online grocery shopping during 2020 due to the pandemic, the overall price level of food items sold by Amazon online did not rise during 2020. Prices did increase for some high-demand food categories. On the other hand, for some categories (such as meat), the development recorded for Amazon was strikingly different from the one captured by the official statistics (Hillen, 2021a). The results presented in Hillen (2021a) support those of Gorodnichenko *et al.* (2018b) that online retailers may not react to aggregate changes in economic conditions. They, however, do not support their suggestion that online shops may be more likely to respond to surges in their individual demand.

Compared to the traditional (often aggregated) official price indices provided by the national statistical offices, scraped online prices can be collected at a significantly higher frequency and with significantly higher granularity. This allows us to conduct a more robust analysis of price-setting behaviour without the presence of biases introduced by data manipulation during the indices construction. Usage of traditional price indices can lead to misspecification of frequency and magnitude of price changes introduced during the construction of the indices which may result in biased results regarding the rigidity of prices (Cavallo & Rigobon, 2016; Cavallo, 2018b).

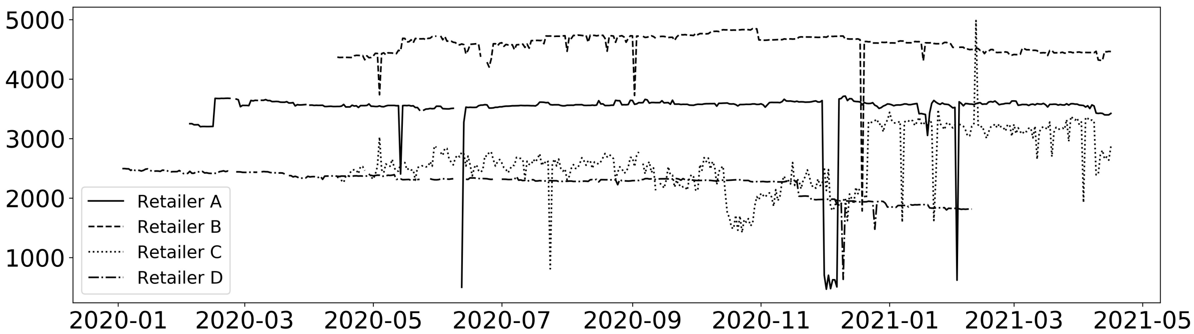
3 Data

I collected more than 4 million daily prices of around 12 thousands products sold by four online grocery retailers operating in the Czech Republic between January 2020 and April 2021. I acquired the data using web scraping, an automated data extraction technique for obtaining information from web pages. Similarly to Macias *et al.* (2023), I randomly assigned characters to the retailers to prevent any unintended interference with any of these retailers' business plans or strategies. I refer to them as Retailer A, Retailer B, Retailer C and Retailer D.

The period covered differs across retailers and does the number of recorded prices per retailer

(Figure 1). For Retailer D, we have data starting from January 3rd 2020, for Retailer A from February 5th 2020, and for Retailer B and Retailer C from April 15th 2020. The last date included in the analysis is April 16th 2021, except for Retailer D, for which the last data are from February 9th 2021. I consider only products with a minimum of 200 daily prices recorded and for which data is available for at least 75% of the days between the first and last recorded price. 11 788 products meet both these criteria.

Figure 1: Number of observations in time



Note: Daily number of scraped prices per retailer available between January 2020 and April 2021.

I use forward fill to impute missing data to create a more consecutive time series. I limit the imputation to a maximum of 7 successive days. The number of prices collected for individual retailers per day varies in time as the selection of goods offered may differ from day to day. There are also some days for which data for some or all the retailers are incomplete or completely unavailable. There are several reasons why this might have happened. Among them are problems with internet connectivity, problems with our server or computer or maintenance of the server or web page of the particular retailer. Based on the number of daily observations and their variability, I identify outliers as days with the number of recorded prices being in the lowest 2.5% of the distribution of daily observations for the given retailer. Even though I have some recorded prices on those days, I exclude these days from the analysis as the data are corrupted and might be misleading. The excluded prices are in most cases imputed using the general forward fill for a maximum of 7 days. I focus solely on the left tail of the distribution as extremely high numbers of observations do not negatively impact the representativeness of the data. After thorough analysis, I consider them valid.

I classified the products using the official consumer basket categories. These categories correspond to the 3rd level of European Classification of Individual Consumption according to Purpose (ECOICOP).[1] Namely, I use the following categories: *"Bread and cereals"*, *"Meat"*, *"Fish and seafood"*, *"Milk, cheese and eggs"*, *"Oils and fats"*, *"Fruit"*, *"Vegetables"*, *"Sugar, jam, honey, chocolate and confectionary"*, *"Other food products not elsewhere classified"*, *"Coffee, tea and cocoa"*, *"Mineral waters, soft drinks and juices"*, *"Wine"* and *"Beer"*.

There are several reasons why I chose this specific segment of on-line retail. One of them is that prices of food account for around 17% of goods included in the Czech CPI and are an essential part of consumption. There is also an important benefit of focusing on groceries of a more technical nature. Compared to other types of goods sold on-line (such as electronics), it is relatively easy to create time series for specific products as the items are homogeneous in time. That means that I do not have to account for different characteristics of the good, their substitution and/or deterioration of value in time. This is for example rather challenging in electronics.

The data collection is a crucial part of this work's contribution. There are several benefits of collecting the data ourselves. Among them is the full control over the data collection and subsequent data processing. This is an important feature of our data since it has been shown that data manipulation during the construction of price statistics can lead to substantial bias in results (Cavallo & Rigobon, 2016; Cavallo, 2018b).

4 Methodology

To assess price rigidity (or price change frequency), I use a binary price change indicator $I_{i,t}^{\Delta p}$ defined as follows:

$$I_{i,t}^{\Delta p} = \begin{cases} 0, & \text{if } p_{i,t} = p_{i,t-1} \\ 1, & \text{otherwise} \end{cases} \tag{1}$$

where i represents a given good, t the given date and $p_{i,t}$ is the price of the given good on the given date. $I_{i,t}^{\Delta p}$ equals 1 on days for which the price the previous day differed from the

[1] `https://ec.europa.eu/eurostat/ramon/nomenclatures/index.cfm?TargetUrl=LST_NOM_DTL&StrNom=COICOP_5&StrLanguageCode=EN`

price on the given day. When examining temporary price changes, such as sales, I define the binary price change indicator as one on the initial day of the sale and zero on all other days.

Since for every product I have data for a different time span, I standardise the recorded number of price changes (N_i) during the given product time span, defined as the number of days between the day of the first recorded price and the day of the last recorded price, to 365 days. I do this for all, permanent and also temporary price adjustments. The standardised number of price changes per year (N_i^S) for product i is then computed as follows:

$$N_i^S = 365 * \frac{N_i}{time\_span_i} \tag{2}$$

I distinguish between all, temporal and permanent price changes. Sales are essential to the analysis if one wishes to examine the overall price-setting behaviour. However, if the results are intended for assessment of price rigidity, in newer studies, temporary price adjustments are often excluded from the data before the computation of price change frequency, and only permanent price changes are considered (Cavallo, 2018a; Gorodnichenko *et al.*, 2018b).

Researchers frequently rely on certain assumptions to identify and exclude temporary price developments. One commonly utilised method involves discarding price decreases that reverse back to their original levels within a predetermined period (i.e. exactly reversed V-shaped sales). When this method is applied, Nakamura & Steinsson (2008) are often referenced as the source for such methodology. The second approach is to exclude price changes labelled in the data as sales if such information is available. However, this procedure did not yield the intended result as not all exactly reversed price changes lasting a relatively short period were marked as sales by the retailers. On the other hand, in some cases, goods without a price change were marked as being in a temporary price offer. This is in line with Hillen (2021a), who does not find a higher price level of groceries sold by Amazon despite a lower share of promotional prices during the first year of the pandemic. She also considers them more of a marketing tool rather than actual repricing.

As my focus is on analysing the price-setting behaviour and price rigidity of online grocery retailers rather than their marketing strategies, I opted to adopt the first approach of categorising price changes that are exactly reversed within a specified period as temporary. For the purposes of this study, I defined temporary price changes as those lasting for a maximum of 90 days, which is notably longer compared to the existing literature, which often limits the duration to 30 or 14 days (Hillen & Fedoseeva, 2021; Cavallo, 2018a; Gorodnichenko *et al.*, 2018b; Cavallo, 2017; Gorodnichenko & Talavera, 2017). I selected a 90-day threshold as the baseline approach. Since many macroeconomic models are quarterly, prices lasting longer than one quarter can be considered non-temporary, influencing price rigidity. In this sense, allowing temporary price changes to last up to 90 days can be considered a conservative approach to assessing price flexibility.

Allowing for a relatively long duration of temporary price adjustments, I can examine the durations of such offers and assess the differences in results if the presumed maximum duration is shortened to 45, 35, 30 and 14 days. This has not been explored in prior research, despite the significance of such analysis for a comprehensive and robust assessment of price rigidity. Additionally, I investigate the impact of considering temporal also not exactly reversed price decreases (so-called asymmetric V-shape sales) and temporal price increases, which are usually not regarded as temporal except for Gorodnichenko & Talavera (2017) and Kehoe & Midrigan (2015).

Price adjustments occurring either at the beginning or the end of the examination period can introduce bias into the final assessment of price flexibility. For price changes towards the ends of the sample, it is substantially harder to evaluate whether they are temporary or not. If permanent price changes are defined as prices lasting at least X days, such price changes will likely be considered temporal. In reality, some of them may have lasted longer than X days but were recorded for less than X days due to the time limitations of the data set. This would result in a possible downward bias in the frequency of permanent price changes and price flexibility. On the other hand, if temporary price changes are defined as price changes that reverse during a specified period (as in this study), this might lead to an upward bias in the frequency of permanent price changes. Prices towards the ends of the sample are taken as permanent. Simply because I did not record the initial or subsequent price adjustment to identify such price changes as temporary because the data are time censored.

In real life, price changes are usually represented in percentage changes (similarly to simple returns in finance). Even though this specification is probably the most intuitive, it is not ideal for statistical analysis, especially if sales are considered. The distribution of exactly reversed price changes (such as sales) is then asymmetric, with price increases being, on average, of higher magnitude than price decreases. To avoid this imbalance and in line with the existing literature (Hillen & Fedoseeva, 2021; Cavallo, 2018a; Gorodnichenko *et al.*, 2018b; Powell *et al.*, 2018; Cavallo, 2017; Gorodnichenko & Talavera, 2017; Dhyne *et al.*, 2006), I define price changes as log-differences as specified in equation 3 (where ln stands for natural logarithm, $p_{i,t}$ for price of good i at time t).

$$\Delta p_{i,t}^{ln} = ln(p_{i,t}) - ln(p_{i,t-1}) \tag{3}$$

The problem with using log-differences for prices of goods and services is that the magnitude of price changes is relatively large. Therefore, the common practice of using log-differences in finance to approximate simple returns may not yield very accurate results in this case. While this issue does not impact the analysis results presented in this paper, it is important to be aware of this potential limitation during interpretation, especially when examining price change magnitudes.

To analyse price change magnitude, I sometimes use the absolute value of the log-differences. The usage of absolute values converts price decreases into positive numbers and allows for a comparison of the distributions of price increases and price decreases.

I test for differences in distributions of price change frequency and magnitude - between permanent and temporary price changes; between price increases and price decreases; and between the four retailers. I use T-tests to test the hypotheses of equal means, and Wilcoxon rank-sum test and the two-sample Kolmogorov-Smirnov test to examine the differences in the overall distributions.

4.1 Probability of price change

To better understand the probability of a price change, I estimate a logit correlated random effects model, sometimes referred to as the hybrid model, mixed-effect model or within-between random effect model (Bell *et al.*, 2019; Wooldridge, 2019). To separate the within and between effects, I follow specifications proposed by Bell *et al.* (2019), Bell & Jones (2015), and others. The constructed model can be generalised to the following equation (I do not use matrix notation for simplification):

$$Pr(I_{i,t}^{\Delta p} = 1) = F(\beta_0 + \beta_1(x_{i,t} - \bar{x_i}) + \beta_2 c_i + \beta_3 \bar{x_i} + \mu_i + \epsilon_{i,t}) \tag{4}$$

where $I_{i,t}^{\Delta p}$ is the binary price change indicator from Equation 1, β_0 is the common intercept, $x_{i,t}$ are all the time-varying variables with $\bar{x_i}$ being their time averages computed for every unit separately. The time-constant variables are represented by c_i. Similarly to Hillen & Fedoseeva (2021) and as suggested by Bell *et al.* (2019), I allow for the estimation of individual intercepts (μ_i) and do not allow for random slopes. The average within effect is then β_1. On the other hand, the average between effect of our interest is captured by β_2. β_3 is also a between effect. However, its interpretation is not of much meaning to our analysis. Nevertheless, it has to be included in the model to ensure correct specification for unbalanced panel.

To analyse the probability of a permanent price change, I exclude price falls and rises exactly reversed within 90 days (deeming this the most conservative approach). I then re-estimate the model for sales (i.e. price decreases exactly reversed within 90 days), as they are the most common type of (temporary) price change in the data.

Following the existing literature, mainly Hillen & Fedoseeva (2021) and Lünnemann & Wintr (2011), and the preliminary findings from the initial analysis of price changes, I estimate several models that include various combinations of explanatory variables. I include the following time-constant variable (c_i): *categories*, dummy variables representing to which category based on ECOICOP the given product belongs to. If I estimate a joint model for all four shops, I also include dummy variables representing from which shop the price was obtained (*shops*).

The time-varying variables ($x_{i,t}$) and their time averages ($\bar{x_i}$) included in the model are the following: dummy variables for *months* and *days* of the week for with the price was recorded (with January and Monday serving as the baselines), *price* (a normalised price level of the product), *attractive* (a binary variable representing if a price of the good ends with "x.9"), *price_duration* (the normalised number of days from the last price change). I also re-estimate the models including time dummies for days following significant macroeconomic events such as a change in the interest rate or introductions of lockdowns during the coronavirus pandemic. I identified eight different significant macroeconomic events in 2020, mostly connected to the coronavirus pandemic: one interest rate rise (February 6$^{\text{th}}$) before the pandemic, three subsequent interest rate cuts (March 17$^{\text{th}}$ and 27$^{\text{th}}$, May 11$^{\text{th}}$), and three introductions of lockdown (March

12$^{\text{th}}$, October 22$^{\text{nd}}$ and December 27$^{\text{th}}$) and one partial re-opening of the economy(December 10$^{\text{th}}$). I examine the effect of such events in the following 7 and 14 days.

Estimating binary panel data is rather challenging, and approximation or simulations are often deployed. Aside from the estimated coefficients (which are not as easily interpretable as in the case of linear models), I also report their odd rations with confidence intervals approximated using the Wald technique in the online appendix. I present results only for the coefficients which interpretation is of interest to this study. The full estimated models (including averages of time dummies) are available upon request. As in the analysis of price change frequency, I analyse data including and excluding sales and separately for sales. Since I have data for four different retailers, I also evaluate the factors influencing the probability of price change separately for the four shops to examine potential differences in their pricing. I standardize the data prior the estimation of the model.

The performance of a non-linear within-between model may not be as convincing as in the case of linear models. The estimated coefficients are no longer equivalent to the fixed effect coefficients since including the mean of time-varying variables may not account for the relationship correctly, and the results might be biased (Bell *et al.*, 2019). However, aside from particular situations that are not likely to happen often (i.e. data for very few individuals/items with high dependence between the group means and individual effects), the introduced bias or inconsistency, if present, is usually relatively marginal and decreases with the sample size dimension on the individual (or cluster) level (Bell *et al.*, 2019; Allison, 2014; Goetgeluk & Vansteelandt, 2008; Brumback *et al.*, 2013). Since the analysed data are relatively rich in this dimension, I rely on this property.

The estimation is further complicated because our panel data is unbalanced. I assume that data are missing at random and include time averages of time dummies to account for the unbalances of the panel. However, in some cases, the monthly averages suffer from multicollinearity, with correlations exceeding 0.98. In such cases, I include as many monthly averages as possible while still avoiding multicollinearity. The same holds for averages of the analysed important macroeconomic events.

5 Results

Frequency of price changes

The four analysed retailers follow distinct pricing strategies, with varying degrees of rigidity and distinct distributions of price changes in general. The frequencies of all, permanent and temporary price changes substantially differ across the retailers. The differences persist even after disaggregation of the products to individual categories. The aggregate average frequency of all price changes ranges among the retailers between 3.10 and almost 11 times per year and the aggregate median of all price changes between 1.21 and 10.83. The average aggregate frequency of permanent price change per year is between 0.68 to 4.04, depending on the retailer and assumed characteristics of excluded temporary price adjustments. That is a substantial difference, particularly because commonly used macroeconomic models are quarterly. The lower bound result would then suggest relatively rigid prices, changing on average less than once a year. On the other hand, the upper bound result would imply that in quarterly models, prices might be flexible, changing on average every quarter.

The strong dependence of price rigidity on the examined retailer and assumptions imposed on temporary price changes highlights the importance of a more thorough analysis of temporary price adjustments, often overlooked in the existing literature dealing with price rigidity. Such analysis is valuable for two main reasons. First, varying distributions of temporary price changes play a significant role in the difference in price rigidity observed among retailers. They vary in magnitude, frequency and in duration. The distinct sales durations profoundly influence the final price rigidity because the definitions of temporary price adjustments, excluded before the computation of price rigidity, commonly impose assumptions on the duration of such price changes. As the durations vary across the retailers, so do the proportions of price changes marked as temporary and permanent. Subsequently, the implied rigidity and the impact of different definitions of temporary price adjustment on price flexibility are not homogeneous across the shops. Second, temporary price developments may account for more than 75% of all price changes (as in our case). They are significantly more common than permanent price changes. Ignoring this information and not using it, at least for robustness analysis of price rigidity, may result in the loss of valuable insights into price-setting behaviour.

Frequency of all price changes

In the entire dataset, without the distinction for individual retailers, around 18% of the products did not record any price change throughout the examination period. This is also the most common outcome. The overall average number of price changes per good is 8.01 per year (Table 1), which is significantly lower compared to the 20.4 price changes for online groceries found by Hillen & Fedoseeva (2021). The implied average duration of prices is slightly above 45 days, compared to 18 days in Hillen & Fedoseeva (2021). The median of the overall number of price changes per year is 5.94, and more than 25% of the goods changed prices more than 13.5 times per year. The maximum number of recorded yearly price changes is more than 57 (recorded for cucumbers).

Regarding the direction of the price changes, results are qualitatively in line with Hillen & Fedoseeva (2021) as, on average, there are slightly more price increases than price decreases. The medians are, however, mostly equal. The third and fourth quantiles are higher for price falls. T-test to compare the average number of price increases and price decreases does not reject the null hypothesis of equal means for a significance level of 10% or less (p-value = 0.1162). Wilcoxon rank-sum and two-sample Kolmogorov-Smirnov test both strongly reject the null hypothesis of the same distribution of price rises and price falls (in absolute values) even at 1% significance level. Regarding the minimum, there are slightly more goods with no price decrease than goods with no price increase. The maximum count of price decreases is higher than the maximum number of price increases.

As already mentioned, the average aggregate count of recorded price changes per year for the individual retailers is relatively dispersed, spanning from 3.10 to 10.89. The variation among the median number of price changes is even more pronounced, with the lowest and highest value being 1.21 and 10.83, respectively. The distributions for the individual retailers bear dissimilarities in several other aspects. Firstly, the proportion of goods recording zero price changes spans from 9.41% for Retailer D to 37.27% for Retailer C. For Retailer A and Retailer B, the proportion of products with stable prices is 12.43% and 17.09%, respectively. Secondly, the shape of the distributions varies. This is visible from the different standard deviations as well as inter-quantile ranges. In contrast to most features of the distributions, the differences in the maximum number of recorded price changes are rather subtle.

Table 1: Number of all price changes per retailer

	mean	s.d.	min	1Q	2Q	3Q	max	count
ALL:								
all	8.01	7.67	0.0	1.21	5.94	13.52	57.53	99 630
positive	4.05	3.83	0.0	0.99	2.98	6.94	27.77	50 284
negative	3.97	3.90	0.0	0.83	2.98	6.82	29.76	49 346
RETAILER A:								
all	10.89	8.65	0.0	2.50	10.83	18.33	51.67	44 202
positive	5.45	4.34	0.0	1.20	5.09	9.17	25.00	22 117
negative	5.45	4.36	0.0	0.99	5.17	9.17	26.67	22 085
RETAILER B:								
all	8.24	7.29	0.0	1.98	6.76	13.89	57.53	35 691
positive	4.14	3.64	0.0	0.99	3.04	6.94	27.77	17 928
negative	4.10	3.70	0.0	0.99	2.98	6.94	29.76	17 763
RETAILER C:								
all	3.10	4.57	0.0	0.0	1.21	3.98	44.63	6 159
positive	1.62	2.31	0.0	0.0	0.99	2.00	21.82	3 228
negative	1.47	2.34	0.0	0.0	0.99	1.98	23.80	2 931
RETAILER D:								
all	7.43	6.33	0.0	2.71	6.12	10.23	50.59	13 578
positive	3.85	3.16	0.0	1.81	3.41	5.42	27.10	7 011
negative	3.58	3.34	0.0	1.01	2.71	5.03	26.20	6 567

Note: Descriptive statistics of the number of all recorded price changes per year for 11 788 products (3 524 for Retailer A, 4 446 for Retailer B, 2 048 for Retailer C and 1 770 for Retailer D). Rows *all* refer to all price changes, rows *positive* to price changes resulting in a higher price and rows *negative* to price changes resulting in a lower price. Column *count* is not scaled to a year and is the actual number of recorded price changes during the examination period.

The differences in pricing among the retailers persist even after the disaggregation to individual product categories (Table A1 in Appendix). There is no clear common pattern in the number of price changes per category among the retailers. The only few similarities are that categories with the most frequent price changes are primarily categories of relatively durable goods and that prices of non-durable goods do not change the most often. For instance, the category "Beer" and category "Coffee, tea and cocoa", as examples of relatively durable categories, are among the top three, respectively top four, most price-varying categories for all four retailers. On the other hand, "Meat" (as a non-durable good) is among the three least price-changing categories for three of the four retailers. These results oppose the findings of Hillen & Fedoseeva (2021), who find prices to change the most for categories "Produce"(including "Fruit" and "Vegetables") and "Dairy", and Dhyne *et al.* (2006), who find that prices of unprocessed food to change substantially more often than prices of processed food. Overall, these results indicate that there are not many common patterns among the retailers in the number of price changes in different categories. Hence, the differences in aggregate results among retailers are not likely to be caused by heterogeneous representations of products from different categories for every retailer. Some of the differences may still be caused by different structures inside the specified categories.

The substantial differences in price changes among retailers and categories indicate that the retailers are likely to use different pricing models. I do not examine the differences in price levels. However, if retailers follow different pricing, then the convergence of prices is likely to be weakened. This would not support Gorodnichenko & Talavera (2017), who propose online prices to converge since shopping around for the best offer is not that costly in online retail. This is certainly a valid argument for some parts of online retail where the comparison of the desired order is relatively easy. Typically, if only one or a few items are bought, it is easy to compare prices across retailers. Buying a computer or other electronics may be an illustrative example of such a scenario. However, if the customer purchases many goods simultaneously, comparing the offers becomes much more demanding. This is likely the case of online grocery shopping. I, as well as Hillen (2021b), presume that most of online grocery orders contain tens of items. Due to delivery costs, shopping online for only a few grocery items may not pay off. Further, some goods, mainly those belonging to the retailer's brand, may not be available elsewhere. This makes the comparison even harder since the consumer has to account for his inner cost of substitution. All these reasons mentioned above are likely to reduce the pressure on online grocery retailers to converge in prices, which might be the difference between online grocery retailers and other e-commerce shops.

Frequency and duration of temporary price changes

Regarding temporary price developments, I start the analysis by defining sales as price decreases that are exactly reversed within 90 days (Table 2). From all the price changes identified in our dataset, 78.5% follow such path. On average, good is on sale 3.14 times per year, indicating that on average a good's price changes 6.28 times per year due to sales. However, up to 32% of products did not record any sale during the examined period, which is substantially more than the 18% of goods that did not record any price change. Slightly more than 50% of the goods were on sale twice or less within a year. On the other hand, more than 25% was on sale at least five times per annum.

Table 2: Number of sales per year

	mean	s.d.	min	1Q	2Q	3Q	max	count
Retailer A	4.48	3.93	0.0	0.83	4.17	7.64	21.67	18 220
Retailer B	3.58	3.37	0.0	0.0	2.98	5.95	17.23	15 530
Retailer C	1.13	1.76	0.0	0.0	0.0	1.98	9.92	2 258
Retailer D	1.7	2.01	0.0	0.0	1.04	2.71	13.55	3 101
all	3.14	3.41	0.0	0.0	1.98	5.43	21.67	39 109

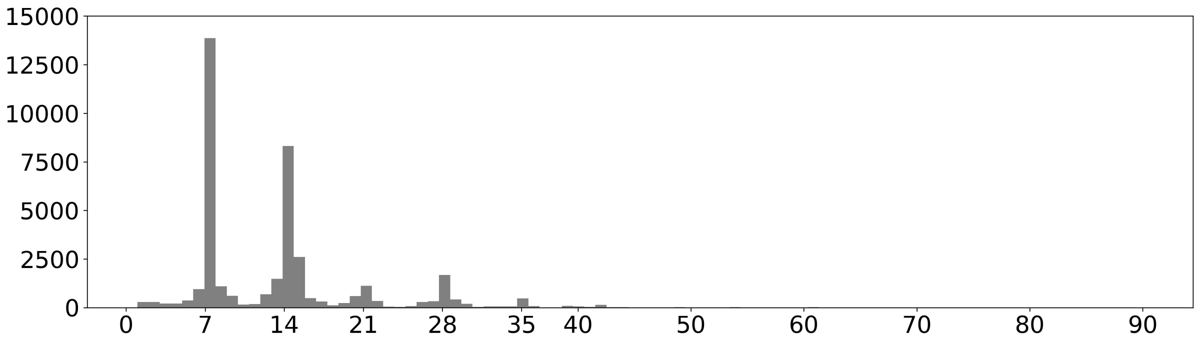
Note: Descriptive statistics of number of sales scaled to a year identified as exactly reversed price falls lasting up to 90 days. Column *count* is not scaled to a year and is the actual number of identified sales during the examination period. The sample for Retailer A consists of 3 524 products, for Retailer B of 4 446 products, for Retailer C of 2 048 products and for Retailer D of 1 770 products.

As in the number of all price changes, there are substantial differences among the number of recorded temporary sales between the retailers. The average number of sales per retailer ranges from 1.13 to 4.48. The relative proportion of identified sales to the total number of identified price changes for the given retailer also varies. The highest proportion of price changes marked as part of temporary sales is recorded for Retailer B (around 87%), which has the second-highest average number of recorded sales. The second-highest proportion of price changes belonging to a discount is recorded for Retailer A (around 82.44%). On the other hand, for Retailer C, which has the lowest number of recorded price changes and sales, only around 46% of all price changes are marked as temporary.

For all four retailers, the categories with the highest number of sales are categories of durable goods (Table A2 in Appendix). This goes hand in hand with the fact that these categories also belong to categories with the highest number of all price changes. As in the case of all price changes, categories of durable goods often record low or at least substantially lower number of temporary price changes. In line with Hillen & Fedoseeva (2021), the proportion of price changes associated with the baseline definition of sales differs among the categories.

The average sale lasts 13.27 days, and the most common sale duration is 7 days (Figure 2). The second most common duration is 14 days. The third and fourth most common values are 15 and 28 days, respectively. Only a minority of the sales last one day or less than seven days (around 1% and 8% of sales lasting 14 or fewer days, respectively). Also in the case of the duration of sales, there are substantial differences among the retailers. Half of the sales are at least 13 days long and at least 25% of the sales are longer than 14 days. More than one-fifth of the recorded sales are longer than 14 days and shorter than 30 days. This indeed shows that assuming only prices lasting maximally 14 days as temporary, without any further analysis, can lead to a substantial upward bias in the number of permanent price changes as in our case.

Figure 2: Histogram of sales duration



Note: Histogram of sale duration in days. Sales are defined as exactly reversed price decreases lasting up to 90 days. The sample consists of 11 788 products (from four different retailers) for the period between January 2020 and April 2021.

Inline with the results regarding their duration, the greater the difference in the maximal defined duration of temporary price offers, the greater the difference in the number of identified sales and the average number of recorded sales per year (Table 3). The differences between

90, 45, 35 and 30 days are relatively marginal. The 90-day sales represent 78.5% of all price changes. If only price changes lasting up to 30 days are regarded as temporary, following for example Cavallo (2018b), the number of identified sales falls only by 3.84%. Sales then account for approximately 75.5% of all identified price changes. On the other hand, if I identify sales as lasting up to 14 days at maximum, as for example in Hillen & Fedoseeva (2021), the number of identified temporary price offers is lower by 26.61% compared to 90-day sales and by 23.68% compared to 30-day sales. This decreases price changes identified as part of a price offer to 57.61% of all recorded price adjustments. Hillen & Fedoseeva (2021) finds that only around 4.24 % price changes in their datasets of grocery prices follow this definition of a sale lasting up to 14 days. For all four retailers the highest marginal decrease in the number of recorded sales is present if the sale time horizon shifts from 30 days to 14 days (see the Online Appendix). The pronounced differences in the number of sales using different maximal time horizons indicate that temporary price declines lasting between 14 and 30 days are very common.

Table 3: Reversed price decreases using different maximal duration

	mean	s.d.	min	1Q	2Q	3Q	max	count
90 days	3.14	3.41	0.0	0.0	1.98	5.43	21.67	39 109
45 days	3.11	3.42	0.0	0.0	1.98	5.42	21.67	38 812
35 days	3.08	3.4	0.0	0.0	1.82	5.32	21.67	38 325
30 days	3.02	3.36	0.0	0.0	1.81	5.0	21.67	37 609
14 days	2.32	2.78	0.0	0.0	1.0	3.97	20.83	28 704

Note: Descriptive statistics of the number of identified sales scaled to a year using different maximal duration of a sale. Sales are identified as price falls exactly reversed within the specified number of days. Column *count* is not scaled to a year and is the actual number of identified sales during the examination period. The sample consists of 11 788 products for four different retailers.

Combining the number of sales with their duration, on average, good is on sale for 11% of the days, with the median value being around 6% of the days. Not being on sale at all throughout the whole period is the most common among the products (recorded by 32% of the items). On the other hand, there are goods marked as on sale for more than 50% of the recorded period. Since exact price reversal is fairly common and price offers last relatively long, the retailers are most likely not using dynamic or algorithmic pricing. This may be viewed as further evidence that online grocery prices are relatively inflexible despite the relatively low physical costs of changing the price tags. However, the indirect menu costs of dynamic pricing might be relatively high as customers often experience displeasure when faced with such pricing strategy (Paraschiv *et al.*, 2023; Gorodnichenko *et al.*, 2018b).

Frequency of permanent price changes and price rigidity

After the exclusion of 90-day symmetric sales, the unweighted aggregate average number of price changes per product is 1.73 (Table 4), which is more than 4.5 lower than the average number of all price changes per year including sales (8.01). No permanent price change was recorded for 40.63 % of the goods, which is also the most common outcome in our sample. In line with existing literature, there are, on average, slightly more permanent price increases than price falls (Hillen, 2021a). T-test strongly rejects the null hypothesis of equal means of price rises and price falls. Both tests for difference in the distributions of the number of positive and negative price changes per year indicate that they do not come from the same distribution.

The exclusion of such defined sales partially mitigates some differences among retailers in the number of price changes. However, formal testing shows that the distributions across retailers are statistically different. Nevertheless, descriptive statistics of the number of permanent price changes are quite similar across the retailers. Cavallo (2017) finds part of price dispersion between online and prices world to be caused by different usage of promotional offers. On the other hand, the rankings of the retailers based on the frequency of price changes within a year somewhat change after excluding sales. Retailer C has the lowest average number of recorded permanent (0.83) and all (3.10) price changes per year. Retailer B follows with 1.08 and 8.24 permanent and all price changes on average yearly, respectively. Retailer A records by several folds the highest number of all price changes per year (10.89). However, regarding permanent price changes, he is much closer to Retailer B and Retailer C, with the third-highest average number of permanent price changes per year (1.93). Retailer D has the second-lowest number of all price changes (7.43), but experiences permanent price changes most frequently (7.43 and 4.04 price change on average, respectively). This may not be surprising, as Retailer D has the lowest proportion of prices following the baseline definition of temporal price changes.

The final price flexibility is significantly affected by altering the assumed duration of temporary offers (Table 5). The highest number of average recorded permanent prices is recorded

Table 4: Number of price changes excluding 90-day symmetric sales per retailer

	mean	s.d.	min	1Q	2Q	3Q	max	count
ALL:								
all	1.73	2.77	0.0	0.0	0.99	2.27	39.67	21 412
positive	0.91	1.45	0.0	0.0	0.00	1.08	18.85	11 175
negative	0.83	1.48	0.0	0.0	0.00	0.99	20.83	10 237
RETAILER A:								
all	1.93	2.52	0.0	0.0	0.92	2.50	28.33	7 762
positive	0.97	1.35	0.0	0.0	0.83	1.64	15.83	3 897
negative	0.96	1.32	0.0	0.0	0.83	1.67	13.33	3 865
RETAILER B:								
all	1.08	1.93	0.0	0.0	0.0	1.44	39.67	4 631
positive	0.56	0.99	0.0	0.0	0.0	0.99	18.85	2 398
negative	0.52	1.10	0.0	0.0	0.0	0.99	20.83	2 233
RETAILER C:								
all	0.83	2.06	0.0	0.0	0.0	0.99	28.76	1 643
positive	0.49	1.12	0.0	0.0	0.0	0.99	14.88	970
negative	0.34	1.09	0.0	0.0	0.0	0.00	15.87	673
RETAILER D:								
all	4.04	4.08	0.0	1.36	3.22	5.42	33.43	7 376
positive	2.15	2.08	0.0	0.90	1.81	2.75	18.07	3 910
negative	1.89	2.26	0.0	0.00	1.14	2.71	18.97	3 466

Note: Descriptive statistics of price changes scaled to a year excluding price falls exactly reversed within 90 days. The sample consists of 11 788 products in total (3 524 for Retailer A, 4 446 for Retailer B, 2 048 for Retailer C and 1 770 for Retailer D). Rows *all* refer to all price changes, rows *positive* to price changes resulting in a higher price and rows *negative* to price changes resulting in a lower price. Column *count* is not scaled to a year and is the actual number of recorded price changes during the examination period.

for 14-day sales since the fewest price changes are considered temporal price discounts for this duration. The mean number of recorded price changes is almost double compared to the exclusion of 90-day sales and by 71.6 % higher compared to 30-day sales. Using 14-day symmetric sales, the median number of permanent price changes is twice as high as that for 90-day and 30-day sales.

Table 5: Permanent price changes using different sale duration

	mean	s.d.	min	1Q	2Q	3Q	max
90 DAYS:							
all	1.73	2.77	0.0	0.0	0.99	2.27	39.67
positive	0.91	1.45	0.0	0.0	0.00	1.08	18.85
negative	0.83	1.48	0.0	0.0	0.00	0.99	20.83
45 DAYS:							
all	1.78	2.80	0.0	0.0	0.99	2.50	39.67
positive	0.93	1.46	0.0	0.0	0.00	1.12	18.85
negative	0.85	1.50	0.0	0.0	0.00	1.00	20.83
35 DAYS:							
all	1.86	2.84	0.0	0.0	0.99	2.50	39.67
positive	0.97	1.48	0.0	0.0	0.83	1.23	18.85
negative	0.89	1.52	0.0	0.0	0.00	1.08	20.83
30 DAYS:							
all	1.97	2.89	0.0	0.0	0.99	2.72	39.67
positive	1.03	1.50	0.0	0.0	0.83	1.67	18.85
negative	0.95	1.55	0.0	0.0	0.00	1.22	20.83
14 DAYS:							
all	3.38	3.84	0.0	0.0	1.98	5.42	41.66
positive	1.73	1.94	0.0	0.0	0.99	2.74	19.88
negative	1.65	2.01	0.0	0.0	0.99	2.71	21.82

Note: Descriptive statistics of price changes after the exclusion of exactly reversed price falls using different maximal duration. The sample consists of 11 788 products in total (3 524 for Retailer A, 4 446 for Retailer B, 2 048 for Retailer C and 1 770 for Retailer D). Rows *all* refer to all price changes, rows *positive* to price changes resulting in a higher price and rows *negative* to price changes resulting in a lower price.

As in all the previous cases, the usage of different sale duration does not have a homogeneous effect across retailers on the implied price rigidity. For Retailer A, shorting the temporary offer horizon has the most profound impact on the permanent price change frequency and hence price duration. Its average number of recorded price changes per year more than doubles, and the median more than quadruples after the sale horizon is shifted from 90 to 14 days. This shows that the specification of excluded temporal price changes is crucial since it can lead to substantial differences in the estimated price flexibility. For Retailer A using the 90-day sales, on average, prices last slightly more than half a year. On the other hand, for 14-day sales, they change more than every quarter. In quarterly macroeconomic models, this would seriously affect the assumed degree of price flexibility.

Even though limiting the horizon to 30 days instead of 90 does not change the number of identified sales drastically for most retailers, it has a non-negligible effect on the final price flexibility. Interestingly the effect is the highest for Retailer A (increase by 0.4 price change per year on average), even though for Retailer A the number of identified sales does not change dramatically for these two different durations. Retailer D, which records the highest decrease in identified sales using the 90 and 30-day definition, has the second highest increase in temporal price changes of around 0.3 per year. The lowest increase (of 0.05 price changes per year) is recorded for Retailer C, which records on average slightly less than one price per year for all the specified sale durations. Except for Retailer C, the changes are even more pronounced for medians. This shows that using one month filter presented by Nakamura & Steinsson (2008) to identify temporary price adjustments leads to only slightly different results for some but not all of the retailers in the presented analysis compared to using a 90-day filter. That is why I opt for the more conservative measure and count all symmetric temporal price changes lasting up to 90 days as temporal.

The definition of temporary prices as strict price reversals is not all-inclusive (Cavallo, 2018a; Nakamura & Steinsson, 2008). Exactly reversed price rises are not as common as exactly reversed price decreases, however, their impact on the implied price stickiness is not negligible. There are cases when following a temporary decrease price does not return to its pre-sale level - so called asymmetric V-shaped sales (Nakamura & Steinsson, 2008). The differences in the number of identified sales using asymmetric sales is relatively mild for aggregate data as well as for 3 of the retailers. This indicates that it is not common to use sales for repricing and that omitting such price developments from temporary price changes should not result in a significant bias in the frequency of non-temporary price changes in most cases.

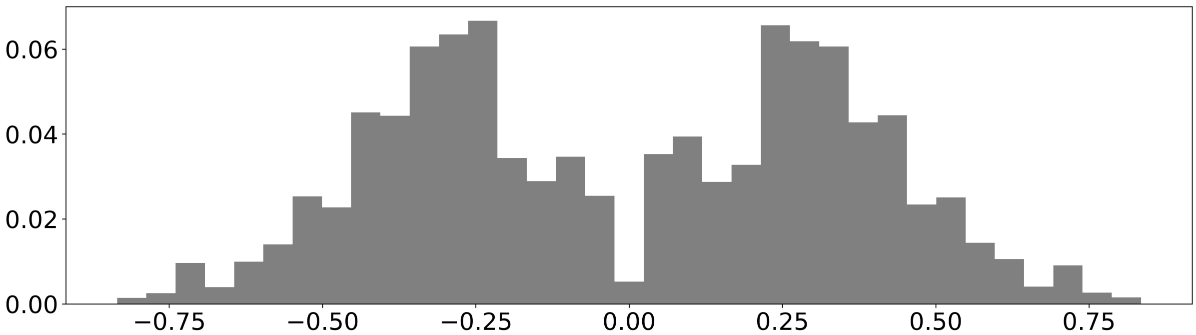
However, this does not hold for Retailer D, for which the number of identified sales rises by slightly more than 18%. Around 8% of all price changes recorded for Retailer D follow such path. Even if I limit the definition to offers that last only up to 30 or 14 days, the increase in the number of identified temporary offers is not marginal. These results provide further evidence that there are differences in pricing among the retailers and that for some this pricing might be relatively common, while for others it is not. Since it is only slightly less frequent for a price to be lower after a sale compared to higher, it appears that using sales prior to permanently increasing prices is not a very common technique. Detailed results allowing for asymmetric temporal price falls and temporal price rises are available upon request.

The findings above indicate that when defining temporary price changes, one should carefully analyse the data and integrate the insights gained from the analysis with the research objective. The assumptions about temporary price adjustments can significantly impact the final results on price flexibility. The considerable variations in pricing across retailers suggest that the estimated price flexibility is also strongly influenced by the observed retailer. This implies that the implied price flexibility also strongly depends on the analysed retailer. Subsequently, assumptions concerning temporary price adjustments that appear appropriate for one retailer may not be adequate or accurate for another retailer, as demonstrated by the substantial contrast in results for Retailer D. All in all, conducting robustness analysis to examine the different definitions of temporary price changes and their implications for price flexibility is crucial for ensuring the validity of the research.

Magnitude of price changes

As in the case of frequency of price change, also the analysis of price change magnitudes shows that the four retailers follow different pricing strategies. Nevertheless, the key general conclusions are consistent across all retailers. The average price change is substantially high compared to the prevailing level of inflation. This also holds for permanent price changes (i.e. after excluding temporary price offers). Most of the (non-zero) price changes are not concentrated around zero (Figure 3). Their distribution does not peak near zero and is not Gaussian. This holds overall but also after the disaggregation on the retailer and retailer and category level. In this sense, the distribution can be viewed as in favour of the theory of menu costs, which is in contrast to the most recent studies conducted by Hillen & Fedoseeva (2021) for Amazon, but supports the findings of Nakamura & Steinsson (2008); Klenow & Malin (2010) for offline prices. However, the issue of menu costs and reputation costs is more complex in online retail (Gorodnichenko & Talavera, 2017). Temporary price adjustments are, on average, of higher magnitude compared to permanent price changes, which is in line with the existing literature (Nakamura & Steinsson, 2008; Klenow & Malin, 2010; Hillen & Fedoseeva, 2021). The difference in means is smaller than Hillen & Fedoseeva (2021).

Figure 3: Magnitude of all price changes



Note: Histogram of price change magnitude in log-differences of all non-zero price changes. Sample consists of 11 788 goods for four different retailers from the period between January 2020 and April 2021. Total of 99 630 non-zero price changes were recorded during the examination period.

Table 6: Average magnitude of all price changes per retailer

	Retailer A	Retailer B	Retailer C	Retailer D	All
including zeros	$-1.82 * 10^{-5}$	$-0.11 * 10^{-5}$	$1.78 * 10^{-5}$	$3.78 * 10^{-5}$	$0.31 * 10^{-5}$
including zeros (abs)	0.0098	0.0082	0.0024	0.0029	0.0069
non-zero	-0.0006	-0.0001	0.0021	0.0022	0.0001
non-zero (abs)	0.3320	0.3660	0.2831	0.1722	0.3194
positive	0.3312	0.3643	0.2720	0.1689	0.3165
negative (abs)	0.3329	0.3678	0.2953	0.1757	0.3223
p-value difference	0.2579	0.1583	0.0002	0.0040	<0.0001

Note: Average price change magnitude in log-differences including all price changes in the sample. The sample consists of 11 788 products in total between January 2020 and April 2021 (3 524 for Retailer A, 4 446 for Retailer B, 2 048 for Retailer C and 1 770 for Retailer D). First two rows include zero price changes in the analysis, the remaining include only non-zero price changes. Row *positive* and row *negative* refer to positive and negative price changes, respectively. Row *p-value* reports the p-values of a two t-test with null hypothesis of equal means. *abs* means that for the computation of the mean, the absolute value of the data is used.

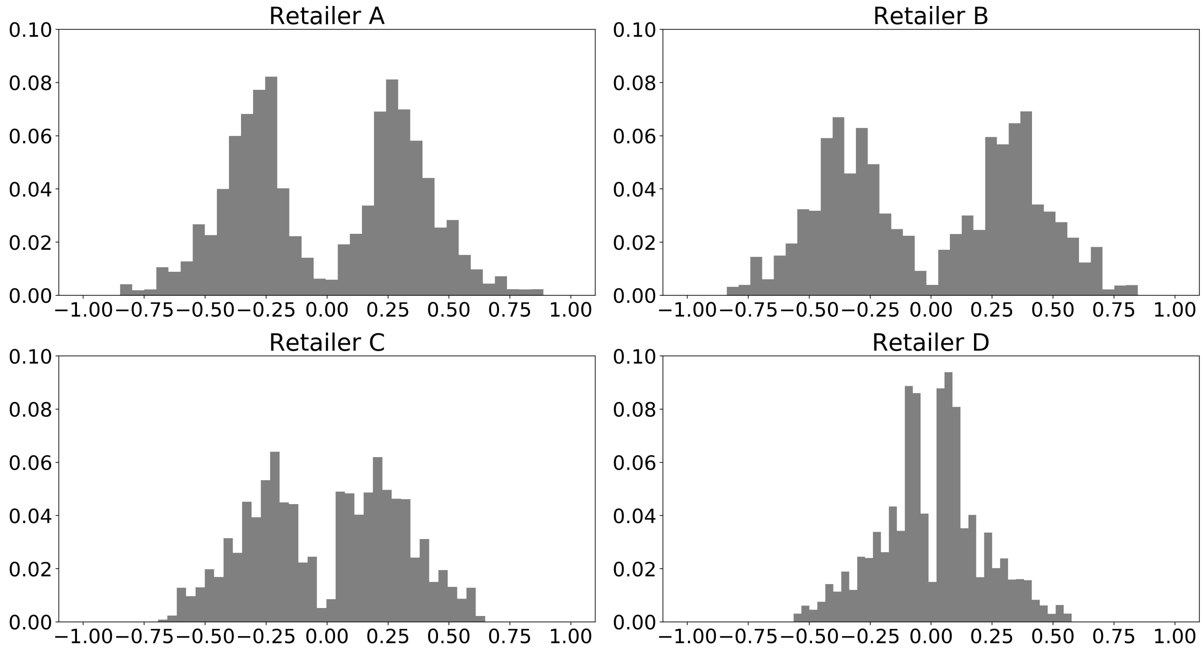
Since around 18% of the goods recorded no price change throughout the examination period and as price changes are relatively scarce compared to day-to-day price stability, the most common average price magnitude is zero. The average absolute size of a price change, including zero price changes, is around 0.007 (compared to 0.319 after their exclusion). The mean size of price increases is slightly lower than that of price decreases, and so are the corresponding medians (Table 6). The null hypothesis of equal means of price rises and price falls is rejected by a t-test and so is the similarity in the overall distributions by both the Wilcoxon rank-sum as well as two-sample Kolmogorov-Smirnov test. The average size of price fall is slightly higher than the average price rise for all the retailers. However, the difference is statistically significant only for two of them. Hillen & Fedoseeva (2021) find no statistical difference between the distributions of price increases and decreases (in absolute values), except for two product categories. The differences in the distributions of price change magnitude among the retailers are not only in the mean but in the overall shape of the distributions, including dispersion.

Excluding zero price changes yields an average price adjustment (in absolute values) of 0.319. Slightly more than 10% of all price changes are lower than 0.1 in absolute terms. More than half is higher than 0.3. Hillen & Fedoseeva (2021) find around 50% of all price changes to be less than 10% in magnitude. On the hand, Dhyne *et al.* (2006); Baudry *et al.* (2007); Klenow & Malin (2010) find the mean and the median of off-line price changes to be relatively significant compared to the prevailing rate of inflation with some incidence of small price changes.

As in the previous cases, there are substantial differences among the retailers (Figure 4). Retailer A and Retailer B record negative average price magnitude, whereas Retailer C and Retailer D positive. The mean of the absolute value of price changes also ranges among retailers, with the highest values recorded for Retailer A and Retailer B. Both of these retailers also record the highest average price increase and price decrease. On the other hand, Retailer D records the lowest average price changes if zero price changes are excluded. Similarly the aggregated results, the distributions of price changes do not peak around zero for any of the retailers (Firgure 4). The differences in the distributions are not only in the mean but in the overall shape of the distributions, including the dispersion.

Aside from differences among the four retailers, the distributions also differ across categories (Table A4 in the Appendix). Again contrary to Hillen & Fedoseeva (2021), the distributions are

Figure 4: Magnitude of all price changes



Note: Histograms of price change magnitude in log-differences per retailer of all non-zero price changes. Sample consists of 11 788 goods from January 2020 to April 2021 (3 524 for Retailer A, 4 446 for Retailer B, 2 048 for Retailer C and 1 770 for Retailer D). Total of 99 630 non-zero price changes were recorded during the examination period (44 202 for Retailer A, 35 691 for Retailer B, 6 159 for Retailer C and 13 578 for Retailer D).

not bell-curved for any of the categories. As in the case of retailers, they differ in shape. The aggregated average size of non-zero price changes ranges from 0.26 to 0.37, the median value from 0.26 to 0.37. Both of these statistics are among the lowest for categories "Beer", "Wine", "Meat", "Vegetables" and "Fruit" and among the highest for "Sugar, jam, honey, chocolate, conf." and "Fish, seafood". In most cases, average price falls are at least as high as average price rises. For more than half of the categories, the null hypothesis of equal mean or equal distribution is not rejected by a t-test or by Kolmogorov-Smirnov test. Hillen & Fedoseeva (2021) on the other hand finds a significant difference between positive and negative price changes in terms of magnitude only for "Meat & Seafood" and "Dairy", with slightly more pronounced price rises than price falls.

If only strict 90-day reversed price decreases are considered as temporary, then the average absolute size of such price changes is 0.35. The average size of the absolute value of all the remaining price adjustments is 0.21. If we shorten the time horizon of temporary prices to 30 and 14 days, the average size of a temporary price offer is 0.35 and 0.36, respectively. The mean magnitude of the remaining (i.e. permanent) price changes is 0.22 and 0.27, respectively.

The null hypothesis of similar distribution or same means is firmly rejected in all three cases indicating that there is relatively strong evidence that permanent price changes are of lower magnitude than temporary ones.

Excluding both reversed price falls and rises as temporary results in the average size of permanent price adjustments to be around 0.21 for 90-day, 0.22 for 30-day and 0.27 for 14-day temporary adjustments. Yet again, the null hypothesis of the same mean or distribution of temporary and permanent price changes is strongly rejected, supporting the findings of Nakamura & Steinsson (2008); Klenow & Malin (2010) and Hillen & Fedoseeva (2021).

Probability of price change

The analysis of factors influencing the probability of a price change further strengthens the evidence of substantial differences in the pricing of the four retailers. The reaction of the retailers to unprecedented or unanticipated macroeconomic events is somewhat ambiguous across the shops. I do not find evidence that retailers utilise dynamic pricing in response to unprecedented economic events (and the assumed subsequent changes in demand for their goods). The results (as in the previous cases) significantly depend on whether the temporary price changes are excluded and how they are defined. Nevertheless, there are some similarities across the retailers and price change specifications.

Most homogeneous results are found in the estimated average within effects, mainly for price characteristics such as the price level and ending digit of the price quote (Table A8 to Table A10 in Appendix). For all, permanent and temporary price changes, a higher price is associated with a lower probability of price change. In line with findings of Hillen & Fedoseeva (2021) for prices of groceries and Lünnemann & Wintr (2011) for other online sold goods, ceteris paribus cheaper goods tend to change prices or be on sale more often. As Lünnemann & Wintr (2011) and Chenavaz *et al.* (2018) for other categories of online prices, Levy *et al.* (2020) and Herrmann & Moeser (2006) for offline prices of groceries, I find prices ending with ".9" are, keeping all other factors fixed, significantly less likely to change.

The effect of the duration of the price quote (i.e. the number of days since the last price change) is, in most cases, estimated to be insignificant. It is negative and significant for all price changes and sales for Retailer A and for all and permanent price changes for Retailer D.

A negative effect indicates that the longer since the last price change, the lower is the likelihood of price adjustment (corresponding to a downward-sloping hazard function).

The only similarity in the timing of price adjustment is that all, permanent and temporal price changes are the least likely to occur on one of the weekend days (Saturday for Retailer D, Sunday for the remaining three retailers). The days associated with the highest probability of price change differ across retailers. Price changes, in general, are most common on Thursday for Retailer A and Retailer B, on Monday for Retailer C and on Wednesday for Retailer D. As sales account for most price changes, this also holds for temporary price adjustments. Permanent price changes are the most likely to occur on Wednesday, except for Retailer B, which ceteris paribus changes prices permanently the most on Thursday.

The incidence of price changes is somewhat scattered across the year. Contrary to Hillen & Fedoseeva (2021), none of the retailers records permanent price changes to be the most likely in January or during the first quarter of the year. However, they all record the highest probability of a permanent price change at the beginning of a quarter. For Retailer A, prices are the most likely to change in the middle of the year (June). For Retailer B and Retailer C, the highest probability is ceteris paribus at the beginning of the second quarter in April. Lastly, Retailer D is most likely to change prices permanently in October at the beginning of the fourth quarter. Regarding the yearly pattern of sales, the results are again relatively dispersed, with no common pattern among the retailers. For Retailer A, the most sales occur in June and then January. For Retailer B, sales are most likely in the second half of the year (except for December) and in February. For Retailer C and Retailer D, sales are the least probable in January and in January and February, respectively.

The collected sample is short and covers most of the months only once, which makes it hard to estimate the yearly pattern credibly. Moreover, the results regarding the yearly pattern should be viewed with caution since I collected the data during an unprecedented time in history with several abrupt and momentous changes in the economies. Hence, the results might not be representative of the general yearly pattern. I tried to account for some of such disruptions by re-estimating the models including control dummy variables for the initial interest rate rise in February 2020, the subsequent interest rate cuts, and the (re-)introduction of lockdowns in 2020 due to the coronavirus pandemic (Table A11 to Table A13 in Appendix). In line with Gorodnichenko *et al.* (2018b), the results do not show that there would be an apparent significant (and relatively immediate) reaction of online prices to major macroeconomic events. As (Hillen, 2021a), I do not find evidence that the retailers would react to presumed changes in their individual demand caused by the introductions of lockdowns. The estimated effects of such events are heterogeneous among the retailers and in some cases even the assumed duration of the impact. Since several events often happened almost simultaneously (such as the first lockdown and the two first interest rate cuts) and many measures introduced to stop the spread of the virus were introduced and subsequently changed, it is hard to separate the actual effects. In some cases, it is even more complicated since the events coincide with other potential determinants of the frequency of price change (such as Christmas or New Year's Eve).

For both retailers for which I have data for the interest rate hike in February 2020, the estimated impact of the probability of price changes is negative. The effects of the later events connected to the pandemic are ambiguous and depend on the assumed horizon of the impact. The introduction of the first lockdown (12$^{\text{th}}$ March 2020) is associated with a significant increase in the probability of price change in the next 7 and 14 days for Retailer A. However, for Retailer D the impact is positive only in the two following weeks, while in the week directly following the event, it is significantly negative. This discrepancy might be caused by the almost immediate interest rate decrease (17$^{\text{th}}$ March 2020), for which the effect is strongly positive for both horizons in the case of Retailer D. It is, however, negative in both cases for Retailer A. The subsequent interest rate cut (27$^{\text{th}}$ March 2020) is associated with a lower probability of price change for both retailers and both assumed impact horizons. The last interest rate decrease of the year (11$^{\text{th}}$ May 2020) is connected to a lower probability of price change for three of the four retailers (but for one of them it is not statistically significant). For Retailer A, the effect is significantly positive.

The last two events I examined are a partial re-opening of the economy (including retail) before Christmas (10$^{\text{th}}$ December 2020) and the subsequent lockdown following Christmas (27$^{\text{th}}$ December 2020). The estimated impact of this period on the probability of price change is far from unequivocal as it differs among retailers and the assumed time horizons. For these two events, the estimation is likely to be strongly influenced by the potential effect of Christmas. Unfortunately, I cannot control it as our data cover the Christmas period only once. The impact of the initial partial re-opening also captures the pricing behaviour before Christmas. The re-introduction of the lockdown then includes the effect after Christmas and New Year's Eve.

For permanent price changes, the estimated impacts of the events also depend on the retailer and the estimated time window of the effect. In some cases, the estimated coefficients differ from those estimated for all price changes. For Retailer A, the initial increase in interest rate in February 2020 is associated with a higher probability of price change, whereas for Retailer D and for all price changes it is negative. The impact of interest rate decreases and the re-introduction of lockdowns are mostly ambiguous. Even for sales, controlling for the selected events of 2020 leads to inconclusive results as they vary across retailers and the assumed duration of the impact. The estimated coefficients are often qualitatively different from those estimated for permanent and even all price changes.

Lastly, there is not much common pattern among the retailers in the impact of product category on price change probability (Table A5 to Table A7 in Appendix). Contrary to Hillen & Fedoseeva (2021), non-durable goods do not always have a higher chance of a price change compared to relatively more durable items. For example, for Retailer B and Retailer D, products from categories "Wine" and "Beer" are among those with the highest probability of price change keeping all other factors fixed. After the exclusion of temporary price adjustments, the significance and sometimes even the sign associated with different product categories changes compared to the estimates for all price changes. For example, categories "Wine" and "Beer" are no longer among those with the highest probability of price change, suggesting that most of their price changes are temporary price promotions.

In some cases, the differences might be caused by different pricing patterns for different product categories. In the case of all price changes, goods from the categories "Fruit" and "Vegetables" do not generally change prices the most often. However, for three out of the four retailers, they are among the categories with a significantly higher probability of permanent price change (compared to the base and other categories). However, this might be caused by the fact that both categories contain mainly seasonal goods for which the exclusion of exactly reversed price adjustments as temporary might not be accurate. This may not mean they are not often on sale, but because many price adjustments in this category do not comply with our definition of sale as being exactly reversed. These results further strengthen the evidence that temporary price changes should be analysed thoroughly and simple application of filters to identify them used in the previous literature may yield undesirable results.

6 Concluding Remarks

This paper utilises 4 million scraped online daily prices for 11 788 grocery products from four online retailers in the Czech Republic, covering the period between January 2020 and April 2021. In contrast to more commonly used price indices, online data are of much higher granularity and frequency. They are also not aggregated. This enables a more in-depth analysis of pricing behaviour, free from potential biases in pre-processed price indices (Cavallo & Rigobon, 2016; Cavallo, 2018b). Results obtained from such data can provide valuable insights into economic theories and assumptions concerning firms' price-setting behaviour.

I demonstrate that analysis of temporary price changes and their impact on the computed price rigidity is essential as the results may significantly differ from retailer to retailer. The final price flexibility significantly depends on the assumed characteristics of temporary price developments. Providing results using alternative temporary price change specifications can substantially increase the robustness of the presented findings. This is further supported by the fact that the impact of different definitions of temporary price adjustments varies across retailers and that the presented results depart from the findings of the previous literature dealing with online grocery prices. Further, temporary price changes can account for more than 70% of all price changes and have the potential to explain part of the price dispersion across the retailers. Hence, more attention should be paid to such a substantial part of the information prior to its omission from the analysis of price-setting behaviour. Temporary price changes are an essential aspect of price-setting behaviour and more interest should be paid to them in the upcoming research.

References

ALLISON, P. D. (2014): "Problems with the hybrid method." *Statistical Horizons, September* **2**.

ALVAREZ, L. J., E. DHYNE, M. HOEBERICHTS, C. KWAPIL, H. LE BIHAN, P. LÜNNEMANN, F. MARTINS, R. SABBATINI, H. STAHL, P. VERMEULEN *et al.* (2006): "Sticky prices in the euro area: a summary of new micro-evidence." *Journal of the European Economic association* **4(2-3)**: pp. 575–584.

ALVAREZ, S. E. & S. M. LEIN (2020): "Tracking inflation on a daily basis." *Swiss Journal of Economics and Statistics* **156(1)**: pp. 1–13.

APARICIO, D. & M. I. BERTOLOTTO (2020): "Forecasting inflation with online prices." *International Journal of Forecasting* **36(2)**: pp. 232–247.

BAUDRY, L., H. LE BIHAN, P. SEVESTRE, & S. TARRIEU (2007): "What do thirteen million price records have to say about consumer price rigidity?" *Oxford Bulletin of Economics and Statistics* **69(2)**: pp. 139–183.

BELL, A., M. FAIRBROTHER, & K. JONES (2019): "Fixed and random effects models: making an informed choice." *Quality & Quantity* **53(2)**: pp. 1051–1074.

BELL, A. & K. JONES (2015): "Explaining fixed effects: Random effects modeling of time-series cross-sectional and panel data." *Political Science Research and Methods* **3(1)**: pp. 133–153.

BRETON, R., T. FLOWER, M. MAYHEW, E. METCALFE, N. MILLIKEN, C. PAYNE, T. SMITH, J. WINTON, & A. WOODS (2016): "Research indices using web scraped data: May 2016 update." *Newport: Office for National Statistics. Available from www. ons. gov. uk/file* .

BRUMBACK, B. A., H. W. ZHENG, & A. B. DAILEY (2013): "Adjusting for confounding by neighborhood using generalized linear mixed models and complex survey data." *Statistics in medicine* **32(8)**: pp. 1313–1324.

CAVALLO, A. (2017): "Are online and offline prices similar? evidence from large multi-channel retailers." *American Economic Review* **107(1)**: pp. 283–303.

CAVALLO, A. (2018a): "More Amazon Effects: Online Competition and Pricing Behaviors." *NBER Working Papers 25138*, National Bureau of Economic Research, Inc.

CAVALLO, A. (2018b): "Scraped data and sticky prices." *Review of Economics and Statistics* **100(1)**: pp. 105–119.

CAVALLO, A. (2020): "Inflation with Covid Consumption Baskets." *NBER Working Papers 27352*, National Bureau of Economic Research, Inc.

CAVALLO, A. & R. RIGOBON (2016): "The billion prices project: Using online prices for measurement and research." *Journal of Economic Perspectives* **30(2)**: pp. 151–78.

CHENAVAZ, R., J. DROUARD, O. R. ESCOBAR, & B. KAROUBI (2018): "Convenience pricing in online retailing: Evidence from amazon. com." *Economic modelling* **70**: pp. 127–139.

DHYNE, E., L. J. ALVAREZ, H. LE BIHAN, G. VERONESE, D. DIAS, J. HOFFMANN, N. JONKER, P. LUNNEMANN, F. RUMLER, & J. VILMUNEN (2006): "Price changes in the euro area and the united states: Some facts from individual consumer price data." *Journal of Economic Perspectives* **20(2)**: pp. 171–192.

GOETGELUK, S. & S. VANSTEELANDT (2008): "Conditional generalized estimating equations for the analysis of clustered and longitudinal data." *Biometrics* **64(3)**: pp. 772–780.

GORODNICHENKO, Y., V. SHEREMIROV, & O. TALAVERA (2018a): "Price setting in online markets: Does it click?" *Journal of the European Economic Association* **16(6)**: pp. 1764–1811.

GORODNICHENKO, Y., V. SHEREMIROV, & O. TALAVERA (2018b): "The responses of internet retail prices to aggregate shocks: A high-frequency approach." *Economics Letters* **164**: pp. 124–127.

GORODNICHENKO, Y. & O. TALAVERA (2017): "Price setting in online markets: Basic facts, international comparisons, and cross-border integration." *American Economic Review* **107(1)**: pp. 249–82.

HERRMANN, R. & A. MOESER (2006): "Do psychological prices contribute to price rigidity? evidence from german scanner data on food brands." *Agribusiness: An International Journal* **22(1)**: pp. 51–67.

HILLEN, J. (2021a): "Online food prices during the covid-19 pandemic." *Agribusiness* **37(1)**: pp. 91–107.

HILLEN, J. (2021b): "Psychological pricing in online food retail." *British Food Journal* .

HILLEN, J. & S. FEDOSEEVA (2021): "E-commerce and the end of price rigidity?" *Journal of Business Research* **125**: pp. 63–73.

KEHOE, P. & V. MIDRIGAN (2015): "Prices are sticky after all." *Journal of Monetary Economics* **75**: pp. 35–53.

KLENOW, P. J. & B. A. MALIN (2010): "Microeconomic Evidence on Price-Setting." In B. M. FRIEDMAN & M. WOODFORD (editors), "Handbook of Monetary Economics," volume 3 of *Handbook of Monetary Economics*, chapter 6, pp. 231–284. Elsevier.

LEVY, D., A. SNIR, A. GOTLER, & H. A. CHEN (2020): "Not all price endings are created equal: Price points and asymmetric price rigidity." *Journal of Monetary Economics* **110**: pp. 33–49.

LÜNNEMANN, P. & L. WINTR (2011): "Price Stickiness in the US and Europe Revisited: Evidence from Internet Prices." *Oxford Bulletin of Economics and Statistics* **73(5)**: pp. 593–621.

Macias, P., D. Stelmasiak, & K. Szafranek (2023): "Nowcasting food inflation with a massive amount of online prices." *International Journal of Forecasting* **39(2)**: pp. 809–826.

Nakamura, E. & J. Steinsson (2008): "Five facts about prices: A reevaluation of menu cost models." *The Quarterly Journal of Economics* **123(4)**: pp. 1415–1464.

Paraschiv, C., N. Ayadi, X. Rousset, & M. Turinici (2023): "Consumer vulnerability to dynamic pricing in online environments." *Applied Economics* pp. 1–16.

Powell, B., G. Nason, D. Elliott, M. Mayhew, J. Davies, & J. Winton (2018): "Tracking and modelling prices using web-scraped price microdata: towards automated daily consumer price index forecasting." *Journal of the Royal Statistical Society: Series A (Statistics in Society)* **181(3)**: pp. 737–756.

Wooldridge, J. M. (2019): "Correlated random effects models with unbalanced panels." *Journal of Econometrics* **211(1)**: pp. 137–150.

Appendices

Table A1: Mean and median number of all price changes per year per category

	Retailer A	Retailer B	Retailer C	Retailer D	All
Bread and cereals					
mean	11.36	8.00	1.54	5.11	7.77
median	12.50	6.06	0.99	3.65	4.98
Meat					
mean	6.72	3.34	1.99	6.56	4.97
median	2.50	0.00	0.99	5.57	2.50
Fish, seafood					
mean	12.77	7.48	1.72	7.19	8.00
median	12.50	6.50	0.99	5.42	6.50
Milk, cheese, eggs					
mean	11.38	9.03	2.98	6.66	7.91
median	11.67	8.89	0.99	5.69	5.43
Oils, fats					
mean	11.02	7.39	2.64	7.20	7.17
median	11.77	5.95	1.99	6.25	5.42
Fruit					
mean	10.07	5.00	4.05	9.17	6.45
median	8.33	1.98	1.26	7.54	3.41
Vegetables					
mean	11.60	9.49	4.03	8.58	8.67
median	10.49	7.93	0.99	5.42	5.14
Sugar, jam, honey, chocolate, conf.					
mean	12.13	7.84	2.78	6.73	8.70
median	11.95	5.95	1.98	5.43	6.62
Food products n.e.c.					
mean	10.31	8.32	3.20	7.15	8.28
median	10.28	7.93	1.98	6.82	7.50
Coffee, tea, cocoa					
mean	12.92	9.80	4.67	10.68	9.46
median	13.33	9.92	3.97	9.95	8.93
Mineral waters,soft drinks, juices					
mean	12.82	10.73	3.69	9.53	9.99
median	16.67	6.08	2.38	7.34	6.02
Wine					
mean	10.20	11.74	-	10.23	10.90
median	11.21	11.90	-	10.23	11.74
Beer					
mean	12.44	17.61	5.89	15.66	12.59
median	15.83	19.84	4.46	13.55	10.91
All categories					
mean	10.89	8.24	3.10	7.43	8.01
median	10.83	6.76	1.21	6.12	5.94

Note: Rows *mean* present the average count of all price changes for the given category per year, rows *median* the median of the count of all price changes for the given category per year; Sample size: 11 788 products (Retailer A: 3 524 products, Retailer B: 4 446 products, Retailer C: 2 048 products, Retailer D: 1 770 products); for the period between January 2020 and April 2021

Table A2: Mean and median number of recorded sales per year per category

	Retailer A	Retailer B	Retailer C	Retailer D	All
Bread and cereals					
mean	5.02	3.57	0.57	1.38	3.32
median	5.14	2.98	0	0.9	1.98
Meat					
mean	2.65	1.4	0.7	1.59	1.75
median	0.83	0	0	1.01	0.83
Fish, seafood					
mean	5.06	3.22	0.58	1.34	3.01
median	4.87	2.74	0	0.9	1.98
Milk, cheese, eggs					
mean	5.03	4.08	1.25	1.6	3.28
median	5	3.97	0	1.14	1.7
Oils, fats					
mean	4.46	3.15	0.74	1.64	2.6
median	4.28	1.98	0	0.93	1.81
Fruit					
mean	2.92	1.72	1.07	1.5	1.76
median	2.45	0	0	1.14	0.99
Vegetables					
mean	4.15	3.74	0.95	1.48	2.71
median	3.33	1.98	0	0.9	0.99
Sugar, jam, honey, chocolate, conf.					
mean	5.24	3.46	1.15	1.36	3.68
median	5.14	2.98	0.99	0.9	2.71
Food products n.e.c.					
mean	3.85	3.6	1.19	1.66	3.13
median	3.65	3.22	0.99	1.67	2.71
Coffee, tea, cocoa					
mean	5.67	4.25	1.98	2.65	3.9
median	6.64	4.11	1.98	1.91	3.43
Mineral waters,soft drinks, juices					
mean	5.72	4.79	1.65	3.07	4.36
median	7.5	2.97	1.01	2.54	2.68
Wine					
mean	4.04	5.46	-	2.01	4.46
median	4.31	5.95	-	2.27	4.82
Beer					
mean	5.27	7.71	2.65	4.97	5.18
median	6.31	7.93	1.98	3.62	4.52
All categories					
mean	4.48	3.58	1.13	1.7	3.14
median	4.17	2.98	0	1.04	1.98

Note: Rows *mean* present the average count of sales for the given category per year, rows *median* the median of the count of sales for the given category per year. Sales are identified as price falls exactly reversed within 90 days. Sample size: 11 788 products (Retailer A: 3 524 products, Retailer B: 4 446 products, Retailer C: 2 048 products, Retailer D: 1 770 products); for the period between January 2020 and April 2021

Table A3: Mean and median number of recorded permanent price changes per year per category

	Retailer A	Retailer B	Retailer C	Retailer D	All
Bread and cereals					
mean	0.98	0.86	0.42	2.41	1.03
median	0.83	0.99	0	1.81	0.83
Meat					
mean	1.27	0.5	0.6	3.23	1.37
median	0.83	0	0	2.71	0.83
Fish, seafood					
mean	2.51	1.11	0.68	4.32	1.96
median	2.43	0.99	0	2.71	0.99
Milk, cheese, eggs					
mean	1.35	0.76	0.51	3.2	1.27
median	0.83	0	0	2.71	0
Oils, fats					
mean	1.77	1.2	1.16	3.74	1.9
median	0.85	0.99	0.99	2.71	0.99
Fruit					
mean	3.8	1.45	1.84	5.91	2.74
median	2.34	0.99	0.99	4.52	1.04
Vegetables					
mean	2.75	1.65	2.04	5.23	2.88
median	1.67	0.99	0.99	3.61	0.99
Sugar, jam, honey, chocolate, conf.					
mean	1.39	0.87	0.45	3.58	1.19
median	0.86	0.99	0	3.61	0.86
Food products n.e.c.					
mean	2.41	1	0.8	3.82	1.9
median	1.9	0	0.99	3.55	0.99
Coffee, tea, cocoa					
mean	1.04	1.25	0.74	5.29	1.52
median	0.83	0.99	0	4.77	0.99
Mineral waters,soft drinks, juices					
mean	1.19	1.47	0.38	2.95	1.31
median	0.83	0.99	0	3.61	0.99
Wine					
mean	2.44	0.73	-	4.81	1.92
median	2.5	0	-	3.41	1.67
Beer					
mean	1.25	1.64	0.57	5.78	1.9
median	0.83	0.99	0	5.87	0.99
All categories					
mean	1.67	1.03	0.83	3.86	1.61
median	0.83	0	0	3.02	0.99

Note: Rows *mean* present the average count of permanent price changes for the given category per year, rows *median* the median of the count of permanent price changes for the given category per year. Permanent price changes are all price changes that are not price falls exactly reversed within 90 days. Sample size: 11 788 products (Retailer A: 3 524 products,Retailer B: 4 446 products, Retailer C: 2 048 products, Retailer D: 1 770 products); for the period between January 2020 and April 2021

Table A4: Average and median magnitude of all price changes per category

	Retailer A	Retailer B	Retailer C	Retailer D	All
Bread and cereals					
mean	0.3	0.33	0.23	0.18	0.3
median	0.26	0.31	0.19	0.12	0.28
Meat					
mean	0.32	0.35	0.3	0.16	0.28
median	0.31	0.3	0.22	0.13	0.26
Fish, seafood					
mean	0.38	0.45	0.32	0.15	0.37
median	0.37	0.45	0.34	0.1	0.37
Milk, cheese, eggs					
mean	0.33	0.32	0.27	0.16	0.3
median	0.32	0.3	0.27	0.11	0.29
Oils, fats					
mean	0.36	0.36	0.26	0.19	0.31
median	0.37	0.36	0.23	0.13	0.33
Fruit					
mean	0.32	0.37	0.26	0.16	0.29
median	0.29	0.38	0.22	0.11	0.28
Vegetables					
mean	0.29	0.37	0.27	0.17	0.28
median	0.29	0.36	0.22	0.12	0.29
Sugar, jam, honey, chocolate, conf.					
mean	0.38	0.45	0.30	0.19	0.39
median	0.33	0.41	0.28	0.11	0.34
Food products n.e.c.					
mean	0.31	0.37	0.27	0.18	0.31
median	0.3	0.38	0.26	0.11	0.31
Coffee, tea, cocoa					
mean	0.42	0.4	0.3	0.2	0.37
median	0.41	0.39	0.25	0.15	0.36
Mineral waters,soft drinks, juices					
mean	0.33	0.32	0.35	0.15	0.31
median	0.34	0.34	0.35	0.14	0.33
Wine					
mean	0.26	0.32	-	0.14	0.28
median	0.24	0.32	-	0.12	0.29
Beer					
mean	0.28	0.29	0.28	0.17	0.26
median	0.27	0.29	0.25	0.16	0.29

Note: Price changes are expressed in terms of log-returns; rows *mean* present the average size of all price changes for the given category, rows *median* the median value of all price changes for the given category ; Sample size: 11 788 products (Retailer A: 3 524 products, Retailer B: 4 446 products, Retailer C: 2 048 products, Retailer D: 1 770 products); for the period between January 2020 and April 2021

Table A5: Between effects (all price changes)

	Retailer A	Retailer B	Retailer C	Retailer D	All
Intercept					
	-5.381^{***}	-4.942^{***}	1.765	-3.511^{***}	-4.062^{***}
	(0.276)	(0.0.926)	(2.003)	(0.472)	(0.152)
Bread and cereals					
	-	-	-	-	-
Meat					
	0.108***	-0.054	0.027	-0.027	0.060***
	(0.022)	(0.037)	(0.078)	(0.044)	(0.016)
Fish, seafood					
	0.150***	0.005	-0.197	0.040	0.075***
	(0.032)	(0.036)	(0.122)	(0.066)	(0.021)
Milk, cheese, eggs					
	0.094***	0.081***	-0.125*	0.070	0.083***
	(0.021)	(0.021)	(0.067)	(0.043)	(0.013)
Oils, fats					
	0.021	0.093**	-0.067	0.130**	0.043**
	(0.037)	(0.039)	(0.094)	(0.058)	(0.022)
Fruit					
	0.180***	0.023	0.083	0.296***	0.138***
	(0.034)	(0.036)	(0.077)	(0.052)	(0.019)
Vegetables					
	0.086***	0.099***	0.120*	0.210***	0.130***
	(0.022)	(0.026)	(0.068)	(0.042)	(0.014)
Sugar, jam, honey, chocolate, conf.					
	0.038**	0.066***	-0.018	0.006	0.035***
	(0.019)	(0.022)	(0.078)	(0.058)	(0.013)
Food products n.e.c.					
	-0.087***	-0.032	-0.126*	-0.072	-0.066***
	(0.017)	(0.022)	(0.070)	(0.045)	(0.012)
Coffee, tea, cocoa					
	0.031	-0.012	-0.015	0.147***	0.014
	(0.022)	(0.023)	(0.068)	(0.050)	(0.014)
Mineral waters,soft drinks, juices					
	0.030	0.044*	-0.255***	0.070	0.045***
	(0.023)	(0.025)	(0.080)	(0.062)	(0.015)
Wine					
	-0.186***	0.207***	-	0.240**	0.009
	(0.046)	(0.047)	-	(0.113)	(0.030)
Beer					
	-0.008	0.272***	-0.008	0.269 ***	0.141***
	(0.044)	(0.046)	(0.093)	(0.073)	(0.025)

Note: Between effects of a logistic binary correlated random effect model; dependent variable: = 0 if $p_{i,t} = p_{i,t-1}$ and 1 otherwise; 4 151 848 observations - unbalanced panel of 11 788 products from the period between January 2020 and April 2021, Retailer A: 3 424 products, Retailer B: 4 446, Retailer C: 2 048, Retailer D: 1 770 products; "Bread and cereal" is the reference category; *p<0.1, **p<0.05, ***p<0.01

Table A6: Between effects (permanent price changes)

	Retailer A	Retailer B	Retailer C	Retailer D	All
Intercept					
	-6.419***	-9.656***	-4.666	-4.825***	−4.872***
	(0.805)	(3.066)	(3.928)	(0.622)	(0.323)
Bread and cereals					
	-	-	-	-	-
	-	-	-	-	-
Meat					
	0.028	-0.452***	0.067	-0.093	-0.093**
	(0.062)	(0.109)	(0.143)	(0.062)	(0.037)
Fish, seafood					
	-0.029	0.093	0.155	0.024	0.006
	(0.087)	(0.102)	(0.206)	(0.084)	(0.050)
Milk, cheese, eggs					
	0.024	-0.149**	0.105	0.087	-0.011
	(0.077)	(0.071)	(0.136)	(0.063)	(0.037)
Oils, fats					
	0.294***	0.109	0.246*	0.075	0.136***
	(0.097)	0.106	(0.149)	(0.081)	(0.049)
Fruit					
	0.260***	-0.102	0.258**	0.311***	0.176***
	(0.071)	(0.077)	(0.128)	(0.067)	(0.039)
Vegetables					
	0.218***	-0.026	0.434***	0.270***	0.219***
	(0.060)	(0.071)	(0.117)	(0.058)	(0.034)
Sugar, jam, honey, chocolate, conf.					
	0.005	-0.358***	-0.153	0.033	-0.070*
	(0.063)	(0.071)	(0.160)	(0.079)	(0.039)
Food products n.e.c.					
	0.001	-0.173**	0.111	-0.057	-0.090***
	(0.052)	(0.068)	(0.131)	(0.064)	(0.033)
Coffee, tea, cocoa					
	0.029	-0.177**	0.373***	0.123*	0.065*
	(0.082)	(0.070)	(0.131)	(0.068)	(0.039)
Mineral waters,soft drinks, juices					
	-0.086	-0.024	0.087	-0.105	-0.042
	(0.086)	(0.084)	(0.186)	(0.106)	(0.050)
Wine					
	-0.026	-0.654***	-	0.179	-0.142
	(0.122)	(0.201)	-	(0.164)	(0.087)
Beer					
	-0.098	-0.121	0.158	0.104	0.033
	(0.153)	(0.157)	(0.236)	(0.103)	(0.070)

Note: Between effects of a logistic binary correlated random effect model; dependent variable: = 0 if $p_{i,t} = p_{i,t-1}$ and 1 otherwise - excluding price rises and falls exactly reversed within 90 days; 4 151 848 observations - unbalanced panel of 11 788 products from the period between January 2020 and April 2021, Retailer A: 3 424 products, Retailer B: 4 446, Retailer C: 2 048, Retailer D: 1 770 products; "Bread and cereal" is the reference category; *p<0.1, **p<0.05, ***p<0.01

Table A7: Between effects (temporary price changes)

	Retailer A	Retailer B	Retailer C	Retailer D	All
Intercept					
	-6.383^{***}	-6.325	-0.715	-3.397^{***}	-3.913^{***}
	(0.887)	(3.893)	(5.224)	(0.762)	(0.448)
Bread and cereals					
	-	-	-	-	-
	-	-	-	-	-
Meat					
	0.036	0.260	0.240	0.237^{***}	0.316^{***}
	(0.085)	(0.164)	(0.197)	(0.064)	(0.055)
Fish, seafood					
	-0.477^{***}	-0.351^{**}	-0.249	0.211^{**}	-0.016
	(0.124)	(0.178)	(0.338)	(0.101)	(0.076)
Milk, cheese, eggs					
	0.316^{***}	0.391^{***}	-0.328^{**}	0.081	0.161^{***}
	(0.084)	(0.105)	(0.167)	(0.063)	(0.046)
Oils, fats					
	-0.781^{***}	0.630^{***}	-0.190	0.304^{***}	0.037
	(0.140)	(0.188)	(0.252)	(0.083)	(0.077)
Fruit					
	0.116	0.044	-0.321	0.426^{***}	0.073
	(0.127)	(0.170)	(0.210)	(0.073)	(0.067)
Vegetables					
	0.089	0.165	-0.243	0.325^{***}	0.095^{*}
	(0.085)	(0.135)	(0.182)	(0.060)	(0.052)
Sugar, jam, honey, chocolate, conf.					
	-0.488^{***}	-0.118	0.128	0.094	-0.114^{**}
	(0.076)	(0.108)	(0.193)	(0.084)	(0.048)
Food products n.e.c.					
	-0.105	-0.042	-0.225	0.014	-0.047
	(0.066)	(0.107)	(0.177)	(0.065)	(0.044)
Coffee, tea, cocoa					
	-0.645^{***}	-0.255^{**}	-0.072	0.068	-0.287^{***}
	(0.090)	(0.118)	(0.180)	(0.072)	(0.052)
Mineral waters,soft drinks, juices					
	-0.207^{**}	-0.227^{*}	-0.618^{***}	0.094	-0.152^{***}
	(0.093)	(0.132)	(0.204)	(0.083)	(0.057)
Wine					
	0.244	1.469^{***}	-	0.450^{***}	0.674^{***}
	(0.165)	(0.251)	-	(0.154)	(0.108)
Beer					
	-0.063	-0.141	-0.364	0.222^{**}	-0.049
	(0.173)	(0.287)	(0.264)	(0.091)	(0.100)

Note: Note: Between effects of a logistic binary correlated random effect model; dependent variable: = 0 if $p_{i,t} = p_{i,t-1}$ and 1 otherwise - considering price rises and falls exactly reversed within 90 days; 4 151 848 observations - unbalanced panel of 11 788 products from the period between January 2020 and April 2021, Retailer A: 3 424 products, Retailer B: 4 446, Retailer C: 2 048, Retailer D: 1 770 products; "Bread and cereal" is the reference category; *p<0.1, **p<0.05, ***p<0.01

Table A8: Within effects (all price changes)

	Retailer A	Retailer B	Retailer C	Retailer D	All
Price	-1.023^{***}	-2.257^{***}	-2.777^{***}	-1.363^{***}	-1.617***
	(0.035)	(0.033)	(0.072)	(0.057)	(0.020)
Price duration	-0.026^{**}	-0.013	-0.010	-0.028*	-0.024***
	(0.013)	(0.014)	(0.019)	(0.015)	(0.007)
Attractive price	-0.505***	-0.875***	-1.235***	-0.467***	-0.662***
	(0.048)	(0.048)	(0.194)	(0.087)	(0.030)
Tuesday	1.313***	0.522***	-2.000^{***}	1.468***	0.500***
	(0.032)	(0.038)	(0.049)	(0.036)	(0.016)
Wednesday	2.407***	1.409***	-1.392***	1.554***	1.239***
	(0.030)	(0.034)	(0.038)	(0.035)	(0.014)
Thursday	3.048***	3.323***	-2.046***	-0.257***	2.093***
	(0.029)	(0.031)	(0.049)	(0.047)	(0.013)
Friday	0.477***	0.350***	-2.306***	0.341***	-0.231***
	(0.036)	(0.039)	(0.055)	(0.041)	(0.018)
Saturday	0.544***	-0.745***	-2.654***	-0.270***	-0.605***
	(0.036)	(0.053)	(0.065)	(0.047)	(0.021)
Sunday	-1.522***	-1.456***	-3.266***	-0.141***	-1.453***
	(0.068)	(0.068)	(0.085)	(0.046)	(0.028)
February	0.0002	0.130***	0.590***	0.095**	0.133***
	(0.024)	(0.029)	(0.070)	(0.043)	(0.016)
March	0.068***	0.119***	0.347***	0.266***	0.155***
	(0.024)	(0.029)	(0.070)	(0.043)	(0.016)
April	-0.133***	0.173***	0.427***	0.415***	0.112***
	(0.025)	(0.028)	(0.073)	(0.041)	(0.016)
May	-0.202***	-0.067**	0.222***	0.548***	0.007
	(0.029)	(0.031)	(0.077)	(0.042)	(0.018)
June	0.438***	0.174***	0.363***	0.665***	0.364***
	(0.027)	(0.030)	(0.074)	(0.042)	(0.017)
July	-0.029	0.012	0.561***	0.388***	0.106***
	(0.027)	(0.029)	(0.069)	(0.040)	(0.017)
August	-0.300***	0.122***	0.388***	0.159***	-0.001
	(0.029)	(0.029)	(0.070)	(0.043)	(0.018)
September	-0.097***	0.225***	0.426***	0.147***	0.119***
	(0.027)	(0.029)	(0.072)	(0.042)	(0.017)
October	0.117***	0.063**	0.602***	0.739***	0.239***
	(0.026)	(0.028)	(0.072)	(0.038)	(0.016)
November	0.052*	0.007	0.533***	0.302***	0.120***
	(0.027)	(0.030)	(0.070)	(0.043)	(0.017)
December	0.153***	-0.129***	0.478***	0.155***	0.083***
	(0.027)	(0.029)	(0.072)	(0.044)	(0.017)

Note: Within effects of a logistic binary correlated random effect model; dependent variable: $= 0$ if $p_{i,t} = p_{i,t-1}$ and 1 otherwise; 4 151 848 observations - unbalanced panel of 11 788 products from the period between January 2020 and April 2021, Retailer A: 3 424 products, Retailer B: 4 446, Retailer C: 2 048, Retailer D: 1 770 products; "Monday" and "January" are the reference categories; *p<0.1, **p<0.05, ***p<0.01

Table A9: Within effects (permanent price changes)

	Retailer A	Retailer B	Retailer C	Retailer D	All
Price	-1.349***	-0.874***	-0.967***	-0.194**	-0.669***
	(0.119)	(0.097)	(0.185)	(0.082)	(0.051)
Price duration	-0.012	-0.036	-0.022	-0.094***	-0.035***
	(0.019)	(0.023)	(0.038)	(0.027)	(0.012)
Attractive price	-0.807***	-1.842***	-0.860**	-0.710***	-1.235***
	(0.099)	(0.085)	(0.395)	(0.106)	(0.055)
Tuesday	1.900***	-0.014	-0.079	1.675***	1.038***
	(0.116)	(0.058)	(0.095)	(0.049)	(0.031)
Wednesday	3.007***	-0.549***	0.284***	0.401***	0.771***
	(0.111)	(0.066)	(0.086)	(0.057)	(0.032)
Thursday	2.925***	0.660***	0.129	-0.389***	0.850***
	(0.111)	(0.050)	(0.089)	(0.069)	(0.031)
Friday	1.659***	-0.686***	-0.018	0.274***	0.145***
	(0.118)	(0.068)	(0.092)	(0.058)	(0.036)
Saturday	2.527***	-1.275***	-0.403***	-0.212***	0.213***
	(0.112)	(0.086)	(0.105)	(0.065)	(0.035)
Sunday	-0.744***	-2.681***	-1.652***	0.229***	-0.605***
	(0.193)	(0.158)	(0.162)	(0.059)	(0.044)
February	0.907***	0.113	0.204*	0.051	0.367***
	(0.085)	(0.116)	(0.117)	(0.064)	(0.040)
March	1.007***	1.149***	-0.194	0.028	0.547***
	(0.084)	(0.095)	(0.124)	(0.065)	(0.038)
April	1.112***	1.535***	0.549***	0.513***	0.875***
	(0.085)	(0.091)	(0.109)	(0.058)	(0.037)
May	0.893***	0.955***	0.008	0.199***	0.471***
	(0.092)	(0.099)	(0.127)	(0.066)	(0.042)
June	1.418***	0.912***	0.003	0.536***	0.723***
	(0.088)	(0.100)	(0.126)	(0.061)	(0.040)
July	0.768***	0.037	-0.435***	0.316***	0.253***
	(0.091)	(0.114)	(0.133)	(0.060)	(0.042)
August	0.275***	0.672***	-0.766***	0.127**	0.139***
	(0.101)	(0.102)	(0.148)	(0.063)	(0.044)
September	0.290***	0.744***	-0.295**	-0.034	0.151***
	(0.100)	(0.100)	(0.131)	(0.065)	(0.043)
October	0.664***	0.632***	0.079	0.878***	0.632***
	(0.093)	(0.101)	(0.121)	(0.053)	(0.039)
November	0.655***	-0.272**	-0.283**	0.253***	0.168***
	(0.095)	(0.124)	(0.135)	(0.062)	(0.044)
December	0.370***	-0.556***	0.131	0.013	-0.022
	(0.101)	(0.131)	(0.122)	(0.067)	(0.046)

Note: Within effects of a logistic binary correlated random effect model; dependent variable: = 0 if $p_{i,t} = p_{i,t-1}$ and 1 otherwise - excluding price rises and falls exactly reversed within 90 days; 4 151 848 observations - unbalanced panel of 11 788 products from the period between January 2020 and April 2021, Retailer A: 3 424 products, Retailer B: 4 446, Retailer C: 2 048, Retailer D: 1 770 products; "Monday" and "January" are the reference categories; *p<0.1, **p<0.05, ***p<0.01

Table A10: Within effects (temporary price changes)

	Retailer A	Retailer B	Retailer C	Retailer D	All
Price	-14.766^{***}	-15.546^{***}	-10.664^{***}	-4.369^{***}	-10.712^{***}
	(0.137)	(0.138)	(0.205)	(0.098)	(0.065)
Price duration	-0.037^{**}	-0.002	0.013	0.033	-0.006
	(0.019)	(0.021)	(0.032)	(0.022)	(0.011)
Attractive price	-0.834^{***}	-0.814^{***}	-0.781^{***}	0.049	-0.835^{***}
	(0.066)	(0.070)	(0.269)	(0.181)	(0.045)
Tuesday	1.038^{***}	1.037^{***}	-2.460^{***}	1.327^{***}	0.308^{***}
	(0.046)	(0.074)	(0.083)	(0.065)	(0.025)
Wednesday	2.164^{***}	2.456^{***}	-1.831^{***}	2.064^{***}	1.371^{***}
	(0.042)	(0.066)	(0.065)	(0.061)	(0.022)
Thursday	3.024^{***}	4.151^{***}	-2.628^{***}	-0.057	2.251^{***}
	(0.040)	(0.064)	(0.088)	(0.081)	(0.020)
Friday	-0.712^{***}	0.779^{***}	-2.942^{***}	0.348^{***}	-0.696^{***}
	(0.067)	(0.076)	(0.101)	(0.074)	(0.032)
Saturday	0.175^{***}	-0.052	-3.313^{***}	-0.209^{**}	-0.745^{***}
	(0.053)	(0.090)	(0.120)	(0.085)	(0.033)
Sunday	-1.808^{***}	-0.904^{***}	-3.617^{***}	0.163^{**}	-1.506^{***}
	(0.102)	(0.115)	(0.134)	(0.077)	(0.044)
February	-0.129^{***}	0.219^{***}	0.704^{***}	-0.181^{**}	0.059^{**}
	(0.036)	(0.045)	(0.118)	(0.079)	(0.025)
March	-0.047	-0.027	0.300^{**}	0.196^{***}	0.041^{*}
	(0.034)	(0.045)	(0.121)	(0.075)	(0.024)
April	-0.652^{***}	-0.254^{***}	0.076	0.513^{***}	-0.317^{***}
	(0.040)	(0.047)	(0.139)	(0.068)	(0.027)
May	-0.238^{***}	-0.057	0.350^{***}	0.593^{***}	-0.031
	(0.043)	(0.047)	(0.130)	(0.070)	(0.027)
June	0.408^{***}	0.230^{***}	0.675^{***}	0.727^{***}	0.364^{***}
	(0.039)	(0.046)	(0.122)	(0.069)	(0.026)
July	-0.073^{*}	0.107^{**}	0.808^{***}	0.771^{***}	0.140^{***}
	(0.038)	(0.044)	(0.116)	(0.062)	(0.025)
August	-0.373^{***}	0.137^{***}	0.544^{***}	0.182^{**}	-0.049^{*}
	(0.042)	(0.046)	(0.117)	(0.072)	(0.027)
September	-0.185^{***}	0.131^{***}	0.617^{***}	0.320^{***}	0.046^{*}
	(0.040)	(0.044)	(0.119)	(0.067)	(0.026)
October	0.014	0.026	0.711^{***}	0.463^{***}	0.120^{***}
	(0.038)	(0.044)	(0.123)	(0.068)	(0.025)
November	-0.048	0.185^{***}	0.760^{***}	0.379^{***}	0.154^{***}
	(0.039)	(0.045)	(0.117)	(0.070)	(0.026)
December	-0.252^{***}	-0.189^{***}	0.462^{***}	0.185^{**}	-0.105^{***}
	(0.039)	(0.045)	(0.125)	(0.073)	(0.026)

Note: Within effects of a logistic binary correlated random effect model; dependent variable: $= 0$ if $p_{i,t} = p_{i,t-1}$ and 1 otherwise - considering price rises and falls exactly reversed within 90 days; 4 151 848 observations - unbalanced panel of 11 788 products from the period between January 2020 and April 2021, Retailer A: 3 424 products, Retailer B: 4 446, Retailer C: 2 048, Retailer D: 1 770 products; "Monday" and "January" are the reference categories; *p<0.1, **p<0.05, ***p<0.01

Table A11: Macroeconomic events (all price changes)

	Retailer A	Retailer B	Retailer C	Retailer D	All
7-day window					
ir increase(2020-02-06)	-0.148***	-	-	-0.477***	−0.375***
	(0.041)	-	-	(0.092)	(0.036)
lockdown #1 (2020-03-12)	0.623***	-	-	-2.230***	−0.242***
	(0.042)	-	-	(0.116)	(0.039)
ir decrease (2020-03-17)	-1.466***	-	-	1.850***	−0.029
	(0.065)	-	-	(0.071)	(0.043)
ir decrease (2020-03-27)	-0.044	-	-	-1.052***	−0.364***
	(0.046)	-	-	(0.111)	(0.041)
ir decrease(2020-05-11)	0.961***	-0.282***	−0.049	-0.779***	0.194***
	(0.045)	(0.054)	(0.111)	(0.081)	(0.029)
lockdown #2 (2020-10-22)	0.205***	0.132***	0.153	-0.584***	0.043*
	(0.036)	(0.039)	(0.110)	(0.070)	(0.024)
partial re-opening (2020-12-10)	-0.610***	0.272***	0.247**	0.096	−0.076***
	(0.045)	(0.040)	(0.108)	(0.083)	(0.028)
lockdown #3 (2020-12-27)	0.294***	0.644***	-0.499***	-0.009	0.359***
	(0.040)	(0.125)	(0.125)	(0.084)	(0.027)
14-day window					
ir increase(2020-02-06)	-0.043	-	-	-0.876***	−0.321***
	(0.032)	-	-	(0.077)	(0.028)
lockdown #1 (2020-03-12)	0.635***	-	-	0.361***	0.328***
	(0.043)	-	-	(0.108)	(0.037)
ir decrease (2020-03-17)	-0.779***	-	-	0.685***	−0.271***
	(0.049)	-	-	(0.105)	(0.042)
ir decrease (2020-03-27)	-0.491***	-	-	-1.406***	−0.729***
	(0.040)	-	-	(0.084)	(0.034)
ir decrease(2020-05-11)	0.546***	-0.532***	-0.151	-1.064***	−0.134***
	(0.044)	(0.046)	(0.108)	(0.066)	(0.027)
lockdown #2 (2020-10-22)	0.117***	0.137***	0.106	-0.257***	0.054***
	(0.028)	(0.030)	(0.077)	(0.049)	(0.018)
partial re-opening (2020-12-10)	-1.986***	-0.631***	0.229**	0.158**	−0.901***
	(0.044)	(0.043)	(0.100)	(0.072)	(0.026)
lockdown #3 (2020-12-27)	-0.305***	-0.098***	-0.426***	0.259***	−0.054**
	(0.032)	(0.036)	(0.099)	(0.059)	(0.021)

Note: Logistic binary correlated random effect model - effect of selected important events on probability of price change during 7 and 14 days following the given event; dependent variable: = 0 if $p_{i,t} = p_{i,t-1}$ and 1 otherwise; 4 151 848 observations (unbalanced panel of 11 788 products from the period between January 2020 and April 2021 for four retailers); *p<0.1, **p<0.05, ***p<0.01

Table A12: Macroeconomic events (permanent price changes)

	Retailer A	Retailer B	Retailer C	Retailer D	All
7-day window					
ir increase(2020-02-06)	0.545***	-	-	-1.269***	−0.139*
	(0.092)	-	-	(0.185)	(0.079)
lockdown #1 (2020-03-12)	0.129	-	-	-2.441***	−1.252***
	(0.114)	-	-	(0.215)	(0.102)
ir decrease (2020-03-17)	-2.047***	-	-	2.155***	0.505***
	(0.236)	-	-	(0.120)	(0.069)
ir decrease (2020-03-27)	-1.393***	-	-	-0.543***	−1.321***
	(0.182)	-	-	(0.155)	(0.114)
ir decrease(2020-05-11)	1.030***	0.254**	-0.164	-0.288**	0.323***
	(0.102)	(0.116)	(0.219)	(0.120)	(0.061)
lockdown #2 (2020-10-22)	0.369***	0.151	-0.192	-0.672***	−0.197***
	(0.109)	(0.124)	(0.217)	(0.097)	(0.058)
partial re-opening (2020-12-10)	-0.834***	-0.023	0.690***	0.043	−0.136
	(0.167)	(0.244)	(0.190)	(0.128)	(0.083)
lockdown #3 (2020-12-27)	-0.527***	0.486**	−0.244	-0.025	−0.119
	(0.171)	(0.201)	(0.210)	(0.122)	(0.081)
14-day window					
ir increase(2020-02-06)	0.780***	-	-	-1.634***	−0.103*
	(0.077)	-	-	(0.144)	(0.061)
lockdown #1 (2020-03-12)	-0.029	-	-	0.366*	−0.396***
	(0.121)	-	-	(0.190)	(0.095)
ir decrease (2020-03-17)	-1.162***	-	-	1.718***	0.320***
	(0.161)	-	-	(0.191)	(0.100)
ir decrease (2020-03-27)	-1.359***	-	-	-1.524***	−1.645***
	(0.129)	-	-	(0.133)	(0.088)
ir decrease(2020-05-11)	0.513***	-0.142	-0.732***	-0.699***	−0.129**
	(0.102)	(0.110)	(0.198)	(0.108)	(0.057)
lockdown #2 (2020-10-22)	0.088	-0.034	-0.397**	-0.413***	−0.221***
	(0.087)	(0.110)	(0.177)	(0.071)	(0.047)
partial re-opening (2020-12-10)	-1.663***	-0.145	0.661***	0.242**	−0.250***
	(0.152)	(0.212)	(0.185)	(0.111)	(0.073)
lockdown #3 (2020-12-27)	-0.735***	-0.006	-0.425**	0.269***	−0.119**
	(0.131)	(0.155)	(0.170)	(0.086)	(0.060)

Note: Logistic binary correlated random effect model - effect of selected important events on probability of price change during 7 and 14 days following the given event; dependent variable: = 0 if $p_{i,t} = p_{i,t-1}$ and 1 otherwise - excluding price rises and falls exactly reversed within 90 days; 4 151 848 observations (unbalanced panel of 11 788 products from the period between January 2020 and April 2021); *p<0.1, **p<0.05, ***p<0.01

Table A13: Macroeconomic events (temporary price changes)

	Retailer A	Retailer B	Retailer C	Retailer D	All
7-day window					
ir increase(2020-02-06)	0.033	-	-	0.481***	−0.131***
	(0.058)	-	-	(0.138)	(0.051)
lockdown #1 (2020-03-12)	0.673***	-	-	−2.799***	0.085
	(0.061)	-	-	(0.259)	(0.058)
ir decrease (2020-03-17)	−1.227***	-	-	1.733***	−0.285***
	(0.087)	-	-	(0.123)	(0.070)
ir decrease (2020-03-27)	0.317***	-	-	−1.058***	−0.019
	(0.072)	-	-	(0.186)	(0.065)
ir decrease(2020-05-11)	0.850***	−0.180**	0.272	−0.669***	0.227***
	(0.068)	(0.076)	(0.176)	(0.127)	(0.044)
lockdown #2 (2020-10-22)	0.050	0.316***	0.450**	−0.181	0.130***
	(0.054)	(0.059)	(0.176)	(0.116)	(0.036)
partial re-opening (2020-12-10)	−0.720***	0.011	0.006	−0.152	−0.317***
	(0.064)	(0.071)	(0.188)	(0.137)	(0.043)
lockdown #3 (2020-12-27)	−0.438***	0.632***	−0.723***	−0.568***	−0.006
	(0.074)	(0.070)	(0.229)	(0.165)	(0.045)
14-day window					
ir increase(2020-02-06)	0.017	-	-	0.162	−0.174***
	(0.048)	-	-	(0.127)	(0.040)
lockdown #1 (2020-03-12)	0.687***	-	-	−0.070	0.447***
	(0.062)	-	-	(0.192)	(0.056)
ir decrease (2020-03-17)	−0.809***	-	-	0.594***	−0.416***
	(0.071)	-	-	(0.191)	(0.063)
ir decrease (2020-03-27)	0.080	-	-	−1.084***	−0.180***
	(0.063)	-	-	(0.126)	(0.052)
ir decrease(2020-05-11)	0.680***	−0.477***	0.478**	−0.933***	0.004
	(0.066)	(0.065)	(0.203)	(0.106)	(0.040)
lockdown #2 (2020-10-22)	0.223***	0.078*	0.436***	−0.034	0.127***
	(0.041)	(0.046)	(0.118)	(0.082)	(0.027)
partial re-opening (2020-12-10)	−2.160***	−0.555***	−0.173	−0.042	−1.043***
	(0.064)	(0.066)	(0.170)	(0.117)	(0.040))
lockdown #3 (2020-12-27)	−0.560***	0.174***	−0.521***	0.088	−0.103***
	(0.049)	(0.054)	(0.166)	(0.101)	(0.032)

Note: Logistic binary correlated random effect model - effect of selected important events on probability of temporary price change during 7 and 14 days following the given event; dependent variable: = 0 if $p_{i,t} = p_{i,t-1}$ and 1 otherwise - temporary price changes are defined as price rises and falls exactly reversed within 90 days; 4 151 848 observations (unbalanced panel of 11 788 products from the period between January 2020 and April 2021 for four retailers); *p<0.1, **p<0.05, ***p<0.01

IES Working Paper Series

2023

1. Josef Bajzik, Tomáš Havránek, Zuzana Iršová, Jiří Novák: *Are Estimates of the Impact of Shareholder Activism Published Selectively?*
2. Klára Kantová: *Ex-Prisoners and the Labour Market in the Czech Republic*
3. Theodor Petřík, Martin Plajner: *Concurrent Business and Distribution Strategy Planning Using Bayesian Networks*
4. Tijmen Tuinsma, Kristof De Witte, Petr Janský, Miroslav Palanský, Vitezslav Titld: *Effects of Corporate Transparency on Tax Avoidance: Evidence from Country-by-Country Reporting*
5. Zuzana Irsova, Pedro R. D. Bom, Tomas Havranek, Heiko Rachinger: *Spurious Precision in Meta-Analysis*
6. Vojtěch Mišák: *Does Heat Cause Homicides? A Meta-Analysis*
7. Fan Yang: *The Impact of Regulatory Change on Hedge Fund Performance*
8. Boris Fisera: *Distributional Effects of Exchange Rate Depreciations: Beggar-Thy-Neighbour or Beggar-Thyself?*
9. Salim Turdaliev: *Powering Up Cleaner Choices: A Study on the Heterogenous Effects of Social Norm-Based Electricity Pricing on Dirty Fuel Purchases*
10. Kseniya Bortnikova: *Beauty and Productivity in Academic Publishing*
11. Vladimír Benáček, Pavol Frič: *Ossified Democracy as an Economic Problem and Policies for Reclaiming its Performance*
12. Petr Janský, Miroslav Palanský, Jiří Skuhrovec: *Public Procurement and Tax Havens*
13. Katarzyna Bilicka, Evgeniya Dubinina, Petr Janský: *Fiscal Consequences of Corporate Tax Avoidance*
14. Evžen Kočenda, Shivendra Rai: *Drivers of Private Equity Activity across Europe: An East-West Comparison*
15. Adam Geršl, Barbara Livorová: *Does Monetary Policy Reinforce the Effects of Macroprudential Policy*
16. Tersoo David Iorngurum: *Method versus cross-country heterogeneity in the exchange rate pass-through*
17. T. D. Stanley, Hristos Doucouliagos, Tomas Havranek: *Meta-Analyses of Partial Correlations Are Biased: Detection and Solutions*
18. Samuel Fiifi Eshun, Evžen Kočenda: *Determinants of Financial Inclusion in Africa and OECD Countries*
19. Matej Opatrny, Tomas Havranek, Zuzana Irsova, Milan Scasny: *Publication Bias and Model Uncertainty in Measuring the Effect of Class Size on Achievement*
20. Soňa Sivá: *Effects of Government Interventions on Bank Performance*
21. Oleg Alekseev, Karel Janda, Mathieu Petit, David Zilberman: *Impact of Raw Material Price Volatility on Returns in Electric Vehicles Supply Chain*

22. Karel Janda, Barbora Schererova, Jan Sila, David Zilberman: *Graph Theory Approach to Prices Transmission in the Network of Commonly Used Liquid Fuels*
23. Yermone Sargsyan, Salim Turdaliev, Silvester van Koten: *The Heterogeneous Effects of Social Cues on Day Time and Night Time Electricity Usage, and Appliance Purchase: Evidence from a Field Experiment in Armenia*
24. Jan Sila, Evzen Kocenda, Ladislav Kristoufek, Jiri Kukacka: *Good vs. Bad Volatility in Major Cryptocurrencies: The Dichotomy and Drivers of Connectedness*
25. Zuzana Irsova, Hristos Doucouliagos, Tomas Havranek, T. D. Stanley: *Meta-Analysis of Social Science Research: A Practitioner's Guide*
26. Diana Kmetkova, Milan Scasny, Iva Zverinova, Vojtech Maca: *Exploring the Link Between Diet and Sustainability in Europe: A Focus on Meat and Fish Consumption*
27. Fisnik Bajrami: *The Impact of Dollarisation on Economic Growth, Investment, and Trade*
28. Miroslav Svoboda, Michael Fanta, Jan Mošovský: *Effectiveness of Car Scrappage Schemes: Comparative Analysis of European Countries*
29. Nicolas Fanta, Roman Horvath: *Artificial Intelligence and Central Bank Communication: The Case of the ECB*
30. Karel Janda, Jan Sila, David Zilberman: *Fueling Financial Stability: The Financial Impact of U.S. Renewable Fuel Standard*
31. Anna Pavlovova: *High-Frequency Groceries Prices: Evidence from Czechia*

All papers can be downloaded at: http://ies.fsv.cuni.cz

Univerzita Karlova v Praze, Fakulta sociálních věd
Institut ekonomických studií [UK FSV – IES] Praha 1, Opletalova 26
E-mail : ies@fsv.cuni.cz http://ies.fsv.cuni.cz