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Keywords: monetary non-neutrality, State-dependent pricing, identified credit and monetary policy shocks, price-gap proxy, scanner data, PPI microdata

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Measuring Price Selection in Microdata: It's Not There*

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October 2020

Abstract

We use microdata to estimate the strength of price selection – a key metric for the effect of monetary policy on the real economy. We find that price adjustment pressure at the product level does not significantly influence the probability of price adjustment in response to identified monetary and credit shocks, suggesting price selection is absent. This happens even though prices do respond significantly both to aggregate shocks and product-level adjustment pressure directly. Our results are broadly consistent with second-generation state-dependent pricing models and sizable effects of monetary policy on the real economy.

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*All opinions expressed are personal and do not necessarily represent the view of the European Central Bank or the Eurosystem, the Federal Reserve Bank of Cleveland or the Federal Reserve Board System. Part of this research was conducted with restricted access to the Bureau of Labor Statistics (BLS) data. The views expressed here are those of the authors and do not necessarily reflect the views of the BLS. We thank Luca Dedola, Anton Nakov and participants at a seminar at the ECB for their comments and Lydia Cox for excellent research assistance.

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1 Introduction

One of the big questions in macroeconomics is whether monetary policy can have any effect on real economic outcomes, and stabilize business cycles. The ability of policymakers to do so is crucially tied to how we model the reaction of firms to shocks when they choose their prices. Some models imply that firms ignore adjustment pressure at the level of the product when deciding whether to adjust their prices in response to a policy shock (Calvo, 1983). In such a case, the aggregate price level is sluggish and monetary policy can have large real effects. Other models imply that firms respond to the shock if they have accumulated large product-level pressure to adjust prices (Golosov and Lucas, 2007). In that case, the aggregate shock and product-level pressures do interact, the price level becomes more flexible, and monetary policy may have less to even no real effects. What economists call "selection" measures the extent to which we live more in the second or more in the first model world. Our paper measures the extent of selection in the data.

Our main, empirical, and model-free finding is that selection appears to be absent in the data. We arrive at this result by analyzing price responses to an identified aggregate shock. Even though prices do significantly respond to the shock directly, their response is not a function of their product-level adjustment pressure, which selection would require. Instead, aggregate price adjustment is achieved predominantly through the gross extensive margin – how often prices increase versus decrease, not unlike in the popular Calvo (1983) model. These results imply that price adjustment in macroeconomic models generally should feature weak selection and a lot of "Calvoness," implying sizable monetary non-neutrality. Our findings do not mean that state-dependent pricing models – where selection is often discussed – are inconsistent with our results. On the contrary, for example, random menu cost models featuring weak selection (such as Dotsey et al., 1999; Luo and Villar, 2017; Alvarez et al., 2020) are consistent with our findings.

We measure selection by considering how the probability of price adjustment depends on an interaction term, which has two parts. The first part is given by the aggregate shock which hits the economy and exerts a general adjustment force. The second part is given by the product-level adjustment pressure which is already there because some prices have drifted far away from their optimal levels. A non-zero interaction term implies that prices of goods far from their optimum are more likely to adjust when a shock hits the economy. Selection is present: Prices are not only more likely to adjust in response to a shock, but they do so disproportionately strongly to make up for any already existing product-level gap between optimal and actual prices. As a result, the aggregate price level becomes disproportionately flexible. If the prices of goods with a large gap to the optimal price do not react in such a way to a shock, then selection is absent.

The empirical implementation of our model-free approach to measure selection is straight-forward: We use micro price data and regression analysis to assess how the probability of price adjustment statistically depends on an identified aggregate shock interacted with a measure of the product-level gap between optimal and actual prices. Our baseline aggregate shock is a credit shock, but our results are also robust to monetary policy shocks. We calculate proxies for the price gap using the information on the price-setting of close substitutes (as we explain below). We approach our question in a linear-probability panel-regression framework. Our dependent variables are the probability of price increases and price decreases, respectively, over the 24 months following a credit shock. We explain the probability of price changes with three explanatory variables of interest: the previous-period product-level price gap, the current-period aggregate shock, and their interaction. We satiate the regressions with a rich set of fixed effects, control for the age of prices, and cluster the standard errors across time and across product categories. The lagged price gap is predetermined, so its coefficient measures its causal impact on the probability of future price changes, and, by construction, it is also unaffected by the current aggregate shock.

On the data side, we construct our baseline measure of the price gap using a detailed, weekly panel of barcode-level prices in U.S. supermarkets between 2001 and 2012, compiled by the marketing company IRi.[1] In a complementary analysis, we also show that our results are robust to using the producer price microdata that underlies the U.S. producer-price index (PPI). The PPI data covers a much wider set of products than the scanner data, but it is also less granular. The data is available between 1981 and 2015. We identify aggregate credit shocks using timing restrictions as in Gilchrist and Zakrajšek (2012), and monetary policy shocks using high-frequency surprises in interest rates around policy announcements (Gertler and Karadi, 2015; Jarociński and Karadi, 2020).

Given the data, we construct our measures of the price gap. Our baseline measure calculates the distance of the price from the average of close competitors, after controlling for permanent differences in prices coming from variation in geography and amenities (Gagnon et al., 2012). This 'competitor-price gap' is a relevant measure of product-level price pressures as long as sellers strive to keep their prices close to competitors' prices to maintain profitability and market share. We show that our results are robust to using an alternative 'reset-price' gap measure. Here, we build on the approach proposed by Bils, Klenow and Malin (2012), who define reset prices as the unobserved optimal prices that a store would set if pricing frictions were temporarily removed. We follow the algorithm of Bils, Klenow and Malin (2012) to approximate these.

Our results are twofold: First, we find that our price gap measures predict the probability of future price adjustments, and the average size of the adjustment is proportional to the distance between optimal and actual prices. Furthermore, our aggregate shocks also significantly change the probability of price increases and decreases. These findings suggest that both the gaps are valid proxies of product-level price pressures, and the aggregate shocks are relevant. Second, however, we find no evidence of price selection, as the interaction term turns out to be insignificant in all of our regressions. These results hold both when we use competitor-price gaps and reset-price gaps, both when we use the credit shock and the monetary policy shock, in both our retailer- and producer-price microdata sets and both when we use linear and non-linear probability models. Selection is always absent.

Our results pose a challenge to conventional price-setting models. On the one hand, we find evidence of state-dependence in price setting: the farther a price is from its optimum the higher the chance that it adjusts. Furthermore, we find some evidence that aggregate shocks do change the aggregate frequency of price changes, not only the relative share of price increases and decreases. These results are inconsistent with standard time-dependent pricing models (Calvo, 1983). At the same time, we find no evidence that price gaps interact with the aggregate shocks: The selection effect is absent in the data differently from standard menu cost models (Golosov and Lucas, 2007; Karadi and Reiff, 2019). Second-generation menu cost models with random menu costs and small aggregate selection are broadly consistent with such evidence (Dotsey et al., 1999; Luo and Villar, 2017; Alvarez et al., 2020).

Related literature Our work contributes to the strand of literature that imposes minimal structure to estimate the strength of price selection in microdata. A subset of these papers relies on Caballero and Engel (2007), who derive an indirect measure of selection from the unobservable adjustment hazard and density

[1] We would like to thank IRI for making the data available. All estimates and analysis in this paper, based on data provided by IRI, are by the authors and not by IRI.

functions of price gaps. Berger and Vavra (2018) impose flexible functional forms and match unconditional moments of the price-change distribution to estimate hazard functions, finding a sizable selection (see also Petrella et al., 2019). Luo and Villar (2019) match moments conditional on trend inflation rates, and, in contrast, estimate hazard functions which imply weak selection, closer to our results. In contrast to these papers, we generate proxies for the price gaps and report non-parametric estimates of the hazard functions as Gagnon, López-Salido and Vincent (2012). Akin to Gagnon et al. (2012), we find that the probability of price change increases with the absolute value of the price gap, and we document that the relationship is significantly stronger when one controls for unobserved heterogeneity and allows longer adjustment lags. Furthermore, as a novel contribution, we show state-dependence does not imply active selection because the gap does not significantly interact with identified aggregate shocks.

Our paper is complementary also to the work of Carvalho and Kryvtsov (2018), which finds no indication for price selection in US supermarket data, in line with our results. They compare (i) the preset prices of products that eventually get changed to (ii) the average price of close substitutes, and show that this preset-price-relative does not interact with aggregate inflation in a time-series setting. We, instead, construct price gap measures for both adjusted and unadjusted prices and show in a panel-data setting that the price gaps do not interact with identified aggregate shocks (rather than the aggregate inflation rate). We show that our results are robust to using alternative gap measures, in both supermarket and producer-price data and a wide set of empirical specifications. Our work is also complementary to Dedola et al. (2019), who document the presence of a small, but statistically significant selection bias estimating a Heckman-type model using Danish producer-price data. The selection bias is indicative of state-dependence on the unobserved idiosyncratic shocks. We instead, measure (and reject the presence of) selection caused by the interaction of aggregate shocks with observable proxies for predetermined price gaps.

Our work is also related to the long strand of literature that uses observations about micro-level price behavior to infer the level of monetary non-neutrality in fully specified state-dependent price-setting models (Dotsey, King and Wolman, 1999; Golosov and Lucas, 2007; Gertler and Leahy, 2008; Midrigan, 2011; Alvarez, Bihan and Lippi, 2014; Alvarez, Lippi and Oskolkov, 2020). These papers predominantly use unconditional moments of price changes to calibrate their parameters and deliver predictions that are model-dependent. In contrast, we measure price-level adjustments conditional on identified aggregate shocks, and our predictions can be consistent with multiple theoretical models.[2] The strength of monetary non-neutrality in these models depends predominantly on the strength of price selection. Our results favor theoretical models, which find a minimal role for price selection, therefore predict sizable monetary non-neutrality.

The paper is structured as follows. We describe the data in Section 2, and then explain the construction and key features of our baseline price-gap proxy in Section 3. In Section 4, we characterize the dynamic response to aggregate credit shocks, and turn to the evidence on price selection in Section 5. We show that our results a robust using a non-linear specification in Section 6.1, using an alternative reset-price gap measure in Section 6.2, using producer-price microdata in Section 6.3, using monetary policy shocks in Section 6.4, and across heterogeneous product categories in Section 6.5. Section 7 discusses some implications of the empirical results and Section 8 concludes.

[2] Hong, Klepaczy, Pasten and Schoenle (2019) study the informativeness of pricing moments jointly with aggregate price responses to monetary policy shocks, indirectly testing the importance of selection through moments such as kurtosis. Balleer and Zorn (2019) document a small frequency response – similarly to us – and a *decline* in the average absolute size of price changes after an identified monetary policy easing using the German PPI microdata, arguing that these conditional moments are inconsistent with a large selection.

2 Data

We use 2 datasets in our analysis. Our main dataset is a U.S. supermarket scanner dataset. Our secondary dataset is the micro price data underlying the calculation of the U.S. producer-price index at the Bureau of Labor Statistics.

Our first dataset is collected by marketing company IRI (Bronnenberg, Kruger and Mela, 2008), and includes weekly data at the barcode level for 31 food- and health-care product categories (for example, carbonated beverages) in major supermarkets across 50 U.S. metropolitan areas over the sample 2001-2012. The data records total sales as well as quantities. It covers around 15 percent of the consumer expenditure survey.[3]

Given total sale revenues and quantities, we calculate posted prices in the IRi data as $P_{psw} = \frac{TR_{psw}}{Q_{psw}}$, where TR is the total revenue and Q is the quantity sold for each product p in store s in week w. We conduct some straightforward data cleaning. First, we round prices toward the nearest penny, as fractional prices reflect the impact of promotional sales during the week, not actual posted prices.[4] Second, the product identification numbers of *private-label* products are not unique throughout our sample. In particular, the identification number changes for a subset of products in January 2007, January 2008, and January 2012. When constructing price spells, we follow a conservative approach and assume that all private-label goods were replaced with new goods on these dates. We disregard the price- and expenditure changes for these goods during the three dates. Third, in each year, we only include products that are available over the whole year. This way, we exclude entering and exiting products, which might exhibit idiosyncratic pricing behavior, for example, motivated by learning about the products' demand function at introduction (Argente and Yeh, 2020) or during a clearance sale at exit. These idiosyncratic factors would further reduce selection, so excluding these prices is a conservative choice, which raises the probability of us finding selection in the data. This step makes us drop 17% of the products (17.6% of annual expenditure).

We use the modal-price filter of (Kehoe and Midrigan, 2014) to construct a weekly series of reference prices (P^f_{psw}), separating it from the impact of temporary sales. In our subsequent analysis, we concentrate on reference prices but show that our results are robust to the inclusion of sales data. To construct reference prices, we start from a running 13-week two-sided modal price. Then, we iteratively update it to align the time of reference-price change with that of the actual-price change.[5]

We construct monthly observations from our weekly dataset. This helps us concentrate on lower frequency developments in prices, which are more relevant for business cycle fluctuations. The monthly price P_{pst} is defined as the mode of the weekly prices over a calendar month; the highest of them if there are multiple modes. Picking the mode makes sure we do not create artificial prices and price changes, as could happen if we used averages. We calculate monthly expenditure as the sum of weekly sales.

To cross check the external validity of our scanner data, we calculate an expenditure weighted supermarket price index. Our aim is to create a chained-weighted index similarly to the consumer price index, but utilize the wider and deeper information available in our dataset. In particular, we use annual revenue weights $\omega_{psy} = TR_{psy}/\sum_p \sum_s TR_{psy}$, where subscript y reflects the year of the observation month. We measure inflation for posted- and reference prices ($i = p, f$) as $\pi^i_t = \sum_s \sum_p \omega_{pst} \left(p^i_{pst} - p^i_{pst-1}\right)$, where $p^i_{pst} = \log P^i_{pst}$.

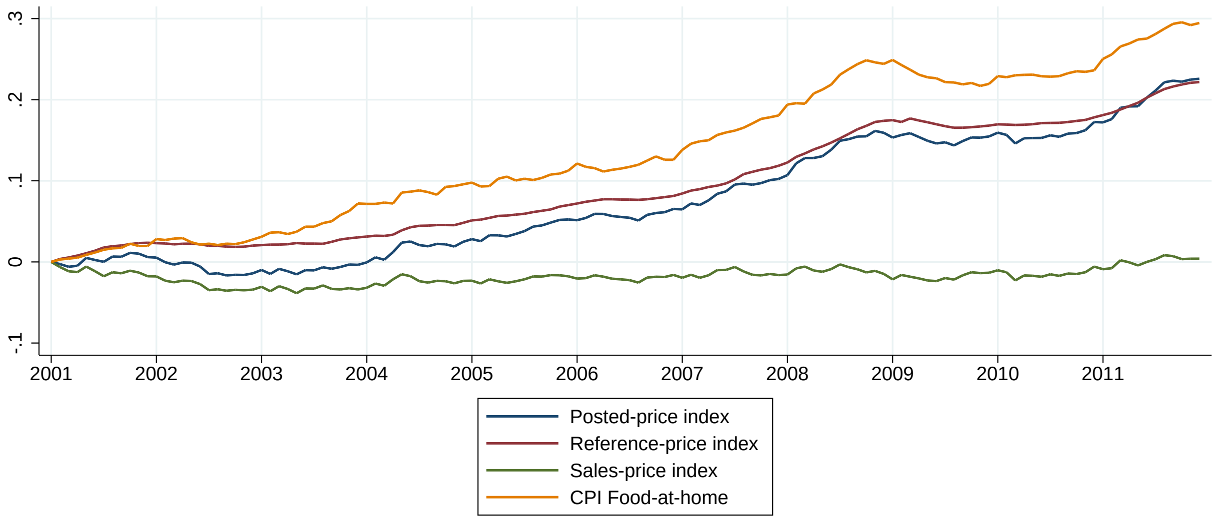
[3]The number of observations in our sample is almost 2.65 billion, it covers over 168 thousand unique products in 3187 unique stores in 169 supermarket chains.

[4]The rounding influences 8.7 percent of the prices in our sample.

[5]Details of the algorithm are described in the Online Appendix.

We define sales-price inflation as the difference between posted- and reference-price inflation $\pi_t^s = \pi_t^p - \pi_t^f$. We seasonally adjust the series using monthly dummies.

Figure 1: Posted, reference and the sales price index



Note: The figure depicts the evolution of posted-, reference- and sales-price indexes over our sample period, and compares them to the food-at-home subindex of the CPI. The reference-price index, which is used in the preceding analysis, tracks the food-at-home index closely at business cycle frequencies.

Figure 1 shows the evolution of the logarithm of our posted-, reference- and sales-price indices ($p_t^i = \log P_t^i, i = p, f, s$) and compares them to the evolution of the food-at-home consumer-price subindex published by the Bureau of Labor Statistics. The (log) indices are normalized to 0 in January 2001 and are constructed as cumulative sums of the inflation rates: $p_t^i = \sum_{j=0}^{t} \pi_j^i$. We see that our price index somewhat underestimates the overall food-at-home price-level increase over the period.[6] The main reason is that our index relies on the price-development of existing products and – differently from the CPI – it ignores the endogenous replacement of old, exiting products with new, entering products, which tend to be more expensive and higher quality. What is more important for our purposes, however, the fluctuations of the price index calculated from our sample at business cycle frequencies track those of the food-at-home index closely.

Our second main dataset is the micro price dataset underlying the construction of the U.S. PPI. These data have been described in detail in several papers that analyze the PPI microdata, such as Nakamura and Steinsson (2008), Goldberg and Hellerstein (2009), Bhattarai and Schoenle (2014), Gorodnichenko and Weber (2016) or Gilchrist et al. (2017). We refer the reader for details of the PPI data to these papers while briefly summarizing its key features in the following paragraph. The PPI data also adds several appealing features to our analysis which we discuss below as well.

The PPI data is a monthly dataset of transaction-based prices. Goods in manufacturing and services are the focus of the PPI data. The dataset contains prices for approximately 28,000 firms and more than 100,000 goods every month, belonging to 540 six-digit NAICS product categories. These NAICS categories

[6] The annualized inflation in posted prices in 1.84%, and in reference prices it is 1.75%. The CPI food-at-home inflation over the same period is 2.7%.

are very narrow categories such as Tortilla Manufacturing (NAICS code 311830). Within this setting, goods produced by each firm are uniquely identified according to their "price-determining" characteristics. These characteristics include the type of buyer, the type of market transaction, the method of shipment, the size and units of shipment, the freight type, and the day of the month of the transaction. Goods remain in the data on average for 70 months while firms exist for approximately 7 years. Sales are very rare in PPI data, as documented by Nakamura and Steinsson (2008) so we work with unfiltered data. However, we check for the importance of product substitutions. Whenever we identify a rare product substitution through a so-called base price change, we assume a price has changed.

The particular appeal of the PPI microdata lies in the following for our analysis: First, the data aim to map out the "entire marketed output of U.S. producers" in a representative fashion (BLS, 2020). Such producer data rather than retail data is particularly suitable for studying how all kinds of firms set prices and how they react to aggregate shocks. Second, the data range from 1981 to 2015 providing a long time series that spans multiple business cycles and provides variation in monetary policy and credit frictions. Third, the producer price data cover a wide range of sectors in manufacturing but also three-quarters of services in the U.S. economy, lending more general validity to our analysis.

3 A price-gap proxy

Our main goal is to measure selection. Our approach to doing so is to assess the interaction of aggregate shocks with product-level motives of price adjustment. This section describes how we measure these idiosyncratic motives – with a proxy of the price gap $x_t = p_t - p_t^*$, which is the distance of the price from its unobserved optimal level. In a wide class of state-dependent models, the price gap is indeed the relevant idiosyncratic state variable (Golosov and Lucas, 2007; Alvarez et al., 2014). The further a price is from its optimum, the higher are the incentives to adjust it.

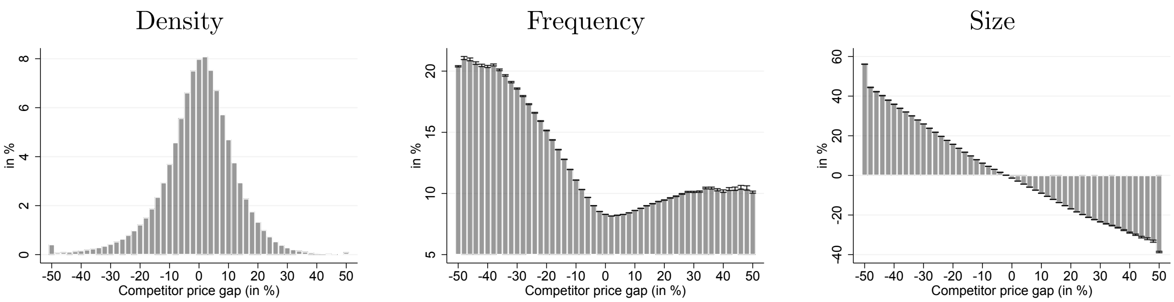
Our baseline price-gap proxy is the competitor-price gap, which measures the distance of the price from a suitably adjusted average price of close competitors. In Section 6.2, we show that our results are robust to using an alternative, the reset-price gap, which measures the distance of a price from its reset-price (Bils et al., 2012). We first describe the details of the construction of the competitors' price gap. The subsequent section then describes the properties of our main identified shock before finally we are ready to discuss selection, the interaction of gaps, and the shock. A reader mainly interested in selection may directly skip ahead to Section 5.

3.1 Competitor-price gap

One of the primary concerns of firms in their price-setting decisions is figuring out how far the price of an item is from the average price of the competitors. Our data allows us to answer this question at a barcode-level granularity after controlling for temporary sales and store (therefore also location) fixed effects. The fixed effects help control for permanent differences between the price-level of stores as a result of differences in demand conditions or costs.

Formally, we formulate the competitor price gap for product p in store s in month t as $\tilde{x}_{pst} = p^f_{pst} - \bar{p}^{*f}_{pt}$, where p^f_{pst} is the logarithm of the reference price and $\bar{p}_{pt}$ is the average reference-price of the same product across stores. We deal with the persistent heterogeneity across stores (i.e. chains, locations) by estimating the fixed effects of a store in a regression $\tilde{x}_{pst} = \alpha_s$, and reformulate the price gap as $x_{pst} = \tilde{x}_{pst} - \alpha_s$.

Figure 2: Competitor-price gap density, and the frequency and size of subsequent reference price changes as a function of the gap



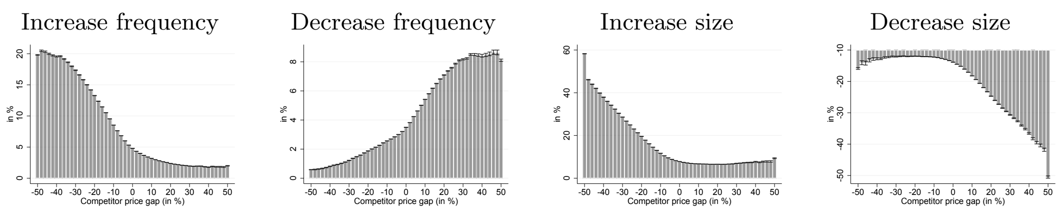
Note: The panels show the unconditional density and frequency- and size responses of subsequent price change in the baseline supermarket dataset. The density of the competitor-price gaps have fat tails (first panel); the frequency of subsequent price adjustment increases with the absolute size of the gap, but the probability of adjustment is higher for negative gaps than for positive gaps, the probability of adjustment is positive at 0 gap, and stays below 20% even for large (over .5 log points) gaps (second panel), and the size of average subsequent adjustments are close to a (minus) one-on-one relationship with the gap (third panel).

Figure 2 shows the density of the competitor-price gap x_{pst} in our baseline IRi supermarket dataset pooled across products and time, and the probability and the size of price adjustment as a function of the price-gap. The density shows that there is a sizable dispersion of price gaps even after we control for temporary sales and permanent differences in the price level of stores.

The second panel of Figure 2 shows the probability of price changes as a function of our measure of the price gap, the price-adjustment hazard. The probability is increasing with the distance away from zero in a V-shaped pattern that is familiar with state-dependent pricing models. This suggests that our proxy indeed captures an important component of the unobserved theoretical price gap (as argued also by Gagnon et al., 2012). Notably, the probability of price change is positive even at a zero lagged price gap, and it does not go higher than 20 percent even at gaps as low as -50 percent. In Section 6.1 we argue that when one controls for cross-sectional heterogeneity and allows for a gradual price adjustment, the estimated slope of the hazard function increases substantially. Furthermore, the probability is asymmetric: the probability of a change for negative gaps is higher than the probability of a change for positive gaps. All the three features (positive hazard at 0, lower than 1 at high gaps, and asymmetry) are in line with the results of Luo and Villar (2019), who estimate flexible hazard functions by matching price-setting moments of U.S. consumer-price inflation. The first two panels of Figure 3 separates the probability of price increases and those of price decreases. The relationship with the gap measures is clearly present in both cases and economically intuitive.

The third panel of Figure 2 shows that the average size of reference price adjustments conditional on the lagged competitors' price gap. The figure shows that there is a close to a one-to-one relationship between the lagged price gap and the inverse of the reference price change. This confirms that distance from competitors' prices indeed proxies an important feature of the theoretical price gap, and actual reference price changes aim at closing this gap, on average. The last two panels of Figure 3 separate the positive and the negative price changes and confirm that for negative price gaps the average size of a price increase is in a tight, close to a linear negative relationship with the gap, and conversely for positive gaps the average size of a decrease is in a close to linear negative relationship with the gaps. The figures also reveal that the competitors' price gap is not the only reason firms change their prices, as we observe price increases for positive gaps and price decreases for negative gaps, though their size seems to be mostly unrelated to the price gaps.

Figure 3: Size and frequency as functions of price gap



Note: The panels shows the subsequent increase and decrease probabilities, and increase and decrease sizes of price changes as a function of competitor-price gaps. The panels are consistent with the view that negative gaps play a primary role in determining the frequency and size of price increases and, vice versa, positive gaps play a primary role in the frequency and size of price decreases. The non-diminishing role of increase (decrease) frequencies at positive (negative) gaps suggest that the competitor-price gap is one, but not the only determinant of price changes. The average size of price changes is mostly unrelated to the gaps in these regions, conforming with this view.

4 Dynamic impact of credit shocks

This section discusses the properties of our baseline identified shock, the aggregate credit shock. We present the response to a monetary policy shock in our robustness section. A complementary purpose of this section is to establish that in response to the shock, reference prices adjust primarily through the extensive margin – whether prices adjust or not – which will hence be our subsequent focus.

Our analysis measures financial conditions using the excess bond premium measure, which is a time series of corporate bond spreads purged from the impact of firms' idiosyncratic default probabilities (Gilchrist and Zakrajšek, 2012). We identify credit shocks with standard exclusion restrictions. To establish the dynamic properties of the credit shock in the economy, we run some simple regressions. We implement the restrictions in a local projection framework by including the contemporaneous values of the excluded variables as controls (Plagborg-Møller and Wolf, 2019).

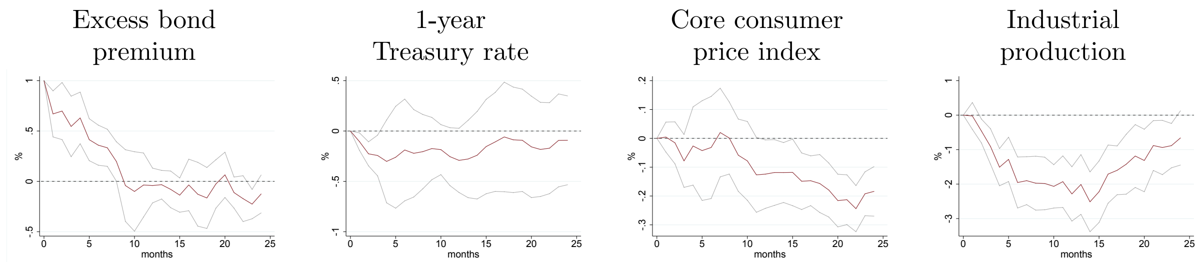
In particular, we run a series ($h = 0, \ldots, 24$ months) of regressions of the form:

$$x_{t+h} - x_t = \alpha_h + \beta_h \text{ebp}_t + \Psi_h X_t + \Gamma_h \Phi(L) X_{t-1} + u_{t,h}, \tag{1}$$

where ebp_t denotes the excess bond premium. Besides the 1 to 12 months lags of the 1-year Treasury rate, the core consumer price index, industrial production and the excess bond premium, the controls X_t include the contemporaneous values of the 1-year Treasury rate, the core consumer price index and industrial production.

The key object of interest is the coefficient β_h on the credit shock. We plot it for all variables of interest for $h = 0, 1, \ldots, 24$ along with 95% confidence bands using the Newey and West (1987) heteroscedasticity and autocorrelation-consistent standard errors (Stock and Watson, 2018). Figure 4 shows the impulse responses over the sample from 2001 to 2012. The figures show that even though the credit shock is accompanied by a quick monetary policy easing, it is associated with a sizable decline in industrial production and the core CPI.

Figure 4: Impulse responses of key macroeconomic variables to a credit shock, 2001-2012



Note: The panels show impulse responses to an identified credit shock over the sample 2001-2012 in a local-projection framework, and 95% confidence bands using Newey-West standard errors. The figures show that the credit tightening causes a sizable drop in activity and the price index despite sizable monetary policy easing.

Key to our subsequent main analysis, we next show that the adjustment of the reference-price level happens primarily through the extensive margin, by modifying the number of price increases and decreases. The adjustment does not take place through the intensive margin, by changing the average size of price increases and decreases. This result justifies us concentrating on the probability of price adjustment in our analysis below. To establish the result, we first decompose the cumulative reference-price inflation between months $t-1$ and $t+h$ into the frequency ($\xi_{t,t+h}$) and the size ($\psi_{t,t+h}$) of price increases and price decreases as follows:

$$p_{t+h} - p_{t-1} = \pi_{t,t+h} = \xi^{+}_{t,t+h}\psi^{+}_{t,t+h} + \xi^{-}_{t,t+h}\psi^{-}_{t,t+h}. \tag{2}$$

Here, the cumulative frequencies of reference-price[7] increases and decreases between months $t-1$ and $t+h$ are defined as

$$\xi^{\pm}_{t,t+h} = \sum_i \bar{\omega}_{it,t+h} I^{\pm}_{it,t+h}, \tag{3}$$

where $I^{+}_{it,t+h}$ and $I^{-}_{it,t+h}$ are indicators that take the value 1 if the reference price of item i (a product in a particular store) increased or decreased, respectively, and 0 otherwise. The weight $\bar{\omega}_{it,t+h}$ is measured as the average weight of the product between t and $t+h$. The average cumulative size of price increases and decreases are defined as

$$\psi^{\pm}_{t,t+h} = \frac{\sum_i \bar{\omega}_{it,t+h} I^{\pm}_{it,t+h}(p_{it+h} - p_{it-1})}{\xi^{\pm}_{t,t+h}}. \tag{4}$$

Table 1 lists some of these moments for our supermarket prices. We can observe around 1.8 percent inflation in our sample. The inflation rate in reference prices is very close to that of the posted prices with 1.75% versus 1.84% while average sales price inflation is only 0.05 percent and not shown. Posted prices change very frequently: every month more than one-third of them change. But the majority of price changes are temporary and only 11 percent of reference prices change each month. Out of these reference-price changes, 6.6 percent are increases and 4.2 percent are decreases. Price changes are large when they occur: the average increases are 12.5 percent while the average decreases are 15.1 percent.

[7] We suppress the superscript f for notational convenience.

Table 1: Average moments

Annualized inflation		Frequency		Reference frequency		Reference size	
Posted	Reference	Posted	Reference	Increase	Decrease	Increase	Decrease
1.84%	1.75%	36.2%	10.8%	6.6%	4.2%	12.5%	-15.1%

Note: The table lists some relevant moments of posted and reference prices. It confirms that posted price changes are very frequent, but they are mostly driven by temporary sales. The probability of reference price changes in a month is around 11%. The size of non-zero reference price change is large, over 10%.

Regression of these three cumulative reference-price outcome[8] measures – cumulative inflation, frequency and size – shows the importance of the extensive margin. Figure 5 summarizes the results from the regression of these 3 outcome variables on the credit shock.[9] We find the following results:

First, as the first panel in the first row of Figure 5 shows, there is a persistent decline in the price level after a temporary and insignificant increase in the price level. This decline is primarily driven by the significant decline in the price-increase frequencies and the increase in the price-decrease frequencies.

Second, and most importantly, we find that there is a strong adjustment on the gross extensive margin within a year of the shock: The cumulative frequency of reference-price increases declines (second panel in the second row), and the cumulative frequency of price decreases rises (second panel in the third row).[10] The decline in the cumulative price increases is not much larger than the increase in cumulative price decreases, so the aggregate frequency declines, but only marginally significantly if so at all. Both of these changes contribute to the decline in the price level, and to the reduction in the average size of price changes (third panel in the first row). The average size of price increases stays unchanged and the absolute size of the price decreases increases only marginally. This finding motivates our choice to concentrate on the frequency of reference-price changes in the subsequent analysis.

[8] We concentrate on reference-prices, because although sales-price inflation responds significantly to the credit shock in our baseline regression (not shown), it is only a feature of the large credit shock of the Great Recession, and it disappears if this period is excluded from the analysis (or only if monetary policy shocks are analysed, which are smaller on average).

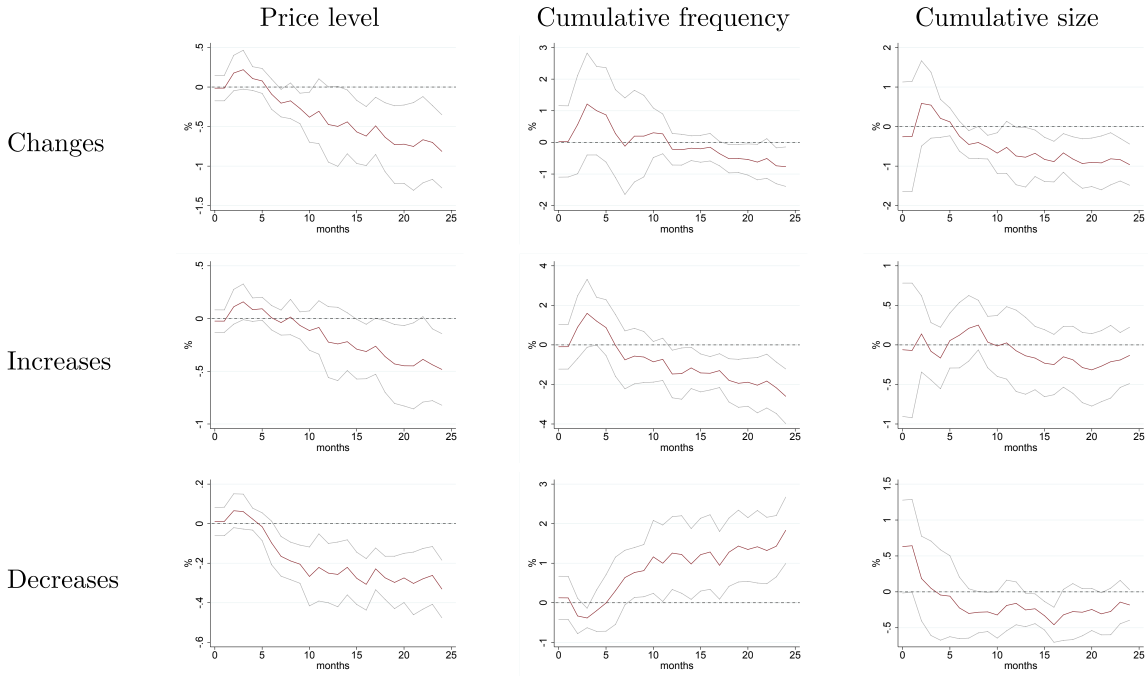
[9] The local-projection analysis controls for a horizon-dependent constant. Therefore, it effectively assesses the impact of the monetary policy shock on the deviation of the dependent variables from their steady state value. The decomposition of the deviation of cumulative inflation from its steady state becomes

$$\pi_{t,t+h} - \bar{\pi}_h = \left(\xi^+_{t,t+h} - \bar{\xi}^+_h\right)\bar{\psi}^+_h + \left(\psi^+_{t,t+h} - \bar{\psi}^+_h\right)\bar{\xi}^+_h + \left(\psi^+_{t,t+h} - \bar{\psi}^+_h\right)\left(\xi^+_{t,t+h} - \bar{\xi}^+_h\right) + \left(\xi^-_{t,t+h} - \bar{\xi}^-_h\right)\bar{\psi}^-_h + \left(\psi^-_{t,t+h} - \bar{\psi}^-_h\right)\bar{\xi}^-_h + \left(\psi^-_{t,t+h} - \bar{\psi}^-_h\right)\left(\xi^-_{t,t+h} - \bar{\xi}^-_h\right).$$

.

[10] Notably, this adjustment in the gross extensive margin (and the accompanying change in the average size of price changes) is not inconsistent with underlying assumptions in time-dependent models (Calvo, 1983), which assume a constant aggregate frequency of price changes and attribute the adjustment after aggregate demand shocks to the intensive margin.

Figure 5: Adjustment on the extensive and intensive margins to a credit shock



Note: The figure shows the impulse responses of supermarket reference-price levels and their components to an identified credit shock, and 95% confidence bands using Newey-West standard errors. The panels illustrate how the negative price-level response (first figure in the first row) is predominantly driven by the decline in the price-increase frequency (second panel in the second row) and the increase in the price-decrease frequency (second panel in the third row), while the sizes of price increases and decreases stay mostly constant (third panels in the second and third rows). This adjustment in the gross extensive margin explains the decline in the cumulative size of the price changes (third panel in the first row). The cumulative frequency (net extensive margin) declines, but only marginally significantly (second panel in the first row).

5 Selection

This section measures the strength of the selection effect. Our main finding is that selection appears to be absent in the data – while both the price gap and the aggregate shocks do have direct effects on price-setting.

To arrive at these results, we estimate the extent to which idiosyncratic price adjustment pressures interact with the credit shock. We approach this estimation in a linear-probability panel-data setting using a random 10% subsample of our IRi supermarket dataset.[11] We control for heterogeneity among items using product-store fixed effects and use two-way clustering across product-category and time. Our main object of interest is the impact of the interaction of the aggregate shock and the price-gap measures on the probability of price adjustment.

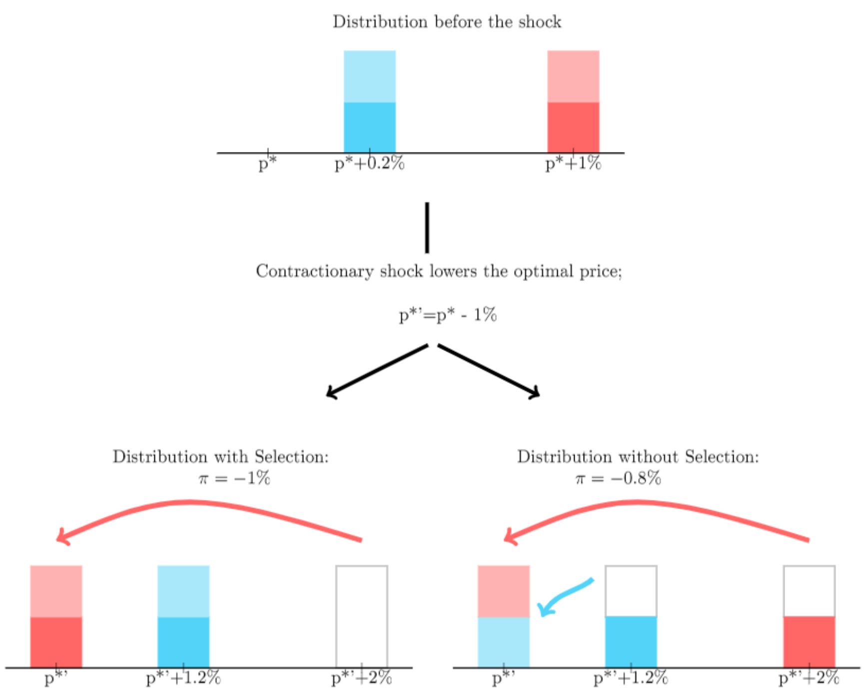
We preface our empirical analysis with a detailed explanation of selection. A reader less interested in the technical details of selection may skip right away to its measurement in the data in Section 5.2.

[11] We limit the size of our IRi sample because of computational constraints. At the 10 percent level, the estimates are invariant to further sample increases. We subsequently show the robustness of our results in the full PPI sample, to monetary policy shocks, using alternative price gap proxies and non-linear probability models.

5.1 What is selection?

As is well known for the class of state-dependent pricing models, *which* prices change in response to a nominal shock can be as important for the real effects of the shock as *how many* prices adjust (Golosov and Lucas, 2007). In fact, if prices adjust that are far from their optimal levels, then nominal shocks and consequently monetary policy can be completely neutral even if only a small subset of prices adjust (Caplin and Spulber, 1987). Selection measures how far new adjusters are from their optimal levels when an aggregate shock hits. Figure 6 illustrates the idea of selection graphically, with a description in the text under the figure. The main text presents a mathematically more formal discussion of selection.

Figure 6: An illustrative example



Note: The figure illustrates the mechanism of price selection. Imagine two supermarkets, setting the prices of 4 distinct product types. Half of the prices are much higher (+1%) than their optimal price (p^*, price of a dominant competitor), and the other half are only slightly higher (+0.2%). Then, an aggregate shock – such as a uniform price cut by a dominant competitor – reduces the optimal prices of all goods by 1%. Changing prices is costly, so both supermarkets reset half of their prices, but they follow different price-setting rules: one with selection, another without selection. The one with selection (left panel) chooses to reset the prices that are furthest from their optimal prices. This rule generates an interaction between the aggregate shock and the product-level mispricing: The mispricing substantially amplifies the impact of the shock. Indeed, that supermarkets price index declines by the same amount as the size of the uniform shock even though only half of the products change prices. Such behavior is typical in state-dependent price-setting models, where selection is high, the aggregate price level is flexible and monetary policy is close to neutral. The other supermarket without selection (right panel) chooses to adjust its prices according to a predetermined rule that is independent of the mispricing. For example, it resets a certain number of prices in each aisle. Thus, it ends up picking half of the prices with smaller and half of the prices with larger mispricing. This generates a supermarket price index that is strictly smaller than the uniform shock. This behavior is representative of time-dependent price-setting models, with no selection, sluggish aggregate price level, and non-neutral monetary policy.

To more formally define selection, consider the useful accounting identity from Caballero and Engel (2007) and Costain and Nakov (2011). It decomposes the impact of a shock on inflation into an (i) intensive-margin (ii) an extensive-margin and (iii) a selection effect. For this decomposition, let x_t denote the price gap (difference of the price p_t from its optima p_t^*) and let $\mu_t(x_t) = -x_t$ be the desired price change (opposite of the price gap). Let $\lambda_t(x_t)$ be the adjustment hazard, which measures the probability of a price change as a function of the price gap x_t. Finally, let $f_t(x_t)$ be the distribution of the price gaps in period t.

Then, by construction,

$$\pi_t = \int \mu_t \lambda_t f_t dx = \bar{\mu}_t \bar{\lambda}_t + \int \mu_t \left(\lambda_t - \bar{\lambda}_t \right) f_t, \tag{5}$$

where $\bar{g}_t = \int g_t f_t dx_t$ for any function $g_t(x)$, and where we express the function $f_t(x_t)$ as f_t for notational simplicity. This first expression is very intuitive: Inflation is the product of the size of price adjustment and the probability of price adjustment at each price gap weighted by the mass at each price gap. The second expression decomposes the sum into the product of average size and average frequency and a covariance term between the distribution of the size and the probability of price adjustment. The covariance term plays a relevant role in state-dependent models, so we will treat indications of their presence as evidence for 'state-dependence'.

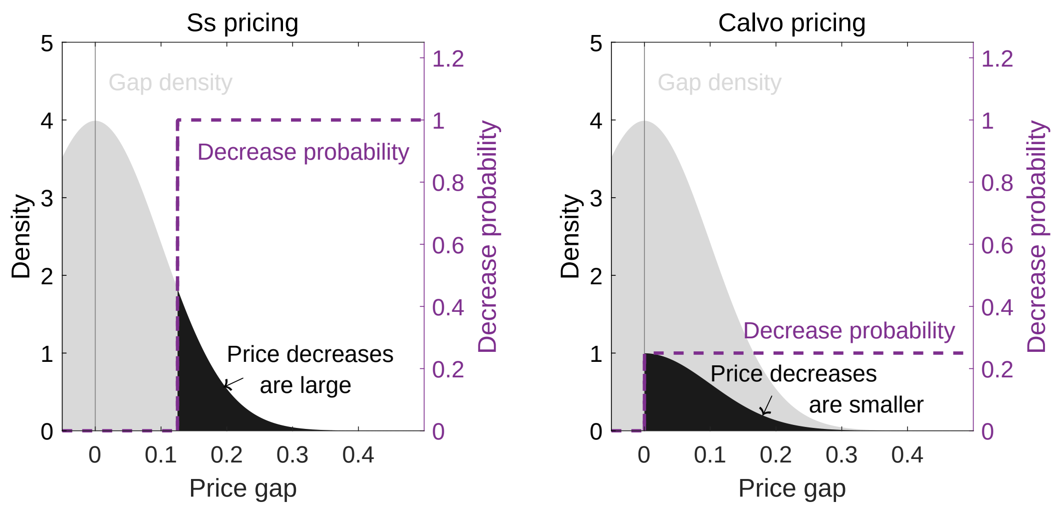
To illustrate this decomposition more plastically, it is instructive to contrast two pricing frameworks: a state-dependent menu-cost model such as Golosov and Lucas (2007) and the time-dependent model of Calvo (1983). First, we discuss the key features of pricing in these two models. Figure 7 focuses on the main objects of interest for the state-dependent-model (left panel) and for the time-dependent model (right panel). For simplicity, the figure concentrates on price decreases and positive price gaps, but the behavior of price increases and negative price gaps are entirely analogous.

The difference between the two models lies in the shape of the adjustment hazard ($\lambda^-(x)$): Firms in the state-dependent model follow an Ss-type pricing rule so they adjust their prices if prices are outside of their inaction band. Consequently, the price-decrease hazard in the state-dependent model is a step function that is zero for small gaps and one for gaps above the inaction threshold, as the left panel shows. By contrast, firms in the time-dependent model change their prices with a fixed probability independently of the price gap. Their hazard function is constant over the range of positive gaps, as the right panel shows. The density of price decreases (black shaded area) comes as the product of the adjustment hazard and the density of gaps ($f(x)$, grey shaded area).

The panels in Figure 7 illustrate the big contrast between the size distribution of price decreases in the two pricing frameworks: While price changes are on average large in the state-dependent model (left panel), they are on average small in the time-dependent model (right panel). The left panel also illustrates two general features of state-dependent models: The probability of price adjustment increases with the size of the price gap (non-decreasing hazard), and the size of price adjustments are generally large. In our subsequent empirical implementation, we concentrate on the first feature and measure the extent of state dependence with the tightness of the relationship between the probability of adjustment and the price gap. We next hammer out the meaning of selection in the context of these adjustment mechanisms both formally and graphically.

According to equation 5, the deviation of the inflation from its long-term average, $\Delta\pi_t = \pi_t - \pi$, condi-

Figure 7: State-dependence



Note: The figure shows the density of the before-shock price-gaps, the price-decrease hazard and the price-decrease density as a function of gaps in a state-dependent menu cost model (left panel) and a time-dependent model (right panel). The difference between the adjustment hazard functions in the models implies large difference in the predicted relationship between the gap and the probability of price decreases.

tional on an aggregate shock, η_t, can naturally be expressed as

$$\frac{\Delta\pi_t}{\eta_t} = \underbrace{\frac{\bar{\lambda}\Delta\bar{\mu}_t}{\eta_t}}_{\text{intensive}} + \underbrace{\frac{\mu\Delta\bar{\lambda}_t}{\eta_t}}_{\text{extensive}} + \underbrace{\frac{\Delta\int\mu_t\left(\lambda_t-\bar{\lambda}_t\right)f_t}{\eta_t}}_{\text{selection}} \tag{6}$$

where $\Delta g_t = g_t - g$ denotes the deviation of g_t from its long-term average value.

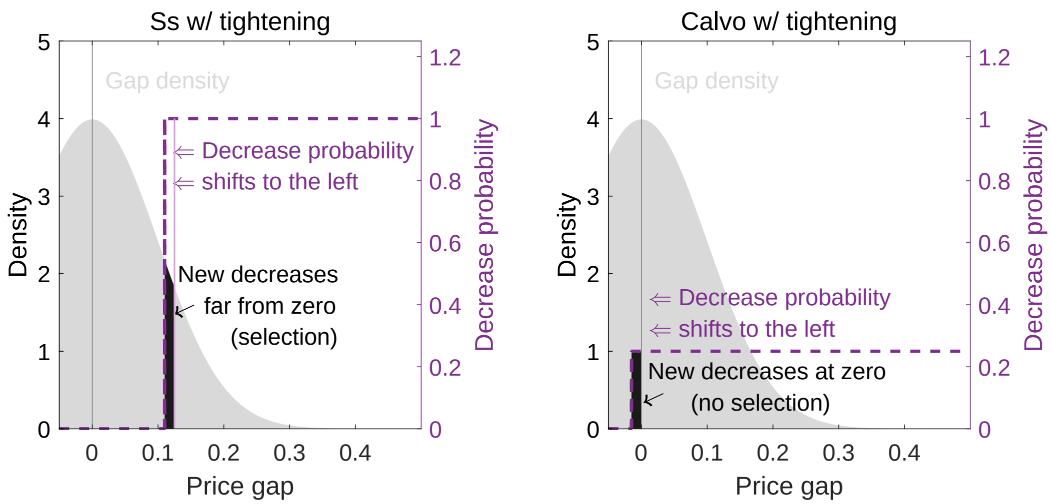
This expression decomposes the inflation effect into three terms. The first is the intensive-margin effect, which measures the impact in the change in the average size of price adjustment at the average steady-state frequency. The second is the extensive-margin effect, which measures the impact of the change in the average frequency of price changes at the average steady-state size. The third is the selection effect, which measures the impact of the change in the covariance between the size and the probability of price changes in the cross-section as a response to the aggregate shock. Importantly, it is not the *level* of the covariance, but its deviation from its long-term average as a *response* to the aggregate shock that matters for selection. In other words, what matters is not whether prices that are far from their optimal levels are *regularly* adjusted with higher probability, which we refer to as state-depencence; rather, what matters is: How large are the *new* price changes that are triggered or canceled (i.e. endogenously selected) by the aggregate shock?

Figure 8 illustrates this point. It depicts a policy tightening, which reduces the optimal price of each firm, raising their after-shock gap, or, equivalently, shifts the adjustment hazard to the left as a function of the before-shock gap, which we use on our horizontal axis.[12] The black shaded areas on the figures show the new adjusters. Their distribution is very different in the two pricing models. In the state-dependent model (left panel), the new adjusters have large gaps, therefore they are far from their optimal prices when the

[12]In this illustration, we treat gaps as the idiosyncratic state variable *before* the aggregate shock hits. This is consistent with our subsequent empirical implementation where we measure gaps as the *lagged* distance from the average prices of competitors.

aggregate shock hits. Consequently, when they decrease their prices, they will decrease it by a lot so as to release their accumulated price pressures. The idiosyncratic price pressures, therefore, amplify the impact of the aggregate shock and make the price level flexible: The selection effect is high. By contrast, in the time-dependent Calvo setup (right panel), the new price decreases are distributed above close-to-zero price gaps. Therefore these prices are very close to their optimal levels, so idiosyncratic pressures will not amplify the impact of the aggregate shock, in other words, selection is absent.

Figure 8: Selection



Note: The figure shows the density of the before-shock price-gaps, the price-decrease hazards and the distribution of new price changes as a function of gaps in a state-dependent menu cost model (left panel) and a time-dependent model (right panel). The figure illustrates a large difference between selection in the two frameworks: while new price decreases are far from their optima in the menu cost model implying large selection, they are close to their optima in the time-dependent model, implying no selection.

This definition of selection motivates our empirical specification, which we detail in the next section. The empirical framework aims at assessing the impact of the *interaction* of the aggregate shock and our price gap measure on the probability of price changes. In other words, it estimates whether prices that are further from their optima change with higher probability *when* the aggregate shock hits. As the price gaps are closely related to the size of price changes (see Figure 2), the estimates, therefore, are informative about the conditional covariance between the size and the probability of price changes as a response to the aggregate shock: The selection effect.

5.2 Empirical specification

Our baseline specification is a linear-probability panel-data model. It regresses the probability of price increases and decreases on the interaction of the aggregate shocks and the price gap proxy. We control for the impact of the price gap in normal times (by including the level of the gap directly) and for the impact of the shock on the average frequency (by including the aggregate shock directly), as well as time-dependence (by including the age of the price) and product-store fixed effects.

$$I^{\pm}_{pst,t+h} = \beta^{\pm}_{xih} x_{pst-1} \hat{\text{ebp}}_t + \beta^{\pm}_{xh} x_{pst-1} + \beta^{\pm}_{ih} \text{ebp}_t + \gamma^{\pm}_h T_{pst-1} + \Gamma^{\pm}_h \Phi(L) X_t + \alpha^{\pm}_{psh} + \alpha^{\pm}_{mh} + \varepsilon^{\pm}_{psth}, \tag{7}$$

where $I^{\pm}_{pst,t+h}$ is an indicator of a price increase or respectively a decrease of product p in store s between periods t and $t+h$. x_{pst-1} is the price gap proxy. ebp denotes the level of the excess bond premium as a measure of aggregate financial conditions. $\hat{\text{ebp}}_t$ is the aggregate credit shock, and we explain its construction below. T_{pst} denotes the logarithm of the age of a price, which we measure as the months since the last price change. Finally, $\Gamma^{\pm}_h \Phi(L) X_t$ are aggregate controls. As aggregate controls, we use the current period industrial production as a measure of economic activity, the core consumer price index (CPI) as a measure of the price level and the 1-year treasury yield as a monetary policy indicator. We allow for up to six lags of all aggregate variables.

To show the robustness of our analysis, we present two additional variants of our main specification. First, we analyze a specification with time-fixed effects, without the direct impact of the identified shock. Second, we analyze a specification with separate coefficients for positive and negative gaps.

We identify the credit shock by exclusion restrictions analogously to the local projections shown in section 4. We measure financial conditions by the excess bond premium (Gilchrist and Zakrajšek, 2012). The direct impact of the shock on the probability of price adjustment is measured by $\beta^{\pm}_{ih}$, as the regression controls for the impact of the current period activity, prices, and treasury yields. Plagborg-Møller and Wolf (2019) have shown that this is equivalent to an identification with Cholesky factorization ordering the credit shock last. To obtain a credit shock measure ($\hat{\text{ebp}}$) for the interaction term, we appropriately purge the excess bond premium. We regress it on the current month industrial production, the core consumer price level, and the 1-year treasury yield and on up to six lags of its own level and the other aggregate variables. Our shock measure is the residual of this regression. As before, this is equivalent to identifying the credit shock using a Cholesky factorization and ordering the credit shock last.[13].

All our specifications include the price gap that is lagged by one month $x_{pst-1} = p_{pst-1} - p^*_{pst-1}$. The advantage of the lagged measure is that it is predetermined, therefore unaffected by the contemporaneous aggregate shock $\hat{\text{ebp}}_t$. Their independence simplifies the interpretation of the empirical results. At the same time, the measure ignores the impact of contemporaneous idiosyncratic shocks, which, together with the contemporaneous aggregate shock, affect the contemporaneous optimal price (p^*_{pst}), which, in turn, determine the size of the price adjustment. Most of the variation in the contemporaneous gap, however, is explained by the lagged gap, and, furthermore, it is in an (inverse) one-to-one relationship with the actual size of the price change. Therefore, if selection were present, the price change probability should be significantly influenced by the interaction of the *lagged* gap and the contemporaneous aggregate shock, as in our specification. The impact of unobserved idiosyncratic shocks on price selection is the focus of the analysis of Dedola et al. (2019), who find some statistically significant, but economically small impact using Danish producer-price data.

Our specifications also control for the direct impact of the price gap β_{xh} on the price adjustment probabilities. The expected sign is negative for price increases (and positive for price decreases), as a larger positive gap indicates more price decreases and a larger negative gap more price increases. In the baseline

[13]The credit shock is a generated regressor. However, as the interaction term is insignificant in our regressions, we do not need to adjust our standard errors (Wooldridge, 2010)

specification, we also control for the direct impact of the aggregate shock. Its coefficient (β_{ih}) has a negative expected sign for price increases (and positive expected sign for price decreases, as a credit tightening ($\hat{\text{ebp}}_t > 0$) implies more increases (and more decreases) and a credit easing ($\hat{\text{ebp}}_t < 0$) implies more increases (and less decreases).

We note that our specification also takes into consideration the time-dependent features of price-setting. First, we measure the age of the price as the number of months since the last reference-price change and include its logarithm as a control variable. Additionally, we also include calendar-month fixed effects in our baseline regression, to control for the seasonality of price changes.

The dependent variable in our baseline regression is the probability of reference-price change in the upcoming 24 months $h = 24$. The horizon is motivated by the peak price-level impact of the shock (see Section 4). On average, the probability of a reference price increase and a decrease at this horizon is 54% and 24%, respectively.

Our main results are twofold. Our first result presents strong evidence for state-dependence in the data: The probability of price adjustment increases with the price gap, and the aggregate credit shock affects the probability of price adjustment. Table 2 illustrates these results in our baseline regressions for the competitor price gap proxy in response to a credit shock. Column (1) is the main specification for price increases and Column (4) for price decreases. Columns (2) and (5) show specifications with time fixed effects while (3) and (6) show results for positive and negative gaps separately.

The coefficients show that the effects are generally economically meaningful. First, a one standard deviation credit tightening (30 basis points) reduces the probability of price increases by around 1 percentage point and increases the probability of a price decrease by the same amount. Second, the probability of a price increase for a product at the first quartile of the price-gap distribution relative to the third quartile is 26 percentage points lower; and for the price decrease, it is 23 percentage points higher. These results are inconsistent with the time-dependent model of Calvo (1983), which ignores product-dependent pressures as a factor in price-adjustment probability.

Our second, surprising main finding in the paper is that there is no evidence of selection: Conditional on the aggregate shock, the chance of new price adjustments is independent of our measure of the price gap. None of the coefficients on the interaction term are statistically significantly different from 0. This is inconsistent with menu cost models with high selection (Golosov and Lucas, 2007; Karadi and Reiff, 2019). As we argue in Section 7, models with random menu costs and weak selection (Dotsey et al., 1999; Luo and Villar, 2017) are, however, broadly consistent with such evidence.

Table 2: Estimates, scanner data, competitor-price gap, credit shock

	(1)	(2)	(3)	(4)	(5)	(6)
	Price increase $\left(I^{+}_{pst,t+24}\right)$			Price decrease $\left(I^{-}_{pst,t+24}\right)$		
Gap (x_{pst-1})	-1.75^{***}	-1.75^{***}		1.55^{***}	1.55^{***}	
	(0.06)	(0.06)		(0.06)	(0.06)	
Shock (ebp_t)	-0.03^{***}		-0.04^{***}	0.03^{***}		0.03^{***}
	(0.01)		(0.01)	(0.01)		(0.01)
Selection ($x_{pst-1}\hat{\text{ebp}}_t$)	-0.00	-0.00		0.01	0.01	
	(0.04)	(0.04)		(0.05)	(0.04)	
Age (T_{pst-1})	0.02^{***}	0.02^{***}	0.02^{***}	0.00^{**}	0.01^{***}	0.01^{***}
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Pos. gap (x^{+}_{pst-1})			-2.26^{***}			2.29^{***}
			(0.13)			(0.10)
Neg. gap (x^{-}_{pst-1})			-1.44^{***}			1.10^{***}
			(0.07)			(0.06)
Pos. sel. ($x^{+}_{pst-1}\hat{\text{ebp}}$)			0.04			-0.04
			(0.06)			(0.05)
Neg. sel. ($x^{-}_{pst-1}\hat{\text{ebp}}$)			-0.03			0.04
			(0.06)			(0.07)
Product x store FE	✓	✓	✓	✓	✓	✓
Calendar-month FE	✓	✗	✓	✓	✗	✓
Time FE	✗	✓	✗	✗	✓	✗
N	$16.1M$	$16.1M$	$16.1M$	$16.1M$	$16.1M$	$16.1M$
within R^2	18.5%	16.6%	18.9%	17.3%	16.4%	18.2%

Note: The table shows estimation results from a linear-probability panel model using scanner data. The regressions are run separately using an indicator with value 1 for reference-price increases (columns 1-3) and an indicator with value 1 for reference-price decreases (columns 4-6). The regressions include product-store and calendar-month fixed effects, control for the age of the price (time spent since last change), and use standard errors with two-way clustering. The baseline regressions (columns 1 and 4) show that even though the price gap and the aggregate shock significantly influence the price-change probabilities, which is evidence for state-dependence, their interaction remains insignificantly different from zero, suggesting selection is absent. The results stay robust to a specification with time-fixed effects (columns 2 and 5) and a specification with separate coefficients for positive and negative gaps (columns 3 and 6).
*Standard errors in parentheses; *: significant at 10%, **: significant at 5%, ***: significant at 1%.*

6 Robustness

In this section, we show the robustness of our results. First, when we consider a non-linear specification, second, when we consider the reset-price gap, third with producer price (PPI) microdata instead of retail data, and fourth if we consider a monetary instead of a credit shock. Selection is again absent in all cases.

6.1 Non-linearity

A potential concern may be found in the linear relationship imposed by our baseline specification between the price-change probabilities and the price gap, as well as the price gap conditional on an aggregate shock size. If these relationships were non-linear, our rejection of the presence of selection could be a rejection of the linearity assumption. This section conducts a robustness check using a non-parametric approach and rules out such concerns.

Our main non-parametric approach begins by assigning price gaps into 5 approximately equal-sized[14] bins. The bin that serves as a reference group includes items with small price gaps, in particular price gaps between $-1\% \leq x_{pst-1} < 1\%$. We compare the price-setting behavior of the reference bing to 2 bins with negative ($-4\% \leq x_{pst-1} < -1\%$) and positive ($1\% \leq x_{pst-1} < -4\%$) medium-size gaps; and 2 bins with negative ($x_{pst-1} < -4\%$) and positive ($4\% \leq x_{pst-1}$) large gaps.

As before, we run separate regressions for positive and negative price changes with item- and time fixed effects and two-way clustering. Instead of the size of the price gap, we now include bin-dummies both as direct regressors and in the interaction terms. We exclude the reference group from among the direct regressors and the interaction terms, so the estimated coefficients compare the average impact of the groups on the price-change probabilities directly, and conditional on an aggregate shock relative to the reference group.

Our results are robust to this non-parametric specification as Table 3 shows. In particular, the probability of price change increases with the average absolute size of the price gaps in the bin, but the interaction terms are not significantly different from the interaction term of the reference bin, implying no detectable selection.

As an additional robustness check, Figure 9 depicts the estimated coefficients of an analogous specification with 15 equal-sized bins instead of 5.[15] Again, the specification includes item- and time fixed effects, and the standard errors are clustered by category and time. The red lines show the impact on price- increase probabilities and price-decrease probabilities of the price-gap groups relative to the group with small gaps. The panels show that the size of the gap has a large impact on the probability of price adjustment in the next 2 years: For example, if the negative gap exceeds 20 percent, the probability of a future price decrease goes up by almost 50 percentage points. The figure also shows that the relationship between the gaps and the adjustment probabilities is monotone and close to linear, justifying our linear baseline specification. The blue lines show the impact of the gap-bin-credit-shock interaction terms on the price-change probabilities. The panels show that the additional impact brought about by the aggregate shock does not significantly vary with the gap, so selection is undetectable. The panels also show that the insignificant results in our baseline specifications are not the consequence of us imposing linearity: there is no detectable interaction between the aggregate shock and the gaps at any gap levels.

6.1.1 A multinomial-probit and an ordered-probit specification

Our baseline results rely on a linear-probability specification. The framework is suitable to assess the average impact of explanatory variables on the binary dependent variables (Wooldridge, 2010), especially if

[14] The bins are not equal-sized to maintain symmetry. In particular, we generate 5 equal-sized bins with negative gaps and 5 equal-sized bins with positive gaps. Then we merge the largest negative and the smallest positive bins and each consecutive negative and positive bin. Because the price gap distribution is approximately symmetric, we obtain 5 approximately equal-sized bins.

[15] The horizontal axis of the figures show the threshold competitor-price gaps of the bins.

Table 3: Non-linear specification, 5 groups

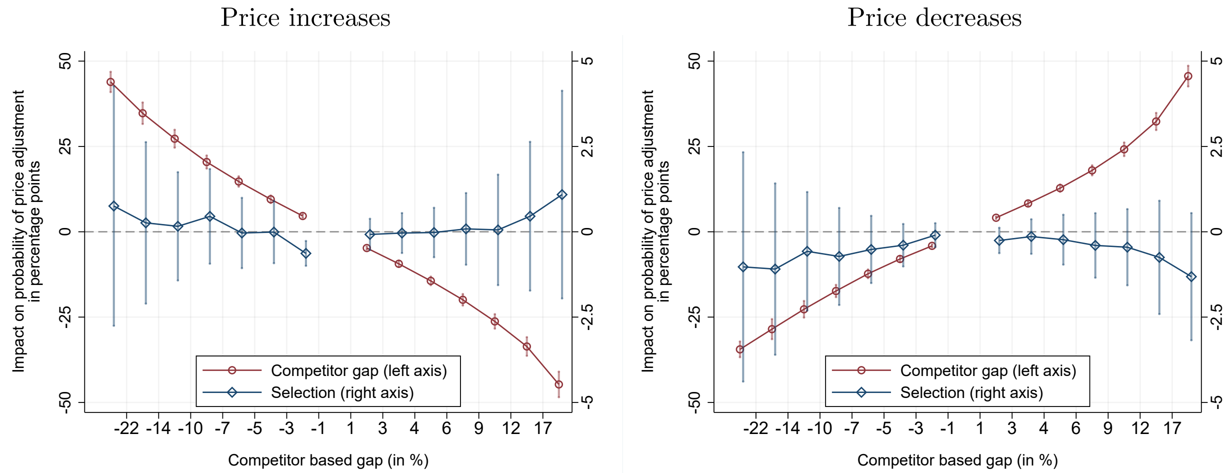
	(1) $I^{+}_{pst,t+24}$	(2) $I^{-}_{pst,t+24}$
Large negative gap ($x_{pst-1} << 0$)	0.35^{***}	-0.28^{***}
	(0.01)	(0.01)
Medium negative gap ($x_{pst-1} < 0$)	0.15^{***}	-0.13^{***}
	(0.01)	(0.01)
Medium positive gap ($x_{pst-1} > 0$)	-0.15^{***}	0.13^{***}
	(0.01)	(0.01)
Large positive gap ($x_{pst-1} >> 0$)	-0.33^{***}	0.32^{***}
	(0.01)	(0.01)
Large negative selection ($x_{pst-1} << 0$)$\hat{\text{ebp}}_t$	0.01	-0.01
	(0.01)	(0.01)
Medium negative selection ($x_{pst-1} < 0$)$\hat{\text{ebp}}_t$	0.00	-0.00
	(0.01)	(0.00)
Medium positive selection ($x_{pst-1} > 0$)$\hat{\text{ebp}}_t$	0.00	-0.00
	(0.00)	(0.00)
Large positive selection ($x_{pst-1} >> 0$)$\hat{\text{ebp}}_t$	0.01	-0.01
	(0.01)	(0.01)
Age (T_{pst-1})	0.02^{***}	0.01^{***}
	(0.00)	(0.00)
Product x store FE	✓	✓
Time FE	✓	✓
N	$16.1M$	$16.1M$
within R^2	16.6%	16.5%

Note: The table shows estimation results from the linear-probability panel model using scanner data. The regressions are run separately on an indicator with value 1 for price increases (columns 1) and an indicator with value 1 for pride decreases (columns 2). The regressions include product-store and time fixed effects and calculates standard errors with two-way clustering. The table shows that even though groups with larger absolute gaps increase the price-change probability, the interaction-term with the aggregate shock always stay insignificantly different from 0. The results confirm that selection is undetectable in the data.
*Standard errors in parentheses; *: significant at 10%, **: significant at 5%, ***: significant at 1%.*

the actual relationship for common values of the dependent variables is indeed close to linear as our evidence just above indicates. However, the linear-probability specification does not take into account restrictions that necessarily exist in our setting, for example, because the sum of the probability of price increase and price decrease cannot exceed 1 for any feasible values of the explanatory variables.

In this section, we show that our results are robust to specifications which explicitly take into account such restrictions on our discrete dependent variables. In particular, we run a multinomial- and an ordered

Figure 9: Non-linear specification, 15 groups



Note: The figures depict the impact on price-increase (left panel) and price-decrease (right panel) probabilities the estimated differential impact of 15 equal-sized groups with various price gaps (red lines) and price-gap-credit-shock interaction terms (blue lines) relative to the group with close to 0 gaps. The specification includes item- and time fixed effects, and the standard errors are clustered along categories and time. The horizontal lines show 95-percent confidence bands. The figure shows that while the gap in itself significantly influences the price-adjustment probability, it has no significant interaction with the aggregate shock at any gap sizes, so selection is not detectable.

probit specification. The multinomial specification assumes that the firm compares the relative advantages of a price increase, no price change, and a price decrease. The specification allows the explanatory variables to differently influence the gains of price increases relative to no change, compared to the gains from price decrease relative to the no-change decision. Consequently, we report separate coefficients for price increases versus price decreases, similarly to our baseline specification. In contrast, the ordered probit specification assumes the decision is ordered: As the unobserved price gap moves from a low level to a high level, so does the predicted probability from a price increase to no change to a price decrease. The specification, therefore, imposes the same coefficients across explanatory variables. In both specifications, we exclude item fixed effects. To control for cross-sectional heterogeneity in the probability of price adjustments, we include the average frequency of price increases and price decreases of close substitutes as additional explanatory variables (see below),[16] and normalize price gaps by removing their means and divide them with their standard deviation at the item level.

We measure the average frequency of price increases and decreases of close substitutes as follows. We calculate reference price increases and decreases of each product p in a particular market M, but we ignore own changes in store s. Formally, the frequency of increases (decreases are analogous) is

$$\xi^{+}_{psM} = \sum_{t=1}^{T} \frac{\sum_{z \in M \setminus s} \omega_{pzt} I^{+}_{pzt}}{\sum_{z \in M \setminus s} \omega_{pzt}}, \tag{8}$$

where $M \setminus s$ is the set of stores in market M except store s, ω_{pzt} is the annual expenditure weights of product p in store z in month t, and I^{+}_{pzt} is an indicator function taking the value 1 if there is a price increase for

[16]These variables would be crowded out by the item fixed effects in our baseline specifications.

product p in store z at month t.

Table 4: Multinomial and ordered probit estimates, scanner data, competitors' price gap, credit shock

	(1)	(2)	(3)
	Multinomial probit		Ordered probit
	Incr. $\left(I^{+}_{pst,t+24}\right)$	Decr. $\left(I^{-}_{pst,t+24}\right)$	Change $(I_{pst,t+24})$
Gap (x_{pst-1})	-3.15^{***}	3.37^{***}	-4.24^{***}
	(0.02)	(0.04)	(0.04)
Shock (ebp_t)	-0.11^{***}	0.05^{***}	-0.10^{***}
	(0.03)	(0.01)	(0.02)
Selection $(x_{pst-1}\hat{\text{ebp}}_t)$	-0.05	-0.21^{**}	0.04
	(0.06)	(0.11)	(0.12)
Age (T_{pst-1})	0.01^{*}	-0.03^{***}	0.02^{***}
	(0.00)	(0.00)	(0.00)
Freq. incr. (ξ^{+}_{psM})	5.17^{***}	2.91^{***}	1.79^{***}
	(0.03)	(0.02)	(0.03)
Freq. decr. (ξ^{-}_{psM})	3.02^{***}	5.84^{***}	-1.33^{***}
	(0.03)	(0.05)	(0.04)
Product x store FE	✗	✗	✗
Calendar-month FE	✓	✓	✓
Time FE	✗	✗	✗
N	$16.1M$	$16.1M$	$14.3M$

Note: The table shows estimation results from multinomial probit and ordered probit models using scanner data. The regressions consider 3 choices (price increase, no change, decrease). The regressions control for the age (time spent since last change) of the price, the frequency of price increases and price decreases among competitor prices in the market (excluding own change), and use standard errors with clustering across product-stores. The results are robust: the price gap and the aggregate shock significantly influence the price-change probabilities, but their interaction is never consistent with a significant selection effect. The interaction term is mostly insignificant, except for price decreases in the multinomial probit model. In this case, the interaction term is significantly different from 0, but has a counterintuitive sign: a higher positive gap and aggregate credit tightening imply fewer (not more) price decreases. Consequently, this estimate is also inconsistent with selection.
*Standard errors in parentheses; *: significant at 10%, **: significant at 5%, ***: significant at 1%.*

Table 4 presents the results. Columns 1 and 2 show the estimated coefficients from the multinomial probit specification and column 3 the estimated coefficients from the ordered probit specification. We find that the price gap and the aggregate shock significantly influence the price-change probabilities, but their interaction is never consistent with a significant selection effect. The interaction term is mostly insignificant, except for price decreases in the multinomial probit model. In this case, however, while the interaction term is significantly different from 0, it has a counterintuitive sign: A higher positive gap and aggregate credit tightening imply less (not more) price decreases. Consequently, this estimate is also inconsistent with selection.

6.2 Reset-price gap

In this section, we show the robustness of our result if, instead of the competitor-price gap, we use an alternative gap measure, the reset-price gap. The reset price is a counterfactual price a firm would choose if price-adjustment frictions were temporarily absent. In a wide class of state-dependent price-setting models, the reset price is the key product-level attractor that drives the probability and the size of price adjustment.

Bils et al. (2012) offers an iterative algorithm to obtain a proxy for the reset price. The algorithm relies on two key assumptions. First, when a firm adjusts its price, it sets it at the reset price. Second, when the firm does not adjust its price, its reset price evolves with the reset-price inflation of its close competitors, which can be measured from the changes in the reset prices of the adjusting competitors. Formally, the logarithm of the reset-price of item i in month t is

$$p_{it}^* = \begin{cases} p_{it} & \text{if } I_{it} = 1 \\ p_{it-1}^* + \pi_{ct}^* & \text{otherwise} \end{cases} \tag{9}$$

where I_{it} is an indicator function that takes the value of 1 if the price of product i changes in the month t, and π_{ct}^* is the reset-price inflation in the product's category c. Reset-price inflation, in turn, is given by

$$\pi_{ct}^* = \sum_{i \in c} \frac{\omega_{it} I_{it} \left(p_{it}^* - p_{it-1}^*\right)}{\sum_{i \in c} \omega_{it} I_{it}}, \tag{10}$$

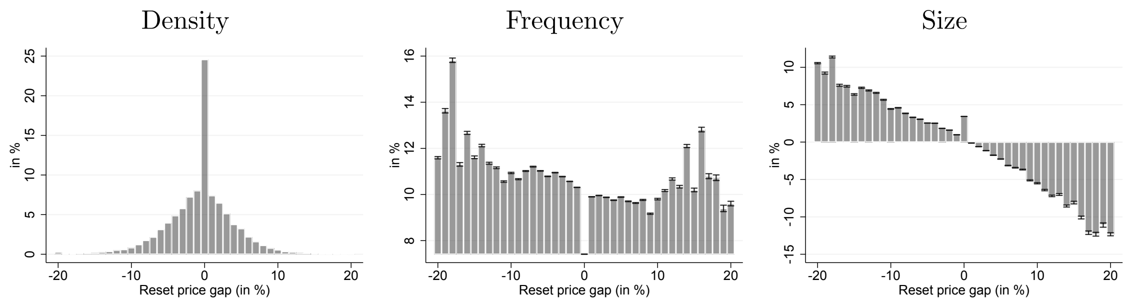
where ω_{it} denotes the expenditure weight of item i.

The reset-price gap is simply the distance of the logarithm of the price from the logarithm of its reset price $x_{it} = p_{it} - \pi_{it}^*$. We assess whether these reset price gaps truly proxy actual price gaps by looking at the average size of price changes conditional on the reset-price gap in the previous month. If the proxy is good, there should be a strong correlation between the size of the price change and the lagged reset price gap. The left panel of Figure 10 shows the relationship across bins of the reset price gap calculated on the pooled data across items and time with 95 percent standard error bands based on the time-variation of the bar heights. The relationship is clearly negative with a close to -1 correlation. The right panel of Figure 10 shows the histogram of the reset price gaps. The distribution has a negative median, fat tails, and is left-skewed.

Figure 11 presents the size and frequency histograms separately for price increases and decreases. The tight negative relationship between the reset-price gap and the moments are salient in the graphs. At the same time, the panels of the figure reveal that the reset-price gap is not the sole factor driving firms price-setting decisions, because we see a non-negligible fraction of firms increasing their prices even though their reset-price gap is positive, and conversely, decrease them even though the gap is negative. Furthermore, the probability of price increases drops down from its peaks after the price gaps become lower than 20 percent (see the bottom left panel on Figure 11).

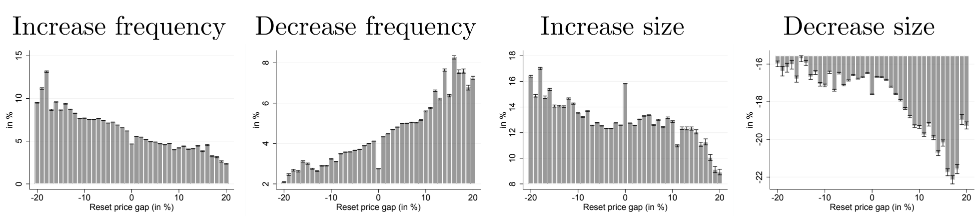
Compared to the competitor-price gap, the effects have qualitatively similar effects. First, a one standard deviation credit tightening (30 basis points) reduces the probability of price increases by around 1 percentage point and increases the probability of a price decrease by the same amount. Second, the probabilities are less sensitive to the reset price gaps than to our baseline competitor-price gap but qualitatively similar: the probability of a price increase for a product at the first quartile relative to the third quartile is 2.25 percentage points lower, and for the price decreases it is 1.6 percentage points higher. The interaction terms remain

Figure 10: Reset-price gap density and the subsequent frequency and size of price changes as a function of the gap, scanner data



Note: The figures show the unconditional density of the reset-price gap and frequency and size of subsequent price changes as a function of the gap in the baseline supermarket dataset. The figures show that the density of the reset-price gaps has fat tails (first panel); the frequency of subsequent price adjustment increases with the absolute size of the gap (the second panel, see also next figure); and the size of average subsequent adjustments are inversely related with the gap (third panel).

Figure 11: Frequency and size of subsequent price changes as a function of reset-price gap



Note: The figures show the subsequent increase and decrease probabilities and increase and decrease sizes as a function of reset-price gaps. The figures show the negative relationship between gaps and the increase-frequency (except for large negative gaps) and the positive relationship between the gaps and the decrease-frequency. The figures also confirm a similar relationship with the average size of subsequent price changes, with the relationship losing its monotonicity for positive gaps and increase sizes and negative gaps with decrease sizes.

insignificantly different from zero, implying a lack of a selection effect as in the case of the competitor-price gaps

6.3 Producer-price microdata

In this section, we show the robustness of our results when, instead of IRi the scanner data, we use the producer-price microdata. The producer price data we consider are the microdata underlying the U.S. producer price index. Even though the data is somewhat less granular than the IRi microdata so it has a disadvantage in the construction of relevant price-setting proxies it has a much longer time-series coverage (1981-2012) and wider cross-sectional coverage than the IRi microdata.

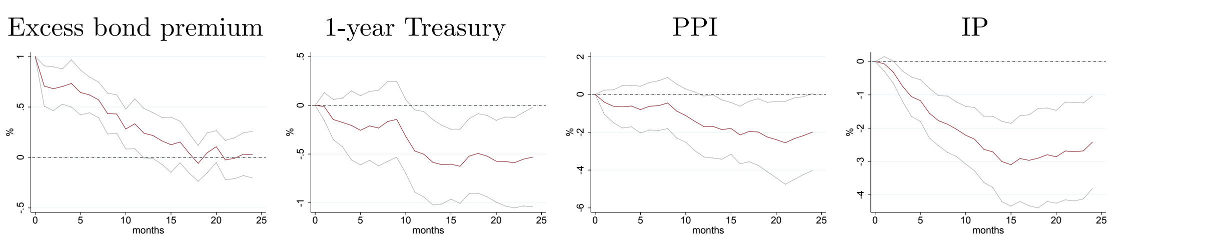
Just like in the case of the IRi retail data, we find in the first step that the impulse response of the PPI to the credit shock is economically intuitive. Despite a monetary easing, industrial production and prices fall following a credit shock. We estimate specification (3) with the PPI on the left-hand side to show these results. Figure 12 summarizes our findings.

Table 5: Estimates, scanner data, reset-price gap, credit shock

	(1)	(2)	(3)	(4)	(5)	(6)
	Price increases $\left(I^{+}_{pst,t+24}\right)$			Price decreases $\left(I^{-}_{pst,t+24}\right)$		
Gap (x_{pst-1})	-0.45^{***}	-0.48^{***}		0.34^{***}	0.37^{***}	
	(0.07)	(0.06)		(0.04)	(0.04)	
Shock ($\hat{\text{ebp}}_t$)	-0.04^{***}		-0.04^{***}	0.03^{***}		0.03^{***}
	(0.01)		(0.01)	(0.01)		(0.01)
Selection ($x_{pst-1}\hat{\text{ebp}}_t$)	-0.14	-0.13		0.12	0.14	
	(0.14)	(0.12)		(0.12)	(0.10)	
Age (T_{pst-1})	0.01^{***}	0.01^{***}	0.01^{***}	0.01^{***}	0.02^{***}	0.01^{***}
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Positive gap (x^{+}_{pst-1})			-0.39^{***}			0.33^{***}
			(0.07)			(0.07)
Negative gap (x^{-}_{pst-1})			-0.49^{***}			0.35^{***}
			(0.13)			(0.07)
Pos. sel. ($x^{+}_{pst-1}\hat{\text{ebp}}_t$)			0.11			-0.03
			(0.15)			(0.13)
Neg. sel. ($x^{-}_{pst-1}\hat{\text{ebp}}_t$)			-0.27^{**}			0.21^{*}
			(0.13)			(0.12)
N	$16.1M$	$16.1M$	$16.1M$	$16.1M$	$16.1M$	$16.1M$
within R^2	2.6%	0.3%	2.6%	1.3%	0.3%	1.3%

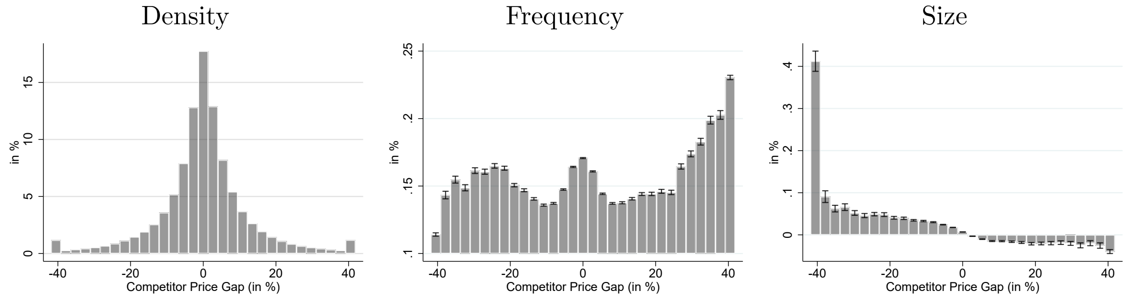
*Note: The table shows estimation results from the linear-probability panel model using scanner data. The regressions are run separately on an indicator with value 1 for price increases (columns 1-3) and an indicator with value 1 for pride decreases (columns 4-6). The regressions include product-store and calendar-month fixed effects, control for the age of the price (months since last change), and calculates standard errors with two-way clustering. The baseline regressions (columns 1 and 4) show that even though the price gap and the aggregate shock significantly influence the price-change probabilities their interaction remains insignificantly different from zero, suggesting selection is absent. The results stay robust to a specification with time-fixed effects (columns 2 and 5, without calendar-month FE) and a specification with separate coefficients for positive and negative gaps (columns 3 and 6). Standard errors in parentheses; *: significant at 10%, **: significant at 5%, ***: significant at 1%.*

Figure 12: Impulse responses of key macroeconomic variables to a credit shock, 1985-2015



Note: The figures show impulse responses to an identified credit shock over the PPI sample 1985-2015 in a local-projection framework, and 95% confidence bands using Newey-West standard errors. The figures show that the credit tightening causes a sizable drop in activity and the price index despite sizable monetary policy easing.

Figure 13: Competitor-price gap density and the subsequent frequency and size of price changes as a function of the gap, PPI data



Note: The figures show the unconditional density, and frequency and size responses of subsequent price change in the PPI dataset. The figures show that the density of the competitor-price gaps has fat tails (first panel); the frequency of subsequent price adjustment increases with the absolute size of the gap, after the gap is large enough; and the size of average subsequent adjustments are negatively related with the gap (third panel).

Moreover, our price gap proxy is informative in a similar fashion as in the IRi retail data, establishing the general validity of our proxy measures. Figures 13 show the density of competitor-price gaps, together with the relation of the gaps and the frequency and size of subsequent price changes. They show that there is a clear negative relationship between the size of the price changes and the proxies for the gaps. The proxies clearly capture a relevant part of the theoretical price gaps, even though the proxies are not as well measured as in the more granular scanner data. The frequency of price changes increases with the price gap as the gap becomes sufficiently large, even though it declines for small price gaps. This might reflect the role of heterogeneity in price-setting frequencies across sectors, as well as the presence of measurement error.

Table 6 shows the linear-probability panel estimations for our baseline and time-fixed effect specifications using the PPI micro-data for the competitor-price gap. Our results are very robust using the PPI microdata. There is evidence for state dependence because larger gaps and the aggregate shocks change the probabilities of price adjustment. At the same time, there is no evidence of selection: conditional on the aggregate shock, the new adjusters do not come from those prices that are further from their optimal levels. All interaction terms are statistically insignificantly different from zero.

The impact of the price gaps and the aggregate credit shocks are again also economically significant: The probability of price increases between a product with a competitor price gap at the third quartile and at the first quartile gets smaller by 23 percentage points, and the probability of price increases gets larger by 22 percentage points. Finally, a one standard deviation credit tightening reduces the price increase probability by around 0.7 percentage points and increases the price decrease probability by a similar amount.

As before, given the similar price gap properties and aggregate dynamics of the economy, we confirm our main result: Selection is absent – the interaction term between the price gaps and the aggregate shocks stay consistently insignificantly different from zero.

6.4 Response to monetary policy shocks

This section describes the properties of our second identified shock, the monetary policy shock, and price adjustment in response. Again, we find that prices adjust predominantly through the gross extensive margin.

Table 6: Estimates, PPI data, competitors' price gap, credit shock

	(1)	(2)	(3)	(4)
	Increases $\left(I^{+}_{pst,t+24}\right)$		Decreases $\left(I^{-}_{pst,t+24}\right)$	
Gap (x_{pst-1})	-0.23^{***}	-0.23^{***}	0.22^{***}	0.22^{***}
	(0.02)	(0.02)	(0.02)	(0.02)
Shock (ebp_t)	-0.023^{***}		0.021^{***}	
	(0.01)		(0.01)	
Selection ($x_{pst-1}\hat{\text{ebp}}_t$)	0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Age (T_{pst-1})	0.035^{***}	0.035^{***}	0.01^{***}	0.01^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
Product x store FE	✓	✓	✓	✓
Calendar-month FE	✓	✗	✓	✗
Time FE	✗	✓	✗	✓
N	$9.7M$	$9.7M$	$9.7M$	$9.7M$
Within R^2	4.4%	3.5%	4.3%	3.7%

Note: The table shows estimation results from the linear-probability panel model using PPI microdata. The regressions are run separately on an indicator with value 1 for price increases (columns 1-2) and an indicator with value 1 for pride decreases (columns 3-4). The regressions include product-store fixed effects, control for the age (time spent since last change) of the price, and use standard errors with two-way clustering. The baseline regressions (columns 1 and 3) show that even though the price gap and the aggregate shock significantly influence the price-change probabilities their interaction remains insignificantly different from zero, suggesting selection is absent. The results stay robust to a specification with time-fixed effects (columns 2 and 4).
*Standard errors in parentheses; *: significant at 10%, **: significant at 5%, ***: significant at 1%.*

Additionally, we also find a response of the size and frequency of price changes inconsistent with time-dependent models.

As we have done with the credit shock, we start by characterizing the dynamic impact of a monetary policy shock on inflation and its components using the local projection method (Jordà, 2005). The local projection framework puts minimal structure on the data generating process. As instruments for monetary policy shocks, we use changes in the 3-month-ahead federal funds futures in a 30-minute window around FOMC press statements like Gertler and Karadi (2015). The identification assumption is that because financial markets incorporate all available information into futures prices before the announcement, the change in the futures price indicates the size of the policy surprise. Furthermore, the narrow window guarantees that no other economic shock systematically contaminates the measure. We restrict our interest to announcements where the interest rate surprise and the S&P blue-chip stock price index moved in the opposite direction over the same time frame. As argued by Jarociński and Karadi (2020), such co-movement is indicative of a dominant monetary policy shock, when the impact of the central bank's contemporaneous announcements about the economic outlook played a minor role. We transform these surprises to monthly variables by simply summing up monetary policy surprises within each calendar month. We run a series ($h = 0, \ldots, 24$

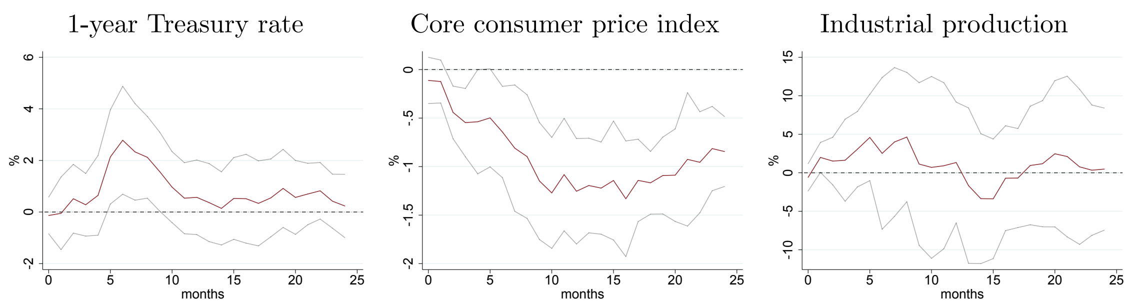
months) of regressions of the following form:

$$x_{t+h} - x_t = \alpha_h + \beta_h \Delta i_t + \Gamma_h \Phi(L) X_t + u_{t,h}, \tag{11}$$

where x_t is the variable of interest (for example the log price level), and Δi_t is our proxy for a monetary policy shock. The local projections also include a set of controls $\Gamma_h \Phi(L) X_t$, where Γ_h is a vector of parameters for each h, X_t is a vector of control variables and $\Phi(L)$ is a lag-polynomial. Unless stated otherwise, the controls we use are the 1 to 6 months lags of the 1-year Treasury rate, the consumer price index, industrial production and the excess bond premium (Gilchrist and Zakrajšek, 2012).

Our key object of interest is the coefficient β_h. In the figures below, we plot β_h, $h = 0, 1, \ldots, 24$ along with 95% confidence bands.

Figure 14: Impulse responses of key macroeconomic variables to a monetary policy tightening



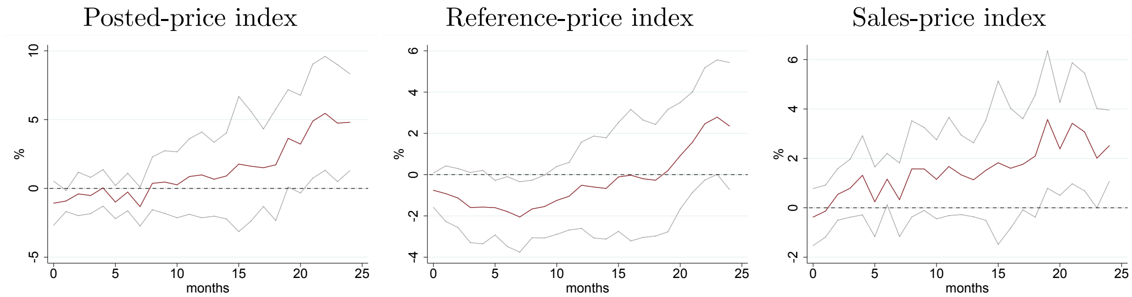
Note: The panels show impulse responses to an identified monetary policy shock in the scanner-data sample between 2001 and 2012 in a local-projection framework, and 95% confidence bands using Newey-West standard errors. The panels show that a monetary policy causes a sizable drop in the price index, but no noticeable drop in activity.

First, when we characterize the response of key macroeconomic variables to the monetary shock, they go in the expected directions. Figure 14 plots these impulse responses. of some key macroeconomic variables to the monetary policy shock. In particular, we plot the response to the 1-year constant maturity treasury rate, the response to the logarithm of the consumer price index excluding food and energy, and the response of the logarithm of the industrial production. Even though the sample is fairly short, the results are broadly in line with standard results (Gertler and Karadi, 2015): the interest-rate increase generates a delayed and hump-shaped decline in the core consumer price index and a slowdown in industrial production, albeit the latter insignificantly.

Second, when we consider the response of our supermarket prices, supermarket reference prices exhibit the expected response to a monetary tightening. Figure 15 plots the impulse responses of supermarket prices. Similarly to the aggregate price index, supermarket reference-prices display a hump-shaped decline with wide confidence bands after a monetary policy tightening (see middle panel). The decomposition of posted prices to reference- and sales-price indices also reveals that the supermarkets respond to the shock by actively adjusting their reference-prices, and not by modifying their strategy on temporary sales. This finding is consistent with views that argue that temporary-sales strategies are predetermined and not an active adjustment margin at business cycle frequencies (Anderson, Malin, Nakamura, Simester and Steinsson, 2017). Consequently, we concentrate on reference-prices in our subsequent analysis.

As before, we find that adjustment of the reference-price level happens through the extensive margin (by

Figure 15: Impulse responses of the supermarket-price indices to a monetary policy tightening



Note: The panels show impulse responses to an identified monetary policy shock in the scanner-data sample between 2001 and 2012 in a local-projection framework, and 95% confidence bands using Newey-West standard errors. The figures show that the reference-price index declines significantly at around a 6-month horizon as a response to the shock. The posted-price index increases significantly at a two-year horizon contrary to standard theory, but the increase is mostly driven by an increase in the filtered-out sales-price index.

modifying the number of price changes) rather than through the intensive margin (by changing the average size of price changes). To show this result, we decompose the cumulative reference-price inflation into the frequency ($\xi_{t,t+h}$) and the size ($\psi_{t,t+h}$) of price increases and price decreases as follows:

$$p_{t+h} - p_{t-1} = \pi_{t,t+h} = \xi^{+}_{t,t+h}\psi^{+}_{t,t+h} + \xi^{-}_{t,t+h}\psi^{-}_{t,t+h}. \tag{12}$$

The frequency of reference-price[17] increases and decreases are defined as

$$\xi^{\pm}_{t,t+h} = \sum_{i} \bar{\omega}_{it,t+h} I^{\pm}_{it,t+h}, \tag{13}$$

where $I^{+}_{it,t+h}$ and $I^{-}_{it,t+h}$ are indicators that take the value 1 if the reference price of item i (a product in a particular store) increased or decreased between months $t-1$ and $t+h$, respectively, and 0 otherwise. The weight $\bar{\omega}_{it,t+h}$ is measured as the average weight of the product between t and $t+h$. The average size of price increases and decreases are defined as

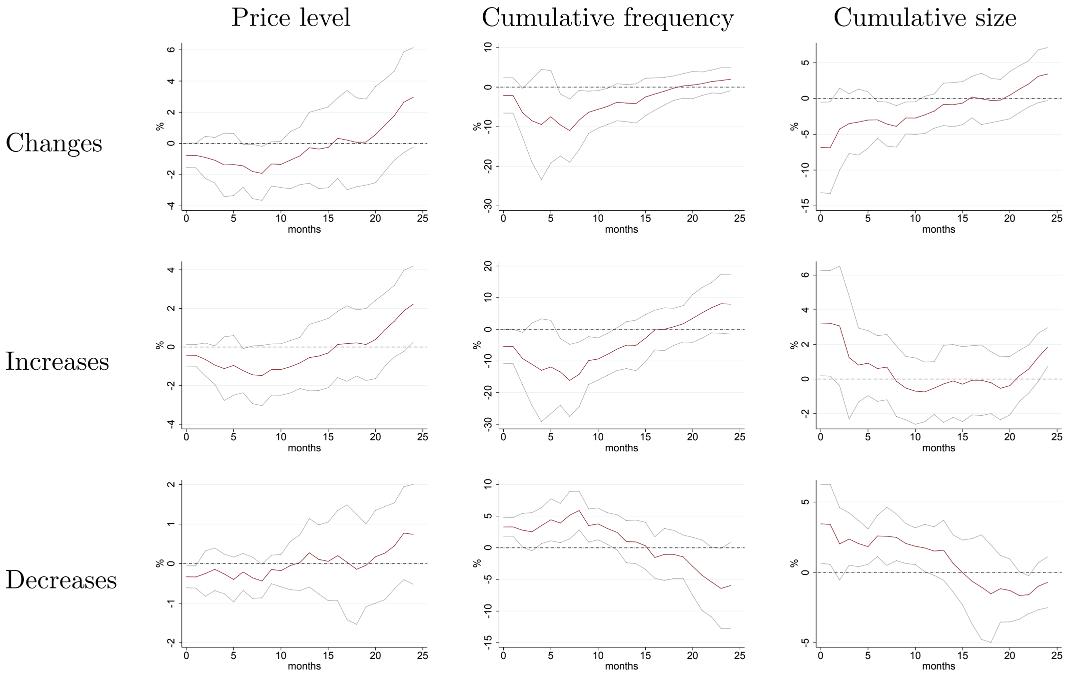
$$\psi^{\pm}_{t,t+h} = \frac{\sum_i \bar{\omega}_{it,t+h} I^{\pm}_{it,t+h}(p_{it+h} - p_{it-1})}{\xi^{\pm}_{t,t+h}}. \tag{14}$$

Following a monetary shock, we find that there is a strong adjustment on the extensive margin within a year of the policy shock: the cumulative frequency of reference-price increases declines, and the cumulative frequency of price decreases rises. The decline in the cumulative price increases is larger (around 15 percent at its peak) than the increase in cumulative price decreases (around 5 percent at its peak), so the aggregate frequency declines. Both of these changes contribute to the decline in the price level, and to the reduction in the average size of price changes. Figure 16 illustrates the decomposition of the response to a monetary policy shock into these adjustment margins.

By contrast, the average size of price increases rises and the absolute size of the price decreases declines.

[17] We suppress the superscript f for notational convenience.

Figure 16: Adjustment on the extensive and intensive margins to a monetary policy tightening



Note: The figures show the impulse responses of supermarket reference-price levels and their components to an identified monetary policy shock, and 95% confidence bands using Newey-West standard errors. The figures show that the negative price-level response (first figure in the first row) is predominantly driven by the decline in the price-increase frequency (second panel in the second row) and the increase in the price-decrease frequency (second panel in the third row). This adjustment in the gross extensive margin explains the decline in the cumulative size of the price changes (third panel in the first row). The cumulative frequency (net extensive margin) also declines (second panel in the first row).

Both of them mitigate the impact of the shock on the price level rate. Such evidence is inconsistent with the underlying assumptions in time-dependent models (Calvo, 1983), which assume a constant frequency of price changes and attribute the adjustment after a monetary policy shocks to the intensive margin. Our evidence instead points to the importance of the extensive margin, as the frequency of price increases and decreases adjust significantly. It challenges the predominance of the intensive margin adjustment, which would predict *lower* price increases and *larger* price decreases. The increase in the frequency and the decline in the average absolute size of price adjustment has been documented after large (e.g. value-added tax, exchange rate) shocks by Karadi and Reiff (2019); Auer et al. (2018), who also showed that menu cost pricing models with leptokurtic idiosyncratic productivity shock (Midrigan, 2011) are consistent with this pattern. To our knowledge, we are the first to document the same pattern after regular monetary policy shocks in U.S. data. In contemporaneous work, a similar pattern has been documented using German PPI data by Balleer and Zorn (2019).

Overall, the estimated coefficients are similar to those from our specification using the credit shock and tell the same story. There is evidence for the state-dependence of price changes as the price gaps and the monetary policy shock significantly impact the probability of price changes, but there is no evidence for selection as they do not interact.

Table 7: Estimates, scanner data, competitors' price gap, monetary policy shock

	(1)	(2)	(3)	(4)	(5)	(6)
	Price increases $\left(I^{+}_{pst,t+12}\right)$			Price decreases $\left(I^{-}_{pst,t+12}\right)$		
Gap (x_{pst-1})	-1.71^{***}	-1.71^{***}		1.36^{***}	1.36^{***}	
	(0.06)	(0.06)		(0.05)	(0.05)	
Shock (Δi_t)	-0.03^{*}		-0.03	0.01^{*}		0.01^{*}
	(0.01)		(0.02)	(0.01)		(0.01)
Selection ($x_{pst-1}\Delta i_t$)	-0.07	-0.07		0.07	0.07	
	(0.06)	(0.05)		(0.06)	(0.05)	
Age (T_{pst-1})	0.03^{***}	0.03^{***}	0.03^{***}	0.01^{***}	0.01^{***}	0.01^{***}
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Positive gap (x^{+}_{pst-1})			-1.92^{***}			1.93^{***}
			(0.10)			(0.09)
Negative gap (x^{-}_{pst-1})			-1.58^{***}			1.01^{***}
			(0.06)			(0.05)
Pos. selection ($x^{+}_{pst-1}\Delta i_t$)			-0.05			0.05
			(0.09)			(0.05)
Neg. selection ($x^{-}_{pst-1}\Delta i_t$)			-0.08			0.08
			(0.12)			(0.08)
Product x store FE	✓	✓	✓	✓	✓	✓
Calendar-month FE	✓	✗	✓	✓	✗	✓
Time FE	✗	✓	✗	✗	✓	✗
N	$23.7M$	$23.7M$	$23.7M$	$23.7M$	$23.7M$	$23.7M$
Within R^2	16.4%	14.7%	16.5%	13.3%	12.7%	13.8%

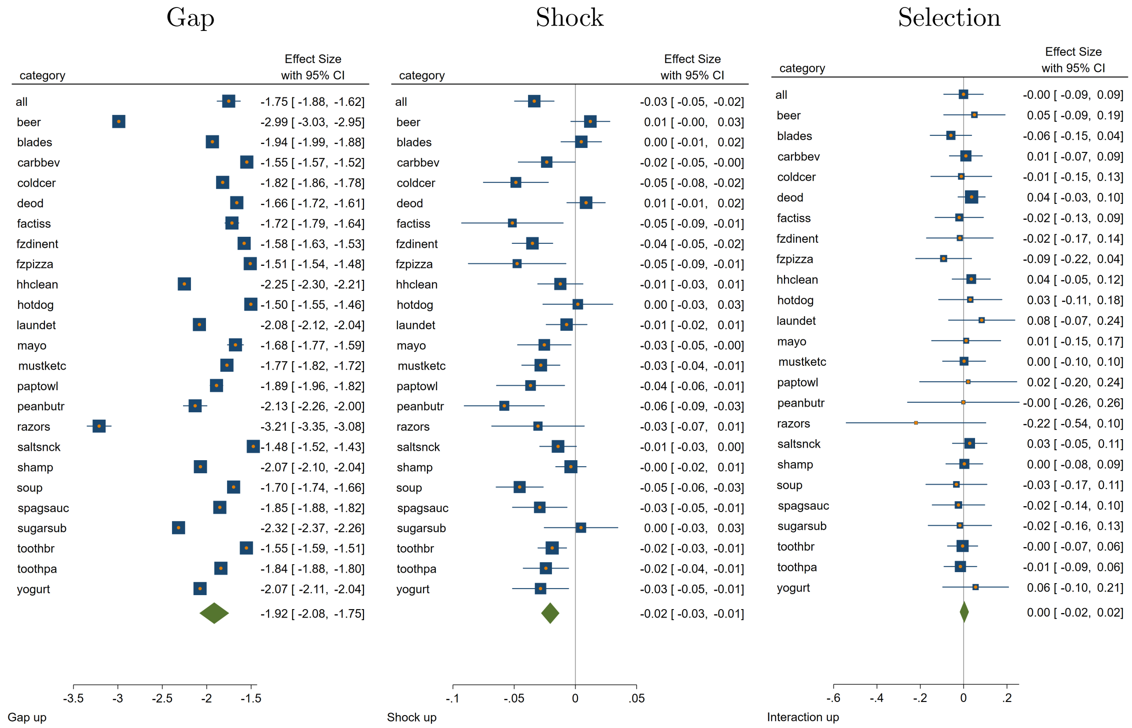
Note: The table shows estimation results from the linear-probability panel model using scanner data and a monetary policy shock. The regressions are run separately on an indicator with value 1 for reference-price increases (columns 1-3) and an indicator with value 1 for reference-price decreases (columns 4-6). The regressions include product-store and calendar-month fixed effects, control for the age of the price (time spent since last change), and use standard errors with two-way clustering. The baseline regressions (columns 1 and 4) show that even though the price gap and the aggregate shock significantly influence the price-change probabilities, which is an evidence for state-dependence, their interaction remains insignificantly different from zero, suggesting selection is absent. The results stay robust to a specification with time-fixed effects (columns 2 and 5) and a specification with separate coefficients for positive and negative gaps (columns 3 and 6).
*Standard errors in parentheses; *: significant at 10%, **: significant at 5%, ***: significant at 1%.*

6.5 Heterogeneity across product categories

We find that our results are robust to heterogeneity across product categories when we run our baseline regression separately for the 31 different product categories. This is reassuring because our baseline regression does not differentiate between potentially heterogeneous responses to idiosyncratic and aggregate volatility across product categories. The heterogeneity in demand elasticities due to different product characteristics and market structure (e.g. alcohol vs. milk) might potentially bias our estimates.

We show in Figures 17 and 18 that indeed the results are robust across product categories. Figure 17 shows the estimated coefficients and the uncertainty surrounding them for price increases, Figure 18 for price

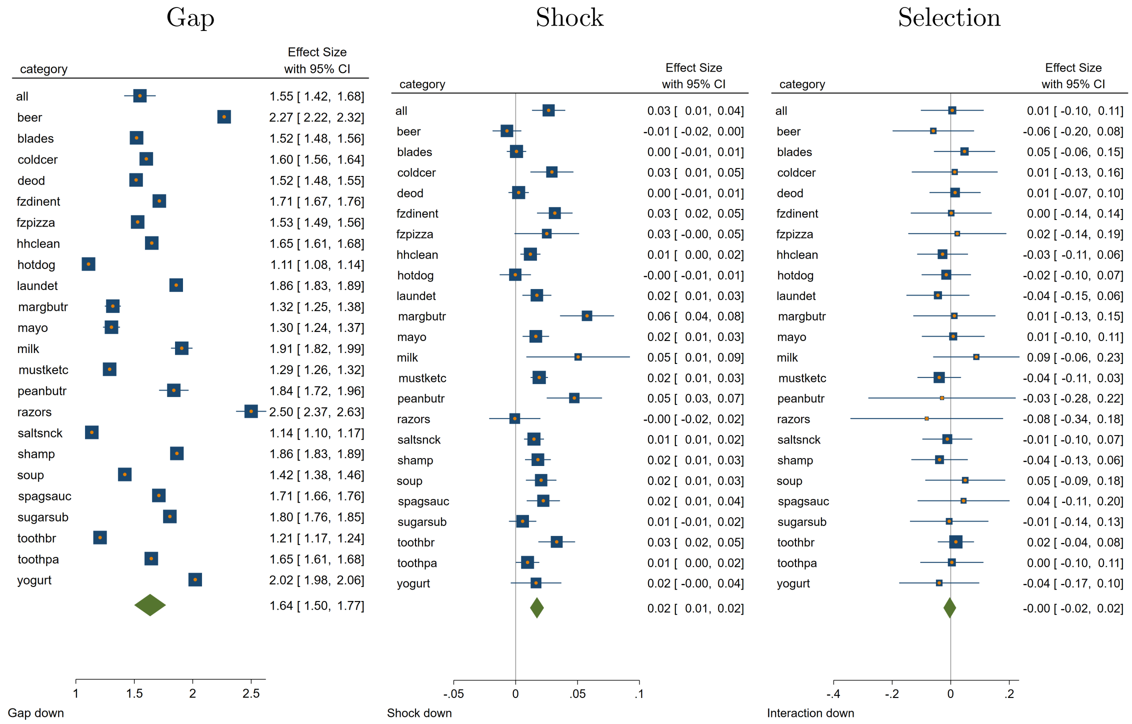
Figure 17: Estimated coefficients across product categories, price increases



Note: The figure shows estimates across product categories for price increases with 95 percent confidence bands for our baseline specification that uses the scanner data, the competitor-price gap measure, and credit shocks. The panels show that the results are robust across product categories: the higher gap significantly decreases the price-increase probability; credit tightening decreases the price-increase probability in the majority of categories, and their interaction is never significant, indicating selection is absent.

decreases. We find that a higher gap significantly decreases (increases) the price-increase (price-decrease) probability; credit tightening decreases (increases) the price-increase (decrease) probability in the majority of categories, and their interaction is never significant, indicating selection is absent.

Figure 18: Estimated coefficients across product categories, price decreases



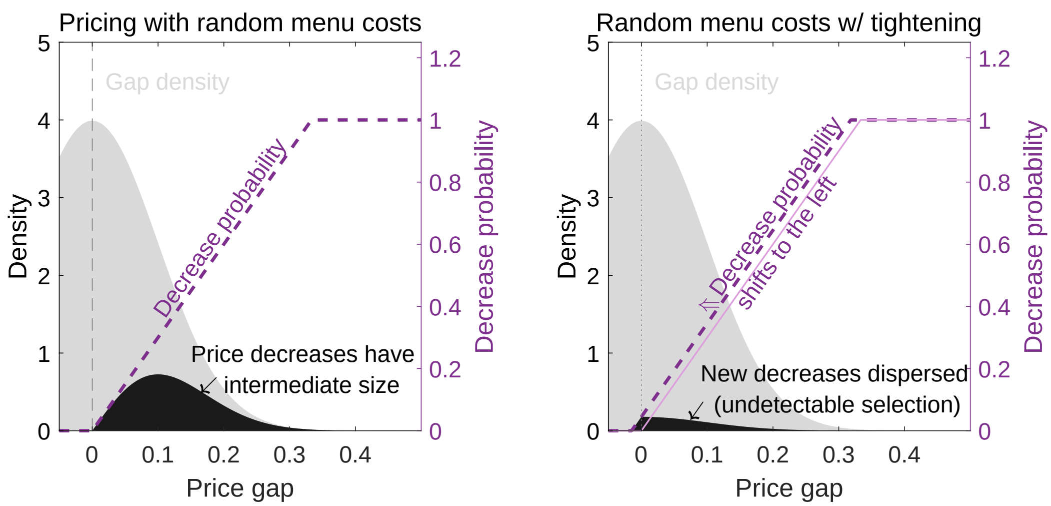
7 Discussion

What does our evidence imply about the extent of monetary non-neutrality? Which theoretical models are consistent with our evidence? While our facts may guide future modeling work, we briefly point out in this section which models are consistent with our findings, and what they imply for monetary non-neutrality.

Recall that we have documented two sets of empirical facts. First, we have found strong evidence for state-dependence, as, *unconditionally*, the probability of price adjustment increases with the larger (absolute) value of the price gap. Second, we have documented the lack of evidence for selection, as, *conditional* on identified credit and monetary-policy shocks, there is no evidence that prices with larger (absolute) price gaps are adjusting with higher probability, even though the relative share of price increases and price decreases does change after a shock.

There are theoretical models in the literature that are in line with most of our empirical results, and they can be used to draw conclusions about the implications of our evidence on monetary non-neutrality. These models assume that price adjustment is subject to a small menu cost, but the size of the menu cost is random like in Dotsey et al. (1999); Costain and Nakov (2011); Luo and Villar (2017); Alvarez et al. (2020). Random menu costs can generate various forms of price adjustment hazards ranging between a step function that takes a value 0 for gaps that have absolute values below a threshold and 1 otherwise, and a constant hazard that is independent of the gaps. The former is achieved if the menu cost takes a fixed value with probability one (Golosov and Lucas, 2007, fixed menu cost as in the left panel of Figure 8) and the latter, when the menu cost takes the value 0 with a certain probability and a very high value otherwise (time dependent Calvo, 1983, model, right panel of Figure 8). Correspondingly, these models can imply a whole range of monetary non-neutralities from almost full monetary neutrality (Golosov and Lucas, 2007) to high monetary non-neutrality (Calvo, 1983). As argued by Caballero and Engel (2007); Alvarez et al. (2020), and illustrated in Figure 8, the shape of the adjustment hazard function has a strong influence on monetary non-neutrality, because it drives the selection effect through influencing the distribution of new adjusters (and new non-adjusters) over the price gap distribution. When the slope of the function increases (as with the standard Ss model), the new adjusters tend to be located at high gap values, which imply large price adjustments and low monetary non-neutrality. In contrast, when the slope is constant, new adjusters are distributed more evenly across the price gap distribution, implying low selection and high monetary non-neutrality.

Figure 19: State-dependence and selection with random menu cost



Note: The figure shows the density of the before-shock price-gaps, the price-decrease hazard, and the price-decrease density as a function of gaps in a random menu cost model. The left panel illustrates state dependence, showing that the probability of price decrease is increasing with the gap size. The right panel illustrates the lack of selection, as after an aggregate tightening, which shifts the price-decrease hazard to the left, the new price decreases are dispersed along the price-gap distribution.

Our empirical results are consistent with a random menu cost model with a hazard function that increases linearly between 0 and 1, as illustrated by Figure 19. First, the embodied underlying model implies state dependence: the probability of price adjustment increases with the price gap, as confirmed by the direct impact of the gap on price-change probability in our baseline regression. Second, the linear hazard function is consistent with the close to linear relationship found between price gaps and the probability of price adjustment imposed in our baseline regressions, and confirmed by the non-linear specification reported in Section 6.1 and Figure 9. Third, under a linear hazard function, the aggregate shock generates an increase in the price-decrease probability, which shows up as the direct impact of the aggregate shock in our baseline regression. And fourth, the linear (and quite flat) hazard function generates no detectable selection, because the new adjusters are dispersed over the price-gap space: The increase in the probability of adjustment (distance between the hazard functions) is the same over most of the price gap space – except close to zero and at very high values with very low mass. Therefore, our baseline regression finds no significant impact on the interaction of the aggregate shock and the price gap on the price-adjustment probabilities. This also means that the correlation between new adjusters and the gap (and therefore the size of price adjustment) is zero. As selection is absent, monetary non-neutrality is high, not unlike in time-dependent models.

8 Conclusion

This paper measures the price selection in supermarket and producer-price microdata. There is no evidence for selection: Conditional on aggregate credit or monetary policy shocks, firms do not choose to adjust prices that are further from their optimal levels. This happens despite the fact that unconditionally they adjust prices with large price gaps with higher probability and they also adjust the probability of price adjustments conditional on a shock. The evidence challenges both standard time-dependent (Calvo, 1983) and state-dependent models with high selection (Golosov and Lucas, 2007; Karadi and Reiff, 2019), but is consistent with state-dependent models with random menu costs (Dotsey et al., 1999; Luo and Villar, 2017; Alvarez et al., 2020) with linear adjustment hazard, which predicts high monetary non-neutrality similar to time-dependent models.

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A Appendix

The appendix shows that our baseline results are robust to excluding product-store fixed effects (Table 8), to using posted prices instead of reference prices (Table 9), and to ending the sample in 2007 just before the Great Financial Crisis (Table 10).

Table 8: Robustness to dropping item fixed effects, scanner data, competitors' price gap, credit shock

	(1)	(2)	(3)	(4)
	Increases $\left(I^{+}_{pst,t+24}\right)$		Decreases $\left(I^{-}_{pst,t+24}\right)$	
Gap (x_{pst-1})	-1.75^{***}	-0.99^{***}	1.55^{***}	0.90^{***}
	(0.06)	(0.10)	(0.06)	(0.10)
Shock (ebp_t)	-0.03^{***}	-0.04^{***}	0.03^{***}	0.03^{**}
	(0.01)	(0.01)	(0.01)	(0.01)
Selection ($x_{pst-1}\hat{\text{ebp}}_t$)	-0.00	-0.01	0.01	0.02
	(0.04)	(0.02)	(0.05)	(0.03)
Age (T_{pst-1})	0.02^{***}	-0.01^{**}	0.00^{**}	-0.03^{***}
	(0.00)	(0.01)	(0.00)	(0.00)
Product x store FE	✓	✗	✓	✗
Calendar-month FE	✓	✓	✓	✓
Time FE	✗	✗	✗	✗
N	$16.1M$	$16.1M$	$16.1M$	$16.1M$
Within R^2	18.5%	8.9%	17.3%	9.3%

Note: The table shows estimation results from the linear-probability panel model using scanner data with and without product-store fixed effects. The regressions are run separately on an indicator with value 1 for price increases (columns 1-2) and an indicator with value 1 for pride decreases (columns 3-4). The regressions control for the age (time spent since last change) of the price and use standard errors with two-way clustering. The baseline regressions (columns 1 and 3) show that even though the price gap and the aggregate shock significantly influence the price-change probabilities their interaction remains insignificantly different from zero, suggesting selection is absent. The results stay robust to a specification without product-store fixed effects (columns 2 and 4).
*Standard errors in parentheses; *: significant at 10%, **: significant at 5%, ***: significant at 1%.*

Table 9: Robustness using posted prices, scanner data, competitors' price gap, credit shock

	(1)	(2)	(3)	(4)
	Increases $\left(I^{+}_{pst,t+24}\right)$		Decreases $\left(I^{-}_{pst,t+24}\right)$	
	Reference	Posted	Reference	Posted
Gap (x_{pst-1})	-1.75^{***}	-1.46^{***}	1.55^{***}	1.25^{***}
	(0.06)	(0.05)	(0.06)	(0.05)
Shock (ebp_t)	-0.03^{***}	-0.04^{***}	0.03^{***}	0.03^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Selection ($x_{pst-1}\hat{\text{ebp}}_t$)	-0.00	-0.01	0.01	0.02
	(0.04)	(0.03)	(0.05)	(0.04)
Age (T_{pst-1})	0.02^{***}	0.01^{***}	0.00^{**}	-0.01^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
Product x store FE	✓	✓	✓	✓
Calendar-month FE	✓	✓	✓	✓
Time FE	✗	✗	✗	✗
N	$16.1M$	$18.6M$	$16.1M$	$18.6M$
Within R^2	18.5%	17.6%	17.3%	14.8%

Note: The table shows estimation results from the linear-probability panel model using scanner data with reference and posted prices. The regressions are run separately on an indicator with value 1 for price increases (columns 1-2) and an indicator with value 1 for pride decreases (columns 3-4). The regressions include product-store and calendar-month fixed effects control for the age (time spent since last change) of the price and aggregate variables, and use standard errors with two-way clustering. The baseline regressions (columns 1 and 3) show that even though the price gap and the aggregate shock significantly influence the price-change probabilities their interaction remains insignificantly different from zero, suggesting selection is absent. The results stay robust using posted prices.
*Standard errors in parentheses; *: significant at 10%, **: significant at 5%, ***: significant at 1%.*

Table 10: Robustness to excluding the Great Recession, scanner data, competitors' price gap, credit shock

	(1)	(2)	(3)	(4)
	Increases $\left(I^{+}_{pst,t+24}\right)$		Decreases $\left(I^{-}_{pst,t+24}\right)$	
	2001-2012	2001-2007	2001-2012	2001-2007
Gap (x_{pst-1})	-1.75^{***}	-1.74^{***}	1.55^{***}	1.50^{***}
	(0.06)	(0.07)	(0.06)	(0.06)
Shock (ebp_t)	-0.03^{***}	-0.03^{***}	0.03^{***}	0.02^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Selection ($x_{pst-1}\hat{\text{ebp}}_t$)	-0.00	0.06	0.01	-0.06
	(0.04)	(0.07)	(0.05)	(0.07)
Age (T_{pst-1})	0.02^{***}	0.02^{***}	0.00^{**}	0.01^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
Product x store FE	✓	✓	✓	✓
Calendar-month FE	✓	✓	✓	✓
Time FE	✗	✗	✗	✗
N	$16.1M$	$9.9M$	$16.1M$	$9.9M$
Within R^2	18.5%	17.7%	17.3%	16.5%

Note: The table shows estimation results from the linear-probability panel model using scanner data with reference and posted prices. The regressions are run separately on an indicator with value 1 for price increases (columns 1-2) and an indicator with value 1 for pride decreases (columns 3-4). The regressions include product-store and calendar-month fixed effects control for the age (time spent since last change) of the price and aggregate variables, and use standard errors with two-way clustering. The baseline regressions (columns 1 and 3) show that even though the price gap and the aggregate shock significantly influence the price-change probabilities their interaction remains insignificantly different from zero, suggesting selection is absent. The results stay robust using posted prices.
*Standard errors in parentheses; *: significant at 10%, **: significant at 5%, ***: significant at 1%.*