Scraped Data and Sticky Prices

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Abstract

This paper provides new stylized facts in the price stickiness literature with the introduction of a novel source of micro-price information, called *Scraped Data*. Scraped data are collected every day from online retailers and have a unique advantage in terms of sampling frequency, accessibility, and country availability. Using a dataset with 34 million prices in four Latin American countries, from October 2007 to August 2010, I present patterns of price stickiness yielding three main empirical results. First, the distributions of the size of price changes are bimodal in most countries, with few changes close to zero percent. Second, hazard functions are hump-shaped, increasing for the first 40 to 90 days. Third, there is daily synchronization in the timing of price changes among closely competing goods. These results differ considerably from previous findings in the literature that uses CPI and scanner data, implying a more important role for adjustment costs and strategic interactions in price setting decisions. The availability of daily prices is essential to measure these empirical patterns and explains some of the differences with previous papers.

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1 Introduction

Starting with Bils and Klenow (2004), a large number of papers have studied the pricing decisions of firms and the implications for price stickiness using micro-level prices from both CPI and scanner datasets.[1] Most of the results of this empirical literature have been recently summarized by Klenow and Malin (2009) in a set of ten stylized facts. There is little controversy over these facts because they consistently appear in CPI and scanner data around the world. From this set, three facts can strongly influence how theoretical models are constructed: 1) the distribution of the size of price changes is unimodal, with a large share of small changes, 2) the likelihood (hazard rate) of a price change does not increase with the time since the last price adjustment, and 3) the timing of price changes is largely un-synchronized across sellers. The theoretical implications of these facts are important: a large number of small changes and non-increasing hazards are not consistent with standard state-dependent models with adjustments costs, while the lack of synchronization can affect the response of prices to shocks in both time-dependent and state-dependent pricing models.

This paper shows that these facts can change dramatically with the introduction of a new source of micro-price information, called *Scraped Data*, which has a unique advantage in sampling frequency, accessibility, and country availability. Using daily prices from four countries, I present three main empirical findings: 1) the distribution of the size of price changes is *bimodal* in most countries, with few changes close to zero percent, 2) hazard functions are *hump-shaped*, increasing for the first 40 to 90 days , and 3) there is a *daily* synchronization in the timing of price changes among close substitutes. Compared to previous findings, my results imply a more important role for adjustment costs and strategic interactions in price setting decisions, and are consistent with models that combine elements of time and state dependent pricing, such as Alvarez et al. (2010) and Woodford (2009).

Scraped data are collected from online retailers using a software that scans the underly-

[1] See Nakamura and Steinsson (2008), Klenow and Kryvtsov (2008), Klenow and Willis (2007), Klenow and Kryvtsov (2008), Dhyne et al. (2005), Boivin et al. (2007), Wulfsberg (2008), Gagnon (2007). and the surveys by Mackowiak and Smets (2008) and Klenow and Malin (2009).

ing code in public webpages and records the relevant price information. The resulting data contain daily prices on the full population of products sold by individual retailers, which greatly reduces the chances for measurement errors and other biases. It is more easily accessible than CPI and scanner data, and can be collected in a large number of countries and economic settings. Here, I focus on four Latin American countries where price stickiness has not been extensively studied before: Argentina, Brazil, Chile, and Colombia. The dataset contains a total of 34 million price observations from over 80 thousand individual products, scraped on a daily basis between October 2007 and August 2010.

I first show that the distribution of the size of price changes is bimodal in Argentina, Chile, and Brazil. The bimodality is caused by a drop in the mass of price changes close to zero percent. This effect, which is robust over time, is shown graphically using detailed histograms, and statistically using two non-parametric tests of modality, Hartigan's Dip and Silverman's Bandwidth tests. The lack of mass close to zero percent is consistent with state-dependent mechanisms, which predict that small changes are not optimal in the presence of adjustment costs.

I then use survival analysis to find evidence of hump-shaped hazard functions in individual price adjustments. Hazards measure the probability of a price change conditional on the time passed since the previous change. In this data, aggregate hazard functions are upward-sloping in all countries for the first 40 to 90 days, and then become downward-sloping over time. Given heterogeneity in individual hazards, the downward-sloping portion can be driven by "survivor bias": goods with more upward sloping hazards tend to disappear faster from the aggregate sample, causing the estimated probability of price changes to drop significantly because only long-lasting duration spells remain. I show that this "survivor bias" is indeed affecting my aggregate estimate by separating goods into three levels of rigidity. In all cases, the shape of the hazard function becomes more upward-sloping over time.

Finally, I show that there is daily price change synchronization among closely competing goods. I focus on goods displayed next to each other in a single webpage or "URL", which corresponds to a narrowly-defined category such as "extra-virgin olive oil" or "ground beef". To measure synchronization I develop a simple test of independence based on the binomial distribution. For each day, I count the number of products that adjust their prices at the same time. Under a null of no synchronization, this is a collection of independent Bernoulli random variables that is binomially distributed. Using the number of competing products in each URL and the observed frequencies of simultaneous changes, I obtain the implied probability that is consistent with the binomial distribution. If there is no synchronization, then this probability is constant. By contrast, when there are incentives to synchronize changes, the probability increases when another product changes its price. The rate at which the probability increases with each additional price changes is taken as a measure of the degree of price change synchronization (or departure from the null hypothesis of no-synchronization). Taking the mean across URLs, I find positive degrees of synchronization in every country, both for price increases and decreases.

These results differ considerably from previous findings in the literature. Klenow and Kryvtsov (2008) and Midrigan (2005b) found unimodal distributions in the size of changes, with a large share of small price changes. Klenow and Kryvtsov (2008) and Nakamura and Steinsson (2008) found hazard functions that are either downward-sloping or flat in US data. Although there is no evidence for price change synchronization in CPI data, a few studies such as Lach and Tsiddon (1996) and Midrigan (2005b) found synchronization with grocery-store data for selected categories of goods at weekly or monthly frequencies. This paper goes further, focusing exclusively on closely competing brands of products and finding synchronization on a daily basis.

Most of the differences with the literature are due to the sampling characteristics of scraped data. In CPI and scanner data, prices are often adjusted to reflect changes in the use of coupons and other discounts; while in scanner data, prices are typically computed as weekly averages and "unit values", dividing the sales volume over the number of units sold in a week. Both of these factor can increase the number of small changes observed in the data. Indeed, I show that using weekly averages in my data makes the distributions appear unimodal in all countries.[2] Daily prices are also important to observe the initial increase in hazard rate for the first two or three months. In adition, high-frequency data and the fact that I can observe prices for all products in each retailer allows me to measure daily synchronization only between goods that are closely competing with each other in narrowly-defined categories.

There are other possible reasons for the differences with the literature, but they appear to be less important. First, differences in inflation levels could affect some the stylized facts, but the main results are still present in Chile, where inflation is comparable to that of the US and Europe during periods analyzed by the literature. Second, *online* prices do not appear to have any special dynamics that could alter the stylized patterns in the data. Using a survey of offline data collected in physical stores, I show that there are no significant differences in the timing and size of changes for online and offline prices in these retailers. To further test the representativeness of online data, I also show that the data can match country-level trends in inflation: simple price indexes constructed using online scraped data can closely track official CPI statistics in Brazil, Chile and Colombia.[3]

The paper is organized as follows. In section 2, I describe the collection methodology and characteristics of scraped data. In section 3, I present results for the distribution of the size of changes, the hazard functions, and price synchronization. In section 4, I address concerns about the representativeness of *online* data. Section 5 concludes.

[2]Bimodality is still present in my data when I use a monthly sampling that replicates some characteristics of CPI data. However, the BLS CPI manual describes adjustments that include corrections for coupons, seasonal items, and hedonics that could explain many of the small changes that appear in the data.

[3]Argentina is the only country where scraped data inflation is significantly different from official estimates, with a 17.1% average annual inflation over the whole period versus the official estimate of 7.6%. This does not mean that scraped data are biased, but quite the opposite: official statistics have become widely discredited in Argentina since 2007, when the government intervened the National Statistical Institute. See Cavallo (2010)

2 Scraped Data

2.1 The Scraping Methodology

A large and growing share of retail prices are being posted online all over the world. Retailers show these prices either to sell online or to advertise prices for potential offline customers. This source of data provides an important opportunity for economists wanting to study price dynamics, yet it has been largely untapped because the information is widely dispersed among thousands of webpages and retailers. Furthermore, there is no historical record of these prices, so they need to be continually collected over time.

The technology to periodically record online prices on a large scale is only now becoming available. Using a combination of web programming languages, I built an automated procedure that scans the code of publicly available webpages, identifies each relevant piece of information, and stores the data in an electronic file. This technique is commonly called "web scraping", so I will use the term *Scraped Data*.

The scraping methodology works in 3 steps: First, at a fixed time each day, a software downloads all public web-pages where product and price information are shown. These pages are individually retrieved using the same URL or web-address every day. Second, the underlying code is analyzed to locate each piece of relevant information. This is done by using custom characters in the code that identify the start and end of each variable, according to the format of that particular page and supermarket. For example, prices are usually shown with a dollar sign in front of them and two decimal digits at the end. This set of characters can be used by the scraping software to identify and record the price every day. Third, the scraped variables are stored in a panel database, which contains one product record per day. Along with the price and product characteristics, retailers show an id for each product in the page's code (typically not visible when the page is displayed to the customer), which allows us to uniquely identify each product over time.

2.2 Comparing Data Sources

The differences between scraped data and the two other sources of price information commonly used in studies of price dynamics, CPI and Scanner Data, are summarized in Table 2.

Scraped datasets do have some disadvantages. First, they typically cover a much smaller set of retailers and product categories than CPI prices. This limitation will recede over time, as a growing number of firms start posting their prices online. It is not a major issue for this paper because supermarket products represent over 40% of all CPI expenditure weights in these four Latin American countries. Second, scraped data do not include information on quantities sold, as scanner datasets typically do. In the context of measuring stickiness, quantities can be used to measure elasticities or determine category weights in frequency statistics, but they are not needed to study the stylized facts discussed in this paper.

On the other hand, scraped data have important advantages that make them a unique source of information. First, these datasets contain *daily* prices, which can greatly reduce measurement error biases in some cases, as is later shown in this paper. Second, the data are available for a much larger set of countries. In this paper, I focus on *developing* countries, where scanner data are scarce and product-level CPI prices are seldom disclosed.[4] Third, scraped data contain detailed information on the full array of a retailer's products. In particular, the ability to identify products displayed next to each other plays a key role in measuring price synchronization among close substitutes. Fifth, there are no *forced* item substitution, which occur frequently in official statistics to measure inflation in out-of-stock, seasonal or discontinued products.[5] Sixth, scraped datasets are directly comparable across countries,

[4]The study of stickiness in developing countries is rare in the literature. A recent exception is Gagnon (2007), who provides a detailed analysis of sticky prices in Mexico using disaggregated CPI data.

[5]Forced substitutions occur in CPI data when the agent surveying prices does not find the item she was looking for, and decides to replace it with another product, which becomes the surveyed item from then on. In practice, if the old item is supplied again and/or the new product was being supplied before, official statistics ignore their prices, effectively censoring the price series. By contrast, in scraped data, prices are recorded from the first moment they enter the sample until the last day they have been offered to consumers, which solves substitutions for items that go temporarily out of stock. I do not attempt to link price series of goods that are discontinued with those of similar goods that may replace them, but such substitutions could

with prices on the same categories of goods and time periods. This makes it possible to perform simultaneous cross-country analyses. Finally, scraped data are available on a real-time basis, without any delays to access the information. This can be used to provide estimates of stickiness that quickly capture changes in the underlying economic conditions.

2.3 The Data in this Paper

I use a dataset with more than 34 million supermarket prices in Argentina, Brazil, Chile, and Colombia. The data come from the online price tables of four different retailers, one in each country, from October 2007 to August 2010.

All the supermarkets included in the dataset are market leaders, with market shares of approximately 28% in Argentina, 15% in Brazil, 27% in Chile, and 30% in Colombia. With hundreds of physical stores, they also sell online in cities such as Buenos Aires, Santiago, Rio de Janeiro, and Bogotá. Every day, for nearly three years, I accessed these websites and recorded all this information for every good on display. Because buyers cannot physically see the products, these retailers make an effort to display detailed information on each item, including a price, the product's identification number (id), name, brand, package size, category, and whether it is on sale or under price control.

Table 1 provides details on each country's database. There are roughly 18,000 daily prices for each country in Argentina, Chile, Brazil, and 5,000 in Colombia. The initial date for each database differs by a few days around October 2007, but they all end on October 12th 2008. To compare results for the same product categories across countries, I matched each supermarket's classifications into 95 standardized categories containing a large variety of foods and household items.[6] Products can be further classified into "pages". A page is an URL or web address for a list of competing products, corresponding to the narrowest grouping of items in each supermarket.

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be potentially achieved in this data using product descriptions.

[6]See Table A4 in the Appendix for a complete list of product categories. These are based on the ELI classification used by the US Bureau of Labor Statistics.

Tables 3 to 5 present general price change statistics are important to interpret the results in later sections. In particular, I focus on inflation rates and measures of price stickiness. First, I measure inflation in all countries using simple price indexes with online scraped data.[7] Argentina has the highest inflation by far, with an average annual rate of 17.1% over this period, followed by Brazil at 5.1%, Colombia at 4.2%, and Chile at 2.7%. Second, as a measure of price stickiness, I computed the median frequency and implied durations following the methodology in Bils and Klenow (2004). Results are shown in Table 4. On one extreme, prices are stickiest in Chile and Argentina, with the daily median frequency of 0.015 and an implied duration of 66 days. They are followed by Colombia, with a median frequency of 0.019 and an implied duration of 52 days. Finally, Brazil is the most flexible country, with a median frequency of 0.027 and an implied median duration of 36 days. [8]

3 Re-evaluating Three Stylized Facts

3.1 Standard Models and Empirical Literature

Before presenting the empirical results, I briefly discuss the standard models in the literature. Many microeconomic mechanisms have been proposed to explain why prices are sticky, but most of them can be broadly classified into either time-dependent or state-dependent pricing behaviors.

In time-dependent pricing (TDP) models, the decision to adjust prices is driven by time. In standard models, the timing is exogenous and adjustment can occur after a fixed number

[7] Section 4 compares these findings with official statistics and provides more details on the use of daily online indexes.

[8] In principle, we would expect price stickiness and inflation to be negatively correlated across countries. That is the case for Chile, Colombia, and Brazil, where less stickiness leads to progressively higher levels of inflation. However, Argentina breaks this pattern completely. It is both the stickiest country (shared with Chile) and the one with the highest inflation rate by far. It would appear that low frequency of changes is compensated with a large mean size of changes, as seen in Table 5. However, the size of price increases and decreases, when computed separately, is not larger than in the other countries. The reason the mean size of changes is so large in Argentina is that there are more increases than decreases, as can be seen in Table 5. The last rows in Table 4 suggest that what is important for inflation is not the overall level of stickiness, but the relative stickiness of price increases over price decreases.

of periods, as in Taylor, 1980, or randomly every period, as in Calvo, 1983. More recent versions of these models can endogenously generate the timing of adjustment with imperfect information and observations costs. In all cases, the price stickiness (and therefore the real effects of monetary policy) comes from the fact that firms are not constantly monitoring their prices, which is done only at specific times.

In state-dependent pricing (SDP) models, by contrast, the decision to change prices depends directly on how far the current price is from the optimal price. Early examples include the menu cost models of Barro (1972) and Sheshinski and Weiss (1977), and more recently Dotsey et al. (1999) and Golosov and Lucas (2007). In these models, firms are able to change their prices at any time, but must pay an adjustment cost to do so ("menu cost"). [9] Whether a firm changes its price or not depends on whether the benefits from adjustment (given by the curvature of the profit function) are greater than the menu costs.

A more recent strand of the literature combines TDP and SDP mechanisms using a mixture of information and adjustment costs. Examples include Alvarez et al. (2010), Woodford (2009), and Bonomo et al. (2010). These are imperfect information models where firms must pay an information-gathering cost to compute their optimal price, and then pay an additional menu cost if they decide to adjust the current price. If the information cost is relatively high, firms will prefer a time-dependent rule. If, by contrast, the menu cost is relatively more important, firms will tend to have a state-dependent pricing rule.

Over time, the empirical literature has produced a set of stylized facts that can be used to test models. Most of them where found first in the US CPI data and later confirmed with European CPI and US scanner datasets. Klenow and Malin (2009) provide ten stylized facts that are becoming part of the conventional wisdom on price stickiness. In this paper, I will

[9] "Menu costs" may include various kinds of adjustment costs, such as labor costs to stamp new prices, managerial costs to make a decision, or even "customer anger" costs linked to the consumer's reaction after a price adjustment. An example of models that include the latter are "Fair Pricing" models, such as Rotemberg (2005), Rotemberg (2008), and L'Huillier (2009). These are based on the idea that prices are sticky because firms do not want to antagonize customers. Blinder et al. (1998) found in a survey of price setters that this was a major concern for firms setting prices in the US. I explore some evidence for this type of mechanisms in the Appendix, using data from Argentina.

re-evaluate the robustness of three: First, that there are many small price changes; second, that the hazard rate of price changes does not increase with the age of the price; third, that the timing of price changes is little synchronized across sellers. I focus on these three facts because standard models have sharp predictions about them, so they can greatly influence the way models are built in the future.

3.2 Bimodal Distributions of the Size of Changes

Different theories of price stickiness have different implications for the size of price changes. Standard SDP models, such as the menu-cost model of Golosov and Lucas (2007), predict *bimodal* distributions with relatively few changes close to zero percent. The intuition is that small price changes are not optimal once the menu costs are taken into account. That is, the benefit from correcting small deviations from the optimum price may not be enough to cover the adjustment cost. In the end, what the menu cost does is to create a dip in the distribution of changes around zero percent. By contrast, in standard TDP models such as Calvo (1983), any size of price change is possible. The timing of the adjustment is not driven by how far the current price is from the optimum. Therefore, when the time comes to adjust, firms will make all price changes, regardless of their size, so that the predicted distribution of sizes tends to be unimodal.[10]

Many papers in the empirical literature have looked at the distribution of price changes using CPI and Scanner data. Examples include Kashyap (1995), Midrigan (2005b), Klenow and Willis (2007), Baudry et al. (2007), and Kackmeister (2007). In most cases, the distributions where found to be unimodal and centered at zero percent, with a large number of small changes. This has shaped the theoretical literature considerably. For example, Midrigan (2005b) builds a SDP model with economies of scope in menu costs that can generate

[10]In fact, the distribution of the size of changes will inherit the properties of the distribution of shock to marginal costs. If shocks are approximately normal, the distribution of changes will be *unimodal* with a mode that depends on the overall level of inflation. If inflation is low, the mode is close to zero percent, and there are many small price changes. If inflation is high, the shape would still be unimodal but the mode would shift to some positive value given by the average level of inflation.

small price changes, while unimodality contributes to the conclusion by Woodford (2009) that the predictions in Calvo's time-dependent model are more reliable than what has often been suggested. Indeed, the existence of small price changes has key implications for the parametrization of recent models like Woodford (2009) and Alvarez et al. (2010).

Scraped data can be used to examine the distribution of the size of changes in much greater detail. The daily nature of the data means that there are between 108K and 366K observed price changes in each retailer, which allows me to focus on what happens with the mass of price changes close to zero percent, by using histograms with very narrow bins (0.1%) in Figures 1 and 2. These distributions are conditional on a price change (no mass at zero), and are truncated at an absolute value of 50% to facilitate the graphical analysis.

The most striking feature for most distributions is their bimodality, with a sharp dip of mass close to zero percent. This happens in Argentina, Brazil, and Chile. The effect can be seen clearly in the smoothed kernel densities shown for each graph. Table 5 presents some statistics that emphasize the lack of small changes. The share of changes below 1% in absolute value is only 4.2% in Argentina, 4.3% in Brazil, and 3.6% in Chile, significantly lower than the 11.3% reported by Klenow and Kryvtsov (2008) using US CPI data. This bimodality provides evidence of the existence of adjustment costs in price changes, and is consistent with the predictions of SDP models like Golosov and Lucas (2007), and also mixed imperfect information models like Alvarez et al. (2010).[11]

The only case where the distribution appears unimodal is Colombia, where 7% of price changes are smaller than 1% in absolute value. The shape of the distribution is roughly unimodal and centered at 0%. In fact, this shape closely resembles the findings in previous papers in the literature, not only with a large share of small changes, but also relatively fat tails. The unimodality could be an indication of time-dependent pricing in this supermarket. It is also consistent with menu costs under certain conditions. For example, there could be

[11] An alternative explanation for the dip, independent of any shocks, is that many of these changes are connected to sale events, and *small* sales may be unattractive to customers and ineffective for the store to use. However, the *dip* in the distribution does not disappear once sales are excluded, as shown in Figure 6.

different menu costs for different goods (as in Dotsey et al., 1999), one menu cost for a large number of goods (as in Midrigan, 2005b), or simply smaller menu costs for online pricing.

The shape of these distribution in all countries is robust over time. In Figure 3, I plot three distributions for each country, one for each year of data. In all cases, the number of major modes remains the same.

This graphical analysis can be formalized with the use of non-parametric modality tests. Using non-parametric methods is essential to avoid having to make ex-ante assumptions on the number of possible modes. In the statistics literature, two tests are commonly used to determine modality: Hartigan's Dip and Siverman's Bandswidth tests. These tests are useful to obtain a measure of the departure from unimodality and show that, not only graphically but also statistically, the distributions in Figures 1 and 2 do not have a single mode at zero percent.

Hartigan's Dip is a test of unimodality. It relies on the fact that the cumulative distribution function of a density function f with a single mode at m_f is convex on the interval $(-\infty, m_f)$ and concave on the interval (m_f, ∞).[12] In other words, at the left side of the mode, the density is non decreasing, while the opposite occurs at the left of the mode. With this insight, one can find the unimodal distibution that minimizes the difference with the observed empirical distribution. This difference is measured by the dip statistic, which can be used as a sort of "score" to measure the departure from unimodality. Therefore, positive dip values provide evidence to reject the null hypothesis of unimodality. To determine the statistical significance of a positive dip, Hartigan and Hartigan (1985) sets the null hypothesis equal to the uniform distribution, for which, asymptotically, the dip value is stochastically largest among all unimodal distributions.[13]

Silverman's Bandwidth method can be used to test for multiple modes. It uses the non-parametric smoothed kernel density to evaluate the number of modes in an empirical

[12] See Hartigan and Hartigan (1985)

[13] Hartigan and Hartigan (1985) also show that this is not always the case with small samples. To address this concern, I use a calibration of the dip test proposed by **?**.

distribution. The basic insight in Silverman (1981) is that the larger the smoothing applied, the fewer the number of modes in the estimated density. So for the null hypothesis of unimodality, he proposed using as a test statistic the minimum smoothing required for the density to have a single mode. Large values of this statistic (the "critical bandwidth") are evidence against the null hypothesis of unimodality, because they mean that larger degrees of smoothing are needed to eliminate additional modes in the density estimate. The statistical significance of the score can be evaluated using a smoothed bootstrap method.[14]

Table 6 reports the results for both tests in all countries. The dip score, shown in column one, is consistent with the graphical analysis: Argentina has the largest departure from unimodality, while Colombia the smallest. Statistically, we can reject the null hypothesis of unimodality in all countries. Silverman's test provides similar results: the critical bandwidth is largest in Argentina and smallest in Colombia. We can also reject the null of unimodality in every country (for Colombia at a higher statistical significance of 3%).[15]

3.2.1 Differences with the Literature

As mentioned before, the bimodality in the size of price changes is at odds with previous findings in the literature.

The differences with papers that use scanner data can be fully explained by the use of daily data. In scanner datasets, prices are typically constructed as "unit values", taking the ratio between revenues and quantities sold for a product during a period of time (usually a week). This means that prices are being averaged along two dimensions. First, at the same point in time there may be different prices for different units sold of the same product, because

[14]See Henderson et al. (2008) for more details for both statistical tests.

[15]Note that with Silverman's test we stop rejecting in Colombia when there are two or less modes, and in the other countries when there are three or less modes. In other words, the test suggests there is bimodality in Colombia and three or more modes in the other countries. The reason is that this test is sensitive to tiny bumps in the distribution, a problem that is derived from the use of a single bandwidth in the kernel smoothing estimates. This leads to frequent rejections of the null hypothesis in large samples. Another issue, which applies to both tests, is that they do not tell us if the rejection of unimodality is caused by what happens around zero percent (the focus of the sticky price literature). In a related paper with Roberto Rigobon, we propose an alternative modality test that solves both issues by ignoring tiny bumps in the distribution and focuses on the relative mass close to zero percent.

consumers sometimes purchase products with coupons, loyalty cards, or in bundles. This can create additional price changes in the data. Second, because scanner data are reported on a weekly basis, prices are also averaged over time. As Campbell and Eden (2005) pointed out, this can make one large price change appear like two smaller ones. For example, consider a three-week period with a single price change in the middle of the second week. Computing weekly averages would yield three different prices, one for each week, and two small price changes instead of a single larger change. I can test this by looking at the effects of weekly averages in my own data, as shown in Figure 4. Using weekly averages greatly increases the number of small price changes and the bimodality disappears (or is hardly visible) in every country.

The differences with CPI results are harder to reconcile. Monthly-sampled prices could potentially change the distribution of sizes by aggregating price changes over time. Yet the effect can have opposite implications depending on the nature and persistence of price changes. In a low inflation context with many temporary shocks, several price changes that go in opposite directions could end up looking like a single small change in monthly data. In a high inflation context, the opposite would be true: persistent increases in prices could be accumulated over a month and look like a single larger change. I simulated the effects of monthly sampling with scraped data by randomly selecting a price for each product every month and re-calculating the distributions, but Figure 5 shows that monthly sampling has no impact at all in the number of modes in my data.

A more likely explanation for the differences with CPI data is directly related to the way official prices are recorded. In the US, for example, the BLS Handbook of Methods[16] describes several adjustments in individual prices that can affect the distribution of the size of changes. First, changes in a price spell can occur because of *forced item substitutions* that happen when an item can no longer be found in a store. In these cases, the BLS estimates a price change using the average price change for that category of products or

[16]See , Chapter 17, pages 30 to 33.

using hedonic quality adjustments. Second, even when no product substitutions occur, the BLS sometimes imputes prices that are considered to be temporarily missing, like seasonal items. Third, individual prices can also be adjusted for coupons, rebates, loyalty cards, bonus merchandize, and quantity discounts, depending on the share of sales volume that had these discounts during the collection period. Finally, some food items that are sold on a unit basis –like apples– are sometimes weighted in pairs to calculate an average-weight price. Unfortunately, at this stage I do not have access to CPI prices to know how frequent these changes really are in the data.[17]

3.3 Hump-Shaped and Upward-Sloping Hazards

A second stylized fact that has received a lot of attention in the empirical literature is the shape of the hazard function, because it can also be used to distinguish between models. The hazard is the instantaneous probability of price change at time t, conditional on the price not changing until that point in time. In Calvo (1983)'s TDP model, the hazard function is flat because the probability of price change is fixed and exogenously determined. In Taylor (1980)'s model, the hazard is equal to one at the time when all price changes take place (eg. a month). With heterogeneity across goods, this can be generalized to have hazards with "spikes" at given frequencies.

By contrast, in SDP models hazard functions tend to be upward-sloping. The intuition is that shocks increase deviations from the optimal price over time, so as the price gets "older", the conditional probability of a price change also rises. Upward-sloping hazards are intuitively appealing, but there is no evidence for them in the current empirical literature. Nakamura and Steinsson (2008) found evidence of downward sloping hazards in US CPI prices, while

[17]There are other possible explanations for the differences with both CPI and scanner data. First, the countries studied here have higher inflation levels. Inflation could be moving the mass of price changes away from zero, simply because the marginal cost shocks experienced by the firms are larger. However, this does not appear to be an important explanation because I find bimodality in both high-inflation Argentina and low-inflation Chile (see the critical bandwidth scores in Table 6). In addition, the average size of price increases and decreases does not vary much across countries, as can be seen in Table 5, so it is hard to argue that inflation moves prices further away from zero. Second, this paper uses online prices that could behave differently from offline prices. In section 4, I use a survey of offline prices to show that this is not the case.

Klenow and Kryvtsov (2008) found mostly flat hazard functions in similar data.

Scraped data has some advantages for the study of hazards rates. First, we can look at hazards in countries with higher inflation that the US or Europe. In contexts where aggregate shocks are strong and persistent, it should be easier to find evidence of upward-sloping hazards. Second, we can see how the probability of change varies on a daily basis, which is important when most goods adjust within a few months.

I measure hazard rates using standard Survival Analysis, which studies the time elapsed from the "onset of risk" until the occurrence of a "failure" event. In a price-setting context, we are interested in the time between the firm's optimal price adjustments. The set of constant prices between these two dates is called a "price spell" and the duration (measured in days) is the length of the spell.

To estimate hazards, I use a non-parametric approach due to Nelson (1972) and Aalen (1978), which does not require any distributional assumptions.[18] It provides a simple estimate of the cumulative hazard function $H(t)$, given by:

$$\widehat{H}(t) = \sum_{j|t_j \leqslant t} \frac{c_j}{n_j} \tag{1}$$

where c_j is the number of price changes at time t_j and n_j is the number of price spells that can still change at time t_j. The incremental steps c_j/n_j are an estimate for the probability of price change at t_j, taking into account only those price spells that have survived until that point in time.

To obtain the smoothed hazard function $\widehat{h}(t)$, I take the discrete changes in $\widehat{H}(t)$ and weight them using a kernel function:

$$\widehat{h}(t) = \frac{1}{b} \sum_{j \in D} K\left(\frac{t - t_j}{b}\right) \Delta \widehat{H}(t_j) \tag{2}$$

[18] I choose this method because I want to study the *shape* of the hazard function $h(t)$, not the effects of any covariates. My results are robust to the use of a semi-parametric Cox model that can incorporate covariates and account for unobserved heterogeneity at the category level.

where K is a symmetric kernel density, b is the smoothing bandwidth, and D is the set of times with price changes. Following the literature, I implicitly assume that each price change restores the optimum price and treat all duration spells independently. I include right-censored spells, because we know for certain how old they are at each point in time, affecting n_j in equation 1. However, I exclude left-censored spells, for which the time since the last adjustment is unknown. I further exclude sale prices in Argentina, Brazil, and Colombia. Sale events are likely to have special dynamics and have short durations that can increase survival biases, as explained below.[19] Finally, spells of all durations are used to construct hazard estimates, but since only a small fraction of spells lasts more than six months as shown in Figure 7, I focus the discussion on shape of the hazard functions during the first six months.[20]

Figure 8 plots the estimated hazard with 99% confidence intervals, with a y-axis scale matched to facilitate comparisons. In all countries, the aggregate hazard function has a hump-shaped pattern. For a short period of time, lasting from 1 to 3 months, the probability of price change increases with the age of the price spell.[21] In Argentina, the hazard reaches its maximum at approximately 90 days. In Chile, this occurs sooner, at 40 days (likely affected by the inclusion of sales, which tend to have very short durations). Both Brazil and Colombia have peaks at 60 days.

These hump-shaped patterns do not fit standard TDP or SDP models. However, they could potentially be explained with TDP models if there are multiple firms adjusting at different times, with a majority of goods doing so at 40, 60, or 90 days. They could also be explained with SDP models when temporary shocks are relatively important, as Nakamura and Steinsson (2008) point out, because these shocks would cause a reversal of the adjustment

[19] The median duration of sale prices is extremely short, with 6, 7 and 17 days in Argentina, Brazil, and Colombia respectively. Hazards including sales are shown in the Appendix. Sales significantly increase the hazard rate at durations with one and two weeks, although hazard functions continue to be hump-shaped.

[20] Hazards are either flat of downward sloping after six months, but there are few spells on which to base the analysis. See the Appendix for the full three-year hazard functions.

[21] Also, the overall level of each hazard function is consistent with the fact that prices more flexible in Brazil and Colombia, and stickier in Chile and Argentina.

within a short period of time.

The evidence for SDP is reinforced by the fact that Argentina, with its high-inflation rates, has an upward-sloping hazard for a longer period of time. Even in standard SDP models, the higher the inflation rate, the more upward-sloping hazards become, because the deviation from the optimal price increases over time.

The methodology makes it hard to find upward-sloping functions because I am not correcting for the "survival" bias caused by heterogeneity across products in the shape of individual hazards. This bias is illustrated in Figure 9 with a hypothetical example. Consider two types of goods with upward sloping hazards. One type changes prices more frequently, so it has higher hazard rates and will disappear from the sample faster. If we estimate the aggregate hazard for both goods, initially we would be using spells from both of them, but at some point in time we would start using only spells from goods with the lower hazard rates. This"survival" bias would tend to flatten the estimate, creating hump-shaped results. This is a well-know result in survival analysis.

I find evidence of the existence of survival bias in Figure 10, where I separate goods in terms of their average durations and re-estimated their hazard functions. The dotted line represents goods that have average durations of less than 50 days, the dashed line is for goods with average durations of 50 to 100 days, and the solid line represent stickier goods with average durations over 100 days. The patterns are indeed very similar to the example in Figure 9. The more flexible goods still have hump-shaped patterns, but the confidence interval widens as the hazard becomes flat, reflecting the fact that there are fewer spells with which to obtain an estimate. Furthermore, as we separate goods into different categories, each one of these hazards became more consistently upward sloping.[22]. The hump-shaped patterns does not disappear completely because each one of these three hazards is itself constructed by aggregating across many goods, and therefore they are still affected by

[22]This is not caused by the peak moving further to the left, which naturally occurs with stickier goods if the hazards are really hump-shaped. The interesting finding is that hazards for all categories become closer to straight upward- sloping lines as we separate them into different categories

survivor biases. Completely controlling for heterogeneity at the individual-good level requires far more price changes per product that I currently have, but these results are an indication that the underlying hazard rates tend to be far more upward sloping than what the aggregate estimate is showing. As such, my results should be taken as a lower-bound indication for the existence of upward-sloping hazards.

Once again, my results differ considerably from previous findings in the literature, where hazards are typically flat or downward sloping hazards. One possible reason for this difference is that these scraped prices come from higher inflation economies, were SDP models predict more upward-sloping hazards. In principle, if there is some sort of state-dependent pricing behavior, it should be easier to find upward-sloping hazards in a country like Argentina. However, a major reason for the difference is related to the use of daily data. At least for this sample of products, where the mean duration is just a few months, having daily data is essential to obtain the upward sloping portion of the aggregate hazards.

3.4 Synchronization of Price Changes

A third stylized fact that has received attention in the literature is the degree of synchronization in the timing of price changes. In TDP models, higher synchronization reduces the persistence of the real effects of monetary policy. In SDP models, synchronization is closely linked to *strategic complementarities*. Firms selling strategic complements will imitate each other's actions by attempting to synchronize the timing of their price changes.[23]

Synchronization with product-level price data has been studied before in the macro literature, but at lower sampling frequencies, with smaller samples and broader categories of

[23] See Cooper and Haltiwanger (1996) for a general discussion of how agents have an incentive to synchronize discrete decisions under strategic complementarities. Strategic complementarities have been introduced in state-dependent models as a form of *real rigidity*, in the spirit of Ball and Romer (1990), to increase the real effects of nominal frictions. The intuition is that while some firms are free to adjust their prices, they may decide to wait until competitors react to the shock. The fact that some firms have not yet adjusted (due to a nominal friction like menu costs), could make other firms delay their own price changes (a real rigidity). See Klenow and Willis, 2006 and Burstein and Hellwig, 2007. See Midrigan (2005b) for results on synchronization with scanner data and Neiman (2008) for evidence of synchronization in international trade data.

goods. For example, Lach and Tsiddon (1996) found evidence of monthly within-store synchronization for wines and meat products in Israel, while Midrigan (2005b) found weekly synchronization with scanner data in the US. To the best of my knowledge, no paper has been able to study synchronization on a *daily* basis and focusing only on goods that are close competitors.

Scraped data are especially well suited to find close competitors because a URL indicator is available to identify products displayed next to each other. In addition, the daily nature of the data is key for price interactions in high-inflation countries because, as Lach and Tsiddon (1996) noted, a sufficiently long sampling interval would ensure that all prices appear to change simultaneously regardless of the degree of synchronization.

To focus on synchronization between competing firms, I consider only one product *per brand* in each URL. This eliminates simultaneous price changes caused by the same good with different package sizes and flavors, or different goods sold by the same firm under a single brand.[24]

3.4.1 A Non-Parametric Test of Synchronization

To measure the degree of synchronization in each URL, I propose a simple method based on the binomial distribution.[25] I start by looking at Y_{jt}, the number of products that change their price in URL j on day t:

$$Y_{jt} = \sum_i X_{ijt} \tag{3}$$

X_{ijt} is a binary indicator equal to one if good i changed its price at time t. Let $P_{ijt} = Pr(X_{ijt} = 1)$ be the probability that the price of that product changes that day. Then X_{ijt} is

[24] For each brand, I keep the product with the largest number of price observations available. The results are qualitatively robust to a random selection criteria per brand, or the inclusion of all products in an URL. This sample still includes products from the same manufacturer that are sold under different brands (within the same URL). Unfortunately, there is no manufacturer information for individual products or simple ways to link brands to manufacturers.

[25] Similar results can be with the Fisher-Konieczny index (Fischer and Konieczny, 2000).

a Bernoulli random variable, with success probability p_{ijt}. Assuming all products in an URL are identically distributed with a constant probability of price change, then $p_{ijt} = p_j, \forall i, t$.

If there is no synchronization in price changes, then X_{ijt} is independent across products, and Y_{jt} is distributed as a $Binomial(N_j, p_j)$, where N_j is the number of products in the URL. Therefore, to determine whether prices are synchronized or not, we can observe the distribution of Y_{jt} in each URL and compare it to the binomial distribution. This is done by computing the *implied probabilities* under the assumption of a binomial distribution. That is, given the number of products N_j in a particular URL, we can find the individual probability p_j that would generate the observed frequency of simultaneous changes under the assumption of a binomial distribution. If price changes were really independent, then these implied probabilities would be constant; however, if there are incentives to synchronize changes, the implied probabilities would increase with the number of items adjusting at the same time.

To illustrate this methodology, I use the "Rice" URL in each country as an example. First, I compute the distribution Y_{jt} in Figure 11, by plotting the fraction of days with a given number of synchronized price changes. For example, the value at two (e.g. 0.046 for Argentina) indicates the fraction of days where only two products in that URL changed their price, or $Y_{jt} = 2$.

Second, under the hypothesis of a binomial distribution, I calculate the implied probabilities. For example, since there are 25 products in the "Rice" URL for Argentina, when $Y_{jt} = 2$ the implied probability p solves the equation:

$$\Pr[Y_{jt} = 2] = 0.046 = \begin{pmatrix} 25 \\ 2 \end{pmatrix} p^2 (1-p)^{25-2} \tag{4}$$

In this case, p is equal to 0.0145. The same calculation is repeated for all values of Y_{jt}, up to $Y_{jt} = 10$.[26]

Figure 12 plots the implied probabilities for the "Rice" URL in all countries. In all cases, the probabilities increase with the number of simultaneous price changes, consistent with synchronization.

For a single URL, we can measure the degree of synchronization by fitting a linear trend and obtaining the slope of implied probabilities. The higher the slope, the larger the deviation from the binomial distribution and, therefore, the stronger the synchronization.

We can further generalize the analysis and average all URL slope coefficients to get a country-level measure of synchronization.[27]

Table 7 shows high levels of synchronization in all countries. The average slope of implied probabilities is 0.008 in Argentina, 0.007 in Brazil, 0.007 in Chile, and 0.012 in Colombia. Compared to the median frequencies (the *unconditional* probabilities of daily price change reported in Table 4), these coefficients imply that the probability of price change increases by 63% in Argentina, 25% in Brazil, 46% in Chile, and 63% in Colombia every time an additional price change occurs at the same time.

Table 7 also shows that synchronization is not affected by the exclusion of sales. There is, however, a large difference between price increases and decreases, when considered separately. In Argentina, Brazil, and Colombia, price increases are more synchronized than price decreases.

There are several possible causes for price change synchronization within URL. Prices could be driven by a common sectoral shock affecting the URL. Even with no common shocks, supermarkets may choose to change the prices of many similar products at the same time to save on adjustment costs. For example, when there is a fixed cost to walk to an URL and manually change prices (or connect to a database and input the new values) but low

[26]To obtain a unique solution, I solve for p in:

$$\frac{\Pr[Y_{jt} = k]}{\Pr[Y_{jt} = 0]} = \binom{N_j}{k} \left(\frac{p}{1-p}\right)^k, \ \forall k \in [1, 10]$$

[27]Only urls with at least 3 products are considered. In addition, urls with slope coefficients that are not statistically significant in a 95% confidence interval are assumed to have no synchronization.

marginal costs to change the price of additional items within the URL. This is the case of increasing returns to scale in adjustment costs, studied by Midrigan (2005b). In addition, if there are strategic complementarities across products, firms will try to match the timing of each other's price changes.

4 Online vs Offline Prices

In the previous section, I have used scraped data to challenge some commonly-held views in the empirical sticky-price literature. However, a common concern with scraped data is that they may not be representative of a country's pricing behaviors, because online purchases are still a small share of transactions in most countries. In this section, I address this concern in two parts. I first consider whether online and offline prices behave similarly for each retailer. I then examine whether these supermarkets are representative of each country's aggregate inflation trends.

4.0.2 Matching Offline Price Behaviors

Between December 2008 and February 2009, I conducted simultaneous surveys of *offline* and *online* prices in all the supermarkets where I collect the data. These surveys took place in Buenos Aires, Santiago, Rio de Janeiro, and Bogotá, with the help of four local volunteers. They were asked to select any branch of the supermarket and randomly buy 100 products, divided in 10 pre-defined categories. These categories were chosen to ensure some variety in the type of goods purchased: Dairy, Bakery, Beverages, Cereal and Flours, Fats and Oils, Meats, Pasta and Rice, Fruits and Vegetables, Cleaning Products, and Bath Products. After the first purchase, we checked which of these random products were also being sold online by comparing product ids and descriptions.[28] Those items that could not be matched to the online database were removed from the product list for subsequent purchases. In total, four

[28] In Argentina, Brazil, and Colombia, the matching was based exclusively on product ids. In Chile the matching was based on the item's name, description, and package size.

purchases took place in each supermarket, at 15-day intervals, always in the same branch. The same items were bought every time, with identical flavors and package sizes. If a product was out of stock, no price was recorded for that day, but we attempted to buy the product again in subsequent purchases.

Table 9 shows the results from this validation exercise. The percentage of offline products that were also available online is high in all countries. It ranges from 74% in Colombia to 100% in Argentina. Most of the products that could not be matched are raw-food items, which tend to be re-packaged for online sales and have different id numbers and descriptions.

I then compare prices both in terms of their *levels* and the timing and size of *changes*. Even though price levels are not always the same across samples, online and offline price *changes* behave similarly in terms of timing and size of adjustments in all countries.

In Chile, the matching of price levels is extremely close. 361 out of 388 comparable prices were exactly the same. The 27 price discrepancies, which averaged 2% in size, were concentrated in only 12 goods (mostly raw-food products), so that 89% of products have identical price levels across samples. This ensures that price changes behave almost identically across samples.

In Argentina, price levels are typically higher online: 252 out of 323 comparable prices were higher in the scraped database. Yet in nearly every case, there was a difference of 5% across samples. This constant markup means that price *changes* are highly correlated: 93% of products that have identical price *change* series (defined as an indicator variable, conditional on a change). Furthermore, the ratio of all price changes over total observations is 0.215 in both samples, and the mean size of these changes is 1.6% offline and 1.4% online.

The cases of Brazil and Colombia are more complex, but the samples still show similar price change behaviors. The evidence suggests these supermarkets treat their online stores as independent branches, with similar strategies in terms of price adjustments. In Brazil, price levels are identical 42% of the time. Unlike Argentina, online prices can be either higher or lower depending on the product. In terms of price changes, the matching is much better because most of the timing differences are concentrated in a small share of products: 75% of all goods have identical price change series across samples. For all products, the ratio of changes over total observations is 0.356 offline and 0.411 online, while the mean size of changes is 4.9% offline and 5.3% online. In Colombia, the matching of price levels, at 29%, is lower than in Brazil. However, price differences are small, while the matching of price changes is high, with 67% of identical price changes series. The ratios of changes over total observations match perfectly, at 0.433 in both samples, while the mean size of changes is 8.1% offline and 8.2% online.

4.0.3 Tracking Official Statistics

Online price changes in these supermarkets are also able to capture country-level inflation trends. I show this by comparing scraped price indexes with official price statistics.

Figure 14 plots a supermarket index (constructed using daily scraped data) and the official CPI index in each country. I focus the comparison on CPI indexes to emphasize aggregate levels of inflation and because products sold in supermarkets represent over 40% of CPI weights in all these countries. In Section A.3 of the Appendix I provide similar results comparing only a subset of food indexes.

Figure 14 shows that daily online indexes closely track the official CPI series in Brazil, Chile, and Colombia. Although these scraped indexes are meant to be just rough approximations to the official statistics, they can still capture the main trends in inflation, particularly when measured at lower frequencies, smoothing from the short-term volatility of daily prices. This can be seen clearly in Figure 15, where I plot a daily estimate of annual inflation in every country. This price series is constructed as the percentage change in the average daily index during the past 30 days versus the same period a year ago.

Argentina is the only country where scraped indexes are not consistent with official statistics. The scraped data show a mean annual inflation rate of 17.1%, but the mean CPI inflation was only 7.6% per year during this time period. However, the difference is not surprising because official data have become widely discredited since January 2007, when the government started interfering with the construction and publication of price indexes at the National Statistics Institute (INDEC).[29]

5 Conclusions

This paper introduces a new way of collecting price data and applies it to re-evaluate some important stylized facts in the price stickiness literature. Scraped data, obtained directly from online sources, are a unique source of price information. Scraped prices are easier to collect than CPI and scanner data, and can provide information at daily frequencies for all products sold by hundreds of retailers in many countries around the world. The data can be collected without any delays and the collection methodology can be customized to match the specific needs of the researcher. Furthermore, *online* prices behave similarly to offline prices in terms of timing and size of changes, and price indexes created with scraped data can capture the main inflation patterns in official statistics.

Using these unique data, the paper provides three new stylized facts for the sticky-price literature. First, the distribution of the size of price changes tends to be bimodal, with few changes close to zero percent. Second, aggregate hazard functions are hump-shaped, with the conditional probability of a price change increasing for a period between 30 and 90 days. Third, there is daily price synchronization among closely competing goods. Although these results do not offer conclusive evidence in favor of any standard sticky-price model, they are mostly consistent with models that combine elements of both TDP and SDP, such as Alvarez et al. (2010).

Still, a great deal of research with scraped data is needed. What happens to these stylized facts when scraped prices are used in countries like the US and Europe? How do these and other stickiness patterns change with different levels of inflation, market structures, and over time? There are also some puzzling patterns in these data that deserve further attention. The

[29]For more on Argentina's inflation, see Cavallo (2010).

distribution of the size of changes becomes more asymmetric with higher levels of inflation, but the mean size of price increases and decreases stays relatively constant. More puzzlingly, countries with higher inflation can also have *stickier* prices. Although this observation is based on a small cross section of countries, it suggests that what is important for inflation (and therefore output) may not be the overall level of stickiness, but the relative rigidity of price increases over price decreases.

More generally, the potential uses of scraped data in macroeconomics go far beyond those explored in this paper. Scraped prices can be used to create daily price indices that complement official statistics, compare and test theories of international prices, exchange rate and commodity pass-through, study the pricing effect of new product introductions, and provide real-time estimates of sectoral stickiness. These are all topics to explore in future research.

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Tables

Table 1: Database Description

	Argentina	Brazil	Chile	Colombia
Total observations	10.8M	9.8M	9.7M	3.9M
Total Products	28813	23115	24336	9526
Initial date	10/7/2007	10/10/2007	10/24/2007	11/13/2007
Final date	08/13/2010	08/13/2010	08/13/2010	08/13/2010
Days	1041	1038	1024	1004
Categories	74	72	72	59
Urls	993	319	292	122
Product Description	Yes	Yes	Yes	Yes
Sale indicator	Yes	Yes	-	Yes
Price Controls	Yes	-	-	-
Brand, Size, Bulk Price	Yes	Yes	Yes	Yes
Missing obs. within spells	32%	25%	33%	22%
Obs with sales	2.99%	4.38%	-	7.55%
Products with sales	39%	22%	-	25%
Products with price controls	1.5%	-	-	-
Life of goods (in days, Mean/Median)	549/540	558/502	590/634	523/525
Obs per good (Mean/Median)	375/304	423/376	398/380	410/349

Notes: The missing values are caused by items that go out of stock or failures in the scraping software that tend to last for only a few days. Gor the analysis in this paper, I replaced missing values *within* price series with the previous price available for that particular product. For those results that exclude sales, I created a regular price series by replacing all sale prices with the previous non-sale price available for that product. I also removed all price changes exceeding 500%. These represent a negligible number of observations but can bias statistics related to the magnitude of price changes. See Section A.1 in the Appendix for more details on data treatments.

Table 2: Alternative Data Sources

	Scraped Data	CPI Data	Scanner Data
Product Categories Covered	Few	Many	Few
Retailers Covered	Few	Many	Few
Quantities Sold	No	No	Yes
Data Frequency	Daily	Monthly - Bi-Monthly	Weekly
Countries Available for Research	$\sim$50*	10-15	<5
All Products in Retailer (Census)	Yes	No	No
Comparable data across countries	Yes	Limited	Limited
Details: sale, price control, other	Yes	Limited	Yes
No *Forced* Substitutions	Yes	No	Yes
Real-Time data availability	Yes	No	No

Notes:

*Data from over 50 countries are currently being collected by the Billion Prices Project (www.billionpricesproject.org).
** Only goods purchased are scanned.

Table 3: Price Changes by Country and Sale Treatment

	Including Sales				Excluding Sales*		
	Arg.	Brazil	Chile	Col.	Arg.	Brazil	Col.
Price Changes	244K	366K	221K	108K	195K	289K	
Products with no price changes	19%	10%	21%	14%	22%	10%	21%
Price changes per good (Mean/Median)	8/5	15/8	9/4	11/7	2.6/2	13/7	4/3
Price increases (% of price changes)	68%	57%	54%	56%	84%	59%	57%
Price decreases (% of price changes)	32%	43%	46%	43%	16%	41%	43%
Inflation (%, average annual rate)	17.1%	5.1%	2.7%	4.2%			

Notes: *No sales information is available for Chile.

Table 4: Median Frequencies by Country - Increases and Decreases

	Including Sales				Excluding Sales		
	Arg	Brazil	Chile	Col	Arg	Brazil	Col
Daily Frequency	0.015	0.027	0.015	0.019	0.012	0.023	0.016
Implied Durations (days)	66	36	66	52	83	43	62
Implied Durations (months)	2.2	1.2	2.2	1.73	2.8	1.4	2.1
Frequency of Increases (Freq+)	0.010	0.016	0.008	0.011	0.009	0.014	0.009
Frequency of Decreases (Freq-)	0.004	0.0011	0.007	0.008	0.003	0.009	0.007
Freq+/Freq-	2.5	1.5	1.1	1.4	3	1.6	1.3

Notes: *Bils and Klenow (2004) methodology taking the mean within categories and then the median across categories.

Table 5: Size of Price Changes by Country and Sale Treatment

	Including Sales				Excluding Sales		
	Arg.	Brazil	Chile	Col.	Arg.	Brazil	Col.
Size of changes (Mean*)	5.0%	1.7%	3.6%	2.0%	5.5%	1.5%	1.8%
Size of price increases (Mean*)	13%	11.7%	17.6%	11.8%	11.8%	9.3%	8.3%
Size of price decreases (Mean*)	-11%	-11.9%	-14.2%	-10.5%	-10.2%	-10.1%	-7.4%
Share of price changes under |1%|	4.3%	%	3.6 %	%	%	%	%
Share of price changes under |5%|	27%	33%	25%	37%	%	%	%

Notes: *Mean size of changes per individual good, then the mean per category, and finally the mean across all categories.

Table 6: Tests of Modality

	DIP Test (calibrated)		Silverman's Test			
	Dip Stat.	Null 1 mode p-value	Critical Band.	Null 1 mode p-value	Null 2 modes p-value	Null 3 modes p-value
Argentina	0.07	0.00	1.92	0.00	0.00	0.06
Brazil	0.02	0.00	1.12	0.00	0.00	0.07
Chile	0.03	0.00	1.74	0.00	0.00	0.18
Colombia	0.01	0.00	0.66	0.03	0.97	0.99

Table 7: Mean Synchronization within Urls

	Argentina	Brazil	Chile	Colombia
All Price Changes	0.008	0.007	0.007	0.012
Excluding Sales	0.008	0.006	-	0.011
Price Increases	0.007	0.006	0.002	0.006
Price Decreases	0.003	0.005	0.002	0.004

Notes: Results based on 824 urls in Argentina, 281 in Brazil, 256 in Chile and 103 in Colombia.

Table 8: URL Synchronization and the Coefficient of Variation in Size of Changes

	Argentina	Brazil	Chile	Colombia
Correlation*	-0.24	-0.23	-0.27	-0.10
p-value	0.00	0.00	0.01	0.44

Notes: *Correlation between the degree of URL synchronization and the mean coefficient of variation (CV) in the URL. CV is the daily sd/mean of the absolute size of price changes. All price changes included in calculations, but CV is estimated only when there are two or more price changes in a day within an URL. Only URL with significant coefficients for synchronization (95% level) are included.

Table 9: Online vs. Offline Prices

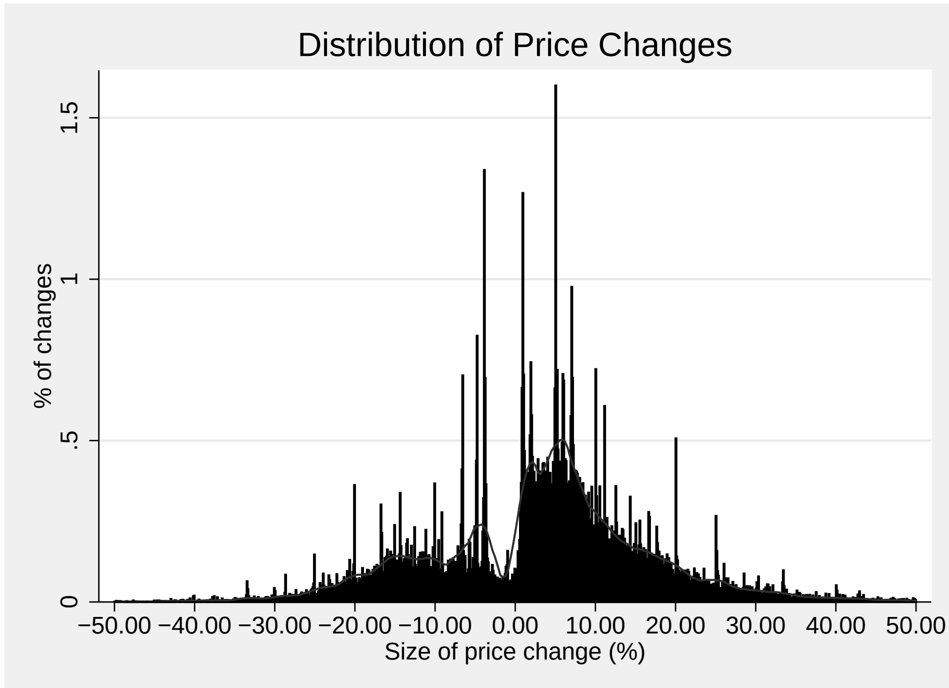
	Argentina	Brazil	Chile	Colombia
Matching ids	Yes	Yes	No	Yes
% Available Online	100%	80%	90%	74%
PRICE LEVELS				
online=offline	18%	42%	93%	29%
online>offline	78%	34%	4%	32%
Price Difference (Mean %)	5	9	2	0
PRICE CHANGES				
Products with Identical Change Series*	93%	75%	94%	67%
Ratio of Changes over Observations				
Offline	0.215	0.356	0.274	0.433
Online	0.215	0.411	0.249	0.433
Mean Size of Changes (%)				
Offline	1.6	4.9	1.4	8.1
Online	1.4	5.3	1.3	8.3

Notes: *Indicator variable: 1 if the price increased, -1 if it dropped, 0 if it is constant.

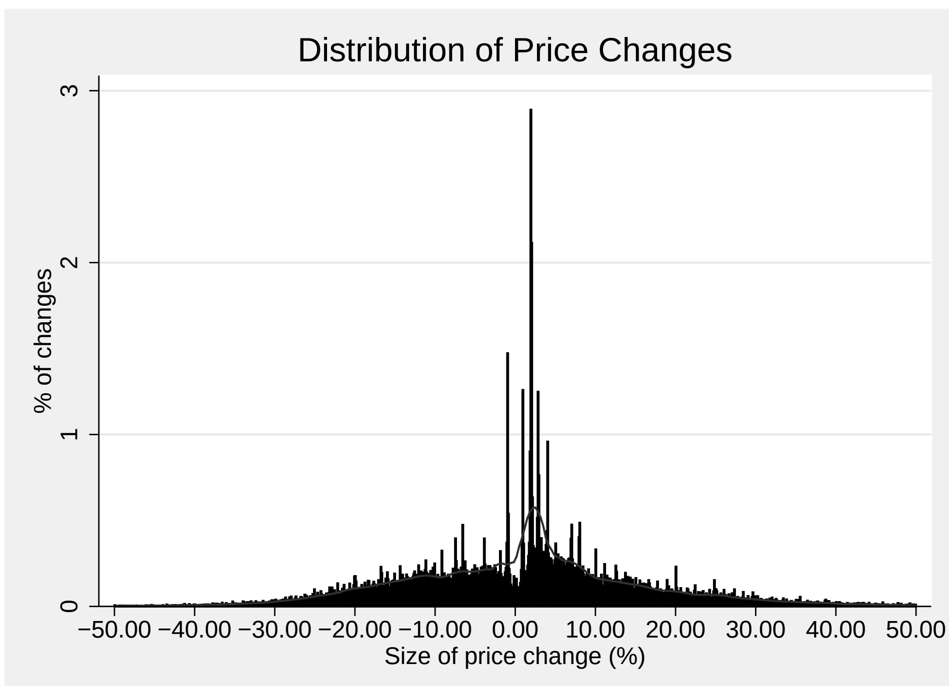
Table 10: Average Annual Inflation (% per year)

	Scraped Supermarket Index	Official Consumer Prices (CPI)
Argentina	17.1	7.6
Brazil	5.1	4.6
Chile	2.7	3.7
Colombia	4.2	6.1

Figures

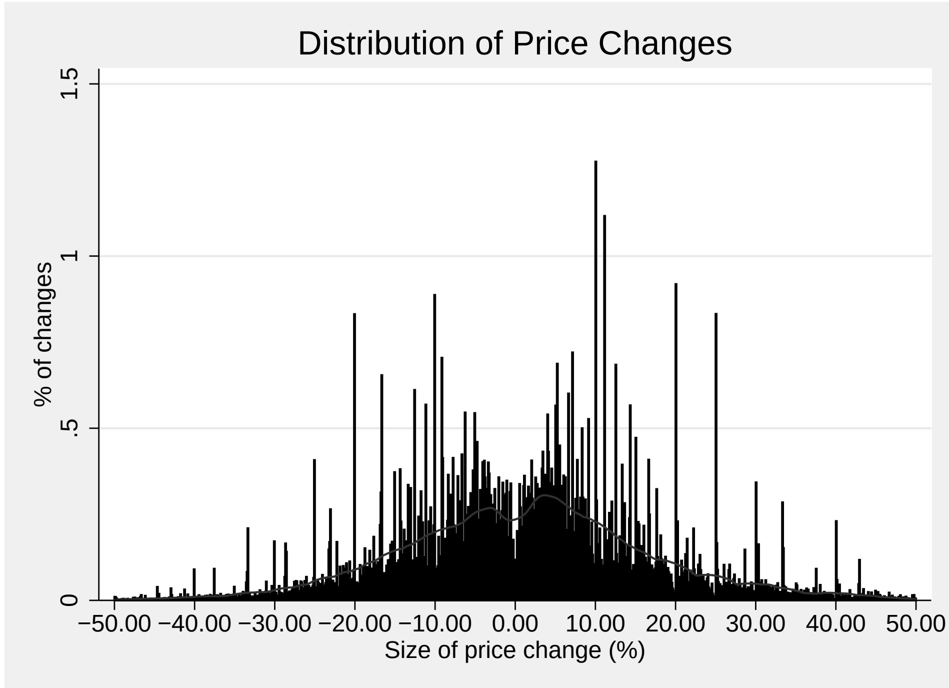


(a) Argentina

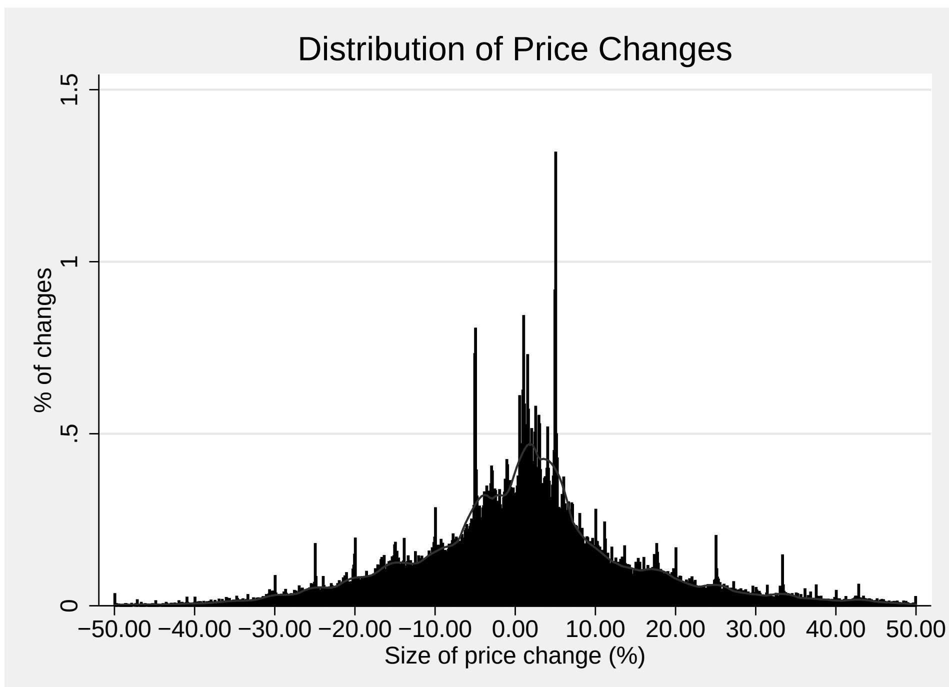


(b) Brazil

Figure 1: Distribution of the Size of Price Changes
Notes: Bin size is 0.1%. Smoothed kernel density shown.



(a) Chile



(b) Colombia

Figure 2: Distribution of the Size of Price Changes
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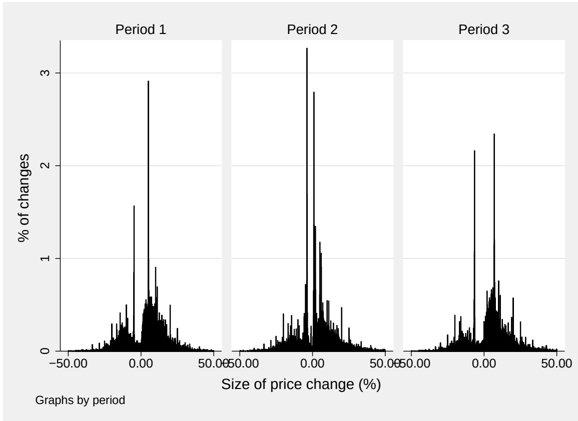
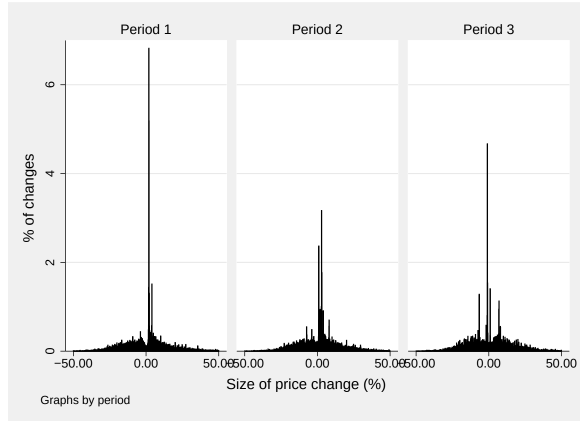
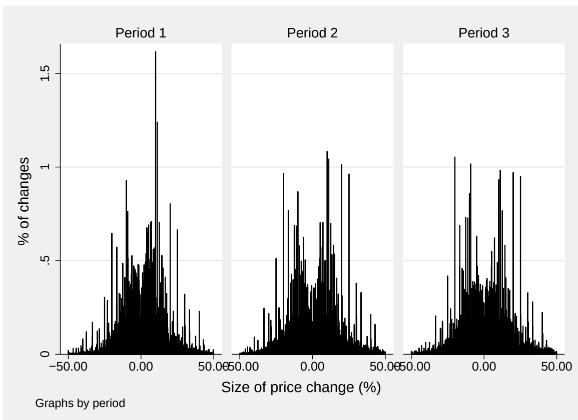


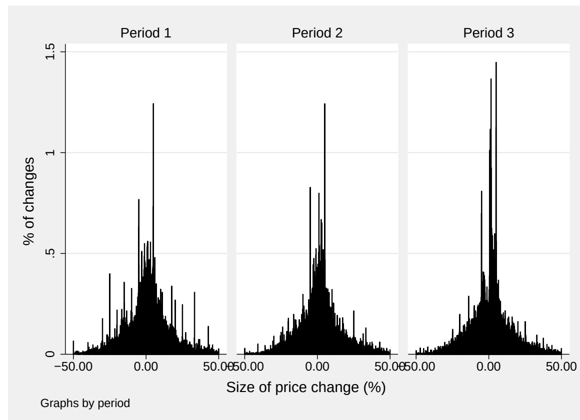
(a) Argentina



(b) Brazil

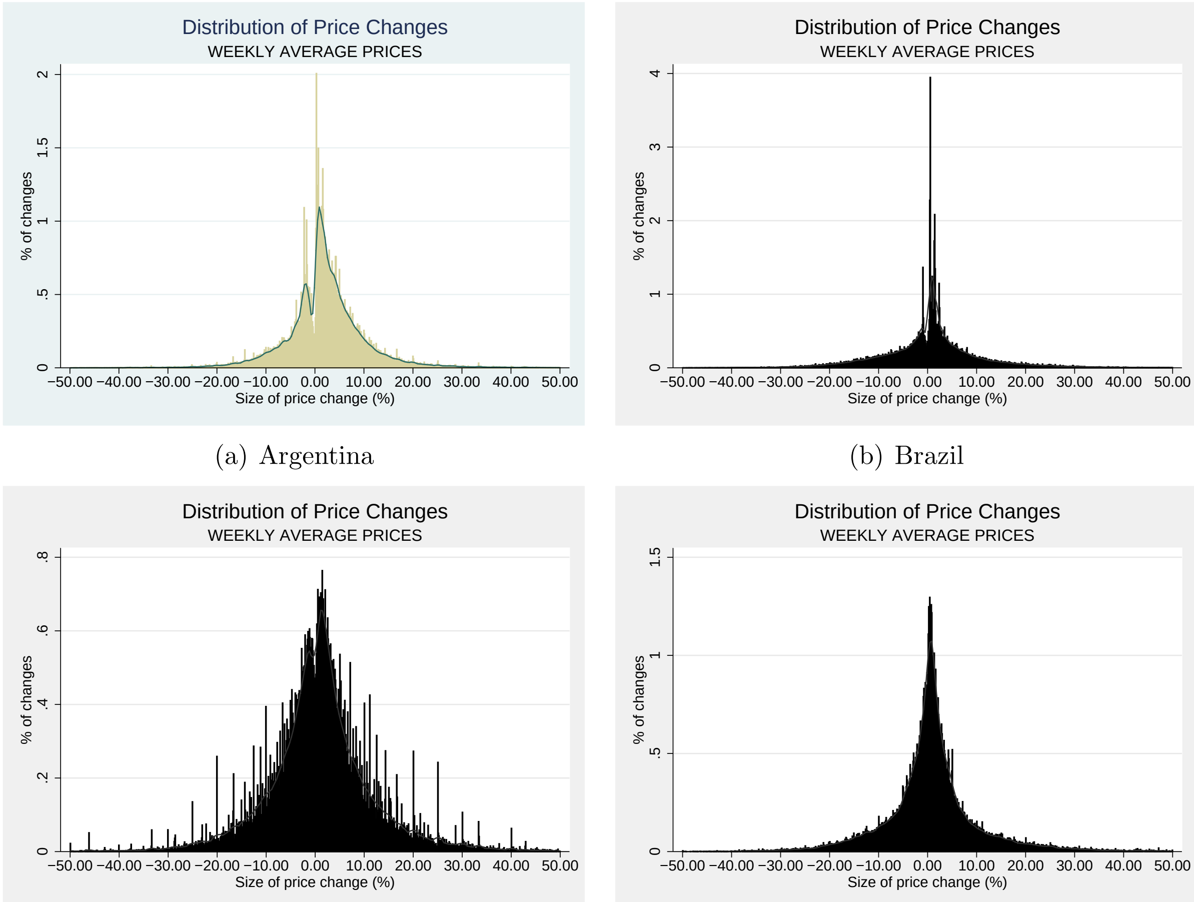


(c) Chile



(d) Colombia

Figure 3: Distribution of the Size of Price Changes - Over Time
Notes: Bin size is 0.1%. Smoothed kernel density shown.



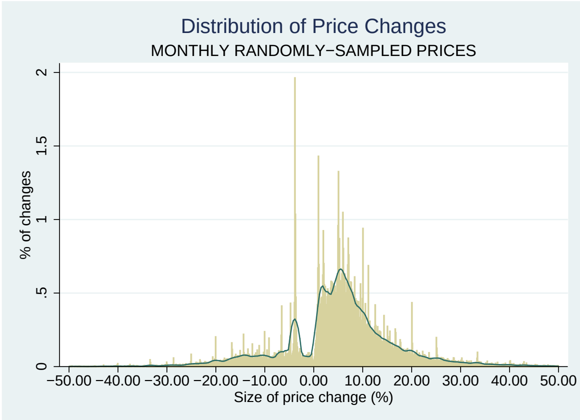
(a) Argentina

(b) Brazil

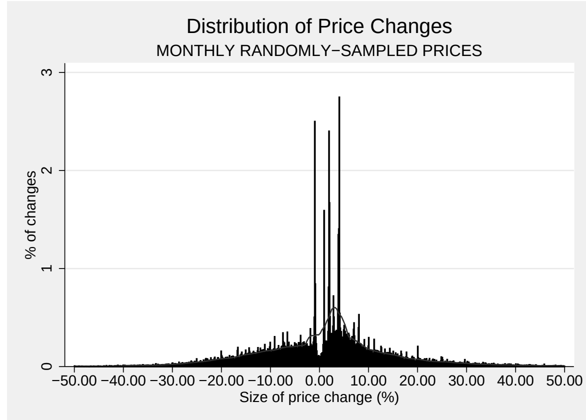
(c) Chile

(d) Colombia

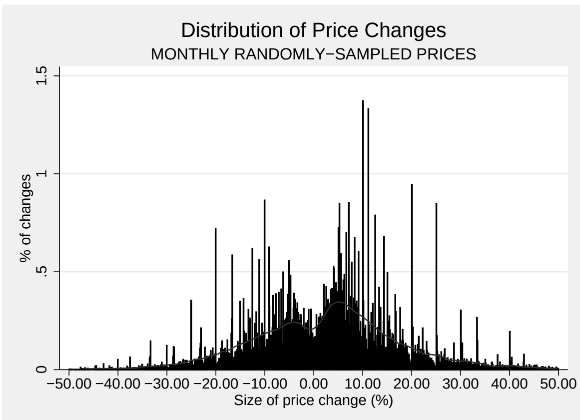
Figure 4: Distribution of the Size of Price Changes - Weekly Averages
Notes: Bin size is 0.1%. Smoothed kernel density shown.



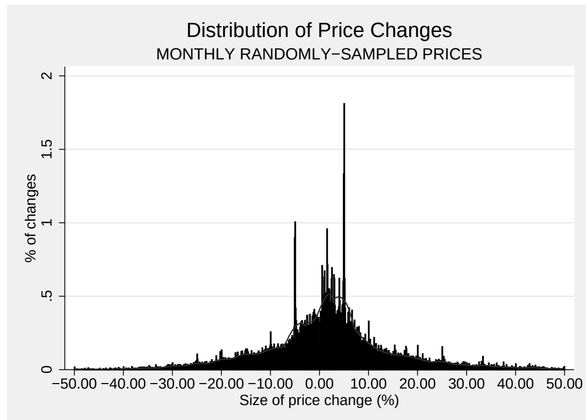
(a) Argentina



(b) Brazil

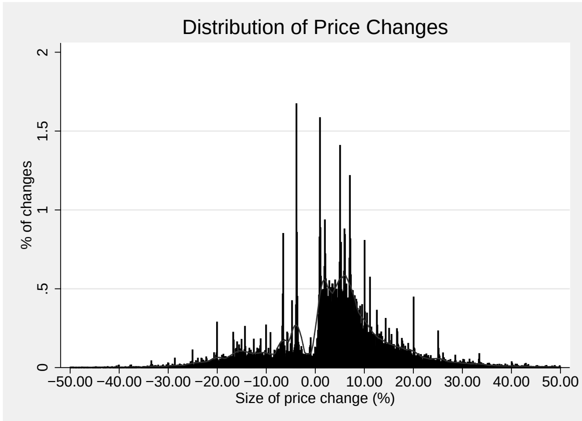


(c) Chile

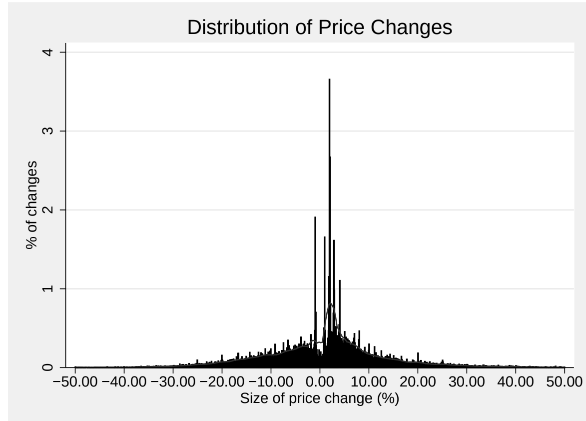


(d) Colombia

Figure 5: Distribution of the Size of Price Changes - Monthly Sampling
Notes: Bin size is 0.1%. Smoothed kernel density shown.

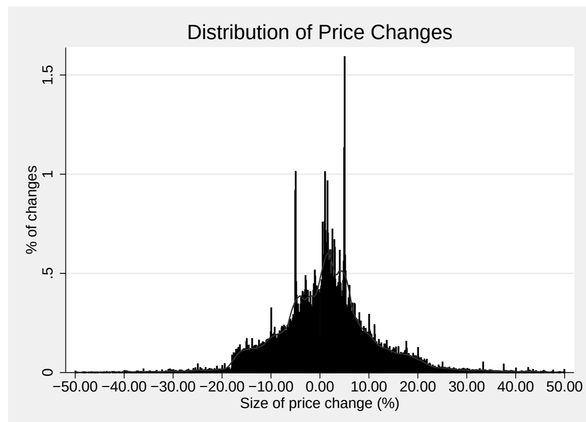


(a) Argentina



(b) Brazil

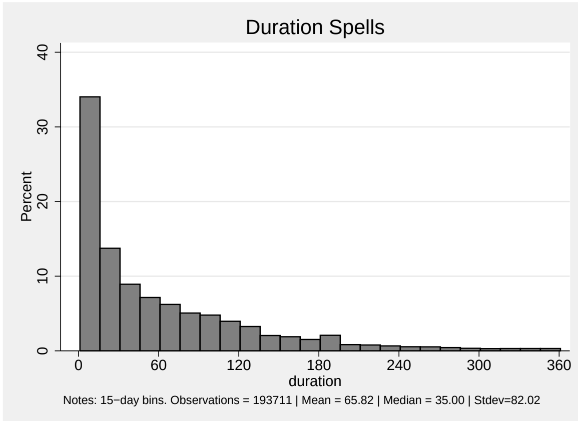
(c) Chile - Not Available



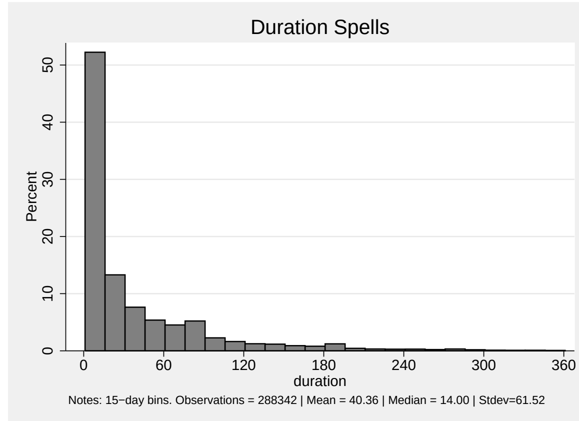
(d) Colombia

Figure 6: Magnitude of Price Changes - Excluding Sales

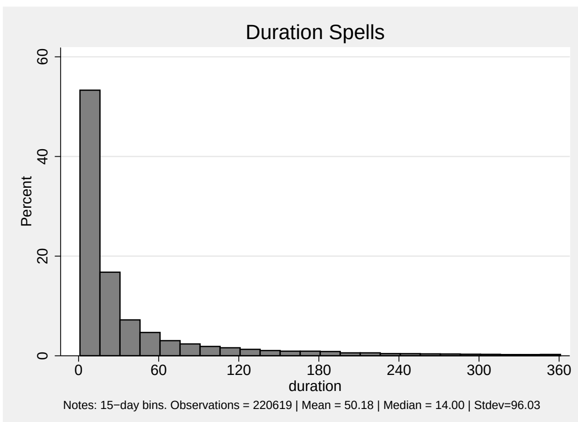
Notes: Bin size is 0.1%. Estimated kernel density shown. Brazil shown without changes on 15/12/07 and 29/12/07.



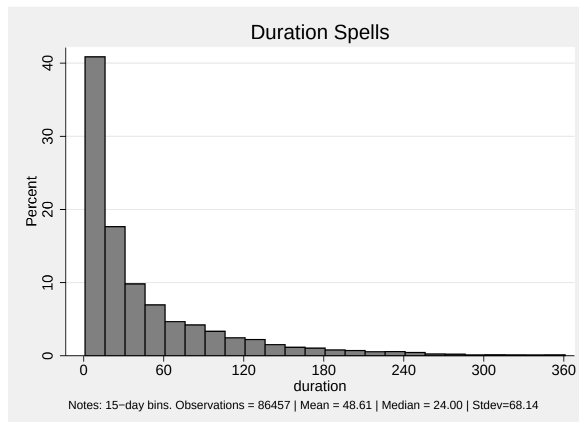
(a) Argentina



(b) Brazil

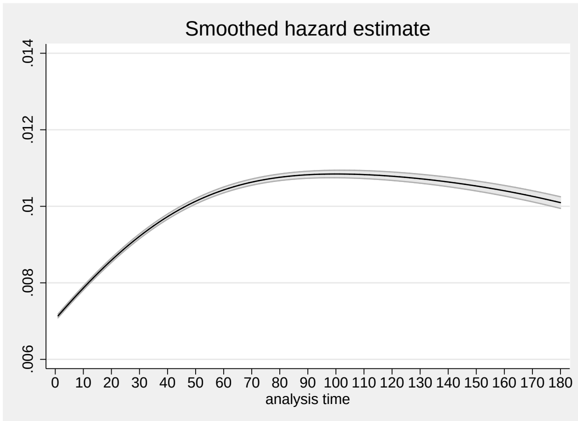


(c) Chile

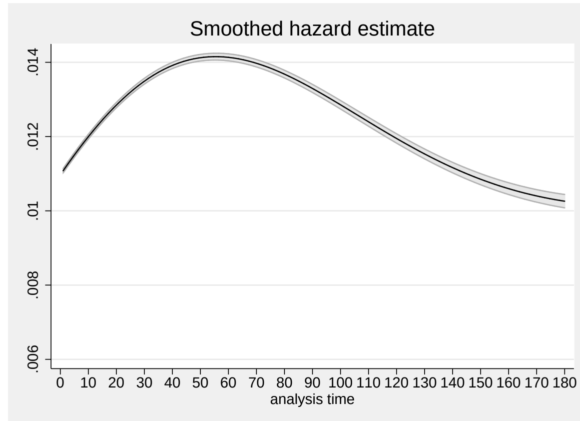


(d) Colombia

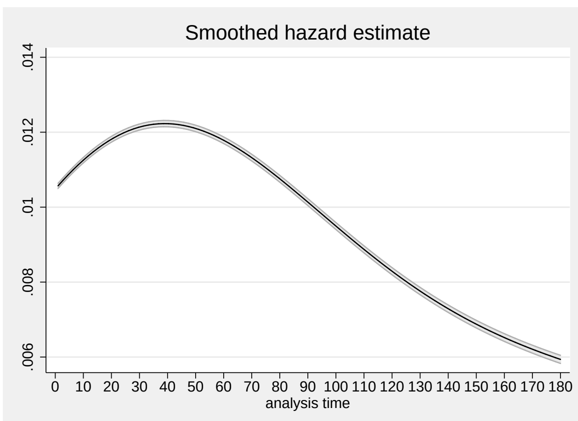
Figure 7: Histogram of Duration Spells



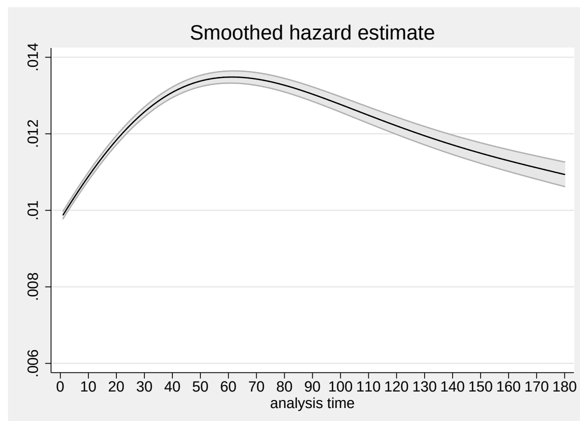
(a) Argentina



(b) Brazil



(c) Chile



(d) Colombia

Figure 8: Smoothed Hazard Functions

Notes: Left-censored spells are excluded. Sales are excluded in Argentina, Brazil, and Colombia. Initial 180 days shown.

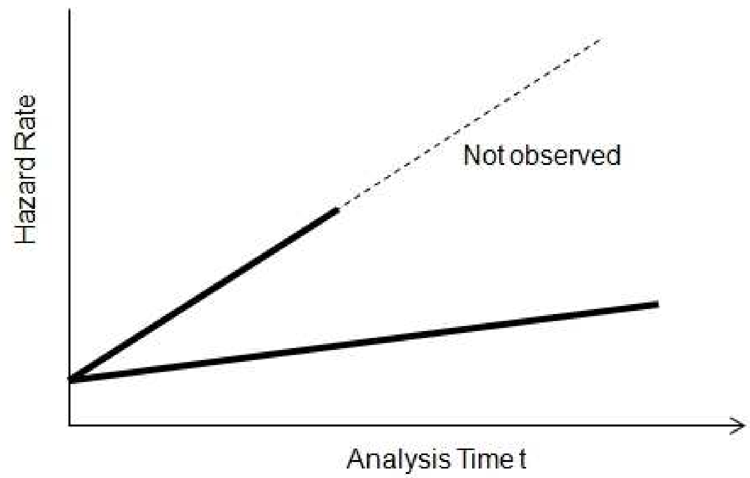
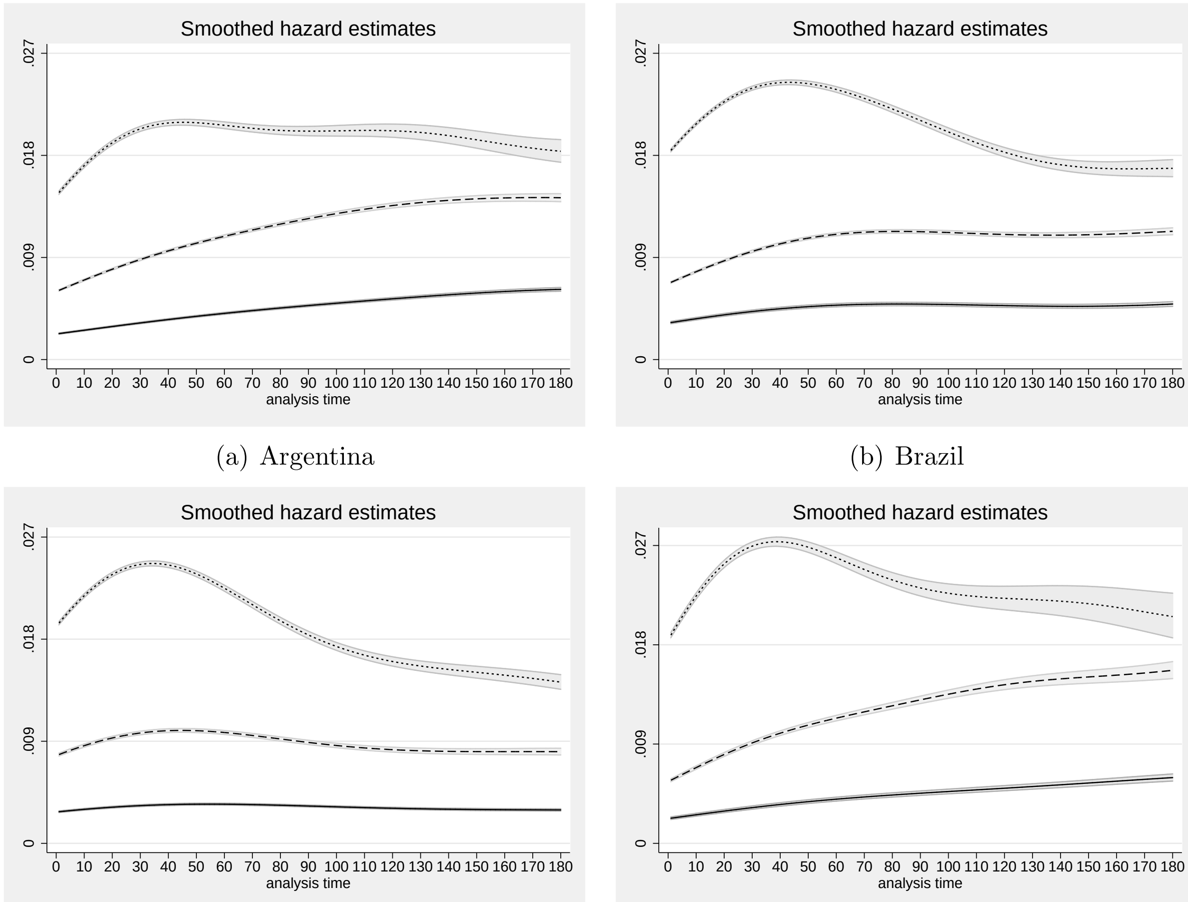


Figure 9: Heterogeneity and Survival Bias



(a) Argentina

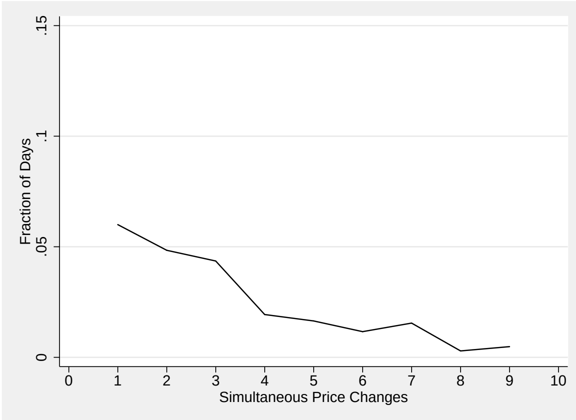
(b) Brazil

(c) Chile

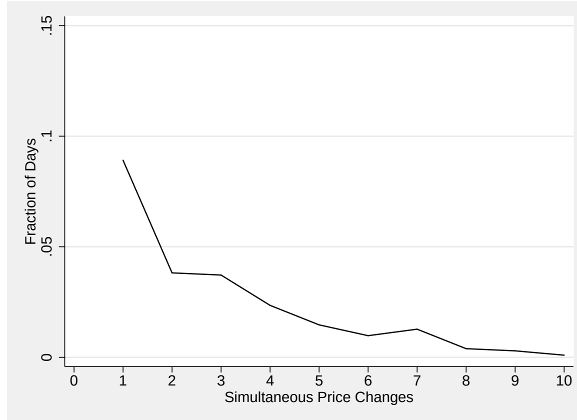
(d) Colombia

Figure 10: Hazards for Different Duration Groups

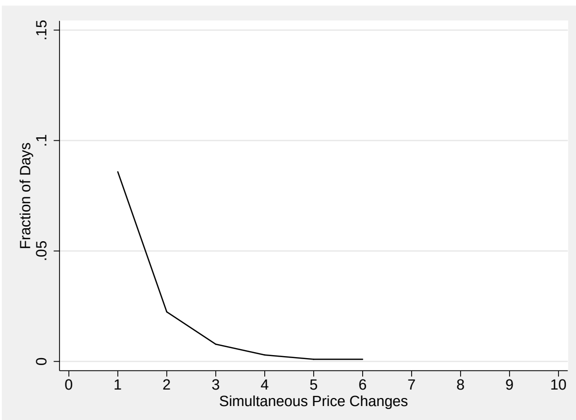
Notes: Notes: Left-censored spells are excluded. Sales are excluded in Argentina, Brazil, and Colombia. Initial 180 days shown.



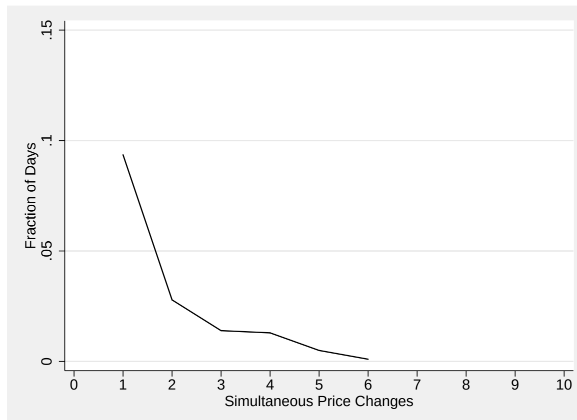
(a) Argentina



(b) Brazil

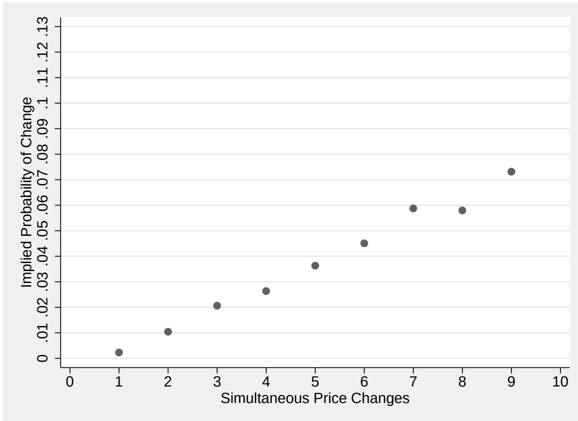


(c) Chile

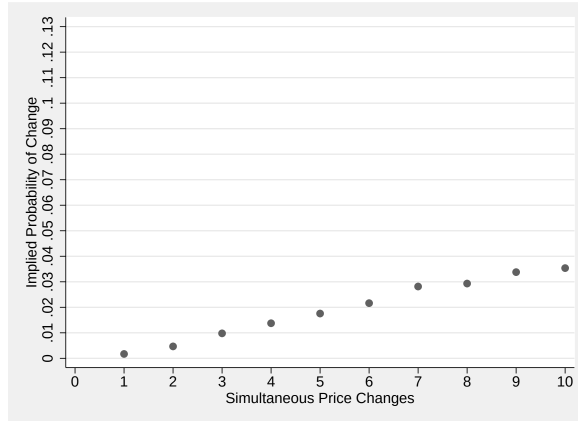


(d) Colombia

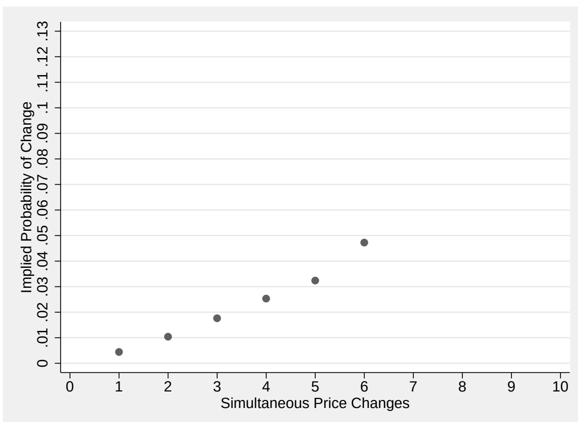
Figure 11: Distribution of Synchronized Changes - Example with Bottled Water Urls



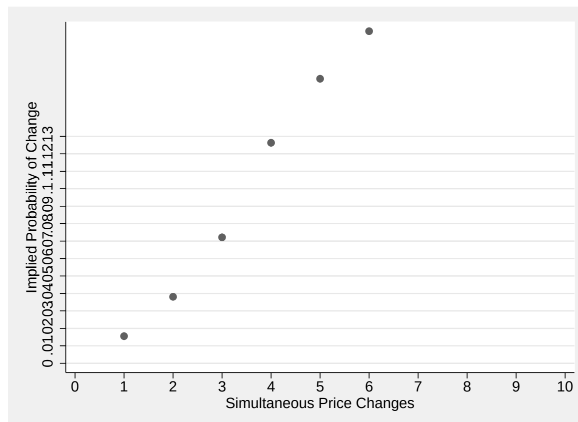
(a) Argentina



(b) Brazil

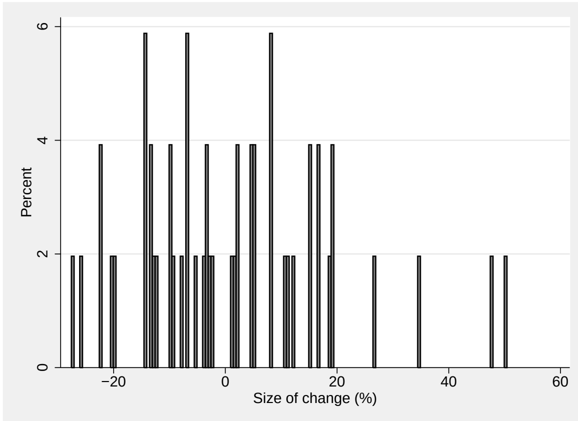


(c) Chile

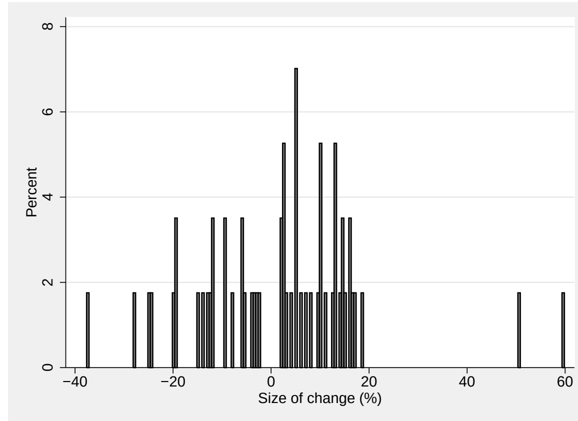


(d) Colombia

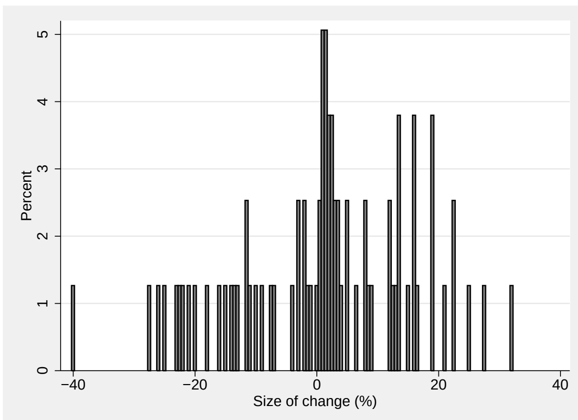
Figure 12: Implied Probabilities - Example with Bottled Water Urls



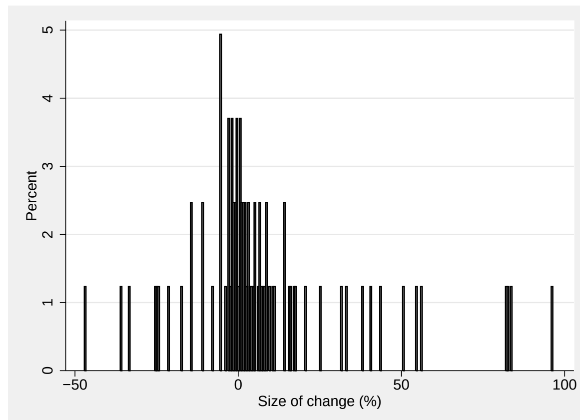
(a) Argentina



(b) Brazil



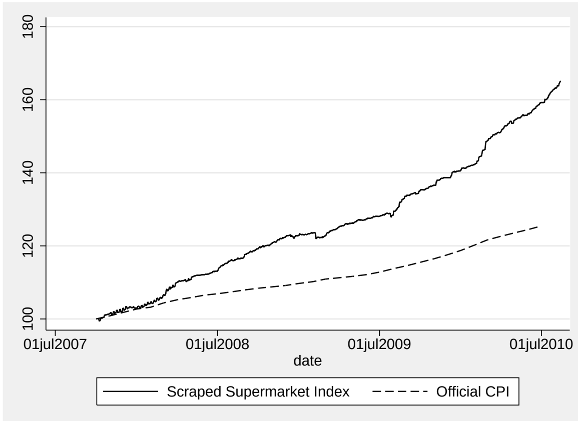
(c) Chile



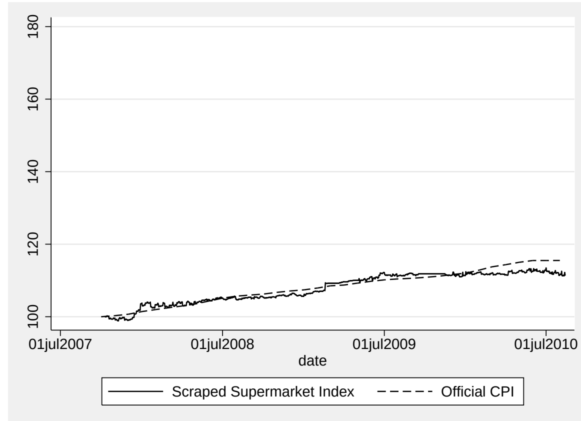
(d) Colombia

Figure 13: Magnitude of Offline Price Changes

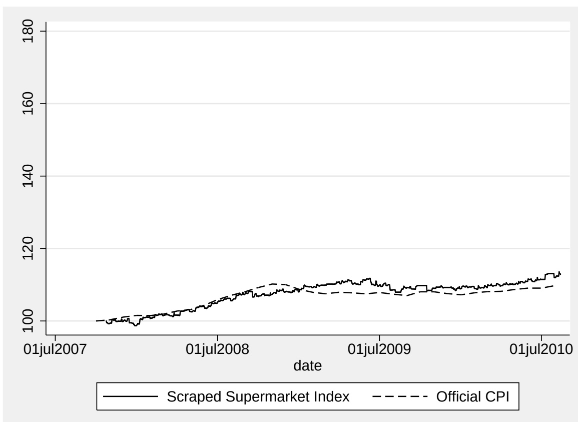
Notes: Bin size is 0.5%



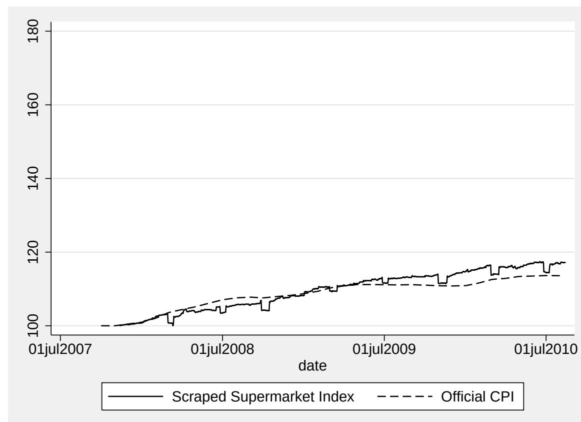
(a) Argentina



(b) Brazil

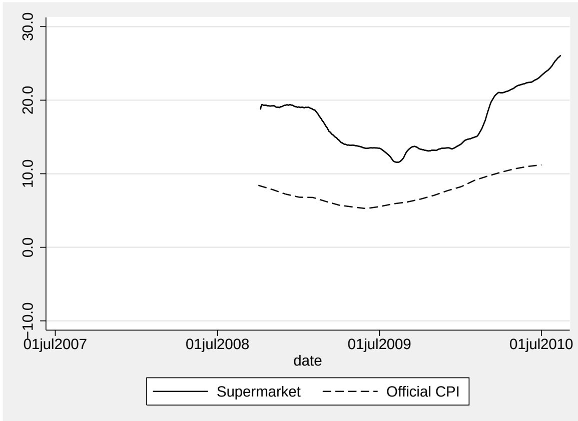


(c) Chile

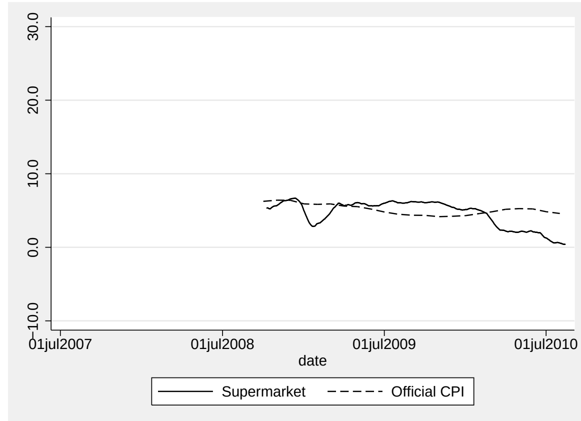


(d) Colombia

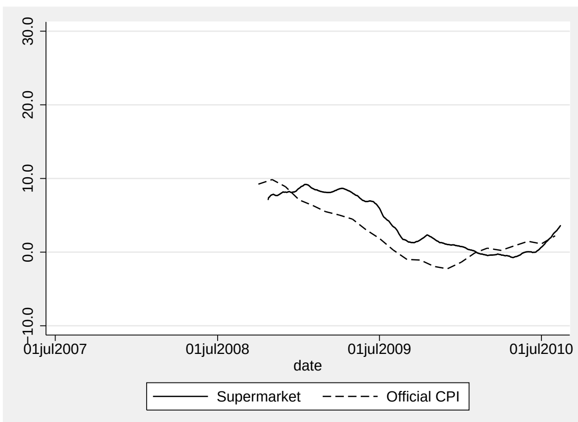
Figure 14: Scraped Index vs. the Official CPI



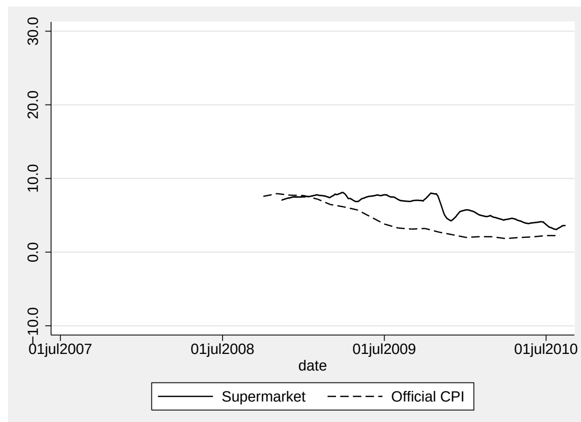
(a) Argentina



(b) Brazil



(c) Chile



(d) Colombia

Figure 15: Annual Inflation: Scraped vs CPI