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SCRAPED DATA AND STICKY PRICES

Alberto Cavallo*

Abstract—I use daily prices collected from online retailers in five countries to study the impact of measurement bias on three common price stickiness statistics. Relative to previous results, I find that online prices have longer durations, with fewer price changes close to 0, and hazard functions that initially increase over time. I show that time-averaging and imputed prices in scanner and CPI data can fully explain the differences with the literature. I then report summary statistics for the duration and size of price changes using scraped data collected from 181 retailers in 31 countries.

I. Introduction

STICKY prices are a fundamental element of the monetary transmission mechanism in many macroeconomic models. Over the past twenty years, a large empirical literature has tried to measure stickiness and understand its microfoundations.¹ These studies have produced a set of stylized facts, summarized by Klenow and Malin (2010), which have been used to motivate many theoretical papers.²

The increase in empirical work has been possible due to unprecedented access to microlevel Consumer Price Index (CPI) data and scanner data sets in several countries. While valuable, these data sets are not collected for research purposes and their sampling characteristics can introduce measurement errors and biases that affect some of the stylized facts in the literature.³

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¹ Cecchetti (1986), Kashyap (1995), and Lach and Tsiddon (1996) provided pioneering contributions to the literature using samples of goods such as magazines and groceries. Bils and Klenow (2004) made a seminal contribution with microlevel U.S. CPI data. They were followed by papers such as Nakamura and Steinsson (2008), Klenow and Kryvtsov (2008), Klenow and Willis (2007), Dhyne et al. (2006), Boivin et al. (2009), Wulfersberg and Ballangrud (2009), and Gagnon (2009), to name just a few. For a recent survey of the literature, see Nakamura and Steinsson (2013).

² Some examples are Midrigan (2011), Gorodnichenko (2008), Woodford (2009), Bonomo, Carvalho, and Garcia (2011), Costain and Nakov (2011), and Alvarez and Lippi (2014).

³ For previous discussions of measurement error in the literature, see Campbell and Eden (2014), Cavallo and Rigobon (2011), and Eichenbaum et al. (2014).

In this paper, I use a new type of microlevel data based on online prices, called “scraped data,” to explicitly document the impact of measurement biases on some key stickiness statistics. In particular, I argue that two main sampling characteristics, time averaging and imputations of missing prices, can greatly affect the observed duration and size of price changes in traditional data sources.

Time averages are intrinsic in scanner data sets, such as Nielsen’s Retail Scanner Data, which reports weekly averages of individual product prices. Imputed prices for substitutions and temporarily missing products are a common characteristic of CPI data sets. In the United States, the Bureau of Labor Statistics (BLS) imputes many of these missing prices with cell-relative imputation, a method that uses the average price change within related categories of goods.⁴ These two sampling characteristics, while reasonable for the purposes of the original data collection efforts, can greatly increase the number of price changes observed in the data, reduce the perceived size of these changes, and affect key statistics such as the distribution of the size and the hazard rate of price changes.

Scraped data are not affected by these sources of measurement bias. Online prices are collected using specialized software that scans the websites of retailers that show prices online, finds relevant information, and stores it in a database. Once it is set up, the software can run automatically every day, providing high-frequency information for all goods sold by the sampled retailers in a set of selected countries. The scraped data set used in this paper was collected by the Billion Prices Project at MIT every day between October 2007 and August 2010 for over 250,000 individual products in five countries: Argentina, Brazil, Chile, Colombia, and the United States. I also used a larger data set collected by PriceStats, a private company, to report summary statistics on price stickiness from 181 retailers in 31 countries.⁵

The first contribution of this paper is the use of online data in five countries to document the impact of measurement bias on three common statistics in the literature: the duration of price changes, the distribution of the size of price changes, and the shape of their hazard function over time.

To show the impact of time averages in scanner data, I directly compare my findings to those using data provided by Nielsen for the same retailer, location, and time period. I also simulate the weekly time averaging in my data, which produces a close match to the scanner data results.

⁴ See Bureau of Labor Statistics (2015a). Before January 2015, the BLS imputed prices using relatively broad item strata and geographic index areas. The latest methodology uses narrower elementary-level items (ELIs) and metropolitan areas. This change is explained in Bureau of Labor Statistics (2015b). My results suggest that this is likely to reduce the magnitude of the imputation bias in the U.S. CPI data in the future.

⁵ I am a cofounder of the Billion Prices Project and PriceStats.

As Campbell and Eden (2014) suggested, the weekly averages make a single price change look like two consecutive smaller changes. This creates more frequent and smaller price changes, completely altering the shape of their size distribution. Furthermore, it causes the hazard rate to appear highest on the first week after a change, producing fully downward-sloping hazard functions over time. Overall, time averaging the data produces similar results to those in papers that use scanner data, such as Eichenbaum et al. (2011) and Midrigan (2011).

To determine the effects of imputations in CPI data, I simulate the cell-relative imputation for temporarily missing prices in my online data. I show that imputing missing prices with average changes in the same category also increases the frequency of price changes and greatly reduces their size, making the size distribution completely unimodal, as in Klenow and Kryvtsov (2008). This effect is separate from the one caused by forced item substitutions, previously discussed in the literature.⁶ The bias is strongest when broader categories of goods are used as the reference for imputation, as BLS did until January 2015. I also show that daily prices are needed to detect the initial increase in hazard rates during the first few months. Instead, if cell-relative imputation is applied to monthly data, the hazard function resembles those in papers with CPI data, such as Nakamura and Steinsson (2008).

The second contribution of the paper is the use of scraped data to compute a set of summary statistics for durations and sizes of price changes in 31 countries. These results can be used to study the robustness of stylized facts across countries, parameterize models, and make cross-country comparisons. In particular, I show that prices are stickier than comparable results reported by various papers from fifteen countries summarized by Klenow and Malin (2010) and that the share of small price changes is low in most countries. In the appendix, I further use the cross-country data to show that inflation is not correlated with the overall frequency (size) of price changes but rather with the relative frequency (size) of price increases over decreases. The fact that the same types of data are used in every country ensures that these findings are not driven by differences in sampling characteristics or the way the statistics are computed, which complicated previous comparisons in the literature.

My findings have several applications and implications. First, they show that some stylized empirical patterns in the literature, such as the prevalence of very small price changes, are driven by the sampling characteristics in traditional data sources. Documenting and adjusting to these biases are critical for papers that rely on these statistics to study the real effects of monetary policy, as done in Alvarez

et al. (2016). Second, I provide statistics on the frequency and size of price changes that are free from time averages or cell-relative imputations and can be used to evaluate or parameterize alternative models in the literature, such as those in Woodford (2009) and Alvarez, Lippi, and Paciello (2011). Third, this paper illustrates how new data collection techniques allow macroeconomists to build customized data sets, designed to minimize measurement biases and address specific research needs. As Einav and Levin (2014), pointed out, the emergence of big data requires economists to develop new capabilities, and data collection skills are an essential part of that process. The role of online data in this context is discussed in detail in Cavallo and Rigobon (2016).

My paper directly relates to others in the price stickiness literature that discuss potential sources of measurement bias. Campbell and Eden (2014) identified prices that could not be expressed in whole cents in an Nielsen scanner data set, noting that technical errors and time aggregation likely caused them. My results with scanner data confirm their argument. Eichenbaum et al. (2014) use CPI and scanner data from multiple stores to show how unit-value prices, reported as the ratio of sales revenue of a product to the quantity sold, affect the prevalence of small price changes. While they also use daily data, their focus is on the effects of unit values and averaged prices across stores. Instead, I compare the weekly averaged prices to show that even scanner data sets not affected by unit values, such as Nielsen's Retail Scanner Data, can still produce biased results for frequency, size, and hazard rates of price changes. Eichenbaum et al. (2014) also study the effect of unit values and bundled goods in CPI categories such as Electricity and Cellular Phone Services. Instead, I focus on price imputations for missing prices, which affect nearly all CPI categories.

My work is also related to papers that use online prices, such as Brynjolfsson, Dick, and Smith (2009), Ellison and Ellison (2009), Lunnemann and Wintr (2011), Gorodnichenko, Sheremirov, and Talavera (2014), Ellison et al. (2015), and Gorodnichenko and Talavera (2017). These papers find that online prices tend to be more flexible and have smaller price changes than offline prices. The difference with my results likely comes from the fact that they focus on retailers that participate in price-comparison websites. As Ellison and Ellison (2009) showed, this type of retailer faces a different competitive environment that tends to increase the frequency and reduce the size of their price changes. Instead, I use data from large multichannel retailers that have an online presence, but sell mostly offline. Lunnemann and Wintr (2011) note that multichannel retailers represent only 9% of all price quotes in their sample.

The paper is organized as follows. Section II, describes the collection methodology and characteristics of scraped data. Section III uses daily data from five countries to document the impact of measurement error by comparing the duration of prices, the distribution of the size of price changes, and the hazard functions with previous results in the literature, sampling simulations, and a comparable scanner data set.

⁶ Klenow and Kryvtsov (2008) exclude temporarily missing imputations but include price changes caused by substitutions in their calculations of the size of price changes, which are also imputed with cell-relative methods by the BLS. For a discussion of the impact of forced substitutions on price frequencies, see Klenow and Kryvtsov (2008) and Nakamura and Steinsson (2008, 2013).

Section IV provides further robustness checks for exclusion of sales, using data from different retailers and sectors, and the effects of the sampling interval. Section V discusses implications for the literature and uses scraped data from 31 countries to document the duration and size of price changes. Section VI concludes.

II. Description of Scraped Data

A. Data Collection Methodology

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

The technology to periodically record online prices on a large scale is now more widely available. Using a combination of web programming languages, I built an automated procedure that scans the code of publicly available web pages every day, identifies relevant pieces of information, and stores the data. This technique is commonly called web scraping, so I use the term *scraped data* to describe the information collected for this paper.

The scraping methodology has three steps. First, at a fixed time each day, a software program downloads a selected list of public web pages where product and price information are shown. These pages are individually retrieved using the same web address (URL) every day. Second, the underlying code is analyzed to locate each piece of relevant information. This is done by using special characters in the code that identify the start and end of each variable, which have been placed by the page programmers to give the website a particular look and feel. For example, prices may be shown with a dollar sign in front of them and enclosed within `<price>` and `</price>` tags. Third, the software stores the scraped information in a database that contains one record per product per day. These variables include the product’s price, the date, category information, and sometimes an indicator for whether the item was on sale.

B. Advantages and Disadvantages

The main 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 1.

Scraped data have some important advantages. First, these data sets contain posted daily prices that are free from unit values, time averaging, and imputations that can greatly affect some stickiness statistics, as I shown in this paper. The daily data are also useful to better identify sales and other price changes that might be missed with monthly data. Second, detailed information can be obtained for all products

TABLE 1.—ALTERNATIVE DATA SOURCES

	Scraped Data	CPI Data	Scanner Data
Data frequency	Daily	Monthly, bimonthly	Weekly
All products in retailer (census)	Yes	No	No
Product details (size, brand, sale)	Yes	Limited	Yes
Uncensored price spells	Yes	No	Yes
Countries available for research	~60	~20	< 5
Comparable data across countries	Yes	Limited	Limited
Real-time availability	Yes	No	No
Product categories covered	Few	Many	Few
Retailers covered	Few	Many	Few
Quantities sold	No	No	Yes

The Billion Prices Project (bpp.mit.edu) data sets contain information from over 60 countries with varying degrees of sector coverage. Nielsen U.S. scanner data sets are available for research application through the Kilts Center for Marketing at the University of Chicago. Klenow and Malin (2010) provide stickiness results with CPI data sourced from 27 papers in 23 countries.

sold by the sampled retailers instead of a few (as in CPI data) or selected categories (as in scanner data). Third, there are no censored or imputed price spells in scraped data. Prices are recorded from the first day they are offered to consumers until the day they are discontinued from the store. In CPI, by contrast, there are frequent imputations and forced substitutions when the agent surveying prices cannot find the item. Fourth, scraped data can be collected remotely in any country where price information can be found online. In particular, in this paper, I use data for four developing countries, where scanner data are scarce and product-level CPI prices are seldom disclosed.⁷ Fifth, scraped data sets are comparable across countries, with prices that can be collected for the same categories of goods and time period using identical techniques. This makes it easier to perform simultaneous cross-country analyses, as I discuss in section V.⁸ Finally, Scraped data are available in real time, without any delays in accessing and processing the information. Eventually this could be used by central banks to obtain real-time estimates of stickiness and related statistics.

Table 1 also shows the main disadvantages of scraped prices. First, they typically cover a much smaller set of product categories than CPI prices. In particular, the prices used in the paper cover only between 40% and 70% of all CPI expenditure weights in these countries. While this is enough to demonstrate the effect of measurement errors on pricing statistics, the quantitative findings on stickiness and size of changes shown here should not be viewed as representative

⁷Gagnon (2009) provides a detailed analysis of sticky prices in Mexico using disaggregated CPI data manually digitalized from printed books. Alvarez et al. (2015) use CPI data from Argentina to document the behavior of price stickiness from the hyperinflation in the late 1980s to the period of low inflation in the 1990s.

⁸Past cross-country comparisons in the literature have had to rely on results provided by different papers, often with different data sources, time periods, event methods, and data treatments. See, for example, Klenow and Malin (2010). An exception is Dhyne et al. (2006), who were able to use similar data from multiple countries thanks to the coordination provided by the European Inflation Persistence Network at the European Central Bank.

TABLE 2.—DATABASE DESCRIPTION

	United States	Argentina	Brazil	Chile	Colombia
Retailers	4	1	1	1	1
Observations (millions)	28	11	10	10	4
Products (thousands)	172	28	22	24	9
Days	865	1,041	1,026	1,024	992
Initial date	03/08	10/07	10/07	10/07	11/07
Final date	08/10	08/10	08/10	08/10	08/10
Categories	49	74	72	72	59
URLs	16,188	993	322	292	123
Daily missing observations (%)	37	32	26	33	22

The number of observations in the second row does not include missing values within a price series. The data contain missing values caused by items that go out of stock or by failures in the scraping software that tend to last for a few days. Following Nakamura and Steinsson (2008), missing prices are replaced for the first five months of the price gap with the previous price available for each product. I also ignore all price changes exceeding +200% and -90%, which represent a negligible number but can significantly bias statistics related to the magnitude of price changes. See the appendix for more details on data treatments.

of services and other sectors that cannot yet be covered with online data. Second, the data come only from large multi-channel retailers that sell both online and offline. Currently the vast majority of retail sales take place in this type of retailer, but in principle, this may represent a form of sampling bias compared to the CPI (though not due to the online nature of the data, as I show below). Finally, a major disadvantage of scraped data relative to scanner data sets is the lack of information on quantities sold. In measuring stickiness, quantities are useful in obtaining detailed expenditure weights for narrowly defined categories, so in this paper, I use CPI category weights when needed.

C. Eight Large Retailers in Five Countries

The main data set used in this paper has more than 60 million daily prices in five countries: Argentina, Brazil, Chile, Colombia, and the United States. (It is available for download at bpp.mit.edu.) Table 2 provides details on each country's database. The data come from the websites of eight different companies, with prices collected daily between 2007 and 2010.

For the United States, I have data from four of the largest retailers in the country: a supermarket, a hypermarket/department store, a drugstore/pharmacy retailer, and a retailer that sells mostly electronics.⁹ In the other countries, I have data from a large supermarket in each country. All of these retailers included are leaders in their respective countries, with market shares of approximately 28% in Argentina, 15% in Brazil, 27% in Chile, and 30% in Colombia. The market shares for the U.S.-based retailers are not revealed for confidentiality reasons.¹⁰

In the United States, the data are categorized under the United Nations COICOP structure, which is used by most countries to classify CPI information. A narrower category indicator is the URL (or web address) where the products are found on the website. The retailer's website design and menu pages determine the number of URLs available in each country.

⁹ See the appendix for a similar table with details for each U.S. retailer.

¹⁰ Revealing information on the U.S. supermarket, in particular, is strictly forbidden by the conditions of the scanner data provided by the Kilts Marketing Center at the University of Chicago Booth School of Business, used in Section IIIA of the paper to compare the results with online data.

Missing values are common in daily data because products may be out of stock or not correctly scraped on a particular day. Depending on the country, the percentage of these missing values is between 22% and 37% of all observations, as shown in table 2.¹¹ Price gaps, however, do not last for more than a few days. Following the literature, I therefore complete missing values by carrying forward the last recorded price until a new price is available. I do this only for the first five months of the price gap to match the approach taken by Nakamura and Steinsson (2008). In the appendix, I show that my results are similar if I do not impute any missing values and focus exclusively on consecutive observations.

There are a few price changes in each country that seem too large and are most likely the result of scraping mistakes. Although these are a negligible part of all observations, they can affect statistics related to the magnitude of price change. Consequently, all daily price changes that exceed 200% or -70% are excluded from all duration and size calculations.

Online versus offline prices. Online purchases are still a small share of transactions in most countries, so it is natural to question the representativeness of scraped data. Are online prices similar to those that can be collected in the physical stores of these retailers? To answer this question, in Cavallo (2017), I simultaneously collected online and offline prices for over fifty of the largest multichannel retailers in ten countries, including those in this paper. I show that on average, 72% of the prices are identical across samples and that price changes have similar frequencies and sizes. I also conducted a separate online-offline data collection for the specific retailers included in this paper in 2009. These results, shown in the appendix, confirm that price levels are often identical and that price changes behave similarly in terms of the frequency and size of adjustment. Finally, another way to test the validity of scraped data is to see if the inflation dynamics obtained from this small sample of retailers can resemble those in CPI statistics, which are constructed using surveys from a large number of offline stores. In Cavallo (2013), I showed that online price indexes can indeed closely match the CPI

¹¹ The share of missing observations for monthly sampled data is only 1.74% in the United States. This is lower than the 12% reported by Klenow and Kryvtsov (2008) for the U.S. CPI data.

TABLE 3.—DURATION AND THE MEAN ABSOLUTE SIZE OF CHANGES

		United States	Argentina	Brazil	Chile	Colombia
Duration (months)	Weekly online data	2.91	2.43	1.48	2.92	1.99
	Weekly average	1.69	1.4	.91	1.69	1.1
Mean absolute size	Weekly online data	21.98	12.22	11.46	14.66	10.74
	Weekly average	11.06	6.09	6.57	8.24	5.95

I first obtain the frequency per individual good by calculating the number of price changes over the number of total valid change observations for a particular product. Next, I calculate the mean frequency per good category and, finally, the median frequency across all categories. I then compute implied durations using $-1/\ln(1 - \text{frequency})$ and convert them to monthly durations for comparisons across samples.

inflation rates in Brazil, Chile, and Colombia; Cavallo and Rigobon (2016) shows the same for the U.S. data.

III. How Sampling and Measurement Error Affect Pricing Statistics

Measurement error has been discussed in the literature on price stickiness before. For example, Campbell and Eden (2014) identified and removed prices that could not be expressed in whole cents in a Nielsen scanner data set. They noted that technical errors and time aggregation could be the cause for those “fractional prices.” Cavallo and Rigobon (2011) further discussed the potential effect of time averaging and unit values on the distribution of size changes and simulated the impact on the distribution of size changes using online data in a large number of countries. Eichenbaum et al. (2014) used CPI and scanner data from multiple stores to show how unit-value prices, reported as the ratio of a product’s sales revenue to the quantity sold, affected the prevalence of small price changes.

In this paper, I focus on two topics that have the biggest impact: time averages and price imputations. An advantage relative to previous papers is that I rely on a data source unaffected by these issues. I am therefore able to recompute some classic statistics in the literature, see the effect of each sampling characteristic, and compare them to results in previous papers. In particular, I can simulate some of the sampling characteristics in scanner and CPI data to show that they generate more frequent and smaller price changes. More explicitly, in the case of supermarket data, I can directly compare both online and scanner data from the same retailer, geographic location, and time period and show that time-averaging accounts for all the differences observed with scanner prices.

A. Time Averages and Scanner Data

A major source of measurement error in scanner data is the use of weekly averaged prices. The potential effect of time averaging in scanner data was first discussed by Campbell and Eden (2014). Their focus was not on the size of changes, but they described some complications caused by weekly averages using an example of a three-week period with a single price change in the middle of the second week. Instead of a single price change, the weekly averaged price data produce two price changes of smaller magnitude. The prevalence of examples such as this can potentially double the frequency of changes and greatly reduce the size of price

changes. The measurement bias, however, can also go in the opposite direction. For example, if there are many small increases (or decreases) during a week, then using weekly averaged prices would reduce the measured frequency and increase the absolute size of price changes.

To empirically estimate the effects of time averaging, consider the results in table 3, where I compare the implied monthly duration and the mean size of absolute price changes for both weekly sampled online data and a simulated weekly average data set that resembles the type of data available in scanner data sets. The sampling simulation is run on the original online data by simply computing the weekly average price for each good.¹² In all cases, I compute monthly durations using standard methods in the literature.

Both the duration and the absolute size of the price change fall by approximately 50% when weekly averaged data are used, and the effect is similar in all countries. This effect can explain why the literature that uses scanner data has tended to find that prices are so flexible. Given that scanner data are available mainly for groceries and supermarket products, I show in table 4 the results obtained only from the supermarket in my U.S. sample and compare them to estimates obtained from previous papers in the literature. In particular, I include results from Dominick’s Supermarket and another “Large U.S. Supermarket,” both reported by Eichenbaum et al. (2011).

The implied duration of 1.53 months in online data is much higher in relative terms than that found in previous papers in the literature that used scanner data from U.S. supermarkets, which ranged from 0.6 to 1 month. Using weekly averaged data produces an implied duration of 0.8 months, the average in the two papers mentioned above. While this is consistent with measurement error in scanner data, there are other reasons that could cause these differences. For example, the time periods of the data are quite different across papers, so it is possible that goods have become stickier in recent years. In addition, the retailers may not be the same or even similar in their pricing behaviors and other characteristics.

To make the comparison more explicit, I purchased scanner data for the same retailer, location, and time period. The

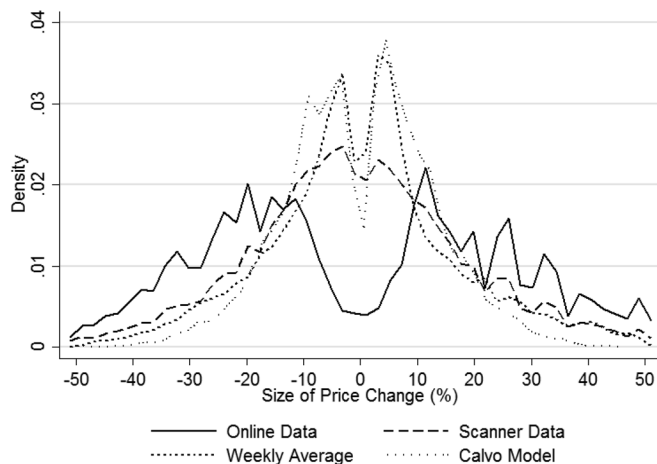
¹² Note that these prices are naturally free from unit values and averaging across stores, as they come from a single retailer and location. Unit values are imputed prices computed as the ratio of total sales over quantities. As Eichenbaum et al. (2014) show, unit values reported by scanner data sets can generate spurious small price changes. Although this was common in early data sets available in the literature, most of the scanner data used today, such as Nielsen’s Retailer Scanner Data, do not have unit values.

TABLE 4.—IMPLIED DURATION IN U.S. SUPERMARKET DATA

	Scraped	Scanner Large Retailer	Scanner Dominik's	Scraped Weekly Average	Scanner Same Retailer
Period	2008–2010	2004–2006	1989–1997	2008–2010	2008–2010
Sampling interval	Weekly	Weekly	Weekly	Weekly	Weekly
Duration (months)	1.53	0.6	1.0	0.8	0.8

The scanner data results in columns 3 and 4 come from table 3 in Eichenbaum et al. (2011). The scanner data results on the last column use prices from Nielsen provided by the Kilts Center at the University of Chicago, matching with the same retailer, postal code, and time period of the online scraped data.

FIGURE 1.—DISTRIBUTION OF THE SIZE OF PRICE CHANGES IN A U.S. SUPERMARKET



The online and scanner data in the United States were collected at the same retailer during the same time period. Scanner data were collected by Nielsen and provided by the Kilts Center at the University of Chicago Booth School of Business. Results from the Calvo model were obtained by using the code and calibration parameters from Nakamura and Steinsson (2010a), adjusting the price change probability to match the observed monthly frequency of changes, and increasing the size of idiosyncratic shocks to match the absolute size of price changes (approximately 11%). See the appendix for similar results in other U.S. sectors.

challenge was to find the same retailer in both data sets because the scanner data set, collected by Nielsen, does not explicitly identify retailers. It only provides a retailer ID, the type of store, and the postal code of each store. Fortunately, retailers tend to have a distinctive pattern of stores in different postal codes. By counting how many stores each supermarket chain in the scanner had in a given set of postal codes, I was able to find a perfect match to the retailer I used to collect the online data.¹³

The last column in table 4 shows that the scanner data also have a duration of 0.8 which is identical to the weekly averaged online data from the same retailer, postal code, and time period. Time averaging is all that is needed to replicate the low-duration results in scanner data. To be fair, few papers in the stickiness literature have emphasized the duration levels in scanner data, mainly because they are available only for groceries. What has received far more attention in the literature, however, is the distribution of the size of price changes.

The effects of time averages on the size of price changes are even more evident. Figure 1 shows the distribution of

the size of price changes in the online data, the simulated weekly-averaged data, and the scanner data for the same U.S. retailer and time period.

The distribution of the size of online price changes is clearly bimodal, with very few changes (close to 0%). The scanner data, by contrast, generate a unimodal distribution with a large share of small price changes close to 0%. This is the type of distribution that has been prevalent in the literature and motivated many papers, such as Midrigan (2011), to develop models that can account for small price changes.

Once again, weekly averaged prices can help explain the difference. They completely change the shape of the distribution by turning larger price changes from the tails into smaller ones at the center. Indeed, the weekly averaged and scanner data set distributions are very similar, with the exception of the two spikes that remain in the weekly averaged data near 0%. One explanation for the lack of spikes in scanner data could be the effect of coupons and loyalty cards, which can create additional tiny price changes to further smooth the distribution.

In figure 1, I also plot the distribution predicted by a simple Calvo model parameterized to match the frequency and mean absolute size of price changes in the weekly averaged online data.¹⁴ The resemblance with the unimodal distribution obtained from scanner data sets explains why papers such as Woodford (2009), which have models that can accommodate both state and time-dependent pricing, have tended to favor mechanisms that match the patterns predicted by the Calvo model. Instead, the actual distribution of price changes, observed with online data, has very little mass near 0% and two modes. This is consistent with a greater role for an adjustment or “menu” cost that makes small price changes suboptimal. In fact, the online data distribution is close to the predictions of the model in Alvarez et al. (2011), which combines both adjustment and information costs into the price-setting decision.¹⁵ In Section V, I provide a set of additional statistics that can be used to test and parameterize these types of models.

Finally, time averages also have an impact on the estimated hazard rates of price adjustment. Hazard rates measure the probability of a price change as a function of the time since the previous adjustment, and different sticky-price

¹³ The distribution of stores across postal codes in the online data was found by scraping the “find a store” form available on the retailer’s website.

¹⁴ The model, based on Calvo (1983), was simulated using the code from Nakamura and Steinsson (2010a). More details for the simulation are provided in the appendix.

¹⁵ See figure VI in that paper.

models will have different predictions about the shape of the hazard function over time. Adjustment-cost models, for example, tend to generate upward-sloping hazards if the shocks are persistent over time. Time-dependent models, by contrast, generate spikes in the hazard function at the dates when adjustment takes place.

Figure 2 shows the daily hazard rates using the daily scraped data, the weekly averaged data, and the scanner data for the same retailer. Details for the construction of these estimates are provided in the appendix.

The scraped data hazard function has a hump-shaped pattern, initially increasing and then gradually falling over time. By contrast, both the weekly averaged and the scanner data produce a fully downward-sloping hazard function. The reason is that with weekly averages, most of the probability of a price change occurs in the first week after the previous observed change. This makes the trend of the hazard downward sloping from the start, similar to those found in Campbell and Eden (2014) with scanner data. The online data results also show weekly spikes in the hazard rates that are not observable in the other data.¹⁶

Once again, measurement error distorts the stylized facts in the literature. The positive trend at the beginning of the online hazard functions suggests that adjustment costs play an important role, with “older” prices having a higher probability of experiencing a change.¹⁷ The weekly spikes are also consistent with models that have information costs, as in Alvarez et al. (2011).

B. Imputations and CPI Data

CPI microdata are not affected by time averaging because prices are usually collected once a month.¹⁸ There is, however, a sampling characteristic that can have an equally significant effect on price stickiness patterns: the imputation of missing prices.

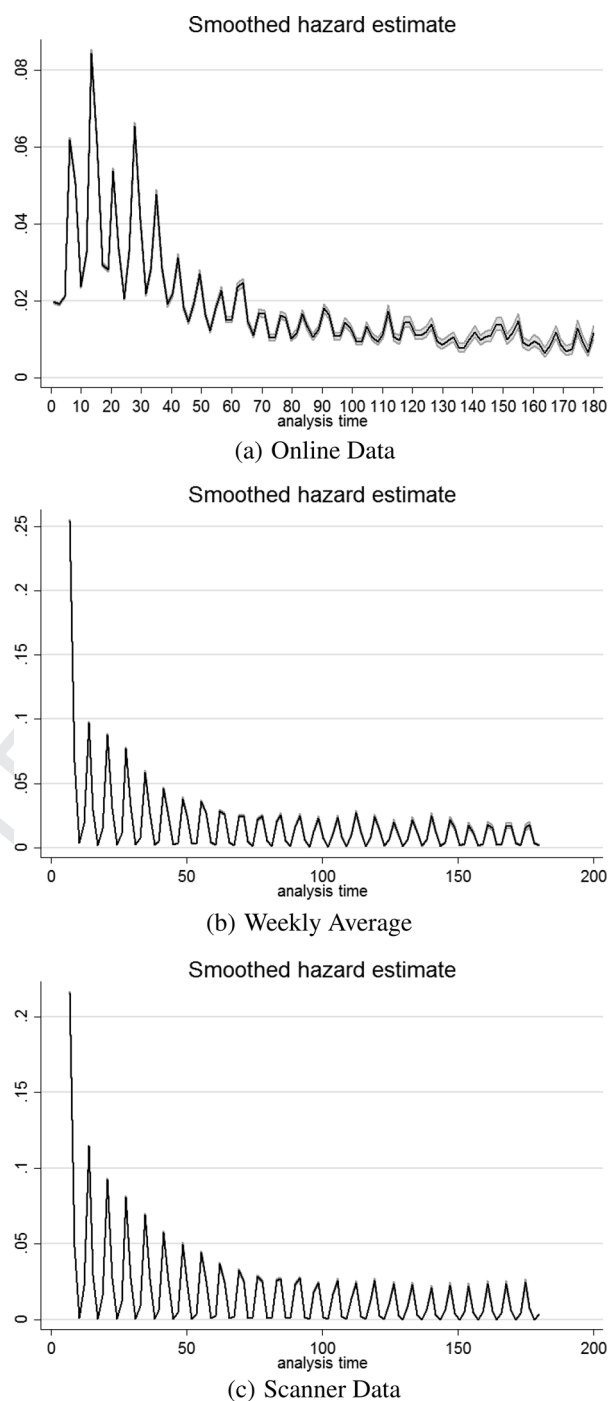
Missing prices occur when the person collecting the data at the store is unable to find a particular good in stock. In some cases, a product substitution may take place; at other times, the product may be simply marked as temporarily out of stock or as a seasonal product. Many national statistical offices, including the U.S. Bureau of Labor Statistics (BLS), use an imputation method, cell-relative imputation, to fill

¹⁶ The spikes that can be seen in figures 2b and 2c are just an artifact of the daily scale of the graphs. Plotting them on a weekly scale would make the hazard function appear smooth and completely downward sloping.

¹⁷ These hazard functions are still affected by survival bias, which makes hazard rates fall steadily over time as the share of stickier duration spells becomes more important. I provide some evidence of survival bias in the appendix.

¹⁸ There are some exceptions. The 2009 UN “Practical Guide to Producing Consumer Price Indices” (ILO et al., 2009) notes in point 10.26 on page 151 that “in many countries, prices for at least some products are collected more than once a month.” It argues that “it is inappropriate to use these individual prices [in a price index] as this will imbalance the sample of price quotations” and recommends that “instead the prices should be averaged before compiling the elementary price index.”

FIGURE 2.—HAZARD FUNCTIONS IN A U.S. SUPERMARKET



Initial 180 days shown. Left-censored spells are excluded.

noncomparable product substitutions or temporarily missing prices. Under this approach, prices are imputed using the average observed change in the prices of goods for a similar category. This can mechanically increase the frequency of observed price changes and also reduce their size. In particular, if these average changes used in the imputation come from a large number of items, some of which may

TABLE 5.—CELL-RELATIVE IMPUTATION OF TEMPORARILY MISSING PRICES

		United States	Argentina	Brazil	Chile	Colombia
Duration (months)	Monthly online data	4.7	3.43	2.03	4.38	2.29
	Cell-relative imputation (broad)	1.67	2.03	1.77	3.47	1.51
	Cell-relative imputation (narrow)	3.35	3.11	1.85	4.3	1.71
	Klenow and Kryvtsov (2008)	3.7				
Mean absolute size	Monthly online data	20.82	11.54	10.07	14.29	9.92
	Cell-relative imputation (narrow)	16.15	9.94	9.53	10.91	8.28
	Klenow and Kryvtsov (2008)	14.0				
Price changes below 1%]	Monthly online data	1.54	4.22	7.7	3.67	7.87
	Cell-relative imputation (narrow)	7.22	11.39	12.07	13.77	16.61
	Klenow and Kryvtsov (2008)	11				
Price changes below 5%	Monthly online data	7.09	33.68	42.48	25.32	41.72
	Cell-relative imputation (narrow)	25.16	47.19	49.79	41.79	55.54
	Klenow and Kryvtsov (2008)	40				

I first obtain the monthly frequency per individual good by calculating the number of price changes over the number of total valid change observations for a particular product. Next, I calculate the mean frequency per good category and, finally, the median frequency across all categories. I then compute implied durations using $-1/\ln(1 - \text{frequency})$, and convert them to monthly durations for comparisons across samples. Klenow and Kryvtsov (2008) results taken from tables II and III in that paper.

be increasing and some falling, the size of imputed price changes can be quite small in absolute value.¹⁹

Imputed prices due to item substitutions have been discussed in the literature before. These are usually clearly identified in the CPI data sets and can be excluded from the analysis. For example, both Klenow and Kryvtsov (2008) and Nakamura and Steinsson (2008) emphasize that removing price changes from forced substitutions reduces the frequency of price changes. Their impact on the size of price changes, however, has not been documented before. In fact, they are typically included when computing the distribution of the size of price changes in papers that use CPI data, such as Klenow and Kryvtsov (2008).

Imputed prices due to temporarily missing prices are also common in CPI data sets, though they have received less attention in the literature. They occur naturally as products go in and out of stock and can be quite prevalent in categories such as food, apparel, and electronics. Papers with U.S. CPI data tend to exclude them, as in Klenow and Kryvtsov (2008) and Nakamura and Steinsson (2008), but it is unclear how common or well identified they may be in other CPI data sets or whether all cell-relative imputations are clearly marked in the research databases.

To illustrate the effect of cell-relative imputations on both duration and the size of price changes, I simulate the procedure with the missing prices in monthly sampled online data. I first take the original scraped data (eliminating prices that had been carried forward) and keep only the price for the 15th of each month. Then, for each good, I impute missing prices within price spells by multiplying the previously available price by the geometric average of price changes for goods in the same category. The magnitude of the effect

depends on the frequency of missing prices (imputations) and also on the size of the cell chosen to compute these average price changes. In table 5, I compare the duration and size results from monthly sampled online data with those produced by using a cell-relative simulation with both a broad (COICOP class) and a narrow category definition (URL).

Table 5 shows that cell-relative imputation dramatically reduces the duration of prices. The drop is smaller when the imputation is based on the URL categories, which narrowly identify similar goods. For the rest of the paper, I use this narrower definition.

The monthly duration of prices falls from 4.7 to 3.35 months. This is close to the 3.7 months reported by Klenow and Kryvtsov (2008) for the duration of posted prices in the U.S. CPI research database. For their estimate, Klenow and Kryvtsov (2008) excluded imputed prices for out-of-stock or seasonal items but included price changes due to item substitutions, which the BLS also adjusts with cell-relative methods.²⁰

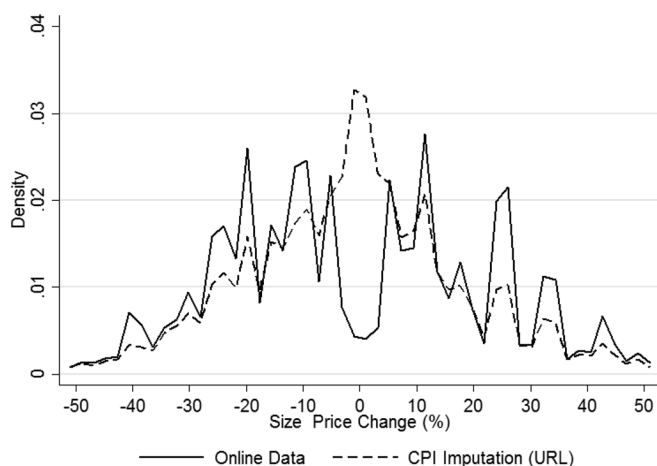
The effect of imputations on the size of price changes is equally large but has received no attention in the literature before. In the lower panels of table 5, I show the impact on the absolute size of price changes and the percentage of changes below the thresholds of 1% and 5%, which are also reported by Klenow and Kryvtsov (2008) on the U.S. CPI data. Cell-relative imputation decreases the absolute size of changes from 20.82% in the United States to 16.15% and makes the share of small changes increase significantly at the 1% and 5% thresholds, closer to the results reported by Klenow and Kryvtsov (2008).²¹

¹⁹ Until January 2015, the BLS used item strata, which are relatively broad product categories, as the “cell” used for imputation. It has now moved to using narrower elementary-level items (ELIs). See Bureau of Labor Statistics (2015b) for a description of the recent changes. In addition to the categories of goods, the imputations are applied for a given geographical aggregation level. This was traditionally the CPI index area and is now being replaced with the narrower primary sampling unit. Geographical aggregation does not apply to the data of this paper, but it is potentially another reason for measurement bias in CPI microdata.

²⁰ Klenow and Kryvtsov (2008) report an implied median duration of 8.7 months for items excluding substitutions and all sales (regular prices). They also report a duration of 7.2 for regular prices. Assuming the only difference between these two numbers is due to substitutions, the impact of 1.5 additional months for excluding substitutions is also close to the 1.4 months I get in my estimates with posted prices.

²¹ While cell-relative imputation is able to bridge the gap between my results and previous papers with CPI data, the comparison is complicated because there are many things that are potentially different between the data sets. In particular, Klenow and Kryvtsov (2008) used data from a larger

FIGURE 3.—DISTRIBUTION OF THE SIZE OF PRICE CHANGES IN THE UNITED STATES



Data for all U.S. retailers are included. See the appendix for similar results across U.S. sectors and other countries.

Figure 3 compares the distribution of price changes for both online and the cell-relative imputation. The online data distribution is different from the one in figure 1 because I include all sectors (not just supermarket products). There are more spikes than before, but a feature that is common is the lack of small price changes, particularly between -5% and $+5\%$. Just as with time averages, the main impact of cell-relative imputation is to increase the number of small changes. This makes the distribution unimodal, with a large mass of price changes close to 0% and a kurtosis that rises from 3.96 to 5.45.

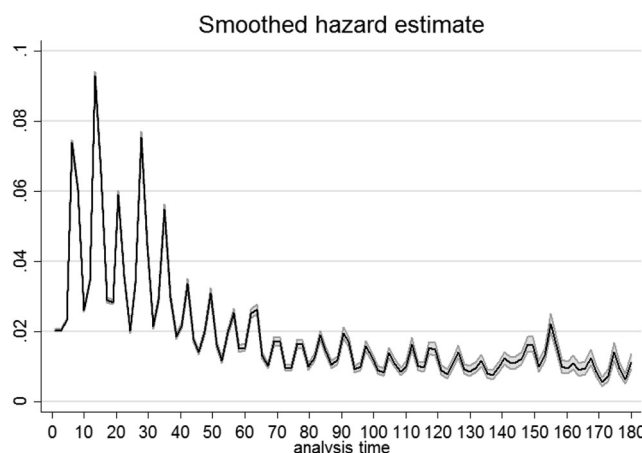
Finally, figure 4 shows that cell-relative imputation produces a downward-sloping hazard function similar to the ones found in the literature. In this case, I plot the hazard function for Food and Non-Alcoholic Beverages to compare with the results for “Processed Food” reported by Nakamura and Steinsson (2008), but the results are similar for all categories. The effect of imputations is analogous to that of time averages, although the cause is different. Using an imputed price tends to create two consecutive price changes in the data, increasing the hazard rate in the first month.

Although the hump-shaped pattern of the online hazard function does not match the empirical results in Nakamura and Steinsson (2008), it resembles the predictions of the model in that paper. In fact, the authors mention that the main difference between their model’s prediction and the results with the CPI data is precisely the behavior of the hazard during the first few months, which increases in the model but not in the CPI data.²² In categories such as Food and

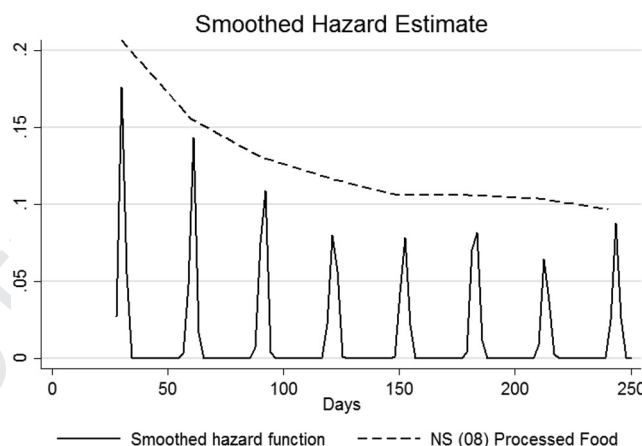
number of sectors in the United States and for a different time period (1988–2004). I discuss possible sector differences later, when I make comparisons to the Nakamura and Steinsson (2008) results.

²² See Nakamura and Steinsson (2008).

FIGURE 4.—HAZARD FUNCTION WITH CELL-RELATIVE CPI IMPUTATION



(a) Online Data



(b) Cell-Relative Imputation

Hazard function for Food and Non-Alcoholic Beverages in the United States. Initial 180 days of smoothed hazard function shown. Cell-relative imputation of temporarily missing prices in the monthly data done at the URL level. Left-censored spells are excluded. Results labeled “NS (08)” are the monthly hazards for “Processed Food,” obtained from Nakamura and Steinsson (2010b).

Non-Alcoholic Beverages, this initial increase is not even observable unless we use daily data.

Despite these results, the actual bias in CPI data sets may be lower than what is implied in my simulations, particularly for the U.S. data. There are two main reasons for this. First, the number of temporarily missing observations generated by my simulation is higher than the 7% reported by Klenow and Kryvtsov (2008) for BLS data. Second, not every missing price may be imputed using cell-relative methods. Still, there are other sampling characteristics in CPI data that can increase the magnitude of the bias. For example, Eichenbaum et al. (2014) show that unit values and composite-good pricing can account for a large share of changes smaller than 1% in the U.S. CPI data. And nonmissing prices can also be affected by imputations, as statistical offices often adjust price observations for coupons, rebates, loyalty cards, bonus merchandise, and quantity discounts, depending on the share of sales that had these discounts during the collection period.

TABLE 6.—SALES AND REGULAR PRICES

		United States	Argentina	Brazil	Chile	Colombia
Observations with sales		4.68%	2.55%	3.04%	3.7%	2.97%
Duration (months)	Monthly online data (posted)	4.7	3.43	2.03	4.38	2.29
	Monthly online data (ex-sales)	7.62	3.92	2.26	6.03	2.6
	Weekly average(ex-sales)	3.19	2.17	1.21	2.93	1.45
	Cell-relative imputation (ex-sales)	4.7	3.51	2.00	5.47	1.91
Mean absolute size	Monthly online data (posted)	20.82	11.54	10.07	14.29	9.92
	Monthly online data (ex-sales)	19.12	10.7	9.17	12.76	8.9
	Weekly average (ex-sales)	10.87	5.58	5.34	7.41	5.15
	Cell-relative imputation (ex-sales)	13.93	9.32	8.63	9.61	7.34

To obtain monthly implied durations, I first compute the monthly frequency per individual good by calculating the number of price changes over the number of total valid change observations for a particular product. Next, I calculate the mean frequency per good category and then the median frequency across all categories. Finally, I compute implied durations using $-1/\ln(1 - \text{frequency})$ and convert them to monthly durations for comparisons across samples.

Examples of these and other price adjustments are described in the BLS *Handbook of Methods*.²³

Overall, my results strongly suggest that imputed prices can be an important source of measurement bias for some stickiness statistics. The extent to which specific CPI data sets are affected is likely to vary across countries and time periods, but researchers need to be aware of the potential biases and should try to adjust for them.

IV. Robustness: Sales, Retailers, and Sectors

One of the main implications from the previous section is that differences in the sampling characteristics of the data can have a big impact on stickiness statistics. In this section, I show that these results are robust to the removal of sale prices and the use of alternative retailers or sectors. I also show how different sampling intervals can affect durations. Other robustness tests are provided in the appendix.

A. Sales

In principle, we could expect the effects of the two sampling characteristics described in this paper to be particularly large in when there are many sales. For example, most of the price changes that monthly sampling miss may be caused by short-term sales or stock-outs that lead to cell-relative imputations. To see if a measurement bias exists in regular prices, I repeat the analysis after excluding sale observations. Sale prices can be identified in online data using sales flags that are sometimes captured by the scraping software from images or explicit mentions of a sale price. But not all the data sets in this paper have that information, so in this section, I use a simple algorithm to identify sales based on the behavior of prices over time. In particular, I use a V-shaped sale algorithm used by Nakamura and Steinsson (2008) and others in the literature. It identifies sales by looking for prices that initially fall and then return to the same previous level for a period no longer than thirty days. While it may fail to identify sales that have more complicated patterns, including those that end with prices that are slightly higher than the previous price, an important advantage is that I can apply it to all retailers and countries.

Table 6 shows that the number of V-shaped sale prices as a share of all observations is highest in the United States, at 4.68%. The duration and size results labeled “Ex-sales” are computed after excluding sale prices by carrying forward the last available nonsale price until the sale ends. The resulting prices are then used to compute the monthly durations and size of price changes, using the same methods as before for all sampling simulations.

As both Klenow and Kryvtsov (2008) and Nakamura and Steinsson (2008) have previously documented, durations are higher for regular prices that exclude sales. For example, the implied duration for the United States rises from 4.7 to 7.62. The effects are naturally stronger in countries with a larger share of V-shaped sales, such as the United States and Chile.

More important for this paper, removing sales does not eliminate the problems associated with the sampling and measurement biases in scanner and CPI data. The use of weekly averages or cell-relative imputation also tends to decrease implied durations and reduce the size of price changes. In the case of the United States, durations fall from 7.62 to 3.19 and 4.7 months, while the absolute size of changes drops from 19.12% to 10.87% and 13.93%. A similar effect can be seen in the other countries. In fact, the countries with the largest share of sale observations, the United States and Chile, are still the ones where the effect of the sampling bias is strongest.

B. U.S. Retailers and Sectors

My findings are also robust when using data from different retailers, as shown in the appendix with the U.S. data. The fact that these companies sell different types of goods suggests that my results are not limited to a single sector. This can be seen more explicitly in table 7, where I present the results for the U. S. data categorized at the first level of the UN’s COICOP classification structure.

All sectors are affected in similar ways. Weekly averages reduce monthly durations by approximately 70% on average, while cell-relative imputation makes them fall by about 30%. The median absolute size of price changes falls by approximately 50% with weekly averages and 25% with cell-relative imputations.

²³ See Bureau of Labor Statistics (2015a).

TABLE 7.—U.S. SECTORS

COICOP Code*	Food and Beverages 100	Alcoholic Beverages 200	Apparel 300	Household Goods 500	Health 600	Communications 800	Electronics 900
Duration (months)							
Monthly online data	2.12	2.3	3.09	5.84	4.69	5.8	7.41
Weekly averaged	0.73	0.66	1.69	1.64	1.05	1.68	2.66
Cell-relative imputation (narrow)	1.45	2.07	2.35	4.11	3.35	3.82	4.27
Nakamura and Steinsson (2008)	2.6	2.4	2.69	3.8	5.91	5.2	3.18
Median absolute size							
Monthly online data	23.89	16.14	28.6	14.25	25.18	20.39	15.18
Weekly averaged	14.16	8.73	14.2	7.89	12.21	9.46	7.99
Cell-relative imputation (narrow)	19.79	13.78	24.23	10.64	15.63	14.23	11.18
Nakamura and Steinsson (2008)	27.66	11.09	30.7	12.59	18.41	22.2	14.26
Missing observations	21.19%	26.90%	27.78%	31.16%	34.14%	36.23%	41.08%

*Weighted monthly duration and weighted median absolute size. Results computed using only the subsectors for which there are also online data. ELI-level CPI results obtained from Nakamura and Steinsson (2010b).

TABLE 8.—SAMPLING INTERVAL: DAILY, WEEKLY, AND MONTHLY

		United States	Argentina	Brazil	Chile	Colombia
Duration (months)	Daily online data	2.68	2.12	1.27	2.5	1.7
	Weekly online data	2.91	2.43	1.48	2.92	1.99
	Monthly online data	4.7	3.43	2.03	4.38	2.29
Mean absolute size	Daily online data	22.08	12.45	11.52	15.07	10.98
	Weekly online data	21.98	12.22	11.46	14.66	10.74
	Monthly online data	20.82	11.54	10.07	14.29	9.92

Weekly data are sampled each Wednesday. Monthly data are sampled on the 15th of each month. Results are robust to picking other days of the week or month. To compute the duration of price changes, I first obtain the frequency per individual good by calculating the number of price changes over the number of total valid change observations for a particular product. Next, I calculate the mean frequency per good category and the median frequency across all categories. I then compute implied durations using $-1/\ln(1 - \text{frequency})$ at the sampled interval. Finally, I convert daily and weekly durations to monthly durations for comparisons across samples. The statistics for the United States are weighted using CPI expenditure weights. No weights are used in the other countries.

These sector-level results can be compared to the U.S. CPI statistics reported by Nakamura and Steinsson (2008). To make the comparison as close as possible, in table 7, I show the median size of absolute changes and recompute the CPI results reported by Nakamura and Steinsson (2008) to include only the subsectors that are covered by the online data. Given that Nakamura and Steinsson (2008) explicitly exclude imputed prices due to substitutions and temporarily missing observations, we can expect the monthly sampled online prices to approximate their results. This seems to be the case for durations in Food and Beverages, Alcoholic Beverages, Health, and Communications, where their implied durations are closer to the result with online data. However, the estimates for Household Goods and Electronics appear to be quite different in terms of duration. In these cases, the Nakamura and Steinsson (2008) durations are shorter and closer to the numbers produced by cell-relative imputations. The last row in table 7 suggests that the differences might be related to the share of missing observations in the data. In particular, electronics tend to be frequently missing and out of stock in the online data, so it is possible that some of the prices in the CPI microdata were also missing and imputed but not identified as such in the database. Nevertheless, there are many other reasons why the online results could be different from those in Nakamura and Steinsson (2008), including the fact that time periods are not the same.

C. Sampling Interval: Daily, Weekly, and Monthly Data

Online prices are collected every day, scanner data sets every week, and CPI data once a month (and sometimes

every two months). These differences in sampling intervals can affect the measured duration and size of price changes, even if the underlying price data remain the same. The reason is that many price changes can take place within the sampling interval. For example, monthly sampled data can miss a lot of price changes that occur during the month. An advantage of using high-frequency online data is that I can quantify these sampling-interval effects.

To isolate the differences that come exclusively from the sampling interval, in table 8, I compare the duration and absolute size of price changes in online data when sampled at daily, weekly, and monthly intervals. The weekly data are sampled each Wednesday, and the monthly data are sampled on the 15th of each month. Results are robust to picking other days of the week or month, as shown in the appendix.

Daily and weekly sampled data have similar durations and sizes of price changes in every country. This is because few products have more than one price change per week. Picking a different day of the week to do the sampling can affect the timing somewhat, but does not affect the implied duration or statistics related to the size of price changes.

Monthly sampling, by contrast, tends to increase durations considerably. Prices appear stickier with monthly data because many price changes within the month are not observed. This affects durations but not the absolute size of price changes, as can be seen in the lower panel.

Whether these effects are relevant depends on whether we care about high-frequency price changes. Many of these temporary price changes are connected to sales, which the literature has tended to exclude in the past. At the same time,

if the bias introduced by monthly sampling is stable over time, then papers that focus on the dynamic properties of stickiness are not significantly affected. On the other hand, using daily data can compensate for the other two biases mentioned in the previous section. In any case, care should be taken to acknowledge and account for any effects introduced by the use of high-frequency sampling in the data.

V. Implications for the Stickiness Literature

The results thus far suggest that some empirical “stylized facts” in the stickiness literature are affected by sampling biases and measurement errors caused by the characteristics of both scanner and CPI data sources.

For the empirical literature, this implies that prices are stickier than we previously thought (conditional on the sampling interval) and that most of the “small” price changes observed in previous papers are spuriously caused by the sampling characteristics of both scanner and CPI micro data sets.

For the theoretical literature, my findings suggest that more caution is needed when interpreting the stylized facts. In particular, less emphasis should be placed in trying to explain frequent and small price changes or downward-sloping hazards. Many of the pricing behaviors observed with online data resemble the ones produced by models that allow for a combination of observations and menu costs, such as the one developed by Alvarez et al. (2011). But this paper is not meant to provide empirical evidence for a particular model. In fact, for some applications, the specific model used might not really matter. For example, Alvarez et al. (2016) show that a sufficient statistic for the real effects of monetary policy in a large set of models is given by the ratio of the frequency and the kurtosis of the distribution of price changes. To rely on these statistics, however, we need to make sure we measure them with as little error or bias as possible. Furthermore, the heterogeneity in pricing behaviors, documented in this and other papers, implies that we need to measure them with a large number of retailers, sectors, and even countries in order to have robust estimates.

With this goal in mind, in table 9, I show summary statistics using scraped data from 181 retailers in 31 countries. This large cross-section of retailer-level information can be used to evaluate the robustness of my findings in the previous section, parameterize models, and shed some light on pricing behaviors across countries. These prices were collected by PriceStats, a private company connected to the Billion Prices Project at MIT.²⁴ I used them to compute the monthly duration for both posted and regular prices, as well as several moments of the distribution of price changes. Additional statistics are shown in the appendix. The last column shows the monthly implied durations from the literature, summarized

and reported by Klenow and Malin (2010) from different papers that use CPI data in different countries.

Comparing columns 3 and 9, it is clear that monthly sampled online prices are stickier than those previously reported in the literature that uses CPI data. For example, in the United States, the mean implied duration for monthly sampled online prices is 9.5 months when computed with 29 retailers, much higher than the comparable implied durations from mean frequencies reported by Klenow and Malin (2010) at 3.2 months. It is also stickier than the 4.6 months from weighted medians reported by Nakamura and Steinsson (2008). If I remove symmetric V-shaped sales, the duration rises to 12 months, twice as high as the 6 months for the equivalent estimate in Nakamura and Steinsson (2008).²⁵ Something similar happens in other countries where mean durations have been reported by various papers in the literature. It is important to note that some of the differences may be caused by compositional differences. As mentioned before, there are many things that are different in my results relative to others in the literature, including the time periods and the sectors covered in each country. Nevertheless, the pattern of higher durations with online data seems to be quite robust.

Table 9 also shows that the results obtained for five countries in section IIIB are typical of a much larger set of economies. In particular, the median duration for posted prices is relatively high, at 9.7 months; small price changes are also rare in most countries, with a median of 4.1% below 1% in absolute value, and the median absolute size of price changes is large, with a median of 17.6%.

There is also a lot of heterogeneity in results across countries that can be used to make cross-country comparisons. One interesting fact is that inflation rates are not correlated with the overall frequency or size of price changes. For example, Venezuela has an annual inflation rate of 37.5%, yet it is one of the stickiest countries in the sample. Turkey has twice as much inflation as Chile, even though the duration and size of changes are similar. In the appendix, I show that inflation is correlated with the relative frequency of increases over decreases and the relative size of price increases over decreases. This suggests that factors at the country level can make prices in a country more “flexible” or “sticky” or have “big” or “small” changes, but what really matters for inflation (and therefore the real effects of monetary policy) is how much more frequent (or larger) price increases are relative to decreases.

Understanding the results for each country is beyond the scope of this paper, but table 9 illustrates how online data can be used to produce these statistics with identical methods across countries and over time. Doing so will not only allow researchers to better understand price stickiness, but, perhaps more important, eventually provide central bankers

²⁴ I cofounded PriceStats LLC. More details on the data collection methods and applications for these data sets can be found in Cavallo and Rigobon (2016).

²⁵ See table IX in Nakamura and Steinsson (2008). I use a sales filter equivalent to the one they label “Sale Filter B, 1-month window,” which has a monthly frequency of 15.3%, and therefore an implied duration of 6.02 months.

TABLE 9.—IMPLIED MONTHLY DURATIONS AND SIZE OF CHANGES IN 31 COUNTRIES

Country	(1) Retailers	(2) Inflation (%)	(3) Duration (months)	(4) Duration ExSales (months)	(5) Size Under 1%	(6) Size Under 5%	(7) Mean Absolute Size	(8) Kurtosis	(9) Duration Literature (months)
Argentina	14	17.1	4	4.3	2.8	22.4	13.8	7.1	
Australia	5	2.5	6.2	7.1	5.4	16.1	25	3.7	
Austria	2	2.4	9.7	16.3	16.4	50.8	11.9	3.4	6.1
Belgium	4	1.9	11.8	12.9	8.2	49.3	9.3	6.7	5.4
Brazil	5	6.1	4.5	5.1	4.1	24.7	12.9	5.2	2.2
Canada	7	1.8	12.3	14.7	2.3	13.8	21.3	4.7	
Chile	5	3.2	7.4	8.8	3.5	19.5	18	4.8	1.6
China	3	3.2	21.9	26.5	4.7	27.5	17.2	6.1	
Colombia	4	2.9	11.3	12.3	3.5	23.2	16.7	5.7	
France	2	1.5	8.9	10.1	11.2	37.3	15.9	4.9	4.8
Germany	6	1.7	19.5	21.9	3.4	25	15	5.6	8.3
Greece	6	.5	7	8.2	12.1	28.1	15.8	4.9	
Hungary	3	2.8	11.6	15.4	4	12.3	23.8	3.2	6.1
India	3	8.7	9.8	11.8	3.2	20.5	18.2	4.4	
Indonesia	4	5.5	10.8	13.1	4.9	30.3	12.3	6.6	
Ireland	6	1	7.1	9.8	1.6	8.7	29.2	3.5	
Israel	4	1.8	8.3	10.3	3.9	10.2	23.2	3.5	3.6
Italy	4	1.9	8.7	9.7	10.1	25.2	20.3	4	9.5
Korea	6	2.2	15.2	17.9	2.2	17.4	17.5	4.4	
Netherlands	2	2	18.7	20.6	3.2	14	21.2	4.9	5.5
New Zealand	5	1.9	6.6	10.6	2.4	8	27.4	3.2	
Norway	4	1.5	6.3	7.4	7.9	21.1	19.9	5	4.0 (4.2)
Portugal	2	1.7	16.4	19.1	5.5	24	15	5.1	4.0
Russia	5	7	8.5	8.9	14.4	36.7	13.1	5.6	
Singapore	7	3.3	17.8	21.7	1.5	12.8	17.4	4.1	
South Africa	5	5.6	10.2	11.5	7.5	14.9	19.8	5.3	
Spain	9	1.7	11.4	13	7	35.9	12	5.3	6.2
Turkey	7	7.9	7.5	9.8	1.9	22.3	17.6	4.1	
United Kingdom	11	2.8	8.6	21.7	4.9	14.1	26.2	4.1	4.7 (6.2)
United States	29	2.1	9.5	12.1	2.5	10.1	24.8	4.3	3.2 (4.2)
Venezuela	2	37.5	13.7	13.8	5.2	24	23.6	3.4	
Mean	6	4.6	10.7	13.1	5.5	22.6	18.6	4.7	
Median	5	2.4	9.7	12.1	4.1	22.3	17.6	4.8	

I use monthly-sampled data collected from 181 large multichannel retailers in 31 countries selling food, electronics, apparel, furniture, household, and related goods. Prices were collected between 2007 and 2014, with different start dates for each retailer. Each statistic is calculated at the retailer level and then averaged within countries. The average gives the same importance to each retailer within a country. The simple mean and median over all countries are reported on the last rows. The mean duration for Eurozone countries is 11.9. The column labeled "Literature" shows the implied monthly durations computed from the mean monthly frequencies reported in table 1 of Klenow and Malin (2010), with results that exclude sales in parentheses. See that paper for primary sources, including Alvarez (2008). Average annual inflation rates for the period 2008 to 2014 from the IMF World Economic Indicators database. Argentina's inflation from Cavallo (2013). The kurtosis of the distribution of the size of price changes is computed using standardized price changes at the URL level.

with measures of stickiness that can be used to inform policy decisions in real time.

VI. Conclusion

This paper introduces a new way of collecting price data and applies it to study basic stylized facts in the price stickiness literature. Scraped data, obtained directly from online retailers, provide a unique source of price information. Prices are easier to collect than in CPI and scanner data, and can be obtained with daily frequency for all products sold by retailers around the world. The data are available without any delay, and the collection methodology can be customized to satisfy the specific needs of sticky-price studies. More important for the stickiness literature, scraped data are free from common sources of measurement error, such as time averages and imputation methods that can affect traditional microprice data sets.

I use the scraped online data to show how measurement bias affects three common stylized facts in the literature:

the duration of price changes, the distribution of the size of changes, and the hazard functions. I argue that two sampling characteristics in scanner and CPI data sets can produce biased results for these statistics. Weekly averages and price imputations tend to reduce the duration of price changes (particularly in scanner data), decrease their size, and make the hazard function more downward sloping over time. I show this with sampling simulations in my own data and confirm this explicitly in scanner data by comparing both online and scanner data collected from the same retailer and time period. I further show that these sources of measurement error account for nearly all the differences between my results and those in the existing empirical literature. The paper ends with a discussion of the implications for the stickiness literature and a table with summary statistics on stickiness that uses online data from 31 countries. These results provide confirmation that prices are stickier than previously reported in many countries and that small price changes are actually rare in the data. Furthermore, they provide cross-country statistics that can be used to parameterize sticky-price models and make cross-country comparisons.

I emphasize that there is nothing intrinsically wrong with scanner and CPI data sets. For all types of data, including scraped online prices, there are advantages and disadvantages for various uses. The point I make in this paper is that researchers need to be aware of how the sampling and other characteristics of the data may affect what they are trying to measure. Scanner and CPI data were not designed for the measurement of price stickiness, so it is natural that some of the sampling decisions made by the data collectors are not ideal for this purpose. Unfortunately, it is not always possible to control for these biases because the data sets available for research may not provide enough details on how prices are treated or adjusted. One of the main advantages of online data is that we can collect the prices as posted by the retailers, and we can decide how to treat them depending on the particular statistic that we are trying to measure. The ability to access the raw and unfiltered data is a characteristic of many other new big data sources of information, from crowd-sourced data, to Twitter feeds and mobile phone sensors.

This paper focuses on how scraped online data affect the measurement of stylized facts in the stickiness literature, but the potential uses of scraped data in macroeconomics go far beyond those explored here. For example, scraped prices can be used to create daily price indexes that complement official statistics, compare and test theories of international prices, and better measure exchange rate and commodity shocks pass-through. As discussed in Cavallo and Rigobon (2016), many of the data sets collected by the Billion Prices Project, including those in this paper, are publicly shared at bpp.mit.edu, so other researchers can further explore these and other potential uses of scraped data.

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