Reply to:
"The Cyclicality of Sales, Regular and Effective Prices: Comment"
by Etienne Gagnon, David Lopez-Salido, and Jason Sockin

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Gagnon et al. (2015) claim to identify five issues in Coibion et al. (2015):

1. Truncation of monthly item and UPC price movements (Section 3.1).
2. Treatment of missing observations (Section 3.2)
3. Incorrect time aggregation of posted and effective price inflation (Section 3.3)
4. Spurious index jumps due to clearance sales (Section 3.4)
5. Incorrect stitching of subsample (Section 3.5)

In this reply to Gagnon et al. (henceforth GLS), we address each of the comments and demonstrate that while some concerns may be warranted, they do not have material effects on the main conclusions of Coibion et al. (henceforth CGH). We discuss each issue in turn.

1. Truncation of monthly item and UPC price movements

Micro-level data are characterized by enormous volatility. As documented by Midrigan (2011) and others, the distribution of price changes is leptokurtic and so moments of the data in small samples (e.g., mean price change) may be sensitive to a few observations. Hence, it is conventional to limit variation at the most disaggregated level to avoid aggregate statistics being driven by a handful of outliers.[1] Consistent with this practice, CGH limit micro-level variation in price changes.

GLS claim that CGH use an aggressive approach to truncate observations with large price changes which dampens the response of posted-price inflation and makes effective-price inflation look more sensitive to unemployment than posted-price inflation.

To be clear, CGH do not *truncate* the sample as claimed in GLS; that is, CGH *do not* discard observations with large price changes. Instead, CGH *censor* price changes; that is, if a price change is greater than X%, CGH set the price change to X%. In contrast, GLS discard the *whole* price history for a given good (that is, a given UPC in a given store in a given city) if *any* of the price changes for this good are greater than X% in absolute value. Their threshold for X is set at 666% (annualized). This "filter" removes 0.5% percent of price data.

[1] For example, in a famous analysis of micro-level job flow data, Davis, Haltiwanger and Schuh (1996) construct statistics for job flows such that the variation is limited to [-1,1].

As indicated in GLS, changing the "truncation point"—again, while we continue to use the term from GLS, one should be clear that this is a censoring point in our analysis (i.e., we cap price changes rather than remove price observations)— makes little difference for the results in CGH when one aggregates across goods and stores using equal or expenditure-based weights. We reproduce this result in rows (1)-(3) in each panel of Table 1. In short, the sensitivity of effective-price inflation to local unemployment is larger than the sensitivity of posted-price inflation.

To give a better sense of how changes in the truncation point affect the series, Figure 1 presents time series for effective- and posted-price inflation rate for a typical good sold (a Unilever detergent) in Chicago. Note that increasing the truncation point adds considerable noise in monthly inflation. However, this increased variation is short-lived so that when we aggregate inflation to the annual frequency the difference in high-truncation-point series and low-truncation-point series is smaller and the two series track each other rather closely. In other words, raising the truncation point mainly adds noise to the series.

GLS observe that when one uses expenditure shares to weigh *city/category* observations the difference in estimated sensitivities shrinks to zero as the truncation point increases. We also reproduce this result in Table 1 (e.g., compare columns (1) and (5) in the table in rows (4) and (5) of each panel). At the same time, the small difference in the sensitivity depends on a particular weighting used in CGH. Specifically, CGH's weights for categories are set in the following way:

$$\omega_{cmt} = \frac{TS_{cmt}}{\sum_c \sum_m TS_{cmt}}$$

where m, c, t index market (city), category, and time (month), TS is the volume of sales. Note that the weight depends on the size of the market and the city. That is, the category "carbonated beverages" (a big category) in New York City (a large market) has a much larger weight that the category "paper towels" (a small category) in Pittsfield, MA (a small market).[2] As a result, the sensitivity estimated in weighted regression will tend to depend disproportionately on a few categories in a few markets.

To ensure that results are not determined by a handful of categories in New York City and other super-sized cities, one can consider an alternative approach to construct weights:

$$\widetilde{\omega}_{cmt} = \frac{TS_{cmt}}{\sum_c TS_{cmt}}$$

so that weights add up to one for each city and each city receives an equal weight. This approach is desirable because it treats each geographic market as an equally informative source of variation, which seems like the most natural benchmark to use. Results in rows (6) and (7) of Table 1 demonstrate that using $\widetilde{\omega}_{cmt}$ yields a large difference between effective- and posted-price inflation across *all* truncation points.

Also note that the difference between results in rows (4) and (6) and rows (5) and (7) is small when the truncation point is low (as in CGH's baseline; column (1)) but it becomes large as the truncation point increases. At the time of writing our paper, we experimented with alternative sets of weights and we

[2] Because the weights may vary over time due to trends and seasonal factors, CGH use the median (over time) value of ω_{cmt} to weight observations.

found that the choice between ω_{cmt} and $\widetilde{\omega}_{cmt}$ made little difference and so we settled on ω_{cmt}. GLS did not explore these alternatives and came up with a strong conclusion that the CGH results are sensitive to the truncation point. We hope that our discussion makes clear that such a conclusion is unwarranted and instead only obtains when a large weight is applied to a few very large categories in large markets.

2. Treatment of missing observations

GLS make two claims with regards to missing observations. First, the share of missing values is 39% and so the problem is massive. Second, CGH impute zero for missing price changes. GLS use the average inflation rate in a strata to impute the inflation rate.

These two claims are problematic for several reasons.

First, if one uses expenditure weights to aggregate across stores and UPCs and condition on having at least one non-missing price observation in a month, the share of missing observations is less than 16%. Thus, the scope of the problem may be exaggerated by GLS. Also note that missing values are not really a problem for the effective price inflation because some store in some week of a given month is very likely to sell a given UPC. In other words, the missing value problem applies only to the posted-price inflation.

Second, an implicit argument in GLS is that during times of high unemployment, price-change observations are increasingly missing, CGH impute zero to these observations and thus CGH create a systematic bias in the sensitivity of posted-price inflation to unemployment. To assess the validity of this logical chain, we calculate the number of non-missing price observations for each good/store/city/month. For example, if we have two non-missing observations in a month and the month has 5 weeks, the share is 2/5. Then we aggregate results to the category/city/month level and regress the resulting average on local unemployment rate (controlling for time, city/category fixed effects, which is the preferred specification in CGH). The estimated effect reported in Table 2 is positive and significant: that is, one is *less* likely to observe missing values in recessions and so the quality of data is if anything *better* in recessions. However, the magnitude of the sensitivity is relatively small: a one-standard deviation increase in UE rate (0.021) is associated with 0.33*0.021=0.007 increase in the share of missing observations. In summary, we find little (if any) evidence that missing values are systematically more important in recessions than in expansions and, thus, imputing zeros creates a bias.

Third, using the average inflation rate in a strata as done by GLS is built on the assumptions that i) a price change in store A is a good predictor of a price change in store B; and ii) the pattern in assumption i) is stable. Neither of these assumptions is valid. Specifically, there is overwhelming evidence rejecting assumption i). For example, Klenow and Malin (2011), Dhyne and Konieczny (2007), and Gorodnichenko et al. (2014) document that synchronization of price changes across stores or across goods is very low. In other words, what store A does is largely irrelevant for predicting the behavior of store B. To test assumption ii), we construct synchronization rates for each UPC in a given week and market, aggregate it to monthly frequency at the level of market and product category, and regress the resulting synchronization rate on the local unemployment rate. We find (Table 3) that synchronization tends to fall in recessions thus making the relationship unstable and, consequently, potentially unsuitable for imputation.

Fourth, Klenow and Malin (2011) survey the vast literature on pricing behavior at the micro level and conclude that price changes are fairly infrequent. As a result, if one wants to predict what happens with a price, predicting no-change (i.e., zero price change) is a sensible option.

Fifth, GLS impute nearly 40 (!) percent of observations. Such massive imputation effectively creates a new data set. As a result, the properties of inflation series are likely to be altered and the results may be spurious.

Finally, we construct a measure of posted-price inflation which does not use *any* imputation and instead drops all observations that are missing values, and we explore its sensitivity to the local unemployment rate in Table 4. By avoiding all imputations, this is the most conservative approach toward dealing with the issue of missing observations. The results for no-imputation inflation are nearly identical to our baseline set of results. This is not surprising given that the two series (with and without our imputation) are highly correlated: the correlation is above 0.98.

We conclude that our imputation is unlikely to make a systematic bias in the sensitivity.

3. Incorrect time aggregation of posted and effective price inflation

While the raw data are at the weekly frequency, the CGH analysis is done at the monthly frequency because the unemployment rate is only available at the monthly frequency. GLS claim that the aggregation from weekly to monthly frequency for the *effective-price* inflation is incorrect because CGH fail to adjust aggregation for the fact that some months have five weeks.

To make the discussion of this point clear, we explain how CGH calculated the inflation rate in their code, then we show a simple modification to the code to address the five-week problem, and show how much difference it makes for the results.

For a given UPC/market/month, CGH calculate effective price as

$$p_{gmt}^{eff} = \frac{\sum_m \sum_s \sum_{w \in t} TS_{gmst}}{\sum_m \sum_s \sum_{w \in t} Q_{gmst}}$$

where g, m, s, w, t index goods (UPCs), markets, stores, weeks, and months, TS is total sales (dollars), Q is the number of units sold. Note that the summation over weeks is done only for weeks that fall in a given month. In the code, while p_{gmt}^{eff} is immediately calculated at the monthly frequency, the data are still in the weekly format. That is, for all weeks in month t, the effective price p_{gmw}^{eff} (note that the time index here is now week w rather than month t) is set equal to the same value of p_{gmt}^{eff}. Then CGH calculate the inflation rate using the four-week difference for each week: $\pi_{gmw}^{eff} = \log(p_{gmw}^{eff}/p_{gm,w-4}^{eff})$. Then CGH collapse the "weekly" inflation to monthly frequency: specifically, they calculate the *average* inflation rate across four (or five) weeks observed in a given month. More formally, the inflation rate for effective prices was calculated as:

$$\pi_{gmt}^{eff} = \frac{\sum_{w\in t} \pi_{gmw}^{eff}}{\#\{w \in t\}}$$

This was a simple (perhaps, an oversimplified) approach to control for seasonality in the data (specifically the varying length of months).

An alternative approach is to calculate π_{gmt}^{eff} directly from p_{gmt}^{eff} as follows:

$$\tilde{\pi}_{gmt}^{eff} = \log\left(\frac{p_{gmt}^{eff}}{p_{gm,t-1}^{eff}}\right)$$

Table 5 presents a fictitious example to illustrate potential differences between π_{gmt}^{eff} and $\tilde{\pi}_{gmt}^{eff}$. While there could be discrepancies between these two measures of inflation, π_{gmt}^{eff} and $\tilde{\pi}_{gmt}^{eff}$ co-move strongly. Also note that any discrepancy in one direction is offset later by a discrepancy in the opposite direction because π_{gmt}^{eff} can overlap across months. Thus, values of π_{gmt}^{eff} and $\tilde{\pi}_{gmt}^{eff}$ averaged over several months are even more strongly correlated. Note that in the empirical analysis of the sensitivity to unemployment, CGH cumulate monthly inflation rates into annual inflation rates thus reducing potential differences between π_{gmt}^{eff} and $\tilde{\pi}_{gmt}^{eff}$ to the point where it makes little difference for the estimated sensitivity.

To formally assess this conjecture, we re-run our analysis for effective price inflation using $\tilde{\pi}_{gmt}^{eff}$ instead of π_{gmt}^{eff}. Panel A of Table 6 shows results for π_{gmt}^{eff} (baseline reported in CGH), while Panel B of Table 6 shows results for $\tilde{\pi}_{gmt}^{eff}$. The results are very similar across panels. Hence, while using π_{gmt}^{eff} may create some mistiming in effective-price inflation, the practical importance of this mistiming is negligible.

4. Spurious index jumps due to clearance sales

GLS claim that removing clearing sales makes posted-price inflation more sensitive to unemployment to the extent that one cannot reject equality of estimated sensitivities of posted-price and effective price inflation. To remove clearance sales, GLS drop the last quarter of observations for every price history.

While this analysis by GLS is potentially interesting, it is not convincing for several reasons.

First, Nakamura and Steinsson (2008, Supplemental material) document that clearance sales are relatively frequent in apparel, home furnishings, and recreational goods. However, they conclude (p. 6 of the supplement), “Clearance sales play … essentially no role in other product categories.” Nakamura and Steinsson (2008) report that the fraction of price changes due to clearance sales is 0.3-0.5 percent. Given this tiny fraction, clearance sales appear to be highly unlikely to have a major impact on the level of prices at an aggregate level.

Second, the cyclical properties of clearance sales are far from being well-understood. Clearance sales may be due to price discrimination, inventory management reasons, or other reasons. The literature cited by GLS reveals the dearth of research on the matter rather than supports GLS’s claim. For example, GLS cite Broda and Weinstein (2010) documenting evidence on cyclical creation and destruction of barcodes (UPCs) in scanner data to support GLS’s claim. However, Broda and Weistein *do not* study clearance sales. They study only the effect of entry and exit on the conventionally constructed price index. To the best of our knowledge, there is no comprehensive analysis of the cyclical properties of clearance sales.

Third, GLS remove clearance sales by eliminating the last quarter of data for every price history. While this may seem an innocuous “filter”, one should bear in mind that product lives are relatively short in grocery-type data. Indeed, the median product life (measured as the difference between the first time a given UPC appears in a given store and the last time it appears in the store) is about 80 weeks. Eliminating a quarter of data effectively removes 15 percent of the data for a typical good. This appears to be a rather dramatic reduction in the sample.

Fourth, the life of a given UPC in a given store is much shorter than the life of a given UPC across stores in a given market. The median value for the latter is about 190 weeks. In other words, the fact that a UPC “exits” one store does not mean that the UPC exits the market. Thus, using the last observation of a given price history (i.e., a price path of a given UPC in a give store) is almost certainly not reflecting the product life as typically understood by economists.

GLS are apparently aware of the many thorny issues associated with the treatment of clearance sales and product entry/exit. Indeed, there could be not only clearance sales but also “introductory” price reductions which are not treated in the GLS analysis. GLS (p. 12) admit, “A fully satisfying treatment of biases due to basket turnover is beyond the scope of this paper [that is, their comment] because it would require the judgmental linking of millions of entering and exiting lines.” We agree that GLS do not provide a satisfactory treatment because their judgmental calls are not supported by rigorous testing of their assumptions, meticulous matching of entering and exiting goods, and their results are at odds with evidence documented in previous studies.

5. Incorrect stitching of subsample

GLS claim that CGH incorrectly join (“stitch”) two parts of the IRI Symphony data set (the first part covers 2001-2007, the second part covers 2008-2011).

GLS’s claim is based on the replication files provided by CGH. CGH’s code is a series of steps. Unfortunately, CGH mistakenly uploaded an outdated file for the step that corresponds to stitching the parts. The use of this incorrect file could not replicate the published results of CGH. The correct file, which is now uploaded to the AER website and can be used to replicate the published results of CGH, does not have problems with stitching.

At the same time, comparison of GLS and CGH codes reveals that GLS have an incorrect stitching. Specifically, IRI Sympshony changed the coding of some UPCs in such a way that different goods may be assigned the same UPC in the two parts of the data. CGH identify these cases by inspecting differences in the description of UPCs. Specifically, CGH check if the weight of an item is the same in the two parts. If the weight is different, a new UPC is assigned for the second part of the data. While the prevalence of this inconsistency in the UPC coding varies across categories, the incorrect stitching in GLS can create a spike in the frequency of price changes in 2008, the point of stitching. However, the quantitative significance of this mistake in stitching is small as the vast majority of goods continue to have consistent UPC coding.

6. Concluding remarks

Working with micro-level data involves making numerous judgement calls. The purpose of robustness checks is to explore the sensitivity of results to using alternative assumptions and conventions. Because the list of such checks is almost always very large, the published versions of academic papers typically contain only a limited set of results. The purpose of the replication files is to allow other researchers to tweak the code and thus provide additional sensitivity and robustness checks.

GLS identify several areas where the original CGH code may be modified. However, the changes proposed in GLS do not alter the main message of CGH that effective-price inflation is more sensitive to unemployment than posted-price inflation. Three suggestions ("truncation", "missing values", "aggregation") lead to minimal changes in the results. One suggestion ("clearance sales") is a radical departure from the conventional approach in measuring price changes in the literature and thus it is hard to evaluate the validity of the suggestion. It deserves a separate, thorough, complete analysis and a full scrutiny of the profession. And the last suggestion ("stitching") is incorrect.

We conclude that while GLS provide a valuable service in checking results of CGH, the results reported in CGH are qualitatively unchanged.

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Table 1. Effect of using alternative truncation points for CGH results.

row	Weight used in aggregation across stores and UPCs	Weighted regression	Weights to cities	Truncation point				
				1 (baseline)	2	3	4	5
				(1)	(2)	(3)	(4)	(5)
				Panel A: Effective-price inflation				
(1)	unweighted	No	Equal	-0.219***	-0.309***	-0.357***	-0.388***	-0.411***
				(0.0242)	(0.0294)	(0.0297)	(0.0300)	(0.0308)
(2)	city specific weights	No	Equal	-0.201***	-0.264***	-0.277***	-0.278***	-0.277***
				(0.0309)	(0.0360)	(0.0363)	(0.0354)	(0.0326)
(3)	country weights	No	Equal	-0.205***	-0.268***	-0.281***	-0.286***	-0.288***
				(0.0334)	(0.0386)	(0.0387)	(0.0379)	(0.0368)
(4)	city specific weights	Yes	Expenditure share	-0.136***	-0.136***	-0.112***	-0.0970**	-0.0902**
				(0.0271)	(0.0381)	(0.0424)	(0.0424)	(0.0414)
(5)	country weights	Yes	Expenditure share	-0.146***	-0.161***	-0.140***	-0.130***	-0.128***
				(0.0279)	(0.0389)	(0.0434)	(0.0456)	(0.0460)
(6)	city specific weights	Yes	Equal	-0.182***	-0.207***	-0.180***	-0.161***	-0.153***
				(0.0308)	(0.0457)	(0.0513)	(0.0498)	(0.0470)
(7)	country weights	Yes	Equal	-0.184***	-0.222***	-0.196***	-0.181***	-0.177***
				(0.0299)	(0.0448)	(0.0500)	(0.0498)	(0.0490)
				Panel B: Posted-price inflation				
(1)	unweighted	No	Equal	-0.0605***	-0.0833***	-0.0984***	-0.111***	-0.123***
				(0.0166)	(0.0263)	(0.0336)	(0.0389)	(0.0433)
(2)	city specific weights	No	Equal	-0.0773***	-0.0970***	-0.118***	-0.136***	-0.149***
				(0.0205)	(0.0296)	(0.0350)	(0.0383)	(0.0417)
(3)	country weights	No	Equal	-0.0751***	-0.0961***	-0.114***	-0.130***	-0.142***
				(0.0229)	(0.0337)	(0.0397)	(0.0437)	(0.0477)
(4)	city specific weights	Yes	Expenditure share	-0.0523**	-0.0718**	-0.0927***	-0.0994***	-0.0977**
				(0.0256)	(0.0318)	(0.0322)	(0.0335)	(0.0389)
(5)	country weights	Yes	Expenditure share	-0.0591**	-0.0935**	-0.122***	-0.128***	-0.120**
				(0.0288)	(0.0361)	(0.0367)	(0.0393)	(0.0464)
(6)	city specific weights	Yes	Equal	-0.0380	-0.0287	-0.0381	-0.0542	-0.0673*
				(0.0263)	(0.0334)	(0.0375)	(0.0384)	(0.0405)
(7)	country weights	Yes	Equal	-0.0341	-0.0430	-0.0602	-0.0751*	-0.0820**
				(0.0283)	(0.0367)	(0.0396)	(0.0387)	(0.0406)
	Observations			187,426	187,426	187,426	187,426	187,426
	Number of groups			1,550	1,550	1,550	1,550	1,550

Notes: The table reproduces table 1 in CGH (2015) for different values of the truncation point. The table reports estimated coefficients on local unemployment rate when we regress a measure of city/category inflation on local unemployment rate after controlling for city/category and month fixed effects. The truncation point X sets $(dlogP) * 12 = -X$ if $(dlogP) * 12 < -X$ and $(dlogP) * 12 = X$ if $(dlogP) * 12 > X$ for a price change at the level of good/store/category/city. Driscoll-Kraay (1998) standard errors are reported in parentheses. ***, **, * denote statistical significance at 1, 5 and 10 percent levels

Table 2. The sensitivity of the share of non-missing price observations to local unemployment rate

VARIABLES	weights		
	unweighted	city specific weights	country-level weights
	(1)	(2)	(3)
UE rate	0.227***	0.204***	0.238***
	(0.054)	(0.056)	(0.061)
Observations	204,476	204,476	204,476
Number of groups	1,550	1,550	1,550

Notes: The table reports results for regressing the share of non-missing observations on the local unemployment rate. City/category and month fixed effects are included but not reported. Driscoll-Kraay (1998) standard errors are reported in parentheses. ***, **, * denote statistical significance at 1, 5 and 10 percent levels.

Table 3. The sensitivity of the synchronization rate for price change observations to local unemployment rate (Approach D)

VARIABLES	weights		
	unweighted	city specific weights	country-level weights
	(1)	(2)	(3)
UE rate	-0.111	-0.372**	-0.343**
	(0.157)	(0.159)	(0.154)
Observations	204,476	204,476	204,476
Number of groups	1,550	1,550	1,550

Notes: The table reports results for regressing the synchronization rate for price changes on the local unemployment rate. City/category and month fixed effects are included but not reported. Driscoll-Kraay (1998) standard errors are reported in parentheses. ***, **, * denote statistical significance at 1, 5 and 10 percent levels.

Table 4. Effect of imputation for the sensitivity of posted-price inflation to local unemployment rate.

Weight used in aggregation across stores and UPCs ts	Truncation point				
	1 (baseline)	2	3	4	5
	(1)	(2)	(3)	(4)	(5)
Panel A: No imputation					
unweighted	-0.0763***	-0.102***	-0.119***	-0.135**	-0.148**
	(0.0218)	(0.0349)	(0.0450)	(0.0522)	(0.0580)
city specific weights	-0.0853***	-0.106***	-0.129***	-0.149***	-0.163***
	(0.0242)	(0.0355)	(0.0425)	(0.0468)	(0.0506)
country weights	-0.0850***	-0.107***	-0.127***	-0.145***	-0.158***
	(0.0272)	(0.0403)	(0.0481)	(0.0529)	(0.0573)
Panel B: Imputation (Baseline)					
unweighted	-0.0605***	-0.0833***	-0.0984***	-0.111***	-0.123***
	(0.0166)	(0.0263)	(0.0336)	(0.0389)	(0.0433)
city specific weights	-0.0773***	-0.0970***	-0.118***	-0.136***	-0.149***
	(0.0205)	(0.0296)	(0.0350)	(0.0383)	(0.0417)
country weights	-0.0751***	-0.0961***	-0.114***	-0.130***	-0.142***
	(0.0229)	(0.0337)	(0.0397)	(0.0437)	(0.0477)
Observations	187,426	187,426	187,426	187,426	187,426
Number of groups	1,550	1,550	1,550	1,550	1,550

Notes: The table reproduces table 1 in CGH (2015) for different values of the truncation point. The table reports estimated coefficients on local unemployment rate when we regress a measure of posted-price city/category inflation on local unemployment rate after controlling for city/category and month fixed effects. The truncation point X sets $(dlogP) * 12 = -X$ if $(dlogP) * 12 < -X$ and $(dlogP) * 12 = X$ if $(dlogP) * 12 > X$ for a price change at the level of good/store/category/city. Panel A shows results when no imputations are used. Panel B shows results for the approach used in CGH (2015). Driscoll-Kraay (1998) standard errors are reported in parentheses. ***, **, * denote statistical significance at 1, 5 and 10 percent levels.

Table 5. 4/5-week effective-price inflation (fictitious example)

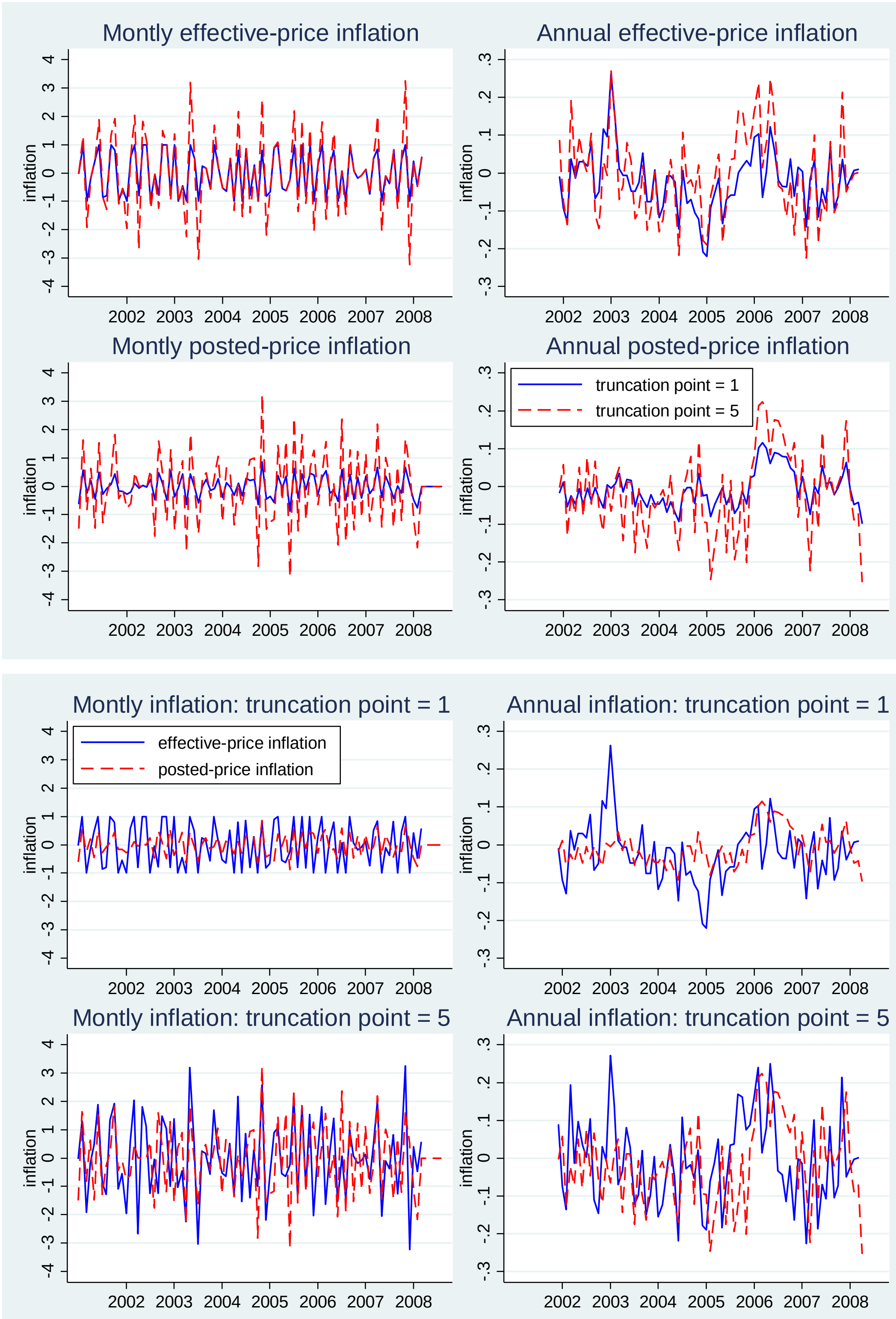
Month	Week	Effective price, p_{gmt}^{eff}	"Weekly" Inflation π_{gmw}^{eff}	Implied monthly inflation, π_{gmt}^{eff}	"Monthly" inflation, $\tilde{\pi}_{gmt}^{eff}$
1	1	2			
1	2	2			
1	3	2			
1	4	2			
2	5	2	0	0	0
2	6	2	0	0	0
2	7	2	0	0	0
2	8	2	0	0	0
2	9	2	0	0	0
3	10	3	50	50	50
3	11	3	50	50	50
3	12	3	50	50	50
3	13	3	50	50	50
4	14	3	0	0	0
4	15	3	0	0	0
4	16	3	0	0	0
4	17	3	0	0	0
4	18	3	0	0	0
5	19	2	-33	-27	-33
5	20	2	-33	-27	-33
5	21	2	-33	-27	-33
5	22	2	-33	-27	-33
5	23	2	0	-27	-33
6	24	3	50	50	50
6	25	3	50	50	50
6	26	3	50	50	50
6	27	3	50	50	50
7	28	4	33	27	33
7	29	4	33	27	33
7	30	4	33	27	33
7	31	4	33	27	33
7	32	4	0	27	33
8	33	3	-25	-25	-25
8	34	3	-25	-25	-25
8	35	3	-25	-25	-25
8	36	3	-25	-25	-25

Table 6. Effect of imputation for the sensitivity of effective-price inflation to local unemployment rate: 4-week vs 5-week.

Weight used in aggregation across stores and UPCs ts	Truncation point				
	1 (baseline)	2	3	4	5
	(1)	(2)	(3)	(4)	(5)
Panel A: 4-week calculation, π^{eff}_{gmt} (baseline)					
unweighted	-0.219***	-0.309***	-0.357***	-0.388***	-0.411***
	(0.0242)	(0.0294)	(0.0297)	(0.0300)	(0.0308)
city specific weights	-0.201***	-0.264***	-0.277***	-0.278***	-0.277***
	(0.0309)	(0.0360)	(0.0363)	(0.0354)	(0.0326)
country weights	-0.205***	-0.268***	-0.281***	-0.286***	-0.288***
	(0.0334)	(0.0386)	(0.0387)	(0.0379)	(0.0368)
Panel A: 4/5-week calculation, $\tilde{\pi}^{eff}_{gmt}$					
unweighted	-0.233***	-0.328***	-0.384***	-0.422***	-0.453***
	(0.0269)	(0.0341)	(0.0354)	(0.0357)	(0.0363)
city specific weights	-0.209***	-0.265***	-0.272***	-0.271***	-0.271***
	(0.0327)	(0.0395)	(0.0415)	(0.0416)	(0.0390)
country weights	-0.211***	-0.267***	-0.272***	-0.272***	-0.272***
	(0.0347)	(0.0409)	(0.0426)	(0.0427)	(0.0418)
Observations	187,426	187,426	187,426	187,426	187,426
Number of groups	1,550	1,550	1,550	1,550	1,550

Notes: The table reproduces table 1 in CGH (2015) for different values of the truncation point and different approaches to calculate month-to-month effective-price inflation rate. The table reports estimated coefficients on local unemployment rate when we regress a measure of annual effective-price city/category inflation on local unemployment rate after controlling for city/category and month fixed effects. The truncation point X sets $(dlogP) * 12 = -X$ if $(dlogP) * 12 < -X$ and $(dlogP) * 12 = X$ if $(dlogP) * 12 > X$ for a price change at the level of good/store/category/city. Panel A shows results for the approach used in CGH (2015), that is, month-to-month inflation is calculated as the monthly average of four-week change in effective prices. Panel B shows results when month-to-month inflation is calculated directly at the monthly frequency. Driscoll-Kraay (1998) standard errors are reported in parentheses. ***, **, * denote statistical significance at 1, 5 and 10 percent levels.

Figure 1. Effect of using alternative truncation points on time series of effective- and posted-price inflation rates.



Notes: The product is a Unilever detergent (+ALL FSHRN LNDTG UCONC LIQ 100OZ) sold in Chicago.