A “Shocking” Rise of Inflation Transparency and Price Dispersion in Post-Peronist Argentina

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Abstract

Transparency about inflation can help firms coordinate on pricing decisions, reduce aggregate economic price dispersion, and potentially increase social welfare. In this paper, I address the question of whether welfare can observably increase when a government suddenly stops misrepresenting inflation.

I leverage a recent surprise election victory in Argentina in 2015 which led to the reformation of its statistical reporting agency. Using a difference in differences approach with Uruguay, I find supporting evidence of downward price dispersion pressure in line with the hypothesis. A more pronounced upward pressure on dispersion, however, is observed, attributed to macroeconomic volatility during the political regime change, and obscuring my results. I also find a possible asymmetry of magnitudes depending on the sign of the shock.

Beyond its contribution to transparency literature, this study raises questions about the benefits of transparency reform versus other government priorities and under the presence of alternate trusted signals. Suggested follow on studies include re-analyzing a broader dataset and modeling the demographic makeup and composition of signals.

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1 Introduction

Can clearer public signals about inflation improve firms' abilities to set prices optimally in medium inflation settings? To answer this question, I examine ever fascinating Argentina. It recently experienced two remarkable inflation transparency shocks: a corruption of its inflation index in 2007, and later, the surprise undoing of this incident.

When price levels move quickly and volatilely, price-setting mistakes can lead to substantial social welfare loss. Firms preferring to stay solvent invest significant time gathering price information, especially in medium inflation countries where the cost of ignoring inflation is high (Cavallo et al 2017). Under clear, trusted, and unbiased public signals, firms can better align to market demand, with more efficient price setting and potentially higher welfare benefit (e.g., Calvo 1983). This can have direct implications when governments are making policy tradeoffs.

Empirically demonstrating the link from transparency to welfare is difficult, due in part to a shortage of suitable quantifiable transparency measures and shock events. Luckily, in 2007, Argentina's statistics reporting agency 'INDEC' suddenly began severely misreporting its inflation (see e.g., Cavallo 2016). Drenik and Perez (2016) quantified the impact of this transparency shock on price dispersion, as well as its negative impact on welfare, the first study to have shown this link end to end. Then, the reverse shock happened in 2016. A newly elected non-Peronist regime decided to reform the INDEC and released a trusted index. I analyze this second shock and add empirical confirmation of the transparency-welfare link, now with a shock of the opposite sign.

Following Drenik and Perez (2016)'s methodology, I first use household perception disagreement data and CPI dispersion to identify the signal shock, though it is less clearly identified in 2016 than in 2007. I then employ a difference in differences analysis with Uruguay. I include time and category fixed effects and macroeconomic controls to attempt to generate a set of comparable populations. My dependent variable is a panel of price dispersion metrics as the dependent variable by aggregating prices at a broad category level matched across countries. The panel has monthly data over 3 years and consists of 19 categories representing around 40% of Argentina's CPI, created by aggregating 25 million online scraped price observations from two online multi-channel retailers. The data source is the Billion Prices Project (BPP) at MIT[1], a novel solution that quantified and demonstrated the corruption of the inflation index in 2007 (Cavallo and Rigobon 2016).

My results imply that transparency somewhat reduced price dispersion in 2016, but that the magnitude of this effect was smaller than that of the opposite shock in 2007. Concurrent macroeconomic volatilities applied competing upward pressure on dispersion. Furthermore, the presence of alternate inflation indices in the interim provided a complement that lessened the need for firms to switch back to the INDEC's index. This scenario raised the question of what is the optimal public/private signal environment when there are multiple trusted public indices.

These results add to the existing literature by building some support to Drenik and Perez (2016)'s findings that clearer transparency signals may increase welfare. Furthermore, these results explore the effect of the opposite sign shock. I also provide some empirical comparability between transparency-based welfare improvements and macroeconomic effects, as well as an empirical demonstration of a signal environment that lessens the effectiveness of a government transparency policy. For all this, I leverage a unique data set of high volume online scraped data, which was born as a positive externality of the initial 2007 shock.

The remainder of the paper is organized as follows. Section 2 presents a brief literature review. Section 3 explains the identification strategy, shock event, empirical framework, and data used. Section 4 presents the results and discussion. Section 5 concludes.

2 Literature Review

There is a broad literature debating when information transparency from a government may be good or bad for society. Some assert that the contexts under which increased transparency is beneficial can depend on the specific measure or on payoff externalities (Morris and Shin 2002). Others assert that greater transparency cannot reduce welfare (e.g. Svensson 2006, Roca 2010). Specifically considering price signals, Amador and Weill (2010) predicted lesser welfare with increasing information under endogenous price setting scenarios. Others claim that properly understanding inflation is key to firm survival in medium inflation and/or volatile countries (e.g. Cavallo et al 2017),

[1] BPP data can be obtained from PriceStats, a private company that provides access to BPP data, both micro data and macro indices, for academic and commercial use. See https://www.pricestats.com/

while the New Keynesian model clearly asserts that more efficient price setting provides net benefits (Calvo 1983).

Empirically, case by case, it tricky to know under which structural regime one sits and which of the competing hypotheses dominates. A complex set of factors needs to be known, and furthermore transparency is difficult to precisely measure. One way to get around the latter problem is to find a clearly identified exogenous transparency shock. A very recent study was able to link inflation transparency to welfare improvements through the price dispersion channel, taking advantage of the sudden intentional manipulation of Argentina's published inflation index in 2007 (Drenik and Perez 2016). There is clear benefit to add on this, with additional empirical verifications of their hypotheses, both under comparable and distinct scenarios, and furthermore, to analyze shocks of the opposite sign. My study begins to fill this gap analyzing a reverse shock in Argentina, within the same country and perpetrating agency and with nearly the same political and business environment, thereby allowing for some comparability.

This study contributes to the literature in three ways. First, I provide a second verification of the causal link of inflation transparency on welfare through the price dispersion channel, by considering a second exogenous event in Argentina; while statistically significant conclusions are not obtained, directional supporting evidence is presented. Second, I specifically address the question of whether the reverse sign of transparency signal shock from that of Drenik and Perez (2016) can also have an observable effect and comparable magnitude. Third, I illustrate several confounding factors that may affect the relative magnitude of transparency's impact on welfare in comparison to other effects: namely, that concurrent underlying macroeconomic changes as well as differences in public and private signal environments can alter the degree of transparency impact. Such conclusions can have direct policy implications. While identification complexities are present in my study, I shed some light on each of these topics.

Notably, studies on Argentina's inflation particularly intensified after its statistical manipulation in 2007, making much new data readily available for study. These include scraped high frequency online price data broadly representative of economic activity for many countries, e.g. from the MIT-affiliated Billion Prices Project (BPP), as well as household inflation perception surveys and alternate macroeconomic indices (Cavallo and Rigabon 2016). Here I demonstrate yet another application of these innovative data sources.

3 Empirical Approach

3.1 Overview

Following the approach of Drenik and Perez (2016), my identification strategy is a difference in differences analysis between Argentina and Uruguay, with the dependent variable being a price dispersion metric aggregated at a broad category level and matchable across countries. My panel is monthly over 3 years, rather than quarterly over 10 years, with the potential of increased significance of measured effects. My treatment group "Pre" refers to the corrupted signal period prior to INDEC's reformation. That is, a positive interaction effect implies that price dispersion was higher in the "Pre" period, before the transparency increase.

In using a difference in differences approach to analyze shock impact across time, an ideal signal path would satisfy three assumptions: a steady state public signal value that existed prior to the shock, a new steady state attained afterwards, and a well identified single date for the shock with a fast dissipation. Furthermore, the shock should be exogenous, and there should not be any other shocks during the period that may influence price dispersion. I discuss each these assumptions in the next section: unlike the 2007 event, which met most of these assumptions, the 2016 event is less well identified, and I present caveats.

Additionally, the underlying populations of such a study should comparable across countries and over time. This relates to the choice of Uruguay as control country. In addition to applying category and time fixed effects in order to normalize the categories, it was important to choose a control country that "looks" similar to Argentina–namely, one that demonstrates similar consumer purchasing behavior and macroeconomic parameters. Given that Drenik and Perez (2016) selected Uruguay on the basis of this same criteria, I assessed Uruguay for its reusability for my study, given the potential benefits of comparability. I found the two countries' macroeconomic comparabilities to be similar during the two time periods. A number of comparison charts of the two countries are included in the Appendix. Uruguay's PPP adjusted per capita income and per capita GDP is generally comparable to Argentina's, and their annual GDP growth from 2008-2017 correlate at 0.60, higher than in the 2007 study. The caveat is that Argentina experienced a period of exchange rate and inflation volatilities during this period (see A2 and A3 in the Appendix), likely to confound results. In terms of additional requirements for a suitable control, Uruguay had no notable signal transparency changes during the period, and there is available cross-country comparable high frequency price data required for this analysis. With the potential upsides in comparability in this and follow on studies, and noting the caveats about macroeconomic differences, I selected Uruguay as the control country.

Next, I provide greater detail about the signal shock event and key identification assumptions. Afterwards, I detail the empirical approach and the data I utilized.

3.2 Transparency Shock, May 2016

In 2007, Argentina's official statistical reporting body, 'INDEC,' began severely underreporting inflation by around 10%, marking a decrease in inflation signal transparency. A variety of private, academic, and government entities began publishing alternate CPIs in 2007, which diverged significantly from the official measure (see Figure 1). Meanwhile household, inflation perceptions diverged from the official to track these unofficial measures, and cross sectional inflation perception disagreement increased (see Figure 2). Furthermore, the IMF eventually ceased publishing the official statistic, issuing a motion of censure against Argentina in February 2013, while the Economist and IMF both started reporting the PriceStats index for Argentina[2].

Along with the qualitative evidence from public trust removal, the CPI dispersion household survey evidence together have been taken by multiple studies (e.g., Cavallo et al 2016, Cavallo and Rigobon 2016, Drenik and Perez 2016) as evidence of a reduction of trust and introduction of bias in the published inflation numbers. Together this implies a reduction in inflation transparency, identified at the beginning of 2007.

I use a similar argument to demonstrate that another transparency shock occurred in Argentina in May 2016, this time with the opposite sign. In December 2015, INDEC was taken offline and reformed. In May, it started publishing a new index, initially only for Buenos Aires, but by January 2017 it also released a national index. Figure 1(b) shows CPI dispersion qualitatively decreasing around 2016. INDEC's new annual CPIs[3] track well with the alternate CPIs, and its monthly inflation index (not shown here) closely aligned with alternate measures beginning in May 2016. Interquartile

[2] This timeline is available from numerous academic and media sources. See for instance: Cavallo et al (2016) for a timeline spanning 2006-2016; "Welcome back Argentina's new, honest inflation statistics", Economist: https://www.economist.com/the-americas/2017/05/25/argentinas-new-honest-inflation-statistics for 2015-2017 sequence; https://www.reuters.com/article/argentina-imf-idUSL1N0YO2PL20150603 for comments during the 2016 adjustment.

[3] INDEC released a reformed Buenos Aires monthly inflation index starting in May 2016 and a reformed national monthly index starting in January 2017.

household perception disagreement[4] somewhat declines after the May 2016 event, and the trajectory appears to continue downward. By mid 2017, the IMF, the Economist, and other official sources recognized this new official index's credibility and began publishing it, crediting the bias as removed and acknowledging an increased trust in the signal. Together this implies an increase in transparency.

Compared to the 2007 shock, May 2016 is less well identified as a clean shock date. Whereas 2007 showed fast divergence of CPI indices following the event, there is evidence of up to a ~1 year delay before full convergence of signals after 2016. Median household perceptions do not converge well with the indices until mid 2017, although, this may be due to perceptions not fully adjusting to the mid 2016 inflation shock, an information rigidity effect manifested as incomplete perception adjustments studied by Coibon and Gorodnichenko (2012). Furthermore, the perception disagreement data prior to May 2016 shows a nonconstant signal environment. First, rather than a constant steady state from 2009-2015, there is a slight downward trend, consistent with firms potentially placing greater trust in these alternate signals (Cavallo et al 2014). Additionally in 2015, the signal suddenly dips then rises significantly. It is difficult to identify the source of the latter effect, whether an artifact of election year uncertainty, a tracking of an inflation dip that year, or another cause.

The transparency shock impact appears to take greater time to propagate. Disagreement hovers above 25% prior to the shock and drops below 20% by 2018, comparably to earlier mid-2007 levels, but a lower steady state has not been achieved by 2018 as disagreement appears to trend downward. There is a lesser decrease in disagreement than the corresponding increase seen in 2007-2008 (from 10% in 2007 to over 30% in 2008). Although the anticipation of the INDEC reform dated to Macri's victory, trust is typically lost faster than it is rebuilt, and while the Buenos Aires index released in 2016 tracked the national index closely, is was still not the full national index. The INDEC reformation also experienced delays and some early criticisms of its methodology.

Therefore, given the downward trend in 2009-2015 and the delayed shock propagation, I expected that I would observe a smaller treatment effect than did Drenik and Perez (2016).

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[4] For perceptions, I use household survey data over professional estimates. Household survey prediction errors have been shown to be smaller or comparable to surveys of professionals or forecasters (e.g. Mankiw et al 2004). Furthermore, households are relatively well informed about inflation levels in Argentina (Cavallo et al 2016), professional estimators in Argentina may have been subject to distortive government incentives, and households may provide a larger and more heterogeneous sample.

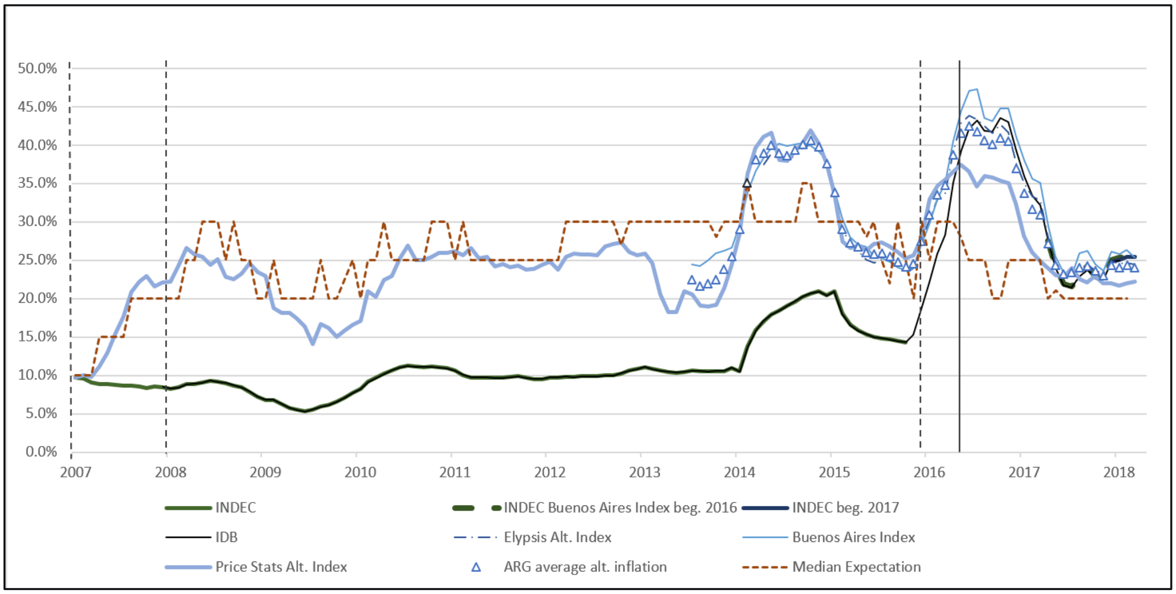
Furthermore, since my data in this study was limited to 2015-2017, placing heavy reliance on this idiosyncratic year with lower disagreement, there was likely to be a negative bias in the results. It is worth performing a follow up study with additional years of pre-shock and post-shock data for this reason. I do perform a robustness check to attempt to marginalize the effect of the idiosyncratic year by considering only data from years 2016 and 2017.

Additionally, macroeconomic factors have been shown to impact price dispersion, although the direction of many biases still seem disputed. Inflation levels and exchange rate movements are predicted by some models to increase with price dispersion (e.g., Calvo 1983, Gopinath et al 2010), while GDP cycles and depreciation may be inversely related (e.g., Bloom 2009, Bachmann and Moscarini 2012), although there have been examples of reverse effects. Since in December 2015, close to the date of the shock, Argentina's inflation and exchange rate volatilities increased as it floated its peso, lifted its trade controls, and changed its fiscal policies, it is important to include these controls. I therefore follow Drenik and Perez (2016)'s approach in including a vector of macroeconomic control variables in the baseline regression. I further to try to isolate the effects of the December 2015 policy changes by performing additional date-based robustness checks.

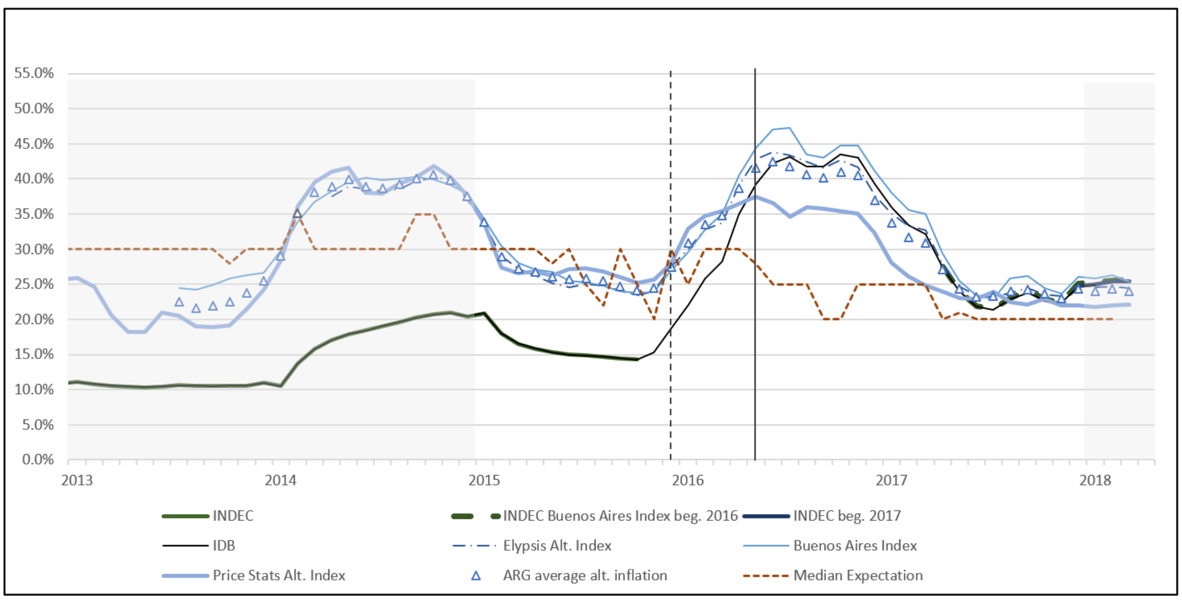
A last key identification assumption is the exogeneity of the shock. For it to be exogenous, the INDEC reformation should have been unexpected. I claim that INDEC's reformation was unexpected because Macri's win was unexpected. Newspapers at the time, claims by Argentinian economists, and my firsthand experience in Buenos Aires during the election all support this claim. Furthermore, for the shock to be exogenous, price dispersion levels prior to May 2016 must not have been causal to the election result. This claim is somewhat tenuous, as government de-corruption was included in Macri's platform. Price dispersion might have also had an indirect causal effect on the state of the economy, which positioned Macri to win. However, as we will see, given that price dispersion rose rather than fell after Macri's election, if rational expectations are applied, then dispersion might be ruled out as a causal factor. For this study, I make the assumption that Macri's win was caused by factors other than price dispersion. Under the above stated assumptions, the inflation transparency shock in 2016 is treated as exogenous.

Figure 1. Argentina Inflation Indices

(a) Official and Alternate Inflation Measurements and Perceptions in Argentina: 2007-2018



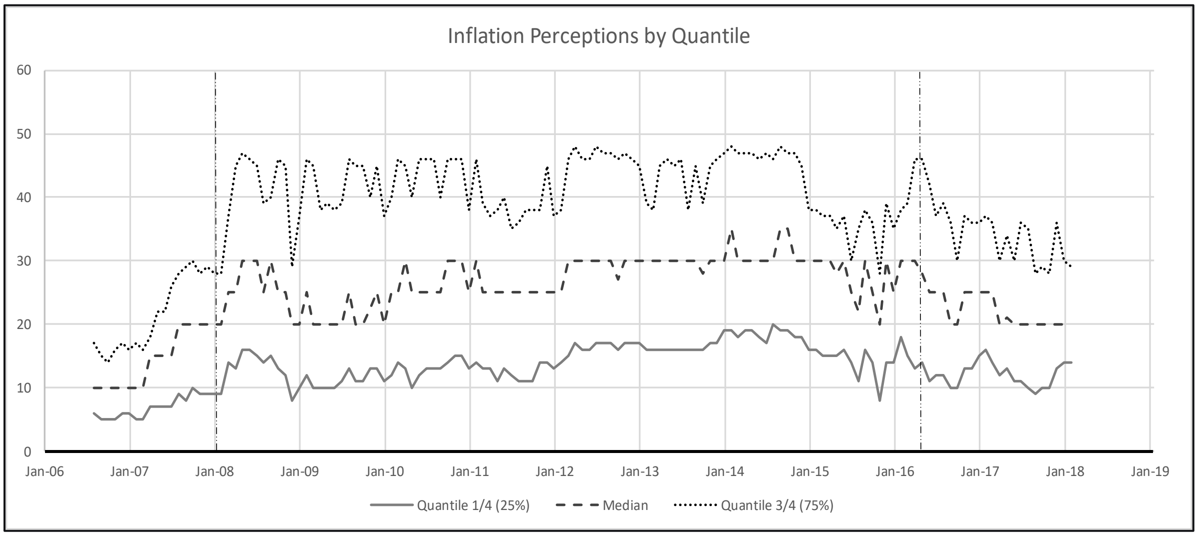
(b) Measurements and Perceptions Up Close: 2013-2018



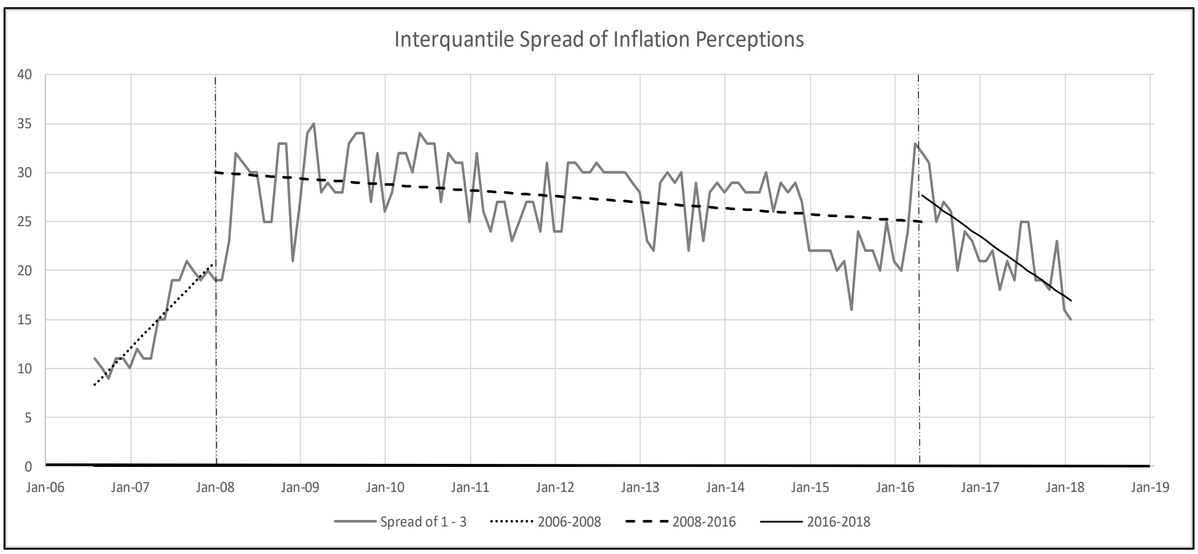
Note: Three separate measures for INDEC shown: the misrepresented index until 2015, and the reformed Buenos Aires and national indices beginning after 2016. Data from INDEC's new indices begin one year delayed since yearly rates are plotted here, although monthly rates were available one year earlier. The IDB index tracks INDEC initially, then diverge to alternate estimates. Additional indices included are from Elypsis and the government of Buenos Aires. Median inflation expectations are shown in dashed brown for comparison. Vertical demarcations indicate the initial inflation manipulation in 2007, the adjustment period in 2008, Macri's surprising 2nd placement in October 2015 leading to the runoff election in November, and INDEC's release of the new Buenos Aires index in May 2016. The study period 2015-2018 is indicated by shading in (b).

Figure 2. Household Inflation Perceptions

(a) Argentinian Household Surveyed Inflation Perception Quartiles



(b) Interquartile Perception Disagreement and Piecewise Trends



Note: Data is based on the Survey of Inflation Expectations conducted by Universidad Torcuato di Tella. On a monthly basis, it asks an average of 1,100 households to predict price increases in 12 months. This same survey was leveraged other analyses of Argentinians' inflation perceptions (e.g., Drenik and Perez 2016 and Cavallo et al 2016). Median data was provided directly by the university, while I estimated first and third quartile data by interpolating provided binned values. In (b), I estimate piecewise linear fits by simple OLS. The first vertical line at 2008 indicates the date the dispersion re-settled (Drenik and Perez 2016), although I found the trendlines to be robust to a 2009 demarcation at the disagreement peak. The vertical line at May 2016 separates the second period from the third and demarcates the date of the 2016 transparency shock.

3.3 Analytical Framework

My observations are a panel consisting of 36 observations (1 per month for 3 years) for each of 19 categories, and each these for both Argentina and Uruguay. In order to maintain consistency with Drenik and Perez (2016) and to allow to select a control country without signal bias, observations within the period Jan 2015 to May 2016 are the treatment group; observations after May 2016 are the untreated group. That is, the 2016 event restores Argentina to a "normal" untreated state[5].

My dependent variable is computed as the normalized standard deviations of prices of very similar or identical products. I use the most granular product group available from the retailer ("url_id") and calculate a normalized dispersion metric per url_id per week, using each product's median price that week[6]. These are retailer-specific and not suitable for cross-country comparison. Therefore, I take the median of these normalized measures within a broader, cross-country comparable category ("coicop") for an entire month. These category-specific normalized price dispersions per country per month constitute my final dependent variable.

Mathematically, the price dispersion observation d_{cat} is calculated for product category c within month m for a given country a. It aggregates url_id subcategories r, with individual scraped product prices given as p_{iaw} (median price for product i *in* country a, within week w) and a subcategory mean product price μ_{rw} during week w. That is:

$$d_{cat} = Med_{r\, \varepsilon\, c,\ w\, \varepsilon\, t} \left\{ \frac{\sqrt{\sum_{i=1}^{n} (p_{iaw} - \mu_{raw})^2}}{\mu_{raw}} \right\}$$

In the baseline regression, I interact the country and pre-May 2016 treatment using indicator dummies. The baseline configuration includes country-category fixed effects, monthly fixed effects, and macroeconomic controls to create a set of observations that are comparable across country-categories and over time, as described in the Approach section. The conceptual idea is to transform a set of time-based data of heterogeneous categories and under different macroeconomic conditions into

[5] Given that the time element is absent from the regression in a difference in differences analysis (save for time fixed effects and the treatment dummy), the time order of treatment does not matter. It is also more straightforward to find a macroeconomically comparable control country with a consistently stable signal than the opposite.

[6] I use a monthly approach to reduce sensitivity to outliers, as well as smooth price-change trends across a given month. An alternate method may be to calculate this metric weekly or daily, then aggregate.

a set of comparable populations, as per a typical difference in differences analysis. The equation is defined as:

$$d_{cat} = \delta_1 \mathbf{1}_{(\text{Arg})} + \delta_2 \mathbf{1}_{(\text{Pre})} + \delta_3 \mathbf{1}_{(\text{Arg x Pre})} + \beta \mathbf{X}_{at} + \alpha_t + \boldsymbol{\alpha}_{ca} + \varepsilon_{cat}$$

The dependent variable d_{cat} is again price dispersion, the calculated rolled-up median normalized standard deviation of prices for a given month, category, and country; δ_i correspond to dummies for Argentina, "Pre" treatment, and interaction, respectively; X_{at} is the vector of country-level time dependent macroeconomic control variables; α_t represents country-category invariant time-based fixed effects; α_{ca} represents time-invariant country-category level fixed effects; ε_{cat} is the error term.

My goal is to estimate the coefficient δ_3 of the "Arg x Pre" indicator variable. Under ideal identification, this will indicate the impact of the transparency shock on the dependent variable, price dispersion.

As an additional identification test, I run a simple parallel trends check to look for a "before and after" effect. Given that my treatment is the period prior to the shock, this test is for anticipatory effects prior to the un-treated state, but mathematically they are equivalent:

$$d_{cat} = \delta_1 \mathbf{1}_{(\text{Arg})} + \boldsymbol{\delta_2} \mathbf{1}_{(\text{Month dummies})} + \boldsymbol{\delta_3} \mathbf{1}_{(\text{Arg x Month dummies})} + \varepsilon_{cat}$$

The treatment dummy is replaced by a vector of month dummies, which are interacted with the country indicator. The controls are dropped in order to see raw effects of the difference in differences. Although the plane of the treatment effect will not be as comparable without the controls, this provides a macro sense of the overall trend of the dispersion change.

I then perform a number of robustness checks, primarily to test for sensitivity to control data sources (as described below), as well as shock date identification and macroeconomic impact as described previously.

3.4 Price Data

This analysis requires calculating cross sectional dispersion of prices for a given product type and aggregating at a category level that is matchable across Argentina and Uruguay. To do this, I leverage the MIT Billion Prices Project (BPP)[7], which scrapes daily online prices of multi-channel retailers, which represent the majority of retail sales and have both online and physical store presence[8]. BPP data has broad CPI coverage in categories that are comparable across countries, with online prices shown to closely track in-store prices (Cavallo 2017).

Table 1 provides summary statistics of the products used in the final analyses. I use nearly 25 million price observations for my analysis, using daily online prices for from 2015 through the end of 2017. The data represents one multi-channel retailer in each country, with 19 categories that include food, beverages, electronics, appliances, and health and personal care items. The set of categories represented in the raw data corresponds to roughly 40% of CPI weights.

Each price observation has two category labels: one that is retailer-specific ("url_id") and one that is broad and standardized ("coicop"). The url_id is a very narrow retailer-specific category that corresponds to a product or product type, like skim milk, while the coicop is a broad roll-up category, standardized across countries for which BPP collects data.

The BPP data presents a few biases, as it still does not represent the CPI basket in its entirety and is limited to the multi-channel retailers. These introduce a bias whose directionality is difficult to predict, though these shortcomings are true of most high frequency online price data sources currently. Additionally, BPP lacks information on quantities sold. This may cause an overestimate of welfare impacts because suboptimal prices included in our dispersion calculations may not be chosen by the market, while causing an unpredictable bias because the representation of products purchased in the economy will not match this simple weighting scheme. This analysis therefore presents a somewhat filtered view of dispersion; the closer the data and weightings can resemble the CPI in future analyses, the more meaningful the results will be.

The specific dataset I obtained access to presents additional limitations. First, the dataset is limited to one retailer per country. Each is constrained to the brands it choses provide, with fewer brands to show differences in price setting behavior introduces bias in dispersion calculations. Furthermore,

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[7] BPP data can be obtained from PriceStats, a private company that provides access to the BPP scraped micro data and macro indices for academic and commercial use. See https://www.pricestats.com/

[8] Drenik and Perez (2016) used published price data from the largest e-Trade platform in Latin America, similar to Amazon, selling mostly durable products. I chose a different data source in part because Drenik and Perez (2016)'s original dataset was not available yet for 2015-2017, and in part because BPP data may be more representative of economic behavior, given the prices come from large multi-channel retailers. In future studies, it would be useful to compare the results of this study using the two differing datasets.

Table 1. Summary Statistics of Price Data

	(1) Argentina	(2) Uruguay
Observations	13,994,849	10,986,392
Retailer Categories	2662	707
Comparison Categories	19	19
Mean Product Price	140	221
Minimum Product Price	7	7
Maximum Product Price	9999	9999
Mean Dependent Variable	0.519	0.605

idiosyncratic private signals at the retailer level may conflate public signals. Although price dispersions at intermediate value chain steps still propagate, modulo dampening effects, it is recommended in future studies to expand the set of retailers and remove these two biases. Additionally, the limit of my data to 2015-2017 introduces sensitivity to the signal idiosyncrasies of 2015 previously discussed. It is also likely be too short a period of time for the shock to fully propagate; in the 2007 study, it took between 1 and 2 years for the signal change to complete its propagation. It would be ideal to repeat this study with a full 3-5 years of data both before and after 2016 event, once it is available. Finally, price ranges of products in comparable categories do not quite align, as seen in different median product prices. If dispersive effects vary by product price then an additional bias may have been introduced; correcting for this could be done by a more manual category composition selection process for each country's data.

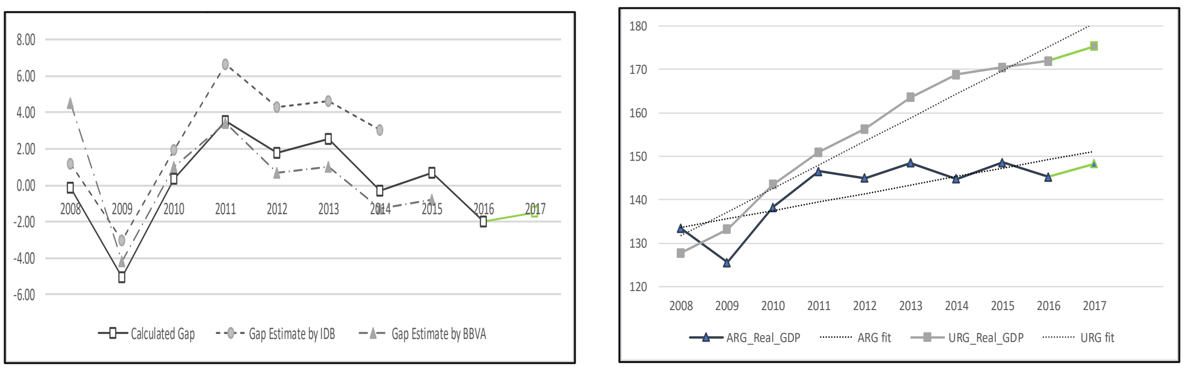
3.5 Macroeconomic Control Data

I used standard sources for most macroeconomic data, with exceptions noted here. First, inflation indices over the time period were unreliable, as described. As a proxy index for my analysis therefore, I created a representative index comprised of a simple average of alternate indices: Elypsis, Buenos Aires's index, INDEC's index for Buenos Aires beginning mid-2016, and INDEC's national index beginning in 2017, as shown in Figure 1. I also used the PriceStats's BPP inflation index, separately, for a robustness check.

Second, the treatment period was characterized by a number of different official and unofficial exchange rates used across the economy (see A4 in the Appendix), including a practically legal high volume black market rate known as "dolar blue". The baseline specification uses the official rate, while for robustness I also use a Purchase Power Parity (PPP) based exchange rate proxy index also generated by PriceStats. This is seen to hover between the unofficial and dolar blue rates and therefore may be a useful statistic to check against.

Figure 3. Output Gap Model

(a) Model Comparison (b) GDP Cycles Versus Fitted Trends



Note: Gap estimates for Argentina in (a) provided by IDB and BBVA. GDP estimates in (b) provided by IDB.

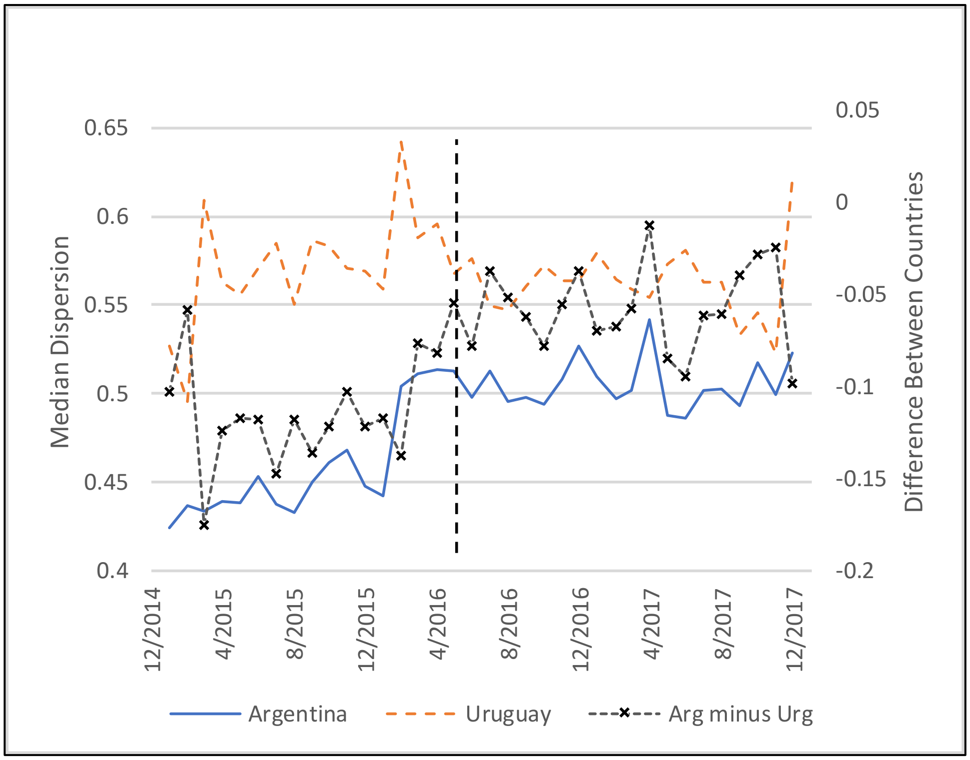
Third, for output gap calculations, I found inconsistent measures amongst official sources and furthermore no official data past 2016. I therefore generated a simple output gap model which estimates the gap as half the difference between Argentina's real GDP and a 9-year linear fit from 2008 to 2017 (see Figure 3). Checked qualitatively against IDB and BBVA output gap estimates for Argentina, this model roughly spits the difference, and I use it for my baseline output gap control.

4 Results and Discussion

4.1 Results

A simple visual plot of the median dependent variable for Argentina and Uruguay from January 2015 to December 2017 provides an initial intuition of the results (see Figure 4). Argentina shows a lower median dispersion than Uruguay throughout the period. Around midway through the period, this gap decreases, with an apparently persistent effect. Qualitatively, Argentina's price dispersion appears to relatively increase, rather than decrease as the transparency hypothesis suggests, after the shock period.

Figure 4. Median Price Dispersion Results



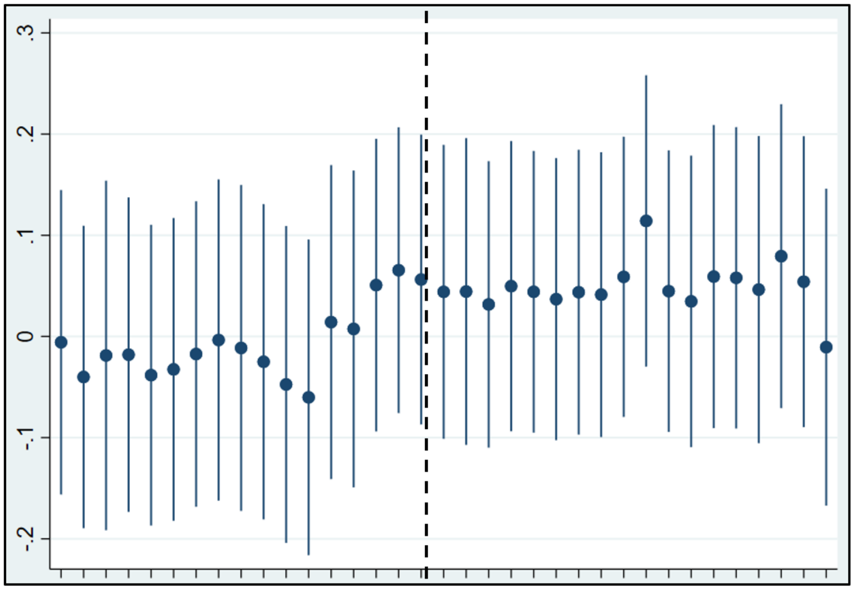
Note: Argentina's relative dispersion is indicated in black and uses the right side scale.

The parallel trends results are also consistent with a relative aggregate price dispersion increase after the signal transparency shock (see Figure 5). Plotted are the coefficients of the monthly interaction dummies, with the treated "Pre" group on the left hand side. In the ideal identification, the right hand side would be constant, while the treated group would show a statistically significant shift or trend. Qualitatively, I see that in the treated state, the coefficients have a negative shift, consistent with lower relative price dispersion. The data is too noisy to draw substantial conclusion, but I am unable to reject the null hypothesis of a constant parallel trend with no shift.

The date of the shift cannot be definitively identified at May 2016 (month 17); there may be a jump around January 2016, or there may be a slope change around April 2016. The apparent jump in January immediately followed Macri's inauguration and swift monetary policy changes. In robustness checks, I look for supporting evidence of this as a driver of the dispersion by trying December 2017 as my shock date.

The key takeaway from these two graphs is that a dispersion shift across the two time periods should be expected, with a negative interaction effect.

Figure 5. Parallel Trends Check Results



Note: Interaction coefficients Arg x Month-Dummy plotted by month. The vertical line demarcates the May 2016 shock.

The results of the baseline regression are consistent with these graphs (see Table 2). The coefficients for the Argentina dummy, treatment dummy, and interaction term are negative and significant[9]. The impact of the shock on price dispersion was 0.603, or a 13% increase (i.e. negative interaction coefficient), significant to the 1% level, before adding controls or fixed effect. Even after controlling for macroeconomic variables and fixed effects, the effect retains its significance, while increasing the power and reducing the standard error of the result. The impact is slightly reduced from

[9] Recall that since the treatment group is the "before" period, a negative treatment effect implies positive pressure on price dispersion after the shock.

13% to 9% at 5% significance, suggesting that some dispersion may be attributable to macroeconomic factors varying over this time period.

This baseline configuration matches the configuration used by Drenik and Perez (2016), yet the effect is of the opposite sign. As was discussed, a number of identification assumptions had predicted a smaller overall impact of the transparency shock, as well as a negative bias from confounding factors, so this is not unexpected.

Table 2. Difference in Differences Baseline Results

	(1)	(2)	(3)	(4)	(5)
VARIABLES	No Controls/FE	Categ FE	Time FE	All FE	Baseline Controls
Argentina x Pre	-0.0603***	-0.0636***	-0.0597***	-0.0630***	-0.0453**
	(0.0182)	(0.00816)	(0.0183)	(0.00808)	(0.0186)
Pre (= Treated)	0.0119	0.0129**	-0.0554	-0.0538**	-0.106*
	(0.0141)	(0.00628)	(0.0416)	(0.0253)	(0.0546)
Argentina	-0.0568***	-0.0707***	-0.0568***	-0.0710***	-0.113***
	(0.0117)	(0.0103)	(0.0118)	(0.0102)	(0.0234)
Output Gap					0.0117
					(0.0144)
Inflation					0.158*
					(0.0848)
Devaluation					0.0245.
					(0.0447)
Exch Rate Volatility					-0.0720
					(0.133)
Infl. Volatility					0.0217
					(0.0591)
Constant	0.599***	0.528***	0.626***	0.556***	0.573***
	(0.00824)	(0.00928)	(0.0313)	(0.0167)	(0.0345)
Observations	1,351	1,351	1,351	1,351	1,351
R-squared	0.069	0.823	0.824	0.825	0.825
Mean Arg.	0.506	0.506	0.506	0.506	0.506
Mean Ur.	0.588	0.588	0.588	0.588	0.588
Time Fixed Effects			Yes	Yes	Yes
Country-Category FE		Yes		Yes	Yes
Baseline Controls					Yes
Arg Inflation Index	Averaged	Averaged	Averaged	Averaged	Averaged
Arg. Exch Rate Data	BCRA	BCRA	BCRA	BCRA	BCRA

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

After running a few robustness tests, I found that the interaction effect shows sensitivity to GDP. When adding GDP as a control, the sign of the interaction flips. The transparency shock now generates a 2% decrease in price dispersion, though no longer statistically significant, while the GDP coefficient has a statistically significant negative value that places an opposing upward pressure on dispersion.

This reversal of sign of the interaction coefficient may point to an omitted variable bias or an endogeneity issue. An endogeneity issue is possible if price dispersion impacts growth in near-future periods, or if both the price dispersion shock and GDP fluctuations were caused by the same exogenous political shock (Macri's surprise victory). Further, the correlation might be spurious since Argentina's GDP and dispersion both have an upward trend in 2015-2017. These issues might be further investigated by using more sophisticated tests to look for cointegration, running a lagged dependent model, or using an instrument for GDP that could encapsulate its direct effect.

In considering the omitted variable bias possibility, note this sign flip only occurs when inflation volatility or exchange rate volatility is also included as a control (see A6 in the Appendix). Since we saw that these two variables were not as well matched with Uruguay in this study, the policy shocks of December 2015 might explain why inclusion of GDP and inflation or exchange rate controls has this drastic impact. Therefore, to help test whether Macri's monetary shock and initial fiscal policy changes had a strong impact in dispersion, I reran the regression using December 2015 as the shock date. If this date were providing the dominant shock, I would expect a negative interaction term, whether or not GDP was being controlled, and this is in fact what is observed to a 5% significance even without the GDP control. This is supporting evidence that macroeconomic factors had a greater impact on dispersion than the transparency shock. I also tried isolating out the volatilities seen primarily during 2016 as well as some of the signal propagation time, by analyzing only 2015 and 2017 data. If the INDEC transparency effect dominated, I would expect a positive interaction effect, regardless of GDP control inclusion. In fact this is what we see, though not to a significant level. It implies qualitatively that the transparency effect is indeed present.

To test for control variable integrity, I retested using the alternate PPP-based exchange rate data from PriceStats, as well as with PriceStats' inflation data for Argentina, as previously described. The original results were robust to these alternate data sources.

Finally, I asked whether certain product segments are more susceptible to the competing dispersion-driving factors. As a coarse foray into this question, I replicated the baseline analysis considering only lower priced categories, then only higher priced categories. Lower priced categories were more susceptible to the transparency effect, while in higher priced categories the macroeconomic effect dominated. A more granular segmentation investigation would be an interesting follow on, to understand which industries or product characteristics are more susceptible to the signal propagation, and whether they have greater representation in the CPI. (See Table 3 for the robustness summary.)

Table 3. Robustness Summary Results

VARIABLES	(1a) Baseline	(1b) With GDP Control	(2a) Alt. Exch Rate	(2b) With GDP Control	(3a) Alt. Inflation	(3b) With GDP Control	(4a) 2015 & 2017	(4b) With GDP Control
Argentina x Pre	-0.0453**	0.0100	-0.0522***	0.0171	-0.0636***	0.0121	0.0110	0.00399
	(0.0186)	(0.0242)	(0.0126)	(0.0256)	(0.0132)	(0.0217)	(0.0184)	(0.0256)
Real GDP		-0.0172**		-0.0204***		-0.0161***		0.00684
		(0.00729)		(0.00677)		(0.00453)		(0.0207)
Observations	1,351	1,351	1,351	1,351	1,351	1,351	867	867
Mean Arg.	0.506	0.506	0.506	0.506	0.506	0.506	0.512	0.512
Mean Ur.	0.588	0.588	0.588	0.588	0.588	0.588	0.602	0.602
Date Range	2015-2017	2015-2017	2015-2017	2015-2017	2015-2017	2015-2017	2015 & 2017	2015 & 2017
Shock Date	May 2016	May 2016	May 2016	May 2016	May 2016	May 2016	May 2016	May 2016
Price Range	All	All	All	All	All	All	All	All
Arg Inflation Index	Averaged	Averaged	Averaged	Averaged	PriceStats	PriceStats	Averaged	Averaged
Arg. Exch Rate Data	BCRA	BCRA	PriceStats PPP	PriceStats PPP	BCRA	BCRA	BCRA	BCRA

VARIABLES	(5a) 2016 & 2017	(5n) With GDP Control	(6a) 12/2015 Shock	(6b) With GDP Control	(7a) Low Priced	(7b) With GDP Control	(8a) High Priced	(8b) With GDP Control
Argentina x Pre	-0.131***	-0.198	-0.0823***	-0.0556**	-0.0200	0.0163	-0.0699**	-0.0425
	(0.0359)	(0.120)	(0.0254)	(0.0279)	(0.0225)	(0.0295)	(0.0335)	(0.0396)
Real GDP		0.0115		-0.0108*		-0.0105		-0.00950
		(0.0201)		(0.00593)		(0.00838)		(0.0118)
Observations	903	903	1,351	1,351	647	647	707	707
Mean Arg.	0.543	0.543	0.506	0.506	0.480	0.480	0.608	0.608
Mean Ur.	0.603	0.603	0.588	0.588	0.531	0.531	0.631	0.631
Date Range	2016 & 2017	2016 & 2017	2015-2017	2015-2017	2015-2017	2015-2017	2015-2017	2015-2017
Shock Date	May 2016	May 2016	Dec 2015	Dec 2015	May 2016	May 2016	May 2016	May 2016
Price Range	All	All	All	All	Low	Low	High	High
Arg Inflation Index	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged
Arg. Exch Rate Data	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In terms of macroeconomic drivers of dispersion, in most cases the results did not conclusively support or reject prevailing theories of their impact on dispersion. The only coefficients consistently significant were devaluation and real GDP, which tended to be positively correlated and negatively correlated to dispersion, respectively.

In summary, these tests, while not perfectly identified, are consistent with the theory that a positive inflation transparency policy is being captured by price setting behavior. However, including GDP as a control, I cannot reject the null hypothesis that the shock had no impact on dispersion. The results also imply a dominating counter-pressure: GDP appears to be a key driver of the negative interaction effect when inflation and exchange rate are volatile.

4.2 Discussion

A few explanations can be suggested for the differences between these results and Drenik and Perez (2016)'s. There was likely a negative bias due to the idiosyncratic year of 2015 as well as the macroeconomic volatility. Once isolating the macroeconomic impacts, a small positive treatment effect of the transparency shock was suggested, but this is still biased by the signal environment of 2015, fundamentally difficult to isolate. The bias might be lessened by repeating the experiment with a greater span of pre-shock and post-shock dates.

Suppose that we adjust for these negative biases. The question then emerges as to why the transparency treatment magnitude was so much different in the two setups: a 9% dispersion change in 2007 versus a 2% change in 2016.

One set of explanations has to do with experiment setup, which may be improved by gathering data from more retailers and over more years. More fundamentally, perhaps it just takes longer to rebuild trust and increase transparency than to lose it. This is supported by the CPI and perception survey data, as it appears the new steady state transparency level has yet to be reached. Remarkably, the IMF and Economist did not officially recognize the signal degradation for 5 years after the event, while the index was re-recognized just one year after the May 2016 reverse shock. Still, it seems a plausible explanation that there is slower signal propagation under transparency gain than under transparency loss. Again, more years of data can overcome this magnitude underestimation.

Once adjusting for this, a residual magnitude difference is still expected due to the fundamental difference in signal environment. I have shown supporting evidence that by 2016, the public placed less weight on the INDEC than in 2007, while placing trust in new alternative indices. One must incorporate these alternate indices into the aggregate public signal transparency measure rather than simply consider the official public signal. Thus, if aggregate transparency was greater than the public signal alone would suggest, a smaller magnitude shock should be expected, and hence a smaller shift in dispersion.

As a next step, this signal environment could be modeled by considering two public signals over three periods. In the first period prior to 2007, a one-signal environment exists, with a sudden degradation of trust and increase of bias in 2007. From 2008 to 2016, a two-signal environment exists, the second characterized by low bias and gradual trust growth but with some dispersion due to multiple measures, and the first characterized by high bias and great mistrust. After 2016, a two-signal exists, the first characterized by a sudden increase in trust and sudden decrease of bias, and the second characterized by a continued increase in trust and decrease of bias. The model would presume a high degree of desired coordination, and it would parameterize reliance on either of the two public signals as well as an idiosyncratic signal, with transparency represented by signal dispersion and bias.

Note that segmenting the transparency impact by category or demographic may also have interesting policy implications. One might start by decomposing by market segment, product type, or CPI weight. Both shock dates could be analyzed under this lens. Furthermore, given that inflation predictions have been shown to differ by age (Malmendier and Nagel 2015) as firm owners may tend towards a certain age, the drivers of dispersive effects might be pinpointed to certain population segments which are more influential in generating disperse outcomes.

All else equal then, should the same magnitude shock in the positive direction have a lesser impact on dispersion? In our experimental setup this can only be tested by process of elimination: by collecting more data until the effect fully settles, quantifying the bias from macroeconomic factors, correcting for aggregate signal transparency bias, and removing reliance on the idiosyncratic effects of 2015. The first and fourth might be solved by data, while the second and third might be solved through structural modelling.

This may seem difficult to do precisely. However, finding a cleaner shock-and-reverse real world experiment than Argentina's case may prove even more difficult!

5 Conclusion

Helping policymakers understand the impact of inflation transparency on the economy can have real welfare impacts. This is especially true in medium inflation countries, where transparency improvements may be the most needed and citizens by necessity are highly aware of price levels. In decomposing this story, I leveraged a recent surprise election victory in Argentina to try to link inflation transparency to public welfare.

In doing so, I found that although inflation transparency did increase in 2016, in fact relative price dispersion increased rather than decreased after this shock. A number of factors complicated the identification, from a regime change and concurrent macroeconomic volatilities, to an idiosyncratic pre-election year heavily leveraged by my data, to a signal environment complicated by multiple indices. Robustness tests provided some indication that GDP, inflation, and exchange rate volatility played a role in driving price dispersion. The tests still provided support for the hypothesis that transparency puts downward pressure on dispersion in this environment. Hence, while I did not find statistically significant supporting evidence for this effect, the results of this study do not reject the hypothesis that inflation transparency decreases dispersion and hence decreases welfare.

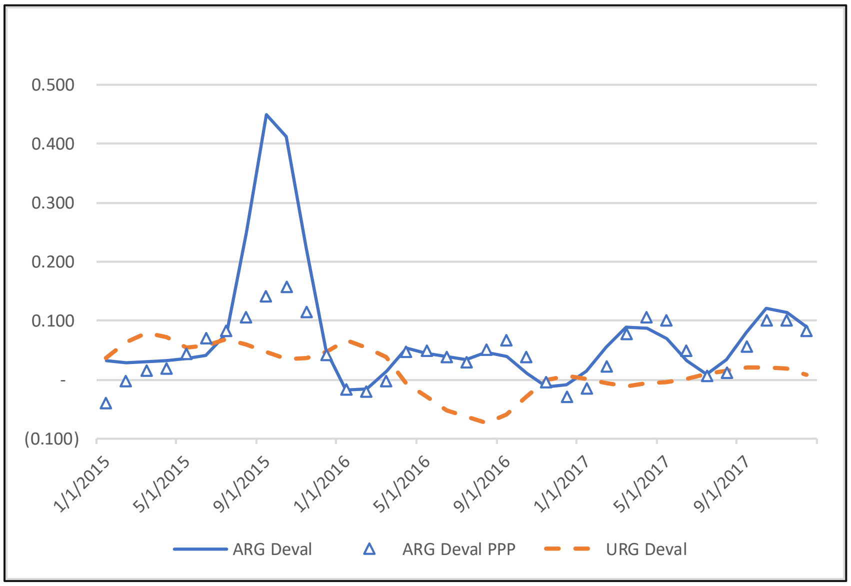
Follow on studies could start by confirming these preliminary results with a broader set of data, both in terms of years and number of retailers, as well as re-running the analysis with Drenik and Perez’s (2016) original 2007 data source once available for direct comparability. Following this, it might be possible to derive a magnitude comparison of the shock and reverse shock pair. A structural model could further decompose the relative impacts of macroeconomic and transparency signal factors, and a nuanced signal model could derive an adjustment to the apparent transparency magnitude shift. Macroeconomic effects could be quantified, 2015 idiosyncrasies removed, and magnitude of the 2016 shock adjusted.

Then, we might create a more accurate assessment of a positive transparency shock under a complicated macroeconomic and signaling environment.

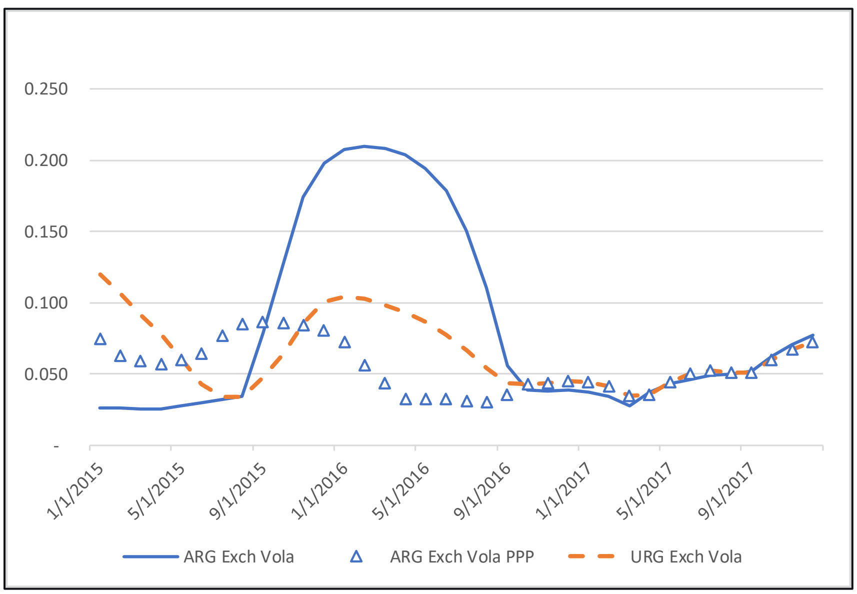
In the case of Argentina, perhaps in the end this transparency reformation was less critical to societal welfare than other changes which accompanied the Macri surprise.

6 Appendix

A 1. Argentina and Uruguay Devaluation 2015-2018



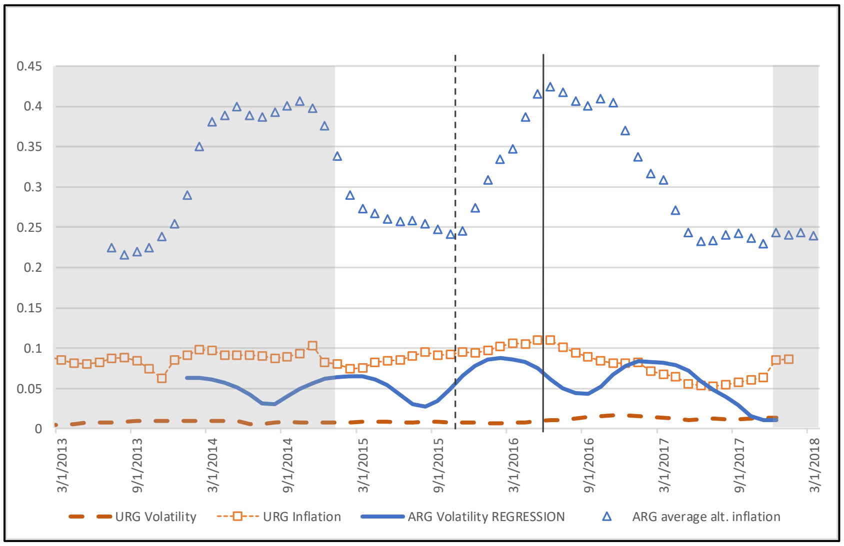
A 2. Argentina and Uruguay Exchange Rate Volatility 2015-2018



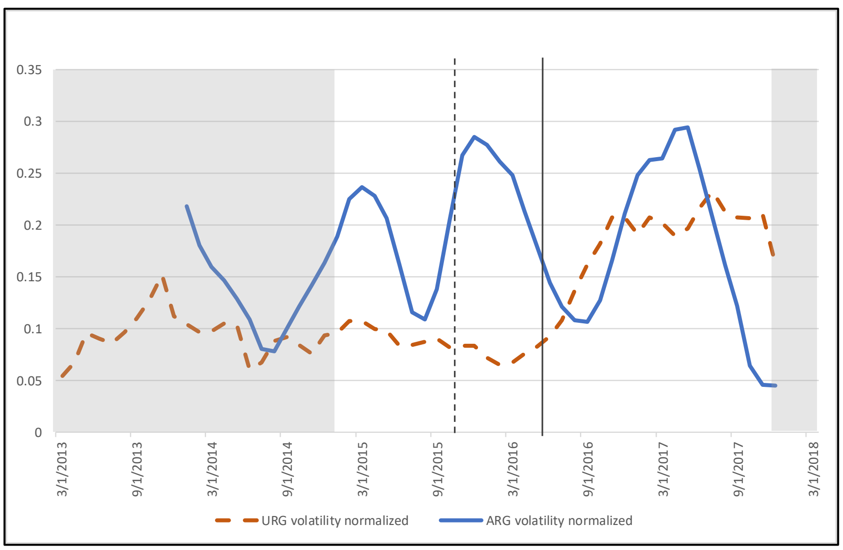
Note: Argentina PPP indices provided by Price Stats. Official Argentina exchange rate data was provided by the Central Bank of Argentina. Official Uruguay exchange rate data provided by the Central Bank of Uruguay.

A 3. Argentina and Uruguay Inflation and Inflation Volatility

(a) Inflation and Volatility, Not Normalized



(b) Volatility, Normalized



Note: Volatilities calculated as 12-month standard deviation rolling window. Argentina data is taken as the average of alternate indices, shown in Figure 1. Uruguay inflation data is from the IDB.

A 4. Argentina Exchange Rates: Official, 'Dolar Blue' Black Market, and PriceStats PPP-Based

Note: "PPP based" index is PriceStats Adjusted PPP index. Official and Dolar Blue exchange rates obtained from La Nacion Argentinian media source at: https://www.lanacion.com.ar/data.

A 5. Regression Results with Built-up Control Variables Without GDP

VARIABLES	(1) All FE	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13) Baseline
Argentina x Pre	-0.0630***	-0.0601***	-0.0577***	-0.0629***	-0.0647***	-0.0670***	-0.0498***	-0.0606***	-0.0569***	-0.0501***	-0.0431**	-0.0523***	-0.0453**
	(0.00808)	(0.00856)	(0.00880)	(0.00807)	(0.00871)	(0.0112)	(0.00958)	(0.00850)	(0.00876)	(0.00955)	(0.0184)	(0.0121)	(0.0186)
Pre (Treatment)	-0.0538**	-0.0649**	-0.0605**	-0.0536**	-0.0517**	-0.0529**	-0.0873***	-0.0627**	-0.0609**	-0.0847***	-0.104*	-0.0835***	-0.106*
	(0.0253)	(0.0281)	(0.0264)	(0.0254)	(0.0259)	(0.0253)	(0.0311)	(0.0280)	(0.0264)	(0.0309)	(0.0537)	(0.0306)	(0.0546)
Argentina	-0.0710***	-0.0717***	-0.0960***	-0.0721***	-0.0732***	-0.0703***	-0.104***	-0.0725***	-0.101***	-0.107***	-0.113***	-0.106***	-0.113***
	(0.0102)	(0.0103)	(0.0177)	(0.0106)	(0.0104)	(0.0102)	(0.0185)	(0.0107)	(0.0183)	(0.0192)	(0.0234)	(0.0191)	(0.0234)
Output Gap		0.00320					0.00722	0.00264		0.00644	0.0110	0.00627	0.0117
		(0.00743)					(0.00779)	(0.00742)		(0.00775)	(0.0141)	(0.00771)	(0.0144)
Inflation			0.0981*				0.124**		0.109*	0.130**	0.157*	0.128**	0.158*
			(0.0566)				(0.0584)		(0.0566)	(0.0588)	(0.0842)	(0.0586)	(0.0848)
Devaluation				0.0195				0.0152	0.0338	0.0261	0.0222	0.0279	0.0245
				(0.0442)				(0.0437)	(0.0445)	(0.0441)	(0.0440)	(0.0449)	(0.0447)
Exch Rate Volatility					0.0697						-0.0593		-0.0720
					(0.0694)						(0.128)		(0.133)
Infl. Volatility						0.0274						0.0128	0.0217
						(0.0559)						(0.0567)	(0.0591)
Real GDP													
Constant	0.556***	0.562***	0.551***	0.555***	0.554***	0.552***	0.566***	0.561***	0.550***	0.564***	0.574***	0.562***	0.573***
	(0.0167)	(0.0219)	(0.0164)	(0.0168)	(0.0170)	(0.0169)	(0.0221)	(0.0219)	(0.0165)	(0.0220)	(0.0348)	(0.0220)	(0.0345)
Observations	1,351	1,351	1,351	1,351	1,351	1,351	1,351	1,351	1,351	1,351	1,351	1,351	1,351
R-squared	0.833	0.833	0.833	0.833	0.833	0.833	0.833	0.833	0.833	0.833	0.833	0.833	0.833
Mean Arg.	0.506	0.506	0.506	0.506	0.506	0.506	0.506	0.506	0.506	0.506	0.506	0.506	0.506
Mean Ur.	0.588	0.588	0.588	0.588	0.588	0.588	0.588	0.588	0.588	0.588	0.588	0.588	0.588
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Category FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls													Yes
Arg Inflation Index	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged
Arg. Exch Rate Data	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

A 6. Regression Results with Built-up Control Variables With GDP

VARIABLES	(1) All FE with GDP	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13) Baseline with GDP
Argentina x Pre	-0.0367**	-0.0112	-0.0336	-0.0319*	-0.0383**	-0.0348	-0.00593	-0.00869	-0.0283	-0.00305	0.00772	0.00243	0.0100
	(0.0171)	(0.0170)	(0.0220)	(0.0172)	(0.0175)	(0.0218)	(0.0210)	(0.0172)	(0.0222)	(0.0212)	(0.0215)	(0.0256)	(0.0242)
Pre (= Treated)	-0.106**	-0.179***	-0.113**	-0.114***	-0.104**	-0.107**	-0.192***	-0.182***	-0.122**	-0.196***	-0.225***	-0.201***	-0.226***
	(0.0426)	(0.0461)	(0.0533)	(0.0428)	(0.0431)	(0.0444)	(0.0535)	(0.0462)	(0.0536)	(0.0537)	(0.0569)	(0.0561)	(0.0571)
Argentina	-0.0704***	-0.0732***	-0.0614**	-0.0735***	-0.0728***	-0.0707***	-0.0595**	-0.0756***	-0.0631**	-0.0611**	-0.0684**	-0.0612**	-0.0677**
	(0.0101)	(0.0105)	(0.0275)	(0.0106)	(0.0104)	(0.0101)	(0.0274)	(0.0110)	(0.0276)	(0.0275)	(0.0326)	(0.0274)	(0.0335)
Output Gap		0.0136*					0.0140*	0.0128*		0.0132*	0.0198	0.0137*	0.0195
		(0.00740)					(0.00728)	(0.00740)		(0.00729)	(0.0127)	(0.00721)	(0.0130)
Inflation			-0.0352				-0.0536		-0.0404	-0.0569	-0.0220	-0.0583	-0.0261
			(0.100)				(0.0982)		(0.100)	(0.0983)	(0.126)	(0.0991)	(0.132)
Devaluation				0.0516				0.0431	0.0523	0.0438	0.0387	0.0411	0.0375
				(0.0444)				(0.0445)	(0.0445)	(0.0445)	(0.0449)	(0.0451)	(0.0452)
Exch Rate Volatility					0.0726						-0.0850		-0.0771
					(0.0687)						(0.123)		(0.132)
Infl. Volatility						-0.00969						-0.0238	-0.0143
						(0.0595)						(0.0595)	(0.0634)
Real GDP	-0.00855**	-0.0129***	-0.0102	-0.0100**	-0.00862**	-0.00870*	-0.0154**	-0.0139***	-0.0119	-0.0166**	-0.0169**	-0.0171**	-0.0172**
	(0.00433)	(0.00420)	(0.00737)	(0.00437)	(0.00431)	(0.00456)	(0.00695)	(0.00426)	(0.00740)	(0.00702)	(0.00686)	(0.00733)	(0.00729)
Constant	1.451***	1.934***	1.621**	1.605***	1.456***	1.468***	2.204***	2.034***	1.802**	2.323***	2.368***	2.382***	2.399***
	(0.456)	(0.445)	(0.776)	(0.460)	(0.454)	(0.482)	(0.732)	(0.451)	(0.779)	(0.739)	(0.715)	(0.774)	(0.761)
Observations	1,351	1,351	1,351	1,351	1,351	1,351	1,351	1,351	1,351	1,351	1,351	1,351	1,351
R-squared	0.833	0.834	0.833	0.834	0.834	0.833	0.834	0.834	0.834	0.834	0.834	0.834	0.834
Mean Arg.	0.506	0.506	0.506	0.506	0.506	0.506	0.506	0.506	0.506	0.506	0.506	0.506	0.506
Mean Ur.	0.588	0.588	0.588	0.588	0.588	0.588	0.588	0.588	0.588	0.588	0.588	0.588	0.588
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Category FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls													Yes
Arg Inflation Index	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged
Arg. Exch Rate Data	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

A 7. Full Robustness Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
VARIABLES	Alt. Exch Rate	Alt. Exch Rate	Alt. Inflation	Alt. Inflation	2015 & 2017	2015 & 2017	2016 & 2017	2016 & 2017	12/2015 Shock	12/2015 Shock	Low Priced	Low Priced	High Priced	High Priced
Argentina x Pre	-0.0522***	0.0171	-0.0636***	0.0121	0.0110	0.00399	-0.131***	-0.198	-0.0823***	-0.0556**	-0.0200	0.0163	-0.0699**	-0.0425
	(0.0126)	(0.0256)	(0.0132)	(0.0217)	(0.0184)	(0.0256)	(0.0359)	(0.120)	(0.0254)	(0.0279)	(0.0225)	(0.0295)	(0.0335)	(0.0396)
Pre (= Treated)	-0.0771**	-0.213***	-0.0461	-0.231***	0.166	0.214	0.0832	0.240	-0.0150	-0.0779	-0.122**	-0.204***	-0.0286	-0.0788
	(0.0316)	(0.0535)	(0.0406)	(0.0576)	(0.143)	(0.227)	(0.0949)	(0.280)	(0.0701)	(0.0718)	(0.0611)	(0.0657)	(0.0927)	(0.0976)
Argentina	-0.111***	-0.0491*	-0.0833***	-0.0808***	-0.0268	-0.0354	-0.0244	-0.0506	-0.0711**	-0.0398	-0.0898***	-0.0613	0.133***	0.160***
	(0.0191)	(0.0264)	(0.0146)	(0.0147)	(0.0844)	(0.0812)	(0.0440)	(0.0648)	(0.0295)	(0.0355)	(0.0301)	(0.0419)	(0.0455)	(0.0608)
Output Gap	0.00399	0.0133*	-0.00254	0.0215*	-0.0633	-0.0690	-0.0316	-0.0557	-0.00949	-0.00715	0.0177	0.0244	-0.0130	-0.0120
	(0.00812)	(0.00795)	(0.0111)	(0.0110)	(0.0644)	(0.0713)	(0.0243)	(0.0471)	(0.0180)	(0.0176)	(0.0175)	(0.0152)	(0.0227)	(0.0221)
Inflation	0.118**	-0.113	0.0203	0.0117	-0.362	-0.323	-0.107	0.0125	-0.00680	-0.132	0.167	0.0549	0.0152	-0.0953
	(0.0583)	(0.0947)	(0.0482)	(0.0485)	(0.321)	(0.311)	(0.180)	(0.281)	(0.111)	(0.139)	(0.104)	(0.160)	(0.150)	(0.225)
Devaluation	0.112	0.109	0.0211	0.0341	0.308***	0.334**	0.0967	0.127	0.0997*	0.0932*	-0.0493	-0.0450	0.128	0.136
	(0.0715)	(0.0711)	(0.0446)	(0.0451)	(0.109)	(0.140)	(0.0642)	(0.0850)	(0.0543)	(0.0544)	(0.0487)	(0.0489)	(0.0986)	(0.0988)
Exch Rate Volatility	0.166	-0.0822	0.0895	-0.102	-0.0502	-0.111	0.159	0.387	-0.148	-0.0335	-0.192	-0.214	0.136	0.160
	(0.192)	(0.182)	(0.104)	(0.100)	(0.178)	(0.226)	(0.257)	(0.459)	(0.101)	(0.135)	(0.168)	(0.160)	(0.224)	(0.235)
Infl. Volatility	0.0272	-0.0306	-0.00190	-0.00210	-0.0856	-0.0909	0.0243	0.0325	-0.0370	-0.0200	0.0262	0.00149	0.0528	0.0305
	(0.0570)	(0.0598)	(0.00207)	(0.00209)	(0.136)	(0.141)	(0.0761)	(0.0781)	(0.0593)	(0.0577)	(0.0708)	(0.0761)	(0.0980)	(0.104)
Real GDP		-0.0204***		-0.0161***		0.00684		0.0115		-0.0108*		-0.0105		-0.00950
		(0.00677)		(0.00453)		(0.0207)		(0.0201)		(0.00593)		(0.00838)		(0.0118)
Constant	0.549***	2.735***	0.548***	2.291***	0.461***	-0.267	0.471***	-0.804	0.543***	1.680***	0.573***	1.696*	0.531***	1.536
	(0.0272)	(0.718)	(0.0284)	(0.484)	(0.112)	(2.233)	(0.0591)	(2.224)	(0.0373)	(0.615)	(0.0426)	(0.875)	(0.0544)	(1.229)
Observations	1,351	1,351	1,351	1,351	867	867	903	903	1,351	1,351	647	647	707	707
R-squared	0.833	0.834	0.833	0.834	0.896	0.896	0.822	0.822	0.834	0.835	0.763	0.764	0.837	0.837
Mean Arg.	0.506	0.506	0.506	0.506	0.512	0.512	0.543	0.543	0.506	0.506	0.480	0.480	0.608	0.608
Mean Ur.	0.588	0.588	0.588	0.588	0.602	0.602	0.603	0.603	0.588	0.588	0.531	0.531	0.631	0.631
GDP Control		Yes		Yes		Yes		Yes		Yes		Yes		Yes
Date Range	2015-2017	2015-2017	2015-2017	2015-2017	2015 & 2017	2015 & 2017	2016 & 2017	2016 & 2017	2015-2017	2015-2017	2015-2017	2015-2017	2015-2017	2015-2017
Shock Date	May 2016	May 2016	May 2016	May 2016	May 2016	May 2016	May 2016	May 2016	Dec 2015	Dec 2015	May 2016	May 2016	May 2016	May 2016
Price Range	All	All	All	All	All	All	All	All	All	All	Low	Low	High	High
Arg Inflation Index	Averaged	Averaged	PriceStats	PriceStats	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged	Averaged
Arg. Exch Rate Data	PriceStats PPP	PriceStats PPP	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA	BCRA

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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