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Youssef Benzarti
Santiago Garriga
Darío Tortarolo

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Can VAT Cuts and Anti-Profiteering Measures Dampen the Effects of Food Price Inflation?
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ABSTRACT

This paper estimates the effect of a temporary and large (21 p.p.) value-added tax (VAT) cut along with anti-profiteering measures on food necessities during a period of high inflation in Argentina. Using barcode-level data across more than 3,000 supermarkets, we find that (1) absent the anti-profiteering measures, the pass-through of the temporary VAT cut to prices was asymmetric: prices responded less to the VAT cut than its repeal resulting in prices that were higher than their pre-VAT cut levels;(2) imposing anti-profiteering measures, such as setting a ceiling on price increases, led to symmetric pass-through rates. Using a household welfare model, we show that the VAT cut resulted in progressive welfare effects and that the anti-profiteering measures were successful at dampening the regressive welfare effects of the asymmetric pass-through. However, we show that these policies benefited high-income households more because pass-through rates are more asymmetric in independent grocery stores, which is precisely where low-income households tend to shop the most.

Youssef Benzarti
Department of Economics
University of California
Santa Barbara, CA 93106
and NBER
youssef.benzarti@gmail.com

Darío Tortarolo
The World Bank
1818 H Street, NW
Office MC3-361
Washington, DC 20433
dtortarolo@worldbank.org

Santiago Garriga
CEFIP-IIE-FCE-UNLP
Calle 6 No 777
La Plata, Argentina
garrigasantiago@gmail.com

1 Introduction

With rampant inflation, many countries are either implementing or considering temporary Value-Added Tax (VAT) cuts on basic necessities to help the vulnerable cope with the soaring cost of living.[1] These cuts are unprecedented in their magnitude and prevalence around the world. In fact, rising inflation has led the European Union to reverse course on its long-standing goal of harmonizing VAT rates across member states by allowing them to freely cut VAT rates on essential necessities. Even in the United States, pundits have been lamenting the absence of a federal consumption tax, which could have been cut in times of high inflation.[2] These VAT cuts are especially important due to their high fiscal cost: given their extensive scope and magnitude, they are likely to amount to a substantial portion of the revenue collected.

Are temporary VAT cuts an effective policy tool to dampen the effects of inflation on purchasing power? In theory, if supply is more elastic than demand then VAT cuts will be passed through to prices thus achieving their policy goals. Empirically, there is a growing body of evidence showing that VAT cuts have limited effects on prices. However, all of this evidence is based on low-inflation periods and is thus uninformative, because, prices are more flexible during periods of high inflation, which will affect how VATs are passed through to prices. Estimating the effect of VAT cuts on prices during periods of high inflation has proved to be difficult because inflation has been very low in most OECD countries in the past decades.

In this paper we answer this question using Argentina as a laboratory, which has struggled with rampant inflation over the past two decades and is the perfect setting to estimate the effect of such policies. We analyze a temporary 21 percentage point VAT cut that was implemented on August 16th of 2019 and repealed on December 31st of the same year and applied to basic food necessities. The policy was implemented following a surprising presidential election result, which led to the collapse of the Argentinian Peso and fears that low-income households would struggle to afford

[1] Here are some examples of countries that have recently cut the VAT rate on foodstuffs to 0%: Peru, Poland, Portugal, Spain, Bulgaria, North Macedonia, Lithuania, Cyprus, Uruguay, Fiji, Oman, and Togo. Bosnia cut its rate from 17% to 5%, Croatia from 13% to 5%, Latvia from 21% to 5%, Turkey from 8% to 1%, DR Congo from 16% to 8%, Costa Rica from 13% to 1%, Romania from 9% to 5%, and Greece from 24% to 13%. Italy, Germany, Belgium, Austria, Slovakia, Estonia, Angola, and the Netherlands are currently considering cutting the VAT rate on foodstuffs.

[2] See, for example, this Ezra Klein opinion piece from October 2022, in the New York Times.

basic food.

Our empirical strategy is a simple dynamic difference-in-differences approach. We use the fact that when the government implemented the VAT cut, it applied to certain goods but excluded other ones that were otherwise similar. The category of goods that were part of the treatment group were largely based on the consumption habits of Argentinians (it was a subset of the basic food basket used to compute the cost of living). But otherwise similar goods, that were also part of the basic food basket, were not included in the treatment group and we use them to construct the control group. For example, the VAT rate was cut for sunflower, corn and mixed oils but not for olive, soy, and canola oils. Similarly, the VAT rate on tea and sugar was brought down to zero but not for coffee and salt. We use this feature of the VAT reform to classify goods into treatment and control groups. Note that the main assumption of our empirical strategy is not random assignment of goods into control and treatment groups, but rather that the control and treatment groups would have evolved similarly had there been no VAT changes. A common test of this assumption is to ensure that the pre-reform trends are parallel. We implement this test and find that both groups follow parallel trends, throughout our analysis. We also address the possibility of spillovers from the treatment to the control group, using two additional approaches, which we discuss below.

Our empirical analysis relies on two main pieces of data. First, we use high-frequency barcode-level retail scanner data from a private company called Scentia, which collects prices directly from the stores, weekly for chain supermarkets and monthly for independent supermarkets.[3] The dataset covers the period from January 2018 to June 2021. It spans 15,126 barcodes, which corresponds to 1,082 brands and 536 producers. The dataset reports the weekly (or monthly, depending on the type of store) tax-inclusive price of a given barcode and its description. In addition, we also observe the quantity sold of each barcode for each period. We also use detailed expenditure microdata from the 2017-2018 National Household Expenditure Survey to further assess the distributional effects of the VAT cut. Note that the share of informal consumption, when defined as transactions occurring in stores that are not registered for the VAT, is only 3% in Argentina (INDEC, 2019).

[3]The latter include so-called "Asian supermarkets" (which is a term used in Argentina to refer to supermarkets operated by individuals of Asian descent, which are relatively common) and a few regional chains, but do not include "mom and pop" shops or convenience stores.

We find that prices respond asymmetrically to the VAT cut and its repeal: roughly 50% of the VAT cut and 90% of the subsequent and equal-sized VAT increase are passed through to prices. Moreover, prices in the treatment group remained higher, in the medium run, relative to the control group after the VAT cut is repealed. Using a simple Constant Elasticity of Substitution (CES) welfare model, we show that this hysteresis had strong and negative welfare consequences: while the VAT cut had positive welfare effects, when in place, these welfare gains were erased by the repeal of the VAT cut because it led to prices that were even higher than their pre-VAT cut levels (even accounting for inflation). On net, and over the medium run, we find that the welfare effects of the temporary VAT cut were negative.

Perhaps as a preemptive measure against the asymmetric responses of prices and resulting hysteresis, the government implemented additional policies aimed at controlling the pass-through of the VAT increase on a subset of the goods treated by the VAT cut. These "anti-profiteering" policies were enforced by the same government agencies that would enforce anti-trust policies. These policies mandated the incidence of the VAT increase by imposing restrictions on the rate at which the price of several goods could increase when the VAT cut was repealed. Moreover, the government was actively monitoring prices in chain supermarkets.[4] These policies have been implemented in several other countries around the world, and are especially common when dealing with the introduction of the VAT or its aftermath. France mandated the incidence of a large VAT cut on the sit-down restaurant industry in 2009 by requiring restaurants to pass through one third of the VAT cut to prices, one third to profits and one third to wages.[5] Similarly, Australia and India recently included specific provisions on anti-profiteering in their 2017 Tax Acts aimed at ensuring that businesses pass on Goods and Services Tax cuts to consumers (Nair & Eapen, 2017). In a different context, California passed bill SBX1-2 which allows the government to monitor the profit margins of oil companies and sue them if they are considered to be too high.[6] In spite of these anti-profiteering measures being implemented and recently becoming more popular, there is

[4]Importantly, the data that we exploit, is not used by the government to monitor prices and so it is not subject to reporting issues aimed at avoiding price regulations.

[5]Tait (1988) discusses the implementation of anti-profiteering measures during the adoption of the VAT in Germany, the Netherlands, Korea, Belgium and Ireland.

[6]See link. The US House of Representatives passed a similar bill, which failed in the Senate.

no empirical evidence on their effectiveness. We use a similar empirical strategy to analyze the effect of these anti-profiteering policies: some of the goods that were treated by the VAT increase were also treated by the anti-profiteering policies, which constitute our treatment group and are compared to the same control group as before.

We show that these policies were successful at dampening the asymmetric effect of the VAT cut by reducing the response of prices to the VAT increase by a factor of two in chain supermarkets. While these policies were not in place in independent supermarkets, one would reasonably expect that the prices of otherwise identical goods in independent grocery stores would still be affected by the anti-profiteering policies through competitive forces. Instead, we find that these caps created a gap in the price of identical goods sold at chain and independent supermarkets. Even more surprisingly, these gaps persist over the medium run, implying that competition is not strong enough to lead to price convergences in chain and independent supermarkets. Importantly, when estimating the welfare effects of the temporary VAT cut along with these anti-profiteering policies, we find that they led to substantial improvements in welfare relative to a counterfactual without any anti-profiteering policies. However, we show that these policies benefited high-income households more because pass-through rates are more asymmetric in independent supermarkets, which is precisely where low-income households tend to shop the most.

One important concern with our analysis is that our treatment effect might be biased because consumers can substitute goods in the control group with those in the treatment group.[7] For example, if the price of tea decreases because the VAT on tea is cut, some consumers may substitute coffee for tea in order to take advantage of tea's lower price. This would lead to a higher demand for the treated goods, and thus would presumably increase their prices, biasing our effects. We address this concern using two main approaches. First, while it is true that some goods in the control group have plausible substitutes in the treatment group (such as tea and coffee or cooking oils), most goods have not (see Table 1). Goods such as breakfast cereal, salt, herbs, dulce de leche and many

[7]Another bias threat is the quasi-simultaneous depreciation episode, which happened three days prior to the VAT cut. For instance, it could have been possible that treated and control goods were affected differently by this shock. Using another depreciation in 2018, we show that the prices of basic necessities targeted by the VAT cut indeed responded more than the control group. However, the estimates are relatively small and imply that, absent the depreciation, our VAT pass-through rates would be 1.4 percentage points larger.

others do not have obvious substitutes in the treatment group which mitigates this substitution concern. Moreover, when considering goods that have obvious substitutes, such as coffee and tea, we estimate that even then there is very little substitution occurring. Second, we re-estimate our main effects using an alternative control group, made of non-food items such as cleaning products, and thus very unlikely to be substitutes, since our treatment group is exclusively made of food items and find very similar estimates. Overall, our evidence suggests that substitution barely affects the treatment effects.

Our paper contributes to three main literatures. First, we contribute to the tax incidence literature in two ways: (1) we are the first to assess the welfare effects of temporary VAT cuts; and, (2) we uncover new facts about tax incidence. Regarding (1): we show that temporary VAT cuts have positive welfare effects in the short run, while the VAT cut is in place, but tend to result in welfare losses on net, over the medium run, after the VAT cut is repealed, even accounting for the welfare gains of the temporary VAT cut.[8] This is important because temporarily cutting the VAT has become a ubiquitous and at the same time very expensive policy. If it tends to lead to negative welfare losses, then governments should be aware of that in order to assess if such policies are suitable to achieve their goals. Related to (2): we uncover three new facts about tax incidence. First, we show that tax incidence mandates, in this case, in the form of anti-profiteering policies, can be effective at controlling how much/little of the tax is passed through to prices. These types of policies have been implemented in other countries (France in 2009, Australia in 2017, etc.) and are being considered in others, including in the US.[9] Moreover, several US States have mandated the incidence of sales taxes on consumers, legislating that prices should not be affected by sales taxes. In spite of governments around the world using such mandates, there has not been any research assessing their effectiveness. We show that tax incidence mandates are effective at dampening the effect of VAT increases on prices, in spite of the fact that not all firms comply with such mandates,

[8]See also Sidhu (1971), Chouinard & Perloff (2004), Delipalla & O'Donnell (2001), Anderson et al. (2001), Doyle & Samphantharak (2008), Kopczuk et al. (2016), Poterba (1996), Kosonen (2015), Gaarder (2018), Carbonnier (2007), Besley & Rosen (1999), Genakos & Pagliero (2022), Buettner & Madzharova (2021) and Fuest et al. (2021) for other tax incidence research. Kotlikoff & Summers (1987) and Fullerton & Metcalf (2002) provide a survey of the earlier empirical and theoretical tax incidence literatures.

[9]Some have proposed to mandate the incidence of the cost of employer sponsored health insurance plans on workers in case of a switch to Medicare for all.

which suggests that their enforcement might be difficult. Second, we uncover new facts about the asymmetric pass through of VAT changes. While Benzarti et al. (2020) shows that the pass through of VATs to prices is asymmetric, we show that this asymmetry exists even in economies that are experiencing high rates of inflation. This is surprising because most (if not all) of the explanations of the asymmetric pass through of VATs are based on some form of price rigidities (menu costs, fairness considerations that prevent firms from freely changing their prices, etc.). Therefore, if inflation is high, as is the case in Argentina during the period we consider, then prices should be very flexible (in fact, we show, using our data, that prices are constantly changing). This makes the finding of asymmetric pass through even more puzzling than previously thought and calls for further investigating its potential mechanisms beyond those based on price rigidity. Third, we show that tax incidence can vary significantly depending on the type of supermarket consumers shop at. This adds to a nascent body of literature that documents empirical tax incidence anomalies, such as Harju et al. (2018b) who show that restaurants respond differently to VAT cuts depending on whether they belong to a chain or are independent.[10] The importance of these tax incidence anomalies for welfare is emphasized in Saez & Zucman (2023).

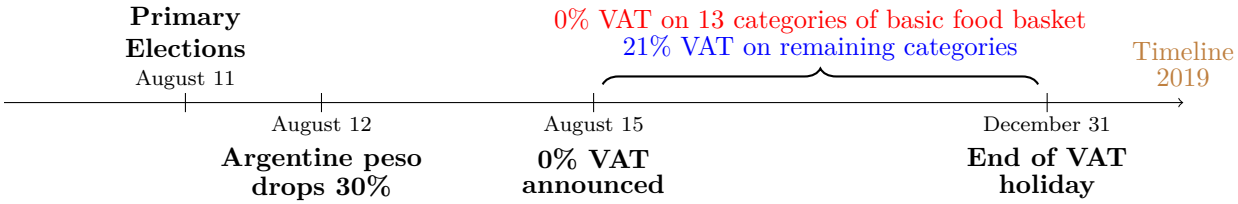
Lastly, we contribute to a sub-literature discussing VATs as a policy tool governments could use to affect the economy, in this case prices in times of high inflation (see Blundell (2009) or Crossley et al. (2009), for example). D'Acunto et al. (2022), for example, consider the suitability of VATs as an alternative to conventional fiscal policy, especially in times when nominal interest rates are close to zero. Our paper shows that, while such policies can be effective at lowering prices, their distributional effects can be unintended.

2 Institutional Setting

The main identifying policy variation we exploit consists of a temporary 21 percentage point VAT cut on essential food items, and anti-profiteering measures when the VAT was reinstated. In this section we briefly describe the main features of the reform as well as its context.

[10] Which is also related to DellaVigna & Gentzkow (2019).

Macroeconomic context and VAT holiday: The VAT change took place in a context of high inflation ($\sim$ 55% annually in 2019), presidential elections, and a sharp depreciation of the Argentine peso. The timeline of events is shown below. On August 11, President Macri lost the primary presidential elections to the left-wing candidate Fernandez by a 15.5 percentage point margin, which was much wider than expected. This triggered a strong (and negative) market reaction the following day, and led to a large decrease in the Argentinian Peso by 30% relative to the US dollar.[11] Three days later, on August 15, the government implemented a 4.5-month long VAT holiday on basic food, with the official goal of containing the impact of the depreciation of the Peso on prices (Executive Order 597/2019). As a consequence, the VAT cut was fully unexpected. It was also announced on that day that the VAT cut would be temporary, and that it would be repealed on December 31, 2019. According to the Minister of Finance, the fiscal cost was projected to be 10 billion pesos (equivalent to USD 160 million, roughly 7% of monthly VAT revenue or 0.6% of the annual VAT revenue). It was funded through the reallocation of budget items.



The tax rate decreased from 21% to 0% on a list of 13 goods from the Basic Food Basket, while other basic food products remained taxed at the standard 21% rate. The Basic Food Basket is used to compute the Extreme Poverty Line and is part of the Consumer Price Index used to measure inflation. All the goods analyzed in this paper are normally taxed at the 21% standard rate, except wheat flour and bread, taxed at the 10.5% reduced rate. According to the National Statistical Institute, the categories with temporary 0% VAT accounted for 26% of total food expenditure from the Household Expenditure Survey. Importantly, the VAT cut only applied to sales made to final consumers, and supermarkets could claim back any VAT credit generated from purchases to suppliers or use it against other tax bills.[12] The left panel of Table 1 shows the list of goods targeted

[11] See Figure A.1. For more details, see this NY Times article.

[12] Treated goods were taxed at a 0% rate during the reform period (as opposed to exempted goods which are untaxed and thus cannot claim for VAT credits on their inputs).

by the policy and the right panel of Table 1 shows other goods that were excluded from the holiday but are otherwise very similar. For example, the VAT rate was cut for sunflower, corn, and mixed oils but not for olive, soy, and canola oils. Similarly, the VAT rate on tea and Yerba Mate was cut but not for coffee. In our empirical analysis, we leverage this feature of the VAT change to estimate price and responses using a simple difference-in-differences approach.

Anti-Profiteering Measures: Although the new Fernandez administration did not extend the VAT holiday, it regulated the re-introduction of the 21% VAT rate on most of the goods that were treated by the VAT cut. In effect, the VAT rate was reverted back to its pre-VAT-holiday level of 21% but the government limited the price increase with caps that varied across categories, which is shown in Table 2.[13] The majority of products treated by the VAT cut were allowed to increase their prices, once the VAT cut was repealed, up to a maximum of 7%. However, some of the treated goods had no cap and could therefore increase prices up to 21% (e.g., canned fruits), and some others were required to keep prices unchanged (e.g., fluid milk). Importantly, this price regulation only applied to supermarket chains, which means that local chains and independent stores could adjust their prices freely. This capped VAT increase therefore provides an unprecedented source of variation to analyze how governments can influence the pass-through of VAT changes.

Both the VAT cut and subsequent VAT increase were highly publicized in the media and in supermarkets, suggesting that both were very salient. For example, Figure A.3a shows the front page of the two main newspapers in Argentina one day after the VAT holiday was announced. In both cases, the front page articles are about the VAT cut. Similarly, Figure A.3b shows the front page of the same newspapers one day after the VAT cut was repealed. The main articles are about the VAT change and how price increases were regulated with different caps. Finally, Figure A.4 shows the way supermarkets communicated the VAT cut to their customers using flyers and price tags, which were mandated by the government.

Real-time price monitoring in supermarket chains: Another institutional feature that is relevant to the interpretation of our findings is the presence of real-time monitoring of prices in

[13] Anecdotal evidence from newspapers mentions that there was a heated meeting on December 31 that lasted 7 hours, where the government, producers, and supermarkets negotiated how the VAT increase would be passed on to prices.

supermarket chains but not in independent supermarkets. This is because, in 2016, the government launched the Electronic Price Advertising System (SEPA) to monitor the prices of supermarkets in real time (Resolution 12/2016). This program, popularly known as "Precios Claros", is currently in place, and is administered and enforced by the Consumer Protection Office. The official goal of "Precios Claros" is to increase the visibility and transparency of prices so that consumers can compare prices across stores and make a more informed decision, especially in times of high inflation when prices are constantly changing.

In practice, the government provides a processing software with detailed guidelines that supermarkets must use to report daily price data for every barcode and point of sale. Stores must complete and send the spreadsheets every day before 6am, which can be rectified until 10am. This information is then shared on an online platform where consumers can search for prices in individual stores using a computer or a mobile phone app.[14] Importantly, in the case of independent supermarkets, participation in the program is optional due to its administrative burden (Art. 4, Res. 12/2016). For the tax reform analyzed in this paper, this means that VAT changes are easier to enforce in supermarket chains because they are under constant scrutiny, since both the government and the public can access their prices daily. Hence, the pass-through of the VAT cut is expected to be higher in chains and lower in independent stores.

Taken together, the temporary and large VAT cut, the regulated VAT increase, and the pre-existing price monitoring system provide an ideal setting to understand how governments can influence, mandate, and enforce VAT incidence in contexts of rampant inflation.

3 Data and Empirical Strategy

3.1 Data

Supermarket Scanner Data. Our analysis is primarily based on retail scanner data provided by the marketing consulting company Scentia LLC. These data consist of high-frequency sales

[14] See Figure A.5 for an example of the salience of "Precios Claros".

information generated by point-of-sale systems across Argentina. In particular, Scentia gathers all scanner-based price and quantity information from chain and independent supermarkets.[15] In the case of supermarket chains, the sample includes the top 12 retail chains that share data from all of their 2,317 stores (e.g., Walmart, Carrefour, Coto, La Anonima, etc.). In the case of independent supermarkets, Scentia collects information from a sample of 800 points of sales (representative of 18,700 total stores in Argentina). These stores mostly comprise independent supermarkets and a few regional chains owned locally rather than "mom and pop" shops or convenience stores.[16] Note that because the data are all scanner-based, they include both sales made with and without receipts, the latter being a relatively common practice in independent supermarkets.

Scentia's database contains the following variables: time period, EAN barcode, unit price paid at the cash register (including taxes and discounts), purchased quantities, total volume, a detailed label describing the item, the brand, the producer, and the region. All products in the dataset are classified into broad categories (e.g., oil, coffee, rice, etc.), which are themselves subdivided into subcategories (e.g., sunflower oil, corn oil, olive oil, ground coffee, coffee beans, coffee pods, etc.) and contain very detailed descriptions (e.g., Nescafé Gold Intense Instant Coffee Jar 200g). This rich set of variables allows us to accurately classify products into treatment and control groups (since some treatments are at the barcode level), as shown in Table 1.

For confidentiality reasons, the database was aggregated at the barcode-region-time level. That is, for each region and time period, the data were aggregated across stores. For chains, we observe weekly information from barcodes in 10 different geographic areas. For independent supermarkets, we observe monthly information from barcodes split into 5 regions.[17] Our dataset covers January 2018 through June 2021 (181 weeks for chains and 42 months for independent supermarkets).

When aggregated to the region-by-barcode-by-month level, each month covers an average of US$170 million worth of grocery sales across 3,117 individual stores in more than 60 disaggregated product categories and across 19,304 barcodes belonging to 642 producers of 1,248 brands.

[15] Scentia also collects scanner data from pharmacies and convenience stores located at gas stations. However, these are not part of the data we purchased.

[16] Some examples are: Cordiez, Buenos Dias, El Nene, Josimar, SuperMax, among others.

[17] The 10 regions are: Capital Federal, Periferia, Cordoba, Litoral Norte, Litoral Sur, Resto Pcia BSAS, Cuyo, NOA, Sur, Austral. The 5 broader areas are: Andina, Cordoba, GBA, Litoral, Resto Pcia BSAS + Sur. See Figure A.6 for more detail about geographic variables.

National Household Expenditure Survey. In addition to the datasets described above, we use detailed expenditure survey microdata from the 2017-2018 National Household Expenditure Survey (ENGHo), which is conducted by the National Institute of Statistics and Censuses (INDEC). This database provides product-level information on food and non-food expenditures, type of stores shopped at, forms of payment used, as well as various characteristics of households. The data were gathered through a questionnaire answered by the head of the household, and diaries that were kept for a week to record daily household expenditures. The survey was conducted between November 2017 and November 2018 in towns with 2,000 or more inhabitants throughout the country. The total number of households in the sample is 45,000, representing 86.7% of the total population. We use this cross-sectional survey dataset to better assess the distributional effects of the VAT cut. In particular, we use it to estimate the share of food expenditure in products subject to the VAT cut as well as the types of supermarkets where those purchases take place.

3.2 Empirical Strategy

Our empirical specification is a simple dynamic difference-in-differences specification. We split our data into treatment and control groups depending on whether a barcode is subject to the specific treatment we analyze (VAT cut, VAT increase with price caps, etc.).[18] First, we provide some graphical and non-parametric evidence by plotting the unconditional mean of prices for the control and treatment groups separately before and after the VAT cut and its subsequent repeal. In each case, we normalize every barcode series to 100 in the week (or month) before the VAT cut was implemented.

Our empirical specification is as follows:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{t \neq 2019w32}^{2020w10} \beta_t D_{it} + \epsilon_{it} \tag{1}$$

where Y_{it}, our main outcome of interest, represents the tax-inclusive price of a given good (barcode)

[18] Note that because this is not an event-study design, the criticism of De Chaisemartin & d'Haultfoeuille (2020) does not apply.

i at time t (weighted average across stores). Y_{it} is normalized to 100 for each barcode i in week 32 of year 2019.[19] D_{it} is equal to one if barcode i is treated in week t and zero otherwise. The main coefficient of interest is β_t which estimates the average difference between the treatment and control groups across all barcodes at time t, relative to week 32 of year 2019. Finally, note that we restrict our dataset to a balanced panel of ≈5,000 barcodes with positive weekly sales between January 2019 and March 2020.

The treatment and control groups include all barcodes that are part of the food categories described in Section 3.1 and shown in Table 1. The control group includes all barcodes that fall under the following categories: Other cooking oils (olive, soy, canola); Rice-based meals; Breakfast cereal; Coffee; Salt; Herbs, Spices, & Seasonings; Dulce de leche; Jam and Jelly; Other flours; Crackers and Biscuits; Chocolate; Mayonnaise; Vinegar; Dried legumes and beans.

As seen below, the results from estimating this dynamic difference-in-differences specification mirror those of the unconditional means graphical evidence. This is reassuring and mitigates concerns that our results are significantly affected by the particular specification we use.

4 Results

We first show the effect of the VAT cut and its repeal on the prices of goods that were not subject to the anti-profiteering measures. We then estimates the effect of the VAT cut and its repeal on those goods that were subject to the anti-profiteering measures. And we also break-down the estimates by chain (where the anti-profiteering measures were implemented) and independent stores.

4.1 VAT Cut and Increase *Without* Anti-Profiteering Measures

Figure 1a shows the non-parametric effect of the VAT cut and its repeal on prices, in the control and treatment groups. Here, the treatment group only includes the barcodes that were not subject to the anti-profiteering measures, i.e., there were no caps on the rate of increase of prices when the

[19] For monthly regressions, the outcome variable is normalized to 100 in July 2019.

VAT was increased. The dataset used in this Figure pools chain and independent supermarkets together, thus the observations are at the monthly level.

Prices are normalized to 100 in the month prior to the VAT cut, i.e., July 2019. Four findings are worth highlighting. First, the trends for the control and treatment groups are parallel as can be seen in the six months preceding the VAT cut. Second, there is a sharp break in the series immediately after the VAT cut is implemented, as prices in the treatment group grow at a substantially lower rate than those in the control group. Note that prices trend positively, since we are plotting nominal prices and inflation is high (about 50% in 2019). Third, there is another break in the series when the VAT cut is repealed, i.e., January 2020. Here, prices in the treated group increase above the level of those in the control group, thus exceeding their counterfactual pre-VAT cut levels. Fourth, prices in the treatment group do not appear to be converging down to those of the control group, suggesting that the asymmetric response of prices to the VAT cut and increase might be long-lived. Note that we stop the series in March 2020 because of the onset of Covid-19 (but our data would allow us to extend them further).

Figure 1b plots the result of estimating equation (1) on the exact same data as in Figure 1a, which allows us to add standard errors and also precisely estimate the magnitude of the effect of the VAT cut on prices. Overall, the results we get from estimating (1) closely match those of the unconditional means plotted in Figure 1a. First, we find that the trends are mostly parallel with a small difference between treatment and control groups pre-reform, which might be biasing our pass-through estimates downwards. Note that this parallel trend issue disappears once we consider a larger sample below. We also find that prices decrease on average over the four-month period following the VAT cut by 9.3 percentage points.[20] This corresponds to a pass through of the VAT cut to prices of 53% relative to the full pass-through rate of 17.4 percentage points.[21] Finally, our estimation confirms that prices respond to the repeal of the VAT cut in the treatment group and exceeds its pre-VAT cut levels by 5.9 percentage points, with no evidence of convergence to zero. We provide a summary of the price effects in Table 4.

[20] We exclude the point estimate from August in this calculation as it is mechanically partially treated (the VAT cut was passed on August 16th).

[21] Note that the VAT rate is decreasing from 21% to 0% corresponding to a -0.21/1.21 x 100 = 17.4% decrease in prices in the case of full pass-through.

Overall, the VAT cut resulted in substantial price effects, but was short of full pass-through. And the VAT increase led to prices that were higher than their pre-VAT cut level, with evidence of hysteresis.

4.2 VAT Cut and Increase *With* Anti-Profiteering Measures

While there was no formal government regulation of how much of the VAT cut supermarkets should pass through, several anti-profiteering measures were put in place for the VAT increase (see Table 2).[22] In particular, regular rice (long grain white), dried pasta, tea, yerba mate, mate cocido, sugar, canned vegetables and beans, corn and wheat flour and regular yogurt were subject to a 7% cap on price increases. Furthermore, milk was subject to a 0% price increase, i.e., its price was held nominally fixed. On the other hand, corn oil, other rice (basmati, brown, and organic), canned fruits, and yogurts with fruits or cereals mixed in, were not subject to any price controls. Importantly, these price controls only applied to chain supermarkets, but not to independent supermarkets; this was mostly due to the fact that the government has limited capacity to enforce the regulation and monitor prices in the more than 18,000 independent stores around the country.

The experiment at hand offers a unique opportunity to show that governments can affect tax incidence. In order to assess the effect of these price controls at the time of the VAT increase, we break down the list of goods into a control group, which is unaffected by the VAT cut and a treatment group which is made of those barcodes that are treated by the VAT cut and that are subject to the price caps when the VAT is increased. We have several empirical findings, which we discuss below.

Chain and independent supermarkets pooled together: Figure 2a shows the non-parametric effect of the VAT cut and its repeal on prices, in the control and treated groups for those goods that were subject to the price caps. In this figure, we pool chain and independent supermarkets together (and estimate them separately in the next section). There are three main findings. First, the trends

[22] We refer to price controls as *caps* on how much prices could increase to mitigate the VAT reintroduction. We do not refer to price controls as nominal price freezes. In a companion paper, we are separately analyzing the effects of price freezes in Argentine supermarkets, which were introduced in 2014 on a basket of about 500 barcodes.

for the control and treatment groups are parallel. Here, since the sample of goods we consider is substantially larger than for uncapped barcodes, the parallel trend assumption holds even better. Second, there is a sharp break in the series immediately after the VAT cut is implemented. Third, there is another break in the series when the VAT cut is repealed. Here, prices in the treated group increase enough to match price levels in the control group, thus restoring the previous equilibrium (with no asymmetry).

Figure 2b plots the result of estimating equation (1) on the exact same data as Figure 2a. Overall, the results we get from estimating (1) closely match those of the raw means plotted in Figure 2a. First, we find that the trends are indeed parallel with no substantial price effects estimated pre-reform. We also estimate an average price decrease of 10.3 percentage points following the VAT cut. This corresponds to a pass through of the VAT cut to prices of 60% (very similar to the uncapped goods). Finally, our estimation confirms that prices respond to the repeal of the VAT cut in the treatment group enough to revert back to the levels in the control group (Table 4).

Pooling chain and independent supermarkets together allows us to assess the overall effect of the VAT cut along with the anti-profiteering measures, even though independent supermarkets were not subject to these measures. Overall, we find that the anti-profiteering measures were successful, even on aggregate, at mitigating the asymmetric pass-through. We estimate the welfare effect of the anti-profiteering measures formally in Section 5.

Chain and independent supermarkets separately Figure 3a shows the price effects of the temporary VAT cut in supermarket chains and Figure 3b in independent supermarkets.[23] The empirical specification counterparts of these two figures are plotted in Figure 4a and 4b, respectively. When considering these two types of supermarkets separately, we estimate dramatically different pass-through rates of the VAT cut and its repeal. Similarly to Figures 2a and 2b which pool both chain and independent supermarkets, we find that the pre-trends are parallel and estimate a break in the series at the time of the VAT cut and when it is repealed as well. The main difference is that the response to the VAT cut and the VAT increase is substantially larger when considering

[23]For this exercise, the treated group includes those goods that were subject to the cap during the repeal of the policy and, also, those who were not.

supermarket chains. This is true both in the unconditional mean figures (Figures 3a and 3b)[24] as well as using our empirical specification (Figures 4a and 4b).

Overall, we estimate that the pass-through rate of the VAT cut is 84% for supermarket chains and 35% for independent supermarkets. Note that observations are at the weekly level for supermarket chains and at the monthly level for independent supermarkets. This is due to the frequency at which the data provider collects this information. To ensure that the level of aggregation is not driving this difference, we aggregate the price observations for supermarket chains at the monthly level and plot the estimates in Figure 4b. We find that aggregating the data at the monthly level barely affects the estimates or general trends. The price changes following the VAT cut are 14.7 percentage points at the weekly level and 14.9 percentage points at the monthly level for supermarket chains, a difference that amounts to approximately 1% of tax incidence and is therefore not meaningful (Table 4).

This stark difference in the pass-through rate of the VAT increase in chain and independent supermarkets is likely due to the fact that the anti-profiteering measures were in place in the chain supermarkets only. To further confirm this, we next estimate the effect of the VAT increase for barcodes subject to the anti-profiteering measures to those that were not in chain supermarkets only. We also discuss some of the reasons why the VAT cut pass-through rates are different in chain and independent supermarkets in Section 6.

Comparing capped and uncapped goods by store type Figure 5a compares the change in prices for those commodities that are subject to the 7% price increase cap, to those with no price caps, in chain supermarkets. In both cases, the control group is the original set of barcodes facing a 21% VAT rate. This figure shows that the goods with no price caps experience a price increase that is almost double that of those that are subject to the 7% cap.[25] However, while the 7% cap is effective at mitigating price increases, Figure 5a shows that supermarkets are able to increase

[24]Interestingly, even in a context of relatively high inflation, nominal prices go down for the treated necessities in chain stores right after the VAT cut was implemented.

[25]Figure A.7 provides two case studies that add credibility to the finding. The figure compares regular rice versus other rice, and canned fruit versus canned vegetables. Although prices respond similarly to the VAT cut, the response to the VAT increase is remarkably different.

prices by more than 7%. This is likely due to the fact that monitoring percentage increases can be difficult. This is confirmed in Figure 5b, which shows that when price controls take the form of a price freeze, i.e., holding the nominal price fixed, as is the case for milk, prices experience no increase at the time of the VAT increase.

Next, we show that the anti-profiteering measures had long-lasting effects, even after the caps were no longer in place. Figure 5 shows that the price gap between products with and without price caps is stable until the end of 2020 (which is when our data stops). This "hysteresis" effect is especially surprising in a context of high inflation, where supermarkets could easily game the regulation by simply staggering price increases over several weeks, while ensuring that any increase in a given period is smaller than 7%.

Finally, we estimate the degree to which the anti-profiteering measures spilled over from chain to independent supermarkets. The anti-profiteering measures applied to certain goods in chain supermarkets only. If there is some spillover from chain to independent supermarkets, possibly because of competition, we may observe a price effect for those goods in independent supermarkets, even though they were not subject to anti-profiteering measures. To investigate this, we re-estimate equation (1) for independent supermarkets on barcodes that are subject to the anti-profiteering measures and those that are not (in chain supermarkets). We find that prices in both groups responded very similarly to the VAT increase (and the VAT cut), suggesting that lower prices in chain supermarkets due to the anti-profiteering measures did not lead to lower prices in independent supermarkets (see these results in Figure 6). This suggests that price competition between chain and independent supermarkets is relatively weak, which could explain why the difference in prices in the control and treatment groups in chain and independent stores do not appear to converge even a year after the VAT increase is implemented, as shown in Figure 4b.

Overall, the anti-profiteering measures were successful at mitigating the asymmetric pass-through of the temporary VAT cut in chain supermarkets. However, the effect of these anti-profiteering measures did not spill over onto independent supermarket prices, where we still estimate that pass-through was asymmetric. We investigate the welfare effects of the temporary VAT cut and the anti-profiteering measures more formally in Section 5.

4.3 Robustness: Substitution and Currency Depreciation

Substitution across products in treatment and control: One concern with our difference-in-differences empirical strategy is that our treatment effect might be biased because consumers can substitute goods in the control group with those in the treatment group. For example, if the price of tea decreases after the VAT cut, some consumers may substitute tea consumption with coffee in order to take advantage of the lower prices. This would lead to a higher demand of the treated goods, and thus would presumably increase their prices, biasing our effects downwards.

We address this "SUTVA (Stable Unit Treatment Value Assumption) violation" concern using two main approaches and we summarize our findings in Figure 7. First, while it is true that some goods in the control group have plausible substitutes in the treatment group (such as tea and coffee or cooking oils), most goods have no obvious substitutes. As can be seen in Table 1, goods such as breakfast cereal, salt, herbs, dulce de leche and many others do not have obvious substitutes in the treatment group which mitigates this substitution concern. We formalize this idea by redefining our control group by excluding the categories that are likely close substitutes of some of the treated goods— in this case, rice-based meals, coffee, cooking oils, dried legumes, other flours, soups and prepared pasta. We then re-estimate our dynamic difference-in-differences empirical specification on chain supermarkets simply because the effect of the VAT is significantly larger than in independent supermarkets thus providing the most opportunity for finding any substitution effects (there is no reason to substitute to other goods if the price of these other goods do not decrease).

Figure 7a shows that even when accounting for goods that have obvious substitutes, such as coffee and tea, the results barely change. The average decrease in prices after the VAT was cut is 15.2 percentage points in the specification that excludes close substitutes from the control group, implying a pass-through rate of 87%. This price effect is slightly larger than the 14.7 percentage point decrease found using our original control group. Indeed, substitution operates in the expected direction slightly biasing our estimates downward. Nevertheless, the difference is very small and does not change the conclusions of the paper.[26]

[26]For transparency, the left panel of Figure A.8 illustrates the anatomy of the substitution mechanism by comparing the price changes in tea and different types of coffee relative to the remaining categories in the control group. While instant coffee exhibits a decrease in prices, ground coffee does not. In contrast, the right panel shows that the

Second, we re-estimate our main effects using an alternative control group constituted solely of non-food items (which were previously excluded from our approach), and thus very unlikely to be substitutes, since our treatment group is exclusively made of food items.[27] Note that, we only use scanner data from one region, namely *Periferia* because, we were only able to purchase non-food categories for this region, thus the larger standard errors. The results are shown in Figure 7b. We find that the average price of the treated goods decreased by 15.7 percentage points relative to this alternative control group made with non-food products. For comparison, we superimpose the effect estimated with the original control group, which was an estimated price decrease of 15.1 percentage points. Although substitution might be present in our setting, it barely affects the results. Indeed, the pass-through rates of the VAT cut are 90% or 87% depending on the control group used.

Pass-through of the Peso depreciation: Another threat to our research design is the quasi-simultaneous depreciation episode, which happened three days prior to the VAT cut was enacted (see Section 2). If the sharp depreciation of the Argentine peso against the US dollar affects basic necessities subject to the VAT cut more strongly than untreated food products then, ceteris paribus, one would expect the prices of goods in our treatment group to increase more than in the control group. Hence, the pass-through of the VAT cut to prices would be partially offset by this depreciation shock, thus making our pass-through rates a conservative estimate. In other words, absent the depreciation of the peso, the prices of the zero-rated goods would have decreased even more.[28]

To address this concern, we leverage another depreciation episode that took place exactly one year before the VAT change and compare the evolution of prices in treatment and control. On August 30, 2018, Argentina experienced the second most important depreciation of the peso since the year 2002—similar in magnitude to the depreciation episode of August 12, 2019 (Figure A.1).

average price of breakfast cereal (not affected by the VAT cut) does not seem to respond while the price of sliced bread decreases sharply during the VAT holiday.

[27]Non-food products include office supplies, body moisturizers, antiperspirants, hand soap, laundry detergent, bleach, surface cleaners, toilet paper, shampoo, and cleaning wipes.

[28]In principle, imported goods are more likely to be affected by the depreciation of the Argentinian Peso. Therefore, we re-estimate Figure 2a while excluding goods that are imports. We plot these new estimates in Figure A.10: we find very similar pass-through estimates, suggesting that the control and treatment groups do not respond differently to the currency depreciation episode.

In Figure 8a, we run our dynamic difference-in-differences specification (1) in supermarket chains for the years 2018 and 2019 up to the week before the VAT was cut. We omit, from the regression, the first week of 2018 so that all the coefficients are measured relative to that week. As a reference, we overlay the nominal exchange rate which is measured on the right axis.

Figure 8a shows that the prices of basic necessities targeted by the government for the VAT cut indeed responded more to the depreciation of the Peso back in 2018. Indeed, the price gap between treatment and control groups closely tracks the evolution of the exchange rate. Relative prices remain stable up to week 25 of 2018, then start to increase *pari-passu* with the exchange rate and stabilizes again after week 45. This evidence for 2018 strikes us as remarkable and suggests that the government might have been right in targeting necessities after the 2019 peso depreciation to alleviate the burden on low-income households.[29]

Nevertheless, the magnitude of the effects of inflation on prices are small and do not seem to pose a threat. On the one hand, according to Figure 8a, the nominal exchange rate roughly increased from 20 to 40 pesos per dollar—corresponding to a 100% increase. On the other hand, the prices of the (later) zero-rated goods increased by a modest 6% relative to the control group. By scaling this price change relative to the change in the exchange rate we obtain an elasticity of 0.06. By applying this elasticity to the depreciation of the peso of 24% in 2019 (Figure A.1), we conclude that—absent the VAT cut—prices of treated goods would have increased by $0.06 \times 0.24 = 1.44\%$ relative to the control group. This means that, absent the depreciation, the price drop reported in Figure 4a would be 1.44 percentage points larger.

[29] To aid the interpretation of the exchange rate change as causal, we use aggregate data from INDEC, classify the categories of the CPI into treatment and control, and run our diff-in-diffs specification to estimate the effect back in 2017. Figure A.9 shows convincing evidence that the prices of treatment and control did not change differently in 2017 when the exchange rate was indeed very stable.

5 Welfare and Distributional Effects of the Temporary VAT Cut

While there may have been political reasons underlying the decision to temporarily cut the VAT, the explicit policy goal was to ensure that low-income households would still have access to a basket of necessities during a period of higher-than usual inflation triggered by the depreciation of the Argentinian Peso caused by the surprising election results. In this section, we investigate the welfare and distributional effects of the VAT cut and assess whether it achieved its policy goals.

5.1 Data

Ideally, one would be able to observe the income of every shopper at every supermarket, which would allow us to precisely track the distributional consequences of the VAT cut, given that different income households may shop at different stores and may purchase baskets of different composition. Unfortunately, we do not observe this type of information. Therefore, we complement our analysis with the household expenditure survey data described in Section 3.1. In particular, we use the consumption structure of Argentine families and estimate the share of food expenditure in products subject to the VAT cut as well as the types of supermarkets where those purchases take place.

5.2 Stylized Facts

Figure 9 reports the share of products treated by the VAT cut in the total household food expenditure, by income deciles. In other words, it shows how relevant zero-rated food items are for household budgets across the income distribution. This share decreases with income, with the lowest decile spending 27% of the food budget on the goods subject to the VAT cut and the richest decile spending only 15% (the national average is 20%). This pattern suggests that the government was right in its motivation to cut the VAT rate on those goods as they represent a higher share of expenditures in the food budget of low-income households. Nevertheless, the bottom panel shows that household expenditure on zero-rated goods (in absolute values) increases with income. This

suggests that richer households possibly benefited the most (in absolute terms) from this subsidy. We investigate these effects more formally in Section 5.3 below.

We complement the previous empirical fact by plotting the propensity to shop at chain versus independent supermarkets by income groups. Figure 10 shows the share of money spent on food by income decile and store type (independent and chain supermarkets).[30] The share of money spent on food items subject to the VAT cut by the lowest-income decile in independent stores is 48% as opposed to 22% in chain supermarkets. The relationship between income and money spent on treated food items at chain supermarkets is increasing, and decreasing for independent supermarkets. At the other end of the income distribution, the top decile of households spend 58% of the their food expenditure at supermarkets and 25% at independent ones.

This finding, that the propensity to purchase food items at chain supermarkets increases with the income of households, coupled with the fact that the pass through of the VAT cut in chain supermarkets was more than twice that of independent supermarkets implies that the VAT cut likely benefited richer households more. And while there is no doubt that lower income households benefitted from the VAT cut, both because some of them shop at chain supermarkets and because independent supermarkets pass-through some of the VAT cut, this evidence implies that there was scope for the VAT cut to be better targeted.

Next, we derive our welfare model and estimate it. Our estimates are mostly a reflection of the two facts presented above.

5.3 Quantifying the welfare effects of VAT changes

In this section, we derive a general expression for assessing the impact of VAT changes on household welfare as a function of various moments observed in our micro-level data. Consider a household of

[30] Specialized stores, like butcheries and bakeries, are not in our data and were not part of the VAT change. Similarly, purchases from street vendors are negligible in our setting.

type h with income w_h and CES preferences for good g purchased at store type s:

$$U_h = \left(\sum_{g(s)} \beta_{g(s)h} q_{g(s)h}^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}$$

where $\beta_{g(s)h}$ are taste-shifters and σ is the constant elasticity of substitution across varieties and store type. To save on notation, we use g instead of $g(s)$. Then, the expenditure share in product g at store type s for household h ($\alpha_{g(s)h}$) is:

$$\alpha_{g(s)h} = \frac{p_{gh} \cdot q_{gh}}{e(\mathbf{p}, u)} = \frac{\beta_{gh}^{\sigma} p_{gh}^{1-\sigma}}{\sum_g \beta_{gh}^{\sigma} p_{gh}^{1-\sigma}} = \frac{\beta_{gh}^{\sigma} p_{gh}^{1-\sigma}}{P_h^{1-\sigma}} \tag{2}$$

where

$$P_h \equiv \left(\sum_g \beta_{gh}^{\sigma} p_{gh}^{1-\sigma} \right)^{\frac{1}{1-\sigma}} \tag{3}$$

Welfare can be measured with the indirect utility function V_h. From the consumer problem:

$$V_h(\mathbf{p}, w_h) = \frac{w_h}{\left(\sum_g \beta_{gh}^{\sigma} p_{gh}^{1-\sigma} \right)^{\frac{1}{1-\sigma}}} = \frac{w_h}{P_h}$$

we have that

$$\frac{dP_h}{dp_{gh}} = \frac{\left(\sum_g \beta_{gh}^{\sigma} p_{gh}^{1-\sigma} \right)^{\frac{1}{1-\sigma}}}{\left(\sum_g \beta_{gh}^{\sigma} p_{gh}^{1-\sigma} \right)} \frac{\beta_{gh}^{\sigma} p_{gh}^{1-\sigma}}{p_{gh}} \tag{4}$$

$$\frac{dP_h / P_h}{dp_{gh} / p_{gh}} = \frac{\beta_{gh}^{\sigma} p_{gh}^{1-\sigma}}{P_h^{1-\sigma}} \tag{5}$$

$$\frac{d \ln P_h}{d \ln p_{gh}} = \alpha_{gh} \tag{6}$$

Totally differentiating we get that

$$d \ln V_h = d \ln w_h - \sum_g \frac{\beta_{gh}^{\sigma} p_{gh}^{1-\sigma}}{P_h^{1-\sigma}} \cdot d \ln p_{gh} = d \ln w_h - \sum_g \alpha_{gh} \cdot d \ln p_{gh} \tag{7}$$

Assuming that earnings do not change yields:

$$d \ln V_h = -\sum_{g(s)} \alpha_{g(s)h} \cdot d \ln p_{g(s)h} \tag{8}$$

While our CES approach imposes a particular structure on household demand, it yields a simple and transparent formula that is solely based on observable expenditure shares and price changes. In this sense, this approach is equivalent to a Laspeyres price index which uses initial consumption weights ($q \cdot p'/q \cdot p$). In practice, for each decile h, we multiply the observed pre-reform expenditure share on food group g at store type s (independent or chain), $\alpha_{g(s)h}$, by the estimated price change of food group g at store type s induced by the VAT change, $d \ln p_{g(s)h}$. For each decile, the total welfare change is the sum of these moments across the g food groups and store types (independent and chain). We perform this exercise separately for the VAT cut and the VAT increase.

There are several limitations to this model, mostly due to its simplicity and tractability. The first limitation is that it holds pre-reform market shares fixed, assuming away any welfare effects due to product or store switching behavior induced by the VAT cut. Second, the utility function assumes the same elasticity of substitution between products and stores.[31] Third, the model only estimates the immediate impact of the policy and does not account for any longer term effects. Because the effects of the VAT increase are longer lived than those of the VAT decrease, lasting at least 1 year, whereas the VAT cut was only in place for 4.5 months, we are likely underestimating the welfare effects of the VAT increase relative to the VAT cut. This is likely to be problematic if we try to assess the net welfare of the policy, which we do not attempt (in part because of this limitation). Fourth, our model does not account for any changes in firm profits or government revenue, and only focuses on the welfare of consumers.

[31] A more sophisticated exercise could consider a nested CES model like the one proposed by Atkin et al. (2018). This, however, would complicate the analysis and require to estimate additional parameters (e.g., elasticities of substitution). Note also, that while the demand system is homothetic, we capture potential heterogeneity across the income distribution by allowing income deciles to differ in their preferences for consumption bundles at different stores and product groups and their expenditure shares across product groups (Atkin et al., 2018).

5.4 Welfare Estimates

Figure 11 plots the result of estimating the welfare model for the VAT decrease (panel (a)) and the VAT increase panel (b), by income deciles.

Panel (a) plots the welfare effects of the VAT decrease as observed and the welfare effect of a counterfactual where we assume that the VAT cut was fully passed through to prices. We find that the VAT cut resulted in positive welfare impacts and was progressive, benefitting the lowest income decile three times more than the highest ones. This is mostly due to the fact that the share of zero-rated goods in the expenditure of low-income households is substantially higher than for higher-income households. Note, however, that the welfare increase is substantially lower than the counterfactual that assumes full pass through. This "welfare leakage" is mostly explained by the limited pass-through in independent grocery stores, which is precisely where low-income people tend to shop at, which raises important policy implications when designing VAT cuts aimed at low-income households.

Panel (b) plots the welfare effect of the VAT increase. Overall, the impact of the VAT increase along with the anti-profiteering measures was regressive, hurting low-income households more than high-income ones. This is mostly due to the fact that low-income households tend to shop at independent stores more, where prices exhibit more asymmetric pass-through, while high-income households shop at supermarket chains, where the anti-profiteering measures were successful at keeping prices from increasing too much. We compare these observed effects to a counterfactual where we assume that there were no anti-profiteering measures. We construct this counterfactual by taking our estimates of pass-through rates on those goods that were not subject to the anti-profiteering measures and applying these estimates to the goods that were subject to these measures. Both types of goods were following similar trends prior to the VAT increase and so we believe that our approach is reasonably sensible. We find that, absent the anti-profiteering measures, the impact of the VAT increase would have been significant and regressive, affecting the lowest-income decile four times more than the highest one. Given that observed welfare is substantially larger for all income deciles and somewhat similar, these anti-profiteering measures appear to have benefitted low-income deciles substantially more. And because these estimates do not account for any of the

long-term effects, i.e., the fact that the asymmetric pass-through seems to persist over at least a one-year horizon, the effect of the anti-profiteering measures are likely to be under-estimates, i.e., we would expect the counterfactual welfare estimates with no anti-profiteering measures to be substantially more negative when accounting for longer run horizons.

6 Why is the pass-through of the VAT cut smaller in independent stores?

While we do not know with certainty what could be driving the differences in pass-through rates of the VAT cut in independent versus chain supermarkets, our understanding of the political environment at the time of the VAT cut suggests that this might be due to two complementary facts: (1) the government exerted significant political pressure on supermarkets to try and pass through as much of the VAT cut as possible. Government officials even had meetings with the executives of the four largest supermarket chains (Carrefour, Walmart, Jumbo, La Anonima) to try and have them cut prices as much as possible following the VAT cut. For this reason they may have been more receptive to the political pressure; and, (2) the government's price monitoring system (which is not the dataset we use in our analysis) mostly collects data from supermarket chains. Hence, since independent supermarkets know that the government cannot easily observe the prices they charge, they can more easily avoid cutting prices without incurring much political fallback.

In addition we believe there are three alternative explanations for the muted response to the VAT cut in independent supermarkets (which we describe below in more details): (1) a higher propensity to evade VATs in independent supermarkets, (2) pricing strategy differences across store types, and, (3) different levels of competition across store types.

6.1 Evasion

A possible explanation for the fact that chain and independent supermarkets respond differently to the VAT cut is the fact that chain supermarkets are likely to operate more formally than independent

ones, thus more likely to issue receipts and charge the VAT. Conversely, independent supermarkets might evade the VAT by relying more on cash transactions. If that is the case, VAT changes would have dampened effects on prices in independent supermarkets, leading to muted pass through rates of both VAT cuts and VAT increases. Bachas et al. (Forthcoming) provide very compelling evidence in support of this explanation in low-income countries around the world. They show that consumers at so called "traditional stores", which are small mom and pop shops, tend to bear less of the VAT than consumers at modern stores. There are two important distinctions between our setting and the one Bachas et al. (Forthcoming) consider. First, Argentina is a more advanced economy than the set of countries that Bachas et al. (Forthcoming) consider. For example, the share of informal consumption, defined as transactions occurring in stores that are not registered for the VAT, is only 3% in Argentina.[32] This share is substantially higher in the countries considered by Bachas et al. (Forthcoming) and averages more than 30%. Second, the independent supermarkets we consider would actually be classified as modern stores by Bachas et al. (Forthcoming) as opposed to traditional stores, which are mom and pop shops and are excluded from our analysis. Nevertheless, this explanation could be at play in our setting. While gathering evidence of tax evasion is impossible using our data, we provide a model that shows such pass-through effects would exist if evasion is more prevalent in independent supermarkets.

The following model builds on Kopczuk et al. (2016). The equilibrium condition is given by $D(p) = S(p, t)$, where D is demand, S is supply, p is price and t is a per unit tax remitted by the supply side. Denote by: $c(q)$ the variable cost of producing q units of the good, F the fixed costs of production, $\Phi(e)$, the cost of evasion, where $0 \leq e \leq 1$ is the quantity of evasion.

Firm profit is given by $\Pi(q, e) = p.q - c(q) + t.e - \Phi(e) - F$

Optimal evasion is simply given by: $t = \Phi'(e)$: the higher the tax rate, the more evasion the company engages in. This equation implicitly defines the optimal evasion rate: $e^*(t)$.

The production decision is given by the first order condition: $p = c'(q)$, which implicitly determines the optimal quantity: $q^*(p)$.

[32]Source: See page 31, Table 2.4 in https://www.indec.gob.ar/ftp/cuadros/sociedad/engho_2017_2018_resultados_preliminares.pdf

Two additional conditions are needed to close the model: (1) a zero profit condition: $\Pi = \Pi^v(p) - (F - R(t)) = 0$, where $\Pi^v(\cdot)$ is operating profits (no fixed costs) and $R(\cdot)$ is the revenue from tax evasion; and (2) a free entry condition: $Nq^*(p) = Q(p+t)$, where $Q(\cdot)$ is total demand and N the number of firms.

The first order condition is given by $\frac{\partial \Pi^v}{\partial p}\frac{dp}{dt} + R'(t) = 0$. Using the envelope theorem for $\Pi^v(\cdot)$ and $R(\cdot)$, we get the following pass through formula:

$$\frac{dp}{dt} = \frac{e^*(t)}{q^*(p)}$$

If the tax rate increases, the net of tax price received by producers falls by $\frac{e^*}{q^*}$, hence consumer price increases by $1 - \frac{e^*}{q^*}$. If tax evasion is $e = 0$, then the full incidence is on consumers, which is due to the fact that supply is perfectly elastic given the free entry and zero profit conditions. If tax evasion is indeed higher in independent supermarkets, then we would expect a muted pass-through of the VAT cut, even with identical supply and demand elasticities, relative to supermarket chains, which would be consistent with our empirical evidence.

6.2 Competition

Differences in pass-through may be due to different levels of competition in chain and independent supermarkets. For instance, it could be that independent stores are located in more isolated places. Genakos & Pagliero (2022) show how pass-through varies with competition in isolated oligopolistic markets in Greece. The setting in this paper is different from ours for two reasons. First, they focus on a particular and specific market (gasoline). Second, they look at a specific geographical setting (islands).

We show in Figure 12 that competition can explain some (but not all) of the differences in pass-through rates. Figure 12 pools chain and independent supermarkets and breaks down pass-through rate estimates for goods at the barcode level that are sold in both types of supermarkets versus goods that are sold in either one of them but not both. Presumably, goods that are sold

in both chain and independent supermarkets will be more competitive, probably leading to higher pass-through rates of the VAT cut. This is indeed what Figure 12 shows: the pass-through rate for goods that are present in both supermarket and independent chains is 12 percentage points, while that of goods that are only present in one of them is 9 percentage points. This suggests that competition is likely driving some of the differences in pass-through rates.

6.3 Different Pricing Strategies

Another explanation could be that chain and independent supermarkets follow different pricing strategies. DellaVigna & Gentzkow (2019) and Harju et al. (2018a) provide compelling evidence of such behavior in different settings. DellaVigna & Gentzkow (2019) provide evidence that national chains respond differently to local shocks compared to local supermarkets in the US. While, Harju et al. (2018a) estimate the incidence of a large VAT cut on chain versus independent restaurants in Finland and find that chain restaurants pass through 100% of the VAT cut in the short run, while independent restaurants pass through 0% of the VAT cut. Notably, these patterns revert in the medium run, whereby chain restaurants start raising their prices leading their pass through rates to converge to those of independent restaurants. At first glance, this behavior appears consistent with the findings from our paper. However, there are two main differences. First, the shock we analyze is not a local shock, and is instead a national policy, which is inconsistent with the explanation from DellaVigna & Gentzkow (2019). Second, we do not observe a convergence in pass through rates in chain and independent supermarkets as is shown in Harju et al. (2018a).

We show, for example, that chain and independent supermarkets respond very similarly to other economic shocks when there is no government interference. In particular, we provide evidence that chain and independent supermarkets display similar pricing behavior when responding to changes in currency value which directly affect prices. Indeed, the Peso experienced a large and sudden devaluation in August 30th, 2018, causing a 24% increase in the exchange rate of the Peso against the US Dollar, which is plotted in Appendix Figure A.1. As a consequence, supermarkets had to adjust their prices, especially for imported commodities. In Appendix Figure, A.2 we plot the distribution of price changes in supermarket chains in the upper panel and in independent stores in

the bottom panel as a response to the large and sudden devaluation of the peso. The red distribution plots the differences in prices between September 2018 and July 2018, effectively capturing the pass-through of the devaluation to prices. As a placebo, we also plot, in gray, the difference in prices between July and May. The distribution of pass-through of the devaluation are very similar for chain and independent stores, suggesting that, when there is no political pressure exerted by the government, chain and independent supermarkets behave very similarly.

In addition, Figure 8b reports the average effect of the depreciation on the prices of goods that were later subject to the VAT cut relative to those that were not. In contrast to the differential response of chain and independent supermarkets to the VAT cut, the figure suggests that supermarkets responded similarly to the currency depreciation shock. Overall, our evidence suggests that pricing strategies that are inherently different for chain and independent supermarkets are unlikely to explain the difference in pass through rates.

7 Conclusion

Our findings have policy implications for the ubiquitous temporary VAT cuts implemented around the world. First, VAT cuts without anti-profiteering measures improve household welfare, while in place, and also increase supermarket profit margins because of their less-than-full pass-through rates to prices. Moreover, they are likely to lead to negative welfare effects once repealed because the pass-through of VAT increases tends to be significantly higher than that of VAT cuts, leading to prices that are higher than their pre-VAT cut levels. Second, anti-profiteering measures lead to symmetric pass-through rates and restore prices to their pre-VAT cut levels once the VAT is repealed. This avoids price hysteresis and its negative (and mostly regressive) welfare effects. Anti-profiteering measures, however, tend to have distributional effects, since they are more easily implementable in supermarket chains where high-income households tend to shop more.

Answering the normative question of whether VAT cuts, in times of high inflation, are a desirable policy is beyond the scope of our paper because it would require estimating the fiscal cost of these policies as well as their effect on supermarket profits and applying appropriate social welfare weights.

Instead, we have estimated the effect of these policies on household welfare, which is often the stated goal of implementing VAT cuts.

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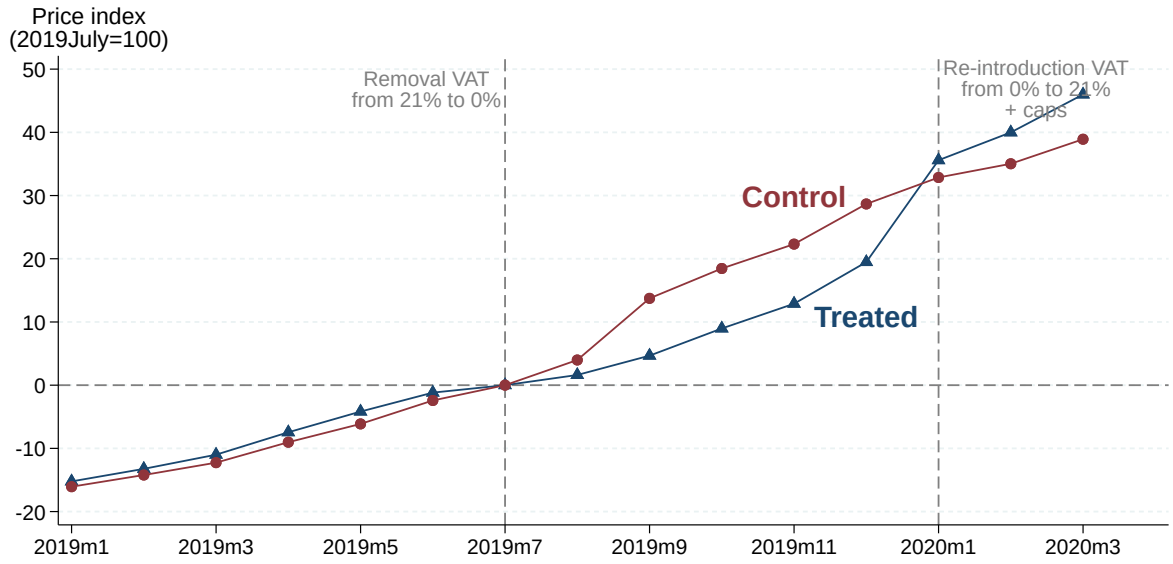
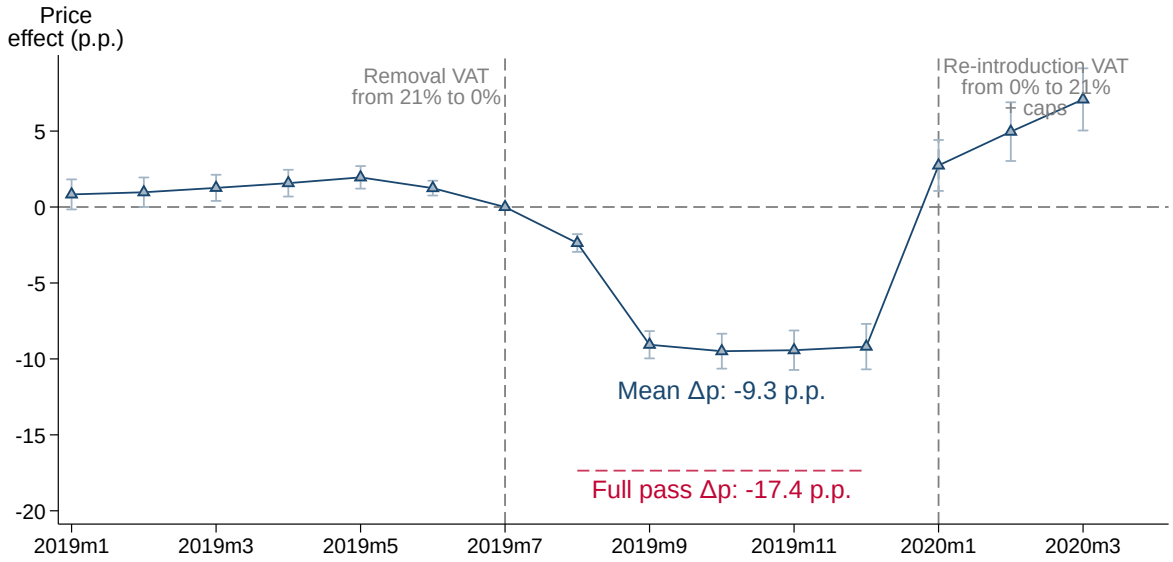
Figures and Tables

Figure 1: Price Effect of the Temporary VAT cut *without* Anti-Profiteering Measures

(a) **Unconditional Means (no caps)**



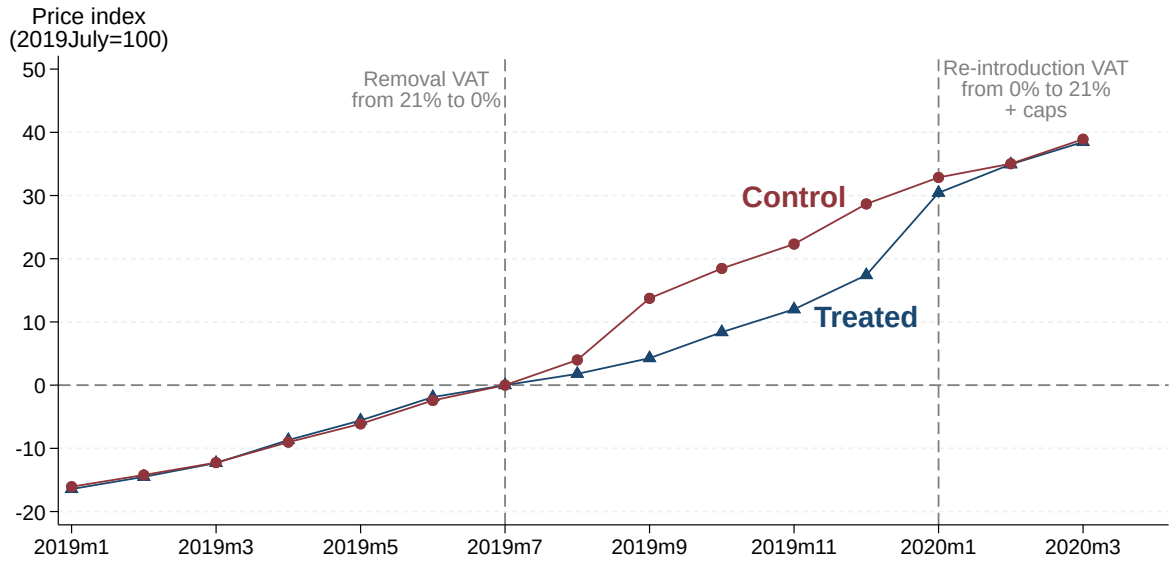
(b) **Difference-in-Differences Estimates (no caps)**



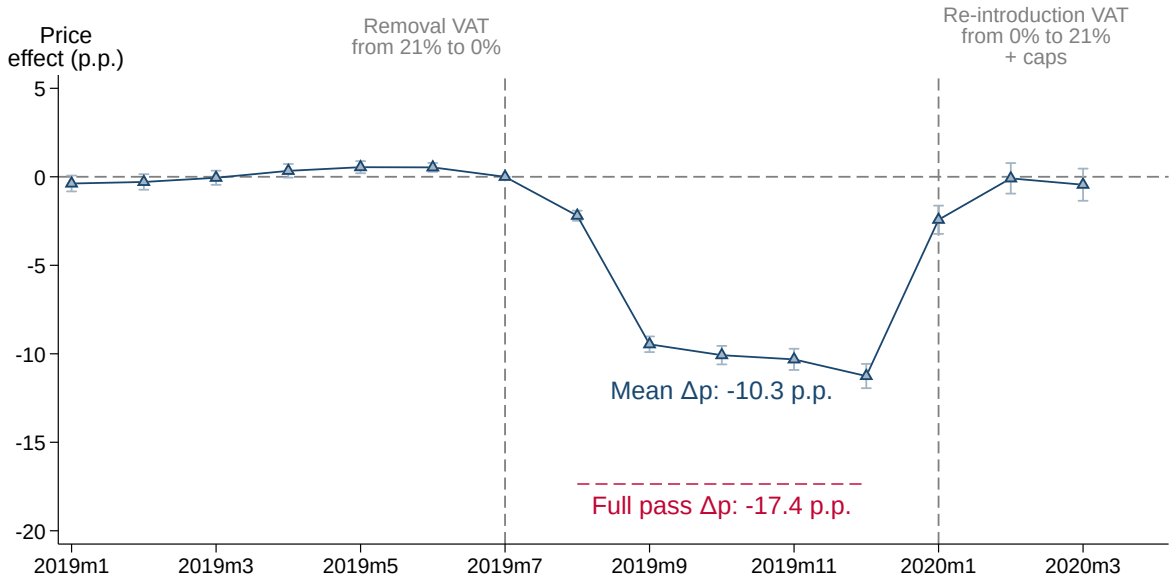
Notes: This figure shows the price pass-through of the VAT holiday for both chain and independent supermarkets using the goods that were not subject to the price caps. We group barcodes into treatment and control as shown in Table 1. The top panel plots the unconditional mean of the average price level for control and treatment food products separately before and after the VAT cut and its subsequent repeal. In each case, we normalize every barcode series to 100 in the month before the VAT cut was implemented (July 2019). The bottom panel shows the results of estimating the dynamic difference-in-differences specification (1). The first vertical dashed line indicates the time when the VAT was decreased to 0% for goods in the treatment group. The second vertical dashed line indicates the time when the VAT was reinstated at 21% for goods in the treatment group with differential caps in the allowed price increase. The red dashed line indicates the hypothetical situation with full pass-through to prices [(1-1.21)/1.21 x 100 = -17.4%].

Figure 2: Price Effects of the Temporary VAT cut *With* Anti-Profiteering Measures

(a) **Unconditional Means (caps)**



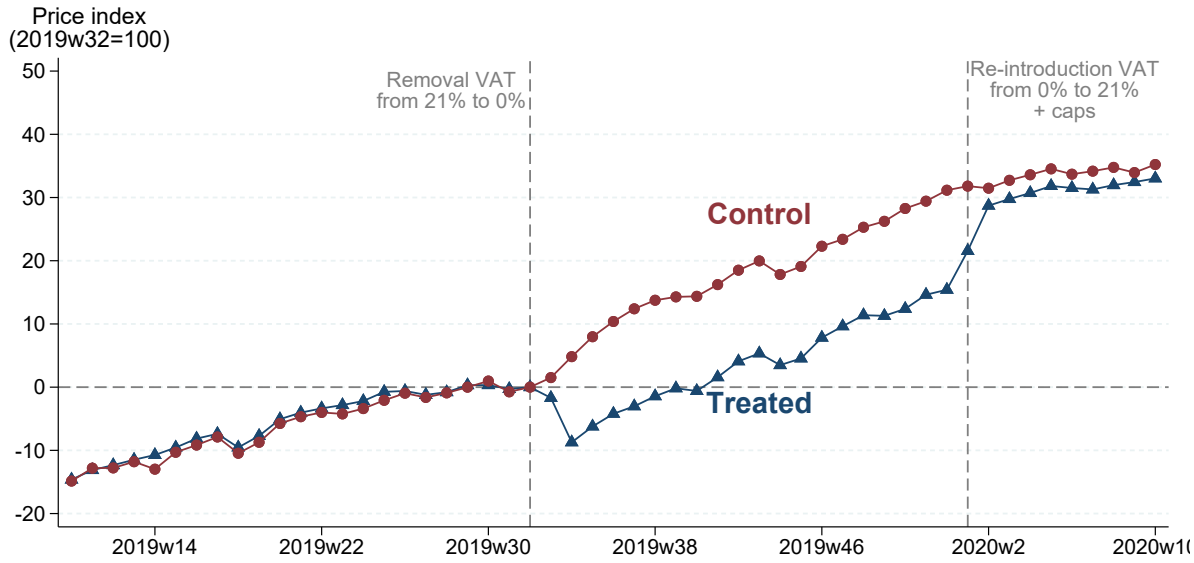
(b) **Difference-in-Differences Estimates (caps)**



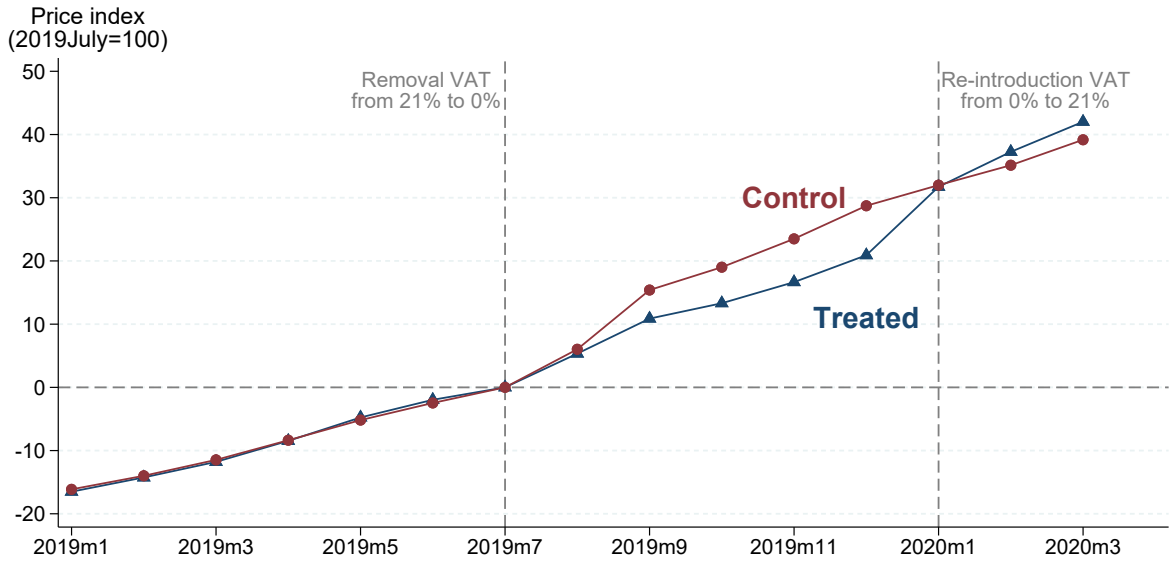
Notes: This figure shows the price levels and price pass-through of the VAT holiday pooling together chain and independent supermarkets. We group barcodes into treatment and control as shown in Table 1. The top panel plots the unconditional mean of the average price level for control and treatment food products separately before and after the VAT cut and its subsequent repeal. In each case, we normalize every barcode series to 100 in the month before the VAT cut was implemented (July 2019). The bottom panel shows the results of estimating the dynamic difference-in-differences specification (1). The first vertical dashed line indicates the time when the VAT was decreased to 0% for goods in the treatment group. The second vertical dashed line indicates the time when the VAT was reinstated at 21% for goods in the treatment group with differential caps in the allowed price increase. The red dashed line indicates the hypothetical situation with full pass-through to prices [(1-1.21)/1.21 x 100 = -17.4%].

Figure 3: Price Effect of the Temporary VAT Cut in Chain and Independent Supermarkets

(a) **Chains**



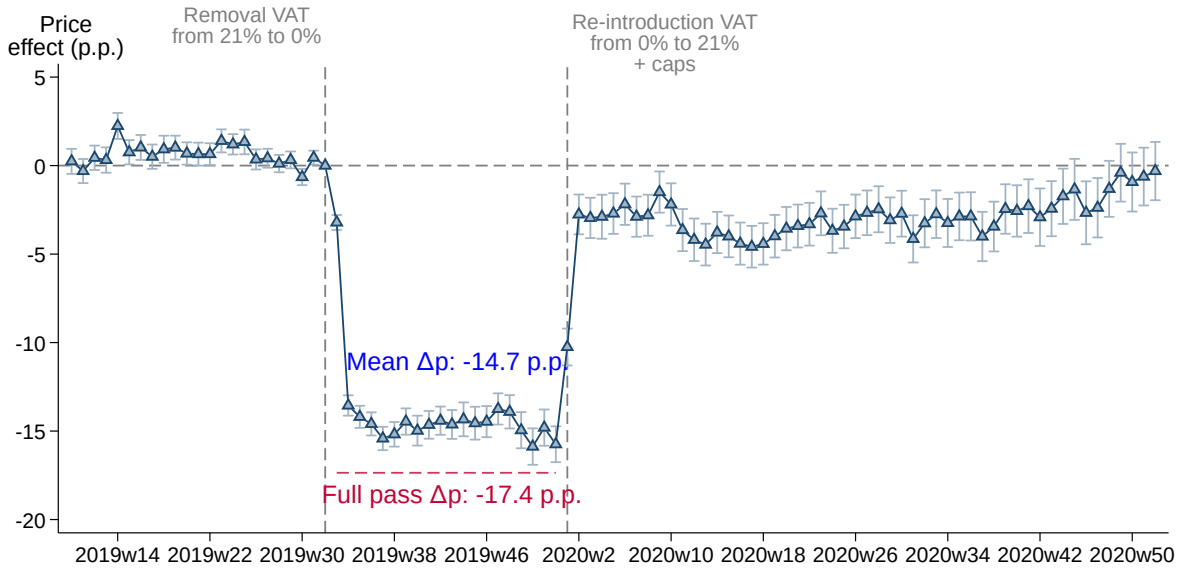
(b) **Independent stores**



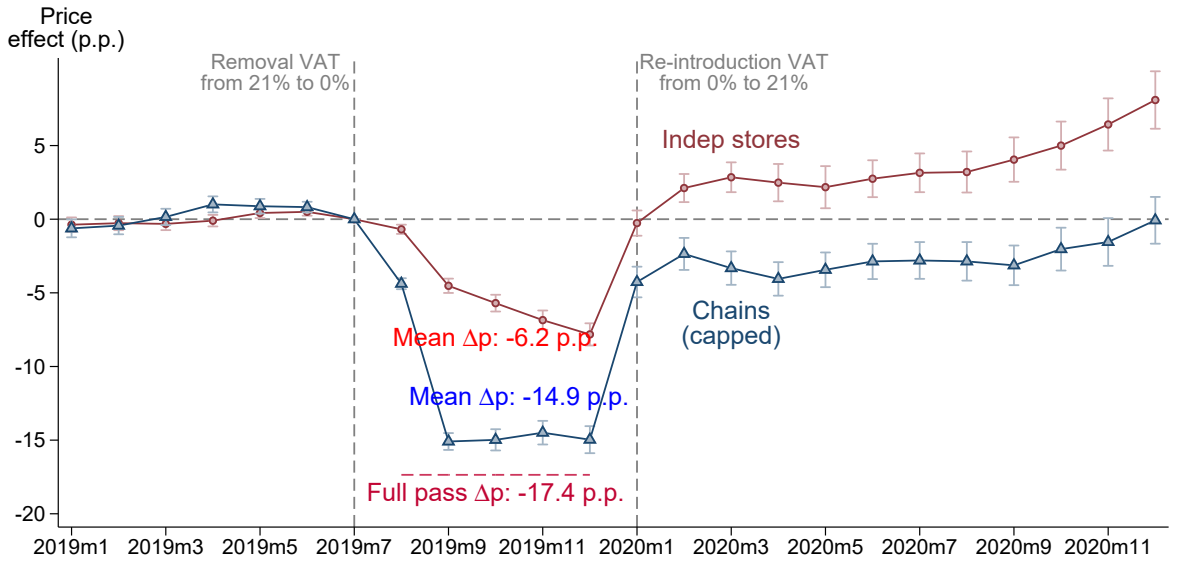
Notes: This figure plots the unconditional mean of the average price level for control and treatment food products separately before and after the VAT cut and its subsequent repeal. In each case, we normalize every barcode series to 100 in the week/month before the VAT cut was implemented. Panel (a) corresponds to supermarket chains and panel (b) shows the series for independent supermarkets with retail scanner data collected at the monthly level.

Figure 4: Price Effect of the Temporary VAT Cut in Chain and Independent Supermarkets

(a) **Chains (weekly data)**



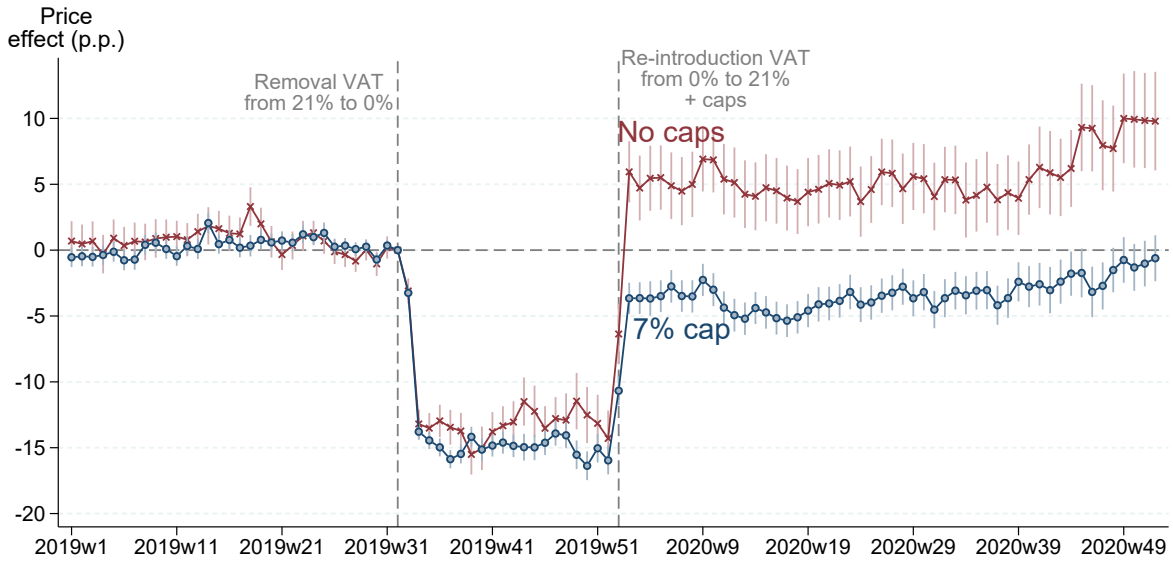
(b) **Chains and Independent stores (monthly data)**



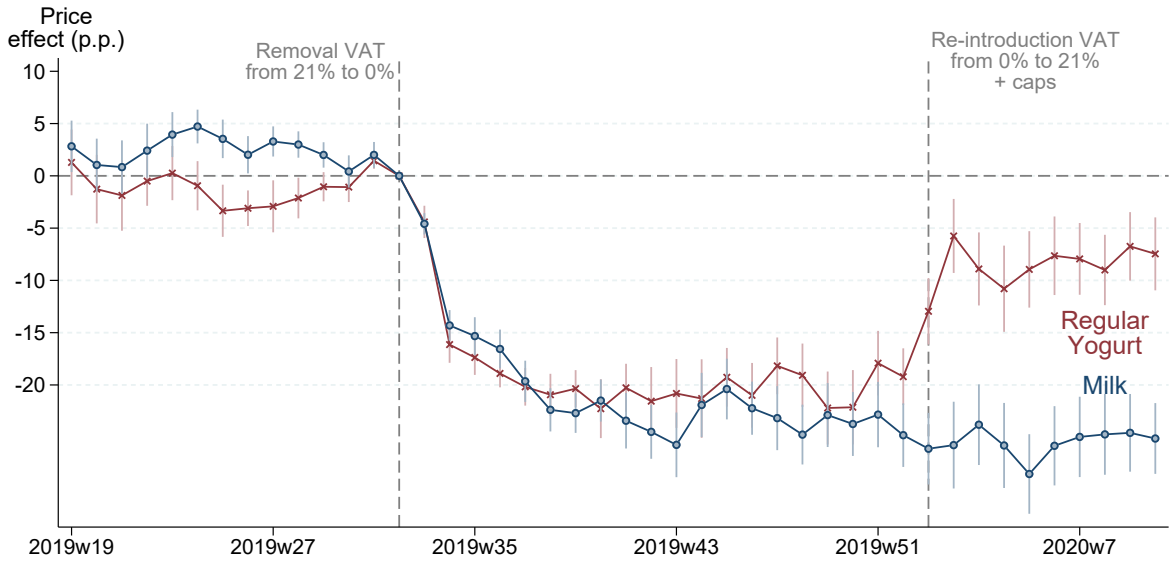
Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) before and after the VAT cut and its subsequent repeal. We group barcodes into treatment and control as shown in Table 1. The dependent variable is the price of each barcode normalized to 100 in the week or month before the VAT was cut. Panel (a) shows the pass-through rate for chains where we use weekly data. Panel (b) does this for independent supermarkets where we use monthly data. For comparison, in Panel (b) we also add the effect for supermarket chains where we collapse the weekly data at the month level. The first vertical dashed line indicates the time when the VAT was decreased to 0% for goods in the treatment group. The second vertical dashed line indicates the time when the VAT was reinstated at 21% for goods in the treatment group with differential caps in the allowed price increase. The red dashed line indicates the hypothetical situation with full pass-through to prices [(1-1.21)/1.21 x 100 = -17.4%].

Figure 5: Price Effect of the Anti-Profiteering Measures in Chain Supermarkets

(a) **7% cap versus no cap**

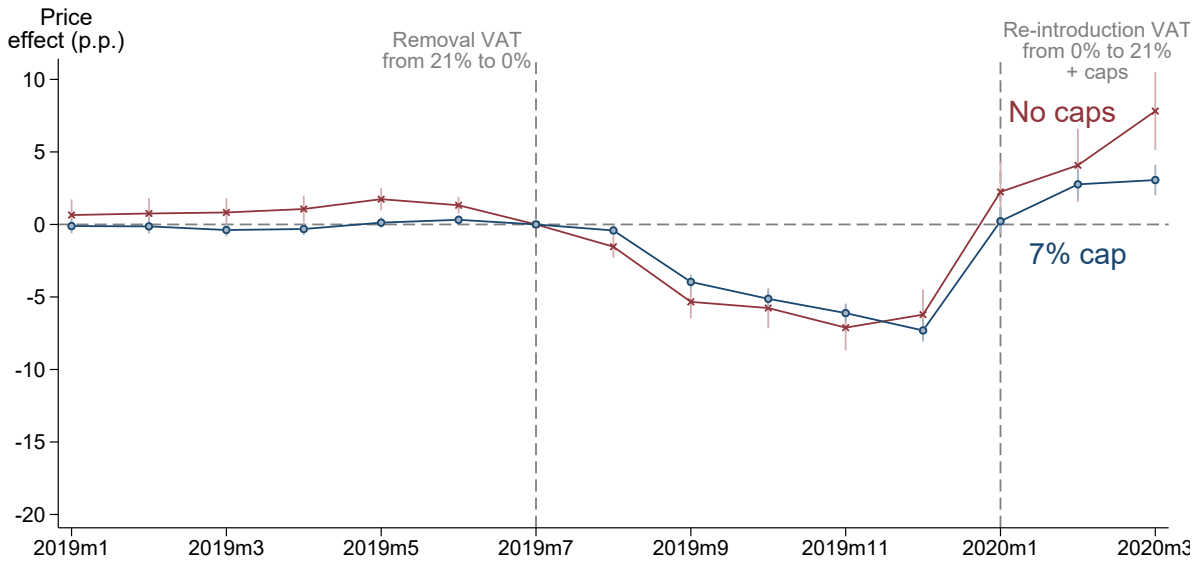


(b) **0% cap (milk) versus 7% cap (yogurt)**



Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) in supermarket chains before and after the VAT cut and its subsequent repeal. We break down the list of barcodes from the treatment group into food categories that are subject to a capped price increase and food categories with no cap in their price increase (i.e., allowed to flexibly increase prices). We compare each group relative to food products in the original control group. For a list of the different caps across categories see Table 2. The dependent variable is the price of each barcode normalized to 100 in the week before the VAT was cut. Panel (a) compares the change in prices for those commodities that are subject to the 7% price increase cap and those that are fully flexible (relative to the original control group). Panel (b) compares the change in prices for milk products which were not allowed to increase prices at all relative to goods in the original control group. For comparison, in Panel (b) we also add the effect for regular yogurt who faced the 7% price increase cap. The first vertical dashed line indicates the time when the VAT was decreased to 0% for goods in the treatment group. The second vertical dashed line indicates the time when the VAT was reinstated at 21% for goods in the treatment group with differential caps in the allowed price increase.

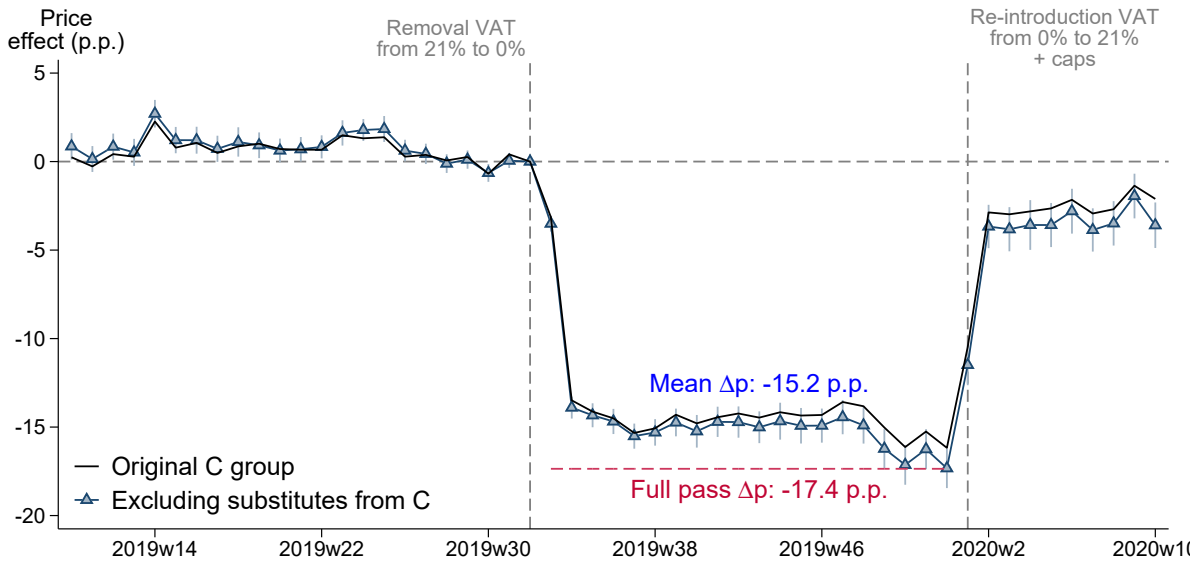
Figure 6: Do the Anti-Profiteering Measures Affect Prices at Independent Supermarkets?



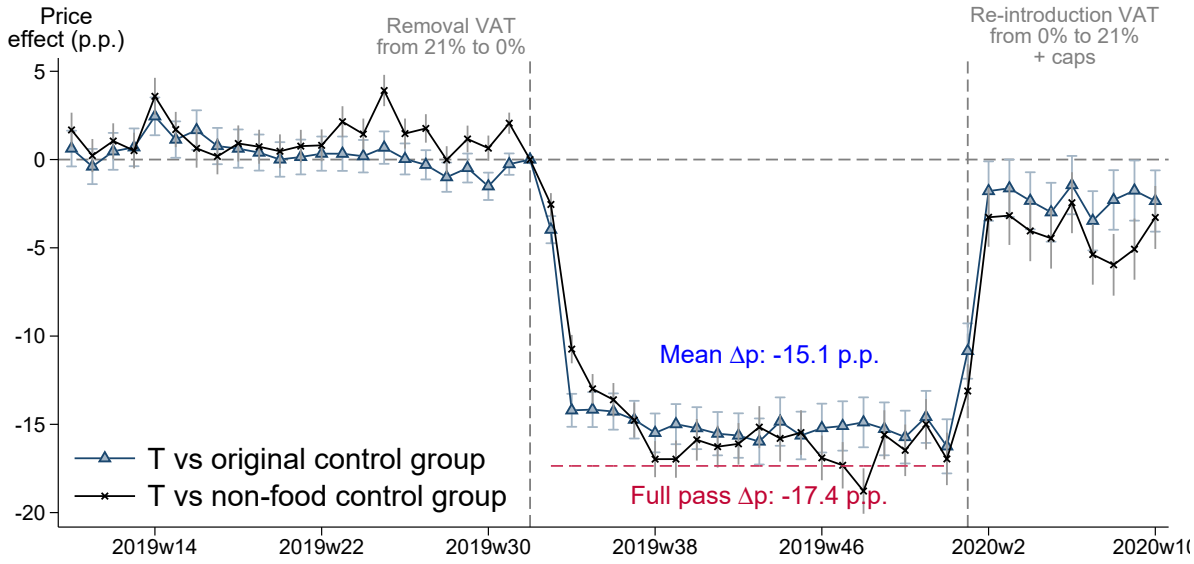
Notes: This figure shows that the prices of zero-rated goods with and without caps respond similarly in independent supermarkets when the VAT was reinstated to 21%. Unlike supermarket chains, the government did not impose differential caps in independent stores. The figure displays the results of our dynamic difference-in-differences specification (1). We followed the same strategy as in Figure 5.

Figure 7: Does substitution across food products bias our price effects?

(a) **Including and excluding close substitutes in the control group**



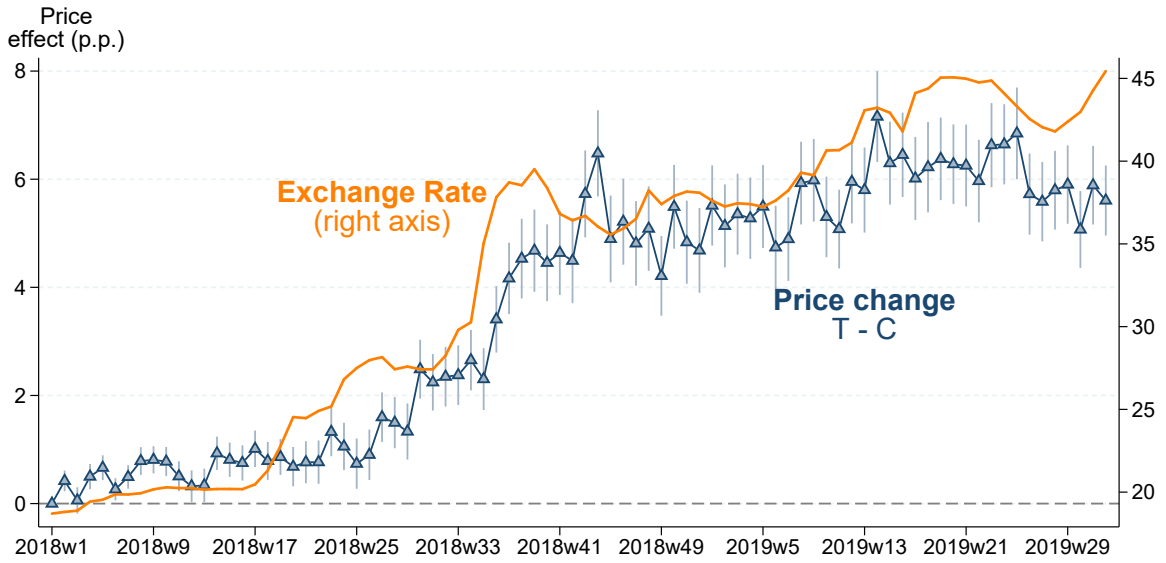
(b) **Using food and non-food products in the control group (region Periferia)**



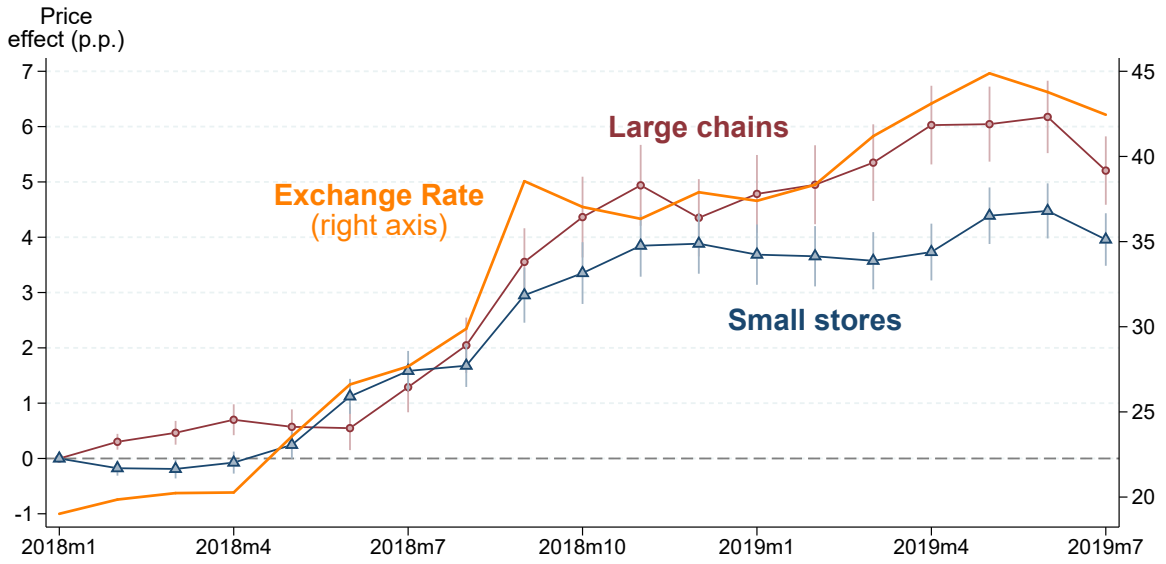
Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) on prices. Panel (a): The black line corresponds to the estimation using the original treatment and control groups as shown in Table 1. The blue line uses the same treatment group and an alternative control that excludes close substitutes (cooking oil, rice, coffee, dried legumes, flour derivatives, soup and prepared pasta). Panel (b): The blue line corresponds to the estimation using the original treatment and control groups. The black line uses the same treatment group and an alternative control group comprised of non-food categories (office supplies, body moisturizers, antiperspirants, hand soap, laundry detergent, bleach, surface cleaners, toilet paper, shampoo, and cleaning wipes). The bottom figure is constructed using scanner data from the region Periferia because non-food categories were only purchased for that region. The red dashed line indicates the hypothetical situation with full pass-through to prices [(1-1.21)/1.21 x 100 = -17.4%]. In all, both figures suggest that substitution is not a big concern in our setting.

Figure 8: Pass-through of the 2018 peso depreciation

(a) **Prices of treatment and control in chains**

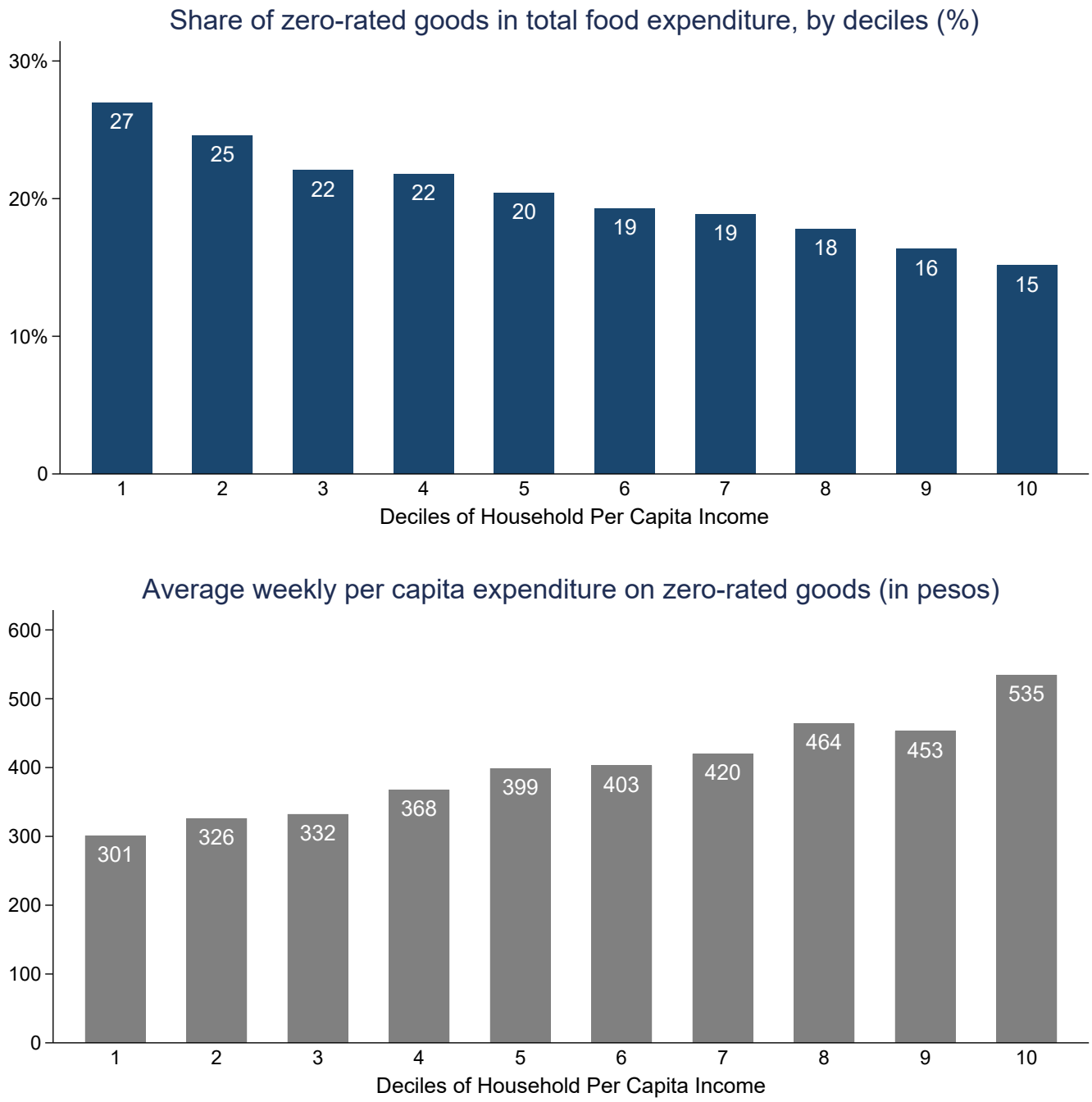


(b) **Prices of treatment and control in independent and chain stores**



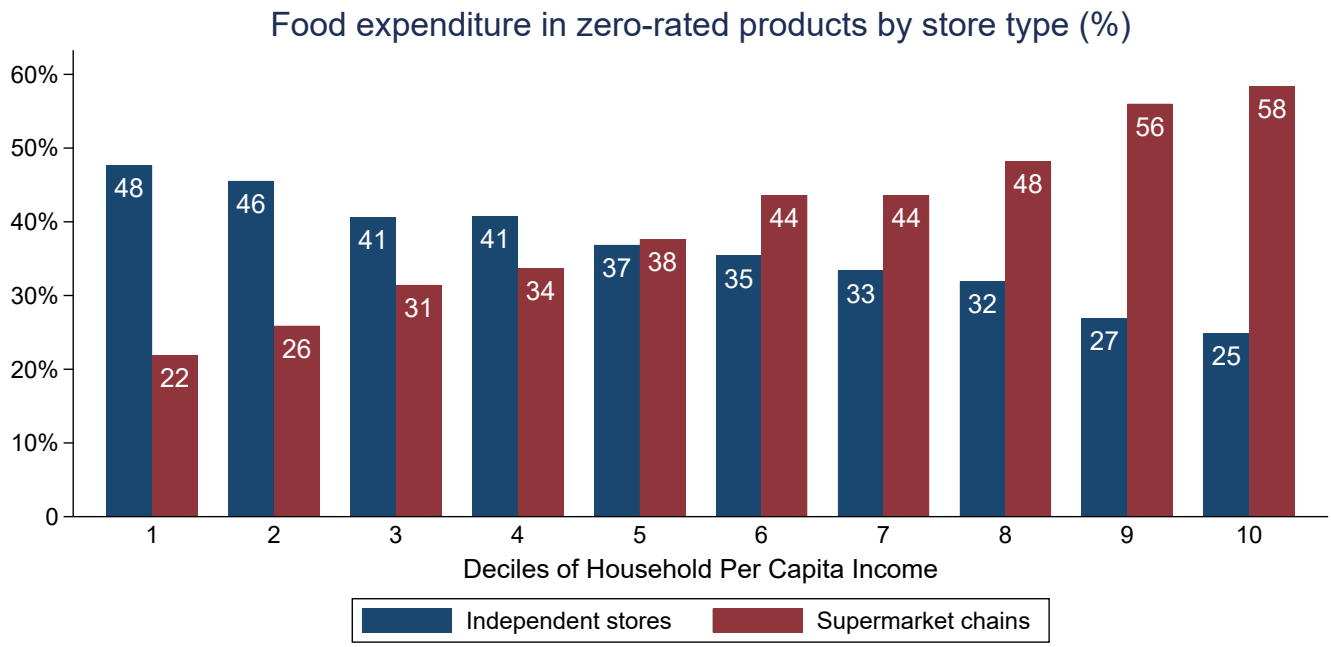
Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) on the prices of chain and independent supermarkets. The orange line displays the nominal exchange rate between the Argentine peso and the US dollar (right axis). The blue line in the top panel shows the percentage change in prices relative to week 1 of 2018 between treated and control goods as classified in Table 1. The bottom panel runs the same regression using monthly data in supermarkets chains (red line) and independent supermarkets (blue line). In Section 5.6 we explain that the effect of the depreciation does not pose a threat to our subsequent findings of the VAT holiday.

Figure 9: Participation of treated products in total food expenditure and weekly expenditure



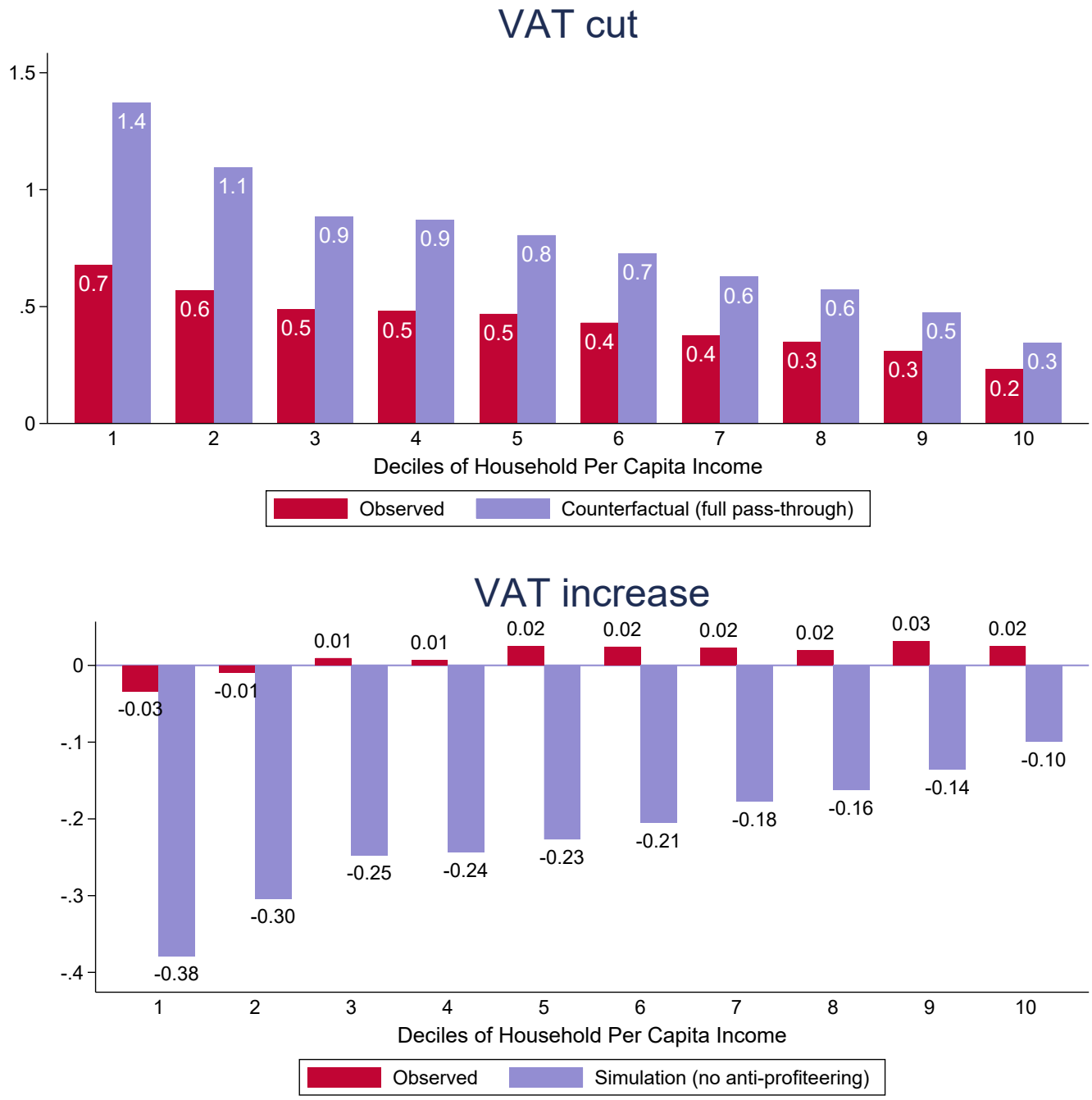
Notes: The top panel displays the share of zero-rated goods in total food expenditure. The national average is 20%. The bottom panel shows the average household per capita expenditure on zero-rated goods (in pesos) for the reference week of the survey.
Source: authors' calculations using the 2017/2018 National Household Expenditure Survey (ENGHo).

Figure 10: Where do the poor and the rich shop for groceries?



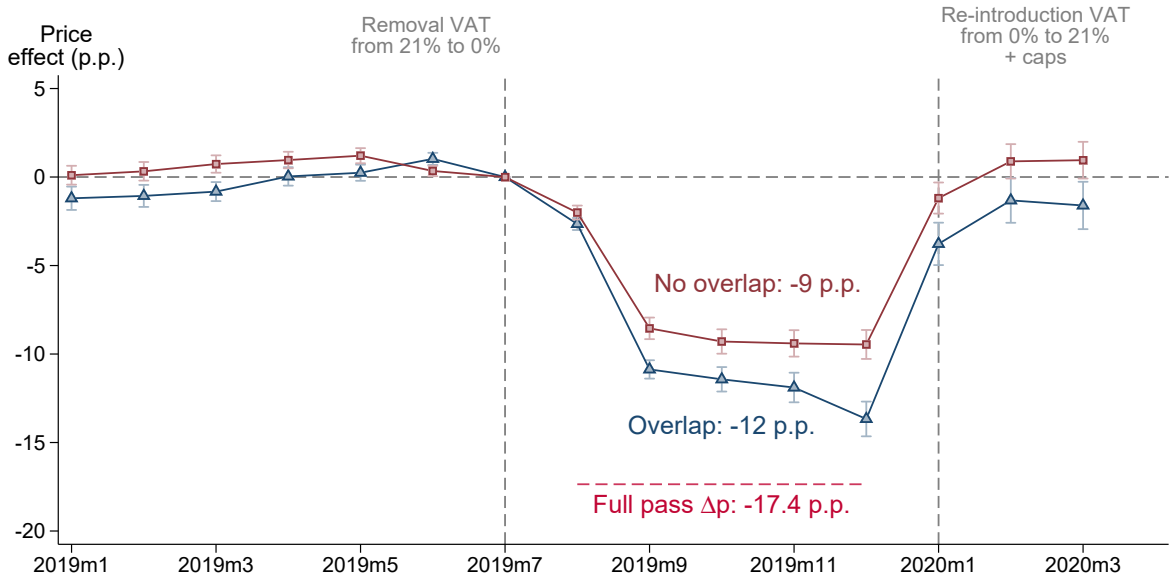
Notes: This figure shows the food expenditure share of zero-rated goods by type of store and across deciles of household per capita income. The blue bars display the expenditure in treated goods in independent stores. The red bars correspond to chain supermarkets.
Source: authors' calculations using the 2017/2018 National Household Expenditure Survey (ENGHo).

Figure 11: Welfare Estimates



Notes: These Figures show the results of estimating our welfare model (section 5) for the VAT decrease in panel (a) and the VAT decrease in panel (b). In addition to estimating the welfare of the policies as implemented, in panel (a) we also include the welfare for a counterfactual full-pass through scenario; and in panel (b), we estimate the effect of removing the anti-profiteering measures.

Figure 12: Price Levels for Barcodes Sold in Both Chain and Independent Supermarkets (overlap) Compared to Barcodes sold in One or the Other (no overlap)



Notes: This figure pools chain and independent supermarkets and breaks down pass-through rate estimates for goods at the barcode level that are sold in both types of supermarkets versus goods that are sold in either one of them but not both. Presumably, goods that are sold in both supermarket and independent chains will be more competitive, leading to higher pass-through rates of the VAT cut.

Table 1: Classification of data into treatment and control

Treatment Temporary 0% VAT	**Control** Standard 21% VAT
Categories	**Categories**
Cooking oils (sunflower, corn, mix)	Other cooking oils (olive, soy, canola)
Rice	Rice-based meals
Dried pasta	Breakfast cereal
Tea, Yerba Mate, and Mate Cocido	Coffee
Sugar	Salt
Canned vegetables and beans	Herbs, Spices, & Seasonings
Canned fruits	Dulce de leche (caramel)
Corn flour (*polenta*)	Jam and Jelly
Wheat flour	Other flours
Fluid milk (whole/skim)	Crackers, Biscuits, Toasts, Puddings
Yogurt (whole or skim)	Chocolate
Eggs	Mayonnaise
Bread	Vinegar
Breadcrumbs and/or batter	Dried legumes and beans

Notes: This table shows the split of our data into treatment and control categories. Wheat flour and Bread are taxed at the reduced rate of 10.5%. Source: Treatment categories are determined based on Decree 567/2019–Annex. Control products include the remaining categories in our data.

Table 2: Regulated VAT increase with capped pass-through rates

Treated: VAT back to 21%

Categories	Δp cap
Oil (sunflower & mix)	9%
Oil (corn)	No cap
Rice (regular: long grain white)	7%
Rice (other: basmati, brown, organic)	No cap
Dried pasta	7%
Tea, Yerba Mate, and Mate Cocido	7%
Sugar	7%
Canned vegetables and beans	7%
Canned fruits	No cap
Corn flour	7%
Wheat flour	7%
Fluid milk (whole/skim)	0%
Yogurt (regular)	7%
Yogurt (other: w/cereal, fruit chunks)	No cap
Eggs	7%
Sliced Bread (white)	7%
Sliced Bread (rest)	No cap
Breadcrumbs and/or batter	10.5%

Notes: This table shows the list of treated products with differential caps when the VAT was reintroduced. Although the VAT rate went effectively back to the pre-holiday level of 21%, the new administration limited the price increase with different price caps. This mandate was enforced with the price monitoring app. "No cap" flags the uncapped food products with flexible prices.

Table 3: Summary Statistics at Baseline (July 2019)

		Stores				
		All stores	Chain stores	Independent stores		
Treated	All goods	3,562	1,934	2,563		
	No caps	471	265	343		
	Caps	3,091	1,669	2,220		
	No overlap	3,307	1,444	1,973		
	Overlap	915	915	915		
			Price difference		Barcodes	Observations
			-0.029***		915	5,696
			(0.005)			
Control	All goods	4,386	2,736	3,008		
	No overlap	3,875	2,083			
	Overlap	1,243	1,243			
			Price difference		Barcodes	Observations
			-0.043***		1,243	7,492
			(0.005)			
All goods	All goods	8,115	4,768	5,687		
	No overlap	7,182	3,527	3,934		
	Overlap	2,158	2,158	2,158		
			Price difference		Barcodes	Observations
			-0.037***		2,158	13,188
			(0.003)			

Notes: This table presents the *unique* number of barcodes (note that a given barcode may appear multiple times as it can be purchased in different store types s and regions r). Also, a certain barcode can belong to the overlap group in one region e.g., GBA, and to the no-overlap in another e.g., Cordoba. For the price difference results, we show the β coefficient of the following regression: $log\ P_{i,s,r} = \alpha_{i,r} + \beta\ Independent\ Stores_s + \epsilon_{i,s,r}$, where $log\ P_{i,s,r}$ is the log of the observed price of barcode i, in store type s in region r the month before the reform (t_{-1}). α are barcode-region fixed effects while *Indepedent Stores* is a dummy that identifies those store types. Note that we run this specification the month prior to the reform for those overlapping goods i.e., barcodes that are sold in chain and independent stores at the same time in a given region. As opposed to most of the regressions, in this one we use the log of the *observed* price instead of the *normalized* to the pre-reform one. Standard errors clustered at barcode level in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% level.

Table 4: Price Effects of the Temporary VAT Cut

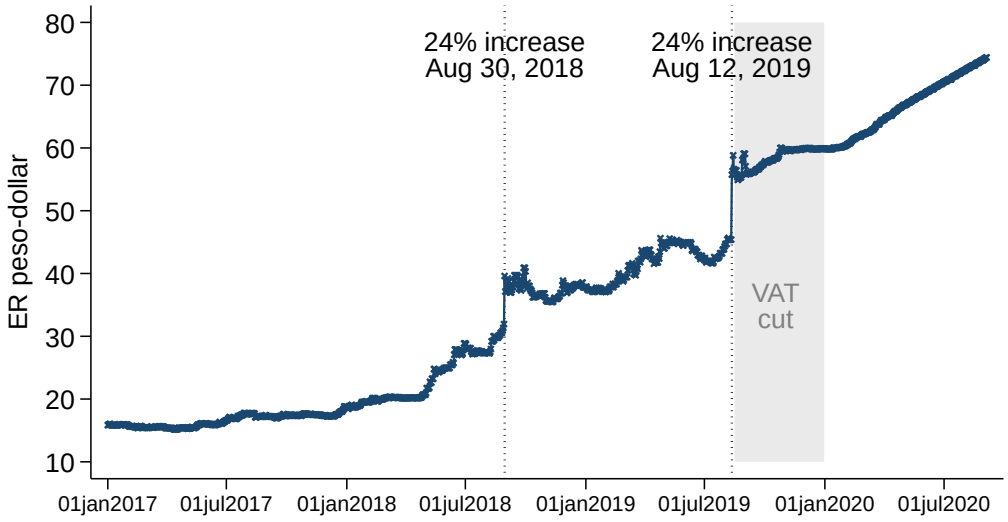
Regions	Frequency	Stores	Goods	Estimates		Number of	
				Removal	Re-intro	Barcodes	Observations
All	Monthly	All stores	T: all goods	-10.462***	-0.161	8,115	632,848
				(0.249)	(0.425)		
			T: no caps	-9.301***	5.991***	4,857	397,155
				(0.550)	(0.995)		
			T: caps	-10.278***	-0.267	7,477	584,743
				(0.256)	(0.443)		
All	Weekly	Chain stores	T: all goods	-14.656***	-2.543***	4,645	2,515,923
				(0.339)	(0.548)		
			T: no caps	-13.260***	5.529***	2,878	1,624,588
				(0.660)	(1.187)		
			T: caps	-14.926***	-3.275***	4,282	2,336,480
				(0.356)	(0.577)		
All	Monthly	Chain stores	T: all goods	-14.883***	-2.839***	4,768	338,264
				(0.340)	(0.553)		
			T: no caps	-12.929***	6.050***	3,001	219,853
				(0.709)	(1.264)		
			T: caps	-15.098***	-3.481***	4,405	314,431
				(0.354)	(0.574)		
All	Monthly	Independent stores	T: all goods	-6.222***	2.481***	5,687	294,584
				(0.281)	(0.488)		
			T: no caps	-6.135***	5.886***	3,351	177,302
				(0.671)	(1.288)		
			T: caps	-5.622***	2.923***	5,228	270,312
				(0.283)	(0.505)		
All	Weekly	Chain stores	C: exclude close substitutes	-14.608***	-2.506***	4,541	2,462,247
				(0.344)	(0.554)		
Periferia			C: non-food only	-15.670***	-4.121***	3,407	337,255
				(0.485)	(0.806)		
All			T & C: exclude imported goods	-14.629***	-2.786***	4,153	2,270,015
				(0.356)	(0.579)		
All	Monthly	All stores	T & C: no overlap barcodes	-9.175***	0.920*	7,182	336,648
				(0.327)	(0.498)		
			T & C: overlap barcodes	-11.964***	-1.462**	2,158	296,200
				(0.350)	(0.655)		

Notes: This table presents the point estimates of the pass through. In particular, the specification pools the individual coefficients identified by the original equation (1) in the following way: $P_{i,s,r,t} = \alpha_{i,s,r} + \gamma_t + \delta\ W_t \cdot Treat_{i,s,r} + \beta\ W_i \cdot Post_i \cdot Treat_{i,s,r} + \epsilon_{it}$, where $P_{i,s,r,t}$ refers to the price of barcode i in store type s, in region r in a certain week/month-year denoted by t. α and γ are barcode and time fixed effects respectively. $Treat$ equals one for those goods affected by the VAT change while $Post$ identifies the period after the reform. $Window$ is a dummy that equals one for the time horizon comprised between the week/month prior to the reform and up to four months after its implementation (excluding the immediate week/month post reform). The table presents the β coefficient which measures the change in prices relative to the pre-reform period. T and C refer to treated and control goods respectively. The values that appear in the seventh column, stand for *unique* barcodes (note that a given barcode may appear multiple times as it can be purchased in different store types s and regions r. Standard errors clustered at barcode level in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% level.

Supplementary Materials for: "Can VAT Cuts and Anti-Profiteering Measures Dampen the Effects of Food Price Inflation?"

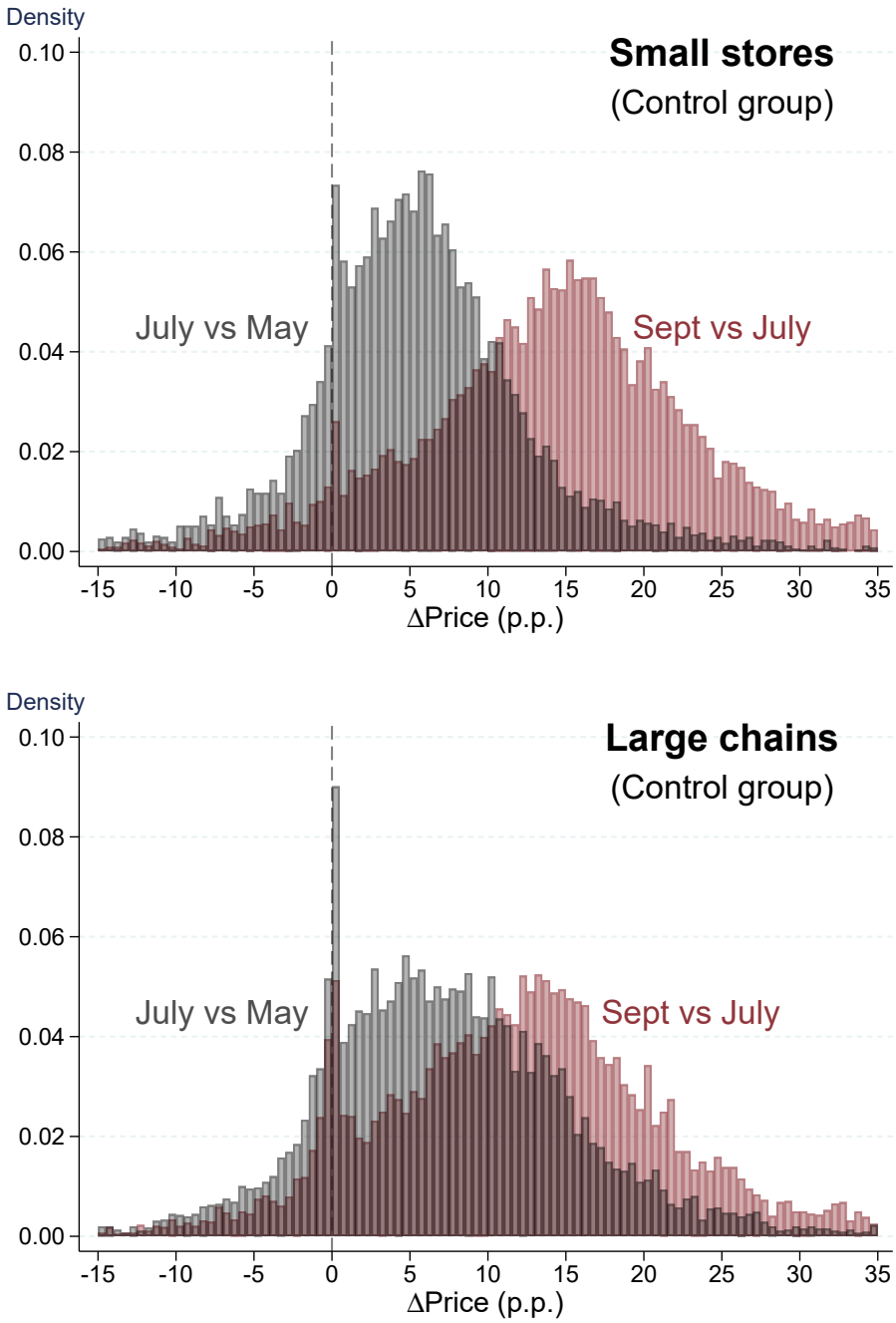
A Additional figures and tables

Figure A.1: Exchange rate (pesos per dollar)



Source: BCRA, Tipo de Cambio de Referencia - Comunicación "A" 3500 (Mayorista).

Figure A.2: Distribution of price changes in independent and chain supermarkets (Depreciation Episode)



Notes: This figure shows the distribution of price changes in independent supermarkets (top panel) and supermarket chains (bottom panel) for barcodes in the control group. The gray area displays the difference in prices between July and May 2019 *before* the peso depreciation. The red area displays the difference in prices between September and July 2019 *after* the peso depreciation. The figure shows that the prices of goods unaffected by the VAT cut respond similarly in chain and independent supermarkets to other types of macro shocks (the depreciation, in this case).

Figure A.3: Media coverage of the VAT cut and subsequent hike

(a) **Media coverage of the VAT cut**

Clarín

Viernes 16.8.2019

Spot
En la piel de Carlos Tevez

Balthazar Murillo, un chico de 15 años, interpreta al crack de Boca en la serie "Apache", que se estrena hoy.

Soldano, la nueva apuesta de gol en Boca

"Mi desafío más grande", afirma. Debutá el domingo.

Tema del día • Nuevas medidas económicas

Eliminan IVA en alimentos, congelan cuotas UVA y habría cambios de Gabinete

Del Editor
Ricardo Kirschbaum

El diálogo y un atisbo de tregua

Para Alberto F. el dólar a $ 60 está bien y no hay razón para que aumente

Diosdado Cabello: "Que no vaya a creer que lo están eligiendo porque es él"

Todos juntos. En el CCK, con Vidal, Larreta, Carrió, Pichetto y el resto de la "tropa", Macri relanzó la campaña.

En busca de la mística perdida

Nuevo ataque del hacker que filtró datos de la Federal

En espera. Promueve acuerdos básicos.

Lavagna suspendió la campaña y pidió que lo imiten

TEVEZ LLEGA A NETFLIX
SE ESTRENA HOY APACHE, VIDA Y OBRA DE CARLITOS

VILLA LA ANGOSTURA
TRABAJAN CONTRA RELOJ PARA REABRIR LA RUTA 40

EMERGENCIA AÉREA
UN AVIÓN ATERRIZÓ EN RUSIA SIN RUEDAS NI MOTORES

LA NACION

Viernes 16 de agosto de 2019

Macri eliminó el IVA de 14 alimentos y congeló las cuotas de los créditos UVA

Dólar: cayó por primera vez desde las PASO

Influyó una medida del Central, para Fernández, $60 es un precio lógico

Presión militar china a Hong Kong

Permitirán estacionar junto a algunas bicisendas

Filtraron archivos de la Federal sobre Chocobar

Además

Ejecutaron a tres hombres en un búnker de drogas

Devaluación. Cómo se protegen los clubes

(b) **Media coverage of the VAT increase**

Notes: These pictures show the media coverage of the VAT removal (panel a) and VAT reintroduction (panel b) in the two main newspapers of Argentina. The left panels correspond to "Clarin" newspaper and the right panels to "La Nacion" newspaper. In both newspapers, the main news of the day discusses the VAT cut (panel a) and the regulated VAT reintroduction with capped price increases (panel b).

Figure A.4: Salience of the VAT holiday

Notes: These pictures illustrate the salience of the VAT holiday in supermarkets. The top left panel shows a banner displayed at the entrance of a store informing the 13 products that now face a temporary 0% VAT rate. The bottom left panel shows a large banner inside a store informing that more than 1,900 products (within the 13 treated categories) now face a temporary 0% VAT rate. The two right panels show mandatory tags that supermarkets had to display next to treated products.

Figure A.5: Salience of the monitoring app "Precios Claros"

Notes: These pictures illustrate the salience of the monitoring app "Precios Claros" launched by the government in 2016. The top left panel shows the front page of one of the main newspapers in Argentina informing that the government launched a monitoring system for consumers to control prices in supermarkets. The bottom left panel shows the official webpage where consumers can consult any price in any store of Argentina. The bottom right panel shows an example of how the query looks like. The top right panel shows that the same information can be accessed through an app.

Figure A.6: Geographic variables in the data

(a) **Supermarket chains**

GBA	CAPITAL FEDERAL		Capital Federal
	PERIFERIA		Suburbio Norte, Suburbio Sur, Suburbio Oeste
INTERIOR	BS. AS. RESTO		Pcia Bs As NO incluídas en la periferia
	CORDOBA		Pcia Córdoba
	ANDINA	CUYO	Pcias Mendoza, San Juan, San Luis
		NOA	Pcias Tucumán, Catamarca, Jujuy, La Rioja, Salta, Santiago del Estero
	LITORAL	LIT NORTE	Pcias Corrientes, Chaco, Formosa, Misiones
		LIT SUR	Pcia Santa Fe y Entre Ríos
	SUR		Pcias La Pampa, Neuquen, Río Negro
	AUSTRAL		Pcias Chubut, Santa Cruz, Tierra del Fuego

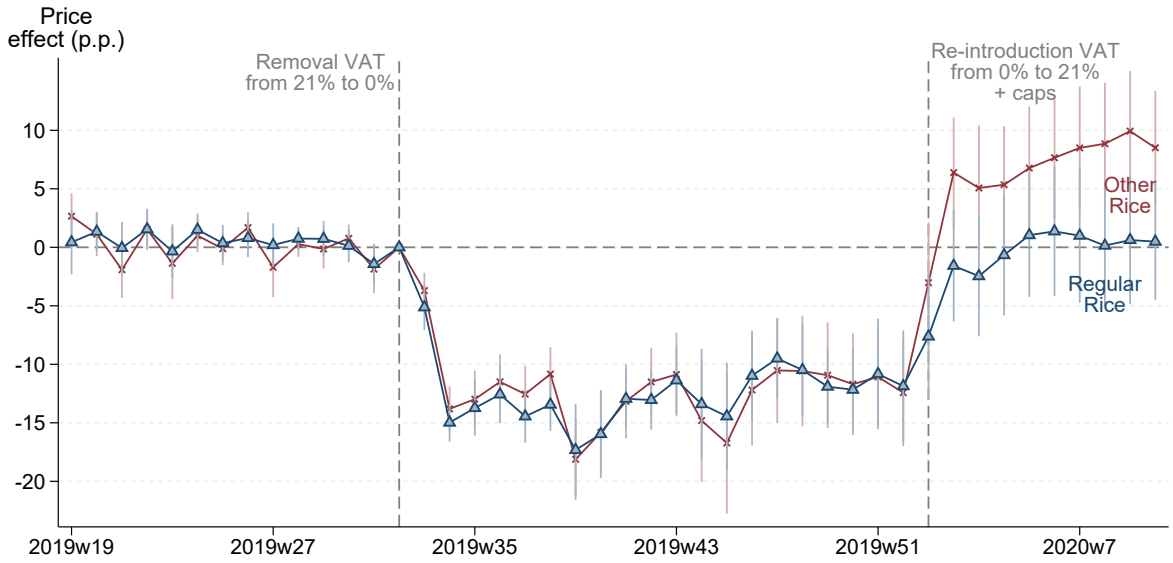
(b) **Independent supermarkets**

GBA	GBA	Capital Federal, Suburbio Norte, Suburbio Sur, Suburbio Oeste
INTERIOR	BS. AS. RESTO + SUR	Pcia Bs As NO incluídas en la periferia + Pcias La Pampa, Neuquen, Río Negro, Chubut, Santa Cruz, Tierra del Fuego
	CORDOBA	Pcia Córdoba
	ANDINA	Pcias Mendoza, San Juan, San Luis, Tucumán, Catamarca, Jujuy, La Rioja, Salta, Santiago del Estero
	LITORAL	Pcias Corrientes, Chaco, Formosa, Misiones, Santa Fe y Entre Ríos

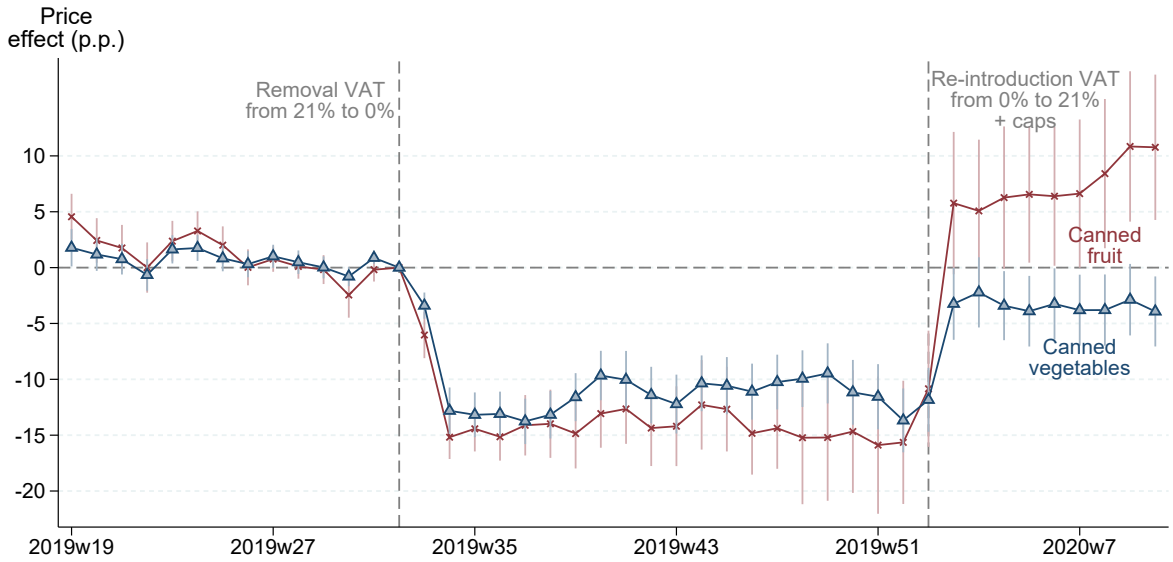
Notes: This figure shows the structure of our geographic variables in our databases. Overall, stores can be located in Gran Buenos Aires (GBA) or the rest of the country (Interior). Within GBA, they can be in the capital of Argentina (Capital Federal) or the rest of GBA area (Periferia). The Interior of the country is classified into: the rest of the province of Buenos Aires (BS AS Resto), Cordoba, Andina region (further split into Cuyo and Northwest NOA), Litoral region (north and south), South, and Austral.

Figure A.7: Regulated VAT increase with capped pass-through rates

(a) **7% cap (regular rice) versus no cap (other rice)**

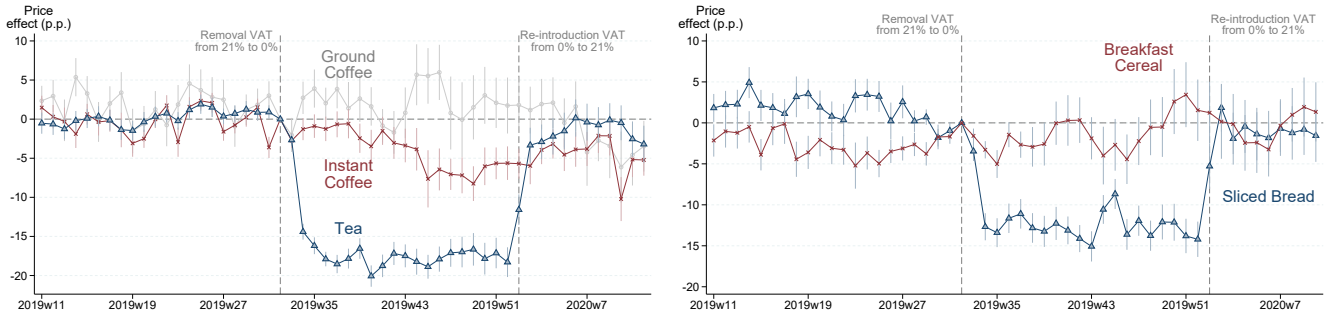


(b) **7% cap (canned vegetables) versus no cap (canned fruit)**



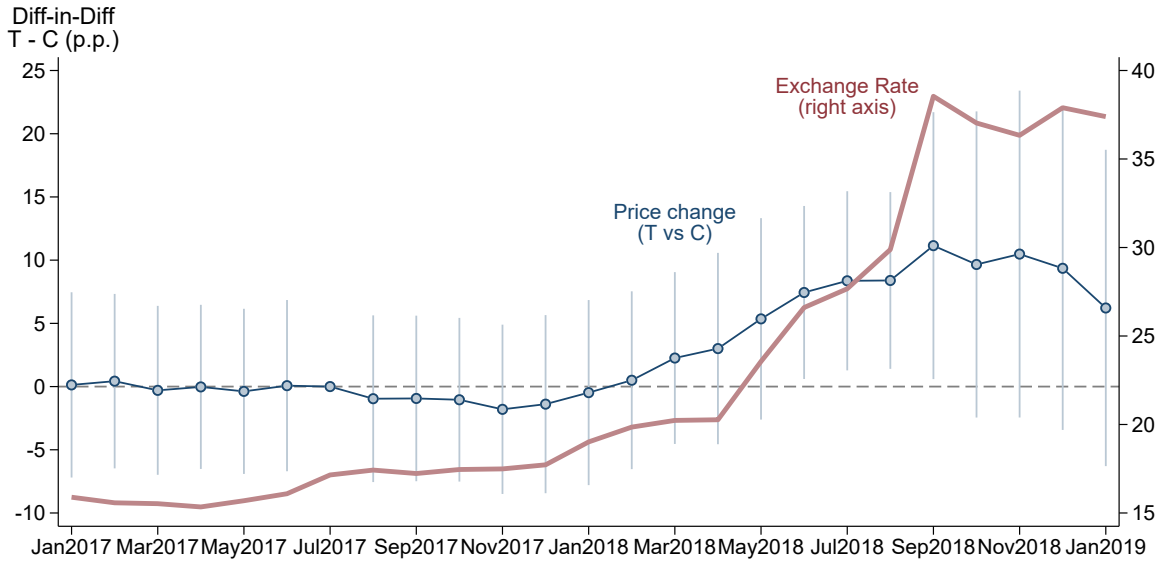
Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) in chains before and after the VAT cut and its subsequent repeal. We break down the list of barcodes from the treatment group into food categories that are subject to a capped price increase and food categories with no cap in their price increase (i.e., green light to flexibly increase prices). We compare each group relative to food products in the original control group. For a list of the different caps across categories see Table 2. The dependent variable is the price of each barcode normalized to 100 in the week before the VAT was cut. Panel (a) compares the change in prices for regular rice products subject to the 7% price increase cap and other rice products that are fully flexible (relative to the original control group). Panel (b) compares the change in prices for canned vegetables subject to the 7% price increase cap and canned fruit that are fully flexible (relative to the original control group). The first vertical dashed line indicates the time when the VAT was decreased to 0% for goods in the treatment group. The second vertical dashed line indicates the time when the VAT was reinstated at 21% for goods in the treatment group with differential caps in the allowed price increase.

Figure A.8: The extent of substitutability in the control group (case studies)



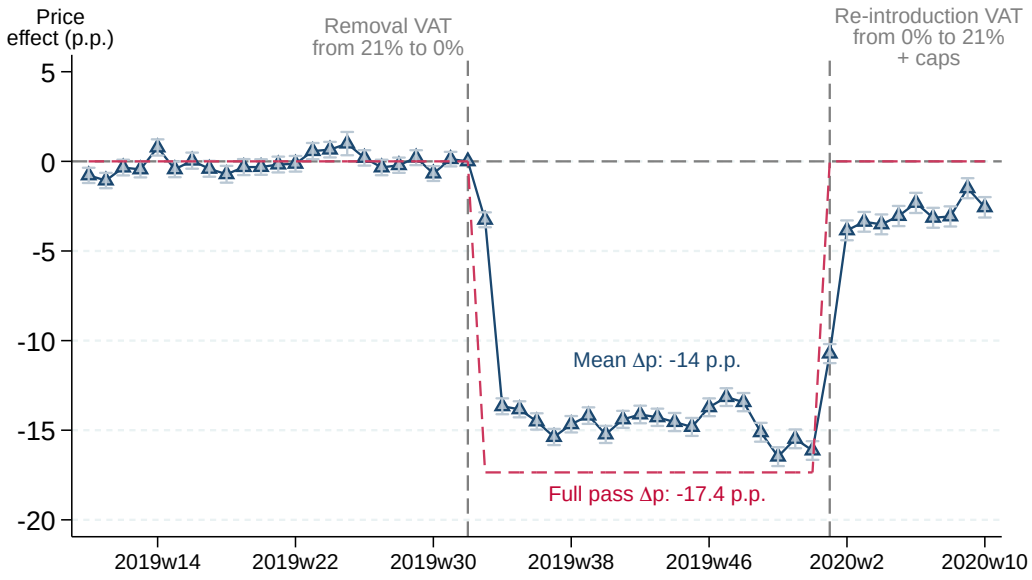
Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1). We focus on specific treated goods (T) and related goods vis-a-vis the remaining categories in the control group. The left panel estimates the price change for barcodes in tea (T), instant coffee (C), and ground coffee (C). The right panel estimates the price change for barcodes in sliced bread (T) and breakfast cereal (C) relative to the rest of the control goods.

Figure A.9: Pass-through of the 2018 peso depreciation using aggregate data from INDEC



Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) using official aggregate price data from INDEC. The pink line displays the nominal exchange rate between the Argentine peso and the US dollar (right axis). The blue line shows the percentage change in prices relative to week 1 of 2018 between treated and control goods as classified in Table 1 (for the categories available in the basket used to construct the CPI).

Figure A.10: Excluding imported goods



Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) before and after the VAT cut and its subsequent repeal in chains. In particular, we restrict the estimation sample to those goods that are locally produced and thus are less subject to the large depreciation that happened in mid August 2019. Considering the full estimation sample, only ten percent are not locally produced and, interestingly, this percentage is equally split in treated and control goods.