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

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Abstract

We estimate the effect of a temporary VAT cut and its re-introduction, along with anti-profiteering measures, on food necessities during a period of high inflation in Argentina. Using barcode-level data from over 3,000 supermarkets, we find that: (1) without anti-profiteering measures, prices rose more after the VAT was reinstated than they fell after the cut, with this asymmetry persisting over time; and (2) anti-profiteering measures curbed price increases following the VAT hike. A welfare model shows that the VAT cut had progressive effects and that anti-profiteering measures mitigated the regressive impact of asymmetric pass-through.

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1 Introduction

Inflation has long posed critical challenges for Low- and Middle-Income Countries (LMIC), where high and sustained price increases often trigger significant economic and political instability. This issue has gained renewed urgency in the wake of the COVID-19 pandemic, which has fueled inflationary pressures across much of the developing world. In response, LMIC governments have relied on a mix of conventional tools—such as raising interest rates—and unconventional interventions, including price controls and consumption tax cuts. While conventional policies have been extensively studied, the effects of unconventional approaches remain far less understood, especially in LMICs.

One increasingly common policy, adopted in both LMICs and OECD countries, has been the temporary reduction of Value-Added Tax (VAT) rates with the hope that it would lower consumer prices and shield low-income households from inflation. By our count, at least thirty one LMICs have adopted such temporary VAT cuts in recent years (see Appendix Table A1). The pattern is clear: in times of crisis, reducing or repealing the VAT on essential goods is a widely used tool to make these goods more affordable, albeit often at the cost of substantial tax revenue loss. These policies have been pursued despite mounting empirical evidence—primarily from high-income country settings—showing that VAT cuts tend to benefit firms more than consumers.[1]

This paper studies one such unconventional policy in an upper-middle-income country: Argentina's temporary VAT cut in 2019, paired with anti-profiteering measures aimed at limiting price increases upon repeal. Argentina provides a compelling case, having experienced chronic inflation over the past two decades and repeatedly relied on both conventional and unconventional stabilization tools. The reform temporarily zero-rated (cut the VAT from 21% to 0%) a set of basic food necessities from August 16 to December 31, 2019. Subsequently, the original VAT rate was reinstated. The policy followed a surprise presidential primary election result that led to a sharp depreciation of the peso and raised concerns about food affordability. Notably, when the VAT was reinstated, the government selectively applied price caps to a subset of the affected goods. This variation provides a compelling empirical setting to estimate the impact of anti-profiteering policies on consumer price pass-through following VAT increases.

As part of previously enacted legislation, the Argentine Government was empowered to control and monitor grocery prices in supermarket chains. In practice, for some products that had been zero-rated, authorities capped post-VAT repeal price increases at no more than 7% above their December 31, 2019 levels. Some items faced stricter controls—such as liquid milk, which was required to remain at pre-repeal prices—while others, like organic rice, were not subject to any caps. These anti-profiteering measures were enforced by the same agency responsible for antitrust regulation, drawing on a real-time price monitoring system launched in 2016. Firms found in

[1] See Kosonen (2015), Benzarti & Carloni (2019), Harju et al. (2018b), Benzarti et al. (2020) for examples of such evidence and Benzarti (Forthcoming) for a review of this literature.

violation could face fines and other sanctions, and no expiration date for the caps was publicly announced. Enforcement ultimately ceased with the onset of the COVID-19 pandemic in March 2020.

While the joint implementation of VAT changes and anti-profiteering measures is a relatively rare and novel policy, it is not unique to Argentina. Similar approaches have been adopted in other LMICs. For example, India introduced anti-profiteering measures in 2017 to ensure that reductions in the Goods and Services Tax were passed through to consumers rather than captured by firms.[2] The vagueness and discretionary enforcement of Argentina's policy mirror how these measures are often deployed elsewhere, as discussed in Section 2.3. We believe Argentina's experiment offers useful insights for other LMICs that have implemented or are considering temporary VAT cuts—particularly during periods of economic distress. In such settings, questions often arise about how to manage the expiration of temporary VAT cuts and whether complementary policies, such as anti-profiteering measures, can influence price dynamics. Our paper helps inform these debates by documenting both the intended and unintended effects of such interventions.

Our empirical strategy is a dynamic difference-in-differences design that leverages the selective application of both the VAT cut and the anti-profiteering measures. The VAT cut applied to a subset of basic food items commonly consumed in Argentina—based on the official food basket used to compute the cost of living—but excluded other, otherwise similar, products. For instance, the VAT was repealed on tea, milk, eggs, and rice, but not on coffee, breakfast cereals, or crackers. Among the items subject to the VAT cut, only a subset was later targeted by anti-profiteering measures when the tax was reinstated. This variation enables us to compare outcomes across three groups: (1) goods treated by both the VAT cut and the anti-profiteering caps; (2) goods affected only by the VAT cut and free to adjust prices upon repeal; and (3) a control group of goods unaffected by either policy. Our identification does not rely on random assignment into these groups, but rather on the standard parallel trends assumption—that, absent the VAT reform, treated and control goods would have evolved similarly. We test this assumption and find that pre-reform trends were indeed parallel across all three groups. We also address the possibility of spillovers from the treatment to the control group using two alternative strategies, and conduct a battery of robustness checks to validate our findings.

Our empirical analysis relies on two main data sources. First, we use high-frequency barcode-level scanner data from Scentia, a market company that collects prices and sales volumes directly from over 3,000 grocery stores—weekly for chain supermarkets and monthly for independent supermarkets. The "independent supermarkets" category does not include small corner shops or convenience stores ("mom and pop" stores), but instead stand-alone supermarkets and small regional supermarket chains.[3] The dataset spans the period January 2018 to June 2021 and includes

[2] Among LMICs, Malaysia is another example. Several OECD countries have implemented similar anti-profiteering measures when adopting a VAT, including Germany, the Netherlands, Korea, Belgium, and Ireland (see Tait, 1988).

[3] Street vendors and other informal retailers account for a very small share of food expenditures in Argentina. Appendix Figure D.1 shows that they represent at most 2% of the zero-rated food expenditures across the income

15,126 barcodes, corresponding to 1,082 brands and 536 producers. For each barcode, we observe VAT-inclusive prices, product descriptions, and quantities sold—recorded weekly or monthly, depending on the supermarket type. Second, we also use detailed expenditure microdata from the 2017–2018 National Household Expenditure Survey to further assess the distributional effects of the VAT cut.

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 were passed through to consumer prices. Moreover, in the medium run, prices for goods treated by the VAT cut but not subject to the anti-profiteering caps remained higher relative to the control group after the tax was reinstated. Using a three-tier Constant Elasticity of Substitution (CES) consumer model, we show that this price hysteresis had strong and negative welfare consequences: while the VAT cut initially generated welfare gains, these were more than offset by the tax reversal, which resulted in post-repeal prices that exceeded pre-reform levels (even after adjusting for inflation). On net, we find that the consumer welfare effects of the temporary VAT cut in the absence of anti-profiteering measures were negative because of the asymmetry in the way prices respond to VAT cuts and increases.

We also find that the anti-profiteering measures were relatively effective. First, we show that goods subject to price caps and treated by the temporary VAT cut experienced lower prices—following the VAT increase—relative to similar goods that were also treated by the VAT cut but not subject to anti-profiteering measures. This price gap persists for at least one year after the VAT increase. Second, we provide evidence that these price caps were binding: when plotting the distribution of price changes one week before and one week after the VAT increase, we observe significant bunching at the price caps. We also find that some prices exceeded the price caps, suggesting imperfect compliance. This is likely due to the difficulty of monitoring percentage changes in prices, as opposed to nominal price levels. Indeed, we show that goods subject to nominal price controls (i.e., 0% price change caps) are much more compliant with these caps: their average prices remained constant (despite high inflation).

Overall, we estimate that the anti-profiteering policies reduced 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 because of competition. Instead, we find that these caps created a gap in the price of identical goods sold at chain and independent supermarkets. These gaps also persist over the medium run, implying that competition is not strong enough to equalize prices. Importantly, when estimating the welfare effects of the temporary VAT cut in combination with these anti-profiteering policies, we find that they led to substantial improvements in welfare relative to a counterfactual without any anti-

distribution. Their limited prevalence contrasts with the settings studied in Bachas et al. (2023), which focus more on lower-income countries.

profiteering policies. However, these gains were unevenly distributed: higher-income households benefited more because pass-through asymmetries were larger in independent supermarkets, which is precisely where low-income households tend to shop the most.

One important concern with our analysis is the potential for bias arising from consumer substitution between treated and control goods. For example, if the price of tea decreases due to the VAT cut, some consumers may switch from coffee to tea, increasing demand and potentially raising the price of tea—thereby biasing our estimates. We address this concern using two main approaches. First, while some treated and control goods are plausible substitutes—such as tea and coffee or sunflower and olive oils—most are not (see Table 1). Many control goods, including breakfast cereal, salt, herbs, jams, and many others, lack close substitutes in the treatment group, which mitigates this substitution concern. In fact, when we re-estimate our main effects after removing obvious substitutes from the control group, such as coffee for the case of tea, we find quantitatively similar pass-through rates. Second, we repeat the analysis using an alternative control group composed of non-food items (e.g., cleaning products), which are highly unlikely to be substitutes for the food items affected by the VAT cut. Again, we find very similar estimates. Taken together, our evidence suggests that substitution barely affects the treatment effects we have estimated.

Another identification threat is the depreciation of the peso, which happened just before the VAT cut. The concern is that this shock may have affected treated and control goods differently, thereby confounding our estimates. Using another peso depreciation shock 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 have been 1.4 percentage points larger.

This paper's main contribution is to show that VAT cuts combined with anti-profiteering measures can be effective at dampening the effects of high inflation in LMICs. This is particularly relevant because several LMICs have been struggling with high levels of inflation in recent years and have used VAT cuts to mitigate the burden on consumers. However, few LMICs have also implemented concurrent anti-profiteering measures. As these VAT cuts are repealed, prices may rise sharply—often more than they fell during the initial VAT reduction—because VAT changes tend to be passed through asymmetrically when anti-profiteering measures are not in place, as we show in this paper. This dynamic can undermine the intended benefits of temporary VAT relief by reducing real purchasing power in the medium run. Instead, we find that deploying anti-profiteering measures upon repeal—alongside a price-monitoring infrastructure—can help reduce the asymmetric effects of VAT increases.

Relatedly, we contribute to the tax incidence literature by assessing the welfare effects of temporary VAT cuts with and without anti-profiteering measures. A closely related paper is Bachas et al. (2023), who compellingly show that the incidence of consumption taxes can be different in informal "mom and pop" shops because these types of stores are more likely to evade them (which

differs from our setting because Argentina has few of these stores).[4] 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. 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 welfare losses, then governments should be aware of that in order to assess whether such policies are suitable for achieving their goals.[5]

Finally, we contribute to a literature discussing VATs as a policy tool governments could use to affect the economy, in this case using VAT cuts and anti-profiteering measures to affect prices in times of high inflation (see Blundell, 2009; Crossley et al., 2009, for example). D'Acunto et al. (2022), for instance, 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 welfare effects in the medium run can be negative, unless paired with carefully calibrated anti-profiteering measures.

2 Institutional Setting

The main identifying policy variation we exploit consists of a temporary 21 percentage point VAT cut on essential food items, along with anti-profiteering measures implemented when the VAT was reinstated. In this section we describe the main features of these reforms as well as their political and economic context.

2.1 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. 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 30% depreciation of the Argentine peso relative to the U.S. dollar.[6] 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 peso's depreciation on prices (Executive Order 567/2019). As a consequence, the VAT cut was entirely unexpected. It was

[4] They represent less than 2% of the total consumption of the goods we consider (basic food items).

[5] See the following for examples of articles studying the incidence of consumption taxes, the majority of which are from OECD countries: 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). Kotlikoff & Summers (1987), Fullerton & Metcalf (2002) and, more recently, Benzarti (Forthcoming) provides a survey of the tax incidence literature.

[6] See Appendix Figure D.2. For more details, see this NY Times article. Table A1 also presents significant double-digit food inflation rates in countries that have experienced temporary VAT reductions recently.

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% (0.6%) of monthly annual VAT revenue.[7] The revenue loss due to the VAT cut was funded using budget reallocation.

The tax rate was reduced 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 VAT rate, except for wheat flour and bread, which are taxed at the 10.5% reduced rate. According to the National Institute of Statistics (INDEC), the categories with temporary 0% VAT accounted for 26% of total food expenditure. The selection of goods targeted by the VAT cut was largely based on prevailing consumption habits in Argentina, a practice consistent with similar food-based VAT interventions in other countries (see Appendix Table A1).

Importantly, the VAT cut only applied to sales made to final consumers, and supermarkets could claim back any VAT credit generated from purchases from suppliers or use it to offset other tax liabilities.[8] The left panel of Table 1 shows the list of goods treated by the temporary VAT cut and a list of control goods that are also part of the basic food basket. This table shows that otherwise similar goods were not included in the treatment 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 for tea and yerba mate was cut, but not for coffee. In our empirical analysis, we use these two groups to estimate price responses using a simple difference-in-differences approach.

2.2 Real-time Price Monitoring in Supermarket Chains

The government was able to monitor prices daily in chain supermarkets, but not in independent supermarkets. This is because, in 2016, the government launched the Electronic Price Advertising System (SEPA, for its acronym in Spanish) to monitor the prices of supermarkets in real time (Resolution 12/2016). This program, popularly known as *Precios Claros* is administered and enforced by the Consumer Protection Office. Its official goal is to enhance price transparency and comparability, allowing consumers to make more informed purchasing decisions—especially important in a high-inflation context where prices change frequently.

In practice, the government provides detailed technical guidelines and software that chain supermarkets must use to report daily price data for each barcode and point of sale. These stores are required to submit their full price list before 6:00 a.m. each day, with a window for corrections until

[7] The tax expenditure associated with this reform was calculated at 0.07% of the GDP, based on *ex-post* official estimations (Source: *Subsecretaría de Ingresos Públicos. Secretaría de Hacienda, Ministerio de Economía*). We provide additional details in Appendix Section C.

[8] Technically, treated goods were taxed at a 0% rate rather than exempted from the VAT. This is important because firms cannot deduct the VAT on the intermediate inputs of exempted goods, whereas they can for zero-rated goods.

10:00 a.m. The submitted prices are then published on an online platform accessible via computer or mobile app, allowing consumers to search and compare prices store by store.[9] Importantly, participation in *Precios Claros* is optional for independent supermarkets, given the administrative burden involved (Art. 4, Res. 12/2016). In the context of the VAT reform we study, this distinction is important: VAT changes were more easily enforced in chain supermarkets because their prices were subject to daily public and governmental scrutiny. It is worth noting that the price data we exploit in this paper originates from a different source (it is collected from supermarkets by a private firm, as we explain in Section 3.1).

2.3 Anti-Profiteering Measures

The new Fernandez administration did not extend the VAT holiday and instead regulated the reintroduction of the 21% VAT rate on a large share of the previously zero-rated goods. To limit the impact on consumer prices, the government implemented anti-profiteering measures that capped how much prices could increase upon the reinstatement of the VAT. In this section, we describe these anti-profiteering measures, how they were implemented, and situate them within the broader international experience.

Legal Framework The anti-profiteering measures were implemented under two preexisting laws. The first is the "*Ley de Abastecimiento de Comercio Interior*" (Law 20,680/1974), which grants the government broad powers to intervene in markets. The second is a 2013 law that introduced the "*Precios Cuidados*" program, allowing the government to set price controls on grocery items. Together, these laws provided the legal basis to cap price increases in supermarket chains following the repeal of the VAT cut. The government determined and announced the caps on December 31, 2019, communicating them to supermarkets and the public via press releases.

While the prices of some of the goods that were treated by the VAT cut and its repeal could change freely, others were only allowed to increase by up to 7% or 10.5% of their December 31, 2019 price once the VAT cut was repealed. Still others were mandated to remain unchanged (e.g., liquid milk). Table 2 summarizes these caps by product category. Importantly, these caps applied only to supermarket chains, which meant that independent stores could adjust their prices freely.

Enforcement Enforcement fell under the jurisdiction of the agencies responsible for consumer protection and antitrust regulation. Chain supermarkets were monitored daily through the *"Precios Claros"* system described above. This infrastructure was also used to track compliance with the anti-profiteering measures following the VAT increase. In interviews with senior officials—including Matías Kulfas, then Minister of Productive Development—we confirmed that they received daily price reports showing how much prices changed from day to day. Kulfas, who chaired the December

[9] See Appendix Figure D.3 for an illustration of *Precios Claros'* salience.

31, 2019 meeting with food manufacturers and supermarket associations, noted that violations could trigger investigations and sanctions by the Commerce Department under the newly enacted Fair Commerce Law (Decree 274/2019). While rare, exceptions to the caps were granted in cases where firms could justify price increases based on higher input costs.[10]

This new Fair Commerce Law empowered the government to impose a wide range of penalties for non-compliance, including formal warnings, monetary fines ranging from 1 to 10 million Mobile Units (each initially valued at 20 pesos in 2018 and indexed to inflation), suspensions from the National Register of Government Suppliers, revocation of concessions, and even temporary business closures for up to 30 days. Several firms were sanctioned under this framework, as documented by contemporaneous media reports.[11]

We provide empirical evidence on compliance with these measures and the challenges with enforcing it in more detail in Section 4, where we show that these caps were binding, but that there was some non-compliance.

Caps Horizon The duration of the anti-profiteering measures was never formally specified, leaving supermarkets uncertain about how long they were expected to comply with the price caps. In practice, the Ministry of Productive Development and the Commerce Department monitored and investigated price changes on a daily basis from January 1, 2020 until the onset of the COVID-19 pandemic in March 2020, when enforcement activities were effectively suspended.

Anti-Profiteering Measures in Other Countries Appendix Section A provides a non-exhaustive overview of anti-profiteering policies implemented across other developing countries. It might come as a surprise that some features of the anti-profiteering measures were vague, such as the time horizon over which they were expected to apply. However, anti-profiteering measures in other countries are also often imprecise. This is the case with India's enactment of its anti-profiteering measures in 2017, which were targeting consumption tax cuts and credits. Similar to the Argentine experience, the legal framework of the Indian anti-profiteering measures is relatively vague in that it does not define profiteering explicitly and leaves it to the Competition Commission of India—an institution akin to the U.S. Federal Trade Commission—to decide on a case-by-case basis (see Kir, 2023).[12]

These anti-profiteering policies are also relatively unclear when implemented in OECD countries. For example, 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

[10] Appendix Figure D.4 shows an excerpt from *Clarin*, one of Argentina's leading newspapers, discussing enforcement of the anti-profiteering measures. The image features Matías Kulfas, who led the implementation of these measures.

[11] See, e.g., La Nación, El Diario Córdoba, and Gobierno de Río Negro.

[12] Section 171 of the CGST Act is remarkable in that states: "Any reduction in rate of tax on any supply of goods or services or the benefit of input tax credit shall be passed on to the recipient by way of commensurate reduction in prices." Any consumer or organization deeming that the reduction in prices is not sufficient can file a claim against the party at fault, which is then investigated by the Competition Commission of India. See Section 171 of the CGST Act and the Competition Commission of India website: https://www.naa.gov.in/page.php?id=anti-profiteering

profits, and one third to wages but did not specify how long these mandates should hold or how they would be enforced. Similarly, California recently 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, but does not specify what is considered to be too high.[13]

2.4 Salience

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, Appendix Figure D.5a 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, Appendix Figure D.5b 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, Appendix Figure D.6 shows how supermarkets communicated the VAT cut to their customers using fliers and price tags, which were mandated by the government. It is unclear whether supermarkets expected that the government would impose caps on prices during the VAT increase. On the one hand, these anti-profiteering measures were announced on December 31, a day before they were enacted. On the other hand, Argentina has implemented several price caps and freezes in the past, and so such a policy may not have been a surprise to supermarkets.

3 Data and Empirical Strategy

3.1 Data

We provide a summary of all the datasets we use in Appendix Table A2 and describe them in detail below.

Supermarket Scanner Data. Our analysis primarily relies on retail scanner data provided by the marketing consulting firm Scentia LLC.

These data consist of high-frequency sales records generated by point-of-sale systems across Argentina. In particular, Scentia gathers all scanner-based price and quantity information from both chain and independent supermarkets.[14] Among supermarket chains, the sample covers the 12 largest retail chains in the country, encompassing all 2,317 stores (e.g., Walmart, Carrefour, Coto, La Anonima, etc.). For independent supermarkets, Scentia collects data from a representative sample of 800 stores, drawn from a universe of roughly 18,700 establishments nationwide. These stores consist mostly of independent supermarkets and a few regional chains owned locally rather

[13] See link.

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

than "mom and pop" shops or convenience stores.[15] Because these data are derived from barcode scanners used for inventory and stock management, they capture all scanned items regardless of payment method (cash or card) or whether a receipt is issued. As such, scanner-based systems ensure comprehensive coverage of transactions as long as the product is scanned at checkout.

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 are categorized into broad categories (e.g., oil, coffee, rice, etc.), which are further subdivided into subcategories (e.g., sunflower oil, corn oil, olive oil, ground coffee, coffee beans, coffee pods, etc.). In addition, each product carries a rich textual description (e.g., Nescafé Gold Intense Instant Coffee Jar 200g), which provides a level of granularity below the subcategory classification. This detailed labeling allows us to accurately assign products to treatment and control groups, as shown in Tables 1 and 2.

We use three different extracts of the data owned by Scentia. The first and primary data extract used in the analysis is aggregated across stores at the barcode-region-time level. We refer to this data extract as the "main sample". For chains, we observe weekly information from barcodes in ten different geographic areas. For independent supermarkets, we observe monthly information from barcodes split into five regions.[16] Our dataset covers January 2018 through June 2021 (181 weeks for chains and 42 months for independent supermarkets). When aggregated at the region-by-barcode-by-month level, each month covers an average of USD 170 million worth of grocery sales across 3,117 individual stores, in more than 60 disaggregated product categories, and across 19,304 barcodes, which correspond to 642 producers and 1,248 brands.

In our analysis, we use a balanced panel of the main sample that includes all barcodes with positive sales in the 62-week period from January 2019 to March 2020. Appendix Table A3 provides summary statistics for this panel across chain and independent supermarkets, disaggregated by whether they were subject to the VAT cut and its repeal, and whether their prices were capped by the anti-profiteering measures. The table also presents baseline average price differences. The two samples are relatively similar, but also different in important ways: (1) there is a significant portion of barcodes that are only sold in one type of store; and, (2) average prices in independent supermarkets, at baseline (i.e., prior to the VAT cut), tend to be 3 to 4% lower than in chain supermarkets, which could reflect compositional differences (see point (1)) or intrinsic differences in prices levels.

We use the second and third data extracts of the dataset owned by Scentia for robustness checks. The second data extract is identical in its nature and structure to the main sample, but

[15] Examples include: Buenos Dias, Cordiez, El Nene, Josimar, Toledo, among others.

[16] The ten regions are: Capital Federal, Periferia, Córdoba, Litoral Norte, Litoral Sur, Resto Pcia BSAS, Cuyo, NOA, Sur, Austral. The five broader areas are: Andina, Córdoba, GBA, Litoral, Resto Pcia BSAS + Sur. Importantly, the data is representative of the whole country. See Appendix Figure D.7 for more details about geographic variables.

also includes data on beverages and non-food items and is restricted to the region of Periferia and chain supermarkets. It contains the same variables as the main sample. We refer to this extract as the "Periferia sample".

The third data extract is more disaggregated than the main and Periferia samples: it contains observations at the supermarket level. The sample includes information on food and non-food items as well as beverages in the region of Córdoba. It includes the same variables as the main sample as well as a variable that indicates whether the supermarket is part of a chain or an independent supermarket. Moreover, it contains precise geographical information for chain supermarkets.

VAT return data. This dataset includes information from aggregated VAT returns and is used to estimate the effect of the VAT reform on government revenue. It is produced by the National Tax Agency and covers all regions of Argentina annually from 2015 to 2023. It is aggregated at the sectoral level.

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), conducted by the National Institute of Statistics and Censuses (INDEC). The ENGHo provides detailed product-level information on both food and non-food expenditures, including the types of stores where purchases were made, the forms of payment used, and a range of household characteristics. The data were collected through a questionnaire answered by the head of the household and weekly diaries used to record daily household expenditures. The survey was conducted between November 2017 and November 2018 in localities with 2,000 or more inhabitants, yielding a nationally representative sample of approximately 45,000 households.

Consumer Price Index. This dataset, produced by the National Statistical Office, contains the product-level prices used to construct Argentina's Consumer Price Index (CPI). We use it to validate the representativeness of the Scentia scanner data in capturing national price dynamics.

3.2 Empirical Strategy

Our empirical approach relies on a straightforward dynamic difference-in-differences specification. We classify products into treatment and control groups depending on whether a given barcode was subject to the policy intervention under analysis (VAT cut, VAT increase with price caps, etc.). First, we provide some graphical and nonparametric evidence by plotting the unconditional mean 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 given by:

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

where Y_{it} denotes the tax-inclusive price of a given good (barcode) i at time t, computed as the simple average across stores. Y_{it} is normalized to 100 for each barcode i in week 32 of year 2019.[17] The indicator D_{it} equals one if barcode i is treated in week t and zero otherwise. The coefficients of interest β_t capture the dynamic treatment effects—i.e., the average price difference between treated and control barcodes in each week t, relative to the baseline week (2019w32). Standard errors ϵ_{it} are clustered at the barcode level i. We restrict our dataset to a balanced panel of 4,645 barcodes with positive weekly sales between January 2019 and March 2020.

The treatment and control groups include all barcodes belonging to the food categories described in Section 3.1 and listed in Table 1.[18] The control group comprises all barcodes in 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 graphical evidence plotting unconditional means. This is reassuring and mitigates concerns that our results are significantly affected by the particular specification we use.

4 Results

We begin by estimating the effect of the VAT cut and its subsequent repeal on the prices of goods that were not subject to anti-profiteering measures. We then turn to goods that were covered by such measures and estimate the corresponding price effects. Finally, we disaggregate the analysis by store type—chain supermarkets, where anti-profiteering measures were enforced, and independent stores, where such measures did not apply.

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

Figure 1a presents the nonparametric effect of the VAT cut and its repeal on prices in the control and treatment groups. Here, the treatment group only includes barcodes that were not subject to anti-profiteering measures, i.e., there were no caps on the rate of price increases when the VAT was increased. The dataset used in this figure pools chain and independent supermarkets together, thus the observations are at the monthly level.

As mentioned, prices are normalized to 100 in the month prior to the VAT cut (July 2019). Four findings are worth highlighting. First, prices in the treatment and control groups follow parallel trends in the six months preceding the VAT cut. Second, there is a sharp break in the series immediately after the VAT cut was implemented, as prices in the treatment group grow at a

[17] For specifications using monthly data, prices are normalized to 100 in July 2019.

[18] Although the VAT rate on wheat flour was reduced to zero, we exclude these barcodes from the analysis because flour was already subject to a lower baseline VAT rate of 10.5%, rather than the standard 21%.

substantially lower rate than those in the control group. Note that prices trend positively, since we are plotting nominal prices and inflation was high ($\approx 55\%$ yearly in 2019). Third, another break in the series occurs in January 2020, when the VAT cut is repealed. Here, prices in the treated group rise sharply—surpassing those in the control group and 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 the COVID-19 pandemic.

Figure 1b plots the results of estimating equation (1) on the exact same data as in Figure 1a, which allows us to add standard errors and 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 in the pre-reform period, which might be biasing our pass-through estimates. Note that this parallel trend issue disappears once we consider a larger sample below. We also find that prices decrease on average by 9.3 percentage points over the four-month period following the VAT cut.[19] This corresponds to a pass-through rate of the VAT cut to prices of 53% relative to the full pass-through benchmark of 17.4 percentage points.[20]

Finally, our estimates confirm that prices respond to the repeal of the VAT cut in the treatment group and exceed their 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 Appendix Table A4. Overall, the VAT cut resulted in substantial price effects, but fell short of full pass-through. Additionally, the VAT increase led to prices that were higher than their pre-VAT cut levels, with some 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).[21] 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% cap, 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 had limited capacity to enforce the regulation and monitor

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

[20] A full pass-through would yield a price decline of -0.21/1.21 x 100 = 17.4%, since the VAT rate fell from 21% to 0%.

[21] We refer to price controls as *caps* on how much prices could increase to mitigate the VAT reintroduction.

prices in the more than 18,000 independent stores around the country.

This setting offers a unique opportunity to estimate the effect that governments can have on tax incidence. In order to assess the effect of these anti-profiteering measures, we break down the list of goods into a control group (unaffected by the VAT cut) and a treatment group, which corresponds to those barcodes that were treated by the VAT cut and that were also subject to the price caps when the VAT was increased.

Chain and independent supermarkets pooled together. Figure 2a shows the nonparametric effect of the VAT cut and its repeal on prices, in the control and treatment groups (the latter includes only those goods that were subject to the price caps). In this figure, we pool chain and independent supermarkets together. There are three main findings. First, the trends for the control and treatment groups are parallel. Here, since the sample of goods we consider is substantially larger than that for uncapped barcodes, the parallel trend assumption holds even better than above. Second, there is a sharp break in the series immediately after the VAT cut. Third, there is another break in the series when the VAT cut is repealed. Here, prices in the treatment group increase enough to match those in the control group, thus restoring the previous equilibrium (with no asymmetry).

Finally, our estimates confirm that prices respond to the repeal of the VAT cut in the treatment group enough to return to the levels in the control group (Appendix Table A4).

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 in the 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 shows them for independent supermarkets.[22] The corresponding estimates of these two figures are plotted in Figures 4a and 4b, respectively. When considering these two types of supermarkets separately, we find dramatically different pass-through rates of the VAT cut and its repeal. Similar 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 supermarket chains. This is true both in the unconditional mean figures (Figures 3a and 3b) and 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

[22] In this case, the treated group includes those goods that were subject to price caps and those that were not.

and 35% for independent supermarkets. To ensure that the level of aggregation (monthly versus weekly) 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 (less than 1% of the overall pass-through).[23]

This large 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 only in the chain supermarkets. To further confirm this, we next estimate the effect of the VAT increase for barcodes subject to the anti-profiteering measures compared with those that were not, in chain supermarkets only.[24]

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, with those with no price caps in chain supermarkets. In both cases, the control group is the original set of barcodes subject to the 21% VAT rate. This figure shows that goods with no price caps experience a price increase (when the 21% rate is reintroduced) that almost doubles that of those subject to the 7% cap.[25]

However, while the 7% cap is effective at mitigating price increases, Figure 5a shows that some barcodes exhibit price increases of more than 7%. This could be due to the fact that monitoring percentage increases in prices can be difficult in a high-inflation environment. 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 was the case for milk, there is more compliance, since average prices experience a limited increase at the time of the VAT increase.

We also 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 remains stable until the end of 2020.

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 was 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

[23] Note that the series "Chains (capped)" in Figure 4b only includes capped goods, which allows us to pin down the effect of caps. The price effect for chain supermarkets following the VAT cut is 14.7 percentage points at the weekly level and 14.9 percentage points at the monthly level (see Appendix Table A4).

[24] We also discuss some of the reasons why the VAT cut pass-through rates are different in chain and independent supermarkets in Appendix Section B.2.

[25] Appendix Figure D.8 provides two case studies that add credibility to the finding. This 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 consistent with the estimates from Figure 5a.

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 Appendix Figure D.9). This suggests that price competition between chain and independent supermarkets is not strong enough to cause prices to converge even a year after the VAT increase was implemented, as shown in Figure 4b.

Pass-through Distributions. Figure 6 shows the distribution of pass-through rates for three groups of goods: (1) control goods, which were unaffected by the VAT changes (and thus also unaffected by the anti-profiteering measures); (2) goods treated by the VAT changes that were subject to the 7% anti-profiteering caps on how much prices can rise; and (3) goods treated by the VAT changes that were not subject to anti-profiteering measures.[26] For each group, we calculate the pass-through rate as the percentage point change in prices between one week before the VAT increase and subsequent weeks. Therefore, we are not differencing out the control group in these figures. Instead, we are plotting it alongside the treated groups for visual comparison.

Figure 6 presents the pass-through distributions for weeks 0 to 3 following the VAT increase (i.e., the first four weeks of January 2020). In week 0, the three distributions appear relatively close to one another, though some differences begin to emerge. From weeks 1 to 3, the patterns become more pronounced. First, the distributions for the three groups are clearly distinct. Second, we observe visible bunching at key thresholds: (1) at 7% for treated goods subject to anti-profiteering caps, (2) at 21% for treated goods not subject to such caps—corresponding to the full VAT increase, and (3) at 0% for control goods, which were unaffected by the VAT change.

These patterns imply that the anti-profiteering measures were binding: the price of many of those goods that were subject to them was increased just enough to match the 7% cap. In contrast, the price of uncapped goods increased by 21%, implying that the VAT increase was fully passed through to consumers when there were no anti-profiteering measures. Nevertheless, we observe substantial non-compliance with the anti-profiteering measures, since the pass-through of several of the goods treated with caps exceeds the 7% cap. This suggests that while the anti-profiteering measures were successful at keeping prices low, prices could have been even lower under full compliance.

The distributions for weeks 4 through 11 (relative to week -1) are shown in Appendix Figures D.12 and D.13. The bunching patterns mirror those from Figure 6: the three distributions remain clearly distinct, with bunching around 0% for the control group, 7% for capped goods, and 21% for uncapped treated products. While the bunching becomes more diffuse over time, the separation between groups persists. This is consistent with the evidence in Figure 5a, which shows that the average price of capped goods remains substantially lower than that of uncapped goods for at least

[26] Milk, which was subject to a 0% cap (i.e., prices were mandated to remain unchanged), is shown separately in Appendix Figures D.10 and D.11. These distributions are considerably noisier due to the smaller sample size.

a year after the VAT increase.

In spite of the government being vague about how long these caps were meant to remain in place, on average, firms abided by them. Despite some degree of non-compliance, average prices of capped goods stayed lower than those of uncapped goods. These averages are reflected in the pass-through distributions: even when the bunching at the 7% cap becomes more diffuse, the distribution of pass-through rates in the capped categories clearly lies to the left of that of the uncapped goods.

4.3 Robustness Checks

Substitution across products in the treatment and control groups. One concern with our difference-in-differences strategy is that treatment effects may be biased if consumers substitute goods in the control group with those in the treatment group. For example, if the price of tea falls due to the VAT cut, some consumers may shift from coffee to tea to take advantage of the lower price. This would increase demand for the treated goods, presumably increasing their prices, thereby biasing our estimated effects downward (e.g., see Minton & Mulligan, 2024).

We address this potential violation of the Stable Unit Treatment Value Assumption (SUTVA) using two strategies, with results summarized in Figure 7. First, although some control goods have close substitutes among treated products (such as tea and coffee, or different cooking oils), many do not. As shown in Table 1, items like breakfast cereal, salt, herbs, jams, and many others do not have obvious substitutes in the treatment group, mitigating substitution concerns. We formalize this idea by redefining our control group, by excluding categories likely to be close substitutes for 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 specification on chain supermarkets, where the VAT effect is strongest and thus most likely to cause substitution (substitution effects are less relevant in independent stores, where price responses were smaller).

Figure 7a shows that even after excluding control group items with obvious substitutes, the results remain largely unchanged. The average decrease in prices after the VAT cut is 15.2 percentage points when close substitutes are excluded from the control group (e.g., coffee), compared to 14.7% when including them. Substitution operates in the expected direction, slightly biasing our estimates downward, but the difference is minimal.[27]

Second, we re-estimate our main effects using an alternative control group composed exclusively of non-food items, which are highly unlikely to be substitutes for food products.[28] For this analysis, we use the Periferia sample, which is restricted to chain supermarkets in a single region (*Periferia*),

[27] The left panel of Appendix Figure D.14 provides more details on 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 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.

[28] Non-food products, previously excluded from our specification, include office supplies, body moisturizers, antiperspirants, hand soap, laundry detergent, bleach, surface cleaners, toilet paper, shampoo, and cleaning wipes.

as we were only able to purchase non-food scanner data for that region. This restriction results in larger standard errors. The results, shown in Figure 7b, indicate that the average price of treated goods decreased by 15.7 percentage points relative to the non-food control group. For comparison, we overlay the estimate using the original control group. While some degree of substitution may exist in our setting, it barely affects our results.

Income effects. The VAT reform caused a large price decline for a substantial share of the household consumption basket, particularly for low-income households. This raises the possibility that some of the effects we estimate may reflect income effects, whereby consumers' budget constraints are relaxed because many of the goods they purchase are now cheaper. Income effects are notoriously difficult to isolate and are often assumed away in the Public Finance literature (Saez et al., 2012). However, it is worth noting that if income effects impact both control and treatment goods similarly, their net effect would be differenced out in our estimation framework.

We address this concern in two ways. First, we test for aggregate income effects using semi-aggregate VAT return data. If income effects were substantial, we would expect to see an increase in the consumption of untreated goods (assuming they are normal goods), leading to higher VAT revenue from those items. However, as shown in Appendix Section C, VAT revenue from untreated goods shows no meaningful change around the reform, at least in aggregate.

Second, we exploit store-by-barcode data from the Córdoba sample to test for income effects by comparing sales of untreated goods—i.e., goods not affected by the VAT cut—between chain and independent supermarkets.[29] This exercise is especially well-suited for detecting income effects, because pass-through of the VAT cut was much higher in chains than in independents, meaning consumers shopping at chains experienced a larger effective income shock. If income effects were present, we would expect higher demand for untreated items in chain supermarkets relative to independents, even though these goods were not directly affected by the VAT cut. The key identifying assumption is that consumers do not switch store types in response to the reform.

Appendix Figure D.15 presents the results for untreated goods and, for comparison, also includes the same analysis for treated products. We find no evidence of a differential increase in prices or quantities of untreated goods between chain and independent stores following the VAT cut. Conversely, quantity effects for treated goods are large, as expected given the higher pass-through in chains. This pattern strengthens our interpretation: the VAT reform produced large direct effects, but little to no indirect demand spillovers via income effects. The Córdoba analysis provides unusually clean evidence of quantity responses by holding constant the item barcode and region, while exploiting variation in effective treatment intensity across store types—a rare and powerful identification strategy in the price incidence literature.

[29] We focus on Córdoba because it is one of Argentina's largest provinces, the estimated pass-through rates there closely match our national results, and purchasing disaggregated data for all stores was feasible within our budget.

Pass-through of the peso depreciation. Another threat to our research design is the quasi-simultaneous depreciation of the peso, which happened three days before the VAT cut. If this depreciation affects basic necessities subject to the VAT cut more than the control group, then we would expect an upward bias in prices. Hence, the pass-through of the VAT cut to prices would be partially offset by this depreciation episode, thus leading to a more conservative estimate of the pass-through rate. In other words, absent the depreciation of the peso, the prices of the treated goods would have decreased even more.[30]

To address this concern, we use another depreciation episode that took place exactly one year before the VAT change and compare the prices of treated and control products. On August 30, 2018, Argentina experienced the second most important depreciation of the peso since 2002—similar in magnitude to the depreciation episode of August 12, 2019 (Appendix Figure D.2): between January and September of 2018, the exchange rate doubled and remained stable thereafter. We run our baseline specification (1) in supermarket chains for the years 2018 and 2019, up to the week before the VAT was cut.[31]

Appendix Figure D.16a shows that the prices of the treated goods indeed responded more to the depreciation of the peso back in 2018. The price gap between the 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 stabilize again after week 45. This evidence suggests that the government might have been successful in targeting necessities likely to be affected by the 2019 peso depreciation to alleviate the burden on low-income households.[32]

Nevertheless, the magnitude of the effects of inflation on prices is small and does not seem to pose a significant threat to identification. According to Appendix Figure D.16a, the nominal exchange rate roughly increased from 20 to 40 pesos per dollar—corresponding to a 100% increase. In contrast, the prices of the treated goods increased by 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 (Appendix Figure D.2), 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 of the peso, the price drop reported in Figure 4a would be 1.44 percentage points larger.

Overall, our main estimates change very little, even when accounting for the substitution and peso depreciation effects, as can be seen in Appendix Figure D.19.

[30] In principle, imported goods are more likely to be affected by the depreciation of the Argentine peso. Nonetheless, we find very similar pass-through rates when we exclude imported goods from the treatment and control groups (Appendix Figure D.17), suggesting that the currency depreciation episode did not affect these groups differently.

[31] We omit the first week of 2018 from the regression so that all the coefficients are measured relative to that week.

[32] To provide further evidence that the effect of the exchange rate change we estimate is causal, we use aggregate data from INDEC, classify the categories of the CPI into treatment and control, and estimate our baseline specification in 2017, as a placebo check. Appendix Figure D.18 shows that prices in the treatment and control groups did not change differently in 2017 when the exchange rate was very stable.

4.4 What Explains the Asymmetric Pass-Through

While the canonical tax incidence model predicts symmetric pass-through of equal-sized tax changes (see Fullerton & Metcalf, 2002; Benzarti, Forthcoming), this section shows that there are macro pricing models that can generate some degree of asymmetry. We describe some of these models below and discuss how well they fit our pass-through estimates (see Appendix Section B for more details).

There are two main classes of macro pricing models (see Nakamura & Steinsson, 2013): Calvo pricing models (Calvo, 1983) and menu cost models (Caplin & Spulber, 1987). Calvo models, which assume that opportunities to adjust prices occur at random, are unlikely to fit our data (as we discuss in more detail in Appendix Section B). Intuitively, these types of models predict sluggish price adjustments, but our evidence shows that prices respond immediately to VAT changes.

In contrast, menu cost models can generate asymmetric pass-through, whereby prices respond more to tax/cost increases than to decreases, in the presence of inflation. In this section, we provide intuition for the main mechanism through which these models generate asymmetric pass-through, and we describe them in more detail in Appendix Section B.

Assume that firms incur a menu cost every time they change prices. In this case, we show that firms decrease prices immediately after the VAT cut to account for the effect of the VAT cut, and also bundle some future price increases due to inflation, thus saving on menu costs. The price response to the VAT cut is therefore mitigated by the bundling of future price increases due to inflation.

Similarly, following the VAT increase, firms may increase prices above those of the control group, bundling future price increases due to inflation with the price increase due to the VAT increase and thus saving on menu costs. This results in aggregate nominal prices increasing in the treatment group and overshooting relative to the control group following the VAT increase. Treatment group prices remain stable for a few periods after the VAT increase (the number of which will depend on the relative magnitude of the menu costs and inflation) until the control group catches up with the treatment group. At this point, the two groups should have similar price levels.

While such a simple model can predict asymmetric pass-through and short-run price hysteresis, it is likely incomplete. In particular, the price difference between the control and treatment groups is supposed to converge to zero a few periods after the VAT increase according to the menu cost model predictions. In contrast, we observe that prices in the treatment group remain high several months after the VAT increase. For these reasons, additional features and assumptions or a different class of models, may be needed to match our empirical findings. To the best of our knowledge, there are no macro pricing models that predict medium- to long-run price hysteresis.

5 Welfare and Distributional Effects of the Temporary VAT Cut

The official goal of the VAT cut was to preserve low-income households' access to a basic basket of necessities during a period of unusually high inflation, driven by a sharp depreciation of the Argentine peso following unexpected election results. In this section, we quantify the consumer welfare and distributional effects of the temporary VAT cut and assess the extent to which it achieved its intended goals.

5.1 Data

Ideally, we would like to observe the income of each shopper at every supermarket, allowing us to precisely measure the distributional consequences of the VAT cut—accounting for differences in shopping behavior and basket composition across income groups. Since such data are not available, we complement our analysis with the household expenditure survey data described in Section 3.1. In particular, we use the household-level consumption data to estimate the share of food expenditure on goods affected by the VAT cut, and the types of supermarkets these households shop at.

5.2 Stylized Facts

The top panel of Figure 8 reports the share of products treated by the VAT cut in the total household food expenditure, by income deciles. This share decreases with income: the lowest decile spends 27% of their food budget on treated goods, while that figure is 15% for the richest decile (the national average is 20%). This pattern suggests that the government was correct 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. However, the bottom panel of Figure 8 shows that household expenditure on zero-rated goods in Argentine pesos 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.

Next, we plot the propensity to shop at chain versus independent supermarkets by income groups. Figure 9 shows the share of money spent on zero-rated food by income decile and supermarket type.[33] The share of the budget spent on treated goods 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 goods rises with income in chain supermarkets, but declines in independent supermarkets.

This finding, that the propensity to purchase food items at chain supermarkets increases with the household income, 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

[33] Specialized stores, such as butcheries and bakeries, are not included in our data and were not part of the VAT change. Similarly, purchases from street vendors (a.k.a. "mom-and-pop" shops) are negligible in our setting.

richer households more. While it is likely that low-income households benefited from the VAT cut, both because some of them shop at chain supermarkets and because independent supermarkets did pass through some of the VAT cut, this evidence implies that there was scope for the VAT cut to be better targeted.

Next, we estimate a consumer welfare model to formally quantify the distributional effects of the VAT cut. The model also serves as a guide for identifying which outcomes should be estimated and which are irrelevant for welfare. For instance, it shows that tax evasion does not need to be estimated separately, as long as its effects are fully reflected in observed price changes.

5.3 Quantifying the Welfare Effects of VAT Changes

In this section, we propose a consumer welfare model to assess the impact of VAT changes on households, as a function of moments observed in our data. The model is adapted from the trade and development literatures that are concerned with estimating the effect of government policies using scanner-level data (e.g., Atkin et al., 2024). Despite their relevance, such models remain underutilized in the Public Economics literature, even though they offer valuable insights into the welfare effects of tax changes. They rely on standard assumptions, such as assuming a nested Constant Elasticity of Substitution (CES) utility function to model consumer preferences (e.g., Atkin et al., 2018; Redding & Weinstein, 2020; Handbury, 2021; Faber & Fally, 2022). Although some of these assumptions might be strong (as we discuss below), these models have the advantage of being tractable and parsimonious.

We adapt the nested CES approach from Atkin et al. (2018), which uses a three-tier demand system. Consider a household in income decile h with income w_h. In the upper tier, we assume Cobb-Douglas preferences over product groups $g \in G$ (e.g., coffee); in the middle tier, we assume asymmetric CES preferences over local retailers $s \in S$ (e.g., supermarket chains and independent grocery stores) that sell the product group considered; and in the final tier, we assume preferences over individual products within product groups $b \in B_g$ (e.g., NESCAFÉ Clásico Instant Coffee, Dark Roast, 7 oz. Jar):

$$U_h = \prod_{g \in G} (Q_{gh})^{\alpha_{gh}} \tag{2}$$

$$Q_{gh} = \left[\sum_{s \in S_g} \beta_{gsh} q_{gsh}^{\frac{\sigma_{gh}-1}{\sigma_{gh}}} \right]^{\frac{\sigma_{gh}}{\sigma_{gh}-1}} \tag{3}$$

$$q_{gsh} = \left[\sum_{b \in B_g} \beta_{gsbh} q_{gsbh}^{\frac{\sigma_{gsh}-1}{\sigma_{gsh}}} \right]^{\frac{\sigma_{gsh}}{\sigma_{gsh}-1}} \tag{4}$$

where α_{gh}, β_{gsh}, and β_{gsbh} are income group-specific preference parameters that are fixed across periods; Q_{gh} and q_{gsh} are product group and store-product group consumption aggregates with associated price indexes P_{gh} and r_{gsh}, respectively; σ_{gh} is the elasticity of substitution between product group g across chain and independent supermarkets; and σ_{gsh} is the elasticity of substitution between individual products b within store and product groups. For each broad product group g (e.g., coffee), consumers choose how much to spend at different stores based on the store-level price index r_{gsh}, which itself depends on the products they anticipate buying in each store, given its specific product mix and product-level prices.

Note that this is a general framework, in which we allow households of different incomes h to differ in their preferences and elasticities of substitution across groups of products, stores, and individual products within those stores. Using the CES properties, we derive the following indirect utility function:

$$V_h = \frac{w_h}{P_h},$$

where w_h is income and P_h is the CES ideal price index

$$P_h = \prod_g P_{gh}^{\alpha_{gh}}$$

$$P_{gh} \equiv \left(\sum_s \beta_{gsh}^{\sigma_{gh}-1} P_{gsh}^{1-\sigma_{gh}} \right)^{\frac{1}{1-\sigma_{gh}}}$$

$$P_{gsh} \equiv \left(\sum_b \beta_{gsbh}^{\sigma_{gsh}-1} p_{gsbh}^{1-\sigma_{gsh}} \right)^{\frac{1}{1-\sigma_{gsh}}}$$

We can estimate welfare change as follows:

$$d \ln V_h = d \ln w_h - d \ln P_h, \tag{5}$$

Ignoring the income effects yields:

$$d \ln V_h = -\sum_g \alpha_{gh} d \ln P_{gh}, \tag{6}$$

where α_{gh} is the initial expenditure share of household h in varieties from group g, and the change in the price index of products from group g is given by:

$$d \ln P_{gh} = \left(\frac{1}{1-\sigma_{gh}} \right) \left(\sum_s \beta_{gsh}^{\sigma_{gh}-1} P_{gsh}^{1-\sigma_{gh}} \right)^{-1} \sum_s \beta_{gsh}^{\sigma_{gh}-1} P_{gsh}^{1-\sigma_{gh}} \frac{(1-\sigma_{gh}) dP_{gsh}}{P_{gsh}}, \tag{7}$$

Simplifying, we obtain:

$$d \ln P_{gh} = \sum_s \alpha_{gsh|g} d \ln P_{gsh}, \tag{8}$$

where

$$\alpha_{gsh|g} = \frac{\beta_{gsh}^{\sigma_{gh}-1} P_{gsh}^{1-\sigma_{gh}}}{\sum_{s'} \beta_{gs'h}^{\sigma_{gh}-1} P_{gs'h}^{1-\sigma_{gh}}}$$

corresponds to the initial expenditure share in products from group g at store type s. Performing the same analysis to P_{gsh} in the third nest, we obtain:

$$d \ln P_{gsh} = \sum_{b} \alpha_{gsbh|s_b} d \ln p_{gsbh}. \tag{9}$$

Substituting equation (8) and (9) into (6) yields the following key welfare expression:

$$d \ln V_h = - \sum_{gsb} \underbrace{\alpha_{gh} \alpha_{gsh|g} \alpha_{gsb|s_b}}_{\alpha_{gsbh}} d \ln p_{gsbh}, \tag{10}$$

where α_{gsbh} corresponds to the initial expenditure share of household h on product b from product group g at store type s.[34] The change in welfare only depends on the *initial* expenditure shares and the change in tax-inclusive prices p_{gsbh}. Therefore, to a first-order approximation, there is no need to estimate the elasticities of substitution or any changes in the expenditure shares.[35]

Although this nested CES model is appealing because it is tractable and can be easily mapped onto the data, it presents certain limitations that are worth emphasizing. First, the model captures 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 one 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 impact of the policy, which we do not attempt (in part because of this limitation). Second, our model does not account for any changes in firm profits or government revenue, and only focuses on the welfare of consumers. However, we discuss the effect that the reform had on government revenues and on firm profits by expanding the model in Section 5.5.2. Third, our welfare strategy suffers from the so-called "missing intercept" problem (e.g., see Adão et al., 2019; Wolf, 2023): it may not recover the aggregate, general equilibrium impact of the VAT shock if product groups or stores are somehow interconnected—for example, when there are demand spillovers, or substitution or complementarities across products and store types (Minton & Mulligan, 2024). Addressing this would require us to adopt a fully structural approach, which is beyond the scope of this paper.

In all, while our CES approach imposes a particular structure on household demand, it yields

[34] Note 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 and store types (Atkin et al., 2018).

[35] See Baqaee & Farhi (2019) for a similar argument used in macroeconomics to study the effect of productivity shocks at the national level.

a simple and tractable formula (10) that solely depends on observable expenditure shares and tax-inclusive price changes. In this sense, the 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 of food group g at store type s (independent or chain), α_{gsh}, by the estimated price change of food group g at store type s induced by the VAT change, $d \ln P_{gsh}$. For each decile h, 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.

Note that there are many features of this environment that are not explicitly modeled. For example, tax evasion might be relevant for welfare. However, the welfare effect of these features is accounted for by our model as long as they are captured by price changes. For example, if retailers are evading the VAT, then the price response to VAT changes will be muted (see Appendix Section B.2.1). Therefore, the welfare effect of evasion decisions would indeed be picked up by our reduced-form price-VAT pass-through estimates. In other words, the retail-product-specific price changes and consumption shares of different categories in chain and independent supermarkets would include any behavior that is reflected in prices and quantities (which is true for most economic behavior), without requiring us to model it explicitly, similarly to a "revealed preference mechanism".

5.4 Welfare Estimates

5.4.1 Consumer Welfare Effects

Figure 10 plots the results of estimating this welfare model for the VAT decrease (Panel a) and the VAT increase (Panel b), by income deciles. Note that welfare is measured in real income and thus the y-axis in these figures corresponds to a percentage change in real income. In addition, both of these estimations are expressed relative to the pre-reform situation, and on *impact*, i.e., they only consider the effect of VAT changes one month after they occur.

Panel (a) plots the welfare effects of the VAT decrease as observed and the welfare effect of a counterfactual in which we assume that the VAT cut was fully passed through to prices.[36] We find that the VAT cut resulted in positive welfare impacts and was progressive, benefiting the lowest income decile about three times more than the highest income decile. 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 that of 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 the most.

Panel (b) plots the welfare effect of the VAT increase. Overall, the impact of the VAT increase,

[36] Note that the counterfactual does not account for tax evasion, which would likely lead to a smaller effect (since the price pass-through would be smaller under evasion as shown in Appendix Section B.2.1).

combined with the anti-profiteering measures, was regressive, hurting low-income households more than high-income households. This is mostly due to the fact that low-income households tend to shop at independent stores more frequently, where prices exhibit more asymmetric pass-through, while high-income households shop at supermarket chains, where the anti-profiteering measures mitigated price increases.

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 for those goods that were not subject to the anti-profiteering measures and applying them to the goods that were subject to such 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 about four times more than the highest decile. Given that the observed welfare is substantially higher for all income deciles and somewhat similar across them, the anti-profiteering measures appear to have benefited low-income deciles substantially more. And because our estimates do not account for any of the observed price hysteresis, the effect of the anti-profiteering measures is likely to be underestimated, i.e., we would expect the counterfactual welfare estimates with no anti-profiteering measures to be substantially more negative when accounting for longer-run effects.

5.4.2 Effect on Government Revenue

To assess the effect of the temporary VAT cut on government revenue, we use a macro-level difference-in-differences approach applied to VAT return data. We summarize our approach here and provide more details in Appendix Section C. First, we create a panel dataset of VAT returns for the nine retail sectors reported in the VAT revenue data. Our main outcome of interest is the net VAT liability for each of these sectors, which allows us to estimate the VAT revenue lost due to the VAT cut. One of the nine sectors is the grocery store sector (which pools chain and independent supermarkets together), and so we compare this sector to the remaining eight.

We estimate the effect of the reform on non-taxable sales (i.e., sales of products that are zero-rated), comparing the grocery sector to the remaining eight retail sectors. Appendix Figure D.20 shows a sharp increase in the sale of zero-rated goods, confirming that many goods were reclassified from taxable to non-taxable as a consequence of the temporary VAT cut. However, the sales of all goods, both taxable and zero-rated, did not experience an increase in 2019, which implies that the increase in the sales of zero-rated goods was compensated by a decrease in the sales of taxable goods (see Appendix Figure D.20). This mitigates concerns over "general-equilibrium" effects, such as sales spillovers from the goods treated by the VAT cut to those that were not, or any sizable income effects. Note that there is a substantial increase in total sales in 2020, which is likely due to the onset of the COVID-19 pandemic in March 2020 (i.e., general increase in prices and substitution toward home production).

Overall, we estimate that the temporary VAT cut led to a 30% reduction in VAT remitted by grocery retailers in 2019 relative to other retail sectors (see Appendix Figure D.21). As a benchmark, it is useful to compare this effect to the sharp increase observed in 2020, which is explained by the COVID-19 pandemic, during which grocery stores remained open while other retailers (e.g., electronics, hardware, clothing) were subject to lockdown restrictions. Reassuringly, the two groups exhibit parallel pre-trends and no discernible differences from 2021 onward.

According to official estimates, the tax expenditure (i.e., forgone revenue) of this reform was equivalent to 0.07% of GDP, or 0.21% of GDP when annualized (since the VAT cut only lasted 4.5 months). In comparison, Argentina's conditional cash transfer program (AUH), which is its most important welfare program, cost 0.4 to 0.6% of GDP. Therefore, the VAT cut cost 33% to 50% of the cost of the most important welfare program, which we believe is substantial.

5.5 Model Extensions

We consider several extensions to the welfare model introduced in Section 5.4 to better interpret our estimates. First, we show that the model can be readily generalized beyond CES preferences, as long as preferences remain homothetic. Second, we show how to incorporate income shocks to account for the possibility that supermarket profits are redistributed to individuals. Third, we briefly discuss the potential implications of second-order changes in expenditure shares.

5.5.1 Non-CES Utility Functions

Assuming CES preferences primarily simplifies demand derivation. However, the key expression—the welfare impact of VAT changes as a function of expenditure shares and price changes—follows more generally for any continuous utility function that represents locally non-satiated, convex, and homothetic preferences. Under homotheticity, we can write the indirect utility function as real income:

$$V(p, w) = \frac{w}{P(p)},$$

where $P(p) \equiv \sum_g p_g h_g(p, 1)$ from the expenditure minimization problem (noting that $e(p, u) = P(p) \cdot u$). Using total differentiation, Shephard's Lemma, and the fact that the demand function is homogeneous of degree 1, we get:

$$d \ln V = d \ln w - d \ln P(p), \tag{11}$$

$$d \ln V = d \ln w - \sum_g \left(\frac{h_g(p, 1) p_g}{P(p)} d \ln p_g + \frac{p_g}{P(p)} \frac{\partial h_g(p, 1)}{\partial p} \cdot dp \right) \tag{12}$$

Defining expenditure shares as $\alpha_g(p) \equiv \frac{p_g h_g(p,1)}{\sum_g p_g h_g(p,1)}$ yields

$$d \ln V = d \ln w - \sum_g \alpha_g \cdot d \ln p_g + (\text{second-order terms}) \tag{13}$$

From the envelope theorem, changes in demand caused by price changes have no first-order effect on consumers' *expenditure* as long as we are located at an optimum in the expenditure minimization problem:

$$d \ln V = d \ln w - \sum_g \alpha_g \cdot d \ln p_g, \tag{14}$$

which can be interpreted as a first-order approximation of the true welfare change. Note that with non-homothetic preferences, a VAT cut might have different welfare effects across income levels because spending patterns shift as people get richer or poorer. Homothetic preferences simplify the analysis because expenditure shares depend only on prices and not on income. They allow us to express the expenditure function as $e(p, u) = P(p) \cdot u$, where $P(p)$ is homogeneous of degree one with respect to prices. In practice, we assume homothetic preferences within income deciles, retaining the tractability of homotheticity while allowing for heterogeneous expenditure shares across the income distribution (this is similar to Faber & Fally, 2022, who define five consumer income groups in terms of quintiles of the U.S. income distribution). Hence, the standard first-order welfare formula still applies within each decile. This approach captures most of the relevant heterogeneity while avoiding a full structural model (since non-homothetic preferences would introduce second-order effects such as income elasticities affecting demand). Note that our approach fails if (i) the VAT change induces major shifts in spending patterns across deciles (e.g., if lower-income households dramatically shift consumption toward or away from certain products), or (ii) there are strong income elasticities that drive substitution between necessities and luxuries (non-homothetic preferences would matter even within deciles, requiring a more flexible approach).

5.5.2 Incorporating Income Shocks for Shareholders

Supermarket profits (and losses) due to the temporary VAT cut should, in principle, be redistributed to those consumers who own supermarkets or hold shares in the grocery retail sector, which may affect the welfare estimates we derived in Section 5.4. Given that wealth is typically concentrated at the top of the income distribution, accounting for profits in the welfare estimates may predominantly affect the top-income decile and potentially alter our conclusions about the distributional effects of the temporary VAT cut.

To address this concern, we extend our welfare model to account for profits by incorporating income effects. We allow $d \ln w_h$ to change as a function of firms' profits, which are affected by the reform. This is particularly relevant if consumers hold equity in firms whose profits change due to VAT adjustments. Starting from our general expression (5):

$$d \ln V_h = d \ln w_h - \sum_g \alpha_{gh} d \ln P_{gh}$$

Suppose household income includes labor income and capital income, $w_h = w_h^L + w_h^K$. For consumers who are also firm owners (shareholders), their capital income depends on firm profits:

$$w_h^K = \sum_f \lambda_{hf} \pi_f, \tag{15}$$

where λ_{hf} is the share of firm f's profits owned by household h and π_f is the profit of firm f. Thus, changes in household income can be written as:

$$d \ln w_h = s_h^L \cdot d \ln w_h^L + \sum_f s_h^K \cdot d \ln \pi_f, \tag{16}$$

where $s_h^L = w_h^L / w_h$ and $s_h^K = \lambda_{hf} \pi_f / w_h$ are the income shares. Assuming no direct wage effects ($d \ln w_h^L = 0$) yields:

$$d \ln V_h = \sum_f s_h^K d \ln \pi_f - \sum_g \alpha_{gh} d \ln P_{gh}. \tag{17}$$

This shows that welfare changes depend not only on the price effects but also on how firm profits change and how much of those profits are owned by a given household. Using the profit function:

$$\pi_f = \sum_g (p_g^f - \mu_g) q_g,$$

where p_g^f is the *tax-exclusive* price of product g and μ_g is the marginal cost. Assuming a constant markup, $\mu_g = \frac{\sigma_g - 1}{\sigma_g} p_g^f$, then:

$$\pi_f = \sum_g \frac{p_g^f q_g}{\sigma_g},$$

$$d \ln \pi_f = \sum_g \vartheta_g (d \ln p_g^f + d \ln q_g), \tag{18}$$

where $\vartheta_g = \frac{p_g^f q_g}{\sum_r p_r^f q_r}$ is the initial revenue share of product g in the firm's total revenue.

If a household holds a lot of equity (λ_{hf} is large), their income can increase if supermarkets benefit from the reform. Conversely, if firms suffer losses, shareholder households may face income declines, reducing their welfare beyond just the price effects. This matters for heterogeneity—higher-income households with more capital may experience different effects than wage-earning households.

Estimating equation (18) is difficult because it requires estimating changes in prices and quantities of each good considered and assigning these estimates to consumers in proportion to how much of the supermarket they own. Rather than implementing this approach, we show that changes in supermarket profits are unlikely to qualitatively matter for welfare because supermarket ownership

is limited, even for the top income decile. This might be due to the fact that the wealth distribution in Argentina is very skewed or because several of the supermarket chains are foreign-owned (such as Carrefour and Walmart).

We use data from the household expenditure survey described above to estimate the ownership of "retail and wholesale" firms by income decile. The dataset does not contain a specific grocery retail category and so the estimates are likely to be an upper bound. We find that fewer than 2% of individuals in the top decile own any firms in the "retail and wholesale" sector (Appendix Figure D.22a).

Similarly, we plot the distribution of business and capital income as a share of total income in Appendix Figure D.22b. Even for the top income decile, only 4.5% of total income is derived from business or capital ownership.[37] These estimates are also an upper bound, since they include ownership of any business or capital and are not restricted to grocery stores. Overall, this evidence mitigates the concern that supermarket profits are likely to qualitatively affect our conclusions about the distributional effects of the temporary VAT cut and anti-profiteering measures.

5.5.3 Changes in Expenditure Shares

In this Section, we show that changes in expenditure shares are second-order. Let $\alpha_{gh}(p)$ denote the expenditure share. At the initial prices p_0, the expenditure share $\alpha_{gh}(p_0)$ is known. When prices change due to the VAT reform, the expenditure share may shift. But this shift can be written as a Taylor expansion around p_0

$$\alpha_{gh}(p) = \alpha_{gh}(p_0) + \frac{\partial \alpha_{gh}}{\partial p}\Delta p + O(\Delta p^2). \tag{19}$$

Thus, the initial expenditure shares are first order while changes in expenditure shares are second order. This is because the first derivative of the indirect utility function depends on the initial shares, and the change in welfare comes from the initial level of expenditure shares, not their change. Any changes in α_{gh} caused by price changes will contribute to higher-order terms, which are second-order effects and are thus small.

6 Conclusion

Our findings have policy implications for the ubiquitous temporary VAT cuts implemented in LMICs. First, VAT cuts without anti-profiteering measures improve household welfare, while they are 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

[37] These figures are consistent with what researchers in the top incomes literature have documented for Argentina (see Alvaredo, 2010).

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 and price monitoring can dampen this asymmetric effect. 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. Third, governments can influence tax incidence by using legislation, when appropriate monitoring systems are in place. This ability can be useful: by shifting the burden of a tax from one party to another, governments can generate important distributional effects. However, there are also pitfalls to this approach: while price monitoring can be implemented relatively easily, firm-level monitoring is challenging. Shifting tax incidence without knowing how it affects firm-level outcomes can have unintended distributional effects.

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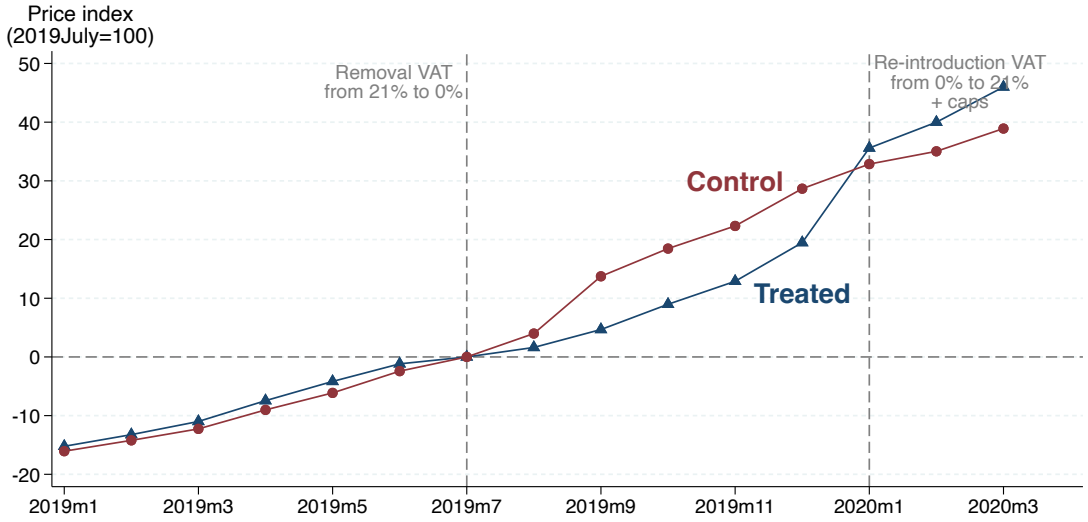
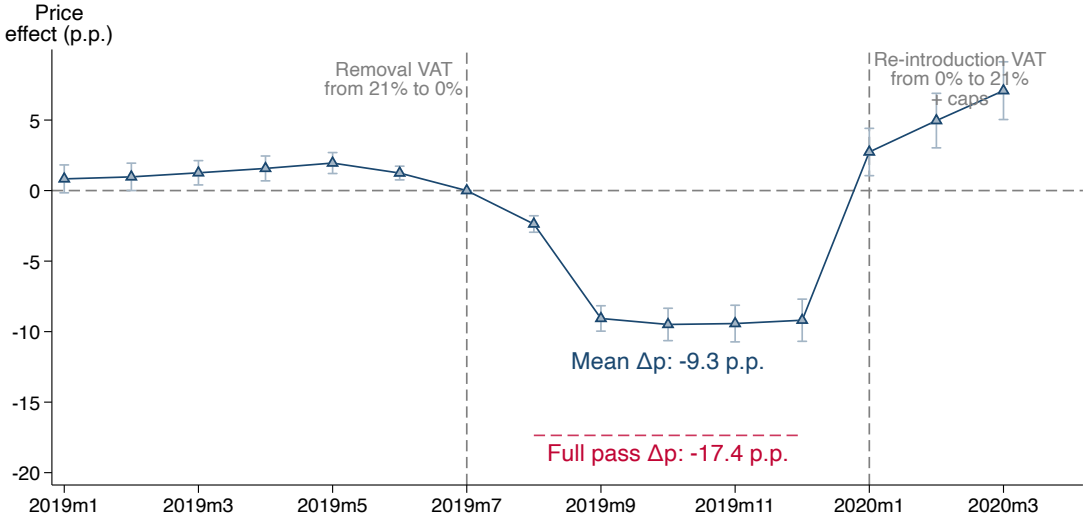
Figures and Tables

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

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



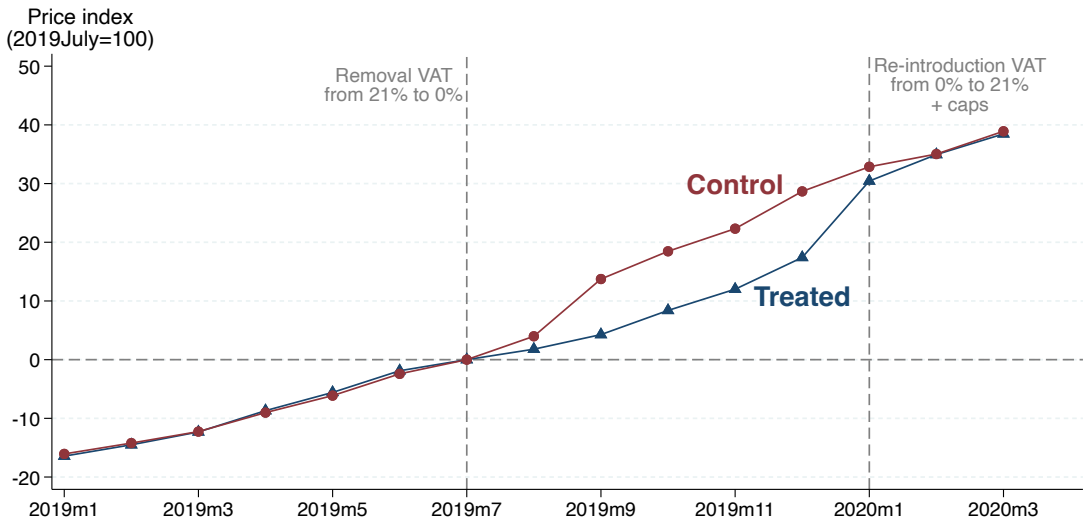
(b) **Difference-in-Differences Estimates (no caps)**



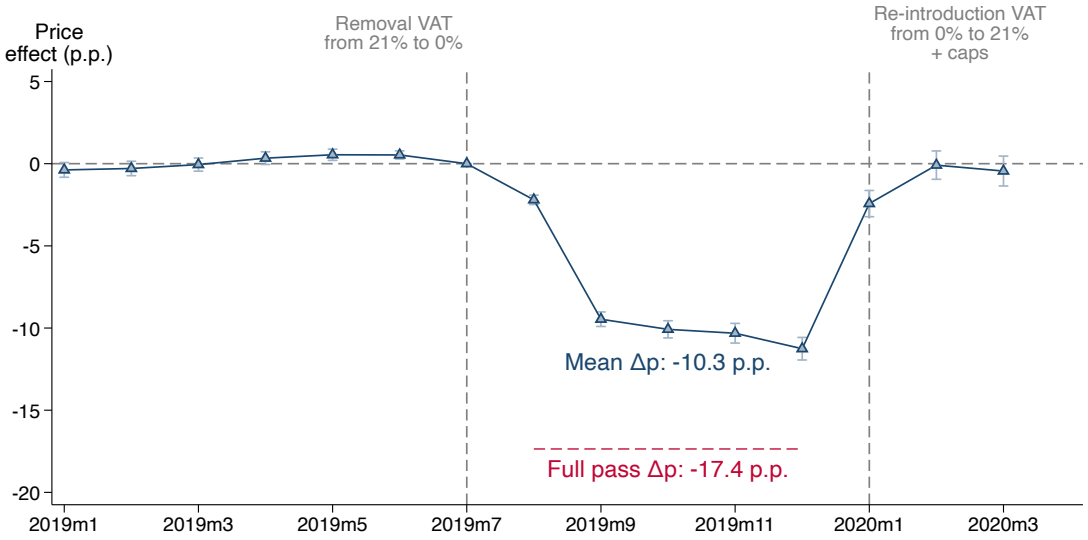
Notes: This figure shows the price pass-through of the VAT holiday for both chain and independent supermarkets using the goods that were not subject to the price caps. We group barcodes into treatment and control groups as shown in Table 1. The top panel plots the unconditional mean of the average price 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 cut 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 on allowed price increases. The red dashed line indicates the hypothetical scenario of 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)**



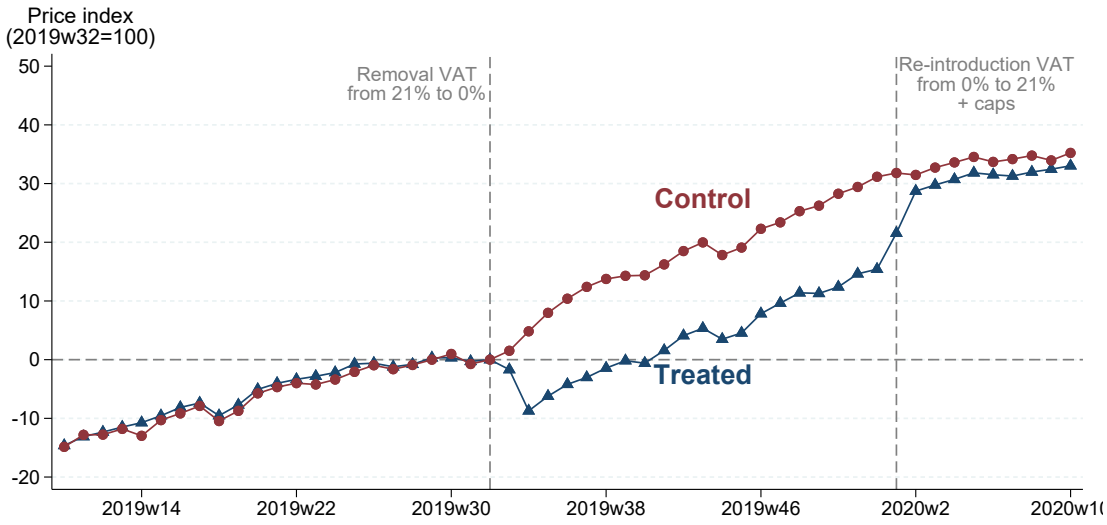
(b) **Difference-in-Differences Estimates (caps)**



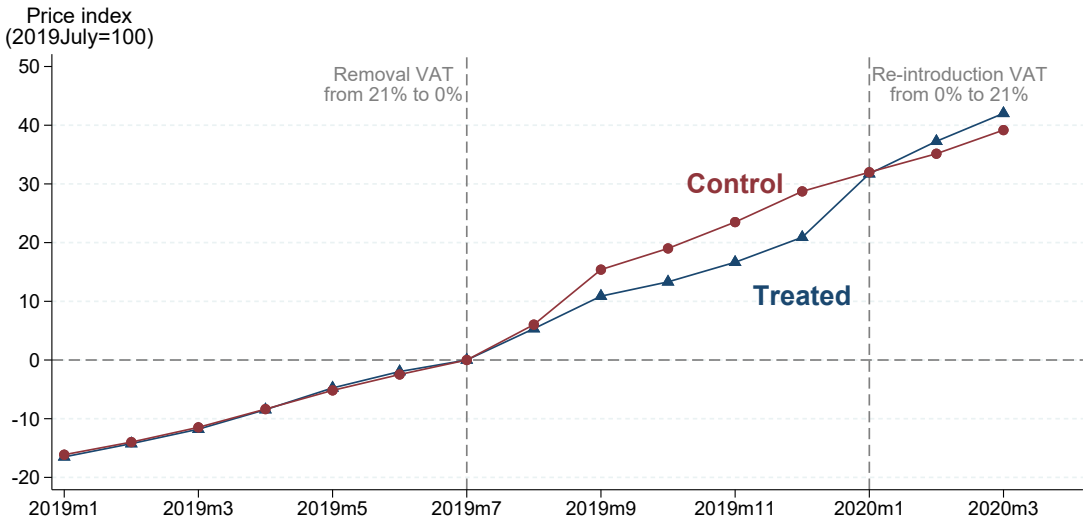
Notes: This figure shows the price levels and price pass-through of the VAT holiday pooling together chain and independent supermarkets. We group barcodes into treatment and control as shown in Table 1 (the former includes only barcodes that were subject to the price caps). The top panel plots the unconditional 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 reduced 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 on allowed price increases. The red dashed line indicates the hypothetical scenario of 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**



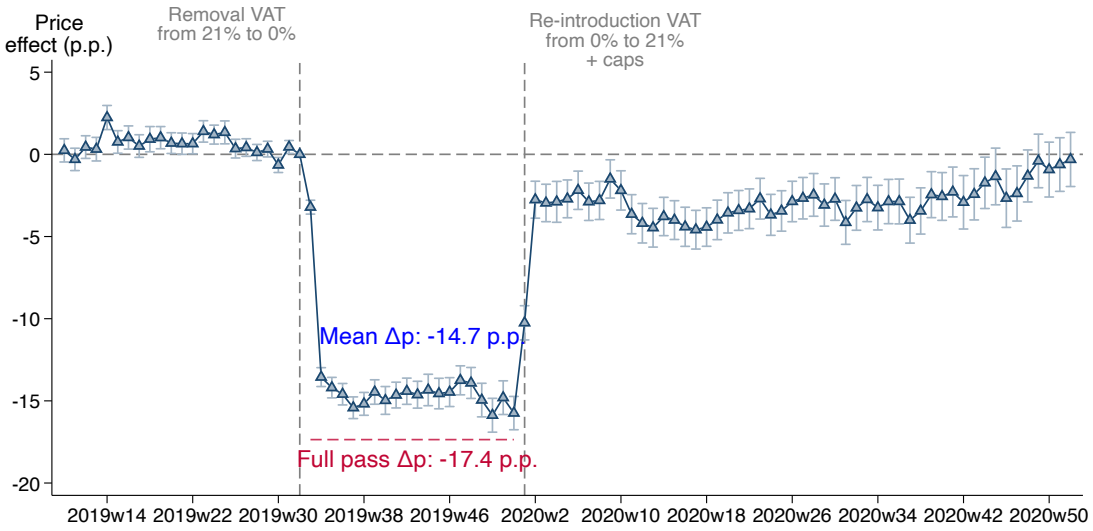
(b) **Independent stores**



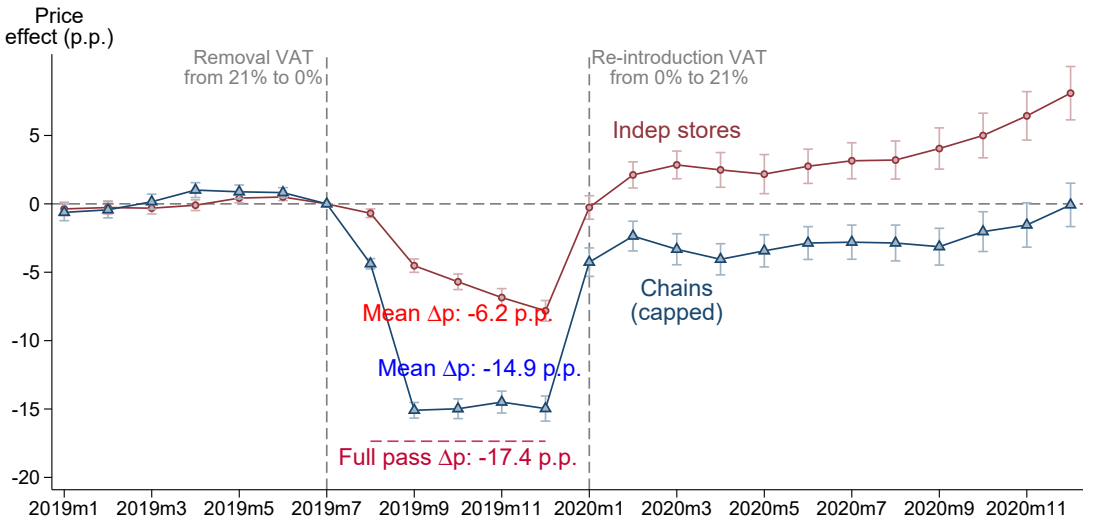
Notes: This figure plots the unconditional mean of the average price level for control and treatment food products separately before and after the VAT cut and its subsequent repeal. In each case, we normalize every barcode series to 100 in the week/month before the VAT cut was implemented. Panel (a)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



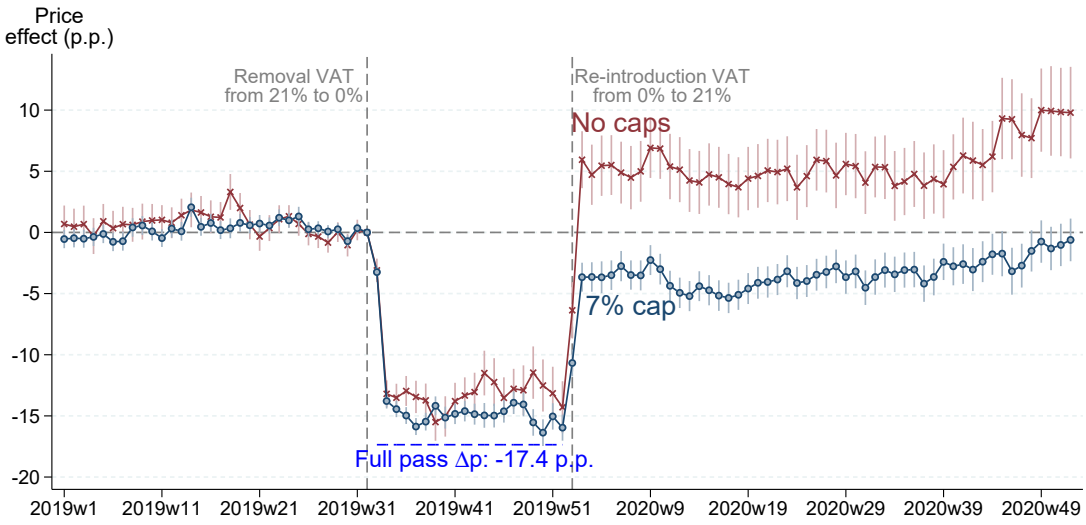
(b) **Chains and Independent stores (monthly data)**



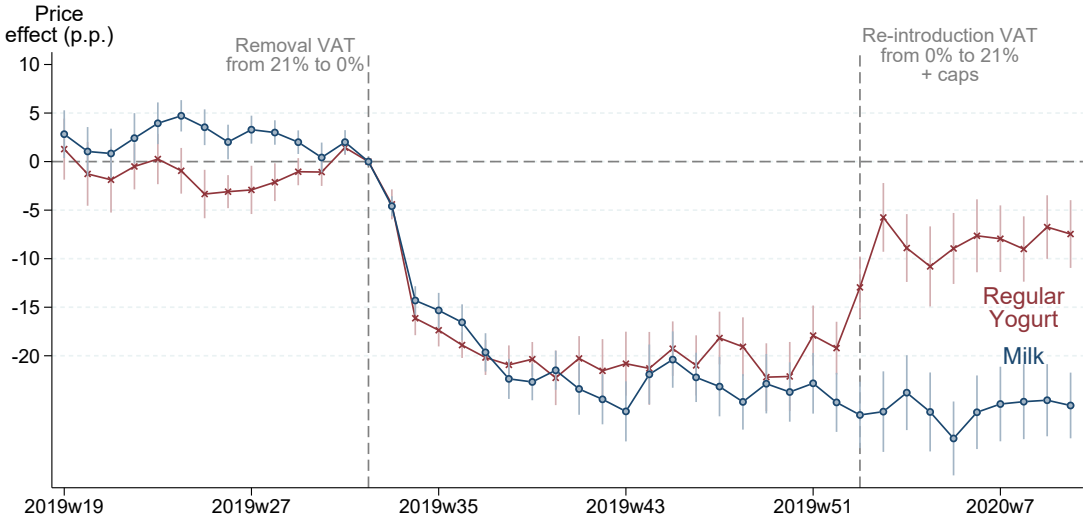
Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) before and after the VAT cut and its subsequent repeal. We group barcodes into treatment and control as shown in Table 1. The dependent variable is the price of each barcode normalized to 100 in the week or month before the VAT was cut. Panel (a) shows the pass-through rate for chains using weekly data. Panel (b) presents results for independent supermarkets using monthly data. For comparison, in Panel (b) we also add the effect for supermarket chains by collapsing the weekly data to the month level. The first vertical dashed line indicates the time when the VAT was reduced 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 on allowed price increases. The red dashed line indicates the hypothetical scenario of 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**

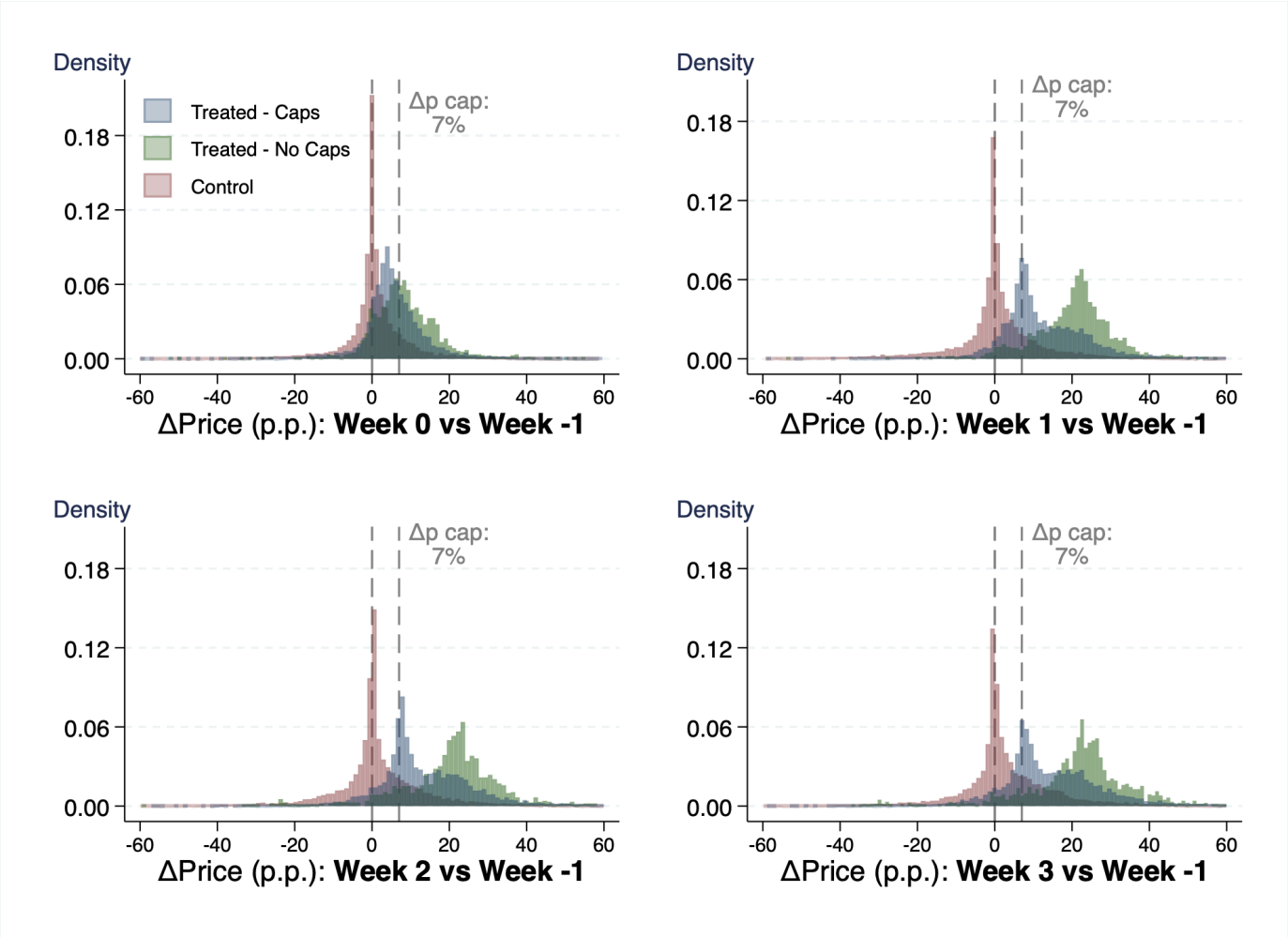


(b) **0% cap (milk) versus 7% cap (yogurt)**



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 on their price increase (i.e., those 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 include the effect for regular yogurt that faced the 7% price increase cap. The first vertical dashed line indicates the time when the VAT was reduced 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 on allowed price increases.

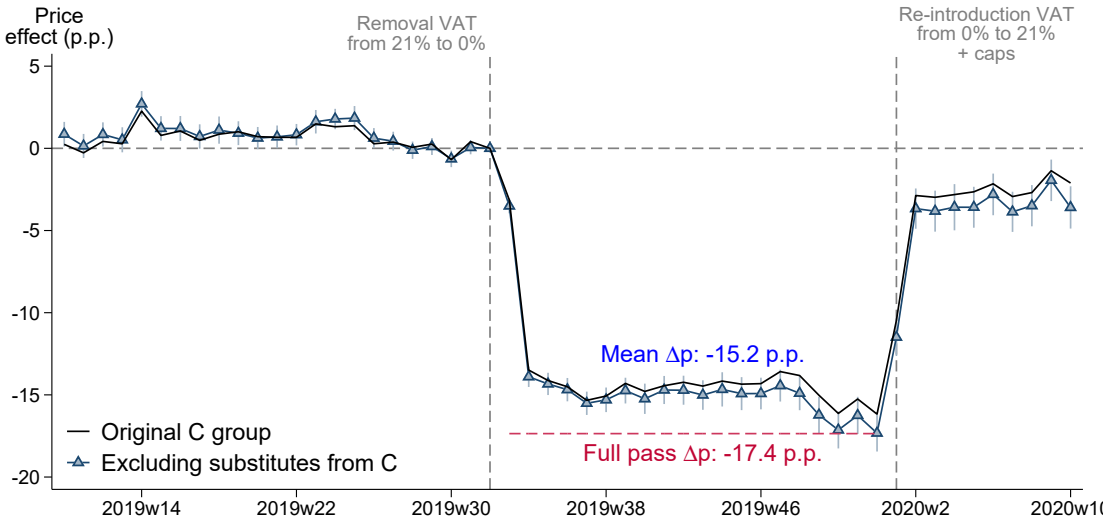
Figure 6: Distribution of Price Changes Following the VAT Increase



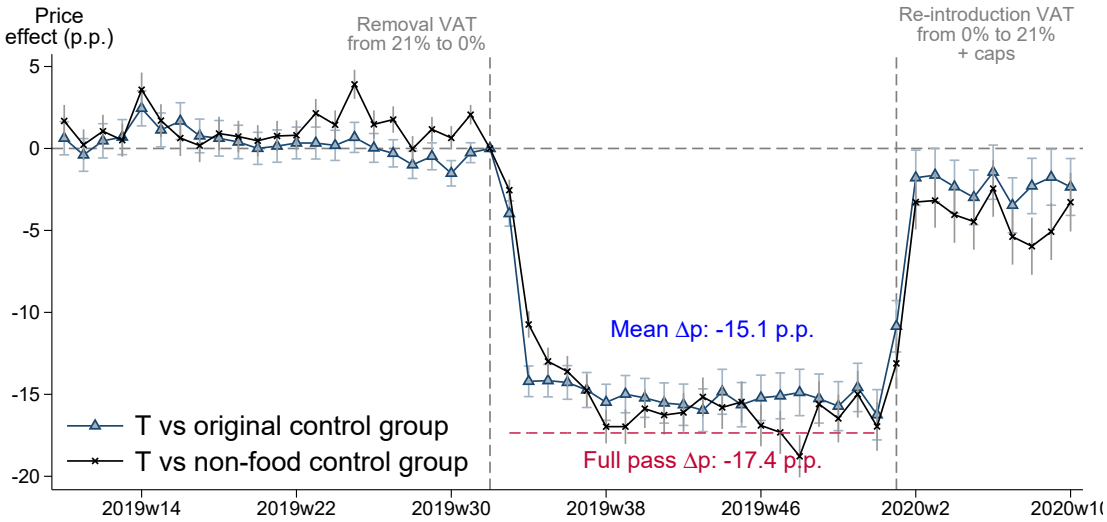
Notes: These figures show the distribution of price changes (at the barcode level) before and after the VAT increase at different time horizons. Among the treated goods, we exclude milk, which was subject to a 0% price increase cap. The reference week ($week - 1$), refers to the last week of 2019 (the last week before the VAT rate of 21% was reintroduced). $Week$ 0 denotes the first week with a rate of 21%, $Week$ 2 the second, and so on. In each chart, we include two vertical dashed lines to illustrate the situation of barcodes with no price change ($\Delta 0\%$) and of those with changes of equal size to the cap ($\Delta 7\%$). Longer horizons (weeks 4 through 11) are included in Appendix Figures D.12 and D.13.

Figure 7: Do Substitution Effects Bias the Pass-Through Estimates?

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

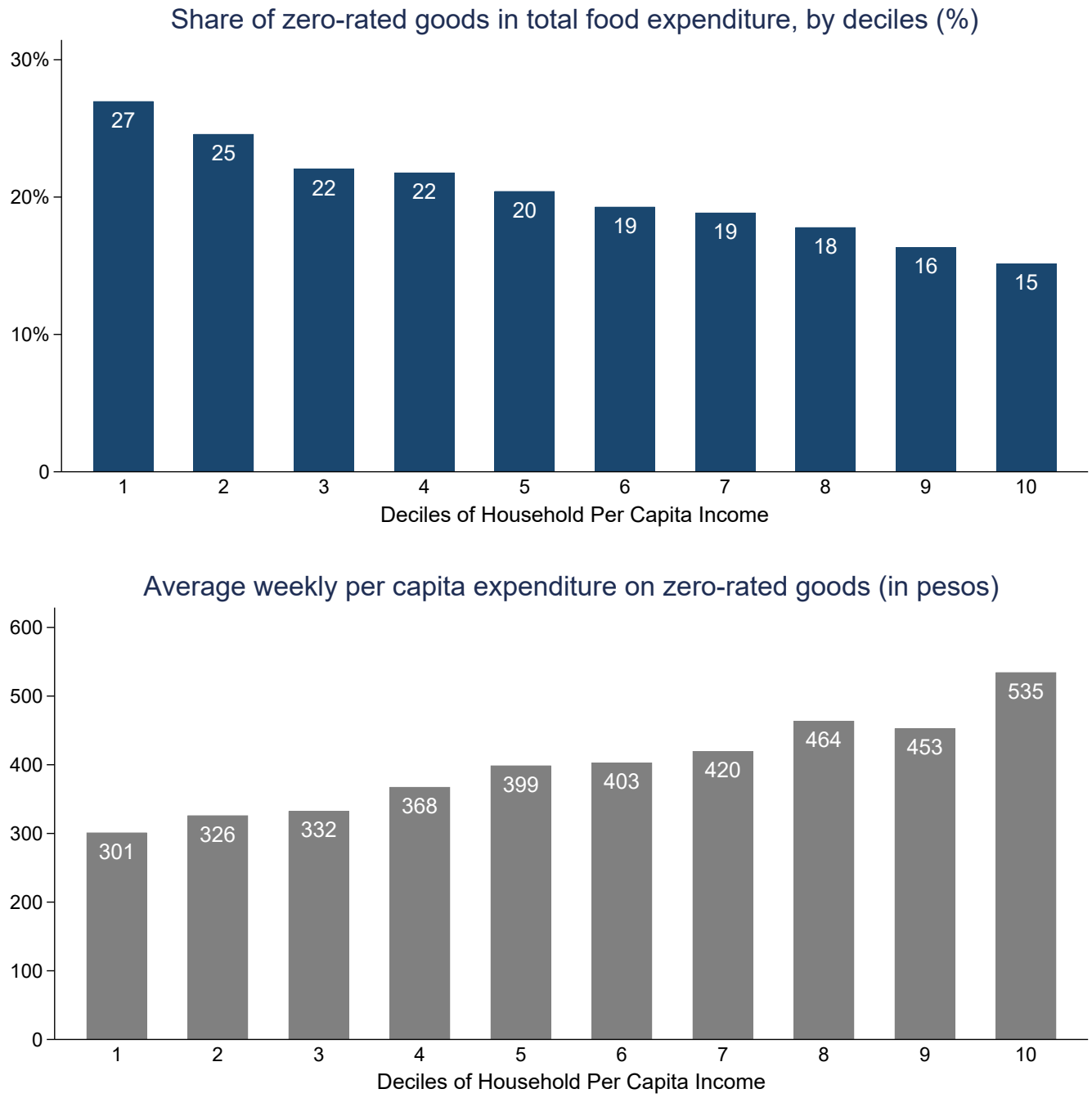


(b) **Using food and non-food products in the control group (Periferia sample)**



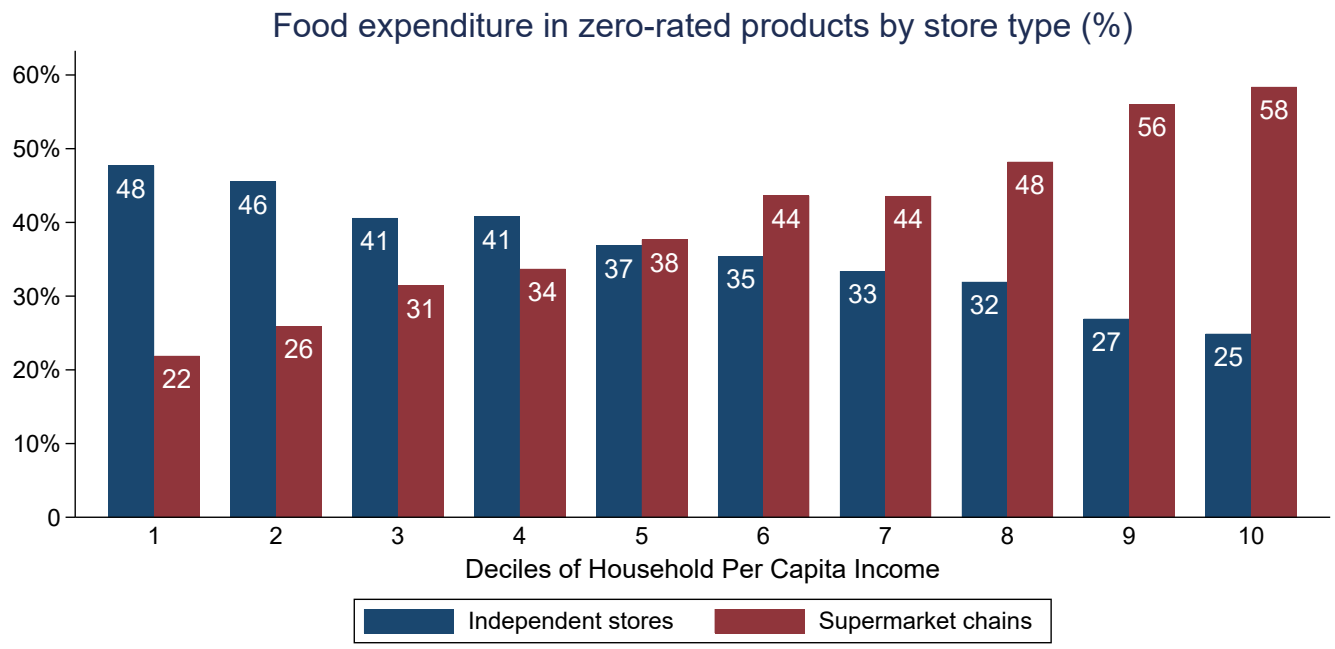
Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) on prices. Panel (a): The black line corresponds to the estimation using the original treatment and control groups as shown in Table 1. The blue line uses the same treatment group and an alternative 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 of Periferia because non-food categories were only purchased for that region. The red dashed line indicates the hypothetical scenario of full pass-through to prices [(1-1.21)/1.21 x 100 = -17.4%]. Overall, both figures suggest that substitution is not a big concern in our setting.

Figure 8: Share of Treated Products in Total Food Expenditure and Weekly Expenditure



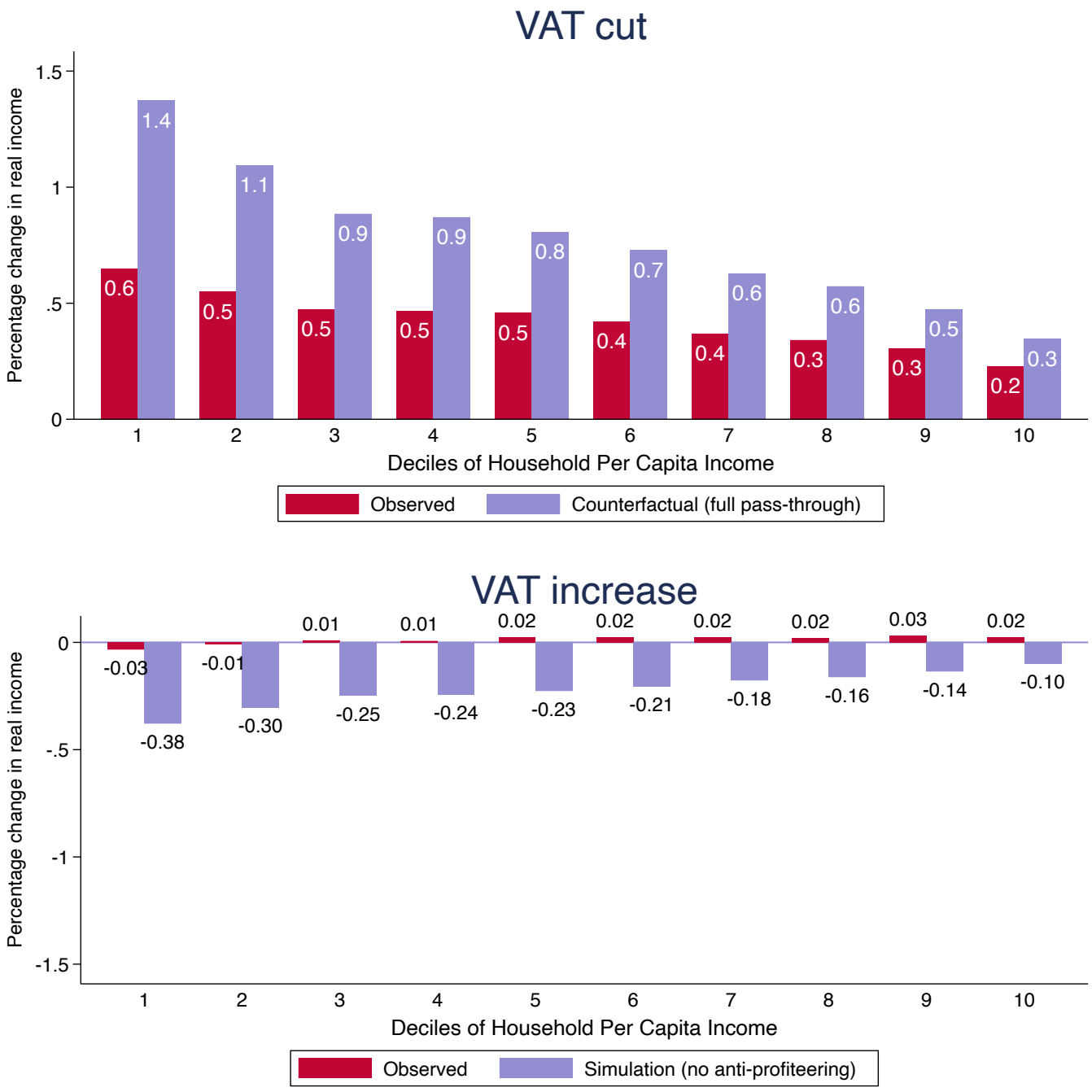
Notes: The top panel displays the share of zero-rated goods in total food expenditure. The national average is 20%. The bottom panel shows the average per capita household expenditure on zero-rated goods (in pesos) for the reference week of the survey.
Source: authors' calculations using the 2017/2018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

Figure 9: Where do Low- and High-Income Households Shop?



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 on 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 10: Welfare Estimates



Notes: These figures show the results of estimating our welfare model (section 5) separately for the VAT decrease in Panel (a) and the VAT increase in Panel (b). In addition to estimating the welfare impact of the policies as implemented, in Panel (a) we also include the welfare effect for a counterfactual full-pass through scenario; and in Panel (b), we estimate the effect of removing the anti-profiteering measures. Both of these effects are estimated relative to the pre-reform situation, and on *impact*, i.e., they only consider the effect of VAT changes one month after they occur. Note that welfare is measured in real income and thus the y-axis corresponds to the percentage change in real income.

Table 1: VAT Cut Treatment and Control Groups

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 how the data is split 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 the data.

Table 2: Anti-Profiteering Measures

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 rate was reverted back to its pre-VAT-holiday level of 21%. This mandate was announced on December 31 and enforced with the price monitoring app. "No cap" flags the uncapped food products with flexible prices. Capped products were permitted to increase their prices by up to $X\%$ of the December 31 price (7% for most of the goods). According to reports in newspapers, there was a contentious six-hour meeting on December 31 involving the government, producers, and supermarkets to discuss how the VAT increase would be reflected in prices. For more details, see La Nación's coverage here.

A Anti-Profiteering Measures in Other Countries

This section documents the use of anti-profiteering policies in other developing countries during consumption tax reforms. While these measures vary in design and enforcement, they share the goal of ensuring that tax changes are reflected in consumer prices rather than captured as excess profit by firms. This non-exhaustive overview places the Argentine experience within a broader international landscape.

India. India enacted a comprehensive anti-profiteering framework as part of its Goods and Services Tax (GST) reform in 2017. Section 171 of the CGST Act requires that any reduction in tax rates or new input tax credits be passed on to consumers through commensurate price reductions. To enforce this, India established the National Anti-Profiteering Authority (NAA), which investigates complaints and can impose penalties. The law does not define "profiteering" precisely and leaves room for case-by-case interpretation by the Competition Commission of India. High-profile cases have targeted firms in sectors ranging from FMCGs to restaurants, where prices did not drop despite tax reductions.

Malaysia. Malaysia introduced the Price Control and Anti-Profiteering Act in 2011, ahead of its GST implementation in 2015. The Act limited retailers from increasing their net profit margins beyond a regulated baseline during the GST transition. When GST was later abolished in 2018 and replaced by a Sales and Services Tax (SST), the same framework was used to monitor that prices declined accordingly. Enforcement was carried out by the Ministry of Domestic Trade, with fines and inspections used to deter violations (e.g., see here). This Act served as a central tool to manage price behavior in response to both tax increases and reductions.

Bangladesh. Bangladesh operates a Maximum Retail Price (MRP) regime that functions as a *de facto* anti-profiteering mechanism. All packaged goods must display an MRP that includes VAT, and retailers are prohibited from charging above it. When VAT structures were modified in 2025, the National Board of Revenue clarified that supermarkets could not add VAT on top of MRP, reinforcing that any tax changes must be absorbed within the existing price cap. In regulated sectors such as pharmaceuticals, MRPs are determined by authorities based on import prices and cost structures, ensuring that tax changes are passed on to consumers.

Indonesia. Indonesia uses a system of government-declared price ceilings known as Harga Eceran Tertinggi (HET) for essential goods like rice, sugar, and cooking oil. These ceilings cap final consumer prices, including taxes. During VAT increases (e.g., from 10% to 11% in 2022), the government adjusted HETs cautiously or temporarily absorbed part of the tax increase to avoid sharp retail price hikes. The HET mechanism thus attempts to limit the pass-through of tax increases and ensure that tax reductions or exemptions result in lower consumer prices, especially for subsidized goods.

Sri Lanka. Sri Lanka applies maximum retail price (MRP) controls on a range of essential goods, including food items, bottled water, and cooking gas. These MRPs are set by the Consumer Affairs Authority and are legally binding, limiting how much retailers can charge regardless of changes in input costs or taxes. When consumption tax changes occur—such as adjustments to VAT or import

duties—the government can use MRP regulations to delay or limit price increases. Conversely, when tax reductions are enacted, MRPs may be revised downward or retailers are expected to adjust prices within the existing cap. MRPs are designed to curb opportunistic pricing and ensure that tax relief reaches consumers.

Philippines. The Philippines uses Suggested Retail Prices (SRPs) as a key consumer protection and price stabilization tool. The Department of Trade and Industry (DTI) regularly issues and updates SRPs for a list of basic food necessities and commodities. While not legally binding like MRPs, SRPs serve as reference prices and retailers are strongly discouraged from exceeding them, especially during periods of economic distress or fiscal reform. In the context of VAT changes, SRPs act as a soft ceiling, and enforcement agencies can conduct market inspections and penalize unjustified overpricing.

South Africa. When South Africa increased its VAT rate from 14% to 15% in 2018, the government did not enact a formal anti-profiteering law but relied on public warnings and oversight by the Competition Commission. Consumers were encouraged to report unjustified price increases, particularly on zero-rated goods. The government later expanded the list of VAT-exempt items to mitigate regressivity and expected retailers to fully pass through the tax relief. Although no legal caps were implemented, the policy environment emphasized voluntary compliance and reputational enforcement.

Other Examples. Several other LMICs have used anti-profiteering approaches informally or through general price regulations:

- **Kenya:** During a temporary VAT cut in 2020, the government warned retailers to adjust prices accordingly. Enforcement was left to the competition authority and consumer pressure.
- **Nepal:** Introduced mandatory MRP labeling to combat unfair pricing during tax and cost shifts, facilitating consumer enforcement.
- **Pakistan:** Fixes retail prices in sensitive sectors such as pharmaceuticals and fuel, using general price controls to manage the pass-through of taxes.

While the institutional mechanisms vary, many developing countries have implemented anti-profiteering measures—either explicitly through laws and monitoring agencies or implicitly through statutory price caps like MRPs or HETs. These tools constrain how tax changes are passed through to consumers and help ensure that the intended effects of consumption tax reforms are realized in retail prices. The Argentine case is unique in targeting the *repeal* of a tax cut with formal price caps and provides rare empirical evidence on compliance, placing it among a limited set of documented international experiences with anti-profiteering enforcement when the VAT is reinstated.

B What Explains the Observed Price Dynamics

B.1 Macro literature

There are two main macro pricing models: Calvo models and menu cost models. We describe each below and discuss how well they fit our empirical findings of asymmetric pass-through and price

hysteresis.

B.1.1 Calvo Model

Denote by p_t the log aggregate price level in the economy, and assume that each firm adjusts prices with a probability $1-\alpha$ in every period. Thus

$$p_t = (1-\alpha)p_{it}^* + \alpha p_{t-1} \tag{20}$$

where p_{it}^* is the log price of those firms that change prices in period t.

Assume that log nominal marginal costs mc_t are equal to log nominal wages w_t, i.e., firms produce using labor as the only variable input and a linear production technology. Denote by β the per-period discount factor and assume that the demand curve is given by $y_{it} - y_t = -\theta(p_{it} - p_t)$ where y_{it} denotes log demand for product i, y_t denotes log real aggregate output and p_{it} denotes the log real price of product i. One can show that:

$$p_{it}^* = (1-\alpha\beta)\sum_{j=0}^{\infty}(\alpha\beta)^j E_t mc_{t+j} \tag{21}$$

i.e., firms will set a price that will be a discounted average of the future marginal costs of the periods when prices will remain fixed.

Denote by m_t log nominal output, thus $m_t = y_t + p_t$ and assume that $m_t = \nu + m_{t-1} + \epsilon_t$, i.e., nominal output follows a random walk with drift. Assume that $\nu = 0$ for simplicity, i.e., there is no average growth in the economy. Assume that household utility is given by $c_t - L_t$ where c_t is log consumption and L_t is labor. This utility function implies that labor supply is perfectly inelastic, thus $c_t = w_t - p_t$. Since $mc_t = w_t$ and $m_t = y_t + p_t = c_t + p_t = (w_t - p_t) + p_t = w_t = mc_t$, and, because $m_t = m_{t-1} + \epsilon_t$, it follows that $E_t mc_{t+j} = E_t m_{t+j} = m_t$. Thus $p_{it}^* = (1-\alpha\beta)m_t \sum_{j=0}^{\infty}(\alpha\beta)^j = m_t$. Therefore:

$$p_t = (1-\alpha)m_t + \alpha p_{t-1} \tag{22}$$

This equation governs the evolution of log aggregate nominal prices, which respond sluggishly to aggregate shocks because they depend on previous prices. How sluggish prices are depends on α, i.e., the proportion of firms that keep prices unchanged in every period.

It is relatively straightforward to show that the Calvo pricing model does not fit our empirical findings well. Under a Calvo model, aggregate prices should adjust in a step-wise fashion. The intuition for this is easy to grasp: the Calvo model predicts that only a portion α of firms will adjust prices in every period, and so the impact on aggregate prices should be α times the optimal price in the first period, 2α in the second period etc. This should lead to a step-wise decrease in prices following the VAT cut and a step-wise increase in prices following the VAT increase. Because we observe immediate adjustments to prices for the VAT decrease and increase, Calvo pricing models are unlikely to match our empirical findings.

B.1.2 Menu Cost Models

The menu cost model we describe is based on Caplin & Spulber (1987). Using the same notation as above, assume that $m_t = y_t + p_t$ is always increasing (i.e., there is relatively high inflation).

Contrary to the Calvo model, we assume that firms are able to change prices in every period (i.e., $\alpha = 0$) but they pay a fixed cost whenever they change prices. These assumptions imply that firms will follow a so-called sS policy: they change their relative prices by $S - s$ whenever relative prices drop below a trigger s. In this model, firms that change prices are not selected at random, as is the case in the Calvo model, but are selected to be the ones with the strongest incentive to change prices. As a consequence, these firms will change prices by a larger magnitude than in the Calvo model, leading to higher responses of aggregate price levels, which is often referred to as the "selection effect" (Golosov & Lucas (2007)).

The main intuition for why menu cost models may predict asymmetric pass-through rates of VAT changes to prices is that menu costs can cause firms to bundle several price changes together. This relies on the (standard) assumption that menu costs are a fixed cost, i.e., it is as costly to change prices by a little or by a lot. In the case of the VAT cut, firms bundle several future inflation-induced price increases with the VAT cut-induced price decrease. In the case of the VAT increase, firms may bundle the increase in price due to the VAT increase with future price increases due to inflation, thus saving on future menu costs in both cases.

We derive a very simple menu cost model to illustrate these patterns. Assume that the discount rate is set to zero, that there is constant inflation π at every period and that firms are price setters. Denote by p_t posted prices and p_t^* optimal prices in period t. Denote by $f_t(p_t - p_t^*)$ a loss function equal to the cost of having posted prices deviate from optimal prices and μ the menu cost, which is incurred whenever prices are changed (by any amount). A key assumption is that $f(\pi) < \mu$, i.e., for a given period, it is less costly to adjust prices to account for inflation than to pay the menu cost. It is also the case that there exists c such that $\sum_t^{t+c} f_t(\pi) > \mu$, i.e., that keeping prices fixed for more than a number of periods c, is more costly than incurring the menu cost μ.

Denote by $t = d$ the period of the VAT decrease and $t = i$ the period of the VAT increase. Firms will adjust prices in period t if $\sum_{t=i}^{T} f_i(p_{t-1} - p_i^*) > \mu$, i.e., if keeping period t to T posted prices equal to those of period $t - 1$ is more costly than incurring the menu cost μ.

Following the VAT decrease, firms will fully adjust prices if $f_d(p_{d-1} - p^*d) > \mu$. They decrease prices to account for the VAT cut but also bundle some of the future price increases due to inflation. Therefore, they decrease prices by the amount needed to account for the VAT decrease minus πT^*, where T^* is given by $\sum_{t=i}^{T^*} f_i(p_{t-1} - p_i^*) = \mu$. And they hold prices fixed for T^* periods after that.

Inflation acts in an asymmetric way on incentives to change prices for the VAT increase and decrease, which is what causes the asymmetry in price adjustments. Following the VAT increase, firms will adjust prices to account for the VAT increase and also bundle some future price increases due to inflation. The number of periods that are bundled into the price increase due to the VAT increase is also given by T^* such that $\sum_{t=i}^{T^*} f_i(p_{t-1} - p_i^*) = \mu$. Therefore, firms are increasing prices by the amount of VAT increase pass-through and also by πT^*. Firms pass through the VAT increase and future price adjustments due to inflation, in order to save on menu costs. Therefore, prices will over-shoot above those of the control group by πT^* and will remain unchanged for a number of periods T^*. This is the over-shooting we observe in our empirical evidence, and the hysteresis should last T^* periods.

While this simple model predicts that prices will respond asymmetrically to the VAT decrease and increase, and that there will be some degree of price hysteresis following the VAT increase, there are several predictions that do not match our empirical findings.

The first one is that the model predicts step-wise adjustments in prices as a response to inflation: outside of the VAT change periods, firms will also bundle future inflation changes into one price

change. This predicts that prices adjust every T^* period rather then every period. This step-wise function can easily be smoothed by assuming that firms face a distribution of idiosyncratic shocks (in addition to the two VAT changes), leading a set of firms to increase prices in every period.

The second prediction that does not match our empirical findings is that prices in the treatment group should converge to those of the control group after T^* periods. This prediction is clearly inconsistent with our findings, which show that prices in the treatment group remain higher than those of the control group for at least a year. T^* equal to one year would imply unrealistically large menu costs. There is no "easy fix" for this assumption, and to our knowledge, more sophisticated models cannot account for this degree of hysteresis. Therefore, this finding may require a new class of models, such as coordination failure-based models (Cooper & John (1988) and Ball & Romer (1991)), which generate significantly more price rigidity.

B.2 Micro literature (Evasion, Competition and Pricing Strategies)

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.

Here, we address 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.

B.2.1 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. (2023) 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. (2023) consider. First, Argentina is a more advanced economy than the set of countries that Bachas et al. (2023) 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.[38] This share is substantially higher in the countries considered by Bachas et al. (2023) and averages more than 30%. Second, the independent supermarkets we consider would actually be classified as modern stores by Bachas et al. (2023) 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: $q^*(p)$.

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.

B.2.2 Competition

Differences in pass-through may be due to different levels of competition in chain and independent supermarkets.[39] 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.

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

[39] Another market structure explanation could be that there are differences in vertical integration in chain and independent supermarkets (See Bajo-Buenestado & Borrella-Mas (2022), Fuest et al. (2024), Gopinath & Itskhoki (2010) and Hellerstein & Villas-Boas (2010). Such differences could be due, in theory, to markups attenuating the effect of changes in marginal costs (see Hong & Li (2017)).

First, they focus on a particular and specific market (gasoline). Second, they look at a specific geographical setting (islands).

We show in Appendix Figure D.23 that competition indeed affects pass-through rates, in a minor way. Appendix Figure D.23 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, thus affecting pass-through rates (Weyl & Fabinger (2013)). This is indeed what Appendix Figure D.23 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.

B.2.3 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 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 Argentine peso experienced a large and sudden devaluation on August 30th, 2018, causing a 24% increase in the exchange rate of the peso against the US dollar, which is plotted in Appendix Figure D.2. Moreover, the cumulative depreciation between January and September 2018 was 100% and the exchange rate remained relatively stable thereafter. As a consequence, supermarkets had to adjust their prices. In Appendix Figure D.24, 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 is 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, Appendix Figure D.16b 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 sharp differential response of chain and independent supermarkets to the VAT cut, the figure

suggests that supermarkets responded similarly to the currency depreciation shock.[40] 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.

C Effect of the VAT Cut on Government Revenue

To assess the macro revenue consequences of the VAT cut, we further use semi-aggregate administrative data from the tax administration's Statistical Yearbooks and apply a macro diff-in-diffs approach. These tax tabulations—derived from VAT returns F.731 and F.2002—contain detailed 3-digit sector-level information by year, including the number of tax returns per sector, input VAT paid on purchases, output VAT charged on sales, and the volume of taxable and non-taxable sales. Appendix Table A5 presents an excerpt for 2019.

We compiled a panel covering nine retail sub-sectors from 2015 to 2023 (see Appendix Table A5 and here for a full list of sub-sectors in the data). For each sector, we construct the net VAT liability by subtracting the total input VAT from the total output VAT. This is our main outcome in the diff-in-diffs analysis below.

Two remarkable features of the data are worth highlighting. First, among the nine sectors, we observe grocery stores separately (sector 471 in Appendix Table A5). This comprises both chain supermarkets and independent grocery stores affected by the policy, allowing us to accurately isolate tax revenue effects arising from the treated sector. Second, firms selling zero-rated foodstuffs were required to report these sales under "non-taxable and exempt operations," a separate field in the tax return recorded in the yearbooks (column 3 of Appendix Table A5). This distinction enables us to verify the mechanism at play: any decline in VAT revenue should correspond to an increase in reported non-taxable sales.

Using these data, we estimate a diff-in-diffs regression comparing grocery stores (sub-sector 471) with the remaining retailers. Note that this mirrors the comparison we would conduct if we had firm-level tax microdata, albeit using sector-aggregated volumes. The main drawbacks of our approach are reduced statistical power—due to sector-level rather than firm-level data—and limited control flexibility (e.g., we cannot include firm-fixed effects). Our outcomes include non-taxable sales, total sales, and net VAT revenue. We exclude two subsectors: (i) subsector 472 (Retail sale of food, beverages, and tobacco products in specialized stores), which contains a mix of treated and mostly untreated firms, and (ii) subsector 478 (Retail sales in mobile stalls and markets), a small, largely informal sector with about 450 returns that slightly distort the trends.

Two key results emerge from this exercise. First, Figure D.20 shows a sharp, statistically significant temporary increase in non-taxable sales reported in 2019 (blue line).[41] As mentioned before, this reflects a mechanical response: firms reclassified the (now) zero-rated sales in the designated tax return field. Notably, the macro data capture this pattern clearly. The figure also presents the diff-in-diffs effect on total sales (red line). The null effect in 2019 confirms that firms

[40] Figure D.25 extends the analysis to encompass the full period from 2018 to 2020, using July 2019 as the reference month. The fluctuations observed in 2018 are primarily attributed to the depreciation of the peso (Appendix Figure D.16a). Importantly, this event appears to have had a similar impact on both chain and independent stores. Furthermore, during the initial eight months of 2019—a period characterized by a fairly stable exchange rate—both treated and control products exhibit parallel trends in these stores. This evidence mitigates the concern that the observed differences in how chain and independent stores responded during the VAT holiday were due to the potential varying impact of the peso's depreciation on their profit margins.

[41] The share of non-taxable sales over total sales for grocery stores was 2.4% in 2018.

merely shifted sales from taxable to non-taxable categories, with no significant general equilibrium effects. The positive sales effect in 2020 reflects the pandemic, during which most retail sub-sectors saw declines while grocery stores remained open. Reassuringly, both outcomes exhibit parallel pre-trends before the reform, strengthening the credibility of our exercise.

Second, Figure D.21 reveals a substantial 30% decline in net VAT revenue for grocery stores in 2019 relative to other retailers. The top panel displays log levels, while the bottom panel shows diff-in-diffs coefficients. The net VAT rebound in 2020 aligns with the pandemic-driven patterns seen in Figure D.20.

Exact fiscal implications from official tax expenditure reports. Are these effects economically significant? Our analysis suggests they are. According to our estimates, the federal tax agency forwent about one-third of VAT revenue from grocery retailers—a major sector of the economy—during the period of the policy. Remarkably, this fiscal impact is independently confirmed by official tax expenditure reports, which systematically track revenue forgone due to exemptions, deductions, and preferential rates across all major taxes (see here). Argentina's tax expenditure system is unusually detailed by international standards: since 2001, it has reported annual estimates disaggregated by tax type and policy.

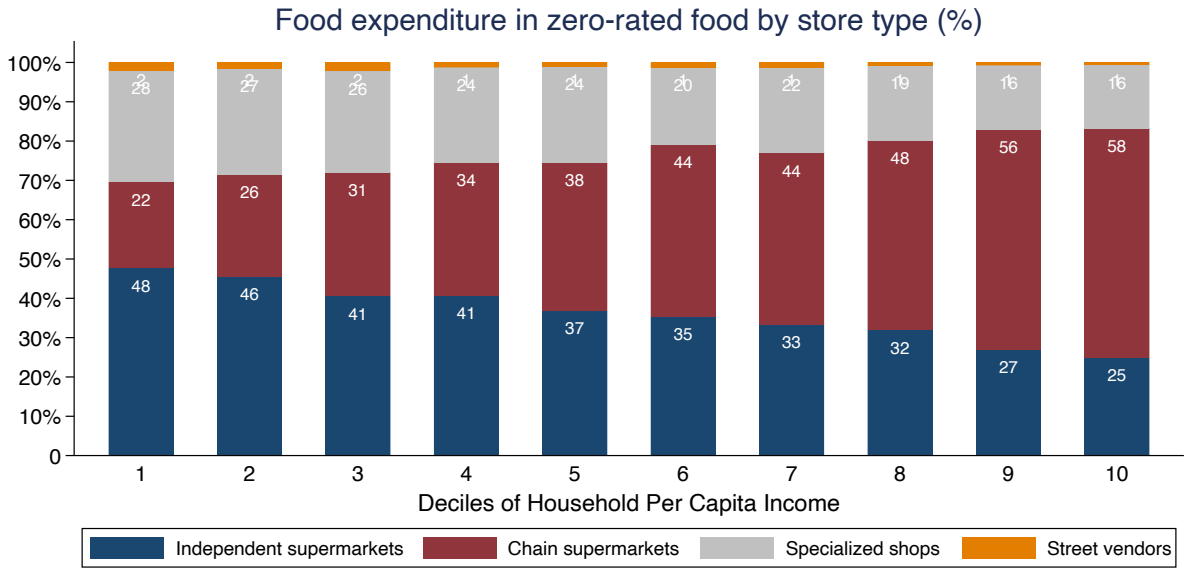
What makes the 2019 report particularly valuable is that it included a dedicated line quantifying the fiscal cost of the four-month VAT cut on basic food items. This figure—0.07% of GDP—was not based on assumptions or modeling, but rather derived directly from administrative data: under the reform, firms were required to report sales of newly zero-rated goods in a specific field of the VAT return ("non-taxable and exempt operations"), which enabled the government to measure the revenue impact with unusual precision.

For context, Argentina's flagship conditional cash transfer program (AUH) costs approximately 0.4% to 0.6% of GDP annually. Annualizing the VAT holiday's cost yields 0.21% of GDP—about half the cost of the AUH. This comparison underscores that the temporary VAT cut entailed a nontrivial fiscal cost, especially given its limited pass-through to consumer prices.

Supplementary Materials for: "Can VAT Cuts and Anti-Profiteering Measures Dampen the Effects of Inflation in Low- and Middle-Income Countries?"

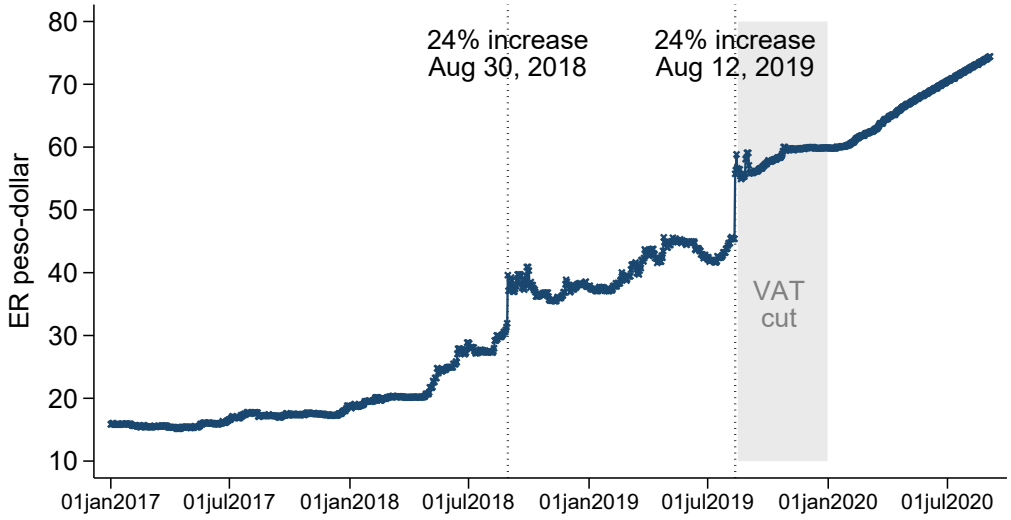
D Additional figures and tables

Figure D.1: 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 on treated goods in independent stores. The red bars correspond to chain supermarkets. The grey bars correspond to specialized shops (butchers, etc.). The orange bars correspond to street vendors, also known as "mom and pop shops".
Source: authors' calculations using the 2017/2018 National Household Expenditure Survey (ENGHo).

Figure D.2: Exchange rate (pesos per dollar)



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

Figure D.3: 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 in 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 D.4: Enforcement of the VAT increase and caps

Notes: This image shows media coverage of the reintroduction of the VAT and the government's anti-profiteering measures. The screenshot is from a Clarín news article published on January 1, 2020, titled *"The government assures that it will control 'online' that the new food price agreement is fulfilled."* (link). The article explains that the Ministry of Commerce would use its electronic monitoring system to track compliance in real time. A public official is quoted saying: *"Supermarkets report their prices online to the Ministry of Commerce. The database is updated as soon as they upload the price lists, and we can see it. The sector already showed goodwill by working with us until December 31 and committed to absorbing two-thirds of the impact. But obviously, we'll be monitoring them."* The image features Matías Kulfas, then Minister of Productive Development, who led the implementation of these measures. A separate article from Clarín, also published on January 1, 2020, confirms that independent grocery stores were not covered by the anti-profiteering policy (link).

Figure D.5: 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. Debuta 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"

En busca de la mística perdida

Nuevo ataque del hacker que filtró datos de la Federal

Lavagna suspendió la campaña y pidió que lo imiten


LA NACION

Viernes 16 de agosto de 2019

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

VILLA LA ANGOSTURA
TRABAJAN CONTRA RELOJ PARA REABRIR LA RUTA 40
SOCIEDAD | Página 25

EMERGENCIA AÉREA
UN AVIÓN ATERRIZÓ EN RUSIA SIN RUEDAS NI MOTORES
EL MUNDO | Página 9

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

Dólar: cayó por primera vez desde las PASO

Presión militar china a Hong Kong

Permitirán estacionar junto a algunas bicisendas

Filtraron archivos de la Federal sobre Chocobar

Ejecutaron a tres hombres en un búnker de drogas

Devaluación. Cómo se protegen los clubes

Además

Bibliography

(b) **Media coverage of the VAT increase along the anti-profiteering caps**

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 D.6: 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 that 13 products 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 D.7: 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

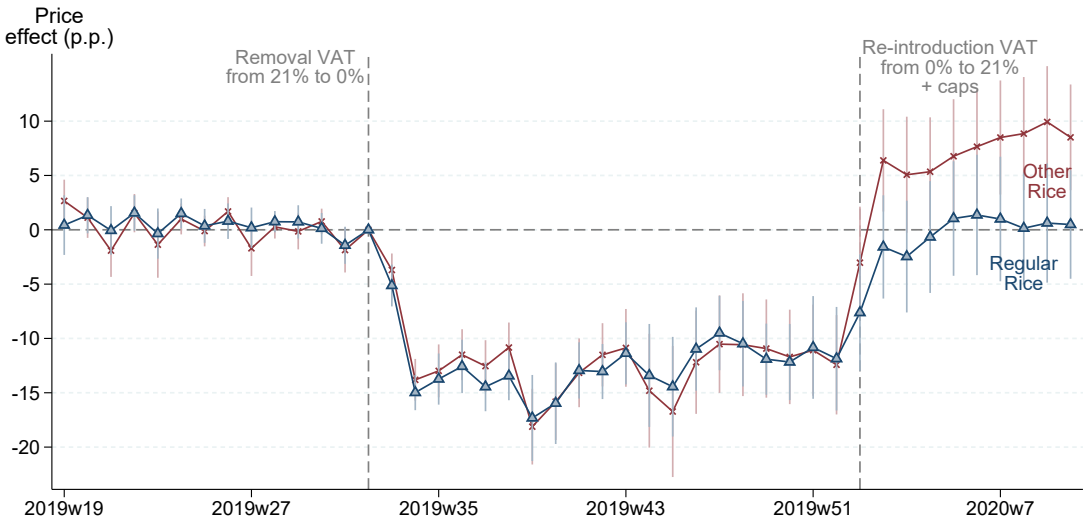
(b) **Independent supermarkets**

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

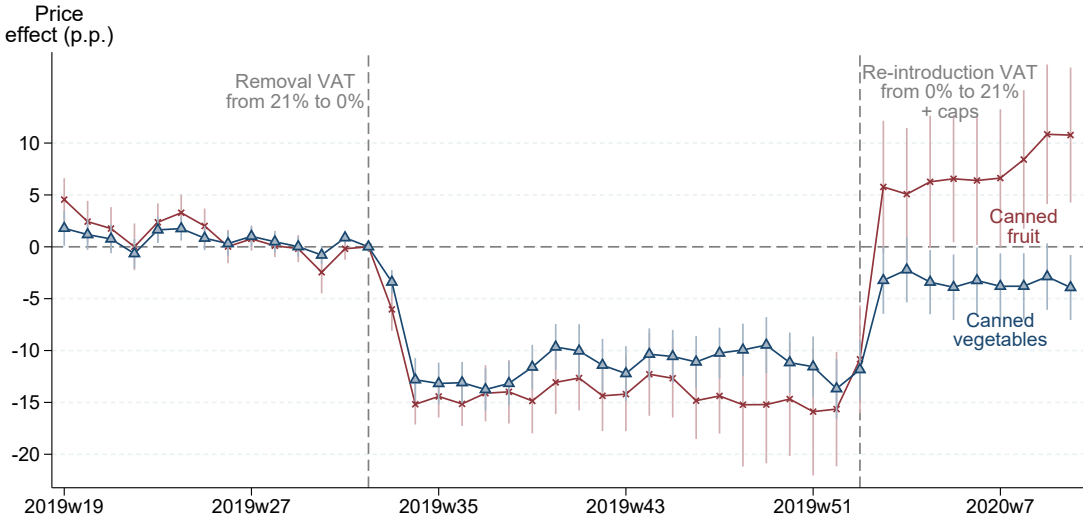
Notes: This figure shows the structure of our geographic variables in our databases. Overall, stores can be located in Gran Buenos Aires (GBA) or the rest of the country (Interior). Within GBA, they can be in the capital of Argentina (Capital Federal) or the rest of GBA area (Periferia). The Interior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

Figure D.8: Regulated VAT increase with capped pass-through rates

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

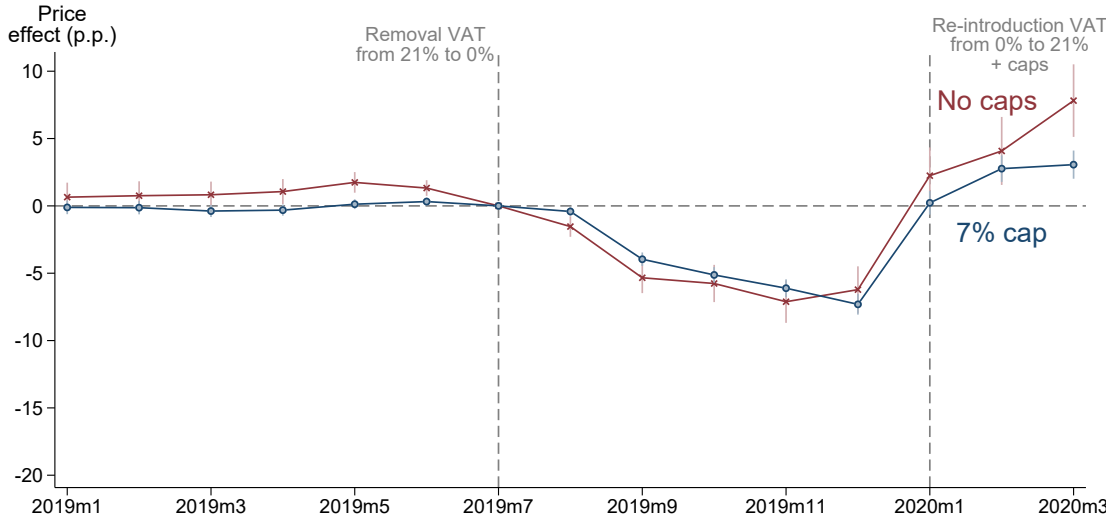


(b) **7% cap (canned vegetables) versus no cap (canned fruit)**



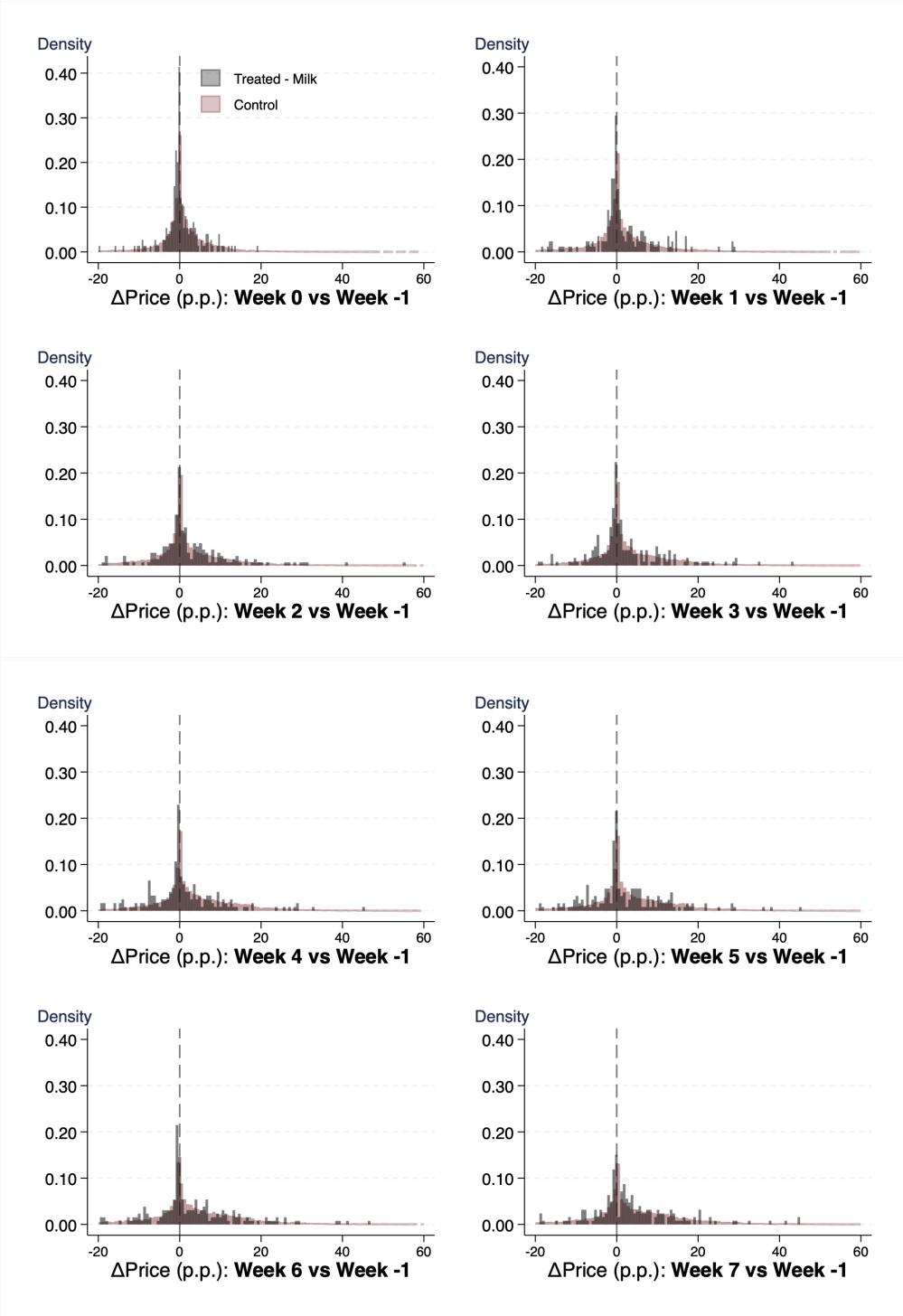
Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) in chains before and after the VAT cut and its subsequent repeal. We break down the list of barcodes from the treatment group into food categories that are subject to a capped price increase and 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 reduced 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 D.9: Do the Anti-Profiteering Measures Affect Prices at Independent Supermarkets?



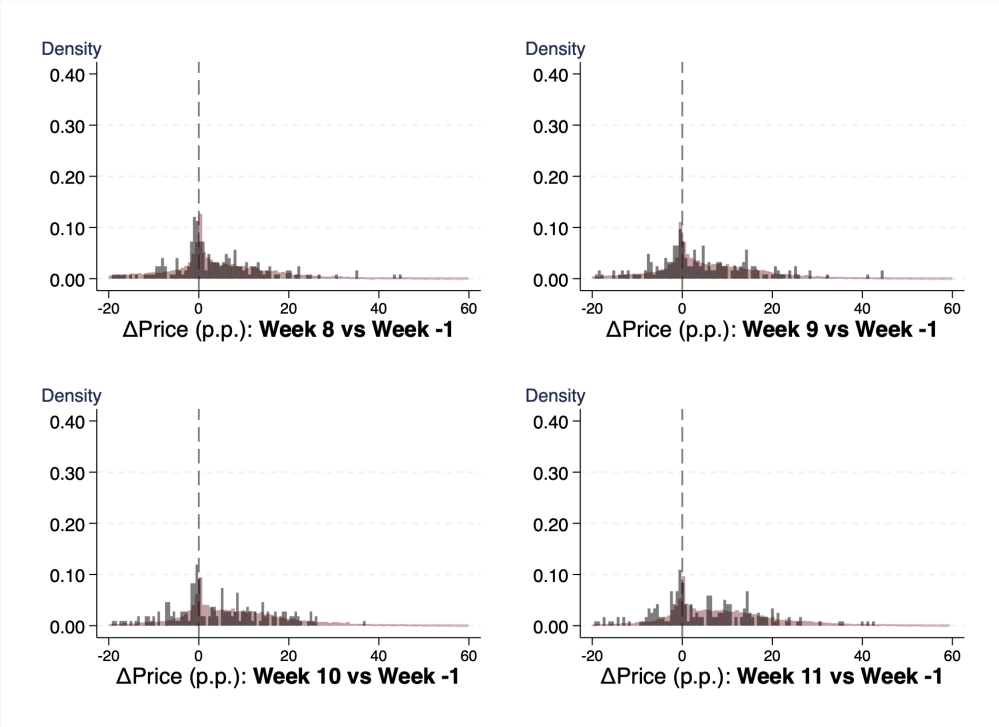
Notes: This figure shows that the prices of zero-rated goods with and without caps respond similarly in independent supermarkets when the VAT was reinstated to 21%. Unlike supermarket chains, the government did not impose differential caps in independent stores. The figure displays the results of our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

Figure D.10: Distribution of Price Changes Following the VAT Increase (milk only)



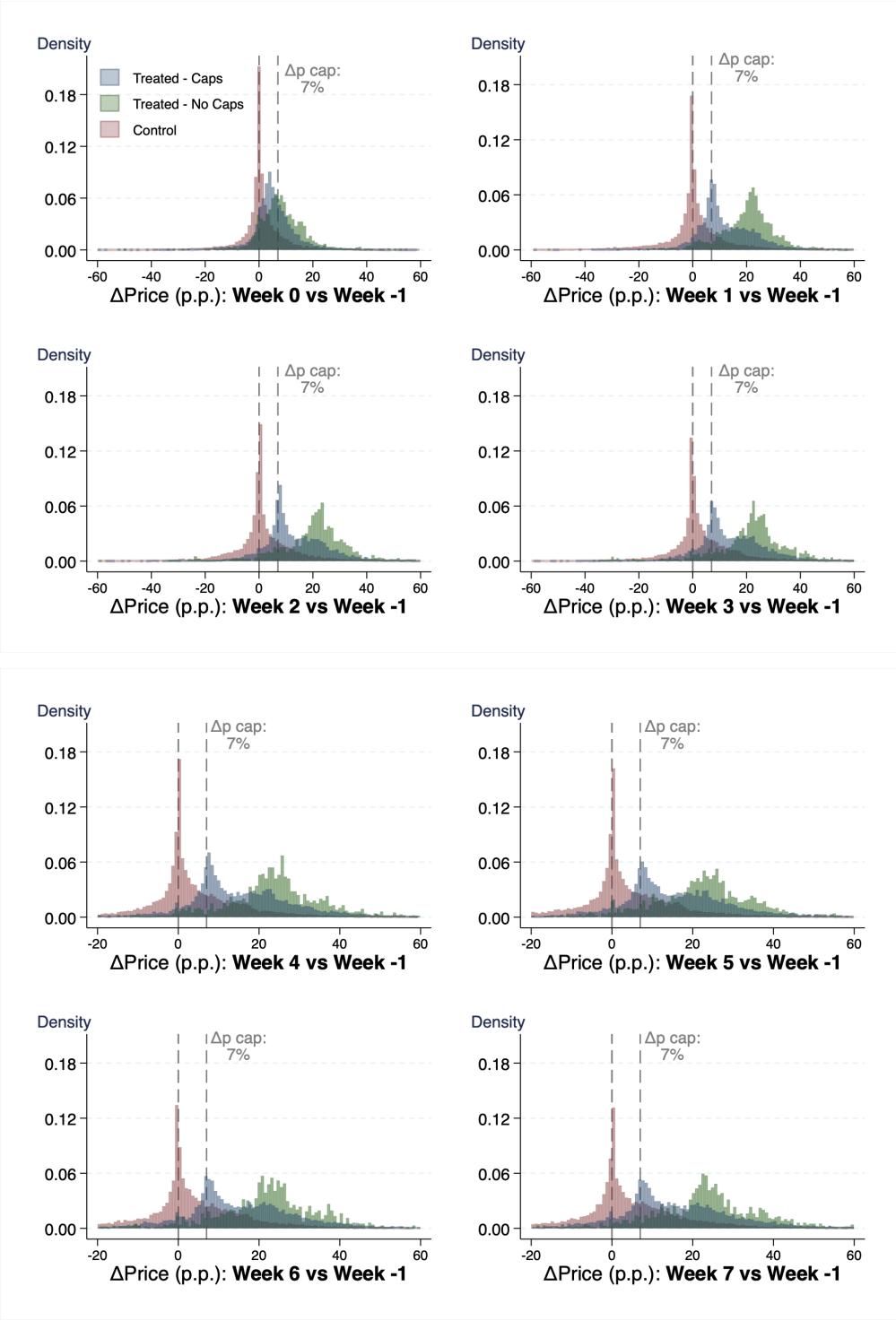
Notes: These figures show the distribution of price changes (at the barcode level) before and after the VAT increase for all goods in the control group and only milk in the treated one (subject to a 0% price increase cap). The reference week ($week-1$), refers to the last week of the year 2019 (the last week before the VAT rate of 21% was reintroduced). $Week$ 0 means the first week with a rate of 21%, $Week$ 2 the second, and so on. In each chart, we include a vertical dashed line to illustrate the situation of barcodes with no price changes ($\Delta 0\%$).

Figure D.11: Distribution of Price Changes Following the VAT Increase (milk only)



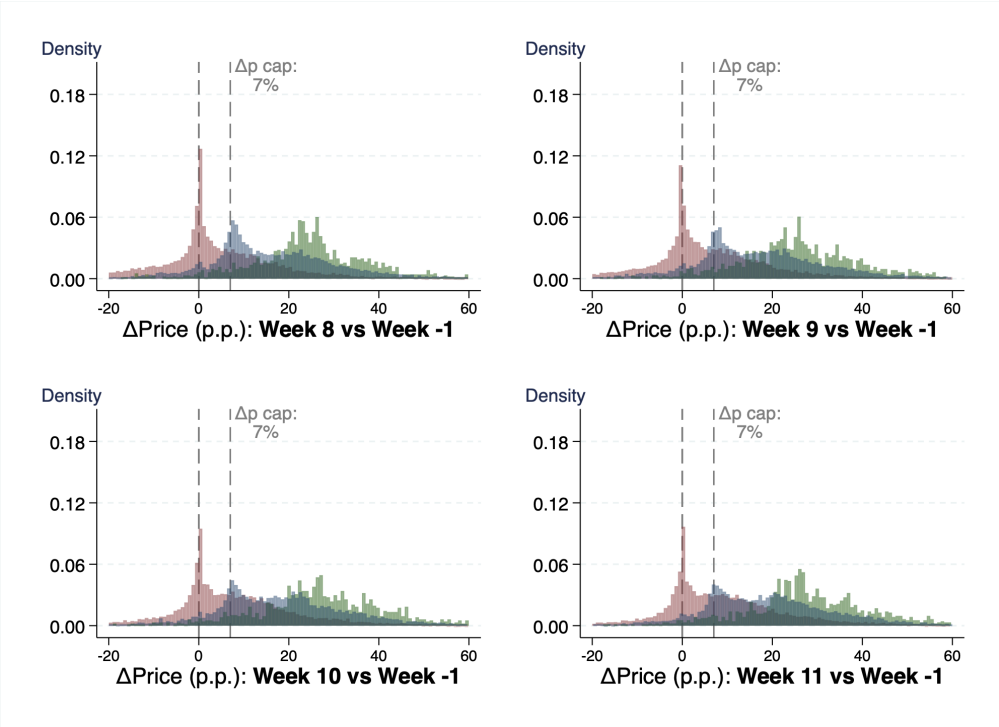
Notes: These figures show the distribution of price changes (at the barcode level) before and after the VAT increase for all goods in the control group and only milk in the treated one (subject to a 0% price increase cap). The reference week ($week-1$), refers to the last week of the year 2019 (the last week before the VAT rate of 21% was reintroduced). $Week$ 8 means nine weeks with a rate of 21%, $Week$ 9 ten, and so on.. In each chart, we include a vertical dashed line to illustrate the situation of barcodes with no price changes ($\Delta 0\%$).

Figure D.12: Distribution of Price Changes Following the VAT Increase (all goods, excluding milk)



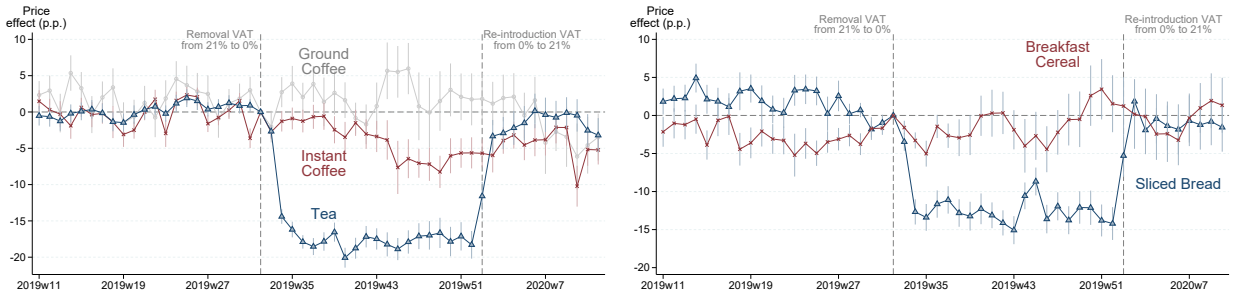
Notes: These figures show the distribution of price changes (at the barcode level) before and after the VAT increase for all goods (excluding milk, which was subject to a 0% price increase cap). The reference week ($week - 1$), refers to the last week of the year 2019 (the last week before the VAT rate of 21% was reintroduced). $Week$ 0 means the first week with a rate of 21%, $Week$ 2 the second, and so on. In each chart, we include two vertical dashed lines to illustrate the situation of barcodes with no price changes ($\Delta 0\%$) and of those with changes of equal size of the cap ($\Delta 7\%$).

Figure D.13: Distribution of Price Changes Following the VAT Increase (all goods, excluding milk)



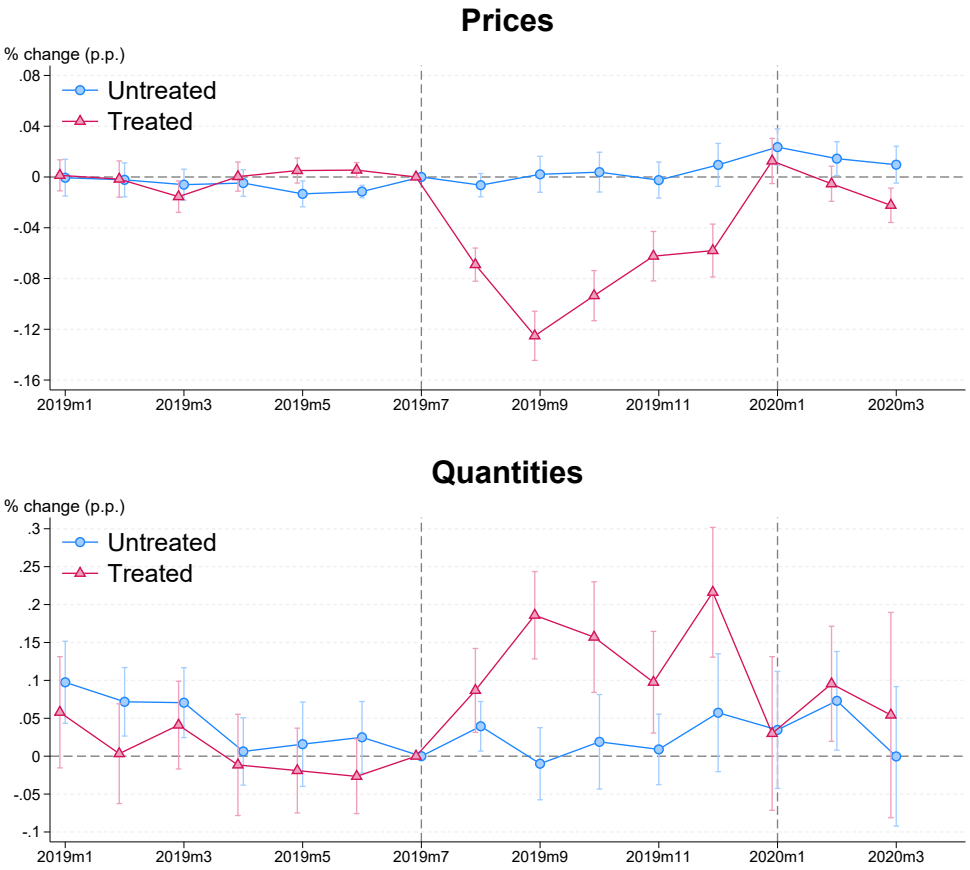
Notes: These figures show the distribution of price changes (at the barcode level) before and after the VAT increase for all goods (excluding milk, which was subject to a 0% price increase cap). The reference week ($week - 1$), refers to the last week of the year 2019 (the last week before the VAT rate of 21% was reintroduced). $Week$ 8 means nine weeks with a rate of 21%, etc. In each chart, we include two vertical dashed lines to illustrate the situation of barcodes with no price changes ($\Delta 0\%$) and of those with changes of equal size to the cap ($\Delta 7\%$).

Figure D.14: 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.

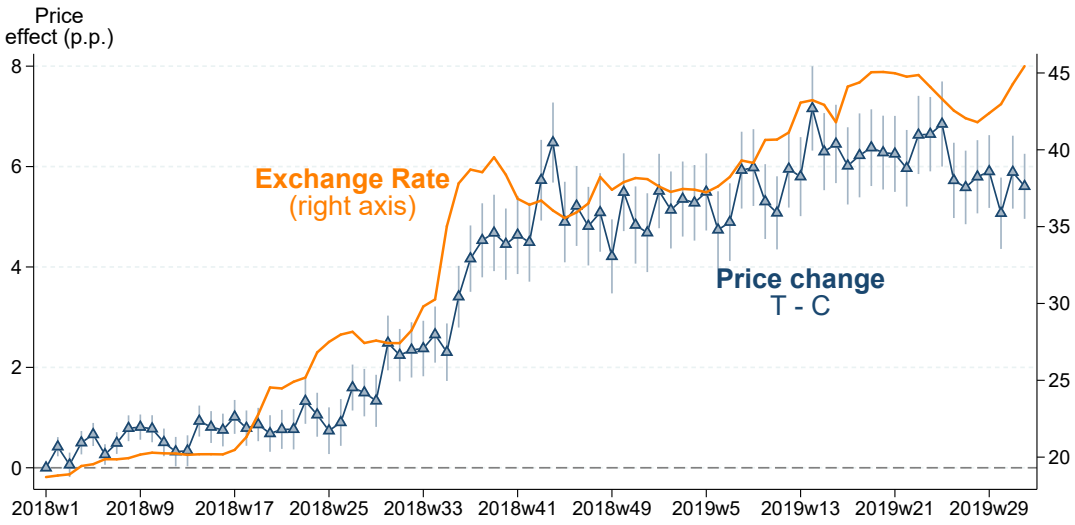
Figure D.15: Income Effects



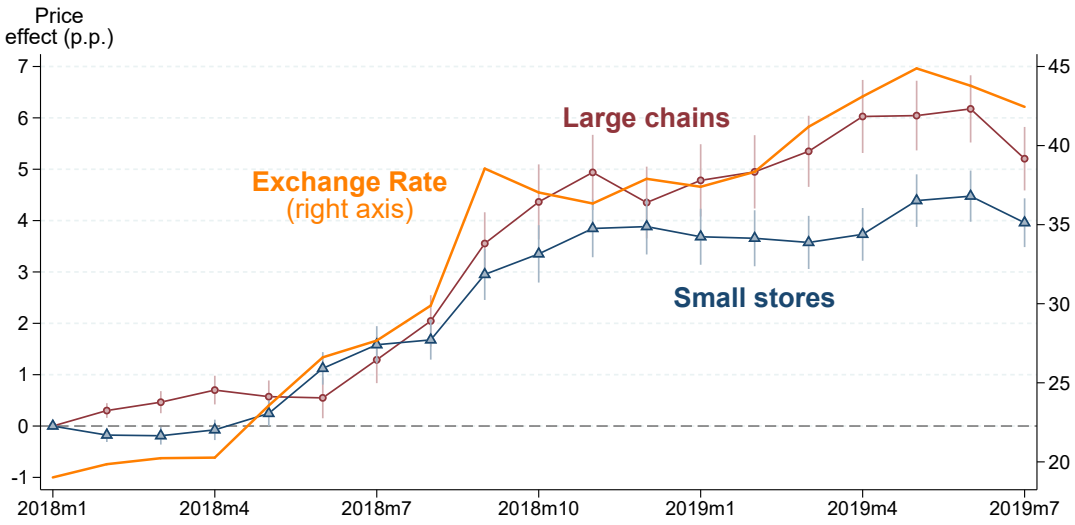
Notes: These figures plot the effect of the VAT changes on the difference between chain and independent supermarkets. We do the analysis separately for prices and quantities, top and bottom panels, respectively. Also, we run separate regressions, one where we focus on goods that are not directly treated by the VAT cut (untreated, including non-food items) and another on goods directly treated by the VAT cut (treated) in chain versus independent supermarkets.

Figure D.16: Pass-through of the 2018 peso depreciation

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

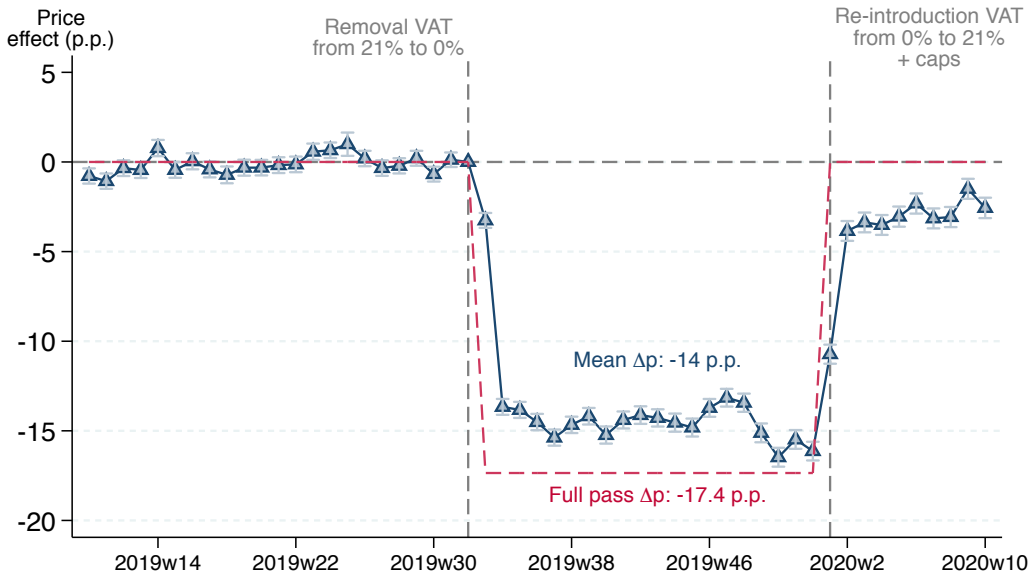


(b) **Prices of treatment and control in independent and chain stores**



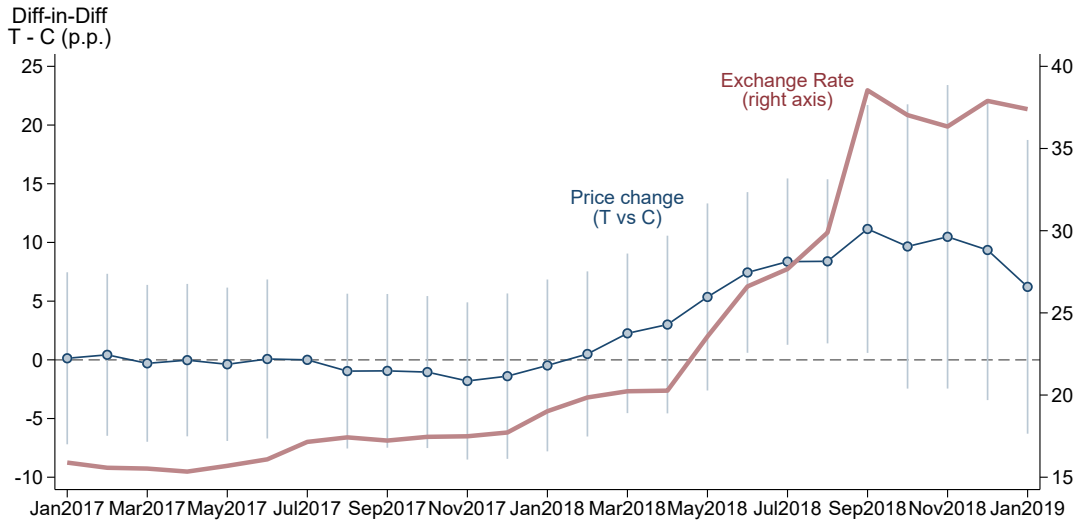
Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) on the prices of chain and independent supermarkets. The orange line of both charts displays the nominal exchange rate between the Argentine peso and the US dollar (right axis). The blue line in 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 supermarket chains (red line) and independent supermarkets (blue line). All the coefficients are estimated relative to January 2018 (the omitted month in the regression). In Section 4.3, we explain that the effect of the depreciation does not pose a threat to our subsequent findings of the VAT holiday. See Figure D.25 for the price effects spanning the full period 2018-2020.

Figure D.17: Excluding imported goods



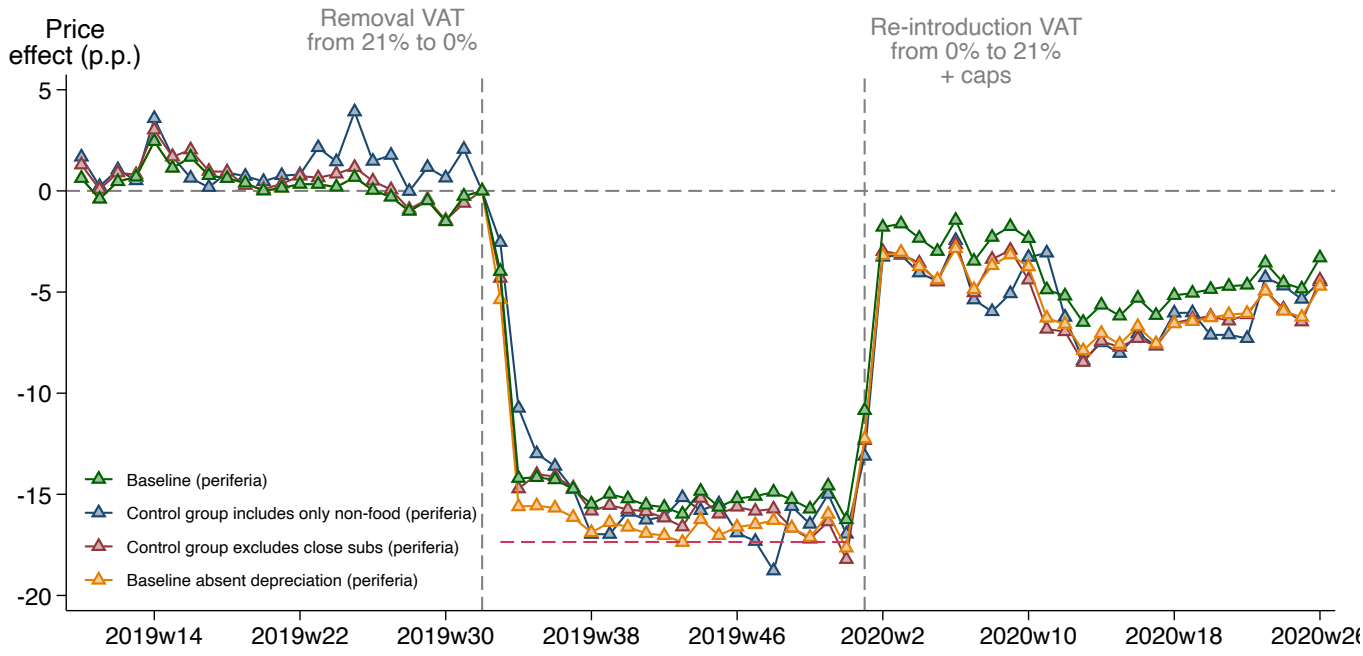
Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) before and after the VAT cut and its subsequent repeal in chain supermarkets. In particular, we restrict the estimation sample to those goods that are locally produced and thus are less subject to the large depreciation that happened in mid-August 2019. By construction, barcodes have 13 digits where the first three refer to the GS1 Member Organization of the manufacturer. Although imperfect, this is a good proxy for where the product is manufactured (a firm could be incorporated in a different country than where it manufactures its product). We tag local goods as those with codes 779 and 780, which correspond to Argentina; the remaining codes as imported goods. Considering the full estimation sample, only ten percent are not locally produced, and, interestingly, this percentage is equally split between treated and control goods.

Figure D.18: Pass-through of the 2018 peso depreciation using aggregate data from INDEC



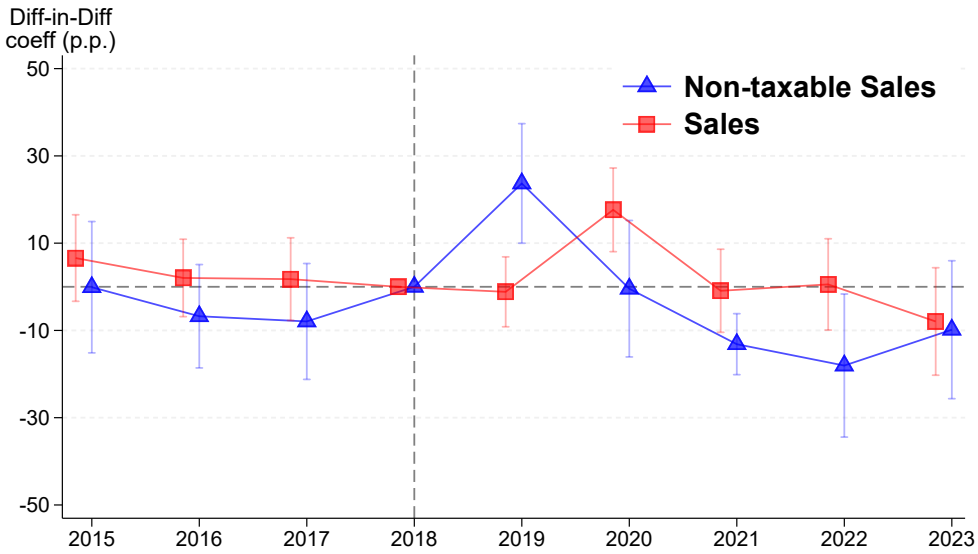
Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) using official aggregate price data from INDEC. The pink line displays the nominal exchange rate between the Argentine peso and the US dollar (right axis). The blue line shows the percentage change 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 D.19: Price Effects in Chain Supermarkets Using Alternative Specifications



Notes: This figure plots the price effect of the VAT cut and increase in chain supermarkets using the following specifications: (a) baseline estimates, (b) control group made of non-food items only, (c) control group that excludes close substitutes, and, (d) baseline estimates netting out the effect of the currency depreciation. All these estimates are restricted to the region of '*Periferia*' because non-food data are only available for this region.

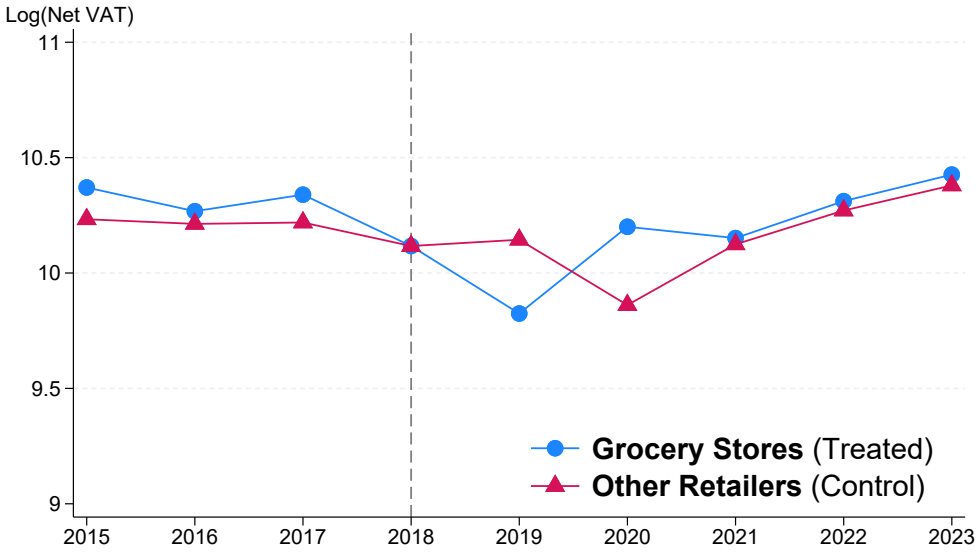
Figure D.20: Reporting of non-taxable sales and total sales in VAT return data



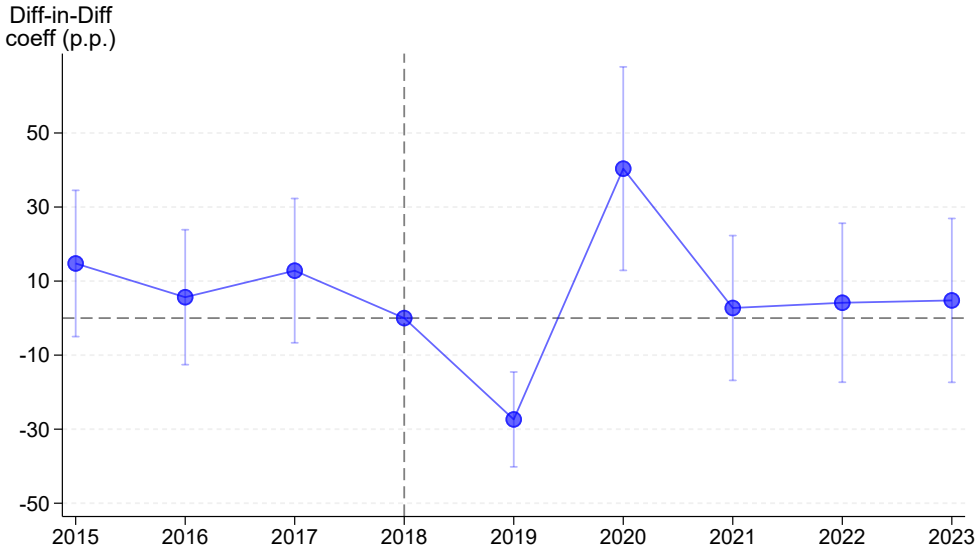
Notes: This figure shows the beta (β) coefficients of the following difference-in-differences specification $Y_{it} = \alpha_i + \gamma_t + \sum_{t \neq 2018} \beta_t D_{it} + \epsilon_{it}$ where i indexes retail subsectors and t denotes calendar years. The treatment indicator D_{it} equals one for grocery stores and zero for the other eight retail sectors (see Table A5). We estimate two separate regressions in which the outcome variable Y_{it} is the log of non-taxable sales (blue triangles) and the log of total sales (red squares), respectively. Semi-log coefficients are converted into percentage changes, and robust standard errors are computed using the Delta Method. Regressions are weighted by the number of VAT returns in each sector-year cell. The share of non-taxable sales over total sales for grocery stores was 2.4% in 2018.
Source: Authors' calculations based on ARCA's Statistical Yearbook (FY2015–2023), which reports disaggregated VAT return data for 230 retail sub-sectors.

Figure D.21: Effect on net VAT for affected and non-affected retailers

(a) Levels



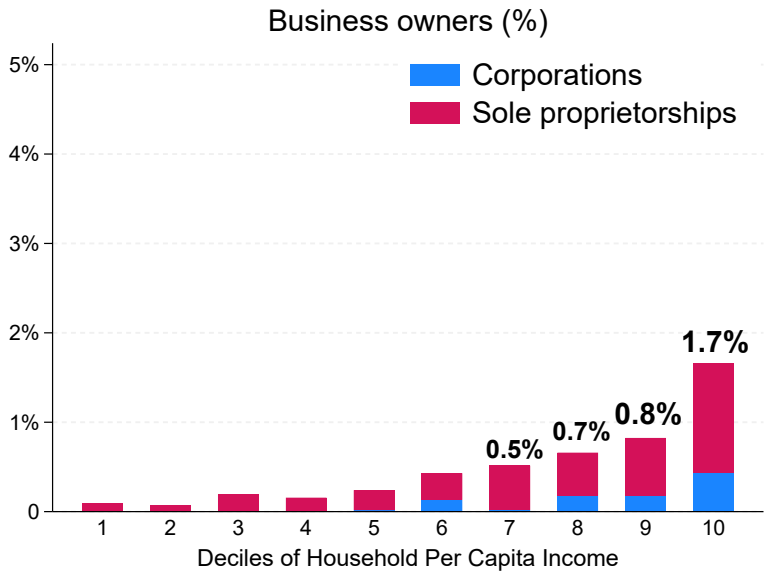
(b) Differences



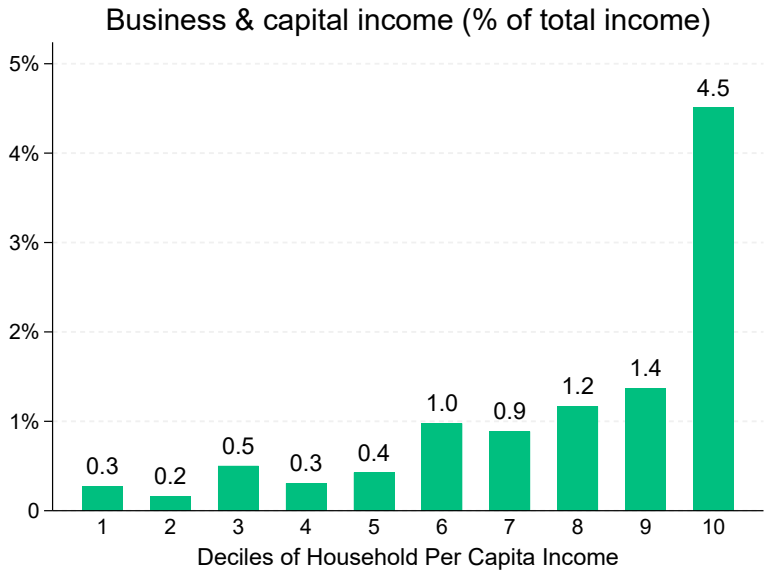
Notes: This figure shows the effect of the temporary VAT cut on VAT liability, as reported in VAT return data. The main outcome variable, Y_{it}, is the log of net VAT—defined as VAT debit from sales minus VAT credit from purchases. The top panel plots its evolution separately for grocery stores (blue circles, treated group) and other retailers (red triangles, control group), as defined in Table A5. All Values are expressed in constant 2019 pesos to account for inflation. To facilitate visual comparison, the series for the treated group has been re-scaled to match the level of the control group in 2019. The bottom panel presents estimated β coefficients from the following difference-in-differences specification: $Y_{it} = \alpha_i + \gamma_t + \sum_{t \neq 2018} \beta_t D_{it} + \epsilon_{it}$, where i indexes retail subsectors, t denotes years, and D_{it} is the treatment indicator. The semi-log coefficients are converted into percentage changes, and robust standard errors are computed using the Delta Method. Regressions are weighted by the number of VAT returns in each sector-year cell. *Source:* Authors' calculations based on ARCA's Statistical Yearbook (FY2015–2023), which reports disaggregated VAT return data for 230 retail subsectors.

Figure D.22: Business and Capital Ownership and Income by Income Deciles

(a) Retail and Wholesale Ownership

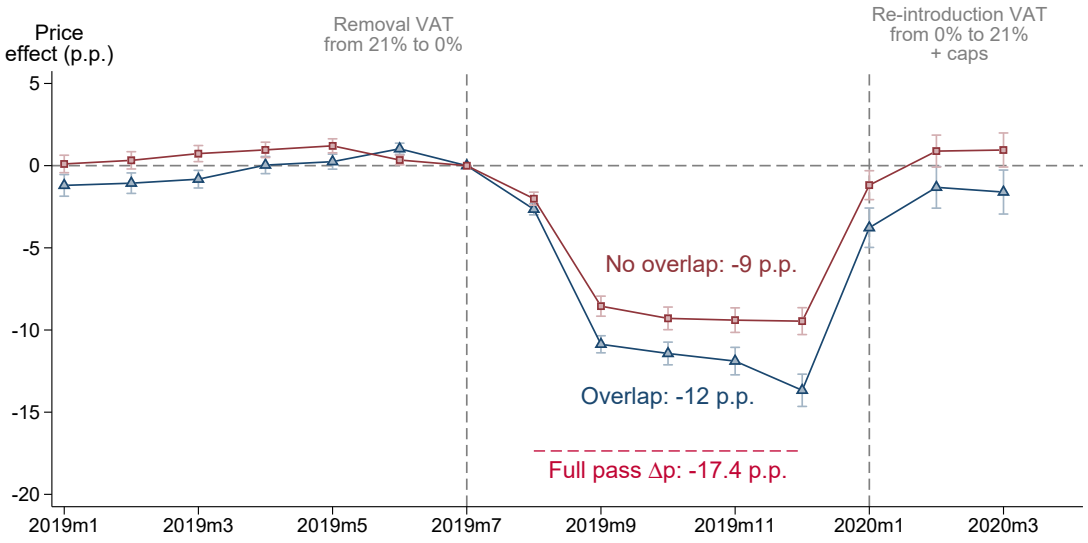


(b) Income Derived from Capital and Business Ownership



Notes: These figures plot the distribution of firm ownership in the "Retail and Wholesale" sector (upper panel) and the distribution of income from businesses and capital sources (lower panel), by income decile. In Panel (a), the ownership indicator captures individuals who report owning a business and employing workers in sector G (Retail and Wholesale). Blue bars represent corporations (e.g., supermarket chains), while red bars represent sole proprietorships (e.g., independent grocery stores). This definition is conservative, as it includes all retail businesses—not only packaged food-related ones like grocery stores. In Panel (b), we compute total income derived from business ownership (e.g., distributed profits, honoraria, salaries) and capital income for the same individuals. This is also a conservative measure, as capital income may include returns from investments in sectors beyond grocery stores—and thus unaffected by the VAT change.
Source: Authors' calculations using the 2017/2018 National Household Expenditure Survey (ENGHo).

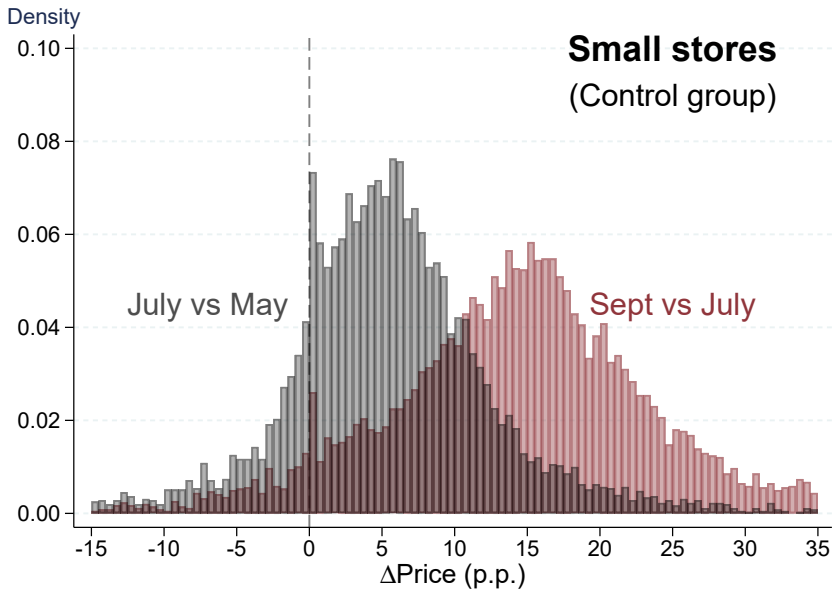
Figure D.23: Price Levels for Barcodes Sold in Both Chain and Independent Supermarkets (overlap) Compared to Barcodes sold in One or the Other (no overlap)



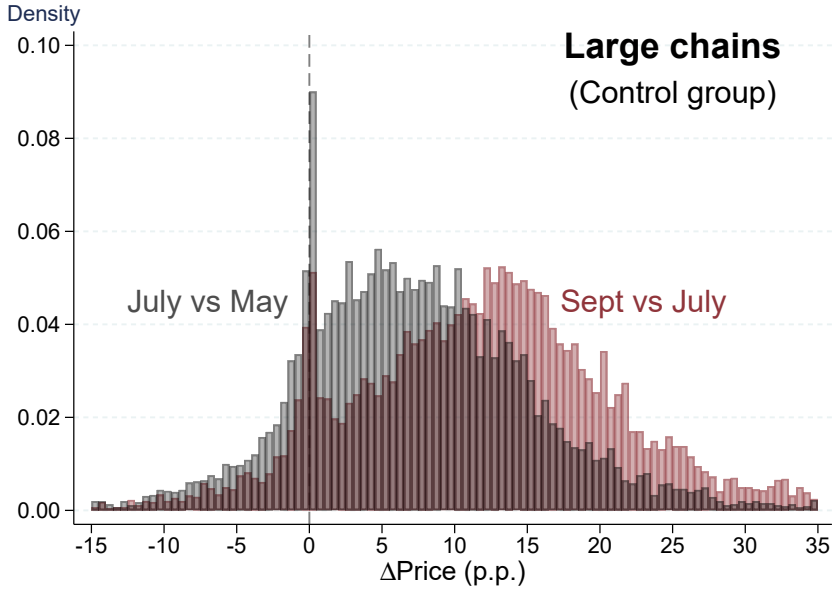
Notes: This figure pools chain and independent supermarkets and breaks down pass-through rate estimates for goods at the barcode level that are sold in both types of supermarkets versus goods that are sold in either one of them but not both. Presumably, goods that are sold in both supermarket and independent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

Figure D.24: Distribution of price changes in independent and chain supermarkets (Depreciation Episode)

(a) Independent stores

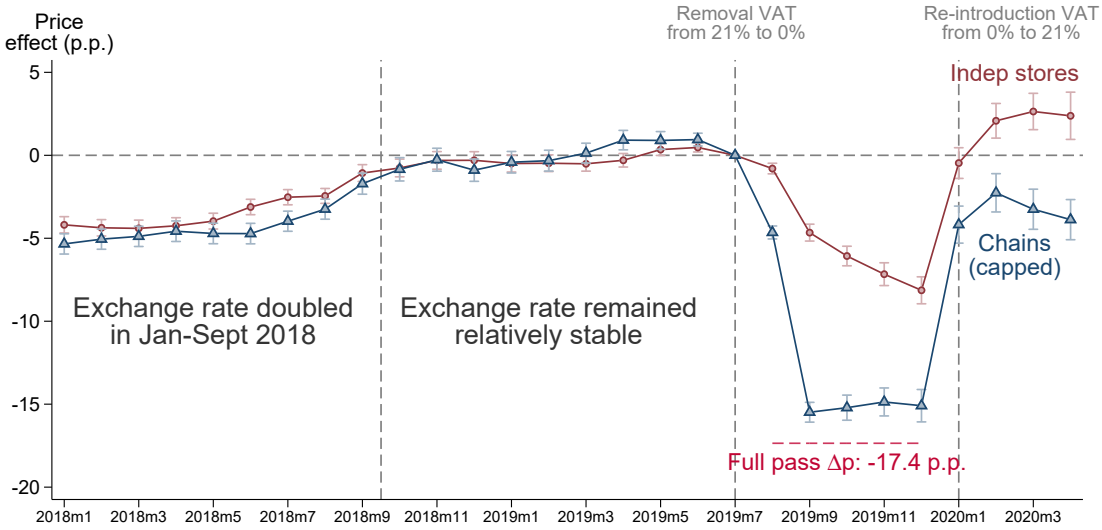


(b) Chain stores



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 D.25: Pass-through of the 2018 peso depreciation using scanner data



Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) on the prices of chain supermarkets (blue line) and independent stores (red line). We use monthly data for a balanced sample of barcodes with positive sales in the 27 months between January 2018 and March 2020. The effects are estimated relative to July 2019 (the omitted month in the regression). Appendix Figure D.2 shows the evolution of the exchange rate in the analyzed period. In Section 4.3, we explain that the effect of the depreciation does not pose a threat to our subsequent findings of the VAT holiday.

Table A1: Temporary Value-Added Tax Cuts Aimed at Lowering Consumer Prices 2020–2025

Country	Temporary VAT cuts	Food Specific?	Yearly inflation	
			Food	All items
	(1)	(2)	(3)	(4)
Algeria	0%	Yes		9.3%
Angola	Cut 14% to 5%	Yes	24%	21%
Argentina	0%	Yes	35%	37%
Bahamas	Cut 10% to 5%	Yes		5.5
Bosnia and Herzegovina	Cut 17% to 5%	Yes	14%	6%
Bangladesh	Cut 15% to 7.5%	Yes (oil & biscuits)	8.6%	7.7%
Botswana	Cut 14% to 12%	No (standard rate)		12.4%
Bulgaria	0%	Yes	22%	15%
Congo DR	Cut 16% to 8%	Yes	6%	3%
Costa Rica	Cut 13% to 1%	Yes		8%
Ecuador	Cut 14% to 12%	No (standard rate)		3.7%
Fiji	0%	Yes	6%	5%
Jamaica	Cut from 16.5% to 15%	No		9.4%
Kazakhstan	Cut from 12% to 8%	Yes	23.1%	18.8%
Laos	Cut from 10% to 7%	No (standard rate)		39.3%
Morocco	0%	No (agricultural products)		8.3%
Mozambique	Cut from 17% to 16%	No (standard rate)		10.9%
North Macedonia	0%	Yes		18.7%
Oman	0%	Yes	4%	3%
Papua New Guinea	0%	Yes	9.9%	5.3%
Peru	0%	Yes		8%
Romania	Cut 9% to 5%	Yes	14%	16.4%
Russia	Cut 10% to 5%	Yes		11.9%
South Africa	0%	Yes	12.4%	7.2%
Tajikistan	Cut 15% to 14%	No		4.2%
Togo	0%	Yes	12%	8%
Tunisia	0%	Yes (coffee and tea)		10.1%
Turkey	Cut 8% to 1%	Yes	86%	72%
Uzbekistan	Cut from 15% to 12%	No (standard rate)		12.3%
Vietnam	Cut from 10% to 8%	Yes		4.5%
Zimbabwe	0%	Yes		243.8%

Notes: Column (1) lists the countries that recently cut their value-added tax rate. Column (2) indicates whether the VAT cut targeted food items. Columns (3) and (4) present food inflation and overall annual inflation, respectively. Yearly inflation corresponds to the year 2022 (relative to 2021) for all countries except for Argentina (which corresponds to the value observed between July 2018 and 2019, right before the reform we study).

Sources: IMF and the Argentine National Statistics Office (INDEC, for its acronym in Spanish) for inflation data. The information about the value-added tax cuts on basic food corresponds to the aftermath of the COVID-19 pandemic and was retrieved from VatCalc.com and online sources.

Table A2: Description of Data Sources

	Scanner Data			VAT returns	Expenditure Survey	CPI Prices
	All country	**Periferia Region**	**Córdoba Region**		**ENGHo**	
Purpose	Main sample	Substitution effects	Income/GE effects	Effect on govt revenue	Welfare estimates	Robustness
Coverage	National - All regions (5)	Only *Periferia*	Only *Córdoba*	National	National (except rural)	National
Period	2018-2021 (Jun)	2018-2022	2019-2020	2015-2023	2017/18	2017-2018
Frequency	Large chains (weekly) Independent stores (monthly)	Weekly	Monthly	Annually	–	Monthly
Unit	Barcode-by-Store type-by-Region	Barcode-by-Store type	Barcode-by-Store Id	Subsectors	Household-by-product	Subcategories
Products	Food	Food Beverages Non-food	Food Beverages Non-food	All purchases	All purchases	All purchases
Supermarkets	Chain Independent	Chain	Chain Independent	All places of purchase	All places of purchase	All places of purchase
Source	Scentia Market company	Scentia Market company	Scentia Market company	ARCA National Tax Agency	INDEC National Statistical Office	INDEC National Statistical Office
Availability	Privately owned	Privately owned	Privately owned	Publicly available	Publicly available	Publicly available

Notes: This table summarizes the data sources used in the analysis and some of their features.

Table A3: Summary Statistics at Baseline (July 2019).

Number of Barcodes and Price Differences Across Store Types

		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 group in another, e.g., Córdoba. 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 the barcode level in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

Table A4: 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 the barcode level in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

Table A5: Retail Sector VAT Data - Year 2019 (in millions AR pesos)

Sector	**Description (retail sales in...)**	**N Tax Returns** (1)	**Taxable Sales** (2)	**Non-taxable Sales** (3)	**Output VAT** (4)	**Input VAT** (5)
471	Grocery stores	26,781	793,372	26,533	155,764	137,282
472	Food products, beverages, and tobacco in specialized stores	35,148	319,667	13,999	54,809	48,771
473	Fuel for motor vehicles and motorcycles	4,797	476,836	43,113	99,356	84,021
474	Computer accessories and software; telecomm. equipment in specialized stores	4,170	55,971	451	9,675	7,270
475	Household equipment (textiles, hardware, electronics) in specialized stores	33,430	700,968	7,592	144,142	113,497
476	Cultural and recreational goods (books, magazines) in specialized stores	7,191	67,312	6,546	14,083	10,805
477	Other products (apparel, footwear, pharmaceutical, etc) in specialized stores	60,615	517,355	279,653	106,215	82,289
478	Mobile stands and markets	447	2,003	84	386	350
479	Not carried out in shops, stands, or markets (online, vending machines, etc)	3,750	24,234	331	4,863	4,066
Total	Retail sector	176,328	2,957,719	378,302	589,294	488,351
	All sectors (universe)	978,908	27,571,390	2,844,143	5,289,592	4,373,756

Notes: This table presents an extract of VAT administrative data for 3-digit retail sectors in the year 2019. The federal collection agency, ARCA, computes these aggregates based on microdata from VAT returns F.731 and F.2002. Values are expressed in millions of Argentine pesos. The number of tax returns corresponds to the monthly average. *Source*: Tax statistical yearbooks from the federal collection agency ARCA (calendar year 2019).