Small Incentives May Have Large Effects: The Impact of Prices on the Demand for Plastic Bags

Cabrera, José María and Caffera, Marcelo and Cid, Alejandro

Universidad de Montevideo, Universidad de Montevideo, Universidad de Montevideo

5 May 2020

Small Incentives May Have Large Effects: The Impact of Prices on the Demand for Plastic Bags

José María Cabrera* Marcelo Caffera** Alejandro Cid*

May 5th, 2020

Abstract

Improper disposition of single-use plastic bags causes significant environmental impacts. Awareness of these detrimental effects has increased, according to the number of policies to reduce the consumption of plastics bags implemented worldwide. Yet, impact evaluations of these initiatives are scarce. This is particularly true for evaluations of the impact of levies on plastic bags. In this paper, we quantify, for the first time, the impact of pricing disposable plastic bags on the quantity used over a one-year time window with respect to a pre-treatment period of no regulation. Specifically, we evaluate the effect of different prices on the number of single-use plastic bags used by customers of a national supermarket chain, before and after it implemented a staggered rollout across the country. Using a difference-in-difference identification strategy, we estimate a sizable drop in the demand of single-use plastic bags in the range of 70% to 85%, compared to the control group of branches that did not price plastic bags. These estimates do not change in magnitude and are statistically robust to (i) different specifications of our basic equation, (ii) the use of synthetic controls as an alternative identification strategy, (iii) the estimation of anticipation effects, and (iv) placebo tests. We do not find evidence consistent with the effect been driven by a loss of sales. Our estimates are consistent with the evidence of large elasticities around zero prices found in other settings.

* Departamento de Economía, Universidad de Montevideo.
** Corresponding author: Departamento de Economía, Universidad de Montevideo, marcaffera@um.edu.uy.

We thank Giorgio Chiovelli, Álvaro Forteza and Marica Valente for providing helpful feedback. We also thank comments received by seminar participants at Uruguay Central Bank, Argentinian Catholic University, EfD Annual Meeting, dECON.

1 Introduction

Plastic bags can weight only 3 to 5 grams but carry several hundred times that weight. They are also relatively cheap to produce. Both characteristics made them the worldwide dominant choice for shopping. Nevertheless, improper disposition of used plastic bags causes significant negative impacts on the environment. For example, accumulated plastic debris in terrestrial ecosystems and open sea poses considerable risks to wildlife, which may suffer from choking, starvation, ingestion of micro-plastics and absorption of toxic chemicals (Barnes et al., 2009).[1] In addition, the accumulation of plastic bags and debris in shores negatively affects economic activities such as tourism, shipping and fishing. A "significant underestimate" of the external costs of the pollution of marine environments with plastic is US$ 13 billion per year (UNEP, 2014).

Awareness of these impacts have increased globally, according to the current number of initiatives to reduce the consumption of plastics bags, at all government levels, around the world. (See section 10.1, in the appendix, for a review of these initiatives). We note two things after a general examination of these initiatives. First, although it is difficult to argue that the quantity of production and consumption of plastic bags that maximizes total welfare is zero, bans are very common. Second, in spite of its impressive number, evaluations of the impacts of these initiatives on the number of bags used are scarce. This is particularly true for the impact of levies.

In this work, we evaluate the effect of a price on the number of disposable plastic bags used by customers of a supermarket chain. To do so, we collect data on the total number of single-use plastic bags delivered or sold by month in all 90 branches of a national supermarket chain. The data covers 24 months, from a year before up to a year after pricing the bags. With this data, we use two different identification strategies (a difference-in-difference approach and a synthetic control method) to estimate the effect of the prices on the quantity of plastic bags demanded.

We find that putting a price of UY$2 (approximately US$ 0.07, at that time) or UY$3 (approximately US$ 0.1), causes a drop in the demand of single-use plastic bags in the range of 70% to 85%, depending on the cities and identification strategy, with no clear difference between the two prices. We do not find evidence consistent with a loss in sales being the mechanism behind this drop. Furthermore, our long pre-treatment period allows us to disentangle a strategic anticipatory behavior by customers that would otherwise bias our estimates. Finally, our estimates are consistent with the large elasticities around zero-prices found in other settings (i.e.: education and health).

There are only two rigorous evaluations of the impact of levies on the use of plastic bags. Homonoff (2018) studies the impact of a US$ 0.05 levy on disposable bags in Montgomery County, USA, effective January 1, 2012. Some stores in Montgomery County had also a US$ 0.05 subsidy for each reusable bag that customers brought to the supermarket. Her objective was to compare the effect of the tax with that of the subsidy. The second rigorous evaluation is Homonoff et al. (2020). They studied the effect of a US$ 0.07 tax on disposable bags, effective in the city of Chicago since February 1, 2017.

---

[1] Plastic bags are particularly risky to sea turtles, as well as other 26 species of cetaceans (Moore, 2008). Concentrations of micro-plastics affects hatching, feeding and fleeing behavior and growth of larval fish at relevant levels (Lönnstedt and Eklöv, 2016).

Our work fills several gaps in this literature. We quantify, for the first time, the impact of a price on the demand for plastic bags, over a one-year time window, with respect to a pre-treatment period of no regulation. Homonoff (2018) estimates the effect of a tax with a pre and post treatment period of three months. Homonoff et al. (2020) do observe customers up to one year after the tax. Nevertheless, their pre-treatment reference period is either the last two months of a ban or a month in between the ban and the tax. Moreover, Chicago legislation did not tax all type of plastic bags, and supermarkets had thicker plastic bags available. Therefore, the fading effect that Homonoff et al. (2020) find during the first year of implementation may be the result, at least in part, of a substitution of costly thinner plastic bags for costless thicker disposable plastic bags. In our study case, customers did not have this option.

The lack of an estimation of the effect of a price on the demand for plastic bags beyond the very short run is an important gap in our knowledge, at least for two reasons. The first one is that an anticipation of the charge by customers may bias short-run estimations. Customers may anticipate the tax simply because governments and stores inform them about it before the implementation date. For example, the city of Chicago announced the tax two months before its implementation (Homonoff et al., 2020). If customers behave strategically, they might increase the demand for costless bags *before* the implementation of the tax, to save money afterwards. Alternatively, they may buy reusable bags in advance. In such cases, a short-run diff-in-diff estimation may produce different results than those in the longer-run. Nevertheless, anticipation may not be the only reason why customers' reaction to the tax may vary through time. Homonoff (2018) finds evidence consistent with loss aversion in the behavior of customers facing a tax on plastic bags. Since customer´s reference price is zero, a (relatively small) tax would feel like a loss, explaining why customers react more to a tax than to a subsidy. Although there is not much evidence about how subjects determine their reference points (Kőszegi and Rabin, 2006), Tversky and Kahneman (1991) argue that these may be influenced by norms or social comparisons. Therefore, if paying for the bags becomes a norm and/or "everybody" is using reusable bags for shopping after a period, the reference point may change, possibly decreasing the effect of a price on the use of plastic bags. This is the second reason why a lack in longer-run policy evaluations of the effect of a tax on plastic bags is an important gap in our knowledge. We contribute to fill this gap by using data that spans continuously (monthly) from one year before to up to one year after the implementation of the price. This allows us both to factor in a possible "anticipation" behavior by the part of the customers in the months before the implementation of the price and to capture possible variations in the effect during the first year.

Our work is also the first to test the impact of different prices on the demand for plastic bags.

Lastly, our work also provides evidence to assess the external validity of previous studies, both in a limited number of US cities. Given the popularity of initiatives to reduce the use of single–use plastic bags all over the world, assessing the external validity of these results remains a crucial policy issue. We provide the first evaluation of the effect of a price on single-use plastic bags outside the US that uses a counterfactual control group and actual quantitative data on bags consumption.

We organize the paper as follows. In section 2, we provide a review of the empirical literature on the effect of prices and levies on the use of plastic bags. In section 3, we describe the institutional context in which the intervention took place. In section 4, we describe the data. In section 5, we provide our estimation of the average effect, when pooling the data across branches and months. In section 6, we present some robustness checks. Finally, we discuss our results in section 7 and conclude in section 8.

2 Literature review

As noted in the introduction, evaluations of the effect of measures to limit the level of consumption of plastic bags are rather scarce. Moreover, several suffer from important methodological shortcomings. Some are simple "before and after" evaluations, without a control group. The studies of the effect of levies on plastic bag programs in Ireland by Convery et al. (2007), in South Africa by Dikgang et al. (2012a and 2012b), in China by He (2012) and in Portugal by Martinho et al. (2017) are examples of this kind of studies. Other studies, in addition to being a "before and after" evaluation, rely on self-reported categorical data or self-reported quantitative data collected in non-face-to-face surveys. Rivers et al. (2017), who analyze the impact of a disposable bag levy of C$ 0.05 in Toronto, Canada is an example of the former. Poortinga et al. (2013) is an example of the later. These authors found that a five pence charge for each single use bag introduced in Wales in October 1, 2011, increased the proportion of respondents declaring to bring their own bag in their last visit to the supermarket.

As mentioned above, there are only two rigorous (i.e.: with a control group) evaluations of the impact of a levy on the quantity of plastic bags used: Homonoff et al. (2020) and Homonoff (2018).[2] Homonoff et al. (2020) studied the effect of a US$ 0.07 tax on all disposable *paper and plastic* bags, effective in the city of Chicago since February 1, 2017. The authors interviewed 24,002 customers at large chain grocery stores inside the city of Chicago and outside (where there was no tax on disposable bags), before and after the tax. Interviews took place at four different points in time, the last one a year after the implementation of the tax. In these interviews, they gathered information on the number and type of bags used by customers per trip. This data enables them to perform a difference-in-difference analysis. An important finding of this study is that the tax exhibited a decreasing effect over the first year of implementation. In particular, on the extensive margin, while the likelihood of a consumer using any positive number of disposable bags decreased 33 percentage points in the first two months after the tax (from an average percentage of 82 points before the tax), this number was 24.8 percentage points a year later. Similarly, the effect of the tax on the average number of disposable bags used per bag user, which decreased almost one bag per trip on average in the first two months (from an average of 2.3 bags per trip in the before the tax), did not exhibit an effect statistically different from zero in the following periods. However, this fading effect in the number of bags used may be the result of a substitution away from thinner to thicker bags by Chicago shoppers, as the tax is the same for all disposable bags, but thicker bags can carry more goods. Although authors estimate that the Chicago tax decreased the total amount of plastic used, they do not provide an estimation of the effect of the tax on the number of thin disposable bags used by shoppers during the first year of implementation.

In an earlier work (the first in this literature), Homonoff (2018) studies the impact of a US$ 0.05 levy on disposable *paper and plastic* bags in Montgomery County, USA. Unlike Chicago, some stores in Montgomery County had also a US$ 0.05 subsidy for each reusable bag that customers brought to the supermarket. Homonoff´s (2018) main objective was to compare the effect of the tax on disposable bags (paper or plastic) with that of the bonus on the use of reusable bags. Using an identical data collection strategy as that described above for Chicago, she estimated that the overall effect of the tax on the number of bags used was, roughly, a decrease of 50% (one bag per customer per trip).

[2] Bharadwaj, et al. (2019) found that difference in compliance to complete or partial plastic bag bans across selected municipalities in Nepal correlates with proxies of the level of enforcement of these bans.

Comparable with Homonoff's works, Jakovcevic et al. (2014) interview a sample of 457 customers in supermarkets in the city of Buenos Aires and Great Buenos Aires, before and after the former, but not the latter, implemented a charge for disposable plastic bags. The charge was US$ 0.25 for small bags and US$ 0.4 for big bags. The authors conducted the survey in four points in time. The customers from Great Buenos Aires acted as the control group for all points in time. Big supermarkets in the city of Buenos Aires started charging the bags in October 9, 2012. Supermarkets owned by Chinese residents started charging the bags on December 10. As a result, customers from the latter act as an additional control group in the first three waves of the survey. Nevertheless, the authors do not measure the number of plastic bags used by customers in their surveys. Instead, they classified interviewed customers in three categorical groups: (a) those using only plastic bags, (b) those using only reusable bags and (c) mixed customers. They observe that the charge steadily increased the use of reusable-bags.

Taylor and Villas-Boas (2016) is another comparable impact evaluation work in the literature, but it does not evaluate the impact of a tax. What these authors evaluate is the impact of a *ban* on single-use plastic bags coupled with a mandatory provision by which retail stores must charge at least US$ 0.05 for each single-use *paper* bag or any other (e.g.: thick-plastic) reusable bag provided to customers. Observing customers at retail stores in towns of California with and without the ban and using an approach similar to that of Homonoff (2018), they show that the ban have similar effects on the use of reusable bags as did the five-cent levy on disposable bags in Montgomery County.[3]

3 Intervention context

In December 2017, worried by the presence of used plastic bags in the streets and public spaces, the Industrial and Commercial Center of Salto (the union of the city local businesses; equivalent to a U.S. city chamber of commerce) launched a campaign to decrease the use of plastic bags.[4] The campaign´s main proposal was a price for disposable plastic bags.[5] The price was optional; therefore, authorities of the center spent the following months convincing stores in town to implement such a price. A number of stores sufficient to inform the public about the imminent future pricing of the bags were "on board" by the end of December 2017. Flyers were printed and put in the doors of these stores saying that the second plastic bag was going to be charged UY$ 2 (2 Uruguayan pesos; around 7 cents of US dollar), starting on January 1 2018. Nevertheless, this never took place. During January 2018, the agreement evolved to the following proposal: a price of UY$ 2 for the common single-use plastic bags and UY$ 3 (around US$ 0.1) for "big bags" (Centro Comercial e Industrial de Salto, 2018).[6] Clogging sewers, consuming space in a landfill close to its capacity and demanding cleaning resources, improperly disposed plastic bags were also a problem for the municipal government. As a result, on February 21, 2018 it formally adhered to the campaign (Resolution 074/18. Intendencia de Salto, 2018). After that, the campaign

[3] Using cashier scanner data from California, Taylor (2020) finds that a ban of disposable bags increases 3.1% the checking out time at supermarkets and decreases sales by 1.4%. Interestingly, using similar data from Washington DC, she finds that a tax on disposable bags causes a similar increase in checking out time but the effect lessens over time. Using the same scanner data plus observational data at checkout points, Taylor (2019) estimated that the ban on plastic bags in California increased trash bag purchases of small, medium and tall sizes by 79%, 50% and 6%, respectively.

[4] Salto (105,000 inhabitants) is the second most populated city in Uruguay behind Montevideo, its capital city.

[5] Unlike the US, paper bags are not available in Uruguayan supermarkets.

[6] The center did not propose any size or characteristic for the bags and the stores did not coordinate on this issue. Therefore, sizes may have differed between stores.

reached full swing. More stores got on board of the initiative. In particular, the supermarket chain from which we obtain the data. This was fundamental for the implementation of the price because of its size (its market share is between 40% and 50%, according to conversations with officials from the Commercial Center of Salto). A media campaign (TV, radio, internet media), launched jointly by the municipal government and the commercial center informed citizens that the price would be effective by April 2, 2018 (Industrial and Commercial center of Salto, e-mail communication, March 13, 2020). Adhered stores displayed the campaign sign at their entrance. The sign informed readers that "(f)rom 04/02/18, standard T-shirt type plastic bags will have a cost of UY$ 2 (tax incl.), and UY$ 3 the bigger ones".[7] As a result, it is very possible that most of the citizens knew about the price before its actual implementation. In fact, this was the conclusion of a local newspaper that surveyed stores and customers during the first day of the implementation of the price (Diario El Pueblo, 2018).

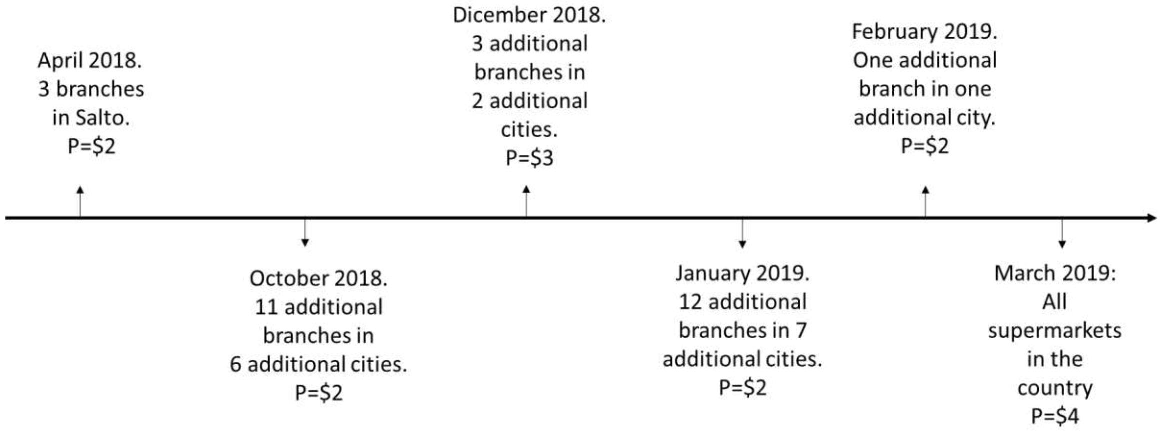
There were four supermarket chains in town at that time. Three of them were local chains. The fourth was the national chain from which we collect the data. One of the local supermarkets sold only common plastic bags at UY$ 2. The other two sold both UY$2 and UY$3 bags. The national chain sold only one bag (45x60 cm) at UY$ 2. Before pricing the bags, the national chain was giving its customers these same bags free of charge. During the period of twenty days before starting to price the bags, the national chain gave one reusable bag to customers with a loyalty card. The biggest local supermarket did the same. Customers without a loyalty card in this supermarket could also get a reusable bag by spending more than UY$ 1,500 (Diario El Pueblo, 2018). The other stores did not do this.[8]

In September 2018, the center conducted a survey to assess how many stores in town adhered to the campaign. According to this survey, there were around 80 stores charging bags (90% of which since day one); 60% of grocery stores, 50% of markets, 40% of bakeries and 35% of butcher shops, apart from all supermarkets (Manager of Industrial and Commercial Center of Salto, e-mail communication, 02/07/2020). Because all supermarkets adopted the price, the percentage of sales subject to the charge of bags may be larger than the percentages of businesses.

[7] Figure A.1 in the Appendix (section 10.2), shows a picture of the sign.

[8] There is anecdotal evidence that during the first days some supermarkets put boxes near cashiers or gave some angry customers a number of bags free of charge (Diario El Pueblo, 2018). We do not have information about these measures being in place after those first days. Nevertheless, we think that this type of implementation "noises" is something that one can expect to see during the first days of interventions such as this. Certainly, depending on the magnitude of these type of "compensation" measures on the part of retailers, they may affect the estimation of the impact of the price in the very short run. Nevertheless, this "noise" should fade away in longer-run estimations, such as the ones that we present here.

Figure 1: Time line of the price rollout

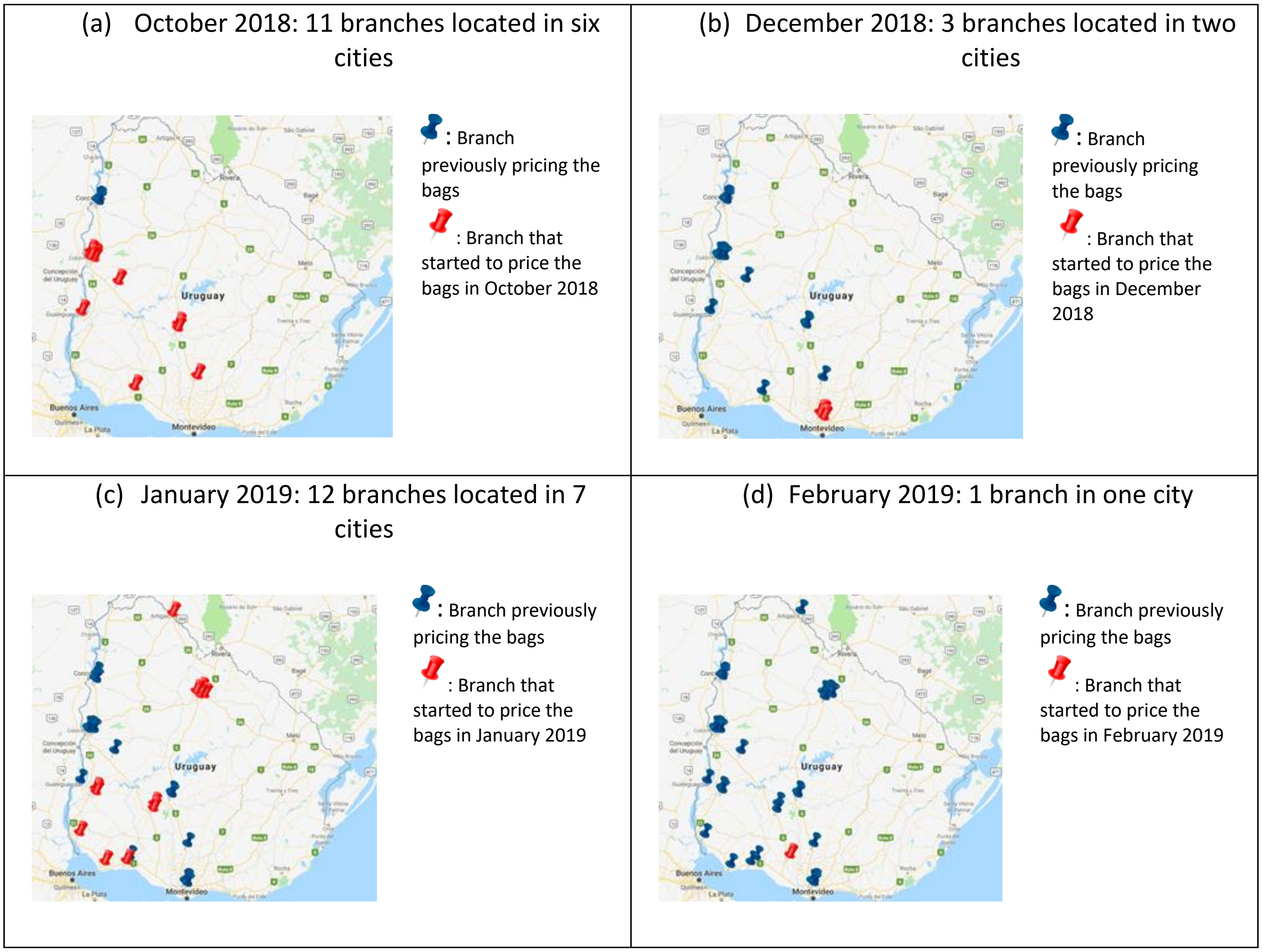


Six months after pricing the bags in Salto, the supermarket chain individually started a staggered rollout of the price to stores in other towns and cities (See Figure 1). In a second wave of the experiment, in October 2018, it started to charge $2 the plastic bags in 11 additional branches located in six other cities and towns.[9] (See Figure 2, panel (a)). In December 2018, it started to charge UY$ 3 the bags in three branches located in two cities, La Paz and Las Piedras (See Figure 2, panel (b)). The reason for pricing the bags UY$ 3 in these two towns instead of UY$ 2 was that the price in these two towns was the result of an agreement among all supermarkets in these towns. In the rest of the cities where the supermarket rolled out the pricing of the bags, it was the only one doing it and it charged the bags UY$ 2. A fourth wave occurred a month later, in January 2019. In this wave, the supermarket added 12 branches in seven cities (See Figure 2, panel (c)).[10] Lastly, it added one more branch in the city of San José in February 2019.

[9] One branch in the cities of Florida, Fray Bentos, Rosario, Young; two in Durazno and 5 in Paysandú

[10] Four branches in Tacuarembó, two in Trinidad, two in Mercedes, and one in Artigas, Carmelo, Colonia and Juan Lacaze.

Figure 2: Supermarket chain rollout of the pricing of plastic bags across Uruguayan cities



Notes: each pin corresponds to a branch.

The rollout ended in March 2019, when all the supermarkets in the country agreed to price the bags UY$ 4. The Uruguayan government had passed a law in August 2018 (Law # 19655) establishing (a) the ban of non-compostable bags and (b) a minimum UY$ 4 price for compostable plastic bags, for the whole country, starting June 30 2019. The law prohibited imports of non-biodegradable bags by March 2019. Supermarkets argued that because of this they had to start pricing the (non-biodegradable) bags UY$ 4 to avoid running out of bags while they were in the process of substituting the old bags by the new biodegradable ones (El Observador, 2019). Since June 30 2019, plastic bags in Uruguay are compostable and have a minimum price of UY$4. Figure 3 shows the value of the price of plastic bags in the supermarket chain through time, by rollout wave (except the last one, of only one store). At the end of the time line (April 2019), all branches in the sample end up selling bags at UY$ 4. This gives us three different price increases, from UY$ 0 to UY$ 4 in 56 branches, from UY$ 2 to UY$ 4 in 26 branches and from UY$ 3 to UY$ 4 in three branches. As explained below, we exploit this staggered rollout to estimate the effect of a price of UY$ 2 and UY$ 3 on the demand for plastic bags with two different identification strategies: differences-in-differences OLS regressions and synthetic control methods. We also analyze the effect of the UY$ 4 price, but using a simpler “before-and-after” strategy.

Figure 3: Time-line of the rollout and value of prices for plastic bags in the supermarket chain

4 Data

We collected data on the total number of single-use plastic bags delivered by the 90 branches of the aforementioned supermarket chain, by month, between April 2017 (twelve months before they started pricing the bags in Salto) and April 2019 (the month in which all supermarkets in Uruguay started to price the bags).[11] This supermarket chain is a discount store chain, with an explicit marketing strategy based on low prices. Its 90 branches spread over 28 cities and towns of Uruguay. It is the only supermarket chain with such a national presence.

When the observation belongs to a branch that did not charge the bags during the entire sample period, or to a month before the date in which the branch started to charge them, the number of "delivered" bags is the number of bags given out free of charge. The number is the result of the difference in monthly stocks of plastic bags in that branch. When the month-branch observation corresponds to a branch and month during which the bags had a price, the number of bags "delivered" correspond to the number of bags sold in that branch during that month, according to cashier data.

Table 1 presents the descriptive statistics of our database.

[11] Our 90*25, store-month panel data is not balanced. We have missing information on bag consumption in four store-month observations. Additionally, four branches went out of business during 2017 (before the first wave of the experiment). The total number of observations is 2,161.

Table 1: Descriptive Statistics

Variable	mean	sd	min	max	obs
Bags delivered/sold by month (000)	65.20	49.00	-4.00	395.00	2,161
Bags delivered/sold (moving average)	65.07	43.06	1.82	315.38	2,161
Price	0.31	0.92	0	4	2,161
Price = 0	0.89	0.31	0	1	2,161
Price = 2	0.06	0.25	0	1	2,161
Price = 3	0.01	0.07	0	1	2,161
Price = 4	0.04	0.20	0	1	2,161
Treated April 2018	0.03	0.18	0	1	2,161
Treated October 2018	0.13	0.33	0	1	2,161
Treated December 2018	0.03	0.18	0	1	2,161
Treated January 2019	0.14	0.35	0	1	2,161
Other stores	0.67	0.47	0	1	2,161
Store number	45.20	25.53	1	90	2,161
State number	5.42	5.71	1	19	2,145
City number	7.75	8.88	1	28	2,145
Year	2017.8	0.69	2017	2019	2,161
Month	6.38	3.42	1	12	2,161

The average branch delivered 65,200 single-use plastic bags per month. The largest branch delivered 230,740 bags in an average month and 395,000 bags in the busiest month. The smallest branch delivered, on average, 17,670 bags per month to his customers. We have two observations with a negative number of bags delivered by a store in a month. In both cases, this occurs after a month in which the number of bags delivered was significantly above the average. Therefore, we think that what may have happened in these two cases is that these two stores replenished stocks of bags in an unusually high manner.[12] In our full experiment, we have four different prices. Most branches (56) didn´t price the plastic bags during the 24 months prior to April 2019. The 6% of the observations that have a price equal to UY$ 2 is comprised of three branches (April experiment) that charged UY$ 2 between April 2018 and March 2019, plus 11 branches that started charging the bags in October 2018, plus 13 branches that charged the bags at UY$ 2 from January 2019 to March 2019. Three branches charged a price of UY$3 during four months (December 2018 to April 2019). In relation to the geographic distribution of the branches, it is important to note that there is at least one branch of this supermarket in *every one* of the 19 departments of the country.

[12] Counting delivered bags free of charge as the difference in stocks may add some variation to the series. In order to smooth the series shown in some figures, we use a three-month moving average of total bag consumption.

5 The average effect for the full experiment

The staggered rollout of the pricing initiative constitutes a unique opportunity to estimate the average effect of two prices on the demand for plastic bags. To do it, we estimate the following equation:

$$B_{bm} = \alpha + \delta_m + \mu_b + \beta_{p=2}P2_{bm} + \beta_{p=3}P3_{bm} + \varepsilon_{bm} \qquad (1)$$

$B_{b,m}$ represents the number of bags delivered by branch *b* on month m, and δ_m and μ_b are month and branch fixed effects, respectively. $P2_{bm}$ is an indicator variable equal to one if the price of the bags is UY$2 in branch *b* and month m; 0 otherwise. The coefficient $\beta_{p=2}$ is therefore the difference-in-difference estimation of the effect of the UY$2 price on the number of bags demanded, with respect to the number of bags delivered when the price is zero, averaged across branches and months (α). $P3_{bm}$ is an identical indicator variable equal to one if the price of the bags is UY$3 in branch *b* and month m; 0 otherwise. Consequently, $\beta_{p=3} - \beta_{p=2}$ is the estimation of the additional decrease in the number of bags used when the price of the bags increases from UY$2 to UY$3.

Before presenting the formal results of the estimation, Figure 4 shows a graphic illustration of the effect of the prices on the quantity of plastic bags delivered by the average branch in each wave. The green line depicts the average number of bags delivered at zero cost by the average control branch. This control group is comprised of the 56 branches that did not price the bags during the period of analysis, April 2017 – March 2019. The rest of the lines depict the number of bags delivered or sold by the average branch in each of the different sets of treated branches (a set of branches is the group of branches that started pricing the bags in the same month, indicated by the vertical lines). Figure 4 illustrates a sharp decrease in the number of bags used by customers of branches pricing the bags. The drop in bags used occurs in the same month in which the supermarket started pricing them. Moreover, this drop does not seem to rebound after three, four, six or 12 months. The last month in the graph shows the additional decrease in the demand for plastic bags caused by the price of UY$ 4 in the branches already pricing the bags, and in the branches not pricing the bags (green line). More on this below.

Figure 4: Average number of plastic bags delivered by group of branches, before and after the supermarket priced the bags in each group

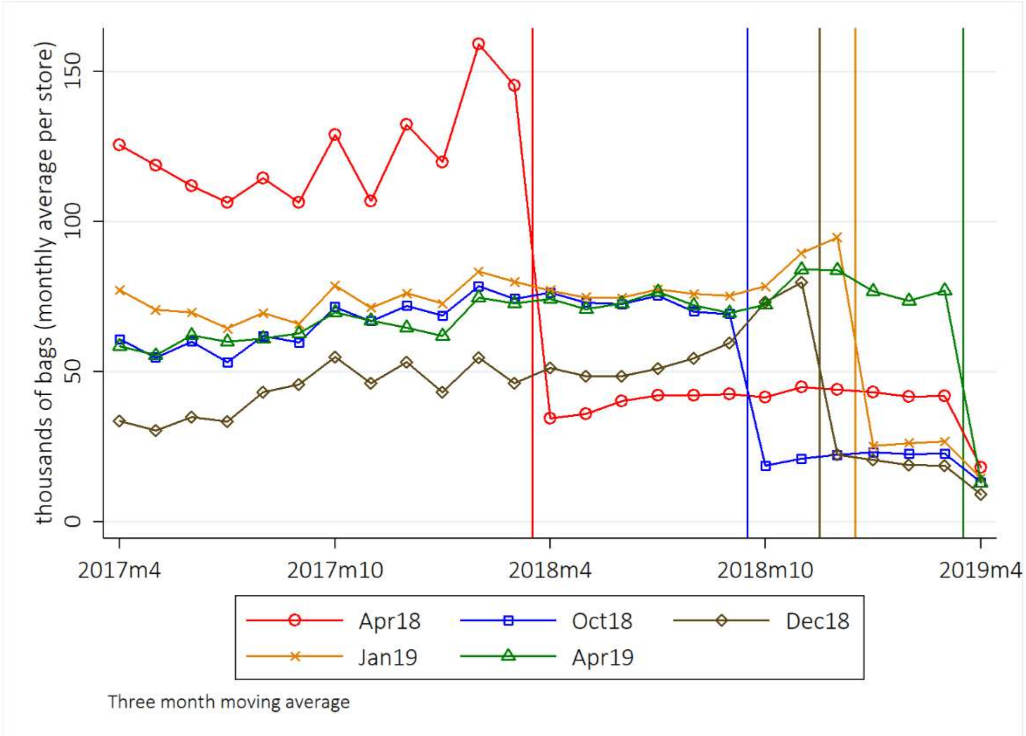


Table 2 shows the diff-in-diff OLS estimates and standard errors of the parameters of interest ($\beta_{p=2}$ and $\beta_{p=3}$, in equation 1). Lines 2 to 5 refer to the estimation of the average effect of putting a price of UY$ 2. It shows that the point estimate of this effect is an average decrease of 63,590 bags, per branch, per month. This represents a percentage drop of 85.0% from the mean number of bags delivered, on average, in the same branches, when the price was zero (74,811 bags).

Table 2: Full experiment results

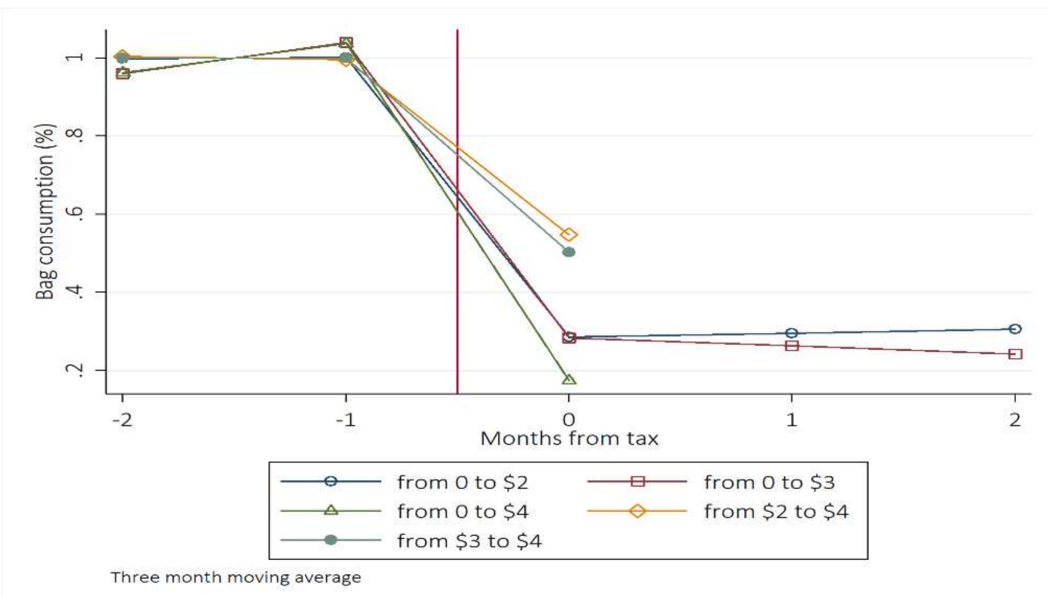
Price = 2	
Estimate of $\beta_{p=2}$	-63.59***
Standard error	(5.842)
Pre-treatment mean of treated with P=2	74.81
Percentage change (P=2)	-85.0%
Price = 3	
Estimate of $\beta_{p=3}$	-41.97***
Standard error	(7.025)
Pre-treatment mean of treated with P=3.	49.93
Percentage change (P=3)	-84.1%
N	2,075

Notes: the table shows the results of an OLS estimation of equation (1). The outcome variable is the number of bags delivered/sold by branch, by month. Controls include month and branch fixed effects. Standard errors (in parenthesis) clustered at the branch level. * p<0.10, ** p < 0.05, *** p < 0.01

With respect to the effect of a UY$ 3 price, lines 7-10 of table 2 show that the estimated effect of this price on the quantity of plastic bags used by customers is a drop of 84.1%.

The reduction in plastic bag consumption estimated for a price of UY$ 3 is almost identical to the one estimated for p = UY$ 2, in percentage terms. Nevertheless, we do not argue that the value of the price does not matter. First, we only have 12 observations for p = UY$3. Second, in section 6.1, where we estimate wave-specific effects for the prices, results differ. Finally, it is easy to see in Figure 4 that the price of UY$ 4 produced an additional drop in the demand for plastic bags in all branches, independently of the level of the price they were charging. A simple before and after analysis, normalizing to one the average number of bags consumed in the three months before the change in price, shows that the value of the price matters, as one should expect (See Figure 5). In particular, the graph shows a significant additional drop in those sets of branches already charging UY$ 2 and UY$ 3 per bag (orange and green lines). Of course, a before and after analysis is a methodology that requires more identification assumptions. However, assuming these, the price of UY$4 caused an additional effect between 40 and 50% in those branches, respectively. For reference, using this simple identification strategy to estimate the average short-run effect of the price, a change in price from zero to UY$ 2 decreases the demand for plastic bags by around 70%. This effect is almost identical to the effect of a change from zero to $3, according to this methodology also. Lastly, a before and after comparison shows that putting a price of UY$4 in branches that were giving the bags for free, decreased the use of bags by more than 80%.

Figure 5: Before and after percentage change in bags used



6 Robustness checks

6.1 Wave-specific treatment effects

In this section, we conduct wave-specific estimations to explore possible differences in the effect of the price among waves. A source of possible differences between waves is that in the first one (April 2018) and the third one (December 2018), the supermarket was not the only store in town pricing the bags, while it was the only one in the other waves. This may be important. Although the relative low prices and the relative big size of branches act as incentives against the substitution away from this supermarket, it may have been less costly for customers to move to other stores in towns where this supermarket was the only one charging the bags. Wave-specific estimations could illustrate whether being the only one charging the bags could make a difference in the effect of the prices.

We conduct two sets of wave-specific estimations. In the first set, we use, for every wave (a) the same period of analysis (April 2017 – March 2019) and (b) the same control group (comprised of the 56 branches that did not price the bags during the whole period). In the second set, we use a wave-specific synthetic control (the donor pool is always the pool of 56 branches that did not price the bags in the period).

A critical assumption in the difference-in-difference methodology is that of the parallel trends of the control and the treatment group in the pre-treatment period. To analyze the validity of this assumption, we perform a parallel trend analysis for each of the waves. The results of these analyses are included in the appendix. In general, the change in the difference between the average number of bags delivered by treatment branches and control branches in the first month of the sample is not statistically different from zero in most of the following months of the pre-treatment period. Anyway, we address possible differences in trends by incorporating branch-specific time-trends in the difference-in-difference estimation, and when we discuss the possible existence of anticipation effects in section 6.3.

The waves used in this exercise are those in Table 3, already presented above, with the exception of the February wave (comprised of only one branch, one month before the end of the natural experiment):

Table 3: Waves of the rollout of prices

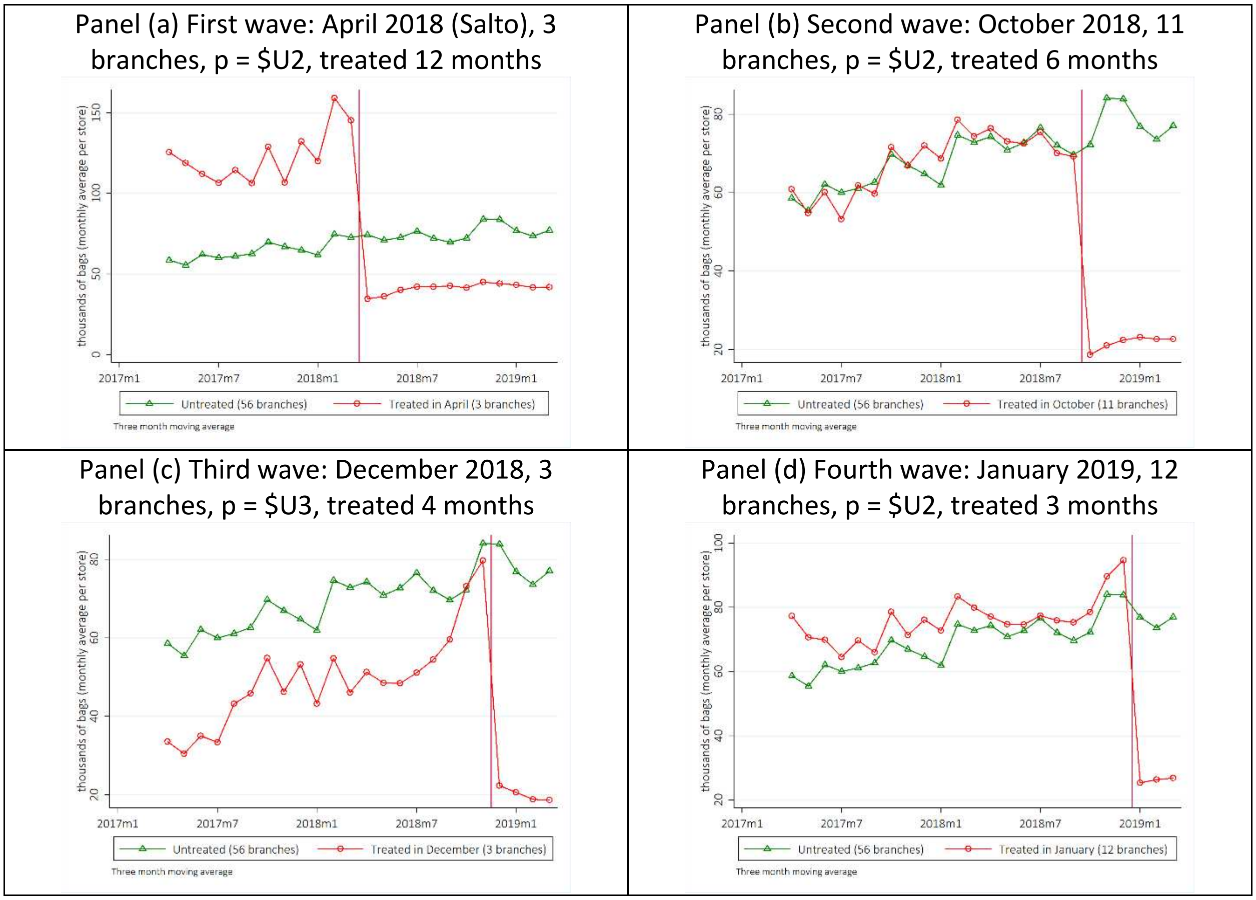
	Experiment	Treated branches	Treated cities	Price	Treatment duration (months)
1	April 2018 (Salto)	3	1	$U 2	12
2	October 2018	11	6	$U 2	6
3	December 2018	3	2	$U 3	4
4	January 2019	12	7	$U 2	3

6.1.1 Wave specific regression results

Figure 6 graphically illustrates the effect of the price for the four waves. In each case, the red line (hollow circle) plots the monthly number of bags delivered by the average treated branch in the corresponding set and the green line (hollow triangle) plots the monthly number of bags delivered by the average branch in the set of the 56 branches that did not price the bags. The vertical line marks the beginning of the treatment. Panel (a) of Figure 6 shows the first wave (Salto). In this case, the period covers 12 months before and 12 months after the price. Panel (b) shows the second wave of the pricing initiative. In this case, the post treatment period covers six months. Panel (c) shows the third wave. This is the only wave in which p = UY$3. In this case, our sample covers the first four months of the post-treatment period. Lastly, Panel (d) shows the fourth wave. In this case, our sample covers the first three post-treatment months.

The overall picture is that pricing for single-use plastic bags had a large, immediate and persistent negative effect on the quantity of bags used by customers, relative to the control group, regardless of the location of the branch and the date of implementation.

Figure 6: Average number of bags delivered by treated branches in each wave and control branches, by month



To formally determine the magnitude and significance of the effects, we estimate the following equation, for each wave:

$$B_{b,m} = \alpha + \delta_m + \mu_b + \beta(Treated \times Post)_{b,m} + \gamma Treated_b + \delta Post_m + \varepsilon_{b,m} \qquad (2)$$

As in the case of equation 1, here $B_{b,m}$ represents the number of bags delivered by branch *b* on month *m*, and δ_m and μ_b are month and branch fixed effects, respectively. $Treated_b$ and $Post_m$ are indicator variables for the branches pricing the bags and the months after the price, respectively, and $\varepsilon_{b,m}$ is the error term, clustered by branch. Again, β is our coefficient of interest, capturing the difference-in-difference effect of the price for the average treated branch in the wave in question, compared to the average control branch in the set of the 56 branches that did not price the bags. We also estimated variations of the above equation, including combinations of branch fixed-effects, month fixed-effects, and branch-specific time trends.

Table 4 shows the results of the OLS estimation of equation (2), for each wave.[13] The 12-month effect of the price in Salto (column (A)) is -74.9%. Column B shows that the UY$2 price decreased the demand for bags 85%, on average, in the branches and cities that constituted the second wave, during the first six months. Finally, the same price produced a decrease of 70.5% in the use of bags in the first three months in the branches and

[13] The full set of results for each wave are included in the Appendix.

cities of the fourth wave, on average. In sum, compared to the average branch in the set of 56 that did not price the bags during the period, a price of $U 2 produced a drop between 70.5% and 84.7%. A price of UY$3 (third wave) produced a drop of 81%. Summed over branches and months, the UY$2 price discouraged the use of an estimated 9.085 million bags and a price of UY$3 discouraged the use of 485,400 bags.

Table 4: Wave specific regression results

	(A)	(B)	(C)	(D)
	Salto April 2018	Second wave October 2018	Third wave December 2018	Fourth wave January 2019
Price = 2				
Estimate of β	-93.52***	-57.20***		-53.98***
Standard error	(7.44)	(7.06)		(8.54)
Pre-treatment mean of treated with P=2	124.85	67.57		76.52
Percentage change	-74.9%	-84.7%		-70.5%
Price = 3				
Estimate of β			-40.45***	
Standard error			(7.08)	
Pre-treatment mean of treated with P=3			49.93	
Percentage change			-81.0%	
N	1,429	1,621	1,428	1,644

Notes: Difference-in-difference estimates. Outcome variable: number of bags delivered/sold by branch, by month. Control group: 56 stores that did not change prices. Mean before treatment is the average number of bags delivered by treated stores (in each experiment) when price was zero (pre-treatment). Controls include month and branch fixed effects. Standard errors (in parenthesis) clustered at the branch level.
* p<0.10, ** p < 0.05, *** p < 0.01

The DiD estimate from our full experiment (Table 2) is a weighted average of all the possible two-group/two-period DiD estimators (Goodman-Bacon, 2019). Using his decomposition, we can study how each of these estimators contribute to the main estimates. We find that 87% of the variation in the data that is responsible for our main estimates comes from the comparison of the treated units against the pure control group of the 56 stores that did not price the bags.[14] This result means that our wave specific estimates from Table 4 are the main force behind the full-experiment results in Table 2. Moreover, our waves specific individual DiD results can be easily linked one-to-one with the Bacon decomposition of the full experiment (see appendix 10.7). Doing this, we find that the DiD main estimation places the largest weight on the October wave of the experiment, followed by the January and April experiments. All 2x2 comparisons yield a negative treatment effect.

[14] The other sources of variation for our DiD are the comparison of (1) earlier treated branches with later treated ones acting as controls (weighing 10%), and (2) later treated branches with earlier treated ones acting as controls (weighing 3%). For this exercise, we define treatment as pricing the bags, either 2 or 3 UY$. The combined effect of the two prices is a drop of 62 thousand bags (a weighted average between the drops of 64 and 42 thousand bags from Table 2, corresponding to a price of UY$2 and UY$3, respectively). To have a balanced panel, we excluded the four branches that went out of business before the first wave of the experiment (see footnote 10).

6.1.2 Wave-specific synthetic controls

In this section, we use synthetic controls as another identification strategy for the estimation of the wave-specific effects.

Although we test for the parallel trend assumption in the previous diff-in-diff analyses, the ability of the control group to reproduce the counterfactual outcome trajectory that an average branch in each of the waves would have experienced in the absence of the intervention may still be questioned. There are several reasons why. One is that the different sizes of branches in the treatment and the control groups. Another reason is, as in any diff-in-diff analysis, the possible presence of unobservable, time-varying differences between treated and control branches. For all these reasons, we repeat the analysis using a synthetic control method (Abadie and Gardeazabal, 2003). The idea behind this method is that a combination of untreated branches may provide a better comparison for the branches exposed to the price. The synthetic control method introduces control for the time-varying heterogeneity because the combination of branches comprising the synthetic control is the result of an optimization across branches and time (Bueno and Valente, 2019).

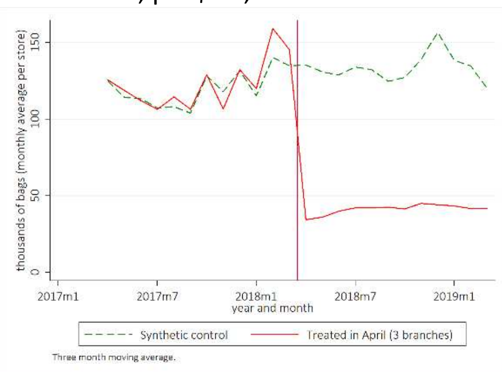
The donor pool in every case is comprised of the 56 branches that serve as control in the diff-in-diff estimations, but the length of the pre-treatment period for each wave of the experiment is different. To fit each of the wave-specific synthetic controls, we use all the observations of the pre-treatment period for that wave of the experiment, except for a *validation* period comprised of the last three months before the intervention.

Figure 7 shows the monthly levels of bags delivered or sold by the average treated branch in each wave of the experiment and its synthetic control.[15]

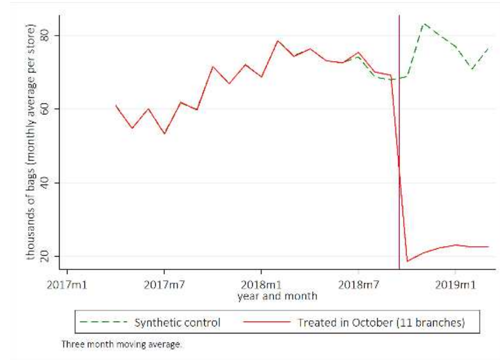
[15] In Appendix 10.8 we show that results are robust to leave-one-out estimations of the synthetic control (Abadie, forthcoming).

Figure 7: Average number of bags delivered by treated branches in each wave (red line) and its synthetic control (green line), by month

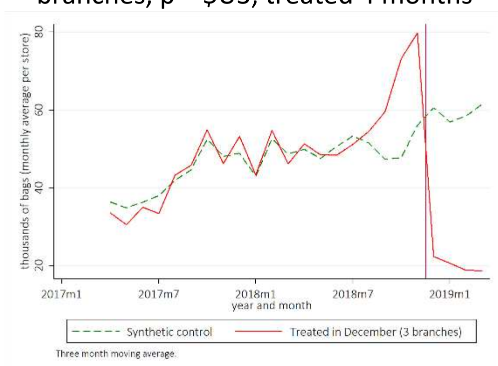
Panel (a) First wave: April 2018 (Salto), 3 branches, p = $U2, treated 12 months



Panel (b) Second wave: October 2018, 11 branches, p = $U2, treated 6 months



Panel (c) Third wave: December 2018, 3 branches, p = $U3, treated 4 months



Panel (d) Fourth wave: January 2019, 12 branches, p = $U2, treated 3 months

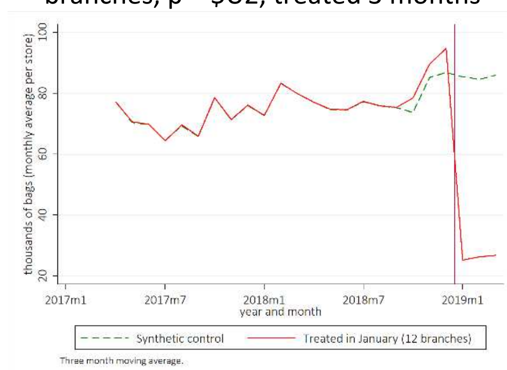


Table 5 shows the diff-in-diff estimation of the effect of the price, for each wave, against a wave-specific synthetic control. In general, these are similar to those showed in Table 4. The only exception may be the fourth wave. In this case, the (three-month) effect of the price estimated with a synthetic control is -79%, while it was -70.5% when estimated by equation 2.

Table 5: Wave-specific treatment effects, synthetic controls

	(A)	(B)	(C)	(D)
	Salto April 2018	Second wave October 2018	Third wave December 2018	Fourth wave January 2019
Price = 2				
Average pre-treatment difference	2.99	0.19		0.89
Average post-treatment difference	-92.31	-54.30		-59.27
Difference in difference	-95.30	-54.49		-60.17
Percentage change	-77.4%	-80.4%		-79.0%
Price = 3				
Average pre-treatment difference			2.83	
Average post-treatment difference			-39.22	
Difference in difference			-42.05	
Percentage change			-85.2%	
N	48	48	48	48

Notes: Outcome variable: thousand bags delivered/sold by branch, by month. Control group: wave-specific synthetic control from donor pool of 56 stores that did not change prices. Percentage change is the difference-in-difference drop as percentage of the average number of bags delivered by treated stores (in each experiment) when price was zero (pre-treatment).

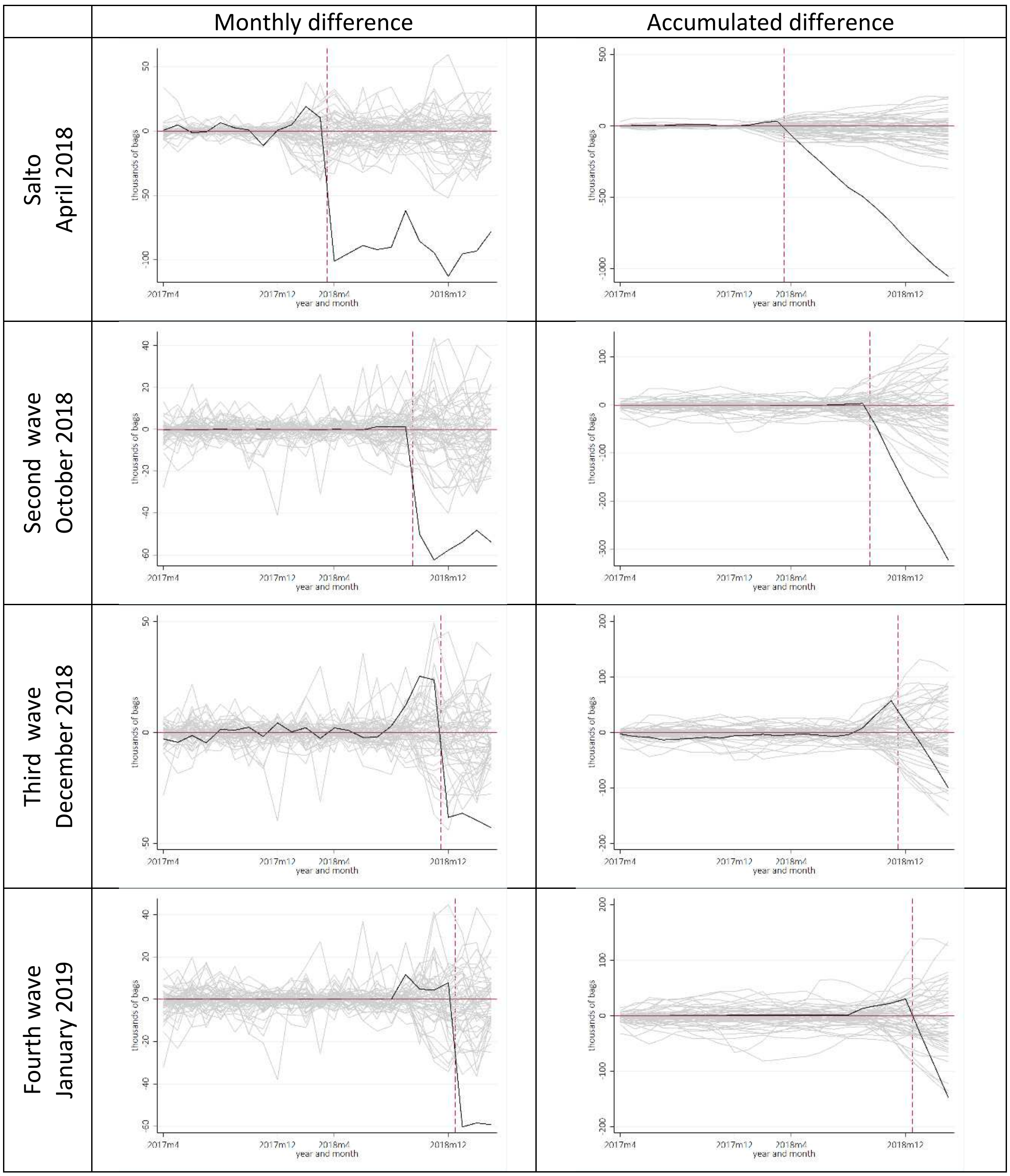
Overall, what the wave-specific results show is that putting a price of UY$2 or UY$3 decreased the demand for single-use plastic bags considerably. The size of the drop lies between 70% and 85%, depending on the cities and identification strategy, with no clear difference between the two prices. They show also that the size of the average effect of the price of UY$ 2 is robust to all the possible differences introduced with the rollout. In particular, city and branch sizes, time spell of the intervention, and more remarkably, the lack of agreement on pricing the bags with other stores in town.

6.2 Placebo tests

In this section, we perform placebo tests to assess the statistical significance of the reduction in bag consumption in the synthetic control estimation. In these tests, we assign the treatment status to each unit in the control group and we estimate "placebo effects" by applying the synthetic control method. As suggested by Abadie (2019), we exclude the treated units from the donor pool in the placebo iterations, and we exclude counterfactuals with a poor pre-treatment fit, defined as the five placebo units with worst pre-treatment MSPE. Figure 8 compares the actually treated average branch in each wave with the placebo distribution that results from the permutation exercise. It shows the difference in the number of bags delivered between each of the 56 placebo branches and its synthetic (grey lines) and the treated branch and its synthetic (black lines). Panels on the left show the difference by month and panels on the right show the accumulated difference. The difference between the average treatment effect on the actually treated branches and that on the placebo branches is easy to observe. In each experiment, the effect of the pricing on the branches affected by the treatment is an extreme value relative to the permutation distribution. We can therefore conclude that the decrease in the consumption of plastic bags does not seems to be random. The estimated Average Treatment effect on the Treated (ATT) is larger than the estimated ATT for the placebo branches.

The accumulated differences (right panel) are also an extreme value in the placebo distributions. A possible exception may be the third wave (December 2018 experiment), in which the accumulated difference is not the lowest value of the series. The reason may be that the three treated branches increased the delivery of bags in a notorious way in the months before the beginning of the policy. (We will address this issue in the next section). Another possible reason is that we only observe three months of the post-treatment period for this wave. In other words, it is possible that the accumulated difference between the units treated in December and their synthetic control would become the largest if we could have observed more months in the series, as it does in waves with longer post-treatment periods.

Figure 8: Placebo tests: difference in number of bags delivered by treated (black lines) and placebo (grey lines) branches with corresponding synthetic control



Based on the result of the placebo tests, we conclude that it is difficult to argue that the drop in the consumption of single-use plastic bags was random, and not caused by the price. Figure 8 also shows that the effect of the price persists over time.

6.3 Anticipation effects

With the only exception of the second wave (October 2018), Figure 8 shows that treated branches exhibit a rise in the consumption of bags during the last months before the intervention, relative to their synthetic control. This rise is not an extreme value in the placebo distribution, but it is nonetheless an upper value. An increase in consumption before the pricing started may be the result of an anticipation effect. This occurs when treated subjects know in advance that they are going to be treated and they react strategically. We cannot rule out the possibility that clients may have known that bags would be priced in advance, at least in some cases. In Salto, for example, clients were informed of the future price during the campaign led by the city chamber of commerce. Figures 7 and 8 show that customers may have increased the use of bags before that price went into effect, possibly to stock costless bags. We need to take into account this possibility in the estimation of the effect of the price because, if present, it may bias the result. This is what we do in this section.

To include the anticipation effect in the estimation, we backdate the intervention period and divide it in two: an announcement period and an implementation period. The announcement period is the period in which subjects are informed about the future implementation of the price. The actual implementation of the price marks the start of the implementation period. We set the length of the announcement period to 4 months. The reason for choosing 4 months (for all waves) is that the municipal government and the chamber of commerce of Salto (first wave) held a press conference to launch the campaign four months before the price (in December 4, 2017). No press conference took place in the rest of the waves, in which the price was a private decision. Nevertheless, we use the same length for the rest of the waves for consistency, although more customers may have found out about the price at an earlier date in Salto than in the rest of the waves.

By dividing the intervention period in two, we are able to estimate separately the effect of the anticipation of the price and the effect of the price itself. To do this, we estimate the following equation, for each wave of the experiment:

$$B_{b,m} = \alpha + \delta_m + \mu_b + \beta_1(Treated \times Anticipation\_period)_{b,m} + \beta_2(Treated \times Pricing\_period)_{b,m} + \beta_3 Treated_b + \beta_4 Anticipation\_period_m + \beta_5 Pricing\_period_m + \varepsilon_{b,m} \quad (3)$$

As in the case of previous equations, here $B_{b,m}$ represents the number of bags delivered by branch *b* on month *m*, and δ_m and μ_b are month and branch fixed effects, respectively. $Treated_b$ is an indicator variable for the branches pricing the bags. $Anticipation\_period_m$ is an indicator variable that takes the value of 1 in the months previous to the implementation of the price. During these months, the bags where still free, but clients could have known that the supermarket would price them at the implementation date. $Pricing\_period_m$ is another indicator variable for the months in which the price was in effect. Finally, $\varepsilon_{b,m}$ is the error term, clustered by branch. Our coefficients of interest are β_1 and β_2. The former captures the difference-in-difference effect of the announcement. The latter, the effect of the price. In both cases, the estimation compares the average treated branch in the wave in question with the average branch in the set of the 56 branches that did not price the bags during the whole period of analysis.[16]

Results of the estimation of equation (3) are in Table 6. Each column (A) to (D) represents the results of a specific wave. As shown in line 3 of Table 6, we find evidence consistent with an anticipation effect for the cases of the first and third waves. In the case of the first wave (column A), clients of the supermarket increased the demand for bags by an average 10.6% during the 4 months previous to the implementation of the price, relative the average pretreatment mean number of bags delivered by the treated branches. This number is 37.7% in the case of the third wave (column C). On the other hand, we do not find a statistically significant diff-in-diff number of bags in the second and fourth waves. In waves one and three, the supermarket in question was not the only store pricing the bags in town. On the contrary, in waves two and four, it was the only one. Therefore, these findings are consistent with our hypothesis that the clients in Salto, informed about a future price, demanded more costless bags before the implementation of the price, possibly for future use. It also suggests that the supermarket informed their clients about the implementation of the price, or that this information leaked, in the towns of the third wave, where the price was the result of an agreement with more supermarkets. On the other hand, clients do not seem to have anticipated the price in the towns in which the supermarket was the only one pricing the bags.

Table 6: Anticipation effects

	(A) April experiment	(B) October experiment	(C) December experiment	(D) January experiment	(E) Full Experiment
Pre-treatment mean	119.7	66.57		74.58	72.22
for treated			45.02		45.02
DiD anticipation	12.74***	-2.55	16.97***	-0.47	3.89
	(3.47)	(1.98)	(3.45)	(4.03)	(2.44)
pct change	10.6%	-3.8%	37.7%	-0.6%	5.6%
DiD Price = 2	-89.27***	-57.77***		-54.07***	-62.31***
	(7.00)	(7.04)		(8.59)	(5.64)
pct change	-74.6%	-86.8%		-72.5%	-86.0%
DiD Price = 3			-37.00***		-40.92***
			(7.50)		(7.09)
pct change			-82.2%		-90.9%
N	1,429	1,621	1,428	1,644	2,075

Notes: Difference-in-difference estimates corresponding to equation 3. Each column is a different regression. Outcome variable: number of bags delivered/sold by branch, by month. Control group: 56 stores that did not charge prices. Controls include month and store fixed effects. Standard errors (in parenthesis) clustered at the branch level. * p<0.10, ** p < 0.05, *** p < 0.01

Regarding the direct effect of the price on the quantity of bags delivered, we find that not taking into account the anticipation effect would overestimate the true impact of the policy. Take the case of Salto, for example. In Table 4, the estimated treatment effect was -93.520 bags per month. When we disentangle the

[16] We also estimated, for each wave, variations to equation (3), including combinations of branch fixed-effects, month fixed-effects and branch-specific time trends. The results are in the appendix.

anticipation effect from the longer-term effect (Table 6), we find that the policy caused a drop of 89.270 bags.[17] The difference between the two models is larger in the case of the third wave (December), since the bias introduced by the anticipation effect was also larger.

Another way to see the advantage of having a long pre-treatment monthly period to rule out anticipation effects is to directly study what our policy evaluation would have been if we only had three months of pre-treatment data. As shown in Table 7, the difference-in-difference OLS coefficient of the effect of the price in Salto when using only three months before and three months after the price (line 4 of column A) is 28% higher than that presented in Table 4 (showed again in Table 7, line 1). In the case of the third wave, column C, the coefficient is 41% higher. In other words, short-term impact evaluations may look very different from longer-term evaluations, particularly when subjects anticipate the policy.

Table 7: Three-month vs longer run effects

	(A) April experiment	(B) October experiment	(C) December experiment	(D) January experiment
A. 24 months estimation	-93.52***	-57.20***	-40.45***	-53.98***
	(7.44)	(7.06)	(7.08)	(8.54)
N	1.429	1.621	1.428	1.644
B. 6 months estimation	-119.27***	-61.27***	-57.20***	-52.89***
	(11.08)	(8.88)	(5.72)	(9.71)
N	353	401	353	408
Difference in coefficients	28%	7%	41%	-2%

Notes: Difference-in-difference estimates. Outcome variable: number of bags delivered/sold by branch, by month. Control group: 56 stores that did not change prices. Controls include month and branch fixed effects. Standard errors (in parenthesis) clustered at the branch level. * p<0.10, ** p < 0.05, *** p < 0.01

7 Discussion

A possible concern regarding our results is that the drop in the use of bags that we observe may not only be the effect of prices, but also the consequence of a loss of sales. Because the supermarket chain was the only store pricing plastic bags in most of the cities, clients could have well opted to go to other stores that were not charging the bags in the same cities. Even in Salto (wave 1), La Paz and Las Piedras (wave 3), where the supermarket was not the only one pricing the bags, there were stores giving out plastic bags for free, so clients could have opted to go to these other stores in these cities also. To assess whether this actually happened, and to what extent, the ideal test would be to conduct a diff-in-diff analysis between the monthly value of sales of all branches that priced the bags (treated) and those branches that did not (control). However, we do not have this data. Nonetheless, we could gather data of monthly sales for the three branches in the city of Salto (treated) and two of the 56 control branches that did not price the bags during the sample period. This data spans, as the bags

[17] When we express the coefficient in terms of percentage change of the pre-treatment mean, the two magnitudes are almost identical: -74.9 vs -74.6. The reason is that the pre-treatment mean is not the same in Table 4 and 6. In the first case, that mean is higher because it includes the anticipation period. Therefore, both the estimated coefficient and the pre-treatment mean are higher.

data, from April 2017 to March 2019. The two control branches are located in the close cities of San Carlos and Maldonado. Maldonado is the twin city of Punta del Este, an international summer resort located on the southeastern Atlantic coast of Uruguay. It therefore experiences more seasonal variation than the rest. With this data, we perform a diff-in-diff OLS estimation identical to those above (using branch and month fixed effects, and errors clustered at the branch level) to estimate a possible effect of pricing the bags on sales. Results show that sales in Salto decreased 4.3% relative to the control branches, on average, although the result is not statistically significant at the 10% level (p value = 0.197; 95% CI: -12.1, 3.4). Consistently, when comparing Salto to Maldonado, we see that the OLS estimate of the average effect of the price on the number of bags is a drop of 83.9% (p value = 0.000, 95% CI: -129.8, -79.7 monthly thousand bags). At the same time, the effect on the number of bags *per dollar of sales* is a drop of 82.7% (p value = 0.000, 95% CI: -0.119, -0.089 bags per dollar of sales). Thus, even considering a possible loss of sales, the policy had a large impact, similar to our main results. Another way of expressing this result is that the average customer expenditure per disposable plastic bag increased from 9.2 to 27.1 dollars.

Although we would need more data to formally rule out the possibility that pricing the bags caused a loss in sales, a careful look at the context and some of the results above suggests otherwise. If clients had moved to others stores as a response to the price of plastic bags, we would observe lower estimates of the effect of the price in cities were the supermarket was the only store pricing the bags than in those in which it was not (Salto, La Paz, Las Piedras). Nevertheless, we can see in Table 4 that the effect of p = 2 when the supermarket is the only one pricing the bags is -85% (October 2018 wave) and -71% (January 2019 wave), while it is -75% (Salto) and -81% (December 2018 wave) when it is not. Not finding consistent evidence that pricing plastic bags caused the supermarket to lose sales makes sense. Take the first wave (Salto). A loss of clients in this case is improbable because, as commented in Section 3, the estimated market-share of the supermarket there is between 40% and 50%. Moreover, all supermarkets in Salto, and a significant proportion of grocery stores, street markets, bakeries and butcher shops in the city adopted the price. Very possibly, this made the substitution of the supermarket for others stores very costly for its clients, who would have needed to walk to separate stores to shop for different products. In addition, these smaller stores surely had higher prices. Therefore, the cost of substituting the bags for the clients of the supermarket was not only that of the opportunity cost of walking-time, but also that of the higher expenditure in groceries and other products. The substitution of the supermarket for other stores may even had been physically impossible in the short run, as those relatively small and few stores needed to serve a relatively large number of customers. Although to a lesser extent, similar arguments are valid for the towns of the third wave (La Paz and Las Piedras), where all supermarkets priced the bags, and even in the cases in which the supermarket was the only store in town pricing the bags. Finally, if the supermarket had lost clients because of pricing the bags, it would have not rolled out the price to other branches in other cities, as it did. The *voluntary* rollout suggests, quite the contrary, that pricing the bags may even have been profit increasing.

A permanent drop between 70% - 85% is larger than what previous studies report. A US$ 0.07 tax on all disposable *paper and plastic* bags in the city of Chicago decreased the use of these bags almost one bag per trip on average in the first two months, from a baseline of 2.3 (a decrease of 56.5%). Nevertheless, the tax did not exhibit an effect statistically different from zero in the following months of the first year (Homonoff et al., 2018). A US$ 0.05 levy on disposable *paper and plastic* bags in Montgomery County produced an average decrease of 0.22 disposable bags used per user per trip in the first three months; a change of 8% (Homonoff, 2018). A possible reason behind this difference is that we perform the analysis in a less-developed country, where customers are poorer than customers in the US. Nevertheless, we are cautious with this argument. The reason is that Homonoff et al. (2020) did not find evidence of different impacts across neighborhoods with different incomes in Chicago, and neither do we in Montevideo, albeit with a simpler, cross-section comparison.[18] In any case, our results are consistent with the argument that a zero price is a special price. As documented by Shampanier et al. (2007), people consume free goods in excess of what a standard cost-benefit analysis would predict. The reason behind this seems to be that people experience an additional affection for free goods and services. Our results are also consistent with the findings that small prices cause relatively large effects on the demand for goods and services in education and health (Holla and Kremer, 2009; Banerjee and Duflo, 2009).

Finally, a cautionary note. Although our results indicate that a relatively low price can cause a large drop in the use of plastic shopping bags, readers should not interpret this drop as a measure of the environmental effect of the policy. A reason for this is that a charge on disposable plastic shopping bags may increase the demand for plastic trash bags, a known unintended effect. As documented by Taylor (2019), the increase in the demand for trash bags, measured in plastic weight, may be as large as 1/3 of the drop caused by the tax on shopping bags. Unluckily, we do not have the data to estimate the possible increase in the demand for plastic trash bags in this work.

8 Conclusion

We find that a US$ 0.07 and US$ 0.1 prices per plastic bags caused a relatively large drop in the number of plastic bags used by customers of a discount supermarket chain. Depending on the number of months in the post-treatment period, the number of treated branches, the control group and the price, the estimated drop lies between 70% and 85%. To estimate these numbers, we perform diff-in-diff estimations, using the supermarket branches that were not pricing the bags as control group. Estimates are of similar value and statistically robust to different specifications of our basic estimated equation and the use of synthetic controls. Placebo tests lead us to conclude that the effect that we find is not the result of chance. In some waves of the rollout of the pricing initiative, the clients may have anticipated, or simply been informed about the upcoming price. In these cases, we find evidence of a strategic increase in the demand for zero-price bags during the months right before the implementation of the price. This strategic behavior considerably increases the estimated effect of the price in the short run. We do not find evidence consistent with a loss in sales being the mechanism behind the drop in the demand for bags. Our estimates are consistent with the evidence in the literature that zero prices are special prices and therefore elasticities around these prices tend to be large. Finally, although our results indicate that a relatively low price can cause a large drop in the use of plastic bags, readers should not interpret this drop as a measure of its environmental effect. A charge on disposable plastic shopping bags may increase the demand for plastic trash bags, a known unintended effect. A comprehensive evaluation of the environmental impacts of a charge on disposable plastic bags should consider this rebound effect.

[18] The drop in the quantity of bags across neighborhoods in Montevideo is not statistically correlated with their average income. To obtain this result, we first computed the average neighborhood income for the period 2013 – 2018, using data from the Continuous Household Survey of the National Statistics Institute (Instituto Nacional del Estadística, 2018). We then computed the drop in bags consumption for each of the 43 branches that the supermarket has in 27 neighborhoods of Montevideo, between February 2018 and April 2018, when all supermarkets in the country priced the bags UY$ 4. The average drop across neighborhoods is 82.6% and it is negatively correlated, but not statistically significant (p value = 0.273) with household income.

9 References

Abadie, A. (2019). Using Synthetic Controls: Feasibility, Data Requirements, and Methodological Aspects, *Journal of Economic Literature*, forthcoming.

Abadie, A. & Gardeazabal, J. (2003). The economic costs of conflict: A case study of the Basque Country. *American Economic Review,* 93(1), 113-132.

Banerjee, A. V., & Duflo, E. (2009). The experimental approach to development economics. *Annu. Rev. Econ.*, *1*(1), 151-178.

Barnes, D. K., Galgani, F., Thompson, R. C., & Barlaz, M. (2009). Accumulation and fragmentation of plastic debris in global environments. *Philosophical Transactions of the Royal Society B: Biological Sciences*, *364*(1526), 1985-1998.

Bueno, M., & Valente, M. (2019). The effects of pricing waste generation: A synthetic control approach. *Journal of Environmental Economics and Management*, *96*, 274-285.

Centro Comercial e Industrial de Salto (2018). Información práctica sobre la Campaña "Somos limpios, somos felices".

Convery, F., McDonnell, S., & Ferreira, S. (2007). The most popular tax in Europe? Lessons from the Irish plastic bags levy. *Environmental and Resource Economics*, *38*(1), 1-11.

Diario El Pueblo (2018). "De manera dispar comenzó la aplicación de la campaña que busca disminuir el uso de bolsas de nylon en los comercios. April 3, 2018.

Dikgang, J., Leiman, A., & Visser, M. (2012a). Elasticity of demand, price and time: lessons from South Africa's plastic-bag levy. *Applied Economics*, *44*(26), 3339-3342.

Dikgang, J., Leiman, A., & Visser, M. (2012b). Analysis of the plastic-bag levy in South Africa. *Resources, Conservation and Recycling*, *66*, 59-65.

El Observador (2019). Arranca el cobro de las bolsas aunque estas no se ajustan a los requisitos de la ley. April 1, 2019.

Goodman-Bacon, A. (2019). Difference-in-Differences with Variation in Treatment Timing.

Goodman-Bacon, A., Goldring, T. & Nichols, A. (2019) *bacondecomp*: Stata module for Decomposing difference-in-differences estimation with variation in treatment timing.

He, H. (2012). Effects of environmental policy on consumption: lessons from the Chinese plastic bag regulation. *Environment and Development Economics,* 17, 407-431.

Holla, A., & Kremer, M. (2009). Pricing and access: Lessons from randomized evaluations in education and health. *Center for Global Development working paper*, (158).

Homonoff, T. A. (2018). Can Small Incentives Have Large Effects? The Impact of Taxes versus Bonuses on Disposable Bag Use. *American Economic Journal: Economic Policy*, *10*(4), 177-210.

Homonoff, T., Kao, L. S., Selman, J., D., & Seybolt, C. (2020). Skipping the Bag: The Relative Effectiveness of Bans versus Taxes

Intendencia de Salto (2018). Resolución Municipal 074/18

Instituto Nacional de Estadística (2018). Encuesta Continua de Hogares.

Jakovcevic, A., Steg, L., Mazzeo, N., Caballero, R., Franco, P., Putrino, N., & Favara, J. (2014). Charges for plastic bags: motivational and behavioral effects. *Journal of Environmental Psychology*, 40, 372-380.

Lönnstedt, O. M., & Eklöv, P. (2016). Environmentally relevant concentrations of microplastic particles influence larval fish ecology. *Science*, *352*(6290), 1213-1216.

Kőszegi, B., & Rabin, M. (2006). A model of reference-dependent preferences. *The Quarterly Journal of Economics*, 121(4), 1133-1165.

Martinho, G., Balaia, N., & Pires, A. (2017). The Portuguese plastic carrier bag tax: The effects on consumers' behavior. *Waste management*, *61*, 3-12.

Moore, C. J. (2008). Synthetic polymers in the marine environment: a rapidly increasing, long-term threat. *Environmental research,* 108(2), 131-139.

Nielsen, T. D., Holmberg, K., & Stripple, J. (2019). Need a bag? A review of public policies on plastic carrier bags–Where, how and to what effect? *Waste management*, *87*, 428-440.

Poortinga, W., Whitmarsh, L., & Suffolk, C. (2013). The introduction of a single-use carrier bag charge in Wales: Attitude change and behavioural spillover effects. *Journal of Environmental Psychology*, *36*, 240-247.

Rivers, N., Shenstone-Harris, S., & Young, N. (2017). Using nudges to reduce waste? The case of Toronto's plastic bag levy. *Journal of Environmental Management*, *188*, 153-162.

Shampanier, K., Mazar, N., & Ariely, D. (2007). Zero as a special price: The true value of free products. *Marketing science*, *26*(6), 742-757.

Taylor, R. L. (2019). Bag leakage: The effect of disposable carryout bag regulations on unregulated bags. *Journal of Environmental Economics and Management*, *93*, 254-271

Taylor, R. L. (2020). A Mixed Bag: The Hidden Time Costs of Regulating Consumer Behavior. *Journal of the Association of Environmental and Resource Economists*, *7*(2), 345-378.

Taylor, R. L., & Villas-Boas, S. B. (2016). Bans vs. Fees: Disposable Carryout Bag Policies and Bag Usage. *Applied Economic Perspectives and Policy*, *38*(2), 351-372.

Tversky, A. & Kahneman, D. (1991). Loss aversion in riskless choice: A reference-dependent model. *The Quarterly Journal of Economics,* 106(4), 1039-1061.

UNEP (2014). *Valuing Plastics: The Business Case for Measuring, Managing and Disclosing Plastic Use in the Consumer Goods Industry*. http://hdl.handle.net/20.500.11822/9238

UNEP (2018). *From birth to ban: A history of the plastic shopping bag.* http://www.unep.org/news-and-stories/story/birth-ban-history-plastic-shopping-bag

UNEP (2018). *Legal Limits on Single-use Plastics and Microplastics: A Global Review of National Laws and Regulations* http://hdl.handle.net/20.500.11822/27113

Xanthos, D., & Walker, T. R. (2017). International policies to reduce plastic marine pollution from single-use plastics (plastic bags and microbeads): A review. *Marine Pollution Bulletin,* 118, 17–26.

10 Appendix

10.1 Initiatives to reduce the consumption of plastics bags around the world

At the national level, Germany in 1991 and Denmark in 1994 appear to be the first countries that implemented taxes or levies to producers of plastic bags and retail stores delivering them (Xanthos and Walker, 2017).[19] Bangladesh appears to be the first country in the world to ban thin plastic bags in 2002, after a disastrous flooding (UNEP, 2018). During the same year, Ireland implemented a famous levy (Convery et al., 2007). Starting with South Africa in 2002, several African and Asian countries introduced bans on plastic bags in the following years. In 2007, Botswana introduced a levy of approximately 5 cents of US dollar and Kenya one for thicker bags (Xanthos and Walker, 2007). South Korea (1999) led the way for pricing mechanisms in Asia, followed by several attempts in Taiwan (starting 2003), China (2008), Hong Kong (2009) (Nielsen et al., 2019). Several countries, cities and provinces around the world followed.[20] In North America, six Canadian municipalities banned plastic bags between 2007 and 2010. Prime Minister Justin Trudeau announced on June 17 2019, that Canada would ban plastic bags in 2021.[21] In the US, between 2007 and today, cities, counties and states passed 156 norms regulating the use of disposable single-use carryout bags.[22] Of these, only 12 are levies (10 cities, Suffolk County, NY, and Washington DC). The rest are bans, some combined with a charge on paper bags, such as the one that recently (March 1st, 2020) came into effect in NYC.

The European Union passed the Directive 2015/270 in April 2015, which requires Member States to take either or both of the following measures. First: ensure that the annual consumption does not exceed 90 lightweight plastic carrier bags per person by 31 December 2019 and 40 lightweight plastic carrier bags per person by 31 December 2025, or equivalent targets set in weight. Second: adopt instruments ensuring that points of sale of goods or products do not provide lightweight plastic carrier bags free of charge by 31 December 2018, unless equally effective instruments are implemented (EU, 2015). In South America, the city of Buenos Aires established a charge for plastic bags at the end of 2012 (Jakovcevic et al., 2012) and later banned plastic bags in supermarkets in 2017. Chile became the first Latin American country to ban plastic bags in supermarkets by law, since February 2019. (See Nielsen et al. (2019) for a more comprehensive account of bans and levies across the world). The above list of initiatives does not cover voluntary agreements between governments and retailers to reduce plastic bags, private company initiatives, social awareness campaigns, waste management systems improvements or promotion of ecological alternatives. It does no cover, also, other regulations such as thickness requirements, material composition, production volume or number restrictions, extended producer responsibility, etc. As of July 2018, one hundred and twenty-seven (127) countries out of 192 reviewed have adopted some form of legislation to regulate plastic bags (United Nations Environment Programme, 2018).

---

[19] Some authors use "levy" to refer to the charge on disposable bags. Others, fee. The different names that the charge takes may be the result of the name it takes in the regulations. In any case, as it is customary in the literature, we use the terms "tax", "levy", "fee" and "charge" interchangeably.

[20] The site https://www.earthday.org/plasticban/ maintains an updated list of efforts of regions, countries, cities and businesses to ban the use of plastics bags.

[21] https://web.archive.org/web/20191018102313/https://www.theguardian.com/world/2019/jun/10/canada-ban-single-use-plastics-bags-bottles-straws-2021

[22] A US list of Plastic Bag ordinances is available at https://www.cawrecycles.org/list-of-national-bans. *Accessed June 6, 2019.*

10.2 The campaign sign

Figure A.1 shows the campaign sign that the Municipal Government and the Industrial and Commercial Center displayed in supermarkets and stores in Salto, during the launch of the pricing initiative, at the entrance of supermarkets and stores. The signs informed readers about the existence of a campaign to reduce the use of plastic bags in the city ("We are reducing plastic bags in Salto!"). It also informed the readers that the initiative was a joint effort of the municipal government and the commercial center (it included the number of the resolution by which the municipal government adhered to the center initiative, the logos of the two institutions, below the phrase "we join the initiative"). Finally, it included the campaign slogan ("We are clean. We are happy").

Figure A.1 The campaign sign in Salto

10.3 Salto (April 2018)

Table A.1 shows the number of plastic bags (thousands) delivered or sold by branches in Salto (treated) and the 56 branches in the rest of the country that did not price the bags during the period, before and after the branches in Salto started pricing the bags. It also shows difference in differences calculation of the effect of the price: 94,490 bags, per month and branch, on average.

Table A.1: Average number of bags delivered by branches in Salto (treated) and 56 branches that did not price the bags (control), before and after pricing the bags

	Before	After	Difference
Control	64.45	75.31	10.87
	(5.18)	(5.94)	(1.41)***
Treated	124.85	41.23	-83.62
	(11.95)	(4.55)	(8.70)**
Difference	60.41	-34.08	-94.49
	(11.12)***	(7.03)***	(7.26)***

Average bags delivered in each store (in thousands per month). Number of observations for the DiD regression: 1,429 Robust standard errors clusterd at the store level in parenthesis.

Figure A.2 shows the change in the initial difference in the number of delivered bags by branches in Salto and control branches, by month, and the 95% confidence interval (dashed lines). The figure illustrates that this difference was not statistical different from zero during the pre-treatment period, except for two months. The figure also shows a possible anticipation effect in the last months before the implementation of the price. As discussed in the main text, the possibility of an anticipation effect motivates our synthetic control analysis.

Figure A.2: Changes in the initial difference of bags delivered by month between control and treated branches

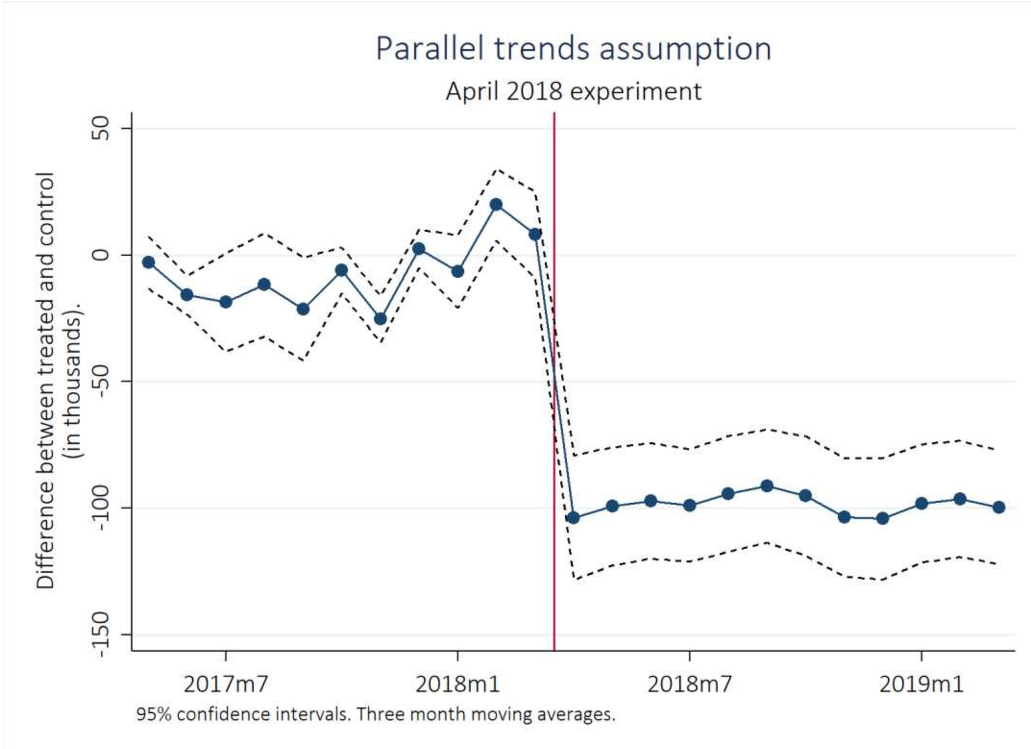


Table A.2 shows the OLS estimation results of the diff-in-diff coefficient estimation of the average treatment effect of the UY$2 price in Salto in first twelve months of implementation. These results are variations of equation (2). Column (A) presents de basic specification of the equation and the rest of the columns show the results with different combinations of branch fixed effects, month fixed effects and timer trends. Column (D) presents the results from our preferred specification included in the main text (see column (A) of Table 4).

Table A.2: Difference-in-difference OLS estimation

	(A) Basic DiD	(B) Branch FE	(C) Month FE	(D) Branch + Month FE	(E) D + Time trends
Treated*After	-94.49***	-93.52***	-94.34***	-93.52***	-100.74***
	(7.26)	(7.38)	(7.31)	(7.44)	(9.32)
Branch FE	No	Yes	No	Yes	Yes
Month FE	No	No	Yes	Yes	Yes
Time trends	No	No	No	No	Yes
N	1,429	1,429	1,429	1,429	1,429

Standard errors (in parenthesis) clustered at the branch level. Outcome variable: average number of bags delivered/sold by branch, by month. Sample period: 24 months between 2017m4 and 2019m3. Control group: 56 stores that were not treated in that period. * p<0.10, ** p < 0.05, *** p < 0.01

Table A.3 shows different specifications of equation (3), with data from the first wave of the experiment. Each column corresponds to a different regression. The first coefficient is the difference-in-difference effect of the *announcement* of the pricing (four months before the effective implementation) on the number of consumed bags. The second coefficient is the treatment effect for the *pricing* period. The results that we present in the main text (Table 6) are those of our preferred specification (column D, Table A.3).

Table A.3: Anticipation effects

	(A) Basic DiD	(B) Branch FE	(C) Month FE	(D) Branch + Month FE	(E) D + Time trends
DiD anticipation period	11.87***	12.92***	11.84***	12.74***	13.65***
	(3.46)	(3.43)	(3.50)	(3.47)	(3.84)
pct change	9.9%	10.8%	9.9%	10.6%	11.4%
DiD pricing period	-90.51***	-89.21***	-90.40***	-89.27***	-87.03***
	(6.86)	(6.95)	(6.91)	(7.00)	(8.29)
pct change	-75.6%	-74.5%	-75.5%	-74.6%	-72.7%
Branch FE	No	Yes	No	Yes	Yes
Month FE	No	No	Yes	Yes	Yes
Time trends	No	No	No	No	Yes
N	1,429	1,429	1,429	1,429	1,429

Difference-in-difference estimates corresponding to equation (3) in the main text. Each column is a different regression. Outcome variable: number of bags delivered/sold by branch, by month. Control group: 56 stores that did´t change prices. Standard errors (in parenthesis) clustered at the branch level. * p<0.10, ** p < 0.05, *** p < 0.01

10.4 Second wave – October 2018

Table A.4 shows the number of plastic bags (thousands) delivered or sold by branches in the second wave (treated) and the 56 branches in the rest of the country that did not price the bags during the period (control), before and after the treated branches started pricing the bags. It also shows difference in differences calculation of the effect of the price: 57,770 bags, per month and branch, on average.

Table A.4: Average number of bags delivered by branches in the second wave (treated) and 56 branches that did not price the bags (control), before and after the former priced the bags

	Before	After	Difference
Control	66.83	78.88	12.06
	(5.39)	(6.06)	(1.46)***
Treated	67.57	21.86	-45.71
	(8.9)	(2.02)	(7.00)***
Difference	0.74	-57.02	-57.77
	(10.10)	(6.36)***	(6.88)***

Average bags delivered in each store (in thousands per month). Number of observations for the DiD regression: 1,621. Robust standard errors clustered at the store level in parenthesis.

Figure A.3 shows the change in the initial difference in the number of delivered bags by treated branches and control branches, by month, and the 95% confidence interval (dashed lines). The figure illustrates that this difference was not statistical different from zero in any of the month of the pre-treatment period.

Figure A.3: Changes in the initial difference of bags delivered by month between control and treated branches

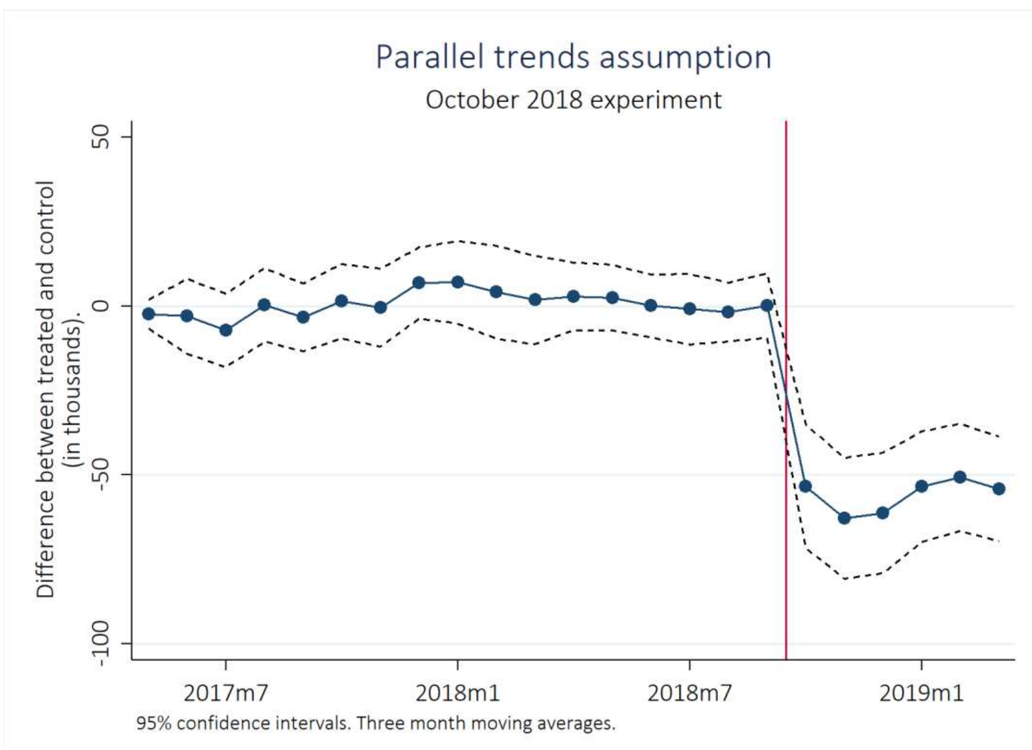


Table A.5 shows the OLS estimation results of the diff-in-diff coefficient estimation of the average treatment effect of the UY$ 2 price in the branches in the second wave of the experiment in first six months of implementation. Column (A) presents de basic specification of the equation and the rest of the columns show the results with different combinations of branch fixed effects, month fixed effects and timer trends. Overall, results show a very consistent estimation of a drop of 57 thousand bags, per branch, per month. A minor exception is the case in which we include time trends. In this case, the effect increases to 59 thousand bags.

Table A.5: Difference-in-difference OLS estimation

	(A) Basic DiD	(B) Branch FE	(C) Month FE	(D) Branch + Month FE	(E) D + Time trends
Treated*After	-57.77***	-57.18***	-57.66***	-57.20***	-59.20***
	(6.88)	(7.01)	(6.92)	(7.06)	(8.53)
Branch FE	No	Yes	No	Yes	Yes
Month FE	No	No	Yes	Yes	Yes
Time trends	No	No	No	No	Yes
N	1,621	1,621	1,621	1,621	1,621

Standard errors (in parenthesis) clustered at the branch level. Outcome variable: average number of bags delivered/sold by branch, by month. Sample period: 24 months between 2017m4 and 2019m3. Control group: 56 stores that were not treated in that period. * p<0.10, ** p < 0.05, *** p < 0.01

Table A.6 shows different specifications of equation (3), with data from the second wave of the experiment. Each column corresponds to a different regression. The first coefficient is the difference-in-difference effect of the *announcement* of the pricing (four months before the effective implementation) on the number of consumed bags. The second coefficient is the treatment effect for the *pricing* period. The results that we present in the main text (Table 6) are those of our preferred specification (column D, Table A.6).

Table A.6: Anticipation effects

	(A) Basic DiD	(B) Branch FE	(C) Month FE	(D) Branch + Month FE	(E) D + Time trends
DiD anticipation period	-3.39*	-2.58	-3.22	-2.55	-7.98**
	(2.00)	(1.96)	(1.99)	(1.98)	(3.83)
pct change	-5.1%	-3.9%	-4.8%	-3.8%	-12.0%
DiD pricing period	-58.49***	-57.75***	-58.37***	-57.77***	-66.32***
	(6.87)	(6.99)	(6.91)	(7.04)	(10.35)
pct change	-87.9%	-86.8%	-87.7%	-86.8%	-99.6%
Branch FE	No	Yes	No	Yes	Yes
Month FE	No	No	Yes	Yes	Yes
Time trends	No	No	No	No	Yes
N	1,621	1,621	1,621	1,621	1,621

Difference-in-difference estimates corresponding to equation (3) in the main text. Each column is a different regression. Outcome variable: number of bags delivered/sold by branch, by month. Control group: 56 stores that did´t change prices. Standard errors (in parenthesis) clustered at the branch level. * p<0.10, ** p < 0.05, *** p < 0.01

10.5 Third wave – December 2018

Table A.7 shows the number of plastic bags (thousands) delivered or sold by in the third wave (treated) and the 56 branches in the rest of the country that did not price the bags during the period (control), before and after former priced the bags. It also shows difference in differences calculation of the effect of the price: 40.95 thousand bags, per month and branch, on average.

Table A.7: Average number of bags delivered by treated and control branches, before and after pricing the bags

	Before	After	Difference
Control	67.90	79.46	11.55
	(5.43)	(6.21)	(1.82)***
Treated	49.93	20.53	-29.39
	(12.07)	(4.63)	(8.29)*
Difference	-17.98	-58.92	-40.95
	(11.33)	(7.30)***	(7.02)***

Average bags delivered in each store (in thousands per month). Number of observations for the DiD regression: 1,428. Robust standard errors clusterd at the store level in parenthesis.

Figure A.4 shows the change in the initial difference in the number of delivered bags by branches in the third pricing wave and control branches, by month, and the 95% confidence interval (dashed lines). The figure illustrates that this difference was not statistical different from zero during the pre-treatment period, except for three months before the implementation of the price (a possible anticipation effect). This motivates our synthetic control analysis

Figure A.4: Changes in the initial difference of bags delivered by month between control and treated branches

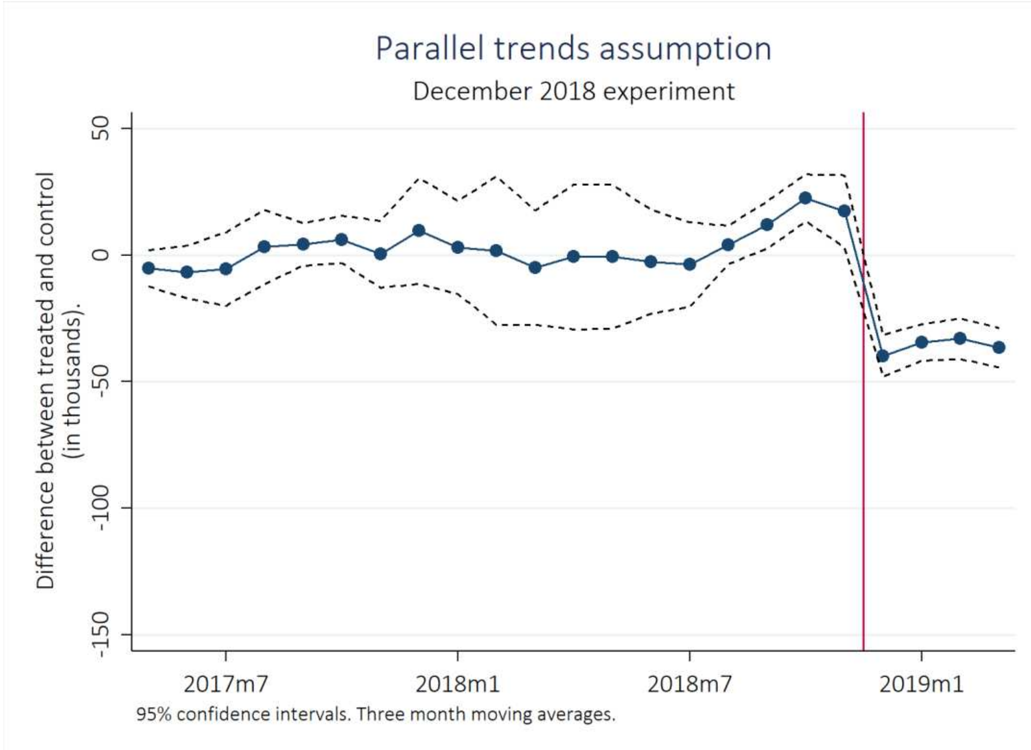


Table A.8 shows the OLS estimation results of the diff-in-diff coefficient estimation of the average treatment effect of the UY$3 price (in third-wave branches), in first three months of implementation. Column (A) presents de basic specification of the equation and the rest of the columns show the results with different combinations of branch fixed effects, month fixed effects and timer trends. Overall, the results show a very consistent estimation of a drop of 40 thousand bags, per branch, per month. An exception is the case in which we include time trends. In this case, the effect increases to 51 thousand bags.

Table A.8: Difference-in-difference OLS estimation

	(A) Basic DiD	(B) Branch FE	(C) Month FE	(D) Branch + Month FE	(E) D + Time trends
Treated*After	-40.95***	-40.61***	-40.62***	-40.45***	-51.28***
	(7.02)	(7.07)	(7.02)	(7.08)	(12.22)
Branch FE	No	Yes	No	Yes	Yes
Month FE	No	No	Yes	Yes	Yes
Time trends	No	No	No	No	Yes
N	1,428	1,428	1,428	1,428	1,428

Standard errors (in parenthesis) clustered at the branch level. Outcome variable: average number of bags delivered/sold by branch, by month. Sample period: 24 months between 2017m4 and 2019m3. Control group: 56 stores that were not treated in that period. * p<0.10, ** p < 0.05, *** p < 0.01

Table A.9 shows different specifications of equation (3), with data from the third wave of the experiment. Each column corresponds to a different regression. The first coefficient is the difference-in-difference effect of the *announcement* of the pricing (four months before the effective implementation) on the number of consumed bags. The second coefficient is the treatment effect for the *pricing* period. The results that we present in the main text (Table 6) are those of our preferred specification (column D, Table A.9).

Table A.9: Anticipation effects

	(A) Basic DiD	(B) Branch FE	(C) Month FE	(D) Branch + Month FE	(E) D + Time trends
DiD anticipation period	16.25***	16.73***	16.65***	16.97***	15.61
	(3.49)	(3.37)	(3.58)	(3.45)	(11.60)
pct change	36.1%	37.2%	37.0%	37.7%	34.7%
DiD pricing period	-37.59***	-37.17***	-37.24***	-37.00***	-39.03*
	(7.46)	(7.49)	(7.46)	(7.50)	(20.91)
pct change	-83.5%	-82.6%	-82.7%	-82.2%	-86.7%
Branch FE	No	Yes	No	Yes	Yes
Month FE	No	No	Yes	Yes	Yes
Time trends	No	No	No	No	Yes
N	1,428	1,428	1,428	1,428	1,428

Difference-in-difference estimates corresponding to equation (3) in the main text. Each column is a different regression. Outcome variable: number of bags delivered/sold by branch, by month. Control group: 56 stores that did´t change prices. Standard errors (in parenthesis) clustered at the branch level. * p<0.10, ** p < 0.05, *** p < 0.01

10.6 Fourth wave – January 2019

Table A.10 shows the number of plastic bags (thousands) delivered or sold by branches in the fourth wave (treated) and the 56 branches in the rest of the country that did not price the bags during the period (control), before and after the treated branches started pricing the bags. It also shows difference in differences calculation of the effect of the price: 54,600 bags, per month and branch, on average.

Table A.10: Average number of bags delivered by branches in the second wave (treated) and 56 branches that did not price the bags (control), before and after the former priced the bags

	Before	After	Difference
Control	69.27	73.64	4.37
	(5.51)	(5.93)	(2.08)**
Treated	76.52	26.29	-50.23
	(12.16)	(4.05)	(8.38)***
Difference	7.25	-47.35	-54.60
	(12.96)	(7.11)***	(8.33)***

Average bags delivered in each store (in thousands per month). Number of observations for the DiD regression: 1,644. Robust standard errors clusterd at the store level in parenthesis.

Figure A.5 shows the change in the initial difference in the number of delivered bags by treated branches and control branches, by month, and the 95% confidence interval (dashed lines). The figure illustrates that this difference was not statistical different from zero in any of the month of the pre-treatment period.

Figure A.5: Changes in the initial difference of bags delivered by month between control and treated branches

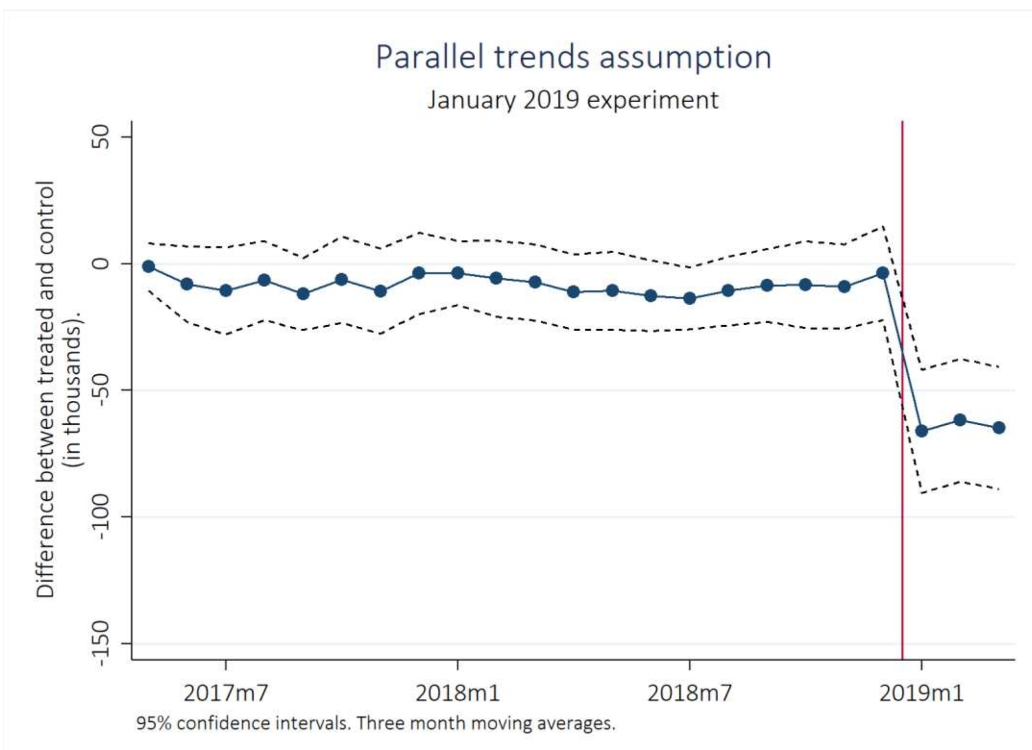


Table A.11 shows the OLS estimation results of the diff-in-diff coefficient estimation of the average treatment effect of the UY$2 price in the branches of the fourth wave of the experiment in first three months of implementation. Column (A) presents de basic specification of the equation and the rest of the columns show the results with different combinations of branch fixed effects, month fixed effects and timer trends. Overall, the results show a very consistent estimation of a drop of 54 thousand bags, per branch, per month. An exception is the case in which we include time trends. In this case, the effect drops to 49 thousand bags.

Table A.11: Effect of a price of US$ 0.07 on the quantity demanded of single-use plastic bags
Difference-in-difference OLS estimation

	(A) Basic DiD	(B) Branch FE	(C) Month FE	(D) Branch + Month FE	(E) D + Time trends
Treated*After	-54.60***	-53.99***	-54.46***	-53.98***	-48.86***
	(8.33)	(8.47)	(8.39)	(8.54)	(9.76)
Branch FE	No	Yes	No	Yes	Yes
Month FE	No	No	Yes	Yes	Yes
Time trends	No	No	No	No	Yes
N	1,644	1,644	1,644	1,644	1,644

Standard errors (in parenthesis) clustered at the branch level. Outcome variable: average number of bags delivered/sold by branch, by month. Sample period: 24 months between 2017m4 and 2019m3. Control group: 56 stores that were not treated in that period. * p<0.10, ** p < 0.05, *** p < 0.01

Table A.12 shows different specifications of equation (3), with data from the fourth wave of the experiment. Each column corresponds to a different regression. The first coefficient is the difference-in-difference effect of the *announcement* of the pricing (four months before the effective implementation) on the number of consumed bags. The second coefficient is the treatment effect for the *pricing* period. The results that we present in the main text (Table 6) are those of our preferred specification (column D, Table A.12).

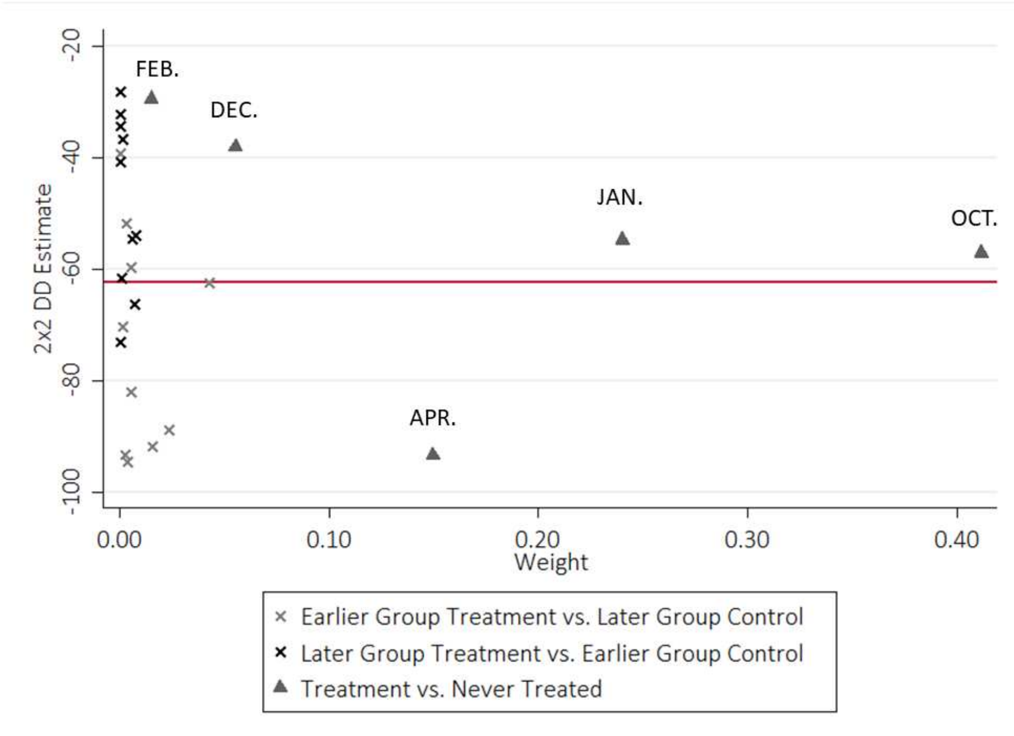
Table A.12: Anticipation effects

	(A) Basic DiD	(B) Branch FE	(C) Month FE	(D) Branch + Month FE	(E) D + Time trends
DiD anticipation period	-1.35	-0.55	-1.14	-0.47	7.61
	(3.95)	(4.00)	(3.97)	(4.03)	(5.50)
pct change	-1.8%	-0.7%	-1.5%	-0.6%	10.2%
DiD pricing period	-54.82***	-54.08***	-54.67***	-54.07***	-43.37***
	(8.39)	(8.52)	(8.45)	(8.59)	(11.02)
pct change	-73.5%	-72.5%	-73.3%	-72.5%	-58.2%
Branch FE	No	Yes	No	Yes	Yes
Month FE	No	No	Yes	Yes	Yes
Time trends	No	No	No	No	Yes
N	1,644	1,644	1,644	1,644	1,644

Difference-in-difference estimates corresponding to equation (3) in the main text. Each column is a different regression. Outcome variable: number of bags delivered/sold by branch, by month. Control group: 56 stores that did´t change prices. Standard errors (in parenthesis) clustered at the branch level. * p<0.10, ** p < 0.05, *** p < 0.01

10.7 DiD decomposition

Figure A.6: Goodman-Bacon decomposition

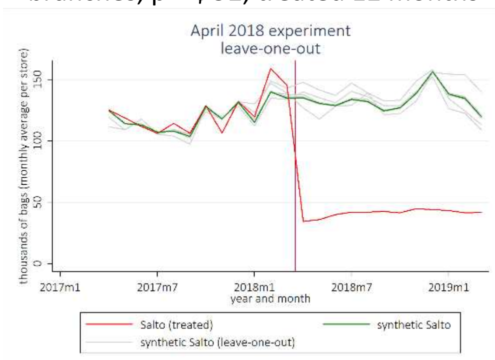


Estimates from the Stata *bacondecomp* package (Goodman-Bacon, Goldring, and Nichols 2019).

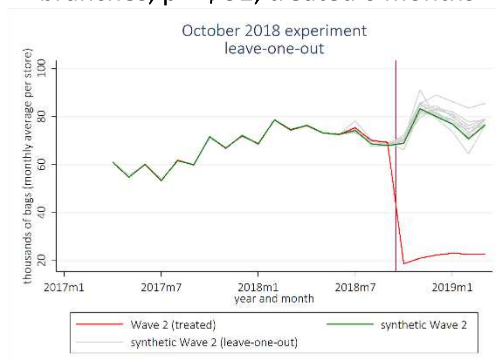
10.8 Leave-one-out synthetic controls

Figure A.7: Average number of bags delivered by treated branches in each wave (red line) its synthetic control (green line), and the leave-one-out synthetics (grey lines), by month

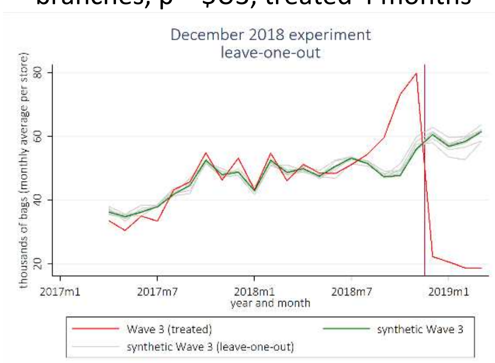
Panel (a) First wave: April 2018 (Salto), 3 branches, p = $U2, treated 12 months



Panel (b) Second wave: October 2018, 11 branches, p = $U2, treated 6 months



Panel (c) Third wave: December 2018, 3 branches, p = $U3, treated 4 months



Panel (d) Fourth wave: January 2019, 12 branches, p = $U2, treated 3 months