Modest and incomplete incentives may work: Pricing plastic bags in Uruguay

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Abstract: *We study the impact of voluntary prices on the demand for plastic bags, using two years of administrative data from a national supermarket chain in Uruguay. We find that prices of US$ 0.07 and US$ 0.10 per unit decreased the number of bags used by its customers in the range of 70% to 85%. We also find that in anticipation of the price, customers increased the use of bags by up to 38% in some branches before its implementation. This result has strong implications for future studies, regarding the need to use long pre-periods. According to our data, short pre periods could bias upward the estimation of the effect of the prices by up to 41%. Finally, we do not find evidence consistent with the supermarket suffering a loss of sales, due to clients moving to stores not pricing the bags. The fact that the supermarket is a discount chain (clients are more price sensitive) and that it seems to have chosen the cities and towns in which it had more market share to rollout the prices, may inform policy makers about the conditions under which incomplete and moderate incentives may work.*

KEYWORDS: plastic bags, price, consumer behavior, difference in difference, synthetic controls, anticipation effects.

JEL Codes: D04, D12, D62, H23, M21, Q53

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1 Introduction

Plastic bags can weigh 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 disposal 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 around the world, at all government levels (see appendix section 10.1 for a review of these initiatives). Despite its impressive number, evaluations of the effectiveness of these initiatives with a proper identification strategy are rather scarce. This is particularly true for the impact of levies.

In this work, we evaluate the effect of different prices on the number of single-use plastic bags used by customers of a discount supermarket chain in Uruguay. To do so, we collect administrative data on the total number of these bags provided to customers at checkout, by month, in all the 90 branches that this chain has across the country, before and after it implemented a staggered rollout of the prices to different cities. The data covers 25 months, from April 2017 (a year before pricing the bags in the first branches) to April 2019 (a year after). To identify the effect of the prices, we use different strategies. We use three different identification strategies to identify the effect of a price of UY$ 2 (two Uruguayan pesos; around 7 cents of US dollar in April 2018, when it came into effect) and a price of UY$3 (approximately US$ 0.10): differences-in-differences OLS regressions, event studies, and synthetic control methods. To identify the effect of a UY$ 4 price, for which we do not have a control group, we use a simpler pre-post analysis.

We find that prices of UY$ 2 and UY$ 3 per bag decreased the demand of single-use plastic bags in the range of 70% to 85%, with no clear difference between the two prices. Estimates are robust in magnitude and statistical significance to different methods of estimation, different specifications of the estimated equation and placebo tests. They are robust to the estimation of strategic anticipatory behavior by customers. We do not find evidence consistent with a loss in sales being the mechanism behind this drop. In addition, using a pre-post analysis, we find that the price of UY$ 4 produced sizable additional drop in the demand for plastic bags, in all branches, independently of the previous level of the price. We estimate that this price may have decreased the overall demand by an additional 40%, on average.

There are a considerable number of empirical studies on the effect of taxes or levies on the consumption of disposable single-use bags. Nevertheless, these largely rely on pre-post analyses or self-reported ordinal data on the use of bags (see the literature review section 10.2, in the appendix). We are aware of only two studies that use a causal inference technique and rely on observational cardinal data at the same time.[2] Both of them

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[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).

[2] Three additional US studies evaluate the impact of a ban (Taylor and Villas-Boas, 2016; Taylor, 2019 and 2020).

were conducted in the US. Homonoff (2018) found that a tax of US$ 0.05 on paper and plastic bags in Montgomery County decreased their use by roughly 50%. Homonoff et al. (2020) found that a tax of US$ 0.07 in the city of Chicago had somewhat smaller effect in the first two months. Jakovcevic et al. (2014), to our knowledge the only study using a causal inference technique outside the US, collected observational ordinal data from supermarket customers in Buenos Aires, Argentina.[3] They found that the charge increased the proportion of customers using reusable-bags during the first two months. Therefore, a basic contribution of our study is to provide the first estimation of the effect of a charge on the quantity demanded of single-use plastic bags outside the city of Chicago and Montgomery County that is based on a causal inference technique. This is an important contribution. Determinants of plastic bags consumption (income, preferences, relative prices and regulations) may differ between the US and less developed countries, which play a significant role in worldwide plastic pollution and where initiatives to reduce the use of single–use plastic bags are becoming ubiquitous. Assessing the external validity of results obtained in a limited number of US cities, informs about the effectiveness of a price when these factors vary.

In addition, the characteristics of our data allows us to contribute to the literature in other dimensions. First, our data is administrative, instead of observational, as the core data in the previous studies. This allows for a more exact estimate of the overall effect of the price. Second, the length of our pretreatment period gives us the possibility to estimate and disentangle the effect of the price from an anticipation effect. Customers may anticipate a charge simply because governments or 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, estimations based on a short pre-treatment period may bias upward the estimated effect of the price. We find that the demand for plastic bags increased 10.6% during the four months before the implementation of the price in a city where the clients were informed about the price in advance, and 37.7% in two cities where this information could have leaked. We also find that using a pre and post treatment period of three months, as Homonoff (2018) and Homonoff et al. (2020) did, biases upward our estimates of the price effect by 28% in the first city and 41% the other two.

An important determinant of the choice set of plastic-bag consumers is the scope of the regulation. Incomplete regulation occurs when the regulation applies only to a subset of bags, stores, or jurisdictions. The pricing initiative in our study case is a private one. In every city but two where it voluntarily decided to price plastic bags, the supermarket was the only store pricing them. Therefore, we can characterize our case study as one of incomplete pricing. The problem with incomplete regulation is "leakage". Homonoff et al. (2020) reports the introduction of thicker free bags in response to a ban on thin plastic bags. Taylor (2019) finds that sales of trash bags increased with a ban on single-use plastic bags. In our case, customers could have substituted the supermarket for other stores in the same city that were not pricing the bags. As far as we know, the empirical literature on plastic bags remains silent about the conditions under which incomplete pricing may still be effective. Our work contributes in this direction. Our unique setting and data allows us to compare the effect of the prices in the cities where the supermarket was the only one pricing the bags to the effect in the cities where

[3] In a very interesting study, Antinyan and Corazzini (2021) performed an RCT with loyalty card holders of a supermarket in Yerevan, Armenia, to evaluate the effect of nudges and consumption-level-based bonuses, with and without the provision of free reusable bags, on the purchases of disposable plastic bags.

it was not. Moreover, to test for leakage, we estimate the effect that the price had on the sales of a subsample of branches. By doing this, we are able to inform policy makers and non-governmental organizations on the effects of selectively targeting large, organized, easy-to-monitor actors when complete implementation is out of reach.[4] This is important because the political, technical, fiscal and institutional factors behind the existence of incomplete regulation could be particularly binding in less developed countries. Moreover, one could argue that the presence of an informal sector makes incomplete regulation the only alternative in these countries.

Another important issue when estimating the effect of a price on the demand of disposable bags is that customers' reaction to a price may vary through time. A possible reason for this is loss-aversion. Homonoff (2018) finds evidence consistent with this hypothesis. 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 or a sufficiently high proportion of the population starts using reusable bags after a period, the reference point may change, possibly decreasing the effect of a price on the use of plastic bags. Using scanner data from a large retail chain, Homonoff (2018) tracks the evolution of disposable bag use for up to 2.5 years in DC and six months in Montgomery County. However, this analysis only includes treated stores in the post-tax period. Homonoff et al. (2020) do observe customers on treated and control units up to one year after the tax. Unlike Homonoff (2018), they found that, a year after the tax, the proportion of customers using at least one disposable bag rebounds by roughly a quarter of the initial effect. Having data on the number of bags provided to customers at checkout, per month, in treated and control stores, for up to one year after the introduction of the price, allows us to contribute to this state of knowledge regarding the evolution of the demand for plastic bags during the first year of the implementation of a price.

Another issue that the literature has not solved yet is whether the magnitude of the bag price matters or not. Under incomplete information on the marginal private benefits of plastic bags, setting a price for plastic bags could result in undesired welfare losses. These may result from pollution damages from the excessive use of plastic bags (when the price is set too low) or from forgone benefits associated with shopping with single-use plastic bags (when the price is set too high). Knowing how consumers react to different prices gives policy makers valuable information in this respect. We contribute in this direction by estimating the effect of three different prices on the demand for single-use plastic bags. As discussed in more detail below, some caveats are in order with respect to this contribution. First, we have a relatively low number of observations for branches charging UY$ 3 per bag. Second, we are not able to disentangle the pre-post estimated additional effect of the UY$ 4 price with that of the complete/incomplete regulation.

We organize the paper as follows. In section 2, we describe the institutional context in which the intervention took place. In section 3, we describe the data. In section 4, we estimate the pooled effect of the prices. In section 5, we estimate wave-specific effects. In section 6, we study the differences in the effects of the different prices. In section 7, we present some robustness checks, namely the construction of synthetic controls, a quantification of anticipation effects and the effect on sales. Finally, we discuss our results and conclude in section 8.

[4] We thank an anonymous referee for pointing this out.

2 Intervention context

The supermarket chain from which we obtain the data priced the plastic bags voluntarily. It did so firstly in one city (Salto), adhering to a private initiative to price plastic bags pushed by the city chamber of commerce. After six months, it started rolling out the pricing to other cities across the country. In this section, we present the context of these decisions and we provide an account of the chain of events.

2.1 A private citywide initiative to price plastic bags

In December 2017, the Industrial and Commercial Centre of Salto, Uruguay (equivalent to a U.S. city chamber of commerce) launched a campaign to decrease the use of plastic bags in this city.[5] The campaign´s main proposal was a voluntary price for disposable plastic bags.[6] By the end of the month, had convinced a number of stores sufficient to inform the public about the imminent future pricing of the bags. Adhered stores put flyers in their doors. During January 2018, the stores and the center agreed on 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). 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.

On February 21, 2018, the municipal government formally adhered to the campaign (Resolution 074/18, Intendencia de Salto, 2018). After this, the campaign reached full swing. More stores followed. One of these was the supermarket chain whose adherence was essential for the implementation of the price because of its share of the city grocery market, estimated to be between 40% and 50% (according to conversations with officials from the Commercial Center of Salto).[7] This is the supermarket from which we obtain the data. During those same weeks, the municipal government and the commercial center launched a media campaign on TV, radio and internet, informing 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.[8] As announced, the adhered stores started pricing the bags in April 2, 2018, making Salto the first city in Uruguay to price single used plastic bags.

There is anecdotal evidence that during the first days some supermarkets put boxes near registers or gave some angry customers a number of bags free of charge (Diario El Pueblo, 2018). More formally, the supermarket chain from which we obtain the data gave one reusable bag to customers with a loyalty card during

[5] With a population of 105,000, the city of Salto is the second most populated city of Uruguay, where roughly 3.5 million people live. Unlike the US, paper bags are not common in Uruguay.

[6] On December 6, 2017, the Senate passed a bill to regulate the production, distribution and consumption of plastic bags. This bill included an article establishing a mandatory minimum price for carryout plastic bags. We have mixed evidence on whether the pricing initiative in Salto was the result of the previous conversation that led to the bill or not. In April 2018, the manager of Salto Chamber of Commerce declared in the press that their "*… initiative came before the bill was created. … and if our initiative helps to achieve the other half sanction* [in the House of Representatives]*, it would be spectacular*." (El Observador, 06 April 2018). On the other hand, according to the director of the national environment directorate at that time, although it did help in the approval of the bill and the following regulation, Salto´s initiative was born from the national conversation preceding the bill (Telephone conversation, October 11, 2020).

[7] There were four supermarket chains in Salto at that time. Three of them were local chains. The fourth was the national chain from which we collect the data.

[8] The sign informed readers that "from 04/02/18, standard T-shirt type plastic bags will have a cost of UY$ 2 (tax incl.), and UY$ 3 the bigger ones". Figure A.1 in the Appendix (section 10.3), shows a picture of the sign.

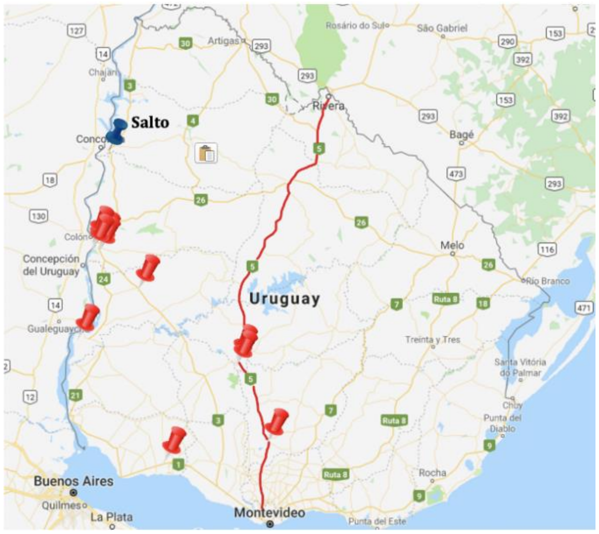
the period of twenty days before starting to price the bags. Customers without a loyalty card could also get a reusable bag by spending more than UY$ 1,500 (Diario El Pueblo, 2018). The biggest local supermarket also gave bags free of charge to its customers. The rest of the stores did not follow suit. These compensation measures by retailers are something to expect to see during the first days of interventions such as this. Certainly, they may affect the estimation of the impact of the price in the very short run. Nevertheless, these measures were in place only in the days or weeks that precede or followed the implementation date. After those first days, the supermarkets stop giving bags free of charge. Therefore, this noise should fade away in longer-run estimations, such as the ones we present here.

Since the pricing was optional, a question that arises naturally is how many stores in Salto adhered to the measure? In September 2018, the Commercial Center of Salto conducted a survey to answer this question. According to this survey, there were around 80 stores charging bags (90% of which since day one). These represented all the supermarkets, 60% of grocery stores, 50% of markets, 40% of bakeries and 35% of butcher shops (Manager of Industrial and Commercial Center of Salto, e-mail communication, July 2, 2020). These numbers show that a considerable number of (small) stores could not have priced the bags during our sample period, which raises the issue of leakage. We address this in section 0. Nevertheless, it is important to note that, because all the supermarkets in the city adopted the price, the percentage of sales subject to the price of bags may be larger than the percentages of businesses.

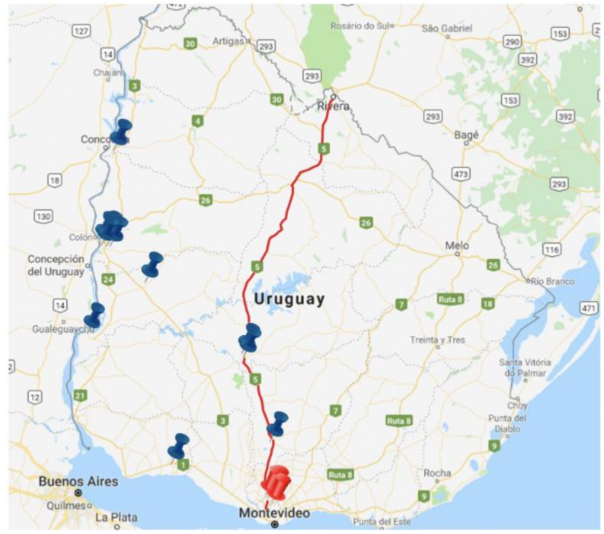
2.2 The supermarket chain rollout of the price to other cities

In October 2018, six months after pricing the bags in Salto, the supermarket chain voluntarily started a staggered rollout of the price to 27 additional branches in 16 additional cities and towns. The price rollout had four waves. Figure 1 maps these four waves.

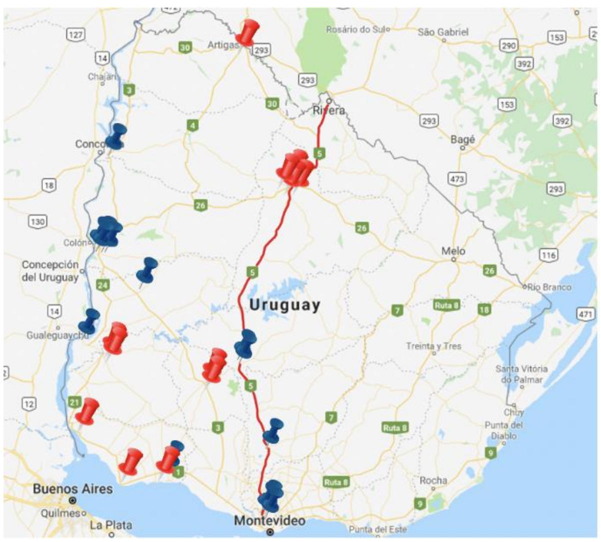
(a) October 2018: 11 branches located in six cities



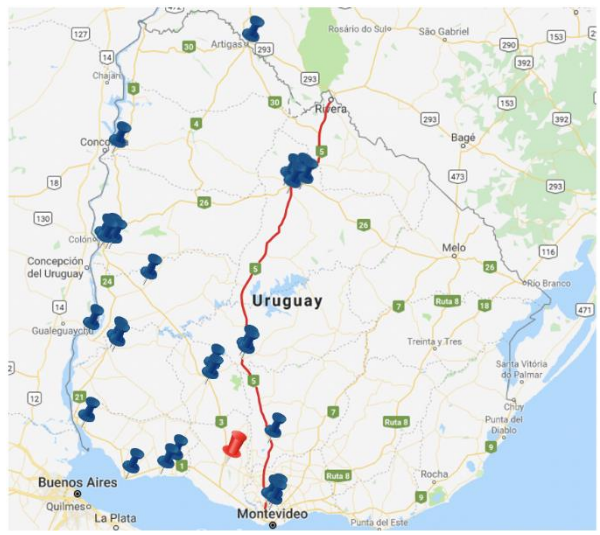
(b) December 2018: 3 branches located in two cities



(c) January 2019: 12 branches located in 7 cities



(d) February 2019: 1 branch in one city



Branch previously pricing the bags

Branch that started to price the bags in each wave

FIGURE 1: SUPERMARKET CHAIN ROLLOUT OF THE PRICING OF PLASTIC BAGS ACROSS URUGUAYAN CITIES

Notes: The figure shows the location of each treated branch across the country. Each pin corresponds to a branch. The red line, running from South to North, is Route 5. In addition to the branches marked on the map, the supermarket chain has another 56 branches, located in 11 cities, covering the 19 departments of the country. Together with the marked branches, these other branches began to charge UY$ 4 each bag in April 2019, a move agreed upon by all supermarkets in the country, after the approval of a law that would impose such a minimum price for bags in June 2019.

The supermarket set the same UY$ 2 price in every branch and city of all waves except in three branches in two towns (second wave, December 2018), in which it set a price of UY$ 3. The reason is 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, the supermarket was the only store pricing the bags. The rollout ended in April 2019, when all the supermarkets in the country agreed to price plastic bags UY$ 4. Table 1 summarizes the rollout and the characteristics of each wave.

TABLE 1: CHARACTERISTICS OF THE PRICE ROLLOUT

Experiment	Treated branches	Treated cities	Price	Treatment duration (months)	Only store in town pricing the bags	Free reusable bag distribution in weeks before the price
April 2018 (Salto)	3	1	UY$ 2	12	No	Yes
October 2018	11	6	UY$ 2	6	Yes	In some towns
December 2018	3	2	UY$ 3	4	No	No
January 2019	12	7	UY$ 2	3	Yes	No
February 2019	1	1	UY$ 2	2	Yes	No
April 2019	All	All	UY$ 4	1	No	No

Notes: The table summarizes information about the six waves of the pricing rollout across 30 branches located in 17 cities in Uruguay. The supermarket chain also has 56 untreated branches, located in 11 different cities. These branches did not charge for the bags until April 2019. These 56 branches will serve as controls in some analyzes. In April 2019, all branches began to charge UY$ 4, in a national agreement between all supermarkets, after the approval of a law that would impose such a minimum price for bags in June 2019.

The reason why all the supermarkets in the country started to price the bags UY$ 4 in April 2019 is the following. In August 2018, the Uruguayan parliament passed a law establishing (a) a national ban on non-biodegradable or non-compostable bags and (b) a national minimum price of UY$ 4 for the permitted plastic bags. The law´s regulatory decree established that the prohibition to import or manufacture non-compostable or non-biodegradable bags began on March 1, 2019 and that the national minimum price began on June 30, 2019 (Art. 19, Decree # 3/019). The association of supermarkets and other stores, arguing that they could run out of the old, non-compostable bags before June, decided to start pricing these bags UY$ 4 on April 1, 2019 (El País, 2019; El Observador, 2019). Since June 30, 2019, carryout disposable plastic bags in Uruguay are compostable or biodegradable and have a minimum price of UY$ 4 (adjusted by inflation).

Figure 2 shows the value of the price of plastic bags in the supermarket chain through time, by rollout wave (except February 2019, with only one branch). The staggered rollout 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), in addition to the price increases from the rollout. As explained below, we exploit this to estimate the effect of a price of UY$ 2 and UY$ 3 on the demand for plastic bags with three different identification strategies: differences-in-differences OLS regressions, event studies, and synthetic control methods. We also analyze the effect of the UY$ 4 price, using a simpler pre-post strategy.

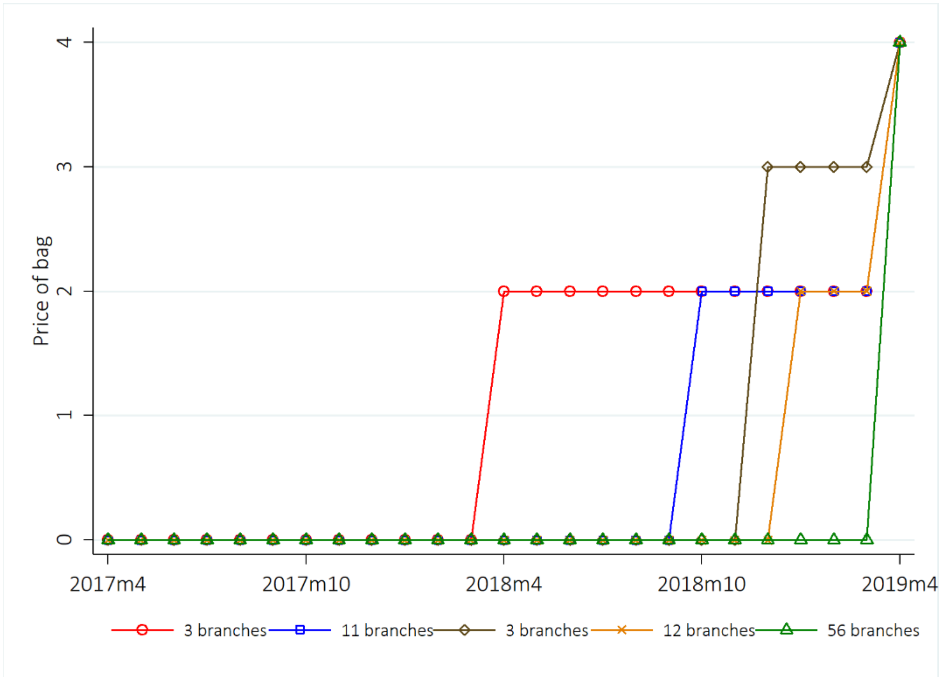


FIGURE 2: TIME LINE OF THE ROLLOUT AND VALUE OF PRICES FOR PLASTIC BAGS IN THE SUPERMARKET CHAIN

Notes: The figure plots the time evolution of the price of the plastic bags in the supermarket chain, by rollout wave. Prices are expressed in Uruguayan pesos (UY$). For ease of viewing, we omitted a branch that began charging UY$ 2 in February 2019. Nevertheless, this branch is included in all econometric analyzes. In April 2019, Uruguayans supermarkets agreed to price the bags UY$4, across the country.

Finally, it is important to note that the supermarket chain did not implement the policy of giving one reusable bag free of charge to loyal customers in the rest of the cities in which it priced the bags, as it did in Salto. Instead, the supermarket put them on sale.[9]

3 Data

Our dataset is comprised of two subsets of data. One is the subset of variables that we obtain from the supermarket chain, comprised by the monthly number of bags used at checkout in each branch, the level of the prices and the location of the branches. The other subset of data is comprised of variables measuring socio-demographic characteristics of the city or towns where the branches are located. These data come from several sources. In the following paragraphs, we first describe the supermarket data and then the socio-demographic data.

We collected administrative data on the total number of single-use plastic bags provided to customers at checkout in each of the 90 branches of the 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). This supermarket chain is a discount store chain, with an explicit marketing strategy

[9] According to a former manager of the supermarket that worked in the rollout (personal communication, October 9, 2020), only "a few" of the remaining bags were given free of charge to customers in some of the cities that comprised the second wave.

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 corresponds 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 bags corresponds to the number of bags provided by the supermarket at checkout free of charge. The number is the result of the difference in monthly stocks of plastic bags in that branch. When the observation corresponds to a branch and month during which the bags had a price, the number of bags corresponds to the number of bags sold in that branch during that month, according to scanner data.

The second subset of data consists of information from four sources. The national Competition Defense Commission provided data on the location, surface and number of registers of 460 stores throughout the country. (Stores with two or more branches, or with a single branch with three or more registers, are obliged to report this information). With this data, we construct different measures of market share for the supermarket chain that we study. The next two data sources are the last national Census (2011) and the Continuous Household Surveys (*Encuesta Continua de Hogares,* 2016-2019). We use data from these sources to control for sociodemographic differences across the cities where the supermarket chain operates. Finally, the last source is geodata from the OpenStreetMap project, where we collected other characteristics of the cities (mainly the number of other shops).

Our final database is an (unbalanced) monthly panel with 2,161 observations. It consists of the 90 branches of the supermarket chain, located in 28 cities, across the 19 departments of Uruguay, during 25 months.[10] Table 2 presents the descriptive statistics of our database.

[10] We have missing information on bag consumption in four branch-month observations. Additionally, four branches went out of business during 2017 (before the first wave of the experiment).

TABLE 2: DESCRIPTIVE STATISTICS

Variable	*mean*	Sd	Min	max
Supermarket chain data				
Bags provided for free by month (000)	70.52	49.20	-4.00	395.00
Bags sold by month (000)	22.22	13.05	1.82	59.79
Price	0.31	0.92	0	4
Price = 0	0.89	0.31	0	1
Price = 2	0.06	0.25	0	1
Price = 3	0.01	0.07	0	1
Price = 4	0.04	0.20	0	1
Treated April 2018	0.03	0.18	0	1
Treated October 2018	0.13	0.33	0	1
Treated December 2018	0.03	0.18	0	1
Treated January 2019	0.14	0.35	0	1
Other branches	0.67	0	0	0
Market share in city				
by area of stores (m2)	0.34	0.31	0.07	1
by number of stores	0.33	0.26	0.06	1
by number of registers	0.38	0.29	0.1	1
Number of stores in city	23.1	20.9	1	44
Largest store in town belongs to chain	0.26	0.44	0	1
Cities data				
Western city	0.88	0.33	0	1
Number of supermarkets in city	130.4	124.6	1	255
Total supermarkets area (000 m2)	89	84	0.6	173
Cash registers in city	765.6	722.8	6	1,488
Population (000)	672.6	626.1	10.1	1,298.6
Female (%)	0.53	0.01	0.5	0.54
Age	36.6	1.4	33.5	39.4
Children (%)	0.20	0.02	0.18	0.24
Married (%)	0.06	0.01	0.05	0.09
Retired (%)	0.20	0.02	0.13	0.27
Low education level (%)	0.54	0.09	0.37	0.7
Occupied (%)	0.60	0.02	0.56	0.69
Unemployed (%)	0.04	0.01	0.03	0.06
Income (UY$ 2019)	66,583	13,734	32,898	93,939
Below poverty line	0.06	0.03	0	0.2

TABLE 2: DESCRIPTIVE STATISTICS (CONT.)

Variable	*mean*	Sd	Min	max
Cities data, excluding Montevideo				
Supermarkets (#)	5.4	5.1	0	34
Convenience Stores (#)	4.8	7.1	0	37
Schools (#)	9.2	7.6	1	39
Gas Stations (#)	5.8	2.9	2	13
Pharmacies (#)	3.7	4.5	0	28
Banks (#)	3.6	2.0	0	8
Other amenities (#)	117.0	155.8	4	966

Notes: The table presents descriptive statistics for the entire database. We constructed this database using information from five sources. The first source is the supermarket chain, from which we obtain information on bags provided for free or sold in every branch in every month, and the corresponding prices. The second source is the Competition Defense Commission database, from which we obtain information to calculate the market share of the supermarket in each city. The third source is the Census from 2011, from which we obtain information on the sociodemographic characteristics in each city (population, unemployment rate, etc.). The fourth source is the Continuous Household Survey (2016-2019), from which we obtain information on monthly income and poverty in each city; and. The fifth source is the OpenStreetMap project, from which we obtain additional information on the cities, excluding Montevideo. *Western city* is a dummy variable that takes the value of one for those cities that are located on or west of Route 5. The first line of the table shows the average monthly number of bags provided free of charge, while the second line shows the average monthly number of bags sold by a branch that is pricing thee bags. OpenStreetMap defines a supermarket as a large store with groceries and other items, and a convenience store as small local shop carrying a small subset of the items one would find in a supermarket. “Other amenities” summarizes approximately 200 types of additional amenities in the database, such as churches, hotels, parking lots, bakeries, ATMs, fast food stores, car repair shops, hospitals, butcher shops, currency exchange houses, police stations, hairdressers, clinics, etc. The total number of observations from this (unbalanced) monthly panel is 2,161. The data period for bags consumption ranges from April 2017 to April 2019. Treated branches charged a price of UY$ 2, UY$ 3, before April 2019 when all branches charged a price of UY$ 4.

The average number of single-use plastic bags provided at checkout free of charge by a branch in a month was 70,520. When charging the bags (UY$ 2, 3 or 4), the average number of bags sold by a branch in a month was 22,220. The standard deviation of the number of bags provided at checkout for free is considerable. The largest branch provided 230,740 bags in an average month and 395,000 bags in the busiest month. The smallest branch provided, on average, 17,670 bags.

The minimum value of -4 thousand bags in the monthly number of bags provided at checkout for free deserves a clarification. As mentioned before, the supermarket used two measurement methods to construct our outcome variable. For month-branch observations in which the supermarket sold the bags, it used scanner data. For month-branch observations in which the supermarket provided the bags free of charge, it used differences in monthly stocks. More specifically, *the number of bags provided free of charge in a month = the number of bags in stock at the beginning of the month + restocks during the month – the number of bags in stock at the end of the month.* This calculation could result in a negative number of bags if stocks or restocks are not counted properly. The supermarket inform us that counting stocks may suffer from delays when the opportunity cost of employees peaks, as in the holiday season, for example (e-mail communication, November 2018). In these cases, employees impute a final stock to the previous month at the end of the following month (January). Although this situation is not ideal, the delays in counting were the exception rather than the norm. In fact, we only have two branch-month observations with a negative number of bags. In one of these cases, this occurs after a month in which the number of bags used at checkout was significantly above the average, which is consistent with the hypothesis that they did not count the stock of bags at the end of that month. In any case, because the number of bags used in the two-month period involved is correct, we do not have a consistent measurement error. Reassuringly, when we use only stocks data, our results are almost identical to the ones we obtain when we use stocks and scanner data (with no measurement error).[11] These results also act as a robustness check for possible concerns regarding the perfect collinearity between treatment and type of data. Finally, because counting stocks may add some variation to the series, we use a smoother three-month moving average series of total bag consumption in some figures. We also perform some of the analyses using these series, whose minimum value is 1,820. Nevertheless, we use the original monthly data to perform the main regressions. Results do no change significantly if we use the smoothed data.

In our sample period, we have four different prices. Most branches (56) did not 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 (in Salto) 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.

The supermarket chain has an average share of 1/3 of the city market (considering the area of the stores, the number of stores, or the number of registers). In four cities, the supermarket chain is the only large store in town. In 20 cities, it owns half or less of the total number of supermarkets. Montevideo, the capital city, is a special case, since half of the population of the country lives there, and has 65% of the stores. The bottom panel of Table 2 presents information at the city level. This information reveals that there are some significant differences between the cities in our sample (see subsection 5.1). As some of these cities house our treated branches and other our control branches, we control for these differences in some of the regressions that we present below.

4 Pooled effect

4.1 Difference in differences specification

We start by estimating the pooled effect of pricing the bags using the following equation:

$$B_{bcm} = \alpha + \delta_m + \mu_b + \beta P_{bm} + \boldsymbol{\gamma_x X}_{cm} + \varepsilon_{bcm} \tag{1}$$

In equation (1), B_{bcm} represents the number of bags provided at checkout by branch *b,* located in city *c,* on month *m*.[12] The parameters δ_m and μ_b are month and branch fixed effects, respectively. P_{bm} is a branch-month indicator variable. It is equal to zero if the supermarket provided the single-use plastic bags to its customers for free in branch *b* in month *m*, and equal to equal to one if it priced the bags. The coefficient β is therefore the difference-in-difference estimation combined effect of pricing the bags, averaged across branches

[11] We do this only for the last five months of the sample where we have both types of data. Results available upon request.
[12] Our main results and conclusions are robust to using log(bags) as our dependent variable. In appendix section 10.12 we discuss the use of absolute quantities of bags as our outcome variable and we present the results of our main estimations when using log(bags).

and months (α). The vector $\boldsymbol{X}_{cm}$ is a set of time-varying covariates, measuring socio-demographic characteristics of the city in which the corresponding branch is located. We obtained these variables from the Continuous Household Survey. They are the following: household income (in January 2019 pesos) and the proportions of people in the city that are: employed, unemployed, under the poverty line, women, younger than 14 years old and over 60 years of age. Finally, ε_{bcm} is the error term, clustered by branch.[13]

Column A, in Table 3 shows the diff-in-diff OLS estimate and standard errors of β, when we estimate it *without* the time-varying controls $\boldsymbol{X}_{cm}$. The point estimate of the pooled effect of pricing the bags is an average decrease of 61,720 bags, per branch, per month. This represents a percentage drop of 85.5% from the mean number of bags provided in the same branches when the price was zero (72,220 bags). Column B in Table 3 presents the same estimate when including the time varying controls $\boldsymbol{X}_{cm}$. Results remain robust and almost identical with the inclusion of these controls. [14]

TABLE 3: POOLED EFFECT OF PRICES ON THE QUANTITY OF BAGS, DID SPECIFICATION

	(A)	(B)
Treatment effect of pricing	-61.72***	-61.67***
	(5.56)	(5.58)
Pre-treatment mean of treated	72.22	72.22
Percentage change	-85.46	-85.40
Controls	NO	YES
N	2,075	2,075

Notes: The table shows the results of an OLS estimation of equation (1). The outcome variable is the number of bags provided for free/sold by branch, by month. Controls in columns A and B include month and branch fixed effects. Controls in column B also include a set of time-varying covariates, measuring socio-demographic characteristics of the city in which the corresponding branch is located. We obtained these variables from the Continuous Household Survey. They are the following: household income (in January 2019 pesos) and the proportions of people in the city that are: employed, unemployed, under the poverty line, women, younger than 14 years old and over 60 years of age. We also include a dummy variable for any of the controls missing. Standard errors (in parenthesis) clustered at the branch level. * p<0.10, ** p < 0.05, *** p < 0.01.

4.2 Event study design

Goodman-Bacon (2019) points out that, in studies in which there is variation in the moment when the treatment status turns on like ours, the coefficients of a DiD specification may be biased if the treatment effect varies monotonically over time (i.e. gets bigger with time since treatment). In this case, he suggests to present results from an event study design with a more transparent control group. Although our data does not support the fact

[13] In Appendix 10.13 we show that our results are robust to different levels of clustering and to using a wild bootstrap method for small number of clusters.

[14] The size of the disposable thin-plastic bags provided at checkout free of charge was 40*50 cm. The size of those sold was 45 * 60 cm. Given that the priced bag is somewhat larger that the non-priced bag, all else equal, our evaluation probably provides a lower estimate than what we would obtain if bags were of the same size. This is remarkable, given the size of the effect that we find and reinforces our conclusions.

that the treatment effect increases with time since treatment, we conduct the estimation of the effect of the prices based on an event-study design. We present the results of this analysis in this section. To restructure the DiD setting into an event study, we set the month when branches started charging the bags in each wave as the event-month zero. Following Goodman-Bacon, we drop the observations of the already treated branches (branches that entered the rollout in previous waves) from the control observations and we focus on the three months before and after treatment. We then append the four datasets, in what sometimes is labeled as a *stacked DiD.* We estimate the following equation

$$B_{bt} = \alpha + \delta_t + \mu_b + \rho P_b + \sum_{t=-2}^{t=3} \beta_t (P_b * Time_t) + \boldsymbol{\gamma_x X_{bt}} + \varepsilon_{bt} \quad (2)$$

In equation 2, the variable B_{bt} represents the number of bags used at branch *b* in event time *t,* and δ_t and μ_b are time (*t*=-2,…,3) and branch fixed effects, respectively. The variable P_b is an indicator variable that takes the value of one for branches charging a price of UY$ 2 or UY$ 3. Our main coefficients of interest are the β_t, which capture the monthly difference between treatment and control branches, relative to event time -3. The pre-treatment betas serve as a test for the parallel trend assumption. Estimates of the pre-treatment's betas not statistically different from zero are consistent with this assumption. The vector $\boldsymbol{X_{bt}}$ consists of the same set of time-varying covariates, measuring socio-demographics, defined in equation (1). Finally, ε_{bt} is the error term, clustered by branch.

We present the results of the OLS estimation of the β_t in Table 4. These results show, first, that the data supports the assumption of parallel trends. The treated and control branches were delivering bags at a similar trend in the three months before treatment. Second, the average effect of the prices was a monthly decrease of around 65 thousand bags per month, per branch, in the first three months of the price. This corresponds to an average drop of 74%, relative to the pre-treatment mean. This result does not change if we include the set of time-varying covariates (column B) or not (column A). These results are similar to the results that we obtain with our DiD analysis and therefore provide evidence in favor of their robustness.

TABLE 4: POOLED EFFECT OF PRICES ON THE QUANTITY OF BAGS, EVENT STUDY DESIGN

	(A)	(B)
Price * (time=-2)	-3.76	-4.77
	(6.71)	(7.31)
Price * (time=-1)	-1.14	-4.13
	(6.89)	(7.31)
Price * (time=0)	-65.38***	-65.42***
	(8.53)	(9.49)
Price * (time=1)	-62.35***	-65.00***
	(8.08)	(8.70)
Price * (time=2)	-67.55***	-67.90***
	(8.22)	(9.21)
Controls	No	Yes
N	1,755	1,755

Notes: The table shows the results of an OLS estimation of equation (2). The outcome variable is the number of bags provided for free/sold by branch, by event time. Controls in columns A and B include month and time fixed effects and a dummy for treatment status. Controls in column B also include a set of time-varying covariates, measuring socio-demographic characteristics of the city in which the corresponding branch is located (see notes to Table 3). Standard errors (in parenthesis) clustered at the branch level. * p<0.10, ** p < 0.05, *** p < 0.01

Figure 3 illustrates the effect of the prices in both levels and trends.[15] Panel (a) shows, the parallel evolution of bag use in treated and control branches prior to the start of treatment and the instant and persistent drop subsequent three months. Panel (b) in Figure 3 plots the estimated β_t from equation (2).

[15] Although DiD doesn't require that treated and control branches deliver a similar number of bags per month, identification assumptions will be generally more plausible if treated and control branches are similar in levels and not just in trends (Kahn-Lang and Lang, 2019). This is the reason why we present results in levels and in differences. Nonetheless, in our setting, except for the April wave (Salto), the rest have similar pre-treatment levels (Figure 4).

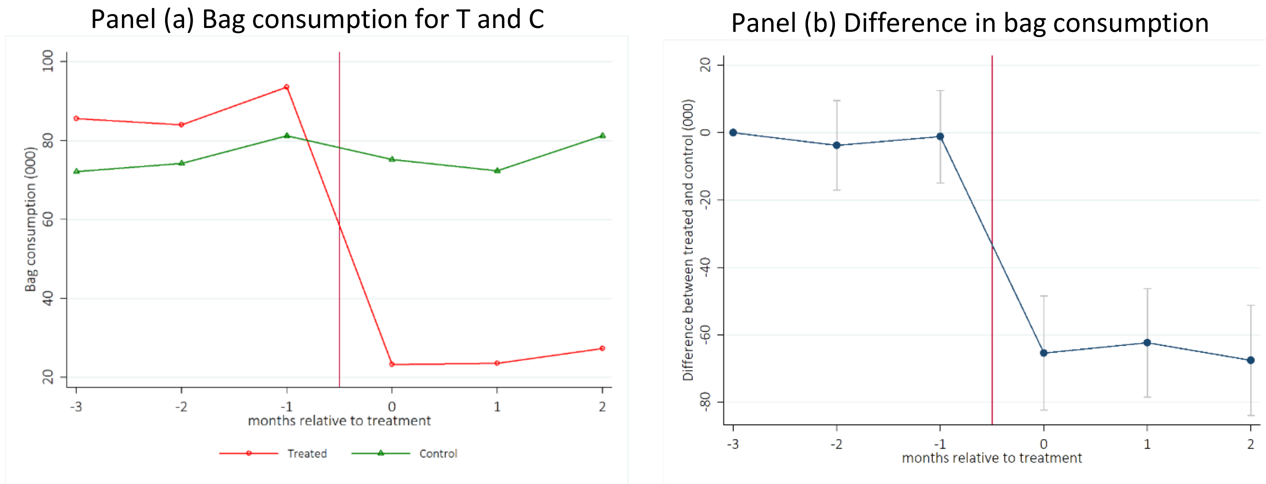


FIGURE 3: POOLED EFFECT OF PRICES ON THE QUANTITY OF BAGS, EVENT STUDY DESIGN

Notes: The figure shows the impact of charging for plastic bags on consumption, for treated and control branches. Panel (a) shows the series in levels, while panel (b) shows the difference between treated and control branches, with the respective 95% confidence interval, corresponding to the model from Table 4, column (B). Treated branches charged for the plastic bags at event time zero. Control branches consist of branches that did not charge for bags during those six months. We exclude branches that were already charging for the bags in pre-treatment event time. The four main experiments from Figure A.3 are included.

Difference in differences regressions with staggered implementation of treatment, as in our setting, are increasingly under scrutiny, because of the potential negative weighting of groups and periods in the average treatment effect (de Chaisemartin and D'Haultfœuille (2020)). We performed the diagnostic test proposed by de Chaisemartin and D'Haultfœuille (2020) and found that there are no negative weights in any of the different comparisons that comprise our average treatment effect on the treated (ATT). Two possible reasons why, are that we have a large never-treated branches in the control group and that the treatment is binary. Nonetheless, in appendix section 10.4 where we perform robustness checks for our basic DiD using three novel methods (de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021; and Borusyak, Jaravel, and Spiess, 2021). These methods rely on different assumptions about the parallel trends, use different groups of units as controls, and ultimately propose different estimators for the parameter of interest. We find that our results are remarkably robust to these three methods (see Figure 3 panel b, and figure A.2).

5 Wave-specific effects

5.1 The comparability of the different cities

Pooling the waves of the rollout could raise the question of the comparability of the different cities. These may differ in uncontrolled characteristics that may confound the effect of the price, such as income distribution, preferences towards the environment and the market share of the supermarket in the city. Relatedly, the supermarket was not the only store in town pricing the bags in the cities of the first and the third waves, but it was the only one in the cities of the second and fourth waves. This may be an important difference, because it affects the consumers' cost of substituting the supermarket for alternatives stores as a mean to avoid the price.

To assess the comparability of the treated and control cities, in Table A.2 we perform difference of means tests for selected characteristics of the supermarket branches and the cities. We exclude from this analysis the cities of Salto and Montevideo. As it can be seen, treated and control cities are similar in most of these characteristics. In particular, they are similar in the average number of bags provided to customers by branch, and the size, age composition, employment/unemployment rate, education, income and poverty rate of their population. They are also similar with respect to the number of supermarkets, convenience stores, schools, gas stations, and other amenities. Treated and control cities differ significantly only in the market share of the supermarket and their geographic location. With respect the latter, as shown in Table A.2, all the treated branches are in towns or cities located west or on Route 5, which runs from Montevideo (south) to Rivera (north), through the center of the country. Figure 1 illustrates this graphically. The explanation for this geographic distribution of treated branches is logistics, according to a former manager of the supermarket chain that worked in the rollout (personal communication, October 9, 2020). The chain´s distribution center is located in the south of the country, right north of Montevideo. Pricing the plastic bags in Salto saved the supermarket the transportation of around 282,000 bags per month to Salto (94,000 bags per branch, according to our results, times 3 branches). This liberated approximately 3 m3 of space in trucks. To maximize savings in distribution costs, the supermarket decided to rollout the initiative in branches that are in north-west bound routes, the direction of Salto (Figure 1).

The other statistically significant difference between treated and control cities is the average share of the supermarket chain in the grocery market. According to information presented by the supermarket to the national Competition Defense Commission, this share is 65% in treated cities and 24% in control cities, as measured by area (m2) of stores. These percentages are remarkably similar (64% and 27%) if we use data from Open Street Map. Reassuringly, if we use other variables to measure market share, we obtain similar numbers. The chain owned 56% of the stores in the cities or towns in which it priced the bags, and 29% in those cities in which it did not. Similarly, the chain had 66% of the number of registers in the cities or towns where it priced the bags and 31% in those where it did not. Relatedly, in the cities where it priced the bags, the average number of supermarket stores is 3.9, while it is 6.3 in the control cities. These differences are consistent with the hypothesis that, secondary to the logistics criterion, the supermarket managers decided to price the bags in cities or towns in which they faced less competition, possibly trying to minimize the chance of losing clients to other stores because of the price.[16]

Kahn-Lang and Lang (2019) point that DiD assumptions may fail if pre-treatment differences in levels between control and treated stores were originated by differences in trends. They argue that parallel trends are neither necessary nor sufficient to guarantee parallel counterfactual trends. We partially addressed this issue by including city-level fixed-effects and time-varying covariates, which may influence store sizes and the number of

[16] A challenge for the identification strategy would be that managers decided to charge for plastic bags in those branches where they expected the consumption to decrease in the absence of treatment. In another related analysis, available in the replication files for the paper, we regress the probability that a city is treated on (observable) variables included in Table A.1. We find that a dummy variable for being a western city washes out the statistical significance of the other variables, in particular market power, the number of bags used, or the income of the city's households. We conclude that the main determinant of a city being treated is being on or to the West of Route 5 (which is absorbed by the fixed effects in all the regressions). In other words, conditional on being on or to the West of Route 5, treatment status can be seen as good as randomly assigned (under the plausible assumption that customers did not change the city where they shopped as a consequence of the treatment).

bags consumed, in the pooled estimation presented in the previous section. We further address the issue by estimating wave-specific treatment effects in the next two subsections. Finally, we also address it by creating synthetic controls with the same pre-treatment level of the main variables in the robustness checks.

5.2 A graphic illustration

Figure 4 shows a graphic illustration of the effect of the prices on the quantity of plastic bags consumed at the average branch in each wave. The green line (marked with triangles) depicts the number of bags provided at zero cost by the average branch in the control group, comprised of fifty-six (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 provided at checkout for free or sold by the average branch in each wave. The vertical lines mark the date at which the set of branches each wave started pricing the bags. 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 (April 2019) 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.

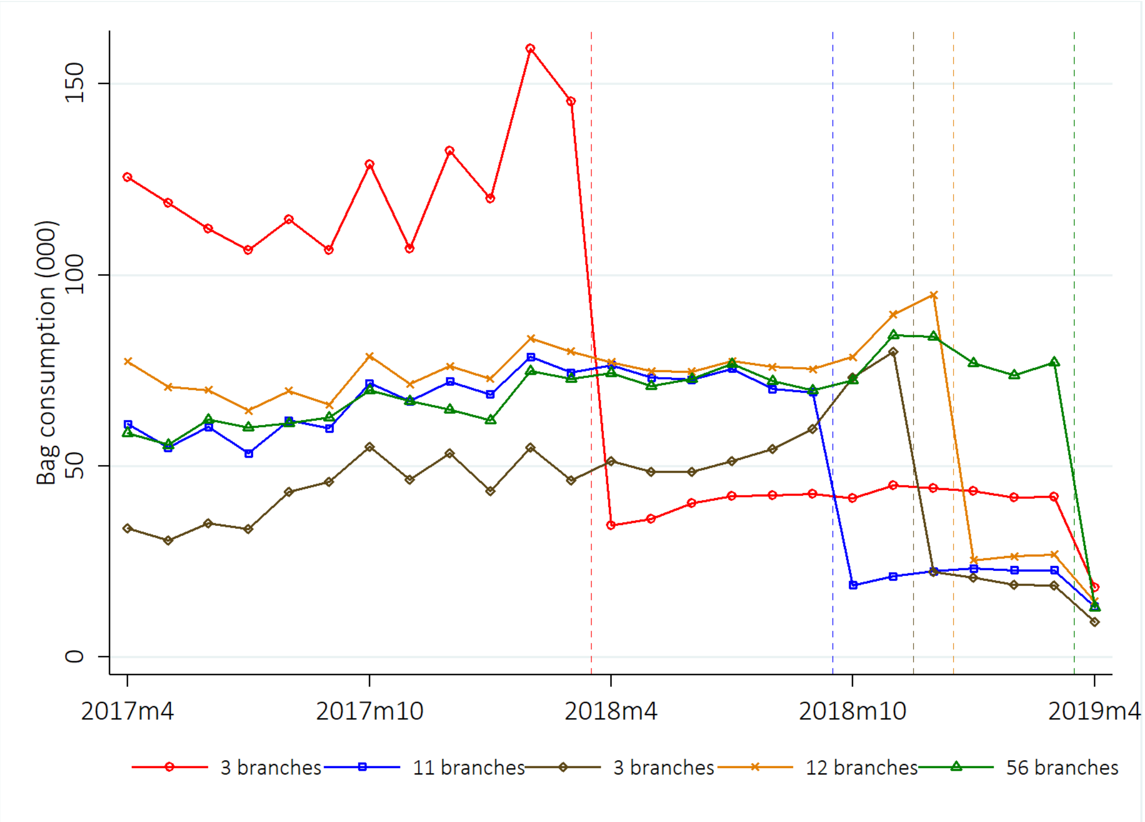


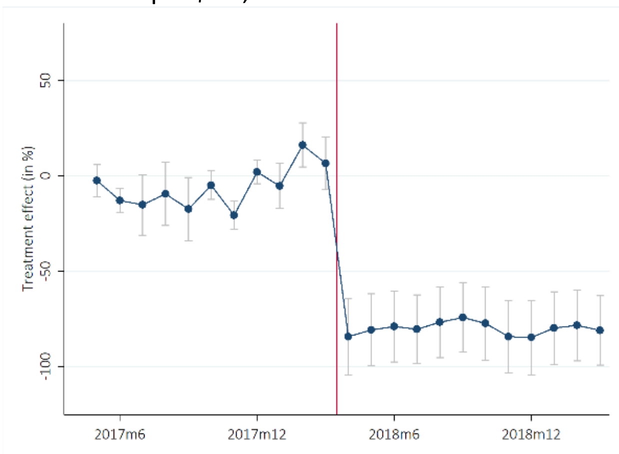
FIGURE 4: AVERAGE NUMBER OF PLASTIC BAGS PROVIDED BY GROUP OF BRANCHES, BEFORE AND AFTER PRICING THE BAGS

Notes: The figure plots the three-month moving average number of plastic bags provided free of charge or sold by group of branches. We compute the three-month moving average without overlapping the pre- and post-treatment periods. For ease of viewing, we omitted the line corresponding to a branch that began charging UY$ 2 in February 2019. This branch is included in all the econometric analyzes, and behaves with the same pattern as other waves. The vertical lines mark the month when each of the groups of stores introduced the price. The magnitude of the price increases is shown in Figure 2. The rollout experiment ends in April 2019, when all groups began to charge a price of UY$ 4.

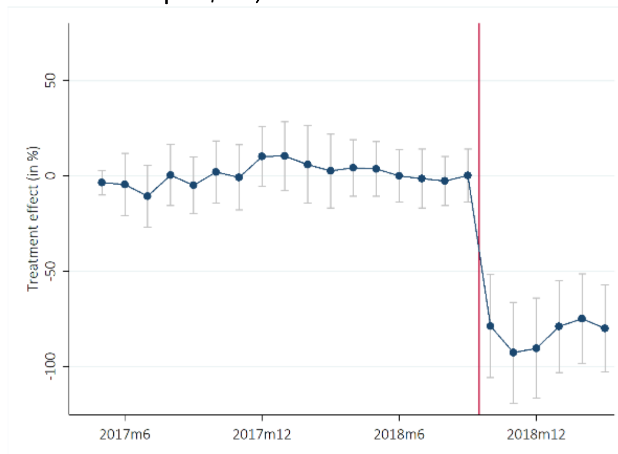
5.3 Parallel trend analysis

Before turning to the estimation of the wave specific effects of the prices, which we base on a diff-in-diff strategy, we perform a parallel trend analysis for each of the waves. Figure 5 illustrate the results. We leave aside the last month in the sample (when all branches priced the bags UY$ 4). In this figure, we plot the coefficients of the interaction between a dummy indicator for treatment status and month dummy variables, in a linear OLS regression including a full set of month and branch fixed effects and a dummy for treatment. The change in the difference between the average number of bags provided by treatment 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. The exceptions are two months, right before the implementation of the prices in wave one and three months in wave three. These could be the consequence of anticipation effects. Anticipation occurs when subjects know in advance that they are going to be treated and they react strategically. We discuss the possible existence of anticipation effects and we estimate them in section 7.2 as one of our robustness checks.

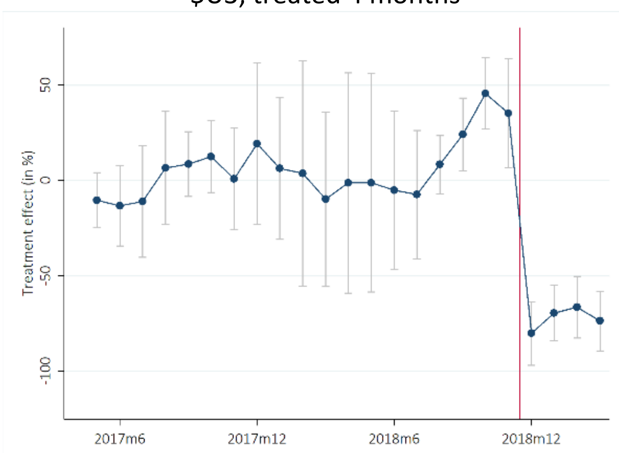
Panel (a) First wave: April 2018 (Salto), 3 branches, p = $U2, treated 12 months



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

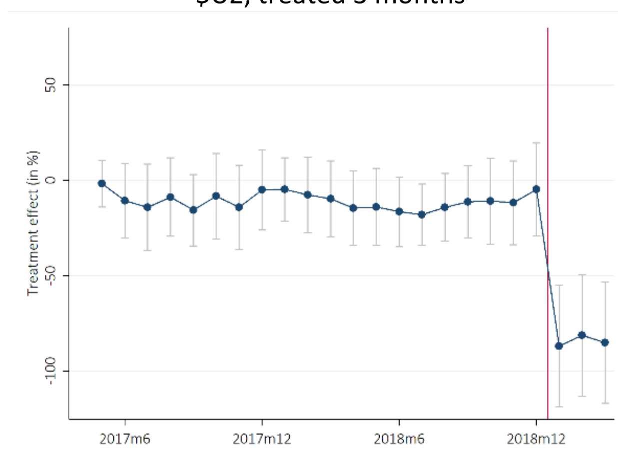


FIGURE 5: CHANGES IN THE INITIAL DIFFERENCE OF BAGS PROVIDED BY MONTH BETWEEN CONTROL AND TREATED BRANCHES

Note: The figure presents a test for parallel trends. We plot the coefficients of the interaction between a dummy variable indicating a treated branch and time dummies in a linear OLS regression including a full set of month and branch fixed effects and a dummy for treatment. The outcome variable is the total number of plastic bags (in thousands) provided free of charge or sold by branch, smoothed with a three-month moving average. Coefficients are rescaled by the pre-treatment mean of the treated stores in each wave, and expressed in percentage. Standard errors are clustered at the branch level. We include upper and lower 95% confidence limits.

5.4 Regression Results

In this subsection we present the results of the wave-specific, DiD estimations. 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).

Figure A.3 illustrates the effect of the price for the four waves. 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, regardless of the location of the branch and the date of implementation.

To determine the magnitude and significance of the effects in a formal manner, we estimate the following equation, for each wave:

$$B_{bm} = \alpha + \delta_m + \mu_b + \beta(Treated \times Post)_{bm} + \gamma Treated_b + \delta Post_m + \varepsilon_{bm} \tag{3}$$

As in the case of equation 1, here B_{bm} represents the number of bags provided by branch *b* in 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 ε_{bm} 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 5 shows the results of the OLS estimation of equation (3), for each wave.[17] 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 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 UY$ 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 5: WAVE SPECIFIC REGRESSIONS RESULTS

	(A)	(B)	(C)	(D)
	Salto April 2018	Second wave October 2018	Third wave December 2018	Fourth Wave January 2019
Price	UY$ 2	UY$ 2	UY$ 3	UY$ 2
Average treatment effect of the Price	-93.52***	-57.20***	-40.45***	-53.98***
	(7.44)	(7.06)	(7.08)	(8.54)
Pre-treament mean of treated	124.9	67.57	49.93	76.52
Percentage change	-74.9%	-84.7%	-81.0%	-70.5%
N	1,429	1,621	1,428	1,644

Notes: The table shows the difference-in-difference estimates from equation (3). Outcome variable: number of bags provided for free/sold by branch, by month. Control group: 56 branches that did not price the plastic bags during the sample period. Mean before treatment is the average number of bags provided by treated branches (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

[17] The full set of results for each wave are included in Appendices 10.7 to 10.10.

6 Price level analyses

Knowing how consumers react to different prices is valuable information for policy makers in charge of setting a tax. Because we observe three different prices, we can contribute in this direction. This is what we do in this section. We start by estimating the pooled effect of a price of UY$ 2 and UY$ 3 on the consumption of single-use plastic bags using the following equation:

$$B_{bcm} = \alpha + \delta_m + \mu_b + \beta_{p=2} P2_{bm} + \beta_{p=3} P3_{bm} + \boldsymbol{\gamma_x} \boldsymbol{X}_{cm} + \varepsilon_{bcm} \tag{4}$$

Equation (4) is identical to equation (1), except that distinguishes the treatment with a UY$2 price from that of a UY$3 price. $P2_{bm}$ is a branch-month indicator variable, equal to zero if the supermarket provided the single-use plastic bags to its customers for free in branch *b* on month *m*, and equal to equal to one if the price of the bags was UY$ 2. The coefficient $\beta_{p=2}$ is therefore the difference-in-differences estimation of the effect of the UY$ 2 price on the number of bags demanded, with respect to the zero-price situation, averaged across branches and months (α). $P3_{bm}$ is an identical indicator variable for the UY$ 3 price. Consequently, $\beta_{p=3} - \beta_{p=2}$ is the estimation of the additional effect of a price of UY$ 3 with respect to a price of UY$ 2 on the number of bags used. The rest of variables and procedures of estimation are identical to those of equation 1.

Column A, in Table 6 shows the diff-in-diff OLS estimates and standard errors of $\beta_{p=2}$ and $\beta_{p=3}$ when we estimate them without controlling for the time-varying controls $\boldsymbol{X}_{cm}$. The point estimate of putting a price of UY$ 2 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 provided in the same branches when the price was zero (74,811 bags). Column B presents the same estimates when including the time varying controls $\boldsymbol{X}_{cm}$. Results remain robust and almost identical with the inclusion of these controls.

TABLE 6: AVERAGE EFFECT OF PRICES ON THE QUANTITY OF BAGS IN THE FULL EXPERIMENT

	(A)	(B)
Price = 2	-63.59***	-63.61***
	(5.842)	(5.870)
Percentage change	-85.0%	-85.0%
Price = 3	-41.97***	-42.19***
	(7.025)	(7.079)
Percentage change	-84.1%	-84.5%
Controls	NO	YES
N	2,075	2,075

Notes: The table shows the results of an OLS estimation of equation (4). The outcome variable is the number of bags provided for free/sold by branch, by month. Controls in columns A and B include month and branch fixed effects. Controls in column B also include a set of time-varying covariates, measuring socio-demographic characteristics of the city in which the corresponding branch is located. We obtained these variables from the Continuous Household Survey. They are the following: household income (in January 2019 pesos) and the proportions of people in the city that are: employed, unemployed, under the poverty line, women, younger than 14 years old and over 60 years of age. We also include a dummy variable for any of the controls missing. Standard errors (in parenthesis) clustered at the branch level. * p<0.10, ** p < 0.05, *** p < 0.01.

The reduction in plastic bags use estimated for the price of UY$ 3 is almost identical to the one estimated for the price of UY$ 2, in percentage terms. The estimated effect is a drop of 84.1% without controls and a drop of 84.5% when we include controls. Nevertheless, we do not argue that the value of the price does not matter. First, we only have 12 observations for p = UY$3. Second, the effects are not this similar when we estimate wave-specific treatment effects, in section 5. Finally, it is easy to see in Figure 6 that the price of UY$ 4 produced an additional drop in the demand for plastic bags in all branches, independently of the previous level of the price.

The DiD estimate for the pooled effect (Table 3) 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 the above estimates comes from the comparison of the treated units against the pure control group of the 56 branches that did not price the bags.[18] This result means that our wave specific estimates from Table 5 are the main force behind the full-experiment results in Tables 3 and 6. Moreover, our wave-specific individual DiD results can be easily linked one-to-one with the Bacon decomposition of the full experiment. (See Figure A.4 in the appendix section 10.11). 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.

[18] 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 6, 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 12).

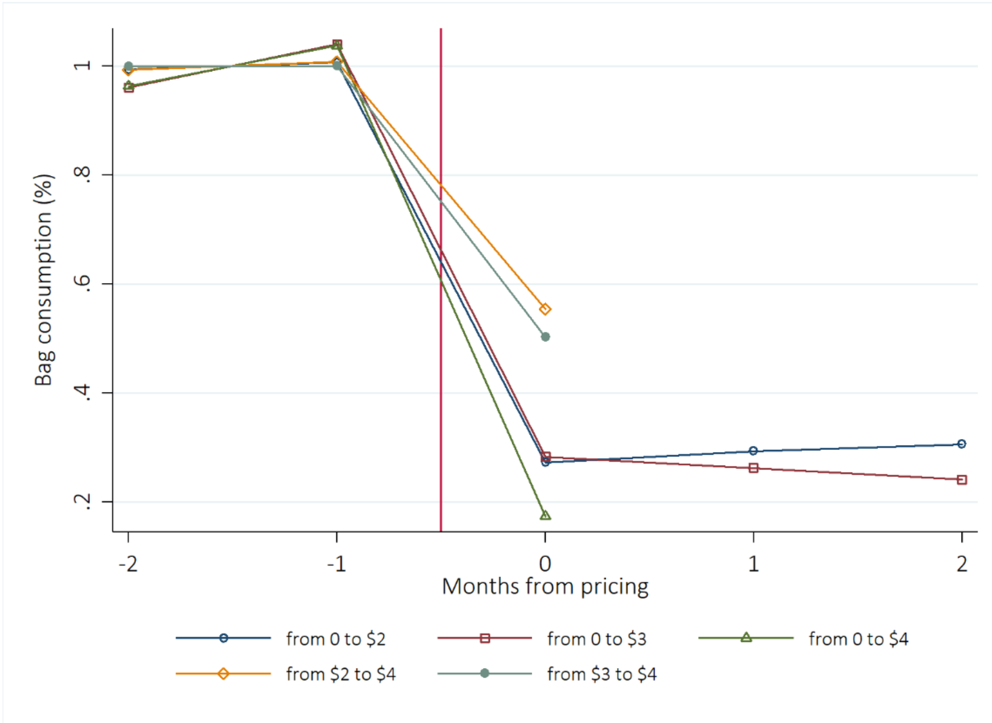


FIGURE 6: PRE AND POST PERCENTAGE CHANGE IN BAGS USED

Notes: The figure shows a pre-post analysis of the effect of five different price increases on the quantity of plastic bags used. X-axis: event time 0 is the month when price increase took place. Y-axis: bags use, normalized to one at the average level of pretreatment months -1 and -2. The line that plots consumption for those stores that increased prices from 0 to 2 includes 27 branches from four waves (April, October, December 2018, and February 2019). See Figure 2. The second line plots the consumption of bags in the three branches that started pricing the bags UY$3 in December 2018. The line "from 0 to $4" consists of 56 branches that did not price until April 2019. The fourth line, "from $2 to $4" consists of the 27 branches that were charging UY$ 2 (and thus are included in the first line), when they started charging $4 in April 2019. The last line are the three branches that increased the price from UY$3 to UY$4. We do not have data for the months following the $4 price increase.

To estimate the additional effect of a price of UY$ 4, we perform a pre-post analysis. To do it, we define five events, according to the number of different price increases in Figure 6. Second, we trimmed the database, dropping all the observations corresponding to the months that were more than two months ahead and more than two months after a month in which a price increase took place. Third, we restructure the database as in an event study; normalizing to zero the months in which the price increases took place. Fourth, we defined five groups of branches, according to the number of events (i.e., *Group1* is comprised by the branches that increased prices from UY$ 0 to UY$ 2, *Group2* is comprised by the branches that increased prices from UY$ 0 to UY$ 3, and so forth). Finally, for each branch, we normalize to one the average number of bags consumed in the two months before the change in price. With the resulting database, we estimate the following equation:

$$B_{bgm} = \alpha + \sum_{g=2}^{g=5} \mu_g Group_g + \sum_{g=1}^{g=5} \delta_g \left(Group_g * After_m\right) + \varepsilon_{bgm} \tag{5}$$

The variable B_{bgm} in equation 5 represents the (normalized) number of bags used at branch *b,* of group *g,* in month *m*. $Group_g$ is an indicator variable that takes de value of 1 if branch *b* belongs to group *g*, and zero otherwise. The constant (α) represents the pre-treatment number of bags used at branches that belong to $Group_1$. We normalized this quantity to one, so α is equal to one by construction. $After_m$ is an indicator variable that takes the value of one for observations in event-time = 0, 1 or 2. The coefficients δ_g are our coefficients of interest, representing the *pre-post* impact of different price increases on the number of bags demanded.

TABLE 7: RESULTS OF THE PRE-POST ESTIMATION OF THE EFFECTS OF FIVE DIFFERENT PRICE INCREASES ON THE QUANTITY OF BAGS

		p value: equality of coefficients				
	(A)	from \$0 to \$2	from \$0 to \$3	from \$0 to \$4	from \$2 to \$4	from \$3 to \$4
Price change:						
from \$0 to \$2	-0.709***	.	0.6119	0.0000	0.0000	0.0364
	(0.011)					
from \$0 to \$3	-0.737***	0.6119	.	0.0053	0.0000	0.0453
	(0.025)					
from \$0 to \$4	-0.826***	0.0000	0.0053	.	0.0000	0.0004
	(0.007)					
from \$2 to \$4	-0.446***	0.0000	0.0000	0.0000	.	0.6119
	(0.034)					
from \$3 to \$4	-0.497***	0.0364	0.0453	0.0004	0.6119	.
	(0.079)					
N	407					

Notes: The left panel of the table shows the results of an OLS estimation of equation (5). The outcome variable is the number of bags provided for free/sold by branch, by month, normalized to one at the average level of pretreatment months -1 and -2. The construction of the pre-post database is explained in the main text. The right panel presents the results of 10 Wald tests for the equality of the coefficients from the regression in column (A). P-values are adjusted with the Holm-Bonferroni method for multiple hypothesis testing. Standard errors (in parenthesis) clustered at the branch level. * p<0.10, ** p < 0.05, *** p < 0.01.

Recognizing that a pre-post analysis is a method that requires more identification assumptions, Table 7 illustrates that both price increases from UY$ 2 to UY$ 4 and from UY$ 3 to UY$ 4 are associated with significant additional drops in the use of plastic bags. More specifically, the average number of bags used by clients in a month decreased an additional 44.6% in those branches that were pricing the bags UY$ 2 after they increased the price to UY$ 4. Moreover, it decreased an additional 49.7% in those branches that were pricing the bags UY$3. For reference, using this identification strategy, a change in price from zero to UY$ 2 decreases the demand for plastic bags by 71%. This effect is similar to the effect of a change from zero to UY$ 3: a decrease of 74%. In the right panel of Table 7 we show that the decrease in consumption with a price of UY$ 2 is not statistically different from a price of UY$ 3 (p-value = 0.6119), but it is statistically different from the decrease in consumption with a price of UY$ 4.

The conclusions form the price level analyses presented in this section are that putting a price of UY$2, UY$ 3 and UY$ 4 had a significant effect on the quantity of bags previously provided free of charge by the supermarket. In addition, the effect of putting a UY$ 2 price is not statistically different from putting a UY$3 price. Lastly, increasing the price to UY$ 4, seems to have produced a significant additional effect. Some caveats are in order, though. Apart from not having a control group for the UY$ 4 price, we are not able to disentangle its effect from that of the complete/incomplete pricing. Second, we have a relatively small number of observations for the UY$ 3 price.

7 Robustness checks

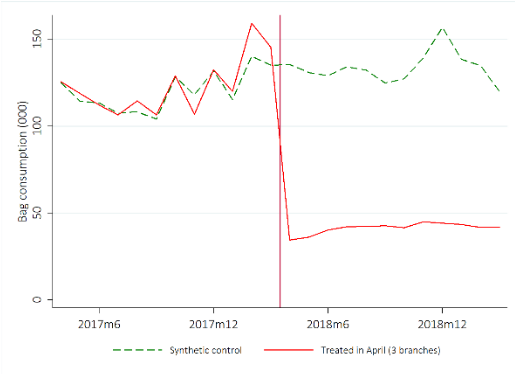
7.1 Wave-specific synthetic controls

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 at least two reasons why. One is the different sizes of the branches in the treatment group and those in the control group. Another reason is, as in any diff-in-diff analysis, the possible presence of unobservable, time-varying differences between treated and control branches that may correlate with the treatment. For these reasons, in this section we replicate the wave-specific estimations presented in in section 5 using another identification strategy based on synthetic controls (Abadie and Gardeazabal, 2003) as a robustness check. 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).

In this analysis, the donor pool is comprised in every case by the 56 branches that serve as control in the diff-in-diff estimations. We match using pre-intervention values of the outcome variable (number of bags provided by branch by month). 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 provided for free or sold by the average treated branch in each wave and its synthetic control.[19]

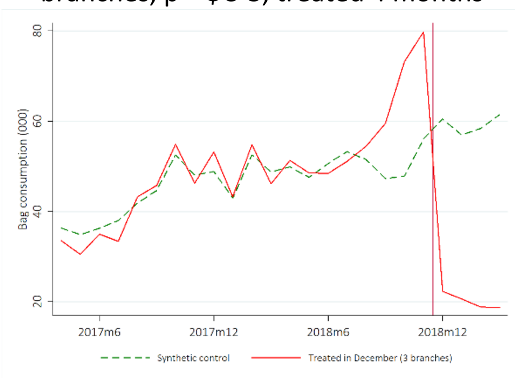
[19] In Figure A.5 we show that results are robust to leave-one-out estimations of the synthetic control (Abadie, 2021).

Panel (a) First wave: April 2018 (Salto), 3 branches, p = $U 2, treated 12 months



Panel (b) Second wave: October 2018, 11 branches, p = $U 2, treated 6 months

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



Panel (d) Fourth wave: January 2019, 12 branches, p = $U 2, treated 3 months

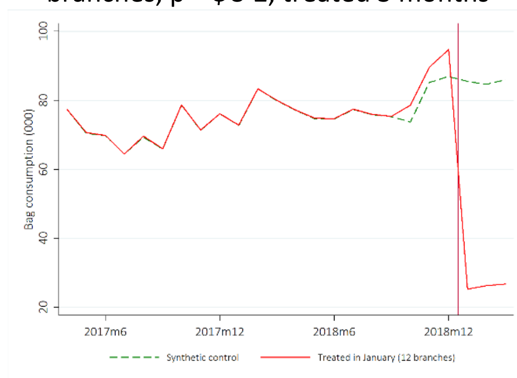


FIGURE 7: AVERAGE NUMBER OF BAGS PROVIDED BY TREATED BRANCHES IN EACH WAVE (RED CONTINUOUS LINE) AND ITS SYNTHETIC CONTROL (GREEN DOTTED LINE), BY MONTH

Notes: These figures show the three-month moving average number of bags provided for free or sold by treated branches and a corresponding synthetic control, for each wave, expressed in thousands of monthly bags per store. In every case, the donor pool for the synthetic control is comprised of the 56 branches that did not price the bags during the period. The fit between treated units and the synthetic control is achieved by minimizing a quadratic loss function base on the values of the outcome before the treatment period. The pre-treatment timespan is divided into a training period and a validation period consisting of the last three months before treatment.

Table 8 shows the diff-in-diff estimation of the effect of the price for each wave, against the corresponding synthetic control. In general, these results are similar to the DiD estimates obtained when using the 56 branches that did not priced the bags as control (shown in Table 5). 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 3.

TABLE 8: 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	UY$ 2	UY$ 2	UY$ 3	UY$ 2
Average pre-treatment difference	2.99	0.19	2.83	0.89
Average post-treatment difference	-92.31	-54.30	-39.22	-59.27
Difference in difference	-95.30	-54.49	-42.05	-60.17
Percentage change	-77.4%	-80.4%	-85.2%	-79.0%
N	48	48	48	48

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

As a last step, we perform placebo tests to assess the statistical significance of the reductions in bag consumption that we obtain when using wave-specific synthetic controls. 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 (2021), 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. The grey lines in Figure 8 show the difference between the number of bags used at each of the 56 placebo branches and the number of bags used at its synthetic. The black lines show the same difference between the actually treated branch and its synthetic. Panels on the left show the difference by month and panels on the right show the accumulated difference. It is easy to observe that, in each experiment, the actual effect of the price is an extreme value relative to the permutation distribution. We can therefore conclude that the decrease in the consumption of plastic bags does not seem to be random. The estimated ATT is larger than the estimated ATT for the placebo branches.

The accumulated differences (right panel) of the effect of the treated branches are also an extreme value with respect to 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 provision 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.

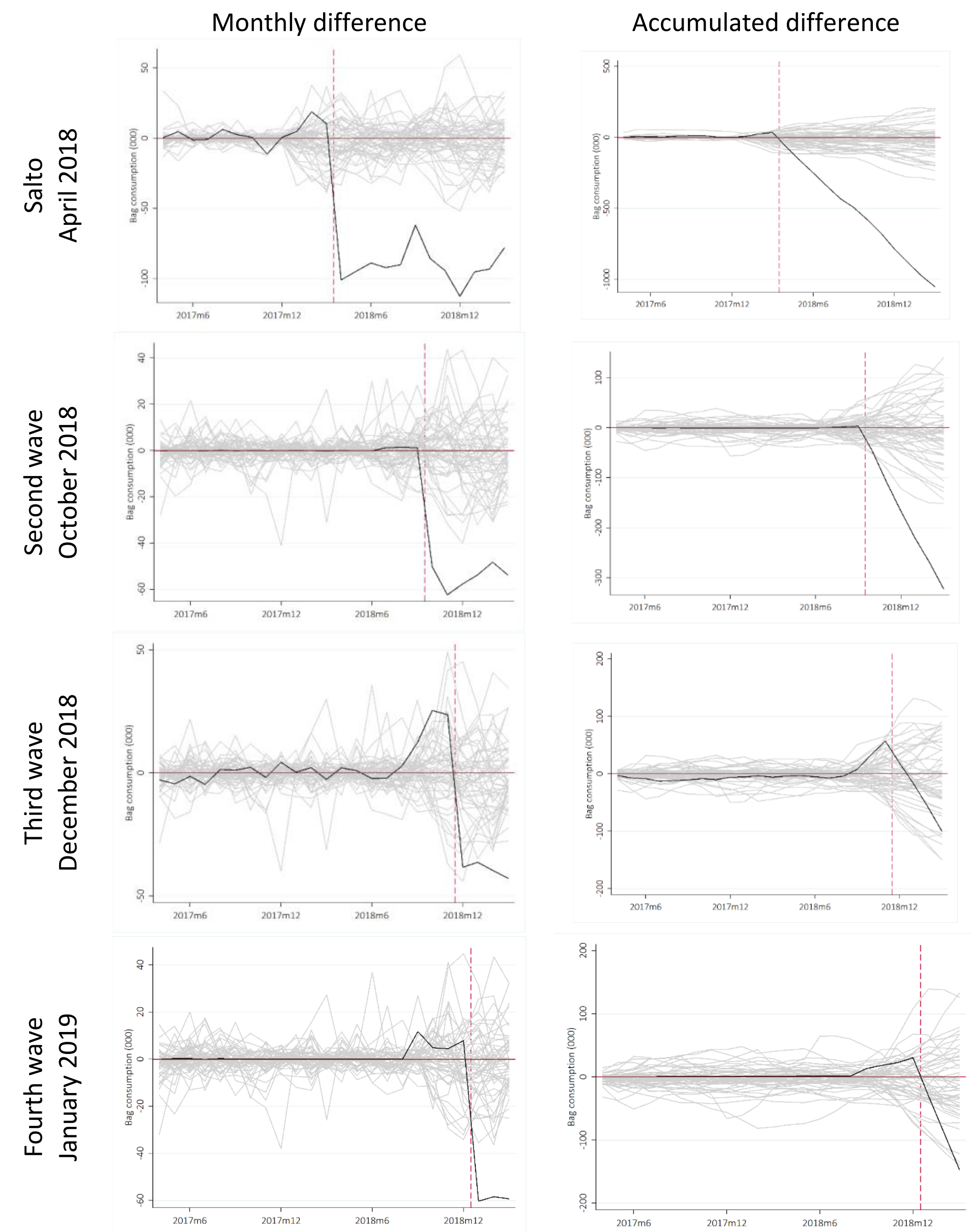


FIGURE 8: PLACEBO TESTS: DIFFERENCE IN THE NUMBER OF BAGS USED AT TREATED (BLACK LINES) AND PLACEBO (GREY LINES) BRANCHES WITH THAT OF THE CORRESPONDING SYNTHETIC CONTROL

Notes: These figures show placebo tests for the synthetic control estimations by wave. On the left panel we show (in black lines) the difference between the number of monthly bags provided at the treated branches and their synthetic control. It corresponds to the difference between the solid and the dotted line in Figure 7. The grey lines are the placebo treatment effects for each one of the 56 control branches, as explained in the main text. Panels on the right show the accumulated monthly difference, for the treated unit and each of the placebo treatments.

To conclude, 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. When using a DiD identification strategy, the results do not show a clear difference between the impacts of the two prices on demand. When using synthetic controls, the estimation of the effect of the UY$ 3 price is around 5 percentage points larger. In addition, 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, the estimate of the average effect is robust to city and branch sizes, time spell of the intervention and whether the supermarket is the only one pricing the bags in town or not. Based on the result of the placebo tests, we can conclude that it is difficult to argue that this effect was random, and not caused by the price. Figure 8 also shows that the effect of the price persists over time.

7.2 Anticipation effects

With the only exception of the second wave (October 2018), Figures 7 and 8 show that treated branches exhibit a rise in the consumption of bags during the last months *before* the intervention, relative to their control. This rise 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 this possibility in our case study. In Salto, for example, as explained in section 2.1, clients were actually informed about the future price by the media campaign. Moreover, adhered stores displayed the campaign sign at their entrance, communicating their shoppers that bags a price for plastic bags would come into effect in April 2, 2018. As a result, it is fair to conclude that clients 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).

Knowing in advance that the plastic bags would be priced, customers may have increased the use of bags before the price went into effect to stock costless bags. Because this would bias our estimation on the effects of the prices, we need to take it into account. 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, but this have not yet taken place. The implementation period starts with the actual implementation of the price. 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. Nevertheless, we use the same length for the rest of the waves for consistency.

By dividing the intervention period in an announcement period and an implementation period, 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:

$$B_{bm} = \alpha + \delta_m + \mu_b + \beta_1 (Treated \times Anticipation\_period)_{bm} + \beta_2 (Treated \times Pricing\_period)_{bm} + \beta_3 Treated_b + \beta_4 Anticipation\_period_m + \beta_5 Pricing\_period_m + \varepsilon_{bm} \quad (6)$$

As in the case of previous equations, here B_{bm} represents the number of bags provided to customers at checkout in branch *b* in 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 four 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, ε_{bm} 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.[20]

Results of the estimation of equation (6) are in Table 9. Each column (A) to (D) represents the results of a specific wave. As shown in line 3 of Table 9, 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), the supermarket increased the number of bags provided to customers free of charge by an average 10.6% during the 4 months previous to the implementation of the price, relative to the pre-announcement period. 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 results for the second and fourth waves.

What could explain this difference in behavior? The simplest answer is that customers in Salto, Las Piedras and La Paz knew about the future price in advance and that customers in the other cities did not. As just recalled, we know from section 2.1 signs in the supermarkets´ doors and a media campaign preceded the price in Salto, the first wave. We are not aware of such an information campaign in the cities of Las Piedras and La Paz. Nevertheless, because the implementation of the price in these two cities was coordinated with other stores, it is possible that this information leaked. In support of this hypothesis, we find an anticipation effect only in the cities where the supermarket was not the only store pricing the bags. Another possible answer is that customers of branches in the second and fourth waves were given reusable bags for free, before the implementation of the price. Nevertheless, as we saw in section 3.2, this was not the case. Quite the contrary, the supermarket gave one reusable bag to customers with a loyalty card in Salto during the last three weeks and just a few bags in some of the cities that comprised the second wave. Therefore, that customers in Salto, Las Piedras and La Paz knew about the future price in advance and customers in the other cities did not is the most plausible answer.

[20] 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 Appendices 10.7 to 10.10.

TABLE 9: ANTICIPATION EFFECTS

	(A) April experiment	(B) October experiment	(C) December experiment	(D) January experiment	(E) Full Experiment
Pre-treatment mean for treated	119.7	66.57		74.58	72.22
			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: The table shows the difference-in-difference estimates corresponding to equation 6. Each column is a different regression. Outcome variable: number of bags provided for free/sold by branch, by month. Control group: 56 branches that did not charge price plastic bags during the sample period. 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

Comparing the results in Table 5 and those in Table 9 above, we can see how not considering the anticipation effect causes to overestimate the true impact of the price. Effectively, in Table 5 we show that the effect of the price in Salto was a drop of 93.520 bags per month, per branch. When we disentangle the anticipation effect from the longer-term effect (Table 9), we find that the policy caused a drop of 89.270 bags. In other words, the naïve estimation over-estimate the effect of the price by 4.25 thousand bags per month per branch.[21] The bias introduced by the anticipation effect in the case of the third wave (December) is of 3.45 thousand bags per month, per branch.

The existence of anticipation effects illustrates the advantage of having a long pre-treatment period, to being able to disentangle them from the actual treatment effect. To see this, we ask ourselves what our policy evaluation would have been if we only had had only three months of pre-treatment data. As shown in Table 10, 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 (column A) is 28% higher than that presented in Table 5 (showed again in Table 10, line 1). In the case of the third wave, column C, the coefficient is 41% higher. In other words, impact evaluations with short pre-treatment periods may look quite different from longer-term evaluations, particularly when subjects anticipate the policy.

[21] 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 5 and 9. 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.

TABLE 10: THREE-MONTH VS LONGER RUN EFFECTS

	(A) April experiment	(B) October experiment	(C) December experiment	(D) January experiment
A. 24-month 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-month 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: The table shows the wave-specific difference-in-difference estimates of the effect of the prices. The 24-month estimation shows the effect of the prices when using data for the entire period. The 6-month estimation shows the results when using data for a censored period that includes only the last three months of the pre-treatment period and the first three months of the post–treatment period. Outcome variable: number of bags provided for free/sold by branch, by month. Control group: 56 branches that did not priced the plastic bags during the sample period. 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

7.3 Effect on sales

A possible concern regarding our results is that the drop in the use of bags that we observe may not only be the result 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 where it did, to avoid paying for a bag, clients could have well opted to go to other stores that were not charging the bags. Moreover, 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. 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 values 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. This data spans 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. The branch located in Maldonado, therefore, experiences more seasonal variation than the rest of the branches in this comparison. Taking this consideration, 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 inflation-adjusted sales.

TABLE 11 – DiD EFFECT OF CHARGING FOR BAGS ON SALES

	(A)	(B)	(C)
	Sales	Bags	Bags/Sales
Treated (Salto)	-4.41	-104.74***	-0.104***
	(2.86)	(9.03)	(0.005)
mean before	102.08	124.85	0.126
pct change	-4.32	-83.89	-82.71
N	120	120	120

Notes: This table shows the DiD estimate of the effect of pricing the plastic bags on total sales at the branch level. We perform the analysis with three treated stores from Salto and two control stores (one in Maldonado and one in San Carlos). The total number of observations corresponds to five branches times 24 months. Column A: sales when netting out the revenue generated by charging the bags in Salto after April 2018. They are expressed in Uruguayan pesos of January 2019 and for confidentiality reasons were transformed them to an index with base average monthly level of sales for the entire sample equal 100. Column B: bags are measured in thousands. Column C: The number of bags is divided by the sales in dollars. Controls include month and branch fixed effects. Standard errors (in parenthesis) clustered at the branch level. In Appendix 10.13 we present results with wild bootstrap p-values with different clustering levels. * p<0.10, ** p < 0.05, *** p < 0.01

Results (Table 11) show that sales in Salto decreased 4.3% in real terms relative to the control branches, on average, although the result is not statistically significant at the 10% level (p value = 0.197; 95% CI: -12.34, 3.52). At the same time, consistent with the main results above, when estimating the effect of the price on the number of bags with data for these five stores, the OLS estimate is a drop of 83.9% (p value = 0.000, 95% CI: -129.8, -79.7 monthly thousand bags). Reassuringly, the effect of the price on the number of bags *per 2019 dollar of sales* is a drop of 82.7% (p value = 0.000, 95% CI: -0.112, -0.096 bags per dollar of sales). Another way of expressing this result is that the average customer real expenditure per disposable plastic bag increased, from $9.2 to $27.1 in 2019 dollars. Thus, even considering a possible loss of sales, the policy had a large impact.

Although we 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 suggest otherwise. If clients had moved to other stores as a response to the price of plastic bags, we would observe lower estimates of the effect of the price in cities where 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 5 that the effect a UY$ 2 price 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. A loss of clients in this case is improbable because, as commented in Section 2, the estimated grocery market-share of the supermarket in Salto is between 40% and 50%. Moreover, all supermarkets in Salto, and a considerable 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 other 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. 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. Quite the contrary, the *voluntary* rollout suggests that pricing the bags may have been profitable.

8 Conclusion and discussion

We find that prices of US$ 0.07 and US$ 0.10 per bag caused a very large drop in the number of plastic bags used by customers of a discount supermarket chain in Uruguay. The estimated drop lies between 70% and 85%, with no clear difference between the two prices. Estimates are robust in magnitude and statistical significance to different methods of estimation and different specifications of the estimated equation. Placebo tests lead us to conclude that the effect that we find is not the result of chance. Despite limited data, we fail to find evidence that the supermarket´s sales decreased as a consequence of pricing the bags. Quite the contrary, the fact that the chain rolled out the price to other cities voluntarily is consistent with the hypothesis that pricing the plastic bags increased its profits. Finally, although we were unable to identify a clear difference in the effects of the two prices, a price of US$ 0.14 seems to have had a significant additional effect in the demand for plastic bags in all branches, independently of the previous level of the price. We estimate that this price may have decreased the overall demand by an additional 40%, on average.

We also find evidence of an increase in the demand for zero-price bags during the period of three months right before the implementation of the price. This evidence is consistent with a strategic behavior from clients who were informed about the upcoming price, or may have anticipated it. This strategic behavior biases upward the estimated effect of the price in the short run.

The large impacts on bag use that we report are similar in cities in which the supermarket was the only one pricing the bags and in cities in which it was not. Notwithstanding, we do find evidence consistent with the hypothesis that, when acting uncoordinatedly, the supermarket chose to price the bags in cities in which it had a relatively larger market share.

Our results are consistent with the argument that a zero price is a special price and the findings that putting a small price to goods and services in education and health that were originally free of charge causes relatively large effects on their demand (Holla and Kremer, 2009; Banerjee and Duflo, 2009). 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. Nevertheless, a permanent drop between 70% and 85% is larger than what previous studies report. Homonoff et al. (2020) report that a US$ 0.07 tax on all disposable (paper and plastic) bags in the city of Chicago decreased its use by 40% on the extensive margin and by 60% on the intensive margin during the first two months and was not statistically different from zero at the end of the first year. Homonoff (2018) finds that a US$ 0.05 levy on disposable paper and plastic bags in Montgomery County decreased its use by over 50% in the first three months, combining the extensive and the intensive margin. Moreover, these estimates may be biased upward because Homonoff and colleagues did not consider the possibility of an anticipation effect.

To discuss what may be the reasons behind the difference between the effects found by these studies and the effects that we find, below we look at possible differences between the US and Uruguay in the other determinants of bag consumption. These include: (a) income, (b) preferences, (c) the available alternatives in the choice set and (d) the relative cost of these alternatives.

The difference in households' income between Uruguay and the US is an obvious candidate for explaining the large difference between the effects that similar prices had in these countries. Income determines not only the size of the choice set, but also shopping patterns, such as its frequency and mode of transport. The sample period in Homonoff et al. (2020) is November 2016 – March 2018. For reference, the average household income in the city of Chicago in 2017 was USD 84,552 (U.S. Census Bureau, 2019). In the case of Homonoff (2018), the sample period is October 2011 – April 2012. According to the same source, the average household income in the treated Montgomery County, Maryland, during 2011-2012 was USD 125,397. In the same period, the average household incomes in the control city of Washington, DC and Arlington County, Virginia, was USD 75,002 and USD 131,758, respectively. For reference, as we report in Table 2, the average household income during the sample period (April 2018 – March 2019) in Uruguay was USD 21,192 for the treated cities and USD 20,231 for the control cities. In other words, relative to the average income during the sample periods, a bag in the treated cities in Uruguay cost four times what it cost in Chicago, and eight times what it cost in Montgomery County. Moreover, the chain from which we get the data is a discount store chain, with an explicit marketing strategy based on low prices. According to the chain´s CEO (personal conversation, April 25, 2019), the set of their customer does not intersect with the set of customers from another chain targeting high-class clients. In other words, customers of our sampled discount chain may belong to relatively lower deciles in the Uruguayan income distribution. For these reasons, we conclude that differences in incomes could be an important explanatory factor behind the difference in the effects of similar prices between Uruguay and the US. In line with this argument, Taylor and Villas-Boas (2016) find that discount chain shoppers in California may have a more price-elastic demand than that of shoppers at a national chain. On the other hand, Homonoff et al. (2020) did not find evidence of different impacts across neighborhoods with different incomes in Chicago. Similarly, we do not find evidence of different impacts across different neighborhoods of Montevideo and nine other towns, albeit with a simpler, cross-section comparison.[22] Notwithstanding, the variation in average incomes within Montevideo neighborhoods and between Uruguayan cities is much lower than the variation between US and Uruguayan sample cities.[23]

Individuals´ preferences is another important determinant of the demand for disposable plastic carryout bags. Cultural and institutional determinants of tastes and beliefs about the environment could explain the differences between US and Uruguayan price elasticities. According to the World Values Survey, 64.2% of respondents agreed with the statement "protecting the environment should be given priority, even if it causes

[22] To obtain this result, using data from the Continuous Household Survey of the National Statistics Institute (Instituto Nacional de Estadística, 2018), we first computed the average household income in the control cities for the period 2013 – 2018. (In the case of Montevideo, we compute the average household income for each neighborhood). We then computed the average drop in bags consumption for each of the 12 branches in those 9 cities and the average drop 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 and cities is 84%, consistent with our results from Table 3. This drop correlates negatively with household income. Nevertheless, this correlation is not statistically significant (p value = 0.685). The results of this exercise are available in the replication files for the paper.

[23] More specifically, while the average income in the City of Chicago is 4 times that in the Uruguayan cities of our sample and the average income in Montgomery County is six times, the city in the 90th percentile of our sample has an average income that is 44% higher than the city in the 10th percentile. In the interquartile range, the average income increases 15%.

slower economic growth and some loss of jobs" in Uruguay in 2011, while only 37.2% agreed with this statement in the US (Inglehart et al, 2014). This percentage had increased to 50% in the US in 2017 (Haerpfer et al, 2020). Unluckily, this wave of the survey was not conducted in Uruguay, and the Director of the firm in charge of this survey in Uruguay told us that he is not aware of any similar question being included in any survey conducted in Uruguay since then (e-mail communication, December 12, 2020). In addition, we do not have comparable information either on the level of plastic bags consumption prior to the prices in the US and Uruguayan cities, to check whether this difference in stated preferences towards the environment correlates with plastic bag consumption. It could be said though that having the Uruguayans so strong preferences for the environment but at the same time reacting in the way they did to a modest price for plastic bags would be at odds with the findings that prices crowd out "moral sentiments" (Gneezy and Rustichini, 2000; Bowles and Polania-Reyes, 2012). For all these reasons, we are not inclined to conclude that higher preferences for the environment in favor of the Uruguayan citizens is a fundamental factor explaining the difference in the effect of prices for plastic bags with respect to the US.

Another determinant of the price elasticity of plastic bags is the relative cost and availability of substitutes. In this respect, we are not aware of any relevant differences between the Uruguayan and US cities sampled in the studies that could explain the differences in the effect of similar prices between these cities. True, there are no paper bags in supermarkets in Uruguay, but in the US these were charged with the same price that plastic bags. Also true, in Salto and in some cities of the second wave, the supermarket chain gave loyal customers a reusable bag for free, before the price went into effect. However, in Montgomery County, some stores subsidized the use of reusable bags, in addition to the price for plastic bags. Moreover, the evidence suggests that neither policy had a significant impact.

In sum, of the list of theoretical determinants of the demand for plastic bags, we conclude that the difference in average income between Uruguay and the US is the strongest candidate to explain the observed differences in the effect of the prices. Bags in Uruguay cost four and eight times more than in Chicago and Montgomery County, respectively, relative to households' income.

There are two main lessons to learn from our study. First, a well-enforced price for plastic bags could have a higher effect on the consumption of these bags in a less developed country than in a developed one. Second, this result could be achieved despite some stores non-compliance. Incomplete environmental regulation is commonly associated with pollution leakage from regulated to non-regulated firms, or from jurisdictions with tighter environmental regulation to those with a weaker one, as in the pollution haven hypothesis (Fowlie, 2009). The Uruguayan experience is consistent with the hypothesis that under uncoordinated, unregulated competition, stores are locked in a Pareto-inferior, zero-price equilibrium and a regulation, or a third party (such as a chamber of commerce), could solve the coordination problem that the stores seem to have. Moreover, this could produce positive environmental leakages. The regulation, or the agreement promoted by the third-party coordinator, albeit incomplete, may provide information about the profitability of pricing the bags to some stores, such as large supermarkets, which voluntarily and individually, may then start pricing the bags in other jurisdictions in which they have a significant share of the grocery market.[24] This positive leakage may be more

[24] Fowlie (2009), using a theoretical model, demonstrated that incomplete regulation of emissions in an imperfectly competitive industry could not necessarily reduce allocative inefficiencies if the regulation targets the more inefficient firms

difficult to observe with a tax because a tax applied to every store in a jurisdiction may provide less clear information about the profitability of being the only stores pricing the bags in other jurisdictions.

Finally, a cautionary note. Although our results show 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 ban on shopping bags. A comprehensive evaluation of the environmental impacts of a charge on disposable plastic bags should consider this rebound effect. Unluckily, we do not have the data to estimate the possible increase in the demand for plastic trash bags in this work. The effect of incomplete regulations, and the mechanisms under which this effect works, could be fruitful areas of future research.

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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).[25] 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.[26] 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.[27] In the US, between 2007 and today, cities, counties and states passed 156 norms regulating the use of disposable single-use carryout bags.[28] 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. In February 2019 Chile became the first Latin American country to ban plastic bags in supermarkets by law. (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 not 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).

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[25] Some authors use levy to refer to the charge on disposable bags. Other, 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.

[26] 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.

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

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

10.2 Literature review

Empirical estimations of the effect of taxes or levies on the consumption of plastic bags that use a causal inference technique and at the same time are able to observe the level of consumption are scarce.

Examples of pre-post studies using observational quantitative data are Convery et al. (2007), for Ireland, Dikgang et al. (2012a and 2012b), for South Africa, He (2012), for China and Martinho et al. (2017), for Portugal. Examples of difference-in-differences studies relying on self-reported ordinal data collected in telephone surveys are Poortinga et al. (2013) and Rivers et al. (2017). Poortinga et al. (2013) found that a five pence charge for each single-use bag introduced in Wales on October 1, 2011, increased the proportion of respondents declaring to bring their own bag on their last visit to the supermarket. Rivers et al. (2017) use data from a periodical household survey in Canada, before and after a disposable bag levy of C$ 0.05 in Toronto. They found that the levy increased the use if reusable bags by 3%.

We are aware of only three empirical that use a causal inference technique and at the same time observe the level of consumption. The first one is Jakovcevic et al. (2014). These authors interviewed a sample of 457 customers in supermarkets in the city of Buenos Aires and in the Great Buenos Aires (outside the city), before and after supermarkets in the former, but not in the latter, put a price on disposable plastic bags. The price was the response of the supermarkets to a provision of the environmental protection office of the government of the city of Buenos Aires that established that the supermarkets in the city "would only be allowed to deliver larger and stronger plastic bags" (Jakovcevic et al. (2014), p. 374). Because the provision implied higher costs to the supermarkets, these decided to put a price of US$ 0.25 for "medium size bags" and US$ 0.4 for "big size bags". The authors evaluated the effect the prices on the proportion of interviewed customers using reusable bags. To do it, the authors conducted the survey in four points in time. The customers from Great Buenos Aires acted as the control group at 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 observed that the charge steadily increased the use of reusable-bags first two months.

Unlike Jakovcevic et al. (2014), Homonoff et al. (2020) and Homonoff (2018) estimate the impact of a levy on the quantity demanded of single-use bags (plastic and paper, taken together) by supermarkets customers. Homonoff et al. (2020) studied the effect of a US$ 0.07 tax on paper and plastic bags of all thicknesses, 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 the city (where there was no tax on disposable bags), before and after the tax. Interviews took place at four different points in time, between November 2016 and March 2018. This sample period spans three policy regimes: (a) a ban on plastic bags less than 2.25 mils thick, (b) a month of no regulation and (c) a tax on paper and plastic bags of all thicknesses. 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. On the extensive margin, the tax decreased the likelihood of a consumer using any positive number of disposable bags (paper or plastic, of any thickness) by 33 percentage points in the first two months (from an average percentage of 82 points before the tax) relative to during the ban. On the intensive margin, the tax decreased the average number of disposable bags used by almost one bag per trip on average in the first two months (from an average of 2.5 bags per trip in the month of no regulation before the tax). An important finding of this study is that the tax exhibited a decreasing effect over the first year of implementation. A year later, the proportion of customers using disposable bags was 24.8 percentage points lower than in the ban, instead of 33, and it did not exhibit an effect statistically different from zero in the third and fourth quarter of implementation. 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 addition, their pre-treatment period is either the last two months of a ban or a month in between the ban and the tax. These may not reflect properly the level of bags consumed in the absence of regulation. For example, because of the ban, customers may have acquired reusable bags, which they may continue to use in that month of no regulation.

In an earlier work, 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. Her 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. The author used observational data, with an identical collection strategy as that described above for Chicago. Using a difference-in-difference strategy, she found that the proportion of customers using at least one disposable bag (paper or plastic) decreased from 82% to 40%. On the intensive margin, she observed that the tax decreased the number of bags used by bag users by 0.22 bags per trip (a decrease of 8%). Combining the extensive and the intensive margin, she found that the tax decreased the number of disposable (paper or plastic) by just over one bag per trip. In addition, the tax increased the proportion of customers using al least one reusable bag by 32.7 percentage points and the proportion of customers using no bag at all by 11 points. It also increased the number of reusable bags by 0.15 bags (an increase of 9%). Using cross-sectional variation across stores, she also found that customers in stores that offered a bonus were as likely to use a disposable bag as those in stores that did not. Finally, using scanner data from a supermarket chain, Homonoff (2018) also tracks the evolution of disposable bag use from the first month of implementation of the tax up to 2.5 years in DC and six months in Montgomery County. She observes that the drop in the proportion of transactions that included at least one disposable bag after the tax decreased by approximately 15% in the two sites, and remained at this lower level for the observed period. Nevertheless, this longer-term analysis does not include a control or a pre-tax period.

Other studies that have a quantitative measure of the number of bags used and a control group evaluate the impact of a *ban,* instead of a levy, on the demand for disposable carryout thin-plastic bags (and other outcomes). Taylor and Villas-Boas (2016) evaluate the impact of a policy effective January 1, 2014, in the neighboring California cities of El Cerrito, Richmond and San Pablo. This policy implemented a *ban* on single-use thin *plastic* bags coupled with a mandatory provision by which retail stores must charge at least US$ 0.05 for each single-use *paper* or any other reusable bag provided to customers (e.g.: thick-plastic). The authors wanted, first, to evaluate whether these twin measures had the intended effects on the types of bags used and, second, to compare its effectiveness with the policy of only taxing disposable bags with a five-cent levy. To do this, Taylor and Villas-Boas recorded information on the number and type of bags used and other variables, from a sample of customers they observed during checkout at a set of stores in these cities and the control cities of Berkeley and Concord. Berkeley had implemented a ban on plastic bags and a minimum price of 10 cents for paper bags. In contrast, no regulation was in place in Concord. In both cases, there was no bag policy change during the sample period. Customers were observed in four visits of 1-2 hours each, in every store in the sample, in November 2013 and in December 2013, before the Richmond policy went into effect. In the post-treatment period, the authors observed the customers in 4 to 6 visits that took place in January 2014 and in February 2014. They also collected follow-up data in March and April 2014. Finally, to compare their results with the policy of only taxing plastic and paper bags five cents, they used the results in Homonoff (2018). Using a difference-in-difference strategy, they find that the policy of banning thin plastic bags coupled with a mandatory price on paper and thick-plastic bags had a similar positive effect on the proportion of customers using reusable bags. It also had a similar negative effect in the proportion of customers using disposable (paper or plastic) bags. However, the twin measures changed the proportion of plastic and paper disposable bags used. The proportion of customers using plastic bags decreased between 80 and 90%, while the proportion of customers using paper bags increased 46%. The authors also find that this increase in the usage of paper bags was significantly lower (10%) in a discount chain charging 10 cents (instead of five cents) for paper bags and offering 15-cent reusable thick-plastic bags.

Finally, Taylor (2019 and 2020) goes beyond the evaluation of the impact of a ban on the quantity of disposable carryout bags used, providing the first contributions towards a full welfare evaluation of this policy. A ban or a price on single-use shopping bags may decrease external, collection and final disposal costs. Nevertheless, on the negative side, Taylor (2020) found that they might also increase the time customers spent at check out. Using observational and cashier scanner data from a supermarket chain, she found that the California ban on single-used plastic bags (coupled with a tax on paper and thicker, reusable plastic bags) increased 3.1% the checking out time at supermarkets. 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.

10.3 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

10.4 Other estimators for the main DiD effect in the Event Study Design

In settings with staggered adoption of treatment, the DiD estimator may be subject to biases. The ATT from a DiD regression with two way fixed effects (stores and months) consists of an average of comparisons between groups treated at different points in time. Some of these are “bad comparisons” (i.e. late treated stores vs. early treated ones) which may lead to negative weights in the presence of heterogeneous and dynamic effects.

Using the *twowayfeweights* Stata command (from de Chaisemartin and D’Haultfœuille), we verify that there are no negative weights in our main estimation. Thus, our main DiD estimations are not subject to this type of biases. Nonetheless, we replicated our main treatment effect estimation using three new alternative estimators, proposed by de Chaisemartin and D’Haultfœuille (2020), Callaway and Sant’Anna (2021), and Borusyak, Jaravel, and Spiess (2021). These provide more flexibility than the naïve DiD, they deal with heterogeneous treatment effects across groups and time (different store sizes, different treatment effects in each wave, etc), and they also improve the weighting of observations far from the treatment period. Each method has different properties and advantages. None of them is superior to the others in every circumstance.

We find that our results are robust to these three methods (see Figure A.2). Moreover, although not identical, the three methods yield very similar point estimates. Borusyak et al estimates have the tightest confidence intervals. We also note that the pre-treatment effects are not different from zero, supporting the parallel trends assumption. The treatment effect arises instantly on the month when stores started pricing the bags and it is persistent and stable during the six post-treatment months.

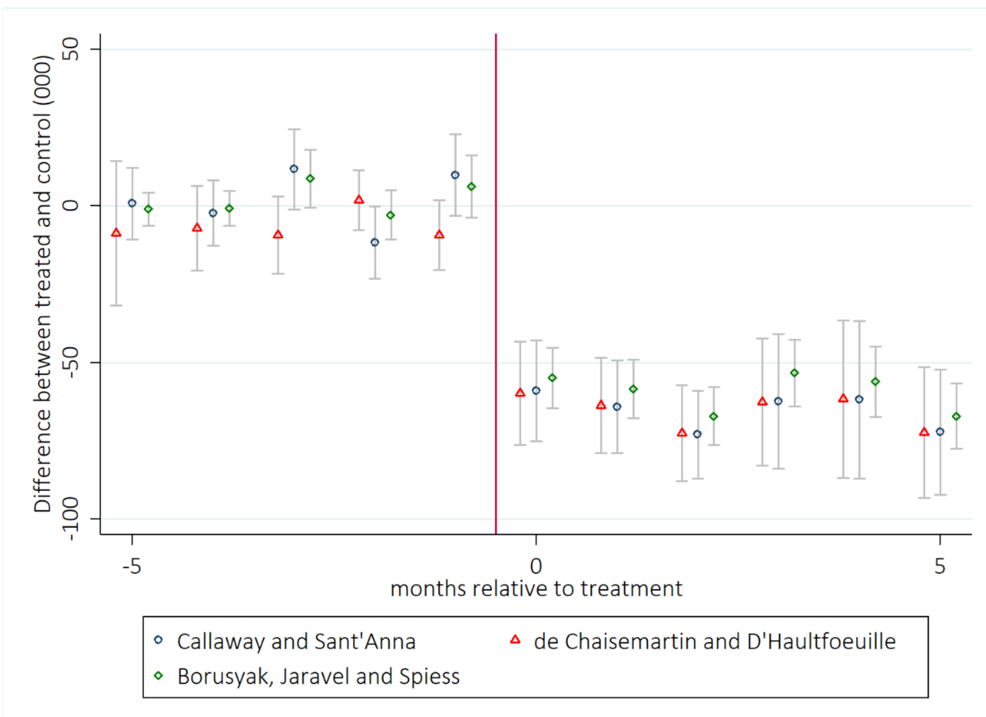


FIGURE A.2: ROBUSTNESS OF THE MAIN TREATMENT EFFECT IN THE EVENT STUDY DESIGN FOR ALTERNATIVE ESTIMATORS

Notes: The figure plots the average DiD treatment effect for five pre-treatment and six post-treatment using the full sample and setting to relative time = 0 the moment in which the store started charging the bags. The tree estimators are from Callaway and Sant’Anna (2021), de Chaisemartin and D’Haultfœuille (2020), Borusyak, Jaravel, and Spiess (2021).

In Table A.1., we present average DiD point estimates of the pre and post treatment monthly effects. In column A, we show those corresponding to the same window of three pretreatment and three post treatment event periods of the Goodman-Bacon event study presented in the main text (section 4.2), also shown. In column B, we show the point estimates corresponding to five pretreatment and six post treatment event periods, as in the Figure A.2. Column A shows that the main point estimate from our event study design (a drop of 64,741 bags per store-month, or 74%), is in the range of the new estimates. In percentage terms, the new estimates of the drop in demand fall within 68% and 78%. Column (B) shows the average DiD point estimates corresponding to the time window of the figure A.2 are also of similar magnitude. In percentage terms, they fall within 75% and 85%.

TABLE A.1: TREATMENT EFFECTS IN THE EVENT STUDY DESIGN WITH DIFFERENT ESTIMATORS

	(A)	(B)
Method \ Time window	3+3 months effect	5+6 months effect
Goodman-Bacon (main text)	-64,741	
Callaway and Sant'Anna	-67,186	-68,729
de Chaisemartin and D'Haultfœuille	-59,022	-59,832
Borusyak, Jaravel, and Spiess	-61,566	-63,593
Mean of T in pre-treatment	86,568	81,159

Notes: This table presents the average DiD point estimate of the pre and post treatment monthly effects. In column B, we show the results corresponding to five pretreatment and six post treatment event periods, as in the figure A.2. In column A, we compare these point estimates relative to the event study estimate in the main text (section 4.2). To do this, we use the same window of three months centered at the beginning of the pricing for each wave, as in the text. The methods to estimate the event study coefficients are those from Goodman-Bacon (2019), Callaway and Sant'Anna (2021), de Chaisemartin and D'Haultfœuille (2020), and Borusyak, Jaravel, and Spiess (2021).

10.5 The comparability of the different cities

TABLE A.2: DIFFERENCE OF MEAN TESTS FOR TREATED AND CONTROL CITIES, SELECTED VARIABLES

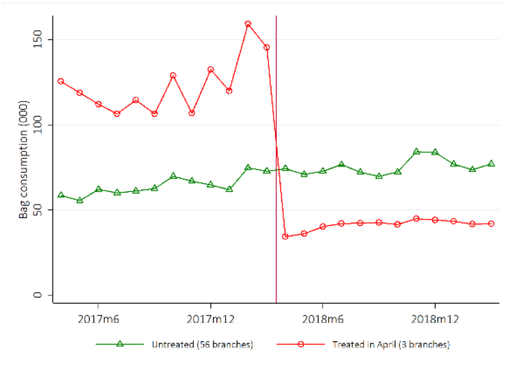
Variable	Mean Treated	Mean Not treated	diff	std. Err.	p-val	
Supermarket chain data						
Bags used at checkout by month in city (000)	110.71	102.44	8.27	30.26	0.79	
Market share in city						
by area of stores (m2)	0.65	0.24	0.41	0.09	0.00	***
by number stores	0.56	0.29	0.28	0.09	0.01	***
by number of registers	0.66	0.31	0.35	0.09	0.00	***
Number of stores in city	1.69	1.30	0.39	0.34	0.26	
Largest store in town belongs to chain	0.75	0.00	0.75	0.11	0.00	***
Cities data						
Western city	1.00	0.20	0.80	0.13	0.00	***
Number of supermarkets in city	3.88	6.30	-2.43	1.79	0.19	
Supermarkets area (m2)	2502	7274	-4772	2309	0.05	**
Cash registers	23.00	56.60	-33.60	19.39	0.10	*
Population	33756	38339	-4583	7117	0.53	
Female (%)	0.52	0.52	-0.01	0.00	0.04	**
Age	36.01	36.02	-0.01	0.63	0.99	
Children (%)	0.21	0.21	0.01	0.01	0.33	
Married (%)	0.06	0.06	0.00	0.00	0.92	
Retired (%)	0.22	0.20	0.02	0.01	0.21	
Low education level (%)	0.64	0.60	0.04	0.03	0.18	
Occupied (%)	0.60	0.62	-0.01	0.01	0.36	
Unemployed (%)	0.04	0.04	0.00	0.00	0.86	
Income (UY$ 2019)	57344	54744	2600	3111.	0.41	
Below poverty line	0.03	0.04	-0.01	0.01	0.38	
Cities data, excluding Montevideo						
Supermarkets (#)	3.19	8.00	-4.81	2.96	0.12	
Convenience Stores (#)	4.44	7.30	-2.86	3.91	0.47	
Schools (#)	6.81	12.10	-5.29	3.62	0.16	
Gas Stations (#)	4.50	5.10	-0.60	1.06	0.58	
Pharmacies (#)	3.06	6.20	-3.14	2.67	0.25	
Banks (#)	2.75	3.10	-0.35	0.74	0.64	
Other amenities (#)	80.50	173.90	-93.40	91.65	0.32	

Notes: The table shows tests for balance between cities during the rollout of the price. Treated cities are the 16 locations where the supermarket chain introduced a price (US$ 2 or UY$ 3) for plastic bags, and the not-treated group consists of 10 cities where the price remained at zero until April 2019. Each line is from a different linear regression at the city level. We exclude the first city (Salto, with 3 stores), and the capital city of Montevideo (with 43 stores). The number of bags used at checkout by month in each city is calculated for the pre-treatment period (before April 2018). See Table 2 for an explanation of the variables and data sources.

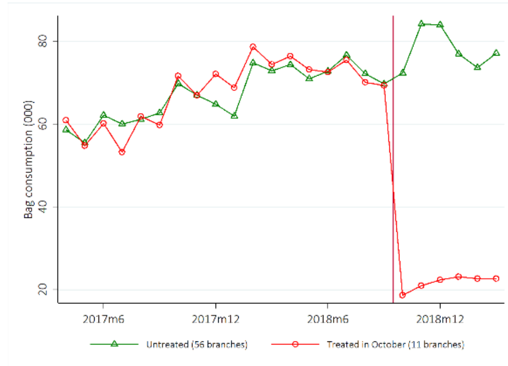
10.6 Figures of the wave-specific effects

Figure A.3 shows a summary of the wave specific effects that we will analyze in detail in the next sections of this appendix. Table 5 in the main text shows the estimation of the drop in consumption plotted in this figure.

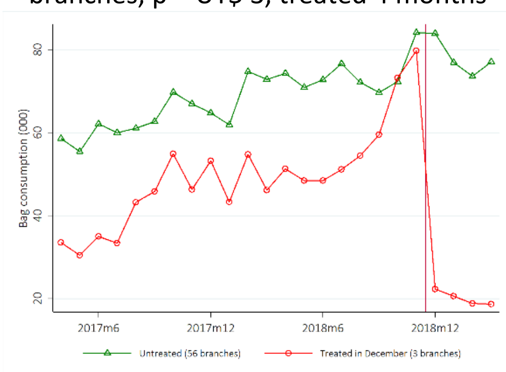
Panel (a) First wave: April 2018 (Salto), 3 branches, p = UY$ 2, treated 12 months



Panel (b) Second wave: October 2018, 11 branches, p = UY$-2, treated 6 months



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



Panel (d) Fourth wave: January 2019, 12 branches, p = UY$ 2, treated 3 months

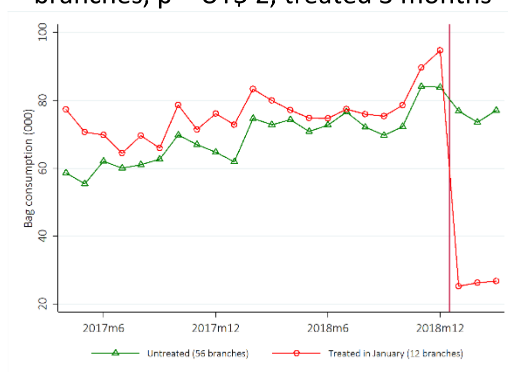


FIGURE A.3: AVERAGE NUMBER OF BAGS USED AT CHECKOUT IN TREATED BRANCHES IN EACH WAVE AND CONTROL BRANCHES, BY MONTH

Notes: The figure shows the wave specific effects, associated with Table 5. The red line (hollow circle) plots the monthly number of bags used at checkout in the average treated branch in the corresponding set and the green line (hollow triangle) plots the monthly number of bags used at checkout in 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 (wave specific). Panel (a) shows the first wave (Salto). In this case, the sample 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.

10.7 Salto (April 2018)

TABLE A.3: AVERAGE NUMBER OF BAGS USED AT 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)***

Notes: The table shows the average number of bags used at each branch (in thousands per month), before and after the price, for branches in Salto and the 56 branches in the rest of the country that did not price the bags during the period. The number of observations for the DiD regression is 1,429. Robust standard errors clustered at the branch level in parenthesis.

TABLE A.4: DIFFERENCE-IN-DIFFERENCE OLS ESTIMATION

	(A)	(B)	(C)	(D)	(E)
	Basic DiD	Branch FE	Month FE	Branch + Month FE	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

Notes: The table shows the OLS estimation of the diff-in-diff coefficient of the average treatment effect. The results are variations of equation (3). 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 time trends. Column (D) presents the results from our preferred specification included in the main text (see column (A) of Table 5). Standard errors (in parenthesis) clustered at the branch level. Outcome variable: average number of bags provided for free/sold by branch, by month. Sample period: 24 months between April 2017 and March 2019. Control group: 56 branches that did not priced the bags in the sample period. * p<0.10, ** p < 0.05, *** p < 0.01

TABLE A.5: ANTICIPATION EFFECTS

	(A) Basic DiD	(B) Branch FE	(C) Month FE	(D) Branch + Month FE	(E) D + Time trends
DiD anticipation effect	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 price	-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

Notes: The table shows difference-in-difference estimates corresponding to equation (3) in the main text. Each column is a different regression. The first coefficient is the difference-in-difference estimate of the effect of the *announcement* of the price (four months before the effective implementation) on the number of consumed bags. The second coefficient is the estimate of the average treatment effect for the *price*. The results that we present in the main text (Table 9) are those of our preferred specification (column D from this Table). Outcome variable: number of bags provided for free/sold by branch, by month. Control group: 56 branches that did not price the plastic bags during the sample period. Standard errors (in parenthesis) clustered at the branch level. * $p<0.10$, ** $p < 0.05$, *** $p < 0.01$

10.8 Second wave – October 2018

TABLE A.6: AVERAGE NUMBER OF BAGS USED AT 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)***

Notes: See comments to Table A.3. In this case, the number of observations for the DiD regression is 1,621.

TABLE A.7: 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

Notes: See comments to Table A.4

TABLE A.8: ANTICIPATION EFFECTS

	(A) Basic DiD	(B) Branch FE	(C) Month FE	(D) Branch + Month FE	(E) D + Time trends
DiD anticipation effect	-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 price	-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

Notes: See comments to Table A.5

10.9 Third wave – December 2018

TABLE A.9: AVERAGE NUMBER OF BAGS USED AT 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)***

Notes: See comments to Table A.3. In this case, the number of observations for the DiD regression is 1,428.

TABLE A.10: 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

Notes: See comments to Table A.4

TABLE A.11: ANTICIPATION EFFECTS

	(A) Basic DiD	(B) Branch FE	(C) Month FE	(D) Branch + Month FE	(E) D + Time trends
DiD anticipation effect	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 price	-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

Notes: See comments to Table A.5

10.10 Fourth wave – January 2019

TABLE A.12: AVERAGE NUMBER OF BAGS USED AT 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)***

Notes: See comments to Table A.3. In this case the number of observations for the DiD regression is 1,644.

TABLE A.13: 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

Notes: See comments to Table A.4

TABLE A.14: ANTICIPATION EFFECTS

	(A) Basic DiD	(B) Branch FE	(C) Month FE	(D) Branch + Month FE	(E) D + Time trends
DiD anticipation effect	-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 price	-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

Notes: See comments to Table A.5

10.11 DiD decomposition

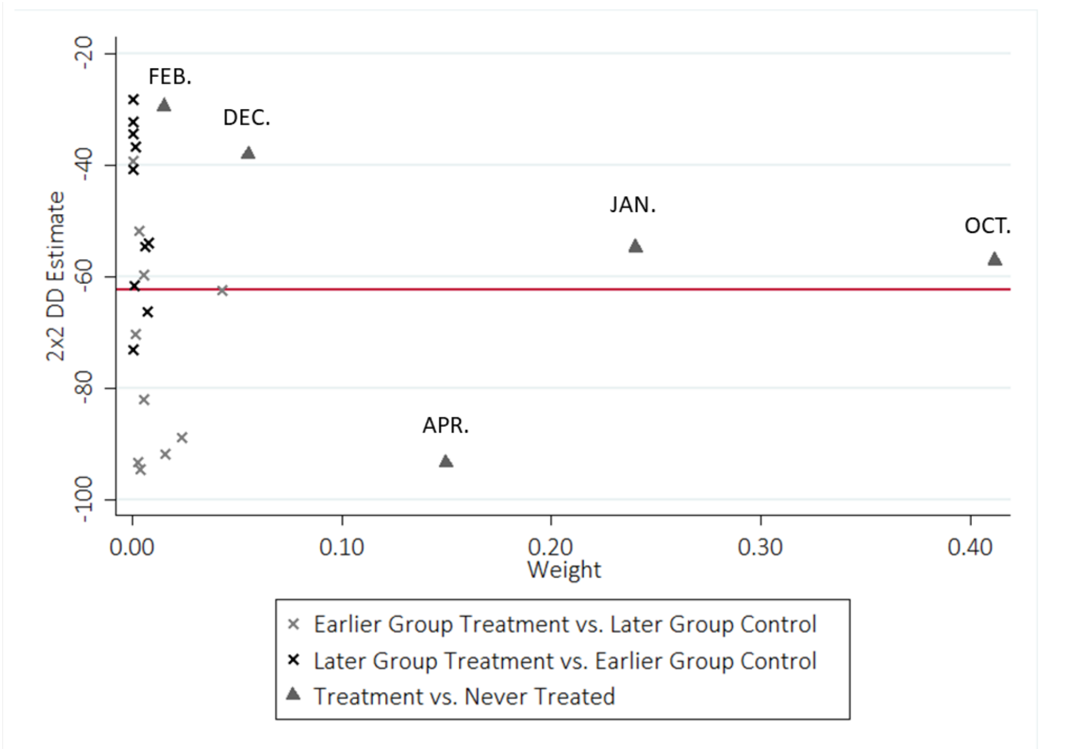


FIGURE A.4: GOODMAN-BACON DECOMPOSITION

Notes: The figure shows the estimates from the Stata *bacondecomp* package (Goodman-Bacon, Goldring, and Nichols 2019). The horizontal red line shows the average difference in difference estimation of the drop in bags consumption after charging. The DiD coefficient is an average of all the 2x2 comparisons. We confirm that the estimate of the average effect of the price for the full experiment is mainly determined by the comparison of each individual wave against the never treated stores. The October wave of the experiment is the one which has the largest weight on the overall DiD estimate. Moreover, “bad comparisons” (i.e. later group treated vs. earlier group controls) have a weight close to zero.

10.12 Results with log(bags) as the dependent variable

We decided to perform the analysis with absolute quantities of bags as the dependent variable, instead of log(bags), because we believe the former gives a more transparent picture of the actual sizes of the effects. An issue with this choice is that the conversion to percentage change we use is a function of the relative size of the treated versus the control branches. In our case, this should not be a major concern. First, the percentage treatment effects are robust to a synthetic control analysis, where the synthetic stores are of the same size of the average treated ones by construction (see Figure 7). Second, the percentage change is similar across waves despite different pre-treatment sizes (see Table 5).[29] Notwithstanding, we conducted the main estimations with log(bags) as the dependent variable and we present the results in the following tables.

Table A.15 presents the pooled effect of the price. We see that when using logs of bags as the dependent variable, the percentage drop in the demand for bags is 69%.

TABLE A.15: POOLED EFFECT OF PRICES ON THE QUANTITY OF BAGS, USING LOG(BAGS) AS DEPENDENT VARIABLE

	(A)	(B)
Treatment effect of pricing	-1.176***	-1.174***
	(0.048)	(0.050)
Percentage change	-69.1%	-69.1%
Controls	NO	YES
N	2,073	2,073

Notes: see notes to Table 3 in the main text. In this case, the dependent variable is the natural logs of bag consumption. The percentage change is calculated as $exp\left(\Delta x\hat{\beta}\right) - 1$, where $\Delta x = 1$ indicates the pricing treatment.

Table A.16 presents the results of the wave specific estimations. When using log(bags) as the outcome variable, the estimated percentage drops in the demand for plastic bags are 72.2% in Salto (first wave), 71% in the cities of the second wave, 59% in the cities of the third wave and 65% in the cities of the fourth wave.

[29] We thank a reviewer for this comment.

TABLE A.16: WAVE SPECIFIC REGRESSIONS RESULTS, USING LOG(BAGS) AS THE DEPENDENT VARIABLE

	(A)	(B)	(C)	(D)
	Salto April 2018	Second wave October 2018	Third wave December 2018	Fourth Wave January 2019
Price	UY$ 2	UY$ 2	UY$ 3	UY$ 2
Average treatment effect of the Price	-1.28***	-1.23***	-0.89***	-1.06***
	(0.05)	(0.06)	(0.16)	(0.09)
Percentage change	-72.2%	-70.8%	-58.9%	65.4%
N	1,427	1,619	1,426	1,642

Notes: see notes to Table 5 in the main text. In this case, the dependent variable is the natural logs of bag consumption. The percentage change is calculated as $exp(\Delta x\hat{\beta}) - 1$, where $\Delta x = 1$ indicates the pricing treatment.

Table A.17 presents the estimation of the price level effects. In this case, putting a price of UY$ 2, on average, decreased the demand for bags 70%, while putting a price of UY$ 3 decreased it 60%.

TABLE A. 17: AVERAGE EFFECT OF PRICES ON THE QUANTITY OF BAGS IN THE FULL EXPERIMENT, USING LOG(BAGS) AS DEPENDENT VARIABLE

	(A)	(B)
Price = 2	-1.201***	-1.200***
	(0.0463)	(0.0486)
Percentage change	-70.0%	-70.0%
Price = 3	-0.912***	-0.905***
	(0.162)	(0.163)
Percentage change	-60.0%	-60.0%
Controls	NO	YES
N	2,073	2,073

Notes: see notes to Table 6 in the main text. In this case, the dependent variable is the natural logs of bag consumption. The percentage change is calculated as $exp(\Delta x\hat{\beta}) - 1$, where $\Delta x = 1$ indicates the pricing treatment.

10.13 Inference using different levels of clustering and wild bootstrap methods

There is no clear consensus what the level of clustering should be. We cluster at the store level, but clustering at the city or double clustering at the city and month levels are valid. Moreover, the clustering level may lead to the small number of cluster problem. In Table A.18 we show the differences in p-values by clustering level and method. The t statistic and the p-value reported correspond to clustering under the large sample assumptions, as in the main regressions of the paper. The following p-values are bootstrapped.

TABLE A.18. BOOTSTRAPED P-VALUES FOR DIFFERENT CLUSTERING LEVELS.
REPLICATION OF TABLE 3.

	(A)	(B)	(C)
Point estimate for P		-61.72	
		clustering level:	
	store	city	city and month
t-statistic	-11.10	-8.82	-7.86
p-value	0.000	0.000	0.000
Bootstraped p-value	0.000	0.000	0.002

Notes: Replication of column (A) from Table 3. In columns A and B we cluster at the store and city level, respectively. In column C we cluster at the city and month level (with months from 1 to 24). The last line presents *wild cluster unrestricted bootstraped* p-values, as implemented by Roodman, MacKinnon, Nielsen and Webber (2019) following Cameron, Gelbach, and Miller (2008).

We conclude from this exercise that the significance level of the pooled effect of pricing plastic bags is not affected by how we perform inference.

Table A.19 shows the results of replicating the diff-in-diff wave-specific regressions in Table 5 with bootstrapped p-values. In this case, we can observe a limited number of minor differences in the p-values, depending on the clustering level and the calculation technique.

TABLE A.19: WAVE-SPECIFIC RESULTS (BOOTSTRAPPED P-VALUES)

	(A) Salto (P=2) April 2018			(B) Second wave (P=2) October 2018		
Estimate for P	-93.52			-57.20		
	clustering level:			clustering level:		
	store	city	city and month	store	city	city and month
t-statistic	-12.57	-112.38	-32.12	-8.10	-12.00	-9.67
p-value	0.000	0.000	0.000	0.000	0.000	0.000
Bootstraped p-value	0.000	0.000	0.000	0.000	0.000	0.002
N	1,429			1,621		

	(C) Third wave (P=3) December 2018			(D) Fourth Wave (P=2) January 2019		
Estimate for P	-40.45			-53.98		
	clustering level:			clustering level:		
	store	city	city and month	store	city	city and month
t-statistic	-5.71	-4.84	-4.96	-6.32	-7.92	-7.54
p-value	0.000	0.000	0.000	0.000	0.000	0.000
Bootstraped p-value	0.000	0.000	0.040	0.000	0.000	0.008
N	1,428			1,644		

Notes: Replication of Table 5. The last line of each panel presents *wild cluster unrestricted bootstraped* p-values, as implemented by Roodman, MacKinnon, Nielsen and Webber (2019) following Cameron, Gelbach, and Miller (2008).

Table A.20 shows the results of replicating the diff-in-diff price level regression with bootstrapped p-values (Table 6 in the manuscript). In this case, bootstrapping the p-values does not alter the statistical significance of the P=2 coefficient. In the case of the P=3 coefficient, we observe a difference in the p-value when we cluster at the city level.

TABLE A.20. BOOTSTRAPED P-VALUES FOR DIFFERENT CLUSTERING LEVELS.
REPLICATION OF TABLE 6.

Estimate for P = 2	-63.59		
	clustering level:		
	store	city	city and month
t-statistic	-10.89	-8.97	-8.04
p-value	0.000	0.000	0.000
Bootstraped p-value	0.000	0.000	0.002
Estimate for P = 3	-41.97		
	clustering level:		
	store	city	city and month
t-statistic	-5.97	-4.98	-5.08
p-value	0.000	0.000	0.000
Bootstraped p-value	0.000	0.076	0.055
N	2,075		

Notes: The last line of each panel presents *wild cluster unrestricted bootstraped* p-values, as implemented by Roodman, MacKinnon, Nielsen and Webber (2019) following Cameron, Gelbach, and Miller (2008).

We conclude that neither the clustering level, nor the small sample bootstrap refinement affect our main results in a significant manner.

Finally, we present the replication of the analysis presented in Table 11 when using wild cluster bootstrap. Results are robust to using the refinements for small number of clusters: sales do not change after charging for bags, the demand for bags decreases sharply, as it does the number of bags used per dollar of sales.

TABLE A.21. BOOTSTRAPED P-VALUES FOR DIFFERENT CLUSTERING LEVELS.
REPLICATION OF EFFECT ON SALES (TABLE 11).

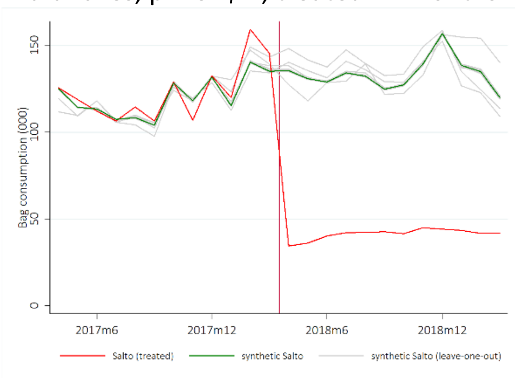
	(A) Sales			(B) Bags			(C) Bags/Sales		
Estimate for P	-4.41			-104.73			-0.104		
	clustering level:			clustering level:			clustering level:		
	store	City	city and month	store	city	city and month	store	city	city and month
t-statistic	-1.54	-1.70	-0.91	-11.60	-223.12	-9.39	-19.67	-58.47	-11.48
p-value	0.197	0.232	0.458	0.000	0.000	0.011	0.000	0.000	0.008
Bootstraped p-value	0.313	0.500	0.361	0.000	0.500	0.001	0.000	0.500	0.001
N	120			120			120		

Notes: Replication of the Table 11. In columns A and B we cluster at the store or city level, respectively. The last line presents *wild cluster unrestricted bootstraped* p-values, as implemented by Rootman, MacKinnon, Nielsen and Webber (2019) following Cameron, Gelbach, and Miller (2008).

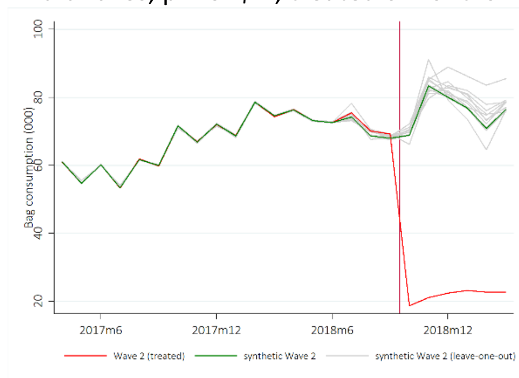
Note that the bootstrapped p-value is always exactly 0.500. The reason is that the algorithm is unable to work properly when clustering at the city level with only two cities. The bootstrapped p-value distribution degenerates in only two mass points.

10.14 Leave-one-out synthetic controls

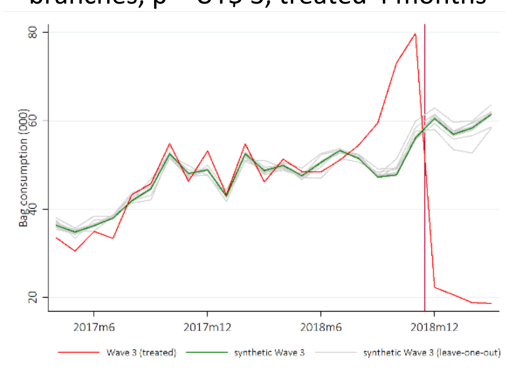
Panel (a) First wave: April 2018 (Salto), 3 branches, p = UY$ 2, treated 12 months



Panel (b) Second wave: October 2018, 11 branches, p = UY$ 2, treated 6 months



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



Panel (d) Fourth wave: January 2019, 12 branches, p = UY$ 2, treated 3 months

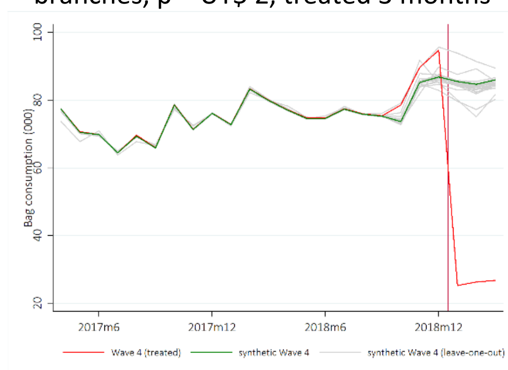


FIGURE A.5: AVERAGE NUMBER OF BAGS USED AT TREATED BRANCHES IN EACH WAVE (RED LINE) ITS SYNTHETIC CONTROL (GREEN LINE), AND THE LEAVE-ONE-OUT SYNTHETICS (GREY LINES), BY MONTH

Notes: These figures show the robustness of the synthetic control to leaving out from the donor pool one branch at a time. These synthetic controls are plotted in grey, while the synthetic control constructed with all the observations is plotted in green and corresponds to the synthetic of the figures in the main text. Treated branches averages in each wave are plotted in red. The y-axis shows average monthly bags consumption per store, in thousands.