The Lasting Effect of a Pledge: a Field Experiment on Soft Commitment and Waste Sorting *

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Abstract

Taking action on climate change requires that citizens participate in pro-environmental activities. Behavioral mechanisms can help to promote such activities. Through an RCT (N=1.519) that uses a novel technology to record real-time data on waste sorting, we find that offering the opportunity to sign a pledge increases the effectiveness of a pro-environmental campaign designed to encourage waste sorting. With a timespan of over four years, the pledge increased waste sorting participation by 4.55-5.10 percentage points (sd=0.1997). The effect is greater immediately after the campaign (around 9-10 percentage points during the first 15 weeks), but it remains sizable and statistically significant 150-210 weeks after signing (3.11-4.45 percentage points). These findings show that light-touch nudges can result in meaningful and long-lasting improvements in the effectiveness of pro-environmental campaigns.

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1 Introduction

Recycling is an important policy for reducing CO_2 emissions and mitigating climate change (Hennessy et al., 2022). Citizen engagement in pro-environmental activities is essential for environmental policies like recycling, saving energy, or reducing water consumption. Companies, governments, and NGOs invest significant resources in campaigns to promote pro-environmental behavior.[1] We study whether being given the opportunity to sign a soft commitment, i.e., a non-binding pledge to recycle (Bryan et al., 2010), can increase the effectiveness of an environmental campaign aimed at encouraging waste sorting.

Traditionally, waste sorting in urban areas has been anonymous. However, new technologies make its monitoring easier and more feasible. At the end of 2018 and beginning of 2019, electronic bio-waste bins were installed in the municipality of Palma, Spain. These bins were locked, and a personal card had to be scanned to open the lid. This feature provided novel real-time data on individual participation in waste sorting. We take advantage of this technology to run a randomized control trial (RCT) evaluating the effectiveness and dynamics of soft commitments with regard to fostering recycling.

For our experiment we partnered with the company in charge of waste management in the municipality. The company was running an environmental campaign that we used to perform the experiment. The campaign was conducted by a group of environmental educators who informed and encouraged citizens to sort bio-waste in various neighborhoods. The educators invited citizens to participate in our study. A random sub-sample of the participants were given the opportunity to sign a soft commitment, a form where participants pledged to sort their bio-waste. Citizens that were given the option to sign the pledge make up our *treatment* group, regardless of whether they actually signed it. Our *control* group is the participating households that were not given the opportunity to sign the pledge. This setting offers us the possibility to neatly estimate the ability of the soft commitment to improve the effectiveness of the pro-environmental campaign.

Our total sample included 1,519 households (46.71% in the control group and 53.29% in the treatment group). The two groups saw a sharp increase in recycling immediately after the campaign. While control and treatment groups behaved identically during the pre-intervention period, the increased participation in waste sorting that followed the campaign was greater in the group that was given the opportunity to sign the pledge. Averaging over a long period (210 weeks after the campaign) we show that the pledge increased participation in waste sorting by 5.10 percentage points (0.1997 sd). In relative terms, the effect represents a 30% increase compared to the control group. The effect is greater immediately after the campaign (around 9-10 percentage points during the first 15 weeks), but it remains sizable and statistically significant 150-210 weeks after (4.49 percentage points).

[1]For example, the 2022 United States Environmental Protection Agency Budget includes an investment of $10.2 million in resource conservation for waste minimization and recycling programs as well as $8.6 million in environmental education for citizens. Available at https://www.epa.gov/sites/default/files/2021-05/documents/fy-2022-epa-bib.pdf.

The use of voluntary commitments to shape individual behavior has recently gained interest in economics and management (see Bryan et al., 2010, for a review). Commitments involving a self-imposed penalty for not complying have proven useful when it comes to helping individuals overcome self-control problems in several contexts, e.g., increasing their savings (Thaler and Benartzi, 2004; John, 2020), going to the gym (Royer et al., 2015), quitting smoking (Giné et al., 2010), reducing alcohol consumption (Schilbach, 2019), and treating digital addiction (Allcott et al., 2022).[2] When voluntary commitments do not carry a penalty for non-compliance, they are labeled as *soft commitments*, which generally involve the signing of a pledge to embrace the desired behavior. Despite their non-binding nature, soft commitments can modify conduct through psychological mechanisms, such as the desire for consistency (Cialdini et al., 1995; Cialdini, 2009), cognitive dissonance (Festinger, 1957), guilt aversion (Charness and Dufwenberg, 2006), or individuals' preferences to keep promises (Vanberg, 2008). If these behavioral mechanisms work, the lack of a penalty can be seen as an advantage because it leads to higher take-up. Soft commitments have been found to work in laboratory experiments (Ellingsen and Johannesson, 2004; Charness and Dufwenberg, 2006; Vanberg, 2008; Koessler, 2022), yet results in the field seem to depend on context and design. For instance, while Ashraf et al. (2006) and Himmler et al. (2019) found positive effects, respectively, in improving savings and student effort, Abaluck et al. (2021) found null results when it came to promoting mask wearing during the COVID-19 crisis in Bangladesh. When it comes to pro-environmental behavior, pledges are a very popular mechanism. For instance, the European Commission specifically encourages pledges and public commitments in its European Climate Pact (`https://climate-pact.europa.eu/about/about-pact_en`).[3] Despite their pervasiveness, evidence on their effectiveness is mixed. They have been found to encourage towel reuse in hotels (Baca-Motes et al., 2013) and promote public transportation (Matthies et al., 2006) but not to save shower water (Dickerson et al., 1992). Pallak and Cummings (1976) and Pallak et al. (1980) found that they were effective at reducing energy consumption only if made public. Due to the difficulty in monitoring waste sorting, evidence on the effects of soft commitments in these cases is restricted to small scale studies and to very specific contexts, like single-family homes (Pardini and Katzev, 1983; Burn and Oskamp, 1986; Katzev and Pardini, 1987; Cobern et al., 1995; Werner et al., 1995; Bryce et al., 1997), student residences (Wang and Katzev, 1990; Dupré, 2014), on-campus college housing (De Leon and Fuqua, 1995), complexes with collective bins (De Young et al., 1995), and retirement homes (Wang and Katzev, 1990). Furthermore, studies mainly focus on short-run effects. This difficulty in monitoring waste sorting also limits sample size, which, together with sample specificity, might explain the overall ambiguity of results across previous studies.[4] Pardini and Katzev (1983); Burn and Oskamp (1986); Katzev

[2]An entire industry based on voluntary commitments has emerged following this line of literature. See, for instance, https://www.gym-pact.com/.

[3]Other examples include Palau's Pledge `https://www.palaupledge.com/`, the California Clean Air Pledge `https://www.cleanairday.org/pledge/individual/`, The American Lung Association Clean Air Pledge `https://www.lung.org/clean-air/stand-up-for-clean-air/pledge`, and The Zero Global Waste Pledge `https://www.zeroglobalwaste.com/environmental-pledge.`

[4]Our sample size is N=1,519. The sample sizes of previous studies are N=27 for Pardini and Katzev (1983), N=194 (only commitment) and N=139 (commitment and message) for Burn and Oskamp (1986), N=30 for

and Pardini (1987); Wang and Katzev (1990) found positive results, while Cobern et al. (1995); De Leon and Fuqua (1995); Werner et al. (1995); De Young et al. (1995); Bryce et al. (1997) obtained null results.[5] Our study is a pioneer in its evaluation of the effect of soft commitments on a large scale, in a densely populated urban area, and over a long time horizon.[6]

Our paper primarily contributes to nudging literature (Thaler and Sunstein, 2009) by applying nudging to civic behavior (John et al., 2009). We show that a light-touch intervention fosters pro-environmental behavior and has long-lasting effects. This finding is consistent with the meta-analyses by Hummel and Maedche (2019) and DellaVigna and Linos (2022), who observed that nudges are especially effective for environmental policy. However, our work complements their findings by providing evidence on soft commitments, a mechanism that was not present in their meta-analyses. We also provide evidence on the usefulness of behavioral mechanisms in combating climate change (Allcott and Mullainathan, 2010). These types of mechanisms can be especially relevant in pro-social contexts, where monetary incentives might not work and may even backfire. (Gneezy and Rustichini, 2000; Bowles, 2008; Gneezy et al., 2011; Bowles and Polania-Reyes, 2012).

Our study also contributes to the recent discussion about the persistence of behavioral interventions over time (Frey and Rogers, 2014; Brandon et al., 2022). Field experiments often only evaluate the outcomes at one point in time or shortly after implementation. By contrast, we can evaluate the effects of a soft commitment over time. The granularity of our data also allows us to study dynamic effects on a weekly basis. By doing so, we observe an "action and backsliding" pattern like the one documented in studies by Allcott and Rogers (2014), Barrera-Osorio et al. (2020), and Gallagher (2014). This pattern is characterized by an initial surge in the promoted behavior, followed by a subsequent reduction over time. However, in our case, the backsliding stopped after around 10-20 weeks, and it does not cancel out the initial effect. Indeed, the effect stabilizes and persists for the rest of the study period (210 weeks). Such persistence contrasts with the short-term effects often seen after behavioral interventions (see, for instance, Shang and Croson (2009), Apesteguia et al. (2013), Coppock and Green (2016), Hallsworth et al. (2017), and the reviews in Brandon et al. (2022) and Hummel and Maedche (2019)). In line with Kahneman (2011) and Byrne et al. (2022), time-of-day transition matrices suggest that habit formation is important for recycling, which could explain the persistence of the effect.

This paper is organized as follows. Sections 2 and 3 describe the experimental design and the data. Section 4 details the identification strategy and presents the main results, while Section 5 explores the robustness of the findings together with further results. Section 6 concludes.

Katzev and Pardini (1987), N=17 (experiment 1) and N=28 (experiment 2) for Wang and Katzev (1990), N=80 for Cobern et al. (1995), N=38 for De Leon and Fuqua (1995), N=105 for Werner et al. (1995), N=72 for De Young et al. (1995), N=203 for Bryce et al. (1997), and N=38 for Dupré (2014). These numbers do not include experimental conditions other than control and commitment.

[5] We consider only the cases where commitments were not combined with other mechanisms.

[6] Previous research has considered the effect of soft commitments on recycling for periods ranging from three weeks to four months after being presented with the commitment (see the meta-analyses by Lokhorst et al., 2013 and Varotto and Spagnolli, 2017 for an overview).

2 Experimental Design

Our experiment was conducted within an existing pro-environmental campaign. The campaign was run by a team of environmental educators who informed citizens about the introduction of bio-waste separation. Throughout the paper we will generally use the term *intervention* to refer to the whole set of actions taken by the environmental educators, including both the pro-environmental campaign and running the experiment.

2.1 The Setting

With around 420,000 citizens, Palma is the largest city in the Balearic Islands, Spain. A municipally owned company called EMAYA is in charge of managing and collecting urban waste. In 2018, EMAYA started a program to introduce bio-waste recycling. Initially, the bins were only brought into some specific areas of the city, and bin installation took place in two stages, the first starting in November 2018, and the second in March 2019.

To avoid improper sorting, which would compromise the processing of bio-waste, the installed bins were locked, and the lid could only be opened by scanning a personal card (residents' city transportation cards).[7] Importantly, every time that a bin is used, it records the number of the scanned card. This provides real-time individualized data on waste sorting.

2.2 Implementation

In the context of an informational campaign on waste sorting, a team of environmental educators set up information points at different locations around the area in which the program was implemented. Banners were used to gain visibility, and gifts were offered to citizens to encourage them to approach the educators.

The interaction between educators and citizens took place as follows. First, environmental educators informed citizens about the introduction of bio-waste separation, responded to citizens' questions about the process, and encouraged them to recycle. Afterwards, citizens received a recycling kit, which included a small bin and recycling bags. Finally, citizens were given the option to participate in our study by signing an informed consent release (see Figures A1 and A3 in Appendix A).

If the citizens (hereafter referred to as "participants") agreed to participate in the study, they signed an informed consent. Afterwards, they were asked about the households' number of weekly disposals of non-recyclable waste. Based on that answer, educators informed them about the expected number of disposals if bio-waste was regularly sorted (MENUCO: Minimum Expected Number of Uses of the COntainer).[8]

[7] Most of the residents in the city have the card, as it is free and its use reduces fares on public transport. Households without any card-holding members, were excluded from the study because they could not be matched with administrative data (see Section 3 below).

[8] Specifically, the MENUCO was set equal to 1 if the number of weekly disposals of non-recyclable waste were 1 or 2, set equal to 2 if there were 3 or 4, and was set to 3 if there were 5 or more such disposals. The numbers were agreed upon with the waste management company on the basis that roughly half of non-recyclable waste corresponds to bio-waste.

For participants assigned to the control group, the interaction ended here. Participants in the treatment group, however, were given the option to sign a pledge, which consisted of a document in which signers would commit their households to recycling bio-waste. This document informed participants that a household was considered to recycle in a given week if it complied with the MENUCO (see Figures A2 and A4 in Appendix A).

Note that, as usual in the evaluation of soft commitments, our treatment is defined as being offered to sign the soft commitment, rather than actually signing it.[9] Doing so not only prevents selection bias, but it also provides the most appropriate analysis from a policy-making perspective.

Participants were allocated to one experimental condition or the other in alternating order of arrival (zipper strategy). That is, if one participant was allocated to the control group, the next was allocated to the treatment group, and so on.[10] Despite sporadic disruptions in the zipper randomization process, recruitment sheets were designed so that environmental educators could visually verify that they were complying with the randomization strategy.[11]

In February 2020, between 12 and 54 weeks after the commitment campaign, we ran a correspondence experiment to provide feedback on recycling performance. The randomization process followed a 2x2 design with a soft commitment. Appendix E provides details on this other intervention.

3 Data

To conduct the study, we used three different types of data: field data, administrative data, and bio-waste disposal data.

3.1 Field Data

During the campaign the educators collected the following information: the participant's name and surname, national identification number, number of members in the household, MENUCO, treatment assignment, and address.[12] We used the latter variable for retrieving income data from the Spanish census.[13]

Field data was manually collected and handwritten on the informed consent sheet (Figure A3 in Appendix A), on the soft commitment sheet (Figure A4 in Appendix A), and in the

[9]Equivalently, one can consider that we are estimating an intended-to-treat effect. The acceptance rate of the soft commitment was 96.54%, which makes the effect on those who were given the opportunity to sign the commitment and those who actually signed the commitment identical.

[10]To avoid spillover effects, when a group of citizens approached the information point as a group (which was infrequent), they were all assigned to the same experimental condition.

[11]In particular, the recruitment sheets introduced visual elements (bold lines) to help recruiters verify that for every 10 participants enrolled in the experiment, 5 were assigned to the treatment group. See Figures A3 and A4 in Appendix A.

[12]National identification numbers were initially only collected from the treatment group when signing the soft commitment. However, this information was later requested from the control group in order to improve matching between field data and administrative records.

[13]Such data is publicly available from the Spanish National Statistics Bureau. Average income is provided for areas of 1,000-2,500 residents, making them quite accurate.

educators' log files. Environmental educators recruited 1,878 households for the experiment from January 28 to November 18, 2019.

3.2 Administrative Data

Waste is generated at the household level. However, electronic bins are able to provide data on the individual level, via card scanning. We sent the list of participants to the local body responsible for issuing and managing the cards. They returned to us anonymized data containing the card numbers of the participants and their cohabitants. This procedure allowed us to aggregate bio-waste disposal at the household level.

We used postal addresses (street and number), national identification numbers, and complete names to match participants' data to their transportation card numbers. The matching process was successful for 80,88% of the participants recruited initially (unmatched cases might be households with no members holding a card or inaccuracies in handwritten field data). This process yields a sample size of 1,519 households, divided into 709 (46.68%) in the control group and 810 (53.32%) in the treatment group.

The imbalance in the size of the two groups comes from the start of the recruitment process, when national ID numbers (the most effective matching variable) were only requested from the participants in the treatment group.[14] This difference in sample sizes during the first weeks of recruitment implies that the average length of the period for which we can observe pre-intervention outcomes for the control group (17.24 weeks) and the treatment group (15.26 weeks) differs significantly (p-value<0.01).[15] As the reason for the imbalance was exogenous and only related to the effectiveness of the matching process, it should not affect the estimation of the treatment effect, only its precision. Nevertheless, in Section C.2 in the Appendix, we exhaustively analyze the implications of this imbalance and provide clear evidence that it does not affect the results. Among other arguments, we show that our results would not change if we corrected the imbalance by repeating the matching process after omitting the national IDs of the treated group in the same period for which this information was not available for the control group.

3.3 Bio-Waste Data

The following information was recorded during each use of electronic bins: a user identifier (anonymized), a bin identifier, the date, the time when the card was scanned, the time when the lid was opened, and the time when the lid was closed.

We consider a period of 210 weeks after the campaign. Given the staggered recruitment process, this period meant different calendar dates based on when participants were recruited. Bins installation was completed in two phases. The first one finished on November 17, 2018, and

[14] Figure B2 in the Appendix shows that the imbalance came from the first ten weeks of recruitment.

[15] Since for many of the participants' electronic bins were installed just a few weeks before recruitment, the sooner a household was recruited, the fewer weeks we can observe pre-intervention outcomes. Hence, the bigger sample size of the treatment group at the beginning of recruitment mechanically results in a shorter average period of available data before recruitment.

the second on March 24, 2019. Most participants (1,179) were recruited after the installation of the electronic bins in their neighborhood (77.62% of the sample), which means we can observe their bin usage before being recruited.

3.4 Outcome Variables and Descriptive Statistics

A novelty of our study is that we can track waste sorting at an individual level in densely populated areas. However, our data does not provide information on the actual amount of waste disposed; it provides only information on bin usage. To construct the outcome variables, we consider bin usage to be a proxy for recycling behavior. We find this step to be legitimate after cross-checking data on lid openings and the aggregate amount of bio-waste that was collected. The two measures follow a very similar pattern over time and have a correlation above 0.95 (see Figure B1 in Appendix B).[16]

Three outcome variables are constructed at the week-household level using bin registries: *#Uses*, which aggregates the weekly number of lid openings; *DoF* which divides *#Uses* over the MENUCO to measure the degree to which households fulfilled their commitment (with truncation at 1 denoting full compliance); and *%Weeks*, which considers weekly participation in waste sorting (i.e., a dummy variable taking value one every week that there is a disposal).[17] The latter captures regular participation in waste sorting, and we argue it is the most relevant and accurate outcome we can obtain with the available data. First, participation measures are more convenient from an environmental perspective because preventing waste generation (precycling) is preferable to recycling. Cardinal measures like *#Uses* and *DoF* do not depend solely on recycling but also on waste generation (e.g., households with more leftovers make more disposals, which is not environmentally better). Secondly, one of the limitations of our data (which measures lid openings but not the amount of waste disposed) makes the measure of participation a more reliable outcome than measures based on the number of uses. Weekly participation is hardly affected by household heterogeneity in waste practices. By contrast, since the amount of waste is not observable, cardinal measures are likely to be affected by such heterogeneity. For instance, some households might make frequent small disposals while others might make less frequent larger disposals but recycle the same. Participation is also less affected by seasonality (more weekly disposals during warmer periods) and household size. Finally, *DoF* has an additional source of inaccuracy as it is based on a self-report. All in all, we use *%Weeks* to display results in the main text. The results are similar when we consider *#Uses* and *DoF* as outcomes (in the Appendix).

[16]There are two potential reasons why our proxy for disposals might differ from actual bio-waste disposals: i) disposing waste other than bio-waste, and ii) scanning the card without disposing anything. The first possibility can be rejected as systematic analyses conducted by the waste management company revealed a contamination level below 10 % (0.5% in 2019 and 9.12% in 2020). The second possibility is also unlikely, as citizens have no incentive to scan their card without making a disposal. Moreover, we do not use card scans but opening the lid to account for disposals. Citizens were not aware that card registries and lid openings could be distinguished, which makes it unlikely that this affects the quality of our data.

[17] To construct the variable *#Uses*, all registries made by the same household within less than 12 hours were considered a single disposal.

Table 1: Descriptive Statistics, Balancing Tests and Pre-Intervention Outcomes

	N	Control	Treatment	Diff	p-value
	(1)	(2)	(3)	(4)	(5)
Panel A. Descriptive statistics and Balancing Tests					
Size	1,505	2.874	2.891	0.018	0.7858
		(0.0485)	(0.0442)	(0.0656)	
MENUCO	1,493	1.854	1.797	-0.057	0.1992
		(0.0332)	(0.0301)	(0.0448)	
%Phase=2	1,519	0.423	0.464	0.041	0.1083
		(0.0186)	(0.0175)	(0.0255)	
Income	1,519	1708.25	1725.95	17.70	0.2808
		(12.15)	(11.07)	(16.41)	
Panel B: Pre-Intervention Outcomes					
%Weeks	1,179	0.081	0.082	0.001	0.948
		(0.0095)	(0.0092)	(0.0133)	
#Uses	1,179	0.174	0.167	-0.006	0.850
		(0.0243)	(0.0228)	(0.0333)	
%Inact. Users	1,179	0.816	0.819	0.003	0.894
		(0.0164)	(0.0155)	(0.0225)	
DoF	1,160	0.068	0.066	-0.002	0.852
		(0.00862)	(0.00783)	(0.0116)	

Notes: Column (1) displays the number of households for which we observe each variable. Columns (2) and (3), show the averages for control and treatment households, respectively. Columns (4) shows the difference in means and column (5) its corresponding p-value for the t-test of equal means. *Size* refers to the self-reported number of people living in the household. *MENUCO* refers to the Minimum Expected Number of Uses of the Container per week. *%Phase=2* shows the proportion of households living in the areas where bins were installed later (second phase). *Income* is a proxy for household income imputed from using the median income at the census tract level. For Panel B, *%Weeks* refers to the proportion of weeks the container is used at least once. *#Uses* refers to the weekly average number of uses of the container. *%Inact. Users* is the percentage of households that never used the container. *DoF* is the degree of fulfillment computed as *#Use/MENUCO* (truncated at 1). Standard errors in parentheses.

Panel A in Table 1 shows the descriptive statistics and balancing tests.[18] The average household in our study includes 2.9 people (sd=1.268), has a MENUCO of 1.8 (sd=0.861), and an average median income of €1717.68. The control and treatment groups are balanced in terms of their observable characteristics. Panel B in Table 1 compares the outcomes of the two experimental groups during the pre-intervention period. The table shows the three outcomes described above (*%Weeks*, *#Uses*, and *DoF*) as well as the fraction of households that never used the bins (*%Inact. Users*). As expected from the random assignment of the treatment, the two groups show identical outcomes before passing by the information desk, and if anything, the point estimates tend to favor the control group.

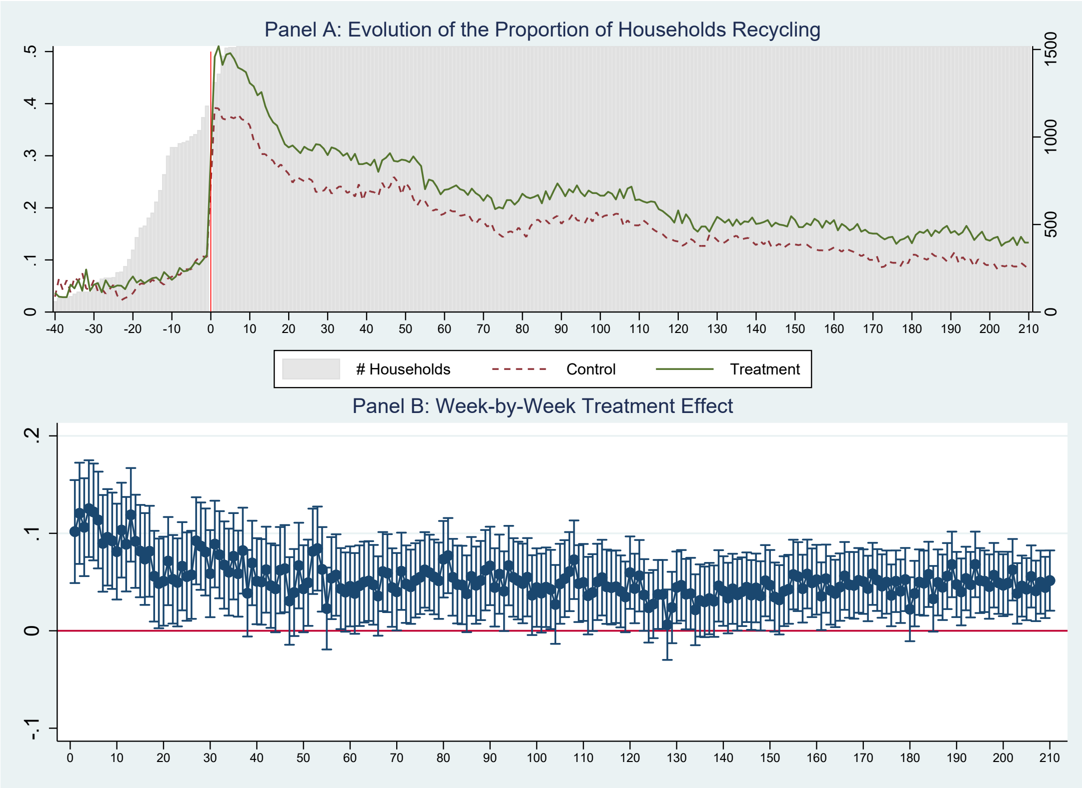
4 Results

Our study considers the long-run effects of offering to sign a pledge. We take into account the maximum available period for all participating households, which covers the 210 weeks after

[18] Sample sizes change slightly for the self-reported variables due to the existence of missing values. Overall, there are 1,483 households (685 corresponding to the control group) with all controls available.

being recruited. Panel A in Figure 1 plots the evolution of the proportion of households that recycled each week during the study period separately for the control and treatment groups. The horizontal axis shows the number of weeks before and after the information campaign (i.e., recruitment). Thus, $t = 0$ corresponds to the intervention week, $t < 0$ to pre-intervention weeks (before recruitment), and $t > 0$ to post-intervention weeks (after recruitment).

Figure 1: Evolution of recycling behavior in the control and treatment groups



Notes: Evolution of recycling behavior in the control and treatment groups (Panel A) and weekly average treatment effects (Panel B) up to 210 weeks after the intervention. The x-axis shows the number of weeks from the intervention, with negative values being the pre-intervention weeks and positive values the post-intervention weeks. In Panel A, lines represent the percentage of households recycling in the control and treatment groups, and the bars show sample size (right axis). In Panel B, the treatment effect for week $j \in \{1, ..., 210\}$ after passing by the table is estimated as $\beta_1 + \beta_3^j$ from the regression $y_{it} = \beta_0 + \beta_1 SC_i + \sum_{j=1}^{210} \beta_2^j WeeksAfter_{it}^j + \sum_{j=1}^{210} \beta_3^j SC_i \times WeeksAfter_{it}^j + \theta X_i + \epsilon_{it}$, where $WeeksAfter_{it}^j$ is a dummy variable taking value one when the weeks elapsed since the treatment for household i at period t is equal to j. Standard errors are clustered at the household level. X_i includes household characteristics (MENUCO, number of inhabitants, bin installation phase, and household income, as well as fixed effects for household postal code and recruitment week).

The figure shows a sharp increase in waste sorting at $t = 0$, which indicates the important effect of the environmental campaign. Before being informed, less than 10% of households were recycling, but immediately after the campaign, close to 40% of households in the control group did so, and nearly 50% in the treatment group. This increase was sharp, and it clearly emerged at the intervention week, showing that the information campaign was responsible for the surge in recycling. Secondly, from $t = 0$ onward, we see a positive gap between the treatment and control groups. This gap represents the ability of the soft commitment to improve the effectiveness of the environmental campaign. Thirdly, the effect of the campaign steadily declined over time for both groups. By contrast, the gap remains quite stable, suggesting that the dynamic effect of the pledge persisted 210 weeks after having been offered the soft commitment.

We confirm the stability of the effect after almost four years by estimating the week-by-week treatment effect in Panel B in Figure 1. The confidence intervals reveal that the effect remains statistically significant for most weeks, and the figure confirms that a light-touch mechanism (being given the opportunity to sign a pledge) is highly persistent, with an effect size of around 4.5 percentage points. We see another remarkable pattern in Figure 1: despite the effect being persistent over time, it seems to backslide until around week 15-20, after which it stabilizes. The econometric analysis in the two subsections below analytically confirms the insights from Figure 1.

4.1 Average Treatment Effect

We follow Bertrand et al. (2004) to estimate the average treatment effect (ATE) and average the outcome variable over the 210 weeks. Consequently, the outcome variable no longer captures weekly participation in waste sorting but the proportion of weeks doing so. The benchmark estimates for the effect of the pledge are obtained through an OLS estimation of the following equation:

$$y_i = \beta_0 + \beta_1 SC_i + \theta X_i + \epsilon_i \tag{1}$$

where SC_i is an indicator taking value 1 if household i was given the opportunity to sign a soft commitment and zero otherwise; X_i is a set of household-specific controls (MENUCO, number of household members, and household income) and fixed effects (postal code and recruitment week); and ϵ_i is an error term.[19] For households recruited after bin installation, we also add a specification in which we control for their waste sorting before being recruited (i.e., the pre-recruitment value of the dependent variable). The main coefficient of interest is β_1, which captures the average treatment effect across the 210-week period of offering the soft commitment on y_i. Considering the fractional or count nature of our outcomes, the residuals do not follow a normal distribution. Thus, the usual Huber-Eicker-White sandwich correction for standard errors was applied.

Columns (1)-(3) in Table 2 show that, on average, during the 210-week period pledges increased the proportion of households that sorted their waste by around 5 percentage points (0.1997 standard deviations). Considering the proportion of households that recycled in the control group (17.3%, the intercept in column (1)), the effect represents a 30% increase. This number can be read as the increase in the effectiveness of the pro-environmental campaign that is obtained by giving citizens the chance to sign a pledge. column (2) adds controls for household characteristics and fixed effects (recruitment week, postal code, and bin installation phase), while column (3) includes the pre-intervention value of the dependent variable as a control (for the sub-sample with this data available). As expected with random assignment, the

[19] Recruitment-week fixed effects not only control for the staggered recruitment, but they also solve any concern that might arise from the imbalance in sample sizes stemming from the matching of field data and administrative records (see Section 3.2). Recruitment week fixed effects make sure that our estimates compare treated and control households that were recruited in the same week; thus, with the same length of the pre-intervention period.

Table 2: Average Treatment Effect and Long-Lasting Effects of the Soft Commitment

	ATE			Dynamic Effects	
	(1)	(2)	(3)	(4)	(5)
SC	0.0534***	0.0510***	0.0455***	0.0449***	0.0311**
	(0.0136)	(0.0139)	(0.0151)	(0.0138)	(0.0153)
Pre-int.			0.373***		0.414***
			(0.0405)		(0.0368)
SCx15 weeks				0.0563***	0.0621***
				(0.0196)	(0.0219)
SCx 16-50 weeks				0.0152	0.0284
				(0.0161)	(0.0176)
SCx51-100 weeks				0.00503	0.0178
				(0.0132)	(0.0145)
SCx101-150 weeks				-0.00663	0.00577
				(0.00925)	(0.0105)
15 weeks				0.250***	0.235***
				(0.0143)	(0.0157)
15-50 weeks				0.141***	0.127***
				(0.0117)	(0.0123)
51-100 weeks				0.0767***	0.0666***
				(0.00904)	(0.00967)
101-150 weeks				0.0431***	0.0411***
				(0.00662)	(0.00731)
Constant	0.173***	0.0413	0.140	-0.0340	0.0786
	(0.00948)	(0.0908)	(0.122)	(0.0867)	(0.115)
N	1519	1483	1153	7415	5765
Adj. R^2	0.00928	0.0366	0.130	0.128	0.196
Controls	No	Yes	Yes	Yes	Yes
FE	No	Yes	Yes	Yes	Yes

Notes: Estimation of equation (1) in columns (1)-(3) and of equation (2) in columns (4) and (5) with the proportion of weeks recycling after the intervention as the dependent variable. Columns (2)-(5) include ZIP-code and Intervention week fixed effects (FE) and household controls. Household controls include the number of people living in the household (*Size*), the Minimum Expected Number of Uses of the Container per week (*MENUCO*), the bin installation phase (*Phase*) and household income (imputed from the census tract, *Income*). *Pre-Int* refers to the average value of the outcome variable up to 40 weeks before the intervention (only available for a sub-sample). Robust standard errors in parentheses (clustered at household level for columns (4) and (5)).* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

change in the coefficient of interest between columns (1)-(3) is negligible.

To complement the analysis, we separately estimate the intensive and extensive margins of the soft commitment. Using a two-part model (Cragg, 1971), we show that the effect is mainly driven by an increase in the number of households that started to recycle (extensive margin) rather than by achieving more adherence among households that already recycled (intensive margin). See Section B.1 in the Appendix for further details.

4.2 Dynamic Effects

Columns (4) and (5) examine the dynamics of the treatment effect. To do this, we break the data down over five time periods: the first 15 weeks after the campaign, weeks 16 to 50, weeks 51 to 100, weeks 101 to 151, and weeks 151 to 210. [20] This leads to the following regression:

$$y_{it} = \beta_0 + \beta_1 SC_i + \beta_2 W_{it}^{1-15} + ... + \beta_5 W_{it}^{101-150} + \beta_6 SC_i * W_{it}^{1-15} + ... + \beta_9 SC_i * W_{it}^{101-150} + \theta X_i + \epsilon_{it} \tag{2}$$

where W_i^j takes value 1 when the observation comes from period j and zero otherwise.

The omitted group is the period of time between weeks 151 and 210, and thus, β_1 captures the average treatment effect in our longest time horizon. The positive and significant coefficient for β_1 confirms that the soft commitment is still effective between 150 and 210 weeks after being given the opportunity to sign the pledge, increasing the proportion of households that recycle by 4.49 percentage points in the full sample (3.11 if controlling for the pre-intervention value of the outcome). However, the effect is two or three times greater in the weeks immediately following the campaign (weeks 1 to 15). Moreover, the analysis shows that the effect declines at the beginning but remains stable and statistically significant afterwards, as shown by β_6 being the only significant coefficient.

The lasting effect of pledges might be surprising, especially considering the relatively short impact often seen in behavioral interventions (Brandon et al., 2022) and the one-shot nature of soft commitments. Thus, the next question is, why does the effect persist as much as it does? From the perspective of social psychologists, such a lasting effect can be explained naturally by humans' desire for consistency as a central motivator of behavior (Cialdini, 2009). Moreover, Hummel and Maedche (2019) and DellaVigna and Linos (2022) explain that nudging has been found to be especially effective when applied to environmental policies and when administered face-to-face, as in the present case. Guilt-aversion (Charness and Dufwenberg, 2006) and preferences for keeping promises (Vanberg, 2008)–two factors that have been found to be relevant in explaining the effects of soft commitments in the lab–may be amplified in the context of a face-to-face pro-environmental campaign that involves a related gift (a recycling kit).

The type of activity under consideration is probably also relevant to its persistence. Waste

[20] This division of time is based on the dynamics observed in Panel B of Figure 1, where the treatment effect is greater during the first 10-20 weeks after the intervention and remains quite constant afterwards. The results are similar when we divide into different periods of time.

sorting is a regular activity performed on a daily basis, and it is prone to habit formation (Frey and Rogers, 2014; Brandon et al., 2022). Evidence for the relevance of habits can be seen in the regularity of some households' disposals, e.g., being done at a similar time. Our data allows us to address this question, as we have information on the time of each disposal. Table B3 in the Appendix shows the probability of making a disposal during each time window conditional on the time window during which the previous disposal was made. According to these transition probabilities, the most likely event is the repetition of the time a disposal is made. This pattern is stronger when looking at modal time windows for making disposals, i.e., from 6pm to 8pm and from 8pm to 10pm, which account for approximately 50% of all disposals: more than 40% of the households that make a disposal in one of these time windows, make their next disposal in the same time window. These regularities highlight the relevance of habits in waste sorting, and they seem to be crucial to the lasting effect of the pledge.

5 Robustness Checks, External Validity, and Feedback

All robustness checks, an analysis and discussion of external validity, and the results of a follow-up intervention based on feedback are available in the Online Appendix. In this section, we summarize our main findings.

5.1 Robustness Checks

Our results are robust to alternative specifications and analyses. First, we replicate Figure 1 and our main estimates in Table 2 by considering the two main outcome variables, *#Uses* and *DoF*. All previous results are confirmed when considering these alternative outcomes despite the estimations being less precise (see section C.1 in the Appendix). For instance, although the size of the effect for the first 15 weeks doubles the size of the effect seen after 151-210 weeks when considering *#Uses* as the outcome variable, the estimation does not identify a statistically significant decline in the treatment effect.

In section C.2 in the Appendix, we exhaustively analyze the consequences of the imbalance in the size of the control and treatment groups originating from the matching of field records and administrative data (see Section 3.2). As explained above (see footnote 19), the inclusion of recruitment week fixed effects eliminates concerns over the differences in the length of the observed pre-intervention period. Additionally, for further assurance, we also show that the results are robust to repeating the matching protocol but omitting national ID numbers in the treatment group for the same period that it was unavailable for the control group. By doing this, the imbalance disappears, and the results remain unchanged.

Furthermore, given the specific features of the outcome variables (i.e., their fractional or count nature, as well as zero inflation), we consider other estimation methods in Section C.3 in the Appendix. Specifically, we used the proposal by Papke and Wooldridge (1996) and a beta distribution to address the fractional nature of the dependent variable and a zero-inflated Poisson to correct for the high prevalence of zeros (see Figure B3 in the Appendix). All results remain unchanged.

Finally, some bin malfunctions were identified (e.g., recording failures). Appendix C.4 analyzes the impact of these incidents on our results, finding that they have no effect on our conclusions.

5.2 External Validity

Our findings show that giving people the opportunity to sign a commitment increases the effectiveness of pro-environmental campaigns. Still, two questions about the external validity of these results could be raised. First, it might be that households that approach the table are especially pro-environmental, limiting external validity. Table D1 in Appendix D shows that participating households exhibited low levels of recycling, indicating they were not particularly environmentally motivated. For example, the control group only used the bio-waste bins 17.3% of the weeks during the study period. Thus, external validity is not challenged by having a sample that was especially motivated by pro-environmental concerns.

Secondly, participants were aware that their recycling practices could be monitored. Although subjects were informed that their data was intended for aggregate rather than individual analysis, one might wonder about its implications for the external validity of the intervention. To address this question, we argue that those concerned about monitoring will focus on fulfilling the MENUCO (the minimum number of times they are told they have to recycle to be classified as recyclers). Consequently, we restrict our analysis to households not complying with the MENUCO to see if the effect of the soft commitment holds among those less likely to be motivated to recycle by the presence of an external observer. The results of this analysis are consistent with those seen in the main specification (see Table D2 in Appendix D). This suggests that a soft commitment would also work if waste sorting were not observable, consistent with the mechanisms of individual self-image (Cialdini and Trost, 1998; Akerlof and Kranton, 2000; Cialdini and Goldstein, 2004), cognitive dissonance (Festinger, 1957), and warm glow (Andreoni, 1990).

5.3 Feedback Intervention

In Appendix E, we analyze the feedback intervention, randomized in a 2x2 design within control and soft commitment groups, that took place in February 2020 . Details and materials from the feedback intervention are available at the AEA RCT Registry (number AEARCTR-0007723).

Although the feedback intervention took place at a later time, its results were mostly null (see Table E1 in the Appendix). The interaction between soft commitment and feedback was never statistically significant, disregarding the possibility that feedback could explain the lasting effect of the soft commitment. At the same time, this null result suggests that feedback cannot be used to reinforce the effect of the soft commitment.

6 Conclusion

Using a novel technology that allows waste sorting in dense urban areas to be tracked, we evaluate the effect of a soft commitment on promoting pro-environmental behavior. We show that a light-touch mechanism, consisting of being given the opportunity to sign a pledge, has a positive and lasting impact on recycling.

One shortcoming of our study, as explained in Section 3.3, is that we do not observe the amount of waste being thrown away but bin usage. More sophisticated technologies would allow us to obtain information on the size of each disposal or even on the specific content of each disposal. Nevertheless, as we show in Figure B1, our data provides a good proxy of the amount of bio-waste collected, and it represents a notable improvement over previous limitations in waste observability. Another limitation of our study is that we cannot assess the effect that offering the soft commitment will have when scaled up to the general population. For instance, a mass mailing campaign to all citizens may result in less adoption of the commitment. This is especially likely considering the increased effectiveness of face-to-face interventions (DellaVigna and Linos, 2022). Strictly speaking, our study provides evidence that soft commitments can greatly improve the effectiveness of face-to-face pro-environmental campaigns. Such campaigns are quite popular when it comes to promoting proper waste separation at the local level.

Our study provides relevant conclusions for policymakers and companies running environmental campaigns. Information campaigns similar to the one we made use of to run our experiment are common, and soft commitments can increase the effectiveness of such campaigns without the potential negative effects sometimes generated by monetary incentives and fees (Ling and Xu, 2021; Caplanova et al., 2022). Given that the additional cost of offering to sign the pledge is zero, information campaigns promoting recycling can increase their effectiveness at a negligible cost.

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Online Appendix

A Experimental Material

Figure A1: Informed consent signed by participants (Original version in Catalan).

Universitat de les Illes Balears

La Universitat de les Illes Balears (UIB) està realitzant un estudi per valorar l'eficàcia de la separació orgànica. Amb aquesta finalitat, farem servir dades que seran tractades d'acord amb la Llei Orgànica 3/2018, de 5 de desembre, de Protecció de Dades Personals i garantia dels drets digitals. **Dóna el seu consentiment a participar i utilitzar les seves dades en aquest estudi**?

Dono el meu consentiment	No dono el meu consentiment	Nom i Cognoms	Domicili	SIGNATURA

Note: The bold line was used to provide visual balance on group assignment. Under correct group assignment (staggered assignment) the informed consent and soft commitment form must reach the bold line at the same time.

Figure A2: Soft commitment form (Original version in Catalan).

Universitat de les Illes Balears

Es compromet a que al seu domicili es dugui a terme la separació de residus orgànics?

Es considerarà que la seva llar duu a terme la separació de residus orgànics si llencen la fracció orgànica d'acord amb el nombre mínim de vegades indicat per l'educador.

Mínim per setmana	Sí em comprometo	NO em comprometo	Nom i Cognoms	DNI	SIGNATURA

Note: The bold line was used to provide visual balance on group assignment. Under correct group assignment (staggered assignment) the informed consent and soft commitment form must reach the bold line at the same time

Figure A3: Informed consent signed by participants (translated from Catalan to English).

UIB Universitat de les Illes Balears

The University of the Balearic Islands (UIB) is conducting a study to assess the effectiveness of organic separation. To this end, we will use data that will be processed in accordance with Organic Law 3/2018, of 5 December, on the Protection of Personal Data and the guarantee of digital rights. Do you consent to participate and use your data in this study?

I give my consent	I do not give my consent	ID	First and last name	Address	SIGNATURE

Note: The bold line was used to provide visual balance on group assignment. Under correct group assignment (staggered assignment) the informed consent and soft commitment form must reach the bold line at the same time.

Figure A4: Soft commitment form (translated from Catalan to English).

Universitat de les Illes Balears

Do you commit to the separation of organic waste at your home?

Your household will be considered to separate bio-waste if your bio-waste disposal is done according to the minimum number of times indicated by the educator.

Minimum per week	YES I commit	I do NOT commit	First and last name	ID	SIGNATURE

Note: The bold line was used to provide visual balance on group assignment. Under correct group assignment (staggered assignment) the informed consent and soft commitment form must reach the bold line at the same time

B Supplementary Tables and Figures

Figure B1: Plot of total lid openings and aggregate mass of bio-waste collected since November 2018 to December 2019.

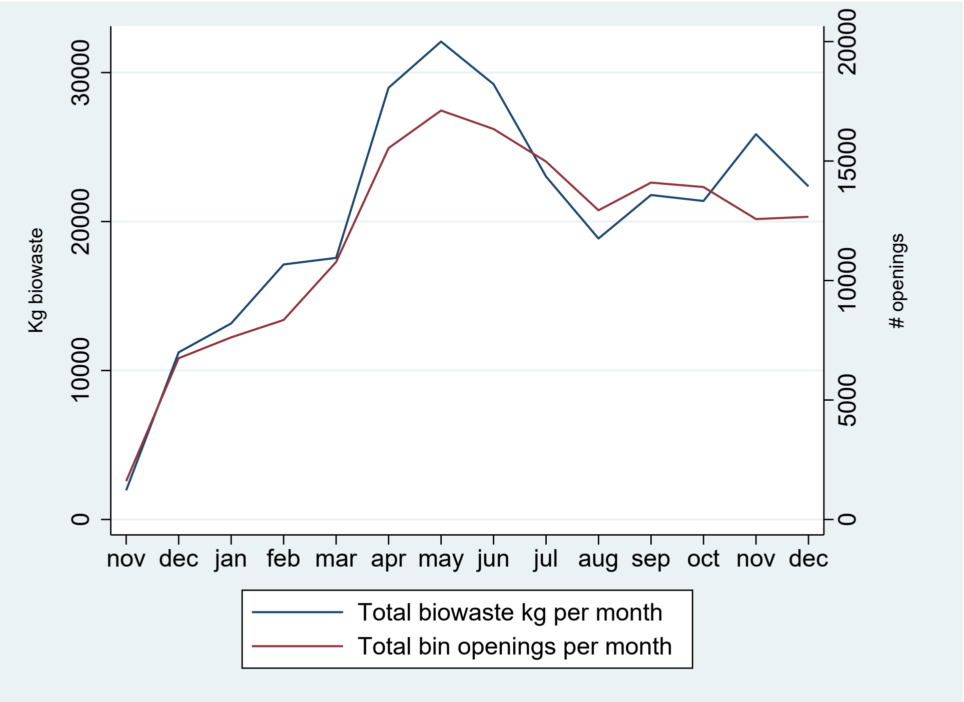


Figure B2: Number of households recruited in each experimental group over time.

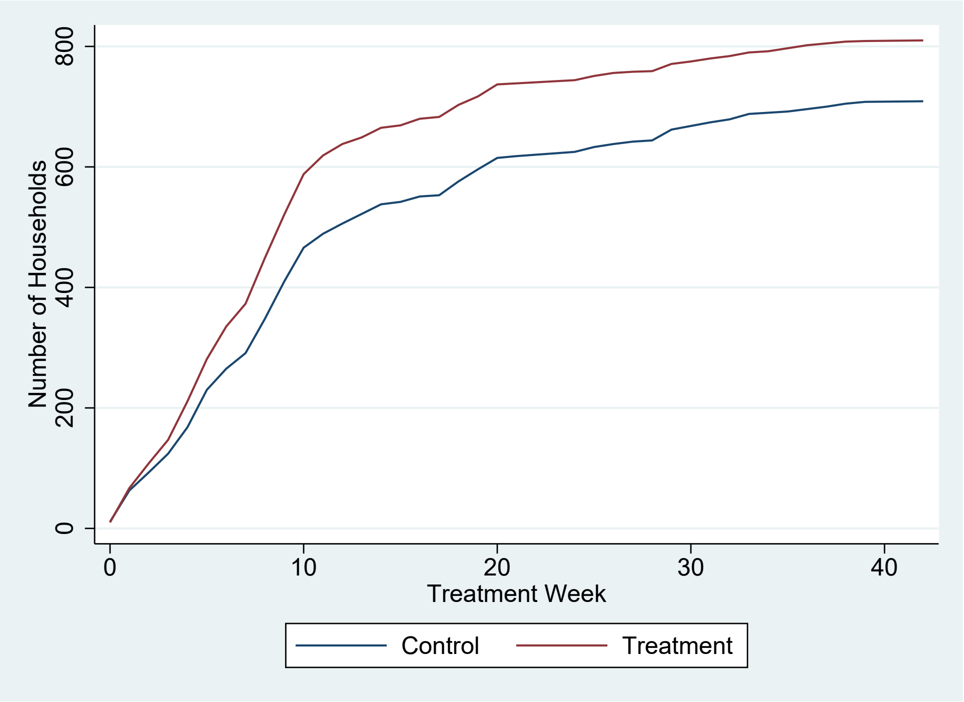
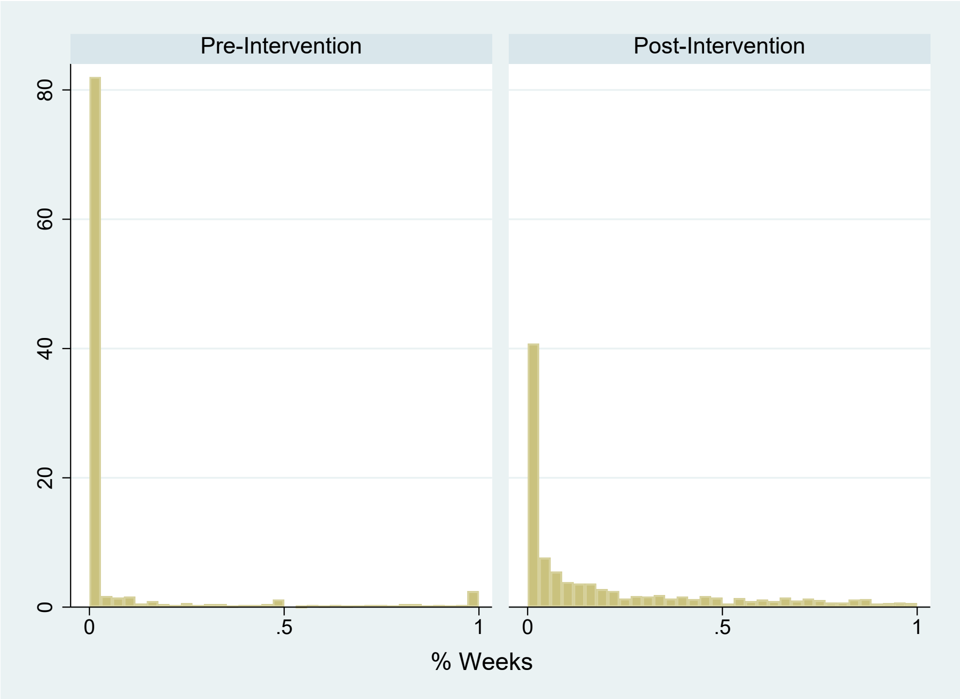


Figure B3: Histogram for % of weeks recycling.



B.1 Extensive and Intensive Margins

Soft commitment might affect the extensive margin by leading households to start sorting waste, or it can affect the intensive margin by increasing households adherence to waste sorting, i.e., increasing their frequency of participation over the observed period.

To formally analyze the role played by the extensive and intensive margins, we run a two-part model (Cragg, 1971).[21] Cragg's two-part model considers the existence of two independent (latent) processes determining the intensive and extensive margins. This method captures the overall effect by means of the unconditional semi-elasticity $S_j(y)$ and decompose it into the extensive $(S_j(P = 1))$ and the intensive $(S_j(y|y > 0))$ margins, so that $S_j(y) = S_j(P = 1) + S_j(y|y > 0)$. Below we provide further methodological details.

Table B1 shows the results from our benchmark regressions using a two-part model. The semi-elasticity of the overall effect $(S_j(y))$ is positive and highly significant, indicating that offering to sign the pledge increases recycling by 23.2%. When decomposing the effect into the two margins we observe that the effect on starting to recycle $(S_j(P = 1))$ is highly significant, with the probability of starting to recycle increasing by 16.7%. In contrast, the change in adherence $(S_j(y|y > 0))$, although positive, is small and statistically non-significant.

Summing up, the analysis of the extensive and intensive margins implies two things. First, soft commitment increases participation in waste sorting but it does not increase adherence of already participating households. Second, the adherence of the households that started to recycle motivated by the soft commitment, is similar to the ones that decided to do so by their own initiative.[22] These results are relevant from a policy perspective. Soft commitments should be considered as a tool for improving the effectiveness of environmental campaigns in places

[21] More specifically, analyses were performed using the model described in equations (7) and (9) in (Cragg, 1971).

[22] If the adherence of the former households were higher (lower) than that of the latter, we should observe a positive (negative) intensive margin.

Table B1: Average Treatment Effect of the Soft Commitment: Extensive and Intensive Margins

	All sample			Sample with Pre-Intervention Info		
	$S_j(y)$	$S_j(y\|y>0)$	$S_j(P=1)$	$S_j(y)$	$S_j(y\|y>0)$	$S_j(P=1)$
	(1)	(2)	(3)	(4)	(5)	(6)
SC	0.233***	0.0650	0.168***	0.213***	0.0490	0.164***
	(0.0604)	(0.0513)	(0.0322)	(0.0651)	(0.0561)	(0.0337)
Pre-int.				2.155***	0.519***	1.637***
				(0.259)	(0.0666)	(0.250)
Observations	1,483	1,483	1,483	1,153	1,153	1,153
Household Controls	YES	YES	YES	YES	YES	YES
Intervention week FE	YES	YES	YES	YES	YES	YES
ZIP FE	YES	YES	YES	YES	YES	YES
Censored Observations	415	415	415	356	356	356
Log-Likelihood	-546.7	-546.7	-546.7	-376.2	-376.2	-376.2
Pseudo R-squared	0.0755	0.0755	0.0755	0.182	0.182	0.182

Notes: Results of the two-part model estimation of equation (1) with the proportion of weeks recycling as the dependent variable. All estimations include ZIP-code and Intervention week fixed effects (FE) and household controls. Household controls are the number of people living in the household (*Size*), the Minimum Expected Number of Uses of the Container per week (*MENUCO*), the phase of the program (*Phase*) and household income (imputed from the census tract (*Income*)). *Pre-Int* refers to the average value of the outcome variable up to 40 weeks before the intervention (only available for a sub-sample). $S_j(y)$ shows the unconditional or total semi-elasticities. $S_j(y|y>0)$ and $S_j(P=1)$ show the semi-elasticities in the intensive and extensive margin, respectively. Robust standard errors for coefficients and delta-method-robust standard errors for marginal effects in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

where an important fraction of the population does not participate in waste sorting, but not where a majority of households already recycle even if they do so only sporadically.

Two-Part Model

Two-part models provide a suitable tool for the context of our paper, where decision making involves two steps. The first one consists on deciding whether to start recycling or not (the extensive margin) and a second one determines the adherence to recycling (the proportion of weeks doing so) conditional on having started to do so (the intensive margin). In our case, households first decide about whether to start recycling. Conditional on having started, they might exhibit different degrees of adherence (they might for instance recycle only sporadically or on a regular basis). Given this decision environment, we opted for presenting the results from Cragg's truncated normal hurdle model (Cragg, 1971).[23] Cragg's truncated normal hurdle model relies on the existence of two independent processes: one determining whether the outcome is either zero or positive and the other determining the exact positive value conditional on a non-zero outcome. Unlike in the Tobit model, these processes are assumed to be independent and potentially determined by a different set of regressors.
More formally, and omitting the subscripts i and t for the sake of exposition, let X be the vector containing the regressors in equation (1) which, in our case, is the same for the two margins. The model assumes that the outcome observed is $y = P \cdot y^*$, where P is an indicator such that $P = 1$ if $X\gamma + u > 0$, $u \sim \mathcal{N}(0, 1)$ and 0 otherwise; and y^* is a latent variable modeled as $y^* = X\beta + \epsilon$ where $(\epsilon|X) \sim \mathcal{N}(0, \sigma^2, -X\beta)$ with $X\beta$ being the lower truncation point. In short, the lognormal hurdle model estimates γ through a probit model for P, while β is

[23]More specifically, the analyses will be performed by using the model provided by equations (7) and (9) in Cragg (1971).

estimated with a truncated normal regression using y^* as the dependent variable and for $y^* > 0$.

An interesting aspect of two-part models is the interpretation of their coefficients which provide us with a deeper understanding of the impact of soft commitment in households' behavior. First, the model allows us to compute the unconditional semi-elasticity ($S_j(y)$), i.e., the percentage change in the dependent variable generated by being exposed to the treatment. This semi-elasticity reflects the overall average effect. Similarly to McDonald and Moffitt (1980), Table B2 shows how we can decompose this overall average effect into two different components

$$S_j(y) = S_j(y|y > 0) + S_j(P = 1) \tag{3}$$

Equation (3) makes clear that the total treatment effect, $S_j(y)$, can be decomposed into the *extensive margin* $S_j(P = 1)$ and *intensive margin* $S_j(y|y > 0)$. This allows to compute the relative contribution of each component by defining $s_j(P = 1) \equiv S_j(P = 1)/S_j(y)$ and $s_j(y|y > 0) \equiv S_j(y|y > 0)/S_j(y)$.

Table B2: – Expectations and semi-elasticities

$E(y\|X)$	$\Phi(X\gamma)\,[X\beta + \sigma\lambda(X\beta/\sigma)]$	
$P(y > 0\|X)$	$\Phi(X\gamma)$	
$E(y\|X, y > 0)$	$X\beta + \sigma\lambda(X\beta/\sigma)$	
$S_j(y) = \frac{\partial E(y\|X)/\partial x_j}{E(y\|X)}$	$\gamma_j\lambda(X\gamma) + \frac{\beta_j\theta(X\beta/\sigma)}{X\beta+\sigma\lambda(X\beta/\sigma)}$	Total Effect
$S_j(P = 1) = \frac{\partial P(y>0\|X)/\partial x_j}{P(y>0\|X)}$	$\gamma_j\lambda(X\gamma)$	Extensive Margin
$S_j(y\|y > 0) = \frac{\partial E(y\|X,y>0)/\partial x_j}{E(y\|X,y>0)}$	$\frac{\beta_j\theta(X\beta/\sigma)}{X\beta+\sigma\lambda(X\beta/\sigma)}$	Intensive Margin

Notes: $\Phi(.)$ denotes the cumulative normal distribution function, $\phi(.)$ its density function, $\lambda(.) = \frac{\phi(.)}{\Phi(.)}$ the inverse Mills ratio, and $\theta(z) = 1 - \lambda(z)[z + \lambda(z)]$. Coefficients from the probit are denoted by γ, and coefficients from the truncated normal regression by β. σ is the standard deviation of the random component ϵ.

B.2 Habit and Transition Matrices

Table B3: Time of the day transition matrices

PANEL A: All Days

	$t+1$												
Time Interval	0-2	2-4	4-6	6-8	8-10	10-12	12-14	14-16	16-18	18-20	20-22	22-24	Obs.
0-2	**.16987542**	.00906002	.0011325	.01585504	.03397508	.03171008	.02718007	.03057758	.02944507	.08607022	.24462061	.3204983	883
2-4	.20454545	**.06818182**	.	.	.06818182	.04545455	.	.	.04545455	.09090909	.15909091	.31818182	30
4-6	.	.	**.34482759**+	.06896552	.0591133	.1182266	.04926108	.04926108	.09359606	.13300493	.0591133	.02463054	17
6-8	.00204666	.	.00450266	**.38374949**+	.12566517	.10274253	.05832992	.03990995	.05566926	.11563651	.08923455	.0225133	1,317
8-10	.00167144	.00015918	.00135307	.05022286	**.32505571** +	.18688316	.07935371	.04568609	.08245782	.1323623	.07823941	.01655524	5,122
10-12	.00199067	.00018663	.00099533	.03097978	.14432348	**.29835148**+	.13069984	.05785381	.08902022	.14127527	.08416796	.02015552	8,235
12-14	.00201965	.0001836	.00091802	.02515377	.09345451	.19250895	**.22500689**+	.07279905	.09143487	.15762416	.1151198	.02377674	6,345
14-16	.00326691	.00049004	.00163345	.03021888	.1009474	.1359033	.12691931	**.17167592**+	.12838942	.15583143	.1131983	.03152565	2,755
16-18	.00211709	.00008143	.00138425	.02507939	.08362511	.11977852	.08240371	.0652227	**.26374074**+	.22424884	.10658741	.0257308	4,796
18-20	.00258444	.00017824	.00056442	.01515016	.04797552	.06835398	.05246116	.02771589	.08353385	**.43593857**+	.22716335	.03838042	13,768
20-22	.00591236	.0002401	.00054022	.01422569	.03061224	.0417467	.03577431	.0189976	.04003601	.22890156	**.47481993**+	.10819328	18,449
22-24	.02840804	.00073787	.00036894	.01051467	.0209371	.02803911	.0274857	.01798561	.02767017	.12036525	.32586239	**.39162516**+	8127

PANEL B: Working Days

	$t+1$												
Time Interval	0-2	2-4	4-6	6-8	8-10	10-12	12-14	14-16	16-18	18-20	20-22	22-24	Obs.
0-2	**.16356877**	.00557621	.	.0204461	.03903346	.03159851	.03159851	.02416357	.0204461	.07434944	.22862454	.3605948	538
2-4	.23076923	**.03846154**	.		.11538462	.	.	.03846154	.07692308	.11538462	.15384615	.23076923	19
4-6	.	.	**.4047619**+	.03571429	.04761905	.10119048	.03571429	.05952381	.11904762	.11904762	.05952381	.01785714	12
6-8	.0021526	.00023918	.00430519	**.43769433**+	.11289165	.0777326	.04185602	.03635494	.06003348	.11624013	.09040899	.02009089	1,145
8-10	.00138092	.00010622	.00148715	.05024432	**.35415339**+	.16624177	.07276397	.04578288	.08699809	.1277884	.07414489	.01890801	4,160
10-12	.0014175	.0002835	.000756	.03033453	.15138915	**.30920431**+	.12587413	.05688906	.09213759	.13664714	.07720658	.01786052	5,464
12-14	.00181514	.	.00083775	.02415526	.09075677	.18640045	**.25062832**+	.07483943	.09452667	.15344876	.10304384	.01954761	4,142
14-16	.00228938	.00068681	.00160256	.03342491	.11057692	.12637363	.11881868	**.19413919**+	.128663	.14720696	.10485348	.03136447	1,943
16-18	.00203644	.	.00150054	.03054662	.08499464	.10535906	.0733119	.06527331	**.29742765**+	.21864952	.09764202	.02325831	3,709
18-20	.00179687	.00012251	.00069425	.01858129	.04778045	.06219627	.04475844	.0256871	.08616817	**.45219913**+	.22383305	.03618246	10,005
20-22	.0050418	.00008403	.00063023	.01617579	.03138524	.03453636	.02966262	.01773035	.0382757	.23011638	**.48510567**+	.11125583	13,179
22-24	.02511734	.0011417	.00050742	.01344666	.02080426	.0229608	.02219967	.01623747	.0266396	.11632627	.32855512	**.40606368**+	5,931

PANEL C: Weekends

	$t+1$												
Time Interval	0-2	2-4	4-6	6-8	8-10	10-12	12-14	14-16	16-18	18-20	20-22	22-24	Obs.
0-2	**.17058824**	.01764706	.	.00882353	.01470588	.07647059	.05882353	.02941176	.02058824	.09117647	.25882353	.25294118	340
2-4	.16666667	**.**	.	.	.05555556	.05555556	.05555556	.11111111	.	.05555556	.16666667	.33333333	11
4-6	.	.	**.18181818**+	.12121212	.12121212	.21212121	.06060606	.09090909		.18181818	.03030303	.	5
6-8	.00293255	.00146628	.00733138	**.46627566**+	.09530792	.09384164	.05571848	.03519062	.03665689	.10703812	.08357771	.01466276	161
8-10	.00130548	.00032637	.00097911	.02447781	**.30319843**+	.23009138	.08746736	.04634465	.06690601	.13446475	.08942559	.01501305	926
10-12	.00448598	.	.00056075	.0128972	.1328972	**.32037383**+	.14	.05308411	.07383178	.14373832	.09252336	.02560748	2,699
12-14	.00166389	.00027732	.00083195	.00859678	.07681642	.20881864	**.2384914**+	.06572379	.078203	.1530782	.13449806	.03300055	2,128
14-16	.00293945	.00117578	.00058789	.01410935	.07466196	.16460905	.14462081	**.1734274**+	.12698413	.13521458	.12580835	.03586126	789
16-18	.00452646	.	.00174095	.00905292	.07277159	.14623955	.08983287	.07277159	**.19846797**+	.25034819	.12151811	.03272981	1,057
18-20	.0052432	.00011156	.00033467	.0075859	.04763498	.08467202	.06659973	.02744311	.08076751	**.41064257**+	.23125837	.03770638	3,685
20-22	.0077461	.00032275	.00043034	.00656267	.02872512	.05723507	.05110274	.02033351	.03571813	.2267886	**.45906401**+	.10597095	5,156
22-24	.03593642	.00103663	.	.00518314	.02073255	.04215619	.0390463	.02176918	.03628196	.12163096	.32584658	**.3503801**+	2,153

Notes: Each cell shows, for each time window of the last disposal (rows, t), the probability that the next time a household makes a disposal it will do so in a given time window (columns, $t+1$). For Panel A, all disposals in the dataset are considered. For Panel B, only disposals on weekdays (Monday to Friday) are considered. For Panel C, only disposal in weekends (Saturday and Sunday) are considered. Notice that the last disposal in the dataset is not included in this analysis (as there is no $t+1$'s disposal), so the sum of observations in Panel B and Panel C is lower than the number of observations in panel A. The elements of the diagonal are in bold displaying the probabilities that two consecutive disposals take place in the same time window. The symbol + denotes that the maximum transition probability is within the same time interval.

C Robustness Checks

This section provides the robustness checks summarized in Section 5 of the main text.

C.1 Other Outcomes

Table C1: Average Treatment Effect of the Soft Commitment: Alternative Outcomes

	Average Weekly Openings ($\#Uses$)				Degree of fulfillment ($\#DoF$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SC	0.0804**	0.0648*	0.0865***	0.0634*	0.0406***	0.0361***	0.0356***	0.0248*
	(0.0339)	(0.0369)	(0.0314)	(0.0354)	(0.0125)	(0.0136)	(0.0124)	(0.0137)
Pre-int.		0.890***		1.002***		0.329***		0.365***
		(0.119)		(0.114)		(0.0375)		(0.0343)
SCx 15 weeks			0.0713	0.0655			0.0519***	0.0529***
			(0.0479)	(0.0533)			(0.0179)	(0.0200)
SCx 16-50 weeks			-0.00709	0.0118			0.0136	0.0252
			(0.0388)	(0.0424)			(0.0145)	(0.0158)
SCx 51-100 weeks			-0.00715	-0.00261			0.00509	0.0143
			(0.0313)	(0.0354)			(0.0121)	(0.0132)
SCx 101-150 weeks			-0.0311	-0.0144			-0.00969	0.000708
			(0.0226)	(0.0259)			(0.00854)	(0.00960)
15 weeks			0.528***	0.501***			0.217***	0.204***
			(0.0348)	(0.0373)			(0.0129)	(0.0142)
15-50 weeks			0.310***	0.278***			0.124***	0.110***
			(0.0293)	(0.0306)			(0.0105)	(0.0110)
51-100 weeks			0.183***	0.170***			0.0688***	0.0592***
			(0.0227)	(0.0257)			(0.00836)	(0.00883)
101-150 weeks			0.111***	0.111***			0.0408***	0.0389***
			(0.0177)	(0.0205)			(0.00624)	(0.00689)
Constant	0.0608	0.285	-0.179	0.0771	0.132	0.218**	0.0826	0.180*
	(0.257)	(0.321)	(0.244)	(0.313)	(0.0837)	(0.108)	(0.0809)	(0.103)
N	1483	1153	7415	5765	1483	1153	7415	5765
Adj. R^2	0.0153	0.101	0.0876	0.154	0.0595	0.146	0.140	0.203
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimation of equation 1 in columns (1)-(2) (cols (5)-(6)) and estimation of equation 2 in columns (3)-(4) (cols (7)-(8)) with the average number of bin uses as a dependent variable (degree of fulfillment, $\#Uses/MENUCO$ with truncation at 1, as the dependent variable). All estimations include ZIP-code and Intervention week fixed effects (FE) and household controls. Household controls are the number of people living in the household (*Size*), the Minimum Expected Number of Uses of the Container per week (*MENUCO*), the phase of the program (*Phase*) and household income (imputed from the census tract (*Income*)). *Pre-Int* refers to the average value of the outcome variable up to 40 weeks before the intervention (only available for a sub-sample). Robust standard errors in parentheses (clustered at household level for columns (3), (4), (7) and (8)).* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure C1: Evolution of control and treatment recycling behavior: Number of Uses as the outcome variable

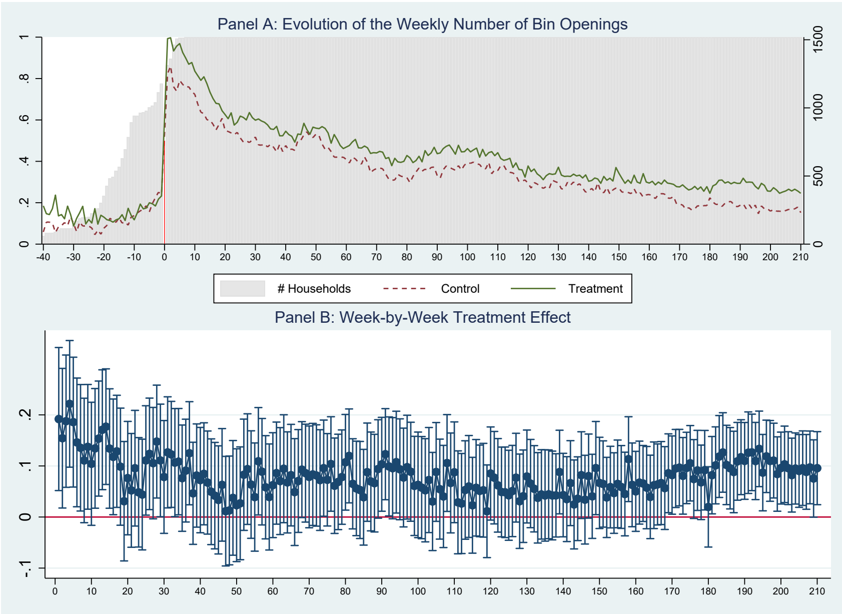
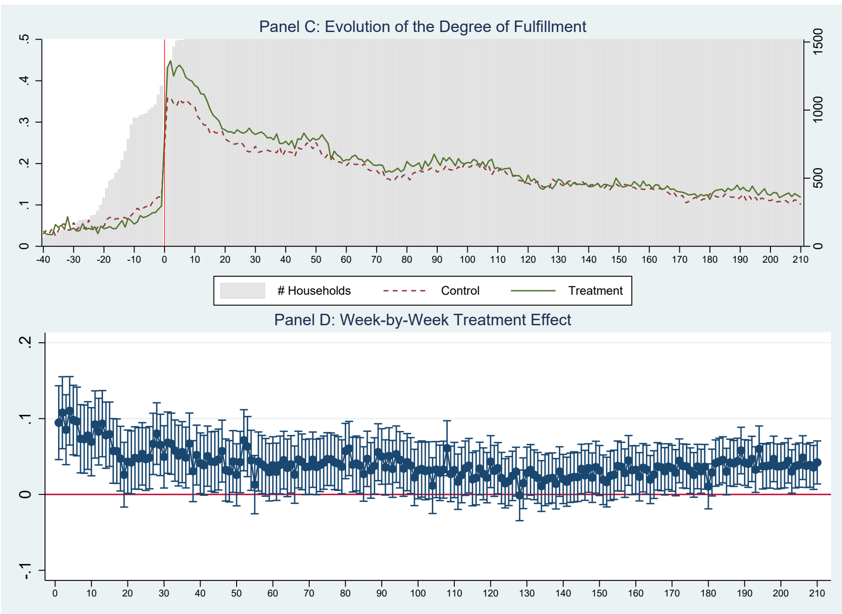


Figure C2: Evolution of control and treatment recycling behavior: Degree of fulfillment as the outcome variable



Notes: Evolution of control and treatment recycling behavior (Panel A) and the plot of weekly average treatment effects (Panel B) up to 210 weeks after the intervention. The x-axis shows the number of weeks from the intervention, with negative values being the pre-intervention weeks and positive values the post-intervention weeks. In Panel A, lines represent the percentage of households recycling for control and treatment group and the bars show sample size (right axis). In Panel B, weekly treatment effects are estimated as $\beta_1 + \beta_3^j$ from the regression $y_{it} = \beta_0 + \beta_1 SC_i + \beta_2^1 WeeksAfter_{it}^1 + ... + \beta_2^{210} WeeksAfter_{it}^{210} + \beta_3^1 SC_i * WeeksAfter_{it}^1 + ... + \beta_3^{210} SC_i * WeeksAfter_{it}^{210} + \theta X_{i,t} + \epsilon_{i,t}$, where $WeeksAfter_{it}^j$ is a dummy variable taking value one when the weeks elapsed since the treatment for household i at period t are equal to j with $j = 1, ..., 210$. Standard errors are clustered at the household level. $X_{i,t}$ includes households' characteristics (MENUCO number of household members, bin installation phase and household income, as well as fixed effects for households' zip code and recruitment week).

C.2 Robustness of the matching protocol

As explained in the main text (Section 3.2), at the beginning of the campaign only the participants in the treatment group were asked for their national ID. Since that information was very effective to match field and administrative data, such difference created an imbalance in the sample size of the two groups (see Figure B2).

As highlighted in the main text, the combination of the imbalance in group sizes and staggered recruitment yields to a discrepancy in the average length of the period for which pre-intervention outrcomes can be observed for the two groups (the period elapsed between bin installation and recruitment for the experiment). In this subsection, we show that this difference does not confound the estimation of the treatment effect.

First of all, it must be noted that our regressions included recruitment week fixed effects. Such inclusion assures that we compare treated and control households recruited in the same week, ensuring they have the same length of the observed pre-intervention period. Therefore, the imbalance may affect the precision of the estimates but cannot not bias them. Besides the use of recruitment week fixed effects, there are other powerful reasons that make the imbalance in sample size unimportant for the identification of the treatment effect. First, treated and control households that were correctly matched showed identical recycling patterns before treatment (see balancing tests in Table 1). Second, the reason for unmatchings was unrelated to households (it was exogenous and only determined by the ability of the matching protocol to identify citizens). Third, the estimation of the treatment effect is very similar if not using recruitment week fixed effects (see columns (1) and (2) of Table 2). Fourth, for an extra assurance, we replicated our main analysis omitting the national ID for the treated group in the same period for which this information was not available for the control group. By doing so, the imbalance in sample sizes before recruitment is no longer statistically significant (see below). As reported in Table C2, the results remain qualitatively unchanged.

Panel A: Main matching protocol				
	Control	Treatment	Diff (T-C)	p-value
	(1)	(2)	(3)	(4)
Number of Weeks (Pre-Int.)	17.24	15.26	-1.98	0.001
	(0.453)	(0.396)	(0.599)	
N	560	619		
Panel B: Robust matching protocol				
	Control	Treatment	Diff (T-C)	p-value
	(1)	(2)	(3)	(4)
Number of Weeks (Pre-Int.)	17.24	16.18	-1.06	0.128
	(0.453)	(0.529)	(0.695)	
N	560	417		

Notes: Columns (1) and (2) display the averages of pre-intervention weeks and sample sizes for control and treatment households, respectively. Column (3) shows the difference in means and column (4) its corresponding p-value for the t-test of equal means. Standard errors in parentheses. Panel A shows the results for the matching protocol used in the main analysis (using national ID as matching variable whenever possible). Panel B shows the results for the matching protocol used to test the robustness of the results (using national ID as matching variable only when available for the control and treatment groups)

Table C2: Average Treatment Effect of the Soft Commitment: Robust Matching.

	(1)	(2)	(3)	(4)	(5)
SC	0.0702***	0.0659***	0.0493***	0.0580***	0.0309*
	(0.0157)	(0.0162)	(0.0175)	(0.0161)	(0.0176)
Pre-int.			0.375***		0.414***
			(0.0423)		(0.0384)
SCx 15 weeks				0.0554**	0.0680***
				(0.0215)	(0.0240)
SCx 16-50 weeks				0.0176	0.0352*
				(0.0178)	(0.0196)
SCx 51-100 weeks				0.00548	0.0159
				(0.0147)	(0.0160)
SCx 101-150 weeks				-0.00646	0.00674
				(0.0104)	(0.0117)
15 weeks				0.250***	0.235***
				(0.0143)	(0.0157)
15-50 weeks				0.141***	0.127***
				(0.0117)	(0.0124)
51-100 weeks				0.0767***	0.0666***
				(0.00905)	(0.00968)
101-150 weeks				0.0431***	0.0411***
				(0.00663)	(0.00732)
Constant	0.173***	0.100	0.287**	0.000510	0.212
	(0.00948)	(0.0936)	(0.144)	(0.0907)	(0.133)
N	1244	1212	955	6060	4775
Adj. R^2	0.0157	0.0348	0.132	0.125	0.199
Controls	No	Yes	Yes	Yes	Yes
FE	No	Yes	Yes	Yes	Yes

Notes: Estimation of equation (1) in columns (1)-(3) and of equation (2) in columns (4)-(5) with the proportion of weeks recycling after the intervention as the dependent variable for the sample resulting from using the robust matching protocol. Columns (2)-(5) include ZIP-code and Intervention week fixed effects (FE) and household controls. Household controls include the number of people living in the household (*Size*), the Minimum Expected Number of Uses of the Container per week (*MENUCO*), the bin installation phase (*Phase*) and household income (imputed from the census tract (*Income*)). *Pre-Int* refers to the average value of the outcome variable up to 40 weeks before the intervention. Robust standard errors in parentheses (clustered at household level for columns (4) and (5)).* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C.3 Fitting Other Distributions

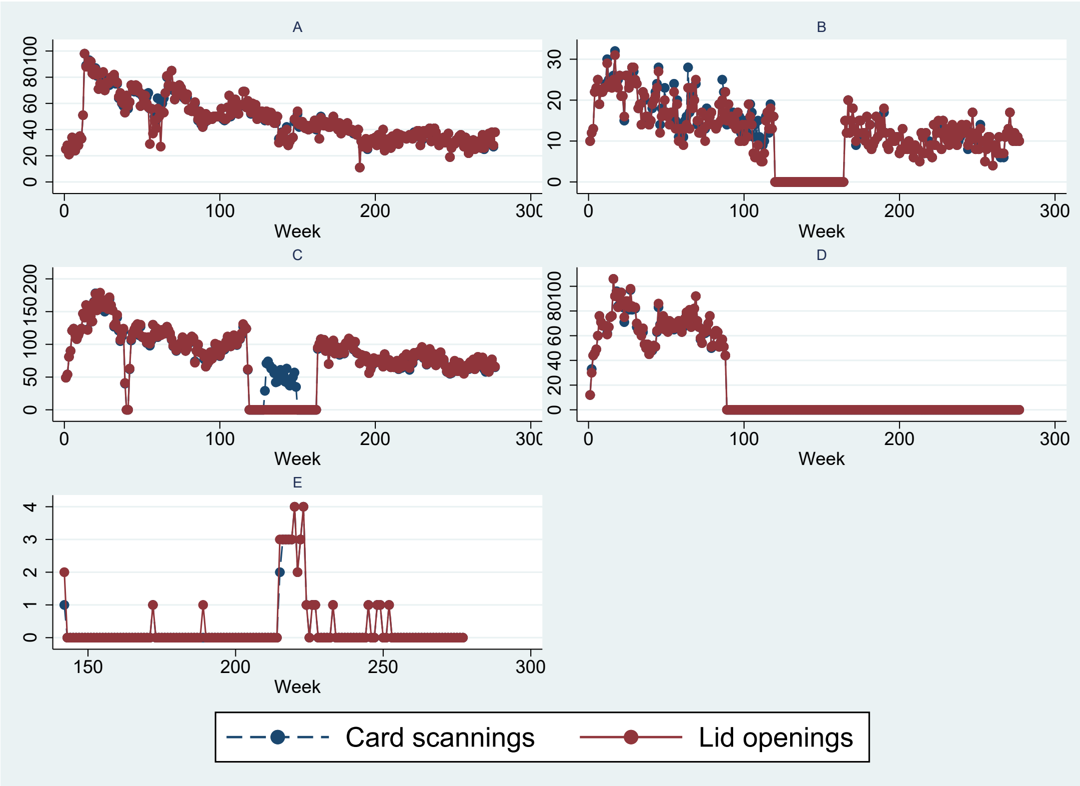
Table C3: Average Treatment Effect of the Soft Commitment: Fitting Other Distributions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
SC	0.0487***	0.0428***	0.0451***	0.0456***	0.0375***	0.0666***	0.0445**	0.0681***	0.0424**
	(0.0130)	(0.0139)	(0.00940)	(0.0121)	(0.0122)	(0.0205)	(0.0226)	(0.0211)	(0.0213)
Observations	1,483	1,153	1,483	1,483	1,153	7,415	5,765	7,415	5,765
Papke & Woolrdige 1996	YES	YES	NO	NO	NO	YES	YES	NO	NO
Beta distribution	NO	NO	YES	NO	NO	NO	NO	NO	NO
Zero Inflation	NO	NO	NO	YES	YES	NO	NO	YES	YES
Pre-intervention control	NO	YES	NO	NO	YES	NO	YES	NO	YES

Notes: Results of estimating equation (1) by different methods with the proportion of weeks recycling as the dependent variable. Marginal effects reported. For columns (6)-(9) the coefficient of SC estimates the effect of the pledge 150-210 weeks after being offered. All estimations include ZIP-code and Intervention week fixed effects (FE) and household controls. Household controls are the number of people living in the household (*Size*), the Minimum Expected Number of Uses of the Container per week (*MENUCO*), the phase of the program (*Phase*), household income (imputed from the census tract (*Income*)) and, for columns (2), (5), (7) and (9), the pre-intervention value of the dependent variable. Robust standard errors in parentheses (clustered at household level for columns (6) to (9)). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.4 Bin Malfunctioning

Figure C3: Weekly lid openings and card scannings for three bins



Notes: Each panel shows the number of weekly lid openings and weekly card scannings for a specific bin.

The incipient implementation of the electronic bin system implies that its reliability was not completely assured. While the system effectively captures bio-waste sorting (see Figure B1 for a joint plot of monthly bin openings and total bio-waste collected), some electronic bins experienced anecdotal malfunctioning incidents. To detect these incidents we considered the whole data generated by the system during the evaluated period aggregated at bin level. These data reveal that there are periods where some bins exhibit abnormal patterns in which zero lid openings and/or zero card scanning were registered. Panel A of Figure C3 provides an example of a bin with a standard pattern in which weekly lid openings and weekly card scanning follow almost identical patterns with no abrupt changes. Panel B in the same figure, provides an example of a bin incident: lid openings and card scanning fall abruptly at zero during a period and then work again. Panel C in the same figure shows another type of bin malfunctioning, where weekly lid openings suddenly fall to zero while card scannings remain at positive levels. The plausible explanation for this irregular pattern is that card scannings were correctly registered but lid openings were not. On aggregate terms these errors are unimportant (they affect less than 2% of the household-week observations in our sample) Anyway, we can address the potential attenuation bias induced by these bin malfunctionings. To deal with the second type of bin malfunctioning, we simply replace lid openings by card scanning in the construction of our proxies for recycling. Table C5 replicates the main results after doing this change in the dependent variable.

For the first type of bin malfunctioning (affecting both bin openings and scannings), what we do is adapting the sample to bin malfunctioning. During the weeks in which a bin is not working properly, we excluded from the sample the households for which the affected bin was the usual spot where they dispose bio-waste.[24] We considered as bin malfunction weeks when bin openings sticks to zero with two exceptions: i) bins that were never reactivated after zero registries (illustrated by Panel D in the Figure, which may correspond to bin reallocation or removal) and ii) bins for which its regular use is small (below 10 registries in its maximal use week) and hence zero registries cannot be attributed to bin malfunction (illustrated by Panel E in the figure) Table C4 replicates the main results after excluding these problematic observations. Overall, Tables C5 and C4 show that results are robust to accounting for these incidences in registries.

Table C4: Average Treatment Effect of the Soft Commitment: Correcting for Bin Malfunctioning

	(1)	(2)	(3)	(4)	(5)
SC	0.0542***	0.0523***	0.0467***	0.0420***	0.0279*
	(0.0142)	(0.0145)	(0.0156)	(0.0144)	(0.0160)
Pre-int.			0.389***		0.426***
			(0.0408)		(0.0374)
SCx 15 weeks				0.0596***	0.0660***
				(0.0197)	(0.0220)
SCx 16-50 weeks				0.0208	0.0346*
				(0.0165)	(0.0180)
SCx 51-100 weeks				0.00389	0.0212
				(0.0136)	(0.0147)
SCx 101-150 weeks				-0.00349	0.00728
				(0.00899)	(0.0103)
15 weeks				0.242***	0.227***
				(0.0143)	(0.0157)
15-50 weeks				0.140***	0.126***
				(0.0119)	(0.0125)
51-100 weeks				0.0813***	0.0665***
				(0.00940)	(0.00973)
101-150 weeks				0.0483***	0.0433***
				(0.00642)	(0.00717)
Constant	0.185***	0.0749	0.168	-0.0193	0.0880
	(0.00992)	(0.0938)	(0.127)	(0.0896)	(0.120)
N	1519	1483	1153	7389	5747
Adj. R^2	0.00874	0.0395	0.135	0.123	0.194
Controls	No	Yes	Yes	Yes	Yes
FE	No	Yes	Yes	Yes	Yes

Notes: Estimation of equation (1) in columns (1)-(3) and of equation (2) in columns (4) and (5) with the proportion of weeks recycling after the intervention as the dependent variable, correcting for bin malfunctioning. Columns (2)-(5) include ZIP-code and Intervention week fixed effects (FE) and household controls. Household controls include the number of people living in the household (*Size*), the Minimum Expected Number of Uses of the Container per week (*MENUCO*), the bin installation phase (*Phase*) and household income (imputed from the census tract (*Income*)). *Pre-Int* refers to the average value of the outcome variable up to 40 weeks before the intervention. Robust standard errors in parentheses (clustered at household level for columns (4) and (5)).* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Other Incidents Another incident in bin registries consists on cases where the action of scanning the card was not registered in the system, while the action of opening the lid was. This registry error explains why in some very unusual cases we observe a slightly greater number of lid openings than card scannings (this is the case for some weeks in Panel B of Figure C3). This is a very infrequent error. Moreover, since our measures of recycling are based on lid openings, this type of malfunctioning is irrelevant for our results. We also detected an error consisting in duplicating some registries. That is, the same action is registered twice under the same time stamp and card identifier. This type of error does not affect our results as we consider a disposals occurring within an interval of 12 hours as a unique one (see footnote 17 in the main text).

[24]For participant households with at least one registry, we computed their modal bin using all registries. For participant households with no openings, we maintained their openings equal to zero.

Table C5: Average Treatment Effect of the Soft Commitment: Card Scannings

	(1)	(2)	(3)	(4)	(5)
SC	0.0549***	0.0524***	0.0471***	0.0463***	0.0326**
	(0.0140)	(0.0143)	(0.0156)	(0.0141)	(0.0157)
Pre-int.			0.384***		0.424***
			(0.0413)		(0.0374)
SCx 15 weeks				0.0560***	0.0621***
				(0.0197)	(0.0220)
SCx 15-50 weeks				0.0157	0.0280
				(0.0165)	(0.0181)
SCx 51-100 weeks				0.00423	0.0170
				(0.0135)	(0.0148)
SCx 101-150 weeks				-0.00550	0.00777
				(0.00932)	(0.0105)
15 weeks				0.241***	0.226***
				(0.0143)	(0.0157)
15-50 weeks				0.147***	0.132***
				(0.0120)	(0.0127)
51-100 weeks				0.0813***	0.0706***
				(0.00925)	(0.00992)
101-150 weeks				0.0498***	0.0476***
				(0.00664)	(0.00727)
Constant	0.179***	0.0499	0.150	-0.0336	0.0791
	(0.00978)	(0.0935)	(0.125)	(0.0896)	(0.119)
N	1519	1483	1153	7415	5765
Adj. R^2	0.00922	0.0383	0.132	0.122	0.191
Controls	No	Yes	Yes	Yes	Yes
FE	No	Yes	Yes	Yes	Yes

Notes: Estimation of equation (1) in columns (1)-(3) and of equation (2) in columns (4) and (5) with the proportion of weeks recycling after the intervention as the dependent variable (using scannings rather than lid openings). Columns (2)-(5) include ZIP-code and Intervention week fixed effects (FE) and household controls. Household controls include the number of people living in the household (*Size*), the Minimum Expected Number of Uses of the Container per week (*MENUCO*), the bin installation phase (*Phase*) and household income (imputed from the census tract (*Income*). *Pre-Int* refers to the average value of the outcome variable up to 40 weeks before the intervention. Robust standard errors in parentheses (clustered at household level for columns (4) and (5)).* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

D External validity

Internal validity of our results is guaranteed by random assignment. In this section, we discuss the external validity of our results. We focus the discussion on *i*) the recruitment process and *ii*) the possibility of monitoring (observing bio-waste).

D.1 Recruitment and participation

Table D1: Proportion of Weeks recycling and Never Recycling Households

	Control	Treatment	Obs. Control	Obs. INC
Never recycling (24 weeks after campaign)	0.423	0.289	709	810
Inactive (less than 10% 210 weeks)	0.597	0.516	709	810
Proportion of weeks recycling (210 weeks)	0.173	0.226	709	810

Columns (1) and (2) display the proportion of recyclers and never recyclers for control and treatment households, respectively while columns (3) and (4) show the sample size for control and treatment, respectively.

In our experiment, participants were recruited after approaching to the informational points. Thus, one might ask how does the self-selection induced by participants approaching to these points affects the external validity of our results. The main concern is the possibility that the control and treated groups are especially motivated towards the environment and waste sorting. The results in Table D1 reject such possibility. After being recruited, 42.3% of the households in the control group did never use bio-waste bins for the 24 weeks after the campaign and 59,7% used the recycling bins less than 10% of the weeks during the studied period. On average, households in the control group only made some use of the bio-waste bins for 17.3% of the weeks during the studied period. This poor performance makes it difficult to qualify the participants in the study as environmentally motivated. Instead, we could verify in our visits to the field that the gift of a recycling kit (consisting in a small plastic bin and some recycling bags) was a powerful incentive to attract participants. As shown by Shampanier et al. (2007), inexpensive goods can generate high demand when offered at a zero price. Indeed, it was frequent to observe the formation of queues to collect the present. Although totally anecdotally, a few participants declared their intention to use the bin for another use different than recycling. Nevertheless, it should be remarked that what our study strictly proves is the effectiveness of soft commitment to improve the performance of a face-to-face environmental campaign. Such campaigns are quite prevalent to promote correct waste separation and pro-environmental behavior.

D.2 Monitoring and Self-Image

The current intervention took advantage of the possibility of evaluating waste sorting through a system of electronic bins and personal cards. This implies that participants were aware that their recycling practices can be monitored. Although monitoring, as subjects are reminded at enrollment, is anonymous in law enforcement and despite it is present in both groups, one still can fairly ask about its implications in terms of the external validity of the intervention.[25] Would

[25]The two groups signing an informed consent also disregards the mere action of providing a signature to drive the effect of soft commitment (the informed consent is available in Figure A3).

soft commitments have a similar effect in a context where waste sorting cannot be observed?

Table D2: Average Treatment Effect of the Soft Commitment: External Validity Self-image vs Monitoring

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Households initially not reaching MENUCO					Weeks when the MENUCO was not met				
SC	0.0296***	0.0301**	0.0264**	0.0223*	0.0170	0.0258***	0.0322***	0.0289***	0.0338***	0.0253**
	(0.0112)	(0.0117)	(0.0122)	(0.0124)	(0.0131)	(0.00851)	(0.00814)	(0.00911)	(0.00893)	(0.00989)
Pre-int.			0.257***		0.304***			0.153***		0.212***
			(0.0789)		(0.0755)			(0.0303)		(0.0334)
SCxWA_15				0.0467**	0.0440**				0.0220	0.0290*
				(0.0186)	(0.0202)				(0.0150)	(0.0168)
SCxWA_15_50				0.0170	0.0127				-0.00270	0.00531
				(0.0151)	(0.0158)				(0.0111)	(0.0124)
SCxWA_51_100				0.000449	0.00273				-0.00358	0.00395
				(0.0118)	(0.0125)				(0.00843)	(0.00956)
SCxWA_101_151				0.00553	0.00786				0.00304	0.00966
				(0.00746)	(0.00848)				(0.00611)	(0.00694)
15 weeks				0.0742***	0.0746***				0.111***	0.106***
				(0.0121)	(0.0135)				(0.0105)	(0.0117)
15-50 weeks				0.0370***	0.0370***				0.0643***	0.0610***
				(0.00973)	(0.0106)				(0.00795)	(0.00852)
51-100 weeks				0.0292***	0.0256***				0.0325***	0.0313***
				(0.00715)	(0.00804)				(0.00536)	(0.00608)
101-150 weeks				0.0167***	0.0149***				0.0140***	0.0147***
				(0.00468)	(0.00526)				(0.00406)	(0.00451)
Constant	0.0677***	-0.187***	-0.106*	-0.246***	-0.150**	0.0571***	-0.232***	-0.190***	-0.334***	-0.279***
	(0.00707)	(0.0630)	(0.0620)	(0.0674)	(0.0663)	(0.00546)	(0.0561)	(0.0709)	(0.0567)	(0.0765)
N	899	893	719	4465	3595	1519	1483	1153	7267	5655
R2_a	0.00663	0.0286	0.0638	0.0814	0.115	0.00518	0.165	0.187	0.188	0.217
Controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
FE	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes

Notes: Estimation of equation (1) and (2) with the proportion of weeks recycling after the intervention as the dependent variable. Columns (1)-(5) considers only those households not meeting the MENUCO for more than 70% of the first eight weeks after the intervention (cols (1)-(3) estimates equation (1) and cols (4)-(5) estimates equation (2)). Columns (5)-(10) excludes those weeks where the MENUCO was met for computing the proportion of weeks recycling (cols (6)-(8) estimates equation (1) and cols (9)-(10) estimates equation (2)). Fixed effects are ZIP-code and Intervention week. Household controls include the number of people living in the household (*Size*), the Minimum Expected Number of Uses of the Container per week (*MENUCO*), the bin installation phase (*Phase*) and household income (imputed from the census tract (*Income*). *Pre-Int* refers to the average value of the outcome variable up to 40 weeks before the intervention. Robust standard errors in parentheses (clustered at household level for columns (4), (5), (9) and (10). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

To address this question, we exploit the specific design of the form used for the soft commitment (see Figure A3 in the Appendix A). The form contains the following question: "*Do you commit to the separation of organic waste at your home?*", and a clarification on the required number of disposals followed: "*Your household will be considered to separate bio-waste if your bio-waste disposal is done according to the minimum number of times indicated by the educator.*" According to this clarification, households who want to be qualified as recycling in a given week by an external observer need to meet the minimum number indicated by the educator, which we called MENUCO (see subsection 2.2 for details on how MENUCO was computed). For households concerned with external observability, reaching this target should be relevant. In contrast, if the operating mechanism is more related to the need of self-consistency (Cialdini and Trost, 1998; Akerlof and Kranton, 2000; Cialdini and Goldstein, 2004), individuals can be less concerned with this external objective despite feeling committed to waste sorting.
Taken the above into account, if the effect of soft commitment were driven by the existence of an external observer, then the positive effect should take place for the households that care about external observability (the ones meeting the MENUCO) but not for the ones who are less concerned with external observability (the ones recycling but not meeting the MENUCO). By contrast, if the positive effect of soft commitment arises from self-image, then we can observe

that the positive effect also takes place for those households that do not meet the external objective.
Consequently, we focus our analysis on households not reaching the MENUCO to see if the effect of soft commitment holds for those households that are less likely to be motivated to recycle by the existence of an external observer. We use two strategies to select a subsample of households not reaching the MENUCO. In the first one (columns (1)-(5) of Table D2), we consider only those households not meeting the MENUCO for at least 70% of the 8 weeks after the campaign (results are similar by considering alternative thresholds). In the second approach, (columns (6)-(10) of Table D2), we use all the households participating in the study, but we exclude those weeks where the MENUCO was met to compute the average participation in recycling. Results of the analysis returns consistent results with the ones shown in the main specification.
Despite the demanding conditions imposed (the first strategy reduces the sample size notably by dropping out the households that made more disposals and thus are more likely to have reacted more), we confirm the positive effect of soft-commitment docuemnted in our main specifications. The only excepction is found in column (6), where we do not find a ststistically significant effect after 150-210 weeks when imposing the most demanding criteria and restricting the sample to the households with pre-intervention values of the outcome. Nevertheless, the joint test confirms we still find a positive impact after 100-150 weeks (p.value=0.0857). Overall, these findings reinforce the external validity of our results, suggesting that soft commitment would also work if waste sorting was not observable, consistent with the mechanisms of individual self-image (Cialdini and Trost, 1998; Akerlof and Kranton, 2000; Cialdini and Goldstein, 2004), cognitive dissonance (Festinger, 1957), or warm-glow (Andreoni, 1990).

E Feedback intervention

As explained in the main text, a feedback intervention took place on February 2020. Treatment assignment of the second intervention was randomized within the control and soft commitment groups in a 2x2 design. In this section, we estimate the effect of the feedback campaign and its interaction with soft commitment. Detail of the materials and design can be found at the AEA RCT Registry (number AEARCTR-0007723).
The results are reported in Table E1.
For the average treatment effect, columns (1) and (2) of Table E1, we restrict the analysis to the period after receiving feedback and average the weekly outcome from that moment to 210 weeks after being recruited. We estimate the following equation by OLS:

$$y_i = \beta_0 + \beta_1 SC_i + \beta_2 FB_i + \beta_3 SC_i \times FB_i + \theta X_i + \epsilon_i \tag{4}$$

As before SC_i is an indicator taking value 1 if household i was given the opportunity to sign a soft commitment. Similarly, FB_i is the indicator for household i being in the group receiving feedback. As before, X_i is a set of household-specific controls (MENUCO, number of household members, and household income) and fixed effects (zip code and recruitment week). When including pre-intervention data, X_i also includes pre-recruitment level of the outcome variable. Finally, ϵ_i is the error term. As in the main text, β_1 captures the average treatment effect of the pledge (now restricted to the period after receiving feedback). The coefficient β_2 is the average treatment effect of receiving recycling feedback and β_3 captures the difference of the effect of feedback for those who were offered to sign the pledge.
In columns (3) and (4) of Table E1, we repeat the analysis including the whole period after being recruited (i.e. the same time period in the main analysis of the soft commitment intervention). Consequently, we add a dummy variable taking value 1 for the period after receiving feedback

Table E1: Soft-Commitment and Feedback

	(1)	(2)	(3)	(4)	(5)	(6)
SC	0.0540***	0.0466**	0.0800***	0.0774***	0.0591***	0.0397*
	(0.0193)	(0.0211)	(0.0257)	(0.0269)	(0.0196)	(0.0216)
Feedback (FB)	0.0358*	0.0286	0.0257	0.0182	0.0351*	0.0265
	(0.0196)	(0.0214)	(0.0266)	(0.0276)	(0.0197)	(0.0220)
SCxFB	-0.0170	-0.0165	-0.00846	-0.0106	-0.0318	-0.0239
	(0.0282)	(0.0308)	(0.0367)	(0.0388)	(0.0289)	(0.0317)
Pre-int.		0.332***		0.436***		0.368***
		(0.0451)		(0.0364)		(0.0449)
After feedback(After)			-0.145***	-0.138***		
			(0.0146)	(0.0159)		
SCxFBxAfter			-0.00514	-0.00212		
			(0.0282)	(0.0313)		
SCxAfter			-0.0263	-0.0304		
			(0.0200)	(0.0222)		
FBxAfter			0.00936	0.0109		
			(0.0208)	(0.0225)		
SCxFB						
15 weeks after (WA)					0.113***	0.0951***
					(0.0153)	(0.0160)
15-50 WA					0.0627***	0.0511***
					(0.0118)	(0.0120)
51-100 WA					0.0398***	0.0348***
					(0.00838)	(0.00864)
SCxWA_15					-0.00977	0.0193
					(0.0220)	(0.0238)
SCxWA_15_50					-0.0119	0.00706
					(0.0178)	(0.0192)
SCxWA_51_100					-0.00356	0.0133
					(0.0123)	(0.0137)
FBxWA_15					0.00619	0.0186
					(0.0227)	(0.0250)
FBxWA_15_50					-0.00675	-0.00913
					(0.0175)	(0.0183)
FBxWA_51_100					0.00549	0.00797
					(0.0130)	(0.0140)
SCxFBxWA_15					0.0503	0.0240
					(0.0327)	(0.0360)
SCxFBxWA_15_50					0.0421	0.0330
					(0.0261)	(0.0284)
SCxFBxWA_51_100					-0.00176	-0.00904
					(0.0182)	(0.0203)
Constant	-0.0177	0.0775	0.120	0.227**	-0.0932	0.0129
	(0.0940)	(0.121)	(0.0893)	(0.114)	(0.0929)	(0.123)
N	1483	1153	2966	2306	5932	4612
r2_a	0.0261	0.0976	0.105	0.191	0.0640	0.130
Controls	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES
Time Period	After feedback	After feedback	After recruitment (diff-in-diff)	After recruitment (diff-in-diff)	After feedback	After feedback

Notes: Estimation of equations (4) (columns (1) and (2)), (5) (columns (3) and (4)) and (6) (columns (5) and (6)) with the proportion of weeks recycling after the intervention as the dependent variable. All columns include ZIP-code and Intervention week fixed effects (FE) and household controls. Household controls include the number of people living in the household (*Size*), the Minimum Expected Number of Uses of the Container per week (*MENUCO*), the bin installation phase (*Phase*) and household income (imputed from the census tract, *Income*). *Pre-Int* refers to the average value of the outcome variable up to 40 weeks before the intervention (only available for a sub-sample). Robust standard errors in parentheses (clustered at household level for columns (3) to (6)). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

($After_{it}$). The analysis becomes a difference-in-difference where we estimate the following equation by OLS, clustering standard errors at household level:

$$\begin{aligned} y_i = \beta_0 + \beta_1 SC_i + \beta_2 FB_i + \beta_3 SC_i \times FB_i + \beta_4 After_{it} + \beta_5 After_{it} \times SC_i + \beta_6 After_{it} \times FB_i + \\ + \beta_7 After_{it} \times SC_i \times FB_i + \theta X_i + \epsilon_i \end{aligned} \tag{5}$$

Now β_1 captures the average treatment effect of the pledge before receiving feedback while β_5 does so for the period afterwards. The coefficient β_2 and β_3 are the effect of the feedback before being received and its interaction with the pledge. thus these two coefficients work as randomization checks and should be not-significant. Finally, β_6 measures the average treatment effect of receiving feedback while β_7 captures the interaction between soft commitment and receiving feedback.

Finally, in columns (5) and (6) of Table E1 we also estimate a dynamic version of feedback and soft commitment by breaking the period after receiving feedback in four parts: the first 15 weeks after receiving feedback, weeks 16 to 50, weeks 51 to 100 and weeks 101 to 155. We estimate the following by OLS clustering standard errors at household level:

$$\begin{aligned} y_{it} = {} & \beta_0 + \beta_1 SC_i + \beta_2 FB_i + \beta_3 SC_i \times FB_i + \rho_1 W_{it}^{1-15} + ... + \rho_3 W_{it}^{51-100} \\ & + \beta_4 SC_i \times W_{it}^{1-15} + ... + \beta_6 SC_i \times W_{it}^{51-100} + \beta_7 FB_i \times W_{it}^{1-15} + ... + \beta_9 FB_i \times W_{it}^{51-100} \\ & + \beta_{10} SC_i \times FB_i \times W_{it}^{1-15} + ... + \beta_{12} SC_i \times FB_i \times W_{it}^{51-100} + \theta X_i + \epsilon_{it} \end{aligned} \tag{6}$$

As in the main text where W_i^j takes value 1 when the observation comes from period j and zero otherwise. The omitted group is the time period between weeks 101 and 155 and thus β_1, β_2 and β_3 captures the average treatment effect of soft commitment, feedback and the interaction between the two in our longest time horizon.

The estimates in Table E1 mostly display null results for the feedback intervention, with the exception of columns (1) and (5), where we find a positive effect at 10% level. Importantly, the interaction between soft commitment and feedback is non-statistically significant in all our specifications, disregarding the possibility that feedback can explain the lasting effect of the soft commitment. At the same time, this null result suggest that feedback cannot be used for reinforcing the soft commitment effect. Finally, we observe that soft commitment displays a positive result in all the specifications confirming the positive and lasting effect documented in the main text.