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Pro-environmental interventions and behavioral spillovers: Evidence from organic waste sorting in Sweden[☆]

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ABSTRACT

This paper evaluates the behavioral spillover effect of organic waste sorting on households' waste reduction. We use an administrative household-level dataset on residential waste from a Swedish municipality. To identify the spillover effects, we utilize a natural experiment triggered by the staggered implementation of a policy that introduced home-based organic waste sorting bins. We find a substantial positive spillover effect of waste sorting on waste reduction. The effect, however, disappears over time.

1. Introduction

Conserving resources through the reduction of incinerated and landfilled waste has been a major priority for policy makers since the 1980s; see, e.g., Vining and Ebreo (1989). The direct link between waste and reduction of carbon dioxide emissions, as well as the use of natural resources, has given waste a pivotal role in achieving several of the UN's Goals of the 2030 Agenda for Sustainable Development (SDG). As an example, SDG 12.3 states that by 2030 food waste should be halved at both retail and consumer level.¹

However, countries struggle to reach these goals. The amount of municipal solid waste per capita in EU-28 has increased every year since 2013 and in September 2018 an investigation concluded that 14 member countries were at risk of missing the 2020 target of 50% reuse/recycling (European Commission, 2018).

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¹ As further examples, SDG 11.6 requires more efficient waste management practices in cities and SDG 12.5 targets a substantial reduction in general waste by 2030.

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Thus, governments are under pressure to make households reduce their waste and increase their reuse and recycling rates. However, recent research has suggested that policies designed to target a certain pro-environmental behavior (in the following, referred to as PEB 1) might impact a behavior not originally targeted by the policy maker (PEB 2), see e.g. [Truelove et al. \(2014\)](#). Such an impact, commonly referred to as a behavioral spillover, can be positive or negative. As an example, water conservation efforts may be associated with higher energy consumption ([Dorner, 2019; Tiefenbeck et al., 2013](#)), while spillovers among activities using the same resources are typically positive ([Margetts and Kashima, 2017](#)). While positive spillovers enhance the effect of the policy in the desired direction, negative spillovers reduce the total effect and may even lead to an inferior final net equilibrium ([Ek, 2018](#)). Understanding policy spillover effects across behaviors is thus particularly important in an environmental context, where agents' contributions to a common social goal can be achieved through multiple activities.

In this paper, we study the relationship between household waste separation and the amount of waste produced by households. To do so, we evaluate a policy that introduced curbside collection of organic waste in Partille, a Swedish municipality. The objective of the policy was to separate organic from non-organic solid household waste. In this context, enrollment in the new waste separation system represents the targeted PEB 1, while generation of waste represents the non-targeted PEB 2.

We use a dataset on household waste that has several unique features. First, it contains the exact amount of waste produced each month by each household from 2012 to 2017. Second, the dataset is administrative and measurements are generated by a scanning device attached to each waste truck, and hence, household waste is objectively measured. Third, a crucial advantage of our dataset is that we observe the precise timing of the policy roll-out, including the dissemination of information regarding its implementation. In particular, we observe (i) the date each household was informed about the content and purpose of the future reform and (ii) the date each household had the option to enroll in a 2-bin waste separation system or remain with the old system where all waste is put into one single bin. Accounting for when households are exposed to information is important as it allows us to capture possible anticipation effects. This, in turn, makes it possible to precisely measure the marginal effect of the policy implementation, as well as the total effect of information and implementation.

Our identification strategy utilizes a natural experiment triggered by the staggered policy implementation in the municipality. In particular, the implementation started at different times in four different sub-regions of the municipality. This geographical division and the timing of the introduction was adopted merely to spread out the work for the waste authority and did not overlap with other administrative partitioning or policy changes. Thus, the staggered implementation provides a source of exogenous variation. To identify the treatment effect of the policy, we compare treated with not-yet-treated outcomes in a staggered difference-in-difference framework, see e.g. [Cameron and Trivedi \(2005\)](#). This strategy resembles the evaluation of phased-in controlled experiments, see [Duflo et al. \(2007\)](#). It allows us to estimate the impact among targeted and complying households and to distinguish between the effect of information and the effect of enrollment in the sorting scheme.

We find that both the policy announcement and the actual implementation of the new waste system significantly reduced the amount of waste generated by households. Our estimates indicate that the total amount of monthly waste reduction was 2.5 kg per household, which is 8% of the average amount. Thus, our results suggest that the impact of the policy was economically significant. Furthermore, an analysis of the heterogeneity of the effect indicates that the highest impact of the policy was achieved for the upper quantiles of the waste distribution. Finally, we also analyze how the policy effect changed over time. We find that an immediate and sizeable drop in household waste was followed by a return to pre-policy levels within eight months. Taking into account the benefits of using organic waste as biofuel, a cost-benefit analysis suggests that it takes approximately five and a half years for the policy to pay off.

We contribute to the literature on behavioral spillovers in an environmental policy context in terms of both findings and methodology.² On the methodological side, a majority of studies have used lab experiments or surveys with self-reported outcomes. Roughly 75% of the papers reviewed by [Maki et al. \(2019\)](#) perform a lab experiment, while all but one of the remaining 25% are based on surveys in which participants self-assess their pro-environmental behavior.³ Lab experiments are prone to a variety of behavioral biases such as the Hawthorne effect; see, e.g., [Galizzi and Whitmarsh \(2019\)](#). Self-assessed outcomes, on the other hand, suffer from measurement errors which in turn result in systematic biases ([Bound et al., 2001](#)). The latter problem is particularly pronounced in situations with activities characterized by low salience such as waste generation. Our study is based on a real policy and on administrative data with observed outcomes resulting from actual behavior. Thus, while our quasi-experimental source of identifying variation still guarantees causal interpretation of the estimates, our results do not suffer from the above mentioned problems.

Furthermore, as highlighted by [Maki et al. \(2019\)](#), a majority of the studies are under-powered (the typical sample size being between 100 and 200) and the corresponding estimates insignificant. Our dataset contains information on roughly 4300 households followed over 60 months, yielding a balanced sample with over 270000 observations.

In addition, to our knowledge, this is the first paper on behavioral spillovers in the context of an actual policy intervention that explicitly incorporates ex-ante effects resulting from anticipation of the policy. Existing empirical evaluations of actual environmental policies typically cannot account for pre-policy information effects because the date of the policy announcement is not observed; see, e.g., [Ek and Miliute-Plepiene \(2018\)](#). Taking into account anticipatory effects is an important contribution because their omission threatens the validity of the (staggered) difference-in-differences approach ([Angrist and Pischke, 2008; Goodman-Bacon, 2018](#)).

Finally, as pointed out by [Galizzi and Whitmarsh \(2019\)](#) and [Maki et al. \(2019\)](#), the literature has not yet agreed upon a definition of a behavioral spillover. In particular, some studies [1] directly evaluate the effect of the policy on the nontargeted PEB 2, while

² For recent surveys of this literature, see [Truelove et al. \(2014\)](#), [Nilsson et al. \(2017\)](#) and [Maki et al. \(2019\)](#).

³ A comprehensive methodological review is provided by the study of [Galizzi and Whitmarsh \(2019\)](#).

others [2] require that the policy intervention has induced an actual change in PEB 1 for a spillover on PEB2 to be possible or [3] simply estimate correlations between PEB 1 and PEB 2. We build a formal causal framework that helps distinguish between these definitions and discuss the assumptions necessary to identify spillovers according to each one of them. While some recent papers recommend using definition [2], it is also the most demanding in terms of identification. In particular, in real-world policy implementations, engaging in PEB 1 is a choice made by the individual. Thus, measuring the effect of PEB 1 on PEB 2 is potentially hampered by endogeneity. Our paper contributes to the empirical behavioral spillover literature by recognizing this problem and linking it to the econometric literature on noncompliance. We estimate the spillovers according to both definitions [1] and [2], and for the second definition, we use the policy implementation as an intention-to-treat variable to instrumentalize for the endogenous enrollment decisions of the individuals.

On the findings side, our results suggest that the mechanism responsible for the positive association between waste separation and reduction is of temporary nature. Few papers in the related literature evaluate the evolution of spillover effects over time (Maki et al., 2019).⁴ Furthermore, our paper provides additional insights on the relationship between the similarity of PEB 1 and PEB 2 and the sign (and size) of the spillover effect. In the literature, positive spillovers have typically been discussed in the context of consistency theory (Rabin, 1994). A major hypothesis of this theory is that an individual who engages in a pro-environmental behavior keeps behaving pro-environmentally to maintain a consistent self-image. One prediction of this theory is that a higher perceived similarity of behaviors is associated with positive spillovers. Our results are in line with this prediction since there is a strong conceptual link between waste separation and reduction. In addition, we discuss limited attention as an alternative driver of the spillover effect. Due to limited attention, repetitive activities such as waste-related behaviors are typically performed in a “default way” without the conscious attention of the individual; see Gabaix (2019) for references. A policy reform targeting a low-salience PEB 1 might therefore induce an attention boost towards a conceptually related low-salience PEB 2, thus generating a spillover (according to definition [2] above).⁵ The time pattern of the spillover effect is compatible with decreased attention and a resulting return of the behavior back to its default value. We show in our paper that this “correlated attention shock” is straightforward to incorporate into a behavioral microeconomic model.

Finally, we also contribute to the literature on household waste (reduction) behavior. To the best of our knowledge, our paper is the first to use observational waste data at a household level. Existing studies use either lab experiments (Xu et al., 2018), aggregate (on community or municipality level) data (Allers and Hoeben, 2010; Buccioli et al., 2015; Bueno and Valente, 2019; Usui, 2008; Wright and Halstead, 2011; Vollaard and van Soest, 2020), or survey data with self-assessed amounts of waste (Jenkins et al., 2003; Whitmarsh et al., 2018, 2017; Thomas et al., 2019). Data pooled at the geographical or administrative unit level does not allow for the analysis of treatment effect heterogeneity. In addition, the units of aggregation (such as municipalities) are often very different in terms of infrastructure and socioeconomic composition, which hampers the econometric identification of the policy effect.⁶ Furthermore, our paper is among the very few studies evaluating the relationship between waste separation and waste reduction. The study that is closest to ours is the one by Ek and Miliute-Plepiene (2018), who analyze the effect of organic waste separation on recycled waste in Sweden using aggregate municipality-level data. They estimate that organic waste separation increases the overall weight of packaging items disposed at dedicated municipal recycling stations. They are not able to account for possible anticipation effects of the policy. They also do not observe individual compliance behavior, implying that they are able to only estimate spillovers according to definition [1] above. Finally, their data does not allow them to study the household response to the policy implementation in terms of organic and residual waste behavior. Thus, our analysis provides novel results that crucially complement the existing evidence: we show that, in response to a policy that promotes the recycling of organic waste, households decrease their overall waste, net of that disposed of at recycling stations. Taken together, the two studies suggest that the introduction of organic waste separation generated higher recycling *and* a reduction of non-recycled and non-composted waste. Another closely related paper is that by Sintov et al. (2019), who find a positive spillover of waste composting on waste prevention.⁷ While their evaluation deploys a field experiment, their outcome measures (composting and reduction) are based on self-assessment within a survey. In addition, since participation in the survey is on a voluntary basis (and encouraged with monetary incentives), the sample selection potentially reduces the external validity of their results.

The paper is organized as follows: Section 2 describes the setting, the policy design and implementation, and the data. Section 3 presents the empirical strategy. Section 4 reports our main results and Section 5 the robustness checks. Section 6 discusses the economic interpretation of our findings and Section 7 presents a cost–benefit evaluation of the policy. Section 8 concludes with policy implications. Supplementary information and additional results can be found in the online appendix of the paper.

2. Background and data

2.1. Institutional background: municipal waste collection before the policy

Swedish municipalities have gradually introduced curbside collection of household organic waste since the 1990s. The context of our analysis is the municipality of Partille, where the waste management authority implemented a policy that introduced the

⁴ See Vollaard and van Soest (2020) for a study on habit formation in the context of waste separation.

⁵ This mechanism is related to the accessibility mechanism discussed by Sintov et al. (2019).

⁶ The potential pitfalls associated with self-assessed outcomes were discussed above.

⁷ A third related paper is the descriptive, survey-based, study by Miliute-Plepiene and Plepys (2015). They find a positive relationship between organic waste sorting and waste reduction behavior.

separation of organic waste in a staggered way along different collection areas, starting in the summer of 2013. The municipality is situated in the south-west of Sweden, on the outskirts of the city of Gothenburg. It consists mainly of residential detached single-family houses. Recyclable waste such as plastic, paper, glass, and metal can be disposed of free of charge at the municipal recycling centers (currently 32). Residual and organic waste is collected door-to-door by a truck that weighs and records the weight of each household's bin. Prior to the implementation of the reform, organic and residual waste was not collected separately, and hence only the total amount of waste (excluding privately composted waste as well as waste discarded at the recycling centers) per household was recorded. The pricing scheme for waste collected at the curb consists of a fixed and a variable component. Households pay a per-unit price for each kilogram of waste they produce (~ 0.20 USD/kg) and a fixed annual fee per bin that depends on capacity — the larger the bin, the higher the fee.

2.2. The policy

The policy consisted of three components: two information brochures, sent to the households per mail, and the actual roll-out of the policy, i.e., the delivery of organic-waste bins, which initiated the collection of organic waste.

The first brochure. The municipality sent a first brochure to all households to inform them about the option of having an additional bin dedicated to organic waste. The brochure also presented an average composition of household waste by type (26% recyclable packaging items, 29% organic, and 45% residual) and illustrated the benefits and pro-environmental consequences of organic waste recycling, such as its transformation into biofuel for use by public transportation buses in a circular system. Furthermore, it described the future enrollment and collection process and provided information on the costs of the different bin options.

The second brochure. The second brochure specified the starting date of the distribution of bins for each area and the costs for each possible bin combination. It also described how the households should indicate their preferred collection option and bin size (details on these options are given below). Finally, the second brochure asked the households to make their choice and to communicate it to the municipality by mail or email by a certain date.

A detailed description of the content and structure of the two brochures can be found in section A.1 in the online appendix.

Content of the reform: new waste separation system, bin sizes and costs. In the new regime, households could choose between three different options: (i) an organic-waste bin and a residual bin (henceforth, the “double-bin” option), (ii) only a residual bin (meaning that the household has a private composting device), and (iii) an unsorted bin, which corresponded to the pre-policy status quo for households that were not composting organic waste on their own. In addition to the three possible waste sorting regimes, households could choose the size of each bin (190, 240, 370, or 660 L) and the frequency of collection (weekly, biweekly, monthly, or less often). The latter two options – size of the bins and the frequency – existed already before the policy. For a capacity of 190 L and biweekly collection (the most prevalent choice), the old and the new price for the unsorted bin is equivalent to 230 USD/year (2255 SEK.), while using the (newly introduced) residual bin costs 120 USD/year (1127.50 SEK), and there is no fixed annual cost for the organic-waste bin. Importantly, the policy maintained both the pre-existing pay-by-weight pricing scheme and the possibility of discarding nonorganic recyclable waste items at the municipal recycling centers free of charge. The only novelty concerning costs is, therefore, the introduction of a different annual price for each bin type, favoring organic waste separation. Households that sign up for the double-bin or the residual regime save about 110 USD/year compared with the default unsorted-only scheme.

As explained in the brochures, if households do not actively report their preferred option, they are assigned to the pre-policy status quo (i.e. to the unsorted-only scheme). Households that sign up for the double-bin option are only allowed to throw organic waste into the organic-waste bins. If collectors find the wrong type of waste in a bin, they issue a warning. If the household continues to violate the waste rules, the authority can put the household back to the unsorted scheme.⁸ Any other type of household waste can be thrown in residual and unsorted bins, including packaging items and paper.⁹

Timing and implementation of the reform. All households received the first brochure in July 2013. The second brochure and the actual implementation followed a staggered design. In particular, the municipality was divided into four administrative subareas (denoted simply as Area 1 - Area 4), see Fig. 1. The timing of each of the two components – the second brochure and the actual implementation – differs between areas but not within an area. Each household was informed in brochure 2 about the timing of the enrollment in its respective area. Table 1 reports the precise timing of the policy implementation for each area. There is variation in the timing of the brochure and the implementation, as well as in the time length between the second brochure and the implementation.

The division of the municipality into four subareas, as well as the timing and the order of implementation of the policy followed purely organizational considerations and were not related to waste generation. In particular, detailed discussions with the municipality waste management authority revealed that the division of the municipality into four policy areas followed natural borders: there is a small river between areas 2 and 3, a small forest between areas 3 and 4, and a larger forest between areas 3 and 1, see Fig. 1. Furthermore, the order in which the areas were treated followed the simple rule “from east to west”.

⁸ Evidence of a change in collection regime in our sample is limited to 0.46% of all post-policy observations. Excluding those observations or dropping the corresponding households from the sample does not affect our estimations. Results are available upon request.

⁹ Electric appliances and dangerous items such as chemicals are excluded.



Fig. 1. The four areas of staggered organic-waste collection implementation in the municipality of Partille (Sweden).
Source: Partille Municipality.

Table 1

Timing of implementation by area.

Source: Authors' elaboration from information obtained by the waste management agency of the Partille municipality.

	First brochure	Second brochure	Org. waste bin distribution	Official start	Actual recording
Area 1	2 July 2013	6 Sept 2013	Nov 2013	25 Nov 2013	Feb 2014
Area 2	2 July 2013	20 Jan 2014	9 May 2014	19 May 2014	June 2014
Area 3	2 July 2013	4 Aug 2014	Oct 2014	10 Nov 2014	Dec 2014
Area 4	2 July 2013	16 Jan 2015	24 April 2015	11 May 2015	June 2015

2.3. Data and descriptive statistics

Our data source comprises the administrative registry of Partille's waste management authority. The data reports the weight of each waste bin (in half kilograms) for each collection, from January 2012 to May 2017. It also includes address and area of collection, bin type (unsorted, residual, or organic), size, weight, and date and time of collection, with unique household and bin identifiers. To match each bin with the respective household, we limit the analysis to single-family dwellings.¹⁰ Our final sample includes 4324 households with 270,475 household-month observations.¹¹

The average amount of monthly waste per household over the entire period is 31 kg, with a standard deviation of 20.7 kg and a maximum of 148.5 kg. Table 2 reports descriptive statistics according to different types of bin choices. 70% of the households adopted the double-bin option when the policy was introduced, while 18.2% opted for only a residual waste bin (and hence for private composting), and 12% continued with the unsorted bin option. Table 2 also reports the average weight before and after the policy for these three groups. "Unsorted" households produced on average 37 kg of waste before the policy change, "double-bin" households 35 kg, and "residual only" households 20 kg (which suggests they may already have had a composting bin). For each of the three groups, the post-policy average amount of waste is smaller than the pre-policy amount. Table A.1 in Appendix A.3 shows

¹⁰ A "single family house" means that only one family/household uses the bins. That implies we can allocate waste bin weights to a specific family/household. Houses with more than one apartment use common bins so it is not possible to know how much waste individual households dispose of — which is the main reason why those bins are excluded from the study. The exclusion of houses where more than one family resides was made at the data extraction stage and in addition, manual exclusions were performed in those cases where it was judged that more than one family could reside.

¹¹ See appendix A.2 for details of the sample construction process.

Table 2
Descriptive statistics.

Household's waste (kg/month) by sorting choice			
	Residual only	Double bin	Unsorted
Number of observations	49 204	188 656	32 812
Number of households	785	3015	524
% of all households	18.2%	69.7%	12.1%
Pre-policy mean waste/hh/month (kg)	19.802	34.850	37.236
Std. dev.	13.26	16.453	16.751
After-policy mean waste/hh/month (kg)	16.122	31.834	34.587
Std. dev.	11.785	15.625	15.896

Descriptive statistics from the Partille municipality's administrative waste collection data (2012–2017). *Double bin* refers to households that adopt an organic-waste and a residual bin.

Table 3
Household's waste (kg/month) by baseline quantile.

Quantile of baseline waste distribution	Average pre-policy waste	Average post-policy waste	Difference post-pre waste	Compliance rate (enrollment)	Number of households
Q1 (0%–25%)	12.258	13.280	1.022	0.947	1080
Q2 (25%–50%)	25.102	24.109	−0.993	0.911	1080
Q3 (50%–75%)	36.654	32.888	−3.766	0.888	1080
Q4 (75%–100%)	55.643	46.922	−8.721	0.876	1078

Descriptive statistics for the quantiles of the waste distribution before the first brochure (baseline waste distribution). The unit of observation is the monthly waste (in kg) in a household.

the composition of bins and waste separately for all four areas and figure A.1 (also in Appendix A.3) illustrates the evolution of waste over time before and after the reform.

Finally, [Table 3](#) presents a characterization of the quantiles of the baseline monthly waste distribution. In particular, we divide the observations into four groups based on the pre-policy (i.e., before the first brochure, baseline) waste weight, see column 1 of [Table 3](#). Column 2 contains the average pre-policy amount for each group, while column 3 presents the average post-policy amount (for the same groups of households as in column 2). Column 4 presents the share of households in each baseline quantile that opted to enroll in the new bin system. Two patterns emerge from this table. First, the difference in average waste weight between pre-policy and post-policy is larger in magnitude for higher baseline quantiles. Second, the rate of compliance (the percentage of households that opted to enroll in the new policy) is higher for lower quantiles.

3. Definitions of spillover effects and empirical strategy

3.1. Treatment effects of interest and definitions of behavioral spillovers

Let the random variable $B_{1it} \in \{0, 1\}$ indicate whether in month t a household i has (already) received the first information brochure, with $B_{1it} = 1$ when the brochure is received. Define B_{2it} analogously for the second brochure. Let P_{it} be a binary variable indicating whether in t the new waste separation system has (already) been implemented in the area in which household i is located. By *implementation*, we mean the actual delivery of organic waste bins and the collection of organic waste from these bins. Let D_{it} denote the actual enrollment indicator of household i with $D_{it} = 0$ corresponding to “not (yet) enrolled” (i.e., the household has an unsorted bin) and $D_{it} = 1$ corresponding to “enrolled” (i.e., the household adopts either an organic + a residual bin or only a residual bin). While $P_{it} = 1$ indicates that household i can participate in the new system at time t , $D_{it} = 1$ means that it actually does.¹² Importantly, D_{it} represents the behavior targeted by the policy (PEB1). Finally, let Y_{it} be the waste produced by household i in month t . Y_{it} represents the nontargeted behavior PEB2. Note that Y_{it} constitutes only the observed part (i.e. the waste collected at the curb) of the solid waste produced by each household. Waste that is either recycled in one of the municipal recycling centers or privately composted is not observed and thus not included in Y_{it} . We address the implications of these unobserved components for our estimates in section B.4 in the appendix.

With this notation, we can introduce the following treatment effects depicted as arrows in the causal graph in [Fig. 2](#). The first two possible effects of interest are the direct information effects of B_1 and B_2 on Y , denoted $TE1$ and $TE2$, respectively. $TE3$ represents the effect of actual enrollment in the new system D on produced waste Y . $TE4$ represents the effect of implementation of the new system P on the decisions of the households to enroll D . Thus, D plays the role of a mediator for P when the focus is

¹² Thus, P_{it} switches values in the period t in which the new system has been implemented in the area of household i , while D_{it} switches value in this period only if the household opts to enroll.

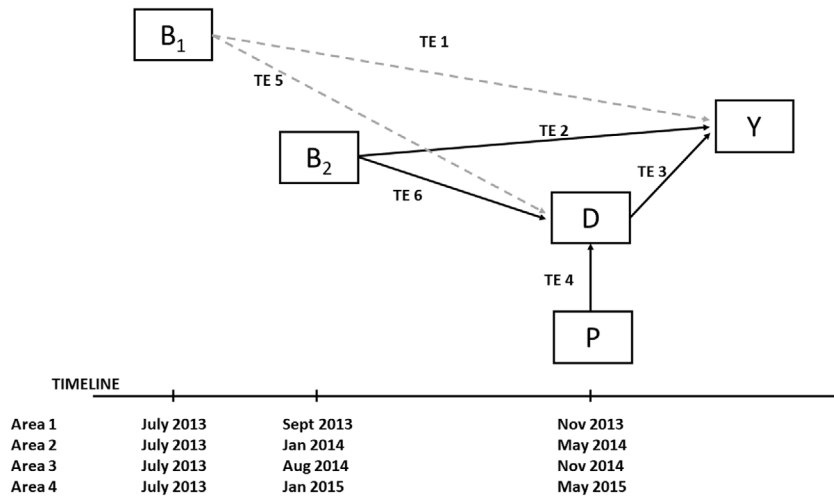


Fig. 2. Causal graph: treatment effects of interest. Notes: B_1 and B_2 correspond to the first and the second brochure; P is the implementation of new waste system; D is enrollment in the new system. Y is household's waste reduction. An arrow corresponds to a treatment effect. Black continuous edges are identified and estimated; effects represented by light gray dashed lines are not estimated. The figure depicts the time dimension of each policy component by area, where the realization of a given variable takes place at different times. Since Y is measured at a number of points in time for each area, its precise timing cannot be reported in the figure.

on the effect of P on Y , while P represents an intention-to-treat (ITT) variable when the focus is the effect of D on Y . Next, $TE5$ and $TE6$ represent the effects of the first and second brochure on the decision to enroll, respectively. Finally, the total policy effect on Y consists of the sum of the effects of B_1 , B_2 and P on Y .

What is a behavioral spillover? For the remainder of the discussion, it is necessary to clarify the meaning of behavioral spillovers in our paper. The meta-analysis by Maki et al. (2019) identifies three different definitions used in the literature. According to the first definition, a behavioral spillover occurs if an intervention P induces a change in a non-targeted behavior PEB2 (here, waste generation Y), regardless of whether there is an actual change in the targeted PEB1 (here waste separation D). A second definition requires that the intervention actually induced a change in the targeted behavior D . Finally, a third definition simply presupposes correlation between the targeted and the nontargeted behavior.

In our context, the first definition corresponds to the total effect of the policy on Y . We evaluate this effect using the model

$$Y_{i,t} = \beta_0 + \beta_2 B_{2,i,t} + \beta_P P_{i,t} + \alpha_i + \delta_t + \varepsilon_{i,t}, \quad (1)$$

where α_i is a household fixed effect, δ_t denotes month and year fixed effects, and $\varepsilon_{i,t}$ is the idiosyncratic noise. Because $P_{i,t}$ changes value at different points in time across four different areas, this is a staggered difference-in-differences (DiD) model; see, e.g., Cameron and Trivedi (2005). The spillover effect is represented by the sum of the coefficients β_2 and β_P . Note that we have omitted the variable B_1 from the equation above because it is absorbed by the time fixed effects.

The second spillover definition corresponds to the effect $TE3$. This is the definition preferred and recommended by Maki et al. (2019) as it describes an impact of one behavior (the targeted one) on another (the nontargeted one). We evaluate this effect using the staggered model

$$Y_{i,t} = \beta_0 + \beta_2 B_{2,i,t} + \beta_D D_{i,t} + \alpha_i + \delta_t + \varepsilon_{i,t}. \quad (2)$$

The spillover effect is now represented by the coefficient β_D . A major difficulty in identifying β_D is the endogeneity of PEB1. In particular, unlike in the case in which a policy intervention is (quasi-)randomized, PEB1 is a choice variable that depends on unobserved household characteristics. In our context, the decision to enroll might reflect unobserved preferences for pro-environmental outcomes or socioeconomic characteristics that affect the outcome variable Y . Thus, D is potentially endogenous, which complicates identification of its effect.

Each of these two definitions is relevant within a certain context. The first definition is relevant when the main question is what the total effect of an already implemented policy was. On the other hand, in the design phase of a new, not-yet-implemented policy, knowledge of the spillovers according to the second definition is crucial as it is necessary to determine the net effect of the policy.

3.2. Identification

Identification of the total effect of the policy (definition 1 of spillovers) Similarly to a standard two-groups \times two-periods DiD approach, the main assumption of the staggered DiD model (1) is the parallel trends assumption; see Goodman-Bacon (2018). This assumption is weaker than full randomization and it allows selection on unobservables into the different areas. We support the parallel trends assumption in two ways. First, we perform an evaluation of pre-treatment trends, estimating a variety of placebo

Table 4
Estimates of average treatment effects.

Outcome: household's waste (kg/month)				
VARIABLES	(1) FE	(2) IV2SLS	(3) First stage	(4) FE
Enrollment (β_D)	-1.219*** (0.249)	-2.121*** (0.203)		
Information (β_2)	-1.299*** (0.219)	-0.829*** (0.183)	-0.014*** (0.001)	-0.800*** (0.184)
Implementation (β_P)			0.894*** (0.005)	-1.896*** (0.181)
Observations	270,475	270,475	270,475	270,475
R-squared	0.037		0.890	0.037
F-stat first stage (Kleibergen-Paap):			36906.65	

Average treatment effects estimated with models (1) and (2). Column 1 reports the FE estimates for model (2), column 2 the instrumental variable (IV) estimation results for model (2), column 3 the corresponding First-Stage results, and column 4 the FE estimates of model (1). All regressions include household, month, and year fixed effects. Standard errors clustered at household level. Asterisks denote statistical significance at the 1(***), 5(**), or 10(*) percent level.

effects in Section 5.1. Second, as described in Section 2.2, the division of the municipality into four subareas, as well as the timing and order of implementation of the policy, followed purely organizational considerations and were not related to waste generation. Thus, it is plausible to assume that B_2 and P are exogenous to unobserved waste-related factors.

Identification of the effect of enrollment (definition 2 of spillovers) To identify the spillover effect in model (2), we need to deal with the potential endogeneity of the choice variable D . We estimate β_D in two ways. Our first approach is to estimate equation (2) with a standard fixed effects (FE) estimator. The underlying assumption is that all unobserved determinants of Y that are also related to D are time-invariant. To account for possible violations of this assumption, in our second approach, we estimate (2) with an instrumental variable (IV) FE estimator. We use the exogenous ITT variable P as an instrument for the endogenous treatment D . In particular, in our main results, we estimate a first stage

$$D_{i,t} = \gamma_0 + \gamma_2 B_{2,i,t} + \gamma_P P_{i,t} + \theta_i + \delta_t + \varepsilon_{i,t}, \quad (3)$$

where θ_i and δ_t are individual and time fixed effects, respectively. The predicted values \hat{D} are then used as regressors in Eq. (2) in a second stage estimation.

Remark. Note that under both Eqs. (1) and (2), the linearity assumption implies constant treatment effects. As a consequence, β_D represents the average treatment effect and the average treatment effect on the treated. We relax the linearity assumption as a robustness check in section B in the appendix.

4. Main results

4.1. Average treatment effects

Table 4 contains the results of our three main specifications.¹³ The fourth column reports the estimates of model (1). The estimated effect of P , β_P , is negative and statistically significant at the 1% level. On average, the implementation of the new waste system induced a 1.896 kg reduction in monthly waste per household. This represents roughly 6% of the average monthly household waste. The effect of the second brochure, β_2 , is also negative and significant and equal to -0.8 . Together, these two estimates capture the behavioral spillover according to the first definition in Section 3.1. This total behavioral spillover is equal to roughly 8% of the average monthly household waste and is thus of economically relevant magnitude.

The first column of Table 4 reports the FE estimates of Eq. (2). The coefficient for enrollment in organic waste separation β_D is negative and statistically significant at the 1% level. According to this estimate, the enrollment in the new system is associated with a reduction in household waste by 1.219 kg per month per household. This represents roughly 3.9% of the average monthly household waste. The effect of the second brochure β_2 is slightly larger in magnitude and amounts to a 1.299 kg, or 4.2%, reduction in monthly household waste. Together, the enrollment in organic waste separation and the second brochure induce an 8% decrease in produced waste. Note that according to the second definition of a behavioral spillover in Section 3.1, only the effect of D (i.e. β_D) is interpreted as a behavioral spillover.

The second column of Table 4 reports the IV estimates of model (2) with P as an instrument for D . The estimated coefficient $\hat{\beta}_D$ is negative and significant and equal to -2.121 . It is larger in magnitude than the $\hat{\beta}_D$ coefficient in the first column by almost 74%, and it corresponds to a spillover effect (according to definition 2) of roughly -6.79% . The difference between the two specifications

¹³ All regressions include household, month, and year fixed effects. Standard errors clustered at household level.

Table 5
Quantile regression, fixed effects estimation.

Outcome: household's waste (kg/month)				
	(1)	(2)	(3)	(4)
Quantiles:	0.25	0.5	0.75	0.9
Enrollment (β_D)	-0.85 (0.58)	-1.15*** (0.39)	-1.52*** (0.27)	-1.92*** (0.42)
Information (β_2)	-1.38** (0.68)	-1.31*** (0.45)	-1.23*** (0.31)	-1.14** (0.49)
Observations	270,475	270,475	270,475	270,475

Quantile FE estimates of treatment effects. Each column reports the coefficient corresponding to a specific quantile (the 25th, 50th, 75th, and 90th percentiles in columns 1, 2, 3, and 4, respectively). All regressions include household, month, and year fixed effects. Standard errors clustered at household level. Asterisks denote statistical significance at the 1(***), 5(**), or 10(*) percent level.

reflects the selection bias arising from the endogenous choice to enroll in the new system. The direction of the bias suggests that households producing less waste self-select into organic waste separation. This selection biases the non-instrumented FE results downwards. One possible explanation for the selection is that households that are more likely to enroll in organic waste separation may also be more efficient in producing lower amounts of waste, and thus have a lower capacity for waste reduction. Alternatively, these more efficient households might generally pay more attention to their waste behavior, so that the new policy does not trigger an attention effect. We support this interpretation with further analysis of the relationship between compliance, capacity for reduction and treatment effect size within a quantile regression framework in Sections 4.2 and 5.3.

Coefficient β_2 from the specification in the second column is also negative and significant, and is equal to -0.829 . This estimate is very close to the corresponding estimate in the fourth column but is smaller than the estimate of β_2 in the FE specification in the first column. The IV results from the second column suggest that enrollment in the policy and the second brochure together induce a 9.5% decrease in produced waste.

Column 3 contains the first stage estimates. The F-statistic is very high, which indicates that the instrument is strong. The coefficient of the instrument is equal to 0.894 and significant at the 1% level. Thus, the new waste system had a high take-up (TE 4).

4.2. Heterogeneous effects

Since we do not observe household-specific characteristics, we cannot estimate the dependency of the effect on pre-treatment characteristics of different subpopulations. Instead, we take an alternative approach and study heterogeneity with respect to the distribution of household waste generation. We follow two different approaches, each of which relies on different assumptions.

First, we estimate a quantile treatment effect model using the approach of Machado et al. (2011). This approach allows us to account for household and time fixed effects and follows similar logic as in our mean regression model: waste quantiles of the treated households are compared with the corresponding waste quantiles of the not-yet-treated households. The underlying assumption is that the individual fixed effects account for the selection into the different areas.

Table 5 displays the results. Each column represents a separate regression for the conditional 0.25, 0.5, 0.75, and 0.9 quantiles of household waste (kg). The effect of enrollment D is negative for all quantiles and significant for all but the 0.25 quantile. The coefficient for the median quantile (-1.15) is close to the one estimated at the mean in our main regression (-1.05 ; see Table 4). For larger quantiles (0.75 and 0.9), the coefficients are larger in magnitude than the mean regression coefficient (-1.52 and -1.92). The estimated effect for B_2 is also negative and it is significant for all quantiles.

Second, to account for possible endogeneity of D , we implement the IV quantile treatment effect estimator developed by Frölich and Melly (2010). Again, we use P as an instrument for D . We allow include time dummies but do not include individual fixed effects. The reason is that, unlike in the standard TSLS case, the fixed effects would not be eliminated from the regression, which implies that their coefficients are estimated. This would lead to the well documented incidental parameter bias.

The results are shown in Fig. 3. For each quantile, the point-estimate is displayed by a solid rectangle and the corresponding confidence interval by a vertical line. The estimates are all negative and significant at the 1% level, and the magnitude increases with the size of the quantile. This pattern is consistent with the pattern in the estimates obtained with the first quantile approach. Furthermore, for each quantile, the magnitude of the IV estimates is larger than their non-instrumented counterparts. This difference points in the same direction as the difference between the corresponding IV and FE results from Section 4.1 and is likely induced by a bias towards zero in the non-instrumented estimates.

We discuss potential problems with the interpretation of these results in Section 5.3.

4.3. Evolution of the effects over time

In this section, we evaluate the persistence of the spillover effects over time. Using an event-study approach, we regress the monthly household waste Y on the enrollment D interacted with post-policy monthly dummies. The results are presented in Fig. 4.

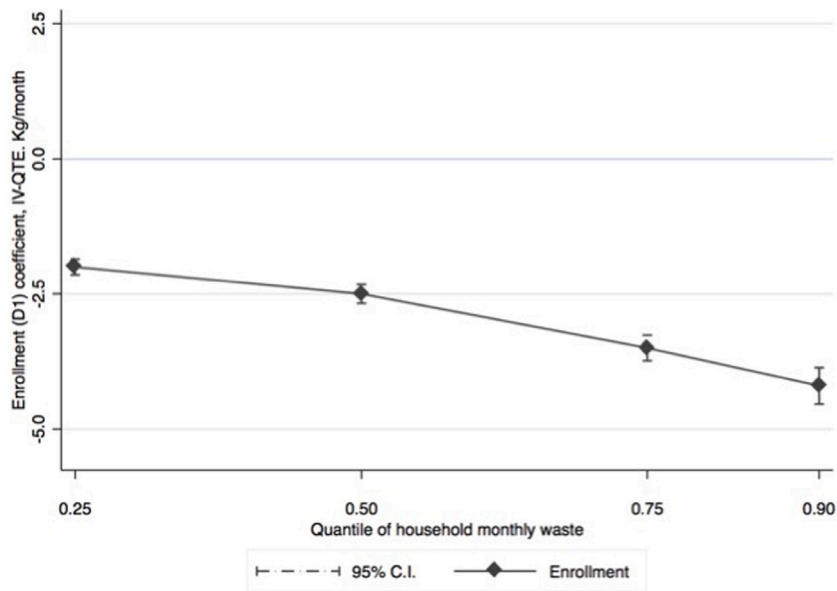


Fig. 3. Instrumental variable Quantile Treatment Effects estimation. The figure reports Quantile Treatment Effect instrumental variable coefficient estimates from the full sample of households in the Partille administrative waste collection data. Each dot represents the coefficient of enrollment in waste separation corresponding to a specific quantile (the 25th, 50th, 75th, and 90th percentiles), with the 95% confidence interval. All regressions include month, and year fixed effects.

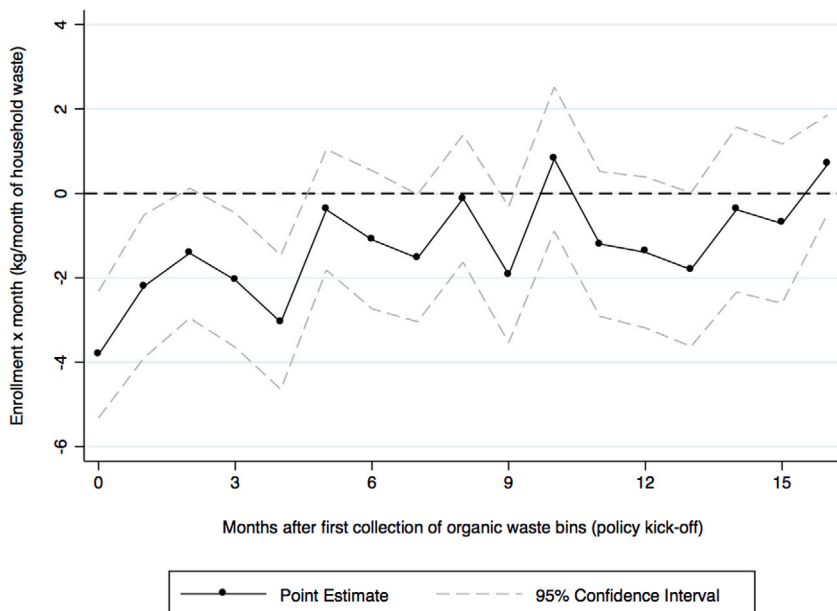


Fig. 4. Effect of D (enrollment into waste separation) on households' monthly waste over time. The figure reports the coefficient estimates and 95% confidence intervals of the interaction between enrollment in the policy (D) and a month-specific dummy variable for each period after the bins collection. The estimates are based on the full sample of households in the Partille administrative waste collection data. Month zero corresponds to the first collection of organic waste bins. Months 15 and above are pooled. The dependent variable is household monthly waste in kg, net of recycled packaging items. The regression includes household, month, and year fixed effects and the brochure dummy variable. Standard errors are clustered at household level.

The effect is negative and significant immediately for the first periods after the policy, which is consistent with our results in Section 4.1. However, it tends to decrease over time and becomes insignificant after 5 months. For all periods afterwards, it remains insignificant and close to 0 in magnitude.

To account for possible endogeneity of D , we re-estimate the event-study model of Section 4.3 but instrument for D with P . For each interaction between a monthly dummy and D , the instrument is constructed as the interaction of the same monthly dummy

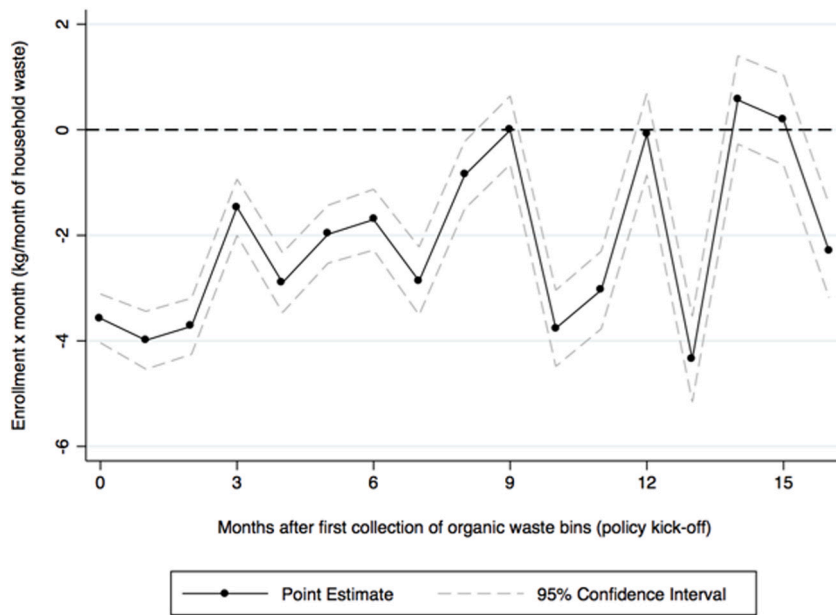


Fig. 5. Effect of D on household monthly waste over time: IV estimates. Estimates and 95% confidence intervals of the effect of the interaction of D (enrollment) and post-policy monthly dummy variables. These interactions are instrumented by interactions of P (implementation of organic waste bins distribution) and monthly dummies. The estimates are based on the full sample of households in the Partille administrative waste collection data. Month zero corresponds to the first collection of organic waste bins. Months 15 and above are pooled. The regression includes household, month, and year fixed effects and the brochure dummy variable. Standard errors are clustered at household level.

but with P . The results are shown in Fig. 5. The effect tends to slowly return to pre-policy level and becomes insignificant at month 8. After month 8, the majority of the effects are insignificant but there is a fuzzy pattern with alternating reductions in waste production and non statistically significant positive coefficients. The negative estimates appear to be decreasing over time. Thus, the effect of D is strongest in the first 8 months. A comparison of the instrumented and the non-instrumented results reveals that the latter have a smaller magnitude, which indicates a bias towards 0. This pattern is consistent with the findings in Sections 4.1 and 4.2.

We perform analogous analysis with the variable P . The results are displayed in Fig. 6. The pattern of the estimated marginal effects of P resembles closely the one found for D , again with strongest effect in the first 8 months. Similarly to the average results presented in Section 4.1, the magnitude of the effects of P are smaller than the magnitude of the effects of D .

Our findings are very similar to the results of Linder et al. (2018) who find that an effect of informational leaflet on the amount of recycled waste in the Swedish municipality Hökarängen, a suburb of Stockholm city. The novelty of in our case is that our estimates represent spillover effects and not behaviors directly targeted by an intervention. Our estimates are also consistent with existing evidence in the context of water and electricity consumption, which shows the impermanence of social norms-based intervention effects after a few months in absence of repeated treatments (Ferraro and Price, 2013; Allcott and Rogers, 2014).

5. Assessment of the plausibility of the assumptions and additional supporting results

5.1. The parallel trends assumption

The main assumption behind the (staggered) DiD approach in our main models (1) and (2) is the parallel trends assumption; see, e.g., Angrist and Pischke (2008) for the standard DiD design and Goodman-Bacon (2018) for its staggered counterpart. In this section we evaluate its plausibility.

Graphical analysis. We first perform graphical analysis. We divide the timeline into three pre-treatment time periods, each corresponding to a different definition of the treatment. For each of these three periods, we plot the average pre-treatment amount of waste for each of the four geographical areas (smoothed with a nonparametric polynomial regression). Fig. 7 (left) displays the graph for the first pre-treatment period, which is defined as the period before the first brochure (i.e., before July 13, 2013). The pre-treatment curves of all areas look roughly parallel and slightly downward sloping. Moreover, there is no spike in the outcomes just before the treatment, and therefore there is no evidence for anticipatory effects associated with the first brochure. Fig. 7 (right) displays the graph for the second pre-treatment period, which is defined as the period after July 13, 2013 and before each of the area-specific points in time of receiving the second brochure. To take into account the staggered timing of the second brochure, the x-axis now measures time before D (in months). The latest point on that graph thus represents -5 months, which is the shortest

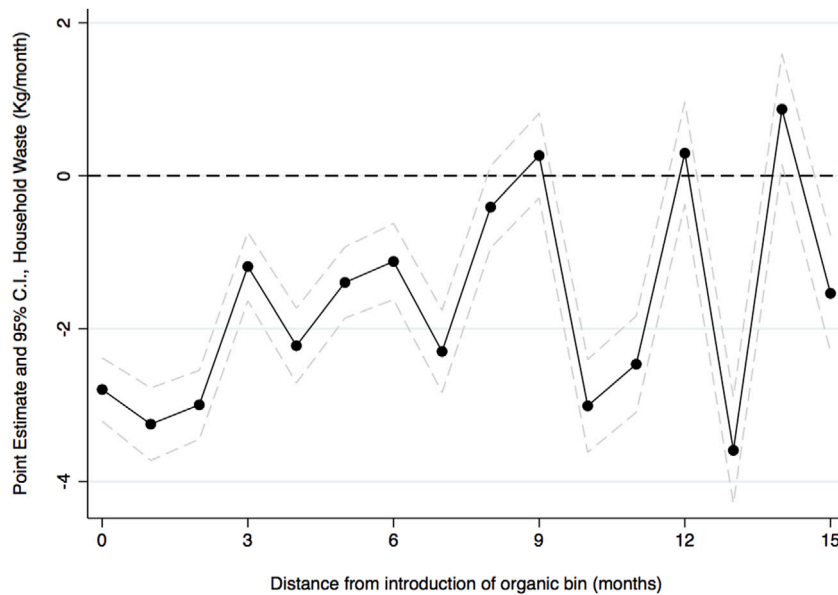


Fig. 6. Effect of P (organic waste sorting system implementation) on households' monthly waste over time. The figure reports the coefficient estimates and 95% confidence intervals of the interaction between the implementation of the new system P and a month-specific dummy variable for each period after the bins collection. The estimates are based on the full sample of households in the Partille administrative waste collection data. Month zero corresponds to the first collection of organic waste bins. Months 15 and above are pooled. The dependent variable is household monthly waste in kg, net of recycled packaging items. The regression includes household, month, and year fixed effects and the brochure dummy variable. Standard errors are clustered at household level.

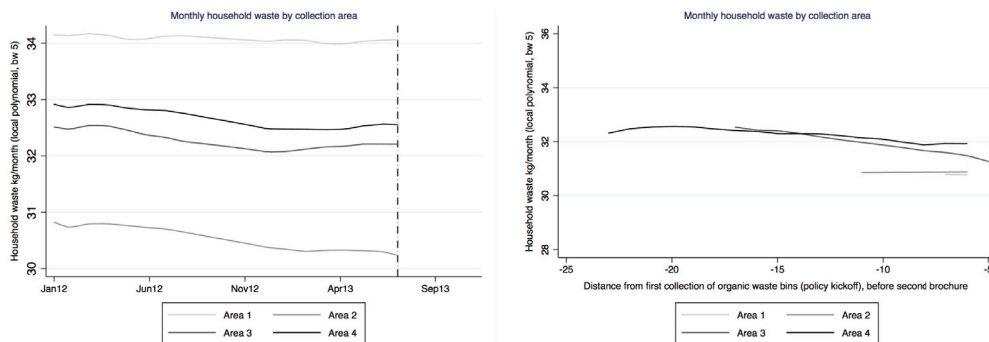


Fig. 7. Average household's waste (kg/month) in the four areas of staggered policy implementation for different pre-treatment periods. *Left panel:* calendar months prior to the first brochure. *Right panel:* period (months) after the first and before the second brochure. Authors' estimations from the full sample of households in the Partille administrative waste collection data. The depicted variable measures average monthly waste per household in kg. Estimation: local polynomial smooth with bandwidth 5.

interval between B_2 and D . Again, the pre-treatment curves of all four areas are nearly parallel and there are no spikes shortly before B_2 . The level has shifted down for some areas, which potentially captures the effect of the first brochure. Finally, Fig. 8 shows the corresponding graph for the third pre-treatment period, which includes the time after the second brochure but before the area-specific points in time of policy implementation. In this period, the trends appear to be near-parallel for Areas 1 and 2, as well as for Areas 3 and 4, but appear to be slightly different between these two groups. Similarly to the previous two cases, there is no evidence for anticipatory effects.

As a robustness check, we implement an event-study analysis on the waste differences across areas prior to brochure 1. This analysis can be found in section B.1 in the appendix.

Statistical analysis We now test whether differences in pre-treatment slopes are statistically significant. We do this in several different ways.

We implement a standard placebo-testing procedure: we repeat the estimation of model (1) but in the past. To be specific, for each household, the placebo treatment variable changes value a pre-specified number of periods, say l , before the actual realization of the treatment for that household. Importantly, the time lag l of the placebo treatment is common for all units of observation. As an example, $l = 12$ months corresponds to placebo P -treatment of all households from Area 1 in February 2013, because the actual

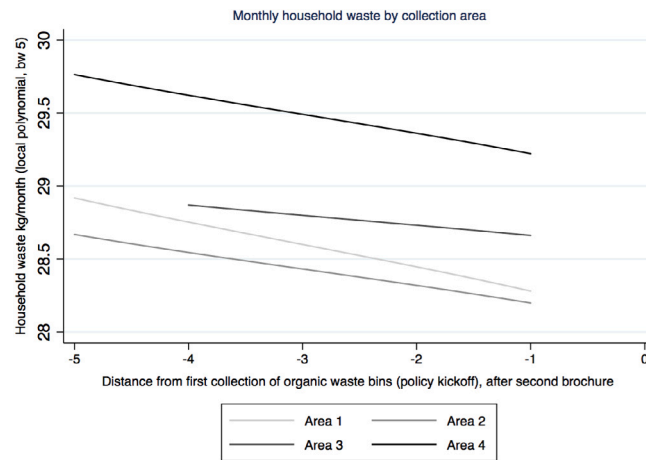


Fig. 8. Average household's waste (kg/month) by area in the pre-treatment period, after the second brochure but before enrollment. Authors' estimations from the full sample of households in the Partille administrative waste collection data. The depicted variable measures average monthly waste per household in kg. Estimation: local polynomial smooth with bandwidth 5.

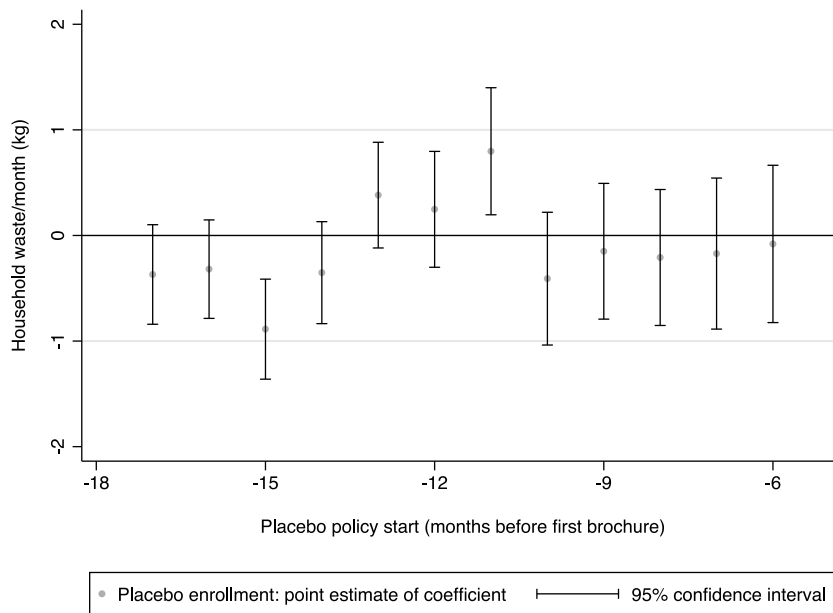


Fig. 9. Estimated placebo effects. The figure reports the coefficients and 95% confidence intervals from the estimations of a placebo replication of policy enrollment, lagged in the past in the pre-policy period (i.e., prior to the first brochure). The estimations are based on the full sample of households from Partille waste collection data. The policy implementation timing replicates the actual distance between brochure distribution and bins distribution across the four areas. The placebo treatment model is specified as in the main model (1): a staggered DiD equation including household, month, and year fixed effects. Each coefficient estimate corresponds to a repetition of the policy procedure for a different lag. Standard errors clustered at household level.

P for Area 1 realizes in February 2014 (see Table 1 for the timing of the reform). Similarly, this time lag corresponds to a placebo P in June 2013 for all households in Area 2. In all other aspects, the placebo treatment model is specified as our main model (1); specifically, household and time fixed effects are included in a staggered DiD equation. We repeat this procedure for different lags l . The estimates and the corresponding confidence intervals are plotted in Fig. 9. The majority of estimates are not significant. Thus, this test provides no evidence for non-parallel pre-treatment trends.

5.2. Interpretability of the staggered difference-in-differences estimates

Recent advancements in theoretical econometrics have shed light on the implicit assumptions behind the staggered DiD estimator, as well as on the pitfalls associated with violation of these assumptions. Goodman-Bacon (2018) shows that the staggered DiD

Table 6Two-groups \times two-periods difference in differences.

	(1) Outcome: household's waste (kg/month) Areas 1-2	(2) Areas 3-4	(3) Areas 2-3	(4) Areas 2-4	(5) Areas 1-4	(6) Areas 1-3
Treated	3.953*** (0.886)	-1.790** (0.909)	-1.821* (1.082)	-2.563*** (0.938)	0.352 (0.834)	-2.967** (1.154)
Post	2.389*** (0.482)	5.059*** (0.387)	-4.667*** (0.589)	-3.104*** (0.364)	-1.792*** (0.407)	-5.882*** (0.713)
Treated \times post	-10.21*** (0.717)	-3.562*** (0.728)	-0.679 (0.817)	-2.242*** (0.673)	-5.670*** (0.691)	-1.581* (0.906)
Observations	30 061	5549	2852	5481	5737	3112
Mean of dep. var.	32.28	32.83	30.01	31.66	29.88	29.83
St dev. of dep. var.	20.83	22.19	20.04	21.11	19.66	19.92

Standard 2 groups \times 2 periods DiD estimates of the effect of B_2 on waste Y . Each column indicates which two groups are selected for the estimation. Sample is reduced to one month before the second brochure and one month after the treatment of the first area in the pair. Observations between the second brochure and the collection of bins are dropped. Standard errors in parentheses are clustered at the household level. Asterisks denote statistical significance at the 1(***), 5(**), or 10(*) percent level.

estimator corresponds to a weighted sum of standard 2 groups \times 2 periods (2×2) DiD estimators, with the weights being proportional to the treatment variation (and hence, related to the time of treatment). De Chaisemartin and d'Haultfoeuille (2019) show that some of the weights might be negative, so that the staggered DiD estimate may be negative even if all the (2×2) DiD estimates are positive. This situation can arise if for example the 2×2 treatment effects are not time-constant or when the parallel-trends assumption is violated. Such an anomaly would hamper the interpretation of the staggered DiD estimate as a treatment effect. In Section 5.1, we provided support for the parallel-trends assumption. In this section, we provide further results that support the interpretability of our main results as treatment effects.

In particular, we evaluate all 2×2 treatment effects separately using standard DiD estimators. As an example, for the two groups ("Households in area 1", "Households in area 2"), we can perform a 2×2 DiD estimator by restricting the time horizon in a way such that group 1 plays the role of the treated (since it is treated earlier) and group 2 plays the role of the non-treated. Contrary to the staggered DiD estimator, it is sufficient that the parallel-trends assumption holds for this 2×2 DiD to have a clear causal interpretation.

We perform six different 2×2 DiD estimations: one for each of the area pairs (1, 2), (1, 3), (1, 4), (2, 3), (2, 4), and (3, 4). In each of these estimations, we restrict the sample to one month before and one month after each treatment B_2 .

Table 6 shows the results. Each column corresponds to one specification (i.e., to one pair of areas). The DiD coefficient is displayed in the third row of the table. All specifications yield a negative coefficient, and all but one of these estimates are significant. Thus, these estimates support our results produced with a staggered DiD estimator.

5.3. Heterogeneous effects

In this section, we address two potential problems with the interpretation of the results in Section 4.2. First, some households may change rank in the distribution of waste production as a result of the treatment. This would imply that the estimates cannot be interpreted as household-specific treatment effects. Second, we do not have information on household size and composition. Thus, we cannot distinguish between different reasons for the increasing magnitude of the quantile effects. As an example, this pattern could be due to a non-linear decrease in the waste generated by each individual, with smaller households occupying the lower part of the waste distribution and displaying smaller marginal household waste reductions. Alternatively, households generating less waste may have less capacity for a reduction. This would be the case particularly if the effect of sorting on household's monthly waste is driven by a boost in attention to waste-related activities — households that generally pay more attention to their waste behavior would be less affected by such an attention "shock". The effect can also be a combination of these two possibilities.

To assess whether the first problem exists, we estimate a measure of rank reversal using Wilcoxon's signed-rank test based on pooled pre- and post-intervention average measures of monthly waste per household. We estimate that 64% of the households maintain their quartile rank position, while 19% (17%) increase (decrease) it. We can reject the null hypothesis that households maintain their quartile rank at the 10 percent level, with a p -value of 0.073. Thus, a comparison of quantiles before the policy with the corresponding quantiles after the policy (a comparison that is implicitly performed by the quantile regression) cannot be interpreted as a household-specific treatment effect, since the treatment itself induces a change in the composition of the quantiles. Instead, the correct way to interpret the quantile regression estimates is that they represent effects on the waste distribution. Enrollment in the policy decreases all quantiles of the distribution, with larger effects for higher quantiles. The impact of the policy is particularly pronounced in the upper tail, which is a desirable outcome from a policy making perspective.

As a next step, we perform an additional estimation to investigate the relationship between the policy and quantiles of baseline waste production. In a linear mean regression framework with waste weight as a dependent variable, we interact the treatment (policy implementation, P) with a categorical variable that allocates households to four quantiles of *baseline* waste weight, computed from averages of all monthly collections before the distribution of the brochure. This robustness check serves two purposes. First, the estimates can be interpreted as an average household-specific treatment effect, which complements the quantile results under

Table 7

Organic waste separation policy and household waste (kg/month). Heterogeneous effects by quartiles of baseline waste production.

Outcome: monthly household waste (kg)	
	(1) FE All
Implementation P	2.227*** (0.289)
P * 2nd quartile	−2.017*** (0.382)
P * 3rd quartile	−4.818*** (0.389)
P * 4th quartile	−9.720*** (0.441)
Information (<i>B2</i>)	−0.787*** (0.181)
Observations	270,245
Number of hh	4,318
R-squared	0.055
Mean of dep. var.	30.97

Results from a fixed effects panel data regression of monthly household waste on the second brochure and the implementation of the new system P, which is interacted with four indicators of the households' respective quartiles of baseline (pre-policy) average monthly waste weight. We use the full sample of households from Partille waste collection data. The regression includes household, month, and year fixed effects. Standard errors in parentheses are clustered at household level. Asterisks denote statistical significance at the 1(***), 5(**), or 10(*) percent level.

a reversed rank. Second, baseline waste quantiles can serve as a proxy for household characteristics, such as preferences for the environment, efficiency in waste generation, as well as different household compositions and sizes. In this way, we address the problem of lack of data on household characteristics. While quantile regression yields the effect of the policy on a particular quantile, this approach identifies the marginal effect of *being* in a particular quantile.

Table 7 reports the results. The policy shows a positive effect in terms of waste reduction only starting from the second quartile. With respect to the coefficient relative to the first baseline quartile (2.2 kg), the interaction between the policy and the following quartiles is always significant and equal to −2, −4.8, and −9.7 kg in the third, fourth, and fifth quintiles, respectively. This increasing interaction mirrors increasing average baseline waste productions across the four groups, showing an effect larger in magnitude for higher quantiles. This result supports the hypothesis of an increasing scope for improvement in waste reduction along the baseline distribution. It suggests that households that produced more waste before the policy have experienced a stronger impact of the policy than households at the lower tail of the baseline waste distribution.

It is worth discussing these results in the context of Table 3 and the size of the bias in our average regression results. Table 3 indicates that lower quantiles have a higher enrollment rate. If we interpret the enrollment rate as a proxy for pro-environmental preferences, the distribution summary in Table 3 suggests that households with stronger pro-environmental preferences were more efficient in producing waste, i.e., produced less waste, already before the policy (possibly because they paid more attention to waste related behaviors). As a result, these households had a smaller scope for reducing waste as a result of the policy (or exhibited a smaller attention boost), which is compatible with the lower magnitudes of the treatment effects presented in this section and in the main results. This interpretation is also supported by the negative bias in the FE results in specification (a) in Table 4. The negative bias of the FE results is compatible with more efficient households self-selecting into the new pro-environmental policy regime. These households are less affected by the policy because they have either lower waste levels before the policy and/or pay more attention to waste in the first place. As an implication, the policy had the largest effect on the least “pro-environmental” households.

5.4. Further robustness checks

In appendix B, we perform several additional robustness checks. First, we challenge the linearity assumption underlying our main staggered DiD estimator. To do so, we implement a nonparametric local average treatment effect (LATE) estimator. Second, we re-estimate our main specifications (1) and (2) under the inclusion of a time trend for each area. Third, we analyze potential contamination of our results through unobserved waste behaviors such as unobserved recycling, private composting and illegal dumping. The results of these robustness checks support our main analysis. In appendix, Section B.5, we provide also some indirect evidence in support of the validity of the exclusion restriction, identifying assumption for the IV estimation approach.

6. Interpretation of empirical results

We now discuss potential channels for the estimated spillover effects and relate our results to the findings in the literature. We focus on the spillover effect according to the second definition, i.e., effect β_D of PEB1 on PEB2.

Limited attention as mechanism driving the spillover effect: a simple model. The empirical and theoretical findings in the bounded rationality literature suggest that when individuals choose an action, they do not actively pay attention to all possible outcomes of this action; see, e.g., [Kahneman \(2003\)](#) and [Tversky and Kahneman \(1974\)](#). An increase in the salience of an outcome therefore leads to an adjustment in the corresponding activities.

Based on these insights, one possible explanation of our results is that increased attention to one activity may spill over to conceptually related activities, which induces a behavioral spillover effect. We present three arguments that support this interpretation. First, waste separation and waste reduction are conceptually related activities. It is thus plausible that increased attention to waste separation due to the policy causes an increase in attention given to waste reduction. The estimated β_D is compatible with this interpretation. Second, as discussed in Section 5.3, we found lower β_D for more environmentally friendly household (with pro-environmental attitude proxied by the rate of compliance and the amount of baseline waste). This is consistent with these households paying higher attention to waste behaviors in the pre-policy period, since higher pre-policy attention to waste reduces the scope for attention spillovers. Third, the literature on habit formation suggests that when habits form as a result of a repetitive activity, the attention paid to that activity decreases.¹⁴ Thus, our finding that β_D diminishes over time is consistent with habit formation w.r.t. PEB1 and a resulting decrease in attention paid to that PEB1. The decreased attention to PEB1 implies a reduced attention spillover to PEB2.

A model. To support this interpretation, we develop a simple microeconomic model of attention spillovers. We build on the framework of [Gabaix \(2014\)](#). Suppose that the utility function u of an individual with limited attention is

$$u(a, x, m) = -\frac{1}{2} \left(a - \sum_{i=1}^p b_i (m_i x_i + (1 - m_i) x_i^d) \right)^2, \quad (4)$$

where a is a vector of actions that the individual takes (such as waste reduction, school choices, and consumption of meat), $x = (x_1, \dots, x_p)$ is the vector containing the true values of each factor relevant for the decision, x_i^d is the individual default value of that variable for the individual, $i = 1 \dots p$, $b = (b_1, \dots, b_p)$ is a vector of weights, and $m = (m_1, \dots, m_p)$ is the attention vector with $m_i = 1$ meaning full attention and $m_i = 0$ no attention paid to factor x_i . When no attention is paid to a factor, its perceived value is equal to the default value assigned by that individual (which is, e.g., formed by a habit). Let x_1 denote PEB1 and x_2 PEB2. Consider a sequence of triples $(a_t, x_t, m_t)_{t=1,2,\dots}$, where t denotes a time period. We adopt the following assumptions:

- (a1) (i) $m_{1,t}$ is exogenously given for all t ; (ii) $m_{1,1} < m_{1,2}$; (iii) $m_{1,t} > m_{1,t+1}$ for all $t \geq 2$;
- (a2) $\frac{\partial m_{2,t}}{\partial m_{1,t}} > 0$ for all t ;
- (a3) $m_{j,t} = m_j$ for all $j \neq 1, 2$.

Assumption (a1) (i) is a simplification of the model of [Gabaix \(2014\)](#), in which all attention components are endogenously chosen. (a1) (ii) introduces an attention increase in PEB1 due to a treatment between periods $t = 1$ and $t = 2$. (a1) (iii) captures the decreasing attention to PEB1 due to habit formation. (a2) represents the attention spillover of PEB1 to PEB2. Finally, (a3) implies that the attention paid to all activities other than PEB1 and PEB2 remains the same over time. The following proposition deduces from the above assumptions that there is a positive behavioral spillover from PEB1 to PEB2 and that it diminishes over time.

Proposition 6.1. Under (a1), (a2) and (a3), it holds that (i) $x_{2,2} > x_{2,1}$, (ii) $x_{2,t} > x_{2,t+1}$ for all $t \geq 2$.

Result (i) reflects the spillover effect of PEB1 on PEB2 caused by an increase in attention between periods 1 and 2. Result (ii) states that under unchanged conditions, the attention decrease causes a reduction of the level of PEB2 (through a reduction in the level of the spillover). The proof is in Appendix C.1.

Remark. Positive spillovers are generally discussed in the context of consistency theory and “cognitive dissonance”; see [Rabin \(1994\)](#), with identity theory being a sub-branch. According to the latter theory, an individual who engages in a pro-environmental behavior consequently keeps behaving pro-environmentally in an effort to maintain a consistent self-image. In this section, we demonstrated that attention spillovers provide an alternative explanation of positive behavioral spillovers. In particular, *observed* consistency of behaviors may reflect an attention spillover and a general pro-environmental preference rather than an attempt by the individual to behave consistently.¹⁵

¹⁴ In particular, attention is a scarce resource and habits help economize on that scarce resource; see [Hussam et al. \(2017\)](#) for a model and further references.

¹⁵ Rational Choice Theory provides further possible channels for the spillover effect. We discuss these channels in Appendix C.2 and conclude that none of them convincingly predict the patterns observed in our results.

7. A cost–benefit analysis of the policy

Finally, we perform an in-depth cost–benefit analysis of the policy, which we describe in detail in Appendix D. Here, we present a short summary of this analysis. The cost associated with the policy includes the one-off costs such as purchasing and delivering the new bins, upgrading the trucks, and the cost of the information campaigns. On the other hand, less monthly waste is associated with several benefits. First, the municipality pays a lower price for the reduced CO₂ transmissions. Second, the organic waste can be turned into biogas. And third, there is a reduced cost for processing waste resulting from the decrease in disposed waste. Comparing these costs and benefits, we find that the policy induced a total one-off cost of about 980,598 USD (8,300,000 SEK), a one-off benefit of about 65,000 USD (546,084 SEK) due to reduced waste (leading to lower operating costs and less CO₂ emissions) and net yearly benefits of about 183,000 USD (1,544,000 SEK). Thus, the policy took roughly five and a half years to reach a net positive balance.

8. Concluding remarks

In this paper, we estimate behavioral spillover effects of waste sorting (PEB1) on waste reduction (PEB2). We use an administrative household-level dataset on waste generation from a Swedish municipality. We find that the change in the targeted PEB1 induces a reduction in generated waste by up to 9%. However, this spillover effect vanishes after five to eight months. Nonetheless, a back-of-the-envelope cost–benefit analysis indicates that the policy had a beneficial effect for the municipality, reaching a net positive balance after five and a half years. An analysis of heterogeneous effects across quantiles of baseline household monthly waste shows that the right tale of the distribution had lower rates of enrollment but generated the largest reductions in waste. Further investigations of the precise mechanism behind these patterns could provide crucial knowledge for the design of new pro-environmental policies. This question is left for future research, if more detailed data on households' characteristics will become available.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary material

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.jeem.2021.102470>.

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