Motivations and Incentives for Pro-Environmental Behaviour in Peru: A Behavioural and Experimental Economics Analysis

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Access Condition and Agreement

Introduction

"The fact is that no species has ever had such wholesale control over everything on earth, living or dead, as we now have. That lays upon us, whether we like it or not, an awesome responsibility. In our hands now lies not only our own future, but that of all other living creatures with whom we share the earth."

— David Attenborough

Understanding human behaviour, and how to encourage behaviour that does not harm the environment – pro-environmental behaviour (PEB) – has never been more important than it is today. Greenhouse gas (GHG) emissions worldwide have increased significantly over the last century, which can be mainly attributed to human activity (Matthews and Wynes, 2022). The concentration of CO2 in the atmosphere during the last decade exceeds any concentration ever recorded in human history and has now reached levels 50% higher than it had been in pre-industrial times (IPCC, 2022; World Economic Forum, 2022). Economic growth worldwide has been accompanied by large amounts of pollution, imposing threats on human health and ecosystems (UNEP, 2017). If consumption and production patterns continue as they are, it will have severe consequences for future generations.

Global agreements such as the Paris Agreement or the Sustainable Development Goals (SDGs) formulated in the 2030 Agenda underline the urgency to act now to protect our environment and support sustainable development (UNFCCC, 2015; UN, 2015). Yet, if current emission trends continue, recent predictions show that the goal formulated in the Paris Agreement to limit global warming to well below 2, preferably to 1.5 degrees Celsius, compared to pre-industrial levels, would be crossed in its lower limit of 1.5°C within the next decade already (Matthews and Wynes, 2022). Crossing this threshold of 1.5°C temperature increase is likely to initiate a number of irreversible climate tipping points with disastrous consequences, such as the collapse of the ice sheets, substantial sea-level rise and biodiversity loss (Lenton et al., 2019). The time to stop these developments is thus limited and our actions during the next years will be crucial.

While certainly a lot of responsibility lies with the governments and industry, every single consumer has a choice, too. Individual lifestyle changes are an important factor in achieving global emission reduction targets (Matthews and Wynes, 2022). The impact

is considerable: it is estimated that around two-thirds of global GHG emissions and between 50% and 80% of total resource use are linked to household consumption (Ivanova et al., 2016). Individuals are faced with several consumption choices every day, from energy use and purchase behaviour over dietary choices to travel patterns. Changes in these consumption patterns to sustainable alternatives offer a great and urgently needed potential for emission reductions (Ivanova et al., 2020). For the first time ever, the Sixth Assessment Report of the Intergovernmental Panel on Climate Change (IPCC, 2022) includes a chapter on demand-side solutions for climate change mitigation (Chapter 5, Working Group III), emphasizing its importance to mobilize another lever to reduce global emissions (Creutzig et al., 2018). As formulated in SDG 12, both sustainable consumption and production patterns are important for ensuring sustainable development (UN, 2015).

Especially the growing middle classes worldwide play a crucial role in this regard. With the rise of the global middle classes particularly in developing and emerging economies, the carbon footprint per person in the Global South has increased as well (Kharas, 2017). Increased income and wealth have brought about a change in consumption patterns towards more energy-intensive and non-sustainable lifestyles. While traditionally industrialized countries have emitted the large majority of GHG emissions, recent developments report a shift in responsibility for global emissions, with emission shares of developed countries being surpassed by those of developing countries (IEA, 2019). Consequently, the positive developments of many people moving out of poverty also impose environmental challenges that need to be addressed (Guarin and Scholz, 2015). Targeting this growing consumer group thus offers great potential for policy makers to increase awareness and change behaviour towards sustainable lifestyles. In contrast, not paying attention to this new emerging middle class could have disastrous effects for global emission levels and our environment.

In my PhD thesis, I shed light on motivations and incentives for pro-environmental behaviour amongst this growing consumer group on the case of Peru. Peru has experienced rapid economic growth over the past decade, with increases in employment and income and sharp reductions in poverty rates (World Bank, 2017; World Bank, 2022a).[1] Moreover, the population in the country has increased constantly over the last years (World Bank, 2022b). In line with this development, consumption expenditures, energy demand and carbon emissions in Peru have been growing rapidly (World Bank, 2022c; World Bank, 2022d; World Bank, 2022e). Future projections predict that this trend is likely to continue, with increasing individual wealth further boosting consumption and emissions in the country, particularly in Peruvian cities (Gouldson et al., 2015; Gouldson et al., 2014). These developments make Peru a highly interesting and relevant case study.

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[1] It should be noted that the economic development in the country was strongly affected by the Covid-19 pandemic (as it was the case for most parts in the world), and that these motivations refer to pre-covid times.

My thesis combines behavioural and experimental economics with environmental and development economics. The core of the thesis is based on three self-contained chapters and draws on unique data collected through a large household survey (Chapter 1) and two field experiments (Chapter 2 and 3) in Lima, Peru. Both field experiments were conducted in the context of a local recycling programme of the municipality of Miraflores in Lima. The first chapter investigates the role of economic preferences for PEB based on correlational evidence. The second and third chapter provide causal evidence on incentives for PEB, specifically recycling, focusing on social norms and beliefs (Chapter 2) and reminders (Chapter 3), respectively. The thesis ends with a short conclusion that summarizes my findings. In the following, I briefly outline the three main chapters in more detail.

The first chapter (**Chapter 1:** *"The role of preferences for pro-environmental behaviour among urban middle class households in Peru"*) of my thesis is based on Fuhrmann-Riebel et al. (2021).[2] In this chapter, I focus on the role of individual preferences for pro-environmental behaviour. I elicit data on preferences and PEB in a household survey with 900 middle class households in Lima, Peru. Pro-environmental behaviour is known to reflect people's social preferences, risk preferences and time preferences. The importance of these preferences for PEB is plausible: pro-environmental behaviour requires engaging in activities that result mainly in collective benefits for the whole society, and thus requires caring about the wellbeing of other people and a propensity to assume that others will engage in the behaviour as well (social preferences). It also requires people to engage in activities of which the benefits are mainly uncertain (risk preferences) and of which the benefits pay off mainly in the future (time preferences). Several studies have shown that social preferences (Gupta and Ogden, 2009; Volland, 2017; Ziegler, 2020), risk preferences (Farsi, 2010; Qiu et al., 2014; Fischbacher et al., 2015) and time preferences (Qiu et al., 2014; Newell and Siikamäki, 2015; Fuerst and Singh, 2018) are relevant for PEB in different contexts.

I extend existing evidence on the role of preferences for PEB in several ways. First, I include a large range of individual preferences in the analysis together, in particular preferences on risk and ambiguity, time, trust, altruism and positive and negative reciprocity. Previous research has mostly looked at one or two preferences in isolation, which means they may proxy for omitted ones, leading to biased estimates. Second, the PEBs I consider in the analysis are habitually saving energy, avoiding the use of plastics, and

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[2] This chapter is joint work with my supervisors Ben D'Exelle and Arjan Verschoor. The paper is published as: Fuhrmann-Riebel, H., D'Exelle, B., & Verschoor, A. (2021). The role of preferences for pro-environmental behaviour among urban middle class households in Peru. *Ecological Economics, 180,* 10685. I was responsible for developing the research idea and study design, programming the survey, organising and overseeing the data collection, conducting the statistical analyses, and writing the first draft of the paper. Both co-authors gave input on the research design and on the analyses and were involved in writing the final draft of the paper that was sent out for publication.

limiting expenditures on electricity, and thus expand the evidence from previous studies that have mainly focused on large, occasional decisions (e.g. the purchase of a new appliance). Third, the literature on preferences and PEB has so far focused on risk preferences only and has not considered ambiguity preferences, which are for some PEBs conceptually more relevant than risk preferences. Fourth, I use an experimentally validated survey module from the Global Preference Survey (GPS) of Falk et al. (2016) and Falk et al. (2018) to elicit data on preferences, which allows for international replication and comparability. Fifth and finally, with the exception of Fuerst and Singh (2018), no evidence exists on the role of preferences for PEB outside a high-income country context.

I show that preferences are of relevance for pro-environmental behaviour in the Peruvian context, and that it is important to include all relevant preferences in the analysis since preferences that matter for one type of PEB do not necessarily matter for another. In particular, I find that social preferences are mainly correlated with saving-energy behaviour, with higher levels of social preferences predicting a higher engagement in measures to save energy in the household, yet hardly with sustainable plastics consumption or with monthly expenditures on electricity. I further show that time, risk and ambiguity preferences matter mainly for the consumption of plastics, with more patient and more risk and ambiguity averse people investing more in avoiding wasteful plastic use. And finally, that time and ambiguity preferences can predict expenditures on electricity, with higher levels of patience and ambiguity aversion predicting lower such spending. The insight that particular preferences matter for particular PEBs has important policy implications.

The second chapter (**Chapter 2:** *"Boosting recycling behaviour among urban households in Peru – A field experiment on the role of social norms and beliefs"*) of the thesis is based on Fuhrmann-Riebel et al. (2022a).[3] In this chapter, I shed light on the role of social norms and individual beliefs about social norms for people's decision to recycle. In cooperation with a local municipality in Lima, Peru, I conduct a field experiment with 1,709 households using phone surveys to increase sign-up rates to the municipality's recycling programme. I make use of different types of social norm information to motivate sign-ups of households, focusing on dynamic and injunctive norms. Current participation rates in the recycling programme in our study area are still low, while the number of households participating in the programme has increased over the last years and the social approval for recycling is high.

Research has shown that informing people about social norms can effectively motivate behaviour when a majority of others is already engaging in the desired target behaviour (see e.g. Hallsworth et al., 2017). Yet, a key challenge of encouraging many PEBs is that this is often not the case, whereas unsustainable norms prevail. This is relevant for a variety of settings, from eating habits through transport behaviour or resource consumption. In such a situation, encouraging a desired behaviour by pointing towards a descriptive majority of others that is already setting a good example is not possible as it does not exist. However, at the same time, many pro-environmental behaviours are increasing in popularity, meaning that more and more people start to engage in them. Recent evidence has shown that in such settings, pointing people towards a positive trend in the behaviour of others (dynamic norms) can successfully encourage people to engage in a behaviour as well, even if there is no descriptive majority yet (Loschelder et al., 2019; Mortensen et al., 2019; Sparkman and Walton, 2017; Sparkman and Walton, 2019; Sparkman et al., 2020).

Moreover, recent literature has shown that biased beliefs, i.e. misperceptions about social norms, matter for people's response to such information, resulting in heterogeneous treatment effects (Andre et al., 2021; Bursztyn et al., 2020; Byrne et al., 2018). I combine these two recent additions to the literature, shedding light on the importance of individual prior beliefs for people's response to social norm information in the context of dynamic norms, where the share of others engaging in the desired target behaviour is increasing while the majority does not yet behave in the required way. In doing so, I add to the few existing studies that combine measuring people's individual prior beliefs with social norm information treatments that directly aim at correcting those beliefs.

By means of phone surveys, I i) first elicit people's prior beliefs about dynamic and injunctive norms regarding participation in the recycling programme, ii) provide people with information about the dynamic norm (dynamic norm treatment), the injunctive norm (injunctive norm treatment), or both (combined treatment), and iii) finally ask about people's decision to sign up to the recycling programme. The treatment groups are compared to a control group that does not receive any social norm information before being asked about their sign-up decision.

I show that individual misperceptions about dynamic and injunctive norms in the recycling behaviour of others can prevent people from signing up to the recycling programme, and that randomly correcting people's beliefs causally raises their willingness to do so. I demonstrate that individual level belief updating can explain heterogeneous responses to social norm information: the social norm information treatments effectively motivate people to sign up to the recycling programme when people initially underestimate the actual norm, while there is no effect for those who overestimate or are correct about it. I find that this holds irrespective of whether the norm is high or low, or whether

it is presented in a static or dynamic way. Moreover, I show that this effect increases in the level of underestimation, i.e. the more strongly people underestimate the social norm information they receive, the greater the effect of the treatment messages that directly correct those beliefs. My findings are in line with the results of Bursztyn et al. (2020) and Andre et al. (2021), who also report significant increases in their respective target behaviours only among the people who underestimate the social norm information they receive. I extend previous evidence on the importance of prior beliefs for people's response to social norm information in the context of dynamic norms and highlight their relevance even in situations where the majority of others is not yet engaging in the desired behaviour.

My findings thus underline the great importance of individual beliefs about social norms for people's decision making, and for the effectiveness of social norm interventions that directly address those beliefs. The results may help to understand why the effect sizes of social norm interventions vary considerably between studies and why they may work in some but not in other contexts. Moreover, my findings underline the effectiveness of dynamic norms to encourage behavioural change even when the share of other people engaging in the desired target behaviour is still low. The findings are relevant for policy makers aiming to encourage sustainable behaviours, especially in situations where unsustainable norms predominantly prevail, which is crucial in view of fighting the climate crisis.

The third chapter (**Chapter 3:** *"Using reminder messages to increase recycling behaviour in Peru"*) of the thesis is based on Fuhrmann-Riebel et al. (2022b).[4] In this chapter, I analyse the effect of sms reminder messages on households' weekly recycling behaviour in the context of a municipal recycling programme in Lima, Peru, in which households can participate voluntarily and free of charge. Although households sign up to the programme voluntarily, few recycle regularly, pointing towards a gap between people's intention to recycle and their actual recycling behaviour. The fact that people have trouble to follow through with good intentions is a common observation in many domains (Rogers et al., 2015), especially in the context of pro-environmental behaviour (Mazar et al., 2021). Research has shown that reminders can help people to follow through with their intentions by addressing the problem of limited attention (Karlan et al., 2016).

In this study, I conduct a randomized controlled trial (RCT) with 1,392 households to

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[4] This chapter is joint work with Ben D'Exelle, Kristian López Vargas, Sebastian Tonke and Arjan Verschoor. The study was pre-registered at the AEA RCT Registry (AEARCTR-0007780). I was responsible for developing the research idea and study design, establishing the contact to the municipality, acquiring additional funding, organising and overseeing the data collection, conducting the statistical analyses, and writing the first draft of the paper. All co-authors gave input on the research design. Kristian López Vargas further helped managing the contact to the municipality in Peru, Ben D'Exelle and Sebastian Tonke gave regular feedback on the statistical analyses, Arjan Verschoor provided feedback on the paper draft. Ben D'Exelle and Arjan Verschoor were also involved in the funding acquisition, Kristian López Vargas and Sebastian Tonke provided additional financial resources, respectively.

test whether sms reminders can increase recycling behaviour of households. In particular, I contrast the effect of continuous vs. interrupted vs. restarted reminders on households' recycling behaviour. I measure recycling behaviour of households over 12 weeks in total. While the first three weeks serve as a baseline measure, the subsequent nine weeks constitute the intervention period where households are randomly assigned to either i) a control group that does not receive any reminders, ii) a group that receives continuous reminders over the whole nine weeks (continuous treatment), iii) a group that receives reminders only for the first three weeks (interrupted treatment), iv) or a group that receives reminders for the first three weeks and for the last three weeks, with a three weeks' pause in between (restarted treatment). The design allows me to analyse the effects of continuous reminders on households' recycling behaviour, persistence of reminder effects after the intervention has ended, as well as restart effects when the intervention is taken up again.

I add to the reminder literature in several ways. First, I extend evidence on the effectiveness of reminders in the context of household recycling behaviour, which has hardly been investigated so far (Chong et al. (2015) is an exception). An important difference of recycling behaviour as opposed to other behaviours studied in the literature is that – as in the case of other PEBs as well – the reminders do not aim to improve the welfare of an individual (as e.g. in the case of gym use (Calzolari and Nardotto, 2017) or financial savings (Karlan et al., 2016)), but to reduce the negative environmental externalities associated with people's decision making (e.g. waste accumulation), which makes the research particularly policy relevant (Carlsson et al., 2021). Moreover, I add new evidence on the effectiveness of reminders in the context of regular, repeated behaviours in contrast to infrequent, one-time decisions, which is important as in the case of regular behaviours habit formation can play a role that may persist even after the intervention was terminated (Calzolari and Nardotto, 2017). The treatment variation allows me to compare persistence effects of an interrupted intervention to a group that still continues to receive reminders at the same time, which is rare in the literature. I further provide new insights on the role of the frequency with which reminders are sent, adding a novel design element on the effect of stopping and restarting the intervention. In addition, I add to the studies that make use of sms reminders to encourage behaviour, of which a great benefit lies in their low implementation costs, which is especially important for studies conducted outside a high-income country context where financial resources may be limited. Finally, I add new evidence on the effectiveness of reminders in Peru, and thereby extend the existing literature that has so far mainly focused on the Global North.

I show that sms reminders are generally effective to encourage recycling behaviour: the percentage of households that recycle during the first three weeks of the intervention period, where all treated households received a reminder, is significantly higher in the treatment groups than in the control group that did not receive any reminders. I further provide evidence for strong continuous reminder effects throughout the whole intervention

period, which speaks in favour of the theory that limited attention on the recycling day itself is a main obstacle for people to recycle regularly that can be addressed by reminding people to recycle on a regular basis. Moreover, I find some evidence for persistence effects of reminders after the intervention was interrupted, which suggests that receiving reminders can induce some form of habit formation in people's recycling behaviour, although the data is less clear in this regard. Unfortunately, data issues during the last three weeks of the intervention period complicate the analysis on potential restart effects of reminders, for which I cannot find evidence based on my results. Finally, my data does not reveal any potential negative effects of sending weekly reminders on households' recycling behaviour. From a policy perspective, the results are promising as I show how a low-cost, easily scalable tool in the form of sending weekly sms reminders can be used effectively to increase households' recycling activity in the Peruvian context.

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Chapter 1

The role of preferences for pro-environmental behaviour among urban middle class households in Peru

Abstract

Pro-environmental behaviour (PEB) is known to reflect people's social preferences, time preferences and risk preferences. Previous research has tended to consider these in isolation, which means they may proxy for omitted ones, leading to biased estimates. Moreover, it has not considered ambiguity preferences, which for some PEBs is conceptually more relevant than risk preferences. Using a survey module from the Global Preference Survey (GPS), we investigate the role of a large range of preferences for PEB in a sample of 900 middle class households in Lima, Peru. The PEBs we consider are habitually saving energy, avoiding the use of plastics, and limiting expenditures on electricity. We find that social preferences matter mainly for saving-energy behaviour; time, risk and ambiguity preferences matter mainly for the consumption of plastics; and time and ambiguity preferences matter for expenditures on electricity. The insight that particular preferences matter for particular PEBs has important policy implications.

1.1. Introduction

Individual consumers can help prevent disastrous climate change and environmental pollution by changing their behaviour. Pro-environmental behaviour (PEB) results both from large, occasional decisions such as having solar cells installed and from small, regular ones such as switching off the TV when nobody is actively watching it.

Economists think of behaviour as resulting from people's preferences. Research has shown that individual preferences can influence decision-making in many domains, including savings behaviour and educational attainment, health-related behaviours such as exercising and smoking, or pro-social behaviours such as donations to charity (Dohmen et al., 2011; Sutter et al., 2013; Falk et al., 2015; Falk et al., 2018).

Several studies have found individual preferences to be important for PEB. A group of these have found social preferences to matter for PEB (Gupta and Ogden, 2009; Volland, 2017; Ziegler, 2020). This is plausible since PEB requires caring about the wellbeing of other people, and a propensity to assume that others, when encouraged to engage in PEB, will do so (Gupta and Ogden, 2009). The social preferences of altruism, trust and reciprocity are therefore expected to be important for PEB.

Other studies looked at the role of risk preferences. The benefits of PEB are uncertain, meaning that deciding to engage in PEB carries the risk that the desired outcomes do not come about. In line with that, greater risk aversion has been found to be associated with the undervaluation of PEB (Farsi, 2010; Qiu et al., 2014; Fischbacher et al., 2015), although not universally so (Volland, 2017). Finally, time preferences are expected to matter. People who discount the future at a lower rate, i.e. people who are more patient, should value PEB more. This has been empirically confirmed by Qiu et al. (2014), Newell and Siikamäki (2015) and Fuerst and Singh (2018).

In this study, unlike in previous research, we consider the role of social preferences, risk preferences, and time preferences for PEB together, rather than one or some of these in isolation. To this we add ambiguity preferences. Ambiguity preferences relate to uncertain future outcomes that occur with unknowable probabilities. We explain below why ambiguity preferences are sometimes conceptually more appropriate than risk preferences for PEB. We collect survey data for a sample of middle-class households from Lima, the capital of Peru. The social preferences we include are altruism, trust and reciprocity (both positive and negative). With the exception of ambiguity preferences, all preferences are elicited using a survey module from the Global Preference Survey (GPS), introduced by Falk et al. (2016) and Falk et al. (2018).

We make the following contributions to the literature on preferences and PEB. First, whereas previous studies consider one or a few preferences in isolation, we include a large range of relevant preferences. As Sutter et al. (2013) point out, omitting relevant

preferences can lead to wrongly attributing behavioural effects to the preferences that have been included in the analysis. Whereas we do not claim to be able to identify causal effects of preferences on PEB, we avoid in this way potential omitted variable bias. For the same reason, we also control in the analysis for variables that are potentially correlated both with PEB and with preferences, such as environmental knowledge, environmental concern, wealth, age, gender and education.

Second, most research in this field has looked at the role of preferences in PEB that results from large, occasional decisions. However, as mentioned PEB consists of regular behaviour, too. To our knowledge, no previous evidence exists on preferences and their importance for regular PEB.[1] We contribute to the literature by considering two types: behaviours that save energy in the household and behaviours that reduce the amount of plastics consumption. We also consider a measure that results from both regular PEB and occasional PEB, the monthly electricity bill. Volland (2017) uses a similar measure for a sample of households in the UK.

Third, we include ambiguity aversion among the relevant preferences, which is a novel contribution as the studies on PEB that look at the role of attitudes towards uncertainty focus on risk aversion (Farsi, 2010; Qiu et al., 2014; Fischbacher et al., 2015; Volland, 2017). When probabilities of outcomes are known or can be estimated, risk preferences are relevant, when they are unknown, ambiguity preferences are (Ellsberg, 1961). In the plausible situation that an individual decision-maker is unable to estimate the probabilities of outcomes of PEB, ambiguity aversion is therefore the relevant concept, which we are able to investigate in this study.[2]

Fourth, by eliciting data on preferences using questions from the Global Preference Survey (GPS) of Falk et al. (2016) and Falk et al. (2018), we are employing a validated methodology that allows for simple comparison within and between countries and thereby provides a basis for replication in future research.

Fifth and finally, with the exception of Fuerst and Singh (2018), who conducted their research in India, no evidence exists for the role of preferences in PEB outside a high-income country context. Peru, a middle income country, is a particularly interesting case because of the rapid rise of the middle class, as a result of sustained economic growth. According to the official news agency of the Peruvian state, Andina, the percentage of people living in middle class households grew from 14.1% of the population in 2004 to 44.7% in

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[1] A recent working paper by Lades et al. (2020) takes a similar approach while using online surveys and different techniques to measure regular PEB.

[2] Millner et al. (2013) and Weitzman (2009) theoretically discuss the relevance of ambiguity for climate policies. Yet, to our knowledge no previous study has ever quantified the effect of ambiguity aversion on PEB in a real world setting. Evidence on individual ambiguity preferences and behaviour outside the laboratory is rare in general (see Trautmann and Van De Kuilen (2015) for a review).

2018, the year of our survey, which amounts to 14.4 million Peruvians (Andina, 2019).[3] As their spending increases, so does their potential to do damage to the environment through their consumption behaviour.[4] Evidence on the preferences that correlate with PEB among a group with a large and rapidly growing environmental footprint may help policy makers understand how to encourage PEB more effectively and thereby prevent much damage.

Our findings may be summarised as follows. We find that social preferences matter mainly for saving-energy behaviour; time, risk and ambiguity preferences matter mainly for the consumption of plastics; and time and ambiguity preferences matter for expenditures on electricity. The insight that particular preferences matter for particular PEBs has important policy implications, which we spell out in the final section of the paper. The paper proceeds as follows: Section 1.2 explains the research design, including the research hypotheses, data collection and measurement of variables. Section 1.3 presents empirical findings based on regression analyses. Section 1.4 ends with a discussion and conclusion.

1.2. Research Design

1.2.1. Research hypotheses

As outlined in the introduction, previous literature has found social preferences to matter for PEB. PEB requires people to make the effort of engaging in activities that result mainly in collective benefits for the society, which again requires people to care about the wellbeing of others. In particular, it requires people to engage in sustainable activities without expecting any direct personal benefit from it (altruism). It also assumes that they trust other people will engage in PEB as well when encouraged to do so (trust), and that they are willing to reciprocate other people's effort for collective benefit (reciprocity).

Volland (2017) finds that trust has a negative effect on residential energy use while Gupta and Ogden (2009) provide additional evidence that more trusting individuals are more likely to buy green products. Ziegler (2020) further finds that higher levels of trust and social preferences in general have a positive effect on switching to green electricity contracts. Moreover, at a macro level, Carattini et al. (2015) show that trust is negatively related with countries' greenhouse gas emissions and per capita energy consumption. Ostrom (2009) further summarizes the importance of trust and reciprocity for solving global collective action problems like climate change mitigation.

All these studies thus find positive correlations between social preferences and PEB.

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[3] Middle class households are defined by Lima's chamber of commerce as those earning between US\$10 and US\$50 per day, corrected for purchasing power parity (ibid.).

[4] See Never et al. (2020) for the carbon-intensity of consumption patterns of the growing middle class in Peru.

Notably, previous literature has focused mainly on trust, while evidence on the importance of other social preferences (altruism as well as positive and negative reciprocity) for PEB is sparse. Based on previous literature, we therefore hypothesise that higher levels of social preferences lead to more energy-saving behaviour and sustainable plastics consumption, and to lower expenditures on electricity. This will be our first hypothesis.

Hypothesis 1.1 (H1.1) *Higher levels of social preferences predict more PEB (i.e. more energy-saving behaviour and sustainable plastics consumption, and less expenditures on electricity).*

A link has also been found between risk preferences and PEB. PEB requires people to engage in activities of which the benefits are mostly uncertain. Qiu et al. (2014) show that more risk averse individuals are less likely to adopt energy-efficient technologies or have installed energy-efficient home improvements. Similar results are reported by Farsi (2010) for adopting energy-efficient systems in rental apartments. On the other hand, Volland (2017) finds that higher risk tolerance increases household energy use. While these findings might seem contradictory (more energy-efficient appliances should lead to lower energy use), Volland (2017) explains this effect with a higher willingness to purchase new appliances in general (energy-efficient or not) of people with higher levels of risk tolerance. Fischbacher et al. (2015) further find that more risk taking homeowners are more likely to have renovated their house for better insulation.

Evidence on the relation between risk preferences and PEB is therefore not as straightforward as for social preferences, even though the majority (with the exception of Volland, 2017) finds that higher levels of risk aversion are associated with less investment in PEB. However, Volland's measure of monthly energy expenditures in the UK comes closest to our dependent variable of the monthly electricity bill and might therefore be more relevant for this particular PEB. Moreover, we include ambiguity aversion in our analysis. When decision-makers are unable to associate probabilities with the outcomes of PEB, ambiguity aversion, not risk aversion, is the relevant concept. Moreover, the strong correlation between the two measures indicates in any case the importance to consider both in the analysis.[5] In line with previous findings, we thus derive the following hypotheses for our analysis.

Hypothesis 1.2a (H1.2a) *Higher levels of uncertainty tolerance (risk and ambiguity) predict more PEB with regards to energy-saving behaviour and sustainable plastics consumption.*
Hypothesis 1.2b (H1.2b) *Higher levels of uncertainty tolerance (risk and ambiguity) predict higher expenditures on electricity (i.e. less PEB in this regard).*

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[5] A correlation matrix of all preferences and PEBs is attached in the appendix 1.A.

Lastly, evidence exists on the importance of time preferences for PEB. PEB requires people to engage in activities in the present of which the benefits pay off mainly in the future. It is therefore plausible to assume that individual discount rates, used as a measure of patience, are important for the decision to engage in PEB.

Newell and Siikamäki (2015) demonstrate that individual discount rates systematically influence households' willingness to pay for energy efficiency. Fischbacher et al. (2015) further find that future-oriented individuals live in homes with higher energy efficiency and have lower energy costs. Fuerst and Singh (2018) provide additional evidence that individuals who are more patient and less present-biased are more likely to invest in energy-efficient appliances. Ziegler (2020) further shows that more patient individuals are more likely to switch to alternative and green electricity contracts. The evidence therefore clearly suggests that higher levels of patience predict more PEB. This leads to our next hypothesis.

Hypothesis 1.3 (H1.3) *Higher levels of patience predict more PEB (i.e. more energy-saving behaviour and sustainable plastics consumption, and less expenditures on electricity).*

1.2.2. Data collection

To elicit information on the variables of interest for our analysis, a household survey was conducted among 900 middle class households in Lima, Peru, in November and December 2018. The data collection was conducted by a local survey firm. To identify middle class households, we first excluded the very poorest and very richest districts by making use of an existing poverty map for Lima (INEI, 2016) as well as the latest national household survey data for Peru (ENAHO, 2017). We next computed the number of households to sample by district through allocating the sample to districts in proportion to the number of middle-income households living in them, using the latest Census (2017) data and the INEI (2016) poverty map. We decided to sample on average five households per block, so divided the number of households to be sampled per district by five in order to determine the number of blocks to sample by district. Blocks were randomly selected.[6]

Within each block, enumerators followed a random walk system and approached every fifth household, thereby sampling approximately five households per block. Enumerators asked eight screening questions before administering the actual questionnaire, in order to ensure that households did indeed belong to the middle class.[7] Enumerators were

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[6] To be precise, we numbered contiguous blocks consecutively on a map, divided the number of district blocks by the number of blocks to be sampled, which gave the number x, and sampled every xth block.

[7] Enumerators observed the appearance of the house, and asked some questions about certain indicative expenditure categories. On the basis of these questions, a score was computed, which if it was in the required range meant enumerators could proceed with the survey. If not, they approached the next house.

instructed to always interview the household head (preferably) or their spouse. The surveys were conducted with tablets using the software SurveyCTO. The monitoring function of the software made it possible to follow the data collection process continuously and to ensure direct quality control of the data.

1.2.3. Measurement of variables

1.2.3.1. Independent variables: Preferences

Data on risk, time and social preferences was collected using questions from the Global Preference Survey (GPS) of Falk et al. (2016) and Falk et al. (2018), which has been implemented worldwide, in at least 76 countries. A key advantage of the GPS is that it is experimentally validated, meaning that the survey items included in the GPS were the best predictors for preferences in incentivised choice experiments. By experimentally validating a survey module on preferences and testing it for cultural sensitivities, the authors provide a low-cost measurement tool for use in large and diverse samples, while still retaining key advantages of experimental approaches (Falk et al., 2016). Moreover, the use of a standardized tool for measuring preferences contributes to facilitating comparability across studies. By using questions from the GPS for our research, we thus take advantage of a tool that can easily be applied in almost any country, thereby facilitating international replication and comparison.

For our analysis, risk preferences are elicited using a so-called "staircase" procedure for the subjective valuation of a hypothetical gamble. In particular, respondents choose between this gamble and a certain payment. If they choose the gamble, then the certain payment is increased in the next choice; if they choose the certain payment, then it is reduced. This continues until the certainty equivalent value of the gamble is approximated, i.e. until the decision-maker is almost indifferent between the gamble and the certain payment. Time preferences are measured using a similar staircase procedure for a hypothetical intertemporal choice (between a payment now and a payment in twelve months), and ambiguity preferences (which are not included in the GPS) by using the same staircase procedure as for risk, but replacing the gamble by an ambiguous outcome, i.e. one in which probabilities are not known by the decision-maker.[8]

We elicit social preferences using questions on altruism, trust, and positive and negative reciprocity, which are all measured through respondents rating their willingness to act in certain emblematic situations, or their self-image in terms of certain character traits, on an 11-point Likert-scale from 0 to 10. For example, preferences for negative reciprocity are captured through scores on the following two questions with equal weights.

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[8] Our method for eliciting ambiguity preferences is inspired by Sutter et al. (2013).

*How willing are you to punish someone who treats **you** unfairly, even if there may be costs to do so?*

*How willing are you to punish someone who treats **others** unfairly, even if there may be costs to do so?*

All survey questions are shown in abbreviated form in Table 1.1 below and can be found in their original longer version in appendix 1.B. For the analysis, we use the z-score of each preference measure.

Table 1.1: Preference measures used in the analysis

Preference	Question in abbreviated form	Answer Scale
Risk	*(Sequence of five interdependent binary choice questions)* What would you prefer: 50 percent chance of receiving x and 50 percent chance of receiving nothing, or the amount of y as a sure payment?	Five choices between a risky and a certain payment
Ambiguity	*(Sequence of five interdependent binary choice questions)* This bag contains 20 balls, which are all either black or white, but you don't know how many of each there are. What would you prefer: a draw from the bag of 20 balls, where you would get amount x if you drew a white ball, and nothing if you drew a black ball, or the amount of y as a sure payment?	Five choices between an ambiguous and a certain payment
Time	*(Sequence of five interdependent binary choice questions)* Please consider the following: would you rather receive amount x today or amount y in 12 months?	Five choices between a payment now and one in twelve months
Altruism	*(Willingness to act)* How willing are you to give to good causes without expecting anything in return?	11-point Likert-scale from 0 to 10
Negative reciprocity	*(Willingness to act)* 0.5 x How willing are you to punish someone who treats **you** unfairly, even if there may be costs for you? 0.5 x How willing are you to punish someone who treats **others** unfairly, even if there may be costs for you?	11-point Likert-scale from 0 to 10
Positive reciprocity	*(Self-assessment)* When someone does me a favour, I am willing to return it.	11-point Likert-scale from 0 to 10
Trust	*(Self-assessment)* I assume that people have only the best intentions.	11-point Likert-scale from 0 to 10

Notes: Own illustration (short form) based on Falk et al. (2016).

1.2.3.2. Dependent variables: Pro-environmental behaviour (PEB)

We capture pro-environmental behaviour (PEB) in a number of different ways (for details see appendix 1.C). First, we measure the extent to which people engage in *energy-saving behaviour*. We do so through constructing an index based on three questions, one focusing on switching off the lights when leaving the room, another on turning off the TV when nobody is actively watching it, and a final one on pro-actively trying to save energy in general. The index constructed is the first component of a Principal Component Analysis (PCA). To verify our assumption that the first component captures PEB rather than something else, we also use an index based on the simple mean of the three items, as a robustness check.[9]

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[9] All robustness checks and other supplementary analyses are available from the authors on request.

Second, we capture whether respondents are aiming for *sustainable plastics consumption.* For this purpose, we construct an index based on two questions, one about reusing materials such as plastic bags and another about trying to avoid taking plastic bags in shops. Again, PCA is used to construct our preferred index while an index based on the mean of the items is used as a robustness check.

Third, we measure *monthly spending on electricity*, which relies mainly on self-reported data.[10] For the analysis of spending on electricity, we removed outliers: all households that claimed to have no spending on electricity at all (19 cases) and those that reported an electricity spending above 600 Soles per month (10 cases, top 1%), leaving 869 observations for the final variable. For the analysis, the logarithm of this variable was used.

1.2.3.3. Control variables

Environmental knowledge (EK) and environmental concern (EC) can be expected to matter for PEB and are therefore included as control variables in the analysis (see e.g. Lange et al., 2014, for a discussion on the relevance of environmental attitudes for residential heating expenditures). Moreover, EK and EC may correlate with both PEB and individual preferences, so that not including these variables would bias the estimated effect of preferences on PEB. The same applies to the other control variables, which include a wealth index (based on a PCA of all assets and characteristics of the house), age, gender and the level of education of the respondent as well as the number of household members (hh members) and household rooms (hh rooms). EK is captured using an additive index based on eight questions eliciting knowledge about the natural environment and humans' influence on it. Our EC index takes the value of the mean of scores on six questions eliciting concern for the environment and for sustainable consumption habits. The questions for EK and EC are based on Thøgersen et al. (2010) and Thøgersen et al. (2019), and can be found in appendix 1.D.

1.3. Empirical Findings

1.3.1. Descriptive statistics

Table 1.2 shows descriptive statistics of the key variables used in the analysis. Respondents are 55% female, and aged between 18 and 75 years, with a mean age of 48 years. Confirming the middle-class nature of our sample, the most frequently occurring levels of

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[10] Only a minority of people allowed us to take a picture of their electricity bill (n=33). In all other cases, people gave their best guess of how much they spent on electricity per month. Whether self-reported numbers are sufficiently accurate in this context has been discussed with key informants in Peru and was found to be the case. We only asked people about their guess on monthly electricity expenditures when they did not allow us to take a picture of their electricity bill. Therefore, we combine the two (actual number stated on electricity bill and best guess from respondent) for the final variable used in the analysis.

education are having completed secondary school (41%) and technical higher education (39%).

For ease of interpreting the regression analyses below, we note here that higher indices of sustainable plastics consumption and saving-energy behaviour indicate a greater degree of PEB, higher monthly electricity spending a lower degree of PEB, and higher EK and EC indices greater environmental knowledge and concern, respectively. The time preference variable being higher indicates greater patience, and the risk preference variable being higher greater willingness to take risk (so lower risk aversion); ditto for ambiguity.

As to the social preferences, negative reciprocity being higher indicates a greater willingness to punish others for behaviour that is perceived to be unfair; altruism higher, a greater willingness to donate to good causes; positive reciprocity higher, a greater willingness to return a favour; and trust higher, a more generous assumption that other people only have the best intentions.

Table 1.2: Summary statistics of EK and EC, preferences and PEB

Variable	Obs.	Mean	Std. dev.	Min	Max
Control variables					
Environmental knowledge	898	5.30	1.85	0	8
Environmental concern	898	3.78	0.58	1	5
Preferences					
Altruism	898	4.93	2.44	0	10
Trust	898	3.22	1.86	0	10
Pos. reciprocity	898	7.49	2.10	0	10
Neg. reciprocity	898	2.67	2.03	0	10
Risk	898	7.29	7.65	1	32
Ambiguity	898	6.72	7.26	1	32
Patience	898	1.96	3.76	1	32
PEB					
Each item individually					
Switching off lights	898	4.48	0.71	1	5
Turning off the TV	887	4.44	0.69	1	5
Trying to save energy	898	4.45	0.66	1	5
Reusing plastic materials	898	3.55	1.31	1	5
Avoiding plastic bags	898	2.08	1.09	1	5
Indices (mean)					
Energy-saving index	887	4.45	0.60	1	5
Plastics consumption index	898	2.81	0.94	1	5
Monthly spending on electricity					
Spending on electricity	869	127.93	80.34	12	556

1.3.2. Regressions

We analyse the relation of preferences and PEB in a multiple regression model

$$Y_i = \text{\ss}_0 + \text{\ss}_1 Altruism_i + \text{\ss}_2 Trust_i + \text{\ss}_3 Positive reciprocity_i + \text{\ss}_4 Negative reciprocity_i$$

$$+ \text{\ss}_5 Risk_i + \text{\ss}_6 Ambiguity_i + \text{\ss}_7 Time_i + \text{\ss}_8 EK_i + \text{\ss}_9 EC_i + \text{\ss}_{10} X_i + u_i$$

where Y_i is PEB (i.e. one of energy-saving behaviour, sustainable plastics consumption or the log of monthly spending on electricity), X_i indicates all other control variables and u_i is the error term.[11]

We specify five models for each of our three measures of pro-environmental behaviour (energy-saving, sustainable plastics consumption, electricity spending), gradually adding regressors to check sensitivity to model specification of coefficients on our key independent variables. In model 1, only social preferences feature; model 2 adds risk and ambiguity preferences; model 3 time preferences; model 4 environmental knowledge and concern; and model 5 the full range of controls.[12]

1.3.2.1. Energy-saving behaviour

Table 1.3 shows the regression results for energy-saving behaviour. All social preferences are statistically significant predictors for energy-saving behaviour, also after adding all relevant control variables (model 5). The sign of the coefficients (positive for altruism, trust and positive reciprocity, negative for negative reciprocity) confirms the hypothesis that more pro-social individuals tend to display higher levels of energy-saving behaviour (H1.1). The size of the coefficients is not very sensitive to adding control variables. Because all variables have been z-standardised, the regression coefficients are directly comparable.

By contrast, we do not find significant results for risk, ambiguity and time preferences. Surprisingly, neither EK nor EC is a significant predictor for energy-saving behaviour in our analysis, which we briefly interpret in the discussion. We do find a positive coefficient for age, indicating that older people engage more in energy-saving behaviour. Finally, women are more likely than men to engage in such behaviour, and so are smaller households.

1.3.2.2. Sustainable plastics consumption

Looking at the regression results for sustainable plastics consumption in Table 1.4, we find that all social preferences are statistically significant predictors in model 1, but most of these effects are not robust, since they largely diminish after all other preferences measures and relevant control variables have been added. In model 5, the coefficients of altruism, trust and negative reciprocity are statistically insignificant, and the only social preferences variable that remains a statistically significant positive predictor is positive reciprocity (which enters with the expected sign, as specified in H1.1).

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[11] We have also run ordered logit and probit regressions on the individual questions of the indices as robustness checks and receive similar results.

[12] Given that pairwise correlations among our independent variables are low (see correlation matrix in the appendix 1.A), multicollinearity is unlikely to be a problem for our analysis.

Table 1.3: OLS Regression analysis of energy-saving behaviour

VARIABLES	(1)	(2)	(3)	(4)	(5)
Altruism	0.197***	0.204***	0.201***	0.234***	0.220***
	(0.0563)	(0.0570)	(0.0574)	(0.0588)	(0.0585)
Trust	0.139***	0.131**	0.130**	0.119**	0.106**
	(0.0535)	(0.0544)	(0.0544)	(0.0545)	(0.0539)
Pos. reciprocity	0.223***	0.222***	0.221***	0.218***	0.215***
	(0.0553)	(0.0556)	(0.0557)	(0.0576)	(0.0580)
Neg. reciprocity	-0.229***	-0.238***	-0.238***	-0.223***	-0.173***
	(0.0545)	(0.0559)	(0.0559)	(0.0570)	(0.0576)
Risk		-0.00632	-0.00869	-0.0288	-0.0280
		(0.0876)	(0.0878)	(0.0881)	(0.0869)
Ambiguity		0.0482	0.0463	0.0624	0.0403
		(0.0877)	(0.0878)	(0.0879)	(0.0870)
Patience			0.0229	0.0309	0.0420
			(0.0513)	(0.0516)	(0.0509)
EK				-0.0823	-0.0565
				(0.0506)	(0.0543)
EC				-0.0883*	-0.0555
				(0.0520)	(0.0521)
Female					0.207**
					(0.0997)
Age					0.251***
					(0.0510)
Wealth index					-0.0318
					(0.0599)
Education					0.0563
					(0.0555)
Hh members					-0.0916*
					(0.0541)
Hh rooms					-0.0525
					(0.0593)
Constant	-0.00427	-0.00440	-0.00427	-0.00540	-0.118
	(0.0482)	(0.0482)	(0.0483)	(0.0481)	(0.0729)
Observations	887	887	887	887	887
R-squared	0.088	0.089	0.089	0.096	0.132

Notes: Standard errors in parentheses (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

By contrast, risk and ambiguity tolerance are both significantly negatively related to sustainable plastics consumption, also after adding all relevant control variables. This means that more risk and ambiguity tolerant people are less likely to engage in this particular PEB, which contradicts our hypothesis H1.2a and which we reflect upon in the discussion. The results for time preferences confirm the hypothesis that more patient individuals show higher levels of sustainable plastics consumption (H1.3).

Table 1.4 also illustrates the importance of considering all relevant preferences. For instance, when risk and ambiguity aversion are not controlled for, negative reciprocity is statistically significant, but it loses significance when these variables are added. This suggests that the significance of the coefficient of negative reciprocity in the incomplete models is spurious.

Table 1.4: OLS Regression analysis of sustainable plastics consumption

VARIABLES	(1)	(2)	(3)	(4)	(5)
Altruism	0.162***	0.113***	0.0977**	0.0343	0.0355
	(0.0413)	(0.0395)	(0.0396)	(0.0398)	(0.0400)
Trust	-0.151***	-0.0847**	-0.0864**	-0.0697*	-0.0568
	(0.0396)	(0.0379)	(0.0378)	(0.0370)	(0.0370)
Pos. reciprocity	0.132***	0.118***	0.113***	0.144***	0.126***
	(0.0405)	(0.0385)	(0.0383)	(0.0389)	(0.0395)
Neg. reciprocity	-0.0899**	0.00116	0.00140	-0.0407	-0.0217
	(0.0405)	(0.0392)	(0.0390)	(0.0389)	(0.0397)
Risk		-0.196***	-0.207***	-0.177***	-0.190***
		(0.0618)	(0.0616)	(0.0604)	(0.0602)
Ambiguity		-0.197***	-0.206***	-0.229***	-0.234***
		(0.0618)	(0.0616)	(0.0603)	(0.0603)
Patience			0.106***	0.0829**	0.0964***
			(0.0347)	(0.0341)	(0.0341)
EK				0.0804**	0.0685*
				(0.0346)	(0.0375)
EC				0.217***	0.206***
				(0.0355)	(0.0359)
Female					0.172**
					(0.0686)
Age					0.0454
					(0.0351)
Wealth index					0.0537
					(0.0409)
Education					0.0800**
					(0.0383)
Hh members					0.0924**
					(0.0374)
Hh rooms					-0.0663
					(0.0409)
Constant	-0.00313	-0.00265	-0.00273	-0.00186	-0.0975*
	(0.0358)	(0.0338)	(0.0337)	(0.0329)	(0.0501)
Observations	898	898	898	898	898
R-squared	0.055	0.161	0.169	0.210	0.228

Notes: Standard errors in parentheses (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Finally, we find evidence that higher levels of EK and EC lead to more sustainable plastics consumption, as predicted, and that women, larger households and more educated people engage in this PEB more.

1.3.2.3. Monthly spending on electricity

Table 1.5 shows the regression results for the logarithm of monthly spending on electricity. No clear picture emerges for the relevance of social preferences. The only social preference that is statistically significant at better than marginal level in the complete model specification is negative reciprocity. Its coefficient is positive, which means that people who say they are more prepared to punish others for behaviour they think is unfair also spend more on electricity. It is not a robust result, since the coefficient on negative reciprocity is only significant in model 5. Altruism is marginally significant in model 3 and model 5, but nowhere else. No social preference is thus robustly statistically significant.

Table 1.5: OLS Regression analysis of monthly spending on electricity (log)

VARIABLES	(1)	(2)	(3)	(4)	(5)
Altruism	0.0154	0.0289	0.0394*	0.0265	0.0410*
	(0.0227)	(0.0228)	(0.0228)	(0.0233)	(0.0210)
Trust	0.0414*	0.0255	0.0271	0.0333	0.0260
	(0.0219)	(0.0220)	(0.0219)	(0.0219)	(0.0196)
Pos. reciprocity	0.0297	0.0276	0.0308	0.0222	-0.0166
	(0.0223)	(0.0222)	(0.0221)	(0.0229)	(0.0209)
Neg. reciprocity	0.0227	0.00560	0.00469	0.00507	0.0549***
	(0.0224)	(0.0228)	(0.0227)	(0.0232)	(0.0212)
Risk		-0.0343	-0.0276	-0.0172	-0.0197
		(0.0352)	(0.0350)	(0.0350)	(0.0313)
Ambiguity		0.111***	0.117***	0.108***	0.0991***
		(0.0351)	(0.0349)	(0.0349)	(0.0312)
Patience			-0.0691***	-0.0684***	-0.0374**
			(0.0194)	(0.0195)	(0.0175)
EK				0.0594***	-0.0130
				(0.0200)	(0.0194)
EC				0.0121	0.0133
				(0.0208)	(0.0188)
Female					-0.0213
					(0.0356)
Age					0.0811***
					(0.0183)
Wealth index					0.216***
					(0.0217)
Education					-0.00910
					(0.0202)
Hh members					0.0779***
					(0.0197)
Hh rooms					0.0509**
					(0.0216)
Constant	4.690***	4.689***	4.690***	4.690***	4.696***
	(0.0194)	(0.0192)	(0.0191)	(0.0190)	(0.0259)
Observations	869	869	869	869	869
R-squared	0.015	0.035	0.049	0.059	0.261

Notes: Standard errors in parentheses (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Ambiguity tolerance is positively related with spending on electricity, which confirms our hypothesis H1.2b and which we reflect on in the next section, and patience is negatively related with such spending, meaning that more patient individuals have lower spending on electricity per month, which is as expected (H1.3).

As for energy-saving behaviour, we find no evidence for a relationship between EK and EC and monthly electricity expenditures, which we briefly discuss in the next section. Age and wealth clearly matter, with richer and older people spending more on electricity. Moreover, spending increases with the number of household members and household rooms. There is a much larger jump in R-squared between models 4 and 5 in Table 1.5 than there is in Tables 1.3 and 1.4. This suggests that, relative to preferences, the socio-economic control variables are more important for electricity spending than for the other two PEBs.

Table 1.6 summarizes the results obtained from the regressions of preferences and PEB for all dependent variables that we consider in our analysis.

Table 1.6: Overview of OLS regression results of preferences and PEB

	Energy-saving behaviour	Sustainable plastics consumption	Monthly spending on electricity
Altruism	+	n.s.	+
Trust	+	n.s.	n.s.
Pos. reciprocity	+	+	n.s.
Neg. reciprocity	–	n.s.	+
Risk	n.s.	–	n.s.
Ambiguity	n.s.	–	+
Patience	n.s.	+	–

Notes: + indicates a positive relationship, – a negative relationship, n.s. non-significant.

1.4. Discussion and Conclusion

In this study, we contribute to the literature that relates PEB to individual preferences. We elicit a full range of individual preferences (risk, ambiguity, time and social) instead of focusing on just one preference in isolation, to make sure preferences do not proxy for omitted ones. We link data on individual preferences to two dependent variables that have not been considered before in this literature (habitual energy-saving behaviour and sustainable plastics consumption) and thereby expand the evidence base on the importance of preferences for PEB that takes place regularly (e.g. switching off lights), as opposed to occasional behaviour (e.g. buying an energy-efficient refrigerator). Unlike previous studies, we consider the role of ambiguity preferences in predicting PEB, which is arguably conceptually more relevant than risk preferences. The reason for this is that the probability of future benefits of PEB is not typically known or easy to estimate.

For eliciting preferences, we make use of a state-of-the-art validated survey measure that allows for international comparability and replication Falk et al. (2016) and Falk et al. (2018). By focusing on households in Peru, we shed light on preference heterogeneity and its importance for PEB outside the context of high-income countries, which is rare in the literature (Fuerst and Singh (2018) for India is an exception). We focus on middle class households, which is a group that is on the rise in low and middle-income countries experiencing long-term economic growth, and the determinants of whose PEB is important to understand for helping ensure that the development of these countries is sustainable. Due to having a rich data set, we are able to control for individual characteristics such as environmental knowledge and concern, wealth, and education that are potentially correlated both with PEB and with preferences. This reduces the risk of omitted variable bias.

We find that social preferences are strongly correlated with saving-energy behaviour (switching off unnecessary lights etc.), which confirms our initial hypothesis (H1.1). Yet,

social preferences are hardly correlated with sustainable plastics consumption and with the monthly electricity bill. This demonstrates that preferences that matter for one type of PEB do not necessarily matter for another. For instance, our finding that a trusting propensity matters for saving-energy behaviour confirms previous studies on the link between trust and PEB (Gupta and Ogden, 2009; Volland, 2017; Ziegler, 2020), while we do not find support for this link with our other two dependent variables. Looking at the different types of PEB that we consider in our analysis, a reason for this finding could lie in their different nature. One the one hand, engaging in regular behaviours to save energy in the household is something that one usually does for oneself without being publically recognized for it. It is not observed by others, except for perhaps roommates or family members, and requires a strong sense of intrinsic motivation, which makes it plausible that social preferences are important. Avoiding the use of plastic bags in shops, on the other hand, is visible to other people and might therefore depend less strongly on a pro-social motivation (even though we do find a positive link for positive reciprocity and sustainable plastics consumption, but not for social preferences in general). Our analysis also shows that it is not just trust that can explain PEB (as mostly focused on in previous literature), but that other social preferences are important to consider as well.

The willingness to take risk and experience ambiguity are both negatively related with sustainable plastics consumption, which is the same as saying that both risk and ambiguity aversion are positively related with it. In other words, when people are less tolerant of risk and ambiguity, they engage more in avoiding wasteful plastic use. As stated earlier, this is at odds with most previous literature that relates PEB and risk aversion (Farsi, 2010; Qiu et al., 2014; Fischbacher et al., 2015) and contradicts our initial hypothesis (H1.2a). In that literature, the rationale given for such a link is that the benefits of PEB are uncertain, which more risk tolerant people mind less, as a result of which they engage more in such PEB. However, it is worth pointing out that it is not just the benefits of PEB that are uncertain: the costs of not engaging in PEB are uncertain, too. A risk or ambiguity averse person may thus avoid the use of plastics since the environmental damage that may result from using plastics is uncertain. Given that the smaller, regular PEBs to avoid plastics that we investigate in our study require less uncertain investment than the PEBs in the studies mentioned above (e.g. purchase of an energy-efficient appliance), the uncertainties about potential damage from not engaging in the behaviour seem to outweigh the uncertain benefits from engaging in it in this case. Our findings might also hint towards the possibility that with regards to the investment in energy-efficient technologies (which has mostly been considered as the dependent PEB in relation with risk preferences in previous research so far), the investment decision itself might dominate the pro-environmental nature of the behaviour. Future research that investigates these links more in depth would be interesting.

Our findings for risk aversion and sustainable plastics consumption are comparable to

what Volland (2017) finds for spending on energy. As illustrated before, he finds for a UK sample that higher risk tolerance is associated with greater such spending (and therefore risk aversion with less of such spending). In other words, both in his case and in our case, uncertainty aversion and PEB are positively associated, as we predicted (H1.2b). However, unlike Volland, we find no link between risk tolerance and the monthly electricity bill. Instead, we do find that the willingness to experience ambiguity is positively related with such spending. Perhaps ambiguity averse people mind the financial uncertainty more that results from profligate spending. It shows in any case the importance of including ambiguity aversion in the analysis of PEB, and not just risk aversion alone.

We find no link between risk and ambiguity aversion and habitual energy-saving behaviour. One possible interpretation is that, in the case of this PEB, the uncertain benefits of engaging in this PEB and the uncertain costs of not engaging in it are not considered to be sufficiently sizeable to be much of a worry.

More patience is positively related with sustainable plastics consumption, negatively related with the monthly electricity bill, and not significantly related with habitual energy-saving behaviour. As outlined before, previous studies have found patience to be positively related with PEB (Fischbacher et al., 2015; Newell and Siikamäki, 2015; Fuerst and Singh, 2018; Ziegler, 2020). Our findings on plastics avoidance and electricity expenditures are consistent with that and confirm our hypothesis on the link between time preferences and PEB (H1.3). The reason offered in these studies is that more patient people discount the future at a lower rate, and therefore value PEB, whose benefits are in the future, more highly. In line with that, we do not find a positive relationship between patience and PEB that also has immediate benefits (people saving money through energy-efficient behaviour) but only between patience and PEB with predominately future benefits (avoiding plastic waste).

Even though environmental knowledge and concern are not our key variables of interest in the analysis, it is worth noticing that both EK and EC positively predict sustainable plastics consumption (as one would expect), while we find no evidence for a relationship with energy-saving behaviour or the monthly electricity bill. While we can only speculate about these results, a reason could be that more environmental knowledge and concern is required to avoid the use of plastics, which is still a rather new topic in the Peruvian context, whereas regular measures to save energy in the household might already have become habits for people, regardless of their level of EK or EC. With regards to electricity expenditures, we have seen that especially socio-economic variables such as wealth or the household size are relevant predictors, which might simply outweigh any efforts resulting from higher levels of EK or EC.[13]

We see three main messages emerging from this study. First, it matters to control for

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[13] EK and EC are also positively correlated with education and wealth, which supports this hypothesis.

all relevant preferences when explaining PEB. Examples abound, in the analyses above, of the statistical significance of coefficients on preferences disappearing as we gradually add more preferences as independent variables. This means that studies that do not control for all relevant preferences may draw the wrong conclusion about which ones matter for PEB.

Second, different preferences matter for different PEBs. For habitual energy-saving behaviour, which brings only tiny benefits to the individual actor and requires a strong sense of a shared responsibility for the well-being of future generations, we found social preferences mainly to matter. For sustainable plastics consumption, we found that patience and risk and ambiguity tolerance matter: people who discount the future at a lower rate and mind more the uncertain damage of not engaging in the behaviour are more likely to engage in this particular PEB. For spending on electricity, which unlike the other two PEBs brings large benefits to the actor, patience and ambiguity aversion matter.

Third, pro-environmental policy can make use of evidence that particular preferences matter for particular PEBs. There seems to be no "one size fits all" solution to encourage PEB by appealing on people's preferences, but policies should rather be targeted specifically to the type of behaviour that one wants to promote. Our results suggest that to promote daily energy-saving habits, policy messages could emphasise that this PEB is an opportunity to care for and take responsibility for future generations. Such a strategy might be especially powerful when the target behaviour is not observed by others and a strong sense of intrinsic motivation is required. To promote the sustainable use of plastics, our results imply that the consequences of not doing so could be vividly shown to people, so that the dreadful future that would result from excessive use feels real. In general, our findings have shown that it is not just the uncertain benefits of investing in PEB that are important, but that the uncertain costs of not engaging in PEB are relevant for people's decision-making as well, which can be used to design messages more effectively. Finally, to promote energy efficiency that results in a lower monthly electricity bill, our results suggest that simple worked examples on financial savings ("you could save X %") in addition to appeals on future benefits may work.

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Appendix

1.A. Correlation matrix of preferences and PEB

Table 1.A.1: Pairwise correlations between preferences and PEB

Var.	Altruism	Trust	Pos. reciprocity	Neg. reciprocity	Risk	Ambiguity	Patience	Energy-saving beh.	Sust. plastics cons.	Electricity exp.
Altruism	1.000									
Trust	0.332***	1.000								
Pos. reciprocity	0.407***	0.264***	1.000							
Neg. reciprocity	0.191***	0.266***	-0.065*	1.000						
Risk	-0.022	0.171***	-0.076**	0.253***	1.000					
Ambiguity	-0.059*	0.183***	-0.034	0.230***	0.833***	1.000				
Patience	0.158***	0.107***	0.098***	0.072**	0.170***	0.165***	1.000			
Energy-saving beh.	0.196***	0.139***	0.237***	-0.104***	-0.014	-0.002	0.050	1.000		
Sust. plastics cons.	0.135***	-0.078**	0.148***	-0.093***	-0.350***	-0.351***	0.050	0.151***	1.000	
Electricity exp.	0.077**	0.101***	0.076**	0.057*	0.108***	0.151***	-0.074**	-0.049	-0.088***	1.000

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

1.B. Measures for preferences

The questions for risk, time and social preferences are taken from the GPS of Falk et al. (2016) and Falk et al. (2018). All questions are available for download online and can be found in various languages, which are also adjusted for local currencies: https://www.briq-institute.org/global-preferences/home. For the data collection, we used the Peruvian (Spanish) version of the GPS (using Peruvian Soles as currency). Here, we present the English wording as it is illustrated in Falk et al. (2016), listing only the questions that we use for our analysis.

1.B.1. Social preferences

We now ask for your willingness to act in a certain way in different areas. Please indicate your answer on a scale from 0 to 10, where 0 means you are "completely unwilling to do so" and a 10 means you are "very willing to do so". You can also use any numbers between 0 and 10 to indicate where you fall on the scale, like 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10.

1.B.1.1. Negative reciprocity

- *How willing are you to punish someone who treats **you** unfairly, even if there may be costs to do so?*

- *How willing are you to punish someone who treats **others** unfairly, even if there may be costs to do so*

  Completely unwilling to do so 1 − 2 − 3 − 4 − 5 − 6 − 7 − 8 − 9 − 10 very willing to do so

1.B.1.2. Altruism

- *How willing are you to give to good causes without expecting anything in return?*

  Completely unwilling to do so 1 − 2 − 3 − 4 − 5 − 6 − 7 − 8 − 9 − 10 very willing to do so

How well do the following statements describe you as a person? Please indicate your answer on a scale from 0 to 10. A 0 means "does not describe me at all" and a 10 means "describes me perfectly". You can also use any numbers between 0 and 10 to indicate where you fall on the scale, like 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10.

1.B.1.3. Positive reciprocity

- *When someone does me a favour, I am willing to return it.*

  Does not describe me at all 1 − 2 − 3 − 4 − 5 − 6 − 7 − 8 − 9 − 10 describes me perfectly

1.B.1.4. Trust

- *I assume that people have only the best intentions.*

  Does not describe me at all 1 − 2 − 3 − 4 − 5 − 6 − 7 − 8 − 9 − 10 describes me perfectly

1.B.2. Risk and ambiguity preferences

1.B.2.1. Risk

- *Please imagine the following situation: You can choose between a sure payment of a particular amount of money, or a draw, where you would have an equal chance of getting amount x or getting nothing. We will present to you five different situations. The draw with the 50/50 chance of receiving amount x or receiving nothing is the same in all situations. The sure payment is different in every situation.*

  What would you prefer: a draw with a 50 percent chance of receiving amount x, and the same 50 percent chance of receiving nothing, or the amount of y as a sure payment?

  [If the participant preferred the gamble, then the sure payment was increased, if they preferred the sure payment, then the sure payment was reduced; and they were asked the question again. This continued until the certainty equivalent value of the gamble was closely approximated (see Figure 1.B.1 for the steps that were taken).]

1.B.2.2. Ambiguity

- *Please imagine the following situation: You can choose between a sure payment of a particular amount of money, or a draw from a bag of 20 balls, where some are white and some are black. You don't know how many balls are black and how many balls are white. If you draw a white ball, you get amount x, if you draw a black ball, you get nothing. We will present to you five different situations. The draw from the bag with black and white balls is the same in all situations. The sure payment is different in every situation*

  What would you prefer: a draw from the bag of 20 balls, where you would get amount x if you drew a white ball, and nothing if you drew a black ball, or the amount of y as a sure payment?

  [The certainty equivalent value of the draw was approximated using the same staircase procedure as the one for risk (Figure 1.B.1).]

Figure 1.B.1: Tree for the staircase risk task
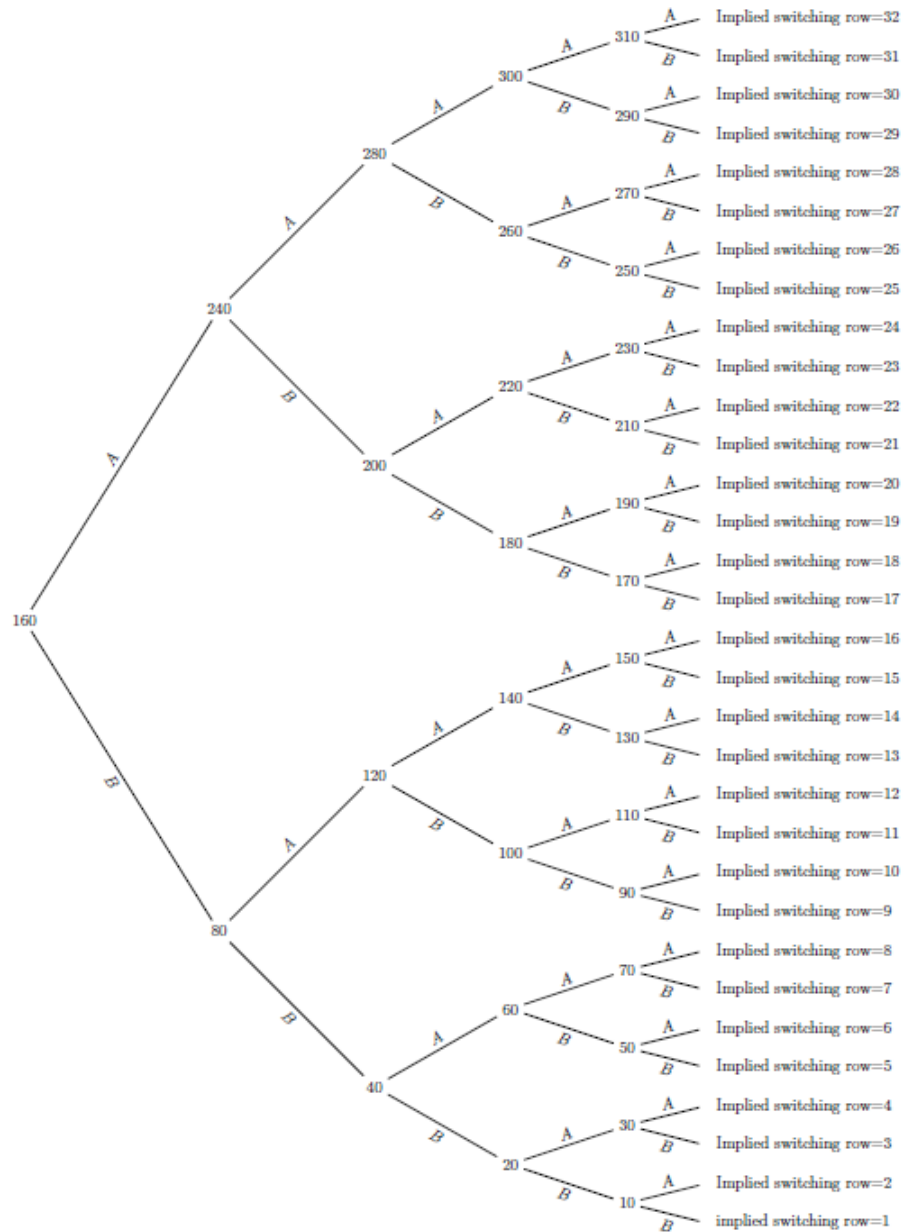
Notes: Numbers = sure payment, A = choice of lottery, B = choice of sure payment; taken from Falk et al. (2016). The lottery considered here is a 50/50 chance of 300.

1.B.3. Time preferences

- *Suppose you were given the choice between receiving a payment today or a payment in 12 months. We will now present to you 5 situations. The payment today is the same in each of these situations. The payment in 12 months is different in every situation. For each of these situations we would like to know which you would choose. Please assume there is no inflation, i.e. future prices are the same as today's prices.*

  Please consider the following: would you rather receive amount x today or amount y in 12 months?

  [The participant then chose five times between amount x, which was kept constant, and a payment in twelve months, which was increased compared to the previous choice if the future payment had been chosen and reduced if the payment today had been chosen (see Figure 1.B.2).]

Figure 1.B.2: Tree for the staircase time task
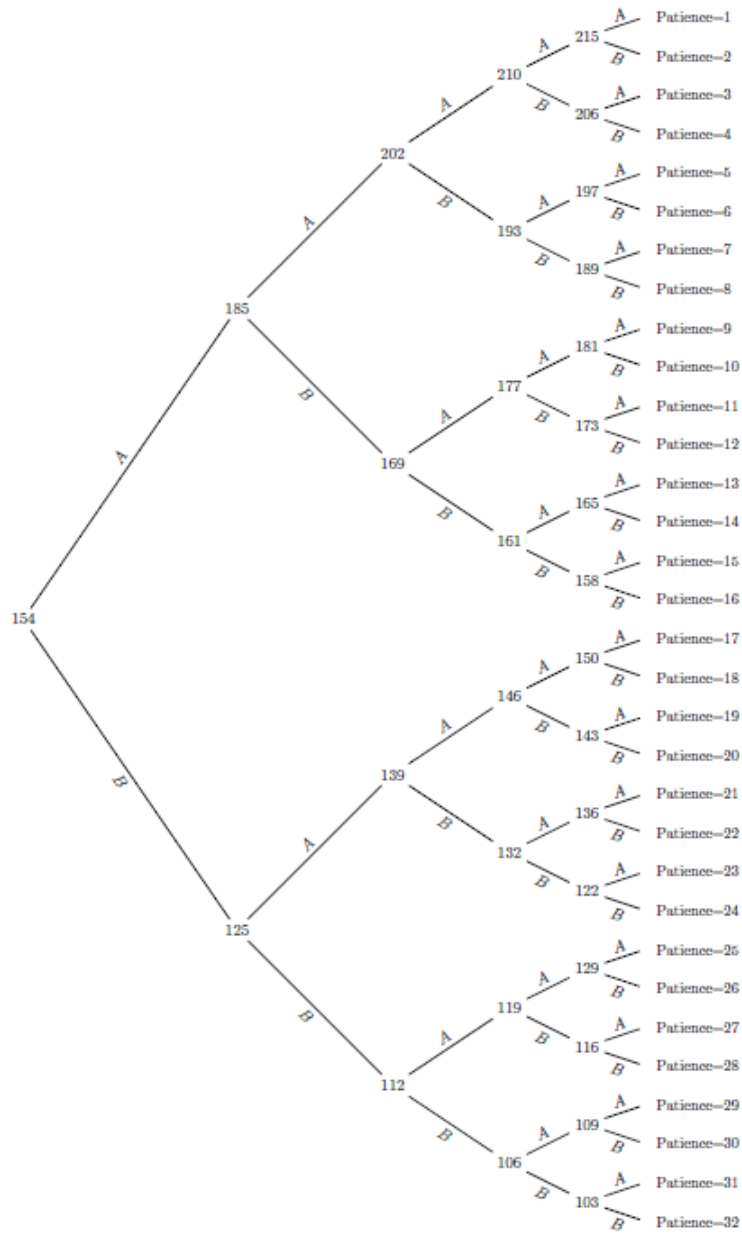
Notes: Numbers = payment in 12 months, A = choice of amount 100 today, B = choice of amount y in 12 months); taken from Falk et al. (2016). The first intertemporal choice considered is 100 today or 154 in 12 months.

1.C. Measures for Pro-Environmental Behaviour

The indices for energy-saving behaviour and sustainable plastics consumption are built based on different usage behaviour questions, which are all measured on a 5-point Likert-Scale.

1.C.1. Energy-saving behaviour

- *Do you usually switch off the lights when you leave the room?*

- *Do you usually turn off the TV if nobody is watching actively?*

- *Do you actively try to save energy in your household?*

no, nearly never (1) – yes, rarely (2) – yes, sometimes (3) – yes, often (4) – yes, nearly always (5)

1.C.2. Sustainable plastics consumption

- *Do you usually reuse materials such as plastic bags?*

- *Do you usually avoid taking plastic bags in shops (e.g. supermarkets)?*

no, nearly never (1) – yes, rarely (2) – yes, sometimes (3) – yes, often (4) – yes, nearly always (5)

1.C.3. Spending on electricity

For spending on electricity, enumerators either copied the number from the electricity bill (when participants allowed us to take a photo), or people were asked the following question.

- *Please give us your best guess how much you spent on electricity in the last month. (in Soles)*

1.D. Measures for control variables

The question for EK and EC are based on Thøgersen et al. (2010) and Thøgersen et al. (2019).

1.D.1. Environmental knowledge

The measure for EK is built using an additive index based on eight questions eliciting knowledge on different environmental dimensions. Each correct answer is counted as one, wrong answers or indifference are counted as 0.

Of the following statements, which one capture your understanding of energy saving and sustainable consumption? If you think a statement is correct, please say "yes"; if you think a statement is false, please say "no".

- *I know a lot about the topic of global climate change.*

- *I know quite a lot about the different possibilities how to save energy in my household.*

- *Compared with others, I have a good understanding of the impact of transport on air pollution.*

- *You can save energy when you set your air con 2 degrees warmer.*

- *Using a lot of energy has a negative impact on the environment.*

- *You can save energy and money in the long run when you buy a new fridge with an energy efficient technology.*

- *Whether I leave the light on the whole day or turn it off when I leave the room does matter for my energy consumption.*

- *Using public transport instead of a private car is better for the environment.*

yes – no – don't know

1.D.2. Environmental concern

The measure for EC is built using a mean index based on six questions eliciting concern for the environment and for sustainable consumption habits, which are all measured on a 5-point Likert-Scale.

How much do you agree or disagree with the following statements?

- *It is important to me that the products that I use do not harm the environment.*

- *I consider the potential environmental impact of my actions when making many of my decisions.*

- *My purchase habits are affected by my concern for our environment.*

- *I am concerned about wasting the resources of our planet.*

- *I would describe myself as environmentally responsible.*

- *I am willing to restrict myself in order to take actions that are more environmentally friendly.*

strongly disagree (1) – disagree (2) – neither agree nor disagree (3) – agree (4) – strongly agree (5)

1.D.3. Wealth index

The wealth index is built based on the following items using PCA:

- Dummy variables for a number of household assets (0 or 1): fridge, freezer, radio, fan, rice cooker, microwave, washing machine, smartphone, laptop, desktop computer, stereo, water heater, car, motorbike, bicycle

- Characteristics of the house (low (-1), medium (0), high (1)): size, material, quality, water supply

- Highest level of education of the household head (low (-1), medium (0), high (1))

1.D.4. Level of education

The level of education of the respondent is measured based on the following question, with answer options coded from 1 to 7:

- *What is your highest certificate of education?*
    - *No education certificate or pre-school (1)*
    - *Primary school / Elementary school (2)*
    - *Secondary school / High school (3)*
    - *Technical higher education (4)*
    - *Bachelor's degree (5)*
    - *Master's degree (6)*
    - *PhD / Doctorate (7)*

Chapter 2

Boosting recycling behaviour among urban households in Peru – A field experiment on the role of social norms and beliefs

Abstract

In this pre-registered study, we shed light on the role of social norms and individual beliefs about social norms for people's decision to recycle, focusing on dynamic and injunctive norms. In cooperation with a local municipality in Lima, Peru, we conduct a field experiment with 1,709 households using phone surveys to increase sign-up rates to the municipality's recycling programme. We show that individual misperceptions about dynamic and injunctive norms in the recycling behaviour of others can prevent people from signing up to the recycling programme, and that randomly correcting people's beliefs causally raises their willingness to do so. We demonstrate that individual level belief updating can explain heterogeneous responses to social norm information: our social norm information treatments effectively motivate people to sign up to the recycling programme when people initially underestimate the actual norm, while there is no effect for those who overestimate or are correct about it. We find that this holds irrespective of whether the norm is high or low, or whether it is presented in a static or dynamic way. Our findings thus underline the great importance of individual beliefs about social norms for people's decision making, and for the effectiveness of social norm interventions that directly address those beliefs. The results may help to understand why the effect sizes of social norm interventions vary considerably between studies and why they may work in some but not in other contexts. Our findings are relevant for policy makers aiming to encourage sustainable behaviours, especially in situations where unsustainable norms predominantly prevail, which is crucial in view of fighting the climate crisis.

2.1. Introduction

There is growing evidence that providing people with information about social norms influences their behaviour.[1] Moreover, recent literature shows that individual beliefs about social norms matter for people's response to such information (Bursztyn et al., 2020), and that information about how the prevalence of a desired behaviour has developed over time ("dynamic norms", Sparkman and Walton, 2017) is relevant for behaviour as well, even if behaviour contrary to the norm still predominates. We combine the two recent additions to the literature, shedding light on the importance of people's individual beliefs about social norms that include dynamic ones, which has not been studied yet. We do so in a context where new behaviour should be encouraged to bring about a vital public good, but is not yet normative in the descriptive sense, i.e. the majority of people do not yet behave in the required way, while at the same time most people privately support the behaviour (i.e. high "injunctive norm"). Many sustainable behaviours that are important in view of fighting the climate crisis fall into this category, from eating habits through transport behaviour and resource consumption. The behaviour that we focus on is that of recycling when most people do not yet do so, in Lima, the capital city of Peru.

The waste sector is a mayor contributor to climate change, and global waste accumulation is posing a serious threat to people and ecosystems (UNEP, 2015). Insufficient waste management is particularly problematic in low- and middle-income countries where the infrastructure for waste management is often worse than in developed countries, with large amounts of waste ending up in local dumpsites, threatening both people's health and the environment. Recycling is one of the key elements for a sustainable waste and resource management. Waste separation in the household is essential in this regard, as without the effort of individual consumers to separate their materials at home, there would be no functioning recycling process (Dai et al., 2015; Varotto and Spagnolli, 2017). In the case of Peru, only 4% of all waste generated in the capital city, Lima, is recycled (WWF, 2018). It is therefore essential to understand how people can be motivated to recycle, especially in those regions where waste generation is growing fast and where the consequences of improper waste management are most severe.

Municipalities are responsible for coordinating recycling activities at the household level in Peru.[2] Some municipalities in Lima have established their own recycling programmes over the last years in an effort to improve their local waste management. However, the uptake and active participation of households in these programmes is still low.

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[1] This has been found for a wide range of domains: health-related behaviours (Galizzi et al., 2022), tax compliance (Hallsworth et al., 2017), driving behaviour (Chen et al., 2017), charitable contributions (Shang and Croson, 2009), voting (Gerber and Rogers, 2009) and environmental behaviour (Allcott, 2011).

[2] For a more detailed mapping of the different actors involved in the Peruvian recycling sector see Borasino and Fuhrmann-Riebel (2021).

For our study, we have teamed up with the municipality of Miraflores, an upper middle-to high-income neighbourhood in Lima. The municipality has established a recycling programme, in which households can participate voluntarily and free of charge. Enrolled households are expected to separate their recyclable materials at home and collect them in a separate bag, which they then need to place outside their house on the street on a specific day per week to be collected by formal recyclers. At the point of data collection, only 12% of households in Miraflores were participating in the municipality's recycling programme.

Inducing behavioural change in the context of recycling is not easy. As for many other sustainable behaviours, recycling constitutes a collective action problem, where people have to incur individual costs for collective benefit (Harring et al., 2019; Sparkman et al., 2020). Individual costs can for example refer to the time and effort needed to separate materials and fill recycling bags, and collective benefit manifests itself as reduced waste accumulation and better environmental quality. Such a social dilemma situation creates incentives for individuals to free-ride, meaning to not contribute to the public good themselves (i.e. to not recycle) while still gaining the collective benefit (e.g. better environmental quality) at the expense of others' efforts. Research has shown that social norms can help to overcome such collective action problems (Bicchieri and Dimant, 2019; Nyborg et al., 2016; Ostrom, 2000; Sparkman et al., 2020).

While the concept of *social norm* has been studied extensively, its definition varies between and within disciplines (see Farrow et al., 2017, for an overview). For our research purposes, it is important to distinguish between two types of norms, descriptive norms and injunctive norms – as commonly done in the economics literature.[3] Descriptive norms say what other people do; injunctive norms refer to what other people think should be done, or approve of doing. As Hallsworth et al. (2017) point out, the two are conceptually different and should be treated as such. Moreover, a more recent stream of literature has introduced the concept of dynamic norms, which indicate how the behaviour of others has evolved over time (Sparkman and Walton, 2017). In principle, both descriptive and injunctive norms can be studied in the dynamic sense, while so far only dynamic descriptive norms have been investigated, which will also be the understanding of dynamic norms in this paper.[4]

A prominent theory in the economics literature for why people respond favourably to social norm information is that people experience moral costs when deviating from the norm, which can be a powerful way to induce behavioural change (Levitt and List, 2007), as applied for example in Allcott (2011), Byrne et al. (2018), Ferraro and Price (2013)

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[3] See for instance Krupka and Weber (2013) or Hallsworth et al. (2017), which can both be traced back to the seminal work of Cialdini et al. (1991).

[4] Dynamic injunctive norms would describe how the social approval of others has developed over time.

and Hallsworth et al. (2017). In terms of the theoretical framework of Levitt and List (2007), our study aims to motivate sign-ups to the recycling programme by increasing the moral costs of participants not to do so through different treatment messages based on social norms.

The moral cost theory is based on the assumption that the target behaviour is already performed by a current majority so that it is morally costly for people not to do so as well. Yet, a key challenge of encouraging pro-environmental behaviours is that this is often not the case. Instead, unsustainable norms such as eating meat, flying, not conserving energy or purchasing disposable products prevail. Accordingly, motivating behaviour by pointing towards a descriptive majority of others that is already setting a good example is often not possible as it does not exist. However, even though still far from representing a majority, many pro-environmental behaviours are increasing in popularity, meaning that more and more people start to engage in them.

Evidence from the dynamic norms literature shows that highlighting a positive trend in the behaviour of others can successfully encourage people to engage in a behaviour, even if there is no descriptive majority yet (Loschelder et al., 2019; Mortensen et al., 2019; Sparkman and Walton, 2017; Sparkman and Walton, 2019; Sparkman et al., 2020). In the case of the recycling programme in Lima we are working with, current participation rates are still low (12%) while the number of households participating in the programme has doubled over the last three years, from 6% in 2017 to 12% in 2020, indicating a clear positive trend. In our study, we investigate whether informing people about the dynamic norm in participation rates can motivate people to sign up to the recycling programme, suggesting that awareness of the dynamic norm raises the moral cost of not recycling.

Since it is not only morally costly to deviate from what other people do, but also from what other people approve of doing (Hallsworth et al., 2017), we also study people's response to information about the injunctive norm regarding participation in the recycling programme. For that purpose, we conducted a pre-survey with 100 households in Miraflores to elicit people's injunctive norm beliefs, i.e. their sense of importance and social approval for participating in the recycling programme. We found that of the 100 people we asked, 97% think that participating in the recycling programme is important for protecting the environment, indicating a strong injunctive norm. Consequently, we face a situation with a low current prevalence (12%), considerable increase over the last years (doubled from 6% to 12%) and high social approval (97%).

The question then arises why so many people privately support the participation in the recycling programme but only few people actually participate themselves. We hypothesize that biased beliefs about both the positive trend and the high social approval can be a reason. If people privately think recycling is important but at the same time believe that recycling is currently rare and not likely to become common, they might not be willing

to make the effort themselves, thinking it is acceptable not no do it. Assuming that people are "conditional cooperators", they should be more willing to engage in recycling behaviour when they believe a higher share of others is or will be doing so as well (Andre et al., 2021; Frey and Meier, 2004). Also, people might believe that other people think recycling is not important since hardly anyone else seems to engage in the behaviour. A situation where most people privately support a norm but incorrectly believe that most other people do not is referred to as "pluralistic ignorance" (Katz and Allport, 1931; Bursztyn et al., 2020; Andre et al., 2021). Given the collective action nature of recycling, the positive effects of someone's own actions would be negligible if hardly anyone else recycled – a phenomenon characteristic for many pro-environmental behaviours (Kinzig et al., 2013). Thus, biased beliefs about the positive trend and the high social approval might hinder people from engaging in the behaviour themselves.[5]

In our study, we measure whether systematic misperceptions about dynamic and injunctive norms in participation of households in the recycling programme exist, and whether correcting such biased beliefs through information treatments can causally increase people's sign-up decision. We test the effect of dynamic and injunctive norm information in a 2x2 design, where one arm corresponds to the injunctive norm information and the other arm to the dynamic norm information. This enables the analysis of each type of norm individually and the combined effect of the two on people's sign-up decision.

In doing so, we are to our knowledge the first to contrast the effect of dynamic and injunctive norm information for behavioural change in a field experimental context. Moreover, we are aware of only few studies existing so far that can clearly identify the role of individual prior beliefs for people's response to social norm information treatments that directly aim at correcting those beliefs.[6] Bursztyn et al. (2020) report the importance of misperceived social norms in the context of female labour force participation in Saudi Arabia; Byrne et al. (2018) document the role of prior beliefs about people's own energy consumption relative to their peers;[7] and a recent paper by Andre et al. (2021) shows the importance of misperceptions about people's willingness to fight climate change. All three studies show that prior beliefs matter, and that they can explain variations in people's response to information treatments that directly address those beliefs, resulting in het-

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[5] The findings of Jachimowicz et al. (2018) on the role of second-order normative beliefs for pro-environmental behaviour (in their case energy saving behaviour) support the hypothesis that beliefs about what other people regard as important matter, and further show that they can be even more important than people's individual first-order beliefs about the importance.

[6] In contrast to previous studies that either document biases in beliefs without correcting them, provide information without measuring baseline beliefs, or measure only beliefs with post-treatment surveys, while still interpreting treatment effects as a result of prior errors in beliefs, as Byrne et al. (2018) point out.

[7] The authors thus use a peer comparison approach to address beliefs, which is different from our design.

erogeneous treatment effects, i.e. treatment effects that differ in accordance with the sign and magnitude of the biased belief. In particular, Bursztyn et al. (2020) and Andre et al. (2021) show that providing people with social norm information is more (and in fact only) effective to motivate behaviour among those people who previously underestimate the actual norm. In this study, we shed light on the role of belief updating in the effects on behaviour of injunctive as well as dynamic norms. To our knowledge, the importance of prior beliefs regarding dynamic norms has not previously been investigated. We thus add important new evidence on the role of belief updating with regards to dynamic norm information, as well as information in view of still dominating opposing norms.

Our findings can be summarized as follows. People's response to information about social norms depends on their prior beliefs about these social norms. This is true both for dynamic and for injunctive norms. For those who underestimate the norm, being provided with information about it significantly increases their sign-up decisions compared to a control group. In the case of the dynamic norm, this applies to both belief updating on the six percentage point increase as well as on the current 12 percent participation rate. In the case of the injunctive norm, it applies to those who underestimate the injunctive norm more strongly. There is no such effect among those who overestimate or are correct about the norm.[8] We further show that the effectiveness of our treatments increases in the size of belief updating, i.e. the more strongly people underestimate the actual norm. Our findings are in line with the ones of Bursztyn et al. (2020) and Andre et al. (2021) who also both report positive treatment effects only among those who underestimate the social norm information they receive.[9]

With this paper, we therefore provide evidence that social norm information can be (and in fact turns out to be only) effective to encourage behaviour when people underestimate the actual norm, irrespective of the size of the norm or whether the norm is presented in a static or dynamic way. In a sense, we therefore show that the moral cost theory of Levitt and List (2007) only holds when people learn that they are deviating from what other people are doing or approve of doing if this information exceeds their initial beliefs about it. These findings can help to explain why the effect sizes of social norm information treatments vary considerably between studies (see e.g. Abrahamse and Steg (2013) for a meta-analysis), which has important policy implications.

We contribute to three important areas of research. Firstly, we contribute to the recent literature that investigates the role of dynamic norms for people's behaviour (Loschelder et al., 2019; Mortensen et al., 2019; Sparkman and Walton, 2017; Sparkman and Walton,

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[8] Nor is there an overall effect on sign-up rates aggregated across the social norm information treatments. The reason for this is that people who underestimate the dynamic norm or more strongly underestimate the injunctive norm are in the minority in this particular context.

[9] In their cases the people that underestimated the norms were in the majority, which resulted in positive average treatment effects.

2019; Sparkman et al., 2020), which is crucial for a variety of settings in view of the many global environmental challenges we are facing.[10] Our paper also connects to recent work that studies the evolution of norms (Young, 2015) and norm change (Andreoni et al., 2021). To our knowledge, we are the first to compare the effect of dynamic and injunctive norms on behaviour in the field, analysing both their individual and their joint influence. Moreover, we add new evidence on the effectiveness of dynamic norms in situations where the desired target behaviour is only performed by a small share of people in society.[11]

Secondly, we contribute to the few papers existing so far that combine measuring people's individual prior beliefs with social norm information treatments that directly aim at correcting those beliefs (Andre et al., 2021; Byrne et al., 2018; Bursztyn et al., 2020), which is important for understanding why social norm information may or may not be effective to motivate behaviour in a certain situation. In this context, we also relate to the work of Jachimowicz et al. (2018), which outlines the importance of second-order normative beliefs for encouraging pro-environmental behaviour. To our knowledge, we are the first to analyse the importance of prior beliefs in the context of dynamic norms, which adds important new evidence on the role of belief updating in view of dynamic information and prevailing opposing norms.

Thirdly, we expand evidence on the growing literature that uses messages based on social influence to encourage behaviour in general and pro-environmental behaviour in particular, such as energy conservation (Allcott, 2011; Allcott and Rogers, 2014; Andor et al., 2020; Ayres et al., 2013; Bonan et al., 2020; Nolan et al., 2008; Schultz et al., 2007), water conservation (Brent et al., 2015; Datta et al., 2015; Ferraro et al., 2011; Ferraro and Price, 2013; Lede et al., 2019; Schultz et al., 2016), recycling (Schultz, 1999) or the reuse of towels in hotels (Goldstein et al., 2008), adding novel evidence on the relevance of social norms for people's decision to recycle in the Peruvian context.[12]

The paper proceeds as follows. We introduce the experimental design, including the

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[10] The effectiveness of dynamic norms compared to other information treatments has recently been underlined in a mega-study by Milkman et al. (2021).

[11] It should be noted that the way dynamic norms have so far been used in the literature differs between studies. Loschelder et al. (2019) only give information about the fact that "more and more" people are engaging in the behaviour without linking it to any descriptive numbers. Sparkman and Walton (2017) give information about the fact that a certain percentage of people (in their case 30%) has started to engage in the behaviour, simply representing the descriptive number of people that is already engaging in the behaviour in a dynamic way (30% have started to do so vs. 30% are doing so). Our approach is more similar to the one of Mortensen et al. (2019), where people are informed about the current prevalence in the behaviour of others, in addition to how this number has developed over time. In the case of Mortensen et al. (2019), however, the number of people engaging in the target behaviour was already at 48%, and thus considerably closer to being a descriptive majority norm than in our case.

[12] A study conducted by Chong et al. (2013) tested the effects of different messages, including descriptive social norms, on recycling behaviour of households in Peru and did not find any significant effects. Yet, the authors acknowledge that certain features in their design might have been the reason for these null results.

experimental procedure and research hypotheses, in section 2.2, followed by the descriptive presentation of the data in section 2.3. We then turn to the results of our analysis on the effectiveness of social norm information treatments based on individual prior beliefs in section 2.4. We end the paper with a discussion and conclusion in section 2.5.

2.2. Experimental Design

The core of the study is three-fold: (1) eliciting prior beliefs about the social dynamic and injunctive norms, (2) delivering information treatments about those norms, and (3) eliciting households' decisions of enrolment in the recycling programme. All three core activities were implemented via a single phone survey detailed below.

For the first core activity, belief elicitation, we used an incentivized mechanism to elicit people's prior beliefs about dynamic and injunctive norms regarding participation in the recycling programme. We define the dynamic norm belief as the difference between people's beliefs about current participation in the recycling programme and the participation three years ago. This calculation is based on the responses to these two questions:

- Belief about the current participation: *"Out of every 100 households in Miraflores, how many do you think are currently participating in the municipality's recycling programme? All numbers between 0 and 100 are allowed."* (integer 0-100)

- Belief about the past participation three years ago: *"Compared to your previous answer, how many out of every 100 households in Miraflores do you think were participating in the programme three years ago (end of 2017)? Again, all numbers between 0 and 100 are allowed."* (integer 0-100)

We then measured people's beliefs about the injunctive norm on the idea that participating in the recycling programme is important. We collected both first-order and second-order beliefs. First-order beliefs refer to whether the respondent considers recycling important and second-order beliefs to the degree that the respondent thinks others regard recycling as important. We use the following questions:

- First-order belief about the importance of recycling: *"Do you think that it is important for the environment that households in Miraflores participate in the municipality's recycling programme?"* (yes, no, don't know)

- Second-order belief about the injunctive norm: *"We asked 100 households in Miraflores to answer the same question we just asked you, so whether it is important for the environment that households in Miraflores participate in the municipality's recycling programme. How many of those 100 do you think said yes? All numbers between 0 and 100 are allowed."* (integer 0-100)

For the second core activity of our experiment, participants were randomly assigned

to one of the four experimental conditions based on a 2x2 between-subject design, where one treatment arm corresponds to the dynamic norm and the other to the injunctive one. Table 2.1 summarises this design.

Table 2.1: Treatment groups

	Injunctive norm message	Dynamic norm message
Control group (A)	–	–
Injunctive norm treatment (B)	✓	–
Dynamic norm treatment (C)	–	✓
Combined treatment (D)	✓	✓

Notes: The table shows the different treatment groups within the 2x2 design. The control group (A) did not receive any social norm message, the injunctive norm treatment (B) received only the injunctive norm message, the dynamic norm treatment (C) received only the dynamic norm message, the combined treatment (D) received both the dynamic and the injunctive norm message.

The treatment messages were proposed by the authors and fine-tuned with inputs from our local partners and officials from the municipality. The English versions of the two types of messages are as follows:

- Dynamic norm message: *"The number of households in Miraflores that are participating in the municipality's recycling programme has doubled, from 6% to 12%, over the last three years!"*

- Injunctive norm message: *"Of the 100 households in Miraflores we asked, 97% think that it is important for the environment that households participate in the municipality's recycling programme!"*

The treatment messages were directly followed by the third core-activity, the question of whether the household would like to sign up for the recycling programme. The sign-up decision was measured as a binary variable (yes/no) and is our main outcome variable.

2.2.1. Experimental procedure

Before we conducted the actual experiment, we deployed an initial pre-survey with 100 households to measure the prevalence of the injunctive norm regarding participation in the recycling programme in Miraflores. In this brief survey, we asked participants their first-order beliefs about the importance of recycling (whether they considered it important that households in Miraflores participate in the municipality's recycling programme, asked in the same wording as the question presented above). These responses, combined with the municipality's information about the recycling programme's participation rate, allowed us to design the treatment messages that we discussed above.

Our experiment was conducted via phone surveys through a survey company in February and March 2021. Participants were recruited from the official data base of the municipality of Miraflores that contains all households registered in the district with respective phone numbers and addresses. Enumerators called households in a randomly

generated order. We programmed the survey using the software SurveyCTO, which enumerators then used to fill in the responses. The software allowed us to monitor incoming data in real-time and apply quality checks promptly when needed.

The protocol for the survey was as follows (see Figure 2.1). When a respondent answered the call, enumerators first explained who they were and stated that they were conducting a survey in collaboration with the municipality's recycling programme. Enumerators then asked whether the household was already enrolled in the programme, and if so, thanked the respondent and ended the call, as we were interested in the households that were not yet part of the programme. If a household was not enrolled yet, the survey continued, and enumerators informed respondents that the data would be treated with confidentiality and analysed anonymously. Respondents were further informed that they could win a prize (one of 15 gift cards of 50 Soles each) for completing the survey and asked to give their verbal consent to participate in the study.[13]

If the respondent agreed to take part in the study, the enumerator proceeded with the actual survey and briefly explained the recycling programme. Enumerators proceeded with the first core activity of the experiment; that is, we elicited people's prior beliefs about dynamic and injunctive norms regarding participation in the recycling programme in the district and their personal first-order beliefs about the importance of recycling. We incentivized respondents to provide their beliefs by offering them the opportunity to win another gift card of 10 Soles for each question where their belief was correct, in addition to the incentive for participation.

We then collected demographic information about the respondent and the household in general, and asked some control questions. These general questions also served as buffer questions between the beliefs questions and the treatment messages, since they both focused on the same information. This way, we aimed to reduce the potential effect that respondents might still be thinking, for example, about the injunctive norm (as they were asked about it) when receiving the dynamic norm information in the treatment message, and vice versa.

In the next step, the treatment messages were conveyed, following the treatment design described above. Respondents were assigned to one of the four conditions (A-D) at random with equal probabilities.

After conveying the treatment messages and asking for people's sign-up decision, we elicited people's post-treatment beliefs about future norms in recycling behaviour as well as personal and collective response efficacy. We further collected information on additional

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[13] Gift cards could be used in the department store Saga Falabella. The incentives were chosen based on discussions with local partners in Peru and represented 50 local currency units (Soles or PEN), which is approximately equal to 15 USD. Using data from household surveys, we estimate the average daily wage in urban Peru ranges between 15 USD and 20 USD.

control variables that are related to recycling and were therefore asked after the sign-up decision to not influence the respondents' decisions and reduce potential experimenter demand effects.

Finally, we collected the contact details for the household (in case the household wanted to sign up for the programme) and informed respondents that they would receive an official registration link from the municipality within the following days via email or WhatsApp (depending on the participant's preference) through which they would need to officially enrol in the programme.[14] All participants were further informed that they would be notified whether they won the gift cards after completion of the data collection.

Figure 2.1: Experimental procedure

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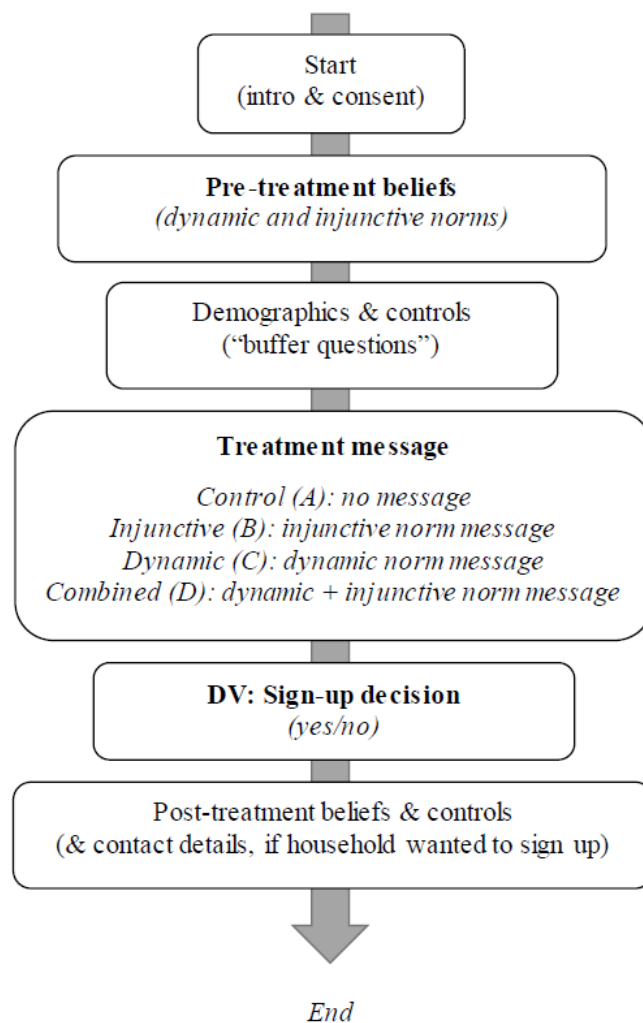
[14] The message contained the link for officially signing up to the programme, as well as a reinforcement of the treatment message that the participant had already heard when the survey was administered. Ideally, we would have liked official enrolment to take place during the survey, but the municipality insisted that this took place in a separate step. We elicited people's sign-up decisions during the survey before informing them about the necessity to make this official later, and therefore can be confident that these decisions reflect their genuine commitment at the time it was expressed. The municipality-imposed official requirement allowed us to check whether this commitment was followed through.

2.2.2. Hypotheses

We hypothesize that the effectiveness of our treatments will depend on the distribution of individual prior beliefs about dynamic and/or injunctive norms, depending on the treatment. In particular, this means that we expect the message in the injunctive norm treatment B to be more effective among those people that previously underestimate the injunctive norm, in the dynamic norm treatment C among those people that previously underestimate the dynamic norm, and in the combined treatment D among those people that previously underestimate both types of norms. We derive these hypotheses based on the moral cost theory of Levitt and List (2007), and argue that not recycling has a moral cost. When people then either learn that 1) more other people than expected think recycling is important (injunctive norm treatment B) or 2) that the number of other people that recycle is increasing more than expected (dynamic norm treatment C) or 3) both (combined treatment D), the moral cost increases. As a consequence, people are more likely to sign up to the recycling programme. Concretely, this leads to the following hypotheses based on individual level belief updating:

Hypothesis 2.1 (H2.1: Belief updating on injunctive norm) *The average sign-up decision among people who underestimate the injunctive norm is higher in the injunctive norm treatment B than in the control group A.*

Hypothesis 2.2 (H2.2: Belief updating on dynamic norm) *The average sign-up decision among people who underestimate the dynamic norm is higher in the dynamic norm treatment C than in the control group A.*

Hypothesis 2.3 (H2.3: Belief updating on dynamic and injunctive norm) *The average sign-up decision among people who underestimate the dynamic norm and the injunctive norm is higher in the combined treatment D than in the control group A.*

We further acknowledge that the dynamic norm treatment C as well as the combined treatment D also convey information about the low current prevalence in participation rates. We therefore expect the effect of the message in treatment C and treatment D to be particularly effective among those people that previously underestimate the current participation rate, which leads to a fourth hypothesis:

Hypothesis 2.4 (H2.4: Belief updating on current participation) *The average sign-up decision among people who underestimate the current participation rate is higher in the dynamic norm treatment C and in the combined treatment D than in the control group A, respectively.*

2.3. Data

2.3.1. Descriptive statistics

2.3.1.1. Demographics

In total, 1,709 people participated in our study.[15] Our sample consists of 38.0% male, 61.7% female and 0.3% diverse people. The average age is 53.7 years, with a minimum age of 18 and a maximum age of 95 years. Most of our participants are either the head of the household (58.9%) or the spouse (26.1%). 59.4% of our participants hold either a technical university or university degree, which reflects the upper-middle class nature of the district where we deployed this study. The mean number of household members is 2.9 people, the average number of children per household is 0.4. The majority of participants (67.4%) state to be responsible for recycling within the household themselves, while 62.0% of the households also indicate that they already recycle through other ways than the municipality's recycling programme. Table 2.B.1 in the Appendix shows that individual characteristics are mostly balanced across the different treatment groups, confirming that the randomization was successful.

2.3.1.2. Prior beliefs

The following table (Table 2.2) gives an overview of the beliefs distribution.[16] From Table 2.2 we can see that, on average, people overestimate the current and past participation in the recycling programme, as well as the dynamic norm (trend in participation rates), although less strongly. On average, people underestimate the injunctive norm. Table 2.B.2 in the Appendix shows that the beliefs distribution is mostly balanced across treatment groups.

Also, 97.7% of the people in our sample answered the first-order injunctive norm beliefs question with yes, i.e. whether they think that it is important for the environment that

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[15] The data base of the municipality contained roughly 40,000 registered addresses with respective phone numbers in total. Of the households that were called, 3,040 picked up the phone. Of those 3,040 households, 2,442 said that they were not yet participating in the recycling programme. Of those 2,442 eligible households, 1,711 agreed to take part in our study. We aimed for a total sample of at least 1,600 households as pre-registered based on power calculations and slightly over-sampled to have some buffer in case of data issues. Two submissions had to be excluded, because enumerators had entered an undefinable household ID in two cases, which led to a final sample of 1,709 households.

[16] Enumerators were instructed to enter "99" for the beliefs whenever a participant did not want to answer the beliefs question. Therefore, all "99" entries were recoded as missing values. This led to 198 missings for the belief about the current participation, and 189 missings for the belief about the past participation. In the case of the injunctive norm belief, "99" was very close to the true value of 97. Therefore, there is a risk that "99" entries did not always mean that the person did not want to answer the question. Therefore, we only recoded "99" entries for the injunctive norm belief as missing values when the other beliefs had "99" entries as well (mostly people either answered all or no beliefs questions), which led to 127 missings. The remaining 26 "99" entries for the injunctive norm belief all had reasonable entries for the other beliefs, supporting our assumption that in those cases the "99" was actually people's true belief.

households in Miraflores participate in the municipality's recycling programme, which is very similar to the 97% obtained from our pre-intervention survey.

Figure 2.2 shows the biases between individual beliefs and true values for the current participation, past participation, dynamic norm and injunctive norm, respectively. A bias of zero (indicated by the vertical red line) means that people's guess is correct, below zero means that people underestimate the true value, above zero means that they overestimate it.

Table 2.2: Beliefs distribution

Belief type	True value	Belief mean	% that underestimate	N
Current participation	12%	35.31%	23.23%	1,511
Past participation	6%	22.31%	28.36%	1,520
Dynamic norm (current-past)	6%	13.04%	38.14%	1,505
Injunctive norm	97%	80.21%	67.13%	1,582

Notes: The table shows the true value, mean and percentage of people that underestimate the true value of the respective belief type. The remaining percentage of people thus overestimate or are correct about it.

We present a correlation matrix of the different beliefs in the Appendix (Table 2.C.1). Not surprisingly, beliefs about the current and past participation are correlated with each other, as well as with beliefs about the dynamic norm, given that the dynamic norm belief is calculated as the difference between the other two beliefs. Interestingly, also dynamic and injunctive norm beliefs are positively correlated with each other. We note that this correlation might complicate our analysis, as the information treatments aim to influence one type of beliefs at a time. We will address potential confounding effects with a more sophisticated regression analysis, which we will explain in detail at a later stage in the paper.

2.4. Results

2.4.1. Average treatment effects

Before we turn to our main results of interest on heterogeneous treatment effects based on individual prior beliefs about the different norms, we look at the average effects of our social norm information treatments across the whole sample. We find that, in total, 70.39% of the participants (1,203 out of 1,709 people) said that they would like to sign up to the recycling programme during our phone survey.[17] We show in Figure 2.E.1 and

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[17] Interestingly, of those 70% that decided to sign up to the recycling programme during the phone survey, only 10% really did so when receiving the link (118 out of 1,203; sign-ups were monitored over the subsequent five months after the survey was conducted). We find this striking since - as indicated before - when being asked during the phone survey whether they would now like to sign up to the recycling programme, people did not know that they would have to do so through a link in a separate

Figure 2.2: Biases in beliefs (guess - true value)

(a) Current participation

(b) Past participation

(c) Dynamic norm
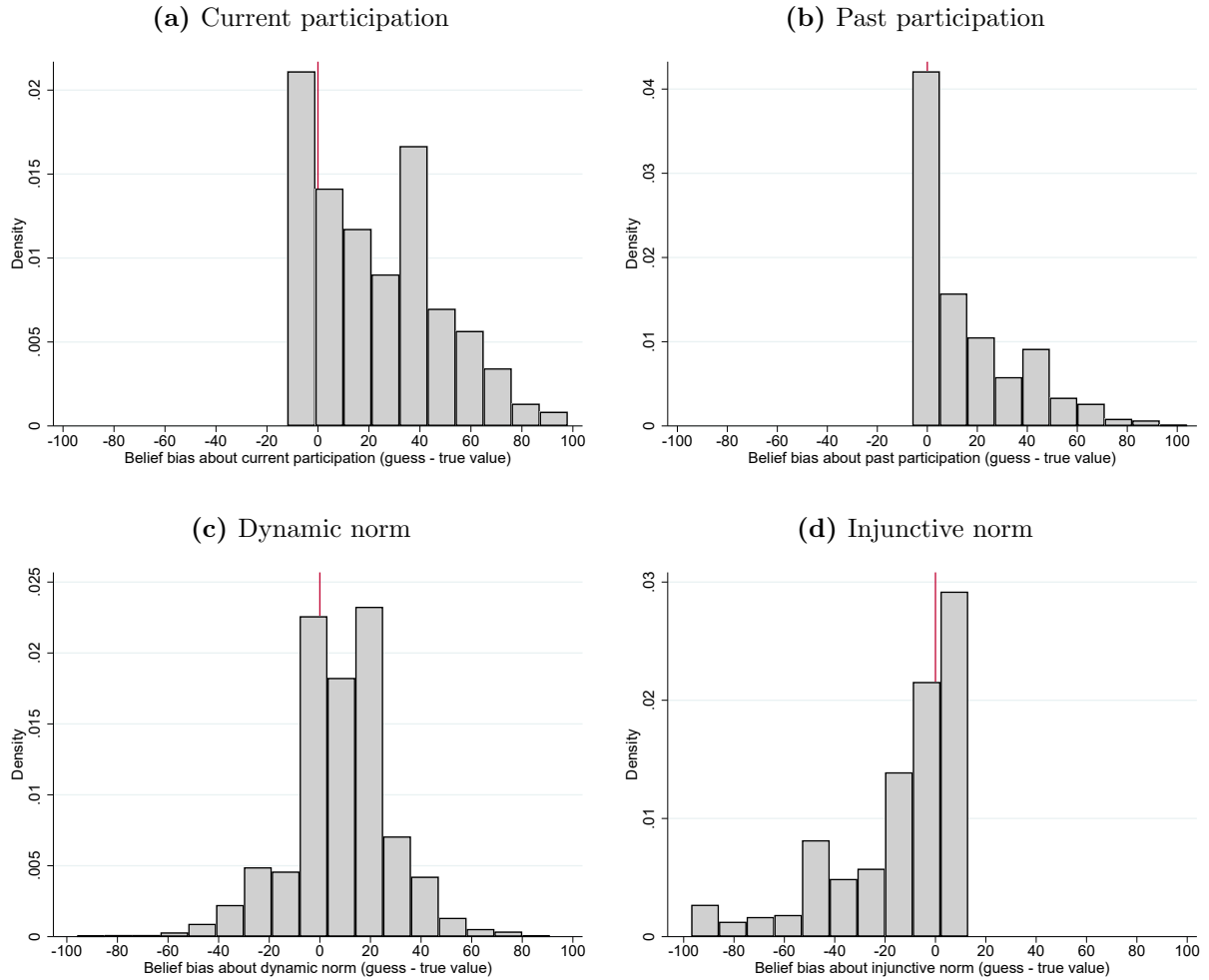
(d) Injunctive norm

Notes: Individual biases in beliefs are calculated as the difference between the participant's guess and the true value of the current participation, past participation, dynamic norm, and injunctive norm, respectively. The vertical red line indicates a bias of zero, meaning that the participant's guess was correct.

Tables 2.E.1 and 2.E.2 in the Appendix that there is no statistically significant difference in people's sign-up decision between the different treatment groups and the control group, using both OLS and logistic regressions and adding various control variables.

Throughout the paper, we control in all regressions for gender (whether the respondent is female), whether the household has children, whether the respondent is the household head, level of education (whether the respondent has a technical university or university degree), level of patience,[18] whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways than the

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step afterwards (they were only informed about this at the end of the survey when their contact details were collected), so that their decision to sign up during the survey can be seen as a robust measure of people's true commitment in that moment. We turn to this in the discussion at the end of our paper.

[18] Research has shown that time preferences matter for sustainable plastics consumption in Peru (Fuhrmann-Riebel et al., 2021).

municipality's recycling programme.[19] We present both OLS regressions for better inter-pretation of the results, as well as logistic regressions as additional robustness checks.

2.4.2. Heterogeneous treatment effects based on individual prior beliefs

2.4.2.1. Beliefs about the dynamic norm

We now look at heterogeneous treatment effects based on individual level belief updating. When looking at people's beliefs about the dynamic norm (trend in participation rates in the recycling programme), we find that correcting people's beliefs in the dynamic norm treatment C has a significant positive effect on people's sign-up decision for those who underestimate the increase in six percentage points, which is as expected (Figure 2.3a): the average sign-up decision increases from 58.16% in the control group to 72.79% in the treatment group, which represents a 25% increase (p=0.010 from testing for equality of proportions, n(A)=141 and n(C)=136). For those who overestimate or are correct about the trend in participation rates, there is no significant difference in sign-up decisions compared to the control group (Figure 2.3b).

We see a slight positive tendency for the combined treatment D in Figure 2.3a as well, although here the difference in sign-up decisions is not statistically significant for those who underestimate the dynamic norm compared to the control group. Again, there is no significant difference in sign-up decisions for those who overestimate or are correct about the trend (Figure 2.3b).

For people in the injunctive norm treatment B, where no information about the dynamic norm is conveyed, there is no significant difference in average sign-up decisions compared to the control group, either, although also here we can see a positive tendency (Figure 2.3a). Figure 2.3b further shows that there is no significant difference in sign-up decisions for those who overestimate or are correct about the dynamic norm in the injunctive norm treatment either.

It is interesting to note that among people in the control group, the average sign-up decision is significantly higher when people have more optimistic beliefs about the trend in participation rates in the recycling programme (58.16% in 2.3a vs. 72.69% in 2.3b, p=0.003 from testing for equality of proportions, n(A) in 2.3a=141 and n(A) in 2.3b=249), and that by correcting people's beliefs when people underestimate the dynamic norm in treatment C, the average sign-up decision increases to similar levels as for people in the control group who already have more optimistic beliefs in advance (72.79% in treatment C in 2.3a vs. 72.69% in the control group A in 2.3b), which is not statistically different from each other (p=0.983 from testing for equality of proportions, n(C)=136 and n(A)=249). These findings provide additional evidence that individual beliefs about dynamic norms

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[19] A correlation table of control variables and beliefs can be found in the Appendix (2.D).

in the recycling behaviour of others matter for people's decision to sign up to the recycling programme.

Table 2.F.1 in the Appendix shows that these results are robust when using OLS regression analyses, including when control variables are added. We provide a further robustness check with logistic regressions in Table 2.F.2. We can thus confirm our hypothesis H2.2 for the dynamic norm treatment C.

Figure 2.3: Heterogeneity in average sign-up decisions between people who under- or overestimate the dynamic norm

(a) Dynamic norm beliefs < 6%   **(b)** Dynamic norm beliefs >= 6%
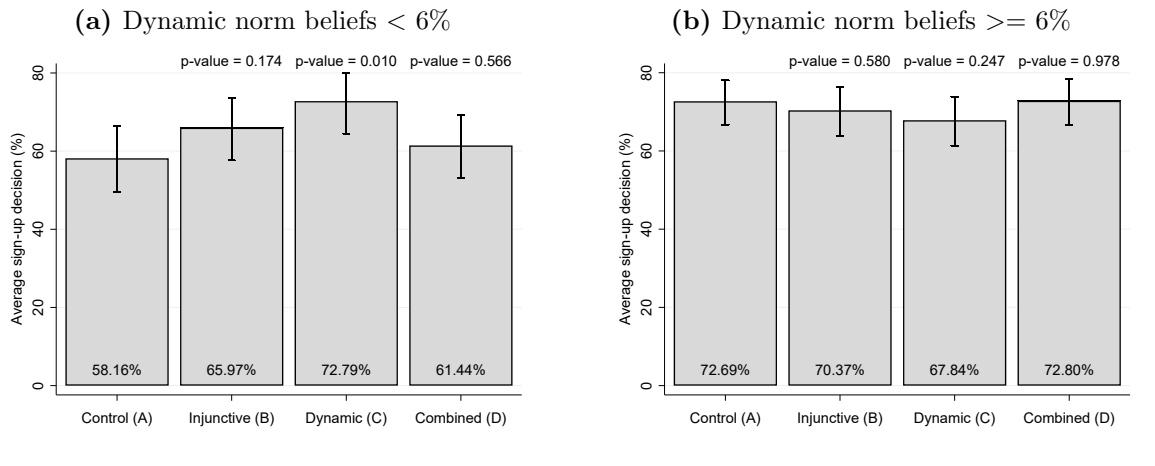
Notes: Average sign-up decision of people who under- or overestimate the dynamic norm, comparing the different treatments B-D with the control group A. n(2.3a)=574, n(2.3b)=931. p-values are obtained from testing for equality of proportions, comparing treatment B, C and D with treatment A, respectively. Graphs show the average sign-up decision by treatment, with 95% confidence intervals for proportions.

2.4.2.2. Beliefs about the current participation

Since the dynamic norm treatment C and the combined treatment D convey information about both the positive trend in participation rates (increase by six percentage points) and the current descriptive participation rate (12%), we investigate heterogeneous treatment effects with regards to beliefs about the current participation rate as well. We find again that correcting people's beliefs in the dynamic norm treatment C has a significant positive effect on people's sign-up decision for those who underestimate the participation rate of 12 percent (Figure 2.4a), which increases from 49.43% in the control group to 68.67% in the treatment group, representing a 39% increase (p=0.011 from testing for equality of proportions, n(A)=87 and n(C)=83). There is no statistically significant difference in people's sign-up decision for those who overestimate or are correct about the current participation rate (Figure 2.4b).

We find a similar pattern for the combined treatment D, where the average sign-up decision for those who underestimate the current participation rate increases from 49.34% in the control group to 63.73% in the treatment group, which represents a 29% increase (Figure 2.4a: p=0.048 from testing for equality of proportions, n(A)=87 and n(D)=102).

Again, there is no statistically significant difference in people's sign-up decision for those who overestimate or are correct about the current participation rate (Figure 2.4b).

Figure 2.4: Heterogeneity in average sign-up decisions between people who under- or overestimate the current participation

(a) Current participation beliefs < 12%   (b) Current participation beliefs >= 12%
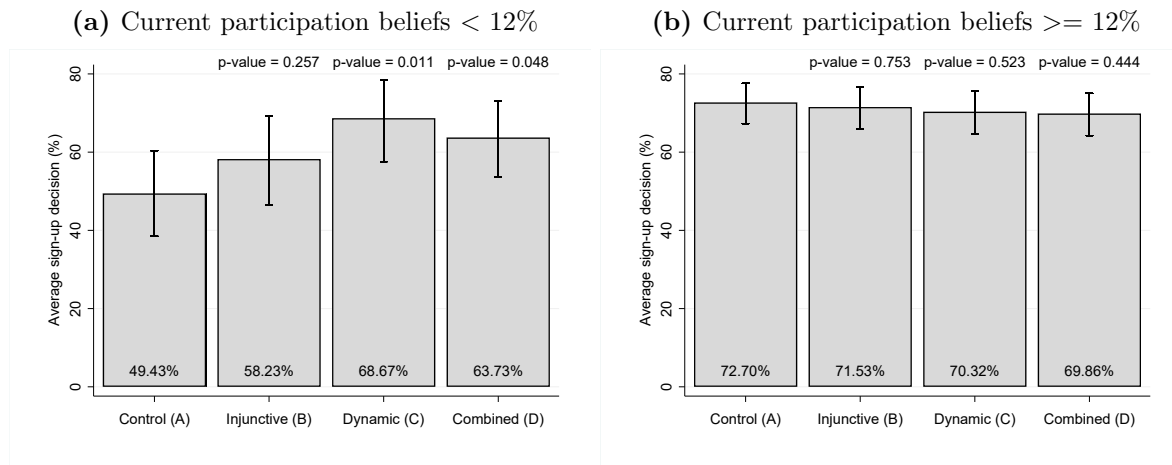
Notes: Average sign-up decision of people who under- or overestimate the current participation, comparing the different treatments B-D with the control group A. n(2.4a)=351, n(2.4b)=1,160. p-values are obtained from testing for equality of proportions, comparing treatment B, C and D with treatment A, respectively. Graphs show the average sign-up decision by treatment, with 95% confidence intervals for proportions.

The average sign-up decision does not increase significantly in the injunctive norm treatment B compared to the control group, where no information about the current participation rate is conveyed, even though again we can see a slight positive tendency here as well (Figure 2.4a). There is no statistically significant difference in people's sign-up decision for those who overestimate or are correct about the current participation rate in treatment B either (Figure 2.4b).

As for people's belief about the dynamic norm, we find that among participants in the control group, the average sign-up decision is significantly higher when people already have more optimistic beliefs about the current participation rate in the recycling programme (49.43% in 2.4a vs. 72.70% in 2.4b, p=0.000 from testing for equality of proportions, n(A) in 2.4a=87 and n(A) in 2.4b=304), and that by correcting people's beliefs when people underestimate the current participation rate in the dynamic norm treatment C, the average sign-up decision increases to similar levels as for people in the control group who already have more optimistic beliefs in advance (68.67% in treatment C in 2.4a vs. 72.70% in the control group A in 2.4b, p=0.470 from testing for equality of proportions, n(C)=83 and n(A)=304). In the case of the combined treatment D, even though the average sign-up decision increases significantly compared to the control group for those who initially underestimate the current participation rate, the level is still lower compared to people in the control group with more optimistic beliefs (63.73% in treatment D in 2.4a vs. 72.70% in the control group A in 2.4b, p=0.086 from testing for equality of proportions,

n(D)=102 and n(A)=304).

Again, we use OLS regression analyses in Table 2.F.3 in the Appendix to show that these findings are robust, including when control variables are added. We provide a further robustness check with logistic regressions in Table 2.F.4. We can thus confirm our hypothesis H2.4 for both the dynamic norm treatment C and the combined treatment D.[20]

2.4.2.3. Beliefs about the injunctive norm

Looking at people's injunctive norm beliefs about participation in the recycling programme, we find a slight tendency that correcting beliefs in the injunctive norm treatment B has a positive effect on people's sign-up decision for those who underestimate the 97 percent approval (Figure 2.5a): the average sign-up decision increases from 67.36% in the control group to 70.04% in the treatment group, although the difference is not statistically significant. For those who overestimate or are correct about the injunctive norm, there is no statistically significant difference in sign-up decisions between treatment and control group either (Figure 2.5b).

Also for the combined treatment D, the difference in sign-up decisions for those who underestimate the injunctive norm is not statistically significant compared to the control group (Figure 2.5a). Again, there is no significant difference in sign-up decisions for those who overestimate or are correct about the injunctive norm (Figure 2.5b).

For people in the dynamic norm treatment C, where no information about the injunctive norm is conveyed, there is no statistically significant difference in sign-up decisions compared to the control group, either (Figure 2.5a). Also here, there is no significant difference for those who overestimate or are correct about the injunctive norm (Figure 2.5b). Tables 2.F.5 and 2.F.6 in the Appendix confirm these findings using OLS and logistic regressions, respectively.

When we look at those who underestimate the injunctive norm more strongly in Figure 2.6, however, the difference in sign-up decisions between the injunctive norm treatment B and the control group A increases and becomes statistically significant for those who underestimate the injunctive norm only slightly more and expect it to be below 90% (Figure 2.6a: 66.67% in A vs. 75.64% in B, p=0.074 from testing for equality of proportions, n(A)=171 and n(B)=156). For the combined treatment D, the difference becomes statistically significant when people underestimate the injunctive norm more strongly and

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[20] Even though we did not pre-register any hypotheses on heterogeneous treatment effects based on beliefs about the past participation rate in the recycling programme, we recognize that the dynamic norm treatment and the combined treatment also convey information about the past recycling behaviour of households. While conceptually this should be less relevant for people's decision to sign up to the programme, we present the analysis on the comparison of average sign-up decisions between people who under- or overestimate the past participation rate nonetheless in the Appendix (2.G).

expect it to be below 50% (Figure 2.6d: 67.86% in A vs. 90.00% in D, p=0.039 from testing for equality of proportions, n(A)=28 and n(D)=30). In general, we find that the difference in sign-up decisions becomes larger the more strongly people underestimate the true value, although the respective sample sizes become quite small.

Figure 2.5: Heterogeneity in average sign-up decisions between people who under- or overestimate the injunctive norm
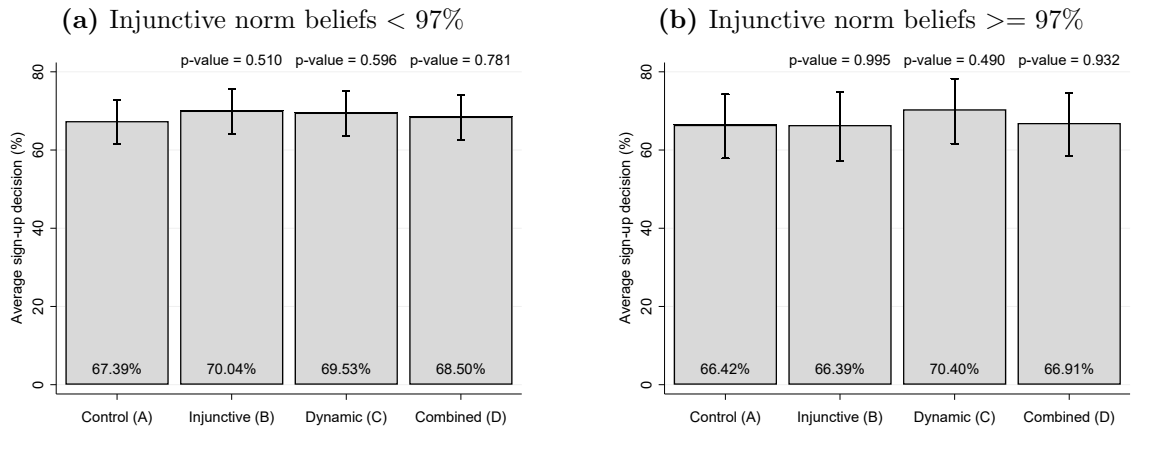
Notes: Average sign-up decision of people who under- or overestimate the injunctive norm, comparing the different treatments B-D with the control group A. n(2.5a)=1,026, n(2.5b)=520. p-values are obtained from testing for equality of proportions, comparing treatment B, C and D with treatment A, respectively. Graphs show the average sign-up decision by treatment, with 95% confidence intervals for proportions.

Interestingly, we also find that the sign-up decision for people in the dynamic norm treatment C increases significantly compared to the control group when people underestimate the injunctive norm more strongly, even though the dynamic norm treatment message does not contain any direct information about the injunctive norm. This suggests that belief updating on the different types of norms might be correlated, which is supported by the fact that individual beliefs about the different norms are correlated, too.

Table 2.F.7 in the Appendix confirms the findings from Figure 2.6 using OLS regressions that for those who underestimate the injunctive norm even slightly more (below 90%), the injunctive norm treatment B does have a significant effect on people's decision to sign up to the recycling programme, which remains robust also when adding control variables. We can further see in the development from columns (1) to (8) that the effect size of the injunctive norm treatment B increases the more strongly people underestimate the injunctive norm, which suggests that the effect of belief updating depends on the individual distance of people's initial beliefs about the injunctive norm and the true value (i.e. the individual bias in beliefs). We turn to this in the next step of our analysis. Table 2.F.8 provides an additional robustness check using logistic regressions. We can thus confirm our hypothesis H2.1 for the injunctive norm treatment B for people who underestimate the injunctive norm more strongly (or considerably more in case of the combined treatment D).

Figure 2.6: Heterogeneity in average sign-up decisions between people who underestimate the injunctive norm more strongly

(a) Injunctive norm beliefs < 90%

(b) Injunctive norm beliefs < 70%

(c) Injunctive norm beliefs < 60%

(d) Injunctive norm beliefs < 50%
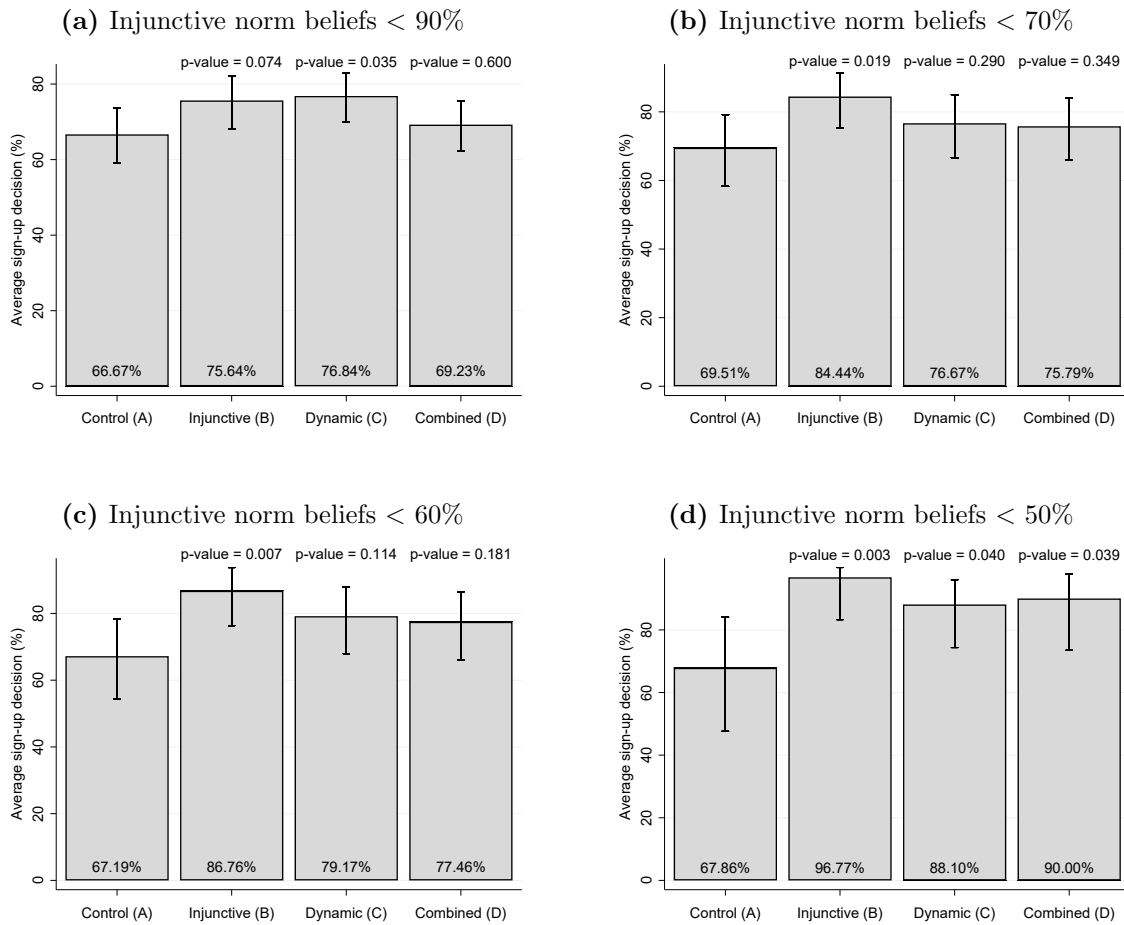
Notes: Average sign-up decision of people who underestimate the injunctive norm more strongly, comparing the different treatments B-D with the control group A. n(2.6a)=699, n(2.6b)=357, n(2.6c)=275, n(2.6d)=131. p-values are obtained from testing for equality of proportions, comparing treatment B, C and D with treatment A, respectively. Graphs show the average sign-up decision by treatment, with 95% confidence intervals for proportions.

2.4.2.4. Beliefs about the dynamic and injunctive norm combined

Besides looking at heterogeneous responses to our social norm information treatments based on the under- or overestimation of the two different norms individually, we also investigate the four possible scenarios of under- vs. overestimation of the two types of norms combined. We can see in Figure 2.7a that there is a tendency that correcting people's combined beliefs in the combined treatment D has a slight positive effect on the average sign-up decision for those who underestimate both types of information, which increases from 59.00% in the control group to 64.36% in the treatment group, although the difference is not statistically significant. The only treatment where the average sign-up decision does increase significantly compared to the control group for people who underestimate both types of norms is the dynamic norm treatment C (Figure 2.7a: 59.00% in A vs. 73.74% in C, p=0.028 from testing for equality of proportions, n(A)=100 and

n(C)=99). In all other scenarios, where people either underestimate one and overestimate the other type of norm (Figure 2.7b and 2.7c), or where people overestimate both types of information (Figure 2.7d), there is no statistically significant effect of the different treatments on people's sign-up decision compared to the control group.

Figure 2.7: Heterogeneity in average sign-up decisions between people who under- or overestimate the dynamic norm and the injunctive norm combined

(a) Dynamic norm beliefs < 6% & Injunctive norm beliefs < 97%

(b) Dynamic norm beliefs < 6% & Injunctive norm beliefs >= 97%

(c) Dynamic norm beliefs >= 6% & Injunctive norm beliefs < 97%

(d) Dynamic norm beliefs >= 6% & Injunctive norm beliefs >= 97%
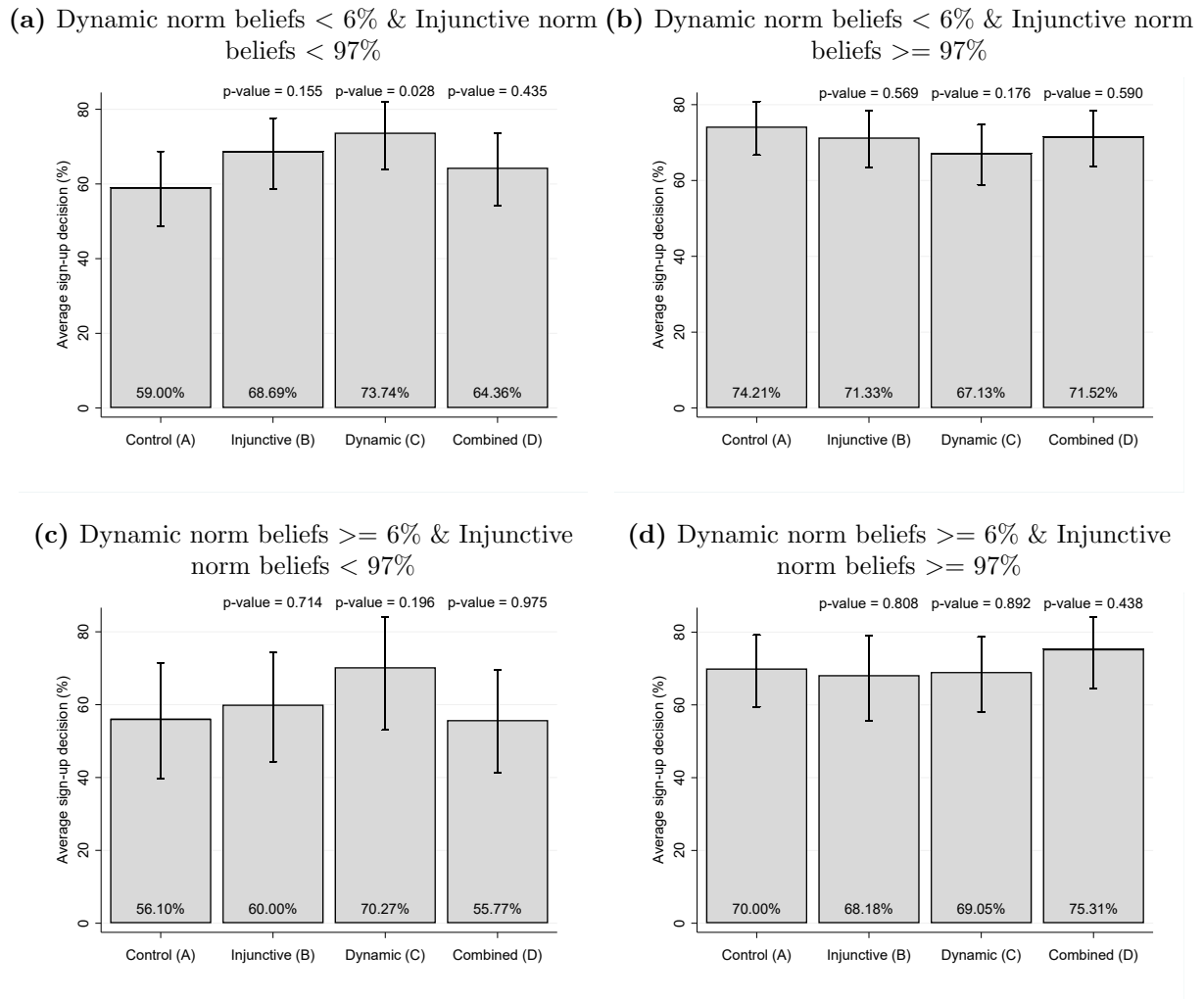
Notes: Average sign-up decision of people who under- or overestimate the dynamic norm and the injunctive norm combined, comparing the different treatments B-D with the control group A. N(2.7a)=399, n(2.7b)=321, n(2.7c)=175, n(2.7d)=610. p-values are obtained from testing for equality of proportions, comparing treatments B, C and D with treatment A, respectively. Graphs show the average sign-up decision by treatment, with 95% confidence intervals for proportions.

Columns (1) and (2) in Table 2.F.9 in the Appendix confirm the findings from Figure 2.7a using OLS regressions that there is no significant effect on people's sign-up decision of the combined treatment D for those who underestimate both the dynamic norm and the injunctive norm. However, as in the figure we can see in the table that there is a significant positive effect of the dynamic norm treatment C, which also holds when control variables are added. Columns (3) and (4) confirm the findings from Figure 2.7b that there is no effect for those who underestimate the dynamic norm and overestimate or are correct

about the injunctive norm, although the dynamic norm treatment C becomes a significant predictor when control variables are added. Columns (5) and (6) and (7) and (8) confirm the findings from Figure 2.7c and 2.7d that there is no significant treatment effect in the other two scenarios. Table 2.F.10 shows similar results using logistic regressions.

Given that the injunctive norm treatment B only showed an effect on people's sign-up decision when people underestimate the injunctive norm more strongly, we perform the same analysis with the combination of dynamic norm beliefs and injunctive norm beliefs with a threshold of 90%. The results can be found in the Appendix in Table 2.F.11, while also here the combined treatment D does not lead to significantly higher sign-up decisions when people underestimate both the dynamic norm and believe the injunctive norm to be below 90%.[21] We can therefore not confirm our hypothesis H2.3 for the combined treatment D. We turn to the interpretation of these findings in the discussion at the end of the paper.

2.4.2.5. The level of underestimation of the different norms

We now go a step further to see whether the effect of our treatments on people's sign-up decision increases in the level of underestimation of the different norms. To do so, we build a continuous measure of people's individual biases in beliefs based on the participant's guess minus the true value of the respective norm (individual bias = participant's guess – true value). This means that, for example, if someone believes the injunctive norm to be at 80% while its true value is 97%, the individual bias in beliefs would equal –17 (80 – 97 = –17). In order to better interpret the results, we use the level of underestimation defined as the negative of this bias for our regressions (i.e. in the example this would then be 17) to be interacted with the different treatment dummies (level of underestimation = individual bias *(–1)). The variable is set to zero for those who overestimate the respective norm as we are interested in the treatment effects based on the level of underestimation.

When we look at Table 2.3, we can see in columns (1) and (2) that the interaction of the level of underestimation of the injunctive norm and the injunctive norm treatment B has a significant positive effect on people's sign-up decision. This means that the more strongly people underestimate the injunctive norm in the injunctive norm treatment B, the greater the effect of the injunctive norm treatment message. Following the argumentation of Bursztyn et al. (2020), if one assumes that people take the information provided in the treatment message to update their beliefs about the injunctive norm, we can interpret the continuous underestimation measure as a continuous measure of belief updating. Based on this reasoning, we find that higher levels of belief updating about the injunctive norm lead to significantly higher sign-up decisions in the injunctive norm treatment B.

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[21] We provide the same analyses for the combination of beliefs about the current participation rate and the injunctive norm in the Appendix as well in Tables 2.F.13, 2.F.14, 2.F.15 and 2.F.16.

Interestingly, and as already indicated before, we can also see that the interaction of the level of underestimation of the injunctive norm with the dynamic norm treatment C significantly increases the sign-up decision. This means that the effectiveness of the dynamic norm treatment message increases the greater the level of underestimation of the injunctive norm. Given that beliefs about dynamic and injunctive norms are correlated (Table 2.C.1), we interpret this in a way that belief updating about the two types of norms seems to be correlated, too. Thus, when people have low beliefs about the injunctive norm and are then informed about the positive trend in recycling behaviour in the dynamic norm treatment message, they not only update their beliefs about the dynamic norm but also about the injunctive norm and are as a consequence more likely to decide to sign up to the programme.

Moving on to columns (3) and (4), we can see that the interaction of the level of underestimation of the dynamic norm with the dynamic norm treatment C has a significant positive effect on people's sign-up decision. Accordingly, the effectiveness of the dynamic norm treatment message increases the more strongly people underestimate the trend in participation rates in the dynamic norm treatment C. Again, if one assumes people use the information provided in the treatment message to update their beliefs about the dynamic norm, we can interpret these findings that higher levels of belief updating about the dynamic norm lead to significantly higher sign-up decisions in the dynamic norm treatment C.

We further see that the level of underestimation of the dynamic norm alone has a significant negative effect on people's sign-up decision. This means that the decision to sign up to the recycling programme for people in the control group decreases in the level of underestimation of the dynamic norm. This suggests, once again, that individual beliefs about dynamic norms in the recycling behaviour of other households matter for people's decision to sign up to the programme.

Looking at columns (5) and (6), we find that the interaction of the level of underestimation of the current participation rate both with the dynamic norm treatment C and the combined treatment D has a significant positive effect on people's sign-up decision. This means that both the effectiveness of the dynamic norm treatment C and the combined treatment D increases the more strongly people underestimate the current participation rate. Following the argumentation from before, we can interpret these findings that higher levels of belief updating about the current participation rate lead to significantly higher sign-up decisions in the two treatments.

We can again see that the level of underestimation of the current participation rate alone has a significant negative effect on the sign-up decision. This means that for people in the control group, the decision to sign up to the recycling programme decreases the more strongly people underestimate the current participation rate. It shows again that

personal beliefs about the current recycling behaviour of others matter for people's own decision to sign up to the programme.

In columns (7) and (8) we take a look at the interaction terms of treatments with the level of underestimation of the dynamic norm and the current participation rate together. Our aim is to disentangle what element of the dynamic norm treatment message drives the change in sign-up decisions in the dynamic norm treatment C as both beliefs about the trend and about the current participation rate seem to matter when investigating their relevance individually. We can see in column (7) that both the interaction of the dynamic norm treatment C with the level of underestimation of the dynamic norm and with the level of underestimation of the current participation rate remain statistically significant when both are included in one model. However, when we include control variables in column (8), the effect size of the interaction of the dynamic norm treatment C with the level of underestimation of the current participation rate decreases by half and is no longer significant, while the effect size of the interaction with the level of underestimation of the dynamic norm remains the same and its significance increases.

The same pattern occurs when we add all interaction terms together in columns (9) and (10). While the significance of the interaction of the dynamic norm treatment C with the level of underestimation of the dynamic norm increases, the interaction with the level of underestimation of the current participation rate is no longer significant. We interpret this in a way that both beliefs about the trend and about the current participation rate are relevant for people's response to the dynamic norm treatment message, while beliefs about the trend in participation rates seem to explain people's behaviour in the dynamic norm treatment C better given that the findings are more robust. Interestingly, also the interaction of the level of underestimation of the injunctive norm with the dynamic norm treatment C remains marginally significant in model (10), suggesting that belief updating on the injunctive norm, as a consequence of correlated beliefs of the two norms, might explain some of the effects in the dynamic norm treatment C as well (although the effect only remains significant when control variables are added, which is less consistent).

We can also see in columns (9) and (10) that the interaction effect of the combined treatment D and the level of underestimation of the current participation rate remains a significant positive predictor when all interactions are included together in one model, as does the interaction of the injunctive norm treatment B with the level of underestimation of the injunctive norm.

Table 2.3: Heterogeneous treatment effects based on the level of underestimation of the different norms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Injunctive (B)	-0.047	-0.046	0.007	0.019	0.009	0.025	0.003	0.024	-0.062	-0.042
	(0.042)	(0.039)	(0.038)	(0.035)	(0.036)	(0.034)	(0.039)	(0.037)	(0.047)	(0.045)
Dynamic (C)	-0.021	-0.014	-0.014	-0.004	-0.010	0.013	-0.040	-0.017	-0.080*	-0.058
	(0.042)	(0.039)	(0.037)	(0.035)	(0.036)	(0.034)	(0.039)	(0.037)	(0.046)	(0.044)
Combined (D)	-0.027	-0.014	0.024	0.031	-0.013	-0.003	0.003	0.009	-0.025	-0.016
	(0.041)	(0.037)	(0.036)	(0.033)	(0.035)	(0.033)	(0.038)	(0.036)	(0.045)	(0.042)
Underestimation of injunctive norm	0.000	-0.001							0.001	-0.000
	(0.001)	(0.001)							(0.001)	(0.001)
Injunctive (B) × Underestimation of injunctive norm	0.004***	0.004***							0.003**	0.004***
	(0.001)	(0.001)							(0.001)	(0.001)
Dynamic (C) × Underestimation of injunctive norm	0.002*	0.003**							0.002	0.002*
	(0.001)	(0.001)							(0.001)	(0.001)
Combined (D) × Underestimation of injunctive norm	0.002	0.002							0.001	0.001
	(0.001)	(0.001)							(0.001)	(0.001)
Underestimation of dynamic norm			-0.006**	-0.006***			-0.004	-0.005**	-0.004	-0.005**
			(0.003)	(0.002)			(0.003)	(0.002)	(0.003)	(0.002)
Injunctive (B) × Underestimation of dynamic norm			0.001	-0.000			0.002	0.001	0.003	0.002
			(0.004)	(0.003)			(0.004)	(0.003)	(0.004)	(0.003)
Dynamic (C) × Underestimation of dynamic norm			0.008**	0.007**			0.007*	0.007**	0.007*	0.007**
			(0.004)	(0.003)			(0.004)	(0.003)	(0.004)	(0.003)
Combined (D) × Underestimation of dynamic norm			-0.002	-0.001			-0.003	-0.002	-0.003	-0.002
			(0.004)	(0.003)			(0.004)	(0.003)	(0.004)	(0.003)
Underestimation of current participation					-0.035***	-0.018***	-0.033***	-0.015**	-0.034***	-0.016**
					(0.007)	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)
Injunctive (B) × Underestimation of current participation					0.001	-0.007	0.000	-0.007	-0.001	-0.008
					(0.010)	(0.008)	(0.010)	(0.008)	(0.009)	(0.008)
Dynamic (C) × Underestimation of current participation					0.025**	0.014*	0.022**	0.011	0.021**	0.011
					(0.010)	(0.008)	(0.010)	(0.008)	(0.010)	(0.008)
Combined (D) × Underestimation of current participation					0.017*	0.015*	0.019**	0.017**	0.018*	0.016**
					(0.010)	(0.008)	(0.010)	(0.008)	(0.010)	(0.008)
Constant	0.668***	0.083*	0.701***	0.123**	0.725***	0.153***	0.742***	0.166***	0.726***	0.168***
	(0.029)	(0.051)	(0.026)	(0.051)	(0.025)	(0.053)	(0.027)	(0.054)	(0.032)	(0.056)
Controls		✓		✓		✓		✓		✓
Adjusted R^2	0.012	0.172	0.012	0.174	0.034	0.169	0.043	0.181	0.062	0.191
Observations	1582	1582	1505	1505	1511	1511	1505	1505	1505	1505

Notes: OLS regressions with dependent variable equal to sign-up decision = yes. The level of underestimation is calculated as the difference between the participant's guess minus the true value of the respective norm. Columns (1) and (2) look at interactions of treatments with degrees of underestimation of the injunctive norm; columns (3) and (4) at interactions of treatments with levels of underestimation of the dynamic norm; and columns (5) and (6) at interactions of treatments with underestimation about the current participation rate. Columns (7) and (8) combine the latter two; columns (9) and (10) include all interaction terms together. Columns (1), (3), (5), (7) and (9) are without, columns (2), (4), (6), (8) and (10) with control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. *** , ** , * indicate significance levels at 1, 5, and 10%, respectively. For the regression table with the full list of control variables see Table 2.F.17 and for the robustness check with logit regressions see Table 2.F.18 in the Appendix.

2.5. Discussion and Conclusion

In this paper, we provide evidence that individual beliefs about dynamic and injunctive norms in the recycling behaviour of others matter for people's decision to recycle as well. In particular, we show that correcting biased beliefs about dynamic and injunctive norms significantly increases people's decision to sign up to the recycling programme of a local municipality in Lima, Peru, compared to a control group when people initially underestimate the true norm. In the case of the dynamic norm, this holds both for the dynamic element of the dynamic norm treatment message about the trend in participation rates as well as for the static low descriptive norm element about the current participation rate; in the case of the injunctive norm, it holds for those who underestimate the injunctive norm more strongly. We show that by correcting biased beliefs, sign-up levels are increased to similar levels as for people in the control group who already have more optimistic beliefs in advance.

With this paper, we therefore contribute to the few papers existing so far that provide evidence that individual level belief updating can explain heterogeneous responses to social norm information. We show that this effect increases in the level of underestimation, i.e. the more strongly people underestimate the social norm information they receive, the greater the effect of the treatment messages that directly correct those beliefs. We are the first to document the importance of individual level belief updating in the context of dynamic norms and show that it is relevant even when only a small share of others is already engaging in the target behaviour while opposing norms predominantly prevail.

Our findings are in line with the results of Bursztyn et al. (2020) and Andre et al. (2021), who also report significant increases in their respective target behaviours only among the people who underestimate the social norm information they receive. A crucial difference to both studies is that in our case the people who underestimate the dynamic norm or more strongly underestimate the injunctive norm are in the minority, while the ones who overestimate it are in the majority, for which we do not find significant treatment effects. While both Bursztyn et al. (2020) and Andre et al. (2021) make the same observations, in their case the people who underestimate the respective norms are in the majority, which results in positive treatment effects among the whole sample, on average. In our case, we cannot identify positive effects of our treatments on average, as the positive effects from the sub-population that underestimates the dynamic norm or more strongly underestimates the injunctive norm information is overruled by the non-significant effects among those that overestimate the norms.

By providing evidence on heterogeneous responses to social norm information as a result of divergent prior beliefs about the respective norms, we identify an important factor to explain why the effectiveness of social norm information treatments may vary considerably between different contexts. In our setting, the effectiveness of our treatments

depends strongly on the distribution of beliefs in our target population: while we do not find an average effect of our treatments on people's sign-up decision compared to the control group, we find unifying evidence that the treatments do have an average effect when we restrict the analysis to those who underestimate the respective norm (or more strongly in the case of the injunctive norm). At the same time, we do not find any significant effects for those who overestimate or are correct about the respective norm. We thus show that social norm information is only effective in encouraging behavioural change when people underestimate the actual norm, irrespective of the size of the norm or whether the norm is presented in a static or dynamic way.

We show that even though the percentage of households that is participating in the re-cycling programme is only at 12% in our study context, we still find that informing people about the positive trend in participation rates from 6% to 12% motivates people to sign up to the programme when they initially underestimate the dynamic norm. In this sense, both information about the six percentage point increase as well as information about the current participation rate of 12% are relevant for people's decision to sign up to the programme, while the dynamic element seems to be of greater importance. Our findings thus underline the great effectiveness of dynamic norms for overcoming collective action problems even when the percentage of people engaging in the desired target behaviour is still very low. These findings are promising in view of the major environmental challenges we are facing, for which we need a norm change in many domains, from dietary choices over transport behaviours to resource consumption. Our results suggest that especially in situations where it can be assumed that the majority of people underestimates the pos-itive trend in the desired target behaviour, informing people about these dynamic norms can be used as a successful tool to encourage new behaviours.

We are the first to investigate the combined effect of dynamic and injunctive norm information. We show that beliefs about dynamic and injunctive norms are positively correlated, and that belief updating about the two norms seems to be correlated as well. Interestingly, we find that the dynamic and injunctive norm messages work better indi-vidually than combined, even when people underestimate both types of information. It may be that the two messages are addressing different, and potentially conflicting, chan-nels: while the injunctive norm message may trigger the response to do what the majority approves, the dynamic norm message may activate the sense of wanting to belong to the new, progressive group of people that is engaging in the behaviour.

Our results help to understand how social norms influence people's pro-environmental behaviour, and how misperceptions about social norms can prevent people from engaging in the behaviour themselves, which has important policy implications. For policy makers aiming to promote new sustainable behaviours in a certain target group, it may be worth gathering information on beliefs about different social norms among the population first.

If the majority of people either underestimates the positive trend in the target behaviour or the social approval by other people, our results (in line with previous evidence) suggest that communication strategies that inform people about those dynamic or injunctive norms will be effective to motivate people to engage in the behaviour. Based on our findings, these communications should rather only address one type of norm individually instead of combining the two. If it is not possible to gather information about beliefs among the target population first, our results also suggest that including dynamic or injunctive norm information in communication strategies should "do no harm". While it may be that the information may simply not have any effect on people's behaviour, we do not find any evidence for potentially negative effects among those who overestimate the actual norm.

Even if not the main focus of our paper, we find it striking and very interesting that only 10% of the people that decided to sign up to the recycling programme during the phone survey really did so when receiving the official registration link afterwards. From a policy perspective, this shows that social norm information can be used to address potentially biased beliefs and effectively motivate behaviour if the actions are immediate and of rather low effort, while the positive effects seem to diminish when additional effort (in our case to click on the link, open the website and fill in the contact details) is required.

Overall, our findings on the effectiveness of injunctive and dynamic norms to motivate sustainable behaviour even in view of dominating opposing unsustainable norms are promising. As long as a meaningful number of people has started to engage in the behaviour or approves the behaviour so that it exceeds people's beliefs about it, social norms can be used effectively to encourage new behaviours.

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Appendix

2.A. Survey questions

The following variables were collected during the phone survey (here presented in English, while the survey was conducted in Spanish).

2.A.1. Prior beliefs

I am now going to ask you about your beliefs about recycling in Miraflores. For every belief that is accurate, you can win an extra 10 Soles Falabella gift card, in addition to the 50 Soles Falabella gift card that you can win for your participation.

- Dynamic norm belief:

  Note: People's dynamic norm belief is calculated as the difference between people's belief about participation in the recycling programme in the present and in the past (three years ago).

  - Belief about current participation: Out of every 100 households in Miraflores, how many do you think are currently participating in the municipality's recycling programme? All numbers between 0 and 100 are allowed. *(integer 0-100)*

  - Belief about past participation three years ago: Compared to your answer before, how many out of every 100 households in Miraflores do you think were participating in the programme three years ago (end of 2017)? Again, all numbers between 0 and 100 are allowed. *(integer 0-100)*

- First-order belief about importance: Do you think that it is important for the environment that households in Miraflores participate in the municipality's recycling programme? *(yes/no/don't know)*

- Second-order belief about injunctive norm: We asked 100 households in Miraflores to answer the same question we just asked you, so whether it is important for the environment that households in Miraflores participate in the municipality's recycling programme. How many of those 100 do you think said yes? *(integer 0-100)*

2.A.2. Demographics and control variables

- Time preferences (based on Falk et al., 2016; Falk et al., 2018): Please answer the following question on a scale from 0 to 10, where 0 means you are "completely unwilling to do so" and a 10 means you are "very willing to do so".

  How willing are you to give up something that is beneficial for you today in order to benefit more from that in the future? *(Likert-scale from 0 to 10)*

- Relation to household head: What is your relation to the head of the household?

  - *Household head*

- *Spouse or partner*

- *Parent or parent-in-law*

- *Child or stepchild or son-/daughter-in-law*

- *Grandchild*

- *Sibling*

- *Housemaid*

- *Other*

- *Does not want to say*

- Gender: Filled in by interviewer. *(male/female/diverse)*

- Age: How old are you? *(integer)*

- Number of household members: How many people live in this household in total (including you)? *(integer)*

- Number of children in household: How many children (younger than 18 years) live in this household? *(integer)*

- Level of education: What is your level of education?

  - *No education or preschool*

  - *Primary school / Elementary school*

  - *Secondary school / High school*

  - *Technical higher education*

  - *Bachelor's degree*

  - *Master's degree*

  - *PhD*

  - *Does not want to say*

2.A.3. Sign-up decision

If you like, you now have the chance to sign up to the recycling programme of the municipality. Would you like to sign up to the recycling programme? *(yes/no)*

2.A.4. Post-treatment beliefs and control variables

On a scale from 1 to 7, please choose to what extent you agree or disagree with the following statements. 1 means strongly disagree; 4 means neither agree nor disagree; 7 means strongly agree. *(7-point Likert-scale)*

- Belief about future participation: The number of households in Miraflores that are participating in the recycling programme is going to increase in the future.

- Personal response efficacy belief: By participating in the recycling programme myself, I can make an important contribution to environmental protection.

- Collective response efficacy belief: If many households in Miraflores participate in the recycling programme, together they can make an important contribution to environmental protection.

- Personal effort of recycling: Recycling means a lot of personal effort for me.

Please answer the following questions about recycling in your household.

- Responsibility for recycling within household: Who is responsible for recycling within this household?
    - *I myself*
    - *My spouse or partner*
    - *Another adult of the family*
    - *A child of the family*
    - *Our housemaid*
    - *Everybody*
    - *Nobody / we don't recycle*
    - *Other*

- Already recycling through other ways: Do you or does your household already recycle through other ways than the municipality's recycling programme? *(yes/no)*
    - *If yes*: Through which other ways do you or does your household recycle?
        * *Recycling stations at supermarkets*
        * *Recycling stations in the streets*
        * *Recycling at my children's school*
        * *I give materials to the recycler in my neighbourhood*
        * *Other*

2.B. Balance tests

Table 2.B.1: Individual characteristics by treatment group

Variable	N	(1) Treatment A Mean/SE	N	(2) Treatment B Mean/SE	N	(3) Treatment C Mean/SE	N	(4) Treatment D Mean/SE	N	(5) Total Mean/SE	(1)-(2)	T-test Difference (1)-(3)	(1)-(4)	F-test for joint orthogonality
Age	322	52.717 (0.860)	293	53.174 (0.907)	292	55.110 (0.974)	333	53.826 (0.845)	1240	53.686 (0.447)	-0.457	-2.392*	-1.108	1.314
Female	451	0.619 (0.023)	406	0.638 (0.024)	409	0.599 (0.024)	443	0.612 (0.023)	1709	0.617 (0.012)	-0.019	0.020	0.007	0.455
Household members	302	2.907 (0.075)	275	3.193 (0.095)	276	2.949 (0.089)	303	2.878 (0.073)	1156	2.978 (0.042)	-0.285**	-0.042	0.029	2.932**
Number of children	321	0.371 (0.039)	296	0.426 (0.044)	288	0.354 (0.044)	314	0.357 (0.040)	1219	0.377 (0.021)	-0.055	0.017	0.014	0.631
Household head	451	0.581 (0.023)	406	0.549 (0.025)	409	0.609 (0.024)	443	0.614 (0.023)	1709	0.589 (0.012)	0.032	-0.028	-0.033	1.525
(Technical) University	451	0.596 (0.023)	406	0.594 (0.024)	409	0.582 (0.024)	443	0.605 (0.023)	1709	0.594 (0.012)	0.003	0.015	-0.009	0.159
Patience	451	7.510 (0.117)	406	7.384 (0.130)	409	7.499 (0.124)	443	7.102 (0.133)	1709	7.372 (0.063)	0.126	0.011	0.408**	2.335*
Responsibility for recycling	451	0.674 (0.022)	406	0.709 (0.023)	409	0.643 (0.024)	443	0.670 (0.022)	1709	0.674 (0.011)	-0.035	0.031	0.004	1.374
Recycling through other ways	451	0.625 (0.023)	406	0.638 (0.024)	409	0.609 (0.024)	443	0.607 (0.023)	1709	0.620 (0.012)	-0.013	0.016	0.018	0.376
F-test of joint significance (F-stat)											0.873	1.092	1.450	
F-test, number of observations											521	512	551	

Notes: The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 2.B.2: Beliefs distribution by treatment group

Variable	N	(1) Treatment A Mean/SE	N	(2) Treatment B Mean/SE	N	(3) Treatment C Mean/SE	N	(4) Treatment D Mean/SE	N	(5) Total Mean/SE	(1)-(2)	T-test Difference (1)-(3)	(1)-(4)	F-test for joint orthogonality
Current participation	391	34.463 (1.187)	360	35.239 (1.214)	366	37.044 (1.355)	394	34.589 (1.200)	1511	35.306 (0.619)	-0.776	-2.581	-0.126	0.913
Past participation	393	21.952 (1.045)	364	22.508 (1.079)	365	22.841 (1.140)	398	21.977 (1.061)	1520	22.305 (0.540)	-0.557	-0.889	-0.026	0.159
Dynamic norm	390	12.533 (1.027)	360	12.772 (1.102)	363	14.245 (1.111)	392	12.681 (1.095)	1505	13.042 (0.541)	-0.239	-1.712	-0.148	0.531
Injunctive norm	413	82.031 (1.066)	376	80.213 (1.237)	381	78.648 (1.324)	412	79.806 (1.157)	1582	80.205 (0.597)	1.819	3.383**	2.226	1.399
F-test of joint significance (F-stat)											0.850	2.453*	0.811	
F-test, number of observations											750	753	782	

Notes: The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

2.C. Beliefs correlation matrix

Table 2.C.1: Beliefs correlation table

Variables	Current participation	Past participation	Dynamic norm	Injunctive norm
Current participation	1.000			
Past participation	0.574	1.000		
	(0.000)			
Dynamic norm	0.571	-0.344	1.000	
	(0.000)	(0.000)		
Injunctive norm	0.175	0.091	0.111	1.000
	(0.000)	(0.000)	(0.000)	

Notes: Pairwise correlations of beliefs about the current participation, past participation, dynamic norm and injunctive norm.

2.D. Correlations of control variables and beliefs

Table 2.D.1: Correlations of control variables and beliefs

	Current participation (1)	Past participation (2)	Dynamic norm (3)	Injunctive norm (4)
Female	3.091**	0.609	2.385**	0.553
	(1.364)	(1.173)	(1.147)	(1.371)
Children	-1.733	2.361**	-4.086***	-1.779
	(1.222)	(1.079)	(1.077)	(1.182)
Household head	-5.058***	-2.781**	-2.239*	1.979
	(1.362)	(1.160)	(1.227)	(1.325)
(Technical) University	-2.016	-0.733	-1.407	-3.821***
	(1.275)	(1.134)	(1.128)	(1.220)
Patience	1.905***	1.381***	0.550***	0.265
	(0.220)	(0.192)	(0.202)	(0.233)
Responsibility for recycling	0.832	4.721***	-4.046***	-10.534***
	(1.592)	(1.392)	(1.398)	(1.450)
Recycling through other ways	-1.502	-4.375***	3.057**	11.077***
	(1.510)	(1.358)	(1.292)	(1.520)
Constant	24.694***	12.318***	12.329***	79.988***
	(2.416)	(1.942)	(2.076)	(2.424)
Adjusted R^2	0.064	0.050	0.022	0.049
Observations	1511	1520	1505	1582

Notes: OLS regressions of beliefs about (1) the current participation, (2) past participation, (3) dynamic norm and (4) injunctive norm about participation in the recycling programme on socioeconomic control variables. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

2.E. Average treatment effects

Figure 2.E.1: Average sign-up decision by treatment group
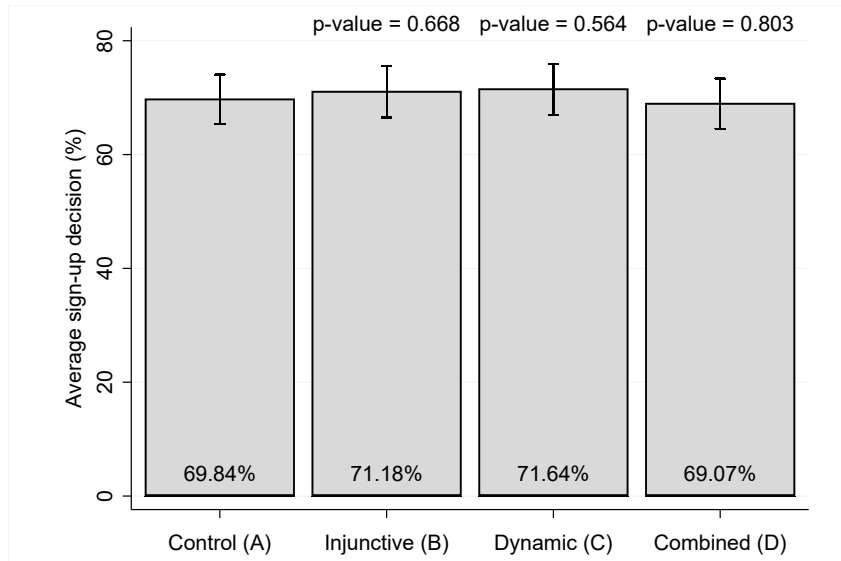
Notes: p-values are obtained from testing for equality of proportions, comparing the different treatments B, C and D with the control group A, respectively. N(A)=451, N(B)=406, N(C)=409, N(D)=443. Graphs show the average sign-up decision by treatment, with 95% confidence intervals for proportions.

Table 2.E.1: Average treatment effects on households' sign-up decision (OLS)

	(1)	(2)
Injunctive (B)	0.013	0.017
	(0.031)	(0.029)
Dynamic (C)	0.018	0.022
	(0.031)	(0.029)
Combined (D)	-0.008	0.006
	(0.031)	(0.028)
Female		0.029
		(0.023)
Children		0.071***
		(0.021)
Household head		0.107***
		(0.023)
(Technical) University		0.100***
		(0.022)
Patience		0.046***
		(0.004)
Responsibility for recycling		0.184***
		(0.026)
Recycling through other ways		-0.133***
		(0.025)
Constant	0.698***	0.138***
	(0.022)	(0.045)
Adjusted R^2	-0.001	0.143
Observations	1709	1709

Notes: OLS regressions with dependent variable equal to sign-up decision = yes. Column (1) includes treatment dummies alone; column (2) adds control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.E.2: Average treatment effects on households' sign-up decision (logit)

	(1)	(2)
Injunctive (B)	0.064	0.106
	(0.150)	(0.163)
Dynamic (C)	0.087	0.134
	(0.150)	(0.163)
Combined (D)	-0.036	0.030
	(0.145)	(0.158)
Female		0.146
		(0.134)
Children		0.414***
		(0.119)
Household head		0.583***
		(0.129)
(Technical) University		0.560***
		(0.119)
Patience		0.229***
		(0.022)
Responsibility for recycling		1.073***
		(0.150)
Recycling through other ways		-0.825***
		(0.150)
Constant	0.840***	-1.926***
	(0.103)	(0.255)
Pseudo R^2	0.000	0.125
Observations	1709	1709

Notes: Logit regressions with dependent variable equal to sign-up decision = yes. Column (1) includes treatment dummies alone; column (2) adds control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

2.F. Heterogeneous treatment effects based on individual prior beliefs

2.F.1. Beliefs about the dynamic norm

Table 2.F.1: Heterogeneous treatment effects: under- vs. overestimation of the dynamic norm (OLS)

	Dynamic norm beliefs $< 6\%$		Dynamic norm beliefs $>= 6\%$	
	(1)	(2)	(3)	(4)
Injunctive (B)	0.078	0.017	-0.023	-0.000
	(0.057)	(0.047)	(0.042)	(0.041)
Dynamic (C)	0.146**	0.119**	-0.048	-0.022
	(0.057)	(0.048)	(0.042)	(0.040)
Combined (D)	0.033	0.044	0.001	0.001
	(0.056)	(0.046)	(0.041)	(0.039)
Female		0.060*		-0.001
		(0.037)		(0.033)
Children		-0.018		0.071**
		(0.033)		(0.030)
Household head		0.152***		0.116***
		(0.038)		(0.032)
(Technical) University		0.096***		0.122***
		(0.035)		(0.030)
Patience		0.070***		0.019***
		(0.006)		(0.006)
Responsibility for recycling		0.173***		0.236***
		(0.041)		(0.037)
Recycling through other ways		-0.074**		-0.143***
		(0.037)		(0.036)
Constant	0.582***	-0.155**	0.727***	0.335***
	(0.040)	(0.068)	(0.029)	(0.066)
Adjusted R^2	0.008	0.322	-0.001	0.087
Observations	574	574	931	931

Notes: OLS regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who underestimate the trend in participation in the recycling programme; columns (3) and (4) to those who are correct about or overestimate it. Columns (1) and (3) include treatment dummies alone; columns (2) and (4) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.F.2: Heterogeneous treatment effects: under- vs. overestimation of the dynamic norm (logit)

	Dynamic norm beliefs < 6%		Dynamic norm beliefs >= 6%	
	(1)	(2)	(3)	(4)
Injunctive (B)	0.333	0.097	-0.114	0.001
	(0.245)	(0.297)	(0.206)	(0.219)
Dynamic (C)	0.655**	0.792**	-0.232	-0.122
	(0.257)	(0.323)	(0.201)	(0.212)
Combined (D)	0.137	0.232	0.006	-0.008
	(0.238)	(0.295)	(0.203)	(0.214)
Female		0.405*		-0.017
		(0.242)		(0.180)
Children		-0.087		0.409**
		(0.218)		(0.162)
Household head		0.949***		0.623***
		(0.243)		(0.171)
(Technical) University		0.623***		0.647***
		(0.225)		(0.158)
Patience		0.394***		0.091***
		(0.041)		(0.030)
Responsibility for recycling		1.091***		1.247***
		(0.260)		(0.200)
Recycling through other ways		-0.526**		-0.827***
		(0.254)		(0.206)
Constant	0.329*	-3.914***	0.979***	-0.889***
	(0.171)	(0.503)	(0.142)	(0.344)
Pseudo R^2	0.010	0.286	0.002	0.083
Observations	574	574	931	931

Notes: Logit regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who underestimate the trend in participation in the recycling programme; columns (3) and (4) to those who are correct about or overestimate it. Columns (1) and (3) include treatment dummies alone; columns (2) and (4) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

2.F.2. Beliefs about the current participation

Table 2.F.3: Heterogeneous treatment effects: under- vs. overestimation of the current participation (OLS)

	Current participation beliefs < 12%		Current participation beliefs >= 12%	
	(1)	(2)	(3)	(4)
Injunctive (B)	0.088	0.021	-0.012	0.003
	(0.076)	(0.058)	(0.038)	(0.036)
Dynamic (C)	0.192**	0.124**	-0.024	-0.005
	(0.075)	(0.057)	(0.037)	(0.036)
Combined (D)	0.143**	0.145***	-0.028	-0.022
	(0.071)	(0.055)	(0.037)	(0.036)
Female		-0.002		0.030
		(0.044)		(0.029)
Children		0.019		0.043
		(0.040)		(0.026)
Household head		0.148***		0.111***
		(0.046)		(0.029)
(Technical) University		0.100**		0.108***
		(0.044)		(0.027)
Patience		0.069***		0.026***
		(0.006)		(0.005)
Responsibility for recycling		0.285***		0.188***
		(0.050)		(0.033)
Recycling through other ways		-0.149***		-0.099***
		(0.046)		(0.031)
Constant	0.494***	-0.174**	0.727***	0.278***
	(0.052)	(0.076)	(0.026)	(0.061)
Adjusted R^2	0.012	0.432	-0.002	0.078
Observations	351	351	1160	1160

Notes: OLS regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who underestimate the current participation in the recycling programme; columns (3) and (4) to those who are correct about or overestimate it. Columns (1) and (3) include treatment dummies alone; columns (2) and (4) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.F.4: Heterogeneous treatment effects: under- vs. overestimation of the current participation (logit)

	Current participation beliefs $< 12\%$		Current participation beliefs $>= 12\%$	
	(1)	(2)	(3)	(4)
Injunctive (B)	0.355	0.137	-0.058	0.018
	(0.313)	(0.410)	(0.185)	(0.195)
Dynamic (C)	0.808**	0.884**	-0.117	-0.036
	(0.319)	(0.449)	(0.183)	(0.192)
Combined (D)	0.586**	1.076***	-0.139	-0.135
	(0.297)	(0.418)	(0.181)	(0.190)
Female		0.031		0.153
		(0.344)		(0.158)
Children		0.256		0.242*
		(0.308)		(0.141)
Household head		1.117***		0.585***
		(0.341)		(0.152)
(Technical) University		0.759**		0.566***
		(0.320)		(0.140)
Patience		0.440***		0.126***
		(0.054)		(0.027)
Responsibility for recycling		2.225***		0.973***
		(0.419)		(0.171)
Recycling through other ways		-1.398***		-0.567***
		(0.414)		(0.172)
Constant	-0.023	-4.612***	0.979***	-1.151***
	(0.214)	(0.688)	(0.129)	(0.313)
Pseudo R^2	0.016	0.411	0.001	0.073
Observations	351	351	1160	1160

Notes: Logit regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who underestimate the current participation in the recycling programme; columns (3) and (4) to those who are correct about or overestimate it. Columns (1) and (3) include treatment dummies alone; columns (2) and (4) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

2.F.3. Beliefs about the injunctive norm

Table 2.F.5: Heterogeneous treatment effects: under- vs. overestimation of the injunctive norm (OLS)

	Injunctive norm beliefs $< 97\%$		Injunctive norm beliefs $>= 97\%$	
	(1)	(2)	(3)	(4)
Injunctive (B)	0.026	0.031	-0.000	-0.003
	(0.040)	(0.037)	(0.059)	(0.054)
Dynamic (C)	0.021	0.033	0.040	0.042
	(0.040)	(0.037)	(0.058)	(0.053)
Combined (D)	0.011	0.020	0.005	0.025
	(0.040)	(0.036)	(0.057)	(0.052)
Female		0.007		0.056
		(0.029)		(0.043)
Children		0.024		0.050
		(0.027)		(0.039)
Household head		0.087***		0.179***
		(0.029)		(0.042)
(Technical) University		0.145***		0.101***
		(0.029)		(0.039)
Patience		0.041***		0.048***
		(0.005)		(0.007)
Responsibility for recycling		0.258***		0.183***
		(0.034)		(0.047)
Recycling through other ways		-0.096***		-0.145***
		(0.031)		(0.047)
Constant	0.674***	0.073	0.664***	0.061
	(0.028)	(0.058)	(0.040)	(0.081)
Adjusted R^2	-0.002	0.166	-0.005	0.162
Observations	1062	1062	520	520

Notes: OLS regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who underestimate the injunctive norm about participation in the recycling programme; columns (3) and (4) to those who are correct about or overestimate it. Columns (1) and (3) include treatment dummies alone; columns (2) and (4) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.F.6: Heterogeneous treatment effects: under- vs. overestimation of the injunctive norm (logit)

	Injunctive norm beliefs < 97%		Injunctive norm beliefs >= 97%	
	(1)	(2)	(3)	(4)
Injunctive (B)	0.123	0.192	-0.002	-0.051
	(0.187)	(0.208)	(0.265)	(0.292)
Dynamic (C)	0.099	0.194	0.184	0.211
	(0.187)	(0.207)	(0.267)	(0.291)
Combined (D)	0.051	0.096	0.022	0.122
	(0.183)	(0.202)	(0.255)	(0.287)
Female		0.026		0.279
		(0.168)		(0.238)
Children		0.150		0.318
		(0.150)		(0.217)
Household head		0.478***		0.923***
		(0.163)		(0.231)
(Technical) University		0.764***		0.562***
		(0.156)		(0.217)
Patience		0.210***		0.238***
		(0.027)		(0.040)
Responsibility for recycling		1.381***		1.000***
		(0.190)		(0.257)
Recycling through other ways		-0.619***		-0.830***
		(0.186)		(0.273)
Constant	0.726***	-2.209***	0.682***	-2.246***
	(0.128)	(0.326)	(0.181)	(0.454)
Pseudo R^2	0.000	0.145	0.001	0.146
Observations	1062	1062	520	520

Notes: Logit regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who underestimate the injunctive norm about participation in the recycling programme; columns (3) and (4) to those who are correct about or overestimate it. Columns (1) and (3) include treatment dummies alone; columns (2) and (4) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.F.7: Heterogeneous treatment effects: stronger underestimation of the injunctive norm (below 90%, 70%, 60% and 50%) (OLS)

	Injunctive norm beliefs < 90%		Injunctive norm beliefs < 70%		Injunctive norm beliefs < 60%		Injunctive norm beliefs < 50%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Injunctive (B)	0.090*	0.097**	0.149**	0.159***	0.196***	0.199***	0.289***	0.270***
	(0.050)	(0.045)	(0.064)	(0.058)	(0.072)	(0.064)	(0.087)	(0.085)
Dynamic (C)	0.102**	0.105**	0.072	0.077	0.120*	0.124*	0.202**	0.241***
	(0.048)	(0.044)	(0.064)	(0.059)	(0.071)	(0.063)	(0.082)	(0.081)
Combined (D)	0.026	0.044	0.063	0.080	0.103	0.132**	0.221**	0.212**
	(0.047)	(0.043)	(0.064)	(0.058)	(0.071)	(0.064)	(0.088)	(0.088)
Female		-0.045		-0.083*		-0.097**		-0.031
		(0.035)		(0.045)		(0.049)		(0.062)
Children		-0.040		0.003		-0.015		0.118*
		(0.032)		(0.042)		(0.047)		(0.061)
Household head		0.068*		0.056		0.025		0.064
		(0.035)		(0.046)		(0.050)		(0.063)
(Technical) University		0.103***		0.099**		0.066		0.085
		(0.035)		(0.044)		(0.049)		(0.064)
Patience		0.047***		0.042***		0.045***		0.025**
		(0.006)		(0.007)		(0.008)		(0.010)
Responsibility for recycling		0.200***		0.222***		0.239***		0.155*
		(0.041)		(0.055)		(0.062)		(0.081)
Recycling through other ways		-0.063*		-0.080*		-0.104**		-0.087
		(0.036)		(0.046)		(0.050)		(0.062)
Constant	0.667***	0.142**	0.695***	0.216**	0.672***	0.217**	0.679***	0.271*
	(0.034)	(0.067)	(0.047)	(0.085)	(0.052)	(0.089)	(0.063)	(0.141)
Adjusted R^2	0.005	0.169	0.007	0.186	0.016	0.222	0.065	0.152
Observations	699	699	357	357	275	275	131	131

Notes: OLS regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who believe the injunctive norm about participation in the recycling programme to be below 90, columns (3) and (4) to those who believe it to be below 70, columns (5) and (6) to those who believe it to be below 60, and columns (7) and (8) to those who believe it to be below 50. Columns (1), (3), (5) and (7) include treatment dummies alone; columns (2), (4), (6) and (8) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.F.8: Heterogeneous treatment effects: stronger underestimation of the injunctive norm (below 90%, 70%, 60% and 50%) (logit)

	Injunctive norm beliefs < 90%		Injunctive norm beliefs < 70%		Injunctive norm beliefs < 60%		Injunctive norm beliefs < 50%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Injunctive (B)	0.440*	0.619**	0.867**	1.295***	1.164***	1.750***	2.654**	2.896**
	(0.247)	(0.278)	(0.377)	(0.443)	(0.446)	(0.546)	(1.094)	(1.213)
Dynamic (C)	0.506**	0.617**	0.365	0.525	0.618	0.839*	1.254**	2.557***
	(0.241)	(0.267)	(0.346)	(0.404)	(0.394)	(0.469)	(0.625)	(0.943)
Combined (D)	0.118	0.223	0.317	0.444	0.518	0.862*	1.450**	1.908**
	(0.224)	(0.249)	(0.339)	(0.387)	(0.389)	(0.465)	(0.731)	(0.929)
Female		-0.296		-0.719**		-0.915**		-0.365
		(0.218)		(0.333)		(0.408)		(0.725)
Children		-0.230		0.044		-0.043		1.567**
		(0.191)		(0.294)		(0.357)		(0.788)
Household head		0.393*		0.388		0.181		0.414
		(0.207)		(0.312)		(0.377)		(0.662)
(Technical) University		0.594***		0.729**		0.637*		1.343*
		(0.199)		(0.305)		(0.374)		(0.760)
Patience		0.249***		0.261***		0.301***		0.262**
		(0.034)		(0.048)		(0.056)		(0.107)
Responsibility for recycling		1.180***		1.645***		1.872***		2.385**
		(0.242)		(0.394)		(0.485)		(1.030)
Recycling through other ways		-0.482**		-0.829**		-1.145**		-1.706*
		(0.233)		(0.372)		(0.454)		(0.919)
Constant	0.693***	-1.961***	0.824***	-1.905***	0.717***	-2.010***	0.747*	-3.773**
	(0.162)	(0.399)	(0.240)	(0.569)	(0.266)	(0.648)	(0.405)	(1.577)
Pseudo R^2	0.008	0.158	0.014	0.209	0.026	0.260	0.102	0.313
Observations	699	699	357	357	275	275	131	131

Notes: Logit regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who believe the injunctive norm about participation in the recycling programme to be below 90, columns (3) and (4) to those who believe it to be below 70, columns (5) and (6) to those who believe it to be below 60, and columns (7) and (8) to those who believe it to be below 50. Columns (1), (3), (5) and (7) include treatment dummies alone; columns (2), (4), (6) and (8) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

2.F.4. Beliefs about the dynamic and injunctive norm combined

Table 2.F.9: Heterogeneous treatment effects: under- vs. overestimation of the dynamic norm and the injunctive norm combined (OLS)

	Dynamic norm beliefs < 6% & Injunctive norm beliefs < 97%		Dynamic norm beliefs < 6% & Injunctive norm beliefs >= 97%		Dynamic norm beliefs >= 6% & Injunctive norm beliefs < 97%		Dynamic norm beliefs >= 6% & Injunctive norm beliefs >= 97%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Injunctive (B)	0.097	0.025	0.039	-0.006	-0.029	0.006	-0.018	-0.016
	(0.067)	(0.057)	(0.106)	(0.084)	(0.052)	(0.049)	(0.074)	(0.072)
Dynamic (C)	0.147**	0.118**	0.142	0.156*	-0.071	-0.034	-0.010	0.000
	(0.067)	(0.057)	(0.112)	(0.089)	(0.052)	(0.050)	(0.069)	(0.068)
Combined (D)	0.054	0.048	-0.003	0.056	-0.027	-0.017	0.053	0.031
	(0.067)	(0.057)	(0.103)	(0.082)	(0.051)	(0.048)	(0.070)	(0.069)
Female		0.042		0.111		-0.025		0.032
		(0.044)		(0.068)		(0.041)		(0.056)
Children		-0.016		-0.018		0.069*		0.083
		(0.041)		(0.060)		(0.036)		(0.052)
Household head		0.133***		0.190***		0.074*		0.172***
		(0.046)		(0.070)		(0.040)		(0.056)
(Technical) University		0.117***		0.065		0.154***		0.110**
		(0.044)		(0.061)		(0.039)		(0.053)
Patience		0.064***		0.081***		0.019***		0.016
		(0.007)		(0.010)		(0.007)		(0.011)
Responsibility for recycling		0.192***		0.134*		0.287***		0.172***
		(0.051)		(0.071)		(0.047)		(0.062)
Recycling through other ways		-0.045		-0.149**		-0.153***		-0.130**
		(0.045)		(0.069)		(0.043)		(0.065)
Constant	0.590***	-0.138	0.561***	-0.191	0.742***	0.313***	0.700***	0.345***
	(0.047)	(0.084)	(0.077)	(0.118)	(0.036)	(0.082)	(0.048)	(0.115)
Adjusted R^2	0.006	0.281	-0.004	0.387	-0.002	0.103	-0.006	0.052
Observations	399	399	175	175	610	610	321	321

Notes: OLS regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who underestimate both the dynamic norm and the injunctive norm about participation in the recycling programme; columns (3) and (4) to those who underestimate the dynamic norm and overestimate or are correct about the injunctive norm; columns (5) and (6) to those who overestimate or are correct about the dynamic norm and underestimate the injunctive norm; and columns (7) and (8) to those who overestimate or are correct about both the dynamic and the injunctive norm. Columns (1), (3), (5) and (7) include treatment dummies alone; columns (2), (4), (6) and (8) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.F.10: Heterogeneous treatment effects: under- vs. overestimation of the dynamic norm and the injunctive norm combined (logit)

	Dynamic norm beliefs < 6% & Injunctive norm beliefs < 97%		Dynamic norm beliefs < 6% & Injunctive norm beliefs >= 97%		Dynamic norm beliefs >= 6% & Injunctive norm beliefs < 97%		Dynamic norm beliefs >= 6% & Injunctive norm beliefs >= 97%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Injunctive (B)	0.422	0.149	0.160	-0.105	-0.145	0.032	-0.085	-0.076
	(0.297)	(0.354)	(0.438)	(0.579)	(0.256)	(0.276)	(0.350)	(0.370)
Dynamic (C)	0.668**	0.753**	0.615	1.129*	-0.343	-0.186	-0.045	-0.004
	(0.306)	(0.378)	(0.478)	(0.657)	(0.254)	(0.272)	(0.330)	(0.344)
Combined (D)	0.227	0.257	-0.013	0.240	-0.136	-0.100	0.268	0.161
	(0.291)	(0.351)	(0.421)	(0.596)	(0.253)	(0.270)	(0.345)	(0.361)
Female		0.285		0.740		-0.144		0.167
		(0.283)		(0.509)		(0.229)		(0.297)
Children		-0.066		-0.108		0.395*		0.476*
		(0.261)		(0.429)		(0.202)		(0.278)
Household head		0.824***		1.233**		0.425*		0.878***
		(0.288)		(0.501)		(0.218)		(0.287)
(Technical) University		0.727***		0.466		0.798***		0.606**
		(0.273)		(0.430)		(0.206)		(0.281)
Patience		0.351***		0.517***		0.094**		0.075
		(0.047)		(0.090)		(0.037)		(0.054)
Responsibility for recycling		1.150***		1.010**		1.529***		0.886***
		(0.309)		(0.507)		(0.262)		(0.321)
Recycling through other ways		-0.330		-1.089**		-0.933***		-0.686**
		(0.295)		(0.527)		(0.264)		(0.341)
Constant	0.364*	-3.714***	0.245	-4.610***	1.057***	-0.966**	0.847***	-0.870
	(0.203)	(0.593)	(0.315)	(1.039)	(0.181)	(0.436)	(0.230)	(0.580)
Pseudo R^2	0.010	0.255	0.010	0.375	0.003	0.101	0.003	0.069
Observations	399	399	175	175	610	610	321	321

Notes: Logit regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who underestimate both the dynamic norm and the injunctive norm about participation in the recycling programme; columns (3) and (4) to those who underestimate the dynamic norm and overestimate or are correct about the injunctive norm; columns (5) and (6) to those who overestimate or are correct about the dynamic norm and underestimate the injunctive norm; and columns (7) and (8) to those who overestimate or are correct about both the dynamic and the injunctive norm. Columns (1), (3), (5) and (7) include treatment dummies alone; columns (2), (4), (6) and (8) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

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Table 2.F.11: Heterogeneous treatment effects: under- vs. overestimation of the dynamic norm and the injunctive norm (threshold=90%) combined (OLS)

	Dynamic norm beliefs < 6% & Injunctive norm beliefs < 90%		Dynamic norm beliefs < 6% & Injunctive norm beliefs >= 90%		Dynamic norm beliefs >= 6% & Injunctive norm beliefs < 90%		Dynamic norm beliefs >= 6% & Injunctive norm beliefs >= 90%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Injunctive (B)	0.142*	0.082	0.034	-0.021	0.034	0.080	-0.072	-0.069
	(0.080)	(0.070)	(0.079)	(0.064)	(0.064)	(0.062)	(0.056)	(0.054)
Dynamic (C)	0.250***	0.220***	0.039	0.039	-0.020	0.013	-0.073	-0.053
	(0.078)	(0.068)	(0.083)	(0.067)	(0.063)	(0.061)	(0.055)	(0.053)
Combined (D)	0.067	0.063	-0.006	0.041	-0.022	0.008	0.017	-0.005
	(0.075)	(0.065)	(0.081)	(0.065)	(0.062)	(0.059)	(0.055)	(0.053)
Female		-0.017		0.120**		-0.043		0.025
		(0.052)		(0.052)		(0.049)		(0.044)
Children		-0.064		0.015		-0.009		0.120***
		(0.048)		(0.047)		(0.044)		(0.040)
Household head		0.133**		0.177***		0.041		0.169***
		(0.053)		(0.053)		(0.048)		(0.043)
(Technical) University		0.113**		0.086*		0.090*		0.143***
		(0.053)		(0.048)		(0.047)		(0.040)
Patience		0.061***		0.079***		0.035***		0.009
		(0.008)		(0.008)		(0.009)		(0.008)
Responsibility for recycling		0.149**		0.165***		0.207***		0.249***
		(0.061)		(0.057)		(0.058)		(0.050)
Recycling through other ways		-0.002		-0.113**		-0.116**		-0.162***
		(0.052)		(0.053)		(0.051)		(0.051)
Constant	0.591***	-0.059	0.573***	-0.234**	0.745***	0.325***	0.716***	0.352***
	(0.055)	(0.099)	(0.057)	(0.093)	(0.045)	(0.096)	(0.037)	(0.093)
Adjusted R^2	0.031	0.283	-0.009	0.350	-0.005	0.087	0.002	0.093
Observations	274	274	300	300	395	395	536	536

Notes: OLS regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who underestimate both the dynamic norm (trend in absolute numbers) and the injunctive norm (below 90) about participation in the recycling programme; columns (3) and (4) to those who underestimate the dynamic norm and overestimate or are correct about the injunctive norm (above or equal to 90); columns (5) and (6) to those who overestimate or are correct about the dynamic norm and underestimate the injunctive norm (below 90); and columns (7) and (8) to those who overestimate or are correct about both the dynamic and the injunctive norm (above or equal to 90). Columns (1), (3), (5) and (7) include treatment dummies alone; columns (2), (4), (6) and (8) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.F.12: Heterogeneous treatment effects: under- vs. overestimation of the dynamic norm and the injunctive norm (threshold=90%) combined (logit)

	Dynamic norm beliefs < 6% & Injunctive norm beliefs < 90%		Dynamic norm beliefs < 6% & Injunctive norm beliefs >= 90%		Dynamic norm beliefs >= 6% & Injunctive norm beliefs < 90%		Dynamic norm beliefs >= 6% & Injunctive norm beliefs >= 90%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Injunctive (B)	0.644*	0.554	0.140	-0.156	0.189	0.509	-0.330	-0.366
	(0.385)	(0.473)	(0.323)	(0.405)	(0.342)	(0.370)	(0.260)	(0.280)
Dynamic (C)	1.295***	1.517***	0.160	0.251	-0.104	0.091	-0.335	-0.286
	(0.413)	(0.503)	(0.343)	(0.456)	(0.327)	(0.349)	(0.256)	(0.272)
Combined (D)	0.288	0.312	-0.024	0.170	-0.115	0.048	0.084	-0.035
	(0.345)	(0.416)	(0.331)	(0.436)	(0.320)	(0.341)	(0.266)	(0.281)
Female		-0.133		0.752**		-0.284		0.122
		(0.365)		(0.348)		(0.297)		(0.233)
Children		-0.419		0.146		-0.055		0.673***
		(0.333)		(0.308)		(0.256)		(0.216)
Household head		0.907**		1.101***		0.218		0.886***
		(0.360)		(0.349)		(0.280)		(0.225)
(Technical) University		0.757**		0.569*		0.510*		0.756***
		(0.350)		(0.313)		(0.263)		(0.209)
Patience		0.348***		0.465***		0.186***		0.042
		(0.056)		(0.064)		(0.048)		(0.042)
Responsibility for recycling		0.981**		1.074***		1.200***		1.276***
		(0.402)		(0.365)		(0.330)		(0.262)
Recycling through other ways		-0.085		-0.767**		-0.777**		-0.872***
		(0.378)		(0.365)		(0.324)		(0.275)
Constant	0.368	-3.435***	0.295	-4.548***	1.070***	-0.998*	0.925***	-0.812*
	(0.250)	(0.731)	(0.233)	(0.736)	(0.237)	(0.528)	(0.178)	(0.474)
Pseudo R^2	0.035	0.277	0.001	0.321	0.002	0.099	0.006	0.093
Observations	274	274	300	300	395	395	536	536

Notes: Logit regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who underestimate both the dynamic norm (trend in absolute numbers) and the injunctive norm (below 90) about participation in the recycling programme; columns (3) and (4) to those who underestimate the dynamic norm and overestimate or are correct about the injunctive norm (above or equal to 90); columns (5) and (6) to those who overestimate or are correct about the dynamic norm and underestimate the injunctive norm (below 90); and columns (7) and (8) to those who overestimate or are correct about both the dynamic and the injunctive norm (above or equal to 90). Columns (1), (3), (5) and (7) include treatment dummies alone; columns (2), (4), (6) and (8) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.F.13: Heterogeneous treatment effects: under- vs. overestimation of the current participation and the injunctive norm combined (OLS)

	Current participation beliefs < 12% & Injunctive norm beliefs < 97%		Current participation beliefs < 12% & Injunctive norm beliefs >= 97%		Current participation beliefs >= 12% & Injunctive norm beliefs < 97%		Current participation beliefs >= 12% & Injunctive norm beliefs >= 97%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Injunctive (B)	0.100	0.017	0.050	0.034	-0.006	0.013	-0.025	-0.022
	(0.090)	(0.071)	(0.142)	(0.104)	(0.046)	(0.044)	(0.066)	(0.064)
Dynamic (C)	0.179**	0.068	0.208	0.279***	-0.034	-0.003	-0.006	-0.003
	(0.088)	(0.070)	(0.143)	(0.103)	(0.046)	(0.045)	(0.064)	(0.062)
Combined (D)	0.156*	0.102	0.127	0.256***	-0.042	-0.028	-0.001	-0.013
	(0.086)	(0.069)	(0.127)	(0.093)	(0.046)	(0.044)	(0.064)	(0.062)
Female		-0.027		0.082		0.019		0.042
		(0.054)		(0.082)		(0.036)		(0.051)
Children		0.055		-0.074		0.028		0.078*
		(0.051)		(0.072)		(0.032)		(0.046)
Household head		0.136**		0.178*		0.078**		0.157***
		(0.055)		(0.091)		(0.036)		(0.050)
(Technical) University		0.105*		0.111		0.140***		0.091*
		(0.056)		(0.079)		(0.035)		(0.047)
Patience		0.064***		0.080***		0.025***		0.028***
		(0.008)		(0.012)		(0.007)		(0.009)
Responsibility for recycling		0.315***		0.284***		0.222***		0.144***
		(0.062)		(0.090)		(0.041)		(0.054)
Recycling through other ways		-0.133**		-0.226**		-0.089**		-0.129**
		(0.055)		(0.096)		(0.037)		(0.055)
Constant	0.525***	-0.142	0.429***	-0.298**	0.731***	0.254***	0.718***	0.290***
	(0.062)	(0.098)	(0.095)	(0.129)	(0.032)	(0.075)	(0.045)	(0.105)
Adjusted R^2	0.008	0.392	-0.005	0.492	-0.002	0.086	-0.007	0.058
Observations	242	242	109	109	769	769	391	391

Notes: OLS regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who underestimate both the current participation rate and the injunctive norm about participation in the recycling programme; columns (3) and (4) to those who underestimate the current participation rate and overestimate or are correct about the injunctive norm; columns (5) and (6) to those who overestimate or are correct about the current participation rate and underestimate the injunctive norm; and columns (7) and (8) to those who overestimate or are correct about both the current participation rate and the injunctive norm. Columns (1), (3), (5) and (7) include treatment dummies alone; columns (2), (4), (6) and (8) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.F.14: Heterogeneous treatment effects: under- vs. overestimation of the current participation and the injunctive norm combined (logit)

	Current participation beliefs < 12% & Injunctive norm beliefs < 97%		Current participation beliefs < 12% & Injunctive norm beliefs >= 97%		Current participation beliefs >= 12% & Injunctive norm beliefs < 97%		Current participation beliefs >= 12% & Injunctive norm beliefs >= 97%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Injunctive (B)	0.409	0.183	0.201	0.334	-0.030	0.083	-0.122	-0.117
	(0.380)	(0.494)	(0.566)	(0.807)	(0.227)	(0.242)	(0.318)	(0.335)
Dynamic (C)	0.769**	0.523	0.847	2.532**	-0.165	-0.021	-0.027	-0.023
	(0.383)	(0.535)	(0.585)	(0.992)	(0.227)	(0.240)	(0.310)	(0.323)
Combined (D)	0.660*	0.779	0.511	2.347**	-0.205	-0.164	-0.006	-0.074
	(0.371)	(0.498)	(0.508)	(0.951)	(0.223)	(0.235)	(0.313)	(0.328)
Female		-0.169		0.805		0.105		0.207
		(0.413)		(0.711)		(0.198)		(0.270)
Children		0.534		-0.738		0.158		0.438*
		(0.385)		(0.673)		(0.174)		(0.246)
Household head		1.046***		1.655**		0.425**		0.803***
		(0.405)		(0.769)		(0.193)		(0.261)
(Technical) University		0.808**		0.783		0.710***		0.493**
		(0.405)		(0.670)		(0.178)		(0.248)
Patience		0.407***		0.615***		0.122***		0.134***
		(0.062)		(0.130)		(0.034)		(0.048)
Responsibility for recycling		2.323***		2.674***		1.138***		0.758***
		(0.492)		(0.948)		(0.218)		(0.283)
Recycling through other ways		-1.239***		-2.110**		-0.530**		-0.702**
		(0.468)		(0.987)		(0.213)		(0.297)
Constant	0.102	-4.403***	-0.288	-6.646***	1.001***	-1.264***	0.937***	-1.107**
	(0.261)	(0.857)	(0.382)	(1.594)	(0.159)	(0.392)	(0.219)	(0.535)
Pseudo R^2	0.016	0.383	0.017	0.521	0.001	0.083	0.000	0.069
Observations	242	242	109	109	769	769	391	391

Notes: Logit regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who underestimate both the current participation rate and the injunctive norm about participation in the recycling programme; columns (3) and (4) to those who underestimate the current participation rate and overestimate or are correct about the injunctive norm; columns (5) and (6) to those who overestimate or are correct about the current participation rate and underestimate the injunctive norm; and columns (7) and (8) to those who overestimate or are correct about both the current participation rate and the injunctive norm. Columns (1), (3), (5) and (7) include treatment dummies alone; columns (2), (4), (6) and (8) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.F.15: Heterogeneous treatment effects: under- vs. overestimation of the current participation and the injunctive norm (threshold=90%) combined (OLS)

	Current participation beliefs < 12% & Injunctive norm beliefs < 90%		Current participation beliefs < 12% & Injunctive norm beliefs >= 90%		Current participation beliefs >= 12% & Injunctive norm beliefs < 90%		Current participation beliefs >= 12% & Injunctive norm beliefs >= 90%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Injunctive (B)	0.149	0.065	0.041	0.006	0.059	0.092*	-0.065	-0.067
	(0.109)	(0.087)	(0.103)	(0.077)	(0.056)	(0.054)	(0.050)	(0.048)
Dynamic (C)	0.309***	0.132	0.054	0.118	0.026	0.059	-0.063	-0.061
	(0.103)	(0.085)	(0.106)	(0.080)	(0.056)	(0.054)	(0.050)	(0.048)
Combined (D)	0.172*	0.114	0.106	0.187**	-0.032	-0.008	-0.025	-0.039
	(0.099)	(0.081)	(0.099)	(0.074)	(0.055)	(0.052)	(0.051)	(0.049)
Female		-0.143**		0.124**		0.005		0.041
		(0.063)		(0.062)		(0.043)		(0.040)
Children		0.037		-0.018		-0.065*		0.121***
		(0.060)		(0.055)		(0.038)		(0.036)
Household head		0.113*		0.201***		0.062		0.152***
		(0.063)		(0.066)		(0.043)		(0.039)
(Technical) University		0.079		0.102*		0.088**		0.127***
		(0.065)		(0.061)		(0.041)		(0.036)
Patience		0.059***		0.080***		0.039***		0.019***
		(0.008)		(0.009)		(0.008)		(0.007)
Responsibility for recycling		0.352***		0.208***		0.138***		0.217***
		(0.073)		(0.070)		(0.050)		(0.044)
Recycling through other ways		-0.084		-0.182***		-0.053		-0.137***
		(0.065)		(0.067)		(0.044)		(0.043)
Constant	0.528***	-0.052	0.471***	-0.296***	0.726***	0.266***	0.728***	0.288***
	(0.076)	(0.119)	(0.070)	(0.101)	(0.040)	(0.087)	(0.034)	(0.085)
Adjusted R^2	0.037	0.404	-0.010	0.451	0.000	0.090	-0.001	0.086
Observations	163	163	188	188	507	507	653	653

Notes: OLS regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who underestimate both the current participation rate and the injunctive norm (below 90) about participation in the recycling programme; columns (3) and (4) to those who underestimate the current participation rate and overestimate or are correct about the injunctive norm (above or equal to 90); columns (5) and (6) to those who overestimate or are correct about the current participation rate and underestimate the injunctive norm (below 90); and columns (7) and (8) to those who overestimate or are correct about both the current participation rate and the injunctive norm (above or equal to 90). Columns (1), (3), (5) and (7) include treatment dummies alone; columns (2), (4), (6) and (8) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.F.16: Heterogeneous treatment effects: under- vs. overestimation of the current participation and the injunctive norm (threshold=90%) combined (logit)

	Current participation beliefs < 12% & Injunctive norm beliefs < 90%		Current participation beliefs < 12% & Injunctive norm beliefs >= 90%		Current participation beliefs >= 12% & Injunctive norm beliefs < 90%		Current participation beliefs >= 12% & Injunctive norm beliefs >= 90%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Injunctive (B)	0.626	0.569	0.162	0.117	0.322	0.587*	-0.309	-0.367
	(0.496)	(0.699)	(0.409)	(0.562)	(0.299)	(0.323)	(0.237)	(0.252)
Dynamic (C)	1.526***	1.315*	0.218	0.853	0.136	0.344	-0.300	-0.334
	(0.531)	(0.749)	(0.423)	(0.629)	(0.289)	(0.307)	(0.238)	(0.251)
Combined (D)	0.736	0.773	0.428	1.483**	-0.157	-0.052	-0.121	-0.224
	(0.455)	(0.619)	(0.397)	(0.617)	(0.274)	(0.290)	(0.243)	(0.256)
Female		-1.317**		0.974*		0.006		0.205
		(0.595)		(0.516)		(0.252)		(0.209)
Children		0.570		-0.130		-0.379*		0.671***
		(0.520)		(0.437)		(0.220)		(0.193)
Household head		0.991*		1.525***		0.332		0.787***
		(0.519)		(0.512)		(0.246)		(0.203)
(Technical) University		0.709		0.749		0.487**		0.667***
		(0.541)		(0.460)		(0.228)		(0.187)
Patience		0.418***		0.546***		0.205***		0.094**
		(0.078)		(0.088)		(0.043)		(0.037)
Responsibility for recycling		2.927***		1.696***		0.793***		1.106***
		(0.697)		(0.587)		(0.281)		(0.226)
Recycling through other ways		-1.045		-1.560**		-0.372		-0.742***
		(0.637)		(0.607)		(0.264)		(0.234)
Constant	0.111	-4.112***	-0.118	-5.740***	0.973***	-1.327***	0.983***	-1.105**
	(0.334)	(1.111)	(0.281)	(1.016)	(0.201)	(0.473)	(0.167)	(0.434)
Pseudo R^2	0.045	0.435	0.005	0.445	0.005	0.096	0.003	0.085
Observations	163	163	188	188	507	507	653	653

Notes: Logit regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who underestimate both the current participation rate and the injunctive norm (below 90) about participation in the recycling programme; columns (3) and (4) to those who underestimate the current participation rate and overestimate or are correct about the injunctive norm (above or equal to 90); columns (5) and (6) to those who overestimate or are correct about the current participation rate and underestimate the injunctive norm (below 90); and columns (7) and (8) to those who overestimate or are correct about both the current participation rate and the injunctive norm (above or equal to 90). Columns (1), (3), (5) and (7) include treatment dummies alone; columns (2), (4), (6) and (8) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

2.F.5. The level of underestimation of the different norms

Table 2.F.17: Heterogeneous treatment effects based on the level of underestimation of the different norms (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Underestimation of injunctive norm	0.000	-0.001							0.001	-0.000
	(0.001)	(0.001)							(0.001)	(0.001)
Injunctive (B)	-0.047	-0.046	0.007	0.019	0.009	0.025	0.003	0.024	-0.062	-0.042
	(0.042)	(0.039)	(0.038)	(0.035)	(0.036)	(0.034)	(0.039)	(0.037)	(0.047)	(0.045)
Dynamic (C)	-0.021	-0.014	-0.014	-0.004	-0.010	0.013	-0.040	-0.017	-0.080*	-0.058
	(0.042)	(0.039)	(0.037)	(0.035)	(0.036)	(0.034)	(0.039)	(0.037)	(0.046)	(0.044)
Combined (D)	-0.027	-0.014	0.024	0.031	-0.013	-0.003	0.003	0.009	-0.025	-0.016
	(0.041)	(0.037)	(0.036)	(0.033)	(0.035)	(0.033)	(0.038)	(0.036)	(0.045)	(0.042)
Injunctive (B) × Underestimation of injunctive norm	0.004***	0.004***							0.003**	0.004***
	(0.001)	(0.001)							(0.001)	(0.001)
Dynamic (C) × Underestimation of injunctive norm	0.002*	0.003**							0.002	0.002*
	(0.001)	(0.001)							(0.001)	(0.001)
Combined (D) × Underestimation of injunctive norm	0.002	0.002							0.001	0.001
	(0.001)	(0.001)							(0.001)	(0.001)
Female		0.026		0.025		0.024		0.025		0.027
		(0.024)		(0.025)		(0.024)		(0.024)		(0.024)
Children		0.027		0.035		0.038*		0.038*		0.037*
		(0.022)		(0.022)		(0.022)		(0.022)		(0.022)
Household head		0.124***		0.118***		0.122***		0.120***		0.123***
		(0.024)		(0.025)		(0.025)		(0.025)		(0.025)
(Technical) University		0.115***		0.103***		0.106***		0.105***		0.101***
		(0.023)		(0.024)		(0.024)		(0.024)		(0.023)
Patience		0.045***		0.044***		0.039***		0.041***		0.041***
		(0.004)		(0.004)		(0.005)		(0.004)		(0.004)
Responsibility for recycling		0.211***		0.229***		0.219***		0.224***		0.204***
		(0.028)		(0.028)		(0.028)		(0.028)		(0.029)
Recycling through other ways		-0.091***		-0.123***		-0.114***		-0.119***		-0.099***
		(0.025)		(0.024)		(0.024)		(0.024)		(0.025)
Underestimation of dynamic norm			-0.006**	-0.006***			-0.004	-0.005**	-0.004	-0.005**
			(0.003)	(0.002)			(0.003)	(0.002)	(0.003)	(0.002)
Injunctive (B) × Underestimation of dynamic norm			0.001	-0.000			0.002	0.001	0.003	0.002
			(0.004)	(0.003)			(0.004)	(0.003)	(0.004)	(0.003)
Dynamic (C) × Underestimation of dynamic norm			0.008**	0.007**			0.007*	0.007**	0.007*	0.007**
			(0.004)	(0.003)			(0.004)	(0.003)	(0.004)	(0.003)
Combined (D) × Underestimation of dynamic norm			-0.002	-0.001			-0.003	-0.002	-0.003	-0.002
			(0.004)	(0.003)			(0.004)	(0.003)	(0.004)	(0.003)
Underestimation of current participation					-0.035***	-0.018***	-0.033***	-0.015**	-0.034***	-0.016**
					(0.007)	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)
Injunctive (B) × Underestimation of current participation					0.001	-0.007	0.000	-0.007	-0.001	-0.008
					(0.010)	(0.008)	(0.010)	(0.008)	(0.009)	(0.008)
Dynamic (C) × Underestimation of current participation					0.025**	0.014*	0.022**	0.011	0.021**	0.011
					(0.010)	(0.008)	(0.010)	(0.008)	(0.010)	(0.008)
Combined (D) × Underestimation of current participation					0.017*	0.015*	0.019*	0.017**	0.018*	0.016**
					(0.010)	(0.008)	(0.010)	(0.008)	(0.010)	(0.008)
Constant	0.668***	0.083*	0.701***	0.123**	0.725***	0.153***	0.742***	0.166***	0.726***	0.168***
	(0.029)	(0.051)	(0.026)	(0.051)	(0.025)	(0.053)	(0.027)	(0.054)	(0.032)	(0.056)
Adjusted R^2	0.012	0.172	0.012	0.174	0.034	0.169	0.043	0.181	0.062	0.191
Observations	1582	1582	1505	1505	1511	1511	1505	1505	1505	1505

Notes: OLS regressions with dependent variable equal to sign-up decision = yes. The level of underestimation is calculated as the difference between the participant's guess minus the true value of the respective norm. Columns (1) and (2) look at interactions of treatments with degrees of underestimation of the injunctive norm; columns (3) and (4) at interactions of treatments with levels of underestimation of the dynamic norm; and columns (5) and (6) at interactions of treatments with underestimation about the current participation rate. Columns (7) and (8) combine the latter two; columns (9) and (10) include all interaction terms together. Columns (1), (3), (5), (7) and (9) are without, columns (2), (4), (6), (8) and (10) with control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.F.18: Heterogeneous treatment effects based on the level of underestimation of the different norms (logit)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Underestimation of injunctive norm	0.001	-0.003							0.005	0.000
	(0.005)	(0.006)							(0.006)	(0.006)
Injunctive (B)	-0.242	-0.318	0.035	0.115	0.044	0.148	0.009	0.150	-0.320	-0.283
	(0.189)	(0.209)	(0.178)	(0.198)	(0.178)	(0.197)	(0.201)	(0.219)	(0.229)	(0.250)
Dynamic (C)	-0.110	-0.122	-0.067	-0.017	-0.049	0.067	-0.203	-0.091	-0.398*	-0.354
	(0.190)	(0.208)	(0.175)	(0.192)	(0.176)	(0.192)	(0.194)	(0.208)	(0.220)	(0.236)
Combined (D)	-0.128	-0.101	0.116	0.164	-0.063	-0.029	0.011	0.042	-0.116	-0.099
	(0.186)	(0.201)	(0.176)	(0.192)	(0.173)	(0.186)	(0.199)	(0.209)	(0.222)	(0.236)
Injunctive (B) × Underestimation of injunctive norm	0.021***	0.030***							0.021***	0.029***
	(0.007)	(0.008)							(0.008)	(0.009)
Dynamic (C) × Underestimation of injunctive norm	0.014*	0.018**							0.011	0.017**
	(0.007)	(0.008)							(0.008)	(0.008)
Combined (D) × Underestimation of injunctive norm	0.010	0.012							0.007	0.009
	(0.007)	(0.007)							(0.008)	(0.008)
Female		0.142		0.133		0.128		0.140		0.165
		(0.137)		(0.142)		(0.141)		(0.143)		(0.144)
Children		0.174		0.216*		0.236*		0.236*		0.235*
		(0.124)		(0.127)		(0.127)		(0.128)		(0.129)
Household head		0.704***		0.640***		0.664***		0.663***		0.723***
		(0.134)		(0.137)		(0.136)		(0.139)		(0.141)
(Technical) University		0.640***		0.567***		0.581***		0.576***		0.576***
		(0.124)		(0.127)		(0.126)		(0.128)		(0.129)
Patience		0.232***		0.222***		0.198***		0.206***		0.217***
		(0.023)		(0.023)		(0.024)		(0.024)		(0.025)
Responsibility for recycling		1.156***		1.249***		1.195***		1.232***		1.140***
		(0.154)		(0.153)		(0.154)		(0.153)		(0.159)
Recycling through other ways		-0.597***		-0.747***		-0.704***		-0.725***		-0.652***
		(0.155)		(0.152)		(0.154)		(0.153)		(0.159)
Underestimation of dynamic norm			-0.025*	-0.030**			-0.021	-0.027**	-0.022	-0.026**
			(0.013)	(0.012)			(0.015)	(0.013)	(0.015)	(0.013)
Injunctive (B) × Underestimation of dynamic norm			0.005	-0.001			0.009	0.003	0.014	0.008
			(0.017)	(0.016)			(0.019)	(0.017)	(0.019)	(0.017)
Dynamic (C) × Underestimation of dynamic norm			0.036**	0.040**			0.036*	0.037**	0.035*	0.035*
			(0.018)	(0.018)			(0.020)	(0.019)	(0.020)	(0.018)
Combined (D) × Underestimation of dynamic norm			-0.009	-0.008			-0.011	-0.011	-0.011	-0.012
			(0.017)	(0.018)			(0.019)	(0.018)	(0.019)	(0.019)
Underestimation of current participation					-0.151***	-0.089***	-0.145***	-0.073**	-0.147***	-0.072**
					(0.031)	(0.033)	(0.032)	(0.033)	(0.032)	(0.034)
Injunctive (B) × Underestimation of current participation					0.003	-0.040	0.002	-0.042	-0.012	-0.056
					(0.044)	(0.045)	(0.045)	(0.045)	(0.046)	(0.048)
Dynamic (C) × Underestimation of current participation					0.106**	0.071	0.094**	0.053	0.089**	0.050
					(0.044)	(0.046)	(0.045)	(0.047)	(0.044)	(0.047)
Combined (D) × Underestimation of current participation					0.073*	0.076*	0.080*	0.085*	0.073*	0.079*
					(0.042)	(0.042)	(0.044)	(0.044)	(0.044)	(0.044)
Constant	0.700***	-2.249***	0.852***	-1.979***	0.966***	-1.828***	1.055***	-1.802***	0.980***	-1.922***
	(0.132)	(0.286)	(0.123)	(0.282)	(0.122)	(0.288)	(0.142)	(0.301)	(0.159)	(0.315)
Pseudo R^2	0.015	0.154	0.013	0.151	0.029	0.147	0.037	0.159	0.058	0.174
Observations	1582	1582	1505	1505	1511	1511	1505	1505	1505	1505

Notes: Logit regressions with dependent variable equal to sign-up decision = yes. The level of underestimation is calculated as the difference between the participant's guess minus the true value of the respective norm. Columns (1) and (2) look at interactions of treatments with degrees of underestimation of the injunctive norm; columns (3) and (4) at interactions of treatments with levels of underestimation of the dynamic norm; and columns (5) and (6) at interactions of treatments with underestimation about the current participation rate. Columns (7) and (8) combine the latter two; columns (9) and (10) include all interaction terms together. Columns (1), (3), (5), (7) and (9) are without, columns (2), (4), (6), (8) and (10) with control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

2.G. Additional analyses: Beliefs about the past participation

Looking at people's beliefs about the participation in the recycling programme in the past, we can see in Figure 2.G.1a that in the dynamic norm treatment C, people's sign-up decision for those who underestimate the participation rate three years ago increases from 56.26% in the control group to 66.04% in the treatment group, although the effect is not statistically significant. We do not find a significant difference in people's sign-up decision for those who overestimate or are correct about the participation in the past, either (Figure 2.G.1b).

The pattern is similar for the combined treatment D (Figure 2.G.1a), where the average sign-up decision for those who underestimate the participation in the past increases from 56.25% in the control group to 67.52% in the treatment group, which is statistically significant (p=0.079 from testing for equality of proportions, n(A)=112 and n(D)=117). Also here, there is no significant difference in people's sign-up decision for those who overestimate or are correct about the participation in the past (Figure 2.G.1b).

The average sign-up decision does not increase significantly in the injunctive norm treatment B, where no information about the past participation is conveyed, although here as well we can see a positive tendency (Figure 2.G.1a). There is no significant difference in people's sign-up decision for those who overestimate or are correct about the past participation rate in treatment B, either (Figure 2.G.1b).

We can see that the average sign-up decision is significantly higher when people have more optimistic beliefs about the past participation rate in the control group, suggesting that individual beliefs about the participation in the past are relevant as well for people's decision to sign up to the recycling programme (56.25% in 2.G.1a vs. 71.89% in 2.G.1b, p=0.023 from testing for equality of proportions, n(A) in 2.G.1a=281 and n(A) in 2.G.1b=112).

Looking at the effects of our treatments for those who underestimate the past participation rate in regression form in Table 2.G.1, column (1) confirms our findings from Figure 2.G.1a that people are more likely to decide to sign up to the recycling programme in the combined treatment D. Column (2) shows that the effect remains robust also when control variables are added. Columns (3) and (4) confirm the findings from Figure 2.G.1b that there is no effect for those who overestimate or are correct about the past participation. The same holds when using logistic regressions in Table 2.G.2.

Figure 2.G.1: Heterogeneity in average sign-up decisions between people who under- or overestimate the past participation

(a) Past participation belief $< 6\%$ **(b)** Past participation belief $>= 6\%$
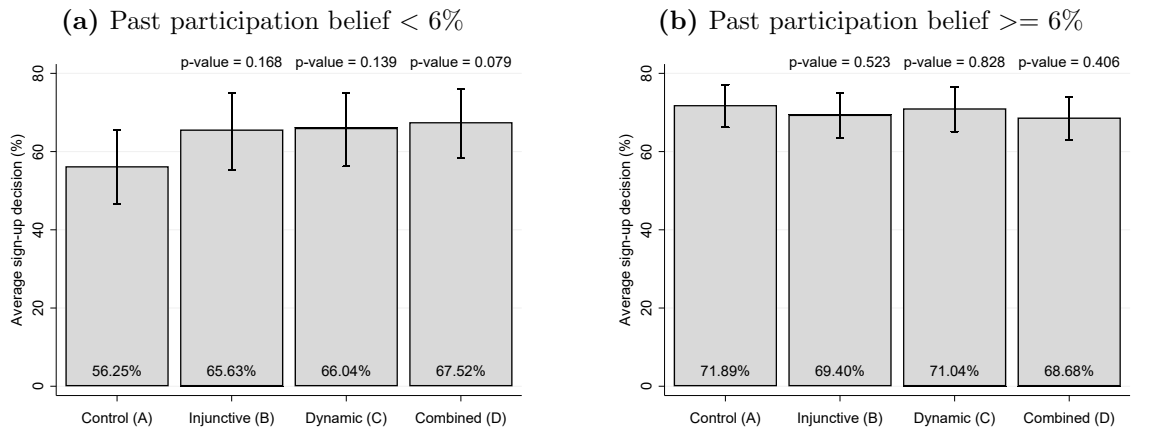
Notes: Average sign-up decision of people who under- or overestimate the past participation, comparing the different treatments B-D with the control group A. n(2.G.1a)=431, n(2.G.1b)=1,089. p-values are obtained from testing for equality of proportions, comparing treatment B, C and D with treatment A, respectively. Graphs show the average sign-up decision by treatment, with 95% confidence intervals for proportions.

Table 2.G.1: Heterogeneous treatment effects: under- vs. overestimation of the past participation (OLS)

	Past participation beliefs < 6%		Past participation beliefs >= 6%	
	(1)	(2)	(3)	(4)
Injunctive (B)	0.094	0.067	-0.025	-0.003
	(0.067)	(0.057)	(0.039)	(0.037)
Dynamic (C)	0.098	0.077	-0.008	0.012
	(0.065)	(0.055)	(0.039)	(0.037)
Combined (D)	0.113*	0.125**	-0.032	-0.020
	(0.064)	(0.054)	(0.039)	(0.036)
Female		-0.041		0.056*
		(0.043)		(0.030)
Children		0.074*		0.019
		(0.040)		(0.027)
Household head		0.040		0.153***
		(0.044)		(0.030)
(Technical) University		0.067		0.127***
		(0.042)		(0.027)
Patience		0.065***		0.031***
		(0.006)		(0.006)
Responsibility for recycling		0.272***		0.215***
		(0.050)		(0.033)
Recycling through other ways		-0.139***		-0.113***
		(0.049)		(0.031)
Constant	0.562***	-0.013	0.719***	0.171***
	(0.045)	(0.073)	(0.027)	(0.064)
Adjusted R^2	0.002	0.293	-0.002	0.114
Observations	431	431	1089	1089

Notes: OLS regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who underestimate the past participation in the recycling programme; columns (3) and (4) to those who are correct about or overestimate it. Columns (1) and (3) include treatment dummies alone; columns (2) and (4) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.G.2: Heterogeneous treatment effects: under- vs. overestimation of the past participation (logit)

	Past participation beliefs < 6%		Past participation beliefs >= 6%	
	(1)	(2)	(3)	(4)
Injunctive (B)	0.395	0.388	-0.120	-0.015
	(0.287)	(0.345)	(0.188)	(0.201)
Dynamic (C)	0.414	0.476	-0.041	0.060
	(0.280)	(0.340)	(0.191)	(0.204)
Combined (D)	0.481*	0.761**	-0.153	-0.127
	(0.274)	(0.336)	(0.185)	(0.197)
Female		-0.266		0.304*
		(0.267)		(0.169)
Children		0.485*		0.131
		(0.255)		(0.146)
Household head		0.268		0.810***
		(0.267)		(0.162)
(Technical) University		0.424*		0.673***
		(0.253)		(0.147)
Patience		0.344***		0.153***
		(0.041)		(0.029)
Responsibility for recycling		1.719***		1.113***
		(0.333)		(0.177)
Recycling through other ways		-0.972***		-0.666***
		(0.332)		(0.177)
Constant	0.251	-2.868***	0.939***	-1.706***
	(0.190)	(0.485)	(0.133)	(0.341)
Pseudo R^2	0.007	0.263	0.001	0.103
Observations	431	431	1089	1089

Notes: Logit regressions with dependent variable equal to sign-up decision = yes. Columns (1) and (2) are restricted to those who underestimate the past participation in the recycling programme; columns (3) and (4) to those who are correct about or overestimate it. Columns (1) and (3) include treatment dummies alone; columns (2) and (4) add control variables for gender, whether the household has children, whether the respondent is the household head, level of education, level of patience, whether the respondent is responsible for recycling within the household and whether the household already recycles through other ways. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

2.H. Other pre-registered analyses: Post-treatment beliefs

We further observe whether receiving information about dynamic and injunctive norms regarding participation in the recycling programme alters people's beliefs about participation rates in the programme in the future (as suggested by Sparkman and Walton, 2017) as well as beliefs about their own and their collective actions as a group being effective (personal and collective response efficacy, as suggested by Doherty and Webler, 2016). The two are interlinked, as only with a large enough number of people engaging in recycling behaviour the own as well as the collective actions as a group will be effective. Previous research has shown that efficacy beliefs are important for motivating pro-environmental behaviour (Bostrom et al., 2019; Doherty and Webler, 2016; Hart and Feldman, 2016). We therefore pre-registered the analysis to test whether biased beliefs about dynamic and injunctive norms might be accompanied by low efficacy beliefs, which could be changed by correcting those beliefs with our information treatments.

Our pre-registered analysis of social norm treatment effects on post-treatment beliefs shows that the dynamic norm treatment C reduces beliefs about collective response efficacy, on average (Table 2.H.1). This is interesting and not as expected. It seems to be driven by the people who overestimate the trend (Table 2.H.2) and the current participation rate (Table 2.H.3), which makes sense as they are in the majority, on average. Conceptually, this is plausible since for people who believe the trend to be stronger and the current participation rate to be higher than it actually is, being informed about a weaker trend and a lower participation rate may reduce the belief that as a group the households together in Miraflores can make an important contribution to environmental protection when they participate in the recycling programme. Based on the same reasoning, it is plausible that the dynamic norm treatment C has a negative effect on people's beliefs about future participation in the recycling programme when people overestimate the trend in participation rates (Table 2.H.2). The dynamic norm treatment C also reduces people's beliefs about future participation in the programme as well as collective response efficacy when people underestimate the injunctive norm (Table 2.H.4). It might be that since the dynamic norm treatment C conveys information about the still rather low current participation rate (and for some people the lower trend than expected), this combined with beliefs about a lower injunctive norm may lead to more negative beliefs about future participation and collective response efficacy (which may be due to the fact that beliefs about the different norms are correlated, as discussed before). Based on the same reasoning, when people have high injunctive norm beliefs, being informed about the trend in the dynamic norm treatment C has a positive effect on people's beliefs about future participation.

Table 2.H.1: Average treatment effects on post-treatment beliefs (OLS)

	Future participation		Personal response efficacy		Collective response efficacy	
	(1)	(2)	(3)	(4)	(5)	(6)
Injunctive (B)	0.071	0.085	0.082	0.092	0.023	0.033
	(0.103)	(0.101)	(0.089)	(0.082)	(0.084)	(0.081)
Dynamic (C)	-0.125	-0.127	-0.009	-0.003	-0.233**	-0.233***
	(0.110)	(0.106)	(0.090)	(0.082)	(0.092)	(0.089)
Combined (D)	-0.019	0.030	0.048	0.114	-0.038	0.013
	(0.103)	(0.100)	(0.089)	(0.082)	(0.083)	(0.080)
Female		-0.004		0.067		0.014
		(0.088)		(0.072)		(0.073)
Children		-0.236***		-0.346***		-0.216***
		(0.075)		(0.059)		(0.061)
Household head		-0.056		-0.037		-0.074
		(0.085)		(0.070)		(0.073)
(Technical) University		-0.042		0.036		-0.044
		(0.079)		(0.060)		(0.064)
Patience		0.094***		0.151***		0.101***
		(0.015)		(0.014)		(0.013)
Responsibility for recycling		-0.308***		0.126		-0.185**
		(0.108)		(0.079)		(0.086)
Recycling through other ways		0.600***		0.252***		0.489***
		(0.109)		(0.078)		(0.093)
Constant	5.687***	4.982***	6.009***	4.752***	6.251***	5.472***
	(0.071)	(0.170)	(0.065)	(0.149)	(0.056)	(0.147)
Adjusted R^2	0.000	0.049	-0.001	0.139	0.004	0.072
Observations	1709	1709	1709	1709	1709	1709

Notes: OLS regressions with dependent variables: (1)-(2) beliefs about future participation in the recycling programme, (3)-(4) beliefs about personal response efficacy, and (5)-(6) beliefs about collective response efficacy. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.H.2: Heterogeneous treatment effects on post-treatment beliefs: under- vs. overestimation of the dynamic norm (OLS)

	Dynamic norm beliefs < 6%						Dynamic norm beliefs >= 6%					
	Future participation		Personal response efficacy		Collective response efficacy		Future participation		Personal response efficacy		Collective response efficacy	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Injunctive (B)	0.228	0.217	0.298	0.186	0.075	0.029	0.008	0.090	-0.069	0.017	-0.035	0.038
	(0.203)	(0.202)	(0.183)	(0.168)	(0.172)	(0.169)	(0.127)	(0.124)	(0.109)	(0.105)	(0.105)	(0.102)
Dynamic (C)	-0.004	0.052	0.069	0.064	-0.300	-0.276	-0.242*	-0.244*	-0.126	-0.113	-0.298**	-0.310***
	(0.220)	(0.213)	(0.193)	(0.176)	(0.186)	(0.180)	(0.138)	(0.136)	(0.107)	(0.101)	(0.119)	(0.116)
Combined (D)	-0.098	-0.044	0.058	0.109	-0.246	-0.202	0.019	0.081	0.031	0.103	0.024	0.081
	(0.206)	(0.202)	(0.188)	(0.175)	(0.179)	(0.176)	(0.124)	(0.120)	(0.103)	(0.098)	(0.095)	(0.092)
Female		0.085		0.187		0.049		-0.169		-0.047		-0.059
		(0.168)		(0.140)		(0.153)		(0.109)		(0.089)		(0.086)
Children		-0.336**		-0.255**		-0.117		-0.181*		-0.296***		-0.236***
		(0.147)		(0.119)		(0.127)		(0.094)		(0.077)		(0.077)
Household head		0.049		0.094		-0.116		-0.101		-0.148*		-0.078
		(0.166)		(0.143)		(0.152)		(0.105)		(0.083)		(0.086)
(Technical) University		-0.038		0.211*		-0.057		-0.024		-0.070		-0.035
		(0.156)		(0.124)		(0.127)		(0.099)		(0.075)		(0.082)
Patience		0.065***		0.172***		0.097***		0.105***		0.129***		0.096***
		(0.024)		(0.022)		(0.021)		(0.020)		(0.018)		(0.018)
Responsibility for recycling		-0.339*		0.047		-0.095		-0.214		0.048		-0.271**
		(0.196)		(0.148)		(0.149)		(0.142)		(0.102)		(0.121)
Recycling through other ways		0.729***		0.186		0.418***		0.534***		0.249**		0.531***
		(0.178)		(0.142)		(0.153)		(0.152)		(0.104)		(0.137)
Constant	5.078***	4.497***	5.681***	4.160***	6.050***	5.304***	6.052***	5.257***	6.337***	5.384***	6.470***	5.735***
	(0.145)	(0.286)	(0.142)	(0.264)	(0.119)	(0.273)	(0.088)	(0.221)	(0.075)	(0.191)	(0.070)	(0.187)
Adjusted R^2	-0.001	0.037	0.000	0.141	0.005	0.046	0.002	0.054	-0.000	0.097	0.009	0.079
Observations	574	574	574	574	574	574	931	931	931	931	931	931

Notes: OLS regressions with dependent variables: (1)-(2) and (7)-(8) beliefs about future participation in the recycling programme, (3)-(4) and (9)-(10) beliefs about personal response efficacy, and (5)-(6) and (11)-(12) beliefs about collective response efficacy. Columns (1)-(6) are restricted to those who underestimate the dynamic norm; columns (7)-(12) to those who overestimate or are correct about it. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.H.3: Heterogeneous treatment effects on post-treatment beliefs: under- vs. overestimation of the current participation (OLS)

	Current participation beliefs < 12%						Current participation beliefs >= 12%					
	Future participation		Personal response efficacy		Collective response efficacy		Future participation		Personal response efficacy		Collective response efficacy	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Injunctive (B)	0.121	0.147	0.350	0.244	0.062	0.047	0.034	0.069	-0.037	0.011	-0.031	0.007
	(0.253)	(0.256)	(0.248)	(0.220)	(0.247)	(0.235)	(0.126)	(0.123)	(0.101)	(0.098)	(0.095)	(0.093)
Dynamic (C)	-0.027	-0.068	0.131	0.035	-0.175	-0.175	-0.199	-0.208	-0.117	-0.100	-0.338***	-0.343***
	(0.282)	(0.268)	(0.253)	(0.216)	(0.237)	(0.223)	(0.133)	(0.129)	(0.101)	(0.098)	(0.111)	(0.108)
Combined (D)	0.158	0.295	0.177	0.271	0.099	0.238	-0.095	-0.077	-0.002	0.038	-0.139	-0.115
	(0.243)	(0.230)	(0.250)	(0.228)	(0.228)	(0.213)	(0.129)	(0.125)	(0.097)	(0.093)	(0.096)	(0.094)
Female		-0.338		0.028		-0.117		0.070		0.045		0.022
		(0.207)		(0.177)		(0.189)		(0.107)		(0.085)		(0.085)
Children		-0.388**		-0.496***		-0.206		-0.283***		-0.230***		-0.206***
		(0.181)		(0.155)		(0.160)		(0.093)		(0.071)		(0.074)
Household head		0.215		0.225		-0.048		-0.090		-0.114		-0.088
		(0.212)		(0.184)		(0.201)		(0.102)		(0.081)		(0.082)
(Technical) University		-0.108		0.026		-0.065		-0.062		0.025		-0.032
		(0.206)		(0.170)		(0.184)		(0.096)		(0.070)		(0.076)
Patience		0.102***		0.174***		0.104***		0.069***		0.119***		0.082***
		(0.027)		(0.027)		(0.026)		(0.019)		(0.017)		(0.016)
Responsibility for recycling		-0.119		0.265		0.204		-0.393***		-0.059		-0.372***
		(0.231)		(0.185)		(0.197)		(0.135)		(0.095)		(0.104)
Recycling through other ways		0.551**		0.097		0.524**		0.767***		0.295***		0.525***
		(0.229)		(0.178)		(0.204)		(0.136)		(0.097)		(0.117)
Constant	5.195***	4.529***	5.460***	4.193***	5.862***	4.932***	5.849***	5.237***	6.286***	5.302***	6.451***	5.863***
	(0.178)	(0.337)	(0.190)	(0.320)	(0.166)	(0.325)	(0.088)	(0.213)	(0.072)	(0.178)	(0.063)	(0.176)
Adjusted R^2	-0.007	0.056	-0.003	0.186	-0.004	0.078	0.001	0.052	-0.001	0.075	0.008	0.060
Observations	351	351	351	351	351	351	1160	1160	1160	1160	1160	1160

Notes: OLS regressions with dependent variables: (1)-(2) and (7)-(8) beliefs about future participation in the recycling programme, (3)-(4) and (9)-(10) beliefs about personal response efficacy, and (5)-(6) and (11)-(12) beliefs about collective response efficacy. Columns (1)-(6) are restricted to those who underestimate the current participation rate; columns (7)-(12) to those who overestimate or are correct about it. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 2.H.4: Heterogeneous treatment effects on post-treatment beliefs: under- vs. overestimation of the injunctive norm (OLS)

	Injunctive norm beliefs < 97%						Injunctive norm beliefs >= 97%					
	Future participation		Personal response efficacy		Collective response efficacy		Future participation		Personal response efficacy		Collective response efficacy	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Injunctive (B)	-0.042	-0.029	0.044	0.053	-0.016	-0.001	0.349*	0.408**	0.147	0.188	0.121	0.162
	(0.140)	(0.135)	(0.119)	(0.109)	(0.118)	(0.113)	(0.179)	(0.179)	(0.154)	(0.147)	(0.128)	(0.126)
Dynamic (C)	-0.371**	-0.319**	-0.069	-0.015	-0.383***	-0.334***	0.355**	0.337*	0.053	0.044	-0.001	-0.009
	(0.151)	(0.142)	(0.118)	(0.107)	(0.130)	(0.123)	(0.178)	(0.172)	(0.165)	(0.153)	(0.137)	(0.128)
Combined (D)	-0.136	-0.065	0.070	0.143	-0.067	0.003	0.242	0.331*	0.005	0.109	-0.020	0.059
	(0.139)	(0.134)	(0.114)	(0.107)	(0.116)	(0.112)	(0.182)	(0.178)	(0.165)	(0.152)	(0.132)	(0.126)
Female		-0.036		0.052		0.011		-0.029		0.022		-0.073
		(0.117)		(0.093)		(0.101)		(0.136)		(0.120)		(0.103)
Children		-0.287***		-0.376***		-0.236***		-0.291**		-0.090		-0.109
		(0.101)		(0.079)		(0.086)		(0.129)		(0.110)		(0.094)
Household head		-0.143		-0.155*		-0.143		0.009		0.029		-0.127
		(0.116)		(0.091)		(0.102)		(0.135)		(0.124)		(0.101)
(Technical) University		0.263**		0.272***		0.232**		-0.300**		-0.250**		-0.318***
		(0.116)		(0.087)		(0.098)		(0.129)		(0.104)		(0.098)
Patience		0.078***		0.142***		0.089***		0.106***		0.165***		0.113***
		(0.019)		(0.017)		(0.017)		(0.024)		(0.024)		(0.020)
Responsibility for recycling		-0.288**		0.081		-0.245**		-0.084		0.150		0.065
		(0.145)		(0.103)		(0.117)		(0.175)		(0.137)		(0.126)
Recycling through other ways		0.844***		0.367***		0.699***		0.257		-0.082		0.023
		(0.141)		(0.100)		(0.125)		(0.192)		(0.146)		(0.136)
Constant	5.645***	4.763***	6.018***	4.686***	6.199***	5.262***	5.693***	5.037***	6.139***	4.969***	6.409***	5.818***
	(0.095)	(0.221)	(0.085)	(0.187)	(0.079)	(0.195)	(0.135)	(0.265)	(0.124)	(0.264)	(0.092)	(0.229)
Adjusted R^2	0.004	0.076	-0.001	0.149	0.008	0.091	0.005	0.052	-0.004	0.129	-0.003	0.086
Observations	1062	1062	1062	1062	1062	1062	520	520	520	520	520	520

Notes: OLS regressions with dependent variables: (1)-(2) and (7)-(8) beliefs about future participation in the recycling programme, (3)-(4) and (9)-(10) beliefs about personal response efficacy, and (5)-(6) and (11)-(12) beliefs about collective response efficacy. Columns (1)-(6) are restricted to those who underestimate the injunctive norm; columns (7)-(12) to those who overestimate or are correct about it. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Chapter 3

Using reminder messages to increase recycling behaviour in Peru

Abstract

The municipality of Miraflores in Lima, Peru, has established a recycling programme, in which households can participate voluntarily and free of charge. Although households sign up to the programme voluntarily, few recycle regularly, pointing towards a gap between people's intention to recycle and their actual recycling behaviour. Reminders can help people to follow through with their intentions by addressing the problem of limited attention. In this study, we conduct a randomized controlled trial (RCT) to test whether sms reminders can increase recycling behaviour of households. In particular, we contrast the effect of continuous vs. interrupted vs. restarted reminders on households' recycling behaviour. We measure recycling behaviour of households over 12 weeks in total. While the first three weeks serve as a baseline measure, the subsequent nine weeks constitute our intervention period where households are randomly assigned to either i) a control group that does not receive any reminders, ii) a group that receives continuous reminders over the whole nine weeks (continuous treatment), iii) a group that receives reminders only for the first three weeks (interrupted treatment), iv) or a group that receives reminders for the first three weeks and for the last three weeks, with a three weeks' pause in between (restarted treatment). We show that sms reminders are generally effective to encourage recycling behaviour, and that sending reminders continuously is most effective over time, which suggests that limited attention on the recycling day itself is a main obstacle.

3.1. Introduction

Environmental pollution, as a result of increasing global waste accumulation, is posing a serious threat to people and ecosystems, with the waste sector contributing significantly to global climate change (UNEP, 2015). As a consequence, sustainable waste and resource management has become crucial for countries' climate change mitigation efforts, of which recycling is one of the key elements. Waste separation in the household is essential in this regard, as without the effort of individual consumers to separate their materials at home, there can be no functioning recycling process (Y. Dai et al., 2015; Varotto and Spagnolli, 2017). Thus, in addition to the adequate provision of recycling services and infrastructure from the political side, behavioural change at the individual level is crucial as well to successfully tackle worldwide pollution (Knickmeyer, 2020).

While countries in the Global North used to account for the largest part in global waste generation, low- and middle-income countries are expected to overtake this position soon, with rising populations, migration to cities and changing consumption patterns leading to rapidly growing waste accumulation (UNEP, 2015). At the same time, the infrastructure for waste management is often worse than in developed countries, while large amounts of waste en up in local dumpsites, threatening both people's health and the environment. This also holds in the case of Peru, where only 4% of all waste generated in the capital city, Lima, is recycled (WWF, 2018). At the same time, a large proportion of waste generated in Peru can be attributed to households, underlining the important role of the individual consumer (Borasino and Fuhrmann-Riebel, 2021).

In Peru, recycling activities at the household level are coordinated by municipalities.[1] Some municipalities in Lima have established their own recycling programmes, in which households can participate voluntarily and free of charge – as has the municipality of Miraflores, an upper middle- to high-income neighbourhood in Lima, with which we have teamed up for this study. The recycling programme works in a way that households separate their recyclable materials at home and collect them in a separate bag, which they then need to place outside their house on the street on a specific day per week to be collected by formal recyclers. Yet, a main challenge for the programme to be successful is that many of the registered households do not recycle regularly.

Since people sign up to the recycling programme voluntarily, we can expect that they have a general interest and intention to recycle – otherwise, people could simply refrain from signing up in the first place. Thus, if people who have voluntarily signed up to the programme do not recycle regularly, they may still intend to do so, but there seem to be certain factors that prevent them from following through with it. As a result, we see a

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[1] Borasino and Fuhrmann-Riebel (2021) provide a more detailed mapping of the different actors involved in the Peruvian recycling sector.

gap between people's intention to recycle and their actual recycling behaviour. The fact that people have trouble to follow through with good intentions is a common observation in many domains, from health-related behaviours like exercising or sticking to diets, over completing important application forms in time to financial decision-making (Rogers et al., 2015). Especially in the context of pro-environmental behaviours, research has shown that people often fail to live up to their own climate-friendly behaviour intentions (Hornsey et al., 2016; Mazar et al., 2021; Tiefenbeck et al., 2018).

An important reason why people fail to fulfil their intentions can be the problem of limited attention (Datta and Mullainathan, 2014; Karlan et al., 2016; Tiefenbeck et al., 2018). In the case of the recycling programme in Miraflores, the recycling bags are collected once or twice a week (depending on the zones) by formal recyclers, always on the same day(s) at approximately the same time. Thus, paying attention to this specific day and time period to place the recycling bags outside on the street is crucial for people to participate successfully. However, attention is a limited resource (Mullainathan and Shafir, 2013) and people may simply forget about the recycling truck passing by on certain days.

Research has shown that sending timely reminders can help to overcome the problem of limited attention, thereby facilitating people to follow through with their intentions, by bringing the specific behaviour "to the top of mind" (Karlan et al., 2016). The effectiveness of reminders has been demonstrated in a variety of settings, such as personal savings (Karlan et al., 2016; Rodriguez and Saavedra, 2016), electricity consumption (Allcott and Rogers, 2014; Gilbert and Zivin, 2014), loan payment (Cadena and Schoar, 2011), gym attendance (Calzolari and Nardotto, 2017; Muller and Habla, 2018; Milkman, Gromet et al., 2021), participation in land conservation programmes (Wallander et al., 2017), the adoption of agricultural technologies (Larochelle et al., 2019), donations to charity (Huck and Rasul, 2010; Sonntag and Zizzo, 2015; Damgaard and Gravert, 2018), returning books in time to a library (Apesteguia et al., 2013), dental health prevention (Altmann and Traxler, 2014) or vaccination uptake (Milkman, Patel et al., 2021), such as recently in the context of covid-19 vaccinations (H. Dai et al., 2021).

In this study, we conduct a randomized controlled trial (RCT) to test whether simple sms reminder messages can increase the recycling behaviour of households in the context of the recycling programme in Miraflores. Our reminders aim to shift people's attention to the recycling programme, which we expect to result in increased recycling activity. In particular, we contrast the effects of continuous vs. interrupted vs. restarted reminders on households' recycling behaviour. Especially for behaviours that require regular engagement such as recycling, it is important to understand whether continuous reminders are needed and effective to successfully encourage behaviour over a longer time period, or whether initial reminders might be enough to induce some form of habit formation

(Charness and Gneezy, 2009) that may persist even after the intervention has ended, or whether stopping and restarting the intervention might be more effective to capture people's attention over time and eventually lead to higher recycling activity. Our design allows us to analyse the effects of continuous reminders on households' recycling behaviour, persistence of reminder effects after the intervention has ended, as well as restart effects when the intervention is taken up again.

We measure households' recycling behaviour over a total time period of 12 weeks. While the first three weeks serve as a baseline measure, the subsequent nine weeks constitute our intervention period where households are randomly assigned to either i) a control group that does not receive any reminders, ii) a group that receives continuous reminders over the whole nine weeks (continuous treatment), iii) a group that receives reminders only for the first three weeks (interrupted treatment), iv) or a group that receives reminders for the first three weeks and for the last three weeks, with a three weeks' pause in between (restarted treatment).

Identifying ways to encourage household recycling behaviour has been a core interest of both researchers and policy makers over the last years (see e.g. Kirakozian (2016), Knickmeyer (2020), Thomas and Sharp (2013), Varotto and Spagnolli (2017) or Miafodzyeva and Brandt (2013) for literature reviews and meta-analyses). With our study, we contribute to a better understanding of how a low-cost, easily scalable tool in the form of sending weekly sms reminders can be used effectively to increase households' recycling activity in the Peruvian context. We show that among all households that received a reminder the percentage of those that recycle is significantly higher than among households in the control group that did not receive any reminders. We further provide evidence for strong continuous reminder effects on households' recycling behaviour, which remain significant throughout the whole intervention period. We also find some evidence for persistence effects of reminders after the intervention was interrupted, although the data is less clear in this regard. Our findings speak in favour of the theory that limited attention on the recycling day itself is a main obstacle for people to recycle regularly, which can be addressed by reminding people to recycle on a regular basis. Moreover, our data indicates that receiving reminders leads to some form of habit formation in people's recycling behaviour, while these results are less robust. We cannot find evidence for positive restart effects of reminders, although data issues during the last three weeks of our intervention period complicate the analysis in this matter. Finally, we do not find any evidence for potentially negative effects as a result of sending reminders, nor do we find that there are any backfiring effects after the reminder nudge has been taken away.

We contribute to the existing literature in the following ways in particular. First, we extend existing evidence on the effectiveness of reminders in the context of household

recycling behaviour.[2] To our knowledge, there is only one other study conducted by Chong et al. (2015) that has done so before. The study was conducted in Peru as well, although in a suburban setting that is not comparable to ours. The authors did not find any significant effects of sending sms reminders on households' recycling activity, while only providing households with a recycling bin showed positive effects. Thus, a proper recycling infrastructure proved to be the main obstacle for households to recycle, which is not a matter of concern in our study context. Another study related albeit different both in terms of its target behaviour and study region as well as the way reminders were transmitted is the one of Essl et al. (2021), in which weekly reminders in the form of stickers and flyers were provided to households to encourage the reuse of plastic bags in the context of weekly food delivery boxes in Switzerland. The authors found significant positive effects of flyer and sticker reminders on households' plastic waste reduction. We add to this so far limited evidence of reminders' effectiveness in the context of recycling and show that they can significantly increase households' active participation in the recycling programme.

By doing so, we shed light on the effectiveness of reminders in the context of a behaviour that differs from many other behaviours so far studied in the reminder literature in the sense that using reminders to encourage recycling behaviour falls under the category of "green nudges" (Carlsson et al., 2021). A nudge in general has been defined by Thaler and Sunstein (2008) as a change in the choice architecture that influences people's behaviour without restricting people's decision making or significantly changing the economic incentives. Yet, Carlsson et al. (2021) point out that it is important to distinguish between so called "self-focused" nudges that aim to improve the welfare of an individual (such as higher financial savings or a healthier lifestyle), and green nudges that aim to influence behaviour to reduce the negative environmental externalities associated with people's decision making, such as waste accumulation or resource use.[3] This means that when making use of green nudges, it is less about increasing the personal benefits of an individual but more about increasing the societal environmental benefits resulting from people's behaviour. Given their high economic relevance, green nudges offer a promising tool for policy makers to address societal challenges. With our study, we add important new evidence on their effectiveness.

Second, we link up to studies that investigate the effectiveness of reminders in the context of regular, repeated behaviours as opposed to infrequent, one-time decisions. Many reminder studies have focused on infrequent behaviours, such as dental health check-ups

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[2] In particular, we investigate the effectiveness of reminders in the context of curbside recycling, where materials are accumulated at home and then left at the curb for collection, as opposed to central location recycling, for which materials are taken to a central location for processing, or public recycling, where people make use of public recycling stations or bins (Osbaldiston and Schott, 2012).

[3] Another example in the reminder literature in a different domain is the study by Allcott and Rogers (2014) in the context of energy use.

(Altmann and Traxler, 2014), the timely returning of books to a library (Apesteguia et al., 2013) or donations to charity (Huck and Rasul, 2010). Our paper adds to the studies that have focused on repeated behaviours, such as regular gym attendance (Calzolari and Nardotto, 2017; Muller and Habla, 2018) or energy conservation (Allcott and Rogers, 2014). Making this distinction is important as in the case of regular behaviours, different mechanisms can be in place. One important aspect is that habit formation can play a role (Calzolari and Nardotto, 2017; Charness and Gneezy, 2009; Mazar et al., 2021; Taubinsky, 2014; Thomas and Sharp, 2013; Wood and Neal, 2016). Engaging in a behaviour repeatedly increases the likelihood that it will come to mind again (Taubinsky, 2014). In the case of recycling behaviour in our study context, people need to organize their internal waste management in the household in advance, collect materials throughout the week, and finally put the bags outside their house on the same day(s) each week. This makes it plausible to assume that habituation and routine in these practices play a role for people's successful participation in the recycling programme. In contrast, unsustainable habits such as not separating their waste during the week may be the reason why people fail to live up to their recycling intentions, which is a common obstacle for many pro-environmental behaviours (Mazar et al., 2021).

With our study, we can test whether reminders are effective to induce some form of habit formation in people's recycling practices that persists even after the intervention was terminated. Similar to the findings of Calzolari and Nardotto (2017) and Muller and Habla (2018) in the context of gym use, our data suggests that some habit formation is happening as a result of receiving reminders for a while, although – as in the case of the aforementioned studies as well – our evidence is not robust in this regard. However, the studies differ from our design in the sense that they only looked at people's post-treatment behaviour in their respective sample populations to derive conclusions about persistence. Our approach is more similar to the one of Allcott and Rogers (2014) as it allows us to compare the persistence effects of sending reminders to a group that continues to receive reminders during that same time period, which the authors investigated as well in the context of energy use.[4] In line with the observations of Allcott and Rogers (2014), our data suggests that while reminder effects persist to some extent after the intervention was interrupted (for which the authors provide even more robust evidence), sending reminders continuously has stronger treatment effects on people's behaviour than discontinued reminders.[5] We therefore provide important new insights on the effects of

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[4] Despite the similarities to Allcott and Rogers (2014), it should be noted that our study differs in several fundamental ways from their design. Most importantly, the authors had a much larger data set and were able to investigate energy usage behaviour of households over a much longer time frame (several years), which allowed for more detailed analyses on long-term and persistence effects. Moreover, the reminders were sent via "home energy reports" instead of via sms. In addition, the reports contained several pieces of information that went beyond a simple action reminder as in our case, such as energy saving tips and social comparisons to neighbours.

[5] This is true when we exclude the last three weeks from the analysis, during which the results are

continued vs. discontinued reminders, which has so far rarely been investigated.

Third, we extend evidence on the role of the frequency with which reminders are sent. Evidence on the resulting effects of sending reminders repeatedly is mixed. Damgaard and Gravert (2018) find both positive and negative effects of repeating reminders (once) compared to a single reminder, as they led to increased donations to a charity on the one hand but also to higher unsubscriptions from the mailing list on the other hand. Altmann and Traxler (2014) found that repeating reminders (once) neither strengthened nor weakened the effects of a single reminder on the likelihood that people would schedule their next check-up at the dentist. Sonntag and Zizzo (2015) found that weekly reminders were no more effective than monthly reminders in increasing charitable giving. Yet, these studies are characterised by the fact that they focus on a one-time or infrequent decision as their respective target behaviour. In the case of repeated target behaviours with regular reminders, as it is the case in our study, the evidence suggests that sending reminders repeatedly has positive effects, as found for example in Calzolari and Nardotto (2017) and Muller and Habla (2018) in the context of gym use, or Allcott and Rogers (2014) in the context of energy conservation. Similar to the authors' findings in their respective contexts, we do not find evidence that the effects of continued reminders diminish over time as a result of people getting used to receiving regular messages.[6] We further find no evidence that sending reminders repeatedly has any negative consequences on people's recycling behaviour as for example suggested by Damgaard and Gravert (2018) and Gravert (2022). The fact that the continuous treatment has a robust and significant effect on households' recycling behaviour throughout the whole intervention period speaks in favour of the theory that limited attention on the collection day itself is a main obstacle for people to recycle regularly.[7]

Moreover, we add a novel element to our design as we investigate what happens when reminders are first interrupted and then restarted again. We can therefore test whether interrupting and restarting the sending of reminders is more effective to capture people's attention over time than sending reminders continuously, as well as whether there are any potential negative consequences resulting from forcibly interrupting a reminder intervention for a certain time.[8]

Fourth, we link up to studies that make use of sms reminders to encourage a certain

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less clear due to data issues, on which we reflect more in detail at a later stage in the paper.

[6] Again, the results weaken if we include the last three weeks in the analysis, although as already mentioned this is likely due to data issues, on which we reflect later on in the paper.

[7] We therefore link up to studies that have identified limited attention to be a main obstacle for behaviour change in other domains, such as Karlan et al. (2016) in the context of personal savings, Calzolari and Nardotto (2017) in the context of gym use or Altmann and Traxler (2014) in the context of dental heath prevention.

[8] The idea of the restarted treatment is linked to the model of inattentive choice and the corresponding experiment of Taubinsky (2014).

behaviour, such as Adams et al. (2021), Cadena and Schoar (2011), Chong et al. (2015), Karlan et al. (2016), Larochelle et al. (2019) or Wagstaff et al. (2019). Reminders can be transmitted in several different ways, from physical reminders in the form of prompts or signs to written reminders in the form of emails, letters or sms (Gravert, 2022). By applying sms reminders, we therefore contribute to the growing literature that makes use of "digital nudging" (Hummel and Maedche, 2019; Weinmann et al., 2016). A great benefit of sms reminders lies in their low implementation costs, which is especially important for studies conducted outside a high-income country context where financial resources may be limited. Moreover, sending sms reminders constitutes an intervention that can easily be scaled up, which is of increasing interest to academics and policy makers and thus of great policy relevance (see e.g. the recent book "The Voltage Effect" by John List (List, 2022)).

Fifth and finally, we conduct our research among urban households in Peru, which is a so far underrepresented consumer group in the relevant literature. As Chong et al. (2015) point out, very little academic work has been conducted on recycling behaviour among countries in the Global South, even though these are the regions that suffer most from inefficient waste management and the resulting environmental consequences. Moreover, based on a quantitative review of empirical nudging studies, Hummel and Maedche (2019) conclude that the whole Latin American region is largely uncovered in the nudging literature, while the vast majority of research has been conducted in the Global North. At the same time, research has shown that insights generated among populations in the Global North cannot necessarily be copied to contexts in the Global South (Henrich et al., 2010).[9] With our study, we therefore provide important new evidence on the effectiveness of reminders to encourage recycling behaviour among a consumer group that has so far received little attention despite its great relevance for both scientific and policy debates on the topic.

The paper proceeds as follows. In section 3.2 we outline our experimental design, including the sampling process and treatment groups, the experimental procedure and hypotheses. In section 3.3 we present the results from our analysis. The paper ends with a discussion and conclusion in section 3.4.

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[9] The strong motivation of their design by studies conducted in the United States is one possible explanation of the authors why Chong et al. (2015) do not find any significant effects of their treatments on households' recycling behaviour in Peru since norms, attitudes and knowledge about recycling vary.

3.2. Experimental Design

3.2.1. Sampling

There are 7,183 households officially registered in the recycling programme (end of March 2021). Of those 7,183 households, 3,480 are registered with a unique address.[10] Since the district of Miraflores contains single family houses as well as apartment buildings and other multiple dwelling units, two households being registered with the same address means that those households are living in the same building or dwelling unit. In our study, we focus on registered households with a unique address only. This means, we include single family houses as well as apartment buildings/dwelling units where only one household is registered. We do so, because in the case of apartment buildings/dwelling units, the bags per building/unit are collected jointly, which makes it impossible to identify to which household the bags belong if there is more than one household registered in the same building/unit.[11]

When households register for the recycling programme, they are asked to leave a phone number as contact details. Households can decide whether they register with a landline or a cell phone number. Since sending reminders via sms requires access to cell phone numbers, we focus on those households of which cell phone numbers are available. Of the 3,480 households that are registered with a unique address, this is the case for 1,392 households. These 1,392 households constitute our overall sample. However, it should be noted that we cannot be sure that all 1,392 cell phone numbers are still valid as the municipality does not verify the contact information in their data base on a regular basis. Thus, our overall sample might be slightly smaller based on how many numbers are still up to date.

3.2.2. Treatment groups and message

We randomly distributed our sample into four groups of equal size. Given that our overall sample consists of 1,392 households, this means there are 348 households per group. The randomization was done at the individual household level and has been performed in Stata. The four groups consist of one control group and three treatment groups, which received the reminders during the intervention period as follows (after a three weeks' baseline period):

- Control group (T0): did not receive any reminders
- Continuous treatment group (T1): received continuous reminders over the whole nine weeks

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[10] Unique means that no other household is registered with the same address in the programme.

[11] In apartment buildings/multiple dwelling units, the bags are usually collected in a shared space and then taken outside on the street by the caretaker or doorman of the building/unit.

- Interrupted treatment group (T2): received reminders only for the first three weeks

- Restarted treatment group (T3): received reminders for the first three weeks and for the last three weeks, with a three weeks' pause in between

The following table (Table 3.1) provides an overview of our design. Given that the control group did not receive any reminders, reminders were sent to 1,044 (348*3) households in total. It should be noted that there was a week of public holiday after the first three weeks of the intervention period, i.e. week 7, where no recycling service was in place and thus no reminders were sent and no data could be collected. The same was the case for the Monday of week 12, where again no recycling service was in place due to a holiday and thus no reminders were sent and no data could be collected for that day.

Table 3.1: Study design overview

| | Baseline | | | Intervention | | | | | | | | |
| | Period 1 | | | Period 2 | | | Period 3 | | | Period 4 | | |
	w1	w2	w3	w4	w5	w6	w8	w9	w10	w11	w12	w13
Control (T0)												
Continuous (T1)				x	x	x	x	x	x	x	x	x
Interrupted (T2)				x	x	x						
Restarted (T3)				x	x	x				x	x	x

Notes: The table provides an overview of the study design, including three weeks of baseline period followed by nine weeks of intervention period. Week 7 was skipped due to a public holiday, as was the Monday of week 12.

The design implies that all treatment groups received a similar treatment during the first three weeks of the intervention period, period 2. In the subsequent three weeks, period 3, only the continuous treatment group (T1) continued to receive reminders, while both T2 and T3 were interrupted and thus not distinguishable from each other up to this point. Only during the last three weeks, period 4, the restarted treatment group (T3) received reminders again while the interrupted treatment group (T2) did not, so that all three treatment groups differed.

The treatment variation based on blocks of three weeks was chosen given the irregular nature of households' recycling behaviour. From pre-covid data (until February 2020) from the municipality we know that only few households recycle regularly every week (or even more than once a week, if the bags are collected twice). Most households recycle irregularly, and rather every second or third week, on average. Some households that are registered do not recycle at all. We do not have any data on households' recycling behaviour afterwards, as the municipality had to stop all measurements due to the pandemic. However, during a one-week pilot that we did in the beginning of May 2021, only 13% of all enrolled households recycled in that week, confirming the irregularity in recycling

behaviour from pre-covid times.[12]

The treatment message of the sms reminder contained a friendly greeting from the municipality and a simple reminder for people to put their recycling bags outside on that day. We also gave people a number they could call in case of doubts and referred to the social media channels of the municipality. The formulation of the reminder message was chosen carefully based on joint discussions with the municipality. All households that received a reminder received the same treatment message.[13] We present the message here in English, while the original message was formulated in Spanish:

IT'S RECYCLING DAY: The Municipality of Miraflores reminds you that the recycling truck will pass by your house today. Don't forget to take out your recycling material! Please find more information on the municipality's social media or by contacting us at "number".[14]

3.2.3. Experimental procedure and data collection

The district of Miraflores is distributed into 14 zones, based on which the recycling programme is organized. Recycling bags are collected on Mondays to Fridays in the mornings and afternoons, and on Saturdays in the mornings. There are always two recycling trucks operating in two different zones at the same time. In some zones (six out of 14), the bags are collected once a week; in the other zones (eight out of 14), they are collected twice a week.

To keep track of households' recycling behaviour, we accompanied the recycling trucks that are responsible for collecting the recycling bags on their daily routes over a total time period of 12 weeks (13 weeks, but with a one week's pause in between, as illustrated above), from mid-June to mid-September 2021. We employed four people to do the measurement of households' recycling behaviour in total – two people accompanied the trucks in the mornings, two other people in the afternoons. The four people (enumerators) followed the recycling trucks by bike. The recording of households' recycling behaviour was done through audio recordings via headsets.[15] Enumerators were instructed to record the fol-

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[12] This percentage is based on the number of addresses that recycled as a fraction of all enrolled addresses.

[13] Research has shown that the content variation in reminder messages often has little or no effect (see for example Altmann and Traxler, 2014, Apesteguia et al., 2013, Larochelle et al., 2019 or Wallander et al., 2017). Based on a review of existing studies that vary the content of reminders, Gravert (2022) therefore concludes that using pure reminder messages directed at the action of interest is often the most effective, and also the most cost-effective approach, especially from a policy perspective.

[14] The word *"number"* stands for the official contact number of the municipality, which we are not disclosing in this paper.

[15] Initially, our people were supposed to sit in the trucks together with the recyclers and record the data directly in their notebooks. Yet, due to covid, being in the recycling trucks was not possible anymore given the risk of contagion, so that we had to revert to this rather unconventional way of data collection.

lowing details: street name, house number, house type (single family house or apartment building/other multiple dwelling unit), and number of bags. The audio recordings were transcribed to an excel sheet afterwards. Regular quality checks were applied to both data collection in the field and transcriptions of respective audio recordings. All enumerators were provided a cyclists insurance for the period of data collection and were experienced in riding a bicycle. They were further instructed to wear a helmet as well as face masks at all times (which were provided to them).

The reminders were sent to households in the early mornings of the collection days via sms through a Peruvian sms provider system. This was adjusted based on the different zones and respective collection days (so that, for example, households from a zone where the bags are collected on Wednesdays received the reminders on Wednesdays in the early morning). For better comparability, we sent reminders to all households only once a week, regardless of whether the bags are collected once or twice a week in the respective zones. In zones with two collection days per week, the reminders were sent in the morning of the first collection day of the week.

3.2.4. Hypotheses

We pre-registered the following hypotheses for our study:

Hypothesis 3.1 (H3.1: Initial reminder effect) *We expect households in all three treatment groups to recycle more during the first three weeks of our intervention period (weeks 4-6), where all treated households receive a weekly reminder, than households in the control group, on average.*

The motivation for this hypothesis is that the reminders will address people's limited attention about the recycling programme, which we expect to result in increased recycling activity.

Hypothesis 3.2 (H3.2: Continuous reminder effect) *We expect households in the continuous treatment group to recycle more during our whole intervention period (weeks 8-10 and 11-13) than households in the control group, on average. However, we expect this effect to decrease over time, so that it will be smaller in weeks 11-13.*

The motivation here is again that the reminders will address people's limited attention about the recycling programme, which will result in higher recycling activity throughout the whole intervention period. However, we expect that the reminder effect will decrease over time, as the attention that people place on the reminders will diminish after a while as people get used to receiving the messages.

Hypothesis 3.3 (H3.3: Interrupted treatment / persistence effect) *We   expect that the initial reminder effect will persist for a while, so that households in the interrupted treatment group will still recycle more in the subsequent three weeks than households in the control group (weeks 8-10) albeit less so, though that this effect will fade away in the course of the remaining three weeks (weeks 11-13) when the reminders are not reinforced.*

The motivation for this hypothesis is that the initial reminders will induce some form of habit formation in recycling behaviour, though that this effect will fade away over time when the reminders are not reinforced.

Hypothesis 3.4 (H3.4: Restarted reminder effect) *As for the interrupted treatment group, we expect that the initial reminder effect will persist for a while, so that households in the restarted treatment group will still recycle more in the subsequent three weeks than households in the control group (weeks 8-10) albeit less so. We then expect recycling behaviour to increase again during the last three weeks (weeks 11-13) when the reminders are sent again.*

The motivation for this hypothesis is that the initial reminders will induce some form of habit formation in recycling behaviour as in H3, and that the reinforcement of the reminders in the last three weeks will prevent that the reminder effect diminishes over time.

Hypothesis 3.5 (H3.5: Continuity vs. restart effect) *We expect households in the restarted treatment group to recycle more during the last three weeks (weeks 11-13) than households in the continuous treatment group.*

The motivation for this hypothesis is that the restarted reminder will capture people's attention better than the continuous reminder so that it will be more effective in the long run.

The following table (Table 3.2) summarizes the hypotheses for the different blocks of three weeks.

Table 3.2: Hypotheses overview

	Intervention period		
	week 4-6	week 8-10	week 11-13
Continuous (T1) vs. control (T0)	T1>T0 (H3.1)	T1>T0 (H3.2)	T1>=T0 (H3.2)
Interrupted (T2) vs. control (T0)	T2>T0 (H3.1)	T2>=T0 (H3.3)	T2=T0 (H3.3)
Restarted (T3) vs. control (T0)	T3>T0 (H3.1)	T3>=T0 (H3.4)	T3>T0 (H3.4)
Continuous (T1) vs. restarted (T3)			T1<T3 (H3.5)

3.3. Results

3.3.1. Data

Table 3.A.1 in the Appendix shows that household characteristics in terms of zone, sign-up year and house type are balanced across the different treatment groups, confirming that the randomization was successful. Table 3.A.5 provides a detailed overview of these characteristics by treatment groups. Zones range from 1 to 14, sign-up years from 2015 to 2021. We further distinguish between single family houses, apartment buildings or other multiple dwelling units, and unknown house types. Table 3.A.3 shows that households' baseline recycling behaviour in terms of whether households recycled at all during the baseline period (yes vs. no), the number of bags recycled per household and the frequency with which households recycled (from never up to three times) is balanced across treatments as well.

Since sign-up years go back to 2015, we decided to focus in our analysis not only on the full sample, but also on households with a recent sign-up year (2019 or later) separately, mainly for two reasons: first, people who signed up to the programme many years ago might have moved away by now and thus not be able to participate in the recycling programme anymore; second, for our intervention, it is crucial that households actually receive the sms reminders on their cell phones, and we expect that more of those recently registered phone numbers are still in use. We therefore provide balance tables for those households with a recent sign-up year separately in the Appendix as well (Tables 3.A.2 and 3.A.4) and can confirm that both household characteristics and households' baseline recycling activity are balanced across treatments in this sub-group also.

3.3.2. General reminder effect

To test whether there is a general reminder effect on households' recycling behaviour as hypothesized in H3.1, we start by looking at all treatment groups together compared to the control group during the first three weeks of the intervention period (week 4-6), where all households in the treatment groups received a reminder. Our main dependent variable of interest is the binary variable of whether a household recycled at least once during this first period of the intervention period (yes vs. no). We can see in Figure 3.1a that the percentage of households in the treatment groups that recycled at least once during the three weeks increases from 17.72% in the baseline period to 19.73% in the first three weeks of the intervention period, representing an 11% increase. In contrast, in the control group, the percentage decreases by roughly the same amount, from 18.10% in the baseline period to 16.38% in the intervention period. Comparing the percentages in period 2 between the two groups, we find that the recycling rate is 21% higher in the treatment groups than in the control group.

If we zoom in on those households with a recent sign-up year ($>= 2019$) in Figure 3.1b, we can see that the recycling rate is generally higher and increases even more in the treatments groups, from 19.50% in the baseline period to 22.00% in the first three weeks of the intervention period, representing an increase of 13%, whereas in the control group, the recycling rate drops by 15%, from 19.00% to 16.50%. In this case, the recycling rate during period 2 is 33% higher in the treatment groups than in the control group.

Figure 3.1: Percentages of households that recycled at least once during the baseline period (week 1-3) and during the first three weeks of the intervention period (week 4-6), comparing all treatment groups together with the control group

(a) Full sample

(b) Recent sign-up year
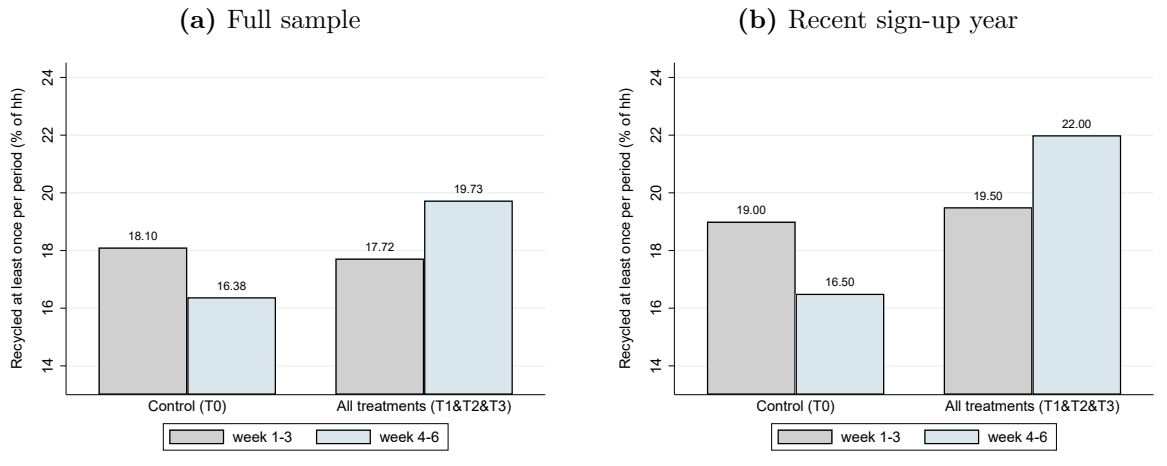
Notes: The figure shows the percentages of households that recycled at least once per period in the control group and in the aggregated treatment groups. Sub-figure 3.1a includes the full sample; N per group = 348, the treatment groups together add up to N = 1,044. Sub-figure 3.1b focuses on households with a recent sign-up year ($>= 2019$); N in the control group (T0) = 200, the treatment groups together add up to N = 641.

To confirm the significance of the difference in households' recycling activity in period 2 between all treated households and the control group, we run the following OLS regression

$$Y_i = \beta_0 + \beta_1 Reminder_i + \beta_2 X_i + u_i \tag{3.1}$$

where Y_i is the binary dependent variable of whether a household recycled at least once in period 2, $Reminder_i$ is a dummy variable that equals 1 if the household received a reminder, X_i is a vector of control variables and u_i is the error term. The control variables we add include controls for households' baseline recycling activity (i.e. whether a household recycled at least once during the baseline period) as well as for the house type (adding a dummy for whether it is a single family house in contrast to multiple dwelling unit or unknown house type) and for the enumerator.

Table 3.3 reports the results of estimating Equation (3.1) and confirms that the differences in recycling rates in period 2 between the treatment groups and the control group are statistically significant. For the full sample, we can see in column (1) that the reminder

treatment increases the percentage of households that recycled at least once during the three weeks by 3.7 percentage points. When focusing on households that signed up to the programme more recently in column (2), it increases the percentage by 5.3 percentage points. Since our dependent variable is binary, we provide a robustness check with probit regressions in the Appendix (Table 3.B.1). We can thus confirm our hypothesis H3.1 that receiving weekly reminders has a significant positive effect on households' recycling behaviour, which is stronger when focusing on households with a more recent sign-up year.

Table 3.3: OLS: average treatment effects on whether a household recycled at least once in period 2, comparing all treatment groups together with the control group

	Period 2	
	Full sample (1)	Recent sign-up year (2)
All treatments (T1&T2&T3)	0.037*	0.053**
	(0.020)	(0.027)
Constant	0.058***	0.034
	(0.022)	(0.028)
Adjusted R^2	0.332	0.331
Observations	1392	841

Notes: OLS regressions were used to estimate treatment effects on households' binary recycling behaviour during the first three weeks of the intervention period (i.e. whether a household recycled at least once during week 4-6 (= period 2)). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year ($>=$ 2019). All regressions include a control for households' baseline recycling activity (never vs. at least once) as well as controls for the house type and the enumerators. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

We show in section 3.B.1.2 in the Appendix that receiving reminders during the first three weeks of the intervention period further increased the weekly recycling behaviour of households (i.e. whether a household recycled at least once a week), which is mildly significant compared to the control group when focusing on those households with a recent sign-up year. It also seems in Figure 3.B.1 that households need some time to get organized to recycle as the reminder effect mainly kicks in from week five onward. In section 3.B.1.3 we can see that the frequency with which households recycled in period 2 (from never up to three times) increased in all treatment groups together compared to baseline levels as well (a t-test confirms that the increase in the recycling frequency from period 1 to period 2 is statistically significant), while here the differences compared to the control group in period 2 are not statistically significant. In section 3.B.1.4 we show that there is no significant effect on the number of bags recycled per household in period 2 in the treatment groups compared to the control group either, while also here a t-test confirms that the number of bags recycled per household in the treatment groups increased significantly compared to baseline levels.

3.3.3. Continuous vs. interrupted reminder effect

In the next step, we turn to the subsequent three weeks of the intervention period, week 8-10, to contrast the effects of continuous vs. interrupted reminders on households' recycling activity. We pool T2 and T3 together for this analysis, since both treatment groups did not continue to receive a reminder during this period. Even though T3 becomes the restarted treatment in week 11-13, up to this point it is not distinguishable from the interrupted treatment T2.

When looking at the recycling rates during period 3 in Figure 3.2, we can see that the percentage of households that recycled at least once during week 8-10 is highest in the continuous treatment group, and that the effect of the continuous treatment is especially strong when focusing on households with a recent sign-up year in Figure 3.2b. Compared to levels in the control group in period 3, we find that for the full sample 20% more (20.98% vs. 17.53%) and for those with a recent sign-up year 44% more (25.12% vs. 17.50%) households recycled at least once in the continuous treatment group than in the control group.

Looking at the interrupted treatment groups (T2 and T3), which only received reminders during the first three weeks of the intervention period, we can see in Figure 3.2 that the percentage of households that recycled at least once during period 3 lies above levels in the control group as well. Compared to baseline levels, the recycling rate is still higher in the interrupted treatment groups in period 3, whereas the recycling rate is slightly lower than during the baseline period in the control group. This difference is particularly visible when focusing on households with a more recent sign-up year in Figure 3.2b.

To confirm the significance of the differences in households' recycling activity between the different treatment groups and the control group in period 3, we run the following OLS regression

$$Y_i = \text{\ss}_0 + \text{\ss}_1 Continuous_i + \text{\ss}_2 Interrupted_i + \text{\ss}_3 X_i + u_i \tag{3.2}$$

where Y_i is the binary dependent variable of whether a household recycled at least once in period 3, $Continuous_i$ is a dummy variable that equals 1 if the household continued to receive a reminder, $Interrupted_i$ is a dummy variable that equals 1 if the household did not continue to receive a reminder (i.e. if the intervention was interrupted), X_i is a vector of control variables and u_i is the error term. Again, the control variables we add include controls for households' baseline recycling activity (i.e. whether a household recycled at least once during the baseline period) as well as for the house type (adding a dummy for whether it is a single family house in contrast to multiple dwelling unit or unknown house type) and for the enumerator.

Figure 3.2: Percentages of households that recycled at least once during the baseline period (week 1-3) and during week 8-10 of the intervention period, comparing the continuous and interrupted treatment groups with the control group

(a) Full sample **(b)** Recent sign-up year
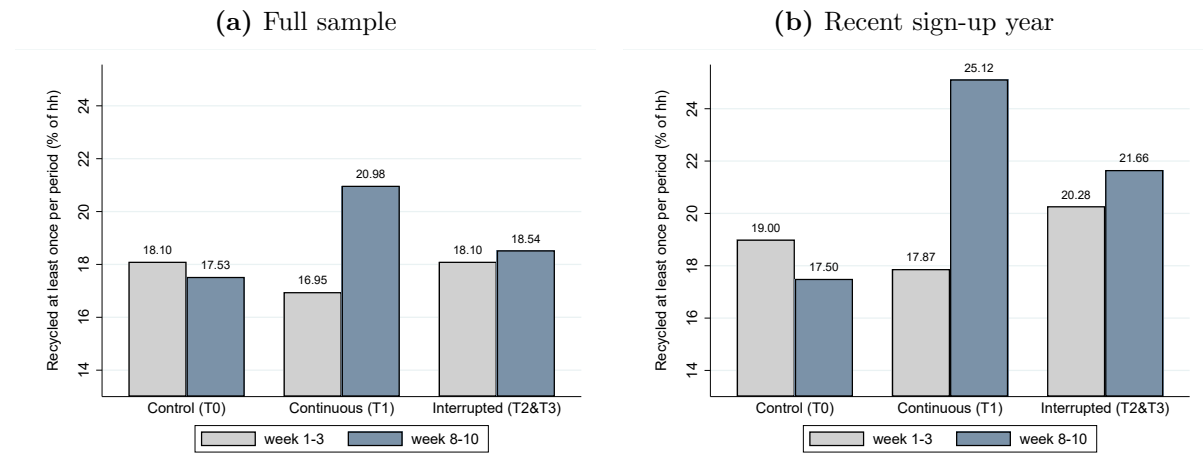
Notes: The figure shows the percentages of households that recycled at least once per period in the control group and in the continuous and interrupted treatment groups. Sub-figure 3.2a includes the full sample; N per group = 348 (hence N = 696 for both interrupted groups together). Sub-figure 3.2b focuses on households with a recent sign-up year (>= 2019); N(T0) = 200, N(T1) = 207, N(T2&T3) = 434. No bags were collected in week 7 due to a public holiday.

Table 3.4 reports the results of estimating Equation (3.2). We can see that the continuous reminder treatment significantly increases the percentage of households that recycled at least once in period 3, and that the effects are especially strong for those households that signed up to the programme more recently. For the full sample, column (1) shows that sending reminders continuously significantly increases the percentage of households that recycled in period 3 by 4.4 percentage points, while we can see in column (2) that for households with a more recent sign-up year the effect size doubles and equals an increase of 8.8 percentage points. In the case of the interrupted treatment groups, we cannot confirm that the differences in recycling rates in period 3 compared to the control group are statistically significant, even though we can note that the coefficients are positive both for the full sample in column (1) and even more so for households with a recent sign-up year in column (2). Again, we provide a robustness check for our analysis with probit regressions in the Appendix (Table 3.B.7).

We can further note in Figure 3.2 that compared to baseline levels, the percentage of households that recycled at least once in the continuous treatment group is with 20.98% in period 3 compared to 16.95% in period 1 24% higher when looking at the full sample, and with 25.12% in period 3 compared to 17.87% in period 1 42% higher when focusing on households with a recent sign-up year. Thus, compared to the initial reminder effect that we found among all treated households in period 2, sending reminders continuously over a longer time period had even stronger effects on households' recycling activity, which is also confirmed by the increase in significance and effect sizes in the regressions in Table 3.4.

Table 3.4: OLS: average treatment effects on whether a household recycled at least once in period 3, comparing the continuous and interrupted treatment groups with the control group

	Period 3	
	Full sample (1)	Recent sign-up year (2)
Continuous (T1)	0.044*	0.088***
	(0.024)	(0.032)
Interrupted treatments (T2&T3)	0.014	0.037
	(0.021)	(0.027)
Constant	0.065***	0.027
	(0.022)	(0.027)
Adjusted R^2	0.331	0.389
Observations	1392	841
T1 vs. T2&T3	p=0.158	p=0.061

Notes: OLS regressions were used to estimate treatment effects on households' binary recycling behaviour during week 8-10 of the intervention period (i.e. whether a household recycled at least once during period 3). Columns (1) includes the full sample, column (2) focuses on households with a recent sign-up year ($>= 2019$). All regressions include a control for households' baseline recycling activity (never vs. at least once) as well as controls for the house type and the enumerators. No bags were collected in week 7 due to a public holiday. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

We show in sections 3.B.2.2 and 3.B.2.3 in the Appendix that sending reminders continuously further significantly increased households' weekly recycling activity as well as the frequency with which households recycled during period 3, respectively, while here again the effects are stronger for those households that signed up to the programme more recently. In section 3.B.2.4 we can see that there is no statistically significant effect on the number of bags recycled per household in period 3 (the number of bags recycled per household increases more strongly in the continuous treatment group than in the control group and significantly compared to baseline levels, yet the differences to the control group are not statistically significant).

In the case of the interrupted treatment groups, we can see in sections 3.B.2.2 and 3.B.2.3 in the Appendix that households' weekly recycling activity as well as the frequency with which households recycled during period 3, respectively, remains higher in the interrupted treatment groups than in the control group as well, which is mildly significant when focusing on households with a more recent sign-up year. As for the continuous treatment group, we find no evidence for a significantly higher number of bags recycled per household in period 3 in the interrupted treatment groups compared to the control group (see section 3.B.2.4).

When we compare the coefficients of the continuous treatment group and the interrupted treatment groups in Table 3.4, we can see that the continuous treatment had a stronger effect on households' recycling activity in period 3 than the interrupted treatments did, and that this difference is statistically significant for households with a more

recent sign-up year. We make the same observation when comparing coefficients of the effects of the different treatments on households' weekly recycling activity in period 3 (see Tables 3.B.8 and 3.B.9).

In sum, we can thus confirm hypothesis H3.2 that there is a continuous reminder effect on households' recycling behaviour, which is stronger for households that signed up to the programme more recently. We also find some evidence for persistence effects in the interrupted treatment groups in the case of households with a more recent sign-up year as hypothesized in H3.3, while here the evidence is less clear and not all differences compared to the control group are statistically significant. In the sub-group analysis of households with a recent sign-up year, the continuous treatment performs significantly better than the interrupted treatment groups. Thus, our data suggest that sending reminders continuously is most effective to encourage recycling behaviour of households over time.

3.3.4. Continuous vs. interrupted vs. restarted reminder effect

We now turn to the final three weeks of our intervention period, week 11-13, to investigate the effects of continuous vs. interrupted and restarted reminders on households' recycling behaviour. First of all, it should be noted that there was a public holiday on the Monday of week 12, on which no recycling service was in place and thus no data could be collected. Hence, one day of data is missing in week 12. Moreover, from discussions with the municipality we know that many people use this public holiday to travel, which might have led to even lower recycling rates during the whole week. Figure 3.B.7 in the Appendix confirms that there is a drop in the percentage of households that recycled in week 12, which explains the lower recycling rates during period 4, on average.

As we can see in Figure 3.3, the percentage of households that recycled at least once during period 4 is considerably lower than during the previous periods and falls below levels from the baseline period in the case of the control group. In case of the continuous treatment group, the recycling rate reverts back to baseline levels yet remains still higher than in the control group.

To confirm the significance of the differences in households' recycling activity between the different treatment groups and the control group in period 4, we run the following OLS regression

$$Y_i = \beta_0 + \beta_1 Continuous_i + \beta_2 Interrupted_i + \beta_3 Restarted_i + \beta_4 X_i + u_i \qquad (3.3)$$

where Y_i is the binary dependent variable of whether a household recycled at least once in period 4, $Continuous_i$ is a dummy variable that equals 1 if the household continued

to receive a reminder, $Interrupted_i$ is a dummy variable that equals 1 if the household did not continue to receive a reminder (i.e. if the intervention was still interrupted), and $Restarted_i$ is a dummy variable that equals 1 if the household was in the interrupted group in period 3 and does now receive a reminder again in period 4 (i.e. if the intervention is restarted). X_i is again a vector of control variables and u_i is the error term, while again we add controls for households' baseline recycling activity (i.e. whether a household recycled at least once during the baseline period) as well as for the house type (adding a dummy for whether it is a single family house in contrast to multiple dwelling unit or unknown house type) and for the enumerator.

Figure 3.3: Percentages of households that recycled at least once during the baseline period (week 1-3) and during week 11-13 of the intervention period, comparing the continuous, interrupted and restarted treatment group with the control group

(a) Full sample

(b) Recent sign-up year
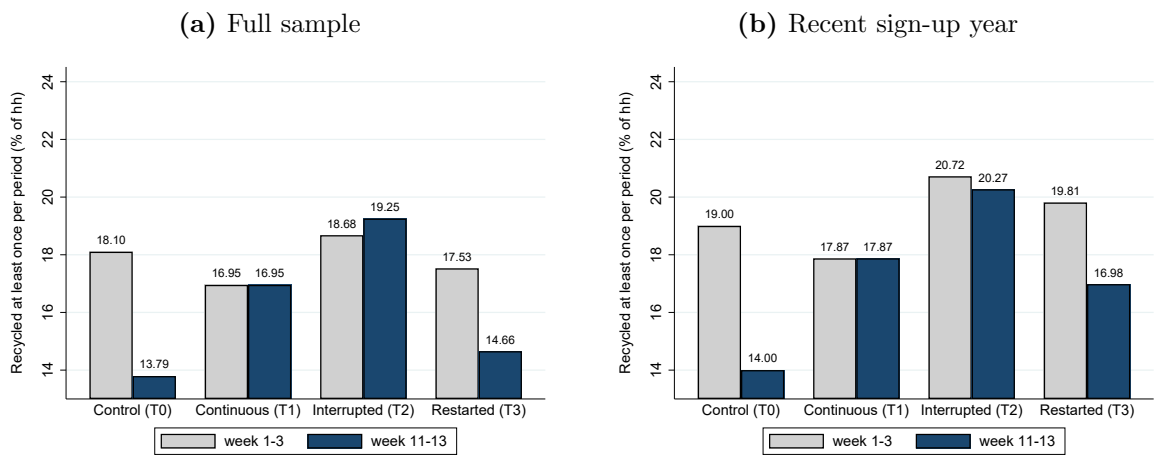
Notes: The figure shows the percentages of households that recycled at least once per period in the control group and in the different treatment groups. Sub-figure 3.3a includes the full sample; N per group = 348. Sub-figure 3.3b focuses on households with a recent sign-up year ($>= 2019$); N(T0) = 200, N(T1) = 207, N(T2) = 222, N(T3) = 212. No bags were collected on the Monday of week 12 due to a public holiday.

Table 3.5 reports the results of estimating Equation (3.3) and shows that the recycling rate in the continuous treatment group is still mildly significantly higher than in the control group in period 4, with 4.0 percentage points for the full sample in column (1) and 4.6 percentage points for households with a recent sign-up year in column (2), although the significance only holds in case of the full sample. A robustness check with probit regressions confirms the significance of the continuous treatment for both groups (Table 3.B.13).

It stands out in Figure 3.3 that the recycling rate in the interrupted treatment group, which is the only treatment group that did not receive any reminders in period 4, remains rather high compared to the other groups, which is unexpected and surprising. Yet, we can note that it reverts back to levels similar to the baseline period, as it is the case in the continuous treatment group, while baseline levels were already higher in the case of the

interrupted treatment group. When we compare the coefficients of the continuous and the interrupted treatment in Table 3.5 and Table 3.B.13, we can see that they are not significantly different from each other. In both groups, households recycle significantly more during period 4 than households in the control group, while the coefficients are larger for the interrupted treatment group.

Table 3.5: OLS: average treatment effects on whether a household recycled at least once in period 4, comparing the continuous, interrupted and restarted treatment group with the control group

| | Period 4 | |
	Full sample (1)	Recent sign-up year (2)
Continuous (T1)	0.040^*	0.046
	(0.023)	(0.031)
Interrupted (T2)	0.053^{**}	0.056^*
	(0.023)	(0.030)
Restarted (T3)	0.013	0.025
	(0.023)	(0.031)
Constant	0.031	0.018
	(0.021)	(0.026)
Adjusted R^2	0.312	0.320
Observations	1392	841
T1 vs. T2	p=0.561	p=0.743
T1 vs. T3	p=0.251	p=0.485
T2 vs. T3	p=0.084	p=0.299

Notes: OLS regressions were used to estimate treatment effects on households' binary recycling behaviour during week 11-13 of the intervention period (i.e. whether a household recycled at least once during period 4). Columns (1) includes the full sample, column (2) focuses on households with a recent sign-up year ($>=$ 2019). All regressions include a control for households' baseline recycling activity (never vs. at least once) as well as controls for the house type and the enumerators. No bags were collected on the Monday of week 12 due to a public holiday. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

As in the other treatment groups, the recycling rate in the restarted treatment group goes down in period 4 due to data issues, while it remains higher than in the control group (more strongly so in the case of households with recent sign-up years). Yet, we cannot confirm that the percentages of households that recycled during the last three weeks of the intervention period are significantly different in the restarted treatment group compared to the control group (see Table 3.5). Comparing the coefficients of the different treatments, we can see that it does not perform worse than the other treatment though, with an exception of a weak significance when comparing the interrupted and restarted treatment for the full sample.

We make similar observations regarding the effectiveness of the three different treatments during period 4 when looking at households' weekly recycling behaviour (see section 3.B.3.2) and the frequency with which households recycled during the last three weeks of the intervention period (see section 3.B.3.3). Again, none of the treatments significantly

increased the number of bags recycled per household per period compared to the control group (see section 3.B.3.4).

Based on our data, we thus cannot confirm our hypothesis H3.4 that there is a positive restart effect on households recycling behaviour in period 4 when the intervention is taken up again, nor can we confirm that households in the restarted treatment group recycle more in period 4 than households in the continuous treatment group, as hypothesized in H3.5. We have to acknowledge though that these findings should be treated with caution as the data quality during period 4 is questionable. With many people being on holidays, a few more or less active households might have had a large impact on the results, and the lower recycling rates might have led to problems of statistical power, on which we elaborate in the discussion.

The continuous reminder effect persists as hypothesised in H3.2, yet weakens compared to period 3 in terms of its significance. We also find evidence for persistence effects in the interrupted treatment in period 4, which did not diminish as hypothesised in H3.3. Yet, again we would like to emphasize that these findings should be treated with caution given the data issues mentioned.

In sum, our evidence is most robust for the effectiveness of continuous reminders, which showed to have a significant positive effect on households' recycling behaviour compared to the control group throughout the whole intervention period.

3.4. Discussion and Conclusion

In this study, we conduct an RCT in cooperation with a local municipality in Lima, Peru, to test whether simple sms reminder messages can increase households' active participation in the municipality's recycling programme. Even though households sign up to the programme voluntarily, many do not recycle regularly, pointing towards a gap between people's intention to recycle and their actual recycling behaviour. Our reminder intervention aims to address this problem by bringing the recycling programme to people's attention on the day when the recycling bags are collected, thereby overcoming the problem of limited attention, which is often an obstacle for people to follow through with good intentions.

Over a total time period of 12 weeks (three weeks of baseline period followed by nine weeks of intervention period), we measure households' weekly recycling behaviour and randomly vary the frequency with which reminders are sent. In particular, we contrast the effects of sending reminders continuously, interrupting the reminder intervention, and first interrupting and then restarting the reminders again on households' recycling activity. Our experiment is organised based on four periods of three weeks each: period 1, which serves as the baseline period; period 2, where all treated households receive a reminder;

period 3, where the continuous treatment group continues to receive a reminder while the two other treatment groups do not, i.e. are interrupted; and period 4, where the continuous treatment group still receives weekly reminders, the interrupted treatment group stays interrupted, and the restarted treatment group (which was interrupted before) receives reminders again.

We first show that reminders are generally effective to encourage households to recycle: the percentage of households that recycle is significantly higher among treated households that received a weekly sms reminder during the first three weeks of the intervention period than among households in the control group that did not receive a reminder. We find that this effect is stronger when focusing on households that signed up to the programme more recently, where the share of households that recycle in period 2 increases by 5.3 percentage points compared to levels in the control group.

We further provide evidence for strong continuous reminder effects on households' recycling behaviour. Throughout the whole intervention period, households that continued to receive a weekly sms reminder are significantly more likely to recycle than households in the control group. We also find that the effect of sending reminders repeatedly increases in the subsequent three weeks compared to the initial reminder effect. Again, we find that the effects are especially strong for households with a more recent sign-up year, among which the percentage of households that recycle increases by 8.8 percentage points in period 3 compared to the control group.

Our data also provides some evidence for persistence effects of reminders, which suggests that sending reminders for a while can induce some form of habit formation in households' recycling behaviour, although our results are less robust in this regard. Our design allows us to directly compare the effectiveness of sending reminders continuously as opposed to persistence effects of reminders after the intervention was terminated. This is rare in the literature, and similar to Allcott and Rogers (2014), while it stands in contrast to most other studies such as Calzolari and Nardotto (2017) and Muller and Habla (2018) that can only derive conclusions about the persistence effects of reminders based on post-intervention data, without being able to compare it to a group that still continues to receive reminders at the same time, and thus adds important new evidence. Up to the last three weeks of our intervention period, our data suggests that sending reminders continuously is most effective to encourage households' recycling behaviour over time.

Data issues during the last three weeks of our intervention period complicate the analysis on a longer comparison of continued vs. interrupted reminders as well as on the effects of restarting the reminder intervention. With our data, we cannot confirm that there are positive restart effects on households' recycling behaviour, yet we have to acknowledge that the validity of this finding should be treated with caution. Due to a public holiday and the related traveling of many people, the recycling rates were generally

much lower among all groups during the final weeks of the intervention period and fell below baseline levels in the control group. This is problematic for two reasons in particular. First, even though our randomization should ensure that this should affect all treatment groups equally, we cannot rule out that by chance some groups might have been more affected than others. Since the percentages of households that recycle are in general rather low, it is possible that a few more or less active households not being present during the final weeks might have biased our findings. It may also be that households that traveled over the holiday are different from those that stayed in terms of their recycling attitudes and responsiveness towards the reminders, which might have aggravated the problem.[16] Second, the lower number of households that recycled during the last weeks might have led to problems of statistical power, so that differences in recycling rates between groups that otherwise might have been significant might now not be significant anymore. Especially the fact that the interrupted treatment group, which was the only treatment that did not receive any reminders during the last three weeks, shows the strongest effect on households' recycling activity in period 4 indicates that the data is not reliable. It is unfortunate that the experimental design element of the restarted treatment falls in this time period, which makes it hard to draw any conclusions about its effectiveness. Future research that investigates potential restart effects of reminders after a prior interruption of the intervention with more robust data would be promising.

The fact that the continuous reminders have a significant and robust effect on households' recycling behaviour throughout the whole intervention period speaks in favour of the theory that limited attention is a main obstacle for households' regular participation in the programme - people need to be reminded every week on the collection day to remember to put the recycling bags outside their house. Moreover, the fact that the effectiveness of repeated reminders increases over time (with the exception of the last three weeks) combined with the evidence we find for persistence effects after the intervention was interrupted (albeit not robust) suggests that habit formation plays a role as well. To successfully participate in the recycling programme, people need to not only remember to put the recycling bags outside their house on the collection day each week, but also to organize their internal waste management in the household in advance, collect and separate the materials and prepare the bags for collection. Developing a routine for these practices makes a regular participation more likely. Our weekly data indicates that households do indeed need a few weeks to get organized as the reminder effect only kicks in with a certain time lag. Our findings thus suggest that attention and habit formation may interact with each other, as for example speculated by Karlan et al. (2016). Moreover, the model and related empirical evidence of Taubinsky (2014) suggest that reminders do

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[16] However, reminder studies that had access to demographic information about their sample found that there were hardly any differences in the effectiveness of reminders among different sub-groups (see e.g. Altmann and Traxler (2014) or Muller and Habla (2018)), so that we do not think that this was likely the main reason for the data issues, even if there were certain differences between households.

not have an additional effect on behaviour when habits to engage in the behaviour are already developed, while without developed habits (specifically after a forced interruption of the behaviour) they do, which the author interprets as evidence for the fact that reminders and habits work as substitutes. It is therefore plausible to assume that the positive effects we find in the continuous treatment are a result of removing the barrier of limited attention combined with the development of recycling habits within households. The interaction of limited attention and habit formation leaves room for future research.

In general, we find that the reminder effects are stronger for households that signed up to the programme more recently (since 2019). One plausible reason for a lower effectiveness among the whole sample (where sign-up years date back to 2015) is that not all phone numbers were valid anymore and some people might have simply never received the reminders. This suggests that we might underestimate the effects of the reminders in our setting, which might be even stronger if we had a sample where we could be sure that all phone numbers were still in use.

Interestingly, we find significant positive effects of sending reminders on the likelihood that households recycle at all on a regular basis (every three weeks or every week, respectively), as well as on the frequency with which households recycle. However, we do not find evidence that reminders significantly increase the number of bags that are recycled per household compared to the control group. During our intervention period, the number of bags recycled per household increases in all treatment groups, and significantly compared to baseline levels, yet the increase is not statistically significant compared to the control group. However, since we do not have any data on the volume of recyclable materials within each bag, this does not necessarily mean that the absolute amount of recycled material did not increase. A screening of bags regarding the amount and quality of the collected material would have been needed to derive conclusions about this, which unfortunately was not feasible in our set-up.

Our paper further adds important new evidence on the effectiveness of reminders in the context of recycling, which has rarely been investigated thus far. Moreover, we extend the focus to Peru and therefore broaden the reminder literature that has so far mainly focused on the Global North. Both additions are of great relevance for both researcher and policy makers. From a policy perspective, our results are good news as we show that a simple, low-cost tool in the form of sending weekly sms reminders can effectively increase households' recycling activity in our context. Given the low implementation costs and the widespread use of cell phones (in our setting and beyond), the intervention can be scaled up easily by policy makers with great prospect of success. Moreover, our data does not reveal any potentially negative effects of sending reminders repeatedly.[17] Based on our evidence, the policy recommendation would thus be to send reminders continuously,

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[17] As for example suggested by Damgaard and Gravert (2018) and Gravert (2022).

which showed the strongest and most robust effects on households' recycling behaviour over time. It may be that after a while, once recycling habits are fully developed, a continuous repetition of weekly reminders might not be needed anymore. It is likely to assume that our study period was too short for households to fully form their recycling habits. Research that investigates recycling behaviour and the importance of reminders in combination with habit formation over a longer time frame would thus be interesting.

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Appendix

3.A. Balance tests and descriptive overview

Table 3.A.1: Balance table: household characteristics

Variable	(1) Control (T0) N	Mean/SE	(2) Continuous (T1) N	Mean/SE	(3) Interrupted (T2) N	Mean/SE	(4) Restarted (T3) N	Mean/SE	(5) Total N	Mean/SE	T-test Difference (1)-(2)	(1)-(3)	(1)-(4)	F-test for joint orthogonality
Zone	348	8.434 (0.226)	348	8.141 (0.235)	348	7.968 (0.232)	348	8.388 (0.222)	1392	8.233 (0.114)	0.293	0.466	0.046	0.911
Sign-up year	348	2018.555 (0.080)	348	2018.638 (0.081)	348	2018.756 (0.079)	348	2018.681 (0.079)	1392	2018.657 (0.040)	-0.083	-0.201*	-0.126	1.109
House type	348	1.761 (0.042)	348	1.770 (0.041)	348	1.807 (0.041)	348	1.770 (0.041)	1392	1.777 (0.021)	-0.009	-0.046	-0.009	0.247
F-test of joint significance (F-stat)											0.409	1.725	0.461	
F-test, number of observations											696	696	696	

Notes: Zones range from 1 to 14, sign-up years from 2015 to 2021. House types are coded 1 for single family houses, 2 for buildings/multiple dwelling units and 3 for unknown cases. The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 3.A.2: Balance table: household characteristics - only recent sign-up years

Variable	(1) Control (T0) N	Mean/SE	(2) Continuous (T1) N	Mean/SE	(3) Interrupted (T2) N	Mean/SE	(4) Restarted (T3) N	Mean/SE	(5) Total N	Mean/SE	T-test Difference (1)-(2)	(1)-(3)	(1)-(4)	F-test for joint orthogonality
Zone	200	8.205 (0.303)	207	8.048 (0.302)	222	8.203 (0.281)	212	8.741 (0.280)	841	8.301 (0.146)	0.157	0.002	-0.536	1.086
Sign-up year	200	2019.710 (0.051)	207	2019.773 (0.050)	222	2019.761 (0.046)	212	2019.745 (0.048)	841	2019.748 (0.024)	-0.063	-0.051	-0.035	0.304
House type	200	2.090 (0.057)	207	2.111 (0.053)	222	2.090 (0.050)	212	2.080 (0.053)	841	2.093 (0.027)	-0.021	-0.000	0.010	0.059
F-test of joint significance (F-stat)											0.375	0.196	0.693	
F-test, number of observations											407	422	412	

Notes: Zones range from 1 to 14, sign-up years from 2019 to 2021. House types are coded 1 for single family houses, 2 for buildings/multiple dwelling units and 3 for unknown cases. The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 3.A.3: Balance table: baseline recycling behaviour

Variable	(1) Control (T0) N	Mean/SE	(2) Continuous (T1) N	Mean/SE	(3) Interrupted (T2) N	Mean/SE	(4) Restarted (T3) N	Mean/SE	(5) Total N	Mean/SE	T-test Difference (1)-(2)	(1)-(3)	(1)-(4)	F-test for joint orthogonality
Recycled at least once	348	0.181 (0.021)	348	0.170 (0.020)	348	0.187 (0.021)	348	0.175 (0.020)	1392	0.178 (0.010)	0.011	-0.006	0.006	0.130
Number of bags	348	0.672 (0.114)	348	0.649 (0.102)	348	0.770 (0.117)	348	0.681 (0.105)	1392	0.693 (0.055)	0.023	-0.098	-0.009	0.233
Frequency	348	0.282 (0.037)	348	0.261 (0.036)	348	0.307 (0.039)	348	0.284 (0.037)	1392	0.284 (0.018)	0.020	-0.026	-0.003	0.259
F-test of joint significance (F-stat)											0.112	0.181	0.102	
F-test, number of observations											696	696	696	

Notes: Balance table for households' baseline recycling behaviour, looking at whether households recycled at least once during the baseline period (yes or no), the number of bags recycled per household, and the frequency with which households recycled (never up to three times). The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 3.A.4: Balance table: baseline recycling behaviour - only recent sign-up years

Variable	N	(1) Control (T0) Mean/SE	N	(2) Continuous (T1) Mean/SE	N	(3) Interrupted (T2) Mean/SE	N	(4) Restarted (T3) Mean/SE	N	(5) Total Mean/SE	(1)-(2)	T-test Difference (1)-(3)	(1)-(4)	F-test for joint orthogonality
Recycled at least once	200	0.190 (0.028)	207	0.179 (0.027)	222	0.207 (0.027)	212	0.198 (0.027)	841	0.194 (0.014)	0.011	-0.017	-0.008	0.199
Number of bags	200	0.665 (0.142)	207	0.686 (0.130)	222	0.905 (0.160)	212	0.863 (0.158)	841	0.784 (0.074)	-0.021	-0.240	-0.198	0.676
Frequency	200	0.305 (0.051)	207	0.266 (0.046)	222	0.342 (0.052)	212	0.330 (0.051)	841	0.312 (0.025)	0.039	-0.037	-0.025	0.462
F-test of joint significance (F-stat)											0.891	0.647	0.557	
F-test, number of observations											407	422	412	

Notes: Balance table for households' baseline recycling behaviour, looking at whether households recycled at least once during the baseline period (yes or no), the number of bags recycled per household, and the frequency with which households recycled (never up to three times), focusing on households with a recent sign-up year only (from 2019 onward). The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table 3.A.5: Overview table: household characteristics by group

		Control (T0)	Continuous (T1)	Interrupted (T2)	Restarted (T3)	Total
Zone	1	29	31	24	21	105
	2	16	19	25	23	83
	3*	21	31	29	19	100
	4*	23	20	25	30	98
	5*	15	12	20	12	59
	6*	14	22	19	16	71
	7	9	9	18	8	44
	8*	13	13	12	10	48
	9	28	25	15	36	104
	10	26	21	25	23	95
	11*	77	58	57	71	263
	12	13	17	15	16	61
	13*	18	25	12	26	81
	14*	46	45	52	37	180
Sign-up year	2015	2	2	4	0	8
	2016	13	12	8	12	45
	2017	117	115	100	109	441
	2018	16	12	14	15	57
	2019	90	82	85	86	343
	2020	78	90	105	94	367
	2021	32	35	32	32	131
House type	Single family house	157	152	140	153	602
	Building/multiple dwelling unit	117	124	135	122	498
	n/a	74	72	73	73	292
	N	348	348	348	348	1392

Notes: A * means that bags are collected on two days per week in the respective zone, without a star means that they are collected once a week.

Table 3.A.6: Overview table: recycling behaviour by group

	w1-3	w4-6	w8-10	w11-13
Recycled at least once per period (%)				
Control (T0)	18.10%	16.38%	17.53%	13.79%
Continuous (T1)	16.95%	20.11%	20.98%	16.95%
Interrupted (T2)	18.68%	20.40%	20.98%	19.25%
Restarted (T3)	17.53%	18.68%	16.09%	14.66%
All	17.82%	18.89%	18.89%	16.16%
Recycled at least once per period (absolute numbers)				
Control (T0)	63	57	61	48
Continuous (T1)	59	70	73	59
Interrupted (T2)	65	71	73	67
Restarted (T3)	61	65	56	51
All	248	263	263	225
Number of bags recycled per household per period				
Control (T0)	0.67	0.81	0.90	0.70
Continuous (T1)	0.65	0.92	0.98	0.76
Interrupted (T2)	0.77	0.96	1.05	0.93
Restarted (T3)	0.68	0.77	0.74	0.74
All	0.69	0.87	0.92	0.78
Total number of bags recycled per group per period				
Control (T0)	234	283	314	244
Continuous (T1)	226	321	340	263
Interrupted (T2)	268	335	367	323
Restarted (T3)	237	269	257	257
All	965	1208	1278	1087

Notes: The table provides a descriptive overview of the percentage of households and the corresponding absolute numbers that recycled at least once per period, as well as the number of bags that were recycled per household per period and the aggregated number of bags recycled per group.

Table 3.A.7: Households that recycled at least once: old vs. recent sign-ups

	Old sign-ups	Recent sign-ups	All
Recycled never	74.77%	68.13%	70.76%
Recycled at least once	25.23%	31.87%	29.24%
N	551	841	1,392

Notes: The table shows the percentages of households that recycled at least once during the whole study period, separated for old and recent sign-ups.

3.B. Additional analyses

3.B.1. General reminder effect

3.B.1.1. Robustness check

Table 3.B.1: Probit: average treatment effects on whether a household recycled at least once in period 2, comparing all treatment groups together with the control group

	Period 2	
	Full sample (1)	Recent sign-up year (2)
All treatments (T1&T2&T3)	0.181*	0.256*
	(0.106)	(0.138)
Constant	-1.521***	-1.659***
	(0.120)	(0.154)
Pseudo R^2	0.284	0.283
Observations	1392	841

Notes: Probit regressions were used to estimate treatment effects on households' binary recycling behaviour during the first three weeks of the intervention period (i.e. whether a household recycled at least once during week 4-6 (= period 2)). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year ($>= 2019$). All regressions include a control for households' baseline recycling activity (never vs. at least once) as well as controls for the house type and the enumerators. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

3.B.1.2. Weekly recycling behaviour

Figure 3.B.1: Percentages of households that recycled at least once per week during the baseline period (week 1-3) and during the first three weeks of the intervention period (week 4-6), comparing all treatment groups together with the control group

(a) Full sample

(b) Recent sign-up year

Notes: The figure shows the percentages of households that recycled at least once per week in the control group and in the aggregated treatment groups. Sub-figure 3.B.1a includes the full sample; N per group = 348, the treatment groups together add up to N = 1,044. Sub-figure 3.B.1b focuses on households with a recent sign-up year (>= 2019); N in the control group (T0) = 200, the treatment groups together add up to N = 641.
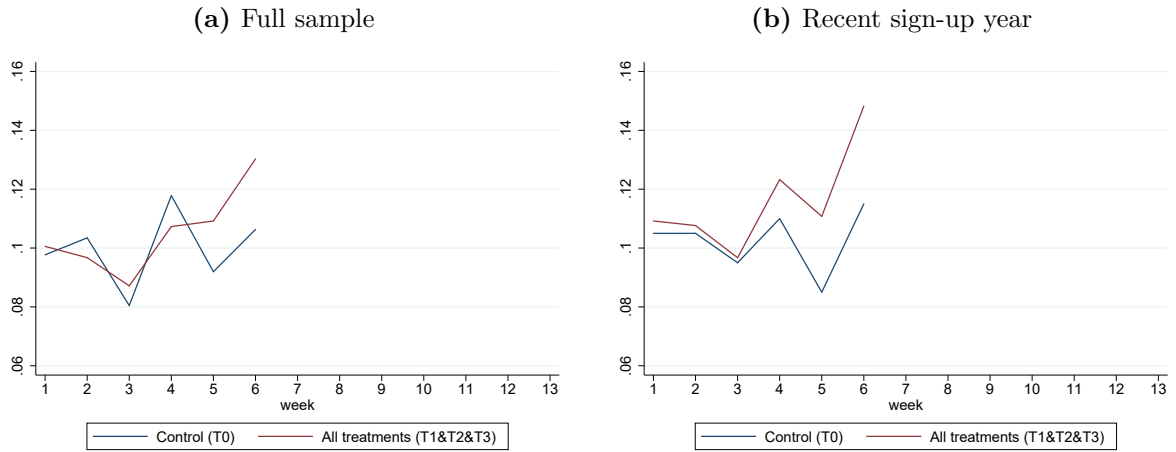
Table 3.B.2: OLS: average treatment effects on whether a household recycled at least once per week in period 2, comparing all treatments together with the control group

	Period 2	
	Full sample (1)	Recent sign-up year (2)
All treatments (T1&T2&T3)	0.010	0.022*
	(0.009)	(0.013)
Constant	0.030***	0.016
	(0.010)	(0.013)
Adjusted R^2	0.329	0.309
Observations	4176	2523

Notes: OLS regressions were used to estimate treatment effects on whether a household recycled at least once per week during the first three weeks of the intervention period (week 4-6 = period 2). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year (>= 2019). All regressions include a control for households' baseline recycling activity (dummy for never, once, twice or three times) as well as controls for the house type and the enumerators. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 3.B.3: Probit: average treatment effects on whether a household recycled at least once per week in period 2, comparing all treatments together with the control group

	Period 2	
	Full sample (1)	Recent sign-up year (2)
All treatments (T1&T2&T3)	0.066	0.151
	(0.072)	(0.094)
Constant	-1.850***	-1.965***
	(0.084)	(0.107)
Pseudo R^2	0.321	0.304
Observations	4176	2523

Notes: Probit regressions were used to estimate treatment effects on whether a household recycled at least once per week during the first three weeks of the intervention period (week 4-6 = period 2). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year (>= 2019). All regressions include a control for households' baseline recycling activity (dummy for never, once, twice or three times) as well as controls for the house type and the enumerators. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.
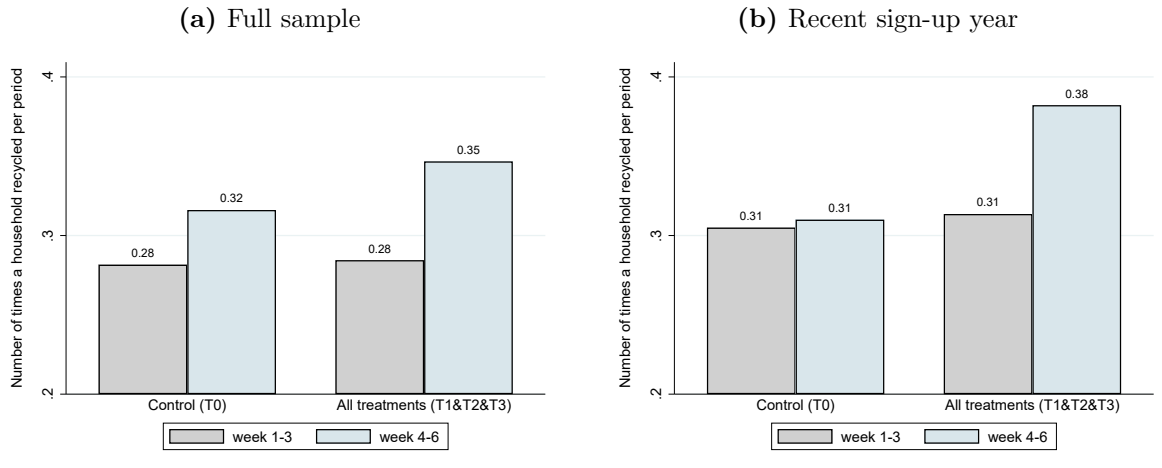
3.B.1.3. Recycling frequency per period

Figure 3.B.2: Number of times a household recycled during the baseline period (week 1-3) and during the first three weeks of the intervention period (week 4-6), comparing all treatment groups together with the control group

(a) Full sample

(b) Recent sign-up year

Notes: The figure shows the average frequency with which a household recycled per period in the control group and in the aggregated treatment groups. Sub-figure 3.B.2a includes the full sample; N per group = 348, the treatment groups together add up to N = 1,044. Sub-figure 3.B.2b focuses on households with a recent sign-up year (>= 2019); N in the control group (T0) = 200, the treatment groups together add up to N = 641. A t-test comparing the frequency with which households recycled during the baseline and intervention period within the treatment groups shows a significant difference (p=0.055) when looking at the full sample, while the difference is not statistically significant in the control group. The increase in the frequency compared to baseline levels in the treatment groups is not statistically significant when focusing on households with a recent sign-up year, which is likely due to lower statistical power given the smaller sample size.

Table 3.B.4: OLS: average treatment effects on how often a household recycled in period 2, comparing all treatment groups together with the control group

	Period 2	
	Full sample (1)	Recent sign-up year (2)
All treatments (T1&T2&T3)	0.030	0.065
	(0.035)	(0.048)
Constant	0.088**	0.051
	(0.038)	(0.050)
Adjusted R^2	0.481	0.459
Observations	1392	841

Notes: OLS regressions were used to estimate treatment effects on households' recycling frequency (never up to three times) during the first three weeks of the intervention period (week 4-6 = period 2). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year (>= 2019). All regressions include a control for households' baseline recycling activity (never up to three times) as well as controls for the house type and the enumerators. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 3.B.5: Ordered probit: average treatment effects on how often a household recycled in period 2, comparing all treatment groups together with the control group

	Period 2	
	Full sample (1)	Recent sign-up year (2)
All treatments (T1&T2&T3)	0.100	0.184
	(0.099)	(0.128)
cut1	1.439***	1.557***
	(0.112)	(0.143)
cut2	2.015***	2.155***
	(0.120)	(0.154)
cut3	2.816***	2.971***
	(0.142)	(0.180)
Pseudo R^2	0.259	0.247
Observations	1392	841

Notes: Ordered probit regressions were used to estimate treatment effects on households' recycling frequency (never up to three times) during the first three weeks of the intervention period (week 4-6 = period 2). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year (>= 2019). All regressions include a control for households' baseline recycling activity (never up to three times) as well as controls for the house type and the enumerators. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.
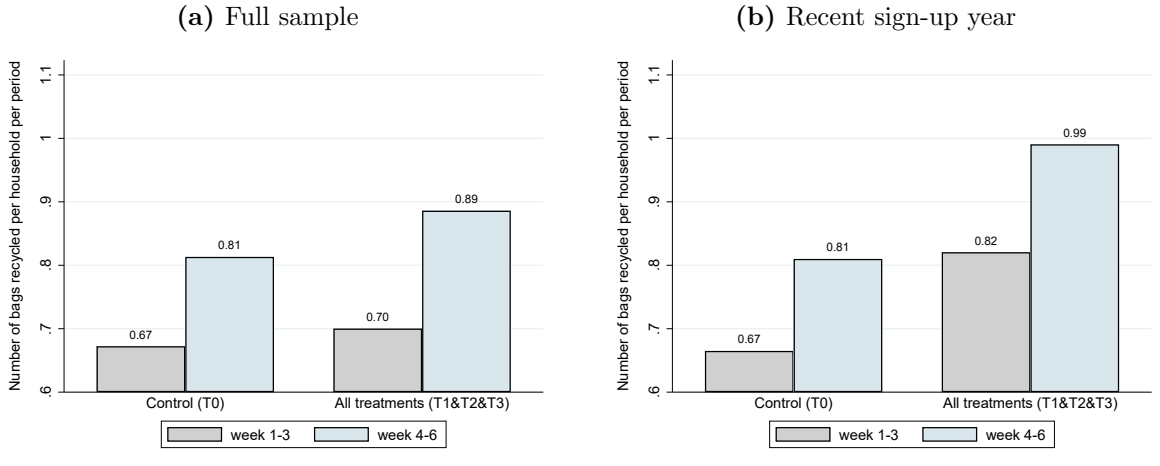
3.B.1.4. Number of bags recycled per period

Figure 3.B.3: Number of bags recycled per household during the baseline period (week 1-3) and during the first three weeks of the intervention period (week 4-6), comparing all treatment groups together with the control group

(a) Full sample   **(b)** Recent sign-up year

Notes: The figure shows the average number of bags that were recycled per household per period in the control group and in the aggregated treatment groups. Sub-figure 3.B.3a includes the full sample; N per group = 348, the treatment groups together add up to N = 1,044. Sub-figure 3.B.3b focuses on households with a recent sign-up year (>= 2019); N in the control group (T0) = 200, the treatment groups together add up to N = 641. A t-test comparing the number of bags recycled per household during the baseline and intervention period within the treatment groups shows a significant difference (p=0.059) when looking at the full sample, while the difference is not statistically significant in the control group. The increase in the number of bags compared to baseline levels in the treatment groups is not statistically significant when focusing on households with a recent sign-up year, which is likely due to lower statistical power given the smaller sample size.

Table 3.B.6: OLS: average treatment effects on the number of bags recycled per household in period 2, comparing all treatment groups together with the control group

	Period 2	
	Full sample (1)	Recent sign-up year (2)
All treatments (T1&T2&T3)	0.049	0.042
	(0.116)	(0.152)
Constant	0.289**	0.224
	(0.126)	(0.157)
Adjusted R^2	0.470	0.479
Observations	1392	841

Notes: OLS regressions were used to estimate treatment effects on the number of bags recycled per household during the first three weeks of the intervention period (week 4-6 = period 2). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year (>= 2019). All regressions include a control for households' baseline recycling activity (mean of recycled bags) as well as controls for the house type and the enumerators. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

3.B.2. Continuous vs. interrupted reminder effect
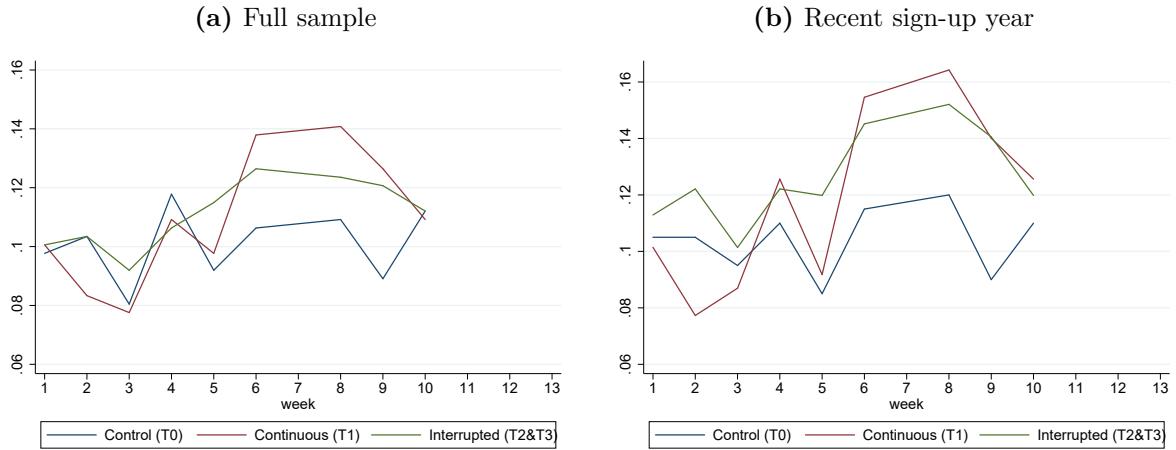
3.B.2.1. Robustness check

Table 3.B.7: Probit: average treatment effects on whether a household recycled at least once in period 3, comparing the continuous and interrupted treatment groups with the control group

	Period 3	
	Full sample (1)	Recent sign-up year (2)
Continuous (T1)	0.230*	0.482***
	(0.128)	(0.172)
Interrupted treatments (T2&T3)	0.087	0.238
	(0.113)	(0.153)
Constant	-1.544***	-1.791***
	(0.122)	(0.164)
Pseudo R^2	0.291	0.342
Observations	1392	841
T1 vs. T2&T3	p=0.192	p=0.078

Notes: Probit regressions were used to estimate treatment effects on households' binary recycling behaviour during week 8-10 of the intervention period (i.e. whether a household recycled at least once during period 3). Columns (1) includes the full sample, column (2) focuses on households with a recent sign-up year (>= 2019). All regressions include a control for households' baseline recycling activity (never vs. at least once) as well as controls for the house type and the enumerators. No bags were collected in week 7 due to a public holiday. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

3.B.2.2. Weekly recycling behaviour

Figure 3.B.4: Percentages of households that recycled at least once per week during the baseline period (week 1-3) and during week 8-10 of the intervention period, comparing the continuous and interrupted treatment groups with the control group - version 2

(a) Full sample   **(b)** Recent sign-up year

Notes: The figure shows the percentages of households that recycled at least once per week in the control group and in the continuous and interrupted treatment groups. Sub-figure 3.B.4a includes the full sample; N per group = 348 (hence N = 696 for both interrupted groups together). Sub-figure 3.B.4b focuses on households with a recent sign-up year (>= 2019); N(T0) = 200, N(T1) = 207, N(T2&T3) = 434. No bags were collected in week 7 due to a public holiday.

Table 3.B.8: OLS: average treatment effects on whether a household recycled at least once per week in period 3, comparing the continuous and interrupted treatment groups with the control group

	Period 3	
	Full sample (1)	Recent sign-up year (2)
Continuous (T1)	0.030**	0.049***
	(0.012)	(0.016)
Interrupted treatments (T2&T3)	0.013	0.024*
	(0.010)	(0.014)
Constant	0.033***	0.007
	(0.011)	(0.014)
Adjusted R^2	0.292	0.315
Observations	4176	2523
T1 vs. T2&T3	p=0.107	p=0.067

Notes: OLS regressions were used to estimate treatment effects on whether a household recycled at least once per week during week 8-10 of the intervention period (week 8-10 = period 3). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year (>= 2019). All regressions include a control for households' baseline recycling activity (dummy for never, once, twice or three times) as well as controls for the house type and the enumerators. No bags were collected in week 7 due to a public holiday. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 3.B.9: Probit: average treatment effects on whether a household recycled at least once per week in period 3, comparing the continuous and interrupted treatment groups with the control group

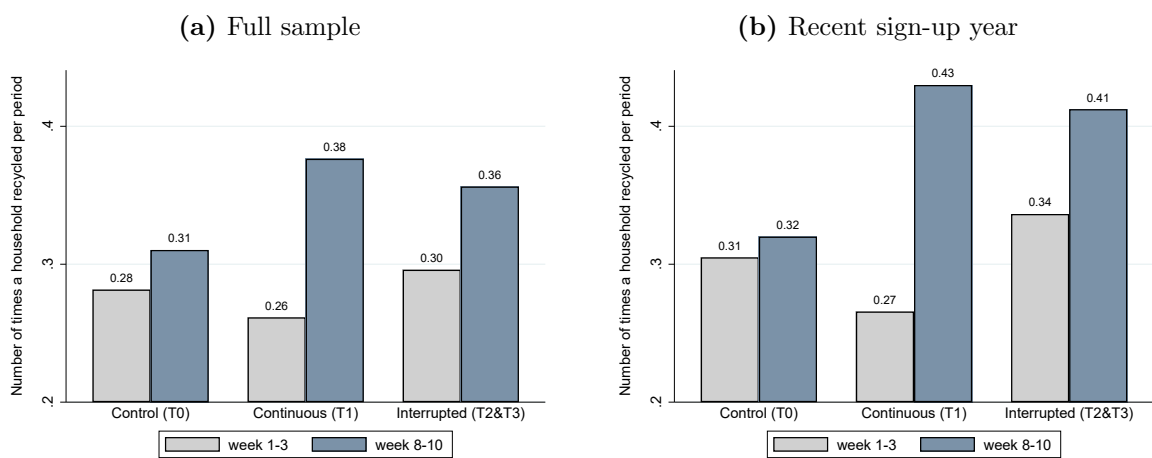|	Period 3	
	Full sample (1)	Recent sign-up year (2)
Continuous (T1)	0.210**	0.360***
	(0.085)	(0.111)
Interrupted treatments (T2&T3)	0.100	0.202**
	(0.075)	(0.099)
Constant	-1.841***	-2.075***
	(0.083)	(0.111)
Pseudo R^2	0.289	0.311
Observations	4176	2523
T1 vs. T2&T3	p=0.122	p=0.076

Notes: Probit regressions were used to estimate treatment effects on whether a household recycled at least once per week during week 8-10 of the intervention period (week 8-10 = period 3). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year (>= 2019). All regressions include a control for households' baseline recycling activity (dummy for never, once, twice or three times) as well as controls for the house type and the enumerators. No bags were collected in week 7 due to a public holiday. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

3.B.2.3. Recycling frequency per period

Figure 3.B.5: Number of times a household recycled during the baseline period (week 1-3) and during week 8-10 of the intervention period, comparing the continuous and interrupted treatment groups with the control group

(a) Full sample   **(b)** Recent sign-up year

Notes: The figure shows the average frequency with which a household recycled per period in the control group and in the continuous and interrupted treatment groups. Sub-figure 3.B.5a includes the full sample; N per group = 348 (hence N = 696 for both interrupted groups together). Sub-figure 3.B.5b focuses on households with a recent sign-up year ($>= 2019$); N(T0) = 200, N(T1) = 207, N(T2&T3) = 434. No bags were collected in week 7 due to a public holiday. A t-test comparing the frequency with which households recycled during the baseline and intervention period within the different groups shows a significant difference in the continuous treatment group both when looking at the full sample (p=0.042) and when focusing on households with a recent sign-up year (p=0.026). The differences are not statistically significant in the interrupted treatment groups or in the control group.

Table 3.B.10: OLS: average treatment effects on how often a household recycled in period 3, comparing the continuous and interrupted treatment groups with the control group

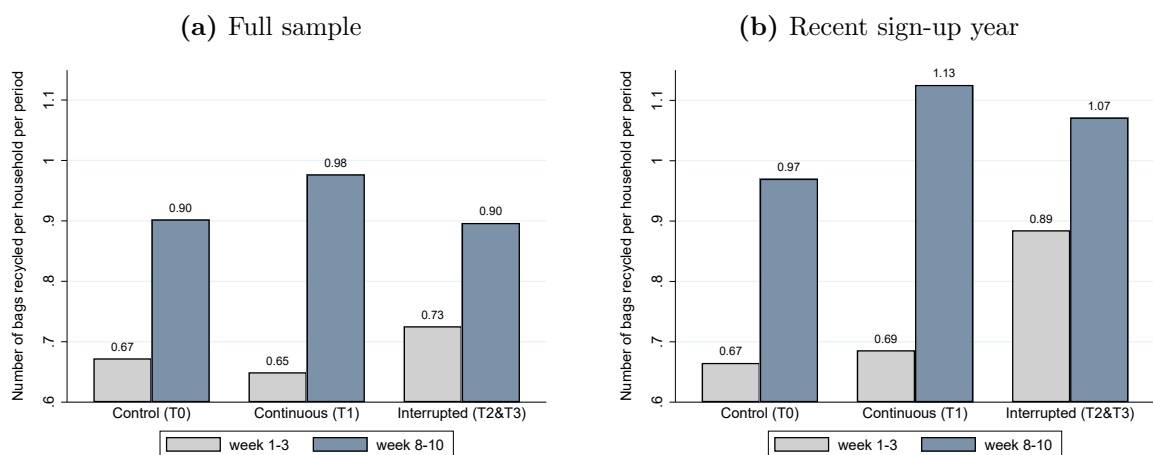|	Period 3	
	Full sample (1)	Recent sign-up year (2)
Continuous (T1)	0.088[*]	0.146[**]
	(0.047)	(0.062)
Interrupted treatments (T2&T3)	0.042	0.073
	(0.040)	(0.053)
Constant	0.100[**]	0.026
	(0.041)	(0.052)
Adjusted R^2	0.417	0.453
Observations	1392	841
T1 vs. T2&T3	p=0.249	p=0.163

Notes: OLS regressions were used to estimate treatment effects on households' recycling frequency (never up to three times) during week 8-10 of the intervention period (week 8-10 = period 3). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year ($>= 2019$). All regressions include a control for households' baseline recycling activity (never up to three times) as well as controls for the house type and the enumerators. No bags were collected in week 7 due to a public holiday. Standard errors in parentheses. [***], [**], [*] indicate significance levels at 1, 5, and 10%, respectively.

Table 3.B.11: Ordered probit: average treatment effects on how often a household recycled in period 3, comparing the continuous and interrupted treatment groups with the control group

	Period 3	
	Full sample (1)	Recent sign-up year (2)
Continuous (T1)	0.226*	0.403***
	(0.118)	(0.155)
Interrupted treatments (T2&T3)	0.107	0.233*
	(0.105)	(0.138)
cut1	1.470***	1.692***
	(0.114)	(0.149)
cut2	1.962***	2.253***
	(0.120)	(0.159)
cut3	2.665***	2.991***
	(0.135)	(0.178)
Pseudo R^2	0.234	0.257
Observations	1392	841
T1 vs. T2&T3	p=0.236	p=0.169

Notes: Ordered probit regressions were used to estimate treatment effects on households' recycling frequency (never up to three times) during week 8-10 of the intervention period (week 8-10 = period 3). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year ($>=$ 2019). All regressions include a control for households' baseline recycling activity (never up to three times) as well as controls for the house type and the enumerators. No bags were collected in week 7 due to a public holiday. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

3.B.2.4. Number of bags recycled per period

Figure 3.B.6: Number of bags recycled per household during the baseline period (week 1-3) and during week 8-10 of the intervention period, comparing the continuous and interrupted treatment groups with the control group

(a) Full sample **(b)** Recent sign-up year

Notes: The figure shows the average number of bags that were recycled per household per period in the control group and in the continuous and interrupted treatment groups. Sub-figure 3.B.6a includes the full sample; N per group = 348 (hence N = 696 for both interrupted groups together). Sub-figure 3.B.6b focuses on households with a recent sign-up year (>= 2019); N(T0) = 200, N(T1) = 207, N(T2&T3) = 434. No bags were collected in week 7 due to a public holiday. A t-test comparing the number of bags recycled per household during the baseline and intervention period within the different groups shows a significant difference in the continuous treatment group both when looking at the full sample (p=0.049) and when focusing on households with a recent sign-up year (p=0.042). The differences are not statistically significant in the interrupted treatment groups or in the control group.

Table 3.B.12: OLS: average treatment effects on the number of bags recycled per household in period 3, comparing the continuous and interrupted treatment groups with the control group

	Period 3	
	Full sample (1)	Recent sign-up year (2)
Continuous (T1)	0.108	0.142
	(0.147)	(0.202)
Interrupted treatments (T2&T3)	-0.041	-0.088
	(0.128)	(0.174)
Constant	0.435***	0.325*
	(0.131)	(0.171)
Adjusted R^2	0.446	0.460
Observations	1392	841
T1 vs. T2&T3	p=0.244	p=0.183

Notes: OLS regressions were used to estimate treatment effects on the number of bags recycled per household during week 8-10 of the intervention period (week 8-10 = period 3). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year (>= 2019). All regressions include a control for households' baseline recycling activity (mean of recycled bags) as well as controls for the house type and the enumerators. No bags were collected in week 7 due to a public holiday. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

3.B.3. Continuous vs. interrupted vs. restarted reminder effect

3.B.3.1. Robustness check

Table 3.B.13: Probit: average treatment effects on whether a household recycled at least once in period 4, comparing the continuous, interrupted and restarted treatment group with the control group

	Period 4	
	Full sample (1)	Recent sign-up year (2)
Continuous (T1)	0.252*	0.314*
	(0.136)	(0.181)
Interrupted (T2)	0.317**	0.358**
	(0.135)	(0.176)
Restarted (T3)	0.092	0.200
	(0.140)	(0.181)
Constant	-1.768***	-1.899***
	(0.133)	(0.176)
Pseudo R^2	0.289	0.301
Observations	1392	841
T1 vs. T2	p=0.609	p=0.788
T1 vs. T3	p=0.232	p=0.498
T2 vs. T3	p=0.088	p=0.335

Notes: Probit regressions were used to estimate treatment effects on households' binary recycling behaviour during week 11-13 of the intervention period (i.e. whether a household recycled at least once during period 4). Columns (1) includes the full sample, column (2) focuses on households with a recent sign-up year ($>=$ 2019). All regressions include a control for households' baseline recycling activity (never vs. at least once) as well as controls for the house type and the enumerators. No bags were collected on the Monday of week 12 due to a public holiday. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

3.B.3.2. Weekly recycling behaviour

Figure 3.B.7: Percentages of households that recycled at least once per week during the baseline period (week 1-3) and during week 11-13 of the intervention period, comparing the continuous, interrupted and restarted treatment group with the control group

(a) Full sample **(b)** Recent sign-up year
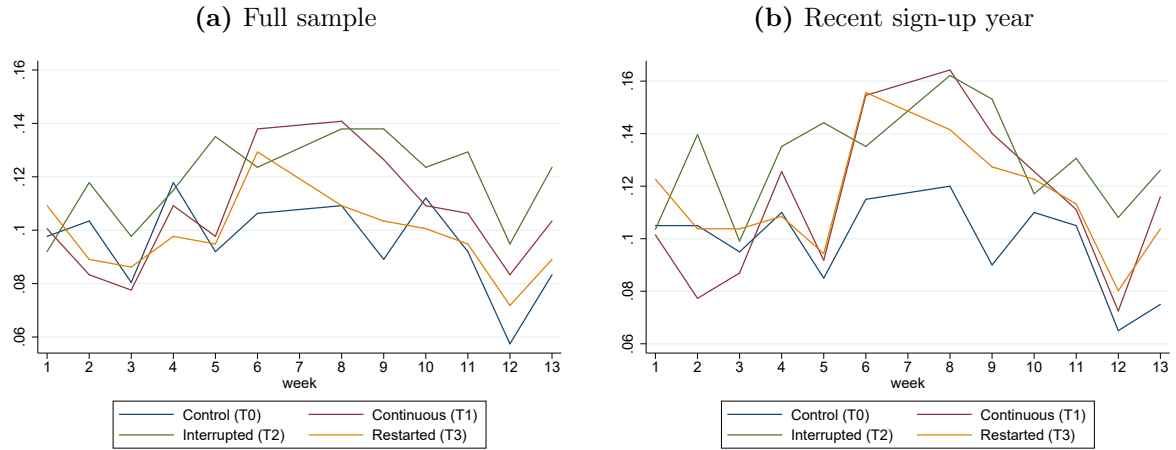
Notes: The figure shows the percentages of households that recycled at least once per week in the control group and in the different treatment groups. Sub-figure 3.B.7a includes the full sample; N per group = 348. Sub-figure 3.B.7b focuses on households with a recent sign-up year (>= 2019); N(T0) = 200, N(T1) = 207, N(T2) = 222, N(T3) = 212. No bags were collected on the Monday of week 12 due to a public holiday.

Table 3.B.14: OLS: average treatment effects on whether a household recycled at least once per week in period 4, comparing the continuous and interrupted treatment groups with the control group

	Period 4	
	Full sample (1)	Recent sign-up year (2)
Continuous (T1)	0.026**	0.027*
	(0.011)	(0.015)
Interrupted (T2)	0.033***	0.033**
	(0.011)	(0.015)
Restarted (T3)	0.008	0.012
	(0.011)	(0.015)
Constant	0.010	0.009
	(0.010)	(0.013)
Adjusted R^2	0.265	0.271
Observations	4176	2523
T1 vs. T2	p=0.514	p=0.670
T1 vs. T3	p=0.093	p=0.298
T2 vs. T3	p=0.020	p=0.138

Notes: OLS regressions were used to estimate treatment effects on whether a household recycled at least once per week during week 11-13 of the intervention period (week 11-13 = period 4). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year (>= 2019). All regressions include a control for households' baseline recycling activity (dummy for never, once, twice or three times) as well as controls for the house type and the enumerators. No bags were collected on the Monday of week 12 due to a public holiday. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 3.B.15: Probit: average treatment effects on whether a household recycled at least once per week in period 4, comparing the continuous and interrupted treatment groups with the control group

| | Period 4 | |
	Full sample (1)	Recent sign-up year (2)
Continuous (T1)	0.244***	0.271**
	(0.093)	(0.123)
Interrupted (T2)	0.294***	0.315***
	(0.092)	(0.119)
Restarted (T3)	0.091	0.166
	(0.096)	(0.123)
Constant	-2.107***	-2.177***
	(0.094)	(0.122)
Pseudo R^2	0.292	0.299
Observations	4176	2523
T1 vs. T2	p=0.566	p=0.688
T1 vs. T3	p=0.092	p=0.356
T2 vs. T3	p=0.023	p=0.175

Notes: Probit regressions were used to estimate treatment effects on whether a household recycled at least once per week during week 11-13 of the intervention period (week 11-13 = period 4). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year (>= 2019). All regressions include a control for households' baseline recycling activity (dummy for never, once, twice or three times) as well as controls for the house type and the enumerators. No bags were collected on the Monday of week 12 due to a public holiday. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

3.B.3.3. Recycling frequency per period

Figure 3.B.8: Number of times a household recycled during the baseline period (week 1-3) and during week 11-13 of the intervention period, comparing the continuous, interrupted and restarted treatment group with the control group

(a) Full sample

(b) Recent sign-up year
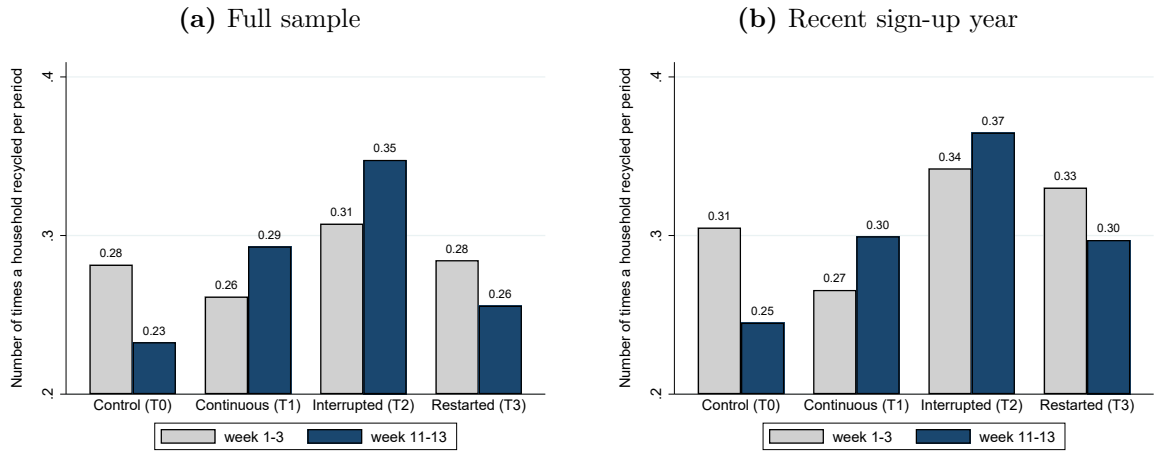
Notes: The figure shows the average frequency with which a household recycled per period in the control group and in the different treatment groups. Sub-figure 3.B.8a includes the full sample; N per group = 348. Sub-figure 3.B.8b focuses on households with a recent sign-up year (>= 2019); N(T0) = 200, N(T1) = 207, N(T2) = 222, N(T3) = 212. No bags were collected on the Monday of week 12 due to a public holiday. T-test comparisons show that none of the differences in the recycling frequency to baseline levels are statistically significant.

Table 3.B.16: OLS: average treatment effects on how often a household recycled in period 4, comparing the continuous, interrupted and restarted treatment group with the control group

	Period 4	
	Full sample (1)	Recent sign-up year (2)
Continuous (T1)	0.078*	0.081
	(0.042)	(0.056)
Interrupted (T2)	0.100**	0.100*
	(0.042)	(0.055)
Restarted (T3)	0.024	0.034
	(0.042)	(0.056)
Constant	0.030	0.027
	(0.038)	(0.047)
Adjusted R^2	0.396	0.412
Observations	1392	841
T1 vs. T2	p=0.601	p=0.736
T1 vs. T3	p=0.201	p=0.392
T2 vs. T3	p=0.072	p=0.227

Notes: OLS regressions were used to estimate treatment effects on households' recycling frequency (never up to three times) during week 11-13 of the intervention period (week 11-13 = period 4). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year (>= 2019). All regressions include a control for households' baseline recycling activity (never up to three times) as well as controls for the house type and the enumerators. No bags were collected on the Monday of week 12 due to a public holiday. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Table 3.B.17: Ordered probit: average treatment effects on how often a household recycled in period 4, comparing the continuous, interrupted and restarted treatment group with the control group

	Period 4	
	Full sample (1)	Recent sign-up year (2)
Continuous (T1)	0.253**	0.311*
	(0.127)	(0.167)
Interrupted (T2)	0.302**	0.339**
	(0.125)	(0.163)
Restarted (T3)	0.084	0.189
	(0.130)	(0.168)
cut1	1.693***	1.788***
	(0.123)	(0.161)
cut2	2.254***	2.342***
	(0.132)	(0.171)
cut3	2.967***	3.201***
	(0.151)	(0.200)
Pseudo R^2	0.236	0.248
Observations	1392	841
T1 vs. T2	p=0.680	p=0.853
T1 vs. T3	p=0.173	p=0.433
T2 vs. T3	p=0.075	p=0.322

Notes: Ordered probit regressions were used to estimate treatment effects on households' recycling frequency (never up to three times) during week 11-13 of the intervention period (week 11-13 = period 4). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year (>= 2019). All regressions include a control for households' baseline recycling activity (never up to three times) as well as controls for the house type and the enumerators. No bags were collected on the Monday of week 12 due to a public holiday. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

3.B.3.4. Number of bags recycled per period

Figure 3.B.9: Number of bags recycled per household during the baseline period (week 1-3) and during week 11-13 of the intervention period, comparing the continuous, interrupted and restarted treatment group with the control group

(a) Full sample **(b)** Recent sign-up year
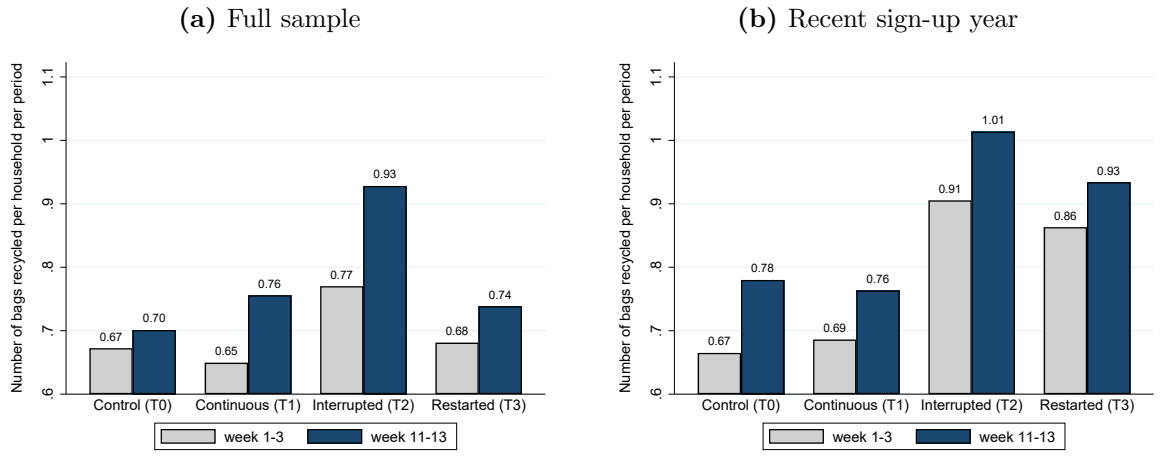
Notes: The figure shows the average number of bags that were recycled per household per period in the control group and in the different treatment groups. Sub-figure 3.B.9a includes the full sample; N per group = 348. Sub-figure 3.B.9b focuses on households with a recent sign-up year (>= 2019); N(T0) = 200, N(T1) = 207, N(T2) = 222, N(T3) = 212. No bags were collected on the Monday of week 12 due to a public holiday. T-test comparisons show that none of the differences in the number of recycled bags to baseline levels are statistically significant.

Table 3.B.18: OLS: average treatment effects on the number of bags recycled per household in period 4, comparing the continuous, interrupted and restarted treatment group with the control group

	Period 4	
	Full sample (1)	Recent sign-up year (2)
Continuous (T1)	0.079	-0.035
	(0.139)	(0.192)
Interrupted (T2)	0.161	0.057
	(0.139)	(0.188)
Restarted (T3)	0.037	-0.008
	(0.139)	(0.191)
Constant	0.200	0.266
	(0.124)	(0.162)
Adjusted R^2	0.407	0.420
Observations	1392	841
T1 vs. T2	p=0.558	p=0.663
T1 vs. T3	p=0.764	p=0.887
T2 vs. T3	p=0.376	p=0.727

Notes: OLS regressions were used to estimate treatment effects on the number of bags recycled per household during week 11-13 of the intervention period (week 11-13 = period 4). Column (1) includes the full sample, column (2) focuses on households with a recent sign-up year (>= 2019). All regressions include a control for households' baseline recycling activity (mean of recycled bags) as well as controls for the house type and the enumerators. No bags were collected on the Monday of week 12 due to a public holiday. Standard errors in parentheses. ***, **, * indicate significance levels at 1, 5, and 10%, respectively.

Conclusion

With my PhD, I contribute to a better understanding of how behavioural and experimental economics can be applied to understand motivations and create incentives for pro-environmental behaviour. By focusing on the case of Peru, I draw on a country with a rising middle class and increasing carbon emissions, generating insights for a highly relevant consumer group that has so far received little attention in the literature. I collect own data through a large household survey and two field experiments in Lima, Peru, shedding light on the role of economic preferences, social norms, individual beliefs and reminders for PEB in particular. My findings are of great relevance for scientific debates in the respective fields, as well as for policy makers that aim to encourage sustainable behaviour.

In the first chapter (**Chapter 1**), I investigate the role of economic preferences for PEB based on a household survey with 900 middle class households in Lima, Peru. The PEBs included in the analysis are habitually saving energy, avoiding the use of plastics and limiting expenditures on electricity. By focusing on the role of preferences for regular PEBs in the household, I extend previous research that has so far mainly focused on occasional behaviour (such as purchasing a new energy-efficient appliance). I include a full range of individual preferences in the analysis, in particular preferences on risk and ambiguity, time, trust, altruism and positive and negative reciprocity. By doing so, I extend the existing literature that has so far mainly looked at one or two preferences in isolation. Given their strong correlation, including all preferences in the analysis is important to make sure they do not proxy for omitted ones, which may lead to biased estimates. I further extend previous evidence by including ambiguity preferences in the analysis, which are conceptually more relevant for PEB than risk preferences, yet have so far been disregarded in the literature.

To elicit preferences, I make use of a state-of-the-art experimentally validated survey measure, the Global Preferences Survey (GPS) of Falk et al. (2016) and Falk et al. (2018). While the way preferences have been measured in previous studies varies, applying this standardized measure facilitates international replication and comparability. Moreover, my rich data set allows me to control for important individual characteristics, such as environmental knowledge and concern, wealth and education, that are potentially correlated both with preferences and with PEB, and to therefore reduce the potential risk of

omitted variable bias. In addition, besides the study by Fuerst and Singh (2018), I am the first to investigate the role of preferences for PEB outside a high-income country context, adding important new evidence on their relevance in the Peruvian case.

My results show that particular preferences matter for particular PEBs, and thus that preferences that matter for one type of PEB do not necessarily matter for another. In the case of social preferences, I find that preferences are strongly correlated with energy-saving behaviour yet hardly with sustainable plastics consumption or with monthly expenditures on electricity. I discuss that this may be due to the fact that engaging in regular behaviours to save energy in the household is something that one usually does for oneself without being publicly recognized for it, which suggests that it requires a strong sense of intrinsic motivation and thus makes it plausible that social preferences are important. My findings further show that it is not just trust that can explain PEB as mostly focused on in previous studies (Gupta and Ogden, 2009; Volland, 2017; Ziegler, 2020), but that it is important to consider other social preferences in the analysis as well.

Regarding risk and ambiguity preferences, I find that both are negatively related with sustainable plastics consumption, meaning that people who are less tolerant of risk and ambiguity, i.e. more risk and ambiguity averse, engage more in avoiding wasteful plastic use. While previous research mostly found evidence for the opposite correlation, i.e. that more risk averse people engage less in PEB (Farsi, 2010; Qiu et al., 2014; Fischbacher et al., 2015), I argue that this can be explained by the fact that not just the benefits of PEB are uncertain, but the costs of not engaging in PEB are uncertain, too. In the case of sustainable plastics consumption, engaging in the behaviour does not entail lots of uncertain investment (as e.g. when purchasing a new appliance), so that the uncertainties about the potential damage from not engaging in the behaviour may outweigh the uncertain benefits from engaging in it. I further find that the willingness to experience ambiguity is positively related with people's monthly spending on electricity, which is in line with what Volland (2017) finds for risk preferences and spending on energy. Since in my case ambiguity preferences are a significant predictor while risk preferences are not, it underlines the importance of including ambiguity preferences in the analysis, and not just risk preferences alone.

In terms of time preferences, I find that a higher level of patience is positively related with sustainable plastics consumption and negatively with monthly spending on electricity, which confirms previous research that has found a positive correlation between patience and PEB (Fischbacher et al., 2015; Newell and Siikamäki, 2015; Fuerst and Singh, 2018; Ziegler, 2020). In line with the previous studies, I argue that this can be explained by the fact that that more patient people discount the future at a lower rate, and therefore value the future benefits of engaging in PEB more highly.

I derive three main message from these findings. First, it matters to control for all

relevant preferences in the analysis when explaining PEB. Second, different preferences matter for different PEBs, and there is no general conclusion that can be drawn about their respective importance. Third, policy makers can make use of the evidence that particular preferences matter for particular PEBs and tailor their communication strategies accordingly: To encourage regular energy-saving behaviour in the household, policy makers could appeal on people's intrinsic motivation and emphasize the social responsibility to care for future generations. When aiming to promote the sustainable use of plastics, my findings suggest that focusing on the negative environmental consequences of not doing so could be a promising strategy. To encourage lower spending on electricity, highlighting the financial savings and appealing on future benefits could be effective.

In the second chapter of this thesis (**Chapter 2**), I investigate the role of social norms and individual beliefs about social norms for people's decision to recycle, with a focus on dynamic and injunctive norms. In cooperation with the municipality of Miraflores in Lima, Peru, I conduct a field experiment with 1,709 households to increase sign-up rates to the municipality's recycling programme. Through phone surveys, I first elicit people's prior beliefs about dynamic and injunctive norms regarding participation in the recycling programme; I then provide people with information about one of the two norms (dynamic or injunctive norm treatment), or both (combined treatment), or none (control group), depending on the treatment; and finally ask about people's decision to sign up to the recycling programme.

I show that individual beliefs about dynamic and injunctive norms in the recycling behaviour of others matter for people's decision to sign up to the recycling programme as well, and that randomly correcting negatively biased beliefs causally raises people's willingness to do so. I provide evidence that individual level belief updating can explain heterogeneous responses to social norm information. In particular, I find that for those who underestimate the respective norm, the social norm information treatments significantly increase people's sign-up decision compared to a control group, while there is no significant treatment effect among those who correctly or overestimate the norm. In the case of the dynamic norm, this applies to both belief updating on the dynamic element about the trend in participation rates as well as on the static low descriptive norm element about the current participation rate; in the case of the injunctive norm, it applies to those who underestimate the injunctive norm more strongly. I thus provide evidence on the importance of belief updating for people's response to social norm information irrespective of the size of the norm or whether the norm is presented in a static or dynamic way. I further show that the effectiveness of the treatments increases in the size of belief updating, i.e. the more strongly people underestimate the actual norm.

With this study, I add to the few existing papers that combine measuring people's individual prior beliefs with social norm information treatments that directly aim at

correcting those beliefs (Andre et al., 2021; Bursztyn et al., 2020; Byrne et al., 2018). In line with previous research, I show that providing people with social norm information can only effectively encourage behavioural change when people underestimate the actual norm. I extend previous evidence on the role of individual level belief updating for people's response to dynamic norm information in a context where the share of others engaging in the desired target behaviour is increasing while the majority does not yet do so.

I link up to the recent literature on dynamic norms (Loschelder et al., 2019; Mortensen et al., 2019; Sparkman and Walton, 2017; Sparkman and Walton, 2019; Sparkman et al., 2020) and provide evidence for their effectiveness even in situations where only a small share of others is already behaving in the desired way. A key challenge of encouraging pro-environmental behaviours is that we are often faced with a situation where current unsustainable norms such as eating meat, flying or not conserving energy prevail. However, at the same time, many sustainable behaviours are increasing in popularity and more and more people start to engage in them, which makes the use of these dynamic norms to encourage people to follow suit promising. My findings are thus of great importance in view of the major environmental challenges we are facing, for which we need a norm change in many domains.

My findings highlight the importance of individual beliefs about social norms for people's decision making, and for the effectiveness of social norm interventions that directly address those beliefs. With my research, I therefore provide important insights that can help to explain why social norm interventions may work in some but not in other contexts, which has important policy implications. My results suggest that, if possible, policy makers should try to acquire information about the distribution of beliefs about social norms among the population before designing policy campaigns that build on social influence. If most people underestimate the social (dynamic or injunctive) norm, my findings (in line with previous evidence) indicate that information campaigns that focus on this norm will be effective to encourage behavioural change, while at the same time my results imply that even if it is not possible to gather information about beliefs first, social norm information campaigns should have no negative effect (but simply none) when most people overestimate the respective information. I show that this is the case even in situations where most people do not yet engage in the desired behaviour, which is crucial for a variety of settings in view of fighting the climate crisis.

In the third chapter (**Chapter 3**), I investigate the effectiveness of sms reminders to encourage weekly recycling behaviour of households based on an RCT conducted with 1,392 households in the context of a municipal recycling programme in Lima, Peru. The fact that households sign up to the programme voluntarily yet many enrolled household do not recycle regularly points towards a gap between people's intention to recycle and their actual recycling behaviour – a phenomenon characteristic for many pro-environmental

behaviours. The reminder intervention aims to facilitate people to follow through with their recycling intentions by overcoming the problem of limited attention (Karlan et al., 2016), reminding people about the recycling programme on the day when the recycling bags are collected.

I observe households' recycling behaviour over 12 weeks in total, where the first three weeks serve as a baseline period and the subsequent nine weeks constitute the intervention period. During the nine weeks of intervention period, I vary the frequency with which reminders are sent based on blocks of three weeks. Households are randomly assigned to either a group that does not receive any reminders during the whole intervention period (control group), a group that receives reminders continuously for the whole nine weeks (continuous treatment), a group that receives reminders only for the first three weeks after which the intervention gets interrupted (interrupted treatment), or a group that receives reminders for the first three weeks, then experiences an interruption for three weeks, before the intervention is restarted again in the last three weeks (restarted treatment).

I show that reminders are generally effective to encourage recycling behaviour, and that the share of households that recycle during the first three weeks of the intervention period is significantly higher among all treated households that received a weekly reminder than among the control group that did not receive any reminders. I further provide evidence for strong continuous reminder effects on households' recycling activity that persist throughout the whole intervention period. My data further reveals some evidence for persistence effects of reminders after the intervention was interrupted, however the results are less clear in this regard.

The strong continuous reminder effects speak in favour of the theory that limited attention on the collection day is a main obstacle for households to recycle regularly. My findings suggest that this obstacle can be addressed by reminding people to recycle on a regular basis. The evidence I find for persistence effects of reminders (albeit not robust) further indicates that reminders can lead to some form of habit formation in households' recycling behaviour. The importance of habits and routine is plausible in my study context as for successful participation in the programme, people need to organize their internal waste management in the household in advance, collect materials during the week and finally prepare the bags for collection. Yet, the evidence is not clear in this regard and it is likely to assume that the study period was too short to observe the role of habit formation in detail.

A direct comparison of interrupted and continued reminders during most of the intervention period (excluding the last three weeks) suggests that sending reminders continuously is most effective to encourage recycling behaviour over time. Being able to directly compare the effect of continued and discontinued reminders is rare in the literature (Allcott and Rogers (2014) is one other example) and adds important new evidence,

which extends most previous studies that can only rely on post-intervention data to derive conclusions about persistence effects without being able to compare it to a group that still continues to receive reminders at the same time (such as Calzolari and Nardotto (2017) and Muller and Habla (2018)).

Data issues during the last three weeks complicate the analysis of a longer comparison of continuous and interrupted reminders, and especially with regards to potential restart effects of reminders, for which I cannot find evidence based on my results. Yet, lower recycling rates during the last three weeks due to a public holiday have likely led to problems of statistical power and biased results so that it is possible that there might have been a positive restart effect that I was simply not able to detect. This is unfortunate and would be promising to investigate again with more robust data in future research.

In general, I find that the reminder effects are stronger for households that signed up to the recycling programme more recently (since 2019). Since it is plausible to assume that more of the recently registered phone numbers are still up to date, this suggests that I might underestimate the effectiveness of reminders with my data, and that they could have even stronger effects on people's behaviour in a context where it would be certain that all people would definitely receive the reminders.

I add important new evidence to the existing reminder literature by expanding its focus to Peru, and thus away from the Global North, and to recycling behaviour, which has rarely been investigated so far. Both is of great relevance not only for researchers but also for policy makers, as I show how a low-cost, easily scalable tool can be used effectively to encourage households to recycle. Sms reminders have very low implementation costs and access to cell phones is widespread, which makes the intervention promising for many contexts. As long as recycling habits are not fully developed, the policy recommendation based on my data would be to send reminders continuously, which showed the strongest and most robust effects on households' recycling behaviour over time.

To conclude, I would like to emphasize that I hope that the findings of my PhD will be useful for both researchers and policy makers, and that they will inspire future investigations on how to encourage pro-environmental behaviour based on behavioural and experimental economics insights. Most importantly, I hope that the findings generated through this thesis can contribute a small piece to the complex puzzle of scientific evidence and policy measures that will be needed to tackle the immense challenge of reducing global waste accumulation and successfully fighting the climate crisis. I would like to end this thesis they way I started it, with a quote – perhaps it will inspire readers as it inspired myself.

"The greatest threat to our planet is the belief that someone else will save it."

— Robert Swan

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