Creating pro-environmental behavior change: Economic incentives or norm-nudges?

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Abstract

To mitigate global warming, collective behavior change is needed. But which tools should policymakers prioritize: economic incentives, nudges, or a combination? Current evidence from social science provides little direct advice, as it either lacks credible identification of causality or objective long-term behavioral data. Addressing both limitations, we present causal evidence from a two-year field experiment, comparing how a small price incentive and a social norm-nudge affect the recycling behavior of more than 2,000 households. The results show a large, immediate, and persistent positive effect of incentives on both the quantity and quality of recycling, but no effect of the norm-nudge. However, the price incentive reduced customer satisfaction, unless it was combined with the norm-nudge, suggesting that appealing to norms can make climate incentives more acceptable.

Introduction

Climate change is one of the most pressing challenges of our time, directly impacting the environment and biodiversity, as well as threatening human health and political stability (*1*). Future technological advances may provide part of the solution, but there is also an urgent need to create new and more sustainable climate habits in society *today*, using the best available tools from social science to promote long-term behavior change (*2*). The overarching question we address is therefore the following: How can policymakers best facilitate pro-environmental behavior in a way that is effective, acceptable, and scalable?

The current research focuses on the habit of *waste recycling*, which is a specific objective within the UN sustainable development goal of responsible consumption and production, as it contributes to the reduction of CO_2-emissions and helps mitigate climate change (*3*, *4*). We consider this a much-needed test case of consequential pro-environmental behavior change, as citizens across the globe generate more than two billion tons of municipal solid waste every

year, of which only 19% are currently recycled or composted (*5*). Recycling also represents a type of continuous daily action that is widely considered important but difficult to change, when people try to make the critical move from good intentions to actual behavior (*6*, *7*). As such, our study is relevant for understanding behavioral change in general, beyond the specific context of recycling.

Broadly speaking, research-based public policies can be divided into two categories: one focusing on systemic interventions such as monetary incentives or regulations; the other focusing on individual-oriented non-invasive behavioral interventions that aim to "nudge" people in a certain direction without limiting their freedom of choice (*8*, *9*). When it comes to promoting pro-environmental behavior, a recent meta-study ranked economic incentives and nudges targeting social norms as the two most promising alternatives (*2*). The authors stressed, however, that the two interventions may have very different effects in the longer term, that it is inherently difficult to compare interventions tested in different contexts, and that there could be additional unexplored benefits from combining incentives and norm-nudges. The choice between the two types of interventions is further complicated by a large scientific literature suggesting that monetary incentives often improve targeted behavior in the short-term but can reduce intrinsic motivation for the task, discourage unincentivized behavior, and lead to reactance (*10*–*12*). The nudging strategy, on the other hand, which is intended to offer predictable behavior change at low cost (*13*), has recently been called into question when it comes to delivering effects at scale (*14*), and that it may crowd out support for more structural policies such as economic incentives in the form of carbon taxes (*15*).

In the current study, we partnered with a large waste management company to offer unique experimental insights into this policy debate (*16*). In particular, we report from a large-scale randomized controlled trial evaluating the impact of (i) a monetary incentive, (ii) a social norm-nudge, and (iii) a combination treatment using both approaches, on household waste recycling over a period of two years. To provide a comprehensive assessment of the effects we use objective behavioral data on both the quantity and quality of actual recycling (from RFID keys, container weights, and waste audits), and combine it with self-reported measures of attitudes and beliefs (from household surveys). Our approach to this question is of broad relevance across the social sciences, as we compare the causal effectiveness of both a norm-nudge and an economic incentive which are frequently studied in psychology and economics, while closely monitoring public acceptability, which is especially important from a political science perspective.

Overall, we make two major contributions to the scientific literature on how to effectively promote pro-environmental behavior. First, by conducting a so-called natural field experiment (*17*), where we implement random assignment of treatment conditions in a natural context without subjects being aware thereof, we avoid the most severe threats to causal identification that typically bias reported treatment effects: confounding factors (*18*), self-selection of study participants (*19*), and experimenter demand effects (*20*). Most importantly, we can ensure that nothing else than our treatments vary systematically across households. In contrast, the few existing economic studies of the relationship between incentives and recycling rely on non-experimental observational data, comparing regions that implemented an incentive with regions that did not (*21*–*26*). However, regions that introduce a monetary incentive to recycle waste are likely to also make changes in the infrastructure

and/or communication towards citizens that could have an independent effect on recycling, leading to a biased estimate of the pure incentive effect.

As the second major contribution, we respond to a recent call for more research on *actual behavior* in environmental psychology and related social science on climate change, as opposed to exclusively relying on self-reported outcome measures of attitudes and intentions (*27*). This is a longstanding general concern in psychology (e.g., (*28*)), but particularly important in the current setting of policy-related climate research, since self-reported pro-environmental intentions only explain a small proportion of the observed variation in actual climate behavior (*29*). For example, a recent study tested the effect of 11 different expert-recommended interventions from behavioral social science in a global online sample (*30*). Although several interventions produced significant positive effects on beliefs in climate change and policy support, none of them caused a change in actual pro-environmental behavior as captured in a real effort task. It is therefore of crucial importance not to take behavioral effects for granted when moving from hypothetical scenarios in surveys to real-life behavior in the field. We alleviate the concerns of self-reported outcome measures by collecting objective behavioral data on both the quantity of recycling (kg waste collected) and the quality of recycling (fraction correctly sorted), over a two-year period. Importantly, having objective measures on both dimensions allows us to differentiate between the desired policy effect (a reduction in unsorted waste and an increase in *correct sorting* of recyclable material), versus an unintended effect that would simply pollute the recycling process (incorrectly throwing non-recyclable waste in the containers for recyclable material).

The Experiment

The Material and Methods section in the supplementary material provides a complete description of the experiment design – here we give a brief summary. The field experiment was carried out between September 2021 and December 2022 in the city of Bergen, Norway. In total, the sample covered more than 2,000 households, generating 1,000,000 kg of waste, from 42 different condominiums that all used the local waste management company's container-based system. These condominiums typically have three waste containers: one for general non-recyclable waste, one for paper and carton recycling, and one for plastics recycling (other recyclable material, such as electronics, glass and metal, is collected in different recycling stations scattered across the city). Importantly, the general waste container in each condominium is constructed with a volume constraint of approximately 30 liters per opening, and households need a unique RFID key to open the container each time. Hence, the system enables the waste management company (BIR) to charge households' depending on how often they open the general waste container, also referred to as a pay-as-you-throw (PAYT) system (*31*).

We implemented our experimental treatments in January 2022. The first treatment was an economic price incentive intended to promote recycling. The incentive implied that households needed to pay a small fee of 9.99 Norwegian Kroner (≈USD 1) for every additional opening of the general waste container, but only after reaching a pre-set number of free openings (8 or 5 per month depending on container type in the condominium). By removing paper, plastic and other recyclable material from their general waste, households could reduce the number of openings, and thereby save money. However, households could also

save money by throwing the same amount of waste on fewer occasions, or by throwing their waste elsewhere (e.g., in the condominium's paper or plastic container). Hence, whether such an incentive actually promotes the quantity and quality of correct recycling is far from obvious, and important to test empirically. The second treatment was a social norm-nudge, inserted as the first paragraph in a general letter about recycling sent to all households (the letter also included information about the economic incentive for the affected households). In favor of recycling, the norm-nudge clearly stated both an injunctive norm (i.e., a moral appeal) and a descriptive norm (i.e., that most people do it). We also included a sticker to place on the RFID key to serve as a reminder about the message. Our first follow-up survey confirmed that the majority of our study participants had read and remembered the information in the letter.

The two treatments were cross-randomized in a 2*2 between condominium design with four groups, meaning that households living in some condominiums only received the incentive (*Incentive*), some only received the norm-nudge (*Norm*), some received both the incentive and the nudge (*Incentive and Norm*), and some were in the control group and only received a generic version of the letter (*Control*). This design allows us to both compare the relative impact of incentives vs. nudges, and to test whether combining incentives and nudges reinforce or counteract each other when it comes to promoting sustainable climate habits such as recycling. To estimate the causal effect with maximum statistical power, we use a difference-in-difference approach (*32*), comparing differences in outcome variables across the randomized treatment groups in the post-treatment period (after January 2022) with the differences observed in the pre-treatment period (before January). For transparency, we pre-registered the primary hypotheses and statistical analyses in advance of the data collection (https://aspredicted.org/D8H_9YT), and have made the data and code openly available for replication (https://osf.io/mtuh9/).

Results

Our pre-registered main outcome variables are (i) the number of *openings* of the general waste container per household and month, (ii) the *weight* of the general waste container per condominium and month, (iii) recycling *quality* identified as the share of recyclable materials in the general waste container, and correspondingly, as the share of non-recyclable general waste in the paper and plastic containers. We also pre-registered that we would report treatment effects on customer *attitudes* from a household survey. All treatment effects reported in the text and figures come from ordinary least squares regressions (OLS) with standard errors clustered by condominium, to account for potential correlation within condominiums and over time (*33*). It was also pre-specified that we would report the incentive treatment effect, using a more powerful specification, by comparing the two incentive groups jointly against the combination of the control and the norm group. Reported *P* -values are from two-sided *t*-tests and the pre-specified threshold for statistical significance (alpha) was set to 0.1 (see Material and Methods in the supplementary material for further details).

Openings. To showcase the direct effect of the incentive, Fig. 1 plots the average number of general waste container openings per household and month, by treatment group. As predicted, there is an immediate drop in the number of general waste openings in the two

treatment groups that faced the incentive. The effect is large, reducing the number of openings by more than 30 percent ($P < .001$), equivalent to almost five openings per month, and highly persistent, showing no evidence of decline over the 11-month incentive period. We also note that the incentive caused a more equal distribution of openings per household and month, as indicated by the smaller standard errors after the incentive was introduced (see also fig. S1, which shows the pre- and post-intervention distributions in the number of openings per month).

In contrast, there is no indication of the norm-nudge having any impact on the number of general waste openings relative to the trend observed in the control group ($P = .512$), nor altering the effect of the incentive as the two groups exposed to the price incentive are statistically indistinguishable from each other ($P = .899$) – see table S1 for the regression-based point estimates of the treatment effects. We now move on to an analysis of the effects on container weight, to see whether the effects on the number of openings map into actual recycling behavior.

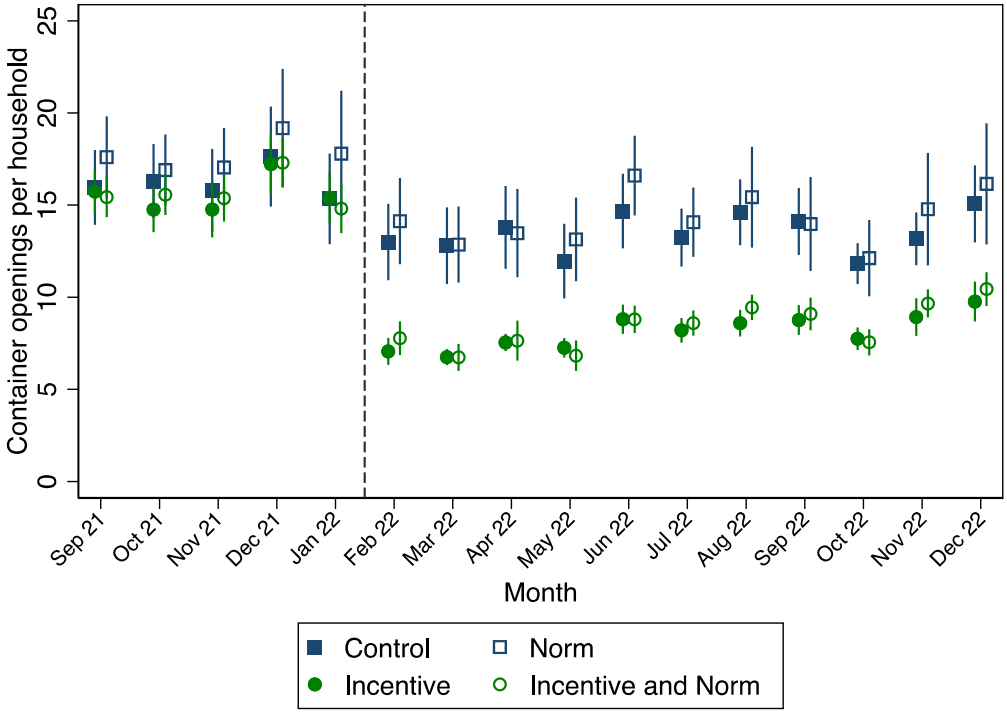

Fig. 1. Openings of general waste container per month. The graph shows the average number of openings of the general waste container per household and month, separately for each treatment group. The dashed vertical line separates the pre-treatment period and the post-treatment period. Error bars indicate +/- one standard error.

Weight. Table 1, column 1, reports the effect of each treatment on the monthly weight of a condominium's general waste container. If our treatments caused more recycling, we would expect the weight of the general waste to decrease. Consistent with this prediction, the results show that the incentive significantly reduced the amount of general waste, on average by 180 kg (or 18%) per condominium and month ($P = .005$).

Table 1. Treatment effects on the amount of waste.

	Outcome variable: (kg per condominium and month)			
	General waste	General waste	Paper and Plastic	Total waste
	(1)	(2)	(3)	(4)
Norm only	-25.17			
	(0.680)			
Incentive only	-180.1***			
	(0.005)			
Incentive & Norm	-150.2***			
	(0.009)			
Incentive		-151.3***	53.05***	-76.15
		(0.002)	(0.007)	(0.338)
Baseline mean	975.49	1073.38	319.47	1526.48
P: Incentive only = Norm only	0.043			
P: Incentive & Norm = Norm only	0.073			
P: Incentive only = Incentive & Norm	0.658			
N	672	672	352	352

Notes: The table reports the treatment effects from OLS panel regressions, accounting for any difference in pre-intervention levels across treatment groups. Column 1 reports effect of each treatment separately against the control group. Column 2-4 reports the treatment effect of being in one of the two incentive groups compared to being in one of the two groups not exposed to incentive. *P*-values based on standard errors clustered at the condominium level are provided in parenthesis.
* *P* <0.1, ** *P* <0.05, *** *P* <0.01

In contrast, the point estimate of the norm-nudge is equivalent to a modest 25 kg reduction, which is not significantly different from zero (*P* = .680). Consistent with these findings and the reported treatment effects on container openings, the combined incentive and norm treatment also caused a reduction in general waste, on average by 150 kg, which is both significantly different from zero (*P* = .009) and from the pure norm treatment (*P* = .073), but not statistically different from the effect of the pure incentive effect (*P* = .658). Hence, the norm-nudge did not have an independent effect on the weight of the general waste, nor did it alter the effectiveness of the incentive. From here on, we therefore report the incentive treatment effect using the more powerful specification in the main text (comparing the two incentive groups jointly against the two groups not facing the incentive) and report the effects separately by treatment group in the supplementary material. In column 2 we report the results from this pre-registered analysis, which shows that exposure to the incentive caused a 151 kg (or 14%) reduction in the general waste (*P* = .002). Fig. 2 illustrates how the incentive treatment effect develops over time. In line with the effect on openings, there is a distinct treatment effect appearing already in the first month of the incentive period that persists at the same level for the duration of the experiment.

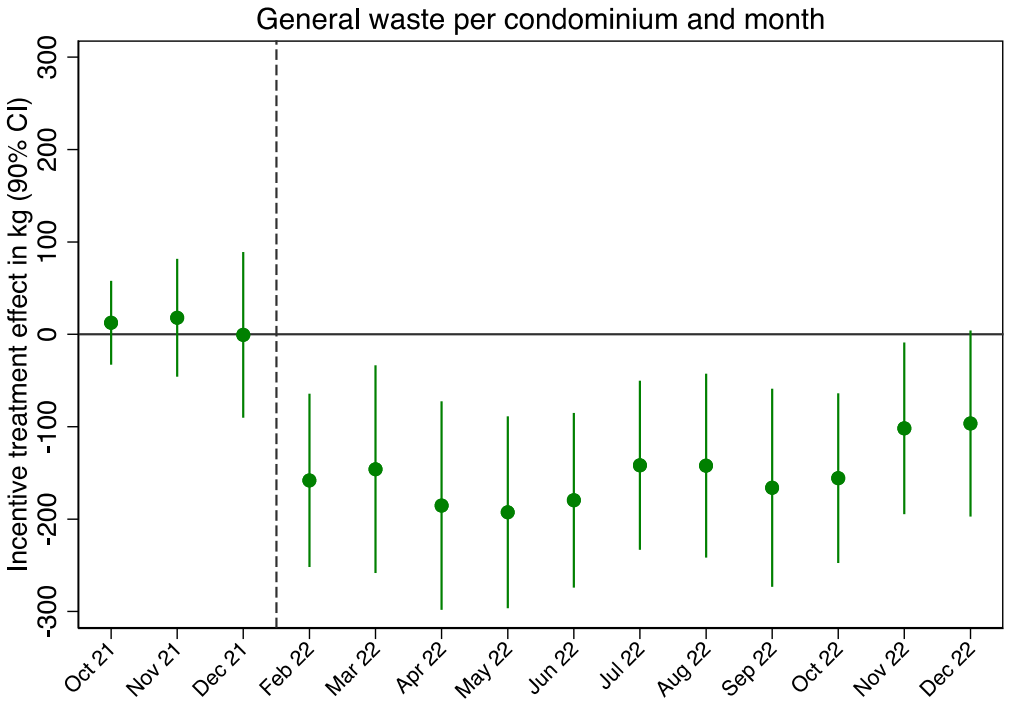

Fig. 2. The persistence of the incentive treatment effect on general waste collected. The graph shows the average monthly incentive treatment effect on the general waste collected by condominium (in kg). The dashed vertical line separates the baseline period and the intervention period. The treatment effect is estimated using panel OLS regression, comparing the two incentive groups against the two groups not exposed to incentive, using September 2021 as the reference month. Confidence intervals are based on standard errors clustered at the condominium level. For visual clarity we use a moving average of the outcome variable (+/- one month).

If the reduction in general waste came through more recycling of paper and plastic, we would expect to observe an increase in the weight of these containers. For a subset of condominiums BIR is able to track the weight of the paper and plastic containers, which allows us to shed light on this issue. As predicted, in column 3 of Table 1, we observe that the incentive caused a significant 15 percent increase in the weight of paper and plastic collected per month, corresponding to 53 kg (P = .007). Moreover, we show, in column 4 of Table 1, that the incentive did not significantly affect the total waste collected per month and condominium (P = .338). Overall, these results indicate that the incentive has caused more recycling, as it shifted some of the waste previously disposed in the general waste container to the containers for recyclable materials, without significantly affecting the total waste generated and collected (table S2 confirms that we see the same pattern for the incentive treatments separately; table S3 confirms that the results are robust to the imputation of missing values). The negative but non-significant point estimate on total waste could reflect that households also responded by either reducing packaging consumption or depose some of their waste at other locations, which is something we investigate further in the analysis of the household survey.

Quality. Having established that the incentive generated more recycling, by reducing the weight of the general waste container and increasing the weight of the paper and plastic containers, we now turn to the question whether it also generated *correct* recycling using the objective waste audit data. Fig. 3, panel A, plots the share of recyclable material in the general waste container before and after the incentive was introduced, separately for the groups exposed to the incentive or not. A lower share of recyclable material in the general waste implies that households are better at sorting. Indeed, the incentive generated a significant 2.5 percentage-point, or 13 percent, reduction in the share of recyclable material in the general waste (P = .020).

In Fig. 3, panel B, we plot the share of general waste material in the paper and plastic containers. A higher fraction of general waste in these containers would be an unintended negative consequence, indicating that the incentive caused people to increasingly throw non-recyclable material in the containers for recycling, either by intention to avoid paying the fee or because of an over-enthusiastic attempt to recycle material not suited for that purpose. The results show no indication of deteriorating recycling quality. The fraction of general waste in the containers for recyclable material is unaffected by the incentive (P = .912), and thus kept at a relatively low pre-intervention level of about 6 percent. Table S4 reports the regression-based point estimates of the treatment effects, which confirms that we see the same pattern in the specification with the incentive treatments separately and no indication of the norm-nudge affecting recycling quality.

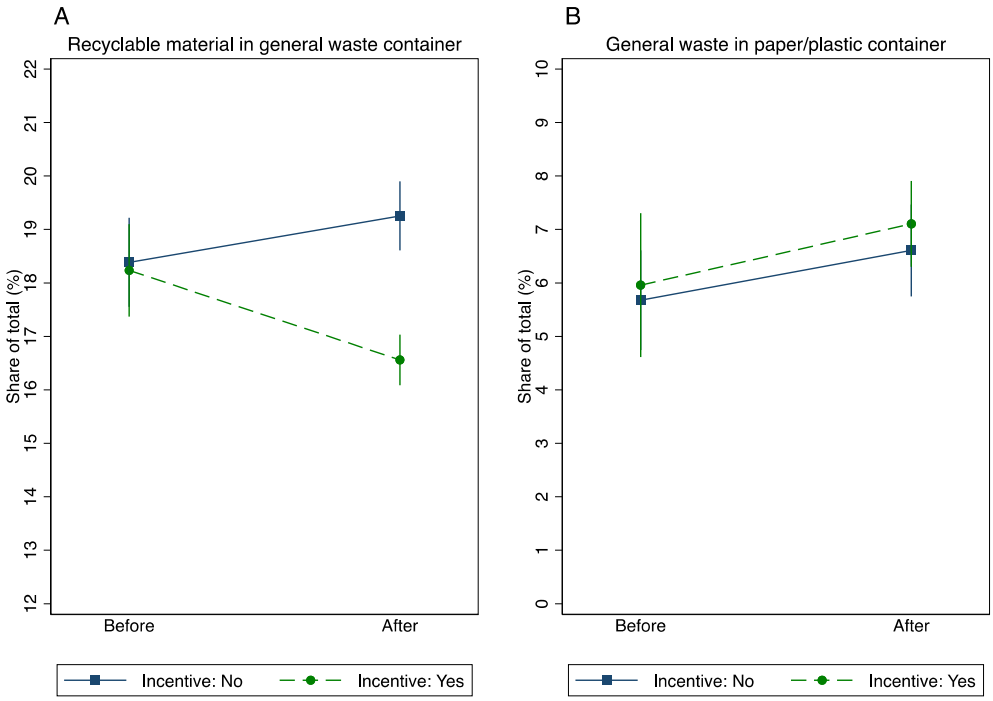

Fig. 3. The incentive treatment effect on recycling quality. The graph shows the share of recyclable material in the general waste container (panel A), and the share of general waste in the paper and plastic containers (panel B), before and after the incentive was introduced separately for the groups exposed to the incentive or not. Error bars indicate +/- one standard error.

Attitudes. The endline survey asked several questions capturing additional self-reported aspects of recycling behavior and household attitudes. Fig. 4 provides a summary of the standardized incentive treatment effects on these outcome variables ten months after. First, we note that there is a positive treatment effect on self-reported recycling (P = .033). Second, when asking explicitly about what kind of recycling activities people did more (or less) of compared to previous years, there is a significant positive treatment effect on sorting out paper and plastic (P = .035), while there is no detectable indication of the incentive causing a reduction in package consumption (P = .174), nor affecting the likelihood to throw trash elsewhere (P = .216) or visiting recycling centers (P = .418). Hence, our survey results are in line with the behavioral effects documented in the objective data, and supportive of the intended process of people becoming better at removing paper and plastic from the general waste.

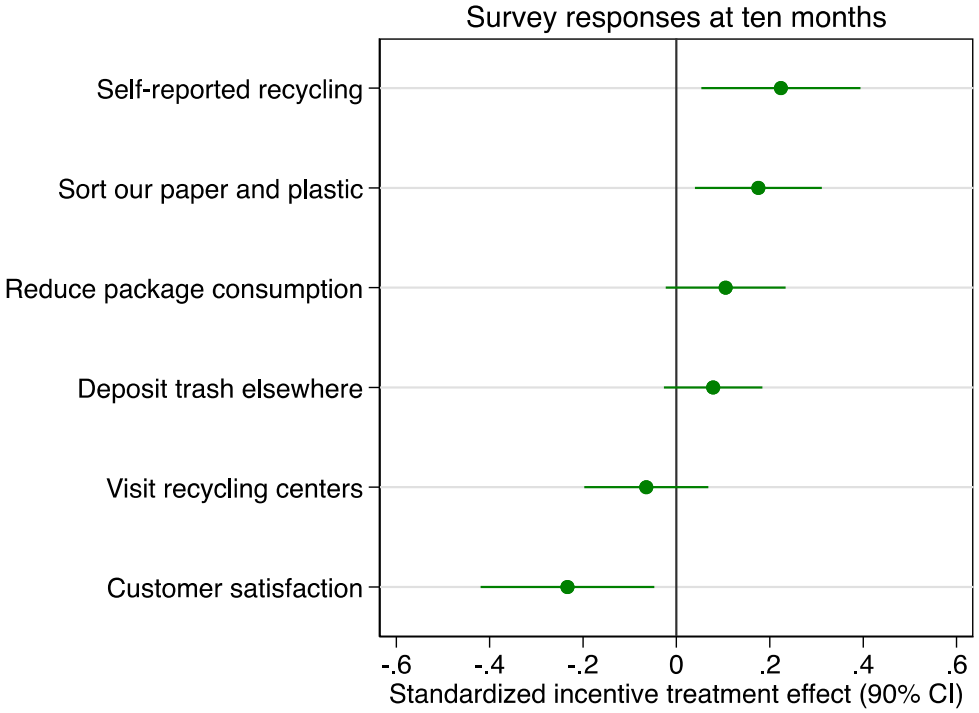

Fig. 4. The incentive treatment effect on households self-reported recycling behavior and satisfaction with the waste management company. The graph shows the standardized incentive treatment effects for six separate questions asked in the endline survey, ten months after the intervention. The treatment effects are estimated using simple OLS regression, comparing the two incentive groups jointly against the combination of the control and the norm group, with standard errors clustered by condominium.

As a final remark, however, the incentive also caused a considerable drop of more than 0.2 standard deviations in customer satisfaction with the waste management company (P = .043). When considering these effects separately by treatment group, there is a consistent pattern, where we again observe no effects of the norm-nudge, while the point estimates in the two incentive groups move in the same direction with one notable exception: The reduction in customer satisfaction is much larger (0.34 SD) and only statistically significant at our pre-specified alpha level (P< 0.10) in the pure incentive group (P = .067), whereas customer

satisfaction in the group that received the combined treatment – with both the incentive and the norm-nudge – remained at the same level as in the control group (P = .819; for details see table S5). That is, the price incentive appears to have had an unintended side-effect of reducing customer satisfaction, unless it was combined with the social norm-nudge.

Discussion

Economic price incentives and social norm-nudges are two commonly used and often recommended approaches to encourage pro-environmental behavior. However, which approach is more effective in a real-life field setting, and whether there are potential complementarities between them, have not been studied in prior research. We bridge this gap in the literature, reporting from a randomized controlled trial that directly tests how incentives and norm-nudges shape household waste recycling over a period of two years. The results reveal a large, immediate, and persistent positive effect of economic incentives on recycling – both in terms of objective weight data, and from waste quality audits that directly inspect the actual container content. In contrast, the norm-nudge had no detectable impact on recycling behavior, neither in the short nor in the longer term. Combining the two approaches did not generate more (or less) recycling behavior compared to the incentive itself, but a complementary household survey suggests that the norm-nudge alleviated the negative impact of incentives on self-reported customer satisfaction, thus contributing to making climate incentives more acceptable in society.

Our findings have both direct and indirect implications for public policy. First, the results provide robust scientific evidence in favor of using economic pay-as-you-throw incentives to stimulate recycling. Some cities in some countries already do – others should follow. Second, as the first study to compare incentives and norm-nudges in the same identical setting, we contribute to the debate on whether to use policies at the systemic level (e.g., incentives) or the individual level (e.g., norm-nudges) to address societal challenges. Clearly, one study alone cannot settle this debate, but our findings show that even a small economic incentive can vastly outperform a common nudging approach when it comes to promoting pro-environmental behavior, in the domain of recycling. Future research should examine the effectiveness and acceptability of similar interventions in other domains of climate behavior, such as food consumption habits and long-distance travel. Third, although both scientists and policymakers often advocate for climate incentives, such as a tax on CO_2-emissions, a key obstacle is to get them implemented in practice as they are unpopular among voters. Indeed, the price incentive was also disliked by the households in our study, but not when combined with the norm-nudge. Opening up a new avenue for nudging research, using social appeals can thus be a wise strategy to get public approval of more effective but unpopular policies, such as economic incentives, by highlighting that the targeted behavior is both common and morally appealing.

The current experiment design offers high internal validity and unique data on real-life behavior, but is not without limitations. To improve external validity, it would have been ideal to implement the experiment in more cities and countries, to learn whether cultural and contextual differences moderate the findings. That being said, the recycling rate in Norway is about 39 percent, which is close to the average in the European Union and comparable to many countries of the OECD (including the United States) thus increasing the relevance of our

results to stakeholders across the globe (*34*). The null-effect of the norm-nudge is also intriguing and could be worth exploring further. Appealing to social norms is a cornerstone of nudge theory, but a growing collection of studies has failed to demonstrate robust and significant effects on behavior change in natural settings (e.g., (*35*, *36*); for a review, see (*37*) or (*38*)). A recent online study showed, however, that people have pessimistic beliefs with respect to the pro-environmental behavior of others, and that correcting these beliefs can positively influence the willingness to act against climate change (*39*). Additional results from our household survey, reported in the supplementary material, confirm that people had pessimistic beliefs about the recycling behavior of others, but the social norm-nudge was not able to affect these beliefs (see fig. S2). We are unable to pinpoint exactly why the norm-nudge did not affect subjective beliefs, but policymakers are likely to meet the same hurdles: Changing expressed beliefs and one-shot pro-climate actions among attentive online study participants is one thing; getting the same message across when communicating with citizens in day-to-day life is likely to be more difficult. Economic incentives, on the other hand, seem to speak for themselves.

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positions, goals, or financial interests relate to this article. Neither does any partner or any close relative have such a position. This research was independent from funders: The funders had no role in study design; collection, analysis, and interpretation of data; writing of the report; or the decision to submit the article for publication. The authors declare that they have no conflicts of interest related to the research described in this study. **Data and materials availability:** To facilitate replication all data and code used in the analysis can be freely downloaded at https://osf.io/mtuh9/. We also preregistered the data collection and analysis (https://aspredicted.org/D8H_9YT).

Supplementary Material:
Creating pro-environmental behavior change: Economic incentives or norm-nudges?

The supplementary material consists of a Material and Methods section that describes the details of the experiment and the estimation strategy, and a set of supplementary tables and figures listed in separate sections.

Material and Methods

Below we present the details of the experiment. We start by describing the criteria that determined our sample, BIR's container system, and the timeline for the experiment. Then we describe the treatments and the randomization process. Finally, we explain the estimation strategy.

Sample, Infrastructure, and Timeline

Our field experiment was carried out in collaboration with the waste management company (BIR) in the city of Bergen, Norway. BIR mainly serves three types of households that have three slightly different types of recycling solutions: i) single-unit homes that use on-street garbage bins, ii) city center apartments connected to an underground waste pipe, and iii) condominiums outside the city center that use BIR's container-based system for waste collection. The last category of households is the target group for the current experiment.

For this category of households, BIR developed a new type of container system with two main features. First, households had to use a unique RFID key to open the condominium's container when deposing general waste. Second, there was a volume constraint of approximately 30 liters of trash per container opening. Hence, the system was adapted to enable billing households' depending on how much general waste they throw, as measured by the number of container openings per month, also referred to as a pay-as-you-throw system (*31*).

For our experiment, BIR identified all condominiums where they could enforce a change to the new container system without consent from the condominium board. This choice had two main advantages. First, it was easier and faster to implement the study. Second, and more importantly, there is no possibility of self-selection of households into the study, as condominiums did not have to approve of the change. The downside is that the sample is smaller than it potentially could have been. That said, BIR was able to identify 42 condominiums to be included in the study, which were scattered across the city of Bergen and covered more than 2,000 households (see table S6 for summary statistics of our sample).

By September 1, 2021, all 42 condominiums had the new container system in place. However, our treatments were not communicated to households until January 2022, meaning that the first 4 months of our experiment serves as a baseline pre-treatment period where all households are accustomed to the new system and treated equally (see table S7 for the timeline of the experiment). It should also be mentioned that some condominiums, for practical reasons, have the containers placed on a surface lot above ground, while others have the actual containers placed below ground and only the opening to the container accessible from above (see fig. S3 for pictures of both container types, as well as the key-accessed container opening system).

Treatments and randomization

We measure recycling quantity and quality at the condominium-month level. We therefore randomized the 42 condominiums, not households, to one of the four treatment conditions. To improve comparability of condominiums across treatments, we stratified randomization based on the type of container system (above or below ground) and the baseline level of recycling quality (high, medium, low). Importantly, as we use a difference-in-difference approach to estimate the causal effects, any baseline differences across treatments will be accounted for in the empirical analysis (*32*).

There were four different treatment conditions in the study (*Control*; *Norm*; *Incentive*; *Incentive and Norm*), and the specific treatment information was communicated to households via postal mail in January 2022. At this time, all households, including those in the *Control* group, received a letter which contained generic information about recycling and a guide on how to sort different packaging materials. Households in the *Incentive* group were also informed about an economic incentive effective from February 1, 2022 (details below). Households in the *Norm* group were exposed to an additional paragraph in the letter stating both an injunctive and descriptive norm of recycling (details below). Households in the *Incentive and Norm* group were exposed to both pieces of additional information. This design ensures that there is nothing else than the incentive and/or norm-nudge that varies across households in the different condominiums.

The Incentive: The economic incentive was determined by BIR and had the same structure as the incentive already in place for households in city center apartments connected to the underground waste pipe. Each household has a fixed number of general waste container openings per month free of charge, but after reaching that threshold the household must pay 9.99 NOK (≈1USD) for every additional opening. Since the volume constraint differed slightly depending on the type of container (above- or below-ground), households in condominiums with above-ground containers had eight free openings per month, whereas households in condominiums with below-ground containers had five free openings per month. Notice that using either the paper or the plastic container was always free of charge.

The Norm-nudge: The social norm-nudge was inserted as the first paragraph in the general letter about recycling sent to all households in January 2022. The social norm-nudge clearly stated both an injunctive norm ("Recycling is important. A higher recycling rate implies more efficient use of the world's resources. This leads to lower carbon emissions and a sustainable future.") and a descriptive norm ("Do as 9 out of 10 Norwegians: Sort out paper and carton, as well as glass-, metal- and plastic packaging – such that everyone can benefit from a better environment."). We also included a sticker with a smiling whale to place on the RFID key to serve as a reminder about the norm-nudge.

Data sources

We use four data sources in our empirical analysis: container openings, recorded via RFID-keys; weight of the container waste, recorded automatically during the scheduled collection by BIR's garbage trucks; recycling quality, recorded in waste audits conducted by an external contractor; customer attitudes, recorded in electronic surveys distributed to households via

BIR's survey partner. Below is a detailed description of each data source and the respective outcome variables.

Container openings: From the RFID-key data, we can track all container openings of a household. From this data we generate a balanced panel data set at the month-condominium level. The key outcome variable is "Number of openings per household", which captures the average number of openings of the general waste container in a given condominium and month. Since BIR was unable to extract the RFID-key data for six of the 42 condominiums (1 Control; 2 Norm; 2 Incentive, 1 Incentive and Norm), N = 576 (16 months and 36 condominiums). In addition, BIR was unable to record RFID-key data for three condominiums during the first three months of the study (September to November 2021). In the main analysis, we impute these missing values with the mean value from the remaining baseline period for the respective condominium. As a robustness check we also report the main results without the imputed values in table S1.

Weight of waste: BIR's garbage trucks collects the waste from the condominiums' containers on a regular basis. Every time a container is emptied, the weight of the waste is automatically recorded. In total, this data included 3701 unique waste collections from general waste containers. From this data we generate a balanced panel data set at the month-condominium level. The key outcome variable is "Kg general waste", which is the sum of general waste collected for a given month and condominium. Occasionally, the system failed to report the weight of the general waste container on a specific pick-up date, resulting in a missing value. There were in total 49 missing weight observations of the general waste. In the main analysis, we impute these missing values with the mean weight for the specific container ID in the specific month, before calculating the monthly sum of "Kg general waste". In three instances there was not a single recorded weight observation in the specific month, resulting in a missing value for the entire month, in which case we replaced it by the mean monthly value in the baseline period or treatment period respectively, depending on when the missing monthly observation occurred. For the condominiums using a below-ground container system we also have data on the weight of the paper and plastic containers, and thus also the total weight of waste collected per month (i.e., general + paper + plastic). Although this data only covers half of the sample, we use it to shed light on the underlying mechanisms. The recorded weight of the paper and plastic containers also included missing observations and we handled these cases in the same way as explained above. In total, the paper and plastic data included 1370 unique waste collections, out of which 43 were missing (26 paper; 17 plastic). As a robustness check we also report the main results without imputing the missing values in table S3.

Recycling quality: To measure recycling quality, BIR hired an external contractor to perform so-called waste audits. In a waste audit, a sample of waste is taken from a specific container and then manually sorted according to which container/material it *actually* belongs to. As such, waste audits can identify how much of the total waste in a container that is correctly sorted, and how much of the waste that ideally should have been deposited elsewhere. Waste audits are labor intensive and time consuming, so it was not possible to have monthly quality checks. Instead, we had three rounds of waste audits: one in the pre-treatment period (November 2021), one in the beginning of the post-treatment period (March 2022), and one at the end of the post-treatment period (November 2022). For each condominium and round,

the waste auditor investigated three separate 100kg samples of waste: one from the general waste container, one from the paper container, and one from the plastic container. From these samples we calculated the two outcome variables capturing recycling quality: "Share recyclables in general waste", which is the sum of all recyclable material identified in the general waste container (in kg) divided by the total weight of the general waste sample, and "Share general waste in paper and plastic", which is the sum of all material not supposed to be in the paper or plastic container divided by the total weight of the paper and plastic sample (notice that paper and plastic too dirty to recycle is classified as general waste in these waste audits). With 42 condominiums and three rounds of audits we have a balanced panel data set at the round-condominium level (N = 126). In the last round of audits the contractor was unable to extract a sample from one condominium's plastic and paper container, so in that one case we impute the missing value for the variable "Share general waste in paper and plastic" using the condominium's value from the other round in the treatment period. As a robustness check we also report the main results without imputed values in table S4.

Customer attitudes: In addition to the objective data on recycling behavior, we surveyed households at three points in time: in the pre-treatment period (December 2021), in the beginning of the post-treatment period (March 2022), and at the end of the post-treatment period (November 2022). The three surveys had three slightly different purposes. The first survey (N = 1006) measured key demographics in our sample, as well as baseline knowledge and attitudes with respect to recycling (see table S6). The second survey (N = 735) served as an attention check asking specific questions about the content in the information letter that was sent to households in January (see table S8 for key take aways). The third survey (N = 906) was included to identify potential treatment effects on attitudes towards recycling as well as potential behavioral adaptations not possible to capture in the objective data (see Fig. 4 and table S5). All three surveys were implemented by BIR's survey provider and distributed to the household head via text message.

Estimation strategy

The panel structure of our objective data enables us to use a difference-in-difference approach to estimate casual treatment effects. To be specific, we estimate OLS regressions of each outcome on a set of treatment dummies labeled *Norm*, *Incentive*, and *Incentive & Norm* which are equal to one for condominiums in the respective treatment group (and zero otherwise); a dummy variable labeled *After* equal to one for observations in the post-treatment period (and zero if in the pre-treatment period); and the interactions between each treatment dummy and *After.* The coefficient of each interaction term captures the causal effect of that specific treatment relative to the control group, and it is these estimates that are provided in the regression tables. The specification also allows us to test whether the effect of the *Norm* treatment is significantly different from the *Incentive* treatment, and whether combining the two treatments generates any additional impact over and above the two treatments individually.

As described in our pre-analysis plan we also use a more powerful approach to estimate the Incentives treatment effect, by collapsing the two groups exposed to the economic incentive into a joint incentive treatment group and compare it to the two groups not exposed to the economic incentive as a joint control group. The additional power we gain using this estimation strategy is particularly valuable for outcomes we only have for a subset of the

sample (i.e., paper and plastic weight) and outcomes measured with less frequency (i.e., recycling quality).

To estimate treatment effects on self-reported attitudes we simply regress outcomes measured in the endline survey on the three treatment dummies *Norm*, *Incentive*, and *Incentive & Norm* (or the joint Incentive dummy). For the self-reported outcomes where baseline survey data is available, we adjust the estimates to account for the pre-existing mean differences across treatments.

In all estimations we cluster standard errors at the condominium level to account for potential correlation within condominiums and over time (*33*). As pre-registered, we use 0.10 as the alpha-level for statistical significance.

Supplementary Figures

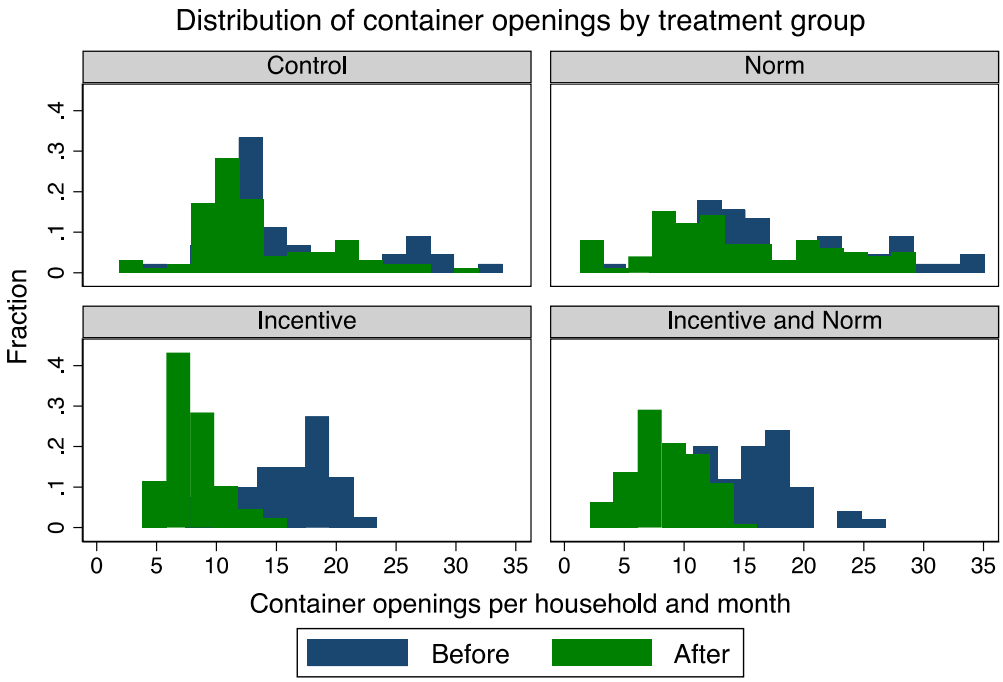

Fig. S1: Distribution of container openings by treatment group and period. The graph presents histograms of the number of container openings per household and month, pre- and post-treatment. There is clear overlap in the two distributions in the treatment groups not exposed to the incentive, while the incentive shifts the distribution to the left and makes it less dispersed.

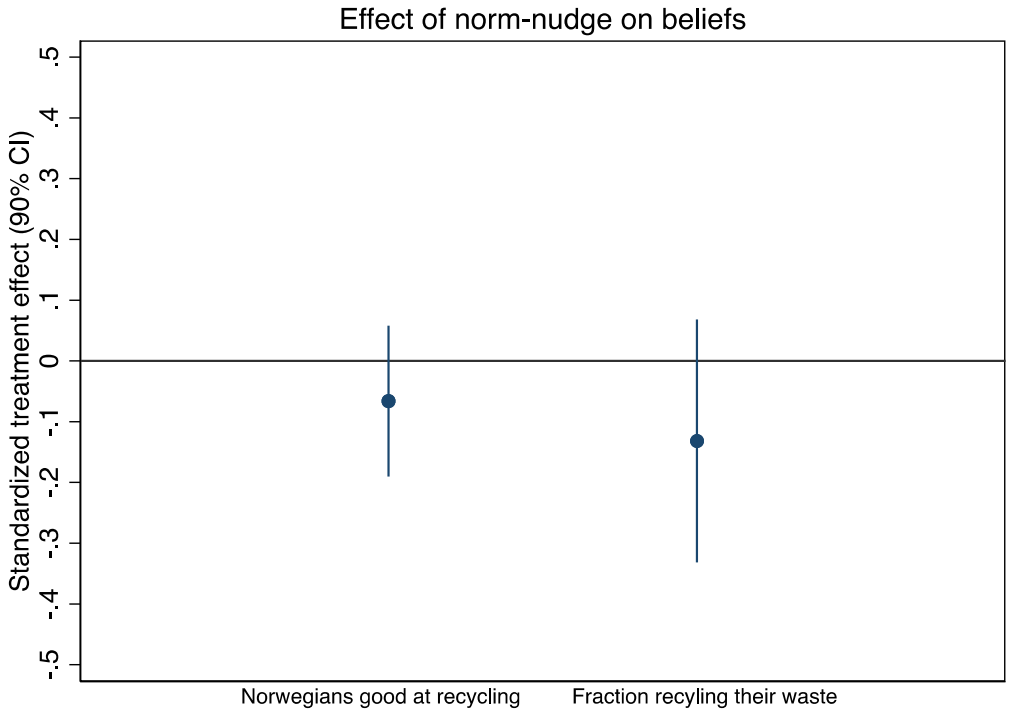

Fig. S2: The treatment effect of the norm-nudge on subjective beliefs. The graph shows the standardized treatment effect of the norm-nudge on two separate questions asked in the second survey, conducted one month after treatment exposure: "How good are Norwegians at recycling?" (0-10; 0=Not good at all, 10=Extremely good); "As a fraction, how many Norwegians do you think recycle their waste?" (0-10, 0=No one, 10=Everyone). The treatment effects are estimated using simple OLS regression, comparing the two groups exposed to the norm-nudge against the two groups not exposed to the norm-nudge. Standard errors are clustered by condominium.

A.

B.

C.

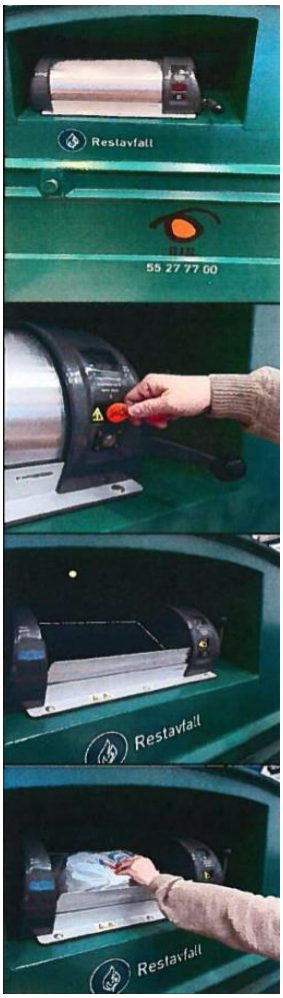

Fig. S3: The containers and key card system. Panel A provides a picture of the above-ground containers. Panel B is a picture of the below-ground containers. For the entire study period, and for all treatment groups and container types, a key card was required to throw trash in the general waste container, shown in Panel C (the general waste container had a volume constraint of roughly 30 liters per opening). Source: BIR AS.

Supplementary Tables

Table S1: Treatment effects on container openings.

	Outcome variable: Openings per household and month			
	(1)	(2)	(3)	(4)
Norm only	-0.726		-0.689	
	(0.512)		(0.551)	
Incentive only	-4.718***		-4.381***	
	(0.001)		(0.004)	
Incentive & Norm	-4.552***		-4.530***	
	(0.000)		(0.000)	
Incentive		-4.263***		-4.137***
		(0.000)		(0.000)
Baseline mean	13.48	13.86	13.48	13.86
P: Incentive only = Norm only	0.018		0.044	
P: Incentive & Norm = Norm only	0.009		0.010	
P: Incentive only = Incentive & Norm	0.899		0.925	
N	576	576	570	570

Notes: The table reports the treatment effects from OLS panel regressions, accounting for any difference in pre-intervention levels across treatment groups. Column 1 reports the effect of each treatment separately against the control group. Column 2 report the treatment effect of being in one of the two incentive groups compared to being in one of the two groups not exposed to incentive. Columns 3 and 4 report the results from the same regressions without imputation of missing values. *P*-values based on standard errors clustered at the condominium level are provided in parenthesis.
* *P* <0.1, ** *P* <0.05, *** *P* <0.01

Table S2: Treatment effects on container weight in the subsample with below ground container.

	Outcome variable: (kg per condominium and month)		
	General waste	Paper and Plastic	Total waste
	(1)	(2)	(3)
Norm only	-13.50	-59.28**	-72.78
	(0.896)	(0.034)	(0.504)
Incentive only	-208.4*	23.98	-184.4*
	(0.057)	(0.261)	(0.080)
Incentive & Norm	-76.72	18.00	-58.72
	(0.401)	(0.465)	(0.492)
Baseline mean	975.39	352.13	1327.52
P: Incentive only = Norm only	0.160	0.001	0.447
P: Incentive & Norm = Norm only	0.574	0.001	0.931
P: Incentive only = Incentive & Norm	0.309	0.786	0.301
N	352	352	352

Notes: The table reports the treatment effects from OLS panel regressions, accounting for any difference in pre-intervention levels across the treatment group and control group. *P*-values based on standard errors clustered at the condominium level are provided in parenthesis.

* $P<0.1$, ** $P<0.05$, *** $P<0.01$

Table S3. Treatment effects on the amount of waste without imputation of missing values.

	Outcome variable: (kg per condominium and month)			
	General waste	General waste	Paper and Plastic	Total waste
	(1)	(2)	(3)	(4)
Norm only	-37.49			
	(0.604)			
Incentive only	-190.5***			
	(0.007)			
Incentive & Norm	-120.7*			
	(0.052)			
Incentive		-135.4**	56.27**	-59.08
		(0.010)	(0.012)	(0.548)
Baseline mean	964.63	1050.57	312.50	1491.80
P: Incentive only = Norm only	0.072			
P: Incentive & Norm = Norm only	0.291			
P: Incentive only = Incentive & Norm	0.344			
N	669	669	347	344

Notes: The table reports the treatment effects from OLS panel regressions, accounting for any difference in pre-intervention levels across treatment groups. Column 1 reports effect of each treatment separately against the control group. Column 2-4 reports the treatment effect of being in one of the two incentive groups compared to being in one of the two groups not exposed to incentive. *P*-values based on standard errors clustered at the condominium level are provided in parenthesis.

* $P<0.1$, ** $P<0.05$, *** $P<0.01$

Table S4: Treatment effects on recycling quality.

	Outcome variable:					
	Share recyclables in general waste		Share general waste in paper/plastic		Share general waste in paper/plastic	
	(1)	(2)	(3)	(4)	(5)	(6)
Incentive	-0.025**		0.002		0.002	
	(0.020)		(0.912)		(0.925)	
Norm only		0.000		0.011		0.011
		(0.991)		(0.632)		(0.655)
Incentive only		-0.039**		0.029		0.029
		(0.012)		(0.240)		(0.259)
Incentive & Norm		-0.013		-0.0112		-0.012
		(0.399)		(0.712)		(0.705)
Baseline mean	0.193	0.200	0.066	0.062	0.066	0.063
P: Incentive only = Norm only		0.009		0.457		0.457
P: Incentive & Norm = Norm only		0.383		0.442		0.442
P: Incentive only = Incentive & Norm		0.103		0.171		0.171
N	126	126	126	126	125	125

Notes: The table reports the treatment effects from OLS panel regressions, accounting for any difference in pre-intervention levels across treatment groups. Columns 1, 3 and 5 report the treatment effect of being in one of the two incentive groups compared to being in one of the two groups not exposed to incentive. Columns 2, 4 and 6 report the effect of each treatment separately against the control group. Columns 5-6 repeat the analysis of column 3-4 without imputation of missing values. *P*-values based on standard errors clustered at the condominium level are provided in parenthesis.

* *P* <0.1, ** *P* <0.05, *** *P* <0.01

Table S5: Effects on survey outcomes separately by treatment group.

	Outcome variable (standardized)					
	Good at recycling	Sort out paper & plastic	Reduce package consumption	Deposit trash elsewhere	Visit recycling centers	Customer satisfaction
	(1)	(2)	(3)	(4)	(5)	(6)
Norm only	0.231	-0.091	-0.000	-0.050	0.116	0.187
	(0.181)	(0.317)	(0.999)	(0.612)	(0.343)	(0.148)
Incentive only	0.270*	0.071	0.142	0.098	0.060	-0.344*
	(0.062)	(0.606)	(0.305)	(0.317)	(0.607)	(0.067)
Incentive & Norm	0.403**	0.200*	0.085	0.009	-0.073	0.039
	(0.012)	(0.085)	(0.426)	(0.927)	(0.608)	(0.819)
N	821	821	821	821	821	821

Notes: The table reports the treatment effects from OLS regressions on six (standardized) outcome variables measured in the endline survey, administered ten months after the intervention. *P*-values based on standard errors clustered at the condominium level are provided in parenthesis.

* *P* <0.1, ** *P* <0.05, *** *P* <0.01

Table S6: Sample demographics.

		Treatment group				
	Overall	Control	Norm	Incentive	Incentive & Norm	*P*-value (*F*-test)
Female (1/0)	0.553	0.526	0.549	0.553	0.579	0.577
Born in Norway (1/0)	0.919	0.947	0.896	0.895	0.941	0.109
University degree (1/0)	0.497	0.390	0.575	0.502	0.505	0.040
Age <35 (1/0)	0.266	0.211	0.302	0.278	0.267	0.546
Age 35-55 (1/0)	0.272	0.167	0.373	0.295	0.242	0.011
Age >55 (1/0)	0.461	0.623	0.325	0.426	0.491	0.022
Correct answers recycling quiz (0-9)	6.329	6.232	6.340	6.435	6.308	0.505
Recycling important for the environment? (0-10)	8.238	8.382	8.134	8.262	8.198	0.632
How good are you at recycling? (0-10)	7.422	7.675	7.369	7.388	7.293	0.520
How good is the typical Norwegian at recycling? (0-10)	5.338	5.579	5.272	5.257	5.271	0.011
N	1006	228	268	237	273	

Notes: The table provide sample means, overall and by treatment group, for a set of questions asked in the baseline survey. *P*-values are from a joint orthogonality test of the four treatment groups, with standard errors clustered at the condominium level.

Table S7: Timeline of experiment.

Period	Month	Event
	Aug. 2021	New containers installed and key cards received
Pre-treatment	Sep. 2021	Start of data collection (container openings and container weight)
	Nov. 2021	Baseline waste audit and first household survey
	Jan. 2022	Treatment assignment: Residents informed in general letter
Post-treatment	Feb. 2022	First month with incentive in place
	Mar. 2022	Short term waste audit and second household survey (attention check)
	Nov. 2022	Long term waste audit and household survey
	Dec. 2022	End of data collection (container openings and container weight)

Notes: The table highlights the timing of key events in the experiment, separated into a pre- and post-treatment period. The waste audits and household surveys took approximately one month to complete. Notice that specific treatment information was communicated in a general letter about recycling that was sent to all households in the study (including the control group).

Table S8: Attention check.

	Percent of subjects that...				
Question	Gave correct answer	Gave incorrect answer	Did not remember	Total	*N*
Attachment	67.89	3.81	28.30	100	735
Free openings	74.12	19.12	6.76	100	340
Payment	63.82	16.18	20.00	100	340
Logo	35.22	3.32	61.46	100	301

Notes: The table provide the distribution of answers to four questions asked in the midterm survey capturing whether people remembered facts provided in the information letter that we sent out in January. *Attachment* was a question about the correct attachment to the letter (a recycling guide); *Free openings* was a question about the number of container openings included in the fixed fee (8 or 5 depending on condominium); *Payment* was a question about the cost per opening for every additional opening (9.99 NOK); *Logo* was a question about the animal appearing on the key card sticker (a whale). The number of observations vary by question since some questions were asked to all participants (*N* = 735), some were asked only to the incentive and the combined group (*N* = 340), and some were asked only to the norm-nudge and the combined group (*N* = 301).

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