Journal of Environmental Economics and Management 127 (2024) 103037

Contents lists available at ScienceDirect

Journal of Environmental Economics and Management

journal homepage: www.elsevier.com/locate/jeem

Widening the scope: The direct and spillover effects of nudging water efficiency in the presence of other behavioral interventions

J. Bonan [a,b,f], C. Cattaneo [b,f,*], G. d'Adda [c,b,f], A. Galliera [d], M. Tavoni [d,b,e,f]

[a] Università di Brescia, Italy
[b] CMCC Foundation - Euro-Mediterranean Center on Climate Change, Italy
[c] Universita degli Studi di Milano, Italy
[d] Università Cattolica del Sacro Cuore (Milano), Italy
[e] Politecnico di Milano, Italy
[f] RFF-CMCC European Institute on Economics and the Environment, Italy

ARTICLE INFO

JEL classification:
Q5
Q25
D9

Keywords:
Social information
Spillover effects
Resource conservation

ABSTRACT

Policymakers and firms use behavioral interventions to promote sustainable development in various domains. A correct impact evaluation requires looking beyond the targeted domain and assessing its interactions with similar interventions. Existing evidence in this area is limited, leading to potential misestimation of behavioural interventions and poor guidance on their design. Here, we test the impact of a two-year social information campaign to nudge water conservation through a large-scale randomized controlled trial implemented with a multi-resource company,. We find that the water nudge significantly decreases water and electricity usage, but not that of gas. Spillovers arise for customers who do not receive nudges targeting the other resources. Customers receiving the water report are also significantly less likely to deactivate their gas and electricity contracts, regardless of whether they receive other reports. Our results suggest that multiple nudges strain users' limited attention and ability to enact conservation efforts. Users' constraints in attending to multiple stimuli need to be accounted in designing policy interventions to foster sustainable practices.

1. Introduction

Promoting sustainable development practices requires fostering behavioral change in various domains, which have different impacts and costs. Behavioral interventions, such as nudges, have been used at large by governments and businesses to promote pro-environmental behaviors among citizens and customers. However, their impact is typically evaluated in a narrow sense. First, most research focuses on the outcome directly targeted by the intervention, ignoring potential spillover effects to related behaviors. Second, impact evaluations focus on consumption, but customer satisfaction and retention are equally, if not more, important outcomes from both policy and business perspectives. The impact of nudges may be reduced if they induce avoidance behavior, which is also a sign of their negative welfare effects. Finally, little evidence exists on the effectiveness of these interventions when similar ones simultaneously target their recipients. For a correct evaluation and effective design of sustainable nudges, it therefore matters whether the behavioral change induced in one domain has positive or negative spillovers in other domains; whether these interventions alienate customers, possibly diverting them towards companies that are less focused on promoting sustainable conservation practices; and whether the combined effect of nudges is smaller or larger than the impact of each in isolation. These

* Corresponding author.
E-mail address: cristina.cattaneo@cmcc.it (C. Cattaneo).

https://doi.org/10.1016/j.jeem.2024.103037
Received 28 December 2023
Available online 4 July 2024
0095-0696/©

considerations are timely and relevant, given policymakers' and firms' expanding use of behavioral nudges and the resulting increase in the likelihood that consumers are exposed to multiple, possibly overlapping interventions.

We address these questions in the context of a social information program for water conservation. We leverage the relationship with a large multi-utility company providing water, electricity, and gas to its customers. Through a large-scale randomized controlled trial (RCT), water customers receive a report with information about their water usage, social comparison with neighbors' usage, and tips for conservation. We evaluate the direct impact of the report on water consumption and the indirect impacts on electricity and gas. We also study the program's impact on customer engagement and retention to measure the implications not just for resource usage but also for customer satisfaction, which is related to individual welfare effects. We exploit the variation in customers' receipt of similar reports targeting other resources, i.e., electricity and/or gas. We stratify assignment to the water report based on the other reports received by customers and assess whether receiving multiple reports influences the effectiveness of the water report. Finally, we test the heterogeneity in treatment effects across pre-specified dimensions, i.e., baseline usage and engagement.

Following our pre-analysis plan (PAP), we find that the water report significantly decreases water usage by 1.4 percent and electricity usage by 0.5 percent but has no significant impact on gas over two years. The magnitude of the spillover effect on electricity is comparable to the direct one of similar programs in Europe (Bonan et al., 2020), and is in line with what could be predicted from the baseline correlation between water and electricity consumption. The water report also leads to higher customer retention. Treated gas and electricity customers are 2.8 and 3 percent less likely to deactivate their contracts than control ones.[1] The results on consumption are robust to multiple hypothesis corrections and changes in the sample to account for attrition due to contract deactivation.

The impact of the behavioral program is driven by customers for whom the water report is the only one received: within this group, the program reduces water and electricity usage by 2.4 and 1.7 percent, respectively. The program's impact on customers receiving other reports at baseline is not statistically significant. The positive impact of nudging on customer retention is independent of whether they receive other reports. We argue that the lack of effect of the water report, when it is added to other reports, is partly due to users' limited ability to attend to multiple stimuli. In particular, the water report reduces the attention that users pay to each single report, limiting the conservation gains that they can achieve in each domain. One way to reduce the cost of attending to multiple reports and thus increase their effectiveness is to send the reports jointly. Among customers receiving multiple reports, we find that conservation is larger if reports are sent simultaneously.

Our results on contract cancellation and engagement indicate that the additional report does not generate negative customer reactions, even though it is ineffective at fostering own and cross-resource conservation.

The interpretation of the heterogeneous effect of the water report by receipt of other reports crucially depends on whether household characteristics simultaneously affect the likelihood of receiving multiple reports and the reaction to this treatment. Our analysis controls for time-invariant household traits that may affect selection into multiple reports through household fixed effects. To further address concerns of time-varying confounders, we exploit information on the contracts offered by the utility to identify samples for whom concerns about self-selection into multiple reports are less relevant. The heterogeneous effects of multiple reports are robust to using these restricted samples. We therefore exclude that the heterogeneous effects are entirely driven by self-selection.

Our study adds to a growing literature that evaluates the effectiveness of social information programs and feedback on resource conservation (Allcott et al., 2011; Allcott and Rogers, 2014; Ayres et al., 2013; Tiefenbeck et al., 2016, 2019; Fang et al., 2023).[2] Several experimental studies have specifically looked at the direct impact of social information about water usage on water consumption, mainly in the U.S. context (Ferraro and Price, 2013; Ferraro et al., 2011; Ferraro and Miranda, 2013; Bernedo et al., 2014; Brent et al., 2015; Hodges et al., 2020). The evidence documents short-term water conservation effects of up to 5 percent. The effect can persist over longer time horizons, although it is 50 percent smaller after only one year (Ferraro et al., 2011; Bernedo et al., 2014).[3] These effects are attributable to short-lived behavioral adjustments and more persistent changes in habits and physical capital. More recently, Jessoe et al. (2021) use high-frequency water consumption data to evaluate a home water program in California during a drought period. They find a 4–5 percent reduction in water usage, but the effect dissipates over five months.

Our paper contributes to this literature in different ways. First, relatively few papers rigorously address the spillover effects of a social information report on the consumption of other resources. Jessoe et al. (2020) examine cross-sectoral spillover using one-year post-treatment data on water and electricity usage in the United States. They find that home water reports induce a 1–2 percent reduction in summertime electricity use, which disappears by 4–5 months post-treatment. Carlsson et al. (2020) find that a social information campaign on water use had a positive and sizeable spillover effect on electricity usage for households experiencing positive direct effects. Goetz et al. (2022) evaluate the effects of a hot-water-saving intervention and find persistent direct and

[1] Although the gas and electricity retail markets are liberalized, and customers can freely choose their providers, the water market is regulated, and customers cannot change providers.

[2] See Gillingham et al. (2018), Abrahamse (2019), and Gerarden et al. (2017) for a broader discussion of the energy efficiency gap and the assessment of energy efficiency policies. An extensive literature also evaluates social information programs in several other domains, from contributions to charitable causes (Frey and Meier, 2004; Shang and Croson, 2009), to technology adoption (Bonan et al., 2021a; Gillingham and Bollinger, 2021), voting (Gerber and Rogers, 2009), waste disposal (Bonan et al., 2023), and financial decisions (Beshears et al., 2015). More broadly, reviews of information-based interventions on residential customers' resource consumption can be found in Nemati and Penn (2020) and Delmas et al. (2013).

[3] In developing contexts, Miranda et al. (2020) find 3–5 percent effects in Costa Rica, while Jaime Torres and Carlsson (2018) find a 6.8 percent water reduction among customers targeted by a home water report and a 5.5 percent decrease on untargeted customers living close-by (cross-individual spillovers) in Colombia.

spillover effects on dishwasher use and toilet flushing but no effect on electricity.[4] Our paper evaluates spillover effects on a broader set of behavioral outcomes, namely electricity and gas usage, and over a more extended period (two years after treatment) allowing us to disentangle considerations of persistence of the effects from seasonality in resource usage.

Second, we evaluate the impact of the water report on customers' retention and engagement. These aspects are crucial for businesses in this sector and policymakers interested in the welfare impacts of these programs. After the liberalization of energy markets, many studies have analyzed household contract-switching choices (or lack thereof) and underlined the roles of both price and non-price attributes (Hortaçsu et al., 2017; Shin and Managi, 2017; Fontana et al., 2019; Schleich et al., 2019). Brent et al. (2015) examine whether a social comparison intervention affects other utility conservation programs, such as free home water audits and rebates for efficient toilets or irrigation controllers. They find that receiving the home water report increases program participation. Far smaller effects are found by Allcott and Rogers (2014). However, the role of customized pro-environmental information campaigns on customer retention appears unexplored, despite its importance for business and society. In our setting, reducing churn was a key objective of our partner utility, which faced yearly contract deactivation rates of 10.5 and 11.5 percent in the liberalized gas and electricity markets, respectively.[5] Our result of lower deactivation of gas and electricity contracts following the water report provides the first experimental evidence of the role of green nudges in boosting customer experience and loyalty.

Third, we assess the effect of receiving multiple nudges. Relatively few studies have tackled this issue and combined different nudges within the same intervention.[6] Yet, this question is relevant for policymakers and businesses as they target a variety of information campaigns to the same behavioral outcomes, often through multiple channels (Montaguti et al., 2016). Whether the cumulative effect of multiple nudges is larger or smaller than the sum of each in isolation is an open empirical question. The marginal effect of additional energy conservation nudges may decrease if the first one has already induced a reduction in consumption. An established finding in this literature is that the impact of nudges decreases as the margins for reduction shrink, even backfiring for low users (Byrne et al., 2018; Bhanot, 2017; Bonan et al., 2020). Similarly, willingness to pay to receive social information nudges, similar to the one we study, is lower among low users (Allcott and Kessler, 2019). Alternatively, recipients may be less attentive to additional nudges if cognitive constraints limit the amount of information they can absorb (Gigerenzer and Gaissmaier, 2011) or if they try to avoid the social pressure of receiving many nudges, as demonstrated by the literature on information and ask avoidance (Andreoni et al., 2017; Exley and Petrie, 2018; Adena and Huck, 2020; Serra-Garcia and Szech, 2022; Golman et al., 2022). This might lead to a backlash against the company and a societal loss arising from additional resource usage. Conversely, multiple nudges may increase individuals' awareness of existing synergies between behaviors, heighten the salience of environmental conservation motives (Bonan et al., 2021b), and reassure about a firm's commitment to sustainable development rather than mere greenwashing.

Previous works have looked at the interaction of different nudges in influencing one or more outcomes within the same behavioral sphere, e.g., electricity usage (Hahn et al., 2016; Brandon et al., 2019; Bonan et al., 2020, 2021b; Fang et al., 2023). The impact of nudge interactions appears heterogeneous and increases with the ability to target relevant and consistent sources of bias effectively. We contribute to this nascent literature by providing evidence on the heterogeneous impact of a report depending on the receipt of other, similar reports. Unlike previous work, we look at the impact of the same nudge targeted to different behavioral spheres, i.e., water, gas, and electricity usage. We provide evidence that multiple nudges deplete consumers' limited attention towards the different resources.

The remainder of the paper is organized as follows. Section 2 describes the setting of the study. Section 3 provides details of the experimental design of the RCT. Section 4 describes the sample and data, and Section 5 presents the empirical strategy and results in detail. Section 6 concludes.

2. Setting

We collaborate with a large multi-utility company that manages the supply of energy, water and environmental services, as well as public lighting and telecommunications to citizens and businesses, which serves 4.3 million customers in 330 municipalities, mainly located in the center-north of Italy, specifically in the regions of Emilia-Romagna, Veneto, Friuli-Venezia Giulia, Marche, Tuscany, and Abruzzo. Our study focuses on customers of water services located in Emilia-Romagna.[7]

The water market in Italy is regulated at the national level by ARERA (Autorita' di Regolazione per Energia Reti e Ambiente). Tariffs are established by utilities at the municipal level to cover operating costs, investments, and financial and tax charges but must be approved by ARERA. The gas and electricity markets were liberalized in 2007. The liberalization of these markets was slow, with over 42 percent of domestic customers still buying their energy at the conditions set by the public authority for energy as of 2021 (ARERA, 2022). Complete transition to a free market was postponed several times but was finally completed in January

[4] Other papers look at spillovers in waste disposal and recycling (Ek and Miliute-Plepiene, 2018; Alacevich et al., 2021; Sherif, 2021). Beyond this small number of studies, literature exists on spillovers in the environmental domain, with mixed evidence. Such variability in results can be partially explained by the significant differences in the methods used to quantify impacts (Galizzi and Whitmarsh, 2019) and to measure outcomes–ranging from behavioral intentions to policy support, self-reported and actual behaviors–(Maki et al., 2019).

[5] At the national level, yearly contract switching in the electricity sector was 15.7 percent (ARERA, 2022).

[6] Several papers have focused on the interaction between nudges and economic incentives in different contexts and reached mixed results (Pellerano et al., 2017; Sudarshan, 2017; List et al., 2017; Holladay et al., 2019; Giaccherini et al., 2020; Bonan et al., 2023).

[7] Specifically in the provinces of Bologna, Forli-Cesena, Ferrara, Modena, Ravenna, and Rimini. In 2019, the partner multi-utility scored third in the domestic retail market for electricity and gas, with market shares of 3.3 and 11.3 percent, respectively. These shares did not vary significantly over the following years.

2024. Until that moment, customers could choose between contracts in the regulated market, with tariffs approved by the authority as in the water market, and contracts in the free market. Utilities compete in the free market through diversified price offers. In the context of our study, an important implication of the distinction between regulated and liberalized markets is that customers in the water market (regulated) cannot change providers, but customers in the gas and electricity markets (liberalized) can. Despite the slow transition to a wholly liberalized market, contract switching is increasingly common in Italy. For instance, in 2021, 15.7 percent of residential electricity customers changed provider at least once over the year (ARERA, 2022).

Since October 2016, the partner multi-utility has delivered to its customers in the free market an "Opower-style" home energy report with information on electricity and gas use. Initially, residential customers choosing the main offer provided by the multi-utility on the free market could opt to receive the report. Since the beginning of 2019, the partner multi-utility has revised the content and layout of the report and included this new version by default in almost all its gas and electricity offers on the free market.[8] The promotional material for these offers did not display prominent information about the presence of the report. Qualitative interviews with the partner multi-utility employees confirm that the report was not a relevant factor in customers' choice of offer, nor a driver of switching from other utilities.

Overall, it appears that the desire to receive the energy report was not a source of selection into the category of multiple report recipients. Among gas and electricity customers with contracts on the free market in our sample, the likelihood of receiving energy reports at baseline is primarily determined by when they signed the contracts. Therefore, selection into receiving multiple reports is related to the choice of a free market rather than a regulated contract and to when this decision was made, and not to the choice of contract within the free market or the utility offering the energy report. We will exploit these sources of variation in the likelihood of receiving multiple reports in our analysis.

3. Experimental design

A new water consumption report was designed and launched in October 2019, targeting all water customers with a valid email address. Eligible customers are randomly assigned to a treatment group that receives the report and a control group that does not.

The water report mirrors in structure, layout, and content the electricity and gas reports sent by the multi-utility to its customers since the beginning of 2019. It includes the following elements (Fig. 1):

a. *Static neighbor comparison*: comparison of the recipient's own average water consumption in the reporting period (about two months) with that of similar customers and the 25 percent most efficient similar customers. Similar customers are defined as having the same household size as the report recipient. The comparison also includes information on the percentage difference in consumption with respect to the average customer. Following the terminology used by Bicchieri (2005), this comparison provides a descriptive norm of behavior with respect to the two reference groups of similar and efficient customers.
b. *Injunctive feedback*: based on their water consumption relative to the two reference groups, customers receive an emoticon and written feedback: a smiling face accompanied by the word "great" if their consumption is below that of the most efficient similar customers; a neutral face with the word "good" if their consumption is lower than the average of similar customers but higher than that of the most efficient ones; and a frowning face paired with the word "decent" if their consumption is higher than the average of similar others. This feedback provides an injunctive norm (Bicchieri, 2005) of water conservation and is aimed at preventing boomerang effects, resulting from the descriptive norm, among customers with below-average consumption (Schultz et al., 2007).
c. *Dynamic comparison* compares the recipient's consumption over the reporting period and since the start of the year with that over the same periods in the previous year. The descriptive information is also accompanied by injunctive feedback, which is negative if the customer's consumption increased relative to the previous year and positive otherwise.
d. *Water saving tips*: tips on how to save water, divided into three categories: behavior change, such as turning off the shower while lathering; small investments, such as replacing the faucet head with an efficient one; or large investments, such as buying a water-efficient washing machine. Tips are season-specific.

The report is sent to customers by email bi-monthly soon after the delivery of the water bill. Therefore, customers receive the report at different times, according to their billing cycle. The report's reference period is the same as that in the bill.

4. Sample and data

Customers eligible for the study have a single water contract in their main residence address, so customers with multiple contracts and multiple houses are excluded. We exclude customers with household sizes greater than 20, possibly indicating condominiums, and with baseline water consumption above 10 times the sample median consumption. Eligibility also requires that customers have non-missing water usage in the 12 months preceding the start of the treatment (October 2018–October 2019). The resulting study sample contains 108,980 customers.

[8] Two offers could not be bundled with the report. One was a fixed monthly bill for energy throughout the year, based on the household's consumption in the previous year, and thus was not compatible with the report. The other was an offer that could not by law be associated with any additional service. These offers were selected by a very limited number of customers in our sample.

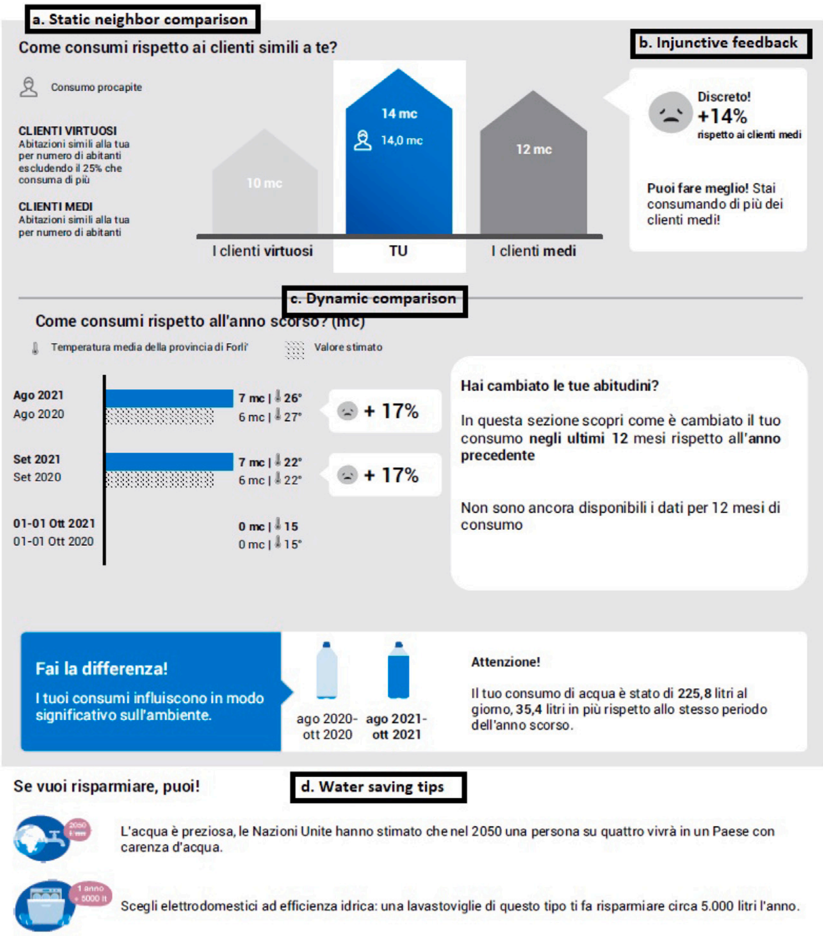

Fig. 1. Water consumption report..

The analysis of spillover effects on gas and electricity usage relies on the sub-sample of water customers with gas and electricity contracts active before the experiment's launch.[9] This leads to 91,690 and 75,193 customers holding an active gas and electricity contract at the program's start, respectively. Of them, 70,339 have both gas and electricity contracts with the utility. In all, 96,544 have a water contract and a contract for at least one other utility. Of the 108,980 water customers, 18 percent receive electricity and gas reports at the start of the intervention, 6 percent receive electricity reports only, 10 percent receive gas reports only, and 66 percent receive none.

The rollout of the study occurred in two waves. A first wave of 70,161 customers (64 percent of the sample) was assigned to treatment and control groups from October 2019. The second wave, with 38,819 customers (36 percent of the sample), was launched in December 2019. Within each wave, we follow a stratified individual-level randomization procedure to maximize ex-ante balance across treatment and control groups along a battery of relevant observable characteristics (Bruhn and McKenzie, 2009). We form strata using the following variables: having an electricity and/or gas contract; receiving the report about electricity and/or gas consumption; having an electricity and/or gas contract in the free versus regulated market; and having performed water self-reading at least once in the previous 12 months. This latter measure is a proxy of baseline engagement with the utility and attention paid to water usage, as explained in greater detail later. In sum, while we cannot randomize the receipt of other reports, we ensure through stratified randomization that the treatment and control groups are balanced along this dimension, which is a pre-specified source of heterogeneity in the impact of the water report.

After excluding strata with fewer than 10 observations, we obtain 32 strata. Within each stratum, we sort customers by water consumption in 2018 and assign adjacent customers to the treatment and control groups. In the first wave, we assign every other

[9] We do not apply the same eligibility criteria (at least one year of pre-treatment observations) to gas and electricity contracts as we do with water contracts.

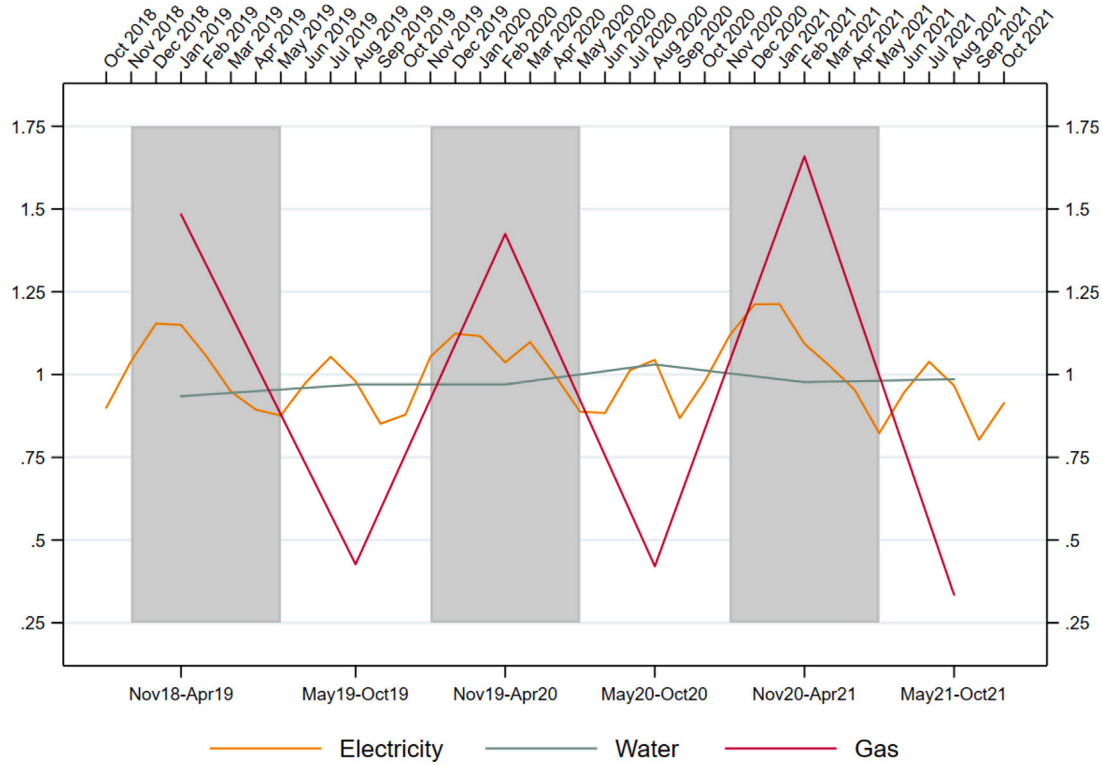

Fig. 2. Resource usage over the study period.
Note: The figure shows normalized usage of water, gas, and electricity over the study period for customers with active gas and electricity contracts at time of treatment. Water and gas consumption are given at the semester level, and electricity consumption is at the monthly level. Shaded regions denote winter periods.

customer to the control group (50 percent treatment and 50 percent control). In the second wave, every eleventh customer is assigned to the control group (91 percent treatment and 9 percent control).[10]

The analysis relies on administrative data provided by our partner utility after being anonymized. Data on water and gas consumption are based on meter readings performed periodically, at least once per year, by the distributor for all customers, and on self-readings provided voluntarily by customers.[11] Given the relatively low and irregular frequency of water and gas readings, we base our analysis on six-month periods, roughly corresponding to winter (November–April) and summer (May–October). On average, we employ three readings to construct the average usage value in a semester, one of which is always entirely included in the semester.[12] Conversely, data on electricity usage rely on actual consumption, measured through smart meters every month. Usage data for the three resources are expressed at the daily level in each month normalized with respect to the control group's mean consumption in the intervention period.[13]

Fig. 2 shows normalized electricity, water, and gas consumption in our sample period, October 2018 to October 2021. The lower frequency of water and gas consumption data relative to electricity is apparent, as is the strong seasonality characterizing gas usage. In 94 percent of households, gas is used for space and water heating and for cooking. Consumption peaks in winter, given that space heating accounts for the majority of the resource use.[14] Electricity usage is also seasonal, with a first peak during the winter months and a second in the summer, likely due to air conditioning. Water consumption is stable across the different seasons.

Table 1 reports descriptive statistics and experimental sample balance for baseline resource usage and selected customer characteristics, namely, the presence of other resource contracts, whether they are in the free (vs. regulated) market, the receipt

[10] The utility had targets in terms of the number of customers to be reached by the water report by the end of 2019, which explains the limited size of the control group in the second wave.

[11] Readings and self-readings are also the basis of water and gas bills. Without actual consumption information, bills are based on estimated consumption. Estimates are built from past usage, household location, and characteristics such as household size and, for gas, whether it is used for space heating, water heating, or cooking.

[12] This represents a departure from the PAP, where the time granularity for all resources was expected to be the month. We needed to reduce the frequency in light of the lower frequency of actual water and gas usage measurements.

[13] The mean in the control group is a weighted average of the two waves.

[14] The remaining share uses gas only for cooking and water heating and is balanced across treatment and control customers.

Table 1
Summary statistics and balance.

	(1) *N*	(2) Control mean	(3) Control SE	(4) ITT	(5) *p*-value
Eligible customers with water contract					
N. of occupants in the house	108,980	2.273	0.006	−0.003	0.694
Active gas contract	108,980	0.920	0.001	0.002	0.399
Active elect contract	108,980	0.774	0.002	−0.000	0.914
Gas contract in the free market	108,980	0.887	0.002	−0.000	0.867
Electricity contract in the free market	108,980	0.766	0.002	−0.000	0.751
No report	108,980	0.559	0.003	−0.000	0.969
Mean daily water usage	108,980	0.305	0.002	0.001	0.780
Engagement index	108,980	0.006	0.004	0.006	0.285
Customers with gas contract					
Gas report	91,690	0.397	0.003	−0.000	0.908
Mean daily gas usage	91,690	2.616	0.011	0.021	0.146
Customers with electricity contract					
Electricity report	75,193	0.401	0.003	0.000	0.918
Mean daily electricity usage	75,193	6.113	0.023	0.026	0.421

Note: This table reports customer-level summary statistics (number of observations, mean, and standard error in the control group) and assesses balance across treatment and control groups using a regression. The dependent variable is listed in the first column and is regressed on a treatment dummy and a binary variable for the wave of the program. All variables are expressed at the baseline or in the pretreatment period.

of other reports at time of program launch, and the engagement index. In terms of baseline daily levels, customers in our sample consume, on average, 0.3 cubic meters of water, 2.6 cubic meters of gas, and 6 KWh of electricity. These values appear relatively similar to the Italian average: 0.5 cubic meters of water, 3.6 cubic meters of gas, and 5.8 kWh of electricity (ARERA, 2022). However, they are far lower than those observed in studies using US-based samples. For instance, Jessoe et al. (2020) report daily household consumption of 1.5 cubic meters of water and 24.5 KWh in California.

Water, gas, and electricity consumption in the pre-treatment period are positively and significantly correlated, as shown in Appendix Table A.1. The Pearson correlation coefficient (ρ) is 0.368 for electricity and 0.297 for gas. In our setting, a correlation between resources could be mechanical, due to appliances that use more than one resource. Water and electricity consumption could be related due to the presence of washing machines and dishwashers in 97.3 and 50.2 percent of Italian homes, respectively. As for water and gas, water heating is the main reason behind the mechanical correlation between these two resources. The majority of households in Italy own gas heating systems while only 16 percent use electricity to heat water (ISTAT, 2022). The correlation between resources could also be due to economic or environmental considerations, as families may desire to save money on all bills or minimize the overall impact on the environment through low consumption of all resources. The data at our disposal do not allow us to identify which specific appliances and actions lie behind the correlation between the usage of different resources.

We complement consumption data with data on contract activation, contract deactivation, number of occupants in the house, and municipality. We also have information at the customer and monthly level on the number of accesses to the web portal, accesses to the app, contacts with customer service, and water meter self-reading, which we consider proxies of the customer's engagement with the utility. We construct an engagement index that aggregates the four variables.[15] We also have data on all reports received by customers, particularly on their date of receipt and contents.

We conduct balance tests across treatment groups for each variable available at the time of treatment assignment. We do this by regressing each baseline variable on a treatment dummy and a binary variable for the wave of the program. The latter is important for two reasons. First, although in the first wave treatment and control groups are equally sized, in the second wave, the control group accounts for only 9 percent of the sample, as described above. Second, customers in the two waves are significantly different along some dimensions. The experimental design guarantees that treatment and control customers are similar within each wave; hence, the inclusion of the wave control guarantees fair sample comparisons. As expected, we do not detect any significant difference in observable characteristics across the two samples (Table 1, column 5).

5. Results

5.1. Resource consumption

5.1.1. Direct and spillover effects

First, we evaluate the direct impact of receiving the water report on water consumption and the indirect effect on gas and electricity usage. We estimate the intention to treat effects (ITT) by separately estimating the following model:

$$y_{it} = a_1 Post_t + \beta_2 Treat_i * Post_t + h_t + g_i + \varepsilon_{it} \tag{1}$$

[15] We employ PCA to aggregate the four variables in a single index. This variable is used as a general customer engagement metric and, in its baseline values, as a pre-specified dimension of heterogeneity.

where y_{it} is the normalized average daily consumption of the specific resource over the period t (semesters for water and gas and months for electricity). $Treat$ is a treatment indicator. The variable $Post$ is set to 1 from November 2019 for the whole sample, regardless of enrollment in the first or second wave.[16] This allows us to compare customers across the same homogeneous semesters for water and gas. The regression also includes period fixed effects, h_t, and household fixed effects, g_i. Standard errors are clustered at the household level, i.e., at the randomization level, to allow for within-customer correlation over time in the error term (Bertrand et al., 2004). This specification is preregistered, as is the rest of the analysis. In what follows, we point out whenever we depart from the preregistered specifications.[17]

Results are shown in Table 2. The program significantly decreases water usage by 1.4 percent and electricity by 0.5 percent over the two post-treatment years. We detect no statistically significant treatment effect on gas consumption. For both water and electricity, treatment effects are stronger in the program's second year, significantly so for water. Spillovers may be due to the mechanical correlation between water and electricity or gas usage or to behavioral factors, such as the water report increasing users' motivation to save money or to preserve the environment by reducing consumption of all resources. The available data do not allow us to test the role of different mechanisms behind the spillover effects that we observe. Nevertheless, we can note that the magnitude of the spillover effect on electricity ($\beta_2 = -0.5$ percent) is consistent with what we would expect, given the size of the correlation coefficient between water and electricity in our setting ($\rho = 0.368$) and of the direct treatment effect coefficient ($\beta_2 = -1.4$ percent). This would suggest an important role for the mechanic reaction of electricity consumption to the reduction in water use induced by the report. Attenuation bias due to measurement error in gas usage, together with the smaller correlation between water and gas use, may be responsible for the small and statistically not significant spillover effect of the water report on gas. Ex-post power calculations suggest that the minimum detectable effects on gas usage are 0.56 percent, which aligns with the magnitude of the spillover effect on electricity (0.5 percent). This suggests that the spillover effect on gas, if existent, is likely to be smaller than that on electricity.[18]

The magnitude of the direct effect on water is smaller than that found by other studies over a similar time horizon (Ferraro et al., 2011; Bernedo et al., 2014), which may be due to the different context of our study. For example, most of these studies examine isolated interventions, that is, interventions not implemented on top of other nudges. In our case, some customers already received the gas and electricity reports. In the following section, we examine heterogeneity by receipt of other reports.

The indirect effect on electricity is comparable to the direct effect of similar social information programs in Italy (Bonan et al., 2020). Jessoe et al. (2020) report a short-lived effect of the water report on electricity use of 1–2 percent, which, however, vanishes after the first four treatment months. By contrast, the spillover effect on electricity that we detect seems to increase over time and persists for at least two years (Column 6). This suggests that spillover effects are persistent and similar to the direct effects of home reports on electricity and water usage in Allcott and Rogers (2014) and Bernedo et al. (2014).

5.2. Customer engagement

We assess how the program impacts customers' engagement with the utility. The first proxy of engagement is the deactivation of electricity and gas contracts. We create a variable taking the value of 1 if the customer deactivates the contract in the post-treatment period and 0 otherwise. In a cross-section, we include a set of controls, strata fixed effects, and the wave of the program. Table 3 reports the results. Levels of contract deactivation are 21 and 23 percent for gas and electricity over the two post-treatment years, respectively, showing how dynamic these markets are and, consequently, how important it is for companies to reduce churn. The program reduces these figures by about 0.6 (Column 1) and 0.7 (Column 3) percentage points, corresponding to relative changes of 2.8 and 3 percent.[19] We also detect an overall positive impact of the program on the engagement index (column 5).

Overall, these results suggest that customers value the water report. If we consider customer loyalty and engagement as indicators of users' utility from the program, these results suggest positive welfare effects of the water nudge. The benefits of the report accrue not only to the customers who value it and pay lower bills but also to the utility in the form of higher customer retention. The industry knows that winning a new customer costs several times more than retaining an existing one and engages in expensive retention campaigns. Yet, it is unclear which campaigns best mitigate churn (Ascarza et al., 2016; Ascarza, 2018).

5.3. Heterogeneity

5.3.1. Multiple nudges

We analyze how the report's impact on usage and contract cancellation varies depending on whether the customer already receives other reports, a pre-specified dimension of heterogeneity. We address this question by estimating the heterogeneous effects of the water report on customers already receiving other reports (electricity, gas, or both) at baseline. For resource consumption,

[16] The actual date of the launch of the water program is October 21st, 2019, and most of the program's customers are enrolled in the first wave. This conservative definition of the intervention period may bias our treatment effect estimates downward.

[17] In the pre-analysis plan, we committed to estimating, besides (1), a model where the three resources are pooled, including resource fixed effects. Given the difference in the frequency of outcomes measurement, this strategy does not appear viable.

[18] One could note that the upper limit of the confidence interval for the spillover effect on gas is −0.4 percent.

[19] Unsurprisingly, water contract cancellation in the control group is low (0.2 percent) and unaffected by the treatment. As explained above, water provision is regulated, and customers cannot choose their provider. The low rate of water contract deactivation is compatible with customers moving to municipalities served by other utilities or dying.

Table 2
Impact on resources usage.

	(1)	(2)	(3)	(4)	(5)	(6)
	Normalized daily usage					
	Water		Gas		Electricity	
Post	0.083***		−1.195***		0.053***	
	(0.006)		(0.004)		(0.003)	
Treat*Post	−0.014**		−0.001		−0.005**	
	(0.006)		(0.002)		(0.002)	
	[0.066]		[0.617]		[0.08]	
Post Y1		0.118***		−1.112***		0.090***
		(0.006)		(0.004)		(0.002)
Treat*Post Y1		−0.006		−0.003		−0.004*
		(0.007)		(0.002)		(0.002)
Post Y2		0.088***		−1.197***		0.054***
		(0.007)		(0.004)		(0.003)
Treat*Post Y2		−0.022***		0.002		−0.007**
		(0.007)		(0.003)		(0.003)
Observations	651,941	651,941	529,364	529,364	2,364,935	2,364,935
No. of households	108,980	108,980	91,690	91,690	75,193	75,193
P(Y1=Y2)		0.0219		0.0898		0.167

Note: This table reports panel estimates of the effect of the water report on normalized daily usage of water, gas, and electricity. Estimates are for the entire period (Oct. 2018–Oct. 2021) in columns 1, 3, and 5, and distinguish impacts in the first (Y1) and second (Y2) post-treatment years. *P*-values for the difference are reported at the bottom. The model includes individual and period fixed effects. Periods are semesters for water and gas, months for electricity. Post takes a value of 1 from November 2019, and 0 before. FDR sharpened *q*-values for prespecified hypothesis are in squared brackets.

*** significance at the 1 percent level.

** significance at the 5 percent level.

* significance at the 10 percent level.

Table 3
Impact on contract deactivation and customer engagement.

	(1)	(2)	(3)	(4)	(5)	(6)
	Contract deactivation				Engagement index	
	Gas	Gas	Electricity	Electricity		
Treat	−0.006**	−0.005	−0.007**	−0.006		
	(0.003)	(0.004)	(0.003)	(0.005)		
	[0.08]		[0.08]			
Treat*No report		−0.002		−0.001		
		(0.006)		(0.006)		
		[0.617]		[0.617]		
Post					3.664***	3.979***
					(0.073)	(0.090)
Treat*Post					0.156***	0.082
					(0.057)	(0.096)
					[0.039]	
Post*No report						−0.564***
						(0.095)
Treat*Post*No report						0.227*
						(0.121)
						[0.087]
Observations	91,685	91,685	75,191	75,191	4,032,260	4,032,260
N. of households	91,685	91,685	75,191	75,191	108,980	108,980
Mean dep var	0.213	0.213	0.229	0.229	0.0688	0.0688

Note: This table reports OLS estimates of gas (columns 1 and 2) and electricity (columns 3 and 4) contract deactivation (churn) in the post-treatment period. All models include average pretreatment resource usage, average pretreatment number of water self-reading, no other reports, n. of occupants, contract length less than 3 years, strata fixed effects, and a dummy for the main wave of program delivery. Main and heterogeneous effects on the engagement index (columns 5 and 6) are estimated using a monthly panel with individual and time fixed effects. The engagement index is calculated using PCA of the following variables: accesses to online portal, accesses to the app, contacts to the customer service, water self-readings. The mean dependent variable is calculated for the control group. FDR sharpened *q*-values for prespecified hypothesis are in squared brackets.

*** significance at the 1 percent level.

** significance at the 5 percent level.

* significance at the 10 percent level.

we separately estimate the direct impact on water usage and the indirect ones on gas and electricity usage, y_{it}, in the following model:

$$\begin{aligned} y_{it} &= \beta_1 Post_t + \beta_2 Prog_i * Post_t + \beta_3 Post_t * NoReport_i + \\ &+ \beta_4 Prog_i * Post_t * NoReport_i + h_t + g_i + \varepsilon_{it} \end{aligned} \tag{2}$$

Table 4
Impact on resources usage by number of reports.

	(1) Normalized daily usage	(2)	(3)
	Water	Gas	Electricity
Post	0.072***	−1.194***	0.046***
	(0.008)	(0.004)	(0.003)
Treat*Post	−0.000	−0.000	0.003
	(0.010)	(0.004)	(0.003)
Post*No report	0.020**	−0.003	0.015***
	(0.010)	(0.004)	(0.004)
Treat*Post*No report	−0.024*	0.000	−0.017***
	(0.013)	(0.005)	(0.005)
	[0.087]	[0.617]	[0.009]
Observations	651,941	529,364	2,364,935
No. of households	108,980	91,690	75,193

*** significance at the 1 percent level.

** significance at the 5 percent level.

* significance at the 10 percent level.

Note: This table reports panel estimates of the effect of the water report on normalized daily usage of water, gas, and electricity. The model includes individual and period fixed effects. Periods are semesters for water and gas, months for electricity. Post takes a value of 1 from November 2019, and 0 before. FDR sharpened q-values for prespecified hypothesis are in squared brackets.

where $NoReport_i$ is a dummy for whether customer i does not receive any other report at the baseline. The results are shown in Table 4. For water and electricity consumption, the program is significantly more effective among customers for whom the water report is the only one received. The program reduces water and electricity usage in this group by 2.4 and 1.7 percent, respectively. This direct impact on water aligns with the effect of similar water programs implemented in isolation (Ferraro et al., 2011; Bernedo et al., 2014). In these studies, the effect, computed two years after program initiation is around a 2.6 percent reduction in water use. Conversely, the program does not affect water and electricity use for those already receiving other reports.[20]

Low statistical power prevents us from exploring the differential impact of the water report by different types (gas vs. electricity) or numbers (one vs. two) of reports received at baseline.[21]

We find that the effect of the water report on contract cancellation is not different for customers receiving other reports at baseline and customers for whom the water report was the only one received (Table 3, columns 2 and 4). We also detect a stronger positive impact of the program on overall engagement with the utility, captured by the engagement index, for those receiving the report for the first time (column 6). The water report has no effect on engagement among customers already receiving other reports. These results indicate that benefiting from the water report through water usage reduction is not a necessary condition for customers to value the report.

The heterogeneity in the impacts of the water report by receipt of other reports at baseline may be interpreted causally, as the marginal effect of an additional report depending on the number of reports already received. However, given that we do not randomize the number of reports each user receives, it may also be due to differences between customers receiving other reports at baseline and those who do not, which are related to their behavioral responses to the report. Our empirical specification, including individual fixed effects, partially addresses this selection issue, as it controls for the effect of time-invariant individual characteristics that differ between multiple and single-report recipients. However, the presence of omitted time-varying factors that affect both the number of reports received and the household's responsiveness to the water report would make the causal interpretation of the interaction effect between treatment and receipt of multiple reports problematic.

In Appendix B, we show that single- and multiple-report recipients do differ along several dimensions that may be related to their reaction to the water report (Appendix Table B.1). We then exploit our knowledge of the context, namely, when the partner multi-utility bundled by default the energy reports to all free-market contracts, to identify a sub-sample of users for whom the timing of other reports' receipt is plausibly exogenous.[22] We show that our results are robust when conducted on this sub-sample (Appendix Table B.2). The analysis lends credibility to a causal interpretation of these heterogeneity results and excludes that these results are entirely driven by self-selection.

The results regarding the engagement index (Table 3, column 6) suggest that users may not attend to additional reports as they did to the first. To shed light on mechanisms driving the lower effectiveness of multiple reports, we exploit data on users' access to the utility web portal and the app following receipt of a report. Recall that customers receive the reports by email and that the report typically invites users to visit their personal pages on the utility's website or the app for further information. If they do so, their access is recorded.[23]

[20] The coefficients attached to Treat*Post represent the ITT for the group already receiving some reports and are never significant.

[21] Conditional on receiving at least one report, customers receiving gas only, electricity only, or both reports are 6209, 10,852, and 19,912, respectively.

[22] Details on the sample definition are provided in Appendix B.

[23] We do not observe whether users open or click on the report but have a proxy for the latter.

We posit that cognitive and attention constraints limit engagement with the different reports, thus reducing their effectiveness. To test this hypothesis, we analyze whether users' reaction to receiving an electricity and/or gas report changes once they start receiving the water report. If the water report reduces users' ability or willingness to attend to other reports, we expect fewer accesses to the web portal and app following an electricity and/or gas report among treated customers in the post-treatment period. For this analysis, we focus on the sample of gas or electricity contract holders who had started receiving reports before the treatment.[24] We use access to the utility's web portal and app in a month, on both the extensive and intensive margins, as our dependent variables. We test how receipt of an electricity or gas report affects these proxies of engagement and whether such engagement changes among users receiving the water report in the post-treatment period. This analysis therefore exploits within-individual variation in the timing of report receipt and random treatment assignment to the water report as sources of identification. The receipt of a report (gas or electricity) is usually associated with an increase in engagement in the month. However, users assigned to receiving the water report with the other report(s) reduce their engagement in the post-treatment period (Appendix Table A.2). These results are consistent with the notion that, upon receiving an additional nudge, users reduce their engagement with the nudges that they were already receiving.

We present a final piece of exploratory and suggestive evidence in support of the notion that the costs of attending to multiple reports matter for their effectiveness. We argue that these costs are lower when reports are received simultaneously, primarily because customers need to open a single email instead of two. We exploit the fact that, among the customers enrolled in the social information programs for water and electricity, some receive the two reports on the same date and others receive them on different dates. Specifically, we focus on the sub-sample of treated customers who receive both water and electricity reports (N=10,204 customers). Of these customers, 16.1 percent consistently receive both reports simultaneously for the whole study period. We estimate the impact of receiving both reports on the same day, relative to that of receiving them on different dates, through a model similar to (1), where the variable $Treat$ is replaced by an indicator of whether the treated customers receive the reports simultaneously ($SameDay$). The results are presented in Appendix Table A.3 and indicate that electricity conservation is larger among customers receiving both reports simultaneously (Column 2) than for those receiving them separately. Although the effect on water is also negative, it is imprecisely estimated. The lack of a direct effect on water may be due to the lower frequency of observations, which prevents us from capturing short-term effects and reduces power. Nonetheless, these results show that sending multiple reports simultaneously results in a reduction in electricity use, relative to receiving them separately, among the less responsive group of multiple report recipients.

5.3.2. Baseline resource usage and engagement

We evaluate the heterogeneity in program impacts by other pre-specified dimensions. We consider baseline resource usage (Table 5, columns 1–3) and baseline customer engagement (columns 4–6) and split customers according to the median baseline value.

First, the direct impact of the treatment on water usage does not vary depending on baseline water usage (Column 1). On the contrary, for gas and electricity, we find that receiving the water report affects customers differently based on their baseline resource usage level. In particular, low gas and electricity users reduce consumption significantly more than high users in response to the treatment (Columns 2 and 3). We do not detect increased gas and electricity consumption among high users in the treatment group (the sum of the coefficients $Treat * Post + Treat * Post * X$ is never statistically significant). We also look at heterogeneity in the indirect effect of the water report by baseline water usage, the dimension on which the water report's contents, in terms of the descriptive and injunctive norm, are based. The results are qualitatively similar (Appendix Table A.4).

The patterns of the indirect effects are not in line with the result established in the literature for direct effects. Social information programs are more effective with high baseline users, both because they have larger margins of reduction and because of boomerang effects among low users (Bonan et al., 2020; Byrne et al., 2018; Bhanot, 2017; Schultz et al., 2007). With the available data, we can only note that, in our sample, the correlation at baseline between the use of water and that of the other two resources is higher among low users and this is consistent with the stronger spillover effects for this group of users. Specifically, the Pearson correlation coefficient between water and electricity usage at baseline is 0.326 among users with below-median baseline water consumption and 0.189 among above-median water users. This pattern makes intuitive sense, as appliances likely to be associated with higher electricity consumption, such as air conditioning and electric stoves, do not consume water. The corresponding figures for the water-gas correlation are 0.223 and 0.197 for below-median and above-median water users, respectively.

We look at the heterogeneous treatment effects by baseline level of resource usage on contract deactivation in Appendix Table A.5.[25] The treatment does not have a differential effect on the probability of canceling the contract among customers with high baseline usage (considering both water and other resources). This means that those who are likely to receive negative feedback in the report are not more likely to leave the utility.

Second, we examine heterogeneity by baseline customer engagement, proxied by the engagement index. Columns 4–6 of Table 5 report the estimates. We do not find significant differences in the treatment effects between the high- and low-engagement sub-samples. This result is reassuring for utilities, in that it suggests that customers with low levels of engagement at baseline also attend to social information nudges.

[24] Recall that this is one of the dimensions over which we stratified treatment assignment.

[25] We acknowledge that this analysis did not feature in the PAP.

Table 5
Heterogeneity by baseline resource usage and baseline engagement.

	(1) X = high resource usage	(2)	(3)	(4) X = high engagement index	(5)	(6)
	Water	Gas	Electricity	Water	Gas	Electricity
Post	0.155***	−1.143***	0.080***	0.101***	−1.203***	0.043***
	(0.005)	(0.004)	(0.003)	(0.007)	(0.004)	(0.003)
Treat*Post	−0.006	−0.004	−0.010***	−0.015*	−0.004	−0.005
	(0.004)	(0.003)	(0.002)	(0.008)	(0.004)	(0.003)
Post*X	−0.151***	−0.107***	−0.057***	−0.034***	0.015***	0.017***
	(0.010)	(0.004)	(0.004)	(0.010)	(0.004)	(0.004)
Treat*Post*X	−0.004	0.011**	0.013***	−0.003	0.009*	0.000
	(0.013)	(0.005)	(0.005)	(0.012)	(0.005)	(0.005)
	[0.617]	[0.066]	[0.039]	[0.617]	[0.101]	[0.617]
Observations	651,941	529,364	2,364,935	651,941	529,364	2,364,935
No. of households	108,980	91,690	75,193	108,980	91,690	75,193

Note: This table reports panel estimates of the effect of the water report on normalized daily usage of water, gas, and electricity. High-usage customers are those with above median baseline usage of each resource. High-engagement index indicates customers above the median index. Regressions include individual and period fixed effects. Periods are semesters for water and gas, and months for electricity. Post takes a value of 1 from November 2019, and 0 before. FDR sharpened q-values for pre-specified hypothesis are in squared brackets.

*** significance at the 1 percent level.

** significance at the 5 percent level.

* significance at the 10 percent level.

5.4. Robustness

Our results are robust to a series of checks. First, we estimate Eq. (1) after excluding outliers in terms of baseline water usage (Appendix Table A.6, columns 1–3) or households with more than five members (Appendix Table A.6, columns 4–6). The results are unchanged and confirm the direct effect of the report on water use and the spillover effect on electricity use.

Second, as we previously observed that the treatment induced selective attrition among gas and electricity customers, we run the analysis on the sample of customers who did not change gas and/or electricity providers over the program period (Appendix Table A.6, columns 7–9). The results are robust to these exclusions. Not only the direction of the effect but also the point estimates are largely unaffected by these changes.[26]

Third, recall that out of 108,980 customers with a water contract enrolled in the intervention, 91,690 have a gas contract, 75,193 have an electricity contract, and 70,339 have both. We want to ensure that the different composition of the samples between single water contracts and multiple contracts does not drive the main result on water usage. Therefore, we check the direct effect of the intervention on water usage for the sub-samples of customers with multiple contracts. This is done in Appendix Table A.7. The results hold.

Fourth, we control for multiple hypothesis testing for all the hypotheses outlined in the PAP. In particular, we calculate and report in square brackets the FDR-adjusted q-values (Benjamini et al., 2006).[27] The results discussed in the paper appear robust to the multiple hypotheses correction, as all q-values attached to statistically significant coefficients ($p < 0.1$) remain below the threshold of 0.1.[28]

6. Conclusion

We study the direct and indirect effects of a water conservation nudge in the presence of similar nudges targeted at other resources. We find significant direct effects of a water nudge on water consumption and indirect effects on electricity, but not gas. These effects are concentrated among customers who do not receive reports for other resources. Report recipients are less likely to deactivate their electricity and gas contracts, regardless of whether they receive other reports, suggesting a positive report valuation among customers. Although we find no indication of alienation of customers who receive multiple nudges on related behaviors, our results suggest that receiving multiple nudges reduce the attention recipients pay to each one. This leads to lower conservation gains within each domain.

These results have important policy and business implications for energy and water companies' design of conservation nudges. Policymakers and businesses should carefully design the first nudge targeted to a given population. Initial interventions are those likely to have the greatest direct and indirect impacts. They should also be aware of the potential diminishing effects of additional

[26] We repeat the exercise for the three resources, after excluding customers who canceled the gas or electricity contract. The results are qualitatively similar and are available upon request.

[27] We simultaneously include 19 hypotheses in the correction. This represents a departure from the approach described in the PAP. There, we corrected separately for hypotheses related to the main and heterogeneous treatment effects. We believe that the current version is more conservative and therefore preferable.

[28] The only exception is the test of heterogeneity by baseline engagement for gas usage, in column 5 of Table 5.

nudges. Policymakers should reduce the cognitive efforts of attending to multiple stimuli, for instance by administering them together. Additional research is needed to understand how nudges should be designed, combined, and implemented for different resources to maximize companies' goals while ensuring societal well-being.

CRediT authorship contribution statement

J. Bonan: Writing – original draft, Methodology, Formal analysis, Data curation, Conceptualization. **C. Cattaneo:** Writing – original draft, Methodology, Formal analysis. **G. d'Adda:** Writing – original draft, Methodology, Formal analysis, Conceptualization. **A. Galliera:** Methodology, Conceptualization. **M. Tavoni:** Validation, Funding acquisition, Conceptualization.

Acknowledgments

We thank Fabrizio Mauri, Marcello Folesani, and Monica Crippa for the fruitful collaboration. We thank Sara Constantino, Lukas Fesenfeld, Elke Weber, seminar participants at Bank of Italy, GATE-Lab, and ETH for helpful discussion and Conference participants at ESA 2022. Milica Vranic and Matteo Muntoni provided excellent research assistance. We acknowledge financial support from the H2020-MSCA-RISE project GEMCLIME, GA 681228; the H2020 project NEWTRENDS, GA 893311; the Energy Demand Changes Induced by Technological and Social Innovations (EDITS) project, funded by the Ministry of Economy, Trade, and Industry (METI), Japan); the European Union - Next Generation EU, in the framework of the project GRINS (PE00000018). This RCT was registered in the American Economic Association Registry for randomized control trials under trial number AEARCTR-0006546.

Appendix. Supplementary material

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jeem.2024.103037.

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