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For better or for worse? Empirical evidence of moral licensing in a behavioral energy conservation campaign

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HIGHLIGHTS

- We measure cross-domain licensing effects in a naturalistic setting.
- We rule out income effects as an alternative explanation for the effects.
- The conservation campaign succeeds in reducing demand for the target resource water.
- Yet participants increase consumption in other domains (electricity demand).
- The energy/CO_2 savings from water are more than offset by higher electricity demand.

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ABSTRACT

Isolated environmental campaigns focusing on defined target behaviors are rolled out to millions of households every year. Yet it is still unclear whether these programs trigger cross-domain adoption of additional environment-friendly behaviors (positive spillover) or reduced engagement elsewhere. A thorough evaluation of the real net performance of these programs is lacking. This paper investigates whether positive or perverse side effects dominate by exemplifying the impact of a water conservation campaign on electricity consumption. The study draws on daily water (10,780 data points) and weekly electricity (1386 data points) consumption data of 154 apartments in a controlled field experiment at a multifamily residence. The results show that residents who received weekly feedback on their water consumption lowered their water use (6.0% on average), but at the same time increased their electricity consumption by 5.6% compared with control subjects. Income effects can be excluded. While follow-up research is needed on the precise mechanism of the psychological process at work, the findings are consistent with the concept of moral licensing, which can more than offset the benefits of focused energy efficiency campaigns, at least in the short-term. We advocate the adoption of a more comprehensive view in environmental program design/evaluation in order to quantify and mitigate these unintended effects.

1. Introduction

It is a common, well-known practice among dieters to treat themselves to a snack or richer meal after having completed an exhausting, demanding, or unpleasant physical task (Fishbach and Dhar, 2005). This is a typical example of moral licensing: feeling entitled to a self-indulgent behavior that one would not permit oneself without first having done a positive action. Recent contributions in consumer research and policy, marketing, and social psychology journals provide evidence of moral licensing in various behavioral domains including purchasing decisions, nutrition, racism, and sexism. A recent online article in Science (Norton, 2012) reports that drivers of hybrid cars violate crosswalk laws more often than drivers of conventional cars, attributing the observed difference to moral licensing. The same pattern may apply to environmental behavior: resource conservation in one area may make people more wasteful elsewhere. And just as the rewarding food treat might contain by far more calories than those consumed during the activity that licensed it, some of our environmental campaigns might do more harm than good to the environment overall when the licensing effect is taken into account. On the other hand, just as people dieting for weight loss also tend to exercise more, behavior change in one environmental

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domain might also open a window of opportunity for positive spillover into other domains through increased awareness or motivation (Lawson and Flocke, 2009). This study sheds light on cross-domain effects of conservation campaigns in an energy-intensive and frequently targeted area: residential energy consumption. Our study thereby responds to the call made by Stern (2011) for research on the effect of taking one pro-environmental action on subsequent actions. He points out the contradictory predictions for subsequent actions made by behavioral scientists as one of the fundamental research questions for future psychological research: "Which of these mechanisms predominates with high-impact behaviors, and under what conditions, are fundamental research questions of obvious importance to limiting climate change."

The residential sector accounts for 21% of the CO_2 emissions from fossil fuel combustion (EPA, 2011a) and for approximately 22% of total primary energy consumption in U.S. Department of Energy (2012). U.S. primary energy consumption in the residential sector has more than doubled since the 1960s (U.S. Energy Administration Information, 2011), while per capita residential electricity consumption more than tripled between 1960 and 2008 (International Energy Agency, 2011). Consequently, residential energy demand has received considerable attention in programs that aim at reducing energy consumption. In the past several years, particular attention has been paid to nonmonetary incentives, such as neighborhood comparisons of the consumption of electricity (Schultz et al., 2007; Ayres et al., 2009; Allcott, 2011) or water (Ferraro and Price, 2011). These programs typically yield savings on the order of 2–5% across the population in the targeted area of utility consumption, equivalent to the effect of a price increase of 11–20% (Allcott, 2011). A large number of such isolated environmental campaigns have been researched, and many more have been undertaken—yet typically only the singular effects of the target behavior in isolation are analyzed. For a full cost–benefit analysis of environmental campaigns, however, the complete change in energy consumption must be taken into account. A better understanding of these mechanisms and the quantification of their impact is crucial for well-informed policy decisions.

This paper explores whether a behavior change campaign in one domain (water consumption and the directly associated energy for water heating) also has a measurable impact on the consumption of other utilities—in this case, electricity. We choose water and electricity consumption as dependent variables for four reasons: first of all, they are the outcome of everyday behaviors and relevant for every household (unlike airplane travel). Second, both account for a large share of a household's carbon footprint: water heating is the second largest energy end use after space heating in residential buildings, accounting for 18% of the site energy use. Water heating accounts for 13%, and electricity accounts for 71% of residential primary energy consumption (U.S. Department of Energy, 2012), Third, water and electricity consumption reflect the aggregated real-world impact of multiple behavioral decisions instead of a single action that may or may not be relevant for a household. And fourth, thanks to existing infrastructure and technology, they are easier to measure than the quantity of waste produced or recycled.

We investigate the impact of a water conservation campaign in a multifamily building complex on residents' electricity consumption. By providing weekly water conservation tips and individual feedback on water usage to half of the study participants, our study investigates whether evidence for the dominance of positive spillover or moral licensing can be detected in the residents' electricity consumption. If the effect on water consumption is viewed in isolation, the campaign can be considered another example of a successful non-price-based behavioral intervention. Yet we take the analysis one step further and show that apartments exposed to the water conservation campaign did indeed increase their electricity consumption relative to the control group, which we attribute to the dominance of moral licensing.

The following section provides an overview of related work including the key findings. Section 3 describes the study setting, the intervention, and the data analysis methods used. Section 4 outlines the data collection procedure and summarizes the impact of the water feedback intervention on residential water and electricity use. Finally, Section 5 concludes with implications for policy and further research.

2. Related work

In recent years, the number of large-scale, energy conservation programs informed by insights from behavioral science has dramatically increased (Allcott, 2011) Despite strong evidence for the influence of behavior change in one area on consumer choices in other environmental domains (e.g., Thøgersen, 1999a), most studies investigate effects on the target behavior only (e.g., Ehrhardt-Martinez et al., 2010; Schultz et al., 2007; Goldstein et al., 2008; Ayres et al., 2009; Ferraro and Price, 2011). Those studies that did investigate effects of an intervention on both a target outcome and side effects on other behaviors can be broadly grouped into two categories: positive and negative side effects. The concept of positive side effects of environmental campaigns is built on individuals' desire for consistency in their actions or at least the appearance of consistency (Festinger, 1957). Thus, many environmental campaigns are motivated by the assumption that "simple and painless" behavioral changes (such as turning off the computer monitor or printing double-sided) will lead to the adoption of higher-impact changes in environmental behavior (Thøgersen and Crompton, 2009). For instance, the UK government's department for environment, food and rural affairs (DEFRA) recommends that "[w]e need to promote a range of behaviors as entry points in helping different groups to make their lifestyles more sustainable—including catalytic (or 'wedge') behaviors if identified through research"(DEFRA, 2008). Similar, the UK's Sustainable Consumption Round Table (2006) suggests that the best way to promote pro-environmental behavior "is to drop new tangible solutions into people's daily lives, catalysts that will send ripples, get them talking, sweep them up into a new set of social norms, and open up the possibility of wider changes in outlook and behavior." The underlying idea of this positive spillover of environmental behavior is that the "adoption of a particular behavior increases the motivation for an individual to adopt other, related behaviors" (Thøgersen and Crompton, 2009), based on environmental values that foster feelings of moral obligation (Thøgersen, 1999b). Kotchen and Moore (2008) found a decrease in energy consumption by participants in a green electricity program who paid a price premium for each unit of electricity consumed. The magnitude of the effect, however, was within the range of the estimated price elasticity for electricity consumption; hence the study could not determine whether the response was due to the voluntary price premium or to positive spillover effects. A Danish study based on phone survey data ($N=1002$) found a positive spillover from recycling on packaging waste prevention (Thøgersen, 1999a). A later study with Danish consumers revealed cases of transfer of environment-friendly conduct between behavioral categories only in a limited number of possible instances and only of modest size; at the same time, they also identified a limited number of negative cross-lagged effects (i.e., two sets of correlations separated by a time interval) (Thøgersen and Ölander, 2003). A common theme among the majority of the existing studies supporting positive spillover is that they are based on self-reported survey data. Yet self-reported data are often criticized for their limited reliability and the limited insights they provide into real behavior and decision making (e.g., Webb et al., 2003; Krampf et al., 1993). More recently, the concept of positive spillover from one simple environmental "entry point" behavior to a wider range of conservation efforts has generally become quite controversial (Thøgersen and Crompton, 2009).

A growing body of research suggests that on the contrary, the behavioral spillover may be negative: the adoption of a more environment-friendly choice in one domain may actually increase the likelihood of less environment-friendly behavior in other areas. In their meta-study on environmental behavior, Steg and Vlek (2009) report that "factor analysis reveals that individuals are fairly inconsistent in their environmental behavior." In general, although most individuals strive to see themselves as moral actors (Jordan et al., 2011), they are tempted to act in ways that make them feel immoral (Merritt et al., 2012). Moral licensing is defined as the phenomenon whereby "people can call to mind previous instances of their own socially desirable or morally laudable behaviors," making them "more comfortable taking actions that could be seen as socially undesirable or morally questionable" (Miller and Effron, 2010). To study this phenomenon, Sachdeva et al. (2009) conducted three experiments looking into the effect of previous actions on donations and environmental decision making. They suggested that affirming moral identity leads people to feel licensed to act immorally, and they proposed a framework of self-regulation that balances moral self-worth and the cost inherent in altruistic behavior. To investigate the behavioral antecedents of the moral licensing, Miller and Effron (2010) reviewed previous studies on psychological licensing and suggested that three major conditions are associated with activating moral licensing: (1) the behavior is relatively unimportant to one's identity, (2) the behavior is framed as progress rather than commitment to a goal and (3) avoiding hypocrisy is of minor concern. All three conditions apply to pro-environmental behaviors and the way environmental campaigns are perceived by the public (see Crompton and Kasser, 2009; Cornelissen et al., 2008; Thøgersen, 1999b). People's tendency to morally "trade" one environmentally friendly action for other less pro-environmental behaviors might even be reinforced by current environmental programs that frame environmental behaviors as interchangeable actions, e.g., the "Pick 5" campaign of the U.S. Environmental Protection Agency (EPA, 2011b), in which participants pledge pro-environmental actions that they pick from a list of items.

Evidence for moral licensing has been found in various domains of human behavior. The majority of studies that investigate moral licensing focus on racism (Merritt et al., 2012; Bradley-Geist et al., 2010; Effron et al., 2009; Monin and Miller, 2001), disclosure of conflicts of interests (Cain et al., 2005a, 2005b), donations (Strahilevitz and Myers, 1998; Khan and Dhar, 2006), sexism (Monin and Miller, 2001), nutrition (Wilcox et al., 2009; Khan and Dhar, 2007), choices with different levels of cultural sophistication (Khan and Dhar, 2007), or the purchasing of luxury goods (Kivetz and Simonson, 2002). Moral licensing is not confined to related actions within the same behavioral domain but has also been observed between behaviors that are not closely related (cross-domain moral licensing). Khan and Dhar (2006) found that the hypothetical choice of volunteering for one community service organization or another licensed participants to express a preference for a luxury good over a utilitarian one. Chiou et al. (2011) report increased smoking among participants who believed that they were taking a dietary supplement. Mazar and Zhong (2010) demonstrated in a series of laboratory experiments that individuals who are given the opportunity to purchase green goods are more prone to negative behaviors in other domains (in their study, stealing and lying). Kruger and Gilovich (2004) as well as Wilcox et al. (2009) found that the pure anticipation of a positive behavior can be sufficient to license morally less laudable behavior. They concluded that people are willing to give themselves credit for their good intentions, even without acting on them. A recent study by Clot et al. (2011) investigated how intrinsic motivation affected participants' willingness to donate money to an environmental organization after a primary virtuous act (dedicating time to an environmental program) that was framed either as voluntary or mandatory. They found that moral licensing occurs among intrinsically motivated individuals facing mandatory conditions as well as among non-intrinsically motivated individuals under voluntary conditions. With the exception of two papers (Conway and Peetz, 2012 and Chiou et al., 2011), all of these studies are laboratory experiments and have the shortcoming that behaviors exhibited there may not be reflective of typical behaviors outside the laboratory. In their reviews on environmental behavior and household energy consumption, Steg and Vlek (2009), Wilson and Dowlatabadi (2007), and Abrahamse et al. (2005) advocate the importance of examining real data and actual energy use. As discussed by Levitt and List (2006), experimental findings can only be extrapolated beyond the lab to a limited extent, since important factors influencing human behavior are fundamentally biased by the nature of laboratory experiments: scrutiny by others, the particular context of a decision, and how participants are selected. In line with Levitt and List (2006), Allcott and Mullainathan (2010) made the case "to do the 'engineering' work of translating behavioral science insights… from the laboratory to the field of practice," arguing that this missing step would have high economic returns.

Similarly to the moral licensing effect, the term "rebound effect" is often used in the economic literature to describe net negative outcomes of energy efficiency increases. In contrast to moral licensing, the rebound effect is rooted in neoclassical economic theory (see Jenkins et al., 2011; Greening et al., 2000 for extensive reviews). It describes phenomena that can be ascribed to substitution effects, price effects, and income effects (see Madlener and Alcott, 2009 for a more recent overview of discussions and context). According to these effects, lower energy consumption (e.g., resulting from more energy-efficient appliances) results in a reduced cost of living and thus higher disposable income, allowing individuals to increase their consumption of these products or other ones that also require energy for their production or operation. These microeconomic mechanisms are driven by changes in supply and consumption rather than by non-monetary, psychological mechanisms influencing individuals' decision making processes as in moral licensing.

Very recently, a number of field studies have been carried out in the domain of household utility use. Jacobsen et al. (2010) analyzed changes in electricity consumption in response to enrollment in a green electricity program with 910 participating households. They found that households that enroll at the minimum level increased electricity consumption by 2.5% (before–after difference between participants and nonparticipants). In a recent pilot carbon offset program with 30,000 customers, Harding and Rapson (2013) found evidence for increased electricity consumption after the adoption of a carbon-offsetting program, and they demonstrated the importance of framing a program to avoid negative side effects. Both of these studies used field data, but both have two essential limitations: first, they are restricted to within-domain licensing (e.g., reducing negative externalities of electricity consumption) and second, they both ignore or cannot exclude income effects as an explanation of their findings. Bento et al. (2010) studied cross-commodity effects and the role of culpability in the willingness to prevent environmental harm. They used web-based contingent valuation in a framed field experiment, supplemented by real-money laboratory experiments. One of their main findings was that the moral licensing effect dominates guilt effects (moral cleansing). The findings of Bento et al. (2010), however, are limited by being based on a hypothetical scenario. Altogether, the evidence from these studies implies that environmental programs targeting a specific behavior might actually yield a much smaller net CO_2 reduction than those reported by program evaluators who focus solely on the change in the target behavior. Even worse, such campaigns might result in a net negative CO_2 outcome, in which the CO_2 reduction of the target behavior is more than offset by higher CO_2 emissions in other environmental domains due to moral licensing. To our knowledge, our study is the first that investigates water consumption with respect to behavioral spillover and moral licensing.

Given the contradicting predictions for behavioral spillover (positive or negative) and the strong evidence for moral licensing across domains, the current field study seeks to overcome the limitations of previous research. Our work expands on the existing research on spillover and licensing effects in several respects. First, it measures the outcome of participants' behavior in a naturalistic and highly relevant setting instead of analyzing self-reported attitudes, behaviors, or responses to hypothetical scenarios. Second, it investigates cross-domain effects (from water use behavior to electricity consumption). Third, the study controls for many non-behavioral variables, such as differences in the heating/cooling system or the type of water faucets, by investigating households residing in a housing complex with similar apartment units. Forth, we are able to rule out income effects as an alternative explanation of the observed differences, since tenants of the study property do not pay for the targeted utility (i.e., water). Finally, we avoid the self-selection bias inherent in many other studies by using an opt-out recruitment strategy whereby practically all households in the apartment complex participate in the project, not just the subset that volunteer for the study.

3. Methodology: Site description, data collection, intervention, and data analysis

We conducted a field study at a multifamily building complex with 200 apartments to investigate the impact of an environmental campaign on water and electricity consumption. Apartment water consumption was measured daily and electricity consumption weekly. After two weeks of baseline data collection, half of the apartments received weekly feedback on their per capita water consumption along with water conservation tips for seven weeks.

3.1. Site description and recruitment of participants

The study was carried out from May to July 2011 at a multifamily property in Lynnfield, Massachusetts, a town in the Greater Boston area. The property consists of 200 apartments in three neighboring five-floor buildings constructed in 2009 with identical floor plans and a similar building orientation (Fig. 1), managed and rented out by a single property management company. Apartment size varies from 74 m^2 (smallest one-bedroom units) to 113 m^2 (largest two-bedroom units) with a mean of 91 m^2, compared to a U.S. average of 129 m^2 for new multi-family building units built in 2010 (U.S. Census Bureau, 2011). According to the property management, residents are a mix of all age groups, with an upper-medium level of income and

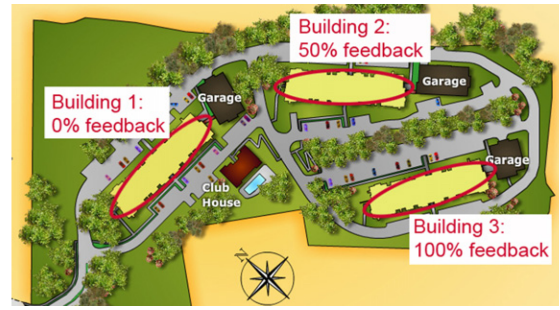

Fig. 1. Property layout with the triplet buildings and their assigned percentage of apartments in the treatment condition.

education. At the beginning of the study, 14 units were vacant, with the majority of the units occupied by one (48%) or two persons (38%) (M=1.72, SD=0.84, N=186). According to the property management, demographics and rental policy are the same across all three buildings.

In contrast to most multifamily buildings in the U.S., all utilities (electricity, gas, and water) are submetered at the apartment level; tenants pay for electricity and gas, but not for water usage. This implies that we can exclude direct microeconomic income effects that would ascribe the increased electricity consumption to the additional disposable income generated by reduced expenses for water. All units are equipped with the same space and hot water heating (gas) and cooling (electric) system built into each apartment, the same water fixtures (faucets, toilets), and the same or very similar major appliances. The only exception is that 25% of the apartments – evenly distributed among the three buildings – have a gas instead of an electric oven). Therefore, we can exclude equipment-specific and building-structural aspects (e.g., level of insulation) as major factors influencing usage and attribute most of the variance in the utility consumption among apartments to behavioral factors and observed factors, such as number of occupants and floor space. The property management company emphasizes the "green living" aspects of the community, e.g., energy-efficient appliances, low-flow water fixtures, and dual flush toilets in all apartments.

3.2. Group assignment

Before information on the planned study was distributed to the residents, we assigned apartments to two experimental conditions, one that would receive weekly water consumption feedback (treatment group) for seven weeks and one that would not (control group). To facilitate the feedback distribution process and reduce the likelihood of information spillover[1] via discussions between participants in the two experimental conditions, we decided to implement a quasi-experimental design using the following group assignment: Building 1 was entirely assigned to the control group, building 3 entirely to the treatment group, and apartments in building 2 were randomly assigned to the treatment and control groups. The two groups did not reveal a significant difference in any of the observed variables.

Two weeks before the study began, every apartment received a one-page information sheet describing the organization conducting it including a contact address, the utility data that would be collected anonymously from each apartment for research

[1] Spillover here is in the sense of information shared between treatment and control group households, not in the sense of behavioral spillover effect as defined above.

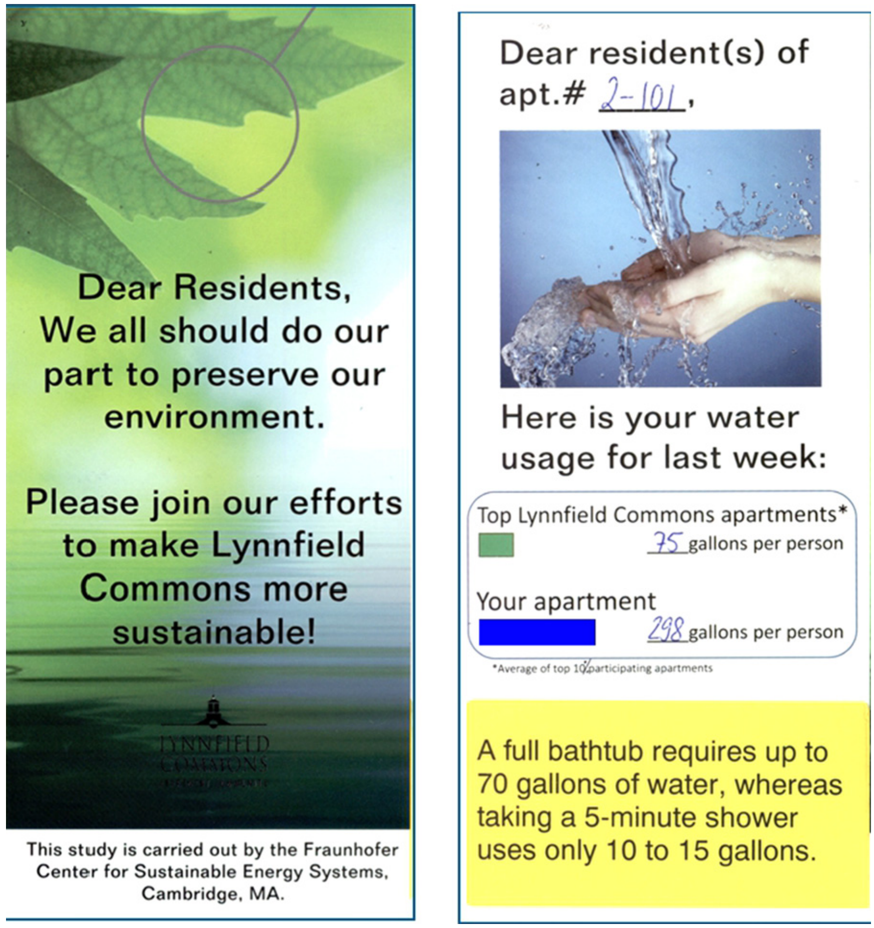

Fig. 2. Feedback flyer distributed to treatment group apartments. From left to right: Front side of weekly flyer and two examples for the personalized back side.

purposes over the next eleven weeks, and the possibility to opt out. The information page for the two experimental conditions differed in two respects: while apartments assigned to the control group were told that they would receive feedback on their utility consumption at the end of the study, treatment group apartments were informed that they would "receive energy-saving tips as well as feedback on your household's consumption in the form of a paper card that a researcher will slip under your door once a week." Their version also included a small image of the feedback flyer to facilitate recognition in the future.

Three apartments opted out before the study began, and another three did so during the study. We excluded them from further data collection and analysis. In contrast to experimental studies requiring participants to actively opt into a study, this opt-out recruitment model allows for collection of data from a more representative sample (only about 3% opted out), while respecting the choice of households who do not wish to participate.

3.3. *Data collection*

Water meter readings for each apartment were collected by the Inovonics' TapWatch submetering system and updated every afternoon. Every apartment's utility meter is connected to a pulse counter/wireless transmitter unit that sends its meter reading once a day to a central data concentrator and communicator unit, from which daily meter readings are retrieved and stored by the system provider. Feedback flyers on the previous week's water consumption were distributed on Wednesdays by members of the research team. During these visits, they also read the electricity meters of all participating apartments.

3.4. *Intervention*

Our intervention consisted of a series of seven double-sided water consumption feedback flyers that were slipped under the door of treatment group households on a weekly basis (see Fig. 2 for an example of a front and back side). We placed the flyers in such a way as to be barely visible from outside the apartments for privacy reasons and to avoid drawing attention from control group households, yet allowing the researchers to check whether they had been picked up from the floor. With the exception of residents who were absent over extended periods, flyers of the previous week had always been picked up by the residents when we distributed the next flyer. We did not choose an electronic format (email) to avoid limiting our study sample only to people with an internet access. The format of the flyers was chosen on the basis of a review of existing programs, featuring elements that are widely used in large-scale campaigns (e.g., social comparison, social appeal to do one's part, concrete conservation action).

To facilitate recognition of subsequent flyers as part of the same campaign, all seven feedback flyers came with the same front side (Fig. 2 on the left) with an appeal to environmental social norms, the property logo to underline community identity and the fact that the campaign was backed by the property management. The backside (Fig. 2 on the right) contained that week's water conservation tip and a personalized section with the

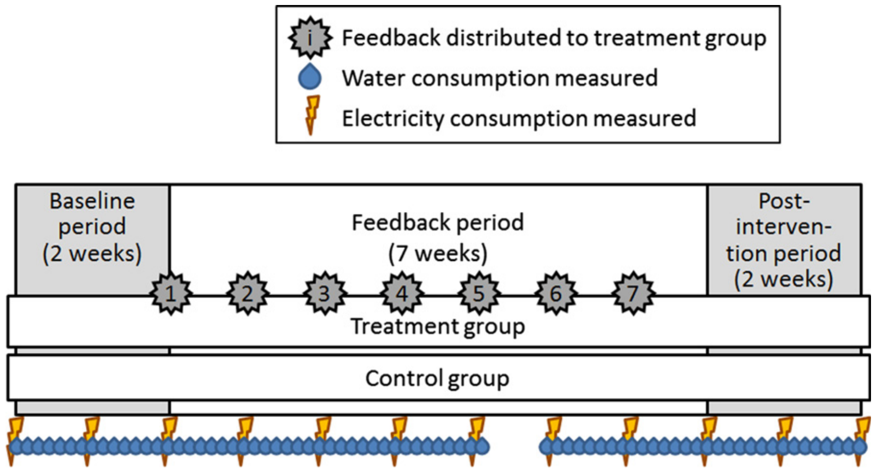

Fig. 3. Illustration of the study (raw dataset) with its three phases baseline period, feedback period (distribution of the seven feedback flyers to the treatment group), and post-intervention period. Water consumption values were collected on a daily basis, electricity consumption values on a weekly basis.

apartment number and its per capita water usage of the last week compared to the "Top Lynnfield Commons apartments (average of top 10% participating apartments"). We chose the top 10% apartments instead of the mean of all apartments to avoid having to distinguish between apartments above and below the mean, respectively. Otherwise, below-average consumers might be encouraged to adjust their water consumption upwards towards the social norm (Schultz et al., 2007). By taking the mean of the apartments below the 1st weekly decile, only two to six apartments per week received a message that their apartment's consumption was below the mean of the 10% reference group. The remaining apartments were in the control condition or above the reference group mean; those below the reference mean were typically units that had not been occupied for several days that week, so their inhabitants would probably not interpret their low consumption value as a consequence of being excessively "green". On the other hand, when asked for the usefulness of that comparison in a follow-up survey, none of the respondents indicated the suspicion that the relatively low consumption value of the reference group might be due to a higher absence rate for that group. All water conservation tips stated a concrete action and its associated weekly water savings potential based on the water flow rate of the fixtures installed at the property (e.g., "Shorten your shower by a minute or two and you'll save up to 20–35 gallons per person per week. Turn off the water while soaping or shampooing"). We did not include advice that would have simultaneously affected electricity consumption, such as behavior concerning the dishwasher or washing machine.

4. Data analysis and results

This section summarizes the impact of the water feedback campaign on water and electricity consumption. We first present the impact of the intervention on residents' water consumption. We offer measurable evidence that residents in the treatment group did engage in behaviors that reduced their water consumption relative to the control group; this might cause side effects on other behaviors according to the licensing effect or (positive) spillover theory.

The raw dataset consisted of daily water and weekly electricity meter readings from May 4, 2011 through July 19, 2011 (11 weeks) of all 200 apartments in the complex. In addition, the property management provided us with data on the number of occupants per apartment, floor space, location of each apartment number in the building, the major appliances installed in the apartments, and a list of move-ins and move-outs during the study. Fig. 3 depicts the study timeline with the seven feedback distribution events after two weeks of the baseline period as well as the measurement events (daily for water, weekly for electricity).

Fig. 4 gives a schematic overview of the steps taken to analyze the data. After several apartments were filtered out (described just below), water and electricity consumption data were controlled for observed variables (e.g., number of occupants per apartment). Thereafter, treatment and control group datasets were separated; both were normalized to the control group mean of each measurement interval to adjust for time-dependent factors (e.g., weather). Then the normalized data of each group were pooled by study periods (Fig. 4).

To ensure that differences between the groups were not simply due to different occupancy patterns, we used water consumption values to infer apartment vacancy. Unlike electricity, water is usually only consumed when someone is at home. This allows an accurate inference of the vacancy of an apartment over several days using the daily water consumption data. An exploratory analysis of the dataset revealed that water meters reported water consumption values up to 2 gallons (8 l) per day for vacant apartments (measuring uncertainty). We therefore considered days with water consumption up to eight liters as "absence days." Apartments with longer periods of absence were entirely excluded from the study (a total of 16 units; see the following paragraph); short periods of absence and single absence days were excluded from the remaining water consumption dataset and controlled for in the weekly electricity data to reduce the variance. Both experimental groups showed no difference in the number of absence days in all phases of the study (see the analysis of electricity data for details).

Based on the same exclusion criteria for all apartments, we excluded 14 apartments due to vacancy/late move in at the beginning of the study; nine apartments were excluded for technical reasons (water meters reporting zero/constant consumption every single day); six due to opt-out of the residents; and another 16 were excluded due to move-outs, change of tenants, or extended periods of absence (absences of 15 consecutive days or more or more than eight days during baseline period). One more apartment was excluded as an extreme outlier (leakage or a defective meter assumed), as its weekly per capita

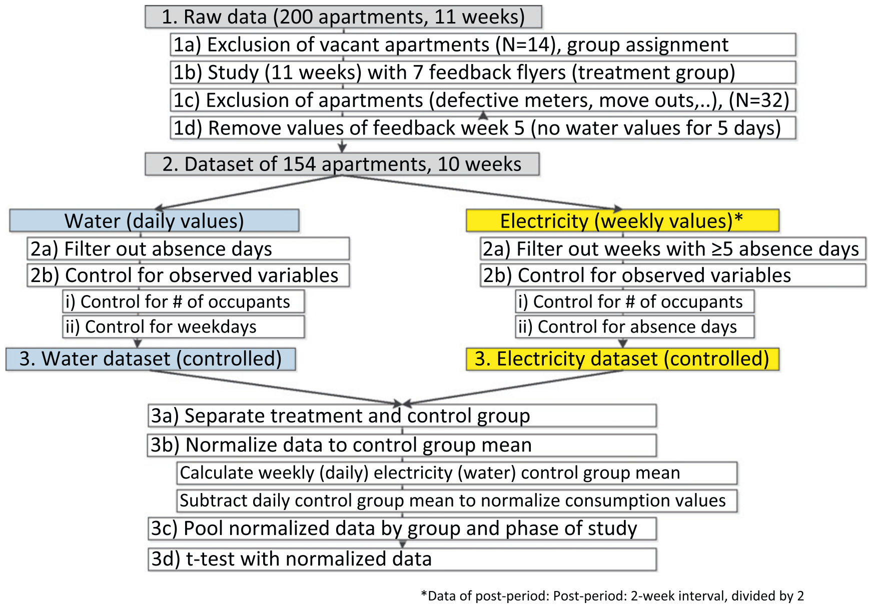

Fig. 4. Steps undertaken to analyze water and electricity consumption data from the raw meter readings to the final dataset analyzed.

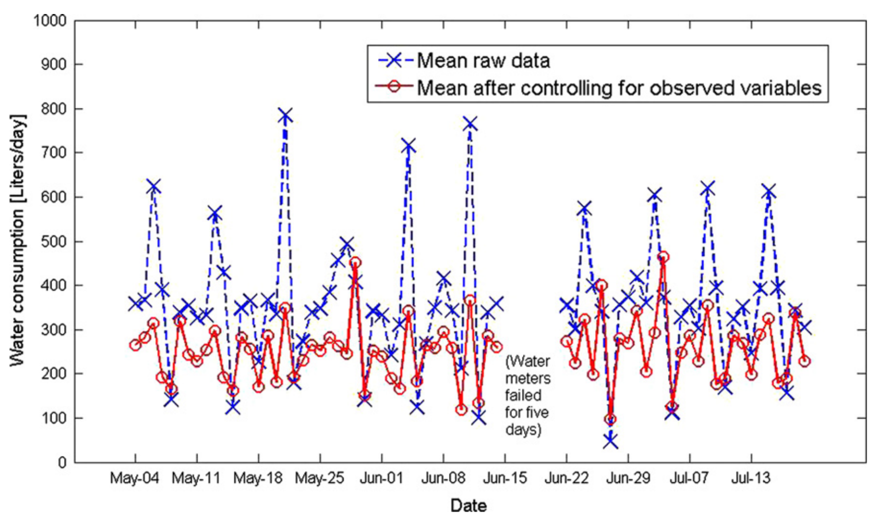

Fig. 5. Means of daily water consumption before and after controlling for absence, number of occupants, and weekdays.

water consumption was up to 10 times the average consumption of the other participants. In the end, data from 154 apartments or a total of 275 individuals were analyzed for the study, with 77 apartments in the treatment and 77 in the control group. The two groups did not show a significant difference in floor space (M_T=90.7 m^2 vs. M_C=90.4 m^2, p=0.89), the number of occupants (M_T=1.71 vs. M_C=1.86, p=0.31), utility consumption (see following sections), percentage of apartments with a gas oven (22% and 23%, respectively), or any other observable factors. Due to a failure of the water meters (five days without daily updates in week 5 of the feedback period, June 17–21), we entirely excluded that week from our analyzes in order to analyze water and electricity data of identical time periods. The final study period therefore covered two weeks of the baseline period, six weeks of the intervention, and two weeks of the post-intervention period.

The following two subsections describe the analysis in greater detail and outline the results for both water consumption data (target behavior of the campaign) and electricity usage data (potential side effects).

4.1. *Effect of the campaign on the addressed consumption behavior (water)*

The variability in the water consumption data was high, both within and between households. An exploratory analysis showed a strong correlation of household water consumption with the number of occupants and weekdays; therefore, we controlled for these factors after excluding absence days. For that purpose, the values of apartments with more than one occupant were adjusted with a correction factor based on the ratio of means: the mean water consumption of all apartments with i occupants (i=1, 2, 3, 4) was calculated for each day. Then we took the ratios of these means on a daily basis; the means of these daily ratios were used as correction factors (for other examples and more details on this ratio correction factor method see e.g., Cundiff et al., 1966; Breslow and Day, 1975; Gfroerer, 1998; Ruijter et al., 2006). The same approach was then followed to control for weekdays (for instance, the mean water consumption on Fridays and Saturdays was on average twice as high as on Sundays and Mondays).

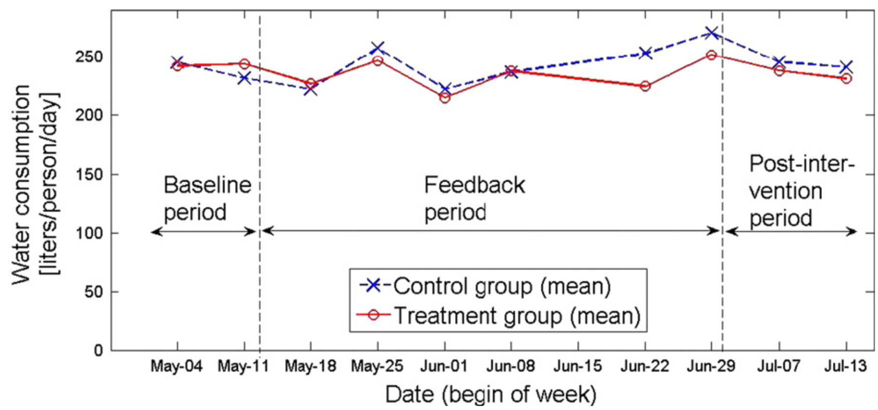

Fig. 6. Daily water consumption means (averaged one weekly basis) after controlling for number of occupants, weekdays, and filtering out absence days.

Table 1
T-test of pooled water consumption values, normalized to daily control group mean, for baseline, feedback, and post-intervention period.

Study phase	*N* treatment (no. of observ.)	*N* control (no. of observ.)	Effect size vs. baseline (%)[a]	*t*-statistic	*p*-value
Baseline	1008	995	/	−0.63	0.529
Feedback	2961	2926	6.0	2.10	0.036*
Post	997	997	5.5	1.09	0.275

[a] Calculation of the effect size with difference in difference method: (cTreatment, Period-cTreatment, Baseline)-(cTontrol, Period-cControl, Baseline); c stands for water consumption, period being a placeholder for feedback period and post-intervention period.

* $p < 0.05$.

Fig. 5 shows the variation of the daily median water consumption before and after excluding absence days and controlling for the number of occupants and weekdays. This procedure reduced the absolute value of the standard deviation of the daily means from 148 to 64 l, and the ratio of the standard deviation/mean of daily means from 0.42 to 0.27. Taking into account all 10,780 observations (70 days, 154 apartments), the mean daily water consumption was 356 l/apartment/day, with a median of 265 l/apartment/day and a standard deviation of 350 l/apartment/day (98% and 132% of the mean and median value, respectively).

Hereafter (step 3a in Fig. 4), the dataset was separated into treatment and control groups. The daily control group mean was subtracted from every apartment's daily water consumption to normalize for unobserved time-dependent effects before pooling the data of the two experimental groups into the three periods of the study baseline (two weeks), feedback (six weeks) and post-intervention period (two weeks).

During the baseline period, the treatment and the control group used a similar amount of water (M_C=238 l/person/day, M_T=242 l/person/day); the daily treatment group mean was on average 1.9% above the control group's. By contrast, during both the feedback and the post-intervention period, the mean daily treatment group consumption was 4.1% below the control group mean on average. Fig. 6 illustrates the mean water consumption (aggregated on a weekly basis) of the two experimental groups. Table 1 shows the test statistics of the daily water consumption for baseline, feedback, and post-intervention period after normalizing both experimental groups to the daily control group mean. Whereas there was no significant difference in the water consumption between the two groups during the baseline period (p_{baseline}=0.53), the treatment group used significantly less water during the feedback period (p_{feedback}=0.0036); consumption in the post-intervention period did not show a significant difference (p_{post}=.27).When we normalized to median values instead of means (medians being more robust to outliers), we obtained similar values (p_{baseline}=.55, p_{feedback}=.033, p_{post}=.26).

We can thus assume that the campaign did have a measurable impact on the target behavior (water consumption).

4.2. Adverse effects of the campaign (electricity consumption)

Electricity consumption data in this study differ from water consumption data in three major respects. First, they were only collected on a weekly basis. Second, they were less subject to absence than water usage, and third, they were highly dependent on outdoor conditions, as air conditioning represents a large fraction of electricity consumption in the summer. As electricity consumption data are aggregated over one week, we could not filter out "absence days" from the analysis as we did for water. Instead, after controlling for the number of occupants (by following the same procedure as described for water), we controlled for the number of absence days per week, inferring absence days per week from the water consumption data. Correction factors were calculated by following the same procedure as for the number of occupants. The weekly mean electricity consumption was calculated over the number of absence days per week, then the values were adjusted with this correction factor. Weeks with five or more absence days were excluded due to the small number of data points. We also analyzed whether there was a difference in absence days per week between the two groups that might explain a difference in electricity consumption. However, the two conditions did not show a significant difference of absence days per week during any phase of the study, neither in the baseline period (treatment: M_T=0.33, SD_T=0.77; control: M_C=0.31, SD_C=0.73, $t(300)=-0.18$, p=0.85), nor the feedback period (treatment: M_T=0.45, SD_T=1.07; control: M_C=0.45, SD_C=1.12, $t(905)=0.01$, p=0.99), or the post-period (treatment: M_T=0.25, SDT=0.68; control: M_C=0.36, SD_C=0.31, $t(291)=0.68$, p=0.50). As electricity data for the post-intervention

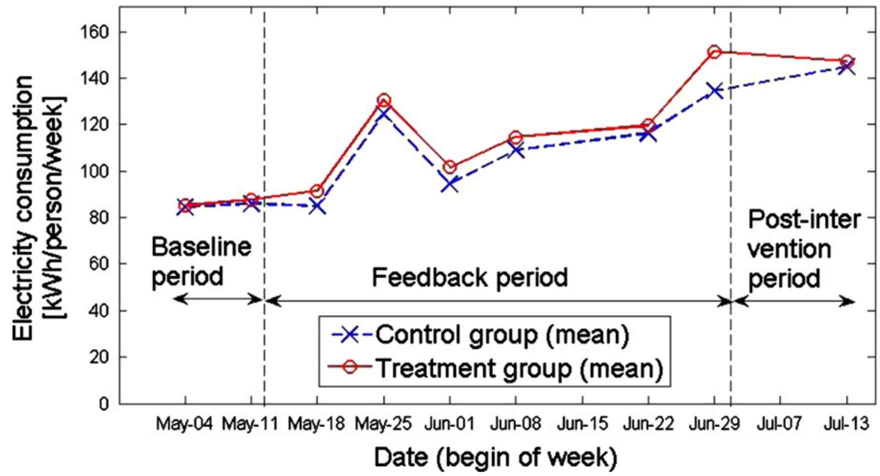

Fig. 7. Weekly electricity usage means of the two experimental groups after controlling for number of occupants.

Table 2
T-test of pooled electricity consumption values, normalized to the weekly control group median, for baseline, feedback, and post-intervention period.

Study phase	*N* treatment (no. of observ.)	*N* control (no. of observ.)	Effect size vs. baseline (%)	*t*-statistic	*p*-value
Baseline	154	149	/	0.03	0.757
Feedback	456	451	5.6	−1.88	0.035*
Post	76	76	0.3	−0.34	0.660

period had only been collected after two weeks' time, we divided this value by two and used half of the sum of absence days of these two weeks as control factors, thus creating a comparable metric for the post-intervention period. In the end, our analysis included 1362 valid observations (154 apartments times 9 (2+6+1) measurement intervals for baseline, feedback, and post-intervention period, reduced by 24 data points for absence over five or more days of the week). The mean daily electricity consumption was 111 kWh/person/week, with a median of 101 kWh/person/week and a standard deviation of 54 kWh/person/week (48% and 53% of the mean and median value, respectively).

Fig. 7 shows the weekly electricity consumption means. As one can see, electricity consumption between the first and last week of the study increases by approximately 75% for both groups. This is due to increased outdoor temperatures and resulting higher electric consumption due to air conditioning: while the first weeks of the study took place in moderate conditions (14 °C daytime average), the last weeks of the study coincided with the warmest days of the year (28 °C daytime average). During the feedback period, the treatment group mean is on average 6.9% above the control group mean, compared to 1.3% during the baseline period and 1.7% during the post-intervention period.

Table 2 shows the test statistics of electricity usage for the baseline, feedback, and post-intervention periods after normalizing both experimental groups to the weekly control group mean. Whereas the difference in the electricity consumption between the two groups was not significant during the baseline period ($p=.757$), a significant difference between the two groups was found during the feedback period ($p=.035$). Additionally, we normalized the values to the control group median (instead of the mean) as a value that is more robust to outliers; the results obtained were very similar (baseline period: $p=.760$, feedback period: $p=.037$).

5. Discussion

This paper presents one of the first quantitative field studies on cross-domain effects in residential utility consumption behavior. We investigated the side effects of a water conservation campaign on residents' electricity consumption and found evidence that people exposed to the water campaign did reduce their water consumption as expected. Yet at the same time, they increased their electricity consumption relative to the control group.

While we do not have a proof for the precise mechanism of the psychological process at work, our findings are consistent with what the majority of articles that analyze incongruous actions describe as moral licensing. In the energy economics field, the concern for increased consumption following the adoption of an environmental action is typically subsumed as "boomerang effect" (Goeschl and Perino, 2009; Harding and Rapson, 2013), yet this also includes phenomena from neoclassical economic theory, e.g., income effects. Some studies that report incongruous actions frame the mechanism as "guilt reduction" (Gneezy and Rustichini, 2000), "moral cleansing" (Sachdeva et al., 2009) or a "warm glow effect" similar to charitable giving (DellaVigna et al., 2009; Kotchen, 2009). Ultimately, the boundaries between the terms are blurry and the topic is currently subject to a rapidly growing body of research across disciplines. After all, the implications of such observations are highly relevant across domains, not only for environmental campaigns and energy policy.

The following paragraph quantifies the impact of behavioral spill-over in this study using a net energy balance and then proceeds to a discussion of its potential limitations. We conclude with direct implications of our findings for the design and evaluation of environmental campaigns and policy. Based on our findings, we advocate the adoption of a more comprehensive system-level perspective in the evaluation of environmental programs.

To quantify the environmental impact of the observed cross-domain effects, we compared the energy saved through water conservation with the increased electricity consumption. In this study, the treatment group reduced its water consumption by 6.0% or 15 l/person/day relative to the control group. Typically, approximately 40% of domestic (excluding yard) water consumption is hot water (U.S. Department of Energy, 2012); assuming that residents saved a similar ratio of hot and cold water, the energy conserved by reduced water usage in the study is 0.5 kWh/person/day.[2] On the other hand, electricity consumption increased by 5.6%, resulting in an additional electricity use of 0.89 kWh/person/day (111 kWh/7 days $\times$ 5.6%). Thus, in terms of on-site energy balance, the energy saved by reduced (hot) water consumption was offset by the increased electricity consumption by nearly a factor of two. Extending our lens to source energy, we would have to take into account source-site factors for losses that are incurred in the production, transmission, and delivery to the site. Using EPA's national average values for electricity (3.34) and natural gas (1.047) (EPA, 2011c), the net energy balance from a source energy perspective is even more negative, by a ratio of about 1:6 (energy savings from hot water conservation vs. additional energy from increased electricity consumption). By comparison, the energy conserved from reduced water consumption in terms of water treatment is much smaller, approximately 0.2 kWh/person/day.[3] Thus, both in terms of on-site usage and even more in source-energy balance, this campaign had a clearly net negative energy outcome despite its success with respect to water conservation.

Although a growing body of literature has found broad evidence for moral licensing, most of these studies have been carried out in a laboratory setting or are based on self-reported behaviors in surveys. A limited number of very recent studies have looked into implications of moral licensing for green electricity tariffs, but negative side effects through moral licensing are still a blind spot in program design and evaluation. After all, these kinds of psychological mechanisms are not limited to patterns of household electricity consumption, but can also extend to energy consumption and supply in general, food, transportation, and overall consumer choice.

Our findings raise many questions about the net outcome of energy efficiency information campaigns and policymaking. First, environmental campaigns that are motivated by a sense of "every bit helps," or the hope that they might create a window of opportunity to more meaningful environmental behavior, can potentially be harmful and should be evaluated carefully. Not only can they waste individuals' time and effort on low-impact activities, they might also generate a warm glow effect of "already doing something," both among individuals in their daily lives and among policymakers in programs they support.

This might be amplified by the general public's poor understanding of the energy consumption impact associated with different behaviors (Attari et al., 2010): people might invoke low-impact behaviors of such campaigns as confirmation of their environmental engagement. As Gardner and Stern (2008) put it in their "Short List" of household actions to curb climate change: "When people are faced with a laundry list of advice, [...] they may carry out one or two actions—probably the easiest to remember and perform. However, the behaviors that are easiest to remember and perform, for example, turning out lights when leaving rooms, tend to have minimal impact on climate change. Thus, long and unranked lists of behaviors are likely to be ineffective at best and may even be counterproductive, if they lead people to feel satisfied that they have done their part after accomplishing very little." In combination with the human tendency to choose the easier alternative of environmental actions for oneself (Attari, 2011), these campaigns might actually crowd out environmental actions that would result in higher energy savings or CO_2-abatement, or license negative behaviors that people might otherwise abstain from, such as increased electricity consumption or airplane travel.

Second, it might be the case that a considerable amount of our environmental program efforts and funds actually generate a much smaller – or even a negative – net impact on CO_2 emissions than our current program evaluations suggest. We should take these considerations into account in the evaluation of future environmental campaigns and policy. In particular, the long-term implications will be of interest here: just as the positive effects of many behavior-based efficiency campaigns fade over time, we need to understand whether the negative side effects caused fade away even more quickly or are more persistent than the positive outcomes. Policymakers need to know whether they have to account for some short-lived side effects, or whether a program might actually create side effects that reduce, negate, or even exceed its benefits not only in magnitude, but also in persistence over time. Our findings are also relevant for the ongoing environmental policy debate on policy strategy and individual responsibility. Energy efficiency can be achieved through individual behavioral change (curtailment) or through better technology and structural changes. The latter is often costly and resulting energy efficiency benefits may be affected by the rebound effect. Consequently, many interventions encourage individuals to change their attitude, values and behavior (the 'ABC' paradigm of attitude, behavior, and choice). However, that kind of policy faces increased criticism for yielding only marginal, incremental improvements, while reinforcing the status quo of the current system, and deflecting attention away from the many institutions involved in structuring possible courses of action (Shove, 2010; Stern, 2000). If individual curtailment programs license other negative behaviors and crowd out investments in better technology, curtailment programs should be analyzed with even greater caution for their potential to solve environmental problems.

Third, we need to acquire a better understanding of these mechanisms that can influence behavior across domains and of their magnitude in order to develop programs that minimize their risk or impact. Therefore, it might be necessary to develop environmental messaging that prevents people from overestimating the positive impact of their pro-environmental actions or that focuses on actions with the greatest impact. It will be a challenge to find the right balance between communicating that an individual's behavior is important, without providing people with a license for less pro-environmental choices in other domains. Theory on moral licensing that has been developed in other behavioral domains could help to address this issue, for example by making environmental behaviors more important to individuals' identity, framing campaigns with respect to goal commitment instead of progress, or making hypocrisy visible (Dickerson et al., 1992; Miller and Effron, 2010).

If these energy policy implications are not taken seriously, the current environmental campaign focus on a single behavior might

[2] Underlying assumptions: Energy E [kWh] required: $E = m \times c_p \times \Delta T/EF$, where m is the mass [kg], c_p the specific heat [kJ/kg/K], ΔT the temperature difference between cold and hot water [K], typically 45 K (from 10 °C to 55 °C), and EF the water heater energy factor, typically 0.61 for gas heaters (U.S. Department of Energy, 2011). This results in energy savings of $15\,l \times 0.4 \times 4.179\,J/l/K \times 45\,K/3600\,J/kWh/.61 = 0.5$ kW h per person per day.

[3] EPRI (EPRI (Electric Power Research Institute) (1996)) reports a total electricity use of 1.4 kWh/1000 gallons of water produced at a typical water treatment plant and 1.8 kWh/1000 gallons for production at a typical groundwater utility, resulting in approximately 15 l/(3.785 l/gallon) $\times$ 1.6 kWh/gallon $\times$ 3.34 = 0.2 kWh source energy conservation per person per day from the reduced amount of water treated (including energy for pumping).

result in our missing crucial parts of the whole picture and could lead to repercussions on other energy-related behaviors that may be greater than the outcome of the targeted behavior. We therefore recommend the adoption of a more comprehensive view in the evaluation of programs and future scenarios that incorporates the potential impact of licensing effect. This will help to develop more accurate economic models and make predictions about CO_2 emissions more realistic and reliable.

The results of this study are subject to a number of limitations. Our research focuses on the analysis of directly measurable short-to-mid-term behavior change, not long-term predictions. We are aware that certain behavior change processes might require more than a couple of weeks to take effect and to stabilize into long-term habits. For instance, in the study by Harding and Rapson (2013), the crowding-out of conservation diminished after 3–6 months. In that regard, research on mid- and long-term processes will certainly contribute important complementary aspects to our analysis of directly measurable short-to-mid-term impacts, but it will also be subject to an even greater variety of other unobservable factors. Our paper makes a valuable contribution to a better understanding of the direct and measurable side effects of behavior change programs; further research is necessary to investigate the long-term effects of such interventions.

Caution is also warranted concerning the interdependence of water and electricity consumption. To a certain extent, household water and electricity usage is coupled. For some behaviors, such as clothes washing or running the dishwasher, conserving water also conserves energy. On the other hand, we have not identified behaviors that substitute electricity consumption for water consumption. From this perspective, savings or increases in water consumption would result in changes in the same direction for electricity consumption. To reduce the interdependence, we did not include behaviors with a known interrelation in our water conservation tips. Nevertheless, as our feedback campaign addressed apartment water usage in general, it is likely that at least some individuals did not only engage in the water-saving actions that were explicitly pointed out as water conservation tips on the flyers. In addition, they might have themselves identified some commonsense means of conserving water (e.g., reducing the number of laundry loads) that would also have affected electricity savings—however, in a positive correlation with water conservation, whereas we found evidence for increased electricity consumption. This implies that the real impact of the licensing effect might be even greater than our data suggest, as the additional electricity consumption of some licensed electricity consumption behaviors would be offset in the data by the electricity savings from a reduced number of laundry loads.

Another limitation of our results concerns the seasonal nature of water and electricity consumption. As parts of the study coincided with some of the warmest weeks of the year, the use of air conditioning highly influenced electricity consumption: the median electricity consumption in the warmer weeks was more than 50% higher than in the more moderate weeks. Both air conditioning in summer and space heating in winter account for a large portion of a household's energy consumption. Besides, both are more subject to regular user adjustments than other end uses, making the occurrence and measurability of licensing more likely than in moderate climate conditions.

Finally, by using water and electricity consumption values that are aggregated on a household level, we cannot specify what behaviors were influenced by the intervention and to what extent. Also, these values do not reveal participants' perceived efforts, attitudes towards the campaign and towards their own behavior. Follow-up research is necessary to better understand and the specific psychological mechanisms that lead to the observed difference in electricity usage between the two groups.

Despite these caveats, the results of our study underscore the need for more research to better understand the underlying psychological mechanisms, to verify and quantify the environmental impact of moral licensing in similar contexts and other areas of environmental consumer behavior, as well as to reliably quantify the magnitude and persistence of such cross-domain effects on a larger scale.

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