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The Economic Value of Habits in Household Production – A Field Experiment *

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Abstract

People are prone to habits but how important are they for economic outcomes? In a randomized field experiment we compare two treatments with identical economic incentives to adjust a range of household production activities. Treatments only differ in the extent to which they allow households to adjust habits relevant for these activities. We utilize smart-metered hourly power consumption to unobtrusively measure treatment effects. We find that preventing habits from being adjusted reduces consumer surplus by 76% suggesting a substantial economic value of habits.

JEL Classifications: C93, D12, D90

Keywords: habits, field experiment, household behavior, text messages, electricity use.

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I. Introduction

Shopping for groceries, doing the dishes and checking e-mails are repeated daily in households across the world. When conditions allow, we tend to perform such repeated activities in the same way. When we do repeated activities in the same way each time, it may be because it is optimal to do so. Most of us would, however, recognize that when we follow the usual routine, it is often because it is easier than thinking through the different alternatives in an attempt to optimize the task at hand every time we need to do it. We economize on cognitive effort by developing habits that allow us to do our daily tasks and chores with less conscious attention. The gain from saving on cognitive resources in this way presumably outweighs the loss from deviating from the strictly optimal way of performing the task on each particular occasion.

The importance of habits[1] for behavior is recognized both in the psychology literature (e.g. Wood and Neal, 2007) and by economists.. Frank (1987) argues that relying on habits and non-rational decision rules to economize on scarce cognitive resources is, in fact, consistent with a "fully rational" response [2] The theory suggested by Becker and Murphy (1988) captures this by implying that past consumption can drive current consumption through a stock of habits. Like capital stocks, this stock of habits is a capital input to household production of final consumption goods from which utility is derived. Households invest in the stock of habits and derive benefits in the form of greater production efficiency when undertaking the pattern of household production supported by these habits. For example, Allcott and Rodgers (2014) appeal to this theory when suggesting that a stock of energy conservation habits can make it less costly for households to reduce energy inputs to household production of final consumption services like heating and lighting.

There is mounting empirical evidence of the importance of habits for household production. Behavioral experiments and observational studies in many contexts find that household production choices that are made frequently are often governed by habit. This literature includes transportation

[1] Verplanken and Roy (2015, p.247) define habits as automatic responses that develop "as people repeat actions in stable circumstances". This definition captures the three main features of habit formation: repetition, automaticity (arising from unconsciousness of the decision and lack of control) and context stability (the behavior is triggered by cues in the environment rather than deliberate decisions).

[2] Other economists that have proposed or assumed models that embrace cognitive costs and habits as a way of economizing on them include Simon (1972), Macey and Brown (1983), Pollak (1970), Deaton and Muellbauer (1980), Jager et al. (2000), Kurz et al. (2015).

choice (e.g. Aarts, Verplanken and Knippenberg 1998, Thogersen 2006, Gardner 2009), shopping behavior (Sheth and Venkatesan 1968, Chiu et al. 2012), recycling (e.g. Tonglet et al., 2004), exercise (Charness and Gneezy, 2009) water conservation (Ferraro et al., 2011) and energy conservation (e.g. Macey and Brown 1983, Ehrhardt-Martinez 2011, Allcott and Rogers, 2014, Ito et al., in press). However, the fact that habits often seem to govern repeated behavior does not necessarily imply that the associated economic gain is large. For this to be the case, habits must imply substantial increases in the net benefits derived from performing repeated tasks in the household. Introspection and the cited empirical studies suggest that this is the case, but we know of no studies that have tried to estimate the economic value of habits in household production. Therefore, this is our goal in the following.

We designed a field experiment that aims to induce households to shift the timing of a range of repeated everyday tasks. Households that accepted the invitation to join the experiment were randomly assigned to two treatments. In both treatments, households were given an incentive to shift power consumption either *into* (increase consumption) or *away from* (decrease consumption) the same specific 3-hour time slot each day over the course of 2 months. In each treatment, incentives were evenly split between moving power *into* the designated time slot, and moving power *away from* the same time slot. The only difference between the two treatments was how these incentives were allocated across households. In the first treatment, called the 'habit' treatment, any given household got the same treatment every day. Any given household either always got an *into* incentive or always got an *away from* incentive. Therefore, all households in this treatment could adjust their habits to a new situation because they knew that they would get the same incentive every day, for the whole duration of the program. In the second treatment, called the 'no habit' treatment, households were given the same incentives to shift tasks either *into* or *away from* the designated time slot. The difference to the first treatment was that incentives to any given household were not the same every day, but changed from one day to another (in a random way). Some days, the households in the treatment got an incentive to shift *into* the specified 3-hour time slot, while on other days they got an incentive to shift *away from* the same time slot. Therefore, the households in this treatment could not adjust their habits to the same extent as households in the first treatment since the direction that they were asked to shift power changed randomly from day-to-day and it could not be predicted. Importantly, however, the *total* number of treatments, their timing and their distribution across into and away from incentives was the same for both treatment groups. Thus, the only difference between the two treatments was that households in the first treatment knew they would face the same incentives every day and could more easily plan ahead and form a new habit. Households could respond to these incentives by changing the time at which they performed a wide range of activities that use power, such as doing the laundry and drying, dish washing, cooking, watching TV, etc. Thus, the incentives we give potentially affect many important repeated daily household tasks for which habits could be relevant.

By utilizing smart meters, we unobtrusively measure shifts in power consumption as in indicator of household reactions to the incentives in our treatments. This allows us to estimate the effect of the incentives when households are allowed or prevented from adjusting (some of) the habits relevant to the repeated activities. We derive the relative economic value of habits for the activities we investigate using revealed preference theory. During the first two months of the experiment, we found that the incentives we applied in the no habit treatment induced one third of the response compared to that of the habit treatment. Assuming standard functional forms, this translates into a 76% reduction in consumer surplus when habit adjustment is impeded. This indicates a substantial economic value of being allowed to form a habit for the bundle of household activities requiring electricity consumption, targeted through this experiment.

In a related study, Ito et al. (forthcoming) use a critical peak pricing experiment to find that economic incentives can change habits associated with electricity consumption and that this change persists after the incentives are discontinued. However, we are not aware of any prior studies estimating the economic value of habits. There is a substantial literature on electricity consumption using pricing experiments similar to our treatments. Studies similar to our habit treatment with price changes at fixed hours each day (so called 'time of use pricing') are numerous (see, e.g. Faruqui and Sergici, 2010 for a comprehensive review of electricity pricing experiments). Recently, a number of experimental investigations similar to our no habit treatment (so called 'real time pricing') have been published (see e.g. Wolak, 2011, Kessels et al., 2016). The innovation in our design is that we combine fixed and dynamic treatments in a randomized trial so as to be able to control for confounders, making it possible to identify the value of allowing habits to adjust. This has not been done before.

Our results are, of course, particular for the type of behavior and field setting we investigate. Also, the estimated economic value of habits that we report applies to the (unobserved) weighted average of power-using tasks that households perform in response to the incentives. Nevertheless, the incentives in our experiment potentially affect the broad set of repeated household activities that use power such as cooking, doing the dishes, doing the laundry, watching TV, etc. If habits are generally valuable (in the sense that they imply substantial increases in the net benefits of doing repeated activities), then settings that allow habit formation would be perceived as more attractive and would induce greater changes in household behavior than settings that do not. If this is the case, policies and products that allow households to utilize habits are likely to be more successful than policies and products that do not. Our result suggests that taking account of habits may, in fact, be very important for understanding economic behavior and ultimately for the design of policies, products and marketing.

In the following section, we introduce the concept of habits in repeated behavior and their economic value. The third section provides a description of the field experiment. The fourth section presents and discusses our results and the fifth section concludes the paper.

II. The economic value of habits

Habits allow us to expend less cognitive effort on life's smaller decisions, thereby reducing the conscious decision-making costs associated with that behavior. Thus, habits imply lower cognitive costs of repeated behavior in connection with household production, but also reduced day-to-day responsiveness to changing conditions around the activity. However, when permanent changes in the conditions are perceived (e.g. a new time slot for your favorite evening news program or the opening of a new store), habits can presumably be re-optimized to take them into account[3]. Verplanken et al., (2008) argue that new habits are formed after a disruptive change in the context in which the habits used to be activated (e.g. changing jobs, moving home)[4]. Allcott and Rodgers (2014) study suggests that disruptive context changes that change habits need not be as dramatic as moving or getting a new job. They find that introducing the O-power energy report feedback to

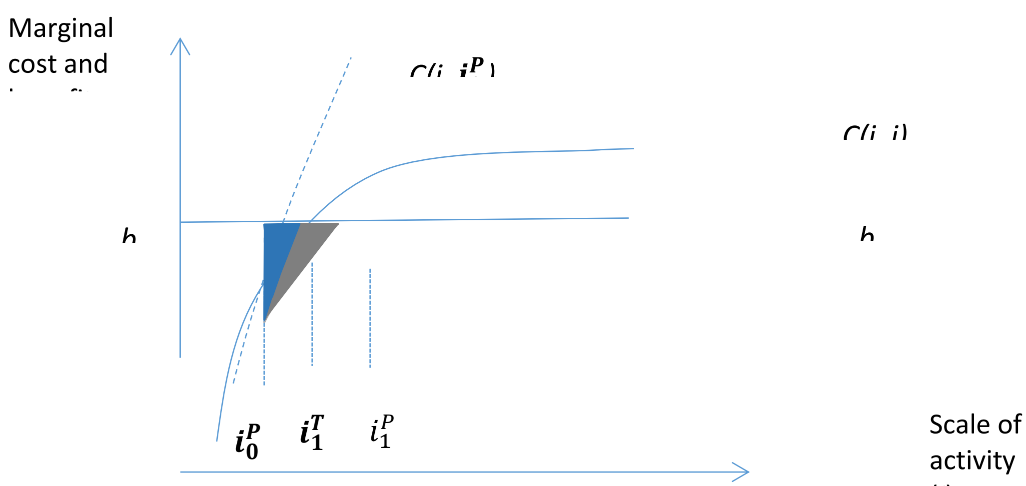
[3] A substantial body of literature suggests this, see e.g. Verplanken and Wood (2006) for a review.
[4] For behavioral experiments that suggest this, see e.g. Wood, Tam, Guerrero Witt (2005) and Werner, Rhodes and Partain (1998).

home owners affects behavior through a combination of changes to (the stock of) energy utilization habits and changes to physical capital stock. Of special relevance for our experiment are Ito et al. (forthcoming) who find that changes in electricity rates can induce changes in habits associated with electricity consumption.

In terms of the habit model of Becker and Murphy (1988), it may be possible for a household to investment in a re-optimization of its stock of habits when prices or other conditions relevant to the behavior in question change. However, such a re-optimization is costly (in terms of effort and time) and so it is more likely to be worthwhile if the change in conditions is perceived as permanent then if the change is perceived as temporary. This implies that the marginal cost of reacting to a change in the conditions relevant for a repeated activity likely depends on whether the change is anticipated to be temporary or permanent. When reacting to a temporary change in conditions, habits may not be re-optimized. Deviating from the pattern for which habits are optimized entails the use of cognitive resources to evaluate, plan and execute the deviation each time the activity is carried out. If the change in conditions is permanent and is perceived as such, then re-optimizing habits to take the changed conditions into account may be worthwhile. If habits are re-optimized, this reduces the marginal cognitive cost and therefore increases the net benefits of reacting to the changed conditions. This implies a stronger reaction to a given change in conditions if it is perceived as permanent then if it is perceived as temporary.

Figure 1 illustrates the net marginal costs (marginal costs minus marginal benefits) as a function of the scale of doing a repeated activity when habits are adjusted and when they are not. This could be the net marginal cost of using the dishwasher as a function of how often this occurs during the week. The scale of the activity is depicted along the x-axis (i) and marginal cost along the y-axis. Let h indicate the scale for which the household's habits with respect to this activity are optimized. This could, for example, be to use the dishwasher after dinner each day, in which case h=7. Let $C(i,h)$ be the marginal net cost of performing the activity i times a week when habits are optimized for doing the activity h times a week. The solid line curve $C(i,i)$ in figure 1 is the marginal cost curve that applies for *permanent* changes in scale where habits are adjusted to the new scale level. Thus, for each scale level i, this cost curve reflects the marginal costs of performing the task at this scale (i) when habits are optimized for precisely this scale level (i.e. so that $h=i$). We see that marginal net permanent costs are initially negative (so doing the activity involves net benefits initially), but these rise as the scale increases. Clearly it would be optimal to perform the activity at the scale (i_0^P.) where marginal net permanent costs just equal zero. Even so, the household could decide to run the dishwasher more often or less often than this *without* adjusting habits. The dashed line curve $C(i, i_0^P)$ in figure 1 is the marginal cost curve that applies when scale is adjusted away from i_0^P while habits are not adjusted and so remain optimized for a task scale of i_0^P. If the household adjusts task scale above or below this level *without* adjusting habits to the new scale (so that $i \neq i_0^P$ and $h = i_0^P$), the marginal costs increase or decrease as indicated by the curve. The marginal cost of increasing the scale above i_0^P without adjusting habits includes the extra time and effort used to perform more of the task and the cost of cognitive reflection and deliberation required when deviating from habitual behavior. This is the marginal cost curve that applies for *temporary* increases in the scale of performing the specific task. In contrast, the marginal costs of performing the activity at a particular scale (differing from i_0^P) when habits are adjusted do not involve as much time and effort for cognitive reflection and deliberation because habits are optimized for this scale. This is why the permanent cost curve is more concave than the temporary cost curve, as it shows that scale increases over i_0^P are costlier when habits are not adjusted to the new scale level (correspondingly, cost savings when reducing scale below i_0^P are lower when habits are not adjusted).

Figure 1: *Experimental identification of the value of habits.*



We expect a long run equilibrium that corresponds to the permanent marginal net cost curve, *C(i,i)*, because we expect that habits will eventually be optimized for scale i_0^P. Even though habits are optimized for i_0^P in this long run equilibrium, households will react to temporary changes in benefits in accordance with the short run cost curve $C(i,i_0^P)$. If marginal benefits at some point in time temporarily increase by *b* everywhere (corresponding to a shift in the optimal scale to the intersection with line *b*), the household will not adjust habits, but it will be advantageous to adjust the task scale to the level i_1^T, reflecting the intersection of the temporary net cost curve and line *b*. If the change in benefits is perceived as permanent and habits are re-optimized, the new scale i_1^P will be given by the intersection of the benefit curve and the habit optimized permanent net cost curve, *C(i,i)*.

From the difference in consumer surplus implied by these two cost curves, we can derive the economic value of habits. The consumer surplus derived from adjusting the scale to the new benefit curve when habits are also adjusted (to i_1^P) is the sum of the blue and grey areas (this consumer surplus is denoted as U^P). In contrast, the consumer surplus derived from adjusting the scale (to i_1^T) without adjusting habits (so $h=i_0^P$) is only the blue area (this consumer surplus is denoted by U^T). Thus, the utility value of being allowed to adjust habits to the new benefit curve is the gray area (that is U^P-U^T). We want to estimate the size of this area relative to the total consumer surplus derived from long run adjustment:

$$(U^P - U^T)/U^P \qquad (1)$$

We call this the *relative economic value of habits* because it indicates the relative importance for utility of being able to adjust habits.

The process of adjusting habits is in itself a costly investment. However, once the investment has been made, these costs are sunk and will not affect the optimal permanent scale conditional on habits being adjusted. However, these costs are important for how fast and flexibly we change habits in response to changing conditions. They are also potentially important for our estimate of the economic value of being allowed to adjust habits. Therefore, it is important to stress that the investment costs associated with actually adjusting habits as evaluated in (1) have *not* been taken into account. If these are low compared to the aggregate surplus over the period for which the new conditions are expected to apply, this omission is not a significant problem. However, if these investment costs are substantial, or the time period for which the new conditions are expected to apply is short, our estimate of the relative economic value of being allowed to adjust habits will be (possibly substantially) upwardly biased. Another potential cause for concern is that when households find it worthwhile to invest in habit changes, they may also find it worthwhile to invest in physical capital stock changes affecting the household production for which the habits we investigate are relevant. If this happens, our estimate of the relative economic value of habits becomes upwardly biased because we attribute marginal benefits to a change in habits that are actually caused by a change in the physical stock of capital. We return to this when we interpret our empirical results.

In order to identify the relative economic value of habits, we undertake a field experiment where we aim to induce a linear shift in the benefit function by giving a monetary payment *b* per unit increase in scale of the household's activity. If we assume that habit adjustment implies a proportional increase in the scale reaction to any given benefit *b*, we can identify the relative economic value of habits in our experiment (see Appendix 1 for the derivation) as:

$$\text{Relative economic value of habits} = 1 - \frac{\boldsymbol{i}_1^{\boldsymbol{T}} - \boldsymbol{i}_0^{\boldsymbol{P}}}{\boldsymbol{i}_1^{\boldsymbol{P}} - \boldsymbol{i}_0^{\boldsymbol{P}}} \tag{2}$$

In our experiment, $\boldsymbol{i}_1^{\boldsymbol{T}} - \boldsymbol{i}_0^{\boldsymbol{P}}$ is the treatment effect estimated in the no habit treatment and $\boldsymbol{i}_1^{\boldsymbol{P}} - \boldsymbol{i}_0^{\boldsymbol{P}}$ is the treatment effect estimated in the habit treatment.

III. Description of the experiment

The field experiment was conducted in Denmark by the electricity utility company SE[5]. A sample of 2,625 households was randomly selected[6] from SE's customer database and invited to join the

[5] More information about the utility company SE can be found on their website: https://www.se.dk/.
[6] Prior to randomization, businesses and seasonal dwellings were excluded.

MovePower program[7]. All customers received an invitation E-mail (see Appendix 2) in April 2014 asking whether they would be willing to be a part of the new program. In this E-mail, all households were given the same general information about MovePower (that they would receive suggested time slots for moving their power use through text messages (SMSs)). After receiving their formal consent to participate, the households were randomly allocated into treatments and only then given more detailed specific information to each treatment.

In total, 93 customers signed up to participate in the MovePower program[8]. Two households were removed from the analyzed sample *ex post* due to faulty remote measurement meters[9]. Both treatment groups were sent SMSs requesting them to move some of their daily power consumption sometimes *away* from and sometimes *into* the same 3-hour time slot (between 8PM and 11PM). The first text messages were sent on the 27th of May 2014. We analyze the households' reaction to the experiment up to 31st July 2014.

Both treatment groups were given the same types of incentives to move *away* from and *into* the time slot. The proportion of *away* from and *into* incentives was the same in the habit and no habit treatments (50% in each direction). Treatment groups differed, however, in the proportion of *away* from and *into* instructions given to the *individual* household and in the frequency with which the instructions were given. The treatment groups are described in Table 1. In the habit treatment group, every household received instructions to shift power use in the *same direction on all days.* Households in this treatment were randomly allocated to one of two sub-groups: one sub-group always received incentives to move power *away* from the time slot (away sub-group), while the other group always received incentives to move power *into* the time slot (into sub-group). Households in the habit treatment received one SMS per week reminding them of their incentive. In the no habit treatment, each household was also given incentives on *all* days to shift power use, but we *randomly varied* these between *into* and *away* from instructions. All households in the no

[7] SE's database of customers who give SE permission to contact them contains 40,490 of SE's more than 247,010 customers in Southern Denmark. Of the 2,625 invited households, 1,175 received the invitation by e-mail, while 1,345 received the invitation by letter.
[8] In total, 131 households signed up, but of these, 38 were allocated to treatments not reported in this paper.
[9] Including them in the analysis with the 18 days and 20 days out of 92 days observation periods where remote measurements reported power consumption has a negligible effect on the results. We chose to remove from the analysis because meter errors that result in report fallout of this magnitude might also corrupt data when meters are reporting. We have no reason to believe that metering errors are affected by treatment allocation.

habit treatment received one SMS per day informing them of which direction they should shift power use that day. As in the habit group, the allocation of incentives was evenly distributed - all households in the no habit treatment were asked to shift *away* on about 50% of the treatment days and *into* on the other about 50% of the treatment days.

Table 1: *Description of treatment groups.*

Treatment	No. of Treatment periods per week	No. of SMS per week
Habit (50% of households always received into incentive and the other 50% of households always received away from incentive)	7	1
No habit (all households received into incentive 50% of the days and away incentives the rest of the days)	7	7

All households served by SE have advanced meters with automated remote registration of their hourly electricity consumption. This made it possible for us to unobtrusively measure power consumption during the designated 3-hour treatment period for all participating households.

By comparing the two treatments, we are able to quantify the economic value of being able to adjust habits to the new incentives, as discussed in section II. In the habit treatment, each consumer knows from the start of the experiment how they should shift the timing of their power use and that they will always be required to shift power in this way. This makes it possible for them to adjust their behavioral habits at the beginning of the experiment in the sense that it is possible to plan ahead and to reduce active deliberation in connection with each instance of shifting. The consumer knows in advance precisely what is required and can form a habit to meet this requirement. In contrast, in the no habit treatment , it is impossible for the individual consumer to predict how power must be shifted and, therefore, it is impossible to plan ahead. This makes it difficult to adjust habits making it necessary to invest more active deliberation in connection with each instance of shifting since the consumer does not know in advance precisely what will be required. Assuming successful randomization in the allocation of households between groups, the aggregate distribution over possible ways of shifting power using tasks and their costs is the same in the two treatments. Hence, the underlying compliance costs of shifting power use are identical. The difference between the two treatments is that one allows consumers to adjust their habits, while the other does not[10].

Incentives are varied across households, but are randomized across treatments to ensure equal incentives in all treatments. Some households received a rebate (0.50 kr., 1.00 kr. or 1.50 kr.[11]) per kWh power moved in accordance with the text messages. Other households also received an environmental incentive, where SE promised to increase wind power production by 0.50 kWh, 1.00 kWh or 1.50 kWh per kWh of power moved in accordance with the text messages.

To check whether randomization worked, we present the summary statistics on monetary and environmental incentives across the habit and no habit treatment groups (Table 2).

Table 2: *Summary statistics on monetary and environmental incentives*

Treatment	No. of house-holds	Average rebate awarded on treatment days (in Kr. per kWh moved)	Average environmental Incentive on treatment days (in kWh per kWh moved)
Habit	47	1.02	0.35
No Habit	44	1.07	0.77

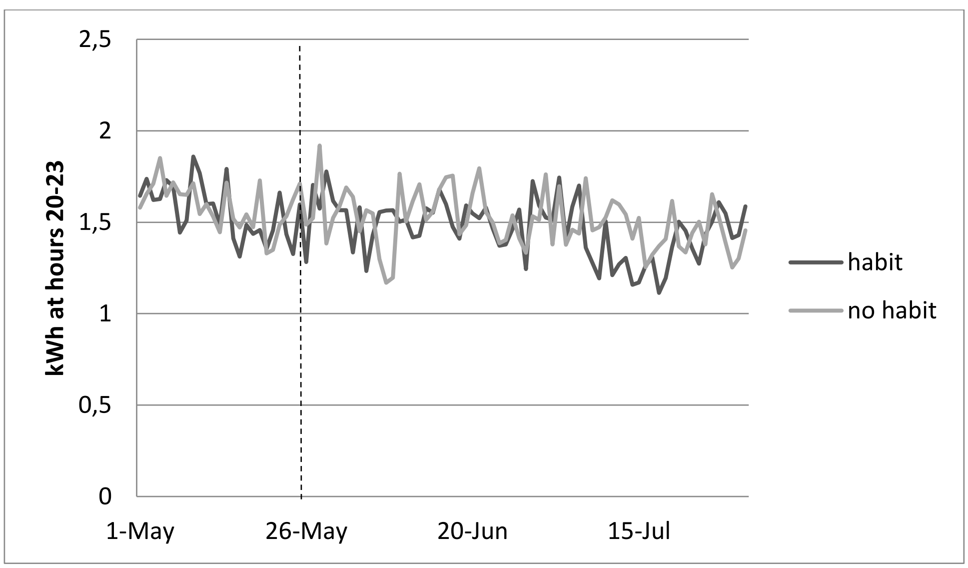
To determine the economic value of habits, we compare the treatments. As shown in Table 2, the monetary incentives are slightly higher in the no habit treatment. Also, the environmental

[10] More precisely, our habit treatment gives consumers greater scope to adjust habits than in the no habit treatment. Consumers in the no habitual treatment can still predict when they will be asked to move power and can draw on experience about how to do this in the two possible directions they know they will be asked to move power consumption and. Therefore, there may be some degree of habit adaptation.
[11] 1 DKK is approximately equivalent to 0.15 USD (Average exchange rate during June 2017).

incentives are substantially higher in this treatment. From Table 1, we see that households in the no habit treatment are reminded about their incentive every day, while households in the habit treatment are only reminded once a week. These differences in incentives and reminder intensity are important when interpreting the results that indicate the economic value of habits in our experiment.

Before examining the estimation results, we present graphs that show the power consumption data measured before and during the experiment. Figure 2 presents daily observations of mean consumption for each of the two treatments during the relevant time slot before and during the experiment. If the effect of *into* and *away* from incentives within each treatment net out the plotted mean power demand for each treatment group, this indicates seasonal variation over the data period. We would therefore like these to follow each other closely both before and during the experiment.

Figure 2: *Daily mean power consumption (during the hours 20:00 – 23:00) habit and no habit treatments*

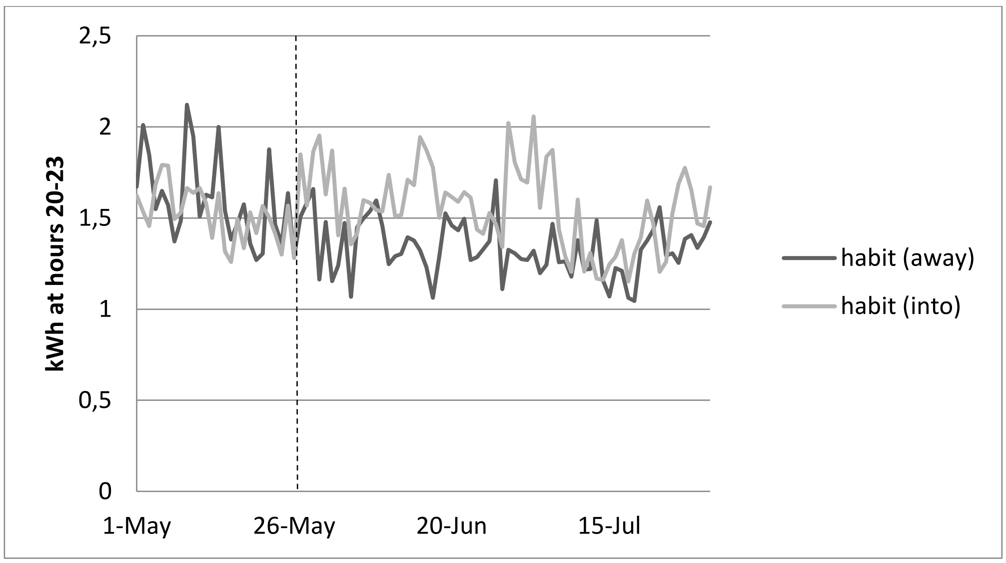


1) The dashed vertical line on 26th May 2014 indicates the start of the treatment.

We see that the mean consumptions in the two treatments do match fairly well both before and after the start of the experiment. This suggests that randomization has worked well and that seasonal variation in total power consumption before and during the experiment at the relevant time slot is similar in the two treatments.

In Figure 3, we present the same mean consumption for each of the two sub-treatments in the habit treatment. If randomization of the households to the *into* and *away from* sub groups within the habit treatment is successful, these curves should follow each other closely before the start of the experiment, then grow apart as the households receiving *into* incentive increase consumption and the households receiving *away* incentives decreases consumption during the treatment hours.

Figure 3: *Daily mean power consumption (during the hours 20:00 – 23:00) of the two habit treatments*

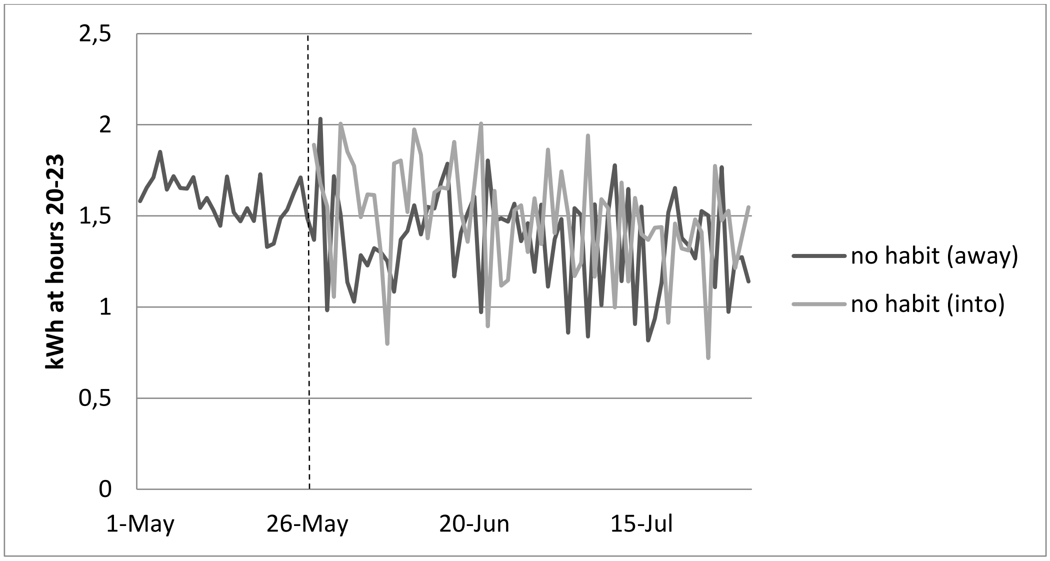


1) The dashed vertical line on 26th May 2014 indicates the start of the treatment.

We see that mean consumptions in the two habit sub-groups do follow each other reasonably well during the pre-experiment period. Mean consumption then deviates in the expected direction after the start of the experiment.

In Figure 4, we present the same mean consumption for each of the two sub-treatments in the no habit treatment. Pre-experiment consumption is represented here by the single curve, the mean consumption of all households. After the start of the experiment, the away curve shows the mean consumption of households that, on a certain day, receive an incentive to move power *away* from the time slot. The habit (into) curve describes the mean consumption of households that, on the same day, received an incentive to move power *into* the time slot. We expect these to deviate during the treatment period as the households receiving *into* incentive increase consumption and the other households receiving the away incentive decreases consumption during the treatment hours.

Figure 4: *Daily mean power consumption (during the hours 20:00 – 23:00) of the two no habit treatments*



1) The dashed vertical line on 26th May 2014 indicates the start of the treatment.

We see some indication that the two habit sub-treatments deviate in the expected direction though much less so than for the habit treatment[12].

To further check the randomization,

Table 3 presents various summary statistics on power consumption before the treatment and dwelling type for the two treatment groups.

[12] We also see a substantial increase in consumption variance at the start of the experiment. This is just an artefact of a taking means of a pre-experiment sample over twice as large as that in the two sub-treatments, post-experiment.

Table 3: *Summary statistics on demographics and power consumption for treatment groups.*

Treatment group no. & type	No. of house-holds	Total power consumption per household[1] (in kWh)	Average daily power consumption: hours 20:00 – 23:00[1] (in kWh)	Average birth year[2]	Type of dwelling Share of house
Habit	47	250	1.551	1959	96%
No Habit	44	256	1.587	1957	94%

1) Before treatment, during the period 1st May 2014 to 26th May 2014.
2) Birth year of the person in the household who signed the household up to the MovePower program.

The resulting differences after randomization are small (and statistically insignificant), which suggests that the randomization procedure has been successful. When estimating the treatment effects below, we control for remaining differences in before treatment power consumption and variation in power use over time by including household and time-specific fixed effects.

IV. Estimation and results

Table **4** presents the average power consumption in the time slot of treatment for the 2-months treatment period and for the 1-month just prior to households receiving the invitation to join the Movepower program[13]. For the no habit treatment group and the two habit treatment sub-groups, the average power consumption during the treatment time slot is calculated for the pre-treatment period (3rd column). For the treatment period, the average consumption during the treatment time slot is calculated for days and households receiving *into* or *away* incentives separately (the 5th column). Finally, in the last column, we present a raw calculation of the diff-in-diff treatment effect (the difference between the into and away differences each of which are calculated as the difference between pre and during treatment power consumption).

Table 4: *Summary statistics of power consumption for the treatment groups*

		Before treatment consumption between hours 20:00 – 23:00 (all days in kWh)	During treatment consumption between hours 20:00 – 23:00 (treatment days in kWh)		Raw Difference in Difference[1]
Treatment	No. of house-holds	Average power consumption	Treat-ment direction	Average power consumption	
Habit (into sub-group)	27	1.517	*Into*	1.557	0.302
			Away	No obs	
Habit (away sub-group)	20	1.596	*Into*	No obs	
			Away	1.334	
No Habit	44	1.500	*Into*	1.595	0.115
			Away	1.480	

1) The difference in difference is equal to: (mean $kWh_{\textit{into},\text{during treatment}}$ – mean $kWh_{\text{before treatment}}$) - (mean $kWh_{\textit{away},\text{during treatment}}$ – mean $kWh_{\text{before treatment}}$).

In

Table **4**, we see that power consumption before and during the treatment is similar, indicating that seasonal variation has little influence. For each treatment group, the average consumption during *into* treatment is higher than for that during the *away* treatment, which was expected. Finally, the

[13] The period before treatment runs from 1st May 2014 to 26th May 2014. The treatment period is 27th May 2014 to 31st July 2014.

row presenting the difference in difference treatment effect for the habit treatment is 0.302 kWh, while the corresponding treatment effect for the no habit treatment is 0.115 kWh, indicating a substantially larger treatment effect for the habit treatment. These treatment effects are, however, not corrected for household-specific effects or seasonal variation, but we take these into account in the following estimation.

To estimate the treatment effects, the data is organized as a panel of 91 households with daily observations of power use before and during the treatment period. The dependent variable in each observation is power consumption in kWh during the 20:00 – 23:00 timeslot, while the explanatory variables are a fixed effect for date, a fixed effect for household and a dummy indicating whether there was a treatment (*away* or *into*) on that day and a dummy indicating whether the treatment on that day was *into*. The following specification was estimated for each of the treatment groups in order to identify the treatment effect:

$$y_{id} = \delta_i + \lambda_d + \beta^0 * TREAT_{id} + \beta^1 * INTO_{id} + \varepsilon_{id} \tag{3}$$

Where

y_{id} is the power consumption during the hours 20:00 – 23:00 of i*th* household on day d,

δ_i is a fixed effect for each household,

λ_d is a fixed effect for each day,

$TREAT_{id}$ is a dummy for days the i*th* household is given either *into* or *away* treatment,

$INTO_{id}$ is a dummy for days the i*th* household is given *into* treatment (the treatment effect),

β^0 is the estimated parameter to *TREAT* for each treatment,

β^1 is the estimated parameter to INTO for each treatment,

ε_{id} is a stochastic error term.

We include household-specific fixed effects in our estimation to control for sampling variation across treatments and date-specific fixed effects to control for weekly and seasonal variation. The parameter β^1 indicates the difference in power use on days with into and away incentives - this is the treatment effect we want to estimate. The size of this treatment effect for the habit and no habit treatments corresponds to $i_1^P - i_0^P$ and $i_1^T - i_0^P$ respectively in equation (2).

The estimation is conducted using the OLS procedure in STATA, using robust standard errors. It is likely that the error terms are heteroscedastic across households, which is corrected for by the robust standard errors (Cameron and Trivedi 2010).

Table 5: *Estimation results*

	Treatments	
	Habit	No Habit
INTO treatment days, β^1 2)	0.599***	0.145***
	(0.135)	(0.036)
ALL treatment days, β^0	-0.057	-0.236
	(0.164)	(0.218)
Fixed effect day, λ_d	Yes	Yes
Fixed effect households, δ_i	Yes	Yes
Observations	4321	4045
No. of households	47	44
R-squared	0.342	0.375

Standard errors are reported in parentheses.

*** indicates that the parameter is significant at 1% level.

2) Two tailed F tests were performed on the difference between estimated treatment coefficients (Pr(diff=0)=0.0013); robust standard errors were used.

As presented in Table 5, both estimated treatment effects (β^1) are significant at the 1% level. In addition, the habit treatment effect is significantly greater than the no habit treatment effect at the 1% level (two sided test). Recall, however, (Table 2) that monetary incentives are slightly greater, the environmental incentives substantially greater and the reminder frequency greater for the no habit treatment, which implies that the estimated difference may be downwardly biased. A bias in this direction implies that we may be underestimating the relative value of habits to some extent.

Assuming that the difference estimate is unbiased, we may use (2) to calculate the:

$$\text{Relative economic value of habits} = 1 - \frac{\boldsymbol{i}_1^{T} - \boldsymbol{i}_0^{P}}{\boldsymbol{i}_1^{P} - \boldsymbol{i}_0^{P}} = 1 - \frac{0.145}{0.599} = 0.76$$

This implies that making it impossible to adjust (some of) the habits relevant for performing the activities for which we induce a shift in timing, reduces the consumer surplus of this shift by 76%. This is a substantial reduction, which implies that habits in our setting are of substantial economic importance.

This reduction is an underestimate for the two reasons noted above. First of all, the estimate itself may be biased downward because of the greater incentives and reminder frequency in the no habit treatment. The second reason we may underestimate is that our experiment has only impaired the adjustment of some of the habits relevant for changing the timing of power consuming activities. This bias strengthens our conclusion.

One potential cause for concern is that households in addition to re-optimizing their stock of habits also could change their stock of physical capital relevant for the repeated activities for which we induce a timing change. Households in our treatments might for example have a greater incentive to invest in appliances with a pre-programming functionality and if this incentive differs between the two treatments so could the change in physical capital stock. Since our intervention only lasted for 2 months and consumers were informed that that the incentive was temporary, it is unlikely that such physical capital stock changes could have had a notable effect. However, we cannot rule it out and therefore, this potentially qualifies our results.

Another potential concern is that the treatments may differ in other ways than the extent to which habit adjustment is impaired. If the randomization of households into treatments has worked as we argue above, the treatments will not differ with respect to underlying costs or the activities for which a timing shift is induced. Nevertheless, such a difference could be induced by a difference between the treatments themselves if these generate a difference in the (dis)utility of performing the activities in question. If, for example, it is more 'fun' to receive incentives that vary randomly than getting the same incentive each day, this could reduce the cost of reacting in the no habit treatment compared to the habit treatment. If this utility difference is the result of preferences for variation in performing the incentivized repeated activities, then clearly such a utility loss is associated with the adjustment of habits, which means it should be included in our estimate as it has been. However, if the utility difference is not associated with preferences for performing the activities in question, but is generated by the specific information channel we use (SMS text messages), then it should not be included when estimating the utility value of habit adjustment. In this case, the utility difference would, to the extent it affects behavior, be an artifact of the experimental intervention we undertake – not a result of habit adjustment.

If there is an intervention-induced utility difference in the indicated direction, this will further bias our estimate downwards. However, one could also imagine the reverse utility difference resulting in an upward bias of our estimate. We cannot rule out the possibility of our experiment resulting in an intervention-induced utility difference between the two treatments. Therefore, this also potentially qualifies our results.

If we accept that our estimate is a possibly downward biased estimate of the specified relative economic value of habits, it is important to stress that our estimate is not corrected for the costs that households in the habit treatment had when they adjusted their habits to the new tariff structure at the start of the experiment. Our estimate indicates the economic value of being allowed to adjust habits if adjustment costs are negligible compared to the gains derived from adjusting. Therefore, we may think of this as an estimate of the gains derived from habit adjustment to a seldom change in otherwise stable conditions surrounding the relevant activities. The value of being allowed to adjust habits when conditions are volatile may be much smaller or zero.

V. Conclusion

In this paper, we use a randomized field experiment to quantify the economic value of being able to adjust habits to changed conditions. In our experiment, we find that impeding adjustment to the new situation of (some of) the relevant habits, reduces the estimated consumer surplus by 76% - when the investment cost of adjusting habits is disregarded.

This implies a substantial economic value of habits connected to the range of household production activities we investigate and suggests that habits may generally be (very) important for economic outcomes from repeated activities. The implication is that stability and predictability of the environments in which such activities are undertaken is important for welfare. This does not necessarily imply that it is costly to periodically change such environments. Critical for this are the investment costs associated with habit change about which we say nothing. But it does imply that short run stability and predictability is important. Stability and predictability allows habits that match the environments in which activities are undertaken to be formed and to guide behavior. Thus taking this into account may be (very) important when attempting to understand behavior and ultimately designing policies, products, work environments etc.

Investigating the economic value of habits in other settings and estimating the economic costs of adjusting habits would seem productive avenues for future research.

VII. References

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Appendix 1

The economic value of habits

As in figure 1, let *h* indicate the scale for which the household's habits with respect to this activity are optimized. Let *C(i,h)* be the short run marginal cost of performing the activity at scale *i* when habits are optimized for *h*, and let *C(i,i)* be the marginal cost curve that applies for permanent increases in scale when habits are adjusted to the new scale level (*h*=*i*). Let i_0^P be the initial long run equilibrium where the initial benefit curve $B_0(i)$ intersects the long run cost curve *C(i,i).* Let $B_1(i)$ be the new benefit curve and $i_1^T$and i_1^P be the new short and long run equilibria corresponding to intersection of the new benefit curves with $C(i,\ i_0^P)$ and *C(i,i)* respectively.

The utility gain from adjusting intensity from i_0^P to i_1^T without adjusting habits is:

$$U^T = \int_{i=i_0^P}^{i_1^T} (B_1(i) - C(i, i_0^P))di \qquad \text{(A.1)}$$

which is the blue area in Figure 1. The utility gain from adjusting intensity from i_0^P to i_1^P when habits are adjusted to the new scale is:

$$U^P = \int_{i=i_0^P}^{i_1^P} (B_1(i) - C(i,i))di \qquad \text{(A.2)}$$

which is the blue and gray area in Figure 1. Thus, the proportion of the total utility gain from fully adjusting to a change in benefits that would be lost if the household was not able to adjust its habits to the new conditions is:

$$\text{Relative economic value of habits} = (U^P - U^T)/U^P \qquad \text{(A.3)}$$

We call this the relative economic value habits because it indicates the relative importance of being able to adjust habits in this particular setting for utility.

In order to identify the relative economic value of habits, we undertake a field experiment where we induce a linear shift in the benefit function by giving a monetary payment in proportion to the household's increase in intensity of *b* (rebate in DKK per kWh shifted).

The first order condition for optimal household scale of the activity now becomes:

$$b = C^*(i,i) \tag{A.4}$$

This implicitly defines that the optimal intensity with habit adjustment as a function of the benefit increase, b, i.e.:

$$i = f(b) \tag{A.5}$$

where $f(b) = C^{*-1}(b)$. Thus, the increase in utility that results from a benefit increase of *b* with habit adjustment can also be expressed as:

$$U^P = \int_{p=0}^{b} (f(p) - i_0^P)dp \tag{A.6}$$

In the same way, we define $g(b) = C^{*-1}(b, i_0^P)$, whereby the increase in utility that results from a benefit increase of *b* without habit adjustment can be expressed as:

$$U^T = \int_{p=0}^{b} (g(p, i_0^P) - i_0^P)dp \tag{A.7}$$

If we assume that habit adjustment implies a proportional increase in the intensity reaction to any given benefit *b* so that::

$$g(b) - i_0^P = \alpha(f(b) - i_0^P) \tag{A.8}$$

we can identify the relative economic value of habits. Inserting (A.8) in (A.7) and then (A.7) and (A.6) in (A.3) we have that:

$$\text{Relative economic value of habits} = 1 - \alpha \tag{A.9}$$

and by (8) that α can be identified empirically as:

$$\alpha = \frac{\boldsymbol{i}_1^{T} - \boldsymbol{i}_0^{P}}{\boldsymbol{i}_1^{P} - \boldsymbol{i}_0^{P}} \tag{A.10}$$

This is the ratio of treatment effects in our experimental treatments with and without habit adjustment.

Appendix 2: Invitation letter

Three versions of the invitation versions were used. One promised participation in a lottery for an iPad, the second asked for help with the transmission to green energy in Denmark, while the third did both. Invitations were randomized across invited customers and the customers who were invited by each invitation were randomized over treatments after recruitment. The invitation letter (combined version) is presented below.

Win an
iPad

Read the newsletter online

Become a test pilot - win an iPad* and help us with a transition to more green energy in Denmark

Dear Customer

We need your help. Become a test pilot and help us find new ways to accelerate the transition to green energy and at the same time take part in a draw to win an iPad to the value of 3,699kr*. As a test pilot, during the test period, you will receive text messages that tell you when it is best to use power and what it will mean if you choose to move your consumption. For example, you can change the time for when you wash clothes / turn on the dishwasher, etc. Of course, it doesn't mean that you have to cook roast pork at 3am. But moving the timing of your power consumption just a little has many advantages. We call the trial MovePower.

Among SE's customers, there is widespread desire to promote green energy, which is something we would like to satisfy. In Denmark, promoting a green transition through wind energy seems the obvious thing to do, but wind power is difficult to use because there are large fluctuations in production during the day. Therefore, it is important to get private households to play a role so we can exploit wind energy better. Initially, only a limited number of customers will be asked to take part in the pilot test. You have been selected as representative of a number of our customers. It is important for us to gather as many different customers' experiences as possible. Therefore, your participation means a lot to us.

What is MovePower?

- During the test period, you will receive text messages that tell you at what time of day it is best to use power.
- It is completely up to you whether you decide to change the timing of your electricity consumption based on the information you receive or not.
- MovePower will not affect your current electricity contract.
- The extra service in MovePower is free.
- MovePower also includes an additional offer of remote control. This is free, and you can decide for yourself whether it's something for you.
- Initially, it will be in the form of a pilot test, which last 1 year and only involves selected Se customers.

If you would like to participate in MovePower, you can sign up on the following website: www.se.dk/testpilot

Use Code: TWELVE DIGIT CODE1

If you don't want to participate, let us know by sending a message on the following homepage: www.se.dk/besked

Use code: TWELVE DIGIT CODE2

Kind regards

SE

P.S. If you have any questions about MovePower, you are welcome to contact us on the following number: 7011 5095

*The winner will be drawn at the start of May 2014 and will be contacted directly. The prize consists of an iPad Air 16GB and WiFi to the value of 3,699kr, which can be exchanged for cash. Employees of SE are not allowed to participate-

SE | E-mail: se@se.dk | Edison Park 1 | DK-6715 Esbjerg N | Telefon +45 7011 5000 | Fax 7011 5001

Appendix 3: Terms of MovePower

Incentives were varied across households, but are randomized across treatments to ensure equal incentives in all treatments. Some households (group 1) received a rebate (0.50 kr., 1.00 kr. or 1.50 kr.[14]) per kWh power moved in accordance with the text messages. Other households (group 2) in addition to the monetary incentive received an environmental incentive where SE promised to increase wind power generation by 0.50 kWh, 1.00 kWh or 1.50 kWh per kWh power moved in accordance with the text messages. Below are the terms of MovePower for the 0.50 kr. and 0.50 kWh incentives.

Here are the terms of MovePower:

- As a pilot test, during the test period, you will receive text messages that tell you at what time of day it is best to use power.
- (group 1) If you choose to follow the recommendations you receive in the text messages and move the timing of your electricity consumption, you will earn 0.50 kr in discount for each kWh you move. In this way, you will save money.
- (group 2) If you choose to follow the recommendations you receive in the text messages and move the timing of some of your electricity consumption, you will earn 0.50 kr in discount for each kWh you move SE will move 0.5 kWh of conventional electricity production to wind-based electricity production for each kWh you move. At the same time, SE will move 0.5 kWh of conventional electricity production to wind-based electricity production for each kWh you move. In this way, you will save money and help to reduce the environmental impact.
- If you choose not to react to the information you receive, nothing will happen. Whether you decide to make use of the information and move the timing of your consumption is entirely up to you.
- To give you an overview, once a month you will receive a text message telling you how many kWh of your electricity consumption you have moved - as suggested in the text messages.
- We calculate how many kWh you have moved through a comparison with your average power consumption from the previous year.
- (group 1) If you earn a discount, you will receive the money when MovePower ends in one year.

[14] 1 DKK is approximately equivalent to 0.15 USD.

- (group 2) If you earn a discount and a reduction in environmental impact, you will receive the money and SE will increase the wind turbine capacity when MovePower ends in one year.

- MovePower is a limited pilot project and you will receive notification when the project starts and ends.

If you would like to know more about how you can move the timing of your energy consumption, go to: www.se.dk/FlytStroem

Appendix 4: Information given to subjects about how to move power

MovePower

In Denmark, promoting a green transition through wind energy seems the obvious thing to do, but wind power is difficult to use because there are large fluctuations in production during the day. Therefore, it is important to get private households to play a role so we can exploit wind energy better. This is what we are testing with MovePower, which encourages you to move the timing of your consumption to when wind energy is available.

When should I move the timing of electricity consumption?

As part of MovePower, during the test period, you will receive text messages that tell you at what time of day it is best to use power and what it will mean if you choose to move the timing of your consumption.

The information in the text messages will sometimes be sent at short notice, but at other times it will be sent several hours in advance, so how much time you have before you need to move the timing of your electricity consumption will vary.

How to move the timing of your electricity consumption

To determine how to move the timing of your electricity consumption, it's a good idea to think about what would be easiest for you. For example, moving the start time of the:

- dishwasher,
- washing machine,
- tumble dryer,

or you could use the automatic timing for, e.g. the dishwasher. You can also postpone or speed up the charging of electronic appliances, e.g. PC/iPads/mobile phones, etc. We hope this has inspired you to move the timing of your electricity consumption.