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Abstract: Experimental evidence of the effect of providing cheap energy saving technology to households is sparse. We present results from a field experiment in which autopoweroff plugs are provided free of charge to randomly selected households. We use propensity score matching to find treatment effects on metered electricity consumption for different types of households. We find effects for single men and couples without children, while we find no effect for single women and households with children. We suggest that this could be because of differences in saving potential (e.g. some households do not have appliances where using a plug is relevant), differences in the skills relevant for installing the technology and differences in the willingness to spend time and effort on installation. We conclude that targeting interventions at more responsive households, and tailoring interventions to target groups, can increase efficiency of programmes.

JEL classifications: C21, D12, Q41
Keywords: autopoweroff plugs, treatment effect, energy consumption, types of households

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1. Introduction

Saving energy is on the political agenda, whilst energy saving programs, aimed at reducing household energy consumption by providing 'cheap' energy saving technology, are becoming popular. From the literature it is clear that private power consumers differ substantially in how they use electricity and in their ability and willingness to undertake power saving investments and change behaviour. Thus, it seems likely that the efficiency of energy saving programs can be increased substantially if they are designed for, and targeted at, the most responsive power consumers. In this paper, we provide experimental evidence of such programme treatment effects that may be used directly as a foundation for the targeting of similar programmes. We also suggest explanations for the heterogeneity in consumer responses that we observe which could be helpful when designing such targeted programmes.

There is a substantial literature which studies energy saving behaviour in households whilst numerous empirical papers have considered financially demanding long term investments in energy reducing technologies (such as energy saving washing machines, dishwashers etc). These studies consistently find that the willingness to undertake investments depends on income, age, household size and the ownership status of the dwelling, while no significant differences because of gender are found in the studies that have investigated this aspect (see e.g. Barr *et al.* 2005, Long 1983, Mill and Schleich 2010b, Sardianou 2003, Young 2008, and e.g. Abrahamse 2005 for a good survey). These results are also consistent with financial constraints and time horizons implied by investment decisions.

More relevant for our context, however, is the much smaller literature that investigates consumers' willingness to change habits and undertake energy savings, such as using autopoweroff plugs, or choosing energy saving compact florescent light bulbs instead of traditional bulbs (Mills and Schleich (2010a), Di Maria *et al.* (2008), Sardianou (2007) Carlsson-Kanyama (2007), Poortinga *et* *al*. (2003) and Scott (1997)). Such "small investments" typically involve purchasing and installing the 'gadget' and a change of habits requiring time and effort, whereas the financial cost of the investment itself is often small. It is this kind of 'change in habits' that the above mentioned energy saving programmes are designed to induce. Results from this literature are much more inconclusive. Some studies find that willingness to change habits depends on income, age, education, and household size, but most studies have not found these effects. The only consistent result seems to be that the ownership status of the dwellings has no effect.

Finally, a third strand of literature investigates the effects of providing information feedback to households about their own power consumption. Using a field experimental design, Sexton (1989) and Matsukawa (2004) estimate the effect of installing continuous-display power-use and cost monitors in households on power consumption. Matsukawa (2004) found that the installation of power monitors reduced power consumption significantly. Sexton (1989) found that the provision of current cost information in a similar setup in which households were subject to time differentiated tariffs, caused a significant consumption shift from the peak period to the off-peak period. Also using a field experimental design, Gleerup *et al.* (2010) found that the provision of power consumption feedback information through SMS-text messages and email reduced energy consumption by 3 %.

Feedback information on consumption has been studied experimentally and found to be effective, presumably because such information induces habit changes. However, the underlying habit changes are not identified in these studies and they probably differ substantially between households so that they provide little insight into the causes and effects of specific habit changes. On the other hand, the prior studies that do investigate specific habit changes e.g. attitude changes, changes in the use of autopoweroff plugs, or energy saving compact florescent light bulbs do not estimate the resulting power savings. This is our point of departure. In this paper, we present estimates of power savings, which result from habit change induced by the provision of autopoweroff plugs. To do this we used a field experimental design in which randomly selected households are given an autopoweroff plug and relevant information about how to install and use it free of charge.

Autopoweroff plugs collect jointly used equipment (such as a PC, printer, and modem) in one power switch making it much easier for consumers to switch electrical equipment on and off. The hope is that this will induce a habit change that will reduce the amount of power that is wasted when idle equipment is left on standby mode. The experiment was conducted in Denmark in 2008 with 748 participating households of which 321 were treated. Treatment consisted of the free provision of autopoweroff plugs, together with information on installation and use and the expected power saving effect. As the treatment effect we estimate the resulting reduction in metered power consumption. The power consumption of households in the treatment and control group is measured in both a 2 month pre-treatment and a 2 month post-treatment period. We use propensity score matching in a difference in difference setup to find average treatment effects on metered electricity consumption for four types of households. We find a substantial effect for single men and couples without children (with estimated savings of 5.5% and 5.1% respectively[2]), while we find no effect for single women and households with children. Our results differ from other studies of habit change and energy saving investments, which find no significant differences because of gender. In our experiment, we document significant differences between men and women and between households with and without children. The use of an experimental design with treatment effects on metered power consumption makes us feel confident about the soundness of the results.

In a post experimental survey to treated participants, just over 25 % of the responding households indicated that they had no appliances relevant for installing the plug, while just over 20 % of the

[2] Per cent of treated households estimated baseline power consumption without treatment in the post treatment period.

responding households indicated that they had already purchased and installed a plug prior to the experiment. The survey suggests that there is a substantial unexploited savings potential to be gained from using the autopoweroff plug provided in the experiment in all four evaluated groups, but also that there are substantial differences in this potential between the four groups. We suggest that the main reasons for the substantial differences in treatment effects between groups are differences in saving potential (e.g. differences in the diffusion of appliances for which installation of the plug is relevant), differences in the skills relevant for installing the provided technology, and differences in the willingness to spend time and effort on installation. The explanations we suggest are, 1) that younger households have a higher savings potential than older households; 2) that time is especially tight in families with children so that they do not allocate the required effort, and; 3) that elderly men have greater, technical skills, and interests relevant for auto-plug installation then elderly women. We conclude that taking account of these differences when designing similar programs in the future could increase program efficiency. More generally, we conclude that the evidence based targeting of interventions aimed at inducing energy savings in households can significantly increase the efficiency of such programs.

The paper is organised as follows. The next section reviews the literature and develops a conceptual model of consumer response to such energy savings interventions. The experiment, data and estimation approach are described in sections 3 and 4, whilst the results are presented and interpreted in sections 5 and 6. In the final section, we conclude and discuss perspectives for optimising energy saving interventions and influencing the energy consumption through such programs.

2. Previous literature and the behavioural framework.

Studies of investments in energy savings (such as appliances and house insulation) consistently find that a number of socio-demographic characteristics have effects that fit well with the time horizon and liquidity constraints that such a long term investment problem implies. Such investments are:

- positively affected by income that makes investment financing easier (Long, 1993; Mills & Schleich, 2010; Poortinga *et al*., 2003; Young, 2008; Walsh 1989, Scott, 1997);
- positively affected by homeownership that ensures ownership of the long term investment (Barr *et al.* 2005; Curtis 1984; Hassett and Metcalf 1995; Scott 1997; Mills and Schleich 2010b);
- negatively affected by age so that reducing time horizons makes long term investments less attractive (Carlsson-Kayama and Linden 2007; Sardianou 2007; Walsh 1989; Linden *et al.,* 2006) and;
- positively affected by household size which increases the savings potential of investments (Poortinga *et al.*, 2003; Young, 2008).

Evidence of the effect of education on investments is mixed (Mills and Schleich, 2010b, Scott, 1997, Curtis, 1984, Poortinga *et al.*, 2003, Sardianou, 2008). Studies which have investigated the effect of gender suggest that it is weak or non-existent (Poortinga *et al.* 2003, Sardianou 2008, Carlsson-Kayama and Linden, 2007).

The adoption of energy saving habits differs from energy saving investments in that habit change is driven by the willingness to spend time and effort, whereas investments are driven by financial considerations. Results from the studies of habit changes also differ from the results from studies of investments:

- Dependence on income is weaker. Mills and Schleich (2010a) and Scott (1997) find that income has no significant effect on the adoption of low energy light bulbs. Di Maria *et al.*

(2008) find a positive effect, whilst Poortinga *et al*. (2003) find a negative effect when investigating attitudes.

- Dependence on age is weaker. Di Maria *et al.* (2008) and Poortinga *et al.* (2003) find that age has no significant effect on the adoption of energy saving habits, whereas Mills and Schleich (2010a) find a positive effect.
- No dependence on home ownership is found. Di Maria *et al.* (2008) and Mills and Schleich (2010a) conclude that house ownership makes no impact on the use of low energy light bulbs.
- There is some evidence to suggest that household size has a positive effect (Mills and Schleich, 2010a).

As with energy saving investments, there is mixed evidence of the effects of education on energy saving habits (Mills and Schleich, 2010a, Scott, 1997, Di Maria *et al.,* 2008) and only weak (if any) effects of gender (Sardianou, 2007, Di Maria *et al*., 2008, Carlsson-Kanyama, 2007).

These differences are not surprising since the adoption of energy saving habits is not a classical investment problem. The initiation of habit change is not nearly as financially demanding as investments and so one would expect the effect of income to be weaker. One would also expect the shorter time span of habit changes to reduce the importance of home ownership and the shorter time horizon with age. Given that it is not useful to think of habit change as a classical investment problem, the question is what determines how motivated a household is to change habits?

At the core of the problem faced by households when considering the adoption of energy saving habits is presumably uncertainty about the resulting effects on energy consumption and the investment in time and effort needed to reduce this uncertainty. Following Mills and Schleich (2010a), Di Maria *et al.* (2008) and others, we see this problem as a two step process. Initially, the household is uncertain about the power saving effect of changing habits and considers investigating the option. If the members of the household invest time and effort into investigation, they become (more) certain of the outcome. Based on this information, they can decide whether to adopt the habit change, or not. Letting U denote net utility of adopting the energy saving habit, we define:

$$U = (1+\theta)s - c - kT_H(e) \qquad (1)$$

Where s is the savings potential measured as the monetary value of power saved by the habit change, c is the financial cost associated with the change and $T_H(e)$ is the effort needed to implement the habit change. The effort costs of habit change depend on an efficiency parameter e (with $T'_H < 0$), which characterises the household's skills in relation to implementing the habit change (e.g. a household with a small e finds it difficult to install and use a autopoweroff plug and therefore needs to expend more effort on the task). The parameter k denotes the household's shadow value of effort.[3] If the household has valuable alternative uses of its time and effort (like working or caring for children) k is large. The parameter θ indicates the household's environmental preferences.[4] If $\theta = 0$, they have no preferences for the environment and they value energy savings according to their monetary value, and if $\theta > 0$, they have environmental preferences and value energy savings above their monetary value. After having investigated the problem, the household knows U. Presumably, the probability of adopting the energy saving habit depends positively on this value i.e.:

[3] We use the term 'effort' rather than 'time' to capture the fact that tasks may not only be time consuming as such, but also tiring and physically/intellectually demanding. However, nothing essential is lost by using the less abstract concept 'time' instead.

[4] The consumers are expected to potentially have preferences for reducing the environmental effects of their own consumption. The value of the parameter θ will therefore differ between types of environmental benefits (water savings, gasoline savings, electricity savings, etc.) and over time as the price of the consumption units saved changes. We assume that θ is constant for this particular type of environmental benefit, and for this limited period of time.

$$P(adoption \mid investigation) = F(U) \tag{2}$$

The probability of investigating depends negatively on the effort costs of investigating and positively on the households' prior expected net utility. This prior expectation presumably depends on actual net utility so we define the probability of investigating as a function of these two variables:

$$P(investigation) = G(U, kT_N(e)) \tag{3}$$

Where T_N is the effort required for the investigation (again depending on the household's efficiency parameter *e*). Thus, the unconditional probability that a household decides to adopt an energy saving habit is (expected signs of partial derivatives are given below the equation):

$$P(adoption) = \underset{+}{F(U)}\,G(\underset{+}{U}, \underset{-}{kT_N(e)}) \tag{4}$$

Clearly, households may differ along all the key parameters. For example, if a household has a higher savings potential (*s*) and lower costs (*c*) of adoption, the probability of adopting will increase. Intuitively, higher environmental preferences (θ) also increase the probability of adopting, whereas a higher cost of effort (k), or lower skills (*e*) reduce the probability of adopting. Reconsidering the effects of socio-demographic characteristics such as income and age in this light, it seems clear that when the strong investment driven financing and time horizon effects disappear, what remains are indirect and multifaceted effects of skills, the shadow value of effort and environmental preferences (e.g. the shadow value of effort typically rises with income, but so may environmental preferences and technical skills).

Now consider an intervention I aimed at inducing adoption with the ultimate goal of reducing power consumption (Y_i). Let $\Delta_I P_i(adoption)$ denote the effect of the intervention on household i's probability of adopting the habit change. The effect we want to induce is energy savings by increasing the probability of adoption, as defined in (4). Thus, given a program cost constraint, we want to design and target our intervention so as to maximise the expected aggregate power savings for the population, i.e. we wish to maximise:

$$\Delta_I \overline{Y} = \frac{1}{n}\sum_i \Delta_I Y_i = \frac{1}{n}\sum_i \Delta_I P_i(adoption)s_i \qquad (5)$$

in which i is a household indicator and n is the number of households. To maximise the aggregate effect, one should target the intervention at households where the intervention is expected to produce a large increase in energy savings (i.e. the households with the largest $\Delta_I P_i(adoption)s_i$). For example, we probably do not want to target households for which the effect of habit change on consumption (s_i) is small, nor do we want to target households that already have a large adoption probability, because here the potential probability increase due to intervention is small and many may already have adopted the new habit. Instead, it may be more efficient to target households which have an adoption probability in the medium range (i.e. households which are 'close to' undertaking the promoted type of energy savings, but tend not do so on their own). Finally, given this we want to design the details of the intervention (how the specific intervention affects s, c, $T_H(e)$ and $T_N(e)$) so as to maximise the increases in adoption probability for the targeted households. This can be achieved by, e.g. providing the targeted group with the specific types of resource that they lack in order to undertake the promoted type of energy savings. For example, if effort use dominates the costs of a specific target group, then it may be more efficient to require payment for the provided 'gadget' and instead use the limited resources available to develop information material and provide a hotline service that could increase households' efficiency parameter (*e*). If, on the other hand, the effort needed for installation is small compared to the monetary costs, then subsidising the provided gadget may be more efficient. With respect to questions like these, we can derive little guidance from the previous empirical literature. In this paper, we provide some initial insights into this.

3. The autopoweroff plug experiment and data

In line with other experimental studies such as Sexton (1989), Matsukawa (2004) and Gleerup *et al.* (2010), the idea of the experiment is to undertake an actual intervention in the field and to estimate the resulting average power savings for different subgroups of treated households, as defined in (5). To do this, we have to be able to measure the daily power consumption of all participants in the experiment. The 1183 participants in the experiment were therefore selected from a pool of households who had advanced meters which enabled automated meter reading of households' daily power consumption. All participants were selected from three *specific* neighbourhoods where the dominant main heating source is district and oil heating (the Aarhus suburbs of Højbjerg and Viby with a large proportion of detached houses and the Skanderborg city centre with a large proportion of apartments[5]). We selected the subject pool from specific neighbourhoods (i.e. from a few specific apartment complexes and specific roads with detached houses) so as to reduce subject pool heterogeneity due to climate, dwelling and lifestyle. The cost of this strategy is that the sample of households is not representative of the Danish population, as such. After selection, households from

[5] The Viby neighbourhood, from which participants were selected, consists of detached houses and town houses built between 1949 and 1967. The Skanderborg neighborhood, from which participants were selected, consists of both rented and owner apartments all built between 1964 and 1973. The Højbjerg neighborhood, from which participants were selected, consists of detached houses built between 1976 and 1980.

each specific neighbourhood randomly allocated to between the treatment group (500 participants) and a control group (683 participants). Households in the control group were not contacted, or in any other way informed about their participation prior to the experiment. The research team was simply given access to metered consumption and existing background data registered by the power company for households allocated to the control group. Each household in the treatment group received one autopoweroff plug in the mail, along with written information about installation and the power cost savings which an average household could expect to achieve if the plug was installed and used (see appendix B). Just like for the control group, the research team was given access to metered consumption and existing background data registered by the power company. Because this method of data acquisition does not give selected participants an option to opt out of the experiment, we have no attrition during the experiment and so avoid the self-selection and potential bias issues this can generate. After completion of the experiment, we deleted all the observations, which exhibited measurement problems with the household's power meter, those for which the household membership had changed during the experiment (in some cases, e.g. a son or daughter moved out, whilst in others the whole family moved and another moved in) and households with electric heating as their main heating source.[6] We have no reason to suspect that this (unavoidable) post experimental selection of the data is correlated with the initial random allocation to treatment and control groups. It is highly unlikely that the experience of receiving an autopoweroff plug in the mail influences a family's or family member's decision to move. It is also (for technical reasons) highly unlikely that installation of the plug could somehow influence the functionality of the power meter. After this deletion procedure, 321 participants remained in the treatment group and 427 participants remained in the control group.

[6] As noted above, the households in the specific neighbourhoods generally have oil and district heating as their main heating source. However, when register data was added after the experiment, 9 households were identified through the BBR register as having electrical heating as the main heating source and these observations were deleted.

In addition to the metered power consumption, we obtained data on the age and gender of all household members at the metered addresses, the type of dwelling (detached house, town house, and apartment), ownership (rental, owner), type of heating system in the dwelling, and postal district. Based on the data, the households were divided into four groups on the basis of the type of household: single males, single females, couples without children and other households (with children or more than two adults) (see the summary description of these groups in table 1).

Table 1. Summary description of the four treatment groups

		Single male	**Single female**	**Couples without children**	**Households with children[1)]**
Number of households		139	143	266	200
Number of persons per household		1	1	2	3.7
Age of adults	Younger than 40 years	34 %	17 %	9 %	36 %
	Between 40 and 60 years	36 %	22 %	34 %	63 %
	Older than 60 years	30 %	60%	57 %	1 %
Type of dwelling	Apartment	81 %	55 %	13 %	18 %
	Town house	5 %	20 %	16 %	4 %
	Detached house	14 %	25 %	71 %	78 %
Dwelling ownership	Owner	40 %	48 %	89 %	83 %
Heating system[2)]	District heating	96 %	91 %	86 %	84 %

1) Singles and couples with children (some households have three or more adults, which we assume are children over 18 still living at home).

We see that single males are younger than single females. The same applies when comparing couples with and without children. When comparing couples with singles; couples own properties more often as opposed to renting an apartment.

Information on energy consumption covers the entire period from the 1st of January to the 1st of May in 2008 with the autopoweroff plug planned to arrive in the mail on the 28th of February.[7] Thus, the metered period covers a pre-treatment period from 1st January until 28th February and a

[7] The Danish mail service is very stable with close to 100% of the mail arriving within a few days of the planned arrival.

post treatment period from 29th February until 1st May. Summary statistics on energy consumption during the two periods are presented in table 2.

Table 2. Summary of data

	No. of house-holds	Pre-treatment Period		Post treatment Period		Test of difference in mean consumption of treated and untreated in Pre-treatment Period
		Mean Consmp. kWh/day	Std. Dev.	Mean Consmp. kWh/day	Std. Dev.	
All treated	321	11.331	6.551	10.044	6.094	
All untreated	427	9.175	6.665	8.005	5.585	t=-4.412*
Single male						
treated:	40	7.572	6.835	6.541	5.543	
untreated:	99	4.590	2.963	4.395	2.713	t=-2.660*
Single female						
treated:	51	5.286	2.348	4.745	2.159	
untreated:	92	5.904	6.713	5.145	5.506	t=0.799
Couples without children						
treated:	135	12.187	6.278	10.213		
untreated:	131	10.244	5.116	8.766	4.284	t =-2.762*
Households with children						
Treated	95	14.944	5.246	12.999	5.056	
Untreated	105	15.028	6.003	13.231	4.487	t =0.105

1) t -test of $\mu_x = \mu_y$, with unknown but unequal variance (υ degrees of freedom)[8],* indicates that the hypothesis cannot be rejected at a 5% level.

We see that the average consumption declines in all treatment groups between the pre and post treatment periods. However, consumption also falls in all the control groups which highlights the importance of seasonality for power consumption. Further, in many cases, we note that both the mean and the variance of pre-treatment power consumption differ between treatment and control groups (in some cases substantially). This suggests that despite the randomised allocation process, treatment and control groups may not be directly comparable in many cases. Properly taking these two potential confounders into account is essential when we estimate the treatment effects.

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[8] The degrees of freedom are given by $\frac{(s_x^2/n_x + s_y^2/n_y)}{(s_x^2/n_x)^2/(n_x+1) + (s_y^2/n_y)^2/(n_y+1)}$, in which n_x, n_y are sample sizes and s_x, s_y are standard deviations.

4. Estimation of average treatment effects

We want to estimate the average effect of the intervention (as defined in equation (5)) on the power consumption of the four treatment groups. Ideally therefore, we would like to compare the measured average power consumption after treatment of the treated groups with the average power consumption that the same households would have had under precisely the same circumstances, but without the treatment. Because we do not observe this counterfactual, our problem is to construct a counterfactual for each group using the pre-treatment consumption measure and the corresponding control group for which we also measure power consumption.

A classical approach to this estimation problem is to use the so called difference in difference (DID) estimator (see, e.g. Angrist and Krueger, 1999, and Imbens and Wooldridge (2009) for a recent exposition):

$$\tau_{DID} = (\overline{Y}_{T1} - \overline{Y}_{T0}) - (\overline{Y}_{C1} - \overline{Y}_{C0}) \qquad (6)$$

in which the first parenthesis is the difference in the average power consumption for the *entire* treatment group between the *post* treatment ($\overline{Y}_{T1}$) and the *pre* treatment ($\overline{Y}_{T0}$) periods. The second parenthesis is the corresponding difference for the *entire* control group. Following Imbens and Wooldridge (2009), this can be estimated using ordinary least squares regression with the specification:

$$Y_{i1} - Y_{i0} = \alpha + \tau_{DID} G_i + \varepsilon_i, \qquad (7)$$

in which $i = 1,...,n$ is a household number indicator and G_i is a dummy which indicates that the household received treatment (i.e. $G_i = 1$ for households in the treated group and $G_i = 0$ for controls). The estimator allows for differences between the two groups' distributions over household characteristics, but these are assumed to only cause a difference in the level of power consumption between the two groups (estimated by α). The underlying seasonal change between the pre and post treatment periods is, on the other hand, assumed to be unaffected by the differences between the two groups. If this assumption does not hold, then part of the estimated treatment effects should instead be attributed to differences in the groups' underlying seasonal variation caused by differences in their distribution of characteristics. This identifying assumption is critical in our case. If two households differ substantially in their level of power consumption at a given time, it seems likely that their variations in consumption over the season will also differ. To avoid making this critical assumption, we estimate treatment effects using matching techniques.

Matching of treated and untreated households

Instead of tackling the problem of distributional differences by making functional assumptions about how these differences influence effect measures, matching techniques tackle the problem directly by harmonising the two distributions. The basic idea is to take the original treatment and controls for which distributions differ, and from these select subsamples that have the same distributions over household characteristics.

In order to do this, we have to observe in the data the household characteristics that influence the evaluated effect and which distributions differ between treated and controls. Formally, the assumption needed for the identification of the treatment effect using matching techniques is the so called unconfoundedness assumption (Imbens, 2004). This assumption states that allocation between treatment and control groups is independent of the potential outcome (here power consumption) when conditioning on the set of covariates used for matching. In addition, the distributions of treated and controls must have common support (i.e. overlap). The condition ensures that for any treated household there is a positive probability of finding 'similar' (in terms of observed characteristics used for matching) households in the control group. In order to satisfy this condition, both treated and untreated units may occasionally have to be deleted from the estimation. The matching procedure then 'reweights' the observations to ensure the harmonisation of the two distributions with respect to the chosen covariates. This is done by choosing pairs of similar (matching) households from the treatment and control group respectively for inclusion in the *subsamples*[9] to be compared (see Caliendo and Kopeinig, 2008. For a recent overview of methods and applications see Heckman and Navarro-Lozano 2004, Sianesi, 2004 and Wren and Storey, 2002). Formally, this matching estimator is:

$$\tau_M = (\overline{Y}_{T*1} - \overline{Y}_{T*0}) - (\overline{Y}_{C*1} - \overline{Y}_{C*0}) \quad (8)$$

in which $\overline{Y}_{T*1}, \overline{Y}_{T*0}, \overline{Y}_{C*1}$ and $\overline{Y}_{C*0}$ is the average power consumption for the *subsample* of the treatment and control group selected through the matching process. Thus, the matching estimator is the DID estimator applied to subsamples for which distributions have been harmonised through matching.

We use propensity score matching so that households are matched on the likelihood of being treated (the propensity score) estimated as a function of the observed underlying household characteristics.[10] Our specification of the set of variables, which potentially influence the treatment effects that are included in the propensity score model on which we match, is critical. Here, the four household types for which we estimate the treatment effects reflect key differences in household composition and also, presumably, in the key variables influencing weather and how households react to the treatment. Within these groups, the most important cause of differences in the effect of treatment is presumably the structure of the household's power consumption i.e. whether the

[9] Subsamples are in the sense that some of the original households may have been deleted. Note, however, that other households may be replicated one or more times.
[10] See, e.g. Becker and Ichino (2002) and Rosenbaum and Rubin (1983) for the original contribution, which describes this technique.

savings potential is substantial enough to induce the household to use the provided plug. We do not observe this directly, but pre-treatment power consumption provides us with a nice indicator. In our main estimation model, we only match households on pre-treatment power consumption, which implies that we assume that if the remaining observed characteristics (type of dwelling type, ownership etc.) influence the treatment effect; it is through their influence on the size and variance of power consumption. This does not seem unreasonable, but to check the robustness of the assumption, we investigate the sensitivity of the estimation results to the expansion of the set of components of the propensity score model.

We use the 'radius matching' technique, which implies that pairs can only be matched if propensity scores fall within a predefined neighbourhood of each other. Treatment units, or controls, for which no close matches are found, are excluded from the estimation (Ravallion, 2008). A control group unit can be replicated and used as matches several times. Compared to other matching techniques, radius matching implies that the similarity between the compared treatment and the control groups is increased. The disadvantage is that the number of matched pairs is reduced, which reduces the statistical efficiency and makes the sample, for which treatment effects are evaluated, less representative of the original pool of treated households. We also investigate sensitivity to changes in the radius matching criteria.

5. Results

Table 3 presents the estimated treatment effects for four types of households (single men, single women, couples without children living at home and households with children) and the estimates of the sensitivity to changes in the propensity score components (the three following rows) and the sensitivity to changes in the radius matching criteria (the following four rows). Finally, in the last row, we present estimates for the standard difference in difference estimator (without matching).

Table 3. Estimated Average treatment effects and sensitivity

	Propensity score components	Radius distance criteria	Estimated average treatment effect (t-values in Parentheses)			
			Single males	Single females	Couples without children	Households with children
Main model	Pre period power consumption	0.1	-0.413** (-2.026)	0.021 (0.174)	-0.618** (-2.238)	0.201 (0.795)
Sensitivity to propensity score components[1)]	Pre period power consumption, Post district, size[1)] ownership, type of dwelling[2]	0.1	-0.414** (-2.104)	0.058 (0.474)	-0.518* (-1.850)	0.360 (1.403)
	Pre period power consumption, post district, size[1)] ownership, type of dwelling[2)], age,	0.1	-0.418** (2.143)	0.053 (0.350)	-0.514* (-1.826)	0.626 (2.193)
	Pre period power consumption, post district, size[1)] ownership, type of dwelling[2)], age, type of heating system[3)]	0.1	-0.420** (-2.156)	0.013 (0.097)	-0.498* (-1.778)	0.618 (2.165)
Radius sensitivity	Pre period power consumption	0.01	-0.399** (-1.996)	0.046 (0.348)	-0.508** (-2.653)	0.138 (0.528)
	Pre period power consumption	0.05	-0.458** (-2.175)	0.019 (0.152)	-0.626** (-2.316)	0.198 (0.775)
	Pre period power consumption	0.2	-0.391** (-1.969)	0.034 (0.282)	-0.554** (-2.016)	0.196 (0.775)
	Pre period power consumption	0.3	-0.362* (-1.827)	0.029 (0.237)	-0.535* (-1.932)	0.196 (0.775)
Difference in Difference			-0.795** (-2.61)	0.217 (1.21)	-0.495* (-1.81)	0.315 (1.17)

** indicates that the parameter is statistically significant at a 2.5% level (t>1.960), one-sided test of negative treatment effect against null-hypothesis. * indicates significant at a 5 % level.
1) Only for households with children.
2) Dwelling types: detached house, town house and apartment.
3) District heating, Oil heating

In the first row (main model), as our main result we find a significant average treatment effect for single males and for couples without children. On the other hand, a negative treatment effect for single females and households couples with children is highly insignificant, which suggests that few households in these groups changed their habits, or that the effect on power savings of such habit changes is negligible.

In the following three rows, we present the same matching estimators for expanded subsets of the propensity score components (indicated in column 2). This shows the sensitivity of our result to a relaxation of the assumption that background variables influence the treatment effect through the volume of pre-treatment power consumption. We see that relaxing this assumption only has a minor effect on the results. Estimated treatment effects for single males and for couples without children continue to be significant or close to significant (and of the same magnitude), while the treatment effects for the remaining groups continue to be highly insignificant.

In the following four rows, we present the original matching estimators, but now for varying values of the radius matching criteria. Again, we see that our result is not sensitive to variations in this estimation parameter.

Finally, in the last row, we see that the difference in difference estimators shows the same basic pattern as our matching estimators. However, the estimated effect for single males is almost twice as large when we do not control for differences in the treatment and control group distributions by matching. Thus, if we had not controlled for distributional differences through our matching estimator, the results would have been noticeably biased.

To illustrate the importance of the estimated treatment effects, we present the estimated treatment effects in percent of the expected power consumption without treatment in table 4. In the parenthesis below, we present the confidence interval of the estimate also in percent of the expected power consumption without treatment (the interval is bounded above at zero).

Table 4. Importance of the estimated effects

	Single male	Single female	Couples without children	Households with children
Effect in % of consumption[1]:				
2½ Percentile:	-0.2 %	0.0%	-0.6%	0.0%
ATE[2]:	-5.5 %*	0.0%	-5.1 %*	0.0%
97½ Percentile:	-10.7%	-4.1%	-9.5 %	-2.0%
Annual effects:				
ATE[2]:	-150 kWh *	0 kWh	-225 kWh *	0 kWh

[1] Estimated effects in percent of estimated average post-treatment power consumption of treated households

[2] Average Treatment Effect.

* indicates that the average treatment effect is significant at a 5 % level.

We see that the estimated power reduction in the two groups with significant treatment effects is 150 kWh and 225 kWh respectively, which is over 5 % of power consumption in both cases. This is an effect within the ballpark of what one would expect.[11] For the two groups with insignificant treatment effects, the estimation upper bounds (97.5 percentile) suggest that we can be fairly confident that the savings in households with children are close to zero (and at any rate substantially smaller than for couples without children: the statistical test of this is highly significant p=0.995, two-sample t-test unequal variances). The confidence band for single females is wider, so we cannot rule out that there may, in fact, be a noticeable treatment effect for this group. However, it is significantly smaller than the effect for both single males and couples without children.[12]

6. Discussion

In this section, we discuss reasons why the estimated treatment effects differ between the four

[11] A large Danish engineering study has calculated that the average power saving of installing the plug for a Danish household is 160 kWh (www.elboligmodel.dk), which is within the confidence interval of both estimates.

[12] P(smaller)= 0.9707 and 0.9820 respectively, two-sample t-tests unequal variances.

treatment groups using the behavioural model presented in section two to structure our discussion. One obvious reason for not using a plug received in the mail is that there is a low saving potential either because the household has no installations where the plug is relevant, or because the household already has a plug installed. Very few Danish households do not have appliances with a stand-by function (such as TVs, stereos and PC -equipment). However, one possible explanation for not using a plug that is received through the mail is if a household already has such a plug installed. We sent a post treatment questionnaire to all treated households after the measurement period (see appendix A). The response rate was low (only about 20%) and the households who used or actively considered using the plug are probably over represented, while the households who did not read the instructions or investigate the possible uses of the plug, are probably under-represented. Virtually none of the responding couples with children said that they had no installations where the plug is relevant. This suggests that most families with children have appliances with a stand-by function (such as TVs, stereos and IT-equipment) which seems plausible. For single women and couples without children, 45 % and 34 % respectively said that they have no installations where the plug is relevant (the difference is not significant[13]). These two groups are older than singles males and households with children and it also seems reasonable that a substantial part of these households do not have such installations. However, most households in these groups still have such equipment. Of those responding, between 9 % and 30 % indicated that they had already installed a plug prior to the experiment where the highest percentages were found for single males and couples without children. This suggests that there is a considerable savings potential among respondents to the survey in all four groups. The survey respondents are probably more motivated to save power and have therefore probably with greater probability already installed an autopoweroff plug of their own accord compared to non-respondents. Thus, it seems safe to conclude that there is a sizable

[13] P(difference=0)=0.3563, two-sample t-test unequal variances.

technical potential for power saving from by installing the provided plugs in all four groups, but that this potential may be substantially smaller in the older groups: single women and couples without children.

Presumably the fact that a power plug and information about its power saving effect arrives in the post dramatically reduces the investigation costs for all households (equation 3) who have not previously investigated the option, since they no longer have to seek out information about the power saving effect of the plug, its purchasing cost or where it can be purchased. Assuming that the treatment reduces investigation costs to close to zero, the decision problem that households face is whether to install the plug and adopt the new habit, conditional on having investigated the possibility. This is captured by equation (9), which indicates the probability of adoption conditional on a household having investigated the option:

$$P(adoption \mid investigation) = F((1+\theta)s - c - kT_H(e)) \qquad (9)$$

In which we recall that s is the monetary value of power saved by the habit change, c is the monetary costs associated with the habit change, $T_H(e)$ is the effort cost of the habit, e is the household's implementation ability and skills (with $T'_H < 0$), whilst the parameter θ indicates the household's environmental preferences (if $\theta = 0$ the household has no preferences for the environment and values energy savings according to their monetary value. If $\theta > 0$, the household has environmental preferences and values energy savings above their monetary value).

In addition to reducing the investigation costs to zero, our experimental treatment also reduces the monetary costs of the habit change (c) to zero for all households. Thus, the remaining possible explanations for differences in the average treatment effects between the four groups is differences in the distribution of potential savings (s), environmental preferences (θ), implementation ability (*e*) and alternative costs of effort (*k*) between the four groups.

From table 1 and 2 above, in which the key characteristics of the groups are summarised, we know that single males (with a significant treatment effect) are younger and have a somewhat larger level of power consumption than single females (who do not react to the treatment). Part of the explanation for the different reactions to the treatment is probably that the savings potential is greater for males, because they have more of the relevant installations without already having installed the plug. We cannot rule out that a difference in environmental preferences is also part of the explanation. On the other hand, the fact that younger males are probably more active on the labour market means that their cost of effort is probably greater, which would tend to reduce the probability that they will react.[14] Another possible explanation for this could be differences in technical skills. Carlsson-Kayama and Linden (2007) note that household tasks are not equally distributed between males and females in most households. While females spend more time on household chores, men typically spend more time on maintenance tasks. In Denmark, this may not apply to younger generations where male and female labour market participation is the same and where more equal task sharing in the home is increasingly becoming the norm. However, it may apply to older generations. Understanding the installation instructions and undertaking the actual installation of the autopoweroff plug may be a typical male task, at least amongst older generations, and this may be why households without a male do not react. In our model, older males may, on average, have a larger efficiency parameter (*e*) prior to treatment than older females. This explanation is supported by the fact that couples without children (who like single women are older and have a substantial proportion of questionnaire respondents who say that they have no installations for which the plug is relevant) react to treatment, while single women do not.

Based on this, we would expect couples with children to react to the treatment as they have

[14] For example, Mills and Schleich (2010b) argue that retirees/elderly have more free time and are therefore more willing to spend time on energy reducing habit changes.

substantially larger power consumption and their questionnaire answers suggest that their potential for power savings (*s*) is greater than it is for single males and couples without children, whilst they have the necessary technical skills (*e*), since a male is part of the household. Once again, we cannot rule out the possibility that the reason that couples with children do not react, while couples without do, is because of differences in environmental preferences, although we think this is unlikely. Rather, we suspect that the explanation is a substantial difference in the average alternative costs of effort (*k*) between the two groups. Households without children are substantially older and probably less active on the labour market and so, as suggested by Mills and Schleich (2010b), probably have lower alternative costs of time and effort. This difference is reinforced by the presence of children who are typically given a high priority by working parents when allocating their sparse free time.

To sum up, we suspect that the differences in reactions to the treatment that we observe are due to:

- A difference in the average savings potential between groups mainly due to a difference in household appliances.
- A difference in the average implementation skills between older men and older women and possibly a difference in how our information regarding installation affects these skills.
- A difference in the alternative costs of time due to labour market participation and children.

If the above are true, then in addition to focusing interventions on responsive groups the study indicates how interventions might be adapted to increase effectiveness in responsive groups, as well as in non-responsive groups, if these are to be targeted. For example, if the installation is in fact performed by men in older households with men, then the instructions and information material sent to these households could possibly be made more effective if developed and written with this target group in mind. If, on the other hand, older single women are to be targeted, a very different information approach might be necessary – perhaps a hot line. If the high alternative cost of time does in fact explain why there is no effect on households with children, targeting these families may not be worthwhile unless a very time efficient installation strategy can be devised.

7. Conclusion

The provision of cheap energy saving technology to households could be an effective means to induce energy saving habit changes. In this paper, we present results from a randomised Danish experiment, which measured the actual power savings resulting from habit change induced by providing households with an autopoweroff plug. We find substantial effects for single men (5.5 % of power consumption) and couples without children (5.1 % of power consumption) while we find no, or only a small, effect for single women and households with children. Our experimental design without attrition, the estimation of effects on metered power consumption and our estimation's robustness to changes in the estimation approach, make us feel confident about the soundness of the results.

The significant differences due to gender and between households with and without children are in contrast to prior studies of habit change and energy saving investments. The estimated treatment effects for the responding groups are sizable compared to the cost of the provided plug technology. The differences in effects between groups could be of use to policy makers who are considering how to target similar interventions.

We consider it unlikely that the differences in reactions to treatment can be completely explained by differences in prior installation of plugs across the groups. We speculate that important reasons for these differences include a difference in savings potential between the groups (because the diffusion of appliances for which use of the plug is relevant differs), differences in the allocation of time and effort (because of different shadow costs of time and effort) and differences in technical skills and interest. Specifically, we suggest three possible explanations: 1) that the savings potential (appliances for which it is relevant to use the plug) is smaller for older households, 2) that older women on average possess less of the required technical skills and interest for successful installation than older men and, 3) the time constraint is especially tight for working families with children and so they did not allocate the required effort. In contrast, both single males and childless couples have both the necessary skills and available time for successful installation. We conclude that taking account of such differences when designing similar programs in the future could increase program efficiency. However, though the specific explanations we suggest are plausible and consistent with our post experimental questionnaire and other findings in the literature, our response rate is low. Thus, we cannot support our suggested explanations with strong empirical evidence from our experiment, although they provide a potential starting point for future research. More generally, it seems that evidence based targeting and the design of interventions aimed at inducing energy savings in households can significantly increase the efficiency of such programs.

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APPENDIX

APPENDIX A: Responses to post experimental questionnaire to all treated participants

	Single male	Single female	Couples without children	Households with children
Number of treated households in the experiment	40	51	139	97
Number and share of households answering questionnaire	10 25 %	22 43%	44 32%	26 33%
Share of households installing the plug	70 %	45 %	55%	81 %
Reasons for not installing the plug:				
- Do not have the time, share	0%	5%	9%	13%
- Not interested, share	0%	0%	0%	0%
- It is too difficult, share	10%	5%	2%	0%
- No appliances for which it is relevant, share	20%	45 %	34%	6%
Share of households which already have a plug installed	30 %	9%	25 %	9%
Share of households which have previously considered buying an autopoweroff plug	40%	36%	59%	75%

APPENDIX B: Information on the autopoweroff sent to households.

Standby Saver track

Your shortcut to save energy that would otherwise be used on standby without changing habits

Standby energy uses approximately 10% electricity of the household. TV sets and computers uses for approximately 1000 DKK a year on Standby energy in the household. With Standby Saver track you can remove Standby energy on all types of devices and appliances.

How does the Standby Saver Work?

You have to choose the most used electrical device for instance The Television. Place the plug in the "Master" socket and other devices such as DVD and/or satellite receivers etc. in to the 'accessories' socket. When you turn off the TV - or put it on standby – the other connected devices will automatically switch off. The opposite happens when you turn on the TV again.

Any electrical device can be used as the 'master'. When you turn off this device the power supply automatically turn off to the other four sockets. Video or other devices that should not be turned off should be plugged in to the 'permanent' socket.

How much can you save?

The savings depends on how much that is connected to the Standby Saver track also model and age of the appliances and how many hours you use it daily. On average there is a saving on 250, - DKK per year per Standby Saver track Check the power consumption of your appliances with a SparOmeter (power measurer) that you can borrow at the library. It reveals both in watts and DKK, how much standby measures.

On www.lokalenergi.dk you can find other tips on how to minimize your home energy consumption. If you have any questions please call your Local Energy's Consultants in Energy Advice on phone number: xx xx xx xx.