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Understanding the financial incentive conundrum: A meta-analysis of the effectiveness of financial incentive interventions in promoting energy conservation behavior

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ABSTRACT

Household energy conservation plays an important role in the mitigation of greenhouse gas emissions and the transition towards a sustainable energy system. Financial incentives have been a popular intervention to facilitate lowering one's electricity consumption, and they have been used to target both overall conservation and conservation at specific times of high demand, also referred to as peak consumption. However, recent findings are ambiguous with regard to the effectiveness of financial incentives, and research has thus far not clearly disentangled the effects of incentives on overall and peak consumption. This study provides meta-analytic evidence on the effectiveness of financial incentive interventions and uses meta-regression techniques to systematically evaluate differences in incentive characteristics and the contexts in which they are implemented. Using data from 72 studies (with 111 observations that include data from over 400,000 households), we compare the effects of financial information and actual incentives (e.g., pricing) on overall and peak electricity conservation. Financial incentives lead to a small decrease in overall consumption −1.83%) and a larger decrease in peak consumption (−10.00%), and effects of financial information are smaller compared to effects of actual incentives. Moreover, we find heterogeneous effects that can be further explained by differences in incentive types, characteristics, enhancing technologies, and study-level characteristics. We discuss theoretical as well as policy implications arising from these findings.

1. Introduction

Reducing greenhouse gas emissions due to the burning of fossil fuels requires a rapid shift towards a more sustainable energy system [1]. Demand-side energy conservation can play an important part in decreasing fossil fuel use and facilitating a sustainable energy transition (e.g., [2,3]). Specifically, households could decrease their overall electricity consumption, to avoid unnecessary energy generation that is currently largely based on fossil fuels (see for example [2]). Additionally, with the ongoing transition toward energy systems based on (more fluctuating) renewable energy generation, it becomes increasingly important that households do not just conserve energy overall, but become more flexible in terms of when they consume electricity, thereby helping to balance available generation with demand [4]. This requires that households reduce their electricity consumption specifically at times of high demand (referred to as peak consumption reduction), allowing the feed-in of a higher share of renewable energies into the grid and reducing the need for further grid investments [5,6]. Importantly, such demand-side decreases in energy consumption require individuals in households to change their behavior [7,8]. Numerous interventions have sought to stimulate energy conservation behavior (defined herein as a desired reduction in consumption through behavior changes) with mixed results (e.g., [9–11]). Among the strategies used to promote conservation behavior, financial incentive approaches are arguably most popular among policy makers (e.g., [12,13]). They are designed to either inform households about the financial consequences of their energy behavior (or any changes thereto) or provide direct rewards or price variations to change behavior. Given this popularity and widespread use of financial incentives in energy conservation programs, evaluation studies need to provide consistent and convincing evidence that these interventions can change behavior in the desired direction.

To date, empirical studies investigating financial incentives to promote energy conservation provided mixed results on their success [11,

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List of abbreviations

TOU	Time-of-Use
RTP	Real-Time Pricing
CPP	Critical Peak Pricing
CPR	Critical Peak Rebate

14–18]. This inconsistency in empirical findings is reflected in meta-analytic studies on behavioral interventions that included (though did not always exclusively focus on) financial incentives. In two earlier meta-analyses, Osbaldiston & Schott [17] and Maki et al. [16] investigated the effect of financial incentives on various pro-environmental behaviors including energy conservation. Their findings revealed small to medium effects, but only a few of the included studies directly targeted energy conservation. Delmas et al. [11] conducted a meta-analysis of information strategies, including financial information and incentives, and found that both were counterproductive, leading to a relative increase rather than a decrease in energy consumption when controlling for study-level characteristics. Among other interventions, Buckley [18] investigated the effectiveness of pricing strategies (i.e., actual financial incentives) and information on costs and savings in more recent smart-grid-era studies. Meta-analytic findings indicated that pricing strategies were ineffective in promoting energy conservation, and savings information even had a counterproductive effect, leading to a relative increase in energy consumption. These findings thus resembled those of Delmas et al. [11]. In even more recent meta-analytic work, Khanna et al. [19] found that financial incentives reduce energy consumption, and show the highest average effect size out of the different interventions examined. Conversely, the meta-analysis by Mi et al. [15] found financial incentives to have smaller effects compared to non-financial interventions, although both showed small to medium-size effects. Thus, even though studies on the effectiveness of financial incentives in reducing energy consumption exist, their findings remain somewhat ambiguous and inconclusive. Notably, none of the meta-analytic studies above focused solely on financial incentives to promote energy conservation but instead aimed to compare different types of interventions at a higher level. Financial incentives only comprised a subset of the included studies and were not examined in greater detail compared to the other intervention types. This leaves important research gaps regarding the impact of different types of financial incentive interventions as well as different contexts in which they are implemented. Apart from assessing their general effectiveness, detailed analyses that examine the conditions under which financial incentive interventions can lead to desired changes in behavior and thus reconcile the ambiguous findings of previous meta-analyses are scarce. Moreover, though studies have examined the effectiveness of incentives targeting both overall electricity consumption and consumption at specified peak times, they have not explicitly distinguished between these two types. We propose that this distinction is important because, from a behavioral science perspective, reducing one's electricity consumption in general or conserving at specific times (and thus possibly shifting usage) likely faces different barriers, which we will elaborate on below.

This study aims to provide meta-analytic evidence on the effectiveness of financial incentive interventions aimed at promoting energy conservation behavior. More specifically, we compare the effects of different types of financial interventions on two key types of energy conservation, namely overall energy conservation and peak conservation. Explicitly distinguishing and comparing incentive effects on overall and peak consumption is a novel approach that has not been considered in prior meta-analyses (cf. [18,19]). We also focus more closely on the characteristics of incentives and on the context in which they are implemented. In line with previous research (e.g., [11]), we categorize incentives as financial information or actual incentives (such as rewards or pricing) but refine the latter to account for different types of pricing incentives. Moreover, this paper extends existing studies by focusing on the contexts and conditions under which these financial interventions are effective, such as positive (e.g., rewards or savings) vs. negative (e.g., costs or fines) incentives and whether an incentive was combined with enhancing technologies or other behavioral interventions. We employ a meta-regression approach to systematically examine the influences of incentive characteristics, implementation context, and other study-level variables to determine their unique influence on electricity conservation.

In the following, we will review the theoretical basis on the effectiveness of financial incentive interventions. Next, we will outline the methodology and the results of our meta-analysis. The final section considers theoretical as well as practical implications of our findings and points to limitations and future research.

2. Theoretical background

2.1. Types of financial incentives

Financial incentives, here defined as any intervention that uses (the prospect of) financial consequences in order to change a target behavior, are a common intervention used to promote energy conservation (e.g., [14,15,20,21]). Financial incentives in the context of electricity consumption can be distinguished into two main categories. First, households can receive financial information about their electricity consumption, for example via feedback on their current or cumulative electricity costs [22,23]. This category thus represents an indirect incentive, as it aims to motivate cost savings from a corresponding reduction in consumption but does not provide any immediate financial benefits [11]. For example, Houde et al. [24] showed in a field experiment that providing real-time cost feedback to households led to a decrease in consumption by around six percent.

Second, actual incentives provide direct financial consequences in response to changes in electricity consumption. These incentives can be further distinguished into rewards for a given reduction in consumption (e.g., [25]) and electricity pricing. The latter typically aims to change electricity consumption at certain times by introducing varying price levels per unit of electricity for different times of the day, also referred to as dynamic pricing [26]. These pricing types are part of demand response approaches that aim to change the demand of electricity in response to varying grid conditions as well as changes in available energy supply [27]. The most common of these types of dynamic pricing are time-of-use (TOU) pricing, real-time pricing (RTP), critical peak pricing (CPP), and critical peak rebates (CPR; [4]). TOU pricing programs vary the price of electricity at fixed amounts and times of the day, thereby increasing the cost of consumption at certain peak times (when demand is generally high) relative to off-peak times. RTP programs use the same principle, but in contrast to TOU pricing the prices can vary more freely (e.g., changing every 30 minutes) based on market or system conditions. Whereas both TOU and RTP pricing confront consumers with changing price levels every day, critical peak programs usually only change the cost of consumption on a limited number of days during the year, when system demand is exceptionally high. CPP programs increase the cost of electricity during these critical peak periods compared to the rest of the time, usually with a substantially greater price ratio compared to TOU programs [28]. In contrast, CPR programs provide incentives in the form of rebates for customers who lower their electricity consumption during specified critical peak times compared to a reference point.

2.2. Economic and psychological research underpinning financial incentives

The popularity of financial incentives is based on the assumption that people behave as rational actors who will respond to an incentive when the benefits of doing so outweigh the costs (e.g., [29]). This assumption is well grounded in economic theories of self-interest and decision making, such as rational choice theory (e.g., [30]). From this perspective, financial incentives, such as changing electricity prices or the provision of information about electricity costs, are seen as an important driver of residential electricity consumption (e.g., [31]).

However, more recent evidence from psychological research suggests that the effectiveness of financial incentives on energy conservation behavior may be at least partly overestimated [32]. First, highlighting the financial consequences of a behavior may focus individuals on narrow cost-benefit calculations: a response to the financial incentive is expected if the financial benefits outweigh the costs of changing one's electricity consumption. However, some research has suggested that the financial gains derived from electricity consumption changes are often small and thus not perceived as worth the effort to change one's behavior [33–35]. Thus, it is questionable to what extent financial incentives that pertain to a small proportion of a household's expenditures can be effective in changing consumption behavior.

Moreover, research has questioned the notion that energy conservation behavior is mainly determined by self-interest, which casts further doubt on financial incentives' effectiveness (e.g., [36]). Indeed, a large body of literature has shown that energy behavior is driven by a multitude of psychological factors [37–39]. Next to an evaluation of individual costs and benefits, individuals also consider wider social consequences of their behavior. For example, energy conservation behavior is often driven by intrinsic environmental motivations such as values or personal norms [40,41], and individuals generally strive to behave consistent with these motivations [42]. In contrast to these intrinsic motivations, financial incentives provide an extrinsic reason to change one's energy consumption. From a perspective of rational behavior, it would be expected that financial incentives thereby provide an additional reason (next to any existing intrinsic motivations) that increases an individual's overall motivation to engage in energy conservation as long as they are in place [13,43]. However, psychological research has questioned whether extrinsic reasons indeed have such an additive effect or whether they might instead undermine existing intrinsic motivations to behave sustainably, such as environmental motivations to conserve energy [13,33]. Thus, highlighting or providing financial incentives may inadvertently decrease individuals' propensity to engage in energy conservation behavior and could even have counterproductive effects if the financial incentives on their own are insufficient to motivate sustainable behavior.

2.3. Variations in incentive characteristics and context

Given the equivocal research on financial incentives' effectiveness, it is likely that their effectiveness on electricity consumption is not determined by the presence of an incentive per se, but depends on how incentives are designed and implemented (see e.g., [44]). Financial incentives can vary in several characteristics, such as whether they provide information about costs or savings versus an actual incentive or price level, as described above. Although previous meta-analyses have examined both of these main types jointly, they did not explicitly compare their effects on electricity consumption [11,18]. Yet, it seems likely that actual incentives would have a greater impact on consumption than providing information on costs or savings. Similarly, a few studies have indicated that some pricing incentives (such as CPP or CPR) have a greater impact on consumption than other types (such as TOU pricing), but this evidence is based on systematic reviews rather than strict meta-analytic assessments [4,12]. We will thus extend the investigation of which incentive types are relatively more effective in the current research.

Another potentially important characteristic is the direction of an incentive: positive incentives represent a potential gain, such as a reward or a rebate, whereas negative incentives represent a potential loss in terms of a cost or price increase [16]. It is often assumed that negative incentives are more effective than positive ones, as people are generally loss averse and thus give more weight to financial costs than equivalent gains [36]. We will examine whether this holds true in the context of electricity consumption, specifically if cost information and prices have a larger impact than rebates or rewards.

Financial incentives are often implemented in combination with other behavioral interventions to increase their effectiveness [2,45]. For example, financial rewards for a certain amount of savings are frequently paired with feedback on the current or cumulative electricity consumption (e.g., [46]). Other behavioral interventions examined in energy conservation studies include commitments, goal setting, modeling, or social influence interventions such as comparative feedback (e.g., [10,21]. By targeting behavior change with a combination of interventions, it is assumed that the overall effect increases [45]. Some studies have provided evidence in favor of combined interventions. In a meta-analysis on feedback interventions, feedback had a significantly larger effect on energy conservation when it was paired with a financial incentive, but the evidence comprised only two observations [22]. In another meta-analysis, combining financial incentives with feedback and motivational interventions led to a combined effect larger than the sum of the individual interventions; however, the opposite was true for combinations of financial incentives and feedback (without motivation) in that the combined effect was lower than that of financial incentives alone [19]. We will examine differences in the effectiveness of financial incentives depending on whether they are paired with other behavioral interventions.

Apart from behavioral interventions, financial incentives can also be combined with so-called enhancing technologies that facilitate behavior change [47]. Also referred to as smart home technologies, they are increasingly seen as one cornerstone of energy efficient buildings [48]. In particular, information technology, such as in-home displays (IHDs), can provide timely and accurate information about a household's energy consumption [35]. Empirical evidence indicates that IHDs could facilitate energy conservation, either by providing information on their own or in combination with incentives such as electricity pricing (e.g., [12, 49]). However, other studies indicate that IHDs may be less effective than commonly assumed or that the type of information presented by an IHD might determine their effectiveness [50,51]. For example, Schultz et al. [52] found IHDs to lower household's electricity consumption when they provided social comparison feedback, but not when they provided consumption or cost feedback. Changes in energy consumption can also be facilitated by the use of automation technology. Automation can be implemented in the form of smart appliances, such as smart thermostats or dishwashers that allow individuals to preprogram certain temperatures or times. Alternatively, households can hand over control of certain appliances (such as air conditioners) to their utility company, which can temporarily reduce their electric load or switch them off entirely unless the consumer uses an override option [4]. Without automation, energy conservation requires a continuous response by individuals, which is not only effortful but requires adequate knowledge and awareness [53]. The implementation of automating technologies could thus enhance the effectiveness of financial incentives by decreasing the need for individuals to consciously and continuously respond. Empirical evidence supports the enhancing potential of automation (e.g., [12]) but conclusive meta-analytic evidence is still scarce.

2.4. Overall and peak electricity consumption

Financial incentive interventions have traditionally focused on causing overall energy conservation effects over the entire time that an intervention is in place, to decrease carbon emissions caused by electricity generation from fossil fuels (e.g., [2]). More recently, many countries have been transitioning towards energy systems based on renewable electricity generation and smart grids that allow a more direct communication with energy consumers [18]. Decentralized renewable energy generation can fluctuate due to weather conditions, and an increasing electrification on the demand side (e.g., from electric vehicles) poses additional challenges. In response, research is increasingly focusing on balancing generation and demand in a way that maximizes efficient grid use with energy from renewable sources and thus minimizes the (additional) use of fossil fuel-based generation [5,6]. This means a shift from merely promoting overall conservation to incentivizing a more flexible energy use, also referred to as demand response [4,35,54]. In particular, interventions can facilitate consumption reductions at specific peak times; if households successfully reduce their peak consumption as targeted by the incentive, they may optionally shift it to a different (off-peak) time rather than eliminating it altogether. Overall and peak consumption are thus two related facets of electricity consumption, although one does not necessarily affect the other (i.e., households could decrease their peak consumption without any changes in overall consumption and vice versa).

This distinction is likely to be important from a behavioral science perspective, implying different outcomes for incentives targeting overall or peak consumption. To achieve overall electricity conservation, households need to consistently change their consumption habits, which requires considerable attention and effort [37,55]. Substantial effects may only be achieved by energy efficiency improvements like the purchase of more efficient appliances, but this would require behavior changes not directly targeted by an energy conservation incentive, which are likely limited. In many households, this may limit conservation behaviors to those with considerable losses in comfort (e.g., turning off air conditioners in summer) or little gain (such as unplugging small appliances on standby). In contrast, reducing electricity consumption only at certain peak hours means that (essential) behaviors can be shifted to adjacent time periods without the need to completely forego them. Such shifts still require considerable attention and effort but extend the range of options for individuals' conservation efforts [47]. For example, rooms could be pre-heated or pre-cooled outside of targeted peak times, given that individuals are adequately aware of these options and sufficient incentives are in place. Reductions in peak consumption thereby also provide greater possibilities for automation than reductions in overall consumption, which could further reduce barriers to conservation [53]. To our knowledge, studies have not directly compared the effects of financial incentives on overall and peak consumption despite the obvious overlaps between these consumption types as well as the commonalities in financial incentives targeting them. For example, the meta-analysis by Buckley [18] describes possible differences in incentive effectiveness between overall and peak conservation and includes some peak pricing studies, but limits the analysis to effects on overall consumption. Similarly, Khanna et al. [19] included peak pricing studies along with other financial and non-financial incentives without examining how these might differentially impact overall and peak consumption. We aim to fill this gap with the current study by systematically disentangling both types of electricity consumption.

2.5. Current research

To summarize, this meta-analysis aims to investigate the effectiveness of financial incentives on electricity conservation behavior, as well as the conditions under which financial incentives can be effective. We do so by examining if incentives targeting peak conservation yield larger consumption reduction effects than incentives targeting overall conservation, based on the reasoning that the difficulty of reducing consumption as such should be higher for a household than merely shifting consumption at certain times. Moreover, in comparing different types of incentives, we examine whether actual incentives lead to bigger changes in consumption than the provision of financial information and further compare the effects of different pricing incentives on peak consumption. We also analyze if negative rewards produce bigger conservation effects than positive rewards. Finally, we investigate if incentives that are combined with other behavioral interventions or enhancing technology will be more effective in reducing consumption than solitary incentives.

3. Method

3.1. Literature search

We identified relevant articles for inclusion in this meta-analysis via two complementary strategies, using the established recommendations for conducting systematic reviews and meta-analyses [56,57]. First, we conducted a backward search on seven related systematic reviews and meta-analyses in the field of energy consumption [11,12,16–18,22,58]. This hand search aided the development of keywords using Boolean logic, specified as follows:

- Terms specifying a financial incentive (e.g., "financial" or "monetary")
- Terms specifying an intervention study (e.g., "experiment" or "trial")
- Terms identifying a change in electricity consumption (e.g., "reduction" or "consumption")
- Terms related to the domain of electricity ("electricity" or "energy")
- Terms specifying the domestic context (e.g., "residential" or "household")

Articles were included if they met four criteria. First, we considered only peer-reviewed articles from academic journals to facilitate study quality. Second, the study design needed to contain an intervention, such as studies conducting a pre-post comparison and/or including a control group. Non-empirical studies or those based on correlational evidence were excluded. Third, studies had to observe electricity conservation in a residential setting as either an overall reduction or a reduction in peak consumption. Studies focusing on other forms of energy consumption (e.g., natural gas) or only providing self-reported consumption were not considered. Fourth, studies had to contain quantitative statistics reporting energy savings either in kWh or percentages relative to a reference group. We used the observed relative reduction in consumption as the basic effect size in this meta-analysis.

Using these terms, we next searched four databases deemed most relevant to our research question, namely PsycINFO, EconLit, and ScienceDirect, and IEEExplore, thereby including publications in the English language from 1975 to 2022. We focused on these databases, as we expected them to cover the fields of economics and social sciences as well as engineering, which we deemed most relevant for this analysis. In total, this search led to 3393 results (PsycINFO: 38; EconLit: 1382; ScienceDirect: 1645; IEEExplore: 328) and an additional 52 results from the backward search. After the removal of 241 duplicates, we scanned the titles and abstracts of all articles and retained 215 articles for further screening (see Fig. 1 for the ROSES flow diagram of the selection procedure based on [59]). We retrieved the full text of these articles and excluded 149 additional articles based on the reasons described in Fig. 1. Most papers at this stage were excluded because they measured a different target variable than electricity consumption (e.g., natural gas or water consumption), did not provide financial incentives (e.g., only consumption feedback), or adopted a different methodology (e.g., simulation or qualitative study). In other cases, relevant statistics were not reported (e.g., only demand elasticities but no consumption data) or the article was not a primary study (e.g., a review). A few articles reported results on the same dataset that was included under a different title or only recorded consumption for selected appliances, rather than the entire household. The final sample consisted of 66 publications comprising 73 independent studies with data from 418,224 households (see Appendix A for the full list of publications). As shown in Fig. 2, most studies were conducted in the US, with smaller portions being conducted in Europe and Japan.

3.2. Data analysis procedure

Meta-analyses aim to provide average effect sizes by combining quantitative statistics across primary studies. This yields an increase in

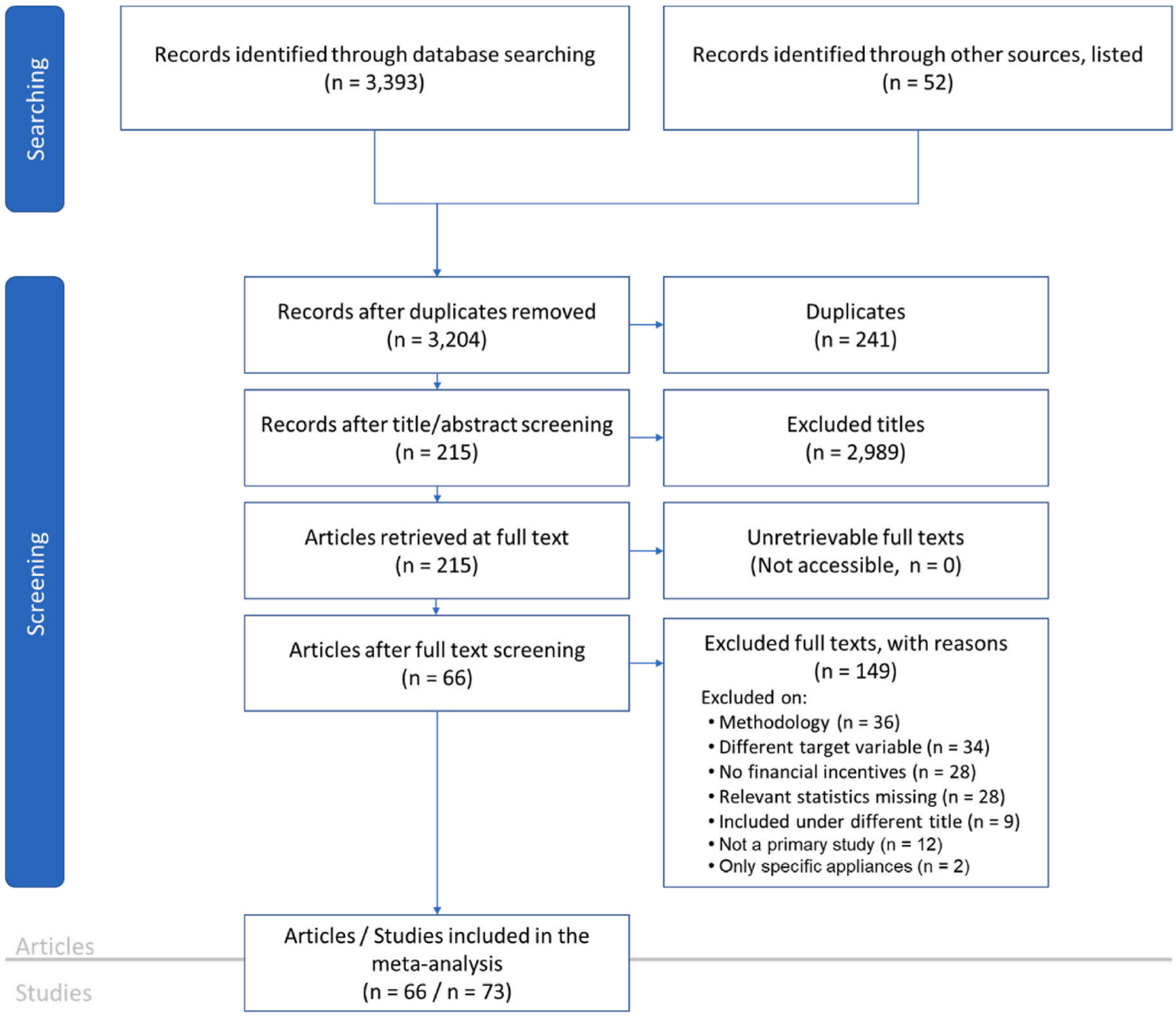

Fig. 1. ROSES flow diagram displaying the study selection procedure.

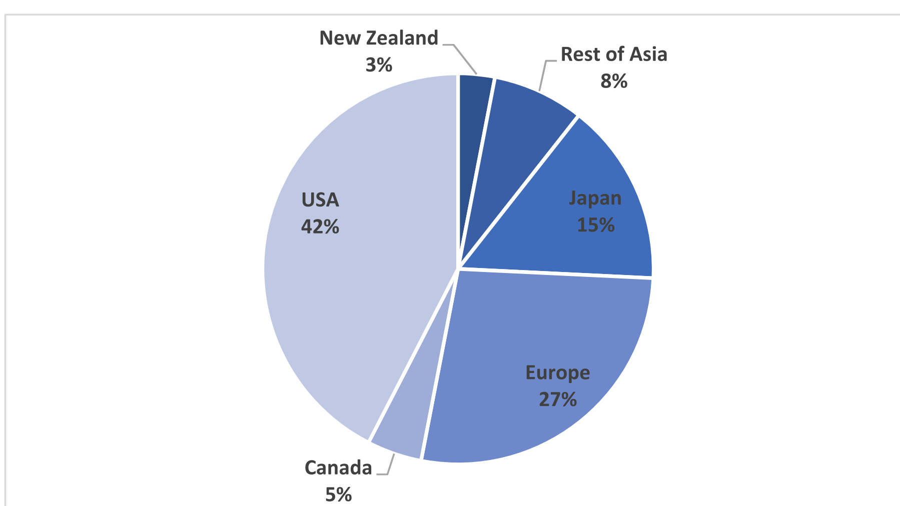

Fig. 2. Included publications by geographical region.

statistical power and a more precise estimate of the true effect [56,60]. The method can also be used to explain heterogeneity in effect sizes between single studies, which is also commonly observed in energy conservation studies [11]. Here, we employ meta-regression methods to explain heterogeneity in effect sizes due to variations in treatments (e.g., different types or combinations of incentives) and study-level characteristics (e.g., presence or absence of a control group). The following notation from Stanley & Jarrell [61] describes the meta-regression model that we relied on:

$$b_j = \beta + \sum_{k=1}^{K} \alpha_k Z_{jk} + e_j \quad \text{where} \quad j = 1, 2, \ldots, L \tag{1}$$

where b_j is the effect size expressed as the percentage change in electricity consumption for the jth primary study and L number of included observations.[1] β expresses the treatment effect when all other included predictors are held constant and α_k are the meta-regression coefficients estimating the biasing effect of k number of moderating variables Z (e.g., dummy variables indicating the presence of a control group or an incentive type). Finally, e_j captures the residual errors. Adopting the analysis approach of Buckley [18], we estimate the meta-regression coefficients via a weighted least squares (WLS) regression using the square root of the primary observations' sample sizes as analytical weights. This procedure addresses the problem of greatly varying sample sizes and heteroscedasticity in the absence of standard errors for the reported percentage effect sizes (see also [11]). Observations with larger samples sizes are thus given more weight, as they are considered more representative of the overall population. In the meta-regression itself, heterogeneity is further mitigated by including variations in study-level characteristics as covariates. An additional problem is the inclusion of multiple effect sizes for some of the independent primary studies, which leads to non-independence in included treatment effects. However, including only one effect size per study is inefficient because it means a loss of information. We therefore address the problem of non-independence by clustering standard errors by primary study.

3.3. Measures and coding strategy

3.3.1. Dependent variable

The dependent variable is expressed as the percentage change in electricity consumption resulting from a financial incentive intervention, based on the results of the primary studies. Reductions in consumption (i.e., savings) are expressed by a negative percentage and vice versa. In our main analysis, we report results separately for trials targeting overall consumption reductions and those targeting peak consumption reductions. We also recorded changes in overall consumption resulting from trials targeting peak consumption but excluded these from the main analysis, as they could pose a bias to the average effect sizes. Instead, we analyzed these changes in a separate step.

3.3.2. Independent variables

We developed a detailed coding table to account for variations in incentive type and how incentives were employed as well as other study-level characteristics relating to the design of the intervention as follows. First, we distinguished financial incentives into two basic categories: financial information merely communicates the costs or savings of electricity consumption (e.g., cost feedback) whereas actual incentives provide direct financial consequences through rewards or variations in electricity pricing. Within the category of pricing strategies we further distinguished the main types of dynamic pricing, namely time-of-use (TOU), critical peak pricing (CPP), critical peak rebates (CPR), and real-time pricing (RTP). Across all studies, we also recorded whether an incentive was a positive (e.g., rewards) or negative (e.g., costs) reinforcement. We also aimed to include the size of an incentive; however, this proved impossible due to a large variation in incentive types, differences in currencies and year in which the studies were conducted.

Second, we noted whether the financial incentive was combined with an additional intervention. Here we distinguished between supporting technological interventions and other behavioral interventions. Technologies included information technologies, such as in-home displays providing real-time feedback or devices displaying current electricity prices (see e.g., [62]). Whereas the presence of information technologies still requires a manual behavior change from households, automation technologies, such as smart thermostats, actively facilitate changes in consumption by automating them, making an active response by households unnecessary in some cases. Behavioral interventions included individual consumption feedback (in kWh), cost feedback, social comparison feedback, education (e.g., energy savings tips), environmental messaging (e.g., information about CO_2 savings), appeals (e.g., via letters), prompts, commitments, group interaction, gamification, or modelling.

Third, we included several study-level characteristics: *control group* captured whether the study compared an intervention group to a group that did not receive the intervention. *Random assignment* means that households were randomly assigned to a group, as opposed to self-selecting a group. *Baseline* indicates that electricity consumption was measured before and after the incentive was introduced (independent of the presence of a control group). Studies with *weather controls* adjusted for changing temperature patterns, which can influence electricity consumption. *Demographic controls* indicate that a study accounted for differences in socio-demographic characteristics (such as education or income) in the model. *Recruitment mode* recorded whether participation in the study was mandatory for households, as opposed to participating voluntarily on an opt-out or an opt-in basis. Since only one study reported a recruitment on an opt-out basis, we created a dummy variable that indicated a mandatory recruitment mode (vs. voluntary participation and studies in which participation mode was not clearly stated). We further recorded the *duration of the incentive* in months. *Frequency* indicated how often the incentive was communicated to households (with levels varying from 1 = *once* to 2 = *bi-monthly*, 3 = *monthly*, 4 = *bi-weekly*, 5 = *weekly*, 6 = *daily*, 7 = continuously) and was treated as a continuous scale in the meta-regression analysis. Missing values for the frequency of the incentive were imputed based on the median value, separately for studies targeting overall and peak savings. We also recorded the *season* in which the incentive was tested in four categories (summer, winter, spring/fall, or throughout a whole year) as well as the *geographic region* of the studies (Asia/Australia, Europe, North America).

We used certain combinations of study-level characteristics to classify observations as high, medium, or low in methodological rigor for the later analysis. High-quality studies were those that included weather controls, had a control group with random assignment as well as a baseline, and statistically controlled for at least some socio-demographic measures, such as income or household size. Medium-quality studies had to include weather controls and a control group (but not necessarily randomly assign households to these groups). Low-quality studies comprised all remaining studies without these controls. We did not consider if recruitment was voluntary (opt-in) or mandatory for this analysis, as including this variable led to almost no high-quality studies in combination with the other control variables.[2]

Some variables are only reported in studies targeting either overall electricity savings or peak savings but not both (e.g., pricing interventions are frequently used to influence peak consumption but not overall consumption). We adapted our analysis to these patterns and thus sometimes only report results for one type of dependent variable (either overall or peak consumption).

4. Results

4.1. Descriptive statistics

The meta-analysis included 73 independent primary studies with a

[1] We use the term *observation* because some of the independent studies in this meta-analysis contain multiple treatment groups. Observations are thus the main unit of analysis, as they contain a single effect size.

[2] Instead, we point to the results of the meta-regression analysis, in which the influence of all study characteristics including type of recruitment is examined.

total of 111 observations (plus a further 23 observations as described below), 50 for overall electricity consumption and 61 for consumption at peak times. In addition, some studies targeting peak consumption additionally reported changes in overall consumption, and these effect sizes ($N = 23$) were separated from the main analysis and examined in a separate step, to explore the question if peak consumption trials lead to overall savings in electricity consumption that were not directly targeted.

With respect to the independent variables, Table 1 shows the relative frequency of the incentive and study-level characteristics. We categorized 35 observations as financial information incentives and 76 observations as financial incentives. The latter category of financial incentives included both pricing incentives ($N = 57$) and rewards ($N = 19$), and these sub-categories were highly correlated with overall vs. peak consumption observations. Specifically, studies targeting overall savings used financial information incentives ($N = 31$) and actual incentives in the form of rewards ($N = 16$), but only three used pricing incentives. In contrast, studies targeting peak consumption overwhelmingly used pricing incentives ($N = 54$) and only a fraction used incentives in the form of rewards ($N = 3$) or financial information ($N = 4$). The large number of peak pricing incentives can be further distinguished into common pricing types, namely time-of-use (TOU) pricing ($N = 26$), real-time pricing (RTP, $N = 3$), critical peak pricing (CPP, $N = 14$), and critical peak rebates (CPR, $N = 10$). In total, 69 incentives had a negative direction (e.g., financial information or an actual price increase) and 42 had a positive direction (e.g., saving rewards or rebates).[3]

As Table 1 shows, most financial incentives were not used in isolation but combined with supporting technologies or other behavioral interventions. The number of interventions tested in addition to the primary financial incentive ranged from 0 to 6, with a median of 1 additional intervention ($M = 1.31$, $SD = 1.34$). Overall, 69% of the observations included some combination, but the occurrence of certain technologies or behavioral interventions varied depending on whether studies targeted overall or peak electricity consumption. Supporting technologies were often combined with peak consumption incentives whereas behavioral interventions were more frequently combined with overall consumption incentives.

Individual consumption or cost feedback and education (such as energy saving tips) were common interventions in addition to the financial incentive, whereas social comparison feedback and environmental messaging were less frequently used. Other intervention types were rarely employed (Ns < 5, namely appeals, commitments, gamification, group interaction, modeling, prompts) and were thus excluded from the further analysis. The bivariate correlations displayed in Appendix B provide further insight into which technologies or behavioral interventions had a tendency to co-occur in a study. For example, individual consumption and cost feedback did not systematically co-occur, but both were more likely to be combined with information technologies.

Regarding study-level characteristics, Table 1 shows that most observations employed a control group and baseline measurements, but fewer observations used random assignment or included socio-demographic variables as statistical controls. Only a minority of the observations report a mandatory participation mode (in 17 cases, this was not clearly reported, those studies were included with the majority of voluntary participation observations). Incentives were in place from one week to six years, with a median of four months and a mean of 8.44 months ($SD = 11.89$). Moreover, most incentives were frequently communicated, the median frequency being 6 (corresponding to a daily frequency, $M = 5.45$, $SD = 1.71$). Most incentives were tested during summer months (44%) or throughout a whole year (35%), with fewer studies taking place in winter (16%) or the spring or fall months (5%).

Table 1
Descriptive statistics displaying the percentage of observations showing a certain characteristic out of the 111 observations included in the main analysis.

Variable	% of observations		
	Overall consumption	Peak consumption	All observations
	($N = 50$)	($N = 61$)	($N = 111$)
Incentive characteristics			
Category: actual incentive (remaining: financial information)	38%	93%	68%
Direction: negative (remaining: positive)	54%	69%	62%
Incentive combinations			
Information technology (e.g., IHDs)	28%	41%	35%
Automation (e.g., smart thermostats)	0%	13%	7%
Ind. consumption feedback (in kWh)	54%	28%	40%
Cost feedback (in monetary units)	30%	21%	25%
Social comparison feedback (e.g., with neighbors as the reference group)	20%	10%	14%
Education (e.g., energy savings tips)	48%	15%	30%
Environmental messaging (e.g., CO_2 savings)	10%	5%	7%
Study-level characteristics			
Control group	82%	84%	83%
Random assignment	72%	66%	68%
Baseline	84%	79%	81%
Weather controls	28%	62%	47%
Demographic controls	28%	18%	23%
Recruitment mode: mandatory (remaining: voluntary)	24%	8%	15%

4.2. Average treatment effects

Table 2 shows the average treatment effect across studies, indicated as the percentage change in electricity consumption due to the financial incentives. Across all studies, financial incentives led to an average consumption reduction of roughly −8%. While this indicates that financial incentives can generally reduce electricity consumption, the standard deviation of about 10% indicates a considerable variation in the effect sizes, warranting a more detailed examination of potential factors causing this heterogeneity. When weighted by the square root of the sample size, the average change in consumption is −5.95%. The effect of financial incentives differed depending on whether overall consumption or peak consumption was targeted. Whereas overall consumption was only reduced by about −2%, trials targeting peak consumption achieved a weighted average effect of −10%. This suggests that financial incentive interventions are more successful when targeting consumption at specific times.

We further examined if the treatment effects differed between studies with higher methodological rigor (i.e., stricter experimental controls) and those with lower rigor. Table 2 shows that both high-quality and low-quality studies show similar weighted ATEs close to the overall weighted ATE, and this pattern is similar for studies targeting

[3] In three studies, the direction of the incentive was ambiguous (e.g., both financial savings or costs were communicated). These observations were coded as incentives with a positive direction.

Table 2
Average treatment effects and effects by study quality.

		Obs.	Min (%)	Max (%)	Median TE (%)	ATE (%)	Weighted ATE (%)	SD (%)
Average treatment effect (ATE)	All studies	111	−33.00	37.77	−6.60	−7.80	**−5.95**	9.83
	High-quality studies	11	−27.20	5.30	−5.00	−7.70	**−7.61**	8.40
	Medium-quality studies	34	−32.50	0.70	−13.12	−13.12	**−14.09**	9.09
	Low-quality studies	66	−33.00	37.77	−5.08	−5.08	**−3.96**	9.40
ATE overall consumption	All studies	50	−33.00	37.77	−3.85	−3.63	**−1.83**	9.36
	High-quality studies	4	−7.33	5.30	−4.78	−2.90	**−3.08**	5.59
	Medium-quality studies	8	−13.80	−1.98	−9.64	−8.73	**−8.57**	4.65
	Low-quality studies	38	−33.00	37.77	−3.04	−2.64	**−1.30**	10.13
ATE peak consumption	All studies	61	−32.50	6.40	−10.00	−11.21	**−10.00**	8.90
	High-quality studies	7	−27.20	−1.00	−9.50	−10.45	**−8.72**	8.81
	Medium-quality studies	26	−32.50	0.70	−13.33	−14.47	**−15.40**	9.74
	Low-quality studies	28	−27.00	6.40	−7.59	−8.39	**−7.80**	7.25

overall savings and those targeting peak savings.

To test for possible publication bias, Fig. 3 plots the treatment effects on the x-axis and the square root of the respective sample size on the y-axis, separately for observations targeting overall and peak savings. In these funnel-plots, it would be expected that individual effect sizes based on larger sample sizes are closer to the true effect, as larger sample sizes generally lead to more accurate treatment effects. If this is the case, effect sizes should cluster relatively symmetrically around the mean effect size. The figure shows that this is roughly the case, indicating that publication bias is not a concern. The figure further shows that effect sizes for changes in overall consumption cluster relatively narrowly around the weighted mean effect size, although the variance seems somewhat larger for studies with fewer observations. In contrast, effect sizes for changes in peak consumption generally show a greater variation around their weighted mean.

As a robustness check of the reported average effects, we followed the suggestion by Stanley et al. [63] and re-ran the analysis including only the 10% of studies with the highest sample size, separate for trials targeting overall and peak electricity savings. For observations on overall electricity savings, the top 10% of observations lead to a consumption reduction of −1.04%, which is slightly smaller than the weighted mean reduction of −1.83% across all observations. Similarly, the analysis suggests that most observations on peak savings may slightly overestimate the treatment effects, as the studies with the highest sample size yield an average consumption reduction of only −8.04% compared to the average reduction of −10.00% across all available studies.

To explore possible changes in effects sizes over time, Fig. 4 plots the individual effect sizes against their publication year, with a trend line for overall and peak consumption effects, respectively. It can be seen that large variations in effect sizes persist over the course of several decades. At the same time, there seems to be a trend for treatment effects to become smaller in more recent publications, which could potentially be explained by changes in energy efficiency standards or higher baselines in environmental practices. While this trend seems to occur for observations on both overall and peak consumption, the latter trend should be viewed with caution, as very few studies targeting peak consumption were published before the year 2005.

Another important criterion is whether treatment effects of financial incentives produce consistent consumption reduction effects over time or whether these effects instead cease over time. Fig. 5 shows the individual effect sizes plotted against the number of months the financial incentive was in place. The Figure shows no effect of incentive duration. However, the figure also shows that most incentives were in place for less than six months, which makes it difficult to draws robust conclusions about the persistence of the effects.

4.3. Effects of peak savings programs on overall electricity consumption

We next examined the question if peak electricity programs produce wider changes in electricity consumption, such as overall savings outside of the targeted peak hours. Out of all the studies targeting peak

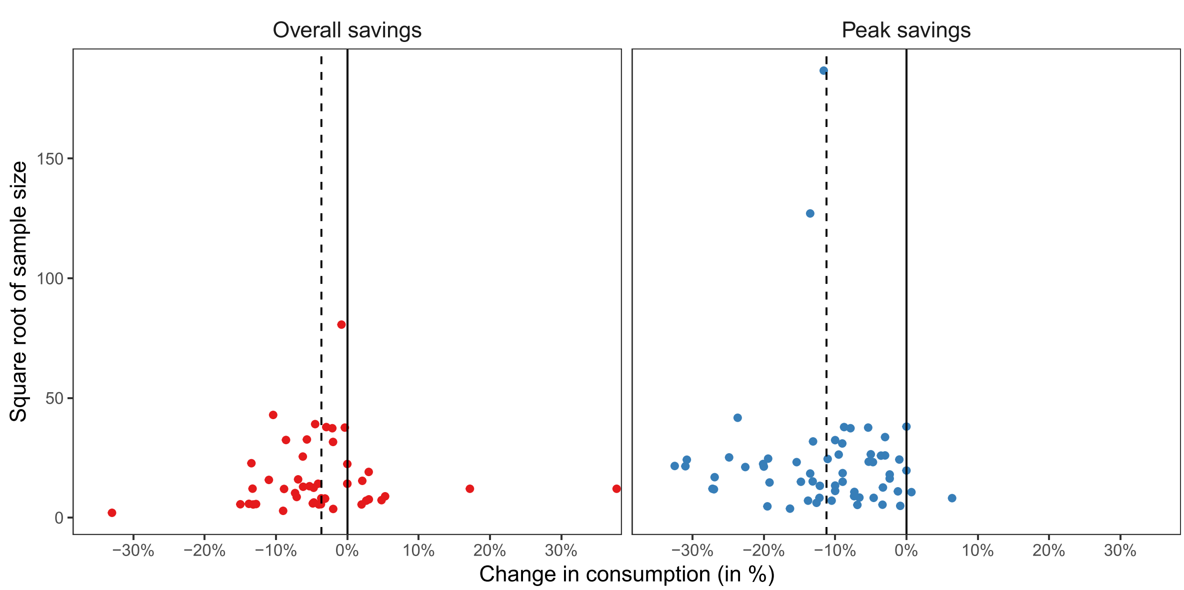

Fig. 3. Funnel plot of effect size vs. sample size *Note*. For better interpretability, two observations with a square root of the sample size >200 are excluded from the plot.

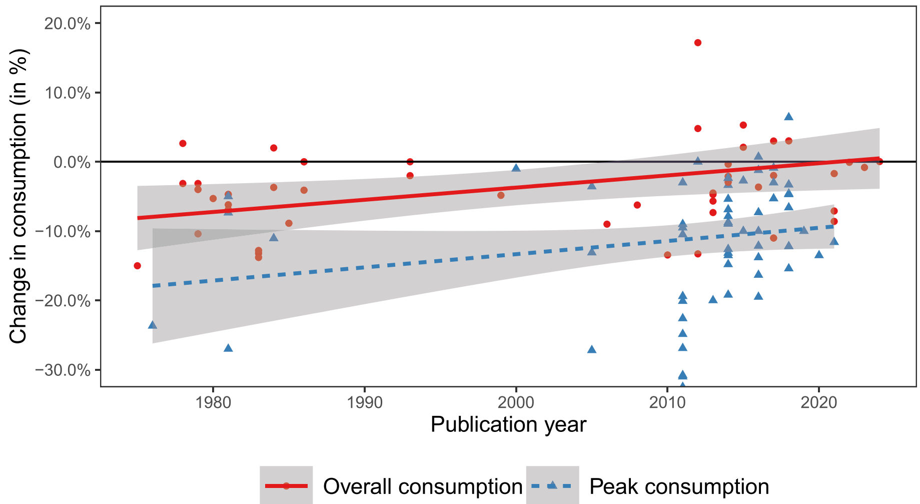

Fig. 4. Effect size vs. publication year *Note.* The lines visualize the trend in the data points (standard errors indicated in grey).

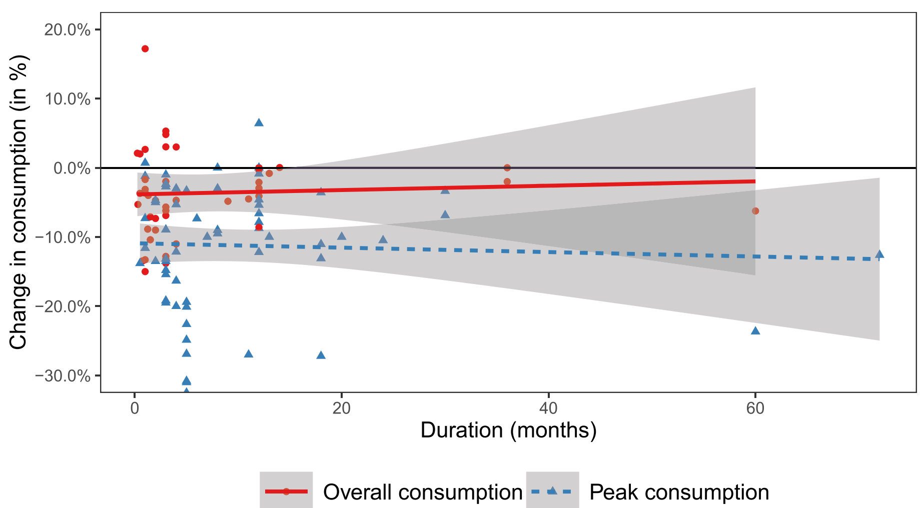

Fig. 5. Effect size vs. incentive duration.

savings, 23 observations (stemming from 14 independent studies) reported effects on households' overall electricity consumption, next to the immediate effects on peak consumption that were the primary target. This subset of observations leads to a weighted mean effect on peak savings of −9.18%, which is very close to the effect across all included studies reported above. Beyond these targeted savings, the 23 observations that were excluded from the main analysis above show a small reduction in overall electricity consumption (weighted M = −1.32%, SD = 7.33). Thus, one the one hand, programs targeting peak savings do not seem to produce adverse effects regarding overall electricity consumption levels, in that households do not seem to (over) compensate their peak hour response at other times of the day. On the other hand, the additional effects on overall consumption are small in size and less than half of what programs targeting overall savings produce on average.

4.4. Variations in treatment effects across different types of financial incentives

The descriptive analysis showed that the two main categories of incentives, namely financial information and actual incentives, were not evenly tested in observations targeting overall electricity savings and those targeting peak savings (Table 1). In particular, while actual incentives were examined both in studies targeting overall and peak consumption, financial information without any actual incentives was almost exclusively observed in studies targeting overall consumption. Fig. 6 shows the percent changes in electricity consumption (i.e., the treatment effect) for each of the main categories of incentives that occurred in the data. For studies on overall consumption, incentives ($M_{weighted}$ = −5.72%, SD = 8.43, 95%CI [−9.78; −1.66]) lead to greater consumption reductions compared to merely providing financial information about costs ($M_{weighted}$ = −1.09%, SD = 9.55, 95%CI [−4.60; 2.41]). For financial information strategies, the confidence interval includes zero, indicating no reliable reduction in overall electricity consumption. Actual incentives lead to substantial reductions in peak electricity consumption ($M_{weighted}$=−10.30%, SD = 9.10, 95%CI [−12.70; −7.88] even greater than those of incentives in observations on overall consumption. Financial information also seems to have an (albeit smaller) effect on peak consumption, but the evidence base for this effect is very weak. Fig. 6 also indicates a large variation in effect sizes among peak incentives, ranging from peak consumption reductions of more than 30% to practically no reductions. Since incentives targeting peak consumption almost exclusively consist of pricing incentives, this warrants a more fine-grained examination of these pricing incentives, which we investigated in more detail via meta-regression analysis.

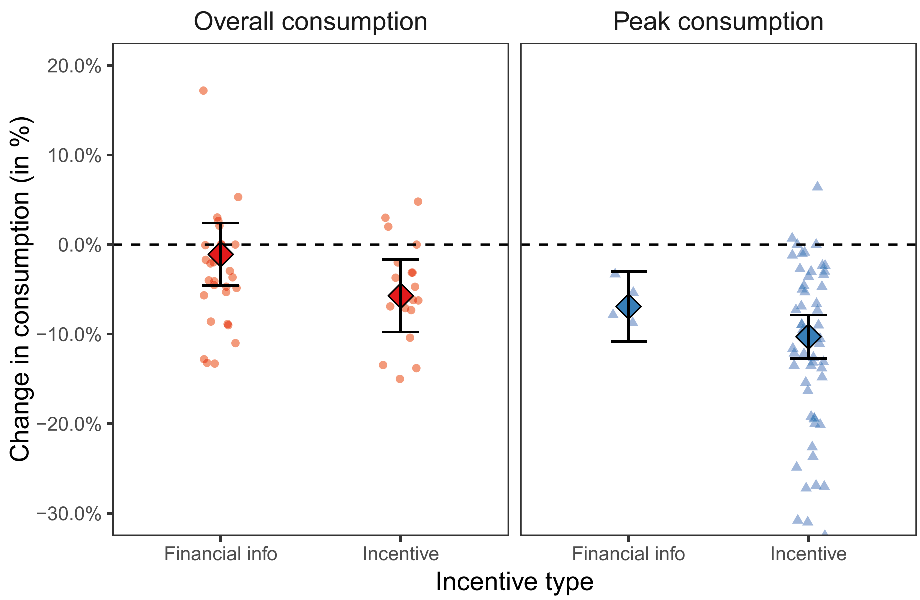

Fig. 6. Average treatment effects by incentive types *Note.* Error bars represent 95% confidence intervals around the weighted mean, the semi-transparent data points represent individual observations. The financial information effects on peak consumption should be treated with caution, as there are few data points and three of the displayed observations are derived from the same study and are thus non-independent.

4.5. Meta-regression

To examine the unique influence of different incentives and study-level characteristics, we conducted a meta-regression analysis consisting of three overarching hierarchical regression models. Model 1 examined all studies from the main analysis reported above, thus including observations on both overall and peak electricity consumption. In three steps, we (1) formally tested the difference between overall and peak consumption incentives, (2) examined the effects of financial information versus actual incentives, and (3) analyzed a full model that additionally examined differences in effectiveness due to the direction of an incentive (positive or negative). Throughout all steps, we controlled for the influence of study-level characteristics, in order to determine the biasing effects of incentive characteristics over and above these differences in study design. We subsequently analyzed observations targeting overall consumption (Model 2) and peak consumption (Model 3) separately to examine variations in effect sizes in more detail. As almost all peak incentives were some type of pricing, we focused on comparing the different types of peak pricing in this third model. Otherwise, Models 2 and 3 followed the same five steps: Step (1) examined the effect of incentive type (financial information vs. actual incentives in the case of overall consumption and different pricing types in the case of peak consumption); Step (2) additionally analyzed effects due to the direction of the incentives, Step (3) the influence of enhancing technologies, Step (4) the effects of combined behavioral interventions, and Step (5) tested a full model with all of the above steps included simultaneously. The inclusion of specific predictor variables slightly differed between the two overarching models, depending on their relevance to overall or peak consumption observations (e.g., no overall consumption studies tested the use of automation).

Table 3 displays the results of the first meta-regression model including all observations. As Table 3 shows, the meta-regression confirms the results from above by showing that targeting peak savings is more effective than targeting overall savings, when all study-level characteristics are taken into account. Furthermore, Step 2 suggests that actual incentives do not lead to substantially different savings compared to financial information, although estimates show a small trend in this direction. Step 3 shows that consumption changes from incentives with a positive direction (e.g., savings rewards or rebates) do not differ from negative incentives. Regarding the study-level characteristics, studies with a baseline lead to a greater consumption reduction. Savings are also greater the longer an incentive is in place, but this effect is small in magnitude (confirming the pattern shown in Fig. 5). Most other study characteristics, such as the year of publication or the location of the study, do not bias the magnitude of the treatment effect.

The second meta-regression includes only observations on overall consumption changes and is displayed in Table 4. The model steps explain almost no variance in the treatment effect when adjusting for the number of predictors and sample size (as indicated by $R^2_{adjusted}$), and the results should thus be viewed with caution. Further analysis suggested that this poor fit might be driven by some influential cases (see Appendix C for model results after removal of influential cases). No variations in incentive characteristics or combinations with other interventions affect any changes in consumption. Specifically, the results indicate no difference between financial information interventions and actual incentives (Step 1), and no effect of an incentive's direction (Step 2) or the use of information technology (Step 3). There are also no significant effects of any combinations with other interventions, such as education or individual consumption feedback, although the signs of these coefficients all point in a negative direction, suggesting that behavioral interventions might slightly enhance the effects of financial incentives (Step 4). Study characteristics are generally unrelated to the treatment effect across all estimated models with few exceptions.

The third meta-regression includes only observations on peak consumption changes. Our descriptive analysis in Fig. 6 showed a great spread in treatment effects particularly for pricing incentives targeting peak consumption, and the meta-regression results in Table 5 show that different types of pricing can explain this variation. Specifically, real-time pricing and time-of-use pricing trials consistently (across Steps 1–5) lead to smaller consumption reductions compared to critical peak pricing, while critical peak rebates lead to roughly the same effect as critical peak pricing. Beyond these effects of different pricing types, there is no significant effect of the direction of the incentives (i.e., positive vs. negative) in Step 2. Step 3 shows that both the use of information technology and automation lead to an additional consumption reduction (though in the full model, only the effect of automation retains significance). Other combinations with behavioral interventions generally don't affect the treatment effect in a significant way (Step 4). Study-level characteristics also affect the measured changes in peak

Table 3
Meta-regression of treatment effects on incentive types and study characteristics.

	Model		
	(1)	(2)	(3)
	Study target	Type	Full model
Target: peak consumption	−0.081***	−0.062***	−0.076***
	(0.015)	(0.018)	(0.018)
Type: incentive (reference: financial info)		−0.039	−0.025
		(0.034)	(0.032)
Incentive direction: positive (reference: negative)			−0.036
			(0.019)
Control group	−0.041	−0.044	−0.038
	(0.030)	(0.030)	(0.024)
Random assignment	−0.047	−0.051	−0.049
	(0.034)	(0.035)	(0.032)
Baseline	−0.051*	−0.052*	−0.035
	(0.025)	(0.025)	(0.025)
Weather controls	−0.025	−0.017	−0.018
	(0.022)	(0.022)	(0.021)
Demographic controls	0.032	0.025	0.005
	(0.021)	(0.020)	(0.026)
Mandatory participation	−0.012	−0.007	−0.009
	(0.027)	(0.029)	(0.022)
Incentive duration (mo.)	−0.003***	−0.003***	−0.003***
	(0.001)	(0.001)	(0.001)
Incentive frequency	−0.002	−0.002	−0.002
	(0.003)	(0.003)	(0.003)
Publication year	0.020	0.028	0.027
	(0.035)	(0.041)	(0.040)
Season: spring/fall (reference: summer)	−0.018	−0.009	−0.003
	(0.029)	(0.030)	(0.030)
Season: winter	0.043	0.045	0.064*
	(0.027)	(0.028)	(0.030)
Season: year	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)
Location: Europe (reference: Asia/ Australia)	−0.002	−0.027	−0.042
	(0.026)	(0.040)	(0.039)
Location: North America	−0.010	−0.024	−0.032
	(0.037)	(0.042)	(0.039)
Constant	0.076	0.109	0.106
	(0.049)	(0.066)	(0.059)
R^2	.501	.509	.537
Adjusted R^2	.422	.426	.452
Number of observations	111	111	111
Number of clusters	71	71	71

Note: $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$; standard errors are displayed in parentheses and are clustered by the primary independent study.

consumption. Specifically, observations with random assignment report greater consumption reductions, whereas the consideration of demographic characteristics has the opposite effect, which might indicate that particular socio-demographic or housing characteristics show larger effects. Moreover, studies with voluntary participation produce a significantly greater consumption reduction than studies in which participation is mandatory. A small effect of publication year confirmed the trend shown in Fig. 4 above that the reductions in peak consumption are smaller in more recent publications compared to older ones. Studies conducted in Europe seem to lead to somewhat greater consumption reductions compared to those conducted in Asia or Australia, but this effect is only significant in two out of the five model steps.

5. Discussion

5.1. Summary and theoretical implications

This study investigated the effectiveness of financial incentives in promoting two types of household electricity conservation, namely overall and peak conservation. Both benefit the sustainable energy transition, as overall consumption reductions can avoid additional generation based on fossil fuels (e.g., [2]) and peak consumption reductions can increase grid efficiency as well as the feed-in of renewable energies by better matching demand to generation and grid conditions [5]. We examined the effectiveness of financial incentive interventions in promoting both types using a meta-analysis of 73 independent studies comprising 134 observations. Using meta-regression, we also systematically examined whether the effectiveness varied across different incentive characteristics and other study-level variables.

Our analysis indicated that financial incentives can affect electricity consumption, but the extent to which they do so depends both on the characteristics of the incentives and on the type of electricity behavior they target. Whereas we found that financial incentives led to an average reduction in consumption of close to −6% across all studies, this effect differed depending on whether overall or peak conservation was targeted. Financial incentives only achieved an average conservation of about −2% when they targeted overall conservation (i.e., including peak as well as off-peak times) but achieved greater reductions of almost −10% when peak consumption was targeted. This finding is important, as it shows that household energy behavior is multifaceted and it is thus crucial to consider the specific behavioral context targeted by an intervention such as a financial incentive. Specifically, the fact that households achieved greater percentage reduction at specified peak times (and particularly on a limited number of critical peak times) indicates that these behavior changes face different barriers (and likely require less effort) compared to behavior changes necessary for overall reductions in consumption.

Our findings regarding financial incentive effects on overall savings are similar to those of previous meta-analyses. Delmas et al.'s [11] descriptive estimates indicated conservation reductions from financial incentives between −5.5 and −7.7% for studies conducted before 2013. Conversely, Buckley's [18] meta-analysis that focused on studies conducted after 2010 found smaller effects of financial incentives between −1.0 and −2.7%, and Khanna et al. [19] found financial incentives to reduce consumption by about 1.6%. Our results of about 1.8% that include both older and more recent studies fall within this range and hence corroborate a growing body of literature showing a small effect of financial incentives on overall electricity conservation (see also [15]). Extending previous studies, we compared the effects of financial information incentives and actual incentives (e.g., in the form of rewards). Our descriptive results suggest that financial information leads to only negligible reductions in consumption (with the confidence interval around the weighted mean including zero), while actual incentives lead to greater reductions in the range of −5% to −6%. Yet, a formal comparison of these two incentive categories via meta-regression did not indicate that actual incentives lead to significantly different conservation effects than financial information when controlling for study-level characteristics.

Meta-analytic research on the effectiveness of financial incentives targeting peak consumption is scarcer and our insights address an important research gap. For example, Srivastava's [58] meta-analysis did not estimate specific conservation effects stemming from pricing programs but used a binary measure of whether a peak consumption program was successful or not. Faruqui & Sergici's [12] review only included a small sample of peak pricing programs and did not provide a systematic meta-analysis of the included studies. They estimated that TOU pricing led to a 3–6% reduction in peak consumption and critical peak pricing reduced peak consumption by 13–20%. More recently, Parrish et al. [4] conducted a systematic review of a greater variety of peak consumption programs. Their descriptive analysis suggested effects of TOU pricing similar to those of Faruqui & Sergici [12] and relatively greater effects of CPP in the range of about −25%, but generally their results suggested a large heterogeneity in individual effects. Our insights expand on this evidence, showing that the effects of pricing incentives are heterogeneous and critical peak pricing and rebate incentives lead to

Table 4
Meta-regression results for studies targeting overall savings.

	Model				
	(1)	(2)	(3)	(4)	(5)
	Type	Direction	Technology	Combinations	Full model
Type: Incentive (reference: financial info)	−0.004	−0.011	0.002	0.002	−0.025
	(0.040)	(0.050)	(0.040)	(0.052)	(0.089)
Incentive direction: positive (reference: negative)		0.014			0.049
		(0.030)			(0.069)
Information technology			0.041		0.080
			(0.027)		(0.058)
Individual consumption feedback				−0.035	−0.018
				(0.029)	(0.033)
Monetary feedback				−0.021	−0.022
				(0.058)	(0.071)
Comparative feedback				−0.012	−0.031
				(0.039)	(0.045)
Education				−0.012	−0.012
				(0.021)	(0.022)
Environmental messaging				−0.030	−0.010
				(0.050)	(0.059)
Control group	0.044	0.046	0.050	0.039	0.065
	(0.044)	(0.045)	(0.043)	(0.055)	(0.058)
Random assignment	−0.071	−0.071	−0.076	−0.075	−0.077
	(0.045)	(0.045)	(0.042)	(0.051)	(0.041)
Baseline	−0.008	−0.009	−0.017	−0.020	−0.033
	(0.027)	(0.027)	(0.028)	(0.038)	(0.044)
Weather controls	−0.054*	−0.056*	−0.059*	−0.032	−0.056
	(0.026)	(0.027)	(0.026)	(0.038)	(0.036)
Demographic controls	0.037*	0.046*	0.031	0.049	0.079
	(0.016)	(0.022)	(0.019)	(0.031)	(0.049)
Mandatory participation	−0.050	−0.047	−0.060*	−0.033	−0.041
	(0.026)	(0.028)	(0.024)	(0.032)	(0.035)
Incentive duration (mo.)	0.013*	0.012	0.018**	0.016	0.017
	(0.006)	(0.007)	(0.006)	(0.011)	(0.014)
Incentive frequency	−0.010*	−0.009	−0.016**	−0.008	−0.019
	(0.004)	(0.004)	(0.005)	(0.005)	(0.012)
Season: spring/fall (reference: summer)	−0.028	−0.033	−0.027	−0.007	−0.037
	(0.032)	(0.031)	(0.034)	(0.047)	(0.050)
Season: winter	−0.039	−0.041	−0.048	−0.060	−0.078
	(0.029)	(0.029)	(0.029)	(0.046)	(0.048)
Season: year	−0.181*	−0.177*	−0.237**	−0.189	−0.259
	(0.077)	(0.077)	(0.074)	(0.175)	(0.196)
Publication year	0.002	0.002	0.001	0.002	0.00002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Location: Europe (reference: Asia/Australia)	0.001	0.012	−0.009	0.005	0.017
	(0.041)	(0.039)	(0.040)	(0.057)	(0.046)
Location: North America	0.051	0.058	0.013	0.041	−0.010
	(0.052)	(0.049)	(0.052)	(0.066)	(0.085)
Constant	0.150	0.122	0.234*	0.197	0.243
	(0.083)	(0.090)	(0.091)	(0.137)	(0.137)
R^2	.422	.424	.435	.452	.473
Adjusted R^2	.142	.117	.133	.030	.000
Number of observations	47	47	47	47	47
Number of clusters	36	36	36	36	36

Note: *$p < .05$; **$p < .01$; ***$p < .001$; standard errors are displayed in parentheses and are clustered by the primary independent study.

significantly greater reductions in peak consumption (approximately an additional 10%) compared to time-of-use or real-time pricing. Notably, both of these reviews included grey literature and could only provide descriptive summary statistics, whereas we confined our analysis to peer-reviewed published articles and additionally provided a quantitative evaluation via meta-regression techniques.

By analyzing a subset of the included observations that recorded the effects of incentives targeting peak consumption on both overall and peak consumption, we shed further light on whether incentives targeting specified peak times can have wider conservation effects or be counterproductive for overall conservation. Our results show a small reduction in overall consumption when peak consumption was targeted. At the least, this suggests no counterproductive effect of peak pricing incentives: households may shift their electricity consumption from peak hours to off-peak hours but they do not overcompensate by increasing the overall consumption. This is important, as such overcompensation effects seem possible for certain appliances uses, such as deliberately pre-cooling a room below a desired temperature prior to an announced peak time.

Using meta-regression techniques, we examined the biasing influence of further incentive characteristics on electricity consumption. Across the three models, we did not find that the direction of an incentive influences its effectiveness. Neither the meta-regression on overall consumption nor the one on peak consumption found that incentive direction matters when controlling for specific incentive types. These findings indicate that general cognitive biases like loss aversion (cf. [36]) may not always be transferrable to specific contexts such as energy conservation, and are in line with recent empirical findings [15].

Interestingly, combining financial incentives with other behavioral interventions seems to neither increase nor decrease the effectiveness of financial incentives on overall and peak consumption, and this finding was consistent for all of the behavioral interventions tested in the meta-

Table 5
Meta-regression results for studies targeting peak savings.

	Model				
	(1)	(2)	(3)	(4)	(5)
	Type	Direction	Technology	Combinations	Full model
Pricing: critical peak rebate (reference: critical peak pricing)	−0.004	−0.009	−0.007	0.001	0.004
	(0.017)	(0.048)	(0.018)	(0.016)	(0.045)
Pricing: real-time pricing	0.077**	0.079*	0.113***	0.092*	0.106**
	(0.027)	(0.036)	(0.020)	(0.037)	(0.032)
Pricing: TOU pricing	0.107***	0.107***	0.084***	0.111***	0.088***
	(0.013)	(0.014)	(0.013)	(0.017)	(0.014)
Incentive direction: positive (reference: negative)		0.007			−0.010
		(0.050)			(0.049)
Information technology			−0.033*		−0.014
			(0.012)		(0.016)
Automation technology			−0.084***		−0.082***
			(0.015)		(0.016)
Individual consumption feedback				−0.015	−0.020
				(0.042)	(0.038)
Monetary feedback				−0.049	−0.026
				(0.033)	(0.018)
Comparison feedback				−0.049	−0.051
				(0.079)	(0.077)
Education				0.034	0.036
				(0.045)	(0.048)
Environmental messaging				0.084	0.117*
				(0.044)	(0.045)
Control group	0.013	0.012	0.024	0.028	0.027
	(0.015)	(0.015)	(0.015)	(0.022)	(0.017)
Random assignment	−0.085***	−0.086**	−0.071**	−0.079***	−0.071*
	(0.022)	(0.025)	(0.021)	(0.021)	(0.027)
Baseline	−0.040	−0.039	−0.042	−0.031	−0.023
	(0.025)	(0.026)	(0.024)	(0.036)	(0.031)
Weather controls	−0.027	−0.026	−0.004	−0.031	−0.015
	(0.016)	(0.020)	(0.013)	(0.019)	(0.022)
Demographic controls	0.066*	0.068*	0.059*	0.067*	0.073*
	(0.027)	(0.031)	(0.024)	(0.027)	(0.032)
Mandatory participation	0.109***	0.108***	0.088**	0.108***	0.090**
	(0.025)	(0.026)	(0.025)	(0.028)	(0.028)
Incentive duration (mo.)	0.00004	0.0001	−0.0002	−0.0002	−0.0005
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Season: winter (reference: summer)	0.007	0.009	0.004	−0.002	0.015
	(0.031)	(0.034)	(0.034)	(0.030)	(0.037)
Season: year	0.001	−0.0003	−0.004	−0.007	−0.005
	(0.029)	(0.032)	(0.028)	(0.027)	(0.029)
Publication year	0.004**	0.004**	0.004**	0.005**	0.005**
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Location: Europe (reference: Asia/Australia)	−0.090*	−0.091*	−0.045	−0.057	−0.038
	(0.036)	(0.038)	(0.034)	(0.056)	(0.059)
Location: North America	−0.033	−0.035	−0.014	−0.010	−0.003
	(0.025)	(0.034)	(0.022)	(0.030)	(0.038)
Constant	−0.076*	−0.076*	−0.093**	−0.110	−0.117*
	(0.035)	(0.035)	(0.032)	(0.058)	(0.055)
R^2	.799	.799	.883	.848	.909
Adjusted R^2	.717	.709	.827	.752	.837
Number of observations	53	53	53	53	53
Number of clusters	28	28	28	28	28

Note: *$p < .05$; **$p < .01$; ***$p < .001$; standard errors are displayed in parentheses and are clustered by the primary independent study.

regression models. Previous meta-analytic findings on this issue have been mixed, but were mostly limited to a small number of observations (e.g., [16,19,22]). Providing robust evidence for specific types of combined interventions might generally be challenging due to a large array of different behavioral interventions that exist. Additionally, studies might not always report in detail how financial incentives were implemented (and whether other types of interventions were used), and it is thus possible that meta-analyses are limited in accurately capturing what particular combinations were used. Notably, we did find that financial incentives can achieve greater reductions in peak consumption (but not overall consumption) when combined with enhancing technologies. Specifically, the use of information technology (such as in-home displays) led to an additional small decrease in peak consumption, although this effect was not significant in the full model and the use of automation technology decreased peak consumption by more than 8% additionally. The enhancing effect of automation is considerable and speaks to recent research on how automation can best complement financial incentives to achieve greater conservation effects [47, 64].

5.2. Policy implications

The insights derived from this meta-analysis have important implications for policy makers and practitioners. The results show that financial incentives have the potential to promote electricity conservation, but effects on overall consumption are small in size and may only have a limited potential in mitigating global carbon emissions when scaled up (see also [2,19]). The small size of the achieved effects could be explained by research highlighting that changes in mostly habitual energy consumption behaviors face a number of barriers and require a significant amount of effort from consumers (e.g., [65]). Thus, carbon emissions might be mitigated more effectively if financial incentives directly target energy efficiency improvements or the uptake of sustainable technologies [2,55], or use other behavioral interventions than financial incentives [11,22], but our insights cannot speak directly to the effectiveness of such alternative approaches. However, we do find that financial incentives have a substantially larger potential in reducing peak consumption, particularly in the case of infrequently occurring critical peak periods. Although this may not directly result in overall reductions of electricity consumption, it might nonetheless be an important part of the sustainable energy transition and help mitigate global carbon emissions. Specifically, the uptake of decentralized and renewable energy generation entails greater fluctuations in energy generation depending on local weather patterns, while an increase in electrification (e.g., heat pumps, electric vehicles) leads to more fluctuating demand at the same time [6]. Balancing the available supply from renewables with the current demand via demand response strategies can avoid the activation of additional generation that is often based on fossil fuels and thus lead to an overall more efficient and less carbon-intensive energy system [27]. Some generation capacity is only activated during a few hours per year [5] and the need for such backup capacity could likely be mitigated by using financial incentives such as critical peak pricing or rebates. Moreover, our analysis shows that the effects of such financial incentives could be further enhanced with the use of appropriate technologies, in particular those that automate responses to incentives. This might open up fruitful synergies for the mutual promotion of financial incentives (e.g., dynamic pricing) and technologies such as in-home displays or smart appliances that automate temporary consumption reductions for heat pumps or electric vehicles. Integrating the increasing number of electric vehicles into (automated) dynamic pricing approaches might foster user engagement overall and thus facilitate a sustainable yet efficient grid (e.g., [66]).

5.3. Limitations and future research

Our analysis included relevant study-level characteristics that could potentially bias the effect an incentive has on changes in electricity consumption, such as whether participation was voluntary or mandatory, or how long an incentive was in place. However, some characteristics could only be captured at an abstract level: for example, we considered if socio-demographic characteristics of the participating households were used in the estimation, but this does not take into account any within-study variations in the socio-demographic characteristics. Yet, other study-level characteristics were not consistently reported in the primary studies. For example, only a fraction of the included study clearly reported if a study was conducted by a team of researchers or a utility. This aspect may influence the results as households may react differently to these sources (e.g., [67]). Moreover, we controlled for broad geographical regions in which studies were conducted (e.g., North America or Europe) but do not have any deeper insights into possible variations due to cultural and regional differences. The meta-regression results suggest that effects did not consistently differ between the broad regions, but it is possible that a more fine-grained analysis of cultural differences could find variations in energy behaviors, appliance stock, or sensitivity to information and incentives that lead to differential outcomes of financial incentive interventions. Thus, while we included studies from different countries to achieve more robust effect size estimates, future research should investigate between-country variations more systematically.

Despite efforts to take into account the size of a given incentive, it was impossible to include this aspect into the meta-regression model due to the large heterogeneity in incentive types, study design, and reporting. Actual financial rewards were often designed as monetary units (of varying currencies) per electricity saved (either in kWh or percentages relative to a certain baseline), while pricing incentives were often expressed as a ratio between peak and off-peak times, and monetary information incentives did not have any particular size at all. A further complication of pricing incentives was that studies sometimes tested a number of different price ratios. However, to avoid an inflation of (non-independent) observations within the same study, we only included weighted averages in this meta-analysis. Hence, the question of whether financial incentive effects on consumption increase with the size of the incentives is an important avenue for future research.

A further methodological limitation is that the effect sizes of the primary studies were not all calculated in the same way. Specifically, percentage changes in consumption naturally require a reference point against which consumption levels during the treatment period are compared. This reference point as well as the statistical calculation of the percent change differed between the included studies, which was noted by other meta-analyses, too [18]. For example, some studies used a simple difference between a treatment period and a baseline period to determine percentage changes while others compared differences between a treatment and a control group, or a combination of both. Some studies used more advanced statistical analyses such as ANOVA with log-transformed consumption measures or econometric models that additionally control for socio-demographic or weather characteristics. Our meta-regression results show that study-level characteristics can influence the estimated changes in consumption and future studies should thus account for such variations whenever possible and clearly report how percentage estimates were calculated.

6. Conclusion

This study provided a meta-analytic evaluation of financial incentive effects on household electricity conservation. We extend previous studies on this topic by distinguishing between two types of conservation, namely overall conservation and peak conservation. Our analysis shows that this is an important approach, as incentives targeting peak consumption lead to significantly greater conservation effects compared to incentives targeting overall consumption (including peak and off-peak times). Moreover, incentive effects are largely heterogeneous and this variation can be best explained by differences in incentive types. Notably, pricing incentives that only target a limited number of critical peak times have the greatest potential to reduce consumption during these specified peak times. Other types of pricing incentives, such as time-of-use pricing, have a significantly smaller conservation potential. Interestingly, combining financial incentives with other behavioral interventions does not seem to change their effectiveness on overall or peak electricity consumption. However, incentives targeting peak consumption can achieve greater consumption reductions if they are combined with enhancing technologies like in-home displays and particularly automation technology. These insights suggests that financial incentives can best target specified times of high demand to avoid the activation of carbon intensive backup generation.

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Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

Appendix

Appendix A

Table A1
List of articles included in the meta-analysis

Author	Year	Journal
Aigner & Lillard	1984	Journal of Business & Economic Statistics
Alahmad et al.	2012	IEEE Transactions on Industrial Electronics
Allcott	2011	Resource and Energy Economics
Andor et al.	2022	European Economic Review
Asensio & Delmas	2015	PNAS
Asmare et al.	2021	Energy Economics
Atkinson	1981	Resources and Energy
Aydin et al.	2018	Energy Economics
Bartusch & Alvehag	2014	Applied Energy
Bartusch et al.	2011	Energy Policy
Battalio et al.	1979	The Review of Economics and Statistics
Bekker et al.	2010	Journal of Applied Behavior Analysis
Bittle et al.	1979	Behavior Modification
Bittle et al.	1980	J. Environmental Systems
Bradley et al.	2016	Energy Policy
Brandon & Lewis	1999	Journal of Environmental Psychology
Carroll et al.	2014	Energy Economics
Chen et al.	2017	Energy Economics
Chrysopoulos et al.	2016	Electric Power Systems Research
Faruqui & George	2005	The Electricity Journal
Faruqui & Sergici	2011	Journal of Regulatory Economics
Faruqui et al.	2015	Energy Economics
Faruqui et al.	2014	The Energy Journal
Hayes & Cone	1977	Journal of Applied Behavior Analysis
Hayes & Cone	1981	Journal of Applied Behavior Analysis
Houde et al.	2013	The Energy Journal
Hutton et al.	1986	Journal of Consumer Research
Ito et al.	2018	American Economic Journal: Economic Policy
Jessoe et al.	2014	Journal of Economic Behavior & Organization
Kato et al.	2016	The Electricity Journal
Katzev & Johnson	1984	Journal of Applied Social Psychology
Kiljander et al.	2019	IEEEAccess
Kim & Kaemingk	2021	Journal of Economic Behavior and Organization
Kotchen & Moore	2008	Environmental Resource Economics
Lifson & Miedema	1981	Energy
Liu et al.	2021	Resources, Conservation & Recycling
Lynham et al.	2016	Energy Economics
Matsukawa et al.	2000	The Energy Journal
McClelland & Belsten	1979	Journal of Environmental Systems
Midden et al.	1983	Journal of Economic Psychology
Mizobuchi & Takeuchi	2013	Energy Policy
Mizobuchi & Takeuchi	2012	International Journal of Energy Economics and Policy
Mizutani et al.	2018	The Electricity Journal
Mukai et al.	2016	Energy Efficiency
Nahiduzzaman et al.	2018	Journal of Cleaner Production
Nguyen et al.	2016	Energies
Nielsen	1993	Energy Policy
Nilsson et al.	2018	Energy Policy
Nilsson et al.	2017	Resources, Conservation and Recycling
Pratt & Erickson	2020	Energy Research & Social Science
Schleich et al.	2013	Energy Policy
Schultz et al.	2015	Energy
Slavin et al.	1981	Journal of Applied Behavioral Analysis
Sudarshan	2017	Journal of Economic Behavior & Organization
Thorsnes et al.	2012	Energy Policy
Torriti	2012	Energy
Ueno et al.	2006	Applied Energy
Wang et al.	2021	Technological Forecasting & Social Change
Wenders & Taylor	1976	The Bell Journal of Economics
Winett & Nietzel	1975	American Journal of Community Psychology
Winett et al.	1978	Journal of Applied Psychology
Wolak	2011	American Economic Review: Papers & Proceedings
Woo et al.	2013	Applied Energy
Woo et al.	2017	The Energy Journal
Zarnikau et al.	2015	The Electricity Journal
Zhang et al.	2016	Procedia - Social and Behavioral Sciences

Appendix B

Table B1
Bivariate correlations

Variable	*M*	*SD*	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1. Average treatment effect	−0.08	0.10															
2. Peak savings program (vs. overall savings)	0.55	0.50	−.39**														
3. Information technology	0.35	0.48	.07	.14													
4. Automation	0.07	0.26	−.44**	.25**	.01												
5. Individual feedback	0.40	0.49	.26**	−.27**	.29**	−.23*											
6. Monetary feedback	0.25	0.44	−.09	−.10	.35**	.08	−.05										
7. Education	0.30	0.46	.18	−.36**	−.02	−.18	.32**	.08									
8. Comparison feedback	0.14	0.35	.16	−.14	.07	−.11	.35**	−.12	.35**								
9. Environmental messaging	0.07	0.26	.08	−.10	−.06	−.08	.06	−.16	.12	−.11							
10. Control group	0.83	0.38	.04	.02	.28**	.13	.07	.10	.14	.19	−.24*						
11. Random assignment	0.68	0.47	−.02	−.07	.13	.04	.15	.08	.23*	.28**	−.11	.67**					
12. Weather controls	0.47	0.50	−.35**	.34**	−.01	.30**	−.21*	−.05	−.18	−.08	.02	.09	−.02				
13. Demographic controls	0.23	0.42	.17	−.12	−.04	.02	.09	.08	.17	.27**	−.07	.13	.27**	−.03			
14. Baseline	0.81	0.39	−.24*	−.07	−.17	.13	−.22*	−.09	.01	.20*	−.04	−.10	.02	.22*	.15		
15. Mandatory recruitment	0.15	0.36	.04	−.22*	−.26**	−.12	.06	−.07	.11	.11	.17	−.21*	−.09	.05	−.11	.21*	
16. Incentive duration (months)	8.44	11.83	−.06	.13	−.15	−.05	−.09	.02	−.07	−.08	.12	−.37**	−.17	−.26**	.11	−.19*	−.17

Appendix C

Meta-regression results with influential cases excluded.

Table C1
Meta-regression of treatment effects on incentive types and study characteristics after removal of influential cases

	Model		
	(1)	(2)	(3)
	Study target	Type	Full model
Target: peak consumption	−0.069***	−0.052**	−0.061**
	(0.017)	(0.019)	(0.019)
Type: incentive (reference: financial info)		−0.034	−0.020
		(0.030)	(0.026)
Incentive direction: positive (reference: negative)			−0.024
			(0.017)
Control group	−0.012	−0.019	−0.018
	(0.037)	(0.036)	(0.036)
Random assignment	−0.041	−0.045	−0.042
	(0.038)	(0.039)	(0.034)
Baseline	−0.043	−0.045	−0.039
	(0.023)	(0.023)	(0.022)
Weather controls	−0.018	−0.011	−0.019
	(0.018)	(0.019)	(0.018)
Demographic controls	0.029	0.023	0.010
	(0.020)	(0.021)	(0.022)
Mandatory participation	0.023	0.030	0.025
	(0.027)	(0.028)	(0.023)
Incentive duration (mo.)	−0.002***	−0.002**	−0.002***
	(0.001)	(0.001)	(0.001)
Incentive frequency	−0.003	−0.004	−0.005
	(0.004)	(0.004)	(0.003)

(*continued on next page*)

Table C1 (*continued*)

	Model		
	(1)	(2)	(3)
	Study target	Type	Full model
Publication year	0.056	0.062	0.058
	(0.041)	(0.045)	(0.038)
Season: spring/fall (reference: summer)	−0.006	0.0001	0.008
	(0.033)	(0.035)	(0.030)
Season: winter	0.059	0.059	0.070*
	(0.043)	(0.042)	(0.029)
Season: year	0.001*	0.001	0.001*
	(0.001)	(0.001)	(0.001)
Location: Europe (reference: Asia/Australia)	−0.028	−0.048	−0.045
	(0.036)	(0.045)	(0.035)
Location: North America	−0.036	−0.046	−0.032
	(0.044)	(0.046)	(0.037)
Constant	0.033	0.064	0.051
	(0.028)	(0.042)	(0.038)
R^2	.547	.555	.578
Adjusted R^2	.472	.475	.495
Number of observations	106	106	106
Number of clusters	67	67	67

Note: *$p < .05$; **$p < .01$; ***$p < .001$; standard errors are displayed in parentheses and are clustered by the primary independent study; cases were identified as influential and removed if their Cook's distance was greater than three times the mean value.

Table C2
Meta-regression results for studies targeting overall savings with influential cases removed

	Model				
	(1)	(2)	(3)	(4)	(5)
	Type	Direction	Technology	Combinations	Full model
Type: Incentive (reference: financial info)	0.015	0.028	0.012	0.011	−0.042
	(0.041)	(0.047)	(0.039)	(0.052)	(0.048)
Incentive direction: positive (reference: negative)		−0.025			0.009
		(0.030)			(0.055)
Information technology			0.057		0.170**
			(0.028)		(0.052)
Individual consumption feedback				−0.008	−0.037
				(0.030)	(0.055)
Monetary feedback				−0.010	−0.014
				(0.023)	(0.024)
Comparative feedback				−0.005	−0.020
				(0.039)	(0.036)
Education				0.026	0.004
				(0.025)	(0.026)
Environmental messaging				−0.090	−0.027
				(0.049)	(0.091)
Control group	0.107	0.100	0.090	0.075	0.061
	(0.070)	(0.068)	(0.064)	(0.119)	(0.115)
Random assignment	−0.075	−0.069	−0.072	−0.087	−0.034
	(0.056)	(0.056)	(0.044)	(0.073)	(0.078)
Baseline	−0.035	−0.039	−0.056	−0.039	−0.109*
	(0.033)	(0.034)	(0.029)	(0.037)	(0.038)
Weather controls	−0.027	−0.024	−0.020	0.022	−0.050
	(0.030)	(0.031)	(0.027)	(0.042)	(0.064)
Demographic controls	0.049**	0.034	0.021	0.027	0.057
	(0.015)	(0.025)	(0.016)	(0.035)	(0.035)
Mandatory participation	−0.012	−0.019	−0.031	−0.018	0.010
	(0.028)	(0.029)	(0.030)	(0.040)	(0.044)
Incentive duration (mo.)	0.022**	0.026**	0.021***	0.021	0.020
	(0.007)	(0.009)	(0.006)	(0.018)	(0.017)
Incentive frequency	−0.008*	−0.009*	−0.019***	−0.008	−0.040**
	(0.003)	(0.004)	(0.005)	(0.006)	(0.012)
Season: spring/fall (reference: summer)	−0.021	−0.017	−0.023	0.002	−0.122
	(0.031)	(0.031)	(0.033)	(0.053)	(0.066)
Season: winter	−0.018	−0.019	−0.030	−0.037	−0.117*
	(0.027)	(0.028)	(0.031)	(0.034)	(0.044)
Season: year	−0.267**	−0.299*	−0.239**	−0.225	−0.384
	(0.095)	(0.112)	(0.073)	(0.205)	(0.221)
Publication year	0.002**	0.002*	0.002	0.003*	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Location: Europe (reference: Asia/Australia)	0.036	0.022	−0.009	0.002	0.002
	(0.047)	(0.043)	(0.040)	(0.055)	(0.051)

(*continued on next page*)

Table C2 (*continued*)

	Model				
	(1)	(2)	(3)	(4)	(5)
	Type	Direction	Technology	Combinations	Full model
Location: North America	0.117*	0.109*	0.057	0.106	−0.0003
	(0.055)	(0.051)	(0.050)	(0.063)	(0.077)
Constant	0.098	0.157	0.216*	0.150	0.463**
	(0.097)	(0.100)	(0.100)	(0.125)	(0.152)
R^2	.649	.658	.673	.652	.824
Adjusted R^2	.447	.440	.471	.304	.608
Number of observations	42	42	43	41	41
Number of clusters	34	34	34	33	33

Note: *$p < .05$; **$p < .01$; ***$p < .001$; standard errors are displayed in parentheses and are clustered by the primary independent study; cases were identified as influential and removed if their Cook's distance was greater than three times the mean value.

Table C3
Meta-regression results for studies targeting peak savings after removal of influential cases

	Model				
	(1)	(2)	(3)	(4)	(5)
	Type	Direction	Technology	Combinations	Full model
Pricing: critical peak rebate (reference: critical peak pricing)	−0.010	−0.022	−0.015	0.002	−0.080*
	(0.026)	(0.048)	(0.027)	(0.019)	(0.035)
Pricing: real-time pricing	0.091**	0.097*	0.106***	0.144*	0.179***
	(0.026)	(0.038)	(0.020)	(0.062)	(0.036)
Pricing: TOU pricing	0.113***	0.114***	0.081***	0.107***	0.090***
	(0.013)	(0.014)	(0.013)	(0.019)	(0.012)
Incentive direction: positive (reference: negative)		0.014			0.086
		(0.047)			(0.043)
Information technology			−0.027**		−0.032
			(0.009)		(0.023)
Automation technology			−0.082***		−0.081***
			(0.013)		(0.015)
Individual consumption feedback				0.078	0.111**
				(0.056)	(0.031)
Monetary feedback				−0.007	0.032
				(0.034)	(0.037)
Comparison feedback				−0.046	0.008
				(0.044)	(0.025)
Education				−0.011	0.094
				(0.094)	(0.091)
Environmental messaging				0.013	−0.030
				(0.052)	(0.051)
Control group	0.074	0.074	0.050	0.048	0.023
	(0.058)	(0.058)	(0.055)	(0.063)	(0.050)
Random assignment	−0.090***	−0.092***	−0.074***	−0.089***	−0.108***
	(0.022)	(0.023)	(0.020)	(0.024)	(0.023)
Baseline	−0.026	−0.024	−0.038	−0.035	−0.013
	(0.021)	(0.021)	(0.025)	(0.032)	(0.030)
Weather controls	0.0003	0.001	0.019	−0.003	0.037
	(0.023)	(0.023)	(0.022)	(0.025)	(0.022)
Demographic controls	0.033	0.037	0.032	0.051	0.056
	(0.031)	(0.041)	(0.031)	(0.035)	(0.034)
Mandatory participation	0.106***	0.106***	0.065*	0.094	0.094*
	(0.026)	(0.027)	(0.029)	(0.055)	(0.041)
Incentive duration (mo.)	0.004	0.004	−0.0002	0.002	0.005
	(0.002)	(0.002)	(0.001)	(0.004)	(0.003)
Season: winter (reference: summer)	0.021	0.024	0.012	0.007	0.060
	(0.028)	(0.028)	(0.036)	(0.035)	(0.030)
Season: year	0.011	0.004	0.037	0.009	−0.038
	(0.052)	(0.066)	(0.053)	(0.067)	(0.052)
Publication year	0.005*	0.005*	0.005*	0.005	0.003*
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Location: Europe (reference: Asia/Australia)	−0.076*	−0.079*	−0.032	−0.072	−0.157*
	(0.031)	(0.034)	(0.031)	(0.062)	(0.062)
Location: North America	−0.039	−0.043	−0.008	−0.024	−0.091*
	(0.033)	(0.040)	(0.029)	(0.046)	(0.038)
Constant	−0.163	−0.161	−0.157	−0.136	−0.057
	(0.081)	(0.084)	(0.080)	(0.101)	(0.083)
R^2	.791	.792	.873	.834	.933
Adjusted R^2	.700	.687	.806	.710	.866
Number of observations	49	49	50	48	47
Number of clusters	25	25	25	24	23

Note: *$p < .05$; **$p < .01$; ***$p < .001$; standard errors are displayed in parentheses and are clustered by the primary independent study; cases were identified as influential and removed if their Cook's distance was greater than three times the mean value.

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