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ABSTRACT

We formalize a research design to uncover the mechanisms underlying long-term reductions in energy consumption caused by a widely implemented nudge. We consider two channels: technology adoption and habit formation. Using data from 38 natural field experiments, we isolate the role of technology adoption by comparing treatment and control homes after the initial resident moves, which discontinues the treatment for a home. We find that the majority of energy reductions persist in the home after treatment ends and show this persistence is consonant with a technology adoption channel. The role of technology in creating persistent behavior change has important implications for designing behavioral interventions and evaluating their long-term social impacts.

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1. Introduction

A growing literature has established that nudges (Thaler and Sunstein, 2008) are a highly cost-effective approach to changing an array of behaviors in the short-term (Allcott and Mullainathan, 2010; Benartzi et al., 2017; Hummel and Maedche, 2019; DellaVigna and Linos, 2022). Less, however, is known about the long-term effectiveness of nudges. In many of the contexts in which nudges are applied, such as education, health or the environment, success requires persistent behavior change.

We study the mechanisms underlying persistent energy reductions produced by one of the most widely studied nudges: the Home Energy Report (HER). The HER provides a social comparison that contrasts the recipient's energy consumption to the energy consumption of their neighbors. The HER has been evaluated in dozens of randomized trials conducted by residential energy providers across the United States (U.S.).

Studies of randomized trials find the HER is highly cost-effective. Although energy consumption is notoriously price inelastic, Allcott (2011); Ayres et al. (2013); Costa and Kahn (2013); Allcott (2015) report that average energy consumption declined by one to two percent among households who received HERs over a period of a year. The evidence for HER effectiveness has led energy providers in the U.S. to widely adopt the HER and policymakers to herald the HER as an important tool to fight against climate change IEA (2021). As a further testament to the success the HER, the company that developed it, Opower, was acquired by Oracle for more than $500 million.

Follow-up studies report that that the HER effect on energy consumption persists beyond a single year. After five years of exposure to HERs, a difference in energy use between households in the treatment group (HER recipients) and the control group (untouched) can still be detected (Bell et al., 2020, and the citations therein). Furthermore, the majority of the short-run effect persists two years after HERs are discontinued (Allcott and Rogers, 2014).

The persistence of the HER effect stands in marked contrast to the persistence of the effects of analogous social comparison nudges in other contexts (Figure A1). In the short term, these nudges increase charitable giving, financial savings, tax and other types of compliance, water conservation, and voter turnout. However, only the effects on water conservation persist after the nudges are discontinued (Shang and Croson, 2009; Apesteguia et al., 2013; Ferraro and Miranda, 2013; Bernedo et al., 2014; Hallsworth et al., 2017; Coppock and Green, 2016; Rogers et al., 2017; Kast et al., 2018).

The challenge of designing nudges that produce persistent effects can be seen in a recent meta-analysis. DellaVigna and Linos (2022) find that the estimated effect of the nudge and the time horizon over which a nudge is evaluated are negatively correlated. After controlling for a variety of observable features, they find that each additional day over which a nudge is evaluated correlates with a 0.7 percent reduction in the average effect of the nudge. While this estimated effect is statistically imprecise (standard error = 0.4), it suggests that the average short-term effect of nudges would disappear after an additional year or two.

Academics and policymakers who wish to induce persistent behavioral change would thus benefit from understanding the mechanisms that underlie the persistent effects of HERs on energy consumption. Yet the evidence about the channels through which HERs affect long-run patterns of energy consumption is limited. Across two evaluations, Allcott and Rogers (2014) found that no more than 2 percent of the HER's long-term effectiveness can be explained by participation in certain energy efficiency programs. Under the assumption that adopters of energy efficient technologies would use these programs to facilitate adoption, the finding suggests that technology adoption is unlikely to be the channel driving the persistence of the HER effect.

Likewise, evaluations of an HER-like intervention for water conservation also fail to provide any evidence of technology adoption driving the persistence of the effect. Ferraro and Miranda (2013) and Bernedo et al. (2014) report that that the estimated effect is no longer statistically significant in the subgroup of homes in which the initial treated resident had moved. They conclude that a change in habits is the most plausible channel for the persistence of the intervention's effect.

These analyses suggest that the long-term effectiveness of the HER reflects changes to something in the people residing in a home, such as their habits or skills, as opposed to something in the home, such as more efficient technologies. However, these results are only suggestive. The research designs are informal, and the identifying assumptions are not clearly defined or tested. Moreover, in the analyses of movers in Ferraro and Miranda (2013) and Bernedo et al. (2014), the samples are small and thus potentially underpowered.

We formalize a research design that decomposes the long-run effect of the HER into components attributable to technology adoption and habit formation. This decomposition is accomplished by exploiting a feature of how HERs were administered in the experiments. If the initial resident in an HER experiment moved to a new home, then the HER was immediately discontinued. Moving, however, did not discontinue observations of energy consumption in the home. We show that, under certain conditions, the post-move HER effect identifies the fraction of the treatment effect attributable to technology adoption. The fraction attributable to habit formation is then the fraction of the HER's long-term impact that is not explained by technology adoption.

Our decomposition of the HER's long-term effectiveness depends on the validity of three assumptions. First, treatment assignment did not influence residents' decisions to move from a home in the experimental sample. Second, treatment assignment did not influence the types of residents that moved *into* a home in the experimental sample. Third, the technology adopted in response to the HER remained in a home after the initial resident moved. While these assumptions seem plausible for an information-based, "light touch" intervention like the HER, we nonetheless derive testable predictions of their validity and find no evidence that they are violated in our experiments.

Using data on nearly 140,000 movers observed across 38 HER experiments, we apply our research design and decompose the long-term effectiveness of the HER. We find that, over the long-term, movers respond to receipt of the HER by reducing their energy consumption by 2.1 percent. Moreover, we find that fifty one percent of this reduction remains in the home after a move, and we show this result is robust to a battery of alternative specifications. Under our decomposition assumptions, these results imply that technology adoption, as opposed to habit formation, was the primary channel responsible for the long-term energy reductions produced by the HER.

Our study makes three contributions. First, it provides a simple explanation for the variation in the persistence of social comparison effects in the literature: variation in the availability of technologies across contexts. In the contexts of energy and water conservation, households can respond to the nudge by adopting long-lived technologies that have long-term impacts by reducing the marginal cost of conservation. Such technologies, however, are scarce for households that wish to donate more to charitable organizations, evade their taxes, contribute to their financial savings, and vote in an election. The contrast between the rapid fade-out of effects produced by nudges that target these behaviors and the persistence of effects produced by nudges that target energy and water conservation can thus be explained by the variation in availability of enabling technologies.

Second, the identification of technology adoption as a critical channel for persistent behavioral change provides policymakers with an insight that can be leveraged to induce more persistent effects from nudges (or avoid such persistence when the goal is only temporary behavior change). Policymakers can target nudges towards behaviors that can be changed with the adoption of technologies. For example, we conjecture that the effect of voter turnout efforts will persist longer when a municipality allows households to default into easier modes of voting in future elections, such as mail-in or on-line ballots. When such technologies do not already exist, policymakers can encourage the development of new technologies that can be paired with nudges. For example, a social nudge promoting charitable giving or financial savings could be combined with an option to set a default donation or contribution rate in the future (Madrian and Shea, 2001; Thaler and Benartzi, 2004; Goswami and Urminsky, 2016; Altmann et al., 2019).

Third, our study illustrates an application of a new approach to decompose the mechanisms of policy effectiveness. Previous research advocates for experimental designs that directly test for a hypothesized mechanism (Ludwig et al., 2011) or econometric analyses that rely on the collection of data that proxy for hypothesized mechanisms (Heckman and Pinto, 2015). Our approach complements these recommended designs and analyses, particularly when there is uncertainty about whether changes in human or physical forms of capital are driving an intervention's effect and when the intervention is a relatively light touch, such as a nudge, and thus will satisfy the three identifying assumptions of our design.

Our study also contributes to several other strands of research. First, it contributes to the nascent literature on the determinants of persistent responses to policy interventions (Frey and Rogers, 2014; Rogers and Frey, 2016). Second, by presenting a cost-benefit analysis of the HER that incorporates the indirect cost of the technology adopted, our study contributes to the literature on identifying the full effect of policy interventions (Heckman and Smith, 1997). Third, our study also contributes to the literature on energy efficient technology adoption by highlighting that nudges like the HER can stimulate the take up of such technologies (Jaffe and Stavins, 1994; Allcott and Greenstone, 2012; Gerarden et al., 2017; Gillingham et al., 2018). Finally, our study contributes to the theoretical and empirical literature on habit formation (Pollak, 1970; Becker and Murphy, 1988; Becker, 1992; Charness and Gneezy, 2009; John et al., 2011; Acland and Levy, 2015; Royer et al., 2105; Fujiwara et al., 2016; Levitt et al., 2016; Beshears et al., 2021; Vollaard and van Soest, 2021; Allcott et al., 2020; Bursztyn et al., 2021; Allcott et al., 2022). We contribute to the literature on habit formation by developing an approach to decompose the relative importance of changes in human factors, such as habits, and changes in non-human factors, such as technologies, for the long-run effectiveness of a policy intervention.

The remainder of this study proceeds as follows. In Section 2 we formalize our research design. Section 3 describes the HER experiments and mover sample. We present our empirical findings and discuss their implications in Section 4. Section 5 concludes.

2. Identification Strategy

In this section, we formalize our strategy for decomposing the long-term effectiveness of the HER into components attributable to habits and technology.

2.1 Setting and Notation

Consider a subsample of homes in an HER experiment from which the initial resident will eventually move. During a baseline period, the electricity consumption of each home is observed. After this period, homes are randomly assigned to remain in the control state of the baseline period or enter a treated state, wherein the home receives an HER in the mail. Receipt of the HER continues for homes in the treated state until the initial resident moves, at which point the HER is discontinued.

More formally, let $i \in \{1, 2, \ldots, I\}$ index each home. Let $t \in \{-12, -11, \ldots, T\}$ index each unit of time over which a home's outcome of interest is observed and suppose this index is measured relative to the end of the baseline period (i.e., $t = 0$ is the time at which treatment is assigned). The outcome of interest in the experiments is electricity consumption, which we denote with $Y_{it} \in \mathbb{R}$. Let $D_{it} \in \{0, 1\}$ be a treatment indicator that denotes whether home i has entered the treated state at time t. During the baseline period, this treatment indicator equals 0 for every home. It then switches to 1 for the homes that receive the HER and stays at 1, regardless of whether the initial resident eventually moves. Let $M_{it} \in \{0, 1\}$ indicate whether the initial resident has moved out of home i at time t. It will also prove convenient to define $\tilde{M}_i \in \{1, 2, \ldots, T\}$ as the value of the time index at which the initial resident of home i moves. This variable is related to the move indicator according to $M_{it} = 1(t > \tilde{M}_i)$, where $1(\cdot)$ is the indicator function.

The relationship between the outcome of interest, Y_{it}, the treatment indicator, D_{it}, and the move indicator, M_{it}, can be described with potential outcomes notation. Let $Y_{it}(d, m)$ denote the potential outcome of electricity consumption in home i at time t if the treatment indicator is fixed at $d \in \{0, 1\}$ and the move indicator is fixed at $m \in \{0, 1\}$. The observed outcome is thus related to the observed treatment and move indicators according to the following expression,

$$\begin{aligned} Y_{it} = (1 - M_{it})(D_{it}Y_{it}(1,0) + (1 - D_{it})Yit(0,0)) + \\ M_{it}(D_{it}Y_{it}(1,1) + (1 - D_{it})Y_{it}(0,1)). \end{aligned} \tag{1}$$

2.2 Mechanisms

Our analysis considers two broad classes of mechanisms that could give rise to the long-term effectiveness of the HER. The first mechanism is a change in the stock of habits or skills in the resident of a home. Let $H_{it}(d,m)$ denote a measure of this stock in the resident of home i at time t when the treatment and move indicators are fixed at $d \in \{0,1\}$ and $m \in \{0,1\}$. The second mechanism is a change in the stock of energy efficient technology in the home. Let $K_{it}(d,m)$ denote a measure of this stock in the home i at time t when the treatment and move indicators are fixed at $d \in \{0,1\}$ and $m \in \{0,1\}$. For simplicity of notation, and without loss of generality, we assume both of these stock variables are measured in units of electricity consumption.

We assume a linear relationship between habits and technology in the production of the potential outcomes,

$$Y_{it}(d,m) = H_{it}(d,m) + K_{it}(d,m) + V_{it}, \tag{2}$$

where the variable V_{it} captures features that are relevant to electricity consumption but invariant to receipt of the HER and the decision to move, such as the weather. Some of these features may be observable, in which case we can express $V_{it} = \gamma X_{it} + U_{it}$, where X_{it} is a vector of observables and U_{it} is unobserved. The linear formulation in equation 2 is a plausible approximation of the true relationship given that the HER targets small changes in behavior that would be locally linear under a more general, all causes, model of the potential outcomes.

2.3 Parameters of Interest

The objective of our analysis is to decompose the long-term effectiveness of the HER into components that can be attributed to changes in habits and technology. Accordingly, we have three parameters of interest: The long-term average treatment effect, the long-term average treatment effect attributable to changes in habits, and the long-term average treatment effect attributable to changes in technology.

The first parameter describes the effectiveness of the HER after a home and its initial resident have been exposed to the HER for a long period of time. We refer to this parameter as the long-term average treatment effect, or ATE for short, and define it as,

$$ATE \equiv E[Y_{it}(1,0) - Y_{it}(0,0)|t > l^*], \tag{3}$$

where $E[\cdot]$ is the expectations operator and l^* is a threshold that denotes long-term exposure to the HER. We delay characterizing this threshold until Section 3.2, as the theory underlying our identification strategy only requires the existence of such a threshold.

The second and third parameters of interest respectively capture the extent to which the effectiveness of the HER was caused by a change in the stock of habits and skills in the residents (H_{it}) or a change in the stock of technologies in the home (K_{it}). The relationship between these parameters and the ATE is obtained by plugging equation 2 into the definition of the ATE,

$$\begin{aligned} ATE &= E[H_{it}(1,0) - H_{it}(0,0)|t > l^*] + E[K_{it}(1,0) - K_{it}(0,0)|t > l^*] \\ &= ATH + ATK, \end{aligned} \tag{4}$$

where the parameters $ATH \equiv E[H_{it}(1,0) - H_{it}(0,0)|t > l^*]$ and $ATK \equiv$

$E[K_{it}(1,0) - K_{it}(0,0)|t > l^*]$ respectively capture the effect of the HER on habits and technology.

2.4 Assumptions and Identification

The primary challenge in identifying our parameters of interest is that habits and technology are unobserved. This challenge can be overcome by using the post-move effect of the HER to point identify the effect of the HER on technology (ATK). Netting the ATK out of the pre-move effect allows for the point identification of the effect attributable to habits (ATH).

The validity of this approach depends on three assumptions, which we present below. After presenting the first two assumptions, we consider how data can be used to assess their validity. Later, in Section 2.6.2, we discuss how relaxing the third assumption, which is untestable, allows for the partial identification of the ATH and ATK.

The first assumption requires that treatment assignment did not influence residents' decisions to move from a home in the experimental sample. More formally, it requires that the potential outcomes are mean independent of the treatment indicator and the time at which the initial resident moves,

$$E[Y_{it}(d,m)|D_{it}, \tilde{M}_i, X_{it}] = E[Y_{it}(d,m)|X_{it}] \text{ for } d, m \in \{0,1\}, \tag{5}$$

where, henceforth, conditioning on the long-term is left implicit. The assumption in equation 5 has been implicitly invoked in every analysis of HER experiments. It holds if, conditional on the observables in the vector X_{it}, receipt of the HER was randomized and the decision to move homes was not made with reference to the HER. Furthermore, this assumption can be tested, with statistically significant differences in treatment and control baseline period electricity consumption and moving rates rejecting the assumption.

The second assumption requires that treatment assignment did not influence the types of residents that moved into a home in the experimental sample. In other words, after the initial resident moves, the habits of the subsequent resident are balanced across treated and controlled homes. Because the HER was immediately discontinued after the initial resident moved, this assumption restricts sorting behavior. Formally it imposes the following restriction,

$$E[H_{it}(1,1)|X_{it}] = E[H_{it}(0,1)|X_{it}]. \tag{6}$$

While the unobservable nature of habits makes testing this assumption impossible for the entire sample, there is a test for a specific subsample of homes. Many of the homes in HER experiments were rentals and rental arrangements typically constrain the adoption of energy efficient technologies (e.g., Davis, 2012). Under the assumption that the technology channel is shut down for rental units, the post-move effect in rental units reveals whether there is sorting into homes based on whether the prior resident received the HER. Rejecting the null hypothesis of zero treatment effect among rentals would be inconsistent the balanced habits assumption.

The third assumption requires that the effect of the HER on technology adoption remains, or is stable, after the initial resident moves. Formally this assumption implies that,

$$E[K_{it}(1,0) - K_{it}(0,0)|X_{it}] = E[K_{it}(1,1) - K_{it}(0,1)|X_{it}]. \tag{7}$$

Intuitively, this assumption requires that a move does not cause the technology adopted in response to the HER to exit the home, depreciate, or spread to control homes. The implications of this assumption are consistent with the HER overcoming persistent frictions in the adoption of long-lived energy efficient technology. Yet, we can also formulate a less restrictive version of the assumption that allows for partial identification of the parameters of interest.

We consider this less restrictive assumption in greater detail in Section 2.6.2.

Under these three assumptions, we can use the effect of the HER before and after the initial resident moves to point identify the ATE, ATH, and ATK. The ATE is identified with the pre-move effect of the HER,

$$\begin{aligned}\text{Pre-Move Effect} &\equiv E[Y_{it}|D_{it}=1, M_{it}=0] - E[Y_{it}|D_{it}=0, M_{it}=0] \\ &= ATE.\end{aligned}$$

The ATK is identified with the post-move effect of the HER,

$$\begin{aligned}\text{Post-Move Effect} &\equiv E[Y_{it}|D_{it}=1, M_{it}=1] - E[Y_{it}|D_{it}=0, M_{it}=1] \\ &= ATK.\end{aligned}$$

The ATH is inferred by netting out the effect attributable to technology (ATK) from the total effect (ATE). Next we describe our strategy for estimation and inference.

2.5 Estimation

We estimate our parameters of interest with the following linear model,

$$Y_{it} = \beta D_{it}(1 - M_{it}) + \delta D_{it} M_{it} + \gamma' X_{it} + U_{it}, \tag{8}$$

where D_{it} is the treatment indicator in the long-term (i.e., $t > l^*$), X_{it} is a vector of observables, and U_{it} is the unobservable. Linking our parameters of interest to the coefficients in equation 8 is straightforward. The pre-move effect of the HER is β, which corresponds to the ATE, and the post-move effect of the HER is δ, which corresponds to the ATK. ATH is then inferred with $\beta - \delta$. We conduct inference on these estimated parameters with standard errors robust to heteroskedasticity and autocorrelation.

2.6 Additional Comments

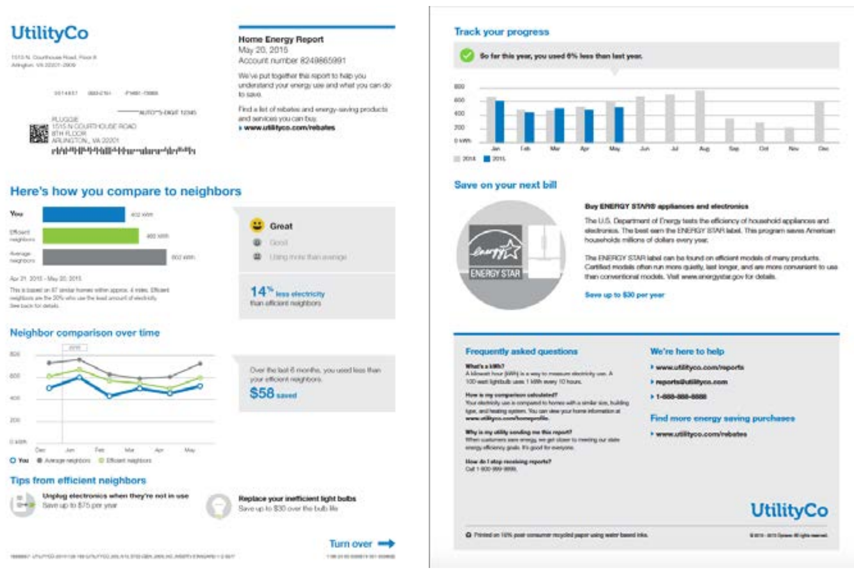
2.6.1 Timing of Moves

Over the course of an HER experiment, moves happen at different times and the timing of a move can influence the weight each home receives in the estimate of pre- and post-move HER effects (see, e.g., Goodman-Bacon, 2021). To evaluate whether our estimates are influenced by the timing of moves, we re-estimate the coefficients in equation 8 using subsamples of our data where homes are observed for the same amount of time before and after the initial resident moves.

2.6.2 Partial Identification

As described in Section 2.4, the validity of our identification strategy relies on three assumptions. Here, we describe how relaxing the third assumption, which is untestable, allows for the partial identification of the ATH and ATK. Recall that the third assumption implies that the effect of the HER on technology adoption remains in the home after the initial resident moves. Such an assumption would be violated if moving causes the technology adopted in response to the HER to exit the home, depreciate, or spread to control group homes. However, all three of these possibilities suggest $E[K_{it}(1,0) - K_{it}(0,0)|X_{it}] \geq E[K_{it}(1,1) - K_{it}(0,1)|X_{it}]$, which allows for the partial identification of ATH and ATK. In other words, the pre-move effect of the HER would still point identify the ATE, the post-move effect would identify the lower bound of ATK. The ATE net of ATK yields an upper bound on ATH.

Figure 1: Example of Home Energy Report (HER)



Front *Back*

Note: The figure presents the front and back of the Home Energy Report (HER). Before moving, treatment households receive HERs regularly (monthly, bi-monthly, or quarterly).

3. Background

In this section, we describe the administration of HER experiments and provide a statistical description of our mover sample.

3.1 Administration of Home Energy Report Experiments

Our analysis uses data from 38 natural field experiments administered by a company called Opower. These HER experiments were conducted between 2008 and 2013 with customers of 21 different residential energy providers across the United States. Figure 1 presents an example of an HER, which compared home and neighborhood electricity consumption, described conservation tips, and provided information on energy-efficient technology adoption.

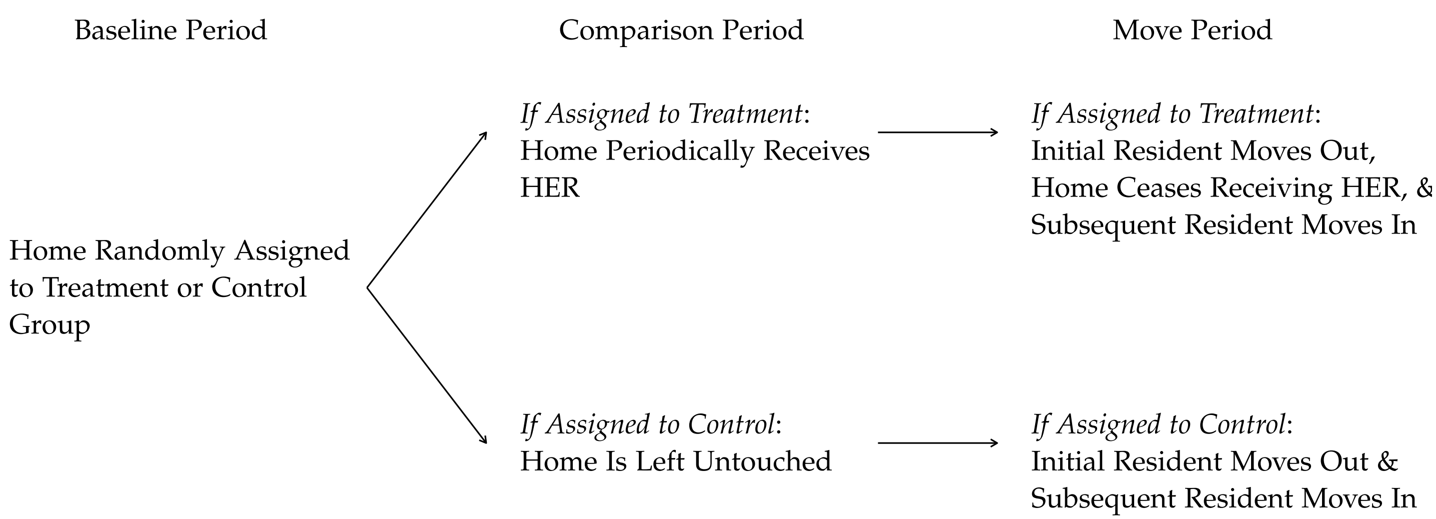
Each of the 38 HER experiments, or waves, used the same design, which is summarized in Figure 2. Homes were observed in the baseline period for twelve billing months and then randomly assigned to a treatment or control group. Homes then entered the comparison period, wherein Opower generated HERs for both groups, but only mailed the HER to treatment group households. Across the 38 waves, the HER was received monthly, bimonthly, or quarterly. We pool across this margin because prior research finds that frequency of receipt does not impact long-term effectiveness (Allcott and Rogers, 2014). Homes exited the comparison period and entered the move period when the initial resident deactivated their electricity service. Upon deactivation, generation of HERs ceased and the home was made ineligible for waves of HER experiments.

3.2 Description of Mover Sample

Our data were obtained via a data sharing agreement with Opower. These data allow us to observe: (i) the electricity bills of homes in each wave, (ii) treatment and control group assignment, (iii) the timeline of HER administration in each wave, (iv) the date on which a household deactivated their electricity service, and (v) household characteristics such as whether the home was a rental.

These data consist of 61,310,166 electricity bills for 1,810,096 homes. Each electricity bill includes the total consumption of electricity in kilowatt hours (kWh) and the length of the billing cycle. On average, an electricity bill covers 30 days, but this coverage varies. Our outcome measure adjusts for this variation by normalizing the electricity consumption by the length of the billing cycle, making average daily consumption over the course of a billing cycle our observed outcome.

Figure 2: Timeline of Homes in Mover Sample of HER Experiment



Note: This figure describes the three periods of an HER experiment for the mover sample. In the baseline period, homes are randomly assigned to a treatment or control group. In the comparison period, treatment group homes periodically receive the HER mailer and control group homes are left untouched. In the move period, the receipt of the HER is ceased for treatment group homes once the initial resident moves out and then the subsequent resident moves. Control group homes in the move period see the initial resident move out and then have the subsequent resident moves into the home.

Table 1: Summary Statistics of Mover Sample

	Prob. in Mover Sample (pp)	kWh/day in Baseline Period	Days in Comparison Period	Days in Move Period	Prob. Home is Rental (pp)
	(1)	(2)	(3)	(4)	(5)
Control	7.72	38.00	491.54	388.86	13.81
	(0.03)***	(0.07)***	(1.10)***	(1.11)***	(0.14)***
Treatment − Control	0.01	0.09	2.25	−2.42	−0.06
	(0.04)	(0.09)	(1.46)	(1.46)*	(0.18)
Sample	Full	Mover	Mover	Mover	Mover
Homes	1,810,096	139,908	139,908	139,908	139,908

Notes: This table summarizes the characteristics of the mover sample. The first row reports the average value of each characteristic for homes assigned to the control group and the second row reports treatment group differences from the control group. Estimates regression-adjust for each HER wave. The first column reports the rate at which the full sample enters the mover sample. The subsequent columns report characteristics of the mover sample. Standard errors robust to heteroskedasticity are reported in parentheses below each estimate. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

To study the effect of the HER that remains in the home after the initial resident moves, we construct a sample of movers from this data. This sample is comprised of homes that had a deactivation of the initial resident's account with their energy provider. Working with Opower, we eliminated homes where the deactivation was prompted by a name change or other changes unlikely to reflect a move by the initial resident.

We further restrict the mover sample to homes where deactivations occurred at or after the fourth HER had been received.[1] We base this restriction on results in Allcott and Rogers (2014), which indicate that the effect of the HER plateaus around the receipt of the fourth HER. On average, the fourth report was generated 145 days, or approximately five months, after the start of the comparison period. We denote this subsample the "mover sample", which includes 5,890,855 electricity bills for 139,908 homes.

Table 1 provides a statistical summary of the mover sample. This summary presents averages of different features of the sample after regression adjusting with a dummy for each wave of an HER experiment. The first column shows that the mover sample is comprised of approximately 8 percent of the treatment and control group homes from the full sample. Subsequent columns show that, on average, mover sample homes consume about 38 kWh/day in the baseline period and spend more than a year in the comparison and move periods. Nearly 14 percent of the mover sample are renters and nearly 14 percent use electricity to heat their home.

Table 1 also provides evidence that supports the first assumption of our identification strategy. Treatment and control group homes select into the mover sample at statistically indistinguishable rates and these homes consume similar quantities of electricity in the baseline period. Furthermore,

[1]This restriction can be applied to both treatment and control group homes, because, as noted above, Opower created HERs for both groups, but only sent out the mailers to treatment group homes.

the two groups spend similar amounts of time in the comparison and move periods.

4. Results

This section presents the estimates that decompose the long-term effectiveness of the HER and then considers the implications of these estimates for the broader literature on nudges.

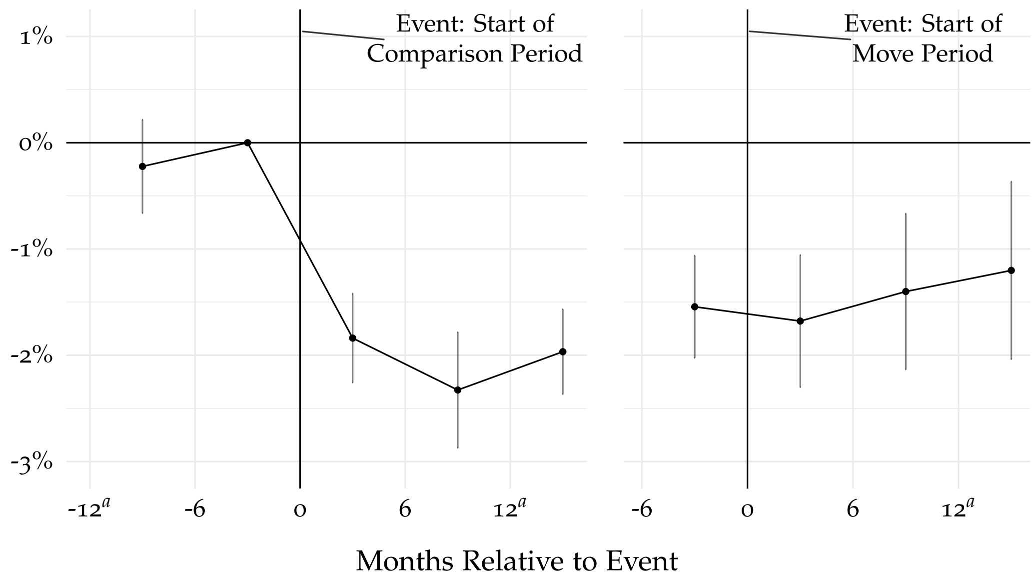
4.1 Estimates

Figure 3 illustrates how the average effect of the HER develops over the course of the experiments. Time is divided into six-month intervals in the baseline, comparison, and move periods. Each estimate is normalized by the level of control group electricity consumption in the baseline period (see column 2 in Table 1) and confidence intervals are constructed with standard errors robust to heteroskedasticity and autocorrelation.

Starting from the left of Figure 3 we see an average difference between treatment and control group homes of approximately -0.2 percent in the baseline period. Scaling this difference by 38 kWh/day converts it to an estimated effect of -0.08 kWh/day. Such an effect is small: Equivalent to treatment group homes using a 60-watt incandescent lightbulb for an extra hour each day. Moreover, the confidence interval on this estimate shows it cannot be statistically distinguished from an effect of zero. This balance in baseline period electricity consumption provides further support for the mean independence assumption discussed in Section 2.4.

Moving to the right of the first vertical line, which denotes the end of the baseline period and the start of the comparison period, the average effect falls significantly. The negative sign on these estimates indicates the HER

Figure 3: Event Study of HER Effect on Mover Sample



Note: This figure reports estimated treatment effects on the mover sample. Each estimated effect is the average effect of treatment assignment at a given point in time. Each effect is presented in terms of percent changes relative to control group electricity consumption in the baseline period. Time is divided into six-month intervals. Observations that fall outside of the plotted intervals are assigned to an absorbing interval indicated on the figure with the superscript *a*. The omitted time period is the first six months of the baseline period. Brackets denote the 95 percent confidence interval. Estimates are obtained by weighting by the duration of each electricity bill and are regression-adjusted with fixed effects for each window of time, home, and year-by-season-by-wave. 95 percent confidence intervals constructed with standard errors robust to heteroskedasticity and autocorrelation.

caused a reduction in household electricity consumption. Figure 3 respectively reports a -1.8 to -2.3 and -2.0 to -1.5 percent average effect in the first and second year of the comparison period, with 95 percent confidence intervals that do not overlap with zero. In levels, these effects are approximately -0.6 to -0.9 kWh/day. To put the magnitude of these estimates into perspective, such an effect is equivalent to treatment group homes using a 60-watt incandescent lightbulb for 10 to 15 fewer hours per day or replacing 2 to 4 60-watt incandescent lightbulbs that are used 5 hours per day with the CFL equivalent.

Moving beyond the second vertical line of Figure 3, we see that much of the average effect of the HER found in the comparison period persists in the move period. Over the first year of the move period the HER continues to produce reductions in electricity consumption of -1.7 and -1.4 percent. The final estimate of Figure 3 shows that more than a year after moving, the estimated average effect is a -1.2 percent reduction in average electricity consumption. The 95 percent confidence intervals on these estimates show that the null hypothesis of no effect during the move period is rejected at standard levels of statistical significance. In levels, these estimated effects equate to approximately -0.5 to -0.6 kWh/day.

We next present our decomposition of the HER's long-term effectiveness. Table 2 provides the estimates for several samples. The first column presents the estimated pre-move effect of the HER for the full sample of homes. The second and third respectively present the estimated pre- and post-move effects for the mover sample and the mover sample of rental homes.

The estimated effects in the first two columns of Table 2 indicate that the majority of the HER's long-term effectiveness can be attributed to increases in technology adoption, with the remainder attributable to changes in habits. To see how this conclusion is derived, recall that the pre-move effect of -2.1 percent in the first and second column of Table 2 identifies the long-term

Table 2: HER Effect on Different Samples

	Electricity Cons. (% of Control in Baseline)		
	(1)	(2)	(3)
Pre-Move Effect	−2.14	−2.10	−1.91
	(0.04)***	(0.18)***	(0.51)***
Post-Move Effect		−1.08	0.78
		(0.29)***	(0.79)
Sample	Full	Mover	Mover & Renter
Bills	58,733,360	5,890,855	735,391
Homes	1,810,096	139,908	19,270
R^2	0.63	0.54	0.49

Note: This table reports coefficients estimated with equation 8 on different samples. The coefficients respectively measure the average effect of treatment assignment after the fourth HER in the comparison period (the pre-move effect) and in the move period (the post-move effect). Each coefficient is presented in terms of percent changes to control group electricity consumption in the baseline period. Column 1 is estimated on the full sample, column 2 is estimated on the mover sample, and columns 3 limits the mover sample to rental homes. Estimates are obtained by weighting by the duration of each electricity bill and are regression-adjusted with fixed effects for each period of time, home, and year-by-season-by-wave. Standard errors robust to heteroskedasticity and autocorrelation are reported in parentheses below each estimate. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

average treatment effect of the HER, i.e., the ATE.[2] The post-move effect of 1.1 percent in the second column of Table 2 identifies the component of the long-term effect attributable to technology adoption, i.e., the ATK. Netting out the component attributable to technology identifies the component attributable to habits, which we call the ATH. For the mover sample the estimated component attributable to habits is -1.0 percent.

Normalizing these components by the ATE implies that 51.4 percent ($s.e. = 13.1$) of the long-term effectiveness is attributable to technology and 48.6 percent ($s.e. = 13.1$) is attributable to habits. Next we consider the robustness of these findings.

Robustness of Findings

The validity of our decomposition depends on three assumptions that were discussed in Section 2.4. The first requires mean independence between the potential outcomes, moving, and receipt of the HER. The second requires the balanced habits of the subsequent resident across treatment and control group homes. The third, and final, assumption requires stability of the technology adopted in response to the HER. Data consistent with this first assumption were discussed in Section 3.2. Next we consider the robustness of our findings to the second and third assumptions.

The third column in Table 2 provides a test of the balanced habits assumption. Consistent with this assumption, the third column of Table 2 reports a null post-move effect for rental homes in the mover sample. This null effect is consistent with the balanced habits assumption because rental agreements typically shutdown the technology channel.[3]

[2] Furthermore, the similarity of these estimates when estimated with the full and mover sample provides support for a stronger version of the mean independence assumption discussed in Section 2.4 that extends to selection into the mover sample.

[3] These estimates are normalized by average electricity consumption in the baseline period for control group renters. This figure was 32.5 kWh/day.

While we have no way of using data to investigate the plausibility of the stable technology assumption, Section 2.6.2 discusses how this assumption is likely to be violated and how such a violation leads to our estimated effects identifying a lower bound of the ATK and an upper bound of the ATH. These partially identified parameter bounds would reinforce our conclusion that the majority of the long-term effectiveness of the HER is due to technology. In other words, by assuming the stability of technology, we generate a conservative estimate of the contribution from technology adoption to the HER's long-term effectiveness.

Across several Online Appendix Tables, we demonstrate the robustness of our findings to additional concerns. Table A1 reports the pre- and post-move effects under a variety of different specifications control variables. Table A2 considers our results when the mover sample includes households moving before and after the receipt of their fourth HER. Table A3 estimates the parameters of interest when the mover sample is observed for a fixed amount of time in the baseline, comparison, and move periods. Table A4 considers the influence of homes that sit idle in the move period by omitting homes that consume an unusually small amounts of electricity. Across every table, the qualitative nature of our findings discussed above remain.

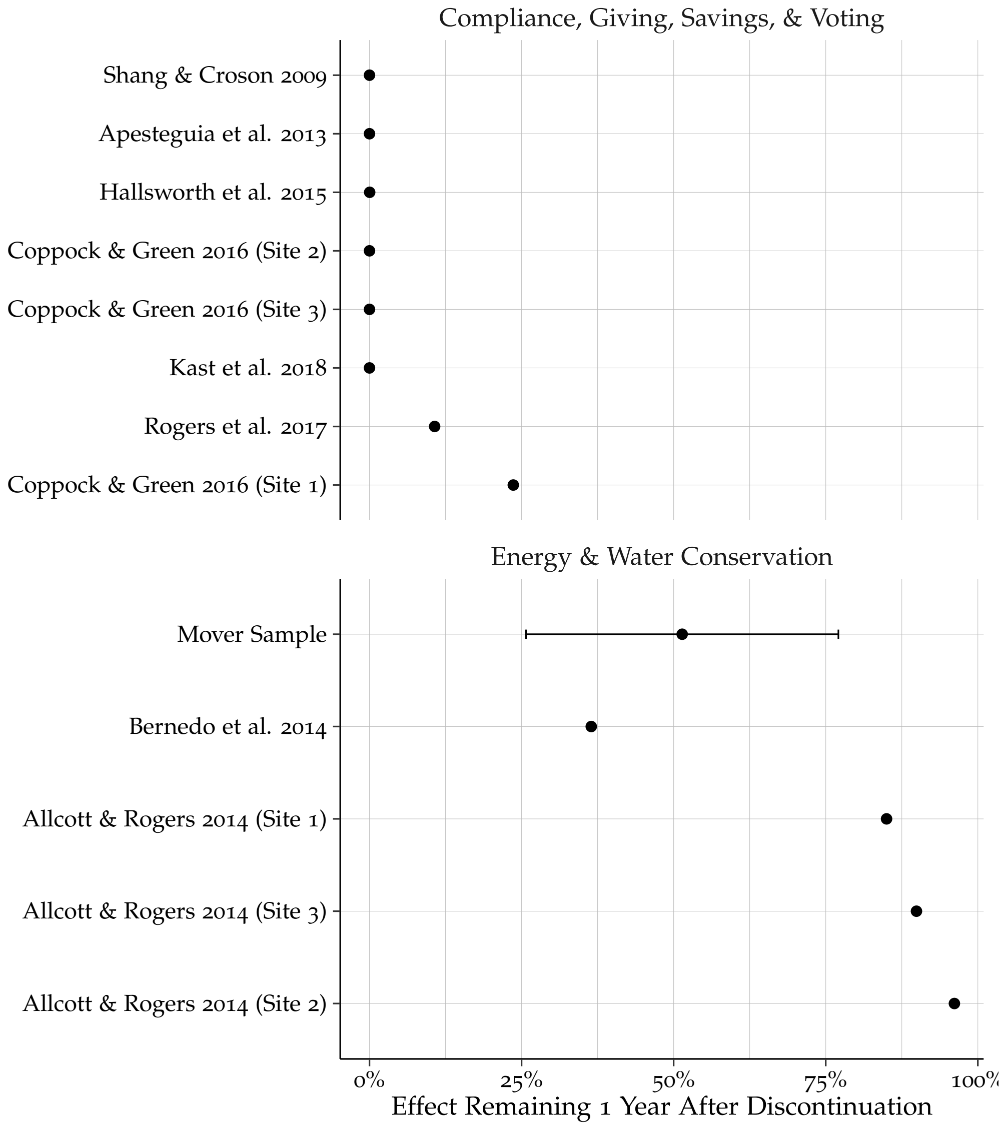
4.2 Implications

Having presented our decomposition of the channels underlying the long-term effectiveness of the HER, we next consider broader implications of our findings for nudges.

Explaining the Persistent Effects of Social Comparison Nudges

The persistence of social comparison nudges in prior studies varies dramatically across contexts. Figure 4 presents the average effectiveness of these

Figure 4: Effectiveness of Social Comparison Nudges After Discontinuation



Note: This figure presents the average effect of a social comparison nudge one year after it is discontinued. When such an estimate is not presented in a study, we predict the effect by fitting an exponential decay model on the data presented in Online Appendix Figure A1. Each effect is normalized by the average effect before discontinuation. The mover sample estimate includes the 95 percent confidence interval.

nudges one year after their discontinuation, with the estimates normalized by the average effect before discontinuation. The divergence in persistence across contexts can be seen by comparing the top and bottom panels of the figure. The top panel plots the average persistence when a social comparison nudge targets compliance, charitable giving, financial savings, or voter turnout. On average, just 4 percent of the initial effect of these social comparison nudges persists one year after discontinuation. In contrast, when a social comparison nudge targets water or energy conservation, 65 percent of the effectiveness, on average, remains a year after discontinuation.

Our decomposition results suggest a simple explanation for these divergent levels of persistence: The relative abundance of technologies for conserving energy and water. Recall that our decomposition of the HER's long-term effectiveness implies that 51.4 percent was attributable to technology adoption. We plot this estimated effect in Figure 4 and label it Mover Sample. As can be verified in the figure, this channel alone produces a level of persistence that is similar in magnitude to the total persistence arising from nudges to energy and water conservation and is much large in magnitude to the total persistence produced by nudging behaviors that are not easily modified by technology adoption, such as voting. We interpret this pattern as indicative of a central role for technology adoption in the persistence of treatment effects after the discontinuation of a social comparison nudge.

This interpretation is, on the surface, at odds with prior research. Allcott and Rogers (2014) uses participation in utility sponsored energy efficiency programs as a proxy for technology adoption and find that technology adoption explains no more than 2 percent of the HER's long-term effectiveness. In the same vein, Bernedo et al. (2014) finds that, after the initial resident moves, the effect of a social comparison nudge does not lead to statistically significant savings in water consumption, and the authors conclude that technology adoption is not an important mechanism underlying persistence. We, however, reject these conclusions based on our decomposition results. Using conventional levels of statistical significance, the 51.4 percent that we attribute to technology adoption is estimated precisely enough to reject the 2 percent attributed to technology adoption by Allcott and Rogers (2014) and the null effect reported by Bernedo et al. (2014). We believe that the imperfect proxy for technology adoption used by Allcott and Rogers (2014) and the low statistical power of the analysis by Bernedo et al. (2014) can explain why their findings diverge from the results of our decomposition.

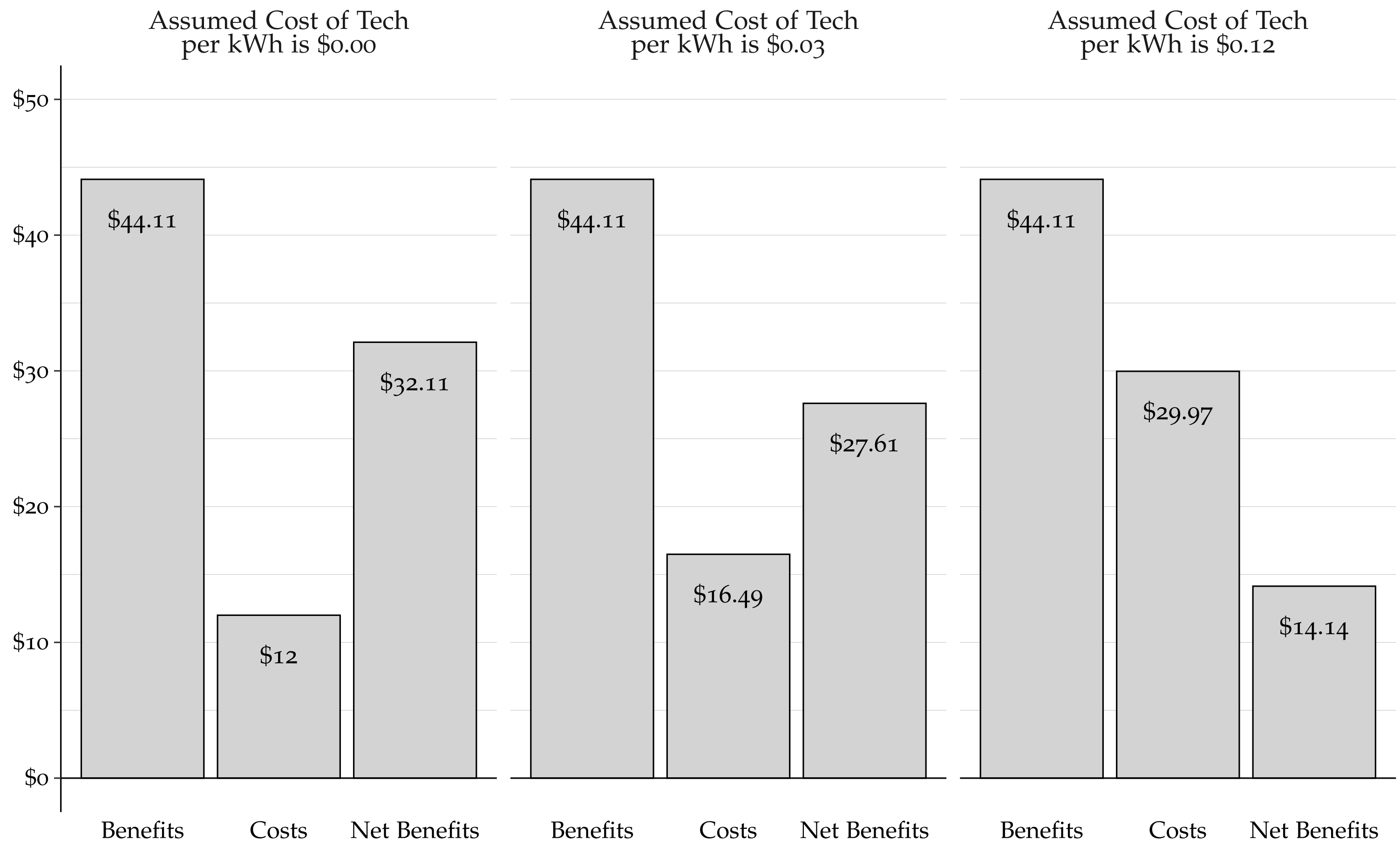
Net Benefits of Nudges

Our decomposition of the HER's long-term effectiveness also highlights a limitation of past evaluations of nudge-style interventions. These interventions have been evaluated by comparing the effectiveness of the interventions to the cost of their administration (Allcott and Mullainathan, 2010; Allcott and Rogers, 2014; Benartzi et al., 2017).[4] This approach to calculating the costs of nudges implicitly assumes that there are no other financial costs created by the intervention (Heckman and Smith, 1998). However, our analysis of the mover sample suggests that receiving a nudge can induce costly adoptions of technology, which should be accounted for in the evaluation of net benefits.

Figure 5 illustrates how accounting for technology adoption can impact the calculation of the HER's net benefits. This analysis assumes the HER is administered monthly for one year and the benefits accrue, undiscounted, over the two years after the start of the HER's administration. Furthermore, our analysis assumes households pay $0.10 per kWh. While we do not observe the financial cost of the technology adoptions induced by the HER, the post-move effect in Table 2 suggests the HER caused technology adoptions

[4] An additional approach implemented in Allcott and Kessler (2019) and Butera et al. (2022) elicits willingness to pay via incentivized surveys.

Figure 5: Net Benefits by Cost of Technology per kWh Saved



Note: This figure presents the benefits, costs, and net benefits of the HER for the mover sample. Each panel assumes a different cost of technology. The calculations assume the HER is administered monthly for a period of one year and benefits accrue undiscounted for two years. The cost of electricity is assumed to be $0.10 per kWh and the cost of administering the HER is assumed to be $1 per HER.

that saved each household 175 kWh per year.[5] Using estimates in the literature of the cost per kWh saved from technology adoption, we can then back out the implied cost of the technology adoptions.

The top panel of Figure 5 reproduces the evaluation with no cost attributed to technology adoption. In this scenario, the HER saves households \$44.38 on their electricity bills, the cost of administering the HER is \$12 (or \$1 per mailer), and net benefits are \$32.38. The middle panel of Figure 5 uses the cost of technology per kWh saved from Gillingham et al. (2018) of \$0.03. Under this assumed cost of technology adoption, the cost of the HER from \$12 to \$17.24, which reduces net benefits to \$27.14. In the final panel of Figure 5, we use the cost of technology per kWh saved from Billingsley et al. (2014) of \$0.12 per kWh saved. Under this assumed cost of technology adoption, the total cost of the HER nearly doubles and net benefits drops to \$11.43 per household. While the HER still passes a cost-benefit analysis, the net benefits drop by as much as 65 percent after accounting for the costs of HER-induced technology adoption.

5. Conclusion

Why do some nudges produce effects that persist and other nudges do not? This study develops a formal research design that addresses this question by decomposing the long-term effectiveness of a nudge into components attributable to habit formation and technology adoption. We apply our research design to the case of the HER, a nudge that is notable for its long-term effectiveness (see, e.g., Online Appendix Figure A1.) We find that a majority of the HER effect stays in a home after the initial resident moves.

[5]This calculation assumes technology was not adopted until the end of the comparison period. We make this assumption because it is the most conservative for illustrating the consequences of incorporating indirect costs in evaluating the net benefits of nudges.

After assessing the plausibility of the identifying assumptions in our design and the robustness of our findings, we interpret our results as providing evidence for the primacy of technology adoption in the long-term effectiveness of the HER. This finding offers several contributions and points to new directions for future work.

First, our study provides a simple explanation for the divergent levels of persistence in treatment effects after social comparison nudges are discontinued. The effect of a social comparison nudge is more likely to persist when the targeted behavior can be augmented by productive technologies, such as in-put efficient technologies to conserve energy and water. The effect is likely to persist when productive technologies are unavailable, such as in contexts where target behaviors are associated with compliance with rules, charitable giving, financial savings, tax evasion, and voting. Future work should explore the extent to which heterogeneity across experiments reflects differences in the costs or availability of productive technologies. For example, it would be fruitful to explore the extent to which differences in such costs and availability explain differences in persistence in multi-site experiments, such as Allcott and Rogers (2014) and Coppock and Green (2016).

Second, our study suggests that policymakers can replicate the long-term effectiveness of the HER in two ways. First, they can target behaviors that can be influenced by readily available technologies. Second, they can combine social comparison nudges with opportunities to adopt new technologies. For example, in the context of voting, our findings predict that the effects of social comparison nudges will persist in municipalities that provide an option to default into easier modes of voting in the future, such as mail-in voting. In the context of givings and savings, policymakers could pair social comparisons with an option for households to default to higher giving or savings rate in the future. Such defaults have been found to increase givings and savings (Madrian and Shea, 2001; Thaler and Benartzi, 2004; Goswami and

Urminsky, 2016; Altmann et al., 2019), but our findings suggest combining these defaults with the framing of a social comparison will produce longer lived effects.[6] Future work should explore this conjecture.

Third, our study illustrates the importance of a full accounting of the costs induced by nudges. In our context, costly technology adoption attenuates previous estimates of HER effectiveness and the associated attractiveness of such interventions vis-a-vis other policies to promote energy conservation. This suggests that future research on nudges should consider the indirect costs induced by the intervention to obtain more robust evaluations.

In addition to these three contributions, our study provides an important methodological contribution. To assess the mechanisms underlying behavioral responses to policies and programs, prior research has relied on survey measurements. However, relative to the cost of administering a nudge, a survey approach would be extraordinarily expensive. Our study thus complements previous work by developing a new research design that is well suited to isolate the mechanisms underlying the effectiveness of nudges. We imagine future research can build on this strategy. Potential applications include using the graduation of students or the separation of employees to understand the extent to which nudges, such as those respectively studied in Bettinger et al. (2012) and Earnhart and Ferraro (2021), produce human capital in the recipients of the nudge and in the organizations in which the recipients are nested.

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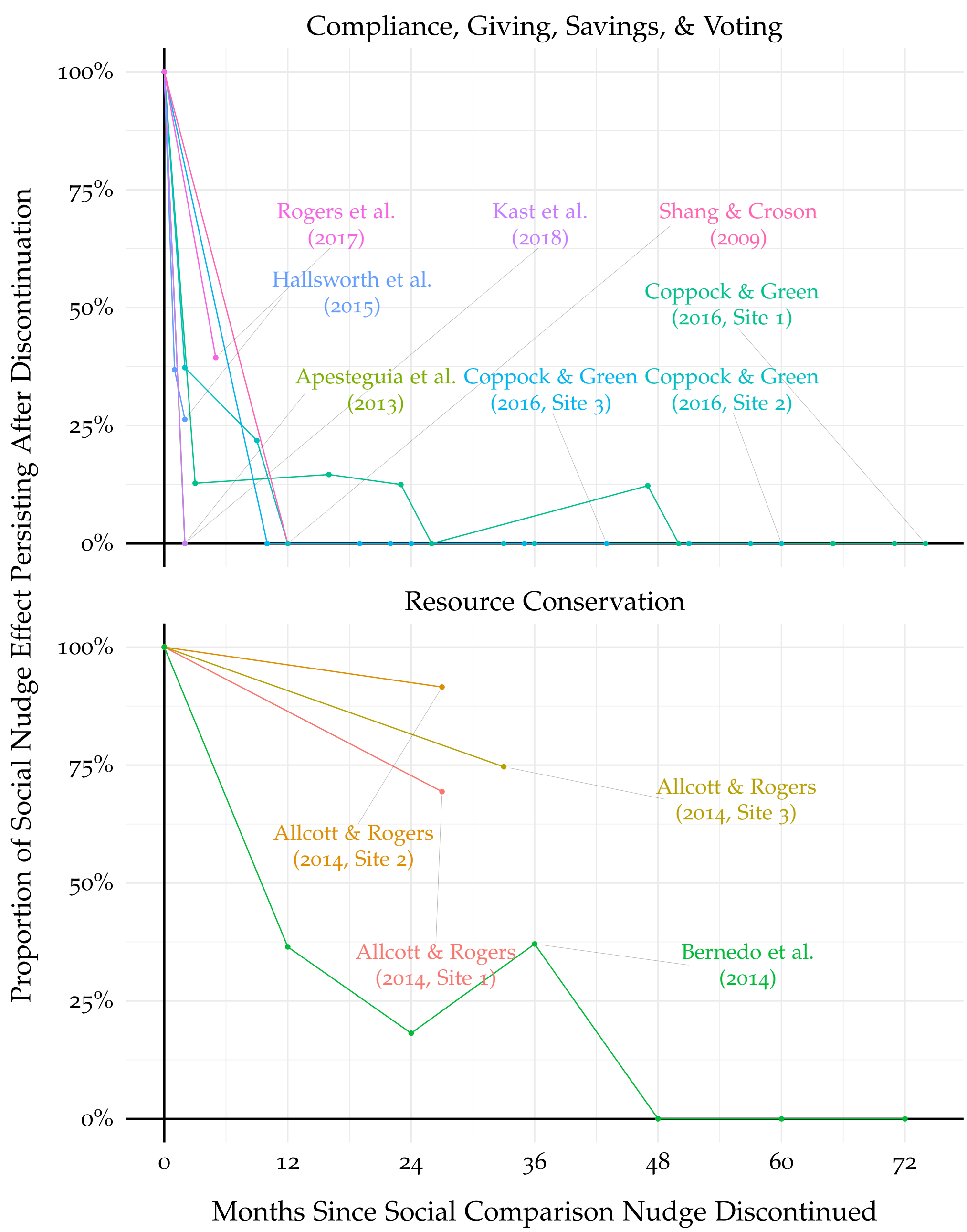
A. Online Appendix

Figure A1: Persistence of Social Nudge Effects



Note: This figure reports the proportion of the effect of a social comparison nudge that persists after it is discontinued. Estimated effects that are not statistically significant at the five percent level are set to zero.

Table A1: Robustness of HER Effects to Control Specification

	Electricity Cons. (% of Control in Baseline)					
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-Move Effect	−4.17	−2.35	−2.14	−3.39	−2.27	−2.12
	(0.25)***	(0.25)***	(0.18)***	(0.70)***	(0.70)***	(0.52)***
Post-Move Effect	−2.14	−0.66	−0.87	0.91	1.51	0.99
	(0.30)***	(0.31)**	(0.30)***	(0.83)	(0.85)*	(0.81)
Controls	Treatment, Period, Wave	Treatment, Period, Wave, Year-by-Season of Bill-by-Wave	Treatment, Period, Wave, Year-by-Season of Move-by-Wave, Year-by-Season of Bill-by-Wave, Avg. Elec. Cons.-by-Baseline Season-by-Wave	Treatment, Period, Wave	Treatment, Period, Wave Year-by-Season of Bill-by-Wave	Treatment, Period, Wave Year-by-Season of Bill-by-Wave Year-by-Season of Bill-by-Wave, Avg. Elec. Cons.-by-Baseline Season-by-Wave
Sample	Mover	Mover	Mover	Mover & Renter	Mover & Renter	Mover & Renter
Bills	5,890,855	5,890,855	5,890,855	735,391	735,391	735,391
Homes	139,908	139,908	139,908	19,270	19,270	19,270
R^2	0.16	0.22	0.47	0.15	0.22	0.44

Note: This table reports coefficients estimated with equation 8 on different samples and specifications of control variables. The coefficients measures the average effect of treatment assignment in the comparison and move periods. Each effect is presented in terms of percent changes to control group electricity consumption in the baseline period. Columns 1-3 are estimated on the entire mover sample, columns 4-6 on the mover sample that renters their home, and columns 7-9 on the mover sample that uses electricity to heat their home. Estimates are obtained by weighting by the duration of each electricity bill and are regression-adjusted with the controls denoted. Standard errors robust to heteroskedasticity are reported in parentheses below each estimate. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

Table A2: Robustness of HER Effects to Mover Sample Cutoff

	Electricity Cons. (% of Control in Baseline)			
	(1)	(2)	(3)	(4)
Pre-Move Effect	−2.08	−2.07	−2.08	−2.08
	(0.18)***	(0.18)***	(0.18)***	(0.18)***
Post-Move Effect	−0.93	−0.96	−1.04	−1.03
	(0.25)***	(0.26)***	(0.27)***	(0.31)***
HER Cutoff	1	2	3	5
Bills	7,334,722	7,017,358	6,486,682	5,282,567
Homes	182,559	173,105	157,415	121,980
R^2	0.53	0.53	0.53	0.53

Note: This table reports coefficients estimated with equation 8 on different constructions of the mover sample. The coefficients measures the average effect of treatment assignment in the comparison and move periods. Each effect is presented in terms of percent changes to control group electricity consumption in the baseline period. Columns 1-4 are estimated on a mover sample that receives at least 1, 2, 3, and 5 HERs before moving. Estimates are obtained by weighting by the duration of each electricity bill and are regression-adjusted with fixed effects for event time, home, and year-by-season-by-wave. Standard errors robust to heteroskedasticity are reported in parentheses below each estimate. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

Table A3: Robustness of HER Effects to Balanced Panel

	Electricity Cons. (% of Control in Baseline)		
	(1)	(2)	(3)
Pre-Move Effect	−3.82	−3.91	−2.25
	(0.34)***	(0.40)***	(0.47)***
Post-Move Effect	−2.18	−2.18	−1.82
	(0.39)***	(0.44)***	(0.58)***
Days Cutoff	182	273	365
Obs.	251,676	157,500	82,035
Homes	83,892	52,500	27,345
R^2	0.73	0.78	0.79

Note: This table reports coefficients estimated with equation 8 on different constructions of the mover sample. The coefficients measures the average effect of treatment assignment in the comparison and move periods. Each effect is presented in terms of percent changes to control group electricity consumption in the baseline period. Columns 1-3 are estimated on a mover sample that is observed in the baseline, comparison, and move periods for at least 182, 273, and 365 days. The unit of observation is aggregated to one observation of average electricity consumption for each household in each period. This average is constructed with the final 182, 273, and 365 days in each period. Estimates are obtained by weighting by the duration of each electricity bill and are regression-adjusted with fixed effects for event time, home, and year-by-season-by-wave. Standard errors robust to heteroskedasticity are reported in parentheses below each estimate. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

Table A4: Robustness of HER Effects to Dropping Low Use Move Period Homes

	Electricity Cons. (% of Control in Baseline)		
	(1)	(2)	(3)
Pre-Move Effect	-2.32	-2.02	-2.05
	$(0.22)^{***}$	$(0.19)^{***}$	$(0.18)^{***}$
Post-Move Effect	-1.48	-1.37	-1.19
	$(0.29)^{***}$	$(0.29)^{***}$	$(0.29)^{***}$
Change	Drop Homes with 1 σ Change in Move Period	Drop Homes with 2 σ Change in Move Period	Drop Homes with 3 σ Change in Move Period
Bills	3,995,428	5,268,494	5,690,485
Homes	94,025	124,746	134,980
R^2	0.54	0.53	0.54

Note: This table reports coefficients estimated with equation 8 on different constructions of the mover sample. The coefficients measures the average effect of treatment assignment in the comparison and move periods. Each effect is presented in terms of percent changes to control group electricity consumption in the baseline period. Columns 1-3 are estimated on a mover sample that has move period electricity consumption within 1, 2, or 3 standard deviations of baseline and comparison period electricity consumption. Estimates are obtained by weighting by the duration of each electricity bill and are regression-adjusted with fixed effects for event time, home, and year-by-season-by-wave. Standard errors robust to heteroskedasticity are reported in parentheses below each estimate. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.