Prices vs. Nudges: A Large Field Experiment on Energy Efficiency Fixed Cost Investments

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Abstract

This paper reports the results of a large scale field experiment which we designed to parse the effects of different social comparison frames and subsidies for electricity use, uptake of in-home energy audits and energy efficient durable good installations. Households were randomized into treatments in which they recieved letters comparing their electricity usage to their neighbors. We used three comparison frames: relative electricity usage in KWhs, relative monthly expenditures in dollars and relative CO2 emissions from electricity usage in tons. In addition, we randomized the price of an in-home energy audit. We find that frames which affected electricity use did not affect audit uptake, and vice versa. Mean use effects varied significantly by household type, as measured by political affiliation. We price out the value of a social comparison at \$40. Our results have several implications for the mechanisms driving how households' electricity consumption patterns respond to non-pecuniary signals.

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1 Introduction

There is increasing evidence that consumers respond to informative signals about how their behavior relates to the behavior of others (Ferraro and Price (2013), Allcott (2011), Costa and Kahn (2011), Schutlz, Nolan, Cialdini, Goldstein, and Griskevicius (2007), and Ito, Ida, and Tanaka (2013)). What is unclear, though, is the mechanism behind how these signals affect behavior. For example, informative signals could lead to changes in behavior through updating of beliefs. Alternatively, relating behavior to a peer group could cause various forms of social pressure to conform (Benabou and Tirole (2006) and Andreoni (1989)). It is also unclear how these signals would relate to a commensurate price change which could affect the same behavior. As a result, there is no metric to relate an informational signal to a purely pecuniary signal. This makes directly calculating the value of sending informative signals about relative behavior impossible.

This paper reports the results of a large scale field experiment which we designed to address each of these outstanding issues in the literature. To align our experiment with the existing literature, we chose to use a well-studied area of informative relational signals: electricity usage. We sent roughly 55,200 households in a medium sized MSA in the U.S. informative signals about how their electricity usage related to other households in their neighborhood. The design of the signals mirrors the well-known O-Power letters (Allcott (2011)). We then offered the households the opportunity to sign up for an in home energy audit in addition to tracking their subsequent electricity consumption. In home energy audits are very common programs offered by electricity utilities and wholesalers designed to reduce household electricity use.[1]

[1]During an audit, employees of the auditing firm enter a household and identify the best ways the

There were 12 treatments in the field experiment in a three by four design, in addition to one control group. We conveyed a household's electricity usage in one of four ways: their absolute use in KWhs with no comparison to their neighbors, their relative use in kilowatt hours (KWhs), their relative use in monthly expenditures ($), and their relative use in CO2 emissions. We also varied the prices of the $50 in-home energy audits by offering cash gift cards of either zero, $20 or $50.

The experimental design allows us to do two things: first, we can directly assess how changing the price on an audit affects audit uptake probability relative to sending electricity usage comparisons. Second, we can relate how highlighting the public good aspect of electricity consumption relates to highlighting the private aspect by identifying how usage and audit uptake rates vary by treatment. Framing comparative signals differently across treatments allows us to help identify what mechanism (informational versus normative) drives changes in electricity consumption decisions. Specifically, the CO2 frame highlights the public good aspect of conservation whereas the expenditure and KWh frames highlight the private good aspect of the good. Importantly, including a treatment which advertises the program and gives absolute use without a comparison lets us parse out the marginal effect of comparisons specifically.

We use the field experiment to test four hypotheses in this paper. First, we test how framing affects economic decisions via highlighting the good, the private costs of the good and the public cost of the good (KWhs, expenditures and CO2 respectively). Using different frames provides evidence as to the mechanisms driving household response to

household can reduce their electricity usage through making energy efficiency enhancing fixed costs investments. The auditor provides engineering estimates of monthly savings and often subsidizes any investments made by the household.

non-pecuniary signals. Specifically we are able to separately identify a purely informational component affecting the private costs of the goods versus a normative component when highlighting the social costs of the good.[2]

Second, we test for differences across the comparisons and prices relative to a control group on energy audit uptake. This second hypothesis is important since it gives of the opportunity to monetize the dollar equivalent of a signal on uptake probability. It is entirely possible that different frames could be monetized at different levels.

Third, by identifying how agents respond to different frames (e.g., CO2 versus expenditures) we provide insight as to which specific actions (reducing electricity use and/or increasing audit probabilities) ameliorate moral costs. It is possible that actions which reduce private costs are different from actions which reduction moral costs highlighted with the CO2 frame. This has important implications for a growing literature related to private versus moral components of utility (Levitt and List (2007) and Ferraro and Price (2013)). We are not aware of other studies which are able to parse this potential asymmetry cleanly for a good with both private and public components associated with its consumption.[3]

Fourth, we test for differences in the probability of actual installations conditional on audit uptake across treatments. This is different than the important policy question of which combination of pecuniary and non-pecuniary signals causes the largest increase in installations. Rather, here we identify whether comparisons lead to installations of new energy efficient durable goods. By looking across frames, this hypothesis also provides evidence of what drives microeconomic decision making for private versus moral components

[2]This builds off of earlier work in completely different contexts by Chen, Harper, and Li (2010) and Chen and Li (2009).

[3]Kotchen and Moore (2008) uses field data and highlights the possibility that lumpy actions like scheduling an audit could be more effective than continuous actions like reducing average electricity use via turning off lights at "purchasing" moral satisfaction.

of utility.

For each hypothesis discussed above we control for the political affiliations of the households in the dataset. To do so, we gathered publically available political registration data from the internet and matched it to households. Importantly, political affiliation is correlated with other demographic characteristics like race and income. We partially mitigate this correlation by using nine-digit zip code fixed effects which proxies for spatially correlated demographic characteristics like income. As a result, we identify political affiliation effects based upon variation within average demographic characteristics at the nine-digit zip level. Although we acknowledge that our estimated effects for political affiliation are not causal, observing heterogeneity by affiliation allows us to determine how non-pecuniary signals can potentially have heterogeneous effects across the population. Identifying different effects across political affilitation addresses the importance of framing non-pecuniary signals across consumer types.

For use, we find that the unconditional effect of treatment is to increase electricity use. However, when we control for political affiliation this increase is driven almost entirely by republican households (+10%). There are several possible mechanisms behind this surprising result including heterogeneous salience effects and spite we discuss in the paper. Interestingly, though, republican households significantly reduce their electricity consumption when given the CO2 comparative use frame. Households with no registered political affilitation significantly decreased their use (-3%). High use households were even more likely to reduce (-5%).

For audits, both the expenditure and KWh frames increased audit uptake but the CO2 comparison did not. As a result, we find some evidence that households respond to different frames across different margins. Subsidies caused an imprecisely estimated increase in uptake probability. Still, the point estimate for the value of a KWh or expenditure signal is on the order of \$40.

We find some evidence that privately framed signals (KWhs and expenditures) affect audits (e.g., extensive margin) and public good framed signals (CO_2) affect use (e.g., intensive margin). It appears, then, that public frames affect economic actions differently than private frames. This is somewhat surprising given that both actions affect consumption levels of precisely the same good: electricity. Conditional on being induced into an audit, though, we find some weak evidence that households reduce use regardless of framing. This is weak evidence that households seeking to reduce usage due to private incentives use both the intensive and extensive margin.

For installs, households who were induced to have audits due to comparisons were significantly less likely to make installations than the control group of untreated households getting an audit. Overall, households who were induced into an audit by treatment made installations at only one third the rate of the control group. As a result, we find evidence that the privately framed signals which were effective at increasing audit uptake were not nearly as effective at inducing actual extensive margin adjustments (e.g., durable good installs).

There are several important contributions of this study. First, we find evidence that framing effects are important. Further, we find that treatment effects and framing effects vary significantly by household type, as measured by political affiliation. This highlights the great care needed when using non-pecuniary signals to affect policy. Second, we find that frames which affect one margin for microeconomic decisions don't affect another margin. This is inconsistent with a utility function containing only a single private consumption component. It is consistent with utility having a moral component affected by public good appeals. Third, we are able to "price out" the value of a non-pecuniary signal across a single economic decision (e.g., audit uptake). Importantly, although we are able to price out a signal along this single economic decision, the effect of prices and signals for other realted economic decisions are different (e.g., installations).

The next section describes the field experiment. Section three describes a simple theoretical model to frame the experiment. Section four presents data and power tests. Section five presents results and offers discussion. Section six briefly concludes.

2 Experimental Design

The field experiment was conducted with the support of a utility and their primary wholesaler. The treatment area was a medium sized MSA served almost entirely by the utility. In this area, the utility provides electricity to all residential customers via average cost pricing. The utility's wholesaler actively operates a home energy audit program with the goal of reducing electricity demand, especially during peak load electricity days and hours. During the study period, the negotiated retail price of electricity was constant.

The goal of the field experiment is to identify how prices and different forms of comparative information affect the probability of signing up for an in-home energy audit. We worked with the utility and the wholesaler to design the experiment in order to find the drivers and increase uptake of in home energy audits. To do this we sent out letters to households inviting them to participate in an audit. Figure 1 shows the decision tree available to all households in the treatment area. We only show the decisions for the treated households but the same decisions are available to control households at every node. We send letters to treated households inviting them to participate in an in-home energy

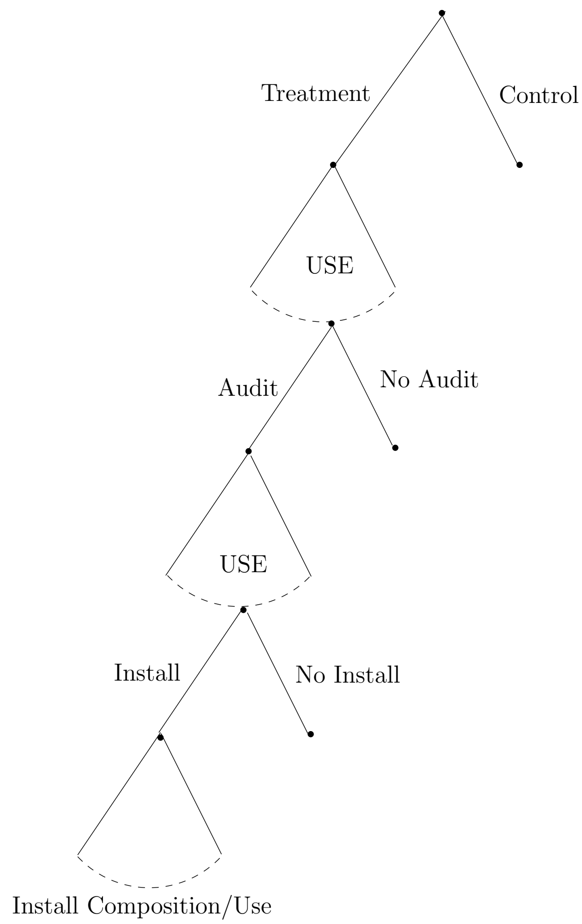

Figure 1: Decision tree for field experiment. Note that control households have same access to same decisions over every node.

audit. All control households have the same opportunity to participate but do not receive a letter. Treated households then decide whether or not to schedule an audit. Conditional on scheduling an audit a household has the opportunity to make an installation which is heavily subsidized ($500 for any durable good installed) by electricity producers.[4] We are able to track monthly electricity use for the universe of households over the entire study period.

The main contribution of the experiment is over the variation embedded across treat-

[4]The same subsidy of $500 for any durable good installed is available to all members of the control group over the entire experimental window.

ments across both the price on an audit and the framing of a comparison. We varied the price of the audit (normally \$50) randomly between \$50, \$30 and \$0. In the letters, we also told household's their average electricity use over 12 months compared to other households in their nine digit zip code.[5] We varied the way in which households received this information, though, by telling households their use (in KWhs), their expenditures (in \$), or their average emissions attributable to electricity use (in tons of CO2 equivalent). We included one set of price treatments in which we gave households no comparative information but did tell them their electricity use using the KWh frame. For each treatment, we conveyed information using the electricity wholesaler's official letterhead (e.g., the entity overseeing in home energy audits in the area). An example of a letter with company information blacked out and with artificial data is in the appendix. The call center that schedules in home audits recorded the caller's code at the bottom of the caller's letter allowing us to match treatment group to households who scheduled an audit.

The three by four design of treatment in the field experiment is graphically displayed in Table 1. There was a large control group so that overall there were 13 groups of households. We employ a weighted design over the price dimension (25%-50%-25%) in order to increase power of identifying non-linear effects of price on likelihood of signing up for an in home energy audit.

In the area of the field experiment, the utility bills households monthly. Each monthly bill from the utility displays the previous 12 months of electricity usage by the household. The bill does not, though, relate this to average local area usage nor does the bill calculate the household's average electricity use over those 12 months. The utility shared electricity

[5] In cases where there were fewer than 10 households in a nine digit zip we used five digit zip code averages.

Table 1: Treatments Used in Field Experiment

Message Subsidy	No Comparison	KWh Comparison	$ Comparison	Emissions Comparison
$0	1	2	3	4
$20	5	6	7	8
$50	9	10	11	12

NOTE: In the "No Comparison" treatment column, households told only their own use measured in KWhs. Numbers indicate individual treatments only. There was also a large control group.

usage data by household-month. People who move are identified in the dataset by having inconsistent billing dates throughout the panel dataset are were dropped from the dataset.

3 Theoretical Model

We test four main hypotheses due to the design of the field experiment. The hypotheses are mainly related to both the existence and function form of different models of economics behavior. In this section we develop a partial equilibrium model of economic action, a, which contains a traditional consumption utility component and an additively separable moral component as in Ferraro and Price (2013).

We are interested on how a consumer maximizes expected utility in response to treatment. There are three different economic actions available to the consumer in our model. One is a continuous economic action (electricity use) and two are fixed cost economic actions (getting an in home energy audit- IHEA- and making an durable good installation to improve energy efficiency). Partnerships with a utility and an auditing agency allow us to observe each one of these outcomes over the study period. The structure of the audit and installation program in our study area, like most auditing programs nationwide, means that the consumer cannot make an installation through the audit program without first getting an audit.[6]

Consider the following characterization of a consumer's expected utility:

$$E[U_i(a, A, I; \delta, s_f|\Theta)] = E\left[c(a|A, I, \delta, s_f, \Theta)\right] - M(a, A, I|\delta, s_f, \Theta) - p_A 1\{A\} - p_I 1\{I\} - p_e a$$

Where the notation is defined as follows: $E\left[c(\cdot)\right]$ is the expected private consumption utility which is a function to electricity use, a. Importantly, a represents a continuous economic action that representing consumption on the intensive margin. If a household gets an energy audit (represented by the indicator variable A) or makes an energy efficient durable good installation (represented by the indicator variable I) it can affect the households information set about needed electricity consumption. The expectations operator highlights uncertainty of this possibility. We subsume a time subscript for notational ease. Importantly, both A and I represents an discrete economic action that associated with an extensive margin of electricity consumption. Each economic action j has a price p_j.

As in Ferraro and Price (2013) there is a second component of utility. $M(\cdot)$ is an additively separable function dictating the moral aspect of the three economic actions (e.g., a , A, and I). We allow for A and I to enter directly into the moral component of utility. This allows for utility to be influenced by benefits and costs not associated strictly with the private benefits and costs associate with electricity consumption and audits. We assume that both the private and moral components of consumption are influenced by a vector of parameters Θ.

[6]Consumers can of course make energy efficiency durable good installations without getting an audit but those installations are not subsidized through the audit program. Since we are only interested in audits occuring through the audit program we don't model associated subsidies. As a result the price variable in our model is the post-subsidy price of any installation.

We allow for utility to be affected by signals (s) about how their electricity use relates to others and the framing f of those signals: s_f. Importantly, there are two mechanisms through which relative use can affect subsequent use. The first is purely informational: if a household learns that they use significanly more electricity than nearby households, it is possible other households have additional information about low cost actions. The second is purely normative: if households have a preference to conform to average behavior then households have a normative component. We designed our frames in order to parse between these two channels.

We used three framings that we argue highlights these two channels differently: average relative electricity use in KWhs/month, average relative electricity expenditures in dollars/month and average relative CO2 emissions related to use in tons/month. These frames highlight the private good, private expenditure and public good benefit to reducing electricity use.

We hypothesize that both the expenditures and KWh comparisons higlight the private aspects of electricity consumption. The CO2 framing highlights public "bad" aspect of electricity consumption. It households have normative preferences with respect to public good preferences then this frame should decrease electricity usage. We note that it is possible for households to reduce electricity consumption directly or schedule an audit in order to decrease (expected) future electricity use.

Importantly, each comparative framing contains the exact same information about *relative* electricity consumption. As a result, if there is a significant difference in use or audit uptake across KWh and expenditure frames, for example, it is evidence of bounded rationality by households. The same is not necessarily true for the CO2 frame since it highlights the externality associated with electricity consumption. There is a significant body of research noting that priming and framing can have important implications for microeconomic decisions (Chen and Li (2009) and Chen, Harper, and Li (2010)). Our contribution along this dimension, however, is that there is both an intensive and extensive margin along which a subject can influence electricity use. Testing for the differences along different margins is a key contribution of our design.

Lastly, we allow for the parameter δ to represent either electricity "expenditure" or "product" salience. The parameters bears similarity to the price salience in DellaVigna (2009), Gilbert and Graff-Zivin (2014) and Sexton (2014) but is in fact different. "Expenditure" salience relates not to the price of a good (e.g., electricity) but the average expenditures and/or consumption of the good over time. This represents the possible presence of recent theoretical work which develops a theory of salience in which outliers are more salient that averages (Bordalo, Gennaioli, and Shleifer (2013)). For example, consumer's might not fully recall their average expenditures as much as their maximum monthly expenditure in the recent past. Conversely, "product" salience relates to the strict attention drawn to electricity consumption via being treated. While δ could certainly affect consumption utility, we allow it to influence the moral component of utility as well since CO2 contributions are one of our frames.

We include a treatment in which households informed about their own usage only and not relative usage. This provides two insights. First, if either expenditure and product salience effects are present, a letter with a household's own electricity usage (rather than relative usage) can affect electricity consumption. Product salience posits that by focusing attention on a product, the agent consumes more of it (Bordalo, Gennaioli, and Shleifer (2013)). As a result, product salience is consistent with increases in consumption. Expenditure salience is consistent with both increases and decreases in electricity consumption depending on whether perceived expenditures are greater than or less than actual expenditures provided to households. Second, this treatment permits us to tease out the marginal effect of comparisons. Previous studies of electricity consumption do not parse between the pure treatment effect (being informed of electricity consumption levels) from the informational and normative treatment effects (being informed of relative electricity consumption levels).

Having both an intensive and extensive margin for electricity use, in conjunction with public good based frames, lets us identify the relative magnitudes of the economic decisions which are arguments of $M(\cdot)$. This question of function form of the moral cost component has been raised in the literature but there is no widely accepted answer to our knowledge (Kotchen and Moore (2008) and DellaVigna, List, and Malmendier (2012)). The question is an important one since it matters for development of increasingly popular policies which uses non-pecuniary methods to affect economic behavior (Ferraro and Price (2013) and Allcott (2011). As in Ferraro and Price (2013), it could be that $M(\cdot)$ is convex in a. However, it could also be that there is a fixed decrease in moral cost from either scheduling an audit (A) and/or making an installation of an energy efficient durable good (I); if we observe that comparisons increase uptake of audits but don't affect electricity use nor installations, we can infer that $M(\cdot)$ is mainly operated upon by the fixed cost action highlighted in the treatment letter (e.g., audits). Conversely if comparisons affect both audits and use then both aspects of the moral component of utility are important. Lastly, if there are no significant differences in any economic decision across different shadings, then we can infer the value of comparisons we purely informational.

From a pure consumption utility perspective the decision to get an audit, A, is a function of the uncertain benefits of getting an audit less the costs of getting an audit:

$$E\left[c(a_1|1, I, \delta, s_f, \Theta) - p_e a_1 - P_I - (c(a_0|0, I, \delta, s_f, \Theta) - p_e a_0)\right]. \tag{1}$$

If there is no moral component to utility and expression (1) is greater than zero, a consumer will schedule an audit. The magnitude of expression (1) is affected by the information content of any signal in the letter s_f. For example, if a household i receives a signal showing their electricity usage is greater than the mean usage in their local area, then the expected benefits of getting an audit in terms of decreased cost of use increase. Therefore audit uptake should increase as well. Similarly, if the price of an audit decreases then audit uptake should increase as well. Alternatively, if there is both a moral and pure consumption component to getting an audit, the expected benefit from getting an audit is:

$$\begin{aligned} E\left[c(a_1|1, I, \delta, s_f, \Theta) - M(a_1, 1, I|\delta, s_f, \Theta) - p_e a_1 - P_I \right. \\ \left. -(c(a_0|0, I, \delta, s_f, \Theta) - M(a_0, 0, I|\delta, s_f, \Theta) - p_e a_0). \right. \end{aligned} \tag{2}$$

Equation (2) highlights a major contribution of this study: pricing out a signals by identifying the subsidy which increases audit update to the same extent as a comparison s_f. We note, though, that the price equivalent variation which induces audits could have different implications for subsequent economic decisions, like making installations. We explore this possibility below.

Taken together, tracking the vector of use, audit uptake, and installs after audits as a function of different treatments inform us about the relative magnitudes of different components of utility and also their functional forms.[7] We now describe the hypotheses the experimental design allows us to test.

3.1 Hypotheses and Predictions

Taken together, the theoretical model and the experimental design allow us to test several important possible explanations of observed behavior. This subection describes those hypotheses and predictions. We also perform a simple cost benefit analysis of this policy if it were to be rolled out on a national scale at the end of the results section.

H_1: Different comparisons affect use asymmetrically.

If different framings affect use differently it addresses the existence and functional form of a moral component to electricity use. Specifically, if a CO2 comparison has an additional marginal effect on use above expenditure comparisons, it is evidence that households "purchase" moral cost reduction through scheduling the audit only rather than reducing use. If being treated but not receiving a comparison affects use directly, it is evidence of a pure treatment effect. Such a pure treatment effect for use is consistent with the existence of either "expenditure" or "good" salience.

H_2: Different framings affect household audit uptake asymmetrically. Subsidies affect audit uptake.

Treatment was, in this case, a letter designed to increase uptake of in home energy audits. From a policy perspective, which framing increases audit uptakes most dramatically is important since it can costlessly increase the value of rolling our this experiment on a larger scale. Similary, we test for an effect of subsidies on uptake. If audits are a good,

[7] We acknowledge it is also possible that households are not able to understand that signals are simply their electricity use measured in different units. The sign and relative magnitudes of economic responses to different treatments imply that this does not explain our results, though.

subsidies shouldl increase audits. Performing these joint tests lets us "price out" the price variation needed to induce the same economic decision along the extensive margin.

H_3: Different framings affect household use and audit uptake differently.

If a particular framing increases audit uptake it is further evidence of whether private or moral utility components drive economic activity. If what drives use is not what drives audit uptake, it is evidence that what drives electricity use decisions on one margin or not the same as what drives another. This provides evidence about the functional form of the moral component of utility, rather than simply its existence.

H_4: The probability of making an installation, conditional on getting an audit, is the same across treatment cells and the control group.

If households who are induced to get an audit by the experiment are less likely to make an eventual installation, it is evidence that self-selection into getting an audit by the control group drives much of the activity for energy efficient investments. Similary, if household that are priced into audits by the experiment make different installation decisions as those who were induced by comparative signals it is further evidence of selection. Importantly, if signals don't drive intallations but subsidies do, it is evidence that pricing the value of information for one economic decision (e.g., audits) does not map to other related economic decisions (e.g., installations).

H_5: The effect of comparison signal frames varies by consumer type (measured by political affiliations)

We test for the importance of household heterogeneity across frames for use and audit uptake. This hypothesis highlights the possible contextual nature of non-pecuniary signals. We note that political affiliation at the household level is correlated with income, age, race and other demographic characteristics. We don't claim our estimates show the effect of political affiliation alone.

4 Data

There were two important datasets which we merged in order to perform the field experiment and then analyze the results. First, the utility shared monthly billing data by address for 30 consecutive months. With this data we constructed average usage for each household. We then constructed local area averages (e.g., average usage within a nine digit zip) in order to form a comparison group.[8] Second, the agency managing in home energy audits in this area shared the history of all in home energy audits in the area during over the same 30 consecutive month we obtained billing from the utility.[9] By merging these two datasets and randomizing treatment we are able to identify the causal effect of each treatment on both uptake of energy audits, installations and electricity use in the months immediately following treatment.

The field experiment was conducted between January 2013 and September 2013. We conducted a series of mailings via first class United States Postal Service mail. There was a total of five mailings ranging in size between 5,000 and 20,000. The treatment area was a medium sized MSA served almost entirely by a single utility. All households in the area who were served for electricity by this utility were eligible to be in the treatment group. All households not in the treatment group were in the control group. There were in total

[8]We chose to use the nine digit zip rather than a higher level of aggregation and the term "local area" due to the large body of literature showing that comparative signals are most effective when the comparison group is more proximate to the agent receiving the signal.

[9]Some households in the dataset moved or had their electricity cut off due to delinquent bills during the field experiment. These households are identified in the billing dataset by non-standard breaks in the billing dates. We drop all of these households from the field experiment. More information about this trimming process is in the appendix.

of roughly 104,000 households in either the treatment or control group. 55,456 households were in one of the twelve treatment groups.[10]

We used data from July 2011-July 2012 to create a twelve month averages of electricity usage for each household. We sent letters to randomly selected households in five different groups of mailings between December 2012 and August 2013. We are unable to determine if households opened their letters. As a result, the effect of treatment on outcome variables is an intent to treat effect. The delay between baseline use and sending letters was unavoidable due to timing constraints of our industry partners. We then observed electricity consumption data for all households through the September 2013 billing cycle. We also observe all signups for in home energy audits from the agency which oversees them for the wholesaler in this region. Using the combined dataset we are able to jointly identify the response of electricity use to differing price treatments and social comparisons on both the extensive margin (audits for durable good purchases) and intensive margin (electricity use).

We also scraped household political affilitations from the local county registrar's website and matched them to addresses. There are four classifications of political affiliations we use below: all registered voters republican (GOP), all registered voters democrat, registered voters from multiple political parties and no registered voters. As stated above, political affiliation at the household level is correlated with income, age, race and other demographic characteristics. We don't claim our estimates show the effect of political affiliation alone.

[10]There were around 3,000 envelops which were returned to sender. We removed these households from the sample.

4.1 Power Tests

We conducted power tests to determine what the likelihood of observing an effect of treatment. Our task was complicated since there are two variables of interest (audit uptake and electricity use) with effect sizes which are likely to differ. As a result, in choosing the size of our treatment and control groups we performed power tests for each variable in order to determine the likelihood of finding statistically significant effects. We briefly summarize those power tests here.

The average monthly rate at which households in our sample sign up for in-home energy audits is slightly over .1%. In conversations with the auditing agency in our study region, they said an optimistic expectation would be for treatment to cause audit rates to double. As a result, we assume in our audit calculations that signup rates will average .19% (e.g., a marginal effect of .09%) meaning that audit probabilities increase by roughly 90% with treatment.

Our control group is roughly 50,000 households and the average treatment group is roughly 4600 households (total of 55,200 households). For our power tests, we assign binary audit uptake decisions to simulated treatment and control households according to a binomial distribution with the uptake probabilities for treatment and control households as described above. We then estimate the marginal effect of being in any treatment group on audit uptake. We reject the null hypothesis of zero effect in 98% of cases. As a result, we will very clearly be able to tell if the field experiment had any effect across treatments.

When we perform the same exercise, disaggregating by treatments so that there are only 4600 treated households per treatment, we reject the null hypothesis in only 41% of cases. The main difference is the precision of the estimates: while the mean coefficient estimate is the same across both power tests, the estimated t-stat of the marginal effect of treatment in the single treatment is roughly one third of the t-stat when all treated groups are aggregated into a single large treated group (e.g., roughly 1.76, sitting just significant at the 10% level). Aggregating across signal types or subsidy levels, improves the power greatly: the average t-stat is 2.67 and we are able to reject the null hypothesis of zero effect 71.5% of the time.

In sum, then, we are reasonably confident that we will be able to detect an effect of treatment on audits given the size of our field experiment when treatments are aggregated and less so for individual treatments. As a result, we are willing to cautiously consider treatment effects that are significant at the 10% level as significant rather than noise. That said, we take great care in trying to eliminate noise in both the treatment and control group in robustness checks. For example, we remove over 2,000 returned letters by hand and remove all households who had an audit within the previous 18 months of the start of the field experiment. Doing this effectively increases the size of each treatment group by 10% and the size of the control by 5%. The effect of doing so increases the average estimated t-stat to 1.83 (up from 1.76) and the percent of successfully rejected null hypotheses to 43% (up from 41%). Given the results of these power tests, distinguishing different treatment effects across individual treatments (e.g., different signal types) requires that treatment vary by at least a factor of two (e.g., the ATE for one of the twelve treatments is twice the ATE for another) to expect that we find any significant effect across individual treatments.

With respect to the effect of treatment on electricity use, the main difference is that the signal to noise ratio is much larger. Households typically exhibit significant heterogeneity in their electricity use across months. Previous studies on the effect of nudges on electricity use find effects in the 1-2% (Allcott (2011)). Further, those effects tend to build over time after subjects receive multiple nudges. Our design involves only a single mailing.

We conservatively set the coefficient of variation on electricity use in .3; put another way, the monthly variation in electricity within a household is 30% of the average use. This leads to conservative power estimates for the effect of treatment on use based upon within household electricity use variation we observe in the data. We first assume the treatment effect is -1%. We generate treatment and control households as in the audit uptake power test. Under these assumptions, we reject the null hypothesis of no effect in all 1000 simulations at the 5% level if all treatment groups are aggregated. When disaggregated so that a treatment group has only 4600 observations but there are still 50,000 control households, the probability of rejecting the null hypothesis falls to 59% due to a decrease in the precision of the estimate. If the ATE falls to .3% (a plausible ATE given we only have a single mailing) the probabilities fall to 37.2% and 10% respectively. In sum, then, finding a ATE for use will largely depend on the size of the ATE. This is, though, in and of itself interesting: if households must learn to reduce electricity use regardless of signal type, it is an important finding in and of itself. Due to these features of the data, though, the probability of a type II error is likely to be small.

Finally, we note that we are actually estimating an intent to treat effect. We cannot force households to open their mail. As a result, any estimate we observe is an underestimate of the true ATE. From a policy perspective, though, this is the estimate of interest since households cannot be forced to open their mail.

5 Results

In this section we present results from the field experiment. We first present results for electricity use followed by uptake of audits.

5.1 Use

We first estimate the effect of receiving different types on treatments electricity use. Our preferred estimating specification is:

$$ln(use_{it}) = \alpha_i + \nu_{tz} + 1\{post\ treatment\}_{it}\gamma + \Sigma_{f=1}^{3} 1\{post\ treatment\ frame_{it}^{f}\}\beta_f + \epsilon_{it}. \quad (3)$$

We include household and month by year fixed effects.[11] The indicator function $1\{post\ treatment_{it}\}$ takes the value one in all months t after a household i receives a letter (inclusive) while the variable $1\{post\ treatment\ shading_{it}^{s}\}$ takes the value of one if a household i received comparison shading type s for all months after month t (inclusive).[12] Defining the tratment indicator in this way estimates the long run effect of a single signal. The indicator function $1\{post\ treatment_{it}\}$ to control for the pure treatment effect. As a result, thecoefficient γ is the pure treatment effect of receiving any letter (e.g., without a social comparison) on use. The coefficient β_f describes the marginal effect of a comparison frame f.

Table 1 summarizes the average treatment effect of being in the treatment group (γ) and the marginal effect of different social comparisons relative to the baseline effect of no

[11] A similar table in the Appendix shows that the point estimates are not greatly effects by inlcuding alternative sets of fixed effects.

[12] For the first month of treatment we use values between one and value which measure the percent of the first billing cycle the household was treated.

comparison (β_s). In this and in most tables, the first column is for the entire sample, the second for above the local area median, the third below the local area median, the fourth above the MSA median and the fifth below the MSA median.

The long run effect of treatment in every specifical is a highly significant 2% increase in use. The marginal effect of a comparison is not significant above the 10% level in any case. This is consistent with both an "expenditure salience effect" if households had priors which were too low about their monthly electricity use. It is also consistent with a "good salience effect" in which case the treatment led to increased electricity use directly by making use of the good more salient.[13] We explore this unexpected result more below. Our results are consistent with the rapid attenuation of the treatment effects found in Allcott and Rodgers (2014). We acknowledge that short post treatment observation windows for some households make definitive statements along that dimension difficult.[14]

Table 2 shows this effect broken out by deciles of relative use. We find a pure treatment effect that is large and signficant for both high relative use and low relative use households. Households in the middle of the distribution (relative to their local area) display no such effect. There is little convincing evidence of a significant marginal effect of comparisons on use.

Table 3 displays these results broken down by political affiliation. Controlling for political affiliation matters a great deal for our use results: we find that the pure treatment effect is signficant and negative for all unregistered households (the baseline). All increases are driven by republican and mixed political affilitation households. Conversely the marginal

[13]In the future we will perform regressions which test of the difference in max use relative to mean use to parse between the "expenditure salience effect" and "good salience effect" as possible explanations.

[14]There is growing literature examining the intertemporal effects on non-pecuniary signals. Allcott (2011) finds that the effects of comparisons build over time. Ferraro and Price (2013) finds a single comparison can have a longer lasting effect on water usage.

effect of a CO2 comparison is significant and positive for unregistered voters and significant and negative for republican and mixed households.

The results of Table 3 clearly indicate heterogeneity in responses to information that correlate with political party. As noted above, political affiliation is correlated with other demographic characteristics like race and income. Although we acknowledge that our estimated effects for political affiliation are not causal, observing heterogeneity by affiliation allows us to determine how non-pecuniary signals can potentially have heterogeneous effects across the population.

As discussed above, the increase in use is consistent with either a "expenditure salience effect" or a "good salience" effect that exists for Republican and mixed party households. Another explanation is spite toward the advertising agency. The langauge in the treatment letter was framed most generally around energy conservation. If spite toward the advertising agency is correlated with political party then it provides evidence that retribution could be a motivating factor for private goods with a public aspect to them (e.g. electricity use). The precise mechanism for this effect is unclear given our design but more research is needed in order to identify the mechanism behind the systematic variation in this unexpected treatment effect.

These results are consistent with a high degree of heterogeneity in how households interpret information. We find evidence that republican and mixed affiliation households respond to the public good frames. This is consistent with some households having a moral component of utility which can be affected by intensive margin actions and some which do not. When controlling for political affiliations, we find the reduction in use due to CO2 comparisons most strong in republican households above mean and absolute use levels.

5.2 Uptake

We next estimate the effect of receiving different types of treatments on uptake of an energy audit. Table 4 shows monthly audit uptake probabilities relative to the control group over the experimental window. Just over .9% of households in our sample receive an energy audit over our experimental window (Dec 2012 - September 2013). The unconditional average treatment effect increased audit uptake by 129%. Relative to the control group, the pure information effect increased audit probability by 34%. Subsidies increase uptake but the effect appears non-linear. This could in part be due to the our weighted 25-50-25 design along the price dimension leading to more noise for the \$50 subsidy. Menawhile, KWh and expenditure comparisons clearly increase audit probabilities. The increase attributable to the CO2 comparison is much smaller. In fact, there is some weak evidence that the CO2 comparison could actually decrease audit probabilities relative to just using price signals.

We are interested in the average monthly audit uptake rate. Our main estimating specification is performed via OLS:

$$1\{uptake_{it}\} = \alpha + \nu_{tz} + 1\{post\ treatment_{it}\}\gamma + \Sigma_{f=1}^{3} 1\{post\ treatment\ frame\}_{it}^{f}\}\beta_s$$
$$+subsidy_{it}\delta_1 + subsidy_{it}^2\delta_2 + \epsilon_{it}. \quad (4)$$

In estimating equation (5) the $1\{post\ treatment\}_{it}$ indicator variable is defined to be one in the month the household receives a letter and the subsequent month. We drop household fixed effects in this specification since the dependent variable is a rarely occuring binary variable. We are again interested in the marginal effect of comparison shadings on audit uptake. We are also interested in the causal effect of subsidies on audit uptake. We estimate both a linear and non-linear term as the deisgn is powered to identify a non-linear price effect if it is present. This specification will allow us to identify the dollar value of a comparison (e.g., nudge) since the effect of a one dollar increase in the subsidy is directly translatable to receiving a nudge. As before, we parse the sample above and below median use.

Table 5 shows the coefficient estimates from estimating equation (5). For expositional ease, we've transformed the subsidy variable to tens of dollars. We estimate the pure treatment effect γ is an imprecisely estimated zero. We find a positive and significant effect of the KWh and expenditure comparison (at the 10% and 5% levels) when we estimate the model using the entire sample. Due to lack of power from low audit uptake rates, though, we lose significance when we split the sample further in all but one case. The magnitude of a KWh or expenditure comparison is roughly a 300% increase relative to the control group. We find no effect of a CO2 comparison on uptake. We estimate the marginal effect of a subsidy to be an imprecisely estimated level of slightly greater that .01 per 10%. Using the point estimate, we price out the effect of a signal to be on the order of $40 although we note the large error bands around that estimate due to lack of power attributable to low audit uptake rates.

Table 6 breaks down the audit uptake probabilities by political affiliation. Although these results suffer due to lack of power, we do find that the expenditure comparison significantly increases audit uptake probabilities for republican households. This is strong evidence, then, that what drives microeconomic decisions assoicated with electricity conservation on the extensive margin (audit uptake) are comparisons around consumption utility frames. Conversely, what drove conservation on the intensive margin (e.g., electricity use) are comparisons around moral utility frames (e.g., C02 comparisons). This asymmetry is statistically significant for republican households. To our knowledge, this is the first documentation of this kind of asymmetry.

To further investigate this asymmetry, Table 7 considers the electricity use decisions of households who schedule an audit between the time they schedule an audit and when they actually have the audit. If households schedule an audit, there are presumably interested in reducing their electricity consumption, regardless if motivated by private versus moral reasons. As a result Table 7 re-estimates the use equation where the *post treatment* variable takes the value one for the month (or fraction of month) between when an audit was scheduled and when it occured.

While only one cell is significant in Table 7 (expenditure comparisons for households above the local median) the point estimates are consistent with households reducing their electricity use when induced to get an audit by the expenditure comparison. This effect is most strong for high use households in columns 2 and 4. Table 8 attempts to increase power by grouping all households who receive a comparison together and comparing them to but the control group and households who receive no comparison. Again we see the same pattern. This is weak evidence that households who schedule an audit due to comparisons reduce their electricity consumption even before the audit takes place. Recall, this effect is driven by a different frame than the earlier use effects since CO2 frames affected use (for republicans) and expenditure and use frames affect audit uptake rates.

5.3 Installs

Next we test for the likelihood of making actual installations conditional on having been induced into signing up for an audit by being treated rather than self-selecting into having an audit. This is an important policy question because if treated households are more likely to sign up for an audit but very unlikely to actually make an installation, it means that both nudges and financial incentives may not be good policies to induce households to make large energy efficient durable good upgrades even though the above results are important from a purely economics perspective.

We estimate the following model on the subset of households who ever had an audit over our sample period.

$$1\{Any\ Install_i\} = \alpha + \nu_t + 1\{any\ treatment_i\}\gamma + \Sigma_{f=1}^{3} 1\{\ treatment\ shading_i^f\}\beta_f + subsidy_i\delta_1 + subsidy_i^2\delta_2 + \epsilon_i. \quad (5)$$

All indicator variables in equation (5) take the value of one for households for which an audit occurs. Similarly, the subsidy levels take the value of the subsidy given that households scheduled an audit. We are confident in the quality of our data since any intall must occur after an audit in order to be eligible for reimbursment, which is sustantial in our study area. We note that treatment induced 88 households to call in and schedule an audit. Further, unconditional means show that households which scheduled an audit in the same month that they received the letter ended up having an installation rate of roughly 33% of households that self-selected into having an audit without treatment (22% vs. 65% respectively).

Table 9 shows the probability of making any installation conditional on a household having had an audit. Households induced into having an audit by comparisons appear significantly less likely at making installations than the control group of untreated households. The negative effect is significant and strong for induced households above median electricity use in their local area. The estimated constant term represents the probability (.55%) a self-selected household makes any installation after an audit when controlling for month-by-year-by-zip-code.

We conclude that while nudges can influence audits, they are less successful at influence actual installations. This result is consistent with an information story: households who get an audit due to private comparison frames appear to do so to save electricity. However, upon having an audit, these households do not make installations. As a result, the expected energy saving attributable to the information conveyed during the audit is not sufficient to induce installations. This is different than what happens when households schedule an audit for themselves since install probabilities are higher for those households. Importantly, none of these effects appear to operate through the moral component of utility. Taken together, then, this raise concerns of the additionality of energy savings attributable to audits. Our evidence is consistent with households who schedule an audit knowing ex ante that they are likely to make installations. Table 10 shows a similar story while limiting installs to being only those within 60 days of an audit (hence the lower estimated constant term). Still, though, Table 11 shows that installs are associated with marginally significant decreases in electricity use. Table 2 in the appendix shows not signficant difference in installs on use by induced versus self-selected audited households.

6 Discussion

There are three main contributions of this study. First, consistent with the priming literature signal framing matters for microeconomic decision making (Chen and Li (2009) and Chen, Harper, and Li (2010)). Further, these effects vary by household characteristics. This highlights the importance of taking care in framing non-pecuniary signals for firms and policy makers trying to induce different microeconomic decisions. Lastly, we observe evidence that either “expenditure” or “good” salience can drive increases in use and that those increases vary by household type. Another explanation is spite that is correlated with political party. The precise mechanism for this effect is unclear given our design but more research is needed in order to identify the mechanism behind the systematic variation in this unexpected treatment effect. Second, frames which induce electricity conservation along the intensive margin are not the same as those which induce the same behavior on the extensive margin. While public good frames induce use conservation by republic households, private consumption utility frames induce audit signups. The mechanism behind this extensive margin effect appears to be a purely informational effect, rather than a social norm effect, since households induced into audit sign ups don't make installations at the same rate as self-selected households. Third, we find that the dollar value of an informative private consumption comparison frame on audit uptake probability is on the order of $40. However, the effect is only present for audit uptake and not actual installations. As a result, mapping signals to dollars is more complicated than a cursory analysis would suggest. More research along this dimension is needed.

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Table 1: Impact of Treatment Letter on Average Daily Electricity Use - All Waves

	1	2	3	4	5
Any Treatment	0.019***	0.017***	0.025***	0.016***	0.026***
	(0.004)	(0.005)	(0.007)	(0.005)	(0.007)
KWH Comparison	0.009*	0.011	0.003	0.011*	0.009
	(0.005)	(0.007)	(0.009)	(0.007)	(0.009)
Expenditure Comparison	0.001	0.002	0.000	0.004	-0.002
	(0.005)	(0.006)	(0.008)	(0.006)	(0.009)
CO_2 Comparison	0.012	0.018	-0.005	0.025*	-0.007
	(0.010)	(0.015)	(0.011)	(0.015)	(0.011)
Constant	3.787***	4.071***	3.435***	4.098***	3.395***
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
Fixed Effects	Household; Month-Year	Household; Month-Year	Household; Month-Year	Household; Month-Year	Household; Month-Year
Sample	Full Sample	Above Local Median	Below Local Median	Above MSA Median	Below MSA Median
R^2	0.626	0.583	0.470	0.565	0.449
N	1563607	798033	615454	804912	603925

Note: Dependent variable is average daily electricity use of household *i* in billing period *t*.

Any Treatment is an indicator for receiving any of the social comparison letters or an information-only letter.

KWH Comparison is an indicator for receiving a social comparison letter with units all in kWh.

Expenditure Comparison is an indicator for receiving a social comparison letter with units all in dollars.

CO_2 Comparison is an indicator for receiving a social comparison letter with units all in pounds of CO_2.

Column 1 is estimated on the full sample of households.

Column 2 is estimated on the restricted sample of households with average pre-experiment use below the median in their local area.

Column 3 is estimated on the restricted sample of households with average pre-experiment use above the median in their local area.

Similarly, Column 4 and 5 are estimated on a sample restricted to houses with average pre-experiment use below the median in the entire MSA and above the median in the entire MSA, respectively

All standard errors clustered at the household level.

We exclude households with audits prior to start of the observation period. Neither above- nor below-median models include households within the median (fifth) decile.

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 2: Impact of Treatment Letter on Average Daily Electricity Use - All Waves for Each Local-Area Decile

Any Treatment	0.052***	0.028**	0.008	0.011	0.009	0.018*	-0.001	0.006	0.034***	0.025**
	(0.014)	(0.014)	(0.014)	(0.012)	(0.011)	(0.011)	(0.012)	(0.011)	(0.011)	(0.010)
KWH Comparison	-0.024	0.005	0.031*	0.001	0.025	0.001	0.028*	0.030**	-0.001	-0.005
	(0.018)	(0.018)	(0.018)	(0.016)	(0.016)	(0.015)	(0.016)	(0.015)	(0.014)	(0.014)
Expenditure Comparison	-0.031*	-0.019	0.015	0.038***	-0.009	0.006	0.016	0.021	-0.017	-0.014
	(0.017)	(0.019)	(0.017)	(0.015)	(0.016)	(0.014)	(0.016)	(0.014)	(0.014)	(0.013)
CO_2 Comparison	-0.038	-0.010	0.017	0.011	0.046*	-0.005	0.020	0.062	-0.012	0.009
	(0.023)	(0.021)	(0.023)	(0.023)	(0.028)	(0.018)	(0.019)	(0.039)	(0.020)	(0.014)
Constant	3.243***	3.380***	3.515***	3.609***	3.716***	3.821***	3.907***	4.028***	4.166***	4.427***
	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Sample	1st Decile	2nd Decile	3rd Decile	4th Decile	5th Decile	6th Decile	7th Decile	8th Decile	9th Decile	10th Decile
R^2	0.448	0.432	0.438	0.441	0.442	0.449	0.461	0.473	0.500	0.606
N	160240	149056	152702	153456	150120	158887	158393	157971	160848	161934

Note: Dependent variable is average daily electricity use of household *i* in billing period *t*.

Any Treatment is an indicator for receiving any of the social comparison letters or an information-only letter.

KWH Comparison is an indicator for receiving a social comparison letter with units all in kWh.

Expenditure Comparison is an indicator for receiving a social comparison letter with units all in dollars.

CO_2 Comparison is an indicator for receiving a social comparison letter with units all in pounds of CO_2.

Columns 1-10 are estimated on the restricted sample of households with average pre-experiment use within the 1st through 10th decile, respectively, in their local area.

All columns include household fixed effects as well as month by year fixed effects. All standard errors clustered at the household level.

We exclude households with audits prior to start of the observation period.

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 3: Impact of Treatment Letter on Average Daily Electricity Use with Political Interactions - All Waves

	1	2	3	4	5
Any Treatment	-0.032***	-0.046***	-0.023**	-0.049***	-0.020*
	(0.007)	(0.010)	(0.011)	(0.010)	(0.011)
Any Treatment X GOP	0.093***	0.111***	0.090***	0.109***	0.083***
	(0.012)	(0.016)	(0.020)	(0.016)	(0.022)
Any Treatment X Dem	0.032	0.104*	-0.008	0.049	-0.025
	(0.038)	(0.061)	(0.053)	(0.060)	(0.051)
Any Treatment X Mixed	0.076***	0.082***	0.084***	0.084***	0.084***
	(0.008)	(0.011)	(0.013)	(0.011)	(0.014)
KWH Comparison	0.001	0.024*	-0.021	0.019	-0.012
	(0.010)	(0.015)	(0.015)	(0.015)	(0.015)
Expenditure Comparison	-0.003	0.005	-0.016	0.013	-0.017
	(0.010)	(0.014)	(0.015)	(0.014)	(0.015)
CO_2 Comparison	0.049***	0.046**	0.028	0.078***	0.015
	(0.016)	(0.020)	(0.019)	(0.027)	(0.018)
CO_2 Comparison X GOP	-0.061***	-0.067**	-0.026	-0.089***	-0.018
	(0.022)	(0.028)	(0.032)	(0.033)	(0.033)
CO_2 Comparison X Dem	-0.042	-0.021	-0.092	-0.002	-0.031
	(0.082)	(0.109)	(0.120)	(0.113)	(0.112)
CO_2 Comparison X Mixed	-0.054**	-0.037	-0.061**	-0.070**	-0.040*
	(0.021)	(0.028)	(0.025)	(0.034)	(0.024)
Constant	3.787***	4.071***	3.435***	4.098***	3.395***
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
Fixed Effects	Household; Month-Year	Household; Month-Year	Household; Month-Year	Household; Month-Year	Household; Month-Year
Sample	Full Sample	Above Local Median	Below Local Median	Above MSA Median	Below MSA Median
R^2	0.627	0.583	0.470	0.565	0.450
N	1563607	798033	615454	804912	603925

Note: Dependent variable is average daily electricity use of household i in billing period t.

Any Treatment is an indicator for receiving any of the social comparison letters or an information-only letter and having missing poitical data.

KWH Comparison is an indicator for receiving a social comparison letter with units all in kWh and having missing poitical data.

Expenditure Comparison is an indicator for receiving a social comparison letter with units all in dollars and having missing poitical data.

Interactions of Expenditure and KWH Comparisons with political variables suppressed to save space.

CO_2 Comparison is an indicator for receiving a social comparison letter with units all in pounds of CO_2 and having missing poitical data.

All interactions cross particular treatment indicators with whether all registered voters are registered Republicans (GOP), Democrats (Dem), or some mix (Mixed; potentially of neither major party).

Column 1 is estimated on the full sample of households.

Column 2 is estimated on the restricted sample of households with average pre-experiment use below the median in their local area.

Column 3 is estimated on the restricted sample of households with average pre-experiment use above the median in their local area.

Similarly, Column 4 and 5 are estimated on a sample restricted to houses with average pre-experiment use below the median in the entire MSA and above the median in the entire MSA, respectively

All standard errors clustered at the household level

Table 4: Energy Audit Uptake Rates During Experiment by Treatment Type

	No Subsidy	\$20 Subsidy	\$50 Subsidy	Total
No Comparison	1.34	2.19	1.71	1.86
KWH Comparison	1.84	2.91	3.78	2.86
Expenditure Comparison	2.52	2.73	2.22	2.55
CO2 Comparison	0.71	2.10	0.72	1.41
Total	1.90	2.54	2.31	2.29

Note: Table reports the ratio of average uptake of energy audits during a month treatment cell compared to the control group. The baseline rate for the control group is 0.00054, or a monthly audit rate of 0.05%. Uptake for households that receive some type of treatment is 129% above the control group.

Table 5: Impact of Treatment Letter and Subsidy on Audit Uptake - All Waves

	1	2	3	4	5
Any Treatment	0.0000	0.0002	0.0000	0.0003	0.0003
	(0.0003)	(0.0005)	(0.0004)	(0.0005)	(0.0005)
KWH Comparison	0.0006*	0.0011*	-0.0004	0.0008	0.0000
	(0.0003)	(0.0006)	(0.0003)	(0.0006)	(0.0005)
Expenditure Comparison	0.0007**	0.0006	0.0008	0.0006	0.0008
	(0.0003)	(0.0005)	(0.0005)	(0.0005)	(0.0006)
CO_2 Comparison	0.0002	0.0002	0.0002	0.0001	-0.0003
	(0.0004)	(0.0008)	(0.0005)	(0.0007)	(0.0003)
Subsidy Amount (10s USD)	0.0001	0.0002	-0.0000	0.0003	-0.0000
	(0.0002)	(0.0004)	(0.0003)	(0.0004)	(0.0004)
Subsidy Amount^2 (USD)	-0.0000	-0.0000	0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Constant	0.0002***	0.0002***	0.0002***	0.0002***	0.0002***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Fixed Effects	Month-Year-ZIP9	Month-Year-ZIP9	Month-Year-ZIP9	Month-Year-ZIP9	Month-Year-ZIP9
Sample	Full Sample	Above Local Median	Below Local Median	Above MSA Median	Below MSA Median
R^2	0.314	0.427	0.468	0.410	0.483
N	1374404	741807	500029	767226	469750

Note: Dependent variable is an indicator of whether household i got an audit in billing period t.
Any Treatment is an indicator for receiving any of the social comparison letters or an information-only letter.
KWH Comparison is an indicator for receiving a social comparison letter with units all in kWh.
Expenditure Comparison is an indicator for receiving a social comparison letter with units all in dollars.
CO_2 Comparison is an indicator for receiving a social comparison letter with units all in pounds of CO_2.
Column 1 is estimated on the full sample of households.
Column 2 is estimated on the restricted sample of households with average pre-experiment use below the median in their local area.
Column 3 is estimated on the restricted sample of households with average pre-experiment use above the median in their local area.
Similarly, Column 4 and 5 are estimated on a sample restricted to houses with average pre-experiment use below the median in the entire MSA and above the median in the entire MSA, respectively
All standard errors clustered at the household level.
We exclude: 1) households with audits prior to treatment or after the end of the observation period and 2) households identified as rentals.
Neither above- nor below-median models include households within the median (fifth) decile.
*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 6: Impact of Treatment Letter and Subsidy on Audit Uptake with Political Interactions - All Waves

	1	2	3	4	5
Any Treatment	0.0003	0.0010	-0.0004	0.0009	-0.0005
	(0.0004)	(0.0010)	(0.0004)	(0.0010)	(0.0004)
Any Treatment X GOP	-0.0008	-0.0026	0.0008	-0.0026*	0.0022
	(0.0012)	(0.0016)	(0.0020)	(0.0015)	(0.0023)
Any Treatment X Dem	0.0002	-0.0003	0.0000	-0.0003	0.0003
	(0.0012)	(0.0029)	(0.0010)	(0.0026)	(0.0010)
Any Treatment X Mixed	-0.0003	-0.0007	0.0006	-0.0006	0.0010
	(0.0005)	(0.0012)	(0.0006)	(0.0011)	(0.0008)
KWH Comparison	0.0003	0.0008	0.0002	0.0009	0.0003
	(0.0006)	(0.0017)	(0.0004)	(0.0016)	(0.0005)
KWH Comparison X GOP	0.0026	0.0045	-0.0012	0.0036	0.0028
	(0.0017)	(0.0034)	(0.0022)	(0.0028)	(0.0037)
KWH Comparison X Dem	-0.0003	0.0000	-0.0003	-0.0003	-0.0004
	(0.0007)	(0.0025)	(0.0004)	(0.0023)	(0.0005)
KWH Comparison X Mixed	0.0000	-0.0001	-0.0009	-0.0007	-0.0008
	(0.0007)	(0.0018)	(0.0006)	(0.0017)	(0.0007)
Expenditure Comparison	-0.0003	-0.0008	0.0002	-0.0007	0.0003
	(0.0004)	(0.0009)	(0.0004)	(0.0009)	(0.0005)
Expenditure Comparison X GOP	0.0022*	0.0025	0.0039	0.0035*	0.0021
	(0.0013)	(0.0016)	(0.0034)	(0.0019)	(0.0029)
Expenditure Comparison X Dem	-0.0030	-0.0058	-0.0003	-0.0057	-0.0004
	(0.0059)	(0.0133)	(0.0004)	(0.0129)	(0.0005)
Expenditure Comparison X Mixed	0.0012**	0.0018*	0.0003	0.0015	0.0006
	(0.0006)	(0.0011)	(0.0006)	(0.0011)	(0.0008)
CO_2 Comparison	-0.0001	-0.0005	0.0001	-0.0002	0.0001
	(0.0007)	(0.0016)	(0.0006)	(0.0016)	(0.0007)
Constant	0.0002***	0.0002***	0.0001***	0.0002***	0.0001**
	(0.0000)	(0.0001)	(0.0001)	(0.0000)	(0.0001)
Fixed Effects	Month-Year-ZIP9	Month-Year-ZIP9	Month-Year-ZIP9	Month-Year-ZIP9	Month-Year-ZIP9
Sample	Full Sample	Above Local Median	Below Local Median	Above MSA Median	Below MSA Median
R^2	0.314	0.427	0.468	0.410	0.483
N	1374404	741807	500029	767226	469750

Note: Dependent variable is an indicator of whether household i got an audit in billing period t.

Any Treatment is an indicator for receiving any of the social comparison letters or an information-only letter and having missing poitical data.

KWH Comparison is an indicator for receiving a social comparison letter with units all in kWh and having missing poitical data.

Expenditure Comparison is an indicator for receiving a social comparison letter with units all in dollars and having missing poitical data.

CO_2 Comparison is an indicator for receiving a social comparison letter with units all in pounds of CO_2 and having missing poitical data.

All interactions cross particular treatment indicators with whether all registered voters are registered Republicans (GOP), Democrats (Dem), or some mix (Mixed; potentially of neither major party).

Subsidy amounts, subsidy amounts interacted with household voter type and CO2 interacted with voter type suppressed to save space.

Table 7: Impact of Treatment Letter on Average Daily Electricity Use Prior to Audit - All Waves

	1	2	3	4	5
Any Treatment	0.026	0.083	-0.027	0.044	-0.003
	(0.081)	(0.107)	(0.126)	(0.102)	(0.142)
KWH Comparison	0.052	0.018	0.079	0.039	0.013
	(0.091)	(0.123)	(0.134)	(0.119)	(0.155)
Expenditure Comparison	-0.140	-0.282*	0.043	-0.274	-0.034
	(0.114)	(0.168)	(0.136)	(0.170)	(0.157)
CO_2 Comparison	-0.067	-0.048	-0.306	-0.001	-0.306
	(0.304)	(0.191)	(0.423)	(0.190)	(0.433)
Constant	3.811***	4.090***	3.413***	4.086***	3.308***
	(0.025)	(0.029)	(0.043)	(0.030)	(0.047)
Fixed Effects	Household; Month-Year	Household; Month-Year	Household; Month-Year	Household; Month-Year	Household; Month-Year
Sample	Full Sample	Above Local Median	Below Local Median	Above MSA Median	Below MSA Median
R^2	0.675	0.636	0.569	0.622	0.544
N	5086	3087	1654	3107	1630

Note: Dependent variable is average daily electricity use of household i in billing period t.
Any Treatment is an indicator for receiving any of the social comparison letters or an information-only letter.
KWH Comparison is an indicator for receiving a social comparison letter with units all in kWh.
Expenditure Comparison is an indicator for receiving a social comparison letter with units all in dollars.
CO_2 Comparison is an indicator for receiving a social comparison letter with units all in pounds of CO_2.
Column 1 is estimated on the full sample of households.
Column 2 is estimated on the restricted sample of households with average pre-experiment use below the median in their local area.
Column 3 is estimated on the restricted sample of households with average pre-experiment use above the median in their local area.
Similarly, Column 4 and 5 are estimated on a sample restricted to houses with average pre-experiment use below the median in the entire MSA and above the median in the entire MSA, respectively
All standard errors clustered at the household level.
We exclude households with audits prior to treatment or after the end of the observation period. Neither above- nor below-median models include households within the median (fifth) decile.
*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 8: Impact of Treatment Letter on Average Daily Electricity Use Prior to Audit - All Waves

	1	2	3	4	5
Any Treatment	0.025	0.081	-0.034	0.044	-0.009
	(0.081)	(0.107)	(0.126)	(0.102)	(0.142)
Comparison	-0.051	-0.129	-0.003	-0.110	-0.067
	(0.096)	(0.124)	(0.154)	(0.121)	(0.171)
Constant	3.811***	4.091***	3.411***	4.087***	3.307***
	(0.025)	(0.029)	(0.043)	(0.031)	(0.046)
Fixed Effects	Household; Month-Year	Household; Month-Year	Household; Month-Year	Household; Month-Year	Household; Month-Year
Sample	Full Sample	Above Local Median	Below Local Median	Above MSA Median	Below MSA Median
R^2	0.675	0.635	0.567	0.621	0.542
N	5086	3087	1654	3107	1630

Note: Dependent variable is average daily electricity use of household i in billing period t.
Any Treatment is an indicator for receiving any of the social comparison letters or an information-only letter.
Comparison is an indicator for receiving a social comparison letter, irrespective of units.
Column 1 is estimated on the full sample of households.
Column 2 is estimated on the restricted sample of households with average pre-experiment use below the median in their local area.
Column 3 is estimated on the restricted sample of households with average pre-experiment use above the median in their local area.
Similarly, Column 4 and 5 are estimated on a sample restricted to houses with average pre-experiment use below the median in the entire MSA and above the median in the entire MSA, respectively
All standard errors clustered at the household level.
We exclude households with audits prior to treatment or after the end of the observation period. Neither above- nor below-median models include households within the median (fifth) decile.
*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 9: Impact of Treatment Letter and Subsidy on Installs Ever (Conditional on Audit) - All Waves

	1	2	3	4	5
Any Treatment	-0.2053	-0.1763	-0.0625	-0.1808	-0.0259
	(0.1460)	(0.1853)	(0.1460)	(0.2096)	(0.3003)
KWH Comparison	-0.2118	-0.3182*	-0.9065***	-0.2910	-0.8053***
	(0.1414)	(0.1865)	(0.1517)	(0.1916)	(0.2305)
Expenditure Comparison	-0.2447*	-0.3451**	0.2556	-0.3240**	0.1512
	(0.1329)	(0.1495)	(0.2475)	(0.1617)	(0.2460)
CO_2 Comparison	0.0731	-0.0907	0.2065	-0.0863	0.1644
	(0.1748)	(0.2918)	(0.2224)	(0.2970)	(0.1934)
Subsidy Amount (10s USD)	0.0583	0.0351	-0.0874	0.0675	-0.1006
	(0.1004)	(0.1397)	(0.1524)	(0.1498)	(0.2214)
subsidy_amount_squ_ever	-0.0001	0.0000	0.0001	-0.0000	0.0001
	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Constant	0.6647***	0.6349***	0.7509***	0.6109***	0.7739***
	(0.0366)	(0.0539)	(0.0441)	(0.0561)	(0.0480)
Fixed Effects	Install Month-Year by ZIP5	Install Month-Year by ZIP5	Install Month-Year by ZIP5	Install Month-Year by ZIP5	Install Month-Year; by ZIP5
Sample	Full Sample	Above Local Median	Below Local Median	Above MSA Median	Below MSA Median
R^2	0.553	0.663	0.893	0.641	0.866
N	321	205	93	200	96

Note: Dependent variable is an indicator for households that made 1 or more energy-efficient installations at some point during the experiment and post-treatment, conditional on getting an audit.
Any Treatment is an indicator for receiving any of the social comparison letters or an information-only letter during the study.
KWH Comparison is an indicator for receiving a social comparison letter with units all in kWh during the study.
Expenditure Comparison is an indicator for receiving a social comparison letter with units all in dollars during the study.
CO_2 Comparison is an indicator for receiving a social comparison letter with units all in pounds of CO_2 during the study.
Column 1 is estimated on the full sample of households.
Column 2 is estimated on the restricted sample of households with average pre-experiment use below the median in their local area.
Column 3 is estimated on the restricted sample of households with average pre-experiment use above the median in their local area.
Similarly, Column 4 and 5 are estimated on a sample restricted to houses with average pre-experiment use below the median in the entire MSA and above the median in the entire MSA, respectively
All standard errors are robust.
We exclude: 1) households with audits prior to treatment or after the end of the observation period and 2) households identified as rentals.
Neither above- nor below-median models include households within the median (fifth) decile.
*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 10: Impact of Treatment Letter and Subsidy on Installs Ever Within 60 Days of Audit (Conditional on Audit) - All Waves

	1	2	3	4	5
Any Treatment	-0.2452**	-0.2340	0.1650	-0.2298	-0.0123
	(0.1164)	(0.1859)	(0.2851)	(0.1781)	(0.2974)
KWH Comparison	-0.0814	-0.0513	-0.6454	-0.0489	-0.5123
	(0.1431)	(0.1799)	(0.4048)	(0.1851)	(0.3225)
Expenditure Comparison	-0.1817	-0.2122	0.0361	-0.1802	0.0771
	(0.1285)	(0.1385)	(0.4255)	(0.1385)	(0.3336)
CO_2 Comparison	-0.0394	-0.1543	0.4694	-0.1550	0.4945
	(0.2026)	(0.2817)	(0.5334)	(0.2836)	(0.4526)
Subsidy Amount (10s USD)	0.0645	0.0618	-0.0459	0.0681	0.0953
	(0.0843)	(0.1256)	(0.2172)	(0.1186)	(0.2018)
subsidy_amount_squ_ever	-0.0001	-0.0001	-0.0000	-0.0001	-0.0003
	(0.0001)	(0.0002)	(0.0004)	(0.0002)	(0.0003)
Constant	0.3649***	0.3576***	0.3450***	0.3489***	0.3192***
	(0.0371)	(0.0534)	(0.0742)	(0.0532)	(0.0769)
Fixed Effects	Install Month-Year by ZIP5	Install Month-Year by ZIP5	Install Month-Year by ZIP5	Install Month-Year by ZIP5	Install Month-Year; by ZIP5
Sample	Full Sample	Above Local Median	Below Local Median	Above MSA Median	Below MSA Median
R^2	0.412	0.556	0.783	0.565	0.715
N	321	205	93	200	96

Note: Dependent variable is an indicator for households that made 1 or more energy-efficient installations at some point during the experiment and post-treatment, conditional on getting an audit.
Any Treatment is an indicator for receiving any of the social comparison letters or an information-only letter during the study.
KWH Comparison is an indicator for receiving a social comparison letter with units all in kWh during the study.
Expenditure Comparison is an indicator for receiving a social comparison letter with units all in dollars during the study.
CO_2 Comparison is an indicator for receiving a social comparison letter with units all in pounds of CO_2 during the study.
Column 1 is estimated on the full sample of households.
Column 2 is estimated on the restricted sample of households with average pre-experiment use below the median in their local area.
Column 3 is estimated on the restricted sample of households with average pre-experiment use above the median in their local area.
Similarly, Column 4 and 5 are estimated on a sample restricted to houses with average pre-experiment use below the median in the entire MSA and above the median in the entire MSA, respectively
All standard errors are robust.
We exclude: 1) households with audits prior to treatment or after the end of the observation period and 2) households identified as rentals.
Neither above- nor below-median models include households within the median (fifth) decile.
*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 11: Impact of Install on Log Electricity Use with Political Interactions - All Waves

	1	2	3	4	5
Post Install	-0.128	-0.192*	-0.016	-0.190*	-0.148
	(0.136)	(0.111)	(0.309)	(0.113)	(0.323)
Post Install X GOP	0.114	0.120	0.063	0.126	0.186
	(0.144)	(0.123)	(0.318)	(0.124)	(0.332)
Post Install X Dem	0.081		-0.067		0.051
	(0.137)		(0.309)		(0.324)
Post Install X Mixed	0.152	0.157	0.029	0.172	0.233
	(0.147)	(0.133)	(0.322)	(0.137)	(0.337)
Constant	7.219***	7.495***	6.834***	7.511***	6.777***
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
Fixed Effects	Household; Month-Year	Household; Month-Year	Household; Month-Year	Household; Month-Year	Household; Month-Year
Sample	Full Sample	Above Local Median	Below Local Median	Above MSA Median	Below MSA Median
R^2	0.611	0.562	0.461	0.548	0.444
N	1374404	741807	500029	767226	469750

Note: Dependent variable is log total electricity use of household i in billing period t.

All interactions cross a post-install indicator with whether all registered voters are registered Republicans (GOP), Democrats (Dem), or some mix (Mixed; potentially of neither major party).

Column 1 is estimated on the full sample of households.

Column 2 is estimated on the restricted sample of households with average pre-experiment use below the median in their local area.

Column 3 is estimated on the restricted sample of households with average pre-experiment use above the median in their local area.

Similarly, Column 4 and 5 are estimated on a sample restricted to houses with average pre-experiment use below the median in the entire MSA and above the median in the entire MSA, respectively

All standard errors clustered at the household level.

We exclude: 1) households with audits prior to treatment or after the end of the observation period and 2) households identified as rentals.

Neither above- nor below-median models include households within the median (fifth) decile.

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

7 Appendix

COMPANY LOGO HERE

Month Day, 2013

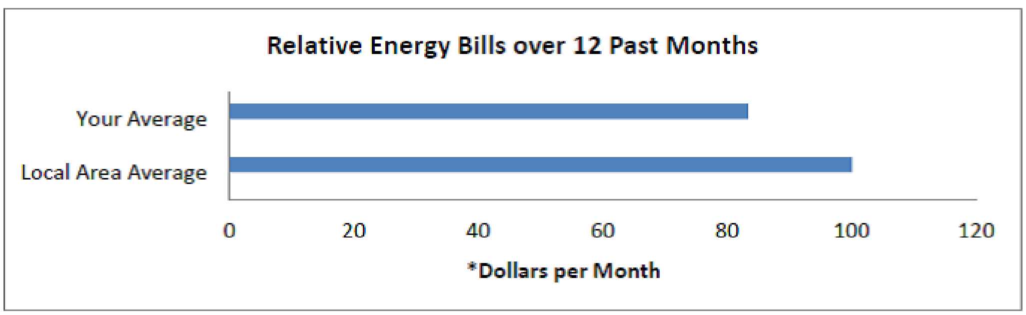
Dear Valued Customer,

There's no place like home, and there's no time like now to make your home more energy efficient. You can conserve energy, save on utility bills, and get cash rebates by participating in [redacted] program. If you qualify, you can also use on-bill financing to pay for IHEE improvements.

If you sign up for an [redacted] will visit your home at a time convenient for you. The advisor will recommend cost-effective ways to increase your home's energy efficiency and will install free CFLs and low-flow water saving measures if you choose.

The [redacted] evaluation fee is $150 (currently with an instant rebate of $100). And you will receive the remaining $50 fee back if you spend $150 or more on qualifying improvements. You will also receive matching rebates of up to $500 for installing eligible improvements. As an additional thank you for participating, if you have an [redacted] within 30 days from the date of this letter you will receive a $XX Visa gift card.

We thought that you might be interested in the following information about your energy bills last year:



**Dollars per month calculated at $.XX per kWh*

Your Average Energy Bill	$82.00
Local Area Homes' Average Energy Bill	$100.00

You consumed xx% more/less energy than other local area homes.

For more information about the [redacted] program, including on-bill financing, call [redacted]. You can also find more information about the program and details about qualifying improvements by following the [redacted] link on the [redacted] home page.

Sincerely,

[redacted]

Ex2123

Table 1: Impact of Treatment Letter and Subsidy on Audit Uptake - All Waves; Examining Different Fixed Effects

	1	2	3	4	5
Any Treatment	0.0001	0.0002	0.0003	0.0001	0.0000
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)
KWH Comparison	0.0005*	0.0005*	0.0006*	0.0005*	0.0006*
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Expenditure Comparison	0.0006**	0.0006**	0.0006**	0.0006**	0.0007**
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
CO2 Comparison	-0.0000	-0.0000	0.0000	-0.0000	0.0002
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)
Subsidy Amount (10s USD)	0.0002	0.0002	0.0002	0.0002	0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Subsidy Amount^2 (USD)	-0.0000	-0.0000	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Constant	0.0002***	0.0002***	0.0002***	0.0002***	0.0002***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Fixed Effects	Month-Year	ZIP5	ZIP9	Month-Year-ZIP5	Month-Year-ZIP9
Sample	Full Sample	Full Sample	Full Sample	Full Sample	Full Sample
R^2	0.000	0.000	0.021	0.001	0.314
N	1374404	1374404	1374404	1374404	1374404

Note: Dependent variable is an indicator of whether household i got an audit in billing period t.
Any Treatment is an indicator for receiving any of the social comparison letters or an information-only letter.
KWH Comparison is an indicator for receiving a social comparison letter with units all in kWh.
Expenditure Comparison is an indicator for receiving a social comparison letter with units all in dollars.
CO2 Comparison is an indicator for receiving a social comparison letter with units all in pounds of CO2.
Column 1 is estimated on the full sample of households.
Column 2 is estimated on the restricted sample of households with average pre-experiment use below the median in their local area.
Column 3 is estimated on the restricted sample of households with average pre-experiment use above the median in their local area.
Similarly, Column 4 and 5 are estimated on a sample restricted to houses with average pre-experiment use below the median in the entire MSA and above the median in the entire MSA, respectively
All standard errors clustered at the household level.
We exclude: 1) households with audits prior to treatment or after the end of the observation period and 2) households identified as rentals.
*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 2: Impact of Induced vs. Self-Selected Audit on Average Daily Electricity Use (Conditional on No Installs) with Political Interactions - All Waves

	1	2	3	4	5
Post Audit	0.049	0.032	0.038	0.033	-0.193**
	(0.151)	(0.261)	(0.188)	(0.259)	(0.077)
Post Audit X GOP	-0.024	-0.140	0.072	-0.068	0.300**
	(0.203)	(0.357)	(0.234)	(0.330)	(0.153)
Post Audit X Dem	-0.034	-0.003		-0.001	
	(0.151)	(0.261)		(0.259)	
Post Audit X Mixed	-0.037	-0.021	0.022	-0.012	0.259**
	(0.156)	(0.265)	(0.201)	(0.262)	(0.104)
Induced Audit	0.070	0.123	-0.414**	0.128	0.010
	(0.157)	(0.263)	(0.188)	(0.261)	(0.097)
Induced Audit X GOP	-0.045	0.093	0.230	-0.001	-0.164
	(0.214)	(0.364)	(0.261)	(0.338)	(0.196)
Induced Audit X Dem	-0.169	-0.230		-0.239	
	(0.157)	(0.263)		(0.261)	
Induced Audit X Mixed	-0.052	-0.110	0.457**	-0.123	
	(0.165)	(0.270)	(0.210)	(0.269)	
Constant	3.818***	4.092***	3.435***	4.108***	3.378***
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
Fixed Effects	Household; Month-Year	Household; Month-Year	Household; Month-Year	Household; Month-Year	Household; Month-Year
Sample	Full Sample	Above Local Median	Below Local Median	Above MSA Median	Below MSA Median
R^2	0.635	0.581	0.482	0.566	0.465
N	1371622	740220	499038	765666	468700

Note: Dependent variable is average daily electricity use of household *i* in billing period *t*.

All interactions cross post-audit or induced-audit indicators with whether all registered voters are registered Republicans (GOP), Democrats (Dem), or some mix (Mixed; potentially of neither major party).

Column 1 is estimated on the full sample of households.

Column 2 is estimated on the restricted sample of households with average pre-experiment use below the median in their local area.

Column 3 is estimated on the restricted sample of households with average pre-experiment use above the median in their local area.

Similarly, Column 4 and 5 are estimated on a sample restricted to houses with average pre-experiment use below the median in the entire MSA and above the median in the entire MSA, respectively

All standard errors clustered at the household level.

We exclude: 1) households with audits prior to treatment or after the end of the observation period and 2) households identified as rentals.

Neither above- nor below-median models include households within the median (fifth) decile.

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.