ENERGY INSTITUTE AT HAAS

EI @ Haas WP 257

The Persistence of Moral Suasion and Economic Incentives: Field Experimental Evidence from Energy Demand

Koichiro Ito, Takanori Ida, and Makoto Tanaka

Revised April 2015

Revised version in the *American Economic Journal: Economic Policy* vol. 10.1: 1-29

The Persistence of Moral Suasion and Economic Incentives: Field Experimental Evidence from Energy Demand*

Koichiro Ito — Boston University and NBER

Takanori Ida — Kyoto University

Makoto Tanaka — GRIPS

This version: April 1, 2015

Abstract

Firms and governments often use moral suasion and economic incentives to influence intrinsic and extrinsic motivations for various economic activities. To investigate the persistence of such interventions, we randomly assigned households to moral suasion and dynamic pricing that stimulate energy conservation during peak demand hours. Using household-level consumption data for 30-minute intervals, we find significant short-run effects of moral suasion, but the effects diminished quickly after repeated interventions. Economic incentives produced larger and persistent effects, which induced habit formation after the final interventions. While each policy produces substantial welfare gains, economic incentives provide particularly large gains when we consider persistence.

*Ito: School of Management, Boston University, 595 Commonwealth Avenue, Boston, MA 02215, and NBER (e-mail: ito@bu.edu). Ida: Graduate School of Economics, Kyoto University, Yoshida, Sakyo, Kyoto 606-8501, Japan (e-mail: ida@econ.kyoto-u.ac.jp). Tanaka: National Graduate Institute for Policy Studies, 7-22-1 Roppongi, Minato-ku, Tokyo 106-8677, Japan (e-mail: mtanaka@grips.ac.jp).

The authors would like to thank Tetsuya Kawamura for excellent research assistance. We thank Hunt Allcott, Masahiko Aoki, Severin Borenstein, Lucas Davis, Liran Einav, Meredith Fowlie, Takeo Hoshi, Caroline Hoxby, Katrina Jessoe, Paul Joskow, Ryan Kellogg, Chris Knittel, David Laibson, Michael Price, Steve Puller, Matthew Rabin, Dave Rapson, Catherine Wolfram, Frank Wolak, and seminar participants at MIT, Harvard, Stanford, UC Berkeley, UC Davis, University of Tokyo, the NBER Summer Institute, the AEA Annual Meeting, the POWER Conference on Energy Research and Policy, and the RIETI for helpful comments. We thank the Japanese Ministry of Economy, Trade and Industry, the prefecture of Kyoto, Kansai Electric Power Company, and Mitsubishi Heavy Industries, Ltd. for their collaboration on this study. We thank the New Energy Promotion Council for financial support. Ito thanks the Energy Institute at Haas and the Stanford Institute for Economic Policy Research for financial support.

1 Introduction

Recent economic theory emphasizes that both intrinsic and extrinsic motivations play important roles in economic decisions (Kreps, 1997; Bénabou and Tirole, 2003, 2006). For example, consider individuals who allocate their time and resources to prosocial behavior such as charitable giving, blood donations, and energy conservation. These decisions can be driven by intrinsic motivation such as warm glow (Andreoni, 1989) or by extrinsic motivation such as financial incentives. A growing number of firms and regulators recognize the importance of intrinsic and extrinsic motivations and design their policies to influence these motivations for a variety of economic activities—increasing donations, promoting smoking cessation, motivating people to exercise, improving academic refereeing processes, enhancing worker productivity, and encouraging conservation of energy, water, and other scarce resources.[1]

A central question for economists, firms, and regulators designing such policies is whether they can generate robust and persistent effects by appealing to intrinsic and extrinsic motivations (Gneezy, Meier and Rey-Biel, 2011). Many studies in psychology emphasize that there are important differences between short-run and long-run psychological processes (Gneezy and List, 2006). For example, individuals may have *hot* versus *cold* decision-making—immediate reactions to an event, called *hot* decision-making, can differ from decision-making in the *cold* phase, which follows the hot phase (Loewenstein, 2005). Another example is potential *habit formation* that can be induced by policy interventions; short-run shocks to current consumption patterns may form a habit pertaining to consumption patterns in the subsequent time periods (Becker and Murphy, 1988). In theory, these factors could interact with intrinsic and extrinsic motivations differently. The resulting differences, if any, give rise to differences in welfare implications between policies targeting intrinsic and extrinsic motivations. Although the answer to this question is key to a variety of economic policies, we are not aware of existing studies that examine the persistence of these two policy interventions in a unified field experimental framework.

In this paper, we use a randomized field experiment to investigate whether policymakers can

[1] A vast amount of literature provides empirical evidence of these policies, including improving academic refereeing (Chetty, Saez and Sándor, 2014), increasing blood donations (Lacetera, Macis and Slonim, 2012), charitable giving (Gneezy and Rustichini, 2000; Landry et al., 2006; Ariely, Bracha and Meier, 2009; Landry et al., 2010), energy conservation (Reiss and White, 2008), motivating people to exercise (Charness and Gneezy, 2009), smoking cessation (Volpp et al., 2009), and tax evasion (Dwenger et al., 2014).

generate robust and persistent effects by appealing to intrinsic and extrinsic motivations. We consider two policy interventions that are most widely used by policymakers in practice. The first intervention is moral suasion, by which policymakers attempt to influence intrinsic motivation for various economic activities.[2] The second intervention is an economic incentive, by which policymakers attempt to influence extrinsic motivation based on standard demand theory. Our main outcome variable is household-level electricity consumption for every 30 minutes. We began by randomly assigning households to one of 3 groups: 1) a moral suasion group, 2) an economic incentive group, and 3) a control group. On peak demand days in summer and winter, we delivered day-ahead and same-day notifications about treatments. For electricity usage during peak demand hours on peak demand days, the moral suasion group received a message requesting voluntary energy conservation with no economic incentives. The economic incentive group was charged high electricity prices during the peak demand hours. We repeated these interventions to analyze *hot* versus *cold* decision-making among the groups. The repeated interventions allowed us to estimate how treatment effects change between the first intervention and subsequent interventions. Moreover, we collected electricity usage data after the final interventions to examine potential habit formation. Finally, we conducted a detailed follow-up survey to investigate the mechanism behind our findings.

We present several findings from the experiment. First, moral suasion induced short-run reductions in electricity usage, but the effect diminished quickly over repeated interventions. The moral suasion group showed a usage reduction of 8 percent for the first few treatment days. However, their usage became statistically indistinguishable from that of the control group for the remaining interventions. The moral suasion effects for the first few treatment days were statistically different from those for the remaining interventions. Second, we found that economic incentives created much larger and persistent effects. The economic incentive group showed usage reductions of 14 percent for the lowest critical peak price and usage reductions of 17 percent for the highest critical peak price. Moreover, the effect was persistent over repeated interventions. Third, we tested whether there was a spillover effect on consumption during nontreatment hours of the treatment days. Our

[2] Moral suasion is widely used in many economic policies in practice. For example, regulators often use moral suasion to encourage energy conservation, including the policies implemented in the United States (Reiss and White, 2008), Brazil (Gerard, 2013), and Japan. "Spare the Air" program in California (Cutter and Neidell, 2009) is a moral suasion policy for mitigating air pollution. In addition, Dal Bó and Dal Bó (2014) note that moral suasion is widely used in many places including educational institutions and work places to incentivize workers.

treatments were specifically targeted to electricity usage during peak demand hours. However, the interventions can also affect usage in nontreatment hours on the treatment days. For the moral suasion group, we do not find such spillover effects. In contrast, there were significant spillover effects, namely, usage reductions during the nontreatment hours, for the economic incentive group. Fourth, we tested potential habit formation by estimating treatment effects using data collected after we withdrew the treatments. We found significant habit formation for the economic incentive group and no habit formation for the moral suasion group. After we withdrew the treatment, the moral suasion group's usage was indistinguishable from that of the control group. On the other hand, the economic incentive group continued to practice energy conservation even after we withdrew the incentives.

What drives the substantially different results for the moral suasion and economic incentive groups? We investigate two potential channels. The first possibility is durable goods investments—households purchased energy-efficient appliances in response to the treatments. If such an effect was systematically large for the economic incentive group, it could explain the persistent usage reductions. The second possibility is behavioral changes in lifestyles. Suppose that some customers had "bad habits" of inefficient energy use at home before we began our experiment. Our interventions may have acted as a trigger to change their lifestyles, thus helping them form good habits, namely, efficient energy use. If such an effect was systematically large for the economic incentive group, it could explain the persistent usage reductions. Using follow-up survey data, we find no statistical evidence for the first hypothesis. In contrast, we find supporting evidence for the second hypothesis. Our data indicate that the economic incentives induced behavioral changes in lifestyles—households in this group formed a habit of efficient energy use for a variety of electric appliances including air conditioners, heaters, computers, washers, and cleaners. Although these results are based on stated survey responses, they provide suggestive evidence about the mechanisms behind our findings.

The findings from the experiment provide important policy implications. Our results indicate that economic policies can have significantly different impacts depending on how policymakers design policy instruments in relation to intrinsic and extrinsic motivations of consumer behavior, particularly when we take account of persistence. To highlight this policy implication, we provide a welfare analysis in the context of electricity markets. One of the largest inefficiencies in electricity markets originates from the fact that retail electricity prices generally do not reflect the marginal cost of electricity supply—consumers pay time-invariant prices although the marginal cost of electricity supply tends to be extremely high in "critical peak hours," which are peak demand hours on peak demand days. This inefficiency has long been discussed by economists and forms a key policy issue in recent energy policies of many countries, particularly because the recent technological progress in "smart meters" may be able to offer a potential solution for the problem through providing dynamic economic incentives or sending real-time messages of moral suasion for voluntary conservation (Wolak, 2011; Joskow, 2012; Joskow and Wolfram, 2012; Jessoe and Rapson, 2014). Our welfare analysis implies that the moral suasion policy can provide significant welfare gains. However, the welfare gains are likely to diminish quickly when we repeat the interventions. In contrast, the economic incentive policy can produce larger welfare improvement, particularly when we consider repeated interventions. Our analysis suggests that a lower bound estimate of the welfare gain for the Japanese electricity market per summer is $77 million for the economic incentive policy and $24 million for the moral suasion policy.

This paper contributes to two growing strands of the literature in recent economics studies. The first strand studies the effects of intrinsic and extrinsic motivations on economic activities.[3] The findings from our study highlight that it is important to examine both short-run and persistent effects when we consider pecuniary and nonpecurinary interventions (Charness and Gneezy, 2009; Landry et al., 2010, 2011; Ferraro, Miranda and Price, 2011; Allcott and Rogers, 2014). In particular, our findings on the persistence of moral suasion and economic incentives have direct policy implications in many fields because firms and governments use these two policy instruments for a variety of policy goals. Our study is the first field experiment that 1) randomly assigned moral suasion and economic incentives, 2) conducted repeated interventions of these two treatments, and 3) observed high-frequency consumption data during and after the treatment interventions. This

[3] Many previous papers examine whether extrinsic incentives crowd out or backfire intrinsic motivation. Examples include Frey and Oberholzer-Gee (1997) for the "Not In My Backyard" problem, Gneezy and Rustichini (2000); Landry et al. (2006) for charitable giving, Lacetera, Macis and Slonim (2012) for increasing blood donations, Dwenger et al. (2014) for tax evasion, and Chetty, Saez and Sándor (2014) for improving academic refereeing. Most of these studies focus on short-run effects, except for Landry et al. (2010), which analyze dynamic effects by studying donor actions more than once. See Gneezy, Meier and Rey-Biel (2011) for a comprehensive survey. Other recent papers test habit formation by providing either an economic incentive or a norm-based intervention. For example, Charness and Gneezy (2009) find that an economic incentive induces habit formation for exercising at the gym. Ferraro and Price (2013); Allcott and Rogers (2014) find that informing consumers about peers' consumption produces long-run conservation effects for water consumers and electricity consumers. To our knowledge, our study is the first field experiment that compares the persistence of a policy targeted to intrinsic motivation and a policy targeted to extrinsic motivation by providing repeated interventions of each treatment.

experimental design enables us to provide new empirical evidence on this research question.

Our paper also builds on recent studies in energy and environmental economics that examine the effects of pecuniary and nonpecuniary policies in the context of energy and environmental policies. A key challenge with nonexperimental data is how to empirically distinguish between intrinsically motivated and extrinsically motivated behavior. This has been challenging in the literature because typical environmental policies implement various types of policy interventions simultaneously. Such research environments make it difficult to identify the effect of each intervention, particularly when researchers intend to estimate persistent effects. Our study used a randomized controlled trial to address this challenge, and its results provide key implications for designing effective energy and environmental policies.[4]

2 Experimental Design, Data, and Hypotheses

We used a field experiment and high-frequency data on household-level electricity usage to test several hypotheses. In this section, we describe our experimental design, data, treatments, and hypotheses.

2.1 Experimental Design and Data

The field experiment was conducted for households in the Keihanna area of Kyoto prefecture in Japan in the summer of 2012 and the winter of 2013. The experiment was implemented in collaboration with the Ministry of Economy, Trade and Industry (METI), the prefecture of Kyoto, Kansai Electric Power Company (KEPCO), and Mitsubishi Heavy Industries, Ltd.

To invite as broad a set of households as possible, we provided generous participation rewards, which included free installations of an advanced meter and in-home display and a participation reward of 24,000 yen (approximately $240 in 2012). We contacted all 40,710 residential electricity

[4] Examples of nonexperimental studies documenting intrinsically motivated conservation include Reiss and White (2008) for the California electricity crisis, Cutter and Neidell (2009) for "Spare the Air" program in California, and Gerard (2013) for the Brazilian electricity crisis. The authors of these papers acknowledge that it is challenging to identify the effects of voluntary conservation separately from those of other policies during their sampling periods. Ferraro, Miranda and Price (2011) and Ferraro and Price (2013) are the first to use field experiments to examine intrinsically motivated conservation. Although their experiments do not include economic incentive treatments, they compare 3 types of nonpecurinary treatments for water conservation: information dissemination on behavioral and technological modifications, appeal for prosocial preferences, and provision of social comparisons.

customers in the area by mail.[5] Of these, 1,659 customers confirmed their participation. We excluded students, customers who had electricity self-generation devices, and those without access to the internet. This process left us with 691 households. Similar to previous field experiments in electricity demand (Wolak, 2006, 2011; Faruqui and Sergici, 2011; Jessoe and Rapson, 2014), our experiment was a randomized controlled trial (RCT) for self-selected participants, as opposed to a RCT for a purely randomly selected sample of the population. Therefore, it is important to carefully consider the external validity of the experiment, although the internal validity of the experiment is guaranteed by random assignment of treatments. To explore the external validity of our sample, we collected data from a random sample of the population in the corresponding geographical area. We analyze the observables between our sample and the random sample below.

We randomly assigned the 691 households to one of 3 groups: control (C), moral suasion (M), and economic incentive (E).

Control Group (C): The 153 customers in this group received an advanced electricity meter, an in-home display, and the participation reward. Other than that, this group did not receive any treatment.[6]

Moral Suasion Group (M): The 154 customers in this group received an advanced electricity meter, an in-home display, and the participation reward. In addition, this group received "moral suasion for energy conservation," which we describe below.

Economic Incentive Group (E): The 384 customers in this group received an advanced electricity meter, an in-home display, and the participation reward. In addition, this group received "economic incentives for energy conservation," which we describe below.[7]

The primary data for this study are high-frequency data on household electricity usage. Advanced electricity meters, often called "smart meters," were installed for all participating households, enabling us to collect household-level electricity usage at 30-minute intervals. We use consumption data from the summer of 2012 to the spring of 2013. In addition to the usage data, we collected

[5]We include the English translation of the recruitment letter in the Appendix.

[6]These advanced electricity meters are sometimes called "smart meters," which record usage at 15-, 30-, or 60-minute intervals. Conventional electricity meters do not record high-frequency usage. These meters typically record only cumulative usage since the installation. Electric utility companies, therefore, need to know usage at the beginning and at the end of a billing cycle to know monthly or bi-monthly usage.

[7]We assigned a relatively large number of participants to the economic incentive group in order to test the effects of different prices. If our sole objective was to compare the effects of the moral suasion and economic incentives, we could have assigned a similar number of customers to each group to minimize the variance of the estimates (Duflo, Glennerster and Kremer, 2007).

data by 3 surveys. We conducted the first survey prior to treatment assignment and collected demographic information. We conducted the second survey upon completion of the experiment to explore the mechanism behind our findings. We conducted the third survey for a random sample of households in the area to investigate the external validity of our sample.

[Table 1 about here]

Columns 1, 2, and 3 of Table 1 present the summary statistics of demographic variables and preexperiment consumption data by treatment group. A comparison across control and treatment groups indicates statistical balance in observables because of random assignment of the groups. Very little attrition occurred in each group. In total, 9 households (1.3 percent) dropped out from our sample because they moved residence. Because this small attrition occurred at approximately the same rate in each group, it is unlikely to significantly bias our estimates.

Column 4 shows the summary statistics for a random sample of the population in the corresponding geographical area. We investigate the external validity of our sample by comparing the mean for each observable variable between the random sample and our control group. Column 5 presents the differences in means and the standard errors of the differences in brackets. The differences are small and statistically insignificant for most variables. We find small but statistically significant differences at the 5 percent level for the age of buildings and household size. Note that there is still a possibility that unobservable characteristics can differ between the random sample and our experimental sample. However, the results in column 5 suggest that these two samples are statistically very similar, at least for the key observable variables for residential electricity demand.[8]

To obtain a sense of weather conditions in the experimental region, we compare the monthly average high and low temperatures between Kyoto prefecture in Japan (our experimental region) and Washington D.C. in the United States. We provide this comparison in Figure A.2 in the

[8]This analysis investigates the external validity of our experimental sample to the population in the corresponding geographical area. Note that there is another important external validity issue when a policy objective is to obtain estimates of the treatment effects for the population outside the experimental region. Allcott (Forthcoming) finds "site selection bias" when he compares the estimates of Opower's home-energy report among its 111 randomized controlled trials. We collaborated with the Ministry of Economy, Trade and Industry to conduct similar dynamic pricing experiments (for different research questions) in 3 other locations in Japan: Yokohama, Toyota, and Kitakyushu between 2012 and 2013. For each location, we find about 15 percent to 20 percent usage reductions from similar dynamic electricity pricing treatments, which implies that the estimates from the current study in Kyoto prefecture are not substantially different from those from other locations. However, this evidence still does not fully address concerns for site selection bias because these four experiments were the first field experiments on dynamic electricity pricing conducted in Japan. The estimates from our experiments, therefore, should be interpreted with this caution when they are applied to policies outside the experimental region.

Appendix. The average low and high temperatures are very similar between the two cities. The average high and low temperatures in the spring and summer months are almost identical between the two cities. In the fall and winter months, the average high and low temperatures are slightly higher in Kyoto, but the difference is less than 4^{o}F in each month, which implies that these two cities have quite similar weather conditions, which determine the majority of electricity usage, namely, usage for cooling and heating.

2.2 Treatments

A fundamental economic inefficiency in electricity markets is that electricity consumers generally do not pay prices that reflect the high marginal costs of electricity during peak demand hours (Borenstein, 2002; Joskow, 2012). Policymakers usually consider two types of economic policies to address this inefficiency. The first policy instrument, which is most frequently used by many countries, is an appeal to intrinsic motivation by using moral suasion for voluntary energy conservation. The second policy instrument, which is motivated by the standard economic theory, is an appeal to extrinsic motivation by introducing dynamic pricing that reflects the time-varying marginal costs of electricity. An important question is whether these two types of policies can generate persistent effects on consumer behavior. To investigate this question, we design two treatments that reflect the two policies being used by regulators in practice.

Our first treatment is "moral suasion," which intends to influence intrinsic motivation for energy conservation. After customers were assigned to the moral suasion group (M), they were educated that substantial energy conservation would be required for the society during "critical peak demand hours" on summer and winter peak demand days, in which electricity supply would be very limited relative to demand. We informed them that we would provide day-ahead and same-day messages for the critical peak demand days (treatment days) based on the day-ahead weather forecast. Note that customers in this group did not receive any extrinsic incentives to conserve energy. We neither provided an economic incentive nor did we disclose information about their conservation efforts to others. Therefore, we interpret the effect of this treatment on conservation, if any, as intrinsically motivated conservation as opposed to extrinsically motivated behavior.

We informed customers about how they would be receiving the treatments. First, their treatment hours were predetermined—1 pm to 4 pm for the summer and 6 pm to 9 pm for the winter.

These hours correspond to the system peak demand hours in Japan. Second, we defined the treatment days as follows. The treatment day had to be a week day in which the day-ahead maximum temperature forecast exceeded 31°C (88°F) for the summer and the day-ahead maximum temperature forecast was lower than 14°C (57°F) for the winter. Customers were told that they would receive day-ahead and same-day notices about the treatment day via a text message and a message on their in-home displays when the temperature forecasts met the criteria.[9]

[Figure 1 about here]

Our second treatment is an "economic incentive," which was intended to influence extrinsic motivation for energy conservation. After customers were assigned to the economic incentive group (E), we informed that they would be charged high electricity prices during the critical peak demand hours on the critical peak demand days. We also informed that we would provide day-ahead and same-day messages for critical peak days based on the day-ahead weather forecast. Figure 1 shows the dynamic electricity pricing schedule for the economic incentive group. This price schedule is called "variable critical peak pricing." On treatment days, the economic incentive group had a price increase either by 40, 60, and 80 cents/kWh. Because the baseline price was approximately 25 cents/kWh, these price increases mean that the critical peak price was either 65, 85, or 105 cents/kWh.[10] There was no price change for the control group and the moral suasion group, and they paid the baseline price, 25 cents/kWh.

For a given treatment day, all customers in the economic incentive group paid the same critical peak price. Across the treatment days, customers in this group experienced different critical peak prices. We used stratified randomization to allocate the 3 critical peak prices to the treatment days. We first defined *treatment cycles*, wherein each cycle consisted of 3 treatment days. We then

[9]This is the exact text of the day-ahead and same-day messages that were sent to the moral suasion group: "Notice of Demand Response: In the following critical peak demand hours, please reduce your electricity usage: 1 pm - 4 pm on Tuesday, August 21." For each treatment day, we sent the day-ahead and same-day messages to customers in the treatment group via a text message and a message on their in-home displays.

[10]Customers paid in Japanese yen, but we use U.S. currency throughout the paper. One Japanese yen was approximately equivalent to one U.S. cent (2012 exchange rate). Note that the baseline marginal price was not exactly 25 cents because customers had an increasing block price schedule with small price differences between tiers. The marginal price was 19.38 cents for monthly usage up to 120 kWh, 24.54 cents for usage between 120 kWh and 300 kWh, and 25.88 cents for usage over 300 kWh. The average monthly usage was 500 kWh for our customers. The baseline marginal price was, therefore, 25.88 cents for most customers and 24.54 cents for some customers. If customers consider their average price rather than their marginal price, the average price was not very different from the marginal price because the prices did not vary much between the tiers. For most customers, the baseline average price was between 23 cents and 24 cents.

randomized the 3 critical peak prices within the cycle. That is, each cycle included a treatment day with 65 cents/kWh, a treatment day with 85 cents/kWh, and a treatment day with 105 cents/kWh, and we randomized the order of the 3 prices within each cycle.[11] Using this stratified randomization, we minimized the correlation between the critical peak prices and the temperatures.[12] We repeated the interventions as long as the temperature forecasts met the criteria to test the persistence of the two treatments. As a result, the treatment groups experienced 15 treatment days (5 cycles) in the summer and 21 treatment days (7 cycles) in the winter.

2.3 Hypotheses

We tested several hypotheses using the field experiment. The first hypothesis is based on the standard demand theory, which predicts that 1) the economic incentive group lowers consumption according to the price elasticity of demand, and 2) the moral suasion group uses electricity in the same way as the control group. We compared consumption between the control group, moral suasion group, and economic incentive group to test the hypotheses. In addition, we used the randomized critical peak prices to estimate whether consumers respond to the marginal price of electricity.

The second hypothesis is related to *hot* versus *cold* decision-making in the psychology literature (Loewenstein, 2005). Immediate reactions to an event, called *hot* decision-making, can differ from decision-making in the *cold* phase, which follows the hot phase. This is particularly important for energy policies because policymakers generally need to acquire persistent responses from consumers by repeating interventions. To test this hypothesis, we repeated our interventions. In total, con-

[11] In our experiment, customers in the economic incentive group did not get a lower price for off-peak hours. In some dynamic electricity price schedules, customers get a lower off-peak price, which partly offsets a higher price for peak demand hours. This is the exact text of the day-ahead and same-day messages that were sent to the economic incentive group: "Notice of Demand Response: In the following critical peak demand hours, you will be charged very high electricity price, so please reduce your electricity usage: 1 pm - 4 pm on Tuesday, August 21. Price will be 85 yen (+ 60 yen) per kWh." For each treatment day, we sent the day-ahead and same-day messages to customers in the treatment group via a text message and a message on their in-home displays.

[12] We minimized the correlation between the critical peak prices and the temperature in order to test whether consumers responded differently to different marginal prices. Note that the stratified randomization was conducted for the treatment days, all of which met the temperature criteria to be a treatment day. The minimum, average, and maximum of the daily maximum temperatures for the summer treatment days were 31.2°C, 33.9°C, and 36.5°C (88°F, 93°F, and 98°F). The minimum, average, and maximum of the daily maximum temperatures for the winter treatment days were 3.5°C, 7.8°C, and 11.4°C (38.3°F, 46.0°F, and 52.5°F). That is, customers experienced hot temperatures for all summer treatment days, and cold temperatures for all winter treatment days. Using stratified randomization, we avoided the possibility of customers experiencing a certain critical peak price on particularly hot or cold days. Because of the stratified randomization, the resulting correlation between the temperatures for the treatment days and the critical peak prices was -0.06 for the summer and -0.05 for the winter.

sumers in the treatment group received treatments 15 times in the summer and 21 times in the winter. We tested whether 1) the economic incentive persisted through multiple treatment days and whether 2) the moral suasion persisted through multiple treatment days.

The third hypothesis refers to potential habit formation that can be induced by our treatments (Becker and Murphy, 1988). To test this possibility, we withdrew the treatments after the final interventions but continued to collect high-frequency electricity consumption data. We tested whether 1) the moral suasion effect disappeared after the final intervention and whether 2) the economic incentive effect disappeared after we discontinued the treatment.

We begin by testing the 3 hypotheses in the next section. We then investigate the mechanism behind our findings by using follow-up surveys on durable goods investments and behavioral changes in energy-efficient lifestyles. Finally, we explore the welfare implications of our findings in Section 4, by analyzing welfare gains from the two policies in the context of electricity markets.

3 Empirical Analysis and Results

We present the results of our field experiment in this section. Recall that the treatment groups experienced several treatment days in the summer and winter. We start by including all treatment days in our regression, to show the overall treatment effects. We then explore their persistence, spillovers, and habit formation in the subsequent subsections.

3.1 Effects of Moral Suasion and Economic Incentives

We begin by showing evidence from the raw data in Figure 2. It plots the mean log electricity consumption for each group over 30-minute intervals on the summer treatment days. The figure indicates that usage in the pretreatment hours is essentially the same for all groups. About an hour before the treatment hours, usage for the treatment groups begins to drop relative to the control group. The reductions are consistent during the treatment hours (1 pm to 4 pm). Immediately after the end of the treatment hours, usage for the treatment groups returns to the control group's level, although we observe small remaining differences for a few hours after the treatment hours. The figure provides visual evidence of the treatment effects for both treatment groups and suggests that the reductions are larger for the economic incentive group.[13]

Table 2 provides a formal econometric analysis with standard errors. We estimate the treatment effects by an OLS regression:

$$\ln x_{it} = \alpha M_{it} + \beta E_{it} + \theta_i + \lambda_t + \eta_{it}, \tag{1}$$

where $\ln x_{it}$ is the natural log of electricity usage for household i in a 30-minute interval t. We use the natural log of usage for the dependent variable so that we can interpret the treatment effects approximately in percentage terms. The treatment effects in the exact percentage terms can be obtained by $\exp(\alpha) - 1$ and $\exp(\beta) - 1$.[14] We report both of the log points and exact percentage terms. M_{it} equals 1 if household i is in group M (the moral suasion group) and receives a treatment in t. Similarly, E_{it} equals 1 if household i is in group E (the economic incentive group) and receives a treatment in t. We include household fixed effects θ_i and time fixed effects λ_t for each 30-minute interval to control for time-specific shocks such as weather. We cluster the standard errors at the household level to adjust for serial correlation. We include data from the preexperiment days and treatment days in this regression. Note that treatment effects can have spillover effects on nontreatment days after the beginning of the experimental period. In this case, including nontreatment days (as control days) will underestimate the treatment effects. We, therefore, do not include nontreatment days in this regression. Recall that the treatment groups had explicit incentives to reduce usage only during the treatment hours—1 pm to 4 pm for the summer and 6 pm to 9 pm for the winter. In this regression, we include only these hours to estimate the treatment effects on the treatment hours. We examine potential spillover effects for nontreatment hours in the following subsection.

[Table 2 about here]

Column 1 shows that moral suasion caused a reduction in peak-hour electricity usage for the summer treatment days by 0.031 log points (3.1 percent). A reduction in peak hour consumption

[13] In critical peak electricity pricing, the treatment (peak) hours are generally defined as the hours in which system-wise aggregate consumption reaches its peak. On typical summer days in Japan, the peak consumption hours occur during the night for residential customers, and therefore, they do not coincide with the system peak hours. On winter days in Japan, the residential peak hours coincide with the system peak hours.

[14] Note that when an estimate (α or β) is negative, its exact percentage term will be smaller than the corresponding log points in absolute terms. For example, when $\beta = -0.167$, we have $\exp(\beta) - 1 = -0.154$.

by 3 percent is economically significant because the marginal cost of electricity is extremely high during critical peak hours.[15] This finding implies that the moral suasion policy can provide a meaningful effect when we consider the effect over all treatment days. The level of the reductions is, nevertheless, much larger for the economic incentive, which produced a reduction in usage by 0.167 log points (15.4 percent). This result implies that the economic incentive policy can produce a substantial reduction in peak hour energy usage.

An important question for the economic incentive effect is whether 1) consumers responded to the changes in the marginal price or 2) they just reacted to a "pricing event." The two possibilities imply different policy implications because the former indicates that policymakers can use price as a tool to achieve certain levels of reductions. This question remains unanswered in the literature because most previous experiments use only one price for critical peak events.[16] We examine these hypotheses in column 2 by estimating the treatment effect for each marginal price.

Recall that the baseline marginal price was 25 cents/kWh, and the economic incentive group experienced the 3 critical peak prices: 65, 85, and 105 cents/kWh. Column 2 of Table 2 shows that, consistent with the prediction of the standard demand theory, the reductions monotonically increase when customers are charged higher marginal prices. In the summer, for example, the critical peak prices of 65, 85, and 105 cents/kWh produced reductions in usage by 0.151 log points (14.0 percent), 0.167 log points (15.4 percent), and 0.182 log points (16.6 percent).[17] This finding implies that households indeed responded to time-varying marginal prices when they received salient price

[15]Reductions in peak hour electricity usage of 3.1 percent (the moral suasion effect) and 15.4 percent (the economic incentive effect) are considerable levels of usage reductions. For example, home energy reports by Opower induced an average reduction of 1.3 percent for daily average electricity usage (Allcott, Forthcoming; Allcott and Rogers, 2014), and a rebate program for energy conservation in California produced a reduction of 5 perfect for daily average electricity usage for households in inland areas of California (Ito, Forthcoming). However, these programs and most energy conservation programs in the past targeted "average daily usage" instead of usage during peak demand hours. In addition, customers in our experiment received day-ahead and same-day messages via in-home display and text message, whereas customers in most previous programs received notification only by mail. For these reasons, these previous energy conservation programs may not be directly comparable to our experiment. More comparable programs are dynamic pricing experiments conducted in the United States. For example, in a critical peak pricing experiment in Washington D.C., Wolak (2010, 2011) find that a critical peak price of 77 cents induced a usage reduction of 13 percent. The implied price elasticity for critical peak pricing is very similar between our experiment and Wolak's experiment in Washington D.C. despite the fact that the two experiments were conducted for customers in two different cities in different countries.

[16]For example, Wolak (2006, 2011) use one price for critical peak events for each experiment. Jessoe and Rapson (2014) include different critical peak hour prices, but the treatment hours and duration of the treatment differ across the prices. The authors acknowledge that this makes it difficult to compare treatment effects across different critical peak prices.

[17]The coefficient estimate for 65 cents is statistically different from that for 105 cents at the 5 percent significance level. We cannot reject that the coefficient estimates for 65 cents and 85 cents are statistically equivalent at the 10 percent significance level.

information. While this result may be unsurprising to economists, it has a real policy implication because regulators and utility companies often believe that electricity consumers do not respond to electricity prices, and therefore, they cannot use a price-based policy. Our findings are in contrast to those of Ito (2014), who finds that electricity consumers in California do not respond to the marginal price of their nonlinear price schedules and rather, they respond to the *average* price of their electricity bills. One obvious difference in the environment is that customers in our experiment have access to salient information about their real-time marginal price via in-home displays and text messages, whereas Californian customers in Ito's study received their price information only through their monthly bills. The difference in the findings is consistent with the literature that emphasizes the importance of price salience (Chetty, Looney and Kroft, 2009; Finkelstein, 2009; Jessoe and Rapson, 2014). The second important difference is that our customers had a single marginal price given an hour, which varies only across hours, whereas the marginal prices in Ito's study vary with each customer's cumulative monthly usage. The different findings between the two studies, therefore, reflect the possibility that consumers are more likely to respond to time-varying marginal prices compared to marginal prices that vary with their cumulative usage during a month. Finally, in columns 3 and 4, we find consistent results for the winter treatment days, in which the treatment hours were between 6 pm and 9 pm. The implied price elasticity estimates (standard errors) are -0.136 (0.017) for the summer and -0.141 (0.018) for the winter.

3.2 Repeated Interventions and Persistence of Treatment Effects

The treatment groups experienced 15 treatment days in the summer and 21 treatment days in the winter. We included many treatment days in order to investigate whether the treatment effects persisted over repeated interventions. This question is relevant to policy in practice because policymakers usually need to have repeated interventions to address multiple peak demand days for a given season. Recall that the treatment days were not necessarily consecutive because the critical peak event days were determined by the criteria based on the day-ahead weather forecasts. We, therefore, had 15 and 21 nonconsecutive days for the summer and the winter. To analyze persistence, we divide the 15 summer treatment days into 5 cycles and the 21 winter treatment days into

7 cycles, with each cycle including 3 treatment days.[18] We estimate OLS regressions for treatment cycles $c = 1, .., 5$ for the summer and $c = 1, .., 7$ for the winter:

$$\ln x_{it} = \sum_{c \in C} (\alpha_c M_{itc} + \beta_c E_{itc}) + \theta_i + \lambda_t + \eta_{it}, \tag{2}$$

where α_c and β_c are the effects of moral suasion and economic incentives for treatment cycle c.

In Table 3, we find substantially different persistence between moral suasion and economic incentives. Column 1 shows that the moral suasion effect is statistically significant only in the first cycle (the first 3 treatment days) for the summer. The effect in the first cycle is a reduction of usage by 0.083 log points (8.0 percent), which is economically significant for electricity usage during the critical peak demand hours. However, when we repeated this intervention, the moral suasion effect diminished quickly—it declined to 0.033 log points (3.2 percent) in the second cycle and diminished to nearly zero in the remaining cycles. In column 3 for the winter result, we find that this decaying effect was, in a sense, "reset" at the beginning of the winter experiment. Customers in the treatment group did not receive a treatment for a few months between the summer experiment and the winter experiment. Column 3 suggests that the moral suasion group started to respond to the treatment again when they received moral suasion for the first time in the winter. Then, the effect began to diminish fairly quickly. For the first cycle in the winter, we find that the moral suasion effect was 0.083 log points (8.0 percent). Then, the effect diminished quickly after the first cycle, similar to the findings for the summer. The results imply that moral suasion can be an effective policy for the short-run, but its effects are likely to diminish quickly when we repeat the interventions.

[Table 3 about here]

In contrast, we find that the economic incentive produces considerably more persistent effects. For the summer, the effect is the largest in the second cycle (0.198 log points; 17.9 percent) and remains fairly stable between the first and third cycles. The effect becomes slightly lower in the fourth and fifth cycles. Nevertheless, the fifth cycle still shows an effect of 0.127 log points (12.0 percent). The winter results show similar patterns with even more stable effects across repeated

[18] As explained in the experimental design, the 3 critical peak prices (65, 85, and 105 cents/kWh) were randomized within the 3 days in a cycle. All customers in the economic incentive group experienced the same critical peak price for a given treatment day.

interventions. The effect is the largest in the second cycle (0.205 log points; 18.5 percent) and is stable from the first to the seventh cycles. We cannot reject the null hypothesis that the economic incentive effects are equivalent in the first and the seventh cycles in the winter. In contrast, the same null hypothesis is rejected for the moral suasion effect. That is, the moral suasion effect for the first cycle is statistically different from that for the final cycle. To compare the treatment effects across cycles more visually, we also plot our estimation results in Figure 3. The figure shows the reductions in usage by treatment cycles, and the interval bar shows one standard error for each treatment effect. These findings suggest that the two policies are likely to have significantly different policy implications, particularly when policymakers intend to generate persistent effects over repeated interventions.[19]

[Figure 3 about here]

3.3 Spillover Effects for Nontreatment Hours on Treatment Days

The treatments in our experiments were specifically targeted to electricity usage during the treatment hours—1 pm to 4 pm for the summer and 6 pm to 9 pm for the winter. For a few reasons, however, the treatments can generate spillover effects for electricity usage in the nontreatment hours on the treatment days. First, households may change their usage immediately before or after the treatment hours. For example, those who faced with a high critical peak price for the treatment hours could increase their air conditioner usage immediately before the treatment hours. Such pre-cooling or pre-heating could be rational given the high critical peak prices for the treatment hours. Similarly, they may increase their air conditioner usage immediately after the treatment hours. Hours immediately before and after the peak hours are called "shoulder hours." In general, when the marginal cost of electricity supply is high during peak demand hours, the marginal cost is also likely to be relatively high during the shoulder hours. Therefore, if customers increase their usage in the shoulder hours, it could attenuate the economic benefits of interventions focused

[19] The economic incentive effects were more persistent in the winter experiment compared to those in the summer experiment. In the summer experiment, the economic incentive effect for the first cycle is statistically indifferent from that for the fourth cycle, but it is statically different from that for the fifth cycle at the 5% significance level. One potential reason is that customers had been more experienced in the winter experiment after they went through the summer experimental period. Another potential reason is that in the summer experiment, the last few treatment days were in September. Households in Japan tend to use air conditioner more in July and August and less in September, which could partially explain why we see lower price elasticities for the last cycle of the summer experiment.

on peak demand hours.[20] This is also an important question for environmental policy because increases in usage in nontreatment hours could potentially increase the total emissions from electricity generation (Holland and Mansur, 2008).

The second possibility is that consumers may shift their consumption to off-peak hours, which are hours outside peak hours and shoulder hours. In most electricity markets, the marginal cost of electricity is much lower in off-peak hours relative to that in peak demand hours. Therefore, such consumption shifting is still likely to provide a meaningful economic benefit. Finally, the third possibility is that consumers could reduce their usage in all hours, including shoulder hours and off-peak hours. For example, consider that consumers have a fixed cost of changing their lifestyle in terms of electricity usage (Wolak, 2011). When they are faced with a substantial increase in peak hour electricity price, they may change their lifestyle as a whole to be more energy efficient. In such cases, it is possible that customers lower electricity usage in all hours when they face interventions that are primarily targeted at peak demand hours.

[Table 4 about here]

Table 4 provides the results of empirical tests for these possibilities. We estimate equation (1) by including data for different hours on the treatment days for each column. Column 1 shows the result for the treatment hours, which is equivalent to the results in Table 2. Column 2 shows the result for the shoulder hours, namely, 3 hours before and after the treatment hours. Finally, column 3 includes data for other nontreatment hours on the treatment days. For both of the moral suasion group and the economic incentive group, we do not find an increase in consumption in the shoulder hours and other off-peak hours. Instead, we find usage reductions for the economic incentive group during the nontreatment hours. For example, we find usage reductions by 0.060 log points (5.8 percent) for the shoulder hours and by 0.022 log points (2.2 percent) for the off-peak hours in the summer experiment. The findings for the winter experiment are also consistent with those for the summer experiment. In contrast, we do not find such spillover effects for the moral suasion group. Their usage in the shoulder hours and off-peak hours are statistically indistinguishable from the control group's usage. These results imply that the economic incentives in our experiment

[20]To address this concern, policymakers can design a dynamic pricing schedule that includes relatively high prices for the shoulder hours in addition to high prices for the peak demand hours, which could reflect the time-varying marginal costs of electricity supply more effectively.

motivated customers to lower their usage in the nontreatment hours as well as the treatment hours.

3.4 Habit Formation

In the previous sections, we find that moral suasion and economic incentives produced substantially different results in terms of the persistence and spillover effects. These findings suggest the possibility of habit formation (Becker and Murphy, 1988) that is induced by the treatment—customers faced with economic incentives may have formed a habit for energy-efficient lifestyles, which could be why we observe consistent reductions in consumption. To explore the potential habit formation effect, we collected usage data for the periods *after* we withdrew the treatments. During this period, customers did not receive any treatments. If the treatments did not induce habit formation effects, we should observe the same levels of consumption between the control group and each treatment group.

[Table 5 about here]

Table 5 provides the results of the empirical tests for habit formation. We examine usage data during the three-month post-experimental period for both the summer and winter experiments, wherein customers did not receive any treatment. We test whether usage levels in peak demand hours during this period differ between the control and each treatment group. We examine usage in peak demand hours on weekdays during the three-month post-experimental periods. The table shows that the moral suasion group's usage is not statistically different from that of the control group. In contrast, the economic incentive group's usage is statistically different from that of the control group as well as the moral suasion group. Consumers who had received the economic incentives continued to have lower consumption, by 0.077 log points (7.4 percent) after the summer experiment and by 0.069 log points (6.7 percent) after the winter experiment. The robust findings for the summer and winter experiments provide evidence that the economic incentives offered during the treatment periods created habit formation. This finding provides an important policy implication because the existence of habit formation could provide additional policy impacts even after policy interventions have ended.[21] Why do we see habit formation effects only for the economic

[21] For an economic incentive policy in a different context, Charness and Gneezy (2009) also find that their monetary incentive policy induced habit formation for college students to exercise at the gym on campus. In addition, a few studies explore the persistent effects of "social comparisons" for water and energy conservation. For example, Ferraro,

incentive group and not for the moral suasion group? We explore the mechanism behind the findings in the final subsection of our empirical analyses.[22] Before we proceed to analyzing the mechanism, we investigate potential heterogeneity in the treatment effects in the next subsection.

3.5 Heterogeneity

Policymakers are often aware of potential heterogeneity in the treatment effects because different types of households may be affected by a policy in different ways. To explore potential heterogeneity in the treatment effects of moral suasion and economic incentives, we interact the treatment dummies with two policy-relevant household characteristics—household income and average electricity usage. Policymakers are often concerned about how these policy instruments affect higher- and lower-income households differently. In addition, it is important to know how high- and low-electricity users respond to these policies when policymakers target a certain level of aggregate usage reductions. We use data on household income collected from the preexperiment survey, and electricity usage data from the preexperiment period to calculate the household-level average daily electricity use in kWh per day. We rescale the unit of the income variable to 100,000 dollars for the regressions.

Table 6 shows the estimation results for regressions that include the interaction terms for household income and usage. Columns 1 and 4 include the interaction terms for household income, columns 2 and 5 include the interaction terms for pre-experiment usage levels, and columns 3 and 6 include both interaction terms. Although we find weak evidence for the moral suasion effect being larger for higher income households, the estimates are not statistically significant. We find a consistent relationship between economic incentives and income—the economic incentive effect is lower for higher income households compared to lower income households. Note that our dependent variable is the log of electricity usage, and the treatment variables are dummy variables. Therefore, for example, the coefficient (0.126 log points) in column 2 implies that an increase in household income by $10,000 would be associated with a 0.0126 log points increase for the coefficient for the economic incentive dummy variable (i.e., a 0.0126 log points *decrease* in the treatment effect).[23]

Miranda and Price (2011) and Allcott and Rogers (2014) find that providing information about neighbors' water usage and electricity usage, respectively, induced long-run conservation effects.

[22] As an alternative explanation for the persistent treatment effect, the interventions may have induced durable goods investment. We explore this possibility in Subsection 3.6.

[23] The table also suggests that the economic incentive effect is larger for high users in the summer experiment,

[Table 6 about here]

3.6 Mechanisms Behind the Effects of the Two Treatments

In the previous sections, we find significant differences for the effects of moral suasion and economic incentives in their persistence, spillovers, and habit formation. Moral suasion was effective only for the first few treatment days and did not induce a persistent effect. Economic incentives, in contrast, produced strong persistent effects on energy conservation. To investigate the mechanisms behind the findings, we conducted a detailed follow-up survey. We examine two potential channels. The first possibility is that the treatments may have induced durable goods investments—households in the treatment groups may have purchased energy-efficient appliances in response to the treatments. If this effect was systematically large for the economic incentive group, it could explain why we observed persistent usage reductions for this group. The second possibility is that the treatments may have induced behavioral changes in lifestyles. Suppose that customers had "bad habits" of using energy inefficiently. It is possible that experiencing high electricity prices may have triggered a change in their lifestyles, encouraging them to become more energy efficient.

[Table 7 about here]

We begin by testing the first possibility in Table 7. We asked customers if they purchased energy-efficient appliances since the start of the experiment. We estimate a linear probability model, which includes a binary choice dependent variable, dummy variables for the treatment groups as independent variables, and a constant term.[24] The constant term, therefore, gives the ratio of control group customers who purchased an energy-efficient appliance. The coefficient for each treatment dummy variable indicates a percentage point increase for the treatment group.

The table suggests that moral suasion increased the purchase of air conditioners by 8 percentage points, whereas economic incentives increased it by 9 percentage points. The estimates suggest that customers in the two treatment groups had similar significant increases in purchasing energy-efficient air conditioners compared to the control group. We do not find statistically significant effects for other products for both treatment groups. The results suggest that durable goods investments

although this effect is not found for the winter experiment.

[24]For robustness checks, we also run probit models. Our results are the same as those of the linear probability model.

are unlikely to explain why we find significant differences in the persistent effects between moral suasion and economic incentives.[25]

[Table 8 about here]

Table 8 explores the second potential channel—behavioral changes in lifestyles. After the experimental period, we asked customers two questions. The first question inquired about their efforts toward adopting an energy-efficient lifestyle. Customers evaluated their lifestyles in terms of energy efficiency on a scale of 1 (lowest) to 5 (highest). We regress this score on the dummy variable for each treatment group and a constant term. Column 1 implies that the economic incentive increased this score by 0.4 from the baseline level of 3.03. We find a slight positive effect for the moral suasion group, but it is statistically insignificant. The difference between the coefficients for moral suasion and economic incentives (0.13 and 0.40) is statistically significant at the 1 percent significance level.

We then asked consumers whether they were using each electric appliance in an energy-efficient way. We asked this question for air conditioners, electric heaters, personal computers, washers, and vacuum cleaners. We estimate a linear probability model, in which the dependent variable is binary choice, and the independent variables include dummy variables for each treatment group. The model also contains a constant term. For each appliance, we find that economic incentives had a statistically significant effect by 8 to 15 percentage points. In contrast, moral suasion did not have statistically significant effects on the energy-efficient use of each appliance.

Although these results are based on stated survey responses, they provide suggestive evidence about the mechanisms behind our findings. The significant differences in the persistent effects between the two treatments are unlikely to originate from durable goods investments. Instead, our findings suggest that experiencing high electricity prices triggered a change in their lifestyles, encouraging them to become more energy efficient.

4 Welfare Implications

When pursuing a variety of policy goals, policymakers can design policies to influence intrinsic and extrinsic motivations. Our empirical findings suggest that these two policy instruments are likely

[25] An important caveat from the evidence about durable goods investments is that customers knew that they were going to receive the treatments only during the experimental period. If customers experienced their treatments for longer time, more consumers may have found durable goods investments economical.

to have different policy implications, particularly when we consider persistence. In this section, we highlight such policy implications by analyzing the welfare gains from the two policies in the context of electricity markets.

4.1 Conceptual Framework

We introduce a simple conceptual framework for a model of electricity consumers to guide our welfare analysis. When consumers receive no treatment, each consumer uses electricity $\bar{x}$ at a given power price P, where $\bar{x}$ can be regarded as a "business as usual" (BAU) consumption level. When they receive moral suasion for conserving energy, they may voluntarily decrease their consumption from $\bar{x}$ to x. Voluntary conservation of electricity, g, is then expressed as the difference between $\bar{x}$ and x. The saved amount in economic terms, Pg, is added to the numeraire y, which totals $Y = y + Pg$. Alternatively, $Y = I - Px$ from the budget constraint of a consumer with income I.

We assume that utility is additively separable into 3 components. The first term, $u(x)$, denotes utility from consuming electricity, which is assumed to be increasing and concave ($u' > 0$ and $u'' < 0$). The second term, $v(I - Px)$, is utility from numeraire consumption, which is assumed to be increasing and weakly concave ($v' > 0$ and $v'' \leq 0$). Lastly, we consider utility from conservation of electricity, $\phi(g;\theta)$. This is also assumed to be increasing and weakly concave ($\phi_g = \frac{\partial \phi}{\partial g} > 0$ and $\phi_{gg} = \frac{\partial^2 \phi}{\partial g^2} \leq 0$). The utility term $\phi(g;\theta)$ may represent a warm glow component, which is a type of impure altruism, as discussed by Andreoni (1989). Let θ be a parameter that represents the frequency of interventions. We assume that utility and marginal utility of electricity conservation are decreasing in the frequency of interventions, that is, $\phi_\theta = \frac{\partial \phi}{\partial \theta} < 0$ and $\phi_{g\theta} = \frac{\partial^2 \phi}{\partial \theta \partial g} < 0$. The subscript notation denotes a partial derivative.

Andreoni (1989) and Kingma (1989) argue that there are several competing theoretical models of charitable contributions. In the case of pure altruism (pure public good), consumers may care about the total contributions to voluntary conservation. Moreover, consumers may take account of the utility cost (disutilily) of social pressure for not contributing or only contributing a small amount toward voluntary conservation, as illustrated by DellaVigna, List and Malmendier (2012). It is not our primary focus to compare these competing models, but note that we can extend our simple model to incorporate other potential mechanisms behind contributions to voluntary conservation.[26]

[26]For example, Kotchen (2006); Kotchen and Moore (2007) consider different participation mechanisms for envi-

The BAU consumption level, $\bar{x}$, in the absence of treatment can be expressed by $\bar{x} = \arg\max\{u(x) + v(I - Px)\}$. Consumers in the economic incentive group have a price change and simply adjust their consumption such that $u' - Pv' = 0$ responding to the price changes. Consumers in the moral suasion group receive moral suasion without economic incentives. When they receive moral suasion, they maximize the following overall utility function:

$$\begin{aligned} &\max_{x,g}\ u(x) + v(I - Px) + \phi(g;\theta) \\ &\text{s.t. } g = \bar{x} - x. \end{aligned} \tag{3}$$

This problem can be rewritten as follows:

$$\max_{x}\ u(x) + v(I - Px) + \phi(\bar{x} - x;\theta). \tag{4}$$

Let x^* denote the optimal solution for the maximization problem (4), namely, the optimal consumption level under moral suasion. Note that x^* satisfies $u' - Pv' - \phi_g = 0$.[27]

The effect of repeated interventions on voluntary conservation can be easily derived by differentiating the first order condition for the optimization problem (4).[28] Simple calculation yields

$$g^*_\theta = \bar{x}_\theta - x^*_\theta = -x^*_\theta = -\frac{\phi_{g\theta}}{u'' + P^2 v'' + \phi_{gg}} < 0. \tag{5}$$

The optimal consumption of electricity is increasing in θ, that is, $x^*_\theta > 0$, while the BAU consumption level $\bar{x}$ is not affected by θ, that is, $\bar{x}_\theta = 0$. Therefore, the model suggests that repeated interventions may decrease voluntary conservation of electricity, which is consistent with our empirical findings. In the next subsection, we use this conceptual framework to highlight the welfare implications of our empirical findings.

ronmental public goods and show how they relate to existing theory on either *pure* or *impure* public goods.

[27] Alternatively, we may consider social pressure instead of warm glow. The utility maximization problem may be represented as $\max_x\ u(x) + v(I - Px) - \phi(x;\theta)$, where $\phi(x;\theta)$ can be interpreted as a utility cost of social pressure for not contributing to conservation. This argument is in line with those in DellaVigna, List and Malmendier (2012) and Gerard (2013). Note that x^* satisfies $u' - Pv' - \phi_x = 0$. Thus, if the functional form of $\phi(\,\cdot\,;\theta)$ is the same for both warm glow and social pressure, we obtain the same results in a marginal sense.

[28] Total differentiation of the first order condition for (4) gives $(u'' + P^2v'' + \phi_{gg})dx - (Pv'' + \phi_{gg}\bar{x}_I)dI - (v' - Pxv'' + \phi_{gg}\bar{x}_P)dP - \phi_{g\theta}d\theta = 0$. Thus, we have $x^*_\theta = \frac{\phi_{g\theta}}{u''+P^2v''+\phi_{gg}} > 0$ with $dI = dP = 0$.

4.2 Welfare Gains from the Two Policies

We examine the welfare implications of two policy instruments that are intended to reduce energy usage during peak demand hours: 1) moral suasion and 2) economic incentives. Recall that the fundamental inefficiency in electricity markets is that consumers do not pay time-varying prices for electricity. Thus, they do not have an incentive to use less energy when the marginal cost becomes very high during peak demand hours. We begin with the assumption that the marginal cost of electricity for the critical peak hours is 65 cents/kWh, which was the peak wholesale price in the Japanese wholesale electricity market, the Japan Electric Power Exchange, during our experimental period. For a few reasons, this number is likely to be a lower bound for the social marginal cost of electricity in the critical peak hours in Japan during the period.[29] Therefore, we provide the same analysis for different assumptions on the marginal cost of electricity supply (85 and 105 cents/kWh) in Tables A.1 and A.2 in the Appendix. Different assumptions on the marginal cost do not change the qualitative results of our welfare analysis, although the welfare gains are larger when we consider a higher marginal cost of electricity supply.[30]

We consider two policies as well as a baseline case with no policy intervention. In the baseline case, consumers pay 25 cents/kWh for their electricity usage, the average residential electricity price in Japan in 2012. The first policy is our economic incentive treatment. We consider that consumers with this policy pay the price that equals the marginal cost, which is 65 cents/kWh. The second policy is our moral suasion treatment. Consumers with this policy pay the baseline

[29]The Japanese electricity market was only partially deregulated during our experimental period. As a result, not all electricity was traded in the centralized Japanese wholesale market. Regulators knew that most of the marginal power plants supplying electricity for peak demand hours were owned by vertically integrated local monopoly power companies, whose electricity was usually not sold in the centralized wholesale market. During our experimental period, these power companies needed to run their old and inefficient power plants to meet unexpected supply shortages after the Fukushima Daiichi Nuclear Disaster. This is one of the reasons why our assumption of 65 cents/kWh is likely to be a lower bound for the marginal cost. In addition, regulators avoided system-wide blackouts by forcing manufacturing firms to stop operating during peak demand hours. If this cost is considered to be a marginal cost for peak hour electricity, the marginal cost can be much higher than the wholesale electricity price in this partially deregulated market. Finally, the wholesale price did not include environmental externalities from electricity generation, the cost of which is likely to underestimate the social marginal cost of electricity.

[30]Another reason why our welfare calculation is likely to provide a lower bound is that it does not consider long-run avoidable investment costs for generation capacity. According to Kansai Electric Power Company, their long-run avoidable cost for a 600 MW thermal plant is $150,000/MW per year, assuming that the payment period is 10 years and the discount rate is 4%. The maximum total electricity load from residential customers in Japan is 46,800MW, which implies that our economic incentive policy would induce a reduction in the maximum load by 7,198 MW ($= 46,800 \cdot 0.1538$). Therefore, a back of envelop calculation of the long-run avoidable cost from the economic incentive policy is $1,080 million ($= 7,198 \cdot 150,000$) per year, which is significantly larger than the welfare gains in Table 9, which does not consider long-run avoidable investment costs for generation capacity.

price but receive moral suasion for energy conservation.

Consider a quasi-linear utility function for equation (4). To be consistent with the empirical estimation for electricity demand from our field experiment, we characterize the electricity demand by $\ln x = a + \alpha D + \epsilon \ln p$, where D equals 1 if consumers receive the moral suasion treatment, p is the electricity price, and ϵ is the price elasticity. We obtain parameters a, α and ϵ from our field experiment.[31] The inverse demand is defined by $p(x) = [x/(\exp(a) \cdot \exp(\alpha D))]^{1/\epsilon}$.

The baseline consumption is $\bar{x} = \exp(a) \cdot 25^{\epsilon}$. When consumers receive the economic incentive, the usage becomes $x_e = \exp(a) \cdot 65^{\epsilon}$. The efficiency gain is characterized by $\int_{x_e}^{\bar{x}} (c - p(x))dx$, the area between the marginal cost c and the inverse demand curve $p(x)$ in the range between x_e and $\bar{x}$. We begin by calculating this efficiency gain for the Japanese electricity market. For a typical summer peak hour, electricity consumption from residential customers is 46,800 MWh. An important assumption in this welfare calculation is that residential customers in Japan respond in the same manner to these two policies as the consumers in our experimental households. We consider two scenarios. In the first scenario, we provide the policy for a short run only, by having only 3 treatment days. In the second scenario, we offer the treatment repeatedly for a total of 15 treatment days. This comparison is consistent with our empirical analyses in the previous section, from which we obtain necessary parameters for our welfare calculation.

[Table 9 about here]

Column 1 of Table 9 shows the efficiency gain from the economic incentive policy. With the short-run policy, the total efficiency gain for the 3 treatment days is \$16.84 million. We then calculate the welfare gains for the repeated policy with 15 treatment days based on the estimated parameters from our experimental findings for the repeated interventions. Because the responses to the economic incentive treatments (β) do not decay much, more treatment days provide further efficiency gains. With 15 treatment days, the efficiency gain is \$76.55 million. The difference between the short-run and repeated polices is \$59.71 million and statistically significant. These results suggest that 1) the economic incentive policy can provide substantial efficiency gains for the electricity market, and 2) repeated interventions can obtain further gains when there are many critical peak demand days, during which the marginal cost of electricity becomes very high.

[31] Recall that we estimated α (the effect of the moral suasion) and β (the effect of economic incentives) in our field experiment. We use β for the case with treatment price 65 cents/kWh to calculate the price elasticity $\epsilon = \beta/\ln(65/25)$.

When consumers receive moral suasion, the usage can be characterized by $x^* = \exp(a) \cdot \exp(\alpha) \cdot 25^\epsilon$. The efficiency gain is $\int_{x^*}^{\bar{x}} (c - p(x))dx$, which we calculate in Column 2. With the short-run treatment, the efficiency gain is \$11.37 million, which is lower than the gain from the economic incentive treatment, but it still has a meaningful magnitude for the market. Because the moral suasion effect decays, the efficiency gain does not increase much with repeated interventions. We cannot reject the null that the efficiency gain from the moral suasion treatment is the same for the short-run policy and repeated policy.

When consumers receive moral suasion, there is one more channel through which the welfare can be changed. In our model in equation (4), consumers who receive moral suasion would change their usage from $\bar{x}$ to x^* because they feel warm glow or self-satisfaction from behaving prosocially. In this case, consumers obtain a surplus from their conservation $g = \bar{x} - x^*$. Note that consumers do not necessarily gain a surplus if we consider different models that could explain their motives. For example, consumers may reduce usage because they feel social pressure (DellaVigna, List and Malmendier, 2012) or obedience for authorities. In such cases, it is possible that consumers may lose a surplus when receiving moral suasion. Given our experimental setting, the primary motive for our consumers was more likely to be warm glow. However, we cannot completely exclude the possibility that our households may have lost a surplus or gained no surplus when receiving moral suasion. Therefore, we provide the welfare change from the efficiency gain and that from (potential) warm glow separately in the table and interpret the gain from warm glow with this caution. Recall that the inverse demand is $p(x) = [x/(\exp(a) \cdot \exp(\alpha D))]^{1/\epsilon}$. The surplus from warm glow is, therefore, obtained by $\int_{x^*}^{\bar{x}} \left([x/\exp(a)]^{1/\epsilon} - [x/(\exp(a) \cdot \exp(\alpha))]^{1/\epsilon} \right) dx$, in which parameters a, α, and ϵ are obtained from the field experiment.

We provide the sum of the efficiency gain and warm glow in the last column of Table 9. The results suggest that if we take account of a positive gain from warm glow, the total welfare gains from the moral suasion policy can be close to the gains from the economic incentive policy in the short-run. However, this is not the case for the repeated intervention, in which the welfare gain is much larger for the economic incentive policy even if we incorporate potential gains from warm glow. Finally, these results suggest that while in theory welfare gains can arise from the warm glow effect in theory, the major welfare gains in our context arise from the efficiency gains—from letting consumers pay prices that reflect the actual marginal cost of electricity during the critical peak hours.

5 Conclusion

In this paper, we used a randomized field experiment to study the persistence of moral suasion and economic incentives and its welfare implications. Using high-frequency electricity usage data at the household level, we found that moral suasion induced a usage reduction of 8 percent in the short run, which is economically and statistically significant for improving economic efficiency in electricity markets. However, the effect diminished quickly when we repeated the interventions. In contrast, the economic incentive group showed usage reductions of 14 percent for the lowest critical peak price and usage reductions of 17 percent for the highest critical peak price. Moreover, the effect was persistent over repeated interventions. Economic incentives also resulted in a habit formation after we withdrew the treatments. Our follow-up survey data indicated that most of the persistent changes were likely to originate from behavioral changes in lifestyle rather than durable goods investments. Finally, in the welfare analysis, we highlighted that both policy instruments would create substantial welfare gains to electricity markets, though the welfare gains from the economic incentive dominate those from moral suasion when we consider persistence for repeated interventions.

References

Allcott, Hunt. Forthcoming. "Site Selection Bias in Program Evaluation." *Quarterly Journal of Economics.*

Allcott, Hunt, and Todd Rogers. 2014. "The Short-Run and Long-Run Effects of Behavioral Interventions: Experimental Evidence from Energy Conservation." *American Economic Review*, 104(10): 3003–37.

Andreoni, James. 1989. "Giving with Impure Altruism: Applications to Charity and Ricardian Equivalence." *Journal of Political Economy*, 97(6): 1447–58.

Ariely, Dan, Anat Bracha, and Stephan Meier. 2009. "Doing Good or Doing Well? Image Motivation and Monetary Incentives in Behaving Prosocially." *The American Economic Review*, 99(1): 544–555.

Becker, Gary S., and Kevin M. Murphy. 1988. "A Theory of Rational Addiction." *Journal of Political Economy*, 96(4): 675–700.

Bénabou, Roland, and Jean Tirole. 2003. "Intrinsic and Extrinsic Motivation." *The Review of Economic Studies*, 70(3): 489–520.

Bénabou, Roland, and Jean Tirole. 2006. "Incentives and Prosocial Behavior." *American Economic Review*, 96(5): 1652–1678.

Borenstein, Severin. 2002. "The Trouble With Electricity Markets: Understanding California's Restructuring Disaster." *Journal of Economic Perspectives*, 16(1): 191–211.

Charness, Gary, and Uri Gneezy. 2009. "Incentives to Exercise." *Econometrica*, 77(3): 909–931.

Chetty, Raj, Emmanuel Saez, and László Sándor. 2014. "What Policies Increase Prosocial Behavior? An Experiment with Referees at the Journal of Public Economics." *Journal of Economic Perspectives*, 28(3): 169–188.

Chetty, R., A. Looney, and K. Kroft. 2009. "Salience and taxation: Theory and evidence." *The American Economic Review*, 99(4): 1145–1177.

Cutter, W. Bowman, and Matthew Neidell. 2009. "Voluntary information programs and environmental regulation: Evidence from 'Spare the Air'." *Journal of Environmental Economics and Management*, 58(3): 253–265.

Dal Bó, Ernesto, and Pedro Dal Bó. 2014. ""Do the right thing:" The effects of moral suasion on cooperation." *Journal of Public Economics*, 117: 28–38.

DellaVigna, Stefano, John A. List, and Ulrike Malmendier. 2012. "Testing for Altruism and Social Pressure in Charitable Giving." *The Quarterly Journal of Economics*, 127(1): 1–56.

Duflo, Esther, Rachel Glennerster, and Michael Kremer. 2007. "Chapter 61 Using Randomization in Development Economics Research: A Toolkit." In *Handbook of Development Economics.* Vol. Volume 4, , ed. T. Paul Schultz and John A. Strauss, 3895–3962. Elsevier.

Dwenger, Nadja, Henrik Kleven, Imran Rasul, and Johannes Rincke. 2014. "Extrinsic and Intrinsic Motivations for Tax Compliance: Evidence from a Field Experiment in Germany." *Working Paper.*

Faruqui, Ahmad, and Sanem Sergici. 2011. "Dynamic pricing of electricity in the mid-Atlantic region: econometric results from the Baltimore gas and electric company experiment." *Journal of Regulatory Economics*, 40(1): 82–109.

Ferraro, Paul J., and Michael K. Price. 2013. "Using Nonpecuniary Strategies to Influence Behavior: Evidence from a Large-Scale Field Experiment." *Review of Economics and Statistics*, 95(1): 64–73.

Ferraro, Paul J, Juan Jose Miranda, and Michael K Price. 2011. "The Persistence of Treatment Effects with Norm-Based Policy Instruments: Evidence from a Randomized Environmental Policy Experiment." *American Economic Review*, 101(3): 318–322.

Finkelstein, Amy. 2009. "E-ZTAX: Tax Salience and Tax Rates." *The Quarterly Journal of Economics*, 124(3): 969–1010.

Frey, Bruno S., and Felix Oberholzer-Gee. 1997. "The Cost of Price Incentives: An Empirical Analysis of Motivation Crowding- Out." *The American Economic Review*, 87(4): 746–755.

Gerard, Francois. 2013. "The Impact and Persistence of Ambitious Energy Conservation Programs: Evidence from the 2001 Brazilian Electricity Crisis." Social Science Research Network SSRN Scholarly Paper ID 2097195, Rochester, NY.

Gneezy, Uri, and Aldo Rustichini. 2000. "Pay Enough or Don't Pay at All." *The Quarterly Journal of Economics*, 115(3): 791–810.

Gneezy, Uri, and John A List. 2006. "Putting Behavioral Economics to Work: Testing for Gift Exchange in Labor Markets Using Field Experiments." *Econometrica*, 74(5): 1365–1384.

Gneezy, Uri, Stephan Meier, and Pedro Rey-Biel. 2011. "When and Why Incentives (Don't) Work to Modify Behavior." *Journal of Economic Perspectives*, 25(4): 191–210.

Holland, Stephen P., and Erin T. Mansur. 2008. "Is Real-Time Pricing Green? The Environmental Impacts of Electricity Demand Variance." *Review of Economics and Statistics*, 90(3): 550–561.

Ito, Koichiro. 2014. "Do Consumers Respond to Marginal or Average Price? Evidence from Nonlinear Electricity Pricing." *American Economic Review*, 104(2): 537–63.

Ito, Koichiro. Forthcoming. "Asymmetric Incentives in Subsidies: Evidence from a Large-Scale Electricity Rebate Program." *American Economic Journal: Economic Policy*.

Jessoe, Katrina, and David Rapson. 2014. "Knowledge is (Less) Power: Experimental Evidence from Residential Energy Use." *American Economic Review*, 104(4): 1417–38.

Joskow, Paul L. 2012. "Creating a Smarter U.S. Electricity Grid." *Journal of Economic Perspectives*, 26(1): 29–48.

Joskow, Paul L., and Catherine D. Wolfram. 2012. "Dynamic Pricing of Electricity." *American Economic Review*, 102(3): 381–85.

Kingma, Bruce Robert. 1989. "An accurate measurement of the crowd-out effect, income effect, and price effect for charitable contributions." *The Journal of Political Economy*, 1197–1207.

Kotchen, Matthew J. 2006. "Green markets and private provision of public goods." *Journal of Political Economy*, 114(4): 816–834.

Kotchen, Matthew J., and Michael R. Moore. 2007. "Private provision of environmental public goods: Household participation in green-electricity programs." *Journal of Environmental Economics and Management*, 53(1): 1–16.

Kreps, David M. 1997. "Intrinsic Motivation and Extrinsic Incentives." *The American Economic Review*, 87(2): 359–364.

Lacetera, Nicola, Mario Macis, and Robert Slonim. 2012. "Will There Be Blood? Incentives and Displacement Effects in Pro-Social Behavior." *American Economic Journal: Economic Policy*, 3(4): 186–223.

Landry, Craig E., Andreas Lange, John A. List, Michael K. Price, and Nicholas G. Rupp. 2006. "Toward an Understanding of the Economics of Charity: Evidence from a Field Experiment." *The Quarterly Journal of Economics*, 121(2): 747–782.

Landry, Craig E, Andreas Lange, John A List, Michael K Price, and Nicholas G Rupp. 2010. "Is a Donor in Hand Better than Two in the Bush? Evidence from a Natural Field Experiment." *American Economic Review*, 100(3): 958–983.

Landry, Craig E., Andreas Lange, John A. List, Michael K. Price, and Nicholas G. Rupp. 2011. "The hidden benefits of control: Evidence from a natural field experiment." National Bureau of Economic Research 17473.

Loewenstein, George. 2005. "Hot-cold empathy gaps and medical decision making." *Health Psychology: Official Journal of the Division of Health Psychology, American Psychological Association*, 24(4 Suppl): S49–56.

Reiss, Peter C., and Matthew W. White. 2008. "What changes energy consumption? Prices and public pressures." *RAND Journal of Economics*, 39(3): 636–663.

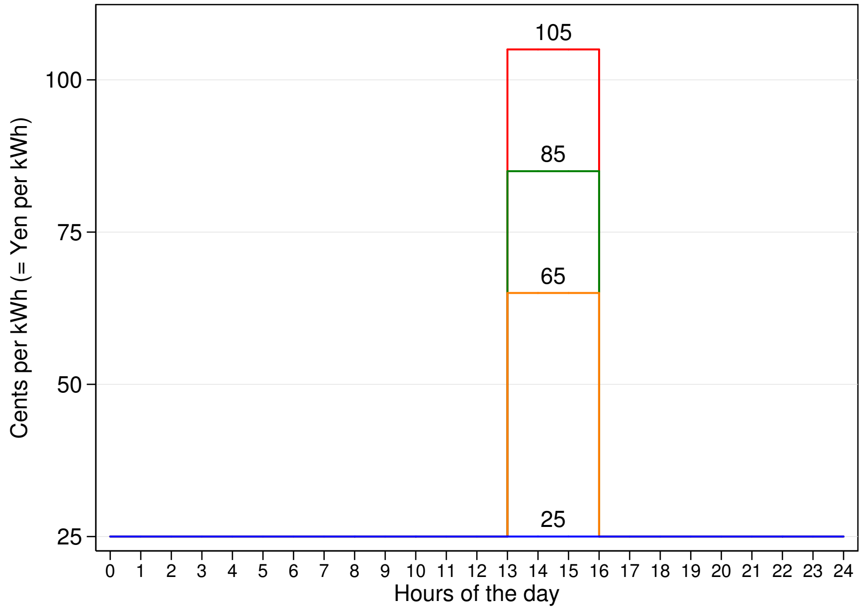
Volpp, Kevin G., Andrea B. Troxel, Mark V. Pauly, Henry A. Glick, Andrea Puig, David A. Asch, Robert Galvin, Jingsan Zhu, Fei Wan, Jill DeGuzman, Elizabeth Corbett, Janet Weiner, and Janet Audrain-McGovern. 2009. "A Randomized, Controlled Trial of Financial Incentives for Smoking Cessation." *New England Journal of Medicine*, 360(7): 699–709. PMID: 19213683.

Wolak, Frank A. 2006. "Residential customer response to real-time pricing: the Anaheim critical-peak pricing experiment." *Working Paper.*

Wolak, Frank A. 2010. "An Experimental Comparison of Critical Peak and Hourly Pricing: The PowerCentsDC Program." *Stanford Univeristy Working Paper.*

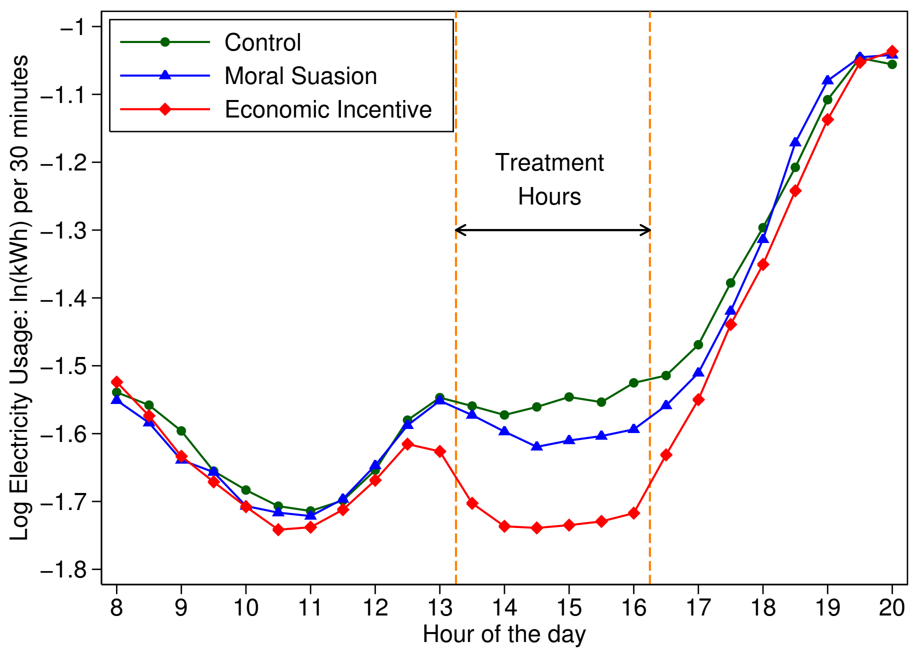
Wolak, Frank A. 2011. "Do Residential Customers Respond to Hourly Prices? Evidence from a Dynamic Pricing Experiment." *The American Economic Review*, 101(3): 83–87.

Figure 1: Economic Incentives: Dynamic Electricity Pricing



Notes: This figure shows the dynamic electricity pricing schedule for the economic incentive group and the baseline price (25 cents/kWh). Although our participants paid in Japanese yen, we use U.S. currency throughout the paper. One Japanese yen was approximately equivalent to one U.S. cent in 2012.

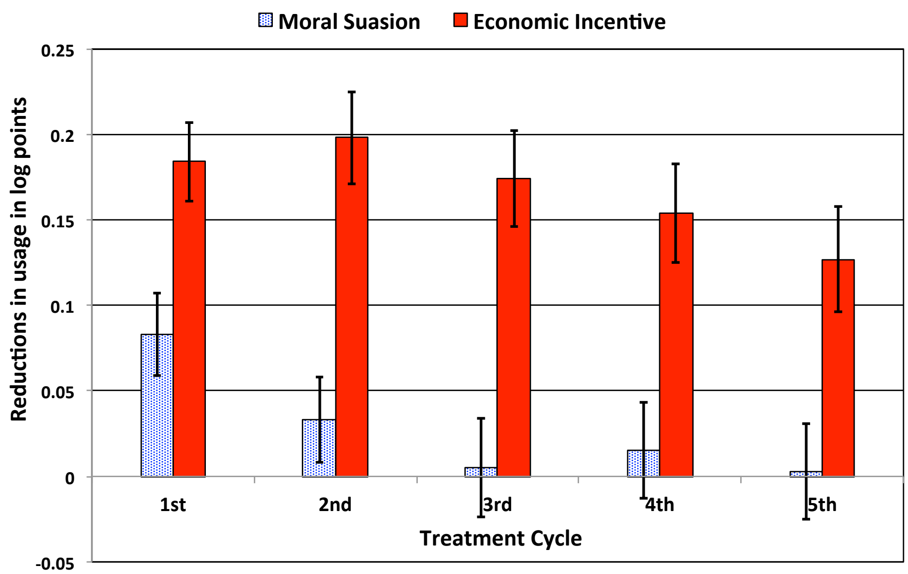
Figure 2: Effects of Moral Suasion and Economic Incentives on Electricity Usage



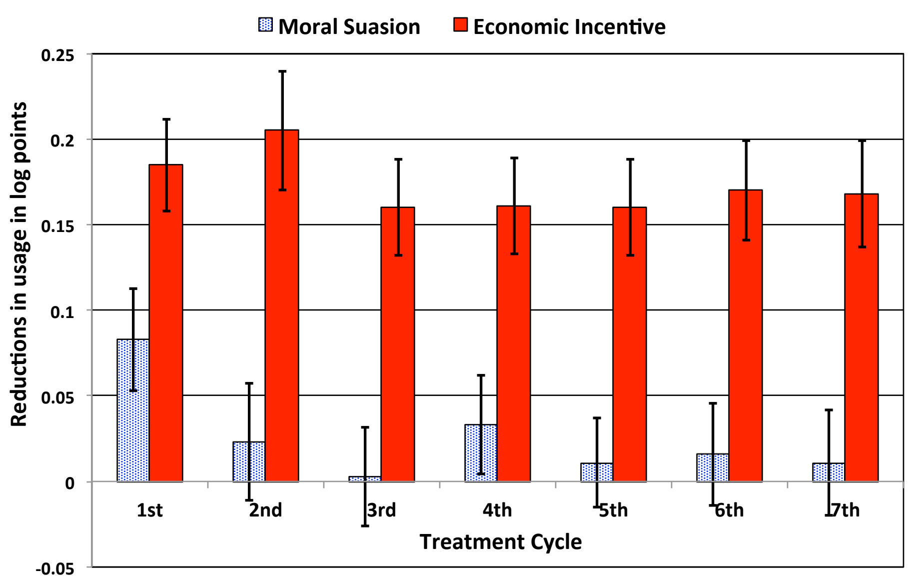
Notes: This figure shows the mean of log electricity usage (kWh) for 30-minute intervals by treatment groups for the summer treatment days. We calculate the mean log usage using data from all treatment days in the summer.

Figure 3: Treatment Effects by Treatment Cycles

Panel A: Summer Experiment



Panel B: Winter Experiment



Notes: This figure shows the treatment effects of moral suasion and economic incentives by treatment cycles in terms of the reductions in electricity usage in log points. The estimates are obtained from the estimation results in Table 3. The interval bars show one standard error of the treatment effect. In the estimation, we include household fixed effects and time fixed effects for each 30-minute interval. The standard errors are clustered at the household level to adjust for serial correlation. Each cycle includes 3 treatment days. There were 15 treatment days in the summer and 21 treatment days in the winter.

Table 1: Summary Statistics

	Sample in the Field Experiment			Random	Difference
	Moral Suasion (M)	Economic Incentive (E)	Control Group (C)	Sample of Population (P)	[S.E.] (C)−(P)
Electricity use (kWh/day)	15.14	15.76	15.92	16.23	-0.31
	(6.91)	(8.49)	(8.47)	(7.97)	[0.86]
Household income (1,000USD)	66.74	66.59	67.06	66.83	0.22
	(31.49)	(31.34)	(31.01)	(41.81)	[3.93]
Square meter of the house	121.49	113.08	122.15	125.90	-3.75
	(57.54)	(46.92)	(46.52)	(59.65)	[5.41]
Number of AC	3.46	3.50	3.68	3.95	-0.28
	(1.93)	(1.67)	(1.64)	(1.71)	[0.16]
Mean age of the household	42.26	42.22	40.31	41.91	-1.60
	(17.67)	(19.07)	(17.38)	(16.76)	[1.56]
Age of building (years)	13.83	13.39	13.12	15.05	-1.92
	(8.25)	(7.54)	(8.20)	(8.11)	[0.75]
Household Size	3.21	3.14	3.32	2.98	0.34
	(1.18)	(1.23)	(1.25)	(1.41)	[0.13]

Notes: The first 3 columns show the sample mean and standard deviation of observables by treatment group. Because of the random assignment, the observables are balanced across the 3 groups. Column 4 shows the mean and standard deviation of observables for a random sample of the population in the area of our experiment. We collected the data to investigate the external validity of our sample. Column 5 presents the difference in the means between the field experiment's control group and the random sample. Standard deviations are in parentheses in columns 1 to 4, and standard errors are in brackets in column 5.

Table 2: Effects of Moral Suasion and Economic Incentives on Electricity Usage

	Summer		Winter	
	(1)	(2)	(3)	(4)
Moral suasion	-0.031	-0.031	-0.032	-0.032
	(0.014)	(0.014)	(0.020)	(0.020)
Economic incentive	-0.167		-0.173	
	(0.021)		(0.022)	
Economic incentive (price = 65)		-0.151		-0.163
		(0.022)		(0.024)
Economic incentive (price = 85)		-0.167		-0.164
		(0.023)		(0.023)
Economic incentive (price = 105)		-0.182		-0.189
		(0.024)		(0.024)
Observations	123106	123106	244891	244891

Notes: This table shows the estimation results for equation (1) for the treatment hours. The dependent variable is the log of household-level 30-minute interval electricity consumption. We include household fixed effects and time fixed effects for each 30-minute interval. The standard errors are clustered at the household level to adjust for serial correlation. The difference between the coefficients for 65 and 105 cents is statistically significant at the 5 percent level. The implied price elasticity estimates are −0.136 (0.017) for the summer and −0.141 (0.018) for the winter.

Table 3: Repeated Interventions and Persistence of Treatment Effects

	Summer		Winter	
	Moral Suasion (α_c)	Economic Incentive (β_c)	Moral Suasion (α_c)	Economic Incentive (β_c)
1st cycle	-0.083	-0.184	-0.083	-0.185
	(0.024)	(0.023)	(0.030)	(0.027)
2nd cycle	-0.033	-0.198	-0.023	-0.205
	(0.025)	(0.027)	(0.034)	(0.035)
3rd cycle	-0.005	-0.174	0.003	-0.160
	(0.029)	(0.028)	(0.029)	(0.028)
4th cycle	-0.015	-0.154	-0.033	-0.161
	(0.028)	(0.029)	(0.029)	(0.028)
5th cycle	-0.003	-0.127	-0.011	-0.160
	(0.028)	(0.031)	(0.026)	(0.028)
6th cycle			-0.016	-0.170
			(0.030)	(0.029)
7th cycle			-0.011	-0.168
			(0.031)	(0.031)

Notes: This table shows the estimation results for equation (2). The dependent variable is the log of household-level 30-minute interval electricity consumption. We include household fixed effects and time fixed effects for each 30-minute interval. The standard errors are clustered at the household level to adjust for serial correlation. Each cycle includes 3 treatment days. There were 15 treatment days in the summer and 21 treatment days in the winter. The treatment days were not necessarily consecutive.

Table 4: Spillover Effects for Nontreatment Hours on Treatment Days

	Summer			Winter		
	Treatment Hours (1pm-4pm)	Shoulder Hours (10am-1pm, 4pm-7pm)	Other Hours	Treatment Hours (6pm-9pm)	Shoulder Hours (3pm-6pm, 9pm-12pm)	Other Hours
	(1)	(2)	(3)	(4)	(5)	(6)
Moral suasion	-0.031	-0.010	-0.008	-0.032	-0.009	-0.007
	(0.014)	(0.010)	(0.005)	(0.020)	(0.015)	(0.012)
Economic incentive	-0.167	-0.060	-0.022	-0.173	-0.034	-0.007
	(0.021)	(0.015)	(0.010)	(0.022)	(0.017)	(0.014)
Observations	123106	248621	634387	244891	482902	1182574

Notes: This table shows the estimation results for equation (1) for the treatment hours and other hours on the treatment days. The shoulder hours are 3 hours before and after the treatment hours. Columns 3 and 6 include nontreatment hours except for the shoulder hours. The dependent variable is the log of household-level 30-minute interval electricity consumption. We include household fixed effects and time fixed effects for each 30-minute interval. The standard errors are clustered at the household level to adjust for serial correlation.

Table 5: Habit Formation After the Treatments Were Withdrawn

	After Summer Experiment (1)	After Winter Experiment (2)
Moral suasion	0.006 (0.028)	0.021 (0.026)
Economic incentive	-0.077 (0.034)	-0.069 (0.022)
Observations	426770	478605

Notes: This table shows the estimation results for equation (1) for the three-month period after we withdrew our treatments. Column 1 shows the result for usage in peak demand hours (1pm to 4 pm) after the summer experiment. Column 2 shows the result for usage in peak demand hours (6pm to 9 pm) after the winter experiment. The dependent variable is the log of household-level 30-minute interval electricity consumption. We include household fixed effects and time fixed effects for each 30-minute interval. The standard errors are clustered at the household level to adjust for serial correlation.

Table 6: Heterogeneity in the Treatment Effects

	Summer			Winter		
	(1)	(2)	(3)	(4)	(5)	(6)
Moral suasion	-0.044 (0.014)	-0.045 (0.014)	-0.045 (0.014)	-0.034 (0.022)	-0.034 (0.022)	-0.034 (0.022)
Economic incentive	-0.168 (0.022)	-0.178 (0.023)	-0.178 (0.023)	-0.178 (0.023)	-0.177 (0.024)	-0.177 (0.023)
Moral suasion × Income	-0.052 (0.029)		-0.054 (0.030)	-0.002 (0.040)		-0.003 (0.040)
Economic incentive × Income	0.119 (0.051)		0.126 (0.050)	0.108 (0.046)		0.100 (0.046)
Moral suasion × Usage		0.058 (0.089)	0.069 (0.089)		0.005 (0.117)	0.007 (0.119)
Economic incentive × Usage		-0.516 (0.178)	-0.531 (0.171)		0.138 (0.117)	0.072 (0.117)
Observations	105107	105107	105107	205357	205357	205357

Notes: This table shows the estimation results for equation (1) with the interaction terms of the treatment dummies with demographic variables. The income variable is in $100,000. The usage variable is the average daily electricity usage in the preexperimental period. The dependent variable is the log of household-level 30-minute interval electricity consumption. We include household fixed effects and time fixed effects for each 30-minute interval. The standard errors are clustered at the household level to adjust for serial correlation.

Table 7: Effects of Moral Suasion and Economic Incentives on Durable Goods Investments

	Dependent variable: binary choice				
	Room AC	Refrigerator	Washer	Electric fan	Light bulb
	(1)	(2)	(3)	(4)	(5)
Moral suasion	0.08	0.01	0.01	-0.00	0.03
	(0.04)	(0.03)	(0.03)	(0.05)	(0.05)
Economic incentive	0.09	-0.01	0.01	-0.01	-0.03
	(0.03)	(0.03)	(0.02)	(0.04)	(0.04)
Constant	0.06	0.08	0.05	0.23	0.29
	(0.03)	(0.02)	(0.02)	(0.03)	(0.04)
Observations	640	640	640	640	640

Notes: We asked customers if they purchased an energy-efficient appliance since the experiment started. We estimate a linear probability model, with a binary choice dependent variable, dummy variables for the two treatment groups as independent variables, and a constant term. The constant term, therefore, provides the ratio of control customers who purchased an energy-efficient appliance. The coefficients for the group dummy variables provide a percentage point increase for the group. The robust standard errors are in parentheses. We had 51 customers who did not respond to this question. However, the number of non-response is balanced across the three groups.

Table 8: Effects of Moral Suasion and Economic Incentives on Behavioral Changes in Lifestyles

	Energy-efficient lifestyle (Degree: 1 to 5)	Energy-efficient use of appliances (Dependent variable: binary choice)				
		AC	Heater	PC	Washer	Cleaner
	(1)	(2)	(3)	(4)	(5)	(6)
Moral suasion	0.13	-0.00	0.08	0.01	-0.03	-0.03
	(0.08)	(0.06)	(0.06)	(0.05)	(0.04)	(0.04)
Economic incentive	0.40	0.13	0.15	0.09	0.08	0.12
	(0.07)	(0.05)	(0.05)	(0.04)	(0.03)	(0.04)
Constant	3.03	0.61	0.53	0.11	0.08	0.07
	(0.06)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
Observations	626	626	626	626	626	626

Notes: After the experimental period, we asked customers two questions. The first question was whether they were trying to have an energy-efficient lifestyle. Customers rated their lifestyles on a scale of 1 (lowest) to 5 (highest) in terms of energy efficiency of their lifestyles. We regress this score on the dummy variable for each treatment group and a constant term. Second, we asked consumers whether they were using each of the following electric appliances in an energy-efficient way: air conditioners, electric heaters, personal computers, washers, and vacuum cleaners. We estimate a linear probability model, which includes a binary choice dependent variable, dummy variables for the two treatment groups as the independent variables, and a constant term. The robust standard errors are in parentheses. We had 65 customers who did not respond to this question. However, the number of non-response is balanced across the three groups.

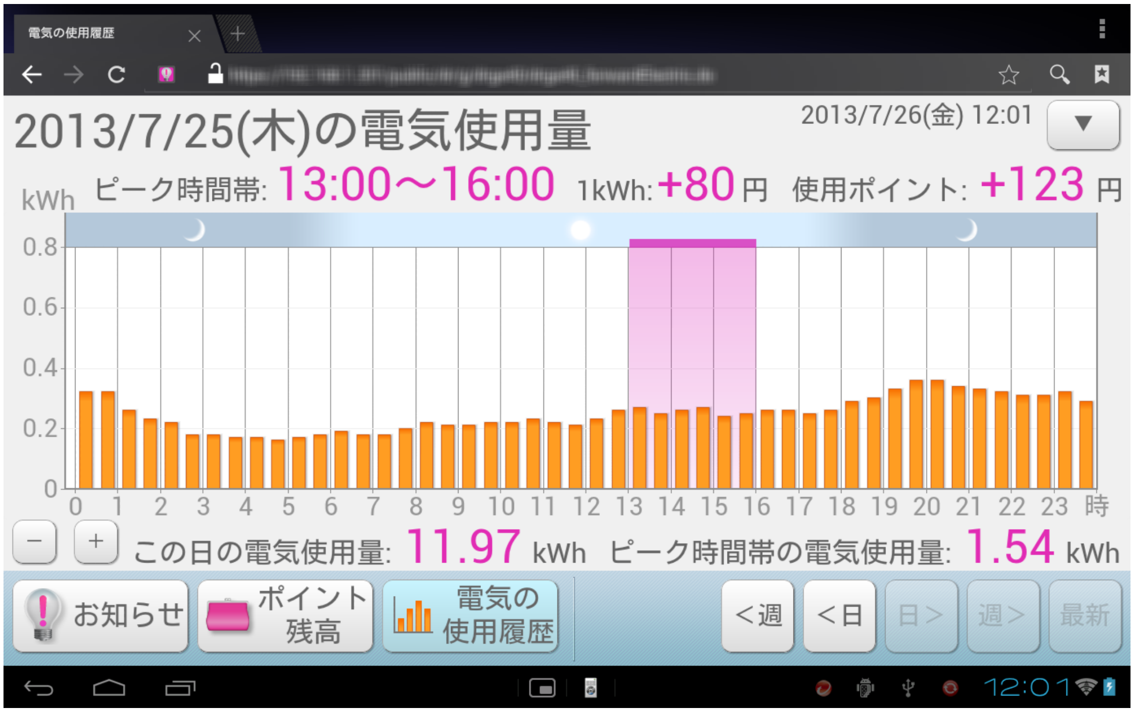
Table 9: Welfare Gains from the Two Policies (Assumption on Marginal Cost = 65 cents/kWh)

	Economic Incentive	Moral Suasion	
	Efficiency Gain ($M)	Efficiency Gain ($M)	Efficiency Gain + Warm Glow ($M)
Short-Run Treatments (3 days)	16.84	11.37	15.02
	(1.99)	(2.55)	(4.62)
Repeated Treatments (15 days)	76.55	24.40	27.32
	(9.04)	(9.92)	(12.38)

Notes: This table shows the estimated welfare gains per season from the two policies in our field experiment. We use 46,800 kWh as the peak hour residential electricity consumption in the Japanese electricity market for the baseline case, which does not refer to either of our policies. We use 65 cents/kWh as the marginal cost of electricity for these critical peak hours. In the Appendix, we provide the same analyses for different assumptions of the marginal cost of electricity.

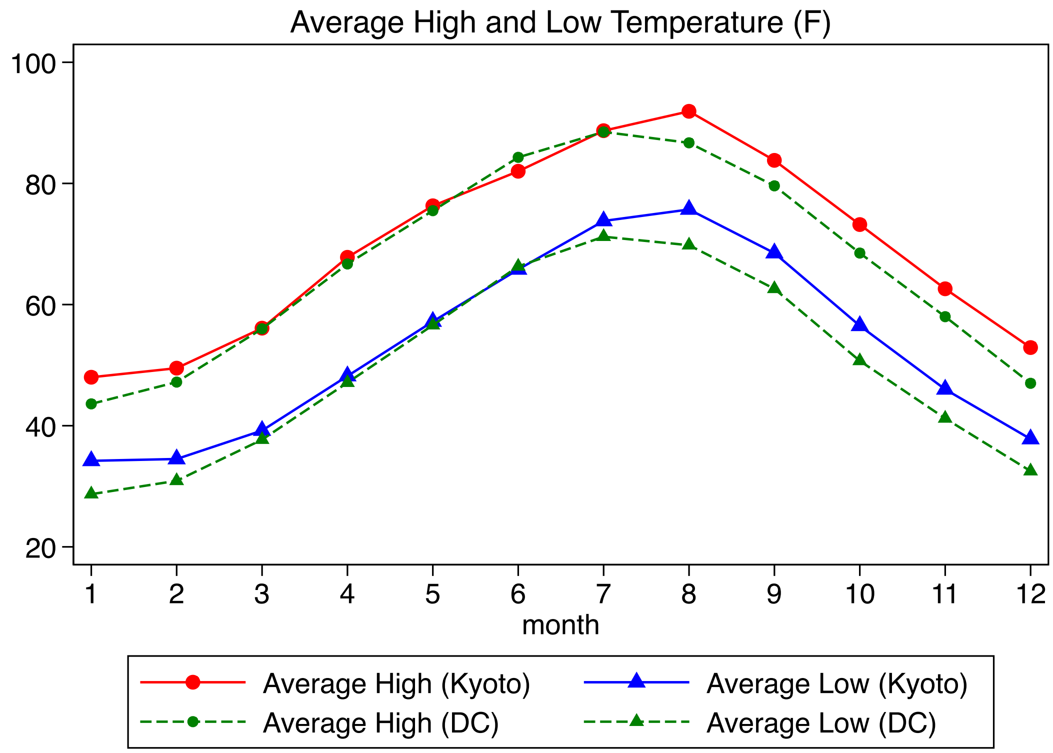
Online Appendix A: Appendix Figures and Tables (Not For Publication)

Figure A.1: Information Provided by an In-Home Display



Notes: This figure shows an example screenshot of the in-home displays that were installed for both the control and the treatment consumers in the experiment. On the top of the figure, it shows "Electricity usage for July 25, 2013. Peak hours: 13:00 to 16:00. The price increase is +80 yen per kWh." The figure in the middle shows usage in kWh for each 30-minute interval from hour 0 to hour 24 of the day. The shaded area shows the peak hours, which are from 13:00 to 16:00. On the bottom of the figure, it shows "The daily electricity usage is 11.97 kWh. Usage for the peak hours is 1.54 kWh."

Figure A.2: Average High and Low Temperatures in Kyoto, Japan and Washington D.C., United States



Notes: This figure compares the average high and low temperatures (°F) in Kyoto, Japan and Washington D.C., United States.

Table A.1: Welfare Gains from the Two Policies (When Marginal Cost = 85 cents/kWh)

	Economic Incentive	Moral Suasion	
	Efficiency Gain ($M)	Efficiency Gain ($M)	Efficiency Gain + Warm Glow ($M)
Short-Run Treatments (3 days)	26.15	17.38	22.11
	(3.08)	(3.98)	(6.69)
Repeated Treatments (15 days)	118.88	36.91	40.65
	(14.03)	(15.13)	(18.29)

Notes: This table shows the estimated welfare gains per season from the two policies in our field experiment. We use 46,800 kWh as the peak hour residential electricity consumption in the Japanese electricity market for the baseline case, which does not refer to either of our policies. For this table, we use 85 cents/kWh as the marginal cost of electricity for these critical peak hours. In the Appendix, we provide the same analyses for different assumptions of the marginal cost of electricity.

Table A.2: Welfare Gains from the Two Policies (When Marginal Cost = 105 cents/kWh)

	Economic Incentive	Moral Suasion	
	Efficiency Gain ($M)	Efficiency Gain ($M)	Efficiency Gain + Warm Glow ($M)
Short-Run Treatments (3 days)	35.78	23.51	29.10
	(4.21)	(5.47)	(8.70)
Repeated Treatments (15 days)	162.65	49.50	53.89
	(19.18)	(20.41)	(24.12)

Notes: This table shows the estimated welfare gains per season from the two policies in our field experiment. We use 46,800 kWh as the peak hour residential electricity consumption in the Japanese electricity market for the baseline case, which does not refer to either of our policies. For this table, we use 105 cents/kWh as the marginal cost of electricity for these critical peak hours. In the Appendix, we provide the same analyses for different assumptions of the marginal cost of electricity.

Online Appendix B: Materials from the Field Experiment

Invitation Letter (Translated in English)

The Keihanna Eco-City Next-Generation Energy/Community System Demonstration Project

Questionnaire for Assessing Interest in Participating in the Smart Power Usage Program

The Keihanna Eco-City Next-Generation Energy/Community System Demonstration Project Promotion Council created with the support of the Ministry of Economy, Trade and Industry, Keihanna Science City's Next-Generation Energy/Community System Demonstration Project consists of a variety of initiatives designed to create a leading low-carbon community in Japan. As part of this project, we have recently started a power usage demonstration program. As part of this program, we request several households to adopt an energy-saving but easily sustainable lifestyle. We have created this questionnaire to assess the interest of Keihanna Science City residents participating in the program. Please take some time to read and complete this questionnaire. Thank you for your cooperation.

Points to Note before Filling Out the Questionnaire

- Respondents who agree to participate in the program and receive an at-home program briefing will be rewarded with a 1,000-yen prepaid card.
- Read the program overview (on the other side of this sheet) before responding to the questionnaire (separate sheet).
- Place the completed questionnaire in the prepaid return envelope provided and mail it before February 13 (Mon).

■ Questionnaire participants

This questionnaire was distributed by Japan Post's Yu-Mail designated delivery area service after selecting survey areas from among the districts of Keihanna Science City (Kyotanabe, Kizugawa, and Seika).

■ Terms of privacy for personal information

Personal information obtained using this questionnaire will be rigorously managed by the questionnaire administrator, Mitsubishi Heavy Industries. It will be used only to implement the Smart Power Usage Program and for no other purposes. If information about your electric power agreement, facilities, or usage is required for the program, Mitsubishi Heavy Industries will request the information from the Kansai Electric Power Company, and the Kansai Electric Power Company will provide Mitsubishi Heavy Industries with the information requested about your electric power agreement, facilities, or usage.

The Keihanna Eco-City Next-Generation Energy/Community System Demonstration Project Promotion Council

Members: Kyoto Prefecture, City of Kyotanabe, City of Kizugawa, Town of Seika, Public Foundation of Kansai Research Institute, Kansai Electric Power Company, Mitsubishi Electric Corporation, Mitsubishi Heavy Industries, other private-sector companies

For inquiries about the questionnaire, contact the questionnaire administrator organization below.

Questionnaire administrator organization: Regional Futures Research Center

Staff members: Horibe, Yoshiura, Tabuchi

Tel. (toll-free): 0120-79-7711 (9:30~17:00, except weekends and holidays)

Please continue to the program overview on the other side.

Program Overview

Three aspects of smart power usage

● The program will use modern telecommunications technology to create smarter and more streamlined power use by equipping households to moderate their power usage volume and adopt energy-saving habits.

✓ Awareness of energy-saving timing

✓ Visibility of power wastage

✓ Advice from other households

Program Description

● Participating households will engage in some of the following activities.

– The activities will vary depending on households and will be set at random according to the needs of the survey.

–Participants will not incur any cost as a result of taking part in these activities.

Activity 1 Setting variable power charges

● We will provide simulated power charges that vary in time slots of rising power demand.

● You will work on moderating your power usage as much as possible in time slots of high power charges (about 2 or 3 hours during the day).

Activity 2 Providing information on power usage

● We will provide a system enabling participating households to check their power usage every hour.

● You will check your power usage in each time slot and devise ways to minimize wasteful power usage.

Activity 3 Providing energy-saving advice

● After analyzing your power usage, we will advise you on areas such as power-usage methods and replacing appliances (in 2013 summer).

● You will follow the above advice to reduce wasteful power use.

You will not be pressured into replacing appliances.

Program Period, Rewards

● We plan to conduct the program from July 2012 through the end of 2014.

● Participants will receive a small reward (in addition to the reward for completing this questionnaire).