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Moral Suasion and Economic Incentives: Field Experimental Evidence from Energy Demand†

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Firms and governments often use moral suasion and economic incentives to influence intrinsic and extrinsic motivations for economic activities. To investigate persistence of such interventions, we randomly assign households to moral suasion and dynamic pricing that stimulate energy conservation during peak-demand hours. We find significant habituation and dishabituation for moral suasion—the treatment effect diminishes after repeated interventions but can be restored to the original level by a sufficient time interval between interventions. Economic incentives induce larger treatment effects, little habituation, and significant habit formation. Our results suggest moral suasion and economic incentives produce substantially different short-run and long-run policy impacts. (*JEL* C93, D83, L94, L98, Q41, Q48)

Firms and governments often use moral suasion and economic incentives to influence intrinsic and extrinsic motivations for a variety of economic activities. For example, such interventions are frequently used by regulators to promote energy conservation (Reiss and White 2008), smoking cessation (Volpp et al. 2009), and tax compliance (Dwenger et al. 2016). Similar tools are widely used by firms and nonprofit organizations to stimulate academic refereeing (Chetty, Saez, and Sandor 2014), blood donations (Lacetera, Macis, and Slonim 2012), charitable giving (Landry et al. 2010), and exercise (Charness and Gneezy 2009).

A central question for economists and policymakers designing such policies is whether appealing to intrinsic and extrinsic motivations can generate persistent effects on economic activities (Gneezy, Meier, and Rey-Biel 2011). In this study,

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we begin with existing theories in economics and psychology that provide three key predictions for an individual's dynamic response to a policy intervention—*habituation* (Thompson and Spencer 1966), *dishabituation* (Rankin and Carew 1988), and *habit formation* (Becker and Murphy 1988). We characterize how these phenomena can interact with policy interventions that target intrinsic and extrinsic motivations.

We then empirically test our predictions in a field experiment in the context of electricity demand. The first treatment is moral suasion, by which policymakers attempt to influence intrinsic motivation to generate pro-social behavior.[1] The second treatment is an economic incentive, by which policymakers aim to affect extrinsic motivation based on standard demand theory. We randomly assign households to one of three groups: a moral suasion group, an economic incentive group, and a control group. Using household-level electricity consumption data of 30-minute intervals, we examine how our treatments affect electricity usage in peak-demand hours in which the marginal cost of electricity is substantially higher than other hours.

The moral suasion group receives messages requesting voluntary energy conservation during the peak-demand hours—between 1 PM and 4 PM on summer treatment days and between 5 PM and 8 PM on winter treatment days. The economic incentive group does not receive this message but is charged one of three high marginal prices for electricity in the same peak-demand hours. We design the experiment to test three primary hypotheses. First, we repeat the treatments over many days to examine habituation of the two treatments. Second, we discontinue the intervention for a while and then restart it, which allows us to test if restarting a treatment generates dishabituation—a decreased response via habituation might be restored back to original response levels. Finally, we collect electricity-usage data before, during, and after the interventions to investigate habit formation.

We present several findings from the experiment. First, moral suasion induces short-run reductions in electricity usage, but the effect diminishes quickly over repeated interventions, indicating strong habituation. The moral suasion group shows a usage reduction of 8 percent initially. However, their usage becomes statistically indistinguishable from that of the control group over further interventions. Second, we find that economic incentives create much larger and persistent effects. The economic incentive group shows 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 is much more persistent over repeated interventions, suggesting relatively little habituation compared to the case with moral suasion. Third, we examine dishabituation—whether habituated responses can be restored back to an original level. After the summer experiment, we purposely give households about a three-months interval before we restart our intervention in the winter. We find that the habituated response to moral suasion is "reset" (i.e., recovers back to the original level) when we restart our intervention in the winter. Fourth, we test potential habit formation by estimating treatment effects using data collected after we withdraw the

[1] Moral suasion is widely used by regulators for energy conservation (Reiss and White 2008, Costa and Gerard 2015), air quality preservation (Cutter and Neidell 2009), incentivizing workers (Dal Bó and Dal Bó 2014), discouraging tax evasion (Dwenger et al. 2016), and law enforcement (Fellner, Sausgruber, and Traxler 2013).

treatments. We find significant habit formation for the economic incentive group and no habit formation for the moral suasion group. After we withdraw the treatment, the moral suasion group's usage is indistinguishable from that of the control group. However, the economic incentive group continues to conserve energy even after the incentive is withdrawn.

What are the mechanisms behind the substantially different results for the moral suasion and economic incentive treatments? We investigate two potential mechanisms. The first possibility is investment in physical capital stock—households might have purchased energy-efficient appliances in response to treatment. If such an effect was systematically large for the economic incentive group, it could explain the persistent usage reductions, including weaker habituation and stronger habit formation. The second possibility is that the treatments might have induced new utilization habits for daily electricity use. Suppose that some households had "bad habits" of inefficient energy use before we began our experiment. Our interventions might have triggered a lifestyle change, helping them form good habits, namely, efficient energy use. If this effect was systematically large for the economic incentive group, it could explain these households' persistent usage reduction. Using follow-up survey data, we find no statistical evidence that investment in physical capital stock can explain our experimental findings. By contrast, we obtain supporting evidence that the changes in utilization habit are a key mechanism behind the results. We provide evidence that the economic incentive treatment induces a new utilization habit for electricity use—households in the economic incentive group form 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 survey responses, they provide suggestive evidence about the mechanisms behind our main findings.

This study makes three primary contributions to the literature and has key implications for economic policy. First, our experiment is the first study to directly compare the habituation, dishabituation, and habit formation of moral suasion and economic incentives. A few recent studies examine a subset of these phenomena—mostly habit formation—for either pecuniary or nonpecuniary incentives. Habit formation is studied by Charness and Gneezy (2009) for monetary incentives on exercise, and Ferraro, Miranda, and Price (2011) and Allcott and Rogers (2014) for nonmonetary incentives on water and energy conservation. Habituation is documented by Allcott and Rogers (2014) for the effect of social comparison. Finally, to our knowledge, dishabituation has not been studied in the economics literature. Our contribution to this new literature is that we characterize commonly phrased *persistence* or *long-run effects* of a treatment by habituation, dishabituation, and habit formation based on theories in economics and psychology, and empirically test all of the three phenomena for monetary and nonmonetary incentives in a unified field experiment.

The second contribution of this study is that we provide new evidence to the growing literature showing that consumers might not necessarily respond to marginal incentives (Borenstein 2009, Kahn and Wolak 2013, Ito 2014, Copeland and Garratt 2015, McRae and Meeks 2016). A central question in this literature is whether providing transparent price information can induce consumers to respond to marginal incentives.[2] Our experiment deliberately gives consumers salient information on hourly marginal prices via text messages and in-home displays, and then tests their responses to different marginal prices. While marginal prices in increasing block pricing and time-varying pricing are not directly comparable, our results suggest that consumers faced with transparent price information on time-varying pricing respond to marginal prices as standard economic theory predicts.

Third, our study contributes to the recent literature on energy and environmental economics studying the effects of pecuniary and nonpecuniary policies on the conservation of scarce resources. It has been challenging to separately identify the effects on monetary and nonmonetary incentives on conservation because, in nonexperimental data, a variety of policies usually affect consumers simultaneously. For example, Reiss and White (2008) and Costa and Gerard (2015) examine energy conservation programs in California and Brazil, acknowledging that it is empirically challenging to separate the effects of voluntary conservation from those of other policies during their sampling periods. Our study addresses this problem by randomly assigning consumers to either moral suasion or economic incentives in a field experiment.[3]

Finally, our results provide important implications for economic policy. Moral suasion has become increasingly common when policymakers aim to promote pro-social behavior. In practice, whenever a country or state encounters an energy crisis, one of the most debated policy topics is whether regulators should use moral suasion or economic incentives to mitigate the crisis. Historically, moral suasion has been favored politically in many states and countries. Our results on habituation, dishabituation, and habit formation have three key policy implications. In the very short run, both moral suasion and economic incentives are likely to be useful ways to induce pro-social behavior. However, moral suasion is more likely to have quick habituation, which makes the policy ineffective when a policy is repeated over time. Our findings on dishabituation or the "reset effect" imply that moral suasion can become effective again when it has not been used for a while, although the impact is again likely to wane with repeated use of the policy. We highlight these implications by providing back-of-the-envelope calculation of a policy evaluation in the online Appendix.

I. Habituation, Dis-habituation, and Habit Formation

Our empirical analysis aims to test the persistent effects of moral suasion and economic incentives. For this purpose, theories in economics and psychology

[2] Ito (2014) describes that a potential reason for his findings—residential electricity consumers in California do not respond to their marginal prices—is that information-acquiring costs are likely to be high for residential electricity consumers who have conventional monthly billing. A few recent studies conduct field experiments related to this question. Kahn and Wolak (2013) and McRae and Meeks (2016) find that information provision changes electricity consumers' responses to marginal prices. However, Chetty and Saez (2013) find that providing information about nonlinear tax incentives does not systematically affect earnings on average, although these authors also find evidence of heterogeneous treatment effects among taxpayers.

[3] Other examples of nonexperimental studies documenting intrinsically motivated conservation include Cutter and Neidell (2009), who study the "Spare the Air" program in California and Ferraro and Price (2013), who investigate water conservation.

provide useful guidance and characterize three key predictions about an individual's dynamic response to a stimulus.

Habituation is a theory formalized by several studies in psychology, including Thompson and Spencer (1966), Groves and Thompson (1970), and Rankin et al. (2009). It implies that repeated presentation of a stimulus might cause a decrease in reaction to the stimulus. For example, animals, such as cats, dogs, monkeys, and rats, strongly react to a stimulus (e.g., a loud sound in a laboratory experiment) when it is presented for the first time, but their responses often gradually wane when the same intervention is repeated over time. The opposite phenomenon is called *sensitization*, which implies that repeated presentation of a stimulus induces an increase in reaction to the stimulus. Because many economic policies in practice are implemented repeatedly over time, whether individuals exhibit habituation or sensitization is a central question for policy evaluation. Existing laboratory evidence in the psychology literature suggests that habituation is more ubiquitous than sensitization for most species. However, we are not aware of studies testing habituation and sensitization for moral suasion and financial incentives in a field experiment.

Another relevant key theory in psychology is *dishabituation*—declined responses, as a result of habituation, can be restored to an original level either by providing a new type of treatment, a stronger or weaker intensity of the same treatment, or the same treatment with a sufficient time interval between interventions. In particular, dishabituation obtained by the last approach—a proper time interval between interventions—is called *spontaneous recovery* of habituation. For policymakers, dishabituation is an important phenomenon because it could make a habituated policy impact effective again. Evidence of dishabituation is mostly from laboratory experiments in psychology, and there is little if any empirical evidence in the economics literature.

Finally, *habit formation* is a theory developed primarily in the recent economics literature (Becker and Murphy 1988, Rozen 2010). A short-run intervention might form a habit of consumption for the future, which influences the prolonged existence of policy impacts after the removal of the stimulus. The theory presented by (Becker and Murphy 1988) suggests that past consumption forms consumption capital, which drives habit formation. In addition, the authors suggest that consumption capital depreciates. That is, treatment effects might continue to exist after the final intervention but decay over time. Empirical evidence of habit formation is documented in many instances, including the effect of monetary incentives on exercise (Charness and Gneezy 2009) and nonmonetary incentives on water and energy conservation (Ferraro, Miranda, and Price 2011; Allcott and Rogers 2014). Our study is the first to test habit formation for moral suasion and compare it to habit formation for economic incentives.

We design our field experiment to test these three predictions—habituation, dishabituation, and habit formation—for moral suasion and economic incentives. In the next section, we begin by presenting our experimental design and data. We then describe hypotheses that we test in the experiment and explain why moral suasion and economic incentives might have different dynamic effects for the three phenomena.

II. Experimental Design, Data, and Hypotheses

A. *Experimental Design and Data*

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

In order to invite as broad a set of households as possible, we provided generous participation rewards, including free installations of an advanced meter and in-home display in addition to a participation reward of 24,000 yen (approximately $240 in 2012). We contacted all 40,710 residential electricity customers in the Keihanna area by mail.[4] 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 an RCT for a purely randomly selected sample of the population. Therefore, it is important to consider carefully the external validity of the experiment, although random assignment of treatments guarantees the internal validity of the experiment. To explore the external validity of our sample, we collected data from a random sample of the population in the corresponding geographical area. We analyzed the observables between our sample and the random sample, as outlined below.

We randomly assigned the 691 households to one of three 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. This group received no other treatment.

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.[5]

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

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

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 used consumption data from the summer of 2012 to the spring of 2013. In addition to the usage data, we collected data by three surveys. We conducted the first survey prior to treatment assignment and collected demographic information. Next, we conducted the second survey upon completion of the experiment to explore the mechanism behind our findings. Finally, we conducted the third survey for a random sample of 717 households in the area to investigate the external validity of our experimental sample.

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. Furthermore, very little attrition occurred in each group. In total, nine households (1.3 percent) were excluded from our sample because they moved residence. Because this small attrition occurred at approximately the same rate in each group, it is unlikely to bias our estimates significantly.

Column 4 shows the summary statistics for a random sample of the population in the corresponding geographical area. We investigated 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 found 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.

This analysis investigated the external validity of our experimental sample to the population in the corresponding geographical area. However, 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 (2015) finds "site selection bias" when he compares the estimates of Opower's home-energy report among its 111 randomized controlled trials. The idea behind site selection bias is that cities that participated in a field experiment might be different from other cities in their observable and unobservable characteristics. Our experiment is not free from the possibility of site selection bias if households in our experimental city (Kyoto) are systematically different from those in other cities.

Two pieces of information are relevant to this point. First, we conducted similar dynamic pricing experiments for different research questions in three other locations in Japan—Yokohama, Toyota, and Kitakyushu—between 2012 and 2013. For each location, we found about 15–20 percent usage reductions in peak-hour electricity usage from similar dynamic electricity-pricing treatments. This finding implies that the estimates from the current study in Kyoto are not substantially different from those from other locations. Second, although Kyoto was the city that hosted the Kyoto Protocol on climate change in 1997, a general perception is that households

Table 1—Summary Statistics

	Sample in the field experiment				
	Moral suasion (M)	Economic incentive (E)	Control group (C)	Random sample of population (P)	Difference between sample and population
Electricity use (kWh/day)	15.14	15.76	15.92	16.23	−0.45
	(6.91)	(8.49)	(8.47)	(7.97)	[0.61]
Household income (US$1,000)	66.74	66.59	67.06	66.83	−0.11
	(31.49)	(31.34)	(31.01)	(41.81)	[2.31]
Square meter of the house	121.49	113.08	122.15	125.90	−8.95
	(57.54)	(46.92)	(46.52)	(59.65)	[3.28]
Number of AC	3.46	3.50	3.68	3.95	−0.43
	(1.93)	(1.67)	(1.64)	(1.71)	[0.10]
Mean age of the household	42.26	42.22	40.31	41.91	−0.11
	(17.67)	(19.07)	(17.38)	(16.76)	[1.03]
Age of the building (years)	13.83	13.39	13.12	15.05	−1.62
	(8.25)	(7.54)	(8.20)	(8.11)	[0.47]
Household size	3.21	3.14	3.32	2.98	0.21
	(1.18)	(1.23)	(1.25)	(1.41)	[0.08]

Notes: The first three columns show the sample mean and standard deviation of observables by treatment group. Because of the random assignment, the observables are balanced across the groups. Column 4 shows the mean and standard deviation of observables for a random sample of the population in the experimental area (randomly sampled 717 households). Column 5 presents the difference in the means between the sample for the field experiment and the random sample of the population. Standard deviations are in parentheses in columns 1 to 4, and standard errors are in brackets in column 5.

in Kyoto are not necessarily more environmentally conscious than are households in other prefectures in Japan. For instance, Ida, Takemura, and Sato (2015) conduct a survey for households in major cities in Japan and find that preferences for energy and environmental policies are not systematically different between average Japanese households and those in Kyoto.[6] While these two points provide suggestive evidence that site selection bias might not be severe in our case, we cannot provide definite evidence for this concern unless we are able to conduct the same experiment for a random sample of households in Japan. Results from our experiments, therefore, should be interpreted with this caution when they are applied to policies outside the experimental region.

To contextualize typical weather patterns in the experimental region, we compared the monthly average high and low temperatures between the Kyoto prefecture in Japan (our experimental region) and Washington DC in the United States. We provide this comparison in Figure A.2 in the online 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°F in each month, which implies that these two cities have quite similar weather conditions, which determine the major part of electricity consumed, namely, usage for cooling and heating.

[6] Ida, Takemura, and Sato (2015) find that these preferences are systematically different between average Japanese households and those in the Fukushima prefecture, where the Fukushima Daiichi nuclear disaster occurred in 2011.

B. *Treatments*

Electricity consumers generally do not pay prices that reflect the relatively high marginal costs of electricity during peak-demand hours. This mismatch is a fundamental economic inefficiency in electricity markets (Borenstein 2002, Joskow 2012). Policymakers usually consider two types of economic policies to address this inefficiency. The first policy instrument, which many countries use most frequently, 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 designed two treatments that reflect the two policies used by regulators in practice.

Our first treatment is "moral suasion," which is intended to influence intrinsic motivation for energy conservation (Kreps 1997; Bénabou and Tirole 2003, 2006).[7] After we assigned customers to the moral suasion group (M), we informed them that energy conservation was required in the critical peak-demand hours. The message sent to this group after the group assignment was "substantial energy conservation will be required for the society in 'critical peak-demand hours' on summer and winter peak-demand days, in which electricity supply will be very limited relative to demand." Note that customers in this group did not receive any monetary incentives to conserve energy.

Before the experimental period began, we informed customers of how they were going to receive 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. A treatment day had to be a weekday in which the day-ahead maximum temperature forecast exceeded 31°C (88°F) for the summer and was lower than 14°C (57°F) for the winter.

To understand when and how consumers received the notifications of their treatments, consider an example treatment date, August 21. On the day before (August 20), the forecast maximum temperature for August 21 was reported to be above the cutoff level for treatment days. We delivered notifications to customers at 4 PM on August 20 by a text message to their in-home displays, cell phones, and computers. They were able to view the message on each device between 4 PM on August 20 and 4 PM on August 21. The text message sent to the moral suasion group was "Notice of Demand Response: In the following critical peak-demand hours, please reduce your electricity usage: 1 PM–4 PM on Tuesday, August 21."

Our second treatment is an "economic incentive," which was intended to influence extrinsic motivation for energy conservation. After we assigned customers to

[7] Note that the term, intrinsic motivation, is sometimes used slightly differently in economics and psychology. Our intervention of moral suasion came from an external authority but was aimed at influencing intrinsic motivation for energy conservation. This approach is similar to previous studies in economics such as Dwenger et al. (2016) that provided a shock to intrinsic motivations for tax compliance.

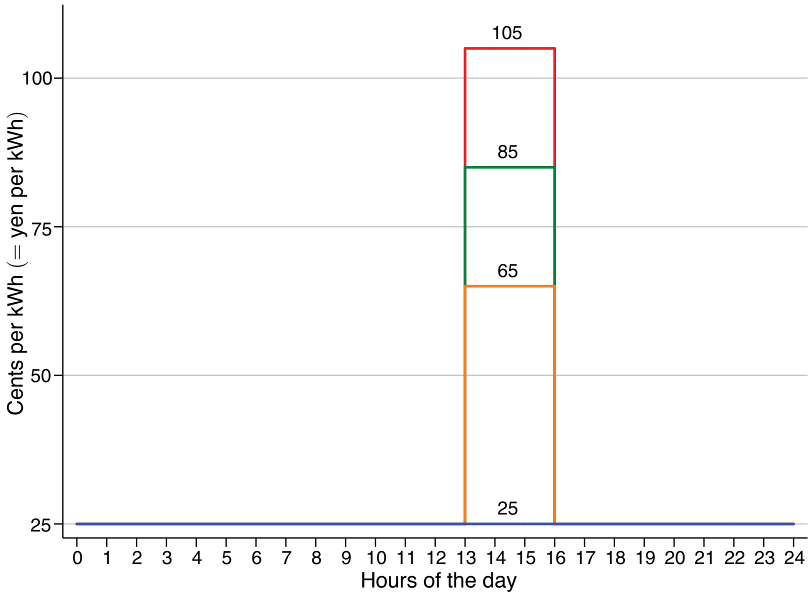

FIGURE 1. ECONOMIC INCENTIVES: DYNAMIC ELECTRICITY PRICING

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 US currency throughout the paper. One Japanese yen was approximately equivalent to one US cent in 2012.

the economic incentive group (E), we informed them that they would be charged high electricity prices during the critical peak-demand hours on the critical peak-demand days. Precisely, the same as we informed the moral suasion group, we told the economic incentive group that we would provide day-ahead notifications of critical peak days based on the day-ahead weather forecast.

Figure 1 shows the hourly price schedule for the economic incentive group. This price schedule is usually called variable critical peak pricing or critical peak pricing with variable peak prices because it consists of variable marginal prices for the critical peak hours. On treatment days, the economic incentive group had a price increase of 40, 60, or 80 cents/kWh. Because the baseline price was approximately 25 cents/kWh, these price increases mean that the critical peak price was 65, 85, or 105 cents/kWh.[8] For example, at 4 PM on August 20, the economic incentive group received this message, "Notice of Demand Response: In the following critical peak-demand hours, you will be charged a very high electricity price, so please reduce your electricity usage: 1 PM–4 PM on Tuesday, August 21. The price will be 85 yen (+ 60 yen) per kWh." Participants were able to view this message on their in-home displays, cell phones, and computers between 4 PM on August 20 and 4 PM on August 21.

For a given treatment day, all customers in the economic incentive group had the same critical peak price. We randomized their prices across the treatment days—depending on the treatment day, the marginal price for the critical peak hours was

[8] Customers paid in Japanese yen, but we use US currency throughout the paper. One Japanese yen was approximately equivalent to one US cent (2012 exchange rate).

different. We used stratified randomization to allocate the three critical peak prices (65, 85, and 105 cents/kWh) equally throughout the experiment period. First, we divided the treatment days into *treatment cycles*, which consisted of three treatment days. Then, we randomized the three critical peak prices within each cycle—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, in which we randomized the order of the three prices in each cycle. We repeated the interventions as long as the temperature forecasts met the threshold. In total, the treatment groups experienced 15 treatment days (five cycles) in the summer and 21 treatment days (seven cycles) in the winter.

As an example, consider two treatment cycles around August 21, which we used as an example of treatment days in the beginning of this subsection. The day-ahead forecasts for the maximum temperatures exceeded the threshold (31°C or equivalently 88°F) for August 17, 21, 22, 28, 29, and 31. Note that the treatment days were not necessarily consecutive because a treatment day had to be a weekday and its forecast maximum temperature had to be above the threshold. In this example, we grouped August 17, 21, and 22 as a cycle and August 28, 29, and 31 as another cycle. We then randomized the three critical peak prices in each cycle. As a result, customers in the economic incentive group had prices of 65, 105, 85, 85, 65, and 105 cents for these six treatment days.

We used this stratified randomization to minimize the correlation between the critical peak prices and temperatures. 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, while customers experienced hot temperatures for all summer treatment days and cold temperatures for all winter treatment days, there was some variation in the temperature across the treatment days. Using stratified randomization, we avoided the possibility of customers experiencing a certain critical peak price only on particularly hot or cold days. 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.

C. *Hypotheses*

We tested four primary hypotheses using data from the field experiment. First, we asked whether standard economic theory could explain household responses to moral suasion and economic incentives. A stylized demand model for electricity usage predicts that the economic incentive group lowers consumption in response to the marginal price of electricity according to a downward-sloping demand curve, and the moral suasion group uses electricity in the same way as the control group, implying that moral suasion should not affect electricity usage. However, a demand model with a warm glow, such as that of Andreoni (1989), predicts that moral suasion could alter usage levels, as described in our model in the online Appendix. To test these hypotheses, we compared consumption between the control group, moral suasion group, and economic incentive group.

The second hypothesis is the habituation of treatment effect, as described in Section II. We examined if the treatment effects of moral suasion and economic incentives stayed constant, decreased, or increased over repeated interventions. Standard demand theory predicts that usage should stay constant regardless of the number of interventions, given a certain marginal price of electricity. By contrast, the theory of habituation suggests that the treatment effects might habituate and thereby diminish over repeated interventions. In particular, Groves and Thompson (1970) and Rankin et al. (2009) present a theory and laboratory evidence that a less harmful treatment that does not involve an extrinsic reward is more likely to produce strong habituation. By contrast, a harmful treatment that involves an explicit reward is less likely to generate habituation. If households were to perceive moral suasion as a treatment that does not involve an explicit reward and is relatively less harmful than the high electricity price, the moral suasion treatment might generate stronger habituation. Using the variation generated by random group assignment, we compared habituation between the moral suasion and economic incentive groups.

The third hypothesis is dishabituation—whether habituated responses can be restored back to an original level. After the summer experiment, we purposely gave households about a three-month interval before we restarted the intervention in the winter. Laboratory experiments in psychology, such as those of Rankin and Carew (1988) and Phelps (2011), find that providing sufficient time intervals between interventions is one of the effective ways to obtain dishabituation for certain species. We know little about whether this method can produce a similar reset effect for economic activities, which we test by comparing the final intervention in the summer and the first intervention in the winter.

Fourth, we tested for habit formation using data from the post-intervention period. We withdrew our treatments after the final intervention but continued to collect high-frequency electricity consumption data. Standard economic theory predicts that electricity usage for the treatment groups should not differ from that for the control group once we withdrew our interventions. However, the theory of habit formation developed by Becker and Murphy (1988) suggests that short-run interventions of economic incentives and moral suasion could form a new consumption habit, which could affect future consumption. Empirically, the literature contains mixed evidence on what drives habit formation. We provide empirical evidence on habit formation both for moral suasion and economic incentives in our field experiment by comparing electricity usage between our groups in the post-experimental period.

In the next section, we test these four primary hypotheses. We then investigate the mechanism behind our findings by follow-up surveys on investment in physical capital stock and utilization habits for electricity usage.

III. Empirical Analysis and Results

We present our experimental results in this section. Recall that the treatment groups experienced many treatment days in the summer and winter. We included all treatment days in our regression, to show the overall treatment effects. We then explored habituation, dishabituation, spillover effects on nontreatment hours, and habit formation in the subsequent subsections.

A. *Effects of Moral Suasion and Economic Incentives*

We begin by showing evidence from the raw data in Figure 2, which 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 one 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 there are small remaining differences for a few hours following treatment. 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.

Table 2 provides a formal econometric analysis with standard errors. We estimated the treatment effects by ordinary least squares (OLS) for a linear equation:

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

where $\ln x_{it}$ is the natural log of electricity usage for household i in a 30-minute interval t. We used the natural log of usage for the dependent variable so that we could interpret the treatment effects approximately in percentage terms. The treatment effects in exact percentage terms can be obtained by $\exp(\beta) - 1$ and $\exp(\gamma) - 1$.[9] In this paper, we report both the log points and exact percentage terms. M_{it} equals one if household i is in group M (the moral suasion group) and receives a treatment in t. Similarly, E_{it} equals one if household i is in group E (the economic incentive group) and receives a treatment in t. We included 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 clustered the standard errors at the household level to adjust for serial correlation. We included 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. Therefore, we excluded nontreatment days in this regression. Recall that the treatment groups have 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 included only these hours to estimate the treatment effects on the treatment hours. We examined potential spillover effects for nontreatment hours in the following subsection.

Columns 1 and 3 of Table 2 show that moral suasion caused a reduction in peak-hour electricity usage by 0.031 log points (3.1 percent) for the summer treatment days and by 0.032 log points (3.2 percent) for the winter treatment days. A reduction in peak-hour consumption by 3 percent is economically significant because the marginal cost of electricity is extremely high during critical peak hours.

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

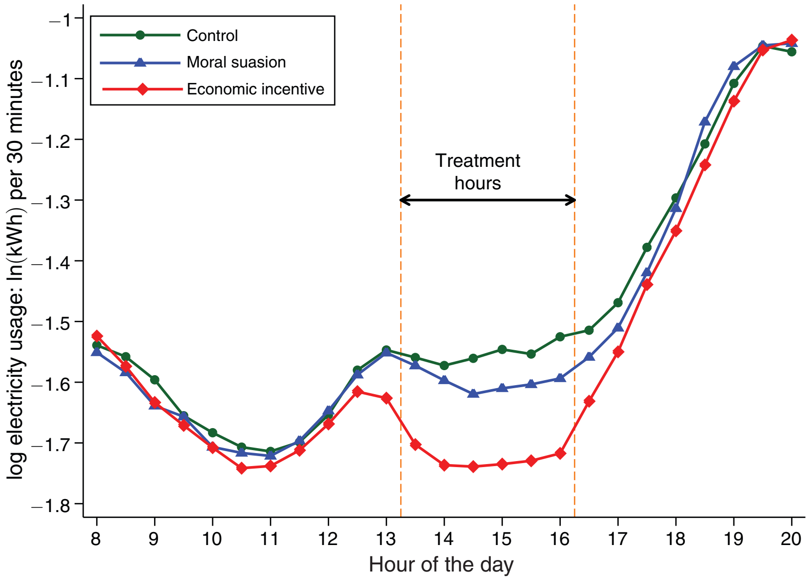

Figure 2. Effects of Moral Suasion and Economic Incentives on Electricity Usage

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.

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)		−0.168		−0.164
		(0.023)		(0.023)
Economic incentive (price = 105)		−0.182		−0.189
		(0.024)		(0.024)
Observations	123,106	123,106	244,891	244,891

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.

This finding suggests that the moral suasion policy could have provided a meaningful impact on electricity usage when we considered the average effect across all treatment days.[10]

[10] We included preexperimental data to obtain household fixed effects, which improved the efficiency of our estimates. An alternative was to calculate a simple mean difference in log usage between the treatment and control groups

Nevertheless, the level of the reductions is much larger for the economic incentive treatment. The average treatment effect is 0.167 log points (15.4 percent) for the summer and 0.173 log points (15.9 percent) for the winter. They are statistically different from the effect of moral suasion at the 1 percent significance level. An important question regarding the economic incentive effect is whether consumers merely reacted to "pricing events" and reduced their usage or responded to the changes in marginal price and consumed electricity according to their demand curves. The two possibilities have different policy implications because the latter indicates that policymakers can use price as a tool to achieve certain levels of reductions in peak-hour electricity usage. The answer to this question remains unclear in previous field experiments on electricity demand. This is primarily because most experiments used only one price for critical peak events and, therefore, could not disentangle the two possibilities. For example, Wolak (2006, 2011) has one treatment price in each of his two experiments. While Jessoe and Rapson (2014) have different treatment prices, the treatment hours and duration differed between the price regimes, which the authors acknowledge made it difficult to compare treatment effects across different prices. A few recent studies attempt to answer this question, but empirical evidence is limited. For instance, Carroll, Lyons, and Denny (2014) estimate the effect of different electricity tariffs on electricity usage for residential customers in Ireland, finding no statistical evidence that customers respond to changes in tariff.

We tested these hypotheses in columns 2 and 4 of Table 2 by estimating a treatment effect for each marginal price. Recall that the baseline marginal price was 25 cents/kWh, and the economic incentive group experienced the three critical peak prices—65, 85, and 105 cents/kWh. The result is consistent with the prediction of the standard demand theory. Consumers reduced usage more in response to higher marginal prices. In the summer, the critical peak prices of 65, 85, and 105 cents/kWh produced reductions in usage by 0.151 log points (14 percent), 0.168 log points (15.4 percent), and 0.182 log points (16.6 percent). The winter results show a similar relationship between the prices and responses. For both seasons, we rejected the equality of the coefficients between 65 cents and 105 cents at the 5 percent statistical significance level—the p-value is 0.043 for the summer and 0.048 for the winter. Although we could not reject the equality of the coefficients between the middle price (85 cents) and other prices at the 5 percent statistical significance level, the point estimates indicate a monotonic relationship between price and response. This finding implies that households indeed responded to marginal prices when they were well informed about their time-varying marginal prices.

This result has an important implication for energy policy because regulators and utility companies often believe that electricity consumers do not respond to electricity prices at all, and therefore, a price-based policy is not a practical solution to mitigate problems on the retail side of electricity markets. In addition, our finding provides new evidence to the growing literature showing that consumers might not necessarily respond to marginal incentives (Borenstein 2009, Kahn and Wolak 2013, Ito 2014, Copeland and Garratt 2015, McRae and Meeks 2016). Our result is in contrast to

during the treatment hours. With this approach, the moral suasion effect is −0.035 (0.016) for summer and −0.034 (0.023) for winter. The economic incentive effect is −0.170 (0.025) for summer and −0.177 (0.027) for winter.

that of Ito (2014), who finds that electricity consumers in California do not respond to the marginal price of their nonlinear price schedules but rather respond to the *average* price of their electricity bills. There are two main reasons why we found different evidence in our study. First, customers in our experiment had 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. Although monthly electricity bills provide information on marginal price, such information is not usually transparent, and consumers receive it with a month lag. The different findings are consistent with the literature that emphasizes the importance of price salience (Chetty, Looney, and Kroft 2009; Finkelstein 2009; Jessoe and Rapson 2014). The second key difference is that our customers had a single marginal price within every hour, which varies only across hours, whereas the marginal price in Ito (2014) varies with each customer's cumulative monthly usage. Therefore, the different findings between the two studies could reflect the possibility that consumers are more likely to respond to time-varying marginal prices than marginal prices that vary with their cumulative usage during a month.

B. *Habituation and Dis-habituation*

To investigate habituation of treatment effects, we repeated our interventions over 15 treatment days in the summer and 21 treatment days in the winter. Recall that we determined the treatment days by day-ahead weather forecasts, and therefore, they were not necessarily consecutive. As described in Section III, we divided the 15 summer treatment days into five cycles, and the 21 winter treatment days into seven cycles so that each cycle has 3 treatment days with 65 cents, 85 cents, and 105 cents as the peak-hour prices. We examined the habituation of treatment effects by estimating OLS with treatment cycles $c = 1, \ldots, 5$ for the summer and $c = 1, \ldots, 7$ for the winter:

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

where β_c and γ_c are the effects of moral suasion and economic incentives for treatment cycle c. Our objective was to test how β_c changed over the repeated interventions.

Table 3 shows the results. Column 1 indicates that the moral suasion effect was statistically significant in the first cycle, but became insignificant in the remaining cycles for the summer. Although it reduced usage by 0.083 log points (8 percent) in the first cycle, this effect diminished quickly in the remaining interventions—the point estimate declined to 0.033 log points (3.2 percent) in the second cycle and nearly zero in the further interventions. The p-values reported in the bottom of the table show that the treatment effect in the first cycle is statistically different from those in the remaining cycles at the 5 or 10 percent statistical significance level.

In column 3 of Table 3, we tested if the habituated moral suasion effect could be restored back to the original level by giving a sufficient time interval between interventions. In the first cycle of the winter, moral suasion induced a usage reduction by 0.083 log points (8 percent). This impact is identical to the effect found for the first cycle in the summer. It implies that while the moral suasion effect was

Table 3—Repeated Interventions: Habituation and Dis-habituation of Treatment Effects

	Summer		Winter	
	Moral suasion	Economic incentive	Moral suasion	Economic incentive
	(β_c)	(γ_c)	(β_c)	(γ_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)
p-values of the differences in the treatment effects relative to the effects in the 1st cycle				
2nd cycle	0.075	0.474	0.124	0.522
3rd cycle	0.024	0.678	0.026	0.394
4th cycle	0.054	0.120	0.194	0.428
5th cycle	0.030	0.050	0.041	0.409
6th cycle			0.080	0.626
7th cycle			0.069	0.608

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 three treatment days. There were 15 treatment days in the summer and 21 treatment days in the winter. The treatment days were not necessarily consecutive.

habituated over repeated interventions in the summer, the impact was reset to the original level—dishabituation—when we restarted the treatment in the winter followed by a three-month interval after the summer.

This finding is consistent with the theory and laboratory evidence of dishabituation and spontaneous recovery in the psychology literature (Thompson and Spencer 1966, Rankin et al. 2009). Furthermore, the process of habituation found in our experiment resembles laboratory evidence for many types of species in previous psychology studies. Columns 1 and 3 of Table 3 suggest that the habituation process—how the treatment effect decayed—was similar between the summer and winter. The habituation emerged approximately as an exponential decay of response in both seasons. This exponential decay is consistent with the pattern of habituation found for a variety of species (Groves and Thompson 1970, Rankin et al. 2009).[11]

[11] An alternative explanation for the decayed moral suasion effect was that households responded less as a result of a decrease in the strength or severity of the appeals. This is unlikely in our case because the moral suasion message was identical across the repeated interventions and the severity of the situation—limited electricity supply—was fairly consistent during the summer and during the winter.

Columns 2 and 4 of Table 3 tested habituation for economic incentives. Compared to moral suasion, our economic incentive treatment produced much more persistent effects and exhibited smaller habituation. In the summer experiment, the treatment effect was the largest in the second cycle (0.198 log points; 17.9 percent) and remained fairly stable until the fourth cycle. The effect in the fifth cycle was economically and statistically significant—a usage reduction of 0.127 log points (12 percent)—but the *p*-value for the difference in the treatment effects between the first and fifth cycles was 0.05. There are two potential reasons for this result. One possibility is that the response to the economic incentive indeed habituated when it came to the fifth cycle in the summer. Another potential reason is that all days in the fifth summer cycle happened to be in September. Households in Japan are accustomed to use air conditioning between June and August, but much less in September. This tendency could partially explain why we find a smaller response in the fifth cycle: there was likely to be less discretionary consumption when households did not use much air conditioning. The winter results show similar patterns with even more stable effects of economic incentives across repeated interventions. The effect is the largest in the second cycle (0.205 log points; 18.5 percent) and is stable from the first to seventh cycles. We cannot reject the null hypothesis that the economic incentive effects are equivalent in the first and seventh cycles in the winter.

To compare the treatment effects across cycles visually, we plotted the estimation results in Figure 3. The figure shows reductions in usage by treatment cycles, and the interval bar shows one standard error for each treatment effect. Our findings on habituation and dishabituation have four key policy implications, particularly for policymakers aiming to generate persistent policy impacts over repeated interventions, as follows. Both moral suasion and economic incentives are likely to produce sizable policy impacts in the short run. However, the effect of moral suasion is likely to habituate fast when the intervention is repeated over time. The habituated response to moral suasion can recover back to an original level by providing a sufficient time interval between interventions. Finally, the effect of economic incentives is much less likely to habituate than moral suasion is.

It is useful to compare the policy impacts of our treatments to those found in previous studies. One way to compare the magnitude of the economic incentive effect is to calculate the implied price elasticity. The implied price elasticity for each cycle is between -0.104 and -0.162 for the summer and between -0.137 and -0.167 for the winter. Combining all cycles, the average price elasticities and standard errors are -0.136 (0.017) for the summer and -0.141 (0.018) for the winter. These estimates are close to the price elasticity estimates for residential electricity customers in the literature. For example, Wolak (2011) and Ito (2014) find that the price elasticity for residential customers in Washington DC and California are around -0.1. Because our study and these two studies examined different samples that had different price schedules, it is notable to observe that the estimated price elasticities for residential customers are similar among these studies.

Another useful exercise is to compare our moral suasion effect—about 8 percent reductions in peak-hour electricity usage for the first cycle and 3 percent average reductions for the entire cycles—to the effects of other nonmonetary incentives on energy conservation. Reiss and White (2008) find that public appeals during

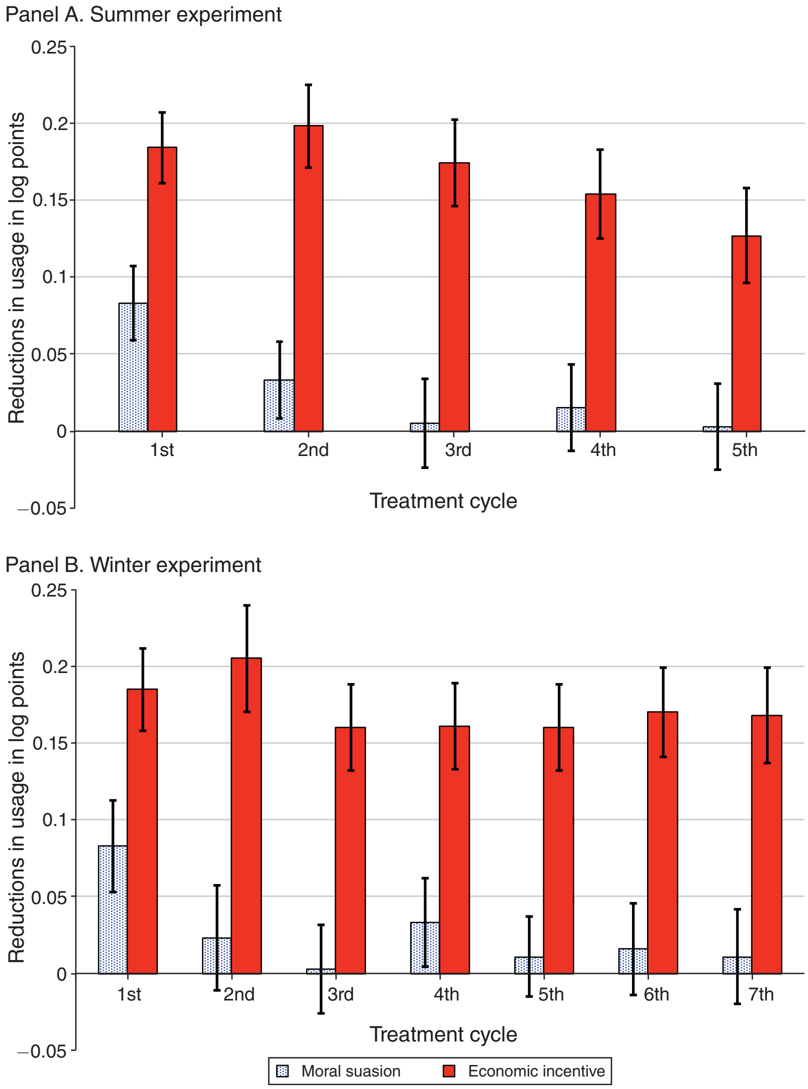

Figure 3. Treatment Effects by Treatment Cycles

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 three treatment days. There were 15 treatment days in the summer and 21 treatment days in the winter.

the California electricity crisis provided about a 7 percent reduction in electricity usage for residential consumers in San Diego.[12] Allcott and Rogers (2014) find that

[12] During the California electricity crisis in 2000–2001, there was an economic incentive (a spike in electricity prices) right before the conservation campaign period. Therefore, there is a possibility that part of the 7 percent effect might include a potential persistent effect of the economic incentive.

sending a report showing peer comparison of energy usage induced about a 1 percent reduction in electricity usage for residential consumers in the United States. Finally, Schwartz et al. (2013), after sending consumers postcards that stated their electricity usage was being observed, find that the treated customers had a 2.7 percent reduction in electricity usage relative to customers who received nothing.[13]

When interpreting the size of the treatment effect, we want to emphasize again that our households were not a random sample of the population. While observable characteristics are not statistically different between the experimental sample and a random sample of the population in Table 1, there can be unobservable factors that are different between the two groups. For example, our sample households could be interested in smart meter technologies and potentially more willing to respond to our treatments. If that is the case, the treatment effect for our sample can be larger than that for the population. We showed that our estimated price elasticity is similar to that found in previous studies in residential electricity demand and that our moral suasion effect is close to the effect of similar interventions conducted in the past. This comparison provided suggestive evidence that our experimental sample is unlikely to be substantially different from the population in terms of responses to treatment. However, this is merely suggestive evidence, and we emphasize that our experiment cannot be entirely free from external validity issues.

C. *Spillover Effects for Nontreatment Hours on Treatment Days*

We specifically targeted the treatments in our experiments at 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 might change their usage immediately before or after the treatment hours. For example, those who had a high critical peak price for the treatment hours could increase their air conditioner usage immediately before the treatment hours. Such precooling or preheating could be rational given the high critical peak prices for the treatment hours. Similarly, households might 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 shoulder hours. Therefore, if customers were to increase their usage in the shoulder hours, this could attenuate the economic benefits of interventions focused on peak-demand hours.[14] This is also an important question for environmental policy because increases in usage in nontreatment hours could increase the total emissions from electricity generation (Holland and Mansur 2008).

[13] In our experiment, both the control and treatment groups received smart meters and in-home displays. Therefore, the effect of being observed was likely to be captured by the control group. However, if there was a Hawthorne effect of getting a message about moral suasion or price changes, such an effect was not fully captured by the control group and included in our treatment effects.

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

Table 4—Spillover Effects for Nontreatment Hours on Treatment Days

	Summer			Winter		
	Treatment hours (1 pm–4 pm) (1)	Shoulder hours (10 am–1 pm, 4 pm–7 pm) (2)	Other hours (3)	Treatment hours (6 pm–9 pm) (4)	Shoulder hours (3 pm–6 pm, 9 pm–12 pm) (5)	Other hours (6)
Moral suasion	−0.031	−0.010	−0.008	−0.032	−0.010	−0.008
	(0.014)	(0.010)	(0.005)	(0.020)	(0.015)	(0.012)
Economic incentive	−0.167	−0.059	−0.021	−0.173	−0.036	−0.008
	(0.021)	(0.015)	(0.010)	(0.022)	(0.017)	(0.015)
Observations	123,106	248,621	634,387	244,891	482,902	1,182,574

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 three 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.

The second possibility is that consumers might 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 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 there is a fixed cost for consumers to change their lifestyles in terms of electricity usage (Wolak 2011). When consumers face a substantial increase in peak-hour electricity prices, they might change their lifestyles as a whole to be more energy efficient. In such cases, it is possible that customers lower their electricity usage in all hours when they face interventions that are targeted primarily at peak-demand hours.

Table 4 provides the results of empirical tests for these possibilities. We estimated equation (1) by including data for different hours on the treatment days for each column. Column 1 shows the results for the treatment hours, which are equivalent to the results in Table 2. Column 2 shows the results for the shoulder hours, that is, three hours before and after the treatment hours. Finally, column 3 includes data for other nontreatment hours on the treatment days. For both the moral suasion group and the economic incentive group, we find no increase in consumption either in the shoulder hours or in other off-peak hours. Instead, we found usage reductions for the economic incentive group during the nontreatment hours. For example, we found usage reductions of 0.059 log points (5.7 percent) for the shoulder hours and by 0.021 log points (2.1 percent) for the off-peak hours in the summer experiment. The findings for the winter experiment are consistent with those for the summer experiment. By contrast, we found no such spillover effects for the moral suasion group. This group's usage in the shoulder hours and off-peak hours is statistically indistinguishable from the control group's usage. These results imply that the economic incentives in our experiment motivated customers to lower their usage in both the nontreatment hours and the treatment hours.

D. *Habit Formation*

In the previous subsections, we found that moral suasion and economic incentives produced substantially different results in terms of habituation, dishabituation, and spillover effects during nontreatment hours. In this subsection, we tested for habit formation (Becker and Murphy 1988)—customers faced with our interventions might have formed a habit for efficient use of energy. To test for habit formation, we collected data for the periods *after* the treatment was withdrawn. During this post-intervention period, households did not receive any treatment. We expected that unless our treatment induced habit formation, there should be the same levels of consumption between the control group and each treatment group during this period.

Table 5 shows the results. We examined usage data during the three-month post-experimental period for both the summer and winter experiments, wherein customers did not receive any treatment. We tested whether usage in peak-demand hours during this period differed between the control and each treatment group. The table shows that the moral suasion group's usage is not statistically different from that of the control group. By contrast, the economic incentive group's consumption is statistically different from that of the control group as well as the moral suasion group. This result implies that consumers who received our economic incentives continued to have a lower consumption of 0.084 log points (8.1 percent) after the summer experiment and 0.089 log points (8.5 percent) after the winter experiment.

This finding has an important policy implication because the existence of habit formation could offer additional policy impacts for post-intervention periods. In the literature, habit formation has been studied largely for "bad" habits. Our finding is among recent studies that investigate the formation of "good" habits. For example, Charness and Gneezy (2009) find that providing monetary incentive for exercising induced a habit formation of exercise for college students on campus. In addition, a few studies explore potential habit formation for the treatment effect of peer comparison in the context of water and energy conservation. For instance, Ferraro, Miranda, and Price (2011) and Allcott and Rogers (2014) find that providing information about neighbors' water usage and electricity usage, respectively, induced conservation effects that lasted even after the intervention was withdrawn. Our finding is consistent with the empirical evidence in Ito (2015), which finds that residential electricity customers in California exhibited similar habit formation of electricity consumption in response to a short-run economic incentive of a subsidy program. Why do we observe habit-formation effects only for the economic incentive group and not for the moral suasion group? We explore the mechanism behind these findings in the final subsection of our empirical analyses. Before we proceed to analyzing the mechanism, in the next subsection, we investigate potential heterogeneity in the treatment effects.

E. *Mechanisms behind the Treatment Effects*

We found significant differences in the effects of moral suasion and economic incentives in their overall impacts, habituation, spillovers to nontreatment hours,

Table 5—Habit Formation after the Treatments Were Withdrawn

	After summer experiment (1)	After winter experiment (2)
Moral suasion	0.017	0.008
	(0.029)	(0.029)
Economic incentive	−0.084	−0.089
	(0.025)	(0.034)
Observations	358,415	333,581

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 (1 pm to 4 pm) after the summer experiment. Column 2 shows the result for usage in peak demand hours (6 pm 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.

and habit formation. Moral suasion was effective only for the first few treatment days and habituated quickly over repeated interventions. By contrast, economic incentives produced strong persistent effects on energy conservation. To investigate the mechanisms behind these findings, we conducted a detailed follow-up survey. We examined two potential mechanisms.

The first possibility is that the treatments might have induced investment in physical capital stock—households might have purchased energy-efficient appliances in response to the treatments. If this effect were systematically large for the economic incentive group, it could explain why we observed less habituation and stronger habit formation for this group. The second possibility is that the treatments might have induced new utilization habits for daily electricity use. Suppose that customers had "bad habits" of inefficient or wasteful energy use. It is possible that experiencing high electricity prices triggered a change in their utilization habits, encouraging them to use electricity more efficiently.

Table 6 tests the first possibility—investment in physical capital stock. We asked customers if they purchased energy-efficient appliances since the start of the experiment. We estimated a linear probability model, which includes a binary choice dependent variable, dummy variables for the treatment groups as independent variables, and a constant term.[15] The constant term gives the ratio of customers in the control group who each purchased an energy-efficient appliance. The coefficients for the treatment dummy variables represent percentage-point increases for the treatment groups.

The table suggests that moral suasion increased the purchase of energy-efficient 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 statistically significant increases in purchasing energy-efficient air conditioners compared to the control group. We found no statistically significant effects for other products for both treatment groups. The results suggest that investment in

[15] In addition, for robustness checks, we run probit and logit models and found the same results.

Table 6—Treatment Effects on Investments in Physical Capital Stock

	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
	(0.02)	(0.02)	(0.02)	(0.04)	(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 nonresponses is balanced across the three groups.

physical capital stock is unlikely to explain why we found significant differences in the persistent effects between moral suasion and economic incentives.[16]

Table 7 explores the second potential mechanism—behavioral changes in utilization habits. After the experimental period, we asked customers two questions related to this point. 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 regressed 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 found a slightly 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 estimated a linear probability model, in which the dependent variable is a binary choice, and the independent variables include dummy variables for each treatment group. In addition, the model contains a constant term. For each appliance, we found that the economic incentive had a statistically significant effect of 8 to 15 percentage points. By contrast, moral suasion had no statistically significant effects on the energy-efficient use of each appliance.

[16] An important caveat of this analysis is that customers knew that they were going to receive the treatments only during the experimental period. If customers had experienced their treatments for a longer time, more consumers might have found investment in physical capital stock economical.

Table 7—Treatment Effects on Utilization Habits

	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)	(0.04)	(0.03)	(0.03)
Economic incentive	0.40	0.13	0.15	0.09	0.08	0.12
	(0.07)	(0.05)	(0.05)	(0.04)	(0.03)	(0.03)
Constant	3.03	0.61	0.53	0.11	0.08	0.07
	(0.06)	(0.04)	(0.04)	(0.03)	(0.02)	(0.02)
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 one (lowest) to five (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 nonresponses is balanced across the three groups.

Although stated survey responses inform these results, they provide suggestive evidence about the mechanisms behind our findings. Investment in physical capital stock is unlikely to explain the significant differences in the persistent effects between the two treatments. Instead, experiencing high electricity prices is likely to trigger a change in utilization habit for electricity use, encouraging customers to use electricity more efficiently.

IV. Conclusion

In this study, we used a randomized controlled trial in residential electricity demand to investigate whether moral suasion and economic incentives generated persistent impacts on economic activities. Using high-frequency electricity-usage data at the household level, we found that moral suasion induced a short-run reduction in peak-hour electricity usage by 8 percent. This short-run effect is economically significant for improving economic efficiency in electricity markets. However, the response to moral suasion habituated quickly—the treatment effect diminished toward zero when we repeated the intervention over time. In addition, we found evidence of dishabituation—the habituated response could be restored back to its original level when we provided a sufficiently long time between interventions. However, the recovered response once again habituated when we repeated the intervention frequently.

We compared the impact of moral suasion to that of a more conventional policy tool—economic incentives for consumers to reduce energy usage during peak-demand hours. The economic incentive treatment produced usage reductions of 14 percent for the lowest treatment price and usage reductions of 17 percent for the highest treatment price. Moreover, compared to the impact of moral suasion, the effect was more persistent over repeated interventions, suggesting little habituation. In addition, the economic incentive induced habit formation—households continued to conserve electricity even after the treatment was withdrawn. Our follow-up survey data indicated that most of the persistent changes were likely to originate from changes in utilization habit of electricity usage rather than investment in physical capital stock.

As described in the experimental design section, an important limitation of our experiment is that subjects were not a random sample of the population. We showed that observable characteristics are statistically indistinguishable between our experimental sample and a random sample of the population. We also presented that our estimated price elasticity is similar to that found in previous experiments and that our moral suasion effect is close to the effect of similar interventions conducted in the past, which provided suggestive evidence that our experimental sample is unlikely to be substantially different from the population in terms of responses to treatment. However, this is merely suggestive evidence, and we emphasize that our experiment cannot be entirely free from external validity issues.

Our results suggest that economic policy can induce significantly different welfare effects depending on whether policymakers use moral suasion or economic incentives to promote pro-social behavior. To illustrate this implication, we conduct a welfare analysis in the online Appendix. One of the largest inefficiencies in electricity markets is that retail electricity prices usually do not reflect the marginal cost of electricity—consumers pay time-invariant prices while the marginal cost of electricity is time variant (Wolak 2011, Joskow 2012, Joskow and Wolfram 2012, Jessoe and Rapson 2014). Our analysis shows that moral suasion can provide significant welfare gains in the short run, but such gains are likely to diminish rapidly when the intervention is repeated over time. By contrast, economic incentives produce greater welfare improvement, particularly when long-run effects, such as habituation and habit formation, are taken into account.[17]

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[17] A key assumption in our welfare analysis is that consumers gain positive utility (e.g., warm glow) from their responses to nonpecuniary interventions. This assumption can be violated if such interventions were to create negative utility (e.g., social pressure). This challenge suggests that an important future research question is how to disentangle such possibilities to provide precise analysis for welfare implications of nonpecuniary interventions. See DellaVigna, List, and Malmendier (2012) and Allcott and Kessler (2015) for more discussions on this topic.

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