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Learn to conserve:
The effects of in-school energy education on at-home electricity consumption

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Abstract

Environmental education for school students, including lessons on recycling, water conservation, and energy reduction, is a popular measure aimed at increasing environmental knowledge, promoting environmental attitudes, and increasing pro-environmental behaviors. Despite the prevalence of such education, there is little empirical evidence to support the efficacy of these programs on tangible outcomes outside of school. This paper contributes an empirical analysis of a series of energy lessons in the United States. Using a differences-in-differences approach with a rich panel data set, we find evidence for short-term reductions on the order of eight percent in electricity use the day of a lesson regarding energy conservation via reducing phantom electric loads (standby power), with evidence of deferment in electricity use rather than true conservation. We find no effect of lessons on energy pathways or wind energy on the days of the lessons. Despite limited evidence of conservation, our results do indicate a connection between lessons learned in school and measurable behavior at home. Importantly, this research indicates that energy education could be a potentially valuable tool for policy regarding energy conservation and efficiency, though future research is needed to optimize the timing and content of such lessons.

Keywords: Energy Education; Electricity; Energy Conservation; Intergenerational Learning

1. Introduction

Among the suite of strategies to promote sustainable environmental behaviors, environmental education for school children is a popular non-price measure to nudge pro-environmental behavior. Environmental education for school students, including lessons on recycling, water conservation, and energy reduction, aims to increase environmental knowledge, promote environmental attitudes, and increase pro-environmental behaviors. Despite the prevalence of such education, there is little empirical evidence to support the efficacy of these programs on tangible outcomes outside of school.

Understanding the impacts of energy conservation programs, including education, is urgent considering the current political landscape. While a growing number of states are adopting environmental goals and passing legislation requiring energy conservation, there is a troubling trend of limiting funding. For example, in 2017, Connecticut legislators voted to divert over $175 million in ratepayer energy efficiency charges to the state's general fund (June SS PA 17-2). With decreasing program budgets and mandates to invest in cost-effective energy conservation, utilities must understand how programs impact electricity use in order to re-optimize their portfolios of offerings and meet state energy and environmental goals.

The link between in-school education and knowledge, attitude, and behavior at the household level is indirect. Education programs target students, including elementary-aged children who have little explicit agency in household management decisions. However, evaluations of environmental education programs show that, while modest, there is potential for intergenerational influence between child and family (see Duvall and Zint (2007) for a review). For example, children treated with environmental education in school encourage their families to engage in pro-environmental behaviors including energy-saving behaviors (Hiramatsu et al., 2014), recycling (Evans et al., 1996), and decreasing household waste (Grodzinska-Jurczak et al., 2003). While the literature on energy behavior, and conservation specifically, is extensive (see Abrahamse et al. (2005) for a review), there have been few evaluations of in-school energy education as a non-price mechanism to nudge residential energy efficiency.

This research seeks to understand the effects of an in-school energy education program on household electricity consumption. We conduct a case study examining the effects of such lessons using household daily electricity load panel data for households of all students who received a lesson within one school, as well as for a set of randomly chosen control households from which we construct an appropriate counterfactual. We employ a differences-in-differences framework to estimate changes in electricity use on the days of energy lessons. Our results are consistent with intuition about how curriculum content might affect energy use at home. We find reductions in electricity consumption on the order of eight percent on the day of an energy lesson about phantom loads – that is, the energy that is used by appliances and electronics that are plugged in but not in use. However, we find an increase in load two days following the lesson of the same magnitude, suggesting deferment of electricity use rather than true conservation. We fail to find effects of lessons on energy pathways and wind energy. Though we cannot say with certainty why we only see an effect of the lesson on phantom electric loads and not energy pathways or wind energy, our intuition points to lesson content. The lesson on phantom loads has direct action items for reducing electricity use at home, while the other lessons offer a more indirect link from content to conservation. We additionally explore heterogeneity in treatment effect for the lesson on phantom loads along dimensions of baseline consumption and house characteristics using assessor data. While small sample size limits our statistical precision, our results suggest that higher-consuming households may reduce more on the day of the lesson, and that reductions may be smaller for newer houses with higher assessed values and enhanced for larger houses.

Through this analysis, we make two primary contributions to the literature. First, we provide critical empirical evidence of how energy education for school students affects electricity use at home, adding a much-needed data point in the sparse literature on energy education. Most studies of energy education rely on self-reports, with mixed findings on energy-related behavior change (DiMatteo et al. 2014, Zografakis et al. 2008). Of the two empirical analyses of energy education on consumption, one fails to find any effect while the other finds small reductions in aggregate geographic locations near schools (Osbaldiston and Schmitz 2011, Agarwal et al 2017). However, neither program is directly comparable to the energy lessons in this analysis because of additional confounding components, like challenges, and timing and duration of the education treatment.

Second, our findings add to the body of literature on intergenerational learning by showing that in-school energy lessons have an affect on energy-related behaviors at home, especially when lesson content includes direct action items for energy reduction. Our findings have important implications for environmental education policy, suggesting that energy education may be an effective way to encourage energy conservation. However, we find that this effect is temporary and results in deferment rather than reduction. Despite limited evidence of conservation, our results do indicate a connection between lessons learned in school and measurable behavior at home. Importantly, this research indicates that energy education could be a potentially valuable tool for policy regarding energy conservation and efficiency, though future research is needed to understand how to optimize timing of energy lessons and curriculum content to achieve deeper and persistent energy reductions at home.

2. Literature Review

According to the most recent Residential Energy Consumption Survey, US households consumed over 10 quadrillion Btu of energy in 2009, resulting in over 1,100 million metric tons of carbon dioxide emissions (EIA 2017). Three-quarters of this energy was used for space heating, appliances, electronics, and lighting (EIA 2013). However, with concerns over climate change mitigation, pollution from electricity generation, and consumer welfare, both policy makers and environmentalists have been working to encourage energy efficiency and conservation.

Non-price strategies include mandatory energy efficient standards for buildings and appliances (Costa and Kahn 2011, Jacobsen and Kotchen 2013), demand response programs including direct load control (Summit Blue 2004, KEMA 2006), commitment devices and goal setting (Becker 1978, Harding and Hsiaw 2014), social comparisons (Allcott 2011), feedback (Jessoe and Rapson 2014, Carrico and Riemer 2011), and education (Agarwal et al 2017). For example, direct load control programs allow the utility provider to automatically curtail sources of electricity use, like air conditioners, in response to total demand during critical peak periods. One reason why direct load control programs are effective is because they obviate the need for behavior change by the consumer. While programs such as this have been shown to be effective, consumer behavior during off peak hours can attenuate predicted energy conservation benefits (Newsham and Bowker 2010, Wolak 2011, Lang and Okwelum 2015). There has been a growing movement for using concepts from psychology and behavioral economics to encourage energy efficiency (Allcott 2016, Gillingham and Palmer 2014, Allcott and Mullainathan 2010). One well-known example is the peer comparison on the OPower home energy reports. Allcott (2011) finds this comparison leads to a persistent two percent reduction in electricity use.

Energy feedback and in-home energy displays can be considered as one type of education geared toward educating energy-related decision-makers about energy use and cost in real-time. These methods have been shown to be effective, especially when coupled with price incentives like critical peak pricing (Jessoe and Rapson 2014, Newsham and Bowker 2010). However, these measures rely on some base knowledge of how to reduce energy use in the home, and motivation to do so. Towards the first point, suggestions for electricity reduction and energy efficiency improvements popular on energy bills (notably also on OPower home energy reports) provide some additional education about how to reduce energy use at home. In-school energy education for students can also provide critical information about actions to reduce energy consumption, as well as some key motivation in the form of persistent nudging from eager children.

Utilities expend significant effort and resources on energy education, funded in part by consumers and mandated by state and federal legislation. The goal of energy education programs is to reduce energy consumption in order to even out daily and seasonal energy demand, save consumers money on utility bills, and curb greenhouse gases emissions. These programs promote behaviors that reduce energy consumption in the short-term (i.e., unplugging appliances when not in use) and in the long-term by increasing propensity to acquire energy efficient capital stock (i.e., purchasing Energy Star certified appliances) (Allcott and Rogers, 2014). However, the tangible effect of energy education in schools on energy consumption at home is not well understood.

While the literature on energy behavior and conservation is extensive (see Abrahamse et al. (2005) for a review), there have been few evaluations of in-school energy education. Although the NEED Project (www.need.org) conducts knowledge assessments of their energy education curricula, they do not address the behavioral impacts of energy education. Two studies that rely on before and after surveys find contradicting effects of energy education in elementary and junior high school. DiMatteo et al. (2014) finds increases in energy knowledge but not self-reported changes in behavior, while Zografakis et al. (2008) do find an increase in self-reported energy-saving behaviors and a decrease in 'energy-squandering' behaviors.

The majority of literature on environmental education in general, and energy education in particular, are problematic for several reasons. First, they rely on self-reported behavior change, which is likely to be overstated (Geller, 1981). Second, they used methods that fail to construct a counterfactual, compare treatment to control, or used a randomized framework, thereby potentially biasing the estimated treatment effect. Third, they do not quantify a treatment effect in terms of actual energy consumption, falling short of what is needed to properly evaluate the impacts that energy education will have on electricity use and its consequences. While some studies find evidence of increased self-reported pro-environmental behavior and intergenerational learning following energy education, there are no robust research studies that empirically estimate the effects of in-school education on observable, quantifiable outcomes at the household level. This research seeks to estimate the effects of in-school energy education on student household energy consumption.

A recent empirical study of an energy education program in Singapore provides some evidence for the effects of energy lessons on household electricity consumption. Argawal et al. (2017) use a quasi-experimental approach to estimate changes in total monthly electricity consumption aggregated for households within 2 km from a school that participated in an energy savings campaign. The campaign included frequent energy lessons and an energy savings challenge with a 10% home electricity reduction goal and prizes for households that reduce the most. As such, the campaign is not directly comparable to the energy education program in our research. However, the findings suggest decreases in electricity consumption for households near treated schools relative to households farther from treated schools on the order of 1-2%. The authors of the study claim this is evidence of both effective nudging and spillover effects to neighbors without school children. However, there are several limitations of this study, including potential selection bias, possible contamination between the treatment and control groups, and confoundedness of the energy education with competition and challenge aspects. [1]

A related vein of literature seeks to understand the impact of water conservation education. Of these, only one study empirically examines change in water usage at the household level. Geller et al (1983) find that educational pamphlets elicit no discernable effect on household water usage and posit that only one household member fully receives educational treatment by actively engaging with the pamphlet when it is delivered.

3. Background on the energy education program and lessons

We partnered with a public electric utility that serves a metropolitan area in New England. Funding for energy efficiency programs comes from a ratepayer surcharge, government initiatives and grants, totaling nearly $150M in 2013. Just over 40 percent of this funding was

[1] Another study to note is Osbaldiston and Schmitz (2011), who attempted to conduct an empirical analysis of two one-hour interventions in ninth grade science classes in a Midwestern US city. Researchers collected household electricity consumption data of participating students and gathered additional data through a pre- and post-survey administered to students and mailed to parents four weeks after the intervention. However, the authors find no significant difference in household electricity consumption across years before and after the energy program and estimates are not included in the published article.

spent on the residential sector. Among other services, this funding goes toward community and childhood education. One of these energy education programs has delivered curricula to K-12 students since 2001. In 2013, this energy education program delivered professional development regarding renewable energy and energy efficiency to 466 educators. Energy lessons cover energy basics, including energy systems, conventional energy generation, and renewable energy sources, and highlight energy conservation behaviors at home. Despite legislation, costs to consumers, and effort on the part of the electric utility, returns to in-school energy education programs are poorly understood.

In the 2015-2016 academic year, educators delivered energy lessons to over 500 students. We study the effects of three of these energy lessons delivered to all fourth and fifth graders at one school.[2] The first lesson taught students about ‘phantom’ electric loads – the electricity used by appliances even when they are turned off. Importantly, students learned that turning off and unplugging appliances and electronics can save energy. This lesson introduced students to basic concepts of electricity, including types of energy, types of fuel and electricity sources, and compared electricity use of common household appliances. Students participated in two exercises that are particularly relevant to reducing electricity at home. First, they recorded which appliances or equipment they used the previous day (e.g. iPad, TV, lights) and estimated how much energy they used in total. Second, students used a wattmeter to measure and record how much electricity various appliances use, both when the appliances are on and off.[3] Students then came up with recommendations for how they could reduce their electricity use at home. Because of the direct ties linkages between this lesson and electricity conservation, we focus on the effects of this lesson in our main analysis. The second lesson described energy pathways. During

[2] Only fourth and fifth grade students received energy lessons in the timeframe of this study.
[3] Activity instructions included in the Appendix.

this lesson, students built circuits to understand how electricity flows and manifests itself, including as light, heat, and sound. The third lesson discussed wind energy systems and included an activity to understand the impacts of different turbine blade shapes. The same educator taught all lessons, and all students were encouraged to discuss the lessons with their families at home.

4. Methods

We employ a differences-in-differences empirical framework to identify the effects of three energy lessons on household electricity consumption. Intuitively, we may expect a school student to talk about something novel or exciting that happened at school at home after school hours. For engaged families, it is not unreasonable to think that such a conversation would include takeaways from an energy lesson. Such communication would provide one mechanism for interfamily learning about energy conservation. Furthermore, some families may even act on specific conservation behaviors or experiment with energy use throughout the house. For example, a family may experiment with turning off all unnecessary lights or be motivated to read a book rather than watch television or use a computer. Perhaps these energy-saving behaviors occur the day of the lesson or possibly persist on days following the lesson. Therefore, we hypothesize that electricity use decreases on days of energy lessons. However, the energy lessons are only a small part of a student's busy schedule. And, as much of the literature shows, permanent behavior change is extremely difficult to achieve. In the absence of persistent reminders and feedback about energy conservation, there is limited incentive to continue to reduce energy use. Therefore, we are primarily interested in changes to household electricity use on the day of a lesson.

To test this hypothesis, we rely on a differences-in-differences model that compares the treatment group (households with a student who received an energy lesson) to the control group (households who do not contain a student who received an energy lesson) and examines whether there is differential energy use on the day of the lesson relative to the pre-treatment period.[4] By comparing electricity use of the treatment and control groups on days before and after the day the treatment group received the lesson, differences-in-differences allows us to estimate the causal effect of the lesson on electricity use. The basic empirical model is as follows:

$$load_{it} = \beta_1 LessonDay_t + \beta_2 TreatmentHH_i$$

$$+\beta_3 LessonDay_t \; x \; TreatmentHH_i + \varepsilon_{it} \qquad (1)$$

where the unit of observation is household-day, the dependent variable $load_{it}$ is electricity use in kwh for each household i on each day t, $LessonDay_t$ is a binary variable equal to one if an energy lesson occurred on day t, $TreatmentHH_i$ is a binary variable equal to one if household i contains a student who received an energy lesson. $LessonDay_t \; x \; TreatmentHH_i$ is the interaction of the two and equal to one for households with a student who received a lesson that day. The coefficient of interest is the interaction term, β_3, because this measures the change in load from pre- to post-treatment for the treated group, relative to the change in load for the control group. A negative, significant coefficient indicates a reduction in electricity consumption for households with a student who received a lesson. The error term ε_{it} is clustered at the household level to allow for correlations in electricity use within each household unit.

[4] Differences-in-differences is commonly used and is a powerful tool for causal inference (Angrist and Pischke 2009). Examples of differences-in-differences applications in electricity consumption include Allcott 2011, Jessoe and Rapson 2014, Fowlie et al. 2017.

The rich nature of our dataset allows us to additionally control for unobservable characteristics that may affect electricity consumption. We extend the model in Equation 1 to control for unobservable household-level average electricity use with a household-specific fixed effect, α_i. Household fixed effects flexibly control for time-invariant factors including family size, house characteristics, preferences for AC use or heating, household appliances, etc. For example, suppose a treated house is very large with poor insulation and energy-inefficient appliances, resulting in higher-than-average electricity use. The household fixed effect will account for that level of energy use, and prevent the model from falsely attributing high energy use to having a lesson.

We also control for average electricity use each day of the sample using a time fixed effect, θ_t. The time fixed effect accounts for determinants of electricity use on each day that affect all households, including weather and day of week fluctuations. For example, suppose one day in our sample was particularly hot and required more air conditioning than average. All households in our sample would have experienced the hot day, resulting in higher-than-average electricity use. The time fixed effect accounts for this anomaly that affects both treatment and control groups. Similarly, a time fixed effect controls for, say, households generally using less energy on Saturdays than Thursdays. The benefit of time fixed effects is being able to control for complex determinants like temperature and humidity without imposing an assumption about the functional form of these relationships. With these fixed effects, our model becomes: [5]

$$load_{it} = \beta_1 LessonDay_t \; x \; TreatmentHH_i + \alpha_i + \theta_t + \varepsilon_{it} \qquad (2)$$

[5] Note that the fixed effects take the place of the other terms from Equation 1. In other words, $LessonDay_{it}$ is accounted for through the day fixed effect and $TreatmentHH_{it}$ is accounted for through the household fixed effect.

We make two more modifications to the basic model. First, we include the summation of coefficients on the three days prior to each lesson, the third term in Equation 2, to test the assumption of parallel trends between the treatment and control groups within the differences-in-differences framework. Doing so bolsters our confidence that we have an appropriate counterfactual for the treatment group. Second, we hypothesize the effect of the lessons attenuates quickly, within a few days. We make the assumption that the effect will attenuate completely within one week following the lesson, and estimate changes in electricity use for each of the seven days following the lessons. Doing so provides insight into how the effect of energy lessons changes over time. Our full specification is:

$$
\begin{aligned}
load_{it} = {} & \beta_1 LessonDay_t \ x \ TreatmentHH_i \\
& + \sum_{\tau=1}^{7} \beta_2^{\tau} NextDay_t \ x \ TreatmentHH_i^{\tau} \\
& + \sum_{\tau=1}^{3} \beta_3^{\tau} PriorDay_t \ x \ TreatmentHH_i^{\tau} \\
& + \alpha_i + \theta_t + \varepsilon_{it} \qquad (3)
\end{aligned}
$$

Our coefficient of interest is again on $LessonDay_t \ x \ TreatmentHH_i$, a binary variable equal to one if a lesson occurred on day t for household i and zero otherwise. The second term in Equation 3 tests for continued changes in electricity consumption over the seven days following each energy lesson. $NextDay_t \ x \ TreatmentHH_i^{\tau}$ is a binary variable equal to one if household i received a lesson τ days before day t, and zero otherwise. The coefficient estimate for β_2^{τ} indicates a change in electricity load on τ days following the lesson. Coefficients that are significantly distinguishable from zero would indicate possible persistence of the treatment effect. $PriorDay_t \ x \ TreatmentHH_i^{\tau}$ is a binary variable equal to one if household i received a lesson τ days after day t, and zero otherwise. If our assumption of parallel trends holds, then the coefficient estimates for β_3^τ would be insignificant, indicating the electricity use prior to treatment is statistically indistinguishable between the treatment and control groups. These coefficients provide evidence for quality of the control group as an appropriate counterfactual for the treatment group.

Our full empirical model described by Equation 3 allows us to test the hypotheses that each lesson caused students' households to change their electricity use. If the coefficient of interest β_2^τ is statistically different from zero, then we can reject the null hypothesis that a lesson does not affect electricity use. In other words, a statistically significantly negative coefficient of interest would imply the lesson caused student households to save energy. We estimate Equation 3 for each lesson separately. We use data for the treatment group of students that received lessons (the same students for each lesson) and a carefully constructed control group of students who did not receive lessons (a different group of students for each lesson, outlined in the following subsection).

4.1 Exploration of Heterogeneity in Treatment Effect

Our main analysis focuses solely on the effects of the lesson on phantom power due to the direct linkages with electricity conservation. In a classic scenario using differences-in-differences, there is measurement of the dependent variable prior to some treatment and then after the treatment. In our empirical setting, we have not just one, but three treatment dates. Running the model given by Equation 3 would result in an estimated treatment effect that is averaged over the three lessons. However, two potential issues arise with doing so. First, the estimated treatment effect will be smaller in magnitude (and potentially lower in statistical significance) if one or two of the lessons do not affect electricity consumption at home. Second, since the lessons are spread throughout the academic year, if the treatment group responds differently to changes in season, then the same control group may not be an appropriate counterfactual. For example, we can imagine a scenario where a treatment group household increases use of heat or air conditioning in response to colder or hotter weather more so than households without children or with lower income families in the control group. With the use of household fixed effect, which essentially subtracts mean daily load for each household, differences in extreme electricity use between treatment and control could bias the estimated treatment effect. To account for these complications, we estimate the treatment effect of each lesson instead of all lessons on average. We use a sample period that includes 30 days prior to the lesson, the day of the lesson, and seven days following the lesson.

We investigate whether the lessons have different effects on electricity use at home. It is plausible that the content of the lesson matters, and curriculum that includes action items for reducing electricity at home may result in deeper energy reductions at home. On the other hand, perhaps any lesson on energy prompts discussion of energy use at home and provides a reminder or cue to engage in energy-efficient behaviors. In other words, the mechanism linking energy lessons to energy conservation may be that the lessons are reminders, rather than instructions. Therefore, understanding how curriculum relates to electricity conservation has critical implications for curriculum development and timing of lessons depending on programmatic goals. One limitation of our empirical setting is that all students received the same lessons on the same days (i.e. Phantom Power was the first lesson for all students and all students received this lesson on October 27, 2015). Therefore, we must interpret results with caution because the order of the lessons is confounded with the content of the lesson.

Finally, we investigate heterogeneity in treatment effect by several household characteristics. We first explore differences in treatment effect by level of electricity consumption. While higher-consuming households potentially have more opportunities to reduce electricity use, lower-consuming households may be more motivated to conserve either because they already have energy efficient capital stock, but may not be engaging in energy-efficient household habits, or because they are more financially constrained. Then, we explore heterogeneity based on house characteristics. We focus on four characteristics: assessed value, gross area, number of bedrooms, and year built. While assessed value is highly correlated with square footage and neighborhood, it may also be an indication of improvements due to remodeling. Remodeled houses may have more efficient features, such as insulation. Above median assessed value may also be an indicator of wealth of the household. Wealthier households may be more likely to have many energy-consuming appliances and electronics, which family members could turn off or unplug to conserve electricity. Gross area and number of bedrooms are both indicators of house size. It is possible that larger houses have more opportunity for energy reductions. Newer homes may include more electronics that can either be unplugged or turned off, or that allow for easy and precise control of energy use, such as for heating or cooling. These features of newer homes may provide more energy savings opportunities. To test for heterogeneity, we interact the variable of interest with a binary variable equal to one for households who are above median[6] in each characteristic, based on the treatment group:

$$load_{it} = \beta_1 LessonDay_t \ x \ TreatmentHH_i$$

$$+ \beta_2 LessonDay_t \ x \ TreatmentHH_i \ x \ Characteristic_i$$

$$+ \alpha_i + \theta_t + \theta_t \ x \ Characteristic_i + \varepsilon_{it} \quad (4)$$

We additionally interact day fixed effects with the indicator for high consumption to control differentially for high-consuming control households.

5. Data

In the 2015-2016 academic year, 586 students received energy lessons in the electric utility's territory. We made a substantial attempt to obtain data for all students, but most schools declined to share the necessary information we needed to match student households with electricity data. We obtained data for 65 fourth and fifth grade students at a single private school in the utility's territory that received three energy lessons. An educator delivered programming on phantom power sources on October 27, 2015, energy pathways on January 12, 2016, and wind energy on May 10, 2016. We obtained electricity consumption data from July 1, 2014 – October 5, 2016 for N=50 households[7] that contained a treated student. We additionally obtained electricity consumption data for N=1,485 households selected at random by the electric utility.[8]

[6] Splitting by treatment group median maximizes the sample size in each bin (e.g. there are an equal number of treated households above and below median).

[7] Data for ten households were not obtained due to students living outside the utility's territory, two households contain two students who are siblings, and three households could not be matched due to issues with addresses.

[8] One possible concern is that the randomly selected control households may contain a student who received an energy lesson at a different school. However, we are not concerned about this possibility because the chances of contamination are roughly 0.16% (468 treated student households not included in our treatment group divided by a residential customer base of roughly 290,000 households).

Importantly, all of our treatment and control households are in the same geographic area and thus experience the same weather conditions, which are capture by the day fixed effect in Equation 3. Depending on meter type installed at each house, data were provided at 15-minute intervals, peak and off-peak hours, or at the daily level. We aggregated all data to consumption per day for each household, resulting in a dataset of N=756,804 household-day observations.

Figure 1 plots a 7-day smoothed moving average of daily electricity consumption treated and untreated households. Mean daily load for the treatment group is significantly higher than for untreated households (means = 31.1 kWh/day for treated households and 18.6 kWh/day for control households, $t(1535) = -5.37$). This difference is likely due to several socioeconomic and demographic factors. All households in the treatment group contain at least one child, whereas only 34% of households in the service territory contain a child under 18 years old (US Census 2000). According to 2015 American Community Survey estimates, family households earn nearly $17,000 more in annual income than non-family households.[9] Furthermore, all households in the treatment group presumably have sufficient disposable income to enroll their child(ren) in a private school.

[Figure 1 about here]

One empirical concern is that the untreated group, which is comprised of randomly chosen households, may not be an adequate counterfactual for the treatment group. We construct a counterfactual for the treatment group by choosing control households with a mean daily load for the thirty days before each lesson that is sufficiently close to the mean daily load of each treatment household in that time period. We match each treatment household to the k=2 nearest neighbors, with replacement, within a caliper of 1 kWh mean daily load. In order to capture the

[9] Median family household income = $45,540. Median non-family household income = $27,724. (ACS 2015)

most appropriate counterfactual, we construct a control group for each lesson specifically, using the thirty days prior to that lesson to calculate mean daily load. Figure 2 illustrates the agreement between treatment and control groups for each lesson.

[Figure 2 about here]

We additionally obtain assessor data for *N*=41 single family homes in the treatment group and *N*=55 homes in the control group for houses transacted since 1995.[10] We use this data to test for heterogeneity in treatment effect by house characteristics.

6. Results and Discussion

We present main results in Table 1. The dependent variable is daily load for each household, with the coefficient of interest on $LessonDay_t \; x \; TreatmentHH_i$ interpreted as a kWh reduction in electricity use on the day of lesson 1 relative to the control group. Column 1 interacts binary variables for days prior to, day of, and after the lesson with a binary variable indicating treatment status. Column 2 instead uses household and day fixed effects. The preferred model is Column 2 and Figure 3 illustrates the results of this model. The coefficients on the prior day indicators are all insignificant, which show no statistically discernible difference in electricity load the three days leading up to the lesson and provide evidence for the appropriateness of the constructed control group. [11]

[Table 1 about here]

[10] Of course a larger sample would be ideal, and this analysis should be viewed as strictly exploratory.
[11] Table 1A and Figure 1A in the Appendix present results from the same models using a control group comprised of randomly chosen households. Note that the coefficient on the indicator for treatment status, 7.894, in Column 1 is further evidence of the difference between treatment and control groups, and incentive for constructing a more appropriate counterfactual through nearest neighbor matching. In contrast, this coefficient in Column 1 of Table 1 of the main paper (using matching methods) is both smaller in magnitude and statistically insignificant.

The coefficient of interest is on the interaction $LessonDay_t \; x \; TreatmentHH_i$, which estimates the reduction in load on the lesson day for the treatment group relative to control. The coefficient estimate is negative and significant at the 10 percent level, indicating a 2.5 kWh reduction in electricity use on the day of the lesson for households that contain a student who received the lesson, an eight percent decrease in electricity load relative to baseline. While the majority of coefficients on the next day indicators are statistically insignificant, the coefficient estimate for two days following the lesson is positive and similar in magnitude to our coefficient of interest. This could be an indication that households defer electricity use the day of the lesson to two days following the lesson. For example, households may defer doing a load of laundry, watching television, or using a computer. Importantly, evidence of deferment indicates a net zero reduction in electricity load due to the energy lesson.

[Figure 3 about here]

Table 2 compares results of the preferred model from Table 1 using various control groups. This table is meant as a robustness check to ensure the consistency of our main finding that electricity consumption is reduced on day of the energy lesson. Columns 1-4 use a constructed counterfactual with k=1 through 4 neighbors, respectively. Column 5 uses all neighbors within the 1 kWh/day caliper.

[Table 2 about here]

The coefficient of interest is negative and significant Columns 2-5, indicating a reduction in electricity use on the order of six to eight percent on the day of the lesson. The coefficient of interest in Column 1 is similarly negative, though larger in magnitude with larger standard errors. Consistency of the coefficient of interest lends confidence to our main finding of a short-term reduction in electricity consumption on the days of energy lessons. Across all columns, the coefficient estimate on the two days later indicator is positive, though significance is lost in Column 5. Again, this suggests an increase in electricity load two days following the lesson, and provides evidence of the treatment effect being caused by deferral of load rather than reduction in load.

[Table 3 about here]

Next, we explore differences in treatment effect by lesson. Table 3 presents the results of this analysis, where each column represents each lesson.[12] Column 1 repeats results of the preferred model in Table 1. Column 2 (3) uses the same model but for the sample period of 30 days before the second (third) lesson, the day of, and seven days following. For each column, the control group is constructed by matching each treated unit to two nearest neighbors based on mean daily baseline load for the thirty days prior to the lesson, subject to a caliper of 1 kWh/day. Coefficients on the three prior day indicators are statistically insignificant for all columns, indicating good agreement between treatment and control groups. Results fail to show a significance difference in load on the days of lessons 2 and 3. Interestingly, we see a significant increase in load for two to four days following the third lesson, roughly equal to a ten percent increase in daily use for each day relative to baseline. While we cannot rule out this finding being an odd effect of the lesson on wind energy, our intuition suggests this effect may be more likely due to some other event or occurrence common to all houses of fourth and fifth graders in this particular school. Lesson content, timing, and dates are confounded in our empirical setting, so we cannot disentangle whether the absence of treatment effects of the second and third lesson is due to lesson content (specifically a lack of energy saving action items) or due to other factors.

[12] Tables 2A and 3A in the Appendix present results from models with one through four nearest neighbors and all neighbors, subject to the caliper of 1 kWh/day, for each of lessons 2 and 3 respectively.

We emphasize caution in interpreting these particular results. However, that we find a treatment effect for the lesson most directly applicable to energy conservation is suggestive that lesson content matters.

Lastly, we explore heterogeneity in treatment effect by several characteristics. Table 4 presents results from this analysis for the lesson on Phantom Power, which was the only lesson to have a statistically significant treatment effect.[13] Column 1 investigates heterogeneity in treatment effect by baseline electricity consumption. We fail to find a statically distinguishable difference in electricity use the day of the lesson between above- and below-median consuming households, though the signs on the coefficients suggest higher-consuming households may reduce more on the day of the lesson. Columns 2-5 explore differences based on house characteristics. All coefficients on $LessonDay_t \, x \, TreatmentHH_i$ are negative, indicating reductions in electricity consumption for households below median in each characteristic. Signs of coefficients on the interactions with the characteristics suggest that these reductions may be smaller for newer households with higher assessed values and enhanced for larger households. While the magnitudes and signs of the coefficients of interest are interesting – and could align with intuition – the standard errors are unfortunately large due to our limited sample size. This is an area ripe for future research.

[Table 4 about here]

7. Conclusions and Policy Implications

In this study, we conduct an empirical analysis of the effects of in-school energy lessons for fourth and fifth grade students on household electricity consumption. Using a differences-in-

[13] Analogous results for the other lessons are included in the Appendix: Tables 4A and 5A.

differences empirical framework with a rich panel data set, we compare electricity load on the day of the energy lesson and on seven days after between households with students who received the energy lesson and control households that closely match treated households in mean daily baseline load. We estimate energy reductions on the order of 2.5 kWh on the day of a lesson regarding phantom loads, roughly equivalent to eight percent of mean daily load. However, we see an increase in load of roughly the same magnitude two days following the lesson, suggesting deferment of electricity use rather than conservation.

We fail to find an effect of lessons about energy pathways and wind energy on electricity consumption the days of those lessons. Our empirical setting cannot allow us to causally attribute this difference in treatment effects to the content of the lessons due to confoundedness with timing and ordering of the lessons. For example, we cannot rule out that students become desensitized to the novelty of the energy lessons by the second lesson. However, intuition points to the curriculum content as a driver of the effect. The curriculum for the lesson on phantom loads includes direct action items for the students to take home. We also investigate whether there is a difference in treatment effect for high-consuming households or households with certain house characteristics. The magnitudes of the coefficient estimates suggest that the treatment effect may be larger for high consuming households and families in larger homes, but smaller for households in higher valued or more recently constructed homes. However, our small sample of treated households limits our ability to identify heterogeneity in treatment effect with statistical precision.

This study contributes an empirical case study to the sparse literature on energy education. While there are benefits to a case study-style analysis using all students from one school, there are also critical limitations that influence our interpretation of the treatment effect.

Most importantly, this case study is of a private, Catholic school, which arguably differs from other schools both in socio-economic demographic characteristics and other unobservable characteristics. For example, one could argue that the households who went through the effort to enroll their children in a private school are also more motivated than households whose children attend public school. More motivated households may also be more receptive to behavior change or more likely to discuss energy lessons with their children after school. Therefore, the effects we find may represent an upper bound on changes in energy use due to in-school lessons.

Despite a limited sample, this research suggests that there is a link between what happens at school and measurable outcomes at home, and warrants more research on how in-school education can influence energy-related outcomes. Future research should include a more diverse set of schools in the analysis. With a larger sample size and experimental design, empirical analysis would also be able to tease out effects lesson content and timing, as well as investigate heterogeneity in effect by student grade, household socioeconomic or demographic characteristics, household characteristics, and consumption levels. With a more thorough understanding of how to optimize factors like curriculum, timing, and duration, in-school education could be a powerful tool for encouraging energy efficiency and conservation at home.

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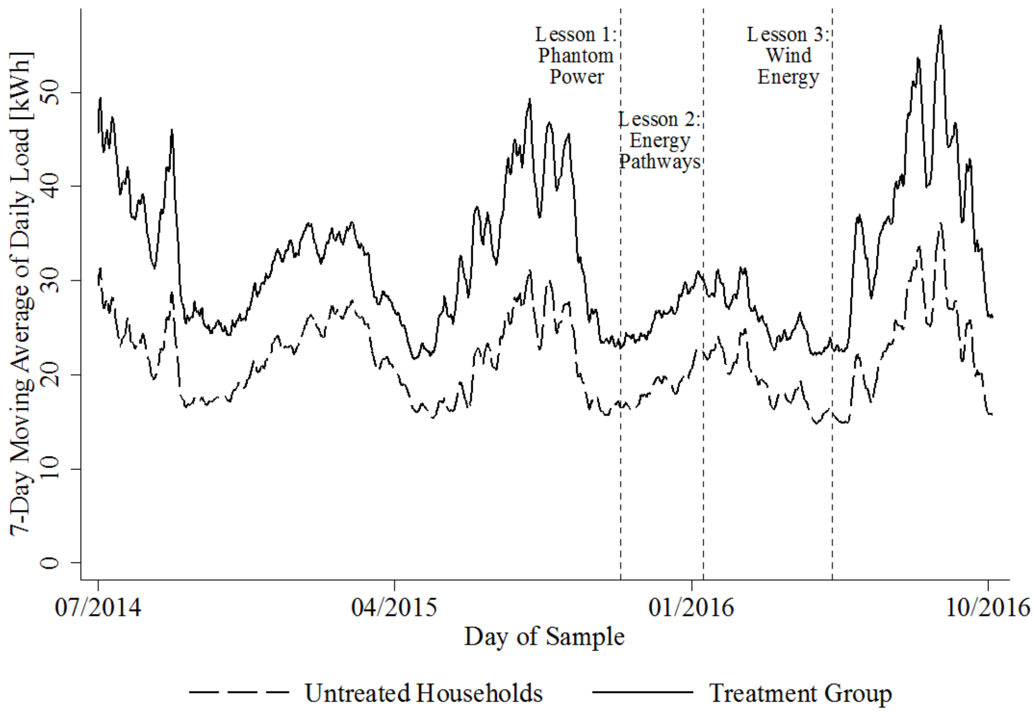
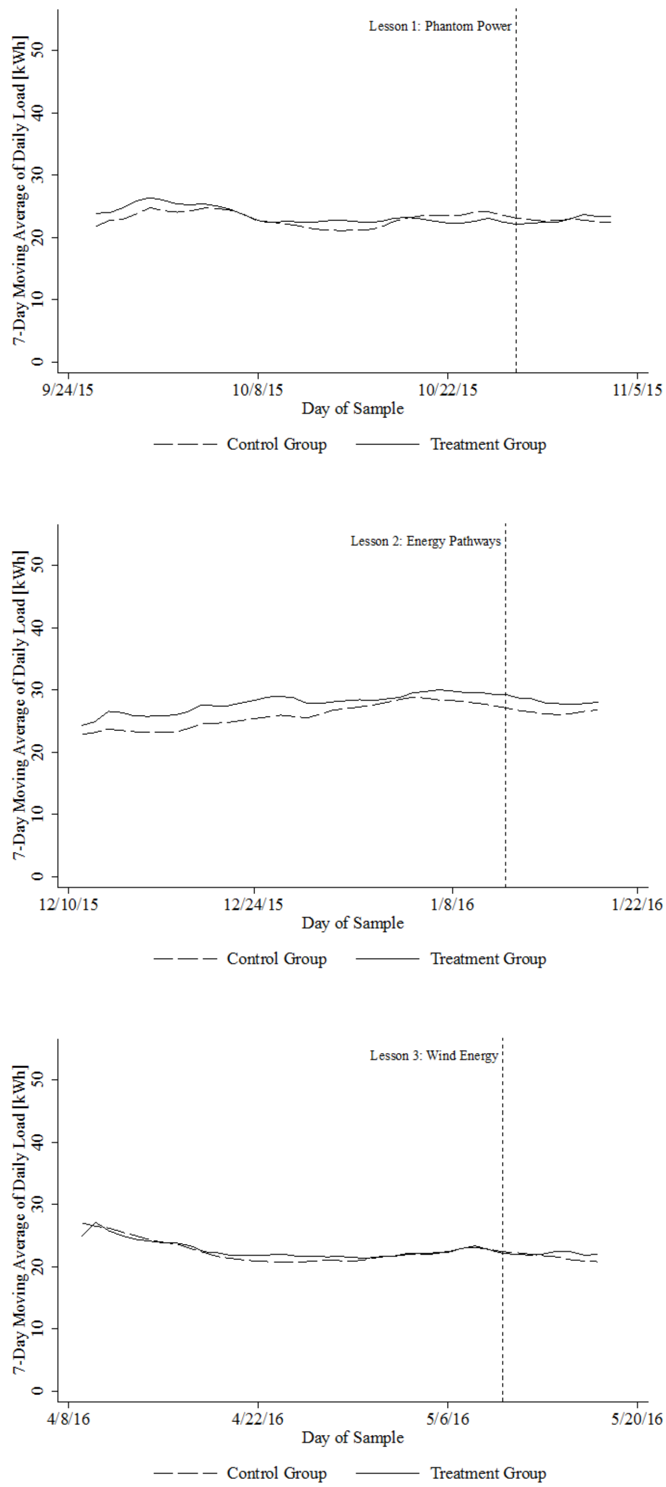
Tables and Figures

Figure 1. Electricity consumption



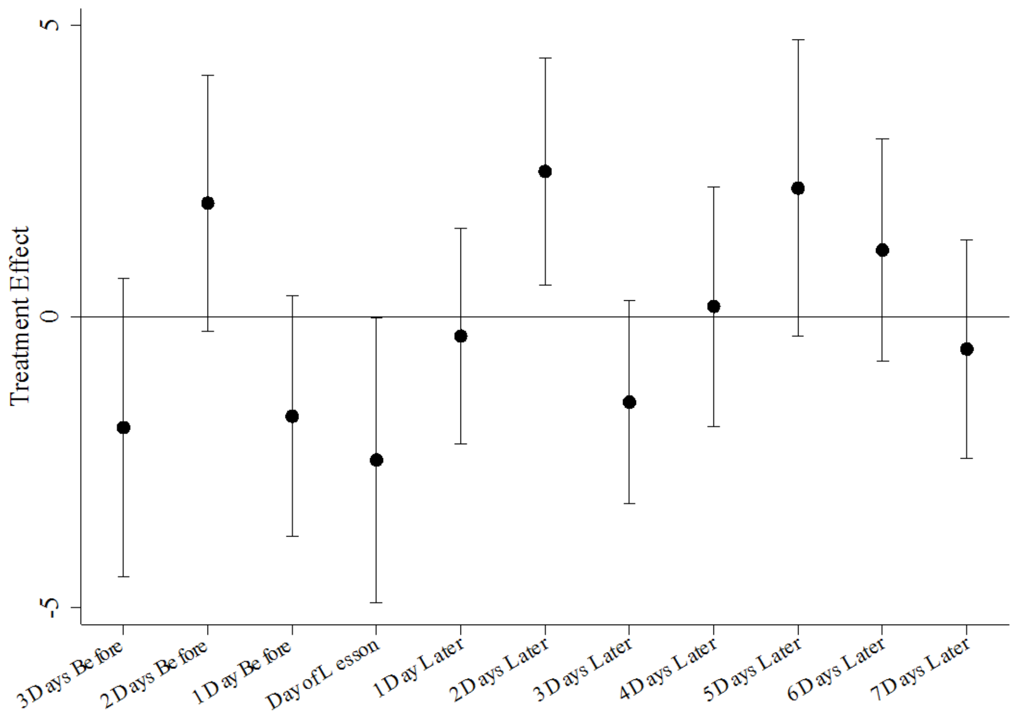
Notes: Figure illustrates smoothed daily load using a 7-day moving average. The treatment group is composed of N=50 households who contain a student who received all three energy lessons (mean daily load = 31.1 kWh/day). Vertical reference lines denote the energy lesson: Phantom Power occurred on 10/27/2015, Energy Pathways occurred on 1/12/2016, and Wind Energy occurred on 5/10/2016. The untreated group is composed of N=1485 randomly chosen households (mean daily load = 18.6 kWh/day).

Figure 2. Electricity consumption



Notes: Figure illustrates mean daily load for the treatment and control groups smoothed using a 7-day moving average. The control group (N=85 households) is constructed by matching on mean daily load for 9/26/2015-10/26/2015, using the two nearest neighbors with replacement and a caliper of 1 kWh/day). Treated households without a match within the caliper are dropped. N=48 households remain in the treatment group.

Figure 3. Treatment effect over time for lesson 1 on phantom power



Notes: Figure plots coefficient estimates for three days prior to the date of lesson 1, the day of the lesson, and the seven days following the lesson. This figure illustrates results in Column 2 of Table 1. Error bars indicate the 90% confidence interval around the point estimate.

Table 1. Treatment Effect of Lesson 1

DV = Daily Load [kWh]	(1)	(2)
Treatment HH	0.486	
	(2.013)	
Lesson Day	0.375	
	(1.295)	
Lesson Day x Treatment HH	-1.771	-2.463*
	(1.609)	(1.479)
1 Next Day x Treatment HH	-0.202	-0.334
	(1.243)	(1.121)
2 Next Day x Treatment HH	1.496	2.495**
	(1.303)	(1.175)
3 Next Day x Treatment HH	-1.165	-1.469
	(1.257)	(1.057)
4 Next Day x Treatment HH	1.151	0.175
	(1.500)	(1.242)
5 Next Day x Treatment HH	3.697**	2.207
	(1.694)	(1.538)
6 Next Day x Treatment HH	1.524	1.145
	(1.375)	(1.154)
7 Next Day x Treatment HH	-0.904	-0.557
	(1.316)	(1.136)
1 Prior Day x Treatment HH	-1.648	-1.713
	(1.346)	(1.248)
2 Prior Day x Treatment HH	1.429	1.951
	(1.409)	(1.326)
3 Prior Day x Treatment HH	-2.778*	-1.906
	(1.649)	(1.544)
Household Fixed Effects	N	Y
Day Fixed Effects	N	Y
Observations	5,103	5,103
R-squared	0.00551	0.689
Adjusted R-squared	0.001	0.678

Notes: Dependent variable is mean daily load for N=48 treatment households and N=85 control households, with data range of 9/26/2015-11/3/2015. Column 1 uses a differences-in-differences model while Column 2 adds household and day fixed effects. Errors are clustered at the household level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels.

Table 2. Treatment Effect of Lesson 1

DV = Daily Load [kWh]	(1)	(2)	(3)	(4)	(5)
Lesson Day x Treatment HH	-3.299	-2.463*	-2.306*	-2.015*	-2.058**
	(2.022)	(1.479)	(1.282)	(1.176)	(0.967)
1 Next Day x Treatment HH	0.275	-0.334	-0.878	-1.009	-1.221
	(1.402)	(1.121)	(1.033)	(0.989)	(0.839)
2 Next Day x Treatment HH	2.721**	2.495**	2.172*	2.023*	1.254
	(1.292)	(1.175)	(1.108)	(1.056)	(0.964)
3 Next Day x Treatment HH	-0.167	-1.469	-1.414	-1.222	-1.487*
	(1.014)	(1.057)	(0.968)	(0.919)	(0.813)
4 Next Day x Treatment HH	0.718	0.175	0.266	-0.359	-0.504
	(1.324)	(1.242)	(1.130)	(1.117)	(0.965)
5 Next Day x Treatment HH	2.313	2.207	1.843	1.384	2.201
	(1.583)	(1.538)	(1.474)	(1.443)	(1.346)
6 Next Day x Treatment HH	1.879	1.145	0.711	0.719	0.872
	(1.249)	(1.154)	(1.136)	(1.091)	(1.000)
7 Next Day x Treatment HH	0.815	-0.557	-0.405	-0.281	0.367
	(1.161)	(1.136)	(1.037)	(0.979)	(0.856)
1 Prior Day x Treatment HH	-0.957	-1.713	-1.451	-0.935	-0.517
	(1.360)	(1.248)	(1.106)	(1.040)	(0.874)
2 Prior Day x Treatment HH	0.852	1.951	1.332	1.018	1.772
	(1.486)	(1.326)	(1.322)	(1.319)	(1.224)
3 Prior Day x Treatment HH	-1.451	-1.906	-2.077	-1.656	-1.469
	(1.900)	(1.544)	(1.440)	(1.377)	(1.236)
Household Fixed Effects	Y	Y	Y	Y	Y
Day Fixed Effects	Y	Y	Y	Y	Y
Observations	3,532	5,103	6,716	7,988	29,842
R-squared	0.696	0.689	0.698	0.700	0.708
Adjusted R-squared	0.683	0.678	0.688	0.690	0.699
Caliper	1	1	1	1	1
Neighbors	1	2	3	4	all

Notes: Dependent variable is mean daily load with data range 9/26/2015-11/3/2015. The treatment group contains N=48 households. The control group is constructed by matching on mean daily load for the pre-treatment period 9/26/15-10/26/2015. Each column uses a different number of nearest neighbors, all subject to a caliper of 1 kWh/day. Errors are clustered at the household level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels.

Table 3. Treatment Effect of Lessons 1, 2, and 3

DV = Daily Load [kWh]	(1)	(2)	(3)
Lesson Day x Treatment HH	-2.463*	-0.0134	-0.404
	(1.479)	(1.326)	(1.062)
1 Next Day x Treatment HH	-0.334	-0.456	0.102
	(1.121)	(1.392)	(0.927)
2 Next Day x Treatment HH	2.495**	-0.232	-0.538
	(1.175)	(1.229)	(1.080)
3 Next Day x Treatment HH	-1.469	-0.512	2.481**
	(1.057)	(1.191)	(1.176)
4 Next Day x Treatment HH	0.175	1.309	3.452***
	(1.242)	(1.381)	(1.272)
5 Next Day x Treatment HH	2.207	-0.443	2.762**
	(1.538)	(1.374)	(1.110)
6 Next Day x Treatment HH	1.145	-1.771	-0.454
	(1.154)	(1.584)	(0.887)
7 Next Day x Treatment HH	-0.557	-1.543	0.364
	(1.136)	(1.525)	(0.834)
1 Prior Day x Treatment HH	-1.713	-0.288	-1.456
	(1.248)	(1.205)	(1.263)
2 Prior Day x Treatment HH	1.951	1.677	1.509
	(1.326)	(1.341)	(1.138)
3 Prior Day x Treatment HH	-1.906	0.257	-0.263
	(1.544)	(1.280)	(1.234)
Household Fixed Effects	Y	Y	Y
Day Fixed Effects	Y	Y	Y
Observations	5,103	5,403	5,219
R-squared	0.689	0.739	0.767
Adjusted R-squared	0.678	0.730	0.758

Notes: Dependent variable is mean daily load and the data spans thirty days prior to seven days after each lesson. Each column corresponds to a lesson. Lesson 1 (column 1) occurred on 10/27/2015. Lesson 2 (column 2) occurred on 1/12/2016. Lesson 3 (column 3) occurred on 5/10/2016. The control group is constructed by matching on mean daily load for thirty days prior to the lesson using two nearest neighbors subject to a caliper of 1 kWh/day. Errors are clustered at the household level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels.

Table 4. Heterogeneity in Treatment Effect of Lesson 1

DV = Daily Load [kWh]	(1)	(2)	(3)	(4)	(5)
Characteristic =	Above Median Consumption	Above Median Value	Above Median Gross Area	Construction Newer than 1955	More than 3 Bedrooms
Lesson Day x Treatment HH	-0.760	-2.468	-0.341	-2.763	-1.769
	(1.247)	(2.108)	(1.244)	(2.585)	(2.044)
Lesson Day x Treatment HH x Characteristic	-4.825	0.454	-4.087	1.296	-1.634
	(3.325)	(3.701)	(3.653)	(3.510)	(4.711)
Household Fixed Effects	Y	Y	Y	Y	Y
Day Fixed Effects	Y	Y	Y	Y	Y
Day x Characteristic FE	Y	Y	Y	Y	Y
Observations	5,103	3,681	3,681	3,681	3,681
R-squared	0.694	0.657	0.656	0.656	0.656
Adjusted R-squared	0.681	0.640	0.639	0.639	0.639

Notes: Dependent variable is mean daily load and the data spans thirty days prior to seven days after each lesson. Each column explores heterogeneity in the treatment effect by some characteristic, a binary variable equal to one if the household is above (treatment group) median for that characteristic. Median consumption = 24.5 kWh/day averaged over all days prior to the first lesson. Median assessed house value = $186,690. Median gross area = 3,274 square feet. Median year of construction = 1955. Median number of bedrooms = 3 bedrooms. The control group is constructed by matching on mean daily load for thirty days prior to the lesson using two nearest neighbors subject to a caliper of 1 kWh/day. Errors are clustered at the household level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels.