Dynamic Pricing, Attention, and Automation: Evidence from a Field Experiment in Electricity Consumption

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Abstract

Dynamic pricing models typically assume that consumers respond to marginal incentives. But how attentive are consumers to these incentives? I use a field experiment to assess the impact of dynamic pricing on residential electricity consumption and find strong evidence of inattention. I propose a model to interpret the results which suggests that the benefits of dynamic pricing may be substantively undermined by inattention. I also explore the role of automation in dynamic pricing, which holds the promise of reducing the cognitive choice frictions that cause inattention and lowering the effort cost of responding to price changes. I report three primary findings. First, households—both with and without automation—significantly respond to a short term price increase by reducing consumption. Second, responses are very insensitive to the size of the price change. A price increase of 31 percent causes consumption to fall by 11 percent on average, whereas a price increase of 1,875 percent causes an average reduction of 13 percent. Third, automation causes responses that are five times larger than the average effect, but are still insensitive to the price level. The results suggest that households use simplifying heuristics when facing dynamic prices and that automation reduces effort costs, but does not resolve inattention. I apply the model to recover bounds on the price elasticity of demand and shed light on the potential attention costs of dynamic pricing.

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1 Introduction

An increasing number of consumer decisions feature dynamic pricing—the application of marginal cost pricing to goods with costs that vary over time. Examples include ride-hailing applications like Lyft and Uber (Cramer and Krueger, 2016), e-commerce websites such as Amazon (Borenstein and Saloner, 2001), and end-use electricity tariffs (Joskow and Wolfram, 2012). Neoclassical models imply that dynamic pricing improves efficiency if demand is elastic and buyers are fully informed, yielding a real-time efficiency benchmark where prices and consumption instantaneously adjust to cost fluctuations. However, evidence from a variety of decision settings finds that consumers can be inattentive to marginal financial incentives such as value-added taxes (Chetty, Looney, and Kroft, 2009) or shipping costs (Hossain and Morgan, 2006).[1] Thus, a necessary component to determining the actual efficiency of dynamic pricing is understanding how attentive consumers are to dynamic marginal incentives. This paper addresses this question within the context of residential electricity demand.

One solution to the inherent tension between dynamic pricing and consumer attention costs is automation. The trend in dynamic pricing follows the wide deployment of internet-connected "smart" devices (e.g., phones and thermostats). In addition to allowing sellers to update prices and monitor consumption at lower cost, smart devices can also enable buyers to automate decisions.[2] If automation accurately represents preferences, it provides a substitute to a more active (and cognitively costly) response to frequent price changes. It is standard to assume that enabling automation has an important role to play when discussing the efficiency potential of dynamic pricing in electricity (Borenstein and Holland, 2005; Joskow, 2012), but there remains little research testing its performance in the field.

This paper explores three lines of inquiry to assess the effect of dynamic pricing and automation on residential electricity consumption. First, how large is the response of demand to dynamic price changes? Second, do consumer responses depart significantly from the behavior implied by standard neoclassical models that assume full information? Third, if information frictions generate substantive differences between observed behavior and neoclassical assumptions, does enabling

[1]See DellaVigna (2009) for a review and more examples of consumer inattention.

[2]Examples include technologies that generate automatic price reminders for e-commerce purchases where prices vary such as airline tickets, hotel rooms, and used automobiles. There are also applications that allow for automatic bidding into online auctions platforms like eBay.

automation reduce them in the context of dynamic pricing?

In order to answer these questions, I conducted a field experiment with 5,531 households that face real economic stakes. The experiment was implemented by a private company that pays its customers to reduce their electricity usage when demand reductions are particularly valuable.[3] During the experiment, households continued to pay a monthly utility bill, but were also exposed to one-hour events that featured a linear financial incentive to reduce consumption. The incentive was offered in the form of points that could be redeemed for dollars, but was specifically designed to replicate changes in the per unit price of electricity.[4] Households were given less than one hour ahead notice each time an event occurred and notified of their performance a few days later. Any net earnings could then be cashed out via the company's website.

Each time an event was called during the first 90 days of enrollment, each household was exposed to a price increase that was randomly varied between \$0.05-\$3/kWh. These levels represent dramatic changes in the marginal price given the non-event average price of electricity was \$0.16/kWh. The experiment also featured a recruit and delay design that randomly assigned a subset of households to serve as a control group upon signup. Together, these two sources of variation allow me to identify the impact of marginal financial incentives on consumption over a wide range of prices. To my knowledge, this is the only dynamic pricing study to use within event random price variation.[5]

Households were also offered an automation service that would shut off connected devices when an event was called. Among the enrolled, half of the households were randomly selected to receive a full-cost rebate for purchasing a smart device and connecting it to the automation service. The rebate subsidized the purchase of one smart thermostat or two smart plugs with retail values of \$240 and \$80, respectively. This caused an 82 percent increase in uptake from 4.9 percentage points in the unsubsidized group to 8.9 percentage points in the encouraged group. I use this variation to estimate the local average treatment effect of automation on energy consumption for those who

[3]Following the taxonomy developed in Harrison and List (2004), this is a *natural field experiment* because households did not know they were part of a study and experimental variation occurred within the company's product.

[4]Households were paid for reducing consumption relative to an individual forecast and penalized for exceeding that forecast at the same marginal rate. This is similar in spirit to the critical peak rebates studied in past dynamic pricing experiments such as Wolak (2006) with the important distinction that there was substantially less asymmetry in the incentive around the forecast. Households faced a linear incentive except when penalizing the household resulted in them losing money from the program overall. This edge case is the only departure from the incentive being financially equivalent to changes in marginal price.

[5]Ito, Ida, and Tanaka (2017) is the closest existing study within the dynamic electricity pricing literature since it uses random price variation between events. The within event randomization I use in this paper has a broader support and identifies the price responsiveness during each event.

adopted as a result of the rebate.

I present three main empirical findings that address the lines of inquiry stated above. First, treated households reduced consumption by 12 percent on average during pricing events relative to control households. The response is precisely estimated and 77 percent of the reduction is explained by households without automation, meaning that the results reflect active decision-making. Second, households were extremely insensitive to the size of the price change. I find that the average reduction during events increased from 11 percentage points to 13 percentage points when the price change increased from the lowest level $0.05/kWh (a 31 percent price increase) to the highest level $3/kWh (a 1,875 percent price increase). Third, among households who took up the automation as a result of the rebate, the automation technology increased average response to any price change by an additional 56 percentage points during events, but did not alter the insensitivity to marginal price. Back of the envelope calculations suggest the additional savings from automation are substantial and the average payback period for purchasing a device at the retail price range from 2 to 10 years, depending on the technology.

To offer an interpretation of the findings, I propose a model of rational agents with limited attention and heterogeneous costs of information, drawing from the framework introduced in Chetty (2012). In the model, agents choose to respond to dynamic prices with a heuristic or exert costly effort to become fully informed. For example, households could be using the expected price change each event instead of the actual price change. The model delivers the standard choice primitive estimated in dynamic pricing experiments, the price elasticity of demand, but relaxes the condition that all households are perfectly attentive.

I provide empirical evidence in support of the model's assumptions over competing mechanisms using a separate set of moral suasion interventions that occurred after the pricing interventions. During these events, households were randomly assigned to receive moral suasion messages promoting the environmental attributes of reducing electricity usage instead of dynamic price changes. Reductions from price messages are larger and significantly different from reductions from moral suasion, which are not significantly different from zero. This suggests that households observe the message content and that responses are driven by price rather than preferences for the environmental attributes of consumption.

I use my experimental variation to show that price elasticity estimates derived from the limited attention model differ substantively from those obtained from the neoclassical assumptions standard to the dynamic pricing literature. I make several simplifying assumptions to give the model empirical tractability and estimate bounds on the price elasticity of demand by assuming a plausible range for the agents' heuristic response. The bounds are -0.483 and -0.051, generally covering the range of estimates from past studies with longer term price variation. Importantly, the range implies the consumer is substantially more elastic than the estimate recovered using the standard method from the literature, -0.047.

Using my estimates of the price elasticity of demand, I examine an important implication of inattention: the misoptimization cost a household experiences by being inattentive to the marginal price. I estimate the average misoptimization cost per one hour event falls in the range of \$0.03 to \$0.44, representing 10 to 142 percent of the average savings. In the program studied here, households experience about 100 events per year, implying the nominal stakes of this dynamic pricing program are small for the average household and supporting the interpretation that inattention could be rational with modest information costs. However, small mistakes could quickly add up if applied to the 130 million customers that make up the \$178 billion U.S. residential electricity sector, over 90 percent of whom do not pay time-varying rates (EIA, 2015).

This paper relates to the larger literature on attention and information provision in economic decision-making. In the energy context this includes investigations into consumer attentiveness to the non-salient energy attributes of durable goods such as cars (Sallee, 2014), refrigerators (Houde, 2017), and light bulbs (Allcott and Taubinsky, 2015). There is also a connection to the literature on rational inattention in the presence of costly information acquisition (Sims, 2003; Bartoš, Bauer, Chytilová, and Matějka, 2016). Within this literature, I make three contributions.

First, the paper provides novel evidence on how information costs manifest in dynamic pricing. This contributes to the nascent economics literature that seeks to empirically identify how consumers actually *perceive* non-salient or complex financial incentives. For example, Rees-Jones and Taubinsky (2016) provide strong evidence that individuals facing nonlinear tax schedules exhibit behavior consistent with "ironing" or linearizing the schedule using their own average rate. Ito (2014) finds similar results in electricity consumption, showing that households respond to lagged average rates rather than marginal incentives when facing nonlinear tariffs in their monthly bills. Within the context of this study, households respond to a positive price change, but not to the marginal rate. This hierarchical simplification of information is consistent with the heuristic phenomena of *scope neglect* shown in contingent valuation studies (Kahneman, 2003).[6] These phenomena could also exist in other dynamic pricing settings such as ride-hailing applications if consumers do not check the price before making the decision to use the service.

My finding that demand is insensitive to the magnitude of the price change extends and complements the existing literature on dynamic pricing in electricity. In previous experiments, Jessoe and Rapson (2014) (henceforth JR) find information provision makes demand more elastic during price changes and Ito, Ida, and Tanaka (2017) (henceforth IIT) find consumers respond much more to price than the non-price motivator moral suasion.[7] JR study six events with two price increases of 200 and 600 percent and find an average response of 16 percent for informed households. IIT study 31 events with three price increases between 260 and 420 percent and uses random variation in the price of each event to estimate the casual effect of the price level. IIT finds increasing the price change from the lowest to the highest level increases reductions from 14 to 17 percent, implying a price elasticity of -0.14. By using five price increases between 31 and 1,875 percent, I characterize the response to marginal incentives over a substantially broader support and thus test for consumer inattention more completely.

Second, the paper is the first to provide conclusive evidence on the price elasticity of residential electricity demand in the very short-run. Previous studies have focused almost exclusively on "day-ahead" dynamic pricing where households were given notice on the day prior to a price change.[8] The fact that households respond to price changes with less than one hour notice (the average was 5 minutes) has special importance in electricity due to the structure of wholesale electricity markets. The timing of the events in this study mimics the deployment of "real-time" price interventions that respond to the spot market for electricity procurement. Previous studies have used theory and simulations to study the potential for real-time pricing to provide efficiency using elasticity estimates from longer-term price variation (Borenstein and Holland, 2005). My results provide the

[6] Findings from psychology and economics show that individuals typically respond to more readily accessible categorical characteristics of a decision (a price change occurring), but not its extensive characteristics (the magnitude of the change).

[7] A number of other studies have used experiments to evaluate consumer responses to dynamic electricity prices (Wolak, 2006, 2010, 2011; Faruqui and Sergici, 2010; Allcott, 2011a; Fowlie, Wolfram, Spurlock, Todd, Baylis, and Cappers, 2017). See Harding and Sexton (2017) for a comprehensive review of the literature.

[8] To the best of my knowledge, JR is the one exception. Three of the six events they study give 30-minute notice. While their results for these periods are statistically insignificant, their point estimates are similar in magnitude to what I find.

first experimental evidence on the efficiency potential of real-time residential pricing programs that better reflect marginal costs from the wholesale market.[9]

Third, the paper provides novel evidence on how automation affects dynamic price responses. To the best of my knowledge, Bollinger and Hartmann (2017) is the only other study of automated price responses using a field experiment, but their research design, like others in the literature, evaluate technology regimes that have been assigned to entire treatment groups. The encouragement design is advantageous because it allows for the invoking of revealed preference when interpreting adoption decisions.[10] This information is valuable because uptake is generally low and households can be sensitive to defaults when making decisions about dynamic electricity tariffs (Fowlie et al., 2017). The results also provide suggestive evidence that automation, while preferred to a manual response, does not perfectly reflect preferences.[11] This derives from the fact that the company's service provided a blunt instrument that did not allow household to enter a reservation price for being automated. While households could override the response, automated households respond significantly to price *and* moral suasion, suggesting a potential default effect. This suggests future dynamic pricing research should address the framing of decisions carefully if automated responses are to be interpreted as preferences.

The paper proceeds as follows: Section 2 develops the theoretical model, Section 3 reports the experimental design and describes the data, Section 4 reports the empirical results, Section 5 applies the model to obtain estimates of demand parameters, Section 6 discusses and interprets the results, and Section 7 concludes.

2 Theoretical Model

In this section, I develop a general model to guide interpretation of the empirical results.[12]I draws upon the model from Chetty (2012) which derives bounds for the price elasticity of demand

[9] Allcott (2011a) studies a program that features hourly price variation, but despite including the term real-time pricing in its title, the hourly price variation households were exposed to in that study came from the day-ahead forward market.

[10] The encouragement rebate also reflects the tools policymakers typically use to deploy new technologies. Examples in energy include subsidizing home energy retrofits (Fowlie, Greenstone, and Wolfram, 2017), fuel-efficient vehicles (Sallee, 2011), and rooftop solar panels (Hughes and Podolefsky, 2015).

[11] Given that automation generally functions as a default, ensuring it accurately captures preferences poses a difficult task as evidenced by the literature exploring default bias in retirement savings (Carroll, Choi, Laibson, Madrian, and Metrick, 2009; Bernheim and Rangel, 2009). I do not address this tension in this study and leave the welfare consequences of automation defaults to future work.

[12] The model was not pre-specified and was developed after preliminary results were obtained.

when consumers misperceive prices.[13] The main feature is that households have limited attention when facing an unexpected change in price and must exert costly effort to become completely informed to its value. The structure delivers two sharp tests of whether there are inattentive households in a population with smooth preferences. It also recovers the price elasticity of demand. I demonstrate the model's application to my setting to recover the price elasticity in Section 5.

2.1 Limited Attention and Dynamic Pricing

Consider a heterogenous population of households with well-defined quasilinear preferences over a dynamically priced consumption good y (i.e., electricity) and a numeraire good x. To simply capture the setting, suppose there are two stages to the model during which one price change occurs. Households have a budget of Z and pay a per unit price p_0 for y. In the first stage, households receive a signal notifying them of the price change and use it to choose an information set that maximizes expected utility. In the second stage, the household optimizes consumption using the chosen information set.

Prior to the first stage, the households set a consumption plan that maximizes utility $U(x, y)$ subject to the budget constraint $x + p_0 y \leq Z$. Let $y_0 \equiv \arg\max_y U(Z - p_0 y, y)$ denote consumption when price is p_0. In the first stage, the household receives a signal m about a change in y's price to $p_d \sim F_p$, which is distributed according to the bounded distribution F_p. In this study, the signal is the message notifying households of a pricing event, but could more generally capture the notification of a price change through a bill or information campaign. This framework is isomorphic to situations where the price is clearly stated, but households face cognitive costs to map the stated price to the cost of services rendered by an input such as electricity consumption.[14]

For expositional simplicity I make several simplifying assumptions. First, assume a household's information choice is discrete such that they choose between a complete information set $S_c \equiv \{p_d\}$ and a "bounded" information set $S_b \equiv \{p_b\}$. The complete information set is the actual price change and the bounded is a heuristic (e.g., the expected dynamic price). Second, assume the relative cost of information is the only form of heterogeneity in the population. Let $\kappa \geq 0$ denote

[13] Chetty (2012) derives bounds on price elasticity under a more general set of adjustment costs which can manifest as price misperceptions or information costs. My model elaborates on the interpretation as price misperceptions.

[14] For example, households may observe the price per unit kWh conveyed in the signal m, but mapping this to the operating cost of lighting versus cooling versus TV or computer use could require substantial cognitive effort. The model can be easily modified to include these scenarios by introducing the costly information decision between m and p_d, where m contains the electricity price, but p_d is the cost of services.

the relative cost of S_c versus S_b in utility terms where $\kappa \sim F_\kappa$ is bounded above.[15] Third, I assume household prior beliefs about the signal content, denoted $\widehat{F}_p$, are exogenous.[16]

Households choose an information set by maximizing expected utility according to von Neumann-Morgenstern preferences. The bounded information is chosen if

$$V(p_b) > \mathrm{E}[V(p_d)|\widehat{F}_p] - \kappa \tag{1}$$

where I suppress the budget term for notational ease so $V(p_s)$ denotes the household's indirect utility for perceived price p_s with $s \in \{b, d\}$.[17] Define households who use the complete information "attentive" and those who use the bounded information "inattentive."

In the second stage, each household uses their information set to solve

$$\max_{x,y} \; U(x, y) = x + u(y; \eta) - \kappa s \tag{2}$$

$$\text{subject to} \qquad p_s y + x \leq Z \tag{3}$$

where $u(y; \eta)$ is the private utility derived from consuming the y for a household with taste parameter η. I assume $u(\cdot)$ satisfies the standard concavity assumptions and is continuously differentiable.[18] The household maximizes utility subject to the perceived budget constraint (3) which depends on their information choice. If they use the complete information set, the cost κ is reflected in their utility. The first-order condition to solving (2) subject to (3) is $u'(y; \eta) = p_s$, so a household will consume until their marginal utility of consumption equals their perceived price p_s with $s \in \{b, d\}$. Let $y^*(p) = (u')^{-1}(p)$ denote the demand function implied by the first-order condition.

To map household decisions to aggregate demand for y, let $k \equiv \mathrm{E}[V(p_d)|\widehat{F}_p] - V(p_b)$ denote the cost at which households are indifferent between the two information sets. Households with $\kappa \leq k$ are attentive to actual price change and households with $\kappa > k$ are inattentive in that they may respond to the price change, but not its actual value. For a unit mass of consumers, observed

[15] The model can be extended simply to include a richer information structure where information sets with greater reductions in entropy have marginally higher costs. That is, higher quality information is more costly.

[16] Households in this study were not informed of the distribution of potential prices before enrolling and there is little evidence of learning in the empirical results so this does not seem to be an implausible assumption for this setting. I relax this assumption and explore making beliefs endogenous in the Appendix. Generally, this delivers a richer set of testable comparative statics, but the study design is not well suited to comment on belief formation.

[17] This is a slight abuse of notation since indirect utility is defined over the true price and the perceived price may differ. The same outcome can be achieved by specifying a decision utility function $\tilde{V}$ for the left hand side of (1) which shares the same functional form as *experienced* utility V which is defined over the realized price.

[18] Specifically, I assume $u''(y; \eta) < 0 < u'(y; \eta), \forall y > 0$, $\lim_{y \to 0} u'(y; \eta) = \infty$ and $\lim_{y \to \infty} u'(y; \eta) = 0$.

aggregate demand is given by

$$Y(p_d, p_b) = \sigma y^*(p_d) + (1-\sigma)y^*(p_b) \tag{4}$$

where $\sigma \equiv F_\kappa(k)$ represents the fraction of attentive households. If we interpret (4) as the demand for a representative agent, σ can easily be recast as the probability of being attentive where κ is a stochastic information cost. Define the *change* in demand that results from a price change as $\Delta Y(p_d, p_b) = \sigma \Delta y^*(p_d) + (1-\sigma)\Delta y^*(p_b)$ where $\Delta y^*(p_s) \equiv y^*(p_s) - y^*(p_0)$ is the consumption change for a household of type $s \in \{b, d\}$.

To gain intuition, assume utility over y follows an isoelastic power form such that $u(y;\eta) \equiv (y^{1-1/\eta} - 1)/(1-1/\eta)$. Letting $\varepsilon \equiv \frac{\partial Y}{\partial p_d} \cdot \frac{p_d}{Y}$ denote the elasticity of consumption with respect to the dynamic price p_d, it follows that $\varepsilon = -\sigma\eta$. If the entire population is made up of attentive households, the aggregate demand elasticity identifies the underlying preference parameter since $\varepsilon = -\eta$. Figure 1 plots an example demand in the solid black line where all households are attentive. Attentive households consume $y_0 \equiv y^*(p_0)$ when the dynamic price is p_0 and $y_p \equiv y^*(p)$ when the price is p. However, if any fraction of households are inattentive such that $\sigma < 1$, identifying η must rely on additional model structure to recover the choice primitives. In the extreme, if all households are inattentive ($\sigma = 0$), aggregate demand is perfectly inelastic to the dynamic price level at level $y_b \equiv y^*(p_b)$, indicated by the dashed gray line in Figure 1. Note that the model still implies a change in aggregate consumption as long as the expected price of the bounded information set is not the original price ($p_b \neq p_0$). Figure 1 also plots a hypothetical demand function for a mixture of households ($0 < \sigma < 1$) in the dashed black line. For the mixture, demand slopes down slightly, but there is a discrete change from y_0 for dynamic prices in the neighborhood of p_0.

The model provides two sharp tests of whether there are inattentive households in a population with smooth preferences. First, a discrete shift in demand ($\Delta Y \neq 0$) for very small price changes should indicate households are using a coarse information set instead of the marginal price. Second, complete inelasticity with respect to the level of the price change would also be consistent with households being inattentive to the marginal price. The model does not explicitly consider the effect of automation on the decision setting, but can be extended to incorporate it.[19]

[19] We can think of automation as affecting two dimensions of the household's decision. First, it reduces the physical adjustment cost of effort for responding to a price change. For example, automating the response of an appliance that is not readily accessible such as an electric water heater or an entertainment console is likely to make the household more elastic. Second, the automation provides a substitute for attention, allowing for price conditional responses.

2.2 Empirical Elasticity Estimates

The model developed above has implications for the standard empirical estimation of the elasticity of demand. To see this, consider the approximation of the elasticity using a single price change and the aggregate change in consumption defined as:

$$\widehat{\varepsilon} \equiv \left(\frac{\Delta Y}{p_d - p_0}\right)\frac{p_0}{y_0} \approx -\eta\left(\sigma + (1-\sigma)\left(\frac{p_b - p_0}{p_d - p_0}\right)\right) \tag{5}$$

where I use the approximation that $\Delta y_s \approx -\eta\left(\frac{y_0(p_s - p_0)}{p_0}\right)$ for $s \in \{b, d\}$. For $\sigma = 1$, we can see that the empirical elasticity approximately recovers the taste parameter η. For $\sigma < 1$, the taste parameter is scaled by the potentially confounding term in parentheses. If $p_b \neq p_d$, we can see that $\widehat{\varepsilon} \neq -\eta$. In the simplified model above, p_b is the expected price. In this case, the model implies that the empirical elasticity will be larger (smaller) in magnitude than implied by the taste parameter if expectations are higher (lower) than the true price.

This highlights an important implication of the model. If there are any inattentive households, recovering the true preference parameter η is contingent upon knowing or making assumptions about the household's information set. While verifying households beliefs is an empirically burdensome exercise, it is important to note that p_d is a random variable in true dynamic pricing and this model. If p_d is a consistent price change with little uncertainty, there would be more support for assuming beliefs are accurate such that $p_b = p_d$. This is the case for many existing dynamic pricing programs, where price changes are of a consistent magnitude during select hours of the day. However, the structure of these programs do not reflect the uncertainty of the real-time pricing ideal.

2.3 Misoptimization Costs

I use the model to examine one implication of inattention: the misoptimization costs a household experiences by using bounded information. These costs represent the utility loss from reducing consumption for which the household has a high (low) willingness-to-pay (WTP) when the price is actually low (high), thus consuming more (less) than is optimal given the true price change. The misoptimization cost for the households can have direct implications for the efficiency of dynamic pricing when the price change p_d is equal to the marginal cost. In such a case, the mismatch of

While the company's automation is overly simple in this respect, it provides a default response we can interpret as responding as if an extreme price change occurred. These features can be incorporated explicitly through the utility function for adjustment costs and the information choice set.

consumption with marginal WTP also reflects an allocative inefficiency.

Quantifying the misoptimization requires me to make an assumption about the household's true preferences. Namely, I assume that households *experience* utility as though they had no information costs such that $\sigma = 1$. That is, preferences are revealed by their consumption level if they observed the actual price change. Under this assumption, the average utility misoptimization for given price change p is

$$a(p, p_b) \equiv \left| \int_{y^*(p)}^{Y(p,\, p_b)} p(y) - p \; dy \right| \tag{6}$$

where $p(y)$ is the inverse demand curve of $y^*(p)$. Visually, the integral in (6) corresponds to the area between the true demand curve and the dynamic price within the range of the observed quantity to the implied fully attentive quantity. Figure 1 shows this area shaded in gray. The order of integration is arbitrarily defined and I take the absolute value since any deviation from y^* is counted as a positive cost. Note that $a = 0$ for $p_b = p_d$ or if $\sigma = 1$ because either would imply $Y = y^*$.

3 Design and Data

This section provides a description of the experimental design and summary statistics.[20]

3.1 Recruitment

The experiment was implemented through the product of a private company that serves as a third-party demand response provider (DRP) in the state of California during the 2017 calendar year. The DRP offers a service where households who enroll are paid for reducing their electricity consumption during hour-long dynamic pricing events. The company also allows customers to automate their consumption during events to facilitate responsiveness by connecting their smart home devices via a web portal. Events and automated response are described in detail below. Enrolled households continue to receive a bill from their utility for their electricity consumption, but by enrolling in the program they have the opportunity to earn money from the DRP and reduce electricity expenditures overall. Households within the service territories of the three major

[20]The section below describes the complete design. At the time this draft was written, enrollment had been completed, but interventions had not concluded. I have collected intervention data through September 2017. The final paper will include data from October 2017- December 2017 and will represent approximately 90 percent of the final data.

California investor-owned electric utilities (IOUs) were eligible to enroll in the study.[21]

Recruitment was conducted by the DRP on an ongoing basis from January 1, 2017 to September 30, 2017. During this period, households were contacted through a variety of outreach channels and told they could earn up to $300 a year for reducing their electricity consumption.[22] Since households chose to enroll in the program, the study sample will not necessarily be representative of the population of eligible households. However, programs like these are unlikely to be mandatory in the near future, suggesting that volunteers are the relevant population. These factors should be noted when interpreting the external validity of the results.

A total of 13,782 households were recruited for the study. I define recruited households as those who created an account on the DRP's website by entering an email address and password. Of these, 6,169 households completed sign-up by providing enough information to connect their utility data so that the DRP could monitor their consumption and calculate payouts.[23] I remove households with erroneous meter data and those who appear to generate electricity on-site to arrive at the study sample of 5,531 households.[24]

3.2 Experimental Platform

A description of the DRP's product provides context that is useful for interpretation and assessment of construct validity. The DRP markets its product as a game where users can earn points during pricing events. Points are awarded with a linear incentive mechanism, where households gain points for using less electricity than the level the DRP forecasts and lose points for exceeding the forecast.[25] The reward (or loss) for an event is calculated as $Reward = Incentive * (Forecast -$

[21] The IOUs are Pacific Gas & Electric, Southern California Edison, and San Diego Gas & Electric.

[22] Recruitment channels included digital advertising, social media, in person outreach, referral bonuses for existing customers, and coverage on a national radio program. Households were also offered rewards for signing up that varied during the recruitment period from $20 to no reward. There were also periods during which the DRP used referral bonuses to existing customers which may have driven recruitment.

[23] According to the DRP, the 45 percent follow-through rate is consistent with what they normally see and can be attributed to the requirements of the enrollment pipeline. In order to enroll, a household is required to sign a series of forms after entering their email in order to connect their utility information and created an account. These forms pertain to disenrolling from any other DR programs run by their utility and entering into an exclusive contract with the DRP. Each IOU runs its own unique DR program and households were required to end participation in all other DR programs as a condition of enrolling in the study.

[24] I identify on-site generation as having a negative meter read during any period prior to or during enrollment.

[25] Forecasts are calculated for each household using the well-established "10 in 10" methodology defined by the California Independent System Operator as a weighted average of past consumption during similar hours of the day (CAISO, 2014). I verified the accuracy of the forecast methodology independently on pre-treatment data and the control group. The forecast produces estimates of consumption which are slightly negatively biased by less than 5 percent of consumption. While the susceptibility of these types of forecasting methodologies to gaming is often noted

$Usage$).[26] Points earned (lost) during an event are then added (subtracted) to a balance the household maintains with the DRP.

The reward mechanism functions financially equivalent to an increase in the per unit price of electricity due to the symmetry of the reward around the forecast except for one exception: the company does not allow negative point balances. Thus, the incentive to consume may be asymmetric around the baseline if the household has zero points in their balance. If the household is at the zero lower bound, the effective price change would be lower than the stated reward level. About 20 percent of households have zero balances during events, but the results are not driven by these consumers.[27] While the reward mechanism is certainly different from a simple price change, it provides the closest example of real-time variation in the marginal incentive to consume electricity of any experiment to-date.

Households were informed of an impending event with an hour or less notice via email and an SMS text message to their mobile phone. Figure 3 shows messaging for an example hour for both SMS and email. In both forms of contact households are told the specific period of the event, the incentive level, and the company's forecast for their usage. The incentive level was communicated as saliently as was possible within the constraints of the product infrastructure. 1 to 3 days after an event occurred, the household's performance was evaluated and communicated via email and the DRP's website. Upon logging in to their account, the resulting number of points gained or lost for the past four events was displayed and the household could scroll to view the complete history of event performance.[28]

Points could be electronically cashed out via a secure online payment system or donated to cause of their choosing. A household's point balance was shown on the upper right section of the home page. One point was equivalent to \$0.01 when cashed out and households could only cash out after they had \$10 worth of points in their balance. The mapping between points and dollars is

in the literature, it often pertains to situations where the user has ample time to change usage prior to an event such as with day-ahead notice. The hour-ahead notice for the events studied in this paper plus their unpredictability make it less likely gaming is an issue.

[26] For example, if the incentive level is 50 points per kWh, the household's forecast is 1.5kWh, and they use 1kWh, they will earn 25 points for the event. Conversely, if they use 1.7kWh, they will lose 10 points.

[27] I provide evidence in the Appendix that the main findings are not driven by the zero balance lower bound. The majority of households start with a positive balance and within a few weeks of enrollment less than 30 percent of households have a zero balance. The Appendix also provides information on how the point balances evolve over the course of the program.

[28] The Appendix shows a screenshot of the home page.

important since it allows me to interpret responses to variation in price as representative of demand for electricity. I provide evidence that households understood the value of the points in Section 6.

The automation service the company provides remotely shuts off connected devices during a pricing event and turns them back on when the event is over. For example, if an appliance is plugged into a connected smart plug, the company will remotely break the circuit between the plug and the appliance so no more load can be drawn from the socket. With connected smart thermostats, the service turns the thermostat off so that the central air-conditioning (AC) or heating does not cycle. This automates the decision to reduce consumption for connected appliances. Households can override the automation by turning their thermostat or plug back on manually or remotely if they have connected their smart device to their mobile phone. The process for connecting a smart device involves going on the company's website and entering appliance information. Importantly, the automation service is not a function of the price level. That is, households cannot set a trigger price above which they would like to be automated. This has implications for the interpretation of results I discuss in Section 6.

It is also important to note that enrolled households had the option to opt-out of certain times of the week they did not want to receive event periods. Specifically, a household could choose via the web portal which hours of the day they wanted to receive event hours for weekdays and weekends separately. For example a household could choose to receive events from 5PM-8PM on weekdays and 12PM-6PM on weekends. Households were defaulted to receive messages between 7AM-10PM for both weekdays and weekends and the majority receive events during all possible hours for this experiment (11AM-10PM). I address this event-level noncompliance directly in the empirical analysis presented in Section 4.

3.3 Treatments

Upon recruitment, households were randomized into three treatment groups: Control (C), Standard (S), and Encouraged (E). Each treatment was exposed to a unique product experience for the first 90 days of enrollment as follows:

- **Control (C):** 1,035 households were informed that their enrollment would be delayed for 90 days. These households did not receive any event messages or other contact during this time.
- **Standard (S):** 2,271 households received *price treatments* and were allowed to connect their smart devices to automate their response.

- **Encouraged (E):** 2,225 households received the same *price treatments* as the Standard group, but also received an *automation encouragement* in the form of a rebate for the full cost of purchasing a smart home device connecting it to the automation service.

Households were probabilistically assigned to C/S/E with a ratio of 20/40/40 percent. The assignment ratios were calculated to maximize the statistical power for comparing Standard to Encouraged, while ensuring there were enough Control households to have a minimum detectable effect of 1−2 percent between pricing levels. The product experience randomization occurred at the time of sign-up to ensure each day's enrollment cohort had comparable households in each treatment arm. This makes the internal validity of the study robust to changes in the recruitment strategy that took place during the study.

Control Group – Control households were granted access to the web-portal, but it did not display any information about their usage and they did not receive any event messaging. Instead, upon enrollment they received an email with the following language: *"Due to overwhelming demand for our service, there will be a delay before we can send you #OhmHours. We estimate this delay will last approximately 3 months. In return for your patience, we'll issue you an extra $10 bonus when your account delay is over."* The extra bonus was meant to combat sample attrition. There is evidence of a small amount of attrition in the control group between the point of recruitment when households enter an email and are assigned and when they connect their utility data. While this poses a potential threat to identification, I show below that the sample is balanced across treatment assignment for a large set of observable characteristics.[29]

Automation Encouragement – In order to measure the causal effect of adopting an automation technology the Encouraged households were offered a rebate for the full purchase price of a new smart home device up to $240 in value. Upon creating an account, these households were shown a pop-up notification on the web-portal in addition to being sent an email notifying them they had been selected to receive a rebate for purchasing a new smart device. The household was offered a choice between 3 smart thermostats ranging in retail value from $198 to $240 or one package of

[29]Random assignment happened prior to households connecting their utility account due to technical constraints within the DRP's product infrastructure. The probability of connecting utility data was 41.6 percent for control households as compared with 45.5 percent in the treatment groups, indicating a small amount of attrition in the control group. The difference is statistically significant and should be noted when interpreting the results. I report the regressions estimating attrition at the two stages of data construction in the Appendix. The results show no difference in attrition between the point of connecting utility data and having appropriate data to be in the study sample. Further, the sample is balanced in the electricity usage and temperature data used in the empirical analysis.

two smart plugs which cost $80.[30]

Price Treatments – In order to measure the effect of the price level households were exposed to *price treatments* where the financial incentive to consume was randomly varied. During the first 90 days, events were called 1-3 times per week and households were notified with one hour ahead notice of an increase in the effective price of electricity consumption via SMS and email. The SMS message said *"Reduce consumption today from 6PM-7PM! We'll award you 100 points for every kWh you reduce below your forecast (1.35 kWh), but you'll lose points if you go over."* The email contained the same information plus additional general strategies on how to reduce consumption. For each event, the incentive level was randomly assigned within household to one of five incentive levels: 5, 25, 50, 100, and 300 points per kWh with equal probability. The only variation between message content was the event period, the price level, and the forecast.

Moral Suasion Treatments – To further investigate the degree to which message content affected responses, households were also exposed to *moral suasion treatments* instead of a financial incentive during events.[31] An additional 90 days after the completion of the *price treatments*, households in all both treatment groups were pooled.[32] Within each event, households were randomly assigned to one of four groups with equal probability. Three treatment groups received events with different messaging and a fourth served as a control. Among the treated, households were assigned to one of the following messages:

- **Moral Suasion Only:** Messages reminded households that reducing energy consumption could improve environmental outcomes instead of offering a financial incentive. An example SMS message was *"Environmental event today from 6PM-7PM! Saving energy now could keep a dirty power plant turned off!"*
- **Price + Moral Suasion:** Messages had the same financial incentive as *price only* plus language from *moral suasion only*. An example SMS message was *"Pricing event today from 6PM-7PM! Get 100 points per kWh saved below your forecast (1.35 kWh). Saving energy now could keep a dirty power plant turned off!"*
- **Price Only:** Messages used the same language as the *price treatments* with an incentive level of 100 points ($1) per kWh.

[30]The households were told that they would have the purchase price equivalent of points added to their balance when they connected the device as to ensure rebates encouraged automation.

[31]Ideally, I could have tested the effect of presenting the same language as the *price treatments* with a zero price, but this was deemed to be too confusing by the DRP. The product generally features messaging on the environmental benefit of reducing, so it is possible that moral suasion is already driving consumption decisions to an extent.

[32]The 90 days between the completion of the *price treatments* and the start of the *moral suasion treatments* featured a different, cross-randomized experimental intervention. Data from days 91-180 are not included in the study.

Households assigned to the control group received no message or financial incentive for the hour.

Survey – The study also utilized a voluntary survey to gain further insight into the mechanisms driving the household decision. Two weeks after the *moral suasion treatments*, households were contacted via email with a link to the survey website and offered $10 to provide feedback on the product.[33] As of November 9, 2017, 2,250 households had been contacted with the offer to complete the survey. 587 households completed the survey—a 26 percent response rate.

3.4 Data and Summary Statistics

Experimental Data – The DRP was the the primary data source for the study. The company receives Advanced Metering Infrastructure (AMI) data for its customers from their respective utilities that allows me to observe each household's hourly electricity consumption during the study and up to a year prior to enrollment.[34] The company provided me with household-specific information on the experimental assignment, the time and content of messages sent to households, the type and number of devices connected to the automation service, and the financial outcome of each event. I also observe each household's 5-digit ZIP code and census block group which I use to merge in hourly temperature data and demographics on the area where the household is located.

Weather Data – I use outdoor temperature data for the centroid of each ZIP code in my sample from the Dark Sky API which combines data from multiple government sources and uses a meteorological model to predict weather for a given geocode.[35]

Census Data – I observe the Census Block and Block Group for 84 percent of households.[36] I use household which I use to merge in demographics on the location of the household from the 2015 American Community Survey 5-year estimates.

Balance and Summary Statistics – Using the above data sources I construct a panel dataset where the unit of observation is a household by hour-of-sample. The company scrapes historical data on usage that allows me to observe household consumption prior to enrollment in the program.

[33] A complete list of survey questions is provided in the Appendix along with summary statistics comparing the responders to the full sample for interpretation.

[34] This data is used to settle the company's wholesale market activity and capacity contracts so the company and the external counterparties independently verify the accuracy of the data. As a further precaution, the CEO of the company has signed a memorandum of understanding that the data has not been tampered with and the design was executed consistent with the research design described here.

[35] Information on the sources used in the Dark Sky API model are summarized here http://darksky.net/dev/docs/sources

[36] I was unable to obtain the census geographic identifiers for the outstanding 16 percent.

Not all households have the same amount of data available due to movers and the creation of new accounts so the panel is unbalanced.

The randomization was performed within the DRP's product infrastructure and assignment could not be feasibly stratified. I test for balance in observables characteristics between treatment groups to provide evidence on the validity of assignment. Table 1 reports summary statistics by treatment group. Column (1), (2), and (3) report means and standard deviations in parentheses for the Control, Standard, and Encouraged groups calculated using pre-enrollment data. Columns (4)-(6) report p-values on the *t*-test on the difference in means permuted between each of the treatment groups.[37] Rows 1−6 report statistics for the data used in the empirical analysis and row 7 and above for the census variables, where available. Only one comparison is significantly different at the 10 percent significance level: the maximum consumption between the Encouraged and the Control group. This provides strong evidence that the assignment was random.

Figure 2 provides further evidence of balance by showing a map of where the households in the study are located in the state of California. Regions shaded green have both Enrolled (assigned to Standard or Encouraged group) and Control households. Regions with only Enrolled are shaded blue and regions with only Control are shaded orange. The map shows that there is considerable coverage over populous regions of the state and that there does not appear to be any correlation between region and assignment. The significant geographic area covered by the sample also provides rich spatial variation not available in previous dynamic pricing studies. However, it should be noted that the sample is observably different from the eligible population which likely reflects the voluntary selection into the experiment.[38]

4 Empirical Strategy and Results

I present the empirical results of the *price* and *moral suasion treatments* in this section. Before proceeding with the statistical analysis, I report summary statistics on the events to assist in interpretation. There were a total of 94 events called between January 1, 2017 and November 5, 2017. The average number of events called per household during the first 90 days was 27 with a standard deviation of 4. The minimum number of messages sent was 7 and the maximum was 30. Table 2 reports summary statistics for the events by hour-of-day. Column (1) reports the number

[37] Standard errors for the balance tests are assumed to be *iid* between households.

[38] I report more complete summary statistics on the sample and the eligible population in the Appendix.

of household-hour observations. Column (2) shows that the majority of events were called between 1pm and 8pm[39] Column (3) shows the proportion of households called during any given event was 0.89. I address this imperfect compliance in my analysis below. Since I did not have explicit control over price randomization, I verify that the average incentive in each event is consistent with the expected value of \$0.96/kWh.[40] Column (6) and (7) show the mean consumption for the control and treatment groups, respectively. From this we can see the enrolled households consumed less electricity than the control households using the raw experimental variation.[41]

4.1 Effect of Pricing Events

Pricing events reduce consumption – As a first exercise, I test whether household consumption responds to pricing events. I separate the effect of enrollment into event hours and non-event hours by estimating the following difference-in-differences (DD) regression on the unbalanced panel of hourly household electricity consumption

$$Y_{it} = \delta D_{it} \times H_t + \xi D_{it} \times N_t + X_{it}'\beta + \varepsilon_{it}. \tag{7}$$

The dependent variable is hourly electricity consumption in log(kWh) for household i in hour-of-sample t. I report results in log terms to assist in interpretation.[42] D_{it} is an indicator variable that household i had enrolled in the program during hour t. H_t is an indicator that an event was called during hour t and N_t is a complementary indicator that an event was *not* called during hour t. The DD specification in (7) is not necessary for identification, but I include a vector of controls X_{it} to improve the precision of the estimates. These include household by hour-of-day fixed effects to remove time-invariant household factors for each hour of the day, hour-of-sample fixed effects to remove factors that effect the entire sample each period, and a flexible function to control for outdoor temperature at household i's location in period t.[43] ε_{it} is the structural error term.

The primary coefficient of interest in (7) is δ and captures the average treatment effect (ATE)

[39]These are the hours often associated with peak capacity and the ramping challenges of high renewables penetration.

[40]The unweighted average is calculated as $(0.05 + 0.25 + 0.5 + 1 + 3)/5 = 0.96$.

[41]I provide further information on the raw experimental variation by plotting the empirical CDFs of enrolled and control households in the Appendix.

[42]Results in terms of kWh are reported in the Appendix and are qualitatively similar.

[43]In my preferred specification I parametrically control for linear dependence on cooling-degree hours (CDH) and heating-degree hours (HDH) separately. CDHs and HDHs are defined as the deviations in outdoor temperature above and below 20°C, respectively. Estimating the model with a quadratic control function in outdoor temperature does not change the results.

of being enrolled in the program during a pricing event on electricity consumption. Since there are multiple pricing events called while a household is enrolled, the effect averages variation *within* and *between* households. Another coefficient of interest is ξ, which captures the ATE of being enrolled on consumption during all non-event hours. I interpret estimates of δ and ξ causally because they are identified by comparing enrolled households to the pure control households that randomly experience a delay. Formally, I assume $\mathrm{E}[\varepsilon_{it}|D_{it}] = 0$ because enrollment is randomly assigned.

While the price level was randomly assigned among households that received messages, not all enrolled households were called each time a pricing event occurred. On average, about 11 percent of households were not called each event. Event noncompliance was driven by a household's decision to opt-out of certain times of the week they did not want to receive events. This poses an identification challenge because I cannot observe who would have opted-out in the control group. Importantly, households decide to opt-out before they are called with an event so selection is not driven by the price level.

The event noncompliance means δ in (7) can be thought of as the average intent-to-treat (ITT) effect where treatment is defined as receiving a pricing event. While the ITT is important for the overall evaluation of the program, I also estimate the local average treatment effect (LATE) for households who received messages to characterize the behavioral response to pricing events.[44] In order to estimate the LATE, I modify (7) and estimate the following regression:

$$Y_{it} = \eta C_{it} \times H_t + \xi D_{it} \times N_t + X_{it}'\beta + \varepsilon_{it} \tag{8}$$

where C_{it} is an indicator variable that household i was contacted with a change in marginal price during period t. Estimates of η obtained using OLS cannot be interpreted causally due to the selection bias potentially introduced by households opting-out of certain event time periods. To address the identification issue, I estimate (8) using 2SLS and instrument the first term $C_{it} \times H_t$ with the randomly assigned enrollment indicator D_{it}. The resulting estimate of η captures the local average treatment effect (LATE) of receiving a pricing event on consumption for the households who do not opt-out. Interpreting the LATE causally relies on the same identifying assumption that $\mathrm{E}[\varepsilon_{it}|D_{it}] = 0$ so that enrollment only affects behavior during events through the calling of pricing events.[45]

[44] Note this is a different LATE than for the automation compliers, which I discuss below.

[45] Identification also relies on a significant first stage (indicated by the Panel B of Table 3), monotonicity of the

Table 3 reports estimates of the ITT and LATE of pricing events on log electricity consumption obtained from estimating (7) and (8), respectively. Each column reports coefficient estimates from separate regressions which include fixed effects and controls. Standard errors are reported in parentheses and clustered by household and hour-of-sample to account for arbitrary correlated errors within household and across sample within each time period.[46] For the remainder of Section 4, standard errors and confidence intervals are estimated using this structure unless otherwise noted. Column (1) reports the estimates of the ITT and columns (2) and (3) report the estimates of the LATE using OLS and 2SLS, respectively.[47]

The results in column (1) of Table 3 indicate average consumption falls by 0.111 log points (10.5 percent) during pricing events as a result of enrolling in the program. The estimate for the non-event periods is reported in the third row and indicates enrollment reduces electricity consumption by 0.004 log points (0.5 percent), but is not significantly different from zero. Column (3) shows calling households with an event reduced consumption by 0.124 log points (11.7 percent) and column (2) shows the LATE is not substantively different from the OLS estimation. This is the first main result of the paper. The LATE is slightly larger than the ITT which follows from the intuition that the ITT is attenuated by households whose consumption was closer to the controls because they did not receive an event. Panel B reports the coefficient on the instrument from the first-stage for the regression in column (6), which confirms the noncompliance rate of 11 percent.

Event responses are precise with no substantial spillovers – I offer graphical evidence of the consumption behavior around an event by using an event-study specification to characterize changes in energy consumption in the 8 hours leading up to and following a pricing event. Specifically, I estimate the following regression:

$$Y_{it} = \sum_{\tau=-8}^{8} \delta_\tau D_{it} \times \mathbf{1}(H_{t+\tau} = 1) + \xi D_{it} \times \mathbf{1}(t \notin \mathcal{H}) + X_{it}'\beta + \varepsilon_{it}. \tag{9}$$

instrument, and SUTVA to hold.

[46]The first dimension of clustering are robust to arbitrary correlated errors within households. This is meant to account for serially dependent errors hourly electricity consumption. The second dimension of clustering are robust to arbitrary correlation within an hour-of-sample. This is meant to account for correlated weather shocks in time across the region due to weather or grid operations. The results are not qualitatively affected by clustering at the day-of-sample level, which is significantly more conservative.

[47]In the Appendix I show how adding controls, fixed effects, and pre-enrollment data change the point estimates in the Appendix. The results are qualitatively similar for all regressions and the DD specification is preferred because it controls for time-invariant characteristics of households. These may be systematically different between treatment groups by chance as I was unable to stratify assignment. The DD specification is more robust to these concerns than the raw experimental variation.

The coefficients of interest are the δ_τ's that capture the average change in electricity consumption caused by enrolling in the program in the period τ hours relative to an event. I also include an indicator for all post-enrollment periods that do not lie within $\mathcal{H}$, the set of the sample periods within an 8 hour neighborhood of any event.

Figure 4a plots the results from estimating (9) using OLS with log consumption as the dependent variable. The event period is shaded in dark gray. The plot shows point estimates for δ_τ using the period two hours prior to an event (-2) as the reference category. Households are notified of an event during the hour before an event (period -1) which is shaded light gray. The vertical bars plot 95 percent confidence intervals.

The results show that being enrolled in the program during pricing event period causes electricity consumption to decrease by 0.098 log points (9.3 percent) relative to the period two hours before an event. Further, there are no distinguishable pre-trends in consumption and the reversion to the pre-event consumption level happens quickly. By the second hour following the event, electricity consumption is statistically indistinguishable from pre-event levels. This is notable because there is a symmetric price change in the periods after the event that provide a second test of household's responsiveness to price changes. This provides evidence that, on average, the *price treatments* caused households to reduce their electricity consumption.

Households reduce with and without automation – Since automation is a feature of the setting, I estimate effect heterogeneity by whether households chose to automate. These estimates provide non-causal information on the degree to which automated versus manual decisions explain the results. The comparison is non-causal because households who choose to adopt the automation technology are likely to be different than those who do not in ways the econometrician cannot observe. For instance, they may engage in additional conservation behaviors during pricing periods if they are motivated by environmental concerns or they may have different preferences over appliance usage.

To investigate heterogeneity, I estimate the same event-study specification as (9) using OLS except I interact the enrollment indicator an indicator i household was automated in period t, denoted A_{it}. The variable is constant post-enrollment and zero in the pre-enrollment period for all households. Figure 4b plots the results from estimating the effect of enrollment event-study and decomposing the effect for automated versus non-automated households. I estimate the coefficients for both groups jointly and omit the period two hours prior to the event for non-automated households as the reference category. Point estimates for automated and non-automated households are plotted with "o" and "x" markers, respectively.

Figure 4b shows automated households reduce consumption significantly more during pricing periods. The point estimate for automation is 0.267 log points (23.4 percent) lower consumption relative to the two hours prior for non-automated households and 0.273 log points (23.9 percent) lower relative to non-automated households during pricing events. The consumption for non-automated households is 0.085 log points (8.1 percent) lower than pre-event consumption. The results also indicate that there are no significant pre-event differences in trends between automated and non-automated households.

Two important results are worth noting. First, consistent with expectations, households who choose to automate their decisions are significantly more responsive than those who do not. Second, the non-automated households still respond significantly to pricing events. Only 23 percent of the of the ITT effect is attributable to automation, suggesting households are making active decisions to reduce consumption during pricing periods.[48] Automation contributes only a small amount to the effect of enrollment because only 7 percent of all enrolled households adopt the technology.

4.2 Effect of Marginal Price

Responses are very insensitive to the marginal incentive – The estimate of η in (8) pools the effect of receiving a message with any positive price level. Define ΔMP_{it} as the change in marginal price household i would receive in period t regardless of whether they were called or not.[49] To recover the effect of the level of the marginal price change on energy consumption, I modify (8) by interacting C_{it} with a vector of indicators for each price level and estimate the regression

$$Y_{it} = \sum_{s \in \mathcal{S}} \eta_s C_{it} \times \Delta MP_{it}^s \times H_t + \xi D_{it} \times N_t + X_{it}' \beta + \varepsilon_{it} \qquad (10)$$

[48] I come to this number by subtracting the non-automated component from the enrollment effect and dividing it by the total effect: $(0.111 - 0.085)/0.111 = 0.23$.

[49] There is a subtle technical point here regarding the generation of the ΔMP_{it} variable. Households were not assigned a price level by the DRP until they were called, so the raw experimental data does not have a price level for households who were not called. To populate these, I randomly assign a counterfactual marginal price to the opt-out households consistent with the design. This can be thought of as randomly assigning prices for all future events at the time of enrollment, but not informing the household. Households decide generally whether to opt out of the program during certain hours, but since they do not observe the price assignments, the opt-out decision is independent of the price level of a given event.

where $\Delta MP_{it}^s \equiv \mathbf{1}(\Delta MP_{it} = s)$ and $\mathcal{S} \equiv \{0.05, 0.25, 0.5, 1, 3\}$ is the set of price levels. To deal with the selection issue, I estimate (10) using 2SLS and instrument each interaction term $C_{it} \times \Delta MP_{it}^s \times H_t$ with $D_{it} \times \Delta MP_{it}^s \times H_t$, the randomly assigned enrollment indicator interacted with the randomly assigned price level indicator. Again the key identifying assumption is that the enrollment only affects behavior through the calling of pricing events during those periods. The coefficients of interest in (10) are $\{\eta_s\}_{s \in \mathcal{S}}$ which recover the TOT of receiving a message with marginal price s in points per kWh on electricity consumption for the called households.

Figure 5a plots estimates of (10) using the 2SLS estimator described in the previous paragraph and log(kWh) consumed as the dependent variable. The vertical axis represents the change in consumption relative to the control group in log points and the horizontal axis represents the price change in \$ per kWh equivalent and ranges from \$0-\$3/kWh. The leftmost point estimate indicates calling an event with a \$0.05/kWh effective price increase caused an average reduction in consumption of 0.113 log points (10.7 percent). The rightmost point estimate indicates an event with a \$3/kWh effective price increase caused an average reduction of 0.138 log points (12.9 percent). Vertical bars plot 95 percent confidence intervals.

I can confidently reject the null that the effects for the smallest and largest incentives are equal ($\eta_{0.05} = \eta_3$) because the p-value is less than 0.01. However, the change in effect size is extremely small given the difference in incentive levels. The results indicate a 60-fold increase in the price increase yields 2.2 percentage points lower consumption, an increase in the size of the treatment effect of only 22 percent. This is remarkable since the average utility price for households is approximately \$0.16/kWh during non-event periods. This is the second main result of the paper.

The dashed line in Figure 5a plots the results of a parametric estimation of the price responsiveness with an intercept for any event being called and a slope for the incentive level within an event.[50] The slope can be interpreted as indicating a \$1/kWh increase in the level of price change causes an additional 0.008 log points (0.008 percentage points) reduction in electricity consumption. The estimate is statistically significant from zero with a p-value less than 0.01. The 95 percent

[50] Specifically I estimate the regression

$$Y_{it} = \eta^{int} C_{it} \times \mathbf{1}(\Delta MP_{it} > 0) \times H_t + \eta^{slope} C_{it} \times \Delta MP_{it} \times H_t + \xi D_{it} \times N_t + X_{it}'\beta + \varepsilon_{it}$$

with 2SLS and instrument the first two terms with $D_{it} \times \mathbf{1}(\Delta MP_{it} > 0) \times H_t$ and $D_{it} \times \Delta MP_{it} \times H_t$, respectively. The dashed line in Figure 5a plots $\widehat{Y}_{it} = \hat{\eta}^{int} + \hat{\eta}^{slope} \Delta MP$ using the estimated parameters.

upper confidence interval indicates we can reject slopes more negative than -0.014.

Figure 5b shows the effect of marginal price decomposed by automation status. Automated and non-automated households are plotted with "o" and "x" markers, respectively. I also plot the parametric estimation of the price responsiveness within each group. The differences cannot be interpreted causally, but the figure provides evidence that the price insensitivity is not driven mechanically by the fact that the automation service does not depend on price. In fact, automated households appear to be marginally more price responsive than non-automated households, but this could be driven by selection.

Households respond less to messages without a price change – To investigate the degree to which the financial incentive in messages drives the results, households were randomly assigned messages with no price change during the *moral suasion treatments*. The interventions happened 90 days after households completed the *price treatments* (180 days after enrollment). Thus, the sample for these interventions is composed of 1,122 households who signed up early enough in the program to reach 180 days enrolled. The sample for these interventions may not be representative of the full sample because recruitment in time was not random, so the *moral suasion treatments* included messages identical to the *price treatments* to benchmark the results. The benchmark results for messages with price are generally consistent across the two treatments, suggesting the results are not substantially confounded with selection in time of recruitment.

To recover the effect of the message content on electricity consumption I estimate the model

$$Y_{it} = \delta_M \, C_{it}^{Moral} + \delta_P C_{it}^{Price} + \delta_B C_{it}^{Both} + X_{it}'\beta + \varepsilon_{it} \quad (11)$$

where C_{it}^{Moral}, C_{it}^{Price}, and C_{it}^{Both} are indicator variables for whether the household received a message with moral suasion and no price change, a standard message with the price change, and a message with both, respectively. I use the same pre-enrollment data as the *price treatments*, but limit the post-enrollment data to periods when a *moral suasion treatment* event occurred. The coefficient δ_M captures the ATE of receiving a message with moral suasion and no financial incentive relative to receiving no message. δ_P captures the ATE of receiving a message with the same language as the *price treatments* and an incentive level of \$1/kWh. δ_B captures the ATE of receiving a message with moral suasion and an incentive level of \$1/kWh. I include the same controls as the *price treatments* estimation.

Table 4 reports the results from estimating (11) with log(kWh) as the dependent variable in column (1). The first coefficient of the second column indicates that sending a message with environmental priming language and no price change causes households to reduce consumption by 0.033 log points (3.2 percent) relative to being enrolled and receiving no event, but the result is not statistically significant. The second coefficient indicates that sending a message with a price incentive of \$1/kWh causes households reduce consumption by 0.077 log points (7.4 percent). The third coefficient indicates a message with a price incentive of \$1/kWh that also includes moral suasion causes households to reduce consumption by 0.094 log points (9.0 percent). We can confidently reject the null that the messages with price are significantly different from zero at the 1 percent level.

Column (2) of Table 4 breaks out the effect by households with and without automation. The coefficients can be interpreted as the causal effect of sending each message type on consumption for automated and non-automated, separately. Comparing automated to non-automated responses cannot be interpreted causally and the encouragement instrument does not have enough statistical power with the smaller sample. Still the results confirm intuitive patterns. Households that made decisions manually responded differently to price as compared with moral suasion. Panel B reports the p-value for the hypothesis test moral suasion and price alone have the same effect is equal to 0.002 or 0.006 for pooled and non-automated households. Automated households on the other hand significant effects for all three message types. This provides evidence in favor of that price is driving the impact on non-automated consumption rather than moral suasion.

4.3 Effect of Automation

Adopting automation causes larger reductions – To estimate the causal effect of adopting the automation technology on household energy consumption I use a randomized encouragement design within the subsample of enrolled households. Specifically, I randomly select half of enrolled households to receive rebate to take-up the automation technology. The encouragement provides an instrument to identify the causal effect of adopting automation.

Consider a model that only uses enrolled households. Using 2SLS, I estimate the regression

$$Y_{it} = \alpha_H A_{it} \times H_t + \alpha_N A_{it} \times N_t + X'_{it}\beta + \varepsilon_{it} \tag{12}$$

instrumenting A_{it} with Z_{it}, an indicator that household i received the encouragement offer prior to period t.[51] The coefficient of interests is α_H, which captures the LATE of automation on consumption during events for the compliers nudged into adopting by the encouragement. The coefficient α_N captures the LATE of automation on consumption during non-events, which may be affected by spillovers. Importantly, the effects are additional to the level of being enrolled without automation, but can be interpreted causally if the instrument is valid.[52]

Table 5 reports the summary statistics for the number and type of connected automation devices in the Standard enrolled and Encouraged groups. Panel A reports counts for the number and types of devices connected in each group. The results show that 90 more households connected at least one device in the Encouraged group and that they connected 187 more devices. The encouragement rebate was offered for thermostats and smart plugs, which both increased significantly in number in the Encouraged group. The Standard group has more electric vehicles and home automation systems connected, but these technologies were not eligible for the rebate and overall represent a trivial share of the sample.

Panel B reports the proportion of households who took up at least one device by the treatment group. Uptake is 8.9 percent in the Encouraged group and 4.9 percent in the Standard group. The difference is statistically significant with a p-value that is less than 0.0001. This shows the encouragement caused higher adoption in the automation technology. Panel B also reports uptake broken out by pre-enrollment consumption quartile to provide information on the type of household taking up automation. Column (1) shows that households with the lowest consumption are less likely to connect to the automation service when they are not offered the rebate. Offering the rebate increases take-up for consumers by 6.3 percentage points for the bottom quartile as compared with 2.8 percentage points in the highest quartile. This is important for interpreting the LATE since it

[51] Since households receive the encouragement upon enrollment, $Z_{it} = D_{it}$ if $Z_{it} = 1$.

[52] Specifically, the effect can be interpreted causally if four assumptions hold. First, the encouragement must affect uptake of the automation technology, which I verify below. Second, the encouragement must not affect consumption in any way except through the automation technology. I cannot formally test this assumption's validity and if the offer of a rebate caused households to change their investments in other electricity consuming durables, this may be a concern. This omitted variation could lead to bias in the estimates of the LATE and should be noted when interpreting results. The sign of this bias is uncertain. For example, if the rebate offer causes the household to invest in other smart home appliances, the overall efficiency of the home could increase if new appliances are more energy efficient than the older ones. Conversely, the new appliances could have more features and consume more energy or the new appliance could lead to higher utilization rates. Third, the stable treatment unit value assumption (SUTVA) must hold. This may be violated if social interactions cause spillovers between treatment and control, but seems unlikely given the low uptake overall. Fourth, the encouragement effect on uptake must be monotonic such that there are no "defiers" who would adopt automation, but choose not to as a result of the rebate.

is evidence that the always-takers are observably different from the compliers.

Table 6 reports the results for estimating (12) with log electricity consumption as the dependent variable using only enrolled households. Column (1) reports the OLS estimates of the average effect of automation on post-enrollment consumption broken out by event and non-event periods. The results show estimates of the difference between automated and non-automated households consistent with the difference shown in Figure 4b. Column (2) reports the reduced form specification, regressing log consumption on the encouragement instrument directly, interacted with the even-period indicators. Column (3) reports the IV estimation. The first stage is reported in Panel B with both regressions collapsed to a single column. I report the coefficients for the first-stage and instrument corresponding to the endogenous variable by event and non-event period. The other coefficients are statistically insignificant from zero. Figure 4c plots the point estimates from the analogous event-study specification, where the period two hours prior to the event is the omitted category.

The results show that the causal effect of automation on price response is substantial. The first coefficient in column (3) indicates automation causes 0.829 log points (56.4 percent) lower consumption during pricing events relative to the non-automated counterfactual for compliers. I cannot identify the consumption of the compliers in the Standard group with the experimental design, but if we use the price event ITT as a benchmark, the results imply 5 to 7−fold increase in responsiveness.[53] These results are dramatic, but it is worth noting that the take-up due to the rebate was disproportionately driven by households with smaller consumption. This is the third main result of the paper. Further, the fact that the OLS estimate of the difference is smaller suggests that the compliers reduce more than always-takers. This suggests that within the range of adoption explored here, 4 to 9 percent, the marginal treatment effect is increasing.

4.4 Persistence

This section reports results showing how the effect of pricing events changes over time. Household's understanding of the program and the technologies may change as they learn new strategies to reduce or make adaptive longer-term behaviors. In order to characterize the effect over time, I examine the *price treatments* for each 30 day period of their 90 day duration. I use the parametric

[53] If we use the pooled result across all automated non-automated, the increase is from 12 percentage points and if we only use non-automated households, the increase is from 8 percentage points.

specification of the response to marginal price shown in Figure 5 to see if households become more or less price elastic as they become more familiar with the product.

Figure 6 plots estimates for how the effects change over time during the *price treatments*. The estimates are broken out by 30 day periods during the 90 days for which *price treatments* occurred. Estimates for days 0-30, 31-60, and 61-90 are represented by triangles, squares, and diamonds, respectively and circles indicate estimates for the full 90 days for reference. Vertical bars show 95 percent confidence intervals. Figure 6a shows the causal estimates of the event price response for all enrolled households. Figure 6b plots the non-causal effect heterogeneity for households with and without automation in blue and black, respectively. Each panel shows estimates from a single regression. Estimates labeled "Event Intercept" and "Event Slope" represent the parametric estimation of the price response of consumption for called households.

The left set of estimates Figure 6a shows the intercept decreases slightly from -0.120 log points to -0.107 log points, but the estimates are not statistically distinguishable at conventional levels. The right estimates show the slope estimate does not change substantially. Figure 6b shows that the results from 6a are generally explained by households without automation, suggesting there does not appear to be significant learning or habituation during the 90 day period.[54]

5 Model Application

The salient presentation of a random price change provides the rare opportunity to estimate the elasticity of household electricity demand using experimental price variation. However, the constant elasticity form commonly used by the literature does not seem wholly appropriate given the insensitivity to change size shown in the empirical results. As a result, I adapt the theoretical model developed in Section 2 to the experimental setting to make it empirically tractable and demonstrate the additional information necessary to recover structural demand parameters. The model application illustrates how adding attention to the model affects elasticity estimates. It also allows me to quantify the potential costs of inattention which gives a measure of the degree to which it impacts household welfare.

[54]The estimates for the automated households show the price response intercept increases significantly from days 0-30 to days 31-60, but then levels off. This is likely due to the fact that there was a lag between enrollment, purchase, delivery, and installation of the automation technologies. Further the slope for the automated households does not follow a clear pattern and while there does appear that spillovers for automated households are gradually more negative, the effects are statistically insignificant.

5.1 Empirical Specification

I make several simplifying assumptions in order to make the model developed in Section 2 empirically tractable and relevant to the literature. First, I assume attentive consumers exhibit preferences for electricity consistent with having a constant price elasticity. Second, I assume a household in any given event can be characterized by the two types introduced in Section 2: attentive and inattentive.[55] Third, I assume the taste parameter η is uncorrelated with the information costs that determine σ.[56] Fourth, I allow for an additive social preferences component to utility to control for the effect of moral suasion explicitly in the model.[57]

I model a household i's consumption in period t as

$$\log(Y_{it}) \quad = \quad \eta\,(\sigma \log(MP_{it}) + (1-\sigma)\log(HP)) + \gamma G_{it} + \alpha_i + \varepsilon_{it} \tag{13}$$

where MP_{it} is the marginal price the household faces in period t, G_{it} is an indicator that the household factored moral suasion into their decision to consume, and ε_{it} is a stochastic taste shock. I also allow for a household specific taste parameter α_i which reflects time-invariant differences in tastes.

Consumption in (13) is governed by four choice primitives: η, σ, HP, and γ. η captures the elasticity of demand with respect to a fully attentive household. σ governs the attention of the household to the marginal price change. Given that I am averaging over households and events, the model follows the representative agent formulation where σ can be interpreted as the probability of being attentive during an event or the average fraction of attentive households. HP governs the heuristic price response of a household with a bounded information set, which is assumed to be constant for all event periods. γ governs a demand shift due to social preferences.

In order to jointly estimate all model parameters, I pool data from the *price treatments*, the *moral suasion treatments* and pre-enrollment consumption and drop post-enrollment non-event

[55] Since I average over variation within households, this does not assume household type is time-invariant. Instead, I assume for any given event, households could be attentive. Given that I have variation in the price level within and between households, I have explored whether households exhibit time-invariant types. The regression approach of estimating individual parametric responses yields noisy responses with no clear differentiation, but the analysis is severely underpowered. Balandat, Gillan, and Zhou (2017) explores the classification of household types in more detail by characterizing individual responses using machine learning techniques.

[56] This assumption is perhaps the most unrealistic as we might assume information costs could be lower for households that are generally more price elastic. However, given that I do not have exogenous shocks to the information set, I cannot separately identify the two with my current design.

[57] This is roughly equivalent to assuming utility follows the form $U(x, y) = x + u(y) - \gamma y$. The fact that I am unable to reject the null that $\delta_M + \delta_P + \delta_B$ from the *moral suasion treatments* provides some support for this assumption.

periods.[58] I use the resulting panel to estimate the following DD specification:

$$\log(Y_{it}) = (\delta_{MP}\log(MP_{it}) + \delta_{HP}\log(HP) + \delta_G G_{it}) \times C_{it} \times H_t + X_{it}'\beta + \varepsilon_{it} \qquad (14)$$

where Y_{it} is consumption for household i in period t in kWh and MP_{it} is the the marginal price in dollars per kWh (including an assumed utility price of \$0.16/kWh).[59] G_{it} indicates the event message included moral suasion. The value for HP is not identified so I assume it is the expected value of the marginal price in my base specification and explore the sensitivity to that assumption. The coefficient δ_{HP} technically recovers the intercept of price response scaled by the log of the heuristic price. I also include the same controls X_{it} as the preferred reduced form specification.

The parameters in (14) map to the choice primitives specified in (13) as follows

$$\delta_{MP} = \eta\sigma \qquad (15)$$

$$\delta_{HP} = \eta(1-\sigma) \qquad (16)$$

$$\delta_G = \gamma \qquad (17)$$

We can recover the elasticity primitive by adding the two price coefficients because $\eta = \delta_{MP} + \delta_{HP}$. We can determine whether demand is perfectly inelastic (with no shift) by testing the null that $\eta = 0$. If rejected and $\eta \neq 0$, we can recover $\sigma = \delta_{MP}/(\delta_{MP} + \delta_{HP})$ which represents the fraction of the price response attributable to the marginal price change versus the heuristic price change.

The model requires $\mathrm{E}[\varepsilon_{it}|HP, MP_{it}] = 0$ for the parameters to be identified. The random assignment of the price level ensures MP is uncorrelated, but if there is unobserved heterogeneity in HP that is correlated with consumption, the elasticity and attention parameters will be biased. For example, if households that have engaged in other conservation behaviors that make them generally more price responsive are also more to use a heuristic price that is above the expected value of marginal price, then η will overestimate the elasticity of demand. Estimates of the elasticity should be interpreted with caution if this is a concern.

[58] I drop the post-enrollment non-event periods due to the fact that control units in the moral suasion treatments vary by event and thus spillovers are not defined as cleanly as in the price treatments. These periods provide additional variation for precisely estimating the effect of the control variables, but is not necessary for identification.

[59] I estimate the model using 2SLS and instrument $\log(MP_{it}) \times C_{it} \times H_t$ and $\log(HP) \times C_{it} \times H_t$ with $D_{it} \times H_{it}$ interacted with the corresponding price variable.

5.2 Demand Estimates

Panel A of Table 7 reports estimates of the demand parameters η and σ obtained from estimating (14) using the same instrumental variables approach as the marginal price exercise.[60] The exercise pools households such that automation is not explicitly modeled. While raising identification concerns, removing automated households from the sample yields similar parameter estimates. I discuss incorporating automation into the model below.

The estimates are obtained by taking the sum and ratio of regression coefficients in MP and HP described in the previous section. Each column reports estimates obtained from separate regressions with a different assumed value of HP, noted in the column heading. Standard errors are reported in parentheses.[61] For all of the regressions the estimate of γ is 0.012 and is statistically insignificant with a p-value of 0.57.

Column (1) reports the results assuming HP is equal to the expected value of the marginal price plus the average price of electricity, \$0.96/kWh + \$0.16/kWh = \$1.12/kWh. The number in the first row shows that the elasticity of demand implied by the model is -0.075 and is significantly different from zero. Further, we can see the estimated fraction of attentive households during any given event is about 10 percent, meaning about 9 out of 10 households are inattentive for any given price change. The fraction is statistically different from zero which reflects the slight downward slope of demand recovered from the marginal price exercise. If the model structure is assumed to be true, households appear to be responding to price, but only a small fraction of the slope explains the overall shift.

For reference, running the double log specification and omitting the heuristic price variable yields an elasticity estimate of -0.047, which is 36 percent smaller and statistically significantly different. To show this more concretely, I estimate the model for the full range of the possible assumptions about HP over the price changes in the study. Figure 7a plots these results with the horizontal axis showing the range of possible estimates. The solid white line shows the model estimated using the standard double log formulation with the gray bars indicating the 95 percent confidence intervals plotted against the left axis. The solid black line shows the estimates of η according

[60] Namely, the model is estimated with 2SLS with $C_{it} \times H_t$ instrumented with the $D_{it} \times H_t$.

[61] Standard errors are estimated in the regression using the same two-way clustering by household and hour-of-sample as the main results. The standard errors for the parameters recovered from combining regression estimates using the delta method.

to the model demand elasticity with dashed lines indicating 95 percent confidence intervals. The hashed line shows the estimates of the σ plotted against the right vertical axis and is generally less sensitive to the change in assumption about HP. The elasticity estimates converge for prices above \$2/kWh, but for the majority of the price distribution, the estimates are substantively different. This suggests the model with inattention recovers meaningfully different estimates and that the standard approach may mischaracterize underlying preferences.

Columns (2) and (3) of Table 7 report the sensitivity of the results to assumptions about the value of HP at the extremes. Column (2) shows estimates obtained from a regression that assumes the heuristic is equal to the minimum of the pricing distribution, \$0.21/kWh, and column (3) the maximum, \$3.16/kWh. These map to the extremes for estimates of η and σ under the model structure. The elasticity results follow general intuition. If the true heuristic is assumed to be at the low extreme of the distribution, then households preferences are very elastic with an estimate of about -0.5, significantly larger than the empirical literature that uses billing data. The results also show only 1 percent of households appear to be attentive since a smaller fraction of the elasticity is represented in the empirical slope. Alternatively, the high price heuristic shows at most 15 percent of households are attentive and demand appears slightly less elastic than the results in column (1). I interpret these results as evidence of a price response that is largely consistent with values of other electricity demand estimates. However, the low estimate for fraction attentive suggests the constant elasticity form alone does a poor job explaining the price response.

5.3 Attention and Misoptimization

The structural estimates presented in Panel A of Table 7 suggest inattention is a factor in decision-making. Panel B of Table 7 reports estimates of $\mathrm{E}[a(MP, HP)]$ using the definition from (6) and the distribution of MP from the *price treatments*. This recovers the expected attention cost of a single event. While the price distribution is not reflective of the true cost of electricity in this setting, it is informative as to the impact of attention on the financial outcomes of the study. The results in the first column show that the average misoptimization cost is \$0.03. The misoptimization is minimized at the expected value so this provides a lower bound to the attention cost using this framework.

The nominal value is small, but sensitive to the assumption about the heuristic. Consider the extreme values to gain intuition. A household whose heuristic is the low extreme consistently under-responds. The low heuristic implies true utility is very elastic, so overconsumption has little value. Thus, large price changes yield large mistakes. The results are significantly less sensitive for the high heuristic. In this case, households consistently consume too little, but are much less elastic so the differences between true utility and what is observed are small. Figure 7b plots the attention cost estimates for the range of HP assumptions showing the costs are much higher for households whose heuristic is below the expected value.

Table 7 also shows estimates of the financial outcomes of an event to provide context. I report the average cost of electricity consumed during the event along with average expected reward. These are calculated individually using the demand estimates, but rounding makes them appear the same. On average households are paying \$0.16/kWh because the level of consumption among treated households during events is about 1kWh and I calculate the average reward to be \$0.15 per event.[62] Column (1) shows the attention costs represent between 10 to 19 percent of the financial stakes depending on the inclusion of the reward.[63] Columns (2) and (3) show the estimate can represent up to 142 to 275 percent of the financial outcome. Aggregating these mistakes over larger consumption suggests potentially large attention costs, but ultimately depends on the number of events called and the distribution of price changes.

5.4 Automation and Attention

This section discusses how to incorporate automation explicitly into the framework developed above. The fact that automation is not price responsive complicates the welfare interpretation since it opens the potential for overcorrection. Further, automation could change the relative cost of information as well as the cost of effort, affecting both the σ and η parameters. For example, otherwise attentive households could become inattentive and engage in "set it and forget it" behaviors. Given these complications, future work should seek to understand the degree to which automation reveals a preference for inattention. This would require a more rigorous evaluation of WTP or the introduction of automation that actively elicits reservation prices as an input.

[62] I assume the forecast for an event is given by the control group. This provides a good approximation for illustrative purposes, but may not perfectly reflect the rewards.

[63] Including the reward may not be appropriate for all dynamic pricing programs, for example those that use price.

6 Discussion

This section interprets the results of the empirical analysis and the model application. I provide justification for the simplifying assumptions of the model, but also comment generally on the mechanisms implied by the reduced form results. I interpret the response to a price, but not the price level, as suggesting that households are making decisions using a simplifying heuristic consistent with a rationally inattentive agent. I show the results are unlikely to be due to moral suasion, incorrect or incomplete valuation of the reward currency, observation of the price incentive, and physical discontinuities in the consumption choice. I also interpret the automation effect as suggesting there are substantial costs to taking action that are unrelated to attention to the price level which affect the overall elasticity of demand.

6.1 Improbable Mechansisms

Moral suasion – While there is evidence that non-price factors such as social comparison (Allcott, 2011b) and moral suasion (Ito, Ida, and Tanaka, 2017) affect residential electricity consumption decisions, the finding that households do not respond to the moral suasion alone provides evidence that the effect is driven by the presence of a financial incentive rather than non-price motivations. Given that households choose to enroll in the program, the concern that non-price motivations could be driving the result is reasonable, but evidence from the survey shows 61 percent of respondents rank "getting financial rewards for saving" as the top reason for joining the program, compared with 21 percent who chose "environmental or sustainability concerns."[64]

Observation of price level – If households are simply reacting to the receipt of a message, there may be a Hawthorne effect where the shock to salience affects energy consumption regardless of the message content. Again, the difference in response to moral suasion versus price messaging goes against this interpretation and suggests households are paying attention to message content.[65]

[64]The other choices were "making my home Smarter and more efficient", "entertainment/gamifying of energy use", "grid reliability", and "donating rewards to a specific cause". While the survey is a non-random subset of the sample, it provides suggestive evidence that financial rewards are the primary motivator for participating households.

[65]The survey also asks two questions designed to test whether households observe message content by asking them to recall the specifics of their last pricing event. 40 percent of households correctly recall their last incentive level (as compared with a 17 percent chance of guessing the correct answer randomly). Households are asked to recall their last incentive level in points per kWh from a set of six numeric choices: 0, 5, 25, 50, 100, and 300 points per kWh. Since recall is likely to be a perfect approximation of observation, the survey also asks a simpler question of when their last event period occurred to gauge recall accuracy unrelated to the price level. The question gave six answers to choose from (today, yesterday, 2, 3, and 4 days ago, and 5 or more days ago). Only 32 percent correctly recalled how

Misunderstanding of monetary value of points – The fact that households respond significantly to pricing events and not to moral suasion again shows that households are making economic tradeoffs between consumption and points. However, if there is confusion over the dollar value of points, then large changes in the points per kWh rate may not map completely to the economic significance when households are making decisions. The monetary value of a point is communicated consistently throughout the program through the rebate and point balance. The survey also tested whether households understand the dollar value of points, by asking them the question: "How much money is 100 points worth?" Given a multiple choice of four numeric answers and a fifth "I don't know" option, 70 percent of survey respondents gave correct answers.[66] This suggests misunderstanding about the economic value is not driving my price insensitivity finding.

Electricity consumption is a discrete choice – If electricity consumption depends on a set of discrete choices, the shift in demand I find may simply be the shape of a step-wise demand curve. This explanation is unlikely because demand represents a full hour of electricity consumption and utilization provides a continuous margin to respond. For example, a household that decided to turn off the lights and take a walk when an event was called might extend the walk for higher prices. Further, the distribution of consumption within household for pre-enrollment periods does not appear to be discrete.[67]

6.2 Scope Neglect and Price Insensitivity

The discussion above makes the case that households are responding to a price change, but are responding much less to its size. This leads to the question: what information are households using to make their decisions? I argue households are using simplified decision rules that I will refer to as heuristics. There is precedent for such behavior in electricity consumption when facing nonlinear tariffs (Ito, 2014), but my findings suggest that there are also information costs for linear incentives.

The finding that households respond to price, but not the size can be explained by the phe-

many days had passed since their last event (compared with a random chance of about 17 percent). The question was incentivized for correctness by offering $0.25 per correct answer. The incentive size was chosen to be small enough as not to encourage individuals to look up their last event, but large enough for them to put some thought into the response. This provides further evidence that people are observing the message content, but suggests attention to the message content is likely to be imperfect.

[66] 20 percent answered "I don't know" and 10 percent answered incorrectly.

[67] In the Appendix I show example distributions that show the continuousness of consumption during a single hour of the day during the pre-enrollment period conditional on temperature.

nomena of *scope neglect* (Kahneman, 2003). The concept comes from evidence in psychology that individuals make decisions based on simple accessible *prototype attributes* and often neglect the scope of *extensional attributes*. [68] In this setting, we can think of the prototype attribute as a price change occurring since households do not respond to the moral suasion intervention.[69] This categorical information is readily available for a quick decision about whether or not to reduce consumption. However, contemplating the extensional attribute, the size of the price change, requires additional cognitive effort.[70] If the effort cost is sufficiently high, households may be making decisions based on the prototype attribute of whether a price change occurred.

6.3 Automation Reduces Effort Costs

The significant effect of automation in response to a pricing event provides evidence that the technology makes it easier for households to respond to price changes by reducing effort costs. Two empirical findings support this interpretation. First, Figure 4c shows clearly that automation causes a significant reduction in consumption during pricing events and no evidence of displacement to other periods. During the event period, the effect is negative and large. During the periods prior to and following the event, the effect of automation is not consistently positive or negative and the estimates are statistically insignificant.[71] Second, rebate compliers consume less than always-takers, on average. If some component of effort costs are independent of quantity of electricity consumed, the benefit of automation would be greater for households who consume more. This implies lowering the technology's cost should disproportionately induce smaller consumers to take-up the automation. Table 5 reports evidence supporting this case since households in the lowest pre-enrollment consumption quartile take-up the automation significantly more than larger consumers. This is also suggestive evidence of rationality on the adoption margin.

To provide a ballpark on the costs of effort automation addresses, I calculate a back-of-the envelope measure of the additional electricity expenditure savings. Households who adopted due

[68] For example, households have been documented in stated willingness to pay (SWTP) elicitations as revealing preference to save an endangered species of bird, but having the same SWTP for 2,000 birds as they do for 200,000 (Kahneman, Ritov, and Schkade, 1999).

[69] I also investigated whether households were more sensitive to several candidate heuristics: the previous price level, a Bayesian prior over past price levels, and whether the last price was higher or lower. The price insensitivity persists for all candidates. I report the results of this exercise in the Appendix.

[70] Again, the effort need not map to observing the price, but rather making the intermediate decisions required to make a marginal response that includes the extensional information.

[71] I also fail to reject the null that the sum of changes in all 8 hours following an event, the 8 hours before and 7 hours prior (excluding hour -2), or the sum of all adjacent hours is different from zero.

to the rebate reduced an additional 0.64 kWh on average during pricing events.[72] If the average reward level is \$0.96/kWh, this suggests a additional reward of about \$0.61 per event.[73] This suggests additional savings on their electricity bill of about \$0.10, leading to a total of \$0.71 per event. Over the course of 3 months, households received about 27 events, implying a total of about \$19 additionally saved.

The study period paid incentives that were not necessarily reflective of the rewards during typical program operation so these numbers should be interpreted with caution when extending them to future periods. If we assume real-time wholesale prices reasonably reflect the future reward rates, a reasonable upper bound would be \$0.30/kWh for the year which gives about \$0.29 per event from the program and bill savings. For a program with 100 events per year, the annual savings are \$29, giving two smart plugs a payback period of about 2 to 3 years with a discount rate of 6 percent. The payback period for a smart thermostat is between 9 to 11 years, depending on upfront cost. Including the additional non-event savings makes these periods even shorter, suggesting the subsidized price is particularly beneficial to the average household.

While the automation suggests household preferences are more price elastic, the fact that price responsiveness does not substantively change suggests the automation does not address inattention. The automation did not differentiate response based on the price change and thus can be interpreted as default response that households could override via their smart phone. This raises questions over whether the automated price response can be interpreted as perfectly reflective of preferences, but the fact that households chose to adopt suggests there is a revealed preference for lower effort costs. Given that the automation was marketed for this purpose, this seems a reasonable conclusion.

7 Conclusion

This paper documents how households respond to dynamic prices in residential electricity consumption. I show evidence that households reduce consumption when price increases, but are insensitive to extreme changes in the marginal price in a manner indicative of scope neglect. I develop a model of limited attention to explain the findings which suggests 9 out of 10 households are inattentive to the size of the price change during any given event and elasticities recovered from models that do not account for attention frictions may give substantively different estimates. I find

[72] This number comes the automation IV with kWh as the dependent variable reported in the Appendix.

[73] I assume the control group proxies for the forecast in this calculation.

the misoptimization costs of inattention are nominally small, but represent meaningful fractions of the financial stakes. Lastly, I report novel evidence that automation can make households generally more responsive to price changes by addressing substantial costs of effort, but does not necessarily resolve inattention unless.

Given the increasing amount of consumer dynamic pricing and the developing landscape of smart devices, this paper highlights several areas for continued research. If attention costs are prevalent, how can dynamic pricing be designed to minimize misoptimization? If automation is the primary strategy, how can reservation prices be elicited in a way that is consistent with preferences? Carefully designed field experiments can contribute to these lines of inquiry and inform future dynamic pricing strategies that take into account the attention costs consumers face in their decisions.

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Table 1: Summary Statistics and Balance of Treatment Assignment

	Control Group (1)	Treatment Groups Standard (2)	Encouraged (3)	p-value on t-test with H_0: $\mu_S = \mu_C$ (4)	$\mu_E = \mu_C$ (5)	$\mu_E = \mu_S$ (6)
Energy & Weather Data						
Daily Consumption (kWh)	16.5	16.0	16.0	0.23	0.21	0.94
	(11.2)	(10.8)	(10.7)			
Max Consumption (kWh)	5.0	4.9	4.8	0.14	0.09*	0.71
	(2.7)	(2.6)	(2.7)			
Hourly Outdoor Temp. (°C)	16.7	16.7	16.6	0.94	0.38	0.23
	(2.6)	(2.7)	(2.8)			
Mean Daily CDHs	33.8	34.0	33.4	0.90	0.69	0.51
	(30.6)	(31.8)	(31.7)			
Mean Daily HDHs	113.8	113.7	115.5	0.98	0.27	0.15
	(41.5)	(41.8)	(43.2)			
Pre-Enrollment Obs.	8755.6	8720.5	8686.5	0.83	0.68	0.79
	(4342.7)	(4362.4)	(4399.8)			
Demographic Data						
% HH Income < $25K	18.8	19.2	19.4	0.59	0.40	0.69
	(14.2)	(13.7)	(13.8)			
% Population Age 21-39	30.1	29.6	29.8	0.36	0.56	0.68
	(11.8)	(12.1)	(12.1)			
% Family HHs	66.3	66.1	66.7	0.85	0.66	0.42
	(19.1)	(19.0)	(18.5)			
% Population w/ Bachelors	22.8	22.5	21.8	0.68	0.14	0.18
	(13.0)	(13.0)	(12.4)			
Median Year Built	1973.5	1973.5	1973.0	0.92	0.54	0.50
	(17.2)	(16.9)	(16.9)			
% Renters	48.7	48.3	48.4	0.79	0.81	0.98
	(26.7)	(26.0)	(26.3)			
% HHs w/ 3+ bedrooms	53.0	52.4	52.8	0.72	0.88	0.78
	(30.3)	(29.7)	(29.1)			
% Detached Units	54.2	54.2	54.4	0.99	0.91	0.89
	(33.9)	(33.0)	(32.2)			
% Electric Heating	28.2	27.9	27.2	0.80	0.31	0.34
	(17.3)	(17.8)	(17.0)			
Median Monthly Rent ($)	1344.1	1361.8	1344.3	0.54	0.99	0.44
	(518.3)	(520.7)	(519.5)			
Households/Observations	1,035	2,271	2,225			

Standard deviations in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The table reports summary statistics by treatment assignment using household-level data. The energy, temperature, and observation counts are calculated by averaging over hourly variation within a household using pre-enrollment energy and weather data. The demographic statistics are calculated using a subset of the study participants for whom we have census block group designations. The census variables are constructed from block-group-level data from the 2015 American Community Survey 5-year estimates. Columns (1)-(3) report the means for each treatment group with standard deviations in parentheses. Columns (4)-(6) report p-values on the hypothesis tests for the difference in means permuted between each treatment group. μ_G denote the parameters for treatment group G with C =Control, S = Standard, and E = Encouraged.

Table 2: Event Period Summary Statistics

Event Period	Observations	Events Called	Proportion Called	Mean Incentive ($/kWh)	Mean Temperature (°C/°F)	Consumption for: Control (kWh)	Treated (kWh)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
11:00-12:00	3,848	2	0.89	0.97	22.2 (71.9)	0.81	0.68
12:00-13:00	8,035	3	0.90	0.94	25.4 (77.7)	0.93	0.76
13:00-14:00	23,905	12	0.90	0.96	27.8 (82.1)	1.07	0.86
14:00-15:00	21,611	12	0.89	0.96	25.7 (78.3)	1.11	0.90
15:00-16:00	12,980	7	0.88	0.97	28.9 (84.0)	1.30	1.03
16:00-17:00	24,626	18	0.89	0.96	25.6 (78.1)	1.06	0.87
17:00-18:00	18,761	11	0.90	0.96	24.5 (76.2)	1.16	0.96
18:00-19:00	11,310	9	0.89	0.96	19.4 (66.8)	0.84	0.75
19:00-20:00	15,399	11	0.89	0.97	21.8 (71.2)	1.26	1.03
20:00-21:00	9,618	8	0.89	0.95	18.7 (65.6)	1.09	1.00
21:00-22:00	1,433	1	0.89	0.94	11.2 (52.2)	0.73	0.70
All Events	151,526	94	0.89	0.96	24.6 (76.3)	1.09	0.90

This table reports summary statistics for the pricing events by hour-of-day called. Column (1) reports the number household-hour observations, column (2) reports the total number of events called, column (3) reports the proportion of treated households sent messages, column (4) shows the average $/kWh offered each event, and column (5) shows the mean temperature. Column (6) and (7) shows the mean consumption in kWh for control and enrolled households, respectively.

Table 3: Effect of Pricing Events

	OLS (1)	OLS (2)	IV (3)
Panel A: Effect Estimates			
Enrolled × Event ($D_{it} \times H_t$)	-0.111^{***}		
	(0.010)		
Called × Event ($C_{it} \times H_t$)		-0.127^{***}	-0.124^{***}
		(0.010)	(0.011)
Enrolled × Non-event ($D_{it} \times N_t$)	-0.004	-0.004	-0.004
	(0.006)	(0.006)	(0.006)
Panel B: 1st Stage Estimates			
Enrolled × Event ($D_{it} \times H_t$)			0.890^{***}
			(0.005)
Households	5,531	5,531	5,531
Observations	59,574,289	59,574,289	59,574,289

Standard errors clustered by household and hour-of-sample in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A reports estimates of the effect of enrollment during pricing event periods and non-event periods on hourly energy consumption measured in log(kWh). Each column report coefficients estimated from separate regressions and all include temperature controls, hour-of-sample fixed effects, households by hour-of-day fixed effects, and pre-enrollment data. Standard errors are reported in parentheses and estimated using a covariance matrix two-way clustered by household and hour-of-sample. Column (1) reports estimates for the DD specification in (7). Column (2) reports estimates for the TOT for called households using OLS. Column (3) reports the 2SLS estimates where called being called is instrumented with enrollment. Panel B reports the first stage coefficient on the instrument.

Table 4: Effect of Price vs. Moral Suasion (No Price)

	OLS (1)	OLS (2)	
Panel A: Effect Estimates			
Moral Suasion Only (δ_M)	-0.033^{*} (0.018)		
$\times$ Non-Automated		-0.018 (0.18)	
$\times$ Automated		-0.191^{***} (0.068)	
Price Only (δ_P)	-0.077^{***} (0.017)		
$\times$ Non-Automated		-0.060^{***} (0.018)	
$\times$ Automated		-0.238^{***} (0.068)	
Moral Suasion + Price (δ_B)	-0.094^{***} (0.021)		
$\times$ Non-Automated		-0.066^{***} (0.022)	
$\times$ Automated		-0.368^{***} (0.064)	
Panel B: Coefficient Tests	*Pooled*	*Non-automated*	*Automated*
H_0: $\delta_M = \delta_P$	0.002	0.006	0.362
H_0: $\delta_M = \delta_B$	0.000	0.003	0.001
H_0: $\delta_P = \delta_B$	0.236	0.745	0.005
H_0: $\delta_M + \delta_P = \delta_B$	0.461	0.544	0.471
Households	1,122	1,122	
Observations	8,534,776	8,534,776	

Standard errors clustered by household and hour-of-sample in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A reports estimates of the casual effect of message content from the *moral suasion treatments* on hourly energy consumption measured in log(kWh). Each column report coefficients estimated from separate regressions and all include temperature controls, hour-of-sample fixed effects, households by hour-of-day fixed effects, and pre-enrollment data. Standard errors are reported in parentheses and estimated using a covariance matrix two-way clustered by household and hour-of-sample. Column (1) reports the model estimated using the DD specification in (11). Column (2) reports estimates for the same model interacting each message type indicator with automation status. Estimates comparing within automation type can be interpreted causally. Panel B reports p-values on tests of whether treatments are equivalent and additive. δ_M representing the parameter for moral suasion only, δ_P representing the parameter for price only, and δ_B representing the parameter for both interventions together. Column (1) reports the p-values for the coefficients that pool automated and non-automated. Column (2) breaks out the p-values by automated and non-automated.

Table 5: Effect of Encouragement on Automation Take-up

	Standard (S) (1)	Encouraged (E) (2)	Difference (E−S) (3)
Panel A: Automation Type and Counts			
Households	2,271	2,225	-
Households with at least one connected device	114	204	90
Total connected devices	242	429	187
Total connected thermostats (subsidized)	126	193	67
Total connected plugs (subsidized)	108	222	114
Total connected home systems (not subsidized)	0	9	9
Total connected electric vehicles (not subsidized)	8	5	-3
Panel B: Automation Take-up			
Households with any automation	0.049	0.089	0.040***
			(0.008)
Take-up by consumption level:			
1^{st} quartile (0-0.34kWh)	0.022	0.084	0.063***
			(0.013)
2^{nd} quartile (0.34-0.55kWh)	0.057	0.092	0.036**
			(0.015)
3^{rd} quartile (0.55-0.85kWh)	0.059	0.094	0.034**
			(0.02)
4^{th} quartile (0.85-6kWh)	0.057	0.085	0.028*
			(0.02)

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A reports counts for the home devices connected to the company's automation service in the Standard enrolled and Encouraged groups. Panel B reports the proportion of households who took up automation and the difference between the two treatment groups. It also reports the proportion take-up by pre-enrollment consumption quartile. The differences are calculated by regressing an indicator for any automation on the encouragement indicator. Standard errors are reported in parentheses and observations are assumed to be independent.

Table 6: Effect of Automation

	OLS	OLS	IV
	(1)	(2)	(3)
Panel A: Effect Estimates			
Adopted × Event ($A_{it} \times H_t$)	-0.213^{***}		-0.829^{***}
	(0.029)		(0.167)
Adopted × Non-Event ($A_{it} \times N_t$)	0.014		-0.181^{*}
	(0.017)		(0.110)
Encouraged × Event ($Z_{it} \times H_t$)		-0.062^{***}	
		(0.011)	
Encouraged × Non-Event ($Z_{it} \times N_t$)		-0.013^{*}	
		(0.008)	
Panel B: Selected First Stage Estimates			
Encouraged × Event ($Z_{it} \times H_t$)			0.075^{***}
			(0.009)
Encouraged × Non-Event ($Z_{it} \times N_t$)			0.074^{***}
			(0.007)
Households	4,496	4,496	4,496
Observations	48,254,453	48,254,453	48,254,453

Standard errors clustered by household and hour-of-sample in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A reports estimates of the effect of automation on log energy consumption for the subsample of enrolled households (excluding controls). Each column report coefficients estimated from separate regressions and all include temperature controls, hour-of-sample fixed effects, households by hour-of-day fixed effects, and pre-enrollment data. Standard errors are reported in parentheses and estimated using a covariance matrix two-way clustered by household and hour-of-sample. All regressions include household by hour-of-day and hour-of-sample fixed effects and temperature controls. Column (1) reports the OLS estimate for the effect of automation on consumption. Column (2) reports the reduced form specification regressing consumption on the encouragement indicator. Columns (3) reports the 2SLS estimates instrumenting the automation indicator with the encouragement. Panel B reports results of the first-stage regressions for the coefficient on the instrument corresponding to the endogenous variable.

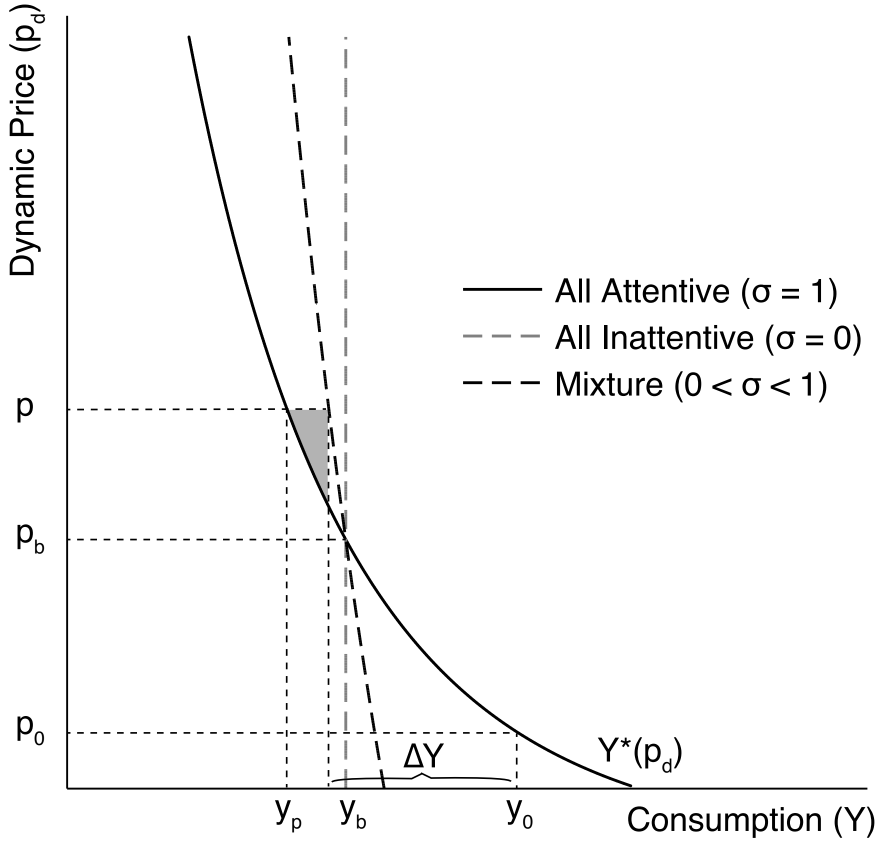
Table 7: Elasticity and Attention Estimates

	$HP = \mathrm{E}[MP_{it}]$ ($1.12/kWh)	$HP = MP_{low}$ ($0.21/kWh)	$HP = MP_{high}$ ($3.16/kWh)
	(1)	(2)	(3)
Panel A: Demand Parameters			
η (elasticity)	-0.074^{***}	-0.483^{***}	-0.051^{***}
	(0.005)	(0.036)	(0.004)
σ (fraction attentive)	0.101^{***}	0.015^{***}	0.147^{***}
	(0.036)	(0.006)	(0.049)
Panel B: Attention Costs per Event			
Average Attention Cost	$0.03	$0.44	$0.06
Average Electricity Cost	$0.16	$0.16	$0.16
Average Reward	$0.15	$0.15	$0.15

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

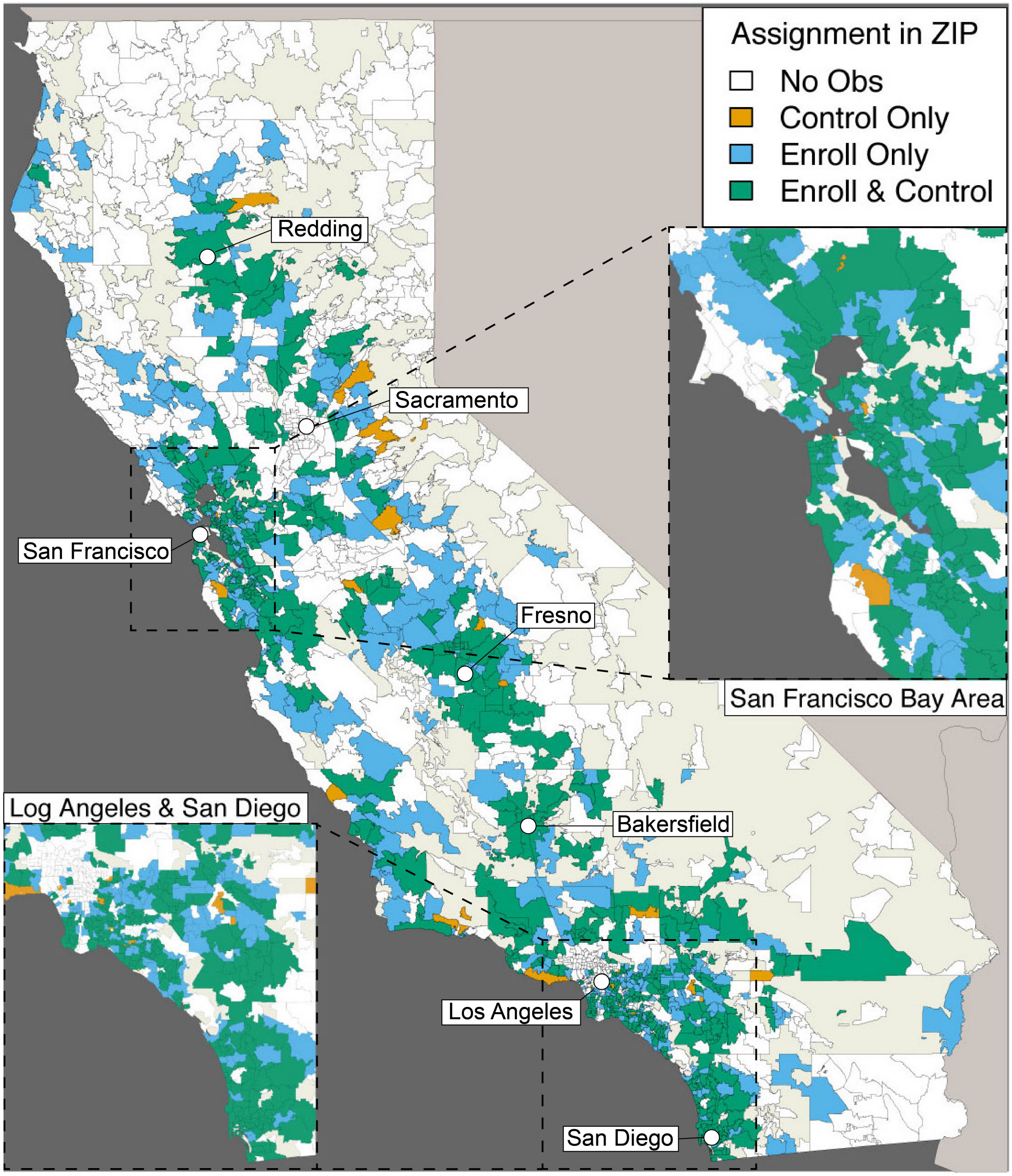
Panel A estimates of the demand parameters obtained from the coefficients estimated using the model in (13). The estimate of γ the moral suasion component of utility is the same for all three specifications and equal to 0.006 (0.021). Standard errors are reported in parenthesis and are calculated using a covariance matrix that is clustered by household and hour-of-sample along with the delta method. Panel B reports estimates on the expected financial outcomes of a pricing event during the *price treatments*.

Figure 1: Model Intuition



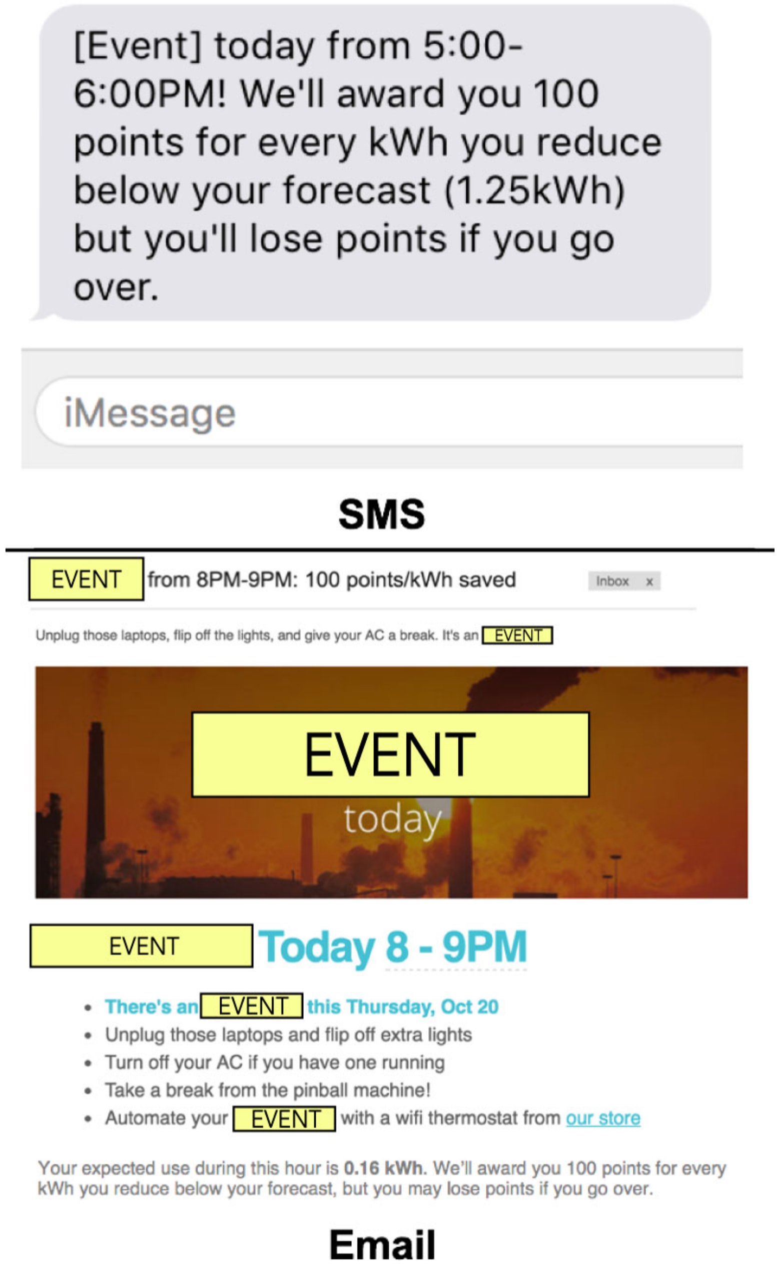
The figure plots the effect of a price change for different fractions of attentive households for the model developed in Section 2. The vertical axis plots the dynamic price and the horizontal axis aggregate consumption. The two types of households are indicated by the solid black and dashed gray lines. The solid black line shows demand if all households were attentive ($\sigma = 1$), the gray dashed line shows demand if all households were inattentive ($\sigma = 0$), and the black dashed line shows demand for a hypothetical mixture with $\sigma \in (0, 1)$. Consumption at the non-dynamic price p_0 is denoted y_0. A hypothetical price change p is indicated and shows that attentive households consume y_p. Inattentive households consume according to the heuristic price, p_b, at y_b. For the mixture case, the observed aggregate demand for a price change p is not labeled, but falls in the range $[y_p, y_b]$. In this case, the consumption change from the price change is shown by the difference ΔY and the shaded gray area indicates the attention costs of misoptimizing using the heuristic.

Figure 2: Enrollment and Assignment by ZIP Code



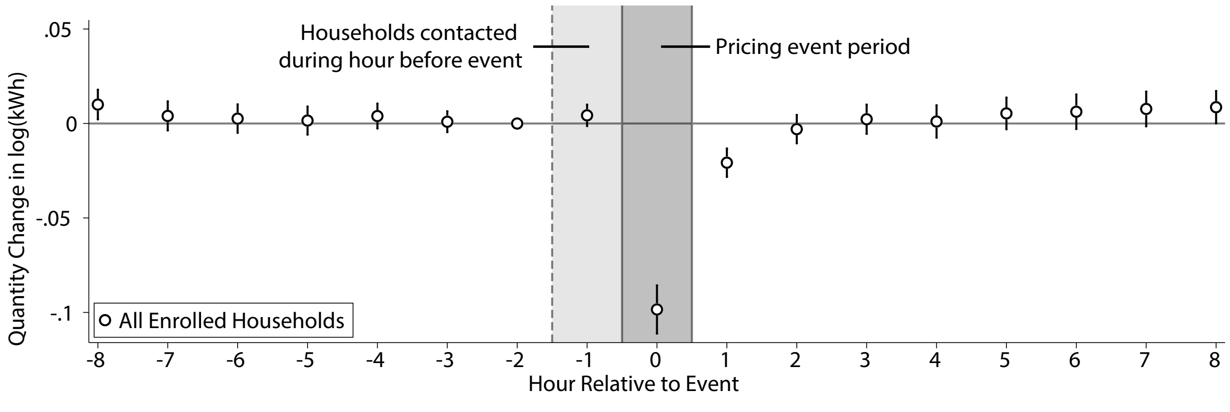
The map plots the ZIP codes with households participating in the study. Regions are shaded green have both Enrolled (assigned to Standard or Encouraged group) and Control households. Regions with only Enrolled are shaded blue and regions with only Control are shaded orange.

Figure 3: Example of SMS and Email Event Messaging

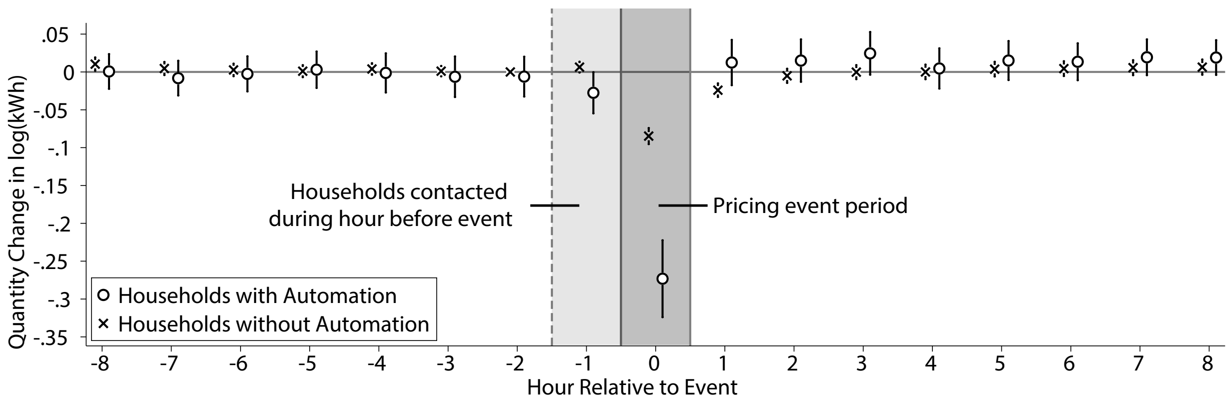


The figure shows messaging for two example events that occurred during the experiment. Both messages convey the time of the event, the incentive level, and the company's forecast. The top pane shows messaging via SMS and the bottom pane shows messaging via email for a different event. The email contains more information on suggested strategies for reducing consumption. The yellow boxes cover the company's proprietary term for events which I have redacted for the purpose of confidentiality.

Figure 4: Pricing Hour Event Studies



(a) Enrolled vs. Control Households

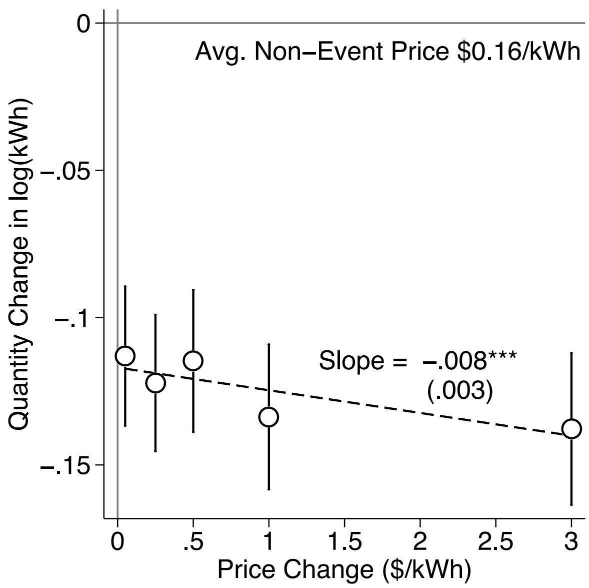


(b) Automated vs. Non-Automated Households

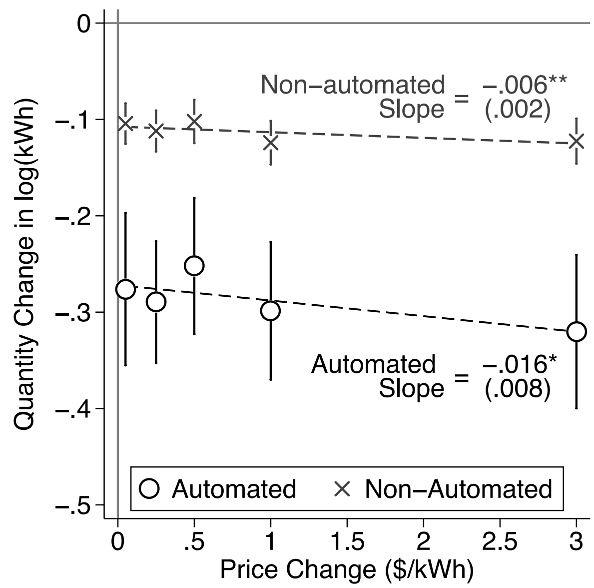
(c) Additional Effect of Automation for Enrolled Households

Note the vertical axis scale varies between the three figures. Panel (a) plots event-study point estimates of the ITT effect that compares the consumption of enrolled households to control households in the hours leading up to and following a pricing event (shaded dark gray). Panel (b) plots the non-causal decomposition of the ITT estimates in (a) by whether households automated their decisions. Panel (c) plots event study estimates of the LATE of adopting automation on consumption during pricing periods. Households are contacted during the hour prior to the event (shaded light gray). I use the period two hours prior as the reference category so the vertical axis is the quantity change relative to period -2 in log points. For (b) the reference category is period -2 for non-automated households. Vertical bars indicate 95 percent confidence intervals clustered by household and hour-of-sample.

Figure 5: Effect of Marginal Price



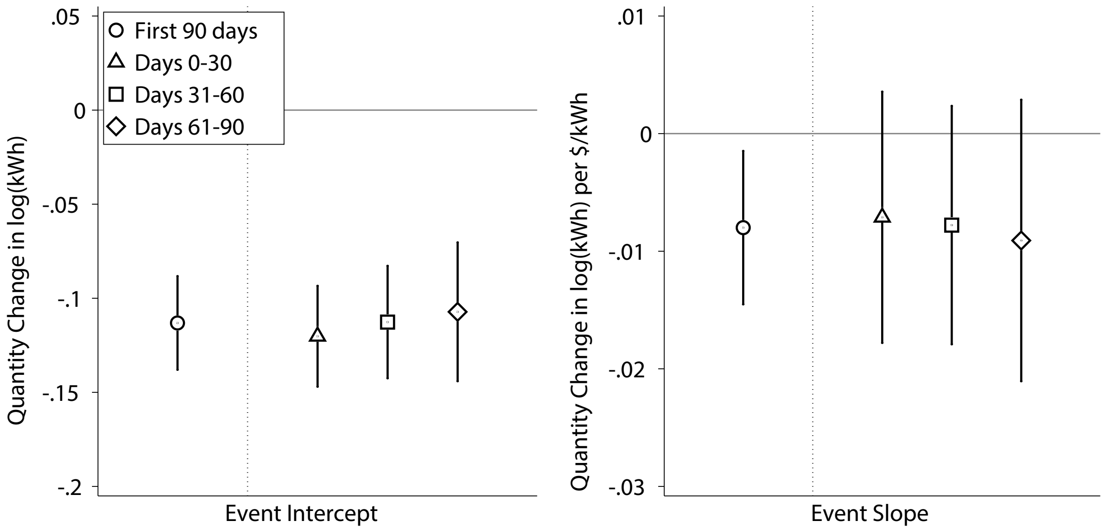
(a) Effect of Price Change in log(kWh)



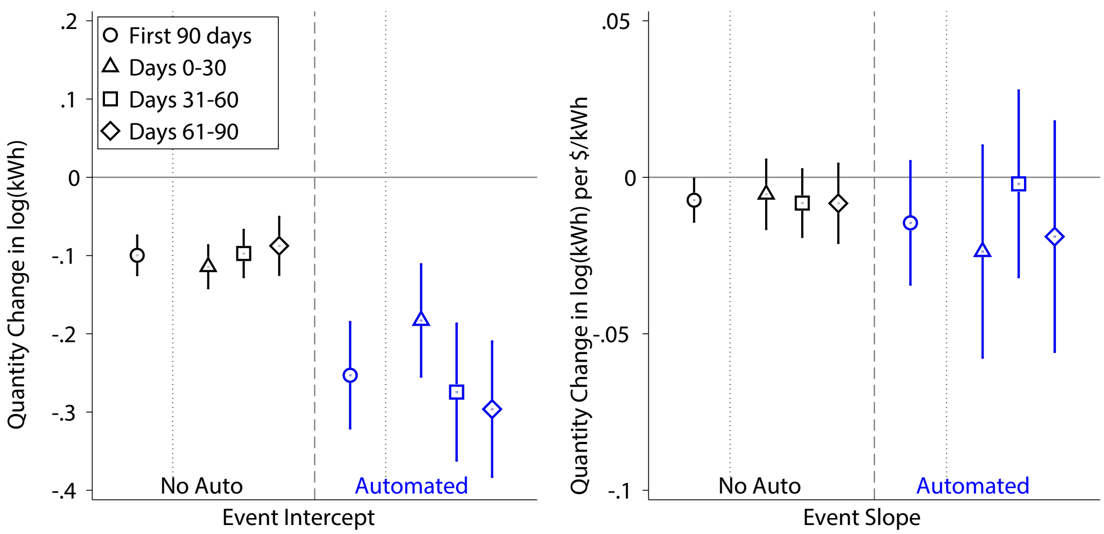
(b) Automated vs. Non-Automated in log(kWh)

The figure plots the point estimates for the change in the conditional mean of consumption as a result of changes in the effective price of electricity in dollars per kWh. Note the difference in the scales of the axes between the left and right panels. The left panel shows the effect with log consumption on the vertical axis and the price change on the horizontal axis. The right panel shows the treatment effect heterogeneity by households with and without automation. Vertical bars in both panels show 95 percent confidence intervals estimated using standard errors clustered by household and by hour-of-sample. The dashed line plots the linear parametric estimation of the response as a function of the price change. The figure also reports the estimates for the slope and intercept of the parametric estimation with two-way clustered standard errors in parentheses.

Figure 6: Persistence of Price Response



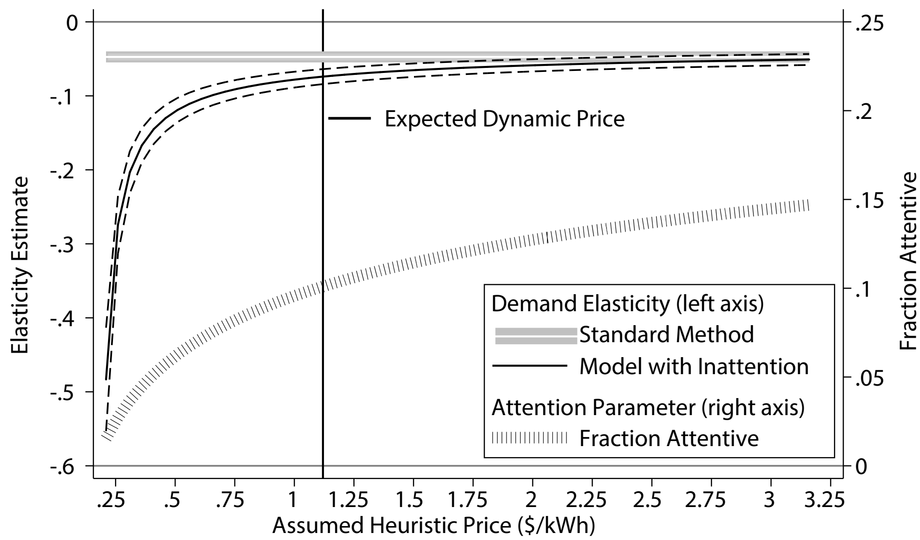
(a) Event Price Response Effect



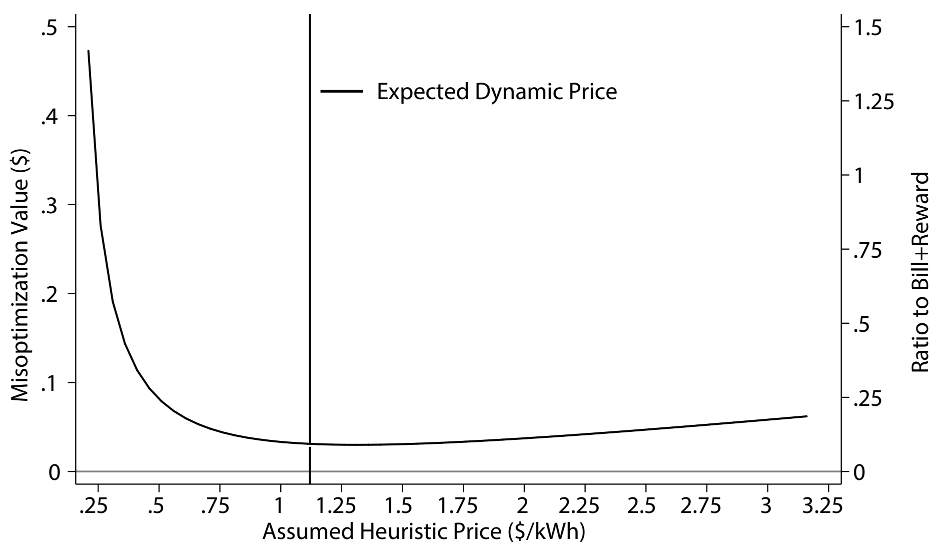
(b) Automated vs. Non-automated Households (non-causal)

This figure shows how *price treatment* effects change as a function of time in the program. The estimates are broken out by 30 day periods during the 90 days for which *price treatments* occurred. Triangles represent days 0-30, squares days 31-60, and diamonds days 61-90. Circles represent the estimate for the full 90 days as reference. Vertical bars show 95 percent confidence intervals constructed from standard errors two-way clustered by household and hour-of-sample. Each panel represents estimates from a separate regression. Panel (a) plots causal estimates of the event price response and non-event enrollment response for all enrolled households regardless of automation choice. Panel (b) plots the same parameters non-causally decomposed to show differences between non-automated (black) and automated (blue) households. Estimates labeled “Event Intercept” and “Event Slope” represent the parametric estimation of the price response of consumption for called households.

Figure 7: Demand Estimation



(a) Demand Elasticity and Attention Estimates



(b) Attention Cost Per Event

This figure shows how the estimates of the elasticity (η) and fraction attentive (σ) vary with assumptions about the heuristic price plotted in $/kWh on the horizontal axis. The solid black line shows the estimates of the own-price demand elasticity plotted against the left vertical axis. The dashed black lines show the 95 percent confidence intervals for the elasticity parameter. The hashed line shows the estimates of the salience parameter which is the fraction of attentive households against the right vertical axis. The solid white line shows the model estimated using the standard double log formulation with the gray bars indicating the 95 percent confidence intervals.