Money for Nothing: Using peer comparisons and financial incentives to reduce electricity demand in urban Indian households

Anant Sudarshan*

Abstract

A randomized field experiment is carried out on a population of upper-middle class urban households in India to evaluate the relative effectiveness of three policy tools designed to encourage reductions in urban household electricity consumption. Household response to price changes, peer group comparisons and peer comparisons augmented with conditional financial incentives is quantified. The pure behavioral intervention using peer comparisons is able to generate sustained reductions in household electricity consumption of the order of 11 percent of electricity consumption over the summer season. Counter-intuitively, introducing financial incentives that reward reductions alongside the same peer comparisons leads to a significant deterioration in impact. This result provides empirical field evidence supporting psychological theories of motivation crowding out in a policy relevant setting. The underlying price elasticities of the experimental population are also independently estimated and found to be low, consistent with the idea that money may have limited motivational effect in this setting. I consider implications for the use of behavioral and financial incentives in demand side interventions in energy consumption.

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1 Introduction

Mitigating the growth of global CO_2 emissions will necessarily involve policy interventions not just on the supply side but also the demand side of energy markets. In recent years policy makers and academics have

*Sudarshan: Kennedy School of Government, Harvard University and Institute for Financial Management and Research, Chennai. Email: Anant_Sudarshan@hks.harvard.edu Acknowledgements: I thank the Sustainability Science Program (Harvard University) for financial support. I thank also Karthik Dinne and Nazuk Kumar for research assistance and seminar participants at The Energy and Resources Institute (TERI) for helpful comments. All errors are my own.

become increasingly interested in demand side management, specifically programs to reduce aggregate energy consumption and encourage investment in energy efficiency.

The most commonly analyzed technique to modify electricity consumption involves changing the prices consumers pay for the energy they purchase through taxation, subsidies or conditional financial incentives. Yet while modifying energy prices is an attractive solution on paper, it has proved neither politically nor administratively easy for policymakers to do so, even granted good reasons grounded in science and economic theory. Partly for this reason some researchers have advocated the increased use of behavioral techniques. These interventions rely primarily on providing carefully framed information or modifying the types of choices consumers are asked to make to 'nudge' them towards the desired behavior (for instance reducing electricity consumption). In an influential article (Allcott and Mullainathan, 2010) argued for the increased use of such behavioral methods in energy policy.

In evaluating the role of behavioral interventions in energy policy, two questions are of central importance. First, how does the effectiveness of behavioral interventions compare to financial incentives, given that traditional economic policy still focuses primarily on monetary incentives? Should these techniques be seen primarily as a second best approach when financial incentives cannot be used (as implied by Stern et al., 2010 in their response to Allcott and Mullainathan, 2010)? Second, do information and behavior interventions and financial incentives work to complement each other or do they interact in more complicated ways?

When it comes to describing the impact of financial incentives on behavior, the economics and psychology literatures have tended to stress very different points of view. Classical economic models treat financial incentives as an unambiguously positive motivational factor, so that the behavior of agents can be systematically and reliably predicted to align itself in a direction maximizing economic returns. In this model, asking people to undertake an action is always less effective than paying them to do it (or taxing them for not taking action). On the other hand, a rich literature in cognitive sciences and psychology has argued that financial incentives can reduce an agent's intrinsic motivation even though they may provide a separate extrinsic motivation (see Sansone and Harackiewicz, 2000). The net result of the incentive is therefore ambiguous and could involve a net increase or decrease in motivation and consequently an increase or a decrease in the likelihood of observing incentivized behaviors. This phenomenon is often referred to as 'motivation crowding out'. The importance of motivation crowding out is difficult to evaluate because evidence showing that financial incentives can reduce performance comes almost entirely from the lab. Real world field evidence is very limited and has primarily focused on pro-social behaviors often involving reputation benefits (see Frey and Jegen, 2002,Gneezy et al., 2011 for reviews). Empirical evidence of this phenomenon in a directly policy relevant setting is even more scarce (one possible example is Frey and Oberholzer Gee, 1997).

This paper provides insight into these questions within the highly policy relevant context of demand side management of residential electricity. A randomized field experiment was carried out in New Delhi on a sample of over 500 households belonging to India's fast growing urban middle class (who contribute to most of the growth in the country's residential electricity consumption). First we find that providing information on the average electricity consumption of other households (a peer comparison treatment) resulted in an approximately 11 percent reduction in electricity consumption averaged over the entire summer season (in comparison to households not provided this information). We are further able to benchmark this impact against the effectiveness of price changes alone by exploiting a unique natural experiment to estimate price elasticities for the same households. The price elasticity of our experimental population is estimated to be -0.14, implying that achieving an equivalent reduction through price changes alone would require an approximately 80 percent increase in price.

Lastly, we provide direct evidence that motivation crowding out may be a real concern in the context of electricity consumption. We show this by providing a second treatment group of households the same peer comparisons, but augmented with additional financial incentives. These financial incentives are effectively a linear increase in price framed relative to the average consumption and thus reward any reductions in consumption. That is to say, we provide a per unit financial reward for households consuming electricity below the population average and a per unit penalty for those consuming more (See Falkinger, 2000 for a theoretical discussion of the same incentive structure and evidence from the lab in a setting where crowding out is not a concern). We find that these additional financial incentives do not improve performance relative to households provided the pure behavioral nudge (as traditional economic theory would predict) but instead result in a significant deterioration of impact. This is consistent with the theory of motivation crowding out or with recent principal agent models in the economics literature that attempt to explain crowding out as a consequence of information asymmetries or reputation concerns (Benabou and Tirole, 2003). Our results suggest some caution in using financial incentives to change electricity consumption behaviors and may help explain the sometimes disappointing impact of similar financial incentives schemes implemented by utilities. For instance Ito (2011) finds that fairly substantial financial incentives offered by utilities to save electricity in California nevertheless had only small impacts in some areas and no effect at all in others.

The existing empirical evidence on household response to prices, norms and incentives make these questions particularly important. A specific intervention that Allcott and Mullainathan (2010) draw attention to is the use of peer comparisons. Unfortunately most of our evidence for their effectiveness comes from the United States and the magnitude of impact is small (though cumulatively significant when aggregated over a large experimental population). At the same time it is unclear how responsive households are to price changes. Reiss and White, 2005 and McRae, 2009 use data from California and Columbia respectively and both find significant heterogeneity in price elasticity with a large fraction of households appearing unresponsive to small price changes. Filipini and Pachauri, 2002 estimate a low price elasticity for urban households in India. In settings where the target populations responsiveness to offered financial incentives is low, it may be the case that financial incentives not only underperform behavioral interventions but could have seemingly perverse negative effects due to crowding out of intrinsic motivation.

These results may also be of some interest to energy policy-makers in India and other developing countries. In India, demand side management of electricity consumption by the urban upper-middle class can be motivated by at least four reasons other than mitigating carbon emissions (over 70 percent of grid electricity generation in India is based on coal). The first of these involves the presence of unpriced health externalities from local air pollution especially from small, unregulated captive units running on subsidized diesel. Such diesel fuel generators are commonly used in India to supplement grid electricity. Muller et al., 2011 attempt to quantify pollution externalities for the United States. Krishnan and Nischal, 2003 provide a more abbreviated analysis for India. The second reason has to do with electricity pricing inefficiencies. India's domestic electricity market is in dire need of pricing reform (Singh, 2006), with a multiplicity of cross subsidies resulting in power that is priced well above marginal cost for some consumers (industry), subsidized to some degree for others (residential consumers) and provided at no cost to a third group (agricultural consumers). Thus reductions in residential electricity consumption can help reduce welfare losses induced by these pricing distortions. A third reason to intervene involves inefficiencies in individual investments in energy efficiency. An extensive literature (Jaffe and Stavins, 1994, Hausman, 1979, Train, 1985) suggests that households systematically underinvest in energy efficiency even where it makes economic sense to do so. Numerous utility run programs in the United States have been motivated partly by this argument (see Gillingham et al., 2006 for a review).

Lastly, crippling electricity shortages and consequent rationing provide a basic reason why reducing residential electricity consumption in rich urban households is likely to be welfare enhancing in India and many other developing countries. A striking reminder of this occured on each of the last two days of July 2012, when over 600 million people in North India found themselves without electricity after three major electricity grids in the country tripped in quick succession, leading to the most widespread blackouts in the country's history. While making headlines across the world, multi-hour blackouts are a daily part of life in much of India, making the most notable part of the July failures their coordinated nature rather than the fact of having no access to grid electricity. Peak demand exceeded supply by 18% in 1996, 13% in 2002 and 13% in 2011 (Central Electricity Authority, 2011, Thakur et al., 2005). Because electricity supply is lower than demand and the difference is met by somewhat arbitrary rationing that imposes high welfare costs on those denied power, the potential economic benefits of any reduction in consumption could be very large.

The remainder of this paper is divided into four parts. In Section 2 we outline a theoretical framework through which to interpret our experiment and results. In Section 3 we describe the experiment design in detail. In Section 4 we provide results and we conclude in Section 5.

2 Incentives, Information and Behavior

Let us begin by noting that households in this study respond to three types of incentives or motivating factors. (i) The underlying electricity prices, (ii) a targeted reward providing a continuous financial incentive to reduce consumption, and (iii) peer group comparisons.

Let households attempt to maximize a utility function $u(e, X \mid t, r)$ s.t a budget constraint $p_e e + X \leq I$ where e is electricity consumed for various services, X is a set of other goods, t is an idiosyncratic type which is not necessarily perfectly observed by the household. Let $f : e, \bar{e} \rightarrow t$ be a mapping from the information contained in peer comparisons (household consumption e and the average consumption of peers $\bar{e}$) to the type t. Let us further assume that when households are not provided peer comparisons they make decisions based on some idiosyncratic belief about their type. p_e is the price of electricity and I is income. r represents financial incentives present in a contract that households may be offered related to their electricity consumption. In the absence of any such contract, $r = 0$ for all households. In general therefore, optimal household consumption of electricity $e^* : \frac{\partial u}{\partial e}|_{t,r} = \lambda p_e$will depend on the nature of the incentive r, knowledge of ones type t and prices p. Note that in setting up this general situation we make no assumptions on how t, r affect electricity consumption. For instance e^* may not change continuously in either t or r, and indeed there is some evidence that motivation crowding out has as much to do with the presence or absence of financial incentives than their level (Gneezy and Rustichini, 2000b) and therefore involves inherent discontinuities.

From an operational point of view, it is easier to think about the effects of incentives, prices and peer comparisons (information) through examining what they might do to the household demand curve. We therefore outline a simple model with a flexible functional form, leading to a household demand function for electricity that can be estimated. Households are assumed to maximize an indirect utility function $v(p, I)$ of the following form

$$v = y + \psi e^{-\theta_p p_e}$$

where y is income, v is total utility, ψ contains other parameters of the utility function and p_e is the price of electricity. It is possible from the indirect utility to derive the demand for electricity using Roy's identity so that

$$x_e = -\frac{\partial v/\partial p_e}{\partial v/\partial y} = \psi \theta_p \exp(-\theta_p p_e) \tag{1}$$

We next proceed to introduce the effects of peer comparisons on consumption by expanding ψaccordingly. Let $ln(\psi) == \beta Z + \theta_t \cdot g(r) \cdot m(t) + \epsilon$. Here βZ represents household specific observables (including an intercept). θ_t is a constant, 'type elasticity'. $t = f(e, \bar{e})$ is the type information the household infers when provided knowledge of $\bar{e}$ through peer comparisons. $g(r)$ is an interaction term that changes responsiveness to type information depending on financial incentives r. We will discuss why incentives and type information might matter in this situation but at its simplest this formulation allows for knowledge of peer consumption (knowledge of t) creates a motivation to change demand and also allows the degree to which this matters to depend on whether households are offered financial incentives as well. In other words $g(r)$ captures the crowding out effects of incentives on behavior. The third element in the demand equation of interest is the price p_e. Clearly the underlying marginal price of electricity will change household consumption behavior. However the underlying marginal price paid may also depend on what financial incentives are offered.

In the case of our experiment we provide a financial incentive contract (described in more detail in Section 3) that is equivalent to an increase in marginal price of electricity. Households offered financial incentives in addition to information were rewarded (penalized) when consuming less (consuming more) than the group average at the rate of 2Rs per unit for grid electricity and 4Rs per unit for diesel electricity. The level of reward r will therefore enter the marginal price term as well so that $p_e = p_e^u + r$ where p_e^u is the underlying utility price and r is the financial incentive level[1]. The incentive r thus has a behavioral effect on the response to information (captured by $g(r)$) and a direct financial effect similar to the response to price.

We are left with a log-linear demand equation of the following form,

$$ln(x_e) = \beta Z + \theta_t \cdot g(r) \cdot t - \theta_p(p_e^u + r) + \epsilon \qquad (2)$$

Of course since the mechanism by which households derive information from peer comparisons (or otherwise react to r) is not known, we will use the experimental data to estimate the most general parameter of interest, namely average treatment effects across groups. The demand function above is helpful to visualize one way in which the three policy interventions of interest, namely price, peer comparisons and additional financial incentives can be expected to change consumption. Before describing the experiment we conduct, we quickly review what we know about the mechanisms through which these interventions might act.

[1]Note that while it is possible that households respond differently to the additional reward r than they do to underlying prices, our assumption is conservative since we now must show that the effects of financial incentives due to their direct price impact are in fact consistent with what we know about household price elasticities.

2.1 Peer Comparisons

Different mechanisms have been proposed for why knowledge of one's type should change behavior (in this case consumption). One explanation, staying within the psychology literature, is that households seek to adhere to 'injunctive norms', which are prescriptions of behavior regarded socially acceptable or normal. On this basis, when households are informed that they are consuming at a level very different from the average, they incur a psychological cost. In order to minimize this cost, they shift behavior to approach the norm. Depending on whether the norm being enforced involves consuming less than the average (where consumption is regarding as a moral 'bad') or consuming close to the average (where the injunction being created is to consume the socially acceptable amount), one might explain why households who are consuming less than average either (i) do not respond or (ii) increase consumption. A number of studies in the psychology literature use this type of framework, and Frey and Meier (2004), Alpizar et al. (2008) take a similar conceptual framework within the economics literature.

A second line of reasoning can be located more closely within classical economic theory. Here we may view the peer comparison as providing a useful piece of information, enabling households to better optimize their own consumption conditional on this signal. Consider a simple model where households attempt to produce an optimal amount of energy conservation behavior, but are uncertain about parameters of their own production function. They may choose to invest resources in learning about the parameter but the value of such information is bounded and therefore some uncertainty is likely to remain in the status quo. However, provided households regard their peers as having production function parameters drawn from the same family (a definition, in a sense, of being a peer), information about the average behavior of the population can be used to infer the value of one's own production function parameters. In turn, this can enable households to re-optimize and over time learn their true production function. This explanation suggests a reduction in the variance in consumption owing to learning. Note that learning from peers has been observed in other contexts as the basis for behavior change (see for example, Conley and Udry, 2010, Cai et al., 2009).

Peer comparisons and social norms have been studied with much interest for their effectiveness as a behavior change tool. Applications have ranged from charitable giving (Frey and Meier, 2004) to modifying retirement savings behavior (Beshears et al., 2011). In the context of electricity consumption, large randomized trials have been implemented in the United States by the company O-Power, using peer comparisons to try and reduce electricity consumption (Allcott, 2011a provides one evaluation of these programs finding small reductions that are nevertheless significant aggregated over a large population). Unfortunately little evidence exists on whether peer comparisons can be effective in changing energy behaviors in less affluent, developing country contexts, and one small contribution of this paper is to add to this evidence.

2.2 Financial Incentives

There is a relatively scarce amount of evidence from the field on crowding out due to financial incentives, with a much larger body of work showing this using laboratory experiments[2]. A useful review of the latter is Camerer and Hogarth, 1999. More recently, Gneezy et al., 2011 look more broadly at how financial incentives have worked in the field. In thinking about what we know about incentives and motivation, it is helpful to differentiate between two types of behavior that have been the subject of much of the research in this area. The first involves actions that are pro-social (charitable giving being a common example). Field evidence on motivation crowding out following the use of incentives tends to have occurred when pro-social actions are encouraged using incentives (see Gneezy and Rustichini, 2000b and for a review of the evidence on pro-social behaviors, see Meier, 2007).

It is possible to explain why incentives may have a negative effect on observed pro-social behavior, while staying within the framework of standard principal-agent economic models, by appealing to reputation effects. Benabou and Tirole (2006) provide a detailed discussion but to summarize briefly, because pro-social behaviors carry reputation benefits (with external observers who update beliefs about agents undertaking such behavior), it is possible that financial incentives might reduce motivation to undertake them, if such rewards also reduce reputation gain. Thus for any observable action (charitable giving, blood donations) the presence of a financial incentive might change the assumptions that external observers make about the motivation behind undertaking the behavior in question. For agents offered a contract with incentives therefore, it becomes necessary to balance out the extrinsic benefit from the incentive directly, with the reputation loss suffered in the eyes of observers.

Another class of behaviors however, are not observed by third parties and do not involve reputation benefits. Much less evidence exists on whether incentives can reduce performance in such situations. One example comes from Frey and Oberholzer Gee (1997). Another well known example is provided in Gneezy and Rustichini (2000a), where parents at a day care reduced the extent to which they were on time when a fine was instituted for delays.

In theory, behavior that is not pro-social may nevertheless be negatively influenced by incentives and again this may be explained with the framework of a classical principal-agent model (see Benabou and Tirole, 2003 for an excellent theoretical overview). In order to do so, it is necessary to assume that the agent who is offered a contract does not have perfect information over costs and benefits and that the agent believes the

[2]Concerns with extrapolating from lab experiments include questions around behavioral similarities exhibited by experimental subjects (typically students from the United States) when compared with a broader population (see a well known critique in Henrich et al., 2010). Second, a critical difference between most real world financial incentives and laboratory experiments are the durations involved. It is plausible that long term real world interventions allow subjects much more time to think about the tradeoffs they face and act accordingly, while short duration laboratory experiments simply do not allow the same learning responses.

principal may have information that she does not. Given uncertainty on the part of the agent and a possible asymmetry of information, the presence of an incentive may signal bad news about the actual benefits or costs involved in the behavior that is being rewarded. In other words, the agent may reason that if a large incentive is offered, this may indicate that the principal has information suggesting the action required will be costlier than anticipated and is therefore attempting to sweeten the pot. In turn, the agent may rationally update her prior beliefs about the costs of the activity involved (or the benefits) and for certain values of the incentive the resulting equilibrium may therefore involve a reduced probability of agent effort.

The setting in this paper is one where incentivized actions are completely invisible to observers and there is no good reason to believe reputation concerns could play a major role. Instead, if incentives still negatively impact performance, the channel for action may instead have to do with opinions formed by households about the individual or public benefits of electricity conservation behavior after being informed that the billing agency is willing to reward them for this. This is important because the billing agency in this experiment is exactly analogous to utilities in more general situations. For the most part, energy efficiency incentive programs in the United States have been implemented through utility companies and it is interesting to ask whether the relationship between utility and consumer is one that makes motivation crowding out in response to incentives more likely.

2.3 Electricity Prices

In a meta survey Espey and Espey, 2004 found that the literature contains estimates of short run price elasticities ranging between -2.01 to -0.004 with a mean of -0.35. The truism that money does matter is well reflected in electricity markets. Having said this, significantly more ambiguity exists as soon as we ask more complicated questions about the nature of electricity price response. Estimates of the magnitude of price elasticities vary widely, including some studies finding elasticities quite close to zero. While much of the literature on electricity demand comes from outside India but Filipini and Pachauri, 2002, Bose and Shukla, 1999 provide some results for India, suggesting that domestic urban consumers have fairly low elasticities.

In addition, while consumers certainly do notice prices, a significant literature in electricity consumption suggests that the nature of household response to financial incentives is not straightforward. There is evidence of substantial heterogeneity in how consumers respond to prices and in particular a significant fraction of the population seems unresponsive to small changes in price (see price elasticities from Reiss and White, 2005 for examples from California and McRae, 2009 for data from Columbia). There is also evidence that consumers respond not to marginal prices (as would be economically most efficient) but to average prices (Ito, 2010,Shin, 1985). This in turn may dampen the impact of non-linear price structures on consumption.

Allcott (2011b) in a study of a real time pricing experiment in the United States, finds that demand falls in high price hours but does not increase in off peak hours where prices were reduced. Wolak (2011) finds that critical peak pricing programs that include a rebate generate significantly lower price responsiveness than peak pricing with no rebates, even though the marginal prices in both cases are identical. (Ito, 2011) finds mixed evidence of the impact of financial incentives offered by utilities to save electricity in California with small impacts in some areas and no effects at all in others.

Overall the literature on electricity price elasticity suggests that while people do respond to moderate price changes, they do not necessarily respond optimally or very strongly. In settings where the price response is low, the responsiveness to financial incentives may not be high enough to overcome any potential impact they have on intrinsic motivation.

3 Experiment Design

Much of the growth in residential electricity demand in India has come from the boom in new urban construction, around India's overcrowded cities. Urban per capita consumption is nearly three times that of the rural domestic sector, in-spite of generally higer tariffs. The residential sector is also the fastest growing electricity sector in India (Central Electricity Authority, 2010). The 2011 census reported over 31 percent of the population living in urban areas, a figure dragged down by the less developed states of the north[3]. This figure is expected to grow quickly. Indeed, the McKinsey Global Institute (Sankhe et al., 2010) estimated that the urban population is likely to rise by over 200 million over the next 20 years.

The capital city of New Delhi is a good example of this nationwide trend. The city and its surrounding suburbs form the second largest urban agglomeration in the world. Over 22 million people live in the so called National Capital Region (Delhi and satellite cities) a figure that is expected to rise to 32 million by 2025 (United Nations, 2012). The dominant form of new housing in the National Capital Region (NCR) are apartment complexes constructed by the government or by private real estate firms and construction companies. These developments range from a few hundred to a couple of thousand living units in size. Individual apartment units are sold or rented to families. The relative lack of good public infrastructure, police and transport services and the fact that many of these areas have relatively high crime rates[4], make this type of gated community a popular choice for both real estate developers and consumers. Within such apartment complexes, internal security and various public services can be privately arranged. These and

[3] With the exception of Andhra Pradesh all states in the south of the country had urban populations greater than 35 percent. Tamil Nadu has a 48 percent urbanization rate

[4] In a June 2011 article, The New York Times looked at Gurgaon, in Delhi's neighboring state of Haryana, a city that is fairly representative of the development trajectory of the NCR as a whole. In describing the city the author Jim Yardley was moved to write: 'Gurgaon...would seem to have everything except...a functioning citywide sewer or drainage system; reliable electricity or water; and public sidewalks, adequate parking, decent roads or any citywide system of public transportation.'

other maintainence services are typically provided by the builder and paid for by all residents on an ongoing basis.

3.1 Experiment Location

The experiment described here involved households located in an apartment complex in one of the urban satellite cities of Delhi (Indirapuram in Ghaziabad district)[5]. The development consists of over 700 separate apartment units spread over a complex with 15 highrise towers. This community is one among several such in the neighbourhood with many others under construction. One of the single most important services that can be provided within such closed communities is the provision of backup electricity. The use of captive diesel based power generating units has become ubiquituous in these contexts. Crippling grid electricity shortages and regular blackouts have resulted in high demand for some form of self generation to cope with North India's intense summers. Figure 4 makes this point quite starkly, showing the fraction of time households in our study had no supply of grid electricity. Since power outages are distributed across the entire region, these figures are representative of the situation in Indirapuram more generally (although the situation may be worse in less developed parts of the town). These numbers are also likely to be fairly similar in other towns across India which have similar electricity shortages.

The community in question is similar in style to other new construction in the NCR and appealing from an experimental point of view. There are only four types of living units available - 2 bedroom units, 3 bedroom units, 4 bedroom units and a few larger penthouse apartments. The majority of apartments are either two or three bedroom designs and our experiment targets this subset of homes. All households in experimental population face identical weather conditions and construction designs are homogenous. Furthermore, at the time of purchase households were offered appliances pre-installed by the builder including air-conditioners in each bedroom. This has become an increasingly common practice, although typically opting out is possible at the time of purchase. This fact, alongside the homogenous construction styles, mean that appliance stocks are likely to be similar across homes. Parenthetically it is worth remarking that these appliances are not the most energy efficient models available and this observation in itself flags an interesting option for policymakers since changing the type of default choice offered by builders could significantly influence new appliance stock.

Households may also be fairly homogenous in terms of income or wealth, since all homes were sold or are rented at similar prices (the monthly rent for 3 bedroom flats is about Rs. 15000, or 300 dollars). The

[5]On May 2 2012 the leading business daily in India, The Economic Times, reported that over 500,000 housing units were under construction in the NCR alone in May 2012 (based on a market overview by the international real estate firm, Knight Frank). A third of these were located in Ghaziabad district. See http://articles.economictimes.indiatimes.com/2012-05-02/news/31538282_1_ncr-market-greater-noida-residential-market

similarity in construction sizes also mean that family sizes may be fairly similar. A survey will be conducted shortly to verify some of these assumptions, and they do not play a direct role in my results, but I mention them to highlight how large scale housing with similar unit designs, may encourage clustering of residents by certain observable lifestyle characteristics.

The fact that the experimental population is fairly homogenous is important for two reasons. First, one might worry that if houses, appliances and household characteristics are similar then electricity use must also be very similar, potentially reducing the impact we might gain from peer comparisons. As it happens there appears to be significant variation in electricity consumption across households suggesting that unobservable and behavioral differences may drive a fair share of variance in consumption. Second, these population similarities influence the generalizability of our results regarding the impact of social norm interventions. The popularity of such standalone housing complexes across the country provide a large population for which external validity might be a small concern. However extrapolating these results to older metropolitan cities (where the bulk of middle-class housing consists of standalone units which differ greatly on observables), may be more difficult.

From the point of view of studying electricity demand, gated complexes provide a particularly interesting testbed. In many of these urban developments electricity billing is not directly carried out by the state utility board[6]. Instead, all power supplied to the development as a whole is metered and charged to the property developer / maintenance company. In turn, the maintenance company can recover these costs from households in whatever way they wish. Among the more common arrangements, also the one present in IC, is for the maintenance company to set up internal metering and billing at the household level. Typically a flat tariff is charged as part of this internal billing.

As mentioned earlier, grid electricity supply in India is unreliable with frequent extended powercuts (see Figure 4, and note that this is a community in the National Capital Region, where infrastructure is generally better than elsewhere in the coutry). This means that the majority of such housing complexes invest in backup power to some degree, normally generated through captive diesel units. What does differ is the degree to which this backup power provision is a perfect substitute for grid electricity, with some builders providing backup power sufficient only for light loads and other developments investing in larger amounts of self generation. The IC community was chosen in part because it provides a complete backup using multiple captive generation units, producing sufficient power to allow all households to run multiple two tonne split airconditioning units simultaneously. Thus the backup power in IC provides a perfect substitute for grid electricity, with the exception of the tariff.

[6]This varies depending on the size of the community and the utility in question, and smaller communities do tend to have direct billing.

The two power sources (grid electricity and diesel generation) are separately metered at the household level and backup diesel power is about 4 times as expensive as grid power (Rs. 12.10/KWh versus Rs. 3.2/KWh). Because the price difference is so large, households need to know what rate they are consuming at, and therefore are provided with a red warning light in the home when backup power is being used. In communities smaller than the one we study, this type of system is sometimes skipped, normally because when captive diesel units are running they are also audible. In other words, thanks to captive power, many households in urban India are already paying for electricity under a dynamic pricing regime (albeit with limited price variation), with higher prices being prevalent at times of grid power shortage.

3.2 Experimental Intervention

The experiment timeline is summarized in Figure 3.Two separate interventions were conducted in experiment households with the aim of understanding how two different types of policy tools - those using information and behavioral incentives, and those using financial incentives - work in this context. From the set of over 700 apartments in IC, occupied two and three bedroom apartments were identified. Baseline data was collected for these homes spanning a 20 day period. Apartments were randomly assigned to two different treatment conditions. 124 households were placed in what we will call the Information Treatment. 240 households were placed in what I will call the Incentives+Information Treatment. 124 homes formed controls. Randomization was stratified by household size (two or three bedrooms).

Information The information only group was sent a weekly reportcard (Figure 1) detailing their own electricity consumption for the past week for both grid (utility) and diesel generated (backup) power. This was compared to the average consumption in other households of the same size (where the relevant averages when calculated were based on similar households in the same treatment condition). The weekly reportcard also contained a general set of tips and recommendations to save energy on the back. Report cards were delivered by the electricity billing agency (maintenance company).

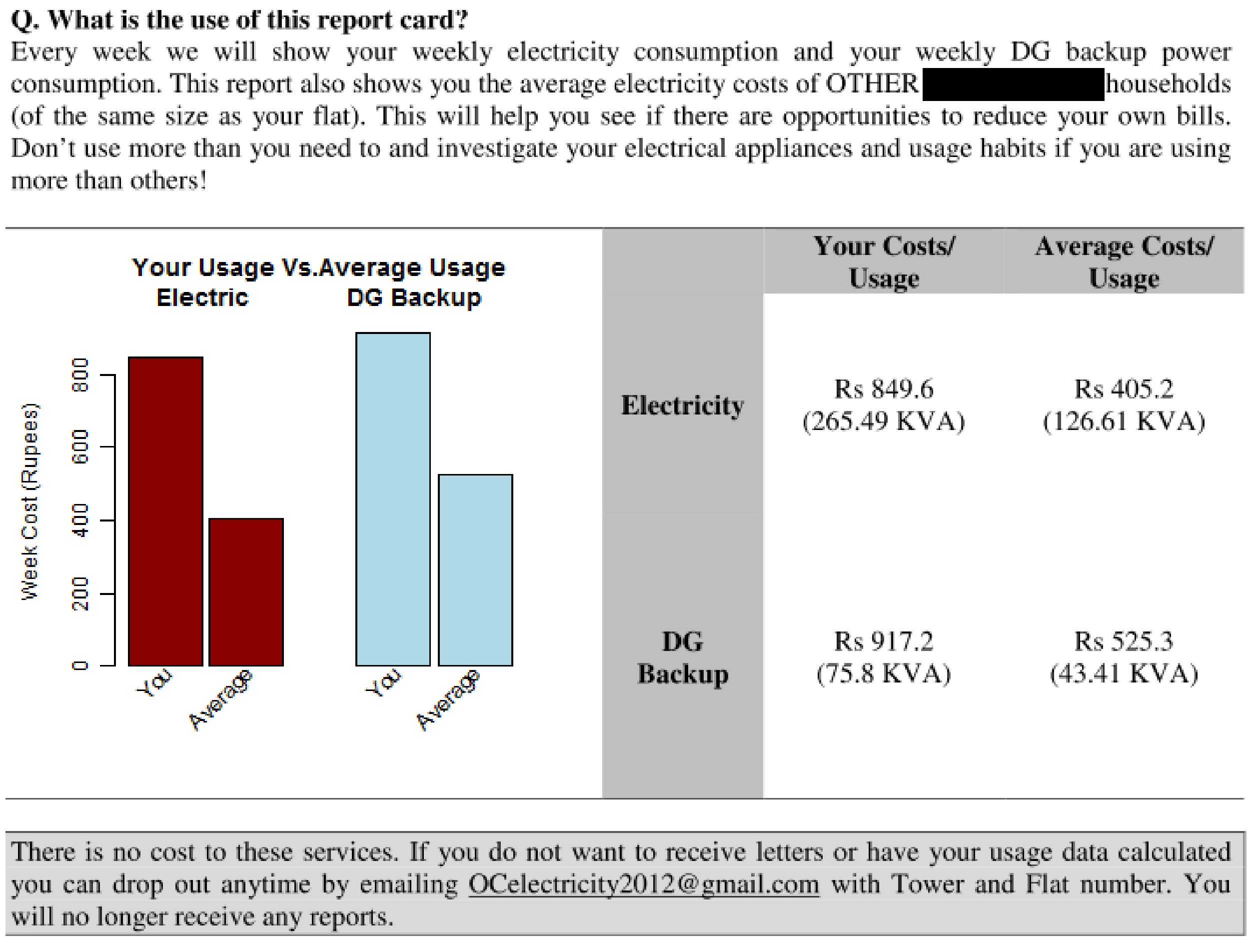
Information+Incentives The incentives group was sent a similar reportcard detailing their electricity consumption and the average in other similar households (Figure 2). In addition however they were enrolled in a reward scheme which functioned as follows. Every household in the incentive group was provided a starting reward balance of 750 Indian Rupees (about 15 US dollars). Thereafter, for the remainder of the experiment, households could add to or subtract from their reward balance depending on whether their weekly consumption was above and below average. When consuming less (consuming more) than the group average, their reward balance was increased (reduced) at the rate of 2Rs per unit for grid electricity and 4Rs per unit for diesel electricity. In effect this simply creates an additional financial incentive to reduce consumption exactly equal to the per unit reward rates. As an example, if household A were to consume 10 units of electricity and the average consumption that week were 20 units, a reward $r = 2 \cdot (20 - 10) = 20$ would accrue. Note that these incentives are significant as a fraction of the per unit price, especially for grid electricity but the actual transfers are still not particularly large even taken over the course of the entire summer. This partly owes to the fact that the incentive applies only to the difference between household consumption and the peer average.

Households could not lose more than their starting reward balance over the course of the experiment, meaning that towards the end of the study a small number of homes (40) had entered a region of zero financial incentives (while still receiving the same information). The specific incentive program described here is closely related to the mechanism described in Falkinger (2000). The structure of this type of incentive mechanism, lends itself to certain attractive theoretical and practical qualities and can be shown to achieve first best efficiency at the right level of incentive. While marginal incentives are identical to a tax, in its unconstrained form this incentive rule is also budget neutral. This is because transfers to households being rewarded exactly equal transfers from those being penalized. In addition the mechanism possesses the politically attractive quality of an endogenously determined baseline, mitigating the accusations of arbitrariness that tend to accompany exogenous targets or standards. Last, and perhaps most important, this incentive structure directly aligns itself with the comparison being made in the pure information treatment.

Control Control households were never contacted but their electricity consumption was recorded over the course of the project.

Report cards were delivered on a weekly basis through the mail. The first report was delivered at the start of May and continued through to the end of the summer months (July end). Note that the summer of 2012 was the hottest in Delhi for 33 years, and we are therefore seeking to find treatment effects in conditions which make it rather difficult to reduce consumption. Electricity use is dominated by cooling requirements with most households in IC possessing multiple air conditioners (as is increasingly common for India's growing middle and upper class population). Towards the end of July temperatures dropped suddenly, and so did electricity consumption, with the advent of a delayed monsoon season.

The assignment to each of the treatments was optional with households able to drop out at any time over the period of the study. Only three households chose to do so (in the first two weeks) and no others thereafter. A few other homes were dropped from the study because baseline electricity consumption data suggested they were not occupying their apartments during the entire baseline period (or at least using no electricity). Thereafter, 119 (out of 124) households remained in the information treatment, 233 (out of 240)



Q. What is the use of this report card?
Every week we will show your weekly electricity consumption and your weekly DG backup power consumption. This report also shows you the average electricity costs of OTHER [redacted] households (of the same size as your flat). This will help you see if there are opportunities to reduce your own bills. Don't use more than you need to and investigate your electrical appliances and usage habits if you are using more than others!

	Your Costs/ Usage	Average Costs/ Usage
Electricity	Rs 849.6 (265.49 KVA)	Rs 405.2 (126.61 KVA)
DG Backup	Rs 917.2 (75.8 KVA)	Rs 525.3 (43.41 KVA)

There is no cost to these services. If you do not want to receive letters or have your usage data calculated you can drop out anytime by emailing OCelectricity2012@gmail.com with Tower and Flat number. You will no longer receive any reports.

Figure 1: Report card format sent to Information Only (Peer Comparison) households

in the incentives treatment and 121 (out of 124) households were controls.

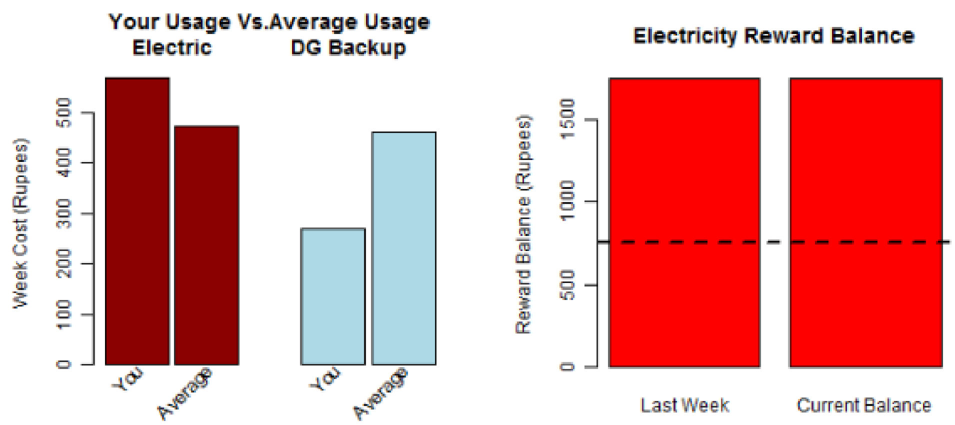
In addition to the control group created during random assignment, electricity consumption data from a further 50 households who did not receive either of the treatments is available to us. These homes were not originally not considered for random assignment into one of the two treatment conditions because electricity records and occupancy status were temporarily unavailable for administrative reasons at the start of the experiment. Since there is no reason to believe that these 50 homes differ in any way from the rest of the sample (we confirm that electricity consumption and all observable characteristics of these households are statistically identical to the rest of the control group) we report results both using the original control group and with an expanded control group including these 50 households. Our results are statistically significant and quantitatively similar in both cases, and comparing results with the expanded control is helpful as a robustness check.

There is no cost to being in the reward programme. After October you will receive a cheque with your reward balance. If you do not want to receive letters or have your usage calculated you can drop out anytime by emailing [redacted] with Tower and Flat number. You will no longer receive any rewards or reports.

Q. What is the use of this report card?
Every week we will show your weekly electricity consumption and your weekly DG backup power consumption. This report also shows you the average electricity costs of OTHER [redacted] households (of the same size as your flat). This will help you see if there are opportunities to reduce your own bills. Don't use more than you need to and investigate your electrical appliances and usage habits if you are using more than others!

Q. What is the electricity rewards programme?
It's simple. **As a selected participant you begin with a balance of Rs 750!** Every week, if you use LESS electricity or backup power than the average of other residences your reward balance will increase (up to a maximum balance of Rs 5000.00). If you use MORE than average your reward balance will go down (minimum balance of Rs 0.00).

For electricity you gain Rs 2.0 for every unit that you are BELOW average (and lose Rs 2.0 for every unit ABOVE average). For DG backup you gain Rs. 4.0 for every unit you are below average and lose Rs 4.0 when above average. eg: If average electricity used is 50 KVA and your use is 30 KVA your reward balance will increase by 2*20=40 Rs.



	Your Costs/Usage	Average Costs/Usage	**Reward Balance** (Starting Balance: Rs. 750)	
Electric	Rs 568.6 (177.68 KVA)	Rs 470.8 (147.12 KVA)	Last Week Balance	Current Balance
Backup	Rs 270.2 (22.33 KVA)	Rs 459.4 (37.97 KVA)	Rs 1747.3	Rs 1748.74

Figure 2: Report card format sent to the group of households provided Financial Incentives and Information

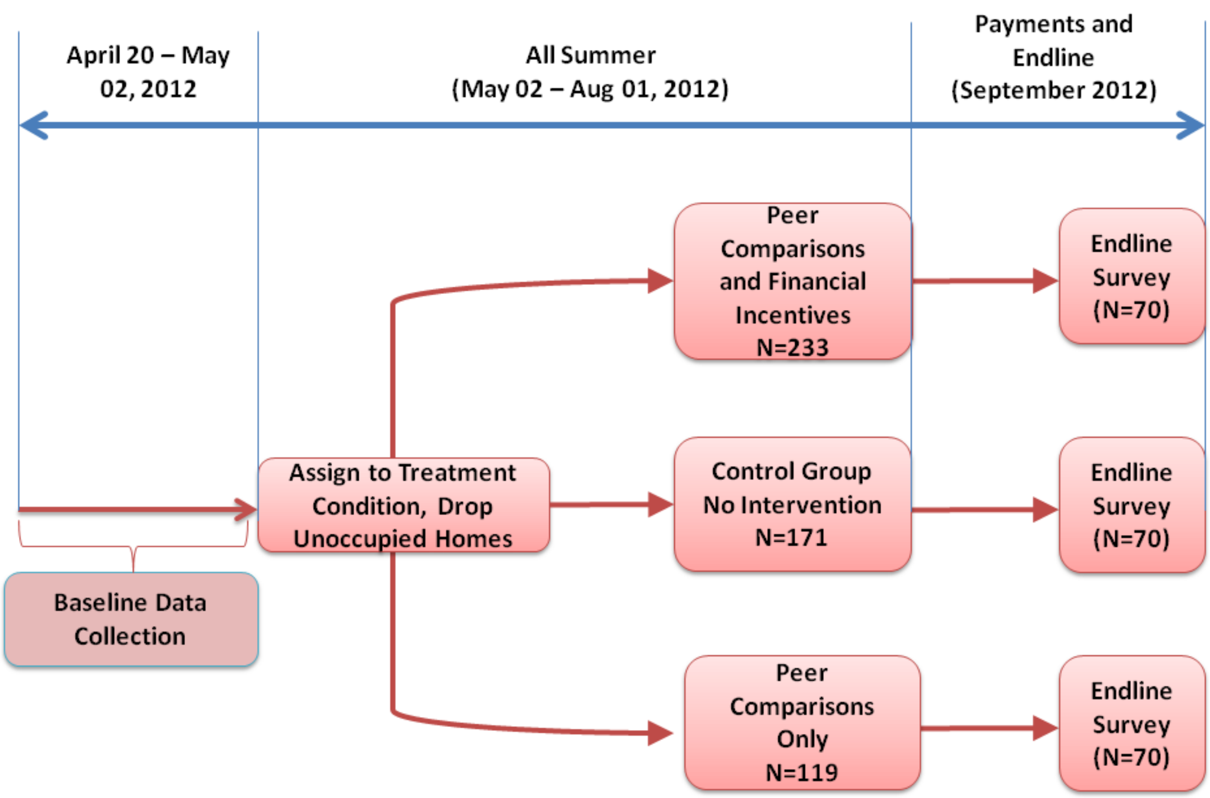


Figure 3: Timeline of experiment

4 Results

We present results in two parts. First we estimate average treatment effects for the peer comparisons (information treatment) and the combination of peer comparisons and incentives on grid supplied electricity. Next we present evidence that price elasticity is low, exploiting the fact that households also consume high cost electricity (diesel generated backup power) during the fairly common power blackouts that occur. A low price elasticity is important not only because this means that the impact behavioral intervention (if effective) cannot easily be achieved by a modest price increase, but it also makes it more likely that crowding out will be an issue since this presumably implies that the extrinsic motivation of financial incentives is not very high.

4.1 Treatment Effects

Treatment effects may be estimated using a simple reduced form equation of the following type

$$Y_{i,t} = 1 + \beta_1 T_1 + \beta_2 T_2 + \gamma_t + \delta_i + \epsilon_{i,t} \tag{3}$$

where $Y^E_{i,t}$ is the electricity consumed in time period t by household i, $\beta_{1,2}$ are the treatment effects associated with receiving either the information treatment or the combination of information and incentives treatment. γ_t, δ_i are time and individual fixed effects (to allow for completely general heterogeneity in household specific intercepts and time shocks) and and $\epsilon_{i,t}$ are unobserved shocks. T_1 and $T_2$are dummy variables corresponding to the two interventions: (i) Peer Comparisons Alone (Information) and (ii) Information with Financial Incentives. Note that electricity consumption data was recorded aggregated over two day intervals (referred to as a time period) and not on a real time basis. Table 1 summarizes our results. All standard errors are robust and clustered at the household level (Arellano-Bond) to account for the possibility of serial correlation in unobserved shocks.

A more direct way of testing whether the addition of financial incentives to the vanilla behavioral intervention results in a statistically significant deterioration can be had by re-framing the reduced form equation we estimate as follows

$$Y_{i,t} = 1^J + \beta_1^J T + \beta_2^J I + \gamma_t^J + \delta_i^J + \epsilon_{i,t}^J \tag{4}$$

Here T is a dummy variable that is 1 for all households treated with any intervention (that is for Information and Incentive groups pooled together) and I the interaction of the treatment dummy with a dummy representing the presence of incentives. If β_2, the coefficient on I is found to be positive and significant

	Original Control	Expanded Control
Information Treatment	-3.50 [1.767]**	-4.614 [1.707]***
Information+Incentive Treatment	0.235 [1.534]	-0.878 [1.467]
HH Fixed Effects	Y	Y
Time Fixed Effects	Y	Y
Mean Daily Consumption (KWh)	20.72 [14.21]	20.80 [14.40]
Information Treatment (% of Daily Mean)	-8.28 [4.16]**	-10.87 [4.01]***
Information+Incentive Treatment (% of Daily Mean)	0.55 [3.61]	-2.05 [3.44]
No. of Households	466	516
No. of Time Intervals	39	39
Significance Codes: '***' 0.01 '**' 0.05 '*' 0.1		

Table 1: Coefficients in rows 1 and 2 are marginal effects from a linear model. Rows 3-5 provide mean treatment effects in percentage terms. All standard errors are robust to serial correlation. Expanded control is as described in Section 3.2.

we may conclude that adding financial incentives to peer comparisons reduces household effort and leads to a deterioration in treatment effects consistent with motivation crowding out. We obtain $\beta_2 = 3.73$ ($p = .018$).

4.2 Price Response

The responsiveness of households to price changes is important both from a policy point of view (comparing the effectiveness of different interventions) and in order to understand whether motivation crowding out is likely to occur in this setting. Theory suggests that financial incentives are most likely to result in a significant and observable reduction of effort in settings where small monetary gains are not in themselves a strong motivator. Settings where intrinsic motivation is significant but extrinsic motivation is low are precisely the type of situation where motivation crowding out is a significant concern.

As we have reviewed in Section 2, there is some indirect empirical evidence that policies to change electricity consumption behavior are operating in this type of setting. We strengthen this assumption by exploiting a unique natural experiment to directly estimate price elasticities of the experimental population showing that they are fairly low. Indeed it is possible that even these modest estimates may over-estimate price responsiveness since to estimate price elasticity we exploit variations in price that are significantly larger in magnitude than the financial incentive we actually provide.

Recall that households in the experiment are subject to frequent and recurring blackouts (common in much of urban India, see Figure 4 for an estimate of magnitudes). During a period of time when grid power is unavailable, they consume diesel generated backup electricity. Grid electricity is billed at a lower price (3.2 INR per KVA) and backup power is significantly more expensive (12.10 INR per KVA). Since metering for the two electricity sources is separated, we are able to observe consumption disaggregated for both types of power. To begin with, one might compare average hourly consumption under low priced (grid) electricity and high priced (diesel) electricity to see whether households change consumption behavior significantly. Doing so gives us the result that average hourly consumption of grid electricity is 0.87 KVA while under the higher priced backup power average hourly consumption is actually higher, 0.93 KVA. This does not indicate that people actually increase their electricity intensity when they pay more, but instead reflects the fact that although power outages are frequent and extended, they nevertheless do not occur with equal likelihood at all times of day and are likely concentrated during times of high demand. It is therefore not possible to directly compare average consumption levels under the two price regimes to estimate elasticities, and we do not observe electricity consumption by time of use. We therefore take a different approach to estimate price elasticity.

Consider a reduced form equation describing price response as follows,

$$y_{i,T} = 1 + \gamma_T + \delta_i + \theta P + \epsilon_{i,T} \tag{5}$$

where T represents a period of time over which we observe cumulative electricity consumption for household i, $y_{i,T}$. γ_T, δ_i are fixed effects for every time period and ever household allowing us to control in a completely general fashion of household specific heterogeneity and time variation shocks to consumption (including temperature, rainfall etc) that affect the experimental population as a whole. P represents the number of hours (or minutes) in this period that high priced power was being consumed. A random source of variation in P (hours on high priced electricity) would thus allow us to estimate θconsistently. At first glance though, since all households are located in exactly the same small geographic area, it is hard to see why any variation in P should exist since we might expect that at any given time all households in the same complex would be either consuming grid electricity or backup power.

Fortuitously thanks to a unique natural experiment we do in fact have access to precisely this type of variation in our data. It so happens that grid power supplied to houses is sourced from not one, but two separate distribution substations. Supply to about half our experimental population is linked to one node of the transmission grid and the remainder of apartments are linked to another. Power blackouts in the area occur often and for significant durations. They follow no systematic announced schedule, occur without prior warning and are distributed across the hundreds of substations operated by the state utility. Thus while on average the two distribution substations located in exactly the same area supply power for a similar fraction of time it is not necessarily the case that blackout periods are synchronized. Over any two day period therefore, the time spent by half the households in our population on high priced backup power may differ from the other half. In effect therefore, for any observed period of time, households are as if randomly

	Estimate	Std. Error	Pr(> |t|)
(Intercept)	93.40	5.55	0.000
DGhours (θ)	-0.356	0.151	0.018
HH Fixed Effects			Y
Time Fixed Effects			Y
Price Elasticity			-0.14

Table 2: Price elasticity calculated using estimated marginal change in consumption for an additional hour of high priced power (θ) combined with average hourly consumption.

assigned to two different regimes differing in the length of time on high priced power. An estimate of θ thus provides a consistent, unbiased estimate of the average response to a unit increase in P, the time spent consuming electricity at the higher tariff.

We estimate coefficients in 5 for the set of those two day periods where the difference in time spent without grid supply in the two feeders was at least two hours in total. Note that aggregating over time (even for a few days) loses us much of the real time variation that exists in the power supply situation in each of the two feeders, because long run average power outages are similar across both. Even so, for 9 separate two day periods distributed over the summer, one half of households received electricity for at least two fewer hours in total than the other half. Table 2 summarizes our results (omitting fixed effects). Two facts seem clear. First, households do respond to price and in the expected way. The coefficient on being exposed to an additional marginal hour of high priced power is negative. Second, even though the response to price has the expected sign the magnitude is small. The implied elasticity works out to about -0.14. It is interesting to observe that this estimate is close to the long run summer season estimates from Filipini and Pachauri (2002) who use a cross-section of national household micro-data from the NSS surveys and conclude that over the summer, urban household elasticities are -0.16.

5 Conclusions

Demand side interventions in residential electricity consumption have a crucial role to play in energy and climate policy across the world. This paper has discussed the relative effectiveness of behavioral and financial incentives in the context of reducing electricity consumption in middle class urban households. Traditional economic policy analysis has focused on the role of interventions that either change underlying prices or use conditional financial incentives to influence the behavior of targeted populations. Nevertheless in recent years interest has grown in behavioral interventions and in the context of electricity consumption one such intervention is the use of peer comparisons.

We have focused on a specific concern that arises when comparing financial incentives (that create an

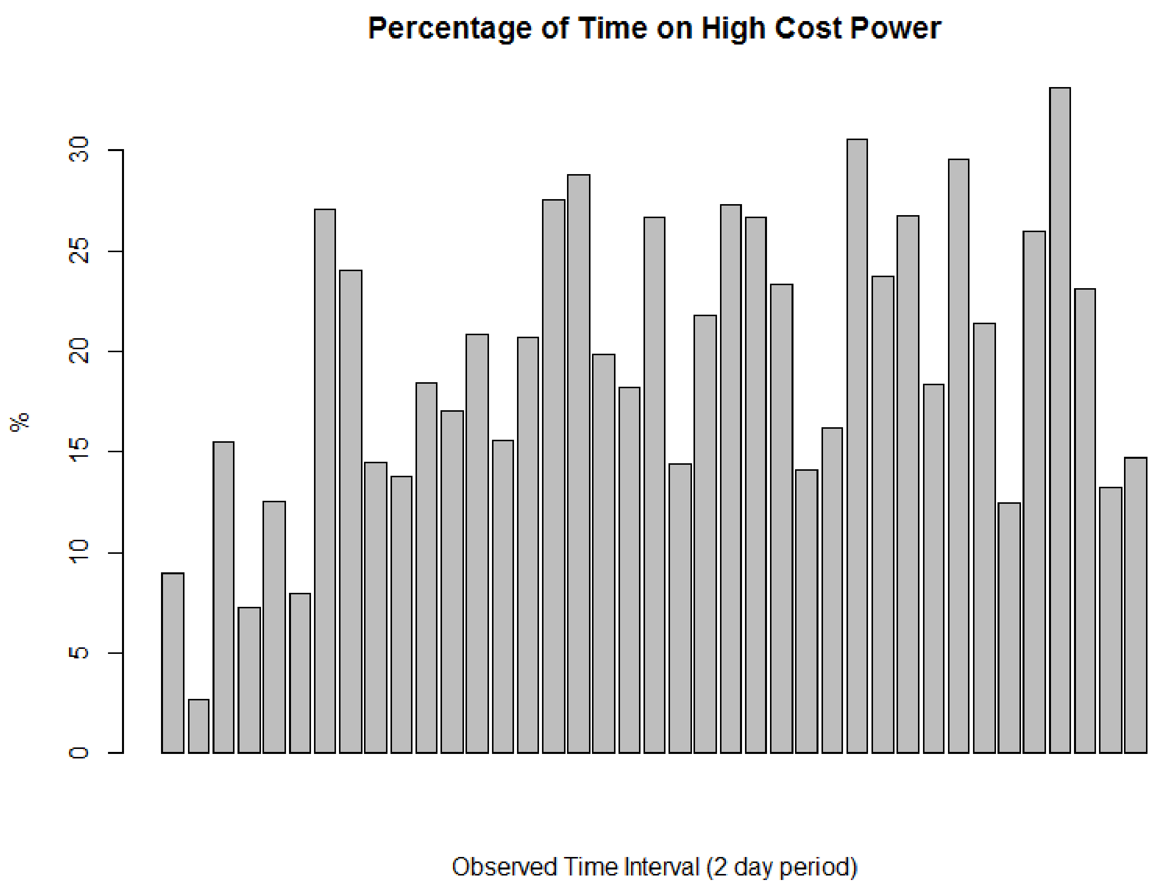


Figure 4: Percentage of time without grid electricity in each successive two day interval (May to July)

extrinsic motivation to change behavior) with pure behavioral interventions and information provision (that may create an intrinsic motivation to change). In settings where price elasticities are low it may be the case that financial incentives not only underperform behavioral interventions but could lead to a significant deterioration in impact owing to crowding out of intrinsic motivation. We test this concern by setting up a carefully designed comparison of a pure behavioral intervention (peer comparisons) with the same peer comparisons now augmented with additional financial incentives. We demonstrate that peer comparisons produce economically significant reductions in electricity consumption taken over the entire course of the North Indian summer season. We further estimate price elasticities for the same sample population and show that they are quite low, suggesting that a price increase of nearly 80 percent would be necessary to achieve the same reductions gained through peer comparisons using price alone. Lastly, we show that the use of a conditional financial incentive leads to a large and significant deterioration in the impact of peer comparisons. This is consistent with the idea that introducing a monetary incentive in this setting can crowd out any intrinsic motivation to change (perhaps created through social norm effects) while at the same time leaving a very low extrinsic motivation. In part this may be because households are not very responsive to a modest price increase. However there are other reasons why the financial incentive may not have been very compelling including the fact that the duration was limited to a single season and because mental accounting and endowment effects may have caused households to value gains and losses associated with the conditional financial incentive differently from their normal expenditures.

In a sense however these shortcomings help us to highlight the potential for a negative impact of such interventions although they do leave open the possibility that differently designed financial mechanisms might be more effective than the intervention tested here. Even so, the potential for motivation crowding out should be expected to remain the same even if the extrinsic motivation were enhanced through different framing or a longer duration. The central message of this paper is therefore, that in at least one highly policy relevant setting (demand side management of electricity) motivation crowding out may be a real concern. We suggest that careful evaluation of financial incentives against other non-monetary policies and testing the design of these incentives to evaluate, for instance, whether impacts change depending on who offers them would be worthwhile. Since one channel through which financial incentives could decrease motivation in our experiment is via households being distrustful of contracts offered to them by their billing agency, it may be worthwhile for policy makers to carefully test whether equivalent programs produce different responses depending on who implements them (federal governments, state and local governments, private utilities, non-profits etc).

Lastly while it is impossible to generalize from one experimental context to another this type of analysis may be worth repeating in other contexts as well. The burgeoning popularity of conditional cash transfers in developing countries (as a tool to change a range of health and education related behaviors) might similarly lead us to worry about the impact of incentives on the intrinsic motivation of targeted populations. While incentives are often very successful, there is also significant variance in their impact (Fiszbein et al., 2009) and many programs have shown small or no impacts on targeted outcomes. Conditional cash transfers often use small financial incentives and are located in settings where we might expect significant intrinsic motivation to perform the incentivized behaviors. In India, for example, a major incentive program, the so called 'Ladli' scheme, seeks to pay households to send girls to school and to delay marriage. Not only is the evidence surrounding the effectiveness of these payments very limited, evaluations have suggested (see Shekher, 2010) that they are further weakened owing to the use of relatively small amounts of money, often difficult to access. This combination, of making payments but keeping them low may well serve to crowd out intrinsic motivations to engage in the right sorts of behaviors (Gneezy and Rustichini, 2000b). Indeed the potentially negative effects of cash transfers on values and motivation have been critiqued (Dreze, 1997, Sunder-Rajan, 2003, Sandel, 2012) although at present very little empirical evidence exists one way or the other. While our results are not directly informative on whether these concerns are legitimate in a different context, a similar experimental design could be usefully replicated in other settings.

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