Journal of Economic Behavior & Organization 97 (2014) 169–184

Contents lists available at ScienceDirect

Journal of Economic Behavior & Organization

journal homepage: www.elsevier.com/locate/jebo

Decision costs and price sensitivity: Field experimental evidence from India

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ARTICLE INFO

Article history:
Received 27 February 2012
Received in revised form 23 June 2013
Accepted 24 June 2013
Available online 2 July 2013

Keywords:
Deliberation costs
Cognitive limits
Social marketing
Cost sharing
Handwashing with soap
Pricing
Behavioral economics
Development economics
Field experiment

ABSTRACT

Poor people often exhibit puzzlingly high sensitivity to low prices of important consumer health goods. This paper proposes decision costs as one explanation: whether a person buys at a price depends on whether she carefully considers the offer, which itself depends on price. A simple model predicts that deliberation costs (1) increase sensitivity to low prices; (2) can prevent cost-sharing from targeting products to buyers with high value; and (3) can have larger effects on poorer people. The principal contribution of this paper is a field experiment that sold hand-washing soap in rural India. Participants were randomly assigned to be offered soap for either a low or very low price, which was experimentally crossed with assignment to a control group or to a treatment that required deliberation. Results matched predictions of the model: the treatment decreased price sensitivity relative to the control group, and increased targeting of product take-up by need.

1. Introduction

Why do poor people often not buy products, such as inputs to health, that are inexpensive, relative to their marginal benefits? There are many possible explanations for high price sensitivity; this paper focuses on one relatively understudied factor that may be particularly important for poor people: decision costs. Deciding whether to buy may sometimes require first deciding whether to *consider* buying. Evidence that economic decision-making can be costly enough to influence economic behavior could have important implications in several fields of economics (*e.g.* Chetty et al., 2009; Chetty, 2011).

This paper first presents a simple, illustrative model in which whether or not an agent should accept an offer is not immediately obvious to her, but she can figure this out by deliberating. If thinking is costly, the agent will not always deliberate, and may ignore valuable offers. Whether the agent buys at a price partially depends on whether she thinks carefully about the offer, which itself depends on the price. Deliberation costs (1) can increase price sensitivity, especially

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[1] I have many people to thank for much (though, of course, errors are my own). Priceton's Research Program in Development Studies and Center for Health and Wellbeing partially funded the fieldwork; Tolani College of Arts and Sciences in Adipur provided support. I thank Abhijit Banerjee, Roland Bénabou, Jim Berry, Anne Case, Angus Deaton, Pascaline Dupas, Thomas Eisenbach, Rachel Glennerster, Tricia Gonwa, Faruk Gul, Karla Hoff, Michael Kremer, Stephen Morris, Simone Schaner, Sam Schulhofer-Wohl, Eldar Shafir, and Marc Shotland. P.M. Thapa assembled the survey team and spared no effort to support the project. Devjibhai got us to the participants and back. Sarita Bangari, Beena Mishra, Mitisha Patel, Meenakshi Sharima, and Shanu Soni actually did the interviews, and did wonderfully. The experiment could not have happened without Diane Coffey. Several hundred Kucchi women have my gratitude.

0167-2681/$ – see front matter
http://dx.doi.org/10.1016/j.jebo.2013.06.012

at low prices; (2) can prevent selling a product (rather than distributing it for free in a social program) from shifting the concentration of product adoption towards users with high value; and (3) can have larger effects on poorer agents.

The primary contribution of this paper is to report a field experiment that sold discounted soap door-to-door in rural Indian villages. Each participant was randomly assigned to one of two prices and to either a control group or a treatment group. In the treatment group, participants answered survey questions that required them to deliberate. Results matched predictions of the model of decision costs: participants with experimentally lowered marginal deliberation costs showed less price sensitivity; among this treatment group, but not in the control group, higher prices caused take-up to be more concentrated among consumers with plausibly higher health-production value for the product.

This paper proceeds in two main parts. First, Section 2 presents a stylized model of consumer choice with deliberation costs. Then, Section 3 describes the field experiment. In the remainder of this introduction, Section 1.1 describes evidence for and treatments of decision costs in the literature; Section 1.2 further introduces the field experiment in the context of other recent experimental studies of consumer pricing among poor people.

1.1. Decision costs and price sensitivity

If thinking and optimizing take effort, then this effort could register as a utility cost and shape how people approach economic problems. Kool et al. (2010) demonstrate that experimental participants avoid mental effort in decision-making tasks but will trade-off some of this disutility of effort in exchange for material incentives. They conclude that "cognitive demand weighs as a cost in the cost-benefit analyses underlying decision making" (677). This is unsurprising, given evidence from the experimental psychology literature that cognitive control of behavior is limited (Botvinivk et al., 2001), that decision-making is subjectively costly (McGuire and Botvinick, 2010), and that making choices depletes finite mental resources (Vohs et al., 2008; Spears, 2011).

This paper belongs to a growing literature that considers the economic implications of a boundedly rational agent who optimally deliberates in the face of a decision cost (*e.g.* Reis, 2006; Goldin and Homonoff, 2010). Acquiring information and deliberating are conceptually and empirically distinct: people gather information from other agents or the environment and produce "deliberation" or "contemplation," two terms that this paper will use interchangably (*cf.* Conlisk, 1996). This paper's model formally resembles one of an agent optimally collecting information. However, participants in the experiment acquire no new information other than the answers they themselves provide; they process what they know to arrive at a more deliberative conclusion — or they do not.[2]

Section 2 presents a model in which an agent is offered a product for sale. She lacks no external information, but cannot effortlessly compute whether buying would increase her net utility. She has three options: accept without deliberating, reject without deliberating, or deliberate before deciding. If she elects to contemplate, she eliminates the risk of wasting money or missing a valuable offer, but she must pay a utility decision cost.

A key implication of the model is an agent's endogenous price threshold. She will accept or reject offers below this threshold price without thinking, but will only accept an offer above her threshold price if she has thought about it. This threshold increases with her wealth. Therefore, a person of moderate wealth might spend a few dollars carelessly, but need to think before spending hundreds of dollars. For a poorer person, this threshold could be very low, so perhaps almost any positive price requires deliberation.

As a result, especially at low prices where the agent is deciding without thinking, buying can be very sensitive to price. In particular, potentially valuable offers could be foregone if the price becomes high enough to require deliberation, but the signal or apparent value is not enough to make deliberation seem worthwhile: in this case, the agent will simply walk away from the offer without thinking. This could explain high price sensitivity at low prices, especially for poor people: when deliberation is costly, an increase in price could require deliberation that the agent decides not to do.[3]

1.2. Pricing for the poor

Whether selling soap is a good way to prevent disease depends in part on how people, especially poor people, decide to spend their money. "Social marketing" programs sell products and services to poor people in order to achieve social goals. Many programs adopt techniques from for-profit firms, and often products are partially subsidized. Social marketing is also known as "cost sharing": by charging, governments or NGOs share the cost of an intervention with recipients, potentially making programs more financially sustainable. Moreover, advocates suggest that charging for products will screen out recipients with little value for the item, targeting adoption to people with the greatest need.

A growing literature within development economics is producing a complex account of what cost-sharing can achieve (for a more detailed description of prior studies, please see appendix A). Unsurprisingly, results are different in different

[2] This may suggest to some readers an infinite regress: how does the agent decide how to decide how to decide, and so on? Like the other papers in this literature, this model stops at one level of bounded rationality, and its predictions match the pattern of empirical results.

[3] Of course, there are many determinants of price elasticity of demand beyond the potential role of deliberation costs. For example field experiments by Tarozzi et al. (2011) and Devoto et al. (2011) illustrate that liquidity constraints are sometimes important barriers to poor people's purchase of health inputs.

programs and contexts (Cartwright and Hardie, 2012). Holla and Kremer (2009) review prior experimental evaluations of social marketing. Across several studies, a stylized fact that Holla and Kremer report is that small price increases, even at highly subsidized prices, can have large effects, especially on poor people: "imposing small costs consistently leads to dramatic reductions in take-up."

This paper extends this health pricing literature to handwashing soap. Diarrheal disease is responsible for one-fifth of deaths of children under five in poor countries (Kosek et al., 2003). Handwashing with soap protects against diarrheal disease. Because "the effectiveness of handwashing and point-of-use water treatment has been well established," Zwane and Kremer (2007) argue that "attention should now be given to efforts to understand effective promotion strategies and how to sustain behavior change." Lifebuoy Swasthya Chetna is a well-known social marketing program that sells soap in Indian villages, intended by Hindustan Lever Limited to combine pro-social goals with profitability (Easterly, 2006).

Section 3 presents a field experiment independent of Swasthya Chetna in which surveyors sold Lifebuoy soap in rural India. The experiment investigates two predictions of the model. First, the behavior of agents facing deliberation costs should exhibit high price sensitivity, but reducing marginal deliberation costs should lower price elasticity. Second, when agents must pay deliberation costs, cost-sharing may not succeed at targeting people who need the product most.[4] To emphasize, the factors that shape price elasticity of demand for socially-marketed soap in rural Gujarat likely differ from those most important in other places. However, a demonstration that decision costs *can* influence economic behavior suggests that they may sometimes be important, both for economic theory and for development program design.

2. Model

2.1. The buyer's problem

An agent is presented with an offer: she can buy a product for a price of p or not buy, a binary decision. If she accepts the offer, she will receive utility $\tilde{x}$ from the product. Her wealth is w and she values wealth according to increasing, concave utility $u(w)$. Assuming additive separability of these two sources of utility, then the agent's overall utility is the sum: $U = \tilde{x} + u(w - p)$ if she accepts the offer and $U = u(w)$ if she does not.

Although there is no external uncertainty in this model — the agent does not lack or demand information — she is not sure about these utility values: how much would she value the product? Is what she will receive better than what she will forgo? She does not immediately understand whether $\tilde{x} > u(w) - u(w - p)$.

It is costly for the agent to fully understand $\tilde{x}$.[5] The agent's particular confusion is simple: upon being made the offer, she sees a signal x about the value of the good. With probability π this signal is correct and $\tilde{x} = x$; with probability $1 - \pi$ the good is, in fact, worthless to her and $\tilde{x} = 0$.[6]

The agent can figure out the value of the good to her by deliberating. In exchange for paying cost c she can find the true value of $\tilde{x}$; c is modeled as a utility cost, not a material or time cost. Therefore, she can choose among three responses to the offer: reject the product without deliberating, accept the offer without deliberating, and deliberate before deciding. If she walks away without deliberating, she keeps her wealth and has utility of $u(w)$. If she buys without deliberating then she loses the price and may gain x, so her expected utility is $\pi x + u(w - p)$. If she deliberates, paying c, she will buy the product with probability π, receiving expected utility of $\pi[x + u(w - p)] + (1 - \pi)u(w) - c$.

2.2. How to decide

For comparison, first consider an agent with no deliberation costs: $c = 0$. Because she still faces $\pi < 1$, she will deliberate about every product that could be worth buying, that is, every case where $x + u(w - p) > u(w)$. This defines a threshold $x > \bar{x}(p, w) \equiv v(p, w)$, where $v(p, w) = u(w) - u(w - p)$, the opportunity cost of spending p. Above the threshold the agent deliberates and may buy; below it she does not. This threshold is increasing in p and decreasing in w, and $v_{pw} < 0$. Of offers that first meet this threshold, she will eventually buy the valuable fraction π.

But the agent does face deliberation costs, and must decide whether to pay them, and whether to buy nevertheless if she does not. She compares each of the three strategies: is buying without thinking better than outright rejection? Is thinking better than walking away? If both are true, is deliberating preferred to simply accepting the offer?

She prefers buying without thinking to rejecting the offer without thinking if and only if

$$x > \bar{x}^{NT}(p, w) \equiv \frac{1}{\pi} v(p, w), \tag{1}$$

[4] This second prediction also has some basis in the existing literature: Holla and Kremer's (2009) review suggests that charging user fees does not "target [a product] to households that could benefit from it the most." In general, they find that purchased products are not more likely to be used by the most vulnerable than products accepted for free, although not all studies find this result (Tarozzi et al., 2011). For more details about this prior literature, please see appendix A.

[5] An equivalent framework that makes the same predictions but models the agent as instead unsure about the marginal utility of money is detailed in an appendix available from the author upon request.

[6] Note that for clarity this models only one type of error or costly deliberation: a risk of overestimating the value of a good. Other situations could involve other forms of uncertainty.

where *NT* indicates no thinking. The fraction $\frac{1}{\pi}$ is greater than 1, so the distance between $\bar{x}$ and $\bar{x}^{NT}$ is greater at higher prices. Both thresholds $\bar{x}$ and $\bar{x}^{NT}$ are zero when price is — any good with the possibility of value is worth accepting for free — but the minimum apparent value required to buy unthinkingly increases in price more quickly than does the minimum value for an agent who contemplates costlessly. In particular, the divergence is steeper when likelihood of error is greater so π is smaller.

For thinking to be valuable, the benefits of thinking must outweigh the decision costs. If the agent deliberates, she will pay p only if she decides to buy, but must pay c whatever the outcome of her contemplation. She prefers thinking to preemptive rejection if and only if

$$x > \bar{x}^{T}(p, w) \equiv v(p, w) + \frac{c}{\pi}. \tag{2}$$

The apparent value x must exceed the value of the price by enough to compensate for deliberating: it would not be worth thinking merely to buy a good only slightly better than its price. The minimum apparent value $\bar{x}^{T}$ is increasing in price at the same rate as $\bar{x}$ but additionally requires $\frac{c}{\pi}$ at the origin.

Finally, whether costly deliberation is preferable to simply buying the good depends both on the utility loss risked by the possibility of wasting money and on the deliberation costs. Deliberation is preferred to thoughtless buying, whether or not either is preferred to merely rejecting the offer, if and only if

$$v(p, w) > \frac{c}{1-\pi}. \tag{3}$$

This defines a price threshold, $p^*(c, w, \pi)$, above which an offer would require deliberation. That is, if the price is above p^* the agent only buys after thinking; if the price is below p^* the agent only buys without thinking. The threshold p^* is independent of x but is increasing in w and c: agents for whom deliberation is more costly or money is less scarce will contemplate only at higher prices.

2.3. Buying behavior

Conditions (1)–(3) jointly determine the agent's behavior. The combined threshold $\bar{x}^{DC}(p, w)$ indicates the minimum apparent value x above which an agent facing deliberation costs will either purchase or consider purchasing the good at each price p.

$$\bar{x}^{DC}(p, w) = \begin{cases} \bar{x}^{NT}(p, w) & p \le p^* \\ \bar{x}^{T}(p, w) & p > p^* \end{cases}. \tag{4}$$

If $x > \bar{x}^{DC}$ and $p \le p^*$ then the agent will buy the product without deliberating. If $x > \bar{x}^{DC}$ and $p > p^*$ then the agent will contemplate before acting on the offer. If $x < \bar{x}^{DC}$ she rejects the offer without considering it, including in cases where $x > v(p, w)$.

Fig. 1 depicts the agent's buying behavior.[7] The threshold that determines behavior, $\bar{x}^{DC}$, is indicated by the bold line. The shaded area between $\bar{x}$ and $\bar{x}^{DC}$ is the set of potentially profitable offers forgone due to deliberation costs. These are offers that an agent with standard costless contemplation would consider and that may offer more utility than the opportunity cost of their prices, but that no agent facing costly deliberation will accept.

Part of poor people's sensitivity to small prices may be due to the ignored offers between $\bar{x}$ and $\bar{x}^{DC}$. Especially at the low prices of some important health inputs, the thresholds for agents with and without deliberation costs are diverging steeply.[8] The higher price sensitivity of would-be buyers under deliberation costs is indicated by the steeper slope of $\bar{x}^{DC}$ above the area of ignored offers. While population price elasticity of demand (as opposed to one person's), η, depends on the distribution of $\tilde{x}$ among buyers, with a uniform distribution of value, for example, price elasticity is unambiguously greater with deliberation costs. The following Remark formalizes this intuition:

Remark. Let $\tilde{x}$ be uniformly distributed in a population of otherwise identical agents. Then, for $p < p^*$:

- $\eta_{c>0} > \eta_{c=0}$: Population price elasticity of demand is greater with deliberation costs than without deliberation costs.
- $\frac{\partial \eta}{\partial \pi} < 0$, if $c > 0$: With deliberation costs, population price elasticity of demand is greater when agents have greater probability of error without deliberation.

A proof is presented in appendix section B.1.

[7] Curves are plotted as straight lines to emphasize that $\bar{x}$ and $\bar{x}^{T}$ have the same slope with a constant translation, while $\bar{x}^{NT}$ has the same slope multiplied by a constant.

[8] Moreover, free goods are not affected by deliberation costs, but offers for sale are.

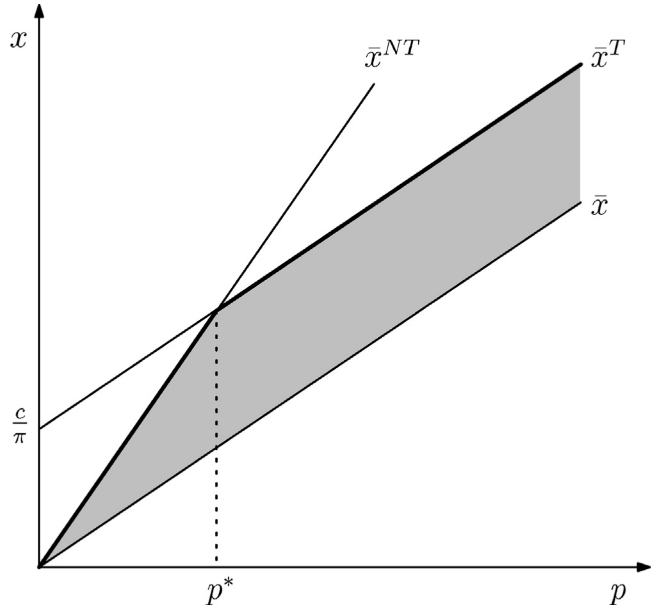

Fig. 1. Buying behavior. The agent will only contemplate or accept offers above the heavy line, which represents $\bar{x}^{DC}$, the agent's combined threshold with deliberation costs. $\bar{x}$ is the buying threshold for an agent without deliberation costs; $\bar{x}^T$ is the threshold for paying deliberation costs and thinking; and $\bar{x}^{NT}$ is the threshold for buying without thinking. p^* is the price threshold above which an agent will contemplate instead of buying without thinking.

2.4. Comparative statics

2.4.1. Eliminating deliberation costs

The field experiment in Section 3 required some participants to deliberate about a purchase, reducing or eliminating marginal contemplation costs at the time of the decision. What effects does this model predict of a change from $c > 0$ to $c = 0$?

If $c = 0$ then condition (3) is satisfied whenever $p > 0$ and $\pi < 1$: thinking is preferred to thoughtless buying whenever money is at stake, as long as there is some possibility of the apparent value being incorrect. Because $\frac{c}{\pi} = 0$, $\bar{x}^{DC}$ collapses to $\bar{x}$ and the agent considers each offer that, if valuable, would be worth more than the opportunity cost of its price.

Fig. 2 adds to the depiction of buying behavior in figure 1 two offers of a good x at illustrative high and low prices, in order to consider the effects of eliminating deliberation costs. As Fig. 2 depicts, this change has a first order and a second

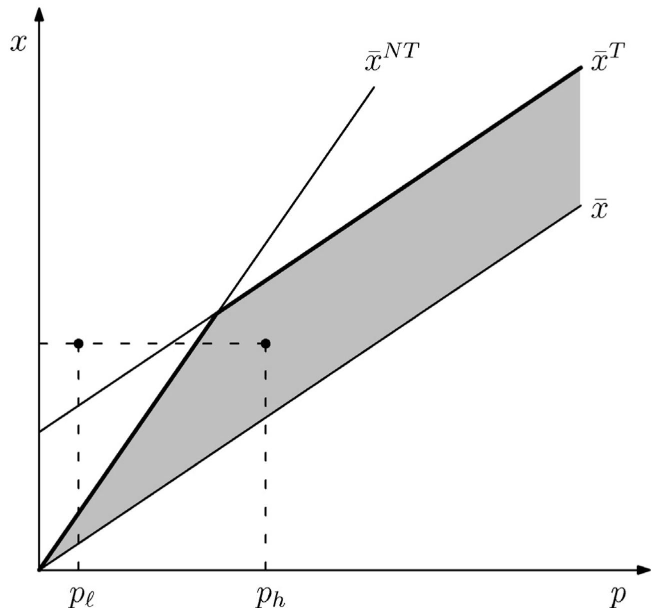

Fig. 2. Eliminating deliberation costs at high and low prices.

order effect, effects which are different at p^ℓ and p^h. The first order effect is to cause all potentially profitable offers to be considered, eliminating the shaded area. Therefore, in the figure, the offer would be considered at the higher price p^h only after deliberation costs have been reduced. If even with $c > 0$ it is the case that x exceeds $\bar{x}^{DC}(p, w)$, then there is no first order effect. Although the ultimate effect depends on the joint distribution of x and p, at lower prices the set of ignored offers occupies a narrower band of x signals.

The second order effect impacts some offers that are accepted with deliberation costs. With $c > 0$, accepted offers with $p > p^*$ were carefully considered and bought because they were indeed advantageous. Accepted offers with $p < p^*$ were not contemplated, however, and a fraction $1 - \pi$ will no longer be purchased when costless contemplation reveals $\tilde{x}$ to be zero. Therefore, at p^ℓ there would be no first order effect of eliminating c, but the probability of acceptance will fall by $1 - \pi$.

In short, eliminating deliberation costs is predicted to have a larger positive effect on take-up at higher prices, where take-up will otherwise be low, and to have a zero or small negative effect on take-up at lower prices, where take-up will otherwise be high. To state this discussion formally:

Proposition 1. *Changing $c > 0$ to $c' = 0$ eliminates the set of ignored profitable offers.*

- *There is no first order effect inducing purchasing if the agent would buy the good at $c > 0$.*
- *The width of the range of signals x in which changing c to 0 induces purchasing is weakly increasing in p, and strictly increasing when p is small.*
- *If the agent would buy the good at $c > 0$ and only if $p < p^*$ then changing c to 0 has a second order effect of reducing the probability of acceptance by $1 - \pi$.*

2.4.2. Wealth and poverty

How do richer agents, with higher w, compare to poorer agents? Fig. 3 presents a representative illustration of the larger effects deliberation costs can have on poorer agents. To quantify losses of consumer surplus due to deliberation costs, label $\mathcal{L}$ the shaded area between the curves: the set of potentially profitable offers (good-price pairs) that deliberation costs will cause an agent to ignore.

Proposition 2. *For any u, π, and c:*

- $\frac{\partial p^*}{\partial w} > 0$*: Deliberation is required to make a purchase at lower prices for poorer agents than for richer agents.*
- $\frac{\partial \mathcal{L}}{\partial w} < 0$*: The area of the set of potentially profitable offers ignored is smaller for richer agents.*

A proof is presented in appendix section B.2. Richer agents can afford to think less about spending decisions. This is a direct result of the assumed concavity of u, the utility of wealth: for richer agents, the marginal loss risked by potentially wasting money is less important. If a richer agent who accepts an offer deliberates, a poorer agent who accepts the same offer also deliberates. In this sense, even at the same level of utility cost c,[9] poorer people pay deliberation costs in more cases and may reject profitable offers more often. [10]

2.4.3. Screening and targeting

"Advocates of charging [for education and health services and products]. . . note that charging may screen out those who place low value on the product or service, thus concentrating take-up on those who value it most" (Holla and Kremer). If so, charging could make a cost-sharing program more effective than one using free distribution: a higher fraction of resources go where they would be most useful.

However, some previous experiments do not find evidence of higher prices "targeting" products to those who otherwise would be identified by distribution programs as those with greater need (see appendix A): buyers of health goods may not be able to make more productive use of them than those who acquired them for a lower price or for free. While this could occur among consumers without deliberation costs, especially if those with the highest need for the good are also the poorest or most credit constrained, deliberation costs can generate or exacerbate a failure of prices to target even if the distribution of x is independent of wealth. This is not a generic implication of the model — any particular outcome depends on the distribution of x — but may be likely in the policy-relevant cases. Plausibly, if deliberation costs blur the optimization behind an allocation, it may be unsurprising if the allocation is inefficient.

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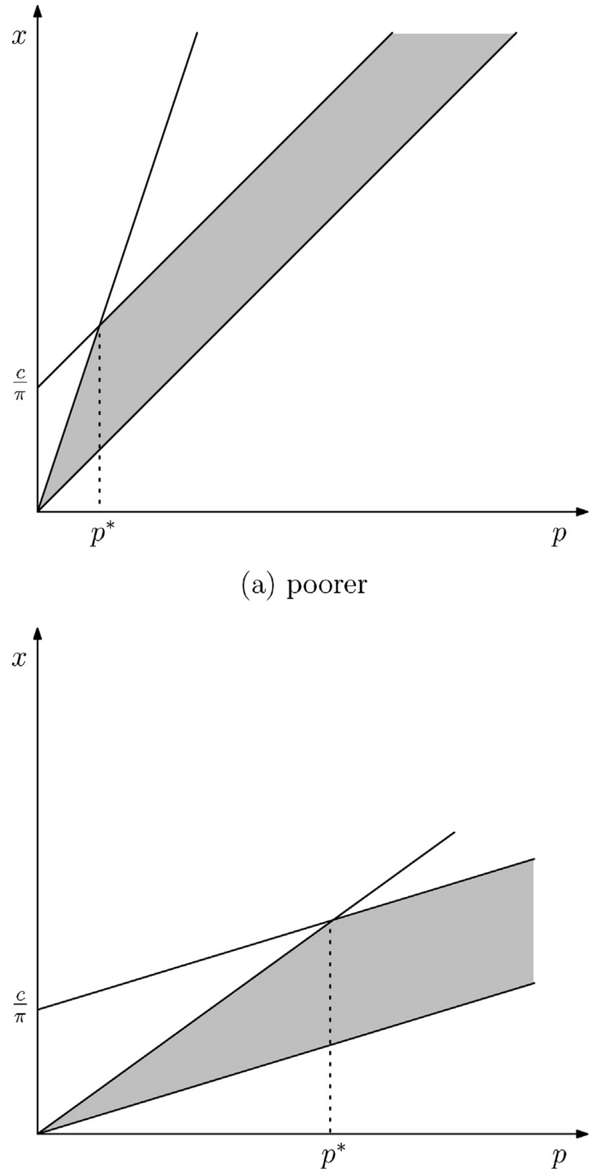
[9] I abstract from individual differences in c to emphasize its consequences in different contexts. However, heterogeneity in the distractions of home life (Banerjee et al., 2008), schools (Case and Deaton, 1999), or early life nutrition and health (Case and Paxson, 2009) might suggest that c can be higher for the poor.

[10] This is a model of one consumer; the ultimate consequences for aggregate buying behavior depend on the distribution of offers (x, p) to poor and rich agents. This experiment is motivated by cost-sharing development programs that set fixed prices, but more broadly, in practice poorer and richer people often buy and are offered different goods. A rich person with a high p^* may ultimately pay more deliberation costs than a poor person with a low p^* if, for example, the rich person receives more offers, or if the offers are sufficiently expensive that the poor agent would not buy them even without deliberation costs. Offers made by social marketing programs are made to the poor and carry low prices; Proposition 2 suggests that in precisely these situations deliberation costs will tax the poorest of this group most.



(a) poorer

(b) richer

Fig. 3. Deliberation costs, foregone offers, and wealth. Note: $p^*(c, w, \pi)$ is an increasing function of wealth; therefore the low p^* in panel (a) reflects lower wealth, and the high p^* in panel (b) reflects higher wealth.

Deliberation costs raise $\bar{x}^{DC}$ above $\bar{x}$, requiring higher perceived value x at every price. Proposition 2 demonstrated that this increased requirement, and its sacrifice of potentially profitable offers, is greater for poorer agents. Yet, it is exactly the poor for whom the difference between two prices — particularly the small prices studied in the social marketing field experiments — may be most relevant and most likely to screen out agents with low values.

Deliberation costs are most likely to prevent poorer agents from considering or accepting an offer. Yet, poorer agents are most likely to be sensitive to price, such that willingness to pay sorts potential buyers by value x. Therefore, deliberation costs can suppress a screening effect of higher prices.

Table 1 illustrates this possibility with an example designed to explain the results of the field experiment in Section 3. Wealth w and value x are jointly uniformly distributed. Agents are either poor, middle, or rich, with $w_p < w_m < w_r$ and have either high or low private value for a good, $x_\ell < x_h$. To emphasize: poor, middle, and rich agents experience the same cognitive costs and have the same distribution of value. The table summarizes who buys when the six agents are offered the good in each of four situations: at a high or low price, $p_\ell < p_h$, and with or without deliberation costs, $c > 0$ and $c = 0$.

Table 1
An illustration of how deliberation costs an interact with screening.

			Poor	Middle	Rich	Count	x^h %
$c > 0$	p^h	x^h			√	1	
		x^l			√	1	
						2	50%
	p^l	x^h		√	√	2	
		x^l		√	√	2	
						4	50%
$c = 0$	p^h	x^h	√	√	√	3	
		x^l		√	√	2	
						5	60%
	p^l	x^h	√	√	√	3	
		x^l	√	√	√	3	
						6	50%

In this example, an equal-sized sixth of the population has each wealth-value combination. A checkmark indicates that a sixth of the population buys the good under a price and deliberation condition. "Count" counts the number of population groups that buy the good. x^h% is the fraction of those who buy the good who have high value for it.

Rich agents have sufficiently low $v(p, w)$ that, with x_ℓ or x_h, they buy the product at either price, even under deliberation costs. All middle agents are like those depicted in Fig. 2: with deliberation costs they buy the good at p_ℓ but at p_h they reject the offer, which falls into their region of missed profitable offers. Without deliberation costs there is no such region and they buy the product at either price, even with the lower value for the good.

Poorer agents are as in Fig. 4. Without deliberation costs, the higher price would screen out agents with value x_ℓ while retaining agents with value x_h. With deliberation costs, neither type considers nor buys the good.

Buying behavior when $c > 0$ corresponds to the control group in the experiment below. There is a large effect of the increase in price on the number of people buying the good, but no effect on the proportion of those who buy who value the good highly. When $c = 0$, the effect of price on acceptance of the offer is smaller and an increase in price increases the fraction of buyers having higher value for the good.

3. Experiment

The model in Section 2 suggests that deliberation costs can cause small prices to have large effects on behavior and can cause potentially valuable offers to be ignored. An experiment in rural villages in Kutch, India tested these predictions with door-to-door sales of handwashing soap at two subsidized prices. Each participant was randomly assigned to a price and independently to either a treatment or a control group, creating four groups. Participants in this experiment's control group were left with whatever deliberation costs they ordinarily face. The other half of the participants received a treatment that

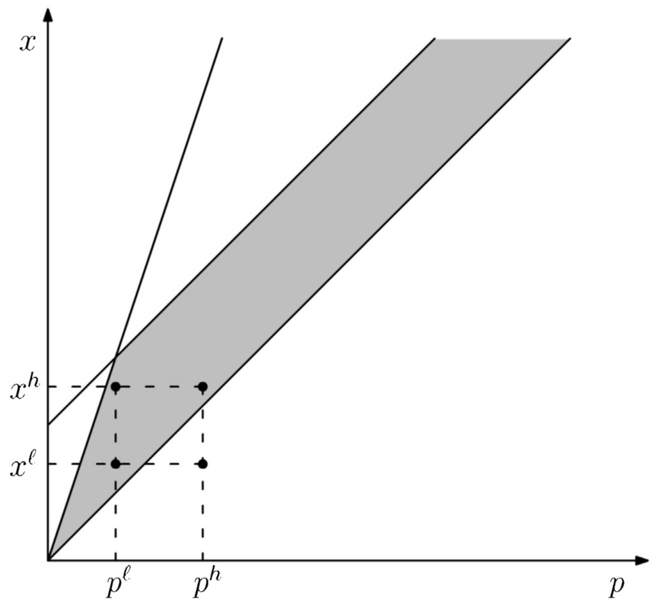


Fig. 4. Deliberation costs suppress screening.

required contemplating the offer in order to reduce, or perhaps eliminate, their marginal deliberation costs at the time of their decision.

3.1. Procedure

3.1.1. Context

In March and April 2009, surveyors visited 13 rural villages in Anjar and Gandhidham blocks of Kutch, the largest district in Gujarat, India. According to a 2001 government map, all villages in the study had a population below 2000 and most between 500 and 1000. The experiment was conducted in Gujarati.

March and April are in the "hot season" of Gujarat. Local conventional wisdom holds that children are especially susceptible to diarrhea during these months, which accords with epidemiological literature on seasonality of diarrhea in rural north India (Bhan et al., 1989). Thirteen percent of women in the sample who have children in their household report at least one child having had a loose stool in the previous week. This is almost double the global median rate of 3.2 episodes of diarrhea per child year found by Kosek et al. (2003).

Surveyors made unannounced visits to participants' houses. They were instructed to visit every house admissible under the experimental protocol.[11] The experiment was conducted only with adult women, one per household. Surveyors were trained not to interview a woman if anybody other than small children were present. In addition to ethically promoting anonymity, privacy ensured that the experiment was focused on individual decision-making, not social preferences or signalling. A research assistant made many spot checks on the surveyors throughout the experiment to enforce the protocol; each surveyor ordinarily was monitored at least once per village.

The surveyors were all economics students at Tolani College of Arts and Science in Adipur. They identified themselves as students working on a school project and explained that they received the soap from the college for this project. Unlike in, for example, the neighboring state of Rajasthan, in rural Kutch government and NGO programs are, unfortunately, largely absent from these villages. Participants were unlikely to mistake surveyors for official service providers, generally were not experienced with government benefit programs, and had little reason to respond to surveyors strategically or deceptively.

3.1.2. Product and prices

Participants were offered a package of two 120 gram bars of Lifebuoy brand soap. Lifebuoy is one of two brands of soap in Kutch marked for health rather than beauty. A social marketing program by Hindustan Lever Limited that sold Lifebuoy in rural Indian villages has been analyzed and publicized by Easterly (2006) and Prahalad (2004).

In almost every village I found at least three brands of soap for sale, and in most about five; Lifebuoy was regularly among them. Yet, many participants generally do not purchase or use body soap. That the soap was pre-packaged into bundles of two bars clarified to participants that they were being offered the set.

Soap was offered at the "low" price of 3 rupees and the "high" price of 15 rupees. Like in previous social marketing experiments, these represent subsidies of 88 and 42 percent of the market price and 87 and 33 percent of the lowest price of soap found in any village. Table 2 records observed market prices of soap.[12] Even at 15 rupees, the experiment's soap was less expensive by weight than any other soap found for sale in these villages.[13]

3.1.3. Thinking treatment

Participants assigned to the treatment group were indirectly made to deliberate about whether they should buy the soap before being required to choose. Participants were asked a series of questions that appeared to be ordinary survey questions. Surveyors wrote down responses as if learning the answers were the goal. However, these questions were in fact designed to lead participants through deliberation about the offer. Participants in the control group were asked a matched set of irrelevant questions. Participants did not know that the survey script was randomized or that certain survey questions would be interpreted as a treatment.

Treatment questions invoked reasons both to and not to buy the soap, and were not an advertisement: for example, participants calculated the flour they might forgo by buying the soap and considered whether their household might come to need the money soon. Neither treatment nor control questions provided any information; they encouraged participants to recall, imagine, and evaluate. Both sets of questions are reported in appendix section C.

Care was taken to equalize the duration and cognitive and emotional intensity of thinking and control questions; balanced questions could avoid spurious effects of mental depletion, confusion, or experimenter demand. Additionally, control questions were written to avoid a direct effect on the soap decision, such as by invoking wealth or social status. Each control

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[11] A house would be inadmissible if a woman declined to participate, if the surveyor could not conduct the interview alone with the participant, or if the surveyor believed the participant may have already witnessed or heard about the experiment, which happened only very rarely.

[12] Unlike the social marketing programs studied by earlier experiments, Lifebuoy Swasthya Chetna did not subsidize soap — not, at least, on a final price per unit basis, and certainly not compared with other brands. "To help people on low incomes a small 18gm bar of Lifebuoy has been introduced... this sells for the equivalent of 2 rupees" Neath (2006). At 0.111 rupees per gram, this soap cost more than the regular bars of Lifebuoy soap I found in Kucchi villages. This increase could partially reflect packaging into small units.

[13] Some soaps' packages report a "toilet soap grade" of 1, 2, or 3. A 0.1 rupees-per-gram increase in the price of the soap in my sample is associated with a 30 percentage point increase in the linear probability of being grade 2 or 3 rather than grade 1 (s.e. = 0.148). Lifebuoy is grade 3.

Table 2
Prices and weights of hand and body soap found in experimental villages.

Soap	Grade	Price (Rs)	Weight (g)	Rs/g
Experiment: p_ℓ	3	3	240	0.013
Experiment: p_h	3	15	240	0.063
Nima	1	7	75	0.093
Nirma: Premium	1	12	125	0.096
Dyna: Sandal & Saffron	1	5	50	0.100
Jo	2	12	115	0.104
Lifebuoy	3	13	120	0.108
Lifebuoy Swasthya Chetna		2	18	0.111
Vatika	2	12	100	0.120
Lifebuoy	3	12	90	0.133
Vivel	2	16	116	0.138
Lux	2	10	54	0.185
Liril	2	18	75	0.240
Dettol	2	19	70	0.271

question was matched to a thinking question in grammar, form of response (numerical, comparative, or open-ended), and expected fatigue. While in English there are 123 words in the thinking questions and 116 in the control questions, in Gujarati there are 126 and 125 words, respectively. A research assistant present for many interviews reports that among a surveyor's interviews, treatment and control questions took approximately the same time.

Care was also taken to avoid prejudicing the results of deliberation or influence the participants' utility for the soap. For example, a question was asked that highlighted the opportunity cost of spending p ("How much flour could you buy for p rupees?" presumably a positive amount) and another was asked demonstrating an upper bound on this opportunity cost ("Could you buy a bucket for p rupees?"; participants could almost certainly not). Additionally participants were asked to give a reason buying the soap would be a good idea and a reason it would not. The order of the good and bad reasons was randomly counterbalanced to avoid question order effects, as was the order of good and bad questions in the control group.[14]

To increase the probability that deliberation happened only according to the experimental protocol, surveyors were instructed not to interview women who appeared to have already heard about the experiment from neighbors. We visited each village only once, and conducted interviews for no more than a few hours, three at the maximum. Results below will confirm that later interviews in a village were not different from earlier interviews.

3.1.4. Econometric strategy

According to Proposition 1, reducing or eliminating deliberation costs should increase take-up at a high price that falls within the range of foregone profitable offers, but should have no effect, or a slightly negative effect, on take-up at a low price. To test this proposition, I make two assumptions: that 15 and 3 rupees constitute such high and low prices, and that the thinking questions reduced marginal deliberation costs at the time of the decision, but had no other effect relative to the control questions.

Causal identification is based on random assignment of individual participants to treatment and control groups, stratified by cluster and surveyor. The experiment used eight different scripts: {3 rupees, 15 rupees} × {treatment, control} × {good reason first, bad reason first}.[15] Each surveyor received experimental forms in paper-clipped packets of eight in opaque envelopes. These forms were in a random order independent across packets. Only after participants had agreed to participate, and after the surveyor was alone with a participant, was the participant assigned a script by the surveyor's removal of the next form from the envelope. Surveyors did not know my hypotheses and were trained not to look in the envelope at upcoming forms. A research assistant and I intercepted surveyors throughout the day to verify their compliance with randomization protocols.

With two experimentally crossed treatments, I can estimate

$$\text{buy}_{ij} = \beta_0 + \beta_1 \text{thinking}_{ij} + \beta_2 \text{highprice}_{ij} + \beta_3 \text{thinking}_{ij} \times \text{highprice}_{ij} + \varepsilon_{ij}, \quad (5)$$

[14] The thinking treatment is similar to "debiasing" experiments in cognitive psychology. Fischhoff (1982) describes debiasing as "destructive testing" of decision-making anomalies, often seeking "to discover the boundary conditions for observing biases." A common debiasing technique used in this experiment's thinking treatment is to ask participants to generate reasons, in this case to buy the soap and not to buy the soap. Schwarz et al. (2007) warn about the "metacognitive" effects of this debiasing strategy, the effects of the experience of thinking. Asking a participant to list many benefits of an activity may make her less likely to select it than asking her to list fewer; the increased difficulty she has finding the marginal benefits persuades her that they are scarce. This is particularly relevant to the experiment in Kucchi villages: almost half of the participants never attended school, and even those who did found the experiment unusual. To avoid this complication, the treatment asks for only one reason of each type.
Social psychologists have found that people are more likely to ultimately undertake actions to which they are asked to verbally commit. Making a commitment appears to change people's preferences by inducing them to signal to themselves that their preferences are different (Bem, 1967). To isolate cognitive mechanisms, the thinking treatment does not ask participants whether they will or should buy the soap.

[15] The order of the request for positive and negative reasons was randomized simply to counterbalance any effect of asking either first; it was not an experimental treatment and had no hypothesized effect.

Table 3
Summary statistics by experimental group.

	Full sample	Control		Treatment		F
		3 rupees	15 rupees	3 rupees	15 rupees	
Household size	5.05	5.29	4.92	5.00	5.00	0.79
	(0.091)	(0.206)	(0.147)	(0.205)	(0.159)	
Household has children	0.855	0.852	0.839	0.842	0.887	0.62
	(0.014)	(0.028)	(0.029)	(0.028)	(0.025)	
Ever attended school	0.515	0.488	0.559	0.497	0.516	0.65
	(0.020)	(0.039)	(0.039)	(0.039)	(0.040)	
Schooling in years	3.08	3.10	3.31	3.06	2.84	0.45
	(0.142)	(0.300)	(0.273)	(0.293)	(0.266)	
Reports child's diarrhea	0.111	0.117	0.099	0.091	0.138	0.72
	(0.012)	(0.025)	(0.024)	(0.022)	(0.027)	
Lives in native village	0.182	0.170	0.199	0.188	0.164	0.24
	(0.015)	(0.030)	(0.032)	(0.031)	(0.029)	
Recently bought soap	0.552	0.580	0.553	0.570	0.503	0.17
	(0.024)	(0.034)	(0.030)	(0.050)	(0.044)	
Stopped interview	0.034	0.037	0.031	0.024	0.044	0.35
	(0.007)	(0.015)	(0.014)	(0.012)	(0.016)	
Order within packet	4.31	4.27	4.37	4.19	4.43	0.29
	(0.090)	(0.179)	(0.181)	(0.177)	(0.185)	
n	647	162	161	165	159	

Standard errors of the mean in parentheses.
F reports the test statistic on the joint hypothesis that the four categories cannot predict the variable.
n differs across groups because surveyors did not always complete packets.

where buy_{ij} is a binary indicator of whether the participant bought the soap, $thinking_{ij}$ is a dummy variable indicating having been assigned to the thinking treatment, and $high\ price_{ij}$ is a dummy for a 15 rupee price. Index i identifies participants; j represents the village-level clustering of the data.

Proposition 1 predicts:

- $\beta_1 \leq 0$. Thinking has no effect, or a slightly negative effect, at three rupees.
- $\beta_2 < 0$. Charging the higher price reduces take-up in the control group.
- $\beta_3 > 0$. Encouraging thinking reduces the effect of the change in price by increasing take-up at the high price.

3.2. Validity

3.2.1. Randomization balanced observable characteristics

Table 3 presents summary statistics for answers to survey questions.[16] The average household has five members, and in 85 percent at least one of these is a child. Approximately half of the participants ever attended school and almost twenty percent live in their native village.

No observed characteristics statistically significantly differ across experimental groups. Table 3 presents means and standard errors for each group, as well as the F statistic testing the fit of a saturated model. The last row verifies that the four treatments were equally likely among the early and late surveys in a village.[17]

Although the independent variables in regression equation (5) use initial assignment, attrition and compliance are not problems in this experiment. Participants had a single, approximately ten-minute interaction with surveyors. Once participants agreed to the experiment and were randomly assigned a treatment group, they were counted as having not bought the soap if the interview stopped, or under any outcome other than buying the soap. Stopping only happened in 3.4 percent of cases; the last row of Table 3 confirms that stopping is not differentially likely across experimental groups. Stopping is no more likely in earlier or later interviews within a village.

3.2.2. Treatment and control

While the *answers* to the treatment and control questions were never intended to be part of the experiment — *asking* them was the point — the surveyors were instructed to report them. These data provide useful, if imperfect, evidence of

[16] These survey questions, asked in order to assess balance of experimental groups, followed the experimental treatments and soap decision, and may have been influenced by them. Survey questions must either precede or follow the experimental decision whether to buy soap; whichever came second could be tainted. Rather than counterbalance their order, I chose to focus on the validity of the experiment.

[17] Moreover, the order surveyed within villages is uncorrelated with observables: those surveyed later do not come from larger households (t = 0.01), nor are they more likely to have gone to school (t = 0.82), or still live in their native village (t = 0.29).

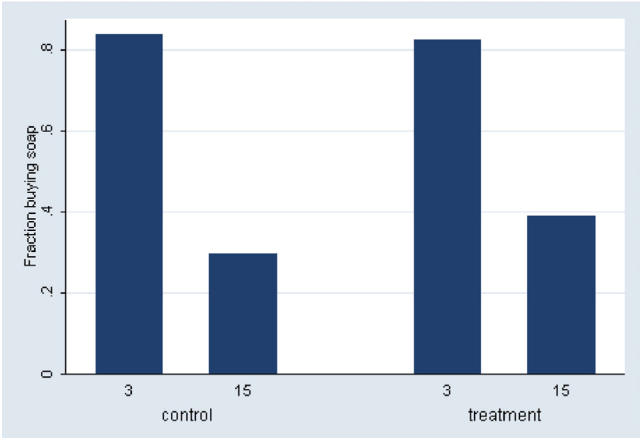

Fig. 5. Fraction buying soap by experimental treatment.

participants' beliefs. For example, while only 5 percent of participants offered soap for three rupees thought they could find the same quantity at a lower price, 19 percent of those offered it for fifteen rupees thought so.

Answers to thinking treatment questions were associated with buying behavior, although sometimes imprecisely. Among the 324 participants asked, participants who believed the soap will last ten days were 5.6 percentage points more likely to buy ($t = 0.98$), those who believed they can get a lower price were 24 percentage points less likely to buy ($t = 3.68$), and those who believed they will need the money in the next ten days were 5 percentage points less likely to buy ($t = 0.70$). These suggest that participants were indeed considering the thinking questions in deciding whether to buy. Moreover, the thinking treatment was not merely a "buy soap!" frame: 47 percent of participants offered soap at three rupees and 64 percent of those at fifteen rupees claimed that by buying the soap they might be giving up something they would need to buy in the next ten days.

There is no evidence that participants inferred from the low prices that the soap was inferior. In fact, although the correlation is unsurprisingly not statistically significant, participants assigned to the very low price were 8 percentage points more likely to think that the soap would last at least ten days ($t = 1.08$).[18] Within the treatment group, people who reported knowing that the experimental price is a discounted price were more, not less, likely to buy.

Because the control questions asked, in part, about villages rather than soap, participants in the control group might have believed that they were being means tested for government benefits and bought strategically. Surveyors identified themselves as students, and such misidentification is almost certainly much less likely in rural Kutch than it may be elsewhere in India where social services are more widespread. Direct evidence that this probably did not happen is available in answers to survey questions. While not statistically significant, participants in the control group were 3 percentage points more likely to report recently having bought full price soap and slightly less likely to report a child's loose stool, both the opposite of an attempt to appear poor.

3.3. Results

3.3.1. Deliberation costs

Fig. 5 presents the main result of the experiment. In the control group 84.0 percent bought soap when offered for three rupees and 29.8 percent bought soap sold for fifteen rupees. Among those asked to deliberate, 82.4 percent bought soap for three rupees and 39.0 percent bought soap for fifteen.

Column 1 of Table 4 rewrites this intention-to-treat result as a linear probability regression: the results match Proposition 1. In the control group, like in the previous pricing experiments, the higher price caused a 54.1 percentage point decrease in take-up. The thinking treatment had almost no direct effect among those offered soap for three rupees; it was associated with a statistically insignificant decline in purchasing of 1.5 percentage points, as the model predicts. While the thinking treatment had little effect at three rupees, it increased acceptance by 10.7 percentage points — or more than one-third — at fifteen rupees, where the model suggests that offers would have been otherwise ignored due to deliberation costs.[19]

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[18] Indeed, this difference is greater in absolute value (interaction $t = 0.51$) for those who report having bought soap recently (and therefore might best know the ordinary price and be most suspicious), but is positive even for those who do not.

[19] This table uses clustered standard errors, which are known to produce tests of larger than theorized size in finite samples. Colin et al. (2008) report Monte Carlo simulations of alternative tests for regression coefficients under clustering, including a specification with a binary dependent variable regressed on two interacted dummies. They find that even with as few clusters or fewer than in this experiment, inference with a wild cluster bootstrapped t distribution has empirical size very close to its theoretical, asymptotic size. An unpublished appendix available from the author upon request verifies that such a bootstrap — as well as other approaches to inference such as separate estimation for each cluster (Ibragimov and Muller, 2010) — all allow inference that $\beta_3 > 0$.

Table 4
Probability of buying soap by experimental group, regression results.

	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) Probit	(7) Probit
Thinking treatment	−0.015	−0.037	−0.027	−0.125	−0.011	−0.061	−0.102
	(0.039)	(0.044)	(0.041)	(0.151)	(0.035)	(0.158)	(0.149)
Higher price	−0.541**	−0.555**	−0.564**	−0.548**	−0.538**	−1.522**	−1.734**
	(0.041)	(0.045)	(0.041)	(0.155)	(0.035)	(0.150)	(0.150)
Interaction	0.107†	0.122*	0.127*	0.220	0.108*	0.311†	0.397*
	(0.050)	(0.056)	(0.053)	(0.205)	(0.049)	(0.180)	(0.187)
Village fixed effects					$F = 23089$		$\chi^2 = 7215$
					$p = 0.00$		$p = 0.00$
Surveyor fixed effects					$F = 4.51$		$\chi^2 = 20.0$
					$p = 0.02$		$p = 0.00$
Controls					√		√
Constant	0.839	0.848	0.872	0.833	0.552	0.992	0.103
	(0.040)	(0.044)	(0.038)	(0.088)	(0.089)	(0.164)	(0.294)
n	647	576	625	90	647	647	647
Clusters	13	13	13	13	13	13	13
R^2	0.25	0.26	0.27	0.21	0.33	0.19	0.28

Two-sided p-values: †$p < 0.10$; *$p < 0.05$; **$p < 0.01$. Clustered standard errors in parentheses. Column 2 omits the last interview in each set of eight. Column 3 omits 22 participants whose interviews ended before the soap decision; otherwise these are counted as not buying. Column 4 includes only the first participants in each village. Controls are for having children, reporting diarrhea, schooling, and recently buying soap. Probit marginal effects in column 6 are −0.023, −0.536, and 0.116.

Is the estimate of β_3, the coefficient on the interaction, robust to plausible respecifications? Column 2 of Table 4 omits the last interview in each packet of eight. In principle, a surveyor could have memorized the first seven interviews and known with certainty which price and treatment assignment would be next. While I have no evidence of this happening, this could have permitted a departure from the protocol of randomly assigning the experimental group only after receiving informed consent. When these interviews are omitted the coefficients are similar, indeed greater in magnitude.

Interviews that were stopped or ended in any way other than a sale of soap are counted as the participant not having bought soap. This is both for external validity — whether a participant acquired soap is the relevant policy question — and because early termination could be an endogenous response to experimental treatments. As a robustness check, column 3 omits the 22 participants whose interviews ended before their soap decision. Again, coefficients are similar and the interaction is greater in magnitude.

We were in each village only briefly, and surveyors were instructed not to interview women who may have heard about the experiment. Yet, any discussion of our experiment may have facilitated deliberation or changed participants' interpretation of the experiment. Column 4 includes only each surveyor's first interview in each village. These first interviews happened simultaneously. While much less precise with a smaller sample, each experimental effect is, if anything, stronger.[20]

Regression equation (5) is a saturated linear conditional expectation. Because offer acceptance is binary, probit may also be appropriate, though it requires more assumptions. Column 6 fits a probit model to the regressors in column 1 and recovers similar marginal effects. Replicating the result with a probit regression suggests it is not a spurious artefact of an upper limit of 100 percent take-up. Columns 5 and 7 are included to demonstrate robustness. They add fixed effects of villages and surveyors, within which randomization was stratified, and observed covariates that might influence demand for soap.[21]

3.3.2. Theoretical implications

These results match the predictions of the model of deliberation costs. Before considering the model's predictions for "screening" by recipient need, are the effects estimated so far consistent with other theories? Section 3.2.2 already used treatment and control questions' evidence about participants' beliefs to rule out certain alternatives: the low price did not persuade agents that the soap was worthless; participants did not behave strategically, believing surveyors were service providers rather than students; and the thinking treatment elicited reasons that both discouraged and encouraged buying.

The small decline in take-up at the low price caused by the thinking treatment could reflect the fraction $1 - \pi$ concluding that soap would not be valuable to them, as Proposition 1 suggests. Or, it could merely be sampling error. Whatever its cause, the negative sign of $\hat{\beta}_1$ makes some alternative explanations unlikely. For example, the thinking treatment might suggest

[20] This similarity further makes unlikely an interpretation that villagers, having heard about the experiment, are acting individually or collaborating to project poverty to outside monitors. Additionally, I created an indicator *late* for interviews in the second half of a village. When *late* is fully interacted with regression (5), the triple difference ($late \times price \times treatment$) is not significant ($t = 0.37$), neither is *late* or any of its interactions individually, neither is an F test on these four additions jointly ($F_{4,12} = 1.85$).

[21] Duflo et al. (2008) recommend "controlling for variables not affected by the treatment" when analyzing experiments. However, Freedman (2008) establishes that, if treatment effects are heterogeneous (which there is no reason to assume is not the case here), then with covariates the estimated coefficients for experimentally assigned variables are biased in finite samples, and the coefficient estimates for regression covariates are inconsistent. I include columns 5 and 7 — where, once again, results are similar and stronger — as robustness checks.

that the participant's "guest" wants her to buy the soap. Yet, social preferences, or this "experimenter demand" would be more probably effective at the three rupee price of a cup of tea that might be served to a guest. Participants would be more likely to buy the soap for social reasons when it is cheaper, not more expensive.

Could the effect at the low price merely indicate that, while a biasing treatment should always increase take-up, some people strongly dislike soap, or simply cannot afford it? A fixed, soap-disliking group appears unlikely in light of pilot results at lower prices where more participants bought soap. More importantly, the decline at the low price is even more pronounced ($\hat{\beta}_1 = 0.06$) for participants who report owning soap, a group that can certainly afford it.

Shampanier et al. (2007) sold discounted chocolate to undergraduate students, although unlike this paper they concentrate on zero rather than low positive prices. They offer a choice between low and high quality chocolates for either 1 and 26 cents, respectively, or 0 and 25 cents; "in the zero-price condition, dramatically more participants choose the cheaper option" (p. 742). They explain special price sensitivity at zero as emotional excitement about the low price that analysis can overcome. Their evidence is a "forced analysis" condition which arguably required participants to pay deliberation costs and, like the thinking treatment, diminished the high sensitivity to low prices. However, in Kutch the thinking treatment (which strictly speaking had no zero price) did not operate by diminishing any excitement attached to the low price; it had no statistical effect at three rupees (although the point estimate is negative). Instead it increased acceptance at the high price. Because participants offered the soap for fifteen rupees had no exposure to the low price, the thinking treatment could not have worked by encouraging cognition to outweigh positive emotion about the low price's bargain, Shampanier et al.'s theory of their results.

Holla and Kremer (2009) recognize that cost-sharing prices are "relatively small short-run costs;" this understandably suggests that high price elasticity found in social marketing experiments such as this one may be explained by time-inconsistent or present-biased preferences (O'Donoghue and Rabin, 1999). Could the thinking treatment have operated not by requiring deliberation, but by making naïve present bias sophisticated? A sophisticated agent recognizes that she will spend more impatiently in the future than she might prefer. This suggests she is effectively poorer, which might make her either buy less at both prices, or be more price sensitive, not less.[22] Another theoretical possibility is that a sophisticated decision-maker would by soap, a durable good, in order to lock in "saving" the money spent on the price. If the thinking treatment indeed caused this, it would probably make take-up *less* associated with health-related need for soap (because it is being purchased for a financial reason unrelated to health); however, Section 3.3.3 shows that the thinking treatment increased screening.[23] Although such an argument cannot definitively rule out a role for sophisticated present bias, this appears unlikely.

3.3.3. *Screening and targeting*

Section 2.4.3 argues theoretically that deliberation costs could prevent prices from targeting distribution to those with the highest value. Is there evidence that this occurred in Kutch?

Fig. 6 indicates that there is. I identify households with greater need for handwashing with soap as those with children and those where the participant reports at least one child having had a loose stool in the previous week.[24] As the theoretical illustration above suggests, in the control group those who bought the soap at the higher price were no more likely to have children and no more likely to report diarrhea. In the treatment group, with lowered marginal deliberation costs, buyers at a higher price were needier by both measures.

Table 5 reports regression results for screening. In the treatment group, the proportion of buyers at the high price who reported diarrhea is more than double the proportion of buyers who did at the low price, an increase of 9.8 percentage points. Among participants asked control questions, charging fifteen rather then three rupees is associated with a statistically insignificant 2.8 percentage point decline in loose stools. Similarly, while charging a higher price is associated with only a statistically insignificant 1.3 percentage point increase in the fraction of buyers having children in the control group, in the treatment group it increases this fraction by 8.9 percentage points.

Columns 3 and 6 permit inference on the effect of the thinking treatment on the screening effect of prices as an interaction. With the sample restricted to the 382 women who bought soap, coefficient estimates are relatively imprecise. For diarrhea, wild cluster bootstrap t one- and two-sided p-values for the interaction term are 0.027 and 0.049. For having children, these p-values indicate insufficient precision to rule out sampling error: 0.23 and 0.45.[25]

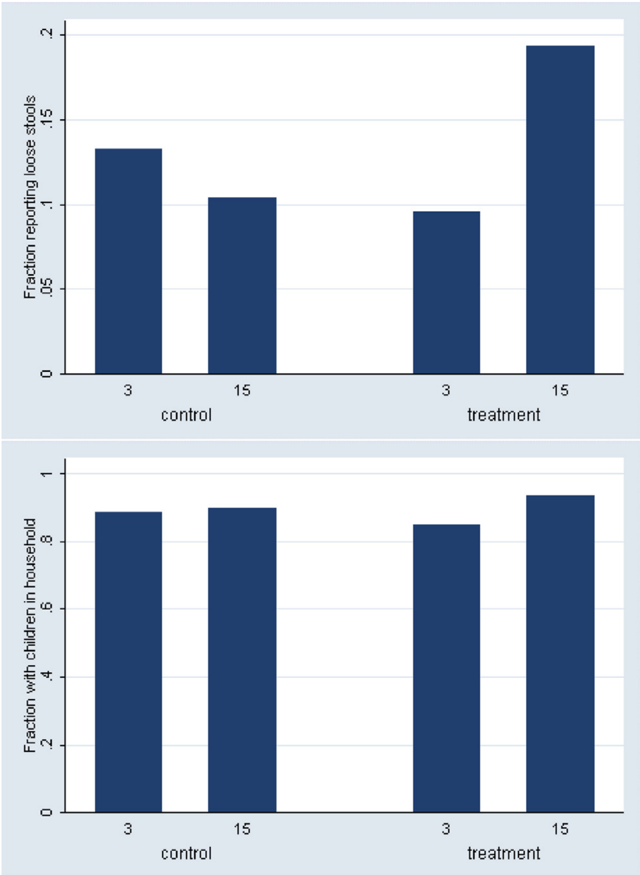
In the control group, as in some prior experiments, there is no evidence of higher prices screening buyers. Without deliberation costs, however, higher prices made soap more likely to be bought by households where children had diarrhea, and perhaps more likely to be bought by households with children.

[22] A present biased agent might not want to buy even discounted soap because she prefers what she can enjoy now with the money to the future health she would buy. However, increasing sophistication does not change this preference, it only makes her more aware of it.

[23] A different way of looking at Section 3.3.3's results is that, among participants offered soap for the higher price, in the control group those who did and did not buy were about equally likely to have a child with recent diarrhea, but in the treatment group households that buy the soap were about twice as likely to have a child with recent diarrhea. This is unlikely if sophisticated participants were merely buying soap as a form of saving.

[24] The biased sex ratio (known to exist in rural India) is one double-check of the credibility of the survey-reported data on children in the household. 53 percent of children are boys (similar to the 52 percent of rural children under five in India's 2005 National Family Health Survey); we can reject an even 50–50 split with an F of 19 with village-level clustering and of 18 without. 65 percent of the 103 households that report exactly one child report a boy, also statistically significantly greater than 50 (F = 10).

[25] Ashraf et al. similarly find in an experiment in Zambia that price does not improve targeting of water disinfectant to households with children.



$n = 382.$

Fig. 6. Screening among those who bought soap, by experimental treatment.

Table 5
Deliberation costs and screening: linear probability of need among buyers.

	Children's diarrhea			Children in the household		
	(1) Control	(2) Treatment	(3) Full sample	(4) Control	(5) Treatment	(6) Full sample
Higher price	−0.028	0.098*	−0.028	0.013	0.089†	0.013
	(0.051)	(0.040)	(0.051)	(0.058)	(0.046)	(0.058)
Thinking treatment			−0.037			−0.037
			(0.026)			(0.043)
Interaction			0.126*			0.076
			(0.058)			(0.091)
Constant	0.132	0.096	0.132	0.882	0.846	0.882
	(0.025)	(0.033)	(0.025)	(0.023)	(0.028)	(0.023)
n (soap buyers)	184	198	382	184	198	382
Clusters	13	13	13	13	13	13
R^2	0.00	0.02	0.01	0.00	0.02	0.01

Two-sided p-values: †$p < 0.10$; *$p < 0.05$; **$p < 0.01$. Clustered standard errors in parentheses.

4. Conclusion

Deliberation costs can cause people to ignore potentially profitable offers, can increase price sensitivity especially among the poor and at low prices, and can prevent prices from targeting products to recipients with the greatest need or value. These predictions were verified by a field experiment in villages of rural Kutch, India.

This paper has presented a stylized model, illustrating one mechanism by which cognitive limits could shape price sensitivity and buying behavior. How do the experimental results compare with the predictions of models of emotional responses to discounts, social preferences, present bias, or standard price theory? Theories without cognitive limits would be challenged to explain these results well, although further research can clarify how to separate requiring costly contemplation from inducing sophisticated present bias. Whether a person buys at a price appears to partially depend on whether she thinks carefully about the offer, which itself depends on the price. Cognitive limits could be important in other domains, especially where π, the trustworthiness of initial signals, is likely to be low or c is likely to be high. These results join Frederick (2005), Benjamin et al. (2006), and Dohmen et al. (2010) in demonstrating the potential importance of cognitive limits for economic decisions.

Appendix A. Supplementary Data

Supplementary data associated with this article can be found, in the online version, at http://dx.doi.org/10.1016/j.jebo.2013.06.012.

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