Eliciting and Utilizing Willingness to Pay: Evidence from Field Trials in Northern Ghana

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Abstract

We utilize the Becker-DeGroot-Marschak (1964) mechanism to estimate the willingness to pay for clean drinking water technology in northern Ghana. The BDM mechanism has attractive properties for empirical research, allowing us to directly estimate demand, compute heterogeneous treatment effects, and study the screening and causal effects of prices with minor modifications to a standard field experiment setting. We demonstrate the implementation of BDM along these three dimensions, compare it to the standard take-it-or-leave it method for eliciting willingness to pay, and discuss practical issues for implementing the mechanism in true field settings.

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1 Introduction

Economists, firms, and policy-makers are often interested in knowing how much an individual is willing to pay for some item. The answer to this question is, of course, the fundamental building block of demand functions and hence our ability to estimate demand elasticities and consumer surplus. Practically, such measures can inform pricing policy, guiding the magnitude and targeting of discounts or subsidies. They also provide an important intermediate input for the study of demand formation, the role of credit constraints, and other determinants of technology adoption, social learning, and health spillovers. In many contexts, willingness-to-pay measures can be used to study the screening and causal effects of prices.[1] Moreover, willingness-to-pay provides a prime source of heterogeneity when evaluating treatment effects and the causal effect of prices.[2]

Ideally, one would like a precise measure of each individual's willingness to pay; however, obtaining such a measure is often difficult. If an individual believes that her answer to the question "How much are you willing to pay for this product?" will affect the actual price, there will be an incentive for her to answer strategically. Economists have considered a range of techniques to elicit a truthful answer, including revealed preference methods such as simple take-it-or-leave-it offers, Vickrey auctions, *n*th-price auctions and stated preference methods such as contingent valuation and conjoint analysis. The simplest method in a field setting is to make take-it-or-leave-it (TIOLI) offers at randomized prices. A researcher asks, "Will you buy this product at price $\$p$?" TIOLI is transparent and simple to implement, but provides limited information. For example, if an individual agrees to purchase a product for $2, the researcher does not know if her willingness-to-pay for the product was $2.01 or $20. The Becker-DeGroot-Marschak mechanism (BDM), commonly used in experimental economics, provides an alternative with attractive properties.

In contrast to TIOLI offers, BDM elicits an individuals' exact willingness-to-pay for a good or lottery (Becker, DeGroot and Marschak, 1964). It operates much like a second-price auction

[1] See, for example, Cohen and Dupas (2010), Ashraf et al. (2010), Karlan and Zinman (2009), Klonner and Rai (2008) and Mahajan, Tarozzi, Yoong and Blackburn (2011).

[2] See Heckman and Vytlacil (2005), Heckman et al. (2006) and Chassang, Padro-i Miquel and Snowberg (2010).

against an unknown or random price.[3] An individual states her bid for an item, then a random price is drawn from a distribution. If her bid greater than or equal to the price, she receives the item and pays the price drawn. If her bid is below the price, she pays nothing and receives nothing. For expected utility maximizers, where preferences over lotteries can be summarized by their certainty equivalent, bidding one's true maximum willingness-to-pay is a dominant strategy.

We implement and assess BDM in the context of studying the willingness-to-pay for household water filters in a sample of 1,265 subjects from 15 villages in rural northern Ghana. The BDM mechanism has a number of attractive properties that make it appealing for field work. First, by providing a precise measure of willingness-to-pay, it allows for direct, non-parametric estimation of demand.[4] BDM shares this advantage with auction mechanisms, but unlike auctions it is robust to intra-community conflict and collusion and allows individuals with low willingness-to-pay to receive the good with positive probability. The researcher rather than the subject pool controls the price distribution. As such, it is more suited to many field settings. This feature allows us inform pricing policy with direct, "out of sample" predictions for take-up and usage.

Second, BDM generates detailed information on heterogeneous treatment effects. Like TIOLI methods with random price draws, BDM provides an exogenous source of variation in product access, allowing researchers to identify causal effects. But unlike TIOLI, BDM allows researchers to estimate heterogeneous treatment effects across the across the common support of the bid and price distributions. Intuitively, BDM reveals each individual's place in the distribution of willingness-to-pay and then allocates the good to each individual with a positive probability. We demonstrate how BDM can allow researchers to estimate local marginal treatment effects in a variety of field settings.[5] In our particular context, we find an inverted U-shaped pattern for the filter's health

[3]The first known attempted implementation of a BDM-type auction was by Johann Wolfgang von Goethe in 1797. Goethe wished to measure the worth of his poem *Hermann and Dorothea*, and offered the publisher Friedrich Vieweg the opportunity to bid against an unknown price (chosen by Goethe) in a sealed envelope. Unfortunately Goethe's lawyer revealed the reserve price to Vieweg, eliminating the opportunity to learn Vieweg's true maximum willingness to pay. See Moldovanu and Tietzel (1998).

[4]Note that the standard BDM mechanism estimates demand for only single unit-demand items, such as the water filter studied in this project. By modifying the mechanism to elicit the willingness-to-pay for additional increments of the good, BDM can be used to estimate demand for products where multiple units may be demanded by a single individual. See Hoffman (2009) for an example.

[5]The ability of BDM to improve information extraction from randomized control trials is emphasized by Chassang

impact, as measured by reductions in children's diarrhea.

Third, BDM induces random variation in price paid, conditional on willingness-to-pay. This produces a double randomization, along the lines of that pioneered by Karlan and Zinman (2009), with which to independently estimate the screening and causal effect of prices on usage. Because the price drawn under BDM is independent of an individual's bid – the single, randomized price draw induces the distribution of quantity and price, conditional on the bid – the second-stage randomization does not need to be a surprise, and BDM can be implemented effectively in environments where individuals know or are likely to discover the allocation mechanism independently. In our particular setting, we find no evidence of sunk-cost effects on effort levels (as measured by cleaning behavior) or usage.

Balanced against these advantages, BDM faces a number of challenges. It is a novel mechanism with limited experience outside the lab. As demonstrated by Karni and Safra (1987) and Horowitz (2006a), BDM is not necessarily incentive-compatible for non-expected utility maximizers, a feature it shares with auctions. Moreover, our study presents a severe test of BDM. Numeracy among our subject pool was low; non-standard beliefs about probability were commonplace; and the good in question was unfamiliar to respondents and yields benefits – reduced episodes of water-related disease – that are uncertain and difficult to quantify.

While there is a substantial literature dealing with the implementation and behavior of BDM in university economics labs,[6] little is known about the practical applicability of BDM in a field setting. To explore this issue, we randomly assign respondents to be offered a water filter using either BDM or TIOLI. Results from both methods of demand elicitation follow a similar pattern; however, we find that TIOLI acceptance rates are consistently higher than those predicted by our BDM-estimated demand curve. We explore a number of potential explanations for the gap, including anchoring and strategic behavior, but the results are inconclusive.[7] We remain agnostic about

et al. (2010), who describe BDM as an example of a "selective trial."

[6]See, e.g., Noussair, Robin and Ruffieux (2004); Irwin, McClelland, McKee and Schulze (1998); Keller, Segal and Wang (1993); Smith (1982)

[7]There are a number of reasons why BDM may yield lower willingness to pay than the TIOLI mechanism. Our experiment is designed to test two. First, respondents may believe that they can influence the future price of the item by bidding low. Second, the TIOLI offer may anchor respondents to a valuation that is higher than what they would

which mechanism provides the more accurate estimates of normal market demand[8] and are encouraged by the demonstrated feasibility of BDM to enrich the information generated by randomized control trials.

The rest of the paper proceeds as follows. Section (2) describes the background of the BDM mechanism. Section (3) explains the experimental setting and implementation. Section (4) describes the results of our comparison of willingness to pay under BDM and TIOLI, and outlines a model of BDM bidding behavior consistent with these results. Section (5) explores the relationship between willingness to pay and baseline characteristics and health behaviors. Section (6) explores the screening and causal effects of prices. Section (7) illustrates the use of BDM to estimate the relationship between willingness to pay and treatment effects. Section (8) concludes and presents a roadmap for future work.

2 Background

2.1 Uses of BDM

There are three major appealing qualities of BDM: precision in demand estimation; quasi-experimental variation in treatment; and random variation in price paid, conditional on willingness to pay. In this subsection, we discuss each in turn.

First, BDM provides an exact value for WTP, with precision limited only by the desired granularity of the researcher. In principle, it is possible to measure exact maximum willingness-to-pay down to the smallest available denomination.[9] The technique provides more precision than a ran-

bid in the BDM mechanism. Respondents may believe that the stated price carries some information about a product (Wollinsky 1983; Milgrom and Roberts, 1986; Ashraf, Jack and Kamenica, 2011). Alternatively, the stated price may make them focus on that price and resolve that their valuation is at least that much. We have collected data from a 50% subsample of participants on various preference measures including cognitive ability, risk aversion, loss aversion, and ambiguity aversion. Ongoing work explores the role of these factors in determining bidding behavior.

[8]Take-it-or-leave-it offers are themselves unusual in environments where fixed, posted prices are rare and bargaining common. Jack (2010), for example, provides evidence that subjects respond less rationally to a take-it-or-leave-it offer of a payments for environmental services contract than when they participate in an auction.

[9]In practice, we limited our prompts to units of GHS 0.50, although nothing prevented respondents from bidding, for example, GHS 1.2 (as one respondent did). At the time of the field work, the exchange rate was approximately GHS 1.5 per US$ 1.

domized take-it-or-leave-it offer, which provides only a bound. For example, if a respondent accepts a TIOLI offer of GHS 4, we can only conclude that her WTP was at least GHS 4. Similarly, if she rejects an offer of GHS 6, we can only conclude that her WTP was less than GHS 6. Under BDM, though, we obtain a precise number, e.g., GHS 5.50. This precision aids in the estimation of demand and understanding how WTP is correlated with important observables (wealth, health status), both of which can inform pricing for firms, social enterprises and NGOs.

Second, BDM provides a quasi-experiment that allows for instrumental variables estimation of treatment effects. Because the respondent's draw is purely random and correlated with take-up, one can estimate the treatment effect of the good in question by instrumenting for take-up with the draw. This is also true of take-it-or-leave it with a randomized price – the researcher can instrument for take-up with the randomly assigned price. However, BDM allows researchers to move beyond the single local average treatment effect estimable by TIOLI and calculate *separate* IV estimates of marginal treatment effects by levels of willingness to pay. The intuition is as follows: BDM first reveals each respondent's willingness to pay and then, conditional on this willingness-to-pay, randomly assigns the treatment to each individual. With sufficient data, a researcher could estimate localized marginal treatment effects for any point in support of the WTP distribution or group subjects together, for example, by WTP quantile, to estimate heterogeneous treatment effects. This allows the researcher to address a fundamental economic question: are those who stand to benefit the most also those who are willing to pay the most?

Third, BDM provides random variation in price paid among those with equal WTP, which allows the researcher to estimate, for example, the causal effect of price paid on use, as in Ashraf et al. (2010) and Cohen and Dupas (2010). That is, BDM allows the following experiment: two subjects, both willing to pay GHS 6 for the filter, both obtain the filter, but one pays 6 GHS and the other pays 2 GHS, with the difference in price paid randomly assigned. The researcher could then study whether the amount paid (or, if zero prices are included in the BDM distribution, the act of paying a positive amount) influences the recipient's behavior with respect to the product. BDM is particularly useful for this sort of two-stage pricing, because, unlike other mechanisms that use a surprise discount (e.g. Karlan and Zinman, 2009; Ashraf et al., 2010), the random variation in price paid does not need to be kept a secret for incentive compatibility. This enables its use in contexts, such as ours, where there is close communication among subjects and the possibility of a surprise discount could never be sustained.

Hoffmann et al. (2009) use BDM to measure the willingness to pay / willingness to accept compensation gap for bednets in Uganda, and find that subjects are less willing to sell a bednet received for free than purchase one at the same price. Coverage of young children is higher when nets are received for free, due to this endowment effect and possibly a psychological effect of free receipt Hoffmann (2009).

2.2 Functioning of BDM in Theory and Practice

Departures from expected utility theory present challenges for revealed-preference methods of eliciting willingness-to-pay. Karni and Safra (1987) show that for individuals who are not expected utility maximizers, BDM is not incentive compatible over lotteries or when the good's value is uncertain. This builds on their earlier results demonstrating similar implications for second-price, sealed-bid (Vickrey) auctions (Karni and Safra, 1985). Horowitz (2006a) extends these results, showing that for non-expected utility maximizers BDM may not be incentive compatible even when there is no uncertainty over the good's value. The intuition in all cases is akin to that embodied in the Allais Paradox: when an individual's preferences cannot be represented by an expected utility function, the value she places on an outcome is no longer independent of how that outcome is determined. For example, winning an auction may directly enter an individual's utility function beyond the value of the good and the price paid. Failing to purchase a good may generate disappointment, and this disappointment or the potential for it can directly impact utility (Gul, 1991).

Horowitz (2006b) describes several empirical comparisons of BDM to other elicitation mechanisms. Many of these compare BDM to Vickrey or *n*th price auctions. BDM bids are generally below those obtained in a Vickrey auction, while there is not a consistent pattern with respect to other auction mechanisms. A recent paper by Miller, Hofstetter, Krohmer and Zhang (2011) tests several mechanisms to elicit willingness-to-pay for a cleaning product in a population of Swiss consumers, primarily university and high school students. Interestingly, they include a real purchase option, presenting subjects with a single, randomly-drawn price in a simulated online shop, finding a close match between BDM bids and real purchase decisions. However, as Horowitz notes, the question of how BDM bids compare to market demand remains largely unanswered.

Another strand of research studies the internal rationality of behavior under BDM by comparing bids under different price distributions. Bohm et al. (1997) find that BDM bids are sensitive to information about the underlying price distribution. Mazar et al. (2010) extends this result in a controlled lab setting with students bidding on a travel mug or a box of chocolates. They demonstrate that this sensitivity cannot be explained by rational inference drawn from the price distribution and consider several minor modifications to the BDM mechanism. Our elicitation mechanism is most similar to their titration-based BDM, which almost entirely eliminated sensitivity to the price distribution. Urbancic (2011) studies 69 subjects' valuation of a gift certificate for a dozen cookies. Subjects played 20 times for the same item, allowing (1) the study of learning over time and (2) within-subject variation in both the shape and support of the price distribution.[10] Urbancic finds evidence of "mass-seeking bias," – that subjects' valuation is pulled toward the mean or median of the price distribution.

3 Experimental Design

3.1 Point of Use Water Treatment in Rural Northern Ghana

Lack of access to clean water is one of the most significant threats to health and welfare in the developing world, particularly rural Africa. Forty percent of Africans—and 54% of rural Africans—lack access to improved sources of drinking water. This has serious health conse-

[10] Incentive-compatibility is maintained by selecting one round at random to be binding. Unavoidably, this limits the scope for learning since the random price is not revealed until after the binding round is chosen.

quences: diarrheal disease causes nearly 1.8 million deaths worldwide each year, and is responsible for 17% of deaths of African children under five years of age. Poor water quality also has large impacts on the health of the living, contributing to other debilitating diseases such as malaria, schistosomiasis, trachoma and worms (World Health Organization, 2004, 2005, 2011).

Our study measures willingness to pay for a particular household water treatment: the *Kosim* filter, a ceramic filter sold by Pure Home Water, a Ghana-based non-profit organization (see Figure 1). This simple product is highly effective at improving water quality and is appropriate for the region, since it removes particulates and pathogens from water without the use of chemicals or electricity. This product provides an excellent platform studying willingness to pay for health products because (1) similar technologies are sold through social marketing organizations, and (2) Pure Home Water itself is interested in learning about demand for its own product and is therefore a responsive partner in the evaluation.

3.2 Village Selection

Our study is based on sales of the *Kosim* filter to households in 15 villages in northern Ghana. Villages were selected according to several criteria. First, we targeted villages in which Pure Home Water had not previously conducted sales, nor had there been a giveaway by UNICEF or any other organization. Second, we selected villages within 2 hours' travel time from our headquarters in Tamale, but a minimum of 30 minutes outside the city. Third, we had to receive the initial cooperation the village chief and health liaison in conducting the study in the village. Fourth, we selected villages which contained between 30 and 60 compounds (extended-family households). Finally, we chose villages in which residents did not already have easy access to clean water. In particular, we excluded villages with access to piped water and villages that were saturated with functioning bore-holes. These criteria produced a list of 24 villages, from which we visited 15 in random order.[11]

[11] Our initial assessment produced a list of 22 villages. After surveying had begun in the 4th village, we determined that 1) our initial estimates of village size were too small, and 2) we had the capacity to survey larger villages. Therefore, we added 10 villages with between 40 and 60 compounds to our list, dropped 7 villages from the original

3.3 Unit of observation

Because the focus of most clean water interventions is child health, our unit of observation is defined as any person with primary caregiving responsibilities for children age 12 or under. In the vast majority of cases, this was the child's mother, although there were some instances of widower fathers, grandparents, aunts and uncles and foster parents caring for children whose parents were absent or deceased. For simplicity of prose, we will refer to these caretakers as "respondents" from this point forward, to distinguish them from other village residents potentially affected by the study or otherwise appearing in our data, who we will refer to generally as "subjects."

Most subjects live in extended patrilineal family compounds, which are small clusters of individual huts, usually enclosed by a wall. Many resources are shared within the compound, although in most cases each mother is responsible for providing water for her own husband and children. As described below, the treatments were randomized at the compound level and all inference is robust to clustering at the compound level.

3.4 Sales Process

3.4.1 Preliminary activities & household survey

INITIAL VISIT AND CENSUS. For each village, we conducted an initial village meeting, during which we provided a demonstration of the filter and the two sales mechanisms. Two surveyors performed a mock version of both BDM and TIOLI for a token item, such as chocolates or a bar of soap. The surveyors also practiced the sales mechanisms with volunteers from the attendees, again for a token item. We informed villagers that a filter would be installed at the health liaison's home and encouraged them to visit the liaison to see the filter working, taste the water and ask questions. We instructed the attendees that we would visit their households in approximately two weeks to offer them an opportunity to purchase the filter via one of the mechanisms we performed.

list with less than 40 compounds and 1 village which our initial visit revealed to have 98 compounds. We then re-randomized the village order. Since our treatments were randomized within villages and stratified by compound size, we do not view this as a problem for our study.

Attendees were encouraged to discuss with their families what they were willing to pay for the filter. The two-week interim period was chosen to allow families time to try the filter and determine their willingness to pay, as well as to obtain the desired level of cash, either through occasional work or by selling assets such as grain.

After the meeting, we conducted a comprehensive census of all residents of the village. With this information, we were able to identify the study subjects as defined above and perform random assignment of the subsequent treatments.

WATER QUALITY TESTING AND HEALTH EDUCATION TREATMENT. In the roughly two-week period between the village presentation and the sale, we visited each household to remind them of the upcoming sales visit and to answer any questions they had about the treatment. During this reminder visit, we took a 100 ml baseline sample of their water for testing in the lab. In addition, we conducted one of two health education treatments in randomly selected households. The first treatment is a general message describing the link between untreated water and health and explaining how the filter helps prevent diseases such as diarrhea. This treatment discusses child health but it is not a particular emphasis. In contrast, the second treatment emphasizes child health. The substantive elements of the treatment are largely similar but the emphasis is placed on the dangers to children of untreated water and the potential benefits of the filter to children. The impacts of these treatments will be analyzed in a separate paper.

HOUSEHOLD SURVEY. Roughly one week after the reminder visit (and, for treatment households, the education treatment), we conducted a survey and sales visit with each respondent. Respondents were compensated with a 1.00 GHS cash gift, awarded at the beginning of the survey.[12]

All respondents were administered a basic survey covering income, assets, education and health status. We also included a few specialized modules, including a module on water collection and treatment practices and a module asking about basic health knowledge, in particular asking respon-

[12]This was awarded in small denomination coins to ensure that respondents could submit reasonably fine-scale bids in the practice WTP game described below. It is possible that a cash gift influenced willingness to pay for the filter by inducing goodwill toward the surveyor. However, because of the length of the survey there was always at least 30 minutes between the gift and the sales offer, which would ameliorate any "house money" effect. Also, we would not expect the gift to affect WTP differentially between the two mechanisms.

dents to list causes of diarrhea in children. This latter module was performed as an unprompted question to avoid suggestion bias. At the end of the survey, we conducted the sales experiment. By conducting the sale at the end of a survey on water and health, we may have primed the respondent's demand for the filter. However, it was not feasible to conduct the sale first, because respondents, and especially respondents who were not able to purchase the filter, would quickly lose interest in the survey. Furthermore, we have no reason to believe that this priming would affect WTP in BDM or TIOLI differentially.

SALE. Respondents were randomly assigned to either a BDM or TIOLI sales treatment. Within each of the two broad categories described above, there are three sub-treatments designed to disentangle potential reasons for different WTP responses in the two treatments. We describe first the basic BDM and TIOLI treatments, then the refinements and the rationale for them. All sales treatments were randomly assigned at the compound level, stratified by number of respondents in the compound.[13]

The scripts for the sales were designed to be as similar as possible across treatments. Each sale began with a practice round for a token item, in most cases a bar of soap with retail value approximately 1.00 GHS. The respondent was then given the opportunity to purchase the soap using the mechanism corresponding to her treatment category. After the practice round was complete, the respondent was given the opportunity to purchase the *Kosim* filter using the same mechanism. [14]

3.4.2 BDM

First, the surveyor reads a brief description of the BDM procedure. We emphasize that the respondent will have only one chance to play, cannot change her bid after drawing from the cup, and must be able to pay that day. The surveyor then plays a practice round for the bar of soap. The respondent is asked for her maximum willingness to pay for the bar of soap. The surveyor reminds her

[13]In separate research, we are exploring whether intra-compound strategic behavior can explain differences between demand under BDM and WTP. We chose to stratify by number of respondents in a household to maximize power to study this hypothesis.

[14]Scripts for the standard BDM and TIOLI treatments are provided in the Appendix. Scripts for the variants of BDM and TIOLI are available from the authors on request.

that if she draws slightly more than her bid, she will not be able to purchase the soap. She is then allowed to adjust her bid. This process repeats until the final bid is established. Before she draws, the surveyor then reviews various hypothetical outcomes to test her understanding. Once the final bid is established, the subject draws a ball from the cup and purchases or does not purchase the soap. The balls in the cup are distributed from 0 to 100 in increments of 5 pesewas (0.05 GHS), with amounts 0,..., 45 receiving double weight (i.e., there are two balls marked with each of these numbers, versus only one ball for each of 50,...,100).

The procedure for the filter is similar. As with the practice round, the respondent is reminded that she will not be allowed to purchase the filter if she draws a price slightly higher than her bid, and is then allowed to adjust her bid upwards if she wishes. The distribution of prices is 0, 0.5, 1, 1.5, 2, 2.5, 3, 3.5, 4, 5, 6, 7, 8, 9, 10, 11, 12 in equal proportions. At the completion of the game, the respondent, if successful, pays for the filter and receives a receipt that can be redeemed for a filter at the village's health liaison's home.[15]

We do not require respondents to present the amount of cash they are willing to bid before the draw is made. Rather, we permit the household to gather the money by the end of that day. Before the draw is made, we ask multiple times whether the respondent will have access to the funds. We do this to maintain realism: households routinely make small loans to each other for purchases. Of the 272 respondents who drew a price less than or equal to their bid, only three were unable to gather the funds. These appear to have been idiosyncratic events – e.g. a neighbor or friend was not home – rather than the result of an attempt to game the mechanism, so we have left the willingness to pay data unaltered for these respondents. Recoding their WTP as zero or dropping them does not change our results.

We also track whether a losing respondent attempts after the fact to purchase at the price drawn (i.e., above her final offer) and ask all losing respondents whether they wish they had bid more.

[15] Perhaps the most salient difference between the procedure for the filter and for the practice round is that the filter is not physically present in front of the respondent during the bidding. We chose not to have surveyors bring the filters to compounds: first because they are bulky and could break; second because there is some instruction on assembly and care that should be given at the time the household receives the filter. This instruction is most efficiently provided at a central location.

Either of these occurrences could suggest that the respondent did not understand the game. In our data, 18 of 332 (5.4%) of BDM losers offered to pay more than their final bid, while 64 (19.3%) of the losers stated that they wished they had bid more. There is some overlap between the two groups, so in total 70 of the losers (21%) gave some indication that they did not bid their maximum willingness to pay. An alternative interpretation is that the act of drawing a higher price actually alters the respondent's willingness to pay. That is, someone who thought her willingness to pay was 5 GHS and drew 5.5 GHS might experience regret at missing by only 0.5 GHS and, upon further introspection, realize that she would in fact have been willing to pay 5.5 GHS.[16]

3.4.3 TIOLI

The standard take-it-or-leave it treatment is a simple sales offer at a randomized price. We randomized at three prices, 2, 4, and 6 GHS. These prices were chosen as roughly the 25th, 50th and 75th percentiles of BDM bids in pilot exercises in similar villages. Before revealing to the respondents, we emphasized that there would be no bargaining. If they accepted the offer price, respondents were allowed until the end of the day to obtain the necessary cash. If the respondent initially agreed to the purchase but is ultimately unable to obtain the funds, we code her as not purchasing but note the attempt to purchase. As discussed below, this is important for the comparison between TIOLI and BDM.

The price is randomized at the level of the compound. It was determined that in a TIOLI setting it would not be acceptable to offer the filter to different members of the same compound at different prices. All analysis is clustered at the household level.

3.4.4 Sub-treatments

Each of the broad categories of BDM and TIOLI contains three sub-treatments. These sub-treatments were included to help disentangle the possible channels through which purchasing

[16]We find that ex-post regret is highest for those who narrowly missed winning in BDM. Roughly 30% of those who missed by just GHS 0.5 wish that they had bid more, with this percentage declining steadily to approximately 10% among those who missed by GHS 5-10. Results are available from the authors on request.

behavior under BDM and TIOLI may differ. Pilots conducted in the summer of 2009 indicated that BDM under-predicted willingness to pay relative to TIOLI (consistent with what we find below), and each sub-treatment is designed to test potential mechanisms for this difference. The two hypothesized mechanisms and the associated sub-treatments are described in the remainder of this section. We denote the standard treatments, described above and without modification, as the "standard BDM" and "standard TIOLI" sub-treatments.

First, we hypothesize that respondents in the BDM treatment could act strategically in hopes of influencing the future price of the filter, which would lead them to bid below their true maximum WTP. To examine whether this is in fact the case, we include two sub-treatments. In the first sub-treatment (the "anchoring BDM" sub-treatment), we inform the respondent of the price of the filter in the Tamale market, 20 GHS. In the second sub-treatment (the "market study BDM" sub-treatment), we tell the respondent that we are using the information from the study to help decide on the future price of the filter in similar villages. If strategic bidding is important, then one would expect that respondents in the anchoring sub-treatment will have higher bids than those in the standard sub-treatment, and that those in the market study sub-treatment will have lower bids than those in the standard sub-treatment.

Second, we hypothesize that the given price in the TIOLI treatment causes respondents to anchor their own valuations to that price. If that is the case, then the "anchoring BDM" sub-treatment may induce a similar form of anchoring and thereby raise bids relative to the standard BDM sub-treatment. However, if anchoring is the key factor influencing bids rather than strategic bidding, one would not expect a difference between the "market study BDM" sub-treatment and the standard BDM sub-treatment.

We were also concerned that the anchoring induced by the BDM anchoring treatment would not increase valuations above those in the standard TIOLI sub-treatment. We therefore include an anchoring sub-treatment (the "anchoring TIOLI" sub-treatment) using the TIOLI mechanism where the respondents are informed of the retail price in town.

As a final test for anchoring, we include a TIOLI sub-treatment (the "random TIOLI" treatment) in which the randomization of the price was carried out in front of the respondent. In this sub-treatment, the respondent drew a price (2, 4 or 6 GHS) from a cup and stated whether she wanted to purchase at this price. We include this sub-treatment to confirm to the respondents that the price at which the filter was offered did not serve as a signal of quality. Furthermore, if respondents are generally uncomfortable with randomness, this "random TIOLI" sub-treatment should match BDM more closely.

The following table summarizes the predictions of bidding behavior in the six sub-treatments, if each of the channels is a factor:

Channel	**WTP implied by bidding/purchase behavior**
Strategic Behavior	Anchoring BDM > Standard BDM Market BDM < Standard BDM
Anchoring	Anchoring BDM > Standard BDM Anchoring BDM = Anchoring TIOLI Random TIOLI < Standard TIOLI

4 Comparison of Demand under BDM and TIOLI

4.1 Sample characteristics

Table 1 displays the number of respondents in each of the 6 categories of sale treatments. Treatment was randomized at the compound level. The first two columns show the number of compounds and individuals assigned to each treatment. To the extent permitted by the preliminary census, we attempted to stratify on the number of subjects in each compound. However, there is still some residual imbalance in the number of respondents assigned to each treatment category.

The second two columns display the number of compounds and subjects for which the survey and sales offer were actually completed. We do not observe any clear differences in attrition rates by treatment, which is reasonable since most attrition occurred when respondents had traveled away or were spending nights at their agricultural plots. In either case, the respondents did not know their treatment at the time of attrition.

4.2 Comparison of BDM and TIOLI Treatments

Figure 2 displays a histogram of the BDM bids. The average bid among all 603 BDM respondents was 3.1 GHS, median 2.5 GHS.

Figure 3 displays the acceptance behavior of the TIOLI treatments at each price. Overall, 92% accepted at 2 GHS (N=238), 47% accepted at 4 GHS (N=219), and 20% accepted at 6 GHS (N=186). Figure 3 also overlays the acceptance frequencies with the (inverse) demand curve implied by the BDM bids. As shown by the figure, at each price, the fraction of respondents in the TIOLI treatments that purchased the filter was greater than the share of BDM respondents with bids that high or higher. In other words, Figure 3 suggests that the fraction of respondents for whom the mechanism revealed a willingness to pay of more than 2, 4, or 6 GHS was higher in the TIOLI treatments than in the BDM treatments.

To estimate the sizes of these differences via regression, we compare the BDM bids with TIOLI acceptance behavior at each price by determining whether the BDM bid implies a purchase at that price (i.e., a willingness to pay of greater than or equal to that price). For example, in order to compare a BDM bid with TIOLI purchase at a price of 2 GHS, we generate a new variable that indicates purchase if the BDM bid is greater than or equal to 2 GHS. At each price, we compare purchase behavior using the following equation:

$$\text{buy}_{icp} = \alpha_0 + \alpha_1 \text{BDM}_c + \varepsilon_{icp} \tag{1}$$

where buy_{icp} indicates whether person i in compound c purchased at price p (under the TIOLI mechanism), or would have purchased at price p given her bid (under the BDM mechanism), and BDM_c is an indicator for whether compound c was assigned to the BDM mechanism.

We estimate the equation separately for each TIOLI price. Note that because the BDM bid gives a point estimate of willingness to pay, an observation assigned the BDM treatment can be used for multiple prices. Therefore, each regression contains about three times as many BDM observations as TIOLI observations. Since the errors are clearly correlated across equations, we estimate the system via seemingly unrelated regression so that we can conduct cross-equation tests.

The regression results are presented in Table 2. The difference between the two mechanisms is significant at the 5% level or greater for each of the three prices. The test of joint significance of all three differences yields a p-value of less than 0.001. While the absolute (percentage point) differences are declining with each price, we cannot reject that all three differences are equal (p-value = 0.18), and there is no such pattern in relative (percentage) differences.

4.3 Comparison of BDM and TIOLI Sub-treatments

We now analyze the BDM and TIOLI sub-treatments, beginning with BDM. Figure 4 displays the BDM demand curves for each of the three BDM sub-treatments. The graph shows slightly higher demand for the market study treatment relative to the standard treatment. The anchoring treatment yields a similar demand curve to the standard treatment.

Table 3 presents the results of two tests that compare the distributions of the BDM sub-treatments using both the Wilcoxon-Mann-Whitney rank-sum and Kolmogorov-Smirnov tests. In both tests, the distribution under the marketing treatment is significantly different from the standard treatment at the 10% level, while neither test rejects the null that the anchoring and standard treatments are the same.[17]

Turning to the three TIOLI sub-treatments, Figure 5 displays the percentage of respondents who accepted in each treatment and at each price. Our hypothesis was that demand would be lowest for the random TIOLI treatment and highest for anchoring TIOLI, with standard TIOLI in between. However, over all three prices, there is no clear pattern. Demand under the random

[17]Cluster-robust significance levels for the distributional tests are constructed via a bootstrap percentile method. We pool data from the two treatments being compared, draw block-bootstrap samples, where the compound is the block, and then randomly assign placebo treatments by compound and run the distributional test in question. Since the placebo treatments are randomly generated, the null hypothesis of equality of distribution is true by construction. By sampling compounds and assigning placebo treatments by compound, we preserve the clustering structure in the data. We repeat this for 999 bootstrap repetitions, and then obtain a p-value for our test by finding where the original test statistic falls in the distribution of bootstrap test statistics.

treatment is lower than under the standard treatment at a price of 2, but is higher at prices of 4 and 6. Demand under the anchoring treatment is higher than under the standard treatment at prices of 4 and 6, but not at a price of 2. In all cases, the 95-percent confidence intervals are overlapping (within each price).

To compare the mean acceptance behavior across treatments, we run the following regression separately for each price:

$$buy_{icp} = \alpha_0 + \alpha_1 \text{TIOLI\_ANCH}_c + \alpha_2 \text{TIOLI\_RAND}_c + \varepsilon_{icp} \tag{2}$$

where TIOLI_ANCH$_c$ is a dummy variable indicating the anchoring treatment, and TIOLI_RAND$_c$ is a dummy variable indicating the random treatment. Table 4 presents the results of these regressions. Among all six of the individual comparisons, the only significant difference is the difference between the anchoring and the standard treatments at a price of 4. However, the joint test that all three differences between the anchoring and standard treatments equals zero is rejected at the 10% level (p-value = 0.081).

In sum, the analysis of sub-treatments does not support the hypotheses put forward in Section (3.4.4). In fact, two of the relationships go in the opposite direction: the market study treatment increases demand relative to the standard BDM treatment, and the TIOLI anchoring sub-treatment seems to depress demand relative to the standard TIOLI sub-treatment. In what follows, we propose a model of BDM bidding consistent with these surprising facts, which can be tested in future laboratory and field work.

We caution against interpreting these results as supporting a conclusion that BDM does not or cannot "work," in a broadly defined sense. First, the maintained hypothesis that TIOLI yields a true measure of willingness to pay is tenuous.[18] Second, we chose a context and designed our experiment in such a way as to provide a severe – perhaps extreme – test of BDM. For example: numeracy among our subject pool was low; superstitious beliefs about probabilities are commonplace; we

[18]For example, Jack (2010) provides evidence that subjects respond less rationally to a take-it-or-leave-it offer of a payments for environmental services contract than when they participate in an auction.

did not tell subjects the distribution of prices; the good in question was new for respondents (although they were provided an opportunity to familiarize themselves with it) and yields benefits – reduced episodes of water-related disease – that are uncertain and difficult to quantify. All of these can work against BDM. Future work will investigate which of these factors, among others, are significant and inform improved versions of BDM.

4.4 A Model of BDM Bidding

While the BDM mechanism has long been described as unconditionally incentive compatible, Karni and Safra (1987) and Horowitz (2006a) show that this feature depends critically on the assumption of expected utility maximization. When preferences do not satisfy the von Neumann-Morgenstern independence axiom, the BDM procedure is not necessarily incentive compatible. More generally, an individual's willingness to pay for a product may depend on any number of the features of the mechanism for the potential purchase.

The following model serves to compare the optimal bidding strategy under BDM to the "true" willingness-to-pay, which we define as the highest price an individual would accept in a single take-it-or-leave-it offer. Note that the concept of a "true" value is somewhat arbitrary as the structure of the take it or leave it mechanism may itself impact an individual's willingness to pay. For example, as described above, the offer price may serve as an anchor or a signal of quality.

Suppose an individual has utility $u(w,c)$ where w is the value of possessing a certain item, W, and c is the monetary value of all other consumption. The quantity of W consumed is $q \in \{0,1\}$—that is, the individual can either consume one unit of W or not—and we can normalize w to q without loss of generality. Her income is Y, and if she purchases the good she pays a price p. We can then write her utility as $u(q, Y - qp)$.

Under BDM, the individual states her maximum bid, b, for the item. The price, p, is then drawn from a distribution $F(\cdot)$. If $b \geq p$ then she buys the item at price p. Otherwise, she pays nothing and does not receive the item. With expected utility, an individual's optimal bid solves:

$$\max_{b} \int_{0}^{b} u(1, Y - x)\, dF(x) + u(0, Y)(1 - F(b)) \tag{3}$$

Taking the first-order condition with respect to b and applying the fundamental theorem of the calculus, we can show that the optimal bid, b^*, satisfies $u(1, Y - b^*) = u(0, Y)$. Note that this is the price such that the respondent is exactly indifferent between purchasing the item and not purchasing, which is precisely the condition for the maximum take-it-or-leave-it offer to which she would agree. BDM elicits the "true" value of the item.

Under expected utility, this result also easily extends to the case of uncertain value to the purchase item. Assume, again without loss of generality, that w is distributed according to $G(\cdot)$ on the interval $[0,1]$. The individual's optimal bid now solves:

$$\max_{b} \left\{ \int_{0}^{b} \int_{0}^{1} u(w, Y - x)\, dG(w)\, dF(x) + u(0, Y)(1 - F(b)) \right\} \tag{4}$$

and the optimal bid, b^*, now satisfies $\int_0^1 u(w, Y - b^*)\, dG(w) = u(0, Y)$. Again, b^* is also the point at which the individual would be indifferent to a take-it-or-leave it offer, and BDM elicits the item's "true" value. Note, however, that this value is not necessarily the expected value of the item. In particular, if $u(w, c)$ is concave in consumption c, the optimal BDM bid and the maximum TIOLI acceptance will be identical but less than the expected value of w.

As shown in Machina (1982) and applied directly in Horowitz (2006a), when preferences do not necessarily conform to expected utility, the utility function is no longer independent of the distribution of prices. However, when preferences are smooth, the individual acts as a standard utility maximizer with a "local utility function" $u(w, c; F_b, G)$, where F_b is the induced distribution of q and c based on b.

Now, the optimal bid under BDM satisfies $u(1, Y - b^*; F_b, G) = u(0, Y; F_b, G)$. However, the maximum TIOLI acceptance price, b_T^*, now satisfies $u(1, Y - b_T^*; F_1, G) = u(0, Y; F_0, G)$, where F_1 is the degenerate distribution at $b = 1$, i.e. the respondent receives the item with certainty at price b_T^*, and F_0 is the degenerate distribution at $b = 0$, i.e. the respondent does not receive the item. Because of the dependence of the utility function on prices, it need not be the case that $b^* = b_T^*$. Thus, the model predicts that optimal BDM and TIOLI behavior may diverge. The size and direction of the divergence between BDM and TIOLI depends on the nature of the deviation from expected utility and the shape of underlying preferences. For example, consider a special case of Gul's model of disappointment aversion (Gul, 1991) where individuals with the opportunity to purchase a filter under BDM expect to "get a good deal" and are disappointed if the mechanism does not generate significant savings. This will lead individuals to underbid in BDM relative to TIOLI, and the extent of this underbidding will increase with greater risk aversion. If, however, individuals are relatively more disappointed by not being able to purchase the filter when they have a chance, then they will tend to overbid in BDM. Again, increased risk aversion exacerbates this deviation.

Continuing work seeks to uncover preferences that predict the deviation between BDM and TIOLI. We are collecting additional data on preference measures including risk aversion, loss aversion, and ambiguity aversion. The direction and magnitude of any divergence will depend on the interaction of these preference parameters and the nature of the independence assumption in expected utility. Rather than make significant assumptions about this functional form, we intend to remain agnostic and estimate reduced-form relationships between preference measures and bidding behavior in hopes of better understanding the deviations between BDM and TIOLI.

5 Baseline Characteristics and Willingness to Pay

While the analysis above presents strong evidence that demand as measured by BDM is lower than demand as measured by TIOLI, it is important to explore what information each measure contains. In this section we analyze the relationship between willingness to pay and baseline characteristics.

The relationship between willingness to pay and baseline characteristics is important to understand how pricing targets different types of households. Previous studies have found mixed evidence that higher willingness to pay for health goods is related to health characteristics or wealth (Ashraf, Berry and Shapiro, 2010; Cohen and Dupas, 2010; Cohen, Dupas and Schaner, 2012)) For our analysis, we model the relationship between willingness to pay and baseline characteristics and behaviors as follows:

$$\text{WTP}_{ic} = \alpha_0 + X_{ic}^{'}\beta + \varepsilon_i \tag{5}$$

where X_{ic} is a vector of the characteristics of interest for subject i in compound c and ε_{ic} is an error term, possibly correlated at the compound level. The characteristics we consider are whether a child aged 0-5 in the household has had diarrhea in the past two weeks, the number of children aged 0-5 in the household, the number of children aged 6-17 in the household, the respondent's years of education, a wealth index (the first principal component of a set of variables on ownership of durables, land and livestock), and an indicator for whether the respondent currently treats her water.

Estimating equation (5) using our BDM sample is straightforward: we regress the BDM bid on the vector of characteristics. Column 1 of Table 5 presents the results of this regression. The BDM bid is positively and significantly related to the child diarrhea indicator, durables ownership, and current treatment of water, all at the 10% level.[19] The p-value of the F-test of the regression equals 0.018, indicating that these characteristics are jointly significantly correlated with the BDM bid.

We now examine whether the relationship between willingness to pay and characteristics varies between our TIOLI and BDM treatments. Although willingness to pay is substantially lower in BDM compared with TIOLI, we might tolerate a level effect as long as the characteristics of those who choose to purchase are not systematically different between the two mechanisms. That is, we would like to be confident that the elicitation procedure is not substantially altering the association between important covariates and demand. If this is the case, then relationships estimated are more likely to provide useful guidance in how pricing is likely to target different types of households. In the remainder of this section, we present evidence addressing this point.

In order to compare demand correlates between the BDM and TIOLI treatments, we must

[19]Water treatment includes use of cloth filters, pipe filters, pot filters, boiling or chemicals.

first reduce the BDM data to simulate the data generated under a TIOLI procedure. From each BDM observation, we create three synthetic observations and assign to each a synthetic price of $p \in \{2,4,6\}$. We then use the BDM bids to simulate TIOLI purchase behavior at each price by creating a simulated purchase variable $\text{Buy}_{i,p} = 1\{\text{WTP}_i > p\}$. For TIOLI, we simply use the actual purchase behavior at the randomly assigned price. We continue to model the relationship between willingness to pay and baseline characteristics as in equation (5). For households assigned to the TIOLI treatment, we do not observe WTP, but a binary indicator for whether WTP is greater than a certain value. That is, for respondent i assigned price p, we observe

$$\text{Buy}_{i,p} = 1\{\text{WTP}_i \geq p_i\} = 1\{\text{WTP}_i - p_i \geq 0\} = 1\left\{\alpha_0 + X_{ic}'\beta + \varepsilon_{ic} - p_i \geq 0\right\}. \tag{6}$$

We therefore run a probit model on the TIOLI data and simulated BDM purchase data. In this case, because the latent variable WTP_i is of primary interest, we focus on the estimated β coefficients rather than on marginal effects. That is, by normalizing the coefficient on price to be equal to -1, the coefficients obtained via probit are directly interpretable in terms of willingness to pay.

Table 5 shows the results of estimating the probit equation above on the BDM (Column (2)) and TIOLI (Column (3)) samples individually.[20] Column (4) displays the differences. Comparing the BDM sample between the OLS in Column 1 and probit in Column 2, the results are broadly similar, but the conversion of the BDM bid data to synthetic purchase decisions has clearly decreased precision. Turning to the relationship between TIOLI willingness to pay and characteristics, most of the estimates are statistically indistinguishable from zero, except that a recent episode of diarrhea has a significantly negative impact on willingness to pay. Column 4 indicates that there are very few significant differences between the estimates for BDM and TIOLI, except for the child diarrhea variable. The difference in the estimates between the two samples is significant at the 1% level. Overall, the joint test that all of the differences between BDM and TIOLI are zero rejects at the

[20]Because each BDM respondent appears three times, the default standard errors will be too small, since they assume triple the sample size that we actually have. Therefore, we compute bootstrap standard errors in which, in each of 1,000 repetitions, we sample just one of the three observations for each respondent and run the probit estimation described above. The sample size reported in Table (5) is the actual number of BDM respondents, not the number of synthetic observations.

5% level (p-value = 0.039). This suggests that willingness to pay, as measured through the two mechanisms, may reflect different underlying characteristics between the two samples.

We also examine the relationships between willingness to pay and characteristics more flexibly. We consider two key continuous covariates, observable water quality and household wealth. We estimate the following equation via a flexible probit:

$$\begin{aligned}\text{Buy}_{icp} \quad = \quad & 1\left\{\alpha_0 + \alpha_1 x_{ic} + \alpha_2 x_{ic}^2 + \alpha_3 x_{ic}^3 + \right. \\ & \left. \beta_0 \text{BDM}_{ic} + \beta_1 \left(\text{BDM}_{ic} \times x_{ic}\right) + \beta_2 \left(\text{BDM}_{ic} \times x_{ic}^2\right) + \beta_3 \left(\text{BDM}_{ic} \times x_{ic}^3\right) + \varepsilon_{icp} > p\right\},\end{aligned} \tag{7}$$

where buy_{icp} is, for TIOLI subjects, an indicator for the subject purchasing at price p, and, for BDM subjects, an indicator for whether the subject's bid was at least p, BDM_{ic} is an indicator for a BDM subject, and x_{ic} is the covariate of interest. We are interested in whether the relationship between the propensity to purchase and x_{ic} varies between BDM and TIOLI subjects. That is, we are interested in the interaction term $\text{BDM}_{ic} \times x_{ic}$ and its powers. Because coefficients on interaction terms in nonlinear models can be difficult to interpret (Ai and Norton, 2003), we instead present predicted probabilities (with confidence intervals) for TIOLI and BDM subjects for a range of values of x_{ic}. We estimate equation (7) for each TIOLI price $p \in \{2, 4, 6\}$. Figures 6, 7 and 8 plot results for prices of 2, 4, and 6, respectively, where the covariate x_{ic} is the turbidity of the subject's household water. While there is certainly a level effect of BDM, the overall shape of the relationship between demand and turbidity is similar between BDM and TIOLI. In Figures 9, 10, and 11, we repeat the same exercise using our wealth index, and again find that the shapes of the relationships are similar.

We view these results as support for the idea that while BDM and TIOLI may not provide exactly the same information on the relationship between demand and observables, BDM nevertheless provides useful and precise information.

6 Screening and Sunk-cost effects

As noted in the introduction, the BDM mechanism allows us to identify whether prices screen out less intensive users, or whether higher prices have a causal effect on use. That is, we are able to separate the extent to which usage of the filter varies by willingness to pay (independent of price paid) and by price paid (independent of willingness to pay). It is possible to identify these effects separately because the BDM elicits willingness to pay before randomly assigning price paid. Because filter usage is only measurable among buyers (i.e., those with bids above their draw), we restrict our estimates to the sample of buyers. Among this group, price paid is still random conditional on willingness to pay.

We utilize four sets of usage measures. First, we include indicators for whether the filter was found in the compound, and whether it was found *and* undamaged. Second, we include two surveyor-observed measures based on the water level in the plastic bucket. Water can only be drawn from the bucket once the water level in the bucket reaches the level of the spigot (after about one-half liter of filtering). Water below the level of the spigot indicates that the bucket has been emptied or has been in disuse for some time. We therefore use a variable indicating the water level at or above the level of the spigot, and one indicating the water level strictly above the level of the spigot. Third, we use an indicator of whether the surveyor observes water in the clay filter pot. This indicates that the filter is actively filtering water. Finally, the one-month followup data include a self-report of how many times per day the respondent fills the filter.

Tables 6A and 6B estimate screening and causal effects of prices among respondents who received the BDM treatment, using the one-month and one-year followup data, respectively.[21] These tables present ordinary-least-squares estimates of the regression of usage on the BDM bid (willingness to pay) and draw (price paid):

$$\text{use}_{ic} = \alpha_0 + \alpha_1 \text{WTP}_{ic} + \alpha_2 D_{ic} + \varepsilon_{ic}$$

[21] The variables indicating whether the filter was found are not included in the two-week followup measures because over 92% of buyers had functional filters as at the time of the survey.

where use_{ic} represents the usage measure, WTP_{ic} is the respondent's BDM bid, and D_{ic} is her draw. Provided that the effects of each of these on usage are linear, the coefficients separately identify screening and causal effects. Tables 6A and 6B indicate that there is little evidence of either effect across all usage measures and both at two-week and one-year followup. Households who bid more reported filling their filter slightly more often at the two-week followup (significant at the 10% level), but given that the evidence is inconsistent across usage measures we cannot conclude that there is a strong screening effect of prices in this context.

We also compare the relationship between willingness to pay and use between the BDM and TIOLI treatments. There are several issues to confront in making this comparison. First, because the TIOLI treatment did not include a second-stage randomization of the price paid, the relationship between prices and use could reflect both screening and causal effects.[22] Second, as in Section (5), we must make the BDM and TIOLI data comparable. We use the same procedure of constructing 3 synthetic purchase decisions, one at each TIOLI price, from each BDM observation. We then include in our regressions actual BDM buyers who would have bought at the generated price ("synthetic buyers"). We run the following OLS regression, both among TIOLI buyers and BDM synthetic buyers:

$$\text{use}_{ic} = \alpha_0 + \alpha_1 1\{\text{WTP}_{ic} \geq X\} + \varepsilon_{ic}$$

The results of this exercise are presented in Tables 7A and 7B. Overall, these tables present little evidence for a relationship between price and use for either the BDM or TIOLI treatments. The results for the BDM treatment are similar to the screening effects results in Tables 6A and 6B, except that at the two-week followup there is now a *negative* relationship between price and whether the ceramic pot contains water (significant at the 10% level). At the one-year followup, higher prices in the TIOLI treatment are positively associated with the respondent having her filter and having the filter undamaged. However, the other measures do not indicate more intensive use at higher prices.

[22] Because we cannot separately identify screening and causal effects in this section, if the true screening and causal effects are different from zero but have opposite signs, we may still find a null result.

7 Heterogeneous Treatment Effects

The random nature of the BDM price draw creates an experiment that allows estimation of treatment effects, in which the random price draw provides an instrument for allocation of the filter. In this BDM is no different than TIOLI, which also provides an instrument: the random offer price. However, BDM additionally allows for the estimation of *heterogeneous* treatment effects, where treatment effects are allowed to vary by willingness to pay.

Heckman and Vytlacil (2005) and Heckman, Urzua and Vytlacil (2006) provide local instrumental variables (LIV) methods for estimating marginal treatment effects when there is unobserved heterogeneity in gains to treatment. Central to their method is the propensity score $p(z) = P(T = 1|Z = z)$, the probability of being treated when the value of the exogenous instrument Z is z. The marginal treatment effect at z, then, is the change in the outcome of interest on those brought into treatment by small changes in Z around z: $\partial E[y|p(z)]/\partial p(z)$.[23] Heckman, Urzua and Vytlacil (2006) provide semi-parametric methods for estimating this quantity.

However, in our context, BDM permits us to observe directly one key dimension of heterogeneity: willingness to pay.[24] We can therefore directly estimate heterogeneous treatment effects by conditioning standard IV methods on the value of willingness to pay.[25]

To illustrate the value of this approach, we first present simple IV estimates using the random TIOLI price as an instrument for take-up among the TIOLI subjects and the BDM draw as an instrument for take-up among BDM subjects. More precisely, we estimate the following outcome equation by linear two-stage least squares:

$$y_{ic} = \beta_0 + \beta_1 T_{ic} + x_{ic}'\beta_2 + u_{ic}, \tag{8}$$

where y_{ic} is the outcome of interest (cases of diarrhea among children age 5 or below in the previous 2 weeks) for subject i in compound c, T_{ic} is a dummy for whether the subject possesses a filter,

[23] Intuition for this object can be developed by seeing it as a differential analogue of the traditional Wald estimator $(E[Y|Z=1]-E[Y|Z=0])/(P[T|Z=1]-P[T|Z=0])$ in the case of a binary instrument.

[24] There may, of course, be other interesting dimensions of heterogeneity, which BDM will not allow us to observe.

[25] We are grateful to Sergio Urzua for discussions on this point.

and x_{ic} is a vector of covariates. To instrument for the endogenous treatment variable, we use the following first-stage equation:

$$T_{ic} = \gamma_0 + \gamma_1 P_{ic} + x'_{ic}\gamma_2 + v_{ic}, \tag{9}$$

where P_{ic} is the TIOLI offer price for TIOLI subjects and the BDM draw for BDM subjects. Since P_{ic} is random, it is uncorrelated with u_{ic} and therefore it is a valid instrument for treatment.

Table 8 presents results from this estimation for our short-term (one month) follow-up data. In columns (1) and (2), we use only the TIOLI observations; with raw 2SLS in column (1) and adding covariates in column (2);[26] in columns (3) and (4), we use only the BDM observations; and in columns (5) and (6) we pool the TIOLI and BDM data. At first glance, it would appear there is little benefit to BDM: the estimates have the same sign and are of similar magnitude, with the TIOLI estimates being somewhat larger in magnitude and showing greater statistical significance.

However, the advantage of BDM in this context is that it allows us to uncover a richer pattern of treatment effects than just one average LATE for all compliers. With BDM, we can consider treatment effects separately at different levels of willingness to pay. To implement this, we conduct a set of kernel IV regressions in the neighborhood of each level of WTP.[27] We perform this exercise for each GHS 0.1 step between GHS 1 and 6 (inclusive), both with and without controls.[28]

The top panel of Figure 12 presents results from kernel IV regressions with the short-term health measure as the dependent variable and no controls. We see evidence of an inverse-U shaped relationship, with benefits appearing to be highest among respondents with WTP of between 3 and 4 GHS (corresponding roughly to the 60th to 75th percentile of the distribution of bids). In the

[26] We use the same controls as the demand regressions in Section 5, including village fixed-effects.

[27] An alternative method would be to divide subjects into bins by willingness to pay (e.g. terciles or quartiles) and estimate separate 2SLS regressions. In our study, this was not suitable because there was substantial lumpiness in bids (i.e. a large number of subjects bid 2 GHS exactly), which made the estimation quite sensitive to the choice of endpoints of the bins. Another alternative would be to interact the instrument with powers of WTP. We did not have an ex-ante reason to expect any particular shape of the relationship between WTP and treatment effects, so we chose to let the data decide via kernel estimate.

[28] Notes on implementation: we do not include village fixed effects in this set of regressions as some villages have zero or one subject in the domain of the kernel. We use an Epanechnikov kernel and Silverman's rule of thumb to choose the bandwidth.

bottom panel, we present sample sizes and Shea's partial R-squared statistics for each level of WTP at which we took a kernel estimate (Shea, 1997). Since the distribution of WTP is concentrated in the lower values (median 2.5), the effective sample size falls as WTP increases. However, our confidence intervals do not grow as rapidly as might be expected, because the instrument is stronger at higher levels of WTP. Intuitively, the instrument has less strength at lower levels of WTP because these individuals have only a small chance of receiving the filter. Figure 13 repeats the exercise, adding controls to the regression, with similar results.

In Table 9 and Figures 14 and 15, we examine our long-term data, collected in a random subsample of half our villages roughly one year after the filter sale. The benefits of the ability to conduct local IV estimation are even more readily apparent in these longer-term data. Table 9 would suggest that the filter is no longer beneficial after one year. However, this overlooks the substantial fraction of the population that does benefit, as can be seen in Figures 14 and 15.

8 Conclusion

This paper provides an empirical test of whether BDM provides an accurate measure of an individual's willingness to pay through the sales of point-of-use water filters in Ghana. We provide strong evidence that BDM under-predicts willingness to pay relative to TIOLI. We find evidence against several potential explanations for this difference, including strategic bidding and anchoring. We demonstrate the usefulness of BDM in measuring heterogeneous treatment effects and testing for screening and sunk-cost effects.

We conclude by describing the direction of our future research. First, we will explore the alternative explanations driving the differences between willingness-to-pay estimates through BDM and TIOLI. As outlined in the model in Section (4.4), these differences could be driven by ambiguity aversion, disappointment aversion, in addition to strategic behavior. To distinguish among these potential channels, we measure risk preferences among our original sample through measurement of risk, ambiguity, and disappointment aversion, as in e.g., Holt and Laury (2002).

Second, we plan to use the results of the analyses described above to refine BDM methods so that it is more readily understood by participants and more amenable to implementation in the field, while retaining the desirable characteristics of BDM, in particular improved precision in measuring WTP, the ability to heterogeneous treatment effects, and to estimate selection and sunk cost effects.

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Table 1: Sample Composition

	Assigned		Sales Offer Made	
	Compounds	Subjects	Compounds	Subjects
BDM Standard	99	249	88	213
BDM Marketing	93	240	81	196
BDM Anchoring	97	242	83	196
Total BDM	289	731	252	605
TIOLI Standard	100	264	85	220
TIOLI Anchor	94	247	86	211
TIOLI Random	99	268	87	229
Total TIOLI	293	779	258	660
Total	582	1510	510	1265

Notes: Our unit of observation is any primary caretaker of one or more children age 12 and under. A compound is an extended patrilineal family of several sub-families living in a cluster of homes. Treatments were randomized at the compound level and all inference is robust to clustering at the compound level. Our activities were conducted in 14 villages, selected according to several criteria described in the text.

Table 2: BDM Effect

	price=2		price=4		price=6	
BDM	-0.185	**	-0.159	**	-0.091	*
	(0.033)		(0.053)		(0.039)	
Constant	0.916	**	0.470	**	0.199	**
	(0.024)		(0.046)		(0.035)	
Observations	841		822		789	
Num. clusters	391		385		374	

+ denotes significant at 0.10; * at 0.05; ** at 0.01

p-value for joint test that BDM=0 in all three equations: 0.000.

Notes: the dependent variable is a dummy variable indicating either that the individual did purchase the filter at the indicated price (for take-it-or-leave-it subjects) or that the individual's willingness to pay was as least as high as the indicated price (for BDM subjects). The intercept represents the share purchasing under TIOLI, while the coefficient on BDM indicated the difference in demand between BDM and TIOLI at that price. Each BDM subject appears in all three regressions, while each TIOLI subject appears only in the regression corresponding to her offer price. The p-value for a joint test that the BDM coefficient is zero in all three equations is calculated from SUR estimation. All standard errors account for clustering at the compound (extended family) level.

Table 3: Equality of Distributions of Bids
Comparison with Standard BDM

	Wilcoxon	
	Market	Anchor
Z-statistic	2.82	-0.92
P-value	0.022	0.758
Num. Obs.	408	407
	Kolmogorov-Smirnov	
	Market	Anchor
D-statistic	0.141	0.061
P-value	0.052	0.756
Num. Obs.	408	407

Notes: this table reports results of nonparametric tests for equality of bid distributions across BDM treatments. The anchoring and marketing treatments (described in the text) are separately compared to standard BDM. P-values are calculated via cluster-bootstrapping with resampling at the compound level.

Table 4: Differences between TIOLI subtreatments

	price=2	price=4	price=6
Random TIOLI	0.009	-0.135	0.024
	(0.063)	(0.105)	(0.084)
Anchoring TIOLI	0.053	-0.226 +	-0.073
	(0.060)	(0.118)	(0.079)
Constant	0.899	0.594	0.218
(Standard TIOLI)	(0.053)	(0.084)	(0.054)
Observations	238	219	186
Num. clusters	119	113	102

+ denotes significant at 0.10; * at 0.05; ** at 0.01

p-val for joint test that coefficient on Random TIOLI = 0 in all three equations: 0.595. p-val for joint test that coefficient on Anchoring TIOLI = 0 in all three equations: 0.081.

Notes: this table reports results of a linear probability model for purchase of the filter at the take-it-or-leave-it price indicated in the column header. The ommitted category is standard TIOLI. The p-values for joint tests across equations are calculated from SUR estimation. All standard errors account for clustering at the compound (extended family) level.

Table 5: Relationship Between Willingness to Pay and Baseline Characteristics

	WTP OLS		Purchase Decision Probit					
			BDM		TIOLI		Diff.	
	(1)		(2)		(3)		(4)	
Child 0-5 had diarrhea past two weeks	0.598	+	0.416		-0.41	+	-0.826	*
	(0.320)		(0.305)		(0.246)		(0.392)	
Number of children 0-5	0.14		0.066		-0.095		-0.162	
	(0.114)		(0.121)		(0.107)		(-0.096)	
Number of children 6-17	0.015		0.053		0.169	*	0.116	
	(0.064)		(0.072)		(0.078)		(-0.051)	
Years education	0.066		-0.007		-0.044		-0.037	
	(0.077)		(0.070)		(0.052)		(-0.053)	
First principal component of durables	0.122	+	0.077		-0.007		-0.083	
	(0.070)		(0.077)		(0.072)		(-0.062)	
Water currently treated	0.413	+	0.42		0.376		-0.044	
	(0.236)		(0.272)		(0.269)		(-0.214)	
Price (GHS)			-1		-1			
			(.)		(.)			
P-value: joint significance	0.018		0.041		0.039		0.021	
Num. Obs.	600		602		660		1,262	

+ denotes significant at 0.10; * significant at 0.05; ** significant at 0.01

Notes: Column (1) displays coefficients from a linear regression of WTP (BDM bid) on key baseline characteristics. Columns (2) and (3) display coefficients from probit regressions of the household's purchase decision on the same covariates. For the BDM subjects (column (2)), the purchase decision is synthetic, constructed as an indicator for the subject's bid being at least as high as a randomly generated synthetic TIOLI price in {2,4,6}. (See discussion in the text.) Because the coefficient on price in columns (2) and (3) is normalized to -1, the estimated coefficients can be interpreted as analogous to willingness to pay and are directly comparable to those in Column (1). Column (4) displays differences between coefficients. Standard errors clustered at the compound (extended family) level in parentheses. See the text for a discussion of the bootstrap procedure used to calculate these standard errors.

Table 6.A: Screening and Causal Effects of Prices
Regression of use on bid and draw

	Dependent Variable			
	Water at or Above Spigot (1)	Water At Spigot (2)	Pot contains water (3)	Fill Freq. (4)
Bid	-0.018	-0.016	-0.007	0.053*
	(0.014)	(0.015)	(0.014)	(0.027)
Draw	0.033	-0.010	-0.012	-0.038
	(0.021)	(0.027)	(0.023)	(0.040)
Mean Dep. Var.	0.757	0.528	0.728	1.521
R-squared	0.013	0.010	0.005	0.024
Num. Obs.	235	235	235	218

+ denotes significant at 0.10; * at 0.05; ** at 0.01

Notes: The sample includes those in the BDM treatment who purchased the filter. Each column presents the results of a separate regression of use on BDM bid and BDM draw, among those whose draw was below their bid.

Table 6.B: Screening and Causal Effects of Prices after One Year
Regression of use on bid and draw

	Dependent Variable				
	Filter present	Filter undamaged	Water at or Above Spigot	Water above spigot	Pot contains water
	(1)	(2)	(3)	(4)	(5)
Bid	-0.022	-0.006	0.020	-0.007	-0.019
	(0.014)	(0.015)	(0.016)	(0.016)	(0.014)
Draw	0.028	0.002	0.022	0.018	0.023
	(0.018)	(0.032)	(0.032)	(0.030)	(0.030)
Mean Dep. Var.	0.880	0.655	0.486	0.310	0.380
R-squared	0.026	0.001	0.027	0.003	0.009
Num. Obs.	142	142	142	142	142

+ denotes significant at 0.10; * at 0.05; ** at 0.01
Notes: The sample includes those in the BDM treatment who purchased the filter. Each column presents the results of a separate regression of use on BDM bid and BDM draw, among those whose draw was below their bid.

Table 7.A: Relationship between WTP and Use

	TIOLI	BDM	Difference
	(1)	(2)	(3)
Panel A: Dependent Variable: Water at or Above Spigot			
Price	-0.001	-0.021	-0.020
	(0.017)	(0.015)	(0.022)
Mean Dep. Var.	0.749	0.738	
R-squared	0.000	0.005	
Observations	319	355	
Panel B: Dependent Variable: Water Above Spigot			
Price	-0.021	-0.034*	-0.013
	(0.022)	(0.016)	(0.027)
Mean Dep. Var.	0.476	0.482	
R-squared	0.003	0.009	
Observations	319	355	
Panel C: Filter Pot Contains Water			
Price	-0.008	-0.020	-0.011
	(0.020)	(0.015)	(0.024)
Mean Dep. Var.	0.794	0.774	0.783
R-squared	0.001	0.004	
Observations	296	327	
Panel D: Dependent Variable: Frequency of filling			
Price	-0.005	0.074**	0.080
	(0.044)	(0.026)	(0.051)
Mean Dep. Var.	1.481	1.620	
R-squared	0.000	0.018	
Observations	289	326	

+ denotes significant at 0.10; * at 0.05; ** at 0.01

Notes: Columns 1 and 2 present the results of regressions of use on price. For TIOLI treatments, price equals the assigned price. Price equals WTP≥X for X={2, 4, 6} for BDM observations. Each BDM observation is split into three observations (one for each price) for the purposes of these regressions. See text for additional details. Column 3 presents the difference in coefficients between the TIOLI sample (Column 1) and the BDM sample (Column 2).

Table 7.B: Relationship between WTP and use after one year

	TIOLI	BDM	Difference
	(1)	(2)	(3)
Panel A: Dependent Variable: Filter Found			
Price	0.037+	-0.015	-0.051*
	(0.018)	(0.014)	(0.023)
Mean Dep. Var.	0.820	0.861	
R-squared	0.015	0.004	
Observations	178	223	
Panel B: Dependent Variable: Filter Undamaged			
Price	0.043+	-0.018	-0.061*
	(0.024)	(0.018)	(0.030)
Mean Dep. Var.	0.635	0.646	
R-squared	0.014	0.003	
Observations	178	223	
Panel C: Dependent Variable: Water at or above spigot			
Price	-0.006	-0.000	0.006
	(0.030)	(0.021)	(0.036)
Mean Dep. Var.	0.382	0.520	
R-squared	0.000	0.000	
Observations	178	223	
Panel D: Dependent Variable: Water above spigot			
Price	0.007	-0.022	-0.029
	(0.028)	(0.014)	(0.032)
Mean Dep. Var.	0.258	0.314	
R-squared	0.000	0.004	
Observations	178	223	
Panel E: Dependent Variable: Pot contains water			
Price	0.009	-0.021	-0.030
	(0.028)	(0.017)	(0.033)
Mean Dep. Var.	0.354	0.368	
R-squared	0.001	0.004	
Observations	178	223	

+ denotes significant at 0.10; * at 0.05; ** at 0.01

Notes: Columns 1 and 2 present the results of regressions of use on price. For TIOLI treatments, price equals the assigned price. Price equals WTP≥X for X={2, 4, 6} for BDM observations. Each BDM observation is split into three observations (one for each price) for the purposes of these regressions. See text for additional details. Column 3 presents the difference in coefficients between the TIOLI sample (Column 1) and the BDM sample (Column 2).

Table 8: IV Estimates of Short-Term Treatment Effects

	TIOLI Subjects		BDM Subjects		All Subjects	
	(1)	(2)	(3)	(4)	(5)	(6)
Filter Purchase	-0.149*	-0.134*	-0.083	-0.108+	-0.118**	-0.121**
	(0.060)	(0.062)	(0.061)	(0.057)	(0.042)	(0.041)
Controls:	No	Yes	No	Yes	No	Yes
Village FEs:	No	Yes	No	Yes	No	Yes
Mean dep. var.:	0.203	0.202	0.184	0.189	0.193	0.195
R-squared	-0.005	0.083	-0.006	0.097	-0.006	0.055
N	424	415	365	358	792	776

+ denotes significant at 0.10; * significant at 0.05; ** significant at 0.01

Notes: Dependent variable is number of cases of diarrhea in children aged 0-5 over the previous 2 weeks, roughly one month after the filter sale. Filter purchase is instrumented by TIOLI price for TIOLI subjects and BDM draw for BDM subjects. Standard errors clustered at the compound (extended family) level in parentheses.

Table 9: IV Estimates of One-year Treatment Effects

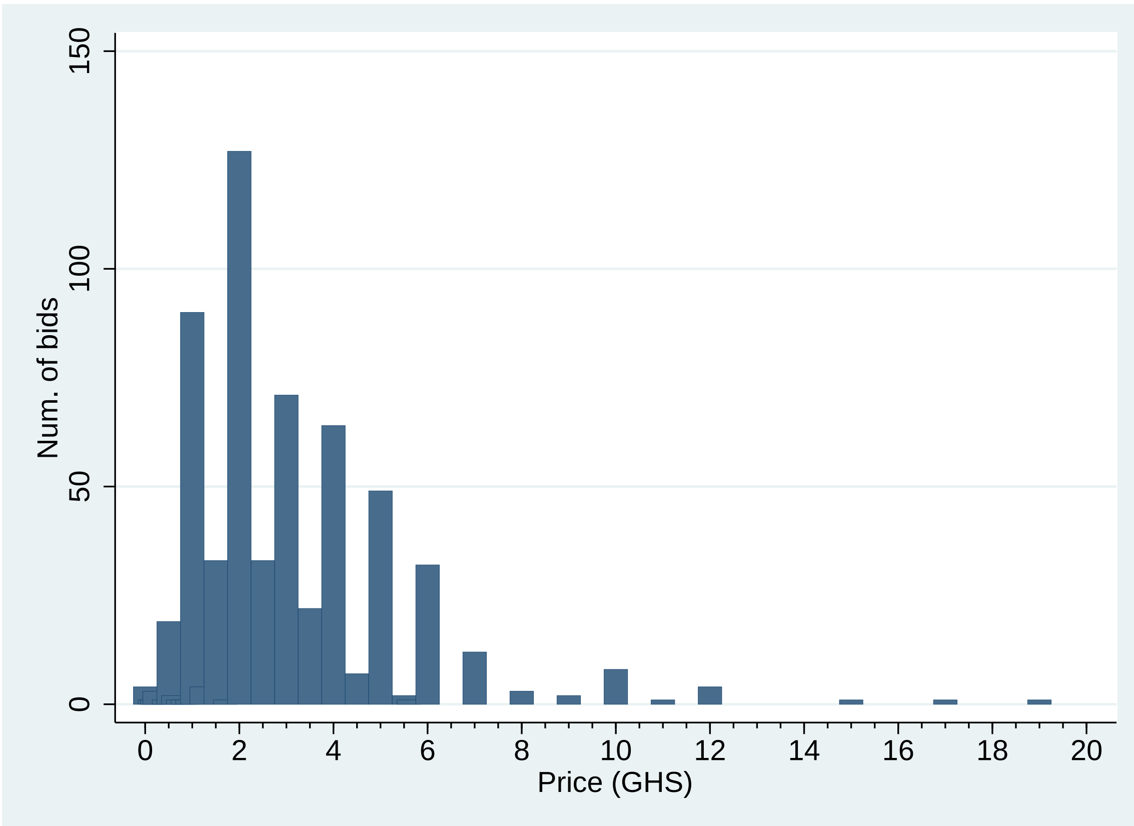
	TIOLI Subjects		BDM Subjects		All Subjects	
	(1)	(2)	(3)	(4)	(5)	(6)
Filter Purchase	0.105	0.128	0.059	0.104	0.069	0.121+
	(0.099)	(0.104)	(0.102)	(0.099)	(0.071)	(0.072)
Controls:	No	Yes	No	Yes	No	Yes
Village FEs:	No	Yes	No	Yes	No	Yes
Mean dep. var.:	0.224	0.221	0.282	0.255	0.253	0.238
R-squared	-0.005	0.035	-0.002	0.067	-0.002	0.031
N	242	236	207	202	449	438

+ denotes significant at 0.10; * significant at 0.05; ** significant at 0.01

Notes: Dependent variable is number of cases of diarrhea in children aged 0-5 over the previous 2 weeks, roughly one year after the filter sale. Filter purchase is instrumented by TIOLI price for TIOLI subjects and BDM draw for BDM subjects. Standard errors clustered at the compound (extended family) level in parentheses.

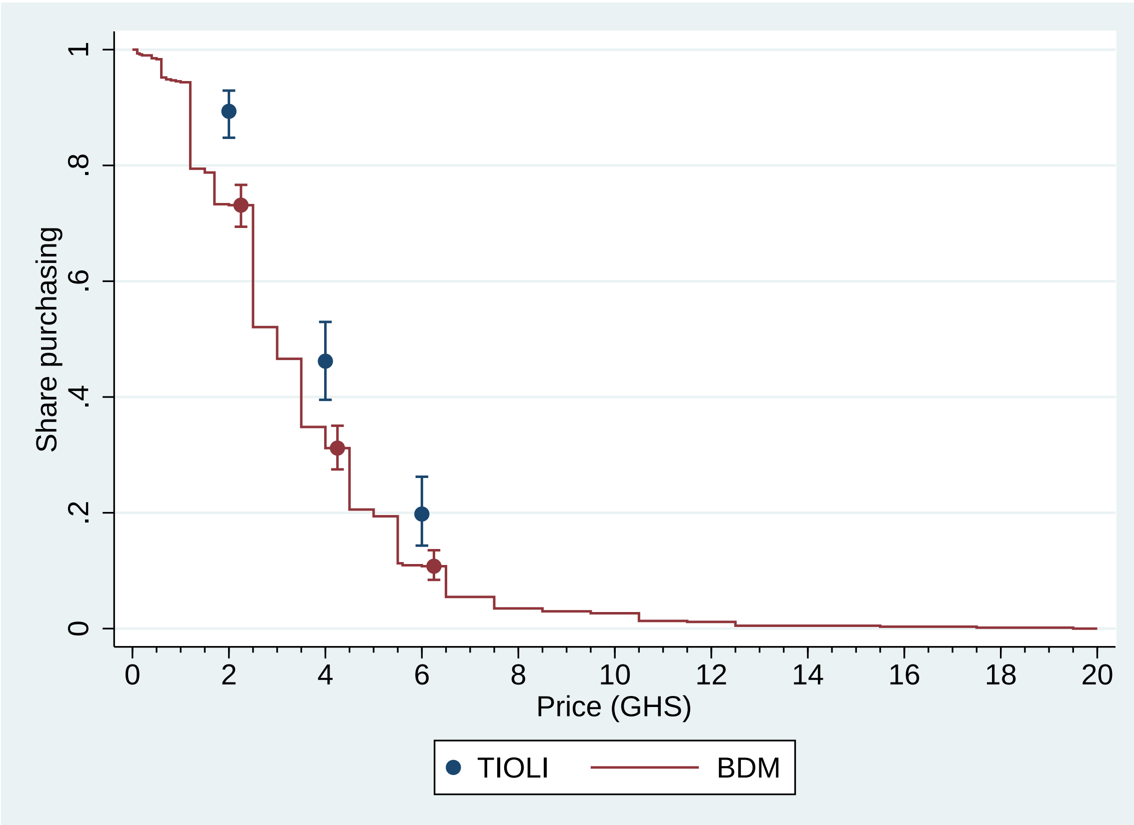
Figure 1
Kosim Filter

Figure 2
Histogram of BDM Bids



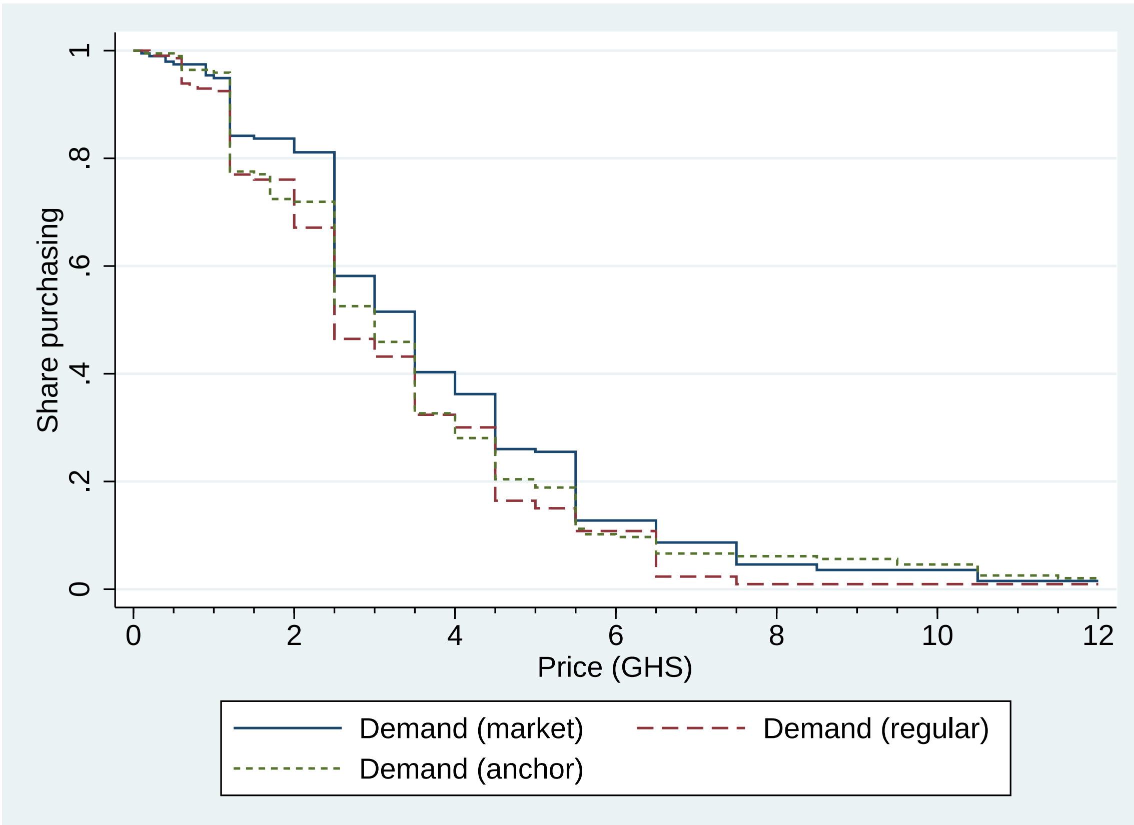
Notes: this figure plots a histogram of all BDM bids for the filter (604 observations total). Approximately 1.5 GHS per USD.

Figure 3
BDM-TIOLI Comparison



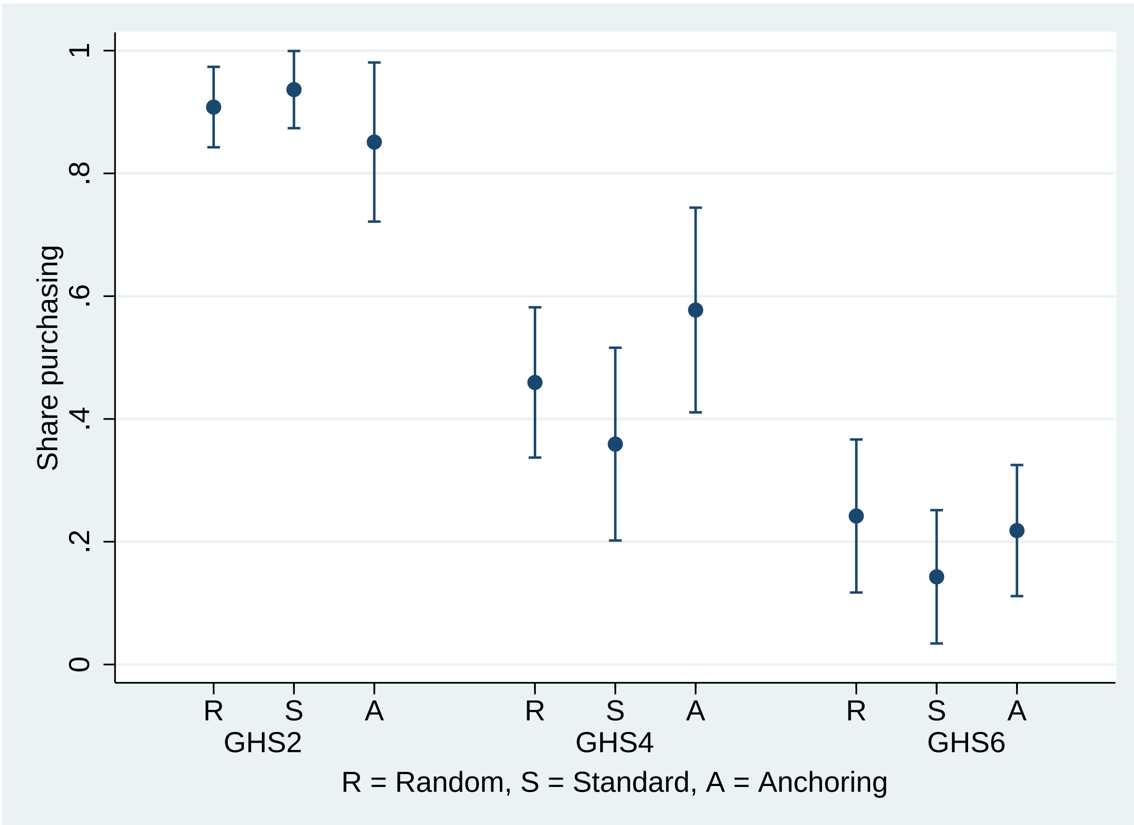
Notes: the solid line plots the share of BDM respondents with bids at or above the indicated price. In the BDM case, the markers indicate the share of respondents with a bid greater than or equal to the indicated price, with 95% confidence intervals. In the TIOLI case, the markers indicate the share of respondents who purchased the filter at the offered price with 95% confidence intervals. 603 BDM observations. 660 TIOLI observations, of which 244 at a price of 2, 223 at a price of 4 and 187 at a price of 6.

Figure 4
Comparison of BDM Sub-treatments



Note: the standard, anchoring and marketing treatments are described in detail in the text. Bids are truncated at 12 GHS because there are no appreciable differences among the three treatments at prices above 12. 605 observations total, of which 213 are standard BDM, 196 are marketing BDM and 196 are anchoring BDM. All treatments were randomized at the compound (extended family level); prices were drawn by individual respondents.

Figure 5
Comparison of TIOLI Sub-treatments



Notes: this graph plots demand for the filter at each take-it-or-leave-it price, for each TIOLI sub-treatment. The random TIOLI, regular TIOLI and anchoring TIOLI treatments are described in detail in the text. Each treatment was randomized at the compound level. For the regular and anchoring TIOLI treatments, the price was also randomized at the compound level. For the random TIOLI treatment, the price was drawn by individual respondents. 660 observations, of which: random 229 (GHS2 87, GHS4 74, GHS6 62); anchoring 220 (GHS2 94, GHS4 71, GHS6 55); anchoring 211 (GHS2 63, GHS4 78, GHS6 70).

Figure 6
Demand and Turbidity: BDM vs. TIOLI

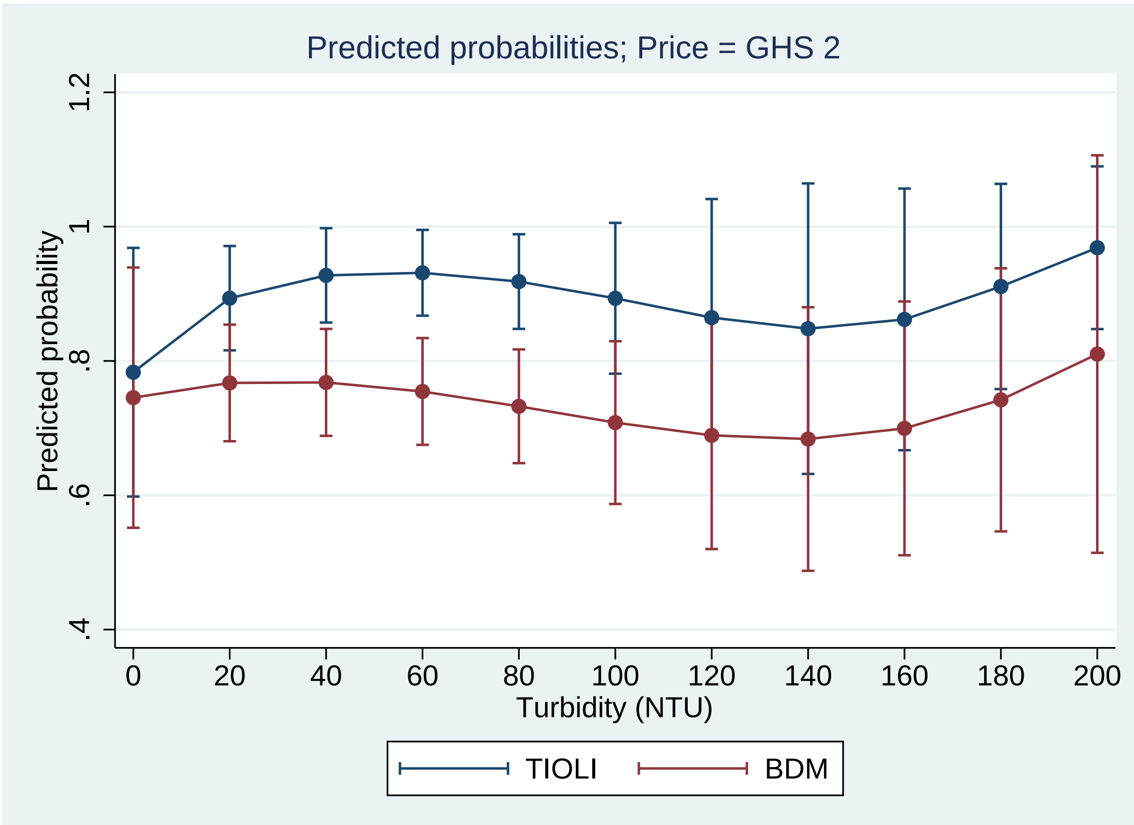


Figure 7
Demand and Turbidity: BDM vs. TIOLI

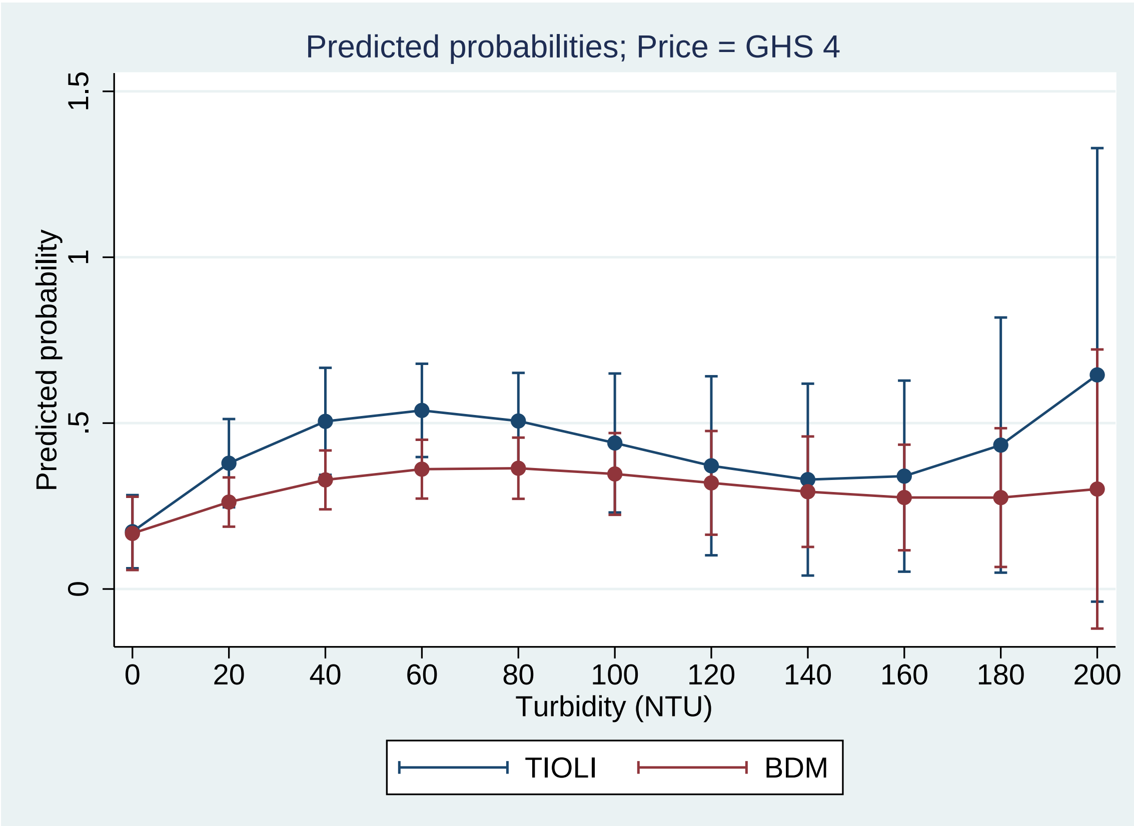


Figure 8
Demand and Turbidity: BDM vs. TIOLI

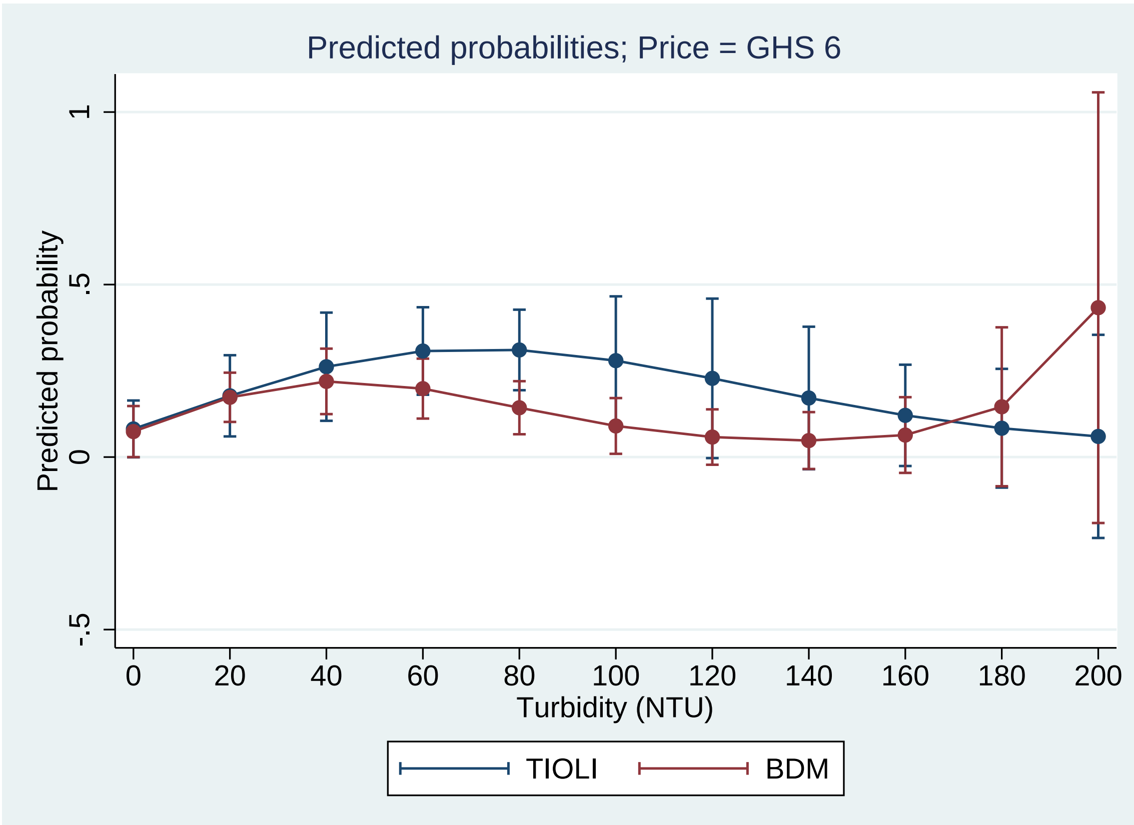


Figure 9
Wealth Index: BDM vs. TIOLI

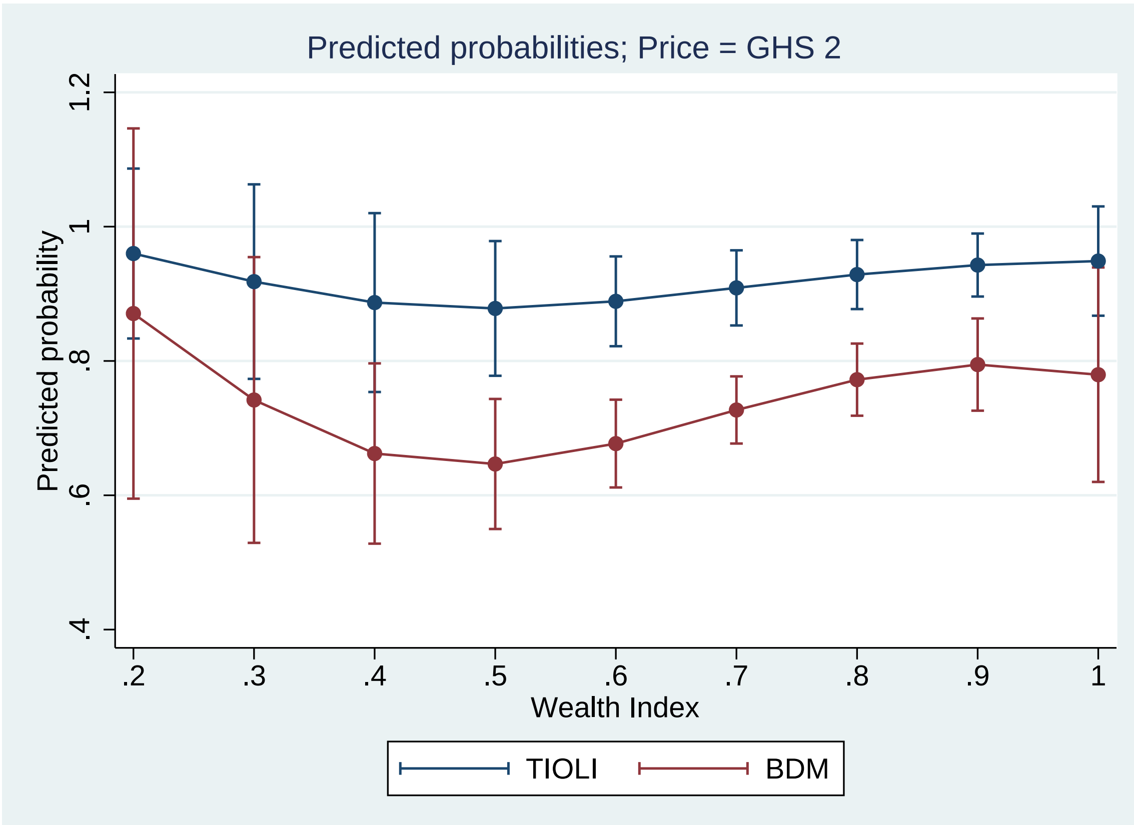


Figure 10
Wealth Index: BDM vs. TIOLI

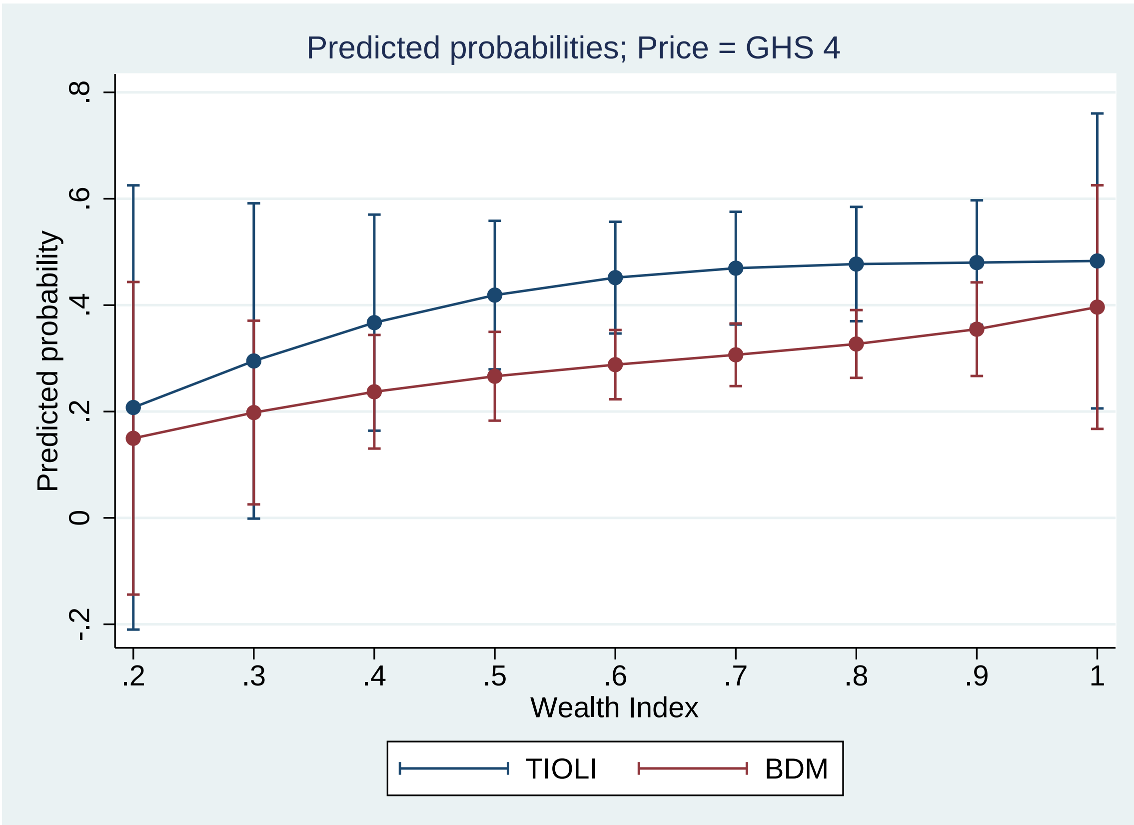


Figure 11
Wealth Index: BDM vs. TIOLI

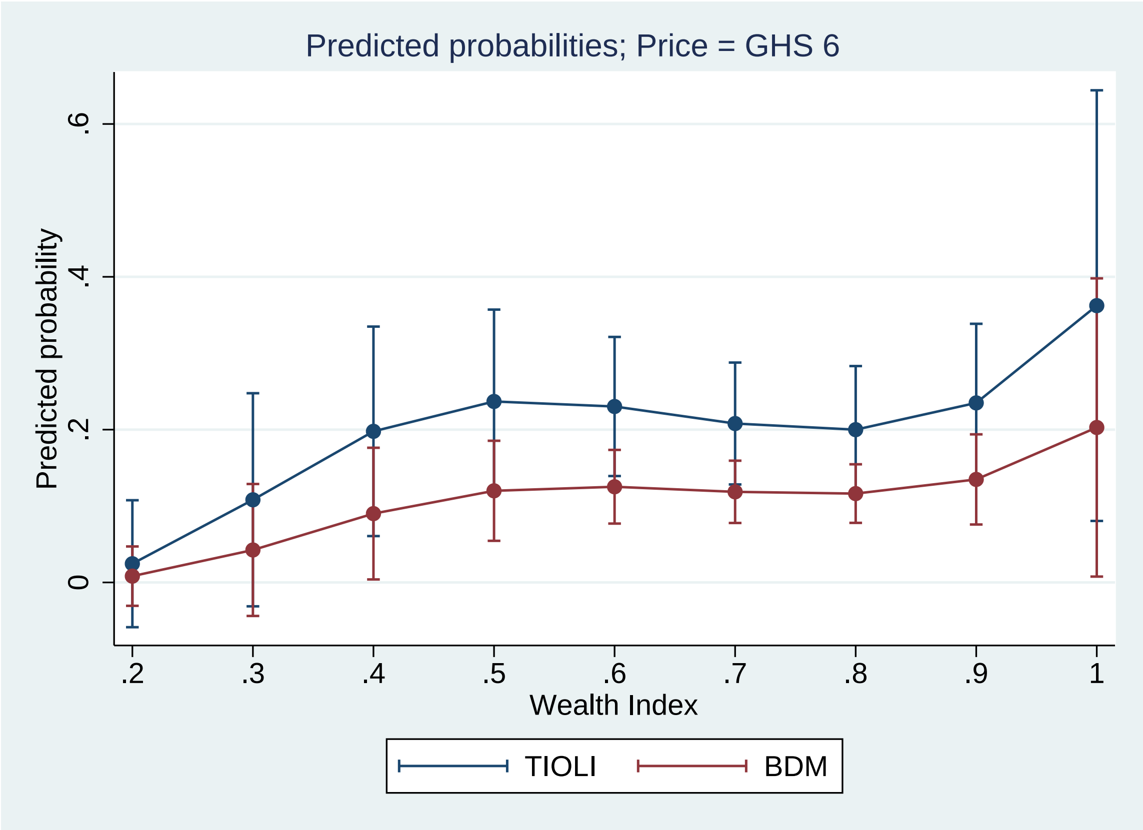


Figure 12
Local IV Estimates of Short-Term Treatment Effects – No Controls

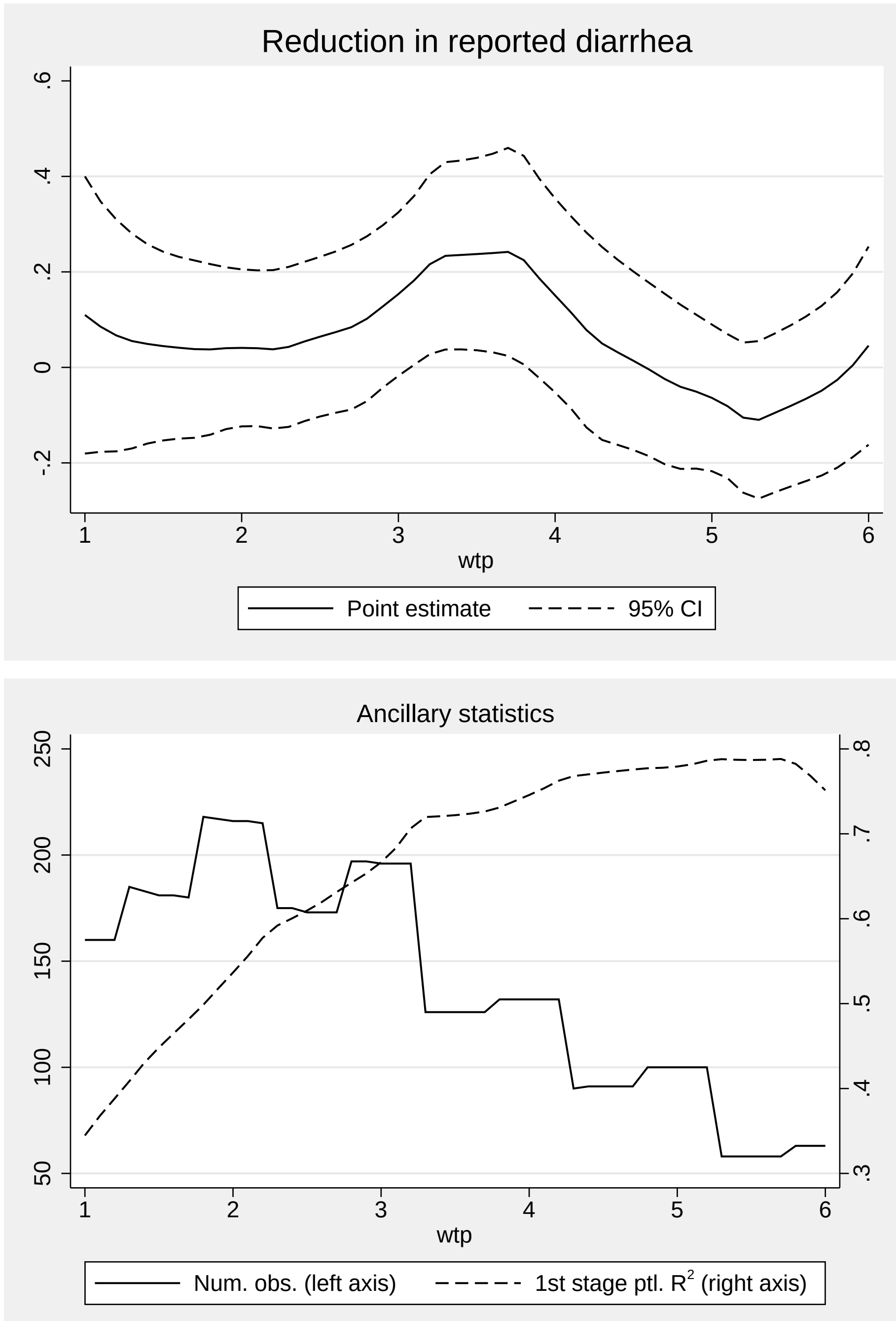


Figure 13
Local IV Estimates of Short-Term Treatment Effects – With Controls

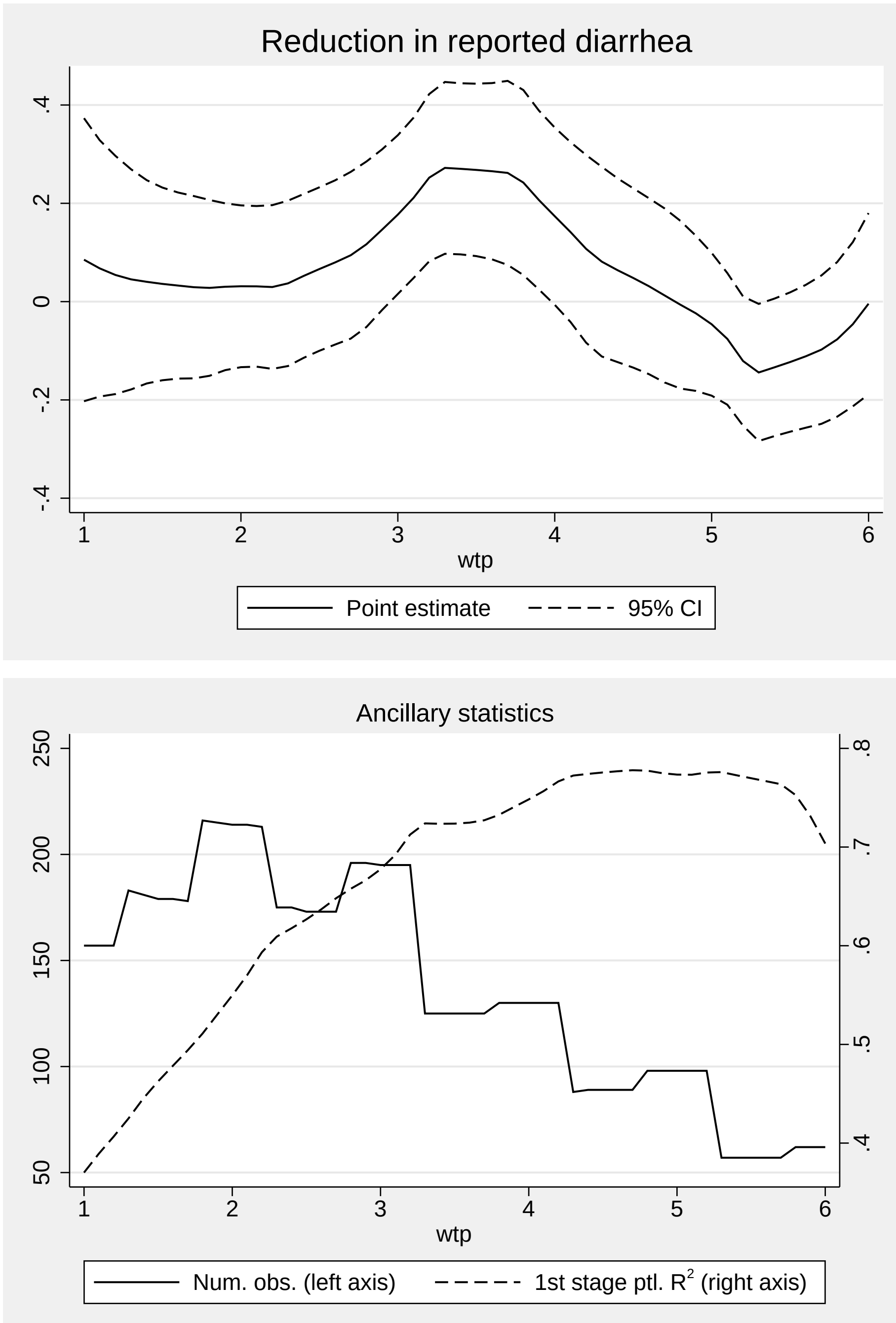


Figure 14
Local IV Estimates of One-Year Treatment Effects – No Controls

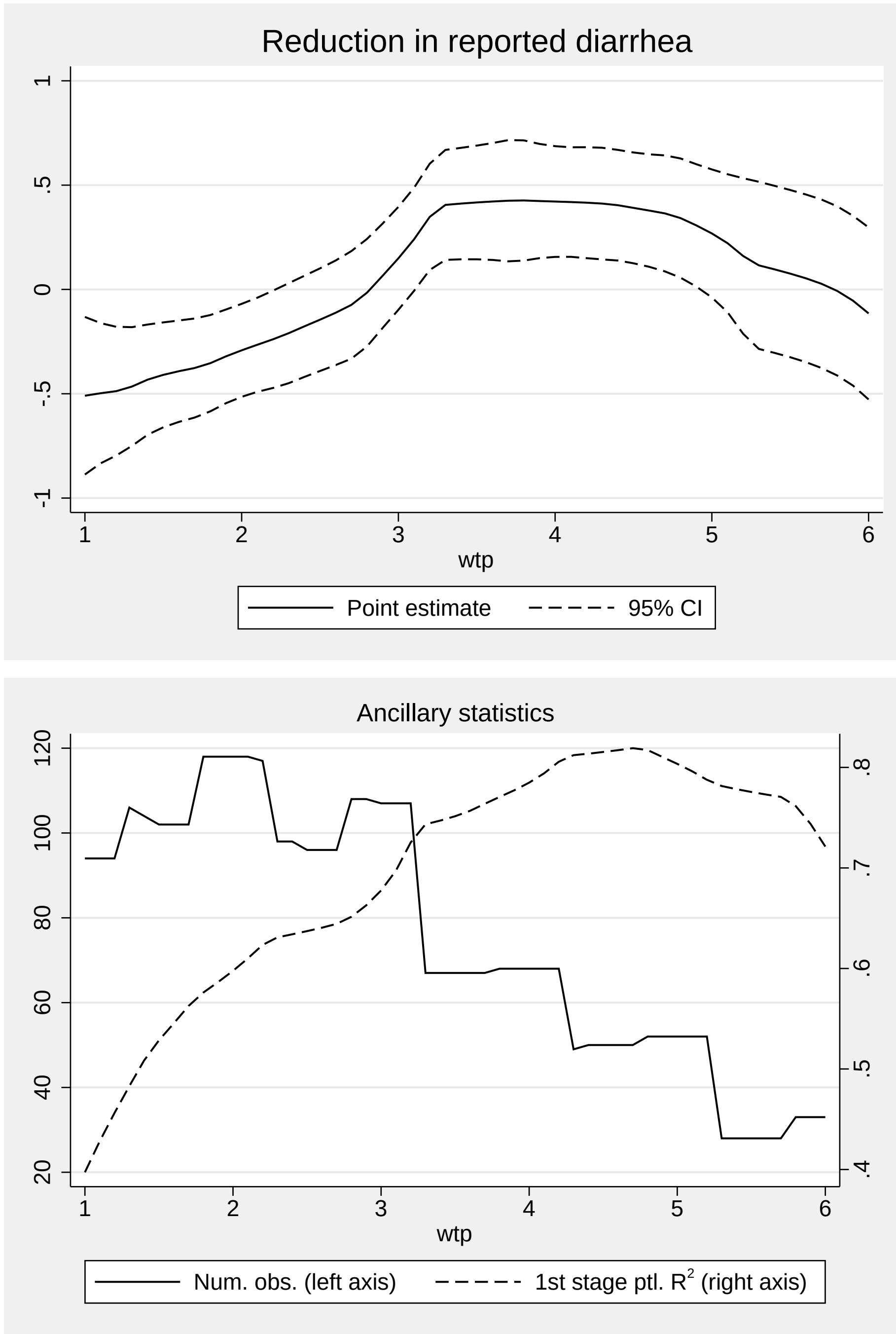
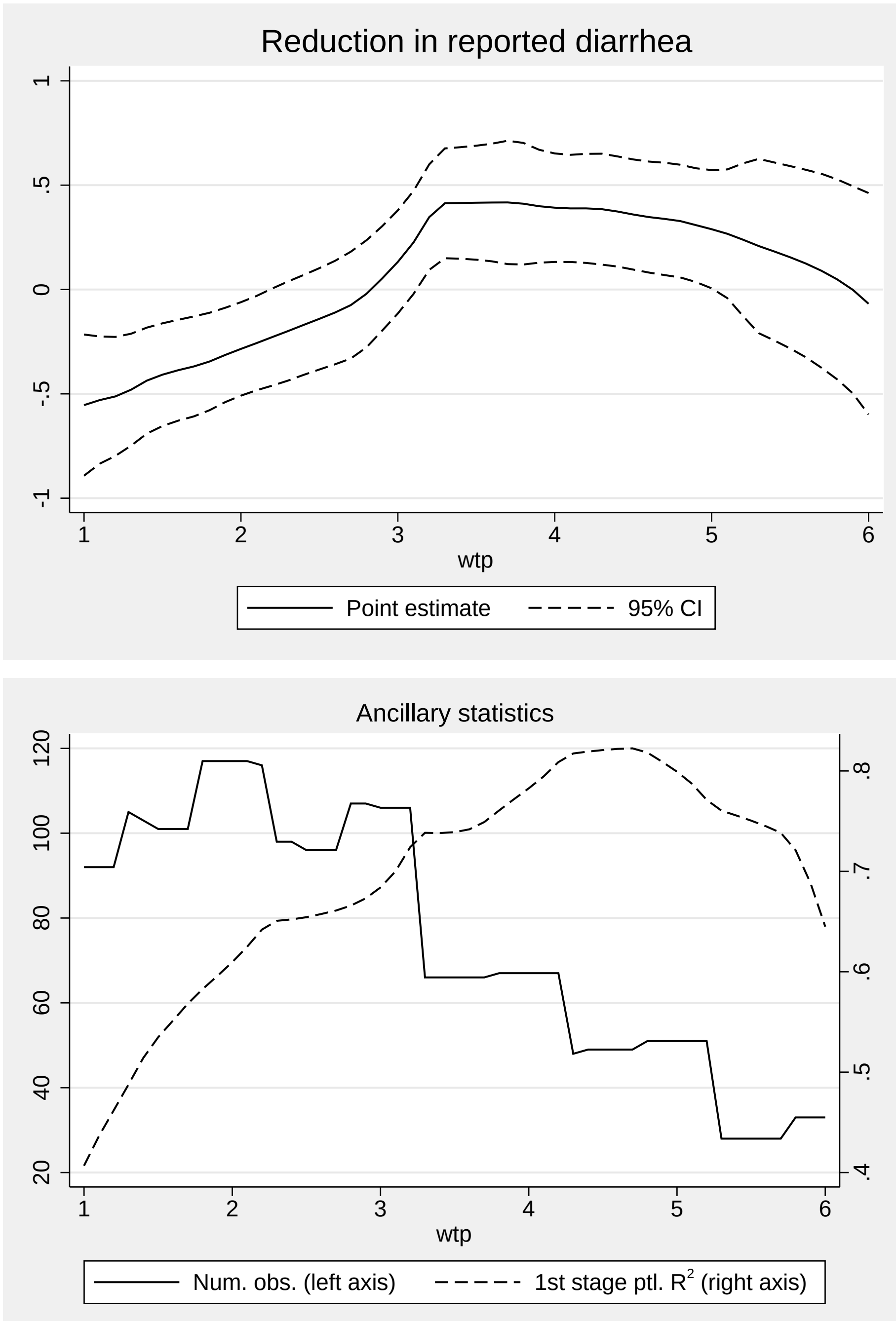


Figure 15
Local IV Estimates of One-Year Treatment Effects – With Controls



Appendix 1: BDM Script

Introduction:

- We would like to sell you a filter but the price is not yet fixed. It will be determined by chance in a game we are about to play.
- You will not have to spend any more for the filter than you really want to.
- You may even be able to buy it for less.

Here is how the promotion works:

- I will ask you to tell me the maximum price (***dan kuli***) you are willing to pay (***ka a ni sagi dali***) for the *Kosim* filter (koterigu di mali lokorigu).
- In this cup, I have many different balls with different numbers on them.
- The numbers represent prices for the filter.
- Then I will ask you to pick a ball from the cup, and we will look at the price together.
- If the number you pick is less than or equal to your bid, you will buy (***ani too dali***) the filter and you will pay the price you pick from the cup.
- If the number you pick is greater than your bid, then you cannot buy the filter.
- You will only have one chance to play for the filter.
- You cannot change your bid after you draw from the cup.
- You must state a price that you are actually able to pay now.
- We will practice in one moment, but for now, do you have any questions?

Practice round:

Before we play for the filter, let's practice the game. We'll play the same game, but instead of playing for the filter, we will play for this bar of soap. ***Show respondent soap.***

1) What is the maximum amount (dan kuli) that you are willing to pay for this soap? **[*Respondent states price X*]**

2) Now, if you pick a number that is less than or equal to X, you will buy the soap at the price you pick. If you pick a number greater than X, you will not be able to purchase the soap, even if you are willing to pay the greater number. You cannot change your bid after you pick a price. Do you understand?

3) Please, tell me - if you pick [X+5 peswas] now, what happens? ***If respondent does not give correct answer, explain the rules again and then ask question again.***

4) And if you pick [X-5 peswas] now, what happens? ***If respondent does not give correct answer, explain the rules again and then ask question again.***

5) If you draw [X+5], will you want to purchase the soap for [X+5]?

IF YES: **→ 6)**

IF NO: **→ 7)**

6) Do you want to change your bid to [X+5]?

IF YES: OK, your new bid is [X+5]. **→ 2) [*use* X+5 *as new* X]**

IF NO: **→ 6)**

7) So, is X truly the most you would want to pay?

***IF YES:* → 7)**

***IF NO:* → 1)**

8) If you pick X, you must be able to pay X. Are you able to pay X now?

IF YES:* → *Record respondent's final bid and proceed to 9

IF NO: What is the maximum price you are willing **and able** to pay now? **→ 2) [*use new X*]**

9) Could you please fetch the amount you have stated you are willing to pay and show it to me?

Wait for respondent to fetch money and check to see she has enough funds for Final Bid.

10) Now you will pick a price from the cup. If you pick X or less, you will buy the soap at the price you pick. If you pick more than X, you will not be able to buy the soap. Are you ready to pick a ball?

Mix balls in cup, hold cup above eye level of respondent and have her pick a ball without looking.

11) Now you can draw a ball from the cup. ***Let respondent draw ball. Together, look at the ball and read the price picked.* [*Drawn price is Y*]**

→ ***Record Drawn Price***

12) Let us look at the ball together.

→ ***Record if Drawn Price is lower/equal to or higher than Final Bid Survey***

a. **[*If Y* <= *X*]:** The price is Y which is [less than/equal to] the amount you said you would be willing and able to pay for this soap. You can now buy the item at this price.

→ ***Exchange payment for soap.***

b. **[*If Y* > *X*]:** The price is Y, which is greater than the amount you said you would be willing to spend. You cannot purchase the soap.

13) Do you have any questions about the game?

Address any questions or concerns respondent has. Make sure she understands rules of game.

Sale:

- Now you will play to buy the filter
- Recall the community meeting on [day of community meeting]
- Have you thought about how much you are willing to pay for the filter?
- Do you have the funds available now?

Let's begin:

1) What is the maximum amount (dan kuli) that you are willing to pay for this filter? **[*Respondent states price X*]**

2) Now, if you pick a number that is less than or equal to X, you will buy the filter at the price you pick. If you pick a number greater than X, you will not be able to purchase the filter, even if you are willing to pay the greater number. You cannot change your bid after you pick a price. Do you understand?

3) Please, tell me - if you pick [X+1 cedis] now, what happens? ***If respondent does not give correct answer, explain the rules again and then ask question again.***

4) And if you pick [X-1 cedis] now, what happens? ***If respondent does not give correct answer, explain the rules again and then ask question again.***

5) If you draw [X+1], will you want to purchase the filter for [X+1]?

IF YES: **→ 5)**

IF NO: **→ 6)**

6) Do you want to change your bid to [X+1]?

IF YES: OK, your new bid is [X+1]. **→ 2) [*use* X+1 *as new* X]**

IF NO: **→ 6)**

7) So, is X truly the most you would want to pay?

IF YES: **→ 7)**

IF NO: **→ 1)**

8) If you pick X, you must be able to pay X. Are you able to pay X now?

IF YES: → Record respondent's final bid and proceed to 9

IF NO: What is the maximum price you are willing **and able** to pay now? **→ 2) [*use new X*]**

9) Could you please fetch the amount you have stated you are willing to pay and show it to me?

Wait for respondent to fetch money and check to see she has enough funds for Final Bid.

10) Now you will pick a price from the cup. If you pick X or less, you will buy the filter at the price you pick. If you pick more than X, you will not be able to buy the filter. Are you ready to pick a ball?

Mix balls in cup, hold cup above eye level of respondent and have her pick a ball without looking.

11) Now you can draw a ball from the cup. ***Let respondent draw ball. Together, look at the ball and read the price picked.*** **[*Drawn price is Y*]**

→ *Record Drawn Price*

12) Let us look at the ball together.

→ *Record if Drawn Price is lower/equal to or higher than Final*

a. **[*If Y <= X*]:** The price is Y which is [less than/equal to] the amount you said you would be willing and able to pay for this filter. You can now buy the filter at this price.

b. **[*If Y > X*]:** The price is Y, which is greater than the amount you said you would be willing to spend. You cannot purchase the filter.

Appendix 2: Take-it-or-leave-it script

- You have been selected by chance (***ti piila gandam gandam***) to receive a special promotion for the Kosim Filter (***koterigu din mali lokorigu***)
- We are offering you the filter at a specific price.
- This is the only time we will offer this filter to you.
- We will not offer you any other price and we cannot bargain.
- If you do accept the special promotion price, you will have to pay now.
- But before we play for the filter we will play the same game for a bar of soap.
- Do you understand?

Practice round:

→ ***Record assigned price for soap***

- Are you willing to purchase the soap at this price?

→ ***Record if Respondent is willing to pay STAND_TIOLI Price***

a. If "Yes": ***Exchange and go to filter sale***
b. If "No": ***Go to filter sale***

Address any questions or concerns respondent has. Make sure she understands rules of game.

Filter sale:

- We will now offer you a chance to buy the filter
- Like I said, we are offering you the filter at a specific price.
- This is the only time we will offer this filter to you.
- We will not offer you any other price and we cannot bargain.
- If you do accept the special promotion price, you will have to pay now.
- Do you understand?

Answer questions and address any concerns respondent may have.

→ ***Check Respondent Assignment Sheet and record assigned filter price***

- Are you willing to purchase the filter at this price?

→ ***Record if Respondent is willing to pay assigned price***

c. If "Yes": →***Receive payment for filter.***
d. If "No": → ***Conclude sale***