Borrowing Constraints and Demand for Remedial Education: Evidence from Tanzania

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Abstract:

We use a cash transfer to relax households' borrowing constraints, then elicit their willingness to pay (WTP) for a remedial education program offering tutoring and life-skills training. Lottery losers were willing to pay 3,300 Tanzanian Shillings for the program, seven percent of per-capita monthly expenditures. For those identified at baseline as able to borrow, WTP increases by three percent upon winning a lottery prize of 3,200 TSh. For those unable to borrow, WTP increases by 27 percent upon winning the lottery. We conclude that borrowing

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constraints limit access to educational programs, and may increase inequality of educational attainment. JEL codes: O15; O16; C93

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1 Introduction

A large body of recent research and policy debates have highlighted low levels of social mobility around the world (see e.g. Chetty et al., 2014). Researchers and policy makers alike suspect a lack of equal access to education as a potentially important source of low social mobility. Currently, educational attainment strongly depends on the socio-economic status of the parents in many countries (Filmer and Pritchett, 1999) including many African countries (Azomahou and Yitbarek, 2016; Alesina et al., 2021). This holds also true for the setting of this study: Tanzania. The attendance rate in primary education is 76%, and falls to only 23% in secondary education (TDHS, 5 16). But the secondary school attendance rate is 41% among children from the top wealth quintile while it is only 6% among those in the lowest quintile. Given empirical estimates of returns to secondary education in Tanzania of around 15% per year (Montenegro and Patrinos, 2014) these figures suggest immense lost potential.

What keeps children from poorer backgrounds from achieving higher levels of education? One leading hypothesis is that poor households are borrowing constrained: despite high returns to education, they cannot raise the funds needed to pay for school or program fees, or other complementary inputs. A competing hypothesis is that other correlates of

poverty, such as the early-childhood parental environment, affect children's cognitive and non-cognitive abilities, and eventual schooling outcomes (Heckman and Carneiro, 2002). Evidence from developed countries suggests borrowing constraints may be of second-order importance for progression to higher education and better labor market outcomes (Heckman and Mosso, 2014). But they may be of first-order importance in developing countries, where borrowing constraints are thought to limit take-up of productive investment opportunities (e.g. Banerjee and Newman, 1993; de Mel et al., 2008; Beaman et al., 2023).

We report results from a randomised control trial designed to study how borrowing constraints affect families' investment in a remedial education program in Tanzania.[1] To that end we collaborated with an NGO that runs free study clubs for girls aged 12-14, corresponding to cohorts who should be attending the final two years of primary education, as part of its efforts to improve girls' education outcomes. The NGO was interested in implementing a participation fee to support long-run program sustainability, but was concerned about how this might affect access. We worked with them to elicit households' Willingness to Pay (WTP) for the program, a measure of each household's demand for education. The experiment and surveys were designed to uncover how borrowing constraints depress demand for education.

The experiment took place in 69 villages that did not previously have a study club from our partner NGO. We conducted a baseline survey of a representative sample of eligible girls and their household head, which included two measures of borrowing constraints: a

[1]Remedial education programs targeting vulnerable children have emerged as a possible way to ameliorate inequalities in educational attainment; and are being implemented by many governments and NGOs around the world (e.g. Banerjee et al., 2007, 2016; Muralidharan et al., 2019). However, they need to be funded, and attempts to charge fees might jeopardise this objective in settings where ability or willingness to pay is low. Thus, even effective interventions may be discontinued in the absence of continued donor funding. For example, the highly effective MindSpark centres in India were forced to close down as insufficiently many families would or could pay the subsidised price (Muralidharan et al., 2019).

household-level dummy for "inability to borrow for an important expenditure," and an index of four binary borrowing constraint measures. We then invited the girl and a responsible adult to a village meeting about girls' education. We gave them a lottery ticket for a prize of 3,200 TSh (PPP USD 4.33), described as a "thank you" for taking the survey, to be drawn during the meeting. This lottery is our experimental treatment of interest, the lottery prize acting as an unconditional cash transfer.

The village meetings started with the draw of the lottery to award prizes to 50% of eligible attendees. Subsequently the program officers explained the study clubs in detail. Last, we elicited WTP to join the club through a "multiple price list" variant of the Becker-DeGroot-Marschak mechanism (Becker et al., 1964). Girls filled out the instrument along with the adult who had joined them, usually a household head, so we interpret the response as the household's WTP.

The experiment and data allow us to (a) measure the household's WTP for their daughter to participate in the study club, (b) study the correlation between WTP and borrowing constraints, (c) identify the causal effect of the cash transfer on WTP, and (d) examine heterogeneity of the effect of the cash transfer with respect to borrowing constraints. We additionally examine long-run effects on program participation.

What should we expect to see if borrowing constraints are depressing demand? Suppose household i values the program at V_i units of consumption. After accounting for pressing spending needs their investable funds are $A_i(B_i)$, where B_i is credit available to them. It is natural to assume that $A' \geq 0$: the more easily they can borrow, the less pressing will be their unmet spending needs and the more they can afford to invest in a new opportunity.

As a result, the maximum amount they are willing to pay for the program is $WTP = \min\{A_i(B_i), V_i\}$.

Our cash transfer treatment can be thought of as increasing investable funds to $A_i(B_i) + T$, and thus WTP to $\min\{A_i(B_i) + T, V_i\}$. So the treatment should have no effect on unconstrained households' (with $A_i \geq V_i$) WTP, while those with $A_i < V_i$ will increase their WTP (the effect is intermediate for those lying close to the constraint). Thus, we expect the cash transfer will increase WTP for the program, and this effect will be concentrated among the borrowing constrained.

We find five main results. First, on average households have substantial WTP for the remedial education program. Non lottery-winning households are willing to pay around 3,300 TSh (PPP USD 4.47) on average, corresponding to 1.4% of total monthly household expenditure, or 7% of monthly per capita expenditure, and is approximately equal to the program fee (3,000 TSh). There is notable heterogeneity: 9% of households were willing to pay 10,000 TSh, while 16% were not willing to pay anything.

Second, there is a negative association between WTP and two proxies for borrowing constraints: among lottery losers, those unable to borrow have approximately 500 TSh lower WTP compared to those who are able to borrow.

Third, winning the 3,200 TSh lottery prize increases WTP by around 300 TSh, or 9%, on average.

Fourth, the effect of the lottery treatment interacts strongly with borrowing constraints measured at baseline. The average effect of the lottery is almost entirely driven by borrowing constrained households. Those who report being able to borrow increase WTP by only 3% (roughly 120 TSh) when they win, while those who cannot borrow increase WTP by 27%

(roughly 850 TSh). Put differently, while WTP is substantially lower for those unable to borrow amongst lottery *losers*, this association disappears for *winners*. The latter finding suggests that the lottery was effective at relaxing borrowing constraints.

This interaction effect is robust to controlling for interactions with a host of observable characteristics that might be correlated with borrowing constraints and WTP, such as distance to school, girl's cognitive skills, preferences, and household expenditures or poverty. This suggests that the heterogeneity we find is not proxying other girl- or household-level characteristics. We also discuss alternative explanations – including income, experimenter demand, "house money", or anchoring effects – and argue that they are qualitatively and quantitatively implausible.

Fifth, due to implementation difficulties, the program was launched with a delay and some changes, including elimination of fees (see Section 2.4). However, borrowing constraints remain strongly predictive of long-run program take-up.

The collection of evidence suggests that borrowing constraints are an important impediment to demand for educational investments.

The paper is related to the literature on the role of borrowing constraints in suppressing profitable investments in general and education investments in particular. While the association between family income and schooling outcomes has been documented in a variety of contexts (e.g. Das et al., 2022), evidence on the role of borrowing constraints is mixed and is largely from developed countries (Heckman and Carneiro, 2002; Cameron and Taber, 2004; Dahl and Lochner, 2012; Caucutt and Lochner, 2020). In developing countries, a number of studies highlight the importance of prices and borrowing constraints for the take-up of health products (Kremer and Miguel, 2007; Hoffmann, 2009; Hoffmann et al., 2009; Ashraf et al.,

2010; Cohen and Dupas, 2010; Dupas, 2014; Tarozzi et al., 2014; Fischer et al., 2019; Berry et al., 2020), insurance (Casaburi and Willis, 2018), and fuel-efficient stoves (Berkouwer and Dean, 2022). In the context of education there are a number of studies quantifying demand for education exploiting (downward) changes in school fees through vouchers (e.g. Angrist et al., 2006), scholarships (e.g. Kremer et al., 2009; Duflo et al., 2021) or fee abolition (e.g. Deininger, 2003; Riphahn, 2012; Bold et al., 2014).

Most notable amongst those studies is the work of Berry and Mukherjee (2019) as they also study out-of-school tutoring centres. They implement a two-part pricing design to induce random variation in the offer and final price. They find that higher offer prices select participants with higher attendance and higher final prices induce participants to drop out. But they do not find evidence that study centres increased test scores amongst the group of participants induced to sign up by a lower price. Their work takes the demand for educational investments as given. Our work complements their work in studying a potentially important source of low demand for education investments: borrowing constraints. In this sense, another closely related study is Dillon (2020), showing that a change in the timing of school fees forced farmers to sell crops early, forgoing profitable opportunities to store them. In a world without borrowing constraints, the timing of education expenditures should be irrelevant.

We also relate to the literature on cash transfers for education (see Baird et al. (2014) and Bastagli et al. (2016) for recent reviews). This literature mostly focuses on conditional cash transfers, which are linked to education take-up (e.g. Evans et al. (2023)). Studies tend to focus on enrolment and participation in education, and typically find positive effects. Benhassine et al. (2015) study a cash transfer that is "labeled" as being for education,

and find it has similar effects on school participation to conditional transfers. In contrast, the lottery in our experiment is deliberately *not* labeled as being for education, but as compensation for participation in the survey, so is more closely related to unconditional cash transfer programs. As an example, Haushofer and Shapiro (2016) find in Kenya that large cash transfers increased monthly educational spending, but the effect is small relative to the size of the transfer ($1 PPP versus transfers of $404 or $1,525 PPP). We find larger impacts relative to the size of the transfer. This may be because our experiment provided liquidity shortly before the opportunity to make an educational investment. This is by design, to mimic the provision of liquidity through credit, and distinguishes our treatment from a typical unconditional cash transfer. Most importantly, our findings demonstrate strong heterogeneity: the lottery payment increases educational investments almost exclusively for households that our baseline survey identifies as unable to borrow, and for them it increases educational investments strongly.

2 Background and Methodology

2.1 The Program

We study demand for, and take-up of, a remedial education program implemented by an NGO in Tanzania.[2] The central aim of the program is to improve learning outcomes of girls at risk of dropping out of school, or who have recently dropped out, and to increase enrolment rates. As part of this effort, the NGO established study clubs, designed to provide subject-based tutoring in Mathematics and English. According to the program design, tuition for in-school

[2]The NGO we collaborated with is BRAC Tanzania.

girls who are in their final two years of primary education was scheduled to take place in the afternoon hours, three times a week, for three hours. The tutoring for out-of-school girls was to take place in the mornings, five days a week, for three hours. In addition, the NGO would register the out-of-school girls under the Institute of Adult Education, enabling them to complete their Form 1 and 2 courses. The tutoring was facilitated by trained teachers who were paid an honorarium for their work. The tutoring followed the primary education curriculum and is intended to prepare pupils for the Tanzanian secondary school entrance exams.[3]

The clubs were established inside villages to make them easily accessible. In the afternoon hours, the clubs were then used as safe spaces for both in-school and out-of-school girls to come together, interact, forge bonds and support each other in their studies. In addition to subject-based tutoring, the clubs provided life skills training through peer mentoring.

2.2 Sample Selection

Here we summarise how we arrive at our analysis sample, see Appendix C.1 for details.

The NGO implemented the remedial education program at 20 branches in the regions of Dar es Salaam, Mwanza, Shinyanga, Tabora and Singida. In September 2013 we randomly selected eight study branches. Within those branches the NGO's field staff identified 105 villages close to potential treatment schools.[4]

[3]The program is similar to remedial education programs aiming to provide a pathway for out-of-school youth to return to formal education. In the East African context, these programs are often provided by NGOs, labeled as "second-chance," "bridging" or "re-entry" programs (Ngware et al., 2018). As an alternative, wealthier families can hire private tutors to assist youth prepare for the secondary school entrance exams.
[4]For simplicity, we always refer to the communities in which clubs are situated as "villages", though in peri-urban areas a better descriptor would be "neighbourhood". Village boundaries were defined by NGO staff.

Villages were grouped in 42 "clusters," with villages close to the same school assigned to the same cluster. Of those, we randomly selected 27 clusters containing 69 villages as study locations. The remaining villages were to be control villages for the purpose of program evaluation, and not relevant to this paper.

We randomised study villages, stratified by branch, to receive a free club (36 villages) or a club with a one-time joining fee of 3,000 TSh (33 villages).[5]

We conducted a short census of girls aged 11 to 18 in the villages in November 2013.

The census served a sampling frame for the baseline survey. Girls in the census were screened for program eligibility.[6] The census sample consisted of 5,968 girls, of whom 5,048 were eligible for program participation.

The baseline survey was conducted in December 2013. The main respondent was the selected girl, followed by a short module addressed to the household head. We aimed to sample only one girl per household, except where the number of available girls was small. In addition, due to challenges finding participants we allowed for some convenience sampling (53 girls). Our full baseline sample contains 1,717 girls.

At the end of the baseline survey, girls received a lottery ticket for a prize of 3,200 TSh if they came to an information meeting about the new education program, and they were told that half of eligible (i.e., baseline-surveyed) attendees would win. This lottery was framed as a thank you for taking the survey. The lottery is the treatment of interest in this paper.

[5]Note that this one-time fee is similar to an annual membership fee, since a given cohort of girls are expected to participate for one year and then take the secondary school entrance exams (i.e. "graduate" from the club). This type of fee structure is common in clubs in our context. For example, BRAC's ELA program evaluated by Bandiera et al. (2020) in Uganda also has a similar fee, although in that case the fee is voluntary and often waived. Non-formal education programs also often charge such one-off fees as a "community contribution".
[6] Eligibility required the girl either (i) had dropped out of school within the last two years [satisfied by 15% of girls], or (ii) was at risk of dropping out (a grade of less than 50% in Mathematics, Science, or English in the last exam) [33%], or (iii) belongs to a poor household, based on a poverty scorecard for Tanzania, developed by Grameen foundation [61%], or (iv) has lost one or both parents [29%], or (v) displays signs of physical or mental disability [3%].

2.3 WTP Elicitation

The information meetings were organised in June 2014. Appendix C.4 contains the meeting script. All baseline girls were invited to attend, as well as any other girls living in the village. They were to be accompanied by a household member, ideally the household head. The meeting was described as an information session about the new education program. Of the 1,717 girls in the baseline, 880 attended a meeting, plus 252 non-baseline girls (who are not included in the analysis).

First, we conducted the lottery for baseline participants (whether or not they remembered their ticket). Prizes were to be awarded through a public draw at which 50% (rounding up) of tickets would win. Winners were told that they were free to do whatever they wanted with the money.

Afterwards the program officers described the study clubs in detail. Information about the program was provided in exactly the same way to all participants. It was emphasised that to join the club, girls needed to sign up on the day of the meeting, and that any fee charged for participation was due at the first club meeting, scheduled to take place around 1 week later.

Last, we elicited WTP to join the club. Participants were told that joining the club might be free or might require a one-time fee. The price had already been decided and it was written inside an envelope that was shown to the audience, but they were not told about the price distribution. Before the envelope was opened, participating girls along with the accompanying household head needed to declare their maximum WTP. They were provided with a sheet of paper listing 11 prices uniformly spaced from 0 to 10,000 TSh.[7] They were

[7] See Appendix Figure C2 for the answer sheet, and C3 for its English translation.

told to tick "Yes" next to each price they would be willing and able to pay to participate in the club, and to tick "No" for prices that they were not willing/able to pay (this could include that they would join the club only if it was free). If the price in the envelope was equal to or below their WTP, they would be required to join the club and pay the fee at the first meeting of the study club. Those whose WTP was below the price would pay nothing and receive nothing. For expected utility maximisers, bidding up to one's true maximum WTP is a weakly dominant strategy.

Our elicitation mechanism is a "multiple price list" variant of the Becker-DeGroot-Marschak mechanism (Becker et al., 1964; Andersen et al., 2006). This implementation helps the participants by breaking down the mechanism into simple take-it-or-leave-it questions. They were reminded that they could not influence the price, and our procedure—where the price was already determined but not revealed—made this very clear.[8]

We began with a few comprehension questions, then a practice exercise, selling bars of soap using the multiple price list procedure. If their WTP was higher than the price in the envelope (400 TSh in all villages), they were required to buy the soap.[9] Participants were instructed not to decline one price, then accept a higher one, on the WTP sheets (i.e., "multiple switching").

After the soap exercise, we elicited WTP to join the study club. After everyone reported their WTP, the answer sheets were collected and the price inside the envelope was revealed (either 0 or 3,000 TSh). Everyone willing to pay at least as much as the price in the envelope

[8] Burchardi et al. (2021) test for optimal bidding under four WTP elicitation mechanisms, including one very similar to ours, with rural participants in Uganda. They find high rates of optimal bidding, averaging 86%, in all four.

[9] We did not collect soap WTP data so cannot examine behaviour in this practice round. Lottery winners may have put some of their winnings into the soap purchase. If so, this would tend to attenuate treatment effects on WTP for the program.

was asked to sign a "contract" promising to join the club and to pay the price at the first club meeting in around 1 week's time.

We have WTP data for 825 of the 880 baseline girls that attended. We infer that the 55 for whom we do not have data chose not to participate in the elicitation. This could be because they were unwilling to participate even at zero price. Our results are robust to assigning zero WTP to these girls.

2.4 Implementation Challenges

In 4 villages lottery winners were not recorded by the enumerators. This leaves us with 65 villages and 805 girls for whom we have WTP and lottery data. The lottery was not perfectly implemented in every village, but we find no evidence that this contaminated the randomisation, see Appendix C.2 for details.

After completion of the WTP meetings, the NGO experienced unanticipated difficulties with the program launch, which was delayed by several months. When it was launched, an altered version of the program was rolled out, with tutoring delivered in schools rather than in clubs (which instead focused on life skills, providing a social space, etc). Presumably due to the delay, there were significant difficulties in collecting fees, so *de facto* the program became free. This is not an issue for our WTP analysis as the WTP elicitation was incentive compatible as the delays were unanticipated.

A followup survey was conducted two years after the baseline (18 months after WTP elicitation). We use two variables from this survey to capture program participation: (1) a binary measure of whether the girl ever attended the club, and (2) her frequency of attendance (days per week).

2.5 Borrowing constraints survey measures

Our main treatment variable of interest is the lottery treatment, which we interpret as alleviating the effects of borrowing constraints among treated households. To explore how this treatment interacts with borrowing constraints, we construct survey-based measures of borrowing constraints.

In the survey, both girls and household heads were asked separately: "If you needed to borrow money for an important expenditure (e.g. health or school related expenditure), how easy would it be for you to borrow the money?" Options were "easy," "not easy, but possible," and "not possible." If the respondent said "don't know" we code them as missing. This gives us two dummy variables for the girl and two for the household, defined as not possible, and not possible OR not easy. Our first survey measure of borrowing constraints is the dummy for borrowing "not possible" according to the household head. Our second measure is a standardised (to mean zero, standard deviation one) index of the four dummy variables, which we refer to as the borrowing constraints index.[10] In the terminology in the introduction, a higher constraints measure is interpreted as a lower value of investable funds, A_i.

2.6 Balance Checks

In the Appendix, we perform a sequence of balance checks capturing each stage of the selection process outlined above. Importantly, girls who attended the WTP meeting were remarkably similar to the general population of baseline girls on a wide range of covariates.

[10] If some of the index components are missing we impute them with sample means, if all are missing we code the index as missing.

Standardised differences in covariates between winners and losers are small and quantitatively unlikely to drive any of our main results.

3 Results

3.1 Estimation

To identify the effects of winning the lottery on the demand for the program, we estimate an OLS regression of the form:

$$\mathrm{Y}_{ihv} = \beta \cdot \mathrm{Lottery}_i + \sum_{j=1}^{65} \gamma_j \mathbb{1}(v = j) + \varepsilon_{ihv} \tag{1}$$

Y_{ihv} is an outcome for girl i from household h in village v. $\mathrm{Lottery}_i$ is a dummy variable equal to 1 if the girl won the lottery, and γ_j are village fixed effects for the 65 villages in which we have lottery data. The parameter of interest is β, the average effect of winning the lottery.

To examine how the lottery treatment interacts with borrowing constraints, we estimate:

$$\mathrm{Y}_{ihv} = \beta \cdot \mathrm{Lottery}_i + \lambda \cdot \mathrm{Constraint}_i + \delta \cdot \mathrm{Lottery}_i \cdot \mathrm{Constraint}_i + \sum_{j=1}^{65} \gamma_j \mathbb{1}(v = j) + \varepsilon_{ihv} \tag{2}$$

where $\mathrm{Constraint}_i$ is a measure of borrowing constraints. In this specification, β identifies the treatment effect when $\mathrm{Constraint}_i$ is zero. For our binary measure these are households who can borrow (either easily or with some difficulty). In our case these are households at the index mean. λ identifies the relationship between borrowing constraints and Y, for those who lost the lottery. δ identifies the interaction effect between constraints and lottery win.

Although the lottery treatment was assigned by randomisation at the girl level, we have some households with multiple participating girls.[11] Intra-household decisions about different girls may be interrelated, and borrowing constraints are partially defined at the household level.[12] In the spirit of clustering at the level of assignment (Abadie et al., 2022), we cluster standard errors at the household level. We report estimates clustered at the village level in Appendix Table B6. We also report randomisation inference p-values for the randomised treatment effect and its interaction with borrowing constraints (Imbens and Rubin, 2015; Young, 2019).

3.2 Demand for Education and Borrowing Constraints

In our full sample, all households would sign up if the program were offered for free. But fees significantly affect demand. 16% of households were not willing to pay more than zero. Roughly 50% were willing to pay the true (not-yet-announced) program fee of 3,000 TSh. Less than 20% were willing to pay more than 5,000 TSh.

Figure 1 displays the demand curve for the program splitting the sample according to whether the household head reported at baseline that they cannot "borrow money for an important expenditure." In both subsamples lottery winners have higher WTP, indicated by a first-order shift to the right of the demand curve. While both subsamples show some response to the lottery, the shift is more pronounced for the borrowing constrained subsample.

[11] Our analysis sample of 805 girls contains 779 households: 755 with one girl, 22 with two, and two with three.
[12] One measure, the household "cannot borrow" dummy is fixed within household, while the index which depends also on the girl module can in principle vary within household.

In particular this group shows a large increase in the share of households willing to pay high prices (5,000 TSh or more).[13]

Table 1 presents the regression equivalents. Panel A, Column 1 displays estimates of specification (1) in the full sample for which we have WTP and lottery data. Lottery losers were willing to pay 3,335 TSh on average, around 7% of monthly per capita expenditures. Winning the lottery increases WTP by 311 TSh, or 9.3% (p-value=0.038) on average.

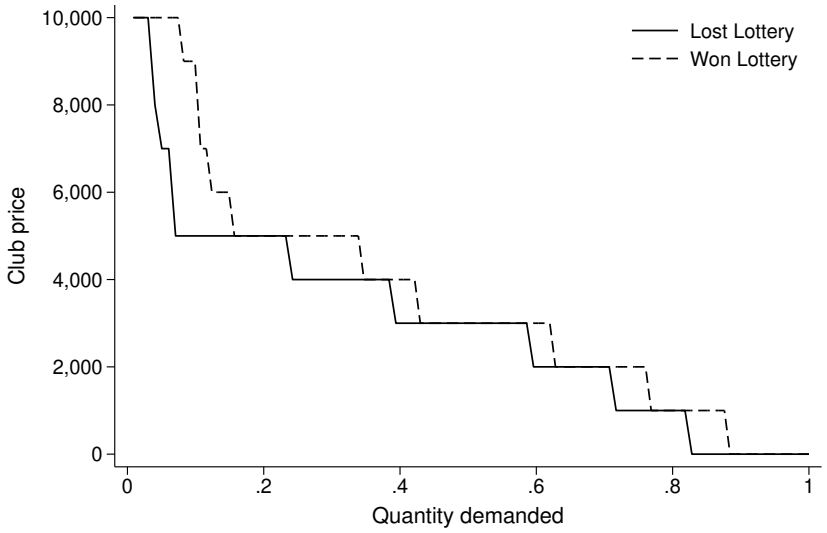
Column 2 reports similar estimates for the subsample for whom we observe the dummy indicating the household cannot borrow (missing if they responded "don't know").

Column 3 estimates heterogenous effects of the lottery (specification (2)). For households who can borrow, average WTP among lottery losers is 3,633 TSh, increasing by only 119 TSh or 3% when they win the lottery. Households who cannot borrow have initial WTP of $3,633 - 522 = 3,111$ TSh, and are substantially more responsive to the lottery, increasing WTP by $119 + 734 = 853$ TSh, or 27%, when they win. Thus the difference in WTP between constrained and unconstrained households is smaller among winners than losers.

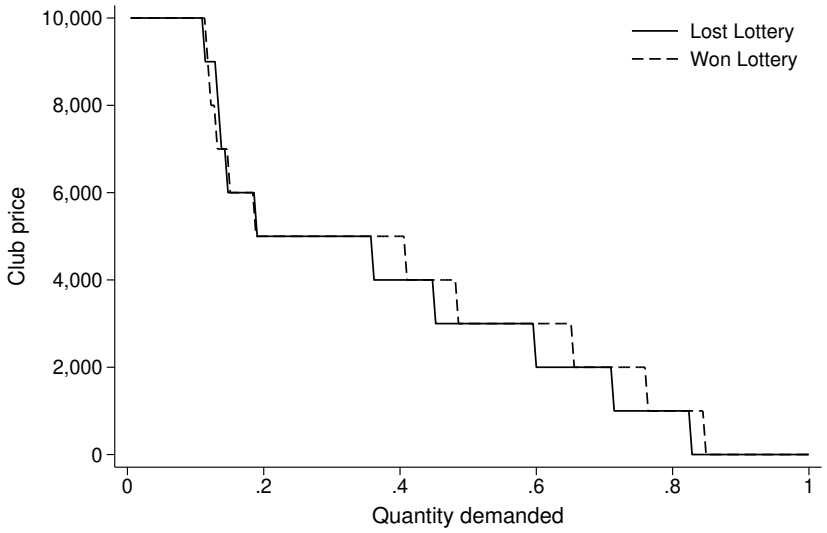
Panel B uses the index measure of borrowing constraints, and finds very similar results to Panel A.[14] A one standard deviation increase in borrowing constraints is associated with 387 TSh lower WTP among lottery losers, while the effect of winning the lottery increases by 432 TSh. The fact that the two coefficients approximately cancel one another implies once again that WTP of lottery winners is relatively insensitive to whether they are borrowing constrained.

[13] The simple theory presented in the introduction predicts that WTP can increase by at most the size of the cash transfer, T. The large shift in the demand curve at high values of WTP in Figure 1a indicates that for some people WTP may have increased by more than 3,200 TSh. This could reflect sampling variation, or a nonlinear response not captured by our model.

[14] Because the index is standardised, the coefficient on lottery win has a different interpretation in Panels A and B. In Panel A it is the effect for participants who "can borrow" while in Panel B it is for those at the mean of the index.



(a) Cannot Borrow



(b) Can Borrow

Figure 1: Demand Curves and Borrowing Constraints

Notes: These figures present demand curves for the remedial education program, separately for four subgroups: in Figure 1a we present results for the subsample of households whose household head responded that they would not be able to "borrow money for an important expenditure", in Figure 1b we present results for the subsample of households whose household head stated that this would be possible; in both graphs we present results separately for households who won and those who did not win the lottery. The full sample corresponds to the sample used in columns 2 and 3 of Table 1, Panel A.

Columns 4 and 5 of both panels report effects on long run club participation and attendance, respectively.[15] Notably, borrowing constraints are negatively associated with both, implying that just as borrowing constraints make club fees difficult to finance, they presumably make costly investments in remedial education (in terms of time and other resources) more difficult in general. 26% of lottery losers who "can borrow" attended the program, falling by 16 percentage points for those who cannot.[16] Directionally, the coefficients on our lottery treatment and its interaction with borrowing constraints are almost always the same as in the main analysis (positive main effects in all specifications and positive interactions in three out of four). These coefficients are relatively small and never statistically significant. That is not surprising since the lottery treatment was designed and only expected to act as a short-run relaxation of borrowing constraints.

3.3 Robustness

We think there are five possible alternative interpretations of our results. First, our borrowing constraints measures might capture factors other than borrowing constraints. Second, our treatment effect might be an income effect rather than a constraint relaxation. Third, we address possible behavioural confounds. Fourth, we discuss the impact of the information session on the estimated effects. And fifth, we discuss the role of households anticipating the option to default on the contract.

[15] The sample size is smaller than in the other columns, which we attribute to program and survey delays, and difficulty matching respondents across surveys. Reassuringly, Appendix Table B9 shows that our main results replicate when we restrict to the sample with followup data.

[16] The overall drop in demand relative to when we elicited WTP is most likely due to changes in life circumstances, and lower WTP due to delays and changes to the program. For sake of illustration, if we assumed everyone experienced a drop in WTP of around 5,000Tsh we would predict take-up very similar to what we observe, see Appendix Figure A1.

Table 1: Demand for Education and Borrowing Constraints

	WTP (TSh)			Participation (0/1)	Attendance (per week)
	(1)	(2)	(3)	(4)	(5)
Panel A: Borrowing constraints dummy					
Lottery Win	311 (152) [0.042]	362 (175) [0.043]	119 (210) [0.605]	0.008 (0.065) [0.953]	0.085 (0.207) [0.698]
Cannot Borrow			-522 (282) [0.065]	-0.160 (0.065) [0.015]	-0.282 (0.265) [0.289]
Cannot Borrow × Lottery Win			734 (379) [0.047]	0.026 (0.103) [0.669]	0.014 (0.376) [0.924]
Village FE	Yes	Yes	Yes	Yes	Yes
Mean Outcome (C)	3335	3414	3414	0.183	0.425
Observations	805	642	642	269	272
R^2	0.474	0.493	0.497	0.429	0.316
Panel B: Borrowing constraints index					
Lottery Win	311 (152) [0.042]	297 (160) [0.067]	312 (159) [0.051]	0.016 (0.041) [0.695]	0.050 (0.125) [0.729]
Borrowing Const. Index			-387 (126) [0.002]	-0.047 (0.035) [0.174]	-0.237 (0.128) [0.065]
Borrowing Const. Index × Lottery Win			432 (168) [0.009]	-0.010 (0.045) [0.721]	0.034 (0.146) [0.886]
Village FE	Yes	Yes	Yes	Yes	Yes
Mean Outcome (C)	3335	3378	3378	0.177	0.422
Observations	805	736	736	316	319
R^2	0.474	0.482	0.490	0.360	0.274

Notes: The table reports ordinary least squares estimates based on equations (1) and (2). The dependent variable in columns (1)–(3) is the household's WTP (in TSh) for the remedial education program, in column (4) it is a dummy for whether the girl ever attended the program's club, and in column (5) the number of times per week she visited. Column (2) shows results from the specification of column (1), but in the sample of column (3). *Lottery Win* indicates whether the individual has been randomly assigned to receive a lottery payout. *Cannot Borrow* is a dummy variable indicating if the household head reported that it would not be possible for them to borrow money for an important expenditure. *Borrowing Constraints Index* is an index combining 4 dummy variables indicating if the respondents (girl or the household head) states that it would not be possible *or* it would be anything but easy to borrow money for an important expenditure. We calculate the index by first normalising each indicator by subtracting the sample mean and dividing by its standard deviation; then taking the average of the four normalised indicators, and normalising again. If only some of these dummies are available we impute the missing ones at the sample mean. All regressions include village fixed effects. Standard errors are clustered at the household level and given in parentheses. In square brackets p-values of the null hypothesis of no effect are provided. For the main effect of *Lottery Win* and interactions with *Lottery Win* these are calculated as randomisation inference p-values, for all other coefficients they are calculated analytically based on the reported clustered standard errors. The randomisation p-values are the percentile of the coefficient estimated under the true assignment in the distribution of coefficients estimated under 10000 alternative assignments. Mean WTP among all lottery losers, and the number of observations, are reported at the bottom of the table. Mean WTP among lottery losers who "can borrow" is 3,633 TSh. Mean Participation among lottery losers who "can borrow" is 26.2%.

Our borrowing constraints measures may be capturing some other underlying differences in girls' or households' characteristics. To address this concern, we assess robustness of our estimates to controls. In particular, we estimate specification (2), controlling for baseline covariates and their interaction with Lottery_i. We include a wide range of covariates capturing education access and attainment (access to tutoring, cognitive skills, distance to school, perceived returns to secondary education), gender attitudes that might affect girls' schooling, preferences (risk aversion and patience), health, household structure. A particular concern is that our constraint measures might simply reflect poverty, so we include measures of per capita expenditures and poverty.

Table 2 presents the results based on the index measure of borrowing constraints (Appendix Table B7 uses the binary measure). Each row reports results of a separate regression, one for each baseline covariate.

The coefficient estimates for β, λ and δ are highly robust in magnitude and precision. Moreover, the additional covariates and interactions mostly have small, nonsignificant coefficients. We conclude that our results are unlikely to be driven by omitted variable bias.[17]

Additionally, Appendix Table B6 shows that the results are robust to alternative fixed effects (branch, enumerator) and clustering (at the village level).

Second, an income effect interpretation of our findings says that winning the lottery increased household wealth, that education is a normal good, and so WTP for the program increased accordingly. In the terminology from the introduction, V_i might be increasing in A_i. We do not have a wealth-preserving borrowing treatment to fully rule this out, but we

[17] We also probe the robustness of our results to the collection of channels tested in Table 2 by (i) including the first principal component of those variables and its interaction with the lottery variable and (ii) implementing the partialling-out LASSO estimator of Chernozhukov et al. (2018). The results are presented in Appendix Table B8. Both exercises leave our main effects essentially unchanged, qualitatively and quantitatively.

Table 2: Demand for Education and Borrowing Constraints Index: Robustness to Controls

Variable	Lottery	Constraint	Lottery × Constraint	Covariate	Lottery × Covariate	N
Tutoring	297 (161) [0.076]	-399 (126) [0.002]	421 (168) [0.011]	-68 (127) [0.592]	42 (167) [0.876]	726
Cognitive Skills	316 (159) [0.048]	-389 (125) [0.002]	428 (168) [0.010]	125 (110) [0.254]	-164 (162) [0.343]	736
Distance to School	312 (159) [0.050]	-387 (126) [0.002]	435 (169) [0.008]	63 (96) [0.514]	-8 (156) [0.814]	736
Returns Second. E.	310 (174) [0.067]	-445 (128) [0.001]	492 (177) [0.005]	281 (129) [0.030]	-350 (175) [0.040]	664
Gender Attitude	284 (162) [0.083]	-380 (127) [0.003]	447 (169) [0.005]	73 (114) [0.518]	80 (161) [0.479]	716
Risk Aversion	307 (165) [0.070]	-383 (130) [0.003]	463 (172) [0.009]	-205 (132) [0.122]	235 (168) [0.104]	703
Patience	281 (171) [0.101]	-386 (133) [0.004]	409 (181) [0.020]	162 (135) [0.231]	-164 (174) [0.506]	684
Illness	310 (162) [0.062]	-384 (128) [0.003]	417 (171) [0.012]	31 (126) [0.807]	72 (163) [0.507]	727
HH kids (no)	316 (162) [0.052]	-399 (126) [0.002]	455 (168) [0.007]	-109 (127) [0.390]	80 (164) [0.645]	726
HH kids (f share)	388 (167) [0.023]	-418 (129) [0.001]	512 (172) [0.004]	68 (125) [0.588]	-34 (168) [0.841]	689
Per Capita Expenditure (TSh)	352 (175) [0.043]	-412 (133) [0.002]	439 (178) [0.013]	-192 (175) [0.275]	277 (196) [0.302]	665
Poverty (<2 USD/day)	352 (175) [0.044]	-418 (135) [0.002]	442 (178) [0.013]	106 (135) [0.435]	-114 (176) [0.541]	665

Notes: The table reports ordinary least squares estimates based on specification (2). *Lottery* indicates whether the individual has been randomly assigned to receive a lottery payout. *Constraint* is an index increasing in borrowing constraints (see footnote for Table 1 for further details). *Tutoring* is a dummy variable indicating if the girl attended any tutoring or study group during the past year. *Cognitive skills* is a normalised index combining the girl's score in a Math exam (EGMA), a reading exam (EGRA) and a Raven's test. *Distance to school* is the shortest time (in minutes) it takes to reach school. *Gender attitude* is based on the girl's responses to questions capturing various gender roles in the family (e.g. 'Who should earn money for the family?'). It is the fraction of questions (out of 7) to which the girl responded with gender-neutral roles. *Risk Aversion* is the girl's response to the question 'On a scale from 0 (not at all willing to take risks) to 10 (very willing to take risks), which number do you give yourself?', inverted. *Patience* is the girl's response to the question 'On a scale from 0 (very patient) to 10 (very impatient), which number do you give yourself?', inverted. *Illness* is a dummy variable indicating if the girl reported having had any serious illness in the last year. *HH kids (no)* is the number of household members younger than 20. *HH kids no (f/m)* is the percentage of females among household members younger than 20. *Per Capita Expenditure* is the monthly household consumption (in Tanzanian Shillings) as reported by the household head, divided by the number of people living in the household. *Poverty (<2 USD/day)* is a dummy variable indicating if the per capita daily expenditure is less than 2 USD PPP. See Table B10 in the Online Appendix for further details on the covariates. The covariate variables have been standardised. All regressions include village fixed effects. Standard errors are clustered at the household level and given in parentheses. In square brackets p-values of the null hypothesis of no effect are provided. For the main effect of *Lottery* and interactions with *Lottery* these are calculated as randomisation inference p-values, for all other coefficients they are calculated analytically based on the reported clustered standard errors. The randomisation p-values are the percentile of the coefficient estimated under the true assignment in the distribution of coefficients estimated under 10000 alternative assignments.

can assess its quantitative plausibility. On average, winning the lottery increased WTP by 9%. The 3,200 TSh prize is around 1.4% of total household monthly expenditures. Assuming the whole amount is spent within a month, this gives an implied "elasticity" of 6. Among the constrained group WTP increases 27%, an elasticity of 19. While we do not have a clear benchmark to compare this to (it is not a traditional income elasticity because the shock is not an income shock and the expenditure is a one-time expense), income effects of this magnitude seem unlikely. It is also comforting that per-capita expenditure is not associated with higher WTP, see Table 2.

Third, certain behavioural mechanisms could increase the WTP of lottery winners. For example, an experimenter demand interpretation posits that winners believed they were expected to pay more, and did so out of reciprocity or perceived social pressure. (Other behavioural channels could include "house money," "mental accounting," or "anchoring" effects.) To mitigate these concerns, we separated the lottery from the program by framing it as a "thank you" for participating in the already-completed baseline survey, and by telling participants they were "free to do whatever you like with this money." We also spent time between the lottery draw and the WTP elicitation, explaining the study clubs.[18] A particular version of this concern is that participants perceived a connection between the credit constraints question and the lottery treatment. Because the question was just one of many in a large baseline survey, and because of the temporal separation, we think this is unlikely.

To the extent that personality traits correlate with such behavioural responses, the results in Table 2 are helpful, as both risk aversion and patience are not strongly predictive of

[18]These are consistent with recommended practices in the experimental literature (Zizzo, 2010; de Quidt et al., 2019). de Quidt et al. (2018) and Mummolo and Peterson (2018) subsequently developed new techniques to measure demand effects, finding they are modest in behavioural experiments conducted online. While the setting is very different, this gives further cause for optimism.

WTP. More importantly, these mechanisms are unlikely to explain the interaction we observe between our treatment and the borrowing constraints measures. It would have to be that more constrained households are more sensitive to these behavioural mechanisms. However, the inclusion of the personality traits and their interaction with the lottery win never changes the magnitude of our main interaction effect in a meaningful way.[19]

Fourth, the information session surely impacted WTP. The same information was provided to all study participants, but baseline knowledge, and hence belief updating, might have been heterogenous and correlated with borrowing constraints. Indeed, the results in Table 2 indicate that households with more experience of tutoring services, or more optimistic beliefs about returns to higher education, had higher WTP. Both variables might proxy for differences in information about remedial education at baseline. The fact that their inclusion does not affect our results suggests that differences in information are not driving the findings.

Fifth, while we described the WTP choice as a firm commitment, bound by a "contract," participants might not have viewed it as such. This could cause them to overbid, expecting to have the option to renege later. However, as with the other threats we do not believe this would generate our interaction effect of interest. If anything it could go the other way: limited liability protects households from being forced to pay a bill they cannot afford, so the cash transfer could actually decrease constrained households' propensity to overbid. As described in Section 2.4, due to unanticipated program issues the fees were ultimately not implemented, so we do not know if participants would have honoured their commitments. Maffioli et al. (2023) conduct a careful review of the WTP literature, and report a wide

[19] As suggested by a referee we also investigated a couple of other personality-like measures: confidence, and self-perceived communication and persuasive abilities; our findings are robust to controlling for these as well (results available on request).

range of non-payment rates. Default appears to be more frequent for instalment plans; ours was intended to be a single up-front payment.

4 Conclusion

Despite improvements in access to primary education, learning achievements remain low in many developing countries, particularly for children from lower socio-economic backgrounds. Remedial education programs have emerged as one possible way to ameliorate inequalities in educational attainment.

We study the role of borrowing constraints in determining families' willingness to pay for a remedial education program in Tanzania. Through a lottery, we distribute cash prizes that exogenously relax some households' constraints. Households are willing to pay 7% of average monthly per capita expenditure for their daughters to participate in the program. Winning 3,200 TSh in a lottery increases willingness to pay by approximately 9%. This effect is almost entirely driven by those households our survey identifies as borrowing constrained, whose WTP is depressed absent the lottery, and who increase their willingness to pay by 27% when they win the lottery. It is robust to controlling for a host of observable correlates.

We conclude that borrowing constraints play a significant role in shaping demand for educational programs: households with the ability to borrow value those opportunities; borrowing constrained households also value them, and in fact value them similarly to unconstrained households, but are not in a position to take up those educational investment opportunities. To the extent that borrowing constraints are correlated with socio-economic status, these results suggest that they are likely to propagate inequality across generations.

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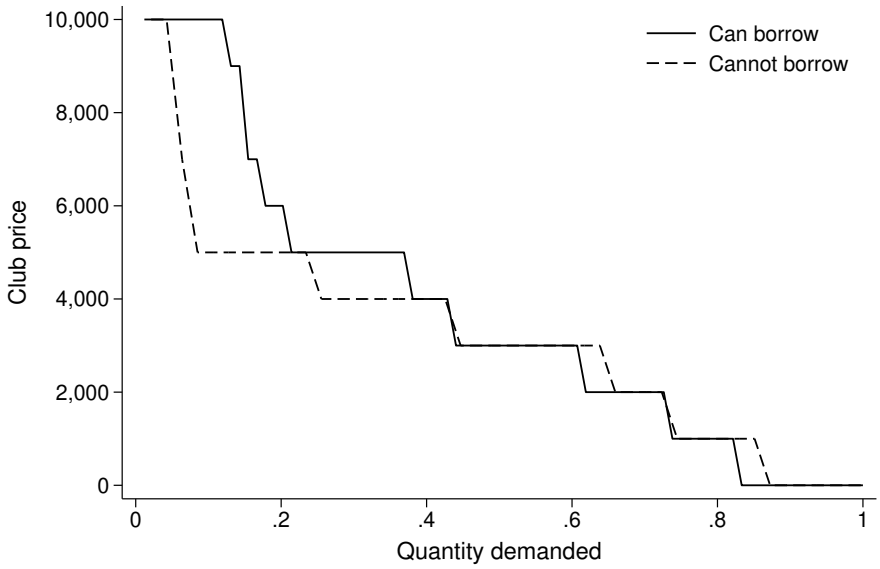
ONLINE APPENDIX FOR

BORROWING CONSTRAINTS AND THE DEMAND FOR EDUCATION: EVIDENCE FROM TANZANIA

Konrad Burchardi, Jonathan de Quidt, Selim Gulesci, Munshi Sulaiman

A Appendix Figures

FIGURE A1: DEMAND CURVES FOR LOTTERY LOSERS, SPLIT BY BORROWING CONSTRAINTS



Notes: Figure presents demand curves for the remedial education program, for lottery losers only, separately for those who can and cannot "borrow money for an important expenditure." Sample restricted to the long-run sample for whom participation data are available.

B Appendix Tables

TABLE B1: TABULATION OF LOTTERY AND BORROWING CONSTRAINTS MEASURES

Panel A: Borrowing constraints dummy		
	Can Borrow	Cannot Borrow
Lottery lose	210	99
Lottery win	212	121
Total	642	

Panel B: Borrowing constraints index		
	Below mean	Above mean
Lottery lose	165	226
Lottery win	161	253
Total	736	

Notes: Panel A shows the cross-tabulation of our lottery treatment and borrowing constraints dummy measure. Panel B shows the same for the borrowing constraints index, divided by above/below mean.

TABLE B2: SELECTION OF MARGINALISED SAMPLE

	(1)	(2)	(3)	(4)	(5)
	Census				
	All	Marginal.	Difference	Norm. D.	N
# Girls in household	1.394 (0.738)	1.420 (0.769)	0.168*** [0.000]	0.257	5965/5045
# Household members	8.778 (8.183)	8.590 (8.174)	-1.219*** [0.000]	-0.149	5965/5046
All children aged 6-17 in school	2.244 (1.439)	2.170 (1.460)	-0.477*** [0.000]	-0.352	5961/5044
Female head/spouse is literate	0.913 (0.281)	0.901 (0.298)	-0.078*** [0.000]	-0.334	5968/5048
Concrete/tiled/timbered floor	0.753 (0.431)	0.719 (0.449)	-0.220*** [0.000]	-0.611	5968/5048
Metal/tiled roof	0.910 (0.286)	0.904 (0.295)	-0.042*** [0.000]	-0.159	5968/5048
HH owns bicycles/vehicles	0.171 (0.376)	0.135 (0.342)	-0.229*** [0.000]	-0.548	5968/5048
HH owns radio	0.618 (0.486)	0.569 (0.495)	-0.320*** [0.000]	-0.772	5968/5048
HH owns lantern	0.482 (0.500)	0.450 (0.498)	-0.206*** [0.000]	-0.424	5968/5048
HH owns iron	0.545 (0.498)	0.488 (0.500)	-0.368*** [0.000]	-0.853	5968/5048
# Tables HH owns	0.830 (0.375)	0.810 (0.393)	-0.134*** [0.000]	-0.416	5968/5048

Notes: The table presents summary statistics for a number of census variables within the sample of census girls in the 69 study villages (in Column 1) and the narrower sample of girls who are marginalised (in Column 2). The mean and standard deviation (in parentheses) of the covariate in each respective sample are shown. We run a regression of the outcome on an indicator of being a member of the sample of Column 2. The coefficient estimate on the indicator is provided in Column 3, and associated p-values testing the null of no difference, based on standard errors clustered at the household level, are provided in square brackets. In Column 4 the normalised difference between the samples in Column 1 and 2 is given. In Column 5 the size of the sample of Column 1 and Column 2 are shown.

TABLE B3: SELECTION OF BASELINE SAMPLE

	(1) Census: Marginal.	(2) Baseline	(3) Difference	(4) Norm. D.	(5) N
# Girls in household	1.420 (0.769)	1.349 (0.649)	-0.103*** [0.000]	-0.140	5045/1573
# Household members	8.590 (8.174)	8.823 (8.292)	0.338 [0.187]	0.041	5046/1574
All children aged 6-17 in school	2.170 (1.460)	2.216 (1.604)	0.067 [0.163]	0.044	5044/1573
Female head/spouse is literate	0.901 (0.298)	0.888 (0.316)	-0.020** [0.039]	-0.066	5048/1575
Concrete/tiled/timbered floor	0.719 (0.449)	0.683 (0.466)	-0.053*** [0.000]	-0.118	5048/1575
Metal/tiled roof	0.904 (0.295)	0.918 (0.274)	0.021** [0.017]	0.071	5048/1575
HH owns bicycles/vehicles	0.135 (0.342)	0.168 (0.374)	0.047*** [0.000]	0.134	5048/1575
HH owns radio	0.569 (0.495)	0.533 (0.499)	-0.053*** [0.001]	-0.106	5048/1575
HH owns lantern	0.450 (0.498)	0.469 (0.499)	0.027* [0.087]	0.053	5048/1575
HH owns iron	0.488 (0.500)	0.460 (0.499)	-0.041*** [0.009]	-0.081	5048/1575
# Tables HH owns	0.810 (0.393)	0.755 (0.430)	-0.080*** [0.000]	-0.198	5048/1575

Notes: The table presents summary statistics for a number of census variables within the sample of marginalised census girls in the 69 study villages (in Column 1) and the narrower sample for whom additionally a baseline was conducted (in Column 2). The mean and standard deviation (in parentheses) of the covariate in each respective sample are shown. We run a regression of the outcome on an indicator of being a member of the sample of Column 2. The coefficient estimate on the indicator is provided in Column 3, and associated p-values testing the null of no difference, based on standard errors clustered at the household level, are provided in square brackets. In Column 4 the normalised difference between the samples in Column 1 and 2 is given. In Column 5 the size of the sample of Column 1 and Column 2 are shown.

TABLE B4: SELECTION OF WTP SAMPLE

	(1) Baseline	(2) WTP	(3) Difference	(4) Norm. D.	(5) N
EGRA (word/min)	40.90 (36.94)	42.73 (46.72)	3.89* [0.069]	0.099	1428/711
EGMA	0.621 (0.124)	0.617 (0.123)	-0.000 [0.955]	-0.063	1517/752
Raven Score	3.567 (1.715)	3.579 (1.724)	0.087 [0.311]	0.014	1631/805
Girl: Cannot Borrow	0.677 (0.468)	0.689 (0.463)	-0.000 [0.985]	0.047	1178/578
Girl: Cannot Easily Borrow	0.927 (0.260)	0.936 (0.245)	0.007 [0.643]	0.068	1178/578
HH: Cannot Borrow	0.342 (0.474)	0.343 (0.475)	-0.019 [0.453]	0.004	1276/642
HH: Cannot Easily Borrow	0.813 (0.390)	0.824 (0.381)	-0.006 [0.785]	0.054	1276/642
Borrowing Constraints Index	-0.028 (1.029)	-0.000 (1.000)	-0.012 [0.814]	0.054	1488/736
Per Capita Expenditure (TSh)	45193 (68651)	48077 (78894)	4389 [0.255]	0.084	1418/710
Tutoring	0.596 (0.491)	0.599 (0.490)	0.008 [0.737]	0.015	1608/789
Cognitive Skills	0.000 (1.000)	0.012 (1.060)	0.081 [0.126]	0.023	1631/805
Distance to School	23.34 (22.53)	23.83 (23.49)	0.79 [0.505]	0.043	1631/805
Returns Second. E.	0.216 (0.412)	0.209 (0.407)	-0.001 [0.948]	-0.033	1492/723
Gender Attitude	0.323 (0.266)	0.311 (0.270)	-0.020 [0.108]	-0.087	1587/775
Risk Aversion	3.324 (3.573)	3.177 (3.579)	-0.050 [0.751]	-0.081	1552/761
Patience	5.324 (3.890)	5.077 (3.998)	-0.307* [0.060]	-0.124	1520/742
Illness	0.473 (0.499)	0.478 (0.500)	-0.001 [0.959]	0.020	1597/790
HH kids (no)	2.903 (1.713)	2.973 (1.704)	0.168** [0.046]	0.081	1586/784
HH kids (f share)	72.72 (27.70)	72.79 (27.51)	0.08 [0.956]	0.004	1504/745

Notes: The table presents summary statistics for a number of covariates within the sample of successfully interviewed baseline girls in the 65 villages where a lottery was conducted (in Column 1) and the narrower sample for whom additionally their WTP was elicited (in Column 2). The sample of Column 2 corresponds to the estimation sample of Table 1. The mean and standard deviation (in parentheses) of the covariate in each respective sample are shown. We run a regression of the outcome on an indicator of being a member of the sample of Column 2 as well as village fixed effects. The coefficient estimate on the indicator is provided in Column 3, and associated p-values testing the null of no difference, based on standard errors clustered at the household level, are provided in square brackets. In Column 4 the normalised difference between the samples in Column 1 and 2 is given. In Column 5 the size of the sample of Column 1 and Column 2 are shown.

TABLE B5: BASELINE BALANCE BY LOTTERY WIN

	(1) Control	(2) Lottery Win	(3) Difference	(4) Norm. D.	(5) N
EGRA (word/min)	42.57 (38.44)	42.88 (53.39)	0.98 [0.804]	0.007	344/367
EGMA	0.616 (0.121)	0.618 (0.126)	0.000 [0.982]	0.017	362/390
Raven Score	3.701 (1.675)	3.464 (1.764)	-0.223* [0.052]	-0.138	391/414
Girl: Cannot Borrow	0.671 (0.471)	0.705 (0.457)	0.063* [0.066]	0.073	283/295
Girl: Cannot Easily Borrow	0.933 (0.251)	0.939 (0.240)	0.020 [0.312]	0.025	283/295
HH: Cannot Borrow	0.320 (0.467)	0.363 (0.482)	0.034 [0.344]	0.091	309/333
HH: Cannot Easily Borrow	0.825 (0.380)	0.823 (0.382)	0.003 [0.909]	-0.006	309/333
Borrowing Constraints Index	-0.037 (0.967)	0.036 (1.031)	0.092 [0.175]	0.073	360/376
Per Capita Expenditure (TSh)	46948 (60523)	49103 (92550)	1213 [0.815]	0.028	338/372
Tutoring	0.606 (0.489)	0.594 (0.492)	-0.006 [0.851]	-0.025	383/406
Cognitive Skills	0.045 (1.007)	-0.019 (1.108)	-0.054 [0.477]	-0.061	391/414
Distance to School	23.86 (24.10)	23.80 (22.93)	-0.04 [0.979]	-0.002	391/414
Returns Second. E.	0.214 (0.411)	0.204 (0.403)	-0.013 [0.669]	-0.026	350/373
Gender Attitude	0.308 (0.275)	0.314 (0.266)	0.010 [0.565]	0.021	377/398
Risk Aversion	3.054 (3.528)	3.293 (3.628)	0.149 [0.498]	0.067	368/393
Patience	5.180 (4.060)	4.982 (3.944)	-0.043 [0.855]	-0.050	355/387
Illness	0.506 (0.501)	0.452 (0.498)	-0.053 [0.111]	-0.109	385/405
HH kids (no)	3.069 (1.674)	2.884 (1.728)	-0.186* [0.093]	-0.108	378/406
HH kids (f share)	72.96 (26.04)	72.62 (28.85)	-0.32 [0.871]	-0.013	362/383

Notes: The table presents summary statistics for a number of covariates for the estimation sample of Table 1, i.e. the sample of girls who have both been interviewed at baseline and a WTP has been elicited. The mean and standard deviation (in parentheses) of the covariate in the sample of girls who won the lottery (Column 2) and who did not win the lottery (Column 1) are provided. To test for differences between those samples along the covariates, we run an ordinary least squares regression of specification (1), i.e. including village fixed effects, with the covariate as dependent variable. The coefficient estimate on *Lottery Win* is provided in Column 3, and associated p-values testing the null of no difference, based on standard errors clustered at the household level, are provided in square brackets. In Column 4 the normalised difference between the samples in Column 1 and 2 is given. In Column 5 the size of the samples of Column 1 and Column 2 are shown.

TABLE B6: DEMAND FOR EDUCATION AND BORROWING CONSTRAINTS: ALTERNATIVE SPECIFICATIONS

	WTP (TSh)				
	(1)	(2)	(3)	(4)	(5)
Panel A: *Branch Fixed Effects*					
Lottery Win	332 (171) [0.061]	359 (192) [0.083]	203 (240) [0.601]	334 (178) [0.071]	355 (176) [0.052]
Cannot Borrow			-739 (295) [0.012]		
Cannot Borrow × Lottery Win			514 (400) [0.123]		
Borrowing Const. Index					-519 (133) [0.000]
Borrowing Const. Index × Lottery Win					473 (178) [0.005]
Branch FE	Yes	Yes	Yes	Yes	Yes
R^2	0.276	0.293	0.300	0.282	0.297
Panel B: *Enumerator Fixed Effects*					
Lottery Win	344 (167) [0.052]	380 (189) [0.062]	240 (234) [0.461]	349 (174) [0.060]	369 (172) [0.044]
Cannot Borrow			-619 (290) [0.033]		
Cannot Borrow × Lottery Win			455 (391) [0.174]		
Borrowing Const. Index					-488 (126) [0.000]
Borrowing Const. Index × Lottery Win					435 (172) [0.011]
Enumerator FE	Yes	Yes	Yes	Yes	Yes
R^2	0.305	0.321	0.326	0.314	0.327
Panel C: *Standard Errors Clustered at Village Level*					
Lottery Win	311 (206) [0.042]	362 (223) [0.043]	119 (257) [0.605]	297 (211) [0.067]	312 (201) [0.051]
Cannot Borrow			-522 (309) [0.096]		
Cannot Borrow × Lottery Win			734 (425) [0.047]		
Borrowing Const. Index					-387 (137) [0.006]
Borrowing Const. Index × Lottery Win					432 (206) [0.009]
Village FE	Yes	Yes	Yes	Yes	Yes
R^2	0.474	0.493	0.497	0.482	0.490

Notes: The table reports ordinary least squares estimates based on specifications (1) and (2). *Lottery* indicates whether the individual has been randomly assigned to receive a lottery payout. *Cannot Borrow* indicates whether the household head responded that she/he would not be able to "borrow money for an important expenditure". *Borrowing Constraints Index* is an index over 4 variables measuring the extent of borrowing constraints. Standard errors are given in parentheses. In square brackets *p*-values of the null hypothesis of no effect are provided. For the main effect of *Lottery Win* and interactions with *Lottery Win* these are calculated as randomisation inference *p*-values, for all other coefficients they are calculated analytically based on the reported clustered standard errors. The randomisation *p*-values are the percentile of the coefficient estimated under the true assignment in the distribution of coefficients estimated under 10000 alternative assignments. Columns 2 and 4 show results from the specification of column 1, but in the samples of columns 3 and 5, respectively. Each panel presents a variation of the specifications underlying the results of Table 1: In Panel A and B branch and enumerator fixed effects are included instead of village fixed effects, respectively. In Panel C standard errors are clustered at the village level instead of the household level.

TABLE B7: DEMAND FOR EDUCATION AND BORROWING CONSTRAINTS DUMMY: ROBUSTNESS TO CONTROLS

Variable	Lottery	Constraint	Lottery × Constraint	Covariate	Lottery × Covar.	N
Tutoring	108 (213) [0.652]	-545 (285) [0.056]	719 (381) [0.058]	-137 (139) [0.324]	184 (181) [0.339]	633
Cognitive Skills	128 (210) [0.567]	-516 (283) [0.069]	721 (379) [0.053]	94 (120) [0.434]	-121 (182) [0.508]	642
Distance to School	124 (210) [0.588]	-511 (283) [0.071]	725 (379) [0.052]	130 (100) [0.196]	-104 (176) [0.450]	642
Returns Second. E.	91 (228) [0.746]	-609 (301) [0.044]	866 (413) [0.032]	370 (144) [0.010]	-441 (201) [0.019]	577
Gender Attitude	81 (215) [0.767]	-483 (285) [0.091]	781 (386) [0.038]	7 (128) [0.955]	191 (183) [0.209]	624
Risk Aversion	77 (219) [0.751]	-498 (293) [0.090]	819 (396) [0.035]	-159 (143) [0.267]	135 (186) [0.324]	613
Patience	84 (223) [0.768]	-520 (309) [0.092]	762 (416) [0.056]	73 (151) [0.628]	-37 (190) [0.900]	600
Illness	114 (212) [0.626]	-529 (284) [0.064]	708 (383) [0.062]	-55 (139) [0.691]	77 (175) [0.481]	635
HH kids (no)	100 (213) [0.673]	-535 (279) [0.056]	762 (377) [0.045]	-104 (142) [0.462]	70 (182) [0.703]	640
HH kids (f share)	111 (219) [0.660]	-574 (286) [0.045]	899 (389) [0.018]	19 (140) [0.894]	10 (186) [0.960]	614
Per Capita Expenditure (TSh)	166 (216) [0.465]	-486 (290) [0.094]	676 (397) [0.086]	-153 (181) [0.398]	261 (199) [0.305]	608
Poverty (<2 USD/day)	166 (216) [0.464]	-484 (290) [0.096]	672 (396) [0.089]	101 (140) [0.471]	-150 (184) [0.440]	608

Notes: The table reports ordinary least squares estimates based on specification (2). *Lottery* indicates whether the individual has been randomly assigned to receive a lottery payout. *Constraint* indicates whether the household head responded that she/he would not be able to "borrow money for an important expenditure". *Tutoring* is a dummy variable indicating if the girl attended any tutoring or study group during the past year. *Cognitive skills* is a normalised index combining the girl's score in a Math exam (EGMA), a reading exam (EGRA) and a Raven's test. *Distance to school* is the shortest time (in minutes) it takes to reach school. *Gender attitude* is based on the girl's responses to questions capturing various gender roles in the family (e.g. 'Who should earn money for the family?'). It is the fraction of questions (out of 7) to which the girl responded with gender-neutral roles. *Risk Aversion* is the girl's response to the question 'On a scale from 0 (not at all willing to take risks) to 10 (very willing to take risks), which number do you give yourself?', inverted. *Patience* is the girl's response to the question 'On a scale from 0 (very patient) to 10 (very impatient), which number do you give yourself?', inverted. *Illness* is a dummy variable indicating if the girl reported having had any serious illness in the last year. *HH kids (no)* is the number of household members younger than 20. *HH kids no (f/m)* is the percentage of females among household members younger than 20. *Per Capita Expenditure* is the monthly household consumption (in Tanzanian Shillings) as reported by the household head, divided by the number of people living in the household. *Poverty (<2 USD/day)* is a dummy variable indicating if the per capita daily expenditure is less than 2 USD PPP. See Table B10 in the Online Appendix for further details on the covariates. The covariate variables have been standardised. All regressions include village fixed effects. Standard errors are clustered at the household level and given in parentheses. In square brackets *p*-values of the null hypothesis of no effect are provided. For the main effect of *Lottery* and interactions with *Lottery* these are calculated as randomisation inference *p*-values, for all other coefficients they are calculated analytically based on the reported clustered standard errors. The randomisation *p*-values are the percentile of the coefficient estimated under the true assignment in the distribution of coefficients estimated under 10000 alternative assignments.

TABLE B8: MAIN SPECIFICATION, ADDING 1ST PC OF COVARIATES OR DOUBLE/DEBIASED LASSO

	WTP (TSh)				
	(1)	(2)	(3)	(4)	(5)
Panel A: Credit Constraints Dummy					
Lottery Win	119	62	70	120	106
	(210)	(261)	(253)	(214)	(204)
Cannot Borrow	-522	-682	-713	-504	-531
	(282)	(342)	(322)	(286)	(269)
Cannot Borrow × Lottery Win	734	1091	972	712	731
	(379)	(487)	(468)	(381)	(357)
Covariates 1st PC		-77		-39	
		(137)		(115)	
Covariates 1st PC × Lottery Win		159		131	
		(167)		(137)	
N	642	485	485	642	642
R^2	0.497	0.506		0.506	
Panel B: Credit Constraints Index					
Lottery Win	312	341	257	304	295
	(159)	(220)	(209)	(161)	(156)
Borrowing Const. Index	-387	-531	-527	-393	-372
	(126)	(149)	(142)	(126)	(125)
Borrowing Const. Index × Lottery Win	432	572	513	440	377
	(168)	(208)	(193)	(168)	(164)
Covariates 1st PC		-23		-36	
		(129)		(106)	
Covariates 1st PC × Lottery Win		71		92	
		(160)		(129)	
N	736	530	530	736	736
R^2	0.490	0.501		0.497	

Notes: Panel A reports results for the borrowing constraints dummy and Panel B for the index. Column (1) reproduces our primary specification from Table 1. Column (2) adds the first principal component of all covariates included in Table 2. We include its main effect and interaction with Lottery Win. This variable is missing if any covariate is missing. Column (3) assesses robustness to LASSO-selected controls from among the same covariate set, plus squares and two-way interactions, using the partialling-out estimator of Chernozhukov et al. (2018). Column (4) is the same as Column (2) but we replace missing covariates with their mean before constructing the first principal component. Column (5) is the same as Column (3) but we replace missing covariates with their mean and include dummies for missing covariate values.

TABLE B9: DEMAND FOR EDUCATION AND BORROWING CONSTRAINTS, LONG-RUN SAMPLE ONLY

	WTP (TSh)			Participation (0/1)	Attendance (per week)
	(1)	(2)	(3)	(4)	(5)
Panel A: Borrowing constraints dummy					
Lottery Win	447 (329) [0.158]	447 (329) [0.158]	134 (394) [0.796]	0.008 (0.065) [0.953]	0.086 (0.209) [0.680]
Cannot Borrow			-817 (510) [0.110]	-0.160 (0.065) [0.015]	-0.276 (0.263) [0.295]
Cannot Borrow × Lottery Win			891 (717) [0.154]	0.026 (0.103) [0.669]	0.006 (0.383) [0.938]
Village FE	Yes	Yes	Yes	Yes	Yes
Mean Outcome (C)	3511	3511	3511	0.183	0.424
Observations	269	269	269	269	269
R^2	0.485	0.485	0.491	0.429	0.312
Panel B: Borrowing constraints index					
Lottery Win	345 (292) [0.232]	345 (292) [0.232]	371 (285) [0.197]	0.016 (0.041) [0.695]	0.033 (0.126) [0.821]
Borrowing Const. Index			-722 (253) [0.005]	-0.047 (0.035) [0.174]	-0.248 (0.128) [0.053]
Borrowing Const. Index × Lottery Win			840 (303) [0.003]	-0.010 (0.045) [0.721]	0.035 (0.149) [0.871]
Village FE	Yes	Yes	Yes	Yes	Yes
Mean Outcome (C)	3532	3532	3532	0.177	0.421
Observations	316	316	316	316	316
R^2	0.457	0.457	0.478	0.360	0.276

Notes: The table reports ordinary least squares estimates based on equations (1) and (2). **We restrict the sample to include only girls that appear in the column (4) specification.** The dependent variable in columns (1)–(3) is the household's WTP (in TSh) for the remedial education program, in column (4) it is a dummy for whether the girl ever attended the program's club, and in column (5) the number of times per week she visited. Column (2) shows results from the specification of column (1), but in the sample of column (3). *Lottery Win* indicates whether the individual has been randomly assigned to receive a lottery payout. *Cannot Borrow* is a dummy variable indicating if the household head reported that it would not be possible for them to borrow money for an important expenditure. *Borrowing Constraints Index* is an index combining 4 dummy variables indicating if the respondents (girl or the household head) states that it would not be possible *or* it would be anything but easy to borrow money for an important expenditure. We calculate the index by first normalising each indicator by subtracting the sample mean and dividing by its standard deviation; then taking the average of the four normalised indicators, and normalising again. If only some of these dummies are available we impute the missing ones at the sample mean. All regressions include village fixed effects. Standard errors are clustered at the household level and given in parentheses. In square brackets *p*-values of the null hypothesis of no effect are provided. For the main effect of *Lottery Win* and interactions with *Lottery Win* these are calculated as randomisation inference *p*-values, for all other coefficients they are calculated analytically based on the reported clustered standard errors. The randomisation *p*-values are the percentile of the coefficient estimated under the true assignment in the distribution of coefficients estimated under 10000 alternative assignments. Mean WTP among all lottery losers, and the number of observations, are reported at the bottom of the table.

TABLE B10: VARIABLE DESCRIPTIONS

Variable	Explanation
Cognitive skills	The normalised index combining three indicators: EGRA, EGMA and Raven score (see below for details of these indicators). To calculate the normalised index we first normalisnormalisede each indicator by subtracting its sample mean and dividing by its standard deviation; then we take the average of the three normalised indicators; and we normalise again.
Borrowing constraint index	Both girls and household heads were asked separately: *'If you needed to borrow money for an important expenditure (e.g. health or school related expenditure), how easy would it be for you to borrow the money?'* with answer options being 'easy', 'not easy, but possible', and 'not possible'. We generate indicators for whether respondents state it is not possible and anything but easy, respectively. We calculate the index by first normalising each indicator by subtracting the sample mean and dividing by its standard deviation; then taking the average of the four normalised indicators, and normalising again. If only some of these dummies are available we impute the missing ones at the sample mean. The resulting index is increasing the more constrained the girl/household head is.
Distance to school	For girls enrolled in school, it is the shortest time (in minutes) it takes to reach school. For girls out of school, it is the average time it takes for (in-school) girls within the same village to reach school.
EGRA	Number of words per minute that the girl is able to read in the reading test. The test contained 50 words that the respondent was asked to read out. We divide the number of correctly read words by the time it took for the respondent to read them to obtain 'words per minute'.
EGMA	Score measuring numeracy skills based on a Math exam (EGMA). The EGMA had 5 sections. Some sections had 10 and some had 20 questions. We aggregate the scores by dividing the number of correct answers given in each section of the exam by the total number of questions in the relevant section (either 10 or 20) to obtain the percentage of correct answers in each section. Then, we take the average of the 5 sections, giving equal weight to each section, to obtain the total score for EGMA.
Gender attitude	Girls were asked the following questions: 'Who should earn money for the family?', 'Who should have a higher level of education in the family?', 'Who should be responsible for washing, cleaning and cooking?', 'If there is no water pump or tap, who should fetch water?', 'Who should be responsible for feeding and bathing children?', 'Who should help the children in their studies at home?', 'Who should be responsible for looking after the ill persons?'. The possible responses were 'Males', 'Females', 'Both males and females'. For each variable, we generate a dummy variable equal to 1 if the response is 'Both males and females'; we then take the average of these indicators, corresponding to the fraction of statements to which the girl responded with gender-neutral attitudes.
Girl: Cannot Borrow	Dummy indicating if the girl reported that it would be 'not possible' to borrow money for an important expenditure (e.g. health or school related expenditure).
Girl: Cannot Easily Borrow	Dummy indicating if the girl reported that it would be 'not easy, but possible' or 'not possible' to borrow money for an important expenditure (e.g. health or school related expenditure).
Lottery win	Dummy indicating if the individual has been randomly assigned to receive a lottery payout.
HH kids (no)	Number of household members younger than 20, as reported by the household head.
HH kids (f share)	Percentage of females among household members younger than 20.
HH: Cannot Borrow	Dummy indicating if the household head reported that it would be 'not possible' to borrow money for an important expenditure (e.g. health or school related expenditure).
HH: Cannot Easily Borrow	Dummy indicating if the household head reported that it would be 'not easy, but possible' or 'not possible' to borrow money for an important expenditure (e.g. health or school related expenditure).
Illness	Dummy indicating if the girl reported having had any serious illness in the last year.
Patience	The girl's response to the question 'On a scale from 0 (very patient) to 10 (very impatient), which number do you give yourself?', inverted.
Per Capita Expenditure	Monthly household consumption (in Tanzanian Shillings) as reported by the household head, divided by the number of people living in the household. Consumption items include: food (purchased), food (produced), tobacco, alcohol, fuel, cosmetics/toiletries/hairdressing, entertainment, transportation, electricity, salary of maid, household utensils, household furniture, household textiles, clothing, rent (for housing), material for ritual ceremonies, alms and gifts, brideprice, legal expenses.
Poverty (<2 USD/day)	Dummy indicating if the per capita daily expenditure is less than 2 USD PPP.
Raven Score	Number of correct answers (0-7) in a test using Raven's Progressive Matrices.
Returns Second. E.	The girl respondent was asked two separate questions: 'If you were to stop studying once you complete primary school, do you think you will be working (in an income generating activity) by the time you are 25 years old?'; 'If you were to stop studying once you complete secondary school, do you think you will be working (in an income generating activity) by the time you are 25 years old?' Based on these, we generate a dummy indicating if the girl reported that she would not be able to get a job at age 25 if her highest qualification is a primary school degree, but she would be able to do so with a secondary school degree.
Risk Aversion	The girl's response to the question 'On a scale from 0 (not at all willing to take risks) to 10 (very willing to take risks), which number do you give yourself?', inverted.
Tutoring	Dummy indicating if the girl reported that she attended any tutoring or study group during the past year.

C Implementation details

C.1 Sample Selection

1. We selected, by simple randomisation, 8 out of 20 of the NGO's branch offices in which to conduct the study.

2. The NGO's field staff identified 105 villages that were close to potential treatment schools, to participate in the study.

3. We select 69 villages to receive the program, as follows:

   (a) For each village, the NGO's field staff provided the identity of the nearest school. In most cases, multiple villages share the same nearest school (or two schools on the same campus). The program was to be assigned at the school/campus level, so either all villages or no villages connected to a given school/campus would receive the program. We call each group of villages connected to a given school a "cluster."

   (b) When a branch had schools connected to only one village, we created clusters by grouping such villages in twos or threes.

   (c) The program as a whole was randomised at the cluster level.

   (d) We only measure WTP for the program in villages assigned to receive the program, so our analysis data comes only from the program villages. Our sampling of study villages is thus clustered at the school/campus level, within the set of study branches.

   (e) We randomised the price of the program (to be revealed after the WTP elicitation) *within* cluster. There were two prices, zero TSh or 3,000 TSh. Thus for each school some villages were assigned free clubs, and others paid clubs. This distinction is not relevant for our analysis as we use only the WTP data, measured prior to the revelation of the club price.

4. In these villages we conduct a census, leading to a sample of 5,968 girls.

5. We screen for eligibility. Eligibility required the girl either (i) had dropped out of school within the last two years, or (ii) was at risk of dropping out (a grade of less than 50% in Mathematics, Science, or English in the last exam), or (iii) belongs to a poor household, based on a poverty scorecard for Tanzania, developed by Grameen foundation, or (iv) has lost one or both parents, or (v) displays signs of physical or mental disability. There are 902 girls who satisfied condition (i), 1,954 girls who satisfied condition (ii), 3,658 girls who satisfied condition (iii), 1,750 who satisfied condition (iv), and 151 who satisfied condition (v). We exclude 920 girls who do not satisfy any of those conditions, leading to a sample of 5,048 girls.

6. We target a sample of 27 girls per village for the baseline, with the goal of not more than one girl per household (in case there are multiple eligible girls in a household). 58 villages have more than 27 households with at least one eligible girl, 4 have more than 27 eligible girls but fewer than 27 households, 7 have fewer than 27 girls even when repeatedly sampling from households.

7. We randomly select a sample of girls that we will attempt to reach first for the baseline survey, along with a "reserve" list in case we cannot find somebody. So for the 58 villages with more than 27 households this involves selecting 27 primary households plus a reserve list, in the 4 villages with more than 27 girls but fewer households, we allow for sampling multiple girls in a household, and for the 7 villages with fewer than 27 girls, all girls are added to the primary list. This leads to a primary targeted sample of 1822 girls in the main sample. Of which 1566 are from villages which have more than 27 households with eligible girls, 108 are from villages which have more than 27 girls but fewer than 27 households, 148 are from villages which have less than 27 girls even when repeatedly sampling from households. There are 1263 girls in the reserve sample. Of which 1255 are from villages which have more than 27 households with eligible girls, 8 are from villages which have more than 27 girls but fewer than 27 households, 0 are - by construction - from villages which have less than 27 girls even when repeatedly sampling from households. We cap the number of reserve girls at 25 per village.

8. Turning to those we actually find and survey in the baseline: of the 1822 girls targeted, 1470 are in the baseline data. Another 188 girls in the baseline data are drawn from the reserve sample. In cases where we could not reach our target sample size from the baseline and reserve list, we allowed for convenience sampling of additional girls. There are 59 girls in the baseline data who fall into this category.

9. Because of challenges finding our targeted number of girls in some villages, we compensated by asking enumerators to keep sampling from the reserve lists in villages where we were able to reach 27 sampled girls without exhausting the primary targeted sample and reserve list.

10. This leaves us with a baseline sample of 1,717 girls. In 6 villages we have exactly 27 girls, in 47 villages we have fewer than 27, and in 16 villages more than 27.

11. We collected baseline survey information from the girls as well as from their household heads.

12. All baseline girls get a lottery ticket that entitled them to a prize draw for 3,200 TSh if they came to an information meeting about the program, and that half of eligible attendees would win. We organised the information meetings which included the elicitation of WTP for participation in the program. All baseline girls were invited to attend, as well

as any other girls living in the village. They were to be accompanied by a household member, ideally the household head.

13. Of the 1,717 girls in the baseline, 880 attended a WTP meeting, and in addition 252 non-baseline girls attended a meeting. However as we do have individual or household covariates for the non-baseline girls, and they were not eligible for the lottery, we do not include them in the analysis.

14. Of the 880 baseline girls that attended a meeting, we have WTP data for 825, we infer that the 55 for whom we do not have data chose not to participate in the WTP elicitation. The 825 girls correspond to 799 distinct households (in 22 households we have two girls and in 2 households we have three girls).

C.2 Lottery implementation

The lottery was intended to be implemented as follows. In each of the 69 villages, all baseline girls that attended the meeting to be eligible for the lottery, conducted via a prize draw, with a 50% chance of winning 3200 TSh (enumerators were to assign prizes to 50% of them, rounding up in case of an off number). In most villages this was implemented as intended but we encountered some minor implementation issues in some villages.

1. In 4 villages the lottery winners were not recorded by the enumerators, so we cannot analyse the lottery variable. This leaves us with 65 villages and 805 girls for whom we have WTP data.

2. In 45 villages, zero non-baseline girls won, and 50% (rounding up) of baseline girls won the lottery. We infer that the lottery was implemented perfectly in these cases.

3. In 11 villages, zero non-baseline girls won, but the number of baseline girls that won was slightly different to the target (equal to 50% rounding up ± 1).

4. In 3 villages, some non-baseline girls won. However the total number of winners within baseline was equal to 50%, rounding up. In these cases we infer the lottery draw was implemented correctly except than non-eligible participants were entered mistakenly.

5. In 3 villages, some non-baseline girls won, and the number of winners within baseline was slightly different to the target, equalling 50% of attendees, rounding up, ± 1.

6. In 1 village, some non-baseline girls won, and 12/18 baseline girls and 5/12 non-baseline girls won the lottery (i.e. 17/30 attendees).

7. In 2 villages, zero non-baseline girls won, and the number of baseline girls that won is more than ± 1 from the target (specifically, the winner/eligible ratios were 7/17 and 5/19).

C.3 Balance checks

Of 5,968 girls in the census, 5,048 were identified as marginalised, to be targeted for the program (see footnote 6). Appendix Table B2 compares all census participants to the marginalised group, by presenting the average outcomes of a number of important covariates in both samples (columns 1 and 2), the difference between those averages conditional on village fixed effects and associated p-values (column 3), the normalised difference (column 4) and the number of girls who reported the covariate in either sample (column 5). Due to the screening, marginalised girls have fewer assets, fewer household members, and lower school attendance in the household.

Appendix Table B3 compares the marginalised sample to the actual baseline sample, following the same format as Table B2. Again we find statistically significant differences between the two samples. Girls in the baseline sample are more likely to come from households with illiterate female heads and households with fewer girls, and the composition of assets in the baseline sample differs from the sample of marginalised girls in the census. However, the magnitude of these differences is generally small, with normalised differences below 0.2 throughout.

Of the 1717 girls who participated in the baseline survey, 805 (around 47%) also attended the WTP meeting, provided a WTP, and lottery winners were recorded in the village. Appendix Table B4 shows that the girls who came to the WTP meeting were remarkably similar to the general population of baseline girls in terms of cognitive skills, socio-economic status, attitudes, schooling related variables and household characteristics. Exceptions to that rule are that girls who attended the WTP meeting had higher reading test scores, came from households with slightly more children and were less patient (judging by a normalised difference great than 0.1 or significant mean differences).

Appendix Table B5 provides balancing tests for the lottery randomisation. Recall that the design specified a treatment probability of 50%, but this was not always implemented perfectly. We report the means of key covariates in the group of girls who did not win the lottery (column 1), who won the lottery (column 2), the difference between these means conditional on village fixed effects and associated randomisation inference p-values (column 3), the normalised difference (column 4) and the number of girls who reported the covariate in either sample (column 5). The table reveals that the randomisation was successful in creating a balanced sample as judged by the normalised differences being generally low, lower than 0.1. The only exception to that rule is that girls who did win the lottery did have lower Raven scores and were from smaller households. This needs to be kept in mind when interpreting the results. Also we note that only 3 of the 18 variables show statistically significant differences. This suggests that the randomisation of the lottery treatments was unlikely compromised and supports our treating the lottery variable as exogenous.

C.4 WTP meeting script

BEFORE THE START

KABLA YA KUANZA

AS GIRLS COME INTO THE MEETING, IDENTIFY THEM.

WATAMBUE WATOTO WANAPOKUJA KWENYE MKUTANO

If they were surveyed at baseline:

Kama walitembelewa wakati wa utafiti wa kwanza:

1. *If they brought their lottery ticket, write the name and number of another piece of paper and put it in the plastic bag.*
   If they did not bring their lottery ticket, make a new lottery ticket where you write the name and a number and give this to the girl. Also write the name and number on another piece of paper and put it in the plastic bag.
   Kama wataleta kadi zao za bahati nasibu, andika jina na namba kwenye kipande kingine cha karatasi kisha ukiweke kwenye mfuko wa plasitiki.
   Kama hawakuleta kadi zao za bahati nasibu, tengeneza tiketi mpya za bahati nasibu ambapo utaandika jina na namba na umpatie mtoto. Pia andika jina na namba kwenye kipande kingine cha karatasi kisha kiweke kwenye mfuko wa plasitiki.
2. *Find their corresponding sticker, stick it to an answer sheet and give them the sheet.*
   Zitafute stika zinazolandana na tiketi mpya, za bahati ibandike kwenye karatasi ya majibu na uwape karatasi hiyo.
3. *Ask them to find a place to sit with their household head.*
   Waombe watafute sehemu watakayokaa na wakuu wao wa kaya

If they were not surveyed at baseline

Kama hawakutembelewa kwenye utafiti wa kwanza

1. *Record their name, age, and household head name on a blank sticker, affix to an answer sheet and give them the sheet.*
   Andika majina yao, umri, na jina la mkuu wa kaya kwenye stika tupu, ibandike kwenye karatasi ya majibu na uwape karatasi hiyo.
2. *Ask them to find a place to sit with their household head.*
   Waombe watafute sehemu watakayokaa na wakuu wao wa kaya.

INTRODUCTION

UTANGULIZI

Hello and welcome to the meeting. At this meeting we will do several things.

Habari na Karibuni kwenye mkutano. Katika mkutano huu tutafanya mambo kadhaa.

1. First we will explain and then find out the winners of the lottery.
   Kwanza tutatoa maelezo na harafu tutawapata washindi wa bahati nasibu.

2. Second we will explain the new BRAC study club to you
   Pili tutatoa maelezo kwenu ya klabu mpya za BRAC.
3. Last we will find out who is going to join the study club
   Mwisho tutaenda kujua ni akina nani watajiunga na klabu za masomo.

If you have any questions at any time, please raise your hand and we will answer.
Kama una maswali yoyote wakati wowote, tafadhari nyoosha mkono wako, uliza na tutakujibu.

LOTTERY
BAHATI NASIBU

The lottery tickets were given to girls who participated in our survey a few months ago, which is why not everybody has a ticket. Today, *NUMBER OF TICKETS* girls have entered the lottery draw, which means that *NUMBER OF TICKETS/2* will win the prize of .... *Tsh*.

Kadi za bahati nasibu zilitolewa kwa watoto walioshiriki kwenye utafiti wetu wa kwanza miezi michache iliyopita, na ndiyo maana siyo watu wote wanazo kadi hizo. Leo, watoto {KIASI} watakaoingia kwenye mchezo wa bahati na sibu inamaanisha kuwa nusu yao watashinda zawadi ya Sh. ..../=

We will do the lottery draw now.
Tutachezesha mchezo wa bahati na sibu sasa hivi.

DRAW THE LOTTERY TICKETS. IF N PEOPLE ENTERED, N/2 SHOULD BE DRAWN (ROUND UP TO NEAREST WHOLE NUMBER, I.E. IF 25 PEOPLE ENTER THERE SHOULD BE 13 WINNERS). ANNOUNCE THE WINNING TICKET NUMBERS.

CHEZESHA MCHEZO WA BAHATI NA SIBU. KAMA WATU X WALIINGiA KWENYE MCHEZO, TOA TIKETI (X/2) NUSU YA IDADI YA WATU WALIOINGIA KWENYE MCHEZO (IKARIBISHE KWENYE NAMBA KAMILI, MFANO; KAMA WATU 25 WALIINGIA KWENYE MCHEZO, INATAKIWA WATU 13 WAWE WASHINDI)

GIVE THE WINNERS THEIR MONEY AND ANNOUNCE:
WAPE WASHINDI PESA ZAO NA UWATANGAZE

You are free to do whatever you like with this money.
pesa hizi uko huru kuzifanyia chochote upendacho

AND ASK THE PEOPLE TO SIGN THE PAYOUT SHEET
NA UWAOMBE WATU KUWEKA SAHIHI ZAO KWENYE KARATASI YA MALIPO

STUDY CLUB EXPLANATION
Now, we will explain the BRAC study club program to you. This is a new program that is starting soon in this village. Any eligible girl can join, but you need to sign up **today**. We will explain how to sign up in a few minutes.

MAELEZO YA KLABU ZA MASOMO

Sasa, tutauelezea kwenu mpango wa klabu za masomo za BRAC. Huu ni mpango mpya unaoanzishwa kwenye Kijiji hiki. Mtoto yeyote mwenye vigezo vilivyoainishwa anaweza kujiunga, lakini itatakiwa kujisajiri **leo**. Tutaeleza namna ya kujiandikisha ndani ya dakika chache zijazo.

NOW THE CLUB LEADER OR PO SHOULD DESCRIBE THE CLUB

JOINING INFORMATION

There may be a fee to join the study club, or it might be free to join. The price **has already been set** and is inside this envelope.

TAARIFA ZA KUJIUNGA

Kujiunga na klabu za masomo, kunaweza kuwa na ada, au inaweza kuwa ni bure. Bei tayari imeshapangwa na ipo ndani ya bahasha hii.

SHOW ENVELOPE WITH CLUB PRICE INSIDE

ONYESHA BAHASHA YENYE BEI YA KLABU NDANI YAKE

Before we open the envelope we are going to do a short survey to find out who wants to join the club, depending on the price. After the survey we will open the envelope and reveal the price. That will tell us who is going to join the club and who is not.

Kabla hatujaifungua bahasha tutaenda kufanya utafiti mdogo kujua ni nani anataka kujiunga na klabu, kulingana na bei. Baada ya utafiti tutaifungua bahasha na kuitambua bei. Hiyo itatuambia ni nani atajiunga na klabu na nani hatajiunga.

Here is how the survey works. Each girl has been given a sheet that looks like this *HOLD UP LARGE SHEET.* On this sheet is a list of prices. The price that is written in this envelope is one of those prices. It could be free!

Hivi ndivyo utafiti utakavyokuwa. Kila mtoto amepewa karatasi inayoonekana hivi *INYANYUE JUU KARATASI.* Kwenye karatasi hii kuna orodha ya bei. Bei iliyoandikwa kwenye karatasi hiii ni moja kati ya bei zote. Inawezekana ikawa ni bure!

For each price on the list, we want to know if you would join the club at that price. You should tick next to each price if you would be **willing and able** to pay that price to join the club.

Kwa kila bei iliyopo kwenye orodha, tunataka kufahamu kama ungependa kujiunga kwenye klabu kwa bei hiyo. Utatakiwa kutiki pembeni mwa kila bei kama ungependa na unaweza kulipia kujiunga na klabu kwa bei hiyo.

You will have to pay the joining fee at the first club meeting.

Utatakiwa kulipia ada ya kujiunga kwenye mkutano wa kwanza wa klabu.

Before we begin the survey, we are going to do a practice to help you to understand how the survey works.

Kabla hatujaanza utafiti, tutaenda kufanya zoezi litakalotusaidia kuelewa jinsi utafiti utakavyofanya kazi.

SOAP PRACTICE - ZOEZI LA SABUNI

The purpose of the game is for us to learn how much you are willing to pay for a bar of soap. This is the soap you can buy today:

Lengo la mchezo ni kwa sisi kujifunza kwa kiasi gani mngependa kulipa kununua kipande cha sabuni. Hii hapa ni sabuni ambayo leo mtaweza kununua:

SHOW THE SOAP.

IONYESHA SABUNI.

The amount that is charged here for this soap has been decided previously, and this price is hidden inside this envelope.

Kiasi cha bei ya sabuni hii kimeshaamuliwa kabla, na bei hiyo imefichwa ndani ya bahasha hii.

SHOW PRICE ENVELOPE ONYESHA BAHASHA YENYE ORODHA YA BEI.

We want to understand how much money people are willing to pay to get the soap. The price may be FREE, 100 TSh, 200 TSh, 300 TSh, 400 TSh, 500 TSh, 600 TSh, 700 TSh, 800 TSh, 900 TSh, or 1000Tsh. You have all received a sheet with all these prices written on it.

Tunataka kufahamu ni kiasi gani cha fedha watu wanapenda kulipa ili kupata sabuni. Bei inaweza kuwa ni BURE, Sh. 100, 200, 300, 400, 500, 600, 700, 800, 900, au 1000. Wote mmepata karatasi zilizoandikwa orodha ya bei zote hizi.

Here is how it works. For each of the prices, we want you to think about whether you would be willing and able to pay that price, **TODAY**, to get the soap. **If you are willing and able to pay the price**, you should tick the box.

Hivi ndivyo itakvyokuwa. Kwa kila bei tunawataka mfikiri kama mngependa na mnaweza kulipa bei hiyo ili kupata sabuni. **Kama ungependa na unaweza kulipia bei hiyo**, LEO, utatakiwa kutiki kwenye kisanduku.

SHOW THE SOAP SHEETS. ONYESHA KARATASI YENYE ORODHA YA BEI ZA SABUNI

So this is our list. You tick the boxes for prices that you are willing and able to pay today.
So if you can and want to pay maybe 200 TSh for the soap, you tick the box next to 200 TSh and the

boxes above.
If you're able and wiling to pay 1000 TSH, you tick 1000 and all boxes above.

SHOW ON THE SOAP SHEET WHICH BOXES SHOULD BE TICKED

It's according to how much you can and how much you want to pay for this soap TODAY.
Kwa hiyo, hii hapa ndiyo orodha yetu. Utatiki visanduku kwenye bei ambayo ungependa na unaweza kulipa leo hii.
Kwa hiyo kama unaweza na labda unataka kulipia sabuni kwa Sh. 200, utatiki kisanduku cha mbele ya Sh. 200 pamoja na visanduku vilivyopo juu yake.
Kamwa unaweza na unapenda kulipia Sh. 1000, utatiki kisanduku cha mbele ya Sh, 1000 pamoja na visanduku vyote vilivyopo juu yake.

ONYESHA KWENYE ORODHA YA SABUNI NI VISANDUKU GANI VITATAKIWA KUTIKIWA
Ni kutokana na kiasi gani unaweza na kiasi gani unataka kulipa LEO kwa ajiri ya sabuni.

After you have all finished filling out the entire sheet, we will open the envelope and find out what the set price is. Everyone who marked on the sheet that he or she is willing to pay that price will get the soap and has to pay the **fixed price from the envelope.** Everyone who did not tick the box next to that price because they are not willing to pay that price will not get the soap and will not pay.

Mara wote mtakapomaliza kujaza karatasi yote, tutafungua bahasha na kuona ni bei gani imewekwa. Kila mmoja aliyeweka alama kwenye orodha kwamba angependa kulipa bei hiyo au zaidi atapata sabuni na atalipia bei **iliyowekwa kwenye bahasha.** Kila mmoja ambaye hakutiki kisanduku kilichopo mbele ya bei kwasababu hakupenda kulipa bei hiyo hatapata sabuni na hatalipa chochote.

To make sure that we all understand it, let's consider some examples:

Ili kuhakikisha kuwa wote tumeuelewa mchezo, hebu tuangalie baadhi ya mifano:

- First consider Neema. She will buy the soap if the price is 600 TSh. Of course this also means that she is willing to pay any price lower than 600 TSh. Therefore Neema should tick the boxes for 600, 500, 400, 300, 200, 100 and FREE. She should NOT tick the boxes for 700, 800, 900, 1000. *SHOW EXAMPLE A*
- Kwanza mfikirie Neema. Atanunua sabuni kama bei itakuwa ni Sh. 600. Hata hivyo hii inamaanisha kuwa anapenda kulipa bei ambayo ni chini ya Sh. 600. Kwa hiyo Neema atatiki visanduku vyenye bei za Sh. 600, 500, 400, 300, 200, 100 na BURE. Hatatakiwa kutiki visanduku vyenye bei za 700, 800, 900, 1000. *ONYESHA MFANO A*
- Now consider Alice. Alice wants the soap but only has 200 Tsh so she cannot pay more than that today. What boxes should Alice tick? Please write down your answer on the sheet.
  ***AFTER** THE RESPONDENTS HAVE THOUGHT ABOUT IT, SHOW EXAMPLE B*
- Sasa mfikirie Alice. Alice anataka sabuni lakini ana sh. 200 tu kwa hiyo LEO hawezi kulipia zaidi ya hiyo. Ni kisanduku gani Alice atatiki? Tafadhari andika jibu lako kwenye karatasi ya majibu.
  ***BAADA** YA WASHIRIKI KUFIKIRI KUHUSU HILI, ONYESHA MFANO B*

- Now consider Grace and Lucy. Grace only wants the soap if it is FREE, and she is not willing to pay anything to buy the soap. Lucy does not like soap and does not want it, even if it is FREE. What boxes should Lucy and what boxes should Grace tick? Please write down your answer on the sheet.
  AFTER THEY HAVE THOUGHT ABOUT IT, SHOW EXAMPLE C.
  So if someone really does not want the soap at all, like Lucy, she should not even tick the "Free" box. But someone who does like to get the soap when it's free should tick this box like Grace.
- Sasa mfikirie. Grace na Lucy. Grace anataka sabuni kama itakuwa ni BURE tu, na hangependa kulipia chochote kuipata sabuni. Lucy hapendi sabuni na haitaki hata kwa BURE. Ni visanduku gani Lucy na Grace watatakiwa kuvitiki? Tafadhari andika jibu lako kwenye karatasi.
  BAADA YA KUWA WEMEWEZA KUFIKIRI JUU YA HILI, ONYESHA MFANO C
  Kwa hiyo kama mtu hataki sabuni kabisa, kama Lucy, hatatakiwa hata kutiki kisanduku cha "BURE". Lakini kwa yeyote anayependa sabuni itakapokuwa ni bure atatiki kisanduku kama Grace.
- Now, consider Kate and Anna. Kate is willing to pay 800 TSh for the soap and Anna is willing to pay 600 TSh. This means their sheets would look like this:
  SHOW EXAMPLE D.
  So Kate will tick all boxes up to 800 and Anna all up to 600. If the price in the envelope is 500 TSh, who will be able to buy the soap? And how much would each of them pay? Please write down the answers on the sheet.
  GIVE THEM TIME TO THINK ABOUT IT
  In this case, both Kate and Anna can buy the soap and both of them pay 500 TSh. Even though Kate was willing to pay more than Anna, both of them only have to pay the price that was written in the envelope.
- Sasa mfikiri Kate na Anna. Kate anapenda kulipia sabuni kwa Sh. 800 na Anna anapenda kulipa Sh.600. Hii inamaanisha kuwa karatasi zao zitaonekana hivi: *ONYESHA MFANO D.*
  Kwa hiyo Kate atatiki visanduku vyote mpaka cha 800 na Anna naye vyote mpaka 600. Kama bei ya kwenye bahasha ni Sh. 500, nani wataweza kununua sabuni? Na kila mmoja wao atalipa kiasi gani? Tafadhari andika majibu kwenye karatasi. *WAPE MUDA WA KUFIKIRI.*
  Kwa jinsi hii, wote wawili Kate na Anna wanaweza kununua sabuni na wote watalipa Sh. 500. Ingawaje Kate angependa kulipa zaidi ya Anna, wote wawili watalipa bei iliyoandikwa kwenye bahasha tu.
- For the last question, I will show you two imaginary answer sheets. One of them has a mistake.
  SHOW EXAMPLE E.
  Can you tell me which one has the mistake and what the mistake is?
  So the sheet from person 1 contains a mistake. The mistake is that it does not make sense to be willing to pay 500, but not 400. Similarly, it does not make sense to say you are willing to pay 100 TSh but not 0 TSh. So if the box for 500 TSh is ticked, all boxes above that should also be ticked.
- Kwa swali la mwisho, nitawaonyesha karatasi mbili za majibu. Mojawapo ina kosa.
  ONYESHA MFANO E.

Mnaweza kuniambia ni ipi ina kosa na ni kosa gani?
Kwa hiyo karatasi kutoka kwa mtu wa kwanza inalo kosa. Kosa ni kuwa haileti maana kuwa ungependa kulipa Sh. 500 na siyo 400. Hivyo hivyo haileti maana kusema ungependa kulipa Sh. 100 na siyo 0. Kwa hiyo kama kisanduku cha Sh. 500 kimetikiwa, basi visanduku vyote vya juu yake pia vinatakiwa kutikiwa.

Are there any questions? Kuna maswali yoyote?

ONCE YOU ARE HAPPY THAT EVERYBODY UNDERSTANDS, MOVE ON TO THE NEXT PART
UTAKAPORIDHIKA YA KUWA KILA MTU AMEELEWA, NENDA SEHEMU INAYOFUATA

So to summarize, the price hidden in the envelope is the price for which you can buy the soap from us today. Everyone who ticked the box next to that price will have to buy the soap for that price. So you should only tick a box if you are **willing and able** to pay this price, TODAY. If you don't have any money on you right now, then you should only tick the box next to FREE. If you don't tick a box, it means you are not allowed to buy the soap for this price if this is the price hidden in the envelope.

Kwa hiyo kwa kuhitimisha, bei iliyofichwa kwenye bahasha ni bei ambayo unaweza kununua sabuni leo kutoka kwetu. Kila mmoja aliyetiki kisanduku kilichoko mbele ya bei hiyo atatakiwa kununua sabuni kwa bei hiyo. Kwa hiyo LEO utatakiwa kutiki kisanduku tu iwapo unapenda na unaweza kulipa bei hiyo. Kama kwa sasa hivi huna pesa yoyote, basi utatakiwa kutiki kisanduku cha mbele ya BURE. Kama hukutiki kisanduku, inamaanisha hautaruhusiwa kununua sabuni kwa bei hiyo kama bei hiyo ni ile iliyofichwa kwenye bahasha.

Ok, please now fill out your soap sheets together with your parent, marking all of the prices that you would be willing and able to pay. Raise your hand if you have any questions. Please remember that **the price is already determined and is hidden inside this envelope** so your answers **cannot** affect the price in any way. Please be quiet as you do the forms. We are interested in what **YOU** are willing to pay. **There are no right or wrong answers.**

Sawa, tafadhari sasa jaza karatasi yako yenye bei za sabuni pamoja na mzazi wako, wekea alama bei zote ambazo ungependa na ungeweza kulipa. Nyoosha mkono wako kama una maswali yoyote. Tafadhari kumbuka kuwa **bei imeshapangwa tayari na imefichwa ndani ya bahasha** kwa hiyo majibu yako kwa vyovyote vile **hayata** athiri bei zilizowekwa. Tafadhari kaa kimya unapojaza fomu yako. Tungependa kujua ni nini mngependa kulipa. **Hakuna majibu sahihi wala ambayo si sahihi.**

NOW GIRLS SHOULD RESPOND ON THEIR "SOAP FORMS" FOR EACH OF THE PRICES WHETHER THEY ARE WILLING TO PAY THAT AMOUNT.

SASA WATOTO WATATOA MAJIBU YAO KWENYE KARATASI ZA BEI ZA SABUNI KWA KILA BEI KAMA WANGEPENDA KULIPA KIASI HICHO

Below the price list, there is a question about why you indicated that you are willing to pay this price and lower prices, but not prices that are higher. There are some answers that you can choose from: the first answer is that you don't have more money than this right now, the second answer is that you think this is how much the soap is worth. If you have any other reasons you can write those down after answer C, where you can specify the reason. Please circle the letters for the answers that are true for you. You may circle as many answers as you like.

Chini ya orodha ya bei, kuna swali kuhusu kwa nini ulionyesha kuwa unapenda kulipa bei hii na bei za chini yake, lakini siyo bei zilizopo juu ya hapo. Kuna baadhi ya majibu ambayo unaweza kuchagua kutoka humo: Jibu la kwanza ni kwamba kwa sasa huna pesa zaidi ya hizi, jibu la pili ni kwamba unafikiri hivi ndivyo thamani ya sabuni inatakiwa kuwa. Kama una sababu zozote unaweza kuziandika zote baada ya jbu C ambapo unaweza kuthibitisha jibu. Tafadhari zungushia herufi za majibu ambayo ni ya kweli kwako. Unaweza kuzungushia majibu mengi kwa kadri upendavyo.

NOW GIRLS SHOULD RESPOND ON THEIR "SOAP FORMS" HOW MUCH THEY ARE WILLING TO PAY FOR THE SOAP. WHEN FINISHED, COLLECT UP THE FORMS AND OPEN THE SOAP PRICE ENVELOPE. ANNOUNCE WHO IS GOING TO BUY THE SOAP AND MAKE THE TRANSACTIONS.

SASA WATOTO WATATAKIWA KUJIBU KWENYE "FOMU ZA SABUNI" NI KIASI GANI WANGEPENDA KULIPIA SABUNI. WAKIMALIZA, ZIKUSANYE FOMU ZOTE NA UFUNGUE BAHASHA YENYE BEI ZA SABUNI. MTANGAZE NI NANI ATAENDA KUNUNUA SABUNI NA UMPATIE SABUNI NAYE AKUPE PESA.

Ok, now we are going to do the same process, but this time we want to know what you are willing and able to pay to join the study club.

Sawa, sasa tutaenda kufanya kwa mtindo huo huo, lakini kwa wakati huu tutapenda kufahamu ni kiasi gani ungependa na unaweza kulipia kujiunga kwenye klabu ya masomo.

The price to join the study club could be FREE, 1000Tsh, 2000Tsh,… 10000Tsh. That price has already been set and is written in this envelope. **Your answers cannot affect the price in any way**.

Bei ya kujiunga na klabu ya masomo inaweza kuwa ni BURE, sh.1000, 2000,… 10000. Bei hiyo tayari imeshapangwa na imeandikwa kwenye bahasha. **Majibu yako hayawezi kuathiri bei kwa namna yoyote ile.**

We want to know, for each price, if you would be willing and able to pay that price to join the study club. We will collect the payment at the opening of the club. So you must be able to pay the fee on that day. BRAC may be charging some money for the participation in the club not to make profits, but to make the project more sustainable, so that more people can benefit from this program!

Tunataka kufahamu, kwa kila bei, kama ungependa na unaweza kulipia bei hiyo kujiunga na klabu ya masomo. Tutayakusanya malipo hayo wakati wa ufunguzi wa klabu. Kwa hiyo ni lazima uweze kulipia ada kwenye tarehe hiyo. BRAC inaweza kutoza kiasi cha fedha kwa kushiriki kwenye Klabu na siyo kutengeneza faida, lakini kufanya mradi kuwa endelevu, ili watu wengi zaidi waweze kufaidika na mpango huu!

The process will be the same as for the soap. First, you fill out the form ticking all of the prices that you would be willing and able to pay at the opening of the club next week. Then we open the envelope and find out what the price is. **Everyone who ticked that price will sign a contract that promises to join the club and pay the price at the first club meeting**.

Utaratibu utakuwa sawa na ule wa sabuni. Kwanza, utajaza fomu ukitiki bei zote ambazo ungependa na unaweza kuzilipa wakati wa ufunguzi wa klabu wiki ijayo. Harafu tutafungua bahasha na kuona ni bei gani iliyopo. **Kila mmoja aliyetiki bei hiyo atasaini mkataba unaoahidi kujiunga na klabu na kulipia bei hiyo wiki ya kwanza ya mkutano.**

Let's begin. Please don't look at what other people answer, we are interested in what **YOU** think only and there is no right or wrong answer. Answer the questions together with your parent.

Hebu tuanze. Tafadhari usiangalie majibu ya mtu mwingine, tunapendezwa na jinsi unavyofikiri tu. Hakuna jibu saa hii wala lisilo sahihi. Jibu maswali pamoja na mzazi wako.

If you have any questions or need help, raise your hand and the staff will come to assist you.
Kama una swali lolote au kutaka msaada, nyoosha mkono wako na kuna mtu atakuja kukusaidia.

PRICE REVELATION
UTAMBUZI WA BEI

ONCE EVERYONE HAS COMPLETED THEIR FORMS, COLLECT THEM UP. THEN OPEN THE ENVELOPE AND ANNOUNCE THE PRICE. GO THROUGH THE FORMS AND PICK OUT EVERYONE WHO WAS WILLING TO PAY THAT PRICE. ANNOUNCE THEIR NAMES, THEN TAKE THE JOINING FORMS AND GET THE GIRL AND HOUSEHOLD HEAD TO SIGN THEM.

MARA KILA MMOJA ATAKAPOKUWA AMEMALIZA KUJAZA FOMU YAKE, ZIKUSANYE. HARAFU IFUNGUE BAHASHA NA ITANGAZE BEI. ZIPITIE FOMU ZOTE NA UCHUKUE YULE ALIYEPENDA KULIPIA BEI HIYO. WATANGAZE MAJINA YAO, HARAFU CHUKUA FOMU ZA KUJIUNGA NA UWAPE WATOTO NA WAKUU WAO WA KAYA KUZIJAZA.

Ok, the price is *PRICE*. The following people will be joining the club. *LIST NAMES*. Now we will sign the joining forms.

Sawa, bei ni *BEI*. Watu wafuatao watajiunga na klabu. *ORODHESHA MAJINA*. *Sasa tutaweka sahihi kwenye fomu za kujiunga.*

Thank you everyone for coming to the meeting. If you are joining the club, please come to the opening and bring your joining fee.
Asanteni kwa kila mmoja wenu kwa kuja kwenye mkutano. Kama unajiunga na klabu, tafadhari njoo kwenye ufunguzi na ulete ada yako ya kujiunga.

FIGURE C2: WTP ELICITATION

BEI		**NDIYO,** Nataka kujiunga na Club kwa bei hii
Sh. 0	BURE!	
Sh. 1,000		
Sh. 2,000		
Sh. 3,000		
Sh. 4,000		
Sh. 5,000		
Sh. 6,000		
Sh. 7,000		
Sh. 8,000		
Sh. 9,000		
Sh. 10,000		

Kwanini ulionyesha kuwa ungependa kulipa bei hizi? (unaweza zungushia majibu mengi)

a. Sina pesa zaidi ya hiyo.
b. Nafikiri hiyo ndiyo gharama yake.
c. Sababu nyingine: Elezea.....

FIGURE C3: WTP ELICITATION, ENGLISH TRANSLATION

PRICE		**YES,** I want to join the Club at this price
Sh. 0	FREE!	
Sh. 1,000		
Sh. 2,000		
Sh. 3,000		
Sh. 4,000		
Sh. 5,000		
Sh. 6,000		
Sh. 7,000		
Sh. 8,000		
Sh. 9,000		
Sh. 10,000		

Why did you say that you would like to pay these prices?

a. I do not have more money than that.
b. I think that's the cost.
c. Another reason: Specify .....

References

Chernozhukov, V., D. Chetverikov, M. Demirer, E. Duflo, C. Hansen, W. Newey, and J. Robins (2018). Double/debiased machine learning for treatment and structural parameters. *The Econometrics Journal 21*(1), C1–C68.