Poor and Rational: Decision-making under Scarcity*

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November 25, 2021

Abstract

A growing literature associates poverty with anomalies in decision-making. We investigate this link in a sample of over 3,000 small-scale farmers in Zambia, who were given the opportunity to exchange randomly assigned household items for alternative items of similar value. Analyzing a total of 5,842 trading decisions, and leveraging cross sectional, seasonal and randomized variation in financial resource availability, we show that exchange asymmetries decrease in magnitude when participants are more financially constrained. This result is robust to a variety of items and experimental procedures, and does not appear to be mediated by changes in cognitive performance. Consistent with the interpretation that scarcity leads to more rational decisions by increasing the utility loss from foregone trading, we show that trading probabilities go up when the market value of the items is exogenously increased.

Keywords: endowment effect, poverty, decision-making, development

JEL classification numbers: C93, D12, O12

*We thank A. Patrick Behrer for excellent research assistance and Rachel Levenson for project management and help throughout the study. We thank Leandro Carvalho, Pam Jakiela, John List, Supreet Kaur, Karen Macours, Charlie Plott, Simon Quinn, Frank Schilbach, and audience members at numerous seminars and workshops for constructive comments. Fieldwork was implemented by Innovations for Poverty Action and supported by Growth and Labor Markets in Low Income Countries (GLM-LIC), the International Growth Centre and an anonymous donor. Dietmar Fehr is grateful for financial support from the German Research Foundation (DFG) through the CRC 649 "Economic Risk". This study is registered in the AEA RCT Registry with the identification number AEARCTR-0001111.

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1 Introduction

A substantial body of evidence documents that individual decision-making is prone to behavioral biases and deviations from normative rationality (e.g., Camerer et al., 2003; DellaVigna, 2009), and that such decision anomalies may be particularly pronounced among the poor (e.g., Duflo, 2006; Mullainathan, 2007; Haushofer and Fehr, 2014). However, the relationship between poverty and decision-making is far from obvious. On the one hand, a lack of financial resources may affect decision-making if an increased focus on financial matters absorbs finite cognitive bandwidth (Mani et al., 2013; Mullainathan and Shafir, 2013). On the other hand, scarce financial resources make the same decisions more consequential. This may help focus attention, minimize mistakes, and improve decision quality (Goldin and Homonoff, 2013; Shah et al., 2015; Maćkowiak et al., 2021; Gabaix, 2019). In spite of potentially wide-spread implications, causal evidence on both how and why the availability of financial resources affects decision-making is largely missing.[1]

In this paper, we use multiple sources of variation in households' financial constraints to test how the scarcity or abundance of financial resources affects real-stakes decision-making in a low-income setting. Our evidence comes from decision experiments with 3,059 small-scale farmers in rural Zambia over a period of 14 months. We focus on behavior in one of the most basic economic decisions: the exchange of goods. A voluminous literature documents that individuals tend to place greater value on goods they own than on identical goods they do not own. The resulting gap between willingness to pay and willingness to accept is commonly referred to as the "endowment effect."[2] This finding has contributed to the development of theories of reference-dependent preferences (see Ericson and Fuster, 2014; O'Donoghue and Sprenger, 2018, for reviews) and has implications for a broad range of economic decisions including homeownership, worker ef-

[1] We specifically refer to naturalistic, real-stakes decisions with a clear normative benchmark. Other papers measure the effect of financial constraints on time or risk preferences, intertemporal choices, and framing or anchoring (e.g., Carvalho et al. 2016; Lichand and Mani 2020; Bartoš et al. 2021), in some cases involving real stakes and tests for rationality violations. Some recent papers also show that financial resources affect productivity (Banerjee et al., 2020; Kaur et al., 2021) but cannot isolate the role of decision-making.

[2] The term "endowment effect" was introduced by Thaler (1980). However, some critics have argued against the use of this term as it suggests an interpretation of the observed anomaly (e.g., Plott and Zeiler, 2005, 2007). While we primarily use the term "exchange asymmetries" to describe the findings in our experiment, we will also use the endowment effect terminology in reference to the broader literature.

fort, technology adoption, migration choices and investment (e.g., Genesove and Mayer, 2001; Hossain and List, 2012; Liu, 2013; Clark and Lisowski, 2017; Anagol et al., 2018). In addition to its status in the pantheon of behavioral biases, measuring the endowment effect is well suited to our study objectives: it provides a naturalistic vehicle for observing real decisions that incur few costs other than cognitive or attentional ones.[3]

Our decision experiments were embedded in a large-scale randomized controlled trial on credit access and labor supply that involved repeated surveys over multiple years (see Fink, Jack, and Masiye, 2020). The standard survey protocol provided all households with a pre-determined item as compensation for their time at the end of the survey. We modified this procedure by randomly endowing participants with one of two roughly equally-valued items midway through the survey. The items were common household necessities worth about USD 0.50 or one-fifth of the daily agricultural wage. At the end of the survey, participants were offered the opportunity to trade the endowed item for the other item. The random assignment of the initial item implies that half of participants received their less-preferred item and should have exchanged it for their preferred item given near-zero trading costs. We find instead that, on average, only 35 percent of participants trade the endowed item.

To investigate whether decision-making is affected by the availability of financial resources, we capitalize on three different sources of variation: (1) we exploit cross sectional variation in wealth at baseline, (2) we compare decision-making over one and a half agricultural cycles, measuring outcomes shortly after the harvest – when households receive most of their income – in 2014, during the lean or hungry season before the harvest in 2015, and again after the harvest in 2015, and (3) we leverage randomized village-level variation in the timing and availability of small consumption loans during the 2015 hungry season.[4] All three sources of variation are predictive of households' consumption

[3]A limitation of our decision experiments is that they do not allow us to assess the normative rationality of each individual decision or decision-maker; instead, they provide a measure of normative rationality at the population level.

[4]The consumption loans provided households with three bags of maize or the cash value equivalent at the start of the hungry season. Repayment was due at harvest time, approximately six months later, with 5 percent monthly interest. Around 90 percent of eligible households took up the loan and around 80 percent fully repaid in the year that the exchange experiments were conducted. Further details are provided in Section 4.2 and in Fink et al. (2020).

levels and food availability.

Across all three sources of variation, we find the same pattern: greater scarcity is associated with reduced exchange asymmetries. First, households in the bottom asset quintile are about 5 percentage points more likely to trade than households in the top quintile. Second, participants are 7 to 12 percentage points more likely to trade during the hungry season than at harvest time. Third, we find that households without access to the randomized hungry season loan intervention are about 18 percentage points more likely to trade than households that received a grain or cash loan in the 3 weeks prior to the decision experiment. Together, these results provide robust evidence of decision-making that is closer to the normative benchmark when households are more financially constrained.

We rule out alternative explanations for the observed relationship between scarcity and decision-making, focusing on alternatives that can accommodate all three sources of variation in financial resource availability. Taking advantage of the control offered by our setting and following Plott and Zeiler (2007), we randomly vary experimental procedures to test the overall sensitivity of trading behavior to implementation details. None of the procedural variants – assignment procedure, participants' attachment to the endowed item (duration of initial assignment and physical possession of the item) or expectations regarding future trading opportunities – affect the measured exchange asymmetries. This makes it unlikely that spurious differences in trading behavior are correlated with all three of the measures of scarcity in our setting. We further assess whether social norms or experimenter demand, which might vary with scarcity, influence trading by adjusting the language of the trading script to request that participants trade their endowed item for the other item.[5] Again, we find no measurable impact on trading decisions. Similarly, we find no evidence that market access is related to trading behavior. Finally, we rule out that the scarcity results are explained by experience, variation in other village, household or individual characteristics or other features of survey implementation.

Next, we examine the potential mechanisms that lead to more rational decision-

[5]The robustness of trading decisions to different experimental procedures also suggests that indifference is unlikely to underlie the high average exchange asymmetry that we document, since individuals who are close to indifferent should respond to the small differences in transaction costs across procedures.

making when financial resources are more scarce. First, we investigate "tunneling," which suggests that scarcity leads to a focus on immediate financial concerns, absorbing cognitive bandwidth and diverting attention away from other decisions (Mani et al., 2013; Mullainathan and Shafir, 2013). Following this literature, we implemented a standard set of unincentivized cognitive tests in a sub-sample of participants, and test the effect of scarcity on cognitive performance using the same cross sectional, seasonal and randomized variation in financial resources used in our main analysis. Even though we see a strong positive cross sectional relationship between wealth and cognitive performance, as predicted by the human capital literature (e.g., Laajaj and Macours, 2019), we find no clear relationship between these measures and seasonal or experimental variation in scarcity in our sample. More generally, we find that cognitive scores are not predictive of exchange decisions in our setting, which allows us to also rule out alternative explanations for the relationship between scarcity and decision-making, such as variation in the opportunity cost of time or in alcohol consumption, both of which should also affect performance on the cognitive tests.

Second, to assess whether scarcity affects decision-making by changing the relative value of the items, we introduced a "high value" item pair that consisted of two items worth around USD 13, which corresponds to about 28 percent of average monthly household income. We find that the likelihood of trading increases by 8.3 percentage points. Strikingly, this reduction in the magnitude of the exchange asymmetry is similar to the observed reduction in the hungry season, which – taken literally – implies that going from a time of abundance at harvest to a time of scarcity during the hungry season is equivalent to a more than twenty-fold increase in the utility value of the exchange items. We discuss two plausible interpretations of the finding that a higher value placed on the items, whether due to scarcity or to the high value item pair, leads to more rational decision-making: a change in attentional allocation, as predicted by models of rational inattention (Sims, 2003; Maćkowiak et al., 2021), or a preference-based mechanism, such as a change in references points (e.g., O'Donoghue and Sprenger, 2018). While we lack the data to definitively distinguish among these alternatives, the evidence we present appears most consistent with rational inattention: an increase in the utility at stake leads to greater attentional investment in the decision. This implies that trading probabilities vary with scarcity not because of variation in rationality but instead because the costs of a mistake depend on the financial resources available to the decision maker. The different interpretations also have different welfare implications, which we discuss in in Section 6.

Our results make three contributions to the literature at the cross-roads of behavioral and development economics. First, we contribute to an emerging literature on the psychology of the poor (e.g., Duflo, 2006; Mullainathan, 2007; Schilbach et al., 2016; Kremer et al., 2019). Previous studies suggest that poverty may affect decision-making and behavior through a number of pathways, including that financial concerns absorb the cognitive bandwidth needed for other decisions (Shah et al., 2012; Mani et al., 2013; Mullainathan and Shafir, 2013), that increased stress and depression interfere with decision-making or increase biases (Haushofer and Fehr, 2014; Haushofer and Shapiro, 2016), or that the living conditions of the poor contribute to worse decision-making (Dean, 2019; Lichand and Mani, 2020; Schilbach, 2019). To date, few papers have traced effects from exogenous variation in scarcity through to real stakes decisions. We fill that gap and provide evidence that scarcity leads to more rational decision-making related to a well-documented behavioral anomaly. We also show that scarcity does not necessarily worsen cognitive performance. Our results complement correlational evidence that low-income people consistently make decisions closer to normative predictions than high-income people in a host of hypothetical choice scenarios (Shah et al., 2015; de Bruijn and Antonides, 2021).

Second, this paper adds to the ongoing debate about the robustness of behavioral anomalies in general (Levitt and List, 2008; Falk and Heckman, 2009; Charness and Fehr, 2015; Camerer, 2015; Kessler and Vesterlund, 2015), and the endowment effect in particular (Ericson and Fuster, 2014). Despite a large literature on the endowment effect, evidence from outside of the laboratory, and particularly from low income settings, is relatively scarce.[6] We present field evidence involving transactions large enough to have a

[6] Some recent work leverages more easily accessible online panels (Chapman et al., 2017; Fehr and Kuebler, 2021). Fehr and Kuebler (2021), for example, provide evidence on small-stakes exchange asymmetries in a representative sample of Germans and show that trading behavior correlates with migration choices and stock market participation.

meaningful impact on household well-being. In this way, our work relates to an influential series of experiments at sport cards shows in the United States demonstrating the relationship between the endowment effect and market experience (List, 2003, 2004). On average, trading rates in that setting are similar in magnitude to our pooled results, though professional dealers are significantly more likely to trade their assigned baseball memorabilia than non-dealers (List, 2003). Related work by Tong et al. (2016) shows that trading experience reduces reliance on the (impulsive) use of loss-aversion-linked neurological processes. Our results suggest that similar shifts away from impulsive decision-making could also occur under scarcity. In a setting more similar to ours – and to our knowledge the only other experimental measurement of exchange asymmetries in a low-income country – Apicella et al. (2014) show that, in a population of hunter-gatherers, participants with more exposure to markets display a stronger endowment effect than those with less market exposure. Both professional sports card traders and hunter-gatherers with little market access arguably face higher stakes in their trading decisions than do amateur traders or hunter-gatherers with more market access, respectively, and may therefore devote greater attention to trading decisions, consistent with our preferred interpretation of our own findings.

Finally, a growing number of field studies in developing countries document real-world behavior consistent with an endowment effect. For example, Anagol et al. (2018) find that winners of an initial public offering (IPO) in India are more likely to hold on to their shares than non-winners. Giné and Goldberg (2017) find that prior savings account holders in Malawi are less likely to switch to a cheaper account than are new customers, but that experience erodes this "endowment effect." The endowment effect may also explain low take-up rates of certain loan types, in particular if they are collateralized by existing assets (Carney et al., 2018). Our study bridges the lab and field literatures by studying the effect of both natural and induced sources of variation in financial resources on real-stakes decisions. Notably, while the specific magnitudes of our findings may not generalize, the robustness of the results to different types of scarcity suggest that the endowment effect varies in predictable ways depending on economic circumstances, and is substantially less pronounced when financial resources are more scarce.[7]

2 Study setting and experimental design

2.1 Study setting

The study was implemented in Chipata District in Eastern Zambia in 2014 and 2015. Most of the district's population (456,000 inhabitants as of the 2010 census) lives in rural areas, and most rural households rely on small-scale farming as their primary source of income. Agriculture is rainfed and agricultural incomes are low. In 2013, average annual household income was around 3,000 Kwacha, which corresponded to approximately USD 600 at the time.[8] With 5-6 household members on average, income per capita is substantially less than USD 1 per day. The rainfed nature of production concentrates income in a single harvest season between May and August, and leads to a pronounced hungry season in the months leading up to harvest, when many households reduce consumption due to a lack of food. With early crops becoming available in April, food shortages and hunger usually spike between January and March (Fink, Jack, and Masiye, 2020).

2.2 Experimental design

The experiments reported here were embedded in household surveys conducted as part of a randomized evaluation of a seasonal loan program (see Fink, Jack, and Masiye (2020) for further detail on the randomized evaluation). As part of the evaluation, households were surveyed up to four times per year. In the first year of the study, all farmers received a small box of commonly used washing powder (called "Boom," the local brand name) as compensation for their time at the end of the survey. In the second year of the study, rather than providing Boom to all households, we implemented a modified version of the Knetsch (1989) exchange paradigm with a subset of households in each survey, ex-

[7] We further discuss external validity, along the lines of List (2020), in Section 6.

[8] In 2013, the exchange rate was around 5 Kwacha per USD. At the time of the data collection reported in this paper, it was 5.5 to 6 Kwacha per USD. We use 6 Kwacha per USD in the remainder of the paper when we report USD equivalents.

plained in detail below. We conducted the decision experiments between July 2014 and September 2015 with a total of 3,059 households across 175 villages. Households were randomly phased in to participation, resulting in between one and three decisions per household over the study period and a total of 5,842 individual decisions. Households not participating in the exchange experiment received the standard compensation (Boom) at the end of each survey. All household surveys were conducted by trained interviewers with adult household representatives – typically the male or female head of household – in their homes, used electronic survey devices (tablets), and took between one and two hours.[9]

Decision task In our baseline experimental procedure (*standard assignment*), the interviewer presented two items with roughly equal value to the participant halfway through the survey and then handed over one of the two items, randomly determined by the survey software. We refer to this as the assigned item and to the other (not-assigned) item as the alternative item. At the end of the survey, the interviewer showed the alternative item again and asked the participant whether he or she wanted to trade the assigned item for the alternative item.[10] After recording the decisions and completing trades (if participants decided to trade), participants were asked a few questions related to the exchange experiment. Note that transaction costs were near-zero in our setting since participants had to answer the trading question in any case and interviewers immediately completed trades (if desired by participants).

We follow the laboratory literature, most notably Plott and Zeiler (2007), and consider several variants on the baseline procedure described above. First, we varied the method of item assignment. Specifically, we either implemented the randomization of items directly through the electronic survey devices (*standard assignment*) or randomized items in front of participants (*lottery assignment*), i.e., either through a coin-flip or by par-

[9]Priority was given to surveying the household head or the participant in prior survey rounds; when that person was unavailable, the spouse or another adult permanent member of the household was surveyed instead. Fourteen percent of households have two different participants in the data; less than one percent have three. We track the participant ID and use it to examine within-subject variation in decision-making over time.

[10]See Appendix A.4 for the exact wording of all procedures.

ticipants drawing a button out of a bag.[11] The main goal of the *lottery assignment* is to minimize the risk of possible inference about the relative valuation of items or signaling by the experimenter associated with the *standard assignment*.[12]

Second, we implemented three sub-procedures designed to reduce participants' attachment to the assigned item: (i) we shortened the time between the assignment of items and the trading decision, with some participants receiving the assignment only minutes before the trading opportunity (*timing* procedure), (ii) we used vouchers redeemable for the specific item, rather than handing over the item itself (*voucher* procedure), and (iii) we directly manipulated participants' expectations regarding subsequent trading by informing them that they would have an opportunity to trade at the end of the survey (*expectations* procedure). Third, to address possible experimenter demand effects and concerns that study participants would perceive trading as impolite or as causing inconvenience for surveyors (Mutunda, 2006), we varied the wording when presented with the trading opportunity (*wording* procedure). Rather than offering the opportunity to trade, participants were asked to trade the item as an implicit favor to interviewers ("would you be willing..").[13]

Our default item pair, implemented across all survey rounds and all procedures consisted of a package (250g) of washing powder ("Boom") and a package (500g) of table salt (Boom – Salt). Both items are household staples with a local price of 3-3.5 Kwacha (USD 0.50-0.58), which corresponded to one-fifth of a typical daily wage at the time of the experiment. We varied the item pairs to test robustness to alternative items. First, we provided cash (3.5 Kwacha) as an alternative to Boom (Boom – Cash). Second, we offered durable goods (a mug and a serving spoon; Cup – Spoon). We refer to these three item pairs as "standard value" pairs. To assess the relationship between the value of the items and trading decisions, we also gave a subsample of participants the choice between a solar lamp and 80 Kwacha in cash (Solar – Cash), which corresponds to over 20 times the value

[11] We switched from the coin flip to the button roughly 20 percent of the way through round 1 to reduce ambiguity around the outcome.

[12] For example, if the randomization is not transparent, participants might incorrectly infer that the assigned item is more valuable than the alternative item, or they may perceive the assigned item as a gift from the interviewer.

[13] This idea is similar to a recently proposed approach to bound experimenter demand by De Quidt et al. (2018), which deliberately introduces demand effects to measure their impact on experimental outcomes.

of the standard value pairs. For all item-pair variants, we randomly selected households in each round for a *choice* condition, where they could simply pick their preferred item at the end of the interview. This allows us to measure item- and season-specific preferences for all item pairs. Appendix Table A.1 summarizes all randomly assigned experimental features, and the number of observations in each, by survey round.

Implementation and randomization To leverage variation in households' financial resources, we conducted experiments over the complete 2014-2015 agricultural cycle. Specifically, we ran our exchange experiments after the 2014 harvest when resources were relatively abundant, during the hungry season 2015, when resources were scarce, and then again after the 2015 harvest. To distinguish effects driven by the external environment from learning and priming effects, we used a phase-in design that generated random variation in participant experience over the three survey rounds. Randomization of item pairs was done at the village level; randomization of specific experimental procedures was done at the household level.[14]

Experiment round 1 (harvest season 2014): The first round took place after harvest in 2014, and ran from July through September. We randomly selected 105 villages and 1,513 households, covering approximately 58 percent of the total study population, to participate in the exchange experiments. In experiment round 1, we used both the *standard* and *lottery assignment* for assigning the item and varied the item pair (Boom – Salt and Cup – Spoon). In addition, we assigned a small sub-sample (n=259, household level randomization) to the *choice* condition.

Experiment round 2 (hungry season 2015): The second round of exchange experiments took place during the hungry season, from late January to March 2015, with a random subset of approximately 8 households across all 175 study villages. In total, 1,367 households participated in the experiments, of which we assigned 143 households to the *choice* con-

[14]We used block randomization to assign households to procedures and villages to item pairs. Blocks were constructed based on the RCT loan treatment, previous round exchange experience, and previous round item pairs.

dition and the remaining households to the exchange experiment. About 40 percent of the households sampled in the second round of experiments also participated in round 1. Loans were disbursed in randomly selected villages as part of the RCT described in Fink et al. (2020) in January 2015, 2-8 weeks prior to the start of experiment round 2.

In experiment round 2, villages were assigned to the Boom – Salt or Boom – Cash item pair. Again, we randomly assigned households to the *standard assignment* or the *lottery assignment*, with a subset of each (n = 236) given the *wording* procedure described above. In addition, we elicited participants' hypothetical willingness to pay (WTP) or willingness to accept (WTA) in the Boom – Cash item pair after they made their decision (see Appendix A.2 for more details).[15]

Experiment round 3 (harvest season 2015): We conducted the third round of exchange experiments after the 2015 harvest between July and September 2015 with all households in the sample (N=2,962 households). We used the same item pairs as in round 2 and added the high-value Solar – Cash pair. In addition, we dropped the *standard assignment* and used only the *lottery assignment*, varying *timing*, *voucher* and *expectations* procedures at the household level. We implemented the high-value Solar – Cash item pair with 400 participants (33 of whom were in the *choice* condition) in 25 villages. The households in this treatment received the *lottery assignment*, with a sub-group also given the *timing* and *voucher* procedures (n=198). As in round 2, we also elicited WTP/WTA from households that were randomized to the Boom – Cash and Solar – Cash item pairs.

3 Empirical strategy

In this section, we describe our approach to estimation and our identifying assumptions. Given the random assignment of items, testing for exchange asymmetries is relatively

[15]We presented participants who either kept or traded for Boom a sequence of ascending hypothetical cash values, starting from a small increment above the value of cash in the item pair. Participants who kept or traded for cash were instead given a decreasing series of cash values. We assumed monotonic preferences and only elicited a unique switching point for each individual, which is a common procedure to avoid multiple switching in experiments with choice lists (e.g., Dohmen et al., 2010). We use the same procedure in the Solar – Cash item pair implemented in round 3.

straightforward: for any distribution of preferences, a null hypothesis of no exchange asymmetry predicts that, in expectation, 50 percent of the sample will receive their less preferred item and thus trade the assigned item for their preferred one. For any item pair, we can estimate the probability of trading and test whether the estimated probability $\hat{p}$ is statistically different from 0.5:[16]

$$\hat{p}(trade) - 0.5 = 0. \tag{1}$$

To test how scarcity relates to trading decisions, we take advantage of (i) cross sectional variation in scarcity, (ii) seasonal variation in scarcity, which coincides with the different survey rounds, and (iii) village-level variation in loan access during survey round 2. We estimate the following linear probability model for individual i in village v and round t to identify (i) and (ii):

$$p(trade)_{ivt} = \alpha + \beta R_t + \phi P_{it} + \gamma I_{ivt} + \rho N_{it} + X_i \delta + \varepsilon_{ivt} \tag{2}$$

where R_t are indicators for survey rounds 2 (hungry season) and 3 (2015 harvest) that capture seasonal differences in trading probabilities relative to the 2014 harvest period. P_{it} and I_{ivt} are vectors of indicator variables for the procedural and item pair variations, respectively. N_{it} indicates the number of prior rounds of experience with the exchange experiment, at the individual or household level. X_i is a vector of time invariant household and participant baseline characteristics, including gender, age, household composition, wealth, and harvest value. We test the relationship between trading and (i) cross sectional variation in scarcity using quintiles of a baseline asset measure included in X_i, and (ii) seasonal differences in scarcity using the survey round indicators, R_t. We cluster standard errors at the village level (v) throughout,[17] and include individual fixed effects in some analysis.

Next, we exploit village-level variation in loan access (iii) by estimating:

[16]In finite samples, the null will differ from 0.5 based on the share of the population that receives each item and preferences between the items. We take this into consideration in our analysis.

[17]The assignment of items pairs and hungry season loan access were both randomized at the village level, and many potential sources of correlated shocks arise at the village level.

$$p(trade)_{iv} = \alpha + \sum_{w=1}^{4} \beta_w LD_{w,iv} + \sigma_t + \xi_c + X_i\delta + \varepsilon_{iv} \tag{3}$$

where β_w captures the effect of loan dropoff (LD) w weeks before experiment round 2 (hungry season) in village v and week t, estimated relative to the control set of villages, who were never given access to the loans ($LD_w = 0$). We include survey-week fixed effects σ_t to absorb time-varying trading probabilities across the hungry season that are common for treatment and control households, and fine-scale geographic controls ξ_c, corresponding to agricultural camps, each of which contains several villages. As a result, the β_w coefficients can be interpreted as time-varying treatment effects identified off of treatment versus control villages within a small geographic area and a survey week. This analysis is restricted to experiment round 2.

While loan access is randomized, the variation in LD_w – the time, in two week intervals, between loan disbursement and data collection among treated households – is not random. The survey timing (two week interval σ_t) was determined largely by random assignment of villages to survey month, while the exact timing of loan delivery was left to the implementation team (within a 10-day window of delivery for all villages).[18] We show that variation in loan dropoff timing (LD_w) is balanced on observables in Appendix Table A.2, where we regress observables on indicators for time since loan dropoff, controlling for survey week and fine-scale geographic controls. F-statistics for a test that all $dropoff_w$ coefficients are jointly equal to zero is reported in the final column. All baseline controls are balanced across dropoff timing, with the exception of the number of children between 5 and 14 years of age. Coefficients on children 5-14 (relative to the control) show a non-monotonic pattern as the time since loan dropoff increases, with similar treatment coefficients in the first and last time bins.

While our main analysis of the relationship between scarcity and decision making consists of a single hypothesis with three different proxies for scarcity, Section 4.2 introduces results that depend on numerous different experimental treatments on the right hand side (P_{it} and I_{ivt} in equation 2, above). In supplemental analysis, we address po-

[18]Our results are robust to alternative specifications that use only the variation in the timing of the survey (not the timing of loan dropoff) or the randomly assigned survey month.

tential concerns about multiple hypothesis testing using the List et al. (2019) procedure, updated to accommodate controls and clustered standard errors (Steinmayr, 2020).[19]

To test the exogeneity of the experimental conditions, we regress household controls on indicators for the survey rounds, item pairs and experimental procedures, and report the results in Appendix Tables A.3, A.4 and A.5, respectively. The t-statistics in parentheses reflect the difference in means between each column and the base group. The randomly assigned item pairs and experimental procedures are balanced and show only three t-statistics above 1.96 out of 100 individual tests. The sample is also balanced across rounds, though the individual-level characteristics – participant gender and age – show some differential selection in the hungry season, while household characteristics remain balanced.

4 Results: Scarcity and exchange asymmetries

We begin by documenting average trading behavior in our sample. We then analyze how trading decisions vary with three sources of variation in scarcity, imposing increasingly strict (exogeneity) requirements on the source of variation. Finally, we examine robustness to a variety of alternative explanations that have the potential to explain the relationship between scarcity and trading decisions.

4.1 Average trading behavior

Table 1 provides an overview of participant decisions by item pair. The first column presents the results from the *choice* condition, which provides a first indication of participants' relative preferences for the experimental items.[20] Participants had the most imbalanced preferences in the Cup–Spoon treatment, with three quarters of participants

[19] We thank Andreas Steinmayr for advice on the implementation of his mhtreg package.

[20] Aggregate preferences in the choice condition are not necessarily informative of the average strength of individual preferences. In other words, we could observe 50 percent of participants choosing each item, but all choices reflecting strong preferences for one item over the other, or – conversely – could observe a very small fraction of participants choosing one of the items even if all participants were near indifferent. The marginal loss of a foregone trade thus depends more on the steepness of the demand and supply functions around the equilibrium than on the location of the equilibrium.

preferring a cup over a spoon (despite similar market value). Preferences were more balanced on average for the other two standard-value item pairs. For each item pair, we also tabulate the number of participants starting and ending with each item. As first evidence of exchange asymmetries, we see that a majority of participants leave our experiment with the item they are randomly assigned; even for the most inferior item (Spoon), we see that around 50 percent of participants assigned a Spoon choose to keep it, while only 25 percent selected it in the choice condition.

The table displays the probability that participants traded the item they were assigned along with a t-test for the theoretical trading prediction in the last column. Given that we randomize items in each item pair, we expect that half of the participants are assigned their less preferred item and thus should trade for their preferred item, resulting in a average trading rate of 50 percent in each item pair. In our finite sample, the actual share of participants randomly assigned the first item in the item pair was 0.51, 0.54, 0.55 and 0.44 in the Boom – Salt, Boom – Cash, Cup – Spoon and Solar – Cash pairs, respectively. Based on preferences measured in the choice experiment, this implies that 0.50, 0.49, 0.47 and 0.49 should have traded in the Boom – Salt, Boom – Cash, Cup – Spoon and Solar – Cash pairs, respectively. We therefore report the adjusted null and the associated p-value in the last column of Table 1.

In all item pairs, the observed trading probability was significantly below the null. The overall likelihood that a participant traded the item that they were assigned is 0.35, similar to the pooled results in other field studies (e.g., List, 2003, 2004). We reject the null hypothesis of $p(trade) = 0.5$ as well as the overall adjusted null with p-values < 0.0001.

4.2 Scarcity and trading behavior

We organize our results around the three sources of variation in financial resource constraints. For each source of variation, we first show how our measure of scarcity relates to consumption or food availability, which represent choice variables that should (endogenously) respond to financial constraints.[21] We then test how the three scarcity measures

[21]Measures of food consumption could be considered as "first stage" or key mediator of scarcity in this setting. In practice, scarcity is likely to affect multiple domains of household well-being beyond nutritional

relate to trading decisions.

Cross sectional variation in wealth As a first indication of the correlation between scarcity and decision-making, we examine cross sectional heterogeneity in asset ownership at baseline. As shown in Figure 1, asset ownership is directly linked to consumption, with wealthier households eating significantly more meals during the hungry season. Next, we plot the baseline ownership of durable goods as a proxy for wealth against the average probability of trading, controlling for the item pair, experimental procedures, experience with the trading decision and household and individual controls (following equation (2)), in Figure 2. The negative gradient indicates more trading by poorer households, i.e., scarcity is associated with higher trading probabilities, though the confidence intervals are large (p-value on the slope is 0.102). Since numerous other factors correlated with wealth may affect trading behavior, we turn to more plausibly exogenous sources of variation in participants' financial resources.

Seasonal variation in wealth and income As described above, pronounced seasonality in income, savings and consumption is a salient feature of the study setting, and thus provides a natural source of variation that we use to analyze how scarcity shapes trading asymmetries. The second round of our experiment coincided with the hungry season (January to March), while the other two rounds took place in times of relative abundance, immediately following harvest (July to September). In our sample, the mean cash savings during the hungry season is around 200 Kwacha (median is 0), or 33 USD, while the mean cash savings at harvest is over 600 Kwacha (median is 120). The share of households in our sample reporting food shortages increases from less than 10 percent around harvest time to over 60 percent in the hungry season (Figure 1).[22] We exploit this variation in seasonal resource availability and compare trading decisions during the hungry season with decisions in two harvest seasons, conditional on random variation in participant experience with the trading decision (following equation (2)).

intake, such as farm investment, school enrollment, and medical expenditures, which may all simultaneously affect patterns of decision-making.

[22]Questions about meals per day were not administered at harvest time. We therefore cannot analyze the variation in meals per day consistently across all panels in Figure 1.

Figure 3 shows the estimated marginal effect of the season on trading decisions, based on the regressions shown in Table 2. As shown in Figure 3, around 30 percent of participants make trades in the 2014 harvest season. During the hungry season, the likelihood of trading increases by between 9 and 12 percentage points relative to the 2014 harvest (Table 2). The point estimate is largest in columns 3 and 5, which include individual fixed effects and limit the sample to inexperienced participants, respectively.[23] Importantly, the effect is specific to the hungry season: the trading probability in the following harvest season is insignificantly different from that in the first harvest season (columns 2-5) and significantly below the hungry season coefficients in most columns, implying a higher probability of trading when resources are more scarce. At the risk of over-interpreting the data, we note that the slightly higher trading rates in the 2015 harvest season are consistent with a much weaker harvest in 2015 than 2014 (see Fink, Jack, and Masiye, 2020, for details).

Finally, it is important to highlight that the observed variation in trading behavior by season does not simply reflect seasonal differences in preferences. Data from the *choice* condition for Boom – Salt, used in all three rounds, shows that preferences for the two items do not vary much by season. While Boom seems to be slightly more attractive in the hungry season (i.e., 65 percent of participants chose Boom over Salt) than in the harvest season 2014 (60 percent) or 2015 (57 percent), these differences are far from statistical significance (Fisher's exact equal means test p-value > 0.3).[24]

Random variation in liquidity While the seasonal variation in trading asymmetries is suggestive of a causal effect of scarcity on trading behavior, other factors may vary across seasons and influence trading decisions (see also our discussion of alternative explanations, below). To address these endogeneity concerns, we leverage a third source of vari-

[23]Previous evidence suggests that trading experience attenuates or eliminates the endowment effect (see e.g., List, 2003; Engelmann and Hollard, 2010). Using random variation in rounds of experience with the trading decision, we can show that trading experience is unrelated to trading decisions (see Appendix Table A.6 and Appendix Figure A.1). Note, however, that the variation in our setting is different from the prior literature, which analyzes accumulated experience over a longer time period and with higher frequency. This suggests that the intensity of experience may be important for overcoming the endowment effect.

[24]We observe a similar pattern for the Boom – Cash item pair. In the hungry season, 67 percent of farmers choose Boom over cash in the *choice* condition. In the harvest season (2015), 65 percent of participants choose Boom over cash.

ation in liquidity, associated with access to hungry season consumption loans. The larger RCT, in which we embedded the exchange experiments, relaxed liquidity constraints in 80 randomly selected villages during the hungry season by providing selected households with 200 Kwacha (around 33 USD) in cash or maize. We compare trading probabilities for households with and without access to the loans. Loans were delivered in early to mid January 2015, while the exchange experiments began in early February, about two weeks later. Figure 1 shows that the biggest effect on consumption occurred in the weeks following receipt of the loan. Figure 4 plots the effect of the loan on trading decisions, allowing the effect to vary with how recently it was received (in 2-week bins), following equation (3). The pattern is striking, though standard errors are large: among households surveyed two to three weeks after receiving a loan, the likelihood that a participant trades her assigned item is over 18 percentage points lower than in control group households surveyed in the same week or located in the same geographic area. This effect wears off with time since loan delivery, paralleling the pattern of effects on consumption. Table 3 shows loan treatment effects, conditioned on different sets of control variables. The reduction in exchange asymmetries is short lived, but large in magnitude, with a greater likelihood of trading under conditions of scarcity.

Alternative explanations The relationship between scarcity and decision-making shows similar patterns across three very different sources of variation in financial resources, which we take as evidence of more rational decision-making when resources are more scarce. Note that any alternative explanation would therefore have to also vary along these three dimensions (cross sectional, seasonal and loan access) to fully account for systematically more rational decision-making under scarcity. In the following, we use both design-based and natural variation in our setting to show that such alternative explanations are unlikely to explain our results.

Prior work has suggested that exchange asymmetries may be an artifact of experimental procedures that lower trading probabilities (see, for example, Plott and Zeiler 2007). We first examine the impact of a range of experimental features including the items involved, the assignment method (*lottery vs. standard assignment*), the duration of the initial assignment (*timing* procedure), and the physical possession of the item (*voucher* procedure) on trading decisions. We find that these experimental variations had no effect on average trading probabilities (see Appendix Tables A.6 and A.7).[25] We take the fact that trading decisions are not sensitive to variation in experimental procedures both as evidence for the robustness of exchange asymmetries in our setting and as evidence against the idea that variation in scarcity may be correlated with other experimental features that could explain our main results.

Next, we investigate the possibility that participants refuse to trade their assigned item because of social norms or experimenter demand effects, which may respond to or be correlated with variation in scarcity. To identify potential social norms and experimenter demand effects, we introduce a script that explicitly asks the participant to trade their assigned item (*wording* procedure). As shown in Appendix Table A.6, this script, which makes trading the more socially acceptable decision, had no measurable effect on trading decisions. As an additional test of social desirability bias, we use an adapted version of the Marlow-Crowne scale from social psychology (Marlow and Crowne, 1961) to show that those who behave or wish to be perceived as behaving in a more socially appropriate way are no less likely to trade (see Appendix Table A.9). The lack of sensitivity to experimental procedures also suggests that participants care about which item they end up with; if they were indifferent, we would expect the variation in trading frictions associated with the experimental variations such as the *timing*, *voucher*, or *wording* procedures (the latter of which places the cost on the decision to not trade) to lead to changes in decision-making. This is not case, implying that indifference to the outcome does not explain the large average exchange asymmetry that we observe.

Other details of implementation may also contribute to the trading decisions, though none are associated with all three sources of variation in scarcity. For example, differences in survey implementation could contribute to the variation in decision-making,

[25] Since we examine the effect of 10 experimental manipulations on the same outcome dataset, we also show p-values correcting for multiple hypothesis testing in Appendix Table A.8. The table also includes corrections for our analysis of the relationship between scarcity and trading decisions and for analysis of mechanisms, discussed in Section 5.1. These tests are variants of the same primary scarcity hypothesis using different proxies for scarcity, and are more correlated with each other than are the large number of randomly assigned procedural manipulations included in the analysis of alternative explanations.

for example, if shorter survey length in the hungry season reduces cognitive load and leads to better decisions. We use time stamps recorded by the survey software to test this directly and see no relationship between survey duration and trading behavior; as described above, experimental variation in the timing between the initial assignment and the trading decision also has no measurable effect. In addition, we examine whether differences in participant characteristics across survey rounds contribute to seasonal differences in trading probabilities using the correlation between individual characteristics and trading (see Appendix Table A.10). Only around 0.2 percentage points – out of the 9-12 percentage point difference between the 2014 harvest and the hungry season – can be explained by individual characteristics.

Finally, we consider the possibility that scarcity is correlated with market access. This may be the case if poorer individuals are less likely to visit markets for other reasons or, conversely, more likely to engage in barter exchange. Market access depends both on proximity to markets and on availability of barter exchange. In our analysis, we rely on observable variation in proximity and local trading opportunities as proxies for market access. As shown in Appendix Table A.11, we see no effect of village size, proximity to markets or paved roads, or of living in a village where more of the other households participated in the trading decisions.

5 Mechanisms

Our findings consistently point to more rational behavior when resources are more scarce. In this section, we explore several possible mechanisms behind this finding, including tunneling and the relative utility value of the items involved in the exchange experiment.

5.1 Tunneling and cognitive performance

We start by considering the most directly relevant set of theories surrounding scarcity and decision-making (Mullainathan and Shafir, 2013), which suggest that tunneling behavior could explain the higher probability of exchange when resources are more scarce. As Shah et al. (2012) write (p. 684): "cognitive load arises because people are more engaged with problems where scarcity is salient. This consumes attentional resources and leaves less for elsewhere." In our setting, if scarcity increases trading probabilities due to tunneling – i.e., participants engage more with basic trading decisions when scarcity is salient – we expect a corresponding decline in cognitive performance due to increased cognitive load. Some papers in this literature have associated a decline in cognitive performance with scarcity (e.g., Mani et al., 2013) though others have failed to replicate this relationship (e.g., Carvalho et al., 2016).

Following Mani et al. (2013), we administered two tests used to measure cognitive and executive function to a randomly selected subsample of participants in each survey round: Raven's Progressive Matrices (RPM) and a numerical version of the Stroop test (see Appendix A.3 for further details).[26] Both tests were unincentivized and conducted prior to the final trading decision. The RPM consists of a series of pictures with geometric shapes where participants choose the missing shape from a set of alternatives. For the Stroop test, we use a modified version in which individuals had to identify the number of displayed digits. In the congruent task, the displayed digits matched the respective counts (e.g., 22 or 4444); in the incongruent task, counts and digits were misaligned (e.g., 44 or 2222). To keep our sample size consistent in this analysis, we restrict our sample to participants who completed both tests and also made trading decisions (N=4,050).

We begin by testing whether cognitive ability declines under conditions of scarcity. We leverage the same three scarcity measures used in Section 4.2. Figure 5 summarizes the results (see also Appendix Table A.12 for the underlying regression results). All outcomes are normalized to a mean of zero and a standard deviation of one, with a higher score corresponding to better performance. The top panel shows that wealthier households have significantly higher scores on the RPM test and on the main two Stroop measures (tasks 2 and 3), confirming established cross sectional relationships between these measures and other proxies for human capital, such as educational attainment (Laajaj and Macours, 2019). The middle panel shows a modest improvement in performance on the

[26] According to the taxonomy provided in Dean et al. (2017), the Raven's test offers a measure of fluid intelligence while the Stroop test is a measure of inhibitory control or executive function.

Stroop tests during the hungry season, but no such relationship in the RPM score. The bottom figure shows effects of loan drop off, relative to the control group and conditional on survey-week and geographic fixed effects. Here we see even less consistent patterns: the RPM scores are lowest immediately following loan access and converge to zero consistently over time. The Stroop test scores, on the other hand, appear to decline with time since loan dropoff for up to 6-7 weeks, then improve significantly.[27] The finding that cognitive performance does not systematically decline with scarcity is in contrast to the findings in Mani et al. (2013), which uses variation before and after arbitrarily staggered harvest dates, and relies on a subsample surveyed only after harvest to implement a separate test for learning.[28]

Finally, we test whether trading decisions are correlated with cognitive performance. Table 4 shows no significant relationship in pooled regressions of trading decisions on each of the cognitive scores. Together, these findings are inconsistent with the predictions of existing theory that explains improvements in decision-making under scarcity as focus on immediate tradeoff tasks as attentional focus that comes at the expense of cognitive load (Mullainathan and Shafir, 2013; Shah et al., 2012).

5.2 Value of traded items

As long as utility functions are concave in consumption, the same goods have higher utility value when resources are more scarce. Decisions involving items of higher value may be processed differently than similar decisions involving lower value items for several reasons, which we discuss below. To assess changes in trading behavior with higher value items, holding scarcity constant, we introduced a high value item pair (Solar – Cash) in

[27] Given the substantial income transfer associated with the high value item pair (Solar – Cash), it might also be the case that the income effect leads to a decrease in tunneling behavior and better cognitive performance. For half of participants (N=149) in the Solar – Cash item pair and the cognitive test sample, the solar lamp or 80 Kwacha in cash were assigned before the cognitive tests were administered (for those in the Timing or Timing+Voucher procedures, the assignment occurred after the tests were administered). We test whether these participants have better cognitive performance than those who received a lower valued assignment (Boom – Salt or Boom – Cash; N = 1,479) prior to the cognitive tests. They do not.

[28] As described in further detail in Appendix A.3, we use a different coding of cognitive scores than Mani et al. (2013), but our results are robust to using their coding, as shown in Appendix Table A.13. They observe that experience improves performance in one out of the three cognitive test outcomes they measure, but do not adjust for this in their main results, while we can directly control for test experience.

the last round of our experiments. Specifically, we offered participants the choice between a solar lamp or an equivalent value in cash (80 Kwacha or USD 13), 23 times higher than the values in our standard item pairs. Table 1 shows that 44 percent of participants trade in this condition, which is only marginally below the adjusted null of a 49 percent trading probability ($p = 0.14$).

For additional insight into trading decisions with high value items, we estimate a specification with the likelihood of ending up with the assigned item as the outcome relative to the probability of choosing that item in the choice condition. We present the results in Table 5, where the constant (estimated without controls) represents the mean in the choice condition (columns 1 and 2). Participants assigned a solar lamp were no more likely than participants in the choice condition to end with a solar lamp (column 1), while participants assigned cash were 14 percentage points more likely to end with cash than in the choice condition (column 2, $p = 0.12$). Column 3 shows our standard empirical specification (equation 2), restricted to round 3. Relative to the Boom – Salt and Boom – Cash item pairs, assignment to the Solar – Cash item pair increases the probability of trading by 8.4 percentage points. This manipulation holds scarcity constant and increases the value of the items, whereas our results in Section 4.2 hold the value of the items constant and increase scarcity. Both lead to a reduction in exchange asymmetries, suggesting that higher value items – relative to available financial resources – result in decisions closer to the normative benchmark.

Why do exchange asymmetries diminish when decisions involve items of higher (utility) value? We discuss two plausible explanations for this finding.[29] First, decisions involving higher value items may receive greater attention, due to a basic decision heuristic, salience or the cost of a decision error.[30] Perhaps most intuitively, decisions involving high value items are likely accompanied by higher decision stakes, i.e., larger utility losses associated with making the wrong decision. Even though higher value item pairs do not

[29] We thank John List for illuminating conversations that helped clarify the potential underlying channels.

[30] For example, item value may affect the salience of certain attributes of the items, which are overweighted in the decision process. Consider, for example, an individual who makes two choices, one between a red and a blue car and one between a red and a blue mug. Her potential utility loss in these decisions may be similar. However, individuals pay greater attention to color in the car decision relative to the mug decision (see e.g., Bordalo et al., 2012, 2013).

necessarily imply higher stakes if market values are similar and trades are common, undoing a trading decision (acquiring or trading for the other item) may be more difficult for more expensive items, and subjective valuations may diverge more from the market value of an item as the value of that item increases. This latter reason can be seen in our data: in our sample, the difference between subjective valuations and the official market price is 70 times higher in the Solar – Cash item pair than in the Boom – Cash item pair (see Appendix A.2).[31]

According to models of rational inattention (e.g., Sims, 2003; Maćkowiak et al., 2021), higher stakes decisions will (rationally) receive additional attention if attention reduces decision errors.[32] This would imply additional attention under scarcity if scarcity also increases stakes. While any mistake in the trading decision can be undone through a future purchase or trade, more constrained households may lack the liquidity to do so immediately and the financial loss associated with undoing a decision error will cause higher disutility if consumption levels are low (and utility functions are concave). In this sense, when resources are more abundant, decision stakes will tend to be lower and individuals may be closer to indifferent to the outcome of the specific trading decision. We find additional evidence that decision stakes matter by testing how the stock of items in the home affect trading decisions. When households are out of the assigned item, they are less likely to trade; when out of the alternative item, they are more likely to trade (see column 2 of Appendix Table A.14).[33]

Second, higher value decisions may affect preferences directly if, for example, individuals perceive outcomes relative to some reference point that itself depends on item value. We have taken exchange asymmetries as given to examine whether scarcity moves decisions closer to the normative benchmark, while questions about the exact formula-

[31] At the 25th, 50th and 75th percentiles, the value difference is, respectively, 100 times, 37.5 times and 44 times higher in the Solar – Cash item pair than in the Boom – Cash item pair.

[32] A recent paper by Enke et al. (2021) shows in a laboratory setting that higher stakes leads to greater attention but not to better decisions. Relative to the decisions they study, the trading decisions we implement are straightforward and potentially improved with attention.

[33] Of course, the stock of items in the home is not randomly assigned and depends on preferences, among other things. However, given the random assignment of the item, variation in preferences should be orthogonal to demand for the assigned item relative to the alternative item. We also note that the stock of the items in the home is predictive of choices in the choice condition.

tion of reference points remain an active area of research (e.g., O'Donoghue and Sprenger, 2018) that are beyond the scope of this paper. That said, some of our findings appear inconsistent with expectation-based reference points. Manipulating expectations directly by informing participants about the trading opportunity at the end of the survey or indirectly via variation in experience with the trading decision (*expectations* procedure or experience with the exchange experiment) has no impact on trading decisions (see Appendix Table A.6 and Appendix Figure A.1).

On balance, the evidence presented in this section rules out tunneling as the driver of more rational decision-making under scarcity in our setting, and is better aligned with models of rational inattention. In this case, greater attentional investment when stakes are higher appears to decrease susceptibility to the frames, defaults or references points that drive exchange asymmetries.

6 Discussion

The broader implications of our findings depend on both the welfare consequences and the generalizability of the behavior that we observe. We address these issues in turn.

6.1 Welfare implications

The results presented in this paper naturally raise questions about the welfare cost of foregone trades, as well as the benefits of more rational decision-making under scarcity. Quantifying welfare is challenging in a setting where preferences are inherently unstable, and willingness to pay or accept is affected by endowments (e.g., Bernheim and Rangel, 2009).

A rational inattention interpretation of trading decisions rests on the assumption that attention is costly and that rational individuals will allocate attention to a decision only if the expected utility gain exceeds the attentional cost. This would imply that the welfare loss due to the endowment effect is near zero, since the foregone gains from trade are offset by less (costly) attention allocated to the decision.

To calibrate the potential welfare losses associated with foregone trades, ignoring attention-related disutility, we rely on participants' stated ex post valuations. We collected stated valuation data after trades involving cash in rounds 2 and 3 of the experiments only (see Appendix A.2 for further discussion of elicitation and data quality). While these data are hypothetical in nature and measured after trades had been implemented, they show – consistent with our average trading result – that households randomly assigned Boom display a 1.75 Kwacha higher average valuation of the item than households assigned cash (see Appendix Table A.15), suggesting a large quantitative effect of endowments on valuations. Since we cannot observe which individuals' valuations or trading decisions are affected by the endowment, and therefore cannot calculate the loss from the exchange asymmetry directly, we instead use the stated difference between individuals' subjective valuation of Boom and the trading price (cash value in the item pair), which we refer to as the value differential, to calibrate foregone gains from trade, and how they are affected by scarcity.

In the Boom – Cash item pair, we first consider the 14 percent of non-traders with the smallest stated value differentials as a conservative estimate of the losses from foregone trades. On average, the implied loss per trading decision is 0.03 Kwacha, or 0.42 Kwacha in a population of 100 individuals.[34] Alternatively, a slightly less restrictive calculation uses the average surplus generated through trading, which is the average of the stated value differential across traders. In this sample, the gains from trade equal 0.36 Kwacha per trader. Multiplying this average gain from trade by the 14 percentage point lower trading probability in the Boom – Cash item pair suggests a 5.04 Kwacha loss in a population of 100 individuals.[35] We can also use these stated values in a rough calculation of the gains from trade associated with scarcity (based on hungry season trading in Table 2) in a population of 100 individuals. Using the valuations among non-traders yields gains between 0.09 to 0.37 Kwacha, depending on the item pair and season, while

[34]This calculation imposes several assumptions, including that the endowment effect on valuations is symmetrical, i.e., that participants who do not trade would have had the same ex ante preference for their (ex post) less preferred item as their ex post preference for the more preferred item, and that the rank order of valuation differentials in the population determines who trades.

[35]This calculation assumes that the valuations among traders can be extrapolated to non-traders should they have chosen to trade. On average, in our stated preference data, the stated valuation differential among non traders is higher than among traders: 0.92 vs 0.36 Kwacha.

using valuations among traders instead yields gains between 1.12 to 4.43 Kwacha.

Relative to the local daily wage of around 15 Kwacha, the average loss associated with a single trading decision is small, though recall that the median household has zero Kwacha in savings during the hungry season. In addition, households make many such transactions over the course of a year, and losses can add up, though to a potentially lesser degree when resources are scarce. This makes sense through a lens of rational inattention: holding attentional costs fixed, the welfare losses associated with a small change in consumption are larger when resources are more scarce. Of course, a rational inattention mechanism would also mean that these welfare losses are zero, implying that this calibration offers an upper bound on the true losses. This calibration highlights that a high rate of behavioral anomalies need not imply large welfare losses if the anomaly is attenuated as the cost of mistakes increases.

6.2 External validity

Our main finding is that scarcity is associated with reduced exchange asymmetries among farmers in rural Zambia. Like most field experiments, our study was designed to ensure internal validity. Drawing on List (2020), we discuss two dimensions of the external validity of this finding: (1) generalizability to other populations and contexts and (2) generalizability to other decisions.[36]

Our study population was designed to be representative of rural households in Chipata District, Zambia, with an initial random selection of households and very limited attrition over time. Chipata District is similar to many rural populations in sub-Saharan Africa, where small-scale farming is the primary source of income, and liquidity and consumption are highly seasonal. By combining this seasonal variation in scarcity with two very different sources of variation (baseline assets and randomized loan access), and documenting consistent relationships with exchange asymmetries across all three, we in-

[36] In the terminology of List's (2020) SANS conditions, (1) accounts for Selection and Attrition by considering how features of the study population affect external validity and (2) addresses Naturalness by discussing the decision problem we study. We are unconcerned about Scaling given that we emphasize theory testing rather than policy interventions (i.e., our study focuses on the boundaries of prior findings regarding scarcity and behavior and explores underlying mechanisms), such that general equilibrium effects or other challenges with scaling are unlikely to apply.

crease the likelihood that our results will hold across samples and contexts experiencing different dimensions of poverty. We also note that trading decisions vary little with observable household or individual characteristics (Appendix Table A.10), suggesting that results may generalize to populations with different average characteristics. Relative to most of the related literature, the generalizability of our results is improved by (a) studying multiple sources of variation in scarcity, (b) focusing on a population that shares primary characteristics (agricultural employment and low incomes) with rural households across low and middle income countries, and (c) studying a sample that is representative of an entire district.

Next, we consider the more challenging question of whether our scarcity results also extend to other trading scenarios or to other types of decisions. While we can only speculate on whether the relationship between scarcity and decision-making would generalize beyond immediate trading decisions to longer term or risky decisions, for example (see Lichand and Mani (2020) for evidence on the relationship between scarcity and risk), we note that trading decisions are among the most basic economic transactions. Even though the specific way that we elicit trading decisions may appear somewhat artificial, the behavioral patterns we uncover are insensitive to the particulars of how trades were elicited. Specifically, we show that trading decisions are remarkably robust to sometimes strong manipulations of the study procedures (see Appendix Table A.6). This suggests that behavior may be robust to other features of the decision environment, though we leave this to future research.

7 Conclusion

In a sample of Zambian farmers, we show that the propensity to trade familiar household items is about 15 percentage points lower than predicted by standard theory, providing new evidence on the relevance and robustness of the endowment effect outside of the laboratory. More strikingly, we show that trading decisions become significantly more rational when financial resources are more scarce and when the value of the items involved is exogenously increased. While more rational decision-making under scarcity is consistent with the idea that scarcity increases focus on essential choices (tunneling, e.g., Mullainathan and Shafir, 2013), our results are more consistent with greater attention to decisions involving higher stakes (Sims, 2003; Maćkowiak et al., 2021). In our experiment, an individual who does not trade Boom for salt, even though her household needs salt, must instead spend her own money to buy salt (plus any necessary travel cost, etc.). If her household is credit or liquidity constrained, this will affect other consumption. Scarcity increases the marginal utility of this lost consumption, and thus makes “mistakes” (foregone trading opportunities) more costly. Whether this scarcity arises from cross sectional differences in wealth, seasonal variation in liquidity or experimental variation in access to a loan, the result is the same: a higher utility cost from a foregone trading opportunity.

Like any empirical case study, our design and project implementation have limitations that open promising directions for future work. First, by focusing on a single measure of decision-making, we are unable to test whether more or less complex decisions are similarly impacted by scarcity. Comparing across different types of decisions under similar sources of variation in scarcity would offer more nuanced tests for different theories of decision-making. Second, the parallels between decisions under scarcity and decisions involving items of higher market value raise questions about why decisions become more rational when they involve higher value items. Rational inattention is one potential explanation that matches our evidence. However, our study was not designed to provide a direct test of rational inattention, and further investigation of channels has implications both for understanding how scarcity affects decision-making and the welfare cost of exchange asymmetries.

Despite the small short-term welfare loss associated with exchange asymmetries, reluctance to trade may have wide-ranging implications for markets in general, and for development in particular. Reluctance to give up existing or endowed assets, goods or acquired rights may at least partially explain (small) business owners or farmers foregoing profitable exchanges or investments (Kremer et al., 2013; Carney et al., 2018), individuals resisting policy changes (Alesina and Passarelli, 2019), and low rates of new technology adoption (Liu, 2013; Giné and Goldberg, 2017). The results we present in this paper suggest that such a reluctance is widespread and highest in times of relative abundance, a point in time when, for example, investments are most viable. Accordingly, opportunities to implement behavior change or to adopt new technologies may not only be population specific, but may also be strongly influenced by temporal and seasonal variation in scarcity. Recognition of this variation may introduce new ways to harness prevalent exchange asymmetries or design policies that help households avoid related biases.

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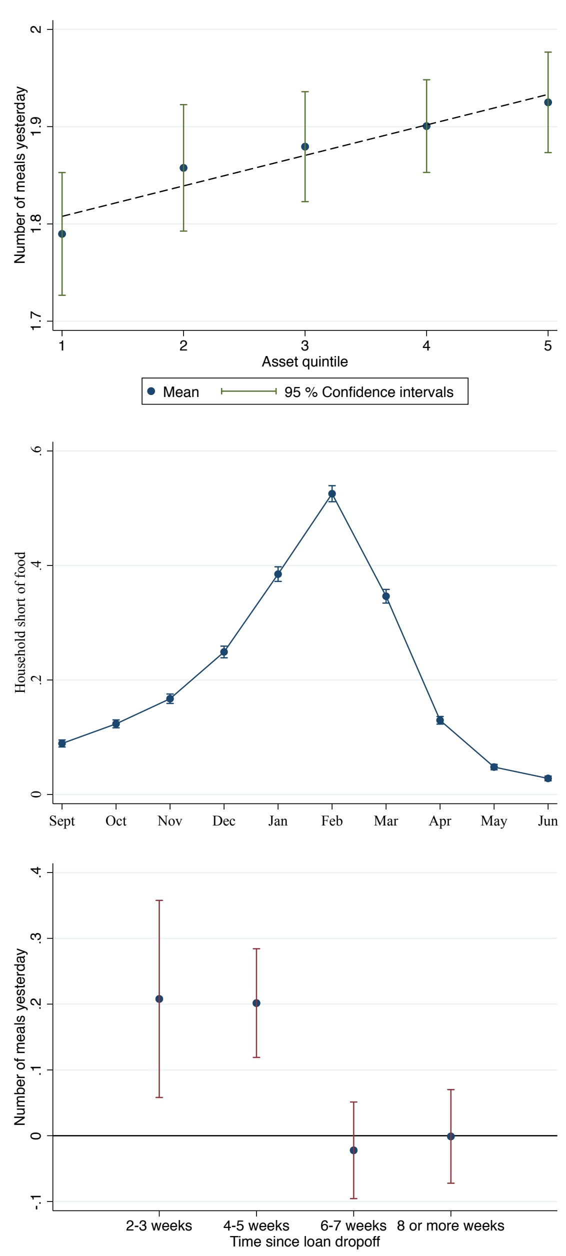

Figure 1: Consumption and food availability by source of variation in scarcity

Notes: The top figure uses baseline variation in assets and hungry season consumption. The middle figure uses variation across months (seasons), where the first and third survey rounds took place between July and September while the second survey round took place from January to March. The bottom figure uses time since loan drop off, following Figure 4, and hungry season consumption. 95% confidence intervals are based on standard errors clustered at the village level.

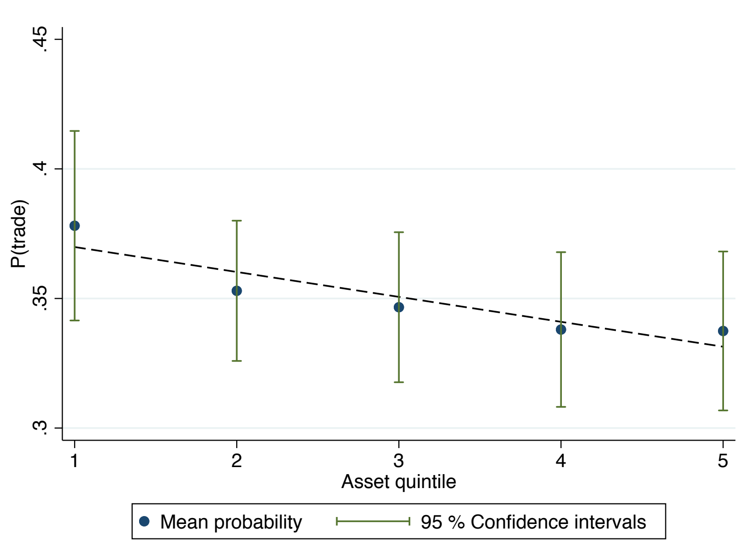

Figure 2: Probability of trading assigned item by baseline assets

Notes: Trading probability by quintile of the baseline household asset distribution (principle component analysis of durable good ownership), conditional on round, procedure, experience, item pair, and household and individual characteristics. 95% confidence intervals are based on standard errors clustered at the village level.

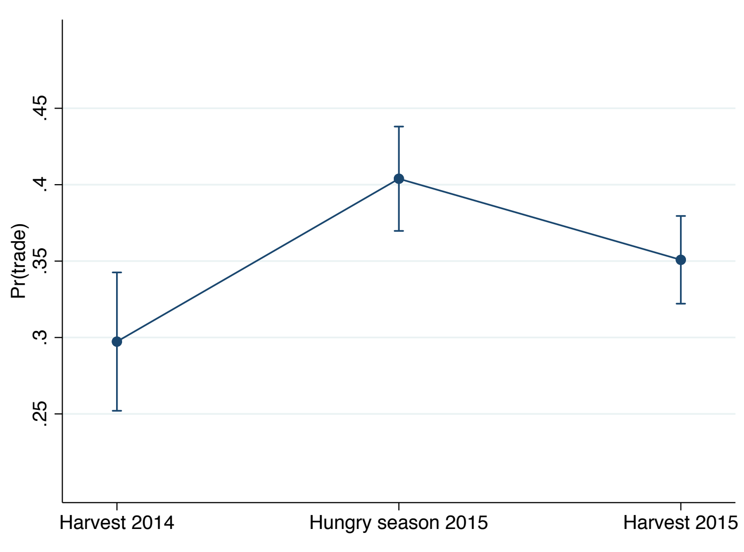

Figure 3: Probability of trading assigned item, by season

Notes: Trading probability by agricultural season, conditional on individual experience with the trading decision, procedure, item pair, and household and individual characteristics. 95% confidence intervals are based on standard errors clustered at the village level.

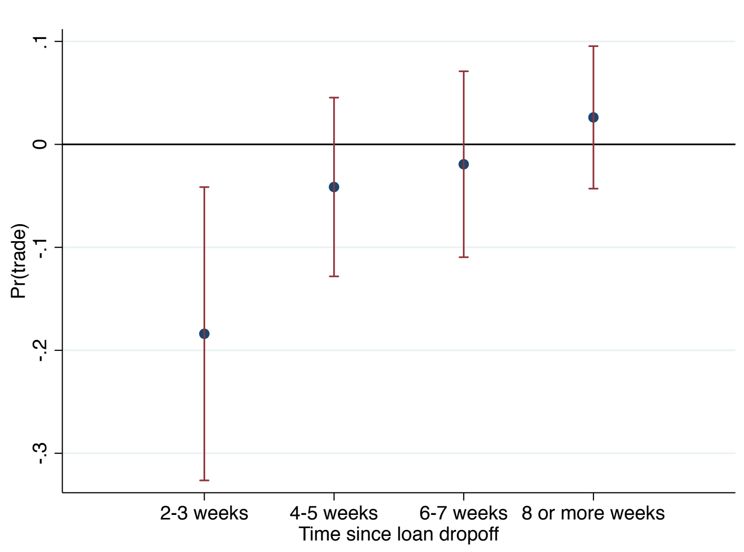

Figure 4: Relationship between weeks since loan receipt and trading probabilities

Notes: Effect of loan timing on trading probabilities, where time since loan dropoff is measured in weeks. The omitted category is the control (no loan) group. Results are conditional on week of survey and geographic fixed effects, and procedure, experience item pair, and household and individual characteristics. 95% confidence intervals are based on standard errors clustered at the village level.

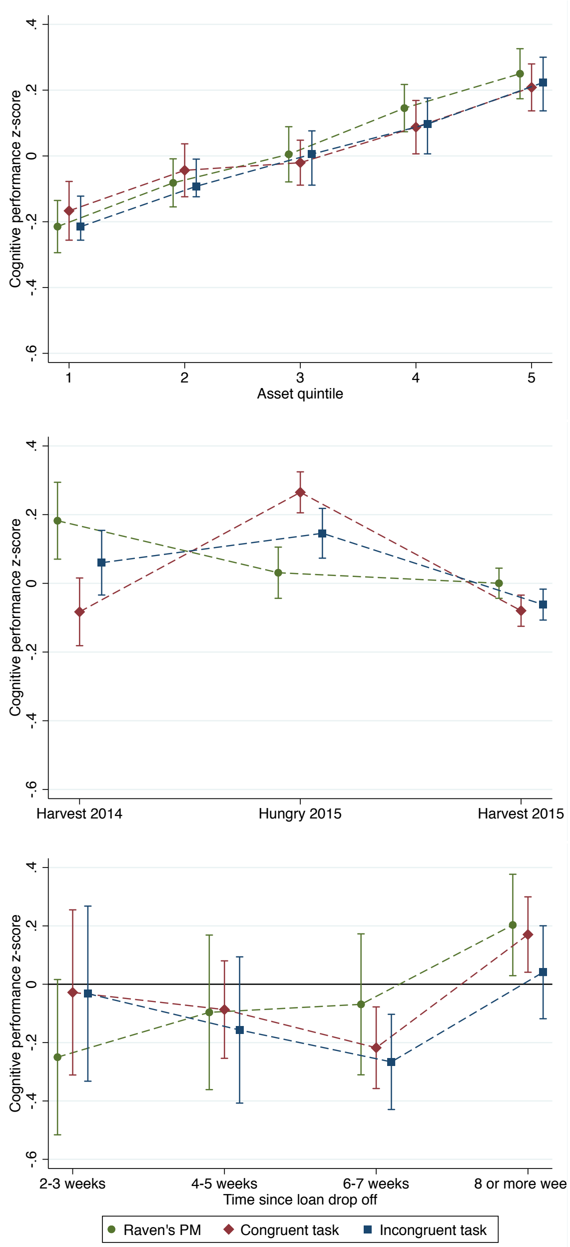

Figure 5: Relationship between scarcity and cognitive performance

Notes: Each sub-figure shows how performance on cognitive tasks (measured as z-scores) varies with a different source of variation in scarcity. The top figure uses baseline variation in assets. The middle figure uses variation across survey rounds (seasons). The bottom figure uses time since loan drop off, measured in two-week bins. The omitted category is the control (no loan) group and results are conditioned on week of survey and geographic fixed effects. All analyses control for round, experience with the cognitive tests, and household and individual characteristics. 95% confidence intervals are based on standard errors clustered at the village level.

Table 1: Descriptive statistics by item pair

<u>Boom-Salt</u>	N = 2766					
			End with		*Overall*	
Pr(chosen)		*Assigned*	Boom	Salt	Pr(trade)	0.34
Boom	0.60	Boom	934 (0.75)	315 (0.25)	p-val (H0=0.50)	0.00
Salt	0.40	Salt	514 (0.43)	669 (0.57)	p-val (H0=0.50)	0.00
<u>Boom-Cash</u>	N = 1962					
			End with		*Overall*	
Pr(chosen)		*Assigned*	Boom	Cash	Pr(trade)	0.36
Boom	0.66	Boom	701 (0.73)	260 (0.27)	p-val (H0=0.50)	0.00
Cash	0.34	Cash	385 (0.47)	431 (0.53)	p-val (H0=0.49)	0.00
<u>Cup-Spoon</u>	N = 714					
			End with		*Overall*	
Pr(chosen)		*Assigned*	Cup	Spoon	Pr(trade)	0.30
Cup	0.75	Cup	286 (0.87)	42 (0.13)	p-val (H0=0.50)	0.00
Spoon	0.25	Spoon	135 (0.50)	133 (0.50)	p-val (H0=0.47)	0.00
<u>Solar-Cash</u>	N = 400					
			End with		*Overall*	
Pr(chosen)		*Assigned*	Cash	Solar	Pr(trade)	0.44
Cash	0.45	Cash	97 (0.60)	66 (0.40)	p-val (H0=0.50)	0.08
Solar	0.55	Solar	96 (0.47)	108 (0.53)	p-val (H0=0.49)	0.14

Notes: Summary of outcomes by item pair. The Pr(chosen) tabulation shows the likelihood that each item in the pair was selected in the choice condition. Assigned and end with tabulates the frequency and probability by assigned item that participants started and ended with each item in the pair. The overall probability that a participant traded the item he or she was assigned is presented in the final column. P-values from tests of a null of 50 percent trading and an adjusted null, accounting for assignment probabilities and preferences revealed in the choice condition, are also reported in the final column (with standard errors clustered at the village level).

Table 2: Probability of trading assigned item, by season

	Pr(trade)				
	(1)	(2)	(3)	(4)	(5)
Hungry season	0.089***	0.107***	0.121**	0.112***	0.123***
	(0.022)	(0.031)	(0.056)	(0.035)	(0.031)
Harvest season 2015	0.066***	0.054	0.053	0.025	0.062
	(0.019)	(0.033)	(0.077)	(0.042)	(0.041)
Harvest 2014 mean pr(trade)	0.30	0.30	0.30	0.29	0.30
Hungry = Endline (p-val)	0.21	0.04	0.17	0.02	0.10
Controls	no	yes	yes	yes	yes
Fixed effects	none	none	individual	none	none
Sample	full	full	full	Boom-Salt	no experience
Observations	5,172	5,171	5,172	2,431	2,987

Notes: Linear regressions of an indicator for whether the subject traded the assigned item, by season. The omitted category is the 2014 harvest season. Columns that include controls condition analysis on round, procedure, experience, item pair, and household and individual characteristics. Individual fixed effects are included in column 3. Column 4 restricts the analysis to the Boom-Salt item pair only (used in all three rounds). Column 5 excludes households with past experience with the exchange experiment from each round. Standard errors clustered at the village level.

Table 3: Probability of trading assigned item, by loan delivery

	Pr(trade)			
	(1)	(2)	(3)	(4)
Loan	-0.011	-0.019	-0.185**	
	(0.029)	(0.029)	(0.090)	
Time since dropoff			0.055*	
			(0.029)	
Dropoff 2-3 weeks ago				-0.184**
				(0.087)
Dropoff 4-5 weeks ago				-0.041
				(0.053)
Dropoff 6-7 weeks ago				-0.019
				(0.055)
Dropoff 8 or more weeks ago				0.026
				(0.042)
No loan mean pr(trade)	0.39	0.39	0.39	0.39
Observations	1224	1224	1224	1224

Notes: Round 2 only. Linear regressions of an indicator for whether the participant traded the assigned item on loan treatment variables. Loan treatment equals one if the household was in a loan treatment village. Column 1 includes survey week and geographic controls only; other columns also include controls for procedure, experience, item pair, and household and individual characteristics. Column 3 includes the time since loan dropoff in two-week bins. Column 4 estimates coefficients on each of these bins. Standard errors clustered at the village level.

Table 4: Cognitive performance and probability of trading

	Pr(trade)		
Cognitive measure:	Raven's PM (1)	Stroop task 2 (2)	Stroop task 3 (3)
Cognitive measure	-0.005	-0.003	-0.004
	(0.008)	(0.009)	(0.009)
Mean pr(trade)	0.36	0.36	0.36
Observations	4,049	4,049	4,049

Notes: Linear regressions of an indicator for whether the participant traded the assigned item. All cognitive measures are normalized Z-scores where a higher score implies better performance. Regressions are restricted to a subsample of participants who completed both Raven's and Stroop tests. All regressions control for round, procedure, experience with both trading and the cognitive test, item pair, and household and individual characteristics. Standard errors clustered at the village level.

Table 5: Probability of trading assigned item, high value treatment

	Pr(End item: Solar) (1)	Pr(End item: Cash) (2)	Pr(trade) (3)
Assigned: Solar	-0.016		
	(0.093)		
Assigned: Cash		0.141	
		(0.087)	
Solar – Cash			0.083**
			(0.038)
Constant	0.545***	0.455***	0.351***
	(0.092)	(0.092)	(0.064)
Standard value mean pr(trade)			0.35
Controls	no	no	yes
Observations	237	196	2,693

Notes: Round 3 only. Columns 1 and 2 estimate the effect of assignment on the probability of ending with the assigned item, restricted to the high value treatment (Solar – Cash). To facilitate interpretation, the regressions do not include controls. The coefficients in columns 1 and 2 are the additional probability of ending up with the assigned item compared to the choice condition. The constant in the regression captures the probability of choosing the item in the choice condition. Column 3 estimates the effect of being assigned to the Solar – Cash item pair relative to the standard value (Boom-Salt and Boom-Cash) item pairs. Column 3 controls for procedure, experience, and household and individual characteristics. Standard errors clustered at the village level.

Appendix to

Poor and Rational: Decision-making under Scarcity

A.1 Tables and figures

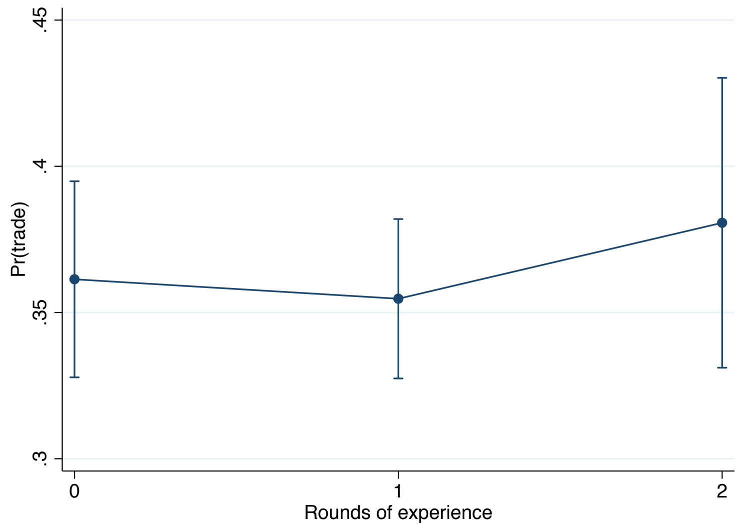

Figure A.1: Probability of trading start item in Round 3 (Harvest 2015), by rounds of participant experience

Notes: Relationship between subject experience with the trading decision and trading probabilities, conditional on season of survey. Analysis is restricted to the third round of data collection (Harvest 2015). Results are conditional on item pair and procedure indicators and individual and household controls. 95% confidence intervals are based on standard errors clustered at the village level.

Table A.1: Experimental setup: Scripts and procedures

Item pair	Procedure	Round 1: Harvest 2014	Round 2: Hungry Season	Round 3: Harvest 2015	Total
Boom vs. Salt					2766
	Free Choice	141	85	108	
	Assigned	416	318	0	
	Lottery	242	276	376	
	Timing	0	0	341	
	Voucher	0	0	359	
	Timing+Voucher	0	0	169	
	Expectations	0	0	273	
	Wording	0	124	0	
Boom vs. Cash					1962
	Free Choice	0	58	127	
	Assigned	0	302	0	
	Lottery	0	328	391	
	Timing	0	0	351	
	Voucher	0	0	354	
	Timing+Voucher	0	0	172	
	Expectations	0	0	223	
	Wording	0	112	0	
Cup vs. Spoon					714
	Free Choice	118	0	0	
	Assigned	345	0	0	
	Lottery	251	0	0	
	Timing	0	0	0	
	Voucher	0	0	0	
	Timing+Voucher	0	0	0	
	Expectations	0	0	0	
	Wording	0	0	0	
Solar vs. Cash					400
	Free Choice	0	0	33	
	Assigned	0	0	0	
	Lottery	0	0	169	
	Timing	0	0	198	
	Voucher	0	0	198	
	Timing+Voucher	0	0	198	
	Expectations	0	0	0	
	Wording	0	0	0	
Total		1513	1367	2962	5842

Notes: Number of households assigned to each item pair and experimental procedure, by survey round.

Table A.2: Balance: Time since loan dropoff

	Weeks since loan dropoff				
	2-3	4-5	6-7	8 or more	F-statistic
	(1)	(2)	(3)	(4)	(5)
Age of hh head	0.16	-0.30	-1.75	0.32	0.60
	(0.10)	(-0.21)	(-1.15)	(0.45)	
Female headed hh	-0.05	-0.01	-0.01	-0.00	0.37
	(-1.15)	(-0.15)	(-0.18)	(-0.21)	
Children under 5	-0.12	0.07	0.12	0.05	1.49
	(-0.99)	(1.00)	(1.19)	(1.49)	
Children 5-14	0.21	0.09	0.17	0.08	0.84
	(1.13)	(0.52)	(1.34)	(1.13)	
Adults 15-64	0.35	0.05	0.19	-0.02	1.69
	(1.81)	(0.41)	(1.44)	(-0.38)	
Adults over 65	0.02	-0.06	-0.05	-0.02	1.09
	(0.35)	(-1.57)	(-1.22)	(-1.04)	
Baseline assets	0.07	0.19	0.18	-0.03	0.99
	(0.36)	(1.21)	(0.87)	(-0.40)	
Baseline harvest value	497.59	-20.74	212.79	-57.80	0.94
	(1.10)	(-0.07)	(1.00)	(-0.42)	
Female respondent	-0.06	-0.02	-0.00	-0.01	0.27
	(-0.83)	(-0.46)	(-0.06)	(-0.69)	
Respondent age	-0.80	1.22	-1.70	0.72	1.13
	(-0.49)	(0.78)	(-1.13)	(1.06)	

Notes: Coefficients and t-statistics on indicators for weeks since loan dropoff, relative to the control group. Each row corresponds to a regression, with household and individual characteristics as the lefthand side variables, and standard errors clustered at the village level. All regressions control for survey week and geographic block. The F-statistic in column 5 is from a joint test that all coefficients are equal to zero.

Table A.3: Balance: Rounds

	Round 1 (1)	Round 2 (2)	Round 3 (3)
Age of hh head	42.71	-0.14	0.15
	[14.74]	(-0.32)	(0.45)
Female headed hh	0.24	0.02	0.01
	[0.43]	(1.35)	(1.54)
Children under 5	0.96	-0.04	-0.01
	[0.93]	(-1.78)	(-0.74)
Children 5-14	1.81	-0.03	-0.04
	[1.50]	(-0.83)	(-1.27)
Adults 15-64	2.45	0.03	0.01
	[1.25]	(0.90)	(0.28)
Adults over 65	0.17	-0.01	0.01
	[0.44]	(-0.48)	(0.93)
Baseline assets	3.00	0.03	0.02
	[1.42]	(0.72)	(0.54)
Baseline harvest value	3132.24	-36.05	-52.19
	[2802.57]	(-0.40)	(-0.67)
Female respondent	0.29	0.12	0.03
	[0.45]	(7.04)	(2.49)
Respondent age	44.07	-1.37	-0.09
	[14.84]	(-2.89)	(-0.26)

Notes: Means and standard deviations of baseline variables for the Round 1 sample shown in column 1. Columns 2-3 show mean differences across rounds, relative to round 1, for each variable, with t-statistics adjusted for clustering at the village level printed below in parentheses.

Table A.4: Balance: Item pairs

	Boom-Salt (1)	Boom-Cash (2)	Cup-Spoon (3)	Solar-Cash (4)
Age of hh head	42.69	0.48	-0.83	0.08
	[14.95]	(0.95)	(-1.13)	(0.08)
Female headed hh	0.25	0.01	-0.03	-0.01
	[0.43]	(0.99)	(-1.63)	(-0.61)
Children under 5	0.94	-0.03	0.07	0.03
	[0.90]	(-1.15)	(1.71)	(0.37)
Children 5-14	1.77	0.03	0.02	-0.00
	[1.51]	(0.56)	(0.30)	(-0.02)
Adults 15-64	2.43	0.06	0.06	0.12
	[1.23]	(1.54)	(1.13)	(1.77)
Adults over 65	0.17	0.01	-0.01	0.02
	[0.45]	(0.92)	(-0.57)	(0.83)
Baseline assets	3.05	-0.06	-0.09	-0.04
	[1.42]	(-1.03)	(-1.23)	(-0.32)
Baseline harvest value	3142.76	-140.74	45.85	-54.78
	[2803.15]	(-1.03)	(0.29)	(-0.25)
Female respondent	0.33	0.03	-0.06	-0.02
	[0.47]	(2.13)	(-2.67)	(-0.78)
Respondent age	43.55	0.46	-0.30	0.58
	[15.10]	(0.92)	(-0.42)	(0.65)

Notes: Means and standard deviations of baseline variables for the Boom-Salt item pair shown in column 1. Columns 2-4 show mean differences across item pairs, relative to the Boom-Salt pair, for each variable, with t-statistics adjusted for clustering at the village level printed below in parentheses.

Table A.5: Balance: Procedures

	Choice	Assigned	Lottery	Timing	Voucher	Timing + Voucher	Expectations	Wording
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age of hh head	43.04	-0.56	-0.39	0.15	0.14	-0.11	-0.79	-0.61
	[15.33]	(-0.69)	(-0.51)	(0.18)	(0.15)	(-0.11)	(-0.78)	(-0.49)
Female headed hh	0.25	0.01	-0.00	0.04	0.03	0.04	-0.00	0.08
	[0.43]	(0.38)	(-0.25)	(1.62)	(1.18)	(1.63)	(-0.01)	(2.15)
Children under 5	0.93	0.03	0.00	0.06	-0.01	0.03	0.03	-0.08
	[1.01]	(0.74)	(0.11)	(1.07)	(-0.22)	(0.59)	(0.45)	(-1.20)
Children 5-14	1.78	0.02	0.01	-0.00	-0.01	-0.02	-0.06	-0.01
	[1.55]	(0.22)	(0.13)	(-0.03)	(-0.14)	(-0.18)	(-0.66)	(-0.08)
Adults 15-64	2.39	0.08	0.09	0.11	0.06	0.06	0.04	0.04
	[1.24]	(1.36)	(1.48)	(1.67)	(0.91)	(0.81)	(0.53)	(0.41)
Adults over 65	0.19	-0.03	-0.03	-0.02	0.00	0.00	-0.01	-0.02
	[0.46]	(-1.19)	(-1.08)	(-0.79)	(0.03)	(0.02)	(-0.18)	(-0.67)
Baseline assets	3.03	0.03	-0.03	0.00	-0.04	-0.05	-0.02	-0.04
	[1.39]	(0.39)	(-0.41)	(0.02)	(-0.51)	(-0.52)	(-0.27)	(-0.34)
Baseline harvest value	3137.25	-25.17	-56.63	-39.10	-155.35	-165.80	7.46	-77.68
	[2605.25]	(-0.17)	(-0.41)	(-0.25)	(-1.00)	(-0.87)	(0.04)	(-0.33)
Female respondent	0.32	0.04	0.02	0.00	0.01	0.02	0.00	0.16
	[0.47]	(1.47)	(0.94)	(0.13)	(0.35)	(0.64)	(0.02)	(4.54)
Respondent age	44.02	-0.77	-0.43	0.02	0.55	-0.09	-0.95	-1.22
	[15.28]	(-0.96)	(-0.58)	(0.03)	(0.64)	(-0.09)	(-0.92)	(-0.98)

Notes: Means and standard deviations of baseline variables for the choice treatment shown in column 1. Columns 2-8 show mean differences relative to the choice treatment for each variable, with t-statistics adjusted for clustering at the village level printed below in parentheses.

Table A.6: Probability of trading assigned item, by item pair and experimental procedure

	Pr(trade) (1)	(2)	(3)
Boom-Cash	0.021	0.004	0.007
	(0.016)	(0.017)	(0.016)
Cup-Spoon	-0.039	0.004	0.001
	(0.024)	(0.031)	(0.030)
Lottery	-0.010	-0.007	-0.015
	(0.019)	(0.021)	(0.021)
Expectations	-0.005	0.008	0.001
	(0.027)	(0.033)	(0.034)
Timing	-0.036	-0.020	-0.025
	(0.032)	(0.034)	(0.035)
Voucher	0.011	0.025	0.021
	(0.033)	(0.039)	(0.039)
Timing + Voucher	-0.018	-0.003	-0.010
	(0.026)	(0.032)	(0.032)
Wording	-0.000	-0.040	-0.035
	(0.033)	(0.035)	(0.035)
Experience	0.016	0.010	0.013
	(0.013)	(0.013)	(0.013)
Controls	none	round	round + hh + indiv
Observations	4,805	4,805	4,804

Notes: Linear regressions of an indicator for whether the participant traded the assigned item. The item pair coefficients (Boom – Cash and Cup – Spoon) correspond to the standard value item pairs and are estimated relative to the Boom–Salt item pair. Other coefficients represent different experimental procedures, including assignment method, scripts and procedures. Additional detail is provided in Section 2.2. Column 1 includes no controls, so coefficients can be interpreted as the effect of the item pair or procedure relative to the Boom–Salt item pair with standard assignment. In column 2, we add controls for the survey round. In Column 3, we also control for household and individual characteristics. Standard errors are clustered at the village level.

Table A.7: Item-specific trading probabilities

	Pr(End item — Assigned item)					
Assigned item:	Boom	Salt	Boom	Cash	Cup	Spoon
Alt item:	Salt	Boom	Cash	Boom	Spoon	Cup
	(1)	(2)	(3)	(4)	(5)	(6)
Assigned item	0.149***	0.164***	0.070*	0.188***	0.126*	0.242***
	(0.029)	(0.030)	(0.038)	(0.039)	(0.066)	(0.073)
Constant	0.599***	0.401***	0.659***	0.341***	0.746***	0.254***
	(0.027)	(0.027)	(0.035)	(0.035)	(0.063)	(0.063)
Observations	1,583	1,517	1,146	1,001	446	386

Notes: Linear regressions of an indicator for whether the participant kept the assigned item. Regressions in each column are restricted to decisions where participants either were given the choice or assigned the item. To facilitate interpretation, the regressions do not include controls. The coefficient on the assigned item shows the additional probability of ending up with the assigned item compared to the choice condition. The constant in the regression captures the probability of choosing the item in the choice condition. Standard errors are clustered at the village level.

Table A.8: Multiple hypothesis test corrections

Dep var	Indep var	Main p-val (1)	List: group (2)	Bonferroni: group (3)	List: overall (4)	Bonferroni: overall (5)
Group: Main scarcity results						
Pr(trade)	Asset q	0.102	0.109	0.306	0.690	1.000
Pr(trade)	Hungry	0.001	0.006	0.003	0.009	0.021
Pr(trade)	Loan	0.042	0.188	0.126	0.696	0.882
Group: Item pairs and procedures						
Pr(trade)	Boom-Cash	0.687	0.996	1.000	0.997	1.000
Pr(trade)	Cup-Spoon	0.950	0.946	1.000	0.950	1.000
Pr(trade)	Solar-Cash	0.014	0.200	0.126	0.186	0.294
Pr(trade)	Lottery	0.473	0.970	1.000	0.968	1.000
Pr(trade)	Timing	0.731	0.994	1.000	0.993	1.000
Pr(trade)	Voucher	0.420	0.968	1.000	0.966	1.000
Pr(trade)	Timing+Voucher	0.400	0.971	1.000	0.969	1.000
Pr(trade)	Wording	0.318	0.951	1.000	0.964	1.000
Pr(trade)	Expectations	0.764	0.988	1.000	0.986	1.000
Pr(trade)	Experience	0.937	0.997	1.000	0.996	1.000
Group: Cognition and scarcity						
Raven's PM	Asset q	0.000	0.000	0.000	0.000	0.000
Raven's PM	Hungry	0.017	0.060	0.153	0.203	0.357
Raven's PM	Loan	0.061	0.349	0.549	0.747	1.000
Stroop congr	Asset q	0.000	0.000	0.000	0.000	0.000
Stroop congr	Hungry	0.000	0.000	0.000	0.000	0.000
Stroop congr	Loan	0.135	0.308	1.000	0.870	1.000
Stroop incongr	Asset q	0.000	0.000	0.000	0.000	0.000
Stroop incongr	Hungry	0.141	0.309	1.000	0.760	1.000
Stroop incongr	Loan	0.280	0.376	1.000	0.971	1.000

Notes: P-values with and without corrections for multiple hypothesis testing. *Dep var* and *Indep var* refer to the dependent and independent variables of interest. Empirical specifications correspond to the main results with a full set of controls. We investigate three main types of hypotheses in the paper and organize all tests into groups by hypothesis: the main scarcity and decision-making hypothesis (*Main scarcity results*); hypotheses related to the robustness of the main results with respect to procedural variations (*Item pairs and procedures*), and hypotheses related to scarcity and cognition (*Cognition and scarcity*).Column 1 reports the p-value based on clustered standard errors presented in the main text and column 2 reports within-group multiple hypotheses testing adjusted p-values following the correction procedure proposed by List et al. (2019). Column 3 shows the same corrections using more conservative Bonferroni corrections. Columns 4 and 5 show List et al. (2019) and Bonferroni corrected p-values when all 22 tests are jointly considered. The List et al.(2019) adjusted p-values in columns 2 and 4 were computed using the mhtreg package (Steinmayr, 2020) based on 10,000 bootstrapping repetitions with clustering at the village level.

Table A.9: Social desirability bias

	Pr(trade)	
	(1)	(2)
Social desirability bias score	0.002	0.002
	(0.003)	(0.004)
Controls	no	yes
Observations	3,906	3,905

Notes: Linear regressions of an indicator for whether the participant traded the assigned item on a continuous measure of social desirability bias. Social desirability bias was measured with an adapted version of the Marlow-Crowne scale from social psychology (Marlow and Crowne, 1961). The sclae includes a series of questions that can be answered in a socially appropriate or inappropriate way, such as ”Are you always courteous, even to people who are disagreeable?”. A higher score on this social desirability scale is indicative of a greater desire to appear socially appropriate. Column 2 conditions analysis on round, procedure, experience, item pair, and household and individual characteristics. Standard errors are clustered at the village level.

Table A.10: Correlates of trading

	Pr(trade)	
	(1)	(2)
Age of HH head	-0.000	0.000
	(0.000)	(0.001)
Female HH head	-0.023	-0.040*
	(0.015)	(0.024)
Members in household under 5 years old	0.011	0.009
	(0.008)	(0.009)
Members in household 5 to 14 years old	0.009*	0.009**
	(0.005)	(0.005)
Members in household 15 to 64 years old	0.005	0.005
	(0.005)	(0.006)
Members in household 65 and over	-0.006	0.010
	(0.014)	(0.020)
Household asset quintile	-0.006	-0.010
	(0.005)	(0.006)
Total harvest value in Zambian KR	-0.000	-0.000
	(0.000)	(0.000)
Gender of respondent	-0.006	0.020
	(0.014)	(0.022)
Respondent Age	-0.000	-0.000
	(0.000)	(0.001)

Notes: Household and individual correlates of trading decisions. Column 1 shows the correlations excluding all other characteristics (each row is from a separate regression). Column 2 shows the multivariate regression output including all characteristics. Both columns condition on round, procedure, experience and item pair. Standard errors clustered at the village level.

Table A.11: Probability of trading assigned item, by access to local trade

	Pr(trade)				
	(1)	(2)	(3)	(4)	(5)
Small village (<28 hh)	0.025				
	(0.016)				
Far from market (>90 min)		0.021			
		(0.013)			
Far from road (>15 min)			0.010		
			(0.015)		
Number of hh making trades				-0.003	-0.003
				(0.003)	(0.004)
Number of households in village				-0.000	-0.000
				(0.000)	(0.000)
Number of households in sample					-0.000
					(0.004)
Observations	5,171	5,171	5,171	4,953	4,953

Notes: Linear regressions of an indicator for whether the participant traded the assigned item on measures of access to local trading opportunities. Village size and walking distance (in minutes) to the nearest market and to a road with transport were estimated by village head person. The indicator for *Small village* corresponds to the bottom quartile of villages, while the indicator for *Far from market* corresponds to above median distances. Columns 4-5 show the effect of within village trading opportunities, conditional on village size. All columns condition analysis on round, procedure, experience, item pair, and household and individual characteristics. Standard errors are clustered at the village level. Information on the size of 7 villages is missing.

Table A.12: Scarcity and cognitive performance

	Raven's PM (1)	Stroop task 2 (2)	Stroop task 3 (3)
Panel A: Baseline wealth variation			
Baseline asset quintile	0.116***	0.087***	0.106***
	(0.014)	(0.014)	(0.014)
Panel B: Seasonal variation			
Hungry season	-0.151**	0.348***	0.086
	(0.063)	(0.058)	(0.058)
Endline	-0.182***	0.003	-0.122**
	(0.062)	(0.055)	(0.053)
Panel C: Variation in loan access			
Loan	-0.431*	-0.282	-0.254
	(0.228)	(0.188)	(0.234)
Time since dropoff	0.153**	0.098*	0.060
	(0.070)	(0.058)	(0.072)

Notes: All outcomes are normalized Z-scores where a higher score indicates better performance. Analysis is restricted to participants who completed both Raven's and Stroop tests. All analyses control for experience with the cognitive tests and household and individual characteristics. Panels A and B also control for round. Panel C also controls for survey week and geographic fixed effects. Standard errors clustered at the village level.

Table A.13: Alternative coding for scarcity and cognitive performance

	Raven's accuracy		Stroop time		Stroop errors	
	(1)	(2)	(3)	(4)	(5)	(6)
Hungry season	-0.575***	-0.505***	-1.911*	-0.830	-0.368**	-0.265*
	(0.153)	(0.171)	(1.001)	(1.057)	(0.152)	(0.145)
Endline	-0.473***	-0.520***	0.253	1.405	0.304**	0.416***
	(0.142)	(0.168)	(0.928)	(1.014)	(0.141)	(0.131)
Dep var mean	5.48	5.48	46.35	46.35	2.43	2.43
Hungry = Endline (p-val)	0.29	0.90	0.00	0.00	0.00	0.00
Observations	4,050	4,049	4,050	4,049	4,050	4,049

Notes: We repeat the analysis of seasonal differences in cognitive performance following the cognitive test scoring used in Mani et al. (2013), specifically Figure 4. Raven's accuracy corresponds to the number of correct items (out of 10). Stroop time and errors correspond to the time (in seconds) and number of mistakes made on the ingruent Stroop task. Odd numbered columns include no controls and do not cluster standard errors (to replicate Mani et al.); even numbered columns include controls for experience with the cognitive tests and individual and household characteristics and cluster standard errors at the village level.

Table A.14: Stock of the items in the home

	Pr(trade)		
	(1)	(2)	(3)
Stock of assigned item	0.001***		
	(0.000)		
Stock of alternative item	-0.000**		
	(0.000)		
Out of assigned item		-0.056***	
		(0.018)	
Out of alternative item		0.037*	
		(0.021)	
Assigned item - Alternative item			0.001***
			(0.000)
Observations	3902	3902	3902

Notes: Linear regressions of an indicator for whether the participant traded the assigned item on the stock of the items in the household, measured in Kwacha equivalents. Data on asset holdings were collected in round 1 and 3 only. Column 1 reports the coefficient on the stock of both the assigned item and the alternate item in levels, column 2 uses indicators for whether the participant was out of the assigned item or the alternative item, and column 3 regresses an indicator for trading on the difference (in Kwacha value) between the assigned item and the alternative item. All columns control for round, procedure, experience, item pair, and household and individual characteristics. Standard errors clustered at the village level.

Table A.15: Willingness to pay/accept

	Willingness to pay/accept for			
	Boom		Solar	
	(1)	(2)	(3)	(4)
Assigned: Boom	1.749***	1.748***		
	(0.105)	(0.104)		
Assigned: Solar			39.613***	36.760***
			(11.324)	(11.142)
Constant	3.025***	3.344***	81.675***	43.192
	(0.077)	(0.394)	(8.516)	(31.062)
Controls	no	yes	no	yes
Rounds	2+3	2+3	3	3
Observations	1,777	1,777	365	365

Notes: Censored normal regression of reported willingess to pay or accept for non-cash item item (Boom or Solar) on whether the item was assigned, for item pairs where the alterantive item was cash. After trading decisions were finalized, participants with the non-cash item were asked a series of hypothetical questions with ascending high cash offers to elicit their willingness to accept the item. Participants with cash were asked as series of descending offers to elicit their willingness to pay. Censored normal regression models were used to account for the censored nature of these observation. All prices are in Zambian Kwacha. Columns with controls condition analysis on round, procedure, experience, and household and individual characteristics. Data on willingness to pay or accept were collected in rounds 2 and 3 only.

A.2 Willingness-to-pay and willingness-to-accept

In survey round 2 and 3 in the item pairs involving cash, we elicited participants' (hypothetical) valuations of the non-cash item after they made their decision. More precisely, we presented participants with a sequence of hypothetical cash values, ascending or descending from the cash amount in the item pair (3.5 Kwacha in Boom – Cash, 80 Kwacha in Solar – Cash). Participants who ended up with the non-cash item (i.e., those kept it or traded for it), were asked if they still would have preferred the non-cash item at increasingly high cash offers, resulting in a hypothetical minimum WTA for the non-cash item.[37] Participants who ended up with cash were instead asked if they would have still chosen cash for a decreasing series of cash values.

We assumed monotonic preferences and only elicited a unique switching point for each individual, which is a common procedure to avoid multiple switching in experiments with choice lists (e.g., Dohmen et al., 2010). For the interval in which the individual switches, valuations were coded to be at the midpoint of the interval. That is, any participant who ended up with Boom was asked if she would have chosen cash if the cash offer had been 3.55 Kwacha; if the answer was affirmative, WTA was coded as 3.525. The range used to elicit WTP/WTA results in censoring at the top and bottom of the distribution. In the Boom – Cash item pair, 17.27 percent of observations fall at the end points of the range (2 and 5 Kwacha during the hungry season; 0 and 10 Kwacha at endline).[38] In the Solar – Cash item pair, 16.5 percent of observations fall at the end points (10 and 350 Kwacha).

Figure A.2 shows the distribution of the stated valuations by initial assignment and item pair, with smoothed density plots. In the Boom–Cash item pair, participants randomly assigned cash have a median valuation of 3.5 Kwacha (mean 3.3), very close to

[37] The exact wording for the Boom – Cash pair was as follows. (1) If switched from Boom to cash: *Would you have traded the Boom for [descending values from 3.45] kwacha?* (2) If kept Boom: *Would you have traded the Boom for [ascending values from 3.55] kwacha?* (3) If switched from cash to Boom: *If we had given you [ascending values from 3.55] kwacha, would you have traded that money for a box of Boom?* (4) If kept cash: *If we had given you [descending values from 3.45] kwacha, would you have traded that money for a box of Boom?* The wording is the same for individuals in the Solar – Cash pair, but the descending values start at 75 and the ascending values start at 85.

[38] After observing the degree of censoring during the hungry season, we expanded the range of values used for elicitation during the endline. Less than 5 percent of the observations fall in the expanded range, however they do impact mean valuations. In our main analysis of the impact of assignment on stated valuations we therefore allow for the full range and condition on round fixed effects.

the market price of 3-3.5 Kwacha and to the value of cash in the item pair. Participants assigned Boom have a median (mean) valuation of 3.8 (4.0). In the Solar – Cash item pair, participants randomly assigned cash have a median valuation of 80 Kwacha (mean 86), again close to the market value of the solar lamp and to the value of cash in the item pair (80 Kwacha). For participants randomly assigned to the solar lamp, the median valuation is again 80 Kwacha, while the mean valuation increases to 114.7 Kwacha. For the Boom – Cash item pair, the average difference between the stated valuation and the trading price of 3.5 Kwacha is 0.67 Kwacha, on average, or around 20 percent the value of the cash available. For the Solar – Cash item pair, the average difference between the stated valuation and the trading price is 49 Kwacha, or 61 percent of the value of the cash available. These valuation differences suggest that participants are on average far from indifferent about which item they end up with.

In Appendix Table A.15 we estimate the impact of item assignment on participants' hypothetical WTP/WTA. Because some responses never stated a willingness to switch items (i.e., are bottom or top censored), we use censored normal regression to account for the censored nature of the stated valuations. As shown in Appendix Table A.15, an initial assignment of Boom increases the mean valuation by around 1.75 Kwacha, or around half of the value of the cash in the item pair (column 2).[39] Initial assignment of the solar lamp increases the average valuation by 36 Kwacha, or around 45 percent of the value of the cash in the item pair (column 5). The similar proportional effects on ex post valuations appear somewhat contradictory to our trading results, where we see a higher probability of trading for participants assigned to the Boom – Cash item pair than to the Solar – Cash item pair. However, as seen in Appendix Figure A.2, the initial assignment of Boom in the Boom – Cash item pair results in a pronounced rightward shift of the full distribution of stated valuations, while the shift in the valuation in the Solar – Cash item pair is more concentrated in the tails of the distribution, which are not directly relevant for trading.

[39] We pool the analysis of the impact of initial assignment on WTP/WTA across the two rounds in which Boom -- Cash was implemented (Hungry season and Endline) since we do not see seasonality effects for this item pair and since the range of values used in each round were different.

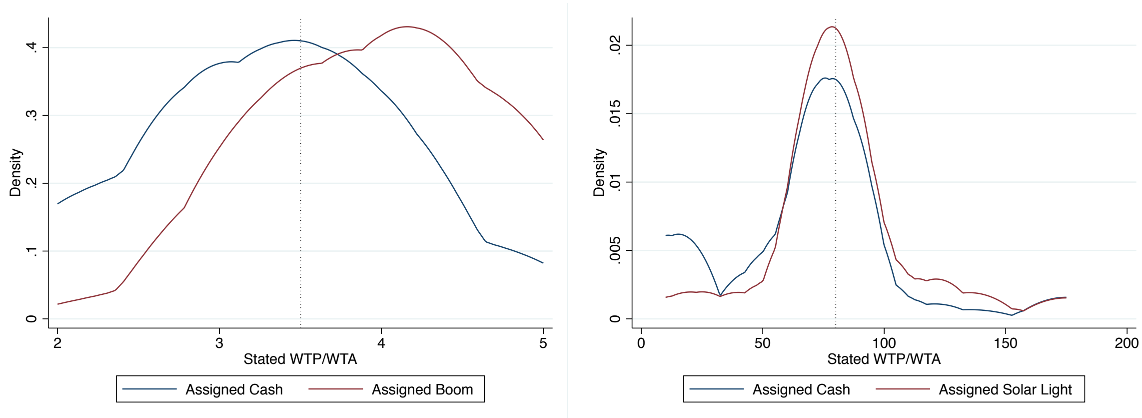

Figure A.2: Hypothetical WTP/WTA

Notes: Hypothetical willingness to pay and willingness to accept for participants assigned to Cash or Boom (left) or Cash or Solar light (right). All observations to the right of the vertical line come from individuals assigned cash (Boom or Solar) who chose to trade (not to trade). The data in the left sub-figure come from both rounds 2 and 3 (N=1,777). The data in the right sub-figure come from round 3 only (N=365), and excludes 41 observations with valuations greater than 175.

A.3 Cognitive function

We implement two commonly used measures for cognitive function: Raven's Progressive Matrices (RPM) and a Stroop test. In the taxonomy provided in Dean, Schilbach, and Schofield (2017) the RPM offers a measure of fluid intelligence, while the Stroop test is a measure of inhibitory control or executive function. All cognitive tasks were unincentivized.

To assess basic cognitive function, we administered a subset of 10 items from Raven's Progressive Matrices (RPM) test battery (Raven, 1983). These items were pilot-tested and calibrated to be of medium difficulty for the average participant.[40] RPM are a nonverbal test designed to measure fluid intelligence, which is the ability to solve novel problems and recognize patterns and relationships independent of acquired knowledge. Prior to the RPM, all participants went through four practice examples. In each case – both for the practice rounds and for the actual test items – an image with a basic pattern was first shown to the study participant, and they had to choose a matching shape and pattern from six possible answers. A sample decision task is provided in Figure A.3.

[40] A total of 17 RPM items were pilot tested; the final 10 items selected all had between 15% and 75% of correct responses in the pilot sample (N=20).

For the main variables in our analysis, we used a two-parameter logistic model (2PL) to construct a single RPM score for each participant. Internal consistency of the final 10 item scale was high, with an estimated Cronbach's alpha of 0.75. We also assessed a simpler linear score, summing up all correct responses. The correlation between the latent factor model score and the linear scale score is 0.99. To facilitate interpretation of regression coefficients, we normalize the 2PL model to have a mean of zero and standard deviation of 1.

The Stroop test (Stroop, 1935) is designed to measure a person's selective attention capacity and their processing speed, and has gained popularity as an easy-to-apply test for executive functioning skills in recent years (Diamond, 2013; Dean, Schilbach, and Schofield, 2017; Scarpina and Tagini, 2017). Stroop tests exist in a variety of formats, including colors, shapes and day-night variations. For the purpose of this study, we used a numeric version of the Stroop test, which required participants be able to read numbers 1-7, but did not require an ability to read or write beyond these numbers. The numeric Stroop test involved two steps in our study. In a first step, we verified participants' ability to read numbers by presenting them with 6 single digit numbers. Participants who were able to identify the majority of these numbers were then allowed to take the main Stroop test. Out of 4,719 participants, we excluded 282 participants (6 percent) due to lacking numeracy. The second step was the main Stroop test which involved three tasks with 25 trials each. In the first task (neutral task), participants were asked to state the number of objects they saw in a trial. Objects were circles and crosses; each trial contained between 1 and 7 identical objects. In the second task, objects were replaced with numbers; once again, participants had to count the number of digits in each trial. In this second task of the Stroop test, printed numbers always matched the number of objects (e.g. four "4"s or six "6"s) - a congruent stimulus condition, with both information sources providing the same information. In the last round, participants had to count objects once again, but this time the objects were single digit numbers that did not match the number of objects in each trial (incongruent task). Figure A.4 excerpts four trials from each task.

As highlighted in a recent review on the Stroop test, researchers have used a wide variety of approaches to score Stroop tests (Scarpina and Tagini, 2017). For example, a commonly used approach normalizes the score on the incongruent task (task 3) by the score on the congruent task (task 2). However, we observed considerably more variation in performance on the congruent task within subjects, and so choose to analyze them separately. Following the scoring modalities outlined in Stroop (1935), we calculate an error-corrected score as the total time plus the number of mistakes times the penalty. The penalty for each incorrect question proposed in the original Stroop test is twice the median time needed for each row/item, which corresponds to1.8 seconds in our sample. The median number of mistakes was 1 in the neutral condition, 0 in the congruent condition, and 2 in the incongruent stimuli condition. To facilitate interpretation, we normalized all scores to mean 0 and standard deviation 1. In order to ensure our results were not driven by specific coding choices, we also independently analyzed the raw scores foreach of the three sub-tasks (neutral, congruent, incongruent). The median time for completing the incongruent task was 42 seconds (mean 45), while the median number of mistakes was 2 (mean 2.4).

Appendix Figure A.5 shows correlations of the different cognitive scores (all coded as z-scores with a higher value indicating better performance). The left panel shows the correlation between the z-score for the Stroop incongruent test and the RPM ($\rho = 0.37$). The middle panel shows the correlation between the congruent and incongruent tasks, measured as error adjusted scores ($\rho = 0.73$). The right panel shows the correlation between errors and time in the incongruent task ($\rho = 0.31$). We include participants who completed both tests and also made trading decisions (N=4,050) to ensure a consistent sample in our analysis.

Our approach closely follows Mani et al. (2013) both in leveraging variation in scarcity from the timing of agricultural harvests and in the specific tests we use to measure cognitive function.[41] However, our main results (Table 4) measures performance using a different scoring approach. For comparability, we therefore replicate our main results using their approach as well (see Appendix Table A.13).

[41]We thank Sendhil Mullainathan for generously sharing the instruments used in their study.

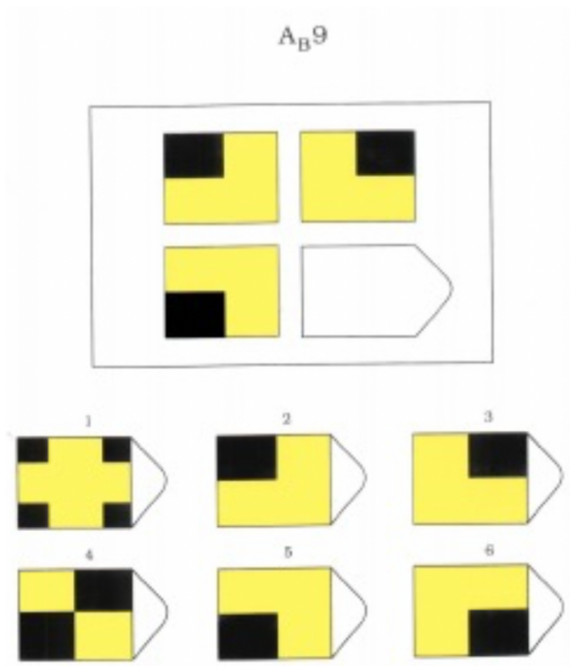

Figure A.3: Raven's Progressive Matrices: Sample decision task

Congruent Task 1

1	OOOO
2	X X
3	O O O
4	X X X X

Congruent Task 2

5	1
6	333
7	55555
8	1

Incongruent Task

9	1111111
10	66
11	22222
12	444444

Figure A.4: Stroop: Sample decision tasks

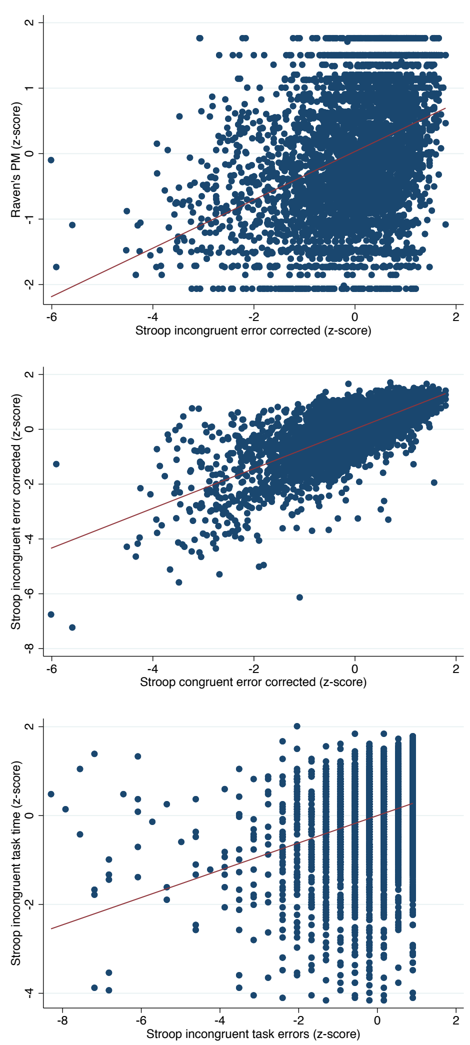

Figure A.5: Correlations across cognitive measures

A.4 Scripts

Round 1: Harvest Survey (July 2014)

Initial allocation:

- [Standard assignment] READ: For doing the survey with us today, we would like to show our appreciation for the time that you have shared with us. We have ${first_item} and ${second_item} and you will get item ${item} today. This item is yours to keep, you own it.
- [Lottery assignment] READ: For doing the survey with us today, we would like to show our appreciation for the time that you have shared with us. We have ${first_item} and ${second_item}. It will be randomly determined which item you get. [Flip a coin: Head is ${second_item}, Tail is ${first_item}]. The coin came up [Tails/Head] so the item you get is [ITEM]. It is yours to keep, you own it.

Trading opportunity: (only one script)

- READ: You now have the option to exchange your [ITEM] for [OTHER ITEM], if you so desire. So that you own [OTHER ITEM], but not [ITEM]. Please make your choice.

Round 2: "Midline" survey (Feb-March 2015)

Initial allocation:

- [Standard assignment] READ: For doing the survey with us today, we would like to show our appreciation for the time that you have shared with us. We have ${first_item} and ${second_item} and you will get item ${item} today. This item is yours to keep, you own it.
- [Lottery assignment] READ: For doing the survey with us today, we would like to show our appreciation for the time that you have shared with us. We have ${first_item} and ${second_item}. We will now let you pick a button from this bag to decide which of the two you will get. In the bag are 8 buttons. 4 of the

buttons are color1 and 4 are color2. (Show buttons and show putting them in the bag.) You will reach into the bag and without looking, select a button. If you pick a color1 button, it means you will get ${first_item}; if you pick a color2 button you will get ${second_item}. Since exactly half the buttons are color1 and the other half are color2, you have the same chance of selecting each color. (Have respondent draw a button) You have drawn a [color1, color2] button, so you get [first_item, second_item]. (Hand respondent their item). This item is yours to keep, you own it.

Trading opportunity: (two scripts: standard and wording)

- [*standard*] READ: You now have the option to exchange your [ITEM] for [OTHER ITEM]. So that you own [OTHER ITEM], but not [ITEM]. Would you like to keep your [ITEM] or exchange it for [OTHER ITEM]?
- [*wording*] READ: Just one question before I go. I know that I gave you [ITEM] today – would you be willing to take [OTHERITEM] instead?

Round 3: Harvest survey (July-Sept 2015)
Initial allocation:

- [Lottery assignment] READ: For doing the survey with us today, we would like to show our appreciation for the time that you have shared with us. We have ${first_item} and ${second_item}. We will now let you pick a button from this bag to decide which of the two you will get. You see here that we have a bag and inside are 8 buttons. 4 of the buttons are white and 4 are blue. (Show buttons and show putting them in the bag.) You will reach into the bag and without looking, select a button. If you pick a white button, it means you will get ${first_item}; if you pick a blue button you will get ${second_item}. Since exactly half the buttons are white and the other half are blue, you have the same chance of selecting each color. (Have respondent draw a button) You have drawn a [color1, color2] button, so you get [first_item, second_item]. (Hand respondent their item). This item is yours to keep, you own it.

- [Expectations procedure]: same as lottery assignment, endowment midway through survey, add announcement after participants got item. READ: "At the end of the survey you will be able to exchange your {first_item} for {second_item}, if you want."

- [Voucher procedure]: script and timing same as in lottery assignment, except last sentence, which says 1) READ [once participants has drawn the button]: You have drawn a [color1, color2] button, so you get [first_item, second_item]. (Hand respondent the voucher) I am giving you a voucher for the item and then when the survey is done, I will give you the actual item. This item is then yours to keep.

Trading opportunity:

- READ: You now have the option to exchange your [ITEM] for [OTHER ITEM]. So that you own [OTHER ITEM], but not [ITEM]. Would you like to keep your [ITEM] or exchange it for [OTHER ITEM]?