

Poverty, Seasonal Scarcity and Exchange Asymmetries*

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September 30, 2019

Abstract

A growing literature associates poverty with biases in decision-making. We investigate this link in a sample of over 3,000 small-scale farmers in Zambia, who participated in a series of experiments involving the opportunity to exchange randomly assigned household items for alternative items of similar value. Analyzing a total of 5,842 trading decisions over a range of household items, we show that exchange asymmetries are sizable and remarkably robust across items and experimental procedures. Using cross sectional, seasonal and randomized variation in financial resource availability, we show that exchange asymmetries decrease in magnitude when subjects are more constrained. Consistent with the interpretation that financial constraints increase decision stakes, we also show that trading probabilities increase when the value of the items involved is exogenously increased.

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

JEL classification numbers: C93, D12, O12

*We are indebted to A. Patrick Behrer for excellent research assistance as well as Rachel Levenson for the overall project management and help throughout the study. We thank Leandro Carvalho, Supreet Kaur, Karen Macours, Simon Quinn, Frank Schilbach, and audience members at numerous seminars and workshops for constructive comments. Fieldwork was 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 biases may be greater among the poor (e.g., Tanaka et al., 2010; Spears, 2011; Bartoš et al., 2018). The conceptual relationship between poverty and decision-making is not obvious. While systematic decision biases contribute to poverty in the long run, scarce financial resources make the same decisions more consequential. This may help focus attention and minimize mistakes (Goldin and Homonoff, 2013; Shah et al., 2015; Gabaix, 2019). Rational inattention models would thus predict decision quality to improve with poverty (Sims, 2003; Maćkowiak et al., 2018).¹ On the other hand, a lack of financial or other liquid resources (which we refer to as scarcity or resource constraints) may also interfere with cognitive function if an increased focus on financial matters absorbs cognitive bandwidth (Mani et al., 2013; Mullainathan and Shafir, 2013). A recent and separate literature suggests that cognitive function is related to preference anomalies, in particular to small-stakes risk aversion and impatient behavior (e.g., Burks et al., 2009; Oechssler et al., 2009; Dohmen et al., 2010; Benjamin et al., 2013). However, evidence of a causal effect of poverty on decision-making is largely missing (Kremer et al., 2019).

This paper presents field evidence on the impact of scarcity on the most basic economic decision: 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. This gap between willingness to pay and willingness to accept is commonly referred to as the "endowment effect."² It is taken as canonical evidence for loss aversion and implies that preferences depend on endowments, which violates one of the central axioms of neoclassical economics (see Ericson and Fuster, 2014, for a review of the recent literature). Besides these fundamental implications, the endowment effect also has potentially far-reaching implications for economic development. If people hesitate to give up their existing assets, they may trade too little and forego profitable investments.³

¹Building on the premise that attention is typically a scarce resource, theories of rational inattention, pioneered by Sims (2003), posit that decision makers choose how much attention to pay to a decision depending on its importance. Arguably, the same decision is higher stakes, relative to income, for a poorer household than a richer household, and thus receives more attention.

²The term "endowment effect" was introduced by Thaler (1980). However, some critics have argued against the use of this term as it already suggests an interpretation of the observed anomaly (e.g., Plott and Zeiler, 2005, 2007). While we will 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 broad literature.

³Unrealized investment opportunities are common in many developing countries despite apparently high returns

While a large literature has investigated the endowment effect in high-income settings with items of low value, field evidence from decisions involving more valuable items remains limited. To gather such evidence and to investigate the relationship between scarcity and decision-making, we conducted a series of decision experiments with 3,059 small-scale farmers in rural Zambia. In 5,842 trading decisions observed over 14 months, we first show that exchange asymmetries are remarkably large and robust in this low-income setting. We then take advantage of both natural and experimental variation in the resource constraints faced by households in our setting to identify their impact on decision-making. We find that scarcity improves the quality of decision-making overall, and also show that trades over higher value items move trading behavior closer to standard economic predictions.

Our data collection was embedded in an ongoing randomized controlled trial on seasonal resource constraints and labor supply that involved repeated surveys over multiple years (see Fink, Jack, and Masiye, 2018). As part of the ongoing surveys, households received a small item as compensation for their time. We intervened in this standard procedure by randomly endowing participants with one of two equally-valued items midway through a survey. Items were common household necessities worth about 1/5th of the daily agricultural wage. At the end of the survey, surveyors offered participants the opportunity to trade the endowed item for the alternative item. Because we randomly assigned the initial item and trading costs were near zero, neoclassical theory predicts that, for any distribution of preferences, half of participants received their less preferred item, and should thus have traded the item received for their preferred item.⁴

We find strong and robust evidence for the existence of exchange asymmetries in our setting. On average, across all item pairs, some of which involved cash, only 34 percent of participants traded the endowed item. We test an extensive range of experimental procedures, following Plott and Zeiler (2007), to measure the extent to which these large exchange asymmetries are driven by features of the design, including the assignment procedure, participants' attachment to the endowed item and uncertainty regarding future trading opportunities, as well as social norms

to capital (e.g., Banerjee and Duflo, 2005; Duflo et al., 2011; Bandiera et al., 2017; Burke et al., 2018). While this empirical observation is beyond the scope of traditional neo-classical models, it may be explained by a number of market frictions and failures, or by behavioral anomalies such as present bias and loss aversion (Kremer et al., 2019).

⁴Knetsch (1989) reports strong evidence of an exchange asymmetry for coffee mugs and chocolate bars from a lab experiment with students. About 89 percent of subjects kept their assigned mug, while only 10 percent of students traded their assigned chocolate bar for the mug. Most subsequent experimental evidence relies either on the described exchange paradigm (Knetsch, 1989) or the valuation paradigm (Kahneman, Knetsch, and Thaler, 1991). In the valuation paradigm, individuals are randomly assigned the role of buyers or seller and have to state their willingness to pay (WTP) or their willingness to accept (WTA) for an item. A higher WTA than WTP is taken as evidence for the endowment effect or WTP-WTA gap.

and experimenter demand. None of the procedural variations had a statistically or economically significant impact on the observed exchange asymmetries in this setting.

To examine the relationship between resource availability and decision-making, we explore three 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 after the 2014 harvest, before the 2015 harvest (“hungry season”) and after the 2015 harvest,⁵ and (3) we leverage random variation in the disbursement of small consumption loans before the 2015 harvest. All three sources of variation are associated with meaningful variation in direct proxies for the availability of liquid resources, such as consumption and grain stocks.

With all three sources of variation, we show that greater resource constraints are associated with a higher propensity to trade and therefore smaller exchange asymmetries. First, households with fewer durable assets at baseline are significantly more likely to trade. Second, we show that individuals are more likely to trade during the hungry season than after harvest. With a single rainy season and harvest each year, farming households in Zambia receive most of their annual income at once, and need to use this income to cover investment and consumption needs over the following 10-12 months. The result is a pronounced period of scarcity (“hungry season”) in the period leading up to the next harvest. During this time, decision stakes are likely higher because the marginal utility of consumption is higher and also because it is more difficult for households to obtain the alternative item or to reverse the experimental decisions in local markets. Consequently, taking one of the two items essentially means giving up the other when resources are scarce (hungry season), but not when abundant (harvest season). Third, we find that households are less likely to trade in the weeks following receipt of the consumption loan. Those households faced a temporary decrease in resource constraints, relative to households that were (randomly) ineligible for the loan.

To further investigate mechanisms, we increased the value of the items traded for a subset of participants. Specifically, we offered these participants a choice between two items worth USD 14 or about 28 percent of monthly household income. In this treatment, the likelihood of trading increased by about 10 percentage points. Given the recent evidence on poverty-induced cognitive deficits, we also conducted a range of cognitive assessments with study farmers. Overall, we see little evidence that variation in cognitive skills or executive function are associated with scarcity

⁵This is similar variation to Mani et al. (2013) and Carvalho et al. (2016), though we observe decisions over multiple harvests or farmer “pay days” and generate separate exogenous variation in experience with the decision over time.

or with the quality of decision-making.⁶

Together, these findings indicate that scarcity *decreases* the magnitude of exchange asymmetries, both within participant across agricultural seasons and across participants within a season. The likelihood of trading the endowed item increases by roughly 10 percentage points during the hungry season compared to the post-harvest period, reducing the exchange asymmetry by more than 50 percent, while access to short-term resources through the larger RCT increases exchange asymmetries by almost the same magnitude (10 percentage points). Strikingly, these reductions in the magnitude of the exchange asymmetry match the observed reduction in exchange asymmetries in our “high value” treatment, 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 forty-fold increase in the value of the exchange items. Overall, our results highlight a strong propensity to hold on to endowed items even in settings where people are poor, on average. This propensity declines substantially when resources are relatively more scarce and when the value of the goods involved is high, suggesting that exchange asymmetries are decreasing in decision stakes, a pattern that appears consistent with models of rational inattention.

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; Dean et al., 2017; Kremer et al., 2019). Previous studies suggest that poverty may have a negative causal effect on decision-making 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 prevalence of stress and depression may interfere with decision-making or increase biases (Haushofer and Fehr, 2014; Haushofer and Shapiro, 2016), or that the living conditions of the poor may contribute to worse decision-making (Dean, 2019; Lichand and Mani, 2019; Schilbach, 2019). We extend this literature by presenting evidence on common trading decisions that are marked by systematic deviations from normative behavior despite their immediate payoff, and by employing a credible research design that relies on repeated observations and a combination of natural and exogenous sources of variation in resource scarcity. To date, few papers have traced effects from exogenous variation in scarcity through to real stakes decisions.⁷ We fill that gap and

⁶We implement a standard set of tests from cognitive psychology in a sub-sample of participants in each survey round. We see limited correlation between cognitive function and either scarcity or exchange asymmetries; in a simple measure of attention, we observe that greater attention is associated with a higher trading probability. More nuanced measures of fluid intelligence or executive function, on the other hand, are uncorrelated with trading probabilities.

⁷Kaur et al. (2019) show a link between scarcity and productivity by varying the timing of causal wage payments in

provide novel evidence suggesting that resource constraints may improve decision-making in a decision associated with one of the most well-studied behavioral anomalies. Past research suggests that the greatest costs from taxing cognitive bandwidth arise in decisions with longer term consequences, involving risk or real effort (Alonso et al., 2013; Mullainathan and Shafir, 2013). Our measure of decision-making represents a salient trade-off between items of immediate and considerable value, and participants may allocate substantial cognitive resources to the decision as a result (see Shah et al. (2015) and Lichand and Mani (2019) for related evidence using hypothetical trade-offs). That said, we find large and robust deviations from rational behavior in the decision we study, that are reduced but not completely eliminated when resources become more scarce.

Second, this paper adds to the ongoing debate about the robustness of behavioral biases 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, there is surprisingly little field evidence, in particular on transactions that are sufficiently large to have a potentially meaningful impact on household well-being.⁸ Perhaps the most influential work on the endowment effect outside of the lab is a series of experiments at sport cards shows in the United States demonstrating the importance of market experience (List, 2003, 2004). On average, trading rates in this specific market are very 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). Unlike sport card show attendees, our study sample is relatively homogeneous in their degree of trading experience and were asked to make decisions about familiar items, diminishing the importance of experience in our context. We also test whether repeated exposure to our trading decision affects behavior; it does not. In a setting more similar to ours – and to our knowledge the only other experimental measurement of exchange asymmetries in a low-income setting – 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.⁹ We find no evidence that access to outside markets affects trading probabilities, though our study population is relatively

India. They find suggestive evidence that lower productivity among subjects who are not paid until the end of the field experiment – as compared to those who receive half of their wages mid-way through – is due to reduced attention.

⁸Taking advantage of more easily accessible online surveys, ?, for example, present evidence on a WTP-WTA disparity for small-stake lottery tickets in a representative US sample.

⁹This finding is potentially consistent with the variation that we observe; households with less market exposure face a higher value decision because the cost of “undoing” their choice is relatively large (they are less exposed to markets). This leads them to trade at a higher rate.

homogeneous in their (generally high) familiarity with market exchange.

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). These studies suggest, in line with our findings, that the endowment effect is not just an artifact of (laboratory) experiments, but can have important implications for market transactions as it may hamper investment activities, technology adoption, and loan take-up. Our results highlight that these consequences depend on economic circumstances and may be more severe during times of abundance.

The paper proceeds as follows. We turn next to the context and experimental design. Section 3 describes our empirical strategy. Our results on the robustness of the exchange asymmetry in our setting are described in Section 4 and results on the relationship between scarcity and exchange asymmetries in Section 5. Section 6 concludes.

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. 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 typically becoming available in April, food shortages and hunger usually spike between January and March (Fink, Jack, and Masiye, 2018).

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 (2018) for further detail on the RCT). 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” after a 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 household survey. We conducted the decision experiments between July 2014 and September 2015 with a total of 3,059 households across 175 villages. Households participated between one and three times in these experiments, resulting in 5,842 individual decisions, and received the standard compensation (Boom) otherwise.

2.2.1 Experimental Procedures

All household surveys were conducted by trained interviewers with adult household representatives – typically the male or female head of household – in their homes, and took between one and two hours.¹⁰ 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. At the end of the survey, the interviewer showed the non-assigned item again and asked the participant whether he or she wanted to trade the assigned item for the other item.¹¹ After recording the decisions and completing trades (if respondents decided to trade), participants were asked a few questions related to the exchange experiment. All surveys were done using electronic survey devices (tablets), which automatically recorded survey length and the time between the initial item assignment and the trading opportunity. Note that transaction costs were minimal in our setting as participants had to answer the trading question in any case and interviewers immediately completed trades (if desired by participants).

To identify the extent to which observed exchange asymmetries are driven by procedural details, 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

¹⁰Note that survey respondents sometimes changed across survey rounds, and also included other adult household members. We use both the respondent ID and the household ID to examine both within-household and within-subject variation in decision-making over time.

¹¹See appendix for the exact wording of all procedures.

assignment. Specifically, we either implemented the randomization of items directly through the electronic survey devices (*standard assignment*) or randomized items in front of respondents (*lottery assignment*), i.e., either through a coin-flip or by respondents drawing a button out of a bag.¹² 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*.¹³

Second, we implemented three sub-procedures designed to reduce participants' attachment to the assigned item: (i) we shortened the time between the endowment of items and the trading decision, with some participants receiving the endowment 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 about 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 or additional work for surveyors, we varied the wording when presented with the trading opportunity (*wording procedure*). Rather than offering the possibility of trading at the end of the interview, participants were asked to trade the item as an implicit favor to interviewers ("would you be willing...").¹⁴

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), which corresponded to about one fifth of a typical daily wage at the time of the experiment. We varied the item pairs in the exchange experiment to test robustness to alternative items. First, we provided cash of similar value (3.5 Kwacha) as an alternative to Boom (Boom – Cash). Second, we offered durable goods (a mug and a serving spoon; Cup – Spoon). Third, we increased the value of the item pair to over 20 times the value of the default pair, i.e., we used a solar lamp and 80 Kwacha in cash (Solar – Cash). In addition to these 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.

¹²We switched from the coin flip to the button roughly 20 percent of the way through round 1, to reduce ambiguity around the outcome.

¹³For example, if the randomization is non-transparent, respondents might incorrectly infer that the assigned item is more valuable than the alternative item making them reluctant to trade. Similarly, they may perceive the assigned item as a gift from the interviewer or researchers in which case trading items may violate norms or social customs.

¹⁴The idea here 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.

Table 1 summarizes all randomly assigned experimental features, and the number of observations in each, by survey round.

2.2.2 Implementation and Randomization

To understand the impact of the external environment on the robustness of exchange asymmetries, we conducted the experiments over the complete 2014-2015 agricultural cycle. More precisely, 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 randomly assigned phase-in design that generated random variation in participant experience over the three survey rounds. Households not part of the exchange experiment sample continued receiving the default compensation of Boom for completing the survey. Randomization of item pairs was done at the village level, while the randomization of specific experimental procedures was done at the household level.¹⁵

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 experiments. In experiment round 1, we used both the *standard* and *lottery assignment* for endowing 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 experiments took place during the hungry season, from January to March 2015, with a random subset of households across all 175 study villages (with approximately 10 households per selected village). In total, 1,367 households participated in the experiments, of which we assigned 143 households to the *choice* condition 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.

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 respondents' (hypothetical) willingness to pay (WTP) and willingness to accept (WTA)

¹⁵We 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.

in the Boom – Cash item pair after they made their decision (see Appendix section A.2 for more details).

Experiment round 3 (harvest season 2015): We conducted the third round of 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 as described in Section 2.2.1 above. 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 given the *timing* and *voucher* procedures (n=198). As in round 2, we also elicited WTP/WTB 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 straightforward: for any distribution of preferences, a null hypothesis of no exchange asymmetry predicts that 50 percent of the sample will receive their less preferred item and thus trade the endowed 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:

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

To test whether trading probabilities depend on details of the experimental procedure or the value of the items involved, we estimate the following linear probability model:

$$p(\text{trade}) = \alpha + \beta P + \gamma I + X\delta + \varepsilon \quad (2)$$

where, in the absence of controls, α is the trading probability of our default item pair (Boom – Salt) and default procedure (standard assignment), P is a vector of indicator variables capturing the procedural variations described above, and I is a vector of indicator variables for alternative item pairs. β and γ are coefficient vectors that capture the estimated changes in the probability

of trading with alternative procedures and item pairs, respectively. X is a vector of additional household and participant controls, such as gender, age, household composition, wealth, and harvest value.

To test for differences in trading asymmetries within an item-pair (A, B), we estimate:

$$\Pr(endA) = \alpha + \beta startA_{it} + \epsilon_{it} \quad (3)$$

where $endA$ equals one if the participant ended the procedure with item A and $startA$ equals one if the participant was randomly assigned item A at the start of the procedure. β is a measure of the “endowment effect,” i.e. the estimated increase in the probability that the participants ends up with item A when the item was initially assigned. In the absence of controls, α is the likelihood of ending up with item A among those who start with item B, i.e., the probability of trading item B for item A. In some specifications, we add controls, in which case α becomes the probability of ending up with item A among individuals with all covariates equal to the reference category or value. We also estimate equation (3) in restricted subsamples of individuals who either had free choice (*choice* condition) or were assigned item A initially. In these regressions, β can be interpreted as the additional probability of ending up with an item relative to what the *choice* condition would predict.

We take advantage of the timing of the surveys to test how seasonal variation in scarcity affects trading probabilities, conditional on experience. Specifically, we estimate:

$$p(trade) = \alpha + \beta N + \rho R + X\delta + \epsilon \quad (4)$$

where N are indicators for the number of times the respondent participated in a trading decision prior to the current decision, and R are indicators for experimental rounds to capture seasonal effects.

In some specifications, we add controls for survey round, participant experience, procedures, item pairs and/or household or respondent characteristics. We cluster standard errors at the village level throughout, and include household or respondent fixed effects in some analysis.

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.1, A.2 and A.3, 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 – respondent gender and age – show some differential selection in the hungry season, though household characteristics remain balanced.

4 Results: Exchange asymmetries

We begin by documenting the extent and robustness of exchange asymmetries in our sample. Our first test comes from estimating equation (1) in the pooled sample. The overall likelihood that a participant traded the item that they started with is 0.34, which rejects the null hypothesis of $p(\text{trade}) = 0.5$, with a p-value < 0.0001 . Interestingly, the magnitude of the pooled trading rate is similar to the pooled results of other field studies (e.g., List, 2003, 2004). These pooled results mask, however, potential heterogeneity in our results across items, experimental procedures, and participant experience with the trading procedure. In the remainder of this section, we examine each of these factors in more detail. We focus on the results from our “standard value” item pairs (Boom – Salt, Boom – Cash and Cup – Spoon), and save discussion of the results on the high value item pair (Solar – Cash) for Section 5, where we discuss mechanisms.

4.1 Robustness across and within item-pairs

Table 2 provides an overview of results by item pair. The first column presents the results from the *choice* condition, which directly measures participants’ relative preferences for each item. For each item pair, we observe variation in preferences across the items (if all were indifferent, we would expect roughly 50 percent to choose each item). Participants had the most imbalanced preferences in the cup vs. spoon treatment, with three quarters of participants preferring a cup over a spoon (despite similar market value). Preferences were less stark 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. For example, in the Boom – Salt pair, we observe that a majority of participants leave our experiment with Boom (about 60 percent). Finally, the table also shows the overall likelihood that participants traded the item they started with and a t-test for whether the trading probability is different than 0.5. In all pairs the likelihood of trading is significantly below 0.5.

Panel A of Table 3 shows the results from estimating equation (2) where the exogenous variables of interest are item pairs. We focus on the results for our three standard-value item

pairs, i.e., Boom – Salt, Cup – Spoon, and Boom – Cash. Column 1 includes no controls. Each estimated coefficient is therefore the estimated effect on the likelihood of trading relative to the trading probability in Boom – Salt. To adjust for the fact that different item pairs were offered during the three seasons (for example, Boom – Salt was offered in all experiment rounds, while Boom – Cash was offered only in the hungry season and last harvest season, i.e., rounds 2 and 3), we add a round indicator to the specification, so that the reference category now becomes the Boom – Salt item pair in the first round (harvest season 2014). There is little difference in trading rates between item pairs with the addition of round controls, or other covariates (columns 2-4).

We also analyze the directionality of trade within item pair, following estimating equation (3). The results are displayed in Figure 1, which shows the probability of participants ending up with item A when it is (randomly) assigned relative to the probability of choosing item A in the *choice* condition (always with respect to the same item B). We see that the effect of initial assignment is similar across the three standard value item pairs with an average increase in the probability of ending up with the assigned item of 15-20 percentage points compared to the *choice* condition. Appendix Table A.4 summarizes the regression results underlying this figure.

All of the items in the experiment are common household necessities that many households purchase in the market on a regular basis. If all items are inframarginal, i.e., all households would have purchased them anyway, then even a very small friction associated with trading could generate large results. We address this concern in two ways. First, we include the Cup – Spoon item pair, which consists of household durables, making it less likely that households would all be in need of both items and therefore indifferent. We observe the largest exchange asymmetry for this item pair. Second, we asked questions about household stock of our experimental items (buried in a longer list of household items) in rounds 1 and 3 of the survey. When regressing the probability of trading on the stock of the endowed item in the household (in logs), we observe that trading decisions reflect downward sloping demand. That is, a one percent increase in the stock of the endowed item in the home leads to about a 1.4 to 2 percent increase in the likelihood of trading (see Appendix Table A.5). Subjects do, therefore, incorporate decreasing marginal returns from the items into their trading decisions, which is also inconsistent with inframarginal goods driving the exchange asymmetry.

4.2 Robustness to experimental procedures and experimenter demand effects

Prior work suggests that exchange asymmetries may be an artifact of experimental procedures that prevent participants from trading (see, for example, Plott and Zeiler 2005, 2007).¹⁶ To address this concern, we implemented several variations of our standard experimental procedures as described in Section 2.2.1. Panel B of Table 3 presents results from estimating equation (2), with experimental procedures on the righthand side. We estimate differences in trading probabilities relative to the *standard assignment* procedure, i.e., the not-transparent randomization of initial items. Column 1 of Table 3 includes no controls. In columns 2-4, we add controls to the model, including round effects, household characteristics, and item pair indicators.

We find no evidence that the assignment method (*lottery* vs. *standard assignment*) changes behavior. The coefficient estimates are close to zero in all specifications. Recall that, to reduce potential attachment to the assigned object over time, we manipulated the time of ownership by reducing the time span between initial assignment and the trading opportunity from approximately 60 minutes to 5 minutes (*timing* procedure) and varied physical proximity of the assigned item by handing over vouchers (that could be exchanged for the assigned or alternative item at the end of the survey) rather than the actual items at the time of assignment (*voucher* procedure). We find no indication of an increase in trading with shorter time spans between initial assignment and the trading opportunity, or when participants get a voucher instead of the actual item. When we combine both procedures (*voucher* and *timing* procedure) we see a significant 6.5 percentage point increase in trading probabilities in column 1, which, however, becomes smaller and statistically insignificant when we control for experiment round in columns 2-4.

Next, we investigate the possibility that participants refuse to trade their assigned item because of social norms or experimenter demand effects. A first indication that social norms (and demand effects) play little role in explaining the results is the lack of a measurable effect of the *lottery assignment*, which transparently randomized the item assignment in front of participants, and thus should have reduced demand effects relative to the *standard assignment* procedure. To more directly test for the relevance of social norms and experimenter demand effects, we implemented an experimental treatment where we explicitly asked, rather than offered, respondent to trade their assigned item (*wording* procedure). If participants refused to trade because of politeness, we would expect this inverted script to increase trading probabilities. As shown in Panel B

¹⁶There is also debate about whether the WTP-WTA disparity is the result of experimental elicitation procedures (Knetsch and Wong, 2009; Isoni et al., 2011; Cason and Plott, 2014; Bartling et al., 2015; Fehr et al., 2015).

of Table 3, this change in wording had no measurable effect on trading probabilities.

As an additional test of a (social) experimenter demand effect, we implemented an adapted version of the Marlow-Crowne scale from social psychology (Marlow and Crowne, 1961) to measure socially appropriate behavior.¹⁷ A higher score on this social desirability scale is indicative of a greater desire to appear socially appropriate. We test whether this score is positively associated with trading behavior. As Appendix Table A.6 shows, we find no evidence that socially desirable reporting influences decision-making in our setting.

4.3 Robustness to experience and expectations

Participants may be reluctant to trade if they lack experience with similar trading situations (see e.g., List, 2003; Engelmann and Hollard, 2010). The longitudinal nature of our data collection, which randomly phased in households, allows us to directly examine the effect of experience. To measure the impact of experience on trading, we analyze trading decisions as a function of previous trade experience, controlling for the item pair, experimental procedures and household controls. The results are shown in Appendix Figure A.1, which is restricted to the third round of data collection, when we observe households randomly selected for zero, one and two prior trading experiences. Experience appears to play a negligible role in improving decision-making, at least over the intervals at which our data collection was spaced and the number of repeated decisions that we observe.

The literature has also highlighted expectations as potential explanation for exchange asymmetries. Arguably, participants with more experience with our experiment may expect this trading opportunity with a higher probability than less experienced or inexperienced participants. If such expectations shape their reference point, we would observe more trading among experienced participants (Kőszegi and Rabin, 2006). To more directly test the importance of participants' priors regarding future trading opportunities, we manipulated participants' expectations about future trading (*expectations* procedure). Explicitly informing participants about a possible trade at the end of the interview should have reduced the uncertainty about subsequent trading and shifted potential reference points. The coefficient estimate for our *expectations* procedure shown in Panel B of Table 3 is close to zero, suggesting that trading probabilities are not affected by the perceived likelihood of subsequent trades.

¹⁷The Marlow-Crowne module 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?"

5 Results: Scarcity

Our results up to this point show evidence for substantial exchange asymmetries in our sample of poor, rural households, which are robust to changes in experimental procedures or participants' experience. This implies that exchange asymmetries are a relevant decision bias in our study population, and therefore a relevant setting for investigating how these asymmetries are affected by variation in resource constraints within this sample. We organize our results around three sources of variation in resource scarcity, imposing increasingly strict (exogeneity) requirements on the source of variation. For each source of variation, we first show the empirical relationship between the scarcity measure and the level of consumption to establish that these sources of variation do, in fact, affect resource availability. Next, we test how the three scarcity measures relate to trading probabilities. Finally, we shed some light on possible mechanisms for our result by investigating behavior in the high-value decision treatment, the role of cognition in the relationship between scarcity and decision-making, and variation in market access.

5.1 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 Appendix Figure A.2, asset ownership is directly linked to consumption, with wealthier households significantly less likely to skip 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 conditions and household and individual controls, in Figure A.3. The negative gradient indicates more trading in poorer households, though the confidence intervals are large (p-value on the difference between the first and fifth quintile is 0.12). Since numerous other factors correlated with wealth may affect trading behavior, we turn to more plausibly exogenous sources of variation in the value of the traded item and participants' available resources below.

5.2 Seasonal variation in wealth and income

As described above, pronounced seasonality in income, savings and consumption is one of the most salient features 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, while the other two rounds took place in times of relative

abundance, immediately following harvest. In our sample, the average cash savings during the hungry season is around 100 Kwacha, or 17 USD, while the average cash savings at harvest is over 600 Kwacha. 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 (Appendix Figure A.2). We exploit this variation in seasonal resource availability and compare trading decision during the hungry season with decisions in two harvest seasons, conditional on random variation in participant experience.

Figure 2 and Table 4 show the estimated marginal effect of the season on trading probabilities. As shown in the Figure, 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 14 percentage points (Table 4). The point estimate is largest in columns 5 and 6, which include individual fixed effects and limit the sample to inexperienced respondents, respectively. Importantly, the effect is specific to the hungry season: the trading probability in the following harvest season are insignificantly different from those in the first harvest season (columns 4-6). 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 greater likelihood of trading following lower yields during the 2015 harvest (see Fink, Jack, and Masiye, 2018, for details).

Finally, it is important to highlight that the observed variation in trading behavior by season does not 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 choose Boom over Salt) than in the harvest season 2014 (60 percent) or 2015 (57 percent), the differences are far from statistical significance (Fisher’s exact test, $p\text{-value} > 0.3$).¹⁸

5.3 Experimental variation in liquidity

While the seasonal variation in trading asymmetries is suggestive of a causal effect of scarcity on trading behavior, several other factors may vary across seasons and influence trading decisions. To address these endogeneity concerns, we leverage random variation 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

¹⁸We 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.

season by providing selected households with 200 Kwacha (around 35 USD) in cash or maize. We compare trading probabilities for households with and without access to the loans prior to their trading decision. Loans were delivered in early to mid January 2015, while the exchange experiments began in early February, about two weeks later. Appendix Figure A.2 shows that the biggest effect on consumption occurred very shortly after receiving the loan.¹⁹ Figure 3 plots the effect of the loan on trading probabilities, as a function how recently it was received (in weekly bins). The pattern is striking, though standard errors are large: two weeks after receiving a loan the likelihood that a participant trades her endowed item falls by over 30 percentage points relative to the control group. However, this effect wears off quickly. Three weeks after loan delivery the likelihood of trading is 16 percentage points lower than in the control group and levels off at a 10 percent lower trading likelihood in the weeks after.

Table 5 shows a series of loan dropoff impact estimates, conditioned on different sets of control variables. On average, we find that a loan dropoff within the last month increased exchange asymmetries by around 10 percentage points, which is remarkably similar to the seasonal variation found in the previous section (columns 3 and 4). The last two columns show the estimates underlying Figure 3, and illustrate that the estimates are robust to the inclusion of a full set of controls.

5.4 Mechanisms

5.4.1 Value of the traded goods

All of the items used in the experiments were chosen to be useful and of value to the average study participant. However, for more resource constrained households, a “mistake” in the trading decision is more difficult to undo, raising the decision stakes, even if the market value or preference gap between items does not change. To more precisely estimate this mechanism, we introduced a high-value item pair (Solar – Cash condition) in the last round of our experiments. More precisely, we offer participants the choice between a solar lamp or an equivalent value cash transfer of 80 Kwacha (USD 14). While trading over higher value items does not necessarily imply higher stakes – especially if differences in item features are small (e.g. a red vs. a blue car) – higher value items may still garner greater attention or focus, because they make the trade off more salient. As shown in Table 3 (Panel A), relative to the estimated trading probability for lower value items, we

¹⁹These figures show effects relative to all control households (whose dropoff week is undefined) and are conditioned on survey week fixed effects.

find a large and significant increase in the trading probability for the high-value item pair. While only 34 percent of participants traded in the three standard-value item pairs, 44 percent traded in the Solar-Cash treatment. This increase in trading is particularly noticeable because we implemented this treatment after harvest when participants were relatively rich and thus displayed larger exchange asymmetries, on average.

For additional insight into the trading behavior with high value items, we separately analyze the Solar – Cash condition. We estimate equation (3) with the likelihood of ending up with the solar lamp as the outcome and present the results in Table 6. Participants assigned a solar lamp at the outset were 10 percentage points more likely to end up with a solar lamp than participants assigned cash. This is robust to controlling for variation in experimental procedures (*voucher* procedure) in column 3, but becomes statistically insignificant once we control for experience and household characteristics (columns 2 and 4). These findings are supported by participants' hypothetical item valuations. Estimating the impact of item assignment on WTP/WTB shows no differences in valuations, suggesting that raising the value of items leads to more rational behavior (see Appendix Table A.9).

Overall, the exchange asymmetries observed in the high value decisions are about one third of the magnitude of the asymmetries observed in the standard value decisions and generally indistinguishable from zero. These findings lend empirical support to the proposed mechanism that resource scarcity increases the relative value of goods, which increases focus, resulting in more rational behavior.

5.4.2 Cognitive ability

The results on trading presented in the previous section consistently point to more rational behavior when resources are more scarce. While this is consistent with predictions from rational inattention models (Sims, 2003; Maćkowiak et al., 2018), other research suggests that scarcity may improve decision-making in some domains (Shah et al., 2015), while worsening it in others, by taxing cognitive bandwidth (Mani et al., 2013; Mullainathan and Shafir, 2013; Dean et al., 2017). To investigate the relevance of this channel, we explore both the relationship between cognition and resource scarcity in our setting and between cognition and trading probabilities.

We administered two commonly used tests to measure cognitive and executive function to a randomly selected subsample of participants in each survey round: a numerical version of the

Stroop test and Raven’s Progressive Matrices (RPM).²⁰ As described in further detail in Appendix A.3, the RPM is a measure of abstract reasoning skills or fluid, non-verbal intelligence. The test consists of a series of pictures with geometric shapes where participants choose the missing shape from a set of alternatives. We pilot-tested and calibrated the test elements such that they were of medium difficulty for the average respondent. The Stroop test is a measure for inhibitory control, which is one particular domain of executive function that regulates attention and the ability to control impulsive reactions (Diamond, 2013). We use a modified Stroop test that consists of three tasks in which participants have to first identify the number of the displayed circles and crosses (task 1 - neutral task), and then have to identify the number of displayed digits (tasks 2 and 3). In task 2, the displayed digits match their number (e.g., 4444, congruent task); in task 3, they do not (e.g., 444, incongruent task). Task 3 thus requires that participants suppress the automatic response (e.g., 4). We examine each of the tasks separately, focusing on tasks 2 and 3, and interpret the congruent task as a measure of attention and the incongruent task as a measure of inhibitory control.²¹ All outcomes are normalized to a mean of zero and a standard deviation of one, with a higher score corresponding to better performance. None of the tasks were incentivized.

We begin by testing whether cognitive ability varies with scarcity. We follow the three relevant scarcity measures in the order they are investigated above. Figure 4 summarizes the results. The top panel shows that higher asset households have significantly higher scores on the Raven’s Progressive Matrices test and on the main two Stroop measures. In the cross section, wealthier households perform consistently better on these cognitive measures. Second, the middle panel shows little consistency between performance on cognitive tests and season (the coefficients are estimated conditional on individual fixed effects; see Appendix Table A.7 for the underlying regression results). The congruent task from the Stroop test shows a similar pattern to trading behavior, with scores that increase during the hungry season, while the RPM score and incongruent Stroop test show much less pronounced variation. Third, the bottom figure shows effects of loan drop off, relative to the control group and conditional on survey-week fixed effects. Here, performance on the two Stroop tasks is slightly better immediately following loan drop off. These patterns are suggestive at best; confidence intervals overlap zero (the control group) for all weeks and all outcomes. Overall, these results suggest an inconsistent relationship between our

²⁰According 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.

²¹The Stroop test is sometimes scored by normalizing the score on the incongruent task (task 3) by the score on the congruent task (task 2) (e.g., Scarpina and Tagini, 2017). We observe considerably more variation in performance on the congruent task 2 within-subject, and so prefer to analyze them separately.

alternative measures of scarcity and cognitive performance.

Finally, we test whether trading probabilities are correlated with cognitive function. Table 7 shows no significant relationship in the pooled analysis (Panel A), which is unsurprising given the contradictory results on the relationship between cognitive performance and scarcity. However, if we focus on the within-respondent variation in cognition and trading probabilities, we see a positive correlation between the neutral and congruent Stroop tasks and trading probabilities. This is driven by seasonal variation in both, where we find a consistent improvement both in these simple attentional measures of cognition and in decision-making.

5.4.3 Market access

Another possible explanation for the observed exchange asymmetries, both in our setting and in other studies, is market access. In settings where participants can easily exchange one item for the other at low cost, experimental choices may be perceived as inconsequential. While this hypothesis is not obvious in our setting where most villages are remote (average distance to the district capital is 30 miles on dirt roads), many goods can be traded in local markets (through either purchase or barter).²² If this is the case, we should see that easier access to market or local trading partners should increase the measured exchange asymmetry.

To investigate this hypothesis, we regress trading outcomes on a series of market access proxies, and summarize results in Appendix Table A.8. Living in a small village (25th percentile of village population size) and living in a village with above median walking times to the market or nearest road served by public transportation increases the likelihood of trading between 1 and 2.5 percentage points, but neither estimate is statistically different from zero (columns 1-3). Being in a village where more households were given the opportunity to make trades, conditional on village size (column 4) and conditional on the total number of households in the village in the larger study sample (who received Boom if they were not trading, column 5) has no effect on trading probabilities. While several of the market access proxies go in the expected direction, effect sizes are all small and statistically insignificant.

²²This hypothesis is directionally consistent with the finding in Apicella et al. (2014), who show that tribes in Tanzania with more market access display greater exchange asymmetries.

6 Conclusion

Although the endowment effect has been extensively documented in laboratory settings, its relevance in day-to-day decision-making remains unclear. Focusing on the trading of common household items in a low income setting, we document two key findings in this paper. First, we show that exchange asymmetries are substantial and remarkably robust to the items involved and to the experimental procedures. The overall propensity to trade familiar household items is about 16 percentage points lower than predicted by standard theory in our low income sample, and remarkably similar in magnitude to previous studies conducted in high-income settings. This suggests that the psychological and behavioral principles underlying exchange asymmetries cut across cultures and levels of educational attainment. Second, we show that despite the substantial average deviations from rationality, the magnitude of the bias moves in predictable ways, decreasing when decision stakes are higher. In particular, we show that exchange asymmetries are significantly smaller when resources are more scarce and when the value of traded goods is high. This suggests that poverty can actually improve decision-making in contexts like the one studied in this paper involving simple trading decisions with immediate payoffs.

Our results also have potentially 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, 2017), and low rates of new technology adoption (Liu, 2013; Giné and Goldberg, 2017).²³ The results we present in this paper indicate that this bias is 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.

²³A prominent but much debated psychological explanation for exchange asymmetries put forward in the behavioral literature is loss aversion, i.e., the notion that the disutility from giving up a good is higher than the utility gain from obtaining it (Ericson and Fuster, 2014; O'Donoghue and Sprenger, 2018). Taken at face value, our results suggest that scarcity reduces the scope of loss aversion during the hungry season, as farmers appear to put greater focus on the trade-off between the two items and may treat the trading decision as opportunity cost rather than a loss of the endowed item.

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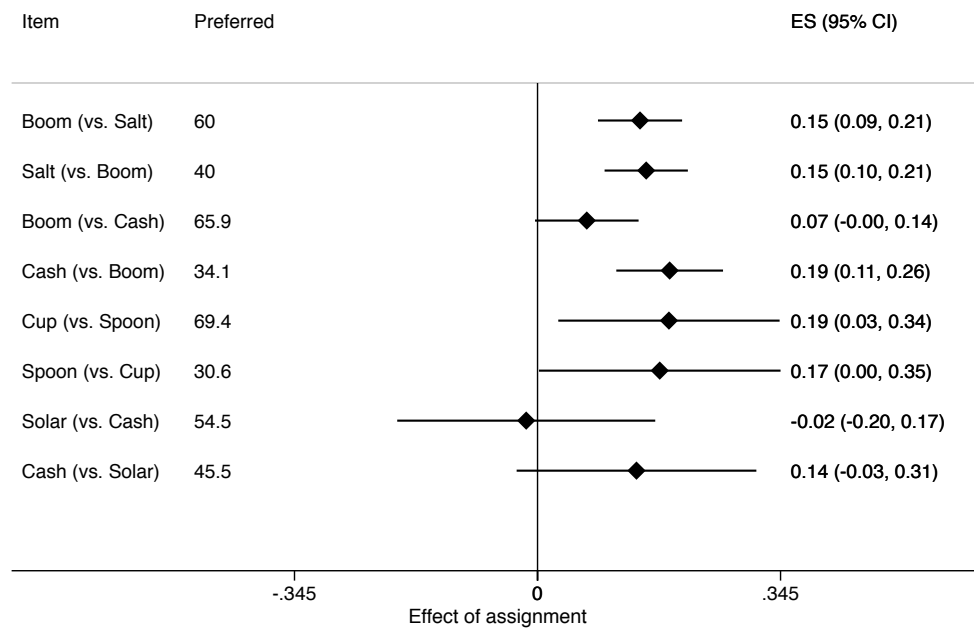


Figure 1: Asymmetries by item preference

Notes: Estimated change in the likelihood of ending with the assigned item as a result of trading, relative to the *choice* condition. *Preferred* column shows the percentage of subjects in the *choice* condition who prefer item to the alternative item. The last column shows the estimated coefficient along with the 95% confidence interval.

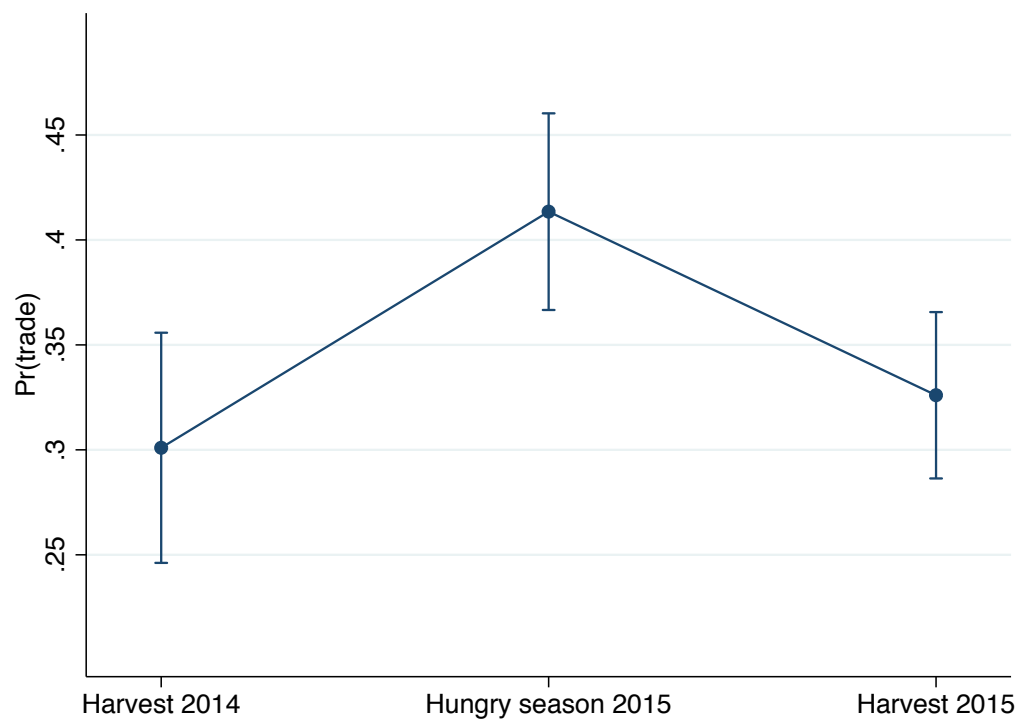


Figure 2: Probability of trading start item, by season

Notes: Relationship between season of survey and trading probabilities, conditional on individual experience with the trading decision. Analysis is 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.

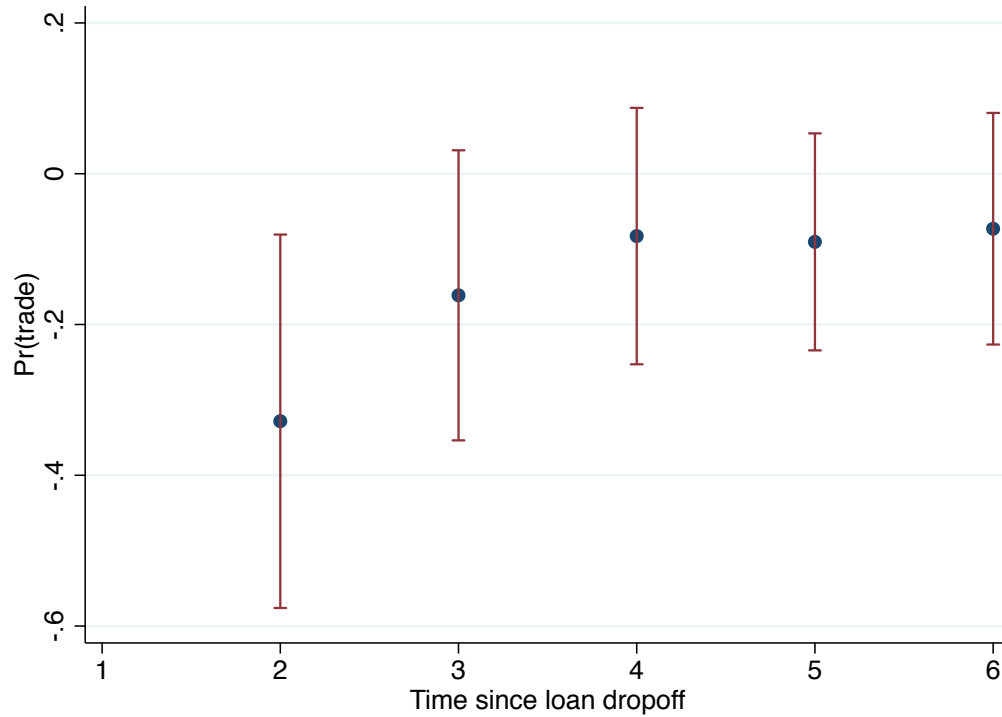


Figure 3: Relationship between weeks since loan receipt and trading probabilities

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 and results are conditional on week of survey fixed effects, and a full set of procedure, experience and item pair indicators and individual and household controls. 95% confidence intervals are based on standard errors clustered at the village level.

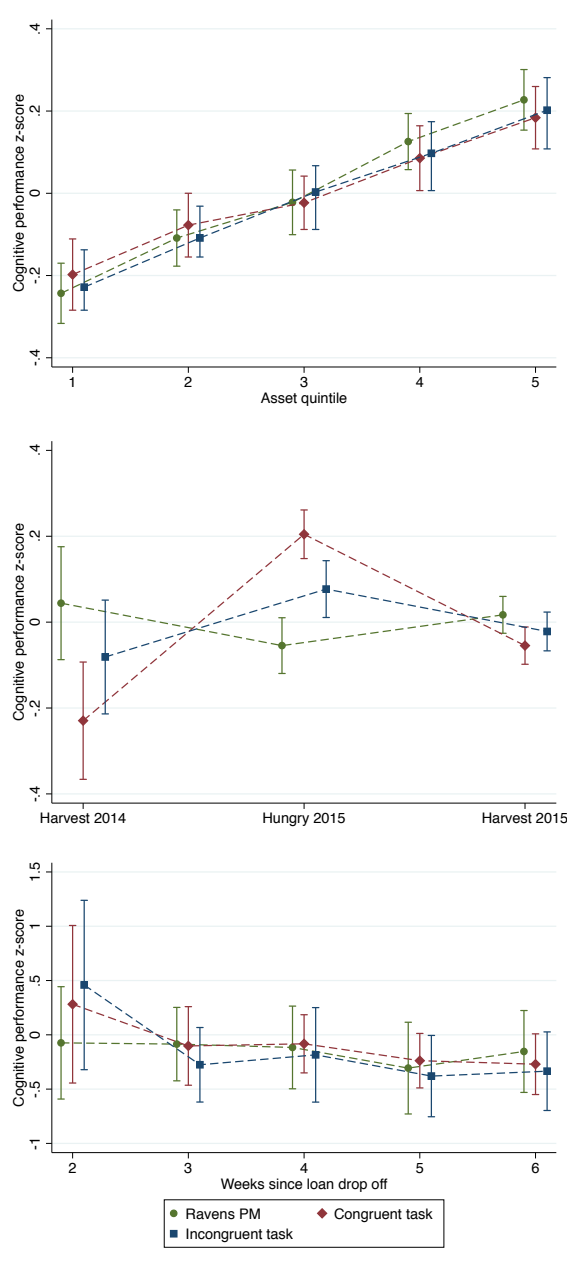


Figure 4: Relationship between scarcity measures and cognitive performance

Notes: Each 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) and is estimated using individual fixed effects. The bottom figure uses time since loan drop off, measured in weeks. The omitted category is the control (no loan) group and results are conditioned on week of survey fixed effects. All analyses control for individual and household characteristics (or individual fixed effects), season, and participant experience with the cognitive tests. 95% confidence intervals are based on standard errors clustered at the village level.

Table 1: Experimental Setup: Scripts and Sub-treatments

Item pair	Procedure	Round 1: Post Harvest 2014	Round 2: Hungry Season 2015	Round 3: Post Harvest 2015	Total
Boom vs. Salt					
	Free Choice	141	85	108	
	Assigned	416	318	0	
	Lottery	242	276	376	
	Timing	0	0	172	
	Voucher	0	0	190	
	Timing + Voucher	0	0	169	
	Expectations	0	0	273	
					2766
Boom vs. Cash					
	Free Choice	0	58	127	
	Assigned	0	302	0	
	Lottery	0	328	391	
	Timing	0	0	179	
	Voucher	0	0	182	
	Timing + Voucher	0	0	172	
	Expectations	0	0	223	
					1962
Cup vs. Spoon					
	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	
					714
Cash vs. Solar					
	Free Choice	0	0	33	
	Assigned	0	0	0	
	Lottery	0	0	169	
	Timing	0	0	0	
	Voucher	0	0	0	
	Timing + Voucher	0	0	198	
	Expectations	0	0	0	
					400
Total		1513	1367	2962	5842

Notes: Summary of randomly assigned item pairs and experimental procedures, by survey round. See text for additional details.

Table 2: Descriptive statistics by item pair

Boom-Salt		N = 2766				
Choice condition			End item		Overall	
Pr(chosen)		Start item	Boom	Salt	Pr(trade)	0.34
Boom	0.60	Boom	934	315	p-val (H0)	0.00
Salt	0.40	Salt	514	669		
Boom-Cash		N = 1962				
Choice condition			End item		Overall	
Pr(chosen)		Start item	Boom	Cash	Pr(trade)	0.36
Boom	0.66	Boom	701	260	p-val (H0)	0.00
Cash	0.34	Cash	385	431		
Cup-Spoon		N = 564				
Choice condition			End item		Overall	
Pr(chosen)			Cup	Spoon	Pr(trade)	0.3
Cup	0.75	Cup	286	42	p-val (H0)	0.00
Spoon	0.25	Spoon	135	133		
Cash-Solar		N = 400				
Choice condition			End item		Overall	
Pr(chosen)		Start item	Cash	Solar	Pr(trade)	0.44
Cash	0.45	Cash	97	66	p-val (H0)	0.08
Solar	0.55	Solar	96	108		

Notes: Summary of choice outcomes by item pair. The Pr(chosen) tabulation shows the likelihood that each item in the pair was selected when subjects were given a free choice. Start item and end item tabulates the frequency that subjects started and ended with each item in the pair in one of the trading decisions. The overall probability that a subject traded the item he or she started with and the p-value from a test that the trading probability is equal to 0.5 is presented in the final column (with standard errors clustered at the village level).

Table 3: Probability of trading start item, by item pair and experimental procedure

	Probability that subject traded start item			
	(1)	(2)	(3)	(4)
Panel A. By item pair				
Boom-Cash	0.022 (0.016)	0.004 (0.017)	0.006 (0.016)	0.007 (0.016)
Cup-Spoon	-0.044* (0.024)	0.004 (0.031)	0.001 (0.030)	0.002 (0.030)
Solar-Cash	0.101*** (0.033)	0.095*** (0.034)	0.096*** (0.035)	0.094** (0.037)
Panel B. By experimental procedure				
Lottery	0.005 (0.019)	-0.007 (0.021)	-0.015 (0.021)	-0.015 (0.021)
Timing	-0.007 (0.030)	-0.024 (0.034)	-0.028 (0.035)	-0.012 (0.035)
Voucher	0.036 (0.033)	0.019 (0.039)	0.015 (0.039)	0.032 (0.039)
Timing + Voucher	0.065** (0.026)	0.048 (0.031)	0.043 (0.030)	0.026 (0.030)
Wording	0.012 (0.033)	-0.040 (0.035)	-0.035 (0.035)	-0.035 (0.035)
Expectations	0.018 (0.026)	0.001 (0.033)	-0.006 (0.033)	0.010 (0.034)
Controls	none	round	round + hh	round + hh + items/ procedures
Observations	5172	5172	5171	5171

Notes: Linear regressions of an indicator for whether the subject traded the start item, by item pair (Panel A) and experimental procedure (Panel B). The omitted category in Panel A is Boom-Salt and in Panel B is assignment. Each column adds control variables. See text for further discussion.

Table 4: Probability of trading start item, by season

	Probability that subject traded start item					
	(1)	(2)	(3)	(4)	(5)	(6)
Hungry Season	0.089*** (0.022)	0.096*** (0.022)	0.100*** (0.021)	0.107*** (0.031)	0.121** (0.056)	0.123*** (0.031)
Endline	0.066*** (0.019)	0.074*** (0.019)	0.077*** (0.020)	0.054 (0.033)	0.053 (0.077)	0.062 (0.041)
Controls	none	hh	hh + experience	hh + experience + procedure	experience + procedure + FE	hh + experience + procedure
Observations	5172	5171	5171	5171	5172	2987

Notes: Linear regressions of an indicator for whether the subject traded the start item, by season. Individual fixed effects are included in column 5. Column 6 excludes households with past experience with the exchange experiment from each round. Standard errors clustered at the village level.

Table 5: Probability of trading start item, by loan delivery

	Probability that subject traded start item					
	(1)	(2)	(3)	(4)	(5)	(6)
Loan	-0.004 (0.028)	-0.001 (0.028)	0.025 (0.030)	0.021 (0.031)		
Recent loan (1 month)			-0.109** (0.050)	-0.106* (0.058)		
Loan 2 weeks ago					-0.254** (0.100)	-0.328** (0.126)
Loan 3 weeks ago					-0.125* (0.065)	-0.161 (0.098)
Loan 4 weeks ago					-0.103 (0.082)	-0.083 (0.087)
Loan 5 weeks ago					-0.091 (0.059)	-0.090 (0.073)
Loan 6 weeks ago					-0.042 (0.072)	-0.073 (0.078)
Loan 7 weeks ago					-0.027 (0.073)	-0.045 (0.082)
Loan 8 weeks ago					-0.111 (0.081)	-0.149 (0.091)
Controls		hh + experience + procedure + items		hh + experience + procedure + items	survey week	hh + experience + procedure + items + survey week
Observations	1224	1224	1224	1224	1224	1224

Notes: Linear regressions of an indicator for whether the subject traded the start item on loan treatment variables. Loan treatment equals one if the household was in a loan treatment village. Recent loan is the additional effect if the loan was received in the past month. Columns 5 and 6 estimate separate effects by week since loan dropoff, conditional on week of survey. See text for further discussion.

Table 6: Probability of trading start item, high value treatment

	Probability that subject ends with solar lamp Cash/Solar high stakes treatment only			
	(1)	(2)	(3)	(4)
Start item = solar lamp	0.101* (0.059)	0.087 (0.059)	0.102* (0.059)	0.088 (0.059)
Voucher assignment			-0.043 (0.050)	-0.028 (0.049)
Controls	none	hh + experience	none	hh + experience
Observations	400	400	400	400

Notes: Linear regressions of an indicator for whether the subject ends the procedure with a solar lamp, restricted to the high stakes treatment (Cash-Solar). Column 1 includes an indicator for starting with the solar lamp. Columns 2 and 4 add household and experience controls. Column 3 and 4 add an indicator for whether a voucher was used instead of the actual item. See text for further discussion.

Table 7: Cognitive ability and probability of trading

Cognitive measure:	Probability that subject traded start item			
	Ravens	Stroop	Stroop	Stroop
	score	task 1	task 2	task 3
	(1)	(2)	(3)	(4)
Panel A: Pooled OLS				
Cognitive measure	-0.006 (0.007)	0.001 (0.008)	-0.003 (0.008)	-0.003 (0.009)
Panel B: Individual fixed effects				
Cognitive measure	0.006 (0.015)	0.050** (0.020)	0.036* (0.019)	0.024 (0.019)
Observations	4280	4039	4082	4063

Notes: Linear regressions of an indicator for whether the subject traded the start 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. Standard errors clustered at the village level. All regressions control for item pairs, experimental procedures and experience with both trading and the cognitive test. Panel A also controls for household and individual characteristics.

Appendix to
Poverty, Seasonal Scarcity and Exchange Asymmetries

A.1 Appendix: Tables and figures

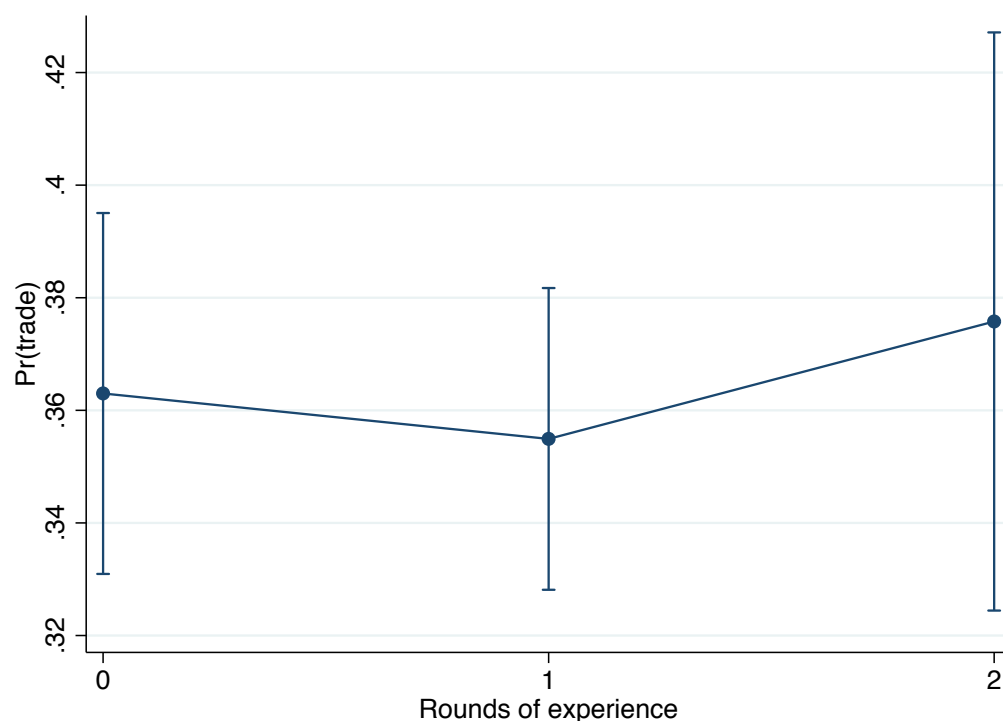


Figure A.1: Probability of trading start item in Boom – Salt pair, by rounds of participant experience

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.

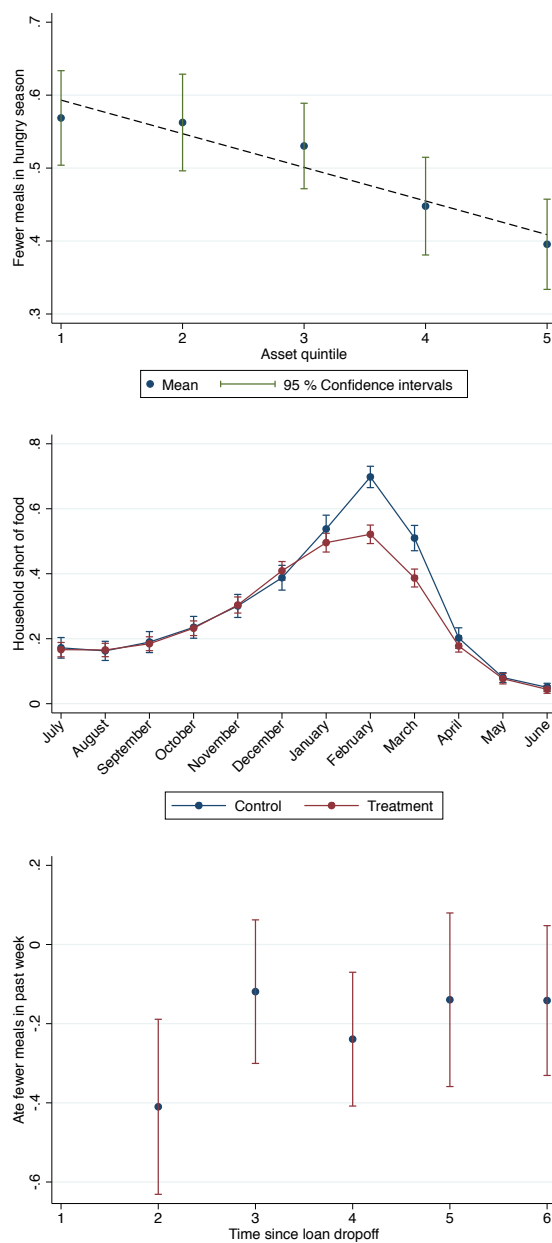


Figure A.2: Variation in consumption and food availability by source of variation in scarcity

Notes: Consumption and food availability measures as a function of different sources of variation in scarcity. The top figure uses baseline variation in assets. 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, measured in weeks. The omitted category is the control (no loan) group and results are conditioned on week of survey fixed effects.

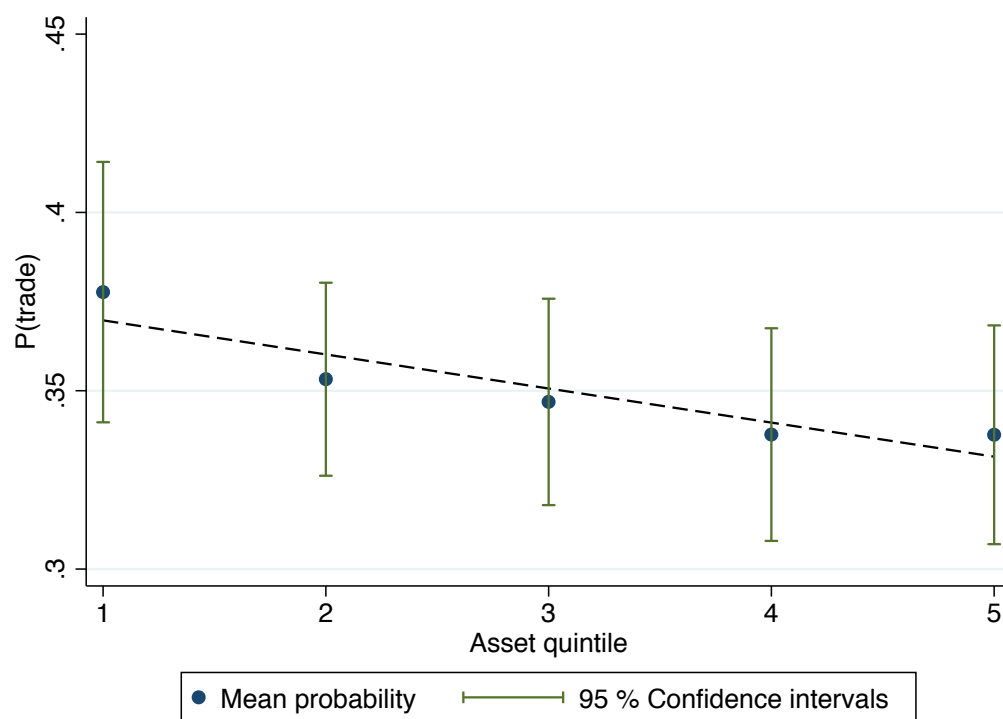


Figure A.3: Probability of trading start item by baseline assets

Notes: Trading probability by quintile of the household asset distribution. 95% confidence intervals are based on standard errors clustered at the village level. 95% confidence intervals are based on standard errors clustered at the village level.

Table A.1: Balance: Rounds

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

Notes: Means and standard deviations of baseline variables for the Round 1 sample shown in column 1. Columns 2-4 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.2: Balance: Item pairs

	Boom-Salt (1)	Boom-Cash (2)	Cup-Spoon (3)	Solar-Cash (4)
Age of hh head	42.69 [14.95]	0.48 (0.95)	-0.83 (-1.13)	0.08 (0.08)
Female headed hh	0.25 [0.43]	0.01 (0.99)	-0.03 (-1.63)	-0.01 (-0.61)
Children under 5	0.94 [0.90]	-0.03 (-1.15)	0.07 (1.71)	0.03 (0.37)
Children 5-14	1.77 [1.51]	0.03 (0.56)	0.02 (0.30)	-0.00 (-0.02)
Adults 15-64	2.43 [1.23]	0.06 (1.54)	0.06 (1.13)	0.12 (1.77)
Adults over 64	0.17 [0.45]	0.01 (0.92)	-0.01 (-0.57)	0.02 (0.83)
Baseline assets	3.05 [1.42]	-0.06 (-1.03)	-0.09 (-1.23)	-0.04 (-0.32)
Baseline harvest value	3142.76 [2803.15]	-140.74 (-1.03)	45.85 (0.29)	-54.78 (-0.25)
Female respondent	0.33 [0.47]	0.03 (2.13)	-0.06 (-2.67)	-0.02 (-0.78)
Respondent age	43.55 [15.10]	0.46 (0.92)	-0.30 (-0.42)	0.58 (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.3: 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 [15.33]	-0.56 (-0.69)	-0.39 (-0.51)	0.15 (0.18)	0.14 (0.15)	-0.11 (-0.11)	-0.79 (-0.78)	-0.61 (-0.49)
Female headed hh	0.25 [0.43]	0.01 (0.38)	-0.00 (-0.25)	0.04 (1.62)	0.03 (1.18)	0.04 (1.63)	-0.00 (-0.01)	0.08 (2.15)
Children under 5	0.93 [1.01]	0.03 (0.74)	0.00 (0.11)	0.06 (1.07)	-0.01 (-0.22)	0.03 (0.59)	0.03 (0.45)	-0.08 (-1.20)
Children 5-14	1.78 [1.55]	0.02 (0.22)	0.01 (0.13)	-0.00 (-0.03)	-0.01 (-0.14)	-0.02 (-0.18)	-0.06 (-0.66)	-0.01 (-0.08)
Adults 15-64	2.39 [1.24]	0.08 (1.36)	0.09 (1.48)	0.11 (1.67)	0.06 (0.91)	0.06 (0.81)	0.04 (0.53)	0.04 (0.41)
Adults over 64	0.19 [0.46]	-0.03 (-1.19)	-0.03 (-1.08)	-0.02 (-0.79)	0.00 (0.03)	0.00 (0.02)	-0.01 (-0.18)	-0.02 (-0.67)
Baseline assets	3.03 [1.39]	0.03 (0.39)	-0.03 (-0.41)	0.00 (0.02)	-0.04 (-0.51)	-0.05 (-0.52)	-0.02 (-0.27)	-0.04 (-0.34)
Baseline harvest value	3137.25 [2605.25]	-25.17 (-0.17)	-56.63 (-0.41)	-39.10 (-0.25)	-155.35 (-1.00)	-165.80 (-0.87)	7.46 (0.04)	-77.68 (-0.33)
Female respondent	0.32 [0.47]	0.04 (1.47)	0.02 (0.94)	0.00 (0.13)	0.01 (0.35)	0.02 (0.64)	0.00 (0.02)	0.16 (4.54)
Respondent age	44.02 [15.28]	-0.77 (-0.96)	-0.43 (-0.58)	0.02 (0.03)	0.55 (0.64)	-0.09 (-0.09)	-0.95 (-0.92)	-1.22 (-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.4: Item-specific asymmetries

Item A Item B	Probability that subject ended with item A							
	Boom Salt (1)	Salt Boom (2)	Boom Cash (3)	Cash Boom (4)	Cup Spoon (5)	Spoon Cup (6)	Solar Cash (7)	Cash Solar (8)
Assigned item A	0.149*** (0.029)	0.164*** (0.030)	0.070* (0.038)	0.188*** (0.039)	0.126* (0.066)	0.242*** (0.073)	-0.016 (0.093)	0.141 (0.087)
Constant	0.599*** (0.027)	0.401*** (0.027)	0.659*** (0.035)	0.341*** (0.035)	0.746*** (0.063)	0.254*** (0.063)	0.545*** (0.092)	0.455*** (0.092)
Observations	1583	1517	1146	1001	446	386	237	196

Notes: Linear regressions of an indicator for whether the subject ended up with the item rather than the alternative. Regressions in each column are restricted to experiments where subjects either were given the choice or assigned the item of interest. The coefficient on the item received captures the additional probability of ending up with the item compared to the free choice condition which is captured in the constant. Standard errors are clustered at the village level.

Table A.5: Stock of the start item in the home

	Probability that subject traded start item				
	(1)	(2)	(3)	(4)	(5)
Stock of start item	0.018*** (0.006)	0.014*** (0.005)	0.014*** (0.005)	0.020*** (0.006)	0.020*** (0.006)
Controls	none	round	round + hh	round + hh + procedure	round + hh + procedure
Observations	3008	3008	3007	3007	3007

Notes: Rounds 1 and 3 only. Linear regressions of an indicator for whether the subject traded the start item on the log measured stock of the start item in the household. Each column adds control variables. Standard errors clustered at the village level.

Table A.6: Social desirability bias

	Probability that subject traded start item				
	(1)	(2)	(3)	(4)	(5)
Social desirability bias score	0.002 (0.003)	0.002 (0.003)	0.002 (0.004)	0.002 (0.004)	0.002 (0.004)
Controls	none	round	round + hh	round + hh + items	round + hh + items + procedure
Observations	3906	3906	3905	3905	3905

Notes: Linear regressions of an indicator for whether the subject traded the start item on a continuous measure of social desirability bias. Each column adds control variables. See text for further discussion.

Table A.7: Seasonal variation in cognitive ability

	Ravens score	Stroop task 1	Stroop task 2	Stroop task 3
	(1)	(2)	(3)	(4)
Panel A: Pooled OLS				
Hungry Season	-0.099* (0.056)	0.297*** (0.051)	0.373*** (0.055)	0.091* (0.052)
Endline	-0.135** (0.055)	0.019 (0.054)	0.031 (0.052)	-0.115** (0.048)
Test experience	0.110*** (0.025)	0.206*** (0.027)	0.177*** (0.026)	0.150*** (0.026)
Panel B: Individual fixed effects				
Hungry Season	-0.062 (0.066)	0.362*** (0.067)	0.405*** (0.071)	0.149** (0.068)
Endline	-0.045 (0.093)	0.190 (0.117)	0.201* (0.111)	0.118 (0.103)
Test experience	0.031 (0.050)	0.086 (0.066)	0.037 (0.062)	-0.026 (0.061)
Observations	4771	4503	4549	4529

Notes: Linear regressions of cognition scores on season. All outcomes are normalized Z-scores where a higher score indicates better performance. Analysis is restricted to a subsample of participants who completed both Raven's and Stroop tests. Test experience indicates the respondent was in a previous round of cognition testing. Standard errors clustered at the village level.

Table A.8: Probability of trading start item, by access to local trade

	Probability that subject traded start item				
	(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	5171	5171	5171	4953	4953

Notes: Linear regressions of an indicator for whether the subject traded the start 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 village size corresponds to the bottom quartile of villages, while the indicators for distance correspond to above median distances. Columns 4-5 show the effect of within village trading opportunities, conditional on village size. All columns include the full set of controls (round, household, experience, procedure and item pair) and cluster standard errors at the village level. See text for further detail.

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

In survey round 2 and 3 in the item pairs involving cash, we elicited participants' (hypothetical) willingness to pay (WTP) and willingness to accept (WTA) after they made their decision. This allows us to obtain a lower and upper bound on participants' actual valuation of items in these two item pairs. More precisely, we presented participants whose start item was Boom (Solar) a decreasing sequence of hypothetical prices if they traded Boom (Solar) for cash and an increasing sequence of prices if they kept Boom (Solar). In both cases they had to state the lowest price for which they would have changed their decision (WTA). Analogously, participants assigned cash either faced a decreasing sequence of prices (if they kept cash) or an increasing sequence of prices (if they traded cash for Boom or Solar). In both cases they had to state their maximum willingness to pay for Boom or Solar (WTP). We assume 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).

In Table A.9 we estimate the impact of item assignment on participants' hypothetical WTP/WTA. The regressions show two key results. First, we observe that the constant in columns 1 and 3 (no other controls), which reflects participants' willingness to pay for an item, is close to its market price (i.e., 3-3.5 Kwacha for Boom and 80 Kwacha for the solar lamp). Second, the initial assignment matters for the low-value item (Boom) but not for the high value item (Solar). Specifically, respondents initially endowed with Boom require a significantly higher price to part with their item. The estimated differences between a participant's WTA and WTP is on average 1.5 Kwacha, which corresponds to an increase of about 50 percent of the average WTP of participants who are initially assigned cash. This finding is in contrast to the results for the high-value item. Participants who are initially endowed with the solar light do not display a higher WTA. In fact, their WTA is approximately the same as the WTP of participants starting with cash. Results are similar when we condition on all controls (columns 2 and 4).

Table A.9: Willingness to pay/accept

	Willingness to pay/accept for			
	Boom		Solar	
	(1)	(2)	(3)	(4)
Start item: Boom	1.520*** (0.094)	1.522*** (0.093)		
Start item: Solar			-0.383 (9.446)	-1.253 (9.116)
Controls		hh + round + experience + procedure		hh + experience + procedure
Observations	1777	1777	259	259

Notes: Censored normal regression of reported willingness to pay or accept for item. After the final item selection, subjects with the item were asked a series of questions to elicit their willingness to accept for the items. Subjects choosing cash were asked a series of questions to elicit their willingness to pay for the item in question. In some cases, WTA and WTP values were outside of the designed brackets. Censored normal regression models were used to account for the censored nature of these observations. All prices are in Zambian Kwacha.

A.3 Appendix: Assessments of cognitive and executive functioning

We use two commonly used measures for cognitive function: the Stroop test and Raven's Progressive Matrices (see Dean, Schilbach, and Schofield, 2017, for an overview of cognitive functions and tools to measure them). 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 more broadly.

To assess basic cognitive functioning, we also 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 respondent. 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.4.

For the analysis, we used a two-parameter logistic model (2PL) to construct a single score for each participant. Internal consistency of the 10 item scale is 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 both scores to mean zero and standard deviation 1. Appendix Figure A.5 illustrates the overall distribution of the scores.

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.6 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). Following the scoring modalities outlined in Stroop (1935), an error-corrected score can be calculated as the total time plus the number of mistakes times the penalty. The penalty for each incorrect question proposed by Stroop is twice the median time needed for each row (1.8 seconds in our sample), and the median number of mistakes was 1 in the neutral condition, 0 in the congruent condition, and 2 in the incongruent stimuli condition. To compute participants’ ability to control interference, we deducted the error-corrected time for completing the neutral task from the error-corrected time for completing the incongruent task. To facilitate interpretation, we normalized all four 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 for each 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.7 shows the correlation between total time needed for this task and the inhibitory-control score. The correlation between task time and inhibitory-control score is -0.38 in our sample.

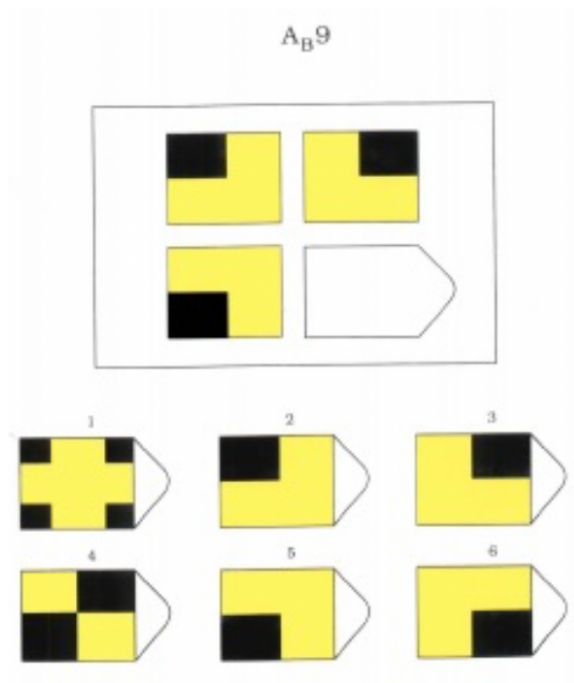


Figure A.4: Ravens Matrices: Sample decision task

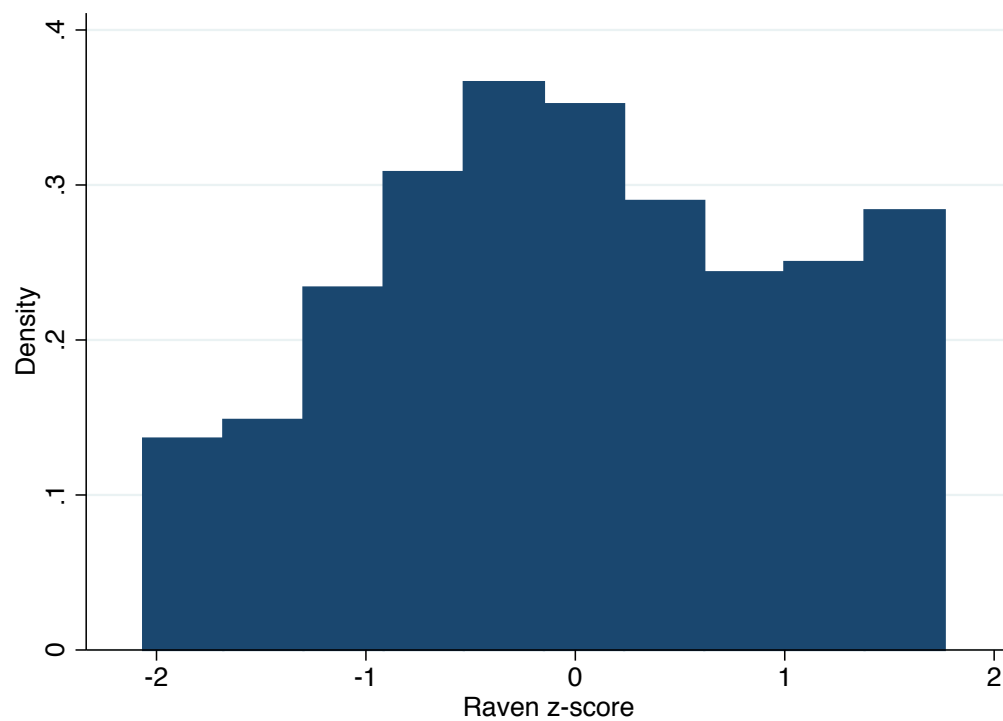


Figure A.5: Distribution of scores in the Raven's Progressive Matrices (RPM)

Congruent Task 1

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

Congruent Task 2

5	1
6	3 3 3
7	5 5 5 5
8	1

Incongruent Task

9	1 1 1 1 1 1
10	6 6
11	2 2 2 2 2
12	4 4 4 4 4 4

Figure A.6: Stroop: Sample decision tasks

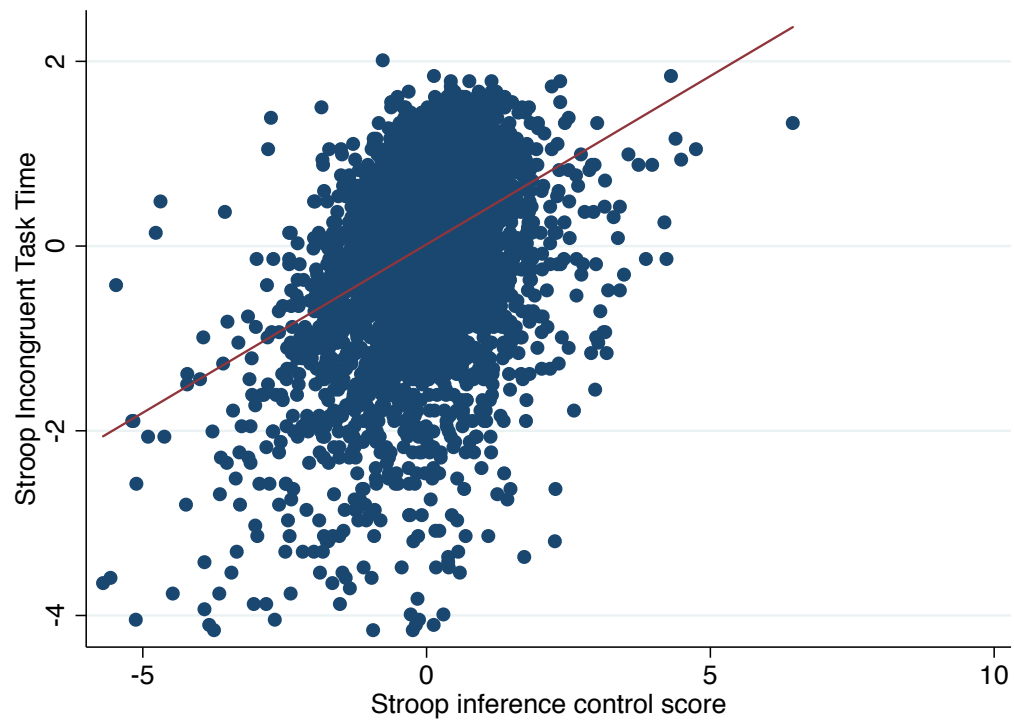


Figure A.7: Correlation between time needed in the incongruent task 3 and the inhibitory-control score (both coded as z-scores)

A.4 Appendix: Scripts and Protocols

A.4.1 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 1) READ and 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 & timing same as above, except last paragraph, which says 1) READ [once participants has drawn the button]: You have drawn a white button, so you get \${first_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]?