Poor and Rational: Decision-Making under Scarcity*

Dietmar Fehr† Günther Fink‡ B. Kelsey Jack§

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Abstract

A growing literature associates poverty with anomalies in decision-making. We investigate this link in a sample of over 3,000 small-scale farmers in Zambia, who were given the opportunity to exchange randomly assigned household items for alternative items of similar value. Analyzing a total of 5,842 trading decisions over a range of items, including cash, 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 variation in decision stakes drive our results, 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

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†University of Heidelberg, *dietmar.fehr@awi.uni-heidelberg.de*

‡Swiss Tropical and Public Health Institute & University of Basel, *guenther.fink@swisstph.ch*

§UC Santa Barbara, *kelseyjack@ucsb.edu*

1 Introduction

A substantial body of evidence documents that individual decision-making is prone to behavioral biases and deviations from normative rationality (e.g., Camerer et al., 2003; DellaVigna, 2009), and that such decision anomalies may be particularly pronounced among the poor (e.g., Duflo, 2006; Mullainathan, 2007; Haushofer and Fehr, 2014). However, the relationship between poverty and decision-making is not obvious. On the one hand, scarce financial resources make the same decisions more consequential. This may help focus attention, minimize mistakes, and improve decision quality (Goldin and Homonoff, 2013; Shah et al., 2015; Maćkowiak et al., 2018; Gabaix, 2019). On the other hand, a lack of financial resources may also affect decision-making if an increased focus on financial matters absorbs scarce cognitive bandwidth (Mani et al., 2013; Mullainathan and Shafir, 2013). In spite of potentially wide-spread implications, causal evidence on both how and why the availability of financial resources affects decision-making is largely missing.

In this paper, we use multiple sources of variation in households' financial constraints – "scarcity" – to show that scarcity improves decision-making in a real-stakes decision with immediate payoffs and that these decision patterns can be explained by decision stakes: when households are poorer, the same decision is more consequential and so decisions move closer to the normative benchmark. Our evidence comes from decision experiments with 3,059 small-scale farmers in rural Zambia over a period of 14 months. We focus on behavior in one of the most basic economic decisions: the exchange of goods. A voluminous literature documents that individuals tend to place greater value on goods they own than on identical goods they do not own. The resulting gap between willingness to pay and willingness to accept is commonly referred to as the "endowment effect".[1] This finding has contributed to the development of theories of reference-dependent preferences (see Ericson and Fuster, 2014, for a review of the literature) and has implications for a broad range of economic decisions including homeownership, worker effort, technology adoption, migration choices and investment (e.g., Genesove and Mayer, 2001; Hossain and List, 2012; Liu, 2013; Clark and Lisowski, 2017; Anagol et al., 2018). In addition to its status in the pantheon of behavioral biases, measuring the endowment effect is well suited to our study objectives: it provides a naturalistic vehicle for observing real decisions that incur few other costs other than cognitive or attentional ones.

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

Our decision experiments were embedded in an ongoing randomized controlled trial on credit access and labor supply that involved repeated surveys over multiple years (see Fink, Jack, and Masiye, 2018). This setting facilitates a research design that combines repeated observation of precise decision measures with credible variation in resource scarcity to identify the link between scarcity and decision-making. As part of the ongoing surveys, households received a small item as compensation for their time at the end of a survey. We modified this standard procedure by randomly endowing participants with one of two roughly equally-valued items midway through a survey. The items were common household necessities of substantial value, worth about 1/5th of the daily agricultural wage. At the end of the survey, participants were offered the opportunity to trade the endowed item for the alternative item. With random assignment of the initial item and near-zero trading costs, neoclassical theory predicts that, for any distribution of preferences, half of participants will prefer the alternative item and choose to trade.[2]

We present two sets of results. First, in over 5,400 trading decisions, we find strong and robust evidence for the existence of exchange asymmetries (i.e., trading rates below 50 percent) in our sample of rural farmers. On average, across all item pairs, some of which involved cash, only 35 percent of participants traded the endowed item. This applies to subjectively "inferior" items as well as more popular items, i.e., items that are preferred by a majority of participants when given a choice. We further rule out indifference between items as an explanation by eliciting a valuation gap for trades involving cash. Endowing a respondent with a household item instead of cash leads to an almost 50 percent higher stated value for the item. Taking advantage of the control offered by our setting, we also examine an extensive range of possible confounds. Following Plott and Zeiler (2007), we randomly vary experimental procedures to test whether our results are driven by design features such as the assignment procedure, participants' attachment to the endowed item (duration of initial assignment), expectations regarding future trading opportunities, and social cues, such as norms and experimenter demand. None of these procedural variants had a statistically or economically significant impact on the measured exchange asymmetries.

Second, to investigate the relationship between scarcity and decision-making, we capitalize on three different sources of variation in resource constraints in our setting: (1) we exploit

[2]Knetsch (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.

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 (the "hungry season") and after the 2015 harvest, and (3) we leverage village-level variation in the timing and availability of small consumption loans during the 2015 hungry season. All three sources of variation are substantial and predictive of households' consumption levels and living conditions.

Across all three sources of variation, we find the same pattern: greater scarcity is associated with reduced exchange asymmetries. First, households in the bottom asset quintile are about 5 percentage points more likely to trade than households in the top quintile. Second, households in our setting receive most of their annual income at harvest time and face a pronounced period of scarcity in the period (the "hungry season") leading up to the next harvest, when household grain and cash reserves are depleted and consumption levels fall. Using this seasonality in financial resource constraints, we find that trading probabilities are 7 to 12 percentage points higher during the hungry season than at harvest time. Third, we find that households without access to the randomized hungry season loan intervention are about 18 percentage points more likely to trade than households that received a loan of either grain or cash in the 3 weeks prior to the decision experiment.[3] Taken together, the consistency of results across these three analyses address potential concerns about any one of them taken in isolation and provide robust evidence of an improvement in decision-making when households are more financially constrained.

For all three sources of variation, fewer resources at the time of the decision can be interpreted as an increase in decision stakes. Lower wealth or income is associated with a higher marginal utility of consumption and also with greater difficulty obtaining the alternative item or reversing the experimental decision in local markets. The utility difference between items in a pair should thus be higher when resources are scarce (such as during the hungry season) than when abundant (such as after harvest). Our finding of improved trading behavior under scarcity is consistent with multiple mechanisms. First, rational inattention models suggest that individuals only expend costly cognitive and mental resources in a given decision when stakes are sufficiently high.[4] Second, the Mullainathan and Shafir (2013) notion of "tunneling" predicts that conditions

[3]The consumption loans provided households with three bags of maize or the cash value equivalent at the start of the hungry season. Repayment was due at harvest time, approximately six months later, with 5 percent monthly interest. Around 90 percent of eligible households took up the loan and around 80 percent fully repaid in the year that the exchange experiments were conducted. Further details are provided in Section 5.3 and in Fink et al. (2018).

[4]Rational inattention, pioneered by Sims (2003), formalizes the idea that attention is costly and that decision makers choose how much attention to allocate to a decision depending on its importance. Accordingly, in some cases it may be optimal to pay less attention to a decision and to rely on heuristics when decision stakes are low, such as after harvest

of scarcity lead to a focus on immediate financial concerns that absorb cognitive bandwidth and divert attention away from long run or complex decisions.[5] In both cases, greater focus may lead to better decision-making in some domains, such as the trading decision that we study.

To shed light on underlying mechanisms, we first increased the value of the items traded for a subset of participants. Specifically, we offered a choice between two items worth USD 14, which corresponds to about 28 percent of average monthly household income. This manipulation holds scarcity constant (i.e., triggers no additional tunneling effect), while increasing the value of items, making the decision more consequential. The aim of the manipulation is not to distinguish between the two interpretations above, but instead to isolate one feature of the rational inattention explanation: that higher value decisions receive more attention, resulting in better decision-making.[6] Consistent with the predictions of a rational inattention framework, we find that the likelihood of trading increases by about 10 percentage points in the high value treatment relative to the standard items in the same decision round. Strikingly, this reduction in the magnitude of the exchange asymmetry matches the observed reduction in exchange asymmetries in the hungry season, which – taken literally – implies that going from a time of abundance at harvest to a time of scarcity during the hungry season is equivalent to a more than twenty-fold increase in the value of the exchange items.

To investigate the relevance of tunneling for our results, we turn to a key implication of tunneling behavior: a depletion of cognitive bandwidth. Following the literature (e.g., Mani et al., 2013), we implemented a standard set of cognitive and executive function tests in a sub-sample of participants. Even though we see a strong positive cross sectional relationship between wealth (asset quintiles) and measures of cognitive performance, we find no clear relationship between these measures and seasonal or experimental variation in scarcity in our sample. More generally, we find that cognitive scores are not predictive of exchange decisions in our setting, which allows us to also rule out alternative explanations for the relationship between scarcity and decision-making, such as variation in the opportunity cost of time or in alcohol consumption, which should

when households are relatively rich. In other cases it is optimal to pay more attention and deliberate more carefully, in particular when decision stakes are high, such as during the hungry season.

[5] Our results complement correlational evidence that low-income people consistently make decisions closer to normative predictions than high-income people in a host of hypothetical choice scenarios (e.g., Shah, Shafir, and Mullainathan, 2015).

[6] Note that the two interpretations differ in some subtle ways. Tunneling effects result from financial resource constraints (i.e., a situation where decision stakes are constant but resources vary), whereas in rational inattention models the decision to pay more attention depends on stakes (i.e., resources are constant, but decision stakes vary). Our discussion of tunneling behavior versus rational inattention can thus be taken as a question of whether scarcity or stakes drive our main results.

also affect cognitive performance. Overall, we interpret the burden of evidence as more consistent with rational inattention given (a) the similarity between the effect of scarcity on decision-making and the effect of an exogenous increase in the value of the items involved, and (b) the lack of clear evidence that cognitive or executive function is negatively impacted by greater scarcity or is negatively correlated with our measure of decision-making.

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 affect decision-making and behavior through a number of pathways, including that financial concerns absorb the cognitive bandwidth needed for other decisions (Shah et al., 2012; Mani et al., 2013; Mullainathan and Shafir, 2013), that increased prevalence of stress and depression interferes with decision-making or increase biases (Haushofer and Fehr, 2014; Haushofer and Shapiro, 2016), or that the living conditions of the poor contribute to worse decision-making (Dean, 2019; Lichand and Mani, 2019; Schilbach, 2019). To date, few papers have traced effects from exogenous variation in scarcity through to real stakes decisions.[7] We fill that gap and provide evidence that scarcity improves decision-making related to a well-documented behavioral anomaly. In particular, we provide novel evidence that immediate decisions respond similarly to an exogenous increase in stakes and to conditions of greater scarcity, highlighting the importance of decision stakes, in addition to poverty-related circumstances such as stress, living conditions or the opportunity cost of time.

Second, this paper adds to the ongoing debate about the robustness of behavioral anomalies in general (Levitt and List, 2008; Falk and Heckman, 2009; Charness and Fehr, 2015; Camerer, 2015; Kessler and Vesterlund, 2015), and the endowment effect in particular (Ericson and Fuster, 2014). Despite a large literature on the endowment effect, evidence from outside of the laboratory, and particularly from low income settings remains relatively scarce.[8] We present field evidence

[7]Some papers have investigated the link between scarcity and cognition (e.g., Mani et al., 2013; Carvalho et al., 2016; Bartoš et al., 2018), while a separate literature finds that cognitive function is related to preference anomalies, in particular to small-stakes risk aversion and impatient behavior (e.g., Oechssler et al., 2009; Dohmen et al., 2010; Benjamin et al., 2013). However, to date, these literatures have not been linked to show a causal relationship between scarcity and decision-making. Kaur et al. (2019) show that scarcity lowers productivity by varying the timing of casual wage payments in 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.

[8]Some recent work leverages more easily accessible online panels (Chapman et al., 2017; Fehr and Kuebler, 2019). Fehr and Kuebler (2019), for example, provide evidence on small-stakes exchange asymmetries in a representative German sample and show that trading behavior correlates with migration choices and stock market participation.

involving transactions that are sufficiently large to have a meaningful impact on household well-being. In this way, our work relates to an influential series of experiments at sport cards shows in the United States demonstrating the relationship between the endowment effect and market experience (List, 2003, 2004). On average, trading rates in 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). 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. In contrast with these papers, we find no evidence that experience with trading or access to outside markets affects trading probabilities. That said, our findings similarly highlight that economic circumstances play an important role in shaping trading behavior.

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). While these studies show that the endowment effect is not just an artifact of laboratory decision-making, they increase policy relevance at some expense to precision. Our study bridges the lab and field literatures by studying decision-making in the field over a sufficiently long time horizon to investigate the effect of both existing and induced sources of variation in financial resources on real-stakes decisions without sacrificing the control or attention to mechanisms of laboratory studies. Notably, our results indicate that the endowment effect varies in predictable ways depending on economic circumstances, and is substantially less pronounced when financial resources are more scarce.

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 randomized evaluation). As part of the evaluation, households were surveyed up to four times per year. In the first year of the study, all farmers received a small box of commonly used washing powder (called "Boom" 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.[9] 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.[10] 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 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.[11] The main goal of the *lottery assignment* is to minimize the risk of possible inference about the relative valuation of items or signaling by the experimenter associated with the *standard assignment*.[12]

Second, we implemented three sub-procedures designed to reduce participants' attachment to the assigned item: (i) we shortened the time between the 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).

[9]Note that survey respondents sometimes changed across survey rounds, and also included other adult household members. Priority was given to surveying the household head or the respondent in prior survey rounds; when that person was unavailable, the spouse or another adult permanent member of the household was surveyed instead. We use both the respondent ID and the household ID to examine both within-household and within-subject variation in decision-making over time.

[10]See appendix for the exact wording of all procedures.

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

[12]For example, if the randomization is 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.

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...").[13]

Our default item pair, implemented across all survey rounds and all procedures consisted of a package (250g) of washing powder ("Boom") and a package (500g) of table salt (Boom – Salt). Both items are household staples with a local price of 3-3.5 Kwacha (USD 0.50), 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. 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 leverage the variations in financial resources in our setting, 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

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

level.[14]

Experiment round 1 (harvest season 2014): The first round took place after harvest in 2014, and ran from July through September. We randomly selected 105 villages and 1,513 households, covering approximately 58 percent of the total study population, to participate in the 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) in the Boom – Cash item pair after they made their decision (see Appendix section A.2 for more details). Loans were disbursed in randomly selected villages as part of the project described in Fink et al. (2018) 2-8 weeks prior to the start of experiment round 2.

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/WTA from households that were randomized to the Boom – Cash and Solar – Cash item pairs.

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

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:

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

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 for individual i in village v and round t:

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

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:

$$\Pr(endA)_{ivt} = \alpha + \beta startA_{ivt} + \varepsilon_{ivt} \tag{3}$$

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 survey rounds to test how seasonal variation in scarcity affects trading probabilities, conditional on experience. Specifically, we estimate:

$$p(trade)_{ivt} = \alpha + \beta N_{it} + \rho R_t + X_i\delta + \varepsilon_{ivt} \tag{4}$$

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.

Finally, we exploit village-level variation in loan access by estimating

$$p(trade)_{iv} = \alpha + \sum_{w=1}^{4} \beta_w dropoff_{w,iv} + \sigma_t + \xi_c + X_i\delta + \varepsilon_{iv}, \tag{5}$$

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

Note that the variation in $dropoff_w$ – the time, in two week intervals, between loan disbursement and data collection among treated households – is not truly random. Even though the survey week, σ_t, was determined largely by random assignment of villages to survey month, the exact timing of loan delivery was left to the implementation team (within a 10-day window).[15]

[15]To accommodate survey logistics, each village was randomly assigned to be surveyed in a particular month. Within the month, this results in balance across treatment arms. Within a month, villages within a geographic block were more likely to be surveyed in the same week. The original treatment assignment was stratified on geographic block, so by including geographic fixed effects, identification of treatment coefficients within a dropoff window is driven by (randomized) treatment-control comparisons, while identification of differences across dropoff windows is driven

We show that variation in $dropoff_w$ is balanced on observables in Appendix Table A.4. We regress observables on indicators for time since loan dropoff, controlling for survey week and fine-scale geographic controls. F-statistics for a test that all $dropoff_w$ coefficients are jointly equal to zero is reported in the final column. All baseline covariates are balanced across dropoff timing, with the exception of the number of children between 5 and 14 years of age in the home. Coefficients on children 5-14 (relative to the control) show a non-monotonic pattern as the time since loan dropoff increases, with similar treatment coefficients in the first and last time bins.

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 (v) throughout,[16] 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, while household characteristics remain balanced.

4 Results: Exchange asymmetries

We begin by documenting general exchange patterns in our sample. 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 preference for each item, absent any endowment. For each item pair, we observe some preference for one of the items (if all respondents were indifferent, we would expect roughly 50 percent to choose each item). Participants had the most imbalanced preferences in the Cup–Spoon treatment, with three quarters of participants preferring a cup over a

by random assignment to survey month, and non-random logistical considerations affecting both the timing of loan dropoff and the timing – within the month – of the survey. As a robustness check, we use only the variation in the time since loan dropoff that comes from the timing of the survey. As a further robustness check, we limit the variation in survey timing to the (randomly assigned) survey months. Our results are robust to these alternative specifications.

[16]The assignment of items pairs and the eligibility for the seasonal loan experiment were both randomized at the village level, and many potential sources of correlated shocks are likely to arise at the village level.

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, we observe that a majority of participants leave our experiment with the item they start with; even for the most inferior item (Spoon), we see that around 50 percent of respondents assigned a Spoon choose to keep it, while only 25 percent selected it in the choice condition.

The table displays the probability that participants traded the item they started with along with a t-test for the theoretical prediction in the last column. Given that we randomize items in each item pair, half of the participants start with their less preferred item and thus should trade for their preferred item resulting in a total trading rate of 50 percent in each item pair. Importantly, this prediction is independent of the preferences for any two items in the study population. That said, randomization in the field did not assign exactly half of participants to each item pair. The share of participants receiving the first item in the item pair was 0.51, 0.54, 0.55 and 0.44 in the Boom – Salt, Boom – Cash, Cup – Spoon and Cash – Solar pairs, respectively. This, combined with the preferences measured in the choice experiment, changes the null slightly in some item pairs, to 0.50, 0.49, 0.47 and 0.49, again in the Boom – Salt, Boom – Cash, Cup – Spoon and Cash – Solar pairs, respectively. This biases us slightly toward finding evidence of exchange asymmetries if we retain a null hypothesis of 50 percent trading therefore we report the adjusted null and the associated p-value in the last column of Table 2.

In all item pairs, the observed trading probability was significantly below the null. The overall likelihood that a participant traded the item that they started with is 0.35, which rejects the null hypothesis of $p(trade) = 0.5$ and the overall adjusted null of $p(trade) = 0.49$, both with p-values < 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

We now turn to a more detailed analysis of trading within each item pair. 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, which reflect the trading rates in Table 2. 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).

Next, we analyze the directionality of trade within item pairs, to see how the random endowment changes the likelihood of ending up with an item relative to its choice probability in the *choice* condition. We follow here the specification in equation (3) and display the results in Figure 1. Despite significant imbalances in subjective valuations within item pairs, particularly in the Cup – Spoon pair, we see that the effect of initial assignment is quite 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.5 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 find substantial valuation disparities for trades involving cash (Boom – Cash). Households randomly endowed with Boom display a 50 percent higher valuation of the item than households endowed with cash (see Appendix Table A.11). Third, 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.6). 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 2007).[17] 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 and precisely estimated 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

[17] There is also debate about whether the WTP–WTA disparity is the result of experimental elicitation procedures (Plott and Zeiler, 2005; Knetsch and Wong, 2009; Isoni et al., 2011; Cason and Plott, 2014; Bartling et al., 2015; Fehr et al., 2015).

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 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.[18] 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.7 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

[18] The 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?"

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.

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 prevalent decision bias in our study population, and therefore provide a relevant vehicle for investigating how these asymmetries are affected by variation in financial resource constraints within this sample. We organize our results around three sources of variation in 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 eating significantly more meals during the hungry season. Next, we plot the baseline ownership of durable goods as a proxy for wealth against the average probability of trading, controlling for the item pair, experimental conditions and household and individual controls, in Figure 2. 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 (January to March), while the other two rounds took place in times of relative abundance, immediately following harvest (July to September). In our sample, the 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).[19] We exploit this variation in seasonal resource availability and compare trading decisions during the hungry season with decisions in two harvest seasons, conditional on random variation in participant experience.

Figure 3 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 12 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 overinterpreting 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 simply reflect seasonal differences in preferences. Data from the *choice* condition for Boom – Salt, used in all three rounds, shows that preferences for the two items do not vary much by season. While Boom seems to be slightly more attractive in the hungry season (i.e., 65 percent of participants 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-value > 0.3).[20]

[19] Questions about meals per day were not administered at harvest time. We therefore cannot analyze the variation in meals per day consistently across all panels in Appendix Figure A.2.

[20] We observe a similar pattern for the Boom – Cash item pair. In the hungry season, 67 percent of farmers choose

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 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 in the weeks following receipt of the loan.[21] Figure 4 plots the effect of the loan on trading probabilities, as a function of how recently it was received (in 2-week bins). The pattern is striking, though standard errors are large: among households surveyed two to three weeks after receiving a loan, the likelihood that a participant trades her endowed item is over 18 percentage points lower than in control group households surveyed in the same week or located in the same geographic area. However, this effect wears off quickly, with the treatment and control groups converging as the time since loan delivery increases. Table 5 shows loan treatment effects, conditioned on different sets of control variables. The reduction in exchange asymmetries is short lived, but large in magnitude, and consistent with patterns of treatment effects on consumption.

5.4 Mechanisms

Our findings consistently point to more rational behavior when resources are more scarce. These findings are consistent both with models of rational inattention (Sims, 2003; Maćkowiak et al., 2018) and with tunneling behavior (Mani et al., 2013; Mullainathan and Shafir, 2013; Dean et al., 2017).. We turn now to further investigation of these two channels, and other competing explana-

Boom over cash in the *choice* condition. In the harvest season (2015), 65 percent of participants choose Boom over cash.

[21] These figures show effects relative to all control households (whose dropoff week is undefined) and are conditioned on survey week fixed effects and fine-scale geographic fixed effects. While the variation in the time between the loan delivery and survey is not randomized, the inclusion of fine temporal and spatial controls improves balance (see Appendix Table A.4) and restricts the identifying variation to be largely arbitrary. To check robustness, we restrict the variation in the time since loan dropoff to come only from the timing of the survey by measuring time between the midpoint of the dropoff days (January 10) and the survey date. Results are very similar. To further check robustness, we limit the variation in the time since loan dropoff to come only from the randomly assigned survey month. Results are again similar, but less precisely estimated.

tions of our main results.

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, the marginal utility of consumption is likely higher and 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 evaluate the importance of decision stakes, we introduced a high-value item pair (Solar – Cash condition) in the last round of our experiments. That is, we offer participants the choice between a solar lamp or an equivalent value cash transfer of 80 Kwacha (USD 14). As shown in Table 3 (Panel A), relative to the estimated trading probability for lower value items, we find a large and significant increase in the trading probability for the high-value item pair. Table 2 shows that, relative to the adjusted null of a 49 percent trading probability, the likelihood of trading in the Solar – Cash condition is not statistically lower. This stands in contrast to the lower than predicted trading probabilities in all of the low-value item pairs. The higher trading probability in the higher-value item pair 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. Across all participants given a choice between a solar lamp and cash, including those in the choice condition, the average probability of ending with a solar lamp is shown by the constant in column 1. In the choice condition, the overall likelihood of choosing the solar lamp was 0.55 (the constant plus the coefficient on the choice condition, column 2), consistent with choice probabilities shown in Table 2. Column 3 estimates the additional probability of ending up with the solar lamp conditional on being randomly assigned it as a starting item. Participants assigned a solar lamp at the outset were 12.5 percentage points more likely to end up with a solar lamp than participants assigned cash. Note that the probability of ending with a solar lamp was no different from the probability of choosing it in the choice condition (i.e., the coefficients are statistically the same), indicating that the weak exchange asymmetry in the high value item pair is driven entirely by participants randomly assigned cash as a starting item. This pattern is also shown in Appendix table A.5 (columns 7 and 8). These find-

ings are supported by participants' hypothetical item valuations. Estimating the impact of item assignment on WTP/WTA shows no differences in valuations, suggesting that raising the value of items leads to more rational behavior (see Appendix Table A.11).

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 often indistinguishable from zero. These findings lend empirical support to the proposed mechanism that resource scarcity increases the relative value of goods, which increases the attention devoted to the decision and results in more rational behavior.

5.4.2 Cognitive performance

An important implication of scarcity is that it is cognitively taxing, and results in lower performance on cognitive tasks (Mani et al., 2013; Mullainathan and Shafir, 2013; Dean et al., 2017). To investigate scarcity-driven tunneling behavior in our setting, we explore both the relationship between cognition and resource scarcity 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: Raven's Progressive Matrices (RPM) and a numerical version of the Stroop test.[22] 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.[23] All outcomes are normalized to a mean of zero and a standard deviation of one, with

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

[23] The Stroop test is sometimes scored by normalizing the score on the incongruent task (task 3) by the score on the

a higher score corresponding to better performance. None of the tasks were incentivized. The test-retest correlation within individual ranges from 0.37 to 0.63 across tests; these correlations are lower than is often found with repeat testing over shorter intervals of time (e.g., Laajaj and Macours, 2019), likely explained at least partially by the long time lag between tests and the fact that tests were embedded in lengthy surveys.

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 5 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, confirming established cross-sectional relationships between the measures and other proxies for human capital, such as educational attainment (Laajaj and Macours, 2019). Second, the middle panel shows a modest improvement in performance on the Stroop tests during the hungry season, but no such relationship in the RPM score (the coefficients are estimated conditional on individual fixed effects and experience controls; see Appendix Table A.8 for the underlying regression results). The neutral and congruent tasks (1 and 2) from the Stroop test show the clearest pattern, increasing by 0.36 to 0.40 standard deviations during the hungry season relative to the 2014 harvest season. They partially revert to baseline levels in the 2015 hungry season. For the incongruent Stroop test (which measures executive function), we observe a similar pattern, but with an improvement in the hungry season that is much smaller in magnitude than for the congruent tasks (which might be best interpreted as measures of attention). Third, the bottom figure shows effects of loan drop off, relative to the control group and conditional on survey-week and geographic fixed effects. Here we see far less consistent patterns: the RMP scores are lowest immediately following loan access and converge to zero consistently over time. The Stroop tests, on the other hand, fluctuate and show no consistent pattern of results (see Appendix Table A.9 for the underlying regression results).

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 across different measures of scarcity. However, if we focus on the within-respondent variation in

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.

cognition and trading probabilities (Panel B), we see a positive correlation between the cognitive measures and trading probabilities, which is statistically significant for the neutral and congruent Stroop tasks. 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. To reiterate, these two tasks should not be directly interpreted as measures of executive function, but as measures of ability to process a simple task, and likely reflect some combination of effort, attention and ability. Note that the finding of unchanged or partially improved cognitive performance during the hungry season relative to harvest is in contrast to the findings in Mani et al. (2013), which uses variation before and after arbitrarily staggered harvest dates, and therefore cannot control for experience in test taking over time. Even if we omit experience controls from the analysis and focus on the difference between the hungry season and 2015 harvest, we see no evidence of decreased cognitive performance in the hungry season on any of the four cognitive measures we employ. This, combined with clear and consistent differences in exchange asymmetries suggests that these may be driven less by cognitive bandwidth and more by increases in the consequentiality of the decision during periods of scarcity.

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).[24] 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.10. 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

[24] This 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.

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.

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 exploit three distinct sources of variation in scarcity to show that exchange asymmetries are significantly smaller when resources are more scarce and decisions more consequential.[25] This suggests that scarcity may actually improve decision-making in contexts like the one studied in this paper. Given the importance of liquidity and credit constraints in many developing country settings, this finding offers good news, since it suggests that some of the external constraints and limitations surrounding investment decisions may be offset by a more careful evaluation of tradeoffs when resources are more scarce.

While our finding that decision-making improves with scarcity is consistent with the idea that scarcity increases focus on essential choices (tunneling, e.g., Mullainathan and Shafir, 2013), it is also consistent with rational inattention (Sims, 2003; Maćkowiak et al., 2018). We present two pieces of additional evidence to examine the relative importance of these explanations. First, we show that an exogenous increase in the value of the goods being traded increases the likelihood of trading, holding scarcity constant. Second, we show that standard measures of cognitive

[25]While the three sources of variation in scarcity that we use are distinct, they are also correlated to some degree. Fink et al. (2018) show both that households that hold less baseline wealth are both more affected by seasonality and experience larger treatment effects from the seasonal consumption loans.

function are unaffected by variation of scarcity and are generally uncorrelated with our measure of decision-making. Together, these patterns are consistent with a rational inattention model in which higher stakes decisions receive a greater share of attentional resources, but not with a model of tunneling in which scarcity itself affects the availability of cognitive bandwidth. From a policy perspective, distinguishing between these mechanisms may be less important than clear and robust evidence that difference sources of variation in scarcity are all predictive of reductions in exchange asymmetries.

A 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 invest greater attention in the trade-off between the two items, possibly treating the trading decision as opportunity cost rather than a loss of the endowed item. This is consistent with lab evidence showing that perceived resource constraints (weekly vs. monthly budgets) affect opportunity cost considerations (Spiller, 2011).

Like any empirical case study, our design and project implementation have limitations that open promising directions for future work. First, by focusing on a single measure of decision-making, we are unable to test whether more and less complex decisions are similarly impacted by scarcity. Comparing across different types of decisions under similar sources of variation in scarcity would offer more nuances tests for different theories of decision-making. Second, by measuring the endowment effect primarily through the exchange paradigm, we are able to quantify the existence of, but not the magnitude of, the endowment effect in our setting. Measuring the gap between willingness to accept and willingness to pay would pin down magnitudes. Finally, scarcity-driven (rational) variation in attention has potential impacts on a broad range of field work and data collection. For example, incentivizing responses to survey questions may differentially change response quality for treatment and control groups if, for instance, the treatment group has been made exogenously less resource constrained.

Our results 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). The results we present in this paper suggest that such a reluctance is widespread in population and highest in times of relative abundance, a point in time when, for example, investments are most viable. Accordingly, opportunities to implement behavior change or to adopt new technologies may not only be population specific, but may also be strongly influenced by temporal and seasonal variation in scarcity. Recognition of this variation may introduce new ways to harness prevalent exchange asymmetries or design policies that help households avoid related biases.

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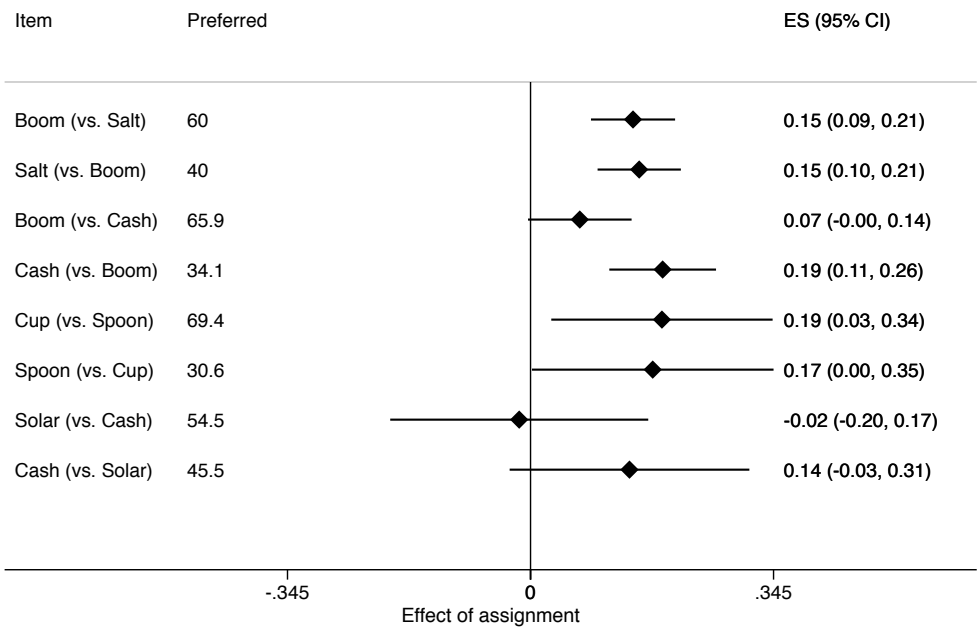

Figure 1: Asymmetries by item preference

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.

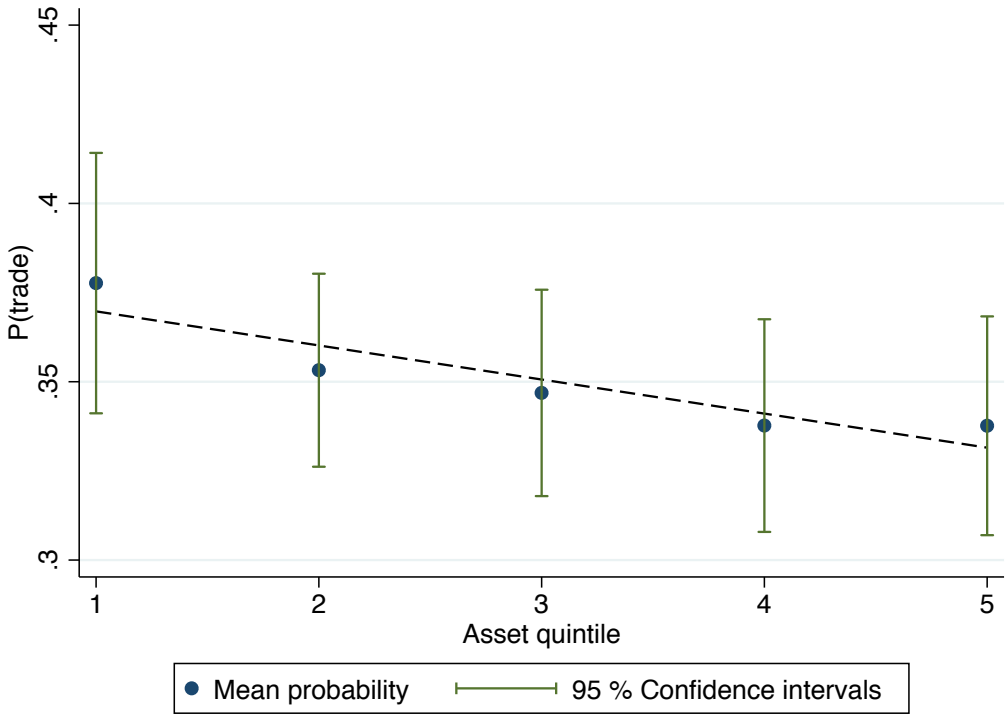

Figure 2: Probability of trading start item by baseline assets

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

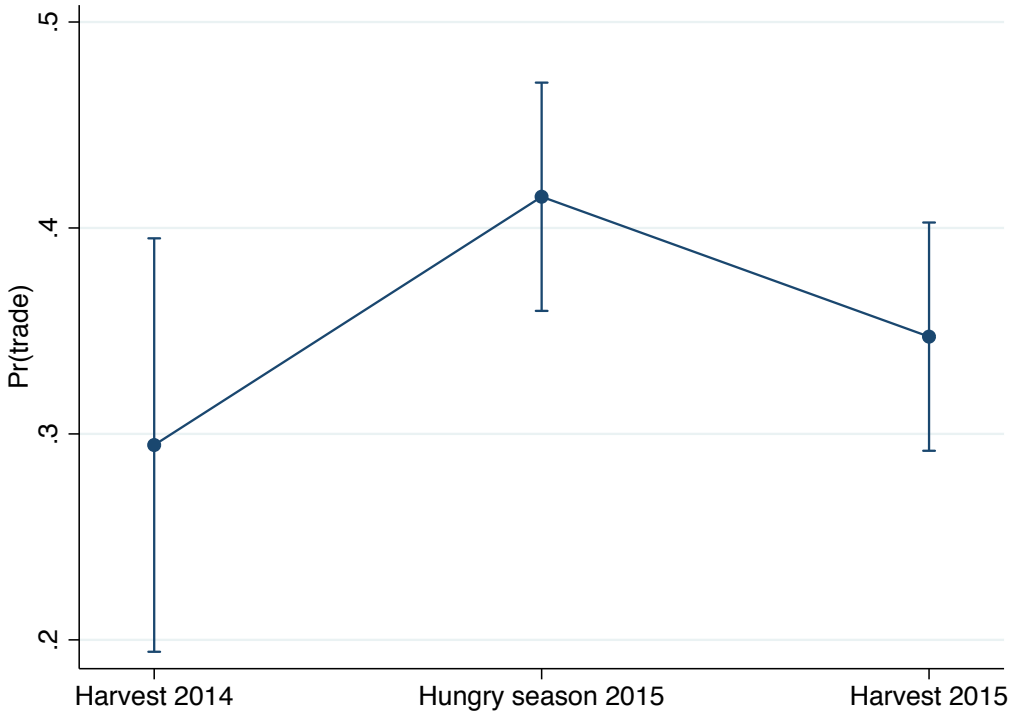

Figure 3: Probability of trading start item, by season

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, household controls and individual fixed effects. 90% confidence intervals are based on standard errors clustered at the village level.

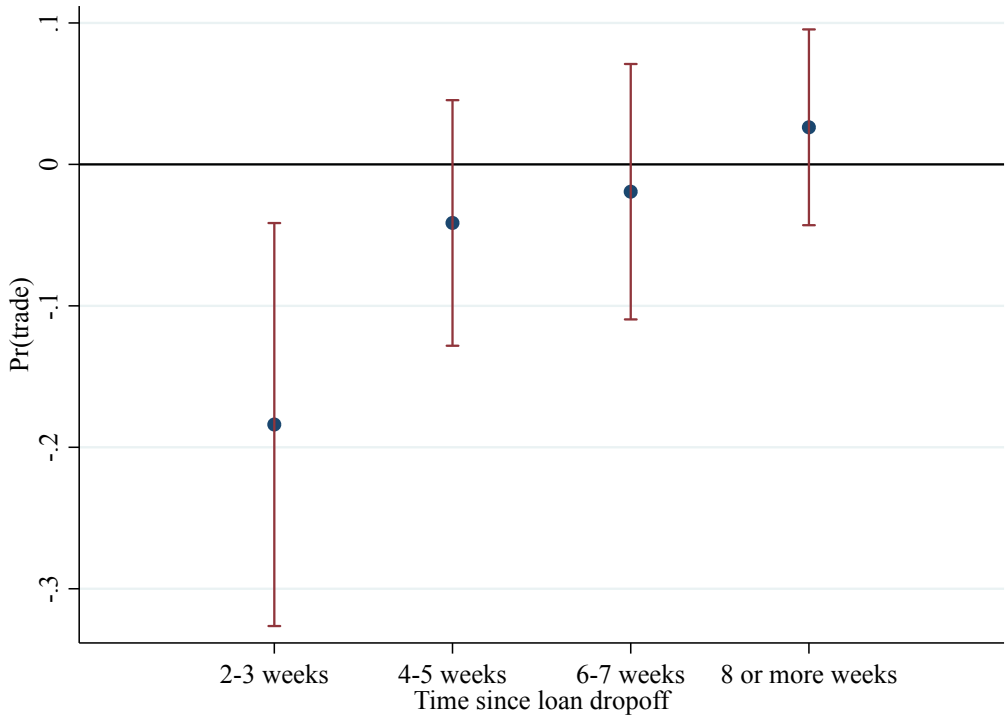

Figure 4: Relationship between weeks since loan receipt and trading probabilities

Notes: Effect of loan timing on trading probabilities, where time since loan dropoff is measured in weeks. The omitted category is the control (no loan) group 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. 90% confidence intervals are based on standard errors clustered at the village level.

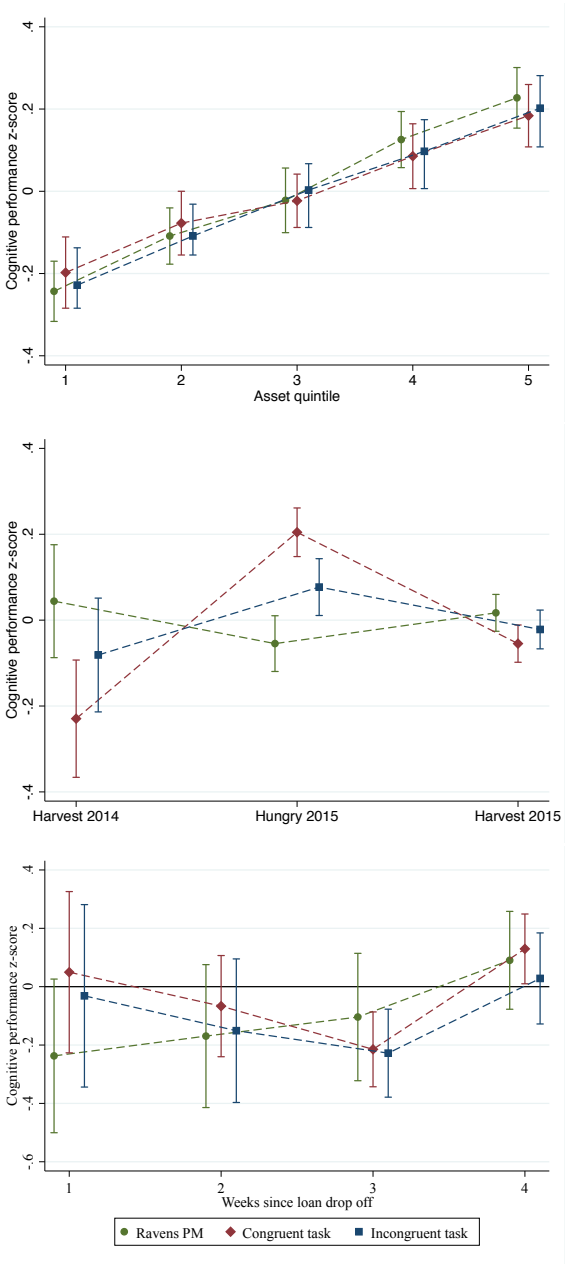

Figure 5: Relationship between scarcity measures and cognitive performance

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 for individual and household characteristics (or individual fixed effects), season, and participant experience with the cognitive tests. 90% 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=0.50)	0.00
Salt	0.40	Salt	514	669	p-val (H0=0.50)	0.00
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=0.50)	0.00
Cash	0.34	Cash	385	431	p-val (H0=0.49)	0.00
Cup-Spoon	N = 564					
Choice condition			End item		Overall	
Pr(chosen)			Cup	Spoon	Pr(trade)	0.30
Cup	0.75	Cup	286	42	p-val (H0=0.50)	0.00
Spoon	0.25	Spoon	135	133	p-val (H0=0.47)	0.00
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=0.50)	0.08
Solar	0.55	Solar	96	108	p-val (H0=0.49)	0.14

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 is presented in the final column. P-values from tests of a null of 50 percent trading and an adjusted null, accounting for assignment probabilities and preferences revealed in the choice condition, are also reported in the final column (with standard errors clustered at the village level).

Table 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	0.004	0.006	0.007
	(0.016)	(0.017)	(0.016)	(0.016)
Cup-Spoon	-0.044*	0.004	0.001	0.002
	(0.024)	(0.031)	(0.030)	(0.030)
Solar-Cash	0.101***	0.095***	0.096***	0.094**
	(0.033)	(0.034)	(0.035)	(0.037)
Panel B. By experimental procedure				
Lottery	0.005	-0.007	-0.015	-0.015
	(0.019)	(0.021)	(0.021)	(0.021)
Timing	-0.007	-0.024	-0.028	-0.012
	(0.030)	(0.034)	(0.035)	(0.035)
Voucher	0.036	0.019	0.015	0.032
	(0.033)	(0.039)	(0.039)	(0.039)
Timing + Voucher	0.065**	0.048	0.043	0.026
	(0.026)	(0.031)	(0.030)	(0.030)
Wording	0.012	-0.040	-0.035	-0.035
	(0.033)	(0.035)	(0.035)	(0.035)
Expectations	0.018	0.001	-0.006	0.010
	(0.026)	(0.033)	(0.033)	(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***	0.096***	0.100***	0.107***	0.121**	0.123***
	(0.022)	(0.022)	(0.021)	(0.031)	(0.056)	(0.031)
Endline	0.066***	0.074***	0.077***	0.054	0.053	0.062
	(0.019)	(0.019)	(0.020)	(0.033)	(0.077)	(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)
Loan	-0.004	-0.011	-0.017		
	(0.028)	(0.029)	(0.028)		
Loan 2-3 weeks ago				-0.191**	-0.184**
				(0.091)	(0.087)
Loan 4-5 weeks ago				-0.032	-0.041
				(0.052)	(0.053)
Loan 6-7 weeks ago				-0.024	-0.019
				(0.054)	(0.055)
Loan 8 or more weeks				0.037	0.026
				(0.041)	(0.042)
Controls		survey week + geography	survey week + geography + hh + experience + procedure + items	survey week + geography	survey week + geography + hh + experience + procedure + items
Observations	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. Columns 4 and 5 estimate separate effects by time since loan dropoff, conditional on week of survey and fine geographic controls. 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)	(5)
Choice condition		0.071	0.141	0.114	0.108
		(0.085)	(0.087)	(0.088)	(0.092)
Start item = solar lamp			0.125*	0.106*	0.106*
			(0.062)	(0.060)	(0.059)
Constant	0.480***	0.474***	0.405***	0.061	0.068
	(0.036)	(0.035)	(0.041)	(0.167)	(0.168)
Controls	none	none	none	hh + experience	hh + experience + procedure
Observations	400	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 estimates the constant, which is overall likelihood of ending with the solar lamp. Column 2 includes an indicator for the choice condition. Column 3 estimates the probability of ending with the solar light in the choice condition and among those assigned a solar light, relative to those assigned cash as their start item. Columns 4 and 5 add controls. See text for further discussion.

Table 7: Cognitive ability and probability of trading

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

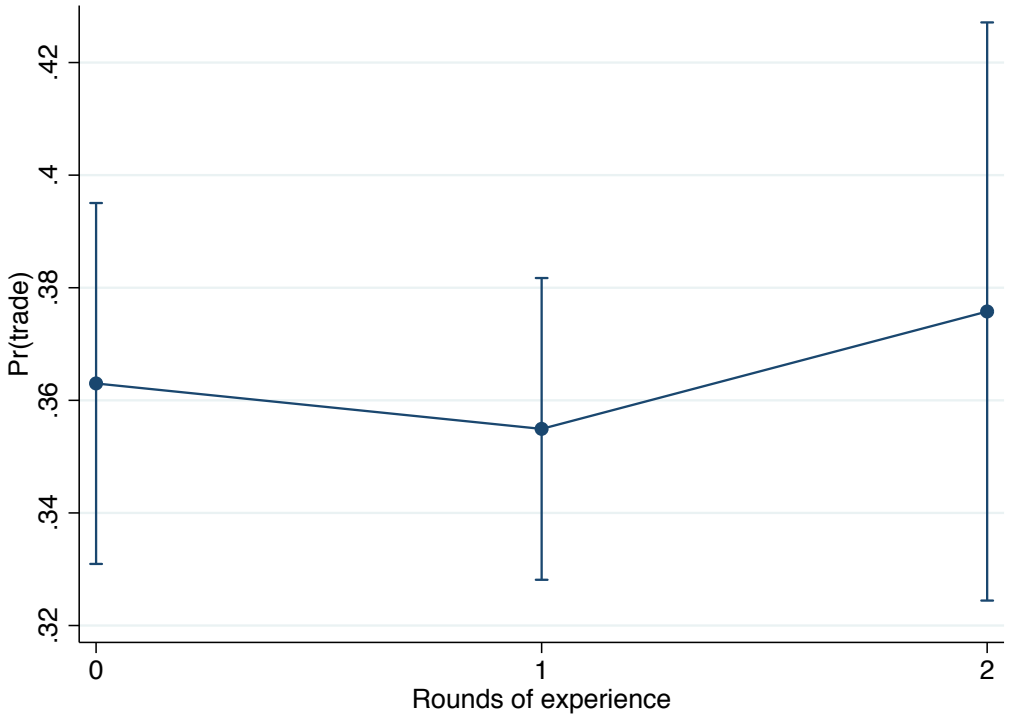

Figure A.1: Probability of trading start item in Boom – Salt pair, by rounds of participant experience

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. 90% confidence intervals are based on standard errors clustered at the village level.

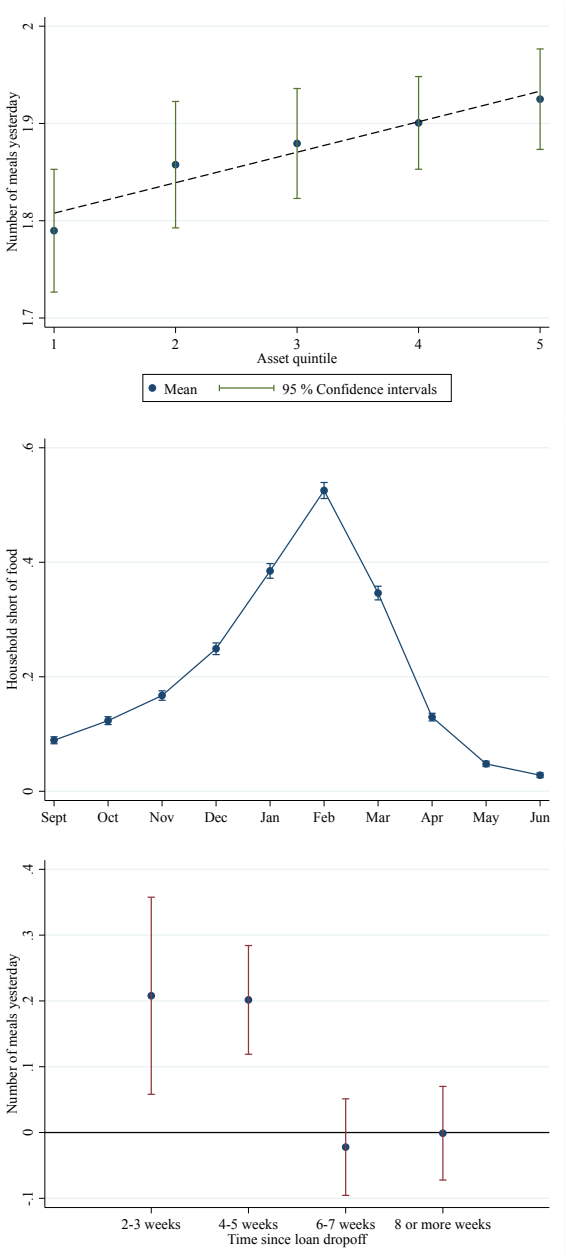

Figure A.2: Variation in consumption and food availability by source of variation in scarcity

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.

Table A.1: Balance: Rounds

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

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

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

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

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

Table A.4: Balance: Time since loan dropoff

	Weeks since loan dropoff				
	2-3	4-5	6-7	≥8	F-statistic
	(1)	(2)	(3)	(4)	(5)
Age of hh head	1.28	0.60	-1.19	1.76	0.89
	(0.64)	(0.38)	(-0.74)	(1.42)	
Female headed hh	-0.05	-0.01	-0.01	-0.00	0.21
	(-0.89)	(-0.18)	(-0.11)	(-0.05)	
Children under 5	-0.23	0.03	0.14	0.11	1.59
	(-1.46)	(0.39)	(1.15)	(1.51)	
Children 5-14	0.38	0.14	0.21	0.33	2.75
	(1.69)	(0.75)	(1.74)	(2.39)	
Adults 15-64	0.45	-0.02	0.11	-0.05	1.37
	(2.05)	(-0.12)	(0.72)	(-0.47)	
Adults over 64	0.03	-0.06	-0.05	-0.01	1.06
	(0.57)	(-1.37)	(-1.15)	(-0.42)	
Baseline assets	-0.04	0.09	0.15	0.03	0.23
	(-0.17)	(0.47)	(0.71)	(0.24)	
Baseline harvest value	538.69	-234.17	217.14	11.79	0.79
	(1.05)	(-0.69)	(0.90)	(0.04)	
Female respondent	-0.13	-0.03	-0.01	-0.01	0.81
	(-1.75)	(-0.59)	(-0.08)	(-0.20)	
Respondent age	1.19	2.38	-0.68	1.96	1.12
	(0.54)	(1.37)	(-0.40)	(1.52)	

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

Table A.5: Item-specific asymmetries

	Probability that subject ended with item A							
Item A	Boom	Salt	Boom	Cash	Cup	Spoon	Solar	Cash
Item B	Salt	Boom	Cash	Boom	Spoon	Cup	Cash	Solar
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Assigned item A	0.149***	0.164***	0.070*	0.188***	0.126*	0.242***	-0.016	0.141
	(0.029)	(0.030)	(0.038)	(0.039)	(0.066)	(0.073)	(0.093)	(0.087)
Constant	0.599***	0.401***	0.659***	0.341***	0.746***	0.254***	0.545***	0.455***
	(0.027)	(0.027)	(0.035)	(0.035)	(0.063)	(0.063)	(0.092)	(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.6: 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***	0.014***	0.014***	0.020***	0.020***
	(0.006)	(0.005)	(0.005)	(0.006)	(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.7: Social desirability bias

	Probability that subject traded start item				
	(1)	(2)	(3)	(4)	(5)
Social desirability bias score	0.002	0.002	0.002	0.002	0.002
	(0.003)	(0.003)	(0.004)	(0.004)	(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.8: 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*	0.297***	0.373***	0.091*
	(0.056)	(0.051)	(0.055)	(0.052)
Endline	-0.135**	0.019	0.031	-0.115**
	(0.055)	(0.054)	(0.052)	(0.048)
Test experience	0.110***	0.206***	0.177***	0.150***
	(0.025)	(0.027)	(0.026)	(0.026)
Panel B: Individual fixed effects				
Hungry Season	-0.062	0.362***	0.405***	0.149**
	(0.066)	(0.067)	(0.071)	(0.068)
Endline	-0.045	0.190	0.201*	0.118
	(0.093)	(0.117)	(0.111)	(0.103)
Test experience	0.031	0.086	0.037	-0.026
	(0.050)	(0.066)	(0.062)	(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.9: Variation in cognitive ability, by loan access

	Ravens score	Stroop task 1	Stroop task 2	Stroop task 3
	(1)	(2)	(3)	(4)
Loan 2-3 weeks ago	-0.237	-0.196	0.050	-0.031
	(0.160)	(0.144)	(0.168)	(0.190)
Loan 4-5 weeks ago	-0.169	-0.070	-0.067	-0.151
	(0.149)	(0.118)	(0.105)	(0.150)
Loan 6-7 weeks ago	-0.104	-0.129*	-0.215***	-0.228**
	(0.133)	(0.073)	(0.078)	(0.092)
Loan 8 or more weeks ago	0.090	-0.017	0.130*	0.028
	(0.102)	(0.081)	(0.073)	(0.095)
Observations	1334	1297	1304	1301

Notes: Linear regressions of cognition scores on weeks since loan delivery, with coefficients estimated relative to the control (no loan) group. 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. Regressions are conditioned on a full set of household, individual and procedural controls including survey-week fixed effects and fine geographic controls. Standard errors clustered at the village level.

Table A.10: 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.11 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 standard-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.11: Willingness to pay/accept

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

Notes: Censored normal regression of reported willingess to pay or accept for item. After the final item selection, subjects with the item where asked a series of questions to elicit their willingness to accept for the items. Subjects choosing cash were asked as 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 observation. 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.3.

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.4 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.5 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). Follow 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 foreach of the three sub-tasks (neutral, congruent, incongruent). The median time for completing the incongruent task was 42 seconds (mean 45), while the median number of mistakes was 2 (mean 2.4). Appendix Figure A.6 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.

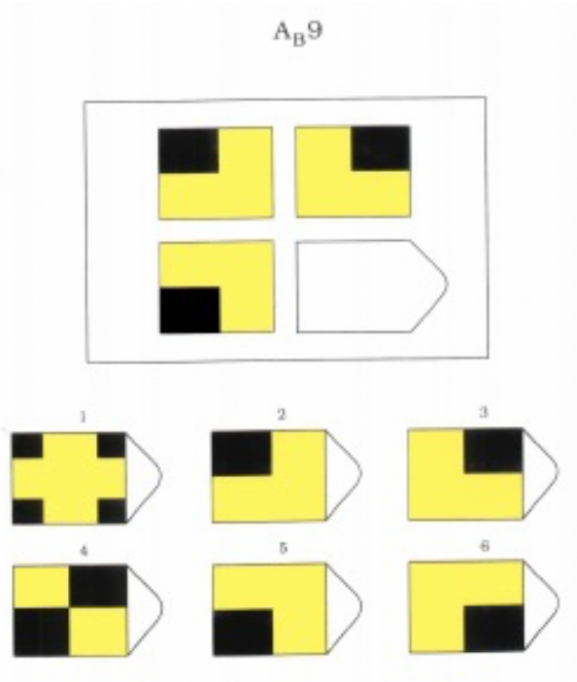

Figure A.3: Ravens Matrices: Sample decision task

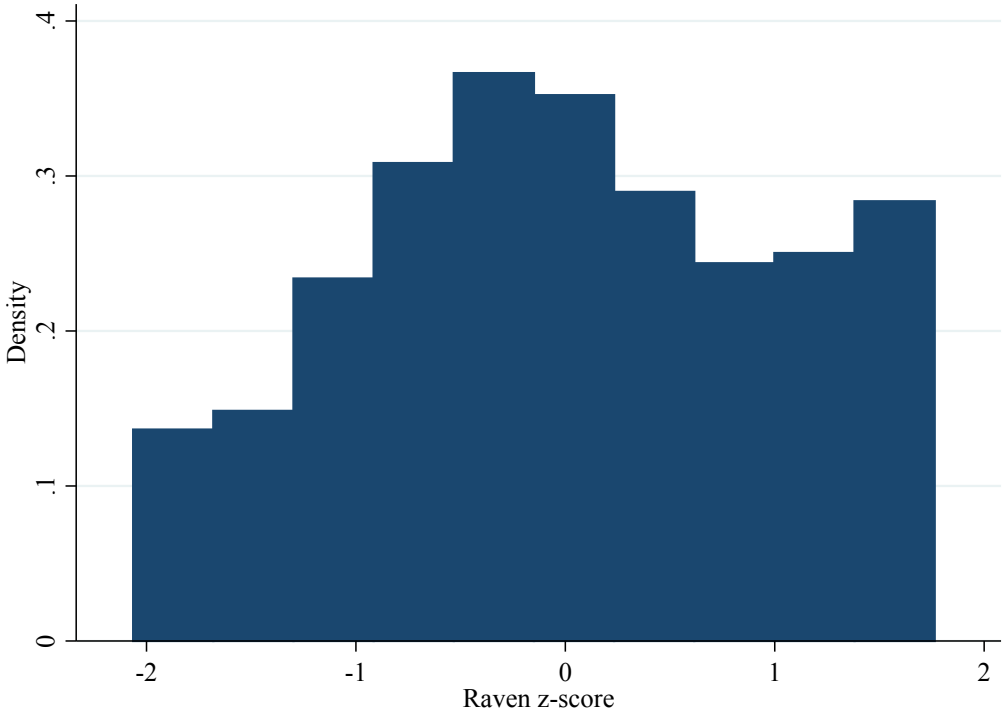

Figure A.4: Distribution of scores in the Raven's Progressive Matrices (RPM)

Congruent Task 1

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

Congruent Task 2

5	1
6	333
7	55555
8	1

Incongruent Task

9	1111111
10	66
11	22222
12	444444

Figure A.5: Stroop: Sample decision tasks

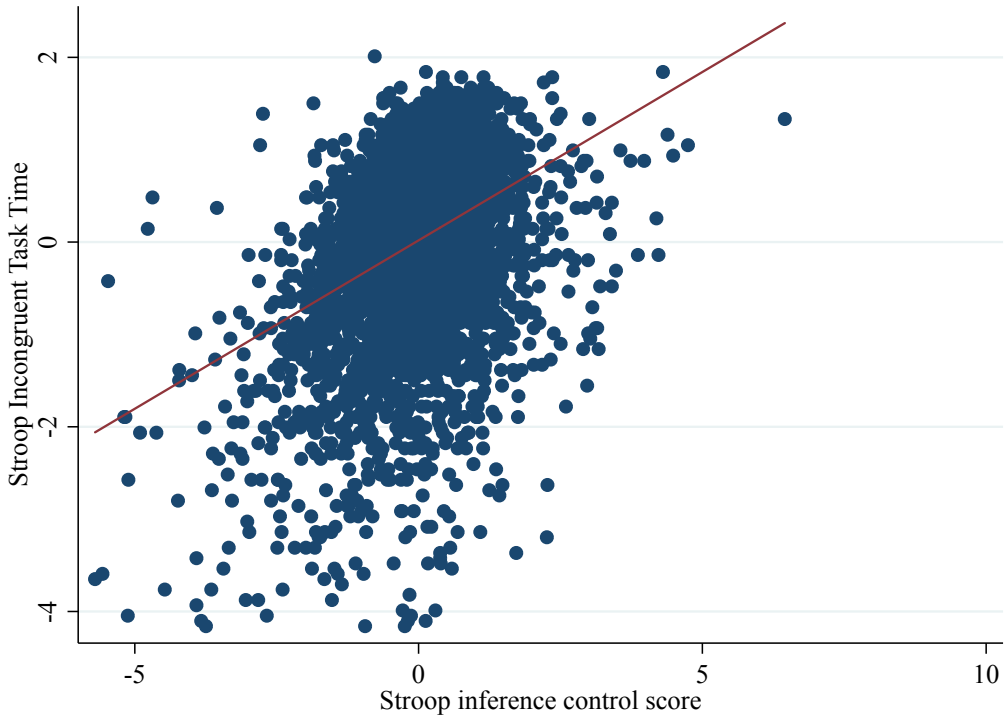

Figure A.6: Correlation between time needed in the incongruent task 3 and the inhibitory-control score (both coded as z-scores)

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]?