Poverty, Seasonal Scarcity and Exchange Asymmetries: Evidence from Small-Scale Farmers in Rural Zambia*

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Abstract

A growing literature associates resource scarcity with biases in decision-making. We investigate this link in a sample of over 3,000 small-scale farmers in Zambia, who completed a total of 5,842 decision experiments involving the opportunity to exchange randomly assigned household items for alternative items of similar value. We observe large exchange asymmetries – the so-called endowment effect – with an average trading probability of 34 percent, 16 percentage points below the trading rate predicted by neoclassical theory. Consistent with both increased attention and larger potential trading losses with higher value items, exchange asymmetries are smallest when the value of the traded items is high and when participants are relatively resource constrained. In our sample, both cross-sectional and seasonal scarcity improves the quality of decision-making, moving behavior closer to standard economic predictions. We find no corresponding systematic relationship between scarcity and performance on cognitive tasks.

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1 Introduction

A substantial body of evidence documents that individual decision-making is prone to behavioral biases and deviations from normative rationality (e.g., Camerer et al., 2003; DellaVigna, 2009). Individuals who are systematically more susceptible to these biases may face worse outcomes over time. However, while some evidence suggests that poverty may actually exacerbate decision-biases (e.g., Tanaka et al., 2010; Spears, 2011), the evidence is far from conclusive. On the one hand, scarcity increases the value at stake in a decision, which may help focus attention and minimize mistakes (Goldin and Homonoff 2013; Shah et al. 2015). This suggests that behavioral biases may be decreasing in decision stakes, which is a key feature of rational inattention models (Sims, 2003; Maćkowiak et al., 2018).[1] On the other hand, scarcity may also interfere with cognitive function as increased focused on financial concerns absorbs cognitive bandwidth (Mani et al., 2013; Mullainathan and Shafir, 2013), with potential consequences for economics outcomes and decision-making. Indeed, a recent and separate literature suggests that lower cognitive function is related to individual decision-making, in particular to impatience and less willingness to take risks, behavior which may perpetuate poverty (e.g., Burks et al., 2009; Dohmen et al., 2010; Benjamin et al., 2013; Andersson et al., 2016). In a recent review paper, Kremer et al. (2019) highlight the conflicting findings of the emerging literature on scarcity and cognition and stress the need both for better identification in the field and clearer links to meaningful outcomes including decision-making.

This paper presents field evidence on these two views on the impact of economic circumstances on an important decision bias that may impair the exchange of goods: the tendency of individuals to place greater value on items they own than on identical items they do not own. This gap between willingness to pay and willingness to accept, commonly referred to as the "endowment effect", suggests that preferences depend on endowments, which violates one of the central axioms of neoclassical economics (see Ericson and Fuster, 2014, for a review of the recent literature).[2] Consequently, it has potentially far-reaching implications for market transactions if people hesitate to give up existing assets, trade or invest in new technologies.

[1] The theory of rational inattention, pioneered by Sims (2003), posits that decision makers choose how much attention to pay to a decision depending on its importance; the same decision is higher stakes, relative to income, for a poorer household than a richer household.

[2] The term "endowment effect" was introduced by Thaler (1980). However, some critics have argued against the use of this term as it already suggests an interpretation of the observed anomaly (e.g., Plott and Zeiler, 2005, 2007). While we will primarily use the term "exchange asymmetries" to describe the findings in our experiment, we will also use the endowment effect terminology in reference to the broad literature. Our study is not designed to provide new insight into the behavioral mechanisms underlying exchange asymmetries, so we also avoid explicit discussion of the theoretical underpinnings of the empirical observation throughout the paper.

Using evidence from 5,842 trading decisions conducted with 3,059 small-scale farmers in rural Zambia, we first show that the endowment effect is remarkably large and robust in this low-income setting. We then take advantage of both natural and experimental variation in scarcity in our setting to assess its impact on decision-making, as well as the role of cognitive function as a mediator. We find that scarcity improves the quality of decision-making overall, moving trading behavior closer to standard economic predictions in our setting. We find only weak evidence that cognitive function varies with scarcity – a measure of attention improves with scarcity – or explains the prevalence of the endoment effect..

Our data collection is embedded in an ongoing randomized controlled trial on liquidity constraints and labor supply that involved repeated surveys over multiple years (see Fink, Jack, and Masiye, 2018). As part of the ongoing surveys, households received a small item as a compensation for their time. We intervened in this standard procedure by randomly endowing participants with one of two equally-valued items midway through a survey. Items were common household necessities worth about 1/5th of the daily agricultural wage. At the end of the survey, surveyors offered the opportunity to trade the endowed item for the alternative item. Because we randomly assigned the initial item and trading cost were near zero, neoclassical theory predicts that, for any distribution of preferences, half of participants received the non-preferred item, on average and should thus trade for their preferred item.[3]

Varying the items involved, we find strong and robust evidence for the existence of exchange asymmetries in our setting. On average, across all item pairs (some of which involved cash), only 34 percent of participants traded the endowed item. We test an extensive range of experimental procedures, following Plott and Zeiler (2007), to measure the extent to which these large exchange asymmetries are driven by features of the design, including the assignment procedures, participants' interaction with the endowed item, social norms, experimenter demand, and uncertainty regarding future trading opportunities. None of the procedural variations had a sizable impact on the observed exchange asymmetries.

We exploit four sources of variation in scarcity to examine the relationship between scarcity and decision-making: (1) we exploit cross sectional variation in wealth at baseline, (2) we compare

[3]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.

decision-making over one and a half agricultural cycles, measuring outcomes after the 2014 harvest, before the 2015 harvest and after the 2015 harvest, (3) we leverage cross sectional random variation in the disbursement of small loans before the 2015 harvest and (4) we increase the value of the traded items to mimic the variation in stakes associated with scarcity. These four sources of variation range from least to most causal in the interpretation of their relationship with trading decisions. All are associated with meaningful variation in direct proxies for scarcity, such as consumption and food availability.

With all four sources of variation, we show that greater scarcity is associated with a higher propensity to trade and therefore a lower endowment effect. Specifically, households with more durable assets at baseline are significantly more likely to trade. Second, the same individual is more likely to trade during the hungry season than after harvest. In the rural agricultural setting studied, households face considerable variation in savings and income over the year. With a single rainy season and harvest each year, study households receive most of their annual income at once, and need to use this income to cover investment and consumption needs over the following 10-12 months. The result is a pronounced period of resource scarcity ("hungry season") in the three month leading up to the next harvest. During this time the value of the trading decision is presumably highest and it may be more difficult to obtain the alternative item or to reverse the experimental decisions in the market. Accordingly, taking one of the two items essentially means giving up the other when resources are scarce, but not if abundant, as is the case after harvest. We exploit the seasonal variation in scarcity to test how decision behavior changes across the agricultural season.[4] Third, we complement this analysis by exploring random variation in resource availability created by the larger randomized controlled trial, which provided a subset of households with access to consumption loans during the hungry season (for more details on the study see Fink, Jack, and Masiye, 2018). Households are less likely to trade in the weeks following receipt of the consumption loan, where we also observe the greatest increases in consumption.

Finally, in a "high-value" treatment, participants made decisions over items worth USD 14 or about 28 percent of monthly household income. We document a significant increase in the likelihood of trading by about 10 percentage points when item values are high. The resulting overall trading propensity (0.44) is close to the standard prediction, but still smaller than the expected trading probability of one half ($p - value = 0.08$). The observed reduction in the magnitude of

[4]Note that this is similar to the variation used in Mani et al. (2013), but we observe one additional round: after harvest - before harvest - after harvest.

the exchange asymmetry with higher value decisions is consistent with respondents paying more attention to their decisions when they involve high-value items and with increased cost of reversing a "mistake" ex-post due to the difficulty of finding an outside market for this high-value item. However, we find no evidence that exchange asymmetries are correlated with variation in the market connectedness of villages in our sample, suggesting that the second explanation is less relevant.[5]

Together, these findings indicate that scarcity *decreases* the magnitude of exchange asymmetries, both within participant across agricultural seasons and across participants within a season. Most strikingly, the likelihood of trading the endowed item increases by roughly 10 percentage points during the hungry season compared to the post-harvest period, reducing the exchange asymmetry by more than 50 percent, while access to short-term liquidity through the larger RCT increases exchange asymmetries by almost the same magnitude (10 percentage points).

To test the extent to which these effects are driven by changes in farmers' cognitive and executive function, we implement a standard set of tests from cognitive psychology in a sub-sample of participants in each survey round. We see limited correlation between cognitive function and exchange asymmetries; in a simple measure of attention, we observe that greater attention is associated with a higher trading probability. More nuanced measures of fluid intelligence or executive function, on the other hand, are uncorrelated with trading probabilities. Across our different sources of variation in scarcity, we see no consistent effect of scarcity on cognitive function. We discuss the sensitivity of these results to different cognitive measures and how they are scored.[6] Importantly, and consistent with Carvalho et al. (2016), we are able to clearly reject the story that scarcity impedes cognitive function and drives the decision biases that we document. Overall, our results suggest that, in the trading context studied, scarcity makes individuals *less prone* to decision biases and shifts behavior towards the prediction of standard choice theory, which could be interpreted as evidence for scarcity increasing the focus of decision-makers.[7]

These findings contribute to a nascent literature on the psychology of the poor (e.g., Schilbach et al., 2016; Dean et al., 2017; Kremer et al., 2019). This literature suggests that poverty may have

[5]This is in contrast with Apicella et al. (2014) who find that trading asymmetries increase with market exposure in rural Tanzania.

[6]Specifically, the Stroop test measures inhibitory control by normalizing performance on an incongruent task relative to performance on a congruent task. The difference in measured inhibitory control across seasons is driven by improvements in the congruent task in the scarce period rather than declines in performance on the incongruent task.

[7]This is in line with Shah, Shafir, and Mullainathan (2015), who present survey evidence that low-income people consistently make decisions closer to normative predictions in a host of choice scenarios that require hypothetical tradeoffs involving money.

a negative causal effect on decision-making through a number of pathways, including that financial concerns absorb the cognitive bandwidth needed for other decisions (Shah et al., 2012; Mani et al., 2013; Mullainathan and Shafir, 2013), that increased prevalence of stress and depression may interfere with decision-making or increase biases ((Haushofer and Fehr, 2014; Haushofer and Shapiro, 2016)), or that the living conditions of the poor may contribute to worse decision-making (Schilbach, 2019; Dean, 2019). To date, few papers have traced effects from exogenous variation in scarcity through to real-stakes decisions, and measured the mediating effect of cognition. We fill that hole and generate the new insight that scarcity may improve some aspects of cognitive function and reduce some types of decision biases. We note that this is not necessarily in contradiction of the view that poverty depletes cognitive bandwidth; our measure of decision-making is an immediate tradeoff that is designed to be relatively familiar. Performance on decisions with longer term consequences or involving risk or real effort might have generated a different set of results.

This paper also adds to the debate about the robustness of behavioral biases in general, and the endowment effect in particular. Perhaps the most influential work on the endowment effect outside of the lab is a series of experiments at sport cards shows in the United States demonstrating the importance of market experience (List, 2003, 2004). On average, professional dealers appear significantly more likely to trade their assigned baseball memorabilia than non-dealers (List, 2003). Moreover, this finding is not confined to the specific domain of sports memorabilia; dealers are also more likely to trade ordinary consumption goods, such as a coffee mug, than non-dealers (List, 2004). We find no evidence that experience with our trading experiment attenuates exchange asymmetries in our study, though the amount of experience (maximum of 3 trading opportunities) and frequency of trading decisions (every 5 or 6 months) may have been insufficient for systematic learning by study participants. 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. We find no evidence that access to outside markets affects trading probabilities, though our study population appears relatively homogenous in their familiarity with market exchange.

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.

Gine and Goldberg (2017) find that prior savings account holders in Malawi are less likely to switch to a cheaper account than are new customers, but that experience erodes this "endowment effect". The endowment effect may also explain low take-up rates of certain loan types, in particular if they are collateralized by existing assets (Carney et al., 2018). These studies suggest that the endowment effect is not just an artifact of (laboratory) experiments, but can have important implications on market transactions as it may hamper investment activities, technology adoption, and loan take-up.

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 then corresponded to approximately US$ 600. With 5-6 household members on average, income per capita is substantially less than US$ 1 per day. The rainfed nature of production concentrates income in a single harvest season between May and August, and leads to a pronounced hungry season in the months leading up to harvest, when many households reduce consumption due to a lack of food. With early crops typically becoming available in April, food shortages and hunger usually spike between January and March (Fink, Jack, and Masiye 2018).

2.2 Experimental Design

The experiments reported here were embedded in household surveys conducted as part of a randomized evaluation of a seasonal loan program (see Fink, Jack, and Masiye 2018 for further detail on the RCT). As part of the evaluation, households were surveyed up to four times per year. In the first year of the study, all farmers received a small box of commonly used washing powder (called "Boom" after a local brand name) as compensation for their time at the end of the survey.

In the second year of the study, rather than providing Boom to all households, we implemented a modified version of the Knetsch (1989) exchange paradigm with a subset of households in each household survey. We conducted the decision experiments between July 2014 and September 2015 with a total of 3,059 households across 175 villages. Households participated between one and three times in these experiments and received the standard compensation (Boom) otherwise, resulting in 5,842 individual decisions.

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 to complete in total.[8] 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 to the household respondent. 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.[9] 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 are minimal in our setting as participants have to answer the trading question in any case and interviewers immediately completed trades.

To identify the extent to which observed exchange asymmetries depend on procedural details, we follow the literature, most notably Plott and Zeiler (2007), and consider several variants on the basic 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 (in the first round of experiments) or by respondents drawing a button out of a bag (second and last round). The main idea of the *lottery assignment* is to minimize the risk of possible inferences about the relative valuation of items or signaling by the experimenter associated with the *standard assignment*.[10]

[8] Note that survey respondents sometimes changed across survey rounds, and also included other adult household members.

[9] See appendix for the exact wording of the initial assignment as well as for the trading opportunity for all experiments.

[10] For example, if the randomization is non-transparent respondents might incorrectly infer that the assigned item is

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 option, with some participants receiving the endowment only minutes before the trading opportunity (*timing* procedure), (ii) we used vouchers redeemable for the specific item, rather than handing over the item itself (*voucher* procedure), and (iii) we directly manipulated participants' expectations about subsequent trading by informing them that they would have an opportunity to trade at the end of the survey (*expectations* procedure).

Third, to address possible experimenter demand effects and concerns that study participants would perceive trading as impolite, causing inconvenience or additional work for interviewers, we varied the wording when presented with the trading opportunity (*wording* procedure). That is, we designed an exchange treatment that reversed expected behavior by requesting participants to trade at the end, rather than inviting them to do so.[11]

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. Both items are household staples with a local price of 3-3.5 Kwacha (USD 0.50), which corresponds 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. Second, we offered durable goods (a mug and a serving spoon). Third, we increased the value of the item pair to roughly 20 times the value of the default pair (i.e., we used a solar lamp and 80 Kwacha in cash). In addition to these item-pair variations, we randomly selected in each round some households 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. That is, we randomly selected some households for a *choice* condition, where they could simply pick their preferred item at the end of the interview. Table 1 summarizes all randomly assigned experimental features, and the number of observations in each, by survey round.

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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.

[11]Note that the idea here is similar to a recently proposed approach to bound experimenter demand by De Quidt et al. (2018), where they deliberately introduce demand effects to measure their impact on experimental findings.

2.2.2 Implementation and Randomization

To understand the impact of the external environment on the robustness of exchange asymmetries, we conducted exchange 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 which gradually included all households over the three survey rounds. Households not part of the exchange experiment sample continued receiving the default compensation of Boom for completing the survey. Randomization of item pairs was done at the village level, while the randomization of specific experimental procedures was done at the household level.[12]

Experiment round 1 (harvest season 2014): The experiment round took place after harvest in 2014, and ran from July through September. Overall, we randomly selected 105 villages and 1,513 households, covering approximately 58 percent of the total study population. 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 already 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. For round 2, we changed the randomization procedure of the *lottery assignment*, i.e., we switched from a coin-flip procedure to drawing a button from a bag with color-coded buttons. 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.3 for more

[12] 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.

details).

Experiment round 3 (harvest season 2015): We conducted the third round of experiments after the 2015 harvest between July and September 2015 with all households in the sample (N=2,962 households). We used the same item pairs as in round 2 and added the high-value Solar – Cash pair. In addition, we dropped the *standard assignment* and used only the *lottery assignment*, varying *timing*, *voucher* and *expectations* procedures at the household level as described in Section 2.2.1 above. We implemented the high-value Solar – Cash item pair with 400 subjects (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.

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, we expect 50 percent of the sample to receive the non-preferred item and thus to trade the endowed item against their preferred one. For any item pair, we can thus simply 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:

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

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:

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

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 zero. 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 compared to the *choice* condition outcomes.

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

$$p(trade) = \alpha + \beta N + \rho R + X\delta + \varepsilon \tag{4}$$

where $N = 0, 1, 2$ captures the number of times the respondent participated in a trading decision prior to the current decision, and R are indicators for experimental rounds ($R = 1, 2, 3$) to capture seasonal effects.

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

To test the exogeneity of the experimental conditions, we regress household controls on indicators for the survey rounds, item pairs and experimental procedures, and report the results in Appendix Tables **??**, A.2 and A.3, respectively. The t-statistics in parentheses reflect the difference in means between each column and the base group. The randomly assigned item pairs and experimental procedures are balanced and show only three t-statistics above 1.96 out of 100 individual tests. The sample is also balanced across rounds, though the individual-level characteristics – respondent gender and age – show some differential selection in the hungry season, though household characteristics remain balanced.

4 Results: Exchange asymmetries in a rural low-income setting

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

4.1 Robustness across and within item-pairs

Table 2 provides an overview of results by item pair. The *choice* condition in the first column of the table directly measures subjects' relative preferences for each item. Of the item pairs used, participants had the most imbalanced preferences in the cup vs. spoon treatment, with three quarters of participants preferring a cup over a spoon (despite similar market value). Preferences were less stark for the other three item pairs. For each item pair, we also tabulate the number of participants starting and ending with each item in the pair. Finally, the table also shows the overall likelihood that participants traded the item they started with and a t-test for whether the trading probability is different than 0.5. In all pairs the likelihood of trading is significantly below 0.5.

Panel A of Table 3 shows the results from estimating equation (2) where the exogenous variables of interest are item pairs. We describe 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. While the trading propensity increases in the the Boom – Cash item pair, it further decreases in the Cup – Spoon item pair, indicating a large exchange asymmetry for the item pair with the most imbalanced preferences. 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 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 Boom – Salt item pair in the first harvest season (round 1). There is little difference in trading rates between item pairs with the addition of round controls, or other covariates (columns 2-4).

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

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; Fehr et al. 2015). To address this concern, we implemented several variations of our standard experimental procedures (see 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. Column 1 of Table 3 includes no controls. In columns 2-4, we gradually increase the number of controls in the model, including round effects, household characteristics, and item pairs.

We find no evidence that the assignment method (*lottery* vs. *standard assignment*) changes behavior. In fact, the coefficient estimates are virtually zero in all specifications. 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 using vouchers (that could be exchanged for the assigned or alternative item at the end of the survey) rather than handing items over directly at the time of assignment (*voucher* procedure). We find no indication for 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 is no longer statistically significant 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 a possible experimenter demand effect. A first indication that social norms (and demand effects) play a minor role in explaining the observed exchange asymmetries is the lack of a measurable effect of the *lottery assignment*, which transparently randomized the item assignment in front of participants. This random assignment is less likely to trigger demand effects than the *standard assignment* procedure where the randomly chosen item is directly handed to subjects by the surveyors. To 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). Given cultural norms of politeness, we expected 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.[13] 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.5 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 some households (randomly) with zero, one and two rounds of prior trading experience. Experience appears to play a negligible role in improving decision-

[13] 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?"

making, at least over the intervals at which our data collection was spaced and the number of repeated decisions that we observe.

A second factor that has been highlighted as potential explanation for exchange asymmetries in the literature are expectations. 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 about future outcomes shape their reference point, we would observe more trading among those participants who are more likely to expect the trading opportunity (Kőszegi and Rabin, 2006). To directly test the importance of participants' priors regarding future trading opportunities, we manipulated participants' expectations about future trading (*expectations* procedure). Explicitly informing participants about a possible trade at the end of the interview should have reduced the uncertainty about subsequent trading and shifted potential reference points. The coefficient estimate for our *expectations* procedure shown in Panel B of Table 3 is close to zero, suggesting that trading probabilities are not affected by the perceived likelihood of subsequent trades.

5 Results: Value, scarcity and cognition

Our results up to this point show evidence for substantial exchange asymmetries in our sample of poor, rural households, which appear remarkably robust to changes in experimental procedures or participants' experience. Even though this suggests that decision biases exist among the poor, our primarily interest is in understanding how these biases vary with scarcity. We organize our results around four sources of variation in scarcity, imposing increasingly strict requirements on the source of variation. For each, we begin by showing the "first stage" or how the scarcity measure translates into consumption or food availability. Next, we show the relationship between the scarcity measure and trading probabilities. Finally, we turn to an investigation of the mediating role of cognition in the relationship between scarcity and decision-making.

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, households with more assets at baseline are also significantly less likely to skip meals during the hungry season. Next, we plot the baseline ownership of durable goods as a proxy of wealth against the average probability of trading, controlling for the item pair, experimental conditions and household and individual controls, in Figure A.3. The negative gradient indicates more trading in poorer households, though the confidence intervals are large (p-value on the difference between the first and fifth quintile is 0.12). Since numerous other factors correlated with wealth may affect trading behavior, we turn to more plausibly exogenous sources of variation in the value of the traded item and participants' available resources below.

5.2 Seasonal variation in wealth and income

As described above, the pronounced seasonality in income, savings and consumption is one of the most salient features of the study setting, and thus provides a natural source of variation that we use to analyze how scarcity shapes trading asymmetries. The second round of our experiment coincided with the hungry season, while the other two experiment rounds took place in times of relative abundance, immediately following harvest. In our sample, the average cash savings during the hungry season is around 100 Kwacha, or 10 USD, while the average cash savings at harvest is over 600 Kwacha. We exploit this variation in seasonal liquidity and compare a farmer's trading decision during the hungry season with their decisions in two harvest seasons.[14]

Figure 2 and Table 4 show the estimated marginal effect of the season on trading probabilities. As shown in the figure, the trading probability in 2014 harvest season is about 30 percent. The likelihood of trading, however, increases by between 9 and 14 percentage points during the hungry season (Table 4). The point estimate is largest in column (5), which includes individual fixed effects. Importantly, the effect is specific to the hungry season: the trading probability in the following harvest season reverts to close to the trading rates observed after the first harvest. At the risk of over-interpreting the data, we note that the slightly higher trading rates in the 2015 harvest season are consistent with a greater likelihood of trading following lower yields durin the 2015 harvest (see Fink, Jack, and Masiye, 2018, for details).

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.

[14] We observe that the preferences for Boom versus salt (our most common item pair) do not vary much over the seasons. While our data from the *choice* condition suggest that Boom is slightly more attractive in the hungry season (i.e., 65 percent of participants choose Boom over Salt) than in the harvest season before (60 percent) and after (57 percent), the differences are not statistically significant (Fisher's exact test, $p - values > 0.3$).

To address these endogeneity concerns, we exploit random variation in liquidity associated with access to hungry season consumption loans. The larger RCT, in which we have embedded the exchange experiments, relaxed liquidity constraints in 80 randomly selected villages during the hungry season. We compare trading probabilities for households with and without access to the loans at the time of their exchange decision. Loans were delivered in early to mid January 2015, while the exchange experiments began in early February, about two weeks after the liquidity increase. Appendix Figure A.2 shows that the biggest effect on consumption occurred very shortly after receiving the loan.[15] Figure 3 plots the effect of the loan, as a function how recently it was received (in weekly bins). The pattern is striking, though standard errors are large: two weeks after receiving a loan the likelihood that a participant trades her endowed item plummets by over 30 percentage points relative to the control group. However, this effect wears off quickly. Three weeks after loan delivery the likelihood of trading is 16 percentage points lower than in the control group and levels off at a 10 percent lower trading likelihood in the weeks after.

Table 5 shows a series of loan dropoff impact estimates using an increasing number of controls in the empirical model. On average, we find that a loan dropoff within the last month increased exchange asymmetries by around 10 percentage points (again, imprecisely estimated), which is remarkably similar to the seasonal variation found in the previous section (columns 3 and 4). The last two columns show the estimates underlying Figure 3. The estimates indicate that the effect dissipates quickly after receipt of resources, which is robust to the inclusion of a full set of controls. The short-lived effect of the loan impact suggests that the loaned resources were quickly depleted.

5.4 Value of the traded goods

A primary mechanism through which scarcity may affect decision-making is the relative value of goods. While the items used in this experiment are of relatively small value for high-income populations, they are of substantial value to lower-income populations. Even though trading over higher value items does not necessarily imply higher stakes – especially if differences in item features are small (e.g. a black vs. a gray car) – higher value items may still garner greater attention or focus, because they make the trade off more salient. To directly assess the extent to which the value of items shapes exchange asymmetries, we introduced a high-value item pair (Solar – Cash

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

condition) in the last round of our experiments. More precisely, we offer participants the choice between a solar lamp or an equivalent value cash transfer of 80 Kwacha (USD 14). As shown in Table 3, relative to the estimated trading probability for lower value items, we find a significant and large increase in the trading probability for the high-value item pair. While on average only 34 percent of participants traded in the three standard-value item pairs, 44 percent traded in the Solar-Cash treatment. This strong increase in trading is remarkable as we implemented this treatment after harvest when participants were relatively rich.

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. As indicated in Table 2, 55 percent of participants prefer the solar lamp over the equivalent amount of cash. Despite these roughly balanced preferences, we see that initial assignment has an effect. Participants assigned a solar lamp at the outset were 10 percentage points more likely to end up with a solar lamp than participants assigned cash. This is robust to controlling for variation in experimental procedures (*voucher* procedure) in column 3, but becomes insignificant once we control for experience and household characteristics (column 2 and 4). Overall, the exchange asymmetries observed in the high-value decisions are only about one third of the magnitude of the asymmetries observed with standard-value items and mostly insignificant.

5.5 Cognitive ability

The results on trading presented in the previous section consistently point to more rational behavior during times of resource scarcity. While this is consistent with predictions from rational inattention models (Sims, 2003; Maćkowiak et al., 2018) and with some previous empirical research (Shah et al., 2015), other research suggests that scarcity may be detrimental to decision-making through its negative impact on cognitive function (Mani et al., 2013; Mullainathan and Shafir, 2013; Dean et al., 2017). To investigate the relevance of this channel, we explore both the relationship between cognition and scarcity and between cognition and trading probabilities.

We administered two commonly used tasks to measure cognitive function to a randomly selected subsample in each survey round: a numerical version of the Stroop test and Raven's Progressive Matrices (RPM).[16] As described in further detail in Appendix A.4, the RPM is a measure

[16] These are also the tests used in Mani et al. (2013). 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

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. The Stroop test is a measure for inhibitory control, which is one particular domain of executive functioning skills that regulates the ability to control impulsive reactions. 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). While in task 2 the displayed digits match their number (e.g., four 4s, congruent task), this is not case in the third task (e.g. 444, incongruent task), and thus requires participants to suppress the automatic response (i.e., 4). While the Stroop test is sometimes scored by normalizing the score on the incongruent task (task 3) by the score on the congruent task (task 2) (e.g., Scarpina and Tagini, 2017). We instead examine each of the tasks separately, and interpret the neutral and congruent tasks as a measure of attention and the incongruent task as a measure of inhibitory control. All outcomes are normalized to a mean of zero and a standard deviation of one, with a higher score corresponding to better performance.

We begin by testing whether cognitive ability varies with scarcity. We follow the three relevant scarcity measures in the order they are investigated above. First, Figure 4 shows that higher asset households have a significantly higher scores on the Raven's Progressive Matrices test and across the main two Stroop measures. In the cross section, wealthier households perform consistently better on these cognitive measures.

Second, we examine seasonal variation in scarcity. Table 7 shows the results from regressing the cognitive measures on a dummy for the hungry season and the following harvest season (i.e., our experiment round 3), controlling for test experience in Panel A and including individual fixed effects in Panel B. We focus on Panel B in summarizing the results. Column 1 shows that fluid intelligence as measured by the RPM is lower in both of the later two survey rounds, though differences are not statistically significant between any two rounds. For the Stroop test, we analyze four different measures (see Appendix A.4 for a detailed description of how we construct these test scores). These scores reveal a more nuanced relationship with scarcity. First, the higher Stroop scores on the neutral (column 2) and congruent task (column 3) during the hungry season suggest that participants perform better during the hungry season on easy tasks, which could be interpreted as evidence for increased effort or attention. Second, the incongruent task also improves during the hungry season, but to a lesser degree (column 4). As a result, the normalized

function.

score on the incongruent task (column 5) declines during the hungry season. We interpret this final result as an artefact of the scoring rather than capturing important variation in inhibitory control by season. We also note that if we restrict our sample to replicate the set up in Mani et al. (2013) by using before-after variation around the 2015 sample and including only respondents who completed these tests for the first time in the pre-harvest period, we replicate their findings for the Ravens Progressive Matrices test (scores improve following harvest) and find no effect for the Stroop measures.

Next, we examine how these measures are affected by exogenous variation in liquidity, following Section 5.3. Figure 5 shows how our four main cognitive measures vary with loan access. Overall, estimates are noisy due to the relatively small sample size and large variation in the outcomes. Like in the seasonal analysis above (Table 7) we observe no systematic pattern for fluid intelligence (RPM). The results for the Stroop measures are, if anything, in opposition to what we observe in response to the seasonal variation: short-term access to liquidity insignificantly improves performance on the incongruent task, and to a larger degree than any effect on the neutral or congruent tasks. Overall, these results suggest an inconsistent relationship between our alternative measures of scarcity and cognitive performance on the same sets of tests.

Finally, we test whether trading probabilities are correlated with cognitive function. Table 8 shows no significant relationship in the pooled analysis (Panel A), which is unsurprising given the contradictory results on the relationship between cognitive perofrmance and scarcity. However, if we focus on the within-respondent variation in cognition and trading probabilities, we see a positive correlation between the neutral and congruent tasks and trading probabilities. This is driven by seasonal variation in both, where we find a consistent improvement both in these simple attentional measures of cognition and in decision-making.

6 Conclusion

Although the endowment effect has been extensively documented in laboratory settings, its relevance in day-to-day decision-making remains unclear. The results presented in this paper suggest that exchange asymmetries generalize to a setting with poor households and are surprisingly pronounced and persistent. The overall propensity to trade familiar household items is about 16 percentage points lower than predict by standard theory, on average. The large number of experiments conducted as part of this study allowed us to test an extensive set of experimental procedures and to rule out a range of behaviors that could drive exchange asymmetries. However, our results do suggest a strong relationship between the magnitude of exchange asymmetries and the value of items traded as well as economic circumstances. In particular, we find that exchange asymmetries are substantially smaller when resources are more scarce, and when the value of the traded goods is higher.

A leading explanation for the observed exchange asymmetries is loss aversion, i.e., the notion that the disutility from giving up a good is higher than the utility gain from obtaining it.[17] Our results suggest that scarcity reduces the scope of loss aversion during the hungry season, as farmers appear to put greater focus on the trade-off between the two items and may treat the trading decision as opportunity cost rather than a loss of the endowed item.

The prevalence of exchange asymmetries and that they are sensitive to scarcity has implications for markets in general, and for development in particular. An unwillingness to give up existing assets, goods or acquired rights, may explain, for example, why (small) business owners or farmers forego profitable exchanges or investments (e.g., Kremer et al., 2013), why individuals resist policy changes (e.g., Alesina and Passarelli, 2017), why loan take-up is low among Kenyan dairy farmers (Carney et al., 2018), and the reluctance to adopt new technology Gine and Goldberg (2017). Our result that exchange asymmetries vary with scarcity indicates that such an unwillingness is highest in times of relative abundance, a point in time when, for example, investments are most viable. Accordingly, opportunities to implement change or to adopt new technologies may not only be population specific, but may also be strongly influenced by temporal and seasonal variations in scarcity. This may introduce potentially new ways to harness prevalent exchange asymmetries or design policies that help households avoid related biases.

Our study sheds further light on the psychology of the poor by investigating the prevalence and generalizability of a prominent decision bias in a rural agricultural setting. Overall, our results do not support the hypothesis that scarcity increases decision biases. We do, in fact, find the opposite: seasonal and cross sectional scarcity moves behavior more closely to the standard theoretical prediction in in our study setting. Furthermore, there is no indication that cognitive function is systematically lower under scarcity. While results are sensitive to which cognition measures we use, within-subject variation in scarcity suggests that higher-order measures of cognition (fluid intelligence and inhibitory control) are unaffected by scarcity, while attention actually

[17]This explanation is much disputed, but more recent theories of reference-dependent preferences provide a flexible framework, preserving the loss aversion explanation, to reconcile a host of empirical findings (e.g., Kőszegi and Rabin, 2006).

improves when resources are scarce, which is associated with reductions in measured exchange asymmetries. Because the trading decisions in our experiment involve necessary consumption items, it is likely that the trade-offs across items are particularly salient when resources are most scarce (see e.g., Shah, Shafir, and Mullainathan, 2015). Other measures of decision-making that involve longer-run consequences or more subtle trade-offs might have resulted in a different set of findings.

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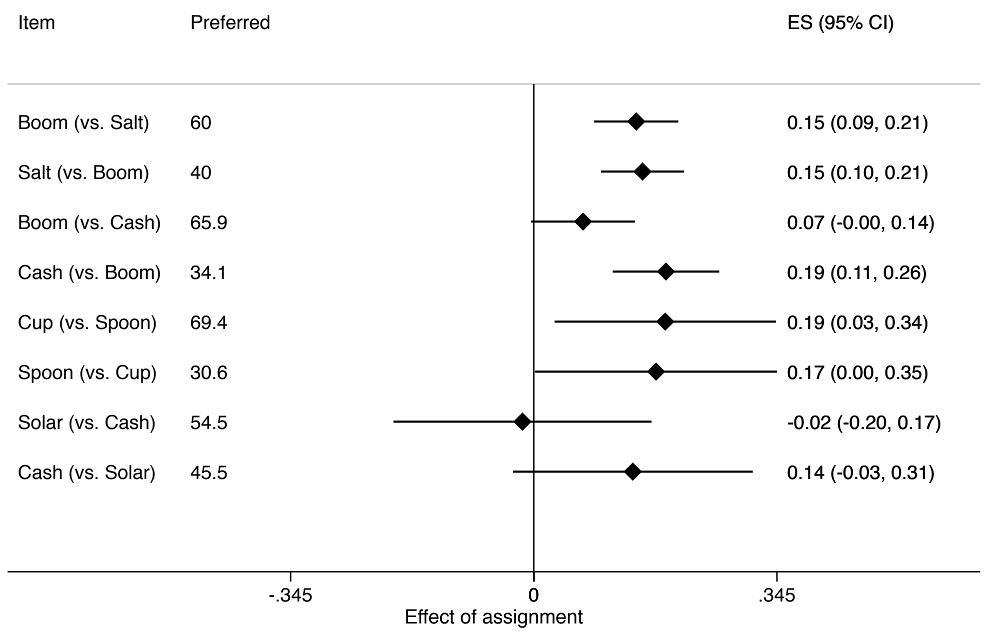

Figure 1: Asymmetries by item preference

Notes: Estimated change in the likelihood of ending with 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 measured estimate along with 95% confidence interval.

Figure 2: Probability of trading start item, by season

Notes: Relationship between season of survey and trading probabilities, conditional on individual experience with the trading decision. Analysis is conditional on item pair and procedure indicators and individual and household controls. 95% confidence intervals are based on standard errors clustered at the village level.

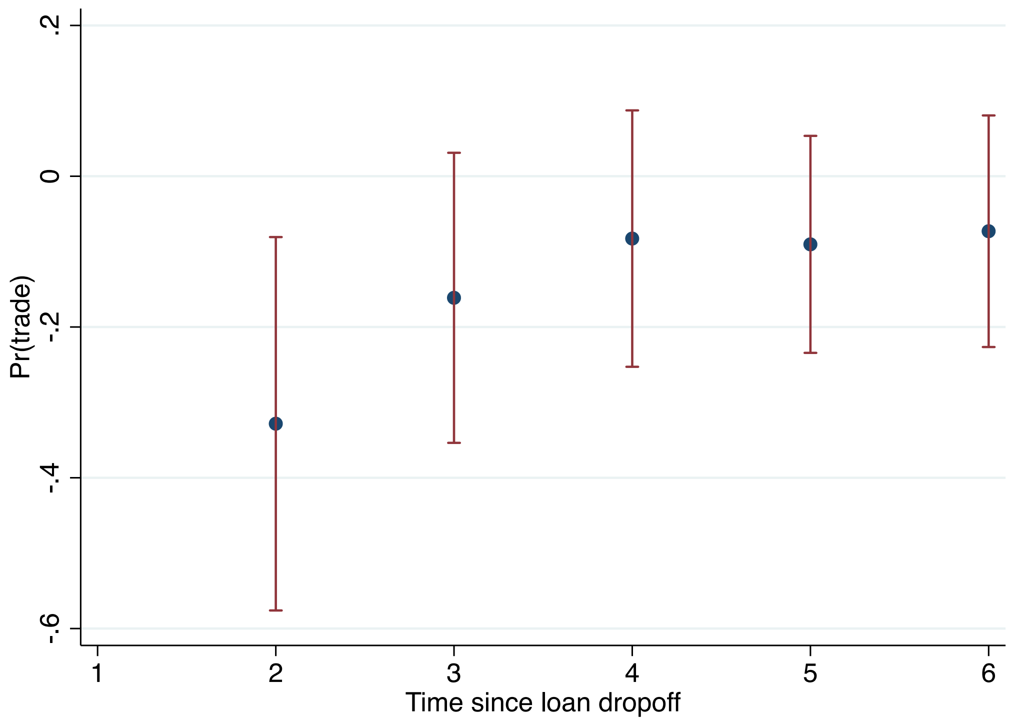

Figure 3: Relationship between weeks since loan receipt and trading probabilities

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

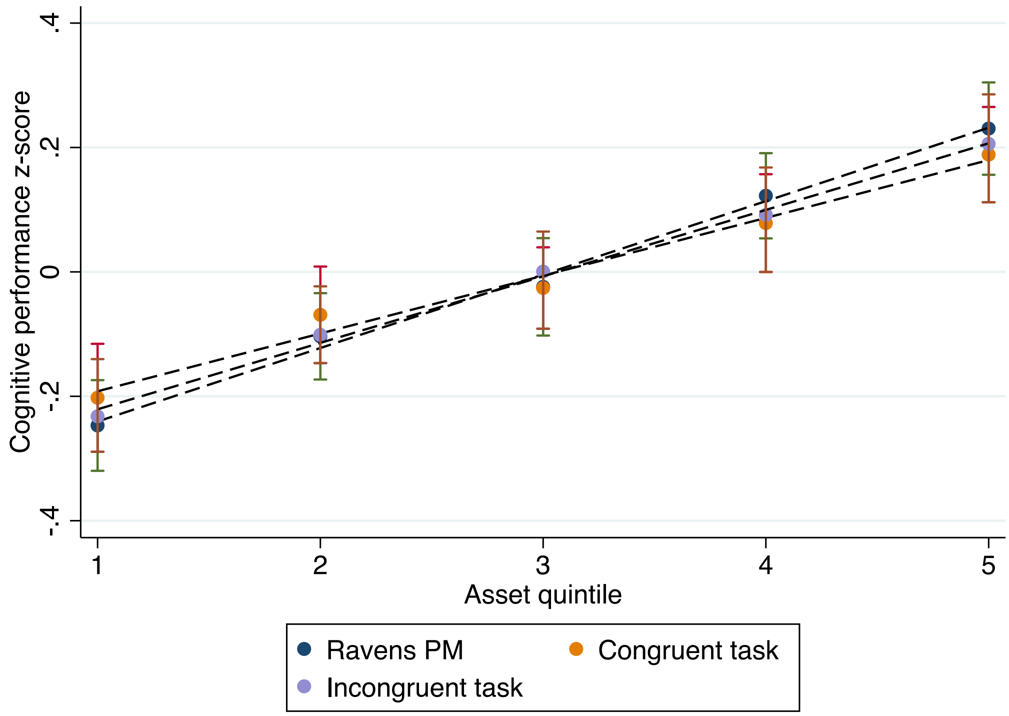

Figure 4: Relationship between weeks since loan receipt and cognitive performance

Notes: Relationship between cognitive peformance, measured through the Stroop test, and baseline assets. The analysis controls for individual and household characteristics, season, and participant experience with the cognitive tests.

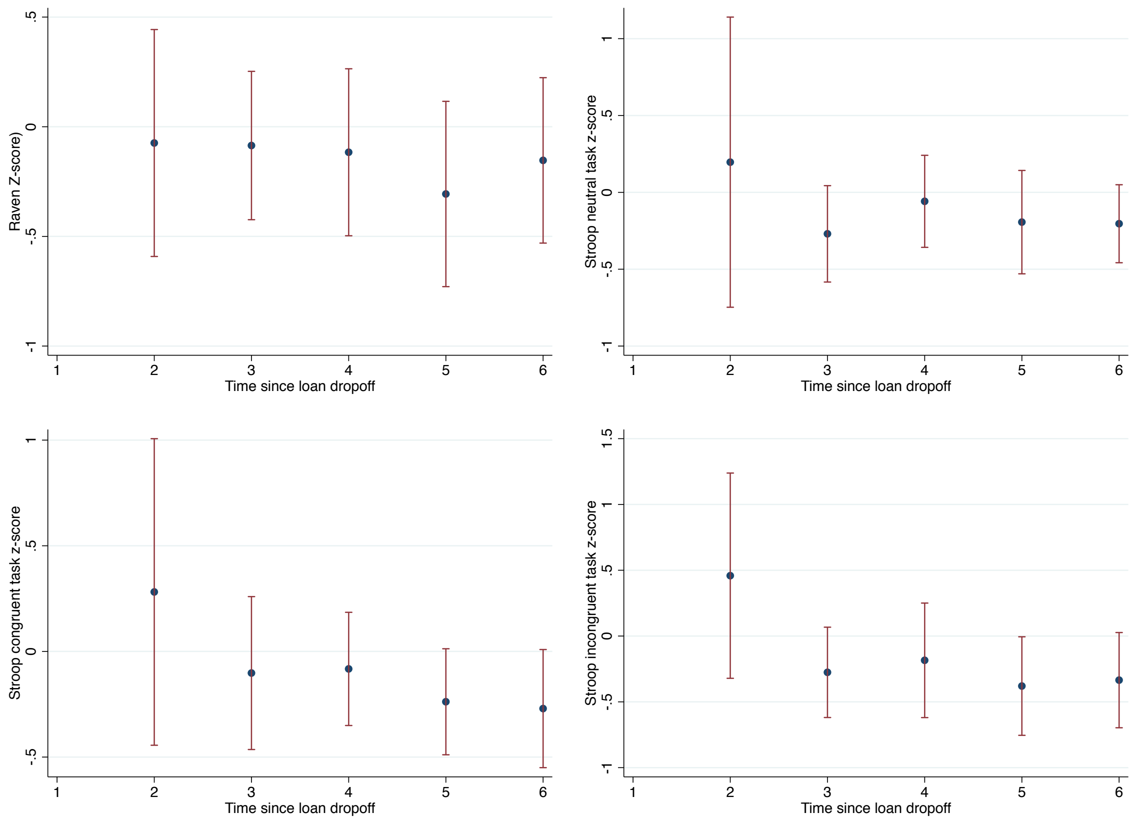

Figure 5: Relationship between weeks since loan receipt and cognitive performance

Notes: Effect of loan timing on cognitive task z-scores, where time since loan dropoff is measured in weeks and z-scores are normalized so that a higher value indicates better performance. The omitted category is the control (no loan) group and results are conditional on week of survey fixed effects, and a full set of procedure, experience and item pair indicators and individual and household controls. 95% confidence intervals are based on standard errors clustered at the village level.

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

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

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

	Probability that subject traded start item			
	(1)	(2)	(3)	(4)
Panel A. By item pair				
Boom-Cash	0.022	0.004	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)
Hungry Season	0.089***	0.096***	0.099***	0.105***	0.145***
	(0.022)	(0.022)	(0.021)	(0.031)	(0.054)
Endline	0.066***	0.074***	0.078***	0.054*	0.125*
	(0.019)	(0.019)	(0.020)	(0.033)	(0.069)
Trading experience			-0.007	-0.009	-0.048
			(0.016)	(0.016)	(0.046)
Controls	none	hh	hh + experience	hh + experience + procedure	experience + procedure + FE
Observations	5172	5171	5171	5171	5172

Notes: Linear regressions of an indicator for whether the subject traded the start item, by season. Trading experience indicates that the subject was in a previous round of trading. Individual fixed effects are in cluded in column 5. Standard errors clustered at the village level.

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

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

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

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

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

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

Table 7: Seasonal variation in cognitive ability

	Ravens score	Stroop task 1	Stroop task 2	Stroop task 3	Task 3 / Task 2
	(1)	(2)	(3)	(4)	(5)
Panel A: Pooled OLS					
Hungry Season	-0.099*	0.297***	0.373***	0.091*	-0.263***
	(0.056)	(0.051)	(0.055)	(0.052)	(0.053)
Endline	-0.135**	0.019	0.031	-0.115**	-0.199***
	(0.055)	(0.054)	(0.052)	(0.048)	(0.051)
Test experience	0.110***	0.206***	0.177***	0.150***	-0.065**
	(0.025)	(0.027)	(0.026)	(0.026)	(0.026)
Panel B: Individual fixed effects					
Hungry Season	-0.062	0.362***	0.405***	0.149**	-0.307***
	(0.066)	(0.067)	(0.071)	(0.068)	(0.086)
Endline	-0.045	0.190	0.201*	0.118	-0.182
	(0.093)	(0.117)	(0.111)	(0.103)	(0.139)
Test experience	0.031	0.086	0.037	-0.026	-0.095
	(0.050)	(0.066)	(0.062)	(0.061)	(0.076)
Observations	4771	4503	4549	4529	4472

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 8: 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	Task 3 / Task 2
	(1)	(2)	(3)	(4)	(5)
Panel A: Pooled OLS					
Cognitive measure	-0.006	0.001	-0.003	-0.003	-0.006
	(0.007)	(0.008)	(0.008)	(0.009)	(0.008)
Panel B: Individual fixed effects					
Cognitive measure	0.006	0.050**	0.036*	0.024	-0.026
	(0.015)	(0.020)	(0.019)	(0.019)	(0.016)
Observations	4280	4039	4082	4063	4009

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.

A.1 Appendix: Tables and figures

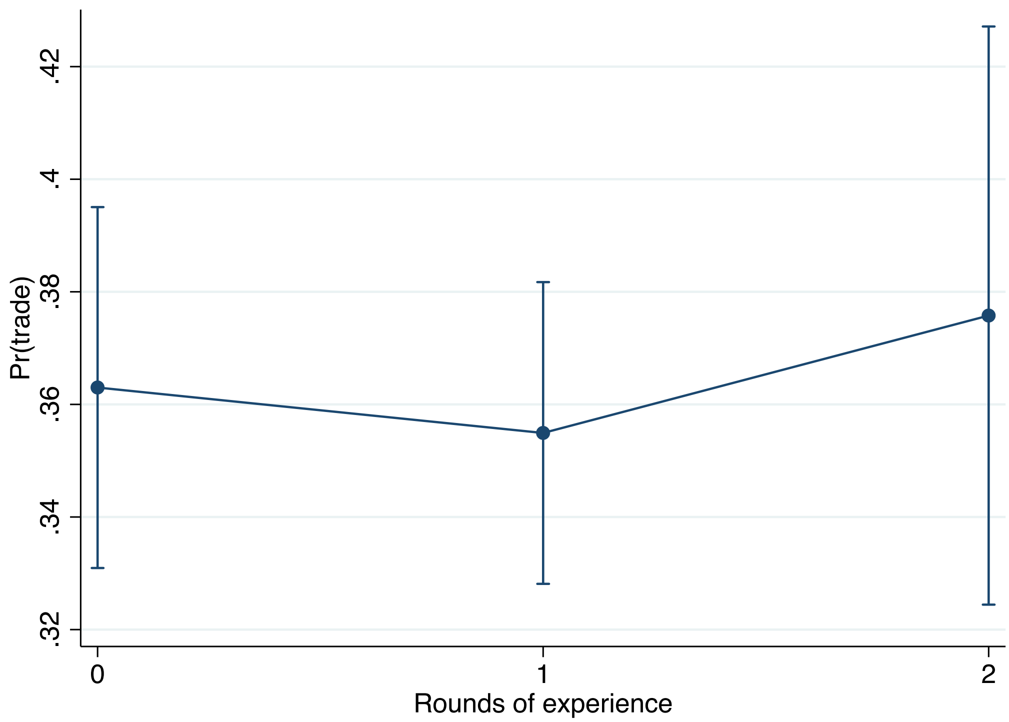

Figure A.1: Probability of trading start item in Boom – Salt pair, by rounds of participant experience

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

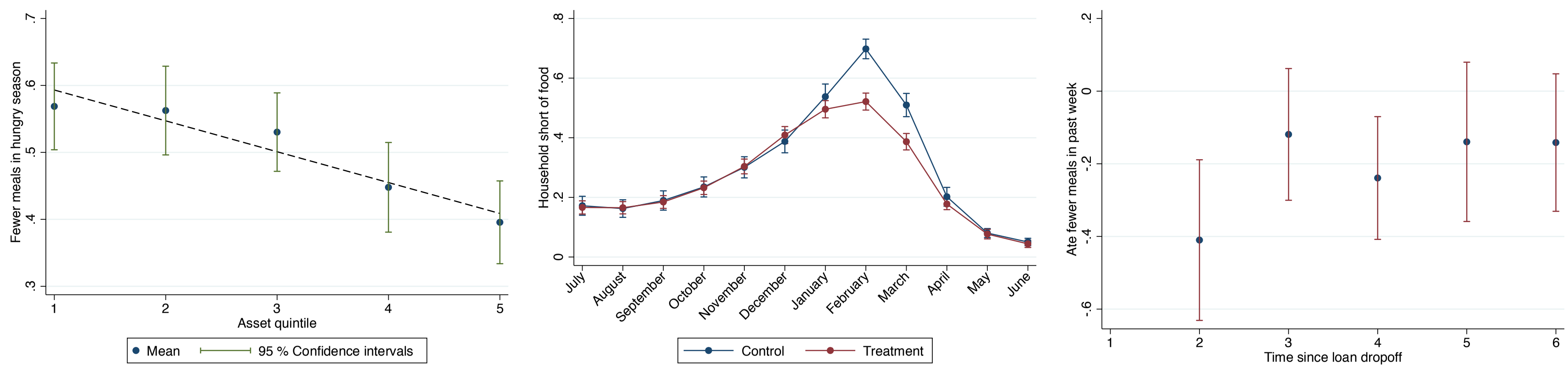

Figure A.2: Variation in consumption and food availability by source of variation in scarcity

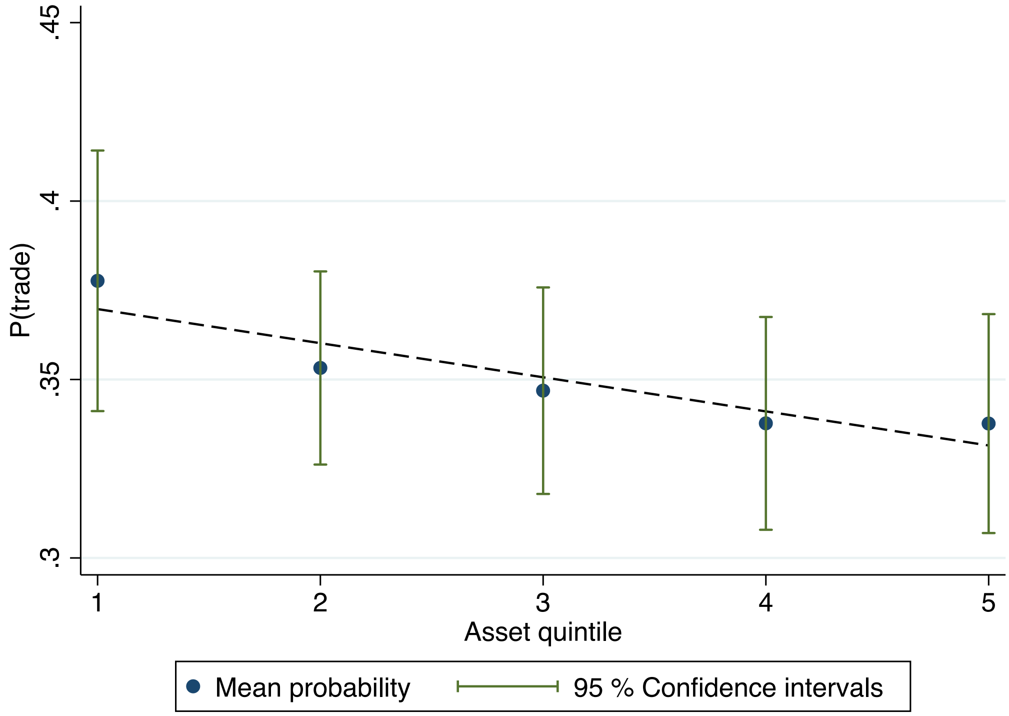

Figure A.3: Probability of trading start item by baseline assets

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

Table A.1: Balance: Rounds

	Round 1 (1)	Round 2 (2)	Round 3 (3)
Age of hh head	42.71	-0.14	0.15
	[14.74]	(-0.32)	(0.45)
Female headed hh	0.24	0.02	0.01
	[0.43]	(1.35)	(1.54)
Children under 5	0.96	-0.04	-0.01
	[0.93]	(-1.78)	(-0.74)
Children 5-14	1.81	-0.03	-0.04
	[1.50]	(-0.83)	(-1.27)
Adults 15-64	2.45	0.03	0.01
	[1.25]	(0.90)	(0.28)
Adults over 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 (1)	Boom-Cash (2)	Cup-Spoon (3)	Solar-Cash (4)
Age of hh head	42.69	0.48	-0.83	0.08
	[14.95]	(0.95)	(-1.13)	(0.08)
Female headed hh	0.25	0.01	-0.03	-0.01
	[0.43]	(0.99)	(-1.63)	(-0.61)
Children under 5	0.94	-0.03	0.07	0.03
	[0.90]	(-1.15)	(1.71)	(0.37)
Children 5-14	1.77	0.03	0.02	-0.00
	[1.51]	(0.56)	(0.30)	(-0.02)
Adults 15-64	2.43	0.06	0.06	0.12
	[1.23]	(1.54)	(1.13)	(1.77)
Adults over 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: 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.5: 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.

A.2 Appendix: Trading and Market Access

One possible explanation for 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 relatively inconsequential. While this hypothesis is not obvious in our setting where most villages are quite remote (average distance to the district capital is 30 miles on dirt roads), one could argue that many goods can be traded locally and that visits to markets may be common enough to allow for such trades. If this was the case, we should see that easier market access should increase the reluctance to trade.

To investigate this hypothesis, we regress trading outcomes on a series of market access proxies, and summarize results in Appendix Table A.6. All regressions control for survey round, item pair, experimental procedures, experience, and household characteristics. Living in a small village (25th percentile of village population size (28 households) 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).

Market access or more specifically trading opportunities outside the experiment might also be driven by the fact that, in most villages, only a portion of village households was ever treated in our exchange experiments in round 1, which should, in theory, allow for opportunities to trade within the village. Columns 4-5 of Appendix Table A.6 show that the number of households in the exchange experiment and the number of households that received Boom as the default compensation for taking the survey are unrelated to the likelihood of trading. Overall, most of the estimated market access proxies go in the expected direction, but effect sizes are all small and statistically insignificant, and therefore are unlikely to explain the larger patterns observed.

Table A.6: 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)	
Number of households given boom					0.011**
					(0.004)
Number of households in sample				-0.000	-0.000
				(0.000)	(0.000)
Observations	5171	5171	5171	4953	2115

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. Column 5 is restricted to participants endowed with Boom. 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.3 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.7 we estimate the impact of item assignment on participants' hypothetical WTA/WTP. The regressions show two key results. First, we observe that the constant in columns 1 and 3 (no other controls), which reflects participants' willingness to pay for an item, is close to its market price (i.e., 3-3.5 Kwacha for Boom and 80 Kwacha for the solar lamp). Second, the initial assignment matters for the low-value item (Boom) but not for the high value item (Solar). Specifically, respondents initially endowed with Boom require a significantly higher price to part with their item. The estimated differences between a participant's WTA and WTP is on average 1.5 Kwacha, which corresponds to an increase of about 50 percent of the average WTP of participants who are initially assigned cash. This finding is in contrast to the results for the high-value item. Participants who are initially endowed with the solar light do not display a higher WTA. In fact, their WTA is approximately the same as the WTP of participants starting with cash. Results are similar when we condition on all controls (columns 2 and 4). These results support our findings that raising the value of items leads to more rational behavior in our study sample.

Table A.7: 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. Subjecsts 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.4 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 (Dean, Schilbach, and Schofield, 2017; Scarpina and Tagini, 2017). Stroop tests exist in a variety of formats, including colors, shapes and day-night variations. For the purpose of this study, we used a numeric version of the Stroop test, which required participants be able to read numbers 1-7, but did not require an ability to read or write more broadly.

To assess basic cognitive functioning, we also administered a subset of 10 items from Raven's Progressive Matrices (RPM) test battery (Raven, 1983). These items were pilot-tested and calibrated to be of medium difficulty for the average respondent. RPM are a nonverbal test designed to measure fluid intelligence, which is the ability to solve novel problems and recognize patterns and relationships independent of acquired knowledge. Prior to the RPM, all participants went through four practice examples. In each case – both for the practice rounds and for the actual test items – an image with a basic pattern was first shown to the study participant, and they had to choose a matching shape and pattern from six possible answers. A sample decision task is provided in Figure A.4.

For the analysis, we used a two-parameter logistic model (2PL) to construct a single score for each participant. Internal consistency of the 10 item scale is high, with an estimated Cronbach's alpha of 0.75. We also assessed a simpler linear score, summing up all correct responses. The correlation between the latent factor model score and the linear scale score is 0.99. To facilitate interpretation of regression coefficients, we normalize both scores to mean zero and standard deviation 1. Appendix Figure A.5 illustrates the overall distribution of the scores.

The numeric Stroop test involved two steps in our study. In a first step, we verified participants' ability to read numbers by presenting them with 6 single digit numbers. Subjects who were able to identify the majority of these numbers were then allowed to take the main Stroop test. Out of 4,719 participants, we excluded 282 participants (6 percent) due to lacking numeracy. The second step was the main Stroop test which involved three tasks with 25 trials each. In the first task (neutral task), participants were asked to state the number of objects they saw in a trial. Objects were circles and crosses; each trial contained between 1 and 7 identical objects. In the second task, objects were replaced with numbers; once again, participants had to count the number of digits in each trial. In this second task of the Stroop test, printed numbers always matched the number of objects (e.g. four "4"s or six "6"s) - a congruent stimulus condition, with both information sources providing the same information. In the last round, participants had to count objects once again, but this time the objects were single digit numbers that did not match the number of objects in each trial (incongruent task). Figure A.6 excerpts four trials from each task.

As highlighted in a recent review on the Stroop test, researchers have used a wide variety of approaches to score Stroop tests (Scarpina and Tagini, 2017). In our primary analysis, we follow the scoring modalities outlined in Stroop (1935), which proceeds in two steps. First, we computed the error corrections for the time to complete each of the three tasks. The penalty for each incorrect question proposed by Stroop is twice the median time needed for each row, which corresponds to 1.8 seconds in our sample. The median number of mistakes was 1 in the neutral condition, 0 in the congruent condition, and 2 in the incongruent stimuli condition. The error-corrected time for a task is then calculated as the total time plus the number of mistakes times the penalty. To compute participants' ability to control interference, we then 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 this difference variable 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 of the last round (incongruent task 3). The median time for completing the incongruent task was 42 seconds (mean 45), while the median number of mistakes was 2 (mean 2.4). Appendix Figure A.7 shows the correlation between total time needed for this task and the difference-based Stroop score. The correlation between task time and normalized inference scores is -0.38 in our sample.

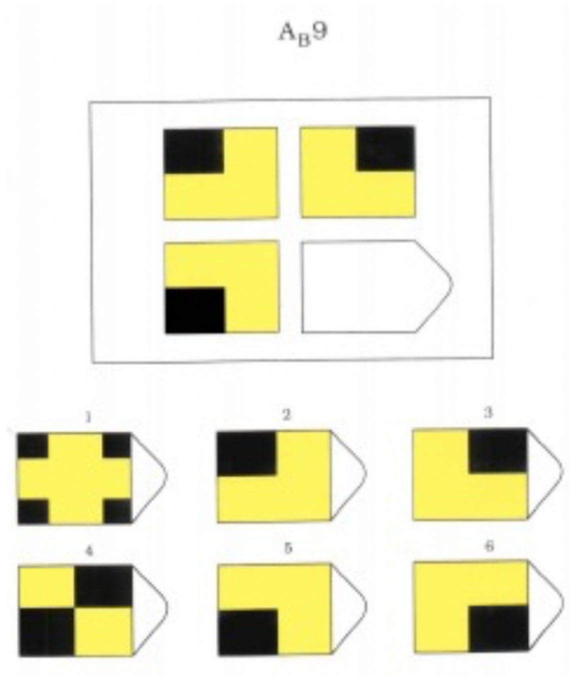

Figure A.4: Ravens Matrices: Sample decision task

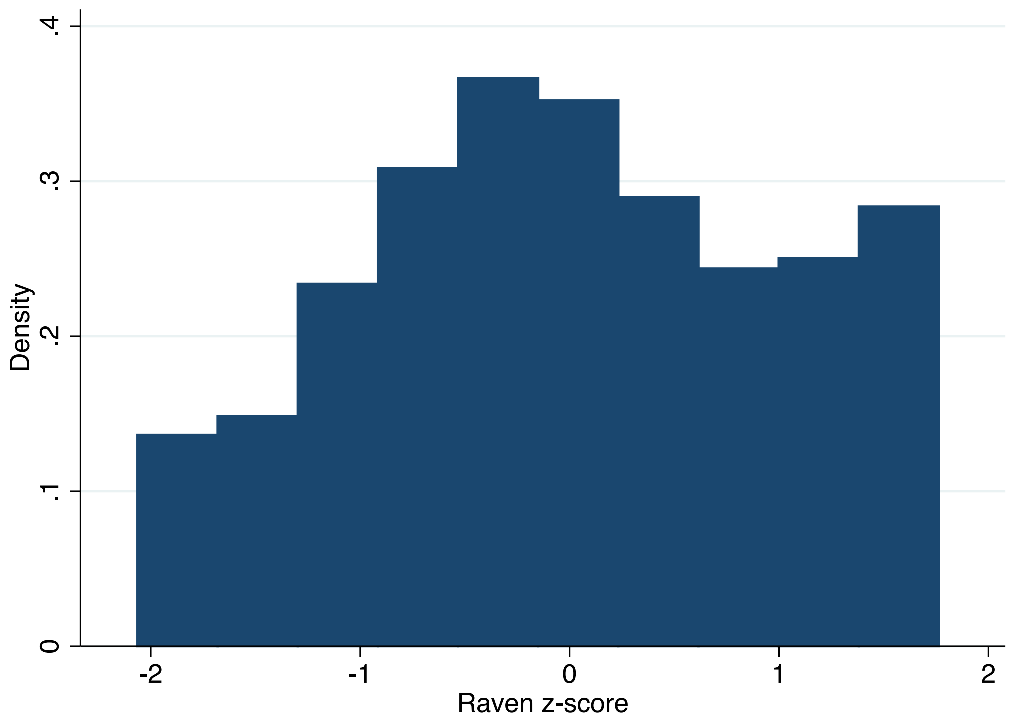

Figure A.5: Distribution of scores in the Raven's Progressive Matrices (RPM)

Congruent Task 1

1	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.6: Stroop: Sample decision tasks

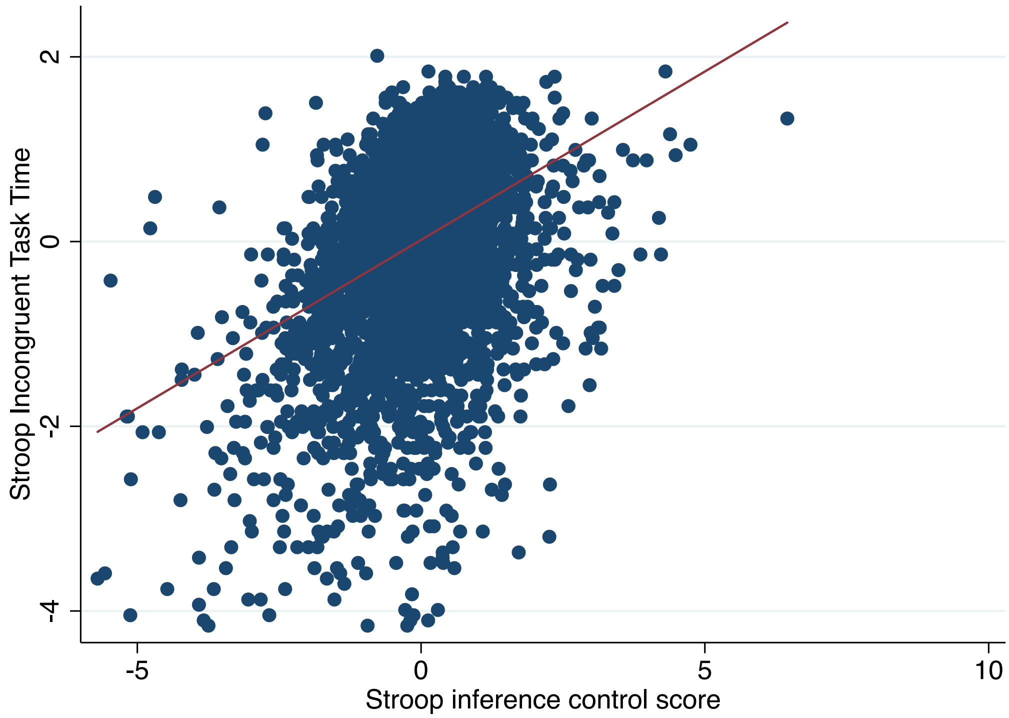

Figure A.7: Correlation between needed in the incongruent task 3 and the difference-based Stroop score (both coded as z-scores)

A.5 Appendix: Scripts and Protocols

A.5.1 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 subjects 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]?