Poor and Rational: Decision-Making under Scarcity

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We investigate the link between poverty and decision-making in a sample of farmers in Zambia, who were given the opportunity to exchange randomly assigned household items for alternative items of similar value. Analyzing a total of 5,842 trading decisions and leveraging multiple sources of variation in financial constraints, we show that exchange asymmetries decrease in magnitude when participants are more constrained. This result is robust to experimental procedures and is not mediated by changes in cognitive performance. Consistent with the interpretation that scarcity leads to more rational decisions by increasing the utility loss from forgone trading, we show that trading probabilities go up when the market value of the items is exogenously increased.

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I. Introduction

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

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

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

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

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

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

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

Across all three sources of variation, we find the same pattern: greater scarcity is associated with reduced exchange asymmetries. First, households in the bottom asset quintile are about 5 percentage points more

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

[4] The consumption loans provided households with three bags of maize or the cash value equivalent at the start of the hungry season. Repayment was due at harvest time, approximately 6 months later, with 5% monthly interest. Around 90% of eligible households took up the loan, and around 80% fully repaid it in the year that the exchange experiments were conducted. Further details are provided in sec. IV.B and in Fink, Jack, and Masiye (2020).

likely to trade than households in the top quintile. Second, participants are 7–12 percentage points more likely to trade during the hungry season than at harvest time. Third, we find that households without access to the randomized hungry-season loan intervention are about 18 percentage points more likely to trade than households that received a grain or cash loan in the 3 weeks before the decision experiment. Together, these results provide robust evidence of decision-making that is closer to the normative benchmark when households are more financially constrained.

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

Next, we examine the potential mechanisms that lead to more rational decision-making when financial resources are more scarce. First, we investigate "tunneling," which suggests that scarcity leads to a focus on immediate financial concerns, absorbing cognitive bandwidth and diverting attention away from other decisions (Mani et al. 2013; Mullainathan and Shafir 2013). Following this literature, we implemented a standard set of unincentivized cognitive tests in a subsample of participants, and we test the effect of scarcity on cognitive performance using the same cross-sectional, seasonal, and randomized variation in financial resources used in our main analysis. Even though we see a strong positive cross-sectional relationship between wealth and cognitive performance, as predicted by the human capital literature (e.g., Laajaj and Macours 2021), we

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

find no clear relationship between these measures and seasonal or experimental variation in scarcity in our sample. More generally, we find that cognitive scores are not predictive of exchange decisions in our setting, which allows us to also rule out alternative explanations for the relationship between scarcity and decision-making, such as variation in the opportunity cost of time or in alcohol consumption, both of which should also affect performance on the cognitive tests.

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

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

Second, this paper adds to the ongoing debate about the robustness of behavioral anomalies in general (Levitt and List 2008; Falk and Heckman 2009; Camerer 2015; Charness and Fehr 2015; Kessler and Vesterlund 2015) and the endowment effect in particular (Ericson and Fuster 2014). Despite a large literature on the endowment effect, evidence from outside of the laboratory, and particularly from low-income settings, is relatively scarce.[6] We present field evidence involving transactions large enough to have a meaningful impact on household well-being. In this way, our work relates to an influential series of experiments at sports card shows in the United States demonstrating the relationship between the endowment effect and market experience (List 2003, 2004). On average, trading rates in that setting are similar in magnitude to our pooled results, though professional dealers are significantly more likely to trade their assigned baseball memorabilia than nondealers (List 2003). Related work by Tong et al. (2016) shows that trading experience reduces reliance on the (impulsive) use of loss-aversion-linked neurological processes. Our results suggest that similar shifts away from impulsive decision-making could also occur under scarcity. In a setting more similar to ours—and, to our knowledge, the only other experimental measurement of exchange asymmetries in a low-income country—Apicella et al. (2014) show that, in a population of hunter-gatherers, participants with more exposure to markets display a stronger endowment effect than those with less market exposure. Both professional sports card traders and hunter-gatherers with little market access arguably face higher stakes in their trading decisions than do amateur traders or hunter-gatherers with more market access, respectively, and may therefore devote greater attention to trading decisions, consistent with our preferred interpretation of our own findings.

Finally, a growing number of field studies in developing countries document real-world behavior consistent with an endowment effect. For example, Anagol, Balasubramaniam, and Ramadorai (2018) find that winners of an initial public offering in India are more likely to hold on to their shares than nonwinners. Giné and Goldberg (2018) find that prior

[6] Some recent work leverages more easily accessible online panels (Chapman et al. 2017; Fehr and Kuebler 2022). Fehr and Kuebler (2022), e.g., provide evidence on small-stakes exchange asymmetries in a representative sample of Germans and show that trading behavior correlates with migration choices and stock market participation.

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. 2022). Our study bridges the lab and field literatures by studying the effect of both natural and induced sources of variation in financial resources on real-stakes decisions. Notably, while the specific magnitudes of our findings may not generalize, the robustness of the results to different types of scarcity suggests that the endowment effect varies in predictable ways, depending on economic circumstances, and is substantially less pronounced when financial resources are more scarce.[7]

II. Study Setting and Experimental Design

A. 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) live in rural areas, and most rural households rely on small-scale farming as their primary source of income. Agriculture is rain fed, and agricultural incomes are low. In 2013, average annual household income was around 3,000 kwacha, which corresponded to approximately 600 USD at the time.[8] With 5–6 household members on average, income per capita is substantially less than 1 USD per day. The rain-fed 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 because of a lack of food. With early crops becoming available in April, food shortages and hunger usually spike between January and March (Fink, Jack, and Masiye 2020).

B. Experimental Design

The experiments reported here were embedded in household surveys conducted as part of a randomized evaluation of a seasonal loan program (see Fink, Jack, and Masiye 2020 for further detail on the randomized evaluation). As part of the evaluation, households were surveyed up to four times per year. In the first year of the study, all farmers received a small box of a commonly used washing powder (called "Boom," the local brand name) as compensation for their time at the end of the survey. In the

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

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

second year of the study, rather than providing Boom to all households, we implemented a modified version of the Knetsch (1989) exchange paradigm with a subset of households in each survey, explained in detail below. We conducted the decision experiments between July 2014 and September 2015 with a total of 3,059 households across 175 villages. Households were randomly phased in to participation, resulting in between one and three decisions per household over the study period and a total of 5,842 individual decisions. Households not participating in the exchange experiment received the standard compensation (Boom) at the end of each survey. All household surveys were conducted by trained interviewers with adult household representatives—typically the male or female head of household—in their homes, used electronic survey devices (tablets), and took between 1 and 2 hours.[9]

1. Decision Task

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

We follow the laboratory literature, most notably Plott and Zeiler (2007), and consider several variants on the baseline procedure described above. First, we varied the method of item assignment. Specifically, we either implemented the randomization of items directly through the electronic survey devices (standard assignment) or randomized items in front of participants (*lottery assignment*), that is, either through a coin flip or by participants drawing a button out of a bag.[11] The main goal of the lottery

[9] Priority was given to surveying the household head or the participant in prior survey rounds; when that person was unavailable, the spouse or another adult permanent member of the household was surveyed instead. Fourteen percent of households have two different participants in the data; less than 1% have three. We track the participant ID and use it to examine within-subject variation in decision-making over time.

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

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

assignment is to minimize the risk of possible inference about the relative valuation of items or signaling by the experimenter associated with the standard assignment.[12]

Second, we implemented three subprocedures designed to reduce participants' attachment to the assigned item: (1) we shortened the time between the assignment of items and the trading decision, with some participants receiving the assignment only minutes before the trading opportunity (*timing* procedure), (2) we used vouchers redeemable for the specific item, rather than handing over the item itself (*voucher* procedure), and (3) we directly manipulated participants' expectations regarding subsequent trading by informing them that they would have an opportunity to trade at the end of the survey (*expectations* procedure). Third, to address possible experimenter demand effects and concerns that study participants would perceive trading as impolite or as causing inconvenience for surveyors (Mutunda 2006), we varied the wording when presenting the trading opportunity (*wording* procedure). Rather than offering the opportunity to trade, participants were asked to trade the item as an implicit favor to interviewers ("Would you be willing . . .").[13]

Our default item pair, implemented across all survey rounds and all procedures consisted of a package (250 g) of washing powder and a package (500 g) of table salt (Boom-salt). Both items are household staples with a local price of 3–3.5 kwacha (0.50–0.58 USD), which corresponded to one-fifth of a typical daily wage at the time of the experiment. We varied the item pairs to test robustness to alternative items. First, we provided cash (3.5 kwacha) as an alternative to Boom (Boom-cash). Second, we offered durable goods (a mug and a serving spoon: cup-spoon). We refer to these three item pairs as "standard-value" pairs. To assess the relationship between the value of the items and trading decisions, we also gave a subsample of participants the choice between a solar lamp and 80 kwacha in cash (solar-cash), which corresponds to over 20 times the value of the standard-value pairs. For all item-pair variants, we randomly selected households in each round for a *choice* condition, where they could simply pick their preferred item at the end of the interview. This allows us to measure item- and season-specific preferences for all item pairs. Table A.1 summarizes all randomly assigned experimental features, and the number of observations in each, by survey round.

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

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

2. Implementation and Randomization

To leverage variation in households' financial resources, we conducted experiments over the complete 2014–15 agricultural cycle. Specifically, we ran our exchange experiments after the 2014 harvest, when resources were relatively abundant, during the 2015 hungry season, when resources were scarce, and then again after the 2015 harvest. To distinguish effects driven by the external environment from learning and priming effects, we used a phase-in design that generated random variation in participant experience over the three survey rounds. Randomization of item pairs was done at the village level; randomization of specific experimental procedures was done at the household level.[14]

Experiment round 1 (2014 harvest season).—The first round took place after harvest in 2014 and ran from July through September. We randomly selected 105 villages and 1,513 households, covering approximately 58% of the total study population, to participate in the exchange experiments. In experiment round 1, we used both the standard and lottery assignments for assigning the item and varied the item pair (Boom-salt and cup-spoon). In addition, we assigned a small subsample ($n = 259$, household-level randomization) to the choice condition.

Experiment round 2 (2015 hungry season).—The second round of exchange experiments took place during the hungry season, from late January to March 2015, with a random subset of approximately eight households in each of the 175 study villages. 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% of the households sampled in the second round of experiments also participated in round 1. Loans were disbursed in randomly selected villages as part of the RCT described in Fink, Jack, and Masiye (2020) in January 2015, 2–8 weeks before the start of experiment round 2.

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

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

[15] We presented participants who either kept or traded for Boom a sequence of ascending hypothetical cash values, starting from a small increment above the value of cash in the item pair. Participants who kept or traded for cash were instead given a decreasing series of cash values. We assumed monotonic preferences and elicited only a unique switching point for each individual, which is a common procedure to avoid multiple switching in experiments

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

III. Empirical Strategy

In this section, we describe our approach to estimation and our identifying assumptions. Given the random assignment of items, testing for exchange asymmetries is relatively straightforward: for any distribution of preferences, a null hypothesis of no exchange asymmetry predicts that, in expectation, 50% of the sample will receive their less preferred item and thus trade the assigned item for their preferred one. For any item pair, we can estimate the probability of trading and test whether the estimated probability $\hat{p}$ is statistically different from 0.5:[16]

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

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

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

where R_t are indicators for survey rounds 2 (hungry season) and 3 (2015 harvest) that capture seasonal differences in trading probabilities relative to the 2014 harvest period; P_{it} and I_{ivt} are vectors of indicator variables for the procedural and item-pair variations, respectively; N_{it} indicates the number of prior rounds of experience with the exchange experiment, at the individual or household level; and X_i is a vector of

with choice lists (e.g., Dohmen et al. 2010). We use the same procedure in the solar-cash item pair implemented in round 3.

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

time-invariant household and participant baseline characteristics, including gender, age, household composition, wealth, and harvest value. We test the relationship between trading and (i) cross-sectional variation in scarcity, using quintiles of a baseline asset measure included in X_i, and (ii) seasonal differences in scarcity, using the survey round indicators, R_t. We cluster standard errors at the village level (v) throughout[17] and include individual fixed effects in some analysis.

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

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

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

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

While our main analysis of the relationship between scarcity and decision-making consists of a single hypothesis with three different proxies for

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

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

scarcity, section IV.B introduces results that depend on numerous different experimental treatments on the right-hand side (P_{it} and I_{ivt} in eq. [2] above). In supplemental analysis, we address potential concerns about multiple-hypothesis testing using the List, Shaikh, and Xu (2019) procedure, updated to accommodate controls and clustered standard errors (Steinmayr 2020).[19]

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

IV. Results: Scarcity and Exchange Asymmetries

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

A. *Average Trading Behavior*

Table 1 provides an overview of participant decisions by item pair. Columns 1 and 2 present the results from the choice condition, which provides a first indication of participants' relative preferences for the experimental items.[20] Participants had the most imbalanced preferences in the cup-spoon treatment, with three-quarters of participants preferring a cup over a spoon (despite similar market value). Preferences were more balanced, on average, for the other two standard-value item pairs. For each item pair, we also tabulate the number of participants starting and ending with each item (cols. 3–5). As first evidence of exchange asymmetries, we see that a majority of participants leave our experiment with the

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

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

TABLE 1
Descriptive Statistics by Item Pair

Pr(chosen)		Assigned	End with		Overall	
(1)	(2)	(3)	(4)	(5)	(6)	(7)
Boom-Salt ($N = 2{,}766$)						
			Boom	Salt		
					Pr(trade)	.34
Boom	.60	Boom	934 (.75)	315 (.25)	p-value (H_0 = .50)	.00
Salt	.40	Salt	514 (.43)	669 (.57)	p-value (H_0 = .50)	.00
Boom-Cash ($N = 1{,}962$)						
			Boom	Cash		
					Pr(trade)	.36
Boom	.66	Boom	701 (.73)	260 (.27)	p-value (H_0 = .50)	.00
Cash	.34	Cash	385 (.47)	431 (.53)	p-value (H_0 = .49)	.00
Cup-Spoon ($N = 714$)						
			Cup	Spoon		
					Pr(trade)	.30
Cup	.75	Cup	286 (.87)	42 (.13)	p-value (H_0 = .50)	.00
Spoon	.25	Spoon	135 (.50)	133 (.50)	p-value (H_0 = .47)	.00
Solar-Cash ($N = 400$)						
			Cash	Solar		
					Pr(trade)	.44
Cash	.45	Cash	97 (.60)	66 (.40)	p-value (H_0 = .50)	.08
Solar	.55	Solar	96 (.47)	108 (.53)	p-value (H_0 = .49)	.14

Note.—Summary of outcomes by item pair. The Pr(chosen) tabulation shows the likelihood that each item in the pair was selected in the choice condition (col. 2). "Assigned" and "End with" tabulate the frequency and probability by assigned item that participants started and ended with each item in the pair (cols. 3–5). The overall probability that a participant traded the item he or she was assigned is presented in col. 7. The p-values from tests of a null (H_0) of 50% trading and an adjusted null, accounting for assignment probabilities and preferences revealed in the choice condition, are also reported in col. 7 (with standard errors clustered at the village level).

item they are randomly assigned; even for the most inferior item (spoon), we see that around 50% of participants assigned a spoon choose to keep it, while only 25% selected it in the choice condition.

The table displays the probability that participants traded the item they were assigned, along with a t-test for the theoretical trading prediction in columns 6 and 7. Given that we randomize items in each item pair, we expect that half of the participants are assigned their less preferred item and thus should trade for their preferred item, resulting in an average trading rate of 50% in each item pair. In our finite sample, the actual share of participants randomly assigned the first item in the item pair was 0.51, 0.54, 0.55, and 0.44 in the Boom-salt, Boom-cash, cup-spoon, and solar-cash pairs, respectively. On the basis of preferences measured in the choice experiment, this implies that 0.50, 0.49, 0.47, and 0.49 should have traded in the Boom-salt, Boom-cash, cup-spoon, and solar-cash pairs, respectively. We therefore report the adjusted null and the associated p-value in column 7 of table 1.

In all item pairs, the observed trading probability was significantly below the null. The overall likelihood that a participant traded the item that they were assigned is 0.35, similar to the pooled results in other field studies (e.g., List 2003, 2004). We reject the null hypothesis of $p(\text{trade}) = 0.5$ as well as the overall adjusted null with p-values $< .0001$.

B. *Scarcity and Trading Behavior*

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

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 figure 1*A*, asset ownership is directly linked to consumption, with wealthier households eating significantly more meals during the hungry season. Next, we plot the baseline ownership of durable goods as a proxy for wealth against the average probability of trading, controlling for the item pair, experimental procedures, experience with the trading decision, and household and individual controls (following eq. [2]), in figure 2. The negative gradient indicates more trading by poorer households; that is, scarcity is associated with higher trading probabilities, though the confidence intervals are large (the p-value on the slope is .102). Since numerous other factors correlated with wealth may affect trading behavior, we turn to more plausibly exogenous sources of variation in participants' financial resources.

2. Seasonal Variation in Wealth and Income

As described above, pronounced seasonality in income, savings, and consumption is a salient feature of the study setting and thus provides a

[21] Measures of food consumption could be considered as "first-stage" or key mediators of scarcity in this setting. In practice, scarcity is likely to affect multiple domains of household well-being beyond nutritional intake, such as farm investment, school enrollment, and medical expenditures, which may all simultaneously affect patterns of decision-making.

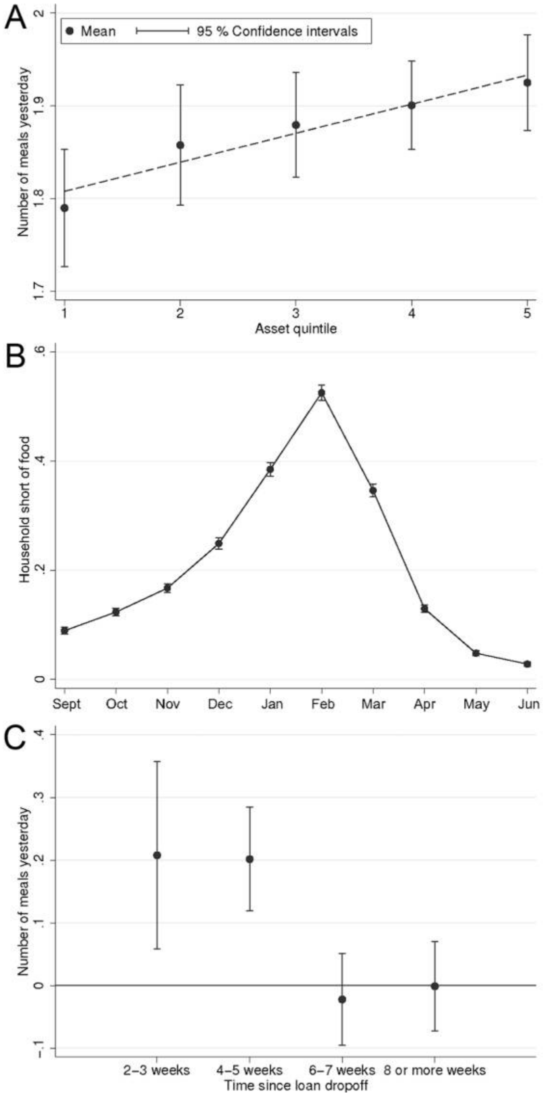

FIG. 1.—Consumption and food availability by source of variation in scarcity. *A* uses baseline variation in assets and hungry season consumption. *B* uses variation across months (seasons), where the first and third survey rounds took place between July and September while the second survey round took place from January to March. Data used in *B* are from Fink, Jack, and Masiye (2020). *C* uses time since loan drop-off, following figure 4, and hungry-season consumption. The 95% confidence intervals are based on standard errors clustered at the village level.

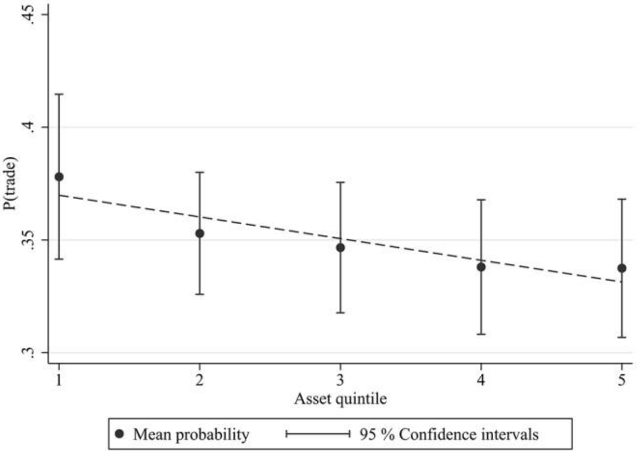

FIG. 2.—Probability of trading assigned item by baseline assets: quintile of the baseline household asset distribution (principle component analysis of durable-good ownership), conditional on round, procedure, experience, item pair, and household and individual characteristics. The 95% confidence intervals are based on standard errors clustered at the village level.

natural source of variation that we use to analyze how scarcity shapes trading asymmetries. The second round of our experiment coincided with the hungry season (January–March), while the other two rounds took place in times of relative abundance, immediately following harvest (July–September). In our sample, the mean cash savings during the hungry season is around 200 kwacha (median is 0), or 33 USD, while the mean cash savings at harvest is over 600 kwacha (median is 120). The share of households in our sample reporting food shortages increases from less than 10% around harvest time to over 60% in the hungry season (fig. 1*B*).[22] We exploit this variation in seasonal resource availability and compare trading decisions during the hungry season with decisions in the two harvest seasons, conditional on random variation in participant experience with the trading decision (following eq. [2]).

Figure 3 shows the estimated marginal effect of the season on trading decisions, based on the regressions shown in table 2. As shown in figure 3, around 30% of participants make trades in the 2014 harvest season. During the hungry season, the likelihood of trading increases by between 9 and 12 percentage points relative to the 2014 harvest (table 2). The point estimate is largest in columns 3 and 5, which include individual

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

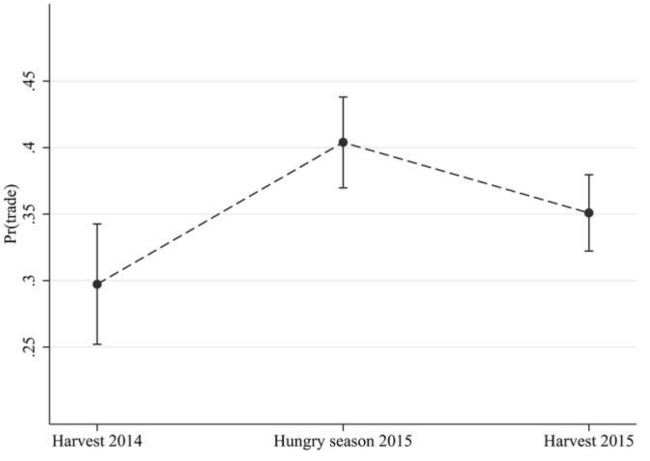

Fig. 3.—Probability of trading assigned item by agricultural season, conditional on individual experience with the trading decision, procedure, item pair, and household and individual characteristics. The 95% confidence intervals are based on standard errors clustered at the village level.

fixed effects and limit the sample to inexperienced participants, respectively.[23] Importantly, the effect is specific to the hungry season: the trading probability in the following harvest season is insignificantly different from that in the first harvest season (cols. 2–5) and significantly below the hungry-season coefficients in most columns, implying a higher probability of trading when resources are more scarce. At the risk of overinterpreting the data, we note that the slightly higher trading rates in the 2015 harvest season are consistent with a much weaker harvest in 2015 than in 2014 (see Fink, Jack, and Masiye 2020 for details).

Finally, it is important to highlight that the observed variation in trading behavior by season does not simply reflect seasonal differences in preferences. Data from the choice condition for Boom-salt, used in all three rounds, shows that preferences for the two items do not vary much by season. While Boom seems to be slightly more attractive in the hungry season (i.e., 65% of participants chose Boom over salt) than in the harvest

[23] Previous evidence suggests that trading experience attenuates or eliminates the endowment effect (see e.g., List 2003; Engelmann and Hollard 2010). Using random variation in rounds of experience with the trading decision, we can show that trading experience is unrelated to trading decisions (see table A.3; fig. A.1). Note, however, that the variation in our setting is different from that in the prior literature, which analyzes accumulated experience over a longer time period and with higher frequency. This suggests that the intensity of experience may be important for overcoming the endowment effect.

TABLE 2
PROBABILITY OF TRADING ASSIGNED ITEM, BY SEASON

	Pr(trade)				
	(1)	(2)	(3)	(4)	(5)
Hungry season	.089***	.107***	.121**	.112***	.123***
	(.022)	(.031)	(.056)	(.035)	(.031)
Harvest season 2015	.066***	.054	.053	.025	.062
	(.019)	(.033)	(.077)	(.042)	(.041)
Harvest 2014 mean Pr(trade)	.30	.30	.30	.29	.30
Hungry = endline (p-value)	.21	.04	.17	.02	.10
Controls	No	Yes	Yes	Yes	Yes
Fixed effects	None	None	Individual	None	None
Sample	Full	Full	Full	Boom-salt	No Experience
Observations	5,172	5,171	5,172	2,431	2,987

NOTE.—Linear regressions of an indicator for whether the subject traded the assigned item, by season. The omitted category is the 2014 harvest season. Columns that include controls condition analysis on round, procedure, experience, item pair, and household and individual characteristics. Individual fixed effects are included in col. 3. Column 4 restricts the analysis to the Boom-salt item pair only (used in all three rounds). Column 5 excludes households with past experience with the exchange experiment from each round. Standard errors (in parentheses) are clustered at the village level.
** Significant at the 5% level.
*** Significant at the 1% level.

season 2014 (60%) or 2015 (57%), these differences are far from statistical significance (Fisher's exact equal means test p-value > .3).[24]

3. Random Variation in Liquidity

While the seasonal variation in trading asymmetries is suggestive of a causal effect of scarcity on trading behavior, other factors may vary across seasons and influence trading decisions (see also our discussion of alternative explanations below). To address these endogeneity concerns, we leverage a third source of variation in liquidity, associated with access to hungry-season consumption loans. The larger RCT, in which we embedded the exchange experiments, relaxed liquidity constraints in 80 randomly selected villages during the hungry season by providing selected households with 200 kwacha (around 33 USD) in cash or maize. We compare trading probabilities for households with and without access to the loans. Loans were delivered in early to mid-January 2015, while the exchange experiments began in early February, about 2 weeks later. Figure 1*C* shows that the biggest effect on consumption occurred in the weeks following receipt of the loan. Figure 4 plots the effect of the loan on trading decisions, allowing the effect to vary with how recently it was received (in

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

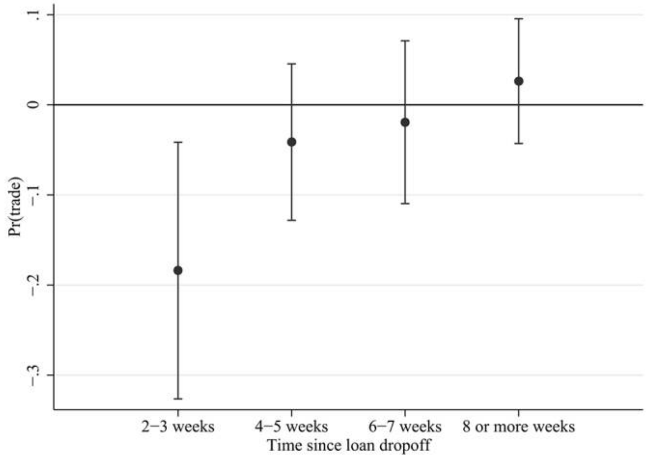

FIG. 4.—Relationship between weeks since loan receipt and trading probabilities: effect of loan timing on trading probabilities, where time since loan drop-off is measured in weeks. The omitted category is the control (no-loan) group. Results are conditional on week of survey and geographic fixed effects, procedure, experience, item pair, and household and individual characteristics. The 95% confidence intervals are based on standard errors clustered at the village level.

2-week bins), following equation (3). The pattern is striking, though standard errors are large: among households surveyed 2–3 weeks after receiving a loan, the likelihood that a participant trades her assigned item is over 18 percentage points lower than in control group households surveyed in the same week or located in the same geographic area. This effect wears off with time since loan delivery, paralleling the pattern of effects on consumption. Table 3 shows loan treatment effects, conditioned on different sets of control variables. The reduction in exchange asymmetries is short-lived but large in magnitude, with a greater likelihood of trading under conditions of scarcity.

4. Alternative Explanations

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

TABLE 3
PROBABILITY OF TRADING ASSIGNED ITEM, BY LOAD DELIVERY

	Pr(trade)			
	(1)	(2)	(3)	(4)
Loan	−.011	−.019	−.185**	
	(.029)	(.029)	(.090)	
Time since drop-off			.055*	
			(.029)	
Drop-off 2–3 weeks ago				−.184**
				(.087)
Drop-off 4–5 weeks ago				−.041
				(.053)
Drop-off 6–7 weeks ago				−.019
				(.055)
Drop-off 8 or more weeks ago				.026
				(.042)
No-loan mean Pr(trade)	.39	.39	.39	.39
Observations	1,224	1,224	1,224	1,224

NOTE.—Round 2 only: linear regressions of an indicator for whether the participant traded the assigned item on loan treatment variables. Loan treatment equals 1 if the household was in a loan treatment village. Column 1 includes survey week and geographic controls only; other columns also include controls for procedure, experience, item pair, and household and individual characteristics. Column 3 includes the time since loan drop-off in 2-week bins. Column 4 estimates coefficients on each of these bins. Standard errors (in parentheses) are clustered at the village level.
* Significant at the 10% level.
** Significant at the 5% level.

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

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

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

Other details of implementation may also contribute to the trading decisions, though none are associated with all three sources of variation in scarcity. For example, differences in survey implementation could contribute to the variation in decision-making, for example, if a shorter survey length in the hungry season reduces cognitive load and leads to better decisions. We use time stamps recorded by the survey software to test this directly and see no relationship between survey duration and trading behavior; as described above, experimental variation in the timing between the initial assignment and the trading decision also has no measurable effect. In addition, we examine whether differences in participant characteristics across survey rounds contribute to seasonal differences in trading probabilities, using the correlation between individual characteristics and trading (see table A.10). Only around 0.2 percentage points—out of the 9–12 percentage point difference between the 2014 harvest and the hungry season—can be explained by individual characteristics.

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

V. Mechanisms

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

A. *Tunneling and Cognitive Performance*

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

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

We begin by testing whether cognitive ability declines under conditions of scarcity. We leverage the same three scarcity measures used in section IV.B. Figure 5 summarizes the results (see also table A.12 for the underlying regression results). All outcomes are normalized to a mean of 0

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

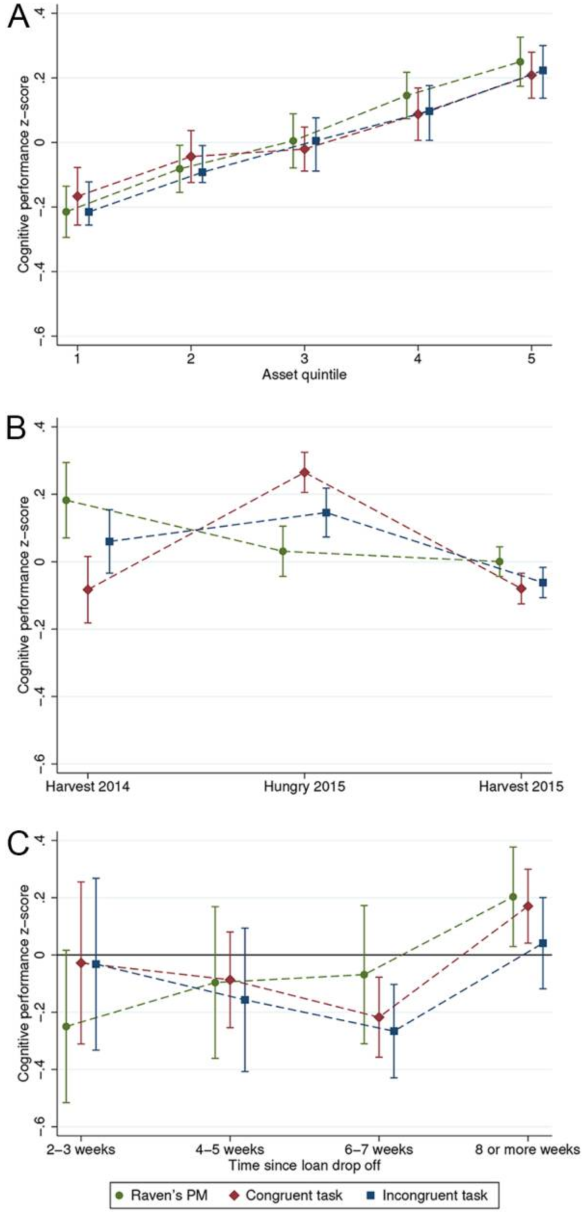

FIG. 5.—Relationship between scarcity and cognitive performance. Each panel shows how performance on cognitive tasks (measured as *z*-scores) varies with a different source of variation in scarcity. *A* uses baseline variation in assets. *B* uses variation across survey rounds (seasons). *C* uses time since loan drop-off, measured in 2-week bins. The omitted category is the control (no-loan) group, and results are conditioned on week of survey and geographic fixed effects. All analyses control for round, experience with the cognitive tests, and household and individual characteristics. The 95% confidence intervals are based on standard errors clustered at the village level.

and a standard deviation of 1, with a higher score corresponding to better performance. Figure 5*A* shows that wealthier households have significantly higher scores on the RPM test and on the two main Stroop measures (tasks 2 and 3), confirming established cross-sectional relationships between these measures and other proxies for human capital, such as educational attainment (Laajaj and Macours 2021). Figure 5*B* shows a modest improvement in performance on the Stroop tests during the hungry season but no such relationship in the RPM score. Figure 5*C* shows effects of loan drop-off, relative to the control group and conditional on survey-week and geographic fixed effects. Here we see even less consistent patterns: the RPM scores are lowest immediately following loan access and converge to zero consistently over time. The Stroop test scores, on the other hand, appear to decline with time since loan drop-off for up to 6–7 weeks, then improve significantly.[27] The finding that cognitive performance does not systematically decline with scarcity is in contrast to the findings in Mani et al. (2013), which uses variation before and after arbitrarily staggered harvest dates and relies on a subsample surveyed only after harvest to implement a separate test for learning.[28]

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

B. Value of Traded Items

As long as utility functions are concave in consumption, the same goods have higher utility value when resources are more scarce. Decisions involving items of higher value may be processed differently than similar

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

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

TABLE 4
COGNITIVE PERFORMANCE AND PROBABILITY OF TRADING

	Pr(trade)		
	RPM Test (1)	Stroop Task 2 (2)	Stroop Task 3 (3)
Cognitive measure	−.005	−.003	−.004
	(.008)	(.009)	(.009)
Mean Pr(trade)	.36	.36	.36
Observations	4,049	4,049	4,049

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

decisions involving lower-value items for several reasons, which we discuss below. To assess changes in trading behavior with higher-value items, holding scarcity constant, we introduced a high-value item pair (solar-cash) in the last round of our experiments. Specifically, we offered participants the choice between a solar lamp and an equivalent value in cash (80 kwacha, or 13 USD), 23 times the values in our standard item pairs. Table 1 shows that 44% of participants trade in this condition, which is only marginally below the adjusted null of a 49% trading probability ($p = .14$).

For additional insight into trading decisions with high-value items, we estimate a specification with the likelihood of ending up with the assigned item as the outcome relative to the probability of choosing that item in the choice condition. We present the results in table 5, where the constant (estimated without controls) represents the mean in the choice condition (cols. 1 and 2). Participants assigned a solar lamp were no more likely than participants in the choice condition to end with a solar lamp (col. 1), while participants assigned cash were 14 percentage points more likely to end with cash than in the choice condition (col. 2; $p = .12$). Column 3 shows our standard empirical specification (eq. [2]), restricted to round 3. Relative to the Boom-salt and Boom-cash item pairs, assignment to the solar-cash item pair increases the probability of trading by 8.4 percentage points. This manipulation holds scarcity constant and increases the value of the items, whereas our results in section IV.B hold the value of the items constant and increase scarcity. Both lead to a reduction in exchange asymmetries, suggesting that higher-value items—relative to available financial resources—result in decisions closer to the normative benchmark.

Why do exchange asymmetries diminish when decisions involve items of higher (utility) value? We discuss two plausible explanations for this

TABLE 5
PROBABILITY OF TRADING ASSIGNED ITEM, HIGH-VALUE TREATMENT

	Pr(End Item: Solar) (1)	Pr(End Item: Cash) (2)	Pr(trade) (3)
Assigned: solar	−.016 (.093)		
Assigned: cash		.141 (.087)	
Solar-cash			.083** (.038)
Constant	.545*** (.092)	.455*** (.092)	.351*** (.064)
Standard value mean Pr(trade)			.35
Controls	No	No	Yes
Observations	237	196	2,693

NOTE.—Round 3 only. Columns 1 and 2 estimate the effect of assignment on the probability of ending with the assigned item, restricted to the high-value treatment (solar-cash). To facilitate interpretation, the regressions do not include controls. The coefficients in cols. 1 and 2 are the additional probability of ending up with the assigned item, compared to the choice condition. The constant in the regression captures the probability of choosing the item in the choice condition. Column 3 estimates the effect of being assigned to the solar-cash item pair relative to the standard-value (Boom-salt and Boom-cash) item pairs. Column 3 controls for procedure, experience, and household and individual characteristics. Standard errors (in parentheses) are clustered at the village level.
** Significant at the 5% level.
*** Significant at the 1% level.

finding.[29] First, decisions involving higher-value items may receive greater attention, because of a basic decision heuristic, salience, or the cost of a decision error.[30] Perhaps most intuitively, decisions involving high-value items are likely accompanied by higher decision stakes, that is, larger utility losses associated with making the wrong decision. Even though higher-value item pairs do not necessarily imply higher stakes if market values are similar and trades are common, undoing a trading decision (acquiring or trading for the other item) may be more difficult for more expensive items, and subjective valuations may diverge more from the market value of an item as the value of that item increases. This latter reason can be seen in our data: in our sample, the difference between stated subjective valuations and the official market price is 70 times higher in the solar-cash item pair than in the Boom-cash item pair (see app. A.2).[31]

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

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

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

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

Second, higher-value decisions may affect preferences directly if, for example, individuals perceive outcomes relative to some reference point that itself depends on item value. We have taken exchange asymmetries as given to examine whether scarcity moves decisions closer to the normative benchmark, while questions about the exact formulation of reference points remain an active area of research (e.g., O'Donoghue and Sprenger 2018) that is beyond the scope of this paper. That said, some of our findings appear inconsistent with expectation-based reference points. Manipulating expectations directly by informing participants about the trading opportunity at the end of the survey or indirectly via variation in experience with the trading decision (expectations procedure or experience with the exchange experiment) has no impact on trading decisions (see table A.6; fig. A.1).

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

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

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

VI. Discussion

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

A. Welfare Implications

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

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

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

In the Boom-cash item pair, we first consider the 14% of nontraders with the smallest stated value differentials as a conservative estimate of the losses from forgone trades. On average, the implied loss per trading decision is 0.03 kwacha, or 0.42 kwacha in a population of 100 individuals.[34]

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

Alternatively, a slightly less restrictive calculation uses the average surplus generated through trading, which is the average of the stated value differential across traders. In this sample, the gain from trade equals 0.36 kwacha per trader. Multiplying this average gain from trade by the 14 percentage points of forgone trades in the Boom-cash item pair suggests a 5.04-kwacha loss in a population of 100 individuals.[35] We can also use these stated values in a rough calculation of the gains from trade associated with scarcity (based on hungry-season trading in table 2) in a population of 100 individuals. Using the valuations among nontraders yields gains between 0.09 and 0.37 kwacha, depending on the item pair and season, while using valuations among traders instead yields gains between 1.12 and 4.43 kwacha.

Relative to the local daily wage of around 15 kwacha, the average loss associated with a single trading decision is small; but recall that the median household has 0 kwacha in savings during the hungry season. In addition, households make many such transactions over the course of a year, and losses can add up, though to a potentially lesser degree when resources are scarce. This makes sense through a lens of rational inattention: holding attentional costs fixed, the welfare losses associated with a small change in consumption are larger when resources are more scarce. Of course, a rational-inattention mechanism would also mean that these welfare losses are zero, implying that this calibration offers an upper bound on the true losses. This calibration highlights that a high rate of behavioral anomalies need not imply large welfare losses if the anomaly is attenuated as the cost of mistakes increases.

B. *External Validity*

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

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

[36] In the terminology of List's (2020) SANS conditions (selection, attrition, naturalness, scaling), dimension 1 accounts for selection and attrition by considering how features of the study population affect external validity, and dimension 2 addresses naturalness by discussing the decision problem we study. We are unconcerned about scaling, given that we emphasize theory testing rather than policy interventions (i.e., our study focuses on the boundaries of prior findings regarding scarcity and behavior and explores underlying

Our study population was designed to be representative of rural households in Chipata District, Zambia, with an initial random selection of households and very limited attrition over time. Chipata District is similar to many rural populations in sub-Saharan Africa, where small-scale farming is the primary source of income and liquidity and consumption are highly seasonal. By combining this seasonal variation in scarcity with two very different sources of variation (baseline assets and randomized loan access) and documenting consistent relationships with exchange asymmetries across all three, we increase the likelihood that our results will hold across samples and contexts experiencing different dimensions of poverty. We also note that trading decisions vary little with observable household or individual characteristics (table A.10), suggesting that results may generalize to populations with different average characteristics. Relative to most of the related literature, the generalizability of our results is improved by (1) studying multiple sources of variation in scarcity, (2) focusing on a population that shares primary characteristics (agricultural employment and low incomes) with rural households across low- and middle-income countries, and (3) studying a sample that is representative of an entire district.

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

VII. Conclusion

In a sample of Zambian farmers, we show that the propensity to trade familiar household items is about 15 percentage points lower than predicted by standard theory, providing new evidence on the relevance and robustness of the endowment effect outside of the laboratory. More

mechanisms), such that general equilibrium effects or other challenges with scaling are unlikely to apply.

strikingly, we show that trading decisions become significantly more rational when financial resources are more scarce and when the value of the items involved is exogenously increased. While more rational decision-making under scarcity is consistent with the idea that scarcity increases focus on essential choices (tunneling, e.g., Mullainathan and Shafir 2013), our results are more consistent with greater attention to decisions involving higher stakes (Sims 2003; Maćkowiak, Matějka, and Wiederholt 2021). In our experiment, an individual who does not trade Boom for salt, even though her household needs salt, must instead spend her own money to buy salt (plus any necessary travel cost, etc.). If her household is credit or liquidity constrained, this will affect other consumption. Scarcity increases the marginal utility of this lost consumption and thus makes "mistakes" (forgone trading opportunities) more costly. Whether this scarcity arises from cross-sectional differences in wealth, seasonal variation in liquidity, or experimental variation in access to a loan, the result is the same: a higher utility cost from a forgone trading opportunity.

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

Despite the small short-term welfare loss associated with exchange asymmetries, reluctance to trade may have wide-ranging implications for markets in general and for development in particular. Reluctance to give up existing or endowed assets, goods, or acquired rights may at least partially explain (small) business owners or farmers forgoing profitable exchanges or investments (Kremer et al. 2013; Carney et al. 2022), individuals resisting policy changes (Alesina and Passarelli 2019), and low rates of new technology adoption (Liu 2013; Giné and Goldberg 2018). The results we present in this paper suggest that such reluctance is widespread and highest in times of relative abundance, a point in time when, for example, investments are most viable. Accordingly, opportunities to implement behavior change or to adopt new technologies may not only be population specific but may also be strongly influenced by temporal and seasonal variation in scarcity. Recognition of this variation may introduce new ways to harness prevalent exchange asymmetries or design policies that help households avoid related biases.

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