

Psychological Effects of Poverty on Time Preferences

Short title: Poverty and Time Preferences

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Abstract

We test whether an environment of poverty affects time preferences through purely psychological channels. We measured discount rates among farmers in Uganda who made decisions about when to enjoy entertainment instead of working. To circumvent the role of economic constraints, we experimentally induced thoughts about poverty-related problems, using priming techniques. We find that thinking about poverty increases the preference to consume entertainment early and to delay work. Using monitoring tools similar to eye tracking, a novel feature for this subject pool, we show that this effect is unlikely to be driven by less careful decision-making processes.

Keywords: Poverty, Scarcity, Time Preferences, Self-control, Inattention

JEL codes: C93, D91, O12

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Can poverty lead people to behave impatiently through channels other than standard budget-constraints or long-term processes of preference formation? In this paper, we focus on direct, immediate effects of poverty on time preferences. We study the behaviour of extremely poor farmers in rural Uganda who made choices in a controlled longitudinal experiment, in which we elicited time discounting of entertainment and exogenously activated thinking about poverty-related problems.

Development economists have long observed that low-income individuals often behave impatiently: many spend surprisingly large shares of their budgets on the consumption of temptation goods, including entertainment and alcohol, do not take advantage of high-return investment opportunities, and repeatedly take out high-interest loans (Banerjee and Duflo 2007; De Mel, McKenzie, and Woodruff 2008; Duflo, Kremer, and Robinson 2011). Using economic experiments, progress has been made in studying the influence of negative income shocks on time discounting, and most of the evidence suggests that having a lower income makes people behave more impatiently.¹ Documenting such effects of financial pressure on behaviour is important, because they may contribute to a self-reinforcing nature of poverty. It remains an open question whether the effects of low income on inter-temporal decision-making are due only to shifts in

¹ Several studies have shown that poor people tend to discount future income more than rich people (e.g., Lawrance 1991; Pender 1996). A number of experiments elicit time preferences by giving subjects a choice between sooner and later cash payments: in Vietnam, Tanaka, Camerer, and Nguyen (2010) use rainfall data as an instrumental variable for income, and find evidence suggesting that income has a causal effect on an experimentally measured discount rate. Using a similar approach in Ethiopia, Di Falco, Damon, and Kohlin (2011) show that severe draughts led to increases in the discount rate. In Southern Uganda, Bauer and Chytilová (2010) exploit variation in access to schools and disruption in the education system to document a causal effect of schooling on time discounting. Focusing on low income households in the US, Carvalho, Meier, and Wang (2015) show that before a pay day, participants are found to be more present-biased in intertemporal choices about monetary rewards. This effect does not extend to intertemporal choices about non-monetary real effort tasks, suggesting that liquidity constraints before the payday are the likely source of apparent present bias in choices for monetary rewards in this setting. Thus, since most previous studies measures inter-temporal choices for cash payments, there is little well-identified evidence that could not simply be explained by changes liquidity constraints, rather than changes in time preference. To address this issue, we use an experimental design that eliminates the role of liquidity and time constraints.

economic constraints, such as liquidity constraints, life expectancy and arbitrage opportunities, or whether they reflect changes in time preferences due to psychological constraints.

Recent work on “scarcity” or the “psychology of poverty” has documented that living in an environment of ubiquitous scarcity consumes cognitive resources and adversely affects emotions (Mani et al. 2013; Mullainathan and Shafir 2013; Haushofer and Fehr 2014; Haushofer and Shapiro 2016). However, little evidence exists of the impacts of these factors on economic behaviour, such as productivity, preferences and decision-making (Kremer, Rao, and Schilbach 2019). This paper contributes to this literature by focusing on psychological effects of poverty on time preferences. Motivated by research in behavioural economics, which highlights that delaying gratification and exercising self-control (Bernheim and Rangel 2004; Fudenberg and Levine 2006; Muraven and Baumeister 2000) can be seen as costly mental processes, we test the idea that the cognitive or emotional burdens associated with living in chronic poverty may tax self-control, and thus directly affect time preferences.

To shed light on this question, this paper offers several empirical innovations. First, we study time discounting of the consumption of a temptation good – watching entertaining videos instead of working. An advantage of implementing the entertainment-discounting task among this population is that it eliminates the role of liquidity and time constraints, and thus mitigates some of the key confounds involved in measuring time *preferences*. Second, to circumvent identification issues and income effects, we directly manipulate concerns about financial difficulties, using priming techniques. Finally, in addition to measuring intertemporal choices, we integrate new tools to monitor attention and information acquisition when participants make decisions. These measures of the *decision-making process* help us to separate the two psychological mechanisms

through which poverty may influence inter-temporal decision-making: higher time preference or reduced attention.

Our subjects are 289 adult subsistence farmers in Northern Uganda, who were recruited to perform a tedious manual task for a fixed work period on two dates, one week apart. The subjects were given a budget of “entertainment minutes” which they could use to watch entertaining videos on tablet computers instead of working on an unpleasant task. To elicit discount rates for the consumption of leisure, subjects were asked to allocate minutes of entertainment over the earlier and later work dates, for five different substitution rates. The subjects made the same entertainment allocation decisions twice: one week in advance and again just before the first work period in which early entertainment could be consumed.

We experimentally induced thoughts about poverty by presenting poverty-related situations to the subjects, as in Mani et al. (2013). Participants were asked how they would go about solving scenarios involving shocks, for example crop damage or a health shock. The scenarios were similar across conditions, except for the severity: half of the subjects deliberated about negative shocks with minor consequences, while the other half considered scenarios with severe consequences before making their inter-temporal choices.

Manipulating thoughts about poverty-related concerns resembles priming techniques, a well-established and frequently used method in psychology, and more recently also in economics and finance (Cohn and Maréchal 2016). It refers to mental activation of primed concepts and enables measurement of their pure psychological impact (via cognition and emotions) on behaviour in subsequent tasks. Such an approach has been employed to study the effects of a business cycle or recollections of violence on risk preferences (e.g., Callen et al. 2014; Cohn et al. 2015). Here, we use this technique to identify the psychological impact of cognitive load and stress

associated with pressing budgetary preoccupations on time discounting. This approach allows us to avoid the confounding influence of liquidity, wealth, access to financial markets, and health, as all of these variables remain unchanged across conditions.²

Our main finding is that thinking about poverty systematically increases preferences for consuming entertainment earlier and delaying work. This effect on discounting is economically meaningful: the poverty-related prime leads individuals to consume 1.7 more minutes of entertainment at an earlier date on a base of 21 minutes.³ The effect is robust to controlling for a long list of observable characteristics, holds for the whole range of prices of early vs. delayed entertainment, and is slightly stronger when allocation of leisure/labour has immediate consequences than it is when subjects make allocation decisions one week in advance.

Further, we show that the effect of the poverty prime on discounting is unlikely to be driven by reduced attention to the task. Using our detailed data on the decision-making process, we find no systematic or significant effects of the poverty prime on decision-making time, patterns of information acquisition, or signs of being distracted while making a decision. We arrive at similar conclusions when analyzing responsiveness to information about the parameters of the choice: the poverty-primed subjects are not less prone to respond to changes in substitution rate, for example.

² An alternative approach to overcoming the challenging issue of how to manipulate poverty-related concerns, without changing actual income and thus liquidity constraints, is in Haushofer, Schunk, and Fehr (2013). The authors randomly assign negative income shocks in a laboratory experiment among undergraduate students at the University of Zurich. An elegant feature of their study is that manipulation of an initial endowment was set up such that the absolute level of income was the same for the groups which experienced an income shock and those which did not. The authors find that the subjects who received a negative income shock exhibited more present-biased behavior than those who did not, suggesting that income shocks can have direct effects on time preferences.

³ The magnitude of the effect is comparable to the effect of earlier entertainment being more tempting, either because earlier entertainment could be enjoyed immediately instead of in one week time (1.2 minutes difference), or because earlier entertainment was made more salient by first presenting to the subjects the options which maximized earlier entertainment rather than the options which maximized entertainment later on (1.8 minutes difference).

Since we employ convex budget sets to elicit time preferences, a relatively complex protocol that was originally devised for sophisticated student subjects (Augenblick, Niederle, and Sprenger 2015; Andreoni and Sprenger 2012), it is important to gauge whether the farmers in our study correctly understood the task, given that, on average, they had five years of schooling. Although we find several intuitive patterns in their discounting, we also find that a non-negligible percentage of our subjects violate monotonicity: around 40% of subjects allocate in one of their decisions fewer minutes of entertainment to an earlier date at a lower substitution rate, as compared to the number of minutes allocated at a higher substitution rate. Such behaviour cannot be reconciled with transitive preferences and may indicate imperfect understanding. Importantly, the main effect of the poverty prime on entertainment discounting is robust to restricting the sample to literate individuals, individuals with full understanding, based on cross-check questions, and to the sub-sample of subjects who did not violate monotonicity. Based on these and other robustness tests, we believe that the observed effect of thinking about poverty on discounting is unlikely to have been driven by inattention or confusion.

Based on existing models of time preferences, we describe several mechanisms through which the poverty prime may affect time discounting. We conclude that our empirical findings most closely match the predictions of the costly self-control model of Fudenberg and Levine (2006) and Fudenberg and Levine (2012), in which individuals are in constant conflict between a short-run self that seeks immediate gratification and a forward-looking long-run self, and in which the parameter capturing costs of self-control is affected by environmental factors, such as anxiety and cognitive load associated with poverty, or the proximity of temptation. An alternative mechanism, denoted in psychology as the myopic-misery hypothesis (Lerner, Li, and Weber 2013; Lerner et al. 2004), is that a negative affect enters individual utility function, and early

consumption of entertainment may compensate for this utility loss. While this mechanism can explain the effect of a poverty prime on discounting, it cannot be the sole explanation since it struggles to explain the other patterns we find.

In addition to manipulating thoughts about poverty, we also exogenously manipulated the number of calories consumed by subjects, prior to making decisions, to test whether calories affect decision-making. Following Gailliot et al. (2007), Wang and Dvorak (2010), Kuhn, Kuhn, and Villeval (2017), we offered participants a drink sweetened either with sugar, or with a sugar substitute containing zero calories (a placebo condition). We do not find evidence that this treatment affected decision-making, in contrast to early work in psychology (Gailliot et al. 2007; Wang and Dvorak 2010), but in line with more recent economic experiments (Kuhn, Kuhn, and Villeval 2017). This does not imply, however, that better nutrition does not affect the decision-making processes of the poor in general. Our results instead indicate that the one-time provision of a relatively small amount of calories, as in our experiment, may not be enough to affect decision-making. For example, evidence from Schofield (2014) documents improvements in the cognitive capacity of rickshaw drivers in India who received daily snacks for five weeks. We provide more details about manipulating calories in our experiment in Appendix E.

Finally, in light of the large literature documenting present-oriented behaviour of the poor, it is noteworthy that, on average, subjects in our experiment do not act particularly impatiently. Although the level of discounting may in part be affected by the discreteness of the choice set, subjects allocate 22.2 out of a possible 45 minutes of entertainment to the earlier date. This is intriguing, given that our experiment employs several design features to mitigate the usual confounds involved in measuring time preferences in choice experiments, specifically liquidity constraints. Our main (causal) finding suggests that the environment of poverty makes the poor

more impatient due to psychological constraints. Yet the relatively low levels of observed discounting also indicate that standard economic constraints play an important role in explaining why previous studies, based on choices between time dated money that cannot isolate the role of liquidity constraints, have often identified very large discount rates among the poor.

Our paper is related to several streams of literature. First, the paper contributes to emerging empirical literature testing the psychological effects of poverty on decision-making. Negative income shocks or concerns about income shocks have been shown to reduce cognitive function (Mani et al. 2013; Lichand and Mani 2019). Kaur et al. (2019) show that scarcity of one's own income (but not priming) reduces productivity. Our paper documents impacts of financial anxiety on economic behaviour in a new domain – whether to delay work and enjoy leisure early - and thus may help to explain why sometimes the poor seem to place surprisingly low priority on engaging in income-generating activities, and high weight on consuming temptation goods (Schilbach 2019; Banerjee and Duflo 2007). Interestingly, a recent paper by Fehr, Fink, and Kelsey (2019) shows that greater scarcity is associated with a lower endowment effect, suggesting that scarcity may reduce decision biases in some domains. Besides shortage of income, other experiments have estimated the psychological effects on economic behaviour of other conditions associated with poverty, such as noise, alcohol, sleep deprivation and lack of food (Dean 2020; Schilbach 2019; Bessone et al. 2019; Schofield 2014). Kremer, Rao, and Schilbach (2019) and Schilbach, Schofield, and Mullainathan (2016) provide comprehensive reviews of this literature.

Second, this paper illustrates the usefulness of using priming techniques to study economic behaviour. Besides the aforementioned work focusing on poverty (Mani et al. 2013; Lichand and Mani 2019; Kaur et al. 2019), other examples include studies on the effects of ethnic, criminal and banker identity on preferences (Benjamin, Choi, and Strickland 2010; Cohn, Fehr, and Maréchal

2014; Cohn, Maréchal, and Noll 2010). Cohn and Maréchal (2016) provide a recent review of the economic literature on the topic, including a discussion of the methodological trade-offs involved in using priming techniques, and conclude that its main limitations, in particular the difficulty of pinning down which mental concept has been activated, is shared with other empirical approaches. Also note that this technique identifies impacts of greater *intensity* of poverty-related thoughts, rather than the overall effects. Thus, to the extent that people in the control condition may also have poverty-related concerns very much at top of mind, this technique may underestimate the actual effects of poverty (Kaur et al. 2019).

Third, the paper speaks to the literature on measuring time preferences. Most previous studies estimate time preferences using intertemporal choices over money -- both in developed (Sutter et al. 2013; Meier and Sprenger 2015; Andreoni and Sprenger 2012) and in developing country settings (Tanaka, Camerer, and Nguyen 2010; Bauer, Chytilova, and Morduch 2012; Giné et al. 2018). We build on recent experiments (Augenblick and Rabin 2018; Augenblick, Niederle, and Sprenger 2015), implemented among US undergraduates, which measure discounting based on choices over time-dated effort. This helps us to avoid several potential confounds associated with using monetary rewards when measuring time preferences and limited self-control, especially the possibility to arbitrage outside of the experiment and the role of liquidity constraints. Our paper focuses on choices of when to enjoy entertainment (i.e., a tempting good), and one of its contributions is a demonstration that elicitation of discounting using choices over time-dated consumption/effort is feasible to implement even among the very poor in a developing country setting. This relates our paper to Andreoni et al. (2016) and Abebe, Caria, and Ortiz-Ospina (2019) who elicit effort discounting among health care workers in Pakistan and applicants for clerical jobs in Ethiopia, respectively.

Fourth, we contribute to literature analysing the decision making process by adapting monitoring tools that have been developed for laboratory experiments, in order to be feasible to implement in a field environment with an important population for which computerized experiments are not suitable. Our effort to gather data on decision-making process is motivated by recent papers, which have cautioned against automatically interpreting heterogeneity in risky or impatient *behaviour* in experiments as reflecting differences in the underlying *preferences*, since choices may also capture differences in effort and attention, and consequently the quality of the decision-making process more broadly. This empirical challenge has been debated by researchers who study the effects of cognitive ability on risk behaviour (Andersson et al. 2018; Dohmen et al. 2018), but it applies to any study that aims to estimate the causal effect of environmental factors or individual characteristics on preferences. Our approach to addressing this issue is inspired by techniques commonly used in computerized experiments, in which researchers complement choice data with data on the decision-making process. For example, several studies suggest that longer response time is a good proxy for greater effort and attention to the task.⁴ Inspired by mouse-tracking techniques, we use a novel video-recording set-up to obtain detailed measures of decision time, information acquisition and other aspects of the decision-making process in a field setting.

The rest of the paper is organized as follows. Section I describes the sample and experimental design. Section II presents the main results and a set of robustness tests. Section III links our findings with theory and discusses which models of discounting are best suited to explain the patterns observed. Section IV concludes.

⁴ Wilcox (1993) finds that subjects exhibit longer response time in a lottery choice task when monetary incentives are higher and the task is complex. Similarly, Chabris et al. (2009) show that the closer the expected utility of the competing options is, and thus the choice is more difficult, the longer the response time is. Recalde, Riedl, and Vesterlund (2018) find that shorter decision-making time is correlated with greater likelihood of making errors and Enke and Graeber (2019) show that people who make faster decisions later report greater uncertainty about whether they made the right decisions in experiments.

I. Experimental design

We present the experimental design in five sub-sections. First, we summarize the sample selection. Second, we describe the forms of work and entertainment to be allocated over time. Third, we describe the tools used to monitor attention allocation during the decision-making process. Then, we describe how we manipulated poverty-related concerns. Last, we provide further details about experimental procedures and the timeline.

A. Sample

The participants are from twelve villages in the Gulu district in Northern Uganda. The data was collected in September-October 2014. In each village, households were randomly selected from a village roster. One member of each household completed a short survey, identifying the age, gender and occupation of all household members. We then randomly selected one individual from each household who was between 20 and 55 years of age and whose primary occupation was farming, stratifying by gender. Thus, the sample is representative of the population of farmers in the villages studied. This setting allows us to study the behaviour of an extremely poor population, for which, a priori, behavioural responses to poverty should be the most relevant.

Table 2 reports the summary statistics. Overall, we have data for 289 subjects, of whom 51 percent are female. Subjects are 35 years old on average. The farmers in our sample are poor, with median reported cash income of just UGX 56,000 (\$21.28) over the previous month. The majority (63.3 percent) live in homes with mud walls. Subjects reported that they usually eat just under two meals per day, on average, and only 13.7 percent reported eating meat more than once a month.

Health shocks are common in our sample: 45.3 percent of respondents reported that they were unable to work or perform other duties over the previous month at least once due to illness. Subjects have 5.16 years of schooling on average, and just over half said they were literate enough to write a letter. The area that we study was exposed to sporadic conflict with the Lord's Resistance Army (LRA) from roughly 1994 to 2005. We asked subjects a set of questions on their conflict experiences, including violence witnessed, received and whether family members had been killed during the conflict (see Appendix G).

B. Elicitation of Time Discounting

When eliciting time discounting about entertainment consumption, we implement a longitudinal experimental design conducted over three weeks. The experiment focuses on the intertemporal allocation of entertainment and work between Week 2 and Week 3. Subjects were informed that in Weeks 2 and 3 there would be an “activity hour,” during which they would have to be present and to work. The length of the activity hour was fixed at sixty minutes in both weeks. The work consisted of a tedious form of labour: sorting yellow and red dried beans by colour. Subjects were informed that a certain fraction of the activity hour in each week would be spent watching videos on tablet computers, instead of working. They could choose from a number of short videos, including traditional dancing, modern music videos, soccer highlight reels and short comedic sketches, in order to satisfy a variety of tastes. None of the available videos involved long

narratives, so that it was not advantageous to concentrate the entertainment time into one activity hour.⁵

The amount of beans that subjects were responsible for sorting was proportional to the amount of time devoted to work in a given activity hour. Therefore, by increasing the proportion of entertainment allotted to a given week, subjects simultaneously decreased the quantity of beans that they were responsible for sorting. This was demonstrated visually: for each 5-minute work interval, they were responsible for sorting an additional cup of beans. If subjects finished their assigned work before the time ran out, they were asked to wait quietly until the work time was over.

In order to provide subjects with experience of how enjoyable the entertainment was and how effortful the work was, they were required to watch videos for five minutes and to work for five minutes, prior to making allocation decisions in both Weeks 1 and 2, and prior to the activity hour in Week 3. Subjects were informed in Week 1 that they would also take part in the same minimum entertainment and minimum work in Weeks 2 and 3.⁶

The subjects were endowed with a fixed budget of entertainment minutes. Using a discrete convex decision environment, subjects allocated minutes of entertainment over the two activity hours. They made decisions on how much of their entertainment endowment to consume at the earlier date (Week 2) or later (Week 3). Sacrificing one minute of entertainment early corresponded to consuming p more minutes of entertainment a week later, where p is an intertemporal substitution rate. Subjects made allocations for five substitution rates: 0.5, 0.75, 1,

⁵ Television ownership is rare and watching videos can be considered a luxury for this population: around 64 percent of subjects report watching TV or videos “never” or “rarely,” while only 7 percent report watching daily. All but two subjects reported that they enjoyed watching the videos.

⁶ This also eliminates the role of discontinuity in preferences for work/entertainment.

1.25, and 1.5. The subjects knew all the substitution rates before making any choices. For each substitution rate, subjects selected between six levels of consumption of entertainment early, with a fixed difference of eight minutes (45, 37, 29, 21, 13, and 5). The amount of entertainment which could be allocated to the earlier date was capped at 45 minutes and implied no entertainment at the later date. Table A1 in the Appendix presents all the choice sets. We made extensive use of graphic aids to help subjects visualize comparisons between the various substitution rates (see Appendix Figure F1).

In order to identify whether our poverty-related manipulations made the subjects more dynamically inconsistent, subjects allocated entertainment minutes between Weeks 2 and 3 twice, once in Week 1 and again in Week 2 (see Table 1). In Week 1, the set of five choices concerns consumption in two future dates, while allocations in Week 2 involve consumption at present and in the future. Before making decisions in Week 1, the decisions to be made in Week 2 were explained. In total, participants made ten allocation decisions (five in Week 1 and five in Week 2). Subjects were aware that one randomly selected decision would be implemented, ensuring the decisions were incentive compatible.

Formally, the present value budget constraint can be represented as:

$$e_t + \frac{e_{t+1}}{p} = m$$

where e_t is the number of entertainment minutes consumed at the earlier date, i.e. either now or one week from now, e_{t+1} is the amount of entertainment minutes consumed at the later date, i.e. either in one week ($t = 0$) or in two weeks ($t = 1$), p represents the substitution rate, and m is the total budget allocation of entertainment minutes across the two weeks, i.e. 45 minutes in each decision.

The time discounting task was designed so that the standard economic constraints, money and time, should not affect entertainment allocations. Liquidity constraints should not affect subjects' intertemporal choices, since the allocation decisions concerned a good that could not be traded outside of the laboratory (time-dated entertainment). Also, the monetary compensation for participation was unrelated to experimental choices: subjects received compensation of 15,000 UGX (\approx USD 5.70), if they successfully completed all elements of the experiment, and a show up fee (UGX 2000) for participation in each experimental session. Out-of-lab time constraints also should not have affected allocations. Since the length of the activity hour was fixed, the allocations affected share of work vs. entertainment, but not the total time spent at an experimental session.

We elicit time preferences using choices over time-dated consumption, rather than choices over time-dated money, since this approach helps to overcome several potential confounds (Augenblick, Niederle, and Sprenger 2015). First, subjects in our experiment could consume the entertainment only during the experimental sessions and thus there was no scope for out-of-lab smoothing opportunities, which could confound estimation of individual time preferences.⁷ Second, the design helps to address a concern that subjects' choices may reflect higher transaction costs of redeeming rewards later, or a low level of trust in the experimenters, rather than their time preferences (Giné et al. 2018; Andreoni and Sprenger 2012). This concern is particularly relevant for a field setting in a developing country, in which extra-lab payment infrastructure (such as automatic bank transfers) is limited and cannot be readily used to reduce transaction cost differences. In this experiment the incentives to come in later weeks were high, since the main

⁷ Arbitrage arguments cast skepticism over time discounting experiments with money, since, in theory, choices over monetary payments should only reveal the subject's out-of-lab borrowing and lending opportunities (Dean and Sautmann 2014; Cubitt and Read 2007; Pender 1996), especially in settings in which financial markets are thick and transaction costs are low.

reward for participation (completion bonus) was disbursed in Week 3. Indeed, the attrition rate was very low (1.4 percent), as we describe in greater detail below.

C. Monitoring the Decision-Making Process

We developed a decision-making environment which is simple to understand and allows us to monitor the decision-making process. For each of the five substitution rates, subjects made choices by flipping through six pages in a small booklet. Each page graphically and numerically displayed one option: the number of minutes of entertainment allocated to earlier and later dates. After being given all instructions and answering cross-check questions on understanding, subjects could, on their own, flip through the booklets and inspect different options, with no time restriction. Five booklets, one for each substitution rate, were mounted on top of one another on a single board (see Figure 1 for a picture of the allocation environment). This allowed subjects to visually compare their choices across all five rates.⁸ When subjects arrived at a final allocation decision, they were asked to leave the booklets open on the page with their desired allocation and to inform the experimenter, who recorded their choices.

We randomly varied which option was presented to subjects first. In the IMPATIENT default condition, when subjects approached the board, all five booklets were open to the page with the maximum number of minutes of entertainment early. In the PATIENT default condition, booklets were open to the page with maximum entertainment at a later date. These conditions were randomly allocated using a between-subjects design, and each subject faced the same default for

⁸ Our efforts to monitor the decision-making process imply that, unlike most studies on time discounting which explore choices made by subjects continuously along a convex budget set, we use a discrete decision environment with six possible levels of consumption for each substitution rate.

all ten choices (i.e. five in Week 1 and five in Week 2). We refer to this manipulation as changing the default allocation, since if subjects abstained from making an active choice and did not flip the pages of a given booklet, the open option is treated as a decision. Note that because there are virtually no transaction costs involved in making an active decision, this manipulation is very subtle compared to other types of default allocation that have been studied in other contexts. We believe it affects decisions primarily by making the open option more salient.

Gathering data about a decision-making process by using mouse-tracking or eye-tracking tools is common in computerized laboratory experiments. Our aim was to develop a portable experimental set-up that is feasible to implement in the field with a subject pool that is not computer literate. When flipping through the options in the booklets, subjects wore empty eye glass frames with a video camera attached.⁹ Since the cameras recorded the subjects' actions on the board and only one option for a given rate can be opened at one point in time, this set up provides us with direct measures of the decision-making process. The data allow us to test whether prime affects inter-temporal decision-making by influencing the amount of attention (measured by the total decision-making time, the number of options inspected, and the likelihood of not making an active choice by sticking to the default option).

D. Manipulating Poverty-Related Concerns

In order to manipulate poverty-related concerns, we adapted the method developed by Mani et al. (2013), who used the technique to prime poor individuals in the US with hypothetical income

⁹ Subjects were informed that their decisions would be recorded, but that the camera would not record their faces. In order to minimize the distraction caused by wearing the cameras, subjects were fitted with the apparatus several minutes beforehand, so that they were used to wearing it by the time they made their decisions.

shocks. Immediately before making allocation decisions, subjects were presented with two hypothetical scenarios, which described negative income shocks arising from crop damage, damage to home, or a health shock. These scenarios were designed to activate actual financial concerns and were developed based on focus group discussions on common sources of income shocks in the area we study.

We experimentally varied the severity of the hypothetical poverty-related problems. In the HARD condition, scenarios involve problems with severe consequences, while in the EASY condition, subjects were presented similar scenarios, but with less severe consequences. Across conditions, pairs of scenarios described the same type of income shock and had similar wording. For example, the wording of a scenario on crop damage in the HARD condition is: “Imagine that hail stones destroy your entire crop and the whole harvest is lost. How do you deal with this situation? Does this require you to liquidate your savings? Do you need to borrow? Do you need to eat less?”, while the corresponding scenario in the EASY condition is: “Imagine that hail stones fall on your crops after the harvest is nearly finished, destroying a small part of the crop that is enough to feed your family for one day.” Individuals were assigned to the same treatment condition in each week of the study.¹⁰ The exact wording of the complete set of implemented scenarios appears in Appendix F. The order in which scenarios were presented was randomized.

The subjects were also asked to rate, on a four-point scale, how difficult it would be to face the given situation and how anxious they would be if facing the given situation. As expected, the average difficulty and self-reported anxiety indeed substantially increases in the HARD

¹⁰ This manipulation and the manipulation of calories, described in Appendix E, were implemented using a 2x2 factorial design.

conditions, as compared to EASY. The likelihood of rating the problem as “very difficult” increases from 25 percent in EASY to 61 percent in HARD and the share of people who would feel very anxious increases from 22 percent in EASY to 47 percent in HARD (Appendix Tables A2 and A3). The effects are remarkably stable across different types of scenarios. Further, since subjects were exposed to HARD or EASY conditions in both weeks, we test whether they responded to primes differently in Week 1 and Week 2. A legitimate concern is that the effects might diminish if subjects are exposed to a similar poverty prime a second time. We find virtually the same magnitude of the effect of HARD vs. EASY conditions on perceived difficulty in both weeks (36 percentage points in Week 1 and 32 percentage points in Week 2). The effects on average anxiety is positive in both weeks, but the magnitude is somewhat smaller in Week 2 (21 percentage points), as compared to Week 1 (32 percentage points).

E. Further Details about Procedures

Baseline characteristics do not systematically differ across experimental conditions, suggesting the randomization was successful (see Appendix Table A4). We took several steps to minimize attrition during the course of the experiment. First, subjects had to take part in all three experimental sessions in order to receive the completion bonus of UGX 15,000. On top of this, they received UGX 2,000 as a show-up fee each week. This is a substantial amount of money for the population we study – median cash income for the sample is 1000 UGX per day. Second, the experiments were implemented in local schools (or community meeting places), in the villages where subjects live. Third, subjects always participated on the same day of the week at the same time throughout the three-week long experiment. Thus, subjects whose session in Week 1 took place on Tuesday, for example, allocated entertainment to be consumed on two future Tuesdays.

Sessions were conducted either in the morning (8 AM) or early afternoon (1 PM), in groups of around ten subjects from the same village, and there was one morning and one afternoon session per week in each village. Local leaders were hired to visit and remind participants before each experimental session. Consequently, attrition was low -- only four subjects who participated in Week 1 failed to show up in Week 2.

Due to technical issues, we failed to gather the decision-making process video data for 39 individuals (13 percent of the full sample). The main reason is that the video cameras participants wore when making their choices were not working properly or were not correctly aimed at the decision-making board. Table A4 shows that the technical issues were evenly distributed across conditions and the main results on choice patterns are robust to excluding subjects for whom the decision-making data are missing (Panel A, Column 2, Table A5).

Table 1 summarizes the timeline of the longitudinal experiment. In Week 1, experimenters explained the timeline of the experiment and how subjects would be compensated for their participation. Subjects were also informed that they were free to leave anytime during the experiment if they did not wish to participate. Then, subjects experienced five minutes of work and five minutes of watching videos. They were informed that the sessions in Week 2 and Week 3 would begin with a similar warm-up.

Next, subjects received instructions on the length and timing of the activity hours, about different substitution rates and how to allocate entertainment minutes between an earlier and a later activity hour. After the group instructions, subjects were taken one by one to an experimenter, and were given further examples and clarifications, before they were asked a set of nine comprehension questions. (See Appendix H for the instructions).

After cross-check questions, subjects were served tea containing either sugar or artificial sweetener. While subjects consumed the tea, the poverty-related scenarios in either the HARD or EASY conditions were presented. Then, the experimenter left and asked subjects to make intertemporal decisions, using the board with booklets. Subjects were asked to wear eyeglass frames with affixed cameras. It was explained that this would help to reliably record their choices. After making their experimental choices, subjects completed a short questionnaire about food consumption earlier in the day and basic demographic information (See Appendix I for questionnaires).

The procedure in Week 2 was very similar to Week 1, up to the point that subjects completed the inter-temporal choices and answered survey questions. After this, experimenters drew a number from a bag, for each subject, to determine which of the 10 decisions would be implemented. Subjects then completed the activity hour, divided between work and entertainment according to the selected decision. In Week 3, there were no decisions. Subjects completed the activity hour, then after answering the set of questions in the HARD or EASY poverty prime, performed a Raven's progressive matrices task to measure cognitive function. They were asked additional questions about their personal characteristics, financial behaviour and conflict history, before they were given the show up fee and completion bonus.

II. Results

Section II.A presents the basic patterns of choices in the time discounting task and describes proxies of understanding of the task. Section II.B estimates the effect of the poverty prime on inter-temporal choices. Section II.C probes whether the main effects can be explained by differences in attention or confusion about the task.

A. Basic patterns: Measures of discounting and the level of understanding of the task

In total, each subject made ten inter-temporal decisions: one for each of the five substitution rates in Week 1 and again in Week 2. The attrition rate is very low -- only four subjects who participated in Week 1 did not show up in Week 2.¹¹ Thus, in total we analyse 2,870 decisions for 289 subjects. We find that, on average, subjects allocate 22.2 out of a possible 45 minutes of entertainment to the earlier date. Thus, subjects behaved relatively patiently and the estimated discount rate is comparable to the results of Augenblick, Niederle, and Sprenger (2015) who used a related task among undergraduate students in US.¹²

There is also a great deal of variation in subjects' choices (Figure A1). The standard deviation for each substitution rate is between 10.52 and 11.72. The subjects do not seem to choose any simple focal point from the choice set, like the mid-point or either of the extremes. On average, the frequency of the four interior choices (13, 21, 29 and 37 minutes of entertainment in the earlier session) is roughly equal, with each option accounting for between 18 and 21 percent of choices. At the same time, the least patient option (all 45 minutes of entertainment allocated to the earlier date) accounts for less than 5 percent of all choices, and the most patient option (5 minutes of

¹¹ The results are robust to excluding these subjects from the analysis.

¹² Appendix Table A6 reports structural estimates of parameters from a beta-delta quasi-hyperbolic discounting model, while Appendix C discusses how the parameters are estimated. The estimated parameters in Augenblick, Niederle, and Sprenger (2015) are $\delta=1.00$ and $\beta=0.91$. In our setting, the estimated parameters are $\delta=1.11$ and $\beta=0.91$. At face value, $\delta>1$ is surprising. Note, however, that interpretation of the estimated δ is difficult, since the estimate is likely affected by the discrete choice space that we implemented. For each substitution rate, subjects had to choose between six options how to allocate entertainment time. By design, sacrificing half of the entertainment early (which implies an equal split for a price equal to one) was not possible. The closest option to an equal split (22.5) was 21 and the neighbouring option was 29 minutes of entertainment early. Thus, such choice space could lead to a greater estimated δ , because subjects with $\delta=1$ could not choose a preferred allocation (equal split), but only a slightly more patient option (implying $\delta>1$). Note that this is less of an issue for estimating the β parameter, since the choice space is constant across weeks. Section III considers a dual-self model as the model of choice, but the structural estimates presented here allow comparisons to earlier literature.

entertainment in the earlier session) accounts for 15 percent of all choices. At the individual level, only around 10 and 5 percent of subjects made choices exclusively at either of the two extremes for all substitution rates in Weeks 1 and 2, respectively.

Next, we observe several intuitive patterns (Table 3). First, as the cost of earlier entertainment increases, entertainment allocated to the earlier week decreases monotonically for choices in both Week 1 and Week 2 (Figure 2). Second, in line with previous work (e.g., Dohmen et al. 2010), we find that cognitive ability (as measured by performance on Raven matrices) predicts patience (Table A7). Third, we also find evidence of dynamic inconsistency: in Week 1, subjects allocated 1.2 minutes less entertainment for the earlier session than when facing the same decision in Week 2 ($p=0.08$).

We use several proxies to gauge the subject's level of understanding. First, we use answers to a set of nine comprehension questions. This measure indicates that the overall level of understanding was relatively high. All comprehension questions were answered correctly by 77 percent of subjects in Week 1 and by 84 percent of subjects in Week 2.

As a second proxy of understanding, we use violations of monotonicity, i.e. we measure the proportion of subjects who responded to an increase in the relative price of entertainment in Week 1 by choosing to allocate more entertainment minutes to this date. Specifically, we consider a pair of choices with adjacent substitution rates as violating monotonicity if fewer minutes of entertainment are allocated to the earlier date at a lower substitution rate, compared to the number of minutes allocated at a higher substitution rate. Given the discrete nature of the choices in our experiment, we can only identify violations of monotonicity of a relatively large magnitude (at least eight minutes). Note that a violation of monotonicity does not necessarily imply that a subject made a mistake – it is a mistake in the sense that it is not reconcilable with transitivity.

We find that a non-negligible percentage of subjects violated monotonicity. In a given week, 56 percent of subjects made no inconsistent choices, 31 percent made one inconsistent choice, and around 13 percent made two, three or four inconsistent choices. Around 40 percent of subjects made no violations of monotonicity in either week. Taking into account all pairs of choices from all subjects in both weeks (2,312 pairs in total) we find that 15 percent are inconsistent.¹³ For comparison, we run 5,000 simulations with choices generated stochastically for each substitution rate from a uniform distribution. This yields 42 percent of inconsistent decision pairs, which is substantially higher than in our data. Further, we use an alternative measure of the extent of violations of monotonicity, by defining the minimum number of “page flips” in all booklets, across the five rows, required to make the allocation consistent with monotonicity. In our data, 11.1 percent of decisions would require three or more “page flips”, compared to 66 percent of the randomly generated decisions (Figure A2).

Thus, given that these patterns suggest that understanding was imperfect for non-negligible fraction of subjects, in the analysis of treatment effects, we pay particular attention to addressing the question whether the main effects can be explained by imperfect understanding or confusion (Section II.C).

¹³ For comparison, the share of inconsistencies in previous studies that use convex budgets to study time discounting was 19 percent among Malawian farmers (Giné et al. 2018), 18 percent among German high school students (Lührmann, Serra-Garcia, and Winter 2018), and 17 percent for a representative sample of Americans (Carvalho, Meier, and Wang 2016). However, these numbers are not directly comparable to our result (15 percent) since we can only identify violations of monotonicity of a relatively large magnitude, due to the discrete nature of the choices in our experiment. Subjects in our experiments are more likely to violate monotonicity than undergraduate students from developed countries: only 8 percent of subjects in Andreoni and Sprenger (2012) and 16 percent of subjects in Augenblick, Niederle, and Sprenger (2015) exhibit some inconsistency, compared to 60 percent in our study.

B. Main results: Do poverty-related concerns affect time discounting?

We find that subjects in the HARD poverty condition behave less patiently in the experiment than subjects in the EASY condition, and this result holds for all five substitution rates (Figure 2). On average, subjects in the HARD poverty condition allocated 23.09 minutes of entertainment to the earlier date, compared to 21.34 in the EASY condition, an economically meaningful difference.

Table 3 shows this pattern in a regression framework.¹⁴ We regress the minutes of entertainment allocated to the earlier week on the poverty prime and calories treatment, and control for the indicator of the patient default, the indicator for initial Week 1 allocations, substitution rate, age and gender, with robust standard errors clustered at the individual level. The HARD poverty prime increases entertainment allocated to an earlier week by 1.7-minutes ($p=0.04$).

In columns 2-6, we break down the decisions by substitution rate and find that the effect of the poverty prime is relatively stable. The HARD condition increases entertainment allocated to the earlier week by 1.42 - 2.13 minutes, and the p-value of the least statistically significant difference between the HARD and EASY conditions is 0.14. The stability across substitution rates provides evidence against the interpretation that the effects of the poverty prime on inter-temporal decisions are driven by changes in the curvature of the utility function. This also provides a first indication that the shift in inter-temporal choices is not due to a difference in the level of attention to information about the substitution rate. We revisit this question in more detail in Section II.C.

The effect of the HARD poverty prime treatment on time allocated to entertainment at the earlier date is robust to several alternative specifications (Tables A5 and A7), including individual-

¹⁴ In the main estimates, we focus on analysis of the raw data, using similar regression specification as in Carvalho, Meier, and Wang (2016), who study the effects of payday on discounting.

level random effects, village-level fixed effects, experimenter fixed effects, individual observable characteristics, including education, income, and household size, and non-linear controls for the substitution rate.¹⁵

An interesting question is whether the poverty prime causes a relatively smaller shift in discounting for many subjects, or whether the effects are driven by the large responses of a few individuals who, after exposure to the prime, lose all self-control and start to behave very impatiently. Since we implemented the prime using a “between-subject” design, we cannot estimate the effect at the individual level. Nevertheless, a comparison of the distributions of discounting choices in the HARD vs. EASY conditions is informative. Figure A3 reveals a relatively smooth shift in the distribution – all three of the most impatient options are more prevalent (and all three less patient options are less prevalent) in the HARD condition, suggesting the treatment effects are driven by the moderate responses of a larger set of subjects.

Next, the effect does not seem to operate through increased salience of a subject’s own mortality, which would lead to an increased discount factor through risk perceptions, as the point estimate of the HARD poverty prime is actually larger, though insignificantly, when restricting the sample to individuals who did not face scenarios related to their own health issues (Table A8).

Finally, we study the effect of the poverty prime across situations in which early consumption of entertainment is more tempting. We consider the role of two manipulations in particular: whether entertainment can be consumed immediately and whether consuming

¹⁵ As discussed above, the population we study had been exposed to conflict. Earlier research has documented that experiencing conflict may have lasting effects on preferences (Voors et al. 2012; Callen et al. 2014; Bauer et al. 2016). Similarly to Voors et al. (2012), we observe that individuals with above median conflict exposure tend to be less patient and allocate more minutes to early entertainment relative to those below the median (22.70 minutes versus 21.84 minutes, $p=0.07$). Although the point estimate for the HARD poverty prime is also higher for the above median group (Panel B, Columns 8 and 9, Table A5), the effects are qualitatively similar for both groups.

entertainment early is the salient option. This allows us to tentatively gauge which decision situations are likely to be the most relevant to the psychological effects of poverty on entertainment discounting, thus potentially providing some insight into what real-life situations the findings are likely generalizable.

First, we test whether the effect of the HARD poverty prime is stronger when entertainment can be enjoyed immediately, and thus the impatient option is more tempting (Columns 1-2, Table 4). We estimate the effects of the HARD prime separately for decisions made in Week 1, when subjects decided how to allocate entertainment over two future dates, and for decisions made in Week 2, when subjects faced a trade-off between immediate and future entertainment consumption. We find that the effect of the HARD poverty prime is larger for immediate rewards. The effect is still positive but smaller and no longer statistically significant when we consider allocation over two future dates. The interaction effect between HARD and delayed early consumption of entertainment does not reach statistical significance, potentially due to lack of statistical power.¹⁶

Second, we explore whether the effect of thinking about poverty is larger when consuming entertainment early is made more salient by being the default option (Columns 3-4 of Table 4). We find that the HARD poverty prime increases the allocation of entertainment to an earlier date by 2.31 minutes in the IMPATIENT default condition, and the effect is statistically significant. In the PATIENT default condition, the effect of the HARD poverty prime has the same sign, but it is small and not statistically significant. The interaction effect between HARD poverty prime and

¹⁶ We come to similar conclusions when comparing values of β in structural estimates of present bias across treatments. We find evidence of present bias on average in both priming treatments: $\beta=0.88$ and $\beta=0.93$, in the HARD and EASY treatments, respectively, though only the former value differs statistically from 1 at the 90 percent level (Table A6). The treatment difference in present bias is not statistically significant ($p=0.29$).

PATIENT default does not reach statistical significance. (Figure A4 plots the coefficients for the HARD poverty prime for both default treatments, separately).

Together, these results show that the poverty prime affects decision making in situations similar to scenarios that subsistence farmers face in their everyday lives. We find that the effect of the poverty prime is statistically significant in Week 2, when there is an immediate consequence of the choices. This is arguably the condition with the most economic significance for poor individuals such as the farmers in our sample, since it is likely that they usually choose how to allocate their time on a daily basis, rather than making binding commitments for work and entertainment in the future (analogous to the Week 1 decision). It is somewhat less clear which of the default conditions is more closely related to decision making out of the lab. In any case, it is reassuring that we find a muted effect of the poverty prime in only a single condition, when earlier consumption is the least salient across both dimensions: i.e. when working sooner is the default option and when decisions involve allocations over future dates only (Columns 5-6 of Table 4, Figure A4). This offers support for the generalizability of our findings.

C. Further results

In this sub-section, we address the question of whether the observed effects of the poverty prime on discounting could be explained by reduced attention to the task, or less understanding and consequently more noisy decisions, rather than by direct effects on time preferences. We proceed in two steps. First, we study the effects of the poverty prime on proxies of attention to task (response time, information acquisition), and on the prevalence of patterns in choice data that could indicate lower quality decision-making (violations of monotonicity, sticking to the default allocation, low sensitivity to the substitution rate). As a next step, we test the robustness of the

main effects among sub-samples of subjects for whom there is less reason to worry about a lack of understanding.

In terms of measures of the decision-making process, in Table 5, Panel A, we show that the poverty prime does not significantly affect overall decision-making time (Columns 1 and 2). Next, subjects in the HARD poverty condition were not more distracted than those in the EASY condition, as measured by the amount of time spent looking away from the decision environment (Column 3), and the poverty prime has virtually no influence on the number of options viewed (Column 4 and 5). Further, in Table A9, we consider additional variables derived from the videos of subjects' decision making, including a single standardized index and a first principal component of all measures, and arrive at the same conclusion: none of the estimated coefficients suggests that the poverty prime reduces attention to the discounting task.¹⁷

If subjects in the HARD condition were less attentive or less able to understand the task, we would expect them to be less sensitive to changes in the substitution rate, more likely to stick with the default option and more likely to violate monotonicity. However, first, the effect of the HARD poverty prime is relatively stable across different substitution rates, indicating that the prime has not reduced the ability to acquire and process information about the substitution rate (Column 1 of Table 5, Panel B). Second, subjects exposed to the HARD poverty prime are not more likely to stick with default option (Column 2) or to make choices with closer distance to the default allocation (Column 3), as compared to the EASY poverty prime. Third, we consider two measures of violations of monotonicity. The first is the number of inconsistencies, which yields values between 0 and 4 for each week. The second measure takes a different approach, by defining

¹⁷ Also, in line with these findings, we find no effect of the poverty prime on cognitive skills, as measured by the number of correctly solved Raven's matrices (Table A10).

the minimum number of flips through the five booklets required to make the allocation consistent with monotonicity. Subjects in the HARD treatment made slightly more violations of monotonicity on average (out of the maximum of four): 0.63 inconsistent choice pairs compared to 0.53 in the EASY condition ($p=0.11$). The coefficients for the HARD poverty prime are positive but not statistically significant for both measures (Columns 4-5, Panel B of Table 5).

To assess whether the lack of statistically significant effects on proxies of the quality of decision-making could be due to insufficient power, we calculate minimum detectable effects (MDEs) for the HARD poverty prime for each measure (Table A11). Appendix D describes how MDEs are calculated. The median MDE obtained is 15% (the range is between 9% and 61%), measured as a minimum detectable change relative to the mean of the respective variables for the EASY poverty prime. While the estimated MDEs are high for some of the measures, we find it remarkable that we did not detect any statistically significant effect for any of the 15 measures of attention, including those with relatively low MDEs.¹⁸

Finally, we test the robustness of the main effect among sub-samples of subjects for whom there is less reason to worry about a lack of understanding. We find that the estimated coefficients are similar in magnitude when we (i) exclude subjects who did not answer all comprehension questions correctly (Column 3, Panel A of Table A5), (ii) exclude subjects who made one or more choices that violated monotonicity in a particular week (Columns 4-5), (iii) control for the number of inconsistent choices (Column 6), and (iv) exclude subjects who are illiterate (Column 7). Further, if the effect were driven by confusion or misunderstanding of the task, it should be larger

¹⁸ We test for joint insignificance of the main variables of interest across Tables V and A9 (HARD poverty prime interacted with the substitution rate in the first column of Panel B in Table 5 and HARD poverty prime elsewhere), using the multi-equation omnibus test of joint insignificance (Young 2019). The results indicate a very high likelihood of joint insignificance ($p=0.99$). The same test rejects joint insignificance of the HARD poverty prime for all models in Table 3 ($p<0.01$) and in Table 4 ($p<0.01$).

in Week 1 when the subjects made choices for the first time than in Week 2, when they were already familiar with the task. Yet, when comparing the effect of HARD poverty prime across the two weeks, we find that it is somewhat stronger (though the difference is not statistically significant) in Week 2 than in Week 1. Finally, using a simulation, we show that the main result is unlikely to be driven by increased prevalence of random choices among the subjects in the HARD poverty prime (Figure A5).

In sum, a rich set of tests does not favour the interpretation that thinking about poverty affects discounting through less attention and/or reduced understanding of the task.

III. Links to theory

This section provides a discussion that aims to illuminate possible mechanisms through which poverty related concerns can affect impatience in our experiment. Since economic constraints, such as time and liquidity constraints, are closed down by design, we do not consider these factors as potential explanations. Also, since we do not find effects of the HARD poverty prime on measures of inattention, we do not formalize this mechanism.

We first consider the well-established dual-self model of Fudenberg and Levine (2012), which assumes that individuals are in constant conflict between a short-run self, seeking immediate gratification, and a forward-looking long-run self. We show that under a plausible assumption that costs of self-control increase with cognitive load, anxiety and salience of the tempting option, the model can fully explain the set of findings. Next, we discuss the idea that, while similar results may be obtained by assuming that individuals have immediate disutility from negative affect or that individuals have state-dependent time preferences, the assumptions would have to be fairly

strong to explain the full set of results we find. In Appendix B we provide a formal framework that aims to illuminate the mechanisms. It is also noteworthy that the psychological mechanisms we describe here neither rely on different psychological constraints of the rich and poor, nor on mental processes that would be specific to poverty. Instead, the aim is to illuminate how general psychological processes are more prone to affect behaviour of the poor, given the difficulties of the environment they live in.

The dual-self model of costly self-control by (Fudenberg and Levine 2012) assumes a constant conflict between a short-run self seeking immediate gratification, and a forward-looking long-run self. The long-run self can only impose its preferences on the short-run self, an ultimate decision-maker, by paying a mental cost of self-control. Earlier research, not necessarily focusing on the role of poverty, suggests that the cost of self-control is affected by external conditions taxing mental capacity (Shiv and Fedorikhin 1999; Gul and Pesendorfer 2001; Toussaert 2018). Our experiment is designed to increase the costs of self-control by several experimental manipulations. First, we assume that the HARD poverty prime increases costs of self-control since the process of actively thinking about poverty and how to resolve the presented scenarios consumes subjects' mental processing resources. Second, the cost of self-control is likely to be higher when individuals are making their choices in period 2 when they can obtain instant gratification from consuming entertainment immediately rather than having to wait. Third, the salience of temptations is also higher when respondents are presented with immediate consumption of entertainment through IMPATIENT default.

Indeed, we observe more impatient choices for each of these experimental manipulations. We document three key findings. (1) Participants are more impatient under the HARD poverty prime than under the EASY poverty prime. (2) The effect of the HARD poverty prime is stronger

when combined with the IMPATIENT default. (3) Although statistically insignificant, the effect of the HARD poverty prime is higher when decisions are made just before the first activity hour, in Week 2. All of these findings are predicted by the costly self-control model.

We consider two alternative models. First, participants may derive disutility from a negative affect when experiencing poverty related concerns (Lerner, Li, and Weber 2013; Lerner et al. 2015). As long as the negative affect enters a utility function, individuals may compensate themselves for it by consuming more entertainment. This would be consistent with the main finding (1), that the HARD poverty prime increases impatience. Our finding (3), a weaker effect of HARD poverty prime in Week 1, relative to Week 2 choices, would also be consistent with a model of negative affect if individuals were able to predict their future emotions and discounted them. Nevertheless, individuals seem to be rather poor in predicting own future emotions (Frederick, Loewenstein, and O'Donoghue 2002; Wilson and Gilbert 2003). Finally, the negative affect model predicts no effect of IMPATIENT default on decision making. Thus, while the negative affect model can explain some of our findings, it cannot be the sole explanation.

Second, we consider a model of state dependent preferences. Following (Becker and Mulligan 1997) we assume that the discounting parameter changes with external conditions, in our case psychological states of the mind of our participants. Yet in order to be able to explain all three key findings, we would also need to impose such state-dependency beyond the effect of the poverty prime, on the domains of time and default. While this is theoretically plausible, we believe it is rather unlikely that such specifically state-dependent parameters would drive all of our results.

IV. Conclusions

This paper presents experimental evidence on the psychological effects of poverty on time preferences. We study farmers in Uganda, exogenously manipulate the extent of their thinking about financial pre-occupations, and then measure their inter-temporal choices in an entertainment discounting task. The results show that concerns about poverty-related problems increase individual preferences for earlier consumption of entertainment. In addition to measuring choices, we employed monitoring techniques and gathered detailed data on the decision-making process, which suggest that the behavioural change induced by the poverty scenario is unlikely to be driven by differences in attention to the task. Taken together, our results support the interpretation that thinking about poverty directly influences time preferences.

Our results speak to a long-standing debate about why the poor behave differently from the rich. “Two-systems” models of individual decision making (Bernheim and Rangel 2004; Fudenberg and Levine 2006) treat decision-making as a result of a strategic interplay between an impulsive agent and a forward-looking agent who can reduce the influence of the impulsive agent only by drawing on a limited budget of cognitive resources. The results indicate that such a two-system model may be a useful way to think about the psychological impacts of poverty. The poor may not necessarily have different hardwired time preferences than the rich, but their impulsive self may more easily affect behaviour due to an increased cognitive load or stress associated with poverty. Also, since such an effect may create a feedback loop between poverty and impatience, our findings provide empirical support for recent efforts to model behavioural poverty traps (see the recent classification of Ghatak 2015), in particular those based on the assumption that poverty directly reduces self-control (Bernheim, Ray, and Yeltekin 2015; Banerjee and Mullainathan 2010).

These findings are potentially important for policy. First, if thinking about poverty-related problems directly increases time preferences, then there may be an additional mechanism, besides the standard economic channels, through which even temporary anti-poverty programs may have lasting positive impacts on economic activity and accumulation of assets. In this context, it is noteworthy that a recent series of randomized evaluations of simple unconditional cash transfers finds promising impacts, documenting positive effects on measures of economic activity and human capital investments, but zero or negative effects on alcohol and tobacco consumption (Blattman and Fiala 2014; Haushofer and Shapiro 2016). Second, the timing of subsidies or offers for products that involve future-oriented decisions may play a large role. In line with this reasoning, the evidence shows that making investment decisions outside of periods of intense scarcity induces more patient choices, such as increased purchases of fertilizers (Duflo, Kremer, and Robinson 2011). Third, perceptions about the sources of inequality have been shown to play an important role in willingness to redistribute from rich to poor (Cappelen et al. 2007). Negative views on helping the poor are often tied to a presumption that poverty originates in reckless behaviour. Enjoying entertainment while putting off work until later – the choice in our experiment - is frequently featured as an example of such condemnable behaviour. Here we provide unambiguous evidence that the relationship between economic circumstances and (lack of) patience is more complex, by demonstrating that it is, at least in part, driven by poverty damaging the ability to exercise self-control.

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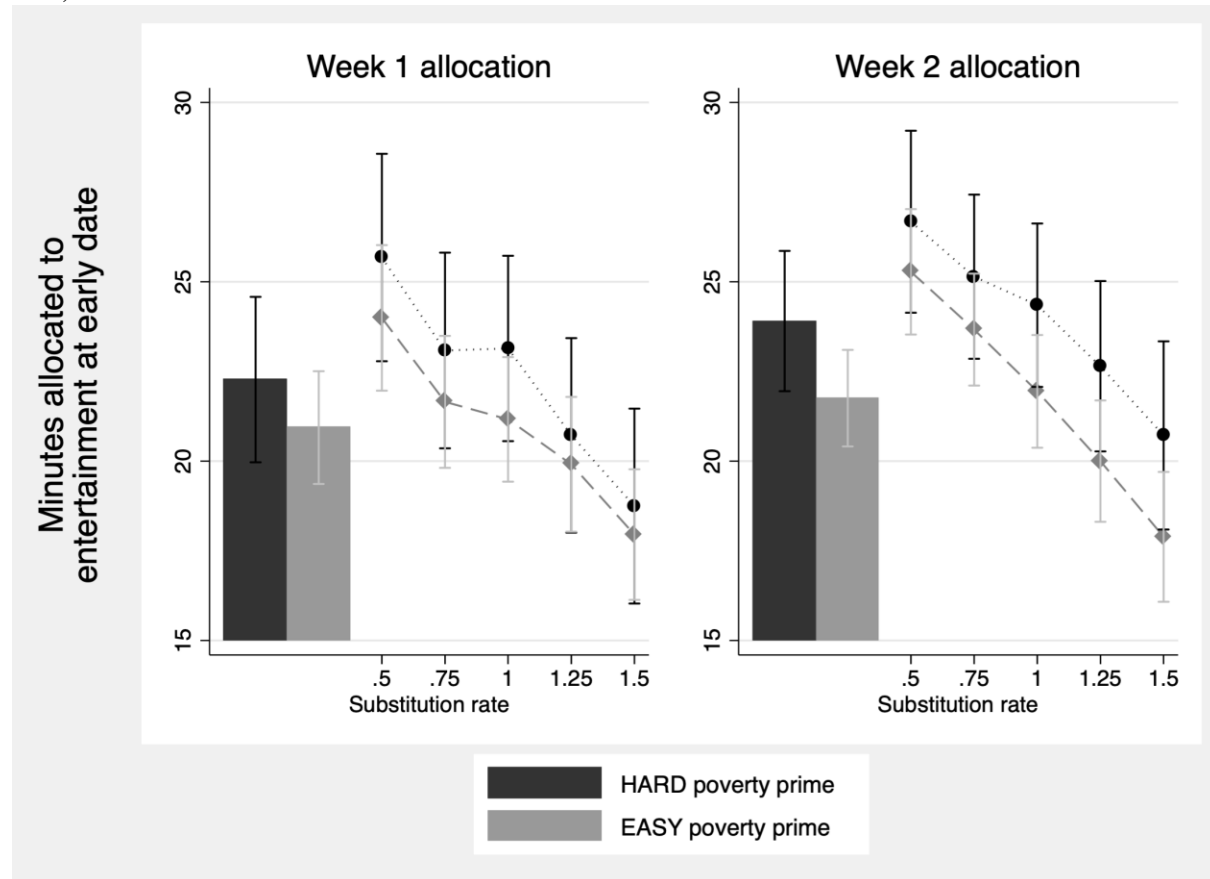
Figures and Tables

Figure 1: Entertainment allocation environment



Notes: Decision-making booklets shown to participants. The left-hand side refers to Week 2 allocation of work and entertainment. The numbers inside the small TV icon (top right corner of each of the cards) and the blue part of the pie-charts refer to minutes of entertainment. The remaining white part of the pie-chart represents the number of minutes of work to the full hour in that week. Analogously, the right-hand side of each booklet refers to Week 3 allocations. Green is used to represent the Week 3 entertainment time. Each row represents a different intertemporal substitution rate. On each row there are six pages corresponding to six levels of early consumption of entertainment (see Appendix Table A1).

Figure 2: Minutes allocated to entertainment at an early date: by poverty prime, substitution rate, and week



Notes: The thick bars represent choices aggregated over all substitution rates, while the dots indicate choices at the respective substitution rates. Error bars represent 95% level confidence intervals from a regression with standard errors clustered at the individual level.

Table 1: Timeline of the Experiment

Beginning of Session →								→ End of session		
								Payment ^f		
	<i>Group</i>	<i>Minimum</i>	<i>Poverty</i>	<i>Work and</i>		<i>Work and</i>	<i>Raven's</i>	<i>Survey^e</i>		
	<i>instructions</i>	<i>work and</i>	<i>prime</i>	<i>entertainment</i>	<i>Draw</i>	<i>entertainment</i>	<i>progressive</i>		<i>Weekly</i>	<i>Completion</i>
		<i>entertainment^a</i>	<i>and</i>	<i>allocations^c</i>	<i>decision</i>	<i>hour</i>	<i>matrices^d</i>		<i>fee</i>	<i>bonus</i>
Week 1	x	x	x	x					x	
Week 2	x	x	x	x	x	x			x	
Week 3		x				x	x	x	x	x

Note: ^aThe minimum work/entertainment consisted of 5 minutes of watching videos and 5 minutes of sorting beans. ^bThe poverty priming procedure and calorie treatment were timed in order that the work and entertainment allocations were completed under elevated stress and blood glucose levels. ^cSubjects made 5 decisions in each week corresponding to various intertemporal substitution rates. ^dThe poverty and calorie priming procedure was repeated for a third time in week 3 directly before the raven's progressive matrices. ^eIn week 1, a short survey on basic demographic information was administered. In all three weeks subjects were asked about diet over the previous week. In week 3, we collected additional information about demographics, conflict and credit history (See Appendix E and G for full survey texts). ^fThe weekly participant fee was UGX 2,000, and the completion bonus was UGX 15,000.

Table 2: Summary statistics

	Mean (1)	Std. Dev. (2)	Min (3)	Max (4)	N (5)
<i>Panel A: Observable characteristics</i>					
Age	35.45	9.96	20.00	57.00	289
Female (dummy)	0.51	0.50	0.00	1.00	289
Married (dummy)	0.35	0.48	0.00	1.00	289
Household size	7.18	3.82	0.00	30.00	289
Education (years)	5.16	3.48	0.00	13.00	289
Able to write a letter (dummy)	0.53	0.50	0.00	1.00	278
Monthly earnings (in thousands. UGX)	241.8	657.3	0.0	8178.8	289
Household owns a bicycle (dummy)	0.63	0.48	0.00	1.00	278
Household owns a radio (dummy)	0.51	0.50	0.00	1.00	278
Household owns cattle (dummy)	0.40	0.49	0.00	1.00	278
Household owns a mobile phone (dummy)	0.55	0.50	0.00	1.00	278
Brick walls (dummy)	0.36	0.48	0.00	1.00	278
Number of meals taken during a day	1.90	0.52	1.00	4.00	278
Number of days unable to work due to sickness during the last 4 weeks	2.69	5.08	0.00	31.00	278
Cognitive skills (0-5)	2.86	1.32	0.00	5.00	289
Index of conflict exposure (0-12)	5.87	3.09	0.00	12.00	240
<i>Panel B: Experimental choices</i>					
Entertainment consumed early (minutes)	22.2	11.4	5.0	45.0	2870
Entertainment consumed early (minutes, Week 1)	21.6	12.0	5.0	45.0	1445
Entertainment consumed early (minutes, Week 2)	22.8	10.7	5.0	45.0	1425
Number of inconsistencies (0-4)	0.58	0.74	0.00	4.00	574
Distance from consistency (average)	0.90	1.35	0.00	8.00	574
Distance from default allocation (average)	2.90	1.65	0.00	6.00	2870
Sticking to the default allocation (dummy)	0.09	0.28	0.00	1.00	2870
<i>Panel C: Process of decision-making</i>					
Total decision-making time (seconds)	220.1	126.8	43.1	880.2	506
Distraction time (seconds)	3.6	7.4	0.0	61.7	506
Number of options viewed (average, out of 6)	3.6	1.6	0.0	6.0	2530
Total number of page views	37.6	24.5	5.0	216.0	506

Notes: Panel A reports summary statistics for the observable characteristics. The cognitive skills variable measures the number of Raven's matrices correctly solved by the individual (out of 5). The index of conflict exposure is the sum of positive responses to 12 questions on different types of exposure to violence (see Appendix G for details). Panel B reports summary statistics for experimental choices. The number of inconsistencies (0 to 4) is defined as the number of violations of the law of demand at adjacent substitution rates within a given week, i.e. if fewer minutes are allocated to the earlier date at a lower substitution rate, compared to the number of minutes allocated at a higher substitution rate. Distance from consistency is defined as the minimum number of flips through the decision-making booklet required to make the allocation consistent with the law of demand. Distance from default allocation is defined as the number of page flips from the default allocation in the booklet at a given substitution rate. Sticking to the default is an indicator for whether the individual selected the allocation provided by the experimenter by default. Panel C reports summary statistics for the decision-making process: the total decision-making time in a given week, the distraction time – the amount of time the individual was looking away from the decision-making booklet, the number of options that the individual viewed at least once at a given substitution rate (out of the 6 options), and the total number of page views (at all substitution rates), regardless of whether the page was visited once or repeatedly.

Table 3: Time discounting

Dependent variable	Entertainment sooner (minutes)					
		Substitution rate				
Choices	All	0.5	0.75	1	1.25	1.5
	(1)	(2)	(3)	(4)	(5)	(6)
HARD poverty prime	1.71** (0.85)	1.56 (1.03)	1.42 (0.97)	2.13** (0.96)	1.69* (1.02)	1.76 (1.08)
CALORIES condition	0.34 (0.84)	0.42 (1.02)	0.18 (0.97)	0.34 (0.96)	0.58 (1.00)	0.16 (1.08)
PATIENT default	-1.79** (0.85)	-1.57 (1.03)	-2.89*** (0.96)	-2.67*** (0.97)	-1.64 (1.01)	-0.19 (1.08)
Only delayed rewards	-1.19* (0.68)	-1.11 (0.91)	-1.98** (0.83)	-0.94 (0.77)	-0.96 (0.81)	-0.94 (0.81)
Substitution rate	-6.29*** (0.61)					
Constant	29.45*** (1.80)	29.76*** (2.12)	26.73*** (1.98)	23.18*** (1.95)	20.06*** (2.04)	16.08*** (2.30)
Observations	2,870	574	574	574	574	574
R-squared	0.054	0.028	0.036	0.029	0.018	0.013
Mean of dep. var. for EASY poverty prime	21.34	24.32	22.65	21.55	19.96	17.92

Notes: OLS, standard errors clustered at the individual level in parentheses. The dependent variable in all columns is the number of minutes allocated to entertainment at an early date (Week 2). All regressions include controls for age and gender.

*** Significant at the 1% level.

** Significant at the 5% level.

* Significant at the 10% level.

Table 4: Effects of prime on time discounting: The role of contextual features

Dependent variable	Entertainment sooner (minutes)					
	Only delayed rewards		PATIENT default		Only delayed rewards and PATIENT default	
Choices	All (1)	Only delayed rewards (2)	All (3)	PATIENT default (4)	All (5)	Only delayed rewards and PATIENT default (6)
HARD poverty prime	2.10** (0.98)	1.37 (1.17)	2.31** (1.13)	0.87 (1.27)	2.68** (1.22)	-0.35 (1.68)
CALORIES condition	0.34 (0.84)	0.69 (1.17)	0.31 (0.85)	1.04 (1.28)	0.32 (0.85)	1.26 (1.71)
PATIENT default	-1.79** (0.85)	-0.74 (1.17)	-1.19 (1.17)		-1.20 (1.17)	
HARD prime x PATIENT default			-1.22 (1.73)		-1.20 (1.73)	
Only delayed rewards	-0.81 (0.91)		-1.18* (0.68)	-0.12 (0.99)	-0.81 (0.91)	
HARD prime x Only delayed rewards	-0.76 (1.35)				-0.74 (1.35)	
Substitution rate	-6.29*** (0.61)	-6.00*** (0.75)	-6.29*** (0.61)	-5.27*** (0.87)	-6.29*** (0.61)	-5.19*** (1.10)
Constant	29.26*** (1.83)	28.54*** (2.43)	29.13*** (1.87)	21.63*** (2.58)	28.94*** (1.89)	23.17*** (3.59)
Observations	2,870	1,445	2,870	1,415	2,870	725
R-squared	0.054	0.038	0.055	0.037	0.055	0.032
Mean of dep. var. for EASY poverty prime	21.34	20.93	21.34	20.75	21.34	21.32

Notes: OLS estimates in all columns. Standard errors clustered at the individual level in parentheses. The dependent variable in all columns is the number of minutes allocated to entertainment at an early date. All regressions include controls for age and gender.

*** Significant at the 1% level.

** Significant at the 5% level.

* Significant at the 10% level.

Table 5: Quality of decision-making

	(1)	(2)	(3)	(4)	(5)
Panel A: Direct measures of attention					
Dependent variable	Decision-making time		Distraction time	Information acquisition	
	Seconds		Seconds	Number of options viewed at a given price	Total number of page views at a given price
Sample	All	Excluding outliers	All	All	All
HARD poverty prime	-8.17 (12.67)	-2.47 (7.80)	0.07 (0.66)	-0.06 (0.13)	-0.24 (0.44)
CALORIES condition	-10.70 (12.58)	0.10 (7.84)	0.25 (0.65)	-0.02 (0.13)	-0.10 (0.45)
PATIENT default	21.53* (12.88)	20.46*** (7.81)	1.10 (0.68)	-0.14 (0.13)	0.68 (0.43)
Only delayed rewards	32.22*** (9.35)	24.18*** (5.89)	1.55** (0.66)	0.17* (0.09)	0.62* (0.37)
Substitution rate				-0.58*** (0.11)	-1.27*** (0.32)
Constant	187.69*** (31.42)	122.89*** (14.99)	2.55** (1.28)	4.31*** (0.28)	8.83*** (1.11)
Observations	506	456	506	2,530	2,530
R-squared	0.028	0.078	0.023	0.023	0.018
Mean of dep. var. for EASY poverty prime	224.06	189.21	3.62	3.67	7.22
Panel B: Patterns of choices					
Dependent variable	Automatic decision-making			Inconsistency in choices	
	Entertainme nt sooner (minutes)	Sticking to the default allocation at a given price (dummy)	Distance from default allocation at a given price	Number of inconsiste ncies (0-4)	Distance from consisten cy
HARD poverty prime	1.40 (1.44)	0.01 (0.02)	-0.10 (0.11)	0.10 (0.07)	0.13 (0.13)
CALORIES condition	0.34 (0.84)	-0.01 (0.02)	0.04 (0.11)	-0.05 (0.07)	-0.07 (0.13)
Substitution rate	-6.44*** (0.86)	0.04** (0.01)	0.14 (0.09)		
HARD poverty prime*Substitution rate	0.32 (1.23)				
PATIENT default	-1.79** (0.85)	0.15*** (0.02)	-1.73*** (0.10)	0.12* (0.07)	0.27** (0.13)
Only delayed rewards	-1.19* (0.68)	0.01 (0.02)	0.14 (0.08)	-0.01 (0.05)	0.03 (0.09)
Constant	29.61*** (1.88)	-0.03 (0.04)	3.06*** (0.22)	0.19 (0.15)	0.24 (0.28)

Observations	2,870	2,870	2,870	574	574
R-squared	0.054	0.071	0.278	0.041	0.033
Mean of dep. var. for EASY poverty prime	21.34	0.08	2.92	0.54	0.84

Notes: OLS, standard errors clustered at the individual level in parentheses. All regressions include controls for age and gender.

Dependent variables in Panel A: (1) the total decision-making time (in seconds), (2) the total decision-making time excluding the 10 percent of observations with the longest decision-making time, (3) the time the individual was looking away from the decision-making booklet (in seconds), (4) the number of options that the individual viewed at least once at a given substitution rate (out of the 6 options), and (5) the total number of page views at a given substitution rate, regardless of whether the page was visited once or repeatedly.

Dependent variables in Panel B are: (1) the number of minutes allocated to entertainment in Week 2, (2) an indicator for whether the individual selected the allocation provided by the experimenter by default at a given substitution rate, (3) the number of page flips from the default allocation in the booklet at a given substitution rate, (4) number of inconsistencies (0 to 4) defined as number of violations of the law of demand at adjacent substitution rates within a given week, i.e. if fewer minutes are allocated to an earlier date at a lower substitution rate, compared to the number of minutes allocated at a higher substitution rate, and (5) the minimum number of flips through the decision-making booklet required to make the allocation consistent with the law of demand.

*** Significant at the 1% level.

** Significant at the 5% level.

* Significant at the 10% level.