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AUGUST 2023

ISSN: 2365-9793

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ABSTRACT

Is Patience Malleable via Educational Intervention? Evidence on the Role of Age in Field Experiments*

We study the age-dependent malleability of patience via educational interventions designed to foster financial decision-making capabilities and to induce a more future-oriented mindset. We conduct a field experiment covering both youths and adults in Uganda and aggregate evidence from earlier experiments to study the generalizability of effects. In our field experiment, we find heterogenous effects by age: adults' patience and discount factors are unaffected by the intervention after 15 months follow-up, but we observe large effects on patience and estimated discount factors and field saving behavior for youth. In the meta-analysis, we find that the results are generalizable across contexts.

JEL Classification: C93, D15, I21, G53

Keywords: patience, time preferences, malleability, field experiment, educational intervention

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* We thank Filder Aryemo, Lucy Haag, Jana Hamdan, Helke Seitz, and Yuanwei Xu for excellent field work and research assistance, and thank seminar and conference participants in Hamburg, Landau, Mannheim, San Diego, and Washington, D.C., as well as Martin Brown, Tabea Bucher-Koenen, Marius Cziriak, Daniel Kamhöfer, K. Jeremy Ko, Fabian Kosse, Annamaria Lusardi, David McKenzie, Alberto Rossi, and Carly Urban for helpful comments. This study is registered in the AEA RCT registry under AEARCTR-0009971. Declaration of interest: none. Funding: This research was partially funded by the German Research Foundation (DFG) through the Research Training Group (RTG) 1723 "Globalization and Development."

1 Introduction

The ability to delay consumption to reach a higher level of consumption in the future is crucial for investments of individuals and societies. Accordingly, it is shown that individuals' higher degree of patience is related to more savings, higher education and better labor market outcomes (e.g., Sutter et al., 2013; Falk et al., 2018; Epper et al., 2020). Qualitatively, the same empirical relationship exists at the country level, such that more patience in societies is correlated with higher human capital (Hanushek et al., 2022) and higher country incomes (Sunde et al., 2023). While, in neoclassical models of individual decision-making, preferences are traditionally assumed to be stable (e.g., Stigler and Becker, 1977), newer theories are often open regarding variability of preferences and personality traits (e.g., Becker and Mulligan, 1997; Doepke and Zilibotti, 2017; List et al., 2021). Empirical studies provide evidence of intra-individual variation in preferences, including time preferences, in response to exogenous shocks and over the life cycle (e.g., Malmendier and Nagel, 2011, Callen et al., 2014, Hanaoka et al., 2018, on risk preferences; Voors et al., 2012, Callen, 2015, on time preferences). Given the evidence of variation in preferences, we ask the general question: is there a causal effect of *educational intervention* on time preferences?

Recent evidence from field experiments may support the hypothesis that time preferences are indeed malleable via targeted educational interventions designed to foster financial decision-making capabilities and to induce a more future-oriented mindset (e.g., Alan and Ertac, 2018; Lührmann et al., 2018; Sutter et al., 2023). As existing evaluations of educational interventions focus on children and youth, this raises the question whether treatment effects are limited to these early stages of the life cycle (Cunha and Heckman, 2007; Cunha et al., 2010). If preferences are mainly shaped during the process of personality formation, then it seems questionable whether educational interventions may still have effects on the preferences of adults. The role of age in shaping personality is self-evident in the *psychological literature*. For example, Caspi et al. (2005, p.468) state in their review that certain personality traits change less with age and that "the majority of personality change occurs in young adulthood." Moreover, there is evidence that preferences are reflected in neuroscience results (e.g., DeYoung et al., 2010) and that brain plasticity is negatively correlated with child age (e.g., Sherwood and Gómez-Robles, 2017).

Thus, we study the interrelated questions whether time preferences are malleable and whether age plays a role for malleability in two ways: first, we conduct a field experiment with participants' age ranging between 16 and 82 years testing a change in time preference due to a financial training. Second, we aggregate the evidence of eleven RCTs in a meta-analysis to study the generalizability of treatment effects. We find that educational interventions impact time preference for children, youth, and young adults only. Both approaches are new to this literature, i.e., the coverage of such a broad age range within a single experiment and the meta-analysis; they are outlined in more detail below. Our study is also among the first of a new generation of experimental work combining primary field experiments with meta-analyses to study the generalizability and external validity of experimental results (see Angrist and Meager, 2023).

We conduct a field experiment comprising 1,217 individuals on revealed time preferences covering both youth and adults in rural Uganda. We study the effect of a financial education intervention on time preferences elicited via an incentivized Convex Time Budgeting Task (Andreoni and Sprenger, 2012) 15 months after treatment. The experiment is designed to cover a broad group regarding age, ranging from 16 to 82 years, allowing investigation of heterogenous treatment effects by age within one context. We find that the time preferences of adults are unaffected by the educational interventions 15 months later, while we observe meaningful treatment effects on patience for youth in our setting: the treatment substantially reduces impatient choices in the task and increases estimated individual discount factors. These heterogenous treatment effects also extend to field saving behavior elicited in a household survey. At the same time, and in contrast to the field experiment by Lührmann et al. (2018), our educational intervention does neither affect respondents' financial numeracy nor their time-consistency, choice consistency, nor the likelihood of allocating the entire budget to a single-payment date instead of splitting payments between two dates. The treatment also has no effect on income or credit at the intensive and extensive margins. Thus, the result on discounting is neither driven by a decrease in narrow bracketing nor by a change in liquidity constraints potentially violating identifying assumptions of utility parameters.

Next, we test the generalizability of the hypothesis of age-depended malleability of time preferences by aggregating the evidence from all available evaluations of educational interventions that combine field experiments with incentivized decision experiments eliciting revealed patience in a meta-analysis. This estimates the average effect of educational intervention on patience and the heterogeneity in true effects across the eleven included studies. We find a tentatively positive average effect of educational interventions on patience, i.e., a 0.05 SD reduction in incentivized measures of impatience, but the 95 percent confidence interval does not rule out null effects. Additionally, heterogeneity in true effects is non-trivial, as 66 percent of variability in estimated treatment effects between studies may be attributed to heterogeneity in true effects rather than within-study measurement error; this raises the question about the sources of heterogeneity in true effects across studies. Thus, we consider the age of students, delay between intervention and time preference elicitation, intensity of education as well as differences across elicitation protocols and country settings as potential mediators of treatment effects. We find that only student age and the delay between intervention and time-preference elicitation are statistically significant predictors of treatment effects across studies: Treatment effects are largest for elementary school children (about 0.18 SD reduction in impatience) and decline with increasing age; one cannot reject zero effects for respondents aged 20 to 30. Additionally, treatment effects appear to fade out at about 20 months after educational intervention.

Our research is related to a large literature about the importance of patience for various life outcomes regarding health, education, labor market, and savings (e.g., Sutter et al., 2013; Golsteyn et al., 2014; Cadena and Keys, 2015; Falk et al., 2018; Figlio et al., 2019; Castillo et al., 2011, 2019; Epper et al., 2020; Angerer et al., 2023; Stango and Zinman, 2023). We also complement the channel of an intergenerational transmission of preferences (Dohmen et al., 2012; Falk et al., 2021; Chowdhury et al., 2022) by adding evidence on the importance of educational inputs outside the family Precisely, we contribute to an emerging literature on the malleability of preferences and non-cognitive skills via educational interventions in general. Several *field experiments* study the malleability of risk preferences (Sutter et al., 2023), time preferences and the quality of intertemporal choice (Alan and Ertac, 2018; Lührmann et al., 2018, Breitkopf et al., 2022; Sutter et al., 2023), effort and grit (Alan et al., 2019; Santos et al., 2022), self-regulation (Sorrenti et al., 2020; Algan et al., 2022; Schunk et al., 2022), social preferences (Cappelen et al., 2020; Kosse et al., 2020), and honesty (Abeler et al., 2023) in formal educational settings. All these studies contribute to the insight that preferences and non-cognitive skills of various kinds may be malleable to some extent, especially among young individuals.

The rest of the paper proceeds as follows: Section 2 describes the setting of our field experiment and time preference elicitation design. Section 3 presents results of this experiment, a discussion of treatment effects on field behaviors and of potentially confounding mechanisms. Section 4 then describes the methods and results of the meta-analysis to study the generalizability of the findings from our field experiment to other settings. Section 5 concludes.

2 Field experiment design and data

2.1 Setting, timeline, and educational intervention

The study is located in the Rwenzori region of rural Western Uganda and conducted among self-employed individuals (shop owners) as a cluster-randomized control trial with randomization occurring at the village-level. The study was conducted with 1,870 individuals from 108 villages between February 2019 (baseline) and April 2021 (endline).[1] We collected baseline data from February to April 2019, implemented the financial education intervention in August and September of 2019, and collected outcome data in October to December 2020 in a phone survey and in an additional in-person tracking effort in April 2021. Thus, outcome data is collected about 15 months after treatment for most individuals (see timeline in Appendix A Table A1). The implementation and data collection were conducted with the help of a local university and a local survey company. The implementers and enumerators were kept unaware of the experimental design and hypotheses to avoid experimenter effects. All surveys were conducted in the local language Rutooro or English (about 3 percent).

We randomly assigned half of the clusters to a financial education program developed jointly by the central bank of Uganda, Bank of Uganda (BoU), and the German Development Cooperation (GIZ) (Appendix Figure A1 shows the location and treatment status of all clusters). The program is delivered as a full day event (approximately four to six hours of exposure to the contents). The educational intervention uses "active learning" teaching methods. The main feature are five distinct stations, each designed to facilitate problem-based learning through mini-cases and group problem-solving. An earlier field experiment (with different sample), evaluating the general effectiveness of this program, finds that this teaching

[1] Respondent-level baseline data were available for each observation prior to the randomization of the clusters. We block stratified the randomization by baseline mobile money account ownership rates (important for the payment of the experimental payoffs in the time preference elicitation task; three strata of "low," "medium," and "high") and baseline financial numeracy scores (six sub-strata; to ensure balance in task comprehension). This leads to 18 strata with six clusters in each stratum randomized into treatment or control.

approach (relative to a traditional community lecture) is effective, especially in increasing savings and business investments (Kaiser and Menkhoff, 2022). The program covers content in the areas of (i) budgeting and personal financial management; (ii) savings and future consumption; (iii) credit and borrowing decisions; (iv) business investing; and (v) mobile payments (see Appendix Text A1 and Table A2 for details). The educational intervention strongly emphasizes the benefits of delaying consumption to gain utility at a later point in time, the benefits of saving, and the importance of having long-term financial goals. Thus, the training studied is similar in content and design to interventions analyzed in the literature (see later Section 4).

2.2 Preference elicitation

We elicit time preferences using a Convex Time Budgeting Task (CTB) (Andreoni and Sprenger, 2012). Because the task is implemented in a phone-survey, we use a simplified version for the CTB developed by Carvalho et al. (2016) for a developing country setting. Specifically, we ask respondents to choose among three allocation options (two corner options and one interior option). As shown in Panel A of Table 1, subjects make choices in four budgets, with varying interest rates (11% and 20%) and time frames. Moreover, by shifting the front-end delay in budget 2 (i.e., the earlier date is "in one month" instead of "today"), we can investigate the possibility of time-inconsistent choices, i.e., present bias. The initial endowment is UGX 6,000 (i.e., about 4.58 USD in 2019 PPP).

< Table 1 about here >

One of the four budgets is randomly selected for payout. We employ several measures to equalize any possible direct or indirect costs of receiving the payments. First, payments are scheduled for payout via mobile money to eliminate any inconvenience arising from physical transactions, i.e., transaction costs are the same across time periods. To further eliminate any residual inconvenience from allocating all payments to a single date, subjects were told they would receive an additional UGX 1,000 "thank-you payment" for participation. The "thank-you payment" was to be received in two payments (500 UGX sooner and 500 UGX later) regardless of the experimental choices and the payoffs were added to these payments (see also Andreoni and Sprenger, 2012, and Appendix C for verbatim instructions). Finally, as the subjects had previously been interviewed in face-to-face interviews (at baseline), we expected relatively high trust among the respondents. Indeed, over 97 percent of respondents stated they had trust in receiving the delayed payments offered in the tasks. Thus, we are confident that these efforts ensure trust between subjects and experimenters as well as equal transaction costs across dates.

Our main measures of patience are based on (i) the proportion of the respective budget allocated to the respective sooner payment date and (ii) a binary indicator whether a respondent chose to allocate the entire respective budget to the earliest possible payment date (i.e., the impatient corner choice).

2.3 Estimation of structural preference parameters

In addition to analyzing the choices in the experimental task, we estimate a structural model allowing joint estimation of utility parameters. Assuming constant relative risk aversion (CRRA), the quasi-hyperbolic utility function (Laibson, 1997) is defined as

$$U(c_t, c_{t+k}) = (c_t - \omega_t)^{\alpha} + \beta_{t=0}\, \delta^k (c_{t+k} - \omega_{t+k})^{\alpha} \tag{1}$$

where δ^k denotes the daily discount factor and β the present bias parameter. β reduces utility from delayed consumption when payments are immediate ($t = 0$). α represents the risk parameter under CRRA, which is jointly estimated in the CTB framework. ω_t and ω_{t+k} denote Stone-Geary consumption minima as used in Andersen et al. (2008). We estimate $\hat{\delta}$ and $\hat{\beta}$ for each individual and use these as additional outcome variables (see Appendix C for details).

2.4 Data

The baseline sample includes 1,870 individuals within 108 clusters surveyed in face-to-face interviews in February to April 2019. Due to the Covid-19 pandemic, the endline survey was conducted in October to December 2020 in a phone survey and in an additional in-person tracking effort in April 2021. We were able to follow-up with 1,655 respondents, i.e., resulting in an attrition rate of 11.5 percent (see Appendix Text A2 and Table A3). We follow Alan and Ertac (2018) and restrict the analysis sample to those who exhibited choice consistency and adequate comprehension of the task, i.e., those whose choices correspond to the law of demand and show no counterintuitive intertemporal preference reversals in the form of "future bias." This leads to the analysis sample of 1,217 subjects. Attrition and comprehension of the CTB task is non-differential between the treatment arms (see also Appendix Table A4 in the Appendix), thus indicating that the reduction in the endline estimation sample size is unlikely to threaten the internal validity of the experiment.

Participation in the financial education session is relatively high with about 70 percent of the invited individuals in treated clusters attending the session. Additionally, attendance is not differential by age (see Appendix Table A5) and there is no possibility of two-sided noncompliance because the course was only offered in treated clusters and our sample does not include untreated individuals in treated clusters.

Sample descriptive statistics at baseline for the endline estimation sample (i.e., post attrition) in Panel B of Table 1 and additional balance tests with more observables as well as the full baseline sample indicate randomization balance (see Appendix Tables A6 and A7). About 60 percent of the individuals are female, age is on average 34 years with a standard deviation of 12, and the age distributions are very similar in treatment and control (see Appendix Figure A2). Only a small part of about 12 percent received tertiary education. We measure domain-specific (i.e., financial) numeracy using two simple items about compound interest and inflation (e.g., Cole et al., 2011; Lusardi and Mitchell, 2014), with an average score of 0.9. The average value of self-reported patience (Falk et al., 2022) is 5.9 on a scale from 1 (not patient at all) to 10 (very patient). Monthly household consumption is about 500,000 UGX, i.e., about 404 USD in 2019 PPP, and thus somewhat above the poverty line (households have on average four members). The stock of savings is 700,000 UGX. Tests for the differences in means shown in column (3) indicate balance on observables at baseline and a test of joint orthogonality indicates that randomization appears to have worked as planned.

3 Results of the field experiment

3.1 Correlates of experimental patience measures

In a study covering representative samples from 76 countries, Falk et al. (2018) generate stylized facts about relations between preferences, among them patience, and other variables. On the one hand, measures of patience are correlated with two outcomes, that is more savings and higher degree of education. Both relations are intuitive as they can be regarded as shifting consumption into the future. On the other hand, measures of patience are positively correlated with individual characteristics. There are three such characteristics, i.e., being male (where the evidence is not entirely conclusive), being older, and having higher cognitive ability (proxied by self-assessed mathematical ability in Falk et al., 2018). Evidence on a positive correlation between patience and cognitive ability is also presented in Stango and Zinman (2023) and Chapman et al. (2023). We test these relations to ensure that the elicited preferences have adequate external validity and follow Alan and Ertac (2018) who run such tests on the control group at endline.

Results in Table 2 show the five empirical relationships for our two measures of impatience, i.e., using the share of the budget allocated to the respective sooner payment dates and a binary measure whenever the entire budget was allocated to the earliest possible payment.

We start in Panel A with the two outcome variables; results mirror earlier findings on savings (columns 1 and 4). By contrast, the relation between impatience and tertiary education has the expected sign but is estimated with a large standard error (columns 2 and 5). This latter result may indicate that education decisions in rural Uganda are driven by other determinants, such as having a funding source, while patience may not play a major role. Finally, we show in columns (3) and (6) the relation between our measures of elicited impatience and a self-reported measure of patience: coefficients are negative, and they are statistically significant (e.g., Falk et al., 2022), suggesting external validity of the experimental measures.

< Table 2 about here >

Turning to the three demographic predictors of impatience (see Panel B of Table 2), the coefficients on the female variable in columns 1 and 4 have the expected sign but are estimated with large standard errors that may reflect the uncertain relation according to Falk et al. (2018). However, the other coefficients for age and the measure of numeracy have the expected signs and are estimated with small standard errors (e.g., in line with evidence in Falk et al., 2018; Chapman et al., 2023; Stango and Zinman, 2023). Overall, the correlations in our sample are largely in line with stylized facts (see also Shamosh and Gray, 2008; Hanushek et al., 2022) and indicate that the experimental measures of impatience appear to have adequate external validity.

3.2 Treatment effects on experimental patience measures

In this section, we first present evidence on allocation patterns and then move to a discussion of average treatment effects on structural parameters. We report treatment effects and standard errors clustered at the village-level (in parentheses) as well as sharpened q-values, correcting for the expected proportion of false rejections of the null hypothesis (i.e., the false discovery rate) in brackets. Additionally, we report P-values from Fisherian randomization inference (Permutation p-value) (Young 2019).

Panel A of Table 3 shows the treatment effects on our main measures of impatience, i.e., the share of the respective budget allocated to sooner payment dates (column 1) and the binary indicator of impatient choice (i.e., allocating the entire budget to the earliest possible payment date) (column 2). Results indicate that the treatment group, on average, does not show differences in allocation patterns suggesting that the treatment does not affect impatience in the full sample.

< Table 3 about here >

To gain additional insight into differences in intertemporal choices, we investigate whether treated participants respond differently to changes in the front-end delay (t), the delay between payments ($t + k$), and the gross interest rate ($1 + r$) within the CTB task. For this purpose, we run regressions of the share of allocations to sooner payments and the binary measure of choosing the sooner payment option (i.e., measures for impatience) on dummies for whether the soon payment is today ($t = 0$) instead of in one month, whether the delay between payment dates is five months ($k = 150\ days$) instead of one month, whether the interest rate is 20 percent (i.e., $1 + r = 1.2$) instead of 11 percent, and the interaction terms between the treatment dummy and the respective variables.

Results in Panel A of Table 3 show that allocations to sooner payments are sensitive to changes in the CTB parameters in the expected way, suggesting internal validity of the elicitation design. Extending the delay between payment dates to five months and changing the front-end delay to “today” corresponds with a higher tendency to allocate payoffs to sooner dates, whereas changing the interest rate to 20 percent is associated with allocations to later payments (columns 1 and 2). We find no interaction effects between the experimental CTB variables and the treatment dummy suggesting that treated individuals do not respond differently to changes in these experimental variables.

In Panel B (columns 1 and 2), we estimate average treatment effects on estimated intertemporal utility parameters at the individual-level, i.e., present bias parameters $\widehat{\beta_i}$ and discount factors $\widehat{\delta_i}$. Again, we do not observe average treatment effects on these parameters.

Next, we investigate heterogeneity in treatment effects. Inspired by theory (e.g., Cunha and Heckman, 2007), we hypothesize that treatment effects may be conditional on respondents' age. To explore this hypothesis, we split the sample at the age of 24 and younger (i.e., those who still may be considered as "youth" in Uganda as in Horn et al., 2023), and run regressions on the same outcomes as in columns (1) and (2) of Table 3 (Panels A and B). Our analysis, in columns 3 and 4 of Table 3, reveals that treatment effects may indeed be conditional on subject age. We find heterogeneous treatment effects on both the share of the respective budget allocated to sooner payment (column 3 of Panel A in Table 3) as well as the binary measure of impatient choice (column 4 of Panel A in Table 3) among younger individuals. Treated participants aged 24 and younger allocate 14.6 percentage points less of their entire budget to sooner payment dates (relative to a control mean of 77 percent) (column 3) and have a reduced probability of allocating their entire budget to the earliest possible date by 17.2 percentage points relative to a control mean of 81.45 percent of impatient choices (column 4). We note that these results come with the other coefficients on the experimental design variables within the CTB being unchanged relative to the full sample. We also repeat this analysis for the single budgets (Figure B1 in Appendix B) and find differential effects for the single choices as well (budget 1 and 2).

Because the cut-off of 24 years may be considered arbitrary, we show in comprehensive exercises that the finding of heterogenous treatment effects does not depend on the exact cut-off. Figure 1 shows average treatment effects with sample compositions including different ages: We start at ages 16-20 and then extend the sample to include more individuals aged up to four years older. We find that the treatment effects are largest in the youngest age bracket, and we cannot reject zero treatment effects once we include individuals 28 and older. Related analyses in the appendix confirm the pattern shown in Figure 1 (i.e., plotting the predicted values of a continuous age and treatment interaction in Appendix Figure B2).

< Figure 1 about here >

3.3 Treatment effects on structural parameters

In line with the findings outline in Section 3.2, we also observe heterogenous treatment effects on estimated individual utility parameters (columns 3 and 4 of Panel B in Table 3). While we do not find treatment effects on time-inconsistent behaviors (i.e., present bias) (column 4), treated younger individuals appear to exhibit significantly larger individual discount factors (column 3). All results are robust to relying on permutation p-values and to addressing the issue of multiple hypothesis testing; we adjust inference by employing false discovery rate corrections (see q-values in brackets in Table 3).

3.4 Treatment effects on field saving behavior

Since the experimental patience measures are correlated with field saving behavior in the control group, we next test whether the educational intervention also affects saving behavior in the field. Again, we find evidence for heterogenous treatment effects by age. The treatment causes an increase in savings both at the extensive and intensive margin only for the young (see the middle panel of Table 4). At the extensive margin, the treatment increases self-reported saving by ten percentage points (relative of a control mean of about 82 percent), i.e., an effect of about 12 percent relative to the control group (column 4). At the intensive margin the effect amounts to about 13 percent relative to control (columns 5 and 6). For respondents aged 25 and older, as well as for the total sample, we find zero effects throughout.

< Table 4 about here >

3.5 Interpretation of field experimental results

The finding of relatively strong treatment effects on patient experimental choices and estimated discount factors raises the question whether the treatment effects on patience reflect changes in deep parameters or whether they are limited to intra-experimental behavior. First, as shown above, we not only observe more patient choices within the experimental elicitation task but also on (self-reported) field saving behavior. Yet, despite this strong evidence, we discuss alternative interpretations of the treatment effects in the following and show that a change in general patience may be the most plausible explanation for the treatment effects on the young realized by this intervention.

Choice bracketing and intertemporal arbitrage. One potential explanation for the treatment effects on intertemporal choice may be that the financial education intervention enables individuals to a) better understand the intertemporal tradeoffs, and b) induces them to bracket choices less narrowly, i.e., by taking extra experimental conditions such as market interest rates or inflation into account. For example, by learning about interest and inflation, treated individuals may compare the intra experimental gross interest rates to rescheduling opportunities outside the lab in the field task: when outside credit is available at a lower cost than the gross interest rate offered within the task, one would be better off delaying the payment to a later date and financing immediate consumption through the external credit market. Vice versa, when a savings-product yields higher returns than the gross interest rate offered within the experiment, taking the sooner payment, and using the extra experimental opportunities will be optimal.

Subjects' experimental choices will only reveal their individual discount factors when they are completely liquidity constrained (Dean and Sautmann, 2021) or bracket choices narrowly. Evidence for a decrease in narrow bracketing in response to financial education treatment has been documented in Lührmann et al. (2018) and we test the resulting choice patterns associated with this phenomenon in our data. First, for such an interpretation to be plausible, one needs to observe an increase in financial numeracy (i.e., knowledge about compound interest and inflation) among treated individuals. Thus, we check for treatment effects on the financial numeracy items included in the survey (see Appendix Table B1). We find no evidence on financial numeracy at any age.

Next, if participants engage in intertemporal arbitrage, they should exhibit the following choice patterns more frequently: they should be more likely to make consistent choices, to allocate the entire budget to a single payment date (i.e., corner choices), and allocate the entire budget to the earlier payment date at low interest rates and then switch to allocating the entire budget to the later payment date at higher interest rates (i.e., interest-sensitive corner choices). We find no evidence for treatment effects on choice consistency (Appendix Table A4), the occurrence of corner choices (Appendix Table B2, columns 1 to 3), and interest-sensitive corner choices (Table B2, columns 4 to 6).

Liquidity. We also find no average or differential treatment effects on borrowing at the extensive and intensive margins, income (proxied via firm profits), and the likelihood to use mobile money as a means of payment (see Appendix Table B3). Thus, we conclude that financial education is unlikely to reduce narrow bracketing (i.e., a cognitive mechanism) or to increase liquidity allowing to treat the experimental payoffs differently to those individuals in the control group.

Trust. As a final alternative mechanism, we discuss the role of trust in receiving the payments as a driver of more patience among the young in the treatment group. As mentioned in Section 2.2, the respondents were familiar with the survey and 97 percent of the sample stated to have trust in receiving the payments. Yet, we rerun the analysis by excluding those 3 percent of the sample who stated to have no trust in receiving the experimental payments (Appendix Table B4). The results are near identical to those including the full sample.

Overall, our results most likely indicate a shift in time preferences (i.e., discount factors) leading individuals to make more patient choices within the experimental task and to increasing their field saving behavior.

4 How generalizable are the heterogenous treatment effects? A meta-analysis

The finding of heterogenous treatment effects in our field context raises the question of the generalizability of this finding. Meta-analyses are well suited to address this question of generalizability and external validity of findings. We rely on a partial pooling (or "random effects") model and jointly estimate both the mean of the distribution of true effects across studies and the heterogeneity in the literature (see also Meager, 2019; Bandiera et al., 2021; Kaiser et al., 2022, Angrist and Meager, 2023, for recent economics meta-analyses relying on these types of models in Bayesian and/or frequentist frameworks). We then incorporate further contextual information about the individuals and study settings into the model to study the generalizability of heterogenous treatment effects by age and other features.

4.1 Literature search and inclusion criteria for prior studies

We compile a complete set of field experiments studying the causal effects of educational interventions on time preferences. We search the *Web of Science*, *EconLit*, and *RePEC* for papers including the key words "time preference" and/or "patience" and "intervention." We only include studies that, first, estimate the causal (intention to treat) effect of an educational intervention by means of a (cluster-) randomized control trial and, second, measure the outcome of interest, i.e., patience, in an incentivized time preference elicitation task. Thus, we exclude studies estimating treatment effects of interventions on self-reported patience or patience elicited in hypothetical choice experiments without incentives.

We focus on educational interventions explicitly designed to affect financial decision-making capabilities and inducing a more future-oriented mindset. Thus, we exclude a study reporting a treatment effect on time-preferences in a sub-sample of respondents included in an early education field experiment studying free pre-school access for disadvantaged children in Chicago (Andreoni et al., 2017) and an accompanying parenting program (Castillo et al., 2020).

4.2 Sample of studies

Applying these inclusion criteria, we arrive at a sample of ten ealier field experiments studying the effects of educational interventions on intertemporal decision-making. While the included studies cover a variety of outcomes related to (the quality of) intertemporal decision-making (e.g., Lührmann et al., 2018), the only common outcome variables are measures of impatience (i.e., measures of allocations to earlier payment dates within the experimental task). Together with the treatment effects reported in Section 3, we extract a total of 45 treatment effects on this type of outcome and convert the reported treatment effect estimates into scale-free standard deviation units, i.e., bias corrected standardized mean differences, Hedges' g. Studies are heterogenous regarding the age-groups covered, the delay between treatment and preference elicitation (mean of 11 months, SD of 13), the content and intensity of the intervention (mean of 13 hours, SD of 10), the methodical details of the time preference elicitation task (60 percent of estimates are based on Convex Time Budgeting Tasks), as well as the country settings (eight countries with average patience at the country-level of 0.03 sigma, SD of 0.26). We briefly summarize the individual studies, organized by the age of their participants (details about these studies are documented in Table D1 in Appendix D). Then we move to a discussion of aggregate treatment effects, i.e., the mean of a distribution of true effects, estimated heterogeneity in true effects, and observable characteristics explaining heterogeneity in treatment effects across studies.

Three studies examine elementary school children: Migheli and Moscarola (2017) study a low-intensity intervention (1 hour) aimed at fostering saving behavior of children in a lab-setting in Italy. They find no effects on impatience, but estimates come with substantial uncertainty. Alan and Ertac (2018) study the causal effect of a financial literacy and patience treatment (with about 16 hours of total classroom exposure) on incentivized measures of patience in Turkey. They find large and persistent effects on patience almost three years after the intervention. Breitkopf et al. (2022) study a 14-hour educational intervention in Bangladesh. They implement the same intervention in multiple grades to study heterogeneity in treatment effects by elementary school grades (2 to 5) about three months after treatment. They report treatment effects on patience and find evidence for treatment effects in grade 2 on incentivized tasks but no evidence of effects in grades 3 to 5.

Three studies are conducted among secondary school students aged 14 to 16: Lührmann et al. (2018) analyze a 4.5-hour financial education program for adolescents in Germany and measure a range of outcomes related to intertemporal choice. While they find no evidence that the treatment affects the degree of patience four to 12 weeks after treatment, students make more time-consistent choices and appear to exhibit decreased narrow bracketing in the experimental task. Similarly, Bover et al. (2018) study a 10-hour financial education intervention with 15-year-olds in Spain. While some empirical specifications suggest more patient intertemporal choice in an incentivized task three months after treatment, the aggregate effect is not statistically significant at conventional levels. Sutter et al. (2023) analyze the effect of an 8-hour financial education program on a sample of 16-year-old students in Germany. The treatment does not affect patience but appears to affect risk-taking in an incentivized task.

Finally, five studies focus on young and middle-aged adults adults: Bjorvatn et al. (2020) examine the effect of an edutainment treatment for 18-year old individuals focused on entrepreneurship and financial management delivered in eleven weekly episodes via television in Tanzania. They do not find effects on intertemporal choice in a short-term follow-up conducted three weeks after the treatment was completed. Horn et al. (2023) study the effects of a 15-hour financial education intervention offered to members of Ugandan youth clubs (aged 24). They find no effects on patience during one-year and five-year follow-ups. Blattmann et al. (2017) study a different type of treatment: The program is based on a cognitive behavioral therapy for criminal young men in Liberia (aged 21) and find no effects on patience measured in incentivized tasks one year after treatment (but on self-reported patience). The curriculum differs from the other programs in its therapeutic approach but also contains components on future orientation, planning, goal setting, money management and saving. Berge et al. (2015) analyze the effect of a business and financial education program delivered in 21 sessions in Tanzania and conduct behavioral experiments with a subsample of respondents about two months after treatment. The respondents are for 37 years old, on average. The results suggest relatively large effects on the patience of women but zero effects on men in their sample. Finally, our experiment described in Sections 2 and 3 studied a 4-6-hour financial education program for both youth and adult entrepreneurs in rural Uganda. As stated in the earlier sections, we find large effects on patience for the young in our setting (average age of 21) and zero effects in the adult sample (average of 36) about 15 months after treatment.

4.3 Meta-analysis model

After extracting the set of (i) estimated average treatment effects and standard errors from each randomized experiment (j) reported above, we estimate the average of the average treatment effect across studies (θ). We rely on a partial pooling (or "random effects") model and jointly estimate both the mean of the distribution of true effects and the heterogeneity in the literature. Formally, the model can be written as:

$$\hat{\theta}_{ij} = \theta + v_j + \epsilon_{ij} \tag{2}$$

with $v_j \sim N(0, \tau^2)$ and $\epsilon_{ij} \sim N(0, \sigma_{ij}^2)$. τ^2 is the between-study variance in true effects that is unknown and must be estimated from the sample of treatment effect estimates. We estimate $\hat{\tau}^2$ relying on restricted maximum likelihood. σ_{ij} is the within-study standard error of the ith treatment effect estimate $\hat{\theta}_{ij}$. While σ_{ij} is unknown, the model treats the estimated standard errors of the extracted treatment effect estimates $\widehat{\sigma_{ij}}$ as known (i.e., $\sigma_{ij} = \widehat{\sigma_{ij}}$). Subsequently, weighted least squares is used to estimate θ with inverse variance weights defined as $w_{ij} = (\hat{\tau}^2 + \hat{\sigma}_{ij}^2)^{-1}$. While this model assumes heterogeneity in true effects, it also nests the case of the common effect model. In case of no heterogeneity in true effects (i.e., $\tau^2 = 0$), the model reduces to $\hat{\theta}_{ij} = \theta + \epsilon_{ij}$ and the weights are then defined as $w_{ij} = (\hat{\sigma}_{ij}^2)^{-1}$.

As a next step, we extend the model defined in (2) to include effect size-level covariates but still allow for residual heterogeneity in true effects. The model is defined as:

$$\hat{\theta}_{ij} = \beta x_{ij} + v_j + \epsilon_{ij} \tag{3}$$

where x_{ij} is a vector of observable characteristics report within studies and β is a vector of the corresponding coefficients. We estimate the above model including different study-level observables in the vector x_{ij} and test for changes in the estimated (residual) heterogeneity in true effects to assess whether the considered study-level covariates may explain heterogeneity in true effects across contexts. As theory predicts the age of students to be a potentially important mediator of treatment effects (Cunha and Heckman, 2007), we incorporate this analysis and check additional variables, such as delay between treatment and time-preference elicitation, intensity of instruction, methodical details of the time preference elicitation as well as country setting in further analyses.

4.4 Results of the meta-analysis

4.4.1 Meta-estimate of average effects and heterogeneity

Column 1 of Table 5 shows the result from the basic meta-analysis model (Eq. 2). In line with the aspirations of these interventions, results indicate negative treatment effects on *impatience* as the estimated mean of the distribution of true effects is negative (-0.056 SD), but the 95 percent confidence interval cannot rule out zero treatment effects. The estimated absolute value of $\hat{\tau}^2$ amounts to about 0.01. Given the relatively small number of diverse studies, it is no surprise that heterogeneity statistics (I^2 of about 66%) indicate non-trivial heterogeneity in true effects. This suggests that treatment effects may depend on contextual features of the sites and/or features of the target groups (see also Meager, 2019).

< Table 5 about here >

4.4.2 Explaining between-study heterogeneity in treatment effects

Thus, we incorporate the study-level observables as potentially important mediators of the treatment effects and estimate the model described in Eq. (3). We first add the average age of students associated with the treatment effect estimate and then move on to include details of the intervention, the elicitation task, and the country setting in blocks. Estimating the specification results in statistically significant linear effect of age in the hypothesized directions, i.e., treatment effects on impatience decrease with increasing age rate (see Table 5, column 2).

Once the age of the students is accounted for, the residual heterogeneity drops by 6.38 percentage points, i.e., a reduction of about ten percent. Next, we model treatment effects as a function of age and delay between treatment and elicitation task (column 3). This covariate appears to be a significant predictor of treatment effects but does not reduce the estimated residual heterogeneity in treatment effects. Further covariates, i.e., the intensity of treatment (column 4), details about the time preference elicitation task (column 5) as well as the country setting (column 6) do not appear to be significant predictors but the full model (column 6) results in virtually no residual heterogeneity (see column 6) as the p-value of a test statistic for residual homogeneity is 0.183, so the null hypothesis of no residual heterogeneity cannot be rejected.

As there is uncertainty in model selection and one may be concerned about fitting too many study-level covariates relative to few observations, we run a LASSO model to select the covariates most predictive of treatment effects. The model selects the age of participants, the delay between treatment and elicitation task, whether preferences were elicited using CTBs, the number of choices in the task, and whether studies are in developing countries as important predictors. We run this model and then use the predicted values to illustrate the external validity of the heterogenous treatment effects regarding age.

Figure 2 shows the predicted treatment effects based on the earlier experiments for target groups aged 5 to 40. Treatment effects are largest for elementary school children (about 0.18 SD reduction in impatience) and decline with increasing age. One cannot reject zero effects for respondents aged 20 to 30. Thus, the results are in line with the heterogeneous treatment effects we have uncovered in our setting. Regarding fade out, our model estimates that effects are largest right after the intervention and fade out after about 20 months.

< Figure 2 about here >

4.5 Auxiliary results and sensitivity analyses

We briefly report auxiliary results and sensitivity analyses while full results are provided in Appendix E. We assess the robustness of the meta-study results (i) by considering alternative models including robust variance estimation with dependent effect sizes as well as common-effects models using unrestricted weighted least squares; (ii) by conducting "leave-one-out meta-analysis;" and (iii) by testing and accounting for publication bias in the literature. All sensitivity analyses suggest that the results are insensitive to changes in estimation methods and/or assumptions.

5 Conclusion

Our study contributes to an emerging literature on the malleability of preferences through educational interventions. We provide a first formal investigation of treatment effects on participants of very heterogeneous age in a field experiment and a novel systematic account of previous results on time preferences in a meta-analysis. Both, the results of the field experiment and the meta-analysis, suggest that patience may be malleable only for relatively young individuals. This heterogeneity in treatment effects motivates future research to better understand for whom and with which kinds of interventions preferences may be susceptible to change.

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Table 1: Descriptive information about the field experiment

Panel A: Time preference elicitation design

Budget	Sooner endowment (UGX)	Interior choice (split endowments) (UGX)	Later endowment (UGX)	t	$t+k$	$1+r$
1	5,400 | 0	2,700 | 3,000	0 | 6,000	0	1	1.11
2	5,400 | 0	2,700 | 3,000	0 | 6,000	1	2	1.11
3	5,000 | 0	2,500 | 3,000	0 | 6,000	1	2	1.20
4	5,000 | 0	2,500 | 3,000	0 | 6,000	1	6	1.20

Panel B: Descriptive statistics and randomization balance at baseline

Variable	Control (*N=629*)	Treatment (*N=588*)	Diff. (p-value)
Female	0.622	0.599	0.657
Age	33.781 (11.162)	34.766 (12.49)	0.365
Tertiary education	0.108	0.134	0.406
Household size	4.024 (2.508)	4.146 (2.643)	0.651
Monthly consumption (UGX)	493,871 (341,309)	503,600 (335,361)	0.797
Total savings (UGX)	701,549 (1620,014)	709,717 (1487,041)	0.756
Total investments (UGX)	1413,484 (2874,804)	1626,736 (3181,338)	0.585
Patience (self-reported)	5.901 (2.637)	5.997 (2.645)	0.470
Financial numeracy	0.898 (0.783)	0.920 (0.806)	0.775

Notes: Panel A lists parameters of four intertemporal budgets used to elicit respondents' patience. Each budget contains one interior choice. In Budget 1, participants decide between a payment today ($t = 0$) or in one month (k) with an interest rate (r) of 11 percent. In Budget 2, participants decide between a payment in one month ($t = 1$) or in two months ($t + k = 2$), with the same interest rate as in Budget 1. Budget 3 has the same delay and payment dates but raises the interest rate to 20 percent. The interest rate and earlier payment date ("in one month") in budget 4 remain the same as in budget 3, but the later payment date ($t + k$) is "in six months." One of the budgets is randomly chosen for payout and payments are made into the subjects' mobile money account. As detailed in Section 3.2 participants receive an additional UGX 1,000 (UGX 500 sooner and UGX 500 later) regardless of their choices to equate transaction costs across time periods. Panel B reports means and standard deviations (in parenthesis) of individual characteristics at baseline for the endline estimation sample by treatment and control. The third column displays p-values (unadjusted for multiple hypothesis testing) testing equality of means across experimental arms, with standard errors clustered at the village level (n=108) for inference.

Table 2: Correlates of experimental patience measures

Panel A: Impatience and field behavior						
	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Savings)	Tertiary education (1/0)	Self-reported patience	ln(Savings)	Tertiary education (1/0)	Self-reported patience
Allocation to sooner payment (share)	-0.784*	-0.006	-0.212**			
	(0.448)	(0.031)	(0.080)			
Impatient choice (binary)				-0.732**	-0.005	-0.176***
				(0.352)	(0.024)	(0.063)
Constant	10.174***	0.087**	-0.339	10.139***	0.087**	-0.363
	(1.043)	(0.035)	(0.236)	(1.041)	(0.034)	(0.237)
R^2	0.043	0.027	0.037	0.045	0.027	0.038
N (budget choices)	2,516	2,516	2,516	2,516	2,516	2,516
N (individuals)	629	629	629	629	629	629
Clusters (villages)	54	54	54	54	54	54

Panel B: Demographic correlates of impatience						
	Allocation to sooner payment (share)			Impatient choice (binary)		
	(1)	(2)	(3)	(4)	(5)	(6)
Female (1/0)	-0.015			-0.011		
	(0.025)			(0.030)		
Age (years)		-0.002*			-0.003*	
		(0.001)			(0.002)	
Fin. Numeracy (z-score)			-0.026*			-0.033*
			(0.015)			(0.020)
Constant	0.650***	0.719***	0.617***	0.644***	0.738***	0.610***
	(0.029)	(0.060)	(0.023)	(0.070)	(0.098)	(0.066)
R^2	0.029	0.033	0.034	0.023	0.026	0.028
N (budget choices)	2,516	2,500	2,516	2,516	2,500	2,516
N (individuals)	629	625	629	629	625	629
Clusters (villages)	54	54	54	54	54	54

Notes: Panel A shows relationships between *impatience measures* in the control group, i.e., the proportion of the budget allocated to sooner payment date and a dummy for whether the respondent chose to allocate the entire budget to the earliest possible date, and the (1) log of total *savings* (winsorized at the 99^{th} percentile), (2) a dummy for whether the respondent received *tertiary education*, and (3) z-scores of *self-reported patience* on a scale from 0 (totally impatient) to 10 (totally patient). Panel B shows demographic correlates (*age*, *gender*, and *numeracy*) with both impatience measures as dependent variable. *Financial Numeracy* scores are based on responses from items asking respondents to conduct simple calculations on compound interest and inflation. All regressions show unstandardized coefficients and include stratification fixed effects. Regressions with binary dependent variables are based on linear probability models. Standard errors are clustered at the individual and village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Treatment effects on patience and utility parameters

	Average treatment effects (full sample)		Heterogeneous treatment effects (≤ 24 years of age)		Heterogeneous treatment effects (> 24 years of age)	
Panel A: Treatment effects on allocation behaviors						
	(1) Allocation to sooner payment (share)	(2) Impatient Choice (binary)	(3) Allocation to sooner payment (share)	(4) Impatient Choice (binary)	(5) Allocation to sooner payment (share)	(6) Impatient Choice (binary)
Treatment	-0.016	-0.023	-0.146***	-0.172***	0.013	0.009
	(0.024)	(0.032)	(0.045)	(0.058)	(0.027)	(0.035)
	[0.329]	[0.329]	[0.017]	[0.017]	[0.622]	[0.622]
Today ($t = 0\ days$)	0.097***	0.126***	0.068***	0.089***	0.104***	0.133***
	(0.014)	(0.017)	(0.018)	(0.025)	(0.016)	(0.020)
Delay ($k = 150$ days)	0.109***	0.129***	0.064**	0.063*	0.119***	0.143***
	(0.013)	(0.016)	(0.029)	(0.035)	(0.013)	(0.017)
Interest rate ($1 + r = 1.2$)	-0.057***	-0.062***	-0.052***	-0.054***	-0.057***	-0.064***
	(0.007)	(0.009)	(0.016)	(0.020)	(0.008)	(0.011)
Treatment × Today	-0.015	-0.013	0.006	-0.007	-0.020	-0.015
	(0.017)	(0.022)	(0.027)	(0.038)	(0.019)	(0.025)
Treatment × Delay	0.021	0.033	0.089*	0.092	0.006	0.020
	(0.020)	(0.028)	(0.047)	(0.058)	(0.020)	(0.028)
Treatment × Interest rate	0.014	0.014	0.029	0.033	0.012	0.011
	(0.010)	(0.012)	(0.021)	(0.025)	(0.011)	(0.014)
Permutation p-value	0.325	0.454	0.005	0.006	0.616	0.779
Control mean	0.687	0.710	0.770	0.815	0.669	0.687
Standardized effect size	-0.045	-0.051	-0.505	-0.442	0.036	0.019
R^2	0.042	0.039	0.102	0.104	0.048	0.045
N (budget choices)	4,868	4,868	836	836	4,032	4,032
N (individuals)	1,217	1,217	209	209	1,008	1,008
Clusters (villages)	108	108	81	81	107	107
Panel B: Treatment effects on individual utility parameters						
	Discount factor $\widehat{\delta_\iota}$	Present bias $\widehat{\beta_\iota}$	Discount factor $\widehat{\delta_\iota}$	Present bias $\widehat{\beta_\iota}$	Discount factor $\widehat{\delta_\iota}$	Present bias $\widehat{\beta_\iota}$
Treatment	0.016	-0.007	0.077***	-0.022	0.004	-0.005
	(0.014)	(0.004)	(0.028)	(0.021)	(0.015)	(0.005)
	[0.313]	[0.175]	[0.017]	[0.313]	[0.789]	[0.510]
Permutation p-value	0.334	0.234	0.042	0.229	0.768	0.402
Control mean	1.063	0.993	1.030	0.999	1.064	0.995
Standardized effect size	0.079	-0.189	0.520	-0.275	0.020	-0.119
R^2	0.013	0.020	0.091	0.109	0.023	0.026
N (individuals)	1,055	1,055	186	186	869	869
Clusters (villages)	108	108	78	78	106	106

Notes: Panel A dependent variables are the proportion of the respective budget allocated to sooner payment date (columns 1 and 3) and a dummy that takes the value 1 if participants choose to allocate the entire budget to the earliest possible date (columns 2 and 4). Panel B shows average and heterogenous treatment effects on estimated individual intertemporal utility parameters, i.e., discount factors $\widehat{\delta_\iota}$ (columns 1 and 3) and present bias parameters $\widehat{\beta_\iota}$ (columns 2 and 4). Utility parameters are estimated via non-linear least squares regressions. All regressions included stratification fixed effects. Standard errors (in parentheses) are clustered at the individual and village level (Panel A) and the village level (Panel B), respectively. P-values from Fisherian randomization inference (Permutation p-value) based on the method described in Heß (2017). Sharpened q-values (Benjamini et al. correcting for the expected proportion of false rejections of the null hypothesis (false discovery rate) in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Treatment effects on field saving behavior

	Average treatment effects (full sample)			Heterogeneous treatment effects (≤24 years of age)			Heterogeneous treatment effects (>24 years of age)		
	(1) Any	(2) log	(3) IHST	(4) Any	(5) log	(6) IHST	(7) Any	(8) log	(9) IHST
Treatment	0.010	0.132	0.133	0.102**	1.370*	1.432*	-0.008	-0.070	-0.078
	(0.021)	(0.298)	(0.313)	(0.051)	(0.744)	(0.780)	(0.021)	(0.300)	(0.315)
Control mean	0.843	10.781	11.354	0.821	10.392	10.950	0.847	10.865	11.442
Std. effect	0.027	0.026	0.025	0.266	0.256	0.255	-0.022	-0.014	-0.015
R^2	0.058	0.076	0.075	0.183	0.207	0.207	0.064	0.079	0.079
Observations	1,217	1,217	1,217	209	209	209	1,008	1,008	1,008
Clusters	108	108	108	81	81	81	107	107	107

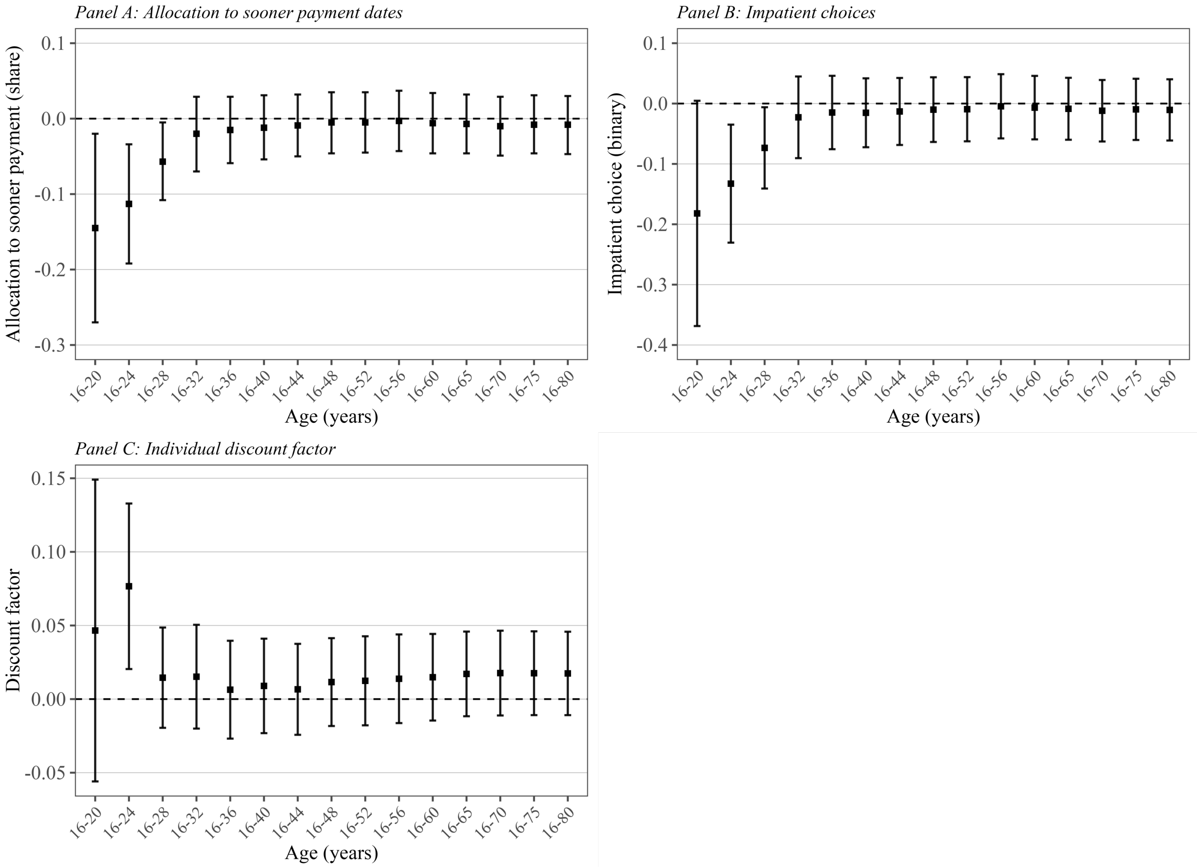
Notes: All regressions included the lagged outcome at baseline and stratification fixed effects. Standard errors (in parentheses) are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Mixed effects meta-regression models

	(1)	(2)	(3)	(4)	(5)	(6)
Age		0.007*	0.007*	0.008*	0.008**	0.019***
		(0.004)	(0.004)	(0.004)	(0.004)	(0.006)
Delay			0.002*	0.002**	0.003**	0.003**
			(0.001)	(0.001)	(0.001)	(0.001)
Intensity				-0.0002	0.001	-0.011
				(0.0025)	(0.003)	(0.024)
Convex Time Budget = 1					0.009	-0.173
					(0.068)	(0.024)
No. of choices					0.010	-0.021
					(0.007)	(0.024)
Developing country = 1						0.000
						(0.254)
Patience (GPS)						0.754
						(0.557)
Meta estimate ($\hat{\theta}$)	-0.056*	-0.190**	-0.204**	-0.218**	-0.259***	-0.364*
	(0.036)	(0.080)	(0.083)	(0.089)	(0.120)	(0.212)
$\hat{\tau}^2$	0.010	0.012	0.017	0.017	0.015	0.048
$\tau^2 = 0$ (p-value)	<0.001	<0.001	<0.001	<0.001	<0.001	0.183
I^2	65.77%	59.39%	60.33%	61.24%	39.91%	22.64%
n (Studies)	11	11	11	11	11	10
N (Treatment effects)	45	45	45	45	45	44

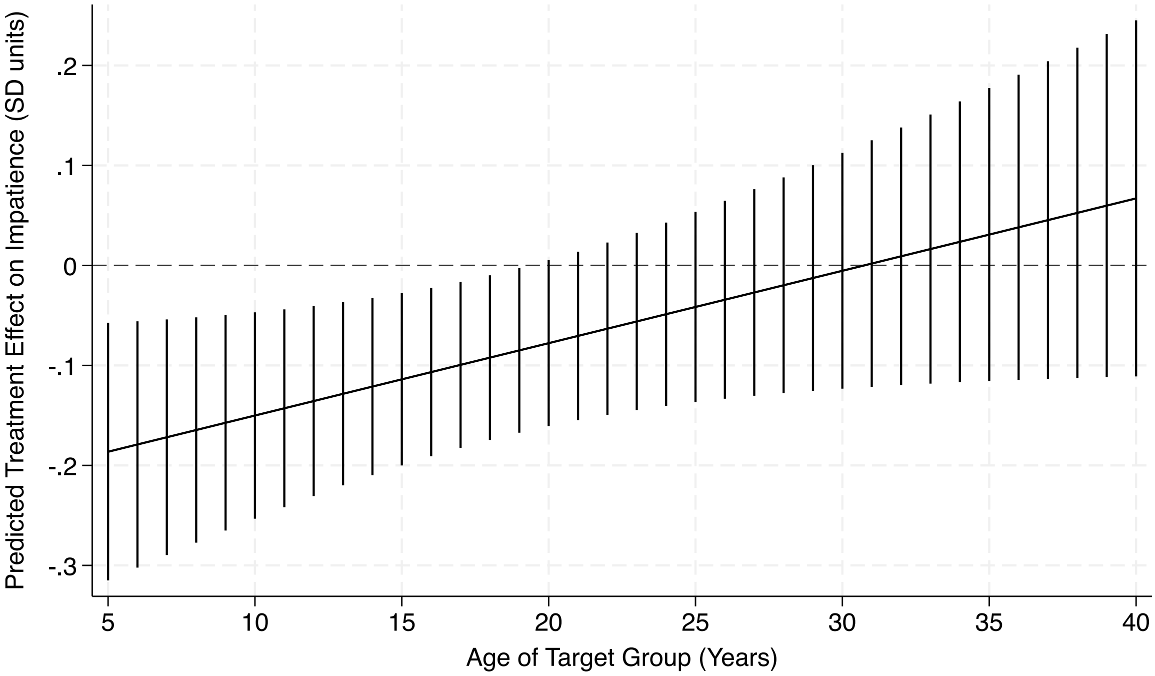
Notes: This table shows results from meta-regression analyses relying on the model defined in Eq. 3. The dependent variable is the standardized treatment effect reported within the studies. *Age* is non-centered and describes the student age in years. *Delay* is non-centered and is defined as the time between treatment and time-preference elicitation in months. *Intensity* is mean-centered and is defined as the intensity of classroom instruction in hours. *Convex Time Budget* is a binary variable indicating whether time preferences where elicited using convex budgets. *No. of choices* is mean-centered and refers to the total number of intertemporal decisions within the elicitation task. *Developing country* is a binary variably indicating whether the country setting is a developing country instead of a high-income country. *Patience (GPS)* is mean-centered and is defined as the average patience (z-score) at the country-level based on the Global Preference Survey (GPS) by Falk et al. 2018. $\hat{\theta}$ indicates the estimated mean of the distribution of true effects of educational intervention on impatience. $\hat{\tau}^2$ is the estimated heterogeneity parameter and I^2 indicates the estimated proportion of between study variance in treatment effects which can be attributed to heterogeneity in true effects rather than within study measurement error. *** p<0.01, ** p<0.05, * p<0.1.

Figure 1: Heterogenous treatment effects by age in the field experiment



Notes: This figure shows treatment effects with 95% CIs for different sample compositions with regard to age. Confidence intervals are based on standard errors clustered at the village and individual level. Dependent variables are the share of budget allocated to the sooner payment date (Panel A), a dummy that takes the value 1 if participants choose to allocate the entire budget to the earliest possible date (Panel B), and estimated individual discount factors based on the structural model described in section 2.3 (Panel C).

Figure 2: Predicted treatment effects by age from the meta-analysis model



Notes: This figure shows effect sizes of financial education interventions as a function of age with 95% Cis. Estimates are based on earlier experiments for target groups aged 7 to 37. The dependent variable "impatience" is standardized to have a mean of 0 and a standard deviation of 1 in the control groups.

ONLINE-APPENDIX

to accompany

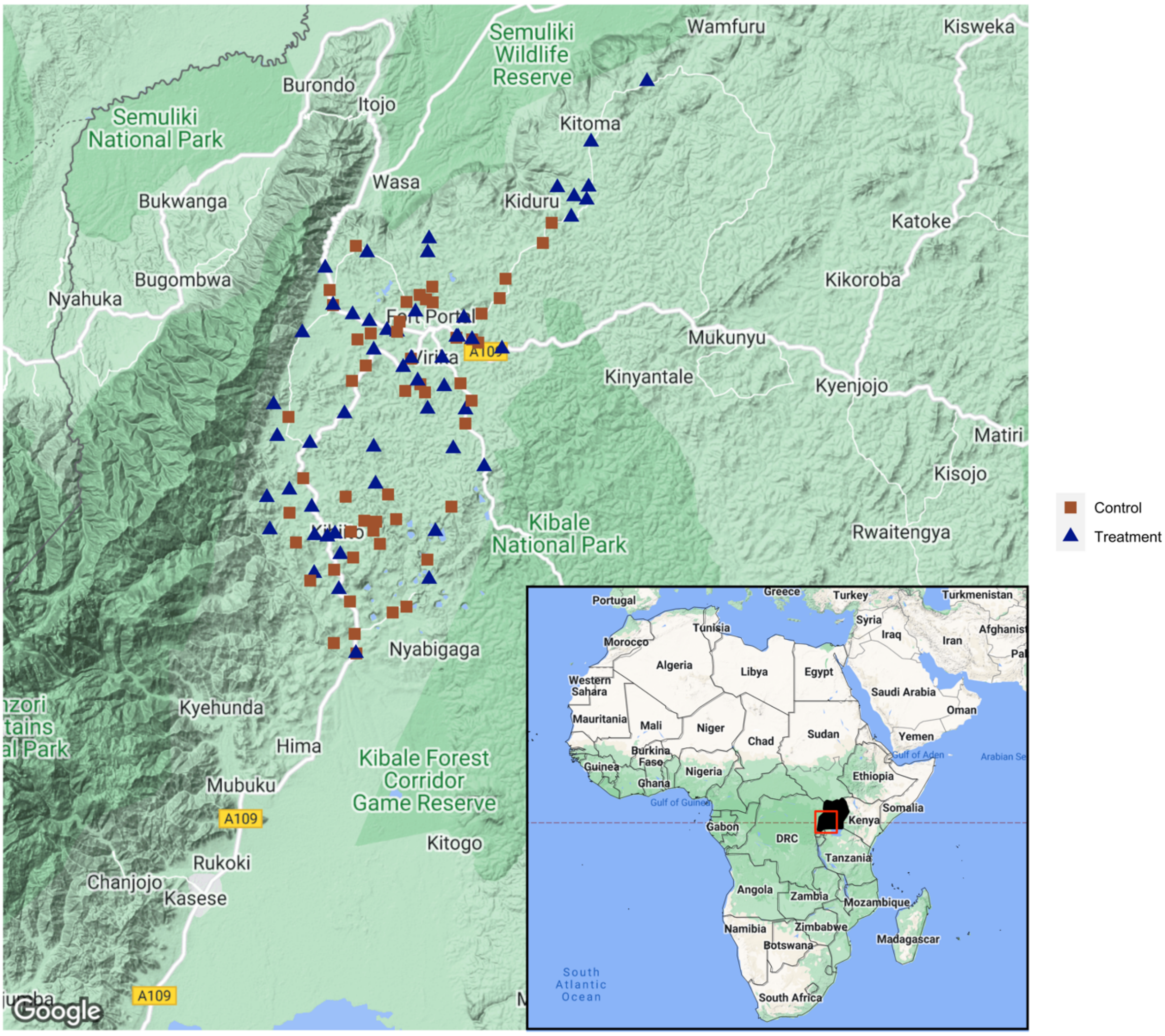
Is patience malleable via educational intervention? Evidence on the role of age in field experiments

APPENDIX A: Field experiment supplementary information

Table A1: Timeline

	Q1/′19	Q2/′19	Q3/′19	Q4/′19	Q1/′20	Q2/′20	Q3/′20	Q4/′20	Q1/′21	Q2/′21
Baseline (field)	X	X								
Treatment			X							
Follow-Up (phone)								X		
Follow-Up (field)										X

Figure A1: Location and treatment status of clusters



Note: This figure displays location and treatment status of 108 trading centers (clusters) in Western Uganda.

A1. Description of treatment studied in the field experiment.

The educational intervention studied here is similar to financial education curricula used around the world. In Uganda the central bank (Bank of Uganda) is the responsible authority to implement a national strategy on financial education. Thus, the Bank of Uganda has partnered with the German Development Cooperation to develop financial education curricula and methods. The financial education curriculum aims to foster financial decision making of households and small-scale entrepreneurs.

While contents of this intervention are conventional, the pedagogical approach relies on "active learning": This type of treatment has been evaluated before in a separate sample experiment (Kaiser and Menkhoff, 2022): The treatment produces large and tentatively lasting effects on financial behaviors such as an increase in total savings. Given the empirical evidence, the program has subsequently been scaled up and also implemented in other countries such as Zambia.

The intervention strongly emphasizes the benefits of delaying consumption to gain utility at a later point in time, the benefits of saving, the costs of consumption credit and the importance of having long-term financial goals. Participants are presented with five stations offering mini cases and group problem solving tasks: (i) budgeting and personal financial management, (ii) saving and future consumption, (iii) borrowing and debt management decisions, (iv) business investing, and (v) money transfer and mobile payments. There are two modifications relative to the pilot "active learning treatment" studied in Kaiser and Menkhoff (2022, detailed description in Table A1). First, the scaled-up treatment lasts longer with about four to five hours instead of two (i.e., about 60 minutes for each station). Second, the station on financial services is quite broad in Kaiser and Menkhoff (2022) but in this case it is more focused on mobile money, and this topic is also touched upon in the savings and borrowing parts.

There are several contents relating to patience and longer-term orientation in the five stations:

(i) The topic of "personal financial management" is built around the budget of a family household. Participants identify inflows and outflows and discuss differences between "needs" and "wants" regarding outflows. The consumption of "Wants" may be delayed to the future. The key message is defined as: "Know your priorities. Spend within your budget." Learning objectives are (a) Understanding what personal financial management means, (b) being able to set financial goals, (c) being able to prioritize needs over wants and being able to make a budget, (d) being aware of challenges when it comes to sticking to a budget.

(ii) The next station on "saving" collects saving motives, discusses ways and forms to save by discussing their benefits and downsides, and finally ten correct statements about the benefits of savings are identified. Participants share their saving motives and are presented with role-models. The station strongly emphasizes the benefits delaying consumption to gain utility at a later point in time and the importance of long-term savings goals. The key message is: "Always save for the future". The learning objectives are: (a) Being able to name three main reasons for saving, (b) being able to make a savings plan, (c) being able to identify secure options for keeping savings in financial institutions.

(iii) The purpose of the "debt management" station is distinguishing between (productive) investment loans and consumption loans, and about the costs of loans. Avoiding expensive loans intended for consumption may imply to delay consumption into the future. Participants are cautioned against borrowing at expensive rates for immediate consumption purposes (especially temptation goods). The key message is: "Use a loan well and repay it on time." The learning objectives are: (a) knowing what a loan is and understanding the loan cycle, (b) being able to identify different loan sources and their advantages and disadvantages, (c) being able to distinguish between loans for productive purposes vs. loans for consumption.

(iv) The "investment" station makes aware about the difference between consumption and productive investments. Then it informs and discusses about risk and return of various investment alternatives. The decision to invest means to expect forgiving consumption today but possibly gaining more in the future. The key message is: "Invest wisely and watch your business grow." The learning objectives are described as: (a) Being able to reflect on common myths about investing, (b) being able to analyze and compare different investment options, (c) making an investment plan and anticipating possible risks associated with an investment.

(v) The final station about "money transfer" focuses on mobile money providers, the rights of users and at some length on the costs of alternative transfer services. While there is no direct emphasis on patient behavior, participants are made aware of the indirect costs associated with physical transactions relative to digital transactions (e.g., the opportunity costs associated with travelling to deliver cash payments). The key message is: "Service providers must respect your rights!" The learning objectives are: (a) knowing the difference between regulated and unregulated service providers, (b) being aware of customer rights and responsibilities, (c) being able to compare direct and indirect costs of alternative formal and informal money transfer options, (d) being aware of the risks associated with the use of informal financial services.

Table A2: Overview of financial education treatment

	Topic	Learning Objectives	Duration
Session #1	Introduction – Financial Providers	- Participants know at least three Financial Service Providers - Participants know at least 3 rights of Financial Service users - Participants are able to choose a financial service provider that suits their demands	30-40 minutes
Session #2	Personal Financial Management	- Participants have the ability to differentiate between financial inflows and outflows - Participants have the ability to differentiate between needs and wants, prioritize needs over wants and making a simple budget for somebody else - Participants are able to budget for themselves and their (family) businesses - Participant know the importance of personal financial management and motivated to start financial planning and record keeping in their business	60-90 minutes
Session #3	Saving	- Participants know the meaning of saving - Participants reflect on the reasons for saving - Participants are able to evaluate saving methods by their accessibility, return and security and making educated choices of financial products for saving	60-90 minutes
Session #4	Debt management	- Participants know what a loan is - Participants can distinguish productive and non-productive loans - Participants know at least three costs of borrowing	60 minutes
Session #5	Investment	- Participants know and reflect about common myths about investing - Participants can analyze different investment options - Participants are able to draft an investment plan and to anticipate possible risks.	60 minutes
Session #6	Money transfer	- Participants know what money transfer is - Participants know about mobile money - Participants are able to compare the costs between different means of money transfer options - Participants are able to compare the risk associated with some informal ways of money transfer.	60 minutes

A2. Response rates, attrition, sample restrictions, compliance, and randomization balance

Table A3 shows response rates for the full sample, respondents dropped out at endline, respondents exhibiting inconsistent choices, and future-biased respondents. An inconsistent choice occurs when subjects violate the law of demand, i.e., choosing the sooner payment when the gross interest rate (1+r) is 1.2 (budget 3), while choosing the later payment when the gross interest rate is 1.11 (budget 3). Choice-consistent allocations corresponding with the law of demand imply a decrease of allocations to sooner payments as the interest rate increases. Future-biased behavior implies that respondents choose the later payment when payments are immediate (t=0) (budget 1) while choosing the sooner payments when the front-end delay is one month (t=1) (budget 2). As choice-inconsistent and future-based behavior indicate that respondents had no understanding of the task, we exclude these observations from our analysis (Alan and Ertac, 2018).

If attrition, task comprehension or take-up, are systematically correlated with treatment status and/or age, these distortions may compromise the internal validity of the experiment. In Table A4 we regress dummies for whether the participant dropped out in the endline survey and exhibited inconsistent choices on the treatment dummy and individual characteristics: treatment assignment is not significantly correlated with being lost in the endline survey and exhibiting inconsistent behavior in the time preference elicitation task. In addition, Table A5 shows that treatment assignment has a strong causal effect on individual take-up but is not differential by age.

In Table A6, we probe for randomization balance by comparing the means and standard deviations of the control and the treatment group for a richer set of individual- (Panel A) and household-level characteristics (Panel B). P-values are based on a linear regression with the treatment dummy as single predictor and the characteristic as dependent variable, with standard errors clustered at the district level, i.e., the unit of randomization. Reported p-values show that the sample is balanced on observables at baseline. In addition, we probe for baseline randomization balance using the full baseline sample, with results shown in Table A7. The results are near identical.

Table A3: Response rates

	N (Control)	N (Treatment)	N (All)
Baseline sample	991	879	1,870
Endline sample	862	793	1,655
Endline sample without inconsistent choices	748	711	1,459
Endline sample without future-biased and inconsistent respondents	629	588	1,217

Notes: Inconsistent choices occur when respondents violate the law of demand in the time preference elicitation task, i.e., choosing the sooner payment when the interest rate is 1.2 while choosing the later payment when the interest rate is 1.11.

Table A4: Determinants of attrition and CTB comprehension

	Attrition (1/0)			Inconsistent or future biased Choice (1/0)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample	≤24 years of age	>24 years of age	Full sample	≤24 years of age	>24 years of age
Treatment	-0.030	0.006	-0.036**	0.001	-0.032	0.010
	(0.019)	(0.050)	(0.018)	(0.021)	(0.038)	(0.025)
Female	0.014	0.003	0.022	0.053***	0.075	0.046**
	(0.014)	(0.045)	(0.014)	(0.020)	(0.048)	(0.022)
Age	-0.002***	-0.007	-0.002**	0.002*	0.009	0.002*
	(0.001)	(0.012)	(0.001)	(0.001)	(0.009)	(0.001)
Married	-0.036**	-0.068	-0.026**	0.003	-0.079	0.022
	(0.014)	(0.043)	(0.013)	(0.022)	(0.052)	(0.024)
Primary education	-0.015	-0.055	-0.007	0.057**	0.030	0.060**
	(0.017)	(0.068)	(0.016)	(0.022)	(0.063)	(0.024)
Catholic	-0.013	-0.014	-0.013	-0.015	-0.058	-0.009
	(0.015)	(0.044)	(0.015)	(0.021)	(0.050)	(0.023)
Number of children	-0.016*	-0.063*	-0.006	0.021*	-0.004	0.026**
	(0.009)	(0.032)	(0.007)	(0.011)	(0.035)	(0.011)
Household size	0.008	0.035*	0.002	-0.019***	0.013	-0.027***
	(0.007)	(0.019)	(0.007)	(0.006)	(0.022)	(0.007)
Constant	0.202***	0.293	0.171***	0.197*	0.018	0.202
	(0.045)	(0.250)	(0.056)	(0.103)	(0.251)	(0.124)
Observations	1,863	343	1,520	1,863	343	1,520
R-squared	0.030	0.115	0.024	0.019	0.112	0.022
Clusters	108	101	108	108	101	108

Notes: Columns 1-3 run a test for differential attrition. The dependent variable is 1 if a participant is lost in the endline survey, 0 otherwise. Column 4-6 checks whether inconsistent choices are associated with treatment assignment. The dependent variable is 1 if a participant made an inconsistent choice or future biased choice in the Convex Time Budgeting Task, 0 otherwise. Results are based on a linear probability model (LPM). Standard errors are clustered at the village level *** p<0.01, ** p<0.05, * p<0.1

Table A5: Determinants of financial education participation

	(1) Full sample	(2) ≤24 years of age	(3) >24 years of age
Treatment (Invited)	0.698***	0.703***	0.698***
	(0.024)	(0.059)	(0.023)
Female	-0.012	-0.065	-0.001
	(0.022)	(0.073)	(0.027)
Age	0.001	0.001	0.001
	(0.001)	(0.018)	(0.001)
Married	-0.028	0.003	-0.029
	(0.027)	(0.069)	(0.032)
Primary education	-0.031	0.009	-0.025
	(0.027)	(0.090)	(0.027)
Catholic	0.033*	-0.026	0.047*
	(0.019)	(0.049)	(0.025)
Number of children	0.006	0.101**	-0.001
	(0.011)	(0.039)	(0.012)
Household size	-0.006	-0.057**	-0.001
	(0.009)	(0.027)	(0.010)
Constant	0.029	0.038	0.018
	(0.058)	(0.424)	(0.068)
Observations	1,210	209	1,001
R-squared	0.486	0.551	0.488
Clusters	108	81	107

Notes: The dependent variable is 1 if the respondent participated in the financial education treatment (individual take-up), 0 otherwise. Results are based on a linear probability model (LPM). Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Table A6: Additional descriptive statistics and randomization balance at baseline (endline estimation sample)

	Control *(N=629)*	Treatment *(N=588)*	p-value
Panel A: Respondent characteristics at baseline			
Female (1/0)	0.622	0.599	0,657
Age	33.781 (11.162)	34.766 (12.49)	0,365
Married (1/0)	0.494	0.527	0,438
Catholic (1/0)	0.485	0.459	0,38
No. of children	1.892 (1.757)	1.927 (1.802)	0,87
Tertiary education (1/0)	0.108	0.134	0,406
Illiterate (1/0)	0.124	0.131	0,859
Financial literacy (no. of correct items)	3.642 (1.637)	3.694 (1.658)	0,592
Self-reported patience	5.901 (2.637)	5.997 (2.645)	0,47
Numeracy	0.898 (0.783)	0.92 (0.806)	0,775
Sum of individual savings (UGX)	701,548.7 (1,620,014.4)	709,717 (1,487,040.6)	0,756
Business investments in past year (UGX)	1,413,483.7 (2,874,803.8)	1,626,735.9 (3,181,338.1)	0,585
Trust in delayed payments (1/0)	0.965	0.976	0,299
Work experience (years)	6.904 (7.537)	7.529 (8.308)	0,346
Risk aversion	5.413 (2.671)	5.25 (2.655)	0,494
Panel B: Household characteristics at baseline			
Household size	4.024 (2.508)	4.146 (2.643)	0,651
No. of rooms	2.374 (1.454)	2.493 (1.548)	0,485
No. of plots owned	1.143 (1.232)	1.31 (1.313)	0,133
Owns own plot (1/0)	0.525	0.548	0,651
Number of assets	36.614 (16.993)	38.752 (18.364)	0,222
Tap water (1/0)	0.583	0.645	0,265
Monthly HH consumption (UGX)	493,870.8 (341,309.3)	503,600.1 (335,361.4)	0,797

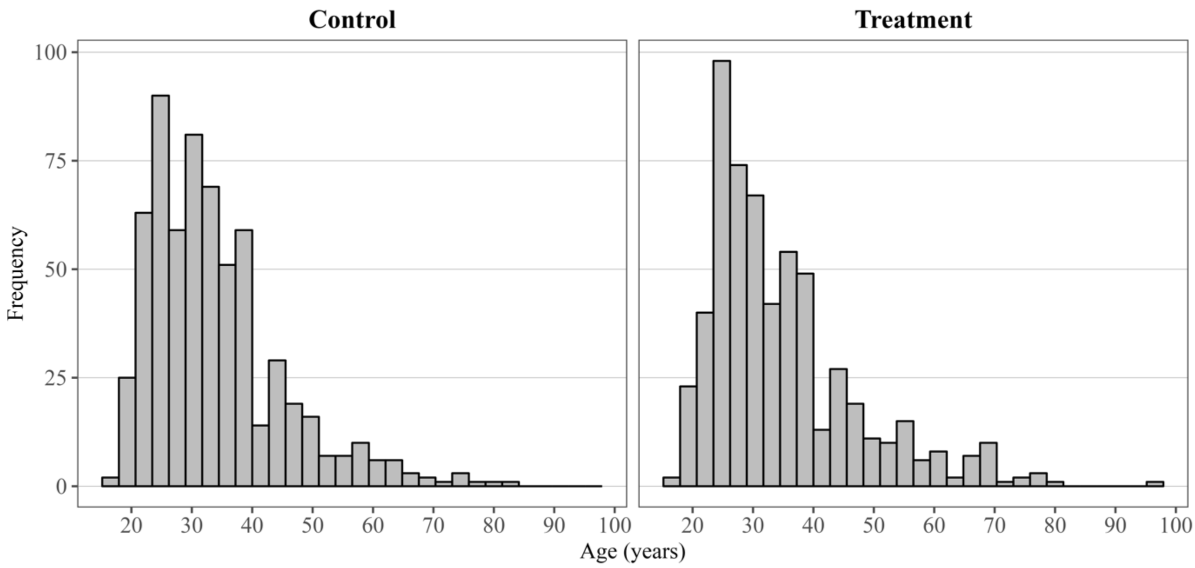
Notes: Means and standard deviations (in parenthesis) of additional individual characteristics (Panel A) and household characteristics (Panel B) at baseline by treatment and control. Financial literacy is measured using adapted versions of five commonly used questions on interest compounding, inflation, risk diversification, mortgages, and bonds. Risk aversion is assessed by asking respondents to report their risk aversion on a scale from 1 (very low) to 10 (very high). P-values are based on a linear regression with the treatment status as single predictor and standard errors clustered at the district level. P-values are unadjusted for multiple hypothesis testing. Sum of savings, investments and monthly household consumption are winsorized at the 99th percentile. F-statistic of test for joint orthogonality is 1.12 (p=0.322).

Table A7: Additional descriptive statistics for the full baseline sample (*N=1,870*)

Variable	Control *(N=991)*	Treatment *(N=879)*	p-value
Panel A: Respondent characteristics at baseline			
Female (1/0)	0.642	0.622	0.746
Age	33.319 (11.368)	34.339 (12.003)	0.194
Married (1/0)	0.486	0.497	0.872
Catholic (1/0)	0.49	0.447	0.115
No. of children	1.817 (1.692)	1.903 (1.783)	0.539
Tertiary education (1/0)	0.115	0.132	0.462
Illiterate (1/0)	0.122	0.115	0.610
Financial literacy (no. of correct items)	3.657 (1.633)	3.667 (1.65)	0.979
Self-reported patience	5.81 (2.678)	5.983 (2.682)	0.149
Numeracy	0.916 (0.789)	0.901 (0.806)	0.602
Sum of savings	655,090 (1,517,493)	712,203 (1,500,488)	0.832
Investments	1,371,897 (2,748,671)	1,499,072 (2,924,460)	0.751
Trust in delayed payments (1/0)	0.968	0.974	0.408
Work experience (years)	6.632 (7.291)	7.402 (8.37)	0.076
Panel B: Household characteristics at baseline			
Risk aversion	5.229 (2.748)	5.235 (2.707)	0.525
Household size	3.919 (2.405)	4.046 (2.578)	0.543
No. of rooms	2.335 (1.481)	2.414 (1.528)	0.400
No. of plots owned	1.139 (1.386)	1.281 (1.303)	0.213
Owns own plot (1/0)	0.495	0.510	0.569
Number of assets	36.429 (17.67)	37.679 (17.932)	0.466
Tap water (1/0)	0.591	0.635	0.389
Monthly HH consumption	479,047(334,673)	498,813 (332,416)	0.558

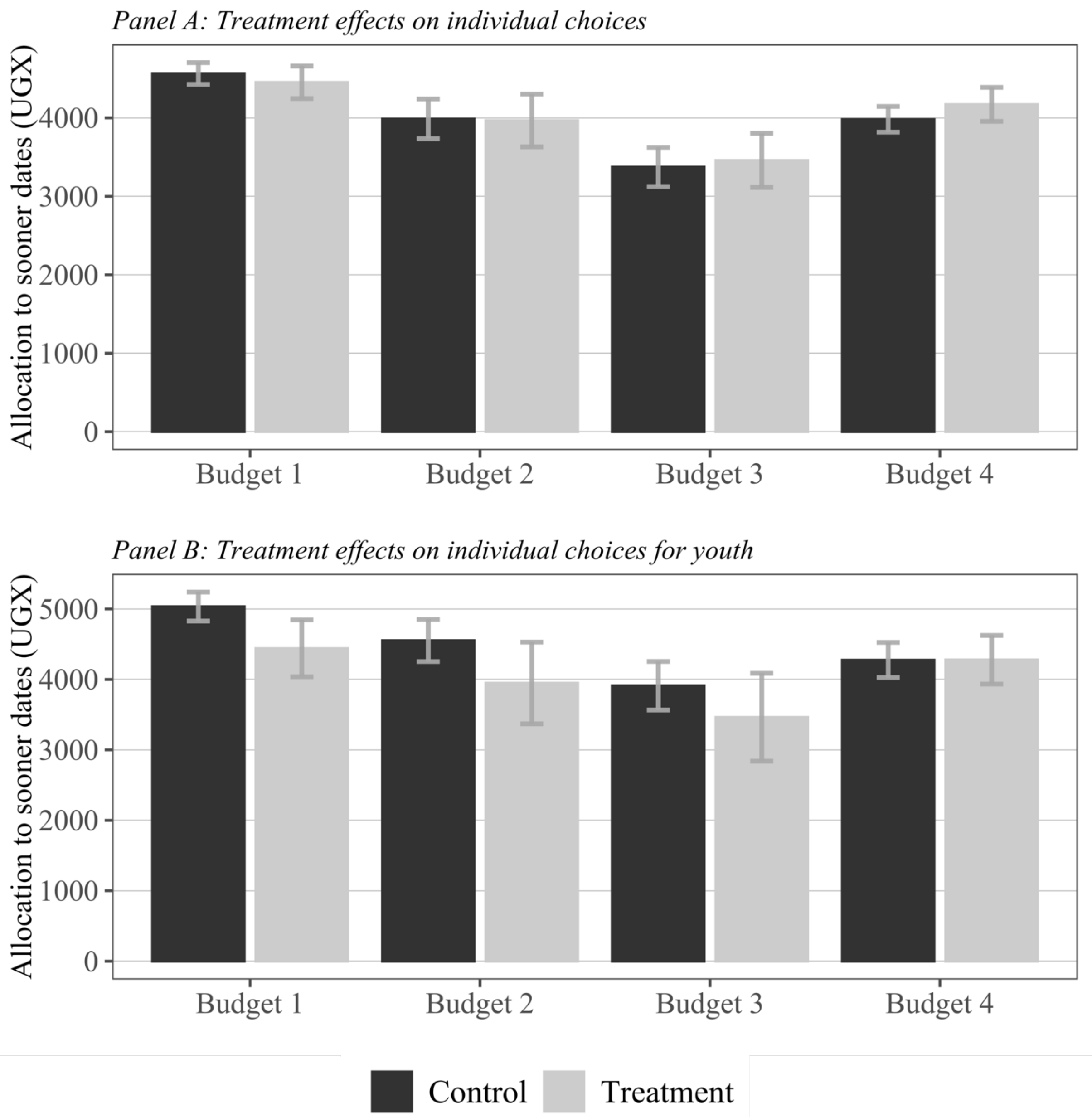
Notes: Means and standard deviations (in parenthesis) of additional individual characteristics (Panel A) and household characteristics (Panel B) at baseline by treatment and control for the full sample at baseline. Variables and p-values are reported as in Table B1 and are unadjusted for multiple hypothesis testing.

Figure A2: Distribution of age by treatment status



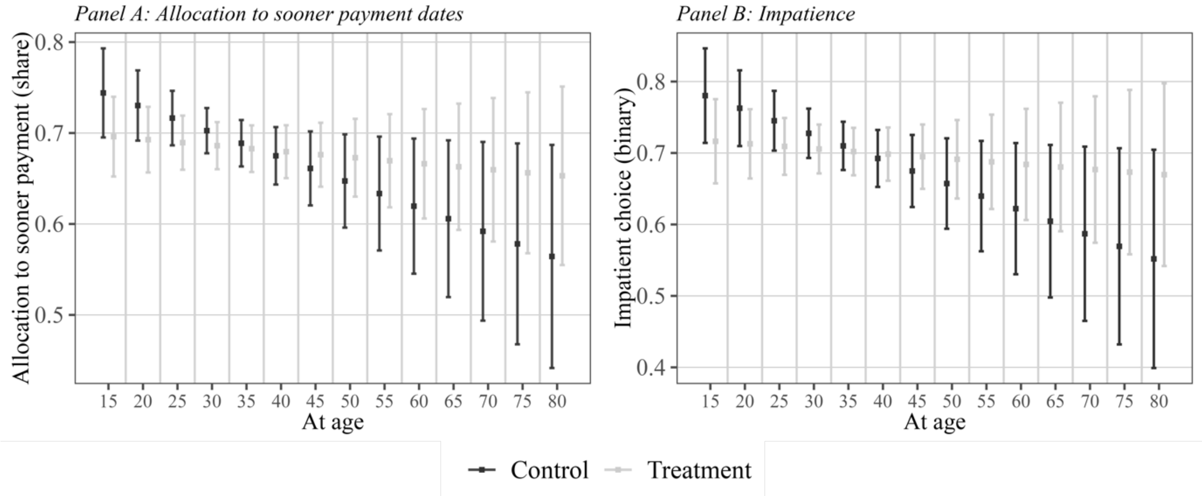
APPENDIX B: Field experiment auxiliary results

Figure B1: Treatment effects on individual choices



Note: This figure shows the average allocation to sooner payment dates across all four CTB budgets with 95%-Cis by the treatment and control group for the full sample (Panel A) and for respondents with age equal to 24 years or below (Panel B).

Figure B2: Treatment effects on impatience depending on age



Notes: This figure shows linear effects of age and treatment and their interaction with 95% Cis. Dependent variables are the proportion of allocations to sooner payment dates and a dummy for whether the sooner payment is chosen.

Table B1: Treatment effects on financial numeracy

	(1) Full sample	(2) ≤24 years of age	(3) >24 years of age
Treatment	-0.016	0.026	-0.005
	(0.050)	(0.132)	(0.060)
Female	-0.127**	0.010	-0.147**
	(0.057)	(0.184)	(0.068)
Age	0.001	0.042	-0.001
	(0.003)	(0.060)	(0.003)
Married	0.235***	-0.130	0.279***
	(0.070)	(0.178)	(0.072)
Primary education	0.324***	0.238	0.332***
	(0.073)	(0.195)	(0.078)
Catholic	0.064	-0.115	0.108*
	(0.061)	(0.160)	(0.060)
Number of children	0.003	0.067	-0.009
	(0.026)	(0.103)	(0.026)
Household size	-0.034*	-0.059	-0.031
	(0.020)	(0.079)	(0.020)
Constant	-0.698***	-0.866	-0.767***
	(0.179)	(1.349)	(0.178)
Observations	1,210	209	1,001
R-squared	0.084	0.139	0.108
Clusters	108	81	107

Notes: This table estimates treatment effects on financial numeracy. The dependent variable *Financial* n*umeracy* is based on equally weighted and standardized scores from responses from items asking respondents to conduct simple calculations on compound interest and inflation. Results are based on ordinary least scares regressions (OLS). Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Table B2: Treatment effects on corner choices

	Corner choice (1/0)			Interest-sensitive corner choice (1/0)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample	≤24 years of age	>24 years of age	Full sample	≤24 years of age	>24 years of age
Treatment	0.010	0.021	0.011	-0.007	-0.018	-0.005
	(0.022)	(0.041)	(0.024)	(0.004)	(0.015)	(0.005)
Today	-0.035***	-0.027	-0.037***			
	(0.010)	(0.023)	(0.011)			
Delay=6 months	-0.014	0.018	-0.021			
	(0.014)	(0.026)	(0.015)			
Price ratio (1+r)	-0.002	-0.009	-0.000			
	(0.007)	(0.020)	(0.009)			
Treatment * Today	-0.008	0.027	-0.014			
	(0.015)	(0.035)	(0.016)			
Treatment * Delay=6 months	-0.020	0.013	-0.026			
	(0.025)	(0.049)	(0.025)			
Treatment * Interest rate	0.003	-0.001	0.004			
	(0.012)	(0.023)	(0.014)			
Constant	0.130*	0.158	0.122**			
	(0.068)	(0.137)	(0.060)			
Observations	4,868	836	4,032	1,210	209	1,001
R-squared	0.018	0.118	0.018	0.017	0.103	0.021
District FEs	YES	YES	YES	YES	YES	YES
Clusters	108	81	107	108	81	107

Notes: . Results are based on ordinary least scares regressions (OLS). Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table B3: Treatment effects on measures of liquidity

	Average treatment effects (full sample)				Heterogeneous treatment effects (age ≤24)				Heterogeneous treatment effects (age >24)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Any loans (1/0)	ln(loans)	ln(avg. monthly profits)	Using MM to get paid (1/0)	Any loans (1/0)	ln(loans)	ln(avg. monthly profits)	Using MM to get paid (1/0)	Any loans (1/0)	ln(loans)	ln(avg. monthly profits)	Using MM to get paid (1/0)
Treatment	-0.023	-0.241	-0.029	0.052*	0.045	0.431	-0.512	0.046	-0.037	-0.403	0.028	0.066*
	(0.024)	(0.320)	(0.227)	(0.031)	(0.064)	(0.759)	(0.532)	(0.063)	(0.026)	(0.352)	(0.254)	(0.039)
Female	0.033	0.070	-0.916***	-0.066**	0.078	0.449	-0.699	-0.053	0.022	-0.051	-0.931***	-0.077**
	(0.029)	(0.349)	(0.196)	(0.028)	(0.086)	(0.912)	(0.512)	(0.070)	(0.031)	(0.392)	(0.254)	(0.031)
Age	-0.002*	-0.022	-0.007	0.000	0.027	0.442	0.187	0.030*	-0.002*	-0.027	-0.010	-0.002
	(0.001)	(0.014)	(0.012)	(0.002)	(0.022)	(0.327)	(0.123)	(0.016)	(0.001)	(0.017)	(0.014)	(0.001)
Married	0.012	0.195	0.071	0.049	0.030	0.050	-0.200	-0.053	0.009	0.221	0.155	0.045
	(0.026)	(0.324)	(0.243)	(0.031)	(0.079)	(0.962)	(0.463)	(0.078)	(0.027)	(0.338)	(0.272)	(0.032)
Primary education	0.014	0.528	0.607*	0.112***	-0.049	0.165	1.273	-0.071	0.013	0.496	0.512	0.131***
	(0.028)	(0.336)	(0.312)	(0.032)	(0.121)	(1.397)	(0.844)	(0.111)	(0.032)	(0.386)	(0.331)	(0.036)
Catholic	-0.004	0.007	0.534**	0.027	-0.015	-0.868	-0.146	-0.029	-0.003	0.133	0.654**	0.049
	(0.026)	(0.344)	(0.208)	(0.032)	(0.070)	(0.783)	(0.487)	(0.076)	(0.029)	(0.383)	(0.250)	(0.033)
Number of children	0.013	0.180	-0.014	0.016	-0.057	-0.553	0.109	-0.027	0.019	0.224	-0.034	0.012
	(0.012)	(0.149)	(0.085)	(0.015)	(0.057)	(0.635)	(0.360)	(0.040)	(0.012)	(0.156)	(0.093)	(0.015)
Household size	-0.003	0.010	0.054	-0.014	0.031	0.624	-0.049	0.011	-0.008	-0.061	0.064	-0.014
	(0.009)	(0.112)	(0.066)	(0.011)	(0.031)	(0.488)	(0.245)	(0.027)	(0.009)	(0.116)	(0.071)	(0.011)
Constant	0.757***	8.965***	9.670***	0.331**	0.119	-2.297	7.158**	-0.274	0.785***	9.595***	9.508***	0.416***
	(0.076)	(0.996)	(0.914)	(0.133)	(0.507)	(7.668)	(2.875)	(0.350)	(0.084)	(1.110)	(1.050)	(0.125)
Observations	1,210	1,210	1,160	1,210	209	209	205	209	1,001	1,001	955	1,001
R-squared	0.034	0.045	0.063	0.056	0.130	0.134	0.184	0.190	0.037	0.050	0.076	0.068
Clusters	108	108	108	108	81	81	81	81	107	107	107	107

Notes: This table shows treatment effects on various field behaviors. Dependent variables are whether the respondent has taken out any loans, the log of the sum of all loans, the log of the average of monthly profits, and whether the respondent received any mobile payments by consumers. Regressions with binary dependent variables are based on a linear probability model (LPM). Standard errors are clustered at the village level. All regression models contain the dependent variable at baseline as predictor. *** p<0.01, ** p<0.05, * p<0.1

Table B4: Treatment effects on impatient choices excluding respondents with no trust

	Average treatment effects (full sample)		Heterogeneous treatment effects (age ≤24)		Heterogeneous treatment effects (age >24)	
	(1) Share of allocated budget to sooner payments	(2) Impatient choice (1/0)	(3) Share of allocated budget to sooner payments	(4) Impatient choice (1/0)	(5) Share of allocated budget to sooner payments	(6) Impatient choice (1/0)
Treatment	-0.017	-0.023	-0.141***	-0.165***	0.011	0.008
	(0.025)	(0.033)	(0.046)	(0.059)	(0.027)	(0.035)
Today	0.098***	0.127***	0.071***	0.093***	0.104***	0.134***
	(0.014)	(0.018)	(0.019)	(0.027)	(0.016)	(0.020)
Delay=6 months	0.110***	0.129***	0.058**	0.056	0.121***	0.144***
	(0.014)	(0.016)	(0.027)	(0.033)	(0.014)	(0.017)
Price ratio (1+r)	-0.054***	-0.058***	-0.046***	-0.046**	-0.056***	-0.060***
	(0.007)	(0.008)	(0.015)	(0.019)	(0.008)	(0.010)
Treatment * Today	-0.014	-0.012	0.003	-0.010	-0.018	-0.013
	(0.017)	(0.022)	(0.028)	(0.039)	(0.019)	(0.025)
Treatment * Delay=6 months	0.020	0.034	0.095**	0.099*	0.005	0.019
	(0.020)	(0.028)	(0.046)	(0.057)	(0.021)	(0.029)
Treatment * Interest rate	0.013	0.011	0.023	0.026	0.011	0.008
	(0.010)	(0.011)	(0.021)	(0.024)	(0.011)	(0.014)
Constant	0.644***	0.647***	0.718***	0.718***	0.632***	0.638***
	(0.036)	(0.057)	(0.128)	(0.167)	(0.035)	(0.048)
Observations	4,724	4,724	820	820	3,904	3,904
R-squared	0.043	0.040	0.103	0.105	0.049	0.046
District FEs	YES	YES	YES	YES	YES	YES
Clusters	108	108	81	81	107	107

Notes: This table replicates Panel A in table 3 in section without respondents who stated that they do not trust in receiving the later payment. All regressions included stratification fixed effects. Standard errors (in parentheses) are clustered at the individual and village level.. *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX C: Structural model and time preference elicitation

C.1 Structural estimation of utility parameters

In our theoretical framework, we assume a time separable CRRA utility function using the model of quasi-hyperbolic discounting described in Laibson (1997), formally expressed as

$$U(c_t, c_{t+k}) = (c_t - \omega_t)^{\alpha} + \beta_{t=0}\delta^k(c_{t+k} - \omega_{t+k})^{\alpha} \qquad (1),$$

where δ^k denotes the daily discount factor and β the present bias parameter. β shrinks utility from delayed consumption when payments are immediate (t=0). α represents the risk coefficient under constant relative risk aversion (CRRA), which is jointly estimated in the CTB framework. ω_t and ω_{t+k} denote Stone-Geary background consumption parameters as used in Andersen et al. (2008). Respondents maximize equation (1) subject to the budget constraint $(1+r)c_t + c_{t+k} = 6000$, which yields (if consumption is immediate) the intertemporal Euler equation

$$\frac{c_t - \omega_t}{c_{t+k} - \omega_{t+k}} = (\beta\delta^k(1+r))^{\frac{1}{\alpha-1}} \qquad (2).$$

By varying the price ratio (1+r), we can estimate the degree of intertemporal substitution and utility function curvature (both denoted by α), whereas variation in the length of delay k allows the estimation of long-run discount factors δ (Table C1). Finally, by shifting the front-end delay from “today” to “in one month”, we estimate time-inconsistent behaviors represented by present bias parameters β. In line with Andreoni and Sprenger (2012), we transform the Euler equation given in equation (2) into the optimal Stone-Geary demand for consumption at sooner payment dates c_t, formally expressed as

$$c_t = \frac{1}{1 + (1+r)((1+r)\beta\delta^k)^{\frac{1}{\alpha-1}}}\omega_t + \left[\frac{\left(\beta\delta^k(1+r)\right)^{\frac{1}{\alpha-1}}}{1 + (1+r)\left(\beta\delta^k(1+r)\right)^{\frac{1}{\alpha-1}}}\right](m - \omega_{t+k}) \qquad (3)$$

We estimate (3) simply using non-linear least squares and recover estimated parameters $\alpha, \delta,$ and β via non-linear combinations. Following Andreoni and Sprenger (2012) and Lührmann et al. (2018), we set Stone-Geary consumption minima ω_t and ω_{t+k} equal to zero.

C.2 Verbatim instructions for the time preference elicitation task

We are almost done with the interview, and we appreciate your cooperation. In this part, we will play a fun exercise. Depending on your choices, you will receive extra money on top of the fixed amount for the survey participation.

What is this part of the study about?

In this game you will be asked to choose between two payments on different time dates. You will make four decisions about allocating a certain money amount between a sooner point in time (e.g., today) or a later point in time (e.g., in one month). One of these four decisions will be randomly selected for actual payments at the end of this study. So, make sure to take every decision as if it were the decision that is paid out.

We show you an example how it works.

Now imagine you have a choice between the following three options:

Option A: You can receive 6,000 UGX today and 0 UGX in one month.

Option B: You can receive 3,000 UGX today and 3,000 UGX in one month.

Option C: You can receive 0 UGX today and 6,000 UGX in one month.

Do you have any questions before we proceed?

In this part of the study, you will have to take more than one decision. In total, you have to make four different decisions with the difference that the today payment today may decrease along the decisions while the amount for the later payment remains constant. Also, the dates of the different payments may vary. For instance, we may ask you to choose between a payment in one month and in six months. Please remember that only one of these four decisions will be randomly selected for actual payment. Therefore, make sure to make decisions that you really want.

Do you have any questions before we proceed?

How are payments going to work? As already indicated, only one out of four decisions will be chosen at the end of the experiment which yields into actual payments. As a “thank you” for participating, you will also receive additional 1,000 UGX which will be split in half across the two payment dates. This means you receive additional 500 UGX per point of time, irrespective of your choices. Let’s assume you chose Option A in the aforementioned example (i.e. you receive 6,000 UGX today and 0 UGX in one month). Then you receive 6,000 UGX plus 500 UGX, i.e., 6,500 UGX, today and 500 UGX (0 UGX + 500 UGX) in one month. You will receive your money via mobile money or airtime transfer.

Do you have any questions before we proceed?

(1) You have the choice between the following three options:

Option A: You can receive 5,400 UGX today and 0 UGX in one month.

Option B: You can receive 2,700 UGX today and 3,000 UGX in one month.

Option C: You can receive 0 UGX today and 6,000 UGX in one month.

(2) You have the choice between the following three options:

Option A: You can receive 5,400 UGX in one month and 0 UGX in two months.

Option B: You can receive 2,700 UGX in one month and 3,000 UGX in two months.

Option C: You can receive 0 UGX in one month and 6,000 UGX in two months.

(3) You have the choice between the following three options:

Option A: You can receive 5,000 UGX in one month and 0 UGX in two months.

Option B: You can receive 2,500 UGX in one month and 3,000 UGX in two months.

Option C: You can receive 0 UGX in one month and 6,000 UGX in two months.

(4) You have the choice between the following three options:

Option A: You can receive 5,000 UGX in one month and 0 UGX in six months.

Option B: You can receive 2,500 UGX in one month and 3,000 UGX in six months.

Option C: You can receive 0 UGX in one month and 6,000 UGX in six months.

The computer has now randomly chosen one question [question number]. You chose option [A, B or C]. Therefore, the payment amounts are:

You will receive in one month on [automatically include date]:

You will receive in two months on [automatically include date]:

You will receive in six months on [automatically include date]:

Do you trust that you will receive your delayed payment? [yes/no]

APPENDIX D: Included studies in the meta-analysis

Table D1: Overview of included studies in the meta analysis

#	Article	Country	Sample size	Mean age (Range)	Reported outcomes	Structural model	Correlates to field behaviors	Treatment	Intensity (h)	Mean delay (weeks)	No. of extracted estimates
1	Breitkopf et al. 2022	Bangladesh	3,222	8.5 (7-10)	Allocation to sooner payment dates	no	no	Social and emotional learning program (Lions Quest Skills for Growing). The program emphasizes emotion regulation, goal setting, caring about others, and acting responsibly.	14	14.65	4
2	Migheli and Mosca-rola 2017	Italy	165	8.5 (8-9)	Allocation to sooner payment dates	no	no	Laboratory procedure aimed at making the children familiar with the utility of saving; Mental exercises to stimulate children thinking about their future selves.	1	0	2
3	Alan and Ertac 2018	Turkey	1,111	9.5 (9-10)	Allocation to sooner payment dates (MPL and CTB)	no	yes	Financial education intervention focused on visualizing the future and evaluating intertemporal trade-offs in a forward-looking manner with eight mini case studies. *Topics:* Imagining future self; self-control against temptation goods; smart shopping; games to make future utilities vivid; saving for a target; evaluating alternative future outcomes; meet a savings target	16	67.2	20
4	Lührmann et al. 2018	Germany	914	14 (13-15)	- Allocation to sooner payment dates - Corner choices (CTB) - Choice consistency - Time consistency	yes	yes	Financial education program offered in schools (ages 13-15) *Modules:* Shopping, planning, saving	4.5	8	1
5	Bover et al. 2018	Spain	4,100	15.5 (15-16)	- Allocation to sooner payment dates (CTB) - Choice consistency	no	no	Financial education program offered in schools (ages 14-15) *Modules:* Saving, Budgeting, responsible consumption, bank accounts, pension funds, insurance vehicles	10	19.57	4
6	Sutter et al. 2020	Germany	645	16 (15-16)	Change in future premium (Impatience) (MPL)	no	yes	Financial education intervention with focus on individual decision-making *Topics:* Individual savings, investments, consumption decisions, including behavioral biases	8	13.25	4

7	Bjorvatn et al. 2020	Tanzania	1,902	17.9 (17-18)	Allocation to later payment dates	no	no	Encouragement design studying a TV show on entrepreneurship and financial decision making with focus on female empowerment; Viewers follow contestants through a number of challenges on how to plan and operate a business. *Topics:* Credit, savings, insurance, market assessment, costumer care, marketing, record keeping, health, appearance	11	3	1
8	Horn et al. 2023	Uganda	2,680	24.5 (St.dev. = 7)	- Discoun-ting index - Single choices	no	no	Financial education course based based on active and customized learning. *Topics:* Saving, formal financial institutions, budgeting, borrowing, interest	15	156.6	2
9	Blattmann et al. 2017	Liberia	947	25.4	- Allocation to sooner payment dates	no	no	STYL curriculum (cognitive behavioral therapy with training). Curriculum includes 11 modules with the following contents: transformation, substance abuse, body cleanliness, garbage/dirt control, anger management, self-esteem, planning, goal setting, money business, money saving, challenges and setbacks	75	52.2	1
10	Kaiser et al. 2023	Uganda	1,217	33.8	- Allocation to sooner payment dates - Corner choices (CTB) - Choice consistency - Time consistency	yes	yes	Financial education programme based on active learning (i.e., discussions and group problem solving). Contents: Financial service providers, personal financial management, saving, debt management, investing, money transfers	4-6	65.25	4
11	Berge et al. 2015	Tanzania	211	37.1	Single patient choice	no	no	Business training with the aim of unleashing entrepreneurship and creating business growth. Topics include: “entrepreneurship and entrepreneurial character,” “improving customer service,” “managing people in your business,” and “marketing strategies.”	15.75	8.7	2

APPENDIX E: Meta-analysis auxiliary results and sensitivity analyses

Table D1: Alternative models

	(1) RVE (τ^2=$\hat{\tau}^2$)	(2) RVE ($\tau^2 = 0$)	(3) UWLS ($\tau^2 = 0$)
$\hat{\theta}$	-0.051	-0.042	-0.123*
	(0.037)	(0.038)	(0.066)
$\hat{\tau}^2$	0.011	0.000	0.000
n (estimates)	45	45	45
n (studies)	11	11	11

Notes: *:* This table shows results from meta-regression analyses relying on the model defined in Eq. 3. The dependent variable is the standardized treatment effect reported within the studies. Columns 1 and 2 rely on Robust Variance Estimation with dependent effect sizes (ρ=0.8). Column 3 relies on unrestricted weighted least squares.

Table D2: Leave-one-out meta-analysis

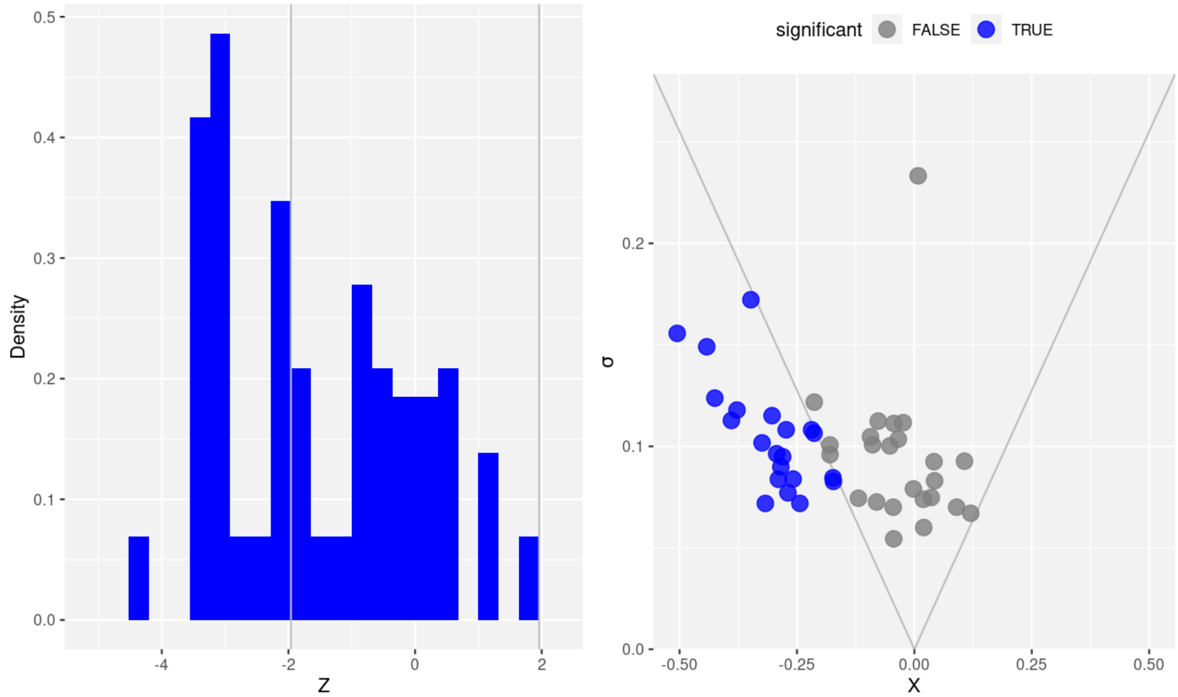
Omitted study	Meta-estimate ($\hat{\theta}$)	Std. Err.	τ^2	I^2
Breitkopf et al. 2022	-0.0675558	0.0380349	0.009828	61.96
Migheli and Moscarola 2017	-0.0545604	0.0387583	0.010803	67.16
Alan and Ertac 2018	-0.0256459	0.0240871	0.002035	40.78
Lührmann et al. 2018	-0.0684996	0.0361503	0.009176	64.83
Bover et al. 2018	-0.0507328	0.0396766	0.011007	68.08
Sutter et al. 2018	-0.0554577	0.0394019	0.011018	68.42
Bjorvatn et al. 2020	-0.0560916	0.0380432	0.010559	66.41
Horn et al. 2023	-0.0691922	0.0373011	0.009419	62.58
Blattmann et al. 2017	-0.0634245	0.0374195	0.010043	65.68
Kaiser et al. 2023	-0.0540810	0.0395943	0.011077	63.50
Berge et a. 2015	-0.0484480	0.0366539	0.009859	66.80

Notes: This table shows estimates of the model defined in Eq(2) of the main text when removing studies from the sample on a case-by-case basis.

D1. Publication bias

The left panel of Figure D1 shows the distribution of z-statistics (the quotient of treatment effect estimate and associated standard error), and the right panel of Figure D1 shows an inverted funnel plot, i.e., plotting the treatment effect estimate against the standard error with solid grey lines indicating the boundary for "statistically significant" results (i.e., where the quotient of treatment effect and standard error equal 1.96 in absolute values. As can be seen, there appears to be an asymmetry. A formal test for selective publication (Andrews and Kasy 2019) results in a conditional publication probability of estimates with $|Z| < 1.96$ of about 60 percent. While this indicates some degree of selective publication of estimates, this number is in line with the evidence presented in Brodeur et al. (2020) who report a conditional publication probability of insignificant estimates of about 50 percent. Accordingly, the publication bias corrected estimate proposed by Andrews and Kasy (2019) where one uses the estimated conditional publication probabilities to reweight the meta-average is not too far from the simple arithmetic mean of treatment effect estimates: The unadjusted average is about -0.16 SD and the adjusted estimate is about -0.12 SD (i.e., about 25 percent smaller).

Figure D1: Funnel plot of treatment effects and histogram of z-statistics



Notes: The left panel shows a binned density plot for the z-statistics (Z= X/ Σ). The solid grey lines indicate the critical values at $|Z| = 1.96$ while the dash-dotted gray line marks $Z = 0$. The right panel plots the extracted estimate (X) against its standard error (Σ). The gray lines mark $|Z| = 1.96$.

Appendix References

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