Is patience malleable via educational intervention? Evidence from field experiments

Tim Kaiser, Lukas Menkhoff, and Luis Oberrauch

Abstract

We study the malleability of patience via educational interventions by aggregating evidence from earlier experiments in a meta-analysis and by conducting a field experiment. We find that the average effect of interventions on patience is positive but uncertain. The age of students explains a large share of between-study heterogeneity in treatment effects. Thus, we conduct a field experiment covering both youths and adults in Uganda. We find heterogenous effects by age: adults' patience measured in incentivized tasks is unaffected by the intervention after 15 months follow-up, but we observe large effects on patience and estimated discount factors for youth.

JEL Codes: C93 (field experiments), D15 (intertemporal household choice), I21 (analysis of education)

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

November 2022

Acknowledgements: We thank Filder Aryemo, Lucy Haag, Jana Hamdan, Helke Seitz, and Yuanwei Xu for excellent field work and research assistance, and thank seminar participants, Fabian Kosse, Annamaria Lusardi, David McKenzie, 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."

Tim Kaiser, University of Koblenz-Landau, D-76829 Landau, Germany (ORCiD: https://orcid.org/0000-0001-7942-6693); kaiser@uni-landau.de.

Lukas Menkhoff, Humboldt-Universität zu Berlin, German Institute for Economic Research (DIW Berlin), D-10108 Berlin, and IfW Kiel, Germany; lmenkhoff@diw.de.

Luis Oberrauch, University of Tuebingen, D-72074 Tuebingen, Germany; luis.oberrauch@uni-tuebingen.de.

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 and better education (Sutter et al., 2013; Falk et al., 2018). Qualitatively the same relation applies at the country level, such that more patience in societies is correlated with higher country incomes (Sunde et al., 2022). 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 endogeneity and variability of preferences and personality traits (e.g., Becker and Mulligan, 1997; Doepke and Zilibotti, 2017). Empirical studies provide evidence of intergenerational transmission of preferences (Dohmen et al., 2012; Falk et al., 2021) as well as 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: is there is a causal effect of educational interventions on time preferences?

Evidence from field experiments may support the hypothesis that time preferences are indeed malleable via 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., 2020). As existing studies focus on children and youth, this raises the question whether treatment effects are limited to the 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.

We study this issue in two steps. First, we test the hypothesis of malleability of time preferences by aggregating the evidence from all available studies that combine field experiments with incentivized decision experiments in a meta-analysis. This estimates the average effect of educational intervention on patience and the heterogeneity in true effects across studies. We find a positive average effect of educational interventions on patience, i.e., a 0.081 SD reduction in incentivized measures of impatience, but the 95 percent confidence interval does not rule out null or even small adverse effects. Additionally, heterogeneity in true effects is non-trivial, as 81 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 as a potentially crucial mediator of treatment effects and find that, once student ages at the study-level is accounted for, residual heterogeneity is estimated to be very small. Treatment effects across studies decline with increasing age, but at a decreasing rate. These results serve as empirical benchmarks and inform the hypotheses for our primary field experiment.

As a second step of our research, we complement the existing literature by conducting a field experiment comprising 1,217 individuals on time preferences covering both youths and adults in rural Uganda. We study the effect of an educational 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 appear to be 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 and increases estimated individual discount factors.

Our results 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., 2020), time preferences and the quality of intertemporal choice (Alan and Ertac, 2018; Lührmann et al., 2018), effort and grit (Alan et al., 2019), self-regulation (Schunk et al., 2022), social preferences (Cappelen et al., 2020; Kosse et al., 2020), and honesty (Abeler et al., 2021). All these studies contribute to the insight that preferences of various kinds may be malleable to some extent, especially among young individuals.

The role of young 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 largest for small children (e.g., Sherwood and Gómez-Robles, 2017). All this is in line with the above finding that educational intervention may have an effect on patience among youth.

The rest of the paper proceeds as follows: Section 2 reviews previous literature on the malleability of time preferences in a meta-analysis. Section 3 describes the setting of our field experiment and time preference elicitation design. Section 4 presents results of this experiment in three steps: First, we report correlations of individual patience with field behaviors; second, we show average treatment effects on choices and estimated preference parameters; and third, we move to a discussion of heterogenous treatment effects by participant age. Section 5 concludes.

2 Meta-analysis of the literature

2.1 Sample of earlier studies

We compile a complete set of field experiments studying the causal effects of educational interventions on time preferences. We searched 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.

Applying these inclusion criteria, we arrive at a sample of nine field experiments on intertemporal decision-making. While the included studies cover a variety of outcomes related to (the quality of) intertemporal decision-making (see, 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). We extract a total of 34 treatment effects on this type of outcome and convert the estimates reported in the studies into scale-free standard deviation units, i.e., bias corrected standardized mean differences, Hedges’ g. For the ease of exposition, we aggregate the treatment effects within studies (3 to 4 on average) to a summary effect size but show results for the full set of treatment effect estimates in the robustness part. Studies are heterogenous regarding the age-groups covered (with most studies addressing interventions for children and youth), the time preference elicitation task, as well as regarding the content and intensity (1 to 16 hours) of the educational intervention. We briefly summarize the individual studies, ordered by the age of their participants (details about these studies are documented in Table A1 in Appendix A). Then we move to a discussion of aggregate treatment effects, i.e. the mean of a distribution of true effects, and estimated heterogeneity in true effects.

Two 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

Three studies were 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. (2020) 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.

Two studies focus on youth and young adults: Bjorvatn et al. (2020) examine the effect of an edutainment treatment for youth 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. (2020) study the effects of a 15-hour financial education intervention offered to members of Ugandan youth clubs. They find no effects on patience during one-year and five-year follow-ups.

Finally, two studies focus on adults. 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 results suggest relatively large effects on the patience of women but zero effects on men in their sample. McKenzie et al. (2022) study an 8-hour financial education intervention coupled with an 8-hour treatment designed to foster financial aspirations of adults in the Philippines. While the authors do not report treatment effects on impatience (but on a measure of present bias), our analysis of their data results in overall insignificant effects on impatience in the incentivized task conducted two years after treatment.

To summarize, the literature often suggests positive treatment effects on patience, but estimates appear to be imprecise in almost all cases. The strongest treatment effects are observed in an evaluation of elementary school students exposed to a relatively intensive treatment (Alan and Ertac, 2018). Thus, we now estimate the average treatment effect across studies and quantify the extent of heterogeneity in true effects across interventions in a meta-study framework.

2.2 Meta-analysis model

After extracting the set of estimated average treatment effects and standard errors from each randomized experiment (j), 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 (see also Meager, 2019; Bandiera et al., 2021; Kaiser et al., 2022, for meta-analyses relying on these types of models in Bayesian and/or frequentist frameworks). With one summary treatment effect per study,[1] the model can be written as:

[1] We also show an approach with multiple correlated effects within studies in Appendix A.

$$\hat{\theta}_j = \theta + \upsilon_j + \epsilon_j \tag{1}$$

with $\upsilon_j \sim N(0, \tau^2)$ and $\epsilon_j \sim N(0, \sigma_j^2)$. 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 (with sensitivity analyses considering other algorithms such as empirical Bayes described in Section 4). σ_j is the within-study standard error of the treatment effect estimate $\hat{\theta}_j$. While σ_j is unknown, the model treats the estimated standard errors of the extracted treatment effect estimates $\hat{\sigma}_j$ as known (i.e., $\sigma_j = \hat{\sigma}_j$). Subsequently, weighted least squares is used to estimate θ with inverse variance weights defined as $w_j = (\hat{\tau}^2 + \hat{\sigma}_j^2)^{-1}$. While this model assumes heterogeneity in true effects, it also nests the case of the common effect (or "fixed effect") model. In case of no heterogeneity in true effects (i.e., $\tau^2 = 0$), the model reduces to $\hat{\theta}_j = \theta + \epsilon_j$ and the weights are then defined as $w_j = (\hat{\sigma}_j^2)^{-1}$.

As a next step, we extend the model defined in (1) to a case where we include study-level covariates but still allow for residual heterogeneity in true effects. The model is defined as:

$$\hat{\theta}_j = \beta x_j + \upsilon_j + \epsilon_j \tag{2}$$

where x_j is a vector of study-level observable characteristics and β is a vector of the corresponding coefficients. We estimate the above model including different study-level observables in the vector x_j 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 intensity of instruction, in further analyses.

2.3 Results of the meta-analysis

Figure 1 shows standardized treatment effect estimates and associated 95% confidence intervals of the single studies as well as results from the meta-analysis model (see Eq. 1), shown at the bottom of Figure 1. 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 (-0.081 SD), but the 95 percent confidence interval cannot rule out zero or slightly positive treatment effects. The estimated absolute value of $\hat{\tau}^2$ amounts to about 0.013 and the studies enter the meta-analysis with weights between 6.32% and 15.28%; i.e., no single study appears to dominate the aggregate result. Accordingly, the result is qualitatively unchanged when we omit one study at a time and then estimate the model for the remaining set of studies (see robustness section).

< Figure 1 about here >

As τ^2 may be estimated with error, we conduct sensitivity analyses covering a range of other possible values of τ^2, which generate estimated true effects between -0.116 and -0.081 SDs for $\tau^2 \geq 0.001$ and $\tau^2 \leq 0.1$ and by altering the estimation algorithm for $\hat{\tau}^2$ (see robustness section). Given the small number of diverse studies, it is no surprise that heterogeneity statistics (I^2 of about 80.84%) indicate substantial 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).

Thus, we take the mean age of the students at the study-level as a potentially important mediator of the treatment effects and estimate the model described in Eq. (2). We include linear and quadratic effects of age as study-level covariates in this meta-regression model to incorporate the idea that preferences may be malleable up to the end of a formative period in the life cycle, but treatment effects may not diminish linearly thereafter. Estimating this specification results in statistically significant linear and quadratic effects of age in the hypothesized directions, i.e., treatment effects on impatience decrease with increasing age but at a declining rate (see Table 1, column 2).

< Table 1 about here >

Once the age of the students is accounted for, the estimated heterogeneity parameter $\hat{\tau}^2$ is reduced to 0.0016, and the residual heterogeneity is only about 25%. Next, we model treatment effects as a function of treatment intensity. This covariate appears to explain some part of the heterogeneity in treatment effects but to a much lesser extent than student age (see column 3). Further covariates, i.e. the delay between treatment and time preference elicitation task (column 4) as well as the country setting (column 5) do not explain heterogeneity in true effects. Accordingly, the combination of considering both “age” and “intensity” 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.

To conclude: The meta-analysis of earlier field experiments suggests that education interventions may reduce impatience to some degree. In particular, the (young) age of students seems to be an important moderator of treatment effects. While this result is consistent with theory, it relies on a between-study comparison which may be subject to unobserved study-level confounders. Thus, we next complement the analysis with an analysis of heterogeneous treatment effects by age within a single study in a field experiment.

3 Field experiment design and data

3.1 Setting and educational intervention

In this section, we describe the experimental design, the setting, the educational interventions, and the preference elicitation methods employed. The study is located in the Rwenzori region of rural Western Uganda and conducted among self-employed individuals as a cluster-randomized control trial with randomization occurring at the village-level. The study was conducted in 108 villages between February 2019 (baseline) and April 2021 (endline).[2]

We randomly assign 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) (Figure B1 in Appendix B shows the location and treatment status of all clusters). The program is delivered as a full day event (approximately four to five hours of direct exposure to the contents). The educational intervention uses "active learning" teaching methods (see Freeman et al., 2014). 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 a different sample), evaluating the general effectiveness of this program, finds that this teaching approach (relative to a traditional community lecture) is effective in affecting financial behaviors, 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 B1 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 studied in the previous literature.

[2] 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.

3.2 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 2, 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 4.95 USD in 2019 PPP).

< Table 2 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 at the sooner date). 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

$$U(c_t, c_{t+k}) = (c_t - \omega_t)^{\alpha} + \beta_{t=0}\delta^{k}(c_t - \omega_{t+k})^{\alpha} \quad (3),$$

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.

3.3 Data

The baseline sample includes 2,067 individuals within 108 clusters surveyed in face-to-face interviews in February 2019. Due to the Covid-19 pandemic, the endline survey was conducted as a telephone interview in April 2021. At endline, we were able to follow-up with 1,879 respondents, i.e., resulting in an attrition rate of 9.1 percent. Out of these, 1,527 were willing to participate in the CTB task (see Table B3 Appendix B). 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 Table B4 in the Appendix), thus indicating that the reduction in the endline estimation sample size is unlikely to threaten the internal validity of the experiment.

Sample descriptive statistics at baseline for the endline estimation sample (i.e., post attrition) in Panel B of Table 2 and additional balance tests on the full baseline sample indicate randomization balance (see Table B1 in Appendix B). About 60 percent of the individuals are female, age is on average 34 years with a standard deviation of 12, and 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 (Dohmen et al., 2011) 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.

4 Results of the field experiment

4.1 Correlates of patience

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). 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 3 show the five relations for 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., Dohmen et al., 2011), suggesting external validity of the experimental measures.

< Table 3 about here >

Turning to the three demographic predictors of impatience (see Panel B of Table 3), 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. 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.

4.2 Average treatment effects

In this section, we first present evidence on allocation patterns and then move to a discussion of average treatment effects on structural parameters. As we observe balanced groups at baseline, treatment effects are not sensitive to covariate adjustment (see Table E1 in Appendix E).

Panel A of Table 4 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 4 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 4 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 $\widehat{\beta_\iota}$ and discount factors $\widehat{\delta_\iota}$. Again, we do not observe average treatment effects on these parameters.

4.3 Heterogenous treatment effects

Next, we investigate heterogeneity in treatment effects. Inspired by results from Section 2 and 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, see e.g., Horn et al., 2020), and run regressions on the same outcomes as in columns (1) and (2) of Table 4 (Panels A and B). Our analysis, in columns 3 and 4 of Table 4, 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 4) as well as the binary measure of impatient choice (column 4 of Panel A in Table 4) 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 observe heterogenous treatment effects on estimated individual utility parameters (columns 3 and 4 of Panel B in Table 4). 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 addressing the issue of multiple hypothesis testing; we adjust inference by employing false discovery rate corrections (see q-values following Anderson, 2008, in brackets in Table 4).

4.4 Auxiliary results and sensitivity analyses

We briefly report auxiliary results and sensitivity analyses while full results are provided in the appendices. These additional analyses cover both the meta-analysis in Appendix D and the field experiment in Appendix E, addressing the following areas: First, we assess the robustness of the meta-study estimates (i) by changing the estimation algorithm to other iterative and non-iterative approaches; (ii) by conducting "leave-one-out meta-analysis;" and (iii) by testing for publication bias in the literature. Since the estimate of the heterogeneity parameter (τ^2) may be estimated with error, we (iv) assess the sensitivity of the meta-estimate (θ) to setting other values of τ^2. Further, we (v) extend the meta-study model to include multiple treatment effects within studies and estimate the meta-average and the meta-regression results using both common-effect and random-effect models.

Next, we probe the robustness of results of the field experiment (vi) by showing treatment effects with covariate adjustment and (vii) by providing auxiliary results on attrition and choice consistency. 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 novel systematic account of previous results on time preferences and a first formal investigation of treatment effects on participants of very heterogeneous age in a field experiment. Both the results of the meta-analysis and the field experiment 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.

References

Abeler, Johannes, Armin Falk, and Fabian Kosse. 2021. "Malleability of Preferences for Honesty." CEPR DP 16164.

Alan, Sule and Seda Ertac. 2018. "Fostering Patience in the Classroom: Results from Randomized Educational Intervention." *Journal of Political Economy*, 126(5), 1865-1911.

Alan, Sule, Teodora Boneva, and Seda Ertac. 2019. "Ever Failed, Try Again, Succeed Better: Results from a Randomized Educational Intervention." *Quarterly Journal of Economics*, 134(3), 1121-1162. doi:10.1093/qje/qjz006.

Andersen, Steffen, Glenn W. Harrison, Morten I. Lau, and Elisabet Rutström. 2008. "Eliciting Risk and Time Preferences." *Econometrica*, 76 (3), 583-618.

Anderson, Michael L. 2008. "Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American Statistical Association*, 103(484), 1481-1495.

Andreoni, James and Charles Sprenger. 2012. "Estimating Time Preferences from Convex Budgets." *American Economic Review*, 102(7), 3333-3356. doi:10.1257/aer.102.7.3333.

Bandiera, Oriana, Greg Fischer, Andrea Prat, and Erina Ytsma. 2021. "Do Women Respond Less to Performance Pay? Building Evidence from Multiple Experiments." *American Economic Review: Insights*, 3(4), 435-454.

Becker, Gary S., and Casey B. Mulligan. 1997. "The Endogenous Determination of Time Preference." *Quarterly Journal of Economics,* 112(3), 729-758.

Berge, Lars Ivar Oppedal, Kjetil Bjorvatn, and Bertil Tungodden. 2015. "Human and Financial Capital for Microenterprise Development: Evidence from a Field and Lab Experiment." *Management Science*, 61(4), 707-722.

Bjorvatn, Kjetil, Alexander W. Cappelen, Linda Helgesson Sekei, Erik Ø. Sørensen, and Bertil Tungodden. 2020. "Teaching Through Television: Experimental Evidence on Entrepreneurship Education in Tanzania." *Management Science*, 66(6), 2308-2325.

Bover, Olympia, Laura Hospido, and Ernesto Villanueva. 2018. "The Impact of High School Financial Education on Financial Knowledge and Choices: Evidence from a Randomized Trial in Spain." IZA DP No. 11265.

Callen, Michael. 2015. "Catastrophes and Time Preference: Evidence from the Indian Ocean Earthquake." *Journal of Economic Behavior and Organization*, 118, 199-214.

Callen, Michael, Mohammad Isaqzadeh, James D. Long, and Charles Sprenger. 2014. "Violence and Risk Preference: Experimental Evidence from Afghanistan." *American Economic Review*, 104(1), 123-48.

Cappelen, Alexander, John List, Anya Samek, and Bertil Tungodden. 2020. "The Effect of Early-Childhood Education on Social Preferences." *Journal of Political Economy*, *128*(7), 2739-2758.

Carvalho, Leandro S., Silvia Prina, and Justin Sydnor. 2016. "The Effect of Saving on Risk Attitudes and Intertemporal Choices." *Journal of Development Economics*, 120, 41-52. doi:10.1016/j.jdeveco.2016.01.001.

Caspi, Avshalom, Brent W. Roberts, and Rebecca L. Shiner. 2005. "Personality Development: Stability and Change." *Annual Review of Psychology*, 56, 453-484.

Cole, Shawn, Thomas Sampson, and Bilal Zia. 2011. "Prices or Knowledge? What Drives Demand for Financial Services in Emerging Markets?" *Journal of Finance*, 66, 1933-1967.

Cunha, Flavio and James Heckman. 2007. "The Technology of Skill Formation." *American Economic Review*, 97(2), 31-47.

Cunha, Flavio, James J. Heckman, and Susanne M. Schennach. 2010. "Estimating the Technology of Cognitive and Noncognitive Skill Formation." *Econometrica,* 78(3), 883-931.

DeYoung, Colin G., Jacob B. Hirsh, Matthew S. Shane, Xenophon Papademetris, Nallakkandi Rajeevan, and Jeremy R. Gray. 2010. "Testing Prediction from Personality Neuroscience: Brain Structure and the Big Five." *Psychological Science*, 21(6), 820-828.

Doepke, Matthias and Fabrizio Zilibotti. 2017. "Parenting with Style: Altruism and Paternalism in Intergenerational Preference Transmission." *Econometrica,* 85(5), 1331-1371.

Dohmen, Thomas, Armin Falk, David Huffman, Uwe Sunde, Jürgen Schupp, and Gert G. Wagner. 2011. "Individual Risk Attitudes: Measurement, Determinants, and Behavioral Consequences." *Journal of the European Economic Association*, 9(3), 522-550. doi:10.1111/j.1542-4774.2011.01015.x

Dohmen, Thomas, Armin Falk, David Huffman, and Uwe Sunde. 2012. "The Intergenerational Transmission of Risk and Trust Attitudes." *Review of Economic Studies*, 79(2), 645-677.

Falk, Armin, Anke Becker, Thomas Dohmen, Benjamin Enke, David Huffman, and Uwe Sunde. 2018. "Global Evidence of Economic Preferences." *Quarterly Journal of Economics*, 133(4), 1645-1692.

Falk, Armin, Fabian Kosse, Pia Pinger, Hannah Schildberg-Hörisch, and Thomas Deckers. 2021. "Socioeconomic Status and Inequalities in Children's IQ and Economic Preferences." *Journal of Political Economy*, 129(9), 2504-2545.

Freeman, Scott, Sarah L. Eddy, Miles McDonough, Michelle K. Smith, Nnadozie Okoroafor, Hannah Jordt, and Mary Pat Wenderoth. 2014. "Active Learning Increases Student Performance in Science, Engineering, and Mathematics." *Proceedings of the National Academy of Sciences*, 111(23), 8410-8415.

Hanaoka, Chie, Hitoshi Shigeoka, and Yasutora Watanabe. 2018. "Do Risk Preferences Change? Evidence from the Great East Japan Earthquake." *American Economic Journal: Applied Economics*, 10 (2), 298-330.

Hanushek, Eric A., Lavinia Kinne, Philipp Lergetporer, and Ludger Woessmann. 2022. "Patience, Risk-taking, and Human Capital Investment across Countries." *Economic Journal*, 132(646), 2290-2307.

Horn, Samantha, Julian Jamison, Dean Karlan, and Jonathan Zinman. 2020. "Does Lasting Behavior Change Require Knowledge Change? Evidence from Savings Interventions for Young Adults." NBER Working Paper 28011.

Kaiser, Tim, Annamaria Lusardi, Lukas Menkhoff, and Carly Urban. 2022. "Financial Education Affects Financial Knowledge and Downstream Behaviors." *Journal of Financial Economics*, 145, 255-272.

Kaiser, Tim and Lukas Menkhoff. 2022. "Active Learning Improves Financial Education: Experimental Evidence from Uganda." *Journal of Development Economics*, 157, 102870.

Kosse, Fabian, Thomas Deckers, Pia Pinger, Hannah Schildberg-Hörisch, and Armin Falk. 2020. "The Formation of Prosociality: Causal Evidence on the Role of Social Environment." *Journal of Political Economy*, 128(2), 434-467.

Laibson, David. 1997. “Golden Eggs and Hyperbolic Discounting.” *Quarterly Journal of Economics*, 112(2), 443-478. doi:10.1162/003355397555253.

Lührmann, Melanie, Marta Serra-Garcia, and Joachim Winter. 2018. “The Impact of Financial Education on Adolescents’ Intertemporal Choices.” *American Economic Journal: Economic Policy*, 10(3), 309-332.

Lusardi, Annamaria and Olivia Mitchell. 2014. “The Economic Importance of Financial Literacy: Theory and Evidence.” *Journal of Economic Literature*, 52(1), 5-44.

Malmendier, Ulrike and Stefan Nagel. 2011. “Depression Babies: Do Macroeconomic Experiences Affect Risk-taking?” *Quarterly Journal of Economics*, 126(1), 373-416.

McKenzie, David, Aakash Mohpal, and Dean Yang. 2022. “Aspirations and Financial Decisions: Experimental Evidence from the Philippines.” *Journal of Development Economics*, 156, 102846.

Meager, Rachael. 2019. “Understanding the Average Impact of Microcredit Expansions: A Bayesian Hierarchical Analysis of Seven Randomized Experiments.” *American Economic Journal: Applied Economics*, 11(1), 57-91.

Migheli, Matteo and Flavia Coda Moscarola. 2017. “Gender Differences in Financial Education: Evidence from Primary School.” *De Economist*, 165, 321-347.

Schunk, Daniel, Eva M. Berger, Henning Hermes, Kerstin Winkel, and Ernst Fehr. 2022. “Teaching Self-regulation.” *Nature Human Behaviour*, doi.org/10.1038/s41562-022-01449w.

Shamosh, Noah A. and Jeremy R. Gray. 2008. “Delay Discounting and Intelligence: A Meta-analysis.” *Intelligence,* 36(4), 289-305.

Sherwood, Chet C. and Aida Gómez-Robles. 2017. “Brain Plasticity and Human Evolution.” *Annual Review of Anthropology*, 46, 399-419.

Stigler, George J. and Gary S. Becker. 1977. “De Gustibus Non Est Disputandum.” *American Economic Review*, 67(2), 76-90.

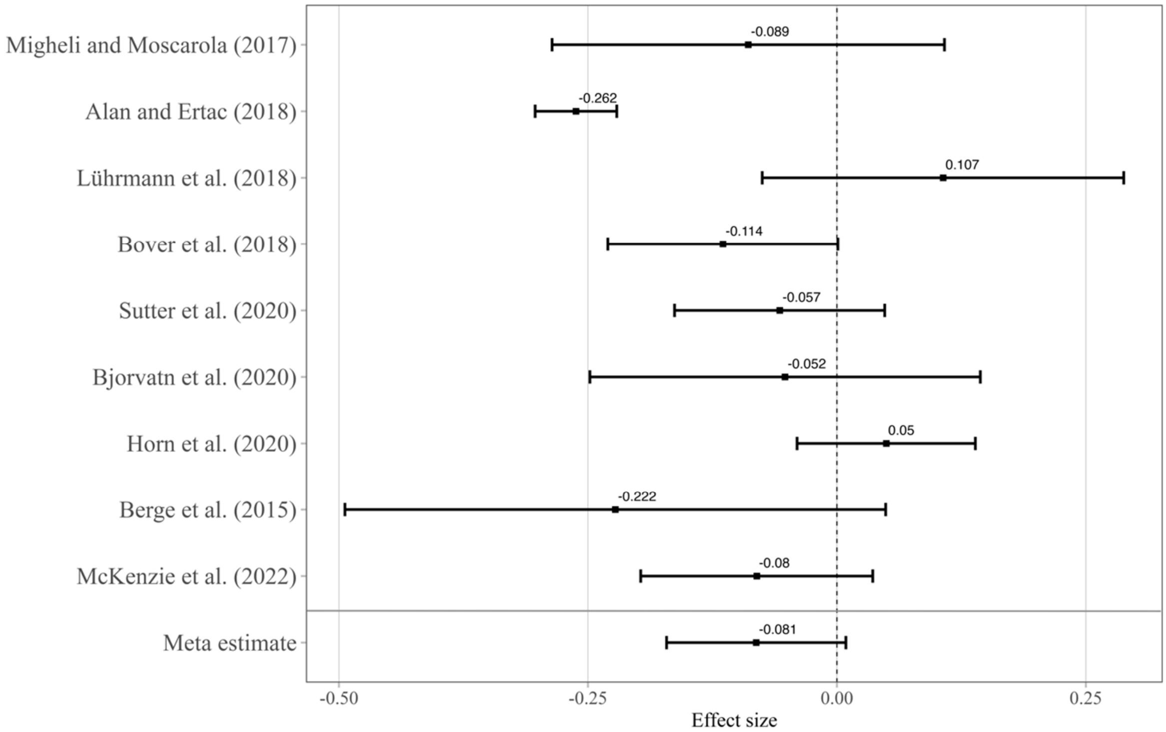
Sunde, Uwe, Thomas Dohmen, Benjamin Enke, Armin Falk, David Huffman and Gerrit Meyerheim. 2022. “Patience and Comparative Development.” *Review of Economic Studies*, forthcoming.

Sutter, Matthias, Martin G. Kocher, Daniela Glätzle-Rützler, and Stefan T. Trautmann. 2013. “Impatience and Uncertainty: Experimental Decisions Predict Adolescents’ Field Behavior.” *American Economic Review*, 103(1), 510-531. doi:10.1257/aer.103.1.510.

Sutter, Matthias, Michael Weyland, Anna Untertrifaller, and Manuel Froitzheim. 2020. “Financial Literacy, Risk and Time Preferences: Results from a Randomized Educational Intervention.” IZA DP No. 13566.

Voors, Maarten J., Eleonora E. M. Nillesen, Philip Verwimp, Erwin H. Bulte, Robert Lensink, and Daan P. Van Soest. 2012. “Violent Conflict and Behavior: A Field Experiment in Burundi.” *American Economic Review*, 102(2), 941-64.

Figure 1: Meta-analysis of earlier field experiments on impatience



Notes: This figure shows treatment effects of single studies on standardized measures of impatience (in SD units) and 95% CIs, as well as the (weighted) average treatment effect using the random-effects meta-analysis model (Eq. 1) estimated via restricted maximum likelihood. Studies are arranged by the average age of individuals within the study samples.

Table 1: Meta-regression analysis

	(1)	(2)	(3)	(4)	(5)	(6)
Age		0.034**				0.034**
		(0.014)				(0.009)
Age × Age		-0.001*				-0.0005**
		(0.0002)				(0.0001)
Intensity			-0.009			-0.013*
			(0.008)			(0.055)
Delay				0.0004		
				(0.0008)		
Developing country					-0.031	
					(0.083)	
Meta estimate ($\widehat{\theta}$)	-0.081	-0.480**	-0.105**	-0.099	-0.067	-0.511***
	(0.039)	(0.149)	(0.043)	(0.056)	(0.059)	(0.085)
$\hat{\tau}^2$	0.01200	0.0003	0.0083	0.01231	0.0101	0.000
$\tau^2 = 0$ (p-value)	<0.001	0.018	<0.001	<0.001	<0.001	0.183
I^2	80.84%	25.57%	71.12%	79.88%	74.72%	0.00%
n (studies)	9	9	9	9	9	9

Notes: This table shows results from meta-regression analyses relying on the random-effects model defined in Eq. 2. *** p<0.01, ** p<0.05, * p<0.1.

Table 2: 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
Monthly savings (UGX)	701,549 (1620,014)	709,717 (1487,041)	0.756
Monthly 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)	0.92 (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 3: Correlates of experimental patience measures

Panel A: Impatience and field behavior

	(1) ln(Savings)	(2) Tertiary education (1/0)	(3) Self-reported patience	(4) ln(Savings)	(5) Tertiary education (1/0)	(6) 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)	
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 99th 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. *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 4: Treatment effects

	Average treatment effects (full sample)		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)
Treatment	-0.016	-0.023	-0.146***	-0.172***
	(0.024)	(0.032)	(0.045)	(0.058)
	[0.329]	[0.329]	[0.017]	[0.017]
Today ($t = 0\ days$)	0.097***	0.126***	0.068***	0.089***
	(0.014)	(0.017)	(0.018)	(0.025)
Delay ($k = 150$ days)	0.109***	0.129***	0.064**	0.063*
	(0.013)	(0.016)	(0.029)	(0.035)
Interest rate ($1 + r = 1.2$)	-0.057***	-0.062***	-0.052***	-0.054***
	(0.007)	(0.009)	(0.016)	(0.020)
Treatment × Today	-0.015	-0.013	0.006	-0.007
	(0.017)	(0.022)	(0.027)	(0.038)
Treatment × Delay	0.021	0.033	0.089*	0.092
	(0.020)	(0.028)	(0.047)	(0.058)
Treatment × Interest rate	0.014	0.014	0.029	0.033
	(0.010)	(0.012)	(0.021)	(0.025)
Control mean	0.687	0.710	0.770	0.815
Standardized effect size	-0.045	-0.051	-0.505	-0.442
R^2	0.042	0.039	0.102	0.104
N (budget choices)	4,868	4,868	836	836
N (individuals)	1,217	1,217	209	209
Clusters (villages)	108	108	81	81
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 $\widehat{\beta_\iota}$
Treatment	0.016	-0.007	0.077***	-0.022
	(0.014)	(0.004)	(0.028)	(0.021)
	[0.313]	[0.175]	[0.017]	[0.313]
Control mean	1.063	0.993	1.030	0.999
Standardized effect size	0.079	-0.109	0.520	-0.275
R^2	0.013	0.020	0.091	0.109
N (individuals)	1,055	1,055	186	186
Clusters (villages)	108	108	78	78

Notes: Panel A shows average treatment effects and heterogeneous treatment effects by age. 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). Additionally, we regress allocations variables on changes in the elicitation design and its interactions with the treatment dummy. 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. Sharpened q-values 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.

ONLINE-APPENDIX

to accompany

Is patience malleable via educational intervention? Evidence from field experiments

APPENDIX A: Included studies in the meta-analysis

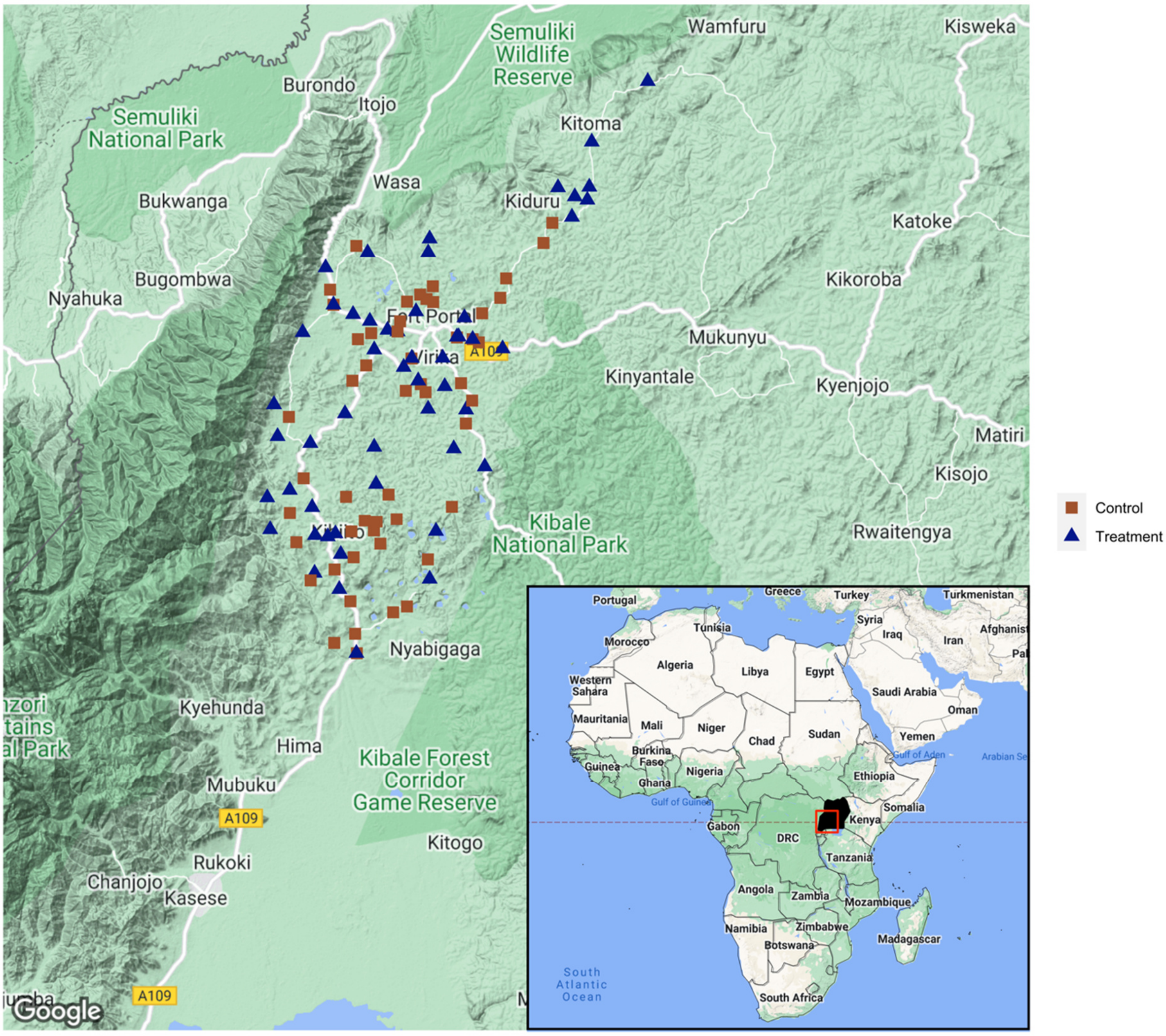
Table A1: Overview of included studies

#	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	Migheli and Moscarola 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	1
2	Alan and Ertac 2018	Turkey	1111	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
3	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
4	Bover et al. 2018	Spain	4100	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	2
5	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
6	Bjorvatn et al. 2020	Tanzania	1902	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

7	Horn et al. 2020	Uganda	2680	24.5 (St.dev. = 7)	- Discounting 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
8	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
9	McKenzie et al. 2022	Philippines	2464	49.4 (21-84)	Time inconsistency (CTB) (we extracted a treatment effect on patience using the replication data)	no	no	Combination of financial aspirations and financial knowledge treatment. The aspirations treatment uses games to build self-confidence and exercises that help participants articulate long-term financial aspirations. The knowledge treatment aims to teach basic financial skills to make sound savings and loan decisions. The program content focusses on assets, liabilities, budgeting, and life-cycle planning.	16	73.95	1

APPENDIX B: Detailed information about the field experiment

Figure B1: Location and treatment status of clusters



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

B1. 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” (Freeman et al., 2014). 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) credit and borrowing decisions, (iv) business investing, and (v) 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 "borrowing and 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 "financial services" 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.

B2. Additional evidence on randomization balance

In this appendix section, 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 fully balanced at baseline indicating that randomization worked. In addition, we probe for randomization balance using the full sample without dropouts, with results shown in Table B2.

Table B1: Additional descriptive statistics and randomization balance at baseline

	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 B2: 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	0,61
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)	0,4
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.

B3. Response rates, take-up, and attrition

In this section, we probe for selective attrition and selective using the original sample without dropouts. Table B3 shows response rates for the full sample, without respondents dropped out at endline, without respondents exhibiting inconsistent choices, and without 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 or inconsistent choices are systematically correlated with treatment status, dropouts might bias our estimates. In Table B4 we regress dummies for whether the participant dropped out in the endline survey and exhibited inconsistent choices on the treatment dummy and individual characteristics. Columns 1 and 2 show that 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, column 3 shows that treatment assignment is strongly correlated with individual take-up.

Table B3: Response rates

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

Notes: Respondents at endline without dropouts. 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 B4: Determinants of attrition and CTB participation/comprehension

	(1) Attrition (1/0)	(2) Inconsistent or future biased Choice (1/0)	(3) Take-up (1/0)
Treatment	-0.030	0.001	0.667***
	(0.019)	(0.021)	(0.020)
Female	0.014	0.053***	-0.010
	(0.014)	(0.020)	(0.021)
Age	-0.002***	0.002*	0.001
	(0.001)	(0.001)	(0.001)
Married	-0.036**	0.003	-0.009
	(0.014)	(0.022)	(0.018)
Primary education	-0.015	0.057**	-0.023
	(0.017)	(0.022)	(0.025)
Catholic	-0.013	-0.015	0.040**
	(0.015)	(0.021)	(0.018)
Number of children	-0.016*	0.021*	0.008
	(0.009)	(0.011)	(0.010)
Household size	0.008	-0.019***	-0.004
	(0.007)	(0.006)	(0.009)
Constant	0.202***	0.197*	0.059
	(0.045)	(0.103)	(0.067)
Observations	1,863	1,863	1,863
R^2	0.030	0.019	0.461
Clusters	108	108	108

Notes: Column 1 runs a test for differential attrition. The dependent variable is 1 if a participant is lost in the endline survey, 0 otherwise. Column 2 checks whether inconsistent choices are associated with treatment assignment. The dependent variable is 1 if a participant made an inconsistent choice in the Convex Time Budgeting Task (i.e., violating the law demand), 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

APPENDIX C: Structural estimation and treatment effects

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

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

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 und 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

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

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

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

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: Meta-analysis auxiliary results and sensitivity analyses

Table D1: Leave-one-out meta-analysis

Omitted study	Meta-estimate ($\hat{\theta}$)	CI 95% lower	CI 95% upper	p-value
Migheli and Moscarola 2017	-0.081	-0.171	0.009	0.079
Alan and Ertac 2018	-0.043	-0.103	0.018	0.168
Lührmann et al. 2018	-0.102	-0.184	-0.019	0.016
Bover et al. 2018	-0.077	-0.17	0.017	0.107
Sutter et al. 2018	-0.085	-0.179	0.009	0.075
Bjorvatn et al. 2020	-0.084	-0.174	0.005	0.066
Horn et al. 2020	-0.105	-0.189	-0.021	0.014
Berge et a. 2015	-0.073	-0.159	0.013	0.098
McKenzie et al. 2022	-0.081	-0.175	0.012	0.088

Notes: This table shows estimates of the model defined in Section 2.2 of the main text when removing studies from the sample on a case-by-case basis.

Table D2: Sensitivity of $\hat{\boldsymbol{\theta}}$ to the choice of $\boldsymbol{\tau^2}$

	(1)	(2)	(3)	(4)
	$\tau^2 = 0$	$\tau^2 = 0.001$	$\tau^2 = 0.01$	$\tau^2 = 0.1$
$\hat{\theta}$	-0.165***	-0.116***	-0.082**	-0.080
	(0.016)	(0.023)	(0.041)	(0.109)
I^2	0.00%	24.78%	76.71%	97.05%
n (studies)	9	9	9	9

Notes: This table shows estimates of the model defined in Section 2.2 of the main text when manually setting τ^2 to the respective values and then estimating the model via weighted least squares.

Table D3: Sensitivity of $\hat{\boldsymbol{\tau}}^2$ and $\hat{\boldsymbol{\theta}}$ to the choice of estimation algorithm

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	ML	REML	Empirical bayes	DerSimonian–Laird	Hunter-Schmidt	Sidik-Jonkman	RVE
$\hat{\theta}$	-0.083**	-0.082*	-0.083**	-0.080*	-0.081*	-0.083*	-0.063
	(0.040)	(0.042)	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)
$\hat{\tau}^2$	0.0093	0.0107	0.0088	0.020	0.0128	0.0095	0.006
I^2	75.42%	77.88%	74.48%	86.99%	80.84	75.82%	-
n (estimates)	9	9	9	9	9	9	34
n (studies)	9	9	9	9	9	9	9

Notes: Column 1 presents results from "random-effects" meta-analysis using (unrestricted) maximum likelihood for estimation whereas column (2) repeats the result restricted maximum likelihood estimator. Column (3) uses Empirical base as the estimator and columns (4) to (6) present results relying on three alternative non-iterative estimators to estimate τ^2.

Table D4: Using multiple estimates per study

	(1) RVE (τ^2=$\hat{\tau}^2$)	(2) RVE (τ^2=$\hat{\tau}^2$)	(3) RVE (τ^2=0)	(4) RVE (τ^2=0)	(5) WLS (τ^2=0)	(6) WLS (τ^2=0)
Age		0.0371*		0.0377*		0.034***
		(0.0125)		(0.0123)		(0.007)
Age × Age		-0.0005*		-0.0006*		-0.0005***
		(0.0002)		(0.0002)		(0.0001)
Intensity		-0.0165		-0.0160		-0.0126**
		(0.0068)		(0.0066)		(0.0038)
$\hat{\theta}$	-0.063	-0.5563*	-0.059	-0.5602*	-0.165**	-0.5110
	(0.039)	(0.1527)	(0.040)	(0.1477)	(0.066)	(0.0619)
$\hat{\tau}^2$	0.0060	0.0013	-	-	-	-
n (estimates)	34	34	34	34	34	34
n (studies)	9	9	9	9	9	9

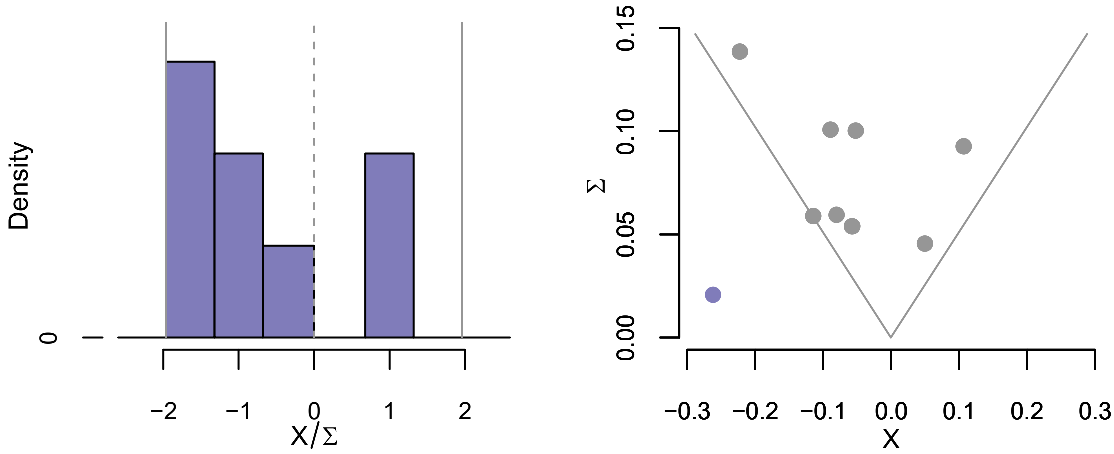
Table D5: RVE with different assumptions

	(1) RVE (τ^2=$\hat{\tau}^2$ and ρ=0.8)	(2) RVE (τ^2=0 and ρ=0.8)	(3) RVE (τ^2=$\hat{\tau}^2$and ρ=0)	(4) RVE (τ^2=0 and ρ=0)
$\hat{\theta}$	-0.0626	-0.0586	-0.0625	-0.1646
	(0.0385)	(0.0400)	(0.0384)	(0.0934)
$\hat{\tau}^2$	0.0060	-	0.0058	-
n (estimates)	34	34	34	34
n (studies)	9	9	9	9

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, only one of the extracted treatment effect estimates is statistically significant at the 5 percent level and the funnel plot does not appear to show any glaring asymmetries. Additionally, the histogram does not appear to show unusual bunching at thresholds marking statistical significance. Accordingly, a formal test for the presence of small-study effects / selective publication (i.e., an Egger test) (Sterne and Egger 2005) does not allow the rejection of the null hypothesis of no publication bias (p-value of 0.521) and no "latent" studies would be imputed in procedures such as "trim-and-fill" (Duval and Tweedie, 2000).

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



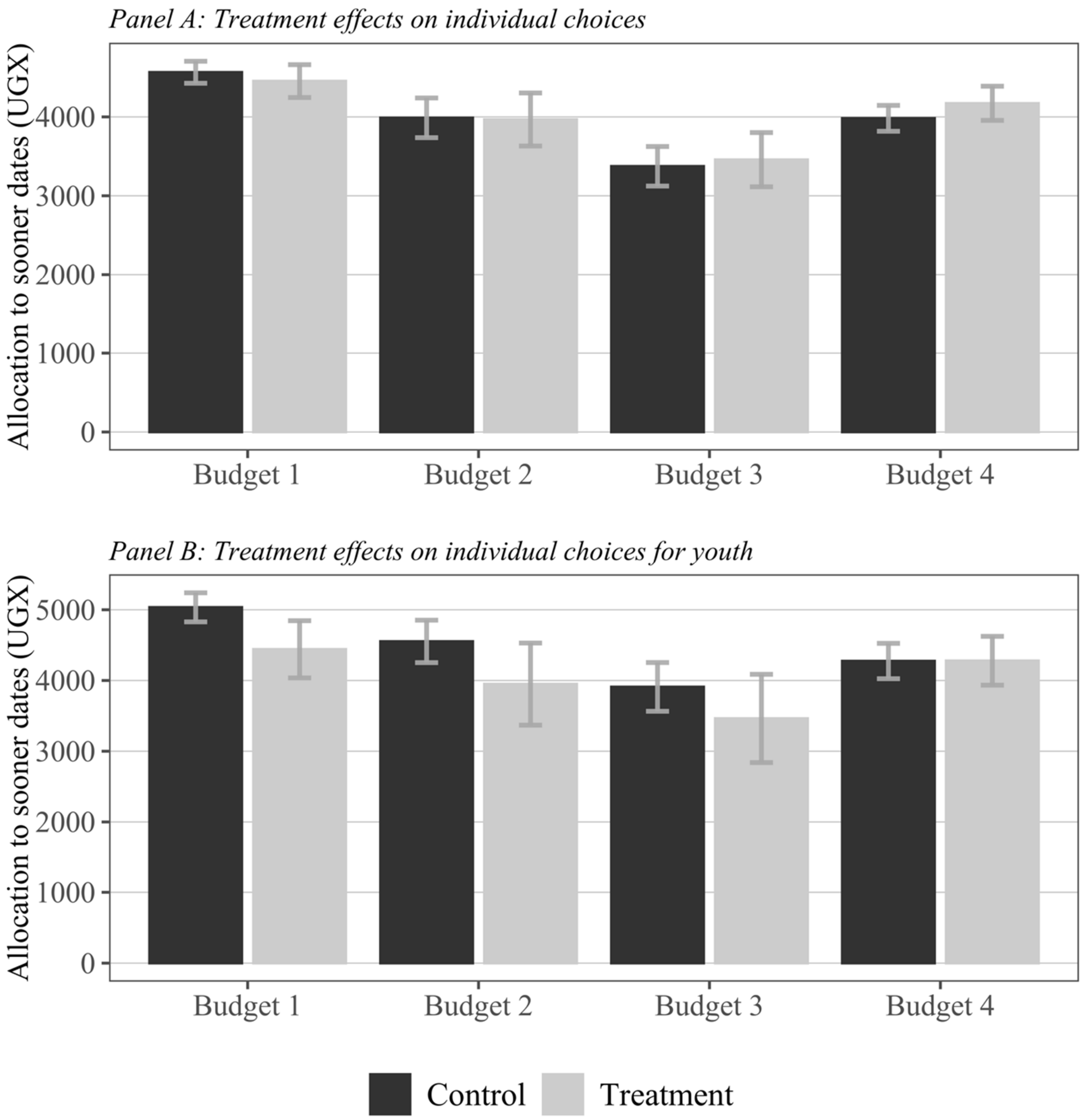
Notes: The left panel shows a binned density plot for the z-statistics (Z= X/ Σ). The solid grey lines indicate the critical values at $|Z| = 1.96$ while the dash-dotted gray line marks $Z = 0$. The right panel plots the extracted estimate (X) against its standard error (Σ). The gray lines mark $|Z|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

APPENDIX E: Field experiment auxiliary results and sensitivity analyses

E1. Average treatment effects on single budget choices

In this section, we investigate whether the treatment affects choices across single budgets. Figure E1 displays the average allocation to sooner payment dates by treatment status with 95% confidence intervals. In line with results shown in Table 4, we observe no significant differences at the 5 percent level between the control and the treatment group.

Figure E1: 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).

E2. Average treatment effects with covariate adjustment

Table E1 accompanies average treatment effects shown in Table 4 by including control variables listed in Table 2. As we observed balance between the treatment and control group, treatment effects are qualitatively similar.

Table E1: Average treatment effects with covariate adjustment

	(1) Allocation to sooner payment	(2) Impatient choice (1/0)	(3) Discount factor $\hat{\delta}$	(4) Present bias $\hat{\beta}$
Treatment	-0.016	-0.022	0.017	-0.006
	(0.024)	(0.032)	(0.014)	(0.004)
Today	0.098***	0.126***		
	(0.014)	(0.017)		
Delay=150 days	0.109***	0.128***		
	(0.013)	(0.016)		
(1+r)=1.2	-0.057***	-0.062***		
	(0.007)	(0.009)		
Treatment*Today	-0.016	-0.014		
	(0.017)	(0.022)		
Treatment*Delay=150 days	0.022	0.034		
	(0.020)	(0.028)		
Treatment*(1+r)=1.2	0.015	0.015		
	(0.010)	(0.012)		
Female (1/0)	0.022	0.042*	0.020	0.006
	(0.019)	(0.024)	(0.013)	(0.005)
Age (years)	-0.002*	-0.002*	0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.000)
Married (1/0)	-0.022	-0.030	-0.012	-0.005
	(0.019)	(0.025)	(0.013)	(0.005)
Catholic (1/0)	0.006	0.011	-0.011	0.001
	(0.018)	(0.024)	(0.012)	(0.004)
No. of children	0.007	0.009	0.002	0.001
	(0.005)	(0.007)	(0.004)	(0.001)
Tertiary education (1/0)	0.007	0.016	0.027	0.012**
	(0.045)	(0.061)	(0.040)	(0.005)
Numeracy (std.)	-0.022**	-0.028**	0.004	0.001
	(0.010)	(0.013)	(0.008)	(0.002)
Illiterate (1/0)	-0.024	-0.025	-0.014	0.007**
	(0.027)	(0.033)	(0.018)	(0.003)
Constant	0.686***	0.691***	1.068***	0.993***
	(0.051)	(0.075)	(0.064)	(0.007)
Observations	4,840	4,840	1,049	1,049
R-squared	0.052	0.050	0.019	0.025
District FEs	YES	YES	YES	YES
Clusters	108	108	108	108

Notes: Dependent variables are the proportion of allocations to sooner payments (column 1), a dummy whether respondents chose the sooner payment option (column 2), estimated individual discount factors (column 3), and individual present bias parameters (column 4). Standard errors in columns 1 and 2 are clustered at the individual and district level, in columns 3 and 4 at the district level. All regressions include district fixed effects. *** p<0.01, ** p<0.05, * p<0.1

E3. Additional results on heterogenous treatment effects

As Table 4 only provides heterogenous treatment effects among youth (i.e. age 24 and below), Table E2 complements results and shows treatment effects for older respondents (i.e., age above 24). While we observe negative treatment effects on impatience and individual discount factors among younger individuals, we detect no difference between the treatment and control group among older respondents. Correlations between design parameters (i.e., whether payments are today, delay is 5 months, and the interest rate rises to 1.2) and impatience measures remain qualitatively the same.

Next, we conduct a similar analysis with a continuous measure of age and consider the linear effects of age and treatment and their interaction. We estimate predicted values of the outcomes "allocation to sooner payment" and "impatient choice" and plot them in Figure E1, in Panel A and Panel B, respectively. Both panels show that the treatment has the intended effect for youth only. The effect on the treated is statistically significantly different from the non-treated up to an age of about 25 years. For older participants there is no effect to be seen.

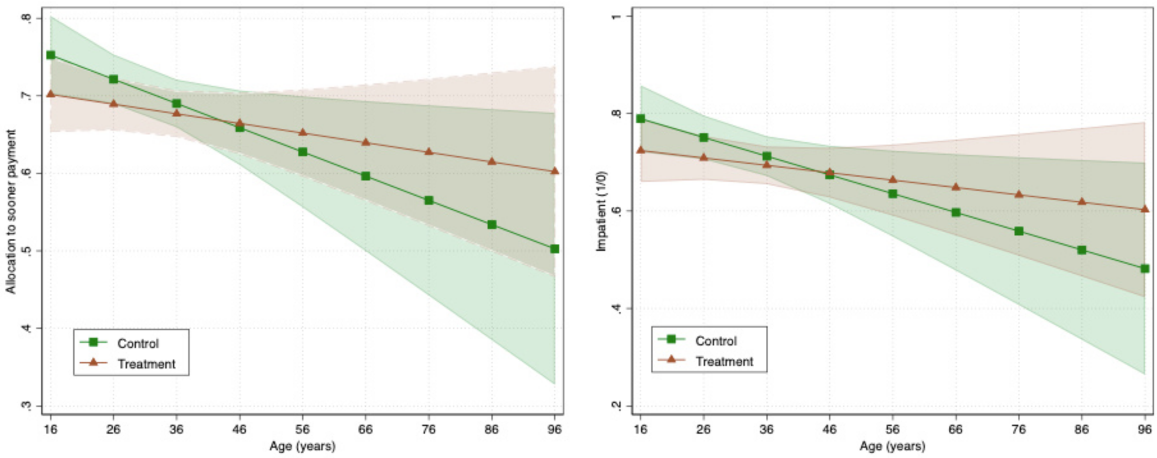
Finally, we explore heterogenous treatment effects by gender and level of education, with results shown in Tables E3 and E4. While female respondents appear to be unaffected by the treatment, we observe negative heterogenous treatment effects on impatience for male individuals (Table E3). Regarding level of education, our data reveal no treatment effects on impatience among respondents with tertiary education and lower education levels.

Table E2: Heterogeneous treatment effects on time preferences by age splits

	Allocation to sooner payments		Impatience		Present bias $\hat{\beta}$		Discount factor $\hat{\delta}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	age ≤24	age >24	age ≤24	age >24	age ≤24	age >24	age ≤24	age >24
Treatment	-0.146***	0.013	-0.172***	0.009	-0.022	-0.005	0.077***	0.004
	(0.045)	(0.027)	(0.058)	(0.035)	(0.021)	(0.005)	(0.028)	(0.015)
Today	0.068***	0.104***	0.089***	0.133***				
	(0.018)	(0.016)	(0.025)	(0.020)				
Delay=5 months	0.064**	0.119***	0.063*	0.143***				
	(0.029)	(0.013)	(0.035)	(0.017)				
(1+r) = 1.2	-0.052***	-0.057***	-0.054***	-0.064***				
	(0.016)	(0.008)	(0.020)	(0.011)				
Treatment * Today	0.006	-0.020	-0.007	-0.015				
	(0.027)	(0.019)	(0.038)	(0.025)				
Treatment * Delay=6 months	0.089*	0.006	0.092	0.020				
	(0.047)	(0.020)	(0.058)	(0.028)				
Treatment * (1+r) = 1.2	0.029	0.012	0.033	0.011				
	(0.021)	(0.011)	(0.025)	(0.014)				
Constant	0.722***	0.640***	0.723***	0.650***	1.014***	0.999***	0.987***	1.101***
	(0.128)	(0.037)	(0.168)	(0.050)	(0.0s17)	(0.005)	(0.060)	(0.069)
Observations	836	4,032	836	4,032	186	869	186	869
R-squared	0.102	0.048	0.104	0.045	0.109	0.026	0.091	0.023
District FEs	YES	YES	YES	YES	YES	YES	YES	YES
Clusters	81	107	81	107	78	106	78	106

Notes: Heterogeneous treatment effects by age on the proportion of allocations to sooner payments, a dummy whether the respondent chose the sooner payment option, as well as estimated individual preference parameters $\hat{\beta}$ and $\hat{\delta}$. Regression estimates in columns (3) and (4) are based on a linear probability model. Standard errors are clustered at the individual and district level. *** p<0.01, ** p<0.05, * p<0.1.

Figure E2: 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 E3: Heterogeneous treatment effects on time preferences by gender

	Allocation to sooner payments		Impatience		Present bias $\hat{\beta}$		Discount factor $\hat{\delta}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Female	Male	Female	Male	Female	Male	Female	Male
Treatment	0.026	-0.096***	0.040	-0.134***	-0.002	-0.016*	0.007	0.030
	(0.031)	(0.031)	(0.038)	(0.045)	(0.005)	(0.009)	(0.018)	(0.020)
Today	0.104***	0.088***	0.133***	0.115***				
	(0.018)	(0.014)	(0.021)	(0.019)				
Delay=6 months	0.125***	0.082***	0.149***	0.094***				
	(0.020)	(0.018)	(0.024)	(0.025)				
(1+r) = 1.2	-0.052***	-0.065***	-0.062***	-0.064***				
	(0.010)	(0.012)	(0.013)	(0.014)				
Treatment * Today	-0.021	-0.006	-0.030	0.012				
	(0.022)	(0.020)	(0.027)	(0.031)				
Treatment * Delay=6 months	0.006	0.049*	-0.003	0.093**				
	(0.026)	(0.029)	(0.034)	(0.040)				
Treatment * (1+r) = 1.2	0.000	0.037**	0.001	0.034*				
	(0.013)	(0.014)	(0.017)	(0.019)				
Constant	0.658***	0.815***	0.672***	0.887***	0.999***	1.008***	1.091***	1.052***
	(0.075)	(0.058)	(0.103)	(0.083)	(0.004)	(0.012)	(0.074)	(0.076)
Observations	2,956	1,884	2,956	1,884	656	393	656	393
R-squared	0.061	0.077	0.059	0.067	0.029	0.077	0.027	0.037
District FEs	YES	YES	YES	YES	YES	YES	YES	YES
Clusters	107	99	107	99	107	94	107	94

Notes: Heterogeneous treatment effects by gender on the proportion of allocations to sooner payments, a dummy whether the respondent chose the sooner payment option, as well as estimated individual preference parameters $\hat{\beta}$ and $\hat{\delta}$. Regression estimates in columns (3) and (4) are based on a linear probability model. Standard errors are clustered at the individual and district level. *** p<0.01, ** p<0.05, * p<0.1.

Table E4: Heterogeneous treatment effects on time preferences by education

	Allocation to sooner payments		Impatience		Present bias $\hat{\beta}$		Discount factor $\hat{\delta}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Non-tertiary	Tertiary	Non-tertiary	Tertiary	Non-tertiary	Tertiary	Non-tertiary	Tertiary
Treatment	-0.013	-0.023	-0.015	-0.051	-0.005	-0.021**	0.021	-0.006
	(0.026)	(0.069)	(0.034)	(0.088)	(0.005)	(0.010)	(0.014)	(0.036)
Today	0.098***	0.099***	0.127***	0.118***				
	(0.014)	(0.034)	(0.017)	(0.042)				
k=150	0.106***	0.132***	0.124***	0.162***				
	(0.013)	(0.038)	(0.016)	(0.052)				
(1+r) = 1.2	-0.057***	-0.053***	-0.059***	-0.088***				
	(0.008)	(0.019)	(0.008)	(0.032)				
Treatment*Today	-0.018	-0.002	-0.019	0.022				
	(0.017)	(0.042)	(0.022)	(0.056)				
Treatment*k=150	0.024	0.004	0.040	-0.010				
	(0.020)	(0.053)	(0.028)	(0.072)				
Treatment*(1+r)=1.2	0.016	0.007	0.010	0.050				
	(0.010)	(0.027)	(0.012)	(0.039)				
Constant	0.703***	0.298*	0.730***	0.053	0.994***	0.990***	1.037***	1.756***
	(0.053)	(0.167)	(0.083)	(0.238)	(0.007)	(0.017)	(0.055)	(0.048)
Observations	4,252	588	4,252	588	925	124	925	124
R-squared	0.054	0.148	0.051	0.160	0.023	0.201	0.016	0.280
Demographic controls	YES	YES	YES	YES	YES	YES	YES	YES
District FEs	YES	YES	YES	YES	YES	YES	YES	YES
Clusters	107	69	107	69	106	63	106	63

Notes: Heterogeneous treatment effects by education on the proportion of allocations to sooner payments, a dummy whether the respondent chose the sooner payment option, as well as estimated individual preference parameters $\hat{\beta}$ and $\hat{\delta}$. Regression estimates in columns (3) and (4) are based on a linear probability model. Standard errors are clustered at the individual and district level. *** p<0.01, ** p<0.05, * p<0.1.

E4. Estimation of treatment effects using data from McKenzie et al. (2022)

McKenzie et al. (2022) test the impact of exogenously inducing higher aspirations and financial knowledge on several financial, entrepreneurial, and non-cognitive outcomes covering a broad group regarding age from 21 to 85. To accompany treatment effects on the amount of control over one's life, they implement an incentivized time preference elicitation task to measure participants' level of present bias. To incorporate their study into our meta-analysis (see Section 2), we use their data and estimate treatment effects on participants' level of impatience, i.e., the proportion of allocated budgets to sooner payment dates. Table E5 shows average and heterogenous treatment effects by age of the combined treatment (i.e., inducing higher aspirations and financial knowledge) on levels of impatience. The analysis reveals that, on average, impatience appears to be unaffected by treatment status. To compare our results on heterogenous treatment effects by age (Table 4) with results from McKenzie et al. (2022), we split their sample by age quintiles, as we use (approximately) the 1^{st} age quintile to analyze heterogenous treatment effects by age in Section 4.3. Similar to the results on average treatment effects, the data of McKenzie et al. (2022) reveal no treatment effects among younger individuals (column 2).

Table E5: Treatment effects on impatience using data from McKenzie et al. (2022)

	Average effect	Heterogeneous effects by age	
	(1)	(2)	(3)
VARIABLES		1st quintile	2nd to 5th quintile
Treatment	-0.026	-0.011	-0.031
	(0.019)	(0.037)	(0.023)
Constant	0.457***	0.386***	0.478***
	(0.042)	(0.076)	(0.059)
Observations	12,600	2,830	9,770
R-squared	0.013	0.014	0.019
Strata FEs	YES	YES	YES
Clusters	94	83	94

Notes: Dependent variable is the proportion of the entire budget (400 Philippine pesos) allocated to the sooner payment date. Treatment takes the value 1 if the respondent received the combined treatment (Aspirations + Knowledge) and 0 if she is in the control group. Standard errors are clustered at the center and individual level. As in McKenzie et al. (2022), all regressions include strata fixed effects. *** p<0.01, ** p<0.05, * p<0.1

References

Alan, Sule, and Seda Ertac. 2018. "Fostering Patience in the Classroom: Results from Randomized Educational Intervention." *Journal of Political Economy*, 126 (5), 1865-1911. doi:10.1086/699007.

Andersen, Steffen, Glenn W. Harrison, Morten I. Lau, and Elisabet Rutström. 2008. "Eliciting Risk and Time Preferences." *Econometrica*, 76 (3), 583-618. doi:10.1111/j.1468-0262.2008.00848.x.

Andreoni, James, and Charles Sprenger. 2012. "Estimating Time Preferences from Convex Budgets." *American Economic Review*, 102 (7), 3333-3356. doi:10.1257/aer.102.7.3333.

Duval, S. and Tweedie, R. (2000). A Nonparametric 'Trim and Fill' Method of Accounting for Publication Bias in Meta-Analysis. *Journal of the American Statistical Association,* 95(449), 89-98.

Laibson, David. 1997. "Golden Eggs and Hyperbolic Discounting." *Quarterly Journal of Economics*, 112 (2), 443-447.

Lührmann, Melanie, Marta Serra-Garcia, and Joachim Winter. 2018. "The Impact of Financial Education on Adolescents' Intertemporal Choices." *American Economic Journal: Economic Policy*, 10 (3), 309-332. doi:10.1257/pol.20170012.

McKenzie, David, Aakash Mohpal, and Dean Yang. 2022. "Aspirations and Financial Decisions: Experimental Evidence from the Philippines." *Journal of Development Economics*, 156, 102846. doi:10.1016/j.jdeveco.2022.102846.

Sterne, J. A. C., and M. Egger. 2005. Regression Methods to Detect Publication and other Bias in Meta-Analysis. In: Publication Bias in Meta-Analysis: Prevention, Assessment and Adjustments, ed. H. R. Rothstein, A. J. Sutton, and M. Borenstein, 99-110. Chichester, UK: Wiley.