The experiment: subject pool

Participants in our study were farmers under contract with (the subsidiaries of) two large tobacco companies in the 2008-2009 growing season. The companies organized the farmers into clubs that range in size from 3 to 43 members. To facilitate timely revisiting, we limited our sample to those farmers located near a main trading center in the town of Mponela (population 13,670), and who lived in six traditional authorities (TAs) in the Dowa and Ntchisi districts. To allow relatively easy access to participants and to facilitate their access to the cash disbursements, we included all farmers in these TAs that were 2008-09 members of clubs in which the median club member lives 25 kilometers or less from the disbursement office, located in Mponela. According to a survey conducted between July and September of 2010 for the savings experiment, participants in this study travelled to the a bank branch in Mponela about once every three months, spending an average of 346.67 MK (US $2.31) per round trip. About 35 percent of these trips combined the visit to the bank with other errands, but there could be other trips to Mponela that did not involve a visit to the bank branch.
Scheduling for the stage one visit was stratified across agricultural zones. Within a zone, the order in which clubs were visited was randomly assigned. Scheduling was on a club-by-club basis in order to facilitate field work since members of the same club often live within the same village or in neighboring villages.

2 Variable definitions

The key dependent variable we analyze is change in sooner allocation upon revisiting (MK), which is the respondent’s allocation to later period \((t=91)\) in the revisit survey minus his/her allocation to later period \((t=91)\) in the baseline survey. All other variables are from either the baseline survey, the revisit survey, or from administrative (project) data.

2.1 Variables collected in baseline survey

Present-biased ratio is fraction of pairs of choices in which a respondent faced the same interest rate but the allocation to sooner in near time frame is more than 100MK larger than the allocation to sooner in far time frame. In all regressions this variable excludes the implemented interest rate from the calculation, but summary statistics are also provided for all choices including the implemented interest rate.

Future-biased ratio is fraction of choices where the allocation to sooner in the near time frame is more than 100MK lower than allocation to sooner in far time frame (again comparing choices in near and far frames for same interest rate). In regressions this variable excludes the implemented interest rate from the calculation.

Fraction sooner is the total number of tokens allocated to sooner in any of the choices, divided by the total number of tokens to be allocated (20 in each of the ten choices). In regressions, this variable excludes the choice at the implemented interest rate in the calculation.
Fraction of decisions consistent with law of demand is the fraction (out of 8) of pairs of choices adjacent in interest rates where allocation to later rises in rate of return.

More elastic in the far time frame is an indicator for whether a respondent’s choices are consistent with a greater responsiveness to the interest rate in the far, relative to the near time frame. For each respondent, we first calculate four values of the change in the share of consumption allocated to later associated with each of the four incremental increases in the interest rate. We then take the average of these four changes in the consumption share within time frame and use this as a measure of the elasticity of intertemporal substitution within time frame. We then create an indicator that takes on the value 1 when that elasticity is (at least 0.1) larger in the far time frame than in the near and 0 otherwise.

More elastic in the near time frame is an indicator defined as above, except that it takes the value 1 when the elasticity of intertemporal substitution is (at least 0.1) larger in the near time frame than the far time frame, and 0 otherwise.

Spouse minus own allocation to sooner (MK) is spousal allocation to the sooner period minus corresponding allocation for respondent, for all choices excluding the randomly-chosen implemented choice.

Implemented interest rate is rate of return to waiting 30 days for funds for the respondent’s randomly-selected choice (out of 10 choices made).

HH total in bank is total value of balances in formal banks reported at baseline (in thousands of MK).

HH total cash is total value of cash held at home reported at baseline (in thousands of MK).

HH items is total value of physical household items and assets owned, reported at baseline (in thousands of MK).

HH animals is total value of livestock owned, reported at baseline (in thousands of MK).

Total HH wealth is sum of HH total in bank, HH total cash, HH items,
and HH animals (in thousands of MK).

2.2 Variables collected in revisit survey

Indicator for death in family takes on the value 1 if a death is registered in respondent’s own household from the baseline survey to the revisit survey.

Shortfall in expected household income is expected household income minus actual household income, where expectation is reported in baseline and actual is reported in revisit survey. Expected income is measured at baseline and refers to April 1, 2010 and actual income is measured at revisit and refers to income since the beginning of February 2010. Thus, the reference periods for the two questions cover approximately the same time frame.

2.3 Variables from administrative (project) data

Days to first disbursement at revisit (targeted) is the randomized number of days prior to the first far time frame disbursement date at which the revisit was targeted to arrive. Randomization assigns days from 2 to 16 in unit intervals with equal probability.

Days to first disbursement at revisit (actual) is actual number of days prior to first far time frame disbursement that revisit survey is carried out.

Indicator for days to first disbursement (targeted) \( \leq 6 \) equal to 1 if days to first disbursement at revisit (targeted) is less than or equal to 6, and 0 otherwise.

3 Supplementary analyses

3.1 Baseline balance

The two randomizations carried out in stage one – the implemented choice, and the revisit date – generated exogenous variation the interest rate that applied to the revision decision and in the targeted revisit date itself. We
provide here an analysis of balance of baseline respondent characteristics vis-a-vis these two exogenously determined variables.

Appendix Table 1 presents results of regressions of several baseline variables on an indicator for targeted days to first disbursement being less than or equal to six (Panel A) and on the implemented interest rate (Panel B). (The specification of the target lag as an indicator is chosen to be consistent with the specification in the main regressions of Table 5, and is discussed further below.) In the top panel, the coefficient on the randomized right-hand-side variable is not statistically significantly different from zero for 11 out of the 14 dependent variables, and in the bottom panel it is not significant for 10 out of 14 dependent variables. Having four out of 14 coefficients turn up significant is close to what would have occurred by chance, and all these variables (and others) will be included as controls in the regression analyses below. Results are similar when these regressions are run with alternative specifications for the randomized right-hand-side variables, such as linear days to first disbursement or dummies for each discrete implemented interest rate.

3.2 Determinants of allocations to later in stage one

We present here analysis of the determinants of the stage one allocations. As highlighted in the discussion of Table 3 in the main text, these allocations exhibit substantial heterogeneity. Appendix Table 2 shows the results of a regression of the difference between the natural log of the allocation to sooner and later on the rate of return and observable characteristics of the participants. Columns 1 and 2 use the sample for the near frame “1 vs. 31 days”, columns 3 and 4 use the sample for the far frame “61 vs 91 days” while column 5 pools both samples. Conditional on the rate of return, those with more wealth at baseline allocate more to later, as do those with more relatives who live in the village although the changes in consumption implied by a change in the number of relatives are small. There is also weak evidence that those who scored higher on the word recall test and the financial literacy
questions allocate more of their endowment to later, but that those who score higher on the Raven’s test allocate less of their endowment to later. Measured in this way, we find no evidence that education has a significant relationship with patience in this domain. The last row of the regressions report the p-value associated to an F-test that all household characteristics (excluding the interest rate) are jointly different from zero. The p-value in column 5 (pooled sample) is 0.01. We note however that given the large number of regressors and the few coefficients with conventional statistical significance, these results are only suggestive.

The estimates in the table have the advantage of being easily interpreted in terms of a simple economic model of intertemporal choice. If we adopt the model in Section 2.1.1 of the main text and assume time-invariant, isoelastic utilities \( u(c) = \frac{c^{1-\rho}}{1-\rho} \), then the coefficient on \( r \) is an estimate of \( \frac{1}{\rho} \). The estimates in the table have the advantage of being easily interpreted in terms of a simple economic model of intertemporal choice. The disadvantage of this specification is that it excludes corner allocations, where the log of consumption at one time or the other is undefined. Analysis of a levels specification gives qualitatively similar results (available upon request) with more evidence of a positive correlation between word recall and the willingness to postpone consumption.

### 3.3 Alternate specifications of target lag

In all regressions of Table 5, the variable for targeted days to first disbursement upon revisiting is specified as an indicator variable for six days or less. Here we elaborate on the justification for this specification.\(^1\)

First, we note that specifying the variable as a linear relationship leads to

\(^1\)All specifications use the targeted lag between the revisit and disbursement because the actual lag is endogenous. Eighty five percent of those in the revisit sample are visited on exactly the targeted date. For the remainder, 84 percent are revisited within two days of the target date and the maximum gap between the targeted and actual revisit date is six days. The correlation between the delay in revisits and the assigned revisit date is not statistically different from zero.
a similar result. If we replace the indicator target lag variable with a linear variable for targeted days to first disbursement in the specification of Table 5, column 6, the coefficient on the linear target lag variable is -9.21 and has a standard error of 5.33 (significant at the 10% level).\footnote{All other coefficients in the regression remain essentially identical.}

It turns out, however, that the linear relationship just described masks the fact that the underlying relationship between the target lag and revisions is better described as a non-linear function. To see this, we again estimate the specification of Table 5, column 6, but now we specify the target lag as separate indicator variables for each of the 14 distinct values of the target lag from two to 15 days prior to first disbursement (the omitted indicator is 16 days). In Appendix Figure 4 we graphically present the estimated coefficients on the target lag indicators. The solid line graphs the series of point estimates, and the upper and lower dashed lines bound the upper and lower 95% confidence intervals.

Point estimates on the indicators for days two through six are all large in magnitude, each exceeding 100 MK, and show no obvious time pattern. In contrast, nearly all the coefficients on the indicators for higher target lags are substantially smaller in magnitude and several are below or just at zero. (The exception is the coefficient on the indicator for 11 days, 141 MK. This is probably a chance occurrence, and the coefficient is not statistically different from zero at standard confidence levels.) Due to lack of power, most of the individual coefficients are not statistically significantly different from zero at conventional levels (although the coefficients on the indicators for days four and six are statistically significantly different from zero at the 10% level).

All told, the relationship appears to be best summarized by a step function with a positive effect for days two to six prior to disbursement, and zero effect thereafter.
3.4 Future bias vs. “present” bias

The analysis in the main text focuses on the predictions of quasi-hyperbolic discounting models with \( \beta \leq 1 \). This is natural given the laboratory evidence and well-developed theory surrounding them and other models of present-bias. Future bias is, however, also possible. Models of future bias would imply that respondents would shift allocations toward later as the intertemporal tradeoffs draw near. Appendix Table 4 considers this possibility, first replacing the fraction present biased variable with a future biased variable defined analogously in column 1 and in column 2 by including the variable “Fraction Future biased” to the specification in column 6, Table 5. Both regressions also include an indicator for “more elastic in the near time frame” and an interaction term between this indicator and fraction future biased. The rest of the variables are identical to those of Table 5, column 6.

Contrary to a theory that attributes future-biased static preference reversals to non-constant time discounting, the coefficient on the main effect of fraction future-biased choices is actually positive in both columns. The coefficient is not precisely estimated, however, and we cannot reject a null hypothesis of no effect, or even a moderate-sized negative effect, at conventional levels of significance. Summing the coefficients on this main effect with its interaction with “more elastic in the near horizon” we again find that those who exhibit static preference reversals that can be easily reconciled with time-specific marginal utilities of consumption exhibit no time inconsistency on average. Unlike the results from Table 5 investigating “present”-bias, however, our inference is limited by the imprecision of the point estimates. One interpretation of these findings is that the future-biased static preference reversals capture predictable changes in the marginal utility of income, more than some form of non-constant discounting. More generally, the future-biased preference reversals appear to be driven by mechanisms that do not induce time-inconsistency.
3.5 Attrition

We attempted to revisit 722 individuals with complete baseline data. We were successful at revisiting 661 (91.6%). This high revisit success rate helps ameliorate concerns over selection bias, but it is still important to ascertain the extent to which key right-hand-side variables are related with attrition, and to think through any resulting directions of bias.

Appendix Table 5 presents regressions of an indicator for inclusion in the sample on key right-hand-side variables. The sample is the 722 individuals we attempted to revisit, so the mean of the dependent variable is the revisit success rate, 0.916. Individuals targeted for revisit six days or less prior to first disbursement are 10.8 percentage points less likely to be included in the revisit sample. This reflects the simple fact that our survey team had less time to find individuals whose target revisit date was close to the disbursement date.\(^3\),\(^4\)

An important question is whether the key results (in Table 5) on the impact of days to first disbursement on revisions could be driven entirely by selection, since the variable is statistically significantly related to revisit success. Given the sizes of the effects in Table 5, this turns out to be implausible.

Consider the coefficient in column 6, Table 5 on the indicator for targeted days to first disbursement less than or equal to six, 124.629. This variable leads to 10.8 percentage points lower inclusion in the sample. For differential selection on this variable to fully explain the coefficient in column 6, Table 5, revision towards sooner of individuals selecting out of the sample due to having days to first disbursement less than or equal to six would have to have been lower by 1,118.20 MK.\(^5\) A change in revisions of this magnitude

\(^3\)The closest randomized target date was two days prior to first disbursement, and the cutoff date for actual revisits was set at 1 day prior to first disbursement. Revisits on or after that date would be nonsensical, since the "sooner" disbursement could already have been made (if the respondent redeemed the voucher immediately on the disbursement date).

\(^4\)In addition, individuals with higher word recall are less likely to be included in the sample. Two additional words recalled (about one and a half standard deviations) leads to a 3 percentage point lower likelihood of revisit success. Revisit success was higher for individuals who are younger and who had lower baseline wealth.

\(^5\)Let there be two types of individuals: type 1, who we always successfully revisit, and
would be extremely large, amounting to roughly the difference between the
10th percentile (-600 MK) to the 83rd percentile (500 MK) of the revision
distribution, or about two standard deviations. It is highly unlikely that all the
individuals selecting out of the sample would have had revisions this different
from other individuals who were successfully revisited.

While it is very unlikely that the estimate of the impact of days to disburse-
ment from column 6, Table 5 is due entirely to selection, selection may still lead
to bias in this estimate. In Appendix Table 6 we present results of an exercise
intended to bound the size of this possible bias, running regressions analogous
to that of column 6, Table 5 but where observations that were previously not
included due to attrition are now included, and where we make several different
assumptions as to the value of the dependent variable for the newly-included
observations. At the top of each column is our assumption regarding revision
on the part of attrited observations. Across columns 1 through 7, we assume
initial allocations to sooner are revised in the amounts (respectively) of 600,
400, 200, 0, -200, -400, and -600. Looking across columns, the stability of co-
efficient estimates on particular independent variables provides a sense of the

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6 The only other difference vis-a-vis the regression in column 6, Table 5 is that we exclude
the shock variables “death in family” and “shortfall in expected household income” from
the right-hand-side of the regression, since these were also measured upon revisit.

7 We of course do not allow revisions to go beyond corners, imposing the restriction that
revised allocations to sooner must stay within the [0,2000] range. For example, in column 1,
where we are assuming that revised allocations are 600 MK higher than attrited individuals’
initial allocations, if an individual initially allocated 1700 MK to sooner, we only allow the
revised allocation to sooner to go to 2000 MK (not 2300 MK).
sensitivity of coefficients to a range of assumptions on how attrited individuals would have revised their allocations.

When assuming positive revisions toward sooner for the attrited observations, the coefficient on the indicator for days to first disbursement less than or equal to six becomes larger in magnitude, reflecting the fact that this variable is positively correlated with attrition. For the same reason, assuming negative revisions toward sooner for attrited observations leads the coefficient on this variable to become smaller in magnitude. The results indicate that the coefficient on the indicator for days to first disbursement less than or equal to six in Table 5 is robust to a wide range of assumptions on attriter revisions, except when attriter revision is assumed to be as much as -600: in this case the coefficient declines enough in magnitude to become statistically insignificant. We view an assumption that attriters revise as much as -600 MK vis-a-vis their initial allocations as farfetched; this change amounts to more than one standard deviation of the revision distribution.

3.6 Males vs. females

In Appendix Table 7 we explore whether estimated effects differ across males and females in the sample, estimating regressions analogous to column 6, Table 5, but where the sample is restricted to females (column 1) and males (column 2). We also present p-values of the F-test that coefficients on each presented right-hand-side variable differ across the female and male regressions.

Owing to smaller sample sizes, the standard errors on the key point estimates are relatively large. As a result, while we can sometimes reject a null hypothesis of no relationship (e.g., on the days to first disbursement indicator variable for male), mostly the coefficients cannot be distinguished statistically from zero.

In addition, for nearly all variables, coefficients are not statistically significantly different across the male and female samples, with a few exceptions.
The coefficient on the Raven’s test score is negative and statistically significantly different from zero among males, and is significantly different from the corresponding (positive) coefficient among females. In the female sample, coefficients on the schooling indicators are negative (indicating that higher schooling leads to less revision towards sooner), statistically significantly different from zero, and statistically significantly different from the corresponding coefficients in the male regression (or nearly so). The male coefficients on schooling, on the other hand, are positive, but none are statistically significantly different from zero. Finally, the coefficient on the death in the family indicator is large and positive for females, smaller in magnitude and negative for males, and marginally statistically significantly different across the male and female regressions at the 10% level.

3.7 Consistent vs Inconsistent individuals

In Appendix Table 8 we explore whether the sample contains individuals that did not understand the experiment. We replicate Table 5 excluding those individuals that are inconsistent in 3 or more pairs in Table 2. If these individuals did not understand the experiment, there would be measurement error and the estimates in Table 5 would suffer from attenuation bias.

We find that most of the results hold, but the coefficients of interest are not larger in absolute value, suggesting that there is no attenuation bias and that the results are not driven by people who simply did not understand the experiment.

3.8 Structural estimates of $\beta$

In this subsection we describe the structural estimation of the discount factors that are included as regressors in column 6 of Table 5. We follow the theoretical framework of Section 2 and posit a flexible “$\delta - \beta$” model that allows the curvature parameter of the utility function to differ by time frame, as in the example of Section 2. Therefore $u_1(c) = u_2(c)$ and $u_3(c) = u_4(c)$. We
assume that the utility function is either CRRA or CARA and we estimate the
discount factors and curvature parameters from the experimental choice data
using Non Linear Least Squares, taking into account corner choices. Given the
concerns raised in Section 3.7, we perform the estimation using either the full
sample or the sub-sample of those who appear consistent.

Because the structural estimation allows to simultaneously estimate the
discount factors and curvature parameters, while taking into account corner
choices, we run several specifications of Table 5 that do not include “Fraction
present-biased”, “Fraction of all tokens to sooner” nor the indicator of “More
elastic in the far time frame”.

A natural hypothesis suggests that the more present-biased the individual
is, as indicated by a lower estimated $\beta$, the larger the revision towards sooner
will be upon revisit. Put differently, the coefficient on the discount factor $\beta$
should be negative and significant. The results reported in Appendix Table 9
suggest that the estimates all have the correct sign but are small in economic
magnitude and tend to be imprecisely estimated.

These estimates are interesting because they underscore the advantages and
disadvantages of this structural approach. The advantage, already mentioned,
is that neither the proxies for preference reversals, corner choices and non-
stationary utility functions nor their interactions are included in the reduced
form analysis. The approach has the disadvantage of relying on functional
form assumptions, and if these fit the data poorly the estimated discount
factor may not have predictive power.

4 Simulations of stochastic choice

In this section we assess the adherence of subjects’ optimizing behavior
to the canonical model of Section 2.1.1 by comparing their choices to those
of hypothetical subjects that choose randomly. Given that more than two
thirds of individuals choose allocations that deviate at least once from the law
of demand, and that more than 90% make at least one different allocation in the “near” compared to the “far” time frame, random choice is a useful benchmark.\textsuperscript{8}

The interest in random choice model is twofold. First, it can be used to alleviate concerns about the low levels of literacy of the subject pool. In particular, we assess whether our results can be generated by individuals that do not understand the experimental protocols and that in the extreme, choose randomly. Second, as we explain in more detail below, we use the results from the random choice model to justify how we deal with implementation error in the analysis of Table 5.

We generate 1,000 random samples of 661 subjects who choose allocations randomly. That is, each possible allocation ([2000,0], [1900, 100(1+r)], \ldots [0, 2000(1+r)]) is chosen with probability 1/21. To construct spousal controls, individuals are matched with their real life spouses and their (random) choices are used to generate the relevant variables. For each sample of random choices we run the specifications of column 6 of Table 5 and of Appendix Table 4 and for each coefficient in the regression, we report its mean and construct the 95\% confidence interval non-parametrically using the 25th and 975th coefficient.

Appendix Table 9 reports the results. We compare coefficients obtained using real data (odd numbered columns) to simulated or random choice data (even numbered columns). Columns 1 and 2 compute the “Fraction Present Biased” variable using all pairs, including the one associated with the implemented interest rate. The variable “Spouse minus own allocation to sooner” is also computed using all interest rate pairs. Columns 3 and 4, in contrast, exclude the pair of the implemented interest rate in both variables. All regressions include all other right hand side variables in the respective comparison.

\textsuperscript{8}Other models based on changes in expected income between the “near” and “far” time frame would only be consistent with individuals being either always or never dynamically consistent. If an individual expects a windfall between the near and far time frame, he or she would appear more patient in the far time frame under all interest rates. These models cannot explain why some individuals are dynamically consistent under some interest rates but not others.
regressions (column 6 of Table 5 and the single regression in Appendix Table 4). They are not reported since by definition they are uncorrelated with the random choices.

Comparing the results in columns 1 and 2 of Appendix Table 10 we see that when all pairs are included, the coefficient on “Fraction Present Biased” using simulated data is more than twice as large than the coefficient when using the real experimental data. Both coefficients are large and significant at conventional levels, suggesting that a null that the coefficient is zero is insufficiently discerning.

As mentioned in the text, the reason for the large coefficient using simulated data is a mechanical relationship between “present” bias-like behavior under the implemented interest rate and revisions to the sooner period. Intuitively, in the second stage of the experiment, an individual who exhibits “present” bias will, by definition, have chosen in the far time frame an allocation to sooner that is lower than that of the near time frame. Thus, even under random choice, the probability that the revised allocation to sooner is larger than the (below average) original allocation is relatively high – hence the mechanical positive relationship between revision to sooner and “present” bias. An analogous argument explains the mechanical negative relationship between revisions to sooner and “future” bias.

If implementation error is independent across choices, however, there should be no relationship between random choices under interest rates other than the implemented one and revision behavior under the implemented interest rate. This therefore suggests the construction of the variables “Fraction Present Biased” and “Spouse minus own allocation to sooner” excluding the choices under the implemented interest rate.

Indeed, the coefficient on “Fraction Present Bias” in column 4 is small in magnitude and statistically insignificant (albeit with a rather large confidence interval). This small coefficient stands in contrast with that of column 3, replicating column 6 of Table 5.

Under random choice, individuals that appear more elastic in the far time
frame do not necessarily revise allocations towards sooner. The point estimates in columns 2 and 4 of the indicator “more elastic in the far time frame” are small and the confidence interval large suggesting that they are not significantly different from zero. The interaction between the indicator and “Fraction Present Biased” is also small and insignificant and a test that the sum of coefficients is different from zero yield again confidence intervals that include zero. In this sense, the simulation cannot generate the result found with real data that the link between present bias and static preference reversals is only found among individuals with stable marginal utility of consumption across time frames.

Columns 5 to 8 study the relationship between future bias and revision behavior. As expected, column 6 displays the mechanical negative relationship between “Fraction Future Biased,” computed using all interest rates and the change in the allocation to sooner. However, this relationship disappears in column 8 when the choices under the implemented interest rate are excluded. As in Appendix Table 4, “future” biasedness and differences in marginal utilities across time frames cannot explain revision behavior.