Revising Commitments: Field Evidence on the Adjustment of Prior Choices

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Abstract

We implement an artefactual field experiment in rural Malawi to study revisions of prior choices regarding future income receipts. This allows examination of intertemporal choice revision and its determinants. New tests provide evidence of self-control problems for some participants. Revisions of money allocations toward the present are positively associated with refined measures of present-bias from an earlier survey, and with the randomly assigned closeness in time to the first possible date of money disbursement. We find little evidence that revisions of allocations toward the present are associated with spousal preferences for such revision, household shocks, or the financial sophistication of respondents.

Keywords: commitment, hyperbolic preferences, lab experiment, consumption smoothing, self-control, Malawi

JEL codes: D81, D91, O10

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The well-being of individuals, especially those who live close to subsistence, depends importantly on the ability to make and execute intertemporal plans. The world over, however, individuals close to subsistence appear to leave consumption unsmooth, save at a low rate, or fail to use inexpensive agricultural and health inputs.

While these observed choices may be optimal given the constraints that individuals face and the incompleteness of markets, researchers have suggested that they may be the result of self-control problems.[1]

In this paper, we investigate several potential sources of failure to pursue intertemporal plans by studying why choices about future consumption are revised. The paper makes two contributions. First, we test for the presence of self-control problems using a novel and robust method. Second, we provide a quantitative analysis of this and other motives for the adjustment of prior choices.

Applied research typically models self-control problems as the result of present-biased (quasi-hyperbolic) time discounting. This modelling strategy is founded, in part, on evidence of non-constant time discounting. Several studies can be interpreted to show that time discount rates decline as tradeoffs are pushed into the temporal distance.[2] In particular, many experimental studies document "static" preference reversals: subjects choose the larger and later of two rewards when both are distant in time, but prefer the smaller and earlier one as both rewards draw nearer to the present.

Interpreted as present-biased time discounting and assuming time-separable preferences, these static preference reversals imply time-inconsistency: the choices (plans) that a person makes now about consumption at a later date are different from the choices she would make when that date arrives.[3] Self-

[1]Some of the seemingly puzzling evidence regarding intertemporal choices of the poor were first summarized by Theordore Schultz in his 1979 Nobel Prize lecture and more recently in Banerjee and Duflo (2011).

[2]Ainslie (1992), Thaler (1991) and Loewenstein and Elster (1992) provide reviews.

[3]Early contributions include Phelps and Pollak (1968), Laibson (1997) and O'Donoghue and Rabin (1999). See DellaVigna (2009) and Bryan, Karlan and Nelson (2010) for recent

control problems and a demand for commitment may thus emerge.[4]

However, until recently there have been no studies in the literature of whether static preference reversals are associated with time-inconsistency. To our knowledge, Halevy (2015) is the sole experiment in which the revision of previous decisions is a variable of interest. Augenblick et al. (2015) study revision of prior choices, focusing on dynamic inconsistency in monetary versus real effort choices. Otherwise, existing work has either studied the static preference reversals themselves, the stability of time preferences, or the relationship between static preference reversals and the demand for commitment.

While demand for commitment is, like time-inconsistency, a signature prediction of (quasi-)hyperbolic discounting models, studies that focus on the demand for commitment may understate self-control problems either because commitment devices are poorly designed and thus not demanded (Beshears et al., 2011) or because demand for commitment requires some sophistication on the part of respondents: individuals who are naïve about their self-control problems should not want to limit their future choices.

Testing the central mechanism linking static preference reversals to self-control problems – by investigating the correlation between them and the revision of prior choices – is important because the static reversals can be driven by different factors.[5] For example, static preference reversals may reflect predictable changes in the marginal utility of consumption.[6] Alternatively, static preference reversals may reflect inattention, confusion about tradeoffs, or responses to perceived experimenter demands.[7] Finally, even if preferences un-

reviews of empirical applications.

[4]Ashraf et al (2006), Duflo et al (2011), Dupas and Robinson (2011), Brune et al (2016).

[5]Halevy (2015) distinguishes between time-consistency, time-invariance, and stationarity, making clear that static preference reversals are identified with non-stationarity but need not imply time-inconsistency.

[6]This observation has been made by Andersen et al. (2008), Andreoni and Sprenger (2012) and Ericson and Noor (2015), who note that proper inference about time discounting requires information about the curvature of the utility function.

[7]Benjamin et al. (2013) document correlations among test scores, cognitive load, and short-term patience.

der commitment were well-described by changing time discount rates, simply making a plan may limit self-control problems.[8] Individuals making static preference reversals for any of these reasons need not exhibit time-inconsistency.

In addition, there may be other explanations for the revision of prior choices. For example, individuals from close-knit communities in developing countries are often obliged to share their income with relatives and friends, and such social pressure may prevent individuals from pursuing privately optimal choices and the revision of previous decisions.[9] Unexpected events could also motivate revisions to otherwise optimal consumption paths. Finally, individuals could simply make mistakes in their original decisions, and seek to revise them later. Our analysis explores the role of these three alternative explanations.

From a policy standpoint, it is important to understand what drives revision behaviour because it will influence the design of commitment devices and their welfare impact. If social pressure, shocks, or mistakes affect revisions, then commitment devices could be designed either to shield resources from one's social network (while maintaining access for oneself), or to allow access in case of emergency or error. In contrast, if self-control problems are important then commitment devices should protect resources from one's future self.

To assess the drivers of revision behaviour, we implement an artefactual field experiment where the key dependent variable is revision of a previous decision under commitment. Our sample consists of several hundred wife-husband pairs in rural Malawi. We elicited intertemporal choices by adapting Andreoni and Sprenger's (2012) convex time budget method, with large real

[8]Making plans or setting goals can affect self-control and self-efficacy (Bandura 1997, Ameriks et al. 2003). This idea is also consistent with economic models of costly self-control such as Gul and Pesendorfer (2001), Ozdendoren et al. (2012), and Fudenberg and Levine (2012), in which consumers may both seek commitment and, yet, not always exhibit time-inconsistency.

[9]See, e.g., Platteau (2000), Maranz (2001), Anderson and Baland (2002), Ligon et al (2002), Hoff and Sen (2006), Ashraf (2009), Baland et al (2011), Jakiela and Ozier (2011) and Schaner (2015).

stakes (roughly a month's wages). Subjects made several choices regarding an allocation of money to be disbursed at two points, 61 and 91 days, in the future. A subset of these subjects was revisited some time prior to $t = 61$ and given the opportunity to revise the allocation between $t = 61$ and $t = 91$. A measure of this revision is our dependent variable. We examine correlates of this revision corresponding to each of the four potential determinants of revision outlined above.

The experiment also provides a complementary test of quasi-hyperbolic discounting models. In those models, average revisions toward sooner should be larger when the time lag between the revision decision and the first disbursement ($t = 61$) is sufficiently small. We randomized the number of days prior to $t = 61$ when each subject had to make the revision decision.

Analysis of initial allocations indicates that they usually, but not always adhere to the law of demand; individuals typically allocated more income to later periods when offered higher rates of return to waiting. We interpret this to indicate that most subjects understood the choices made but that some preference reversals may simply reflect confusion. We also find that "static" preference reversals are frequent, but only slightly more likely to be "present"-biased (as opposed to "future"-biased).[10]

Turning to revision behaviour, we find that revisions are common, often substantial in size, and shift money both sooner and later. We find some evidence that time-inconsistency induces these revisions: subjects shift more money toward sooner when: (1) their initial allocations are "present"-biased, and (2) the time lag to disbursement is shorter (when the revision decision is made six or fewer days prior to day $t = 61$). Importantly, the relationship between "present"-biased and revisions toward sooner is concentrated among individuals that do not exhibit anticipated changes in the marginal utility of consumption. This finding is significant because it demonstrates, in a developing context, that predictable changes in the marginal utility of consumption

[10]This finding contrasts other studies using the multiple price list method, but is consistent with Andreoni and Sprenger (2012).

may drive the observed static preference reversals. Put differently, we find evidence of a reason why not all "present"-biased preference reversals are the result of time-inconsistency.

We find no evidence that social pressure affects revision decisions in a meaningful way: respondents' revisions are not much higher when one's spouse's sooner allocations are larger than one's own, or when they have many other relatives in the village. We also find little evidence that shocks or financial sophistication (a proxy for mistakes) strongly predict revisions (although the impact is less precisely estimated).

The next section presents details of the experimental design, the sample of participants and the experimental setting. Section 2 presents the theoretical framework and derives the testable implications. Then, Section3 describes the choices under commitment and the drivers of revision behaviour. Section 4 clarifies our contribution to the related literature, and section 5 concludes.

1 The Experiment

The experiment proceeded in two stages. In stage one, we elicited intertemporal choices under commitment. Husbands and wives each separately made several independent choices about the allocation of a substantial amount of money over time. Each choice was an allocation of an endowment between two periods, one "sooner" and one "later." In stage two of the experiment, some households were revisited on a randomly selected day in the two weeks prior to the arrival of the first disbursement of their money in the far period and given an opportunity to revise their original far-period allocation. Surveys at both stages measured household wealth, income, and expenditures as well as the participants' expectations for each of these variables.

1.1 The Setting

Rural Malawi has a number of advantages as a setting for experimental study of intertemporal choice. Most important, financial markets are thin especially during the rainy season when the experiment was conducted. During this lean period, study participants have virtually no cash, and borrowing is not merely expensive but it is often impossible. Similarly, short-term saving can be difficult due to limited access to banking institutions, and familial or social demands for what appears like excess cash.[11]

This financial market incompleteness is important because it reduces smoothing opportunities that confound efforts to elicit time preferences in developed economies.[12] When financial markets are thick and transaction costs low, answers to the questions asked in typical time-preference experiments should, in theory, reflect only the market rates of return participants face, and reveal little about their preferences (Fuchs, 1982, Chabris, et al. 2008).[13] Augenblick et al. (2015) address this issue by giving respondents in a US university campus choices over leisure that is hard to smooth instead of monetary prizes.

Our study location also has some disadvantages. Poor infrastructure makes the logistics of a large-scale experiment challenging. In addition, participants have low levels of formal education and may therefore find the experiment difficult to grasp. We therefore evaluate the consistency of participants' choices with a basic prediction of standard models of economic decision-making: the law of demand. The degree of consistency with the law of demand will provide

[11] In Malawian survey data, only 26 percent of respondents use a formal financial product, and around 60 percent had never heard of a savings account (FinScope, 2008).

[12] Grain and other consumption goods in store are used to smooth consumption, but only partially. We rely on the fact that stakes are high and that they involve cash.

[13] To illustrate, suppose that outside of the lab a participant can borrow or save at market rate r without transaction costs. A typical experiment asks the participant to choose between $\$x$ sooner or $\$(1+r_e)\,x$ later, where r_e is the rate of return implied by the later option. The participant may view this as a choice between Option A, $\$x$ sooner and access to the interest rate r and Option B, $\$(1+r_e)\,x$ later and access to the interest rate r. If $r_e > r$, then the set of allocations under option B contains the set under option A, and more. Thus, for any monotonic preference ordering, option B is preferred. Analogously, if $r > r_e$ then is A preferred.

a measure of participants' understanding of the trade-offs involved in their decisions.

1.2 The Sample

Participants in the experiment were recruited in January and February 2010 from a population of rural households in central Malawi who were growing tobacco as their main cash crop. Participants were a subset of respondents who were participating in another simultaneous experiment on savings.[14] To be eligible for inclusion in this experiment, respondents had to be located within 25 kilometers of the town of Mponela, to facilitate our cash disbursements. Due to our interest in interactions within the household, we further restricted our sample to farmers who were part of a married couple.

These sample restrictions left us with 1,268 targeted households. A total of 1,071 households (84.4%) and 2,142 respondents were successfully interviewed at baseline. A subset of 661 respondents (randomly selected from the full set of baseline respondents) make up the stage two sample to be revisited.

Table 1 provides summary statistics of baseline survey responses. In the full sample (Panel A), the median respondent is 46 years old, has 4 years of formal education, lives in a village with 177 inhabitants, including four relatives other than his or her spouse. When compared to typical households from low-income countries, the households in the sample are poor and in the central Malawi region we study, tobacco farmers have similar poverty and income levels to those of non-tobacco-producing households.[15] At the time of the baseline survey, the median household in the household has a zero balance in formal bank accounts, and the 90th percentile of the bank balance distribution is

[14]See the Online Appendix for further details on sampling and Brune et al. (2016) for details on the broader study from which our study participants were drawn. We note that the inclusion of a dummy indicating the treatment status in the savings experiment does not change the results significantly.

[15]Based on our calculations from the 2004 Malawi Integrated Household Survey (IHS), individuals in tobacco farming rural households in central Malawi live on PPP$1.48/day on average, while the average for central Malawian rural households overall is PPP$1.51/day.

just 700 Malawi Kwacha (MK), or approximately US$4.67. Including the self-reported value of assets, the median household held just 4,446 MK of wealth and the 90th percentile held 25,800 MK. Because the baseline survey was conducted during the rainy season, several months would elapse before the cash crop or primary staple (maize) would be harvested in mid-April or early May. As a result, the median household expects virtually no income between the interview date and April 2010.

1.3 Implementation of Stage One

Figure 1 displays the timeline of the experiment. At the baseline interview, the household head and spouse were physically separated. After demographics questions, each made 5 independent choices regarding the allocation of 2000MK between tomorrow ("sooner") and 30 days from tomorrow ("later").

Each participant was given a bowl containing 20 beans (tokens) and two empty dishes, A and B. One token allocated to dish A corresponded to $100MK$ tomorrow. One token allocated to dish B corresponded to $100MK * (1 + r)$ 30 days from tomorrow, where r is the rate of return for waiting. The rate of return took on 5 different values: 0.10, 0.25, 0.50, 0.75, and 1.00. The rates of return rose, in order, with each of the five allocation choices, and participants knew the order before making any choices. For each rate of return, the participant made an allocation of tokens to dishes, the tokens were translated into Malawi Kwacha, and the total was written above each dish on a whiteboard. The participant was then allowed to adjust the allocation. This process was repeated until the participant was ready to make the next allocation.

After completing the first five choices, the participant answered a series of questions from the baseline survey. Then, using the same elicitation method with cup, beans, and dishes, the participant again made five independent choices regarding 2000MK, while facing different rates of return for waiting. This time, each of the five choices concerned the allocation of money between 60 and 90 days from tomorrow (the "far" time frame). Online Appendix Figure

1 presents a schematic of the allocation decision.

The interruption between the five choices in the near time frame and the five choices in the far was intentional. We sought to avoid having participants choose the same allocations in both frames simply for the sake of being (or appearing) consistent. In addition, the order in which the time preference sections of the questionnaire were administered was randomly assigned between households within clubs. With probability $\frac{1}{2}$, a participant was first presented with the "near" time frame allocations; otherwise, the "far" allocations were presented first. Controlling for order effects does not affect the results, and the order in which time frames were presented does not predict choices.

Before making their choices, each participant was told that one member of the couple would be randomly chosen to have one of his or her choices implemented. The randomization was performed on site by rolling dice, and it was designed to favor (with two-thirds probability) the far time frame to have a large enough sample of stage two revisits. Implementation took the form of a voucher, redeemable at a disbursement office set up for this purpose in the nearest town, Mponela. The voucher indicated the allocation and was issued to the member of the couple who was randomly chosen. The recipient's identity was established with a name and a fingerprint placed on the voucher.

We made key aspects of payment delivery symmetric between the "near" and "far" time frames. In particular, we provided two vouchers, one for the "sooner" period (either the day after the visit or 60 days from then) and one for the "later" period (30 days from the day of the visit or 90 days from then, depending on time frame) redeemable for cash at the disbursement office. This symmetry has advantages over a design where near payments are made in cash during the experiment. That design could favor allocations to the "sooner" period in the "near" time frame if participants mistrusted the experimenters or if the infrastructure in the area induced substantial transaction costs to redeeming the "later" period voucher. A disadvantage of this symmetry is that payments were available no sooner than one day after the choices were made. Therefore, we cannot study preferences regarding consumption in

the present. To the extent that changes in time discounting are largest when tradeoffs are pushed just beyond the present, any relationships between choice under commitment and revision behaviour should be attenuated.[16]

1.4 Implementation of Stage Two

Stage two of the experiment was only carried out with those households whose randomly selected decision concerned an allocation in the far time frame.[17]

In stage two, these households were unexpectedly revisited. The target revisit date was randomly selected from the interval between 16 and 2 days prior to day 61 (the first far-frame disbursement date). Revisits occurred even if the household chose an allocation involving no disbursement of funds at day 61.[18] Revisits occurred in March and April 2010.[19]

[16]This "front end delay" payment method has been used in the literature by Pender (1996), Andersen et al (2008) and Bauer et al. (2010), among others.

[17]Recall that in stage one of the experiment, one of each household's 20 decisions (10 of the husband's and 10 of the wife's) was randomly selected to be implemented. If the selected decision concerned an allocation in the near time frame (which happened with probability one-third by design), the experimental intervention was completed for that household. The chosen individual in the household redeemed the allocation and was not interviewed again.

[18]In all that follows, we focus on the randomly-assigned targeted lag (in days) to first disbursement, since it is exogenous to farmer actions. We made the first attempt to revisit each respondent on the date implied by the randomly-assigned target lag. In some cases, the actual lag was shorter than the targeted lag, because some farmers could not immediately be located. The actual lag is highly correlated with the target lag; the correlation coefficient is 0.99. 84.9% of respondents were revisited with exactly the targeted lag, and 97.4% were revisited no more than two days after their target date. The maximum difference between target and actual lag is six days.

[19]In stage one, participants were told, "We will give you one voucher for the money that you want sooner and one voucher for the money that you want later. Each voucher will have a date written on it, you will not be able to change these dates and will not be able to redeem the voucher before the date written on it." Participants were not told that vouchers might be replaced or reissued. This framing, followed by the unannounced opportunity to revise the decision, may be perceived as deception. Inference in the experiment depends on respondents being unaware of the potential revision opportunity. The prohibition on deception in economic experiments derives in large part from circumstances where participants are drawn from a common pool and take part in multiple experiments (Jamison et al., 2008). The concern is that deception in one experiment will induce skepticism about

At the revisit, the wife and husband were physically separated and a survey of wealth, income, and expenditure was taken. Then, the participant whose choice had been selected to be implemented was presented with a bowl with 20 tokens. This time, four dishes were placed in front of the participant: dishes A, B, A' and B'. Dishes A and B contained a total of 20 tokens reflecting the participant's original decision at baseline. Dishes A' and B' were empty. The participant was told that the first set of dishes showed his or her baseline choice; an allocation between what was effectively one to 16 days from the revisit and 30 days thereafter. The participant was also reminded of the rate of return for waiting that applied at baseline, and the tokens on dishes A and B were translated into kwacha using whiteboards.

The participant was then asked to allocate the 20 tokens in the cup between the empty dishes A' and B', with the same rate of return for waiting. The allocation to the second set of dishes was again translated into kwacha and the participant was asked if he or she wanted to adjust the allocation. This process was repeated until the participant indicated he or she was finished. Then a new set of vouchers were issued (regardless of whether the allocation was revised), and the interview was concluded. Appendix Figure 2 presents a schematic of the revising procedure.

Because we sought to measure revisions of prior choices, we made the original allocation decision salient and unambiguous. This procedure is also designed to balance the consequences of implicit experimenter demands. The participant must actively choose an allocation by placing tokens in the dishes, and the status quo is thus discouraged. The mere fact that we revisited the household and allowed a revision might also imply that some change is appropriate. However, because the original allocation is set out just next to new allocation, there should be no difficulty replicating the original allocation and perhaps some mild, implicit encouragement to do so. Given the difficulty of double blind protocols in this field setting, we cannot hope to eliminate

the experimenters' "real" intent and affect behaviour in later experiments. The participants in this field experiment are not part of such common pool.

the consequences of implicit experimenter demands. Instead we designed the experiment to limit the biases they might generate.

A key element of the revisit is that participants recall the allocation they chose at baseline. The experiment therefore does not seek to study the stability of preferences after a fixed time delay (as in Harrison et al 2005). If that were the goal, we would not have reminded participants of their original choice and we would have repeated the elicitation method after a fixed delay. Our decision to make the allocation chosen at baseline salient also implies that the choice made at the revisiting stage is deterministic in a way that the baseline choices were not. The choice made at the revisiting stage will be implemented with certainty, while only one baseline choice (selected at random) was implemented. This difference in the choice setting may attenuate the underlying relationship between baseline choices and choices at revisiting.

The two randomizations carried out in stage one generated exogenous variation in two independent variables of interest in the regression analysis. First, the implemented choice generated exogenous variation in the interest rate that applied to the revision decision. Second the targeted revisit date, generated exogenous variation in the time to first disbursement. Consistent with the fact that these two variables were randomly assigned, both the implemented interest rate and targeted days to first disbursement are for the most part uncorrelated with key baseline respondent and household characteristics. (See Section 3.1 of the Online Appendix for further details.)

2 Theoretical Framework

In this section we develop a theoretical framework to aid interpretation and the definition of measures used to analyse the revision behaviour.

We model participants' choices in stage one as solving a problem that is simple but sufficiently flexible to allow static preference reversals both due to changing time discount rates (quasi-hyperbolic discounting) and due to

time-specific marginal utilities of consumption. We define $U_1(c)$, utility from consumption over four periods as follows:

$$U_1(c) = u_1(c_1) + \beta * \sum_{\tau=2}^{4} \delta^{\tau-1} u_\tau(c_\tau).$$

The familiar "$\beta - \delta$" formulation of the utility function allows static preference reversals if $\beta \neq 1$. This formulation of utility also allows for a certain form of time-dependence. While utility is separable in consumption across periods, the marginal utilities of consumption may depend on time (thus the time subscript s on $u_s(\cdot)$). This captures the possibility that consumption has different marginal value at different times.

Abstracting from the discrete choice set of the experiment, we can interpret the stage one decisions about the "near" time frame as solving

$$\max_{c_1, c_2 \in \mathbb{R}^+} \quad u_1(c_1) + \beta\delta u_2(c_2) \qquad \text{(Near)}$$
$$\text{subject to } c_2 \leq (2000 - c_1)(1 + r)$$

for each rate of return r and assuming an endowment of $2000MK$. Similarly, decisions about the "far" time frame solve

$$\max_{c_3, c_4 \in \mathbb{R}^+} \quad \beta\delta^2 u_3(c_3) + \beta\delta^3 u_4(c_4) \qquad \text{(Far)}$$
$$\text{subject to } c_4 \leq (2000 - c_3)(1 + r).$$

Interior solutions to these two problems satisfy the first-order conditions

$$u_1'(c_1^*) = (1 + r)\beta\delta u_2'(c_2^*) \qquad \text{(FOC Near)}$$
$$u_3'(c_3^*) = (1 + r)\delta u_4'(c_4^*). \qquad \text{(FOC Far)}$$

This formulation is useful as it allows two distinct sources of static preference reversals but additional assumptions on the functional form of utility

are necessary for choices to identify discount factors in problems (Near) and (Far).[20]

We now turn to the choices in stage two of the experiment. If the revisit is sufficiently close to period 3 then the respondent solves

$$\begin{aligned} \max_{c_3,c_4\in\mathbb{R}^+} \quad & U_{revisit}(c_3,c_4) = u_3(c_3) + \beta\delta u_4(c_4) \\ \text{subject to } \; & c_4 \leq (2000 - c_3)(1+r). \end{aligned}$$

Interior solutions here satisfy

$$u_3'(\tilde{c}_3^*) = (1+r)\beta\delta u_4'(\tilde{c}_4^*). \tag{1}$$

Recall, the solution to the stage one problem (Far) satisfied

$$u_3'(c_3^*) = (1+r)\delta u_4'(c_4^*).$$

Thus, abstracting from uncertainty, social pressure, and mistakes, if time discounting is exponential ($\beta = 1$) then the respondent will not revise ($\tilde{c}_3^* = c_3^*$). If instead the respondent is 'present' -biased ($\beta < 1$) then behaviour is time-inconsistent $\tilde{c}_3^* > c_3^*$. Analogously, if ($\beta > 1$) then $\tilde{c}_3^* < c_3^*$.

2.1 The Tests

This deterministic analysis suggests the following two tests of non-constant time discounting.

Test 1 If the respondent exhibits static, "present" -biased preference reversals in stage one, and thus appears to have $\beta < 1$, she will shift more consumption toward sooner upon revisiting. Similarly, if the respondent exhibits static,

[20] More formally, for any $u_1, u_2, \beta\delta$ that can reconcile choices regarding the near term, there exists another $\tilde{u}_1, \tilde{u}_2, \widetilde{\beta\delta}$ that can do so as well and therefore once needs additional assumptions on the functional forms to identify β, δ and the curvature parameters of the utility function.

future-biased preference reversals in stage one and thus appears to have $\beta > 1$, she would shift more consumption toward later upon revisiting.

Test 2 If the revisit occurs sufficiently close to the date of first disbursement (period 3 in the above framework) then first order condition (1) applies and present (or future) bias will be evident in a revision toward sooner (later). If instead the revisit falls far before the date of first disbursement, then first order condition (FOC Far) continues to apply and the model predicts no revision.

2.1.1 Random Choice

Test 1 is appropriate if one assumes that choice data are dictated by the deterministic model above, and so the difference between the choice and the model's prediction (or error) is interpreted as an unobserved determinant of preferences. If, however, we allow for error in the implementation of "true" preferences, estimates of the empirical model may exaggerate the correlation between static preference reversals and time-inconsistency.

To see why, consider an extreme version of that error: a respondent that makes allocations completely at random both in stage one and at the revisit. Now consider choices exhibiting "present"-bias. By definition, the allocation to sooner in the far time frame is lower than for the near time frame. When choice is entirely random, therefore, the individual will, on average, allocate more tokens to sooner upon revision. In this way, participants appearing "present"-biased due to implementation error are mechanically more likely to revise towards sooner.[21] An analogous effect applies to future-biased static preference reversals and revisions toward later.

[21] Consider the following numerical example with interest rate $r = 10\%$. An individual that appears "present"-biased randomly allocates 1000 to sooner and 1100 to later in the near time frame and 600 to sooner and 1540 to later in the far time frame. Note that since the individual appears "present"-biased, the allocation to sooner in the far time frame has to be smaller than the allocation to sooner in the near time frame. In our example, the allocation to sooner is 600. But because this allocation to sooner will tend to be small, the probability that more tokens will be randomly allocated to sooner upon revisit is high, and therefore individuals that appear "present"-biased mechanically will be more likely to allocate more tokens to sooner upon revision.

We tackle this confounding effect due to implementation error in our analysis of Section 3 by constructing measures of "present" or future bias only from the stage one choices that were *not* implemented. If implementation errors are independent of each other, then measuring the tendency for static preference reversals from the non-implemented choices will break the mechanical relationship between reversals and time-inconsistency in the experiment.[22]

2.1.2 Time-specific marginal utilities

Alternatively, while Test 1 assumes that static preference reversals are only due to non-constant time discounting, they can also emerge from time-specific marginal utilities of consumption, which may be relevant in Malawi. For example, the marginal utility of consumption may be especially high at the time of tilling or harvest (when farmers need more calories to maintain work effort) or during the period immediately prior to harvest (when caloric consumption is low).

To illustrate, suppose time discounting is constant ($\beta = 1$) but "flow" utility is a function of time. Suppose, in particular, that utility is iso-elastic and varies only across, but not within, time frame:

$$u_\tau(c_\tau) = \frac{c_\tau^{1-\sigma}}{1-\sigma} \quad \text{for } \tau = 1,2 \qquad \text{and} \qquad u_\tau(c_\tau) = \frac{c_\tau^{1-\rho}}{1-\rho} \quad \text{for } \tau = 3,4 \quad (2)$$
$$\sigma, \rho \geq 0.$$

Interior solutions to stage one problems (FOC Near) and (FOC Far) imply

$$\left(\frac{2000 - c_1^*}{c_1^*}\right)^\sigma = \left(\frac{2000 - c_3^*}{c_3^*}\right)^\rho$$

If optimal consumption (weakly) rises within time frame (i.e. $(1+r) \geq \delta$), then respondents with a higher elasticity of intertemporal substitution in the

[22] See Section 4 of the Online Appendix for simulations that illustrate the consequences of using only non-implemented choices to measure a participant's tendency to make static preference reversals.

"far" time frame will exhibit a "present"-biased static preference reversal and thus appear less patient in the "near".[23] Similarly, if the participant has a higher elasticity of intertemporal substitution within the "near" time frame ($\sigma < \rho$) then $c_1^* < c_3^*$. Such a participant would not revise his or her original allocation (and thus would not exhibit time inconsistency) because the first order condition for the stage one problem (FOC Far) is the same as that of the revisit problem (1).

While this example relies on special functional forms, the insight is general. Differences in the curvature of flow utility across time frames can induce static preference reversals that are not driven by time inconsistency.

We accommodate this in our empirical analysis of Section 3 by identifying respondents who show differences in curvature across time frames and by allowing them to have a different correlation between static preference reversals and revisions of prior choices.

3 Results

We begin with an analysis of whether intertemporal choices are consistent with the law of demand and the prevalence of static preference reversals in stage one choices. We thus use all the 2,142 observations available. We then turn to stage two choices only available for the 661 individuals that were revisited.

3.1 Adherence to the Law of Demand

The additive separability and monotonicity of the flow utilities assumed in Section 2 above makes the strong prediction that if participants solve problems (Near) and (Far), then the allocation to the later period, measured in kwacha,

[23] More formally, if $(1 + r) \geq \delta$ and $\sigma > \rho$ then $c_1^* > c_3^*$.

should increase with the rate of return to waiting r.[24]

We use the degree of consistency with this prediction of standard theory as a metric for judging the appropriateness of simple economic models to interpreting choices in the experiment: if choices are inconsistent with the law of demand, either poor participants did not understand the trade-offs involved, or standard economic models have little validity in this setting.

We evaluate adherence with the law of demand by dividing each participant's ten decisions into pairs, where each element of the pair is an allocation over the same two dates. The first element of the pair is the allocation to later when facing rate of return r. The other element is the allocation to later when facing the next lowest rate of return, r'. For each participant there are eight such pairs, four for each of the two time frames. Out of 17,136 such pairs in the data, in 13,859 pairs the allocation to the later period increased with r. Thus, 81% of pairs were consistent with the law of demand. The median violation is moderate in size in the sense that it could be made consistent with monotonicity with a reallocation of less than two tokens.[25]

Becker (1962) indicates that adherence with the law of demand is not a particularly stringent test of rationality because even random choice will, on average, obey the law of demand. We therefore compare the share of consistent

[24]To see why, think of $\frac{1}{1+r}$ as the price of consumption later in terms of consumption sooner. When r goes up, the price of later consumption goes down. The result is an income effect creating incentives to increase consumption in both periods, and a substitution effect that is positive for consumption in the later period. Thus both income and substitution effects lead to increased consumption in the later period. The near allocation, on the other hand, can go up or down depending on whether the income or substitution effect dominates.

[25]A comparison with existing studies in developed countries is informative as we are not aware of similar statistics being provided in studies based in developing countries. For example, in Andreoni and Sprenger (2012), the percentage of individuals that would have six or more consistent pairs of choices is 92% (using the later allocation). According to Table 2, the percentage in this experiment is somewhat lower at 76%. Similarly, using a multiple price list elicitation format Meier and Sprenger (2015) found that only 11% of a U.S. based sample exhibited multiple switch points and thus violated monotonicity – though studies of risk preferences have exhibited much higher rates of violation (e.g., Jacobsen and Petrie, 2009) than what we observe. Finally, while the published statistics are not directly comparable, the U.S. based subjects in Augenblick et al. (2015) also appear to adhere to the law of demand at higher rates than those in our study.

pairs we observe in the experiment with the share generated from a simulation where the same-sized sample makes choices purely at random (see Section 4 of the Online Appendix for details). In the simulation 57% of pairs are consistent with the law of demand.[26] While substantially lower than the average rate of consistency in the experiment, this simulation suggests some caution in interpreting the choices as resulting from simple optimization and motivates disaggregated analysis.

Indeed there is important heterogeneity in consistency with the law of demand. Table 2 presents the distribution of participants by the number of times (out of eight) they increased their later allocation with a single increase in the rate of return r. Column 1 shows that, measured this way, 31.3% of participants are always consistent and 75.7% are consistent at least in 6 out of 8 allocations. At the other end of the spectrum, 10.2% of the sample violated this form of consistency in at least 4 allocations.[27]

In sum, these levels of consistency with the law of demand suggest that many, but not all, participants understood the trade-offs they were facing and that, for this majority, their violations of monotonicity might be attributed to occasional "trembles" in the allocation process.

Further examination of decisions in stage one reported in Table 3 reveals that choices are usually in the interior of the budget set. For example, at a 50% rate of return to waiting, the median allocation to later is 1,950MK and 700MK to sooner. A minority of allocations (12% to 23%) are "corner solutions." The high frequency of interior allocations is consistent with participants not having adequate tools outside the experiment to facilitate consumption smoothing, and also points (in the absence of very high time discount rates) to the importance of diminishing marginal utilities of consumption.

Another important feature of this distribution of stage one allocations is

[26] In contrast to the actual data, the median violation in the simulation of random choice could be made consistent with an allocation of 6 tokens.

[27] Column 2 reports the simulated distribution of consistent choices if participants were to choose consumption randomly. Virtually no-one is always consistent under random choice and only 16.9% are consistent in at least 6 out of 8 allocations.

the heterogeneity in the willingness to wait in exchange for a larger reward. For example, for "later" allocations in the "near" time frame, at a 25% rate of return, the 10th percentile is 750MK, while at the 90th percentile it is the entire endowment. This heterogeneity is somewhat predictable with observable subject characteristics. Regression analysis in Section 3.2 of the Online Appendix reveals that those with more wealth at baseline allocate more to later, as do those with more relatives who live in the village.

3.2 Static Preference Reversals

Table 3 shows a remarkable stability across time frames. The distribution of allocations to later is not dramatically altered by the change from the "near" to "far" time frame. For example, the mean allocations to later at the 25% rate of return are 1,536MK and 1,565MK in the "near" and "far" time frames, respectively. We find, however, that this average stability obscures substantial volatility of individual choices across time frames and masks heterogeneity in individual tendencies to shift allocations forward or back, depending on the frame.

Each participant makes five pairs of decisions where each element of a pair differs only in time frame. Of all 10,710 such pairs, just 2,927 (27%) are identical and just 4,895 (46%) differ by a token or less. Thus, in more than half of all such pairs the elements are substantially different from one another. There is a modest tendency for these static preference reversals to be "present"-biased. Of the 5,815 pairs that differ by strictly more than a token, 3,061 (53%) allocate more to the sooner date in the near time frame. The remaining 47% allocate more to the later date in the near time frame.[28]

These patterns in stage one indicate that static preference reversals are common and that "present" -biased reversals are only somewhat more common. While the distribution of these static reversals is roughly symmetric

[28]In the simulation of random choice, 4.77% are equal, 13.85% differ by one token or less, and preference reversals are equally split between present and future biased (43% each).

around consistency, there is evidence that they are not just the result of random trembles. Among those participants who exhibit static reversals, 18% is "present"-biased in at least four of five decisions. Simulations of purely random choice indicate that the percentage of individuals with at least four of five "present" -biased pairs would be about 8%. The tendency to be consistent or "present"-biased is also somewhat predictable with observable characteristics of the participants.

Table 4 presents regression results that relate a participant's tendency to be consistent or "present"-biased to observable characteristics. In each column the dependent variable is either the fraction of pairs of decisions in which the participant was dynamically consistent or the fraction the participant was present-biased. Column 1 indicates that males and those with greater maize stores tend to be more dynamically consistent. Column 3 reveals that these variables have similar relationships (with opposite signs) with fraction present-biased, though these relationships are not statistically significant. Indeed, the reported p-value in the last row suggests that household characteristics are jointly insignificant except for column 1.

Columns 2 and 4 reveal however two important relationships. First, there is a strong association between adherence to the law of demand (Section 3.1) and static preference reversals.[29] Greater adherence to the law of demand is associated with more dynamically consistent choices. This suggests that for many the tendency to exhibit static preference reversals may be due to a poor understanding of the choice environment. Second, there is a strong association between being more responsive to the interest rate in the far time frame and present-biased static preference reversals. As explained in Section **??**, below, this is what we would expect if some respondents exhibit static preference reversals because their marginal utilities of consumption depend on

[29]There is no mechanical reason why these two measures must be linked. The first regards the response of allocations to changes in within time frame. The second regards consistency of allocations across time frames. For example, a subject who always violated the law of demand could be perfectly dynamically consistent, simply by replicating his non-monotonic allocations in both time frames.

time. We investigate this possibility, as well as the role of confusion about the experiment, in our analysis of stage two revision behaviour below.

3.3 Revision Behaviour

Before studying the determinants of revision behaviour, we first describe basic features of the choices upon revisiting. Recall that stage two of the experiment applies only to those households whose randomly selected choice was an allocation between 61 and 91 days from the baseline interview. We aimed to revisit 722 respondents and we successfully collected revision choice data from 661 (91.6%).

Revisions are common. While their original choice was clear and salient, 65% of participants (432) made some adjustment to that decision. Implicit experimenter demands may have caused some participants to feel as though some change was expected of them. A large majority (87%) made a reallocation involving a shift of at least two tokens, and 64% made a reallocation involving a shift of at least 4 tokens. Appendix Figure 3 presents a histogram of changes in the participants' allocations to sooner ($t = 61$) upon revisiting, excluding those who made no change (35% of observations), illustrating the frequency of relatively large revisions.

Furthermore, revisions shift the allocation of income forward and backward in time with nearly equal frequency. Of the 432 participants who made some revision, 52% shifted income toward sooner and 48% shifted income toward later. As the histogram also indicates, the revisions toward later tended to be more modest in size. Of these, approximately 56.5% involve the shifting of at least 4 tokens, and just 15.5% involve shifting 10 tokens or more. The comparable figures for revisions toward sooner are 70.2% and 25.8%.

Table 5 presents the results of ordinary least-squares regressions relating revision behaviour to potential determinants of revision. The dependent variable is the change in sooner allocations upon revisiting (in MK).[30]

[30]In Appendix Figure 2's example, the dependent variable would take the value 200, as

In column 1, independent variables are restricted to baseline characteristics and the implemented interest rate. Respondents appear to revise less towards sooner at higher rates of return: the coefficient on the interest rate is negative and statistically significant at the 10% level. Males and younger individuals (those aged 56 or below) revise more towards sooner, while more-educated individuals (primary and more than primary) revise less towards sooner. Characteristics of the respondent's spouse, and baseline maize stores and wealth add relatively little explanatory power. With evidence on these basic correlates of revisions, we now turn to Tests 1 and 2.

Test 1 evaluates "present"-bias as the source of static preference reversals.[31] We construct a non-parametric measure based on the number of times that a respondent made a "present"-biased preference reversal in stage one.[32] We account for the effects of implementation error (see Section 2.1.1) by taking just four of the five pairs of decisions where each element of a pair differs only in the time frame (excluding the pair associated with the implemented interest rate), and calculating the fraction of those four pairs in which the participant exhibited "present"-biased static preference reversals.[33]

As discussed in Section 2.1.2, static preference reversals can also be driven by changes in the marginal utility of consumption. We therefore construct a non-parametric measure of across-time-frame differences in the curvature of utility based on the average responsiveness to the interest rate of the share of

two tokens were added to the time t dish compared to the original allocation.

[31] In the interest of brevity, we focus here on the test for $\beta \leq 1$ and leave analysis of future bias to Section 3.4 of the Online Appendix.

[32] An alternative approach would parameterize the utility functions in problems (Near) and (Far) and estimate individual-specific parameters. We pursue this method in Section 3.8 of the Online Appendix.

[33] To allow for respondent error, we consider it a reversal only if the allocations differ by two tokens or more. Results are very similar if we reduce the tolerance to just one token. In addition, Appendix Table 3 provides results where our preferred measure is replaced on the right-hand-side with the fraction of all five pairs of choices (including the one associated with the implemented interest rate) in which the respondent exhibited a "present" -biased static preference reversal. Coefficient estimates on fraction present-biased are, as expected, larger in magnitude than those of Table 5.

consumption allocated to later for each time frame $f \in \{near, far\}$:

$$\bar{\varepsilon}_f = \frac{1}{4} \sum_{r=0.25}^{1.0} \varepsilon_{rf}.$$

Here, ε_{rf} is the change in the share of consumption allocated to later in time frame f associated with the incremental increase in the rate of return to r.[34] We use ε_{rf} instead of the elasticity of intertemporal substitution, $\frac{d \ln\left(\frac{c_{t+1}}{c_t}\right)}{dr}$ ($\frac{1}{\sigma}$ or $\frac{1}{\rho}$ in example 2) because the latter is undefined for corner solutions and, in practice, the two measures are so well correlated that, among those with interior solutions, the two produce quantitatively very similar results. Then, we take the difference in the average responsiveness across time frames, $\triangle\bar{\varepsilon}_f \equiv \bar{\varepsilon}_{far} - \bar{\varepsilon}_{near}$. When $\triangle\bar{\varepsilon}_f$ is large it indicates that the respondent was more responsive to the rate of return, and thus exhibited less curvature in flow utility, in the far time frame.[35] If such respondents also exhibit present-biased preference reversals, those reversals would not be explained by changes in the marginal utility of consumption but instead point to time-inconsistent preferences.

The importance of hyperbolic discounting for revision could be understated if "present" -bias is positively correlated with an overall reluctance to delay consumption. If so, "present" -biased static preference reversals would be positively correlated with larger initial allocations to sooner that, by definition, leave less room for revisions toward sooner. We therefore also condition on

[34]Thus, if ℓ_{rf} denotes the share of consumption allocated to later in time frame f when the rate of return is r, then

$$\varepsilon_{r'f} = \frac{\ell_{r'f} - \ell_{rf}}{r' - r}.$$

The smallest incremental increase in the interest rate is 0.15, so ε_{rf} can range from ± 6.67.

[35]Among the respondents who were revisited, $\triangle\bar{\varepsilon}_f$ ranges from -2.10 to 2.33 with a median of 0.00 and a mean of 0.01. To reduce the confounding influence of implementation error in responses, we create an indicator variable equal to one if $\triangle\bar{\varepsilon}_f > 0.1$, and zero otherwise. This classifies 33% of the revisited sample as "more elastic" in the later time frame. Using a continuous measure of the across time frame difference in the responsiveness to the interest rate yields very similar conclusions, but with less precision.

a non-parametric measure of patience: fraction of tokens allocated to sooner, across 9 baseline allocations (out of 10), excluding the implemented choice.

Column 2 of the table shows initial results of Test 1. The results are consistent with the model outlined in Section 2 where respondents are heterogeneous in both β and in the time-dependence of flow utility. The coefficient on the main effect of fraction present biased is positive, and statistically significantly different from zero at the 5% level. This effect, however, only exists for individuals that do not appear systematically more elastic in the "far" time frame. Summing the coefficients on the main effect, the indicator for "more elastic in the far time frame" $1(\triangle\bar{\varepsilon}_f > 0.1)$ and on the interaction of fraction "present"-biased with the indicator, we see that those who are more elastic in the far time frame are, on average, time-consistent (the sum of the coefficients is not statistically significant, p-value = 0.29).

Test 2 exploits the randomized revisit date. Column 2 also includes on the right-hand-side of the regression an indicator for the targeted lag to first disbursement being less than or equal to six days.[36] Here the prediction is robust to concerns about time-dependence of marginal utility. If individuals have hyperbolic preferences ($\beta < 1$), they will shift more towards the present if they are sufficiently close to the time of consumption. We chose an indicator of six days or less, which captures a third of the revisited sample, in order to balance concerns about power (which might argue for a linear target lag specification) against the prediction of a non-linear relationship between targeted lag and revision that comes from a model of quasi-hyperbolic time discounting.

The estimates in column 2 provide evidence consistent with quasi-hyperbolic time discounting among some respondents. The coefficient on the indicator for six or fewer days to first disbursement is positive and statistically significant at the 5% level. In addition, as expected, the non-parametric measure of general impatience is negatively correlated with revisions toward sooner. Inclusion of

[36]Section 3.3 of the Online Appendix shows that alternate (in particular, linear) specifications of the target lag yield similar results, and that a highly flexible specification of the target lag suggests that the step-function we use at six days is a reasonable approximation.

this control has little effect on other regression coefficients.[37]

3.4 Other Motives for Revision

In column 3 we add to the regression variables measuring financial sophistication and proxying for mistakes in initial allocations. We examine whether these indicators of error predict revisions, and whether a correlation between these measures and preferences in stage one explain the latter's correlation with revisions. The coefficients on these variables are typically negative, suggesting that those with greater sophistication tend to revise toward later. But the standard errors on these estimates are large, and we cannot reject a null hypothesis of large effects (either positive or negative). A joint significance test yields a similar conclusion.

As discussed in Section 3.2 there is a negative correlation between adherence to the law of demand and static preference reversals. However, including the measure of adherence to the law of demand has virtually no effect on the point estimates of the relationship between "present" -biased static preference reversals and revision behaviour. There is therefore no evidence that this link between stage one preference reversals and revisions is driven by a relationship between the preference reversals and mistakes.

In column 4 we add variables representing shocks experienced since the baseline survey. Coefficients on death in the family and on shortfall in expected income have the expected negative signs. Again, the standard errors are large and we cannot reject a null hypothesis of large coefficients.[38] Inclusion of these

[37] In results available upon request, we also estimate a specification that includes a triple interaction term allowing the effect of distance to first disbursement to differ by both fraction present-biased and the indicator for more being elastic in the far period. The statistical significance of the previously discussed coefficients does not change in this specification; the magnitude of the coefficient on the fraction present-biased increases somewhat. The coefficient on the triple interaction term is positive, consistent with a larger effect of distance to first disbursement among those who are more present-biased and more elastic in the far period, but not statistically different from zero.

[38] Deaths affect approximately 2% of households, and shocks to income tend to be small. Households expected virtually no cash income over this period. Care should therefore be

shock variables has little impact on other regression coefficients.

In column 5, we add to the regression measures of social pressure. The first variable is one's spouse's allocation to sooner minus one's own, averaged across the 9 baseline allocations (out of 10), excluding the implemented choice.[39] This variable should capture pressure to revise one's allocation toward sooner coming from one's spouse. Initial allocations were made without consultation between spouses, but there was ample opportunity to express preferences regarding the implemented allocation (and, implicitly, alternatives) after the allocation was revealed and vouchers issued, and before the revisit. Moreover, even though the initial allocations were made privately, one choice from each spouse was selected for potential implementation and then a dice roll in the presence of both spouses determined which allocation was actually implemented.[40] The second variable is simply the number of relatives one reports having in the village, which should proxy for pressures to share with a wider social network. Both variables enter the regression positively, consistent with the pressure leading to less saving. Their magnitudes are precisely estimated to be economically small; we can reject a null hypothesis of large positive correlations with revisions toward sooner.

In column 6, we add to the set of regressors several characteristics of one's spouse choices and performance on tests in stage one (coefficients omitted for brevity).[41] There is no evidence that any of the results we have described so far are simply be due to omitted spousal variables: their inclusion has little effect on other coefficients of interest.

In sum, the patterns in Table 5 provide some support for a model of quasi-

used in extrapolating these results to other settings subject to greater risk.

[39] As with the present-bias ratio, we exclude the implemented choice from this calculation to guard against a spurious positive relationship caused by random choice.

[40] Revisions towards the spousal allocation could happen unwillingly, as the result of pressure from the spouse (Ashraf, 2009 and Schaner, 2015), or willingly, say on the basis of information provided by the spouse as to optimal actions.

[41] These variables are: fraction present biased across all choices, word recall, Raven's score, financial literacy score, and fraction of decisions consistent with law of demand.

hyperbolic discounting as an account of some respondents' behaviour. Test 1 shows that individuals whose stage one allocations exhibit more "present" -biased preference reversals – reversals that cannot easily be explained by changes in the marginal utility of consumption – revise more towards sooner. Test 2 shows that revisions toward sooner are also larger when individuals make their revision at a time sufficiently close to the funds disbursement date. We estimate quite precisely little effect of social pressure on the tendency to revise. Finally we find no evidence that variables representing financial sophistication or shocks have statistically significant or robust relationships with revision behaviour. Thus, the results provide no support for the idea that mistakes in initial allocations (which should be more prevalent for those with lower financial sophistication) are important determinants of revision over this horizon.

Examining the coefficients from column 6 of Table 5, we can assess their economic magnitude. A useful benchmark for this purpose is the impact of a 50-percentage point reduction in the rate of return to waiting 30 days, which leads to a 111.31 MK increase in revisions toward sooner. In comparison, a one-standard-deviation (0.28) increase in the measure of present-bias is associated with 60.36 MK higher revisions toward sooner, and making one's revision decision within six days of day t=61 raises revisions toward sooner by 124.63 MK.[42]

[42]In the Online Appendix, we provide the following additional analyses. First, we show in Section 3.3 that the indicator we use for the targeted lag to first disbursement is a reasonable approximation. Second, in Online Appendix section 3.4 we show that no pattern similar to that shown by "present-bias" appears for an analogously-defined "future-bias" variable. In results available upon request, we find that the coefficients on the measures of present- and future-bias are not statistically different from each other when included in the same regression, though the magnitude of the coefficient on the present-bias term remains almost 70 percent larger than that of the future-bias term. Third, in Online Appendix section 3.5 we provide an analysis of attrition related to the randomized target lag, showing that while attrition is statistically significantly higher at lower target lags, the magnitude of this relationship is small enough that it would be highly implausible for our results related to the target lag to be driven purely by selection. Fourth, in Online Appendix section 3.6 we estimate the specification of column 6, Table 5 separately for males and female respondents, and find no strong evidence of gender differences in key coefficients. Fifth, in

4 Related Literature

There is a long tradition of evaluating time preferences from observational choices over time. Hausman (1979), Lawrance (1991) and Warner and Pleeter (2001) are prominent examples. In this tradition, the analyst observes the (implicit) price consumers are willing to pay in order to move consumption forward in time. In Hausman (1979), a time discount rate is inferred from the price elasticity of demand for long-run energy efficiency in household appliances. The early contributions to this literature assumed that time discount rates were constant with respect to time. More recently, observational data has been used to estimate potentially non-constant time-discount functions. This literature, which restricts itself to estimating quasi-hyperbolic discount functions, includes Paserman (2008), Fang and Silverman (2009) and Laibson et al. (2007). We depart from this literature by adopting experimental methods for eliciting intertemporal choices and working with non-parametric measures of patience and "present"-bias.

The experimental literature on time preference is large. Influential recent examples include Halevy (2015), Augenblick et al. (2015), Andersen, et al. (2008), Benhabib, et al. (2010), and Andreoni and Sprenger (2012). Frederick, et al. (2002) provides a review. Our paper is distinguished from the bulk of this literature by, among other things, our implementation of a lab-in-the-field experiment with a large and heterogeneous sample. We can thus examine the correspondence between subjects' experimental behaviour and their "real world" characteristics and behaviours.

Our paper thus joins the relatively recent trend to augment lab studies of time preference with experiments in the field, such as Harrison, et al. (2005),

Online Appendix section 3.7, we replicate Table 5 excluding individuals that are inconsistent in 3 or more pairs. One may think that these individuals do not understand the experiment thus contributing to measurement error. We find that most of the results hold and that the coefficients of interest are not larger in absolute value, suggesting that there is no attenuation bias. Finally, using a flexible "$\delta - \beta$" model we structurally estimate the individual discount factor β and include it as a regressor in the specification of Table 5. Appendix Table 9 contains the results. Online Appendix section 3.8 contains the details.

Ashraf, et al. (2006), and Tanaka et al. (2009). Two of these studies are closely related to ours. The first, Ashraf, et al. (2006), fielded hypothetical time preference questions among Philippine respondents who were then later offered a commitment saving product. Women who exhibited present-biased preference reversals on the survey questions were, as predicted by theory, more likely to take up the commitment saving product. Our paper differs from this study by studying directly the link between incentivized intertemporal allocation decisions and revision of prior choices. We measure the extent of preference reversals, as well as the basic consistency of choice with rational economic models, and thus provide a quantitative assessment of the mechanisms behind time inconsistency and the demand for commitment. The second related paper, Harrison, et al. (2005), elicited time preferences among Danish respondents. A subset of respondents were later revisited and asked to perform the same time preference experiment again. Our experiment differs from Harrison, et al. (2005) by, among other things, making a participant's original choice clear and salient. Our goal is not to evaluate the stability of time preference, but rather to measure revisions of intertemporal plans and to shed light on the determinants of such revisions.

5 Conclusion

The consequences of sub-optimal intertemporal choices can be serious, especially among the poor in developing countries. We conducted an experiment among Malawian farmers to investigate why their intertemporal choices may appear not to serve their individual self-interest. More precisely, we provide the first field evidence on the causes and correlates of decisions to revise prior intertemporal choices made under commitment. The experiment allowed subjects to make an intertemporal allocation of substantial funds they would receive at two future times 30 days apart. This future 30-day period was timed to occur during a period of low income and low food stores, during which con-

sumption smoothing of substantial amounts of future income is very difficult. Several weeks later, prior to the first disbursal of funds, we revisited study participants and allowed them to revise their previous allocations over the same 30-day period. We examine these revisions of allocations for evidence of self-control problems as well as other potential mechanisms behind intertemporal choice revision.

We provide a new evaluation of the importance of self-control problems in a developing context. We test, in particular, whether revisions of allocations toward the present are positively associated with measures of ‘present’-bias from an earlier baseline survey, or with the (randomly assigned) closeness in time to the first possible date of money disbursement.[43] These tests complement existing tests of self-control problems based on demand for commitment devices. In contrast to analyses of demand for commitment devices, our approach has the advantage of allowing even naïve individuals (who are not aware of their self-control problems) to contribute to estimates, since naïve as well as sophisticated respondents can display revision behaviour. In addition, analysis of revision behaviour avoids problems of low demand that may arise if commitment devices are poorly designed.

We find that only a minority of our sample exhibits “present”-biased static preference reversals that cannot be easily reconciled by predictable changes in the marginal utility consumption. But the correlation between these reversals and revision of prior choices toward sooner is relatively large. Consistent with a model of self-control problems, the correlation between the time to the first possible date of disbursement and revisions toward sooner is negative. We find no evidence that respondents’ revisions tend to move in the direction of their spouses’ preferences for such revision. Similarly, though with less precision, we find no evidence that mistakes or shocks predict revisions of prior intertemporal choices.

These results suggest cautious optimism about efforts to improve the lives

[43]This result is reminiscent of Kaur, Kremer, and Mullainathan’s (2015) finding that worker effort increases as a worker’s randomly-assigned payday comes closer.

of the very poor in developing countries via interventions that address their problems of self-control. Our results support the view that, if we privilege an individual's preferences at moments relatively far from the present, there may be important benefits of commitment for some people and the costs of such commitments, in terms of reduced flexibility, would be limited.

This view should be tempered, however, by two important caveats. First, our findings show that "present" -bias, as evidenced by static preference reversals, is far from ubiquitous in this population. Many of the participants in the experiment exhibited, at most, just a modest tendency to be "present" -biased. In addition, we provide evidence that some of the revisions towards the sooner allocation, consistent with present-bias, are more likely to reflect anticipated time-varying intertemporal elasticities of substitution, rather than time-varying discount rates. Policy design must take account of this heterogeneity; efforts to help some with their legitimate self-control problems must avoid saddling others with commitments they do not need. Second, and related, our experiment was conducted during the lean season when little income is generated. This makes ours an unlikely context for finding that income fluctuations influence revision. It is possible that income shortfalls may influence revision behaviour in other parts of the Malawian agricultural year, such as in the post-harvest months.

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Figure 1: Timeline of Interviews, Choices, and Disbursement of Funds

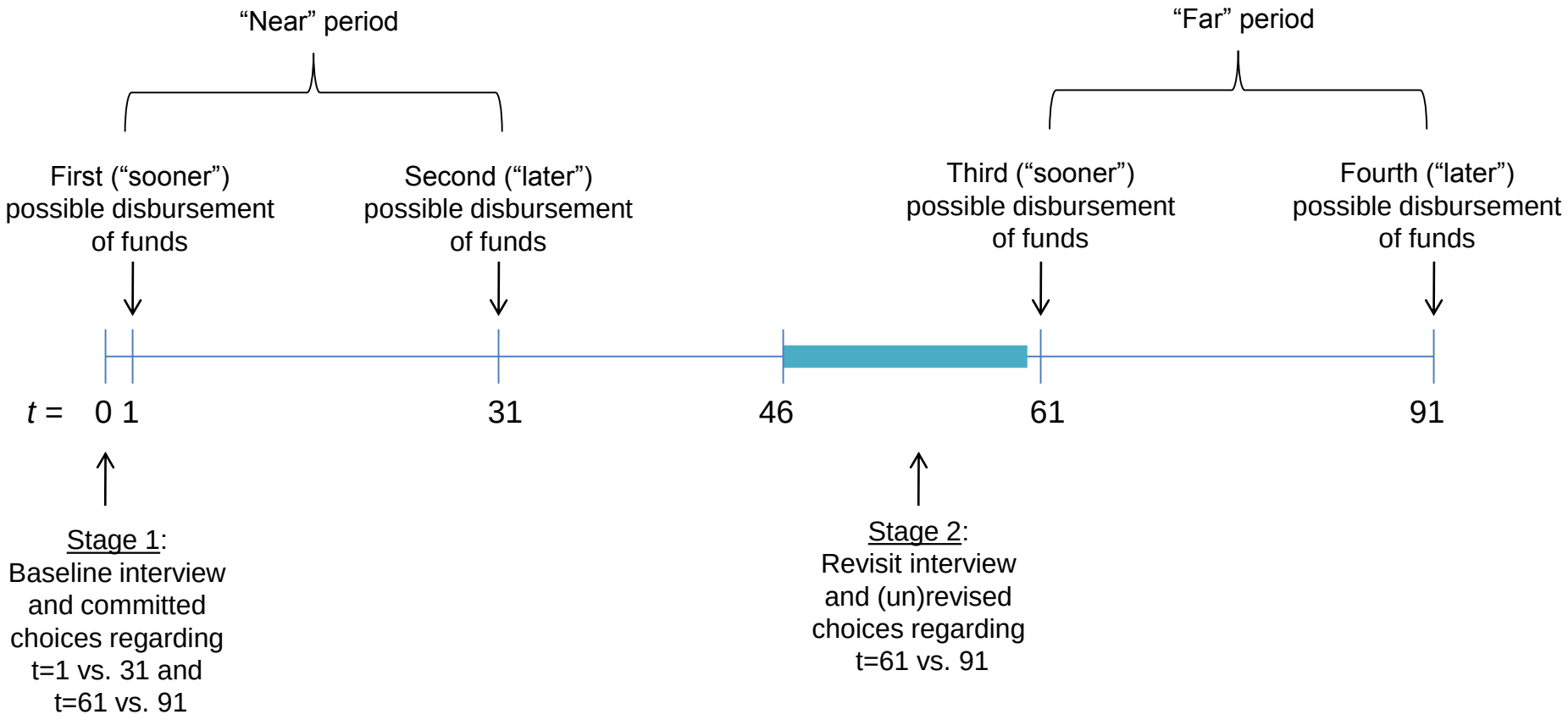


Table 1: Summary Statistics

Variable	N	Mean	Std. Dev	Min	10th pctile	50th pctile	90th pctile	Max
Panel A: Baseline sample (stage 1)								
Change in Allocation to Sooner, Avg. Across All Interest Rates (MK)	2142	15.70	302.66	-2000	-280	0	340	2000
Fraction Present Biased, All Interest Rates	2142	0.29	0.27	0	0	0.2	0.6	1
Fraction Present Biased, Non-Implemented Interest Rates	2142	0.28	0.28	0	0	0.25	0.75	1
Indicator: more elastic in the far time frame	2142	0.33	0.47	0	0	0	1	1
Fraction of Decisions Consistent with Law of Demand	2142	0.81	0.18	0.125	0.5	0.875	1	1
Fraction of All Tokens Allocated to "Sooner"	2142	0.37	0.19	0	0.1	0.389	0.578	
Implemented Interest Rate	2142	0.62	0.33	0.1	0.1	0.75	1	1
Demographics								
Male	2142	0.50	0.50	0	0	0	1	1
Respondent's Own Age	2142	46.47	14.02	18	28	46	65	95
Age 35 or under	2142	0.24	0.43	0	0	0	1	1
36-56 years old	2142	0.51	0.50	0	0	1	1	1
Respondent's Spouse's Age	2142	46.48	14.04	18	28	46	65	95
Years of Schooling	2142	4.47	4.20	0	0	4	8	77
Some Primary School	2142	0.61	0.49	0	0	1	1	1
Primary School	2142	0.15	0.36	0	0	0	1	1
More than Primary School	2142	0.07	0.26	0	0	0	0	1
Have Adequate Maize	2142	0.22	0.41	0	0	0	1	1
Number of Relatives in Village	2142	4.64	8.63	0	0	2	10	132
Total Number of People in Village	2142	177.08	258.47	0	35	120	320	4000
Aptitude Questions								
Words Recalled - First Time	2142	4.81	1.31	0	3	5	6	10
Number Correct on Raven's Matrices	2142	1.53	0.92	0	0	2	3	3
Financial Literacy Questions Correct	2142	0.73	0.99	0	0	0	2	3
Wealth and Income								
Total HH Wealth	2142	11449	27313	40	1020	4446	25800	695025
HH Total in Bank	2142	447.86	2358.96	0	0	0	700	54000
HH Total Cash	2142	156.26	1353.26	0	0	0	100	34000
HH Items	2142	6218	19737	0	600	2346	11625	588290
HH Animals	2142	4627	10776	0	0	1250	12150	123600
Expected Income (in period between baseline and revisit)	2142	1758	6307	0	0	50	4470	137700
Panel B: Revisit sample (stage 2)								
Change in Sooner Allocation upon Revisiting (MK)	661	61.42	595.98	-2000	-600	0	900	2100
Indicator: Change in Sooner Allocation Upon Revisiting is Negative	661	0.31	0.46	0	0	0	1	1
Indicator: Change in Sooner Allocation Upon Revisiting is Positive	661	0.34	0.47	0	0	0	1	1
Fraction Present Biased, All Interest Rates	661	0.30	0.28	0	0	0.20	0.80	1
Fraction Present Biased, Non-Implemented Interest Rates	661	0.30	0.29	0	0	0.25	0.75	1
Indicator: more elastic in the far time frame	661	0.33	0.47	0	0	0	1	1
Fraction of Decisions Consistent with Law of Demand	661	0.81	0.17	0.25	0.63	0.88	1	1
Fraction of All Tokens Allocated to "Sooner"	661	0.36	0.18	0	0.11	0.38	0.57	
Days to First Disbursement at Revisit (Targeted)	661	9.22	4.42	2	3	10	15	16
Days to First Disbursement at Revisit (Actual)	661	8.98	4.45	1	3	9	15	16
Indicator: Days to First Disbursement (Targeted) is 6 days or less	661	0.33	0.47	0	0	0	1	1
Implemented Interest Rate	661	0.58	0.32	0.1	0.1	0.75	1	1
Shocks								
Death in Family	661	0.02	0.15	0	0	0	0	1
Shock to Expected HH Income	661	114.21	714.59	-2985	-90	0	350	13735
Spouse minus own allocation to sooner (MK)	661	712.25	488.89	0	0	700	1400	2000

Notes: Both baseline and revisit datasets are at individual level. Baseline dataset (Panel A) composed of wife-husband pairs interviewed separately in Jan-Feb 2010. Revisit dataset (Panel B) constructed by first randomly choosing 2/3 of households surveyed at baseline and then randomly choosing either husband or wife within household. Revisit interviews occurred in Mar-Apr 2010, with target revisit date randomly chosen to fall between 46 to 59 days after baseline interview (16 to 2 days prior to first "far" period disbursement at day 61).

Table 2: Number (of 8) Positive Changes in Later Allocation with Increase in r

Number of Consistent Pairs	(1) Real Data	(2) Simulated Data
0	0.00%	0.00%
1	0.28%	0.04%
2	0.61%	1.56%
3	1.96%	12.08%
4	7.38%	33.92%
5	14.05%	35.49%
6	21.48%	14.77%
7	22.97%	2.05%
8	31.28%	0.09%

Notes: Table presents share of individuals whose allocations in 8 pairs of choices (with adjacent interest rates) are consistent with law of demand. Data in column (1) are from baseline sample (for details, see Table 1). Data from column (2) are from random-choice simulations described in Online Appendix section 4.

Table 3: Allocations to Later, in Malawi Kwacha, by Time Frame and Rate of Return

			Percentiles					Percent at
	Mean	Std. Dev	10th	25th	50th	75th	90th	a corner
Panel A: Allocations to Later								
Near period								
t+30 at r=10%	1295.9	524.8	660	1100	1320	1650	2090	13%
t+30 at r=25%	1535.8	602.1	750	1250	1500	1875	2500	14%
t+30 at r=50%	1930.5	734.0	1050	1500	1950	2550	3000	16%
t+30 at r=75%	2256.8	885.1	1050	1750	2275	2975	3500	17%
t+30 at r=100%	2713.7	1045.4	1200	2000	2800	3600	4000	22%
Far period								
t+90 at r=10%	1306.7	518.7	660	1100	1320	1650	2090	12%
t+90 at r=25%	1565.4	590.0	875	1250	1500	2000	2500	14%
t+90 at r=50%	1922.9	733.2	900	1500	1950	2400	3000	16%
t+90 at r=75%	2306.5	872.0	1225	1750	2275	2975	3500	18%
t+90 at r=100%	2757.1	1030.8	1400	2000	2800	3800	4000	23%
Panel B: Allocations to Sooner								
Near period								
t+30 at r=10%	821.8	477.2	100	500	800	1000	1400	10%
t+30 at r=25%	771.4	481.5	0	500	800	1000	1400	11%
t+30 at r=50%	712.9	489.3	0	300	700	1000	1300	14%
t+30 at r=75%	710.2	505.9	0	300	700	1000	1400	14%
t+30 at r=100%	643.0	522.6	0	200	600	1000	1400	20%
Far period								
t+90 at r=10%	812.3	471.7	100	500	800	1000	1400	9%
t+90 at r=25%	747.3	471.8	0	400	800	1000	1300	12%
t+90 at r=50%	718.1	488.8	0	400	700	1000	1400	14%
t+90 at r=75%	681.8	498.3	0	300	700	1000	1300	16%
t+90 at r=100%	621.4	515.4	0	100	600	1000	1300	21%

Notes: Data are from baseline sample (for details, see Table 1). Table presents allocations to "later" date (either t=30 or t=90) for each of 10 choices presented to respondents. Baseline interview is at t=0. First set of 5 choices is in "near" period, when allocations are between t=1 and t=31. 2nd set of 5 choices is in "far" period, when allocations are between t=61 and t=91. Rates of return to waiting until "later" date (interest rates) take on values of 10%, 25%, 50%, 75%, and 100%. Allocations between sooner and later date must be made in 100MK increments, out of total budget of 2000MK.

Table 4: Determinants of Fraction Consistent or Fraction Present-biased

	Dependent variable:			
	Fraction consistent		Fraction present-biased	
	(1)	(2)	(3)	(4)
Male	0.029*	0.024*	0.001	0.003
	(0.015)	(0.014)	(0.013)	(0.013)
Age 35 or under	-0.029	-0.013	0.017	0.011
	(0.021)	(0.019)	(0.018)	(0.018)
36-56 yrs old	-0.021	-0.020	0.009	0.008
	(0.017)	(0.015)	(0.015)	(0.014)
Some primary school	-0.032*	-0.030*	0.031*	0.030*
	(0.019)	(0.016)	(0.016)	(0.016)
Primary school	-0.036	-0.021	0.017	0.011
	(0.027)	(0.023)	(0.023)	(0.022)
More than primary school	-0.068**	-0.070**	0.046	0.046
	(0.034)	(0.030)	(0.031)	(0.030)
Have adequate maize	0.032*	0.022	-0.007	-0.003
	(0.018)	(0.016)	(0.015)	(0.015)
Baseline wealth (100s of MK)	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Words recalled	0.004	0.001	0.002	0.003
	(0.006)	(0.005)	(0.005)	(0.005)
Raven's Tests Correct	-0.001	0.006	-0.006	-0.009
	(0.008)	(0.007)	(0.007)	(0.007)
Financial Literacy Questions Correct	0.008	-0.001	-0.004	-0.001
	(0.009)	(0.008)	(0.007)	(0.007)
Number of relatives in the village	0.001	0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Adherence to law of demand ratio [0,1]		0.695***		-0.255***
		(0.038)		(0.034)
Indicator: more elastic in the far time frame		-0.100***		0.044***
		(0.012)		(0.012)
Constant	0.456***	-0.069*	0.252***	0.442***
	(0.032)	(0.041)	(0.028)	(0.040)
N	2142	2142	2142	2142
Adjusted R-squared	0.00	0.20	-0.00	0.04
P-value that all HH characteristics = 0	0.09	0.18	0.75	0.69

Note: Dependent variable in columns 1 and 2 is the fraction of the five choices pairs that were dynamically consistent. Dependent variable in columns 3 and 4 is the fraction of choice pairs that exhibited present bias. Unit of observation is individuals included in the baseline sample. All allocations made in Jan-Feb 2010.

Table 5: Determinants of revisions toward sooner

Ordinary least-squares regressions

Dependent variable: Change in sooner allocation upon revisiting (MK)

	(1)	(2)	(3)	(4)	(5)	(6)
Preferences under commitment						
Fraction Present Biased, Non-Implemented Interest Rates		196.385**	195.808**	196.736**	198.033**	214.286**
		(95.222)	(95.110)	(95.457)	(97.578)	(95.871)
Fraction of all tokens allocated to "sooner"		-523.457***	-512.533**	-495.282**	-472.658**	-468.704**
		(144.727)	(183.875)	(185.305)	(208.907)	(228.647)
Indicator: days to first disbursement (targeted) <=6		107.737**	111.270**	113.743**	113.971**	124.629**
		(50.162)	(50.508)	(50.763)	(50.890)	(51.040)
Indicator: more elastic in the far time frame		127.723*	131.860*	138.471*	137.956*	136.351*
		(76.722)	(77.815)	(77.303)	(77.797)	(78.877)
Fraction Present Biased		-200.517	-204.079	-206.583	-207.747	-216.585
* Indicator: more elastic in the far time frame		(175.339)	(177.404)	(178.197)	(180.001)	(180.320)
Financial sophistication						
Fraction of Decisions Consistent with Law of Demand			0.813	21.584	20.991	6.693
			(177.867)	(178.276)	(178.423)	(178.688)
Words recalled			-1.384	-0.818	-0.737	0.437
			(19.274)	(19.169)	(19.287)	(19.275)
Raven's Tests Correct			-29.722	-30.415	-30.295	-22.245
			(28.502)	(28.697)	(28.608)	(29.117)
Financial Literacy Questions Correct			16.379	14.878	14.671	25.197
			(28.644)	(28.452)	(28.672)	(28.771)
Shocks						
Death in the family (indic.)				62.129	63.379	55.954
				(203.497)	(203.721)	(202.864)
Shortfall in expected hh income (MK)				0.053	0.052	0.049
				(0.041)	(0.041)	(0.038)
Social pressure						
Spouse minus own allocation to sooner (MK)					0.013	0.044
					(0.064)	(0.081)
Number of relatives in the village					0.830	1.589
					(3.489)	(3.382)
Rate of return to waiting						
Implemented interest rate {.1,.25,.5,.75,1}	-143.004*	-230.279**	-227.048**	-217.948**	-216.638**	-222.626**
	(78.279)	(81.576)	(81.585)	(81.543)	(82.219)	(82.654)
Baseline characteristics						
Male	125.169**	102.197**	103.061**	105.891**	101.623*	57.796
	(49.851)	(49.639)	(51.948)	(51.810)	(53.367)	(61.461)
Age 35 or under	198.798**	183.041**	179.567**	177.550**	177.044**	283.013**
	(71.742)	(70.736)	(70.943)	(71.396)	(71.965)	(109.969)
36-56 yrs old	117.670**	107.302**	110.236**	113.512**	112.177**	178.055**
	(54.887)	(54.200)	(53.493)	(53.665)	(55.422)	(64.326)
Some primary school	-66.952	-81.393	-76.593	-74.929	-75.660	-32.505
	(70.212)	(68.760)	(72.070)	(70.397)	(70.858)	(72.125)
Primary school	-159.166*	-164.017*	-160.505*	-169.354*	-170.945*	-126.996
	(85.954)	(84.113)	(90.934)	(90.073)	(90.245)	(92.344)
More than primary school	-215.094**	-230.233**	-218.818*	-222.391*	-222.868*	-148.056
	(105.964)	(105.190)	(118.268)	(116.837)	(116.832)	(119.628)
Have adequate maize	35.744	21.233	25.514	22.494	20.949	3.560
	(56.325)	(55.263)	(56.060)	(56.891)	(57.838)	(57.848)
Total HH Wealth	-0.122	-0.125	-0.125	-0.130	-0.128	-0.124
	(0.089)	(0.086)	(0.085)	(0.084)	(0.085)	(0.088)
Controls for:						
Spousal characteristics	-	-	-	-	-	Y
R-squared (adj.)	0.02	0.05	0.04	0.05	0.04	0.05
N	661	661	661	661	661	661
P-val of F-test: Frac PB + **1**(Δεf) + Frac PB x **1**(Δεf) = 0		0.29	0.30	0.38	0.29	0.26
P-value of F-test: financial sophistication variables jointly 0			0.84	0.84	0.84	0.86

Significance levels: * 10%, ** 5%, *** 1%. Robust standard errors in parentheses.

Notes: Unit of observation is individual included in revisit sample. Spousal characteristics controls are: fraction present biased for all choices, indicators for age category, indicators for education category, word recall, ravens score, financial literacy score, and fraction of choices adhering to law of demand.

FOR ONLINE PUBLICATION
Appendix for "Revising Commitments: Field Evidence on the Adjustment of Prior Choices"

January 26, 2016

1 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 centre 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 kilometres 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 neighbouring villages.

2 Variable definitions

The key dependent variable we analyse 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) <= 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).[2]

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.

[2] All other coefficients in the regression remain essentially identical.

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 disbursement 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.[6] 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.[7] Looking across columns, the stability of coefficient estimates on particular independent variables provides a sense of the

type 2, who are only successfully revisited if days to first disbursement is >6. So when days to first disbursement is greater than six, the sample is composed of both types 1 and 2, while otherwise it is only composed of type 1. Let μ_1 and μ_2 be mean revision for type 1 and 2 individuals, respectively. We observe μ_1, and the problem is to estimate the value of μ_2 such that there is actually no "effect" of days to first disbursement <=6, and all the observed effect in Table 6 is due to selection. The formula for μ_2 is $\mu_2 = \frac{(\alpha+\gamma)(\mu_1-\beta)-\alpha\mu_1}{\gamma}$, where β is the coefficient on days to first disbursement less than or equal to six in the table (124.629), α is the revisit success rate for type 1 individuals (0.861), γ is the reduction in the revisit success rate due to revisiting 6 or fewer days to first disbursement (0.108), and μ_1 is the mean revision for type 1 individuals (mean revision for those with days to first disbursement ¡=6, 150.0). The formula gives μ_2 = -968.20. So $\mu_1 - \mu_2$ = 150.0 - (-968.20) = 1,118.20.

[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 β

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 β, the larger the revision towards sooner will be upon revisit. Put differently, the coefficient on the discount factor β 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 behaviour 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.[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)], ... [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

[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 behaviour 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 behaviour 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.

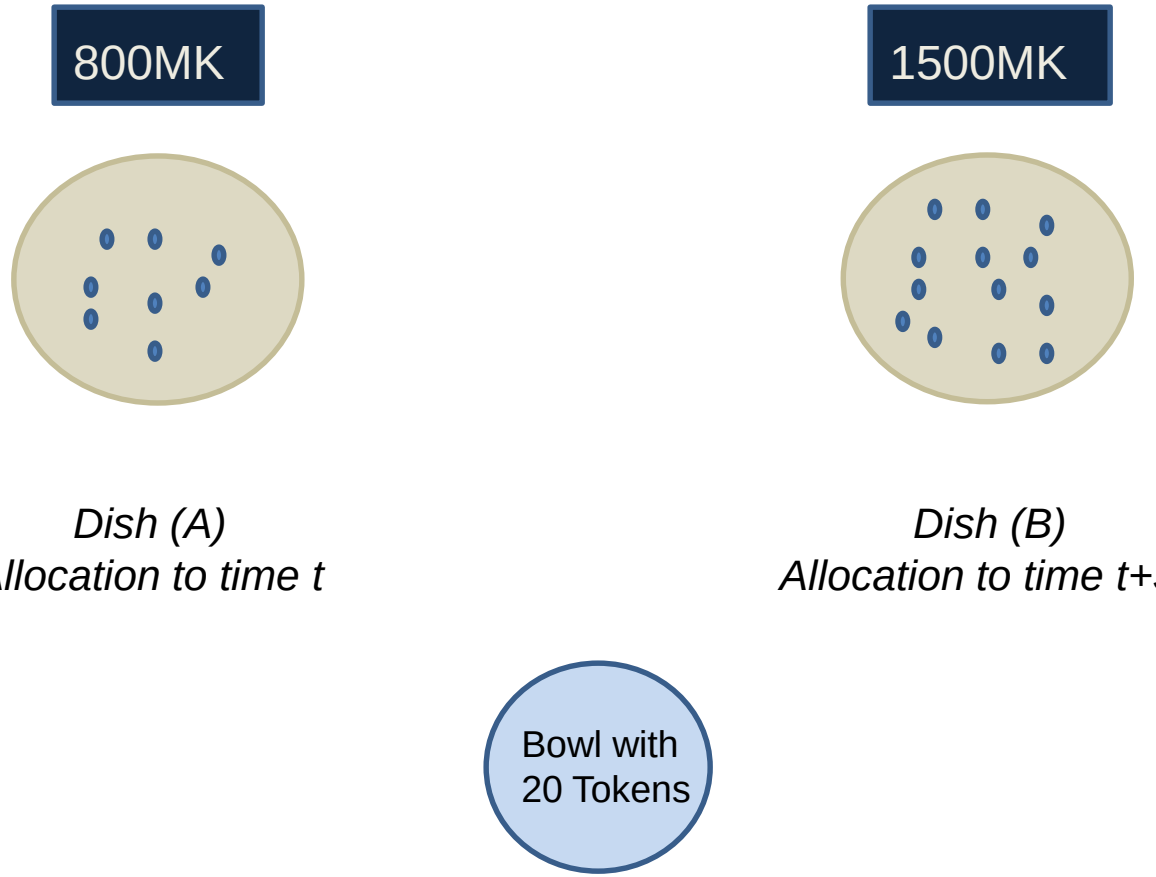
Indeed, the coefficient on "Fraction Present Bias" in column 4 is small in magnitude and statistically insignificant (albeit with a rather large confidence interval). This small coefficient stands in contrast with that of column 3, replicating column 6 of Table 5.

Under random choice, individuals that appear more elastic in the far time

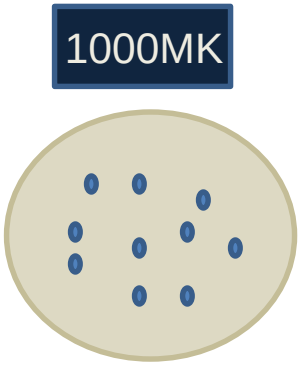
frame do not necessarily revise allocations towards sooner. The point estimates in columns 2 and 4 of the indicator "more elastic in the far time frame" are small and the confidence interval large suggesting that they are not significantly different from zero. The interaction between the indicator and "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 behaviour. 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 behaviour.

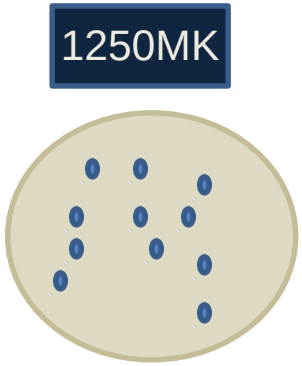
Appendix Figure 1: Schematic of the Preference Elicitation Method; Example with r=0.25



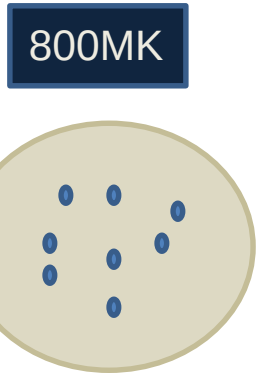
Appendix Figure 2: Schematic of the Revising Procedure; Example with r=0.25



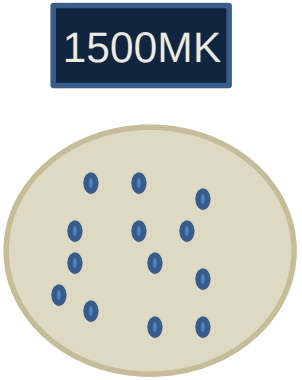
Dish (A')
(Re)allocation to time t



Dish (B')
(Re)allocation to time t+30

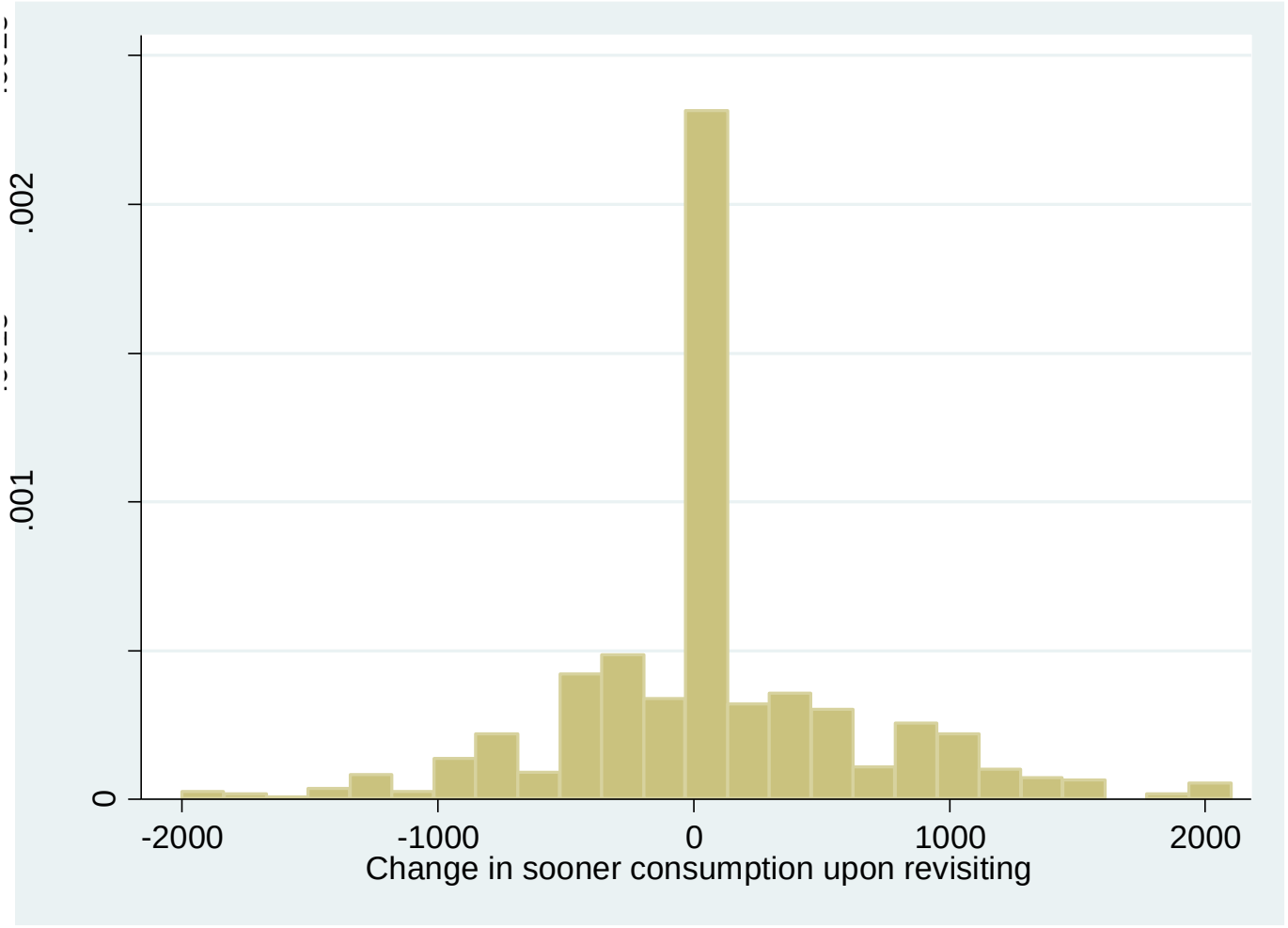


Dish (A)
Original allocation to time t



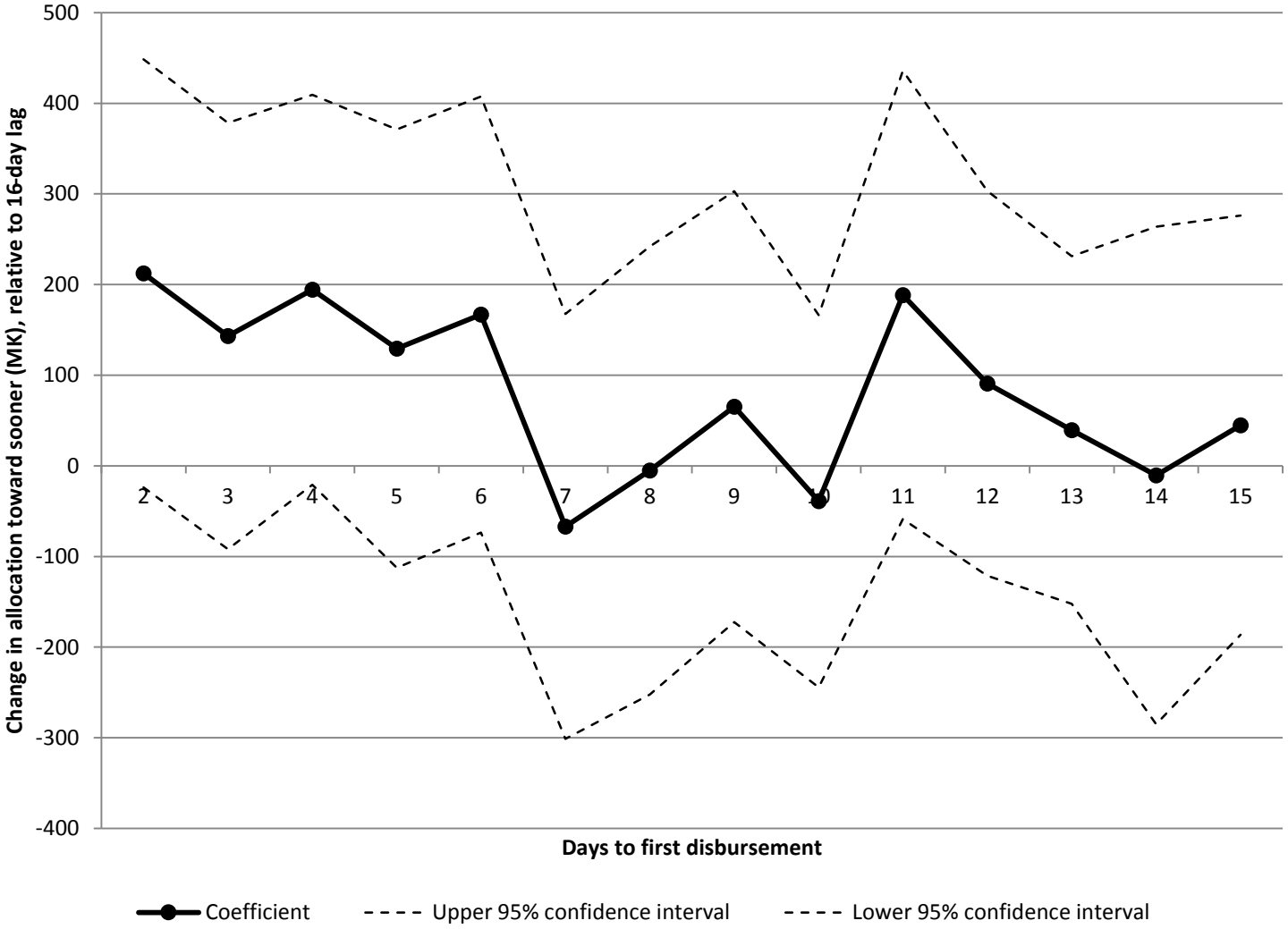
Dish (B)
Original allocation to time t+30

Appendix Figure 3: Distribution of Change in Sooner Allocation Upon Revisiting



Notes: Initial allocations made in Jan-Feb 2010. Revisions made in Mar-Apr 2010 in a revisit targeted at a randomized 2-16 days prior to date of first disbursement in "far" period. (Date of first disbursement in far period is day t=61 from initial visit in Jan-Feb 2010.) N=664.

Appendix Figure 4: Impact of targeted days before disbursement on revisions toward sooner



Notes: Figure plots coefficient (and 95% confidence interval) on indicator variables for each separate value of days to first disbursement (omitted category is 16 days to first disbursement). Dependent variable is change in allocation to sooner upon revisiting (in MK). Other right-hand-side variables are as in Table 6, column 6.

Appendix Table 1: Tests of baseline balance with respect to randomized independent variables
Ordinary least-squares regressions

Dependent variable:	Fraction Present Biased, Non-Implemented Interest Rates	Fraction of all tokens allocated to "sooner"	Indicator: more elastic in the far time frame	Fraction of Decisions Consistent with Law of Demand	Words Recalled	Raven's Tests Correct	Financial Literacy Questions Correct	Spouse minus own allocation to sooner (MK)	Number of Relatives in the Village	Male	Age	Years of Education	Have Adequate Maize	Total HH Wealth
Panel A														
Indicator: days to first disbursement (targeted) <=6	-0.008	-0.035**	0.001	-0.009	0.026	0.062	-0.074	80.415*	-0.381	0.067	-0.030	-0.144	0.002	-58.843***
	(0.022)	(0.016)	(0.039)	(0.015)	(0.117)	(0.074)	(0.080)	(41.852)	(0.573)	(0.041)	(1.145)	(0.263)	(0.036)	(13.074)
R-squared	0.0002	0.0078	0.0000	0.0007	0.0001	0.0010	0.0013	0.0057	0.0006	0.0039	0.0000	0.0004	0.0000	0.0169
N	661	661	661	661	661	661	661	661	661	661	661	661	661	661
Panel B														
Implemented interest rate {.1,.25,.5,.75,1}	-0.033	-0.120***	0.016	0.034*	0.448***	0.075	0.012	129.492**	-1.205	-0.083	-0.996	0.558	-0.058	12.668
	(0.033)	(0.021)	(0.057)	(0.020)	(0.164)	(0.109)	(0.116)	(61.027)	(0.887)	(0.061)	(1.620)	(0.381)	(0.052)	(22.587)
R-squared	0.0016	0.0432	0.0001	0.0040	0.0113	0.0007	0.0000	0.0070	0.0027	0.0028	0.0006	0.0031	0.0019	0.0004
N	661	661	661	661	661	661	661	661	661	661	661	661	661	661

Significance levels: * 10%, ** 5%, *** 1%. Robust standard errors in parentheses.
Notes: Unit of observation is individual included in revisit sample. In Panel A, each column presents results from regression of given dependent variable on indicator for days to first disbursement (targeted) <=6 and a constant. Panel B is similar but independent variable is interest rate on randomly-chosen choice. Constant term included in each regression but not reported.

Appendix Table 2: Determinants of Change in ln(c) From Sooner to Later
Ordinary least-squares estimates

Dependent variable: Change in ln(c) from sooner to later

	delay of 1 vs. 31 days		delay of 61 vs. 91 days	
	(1)	(2)	(3)	(4)
Interest rate (r)	0.948***	0.949***	0.935***	0.937***
	(0.029)	(0.029)	(0.029)	(0.029)
Male		0.026		0.024
		(0.040)		(0.042)
Age 35 or under		0.061		0.061
		(0.056)		(0.058)
36-56 yrs old		0.056		0.041
		(0.044)		(0.047)
Some primary school		0.008		-0.012
		(0.047)		(0.050)
Primary school		-0.038		-0.087
		(0.072)		(0.072)
More than primary school		0.030		-0.010
		(0.097)		(0.089)
Have adequate maize		0.020		0.046
		(0.047)		(0.046)
log(Baseline wealth)		0.021		0.033**
		(0.015)		(0.015)
Words recalled		0.022		0.022
		(0.014)		(0.015)
Raven's Tests Correct		-0.027		-0.039*
		(0.020)		(0.021)
Financial Literacy Questions Correct		0.020		0.032
		(0.026)		(0.025)
Number of relatives in the village		0.005*		0.007**
		(0.002)		(0.002)
Constant	0.384***	0.224**	0.414***	0.279**
	(0.030)	(0.084)	(0.030)	(0.089)
N	8937	8937	8931	8931
Adjusted R-squared	0.09	0.09	0.08	0.09
P-value that all HH characteristics = 0		0.15		0.00

Notes: Data are from baseline sample (for details, see Table 1). Unit of observation is a respondent/choice-pair (10 pairs p
Dependent variable is ln(sooner allocation) minus ln(later allocation) for the given pair. Rates of return to waiting until "la
rates) take on values of 0.1, 0.25, 0.50, 0.75, and 1.0. Standard errors are clustered at the individual level.

Appendix Table 3: Determinants of revisions toward sooner (using all first-stage choices in measure of present bias)
Ordinary least-squares regressions

Dependent variable: Change in sooner allocation upon revisiting (MK)

	(1)	(2)	(3)	(4)	(5)	(6)
Preferences under commitment						
Fraction Present Biased, All Interest Rates		286.665**	286.704**	288.292**	286.870**	300.144**
		(99.115)	(98.648)	(98.901)	(101.714)	(99.220)
Fraction of all tokens allocated to "sooner"		-611.504***	-621.904***	-603.870**	-616.336**	-624.312**
		(143.293)	(181.288)	(182.737)	(208.430)	(228.595)
Indicator: days to first disbursement (targeted) <=6		106.130**	108.715**	111.114**	111.495**	123.182**
		(49.853)	(50.232)	(50.487)	(50.581)	(50.739)
Indicator: more elastic in the far time frame		122.927	127.578*	134.100*	133.827*	134.468*
		(76.574)	(77.377)	(76.689)	(77.155)	(78.120)
Fraction Present Biased		-198.481	-208.243	-210.636	-210.563	-228.184
* Indicator: more elastic in the far time frame		(174.326)	(175.632)	(176.154)	(177.720)	(178.815)
Financial sophistication						
Fraction of Decisions Consistent with Law of Demand			-38.112	-16.931	-17.023	-32.864
			(176.963)	(177.262)	(177.536)	(178.066)
Words recalled			-3.169	-2.656	-2.508	-1.080
			(19.049)	(18.951)	(19.061)	(19.103)
Raven's Tests Correct			-30.248	-30.899	-31.233	-23.157
			(28.241)	(28.431)	(28.353)	(28.778)
Financial Literacy Questions Correct			18.058	16.610	16.426	26.498
			(28.395)	(28.213)	(28.411)	(28.547)
Shocks						
Death in the family (indic.)				56.708	57.508	46.997
				(200.094)	(200.230)	(199.304)
Shortfall in expected hh income (MK)				0.052	0.052	0.049
				(0.041)	(0.041)	(0.038)
Social pressure						
Spouse minus own allocation to sooner (MK)					0.000	0.000
					(.)	(.)
Number of relatives in the village					-0.007	0.016
					(0.064)	(0.081)
Rate of return to waiting						
Implemented interest rate {.1,.25,.5,.75,1}	-143.004*	-222.189**	-218.912**	-210.226**	-210.377**	-215.207**
	(78.279)	(80.267)	(80.157)	(80.151)	(80.712)	(81.341)
Baseline characteristics						
Male	125.169**	93.665*	94.254*	97.065*	94.696*	47.834
	(49.851)	(49.368)	(51.664)	(51.548)	(53.092)	(61.239)
Age 35 or under	198.798**	175.843**	171.018**	169.068**	168.438**	269.208**
	(71.742)	(70.408)	(70.608)	(71.099)	(71.659)	(109.609)
36-56 yrs old	117.670**	100.638*	103.106*	106.326**	105.352*	170.131**
	(54.887)	(54.126)	(53.527)	(53.690)	(55.520)	(64.376)
Some primary school	-66.952	-86.414	-81.035	-79.640	-80.233	-37.182
	(70.212)	(68.346)	(71.568)	(69.926)	(70.310)	(71.616)
Primary school	-159.166*	-167.474**	-164.183*	-172.907*	-172.882*	-129.103
	(85.954)	(83.972)	(90.815)	(89.970)	(90.006)	(92.005)
More than primary school	-215.094**	-235.358**	-223.634*	-227.334**	-227.345**	-155.417
	(105.964)	(103.978)	(116.170)	(114.814)	(114.695)	(117.392)
Have adequate maize	35.744	24.945	28.773	25.964	25.092	7.664
	(56.325)	(54.932)	(55.716)	(56.558)	(57.544)	(57.537)
Total HH Wealth	-0.122	-0.136	-0.138	-0.143*	-0.143*	-0.139
	(0.089)	(0.085)	(0.084)	(0.083)	(0.084)	(0.087)
Controls for:						
Spousal characteristics	-	-	-	-	-	Y
R-squared (adj.)	0.02	0.06	0.06	0.06	0.06	0.06
N	661	661	661	661	661	661
P-value for test that financial sophistication variables are jointly 0			0.82	0.82	0.82	0.83

Significance levels: * 10%, ** 5%, *** 1%. Robust standard errors in parentheses.
Notes: Unit of observation is individual included in revisit sample. Spousal characteristics controls are: fraction present biased for all choices, indicators for age category, indicators for education category, word recall, ravens score, financial literacy score, and fraction of choices adhering to law of demand.

Appendix Table 4: Determinants of revisions toward sooner; investigating future bias
Ordinary least-squares regressions

Dependent variable: Change in sooner allocation upon revisiting (MK)

	(1)
Preferences under commitment	
Fraction Present Biased, Non-Implemented Interest Rates	
Fraction "Future Biased", Non-Implemented Interest Rates	132.499
	(109.337)
Fraction of all tokens allocated to "sooner"	-452.503**
	(230.072)
Indicator: days to first disbursement (targeted) <=6	122.894**
	(51.276)
Indicator: more elastic in the far time frame	
Indicator: more elastic in the near time frame	54.314
	(65.255)
Fraction Present Biased	
* Indicator: more elastic in the far time frame	
Fraction Future Biased	-164.751
* Indicator: more elastic in the near time frame	(189.029)
Financial sophistication	
Fraction of Decisions Consistent with Law of Demand	-25.795
	(177.881)
Words recalled	1.647
	(19.368)
Raven's Tests Correct	-22.046
	(29.346)
Financial Literacy Questions Correct	23.829
	(28.401)
Shocks	
Death in the family (indic.)	74.866
	(204.919)
Shortfall in expected hh income (MK)	0.047
	(0.038)
Social pressure	
Spouse minus own allocation to sooner (MK)	0.040
	(0.081)
Number of relatives in the village	2.069
	(3.376)
Rate of return to waiting	
Implemented interest rate {.1,.25,.5,.75,1}	-203.520**
	(82.844)
Baseline characteristics	
Male	62.044
	(61.747)
Age 35 or under	279.716**
	(110.625)
36-56 yrs old	171.308**
	(65.380)
Some primary school	-21.206
	(72.143)
Primary school	-113.713
	(92.404)
More than primary school	-133.158
	(119.644)
Have adequate maize	0.398
	(57.832)
Total HH Wealth	-0.126
	(0.092)
Controls for:	
Spousal characteristics	Y
R-squared (adj.)	0.04
N	661
P-value of F-test: financial sophistication variables jointly 0	0.87

Significance levels: * 10%, ** 5%, *** 1%. Robust standard errors in parentheses.
Notes: Unit of observation is individual included in revisit sample. Spousal characteristics contr
fraction present biased for all choices, indicators for age category, indicators for education categ
recall, ravens score, financial literacy score, and fraction of choices adhering to law of demand.

Appendix Table 5: Determinants of inclusion in revisit sample
Ordinary least-squares regressions

Dependent variable: Indicator for inclusion in revisit sample

Preferences under commitment		
	Fraction Present Biased, Non-Implemented Interest Rates	0.002
		(0.036)
	Fraction of all tokens allocated to "sooner"	-0.032
		(0.097)
	Indicator: days to first disbursement (targeted) <=6	-0.108***
		(0.025)
	Indicator: more elastic in the far time frame	0.000
		(0.021)
Financial sophistication		
	Fraction of Decisions Consistent with Law of Demand	0.001
		(0.076)
	Words recalled	-0.015*
		(0.008)
	Raven's Tests Correct	0.019
		(0.013)
	Financial Literacy Questions Correct	0.005
		(0.014)
Social pressure		
	Spouse minus own allocation to sooner (MK)	-0.000
		(0.000)
	Number of relatives in the village	0.001
		(0.002)
Rate of return to waiting		
	Implemented interest rate {.1,.25,.5,.75,1}	0.057
		(0.036)
Baseline characteristics		
	Male	0.036
		(0.030)
	Age 35 or under	0.080*
		(0.044)
	36-56 yrs old	-0.037
		(0.031)
	Some primary school	-0.034
		(0.029)
	Primary school	-0.055
		(0.045)
	More than primary school	-0.056
		(0.045)
	Have adequate maize	0.007
		(0.027)
	Total HH Wealth	-0.000***
		(0.000)
Controls for:		
	Spousal characteristics	Y
	R-squared (adj.)	0.05
	N	722

Significance levels: * 10%, ** 5%, *** 1%. Robust standard errors in parentheses.
Notes: Unit of observation is individuals targeted for inclusion in revisit sample. Dependent variable has mean of 0.916. Right-hand-side variables are identical to column 6, Table 5, except for omission of shock variables ("death in family" and "shortfall in expected household income"), because shock variables are not available for attriters. See Table 5 for other notes.

Appendix Table 6: Bounds on bias due to selection into revisit sample
Ordinary least-squares regressions

Dependent variable: Change in sooner allocation upon revisiting (MK), with missing values replaced

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Assumed value of dep. var. is initial sooner allocation (from baseline) plus:	600	400	200	0	-200	-400	-600
Preferences under commitment							
Fraction Present Biased, Non-Implemented Interest Rates	188.075**	188.428**	189.409**	190.389**	191.590**	190.766**	190.480**
	(89.646)	(87.857)	(87.025)	(87.127)	(87.780)	(89.037)	(90.643)
Fraction of all tokens allocated to "sooner"	-431.017**	-431.037**	-435.711**	-440.386**	-472.268**	-504.767**	-532.108**
	(212.525)	(209.926)	(208.729)	(209.229)	(210.069)	(212.175)	(215.454)
Indicator: days to first disbursement (targeted) <=6	190.458***	166.996***	143.898***	120.799***	102.239**	85.558*	69.613
	(47.697)	(46.728)	(46.253)	(46.295)	(46.714)	(47.520)	(48.665)
Indicator: more elastic in the far time frame	118.921	121.014	122.667	124.320*	124.198*	122.206*	119.686
	(76.443)	(75.305)	(74.496)	(74.124)	(73.996)	(74.167)	(74.648)
Fraction Present Biased * I: more elastic in the far time frame	-201.488	-200.695	-199.093	-197.490	-199.087	-194.141	-188.180
	(171.861)	(169.208)	(167.507)	(166.843)	(166.775)	(167.321)	(168.380)
Financial sophistication							
Fraction of Decisions Consistent with Law of Demand	-9.292	-18.216	-24.916	-31.617	-29.723	-23.491	-18.644
	(167.521)	(165.128)	(163.775)	(163.742)	(164.856)	(167.153)	(170.413)
Words recalled	2.599	0.597	-2.044	-4.684	-7.389	-9.646	-11.951
	(18.575)	(18.211)	(17.955)	(17.852)	(17.881)	(18.031)	(18.291)
Raven's Tests Correct	-26.198	-23.466	-20.751	-18.036	-13.969	-11.436	-9.015
	(27.354)	(26.906)	(26.629)	(26.588)	(26.758)	(27.067)	(27.549)
Financial Literacy Questions Correct	18.682	20.090	22.382	24.674	25.011	24.225	22.637
	(26.756)	(26.274)	(26.002)	(26.004)	(26.242)	(26.669)	(27.278)
Social pressure							
Spouse minus own allocation to sooner (MK)	0.048	0.048	0.047	0.046	0.042	0.037	0.037
	(0.075)	(0.074)	(0.073)	(0.073)	(0.074)	(0.075)	(0.077)
Number of relatives in the village	0.904	0.999	1.128	1.258	1.542	1.810	2.027
	(3.002)	(2.958)	(2.954)	(2.989)	(3.052)	(3.134)	(3.234)
Rate of return to waiting							
Implemented interest rate {.1,.25,.5,.75,1}	-237.479***	-228.823***	-221.385***	-213.948***	-205.420***	-197.399***	-189.790**
	(76.074)	(75.169)	(74.722)	(74.892)	(75.447)	(76.350)	(77.603)
Baseline characteristics							
Male	36.907	43.139	49.183	55.226	60.228	65.668	71.304
	(57.667)	(56.386)	(55.688)	(55.611)	(55.853)	(56.536)	(57.626)
Age 35 or under	222.553**	233.238**	246.405**	259.572**	274.053***	284.066***	293.232***
	(104.766)	(103.316)	(102.463)	(102.314)	(102.764)	(103.725)	(105.261)
36-56 yrs old	174.042***	166.743***	159.667***	152.592**	149.573**	148.445**	147.938**
	(62.218)	(60.697)	(59.714)	(59.314)	(59.488)	(60.172)	(61.360)
Some primary school	-11.283	-18.190	-24.808	-31.427	-36.844	-40.713	-43.337
	(69.318)	(68.555)	(68.185)	(68.249)	(68.553)	(69.288)	(70.453)
Primary school	-70.667	-83.080	-96.279	-109.478	-116.624	-120.851	-124.711
	(88.457)	(86.798)	(85.851)	(85.754)	(86.205)	(87.199)	(88.825)
More than primary school	-106.320	-118.121	-129.817	-141.513	-148.054	-149.923	-149.138
	(114.271)	(113.347)	(113.012)	(113.342)	(114.132)	(115.228)	(116.509)
Have adequate maize	0.967	0.939	0.897	0.856	0.536	0.386	0.358
	(53.242)	(52.125)	(51.481)	(51.379)	(51.696)	(52.452)	(53.559)
Total HH Wealth	0.036	0.012	-0.013	-0.037	-0.064*	-0.091***	-0.099***
	(0.044)	(0.040)	(0.037)	(0.035)	(0.034)	(0.034)	(0.038)
Controls for:							
Spousal characteristics	Y	Y	Y	Y	Y	Y	Y
R-squared (adj.)	0.0488	0.0455	0.0431	0.0407	0.0414	0.0428	0.0429
N	722	722	722	722	722	722	722

Significance levels: * 10%, ** 5%, *** 1%. Robust standard errors in parentheses.
Notes: Dependent variable constrained to remain within 0 or 2000 range. Right-hand-side variables are identical to column 6, Table 5, except for omission of shock variables ("death in family" and "shortfall in expected household income"), because shock variables are not available for attriters. See Table 5 for other notes.

Appendix Table 7: Differential effects by gender
Ordinary least-squares regressions

Dependent variable: Change in sooner allocation upon revisiting (MK)

		(1) Females	(2) Males	P-value, F-test of equality of male and female coeffs.
Preferences under commitment				
	Fraction Present Biased, Non-Implemented Interest Rates	87.743	199.450*	0.592
		(170.970)	(118.672)	
	Fraction of all tokens allocated to "sooner"	-571.908*	-320.799	0.557
		(311.144)	(293.251)	
	Indicator: days to first disbursement (targeted) <=6	100.810	158.428**	0.574
		(74.563)	(70.368)	
	Indicator: more elastic in the far time frame	15.146	211.019*	0.208
		(110.801)	(109.072)	
	Fraction Present Biased * I: more elastic in the far time frame	154.424	-433.262*	0.108
		(272.822)	(242.459)	
Financial sophistication		-142.424	125.398	
	Fraction of Decisions Consistent with Law of Demand	-142.424	125.398	0.459
		(259.858)	(250.989)	
	Words recalled	-14.072	29.148	0.294
		(25.342)	(32.410)	
	Raven's Tests Correct	20.486	-75.382*	0.098
		(40.829)	(40.900)	
	Financial Literacy Questions Correct	13.736	36.079	0.718
		(49.662)	(36.949)	
Shocks				
	Death in the family (indic.)	438.137	-247.190	0.097
		(327.464)	(250.228)	
	Shortfall in expected hh income (MK)	0.051	0.038	0.858
		(0.048)	(0.060)	
Social pressure				
	Spouse minus own allocation to sooner (MK)	0.067	0.031	0.822
		(0.105)	(0.121)	
	Number of relatives in the village	1.602	2.912	0.865
		(6.580)	(4.037)	
Rate of return to waiting				
	Implemented interest rate {.1,.25,.5,.75,1}	-247.920**	-181.689	0.692
		(122.849)	(113.634)	
Baseline characteristics				
	Age 35 or under	211.016	288.188*	0.733
		(152.870)	(166.146)	
	36-56 yrs old	53.810	233.604**	0.201
		(116.154)	(78.953)	
	Some primary school	-168.582*	143.000	0.041
		(98.918)	(115.128)	
	Primary school	-296.744*	5.910	0.155
		(172.443)	(124.417)	
	More than primary school	-508.001**	146.502	0.007
		(173.267)	(165.505)	
	Have adequate maize	38.929	-15.836	0.646
		(85.864)	(82.556)	
	Total HH Wealth	-0.339*	-0.040	0.144
		(0.175)	(0.106)	
Controls for:				
	Spousal characteristics	Y	Y	
	R-squared (adj.)	0.06	0.04	
	N	325	336	

Significance levels: * 10%, ** 5%, *** 1%. Robust standard errors in parentheses.
Notes: Unit of observation is individual included in revisit sample. Column 1 restricts to females in revisit sample. Column 2 restricts to males in revisit sample. See Table 5 for other notes.

Appendix Table 8: Sample of consistent individuals
Ordinary least-squares regressions

Dependent variable: Change in sooner allocation upon revisiting (MK)

	(1)	(2)	(3)	(4)	(5)	(6)
Preferences under commitment						
Fraction Present Biased, Non-Implemented Interest Rates		196.385**	195.808**	196.736**	198.033**	214.286**
		(95.222)	(95.110)	(95.457)	(97.578)	(95.871)
Fraction of all tokens allocated to "sooner"		-523.457***	-512.533**	-495.282**	-472.658**	-468.704**
		(144.727)	(183.875)	(185.305)	(208.907)	(228.647)
Indicator: days to first disbursement (targeted) <=6		107.737**	111.270**	113.743**	113.971**	124.629**
		(50.162)	(50.508)	(50.763)	(50.890)	(51.040)
Indicator: more elastic in the far horizon		127.723*	131.860*	138.471*	137.956*	136.351*
		(76.722)	(77.815)	(77.303)	(77.797)	(78.877)
Fraction Present Biased * I: more elastic in the far horizon		-200.517	-204.079	-206.583	-207.747	-216.585
		(175.339)	(177.404)	(178.197)	(180.001)	(180.320)
Financial sophistication						
Fraction of Decisions Consistent with Law of Demand			0.813	21.584	20.991	6.693
			(177.867)	(178.276)	(178.423)	(178.688)
Words recalled			-1.384	-0.818	-0.737	0.437
			(19.274)	(19.169)	(19.287)	(19.275)
Raven's Tests Correct			-29.722	-30.415	-30.295	-22.245
			(28.502)	(28.697)	(28.608)	(29.117)
Financial Literacy Questions Correct			16.379	14.878	14.671	25.197
			(28.644)	(28.452)	(28.672)	(28.771)
Shocks						
Death in the family (indic.)				62.129	63.379	55.954
				(203.497)	(203.721)	(202.864)
Shortfall in expected hh income (MK)				0.053	0.052	0.049
				(0.041)	(0.041)	(0.038)
Social pressure						
Spouse minus own allocation to sooner (MK)					0.013	0.044
					(0.064)	(0.081)
Number of relatives in the village					0.830	1.589
					(3.489)	(3.382)
Rate of return to waiting						
Implemented interest rate {.1,.25,.5,.75,1}	-143.004*	-230.279**	-227.048**	-217.948**	-216.638**	-222.626**
	(78.279)	(81.576)	(81.585)	(81.543)	(82.219)	(82.654)
Baseline characteristics						
Male	125.169**	102.197**	103.061**	105.891**	101.623*	57.796
	(49.851)	(49.639)	(51.948)	(51.810)	(53.367)	(61.461)
Age 35 or under	198.798**	183.041**	179.567**	177.550**	177.044**	283.013**
	(71.742)	(70.736)	(70.943)	(71.396)	(71.965)	(109.969)
36-56 yrs old	117.670**	107.302**	110.236**	113.512**	112.177**	178.055**
	(54.887)	(54.200)	(53.493)	(53.665)	(55.422)	(64.326)
Some primary school	-66.952	-81.393	-76.593	-74.929	-75.660	-32.505
	(70.212)	(68.760)	(72.070)	(70.397)	(70.858)	(72.125)
Primary school	-159.166*	-164.017*	-160.505*	-169.354*	-170.945*	-126.996
	(85.954)	(84.113)	(90.934)	(90.073)	(90.245)	(92.344)
More than primary school	-215.094**	-230.233**	-218.818*	-222.391*	-222.868*	-148.056
	(105.964)	(105.190)	(118.268)	(116.837)	(116.832)	(119.628)
Have adequate maize	35.744	21.233	25.514	22.494	20.949	3.560
	(56.325)	(55.263)	(56.060)	(56.891)	(57.838)	(57.848)
Total HH Wealth	-0.122	-0.125	-0.125	-0.130	-0.128	-0.124
	(0.089)	(0.086)	(0.085)	(0.084)	(0.085)	(0.088)
Controls for:						
Spousal characteristics	-	-	-	-	-	Y
R-squared (adj.)	0.02	0.05	0.04	0.05	0.04	0.05
N	661	661	661	661	661	661
P-value for test that financial sophistication variables are jointly 0			0.84	0.84	0.84	0.86

Significance levels: * 10%, ** 5%, *** 1%. Robust standard errors in parentheses.
Notes: Unit of observation is individual included in revisit sample. Spousal characteristics controls are: fraction present biased for all choices, indicators for age category, indicators for education category, word recall, ravens score, financial literacy score, and fraction of choices adhering to law of demand.

Appendix Table 9: Structural estimation of individual discount factors
Ordinary least-squares regressions

Dependent variable: Change in sooner allocation upon revisiting (MK)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: All individuals							
β (CARA utility function)		-39.356	-39.945	-42.161	-42.509	-34.321	-42.323
		(41.461)	(42.381)	(42.699)	(42.873)	(43.175)	(52.425)
Observations	663	663	663	663	663	663	663
Adjusted R-squared	0.02	0.04	0.04	0.04	0.04	0.04	0.04
β (CRRA utility function)		-0.497	-2.316	-4.346	-5.021	2.153	-9.123
		(41.272)	(41.761)	(41.937)	(42.183)	(42.256)	(54.184)
Observations	663	663	663	663	663	663	663
Adjusted R-squared	0.02	0.04	0.04	0.04	0.03	0.04	0.04
Panel B: Consistent individuals							
β (CARA utility function)		-47.137	-59.173	-60.143	-59.114	-54.405	-36.899
		(52.403)	(53.935)	(54.684)	(54.966)	(55.905)	(65.657)
Observations	663	513	513	513	513	513	513
Adjusted R-squared	0.02	0.03	0.03	0.03	0.03	0.03	0.03
β (CRRA utility function)		-37.19	-53.99	-57.234	-57.313	-50.812	-48.01
		(52.206)	(53.269)	(53.270)	(53.523)	(54.188)	(67.965)
Observations	663	513	513	513	513	513	513
Adjusted R-squared	0.02	0.03	0.03	0.03	0.03	0.03	0.03
Implemented Interest Rate	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Days to First Disbursement at Revisit (Targeted)	No	Yes	Yes	Yes	Yes	Yes	Yes
Fraction of All Tokens Allocated to Sooner	No	Yes	Yes	Yes	Yes	Yes	Yes
Fraction of Decisions Consistent with Law of Demand	No	No	Yes	Yes	Yes	Yes	Yes
Aptitude Questions	No	No	Yes	Yes	Yes	Yes	Yes
Shocks	No	No	No	Yes	Yes	Yes	Yes
Spouse minus own allocation to sooner (MK)	No	No	No	No	Yes	Yes	Yes
Number of Relatives in Village	No	No	No	No	Yes	Yes	Yes
Spouse Characteristics	No	No	No	No	No	Yes	Yes
Indicator: more elastic in the far time frame	No	No	No	No	No	No	Yes
Interaction of beta and Days to First Disbursement at Revisit (Targeted)	No	No	No	No	No	No	Yes

Notes: Unit of observation is individual included in revisit sample. In Panel A the sample includes all individuals, while in Panel B the sample excludes individuals that are inconsistent in 3 or more pairs in Table 2. As in Table 5, the dependent variable is the change in sooner allocation upon revisiting (MK). The bottom of the table reports the explanatory variables from Table 5 that are included in the regression, although the only coefficient reported is that of the individual discount factor β. This discount factor is estimated using NLLS by positing a flexible δ-β model with a four period utility function with two curvature parameters, one for each time frame. See Online Appendix 3.8 for more details.

Appendix Table 10: Random vs. real choice

Ordinary least-squares regressions

Dependent variable: Change in sooner allocation upon revisiting (MK)

	Real	Simulated	Real	Simulated	Real	Simulated	Real	Simulated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Preferences under commitment								
Fraction Present Biased [0,1]	302.639***	697.415						
	(98.564)	[389.6, 1021.9]						
Fraction Present Biased, Non-Implemented Interest Rate			214.286**	34.238				
			(95.871)	[-268.9, 332.0]				
Fraction Future Biased [0,1]					29.766	-679.544		
					(114.235)	[-1001.6, -367.0]		
Fraction Future Biased, Non-Implemented Interest Rate							132.499	-18.802
							(109.337)	[-321.0, 294.0]
Fraction of all tokens allocated to "sooner" [0,1]	-523.857**	-1957.084			-501.659**	-1954.710		
	(226.975)	[-2756.0, -1159.7]			(231.465)	[-2743.4, -1171.1]		
Fraction of all tokens allocated to "sooner", non-Implemented			-468.704**	86.926			-452.503**	85.534
Interest Rate			(228.647)	[-671.0, 843.6]			(230.072)	[-662.7, 827.2]
Indicator: More elastic in the far time frame	116.174	35.188	136.351*	60.365				
	(77.070)	[-195.2, 269.0]	(78.877)	[-166.9, 283.7]				
Fraction Present Biased * More elastic in the far time frame	-180.327	15.67781						
	(177.194)	[-457.5, 479.6]						
Fraction Present Biased, Non-Implemented Interest Rate			-216.585	-75.382				
* More elastic in the far time frame			(180.320)	[-545.2, 378.2]				
Indicator: More elastic in the near time frame					69.279	-18.690	54.314	-46.002
					(66.777)	[-247.8, 221.1]	(65.255)	[-266.9, 180.5]
Fraction Future Biased * More elastic in the near time frame					-229.376	-54.583		
					(198.535)	[-533.1, 400.8]		
Fraction Future Biased, Non-Implemented Interest Rate							-164.751	39.472
* More elastic in the near time frame							(189.029)	[-418.5, 478.4]
Financial sophistication								
Fraction of Decisions Consistent with Law of Demand	-38.007	125.772	6.693	378.397	-134.885	120.780	-25.795	379.639
	(178.238)	[-303.3, 549.9]	(178.688)	[-69.9, 850.0]	(176.775)	[-310.5, 551.8]	(177.881)	[-65.7, 840.3]
Social pressure								
Spouse minus own allocation to sooner (MK)	0.068	0.005			0.061	0.005		
	(0.078)	[-0.26, 0.28]			(0.079)	[-0.27, 0.27]		
Spouse minus own allocation to sooner (MK), non-Implemented			0.044	0.004			0.04	0.004
Interest Rate			(0.081)	[-0.26, 0.26]			(0.081)	[-0.26, 0.26]
Rate of return to waiting								
Implemented interest rate {.1,.25,.5,.75,1}	-213.079***	-1.163	-222.626***	-5.413	-197.844**	-1.349	-203.520**	-0.920
	(81.088)	[-167.3, 167.4]	(82.654)	[-180.1, 166.9]	(81.285)	[-177.0, 167.2]	(82.844)	[-178.5, 171.5]
Controls for Spousal characteristics	Y	Y	Y	Y	Y	Y	Y	Y
N	661	661	661	661	661	661	661	661

Significance levels: * 10%, ** 5%, *** 1%. Robust standard errors in parentheses.

Notes: Unit of observation is individual included in revisit sample. The standard error appears below the coefficient in regressions using the real experimental data (odd numbered columns). The 95% confidence interval appears below the coefficient of regressions using simulated data in square brackets. All regressions include other baseline characteristics controls that are included in regressions of column 6 of Table 5 and of Appendix Table 4.