

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 direct examination of intertemporal choice revision and its determinants. We provide novel evidence of self-control problems: revisions of money allocations toward the present are positively associated with measures of present-bias from an earlier survey, as well as 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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Well-being depends importantly on the ability to make and execute intertemporal plans. This is perhaps especially true among the poor in developing countries, who often live close to subsistence and for whom suboptimal plans can be particularly consequential. The importance of intertemporal choices for the poor spurs concern when they appear to leave consumption unsmooth, save at a low rate, or fail to use inexpensive agricultural and health inputs.

A long tradition in development economics suggests that these observed choices may be optimal given the constraints that individuals face and the incompleteness of markets.¹ More recently however, researchers have suggested that these sub-optimal choices may be the result of self-control problems. This attention is motivated, in part, by the fact that these problems appear common in developed economies, and there is no reason to think that the poor would be immune. The new focus on self-control is also motivated by evidence that low-income people fail to execute their plans for intertemporal choices. A growing literature also reveals demand among the poor for commitment devices designed to overcome problems of self-control.²

In this paper, we investigate several potential sources of failure to pursue what appear to be individually optimal intertemporal choices. We study the reasons why, after choices about future consumption are made, they are often 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 modeling strategy is founded, in part, on evidence of non-constant time discounting. Several studies

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

²Ashraf et al (2006), Duflo et al (2011), Dupas and Robinson (2011), Brune et al (2013).

can be interpreted to show that time discount rates decline as tradeoffs are pushed into the temporal distance.³ 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, these static preference reversals have many implications.⁴ Most important, when utility is time-separable, present-biased time discounting implies 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. Self-control problems and a demand for commitment may thus emerge.

However, there has long been a gap in this literature. Until very recently, there have been no studies of whether static preference reversals are associated with time-inconsistency. To our knowledge, Halevy (2012) is the sole prior experiment in which the revision of previous decisions is a variable of interest. More recently, Augenblick et al (2013) 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. First, 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. Second, the design of commitment devices may be imperfect, further dampening observed demand (Beshears et al., 2011).

³Ainslie (1992), Thaler (1991) and Loewenstein and Elster (1992) provide reviews.

⁴Early 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 reviews of empirical applications.

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 allow several interpretations.⁵ For example, static preference reversals may reflect predictable changes in the marginal utility of consumption.⁶ In addition, static preference reversals may reflect inattention, confusion about tradeoffs, or responses to perceived experimenter demands.⁷ Furthermore, even if preferences under 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.

As we evaluate the relationship between static preference reversals and the revision of prior choices, we provide an assessment of the relative importance of three other explanations for the revision of prior choices. One explanation is social pressure. Individuals from close-knit communities in developing countries are often obliged to share their income with relatives and friends, and such pressure may be an important factor behind an apparent inability to pursue privately optimal choices, including the revision of previous decisions.⁹ Second, unexpected events could motivate revisions to otherwise optimal consumption paths. Finally, individuals could simply make mistakes in their original deci-

⁵Halevy (2012) 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.

⁶This observation has been made by Andersen et al. (2008), Andreoni and Sprenger (2012) and Noor (2011), who note that proper inference about time discounting requires information about the curvature of the utility function.

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

⁸Making 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.

⁹See, 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 (2011).

sions, and seek to revise them later. From a policy standpoint, it is important to understand the mechanisms that drive revision behavior because they determine 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 investigate these issues, we implemented 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 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 adhere to the law of demand; individuals allocated more income to later periods when offered higher rates of return to waiting. We interpret this to indicate that experimental subjects mostly understood the choices made in a way that economic theory has some hope of explaining. 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 behavior, we find that revisions are common, often substantial in size, and shift money both sooner and later. We find qualified support for “present”-biased preferences as a driver of 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 $t = 61$). The support is qualified because the relationship between “present”-biased, static preference reversals and revisions toward sooner is evident only among subjects whose initial preferences appear time-separable. That is, the predicted link between these two behaviors holds only when the static preference reversals cannot easily be reconciled by anticipated changes in the marginal utility of consumption. This finding is significant because it demonstrates the empirical relevance, in a developing context, of an alternative source of static preference reversals. Put differently, we find evidence of an important reason why not all “present”-biased preference reversals are the result of time-inconsistency.

In contrast, 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 2.1 presents the results of stage one of the experiment and choices under commitment. Section 2.2 discusses the results of stage two and choices upon revisiting. Section 3 clarifies our contribution to the related literature, and section 4 concludes.

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

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

¹¹In 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).

¹²Grain 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.

little about their preferences (Fuchs, 1982, Chabris, et al. 2008).¹³ Augenblick, et al. (2013) 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 take special care to 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 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

¹³To 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.

¹⁴See the Online Appendix for further details on sampling. See Brune, et al. (2013) for details on the broader study from which our study participants were drawn.

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 120 inhabitants, including two 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 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

¹⁵Based 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.

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 were aware of it. 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. 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 behavior 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

¹⁶This “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.

¹⁷Recall 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.

61.¹⁸ Revisits occurred in March and April 2010.¹⁹

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.

¹⁸In 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.

¹⁹In 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 the experimenters' "real" intent and affect behavior in later experiments. The participants in this field experiment are not part of such common pool.

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 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 of 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.

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 Results

2.1 Stage One Choices

Our discussion of the results begins with the analysis of stage one choices. This analysis includes a theoretical framework to aid interpretation and the definition of measures derived from behavior that are used when analyzing stage two of the experiment.

2.1.1 Theoretical Framework

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-nonseparability (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 for $\beta \neq 1$. This formulation of utility also allows for a certain form of time-nonseparability. While utility is separable in consumption across periods, the marginal utilities of consumption may depend on time (thus the time subscript on $u_s(\cdot)$). This captures the possibility (especially relevant in Malawi) that consumption has different marginal value at different times. 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). As mentioned, if the marginal utility of consumption depends on time, then decision-makers may exhibit static preference reversals not due to time-inconsistency.

Abstracting from the discrete choice set of the experiment, we can interpret

the stage one decisions about the “near” time frame as solving

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

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

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

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

$$u'_1(c_1^*) = (1 + r)\beta\delta u'_2(c_2^*) \quad \text{(FOC Near)}$$

$$u'_3(c_3^*) = (1 + r)\delta u'_4(c_4^*). \quad \text{(FOC Far)}$$

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). 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.

2.1.2 Adherence to the Law of Demand

While discount parameters are not identified without additional assumptions, additive separability and monotonicity of the flow utilities together make a strong prediction: if participants solve problems (Near) and (Far), then the allocation to the later period, measured in kwacha, should increase with the rate of return to waiting r .²⁰

²⁰To 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

We therefore use this central prediction of standard theory to measure consistency, and view it as a pre-condition for interpreting the choices in stage one. To the extent that choices are inconsistent with the law of demand, it suggests either poor participant understanding, or that 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 this basic prediction of rational choice with monotonic, separable utilities.²¹

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 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 the percentage of consistent pairs is significantly lower at 57%. We interpret this result to indicate that, on average, participants understood the trade-offs they were facing and that the additively-separable model is reasonable for describing their behavior.

The average rate of consistency with the law of demand masks important heterogeneity, however. Table 2 presents the distribution of participants by the number of times (out of eight) they increased their later allocation with

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.

²¹In addition, the typical deviation from consistency is fairly modest in size. The median violation could be made consistent with a reallocation of less than two tokens.

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.²² We investigate the correlates of this heterogeneity below.

2.1.3 Intertemporal Tradeoffs in Stage One

With greater confidence that a standard model can describe participants' behavior, we further examine decisions in stage one. Table 3 presents summary statistics of the distribution of kwacha allocations made in stage one, separately by the near and far time frames.

The first notable feature of this distribution is that participants typically balance allocations somewhat between the two periods, consistent with the canonical model of Section 2.1.1. 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.

The second important feature of this distribution is heterogeneity in preferences. 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.

²²Column 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.

2.1.4 Static Preference Reversals

The third important feature of the distribution in Table 3 is its apparent 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. However, this average stability is misleading. It obscures the 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.²³

These patterns in stage one indicate that static preference reversals are common and that “present”-biased reversals are only somewhat more common. There is evidence, however, that these static reversals are not merely noise. Among those participants who exhibit static reversals, a substantial fraction (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 less than 8%. In addition, the tendency to be consistent or “present”-biased is 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

²³Under random choice, only 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).

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.

Columns 2 and 4 reveal two important relationships. First, there is a strong association between adherence to the law of demand (Section 2.1.2) and static preference reversals.²⁴ Greater adherence to the law of demand is associated with more dynamically consistent choices. This suggests that the tendency to exhibit static preference reversals may be due, in part, 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 2.2.3, below, this is what we would expect if some respondents exhibit static preference reversals because their marginal utilities of consumption depend on time. We investigate this possibility, as well as the role of confusion about the experiment, in our analysis of stage two revision behavior below.

2.2 Stage Two Choices

2.2.1 Theoretical Framework

Having described some basic features of choices under commitment in stage one, we now examine behavior in stage two of the experiment. To guide interpretation, we return to the model in Section 2.1.1. If the revisit is sufficiently

²⁴There 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.

close to period 3 then the respondent solves

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

Interior solutions here satisfy

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

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, it follows that 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 behavior is time-inconsistent $\tilde{c}_3^* > c_3^*$. Analogously, if ($\beta > 1$) then $\tilde{c}_3^* < c_3^*$.

2.2.2 The Tests

This deterministic analysis suggests 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, 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.

2.2.3 Accounting for Time Non-separability

Because these predictions abstract from the effects of uncertainty, social pressure, and mistakes, the empirical analysis will include proxies for these factors. As important, however, Test 1 assumes that static preference reversals have only one source: non-constant time discounting. As noted above, these reversals can also emerge from time non-separability.

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:

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

$$\sigma, \rho \geq 0.$$

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 “far” time frame will exhibit a “present”-biased static preference reversal and thus appear less patient in the “near”.²⁵ Similarly, if the respondent has a higher elasticity of intertemporal substitution within the “near” time frame ($\sigma < \rho$) then $c_1^* < c_3^*$. Such a respondent 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 ac-

²⁵More formally, if $(1+r) \geq \delta$ and $\sigma > \rho$ then $c_1^* > c_3^*$

commodate this in our empirical analysis by (1) identifying respondents who show differences in curvature across time frames, as evidenced by differences in within-frame responsiveness to the interest rate, and (2) allowing their correlation between static preference reversals and revisions of prior choices to differ from those whose responsiveness to r is the same across time frames.

2.2.4 Accounting for Random Choice

Test 1 is appropriate in a deterministic model of choice where the error term in an empirical implementation of the model is interpreted as unobserved determinants 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. An analogous effect applies to future-biased static preference reversals and revisions toward later.

We combat this confounding effect of implementation error 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.²⁶

²⁶See 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.

2.2.5 Qualitative Features of Revision Behavior

Before studying the determinants of revision behavior, we first provide a simple description 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. The randomization was designed to favor (with two-thirds probability) this far time frame, so we aimed to revisit 722 respondents and present them with a revision opportunity. Of these, we were successful in collecting 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. However, 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%.

2.2.6 Stage One Preferences and Revision Behavior

Table 5 presents the results of ordinary least-squares regressions relating revision behavior to potential determinants of revision. The dependent variable

is the change in sooner allocations upon revisiting (in MK).²⁷

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 accounts for static preference reversals due to “present”-bias.²⁸ We construct a non-parametric measure based on the number of times that a respondent made a “present”-biased preference reversal in stage one.²⁹ We account for the effects of implementation error (see Section 2.2.4) 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.³⁰

However, as discussed in Section 2.2.3, static preference reversals can also

²⁷In Appendix Figure 2’s example, the dependent variable would take the value 200, as two tokens were added to the time t dish compared to the original allocation.

²⁸In the interest of brevity, we focus here on the test for $\beta \leq 1$ and leave analysis of future bias to the Online Appendix.

²⁹An alternative approach would parameterize the utility functions in problems (Near) and (Far) and estimate individual-specific parameters. This method has the disadvantage of relying on functional form assumptions. Common functional forms either have counterfactual implications for Malawi (e.g., for constant absolute risk aversion) or require data on baseline consumption (as in the case constant relative risk aversion) that are not available.

³⁰To 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.

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 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 .³¹ Then, we take the difference in the average responsiveness across time frames, $\Delta\bar{\varepsilon}_f \equiv \bar{\varepsilon}_{far} - \bar{\varepsilon}_{near}$. When $\Delta\bar{\varepsilon}_f$ 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. Among the respondents who were revisited, $\Delta\bar{\varepsilon}_f$ 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 $\Delta\bar{\varepsilon}_f > 0.1$, and zero otherwise. This classifies 33% of the revisited sample as “more elastic” in the later time frame.³²

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

³¹Thus, if l_{rf} denotes the share of consumption allocated to later in time frame f when the rate of return is r , then

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

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

³²Using a continuous measure of the across time frame difference in the responsiveness to the interest rate yields very similar conclusions, but with less precision.

Column 2 of the table shows initial results of Test 1. The results are consistent with the model outlined in Section 2.1.1 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 exists, however, only for those who do not appear systematically more elastic in the “far” time frame. Summing the coefficients on the main effect and on the interaction of fraction “present” biased with the indicator for $\Delta\bar{\varepsilon}_f > 0.1$ (more elastic in the far), we see that those who are more elastic in the far time frame are, on average, entirely time-consistent (the sum of the coefficients is very close to zero and not statistically significant, p-value = 0.545).

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.³³ 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 this control has little effect on other regression coefficients.

³³Section 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.

2.2.7 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 2.1.4 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 behavior. 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.³⁴ Inclusion of these 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.³⁵ This variable should capture pressure to revise one’s allocation toward sooner com-

³⁴Deaths 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 used in extrapolating these results to other settings subject to greater risk.

³⁵As with the present-bias ratio, we exclude the implemented choice from this calculation to guard against a spurious positive relationship caused by random choice.

ing from one’s spouse.³⁶ 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).³⁷ 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 support for a model of quasi-hyperbolic discounting as an account of some respondents’ behavior. 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. By contrast, 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 behavior. 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.

The right-hand-side variables that are statistically significantly related to revision are also economically substantial. Consider the coefficients from col-

³⁶Revisions towards the spousal allocation could happen unwillingly, as the result of pressure from the spouse (Ashraf, 2009 and Schaner, 2011), or willingly, say on the basis of information provided by the spouse as to optimal actions.

³⁷These 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.

umn 6 of Table 5. A useful benchmark 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.³⁸

3 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.

³⁸In the Online Appendix, we provide the following additional analyses. First, as mentioned, we show in Section 3.3 that the indicator we use for the targeted lag to first disbursement is a reasonable approximation. 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 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. Finally, 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.

The experimental literature on time preference is large. Influential recent examples include Halevy (2012), Augenblick, et al. (2013), Andersen, et al. (2008), Benhabib, et al. (2010), and Andreoni and Sprenger (2012) (Frederick, et al. 2002 provide 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 heterogenous sample. We can thus examine the correspondence between subjects' experimental behavior and their "real world" characteristics and behaviors.

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), 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.

4 Conclusion

The consequences of sub-optimal intertemporal choices can be serious, especially among the poor in developing countries. Low-income farmers can go hungry for long periods if they fail to make proper smoothing decisions. They can fail to rise out of extreme poverty if they miss opportunities to make high return investments. 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 consumption smoothing of substantial amounts of future income is very difficult. Several weeks later, prior to the first disbursement 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 new evidence for the existence of self-control problems. Revisions of allocations toward the present are positively associated with measures of “present”-bias from an earlier baseline survey, as well as the (randomly assigned) closeness in time to the first possible date of money disbursement.³⁹ These results complement existing evidence for 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 behavior. In addition, analysis of revision behavior avoids problems of

³⁹This result is reminiscent of Kaur, Kremer, and Mullainathan’s (2013) finding that worker effort increases as a worker’s randomly-assigned payday comes closer.

low demand that may arise if commitment devices are poorly designed.

While only a minority of our sample exhibits “present”-biased static preference reversals, the correlation between these reversals and revision of prior choices toward sooner is relatively large. 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 thus suggest cautious optimism about the prospects for improving the lives 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, the benefits of commitment may be substantial and the costs, in terms of reduced flexibility, only 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 behavior in other parts of the Malawian agricultural year, such as in the post-harvest months. Our findings should therefore be tested for robustness using similar research designs in other time periods and contexts.

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

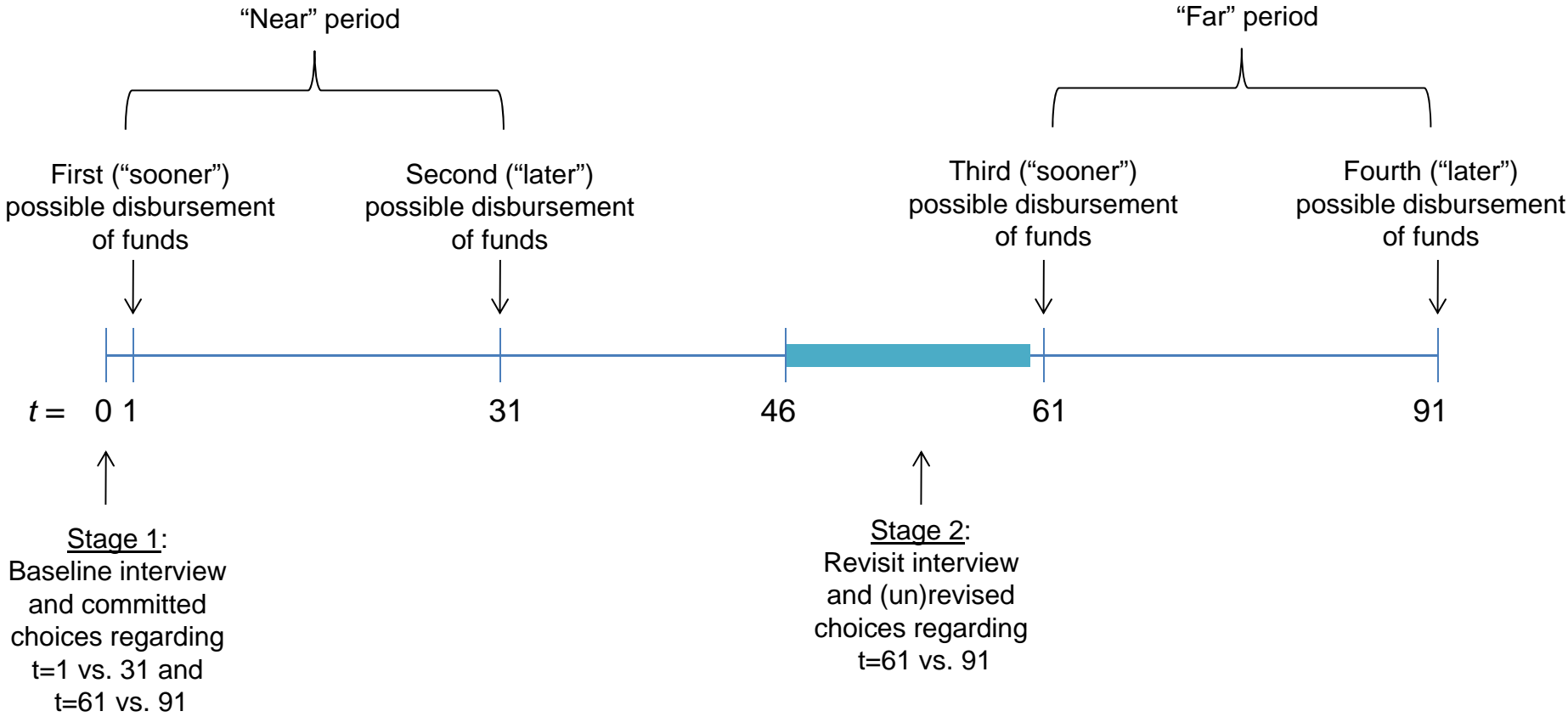


Table 1: Summary Statistics

Variable	N	Mean	Std. Dev	Min	10th pctile	50th pctile	90th pctile	Max
<u>Panel A: Baseline sample (stage 1)</u>								
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
<u>Panel B: Revisit sample (stage 2)</u>								
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

	Mean	Std. Dev	Percentiles					Percent at a corner
			10th	25th	50th	75th	90th	
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%	471.7	100.0	500	800	1000	1400	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

	<u>Dependent variable:</u>			
	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

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-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”

August 5, 2013

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 center in the town of Mponela (population 13,670), and who lived in six traditional authorities (TAs) in the Dowa and Ntchisi districts. To allow relatively easy access to participants and to facilitate their access to the cash disbursements, we included all farmers in these TAs that were 2008-09 members of clubs in which the median club member lives 25 kilometers or less from the disbursement office, located in Mponela.

Scheduling for the stage one visit was stratified across agricultural zones. Within a zone, the order in which clubs were visited was randomly assigned. Scheduling was on a club-by-club basis in order to facilitate field work since members of the same club often live within the same village or in neighboring villages.

2 Variable definitions

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

2.1 Variables collected in baseline survey

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

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

Fraction sooner is the total number of *tokens* allocated to sooner in any of the choices, divided by the total number of tokens to be allocated (20 in each of the ten choices). In regressions, this variable excludes the choice at the implemented interest rate in the calculation.

Fraction of decisions consistent with law of demand is the fraction (out of 8) of pairs of choices adjacent in interest rates where allocation to later rises in rate of return.

More elastic in the far time frame is an indicator for whether a respondent's choices are consistent with a greater responsiveness to the interest rate in the far, relative to the near time frame. For each respondent, we first calculate four

values of the change in the share of consumption allocated to later associated with each of the four incremental increases in the interest rate. We then take the average of these four changes in the consumption share within time frame and use this as a measure of the elasticity of intertemporal substitution within time frame. We then create an indicator that takes on the value 1 when that elasticity is (at least 0.1) larger in the far time frame than in the near and 0 otherwise.

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

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

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

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

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

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

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

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

2.2 Variables collected in revisit survey

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

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

2.3 Variables from administrative (project) data

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

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

Indicator for days to first disbursement (targeted) ≤ 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 10 out of the 13 dependent variables, and in the bottom panel it is not significant for 9 out of 13 dependent variables. Having three out of 13 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. 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. 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 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 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

¹All 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.

²All other coefficients in the regression remain essentially identical.

intervals.

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

All told, the relationship appears to be best summarized by a step function with a positive effect for days two to six prior to disbursement, and zero effect thereafter.

3.4 Future bias vs. “present” bias

The analysis in the main text focuses on the predictions of quasi-hyperbolic discounting models with $\beta \leq 1$. This is natural given the laboratory evidence and well-developed theory surrounding them and other models of present-bias. Future bias is, however, also possible. Models of future bias would imply that respondents would shift allocations toward later as the intertemporal tradeoffs draw near. Appendix Table 4 considers this possibility, replacing the fraction present biased variable with an analogously-defined “fraction future biased” variable. The regression also includes an indicator for “more elastic in the near time frame” and an interaction term between this indicator and fraction future biased. The regression specification is otherwise identical to that 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. This coefficient is not pre-

cisely 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}

³The 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).

⁴In addition, individuals with higher word recall are less likely to be included in the

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 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

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.

⁵Let there be two types of individuals: type 1, who we always successfully revisit, and 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 $\mu_2 = -968.20$. So $\mu_1 - \mu_2 = 150.0 - (-968.20) = 1,118.20$.

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 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.

⁶The 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.

⁷We 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).

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.

4 Simulations of stochastic choice

In this section we assess the adherence of subjects' optimizing behavior to the canonical model of Section 2.1.1 by comparing their choices to those of hypothetical subjects that choose randomly. Given that more than two thirds of individuals choose allocations that deviate at least once from the law of demand, and that more than 90% make at least one different allocation in the “near” compared to the “far” time frame, random choice is a useful benchmark.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 ($[2000,0]$, $[1900, 100(1+r)]$, \dots $[0, 2000(1+r)]$) 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 8 reports the results. We compare coefficients obtained using real data (odd numbered columns) to simulated or random choice data

⁸Other 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.

(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 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 8 we see that when all pairs are included, the coefficient on “Fraction Present Biased” using simulated data is more than twice as large than the coefficient when using the real experimental data. Both coefficients are large and significant at conventional levels, suggesting that a null that the coefficient is zero is insufficiently discerning.

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

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

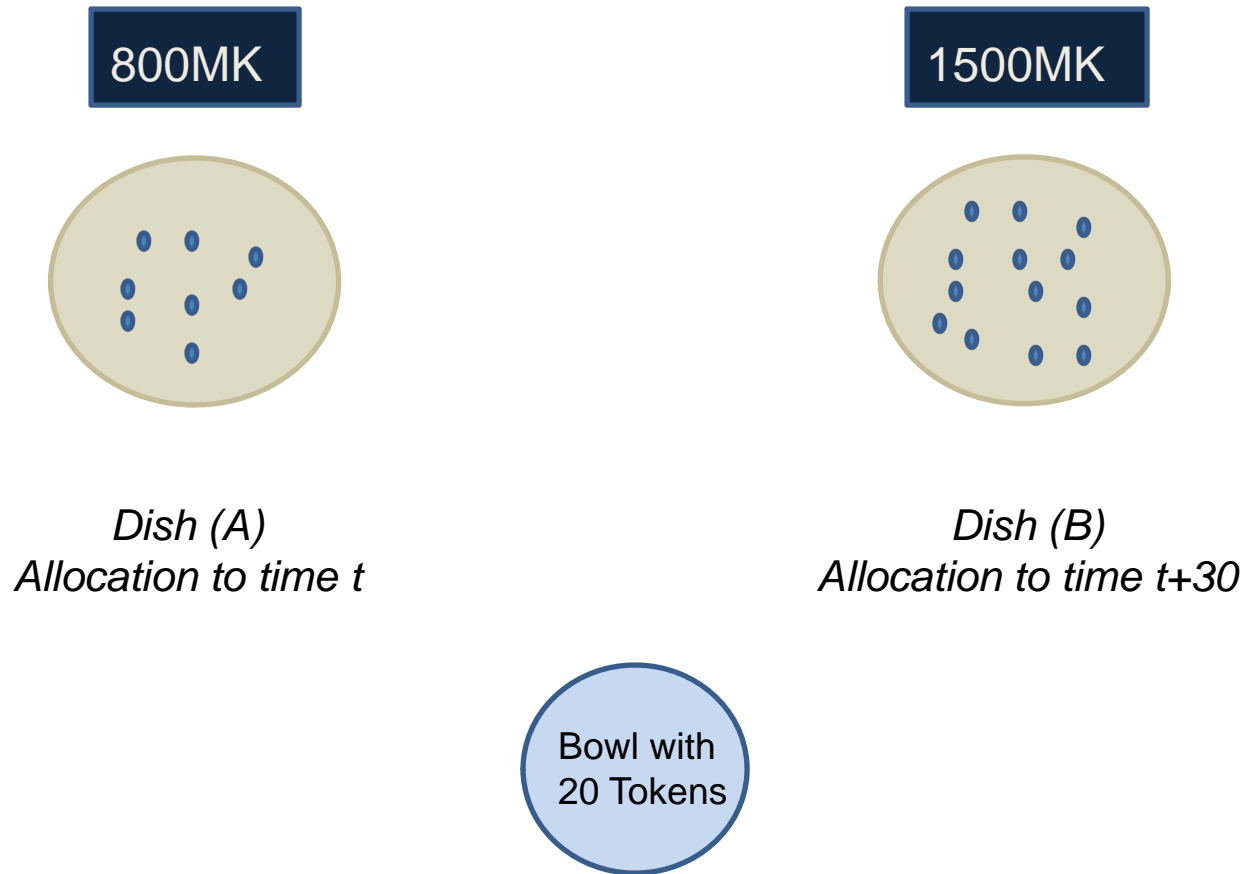
under the implemented interest rate.

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

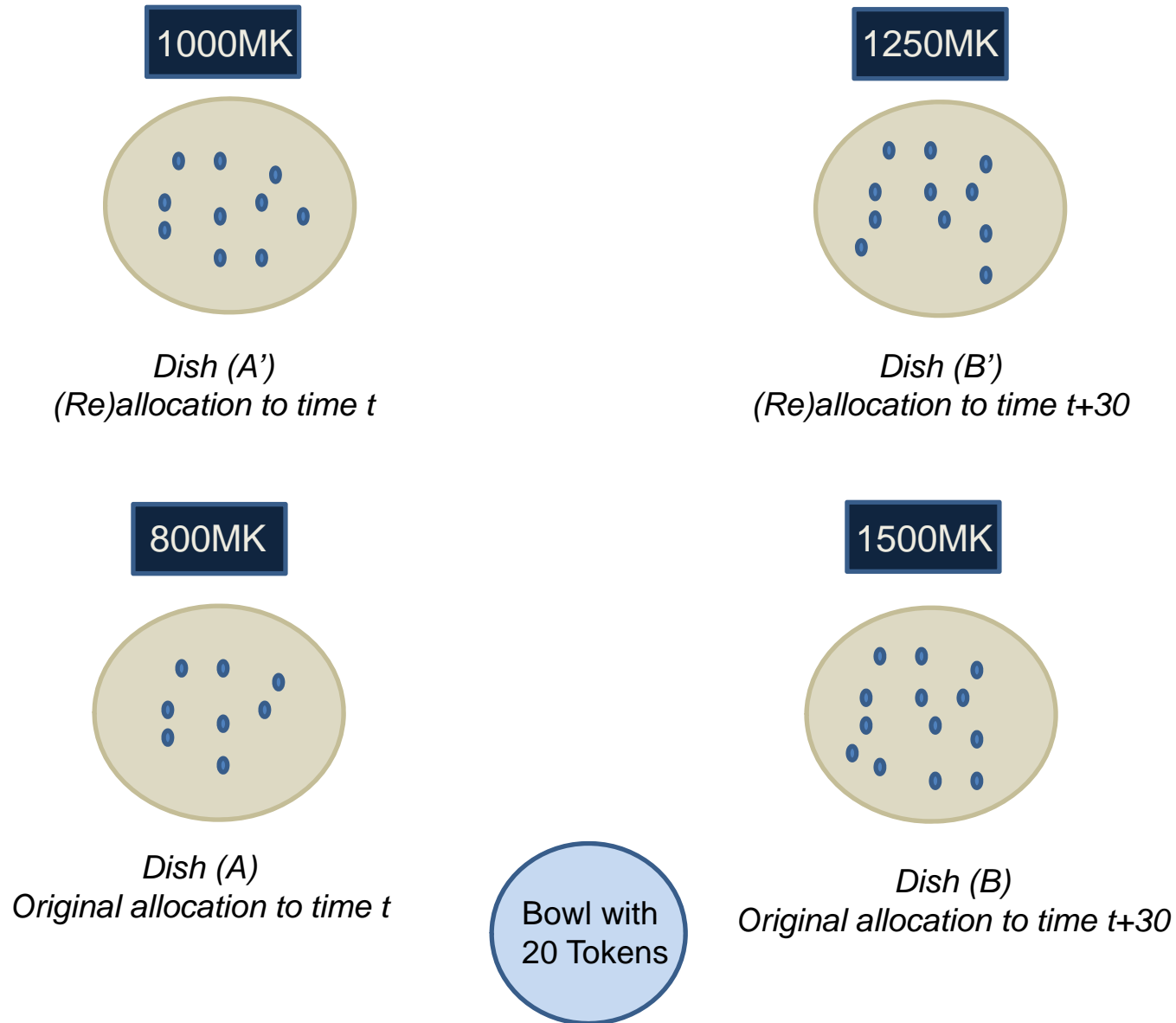
Under random choice, individuals that appear more elastic in the far time frame do not necessarily revise allocations towards sooner. The point estimates in columns 2 and 4 of the indicator “more elastic in the far time frame” are small and the confidence interval large suggesting that they are not significantly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

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

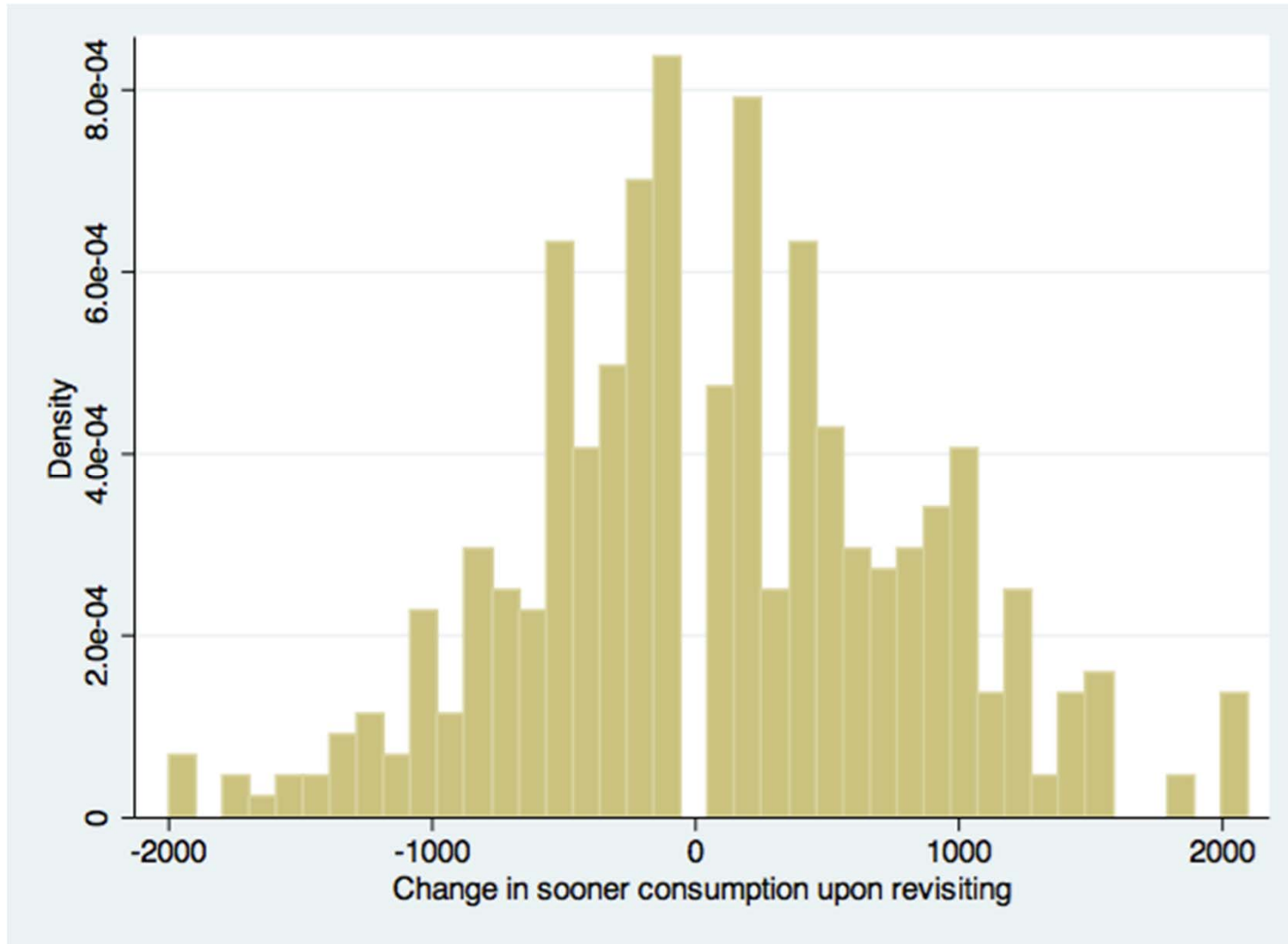
Appendix Figure 1: Schematic of the Preference Elicitation Method; Example with $r=0.25$



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

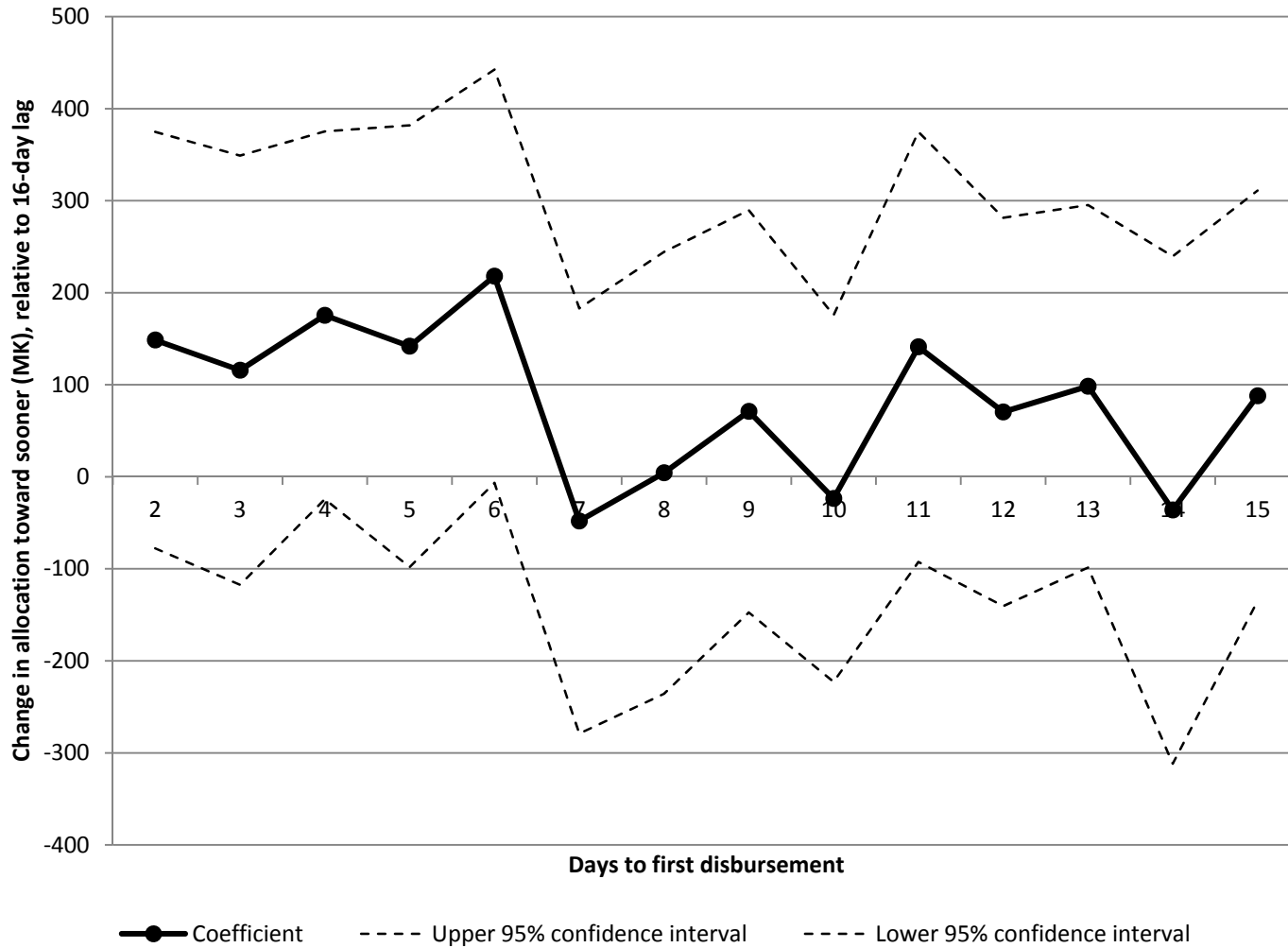


Appendix Figure 3: Distribution of Change in Sooner Allocation Upon Revisiting



Notes: Observations with no change in allocation excluded from histogram. 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.)

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 5, column 6.

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

<u>Dependent variable:</u>	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 (0.022)	-0.035** (0.016)	0.001 (0.039)	-0.009 (0.015)	0.026 (0.117)	0.062 (0.074)	-0.074 (0.080)	80.415* (41.852)	-0.381 (0.573)	0.067 (0.041)	-0.030 (1.145)	-0.144 (0.263)	0.002 (0.036)	-58.843*** (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 (0.033)	-0.120*** (0.021)	0.016 (0.057)	0.034* (0.020)	0.448*** (0.164)	0.075 (0.109)	0.012 (0.116)	129.492** (61.027)	-1.205 (0.887)	-0.083 (0.061)	-0.996 (1.620)	0.558 (0.381)	-0.058 (0.052)	12.668 (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	
Interest rate (r)	0.948*** (0.029)	0.949*** (0.029)	0.935*** (0.029)	0.937*** (0.029)
Male		0.026 (0.040)		0.024 (0.042)
Age 35 or under		0.061 (0.056)		0.061 (0.058)
36-56 yrs old		0.056 (0.044)		0.041 (0.047)
Some primary school		0.008 (0.047)		-0.012 (0.050)
Primary school		-0.038 (0.072)		-0.087 (0.072)
More than primary school		0.030 (0.097)		-0.010 (0.089)
Have adequate maize		0.020 (0.047)		0.046 (0.046)
log(Baseline wealth)		0.021 (0.015)		0.033** (0.015)
Words recalled		0.022 (0.014)		0.022 (0.015)
Raven's Tests Correct		-0.027 (0.020)		-0.039* (0.021)
Financial Literacy Questions Correct		0.020 (0.026)		0.032 (0.025)
Number of relatives in the village		0.005* (0.002)		0.007** (0.002)
Constant	0.384*** (0.030)	0.224** (0.084)	0.414*** (0.030)	0.279** (0.089)
N	8937	8937	8931	8931
Adjusted R-squared	0.09	0.09	0.08	0.09

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

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** (99.115)	286.704** (98.648)	288.292** (98.901)	286.870** (101.714)	300.144** (99.220)
Fraction of all tokens allocated to "sooner"		-611.504*** (143.293)	-621.904*** (181.288)	-603.870** (182.737)	-616.336** (208.430)	-624.312** (228.595)
Indicator: days to first disbursement (targeted) <=6		106.130** (49.853)	108.715** (50.232)	111.114** (50.487)	111.495** (50.581)	123.182** (50.739)
Indicator: more elastic in the far time frame		122.927 (76.574)	127.578* (77.377)	134.100* (76.689)	133.827* (77.155)	134.468* (78.120)
Fraction Present Biased * Indicator: more elastic in the far time frame		-198.481 (174.326)	-208.243 (175.632)	-210.636 (176.154)	-210.563 (177.720)	-228.184 (178.815)
Financial sophistication						
Fraction of Decisions Consistent with Law of Demand			-38.112 (176.963)	-16.931 (177.262)	-17.023 (177.536)	-32.864 (178.066)
Words recalled			-3.169 (19.049)	-2.656 (18.951)	-2.508 (19.061)	-1.080 (19.103)
Raven's Tests Correct			-30.248 (28.241)	-30.899 (28.431)	-31.233 (28.353)	-23.157 (28.778)
Financial Literacy Questions Correct			18.058 (28.395)	16.610 (28.213)	16.426 (28.411)	26.498 (28.547)
Shocks						
Death in the family (indic.)				56.708 (200.094)	57.508 (200.230)	46.997 (199.304)
Shortfall in expected hh income (MK)				0.052 (0.041)	0.052 (0.041)	0.049 (0.038)
Social pressure						
Spouse minus own allocation to sooner (MK)					-0.007 (0.064)	0.016 (0.081)
Number of relatives in the village					0.546 (3.519)	1.315 (3.396)
Rate of return to waiting						
Implemented interest rate {.1,.25,.5,.75,1}		-143.004* (78.279)	-222.189** (80.267)	-218.912** (80.157)	-210.226** (80.151)	-210.377** (80.712)
Baseline characteristics						
Male		125.169** (49.851)	93.665* (49.368)	94.254* (51.664)	97.065* (51.548)	94.696* (53.092)
Age 35 or under		198.798** (71.742)	175.843** (70.408)	171.018** (70.608)	169.068** (71.099)	168.438** (71.659)
36-56 yrs old		117.670** (54.887)	100.638* (54.126)	103.106* (53.527)	106.326** (53.690)	105.352* (55.520)
Some primary school		-66.952 (70.212)	-86.414 (68.346)	-81.035 (71.568)	-79.640 (69.926)	-80.233 (70.310)
Primary school		-159.166* (85.954)	-167.474** (83.972)	-164.183* (90.815)	-172.907* (89.970)	-172.882* (90.006)
More than primary school		-215.094** (105.964)	-235.358** (103.978)	-223.634* (116.170)	-227.334** (114.814)	-227.345** (114.695)
Have adequate maize		35.744 (56.325)	24.945 (54.932)	28.773 (55.716)	25.964 (56.558)	25.092 (57.544)
Total HH Wealth		-0.122 (0.089)	-0.136 (0.085)	-0.138 (0.084)	-0.143* (0.083)	-0.143* (0.084)
Controls for:						
Spousal characteristics		-	-	-	-	Y
R-squared (adj.)		0.02	0.06	0.06	0.06	0.06
N		661	661	661	661	661
P-value for test that financial sophistication variables are jointly				0.82	0.82	0.82

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)

Preferences under commitment

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 near time frame	54.314 (65.255)
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)
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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 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 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 Rate:	0.002 (0.036)
Fraction of all tokens allocated to "sooner"	-0.032 (0.097)
Indicator: days to first disbursement (targeted) $\leq t$	-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)
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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)
<u>Assumed value of dep. var. is initial sooner allocation</u> <u>(from baseline) plus:</u>	600	400	200	0	-200	-400	-600
Preferences under commitment							
Fraction Present Biased, Non-Implemented Interest Rates	188.075** (89.646)	188.428** (87.857)	189.409** (87.025)	190.389** (87.127)	191.590** (87.780)	190.766** (89.037)	190.480** (90.643)
Fraction of all tokens allocated to "sooner"	-431.017** (212.525)	-431.037** (209.926)	-435.711** (208.729)	-440.386** (209.229)	-472.268** (210.069)	-504.767** (212.175)	-532.108** (215.454)
Indicator: days to first disbursement (targeted) <=6	190.458*** (47.697)	166.996*** (46.728)	143.898*** (46.253)	120.799*** (46.295)	102.239** (46.714)	85.558* (47.520)	69.613 (48.665)
Indicator: more elastic in the far time frame	118.921 (76.443)	121.014 (75.305)	122.667 (74.496)	124.320* (74.124)	124.198* (73.996)	122.206* (74.167)	119.686 (74.648)
Fraction Present Biased * I: more elastic in the far time fram	-201.488 (171.861)	-200.695 (169.208)	-199.093 (167.507)	-197.490 (166.843)	-199.087 (166.775)	-194.141 (167.321)	-188.180 (168.380)
Financial sophistication							
Fraction of Decisions Consistent with Law of Demand	-9.292 (167.521)	-18.216 (165.128)	-24.916 (163.775)	-31.617 (163.742)	-29.723 (164.856)	-23.491 (167.153)	-18.644 (170.413)
Words recalled	2.599 (18.575)	0.597 (18.211)	-2.044 (17.955)	-4.684 (17.852)	-7.389 (17.881)	-9.646 (18.031)	-11.951 (18.291)
Raven's Tests Correct	-26.198 (27.354)	-23.466 (26.906)	-20.751 (26.629)	-18.036 (26.588)	-13.969 (26.758)	-11.436 (27.067)	-9.015 (27.549)
Financial Literacy Questions Correct	18.682 (26.756)	20.090 (26.274)	22.382 (26.002)	24.674 (26.004)	25.011 (26.242)	24.225 (26.669)	22.637 (27.278)
Social pressure							
Spouse minus own allocation to sooner (MK)	0.048 (0.075)	0.048 (0.074)	0.047 (0.073)	0.046 (0.073)	0.042 (0.074)	0.037 (0.075)	0.037 (0.077)
Number of relatives in the village	0.904 (3.002)	0.999 (2.958)	1.128 (2.954)	1.258 (2.989)	1.542 (3.052)	1.810 (3.134)	2.027 (3.234)
Rate of return to waiting							
Implemented interest rate {.1,.25,.5,.75,1}	-237.479*** (76.074)	-228.823*** (75.169)	-221.385*** (74.722)	-213.948*** (74.892)	-205.420*** (75.447)	-197.399*** (76.350)	-189.790** (77.603)
Baseline characteristics							
Male	36.907 (57.667)	43.139 (56.386)	49.183 (55.688)	55.226 (55.611)	60.228 (55.853)	65.668 (56.536)	71.304 (57.626)
Age 35 or under	222.553** (104.766)	233.238** (103.316)	246.405** (102.463)	259.572** (102.314)	274.053*** (102.764)	284.066*** (103.725)	293.232*** (105.261)
36-56 yrs old	174.042*** (62.218)	166.743*** (60.697)	159.667*** (59.714)	152.592** (59.314)	149.573** (59.488)	148.445** (60.172)	147.938** (61.360)
Some primary school	-11.283 (69.318)	-18.190 (68.555)	-24.808 (68.185)	-31.427 (68.249)	-36.844 (68.553)	-40.713 (69.288)	-43.337 (70.453)
Primary school	-70.667 (88.457)	-83.080 (86.798)	-96.279 (85.851)	-109.478 (85.754)	-116.624 (86.205)	-120.851 (87.199)	-124.711 (88.825)
More than primary school	-106.320 (114.271)	-118.121 (113.347)	-129.817 (113.012)	-141.513 (113.342)	-148.054 (114.132)	-149.923 (115.228)	-149.138 (116.509)
Have adequate maize	0.967 (53.242)	0.939 (52.125)	0.897 (51.481)	0.856 (51.379)	0.536 (51.696)	0.386 (52.452)	0.358 (53.559)
Total HH Wealth	0.036 (0.044)	0.012 (0.040)	-0.013 (0.037)	-0.037 (0.035)	-0.064* (0.034)	-0.091*** (0.034)	-0.099*** (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)	(2)	P-value, F-test of equality of male and female coeffs.
	Females	Males	
Preferences under commitment			
Fraction Present Biased, Non-Implemented Interest Rates	87.743 (170.970)	199.450* (118.672)	0.592
Fraction of all tokens allocated to "sooner"	-571.908* (311.144)	-320.799 (293.251)	0.557
Indicator: days to first disbursement (targeted) <=6	100.810 (74.563)	158.428** (70.368)	0.574
Indicator: more elastic in the far time frame	15.146 (110.801)	211.019* (109.072)	0.208
Fraction Present Biased * I: more elastic in the far time frame	154.424 (272.822)	-433.262* (242.459)	0.108
Financial sophistication			
Fraction of Decisions Consistent with Law of Demand	-142.424 (259.858)	125.398 (250.989)	0.459
Words recalled	-14.072 (25.342)	29.148 (32.410)	0.294
Raven's Tests Correct	20.486 (40.829)	-75.382* (40.900)	0.098
Financial Literacy Questions Correct	13.736 (49.662)	36.079 (36.949)	0.718
Shocks			
Death in the family (indic.)	438.137 (327.464)	-247.190 (250.228)	0.097
Shortfall in expected hh income (MK)	0.051 (0.048)	0.038 (0.060)	0.858
Social pressure			
Spouse minus own allocation to sooner (MK)	0.067 (0.105)	0.031 (0.121)	0.822
Number of relatives in the village	1.602 (6.580)	2.912 (4.037)	0.865
Rate of return to waiting			
Implemented interest rate {1, .25, .5, .75, 1}	-247.920** (122.849)	-181.689 (113.634)	0.692
Baseline characteristics			
Age 35 or under	211.016 (152.870)	288.188* (166.146)	0.733
36-56 yrs old	53.810 (116.154)	233.604** (78.953)	0.201
Some primary school	-168.582* (98.918)	143.000 (115.128)	0.041
Primary school	-296.744* (172.443)	5.910 (124.417)	0.155
More than primary school	-508.001** (173.267)	146.502 (165.505)	0.007
Have adequate maize	38.929 (85.864)	-15.836 (82.556)	0.646
Total HH Wealth	-0.339* (0.175)	-0.040 (0.106)	0.144
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: Random vs. real choice

Ordinary least-squares regressions

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

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