Revising Commitments: Field Experimental Evidence on Determinants of Intertemporal Plan Revision*

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Abstract

Failures to carry out intertemporal plans can have important welfare consequences. We shed light on the determinants of intertemporal plan revision via a lab-in-the-field experiment in rural Malawi with large real stakes. We make two key contributions. First, we construct a new dependent variable: later revisions of a sequence of future money disbursements (that respondents had initially decided upon several weeks before). This allows us to *directly* examine intertemporal plan revision and its determinants, and makes possible new evidence for the existence of self-control problems: revisions of money allocations toward the present are positively associated with measures of present-bias from an earlier baseline survey, as well as the (explicitly randomized) closeness in time to the first possible date of money disbursement. Second, we investigate other potential determinants of revision, aside from self-control problems. Revisions of money allocations toward the present are positively associated with spousal preferences for such revision, but not with household shocks or the financial sophistication of respondents.

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1 Introduction

Intertemporal tradeoffs are central to many economic decisions. As a result, plans about choice over time are critical to well-being, perhaps especially so in developing countries. For example, investment plans for small business capital, agricultural inputs, education, and health are often key to economic well-being in low-income countries. Similarly, plans to smooth consumption over time, such as farm households spreading intermittent harvest income, are often critical to welfare in poor areas.

There is much evidence that individuals in developing countries do not allocate resources intertemporally in ways that are likely to be welfare improving. For example, in Malawi, a large share of smallholder farmers fails to use sufficient amounts of fertilizer, probably one of the highest-return agricultural investments available to them. Among Malawian farmers in our study, current levels of fertilizer use imply that 94.2% apply less than the recommended amount of nitrogen on maize (the country's main subsistence crop), with 21.1% applying no nitrogen at all.[1] Malawian farmers also fail to achieve smooth consumption over the course of the agricultural cycle, typically experiencing substantial seasonality in their caloric consumption. In the months during and after the annual harvest in May and June, consumption can be roughly a fifth higher than in the lean or "hungry" months preceding the harvest (World Bank 2006).

While there may be many possible reasons behind low investment or low consumption smoothing in developing countries, much attention has been focused on self-control problems that limit individuals' abilities to make or carry out welfare-improving intertemporal plans. Some part of the low fertilizer use in Malawi may in fact be due to failure to carry out plans. We asked farmers in November 2009 how much fertilizer they planned to use in the coming (December through May) planting season, at which point 85% of farmers planned to use some nonzero amount. Then, 8 months later, we asked them how much they actually used during that season. Among farmers who planned to use some fertilizer, 51% reported using less than the full amount they planned to use, with about 40% reporting that their actual use was at or below 50% of their planned use, and 14% reporting using no fertilizer at all.

In this paper, we ask: why don't people follow through on plans made in advance? Put differently: when people make future plans, what explains why they revise those plans later? We make two contributions. First, we test for the existence of self-control problems using a novel approach. Second, we investigate other potential determinants of intertemporal plan revision (aside

[1] Authors' calculations from 2009 socioeconomic survey of the study sample. For phosphorus pentoxide (P2O5), the other key nutrient for maize farming, 83.5% use less than recommended and 52.4% use none at all. Recommended levels for our central Malawi study area are 37.2 kg of nitrogen and 8.5 kg of P2O5 per acre (Benson 1999.)

from self-control problems).

The current literature gives substantial attention to present-biased time preferences as an explanation for failures to carry out intertemporal plans. The long-standing interest in intertemporal choice has, in recent years, been further fueled by evidence of non-constant time discounting and a better understanding of its theoretical consequences. Several studies, drawing mostly on experimental data, can be interpreted to show that time discount rates decline as tradeoffs are pushed into the temporal distance.[2] Specifically, many of these studies document dynamic preference reversals: under commitment, 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 non-constant time discounting, these preference reversals have important implications. When utility is time-separable, non-constant 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 (for individuals who are not fully naïve) thus emerge. If plans set at some earlier point will not be followed, then sophisticated decision-makers will want to limit their own ability to revise decisions about the future. The important consequences of this time-inconsistency and its associated self-control problems have generated a great deal of interest. They have now been studied, with both theoretical and empirical methods, in many different contexts.[3] Evidence for the existence of present-biased time preferences has also been provided by field experimental studies such as Ashraf, Karlan, and Yin (2006), who find that women in the Philippines whose survey responses indicate present-bias have higher demand for a commitment savings device.[4]

This paper aims to fill key gaps in the current literature. First and foremost, there is currently no direct evidence that present-biased preferences are associated with failure to carry out future plans: there has been no previous examination of "revision of previous decisions" as the key dependent variable of interest. Rather, previous work has examined demand for commitment devices. Demand for commitment is useful to examine and potentially revealing of the existence of present-biased preferences, but for two reasons such analyses are likely to understate self-control problems. First, demand for commitment requires some degree of sophistication on the part of respondents: individuals who are fully naïve about their self-control problems should not exhibit

[2] See Ainslie (1992), Thaler (1991) and several papers in Loewenstein and Elster (1992), for reviews of this evidence.

[3] Early contributions include Phelps and Pollak (1968), Laibson (1997) and O'Donoghue and Rabin (1999). See DellaVigna (2009) for a recent review of empirical applications.

[4] Ashraf, Karlan, and Yin (2006) do not find this result holds for men. Similarly, Brune, Giné, Goldberg, and Yang (2011) do not find a relationship between hyperbolic preference measures and demand for a commitment savings device among male tobacco farmers in Malawi.

demand for commitment. Second, the design of commitment devices may be imperfect, further dampening observed demand (Beshears et al, 2011).

Relatedly, it is important to correlate survey-based time preference measures with real-world behavior because individuals who exhibit preference reversals (for example, whose intertemporal allocations in experiments exhibit present-bias) may not necessarily exhibit time-inconsistent behavior (such as failure to carry out plans made previously). Some preference reversals may reflect predictable changes in the marginal utility of consumption. For example, even in a very standard model with exponential time discounting, if income rises (falls) with time and consumption smoothing is incomplete, then a consumer may appear more (less) patient regarding tradeoffs later when the marginal utility of consumption is lower (higher).[5] Such a consumer would not, however, exhibit time-inconsistent behavior. In addition, some preference reversals may reflect inattention, confusion about tradeoffs, or responses to perceived experimenter demands.[6] It is unclear whether consumers who make such plans under commitment would also tend to revise them later, one way or the other, if the commitment is relaxed. Furthermore, even if preferences under commitment were well-described by changing time discount rates, the simple act of making a plan may importantly limit what would otherwise be important problems of self-control.[7] Concerns about self-image and the importance of following through might trump the effects of changing time discount rates. In this case, again, dynamic preference reversals under commitment would not predict time-inconsistent behavior.

Another central gap in the current literature is that little attention has been paid to other potential reasons behind revision of intertemporal plans, so it is unknown how the importance of present-biased time preferences compares with that of other determinants of revision. In particular, we believe three additional determinants of revision are important to examine. First, there is social pressure: the influence of spouses, relatives, and others in one's social network. Particularly in rural communities in developing countries, individuals are often obliged to share their income with

[5] This observation is a special case of the more general point made by Andersen et al. (2008), Andreoni and Sprenger (2010) and Noor (2011), that proper inference about time discounting requires information about the curvature of the utility function.

[6] The correlation between test scores, cognitive load and short-term patience in Benjamin et al. (2006) lends some support to this conjecture.

[7] Academic psychology points to the important effects of making plans or setting goals on self-control and self-efficacy Bandura (1997). Evidence of the importance of planning is also seen in Ameriks et al. (2003). This idea is also broadly consistent with economic models of costly self-control such as Gul and Pesendorfer (2001), Ozdendoren et al. (2009), and Fudenberg and Levine (2010), in which consumers may both seek commitment and, yet, not always exhibit time-inconsistency.

relatives and friends,[8] and such pressure may be an important factor behind revisions of previous plans. Second, shocks such as deaths, illness, or unexpected changes in income could change the optimal time path of consumption, making revisions desirable. Third, individuals could make mistakes in their original plans, and may revise them later upon realizing their error.

A fuller understanding of the relative importance of these four potential determinants of revision – present-biased preferences, social pressure, shocks, and mistakes – has important policy implications. First of all, it is crucial for welfare analysis of commitment devices. If social pressure affects revision, then this may reveal an additional benefit of commitment devices: preventing future plans from being subverted or undone by others who may have different preferences or who may not have one's own interests in mind. If shocks are an important determinant of revision, then this may point to a potential downside of commitment devices: they reduce individuals' ability to deal with the unexpected. If mistakes lead to revisions, then this may also mean that commitment devices can sometimes be harmful, as mistakes cannot later be corrected.

Second, a fuller understanding of the determinants of revision can inform the design of commitment devices. Most commitment devices that exist seek to address present-bias and the corresponding self-control problems that arise, aiming to "tie the hands" of individuals and prevent them from revising their own plans made previously. But if social pressure turns out to be an important determinant of revisions, then alternative commitment devices could be designed to shield resources from one's social network, while maintaining access for oneself.[9]

Finally, understanding whether other determinants of revision are important can suggest other policies and interventions. If shocks are important, this can provide an additional rationale for social protection programs or promotion of the private insurance sector. A finding that mistakes are important can provide a rationale for financial literacy or education efforts.

To address these gaps in the existing literature, we implemented a lab-in-the-field experiment with two key innovations: 1) it takes "revision of a previous decision" as the key dependent variable of interest, and 2) it examines the roles and relative importance of the four potential determinants of revision outlined above. Our sample consists of several hundred wife-husband pairs in rural Malawi, who are interviewed separately and who make independent decisions. We measure the extent of present-biasrsprfhe(en)-1(cs-44stb(u)-1(ding)4stbA(nd)-1(r)1(e)-1(o)1(n)-1it)-275(an)-1(d)4stbS(p)-1(r)1

socioeconomic survey.[10] As part of the CTB implementation, individual subjects specify a desired time pattern of money disbursement between two points, 61 and 91 days in the future.[11] A subset of experimental subjects were revisited some time prior to t=61 and given the opportunity to revise the allocation between t=61 and t=91. The extent of this revision (specifically, the shift in monetary allocations toward the "sooner" date, t=61) is our dependent variable. We examine correlates of this revision decision corresponding to each of the four potential determinants of revision outlined above. Surveys at the baseline and revision stages measured household wealth, income, and expenditures as well as the participants' expectations for each of these variables.

In addition, we implemented a novel test of a basic prediction of hyperbolic discounting models in our experimental context: revisions toward sooner should be larger the shorter the time lag between the revision decision and the first disbursement date (t=61). We randomized the number of days prior to t=61 when each experimental subject had to make the revision decision.[12] To our knowledge, such a test has not been conducted before.

We find, first of all, that initial allocations using the CTB method are sensible, indicating that experimental subjects by and large understood the choices they were making. There is strong adherence to the law of demand, meaning individuals allocated more to "later" periods when faced with higher rates of return to waiting. We also find that intertemporal preference reversals are frequent, but only slightly more likely to be present-biased (as opposed to "future-biased"). This finding contrasts with other studies using the multiple price list (MPL) method, but is consistent with Andreoni and Sprenger's (2011) findings that UC San Diego college students exhibit no present-bias on average.

Turning to revision behavior, we find that revisions are common, usually substantial in size, and shift money both sooner and later. We find support for present-biased preferences as an important driver of revisions: revisions toward sooner are higher when: 1) initial allocations are present-biased, and 2) the time lag to disbursement is smaller (in particular, when the revision decision is made 6 or fewer days prior to day t=61). In addition, we find evidence consistent with social pressure affecting revision decisions: respondents' revisions toward sooner are higher when one's spouse's sooner allocations are larger than one's own. By contrast, shocks and financial sophistication (a proxy for mistakes in initial allocations) do not predict revisions.

The determinants of revision that do matter have large effects. A useful benchmark is the

[10] The use of the CTB method is another distinguishing feature of our research. We describe the advantages of this method in section 2.3.1 below.

[11] These dates fell within Malawi's annual "hungry" season, just prior to the annual harvest, when food stocks are at their lowest and consumption smoothing is a paramount concern.

[12] The revisit date was drawn randomly from a uniform distribution of 2-16 days prior to day t=61.

impact of a 50-percentage point reduction in the rate of return to waiting 30 days. A one-standard-deviation increase in the measure of present-bias has an impact 0.4 times as large, making one's revision decision within 6-days of day t=61 has an impact 1.3 times as large, and a one-standard-deviation increase in the measure of spousal pressure has an impact 1.2 times as large as a 50-percentage-point reduction in the rate of return to waiting.

The rest of the paper is organized as follows. Section 2 presents details of the experimental design, the sample of participants and the setting of the experiment. Section 3 presents the results of stage one of the experiment and choices under commitment. Section 4 discusses the results of stage two and choices upon revisiting. In section 5 we discuss some prior studies of intertemporal choice and explain our contribution to the literature. Section 6 offers some conclusions and suggests avenues for work.

2 The Experiment

In summary, our experiment proceeded as follows. The sample was comprised of approximately 1,100 households (2,200 individuals) in rural Malawi and proceeded in two stages. The timeline of the experiment is presented in Figure 3. In stage one, we adapted the methods of Andreoni and Sprenger (2010) to elicit time preferences under commitment. The head of household and his spouse each 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". Participants were paid a return on the part of the endowment that was saved for later. Each participant made 10 allocation choices; one choice facing each of five different rates of return and regarding two different time frames, one in the near future (the "near" period) and one in the more distant future (the "far" period). Choices regarding the near period allocated money between tomorrow and 30 days from tomorrow. Choices regarding the far period allocated money between 60 days and 90 days from tomorrow. All choices were incentivized; participants knew that either the household head or his spouse would be randomly selected to have one of his or her, randomly selected, choices implemented. The stakes were high. On average, the total allocation amounted to approximately a month's wages for a worker in rural Malawi.

Stage two of the experiment applied only to those households whose implemented choice concerned the more distant future. At a randomly selected day in the two weeks prior to the arrival of the first disbursement of their money in the far period, the household was *unexpectedly* revisited. At that visit, the participant whose choice was implemented was reminded of the decision he or she made at stage one, 45-60 days earlier under commitment. That previous choice was made clear and salient. Then, facing the same rate of return, the participant was allowed to revise the original

allocation decision. That decision (revised or unrevised) was then implemented with certainty. Surveys at both stages one and two measured household wealth, income, and expenditures as well as the participants' expectations for each of these variables.

We now turn to a description of the Malawian setting and its advantages, to be followed by a more detailed description of the experiment.

2.1 The Setting

As a setting for experimental study of intertemporal choice, rural Malawi has a number of advantages. Most important is that financial markets in the area are thin and participants thus lack effective methods for smoothing the relatively large amounts of new consumption that the experiment makes possible.

At the time of year when the experiment was implemented (the rainy season), borrowing substantial amounts of cash in rural Malawi is not merely expensive, it often appears effectively impossible. Similarly, short-term saving is difficult due to limited banking institutions, and familial or social demands for what appears like excess cash.[13] The lack of borrowing and saving opportunities is important because it sharply reduces the smoothing opportunities that should confound efforts to elicit time preferences from standard experiments in developed economies. In economies with thick financial markets and low transaction costs, answers to the questions asked in typical experiments should, in theory, bracket only the market rates of return that participants face, and reveal very little about their "true preferences" (Fuchs, 1982).[14] To the extent that rural Malawi in the rainy season approximates autarchy, smoothing is made much more difficult. This is especially true for the participants in our experiment because the stakes are so large and involve cash rather than grain or other consumption goods. Subjects will find it very difficult to smooth such a large amount of experimental income by consuming from their own stores. In this way, studying intertemporal allocations in rural Malawi provides data that, in principle, is much closer to the theoretical concepts of interest: preferences over the time-allocation of consumption.

[13] Longer-run saving instruments with positive rates of return are more common. They typically take the form of agricultural inputs.

[14] To illustrate, suppose that outside of the lab a participant can borrow or save at market rate r. And suppose the participant faces no financial transaction costs. A typical experiment asks the participant to choose between $\$x$ sooner or $\$(1+r_e)\,x$ later, where r_e denotes 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 of consumption between sooner and later is strictly larger under option B than under option A.Thus, for any monotonic time preferences, option B is preferred. Analogously, if $r_e < r$, then A is preferred for any monotonic time preferences.

Rural Malawi also has disadvantages as a location for experiments on time preference. The region's low population density and relatively poor infrastructure make some experimental logistics difficult. One important consequence is that payments to participants can arrive no sooner than one day after they make their choices. Thus we cannot study preferences regarding consumption in the present; and we cannot observe the consequences of changes in time discounting that occur as just as intertemporal tradeoffs are made strictly later than the present. To the extent that changes in time discounting are largest then, we would expect any relationships between choice under commitment and revision behavior to be attenuated.

Another potential disadvantage of the setting is that participants have low levels of formal education and may therefore find the somewhat abstract experiment especially difficult to grasp, or view it much differently than we would expect. Participants make a living from seasonal crops, so they are uncommonly familiar with the problem of smoothing consumption over time. Nevertheless, because we asked them to make choices in an unfamiliar context, it is natural to worry about participants' abilites to quickly understand the experiment as an economic decision. For this reason, our analysis takes special care to evaluate the consistency of participants' choices with a basic feature of rational economic decision-making – the law of demand.[15] We interpret the degree of consistency with the law of demand as a measure of participants' understanding of the trade-offs involved in their decisions.[16]

2.2 Sample

Participants in the experiment were drawn in January and February 2010 from a population of rural households in central Malawi who grow tobacco as their main cash crop. In the 2008-2009 growing season, these farmers were under contract with (the subsidiaries of) two large tobacco companies. 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. Experimental payments were delivered in the form of vouchers (described below) redeemable for cash from an office set up for this purpose in Mponela. To allow relatively easy access to participants and to faciliate 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

[15] Cf. Choi et al., (2007, 2011).

[16] Alternatively, one could interpret consistency with the law of demand as measure of the appropriateness of interpreting these choices with the tools of economics.

member lives 25 kilometers or less from the disbursement office in Mponela.[17] Finally, to facilitate study of interactions within the household, we further restricted our sample to farmers who were part of a married couple.[18]

These sample restrictions left us with 1,268 targeted farmer households. A total of 1,144 households (90.2%) and 2,285 respondents were successfully interviewed at baseline. A subset of 664 respondents (randomly selected from the full set of baseline respondents in a manner described in section 2.4 below) make up the stage 2 sample to be revisited.

Table 1 provides some 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 2 relatives other than his or her spouse. There is, however, considerable variation in these demographic variables. Despite being drawn from the same population of tobacco farmers in central Malawi, participants are heterogenous.

These summary statistics reveal important features of participants' wealth and income. On a global scale, the households in the sample are poor. In the central Malawi region we study, however, tobacco farmers have similar poverty and income levels to those of non-tobacco-producing households.[19] Focusing on the revisit sample (in Panel B), at the time of the baseline survey, the median participant's household has zero balances in a formal bank, and the 90th percentile of the bank balance distribution is just 7,000 Malawi kwacha (henceforth MK), or approximately US$47.[20] The survey also inventoried the household's non-cash assets including livestock and agricultural and household durables; and participants were asked to estimate how much they could earn by selling those assets. When we include the self-reported value of these assets, we find that the median household held just 4,405 MK of wealth and the 90th percentile held 253,800 MK.

The baseline interview was conducted in the middle of the rainy season; a time of planting and cultivating, not harvesting. At this time of year, cash income is very low, and will be low for the

[17] Scheduling for the stage one visit of this study was done by stratifying scheduling across agricultural zones. Within a zone, the order in which clubs were visited was randomly assigned. Scheduling was done 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.

[18] Each household had also been involved in a previous commitment savings experiment, described in Brune, Gine, Goldberg, and Yang (2011). Our regression analysis controls for effects of the randomly assigned experimental treatments in the previous experiment. None of the experimental treatments in Brune et al (2011) have large or statistically significant effects on the dependent variable we analyze in this paper (revision behavior).

[19] Based on authors' 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.

[20] At the time of the study, the relevant exchange rate was roughly 150 MK per US dollar.

next few months. Neither the cash crop nor the primary staple (maize) is ready to be harvested until mid April or early May. The summary statistics bear this out. The median household expects virtually no cash income between the interview date and the April 2010.

2.3 Stage One: Choices Under Commitment

Figure 3 presents the timeline of the experiment. To gather data on intertemporal choices under commitment, we adapted the methods of Andreoni and Sprenger (2010) for the Malawian field environment. Upon initiating the interview, the household head and his spouse were physically separated. Then, after a few basic questions regarding demographics, each made 5 independent choices regarding the allocation of 2000MK between tomorrow ("sooner") and 30 days from tomorrow ("later").[21]

More precisely, each participant was presented with a small bowl containing 20 beans (tokens) and two empty dishes, dish A and dish B. A token allocated to dish A corresponded to 100 MK tomorrow. A token allocated to dish B corresponded to $100MK * (1 + r)$ 30 days from tomorrow, where r is the rate of return for waiting 30 days. The rate of return took on five different values: 0.10, 0.25, 0.50, 0,75, and 1.00. The rates of return rose, in order, with each of the five allocation choices; and participants knew that. For each rate of return, once the participant set out an allocation of tokens to the dishes, the tokens were translated into Malawi kwacha and the total was written above each dish on a whiteboard. Having seen the allocation in kwacha, the participant had the opportunity to adjust the allocation. This process was repeated until the participant indicated that she was ready to move on to the next allocation choice.

After completing the first five choices, the participant answered a series of other questions from the baseline survey. (See Appendix A for details.) Then, using the same elicitation method with cup, beans, and dishes, the participant again made 5 independent choices regarding 2000MK, while facing different rates of return for waiting. This time, each of the 5 choices concerned the allocation of money between 60 and 90 days from tomorrow (the "far" period). Figure 1 presents a schematic of the method used to elicit intertemporal choices under commitment.

The interruption between the five choices in the near period and the five choices in the far period was intentional. We sought to avoid participants choosing the same allocations in both time 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, in fact, randomly assigned between households within tobacco clubs. With 50 percent probability, a participant was first asked to make

[21] The income was presented as compensation for participation in the survey and the prior research project (Brune et al. 2011).

decisions about the allocation of money in the neare period, between tomorrow and 30 days from tomorrow. Otherwise, a participant was first asked to make decisions about allocations in the far period, between 60 and 90 days from tomorrow. Our regression analysis controls for any effects of these alternative orders.

Before making their choices, each participant knew that one member of the couple would be randomly chosen to have one of his or her choices implemented (also randomly selected). The randomization was performed on site, by rolling dice. Implementation took the form of a voucher, redeemable at the disbursement office in Mponela. The voucher indicated the allocation (some amount at time t and another amount at time $t + 30$) and was issued to the member of the couple who was randomly chosen. Identity of the recipient was established with a name and a fingerprint placed on the voucher.[22]

2.3.1 Advantages of the Elicitation Method

Andreoni and Sprenger's (2010) elicitation method has important advantages. The first feature is that participants choose a single allocation from convex budget set. A traditional elicitation method would ask participants a sequence of unfolding binary choice questions (do you prefer 2000MK tomorrow or 2500MK 30 days from tomorrow?), stopping the sequence when the participant flips her choice from sooner to later (or vice versa). Relative to that method, Andreoni and Sprenger (2010) permits m

consumption.

2.4 Stage Two: Revisiting the Decision

In stage one of the experiment, one of each household's 20 decisions (10 of the husband's and 10 of the wife's decisions) was randomly selected to be implemented. If the selected decision concerned an allocation in the near period, between tomorrow and 30 days from tomorrow, then the experimental intervention was finished for that household. The chosen individual in the household redeemed its allocation and was not interviewed again. Stage two of the experiment applied only to those households whose randomly selected decision concerned an allocation in the far period, between 60 and 90 days from tomorrow.

In stage two, this group of households was unexpectedly revisited. The targeted date for revisiting was randomly selected from the interval between 16 and 2 days prior to the first possible disbursement of funds in the far period. Thus, these households were targeted to be revisited between 45 and 59 days from their baseline interview. Revisits occured even if the household chose a "corner solution" involving no disbursement of funds at 61 days from their interview.[23] Revisit dates occurred in March and April 2010.

At the revisit, the household head and his spouse were physically separated and the survey of wealth, income and expenditure was performed again. After the interview portion of the revisit, the participant whose choice from the baseline interview was set to be implemented was again presented with a cup containing 20 tokens. This time, however, 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 allocated to reflect 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 1 to 16 days from the revisit and 30 days from then. This information was verified with the voucher that was issued at baseline. 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.

[23] In all that follows, we focus on the randomly-assigned targeted lag (in days) to first disbursement, since this cannot be endogenous 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 smaller (closer to the disbursement date) than the targeted lag, because some farmers could not immediately be located and had to be found thereafter. The actual lag is, as should be expected, 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 2 days after their target date. The maximum difference in the sample between target lag and actual lag is 6 days. Actual lags spanned the range of 16 to 1 days prior to first disbursement.

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

The revising procedure is intended to measure the presence and magnitude of revisions of individuals' previous plans. We therefore made the original allocation decision salient and unambiguous. The procedure is also designed to balance the consequences of implicit experimenter demands or connotations of a "right" answer. The participant must actively choose an allocation by placing tokens on the mats, and the status quo is thus discouraged. 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, but implicit encouragement to do so. Especially given the rural setting, and the difficulty of double blind protocols, we cannot hope to eliminate the consequences of implicit experimenter demands. Instead we designed the experiment to limit the biases they might generate.

The conceptual key to the revising procedure is that participants recall, with perfect accuracy, the plan they chose at baseline. We then seek to quantify the extent to which they deviate from that plan, and learn more about why they deviate. In this way, the experiment is importantly different from an experiment designed to evaluate the stability of preferences regarding a delay of fixed length Δ. If that were the goal, we would not have reminded participants of their original choice and we would have repeated the elicitation method for a fixed delay. Instead, we make the original choice unambiguous and randomly choose the delay from a two week interval.

3 Stage One: Choices Under Commitment

3.1 Theoretical Framework

In this section we describe the results from stage one of the experiment, the choices made under commitment. To guide interpretation and fix ideas, consider first a canonical, deterministic problem of choice over time. In that problem the decision-maker solves

$$\begin{aligned} & \max_{\mathbf{c}=(c_1,c_2,..,c_T)\in \mathbb{R}_+^T} U(\mathbf{c}) \\ s.t. \quad & k_{t+1} = (k_t - c_t)(1+r) && (1) \\ & k_0 = \overline{k},\ k_T \geq 0. && (2) \end{aligned}$$

The consumer thus chooses a bundle of consumption, $\mathbf{c}$, the elements of which are indexed by time, to maximize a utility function U. The choice of $\mathbf{c}$ is restricted to a feasible set defined by the intertemporal budget constraints (1) and the boundary conditions (2). r is the interest rate, and k_t is the stock of savings at time t. The usual assumptions are: (1) Monotonicity: U is increasing in each element of $\mathbf{c}$; other things equal more is preferred to less. (2) Diminishing marginal utility: U is concave in $\mathbf{c}$ and so smoother allocations tend to be preferred. (3) Impatience: other things equal, consumption is preferred sooner rather than later. Given a feasible set, choices about the allocation of consumption over time, i.e., time preferences, are driven by all three of these usual assumptions.

We will interpret participants' choices under commitment in stage one, as solving a special version the canonical model, where U is time separable. Abstracting from the discrete nature of the choice set, we will interpret stage 1 decisions as solving, for each rate of return r:

$$\begin{aligned} &\max_{(c_t, c_{t+30}) \in \mathbb{R}^2_+} u_t(c_t) + \delta_t u_{t+30}(c_{t+30}) \qquad \text{(P)} \\ s.t. \quad &c_t + \frac{c_{t+30}}{1+r} = 2000MK \end{aligned}$$

Note that flow utilities and the time discount factors need not be stationary. Non stationary flow utilities allow for the possibility of predictable changes in the marginal utility of consumption. Non-stationary time discount factors allow for the possibility that, as has often been observed, time discount rates depend on the intertemporal distance to the trade-off. While this framework is deterministic, our analysis makes use of survey evidence regarding the real-life uncertainty that participants face.

Note, too, that the specification in problem P is, absent additional assumptions, generically unidentified from choices $(c_t, c_{t+30}) \in \mathbb{R}^2_+$ facing an exogeneous rate of return r. It is trivial to show that for any collection $(u_t, u_{t+30}, \delta_t)$ that reconcile the choice data, there is another collection $\left(u'_t, u'_{t+30}, \delta'_t\right)$ that can also reconcile the data. Restrictions, such as stationarity of the flow utility functions, or restricted form of non-stationarity (such as $u_t(c_t) = \frac{c^{1-\gamma_t}}{1-\gamma_t}$), are required in order to identify the preference parameters.

3.2 Adherence to the Law of Demand (Monotonicity)

While the preference parameters of (P) are not generically identified, time separability and montonicity of the flow utilities makes a strong prediction. If participants solve problem (P) then the allocation to the later period, measured in kwacha, should increase with the rate of return to waiting r. To see why this is true, it is useful to think of $\left(\frac{1}{1+r}\right)$ as the price of consumption later in terms of consumption sooner. When r goes up, the price of later consumption goes down.

The result is an income effect creating incentives to increase consumption in both periods, and a substitution effect that is positive for consumption in the later period. Thus both income and substitution effects lead to increased consumption (kwacha) in the later period.[24]

As a first step in the analysis of stage 1 decisions, we evaluate the extent to which participants' choices are consistent with this basic prediction of rational choice with time-separable, monotonic utilities. We view this as a logical pre-condition for interpreting the choices under commitment as revealing preferences in the context of a canonical model. To the extent that choices are inconsistent with the law of demand, it suggests either that participants did not understand the trade-offs involved in their decisions very well, or that the canonical model is very poorly suited for interpreting and making predictions about their behavior.

Each participant made ten intertemporal allocation decisions. To evaluate adherence with the law of demand, we partition these decisions into pairs, where each element of the pair is a choice regarding an allocation of kwacha over the same two dates. The first element of the pair is the allocation chosen when facing rate of return r. The other element is the allocation chosen when facing rate of return r', the next lowest rate of return. For each participant there are eight such pairs, four for each of the two time frames.[25] A total of 2,285 participants completed stage 1 of the experiment. The data thus contain 18,280 pairs of decisions where r increases by one increment; of these, 14,749 (81%) were such that the allocation to the later period increased with r. Thus, approximately 81% of pairs were consistent with this basic prediction of rational choice with monotonic, time-separable utility. In 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. We interpret these results to indicate that, on average, participants understood the trade-offs they were facing and that the time-separable version of the canonical model is a reasonable starting point for describing average behavior.

The average rates of adherence with the law of demand mask some important heterogeneity, however. Table 2 presents the distribution of participants by the number of times they increased their later allocation with a single increase in the rate of return r. Recall, there are eight such pairs for each participant and thus the number of times a participant can be consistent by this measure ranges from zero to eight. Table 2 shows that, measured in this way, 31.1% of participants are always consistent and 75.4% are consistent at least three quarters of the time. At the other end of the spectrum, 10.6% of the sample violated this form of consistency at least half the time.

[24] The allocation to the earlier period can go up or down depending on whether the income or substitution effect dominates.

[25] Note that a subject who did not change his or her allocation of tokens within a pair would still appear consistent with this prediciton; the allocation to the later period would increase with r.

The violations of this second group are also larger; their violations would require the reallocation of somewhat more tokens, on average, to be made consistent. We will return to consider the consequences of this heterogeneity below.

3.3 Intertemporal Trade-offs Under Commitment

With greater confidence that most participants understood the trade-offs involved in their choices, and that the canonical model is a decent description of their behavior, we further examine decisions under commitment. Table 3 presents some summary statistics of these choices, displayed separately by time frame – tomorrow vs. 30 days from tomorrow (near) and 60 and 90 days from tomorrow (far). More precisely, Table 3 describes the distribution of allocations to the later period, in kwacha.

Several features of this distribution are worth noting. First, participants typically reveal a willingness to balance allocations between the two periods that is consistent with the canonical model described in Section 3.1. For example, when facing a rate of return to waiting of 50%, the median choice allocates 1,950MK to later and, thus, 700MK to sooner. A minority of allocations (12% to 23%) are "corner solutions." This willingness to locate at an interior allocation is consistent with participants not having, or not realizing they have, meaningful smoothing opportunities. The tendency toward interior solutions also points, in the absence of very high rates of time discounting, to the importance of diminishing marginal utilities of income.

A second important feature of this distribution is the heterogeneity in preferences that it reveals. In the nearer time frame, the tenth percentile allocates just 750MK (6 of 20 tokens) to the later period when the rate of return is 25%. The 90th percentile allocates all of its endowment to the later period. This heterogeneity is to some extent predictable with observable characteristics of the participants. Table 4 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 r and observable characteristics of the participants. These results provide some evidence that, 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 some 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 significant relationship with patience in this domain.

The estimates in Table 4 have the advantage of being easily interpreted in terms of a simple economic model of intertemporal choice. If we adopt the canonical model in problem (P) and assume stationary, isoelastic utilities ($u(c) = \frac{c^{1-\rho}}{1-\rho}$), then the coefficient on r is an estimate of

$\frac{1}{\rho}$. The disadvantage of this specification is that it excludes corner allocations, where the log of consumption (0) 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.

3.4 Dynamic Consistency

A third important feature of the distribution choices displayed in Table 3 is its apparent stationarity. Comparing the top and bottom halves of Table 3 shows that the distribution of allocations to later is not dramatically altered by the change in time frame. For example, the mean allocation to later when facing a 25% rate of return is 1,534MK when the trade-off is between tomorrow and 30 days from tomorrow. The analogous number is 1,565MK when the trade-off is pushed 60 days out into the future. This average stationarity is, however, somewhat misleading. It both overstates the stability of individual choices across time frames and masks heterogeneity in individual tendencies to shift allocations forward or back, depending on the time frame.

Each participant makes five pairs of decisions where each element of a pair differs only in the time frame. Of the total of 12,660 such pairs, just 4,362 (34%) are identical and just 6,458 (51%) differ by a token or less. Thus, in nearly half of all such pairs their elements are substantially different from each other. There is a relatively modest tendency for this dynamic inconsistency to be "present"-biased. Of the 6,202 pairs that differ by strictly more than a token, 3,264 (53%) allocate more to the earlier period when the trade-off is between 1 and 30 days. The remaining 47% of these preference reversals allocate more to the later period when the trade-off is between 1 and 30 days from tomorrow. Consistent with the "magnitude effect," preference reversals are more common at lower rates of return for waiting.[26] When the rate of return for waiting is 10%, just 47% of pairs differ by a token or less.

In this way, the results of stage one of the experiment indicate that intertemporal preference reversals are common, but that "present"-biased preference reversals are only somewhat more common than those in which participants appear less patient as intertemporal trade-offs get pushed out into the temporal distance. There is evidence, however, that these dynamic inconsistencies are not merely noise. Among those participants who exhibit preference reversals, a substantial fraction (18%) is present-biased in at least 4 of 5 decisions. In addition, the tendency to be consistent or present-biased is somewhat predictable with observable characteristics of the participants.

Table 5 presents the results of regressions that relate a participant's tendency to be consistent

[26] The magnitude effect refers to the finding that individuals appear more present-oriented when the stakes are smaller. See, e.g., Thaler, (1981) Loewenstein (1987) and Benzion et al. (1989).

or "present"-biased to certain observable characteristics. In each column the dependent variable is either the fraction of pairs of decisions in which the participant was dynamically consistent or the fraction the participant was present-biased. The first column of Table 5 indicates that males and those with greater maize stores tend to be more dynamically consistent. Column 3 of Table 5 indicates 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 an important relationship. There is a strong association between basic consistency as described in section 3.2 and dynamic consistency as measured here. Those with a higher share of pairs of choices that adhere to the law of demand (as in Table 2) are much more likely to make dynamically consistent choices.[27] This link suggests that the tendency to exhibit preference reversals may be attributable, in part, to a poor understanding of the choice environment and the trade-offs involved. We pursue this hypothesis further as we examine the revising behavior in stage 2 of the experiment.

4 Stage Two: Undoing Commitment

Having described some basic features of preferences under commitment, we now turn to examine the relationship between preferences under commitment, and other participant characteristics, and revision behavior in stage two of the experiment. Our goal is to evaluate, quantitatively, the importance of time preferences relative to other motives for revision.

4.1 Qualitative Features of Revision Behavior

Because this is, to our knowledge, a first attempt to study revision behavior in an experimental study of time preference, we begin with 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) the later time frame, so we aimed to revisit 726 respondents and present them with a revision opportunity. Of these we were successful in collecting revision choice data from 664 (91.5%). Figure 5 presents a histogram of changes in the participants' allocations to sooner (t=61) upon revisiting.

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

The first important feature of this distribution is the frequency of revision behavior. Despite an experimental design that made the original allocation choice clear and salient, 65.3% of participants (458) made some adjustment to their allocation decision. It could be that implicit experimenter demands caused some participants to feel as though some change was expected of them. However, among those who revised their original decision, a large majority (86.2%) made a reallocation involving a shift of at least two tokens, and 63.1% made a reallocation involving a shift of at least 4 tokens.

A second important feature of this distribution is that revision decisions shift the allocation of income forward and backward in time with nearly equal frequency. Of the 458 participants who made some adjustment to their allocation decision, 52.4% shifted income backward in time and 47.6% shifted income forward in time. As the histogram also indicates, however, the adjustments that moved allocations forward in time tended to be more modest in size. Of these, approximately 55.5% involve the shifting of at least 4 tokens, and just 16.1% involve shifting 10 tokens or more. The comparable numbers for adjustments that moved allocations backward in time are 70.0% and 26.7%.

4.2 Who Revises?

Having described some basic features of revision behavior, we move on to consider the correlates of these choices. Of primary interest is the extent to which revision behavior is related to preferences under commitment (e.g., the present bias ratio), financial sophistication, shocks, and social pressure.

Table 6 presents results of ordinary-least squares regressions relating revision behavior to variables representing all these potential determinants as well as baseline characteristics. In each column, the dependent variable is the change in sooner allocations upon revisiting (in Malawi kwacha).

In the first column of the table, right-hand side variables are restricted to baseline characteristics and the implemented interest rate. The coefficient on the implemented interest rate is negative and statistically significant at the 5% level. Respondents clearly respond to the rate of return to waiting, choosing to revise less towards sooner at higher rates of return. Coefficients on baseline characteristics indicate that males and younger individuals (those in age categories 57 or below) revise more towards sooner, while those in the highest education categories (primary and more than primary) revise less towards sooner. Maize stores and wealth, on the other hand, have little power to predict revisions.

In column 2, we test for the existence of hyperbolic preferences, including on the right hand side of the regression the fraction of allocations that are present-biased among the choices *other than* the

implemented choice. Specifically, this measure takes the four pairs of decisions where each element of a pair differs only in the time frame (excluding the pair associated with the implemented choice) and gives the fraction of those pairs in which the participant exhibited "present-biased" preference reversals. To allow for respondent error, we consider it a reversal only if the allocations differ by at least two tokens.[28] We exclude the implemented choice from this calculation because simulation analyses (discussed in the Appendix) indicate that any errors introduced into respondents' allocations (and, in the extreme, random choice) introduces a mechanical positive relationship between present bias *for the implemented choice* and revisions toward sooner.[29] Crucially, however, this spurious relationship does not exist when constructing the measure of present bias from the non-implemented choices.

In addition to the present-bias measure, in column 2 we also include on the right-hand-side of the regression the indicator for the targeted lag to first disbursement being less than or equal to 6 days.[30] This variable is included to test the basic prediction that – if individuals have hyperbolic preferences – they will shift more towards the present the closer they are to the time of consumption.

The results of column 2 provide evidence consistent with non-constant time discounting. The coefficient on the indicator for 6 or fewer days to first disbursement is positive and statistically significant at the 5% level.[31] In addition, the coefficient on fraction present biased is positive, but is not statistically significantly different from zero in this specification.

In column 3 we add to the regression variable representing financial sophistication, which is likely to help capture for the extent to which mistakes in initial allocations help explain revisions toward sooner. The fraction of decisions consistent with the law of demand enters the regression with a positive coefficient that is statistically significant at the 10% level. The other variables added in this column (words recalled, Raven's test score, and financial literacy score) do not enter the regression statistically significantly at conventional levels.

In column 4 we add two variables to the regression representing shocks experienced in the time since the baseline survey. The point estimates on both death in the family and on the

[28] Results are quantitatively similar if we reduce the tolerance to just one token.

[29] In essence, if a respondent is making allocations completely randomly in both the baseline and in the revisit, individuals whose sooner allocations are higher in the near period than in the far period will on average have sooner allocations in the far period that are greater than 10 tokens. Then, random choice in the revisit will make such individual more likely to choose sooner allocations than other individuals who did not exhibit "present-bias". See the discussion in section 8 below for further details.

[30] In section 4.2.1 below we explore various specifications of the target lag variable, and show that the relationship between the target lag and revisions towards sooner is indeed best described by a step function of this sort.

[31] This coefficient remains very stable across specifications of the table, reflecting the fact that it was randomly assigned and therefore uncorrelated on average with other variables.

shortfall in expected income both have the expected signs (negative shocks lead to more revision towards sooner), but neither coefficient is statistically significantly different from zero. Adding these measures of shocks also does not much influence the coefficients on other variables.

In column 5, we add to the regression measures of social pressure. The first variable is one's spouses allocation to sooner minus one's own, averaged across the 9 baseline allocations (out of 10) that exclude the implemented choice.[32] This variable should capture pressure to revise one's allocation toward sooner coming from one's spouse. 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, and the spousal variable is statistically significant at the 10% level. Interestingly, inclusion of the social pressure variables causes the coefficient on the fraction of decisions consistent with the law of demand to fall substantially in magnitude and to lose statistical significance.

Finally, in column 6, we add several baseline characteristics of one's spouse to the regression (results not shown for brevity).[33] We include these variables to gauge the extent to which any of the results we have described so far may simply be due to the influence of omitted spousal variables, a concern particularly relevant for the coefficient on the spousal allocation variable in column 5. As it turns out, the coefficient on the spousal allocation variable actually becomes larger in magnitude (and is now significant at the 5%) level. This perhaps suggests that other spousal variables associated with less revision toward sooner were positively correlated with the spousal allocation variable, so that controlling for these variables leads to a larger coefficient on spousal allocations.

Looking across the columns, it is of interest that the fraction present biased variable becomes progressively larger in magnitude with the addition of the various controls, so that in column 6 (the specification inclusive of the most controls) has nearly doubled in magnitude vis-a-vis column 2 and is now statistically significantly different from zero at the 10% level.

In sum, the patterns in Table 6 have identified two robust statistical relationships. Revisions toward sooner are larger when individuals make their revision decision closer to the funds disbursement date, and when their spouses have stronger preferences for sooner allocations (relative to their own). In addition, upon inclusion of the full set of control variables, it appears that individuals whose allocations under commitment in the baseline survey are more present-biased choose

[32] As with the present-bias ratio, we exclude the implemented choice from this calculation because our simulation analysis indicates that its inclusion would lead to a spurious positive relationship. See section 8 in the Appendix for details.

[33] These variables are: fraction present biased across 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.

to re-allocate more towards sooner when that commitment is broken upon revisiting. By contrast, variables representing financial sophistication and shocks do not have statistically significant or robust relationships with revision behavior.

Taken together, these results are most strongly supportive of present-bias and spousal pressure as determinants of revisions toward the present. They provide no support for shocks or mistakes in initial allocations (which should be more prevalent for those with lower financial sophistication) as important determinants of revision.

The right-hand-side variables that are statistically significantly related to revision are also economically significant. Table 7 presents a summary of magnitudes of these key relationships, using the coefficients from Table 6, column 6. 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 101.96 MK increase in revisions toward sooner. In comparison, a one-standard-deviation increase in the measure of present-bias is associated with 40.03 MK higher revisions toward sooner, making one's revision decision within 6-days of day t=61 raises revisions toward sooner by 134.47, and a one-standard-deviation increase in the measure of spousal pressure is associated with 121.39 MK more revisions toward sooner.

4.2.1 Additional analyses and robustness checks

Future bias vs. present bias Given the positive relationship found in column 6, Table 6 between present-biased preference reversals under commitment and revisions toward sooner, it is of interest to examine whether *future*-biased preference reversals exhibit an opposite (negative) relationship with revisions to sooner. Column 7 of Table 6 tests this proposition, replacing the fraction present biased variable with an analogously-defined "fraction future biased" variable.

As it turns out, the coefficient on fraction future biased choices is actually positive, but this coefficient is closer to zero than the coefficient on fraction present biased, and it is far from being statistically significantly different from zero at conventional levels.

Alternate specifications of target lag In all regressions of Table 6, the variable for targeted days to first disbursement upon revisiting is specified as an indicator variable for 6 days or less. Here we elaborate on the justification for this specification.

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 6, column 6, the coefficient on the linear target lag variable is -11.08 and has a standard error of 5.54 (significant at the 5% level).[34]

[34] All other coefficients in the regression remain essentially identical to those reported in Table 6, column 6.

It turns out, however, that the linear relationship just estimated masks the fact that the underlying relationship between the target lag and revisions is better described as a step function. To see this, we again estimate the specification of Table 6, column 6, but now we replace the target lag as separate indicator variables for each of the 14 distinct values of the target lag from 2 to 15 days prior to first disbursement (the omitted indicator is 16 days). Rather than show a regression table, in Figure 5 we graphically present the estimated coefficients on the target lag indicators. The solid line graphs the series of point estimates, and the upper and lower dashed lines bound the upper and lower 95% confidence intervals.

Point estimates on the indicators for days 2 through 6 are all large in magnitude, each exceeding 100 MK, and show no obvious time pattern. In contrast, nearly all the coefficients on the other 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, 189 MK. This is probably a chance occurrence.) Due to lack of power, none of the individual coefficients are different from zero at the 5% level (although the coefficients on the indicators for days 2 and 4 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 2-6 prior to disbursement, and zero effect thereafter.

Attrition We attempted to revisit 726 individuals with complete baseline data. We were successful at revisiting 664 (91.5%). 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.

Table 8 presents regressions of an indicator for inclusion in the sample on key right-hand-side variables. The sample is the 726 individuals we attempted to revisit, so the mean of the dependent variable is the revisit success rate, 0.915.

Individuals targeted for revisit 6 days or less prior to first disbursement are 10.6 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.[35]

In addition, individuals whose implemented choice had a higher interest rate also are more likely to be included in the sample. A 50-percentage-point increase in the interest rate leads to a

[35] The closest randomized target date was 2 days prior to first disbursement, and the cutoff date for actual revisits was set at 1 day prior to first disbursement. Revisits 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).

3.1 percentage point increase in the likelihood of revisit success. Individuals with higher interest rates may have had higher survey attachment simply because the total value of resources being disbursed to them was higher.[36]

An important question to raise is whether the key results (in Table 6) on the impact of days to first disbursement and of the implemented interest rate on revisions could be driven entirely by selection, since both these variables are statistically signficantly related to revisit success. Given the sizes of the effects in Table 6, this turns out to be implausible.

Consider first the coefficient in column 6, Table 6 on the indicator for targeted days to first disbursement <=6, which is 134.47. This variable leads to 10.6 percentage points lower inclusion in the sample. For differential selection on this variable to fully explain the coefficient in column 6, Table 6, revision towards sooner of individuals selecting out of the sample due to having days to first disbursement <=6 would have to have been lower by 1,226.59 MK.[37] A change in revisions of this magnitude would be extremely large; a change of this magnitude is roughly the difference between the 10th percentile (-600 MK) to the 84th percentile (600 MK) of the revision distribution, or about 2 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.

Next, consider the implemented interest rate. A 50-percentage-point increase in the rate of return leads to a 3.1 percentage point increase in the likelihood of revisit success. For differential selection on this variable to fully explain the coefficient in column 6, Table 6, revision towards sooner of individuals selecting out of the sample due to having a 50 percentage point lower interest rate would have to have been lower by 3,160.76 MK.[38] This amounts to roughly the difference between the 1st percentile (-1600 MK) to the 98th percentile (1500 MK) of the revision distribution. Again

[36] In addition, individuals with higher word recall are less likely to be included in the sample. 2 additional words recalled (about one and a half standard deviations) leads to a 2.9 percentage point lower likelihood of revisit success. Revisit success was higher for individuals who are younger who had lower baseline wealth.

[37] Let there be two types of individuals: type 1, who we always successfully revisit, and type 2, who are only successfully revisted if days to first disbursement is >6. So when days to first disbursement is >6, 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 <=6 in the table (134.47), α is the revisit success rate for type 1 individuals (0.861), γ is the reduction in the revisit success rate dur to revisiting 6 or fewer days to first disbursement (0.106), and μ_1 is the mean revision for type 1 individuals (mean revision for those with days to first disbursement <=6, 149.3). The formula gives $\mu_2 = -1077.28$. So $\mu_1 - \mu_2 = 149.31 - (-1077.28) = 1226.59$.

[38] This calculation uses the same formula described in the previous footnote. We let $\mu_1 = 23.2$ (mean revisions for r>=0.75), $\alpha = 0.9$, $\gamma = 0.03$, and $\beta = 101.96$.

it is implausible that all those attriting on the basis of the interest rate differential would have revision differentials of this magnitude.

While it is very unlikely that the estimates of the impact of days to disbursement and of the implemented interest rate from column 6, Table 6 are due entirely to selection, selection may still lead to bias in these estimates. In Table 9 we present results of an exercise intended to bound the size of this possible bias, running regressions analogous to that of column 6, Table 6 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 revision variable are for the newly-included observations.[39] 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.[40] 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, the coefficients on the indicator for days to first disbursement <=6 and on the implemented interest rate become larger in magnitude, reflecting the fact that these variables are, respectively, negatively and positively correlated with revisit success. For the same reason, assuming negative revisions toward sooner leads the coefficients on these variables to become smaller in magnitude. In columns 1-6, the coefficient on the days to disbursement indicator remains statistically significantly different from zero at conventional levels, but in column 7 (where attriters are assumed to revise as much as -600) the coefficient has decline enough so that it is no longer statistically significant at conventional levels. The coefficient on the implemented interest rate, on the other hand, remains statistically significant at conventional levels throughout the range of assumed attriter revisions.

These results indicate that the coefficient on the indicator for days to first disbursement <=6 in Table 6 is robust to a wide range of assumptions on attriter revisions, but when attriter revision is assumed to be as much as -600, then the coefficient declines enough in magnitude to become statistically insignificant. The coefficient on the implemented interest rate is more robust to attri-

[39] The only other difference vis-a-vis the regression in column 6, Table 6 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.

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

tion, reflecting the fact that this variable's relationship with revisit success is smaller in economic magnitude. 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.

Males vs. females In Table 9 we explore whether estimated effects differ across males and females in the sample, estimating regressions analogous to column 6, Table 6, 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, some coefficients do not achieve conventional statistical significance levels, such as those on fraction present-biased (for both genders) and on spousal allocations (for males). But coefficient magnitudes are similar to those in the gender-pooled sample of column 6, Table 6.

For nearly all variables, coefficients are not statistically significantly different across the male and female samples, with the exception of the coefficients on the schooling indicators. 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. The male coefficients on schooling, on the other hand, are positive, but none are statistically significantly different from zero. Another result that emerges more weakly is that 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 (p-value 0.106).

5 Related Literature

There is a long tradition of estimating time preferences from observational data. 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), for example, 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 time preferences.

The experimental literature on time preference is large. Frederick et al (2002) offer a review. More recent examples include, Andersen et al. (2008), Benhabib, et al. (2010), and Andreoni and Sprenger (2010). As noted above, we adapt to our field environment the methods that Andreoni and Sprenger (2010) developed for use in the lab. A key advantage of the Andreoni and Sprenger (2010) design is that, with a convex choice set, it permits the relatively rapid collection of information on time preferences regarding several different rates of return. The method thus allows analysis of the basic consistency of choice as well as quantitative measures of the degree of dynamic inconsistency.

Most recently, experimental studies of time preference have been taken into the field, often in developing countries. Prominent examples include Harrison et al. (2005), Ashraf et al. (2006), and Tanaka, et al. (2009). Two such field experiments are closely related to ours and thus merit special consideration. The first, Ashraf et al (2006), asked a sample of 1,777 in the Philippines hypothetical time preference questions on a survey. Later, a subset of this sample was 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 prior study, most importantly, by studying directly the link between incentivized time preference decisions and revision of intertemporal plans. We measure the extent of preference reversals, as well as the basic consistency of choice with rational economic models thus provide a quantitative assessment of the mechanisms behind time inconsistency and the demand for commitment. The second closely related paper, Harrison et al. (2005), elicited time preferences from 253 participants in Denmark. Of this sample, 97 were later revisited and, importantly, asked to perform the same time preference experiment again. Our experiment is distinguished 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.

6 Conclusion

[TO BE ADDED.]

7 Appendix A: Variable definitions

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

survey, or from administrative (project) data.

7.1 Variables collected in baseline survey

Present-biased ratio is fraction of choices where allocation to sooner in near period is more than 100MK higher than allocation to sooner in far period (comparing choices in near and far periods for same interest rate). In all regressions this variable excludes the implemented interest rate from the calculation, but summary statistics are also included for all choices including the implemented interest rate.

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

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

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).

7.2 Variables collected in revisit survey

Indicator for death in family indicates death in respondent's own household in the time since the baseline survey (reported in 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. Both

expectation and actual income reports refer to same period (time between baseline and revisit survey), reported in revisit survey.

7.3 Variables from administrative (project) data

Days to first disbursement at revisit (targeted) is randomized target number of days prior to first far period disbursement that revisit survey is planned. 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 period 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.

8 Appendix B

Simulation analysis description and results. [TO BE ADDED.]

Leads to conclusion that we should focus on versions of present-bias and spousal-bias variables that involve non-implemented choices only.

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Table 1: Summary Statistics

Variable	N	Mean	Std. Dev	Min	10th pctile	50th pctile	90th pctile	Max
Panel A: Baseline sample (stage 1)								
Change in Allocation to Sooner between Periods Averaged Across All Interest Rates (MK)	2285	15.70241	308.287	-2000	-280	0	340	2000
Fraction Present Biased, All Interest Rates	2285	.2857768	.2734515	0	0	.2	.6	1
Fraction Present Biased, Non-Implemented Interest Rates	2282	.285057	.2867607	0	0	.25	.75	1
Fraction of Decisions Consistent with Law of Demand	2285	.8073851	.1815765	0	.5	.875	1	1
Implemented Interest Rate	2282	.625745	.3288516	.1	.1	.75	1	1
Demographics								
Male	2285	.5006565	.500109	0	0	1	1	1
Respondent's Own Age	2283	44.05694	23.79293	-99	27	46	66	95
Less than 35 Years Old	2236	.2383721	.4261831	0	0	0	1	1
35-57 years old	2236	.4995528	.5001116	0	0	0	1	1
Respondent's Spouse's Age	2285	36.32079	37.29486	-99	23	44	63	95
Years of Schooling	2285	4.375492	4.166196	-9	0	4	8	77
Some Primary School	2283	.6062199	.4886941	0	0	1	1	1
Primary School	2283	.1449847	.3521625	0	0	0	1	1
More than Primary School	2283	.0722733	.2589967	0	0	0	0	1
Have Adequate Maize	2285	.214442	.4105245	0	0	0	1	1
Number of Relatives in Village	2281	4.570364	8.452991	0	0	2	10	132
Total Number of People in Village	2285	175.3217	252.8775	0	34	120	300	4000
Aptitude Questions								
Words Recalled - First Time	2284	4.778021	1.327372	0	3	5	6	10
Number Correct on Raven's Matrices	2285	1.508972	.9256668	0	0	1	3	3
Financial Literacy Questions Correct	2277	.7176109	.9785538	0	0	0	2	3
Panel B: Revisit sample (stage 2)								
Change in Sooner Allocation upon Revisiting (MK)	664	61.89759	594.8827	-2000	-600	0	900	2100
Indicator: Change in Sooner Allocation Upon Revisisting is Negative	664	.311747	.4635563	0	0	0	1	1
Indicator: Change in Sooner Allocation Upon Revisiting is Positive	664	.3403614	.4741879	0	0	0	1	1
Fraction Present Biased, All Interest Rates	664	.3024096	.2815497	0	0	.2	.8	1
Fraction Present Biased, Non-Implemented Interest Rates	664	.2985693	.2948045	0	0	.25	.75	1
Fraction of Decisions Consistent with Law of Demand	664	.8115587	.1736808	.25	.625	.875	1	1
Days to First Disbursement at Revisit (Targeted)	664	9.23494	4.423874	2	3	10	15	16
Days to First Disbursement at Revisit (Actual)	664	8.993976	4.449139	1	3	9	15	16
Indicator: Days to First Disbursement (Targeted) is 6 days or less	664	.3237952	.4682757	0	0	0	1	1
Implemented Interest Rate	664	.5803464	.3214574	.1	.1	.75	1	1
Shocks								
Death in Family	664	.0240964	.1534641	0	0	0	0	1
Shock to Expected HH Income	664	113.5719	713.032	-2985	-90	0	350	13735
Spouse minus own allocation to sooner (MK)	664	712.8012	487.9872	0	0	700	1400	2000
Wealth and Income (reported at baseline)								
Total HH Wealth	664	104.8526	212.0545	.6	9.8	44.05	223.8	3199.3
HH Total in Bank	664	3.58869	16.4789	0	0	0	7	295
HH Total Cash	664	1.294729	13.58916	0	0	0	1.2	340
HH Items	664	57.05044	150.8103	0	5.9	23.155	118.15	2954.3
HH Animals	664	42.91872	100.5479	0	.2	13	110.5	1178
Expected Income (in period between baseline and revisit)	664	17.26942	69.27772	0	0	1	40	1377

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). Revisit dataset restricted to observations with non-missing data on all baseline characteristics used in revisit regression analyses. All wealth and income variables are expressed in thousands of Malawi kwacha.

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

Number of Consistent Pairs	Frequency	Percent	Cumulative
0	3	0.13%	0.13%
1	6	0.26%	0.39%
2	16	0.70%	1.09%
3	48	2.10%	3.19%
4	168	7.35%	10.55%
5	321	14.05%	24.60%
6	482	21.09%	45.69%
7	530	23.19%	68.88%
8	711	31.12%	100.00%
Total	2,285	100.00%	

Notes: Data are from baseline sample (for details, see Table 1). Table presents share of individuals whose allocations in 8 pairs of choices (with adjacent interest rates) are consistent with law of demand.

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

			Percentiles					
	Mean	**Std. Dev**	**10th**	**25th**	**50th**	**75th**	**90th**	**Percent at a corner**
Near period								
t+30 at r=10%	1293.0	522.4	660.0	1100.0	1210.0	1650.0	2090.0	13%
t+30 at r=25%	1534.1	599.0	750.0	1250.0	1500.0	1875.0	2500.0	14%
t+30 at r=50%	1923.8	733.2	1050.0	1500.0	1950.0	2400.0	3000.0	16%
t+30 at r=75%	2250.6	883.4	1050.0	1750.0	2275.0	2975.0	3500.0	17%
t+30 at r=100%	2705.3	1067.9	1200.0	2000.0	2800.0	3600.0	4000.0	22%
Far period								
t+90 at r=10%	1306.0	516.8	660.0	1100.0	1320.0	1650.0	2090.0	12%
t+90 at r=25%	1565.4	587.4	875.0	1250.0	1500.0	2000.0	2500.0	14%
t+90 at r=50%	1916.9	733.1	900.0	1500.0	1950.0	2400.0	3000.0	16%
t+90 at r=75%	2301.3	869.2	1225.0	1750.0	2275.0	2975.0	3500.0	17%
t+90 at r=100%	2746.1	1034.0	1400.0	2000.0	2800.0	3800.0	4000.0	23%

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=30. 2nd set of 5 choices is in "far" period, when allocations are between t=60 and t=90. 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 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***	0.951***	0.932***	0.935***
	(0.028)	(0.028)	(0.028)	(0.028)
Male		0.018		0.018
		(0.040)		(0.041)
Less than 35 yrs old		0.066		0.069
		(0.055)		(0.057)
35-57 yrs old		0.058		0.035
		(0.043)		(0.046)
Some primary school		0.021		0.001
		(0.046)		(0.048)
Primary school		-0.031		-0.071
		(0.071)		(0.071)
More than primary school		0.037		0.014
		(0.096)		(0.089)
Have adequate maize		0.020		0.042
		(0.046)		(0.045)
log(Baseline wealth)		0.022		0.033**
		(0.014)		(0.015)
Words recalled		0.020		0.020
		(0.014)		(0.015)
Raven's Tests Correct		-0.027		-0.040*
		(0.020)		(0.021)
Financial Literacy Questions Correct		0.026		0.033
		(0.025)		(0.025)
Number of relatives in the village		0.004*		0.007***
		(0.002)		(0.002)
Constant	0.382***	0.163	0.406***	0.132
	(0.029)	(0.125)	(0.030)	(0.133)
N	9264	9264	9257	9257
Adjusted R-squared	0.0870	0.0937	0.0837	0.0931

Note: Dependent variable is natural log of later allocation minus natural log of sooner allocation. Unit of observation is choices made by respondents in baseline sample (10 choices per study respondent) that vary in time frame and rate of return for waiting; corner solutions excluded from regression sample. All allocations made in Jan-Feb 2010.

Table 5: Determinants of Fraction Consistent or Fraction Present-biased

Dependent variable:	Fraction consistent		Fraction present-biased	
Male	0.029*	0.028**	-0.001	0.000
	(0.015)	(0.014)	(0.013)	(0.013)
Less than 35 yrs old	-0.029	-0.006	0.021	0.012
	(0.021)	(0.019)	(0.018)	(0.018)
35-57 yrs old	-0.021	-0.019	0.010	0.009
	(0.017)	(0.015)	(0.014)	(0.014)
Some primary school	-0.024	-0.026	0.026	0.028
	(0.018)	(0.016)	(0.016)	(0.015)
Primary school	-0.028	-0.014	0.017	0.012
	(0.026)	(0.024)	(0.023)	(0.022)
More than primary school	-0.054	-0.058	0.041	0.043
	(0.034)	(0.030)	(0.030)	(0.030)
Have adequate maize	0.033*	0.028*	-0.009	-0.007
	(0.017)	(0.016)	(0.015)	(0.014)
Baseline wealth (100s of MK)	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Words recalled	0.002	0.000	0.001	0.002
	(0.005)	(0.005)	(0.005)	(0.005)
Raven's Tests Correct	0.000	0.005	-0.007	-0.009
	(0.008)	(0.007)	(0.007)	(0.007)
Financial Literacy Questions Correct	0.008	-0.004	-0.005	-0.001
	(0.009)	(0.008)	(0.007)	(0.007)
Number of relatives in the village	0.000	0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Adherence to law of demand ratio [0,1]		0.721***		-0.273***
		(0.039)		(0.033)
Constant	0.460***	-0.127**	0.246***	0.468***
	(0.035)	(0.044)	(0.031)	(0.041)
N	2220	2220	2220	2220
Adjusted R-squared	0.0019	0.1719	-0.0020	0.0301

Note: Dependent variable is natural log of later allocation minus natural log of sooner allocation. Unit of observation is choices made by study respondents (10 choices per study respondent) that vary in time frame and rate of return for waiting; corner solutions excluded from regression sample. All allocations made in Jan-Feb 2010.

Table 6: Determinants of revisions toward sooner
Ordinary least-squares regressions

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Hyperbolic preferences							
Fraction Present Biased, Non-Implemented Interest Rates		78.627	106.827	107.725	125.804	142.088*	
		(80.555)	(82.718)	(82.960)	(84.407)	(82.504)	
Fraction "Future Biased", Non-Implemented Interest Rates							78.271
							(91.218)
Indicator: days to first disbursement (targeted) <=6		131.193**	134.478**	135.568**	127.326**	134.473**	135.160**
		(52.910)	(52.960)	(53.071)	(52.651)	(52.524)	(52.802)
Financial sophistication							
Fraction of Decisions Consistent with Law of Demand			260.006*	266.391**	150.765	102.186	100.313
			(135.155)	(135.266)	(151.469)	(155.438)	(160.346)
Words recalled			2.621	3.568	0.654	0.94	2.083
			(19.252)	(19.104)	(19.230)	(19.184)	(19.362)
Raven's Tests Correct			-32.388	-33.348	-28.961	-21.361	-23.218
			(28.725)	(29.009)	(28.745)	(29.155)	(29.391)
Financial Literacy Questions Correct			11.639	10.424	11.599	22.079	24.527
			(29.080)	(28.876)	(28.621)	(28.731)	(28.598)
Shocks							
Death in the family (indic.)				92.573	96.802	99.013	112.239
				(206.914)	(198.733)	(195.365)	(194.462)
Shortfall in expected hh income (MK)				0.055	0.048	0.046	0.046
				(0.040)	(0.041)	(0.038)	(0.037)
Social pressure							
Spouse minus own allocation to sooner (MK)					0.118*	0.177**	0.164**
					(0.060)	(0.069)	(0.067)
Number of relatives in the village					1.558	2.01	2.048
					(3.379)	(3.306)	(3.273)
Rate of return to waiting							
Implemented interest rate {.1,.25,.5,.75,1}	-162.524**	-179.280**	-189.400**	-180.689**	-191.574**	-203.924**	-189.367**
	(78.864)	(78.205)	(79.058)	(78.807)	(78.965)	(80.548)	(80.961)
Baseline characteristics							
Male	125.728**	111.607**	110.952**	113.183**	99.836*	60.821	70.034
	(49.606)	(50.162)	(52.020)	(51.749)	(53.383)	(61.550)	(61.451)
Less than 35 yrs old	197.614***	186.339**	190.777***	188.120**	183.341**	265.888**	275.906**
	(71.869)	(72.851)	(72.778)	(73.193)	(73.303)	(110.626)	(110.260)
35-57 yrs old	119.759**	120.569**	127.703**	131.061**	123.888**	176.758***	175.070***
	(55.120)	(55.085)	(54.585)	(54.784)	(56.404)	(65.652)	(65.556)
Some primary school	-65.607	-71.490	-66.035	-63.781	-64.438	-29.605	-19.783
	(70.004)	(69.812)	(72.266)	(70.630)	(71.251)	(72.854)	(72.924)
Primary school	-162.248*	-152.894*	-144.630	-154.896*	-167.964*	-131.749	-123.775
	(87.447)	(87.935)	(93.716)	(92.814)	(92.610)	(95.525)	(95.066)
More than primary school	-214.153**	-206.063*	-195.854	-199.163*	-202.153*	-132.429	-129.135
	(106.626)	(106.779)	(119.366)	(118.025)	(116.969)	(120.683)	(121.485)
Have adequate maize	40.206	33.232	43.267	39.496	31.375	10.988	4.783
	(55.978)	(55.528)	(56.383)	(57.170)	(57.448)	(57.090)	(57.154)
Total HH Wealth	-0.135	-0.090	-0.098	-0.105	-0.104	-0.106	-0.103
	(0.087)	(0.086)	(0.082)	(0.082)	(0.082)	(0.087)	(0.089)
Controls for:							
Spousal characteristics	-	-	-	-	-	Y	Y
R-squared (adj.)	0.0143	0.0225	0.0249	0.0267	0.0317	0.0406	0.037
N	664	664	664	664	664	664	664

Significance levels: * 10%, ** 5%, *** 1%. Robust standard errors in parentheses.
Notes: Unit of observation is individual included in revisit sample. All regressions also include the following controls: indicators for each of 6 commitment savings treatments implemented in Brune, Gine, Goldberg, and Yang (2011); indicator for whether "near" choices were presented before "far" choices in baseline survey; indicator for project staff error in presentation of baseline allocation upon revisiting. 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.

Table 7: Relative magnitudes of estimated empirical relationships

Independent variable	Hypothetical shift	Effect on revisions to sooner (MK)
Implemented rate of return {.1,.25,.5,.75,1}	50 percentage point reduction	101.96
"Present"-biased ratio [0,1]	0.28 increase (1 std. dev.)	40.03
Days to first disbursement <=6 (indic.)	From 0 to 1	134.47
Spouse minus own allocation to sooner (MK)	MK686 increase (1 std. dev.)	121.39

Note: Calculations use coefficients in Table 6, column 6.

Table 8: Determinants of inclusion in revisit sample
Ordinary least-squares regressions

Dependent variable: Indicator for inclusion in revisit sample

Hyperbolic preferences	
Fraction Present Biased, Non-Implemented Interest Rates	-0.002
	(0.035)
Indicator: days to first disbursement (targeted) <=6	-0.106***
	(0.025)
Financial sophistication	
Fraction of Decisions Consistent with Law of Demand	0.025
	(0.071)
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}	0.061*
	(0.035)
Baseline characteristics	
Male	0.039
	(0.030)
Less than 35 yrs old	0.074*
	(0.044)
35-57 yrs old	-0.045
	(0.031)
Some primary school	-0.039
	(0.029)
Primary school	-0.058
	(0.045)
More than primary school	-0.061
	(0.045)
Have adequate maize	0.008
	(0.026)
Total HH Wealth	-0.000***
	(0.000)
Controls for:	
Spousal characteristics	Y
R-squared (adj.)	0.0514
N	726

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 0.915. Right-hand-side variables are identical to column 6, Table 6, 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 6 for other notes.

Table 9: 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.
Sample:	Females	Males	
Hyperbolic preferences			
Fraction Present Biased, Non-Implemented Interest Rates	140.035	83.365	0.739
	(134.395)	(104.257)	
Indicator: days to first disbursement (targeted) <=6	117.308	157.076**	0.707
	(78.283)	(71.043)	
Financial sophistication			
Fraction of Decisions Consistent with Law of Demand	20.158	137.861	0.721
	(241.159)	(224.848)	
Words recalled	-11.178	29.373	0.333
	(25.398)	(33.282)	
Raven's Tests Correct	15.746	-63.496	0.171
	(41.328)	(40.505)	
Financial Literacy Questions Correct	26.418	14.41	0.85
	(51.553)	(36.841)	
Shocks			
Death in the family (indic.)	506.886	-144.723	0.106
	(326.547)	(234.770)	
Shortfall in expected hh income (MK)	0.06	0.018	0.574
	(0.047)	(0.059)	
Social pressure			
Spouse minus own allocation to sooner (MK)	0.172**	0.155	0.899
	(0.085)	(0.101)	
Number of relatives in the village	3.432	2.904	0.946
	(6.614)	(4.127)	
Rate of return to waiting			
Implemented interest rate {.1,.25,.5,.75,1}	-211.595*	-212.085*	0.998
	(122.876)	(108.815)	
Baseline characteristics			
Less than 35 yrs old	192.210	236.948	0.845
	(159.966)	(163.843)	
35-57 yrs old	46.278	226.424***	0.224
	(122.470)	(82.949)	
Some primary school	-176.921*	176.604	0.021
	(100.326)	(115.136)	
Primary school	-325.024*	47.964	0.099
	(182.825)	(131.762)	
More than primary school	-507.884***	223.637	0.003
	(179.409)	(167.425)	
Have adequate maize	39.359	-28.368	0.569
	(85.601)	(82.287)	
Total HH Wealth	-0.313*	-0.007	0.128
	(0.173)	(0.101)	
Controls for:			
Spousal characteristics	Y	Y	
R-squared (adj.)	0.0431	0.0373	
N	325	339	

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 6 for other notes.

Appendix Table 1: Bounds on bias due to selection into revisit sample
Ordinary least-squares regressions

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Assumed value of dep. var. is initial sooner allocation (from baseline) plus:	600	400	200	0	-200	-400	-600
Hyperbolic preferences							
Fraction Present Biased, Non-Implemented Interest Rates	126.865	126.861*	127.648*	128.434*	127.901*	127.370*	128.502*
	(77.080)	(75.795)	(75.132)	(75.114)	(75.510)	(76.357)	(77.492)
Indicator: days to first disbursement (targeted) <=6	188.703***	165.712***	143.124***	120.537***	102.682**	86.778*	71.094
	(47.599)	(46.653)	(46.212)	(46.295)	(46.766)	(47.620)	(48.782)
Financial sophistication							
Fraction of Decisions Consistent with Law of Demand	87.236	80.604	77.562	74.52	89.117	107.898	120.602
	(144.924)	(142.541)	(141.078)	(140.943)	(142.076)	(144.368)	(147.113)
Words recalled	4.072	2.213	-0.264	-2.742	-5.178	-7.151	-9.32
	(18.382)	(18.020)	(17.770)	(17.670)	(17.705)	(17.864)	(18.129)
Raven's Tests Correct	-26.07	-23.282	-20.547	-17.812	-13.984	-11.685	-9.465
	(27.029)	(26.580)	(26.312)	(26.279)	(26.460)	(26.783)	(27.270)
Financial Literacy Questions Correct	15.569	16.999	19.318	21.637	21.916	21.173	19.648
	(26.635)	(26.153)	(25.875)	(25.865)	(26.088)	(26.496)	(27.070)
Social pressure							
Spouse minus own allocation to sooner (MK)	0.161**	0.159**	0.157**	0.155**	0.156**	0.158**	0.164**
	(0.063)	(0.062)	(0.062)	(0.062)	(0.062)	(0.063)	(0.064)
Number of relatives in the village	0.961	1.041	1.154	1.266	1.522	1.758	1.964
	(3.010)	(2.968)	(2.965)	(3.001)	(3.077)	(3.172)	(3.284)
Rate of return to waiting							
Implemented interest rate {.1,.25,.5,.75,1}	-203.672***	-194.313***	-185.904***	-177.494**	-166.207**	-155.532**	-146.328*
	(72.931)	(71.955)	(71.482)	(71.636)	(72.315)	(73.385)	(74.815)
Baseline characteristics							
Male	38.015	44.493	50.861	57.229	62.947	69.138	75.155
	(57.562)	(56.311)	(55.637)	(55.571)	(55.917)	(56.698)	(57.864)
Less than 35 yrs old	208.573**	217.843**	229.532**	241.222**	254.224**	262.741**	271.685**
	(104.570)	(103.205)	(102.447)	(102.401)	(103.003)	(104.121)	(105.783)
35-57 yrs old	173.861***	164.889***	156.133***	147.376**	142.933**	140.347**	139.981**
	(62.024)	(60.558)	(59.657)	(59.367)	(59.664)	(60.501)	(61.705)
Some primary school	-7.636	-15.696	-23.477	-31.257	-37.854	-42.961	-45.626
	(69.430)	(68.656)	(68.292)	(68.370)	(68.742)	(69.551)	(70.736)
Primary school	-69.781	-82.930	-96.889	-110.849	-118.778	-123.867	-127.818
	(89.383)	(87.722)	(86.777)	(86.678)	(87.219)	(88.312)	(89.998)
More than primary school	-88.704	-101.981	-115.149	-128.316	-135.694	-138.864	-138.117
	(114.586)	(113.663)	(113.324)	(113.636)	(114.448)	(115.551)	(116.780)
Have adequate maize	6.230	6.355	6.495	6.635	6.802	7.059	7.135
	(52.846)	(51.745)	(51.098)	(50.979)	(51.300)	(52.055)	(53.148)
Total HH Wealth	0.044	0.020	-0.005	-0.029	-0.055*	-0.081**	-0.089**
	(0.042)	(0.038)	(0.035)	(0.033)	(0.032)	(0.033)	(0.036)
Controls for:							
Spousal characteristics	Y	Y	Y	Y	Y	Y	Y
R-squared (adj.)	0.0422	0.0383	0.0352	0.0322	0.0317	0.032	0.0317
N	726	726	726	726	726	726	726

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 6, 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 6 for other notes.

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

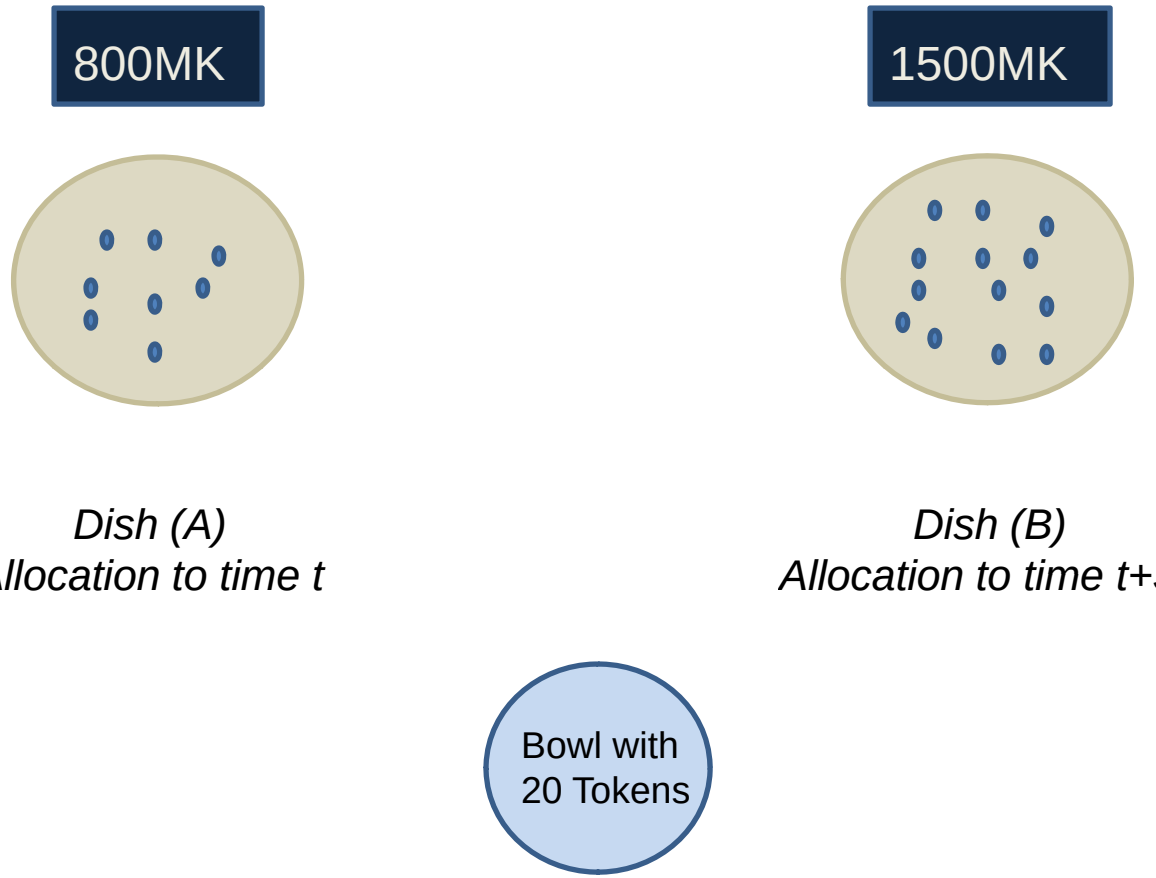


Figure 2: Schematic of the Revising Procedure; Example with r=0.25

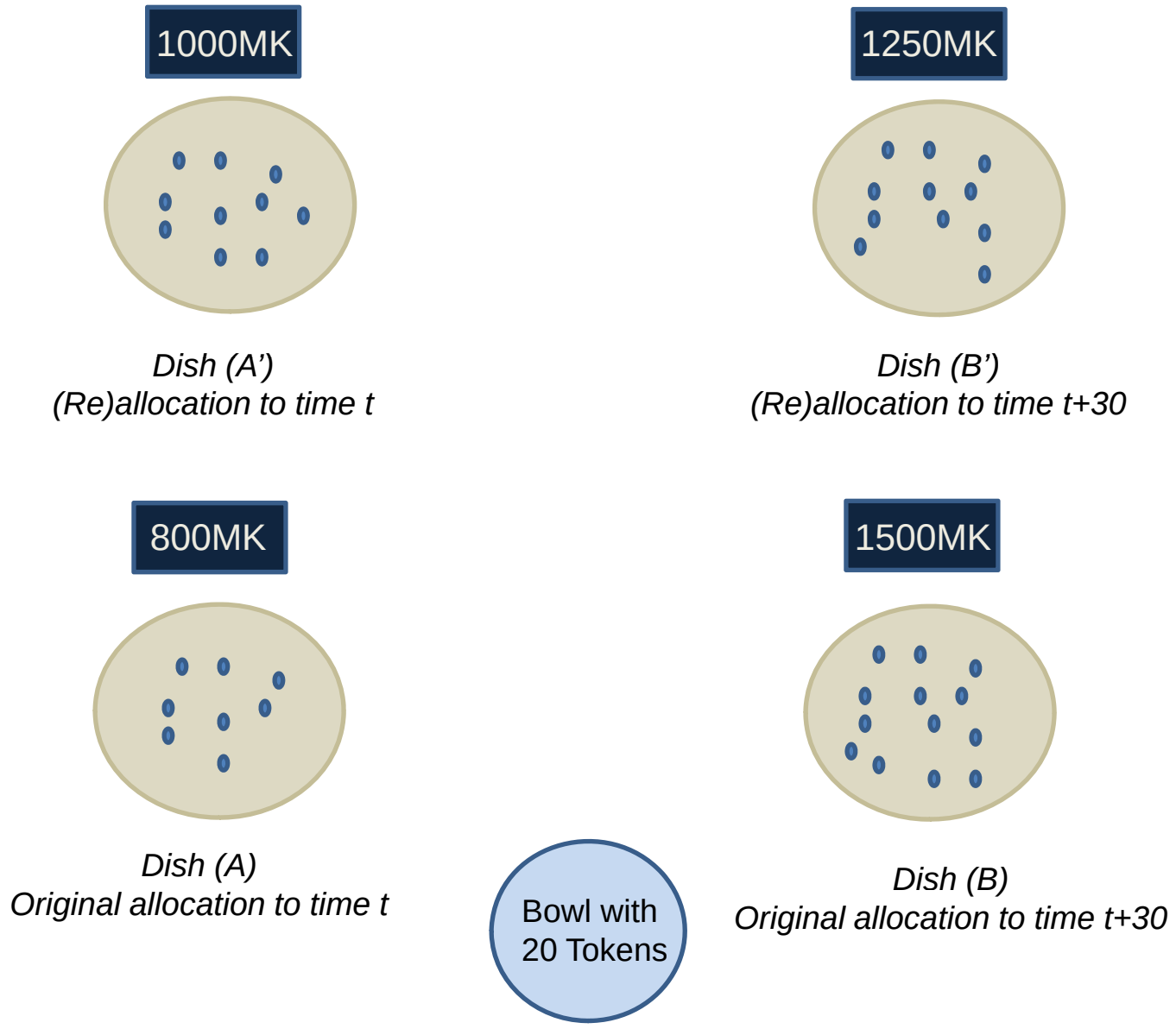
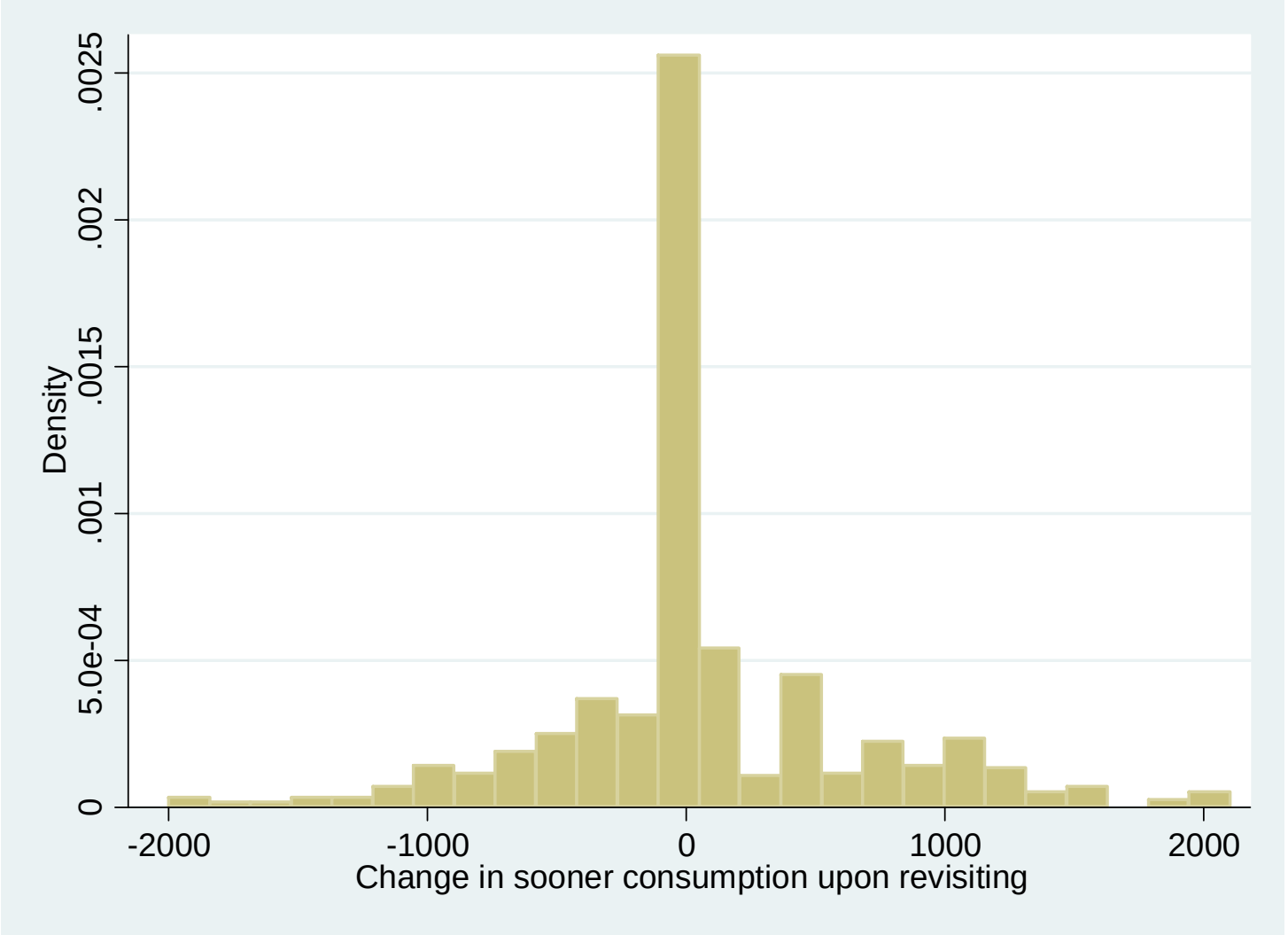


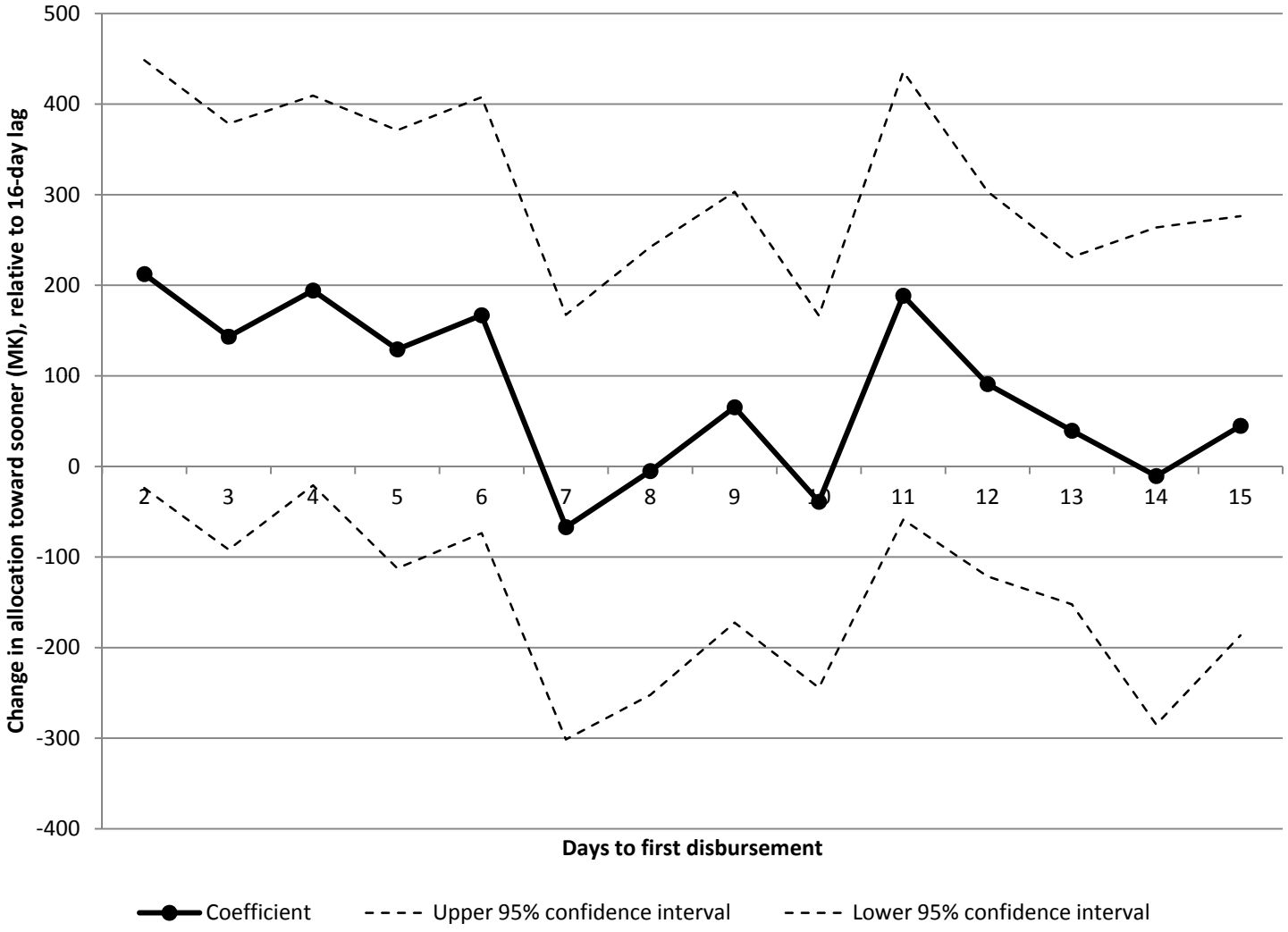
Figure 3: Timeline of Interviews, Choices, and Disbursement of Funds

Figure 4: Distribution of Change in Sooner Allocation Upon Revisiting



Notes: Initial allocations made in Jan-Feb 2010. Revisions made in Mar-Apr 2010 in a revisit targeted at a randomized 2-16 days prior to date of first disbursement in "far" period. (Date of first disbursement in far period is day t=61 from initial visit in Jan-Feb 2010.) Mean is 64.8. 35% make no change. N=701.

Figure 5: Impact of targeted days before disbursement on revisions toward sooner



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