Economic Research Using Laboratory Experiments to Investigate Behavioural Aspects of Savings Decisions

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Research paper prepared for the
Task Force on Financial Literacy

Date of publication: February 9, 2011

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Economic Research Using Laboratory Experiments to Investigate Behavioural Aspects of Savings Decisions

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Executive Summary

In this report we investigate behavioural aspects of savings decisions, using controlled laboratory experiments in which people are asked to make intertemporal choices with real money. In particular, we explore:

- *Default bias* (a postulated tendency towards saving when automatically opting into a savings plan is the default and where a conscious decision must be made to opt out);
- *Save-more-tomorrow* (a postulated tendency for willingness to commit future earned income to savings);
- *Labelling bias* (a postulated bias towards saving less when the money that can be saved is labelled "bonus" or "winnings" rather than "earned");
- *Beliefs and peer influence* (whether beliefs about the savings decisions of others or feedback about the actual decisions of other individuals influence savings decisions);
- *Relative anchoring bias* (a postulated tendency towards choosing to save when the savings option can be easily ranked as superior to a similar alternative);
- *Prior focus on financial decisions, financial information and tradeoffs between current and future income* (whether individuals who are engaged in thinking about financial matters prior to making savings decisions are more likely to save).

Before designing and implementing the above experiments, an experimental protocol (design structure) was developed to allow for the existence of loss aversion when individuals make savings decisions in the laboratory. Loss aversion is a behavioural trait whereby individuals give prospective losses more weight than prospective gains relative to some current reference state. Loss aversion has an important application to savings decisions and savings behaviour because a decision to save is a decision to incur a loss of current consumption in return for future consumption. Thus, loss aversion inhibits decisions to save money. As such, a protocol implementing a sense of loss aversion among laboratory experiment participants is required in order to capture the behavioural traits present in more typical (i.e., non-laboratory) savings decisions.

Since participants in laboratory experiments make decisions with money provided by the experimenter, we had to develop a protocol that would create a sense of entitlement to this money. Our protocol was successful; it demonstrated that when participants in laboratory experiments first earn then retain money prior to making intertemporal choices they exhibit behaviour consistent with loss aversion. Once developed, we then applied this protocol to the six experiments referred to above. The general structure of these experiments is illustrated below.

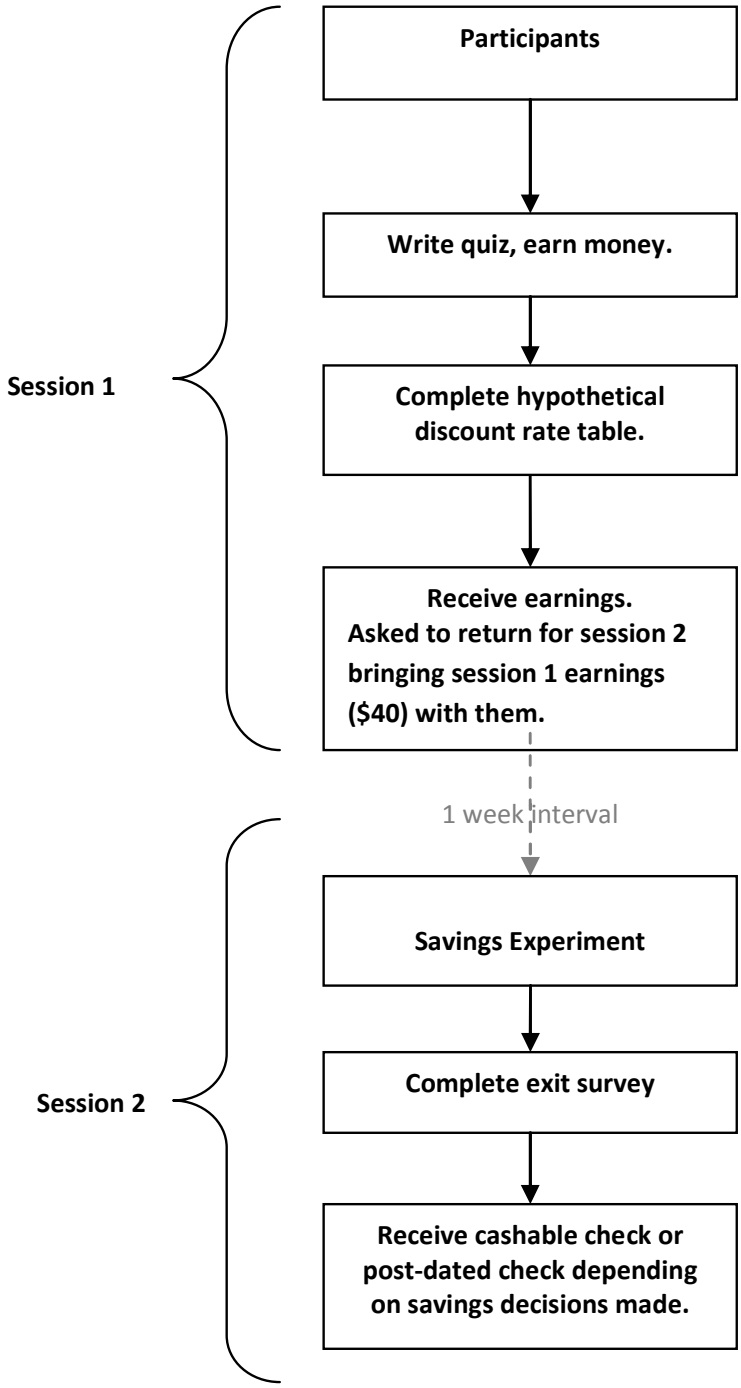
Default Bias, Labelling Bias and Beliefs

Within the context of our loss aversion protocol (section 2) we found no evidence of a default bias in our module 1 experiment. While the literature has discussed the default bias as arising in unfamiliar or non-routine decision environments, our results suggest that the default bias identified in other studies is largely an artefact of decision making over wealth or resources that have not yet been received by an individual. As such, from the standpoint of policy design, this suggests that default biases work best in situations wherein individuals do not yet have a stake in the resources in question (i.e., money in hand or a perception that the money is already theirs).

Similarly, in our labelling experiment, the results suggest that when loss aversion is present (as invoked by our protocol), the labels or semantic differences used to describe various sums of money do not affect intertemporal decision making or savings behaviour. This result should be taken as a cautionary notice that policies which seek merely to re-label funds or resources are unlikely to be successful in eliciting meaningful changes in behaviour.

We also found that beliefs about what others were saving and feedback on what others were actually saving did not influence individual-level behaviour. Furthermore, whether individuals were shown high or low amounts related to the savings behaviour of others, individual decision making was not affected. While evidence elsewhere suggests that individuals often respond to what they perceive to be the "social norm" in a given decision environment, our results indicate that the environment an individual perceives when considering saving is not a decision setting in which social effects exert any measureable effect. However, it is possible that more subtle effects of beliefs or peer influence were washed out by our loss aversion protocol; that is, the desire for immediacy may have overpowered these other effects.

Savings Experiments: General Design



W. G. Morrison and R. J. Oxoby, 2010.

Save-more-tomorrow

Our results in this experiment demonstrate how individuals respond with respect to intertemporal decisions over resources (here, money) which they have not yet acquired. Our results (again within our strong loss aversion protocol) demonstrate that individuals are willing to save money at a much lower rate of interest when, although already earned, it is not yet in their possession. From a policy-making standpoint, our results provide strong support for "save-more-later" types of interventions where individuals have an initial sense of ownership over money but do not yet possess these resources.

Relative Anchoring

In this experiment, we found significant evidence that when individuals are provided with a means of comparing two savings options easily ranked in terms of desirability, they will choose the superior savings option over not saving. Note that this behavioural tendency was strong enough to dominate our loss aversion protocol. This suggests that individuals make relative comparisons when making decisions (rather than being directly able to identify a "utility maximizing choice" in a simple choice environment) and that the simple comparison of savings options can result in greater demonstrated savings behaviour. This result is consistent with literature in behavioural economics and psychology wherein individuals make relative comparisons among available options. From the perspective of policy making, this results suggests that providing references to less desirable saving options in order to demonstrate the clear superiority of a preferred saving mechanism will encourage saving in the preferred option.

Focus on the Context of Financial Decisions Prior to Savings Decisions

We found that individuals are more patient (i.e., are willing to save at lower rates of interest) when they have been primed with a ten-question financial literacy quiz/questionnaire. This suggests that getting individuals to think directly about savings behaviour and the related trade-offs between current and future consumption results in their being more willing to engage in savings. From a policy perspective, this highlights the importance of educating and focusing individuals when they are faced with decisions. For

example, in a savings policy context, individuals should be put in a frame of mind where they are able to consider savings decisions in the appropriate context. Thus, our results provide support for moves to increase financial literacy. Policies that embed savings decisions in the context of future consumption are more likely to yield increased savings than are approaches in which savings policies/decisions are presented in the absence of an explicit intertemporal choice decision.

1. Introduction

Recent evidence on financial literacy in the United States suggests that a significant percentage of individuals are poorly informed about financial matters. For example, Lusardi and Mitchell (2006) administered a special module on financial literacy as part of a health and retirement study – a national longitudinal data set on Americans over 50. In questions testing the financial literacy of this group, 33 per cent of respondents could not correctly answer a rudimentary question on compound interest, 25 per cent could not correctly answer a simple question concerning inflation, and 48 per cent did not know the answer to a simple stock diversification question. 34 per cent could not provide any answer to this question. Similar studies are under way in Canada, and we can expect similar results.[1] Such evidence suggests that many individuals and households are at the very least likely to make poor financial decisions. In the particular case of retirement savings, empirical evidence indicates that many individuals either do not save, even though they recognize that they should, or they have a downward bias concerning how much they actually are saving or need to save to meet a stated post-retirement standard of living (Loewenstein et al., 1999; O'Donoghue and Rabin, 1999, Dominitz et al., 2002).

In addition to the literature on financial capability, there is a growing body of evidence that points to important *behavioural* elements in the ways that humans make financial decisions. In particular, this literature suggests the importance of *choice architecture* (i.e., the way choices are framed and presented) in affecting financial decisions in general and savings decisions in particular. In the absence of explicit recognition by individuals of prior errors in savings decisions, it may thus be possible to influence more sound financial decisions through the way that financial policies and decisions are structured.[2]

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1 For example, a Canadian financial capability survey (CFCS) was conducted by Statistics Canada in 2009; results are pending.

2 This idea has become known "liberal paternalism," as popularized by Thaler and Sunstein (2008). Their book lays out a behavioural case for attention to be paid in public policy to choice architecture, particularly in areas where individuals face complex, infrequent decisions in which there are few opportunities for learning by doing and in where the magnitude and the range of possible payoffs are high.

Below we review a few key behavioural effects as they relate to savings behaviour and decision making.

1.1 Behavioural Effects Relevant to Financial Literacy

1.1.1 Loss Aversion, Endowment Effects, and the Status Quo Bias

In neoclassical (traditional) economic theory, individual rationality is manifest through consistent choice and this is meant to apply equally to risky as well as riskless decision environments. However, a large body of behavioural research has shown that in fact individuals consistently deviate from neoclassical expected utility theory in their evaluation of gains versus losses. Seminal behavioural research by Knetch (1989), Tversky and Kahneman (1991), and Kahneman, Knetch and Thaler (1991) demonstrates that the revealed preferences of individuals exhibit systematic loss aversion; i.e., they give prospective losses more weight than prospective gains. To illustrate the concept of loss aversion, consider an individual who is offered a choice between two bets: bet #1 involves a 60 per cent chance of winning $100 and 40 per cent chance of getting zero, while bet #2 offers $50 with certainty. Note that the expected value of bet #1 is $60 (that is, .6 ($100) + .4 (0)), while the expected value of bet #2 is $50. Suppose the individual chooses (reveals a preference for) bet #1. Now, the same individual is offered a choice between bet #2 ($50 with certainty) and a new bet; bet #3 which offers a 40 per cent chance of losing $50 and a 60 per cent chance of winning $134. Bet #3 has also has expected value of around $60 (.4 (-$50) + .6 ($134)), so we might expect the individual to choose bet #3 over bet #2. However, results in the experimental economics literature suggest that in fact the individual is likely to choose bet #2 over bet #3. The concept of loss aversion explains this behaviour by positing that in the case of bet #3, the individual discounts the potential $134 gain and places more weight on the potential loss of $50.[3]

Loss aversion has an important application to savings decisions and savings behaviour because a decision to save is a decision to incur a loss of current consumption in return for

3 For other examples of loss aversion, see for example Kachelmeier and Shehata (1992).

future consumption. Loss aversion thus implies that individuals will have a built-in bias against saving since it involves a current loss which is weighed more heavily than future consumption.[4] This has led Thaler and Benartzi (2004), and later Thaler and Sunstein (2008), to propose a save-more-tommorrow savings plan, in which savings decisions are timed to coincide with increases in money income. The idea of save-more-later is to exploit a natural tendency driven by loss aversion by asking people to commit to save out of increases in income that have yet to be realized.

At its core, loss aversion suggests that individuals do not measure changes to consumption solely in terms of absolute levels, but also in terms of changes from a status quo. As such, the aforementioned biases are oftentimes referred to as evidence of a status quo bias (Samuelson and Zeckhauser, 1988). Status quo bias is related to loss aversion in that individuals can display bias against any deviation from current behaviour. In some ways, this behavioural tendency overlaps with procrastination. However, there are subtle differences. In the case of status quo bias, the tendency to continue with current choices has a connection to the concepts of bounded rationality and satisficing behaviour. Assuming that individuals are intentionally rational but limited in their ability to be so (lack of time, information, computational ability) to deal with complexity, individuals:

a) Form rules of thumb that allow for timely decisions. and
b) Exhibit satisficing behaviour – a willingness to continue to follow a rule of thumb if the outcomes are deemed acceptable.

This form of behaviour is a departure from the sort of (fully) rational maximization that occurs in neoclassical models of decision making. Individuals who are satisficing are not concerned directly with the possibility that they are maximizing their utility. Rather, if they

4 Related to this are also behavioural notions of myopia and procrastination. In the latter case, making no decision is a form of default bias, where the default is to refrain from a decision which may in itself have several options. If the decision involves weighing easily quantifiable up-front costs against difficult-to-measure future benefits that have some degree of risk and uncertainty, then there may be a bias towards postponing the decision. Even if an individual recognizes that a decision needs to be made, there is a tendency to incrementalize the timing of the decision: ("I don't want to decide today – I'll do it tomorrow.") This can be thought of as another form of myopia, where the focus is on costs rather than benefits and the myopia is a bias towards avoiding costs today.

are satisfied with current outcomes, they are less likely to experiment or to change current behaviour to see if they can do better.

Status quo bias and loss aversion are also related to the *endowment effect,* in which individuals demand more to give up something over which they hold property rights (their "reference state") than they would be willing to pay for the same item.

1.1.2 Default Bias and Status Quo Bias

Default bias is related to status quo bias but draws attention to the fact that institutions and choice architecture without intent can bias decision making. The basic result here is that if opting in is the default, there is relatively little attrition when there is a free choice to opt out. However, when opting out is the default, there is relatively little uptake. Beshears et al. (2007) provide evidence from US retirement savings plans. They find that automatic opt-in defaults in retirement savings plans encourage participation, and further, that automatic opt-in defaults with higher contribution rates result in actual contribution rates that are significantly higher than those plans with lower default contribution rates. The study indicates that this appears to work well on existing as well as new employees and that defaults can also influence asset allocations – that is, over half of employees studied who were participating in the automatic enrolment plan not only retained the default contribution rate but also kept all their funds in the default fund.

1.1.3 Labelling Bias

Several researchers have argued that the way in which choices are labelled can have a significant effect on the choices made. For example, Koorman et al. (2009) study the effects of labelling applied to income in the Netherlands, and how it motivates savings behaviour. Some firms in the Netherlands offer employees the chance to participate in a life course savings (LCS) scheme, in which the firm makes a monetary contribution on behalf of each employee. Money contributed under the scheme is tax-exempt, and employees can save and defer taxes. Under Dutch law, if employers make the LCS scheme available, they must make contributions to all employees, but employees cannot be forced to participate. Employees who choose not to participate simply receive the LCS contribution as current

income. Thus although it is labelled as a LCS contribution to employees, the employer's monetary contribution under the scheme is technically just a component of wages.[5] Koorman finds that when these firm-provided benefits are labelled as earmarked for the LCS scheme, participation is significantly higher – that is, employees do not tend to view the contribution as current income. Similarly, a study of the Dutch child benefit by Koorman (2000) shows that parents are much more likely to purchase child clothing out of child benefit (labelled) income than other income sources. Mak, Chen and Obson (2006) show that when a windfall income gain (through a tax rebate) is labelled "withheld income" respondents indicate that they will spend about 25 per cent of it. If however the windfall gain is referred to as "bonus income" respondents indicate they will spend about 87 per cent of it.

It is interesting to consider how such labelling effects might be tested in an experimental (laboratory) setting. Given that subjects either receive income (as an endowment) or earn income as part of a decision experiment, a question arises as to whether they will regard such income as a windfall gain or as part of their regular income. Additionally, one could construct experiments to test whether placing labels on either endowments or earned laboratory dollars can influence savings behaviour. The experimental design outlined in section 3 can be adapted to investigate labelling effects, and we consider this in more detail in section 5.

1.1.4 Framing and Anchoring Biases

In a study of supplementary savings decisions by individuals already participating in an employer-sponsored pension plan, Card and Ransom (2006) show that supplementary saving by employees changed depending upon how the mandatory component of the plan was presented. Specifically, under the plan, a $1 increase in employer contributions should have the same effect on supplemental contributions as a $1 increase in the mandatory employee contribution. However, their results show that when the employer contribution increases, employee contributions fall by 30 cents on the dollar, but that supplemental

5 See Koorman (2009), p. 6, for a more comprehensive description of the LCS scheme.

contributions fall by 70 cents when mandatory employee contributions increase. This suggests that employee assessments of the choice being made in each case are framed or anchored differently.

More generally, framing and anchoring effects have been studied by several researchers, including Ariely (2008), who demonstrates in several environments how irrelevant alternatives can bias decision making. Consider an individual who is presented with a choice between three alternatives (A, B and C), where alternative A is different from option C and similar but unambiguously superior to option B. Ariely shows that individual choices will reveal a bias towards the alternative A.

1.1.5 The Relevance of Behavioural Biases in Savings Behaviour our Program of Study

All the behavioural effects and evidence mentioned in the previous section suggest a potentially rich area for experimental research into savings behaviour. Much of the existing research on behavioural aspects of savings is based on surveys or context-specific institutional studies. Therefore, experimental methods can contribute to this literature by providing a controlled decision environment that distinguishes one behavioural effect from another or assesses the contemporaneous effect of two or more behavioural aspects of saving decisions — that is, an environment putting controls in place to rule out other confounding effects.

2. Loss Aversion in Laboratory Experiments

Endowment effects and loss aversion present specific challenges to experimental tests of savings decisions. Specifically, a laboratory experiment must be designed in which a decision to save represents a current loss in consumption and not an allocation out of income perceived as a windfall gain or gift.[6] That is, in order to experimentally test savings decisions we must be able to create an environment in which dollars earned in the

6 Note that here we are talking about subject perceptions as distinct from a specific treatment in which income may be intentionally labelled as a windfall gain.

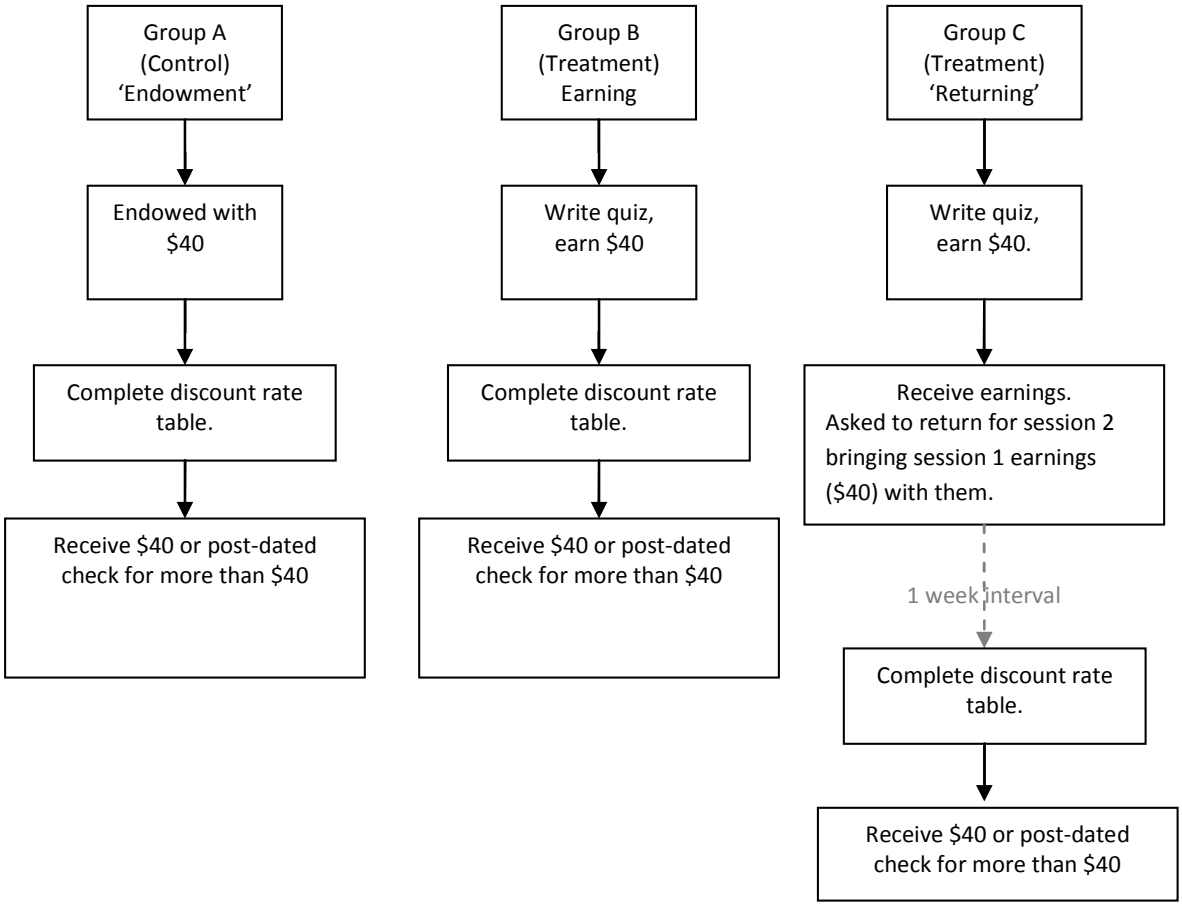
laboratory are perceived by subjects as their own income to which they have entitlement. If this can be done, then loss of this laboratory income will represent a current loss.

Accordingly, before designing a series of experimental studies to test behavioural aspects of savings decisions, we first tried to develop an experimental design to identify behaviour consistent with loss aversion (the tendency for individuals to prefer the avoidance of losses in consumption or wealth over the acquisition of similar gains) in an intertemporal choice setting.

2.1 Experimental Design

The overall design of our experiment is illustrated below in Figure 2-1. In this experiment, we wanted to see how earning or retaining dollars earned in the laboratory (rather than just being given money) would affect intertemporal discount rates elicited from our participants. Intertemporal discount rates reflect the additional amount of money individuals require to compensate them for receiving money later rather sooner. For example, if an individual rejects an offer of $42 in three weeks in favour of $40 today, this means that their discount rate on money in three weeks is equal to or greater than 5 per cent. If the same individual accepts an offer of $44 dollars in three weeks over $40 today, then their implied discount rate on money in three weeks lies between 5 per cent and 10 per cent.

Figure 2-1: Basic Protocol Experimental Design



We elicited discount rates from participants using a method found in many studies of intertemporal decision making (Coller and Williams, 1999; Harrison et al., 2002; McLeish and Oxoby, 2007). This procedure required participants to complete Table 2-1 (see below), indicating whether they preferred option A (a constant amount of money to be received after the experimental session) or option B (a larger amount of money to be received two weeks later), in eleven different payoff alternatives.

Table 2-1: Table Used to Elicit Discount Rates from Participants

Payoff Alternative	Option A (receivable today)	Option B (dated in two weeks)	Preferred (circle A	Option or B)
1	$40	$40	A	B
2	$40	$41	A	B
3	$40	$42	A	B
4	$40	$43	A	B
5	$40	$44	A	B
6	$40	$45	A	B
7	$40	$46	A	B
8	$40	$47	A	B
9	$40	$48	A	B
10	$40	$49	A	B
11	$40	$50	A	B
12	$40	$51	A	B

This table presents later options increasing by 2.5 per cent, representing two-week discount rates ranging from 0 per cent to 27.5 per cent. The point in the table where an individual ceases choosing option A and begins choosing option B (their cross-over point) proxies the individual's two-week discount rate (outcome variable: A-rate). Individuals were paid for their participation based on their answers provided in Table 2-1. At the end of the experiment, one of the twelve questions a participant answered was randomly selected and the individual was paid according to their response to this question. As such, each individual's payment for participation was primarily based on their discount rates, thereby providing an incentive for correct answers according to true preferences. (Instructions for how this portion of the experiment was implemented are available in the appendix.)

For example, question 6 of Table 2-1 might have been randomly chosen to determine payment for the individual's participation. If the response to this question indicated that she preferred to be paid $40 immediately, she received a cheque for $40, dated the day of the experiment. If her response to this question indicated she preferred $45 in two weeks, she received a cheque on the day of the session for $45 but post-dated for two weeks from the date of that session. This procedure was followed for all the treatments described below. In addition to this payment for participation, individuals were also paid a $5 cash show-up fee for attending the session.[7]

It is worth commenting on our design of Table 2-1. Note that we included an option returning 0 per cent as a means of checking individuals' understanding of the problem: any individuals who value receiving money sooner rather than later should prefer option A when both options return $40. Moreover, we chose significantly large discount rates (i.e., larger than those paid by financial institutions) to compensate individuals for any transaction costs (real or imagined) regarding their access to the money. Previous studies (e.g., Coller and Williams, 1999; Harrison et al., 2002) utilized interests rates that more closely mirrored those paid by banks. However, these studies also used longer time horizons (e.g., choices between amounts to be received in three or six months). While we used larger interest rates, recall that our primary interest is in the effect of our treatments on the elicited discount rates, not the value of the interest rates per se.

Further, in our experiments we did not use a front-end delay as recommended by Harrison et al. (2005) and used by Coller and Williams (1999) and McLeish and Oxoby (2007). When using a front-end delay in eliciting discount rates, there is no sum of money available immediately to participants. For example, incorporating a front-end delay in Table 2-1 would have the option A available in, say, one week and option B available in three weeks. Harrison et al. (2005) argue that a front-end delay is necessary to eliminate (or reduce) the

7 The show-up fee was used as an inducement to encourage individuals to attend. We typically over-recruit for each experiment to account for individuals who do not show up. The show-up fee was also paid to all individuals who arrive to participate in the experiment, regardless of whether or not they participate

immediacy of potential consumption and the perceived differences in transaction costs between the two options. We tried to control for perceived transaction cost differences by paying individuals by cheque for their responses to a randomly selected payoff alternative from Table 2-1. With respect to the immediacy of consumption, we specifically did not use a front-end delay because time constraints limited the amount of time we wanted to give to the project. In addition, we wanted to exploit this immediacy to evoke loss aversion in the lab similar to that experienced by individuals in real life when they decide to save (i.e., to defer immediate consumption). Recall, we are not as much interested in the absolute value of discount rates as the effects we identify across the treatments in our experiment.

Finally, we used the same table in each of the treatments below. The only changes we made were the actual dates reflected in the Table 2-1 regarding the dating of the post-dated cheque. In each experiment, the future payment was two weeks from the date the session actually took place. As such, all participants across all treatments faced the same period of delay.

2.1.1 Experimental Treatments

We conducted three treatments in the experiment. In our *endowment* treatment, individuals were simply asked to complete Table 2-1, knowing that their payment would be based on their answer to a randomly selected payoff alternative. As such, participants were given $40 over which they made intertemporal choices. At the end of the session, participants received a $5 cash show-up fee and a cheque payable the day of the session for $40 or post-dated for a larger amount.

In our *earnings* treatment, participants were given a twenty question quiz consisting of pre-selected questions from the Graduate Record Exam (GRE).[8] Participants were told that they could earn either $40 or $20, based on their exam performance: If they answered at least ten questions correctly they would earn $40; for less than ten questions correctly

8 The GRE is a commercially-run standardized test used by many universities in the United States. It purports to measure verbal, quantitative, and critical reasoning.

answered they would receive $20.[9] This earned money was subsequently used when individuals completed Table 2-1. The central motivation in this treatment was to see: (*i*) if earning the money receivable after the session would create a sense of entitlement or legitimacy of these assets, as has been identified in other experiments on social preferences (Cherry et al., 2002; Oxoby and Spraggon, 2008); and (*ii*) if this sense of entitlement or asset legitimacy affected elicited discount rates. Our hypothesis was that individuals would feel they had earned the $40 being used as the default option in Table 2-1.[10] As in the endowment treatment, individuals received a $5 cash show-up fee and a cheque dated for a chosen amount based on a randomly selected alternative they had completed in Table 2-1.

In our final *returning* treatment, individuals earned their money (in the same manner as the earnings treatment) but were paid these amounts in cash at the end of the session. Participants were then told they had an opportunity to return to acquire additional funds in a session the following week but that they had to bring the amount they had earned in the current session to the subsequent session. In the subsequent session, participants were asked to put the money they had previously earned into an envelope labelled as their property. Participants then completed Table 2-1 and received a $5 show-up fee and a cheque for either $40 or post-dated cheque for a larger amount (based on a randomly selected alternative from Table 1). Our hypothesis in this session was that both the earning behaviour (i.e., the twenty question exam) and the fact that individuals had the money in their possession for a week would create a stronger sense of entitlement which would affect their intertemporal decision making. Note that in this treatment, individuals only received a $5 show-up fee for participation in the final session.

At the end of each session in which participants completed Table 2-1 they also completed a short questionnaire regarding demographic information. This questionnaire included two

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9 This threshold was chosen based on previous experiments in order to ensure that most participants would earn $40 while still requiring that they exert significant effort. All participants did sufficiently well on the selected questions to reach the $40 threshold.

10 Similar methods to create a sense of "earnings" or "entitlement" have been effectively used by Cherry et al (2002) and Oxoby and Spraggon (2008).

questions regarding their attitudes towards the money used in the experiment. That is, participants answered the following questions on a Likert-type scale ranging from 1 – 7 where 1 = "strongly disagree" and 7 = "strongly agree":

1. Outcome variable: *asset legitimacy 1*(AS1): I am entitled to the money I received for participating in the experiment.
2. Outcome variable: *asset legitimacy 2* (AS2): I earned the money I am receiving for participating in the experiment.

   In addition, participants in the returning treatment were also asked the following yes/no questions:
3. Is the cash you brought to today's experiment the same bills you were given in the previous session?
4. If not, did you spend the cash you were given in the previous session?

Our hypotheses for this experiment revolve around the idea of loss aversion (Kahneman et al., 1991). We hypothesize that if individuals are resistant to putting off current consumption (i.e., treating a potential reduction in current consumption as a "loss") they should require greater compensation for deferring consumption. This greater compensation would manifest itself in Table 2-1 via the choice of higher crossing points e.g. (choosing option A for higher payoff alternatives effectively requiring a higher interest rate to put off current consumption). We further hypothesize that increasing the sense of asset legitimacy regarding money used in the experiment should increase an individual's sense of entitlement and therefore increase the presence of loss aversion with respect to deferring current consumption.

With respect to a participant's sense of entitlement, we have the following hypothesis:

Hypothesis 1 *Participants' responses AS1 and AS2 will be higher in (*i*) the Earnings treatment relative to the Endowment treatment, (*i*) the Returning treatment relative to the Endowment treatment, and (*i*) the Returning treatment relative to the Earnings treatment.* Subsequently, this should affect the degree of loss aversion experienced by participants in each of the treatments:

Hypothesis 2 *Participants will experience a greater sense of loss aversion in the Earnings and Returning treatments relative to the Endowment treatment. This should be manifest in higher values of* A-rate *(i.e., higher cross-over points) for participants in the Earnings and Returning treatments relative to the Endowment treatment.*

The experiment sessions were conducted in a computer laboratory using the software developed by Fischbacher (2007). We utilized the software developed by Greiner (2004) to recruit participants. The use of this recruitment software ensured that individuals were only able to participate in a single treatment.

2.2 Results

A total of 75 individuals participated in the study, with 24 and 26 individuals participating in the endowment and earnings treatments. A total of 34 individuals participated in the first session of the returning treatment. However, because of attrition, only 25 individuals participated in the second session in which discount rates were elicited.[11] In terms of demographic information regarding participants, across all treatments participants were evenly split across gender (58 per cent male), and ages ranged between 19 and 23.[12] A copy of the questions asked in the experiment is included in the appendix.

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11 The high attrition rate is likely due to the timing of the experiment, just prior to the administration of final exams. Note that we found no systematic differences between those who returned and those who did not in terms of gender, age, educational status.

12 In an analysis of the demographic information collected, we found no differences across the responses of men and women (cf. McLeish and Oxoby, 2007). This is likely due to the larger discount rates in the

Table 2-2 provides the summary statistics from the sessions across the three treatments. The variable A-rate in the table represents the average discount rate at which participants start preferring more than $40 in three weeks time over $40 immediately. We find no statistically significant differences in participants' A-rates between the endowment and earnings treatments.[13] That is, individuals who were endowed with forty dollars did not exhibit discount rates that were significantly different from those who earned $40 by completing the GRE quiz. However, there is a large statistically significant difference between the average A-rates for participants in the returning treatment and the other two treatments.[14] Specifically the discount rate for participants in the returning treatment is much higher at 20.45 per cent, indicating that these individuals require far greater compensation in order for them to forgo receiving their money immediately.

Table 2-2 Summary Statistics: Mean (Standard Deviation)

Variable	**Endowment Treatment**	**Earnings Treatment**	**Returning Treatment**
Mean A-rate	9.13%	12.33%	20.45%
(Std Deviation)	(2.93%)	(3.88%)	(4.35%)
Mean AS1 score	4.96	5.00	5.09
(Std Deviation)	(1.08)	(0.89)	(0.81)
Mean AS2 score	4.125	6.11	6.09
(Std Deviation)	(0.94)	(0.59)	(0.89)

With respect to Hypothesis 1, we find no differences in participants' responses regarding whether or not they were entitled to the money they received (outcome variable AS1). However, we find that participants in the earnings and returning treatments felt they earned the money received more than those in the endowment treatment.[15]

current study, which may have precluded the identification of smaller demographic effects on discounting behaviour.

13 Wilcoxon p=0.623.

14 Statistically we can reject the hypothesis that the *A-rate* responses in the returning treatment are drawn from the same distribution as those in the endowment and earnings treatments (Wilcoxon p<0.01 in each comparison).

15 Wilcoxon p<0.05 for variable AS2 from question 2.

In regards to Hypothesis 2, we identified no differences between the earnings and returning treatments. Thus we only find partial support for Hypothesis 2 with respect to *AS2*, but reject the hypothesis with respect to the variable *AS1*.

With respect to questions 3 and 4 answered by participants in the returning treatment, all participants indicated that the cash brought to the second session was not the same as that given in the previous session and 88 per cent of participants indicated that they had spent the money given to them in the first session. This provides some explanation for the higher discount rates elicited in this treatment. Spending the money suggests that participants considered the money truly to be theirs (as opposed to belonging to the experimenter). As such, deferring use of the money by two weeks would require forgoing current consumption or incurring a loss as suggested by the research of Kahneman et al. (1991). On the other hand, participants in the endowment and earnings treatment may have viewed the money they received as a gain. Since incurring a loss is more costly than the change in well-being initiated by a similarly sized gain, participants in the returning treatment (who could experience a $40 loss to current consumption) required greater compensation for deferring consumption relative to those in the endowment and earnings treatments (where $40 gains were experienced). This greater compensation was manifested through higher elicited discount rates (i.e., requiring a higher rate of interest to wait two weeks for payment).

It is interesting to note that relative to other studies (e.g., Coller and Williams, 1999; McLeish and Oxoby, 2007), we find no differences in the elicited discount rates (i.e., variable A-rate) between men and women. This may be due to the relatively small sample size and the large interest rates employed in Table 2-1 (cf. Coller and Williams, 1999).

3. Using the Loss Aversion Protocol to Implement Six Experiments Regarding Behavioural Aspects of Savings Decisions

The results of the experiment in section 2 indicate that we can indeed generate entitlement to income in the laboratory through students earning and retaining laboratory earned dollars and that such entitlement does produce a strong behavioural effect that is consistent with loss aversion. Having established this important result, the remainder of the report is dedicated to describing the design and implementation of six experiments to test various behavioural aspects of savings decisions.

Between January and May of 2010, we designed and conducted a series of experiments in six modules at the University of Calgary. The participants in our experiments were University of Calgary students; 326 individuals (221 men = 68 per cent) participated in all experimental sessions.[16] The core design of these experiments utilized the protocol developed in section 2, in which participants were given an initial opportunity (session 1) to earn up to $40 by completing a quiz. Our design then required them to make decisions one week later (session 2) and that they bring back and use the amount of money they earned in session 1 when making their session 2 decisions. The money brought to session 2 was set aside in envelopes, each marked with a number identifying the participant. In this way, individuals were sure that they could leave with the money they had brought into the session, and the money was not directly in their possession during the session.

The session 1 quiz was designed to make it very easy to earn the maximum amount ($40), and in all six experiments; all participants were able to earn $40 in session 1. As a benchmark, we elicited hypothetical discount rates from every participant in session 1 of each experiment. Further, as an additional incentive for individuals to participate in the second session of each experiment, we provided participants with a $5 show-up fee paid only in the second session. This amount was paid as either a pure show-up fee (i.e.,

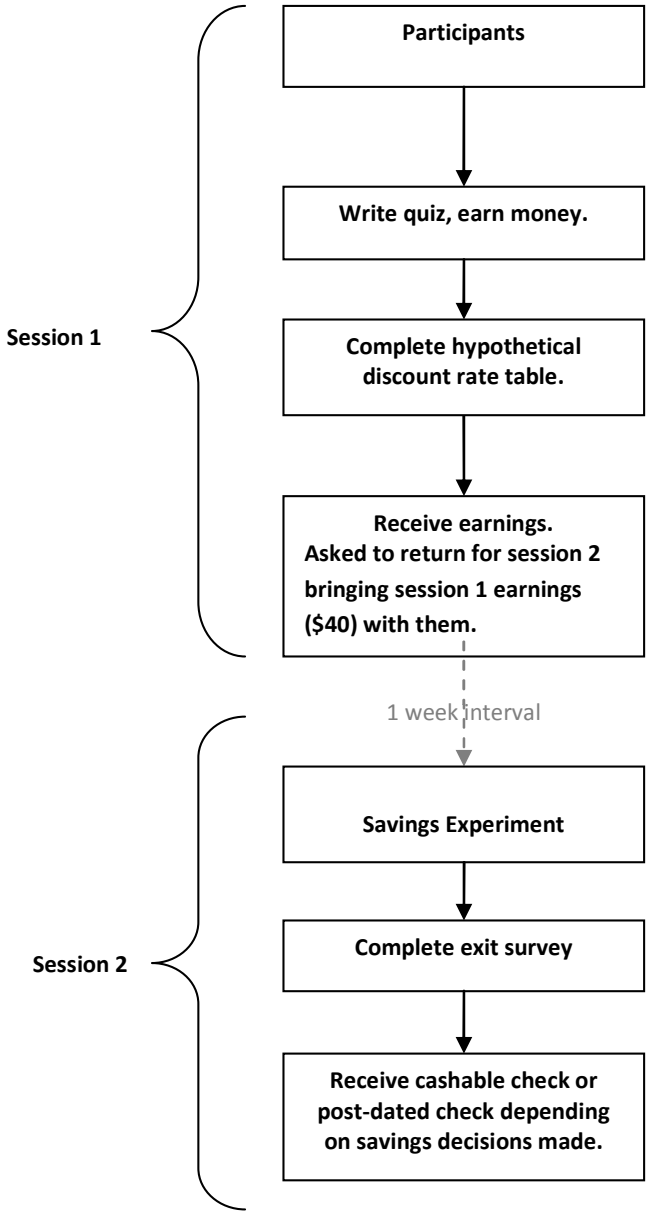
16 Attrition was very low between the initial (earning) sessions and the returning sessions for all the savings experiments; individuals who attended only one session are not included in our total number of 326.

contingent only on their appearance in the second session) or was paid as remuneration for a simple distracting task (a series of short elementary mathematics or logic questions) and was not contingent on intertemporal decision-making in the second session.

All of our savings experiments used the protocol developed in section 2 in order to allow for loss aversion among participants when testing various aspects of savings decisions. That is, in addition to the treatments specific to each of the experiments below, all individuals participating in the experiments below were required to attend an initial session in which they earned money by completing a quiz and were allowed to leave the session with those funds. In the following week, the participants returned with these funds to participate in the savings experiments described in sections 4 to 9 below. The general structure of our six savings experiments is illustrated below in Figure 3-1.

In order to provide some additional degree of control given our use of this loss aversion protocol, we elicited hypothetical discount rates from individuals in each experiment's initial session (i.e., the first session in which they earned their money). To elicit these hypothetical discount rates, we used a table in which participants considered whether they would prefer a hypothetical payment of $100 in two weeks or more than $100 in five weeks. As before, the point at which individuals ceased choosing $100 in two weeks and began choosing B proxy more than $100 in five weeks proxied the individuals' discount rates.

Figure 3-1: General Structure of Our Savings Experiments



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It is important to note that this discount elicitation question does not embody any of the components associated with loss aversion. There are no real dollars at stake. As such, the responses elicited in Table 4-1 provide us with an additional control by which we can compare intertemporal preferences across all the modules discussed below. Given the use

of our protocol, this information allows us to see if the differences in behaviour observed in each saving experiment are a result of the treatment variables employed. Importantly, using responses from the hypothetical discount rate elicitation (see Table 4-1) *we do not find any differences in intertemporal preferences across all the participants in the six experiments reported below*.[17] Moreover, we find no differences in responses to this table across genders.[18]

All of our savings experiments were programmed using the software developed by Fischbacher (2007) and conducted in a computer lab specifically set-up for economic experiments. We also utilized recruitment software developed by Greiner (2004), thereby ensuring that participants could be involved in only one of the modules below. As a result, each of the observations in each of the modules described below represents a single individual (i.e., no individual participated in more than one module).

In the sections that follow, we describe in detail the design, implementation and results of each experimental module. Module 1 is an experiment to test whether we observe a default bias in savings decisions — that is, whether individuals are more prone to save if saving is defined as the default option. Module 2 is a laboratory test of the Thaler/Benartzi/ Sunstein save-more-later idea. It tests whether individuals are more willing to save out of future increases in income.[19] Our module 3 experiment investigates whether we observe a labelling bias in savings decisions – that is, whether the way that money is labelled matters when individuals make savings decisions. In module 4 we test the role of beliefs and peer effects in savings decisions, and investigate whether beliefs or feedback concerning the actions of others affect individual decisions to save or not to save. In module 5 we examine the role played by relative anchoring in savings decisions, and ask whether choice of more than one savings alternative leads to more saving when one alternative is clearly superior

17 Krusal-Wallis p=0.329 for the equality of distributions of discount rates across the six modules.

18 Wilcoxon p=0.245 for the equality of distributions of discount rates across genders.

19 See Thaler and Benartzi (2004) Thaler and Sunstein (2008).

to another. In module 6, we augment our module 1 experiment to assess the effects that ex-ante provision of financial knowledge/information has on savings decisions.

4. Default Bias (Module 1)

This experiment tested the extent to which default bias is significant in the context of a savings decision. The basic experimental design utilized two sessions spread one week apart. In the first session, participants earned money and were asked to complete a hypothetical table to elicit their discount rates over income today versus income in five weeks. In the second session, individuals in the control group were told that they were set to receive the income they earned in session 1 and were then asked if they wished to save the money. Individuals in the treatment group were told the income they earned in session 1 had been placed in a savings plan; they were then asked if they wished to withdraw it. The overall structure of the experiment is illustrated in Figure 4-1, below.

4.1 Module 1 – Session 1

In the first session, participants earned income based on completion of a 20-question quiz using pre-selected questions from the Graduate Records Exam (GRE). The participants were given 20 minutes to complete the multiple-choice quiz. The quiz questions were provided in hard-copy format and participants entered answers on-screen. Each individual's earnings from this quiz were based on her score in accord with the following compensation scheme: individuals answering fewer than 10 questions correctly received $20. Individuals answering at least 10 questions correctly received $40. All those who participated in session 1 answered at least 10 questions correctly and therefore received $40.

We then elicited discount rates using Table 4-1, below. Individuals were instructed to indicate (on the computer) which option they preferred (A or B) by clicking in the appropriate box on the screen. Individuals were not paid for their responses, but rather were simply asked which they would prefer.

Participants were then paid and instructed to return the following week to complete the experiment. As part of the returning session, participants were asked to bring their earnings from session 1 to the subsequent session.

Table 4-1: Table for Eliciting Hypothetical Discount Rates

Alternative	Payment A received in two weeks	Payment B received in 5 weeks
1	100	102.5
2	100	105
3	100	107.5
4	100	110
5	100	112.5
6	100	115
7	100	117.5
8	100	120
9	100	122.5
10	100	125
11	100	127.5
12	100	130

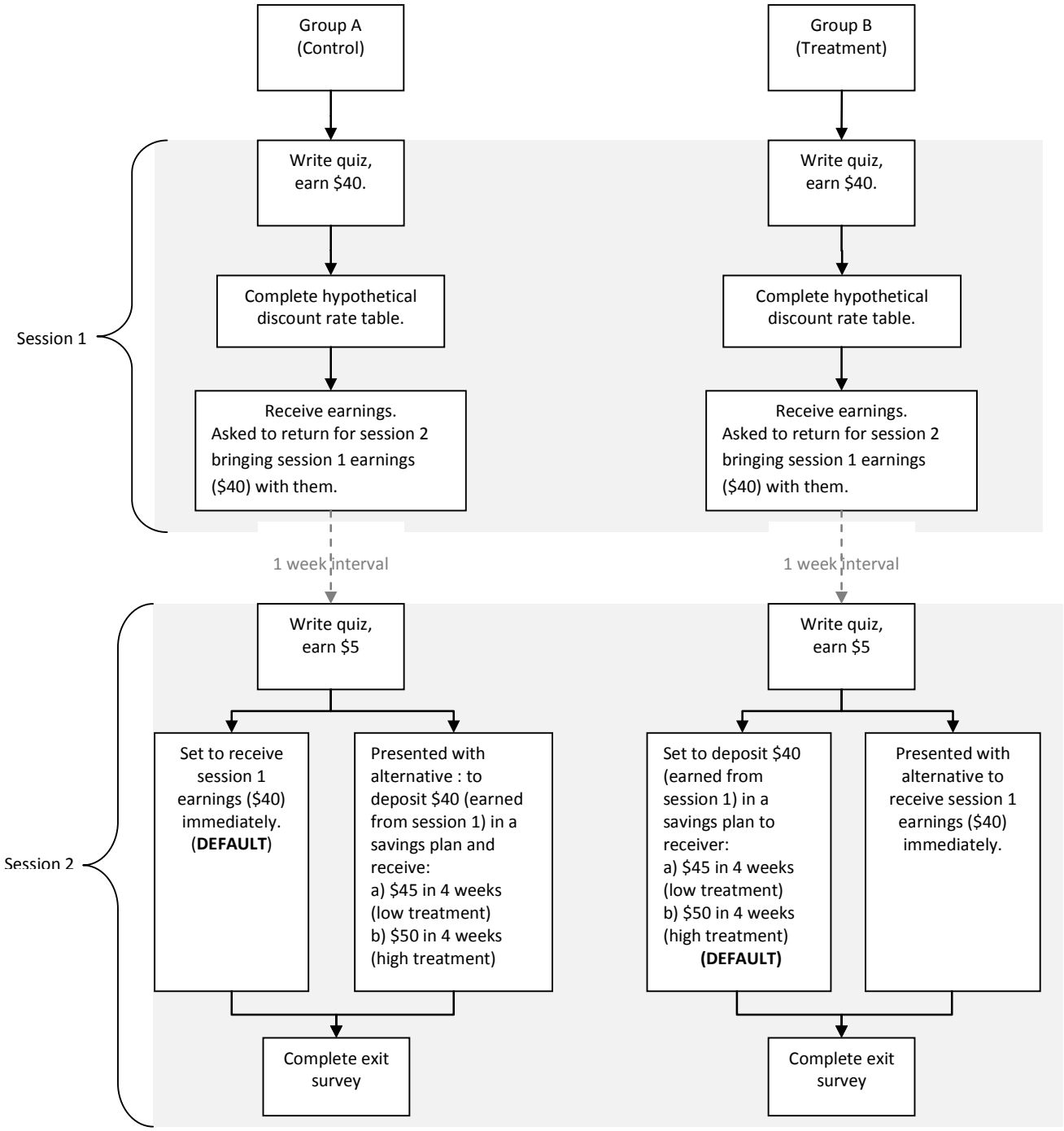
4.2 Module 1 – Session 2

In the returning session (session 2), the money earned in session 1 ($40) was put in individual-specific envelopes labelled with the participant's number (i.e., the number of their computer station). Individuals then completed a 10-minute, 10-question quiz for which they were paid $5 for getting at least 2 questions correct. All subjects did earn $5 from the completion of this quiz.

After completing the quiz participants were assigned to one of the following treatments (default conditions) and asked to indicate their preference on the computer screen.

1. (No default, high) Currently the $40 you brought with you today will be returned to you. However, you can choose to save this money in accord with the following: rather than taking the $40 with you today, you can choose to save it and receive $50 in four weeks. (The $50 payment represents your $40 plus a 25 per cent interest rate payment of $10).

Figure 4-1: Illustration of the Experimental Design for Module 1



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2. (No default, low) Currently the $40 you brought with you today will be returned to you. However, you can choose to save this money in accord with the following: rather than taking the $40 with you today, you can choose to save it and receive $45 in four weeks. (The $45 payment represents your $40 plus a 12.5 per cent interest rate payment of $5).
3. (Default, high) Currently the $40 you brought with you today has been assigned to a savings plan. Under the savings plan, you will receive $50 in four weeks. (The $50 payment represents your $40 plus a 25 per cent interest rate payment of $10). However if you wish, you can choose to take your $40 with you today. That is, rather than receiving $50 in four weeks you can receive $40 today.
4. (Default, low) Currently the $40 you brought with you today has been assigned to a savings plan. Under the savings plan, you will receive $45 in four weeks. (The $45 payment represents your $40 plus a 12.5 per cent interest rate payment of $5). However if you wish, you can choose to take your $40 with you today. That is, rather than receiving $45 in four weeks you can receive $40 today.

After making this choice, individuals answered a short questionnaire (see Appendix 1). Individuals then either left the session with their $40 or a note indicating they would receive either $50 or $45 (depending on their treatment condition). In addition, each participant left with $5 for completing the session 2 quiz. Finally, individuals were given an email address they could write to in the event they changed their mind regarding their choice from the above condition.

4.3 Module 1 Results

A total of 52 individuals (32 men and 20 women) participated in the experiment, with 13 individuals allocated to each one of the above treatments. (Four individuals participated in the first session but not in the second session, resulting in an attrition rate of 7 per cent.) In terms of outcome variables, our primary interests are in the discount rate elicited using and the saving decisions made in the aforementioned default conditions.

With respect to the initial (hypothetical) discount rates elicited in the first session using participants had an average three week discount rate of 13.8 per cent (σ=6.7%). More germane to our interest, we found very few individuals chose to save their money across all the above conditions: In both the high and low No default conditions, all individuals chose to take $40 with them at the end of the second session (i.e., to forgo savings). This is particularly striking in the High condition, where all but two individuals indicated a discount rate from Table 1 of less than 25 per cent. As such, these individuals should have preferred the option of savings at the high rate with respect to their previously earned $40. This suggests that having held onto their money for the week between session 1 and 2 resulted in different character of behaviour with respect to their earned $40 relative to the hypothetical decisions elicited using Table 4-1.

With respect to participants in the Low and High default treatments, only 3 and 2 of 13 participants chose to stay with the default option (i.e., to save their money). While statistically, we can reject the hypothesis that the savings decisions from the default conditions and the no default conditions are drawn from the same distribution (Wilcoxon $p<0.02$), the evidence of a default bias is weak in our data.[20] In pairwise comparisons, we find no difference between the High and Low default conditions ($p=0.94$), suggesting that the interest rate had no effect on individuals' decisions to remain with the default savings allocation. Interestingly, we fail to identify any correlation between the discount rate elicited from Table 4-1 and the decision to remain with the default option. Finally, we find no differences in elicited discount rates between genders or across demographic characteristics. (See Appendix D: Demographic Information on Participants.)

The fact that we observe no effect of defaults when participants are in our loss aversion protocol may have some important implications for our understanding of the savings decisions of lower income individuals. For these individuals, we typically observe low rates

20 That is, we can reject the hypothesis that the behaviour is statistically the same for individuals in two conditions. As such, we are able to deduce that the two experimental treatments resulted in different patterns of behaviour.

of savings beyond those explained purely by their levels of income (see, e.g., Eckel et al., 2005). The current financial state of these individuals may be such that their present consumption needs (or wants) are particularly salient in their minds, thereby outweighing their future consumption needs (or wants). As such, these individuals will be less likely to save and, as suggested by our results, may be less prone to default biases in which the default option is towards deferring current consumption.

4.4 Module 1 Result Summary

Within the context of our loss aversion protocol (section 2) we find no evidence of a default bias. While the literature has discussed the default bias as arising in unfamiliar or non-routine decision environments, our results suggest that the default bias identified in other studies is largely an artefact of decision making over wealth or resources that have not yet been received by an individual. As such, from the standpoint of policy design, this suggests that default biases will work best when individuals do not yet have a stake in the resources in question (i.e., money in hand or a perception that the money is already theirs).

5. Save More Tomorrow (Module 2)

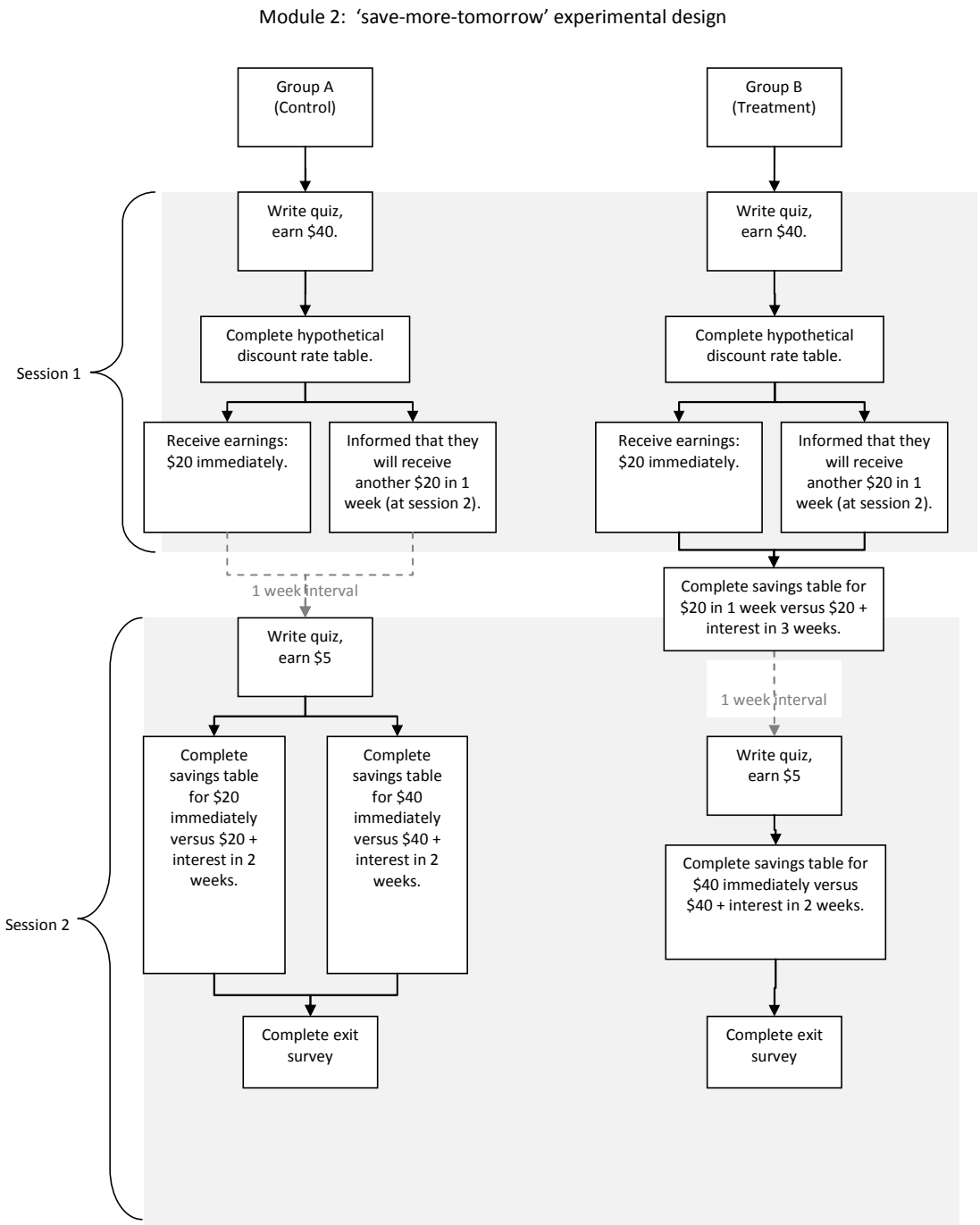
This experiment was designed to test whether a save-more-later scheme significantly affects savings behaviour in the lab. A central thesis proposed by Thaler and Benartzi (2004) and Thaler and Sunstein (2008) is that loss aversion will deter individuals from saving because it represents a loss of income in their present reference state. However loss aversion will be less of a deterrent to saving if individuals are asked to commit some portion of *future increases* in income to savings. Thus save-more-later operates to circumvent loss aversion in two ways; first, individuals are asked to commit future income which is not part of the current reference state and secondly, the amount saved comes from an increase in income which does not imply a loss relative to the current reference state. Since our overall experimental design implements loss aversion, we are in a position to be

able to test whether save-more-later works in the laboratory. Our experimental design for module 2 is summarized in Figure 5-1.

5.1 Module 2 – Session 1

In the first session, participants earned income based on a 20-question quiz using pre-selected questions from the Graduate Records Exam (GRE). The participants were given 20 minutes to complete the multiple-choice quiz. The quiz questions were provided in hard-copy format and participants entered answers on-screen. An individual's earnings from this quiz was based on her score in accord with the following compensation scheme: individuals answering at least five questions correctly received $20. Individuals answering at least 10 questions correctly received $40. All those who participated in session 1 answered at least 10 questions correctly and therefore received $40. Participants were then informed that they would receive a payment of $20 today and a further $20 in the following session one week later.

Figure 5-1: Illustration of the Experimental Design for Module 2 ("Save More Tomorrow")



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Participants were then asked to complete Table 4-1 (see module 1) in order to elicit their discount rates over a hypothetical savings decision.

5.1.1 Baseline Condition – Session 1

In our baseline condition, individuals left the session with their money and were asked to return the following week, bringing the $20 they earned in the first session with them.

5.1.2 Treatment Condition – Session 1

In our treatment condition, individuals in session 1 were presented with Table 5-1, in which they made a decision regarding the $20 they would receive the following week. Individuals were asked to indicate which alternative they preferred regarding these funds. They were told that one option from the table would be randomly selected and implemented (this ensured incentive compatibility).

Table 5-1: Save-More-Later Session 1 Treatment Group Discount Rate Elicitation

Payoff Alternative	Option A (receivable next week)	Option B (receivable in 3 weeks)	Preferred Option (circle A or B)	
1	20	20	A	B
2	20	20.5	A	B
3	20	21	A	B
4	20	21.50	A	B
5	20	22	A	B
6	20	22.50	A	B
7	20	23	A	B
8	20	23.5	A	B
9	20	24	A	B
10	20	24.50	A	B
11	20	25	A	B
12	20	25.5	A	B

5.2 Module 2 – Session 2

In the returning session individuals brought the $20 they had previously earned. This money was put in envelopes labelled with each participant's number (i.e., the number of their computer station). Individuals then completed a 10-minute, 10-question quiz for which they were paid $5 for getting at least two questions correct.[21]

5.2.1 Baseline Treatment – Session 2

In the baseline treatment, individuals were presented with Table 5-1 (above) and Table 5-2. They were informed that one of their choices from the two tables would be randomly selected and implemented.

Table 5-2: Session 2 Discount Rate Elicitation for $40

Payoff Alternative	Option A (receivable today)	Option B (receivable in 2 weeks)	Preferred Option (circle A or B)	
1	$40	$40	A	B
2	$40	$41	A	B
3	$40	$42	A	B
4	$40	$43	A	B
5	$40	$44	A	B
6	$40	$45	A	B
7	$40	$46	A	B
8	$40	$47	A	B
9	$40	$48	A	B
10	$40	$49	A	B
11	$40	$50	A	B
12	$40	$51	A	B

5.2.2 Treatment Condition – Session 2

In the treatment condition, individuals were presented with Table 5-2 and asked to make a decision regarding the total $40 they had received over the two sessions. They were told

21 All individuals were able to earn $5 by completing the session 2 quiz.

that with a 50 per cent probability their previous choice (regarding $20 completed in the previous session) would be implemented and with a 50 per cent probability one option from Table 5-2 would be randomly selected and implemented.

After making this choice, individuals answered a short questionnaire (see appendix A). Individuals then either left the session with their $40 or a note indicating they would receive the payoff they chose in one of the tables (i.e., treatment conditions). In addition, each participant left with $5 for completing the session 2 quiz. Finally, individuals were given an email address they could write to in the event they changed their mind regarding their choice from the above condition.

5.3 Module 2 Results

A total of 56 individuals participated in this experiment (29 men and 27 women), 18 in each of the treatments described above. (Five individuals participated in the first session but not in the second session, resulting in an attrition rate of 8 per cent.) Table 5-3 presents the summary statistics on the elicited discount rates of these individuals.

Table 5-3: Module 2 Summary Statistics: Mean Elicited Discount Rates; Standard Deviation Reported in Parenthesis

	Baseline	Treatment
Table 1 rate: $100 hypothetical	11.2% (4.3%)	9.6% (5.7%)
$20 to be received in one week	12.3% (4.8%)	11.0% (4.3%)
Session 2 rate: $20 table	21.0% (5.8%)	n/a
Session 2 rate: $40 table	23.6% (6.0%)	22.1% (4.9%)

To begin, we find no statistically significant differences in elicited discount rates across the treatments.[22] Further, within the baseline and treatment conditions, we find that discount rates elicited using each instrument (i.e., hypothetical and money to be received) in session 1 are consistent with one another and, in the baseline conditions, discount rates elicited

22 Wilcoxon p>0.4 in all pairwise comparisons.

using each instrument are consistent in session 2. However, we find strong differences between discount rates regarding money to be received in a week (i.e., elicited during session 1) and money to be received on the day of the session (i.e., elicited during session 2): Across both the treatment and the baseline conditions, we can reject the hypothesis that the session 1 discount rates are drawn from the same distribution as the session 2 rates.[23]

This result suggests two important behavioural phenomena. First, the results provide support for the save-more-later idea: individuals appear to display greater patience in making savings decisions over funds that will be received in the future than for funds which are readily available. This could be explained by the absence of loss aversion when the money is not readily available. As a result, individuals display greater patience as they do not perceive deferred consumption as a loss. Secondly, the results suggest an extreme form of hyperbolic or quasi-hyperbolic discounting in which, when money is available immediately and has been earned, individuals display significant impatience.[24]

As in the previous study, we find no differences in elicited discount rates between genders or across other demographic characteristics. (See Appendix D: Demographic Information on Participants).

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23 Wilcoxon p<0.03 in all pairwise comparisons

24 Using the simple model suggested by O'Donoghue and Rabin (1999), discounted utility can be expressed as $U(x_t) = u(x_0) + \beta(\sum \delta^t u(x_t))$ where x_t represents income received in period t, $\delta \in (0,1)$ represents the standard behavioural discount factor, and $\beta < 1$ represents a "present-day" bias wherein the immediate present is more heavily weighted, resulting in inconsistent discounting between the present and a future date and discounting over two periods in the future. Given that we observe a doubling of discount rates in the baseline treatment between sessions 1 and 2, we can estimate the present day bias to be on the order of 0.62.

5.4 Module 2 Results Summary

Our results in the current experiment demonstrate how individuals respond with respect to intertemporal decisions over resources (here, money) which they have not yet acquired. Our results (again within the strong loss aversion protocol developed in section 2) demonstrate that individuals are willing to save money at a much lower rate of interest when earned money is not yet in their possession. From a policy making standpoint, our results provide strong support for 'save-more-tomorrow' types of interventions, where individuals have an initial sense of ownership over money but do not yet have possession of these resources.

6. Labelling Bias (Module 3)

In this experiment, we followed the general structure outlined in module 2 to investigate whether labelling money in different ways affects savings decisions with respect to earned income in the laboratory. There are two prospective designs for this experiment, referred to below as A and B. Design A compares a change in wording from "earned income" to "bonus income," while design B effects a more drastic change by utilizing a lottery ticket mechanism as a means of distributing earned income. The structure of this experiment is illustrated below in Figure 6-1 (design A) and in Figure 6-2 (design B).

6.1 Module 3 – Session 1(A)

In the first session, participants earned resources based on completion of a 20-question quiz using pre-selected questions from the Graduate Records Exam (GRE). The participants were given 20 minutes to complete the multiple-choice quiz. The quiz questions were provided in hard-copy format and participants entered answers on-screen. Subjects were told that they would earn a base amount ($20) in session 1 (irrespective of performance), but if they answered more than 10 questions correctly, they would earn additional income (up to $25) based on their performance. The subjects were told that their performance results on the quiz and any additional income owing to them would be announced a week

later at session 2. They were also informed that if they did not attend session 2, any additional income owed to them would be mailed to them. Participants were also asked to bring the $20 they earned in session 1 with them when they attended session 2. After completing the quiz, participants were asked to complete Table 4-1 in order to elicit their discount rates over a hypothetical savings decision.

Figure 6-1: Labelling Bias; Experimental Design (A)

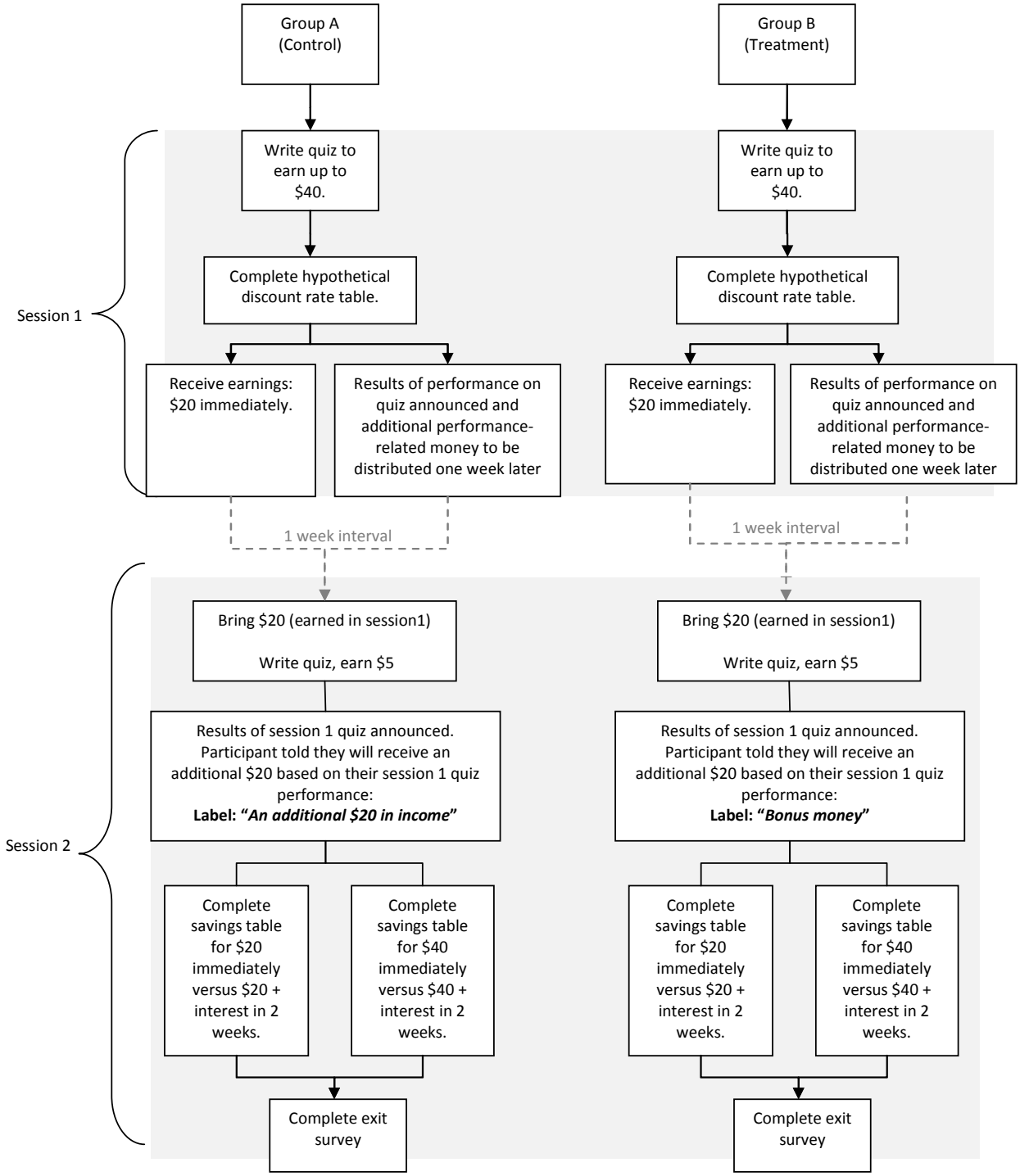

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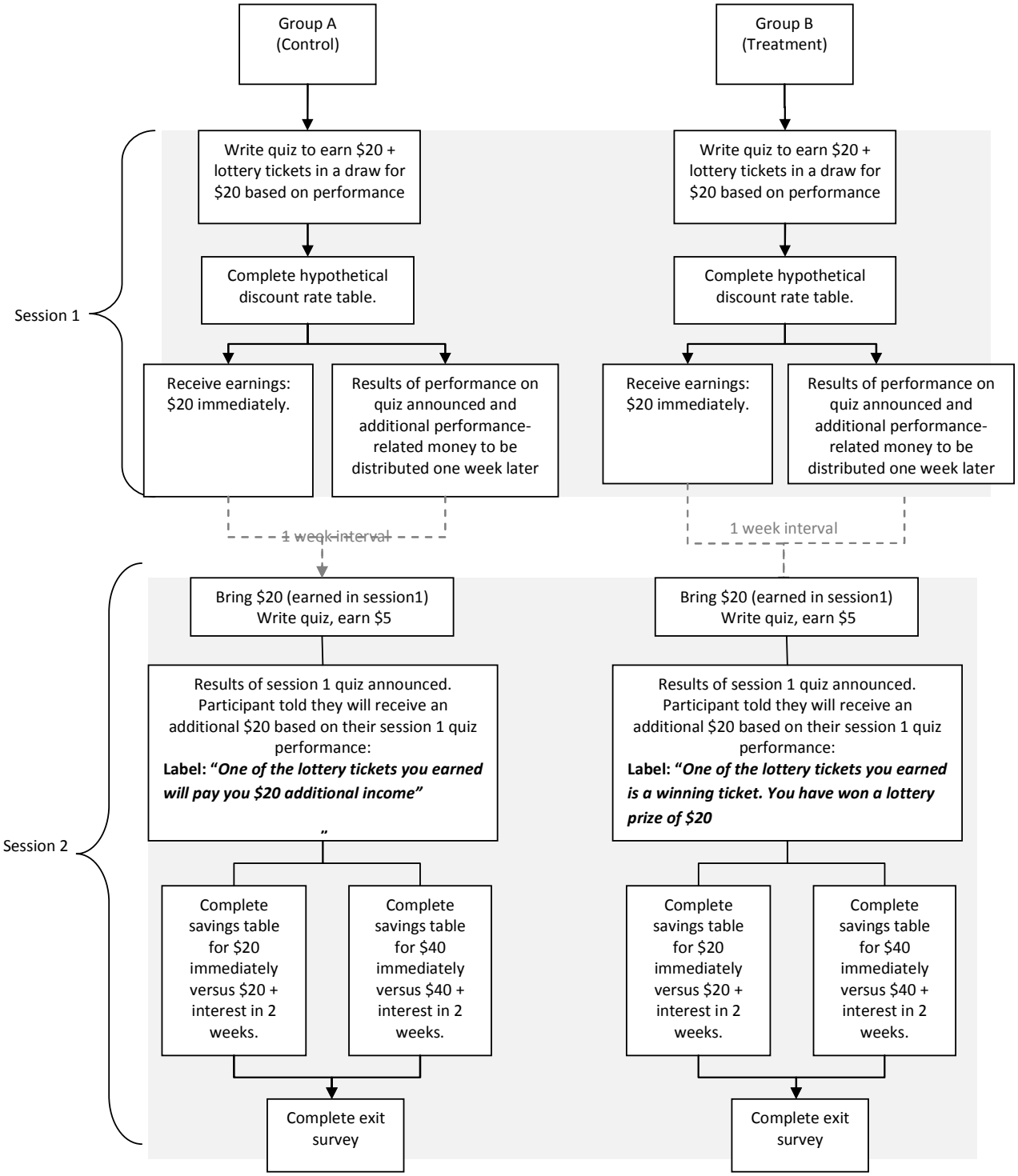
Figure 6-2: Labelling Bias; Experimental Design (B)



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6.2 Module 3 – Session 2 (A)

In the returning session, participants brought the $20 they had previously earned. The money was put in individual-specific envelopes labelled with each participant's number (i.e., the number of their computer station). Participants then completed a 10-minute, 10-question quiz for which they were paid $5 for getting at least two questions correct. All subjects managed to earn $5 for completing this quiz.

6.2.1 Baseline Treatment – Session 2(A)

In the baseline treatment, individuals were informed that they would receive an additional $20 as a result of their performance in session 1. This additional amount was referred to as "*An additional $20 in income you will receive today that you have earned as a result of your performance in last week's session.*" Individuals were then asked to complete Table 5-1 (elicited discount rates for $20) and Table 5-2 (elicited discount rates for $40).

6.2.2 Treatment Condition (A) – Session 2

In the treatment group, individuals were informed that they would receive an additional $20 as a result of their performance in session 1. In particular this additional amount was referred to as "*the bonus money you will receive today as a result of last week's session.*" The individuals were then asked to complete the $20 and $40 tables (Table 5-1 and Table 5-2) and were told that one choice would be randomly selected to be implemented at the end of the experiment.

After making these choices, individuals answered a short questionnaire (see appendix A). They left the session either with their $40 or a note indicating that they would receive the payoff they chose in one of the tables (i.e., treatment conditions). In addition, each participant left with $5 for completing the session 2 quiz. Finally, individuals were given an email address they could write to in the event they changed their mind regarding their choice from the above condition.

6.3 Module 3 – Session 1(B)

In the first session, participants earn resources based on a 20-question quiz using pre-selected questions from the Graduate Records Exam (GRE). The participants were given 20 minutes to complete the multiple-choice quiz. The quiz questions were provided in hard-copy format and participants entered answers on-screen. Subjects were told that they would earn a base amount ($20) in session 1 (irrespective of performance). In addition, based on their performance, they received "lottery tickets," each offering a chance to win an additional $20.

The lottery was a random draw from five ticket numbers. Subjects who answered more than 10 questions correctly received five lottery tickets. All participants did answer more than 10 questions correctly, and were thus set to receive $20 in additional earnings from the lottery. Individuals were asked to bring their $20 of initial earnings and their lottery tickets with them to the returning session. After completing the quiz, participants were asked to complete Table 4-1 in order to elicit their discount rates over a hypothetical savings decision. They then left the experiment with $20 in earnings plus five lottery tickets.

6.4 Module 3 – Session 2(B)

In the returning session, individuals brought the $20 they had previously earned. It was put in envelopes labelled with the number of their computer station. They then completed a 10-minute, 10-question quiz for which they were paid $5 for getting at least two questions correct.

6.4.1 Baseline Treatment – Session 2(B)

In the baseline treatment, individuals were informed of their additional income from the lottery using the following language: "*One of the 5 lottery tickets you earned from your performance on the quiz in session 1 has been selected and will pay you $20 additional*

income." Individuals were then asked to complete the $20 and $40 tables (Table 5-1 and Table 5-2), knowing that one choice from the tables would be randomly selected and implemented.

6.4.2 Treatment Condition – Session 2(B)

In the treatment condition, participants are informed of their additional income from the lottery using the following language: "*One of the 5 lottery tickets you earned in session 1 has been selected as a winning ticket. You have won a lottery prize of $20*". Individuals were then asked to complete the $20 and $40 tables (Table 5-1 and Table 5-2), knowing that one choice from the tables would be randomly selected and implemented.

6.5 All Treatments

After making all choices, individuals answered a short questionnaire (see appendix A). In addition, each participant left with $5 for completing the session 2 quiz. Finally, individuals were given an email address they could write to in the event they changed their mind regarding their choice from the above condition.

6.6 Module 3 Results

A total of 61 individuals (44 men and 17 women) participated in the experiment. (Six individuals participated in the first session but not in the second session, resulting in an attrition rate of 10 per cent.) Table 6-1, below, presents the summary statistics for each treatment described above.

The results show no differences in any of the elicited discount rates across all treatment cells.[25] This suggests that there is no effect of labelling on earned income when individuals have engaged in our protocol to implement loss aversion (i.e., our session 1 procedures described above). Interestingly, in these sessions, a proportion of individuals in each treatment cell indicated that they were unwilling to save their money in this experiment. That is, these individuals indicated that there was no sum of money in Table 5-1 and Table 5-2 that they felt would compensate them for deferring the use of the $20 or $40 for three

25 Wilcoxon p>0.4 in all pairwise comparisons.

weeks. The shares of these individuals in each of the four treatments in Table 6-1 are 43 per cent, 13 per cent, 14 per cent, and 17 per cent. While not statistically significant (p=0.065), there is some suggestion in comparing treatments 1 and 2 that labelling matters in determining whether or not individuals wish to save their money at all. We find no differences across gender or across other demographic information. (See Appendix D: Demographic Information on Participants.)

Table 6-1: Summary Statistics for Module 3: Elicited Discount Rates with Standard Deviation Reported in Parenthesis

	n	Discount rate Hypothetical $100	Discount rate Earned $20	Discount rate Earned $40
1. Baseline A ("additional income")	14	9.6% (5.4%)	21.0% (5.6%)	19.8% (6.3%)
2. Treatment A ("bonus income")	16	12.1% (6.8%)	24.3% (6.8%)	22.2% (9.4%)
3. Baseline B ("lottery ticket will pay")	14	9.8% (4.3%)	22.3% (7.9%)	23.7% (7.2%)
4. Treatment B ("winning lottery ticket")	17	10.0% (6.1%)	21.8% (8.9%)	23.8% (10.5%)

6.7 Module 3 Results Summary

Our results from this experiment suggest that, when loss aversion is present, the labels or semantic differences used to describe various sums of money do not affect intertemporal decision making or savings behaviour. This result (which was conducted within a loss aversion protocol) should be taken as a cautionary notice that policies which seek merely to re-label funds or resources are not sufficient to elicit meaningful changes in behaviour.

7. Belief Elicitation (Module 4)

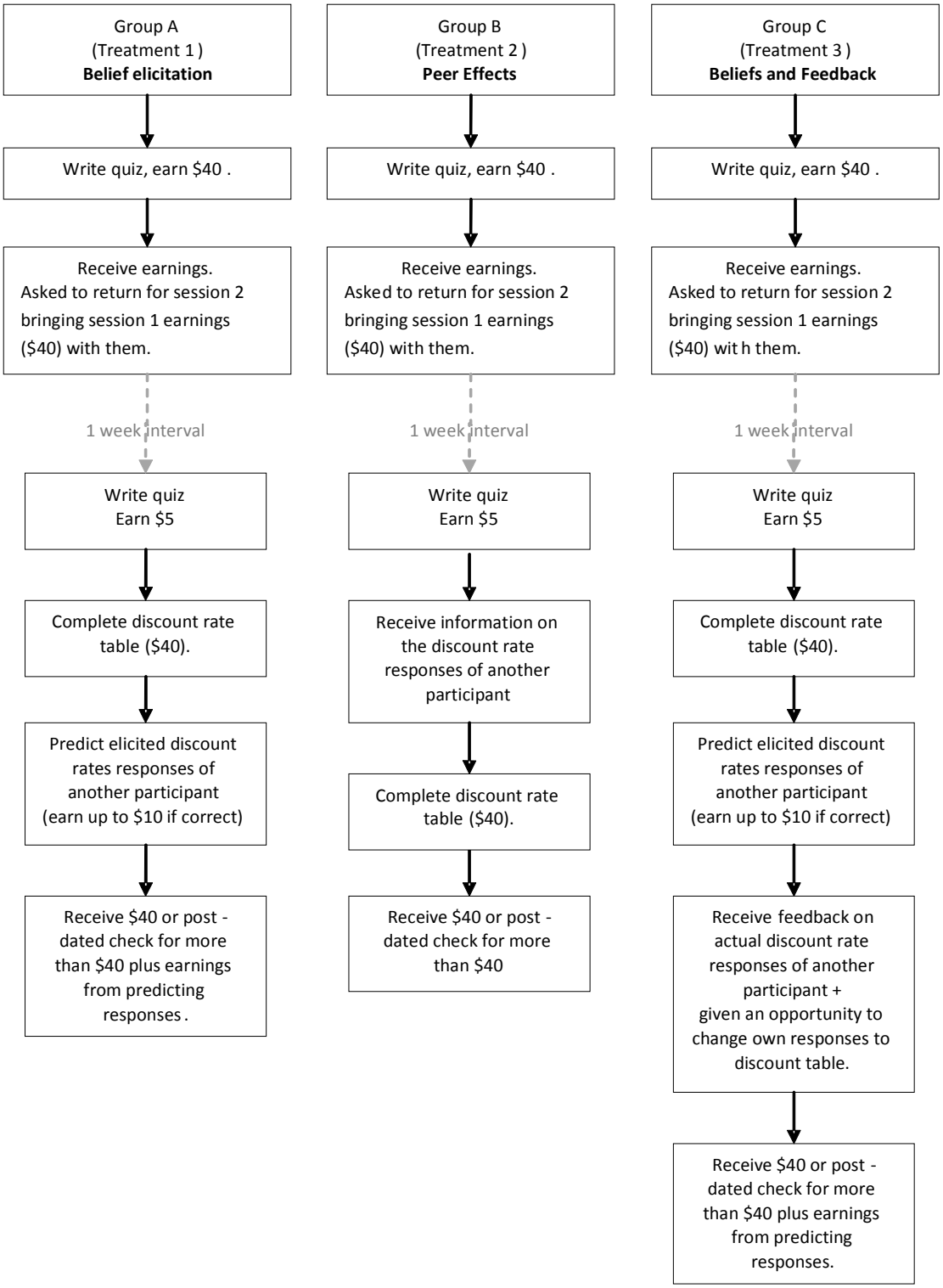
This experiment tested whether beliefs or feedback concerning the saving behaviour of others plays a role in individual savings decisions. The structure of the experiment was based on the general design of modules 1-3. In the first session, subjects earned money and were asked to complete Table 4-1 to elicit their discount rates over hypothetical income

two weeks later versus five weeks later. In the second (returning) session, participants were asked to complete Table 5-2 (discount rate elicitation for $40) on the understanding that one of their responses would be selected at random and that they would be compensated according to that response. They were then asked (in an incentive compatible manner) to reveal their beliefs concerning the elicited discount rates of other participants and/or were given feedback concerning the actual decisions of other participants. There are three different treatments in this experiment: belief elicitation (treatment 1), peer effects (treatment 2) and beliefs and feedback (treatment 3). The designs for each of these treatments are illustrated in Figure 7-1 below.

7.1 Module 4 – Session 1

In the first session, participants earned resources based on a 20-question quiz using pre-selected questions from the Graduate Records Exam (GRE). The participants were given 20 minutes to complete the multiple-choice quiz. The quiz questions were provided in hard-copy format and participants entered answers on-screen. An individual's wealth from this quiz was based on her score in accord with the following compensation scheme: individuals answering fewer than 10 questions correctly received $20. Individuals answering at least 10 questions correctly received $40. We then elicited discount rates using Table 4-1 (for a hypothetical amount of money – see module 1). Individuals were instructed to indicate (on the computer) which option they preferred (A or B) by clicking in the appropriate box on the screen. Individuals were not paid for their responses, but rather simply asked which they would prefer.

Figure 7-1: Belief Elicitation Experimental Designs



W. G. Morrison and R. J. Oxoby, 2010.

Participants were then paid and instructed to return the following week to complete the experiment. As part of the returning session, participants were asked to bring the amount of their earnings from session 1.

7.2 Module 4 – Session 2

In the returning session (session 2) all individuals brought the $40 they had previously earned. The money was put in individual-specific envelopes labelled with the participant's number (i.e., the number of their computer station). Individuals then completed a 10-minute, 10-question quiz for which they were paid $5 for getting at least two questions correct. All participants were able to earn $5 by completing this quiz. Participants were then instructed according to one of the following treatments.

7.2.1 Treatment 1 (Belief Elicitation 1)

This treatment tests whether there is a correlation between the elicited discount rates of participants and their beliefs concerning how other individuals responded to the same questions. Participants were first asked to complete Table 5-2 (module 2: discount rate elicitation for $40). They were told that one of the payoff alternatives they picked from the table would be randomly selected and implemented. They then received the following information and instructions:

> *You now have the opportunity to earn an addition $10 if you can correctly predict the responses of another individual who has been asked to complete the same table as you. We would like you tell us what you think that person's responses are. In particular, we would like you to answer the following question:*
>
> *At what point do you believe this person would switch from preferring $40 today to preferring a larger amount of money in two weeks?*
>
> *To answer this question, please select the lowest payoff alternative (numbered 1-12) at which you think they would switch from preferring option A ($40 right away) to preferring option B (more than $40 in two weeks time).*
>
> *If you guess correctly you will receive $10. If you are within 2 alternatives of the correct choice you will receive $5. Please make your selection now.*

Participants were then shown Table 5-2 on the screen and were asked to select the cross-over point they predicted for another player.

Once each individual had answered this belief elicitation question, they were shown the actual responses of a selected participant from previous runs of the experiment. Those individuals who predicted correctly were paid an additional $10 at the end of the experiment.

7.2.2 Treatment 2 (Feedback – Peer Effects)

This treatment tests whether there is a correlation between feedback on the behaviour of other individuals and the elicited discount rates of participants. Prior to being asked to complete Table 5-2, each participant was shown actual responses to Table 5-2 from another individual who had completed the same table in a previous run of the experiment. Participants were then asked to complete Table 5-2 themselves. The information provided to participants concerning the actual responses of others varied between showing responses of individuals who exhibited high discount rates and individuals who exhibited low discount rates.

7.2.3 Treatment 3 (Beliefs and Feedback)

This treatment mirrored treatment 1 except that after receiving feedback on the actual responses of another individual, participants were offered a chance to change their own responses in Table 5-2. This was to test for the presence of surprise or confirmation effects from being shown responses that are either similar to or different from their initial responses.

7.3 All Treatments

All individuals answered a short exit questionnaire (see appendix 1) and left the session with a cheque for the amount implemented via their responses to Table 5-2 (post-dated if applicable). In addition, each participant left with either $5 from the current session's quiz or $15 (treatments 1 and 3) if they correctly predicted the response of another participant. Finally, individuals were given an email address they could write to in the event they changed their mind regarding their choice from the above condition.

7.4 Module 4 Results

A total of 59 individuals (28 men and 31 women) participated in this experiment. (Eight individuals participated in the first session but not in the second session, resulting in an attrition rate of 11 per cent.) Table 7-1 presents the elicited discount rates and beliefs of individuals in each treatment. Treatments 2 and 3 are presented split based on the conditions of the feedback received. In the "high" feedback conditions, individuals received feedback regarding an individual from the experiments in Morrison and Oxoby (2009) who had a high interest rate (26 per cent). In the "low" feedback conditions, individuals were presented with the behaviour of an individual from the same experiments with a low discount rate (10 per cent).

Table 7-1: Summary Statistics for Module 4: Mean Elicited Discount Rates with Standard Deviation in Parenthesis

	n	Discount Rate Hypothetical $100	Session 2 Discount Rate ($40)	Belief re. other	Discount rate after feedback
Treat. 1	13	13.3% (4.3%)	20.4% (3.4%)	17.5% (4.8%)	n/a
Treat. 2 (high)	11	12.0% (4.1%)	n/a	n/a	22.0% (5.7%)
Treat. 2 (low)	12	11.4% (4.8%)	n/a	n/a	19.0% (4.3%)
Treat. 3 (high)	11	12.1% (3.8%)	21.9% (7.1%)	18.4% (6.5%)	21.3% (5.4%)
Treat. 3 (low)	12	9.8% (5.0%)	21.3% (5.4%)		17.9% (6.1%)

We find no differences across the treatments with respect to discount rates elicited in session 1 regarding the hypothetical $100.[26] Further, we fail to identify any significant effects of beliefs of feedback on discount rates. This is likely due to the fact that savings behaviour is an individual-level decision in the sense that the outcome of a saving decision (at least of the variety implemented in the laboratory) affects only the individual making the decision.

26 Wilcoxon p>0.45 in all comparisons.

It is worth noting that while not statistically significant, individuals in the low feedback conditions did display lower discount rates than did those in the higher feedback conditions. This suggests a process of peer learning wherein individuals may be choosing normative benchmarks (what is the "right" savings decisions) based on the choices of others. We find no differences in discounting across genders.

7.5 Module 4 Results Summary

In this experiment, we find that individual-level behaviour is not influenced by beliefs about what others are saving or by feedback on what others are actually saving. This result demonstrates that, whether individuals were presented with high or low figures related to others' savings behaviour, individual decision making was not affected. From a standpoint of policy design, there is evidence that individuals often respond to what they perceive to be the "social norm" at play in a given decision setting. These results suggest that the environment perceived by an individual when considering savings behaviour is not one in which social effects exert any measureable effect.

8. Relative Anchoring Bias (Module 5)

This experiment tests whether relative anchoring effects bias savings decisions. Specifically, we test to see whether the offer of one savings plan which is clearly superior to another biases individuals to save.

The basic experimental design follows that of module 1. In the first session, subjects earn money and are asked to complete the hypothetical table (Table 4-1) to elicit their discount rates over income in two weeks versus income in five weeks. There are three variations of the returning session in this experiment:

8.1.1 Returning Session 2A (Default Is to Receive Cash Immediately – No Savings)

Individuals in the control group were told that they were set to receive the income they earned in session 1 and were then asked if they wished to save the money. Individuals in the control group were offered one savings plan alternative to the immediate receipt of cash. However, individuals in the treatment group were offered two alternative savings plans, one of which was clearly superior to the other.

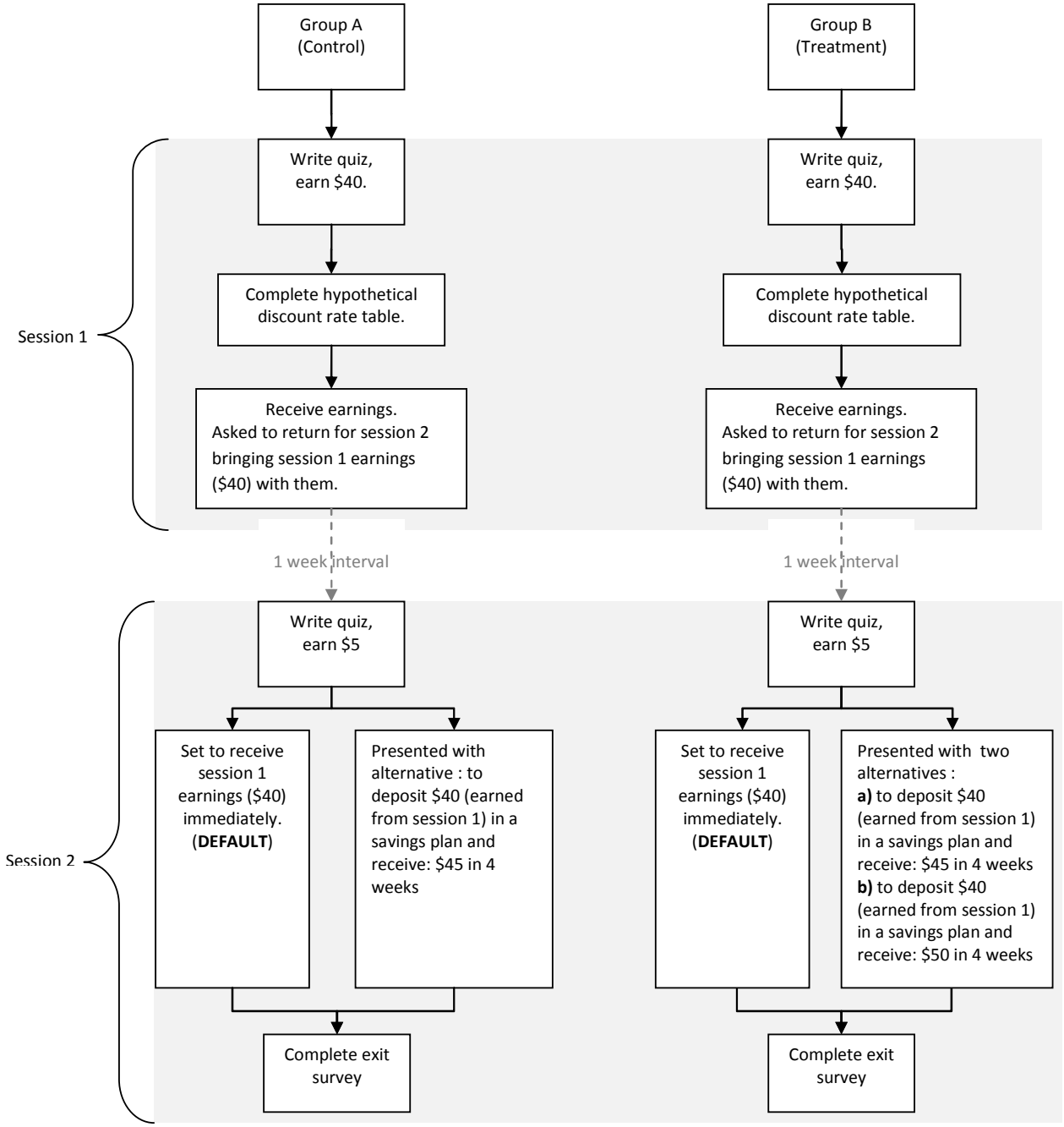
8.1.2 Returning Session 2B (Default Is the More Attractive Savings Plan)

Both the control and treatment groups were told that their money had been selected for the *more* attractive savings plan. The control group was then offered the chance to withdraw their money and receive it immediately. Participants in the treatment group were also offered the chance to withdraw their money but were also offered a chance to switch to the *less* attractive savings plan.

8.1.3 Returning Session 2C (Default Is the Less Attractive Savings Plan)

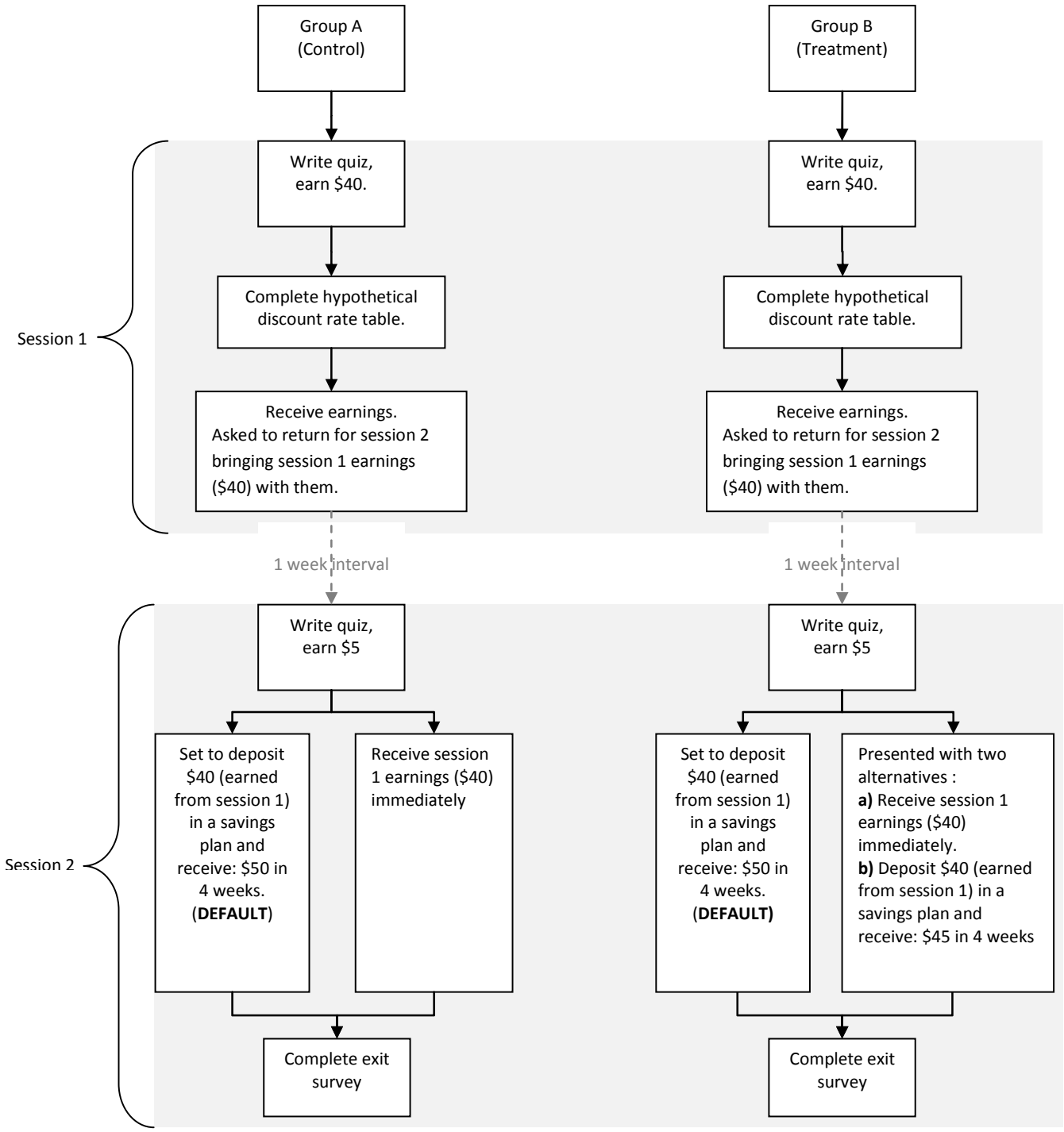
Both the control and treatment groups were told that their money had been selected for the *less* attractive savings plan. Participants in the control group were then offered the chance to withdraw their money and receive it immediately. The treatment group were also offered the chance to withdraw their money but were also offered a chance to switch to the more attractive savings plan. The above designs are illustrated in Figure 8-1, Figure 8-2 and Figure 8-3.

Figure 8-1: Relative Anchoring; "No Savings Plan" Is the Default



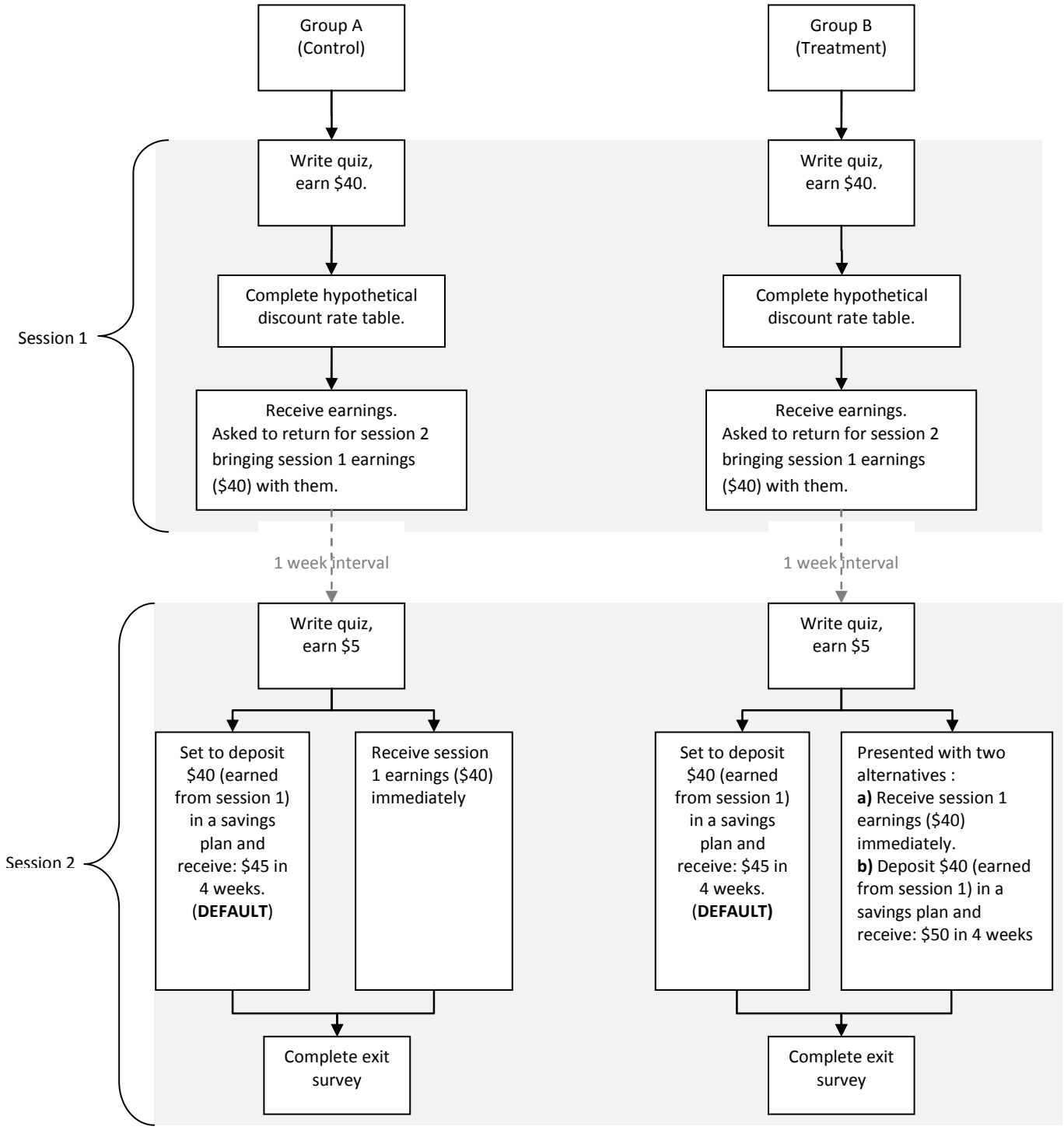
W. G. Morrison and R. J. Oxoby, 2010.

Figure 8-2: Relative Anchoring; “More Attractive Savings Plan” Is the Default



W. G. Morrison and R. J. Oxoby, 2010.

Figure 8-3: Relative Anchoring; “Less Attractive Savings Plan” Is the Default



W. G. Morrison and R. J. Oxoby, 2010.

8.2 Module 5 – Session 1

In the first session, participants earned resources based on a 20-question quiz using pre-selected questions from the Graduate Records Exam (GRE). The participants were given 20 minutes to complete the multiple-choice quiz. The quiz questions were provided in hard-copy format and participants entered answers on-screen. An individual's wealth from this quiz was based on her score in accord with the following compensation scheme: individuals answering at least five questions correctly received $10. Individuals answering at least 10 questions correctly receive $40. All participants were able to answer at least 10 questions correctly and therefore earned $40. We then elicited hypothetical discount rates using Table 4-1. Participants were paid and instructed to return the following week to complete the experiment. As part of the returning session, participants were asked to bring an amount of money equal to their earnings from session 1 to the subsequent session.

8.3 Module 5 – Session 2

In the returning session (session 2) all individuals brought $40 (an amount equal to that which they had previously earned in session 1). The money was put in individual-specific envelopes labelled with the participant's number (i.e., the number of their computer station). Individuals then completed a 10-minute, 10-question quiz for which they were paid $5 for getting at least two questions correct. All participants earned $5 on the session 2 quiz. Once all participants had completed the quiz, they received the following information and instructions:

8.3.1 Control 1; No Default

"Currently the $40 you brought with you today will be returned to you. However, you can choose to save this money in accord with the following: rather than taking the $40 with you today, you can choose to save it and receive $45 in four weeks. (The $45 payment represents your $40 plus a 12.5 per cent interest rate payment of $5)."

8.3.2 Treatment 1; No Savings Default

"Currently the $40 you brought with you today will be returned to you. However, you can choose to save this money in accord with the following: rather than taking the $40 with you today, you can choose between the following two options:

a) Save it and receive $45 in four weeks. (The $45 payment represents your $40 plus a 12.5 per cent interest rate payment of $5).
b) Save it and receive $50 in four weeks. (The $50 payment represents your $40 plus a 25 per cent interest rate payment of $10)."

8.3.3 Control 2; High Interest Default

"*Currently the $40 you brought with you today has been assigned to a savings plan. Under the savings plan, you will receive $50 in four weeks. (The $50 payment represents your $40 plus a 25 per cent interest rate payment of $10). However if you wish, you can choose to take your $40 with you today. That is, rather than receiving $50 in four weeks you can receive $40 today."*

8.3.4 Treatment 2; High Interest Default

"Currently the $40 you brought with you today has been assigned to a savings plan. Under the savings plan, you will receive $50 in four weeks. (The $50 payment represents your $40 plus a 25 per cent interest rate payment of $10). However if you wish, you can choose one of the following options:

a) You can choose to receive your $40 and take it with you today.
b) You can choose to place your $40 in another saving plan that will pay you $45 in four weeks (The $45 payment represents your $40 plus a 12.5 per cent interest rate payment of $5)."

8.3.5 Control 3; Low Interest Default

"C*urrently the $40 you brought with you today has been assigned to a savings plan. Under the savings plan, you will receive $45 in four weeks. (The $45 payment represents your $40 plus a 12.5 per cent interest rate payment of $5). However if you wish, you can choose to take your $40 with you today. That is, rather than receiving $45 in four weeks you can receive $40 today."*

8.3.6 Treatment 3; Low Interest Default

"Currently the $40 you brought with you today has been assigned to a savings plan. Under the savings plan, you will receive $45 in four weeks. (The $45 payment represents your $40 plus a 12.5 per cent interest rate payment of $5). However if you wish, you can choose one of the following options:

a) You can choose to receive your $40 and take it with you today.
b) You can choose to place your $40 in another saving plan that will pay you $50 in four weeks (The $50 payment represents your $40 plus a 25 per cent interest rate payment of $10)."

8.3.7 All Participants

After making this choice, all participants answered a short questionnaire (see Appendix 1). Individuals were paid by cheque (current or post-dated, depending on their treatment condition). In addition, each participant left with $5 for completing the session 2 quiz. Finally, individuals were given an email address they could write to in the event they changed their mind regarding their choice from the above condition.

8.4 Module 5 Results

A total of 96 individuals (52 men and 44 women) participated in the experiment. (Thirteen individuals participated in the first session but not in the second session, resulting in an attrition rate of 12 per cent.) Table 8-1 presents the elicited discount rates for these individuals and the share of individuals choosing to save. In each of the treatments, individuals always chose the option paying the higher rate of interest (when available). That is, we saw no individuals choosing an inferior option when a superior option (i.e., one paying a higher rate of interest) was available.

The first thing to note is that we observe a significant increase in savings behavior when individuals are in the default treatments but offered a choice between options (i.e., Treatments 2 and 3).

Table 8-1: Summary Statistics for Module 5: Mean Elicited Discount Rates with Standard Deviation in Parenthesis

	n	Discount Rate Hypothetical $100	Share (Number) Choosing Savings
Control 1 (no default)	14	10.4% (6.5%)	14% (2)
Treatment 1 (no default)	16	11.6% (5.2%)	25% (4)
Control 2 (high interest default)	18	9.6% (4.8%)	19% (3)
Treatment 2 (high interest default)	17	11.4% (5.8%)	58% (10)
Control 3 (low interest default)	16	13.4% (6.0%)	13% (2)
Treatment 3 (low interest default)	18	12.0% (4.3%)	72% (13)

This is particularly striking in the case of Treatment 3, where individuals were defaulted into a low interest option and given the choice to move into a higher interest option. The differences between observed behavior in Control 2/Treatment 2 and Control 3/Treatment 3 are statistically significant (at $p<0.02$). This suggests that providing individuals with an option within their default is a way to encourage additional savings. We did not identify any differences between the decisions of men and women (see Appendix D: Demographic Information on Participants).

As argued by Rubinstein (1998), individuals in complex or infrequent decision context often look for reasons to support their decisions (i.e., reason-based decision making). In this context, the presence of an inferior option (the low interest rate option) provides individuals with an ostensible reason for saving at a higher interest rate. This line of reasoning is supported in the comparison of the Controls 2 and 3 with Treatments 2 and 3: *In all cases, providing individuals with a choice resulted in a greater propensity to save*.[27]

27 Statistically, we can reject the hypothesis that the behaviors in Controls 2 and 3 are drawn from the same distribution as behaviors in Treatments 2 and 3 ($p<0.03$ in pairwise comparisons).

8.5 Module 5 Results Summary

In this experiment, we find significant evidence that when individuals are given a superior option, they choose that option. This suggests that individuals make relative comparisons when making decisions (rather than being directly able to identify a "utility maximizing choice" in a simple choice environment) and that the simple comparison of savings options can result in greater savings behaviour (as happened here). This result is consistent with literature in behavioural economics and psychology, in which individuals make relative comparisons among available options. From the perspective of policy making, this results suggests that providing references to less desirable saving options in order to demonstrate the clear superiority of a preferred saving mechanism will encourage saving in the preferred option.

9. Ex-ante Training and the Provision of Financial Information (Module 6)

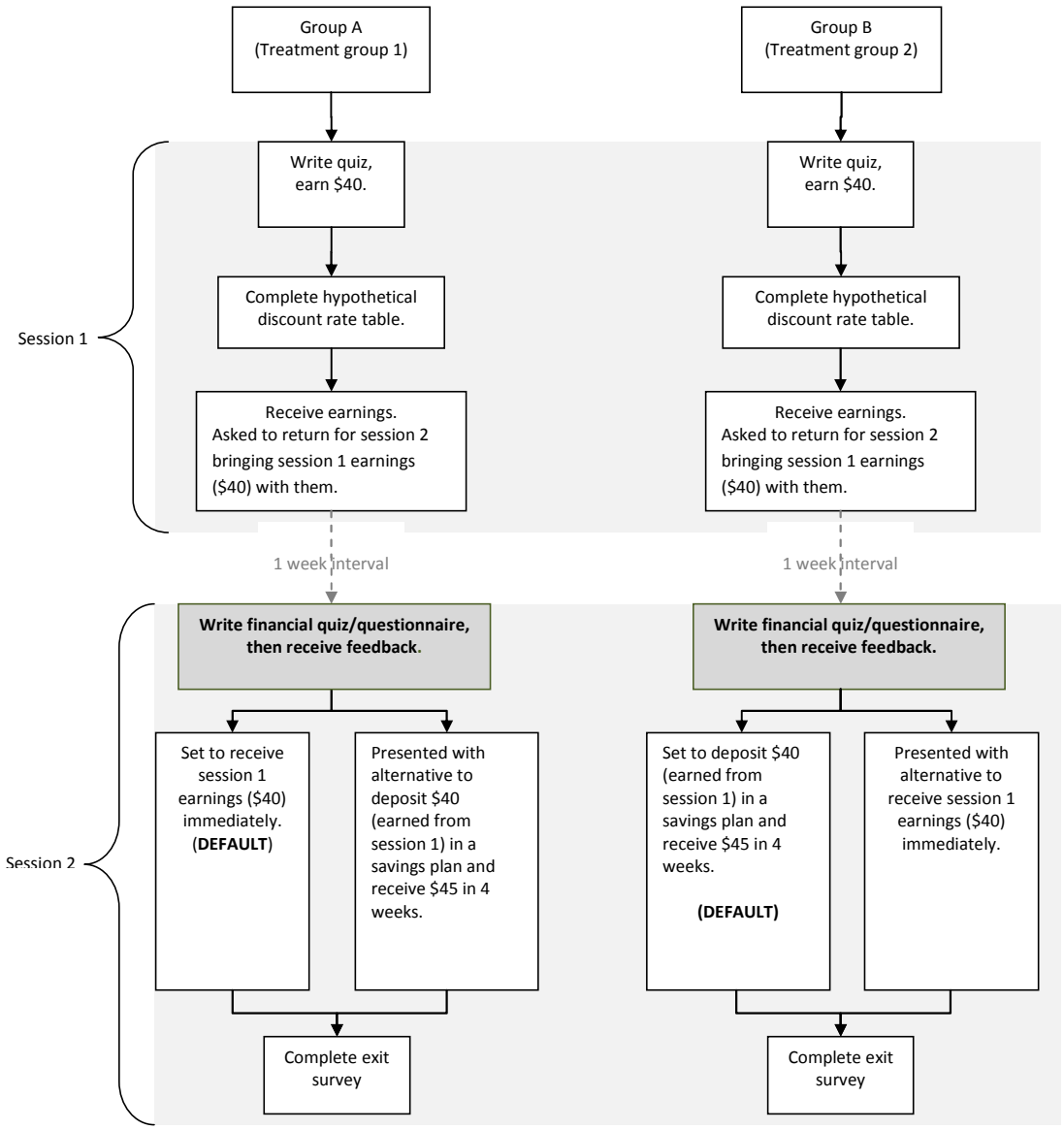
Module 6 tests whether training (through completion of specific quiz and questionnaire questions) and the provision of financial information will lead to an increased willingness to save money. The basic design of this experiment follows that of module 1 (default bias, using the low interest rate savings option), in which participants first earn money by competing a quiz, then (one week later) choose between receiving their money ($40) immediately and receiving a larger amount in two weeks. However, in the returning session (session 2), rather than complete a GRE quiz, participants in the treatment groups complete a quiz/questionnaire in which they are asked to think about financial decisions, make financial calculations and assess their own understanding of financial matters. The control group for this experiment is participants in the (low interest rate) module 1 experiment on default bias. The two treatment group designs for this experiment are illustrated below in Figure 9-1.

9.1 Module 6 – Session 1

In the first session, participants earned resources based on a 20-question quiz using pre-selected questions from the Graduate Records Exam (GRE). The participants were given 20 minutes to complete the multiple-choice quiz. The quiz questions were provided in hard-copy format and participants entered answers on-screen. An individual's wealth from this quiz was based on her score in accord with the following compensation scheme: individuals answering at least five questions correctly received $10. Individuals answering at least 10 questions correctly received $40.

We then elicited discount rates using Table 4-1 (see module 1). Individuals were instructed to indicate which option they preferred (A or B) by clicking in the appropriate box on the computer screen. Individuals were not paid for their responses, but rather simply asked which they would prefer.

Figure 9-1: Provision of Financial Quiz/Questionnaire Prior to Savings Decision



W. G. Morrison and R. J. Oxoby, 2010.

Participants were then paid and instructed to return the following week to complete the experiment. As part of the returning session, participants were asked to bring an amount of money equal to the amount they earned in session 1.

9.2 Module 6 – Session 2

In the returning session (session 2), all individuals brought $40 (the amount they had previously earned in session 1). This money was put in individual-specific envelopes labelled with the participant's number (i.e., the number of their computer station). Individuals then completed a 10-question financial literacy quiz/questionnaire (see Appendix C),[28] each question of which asked for their opinions or tested their knowledge concerning financial decisions. Participants were paid $5 for completing this quiz, regardless of performance.

After completing the quiz, participants were shown the correct answers to all quiz-type questions. They were then assigned to one of the following instructions and asked to indicate their preference on the computer screen.[29]

9.2.1 No Default Treatment

"Currently the $40 you brought with you today will be returned to you. However, you can choose to save this money in accord with the following: rather than taking the $40 with you today, you can choose to save it and receive $45 in four weeks. (The $45 payment represents your $40 plus a 12.5 per cent interest rate payment of $5)."

9.2.2 Default Treatment

"Currently the $40 you brought with you today has been assigned to a savings plan. Under the savings plan, you will receive $45 in four weeks. (The $45 payment represents your $40 plus a 12.5% interest rate payment of $5). However if you wish, you can choose to take your $40 with you today. That is, rather than receiving $45 in four weeks you can receive $40 today."

---

28 Many of the questions were based upon a slide presentation entitled "The City", produced as a financial life skills resource by Financial Consumer Agency of Canada and the British Columbia Securities Commission (2004-2008).

29 Note that from module 1, we already have data concerning the no-default and default treatments for participants who did not complete a financial literacy quiz/questionnaire.

9.2.3 All Participants

After making this choice, all individuals answered a short questionnaire (see Appendix 1). Individuals then either left the session with their $40 or a note indicating they would receive $45. In addition, each participant left with $5 for completing the session 2 financial quiz/questionnaire. Finally, individuals were given an email address they could write to in the event they changed their mind regarding their choice from the above condition.

9.3 Module 6 Results

A total of 55 individuals participated in this experiment, with 26 in the baseline (no default) condition and 29 in the treatment (default) condition.

As in Module 1 above, we find very few people in either condition opting to save: in the baseline condition, 7 (27 per cent) individuals opted to save their money, while in the treatment condition only 9 (31 per cent) people opted to remain in with the default option (savings). Similarly, we find no differences between the discount rates elicited using Table 1 across the baseline and treatment conditions, nor relative to those elicited in the other modules (12.4 per cent and 11.6 per cent in the baseline and treatment conditions of Module 6; $p>0.25$ in all pairwise comparisons).

Note that what distinguishes Module 6 from Module 1 is that individuals in this module completed the financial literacy questionnaire prior to making their decisions regarding savings in session 2. Thus, it is useful to consider Module 1 as a control regarding the effect of the financial literacy questionnaire on behaviour. In theory, the questionnaire should motivate individuals to consider the importance of future outcomes and the benefits of investment/savings to achieve future goals. For this reason, we compared the savings behaviour of participants in Module 1 with those in the current experiment.

Although the sample sizes are small, we are able to reject the hypothesis that the savings behaviour of individuals in the Module 6-baseline and Module 1-no default groups are drawn from the same distribution ($p<0.01$). Similarly, comparing the Module 6 treatment

condition and the Module 1 default conditions, we can also reject the hypothesis that these responses are drawn from the same distribution (p=0.031). As such, while we find no evidence of a default bias, we find evidence that completing the financial literacy questionnaire motivated individuals to be more aware and sensitive to saving options in their decision making.[30] We find no effects of gender in our outcome variables (discount rates). We also find no evidence that any of the specific questions in the financial literacy quiz (Appendix C: Financial Literacy Quiz/Questionnaire) had any relationship with the discount rates elicited in the experiment (Appendix D).

9.4 Module 6 Results Summary

In this experiment, we find that individuals are more patient (i.e., are willing to save at lower rates of interest) when they have been primed with a ten-question financial literacy quiz/questionnaire. This suggests that getting individuals to think directly about savings behaviour and the related trade-offs between current and future consumption makes them more willing to engage in savings. From a policy perspective, this highlights the importance of educating and focusing individuals when they are faced with decisions. For example, in a savings policy context, individuals should be put in a frame of mind where they are able to consider savings decisions in the appropriate context. Thus, our results provide support for moves to increase financial literacy. Policies that embed savings decisions in the context of future consumption are more likely to yield increased savings than are approaches in which savings policies and decisions are presented in the absence of an explicit intertemporal choice context.

30 The absence of a default bias is likely due to our protocol implementing loss aversion (i.e., our session 1 earning activities). In addition, while this result is encouraging, we should remember that in these experiments individuals only faced a binary save/don't save question. This suggests that further research needs to be conducted to ascertain the precise manner in which the questionnaire makes individuals more sensitive or aware of intertemporal aspects of decision making.

10. Strengths and Limitations of the Current Studies

10.1 Strengths of Laboratory Experiments

All the experiments discussed above were laboratory experiments (conducted with student participants in a laboratory rather than field studies that use other pools of participants). This has provided us with an explicit level of control over the types of savings decisions faced by individuals and the context in which participants perceived their decisions. As such, we were able to observe behavioural effects that were solely the result of our treatment variables (for example, facing a default or no default decision). The strength of this approach is that we can clearly identify the independent effect of the treatment variables (i.e., the structure of the saving decisions) on the observed behaviour.

In field studies, one is seldom able to implement this level of control. Rather, individuals making savings decisions in the field over wage income may approach the decisions environment very differently from those making decisions over lottery winnings. In these circumstances, observing a difference in savings behaviour may be the result of the structure of the decision environment or a result of the meaning or perception individuals have regarding how the money was obtained. As such, field studies often cloud the issue of causality (i.e., what determines behaviour) because the investigator cannot control some significant confounding factors which may underlie observed behaviour.

Thus, laboratory experiments and field experiments should be viewed in a feedback loop: laboratory experiments identify research questions that can be tested in the field; the results of field experiments can be tested in the lab under more controlled conditions; these lab experiments inform the design of field experiments.

In our work here, we have worked on both sides of this loop. For example, field evidence already exists on default biases and save-more-tomorrow programs; however, we have tested these interventions in a controlled environment (one including the role of loss aversion that was not in these field studies) to provide an additional test of these programs

prior to policy implementation. Our results which show a lack of default bias suggest caution and a need to better understand the context and specific environment in which default bias may or may not operate. With respect to our results regarding save-more-later programs, our results are consistent with those of previous field experiments (e.g., Thaler and Bernartzi, 2004). This should give policy makers added confidence in the use of these types of programs to encourage savings. The next appropriate step for policy design is therefore to begin testing specific save-more-tomorrow designs.

On the other side of the feedback loop, we have tested other means of influencing savings behaviour (relative anchoring and peer effects, for example). Our results from these experiments can now be used to inform the design of field studies on these topics prior to incorporating such elements into policy design.

As mentioned above, our experiments used a participant pool made of students at a large Canadian university. For many of the reasons already mentioned, students make an ideal pool for use in experiments at this stage of inquiry. Because many of them do not have the financial burdens or immediate financial needs faced by other participant pools, decisions for this pool can be presented in a context-free environment. Moreover, the availability of this group allows research to be performed in a timely and cost-effective manner. Our view is that, as this research begins to be considered for policy design, alternate participant pools can be used to identify the dimensions of policy design which are most effective in obtaining the desired effects. In the same way that our findings can influence new field studies, we can also consider rerunning our experiments with alternate participant pools as an important step in the use of these findings in policy design.

10.2 Limitations of our Experimental Design

While our design permitted us to isolate various effects of framing, anchoring, and other treatment variables on behaviour, it is important to recognize the limitations of the design. First, all the choices we asked of individuals were discrete choices: they always made a selection from a limited number of options (as in Table 5-2) or chose between two

alternatives (as in our experiment on the default bias in section 4). Thus, individuals were not given the option to choose to save a percentage of income. Similarly, individuals never had to choose among a multitude of savings plans, in which the number of elements of each plan and the number of plans would add to the complexity of the choice environment.

From the decisions about employer-funded retirement programs to those involving simple RSP or Christmas club contributions, individuals face much more latitude in their decision making. Everyday decisions are much more complicated than those faced by our participants. As such, the results presented above should serve as a first step in identifying the role of policies in motivating savings behaviour. Nevertheless, we have identified several strong, important effects that are relevant for policy makers. These need to be more rigorously tested through alternate environments to identify the contexts in which they are most robust.

Another important aspect of our decision environments was the lack of limits on the use of money in the experiment. The money was earned in a simple laboratory quiz, and its meaning was determined by each participant. This is very different from the contexts in which individuals often view money. Because individuals attach labels such is “my wages,” “money for vacation,” etc., to their incomes, fiscal policy should be sensitive to the contexts used by individuals for viewing specific resources.

For example, individual income tax returns are often viewed not as “found money” or a “bonus” but rather as money that was withheld in excess by the government. This additional meaning may affect the way individuals opt to use these funds, particularly with respect to savings programs presented by government agencies. This idea is even more important when considering disbursements, such as the settlements to individuals who were in residential schools. These funds are drenched in meaning and carry an emotional component that no experiment (laboratory or field) can characterize.

Finally, we note that our experiments focused on the effects of loss aversion within the context of intertemporal choice (i.e., savings decisions). The original research of loss aversion (Kahneman and Tversky, 1979) and the majority of the research which followed (e.g., Bowman et al., 1999; Kahenman et al., 1991) focused on the effects of loss aversion on risk preferences and risk taking. This research has documented how individuals appear to be risk taking when they face potential losses and risk averse over potential gains. There may be important similarities between risk-taking behaviour and intertemporal decision making (e.g., Lau, 2008); however, in the current research, we have focused only on the intertemporal aspects of loss aversion. Future research should consider experiments and tests which jointly measure risk and time preferences in order to tease out the relationship between these two aspects of choice. Our research to date suggests that, notwithstanding the distribution of risk in financial decision-making, preferences for immediacy and behaviour consistent with loss aversion are strong forces that should not be ignored or underestimated.

11. Concluding Remarks

While a significant body of literature in economics and psychology has studied financial and savings behaviour, little of this literature has explored the role of loss aversion in decision making. Loss aversion (the reluctance to give up something one currently has in possession) is an integral part of any savings decision: savings necessitates giving up current resources (e.g., resources which could be used for current consumption) for future benefits. While the time cost of money underlies theories of savings and interest rates in economics, loss aversion is a key behavioural phenomenon which may underlie the observed savings behaviour. As such, one of our primary goals in this research was to explore the effect of loss aversion on various savings environments with an eye towards informing policy regarding savings behaviour (i.e., increasing savings).

Previous field research has explored the use of defaults, save-more-later programs, and labels to influence the savings behaviour of individuals. In our research, we have found that, while these programs can influence savings, the degree of influence is tempered by

the effects of loss aversion. That is, when individuals make savings decisions about resources over which they have a sense of ownership or entitlement, these instruments may be less successful than initially suggested by previous research. For example, default biases, which are highly touted in much of the literature on financial decision making, fail to encourage savings when individuals are loss averse over the resources in question.

These results should be of significant interest to policy makers. Indeed, in most cases in which individuals are making savings decisions, they are allocating current resources (which may be used in a variety of ways) towards future goals (e.g., consumption). As such, from the standpoint of policy design, a policy will fail to implement its desired objective (e.g., increased savings) unless the potential effects of loss aversion are taken into account. The presence of loss aversions requires policy makers to be more creative in developing policies and, given our results, in exploiting opportunities in which loss aversion does not appear to dampen the effects of the intervention (e.g., save-more-later, relative anchoring effects, priming individuals with general financial questions).

11.1.1 Summary of Overall Research Program

Beginning with the development of a protocol to allow for the existence of loss aversion in behavioural experiments involving intertemporal choice, we have reported on seven experiments. In them, we observe a very strong bias towards immediacy and against saving money when that money is earned and retained by participants prior to making a savings decision. If our results have an overall "take-away" message, it is that loss aversion (or at least behaviour consistent with loss aversion) should not be underestimated in the context of financial decision making. In three of our six savings-decision experiments, the bias towards immediacy dominated other behavioural effects (default bias, labelling and peer effects). However, in our "save-more-later" and relative anchoring experiments, we obtained strong results that were not washed away by the loss aversion protocol. Finally in an experiment to investigate the effect of completing a financial quiz/questionnaire on saving decisions, we found some evidence that getting individuals to think about financial

matters and tradeoffs between current and future consumption does make individuals more likely to save.

11.1.2 Default Bias, Labelling Bias and Beliefs

Within the context of our loss aversion protocol (section 2) we found no evidence of a default bias in our module 1 experiment. While the literature has discussed default bias as arising in unfamiliar or non-routine decision environments, our results suggest that the default bias identified in other studies is largely an artefact of decision making over wealth or resources that have not yet been received by an individual. As such, from the standpoint of policy design, this suggests that default biases will work best when individuals do not yet have a stake in the resources in question (i.e., money in hand or a perception that the money is already theirs). These findings may provide us with insights into the financial behaviour of low-income individuals for whom present consumption needs outweigh future considerations and result in low levels of savings. This should be a key focus of future research, as the financial and savings behaviour of low income individuals is of great importance in the efficacy of programs designed to facilitate the transition out of poverty.

Similarly in our labelling experiment, the results suggest that, when loss aversion is present (as invoked by our protocol), the labels or semantic differences used to describe various sums of money do not affect intertemporal decision making or savings behaviour. This result should be taken as a cautionary notice that policies which seek merely to re-label funds or resources are unlikely to be successful in eliciting meaningful changes in behaviour.

We also found that the beliefs of individuals about what others were saving and feedback on what others were actually saving did not influence individual-level behaviour. Furthermore, whether individuals were shown high or low amounts related to the savings behaviour of others, individual decision making was not affected. While evidence elsewhere suggests that individuals often respond to what they perceive to be the “social norm” in a given decision environment, our results indicate that the environment an

individual perceives when considering saving is not a decision setting in which social effects exert any measureable effect. However, it is possible that more subtle effects of beliefs or peer influence were washed out by our loss aversion protocol; that is, the desire for immediacy may have overpowered these other effects.

11.1.3 Save-more-tomorrow

Our results in this experiment demonstrate how individuals respond when faced with intertemporal decisions over resources (here, money) not yet acquired. Our results (again within our strong loss aversion protocol) demonstrate that individuals are willing to save money at a much lower rate of interest when it is not yet in their possession, even though it has been earned. From a policy-making standpoint, our results provide strong support for save-more-later types of interventions, where individuals have an initial sense of ownership over money that they do not yet possess. An important implication of our results is that we find evidence that save-more-later programs can work in environments where there is (potentially) a great deal of loss aversion. This suggests that there is more to a save-later-program than simply exploiting unearned or unreceived wealth (as in making future saving commitments out of unreceived future raises). Research should explore the specifics of 'save-more-tomorrow' interventions to design policies which can be implemented in a variety of environments involving the future receipt of resources.

11.1.4 Relative Anchoring

In this experiment, we found significant evidence that when individuals are provided with a means of comparing two savings options that are easily ranked in terms of desirability, they will choose the superior savings option instead of not saving. Note that this behavioural tendency was strong enough to dominate our loss aversion protocol. This suggests that individuals make relative comparisons when making decisions (rather than being directly able to identify a "utility maximizing choice" in a simple choice environment) and that the simple comparison of savings options can result in greater demonstrated savings behaviour. This result is consistent with literature in behavioural economics and psychology wherein individuals make relative comparisons among available options. From the perspective of policy making, this suggests that pointing out less desirable saving

options in order to demonstrate the clear superiority of a preferred saving mechanism will encourage saving in the preferred option.

11.1.5 Focus on the Context of Financial Decisions Prior to Savings Decisions

We found that individuals are more patient (i.e., are willing to save at lower rates of interest) when they have been primed with a ten-question financial literacy quiz/questionnaire. This suggests that individuals are more willing to engage in savings when they think directly about savings behaviour and the related trade-offs between current and future consumption. From a policy perspective, this highlights the importance of educating and focusing individuals when they are faced with decisions. For example, in a savings policy context, individuals should be put in a frame of mind to consider savings decisions in the appropriate context. Thus, our results provide support for moves to increase financial literacy. Policies that embed savings decisions in the context of future consumption are more likely to yield increased savings than are approaches in which savings policies and decisions are presented in the absence of an explicit intertemporal choice decision.

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Appendix A: Exit Questionnaire

1. Please indicate your gender.
2. Please indicate your age.
3. Please indicate your major.
4. Please indicate your year in school.
5. Do you work part-time, full-time? (Yes/no for each)
6. What is would monthly income (categorical questions)?
7. Do you have a savings account?
8. What is the interest rate paid on your savings?
9. Do you have a credit card?
10. What is the interest rate your credit card charges?
11. How much of your monthly income do you save (percentage)?
12. Do you carry a balance on your credit card?
13. How do you pay for school (student loans, bursaries or grants, all of the above, none of the above)?
14. What percentage of your monthly income goes to necessities (e.g., rent, utilities)?
15. What percentage of your monthly income do you spend on recreation/entertainment?

Appendix B: Sample Instructions

Earnings Phase; Module 1 – Session1

This is an experiment in decision-making. During the experiment, you will make decisions resulting in a monetary payout. You will receive payment in cash at the end of the experiment. The payment is considered compensation for the time and effort you put into making your decisions. The experiment will last about 60 minutes.

Throughout the experiment, we ask that you refrain from speaking with others. If you have any questions, please raise your hand and the experimenter will assist you.

Broadly speaking, this experiment has two parts; the first part will be during today's session in which you will earn money based on your answers to a short quiz. The second part of the experiment will take place in a session conducted next week. In that experiment, you must bring an amount of money equal to what you earn in today's experiment to utilize in the decision-making task in that session. The decision making task in that experiment will not pay you any less than what you earn today (bring to that session) and will provide you with an opportunity to increase the money you earn today. In addition, you will receive a $5 show-up fee for returning to next week's session.

Today's session will proceed as follows:
When the experiment begins, the monitor will hand out a set of 20 questions constituting a short quiz, the results of which will determine your payment for today's experiment. You will have 20 minutes to complete the quiz.

All your answers must be input on the answer sheet appearing on the screen in front of you. For each question, there is a set of answers (A though E) on-screen. For each question, click the answer you choose and then click the submit button appearing next to that question. Please note that you must click the submit button for your answer to be recorded. If you wish to change an answer, simply change your answer and click the submit button.

There is no penalty for wrong answers.

After the 20-minute quiz period has ended, the screen will go blank and the computer will calculate your score. After this, you will receive feedback on your score and the amount of money you will be receiving for today's experiment. After this information has been reported, there will be a short questionnaire.

Once everyone has completed the questionnaire, the monitor will call you up by computer station and give you your money in private. Please note that you must sign a receipt for any funds you receive in the experiment.

Appendix C: Financial Literacy Quiz/Questionnaire[31]

1. Are you currently saving money for any of the following reasons?
    - To have emergency funds
    - To build an investment fund for the future
    - To save for a particular purchase item (e.g., car, TV, computer, sound system)
    - To build funds for a down payment on a house sometime in the future
    - A holiday or trip (e.g., airfare and hotel costs)
    - To help pay for my education

2. Do you currently have a job that pays money income (not a volunteer position)?
    a) Yes
    b) No

If yes how much of your monthly earned income do you save (pick the statement that best fits your situation)?

- None
- Up to 10 per cent of my monthly earned income is saved
- 10- 20per cent of my monthly earned income is saved
- 20-30 per cent of my monthly earned income is saved
- 30-50 per cent of my monthly earned income is saved
- 50-75 per cent of my monthly earned income is saved
- Over 75 per cent of my monthly earned income is saved
- All

3. Think about the various ways people can save and invest money. Consider the following savings and investment options:
    - a checking account
    - a savings account
    - guaranteed savings certificates (GICs)
    - Government of Canada savings bonds
    - mutual funds
    - stocks

31 The * symbol indicates a correct answer when applicable.

Which one of the following statements best describes your understanding of the differences between these savings and investment options?

a. Good – I understand the important differences between all these options.
b. Satisfactory – I understand most of the important differences between these options
c. Unsatisfactory – I do not understand the main differences between some of these options.
d. Very poor – I do not understand the differences between most or all of these options.

4. Rank the savings and investment options below with respect to how risky you think they are. Select a value from 1-5 for each option, where 5 means 'extremely risky' and 1 means 'not risky at all'.
    - a checking account
    - a savings account
    - guaranteed savings certificates (GICs)
    - Government of Canada savings bonds
    - mutual funds
    - stocks

5. Rate each of the savings and investment options below in terms of the income you are likely to earn from it (i.e., how much money you can make from each option, not counting your initial investment or deposit). Select a value from 1-5 for each option, where 5 means "likely to generate very high income" and 1 means "likely to generate very low income."
    - a checking account
    - a savings account
    - guaranteed savings certificates (GICs)
    - Government of Canada savings bonds
    - mutual funds
    - stocks

6. If you made an investment of $1000 in a savings account that paid 5 per cent *compound interest* annually (compound interest means interest paid on your deposit <u>plus</u> any accumulated interest to date), what is your best guess concerning how much money you would have in your account after 5 years?
    a) $1,076
    b) $1,126
    c) $1,176
    d) $1,226
    e) $1,276*

7. On average, how much money do you think people in your social network (friends, co-workers etc.) save or invest on a regular (ongoing) basis?
    - None
    - Up to 10 per cent of monthly income.
    - 10- 20 per cent of monthly income.
    - 20-30 per cent of monthly income.
    - 30-50 per cent of monthly income.
    - 50-75 per cent of monthly income is saved
    - Over 75 per cent of monthly income.
    - All

8. Joe starts saving $100 per month when he turns 19 years old. He puts the money in an investment that pays a guaranteed annual compound interest rate of 5 per cent per year. When Joe is 28 years old (exactly 10 years later), how much do you think Joe will have available to help him buy a car or put a down payment on a condo? Pick one answer from the following?
    a) About $11,000
    b) About $12,500
    c) About $14,000
    d) About $15,500*
    e) About$17,000

9. The "rule of 72" is a quick calculation that tells you how long it will take to double savings or investment dollars, assuming a constant (average) annual interest rate. For example, if the annual compound interest rate paid on your savings or investment is 5 per cent, then it will take 72/0.05 = 14 years (approximately) for your savings to double. Do you think the "rule of 72" is a useful way to help people decide how much they should save or invest?
    a) Yes
    b) No

10. Suppose that you have a savings account that pays 2 per cent annual interest. If the annual inflation rate is 3 per cent, after one year do you think you would be able to buy more than, the same or less than what you could buy today with the money in this account?
    a) More than what I could buy today
    b) The same as I could buy today
    c) Less than what I could buy today*

Appendix D: Demographic Information on Participants

Below we present the results from our experiments with respect to the demographic information we collected from participants. For each module above we conducted the following regression for our hypothetical $100 discount rate elicitation and across two experiments and treatments treatment:

$$DR = B0+B1 \times Gender + B2 \times Age + B3 \times Income + B4\ Credit$$

where R represents an individual's elicited discount rate, age represents a participant's age, Income represents their income (categorical variable in $10,000 increments) and Credit is a binary variable taking on a value of 1 if the individual owns a credit card and the individual carries a balance. The table below presents the coefficient and standard error for each regression.

	B1	B2	B3	B4
Hypothetical $100 (all experiments)	1.04 (0.80)	0.50 (0.42)	2.08 (2.31)	0.92 (0.87)
Default Bias Treatment 1	1.52 (1.20)	1.85 (1.68)	0.45 (0.44)	0.82 (0.78)
Default Bias Treatment 2	1.62 (1.43)	1.92 (1.69)	0.23 (0.19)	0.36 (0.33)
Default Bias Treatment 3	1.52 (1.40)	1.85 (1.29)	1.52 (1.24)	1.33 (1.09)
Default Bias Treatment 4	0.96 (0.84)	0.92 (0.82)	1.27 (1.19)	1.45 (1.37)
Save More Tomorrow Baseline	1.68 (1.48)	1.96 (1.850	0.45 (0.41)	0.67 (0.59)
Save More Tomorrow Treatment	0.05 (0.05)	1.20 (1.13)	1.48 (1.23)	1.96 (1.91)

As seen in the above table, we find no significant relationships between any of the demographic variables and the discount rates elicited. This holds not only for the variables listed above, but for all the demographic variables elicited in all experiments. These types of insignificant relationships exist for all data collected in our exit questionnaire.

Appendix E: Relationship between Behavioural Measures and Responses on Financial Literacy Questionnaire

Below we test the relationships between elicited discount rates and the questions in our financial literacy questionnaire. We perform non-parametric Wilcoxon tests across the distributions of discount rates elicited in module 6 based on individuals correctly answering the questions in the survey. The null hypothesis in these tests is that the distributions of discount rates across the populations are the same.

	Q 6	Q 8	Q 10
Hypothetical $100 Discount Rate	0.35	0.68	0.62
Session 2 Discount rate	0.74	0.29	0.30

We conduct a similar test on those questions that have binary choice. With respect to questions 2, as in our demographic data analysis, we find no relationship between employment and discount rate (Wilcoxon p=0.71). We obtained a similar result with respect to question 4 (the rule of 72): p=0.65. As such, across these questions we find no relationship between discount rates and the collected data.

With respect to the other questions, we conducted a regression testing the individual's response as a predictor of her discount rate. We find no significant relationships between participant's responses and the elicited discount rates (both when the individual responses are regressed and when the data from the complete experiment is pooled).