Compensated Discount Functions: An Experiment on the Influence of Expected Income on Time Preference*

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Abstract

This paper examines the open empirical question of whether subjects, when choosing among rewards received at different points of time, are influenced by their expected income levels at those times. Moreover, we seek to measure time preferences after compensating for possible income effects. Besides eliciting subjects' preference between standard delayed rewards, the experimental design also elicited their preferences over delayed rewards that are received only if the subject's income remains approximately constant. These preferences along with elicited subjective probabilities of satisfying the condition make the correction possible. We conducted the experiments in Iceland, where prompt availability of income tax returns enabled us to condition delayed rewards on income realizations. We find that background income affects preferences over unconditional delayed rewards. While most people exhibited present bias when comparing unconditional delayed rewards, subjects with stable income did not. The results are similar for the entire sample once we correct subjects' discount functions for income effects. This suggests that income expectations have an effect on choices between future rewards, and that this may account for some of the present-bias observed in experiments.

Keywords: time preferences, hyperbolic discounting, income expectations, rewards conditional on income realization

JEL Classification: C93, D03, D11, D90

1 Introduction

In order to make inferences from their trade-offs between delayed monetary rewards, experimental studies exploring the nature of time preferences typically presume that a subject's marginal utility for money is constant across time. However, several theoretical papers note that subjects may integrate these rewards with their baseline consumption levels (Olson and Bailey, 1981; Rubinstein, 2002; Frederick et al., 2002; Noor, 2009; and Gerber and Rohde, 2010). In this case, anticipated changes in marginal utility for money would influence their trade-offs between delayed rewards.[1] This is also related to the recent experimental and theoretical literature that accounts for an unavoidably uncertain future as a contrast to a certain present.[2] On the other hand, there is the "narrow bracketing" view that subjects treat experimental rewards in isolation from their background expected financial situation. It may be that integration with background plans is too difficult for people to do,[3] or that small rewards are often viewed as windfalls under different mental accounting and enjoyed separately from base consumption. This important open empirical question of whether background expected financial conditions matter for intertemporal choices is the subject of this paper. Furthermore, we show how to compensate for possible background marginal utility effects when measuring discount functions.

We conducted a lab experiment in Reykjavik, Iceland, using a random sample of individuals from the census, conditional on them living in post codes not too far from the lab.[4] The possibility of prompt access to individual

[1]A strand of the literature focuses on timing a consumption stream, or primary rewards (McClure et al., 2007), acknowledging that the value of timed monetary rewards can change with liquidity constraints and other financial frictions, as well as *in vivo* transfers.

[2]See Weber and Chapman (2005), Fernandez-Villaverde and Mukherji (2011), Baucells and Heukamp (2012), Andreoni and Sprenger (2012) and Halevy (2012).

[3]See some arguments along this line in pages 356-357 of Frederick et al. (2002).

[4]This feature of taking lab experiments to field subjects is similar to Andersen at al. (2008). Note that such subject pools very likely include more people with stable income than the canonical lab subjects of students. As consumptions of the latter are

tax information in Iceland allowed us to both observe subjects' income for the two years preceding and following the experiment.

In the first part of the experiment, we used a standard design of asking participants to choose between unconditional present rewards and unconditional rewards received one or two years later.[5] The elicitation was done through a series of binary choice questions, presented as a standard multiple price list. From this we derive the *uncompensated discount function*: for instance, if a subject is indifferent between \$100 in the current period and \$150 in t years, we obtain $D_u(t) = \frac{100}{150}$. This discount function is uncompensated in the sense that expected income changes can create a wedge between the indifference point and the subject's true underlying discount function.

In the second part of the design, we asked participants to choose between (a) unconditional present rewards and (b) rewards received one or two years later *conditional* on the subject's income staying "approximately" constant.[6] The idea is that approximately constant income corresponds to approximately constant marginal utility for money, and so the utility evaluation of the rewards is time-independent. Note that the evaluation of a conditional delayed reward depends on the utility from receiving the reward, the degree to which it is discounted due to temporal delay, and the subject's *beliefs* about the likelihood that the conditioning event will be satisfied. Eliciting the second element – the true discount function – is our objective. By assuming that the rewards are small relative to background

highly volatile (and steeply rising in a matter of years), any confound of income trends we document in our subsample is likely to be even more powerful for subject pools biased towards students.

[5] We used a front-end delay of one week to put both options on an even footing with respect to transaction costs, immediacy, or trust in the experimenters. What we label here as present rewards were specified to be paid a week after the experiment. Similarly, rewards labeled one and two years later were paid one year plus one week, and two years plus one week later.

[6] More precisely, we required that the after-tax inflation-adjusted income of the participant stayed within 4% of base income, for both the month and the year preceding the moment in question. See our online appendix for the translation of our actual questionnaire.

consumption, we assume approximate linearity of utility from the rewards. We elicit beliefs in an incentivized fashion as follows. First, we sought a good whose utility to the subject is plausibly independent of the marginal utility of money – we consider anonymous charity payments, to a charity of the subject's choice, possess this property. Next, we ask subjects to compare (a') a payment h made to a charity of the subject's choice at time t with probability α and (b') a payment h to the same charity at time t conditional on the subject's income staying roughly the same.[7] By observing preferences for various α, we uncover the desired subjective belief. With this in hand, we can then derive the underlying discount function (see Section 2 for details), which we label as the *compensated discount function*, to contrast it with the original uncompensated discount functions that do not take marginal utility expectations into account.

The above design allows us to address the fundamental question of whether expected income levels influence preferences for delayed monetary rewards. With further assumptions, we can make statements about how expected consumption levels (which we do not observe directly) influence these preferences. In particular, our results can be reinterpreted along these lines if we assume that (i) the baseline consumption level is determined by the subject's income, and hence does not change if the subject's income stays the same; (ii) subjects face frictions (these could be external borrowing and lending constraints or internal cognitive constraints) that induce them to consume small rewards at the time when they are received; and (iii) the money amounts we offer subjects are "small" relative to their background consumption, in which case money adds to the agent's background utility approximately linearly. Assumption (ii) is made in most experimental investi-

[7] As all our experimental questions, these choices were incentivized. At the end of the experiment exactly one binary choice problem was randomly selected for each subject, and the subject's reward was based on the choice in this problem. Hence, some subjects received rewards corresponding to anonymous charity contributions to their charities of choice.

gations of time preferences. Indeed, if subjects could freely transfer monetary amounts across time periods then they would only care about the discounted present value of their earnings, and appear to the experimenter as exponential discounters with the interest rate as the discount rate. The extensive evidence for non-exponential discounting in the literature is therefore evidence that the consumption of rewards may not be smoothed over time. Regarding assumption (iii), we consider it to be a good approximation, as the maximum reward a subject could earn in our experiment was approximately $165 (in 2010 currency), which is a small fraction of our typical subject's annual or even monthly income.

Our first set of results only uses the traditional experimental questions that use unconditional future rewards. Consistent with most existing studies, we find that in the full sample the estimated discount factor between one and two years from the experiment is significantly higher than the estimated discount factor between the time of the experiment and one year later.[8] In the quasi-hyperbolic "β-δ" framework, the estimated present-bias parameter β is 0.89 and significantly different from 1, while the estimated long-run discount factor δ is 0.92. We get a very different picture though when we look at subjects with stable incomes. In particular, when we restrict attention to subjects whose real annual income remained within a 10% range of current income in both of the two years following the experiment (31 of the 116 subjects), the discount factor between the first and second year after the experiment is almost identical to the discount factor between the present and one year after the experiment, and the estimated β is 0.97 and not

[8]Many experiments describe discount functions as hyperbolic, implying a present bias. See Frederick et al. (2002) for a review, and more recent evidence by Benhabib et al. (2012). Pender (1996) finds similar results in a field experiment using rice instead of money for rewards. Other papers find no evidence for hyperbolic discounting: see for example Harrison et al. (2002), Andersen et al. (2008) and Andreoni and Sprenger (2012). The experimental finding of present bias generated much theoretical work such as Laibson (1997), O'Donoghue and Rabin (1999), Gul and Pesendorfer (2001) and Fudenberg and Levine (2006), and numerous applications built on these models.

significantly different from 1 (while the estimated δ for this group is almost exactly the same as for the whole subject population). This means that those subjects whose real income remained stable after the experiment made choices consistent with exponential discounting, while other subjects on average made choices that revealed significant present bias.

We get similar results when, instead of realized income, we use subjects' expectations, elicited in an unincentivized survey, at the time of the experiment to identify those who expect stable income in the two years following the experiment. For example, for those subjects who expect to stay in their current job with more than 80% probability for the two years following the experiment, the estimated discount factor between one and two years from the experiment is not significantly different than the discount factor between the time of the experiment and one year later, and the estimated β is 0.94.

We also investigate subjects whose income increases significantly in the year following the experiment, but stays relatively stable afterwards.[9] Given that the expected marginal utility of extra income for these subjects (as long as they could foresee the said income pattern) is lower in both future years than at the time of the experiment, but about the same magnitude in the two future years, we expect a more significant present bias in these cases. In line with the theoretical predictions, the estimated β decreases to 0.79.[10]

To further examine the relationship between income stability and exponential discounting, in the second half of the analysis we investigated subjects' choices regarding conditional rewards, and derived their compensated discount functions. Our first finding is that the estimated compensated discount functions revealed future bias and negative discounting for a significant fraction of our subjects. While this is not unprecedented in the literature –

[9]In the standard model, a one-shot future reduction (resp. increase) in marginal utility of money predicts present (resp. future) bias in unconditional choices, confounding the true curvature of the utility function with uncertainty and trends in consumption.

[10]There were few subjects with a substantial reduction in income to allow a test of the corresponding prediction of higher estimated β.

Augenblick et al. (2013) find that 17% of their subjects are future biased over money, and 29% of them are future biased over effort[11] – there is a possibility that our experimental questions that featured probabilistic and conditional rewards were cognitively too difficult for some of our subjects, and this led to incorrect estimates of their preference parameters, outside the usual ranges of the latter. For this reason, we conducted our analysis based not just on the whole sample, but also on restricted subsamples of subjects whose answers conveyed compensated discount factors in the range typically found in the literature. We did this exercise two different ways. The first one involved simply censoring out subjects whose compensated discount factor in at least one of the two years, computed from their experimental responses, exceeded 1.1. The second method we used is more complicated, but it has the advantage of not having to choose a threshold discount factor level for the exclusion criterion. We fit a finite mixture of bivariate normal distributions on the one-period discount factors and the ones between years 1 and 2 maximizing the Bayesian Information Criterion, with discrete classification into clusters. The number of mixtures fitted was chosen by the same algorithm and the same criterion, also allowing for classification of observations as outliers (noise).[12] Out of the resulting three clusters, two show compensated discount rates in a reasonable range, consisting of 68.1% of the sample (79 observations overall). The likelihood of falling into either of these two clusters is positively correlated with the subject's education level. We emphasize though that we mainly report results for this subasmple as a robustness check, as our main results featuring compensated discount functions are qualitatively the same when using the entire subject pool.

We ran multiple tests of the compensated discount rates being equal

[11]See footnote 36 in Augenblick et al. (2013).

[12]This is an unsupervised algorithm in machine learning parlance. This also implies that the parameters cannot be chosen by cross-validation on test samples after training on training samples. That said, our findings are robust to changing the specifics of this implementation.

to the uncompensated ones (using their difference), or either being 1 (the benchmark of treating future and present equivalently). For β corresponding to present-bias in the quasi-hyperbolic discounting framework, we also tested for differences between the compensated and the uncompensated cases (using their difference), as well as either measure being different from 1. For all these, we employed two-sided t-tests using standard errors robust to heteroskedasticity, and also non-parametric Wilcoxon signed rank tests.

Our first finding is that compensated discount factors are higher than uncompensated ones, for the entire two-year time period covered in our experiment, but also across the two periods separately, in particular for subjects without income stability. This results suggests that experiments that do not take into account expectations regarding future income overestimate subjects' true impatience. Second, we find that compensated discount functions are significantly less present biased than uncompensated ones ($p = 0.24$ for the arithmetic difference being 0, but $p = 0.033$ with the signed rank test). The estimated β parameter, when using compensated discount factors, is higher than when using uncompensated discount factors, and not significantly different from 1, while it is significantly less than one when using uncompensated discount factors. This is true for the entire sample or the groups we labeled reasonable among the algorithmically selected ones or by using our imposed discount rate cutoffs.

To summarize, our two strands of analysis point in the same direction. Both suggest that people's preferences for monetary rewards are affected by their underlying (expected) income, and that their inherent impatience is less than what traditional experiments eliciting time preferences tend to find. When differences in expected future incomes are compensated for, the implied discount function becomes less hyperbolic, and closer to exponential.

There are three recent papers in the literature with a similar motivation as in our paper. Meier and Sprenger (2014), in a field study conducted over two years, find that measured time preferences of most subjects are relatively stable over time, and that changes in time preferences are not correlated with levels or changes in socio-demographic variables and in particular income changes. However, Dean and Sautmann (2014) and Carvalho et al. (2014) find evidence that changes in financial situation do affect subjects' choices over timed financial rewards. Dean and Sautmann (2014) provide both a theoretical analysis and supporting evidence based on a dataset involving weekly measurements of marginal rate of intertemporal substitution, as well as information on spending and income of subjects. Carvalho et al. (2014) investigate intertemporal choices of poor people before versus after payday, an event significantly affecting how liquidity constrained these subjects are. They find that before payday subjects are more present-biased than after payday when rewards are financial, but not when they are not monetary. The main difference between the above papers and our work is that our experiments were designed to directly investigate the role of future income expectations on choices involving timed rewards.

The rest of the paper is structured as follows. Section 2 presents the theory of rewards integrated with other income, including our formula for recovering primitives from elicited choices and subjective probabilities. Section 3 details the experimental design, section 4 the conduct. Section 5 details our empirical strategy, Section 6 gives the empirical results, while Section 7 concludes.

2 Theoretical background

Here we provide theoretical foundations for the experiments investigating conditional discount factors.

2.1 *Overview*

Suppose that subjects evaluate consumption using an expected discounted utility model with (uncertain) background consumption b_t, given a differentiable utility function $u()$. As we compute below, these subjects will evaluate an unconditional reward m received at time t by $D(t) \cdot E[u(b_t + m) - u(b_t)]$, that is, the discounted increase in expected utility due to the reward. Moreover, assuming that m is small relative to b_t, we can approximate the change in utility as $u(b_t + m) - u(b_t) = u'(b_t) \cdot m$. Therefore, the utility of an unconditional reward is

$$D(t) \cdot E(u'(b_t)) \cdot m.$$

Denote current income by b^*.

Traditional experiments elicit the present amount X_t^u which is just as good as a future unconditional reward, yielding the (approximate) equality

$$u'(b^*) \cdot X_t^u = D(t) \cdot E(u'(b_t)) \cdot m.$$

Define the *uncompensated discount function* by

$$D_u(t) = \frac{X_t^u}{m}. \tag{1}$$

Seeing that $D_u(t) = D(t) \cdot \frac{E(u'(b_t))}{u'(b^*)}$, it is clear that the uncompensated discount function correctly estimates the true discount function $D(t)$ if and only if $\frac{E(u'(b_t))}{u'(b^*)} = 1$ for all t, which does not hold for general expectations, and is the reason for the usual assumption of constant marginal utility $u'(b_t)$ across time in experiments that use $D_u(t)$ as an estimate for $D(t)$.

To compensate for income effects (non-constant marginal utility across time), consider payments that are paid only in the "constant income" event that $b_\tau = b^*$ for all $\tau \leq t$. Denote this event by s^t. Then such conditional rewards are evaluated by the discounted increase in expected utility:

$$D(t) \cdot p(s^t) \cdot u'(b^*) \cdot m.$$

So, if an agent states that X_t^c received today is as good as m received at t under the condition s^t, we obtain the equality $u'(b^*) \cdot X_t^c = D(t) \cdot p(s^t) \cdot$

$u'(b^*) \cdot m$, and thus,

$$\frac{X_t^c}{m} = D(t) \cdot p(s^t). \tag{2}$$

Since $\frac{X_t^c}{m}$ is observable, we need only elicit $p(s^t)$ to compute the true discount function D. A means of deriving such beliefs is to assume the existence of a commodity h whose utility $v(h)$ is independent of base consumption b_t and additively separable from the utility for money. We consider anonymous charitable contribution to the charity of the agent's choice to be a plausible example of such commodity. Then a charity payment of h at time t under the condition s^t is evaluated by[13]

$$D(t) \cdot p(s^t) \cdot v(h).$$

If the subjects exhibits indifference between such a charity payment h at time t under the condition s^t and an unconditional charity payment h made at the same time t but with objective probability α^t, then it is clear from the implied equality $D(t) \cdot p(s^t) \cdot v(h) = D(t) \cdot \alpha^t \cdot v(h)$ that

$$\alpha^t = p(s^t).$$

Now the *compensated discount function* can be defined by

$$D_c(t) = \frac{X_t^c}{m} \cdot \frac{1}{\alpha^t}, \tag{3}$$

and indeed $D_c(t) = D(t)$.

In what follows we describe the model more precisely.

2.2 *Model*

Time is discrete and with finite horizon, $\mathcal{T} = \{0, 1, \ldots, T\}$. The set of possible (inflation-adjusted) future base-consumption levels at any time t is given by the finite set $B_t = B \subset \mathbb{R}_+$ with generic element b. This corresponds to

[13] There is no reason that $v(h)$ must be discounted by D, but this is without loss of generality for our purpose.

assuming the the set of possible income levels are bounded and measurable in the unit of monetary exchange. Because base-consumption will be uncertain, the set B is the period t state space. Period 0 base-consumption is given by $b^* \in B$ and below we will assume that this is known. The t-horizon state space is $\mathcal{S}(t) = \Pi_{i=1}^t B_i$, with generic element $s^t = (b_0, \ldots, b_t)$. The full state space is $\mathcal{S} = \cup_{t\in\mathcal{T}}\mathcal{S}(t)$.

The set of (inflation-adjusted) monetary prizes is an interval $\mathcal{M} = [0, M]$. Writing $B_t = \{b_t^0, \ldots, b_t^{N_t}\}$ with $b_t^0 \leq \ldots \leq b_t^{N_t}$, we let M be larger than the grid size for base consumption.[14] In what follows, we will require that M be small, thereby also requiring a fine state space.

A state-contingent reward is a function $x : \mathcal{S} \to \mathcal{M}$ that delivers a prize $x(s^t) \in \mathcal{M}$ at date t conditional on the realization of $s^t = (b_0, \ldots, b_t)$. The set of all state-contingent rewards is denoted X. The primitive of our analysis is a preference $\succsim$ on X.

We maintain relatively weak assumptions in this section just to show that our experimental procedure is not tied to a particular functional form for discounting. But the reader should keep in mind that all the experimental analysis in this paper will be based on the well-known beta-delta model.

Basic assumptions – The subject is assumed to evaluate future consumption according to a discounted utility model where uncertainty is evaluated according to (state-dependent) subjective expected utility theory. Instantaneous utility is given by a strictly increasing and differentiable function $u : \mathbb{R}_+ \to \mathbb{R}$ with a differentiable inverse. The discount function is $D(t) > 0$ and satisfies $D(0) = 1$ but is not necessarily strictly monotone or restricted to take values less than 1.[15] The subject's prior (over future base consumption) is a probability measure p on $\mathcal{S}(T)$.

Integration assumptions – Assume that the subject integrates state-

[14] That is, $M > \max\{\left|b_t^i - b_t^{i+1}\right| : t \leq T,\, i < N_t\}$.

[15] If $D(t) > 1$ is permitted then in order to ensure the existence of present equivalents we assume that u is unbounded.

contingent rewards with her anticipated base consumption and completely consumes any prize in the period and state that it is received. The presumption here is that the rewards in $\mathcal{M}$ are small enough for this to be an acceptable assumption. It follows that the discounted expected utility due to a state-contingent reward x given beliefs p is

$$U(x) = u(b^*) + \sum_{(b_1,\ldots,b_T)\in\mathcal{S}(T)} \left[\sum_{t=0}^{T} D(t)u(b_t + x(b_0,\ldots,b_t))\right] p(b_0,\ldots,b_T). \tag{4}$$

Full support assumption – We assume that $p(s^T) > 0$ for all s^T, that is, unconditional beliefs on each B_t have full support, and positive probability is assigned to future base consumption staying the same as current consumption b^*. Given the strict monotonicity of u, this is equivalent to assuming behaviorally that for any s_T, there exists some prize $m > 0$ such that the state contingent reward x_{m,s^T} that pays m at T in state s^T and 0 otherwise satisfies

$$x_{m,s^T} \succ \omega,$$

where ω denotes the state-contingent rewards that yields 0 at all t and s^t.

2.3 *Deriving* D *from* $\succsim$

The primitive preference $\succsim$ on X has the representation (4) where the utility of a reward $x(s^t)$ received at time t conditional on s^t is *state-dependent.*[16] That is, a dollar received in a given period depends on base consumption b in that period, so that the instantaneous utility in that period is $u(b+1)$, and thus dependent on b. This might lead one to suspect that the representation (4) lacks desirable uniqueness properties, and in particular the key component of interest, the discount function D, may not be unique. This would be problematic since it would imply that D is not pinned down by preferences $\succsim$,

[16] Though the preference over consumption streams has a state-independent representation, the induced representation for the preference over state-contingent rewards is state-dependent.

and in particular, there is no meaningful sense in which it can be extracted from $\succsim$ in any experiment.[17] Therefore, we must establish that any discounted expected utility representation for $\succsim$ must have a unique D. This is the content of the proposition below, the proof of which is relegated to the appendix.

Proposition 1 *The prior p and the discount function D are uniquely determined by $\succsim$.*

Having established the possibility of eliciting p and D from $\succsim$ we now outline a procedure for doing so.

Let x_{m,s^t} be the reward that yields prize m at time t conditional on constant base consumption $s^t = (b^*, \ldots, b^*)$. Denote by $\psi(x_{m,s^t})$ the reward that yields a prize immediately such that $\psi(x_{m,s^t}) \sim x_{m,s^t}$.[18] Identify the immediate prize with $\psi(x_{m,s^t})$. The representation (4) implies that

$$u(b^* + \psi(x_{m,s^t})) - u(b^*) = D(t)p_t(s^t)[u(b^* + m) - u(b^*)].$$

Note that since u is a strictly increasing differentiable function with a differentiable inverse, $\psi(x_{m,s^t})$ is a strictly increasing differentiable function of m that takes the value 0 when $m = 0$. Taking a derivative of the above expression with respect to m yields

$$u'(b^* + \psi(x_{m,s^t}))\frac{\partial \psi(x_{m,s^t})}{\partial m} = D(t)p_t(s^t)u'(b^* + m).$$

[17]For instance, in state-dependent subjective expected utility, a function f that takes states into prizes is evaluated by $\sum_s u(f(s), s)p(s)$. In this representation, the prior is not unique and thus has no behavioral meaning. We could take any $\alpha_s > 0$ for each s, take a monotone transformation of $u(\cdot, s)$ given by $v(\cdot, s) = \frac{1}{\alpha_s}u(\cdot, s)$ for all s, and adopt a different prior given by $q(s) = \frac{\alpha_s}{\sum_s \alpha_s p(s)}p(s)$ for all s. Then it is easy to see that the utility function $f \longmapsto \sum_s v(f(s), s)q(s)$ represents precisely the same preference as before. In contrast, when u is state-independent (as in Savage's subjective expected utility theory), *every* subjective expected utility representation for the preference must share the same prior p, and thus p is uniquely pinned down by preferences. It can be elicited by asking the agent to choose between bets.

[18]The existence of such a reward is implied by the unboundedness and continuity of u. Its uniqueness is implied by the strict monotonicity of u.

Evaluating at $m = 0$ gives $\frac{\partial \psi(x_{m,s^t})}{\partial m}|_{m=0} = D(t) p_t(s^t) \frac{u'(b^*)}{u'(b^*)}$ and so

$$\frac{\partial \psi(x_{m,s^t})}{\partial m}|_{m=0} = D(t) p_t(s^t).$$

In practice we can rely on an approximation via the observation that for small m,

$$\frac{\partial \psi(x_{m',s^t})}{\partial m}|_{m'=0} \approx \frac{\psi(x_{m,s^t}) - \psi(x_{0,s^t})}{m - 0} = \frac{\psi(x_{m,s^t})}{m}.$$

Hence

$$\psi(x_{m,s^t}) \approx D(t) p_t(s^t) m$$

Note that $\psi(x_{m,s^t})$ and m are observable, so if we can identify $p_t(s^t)$ (for instance, as described earlier) then we can find $D(t)$.

2.4 Discussion: *Interpretations and Assumptions*

The theoretical considerations above suggest an experimental design, that will be presented in detail in the next section. To be able to interpret the experiment as measuring how expected background consumption affects time preferences, and as a valid way of uncovering the underlying time preferences, one needs to make the following assumptions. We emphasize that even if the assumptions below do not hold, the experimental design is still valid in testing whether expected future income influences preferences for delayed monetary rewards.

1. Rewards are consumed fully in the period of receipt.

2. The marginal utility of rewards is linear for the range of rewards that we consider.

3. In case income remains unchanged, background consumption does not change.

The first assumption is a standard assumption in the traditional time preference experimental literature. It conflicts with the consumption smoothing property implied by the standard life cycle model, a testable implication of which is that all rewards should be ranked according to its present value and elicited discount functions must be exponential (with a discount rate equal to the market rate of interest). Indeed some studies find no evidence of hyperbolic discounting for money while simultaneously confirming hyperbolic discounting for consumption (Augenblick et al 2013). But the fact that we, like many studies, reject nonexponential discounting it follows that perfect smoothing is not going on.[19] Zero smoothing of small rewards then serves a standard benchmark, which can be justified in several ways. While many theoretical models assume that individuals will smooth their consumption across time, models of mental accounting or dual-self models (e.g. Fudenberg and Levine [2006]) predict that income from experiments may be consumed when received. Furthermore, Andersen et al. (2008) and Booij and van Praag (2009) provide empirical evidence that experimental payments are consumed when they are received.

In our experiment a period is *one year*, and thus the rewards we consider (the maximal possible reward was approximately $165 in 2010 currency) are very small compared with annual consumption. This speaks to our second assumption, which is standard in the traditional time preference literature, but see Andersen et al (2008) for a recent critique.

The third assumption is necessitated by the fact that consumption is difficult to observe directly. Consistent with this assumption, empirical studies show that consumption tracks income over the life cycle (Browning and Crossley (2001)), pointing both to possible frictions in consumption smoothing and to possibly boundedly rational behavior.

We note that even if the assumptions relating with consumption do not hold, the experimental design is still valid in testing whether expected future income influences preferences for delayed monetary rewards. Such dependence would drive a wedge between our elicited compensated and

[19] The heterogeneity of results across experiments could potentially be due to income effects that are not accounted for.

uncompensated discount functions.

3 Experimental Design

The experiment used a questionnaire with three sections, all three translated fully in the online appendix. In the first one we asked subjects to choose between rewards received at different times. Some of the future rewards involved payments that were conditional, i.e. only received if the subject's real income remains "approximately constant" (defined shortly) up to the time of payment. The second part provided subjects binary choice questions involving a payment to the subject's charity of choice either with an exogenously given probability or under the condition the subject's real income remains "approximately constant". Finally, the third part featured an unincentivized questionnaire eliciting demographic and financial information about subjects, as well as subjective beliefs about future income. All in all, this amounted to six sets of decisions that all included a range of binary choices before they turned to a survey – the small number of questions helps keeps the cognitive burden low on the subjects. During instructions at the beginning of the experiment, we explained to the subjects how a dice roll would determine which one of those six choice categories will be used to generate payoffs, and for that choice one random line from the range of binary choices, according to their expressed preferences.[20]

The first section started with questions involving only unconditional payments. A generic question asks for the subject's preference between[21]

(i) a "later" payment of 20,000 Icelandic Kronur (ISK), paid to the subject at t years plus 1 week later,

(ii) a "present" payment in the amount of x paid 1 week later,

[20] It is a dominant strategy to make the binary choices truthfully, with the caveat that if a subject is exactly indifferent between the two options then the choice can be either of them.

[21] In 2010, 20,000 ISK was worth approximately $165.

where t=1 or 2 years, and x ranges from ISK 200 to ISK 22,000 in steps of ISK 200. This series of binary choice questions is presented as a standard multiple price list, that is, in the form of a table.[22] Although subjects could indicate their preference in each cell of the table, for their convenience they were allowed a shortcut where they could indicate two consecutive cells on the table where preferences switch from favoring the present reward to preferring the later reward. Consequently, the subject's indifference point was captured within an ISK 200 interval.

In order to elicit compensated discount functions (and specifically to bring equation (2) into play), we next asked analogous binary choice questions where some payments are paid on the condition that the subject's income remains "approximately constant". We formally defined this condition to consist of two requirements:

(a) the price-indexed disposable annual income of the subject during the year following the experiment (in case of a 2-year delayed reward, in both years) is within 4% of the annual income in the 12 months preceding the experiment;

(b) the price-indexed disposable monthly income of the subject in the last month before payment occurs is within 4% of the monthly income in the month preceding the experiment. In the case of payments two years from the experiment, this has to hold true both one year after the experiment and two years after the experiment.

The idea behind part (a) is that the general income level of the subject remains the same, relative to the time of the experiment, while the motivation for part (b) is to make sure that the subject's overall financial situation is similar to the time of the experiment. We refer to both conditions holding

[22] This is equivalent to a Becker-DeGroot-Marschak (BDM) procedure. Like most of the related literature, we opted for the list of binary questions because we believe the original BDM procedure (in particular, understanding why truth-telling is weakly dominant) to be cognitively more demanding for the subjects. This is also in accordance with our experience from several pilots conducted before the experiment.

together as the subject's income situation remaining constant.

Given this definition, the next set of binary choice questions asked subjects to indicate their preference between:

(i') a "conditional later" payment of 20,000 Icelandic Kronur (ISK) paid to the subject, after t years plus 1 week, *if* income remains approximately constant.

(ii') a "present" payment in the amount of x paid 1 week later,

where as before, t=1 or 2 years, and x ranges from ISK 200 to ISK 22,000 in steps of ISK 200.

Section II obtained the data that allows us to exploit equation (3). The section started with the subject choosing a charity from a list of well known and established charities with different objectives that were briefly described to the subjects. Subjects were told that the forthcoming questions involve rewards in the form of payments to their charity of choice. They were then asked to indicate their preference between:

(i") a "conditional charity payment" of ISK 20,000 to the subject's charity of choice, after time t years plus 1 week, *if* income remains "approximately constant",

(ii") a "random charity payment" of ISK 20,000 to the subject's charity of choice, after time t years plus 1 week, with exogenous probability p,

where t=1 or 2 years, and p ranges from 0.01 to 1 in steps of 0.01. This series of questions was presented in the form of a table as in Section I.

In Section III, subjects were asked to fill out a survey, which asked them, among other things, for their bank information in order to transfer their payments. Using the one week delay in payments, as opposed to exactly at the time of the experiment, as well as one and two years later, allowed us to use the exact same procedures and payment methods, regardless of whether the rewards were delayed or not. This section also included questions on the subjects' social and economic background, as well as expectations on their future economic situation. It also included a direct but unincentivized question on how likely they think their yearly income remains approximately the same one and two years following the experiment. We also elicited subjects' probability assessments of entering a new job by one and two year's time after the experiment, and their probability assessments on losing their current jobs.

4 Experimental Procedures and Background

4.1 Experimental Sessions

The experiment took place on June 9th and 10th of 2010. Recruitment was conducted by phone from a random sample of Icelanders between the ages of 20 and 45 living in western or central Reykjavik, specifically post codes 101, 105 and 107. The sample was collected from the census by Skýrr, an Icelandic IT company and frequent government contractor. Subsequently, the subjects' phone numbers were collected manually through ja.is, the online Icelandic Telephone Directory. The experiment was conducted in groups simultaneously in one location, specifically a lecture hall at the University of Iceland. The hall had a podium and an overhead projector used in the presentation of instructions, which was carried out by one of the researchers (photos of the location are available upon request). Before starting, the subjects were asked to read and sign a consent form. They were also asked not to talk to each other and informed that if they had questions they should raise their hand, rather than speak up, and they would be assisted individually by a researcher or an assistant. Each session consisted of approximately 15 subjects and took a little bit over one hour. Outside the classroom we set up four dice rolling stations at which assistants reported the randomized outcome of the subjects' dice roll and computerized randomization process.

Before the experimental session in 2010, we conducted several small (10-25 subjects) informal pilots in 2007-2009. These pilots featured similar questions as the experiment, but subjects only received a fixed compensation for participating, independent of their answers. These pilots were mainly used to fine-tune how to effectively explain the questions in the experiment.[23]

4.2 Payment Process

As online banking is widespread in Iceland, subjects were paid by bank transfer. In all instances, both for pilots and the experiment itself, subjects received their payments. This happened in the vast majority of instances at the scheduled time. In a few instances with illegible account numbers, payments made with a few days' delay after quick follow-up e-mails or phone calls. Payments were initiated by the finance division at the University of Iceland. In 2010, 48 subjects in the experiment received payments, on average ISK 14,823. In 2011, 17 payments were made, in the amount of ISK 20,673. 12 of those went directly to participants, but five went to charities of their choice. In 2012, 13 individuals received a payment of ISK 21,891 each[24].

4.3 Income Verification

We chose Iceland for the experiment because of the prompt availability of comprehensive income-tax information due to a pay-as-you-earn system where the income tax is continuously withheld at source. That is, the lion's share of income-tax revenue in Iceland is collected monthly, and the Directorate of Internal Revenue (DIR) receives fairly accurate accounts of each individual's income in a timely fashion. For this reason, we signed

[23]We also conducted two small post-experiment pilots, one in 2011 and one in 2012, with slightly altered questionnaires and instructions as the ones used in the experimental session in 2010, to investigate whether these design changes lead subjects to a better understanding of the questions involving conditional rewards. As we did not find any evidence for this, these post-experimental pilots did not lead to a subsequent incentivized experimental session.

[24]These delayed payments were inflation-adjusted equivalents of ISK 20,000 at the time of the experiment.

a contract with the DIR on February 12th 2010, in accordance with the Icelandic Data Protection Act 77/2000 and a notification to the Icelandic Data Protection Authority (S4052). According to this DIR contract and the subjects' informed consent, we did not obtain direct information on subjects' incomes. Instead, income *changes* were calculated by the DIR staff, and they sent us the percentage changes in subjects' monthly as well as yearly incomes, for the specific months and twelve month periods. This was done using the latest information available in the DIR systems, which is generally fairly complete by the 18th of the following month, with only minor adjustments after that.

According to our contract, DIR calculated and delivered the income changes at three points in time. During the week after the experiment took place, as well as one and two years later. [25] Income changes were adjusted for changes in the Consumer Price Index (CPI) which is published by Statistics Iceland by the second to last day of the reference month.

4.4 Economic Situation Around the Time of the Experiment

The seemingly flourishing economy of Iceland suffered a major meltdown less than two years before our main experimental session, when the country's three largest banks collapsed and were nationalized. In a widely-viewed televised address, Prime Minister Geir Haarde announced to the country: "(T)here is a very real danger, fellow citizens, that the Icelandic economy, in the worst case, could be sucked with the banks into the whirlpool and the result could be national bankruptcy" (Prime Minister's Office, 2008). Although a sovereign default did not follow, this is indicative of the volatility and uncertainty of the situation. During the following months, hundreds of firms in the country declared bankruptcy. The announcement of the crisis triggered international consequences, including a decision by the United

[25] We alerted DIR of each deadline with two week's advance notice, and delivery and payments went through with no delay.

Kingdom to freeze the assets of one of the three large banks (Landsbanki), emergency funding from the International Monetary Fund, protests and a subsequent fall of the government in February 2009.

This was a dramatic macroeconomic shock that affected the entire population of this small open economy with its own currency and for which exchange rates and prices changed suddenly and dramatically. Although the experiment and the post-experimental payments took place after the collapse, and our design in principle remains valid no matter what expectations subjects have regarding future income, the crisis should be noted as many subjects may have felt considerable uncertainty as to how the economy would adjust in the coming years. Iceland is one of the world's smallest currency areas, making the Icelandic krona very vulnerable, which affects the price level and real wages. From the time of the experiment and to the payment dates one and two years later, the price level rose by 4.2% and 9.9% respectively and the real wage rate increased by 2.7% and 4.1% respectively. All in all, it can thus be said that most subjects in our sample faced considerable uncertainty at the time of the experiment regarding their future income.

Our means of deriving compensated discount functions is immune to the economic turbulence as long as subjects use discounted expected utility with respect to some beliefs about the future. To the extent that the ambiguity about the future led subjects to assess the future uncertainty and beliefs in an inconsistent way, it would be revealed to us through unreasonable compensated discount functions.

5 Computation of Discount Factors and Statistical Analyses

Recall our notation X_t^u and X_t^c in Section 2 for the present payments that are indifferent to a later conditional and unconditional payment at t respectively.

For simplicity, we estimate these as the lowest payment that the subject indicates as superior to the later payment. For the questions involving only unconditional rewards, if the later payment was paid t years after the experiment, this gives us an uncompensated discount function $D_u(t)$ as defined in equation (1). It will be convenient to define uncompensated discount factors between t and $t+1$:

$$D_u(t, t+1) = \frac{D_u(t+1)}{D_u(t)}.$$

Note that $D_u(0) = 1$ so $D_u(0,1) = D_u(1)$. When interpreting these parameters in the standard $\beta - \delta$ framework, we set $\delta_u = D_u(1,2)$, and

$$\beta_u = \frac{(D_u(1))^2}{D_u(2)} = \frac{D_u(1)}{D_u(1,2)}.$$

To compute compensated discount functions $D_c(t)$, defined by equation (3), we first define p_t as the probability that a subject expects her income t years after the experiment to be approximately the same as at the time of the experiment (as defined in Section 2 and in the experimental instructions). Specifically, this is coded as the lowest reported probability with which a probabilistic contribution to her charity of choice t years after the experiment is indicated as superior to a contribution of the same amount at the same time to the same charity, conditional on her income staying approximately the same. The probabilities computed this way, from incentivized experimental questions, strongly correlate with subjects' reported probabilities of stable income in Section III of the experimental questionnaire, although the latter, unincentivized measures are noisier. Linear regressions of the unincentivized responses on the incentivized ones yield slopes 0.602 and 0.442 with an R^2 of 0.3368 and 0.2154, for the two time horizons respectively.

Given p_t, and X_t^c, which denotes the amount of present payment that makes the subject indifferent to a conditional payment of ISK 20,000 t years after the experiment, the estimated compensated discount function is

$D_c(t) = \frac{X_t^c}{p_t \cdot 20{,}000}$. As with uncompensated discount functions, we define the discount factor $D_c(t, t+1) = \frac{D_c(t+1)}{D_c(t)}$, $\delta_c = D_c(1, 2)$, and $\beta_c = \frac{(D_c(1))^2}{D_c(2)}$.

For the possible reasons outlined in the introduction – namely the possibility of model misspecification and the possibility of imperfect responses by subjects to the more involved conditional questions – the compensated discount functions computed for a subset of our subjects were not plausible. For example, while it is plausible that a subject may exhibit negative discount rates (that is, $D_c(t) > 1$), it is implausible that discount functions may be steeply upward sloped, as we observed in some cases. Indeed, several instances of implausible discount functions were typically the result of hard-to-rationalize choices such as preferring a reward m at time t with probability $\alpha < 1$ to a reward m at t for sure.

In light of this, we proceed by following two separate strategies to analyze our predictions for uncompensated and compensated answers. First, for the simpler, uncompensated choices, we report discount functions by strata of income changes, with different predictions under our assumptions that baseline income levels matter for time preferences over monetary rewards. Besides the full sample, those subsamples are (a) people who experienced relatively stable income for two years after the experiment and would thus be expected to show less present bias under the proposed theory, (b) those who have stable income for two years before and after the experiment and are thus assumed to be individuals with even greater stability of income and thus even less confound in the conventional measures of present bias, (c) those who, at the time of the experiment, assess their probability of having a new job to be small and should thus show a smaller present bias than the full sample, (d) those who, at the time of the experiment, assess the probability that their income remains stable for the two years following the experiment to be higher than 50%, and finally (d) those individuals whose realized income rises in the first year after the experiment took place but plateaus after that, who should, according to theory show greater present bias than the full sample or any of the subsamples described above.

Second, for the compensated questions, we repeat our analysis restricted to subjects with revealed compensated discount factors around the range typically found in experiments. We do this two alternative ways. The first one involves restricting the sample to individuals with both $D_c(0,1)$ and $D_c(1,2)$ being below 1.1. In the second, and technically more involved method, we do not impose a hard upper bound on compensated discount factors, as an exclusion criterion. Instead, we employ the tools of statistical cluster analysis to identify (latent) classes of subjects in the data and focus on groups with ranges of discount factors considered usual. We choose to define clustering in terms of the discount factors $D_c(0,1)$ and $D_c(1,2)$ (the latter only implied by $D_c(1)$ and $D_c(2)$).

For the purposes of clustering, we assume jointly normally distributed discount factors, within an unobserved k class of subjects. Allowing for noise (outliers), the likelihood function that is numerically maximized is

$$\prod_{i=1}^{n}\left[\frac{\tau_0}{V}+\sum_{k=1}^{G}\tau_k\phi_k(\mathbf{x}_i|\theta_k)\right], \tag{5}$$

where V denotes the hypervolume of the data region, data can come from different distributions or simply be noise, which have respective probabilities τ_k (and thus of course $\tau_k \geq 0$ and $\sum_k \tau_k = 1$). The algorithm initializes with noise estimates coming from a nearest-neighbor method and hierarchical clustering applied to the rest of the data (with a simple maximization using the EM algorithm), and the EM algorithm alternating Bayesian updating conditional on the parameter estimates (Expectation step) and maximizing in the parameters conditional on the classification probabilities (Maximization step).

In equation 5 the likelihood contribution for an observation comes from assuming that the densities of the discounts factors follow a bivariate normal distribution. This procedure is conditional on the number of clusters, G. This we let be chosen to maximize the Bayesian Information Criterion (BIC),

$$BIC = 2 \cdot \log L(\mathbf{x}, \theta^{\star}) - (\#\text{parameters}) \log n. \tag{6}$$

Formal hypotheses We conduct a family of tests over two important variants of two measures, using a parametric and a nonparametric test, both two-sided and also one-sided about the economically interesting differences.

Our first set of tests compare first-year and second-year discount factors. The tests are done for both compensated and uncompensated, so we drop the superscript on D below to simplify exposition.

- $H_0 : D(1,2) = D(0,1)$, corresponding to subjects being exponential discounters.
- $H_1 : D(1,2) > D(0,1)$, corresponding to subjects exhibiting present bias in their time preferences.

We also report test results of no present bias without taking the stance on which way a deviation could occur. This amounts to a two-sided test against the alternative hypothesis of inequality.

Our preferred parametric test is a paired t-test, testing the null hypothesis that $H_0 : E[D(1,2)] = E[D(0,1)]$ against the alternative $H_1 : E[D(1,2)] > E[D(0,1)]$ or $H_1 : E[D(1,2)] \neq E[D(0,1)]$. The test makes the usual assumption that the discount factors are normally distributed in small samples, or the samples are large enough that the asymptotic approximation is good enough. The nonparametric test of our choice is a sign test, testing the medians without assuming the two variables have the same distribution: comparing the null hypothesis that $H_0 : med[D(1,2)] = med[D(0,1)]$ against the alternative $H_1 : med[D(1,2)] > med[D(0,1)]$ or $H_1 : med[D(1,2)] \neq med[D(0,1)]$.

We conduct the tests both for the uncompensated and the compensated discount factors.

Finally, we repeat the exact same test procedures for another parametrization of present bias, namely the transformation of the discount factors into the common $\beta = \frac{D(0,1)}{D(1,2)}$ parameter being larger or less than one.[26] Thus for the t-tests the null is $H_0 : E[\beta] = 1$ (no present bias on average) against the alternatives of $H_1 : E[\beta] < 1$ or $H_1 : E[\beta] \neq 1$, while for the sign tests, the analogues with medians. Again, we conduct the tests separately for uncompensated or compensated measures.

6 Experimental Results

6.1 Uncompensated Discount Factors

The bottom pane of Table 1 shows the difference of first and second year uncompensated discount factors $(D_u(1,2) - D_u(0,1))$, as well as the implied β and δ parameters, with the strata by income stability around the time of the experiment and times of payment in separate columns. There are 31 subjects in column 2, whose realized annual real income in both years after the experiment stayed within 10% of real income at the time of the experiment. Not surprisingly, these are slightly older subjects, more likely to be employed, but also better educated and there are more females than in the full sample (The top pane of Table 1 shows summary statistics using our survey in Section III). Reassuringly, these subjects were much less likely to expect job changes after the experiment.[27] Column 3 focuses on subjects

[26] Of course, for the parametric test the transformation can matter because of the approximation being better or worse in our finite samples. The nonparametric test is indifferent to such a transformation.

[27] Those in Column 2, whose realized income stayed stable for two years after the experiment, expected to be in a new job one year (two years) after the experiment with 20.0 (25.6) percentage point more likelihood than those not having such stable income, and this difference has a p-value of 0.003 (0.00). For those in Column 3, who we categorized as having stable income both before and after the experiment, these contrasts with the rest of the sample are 26.8 and 27.1 percentage points, respectively, in the same direction (with

whose real income was stable both before and after the experiment. Because most of our subjects experienced turbulent income in the years before the experiment, due to the financial crisis and the subsequent recession, we define this category as the subjects whose real income in both years before the experiment was within 20% of real income at the time of the experiment, and whose real income in both of the two years after the experiment was within 10% of real income at the time of the experiment. Even this more permissive criterion for the two years before the experiment results in only a small number of subjects (10, with 9 females) being in this category. They are also better educated and slightly older.

As shown in the bottom pane of Table 1, the difference between the second and first year discount factors is large and highly statistically significant for the entire sample of 115.[28] The overall sample shows considerable present-bias (average β is 0.895), and a reasonable amount of impatience (on average δ being 0.922). In contrast to this, for those 31 people whose incomes were stable for two years after the experiment, the difference in discount factors is statistically insignificant, with a point estimate close to 0, and the implied β is only insignificantly below 1, with a point estimate of 0.972. On the other hand, the estimated δ (0.924) is essentially the same as for the whole subject pool. We see the same pattern for those subjects whose income was stable both before and after the experiment.

This difference of mean present-bias parameter β between those with stable income after the experiment and the rest of the subjects is statistically significant at the 5% level ($p = 0.031$), and robust to controlling for other differences between this group and others. Applying the tests in a regression

both p-values being 0.00).

[28]The observation numbers differ because one subject left question 2 in section II of our questionnaire blank. This is what identifies the raw, unconditional (and thus uncompensated) two-period discount factor, so constructs and contrasts involving that measure always have one fewer observation (even within columns labeled $N = 116$ in Tables 1 and 2).

framework, the mean difference in β declines only slightly, from 0.108 to 0.1, when demographic controls from Table 1 are included, to control for selection on observables.[29] However, this exercise is informative only if any remaining selection on unobservables has an effect comparable to that of the observables. In the framework of Oster (2014) for linear regressions, we can calculate that the improvement of the model fit R^2 by 5% when controlling with observables implies that the group with stable income has the larger β unless (similarly confounding) unobservables could improve R^2 by 50% or more. Such a huge increase in explanatory power is not very plausible, thus even this more cautious perspective leads us to the conclusion that incomes stable after the experiment are associated with less present bias.[30]

We can contrast these results with the 11 subjects who experienced a relatively large real income rise (more than 10%) in the first year after the experiment, but then saw their incomes stabilized (remained within 10% of the income in the first year after the experiment).[31] If these subjects foresaw this income path,[32] their expected marginal utilities for monetary rewards in both of the two years following the experiment are lower than at

[29]For this we also include those 8 observations who have BMI, employment or number or children data missing. These observations enter with value zero for the respective variable, but also with an extra indicator in the regression to take out their group-specific mean separately. An alternative model without these controls have less explanatory power but yield a stable-unstable difference only larger than the unconditional point estimate.

[30]All analogous calculations are collected in Appendix Table 1. This table contrasts measures of time-preference for these groups in univariate and multivariate regressions to show how robust these differences are. With explanatory power taken into account, the table also reports the other extreme bound on the contrast from Oster (2014), as well as the minimal explanatory power a model with unobservables would need to have to call the sign of the unconditional point estimate confounded. These calculations assume that unobservables correlate with the group indicator “in exactly the same direction” as observables ($\delta = 1$ in that framework, not to be confused with the time-preference parameter). The bounds on the contrast come from assuming that the all-inclusive models could achieve the theoretical maximum $R^2 = 1$.

[31]Without a one-time change in marginal utility of money the model does not make any relevant predictions for uncompensated discount functions.

[32]This is reasonable in many instances, e.g. for those finishing school or for some other reason expecting to get into a higher-paying job.

the time of the experiment. This should imply more present bias than for the rest of the subject pool (without compensating for the confound of the income process, of course). We find some evidence for this, as the estimated $D_u(1,2) - D_u(0,1)$ difference rises to 0.223, and the estimated β decreases to 0.792 for this subsample, although we have no power in this small sample to establish these changes as statistically significant.

We get similar, although weaker, results if instead of looking at subjects with stable realized incomes, we restrict attention to subjects with reported unincentivized beliefs in Section III reflecting high expected probability of stable income. Column 4 on Table 1 focuses on the 32 people who estimated the risk of have a new job in two years to be less than 20%, while Table 5 focuses on the 24 subjects who believed that the probability of their incomes being roughly the same in both subsequent years as in the year before the experiment to be more than 50%. The estimated β for these subsamples are 0.94 and 0.93. One reason these estimates might be closer to the estimated β from the full sample is that at least some subjects have trouble reporting their expectations in terms of probabilities.

6.2 Compensated Discount Factors

6.2.1 Restricted Samples

The noise and clusters resulting from the procedure in Section 5 are summarized in the top pane of Table 2. The best-fitting distribution is one with no correlations between the two discount rates in any cluster, and three clusters with different means and variances for the normal distributions, and some outliers (noise).[33] Descriptive statistics for clusters are shown in the middle pane of Table 2. The first two clusters, containing 79 of the 116 subjects,

[33]The conclusions are robust to different initializations of the clustering. Though the number of clusters might be higher with a different initialization (e.g. initializing the expected outliers differently), the clusters with "reasonable" discount factors largely overlap, as do the vector of noise-indicators.

have compensated discount factors mostly in a range typically found in experiments. These more standard clusters of subjects are slightly better educated, on average (see Table 2), which is consistent with the hypothesis that these subjects understood the questions involving conditional rewards better, but the overlap between all clusters is apparent for any covariate. For comparison, he rightmost column collects the same descriptives for the subsample with compensated discount factors below 1.1 for both periods.

6.2.2 Main results

Comparisons of uncompensated and compensated discount factors are reported in the bottom pane of Table 2, with standard errors in parentheses. The p-values of our tests outlined above are reported in Table 3. As with the average discount factors before, some statistics for the whole dataset are outside the typical range from other experiments, but they imply the same qualitative conclusions as those we obtain restricting our attention to subjects in either the censored subsample or in the two reasonable clusters. We find the same clear patterns no matter whether we include all subjects, only the ones in the censored subsample, only those in Clusters 1 and 2, or only those in Cluster 2. Compensated discount factors, on all horizons are significantly higher than uncompensated ones, for both periods: $D_c(0,1) > D_u(0,1)$ and $D_c(1,2) > D_u(1,2)$, with p-values in one-sided t-tests or sign tests all below 0.0015 in the full sample. This suggests that standard elicitations of time preferences, which do not take into account future income expectations of subjects, on average overestimate the amount of impatience of individuals. This is consistent with the fact that most subjects expect a rising income path, decreasing the marginal utility of small monetary rewards in the future, relative to the present.

Second, the top pane of Table 3 shows that our tests reject exponential discounting without compensation for the full sample, restating the conclusions of our previous analysis reported in Table 1. In contrast with this, the new results on compensated discount factors in the bottom pane of Table 3 show no evidence (p-values over 0.7 for the full sample) of present bias for the average subject, once we control for the potential confounds of income changes. These results also hold in the reasonable subsamples, including using the simple definition of discount factors below 1.1 in the last column, where none of the test would conclude on rejecting the null hypothesis of no present bias at any conventional level of statistical significance ($p > 0.12$).

The top pane of Table 3 also provides some evidence that compensation affects the estimated discount factors of subjects with stable income less than how much if affects the discount factors for other subjects: For instance, both p-values being larger than 0.06 would not lead to a strong conclusion that $D_c(0, 1) > D_u(0, 1)$, nor would those around of well-above 0.05 to conclude that $D_c(1, 2) > D_u(0, 1)$, among the 24 subjects who reported that they expected stable income in both periods with a probability higher than 50%.

7 Conclusion

We find that in a setting where most people reveal present bias, people with stable incomes are standard exponential discounters. Our results show that subjects' choices over delayed rewards depend on their income expectations, revealing their importance for intertemporal choice experiments. Our experimental design shows how choices on rewards conditional on no income changes, alongside incentivized elicitations of subjective income expectations, can help researchers compensate for this confound. Our results cast doubt on the maintained hypothesis of many other studies that mental accounting implies that subjects evaluate monetary rewards independently of other income.

It is important for future research to obtain a richer empirical picture on how time preferences over monetary payments depend on different characteristics of future income expectations such as trends in expected income, trends in volatility of income, or the amount of autocorrelation of income in future periods. Any conclusion on saving behavior or policy would be overly speculative at this point, but for fundamental work in choice theory, this is an important step towards more conclusive lab experiments and empirical studies.

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A Appendix A: Proof of Proposition

Proof. Denote by p_t the probability measure over $\mathcal{S}(t)$ induced by p. We first show that p_t is pinned down by $\succsim$. Write $B_t = \{b^0, \ldots, b^N\}$ such that $b^0 \leq \ldots \leq b^N$ (the t subscripts are dropped to ease notation). For any $i \leq N$, let x_{m,s^t} be the reward that yields prize m at time t conditional on $s^t = (s_1, \ldots, s_{t-1}, b^i)$. Denote by $\psi(x_{m,s^t})$ the reward that yields a prize immediately such that $\psi(x_{m,s^t}) \sim x_{m,s^t}$.[34] Identify the immediate prize with $\psi(x_{m,s^t})$. The representation implies that

$$u(b^* + \psi(x_{m,s^t})) - u(b^*) = D(t)p_t(s_1, \ldots, s_{t-1}, b^i)[u(b^i + m) - u(b^i)].$$

Note that since u is a strictly increasing diffeomorphism, $\psi(x_{m,s^t})$ is a strictly increasing differentiable function of m that takes the value 0 when $m = 0$. Taking a derivative of the above expression with respect to m yields

$$u'(b^* + \psi(x_{m,s^t}))\frac{\partial \psi(x_{m,s^t})}{\partial m} = D(t)p_t(s_1, \ldots, s_{t-1}, b^i)u'(b^i + m).$$

Evaluating at $m = 0$ gives

$$\frac{\partial \psi(x_{m,s^t})}{\partial m}|_{m=0} = D(t)p_t(s_1, \ldots, s_{t-1}, b^i)\frac{u'(b^i)}{u'(b^*)}. \tag{7}$$

Consider the contingent reward $x_{m,s^t}^{b_1-b_0}$ that gives $b^1 - b^0$ at time t unconditionally and in addition gives m at time t conditional on $s^t = (s_1, \ldots, s_{t-1}, b^0)$. Denote by $\psi(x_{m,s^t}^{b^1-b^0})$ the contingent prize that pays $b^1 - b^0$ at time t unconditionally and also an immediate prize (which, abusing notation, is also denoted by $\psi(x_{m,s^t}^{b^1-b^0})$) satisfies $\psi(x_{m,s^t}^{b^1-b^0}) \sim x_{m,s^t}^{b^1-b^0}$. Then by the representation,

$$\begin{aligned} u(b^* + \psi(x_{m,s^t}^{b^1-b^0})) - u(b^*) &= D(t)p_t(s_1, \ldots, s_{t-1}, b^0)[u(b^0 + b^1 - b^0 + m) - u(b^0 + b^1 - b^0)] \\ &= D(t)p_t(s_1, \ldots, s_{t-1}, b^0)[u(b^1 + m) - u(b^1)]. \end{aligned}$$

[34] The existence of such a reward is implied by the continuity of u together with either the assumption that $D \leq 1$ or that u is unbounded. Its uniqueness is implied by the strict monotonicity of u.

Moreover,

$$\frac{\partial \psi(x_{m,s^t}^{b^1-b^0})}{\partial m}|_{m=0} = D(t)p_t(s_1,\ldots,s_{t-1},b^0)\frac{u'(b^1)}{u'(b^*)}.$$

Similarly, for each $0 < i \leq N$,

$$\frac{\partial \psi(x_{m,s^t}^{b^i-b^{i-1}})}{\partial m}|_{m=0} = D(t)p_t(s_1,\ldots,s_{t-1},b^{i-1})\frac{u'(b^i)}{u'(b^*)}. \tag{8}$$

By assumption, $p_t(s^t) \neq 0$ for all s^t (that is, $\frac{\partial \psi(x_{m,s^t})}{\partial m}|_{m=0} \neq 0$ for all s^t). Using (7) and (8), we get

$$\frac{\frac{\partial \psi(x_{m,s^t}^{b^i-b^{i-1}})}{\partial m}|_{m=0}}{\frac{\partial \psi(x_{m,s^t}^{b^1-b^0})}{\partial m}|_{m=0}} = \frac{p_t(s_1,\ldots,s_{t-1},b^{i-1})}{p_t(s_1,\ldots,s_{t-1},b^i)}$$

for all i. That is, the noted relative probabilities are determined uniquely by preferences. Conclude that $p_t(s^t)$ is uniquely determined for each s^t. In particular, p is uniquely determined.

To see that D is uniquely pinned down by preferences, note that by (7), for $b^i = b^*$ (that is, $s^t = (s_1,\ldots,s_{t-1},b^0)$),

$$\frac{\partial \psi(x_{m,s^t})}{\partial m}|_{m=0} = D(t)p_t(s^t). \tag{9}$$

Thus, the uniqueness of $p_t(s^t)$ implies that of $D(t)$. ■