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Summary

This paper estimates time preference parameters using commonly-applied methodologies, with the aim of investigating the link between these measures and actual economic behaviour. An experiment was conducted in the city of Thies, in Senegal, using the unique reference numbers of banknotes as a means of determining an individual's willingness to save money. The findings of this experiment provide an innovative comparison between real choices, and choices made in the presence of hypothetical rewards. Our research indicates that individuals display a far greater degree of patience, when the possibility of genuine financial gain is made available to them. Our results show that hypothetical time preferences parameters are poor predictors of actual behaviour, prompting questions over the validity of commonly used measurements.

Keywords: Time Preferences, Randomized Experiment, Senegal

JEL Classification: D01, D91, C93, O1

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Abstract

This paper estimates time preference parameters using commonly-applied methodologies, with the aim of investigating the link between these measures and actual economic behaviour. An experiment was conducted in the city of Thies, in Senegal, using the unique reference numbers of banknotes as a means of determining an individual's willingness to save money. The findings of this experiment provide an innovative comparison between real choices, and choices made in the presence of hypothetical rewards. Our research indicates that individuals display a far greater degree of patience, when the possibility of genuine financial gain is made available to them. Our results show that hypothetical time preferences parameters are poor predictors of actual behaviour, prompting questions over the validity of commonly used measurements.

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1. Introduction

The rate at which an individual discounts future rewards underlies the decisions they make in many aspects of their lives. This topic has generated a vast literature encompassing both behavioural economics and experimental psychology (for a general overview see Frederick et al., 2002). Time preferences have been linked to choices relating to savings behaviour, investment in agricultural inputs, education success and even substance abuse (Ashraf et al., 2006; Duflo et al., 2011; Castillo et al., 2011; Kirby et al., 1999). In the context of poverty alleviation, many have questioned the possibility that time preferences and self-control have a role to play in some individuals remaining poor (Lawrance, 1991; Atkeson and Ogaki 1996; Harrison et al., 2002; Tanaka et al., 2010). It has also been suggested that preferences for immediate gratification may be transferred from parents to children, extending the potentially negative outcomes, associated with impatient behaviour, beyond the current generation (Lang and Ruud 1986; Becker and Mulligan, 1997). In spite of the clear importance of accurate measures of time preference, field and laboratory research has progressed significantly, whilst leaving one question with relatively few answers. Do an individual's responses to hypothetical questions truly represent their preferences? This paper offers a plausible answer to this question by presenting an innovative field-study aimed at testing if standard hypothetical techniques can help in predicting an individual's observable behaviour.

In their review of the literature Fredrick et al. (2002) conclude there is no clear evidence of a difference in measures of time preferences elicited using real or hypothetical rewards. In a more recent appraisal of the literature, however, Andersen et al. (2014) state that the evidence is overwhelming that there can be huge and systematic hypothetical biases when using this type of reward. This lack of a clear consensus stems from a paucity of suitable studies that are willing to address this question. Indeed, much of the available literature comes from the field of experimental psychology, where discount rates are commonly elicited from small-scale laboratory experiments (usually involving only a handful of test subjects). In spite of the potential limitations of these methods, the findings of these studies provide some useful insights. Johnson and Bickel (2002) and Madden et al. (2003) both estimated discount rates for small groups of participants (5 and 20 individuals, respectively) using both hypothetical questions and the possibility of receiving real rewards. Both studies found no systematic differences in discount rates elicited using either type of reward. However, using a slightly larger sample (38 individuals), Hinvest and Anderson (2010) find significantly higher levels of self-control in participants offered real (verses hypothetical) rewards. Kirby and Maraković (1995) use two treatment groups to compare hyperbolic and exponential discount functions and find a lower discount rate for hypothetical responses. In their study, one of the two groups was incentivised with real monetary rewards, whereas the other gave responses to purely hypothetical questions. Coller and Williams (1999) also provided a comparison of real versus hypothetical responses through a separate treatment group. They report that discount rates were relatively higher than those for individuals who received real financial rewards. It is important to note that these studies were again comprised of a relatively small number of participants and crucially, that actual and hypothetical rates were estimated for two separate groups of test subjects, thus making a direct comparison between both rates at the individual level impossible. Outside of these laboratory studies, Ubfal (2016) provides another basis for comparison, through field-research conducted in Uganda. His work focusses on ascertaining the difference in discount rates between various goods (including money) and, although initial discount rates were obtained using hypothetical questions, a small sub-sample were re-interviewed with the possibility of obtaining real rewards from one of their responses (randomly selected). The paper concluded no significant variation between the two elicitation methods, suggesting the original responses were not subject to hypothetical bias.

Our paper measures hypothetical time preference via the standard 'Multiple Price-list' (MPL) format (see Andersen et al. 2006), and considers these results alongside an experiment involving real money (conducted on the same group of individuals). We investigate whether the expected consistency in behaviour can be observed between the two approaches. In order to do this, we developed a 'banknote experiment', whereby individuals were given a banknote of 1,000 CFA francs (USD 2) (with the unique serial number recorded). Participants in the experiment were then informed that if they chose to retain this specific note for a designated period of time (2, 7 or 14 days) they would receive a second banknote, in effect doubling their initial endowment (more details on our experimental design can be found below). We use the MPL elicited discount factors to predict the results of our banknote experiment, and to determine to what extent these measures correlate with the incentivised behaviour observed. Our research concludes that an individual's hypothetical time preference choices are a poor predictor of their behaviour in our (real money) experiment.

It is worth noting that, although our experiment provided us with a direct means of measuring individual discount rates, it also has confounding factors and generated new potential sources of bias. For example, the inherent fungibility of money may have led participants, who would otherwise have spent the banknote, to substitute the equivalent amount of money from alternative household savings (or accessible savings/credit from friends, relatives or other contacts). Responses may also have been subject to a 'reputation effect', whereby individuals may have viewed the experiment as a test of their personal credibility, and adjusted their behaviour accordingly. We discuss these issues at length in the following study, where we argue that our main results remain robust to these potentially confounding factors.

The next section describes the context of our study, our elicitations methods and our experimental design. We then present an overview of the theoretical framework, our econometric models and discuss our results. A discussion of the measures taken to assess possible sources of bias follows with the concluding remarks.

2. Data and Experimental Design

2.1 Context of our Study

Our interest in this question sprang from evidence we collected in a large-scale survey conducted in the city of Thies, Senegal, in 2012. The household-level observations gathered during this survey are the basis for the following analysis.

Thies is one of the largest cities in Senegal, with a population of about 240,000 inhabitants (at the time of the experiment). We use data collected between May and July 2012 on 360 randomly selected households across the whole territory covered by the city authorities. This represents an area of approximately 20 square km. We sampled the number of surveyed households across all Thies neighbourhoods according to their respective share of the overall population estimates (based on the 2005 census). More information on our methodology can be found in Appendix A.

For the purpose of this paper, the household is considered as a nuclear unit and consists of spouses, their children and all other members of the family who economically depend on the senior members. Our baseline survey was aimed at obtaining information on the general characteristics of each household member, including religion, level of education and ethnic affiliation. We also gathered information from the respondent concerning his/her work, monthly

income, and a number of other factors, which we describe below in greater detail. For 48% of the households surveyed the respondent was the head of the household.[1] In the remaining cases the respondent was most often the spouse or (in very few cases) another adult member of the household. We investigate below the possible consequences of this.

A brief overview of key variables obtained from the sample can be found in the first column of Table 1. To summarize, the majority of the respondents were female (63%), averaging approximately 45 years of age, and the sampled households contained around six members, on average. Mean household income was around 211,000 francs CFA per month, which is equivalent to approximately US$443 (on the basis of the exchange rates at the time of survey). Due to the sensitivity of obtaining income and salary levels, respondents were given a choice of 11 income intervals[2]. Therefore, income measures represent the mid-point in each interval, unless respondents provided more precise information. The three largest ethnic affiliations within the sample (Wolof, Poular and Sérer) approximately follow those of the country, as a whole.

[Insert Table 1 here]

2.2 Eliciting Discount Rates

Recent contributions to the time preference literature are often based on the 'Multiple Price-list' approach (MPL), as proposed by Coller and Williams (1999). This method generally presents individuals with an ordered list of trade-offs between a fixed, immediate reward and an increasing future amount, subject to a specific period of delay. Given the relative simplicity of communicating this procedure to test-subjects, it is understandable that this approach is often favoured over more complex experimental designs.

It has been suggested that discount rates obtained via this method may be susceptible to framing effects, dependant on the design of the price-list employed (Harrison et al. 2005). We rely on multiple amounts and multiple time delays to mitigate these effects. All the questions used are of a 'yes/no' type, allowing us to ask multiple questions to the same individual over the course of the interview. The set of amounts and time delays used are shown in Table A1 of the appendix, all of which are purely hypothetical (with no real rewards attached). There are two possible values for the immediate reward: 10,000 CFA (approximately US$21) in panel A and 1,000 CFA (approximately US$2) in panel B of the table. By way of comparison, we find that the mean of monthly income per-capita for our sample of households is approximately 41,000 CFA (inclusive of members who are not economically active). Regarding time-horizon, there is no front-end delay and the set of choices start with a delay of 2 days, before increasing up to a period of 6 months (generating observations over periods of 2 days, 7 days, 14 days, 1 month and 6 months).

The questions were designed to identify when the respondent switched from a (smaller) immediate reward toward a (larger) future reward. These questions were posed as follows: 'If

[1] Different reasons can explain why only half of the household heads answered the questionnaire. In many cases they did not live within the dwelling on a permanent basis, either visiting only for work related reasons or to pay regular/irregular visit to the household. A limited number of heads did not have the time to answer the survey, and delegated this responsibility to either their spouse or another adult. We did not meet a household who refused to take part in the survey.

[2] During our pilot, several individuals refused to give a precise value for their income, yet felt more incline to answer if the question was presented as a choice of 11 multiple income brackets (from 0 to 250,000 CFA, in steps of 25,000 CFA, plus 1 choice of income > 250,000 CFA).

you are sure to receive the sums mentioned at the given time, would you prefer accepting (*X*) francs CFA today or (*Y*) francs CFA in (*t*) days/months?' The delayed amount offered was then increased in subsequent questions until the respondent choose to switch. For example, in the case of the smaller initial reward (1,000 CFA, as opposed to 10,000 CFA), the first question proposed 1,000 CFA now and 1,000 CFA in two days. If the interviewee preferred the immediate reward (as would generally be the case), the delayed amount was increased to 1,050 CFA (US$2.19) and they were asked to express their preference again. This process was continued up until the point where the individual switched to the future reward. Beyond this point, we assume transitivity of preferences, such that the switching point (*Y*) is unique for any given initial amount (*X*) and time delay (*t*). Therefore, if an individual preferred a given amount in the future, compared to an initial value, he/she would also prefer larger amounts in the future (given the same time delay).

2.3 Eliciting Risk Preferences

Although this paper focuses primarily on the measurement of time preference, any non-instantaneous choices, from which an individual derives utility, are also likely to dependent on levels of uncertainty regarding future outcomes (Andersen et al. 2008; Andreoni and Sprenger 2012). We thus follow Holt and Laury (2002) and administer another set of yes/no questions to elicit the risk preferences of individuals. Each individual was offered the choice between two binary lotteries (A and B) involving gains (panel A) and losses (panel B, not shown), as outlined in Table A2 of the appendix. However, data obtained from panel B was scarce and therefore was not included in the calculation of risk preferences.[3] Lottery A is relatively more risky and has a higher payoff in the case of success. Lottery B is relatively safe and has a subsequently lower payoff in the event of a successful outcome. We set the probability of success the same for both the risky and safe lotteries. We made the assumption of 'monotonic switching', in the sense that when an individual switched from lottery A to lottery B, as the probability of success decreased, he/she could not switch back to lottery A. We offered monetary payoffs based on a single task, selected at random from across the lotteries.

2.4 The Banknote Experiment

Following the baseline questionnaire, each respondent was given a 1,000 franc CFA banknote. The unique reference number of this note was recorded and the individual was informed that, if they produced the same banknote when the household was visited on a second occasion, they would receive another 1,000 francs, and could retain both notes. The specific date of the second visit was randomly assigned, as 2 days, 7 days or 14 days from the initial visit, and this was announced to each household.[4] One household in three was assigned to each of these three possible treatment groups. One individual refused to partake in the experiment however, reducing the overall sample size to 359. Table 1 shows the tests for random assignments to treatments, through an F-Test comparison of the mean values for key variables within the three treatment groups (2 days, 7 days and 14 days). From our 20 potential baseline controls (shown in the upper section of table 1), only four significant differences are observed across the groups.

[3] This was due to a significant fraction of individuals showing reluctance in providing answer to this part of the experiment (as it involved losses), even when we repeatedly explained that the experiment was purely hypothetical.

[4] Because of organizational, time and resources constraints, it was not feasible for the same individual to play more than one scenario from the MPL. In particular, it was not possible to offer the same individual a banknote of 1,000 CFA and a banknote of 10,000 CFA (or offer second visits over different periods of time). It should also be apparent that any experiments conducted with actual rewards will clearly be limited by financial constraints. For example, were the experiment conducted using a 10,000 CFA banknote, given the proportion of the sample who retained the note in the 1,000 CFA case (and noting that this retention rate could be higher for the larger payments), the basic costs of conducting this experiment alone would, unfortunately, have been beyond our means.

These differences are related to whether the interviewee holds a savings account in a microfinance institution or in a bank, is member of a ROSCA (Rotating savings and credit association) and to our estimated measure of risk aversion.[5] They will be taken into account in the regression analysis that follows.

A more intuitive treatment would have been to offer each respondent *either* a 1,000 franc CFA note today or 2,000 francs CFA in *t* days during a second visit. This would have represented a replication of the MPL questions. Unfortunately, however, during our piloting phase, this approach proved difficult to implement cleanly. When presented with this choice, a significant proportion of respondents opted for the immediate reward because they perceived that the likelihood of us returning for the second visit was small. This was the case in all three treatments (2, 7 and 14 days), in spite of our efforts to assure the participants that our second visit was in no doubt. Our pilot survey indicated that our results were likely to be biased by this lack of trust if we were to attempt to implement this approach in our large-scale survey. In contrast, we found that by initially offering a note of 1,000 CFA surveyed individuals were not inclined to think that our second visit was in any doubt, even in the 14-day treatment group (who would experience the longest period between visits). Offering money during our first visit gave credibility to our experiment, such that this present treatment approach allowed us to avoid 'trust' bias, whilst also allowing easier implementation.

We are aware that our treatment not only elicits time preference, but also will be tainted by how individuals cope with temptation when saving money for short periods of time (how good they are at committing). Both effects are difficult to disentangle, but in an attempt to do so, we use additional information obtained alongside our time preference parameters (see below). It is also important to re-emphasize that our experiment is not an exact replication of our hypothetical MPL questions. As such, we are not directly testing the validity of hypothetical verses incentivized time preference measures. Our goal is rather to check if our hypothetical time preference parameters are good predictors of actual incentivised behaviour.

Once the banknote was received, each individual was asked a series of five questions:
Question 1: 'Do you think that you can keep the money until the specified date?'
Question 2: 'Why do you think you can or cannot?'
Question 3: 'Do you think, yes or no, that you will have difficulties coping with the temptations to spend the banknote?'.
Question 4: 'Do you plan, yes or no, to do something in order to make sure that you will not spend the note?'
Question 5: 'If yes what?'

The first three questions were aimed at determining to what extent the individual believed they could resist temptation during the experiment (the third question addressed this specifically). The last two questions were intended to identify any mechanisms they planned to use to ensure they avoided this temptation, and allow us to check if respondents were considering using any form of commitment device to ensure they did not spend the money. These last questions were also designed to evaluate any potential bias in behaviour, due to the inherent fungibility of the reward. That is to say, we wanted to see how likely the participants were to consider drawing money from an existing pool of cash (or credit), in order to increase their expenditure now, while still

[5] The reason why we observe these differences, given our experimental design, is unclear to us. There was no differential refusal rate to participate in the study by treatments. As far as we can tell, none of our enumerators showed strategic behaviour in selecting households, and our assignment of treatments was conducted in a proper way that should have prevented this outcome. One can suggest that the differences are likely to be related to the small size of the sample.

managing to retain the specific banknote provided. Were this form of expenditure-source switching common within the experiment, the results obtained could be misleading. Our descriptive statistics show however, that only 1.5% (3 out of 205) of our respondents, who answered 'yes' to question 4, planned to use such liquidity (or borrowing), in order to help them to keep the specific banknote. Answers indicative of this were: 'I will borrow around me (from friends or acquaintances), if I need, instead of using the note.' None mentioned the use of savings in ROSCAs (informal saving groups), MFIs or bank accounts. With such a small figure, we argue that this reasoning is likely to be marginal. However, the issue of fungibility is discussed at greater length in section 5.4.

Table 1 shows that those who retained the note (variable 'kept note') accounted for 78% of the sample. This proportion declines from 87.4% for delay of 2 days, to 80% (for 7 days) and 67.5% for 14 days. These differences are significant between 2 and 14 days, and 7 and 14 days, but not between the 2 and 7-day treatments. 74% of our sample indicated that they thought they could keep the note until the specified date (a yes to *Question 1*; variable 'think will keep the note'). As would be expected, this proportion is diminishing with the number of days involved in the treatment (63% for the 14-day treatment, and 79% for 7 and 2-day treatment). Answers to *Question 3* indicate that 26% of respondents think that they will experience difficulties coping with the temptation to spend the banknote (variable 'Temptation'). This proportion is significantly larger for those within the 14-day treatment (37%) than for either the 2 or 7-day treatments (21%).

A large majority of the answers to *question 2* (following a positive answer to *question 1*) highlighted the importance of gaining an additional 1,000 CFA as the primary motivation for keeping the banknote. Answers to *question 2* (from those who believed themselves *unable* to keep the banknote) mostly indicated that debts needed to be repaid or that urgent familial needs would prevent them from saving the note. 57% of the respondents indicated in *question 4* that they planned to do something in order to make sure that they would not spend the banknote. Of these 57%, answers to *question 5* indicated that 23% (48 out of 205) intended to give the note to somebody they trusted, in order to prevent them from using it, and 33% (68 out of 205) intended to hide the note somewhere safe (under their mattress, cupboard, etc.). In general, the pattern of these responses are indicative of the findings of Ashraf, Karlan and Yin (2006), and Dupas and Robinson (2013), who underline the importance of simply having access to a safe place to keep money, as a means of increasing savings.

3. Empirical Analysis

Our empirical analysis at the individual level has two components. The first estimates time preference parameters based on MPL questions. The second investigates whether these parameters have any effect on the actual choice made in the banknote experiment.

3.1 Estimation of the Discounting Parameters

From the first models of time-inconsistent preferences, proposed by Stroltz (1956), various specifications have been considered which allow for relative impatience over short-term rewards. Many of these models are based around 'hyperbolic' or 'quasi-hyperbolic' functional forms (see Laibson, 1994; 1997), and have often been found to fit the data more accurately than standard,

exponential discounting.[6] Benhabib, Bisin and Schotter (2010) provide a general expression for an individual's discount factor, which allows for testing among possible models, namely exponential, hyperbolic and quasi-hyperbolic discounting. We use this nested formulation as a starting point for our empirical analysis.

$$D(y, t, \beta, r, \theta) = \begin{cases} 1 & if \quad t = 0 \\ \beta(1-(1-\theta)rt)^{\frac{1}{1-\theta}} & if \quad t > 0 \end{cases} \qquad (1)$$

In equation (1), the discount factor *D(y, t)* is the value that makes an individual indifferent between two alternative time/reward pairs *(y D(y, t), 0)* and *(y, t).* In addition to the time between rewards (*t*) and the underlying discount rate (*r*), this discount factor is expressed as a function of the parameters *(β, θ),* which are intended to characterise the various forms of discount function considered within this study. Specifically, β is a parameter representing present-bias (in a quasi-hyperbolic specification) and θ parameterizes the curvature of the discount function. Dependent on the restrictions imposed on the parameters, this specification can represent various forms of time preference, through nesting exponential, hyperbolic and quasi-hyperbolic discounting functions, as follows.

i) When $\beta = 1$ and θ is approaching 1, equation (1) represents exponential discounting (e^{rt}), whereby the discount factor increases over time at a constant rate.
ii) When $\beta = 1$ and $\theta = 2$, equation (1) represents pure hyperbolic discounting ($1/(1+rt)$). In this case, the discount factor decreases over time, and displays a non-constant absolute rate of change.
iii) When θ is approaching 1, equation (1) displays future rewards under quasi-hyperbolic discounting (Laibson 1994; 1997). $D(y, t, \beta, r, \theta)$ takes on the form βe^{rt}, allowing for an individual to display a 'present bias' towards immediate reward, with all non-immediate amounts discounted by a factor β.

In our early attempts to estimate the most general form of the discounting equation described above (with an unrestricted θ) our results suffered from high levels of non-convergence (when estimating equation 1 on the various individual sub-samples of responses). As a result, we opted to employ the quasi-hyperbolic discounting specification as in equation (2), with θ approaching 1, which has two unrestricted parameters (r, β). We provide further justifications for the use of the quasi-hyperbolic model in table A3 and A4 of the appendix.[7] Following Tanaka et al. (2010), the term μ is included as a response-sensitivity (noise) parameter.

$$P(X > (Y, t)) = \frac{1}{1 + e^{\mu(X - Y\beta e^{-rt})}} \qquad (2)$$

Under the assumption of quasi-hyperbolic discounting, equation (2) shows the probability that the immediate reward *X* (at time 0) is preferred to the delayed reward *Y* (at time *t*). The parameters of the quasi-hyperbolic discount rate r and β are estimated separately for every individual within the sample, using the logistic function shown in (2). The values of *t, X* and *Y* are obtained from the time delay and amounts proposed in the various MPL questions.

[6]Some notable examples of studies that reject the exponential discounting form include Rachlin et al. (1991), Kirby and Maraković (1995) and Myerson and Green (1995).
[7] In this regard our analysis is similar to Tanaka et al (2010). They estimate the unrestricted version of equation (1), but find that it adds little to the explanatory power of the model (compared to the estimation of the quasi-hyperbolic specification), and so focus attention only on the quasi-hyperbolic discounting version of equation (1).

3.2 Effects of our Measures on the Banknote Experiment

In the second part of our analysis, we run a set of regressions to identify the potential determinants of the actual choices made by individuals during the banknote experiment. Within these regressions, the dependent variable takes the value 1 when the individual kept the banknote (and waited for the next visit to receive the second payment), whereas, it takes the value zero if the individual could not produce the banknote at the later time. The motivation behind these regressions is to investigate the role played by the estimated time preference (and risk-aversion) parameters in determining observable behaviour.

4. Results

4.1 Time Preference Parameters

Table 2 indicates the proportion of respondents who switched at the corresponding future amounts (in the respective time-period) in the lower (1,000 CFA) initial amount MPL questions. For the 6-month time frame, almost all individuals (96%) preferred the immediate reward to all amounts offered (ranging from 1,050 to 3,000 CFA). This 'no switch' proportion reduces to 84%, when the time delay is reduced to 1 month, and decreases further as the delay approaches the present. Table 2 also indicates that 54% of the sample preferred at least double the initial amount when the time delay was 7 days, while 75% preferred at least 1.5 times the initial amount over the shortest time-period stipulated (2 days). These hypothetical results appear to show a high degree of impatience. For example, approximately two-thirds of those sampled were unwilling to accept any of the given future rewards in 14 days, even when the opportunity of tripling their initial endowment was proposed.

[INSERT Table 2 HERE]

For each initial amount in the MPL questions (1,000 and 10,000 CFA), 5 time periods were considered (2 days, 7 days, 14 days, 1 month and 6 months), with 9 questions per time period (corresponding to 9 delayed reward values). Therefore, for each of the participants, MPL questions provided a selection of 45 responses for each of the two initial amounts of money proposed, giving us a total of 90 observations per individual. Using these sub-samples of 90 observations for each respondent, the measures of β and r are estimated within the quasi-hyperbolic specification in equation (2).

Due to our assumptions relating to transitivity, an individual could switch between immediate and future rewards at most once in each time period (providing only one point of variation for each of the given X, t combinations). Subsequently, a small number of respondents had insufficient variation in their responses to allow the non-linear estimation of (2) to converge (For example, where an extremely impatient respondent would almost never select the future reward). As a result, we were only able to estimate β and r values for 327 of the 359 respondents. The individuals for whom β and r could not be measured were found in all three treatment arms and there was no significant difference in the non-convergence rates between the 2-day, 7-day and 14-day banknote treatments (p-value = 0.416).

Table 3 shows the average values and standard deviation of these estimated parameters, for our sample of 327. The mean estimated value of the underlying discount rate (r) is 5.2%, with this

measure being lower than 7% for the majority of individuals in the sample.[8] As the *t* parameter in model (2) is measured in number of days, our estimations of (*r*) represent daily discount rates. This implies that (under the quasi-hyperbolic discounting specification) the average individual in the sample should be indifferent between the following pairings: 675.92 CFA today and 1,000 CFA received in 2 days; 521.17 CFA today and 1,000 CFA received in 7 days, and 362.16 CFA today and 1,000 CFA received in 14 days. The mean value of the estimated present bias parameter β is 0.75. However, as the extent to which an individual favours the present is negatively related to the estimated parameter β, table 3 also shows the measure of present bias (1 – β) which is used in the following investigation. The term μ, a response-sensitivity (noise) parameter, is estimated, but is not used in our subsequent analysis.

[INSERT Table 3 HERE]

4.2 Comparing Responses to the MPL and the Banknote Experiment

We first present a simple comparison between the answers provided to the MPL questions and the behaviour observed within the context of the banknote experiment. For each participant, one question within the MPL replicated the exact time frame and reward pair offered within the banknote experiment.[9] For example, an individual in the 2-day treatment, who claimed in the MPL questions to prefer 2,000 CFA in two days to 1,000 CFA now, should keep the banknote and receive the additional 1,000 CFA in two days (if behaviour is consistent). Table 4 shows whether the corresponding MPL question was able to predict an individual's behaviour in the banknote experiment.

[INSERT Table 4 HERE]

Overall, answers to the hypothetical MPL questions appear to be consistent with behaviour in the banknote experiment for only 50% of the individuals in the sample. Our results also indicate that the ability of the MPL to predict behaviour declines as the time-horizon increases. The consistency between the MPL and the banknote experiment is 69% for the 2-day treatment group, but decreases to only 38% for the 14-day treatment. An important share of our sample (44%) retained the banknote, having given responses to the MPL indicating that they would need a larger remuneration than the additional 1,000 CFA (offered in our experiment) to wait for the stipulated period (2, 7 or 14 days). These individuals appear to be more patient than their MPL responses would suggest. The proportion of such individuals also increases with the time delay, from 24% in the 2-day treatment, to 59% in the 14-day treatment. It is this form of inconsistency (impatience in the MPL, but patience in the experiment) which dominates the results in table 4. These findings are in line with those of Coller and Williams (1999) and Hinvest and Anderson (2010), who also found that the offer of real rewards significantly decreased the impulsivity of their test subjects.

4.3 Risk Aversion Parameter

[8] Given the size of the rewards offered and the time frames under consideration, individuals appear to display similar level of impatience as those suggested by results obtained in Harrison et al. (2002), Botelho et al (2006) and Tanaka et al. (2010).

[9] This would be from the selection of questions where X = 1,000 CFA and Y = 2,000 CFA. One question within this set would ask the subject to choose between these two amounts, over the same period of time as that stipulated in the banknote experiment.

Following the discussion in section 2.3 (regarding the influence of uncertainty in choices over future outcomes), our measure of risk aversion was elicited using the lotteries described in table A2 of the Appendix. Respondents were asked to choose between a relatively risky (choice A) and a relatively safe lottery (choice B), with the probability of success communicated to individuals using a bag containing different combinations of two colours of marbles (from which one marble would be drawn to determine the outcome). As would be expected, the fraction of individuals choosing the risky lottery (A) declines as the probability of the higher payout decreases. This reflects in part the change in the expected income difference between the risky and safe lotteries, which falls from 480 CFA to 180 CFA as the probability of the higher payoff falls from 0.8 to 0.3. A rational, expected-utility maximizing individual, with weakly risk averse preferences, should switch from choosing the risky to the safe lottery at most once over the course of the six tasks of the gain-frame series.

We make the assumption that the individual's preferences over outcomes in this lottery can be represented by a constant relative risk aversion (CRRA) utility function of the form $u(x) = [x^{1-R}]/[1-R]$. This function is used to place bounds on the CRRA coefficient R. An individual choosing the risky lottery in all tasks must have $R \leq 0.22$, whereas an individual choosing the safe lottery in all tasks must have $R \geq 0.82$. Those who switch from risky to safe lotteries between tasks 1 and 6 will have a value of R bounded within a strict subset of the interval (0.22, 0.82). Given that we could only gather data from the panel of lotteries involving gains, we cannot estimate CRRA parameters by maximum likelihood for each person individually (see for example, Harrison et al, 2010). This parameter is therefore, taken directly from the individual's choice in the gains lottery and the risk aversion parameter R calculated from the CRRA function above. Table 5 shows these values and their frequencies within the sample. The mean estimate of R is 0.55, with a standard error of 0.17. These values are broadly in line with results from Harrison et al (2010). As indicated in table 5, seven percent of individual did not switch at any point in the experiment; the risk aversion parameter for these participants is set at 0.22. In spite of random assignment to treatment, table 1 indicates there is some evidence that aversion to risk may be slightly higher in the 14-day treatment group (0.575), relative to the two shorter treatments (0.525 and 0.536 in the 2 and 7-day treatments, respectively) (p value = 0.061).

[INSERT Table 5 HERE]

4.4 Effects of our Hypothetical Measures on the Banknote Experiment

In the second part of our analysis, we sought to determine whether or not each individual's estimated values of 1-β, r and R (present bias, discount rate and risk aversion, respectively) have a significant impact on the behaviour we observe in the banknote experiment. We used these variables as regressors in a set of models, where the dependant variable represents whether or not the banknote was kept (1 if the note was shown to our enumerator upon the second visit, and 0 otherwise). The results described below were obtained via probit regressions, yet our results are similar if we use logit or OLS estimation techniques.

Table 6 presents the estimated effects. In Column 1, only the time frames are controlled for on the right-hand side of the regression, with the base category set as the 2-day treatment frame. In column 2, estimated individual discounting parameters (discount rate and present bias) are added as explanatory variables, and column 3 also accounts for the effect of the estimated risk aversion measure. Column 4 reports the effects of our hypothetical measures without controlling for the time delays. The final model in table 6 controls for a number of additional household and individual characteristics, which could intuitively be expected to exert some influence over the outcome of the experiment.

[INSERT Table 6 HERE]

Across models, the 14-day treatment frame consistently shows a strong, negative correlation with the probability of the note being kept. The 7-day frame displays the expected sign, but is not significant. These results are in line with the descriptive statistics in table 1. Individually, the estimated effects of the discount rate and present bias measure are never significant, in any of our models.[10] The same result applies when we test for their joint significance: the different tests for joint restrictions using a χ^2 distribution show p-values largely above 10% throughout. Our measure of risk aversion also appears redundant across all models.

Amongst the additional variables added in column 5, none seems to be related to the probability that the banknote would be retained. The other potential controls listed in table 1: ethnic group, home-owner, marital status, number of young children, lived in Thies for less than 2 year and neighborhood fixed-effects were also included in the estimation shown in column 5, but their coefficients were found not to be significant (these coefficients are not reported in table 6). Our results remain robust to several variations in the specification presented in model 5.

In order to estimate the parameters of time preferences with a reasonably high level of convergence, using the non-linear model described in (2), we needed to use all available data for each individual. That is to say, for both initial amounts of 1,000 and 10,000 CFA (and all five time frames). To check whether the results in table 6 are robust to changes in the data used to estimate the time preference parameters, the models shown in columns 1 to 5 were re-estimated using measures of β and r, based on the sub-sample of responses fulfilling the following conditions: 1) Only MPL responses for the initial payment of 1,000 CFA, and 2) only MPL responses for time delays of 2, 7 and 14 days (providing a closer comparison to the framing of the banknote experiment). The subsequently smaller number of observations reduced the number of individuals for which these parameters could be estimated from 327 to 294. However, using this smaller sample, our results remain very similar. Again, the estimated measures of time preference and risk aversion were insignificant (both individually and jointly) for all of the proposed specifications in table 6.

As an extension to our main results, we investigated various potential heterogeneous effects from the discount rate and present bias, on the likelihood of keeping the banknote (results are not shown but are available upon requests). We find none that could be linked to income, gender, education or the number of days treated (2, 7 or 14). We also interact our variable 'temptation' with the number of days treated, allowing us to investigate the differential effects of this variable, in specific arms of the treatment, but find no significant effects relating to these interactions either. As such, difficulties in coping with temptation appear to play no significant role in determining the outcome of the experiment. Results from these additional estimations still confirm our main results.

For 52% of our households the head of the household was not the respondent. It is plausible that the respondent (either head or not) consulted with their spouse or somebody else in order to decide what to do with the note. However, it seemed to us that most respondents dealt with the experiment largely privately. Our anecdotal evidence indicate that both spouses manage their

[10] One could argue that our hypothetical time preferences parameters are too imprecise to predict actual behaviour (see table 3), and that this imprecision could explain the lack of any correlation in our data. To attempt to address this concern, we present an alternative approach to estimating the discount parameters in section 5.2.

income/money independently. A large-scale DHS survey conducted in Senegal (DHS, 2016) also confirms this. Given this context, we expect the variables indicating gender, whether a respondent lives in a couple and whether the respondent is the head of household to have no significant effect on the outcome of the experiment. Our results in model 5 show that these variables play no significant role in determining this outcome. These results also seem to indicate that potential differences in preferences towards keeping the note between men and women do not to play a significant role in our context (as was the case in the Danish study conducted by Harrison et al., 2002).

5. Discussion

5.1 Narrow bracketing

We have estimated discounting parameters under the assumption that, individual abstract from the actual living conditions when answering the MPL questions. This is the narrow bracketing assumption in laboratory experiments, an assumption that has often been challenged (notably by Dean and Sautmann, 2015). In Table 7 we investigate whether the estimated discount parameters are correlated with an MPL respondent's characteristics. The first two columns of table 7 show OLS correlations between a number of such characteristics and our estimated discount rate (r), while columns 3 and 4 report the effect of these characteristic on our estimated parameter β. The results indicate that there is a significant correlation between individuals owning a bank account and a higher estimated discount rate (r). This result is intuitively appealing, as individuals who discount the future more heavily should place a higher relative value on future spending, and hence should have a higher propensity to save current income. Table 7 also indicates that a respondent who lives in couple is likely to have a lower estimated discount rate. This could be explained as a life-cycle effect, whereby those respondents living in couples are more likely to have children, arguably tilting their preferences for consumption towards the current period.

[INSERT Table 7 HERE]

The results show a positive effect from being a member of a ROSCA on parameter β, indicating that individuals who are ROSCA members seem to be less biased toward the present. It has been shown that ROSCAs can be used as a commitment device (Dagnelie et al., 2012) and in our case membership may impact on our estimated measure of present bias through this channel. Similarly, experiencing an episode of sickness during the previous year significantly reduces present bias. Such episodes may require unexpected health expenditures and, as such, can show an individual the importance of precautionary savings to mitigate the impact of negative shocks. This may modify one's time preference and put more emphasis on risk-management and accumulation for the future, hence reducing any bias for immediate consumption. In summary, our results seem to indicate that narrow bracketing may be too strong an assumption.

5.2 Pooled sample estimations

Due to only 90 observations per individual, the variables containing the estimates of r and 1-β tend to be relatively noisy (see table 3). An alternatively way of estimating our discounting parameters would be to estimate equation (2) using a pooled sample of observations, while incorporating demographic variables in the logistic function directly, as in Tanaka, Camerer and Nguyen (2010). This is done by defining the parameters in equation (2) as: $\beta = \beta_0 + \Sigma \beta_i X_i$ and $r = r_0 + \Sigma r_i X_i$, where X_i are demographic variables and β_i and r_i are their associated coefficients. Table 8a shows the results of estimating the quasi-hyperbolic discounting model on the pooled

sample, allowing β and r to depend on a selection of demographic variables, as described above. Column 1 and 2 show that several of these variables are significantly related to our time preferences parameters. Again, contradicting the narrow bracketing assumption.

[INSERT Table 8a HERE]

[INSERT Table 8b HERE]

Table 8b shows a summary of the means for β and r, predicted on the basis of the coefficients obtained in table 8a. These variables are broadly in line with those obtained in table 3, except that they display a significantly larger degree of precision (with much smaller standard deviation). This obviously comes as a result of only estimating predicted fitted values of the parameters from a pooled sample (32310 observations, so 90 observations for each 359 individuals), under the strong assumption of no unobserved individual heterogeneity. If we use these estimates in the various models presented in table 6 we obtain very similar results: individually and jointly, the estimated effects of the discount rate and present bias measure are never significant in any of our models (this table is not shown, but is available upon request). Thus, whether we estimate our discounting parameters from individual-specific, sub-samples of responses, or based on pooled data of all sample responses (with the inclusion of demographic variables), we find no significant correlations between hypothetical time discounting rates and actual behaviour in our experiment.

5.3 Enumerator effects

One undesirable effect, which we identified in a small number of our questionnaires during our pilot study, was that some interviewees interpreted being entrusted with the banknote as a test of their trustworthiness in the eyes of the enumerator. Answers suggesting such reputation effects included: 'Because I want to show you (the enumerator) my value', with alternative versions such as: 'I want to show you how I am capable of saving' or 'to show my patience'. We made every effort to eliminate this perception by emphasising that this note was theirs, and that the use they made of it would not to be judged or commented upon. Answers to the question 2,'*Why do you think you can or cannot* (*keep the money until the specified date*)*?'*, suggest that we were able to minimize this effect, as only 1.4% (5 out of 359) of our recipients mentioned anything related to this 'reputation effect' as a potential influence on the decision of whether or not to keep the note.

Although every effort was taken to minimise potential enumerator effects, possible differences in either the methods or style used by the enumerators during the interview (or the characteristics of the enumerators themselves) may have marginally impacted on the outcome of our banknote experiment. We tested whether the characteristics of the enumerator influenced the responses of the individuals, by re-estimating the regressions in table 6 with the inclusion of enumerator fixed-effects. The results were qualitatively similar to those presented earlier and, following a test of the joint significance of the nine enumerator dummy variables, we were unable to reject the null-hypothesis, that the specific enumerator characteristics (or interview style) had no effect on the outcome of the experiment. This would suggest that these enumerator effects are not present in the data.

5.4 Fungibility

It is possible to speculate that a household who had access to liquidity through their own savings would have found it easier to keep the 1,000 CFA note. If this were the case, we would expect

systematic differences in behaviour among the 'cash-constrained' and the relatively 'cash-abundant' participants within the sample. Comparing the results of the experiment between income quartiles, we found a non-significant difference of 0.7% in the probability of keeping the note between the lowest and highest quartiles of the sample. The small magnitude of this difference, and the fact that the coefficient on income in table 6 (model 5) is not significant, suggests that the extent to which liquidity exerted influence on the households' behaviour was negligible. Further evidence against the liquidity hypothesis came from interacting income and our treatment variables (2, 7 and 14 days), within our analysis of heterogeneous effects (see section 4.4). Again, these results (not shown) do not tell a story consistent with savings being used to fund expenditure in place of the banknote. It is likely however, that this type of behaviour would have been more prevalent, had the experiment been conducted with larger sums of money.

Following the discussion in section 2.4, the additional questions 4 and 5, *('Do you plan, yes or no, to do something in order to make sure that you will not spend the note?'* and *'If yes what?'),* were included (in part) to evaluate any potential bias in behaviour, due to the inherent fungibility of the reward. As previously noted, we wanted to uncover the likelihood that participants in the experiment would consider drawing money from an existing pool of cash or credit, in order to increase their expenditure now, while still managing to retain the specific banknote provided. Our descriptive statistics show that only 1.5% (3 out of 205) of our respondents considered such tactics in order to help them to keep the note, implying this effect is likely to be marginal at the sample level. In reference to specific potential sources of replacement funds, no individual mentioned the use of savings in ROSCAs, MFIs or bank accounts, when answering question 4. This is not surprising in the case of ROSCAs, given that these informal devices are notoriously inflexible, and are commonly used precisely for the purpose of rendering savings illiquid. Moreover, we argue that it is unlikely that an agent would visit either her bank or MFI office in order to withdraw such a relatively small amount specifically for this purpose (the financial fixed costs of such a transaction alone, would likely represent a significant share of the 1,000 CFA note received).

Finally, it is worth re-emphasising that both our hypothetical and real tasks may be capturing slightly different decision processes, and that our aim was not to directly compare them. Nevertheless, one should expect MPL-elicited discount factors to have some power in predicting the results of our experiment, evidenced by some significant correlation between these measures and the behaviour we incentivise with real money. This appears not to be the case, even after we consider potential confounding factors (enumerator effects, trust and fungibility), and after controlling for temptation and aversion to risk. Our investigation seems to suggest that hypothetical MPL discounting measures are poor indicators of true preferences. If we are inclined to think that incentivised preferences elicited with real money are better proxies, then hypothetical measures appear to either overestimate present bias β, or underestimate the underlying discount rate r, or both. The reason for this bias is currently unclear to us, although further fieldwork aimed at comparing the two methods directly, may uncover the reason behind this inconsistency.

6. Conclusion

Our findings indicate a disparity between implied discount rates over hypothetical rewards and observed economic behaviour, resulting in individuals within the banknote experiment displaying a far greater degree of patience than would be expected. It could be said that these results present a case of hypothetical bias in the self-reported responses to the MPL questions.

The possibility of inconsistent results drawn from data obtained using different hypothetical approaches should thus be of concern. In this respect, data obtained from direct observation, such as that presented in this paper, could be viewed as an empirical benchmark, against which more experimental procedures can be measured. We concede that it is possible that the inconsistencies we observe between the real and hypothetical measures of time preferences are due to the context of this study. Further investigations, using similar approaches in different contexts, are therefore required in order to give a better and more thorough assessment of MPL derived time preferences.

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Table 1: Means of Main Variables used and F test for Equality of Means

	All		2 Day Treatment		7 Day Treatment		14 Day Treatment		F-test	
	Mean	sd	Mean	sd	Mean	sd	Mean	sd	p-values	signif
Household size	6.067	2.686	6.134	2.531	5.908	2.387	6.158	3.101	0.704	
Household income (100'000CFA)	2.107	1.550	2.124	1.648	2.032	1.446	2.164	1.559	0.782	
Income quintile 1	0.251	0.434	0.277	0.450	0.250	0.435	0.225	0.419	0.649	
Income quintile 2	0.156	0.363	0.143	0.351	0.183	0.389	0.142	0.350	0.622	
Income quintile 3	0.195	0.397	0.176	0.383	0.175	0.382	0.233	0.425	0.457	
Income quintile 4	0.273	0.446	0.252	0.436	0.292	0.456	0.275	0.448	0.788	
Income quintile 5	0.125	0.332	0.151	0.360	0.100	0.301	0.125	0.332	0.488	
Durables (# of items)[1]	7.933	4.656	7.546	4.424	7.533	4.231	8.717	5.194	0.103	
Gender (Male =1)	0.370	0.484	0.370	0.485	0.400	0.492	0.342	0.476	0.647	
Age	44.883	13.637	44.664	12.011	45.583	14.214	44.400	14.614	0.794	
Respondent is household head	0.474	0.500	0.504	0.502	0.508	0.502	0.408	0.494	0.212	
Bank account	0.301	0.459	0.244	0.431	0.283	0.453	0.375	0.486	0.083	*
Education (# of completed grades)	8.131	6.282	8.269	5.956	8.283	6.124	7.842	6.776	0.838	
Savings account in MFI	0.195	0.397	0.134	0.343	0.250	0.435	0.200	0.402	0.067	*
Member of Rosca	0.393	0.489	0.294	0.458	0.392	0.490	0.492	0.502	0.007	***
Home owner	0.755	0.431	0.756	0.431	0.733	0.444	0.775	0.419	0.756	
Less than 2 years in Thiès	0.156	0.363	0.193	0.397	0.158	0.367	0.117	0.322	0.253	
Ethnic Group										
Wolof	0.557	0.497	0.555	0.499	0.533	0.501	0.583	0.495	0.737	
Serer	0.106	0.308	0.118	0.324	0.092	0.290	0.108	0.312	0.799	
Poular	0.189	0.392	0.185	0.39	0.208	0.408	0.175	0.382	0.801	
In couple	0.866	0.341	0.849	0.36	0.892	0.312	0.858	0.350	0.571	
# of Children under 5	0.972	1.170	0.992	1.108	0.917	1.074	1.008	1.319	0.803	
Estimated discount rate (r in %)	5.196	2.598	5.308	2.857	4.860	1.851	5.428	2.942	0.155	
Estimated measure of present-bias (1-β)	0.249	0.138	0.238	0.145	0.259	0.134	0.250	0.136	0.533	
Estimated Risk-aversion (R)	0.545	0.167	0.525	0.146	0.536	0.163	0.575	0.185	0.061	*
Temptation[2]	0.262	0.440	0.210	0.409	0.208	0.408	0.367	0.484	0.010	**
Kept note[3]	0.783	0.413	0.874	0.333	0.800	0.402	0.675	0.470	0.001	***
Think will keep the note[4]	0.868	0.339	0.971	0.168	0.858	0.350	0.788	0.410	0.000	***
N	359		119		120		120			

*** p<0.01, ** p<0.05, * p<0.1

[1] This variable is the sum of a list of items owned by the household comprising (amongst others) home appliances and furniture, mobile phone and means of transportation. The full list of these items is: fridge, colour TV set, car, freezer, DVD player, sewing machine, gas cooker, stereo, bed (wood or metal), stove (camping stove), couch, clock, electric cooker, bicycle, gas lamp, oven, motorbike, petrol lamp, camera, charrette, electric fan.
[2] Temptation is a dummy variable which takes value 1 if the respondent answered yes to *Question 3:* 'Do you think, yes or no, that you will have difficulties coping with the temptations to spend the banknote?' (0 otherwise). We expect this variable to be impacted by the number of days of the treatment (either 2, 7 or 14). Therefore, the difference across groups is expected.
[3] This variable takes value 1 if the respondent did keep the actual note (0 otherwise). We expect this variable to be impacted by the number of days of the treatment (either 2, 7 or 14). Therefore, the difference across groups is expected.
[4] Think will keep the note is a dummy variable which takes value 1 if the respondent answered yes to *Question 1:* 'Do you think you can keep the note until the specified date?' (0 otherwise). We expect this variable to be impacted by the number of days of the treatment (either 2, 7 or 14). Therefore, the difference across groups is expected.

Further note: Discount rate and present-bias statistics are only available for the sample of those individuals, for whom convergence was achieved in equation (2). This sample amounted to 327 individuals overall, with 111, 110 and 106 in the 2, 7 and 14 day treatments, respectively.

Table 2: Proportion of Respondents Who Opted for the Future Reward at the Indicated Amount with an Initial Option of 1,000 CFA (US$2)

Amount	2 days (%)	7 days (%)	14 days (%)	1 month (%)	6 months (%)
'No switch'	18	33	63	84	96
1000	0	0	0	0	0
1050	0	0	0	0	0
1100	1	1	0	0	0
1250	6	1	0	0	0
1500	31	5	2	1	1
1750	8	5	1	0	0
2000	22	19	8	2	1
2500	7	16	10	2	0
3000	6	19	15	10	2
Total	100	100	100	100	100

Table 3: Means of the Estimated Discount Factor Parameters

	Sample size	Mean	Standard deviation	Min	Max
Discount rate (r in %)	327	5.20	2.60	-0.34	17.62
Beta (β)	327	0.75	0.14	-0.14	1.09
Measure of present-bias (1-β)	327	0.25	0.14	-0.09	1.14
Noise parameter (μ)	327	-33.80	30.27	-98.99	-0.97

The parameter t in model (2) is in number of days so our rates are daily discount rates.

Table 4: Consistency of the MPL Preference Questions and Behaviour

	All		2 day		7 day		14 day	
	# Ind	%	# Ind	%	# Ind	%	# Ind	%
MPL predicted banknote experiment	179	50	82	68.9	51	42.5	46	38.3
Note saved (predicted)	122	34	76	63.9	36	30	10	8.3
Note spent (predicted)	57	15.9	6	5	15	12.5	36	30
MPL did not predict banknote experiment	180	50	37	30.1	69	57.5	74	61.7
Note saved (not predicted)	159	44	28	23.5	60	50	71	59.2
Note spent (not predicted)	21	5.8	9	7.6	9	7.5	3	2.5
Total	359	100	119	100	120	100	120	100

Table 5: Distribution of the Estimated Risk Aversion Parameter (*R*)

Value for *R*	Frequency	Percentage
0.22	25	7
0.30	23	6
0.44	99	28
0.56	99	28
0.67	55	15
0.77	13	4
0.82	45	13
Total obs.	359	100

Table 6: Estimated effects of discounting and Risk Parameters (Probit regression) on keeping the banknote or not.

	(1)	(2)	(3)	(4)	(5)
VARIABLES	The dependent variable is Banknote kept =1 (=0 otherwise)				
7 day treatment	-0.2364	-0.2414	-0.2406		-0.3535
	(0.1978)	(0.2017)	(0.2034)		(0.3501)
14 day treatment	-0.5987***	-0.5988***	-0.5968***		-0.8161**
	(0.2012)	(0.2030)	(0.2073)		(0.3754)
Discount rate (r)		-0.4036	-0.4523	-1.6108	-6.1455
		(3.8604)	(3.8769)	(3.7828)	(7.1298)
Present bias (1-β)		0.1359	0.1267	-0.0472	0.2546
		(0.6893)	(0.6945)	(0.6906)	(1.3067)
Risk aversion (R)			-0.0385	-0.2414	-0.6909
			(0.5151)	(0.5079)	(0.9375)
Gender (Male=1)					-0.1165
					(0.6877)
Age					0.0189
					(0.0148)
Respondent is household head					0.1194
					(0.7270)
Household size					-0.0323
					(0.0688)
Income (in 100'000CFA)					-0.0294
					(0.1329)
Durables (# of items)					0.0497
					(0.0397)
Bank account					0.0644
					(0.3901)
Education (# of completed grades)					-0.0082
					(0.0264)
Savings account in MFI					0.5195
					(0.4782)
Member of ROSCA					0.1748
					(0.3416)
Home owner					0.2737
					(0.4031)
Less than 2 years in Thies					-0.1413
					(0.4344)
In couple					-0.0248
					(0.6720)
# of Children under 5					0.1775
					(0.1786)
Constant	1.1449***	1.1337***	1.1584**	1.0728**	0.0048

	(0.1539)	(0.3707)	(0.4811)	(0.4553)	(1.5890)
Ethnic Group Fixed Effects	No	No	No	No	Yes
Neighbourhood Fixed Effects	No	No	No	No	Yes
Observations	327	327	327	327	327
Bootstrapped Replications	200	200	200	200	200

*** p<0.01, ** p<0.05, * p<0.1

-Bootstrapped standard errors in parentheses. Given that β and r are generated regressors, we use bootstrapping to estimate their standards errors (see Mooney and Duval, 1993).

-The estimations are carried out according to the following procedure: A random bootstrap sample of 327 individuals are selected, with replacement, from the full sample. For each individual, their sub-sample of 90 observations are used to estimate the time preference parameters in equation 2. The full sample of 29430 observations is collapsed to one observation for each individual in the bootstrapped sample (327 observation), before the second stage probit model is run. The full sample of 29430 observations is then restored before selecting the second bootstrap sample. Reported standard errors are based on the distribution of the coefficients obtained from 200 random bootstrap samples.

-Concerning the variables 'Savings account in MFI' and 'Member of a ROSCA': membership in these saving devices was measured in our baseline survey prior to our treatments. We also know that no individual in our sample either joined an MFI or a ROSCA during the 2, 7 or 14-day period of our treatment.

Table 7: Correlates of discounting parameters

	r		β	
VARIABLES	(1)	(2)	(3)	(4)
Gender (Male=1)	0.0077	0.0082	-0.0076	-0.0071
	(0.0068)	(0.0068)	(0.0368)	(0.0368)
Age	-0.0002	-0.0001	-0.0007	-0.0008
	(0.0001)	(0.0001)	(0.0006)	(0.0007)
Respondent is household head	-0.0051	-0.0057	0.0216	0.0212
	(0.0070)	(0.0071)	(0.0378)	(0.0380)
In couple	-0.0124*	-0.0128*	-0.0031	-0.0018
	(0.0067)	(0.0067)	(0.0363)	(0.0363)
Education(# of completed grades)	-0.0003	-0.0003	0.0011	0.0009
	(0.0003)	(0.0003)	(0.0014)	(0.0014)
Bank account	0.0102***	0.0103***	-0.0075	-0.0114
	(0.0035)	(0.0038)	(0.0191)	(0.0203)
MFI Account	0.0053	0.0054	-0.0069	-0.0058
	(0.0037)	(0.0037)	(0.0199)	(0.0199)
Member of ROSCA	0.0024	0.0026	0.0388**	0.0355**
	(0.0030)	(0.0030)	(0.0163)	(0.0164)
Episode of sickness last year		0.0024		0.0419**
		(0.0035)		(0.0187)
Income (in 10000CFA)		0.0012		0.0017
		(0.0011)		(0.0058)
Durables (sum of items)		-0.0004		0.0008
		(0.0004)		(0.0019)
Constant	0.0669***	0.0647***	0.7564***	0.7228***
	(0.0087)	(0.0093)	(0.0472)	(0.0499)
Observations (households)	327	327	327	327
R-squared	0.056	0.063	0.024	0.040

OLS estimations; Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 8a: Pooled Sample Discount Parameters

	r	β
VARIABLES	(1)	(2)
μ	-5.5683***	-5.5683***
	(0.1086)	(0.1086)
Gender (Male=1)	0.0158***	-0.0765***
	(0.0010)	(0.0061)
Age	0.0000	-0.0001
	(0.0000)	(0.0001)
Respondent is household head	-0.0158***	0.0687***
	(0.0010)	(0.0065)
In couple	-0.0288***	0.0588***
	(0.0011)	(0.0061)
Education (# of completed grades)	-0.0002***	-0.0007**
	(0.0000)	(0.0003)
Bank account	0.0146***	0.0135***
	(0.0007)	(0.0043)
MFI Account	0.0012*	0.0044
	(0.0006)	(0.0045)
Member of ROSCA	0.0003	0.0061*
	(0.0005)	(0.0036)
Episode of sickness last year	0.0115***	0.1174***
	(0.0006)	(0.0038)
Income (in 100'000CFA)	0.0004**	0.0022*
	(0.0002)	(0.0013)
Assets (sum of items)	-0.0001*	-0.0002
	(0.0001)	(0.0004)
Constant (r0 β0)	0.0613***	0.5713***
	(0.0017)	(0.0093)
Observations	32310	32310
Households	359	359
R-squared	0.479	0.479

Standard errors in parentheses;
*** p<0.01, ** p<0.05, * p<0.1

Table 8b: Means of the Discount Parameters from a Pooled Estimation

	observations	Mean	Standard deviation	Min	Max
Fitted discount rate (r in %)	359	4.75	0.97	2.77	8.71
Fitted Beta (β)	359	0.72	0.05	0.54	0.82
Fitted measure of present-bias (1-β)	359	0.28	0.05	0.18	0.46

Appendix

Appendix A: Survey Methodology

An official map of the city was used to randomly select a number of streets spread across each neighbourhood. Each street was assigned a number of households, according to its length and density. For every street we used a pseudo-random process, by which every fifth lot according to a specific direction was picked. Since many households live on the same lot in semi-detached rooms, enumerators randomly selected one room by lot, according to a clock-wise selection which varied from lot to lot. In the case where a lot was found empty or the head of household was not present, enumerators were instructed to set appointments and revisit the household later. Given the small number of households sampled from such a relatively large area, we argue that spill-overs within the sample are unlikely.

Ten local, independent and qualified enumerators were employed, having previous experience with surveys and field-work. The selected enumerators undertook a two-day training session given by the authors, including special sessions dedicated to translation in to the local language (Wolof) and practical tests to confirm their suitability. In addition, enumerator visits were also assessed ex-post by an experienced local supervisor.

Table A1: Eliciting the Discount Rate

Panel A: Amount proposed for today 10,000CFA

1	A	B	A or B?	2	A	B	A or B?
	Today	In 2 days			Today	In 7 days	
1	10000	10000		1	10000	10000	
2	10000	10500		2	10000	10500	
3	10000	11000		3	10000	11000	
4	10000	12500		4	10000	12500	
5	10000	15000		5	10000	15000	
6	10000	17500		6	10000	17500	
7	10000	20000		7	10000	20000	
8	10000	25000		8	10000	25000	
9	10000	30000		9	10000	30000	

Three additional set of choices were offered where the values in A and B were identical but the time delay was 14 days, 1 months and 6 months.

Panel B: Amount proposed for today 1,000 CFA

1	A	B	A or B?	2	A	B	A or B?
	Today	In 2 days			Today	In 7 days	
1	1000	1000		1	1000	1000	
2	1000	1050		2	1000	1050	
3	1000	1100		3	1000	1100	
4	1000	1250		4	1000	1250	
5	1000	1500		5	1000	1500	
6	1000	1750		6	1000	1750	
7	1000	2000		7	1000	2000	
8	1000	2500		8	1000	2500	
9	1000	3000		9	1000	3000	

Three additional set of choices were offered where the values in A and B were identical but the time delay was 14 days, 1 months and 6 months.

Table A2: Eliciting Risk Preferences

	# Marbles type1	# Marbles type2	Lottery A		Lottery B		Preference	
			Successful payoff	Unsuccessful payoff	Successful payoff	Unsuccessful payoff		
Panel A: gains								
1-1	8	2	600	0	200	100	A	B
1-2	7	3	600	0	200	100	A	B
1-3	6	4	600	0	200	100	A	B
1-4	5	5	600	0	200	100	A	B
1-5	4	6	600	0	200	100	A	B
1-6	3	7	600	0	200	100	A	B

Table A3: Estimated Discount Parameters for the Pooled Sample

	Exponential	Hyperbolic	Q-Hyperbolic	Full Model
Noise parameter (μ)	-3.702	-5.2784	-5.0384	-5.1782
	(0.0965)	(0.1463)	(0.1627)	(0.1634)
Discount rate (r)	0.0987	0.1394	0.098	0.0386
	(0.0005)	(0.0005)	(0.0006)	(0.0019)
Beta (β)			0.8184	0.7592
			(0.0020)	(0.0077)
Theta (θ)				0.8607
				(0.1457)
Observations	32310	32310	32310	32310
Adjusted R-Squared	0.402	0.449	0.425	0.431

Individual-level cluster-robust standard-errors in parentheses

Table A3 shows the estimated parameters for the pooled sample of individuals. The pooled data contains 32,310 observations (90 questions, for each of the 359 individuals). It is worth noting that the estimated parameters of the discount factor are broadly comparable to those in Tanaka et al. (2010). The adjusted R^2 for the models with pooled estimations suggest, surprisingly, that the hyperbolic model explains a greater proportion of the data. The full model, with non-restricted values for Theta (θ), seems to perform only marginally better than the quasi-hyperbolic specification. Furthermore, the hypothesis that the parameter $\theta = 1$ cannot be rejected at any common significance level (p-value = 0.34) and a test of the hypothesis that $\theta = 2$ in the full model is strongly rejected (F-stat = 61.1). We also reject the null that $\beta = 1$ in the last 2 models (with p-values close to 0). If we refer to our discussion above in the text following equation (1): these results seem to indicate that quasi-hyperbolic model is a strong candidate.

Table A4: Models estimated and preferred model

	Model is estimated with convergence		Preferred model	
	(1)	(2)	(3)	(4)
	# Individuals	% (out of 359)	# Individuals	% (out of 359)
Exponential model	320	89.14	42	11.7
Pure Hyperbolic model	327	91.09	44	12.26
Quasi-Hyperbolic model	327	91.09	156	43.45
Full model	265	73.82	92	25.63
Total	359	100	359	100

Note: The preferred model is based on individual adjusted R-squared; '%' refer to the proportions of # individuals out of the total sample of 359.

The first two columns of table A4 below show the sub-sample of individuals for which a given model could be estimated with convergence. It is worth noting that exponential and hyperbolic models required fitting two parameters, the quasi-hyperbolic three, and the full model four. Estimating the full model for each of the individual-specific sub-samples (of 90 observations each per individual) generated a large increase in the rate of non-converge, while arguably adding little to the analysis (based on a comparison between the adjusted R^2 values in columns 3 and 4 of table A3). If we use the adjusted R^2 in the individual level regressions as a criterion for selection amongst our four models, we find that the largest share of those surveyed had

preferences best modelled using the quasi-hyperbolic specification. This result is obtained by first estimating the four possible models (exponential, hyperbolic, quasi-hyperbolic and full model). For each individual, the adjusted R^2 for any of the given model which converged (and returned an adjusted $R^2 > 0$) is retained. Then, for each individual we identify the model with the highest adjusted R^2 which becomes the 'preferred model' for that individual (see columns 3 and 4). Based on this we argue that the quasi-hyperbolic model best represented the time preferences of the largest proportion of our sample, whilst also maximising the number of individuals, for whom equation (1) yielded useable estimates of the required parameters. This quasi-hyperbolic specification also allowed comparison with previous papers (e.g. Tanaka, 2010).

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