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A Pilot Experiment in Timor-Leste

by

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Abstract. We conduct laboratory experiments in Timor-Leste designed to test if individual discount rates vary with the time horizon for which the rate is elicited. Our experiments test a design that has been successfully employed in field experiments in developed countries, and that avoids several confounds of previous procedures. We find that there is considerable heterogeneity in individual discount rates, and that this heterogeneity is associated with observable demographic characteristics. We also find evidence that is consistent with exponential discounting behavior, although our sample sizes do not allow us to definitively reject alternative specifications. We discuss modifications of our laboratory experiments that would facilitate field experiments in Timor-Leste.

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The manner in which people discount the future is obviously important for understanding behavior in developing countries. We undertake some pilot experiments in Timor-Leste to evaluate the application in developing countries of experimental procedures that have been successfully used in developed countries to elicit discount rates.[1] These procedures avoid many of the confounds that have generated some truly "wild" behavior in previous experiments. The most important procedural features are the use of real payoffs instead of hypothetical payoffs, the use of a "front end delay" on the early options so as to avoid confounding time preference with credibility of payment, and the joint elicitation of risk preferences and time preferences. We explain these procedures in section 1.

Timor-Leste[2] is a new country, having gained independence from Portugal in 1975 and then in 2002 from the occupation forces of Indonesia.[3] The population in 2005 is estimated to be around 1 million, although there is considerable uncertainty about these numbers due to the mountainous

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[1] There have also been several small-scale experiments conducted in developing countries, most notably by Pender [1996], Godoy and Jacobsen [1999], Godoy, Kirby and Wilkie [2001] and Kirby et al. [2002]. Pender [1996] anticipates many of the methodological features developed to avoid confounds in eliciting time preference, but does not elicit risk preferences. There have been several large-scale hypothetical surveys eliciting time preferences in developing countries, but these are not experiments as the term is normally used in economics to refer to tasks for which there are real consequences and hence motivated subjects. For example, see Barr and Packard [2002] in Chile, who report an average elicited discount rate of 43% per annum. It is possible that these hypothetical discount rates can be used to predict discount rates, and we discuss later how one can undertake complementary hypothetical and real tasks to expand the sample at reasonable cost. This is particularly attractive if the experiments are to be conducted on a sub-sample from a larger field survey.

[2] Many would know Timor-Leste as East Timor, or formerly as Portuguese Timor.

[3] The history of Timor-Leste is described by the Central Intelligence Agency *Factbook*, at http://www.cia.gov/cia/publications/factbook/geos/tt.html, as follows: "The Portuguese began to trade with the island of Timor in the early 16th century and colonized it in mid-century. Skirmishing with the Dutch in the region eventually resulted in an 1859 treaty in which Portugal ceded the western portion of the island. Imperial Japan occupied East Timor from 1942 to 1945, but Portugal resumed colonial authority after the Japanese defeat in World War II. East Timor declared itself independent from Portugal on 28 November 1975 and was invaded and occupied by Indonesian forces nine days later. It was incorporated into Indonesia in July 1976 as the province of East Timor. An unsuccessful campaign of pacification followed over the next two decades, during which an estimated 100,000 to 250,000 individuals lost their lives. On 30 August 1999, in a UN-supervised popular referendum, an overwhelming majority of the people of East Timor voted for independence from Indonesia. Between the referendum and the arrival of a multinational peacekeeping force in late September 1999, anti-independence Timorese militias - organized and supported by the Indonesian military - commenced a large-scale, scorched-earth campaign of retribution. The militias killed approximately 1,300 Timorese and forcibly pushed 300,000 people into West Timor as refugees. The majority of the country's infrastructure, including homes, irrigation systems, water supply systems, and schools, and nearly 100% of the country's electrical grid were destroyed. On 20 September 1999 the Australian-led peacekeeping troops of the International Force for East Timor (INTERFET) deployed to the country and brought the violence to an end. On 20 May 2002, East Timor was internationally recognized as an independent state."

nature of country and the relatively low urban density (around 24% in 2001). By any of the standard measures employed in development economics, Timor-Leste is a poor country. As noted by the World Bank [2005; p. 4] "... Timor-Leste is among the poorest countries in the world. The GDP per capita is US$405, one in five people live on less than one dollar per day and two in five live below the national poverty line. Inequality is also high, with the poorest 40 percent of the population having an expenditure share of less than 18 percent. Two out of five adults are illiterate, half of children under five are stunted and the under-five mortality rate is 83 deaths per 1,000 live births."

We examine three substantive issues, in addition to the methodological question of transferring procedures to a developing country.

First, what general discount rate does one find? Is it the case that discount rates in poor countries are extremely high, as some might expect from anecdotal evidence? We generally find that discount rates are comparable to those found in developed countries, once these are corrected for the concavity of the utility of money, and are around 12% per annum if one assumed exponential discounting.

Second, how much do discount rates vary with observable demographic characteristics? If discount rates do vary across individuals and households, it will be important to know how much they vary and with which characteristics, so that accurate welfare evaluations of policy can be undertaken. In effect, we ask if the representative household assumption applies with respect to discount rates in developing countries. Although our sample is limited in size and diversity compared to the population as a whole, we do find evidence of considerable heterogeneity.

Third, what discounting function applies to behavior over different time horizons? In other words, do subjects use the same discount rate for different time horizons, as implied by the exponential discounting model? Or do they use higher discount rates for shorter time horizons, as implied by quasi-hyperbolic or hyperbolic discounting models? We find evidence that is consistent with exponential discounting, although caution that our sample sizes are limited as noted above. On the other hand, the data is more supportive of the hyperbolic model in Timor-Leste than in any of previous experimental studies using comparable procedures in developed countries.

Our results allow us to identify procedures that we believe will be feasible and informative for use in field experiments in Timor-Leste and many other developing countries. Larger sample sizes, as well as more demographically divers samples, should allow us to provide more precise answers to the above three questions.

1. Experimental Design

Our experiment procedures are based in those used by Harrison, Lau, Rutström and Sullivan [2005] in field experiments conducted throughout Denmark. These treatments represent simple extensions of the experimental design employed by Coller and Williams [1999] and Harrison, Lau and Williams [2002] in laboratory and field experiments in the United States and Denmark, respectively.

The basic question used to elicit individual discount rates is simple, and is of this general form: do you prefer \$100 today or \$100+x in the future, where x is some positive amount? If the subject prefers the \$100 today then we can infer that the discount rate is higher than x% over the period of the delay; otherwise, we can infer that it is x% or less over the period of the delay. The format of our experiment modifies and extends this basic question in five ways.

First, we pose a number of such questions to each individual, each question varying x by some amount. When x is zero we would expect the individual to reject the option of waiting for a zero rate of return. As we increase x we would expect more individuals to take the future income option. For any given individual, the point at which he switches from choosing the current income option to taking the future income option provides a bound on his discount rate. That is, if an individual takes the current income option for all x from 0 to 10, then takes the future income option for all x from 11 to 100, we can infer that his discount rate lies between 10% and 11% for this time interval. The finer the increments in x, the finer is the discount rate of the individual that we will be able to bracket. We then select one question at random for actual payment after all responses have been completed by the individual. In this way the results from one question do not generate income effects which might influence the answers to other questions.

Second, we provide two future income options rather than one "instant income" option and one future income option. For example, we offer \$100 in 30 days and \$100+x in 90 days, interpreting the revealed discount rate as applying to a time horizon of 60 days. This is intended to neutralize the effect of any fixed, horizon-independent, premia which subjects may attach to future income options. For example, if delayed options involve greater transactions costs than immediate options, then the compensation demanded to wait for the delayed option would include these subjective transactions costs. Similarly, if preferences include a "passion for the present," then the compensation demanded to accept a payment with any delay would include an amount to compensate for this. In either case the revealed discount rate would be biased upwards, unless we account for a fixed premium. By having both options entail future income, we hold these subjective costs constant across the payoff options and hence gain greater control over the comparability of the two monetary payoffs.[4] We refer to this delay on the initial option as a Front End Delay (FED).

Third, we consider six different horizons, ranging from 1 month to 24 months after the FED. Each subject faces each horizon in the natural, ascending order. We are thus able to plot discount rates over a variety of horizons and explicitly test whether rates are related to the time horizon. Of course, this design confounds order effects and horizon, and future designs can address this easily.

Fourth, we provide respondents with information on the implied interest rates associated with the delayed payment option. This is an important control feature if field investments are priced in terms of interest rates. To the extent that some subjects are attempting to compare the lab investment to their field options, this feature may serve to reduce comparison errors since both the

[4] Although the logic of this point is intuitive enough, and appears to have been first stated by Roberts [1991], it was first tested by design in Coller and Williams [1999].

lab and field options are now priced using the same metric.[5]

Fifth, we also elicited the risk attitudes of each subject, using procedures developed by Holt and Laury [2002] that are similar to those used to elicit discount rates. This allows us to correct for the concavity of the utility function when inferring discount rates from choices over monetary prizes. The manner in which we jointly estimate risk and time preferences is discussed later; for our design purposes the important point is that we have elicited risk attitudes from the same sample that we seek to elicit discount rates from.

Table 1 shows a payoff table for the case of a subject facing a 30 day horizon and a 30-day FED. In other words, the later payment, option B, occurs 30 days beyond the earlier payment, option A. The subject circled which option, A or B, he would prefer in each row, or could also express indifference. For subjects facing different time horizons, the annual interest rates were the same as those in Table 1, but the payment amounts for option B were appropriately different.

2. Experimental Procedures

We recruited 30 students from the undergraduate program in Management & Economics of the National University of Timor-Leste, administered in Dili by the Federation of Portuguese Universities (FUP). All required courses in this program are in Portuguese, which is the official language in Timor-Leste along with Tetun.[6] Students were recruited in classes in July, 2004. They were told that there was to be an economics experiment that would pay $5 for participation, and that they could receive a considerable amount of additional money depending partly on chance. They were given no other prior information concerning the nature of the experiment or the expected

[5] Previous experiments provided information on annual and annual effective interest rates. To keep the instructions simple we only provided information on the nominal interest rate.

[6] The range of languages spoken in Timor-Leste is wide. As the World Bank [2003; Volume I, p.96] notes, “The population is ethno-linguistically diverse, with more than 30 languages or dialects in use. Timor-Leste has adopted Portuguese and Tetun as official languages, with English and Indonesian accorded the status of working languages. [However,...] no more than one in twenty are fluent in Portuguese, and one in ten speak Tetun as a mother tongue, though it is more widely spoken by four in five people.” The Bank reports that Tetun is spoken by over 80% of the population it surveyed, Indonesian by over 40%, and Portuguese and English by less than 5%.

payoffs.

The experiment was carried out in one session in one of the classrooms of the National University of Timor-Leste with enough space for the experimenters to walk around, and for subjects' privacy. The session lasted about 3 hours. Prior to starting the session, subjects were provided with snacks and light refreshments.[7]

To begin the session, subjects were welcomed and reminded that they were to be paid $5 for their participation and that they could earn a considerable amount of additional money from the experiment. The experimenter then asked a volunteer to verify the number of balls in two bags that were to be used for the randomization procedures. One bag had 100 balls individually numbered from 1 to 100; another bag with 10 balls individually numbered from 1 to 10. The experiment was carried out manually, and all the instructions were read aloud in Portuguese by the same experimenter (Botelho) with the aid of visual displays.[8]

Following the procedures documented in Harrison, Lau, Rutström and Sullivan [2005], the experiment proceeded in four parts. Part I consisted of a questionnaire collecting subjects' socio-demographic characteristics. Part II consisted of four risk aversion tasks, and Part III consisted of six discount rate tasks. Part IV consisted of another questionnaire collecting information on the subjects' financial market instruments, and eliciting their expectations about their common and own future economic conditions.

After completion of Part I, and prior to starting Part II of the experiment, subjects were informed that they would be asked to make a number of decisions in Part II and Part III of the experiment. They were told that each subject would have a 10% chance of actually receiving the payment associated with one decision in Part II of the experiment *and* a 10% chance of actually

[7] An unfortunate fact of life in Timor-Leste is that some students go without food for extended periods if funds do not arrive for them in a timely manner. In some instances the experimenters have interrupted lectures to provide light food to students who fainted or appeared light-headed. We did not want such events to interrupt the experiment. Our experiences are not unique to the student population, as the discussion of food security in World Bank [2003; Volume II, chapter 7] documents.

[8] The experimenter has considerable experience conducting comparable experiments in the United States and Portugal.

receiving the payment associated with one decision in Part III of the experiment. They were also informed that we would determine whether a subject would receive the payment from Part II by having the subject extract one ball from the bag with 10 balls, and that the same procedure would be used for Part III. These procedures were also explained during the experiment. Finally, subjects were informed that all the randomization procedures were to be implemented only at the end of the entire session.

A. Risk Preferences

In Part II of the experiment we implemented four separate risk aversion tasks, each with different prizes. The four sets of prizes are defined directly in U.S. dollars, since this is the currency used in Timor-Leste. The four prize sets are as follows, where the first two prizes correspond to lottery A and the next two prizes correspond to lottery B: (A1: $64, $51; B1: $124, $3), (A2: $72, $48; B2: $128, $16), (A3: $64, $56; B3: $128, $5) and (A4: $80, $32; B4: $144, $2). The values of the prizes were converted from the values used by Harrison, Lau, Rutström and Sullivan [2005] in Denmark, using the ratio of purchasing power parity (PPP) to official exchange rate listed for Indonesia (because data for Timor-Leste is not available) in the 2003 *World Development Indicators*[9] and the exchange rate from the Danish Kroner (DKK) to US dollars as of May 2nd, 2004. Given that the original DKK values were set for a field experiment using the Danish adult population, their conversion to PPP dollars makes these prizes quite salient to students in Timor-Leste.

Table 2 shows the basic payoff table presented to subjects in the first risk aversion task.[10] The subjects choose A or B in each row, or they may express indifference between A or B. This format mimics the multiple price list (MPL) design devised by Holt and Laury [2002] to measure subjects' risk preferences, where each row corresponds to different probabilities of each prize. We actually implemented a two-stage MPL procedure, but focus here on the responses to the first

[9] Available at http://web.worldbank.org.
[10] Table A1 in the appendix displays the original table in Portuguese.

stage.[11]

B. Individual Discount Rates

When the four risk aversion tasks were completed, the experimenters initiated the six discount rate tasks. Each task corresponded to six different time horizons beyond the front-end-delay of 1 month: 1 month, 4 months, 6 months, 12 months, 18 months, and 24 months. Table 1 shows the basic payoff table presented to subjects in the first discount rate task.[12] Option A in each task offered $100 in one month. The principal of $100 was again converted from the 3,000 Danish Kroner (DKK) used by Harrison, Lau, Rutström and Sullivan [2005] in Denmark. In all other respects the procedures for the discount rate task follow those used for the risk aversion task. After that the first discount rate task was completed, the experimenters started the second discount rate task following the same procedures, and so on.

To ensure credibility of the payment instrument, subjects were informed at the outset of this part of the experiment that a certificate was to be given to those selected to actually receive money as a guarantee of payment. This certificate was signed by all three faculty members and was redeemable for cash, on or after the appropriate date, in the office of the person responsible for the FUP program in Timor-Leste. In this context we believe that this certificate had as much credibility as one could reasonably expect without a considerable administrative bureaucracy.

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[11] In order to elicit more refined measures of risk aversion generated by this set of prizes, we implemented a variant of what Harrison, Lau, Rutström and Sullivan [2005] call an Iterative MPL (iMPL). The implementation of the iMPL proceeded as follows: when collecting the subject's responses in the first payoff table, the experimenters identified the *first* row at which the subject switched from Option A to Option B and then presented the subject another payoff table with the same prizes and layout as the first payoff table but with different probabilities of each prize. That is, if in the first table the subject decided to switch from Option A to Option B between probability values of 0.3 and 0.4, the second table allows the subject to make more choices within this probability interval. After that the first risk aversion task was completed, and the experimenters started the second risk aversion task following the same procedures. This implementation of the iMPL differs from the computerized version used by Harrison, Lau, Rutström and Sullivan [2005] when the subject switches more than once in the first table. In their implementation the second level spans the first switch starting-point and the last switch end-point; in our manual implementation the second level only spans the interval defined by the first switching point. The statistical analysis of decisions presented here does not use the second level of decisions.

[12] Table A2 in the appendix displays the original table in Portuguese.

C. Payments

When the six discount rate tasks were completed subjects' were asked to answer the final questionnaire. The randomization procedures explained earlier were then used to give each subject a 10% chance to actually receive the payment associated with her decision in Part II of the experiment, and an independent 10% chance to actually receive the payment associated with her decision in Part III of the experiment. Randomization procedures were then implemented to determine which task was to be used for payment purposes for each subject selected to receive the payment associated with Part II of the experiment. Finally, the row to be used for those subjects was picked by randomization.[13]

3. Experimental Results

Most of the debates over discounting have to do with the validity of the discounted utility model proposed by Samuelson [1937] and axiomatized by Koopmans [1960], Fishburn and Rubinstein [1982] and others. As explained by Andersen, Harrison, Lau and Rutström [2005], to correctly infer discount rates in this model one must jointly estimate risk preferences and time preferences. We review their statistical procedures, and then apply them to our data.

General Statement

Consider the identification of risk and time preferences in the canonical case of mainstream economic theory. Specifically, assume EUT holds for the choices over risky alternatives, that subjects employ a CRRA utility function defined over the prizes they made choices over, and that discounting is exponential. Then a plausible axiomatic structure on preferences is known from

[13] If the selected row is *not* the one that the subject first chose Option B in the first table of the selected task, the lottery chosen is played out and the subject is paid. If the selected row is the row that the subject switched at, another random draw is made to determine a row in the second table that the subject was presented with, and the payment determined as described. If the selected row is one for which the subject expressed indifference, a random draw is made to determine whether the subject is paid according to Option A or Option B. The same procedures were then applied to Part III of the experiment. These procedures followed the logic explained in Harrison, Lau, Rutström and Sullivan [2005].

Fishburn and Rubinstein [1982; Theorem 2] to imply the stationary structure

$$U(M_t) = 1/(1+\delta)^{\tau}\, U(M_{t+\tau}) \tag{1}$$

where $U(M_t)$ is the utility of monetary outcome M_t for delivery at time t, δ is the discount rate, τ is the horizon for later delivery of a monetary outcome, and U is a utility function for money that is stationary over time. Thus δ is the discount rate that makes the present value of the *utility* of the two monetary outcomes M_t and $M_{t+\tau}$ equal. Most analyses of discounting models implicitly assume that the individual is risk neutral,[14] so that (1) is instead written in the more familiar form

$$M_t = 1/(1+\delta)^{\tau}\, M_{t+\tau} \tag{2}$$

in which δ is the discount rate that makes the present value of the two *monetary outcomes* M_t and $M_{t+\tau}$ equal.

To state the obvious, (1) and (2) are not the same. This observation has considerable implications for the identification of discount rates from observed choices over M_t and $M_{t+\tau}$. As one relaxes the assumption that the decision maker is risk neutral, it is apparent that the implied discount rate decreases since U(M) is concave in M. Thus one cannot infer the individual discount rate without knowing or assuming something about their risk attitudes.

Parametric Structure

We can quickly put some familiar parametric structure on this statement of the identification problem. Let the utility function be the CRRA specification

$$U(m) = m^{1-r}/(1-r) \tag{3}$$

where r is the CRRA coefficient and $r \neq 1$. With this parameterization, $r = 0$ denotes risk neutral behavior, $r > 0$ denotes risk aversion, and $r < 0$ denotes risk loving. The experimental evidence is that the CRRA parameter is roughly 0.5 in extensive laboratory experiments in the United States conducted by Holt and Laury [2002] and Harrison, Johnson, McInnes and Rutström [2005]. The

[14] See Keller and Strazzera [2002; p. 148] for an explicit statement of this assumption, which is often implicit in applied work.

general conclusion from these studies is that *the utility of money function is concave* over the domain of prizes relevant for these experiments.

Of course, the other two parametric components of the specification include the assumption of EUT over risky lotteries, and the assumption of constant, exponential discounting. Andersen et al. [2005] consider the effects of relaxing both of these when one has a positive FED, and we focus on relaxing the latter when one varies the FED by experimental treatment.

Statistical Specification

If one assumes that subjects made choices over uncertain lotteries, as well as over the timing of different prizes,[15] it is easy to write out the likelihood function and jointly estimate the parameter *r* of the utility function and the discount rate δ.

Consider first the contribution to the overall likelihood from the risk aversion responses. Probabilities for each outcome k_n, $p(k_n)$, are those that are induced by the experimenter, so expected utility is simply the probability weighted utility of each outcome in each lottery. If there were 2 outcomes in each lottery, as common in risk elicitation tasks of the type developed by Holt and Laury [2002], then the EU for lottery i is

$$EU_i = \sum_n [\, p(k_n) \times U(k_n) \,] \tag{4}$$

for n = 1, 2.

A simple stochastic specification from Holt and Laury [2002] is used to specify likelihoods conditional on the model. The EU for each lottery pair is calculated for a candidate estimate of r, and the ratio

$$\nabla EU = EU_R^{1/\mu} / (EU_R^{1/\mu} + EU_L^{1/\mu}) \tag{5}$$

calculated, where EU_L is the left lottery in the display and EU_R is the right lottery, and μ is a structural "noise parameter" used to allow some errors from the perspective of the deterministic

[15] Experiments in which the same subjects were asked for both types of responses are reported in Harrison et al. [2005], and used in Andersen et al. [2005]. But we can also pool responses from distinct experimental samples drawn from the same population.

EUT model. The index ∇EU is in the form of a cumulative probability distribution function defined over differences in the EU of the two lotteries and the noise parameter μ.[16] Thus, as $\mu \rightarrow 0$, this specification collapses to the deterministic choice EUT model, where the choice is strictly determined by the EU of the two lotteries; but as μ gets larger and larger the choice essentially becomes random. This is one of several different types of error story that could be used. The index in (5) is linked to the observed choices by specifying that the right lottery is chosen when $\nabla EU > 0.5$.

Thus the likelihood of the risk aversion responses, conditional on the EUT and CRRA specifications being true, depend on the estimates of r and μ given the above statistical specification and the observed choices. The conditional log-likelihood is

$$\ln L^{RA}(r, \mu; y, X) = \sum_i [(\ln (\nabla EU) \mid y_i=1) + (\ln (\nabla EU) \mid y_i=0)] \quad (6)$$

where $y_i = 1(0)$ denotes the choice of the right (left) lottery in risk aversion task i, and X is a vector of individual characteristics.

Turning to the discount rate choices, a similar specification is employed. Equation (4) is replaced by the present value of the utility of the two outcomes, conditional on some assumed discount rate, and equation (5) is defined in terms of those present values instead of the expected utilities. The present value of the utility of M_t at t is just

$$PV_L = U(M_t) \quad (7)$$

and the present value of the utility of $M_{t+\tau}$ at $t+\tau$ is

$$PV_R = 1/(1+\delta)^\tau \, U(M_{t+\tau}) \quad (8)$$

where the subscripts L and R refer to the left and right options in the choice tasks presented to subjects, illustrated in Table 1. The parametric form for the utility function in (7) and (8) is the CRRA form given in (3), so we can rewrite these as

$$PV_L = M_t^{1-r}/(1-r) \quad (7')$$

[16] An alternative approach might be to define an index as the difference of the EU's of the two lotteries, and then specify some cumulative distribution function to link it to the observed choices. For example, the cumulative standard normal distribution leads to the probit specification.

$$PV_R = [1/(1+\delta)^\tau] \times [M_{t+\tau}^{1-r}/(1-r)] \quad (8')$$

An index of the difference between these present values, conditional on δ and r, can then be defined as

$$\nabla PV = PV_R^{1/\nu} / (PV_R^{1/\nu} + PV_L^{1/\nu}) \quad (9)$$

where ν is a noise parameter for the discount rate choices, just as μ was a noise parameter for the risk aversion choices. It is not obvious that $\mu = \nu$, since these are cognitively different tasks. Our own priors are that the risk aversion tasks are harder, since they involve four outcomes compared to two outcomes in the discount rate tasks, so we would expect $\mu > \nu$. Error structures are things one should always be agnostic about since they capture one's modeling ignorance, and we allow the error terms to differ between the risk and discount rate tasks.

Thus the likelihood of the discount rate responses, conditional on the EUT, CRRA and exponential discounting specifications being true, depend on the estimates of r, δ and ν given the above statistical specification and the observed choices. The conditional log-likelihood is

$$\ln L^{DR}(r, \delta, \nu; y, X) = \sum_i [(\ln (\nabla PV) \mid y_i=1) + (\ln (\nabla PV) \mid y_i=0)] \quad (10)$$

where y_i =1(0) denotes the choice of the right (left) option in discount rate task i, and X is a vector of individual characteristics.

The joint likelihood of the risk aversion and discount rate responses can then be written as

$$\ln L (r, \delta, \mu, \nu; y, X) = \ln L^{RA} + \ln L^{DR} \quad (11)$$

and maximized using standard numerical methods. Our implementation uses version 9 of *Stata*.[17]

Two features of this estimation procedure should be noted. First, it is an easy matter to allow each parameter in (11) to be linear function of observable characteristics of individuals and/or treatment effects. For example, we could allow for the CRRA coefficient r to depend on the sex of the subject, so that we would estimate

$$\hat{r} = \hat{r}_0 + (\hat{r}_{FEMALE} \times FEMALE) \quad (12)$$

[17] In documentation available at http://exlab.bus.ucf.edu we provide all source code and data for the estimates reported here.

where $\hat{r}_0$ is the estimate of the constant and $\hat{r}_{FEMALE}$ shows the difference in risk for females. In general we report unconditional estimates of the parameters, since that is sufficient for our methodological purposes. But it is important to know that the specification extends easily, since demographic effects are of considerable policy significance. Second, the statistical specification allows for the possibility of correlation between responses by the same subject.[18]

Results with Exponential Discounting

The results of estimating this model using expoenential discounting are reported in panel A of Table 3. Subsequent panels show the effects of using alternative discounting functions. The results indicate risk aversion by our subject pool, since r>0 and this estimate is statistically significant with *p*-values less than 0.001. This estimated CRRA coefficient is generally consistent with previous evidence from a wide range of laboratory and field subjects, reported in Holt and Laury [2002], Harrison, Johnson, McInnes and Rutström [2005] and Harrison, Lau, Rutström and Sullivan [2005]. The estimates differ slightly as we vary the specification of the discounting function, since the CRRA coefficient is also playing a role in explaining those data, but by and large it is determined by the responses to the risk attitude tasks.

Our results indicate a relatively large "error term" for risk attitude responses: μ is generally around 0.29, which is relatively large compared to comparable estimates from developed countries.

Results with Exponential Discounting

The estimated discount rate is 12.7% per annum from panel A of Table 3. The 95% confidence interval for this estimate is quite wide, ranging from 4.1% to 21.3%. We examine the role

[18] The use of clustering to allow for "panel effects" from unobserved individual effects is common in the statistical survey literature. Clustering commonly arises in national field surveys from the fact that physically proximate households are often sampled to save time and money, but it can also arise from more homely sampling procedures. The procedures for allowing for clustering allow heteroskedasticity between and within clusters, as well as autocorrelation within clusters. Wooldridge [2003] reviews some issues in the use of clustering for panel effects, in particular noting that significant inferential problems may arise with small numbers of panels.

of observed demographic characteristics later.

Quasi-Hyperbolic Discounting

The most important alternative to exponential discounting is the quasi-hyperbolic (QH) specification that assumes that individuals have some "passion for the present" that leads to any future payment being discounted relative to current payments. The standard QH specification proposed by Laibson [1997] then assumes that individuals have constant discount rates for all future time delays.[19] This means that we would replace

$$PV_R = 1/(1+\delta)^{\tau}\, U(M_{t+\tau}) \tag{8}$$

with

$$PV_R = \beta/(1+\delta)^{\tau}\, U(M_{t+\tau}) \tag{8*}$$

where $\beta < 1$ is the parameter capturing a "passion for the present." This implies that the subject has some preference for the M_t outcome in comparison to all of the $M_{t+\tau}$ outcomes, irrespective of $\tau>0$. The decision maker still discounts later payoffs for $\tau>0$, but in the standard manner. Thus, as $\tau \rightarrow \infty$ the discounting function looks more and more like the standard exponential model.

Panel B of Table 3 contains estimates for the quasi-hyperbolic specification. We estimate β to be 0.963, and reject the two-sided hypothesis that it is equal to 1 with a *p*-value of 0.029. The 95% confidence interval for the estimate of β ranges from 0.930 to 0.996.

Psycho-Hyperbolic Discounting

The earliest hyperbolic (H) specifications assumed that individuals had discount rates that declined with the horizon they faced, in contrast to the QH specification that posits an initial decline and then constant (per period) discount rates. The most common functional form of the older literature is due to Herrnstein [1981], Ainslie [1992] and Mazur [1987], and would replace

[19] This specification was first used by Phelps and Pollak [1968] to represent imperfect altruistic preferences across generations.

$$PV_R = 1/(1+\delta)^\tau\ U(M_{t+\tau}) \qquad (8)$$

with

$$PV_R = 1/(1+\gamma\tau)\ U(M_{t+\tau}) \qquad (8^{**})$$

for $\gamma > 0$.

Maximum likelihood estimates are presented in panel C of Table 3. We estimate γ as 0.135, implying a declining discount rate with time. Figures 1 and 2 show the implied estimates of annual discount rates as a function of time, where the bottom axis shows fractions of one year. The dashed lines are the discount rates implied by these estimates of the hyperbolic model, and the solid lines are the estimated discount rates from the exponential model (from Table 3). The hyperbolic model shows clear evidence of some decline in discount rates with horizon, although this mathematical specification virtually ensures that one will find such a result.[20]

Social-Hyperbolic Discounting

Harvey [1986] considered the type of discounting function that would be appropriate for a social planner in an infinite-horizon setting. Although the functional form he proposed had no foundation in the behavior of individual actors, the context could be viewed as appropriate for decisions by an individual or household. In any event, the functional form has proven popular, and would replace

$$PV_R = 1/(1+\delta)^\tau\ U(M_{t+\tau}) \qquad (8)$$

with

$$PV_R = 1/(1+\tau)^\lambda\ U(M_{t+\tau}) \qquad (8^{***})$$

for λ typically > 0. Maximum likelihood estimates using this specification are reported in panel D of Table 3, and λ is estimated to be 0.199.

[20] Imagine that the data actually showed exponential discounting at some positive discount rate. Then maximum likelihood estimates of γ would have to be positive to come close to fitting such data, and would imply the declining slope shown in Figure 2.

Generalized Hyperbolic Discounting

Loewenstein and Prelec [1992] propose a generalized of the hyperbolic specifications in the literature, and their functional form has the nice property that it nests the exponential as one of the parameters tends to 0. Specifically, the propose replacing

$$PV_R = 1/(1+\delta)^{\tau}\, U(M_{t+\tau}) \tag{8}$$

with

$$PV_R = 1/(1+\xi\tau)^{\eta/\xi}\, U(M_{t+\tau}) \tag{8****}$$

for η, $\xi > 0$. As $\xi \rightarrow 0$, $PV_R \rightarrow e^{-\eta\tau}$, which is exponential discounting. If $\eta/\xi = 1$ then this specification collapses to the pscyho-hyperbolic specification in (8**), and if $\xi = 1$ this specification collapses to the social-hyperbolic specification in (8***).[21]

Maximum likelihood estimates using this specification are reported in panel E of Table 3. Although this generalized hyperbolic specification has the attractive property that it nests several specifications, that very generality also means that the sample size we have does not allow precise parameter estimates. For our purposes the important result is that ξ is estimated to be 0.691, but is statistically not different from zero at a *p*-value of 0.751. Thus we *cannot reject the exponential model against the alternative of the hyperbolic specifications*.[22]

We stress that this important conclusion could be due to (a) sampling variability due to uncontrolled variability in the "representative agent" specification estimated in Table 3, (b) sampling variability due to unobserved variability in the observed characteristics of the sample, (c) sampling variability due to small sample sizes, or (d) the invalidity of the hyperbolic specification in relation to the exponential specification. The remedy for alternatives (a) and (c) are simple enough: add controls for observable characteristics, and increase sample size, respectively.

[21] One could also add a β term to the numerator of the right-hand side of (8****) and nest the quasi-hyperbolic specification as $\beta<1$ and $\xi \rightarrow 0$.

[22] To be precise, at the risk of some double negatives, this does *not* say that we cannot reject the exponential against the *quasi*-hyperbolic alternative.

Demographic Controls

The estimates in Table 3, and the displays in Figures 1 and 2, assume that there is just one agent when in fact there were 30 agents. This representative agent assumption can be evaluated, and the effects of demographics identified, by allowing each of the parameters of the discounting functions to be a linear function of observable characteristics.[23]

The demographic characteristics we use are binary indicates for sex (female), whether the subject lives in their own home (home), whether the head of the household works in agriculture or fishing (work), whether the head of the household has more than a primary education (h_education), and whether the person lives in a household with a middle to high income level above $1,644 in 2003 (incomeHI).[24] We also include variables to measure age above 19 (age), noting that no subject was below 19, the year of their undergraduate education (year, which can be 1, 2 or 3), and the number of people in their household. Our sample was quite mixed, with 47% being female, 69% living in their own home, 36% of the household heads working in agriculture or fishing, and 56% of them having a household head that had some post-primary education. The average age was 21.7, with a range between 19 and 37, so our sample is generally quite young.[25] The average household size was 8.5, and ranged from 2 to 15. Finally, 43% lived in households the year before that could be classified as having a middle to higher income level.

The maximum likelihood estimation results are reported in Table 4 for the exponential and quasi-hyperbolic specifications.[26] A Wald test can be used to test the joint significance of the demographic variables for each parameter. In general we can easily reject the null that the

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[23] We also allow the risk aversion coefficient to be a linear function of these characteristics.

[24] The World Bank [2001; Volume I, p.32] defines a consumption-based poverty measure for Timor-Leste of $15.44 per capita per month in 2001. This measure is dominated by food, which accounts for $10.81, or 70%, of the total. The CIA *Factbook* estimates the inflation rate at roughly 4% p.a. in 2003, and with an average of 7.15 people in each household surveyed by the World Bank, the annual poverty line in 2003 for a household would be $1490. So our cut-off income level is only 10% above the implied poverty line.

[25] The United Nations Development Program estimates life expectancy at only 49.3 years in 2002 at http://hdr.undp.org/statistics/data/cty/cty_f_TMP.html.

[26] Detailed results for the other hyperbolic specifications are included in the statistical log file available at http://exlab.bus.ucf.edu.

demographic characteristics have zero effect on the parameter, with the exception of the β parameter in the quasi-hyperbolic specification.[27] In this case the *p*-value from the Wald test is 0.48. Of course, some individual variables are significant for each parameter.

The estimates for the CRRA coefficient are virtually identical for the two specifications, as one would expect from the fact that they are estimated from the same risk aversion responses. They are not identical, of course, since risk attitudes also play a role in explaining the discounting choices, conditional on the specification assumed. Older subjects tend to be less risk averse, as do those that live in their own home.

The estimates for the β parameter of the quasi-hyperbolic specification show some effects from demographics, even if the joint significance of demographic effects is rejected. Older subjects are more likely to have a "passion for the present," although it should be noted that most of our subjects were relatively young. On the other hand, this is more than offset by additional years of undergraduate education: on average, our subjects had 2 years of college education, so they had a β parameter that was roughly 0.14 higher than others. These results suggest that the older subjects that chose to pursue college education had a higher short-term discount rate than their cohorts, but that the effect of accumulating the education (or simply the degree) by itself is to make the short-term discount rate more consistent with the long-term discount rate. When the quasi-hyperbolic specification in Table 4 is estimated with no demographics for β the point estimate is 0.96, and a (two-sided) test of the hypothesis that it is equal to 1 has a *p*-value of 0.039. This we find some evidence that is consistent with the quasi-hyperbolic specification, even though we had a one month front end delay on the earliest option. Thus we would expect to see an even larger effect if we had employed a front end delay that was shorter than one month.

The estimates for the δ parameter show a consistent effect from living in one's own home. For both specifications this increases the estimated discount rate. To the extent that responses to

[27] The *p*-values for the r parameter are 0.0008 and 0.0001 for the two specifications, respectively. For the δ parameter the *p*-values are 0.077 and 0.0091, respectively.

this demographic reflect living in the "family home," it might just reflect an impatience to leave home now that the formal education is nearing a close. On the other hand, responses to this demographic may also have reflected own-ownership of the house one lived in, suggesting a different explanation for the effect on discount rates: a concern to pay off any debts incurred in purchasing the house.

Finally, including demographics for the core parameters does not reduce the estimated size of the error terms μ and ν. Thus we conclude that these are attributable to unobserved characteristics, sampling variability and/or inherent noise in decision-making in this setting.

4. Conclusions

We draw three substantive conclusions, and a series of methodological conclusions about the feasibility of eliciting discount rates in developing countries.

First, we find discount rates that are roughly the same as those found for populations in the United States and Denmark, using closely comparable procedures that correct for the concavity of the utility of money. Second, we find evidence of heterogeneity in elicited discount rates, and discount rate functions, that is correlated with observable demographic characteristics. Thus it is inappropriate to assume one discount rate, or risk attitude, when undertaking welfare evaluations of public policy. Our conclusions about heterogeneity are, of course, constrained by the limited sample size of our subject pool, as well as their homogeneity relative to the population of Timor-Leste. Third, we find some evidence in favor of the quasi-hyperbolic specification, particularly after controlling for observable demographic characteristics. The last finding, in particular, is one that would be important if it survives with larger samples and extensions of procedures.

How would we change our procedures for additional experiments, particularly if one were going out into the field in Timor-Leste? We offer several suggestions from our experiences, motivated by some of the policy issues facing Timor-Leste.

Decisions about the use of durable goods "today" are guided by the high discount rates brought about by proximity to the absolute poverty line, and hence could generate a poverty spell "tomorrow." As noted by Morduch [1994; p.224]:

> In terms of vulnerability, persistence in income patterns is again created for a reason unrelated to the stochastic nature of the income process, as Mark Rosenzweig and Kenneth Wolpin [1993] demonstrate in analyzing investments in bullocks in India. As productive assets are depleted to protect consumption today, poor households will face lower expected income in the future.

Of course, "consumption" here might refer to expenditures on food or medical services needed for physical survival. The results from Coller, Harrison and Rutström [2003] suggest dramatic effects from the ability to delay important inter-temporal decisions for some "brief period." In their design subjects faced either no front end delay, a 7-day front end delay, or a 30-day front end delay. Behavior in the two extreme cases was strikingly different, with extremely high discount rates observed for the case in which there was no front end delay. More consistent decisions result when there was some front end delay. In the development context, such consistency might mitigate the risk that a distress sale of a bullock will plunge the household into longer-term poverty. Normatively, it suggests the potential importance of resources being available to provide the poor with a *temporary* front end delay on such decisions.

In the case of Timor-Leste, there is considerable evidence that food security is a serious matter. Respondents to a household survey indicated that there was not enough food in 3.6 months of the year, although this was as low as 1.8 in the major urban areas of Dili and Baucau (see World Bank [2003; Volume II, Chapter 7]). Perhaps as significant, 86% of the population stated that they had at least one month without enough food, and these rates were generally over 90% outside of the major urban areas. Differences in vulnerability appear to be tightly correlated with dependence on agriculture as a livelihood, and the onset of the rainy season,[28] since there are few alternatives in most rural areas. The *ex post* responses to these shortages include reduced food intake, changes in diet, and the sale of livestock or other assets. There appears to be relatively little reliance on intra-

[28] The rainy season in Timor-Leste is centered around December through May, although it can come sooner and last longer.

household or intra-village transfers to mitigate shortages, since the factors generating the shortage are generally common to all.

This fact of life in Timor-Leste has some immediate implications for the type of design that would be the most informative. Variations in the length of the front end delay would allow one to ascertain the period of time that is sufficient for subjects to avoid the "passion for the present" if that is observed. This would be expected to be particularly important for subjects that are currently in some period of poverty. If the average spells of food insecurity, as one measure of poverty, range from 1 to 3 months, then variations in the front end delay of that range should be considered.

Another implication of the concern about food security is that it might be valuable to consider different ways of presenting the choices to subjects that better reflect the temporal nature of the uncertainty faced. For transparency, we currently use an a-temporal instrument to measure risk aversion, and a non-stochastic instrument to measure time preferences. There may be some virtue in adding an instrument that combines the two, and examines their interaction.[29]

Similarly, there may be some value in examining the use of non-monetary outcomes in these instruments. Money is a natural commodity to use for many purposes, and many decisions faced by individuals deal directly with money, but it is possible that some subjects in the field might exhibit different preferences towards lotteries defined over money than lotteries defined over non-monetary outcomes. There is some evidence that this may be the case when the non-monetary outcome is associated with considerable "background risk" that requires contextual knowledge to evaluate (e.g., see Harrison, List and Towe [2004]). In the field in Timor-Leste, the type of context that would be relevant would presumably be the ability to diversify sources of food and spread consumption over time. For example, Pender [1996] conducted an experiment to elicit time preferences in rural India in which the commodity in question was a bag of rice "sooner" or more rice "later."[30]

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[29] See Anderhub, Güth, Gneezy and Sonsino [2001] for experiments using one such instrument, and Ahlbrecht and Weber [1997] for a similar instrument applied to hypothetical choices.

[30] This study is remarkable for also recognizing the importance of using a front end delay, and being concerned with the effect of censoring from field trading opportunities. These themes were emphasized by Coller and Williams [1999] in a more traditional laboratory setting.

Turning now to more procedural and technical issues, there are six general features of the overall experimental design that we would modify if we were to implement field experiments in Timor-Leste.

First, we would conduct complementary experiments in a laboratory and field setting. The former would be conducted in traditional settings for laboratory experiments, such as the sample used in our pilot experiments. These could be used to check for procedural or treatment effects that might not be easily implemented in the field. For example, as we pare the instructions down to a length appropriate for field application with less literate subjects, it would be valuable to check if these changes had any effect in the original student population. Such effects could then be calibrated with responses from the lab, so as to correct estimates from the field.[31] This is likely to be significant in Timor-Leste, with wide variations in the literacy rate that are correlated with age. The World Bank [2003; Volume I, p.xi] reports a literacy rate of 77% in those aged 15-24, but only 48% in those aged above 15. We would undertake lab experiments in which we examined the effects of reducing the script down to just one page for each of the risk aversion and discount rate instruments, where we used Tetun instead of Portuguese, and where we only presented the instruction orally (instead of in writing and orally). In some field applications in the United States we have successfully used one-page versions of the risk aversion instrument (e.g., see Elston, Harrison and Rutström [2005]). Another issue which can be examined in a laboratory setting is the difference between hypothetical and real questions to elicit time preferences. Coller and Williams [1999] report a significant difference. If it is possible to identify a stable statistical relationship between hypothetical and real choices of this kind, it might be possible to use hypothetical surveys as part of a larger household survey (e.g., Barr and Packard [2002] in Chile) and just use a sub-sample for the experimental tasks with real consequences.

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[31] The idea of statistical calibration in related areas, to address effects of hypothetical bias, is introduced in Blackburn, Harrison and Rutström [1994]. The literature on hypothetical bias also differentiates between *ex ante* "instrument calibration" and *ex post* "statistical calibration," as reviewed by Harrison [2005]. The former is the experimental economists' counterpart of the use of focus groups in survey research.

It would be logistically efficient to initially test some of these variations with convenience samples in laboratory settings in developed countries, such as the United States or Portugal. Some variations, of course, would have to be tested with subjects from Timor-Leste, although one could still employ convenience samples of college students in Dili for some of these tests. The goal would be to refine the instrument as far as possible using such subjects, and then test it in the field prior to full-scale implementation.

Second, we would only implement the first level of the multiple price list procedure. The use of the second level is not needed for the estimation procedures used here, and adds considerable complexity in a field setting. There are some instructional "economies of scale" for subjects that are able to participate for a session lasting an hour or two, since the iterative procedure can be explained in one familiar setting (e.g., willingness to pay for some commodity). But this extra stage should be dropped for field experiments in which one does not have time to undertake such instructions.

Third, a training experiment with some deliverable commodity should be employed to ensure that subjects have some "hands on" experience with the procedure. In previous experiments such trainers have used candy, which obviously provides some additional culinary benefits beyond the pedagogic ones.

Fourth, one could reduce the number of risk aversion tasks to one per subject, to speed up the experiment. Four tasks were used by Harrison, Lau, Rutström and Sullivan [2005] in an effort to provide a more refined estimate of risk attitudes. But one could instead vary the prize sets across subjects, so that the same variation was obtained for the sample as a whole.

Fifth, one could reduce the number of discounting tasks to one per subject,[32] so that each subject is randomly assigned a horizon in the manner of Coller, Harrison and Rutström [2003]. This removes any concerns about horizon effects being confounded with order effects.[33] Or one could

[32] There is obvious benefit from having the same subject complete at least one risk aversion task and at least one discount rate task, so these should not be between-subjects unless necessary in terms of logistics.

[33] Harrison, Johnson, McInnes and Rutström [2005] report evidence of order effects in risk aversion elicitation.

have each subject provide several responses for different horizons, but vary the order: one group could start with the shorter horizons and then do the longer horizons, and another group could start with the longer horizons and then do the shorter horizons. Randomizing the horizons for a given subject is not attractive, since it might generate confusion. These variations are ones that we would ideally want to pre-test in lab experiments in developed countries, and then in lab experiments in Timor-Leste itself, since they are so central to one of the main inferences from these experiments (the nature of the discount rate function). If samples are large enough, one could implement several variations, so that some subjects receive only one horizon selected at random, other subjects receive several horizons in ascending order, and other subjects receive several horizons in descending order. This would allow tests to be developed for an order effect, and statistical corrections to be effected *ex post* if there was any such effects.[34]

Sixth, it would be useful to vary the principal amount offered to subjects in the discount rate experiments. This variation should only be undertaken on a between-subjects basis, to avoid confusion. The reason for this treatment is to be able to better understand the "monetary premium" that subjects have if they exhibit quasi-hyperbolic preferences. That premium can be viewed as either a *fixed monetary amount* required in addition to the principal in order to defer receipt of any monetary amounts, or as a *percentage of the principal* required. Experiments in which every subject receives the same principal cannot be used to determine which of these two best describes behavior, since they are observationally equivalent. But if one varies the principal it would be an easy matter to tease these two hypotheses apart, assuming there is some evidence for quasi-hyperbolic behavior. The reason that this is important is for policy applications of the elicited discount rate functions. If the premium is a fixed one, then it can be assumed to be trivial for larger investment opportunities than those offered in our experiments; but if it is a percentage of the principal then it can be assumes to "scale up" for larger investments.

[34] Andersen et al. [2004] demonstrate how one can use laboratory experiments to check for such order effects, if it is not possible to test for them in the field.

Table 1: Payoff Matrix for One Discount Rate Elicitation Task

Payoff Alternative	**Payment Option A** (pays amount below in 30 days)	**Payment Option B** (pays amount below in 60 days)	**Annual Interest Rate** (AR)	**Preferred Payment Option** (Circle A or B or I)
1	$100	$100.41	5%	A B I
2	$100	$100.83	10%	A B I
3	$100	$101.23	15%	A B I
4	$100	$101.64	20%	A B I
5	$100	$102.04	25%	A B I
6	$100	$102.44	30%	A B I
7	$100	$102.84	35%	A B I
8	$100	$103.23	40%	A B I
9	$100	$103.62	45%	A B I
10	$100	$104.00	50%	A B I

Table 2: Payoff Matrix for One Risk Aversion Elicitation Task

Lottery A				*Lottery B*				**Preferred Payment Option** (Circle A or B or I)
Probability	Outcome	Probability	Outcome	Probability	Outcome	Probability	Outcome	
0.1	$64	0.9	$51	0.1	$124	0.9	$3	A B I
0.2	$64	0.8	$51	0.2	$124	0.8	$3	A B I
0.3	$64	0.7	$51	0.3	$124	0.7	$3	A B I
0.4	$64	0.6	$51	0.4	$124	0.6	$3	A B I
0.5	$64	0.5	$51	0.5	$124	0.5	$3	A B I
0.6	$64	0.4	$51	0.6	$124	0.4	$3	A B I
0.7	$64	0.3	$51	0.7	$124	0.3	$3	A B I
0.8	$64	0.2	$51	0.8	$124	0.2	$3	A B I
0.9	$64	0.1	$51	0.9	$124	0.1	$3	A B I
1	$64	0	$51	1	$124	0	$3	A B I

Table 3: Estimates of Time Preferences in Timor-Leste

Parameter	Estimate	Standard Error	*p*-value	Lower 95% Confidence Interval	Upper 95% Confidence Interval
			A. Exponential Discounting Model		
r	0.615	0.127	0.000	0.367	0.864
δ	0.127	0.044	0.004	0.041	0.213
μ (for RA)	0.289	0.070	0.000	0.151	0.426
ν (for DR)	0.095	0.033	0.003	0.032	0.159
			B. Quasi-Hyperbolic Discounting Model		
r	0.615	0.127	0.000	0.367	0.864
β	0.963	0.017	0.000	0.930	0.996
			(H_o: β=1) 0.029		
δ	0.097	0.039	0.013	0.020	0.174
μ (for RA)	0.289	0.070	0.000	0.151	0.426
ν (for DR)	0.097	0.032	0.003	0.033	0.160
			C. Herrnstein-Ainslie-Mazur Psycho-Hyperbolic Model		
r	0.608	0.121	0.000	0.371	0.845
γ	0.135	0.046	0.004	0.044	0.226
μ (for RA)	0.292	0.067	0.000	0.161	0.424
ν (for DR)	0.097	0.031	0.002	0.036	0.158
			D. Harvey Social-Hyperbolic Model		
r	0.615	0.127	0.000	0.367	0.864
λ	0.199	0.064	0.002	0.073	0.324
μ (for RA)	0.289	0.070	0.000	0.151	0.426
ν (for DR)	0.099	0.035	0.004	0.031	0.167
			E. Loewenstein-Prelec Generalized Hyperbolic Model		
r	0.615	0.127	0.000	0.367	0.863
ξ	0.691	2.180	0.751	-3.581	4.963
η	0.177	0.158	0.262	-0.132	0.486
μ (for RA)	0.289	0.070	0.000	0.152	0.426
ν (for DR)	0.098	0.032	0.002	0.035	0.160

Figure 1: Estimated Discount Rates and Functions

Specifications estimated assuming EUT

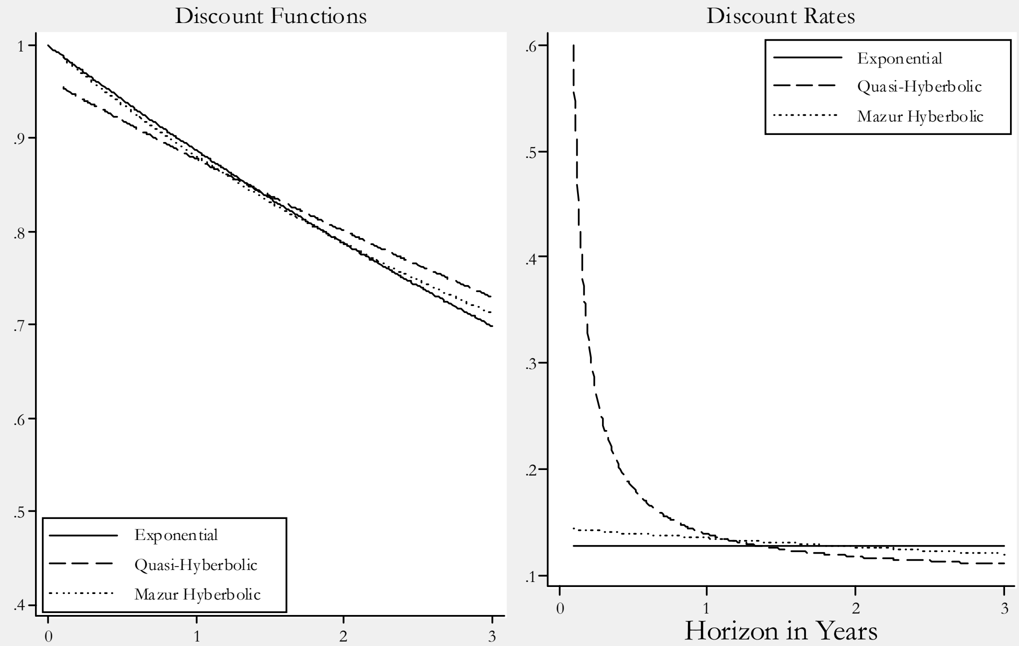


Figure 2: Discount Rates With No Demographic Controls

Estimated rate and 95% confidence interval

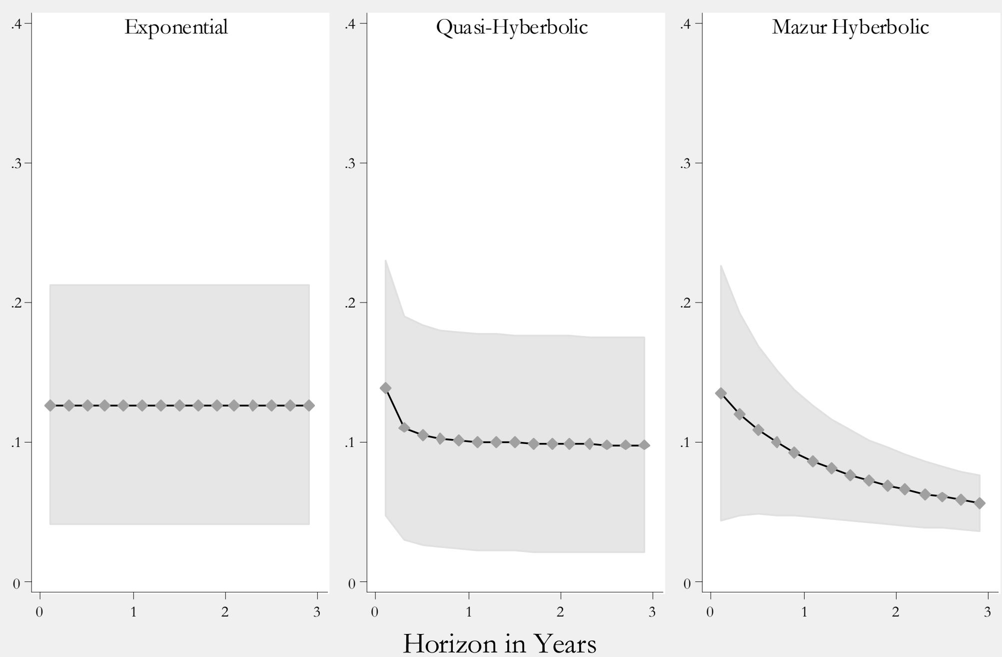
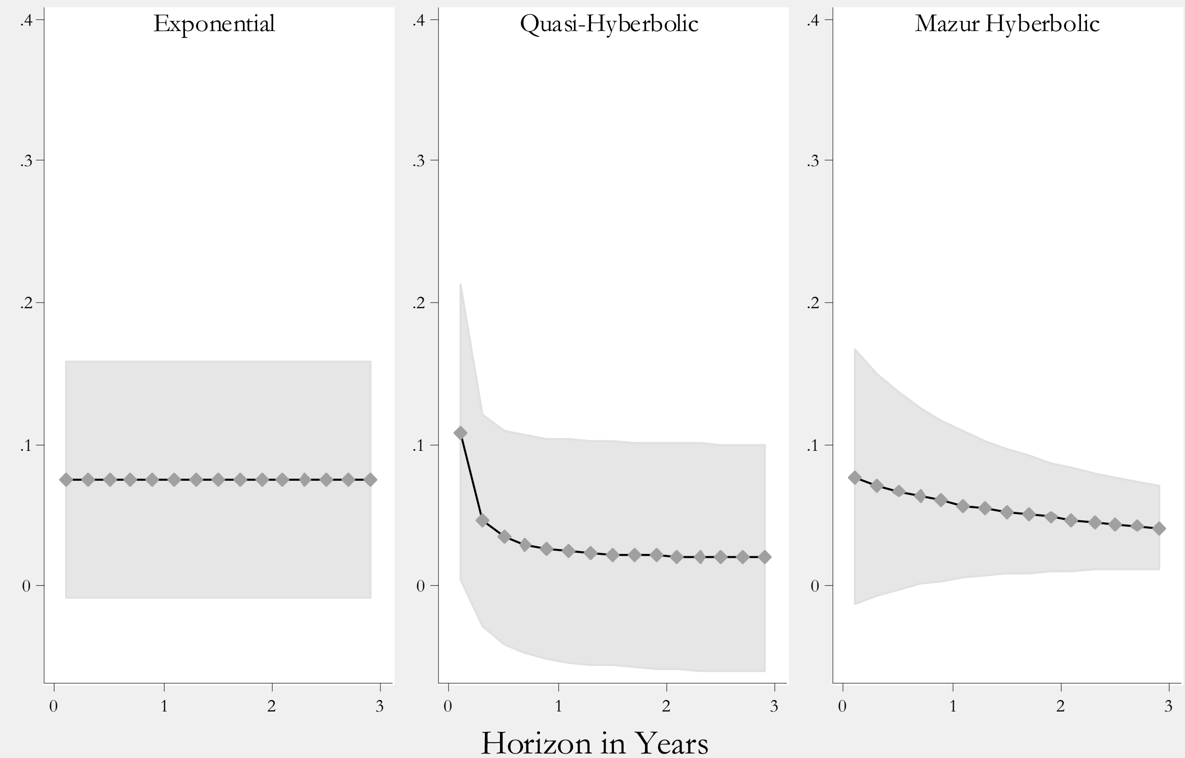


Table 4: Effect of Demographics on Time Preferences in Timor-Leste

Core Parameter	Demographic Variable	Description of Variable	*Exponential* Estimate	*Exponential* *p*-value	*Quasi-Hyperbolic* Estimate	*Quasi-Hyperbolic* *p*-value
r		Constant	0.62	0.01	0.59	0.01
	female	Female	0.10	0.46	0.10	0.49
	age	Age over 19 in years	-0.05	0.06	-0.06	0.03
	year	Year in college	0.12	0.27	0.13	0.23
	home	Live in own home	-0.22	0.02	-0.21	0.03
	work	Works as a farmer or a fisherman	-0.07	0.62	-0.08	0.60
	h_education	Head of household has post-primary education	-0.03	0.82	-0.04	0.79
	nhhd	Number in household	0.00	0.87	0.00	0.79
	incomeHI	Medium or high household income	-0.04	0.77	-0.04	0.78
β		Constant			0.77	0.00
	female	Female			-0.03	0.41
	age	Age over 19 in years			-0.01	0.02
	year	Year in college			0.07	0.04
	home	Live in own home			0.05	0.24
	work	Works as a farmer or a fisherman			-0.06	0.16
	h_education	Head of household has post-primary education			0.06	0.13
	nhhd	Number in household			0.01	0.22
	incomeHI	Medium or high household income			-0.03	0.53
δ		Constant	0.12	0.17	-0.06	0.47
	female	Female	-0.01	0.85	-0.04	0.40
	age	Age over 19 in years	0.01	0.29	0.00	0.92
	year	Year in college	-0.03	0.47	0.02	0.50
	home	Live in own home	0.13	0.01	0.17	0.00
	work	Works as a farmer or a fisherman	0.07	0.23	0.02	0.66
	h_education	Head of household has post-primary education	0.00	1.00	0.06	0.27
	nhhd	Number in household	-0.01	0.38	0.00	0.98
	incomeHI	Medium or high household income	0.01	0.85	-0.01	0.87
μ (for RA)			0.28	0.00	0.28	0.00
ν (for DR)			0.10	0.00	0.10	0.00

Figure 3: Discount Rates With Demographic Controls

Estimated rate and 95% confidence interval



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Appendix: English Translation of Portuguese Experimenter Script

WELCOME TO THE EXPERIMENT

This is an experiment in the economics of decision making. Your participation in this experiment is voluntary. However, we think you will find the experiment interesting.
You will be paid for your participation and you could make a considerable amount of additional money. How much you receive will depend partly on chance and partly on the choices you make in series of decision problems which will be presented to you in a few minutes.

The problems are not designed to test you. What we want to know is what choices you make in them. The only right answer is what you really choose.

The experiment will proceed in four parts:

Part I consists of some questions about yourself. This information is for our records only, and the results of our study will not identify any individual or the choice he or she made in any way. All records will be linked to an anonymous Participant number only.

Part II is a decision problem in which chance may play a part. Your decision problem requires you to make a series of choices between two payment options. This will be described in detail to you once everyone has completed the first part.

Part III is a different decision problem in which chance may also play a part. This part will be described to you after everyone has completed the second part.

Part IV consists of some additional questions about yourself. Again, this information is for our records only and the confidentiality of your responses is assured.

At this time I would like to ask one person to come up here and inspect the two black bags we have here and will use several times during the experiment. Please verify that we have exactly 100 balls numbered from 1 to 100 in the black bag identified with the number "100," and that we have exactly 10 balls numbered from 1 to 10 in the black bag identified with the number "10". I will now ask you to place these balls into the respective black bags.

Now we ask that you answer the questions for Part I using the material that you have on your desk.

Instructions for Part II

We are now ready to go on to Part II of the experiment. Before we start with the detailed instructions for this part, however, I will explain in how your earnings from this experiment will be determined, and would like to make sure you understand it before we go any further.

Earnings

Just for your participation, you will receive a guaranteed participation fee of $5.

Each person in this room will have a chance to receive an additional large sum of money. Each person will have a 1-in-10 chance of receiving the money. The selection will be done using the black bag with 10 balls numbered from 1 to 10. Each of you will extract one ball from this bag, and if the ball numbered 1 is extracted you will receive the money at the end of the session. If any other

number is extracted you will not receive the money. The extraction of balls from this bag will only be carried out at the end of today's session. Actually, we will proceed with the extraction of balls both for Part II and Part III of the experiment only at the end of the session.
As you will see in a moment, in Part II of the experiment you will be asked to make a series of choices between two payment options in several decision problems. However, we will pay you for only one of these decisions. That is, if you are selected to receive the money from this part of the experiment, we will randomly determine the one decision for which you will be paid. The procedures to do so ensure that each decision is equally likely to be chosen. The same applies to Part III of the experiment. The procedures to randomly determine the decision in Part II and in Part III of the experiment for which you will be paid will be explained to you in detail later, and you will be reminded of them before we actually implement them.

The important thing to notice now is that since you don't know whether you will be selected to receive the money, and for which decision you will be paid if you are selected, it is in your best interest to pay attention and think carefully about all the decisions that you will face since all of them involve considerable amounts of money.
Are there any questions?

Detailed instructions for Part II

This part has 4 decision problems that are exactly the same except that the amounts of money involved will differ.

There are <u>10 decisions</u> to be made in each decision problem.

There are <u>2 payment options</u> in each decision: Option A and Option B.

There are <u>2 amounts of money</u> in each payment option: a high amount and a low amount.

In each decision you will be asked to choose whether you prefer to be paid according to option A or according to option B, by marking with an X the square corresponding to your preferred option.

For some decisions you may not care whether you receive Option A or Option B, in which case you should mark with an X the square corresponding to "Indifferent".

Notice that for those decisions you express indifference we will randomly determine whether you will be paid according to Option A or Option B. To do so we will extract a ball from the black bag with 100 balls, and if the extracted number is between 1 and 50, option A will be chosen, and is the extracted number is between 51 and 100, option B will be chosen. This means that each option has an equal chance of determining your earnings if you expressed indifference.

Lets look now at the first decision problem. Please look at the handout on your desk.

As you can see, there are ten decisions listed on the left side of the table, in the column marked Decision.

To see how your choices affect your earnings, let's look at decision 1 at the top of the table.

Decision 1 has two payment options: Option A and Option B. Option A pays a high amount of {AH-value} and a low amount of {AL-value}. Option B pays a high amount of {BH-value} and a low amount of {BL-value}.

We will randomly extract a single ball from the black bag that contains the 100 balls individually numbered from 1 to 100, so that any number between 1 and 100 is equally likely to be extracted.

If the extracted ball is numbered 10 or lower, Option A pays {AH-value} and Option B pays {BH-value}.

If the extracted ball is numbered 11 or higher, Option A pays {AL-value} and Option B pays {BL-value}.

This means that in decision 1, there is a 10-in-100 chance of getting the high amount in each payment option, and a 90-in-100 chance of getting the low amount in each option.

The choice you must make in decision 1 is whether you prefer to be paid according to Option A, or Option B, or whether you are indifferent between these two options.

Now, please look at decision 2. Notice that the amounts of money in Option A and in Option B of that decision are exactly the same as those in decision 1. The only difference of decision 2 compared to decision 1 is that the chances of getting the high amount of option A and option B are now higher than in decision 1 because now a ball numbered 20 or lower will yield the high amount in each payment option. This means that in decision 2, there is a 20-in-100 chance of getting the high amount in each payment option, and a 80-in-100 chance of getting the low amount in each option.

Again, the choice you must make in decision 2 is whether you prefer to be paid according to Option A, or Option B, or whether you are indifferent between these two options.

Actually, that is the choice you must make in each of the 10 decisions, noticing that the only difference between the decisions is that as you move down the table the chances of getting the higher amount in each payment option increases, so that in decision 10 at the bottom of the table you simply have to choose whether you prefer to be paid {AH-value} with certainty or {BH-value} with certainty, or whether you are indifferent between these two certain amounts.

We expect that you will be making one out of four kinds of decisions:

- You may prefer Option A for all decisions;
- You may be Indifferent between Option A and Option B for all decisions;
- You may prefer Option B for all decisions; or
- You may prefer Option A for some decisions, Option B for some decisions, and be Indifferent for other decisions.

Notice again that there are no wrong or right choices. Which kind of choice you make is entirely up to you.

Let me explain you now how we will proceed: Once you have completed all 10 decisions, you must raise your hand so that one monitor collects your decision sheet. At that moment, the monitor will verify if you selected Option B for all decisions, or if you switch from Option A to Option B at some point. If that happened we will give you a Level 2 task before determining your earnings. For example, suppose that someone in Level 1 has selected Option A for decisions 1-3 and Option B for decisions 4-10. This means that this person prefers Option A when the chances of earning the higher amount is 30-in-100 or less, but prefers Option B when the chances of earning the higher amount are 40-in-100 or more. Level 2 then asks this person to choose between Option A and Option B for chances between 30-in-100 and 40-in-100. Thus, the Level 2 task just provides more detail in the range of choices you indicated in Level 1. The table in Level 2 shows you 11 decisions arranged in the same way as the ten decisions in the Level 1 table, and you mark your preferred option using the same procedures as in Level 1.

Again, once you have completed all the decisions in Level 2, you must raise your hand so that one monitor collects your decision sheet.

Once everyone is done with the first decision problem, we will go to the second decision problem where the procedures will be exactly the same.

There is only one final detail we need to explain. Although you will complete four problems, we will not pay you for all four problems. At the end of the session, you will be asked to extract one ball from the black bag with 100 balls again to determine which of the problems we will use for your payment:

- If the ball is numbered 1 to 25, you will be paid for problem 1.
- If the ball is numbered 26 to 50, you will be paid for problem 2.
- If the ball is numbered 51 to 75, you will be paid for problem 3.
- If the ball is numbered 76 to 100, you will be paid for problem 4.

Once we have selected the problem, you will then be asked to extract one ball from the black bag with 10 balls to determine which of the 10 decisions in that problem we will use for your payment. If the number extracted corresponds to a decision that took you to Level 2, we will add ball number 11 to the back bag (since there are 11 decisions to be made in level 2), and ask you again to extract a ball. These are the procedures we will use to determine the binding decision for payment purposes. Once we know the binding decision, you will be asked to extract a ball from the black bag that contains the 100 balls, and the number in that ball determines whether we pay you the high or the low amount in the Option you choose in the binding decision.

Are there any questions?

{*Announce the end of the decision problem, and the beginning of the new one calling participants' attention to the different amounts of money involved in the new problem.*}

Instructions for Part III

We are now ready to go on to Part III of the experiment.

This part has 6 decision problems that are exactly the same except that the payment dates will differ.

There are <u>10 decisions</u> to be made in each decision problem.

There are <u>2 payment options</u> in each decision: Option A and Option B.

In each decision you will be asked to choose whether you prefer to be paid according to option A or according to option B, by marking with an X the square corresponding to your preferred option.

For some decisions you may not care whether you receive Option A or Option B, in which case you should mark with an X the square corresponding to "Indifferent".

Notice that for those decisions you express indifference we will randomly determine whether you will be paid according to Option A or Option B. To do so we will extract a ball from the black bag with 100 balls, and if the extracted number is between 1 and 50, option A will be chosen, and is the extracted number is between 51 and 100, option B will be chosen. This means that each option has an equal chance of determining your earnings if you expressed indifference.

Lets look now at the first decision problem. Please look at the handout on your desk.

As you can see, there are ten decisions listed on the left side of the table, in the column marked Decision.

To see how your choices affect your earnings, let's look at decision 1 at the top of the table.

Decision 1 has two payment options: Option A and Option B. Option A pays {A-value} one month from today and Option B pays { A-value+X=B-value} two months from today.

In the table there is also one column labelled "Annual Interest Rate". In decision 1, Option B pays an annual interest rate of 5% on the {A-value} to be received two months from today. The interests are compounded quarterly so the corresponding quarterly rate is (5%/4)=1.25%. Since the horizon is one month, it corresponds to 1/3 of a quarter. Thus the value to be received two months from now is equal to {A-value}*$(1+1.25\%)^{1/3}$={B-value}.
Now, please look at decision 2. Notice that the amount of money in Option A is the same as that in decision 1. The only difference of decision 2 compared to decision 1 is the amount of money yield by Option B, given that the annual interest rate is now higher than in decision 1.

Actually, notice that the annual interest rate increases by 5 percentage points as you move from decision 1 to decision 2. The same increase in annual interest rates applies to each row, until decision 10 in the last row has an annual interest rate of 50%.

As in Part II of the experiment, you should mark with an X the square corresponding to your preferred option.

Notice again that there are no wrong or right choices. Which kind of choice you make is entirely up to you.

Let me explain you now how we will proceed: Once you have completed all 10 decisions, you must raise your hand so that one monitor collects your decision sheet. At that moment, the monitor will verify if you selected Option B for all decisions, or if you switch from Option A to Option B at some point. If that happened we will give you a Level 2 task before determining your earnings. For example, suppose that someone in Level 1 has selected Option A for decisions 1-5 and Option B for decisions 6-10. This means that this person prefers Option A when the annual interest rate is 25% or less, but prefers Option B when the annual interest rate is 30% or more. Level 2 then asks this person to choose between Option A and Option B for annual interest rates between 25% and 30%. Thus, decision 1 in Level 2 corresponds to an annual interest rate of 25%, and decision 2 to an annual interest rate of 25.5%, and so on until the last decision shows an annual interest rate of 30%. Thus Level 2 just provides more detail in the range of choices you indicated in Level 1.

Again, once you have completed all the decisions in Level 2, you must raise your hand so that one monitor collects your decision sheet.

Once everyone is done with the first decision problem, we will go to the second decision problem where the procedures will be exactly the same.

As in Part II of the experiment, although you will complete six problems, we will not pay you for all problems. At the end of the session, you will be asked to extract one ball from a black bag with 6 balls numbered from 1 to 6 to determine which of the problems we will use for your payment.

Once we have selected the problem, you will then be asked to extract one ball from the black bag with 10 balls to determine which of the 10 decisions in that problem we will use for your payment. If the number extracted corresponds to a decision that took you to Level 2, we will add ball number 11 to the back bag (since there are 11 decisions to be made in level 2), and ask you again to extract a ball. These are the procedures we will use to determine the binding decision for payment purposes.

HOW EXACTLY WILL YOU BE PAID?

At the end of today's session, you will receive a certificate which is redeemable for cash under the conditions dictated by your chosen payment option under the selected payoff alternative. This certificate is signed by the three Professors responsible for this experiment (myself, Professor Lígia Pinto, and Professor Paula Veiga) and holds each one of us legally fully responsible for the payment under the specified conditions. In addition to the exact money amount you are to receive and the date you are to receive it, the certificate also informs you the exact location you are expected to go to receive it, which is the Office of the Responsible for the FUP program in Timor-Leste. The certificate also details the conditions under which the payment will be processed in case you cannot present yourself in person with the certificate at this location on or after the specified date. Basically, the payment will be processed as long as the original of the certificate you will get at the end of today's session is received in this Office on or after the specified date along with a letter signed by you stating to whom, where and how you want us to process the payment. Thus, one important thing we expect you to do in your best interest is to really make sure you hold on to the certificate we hand you at the end of the session.

Are there any questions?

{*Announce the end of the decision problem, and the beginning of the new one calling participants' attention to the different Option B payment dates*}

Part IV

We are now ready to go on to Part IV of the experiment which just consists of some additional questions about yourself.

{*Once everyone is done with Part IV, determine who are the participants selected to receive the money. Ask those who are not selected to orderly leave the room, and have Lígia thank them for participating and pay them the participation fee as they leave the room. After that is done, have Paula take those selected to the end of the room and Lígia pay their participation fee, and then bring one by one to my desk at the front of the room to carry on the procedures to determine the amount of money each is to receive so that only the three of us learn the choices made by each of the selected participants. If a participant is to receive according to Part III of the experiment, fill in the original certificate with the participant's full identification, and have a copy taken for our own records*}.

Table A1: Original Table Used in Timor-Leste in the First Risk Task

Decisão	**Forma A**	**Forma B**	**Tanto Faz**
1	Se bola 1 a 10 recebe $64 Se bola 11 a 100 recebe $51	Se bola 1 a 10 recebe $124 Se bola 11 a 100 recebe $3	
2	Se bola 1 a 20 recebe $64 Se bola 21 a 100 recebe $51	Se bola 1 a 20 recebe $124 Se bola 21 a 100 recebe $3	
3	Se bola 1 a 30 recebe $64 Se bola 31a 100 recebe $51	Se bola 1 a 30 recebe $124 Se bola 31 a 100 recebe $3	
4	Se bola 1 a 40 recebe $64 Se bola 41 a 100 recebe $51	Se bola 1 a 40 recebe $124 Se bola 41 a 100 recebe $3	
5	Se bola 1 a 50 recebe $64 Se bola 51 a 100 recebe $51	Se bola 1 a 50 recebe $124 Se bola 51 a 100 recebe $3	
6	Se bola 1 a 60 recebe $64 Se bola 61 a 100 recebe $51	Se bola 1 a 60 recebe $124 Se bola 61 a 100 recebe $3	
7	Se bola 1 a 70 recebe $64 Se bola 71 a 100 recebe $51	Se bola 1 a 70 recebe $124 Se bola 71 a 100 recebe $3	
8	Se bola 1 a 80 recebe $64 Se bola 81 a 100 recebe $51	Se bola 1 a 80 recebe $124 Se bola 81 a 100 recebe $3	
9	Se bola 1 a 90 recebe $64 Se bola 91 a 100 recebe $51	Se bola 1 a 90 recebe $124 Se bola 91 a 100 recebe $3	
10	Se bola 1 a 100 recebe $64	Se bola 1 a 100 recebe $124	

Table A2: Original Table Used in Timor-Leste in the First Discount Rate Task

Decisão	**Forma A** **1 MÊS**	**Forma B** **2 MESES**	**Tanto Faz**	Taxa de Juro
1	$100	$100,41		*5%*
2	$100	$100,83		*10%*
3	$100	$101,23		*15%*
4	$100	$101,64		*20%*
5	$100	$102,04		*25%*
6	$100	$102,44		*30%*
7	$100	$102,84		*35%*
8	$100	$103,23		*40%*
9	$100	$103,62		*45%*
10	$100	$104,00		*50%*

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