Discounting Behavior and the Magnitude Effect

by

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ABSTRACT.

We evaluate the claim that individuals exhibit a magnitude effect in their discounting behavior, which is said to occur when higher discount rates are inferred from choices made with lower principals, all else being equal. If the effect is robust, as claimed, we should be able to see it using procedures that are more familiar to economists. Using data collected from a representative sample of adult Danes, we find statistically significant evidence of a small magnitude effect, at levels that are *much* smaller than is typically claimed. This evidence only surfaces if one carefully controls for unobserved individual heterogeneity in the population. And it disappears completely if we include discounting choices in which both options have some time delay.

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1. Introduction

One anomaly about discounting behavior is known as the "magnitude effect": the finding that smaller amounts are discounted more than larger amounts. The magnitude effect has been presented as an explanation to declining discount rates with horizon, and would suggest that these differences should disappear if principals were larger. If magnitude effects are sufficiently large, as the literature is suggesting, they may account for behavior otherwise attributed to quasi-hyperbolic preferences. Quite apart from the issue of non-constancy of discount rates, if the magnitude effect is significant, cost-benefit analysis cannot use one discount rate independent of the scale of the project. We present new evidence on the magnitude effect from a methodology that is based on real rewards, transparent elicitation procedures that are incentive compatible, joint estimation of risk and time preferences, and allowing for observable and unobservable subject heterogeneity when inferring discount rates.

One theory that predicts magnitude effects hypothesizes that individuals might be using a fixed monetary premium to decide whether to choose delayed payments rather than earlier payments, as well as some premium for delay that varies with horizon (Benhabib, Bisin and Schotter [2010]). In this case a subject might want to receive a minimum of \$10 before delaying receipt: as the principal increases from \$100 to \$1000, say, the discount rate at which the subject switches from the sooner to the later payment decreases, since \$10 is proportionately less as the principal increases. This theory is important because it suggests that evidence for sharply declining discount rates with horizon would disappear as the principal gets larger and larger. A direct test of the magnitude effect presents itself in this theory formulation: present subjects with two different principals and see if there is a difference in behavior.

We conduct experiments with a representative sample of adult Danes to see if there is anything here that should be of concern to economists. We do in fact find statistically significant evidence for the magnitude effect, after one carefully controls for unobserved individual heterogeneity, but at *levels that are dramatically lower than previously reported*. It disappears completely when we include choices in which both options involve a time delay.

Frederick, Loewenstein and O'Donoghue [2002; p. 363] cite the following studies supporting the magnitude effect, in historical order: Thaler [1981], Ainslie and Haendel [1983], Loewenstein [1987], Benzion, Rapoport and Yagil [1989], Holcomb and Nelson [1992], Raineri and Rachlin [1993], Shelley [1993], Green, Fristoe and Myerson [1994], Green, Fry and Myerson [1994], Kirby and Maraković [1995], Kirby [1997] and Kirby, Petry and Bickel [1999]. To this list we would add Chapman and Elstein [1995], Chapman [1996], Kirby and Maraković [1996], Green, Myerson and McFadden [1997], Du, Green and Myerson [2002], Estle, Green, Myerson and Holt [2007], Benhabib, Bisin and Schotter [2010] and Scholten and Read [2010]. We carefully review the most cited studies in Appendix A, and every other study in Appendix C (available on request). Table 1 contains a summary tabulation of procedures and findings.

In section 2 we present our experimental design, in section 3 we specify a structural econometric model to infer discount rates, and in section 4 we report our findings. Section 5 concludes.

2. Experiments

To set the stage minimally for the discussion about experimental design, we define the discount factor for a given horizon τ to be the scalar D that equates the utility of the income received at time t with the income received at time $t+\tau$:

$$U(y_t) = D\ U(y_{t+\tau}) \tag{0}$$

for some utility function U(.). This general definition permits the special case, much studied in the experimental literature, in which U(.) is linear. There is also nothing in (0) that restricts us to

Expected Utility Theory (EUT).

The discount factor for the Exponential specification is defined as

$$D_t = 1/(1+\delta)^t \tag{1}$$

for t≥0, and where the discount rate d is simply

$$d_t = \delta \tag{2}$$

Although these characterizations are abstract, we view the discount rate on an annualized basis throughout. The key feature of this model, of course, is that the discount rate is a constant over time. The percentage rate at which utility today and utility tomorrow is discounted is exactly the same as the rate at which utility in 7 days and utility in 8 days is discounted. For our immediate purposes the exact form of the discounting function is of no consequence: one can view the Exponential specification descriptively as simply a convenient summary statistic for the effect of magnitude on elicited discount rates.

A. Design

An important aspect of our methodological contribution to the literature on magnitude effects is the provision of sizable monetary incentives that are paid out, rather than stated as hypothetically paid. This sharpens the incentives for respondents to truthfully report their preferences. Subjects are presented with two tasks.[1] The first task identifies individual discount rates, and the second task identifies a-temporal risk attitudes and hence the concavity of the utility function. Observed choices from both tasks are then used to jointly estimate structural models of the discounting function defined over utility of income.

Using real monetary incentives comes with budgetary consequences for the researcher. Two

[1] A complete list of parameter values for all choices is presented in Appendix B (available on request).

different procedures could be used to keep such budgets reasonable: one could either use relatively small money amounts for both the smaller and the larger principal and pay every subject, or one could use larger amounts but only pay for the task with some probability. There are drawbacks with both of these approaches. Using small amounts can lead to confounding behavioral effects if subjects are rounding off the money amounts. Using stochastic payments require sophisticated portfolio analysis of the data or a maintained hypothesis that the independence axiom of Expected Utility Theory holds. We have opted for avoiding the former approach due to the apparent strong presence of rounding off that is prevalent in the Benhabib, Bisin and Schotter [2010] data, as we will show below.[2] We use a 10% chance for a subject to be paid, allowing us to use principals with relatively high numerical values.

We illustrate the potential seriousness of rounding off behavior in Table 2. The first four columns show the behavior that would be expected of a subject in a fill-in-the-blank elicitation task if that subject had a 10% annualized discount rate. In this task the amount of money today in column (1) is given to the subject, and the future amount is the response that would be expected of someone that had a 10% discount rate, assuming no rounding off occurs. The horizon is shown in days, and then in column (4) we see the implied annualized effective discount rate (AER), which by construction here had better be 10%. Each block of rows in Table 2 shows a series of binary choices with the same horizons, with the principal roughly doubling from block to block. Clearly the monetary premia in column (5) are all *very* small.

The last four columns of Table 2 show what happens if the subject rounds up the filled-in response to the next whole dollar. Thus column (6) is the same as column (1), and column (7) is column (2) rounded up to the nearest dollar. The reason for rounding *up*, if one is going to round at

[2] We also have no reason to suspect that violations of the independence axiom would lead to qualitatively different conclusions regarding the role of magnitude effects.

all, is apparent from column (5): rounding to the *nearest* dollar would mean in most cases that the future amount was literally the same as the principal, and that is intuitively false to even the most cognitively challenged subject. But the impacts of this modest amount of rounding on the implied discount rates in column (9) are staggering. Two frequently reported anomalies immediately emerge: the appearance of declining discount rates with horizon, and declining discount rates with the magnitude of the principal.

Some evidence of rounding off behavior is found in Benhabib, Bisin and Schotter [2010]. In their experiment 27 subjects were given the task to fill-in-the-blank for future payments, based on exactly the horizons and principals shown in Table 2. They also did a comparable fill-in-the-blank task with the same 27 subjects in which a future amount was given and the subject had to choose the current amount to match. Out of 1,620 observed responses, a staggering 95.6% reported amounts only in whole dollars.[3]

This chilling arithmetic follows from the very argument that motivates the theory exposition in Benhabib, Bisin and Schotter [2010]. What if subjects discount the future with some fixed monetary premium, irrespective of horizon, rather than some premium that is a proportion of the principal, as assumed in quasi-hyperbolic specifications? The upshot is implied by Table 2, but can be seen immediately by assuming a principal of $10 and the horizons of 3, 7, 14, 30, 91 and 181 days, and a fixed monetary premium of $1. The implied discount rates are 1178%, 500%, 249%, 116%, 38% and 19%, respectively, showing striking evidence of "hyperbolicky" discounting. Then change the principal to $100, and the same monetary premium only implies discount rates of 121%, 52%, 26%, 12%, 4% and 2%, respectively, and a "magnitude effect."

By using relatively large principals we avoid the confounding effects from rounding off

[3] Morever 3.2% were rounded to the nearest $0.50, and 0.6% to the nearest $0.25, leaving only 11 responses, or 0.7% , to be reported with more precision.

behavior, and can more clearly test the presence of magnitude effects.

Individual Discount Rates

Individual discount rates are examined by asking subjects to make a series of choices over two certain outcomes that differ in terms of when they will be received. For example, one option can be 3000 kroner today, and another option can be 3300 kroner in 60 days.[4] If the subject picks the earlier option we can infer that their discount rate is below 10% for 60 days, and if the subject picks the later option we can infer that their discount rate is above 10% for that horizon. By varying the amount of the later option we can identify the discount rate of the individual, conditional on knowing the utility of those amounts to this individual. One can also vary the time horizon to identify the discount rate function. This method has been widely employed in the United States (e.g., Coller and Williams [1999]), Denmark (e.g., Harrison, Lau and Williams [2002]), and Canada (e.g., Eckel, Johnson and Montmarquette [2005]).

We ask subjects to evaluate choices over several time horizons. We consider time horizons between 2 weeks and 1 year. Each subject is presented with choices over four time horizons, and those horizons are drawn at random, without replacement, from a set of thirteen possible horizons (2 weeks, and 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11 and 12 months). This design will allow us to obtain a smooth characterization of the discount rate function across the sample for horizons up to one year.

Of course, the key treatment here is to vary the principal. We employ two levels of the principal, 1500 kroner and 3000 kroner, on a between-subjects basis. We also vary the order in which the time horizon was presented to the subject: either in ascending order or descending order.

[4] Much of the literature in economics employs a front end delay such that the earlier option is delayed by some time period, such as one week or one month (e.g., Coller and Williams [1999], Harrison, Lau and Williams [2002]). Virtually none of the studies documenting a magnitude effect employ a front end delay, so we also avoid it here in our baseline design.

Similarly, we vary the provision of implied interest rates for each choice on a between-subjects basis, and independently of the two other treatments.

These three treatments, the level of the principal, the order of presentation of the horizon, and information on implied interest rates result in a 2×2×2 design. Roughly ⅛ of the sample was assigned at random to any one particular combination.

Risk Attitudes

Risk attitudes were evaluated by asking subjects to make a series of choices over outcomes that involve some uncertainty. To be clear, risk attitudes are elicited here simply as a convenient vehicle to estimate the non-linear utility function of the individual. The theoretical requirement, from the definition of a discount factor in (0), is for us to know the utility function over income if we are to correctly infer the discount rate the individual used. The discount rate choices described above are not defined over lotteries.

Our design poses a series of binary lottery choices. For example, lottery A might give the individual a 50-50 chance of receiving 1600 kroner or 2000 kroner to be paid today, and lottery B might have a 50-50 chance of receiving 3850 kroner or 100 kroner today. The subject picks A or B. One series of 10 choices would offer these prize sets with probabilities on the high prize in each lottery starting at 0.1, then increasing by 0.1 until the last choice is between two certain amounts of money. We present these pairwise choices, one pair at a time to the subject as a "pie chart" showing prizes and probabilities. We gave subjects 40 choices, in four sets of 10 where each set had the same prizes. The prize sets employed are as follows: [A1: 2000 and 1600; B1: 3850 and 100], [A2: 1125 and 750; B2: 2000 and 250], [A3: 1000 and 875; B3: 2000 and 75] and [A4: 2500 and 1000; B4: 4500 and 50]. The order of these four sets was random for each subject, but within each set the choices were presented in an ordered manner, with increments of the high prize probability of 0.1.

The typical findings from lottery choice experiments of this kind are that individuals are generally averse to risk, and that there is considerable heterogeneity in risk attitudes across subjects: see Harrison and Rutström [2008] for an extensive review. Much of that heterogeneity is correlated with observable characteristics, such as age and education level (Harrison, Lau and Rutström [2007]), although here we represent it with a random distribution that describes the full extent of preference heterogeneity.

This design does not assume that behavior is better characterized by EUT or some other model.

B. Sample and Procedures

Between September 28 and October 22, 2009, we conducted experiments with 198 Danes, of which 89 received the lower principal of 1500 kroner and 109 received the higher principal of 3000 kroner. The sample was drawn to be representative of the adult population as of January 1, 2009, using sampling procedures that are virtually identical to those documented at length in Harrison, Lau, Rutström and Sullivan [2005]. We received a random sample of the population aged between 18 and 75, inclusive, from the Danish Civil Registration Office, stratified that by geographic area, and sent out 1969 invitations.[5] The experiments reported here are a sub-sample of the complete sample (which was 413 subjects), only including those who did not have a front end delay to the sooner option.

Our experiments were all conducted in hotel meeting rooms around Denmark, so that travel

[5] That recruiting sample was drawn by us from a random sample which includes information on sex, age, residential location, marital status, and whether the individual is an immigrant. At a very broad level our sample was representative on average: the sample of 50,000 had an average age of 49.8, 50.1% of them were married, and 50.7% were female; our final sample of 413 subjects had an average age of 48.7, 56.5% of them were married, and 48.2% were female.

logistics for the sample would be minimized. Various times of day were also offered to subjects, to facilitate a broad mix of attendance. The largest session had 15 subjects, but most had fewer. The procedures were standard: Appendix B (available on request) documents an English translation of the instructions, and shows typical screen displays. Subjects were given written instructions, which were also read out, and then made choices in a trainer task, which was "played out" so that the full set of consequences of each choice were clear. In fact, subjects were paid *Big Ben* caramels instead of money for all trainers, and the payments were happily consumed when delivered. All interactions were by computer. The order of the block of discount rate tasks and the block of risk attitudes tasks was randomized for each session. After all choices had been made the subject was asked a series of standard socio-demographic questions.

There were 40 discounting choices and 40 risk attitude choices, and each subject had a 10% chance of being paid for one of each set. Average payments on the first block were 228 kroner (although some were for deferred receipt) and on the second block the average was 218 kroner, for a combined average of 446 kroner. The exchange rate at the time was close to 5 kroner per U.S. dollar, so earnings averaged $89 per two-hour session for these tasks. Subjects were also paid 300 kroner or 500 kroner fixed show-up fee, and earnings from additional tasks completed after the tasks of interest here were completed.[6]

For payments to be made in the future, the following language explained the procedures:

> You will receive the money on the date stated in your preferred option. If you receive some money today, then it is paid out at the end of the experiment. If you receive some money to be paid in the future, then it is transferred to your personal bank account on the specified date. In that case you will receive a written confirmation from Copenhagen Business School which guarantees that the money is

[6] An extra show-up fee of 200 kroner was paid to 35 subjects who had received invitations stating 300 kroner, but then received a final reminder that accidentally stated 500 kroner. In general, the additional tasks earned subjects an average of at least 370 kroner (the exact amount depended on later decisions by other subjects), so total earnings from choices made in the session averaged 722.9 kroner, or roughly $145.

reserved on an account at Danske Bank. You can send this document to Danske Bank in a prepaid envelope, and the bank will transfer the money to your account on the specified date.

Payments by way of bank transfer are common in Denmark, Copenhagen Business School is well-known in Denmark, and Danske Bank is the largest financial enterprise in Denmark as measured by total assets.

3. Econometrics

Although the core treatment is a simple one, varying the principal, inferences about implied discount rates requires attention to the definition of a discount rate in terms of utility streams, as shown earlier in (0). The approach we adopt is direct estimation by maximum simulated likelihood of some structural model of a latent choice process in which the core parameters defining risk attitudes and discounting behavior can be estimated. We review the basic inferential logic for estimating risk attitudes, and discuss the extension to discounting behavior. We employ maximum simulated likelihood because we estimate the main structural parameters as random coefficients, to reflect unobserved individual heterogeneity.

A. Estimating the Utility Function

Assume that utility of income is defined by

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

where M is the lottery prize and $r \neq 1$ is a parameter to be estimated. For r=1 assume U(M)=ln(M) if needed. Thus r is the coefficient of CRRA: r=0 corresponds to risk neutrality, r<0 to risk loving, and r>0 to risk aversion. Let there be two possible outcomes in a lottery. Under EUT the probabilities for each outcome M_j, $p(M_j)$, are those that are induced by the experimenter, so expected utility is simply the probability weighted utility of each outcome in each lottery i plus some level of background consumption ω:

$$EU_i = [\ p(M_1) \times U(\omega+M_1)\ ] + [\ p(M_2) \times U(\omega+M_2)\ ] \tag{4}$$

The EU for each lottery pair is calculated for a candidate estimate of r, and the index

$$\nabla EU = EU_A - EU_B \tag{5}$$

calculated, where EU_A is the lottery in Option A and EU_B is the lottery in Option B as presented to subjects. This latent index, based on latent preferences, is then linked to observed choices using the cumulative logistic distribution function $\Lambda(\nabla EU)$. This "logit" function takes any argument between $\pm\infty$ and transforms it into a number between 0 and 1. Thus we have the logit link function,

$$\text{prob(choose Option A)} = \Lambda(\nabla EU) \tag{6}$$

The index defined by (5) is linked to the observed choices by specifying that the lottery in Option A is chosen when $\Lambda(\nabla EU)>\frac{1}{2}$, which is implied by (6).

Thus the likelihood of the observed responses, conditional on the EUT and CRRA specifications being true, depends on the estimates of r given the above statistical specification and the observed choices. Assuming non-random coefficients for the moment, the conditional log-likelihood is then

$$\ln L^{RA} = \ln L(r; y, \omega) = \textstyle\sum_i [\ (\ln \Lambda(\nabla EU)\times \mathbf{I}(y_i = 1)) + (\ln (1-\Lambda(\nabla EU))\times \mathbf{I}(y_i = -1))\ ] \tag{7}$$

where $\mathbf{I}(\cdot)$ is the indicator function, and $y_i = 1(-1)$ denotes the choice of the Option A (B) lottery in risk aversion task i. Harrison and Rutström [2008; Appendix F] review procedures and syntax from the popular statistical package *Stata* that can be used to estimate structural models of this kind, as well as more complex non-EUT models.

We employ a *behavioral* error specification originally due to Fechner and popularized by Hey and Orme [1994]. This error specification posits the latent index

$$\nabla EU = (EU_A - EU_B)/\mu \tag{5$'$}$$

instead of (5), where μ is a structural "noise parameter" used to allow some errors from the perspective of the deterministic EUT model. This is just one of several different types of error story that could be used, and Wilcox [2008] provides a masterful review of the implications of the alternatives.[7] As $\mu \to 0$ 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. When $\mu=1$ this specification collapses to (5), where the probability of picking one lottery is given by the ratio of the EU of one lottery to the sum of the EU of both lotteries. Thus μ can be viewed as a parameter that flattens out the link functions as it gets larger.

An important contribution to the characterization of behavioral errors is the "contextual error" specification proposed by Wilcox [2010]. It is designed to allow robust inferences about the primitive "more stochastically risk averse than." It posits the latent index

$$\nabla EU = [(EU_A - EU_B)\nu]/\mu \tag{5''}$$

instead of (5′), where ν is a new, normalizing term for each lottery pair. The normalizing term ν is defined as the maximum utility over all prizes in this lottery pair minus the minimum utility over all prizes in this lottery pair. The value of ν varies, in principle, from lottery choice to lottery choice: hence it is said to be "contextual." For the Fechner specification, dividing by ν ensures that the *normalized* EU difference $[(EU_A - EU_B)/\nu]$ remains in the unit interval.

B. Estimating the Discounting Function

Assume EUT holds for choices over risky alternatives and that discounting is exponential. A subject is indifferent between two income options M_t and $M_{t+\tau}$ if and only if

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

[7] Some specifications place the error at the final choice between one lottery or after the subject has decided which one has the higher expected utility; some place the error earlier, on the comparison of preferences leading to the choice; and some place the error even earlier, on the determination of the expected utility of each lottery.

where $U(\omega+M_t)$ is the utility of monetary outcome M_t for delivery at time t plus some measure of background consumption ω, δ is the discount rate, τ is the horizon for delivery of the later monetary outcome at time $t+\tau$, and the utility function U is separable and stationary over time. The left hand side of equation (8) is the sum of the discounted utilities of receiving the monetary outcome M_t at time t (in addition to background consumption) and receiving nothing extra at time $t+\tau$, and the right hand side is the sum of the discounted utilities of receiving nothing over background consumption at time t and the outcome $M_{t+\tau}$ (plus background consumption) at time $t+\tau$. Thus (8) is an indifference condition and δ is the discount rate that equalizes the present value of the *utility* of the two monetary outcomes M_t and $M_{t+\tau}$, after integration with an appropriate level of background consumption ω.[8]

We can write out the likelihood function for the choices that our subjects made and jointly estimate the risk parameter r in equation (3) and the discount rate parameter δ in (8). We use the same stochastic error specification as in (5′).[9] Instead of (5′) we have

$$\nabla PV = (PV_A - PV_B)/\mu, \tag{9}$$

where the discounted utility of Option A is given by

$$PV_A = (\omega+M_A)^{(1-r)}/(1-r) + (1/(1+\delta)^{\tau})\ \omega^{(1-r)}/(1-r) \tag{10}$$

and the discounted utility of Option B is

$$PV_B = \omega^{(1-r)}/(1-r) + (1/(1+\delta)^{\tau})\ (\omega+M_B)^{(1-r)}/(1-r), \tag{11}$$

and M_A and M_B are the monetary amounts in the choice tasks presented to subjects. We assume here

[8] Andersen, Harrison, Lau and Rutström [2008b] show that the addition of background consumption is a sufficient condition to avoid negative discount rates when the intertemporal utility function is additively separable.

[9] We do not need to apply the contextual utility correction ν for these choices since they are over deterministic monetary amounts.

that the utility function is stable over time and is perceived *ex ante* to be stable over time.[10]

Thus the likelihood of the discount rate responses, conditional on the EUT, CRRA and exponential discounting specifications being true, depends on the estimates of r, δ and μ, given the assumed value of ω and the observed choices. The conditional log-likelihood is

$$\ln L^{DR} = \ln L\,(r, \delta, \mu; y, \omega) = \textstyle\sum_i \left[(\ln \Lambda(\nabla PV)\times \mathbf{I}(y_i=1)) + (\ln (1-\Lambda(\nabla PV))\times \mathbf{I}(y_i=-1)) \right] \tag{12}$$

where $y_i = 1(-1)$ again denotes the choice of Option A (B) in discount rate task i.

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

$$\ln L\,(r, \delta, \mu; y, \omega) = \ln L^{RA} + \ln L^{DR} \tag{13}$$

where L^{RA} is defined by (7) and L^{DR} is defined by (12). This expression can then be maximized using standard numerical methods. The parameter ω is set exogenously: using data from the household expenditure survey at Statistics Denmark, Andersen, Harrison, Lau and Rutström [2008a; p.600, Appendix D] calculate per capita consumption of private nondurable goods on an average daily basis as being equal to 118 kroner in 2003.[11] We adjust that amount for inflation to the time of our experiments, and assume ω = 130 kroner.

Nothing in this inferential procedure relies on the use of EUT, or the CRRA functional form. Nor does anything rely on the use of the Exponential discounting function. In fact, following the literature on the magnitude effect, we can employ the Exponential discounting function here *descriptively*, since all we are interested in here is whether the discount rate differs as we vary the principal. Our methods generalize immediately to alternative models of decision making under risk,

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[10] Direct evidence for the former proposition is provided by Andersen, Harrison, Lau and Rutström [2008b], who examine the temporal stability of risk attitudes in the Danish population. The second proposition is a more delicate matter: even if utility functions are stable over time, they may not be subjectively perceived to be, and that is what matters for us to assume that the same r that appears in (3) appears in (10) and (11). When there is no front end delay, as here, this assumption is immediate for (10), but not otherwise.

[11] Andersen, Harrison, Lau and Rutström [2008a; p.602] show that estimates are robust to variations of ω between 50 and 200 kroner.

and especially to alternative discounting functions, as demonstrated by Andersen, Harrison, Lau and Rutström [2011].

C. Random Coefficients

We account for unobserved individual heterogeneity through the possibility that the coefficients r and δ are *random coefficients* following some parametric distribution. In other words, one can allow the coefficients r and δ to be distributed in a random manner across the population: each subject behaves as if they have a specific r and δ, but there is variation across subjects and that variation is assumed to be characterized by some parametric distribution.

For example, if δ is assumed to vary according to a Normal distribution, then one would estimate two "hyper-parameters" to characterize that distribution: a population mean of δ and a population standard *deviation* of δ.[12] Each of these hyper-parameters would have a point estimate and a standard *error*, where the latter derives from familiar sampling variability. As the sample size increases, and assuming consistent estimators, the *sampling error* on the population mean and the population standard deviation would converge to 0, but there is no presumption that the point estimate for the *population standard deviation* converge to 0, since it is a characteristic of the population and not sample variability.

We use non-linear "mixed logit" methods developed by Andersen, Harrison, Hole, Lau and Rutström [2010] to estimate such specifications. In fact, we also allow the distribution for r and δ to be a Logit-Normal distribution, which is a logistic transform of a normally distributed variable. Due originally to Johnson [1949], and familiar in bio-statistics, this transformation allows the resulting distribution to closely approximate a flexible Beta distribution: it allows skewness and bimodality.

[12] In fact we allow for a non-zero correlation between these two random coefficients, so their covariance is a third hyper-parameter to be estimated.

The domain is restricted to the unit interval, but it is a simple matter to expand that to any finite interval.

4. Results

A direct test of the magnitude effects is offered by varying the principal offered to respondents. The analysis of the data is therefore a simple matter: did the higher principal generate discount rates that were lower than the discount rates generated by the lower principal? We elicited discount rates over a horizon between 2 weeks and one year. Although this analysis could be performed on quasi-hyperbolic or hyperbolic discounting functions, the exponential discounting model offers a parsimonious one-parameter alternative, and in line with our previous findings in Andersen, Harrison, Lau and Rutström [2011]. Hence we consider a single discount rate across time horizons when comparing the effect of the principal.

Figure 1 illustrates the random coefficient estimates for the pooled data spanning our two treatments, to help better understand these kind of estimates.[13] The parameters of the underlying Normal distribution are shown, and the logistic transform Λ then applied to them. For the risk aversion parameter r we estimate a mean for the Normal distribution of −0.34 and a standard deviation of 0.93: these are *not* the estimates for the parameter r itself, but the parameters defining the argument of the logistic transform, so we end up with the population distribution Λ(N(−0.34, 0.93)) shown in Figure 1. The population distribution is generally risk averse, with a mean of 0.57 and a standard deviation of 0.19. The population distribution for the discount rate is sharply, positively skewed.[14] The median discount rate in the population is 0.055, the average discount rate is

[13] The covariance between the two random coefficients is so close to 0 that we set it to 0 in this pooled estimation to improve numerical stability.

[14] There is one technical issue of importance here, however. As flexible as the Logit-Normal is, it only allows bimodality at the *end-points* of the finite interval allowed. In this case we constrained the domain to

0.101, and the standard deviation of 0.11. Of course, since this is a skewed distribution, one should not infer statistical insignificance from the standard deviation equaling or exceeding the mean. For the same reason, the appropriate measure of central tendency of the population distribution is the median rather than the mean.

For the high principal the covariance between the two random coefficients is −0.35, with a 95% confidence interval between −0.43 and −0.27; so we reject the hypothesis that the two coefficients are independent. This covariance implies a correlation of −0.22, which of course is consistent with the application of Jensen's Inequality to (0), which shows that a *more* concave utility function must *decrease* inferred discount rates for given choices between the two monetary options. We obtain virtually identical estimates of the correlation for the low principal sample.

Figure 2 then displays the estimated population distribution for the discount rate according to the level of the principal. We observe a statistically significant difference, and in the direction of the magnitude effect, although the quantitative magnitude is much smaller than conventionally reported. The median discount rate for the low principal is 6.6%, and the average is 15.9%; the median discount rate for the high principal is 4.3%, and the average is 9.9%. These distributions are significantly different from each other, consistent with the visual impression from Figure 2.

The magnitude effect remains present, but even smaller in relative terms, if we assume linear utility functions. Figure 3 shows the population distributions obtained with this (incorrect) assumption. With the low principal the median discount rate is 0.127, the mean discount rate is 0.281, and the standard deviation is 0.317; with the high principal the median discount rate is 0.123, the mean discount rate is 0.244, and the standard deviation is 0.272. Although there remain

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be between 0 and 0.6, and hence the mode close to 0 *might* be an artefacts of that assumption. Although we know a priori that $\delta \geq 0$, we do not know the upper bound. One can loop through alternative parametric assumptions of the upper bound and evaluate the maximum likelihood at each: these are known as profile likelihoods. In our case the qualitative results are invariant to assuming upper bounds lower than 0.6. A better solution, and common in the statistical literature to allow "internal modes," is to allow mixtures.

significant differences in the two distributions, these are very small differences in discount rates.

Finally, we have actually thrown away roughly one-half of the data we collected by focusing solely on tasks in which the sooner option was to be delivered now. The missing half involved choices in which the sooner option was to be delivered in 30 days from the date of choice, so that both options had some delay. This "front end delay" is intended to reduce the differences in subjective transactions costs of realizing either option. Collecting money at the end of the session is simply more credible than relying on someone to send a bank deposit in one month. The vast bulk of the literature on the magnitude effect assumes no front end delay, hence our main analyses focused on that case. Figure 4 shows the effect of adding in the choices with a front end delay. The magnitude effect disappears. The median discount rates are 13% and 13%, and the mean discount rates are 5.4% and 6.4%, for the low and high principals, respectively. So there is effectively no difference by treatment, and the difference in means is in the *opposite* direction of the conventional magnitude effect.

Choices with a front end delay are appropriate characterizations of many financial decisions, but not all economic decisions. We view each as appropriate in different settings.

5. Conclusions

We use real incentives with large monetary rewards to avoid possible rounding effects. With some exceptions, noted in our literature review, all evidence of the magnitude effect that meets certain minimal standards of salience and design occurs in samples of college-age students. We do observe a statistically significant magnitude effect in the discounting behavior of adult Danes making choices of deferred monetary payments, but it is not very large: the size of the effect depends on whether one looks at median estimates or mean estimates. The preferred median estimates, correcting for unobserved individual heterogeneity and non-linear utility functions, differ only by 2.3

percentage points, from 6.6% to 4.3%, and the mean estimates differ by 6.0 percentage points, from 15.9% to 9.9%. Given the sharp skewness of the population distribution, there is an argument for focusing on the median, but we report each measure of central tendency.

Table 1: Review of Experimental Literature on the Magnitude Effect

Study	Sample (Size)	Elicitation Method	Horizon(s)	Principal Amounts	Discount Rates[a]	Hypothetical or Real?
Thaler [1981]	Students (N ≈ 60)	Open-ended Fill-in-the-blank	3, 6, 12, 36, 60, 120 months	\$15, \$75, \$250, \$1200, \$3000	(120%, 139%), 98%, (34%, 69%, 120%), 69%, 29% for 12 months	Hypothetical
Ainslie & Haendel [1983]	Patients (N=18, 66 choices)	Choice	3 days	\$2 up to \$10	Not reported by magnitude	Real
Loewenstein [1987]	Students (N = 30)	Open-ended Fill-in-the-blank	3 hours, 1 day, 3 days, 1 year, 10 years	\$4 loss and \$1000 loss	8772%, 3395%, 115%, 62%, 11% for \$4, 8772%, 1469%, 25%, 9%. 4% for \$1000	Hypothetical
Benzion, Rapoport and Yagil [1989]	Economics and finance students (N = 204)	Open-ended Fill-in-the-blank	6 months, 1, 2 and 4 years	\$40, \$200, \$1000 and \$5000	46%, 32%, 29%, 19% for 6 months; 18%, 15%, 15%, 11% for 4 years (averaged over scenarios)	Hypothetical
Holcomb and Nelson [1992]	Business students (N = 101)	Binary choice	1, 7 and 14 days	\$5 and \$17	Impossible to determine, but fraction choosing to delay imply magnitude effect	Real
Raineri and Rachlin [1993]	Psychology course requirement (N = 120)	Titration	1, 6 and 12 months (also 1, 5, 10, 25 and 50 years)	\$100, \$10,000 and \$1 million	127%, 71%, 54% for \$100; 12%, 32%, 29% for \$10,000; 12%, 8%, 11% for \$1 million	Hypothetical
Shelley [1993]	Accounting students (N = 74)	Open-ended Fill-in-the-blank	6 months, 1, 2 and 4 years	\$40, \$200, \$1,000 and \$5,000	24%, 15%, 14%, 14% for \$40, \$200, \$1000 and \$5000, respectively, in the neutral frame; no magnitude effect for the payment or expedite frames	Hypothetical
Green, Fristoe and Myerson [1994]	Students (N = 24)	Binary choice with titration on delays	1 week, 1 month and 3 months	\$20 & \$50, \$100 & \$500, \$500 & \$1250	100%, 71% and 30%	Hypothetical
Green, Fry and Myerson [1994]	Young and old adults (N = 12 each group)	Titration	1 week, 1 and 6 months, 1, 3, 5, 10 and 25 years	\$1,000 and \$10,000	Young adults: 63%, 54%, 46%, 32%, 25% for \$1,000; 40%, 34%, 29%, 19%, 15% for \$10,000	Hypothetical
Chapman & Elstein [1995]	Psychology Students (N=70)	Open-ended Fill-in-the-blank	6 months, 1, 2 and 4 years	\$200, \$1000, \$5000 and \$25000	For 6 months (\$200, 400%), (\$1000, 210%), (\$5000 and \$25000, 75%). For 1 year (\$200, 200%), (\$1000, 125%).	Hypothetical
	Psychology Students (N=34)		1, 3, 6 and 12 years	\$500, \$1000, \$2060 and \$4000	For 1 year (\$500, 150%), (\$1000, 125%), (\$2060 and \$4000, 50%)	
Kirby & Marakovic [1995]	Students (N=21)	Open-ended Sealed-bid auction	3 to 29 days	\$14.5 and \$28.5	Not reported by magnitude	Real
	Students (N=18)		3 to 29 days	\$14.5 and \$28.5	Not reported by magnitude	Hypothetical

Chapman [1996]	Students (N=40)	Fill-in-the-blank	1, 3, 6 and 12 years	$500, $1000, $2000 and $4000	For 1 year 115%, 115%, 75%, 75%, respectively. For 3 years 60%, 50%, 40%, 40% respectively	Hypothetical
	Students (N=77)	Fill-in-the-blank	1 and 9 years	Gains and losses of $500, $1500 and $4500	For 1 year gain of principal 130%, 55%, 55% respectively, and for a loss of principal 60%, 25%, 25%.	Hypothetical
Kirby & Marakovic [1996]	Students (N = 621)	Choice	10 - 70 days	$16 up to $83	Not reported by magnitude	"Almost hypothetical"
Kirby [1997]	Students & Others (N = 24, 28, 20)	Bidding	1 - 29 days	$10 or $20 in the future	*Daily* rates of 5.6% (3.6%), 2.4% (1.4%), 3.5% (2.4%) for each sample and the $10 ($20) magnitude, respectively.	Real
Green, Myerson & McFadden [1997]	Students (N=20)	Binary choice with titration on delays	3 and 6 months, 1, 3, 5, 10 and 20 years	$100, $2000, $25000 (and $100,000 in the future)	$100 principal: 75%, 50%, 39%, 17%, 18%, 10% and 8% by horizon; $2,000 principal, 21%, 19%, 12%, 10%, 12%, 10% and 7%; $25,000 principal: 12%, 10%, 9%, 8%, 8%, 5% and 6%	Hypothetical
Chapman & Winquist [1998]	Students (N=50)	Open-ended Fill-in-the-blank	3 months	Taxi ride: $5, $10, $15, $20. Haircut: $7.5, $15, $30, $60. Restaurant dinner: $20, $40, $80, $160.	Values inferred from eyeballing graphs. Roughly 5000% at $5, slightly over 2000% for $20, just under 2000% at $30, and slightly in excess of 1000% at $160	Hypothetical
Kirby, Petry & Bickel [1999]	Adults in control (N = 60)	Choice	7 - 186 days	($25 to $35), ($50 to $60) and ($75 to $85)	Small but statistically significant magnitude effect: no values reported	Real
Du, Green & Myerson [2002]	Chinese, Japanese and American Students (N=79)	Titration	1, 3 and 9 months, 2, 5, 10, and 20 years	$200 and $10000	Values reported graphically, impossible to infer numerically.	Hypothetical
Estle, Green, Myerson & Holt [2007]	Students (N=47)	Titration	1 week, 1 and 6 months, 1 and 3 years	$40 and $100	ANOVA Analysis. Magnitudes not reported	Hypothetical
Benhabib, Bisin & Schotter [2010]	Students (N = 27)	Open-ended Fill-in-the-blank	3 days, 1 & 2 weeks, 1, 3 & 6 months	$1, $4, $8, $15, $40, $80 and many interim values	Not reported by magnitude: inspection of choices for 3 subjects (their Figure 1) broadly consistent with effect	Real
Scholten and Read [2010]	Adults on web (N = 196)	Choice	16, 19 and 22 months	$25, $50, $100 and $250	Not reported by magnitude	Hypothetical

a: Discount rates are reported in order of stake magnitudes. Values in brackets are alternative rates for the same magnitude.

Table 2: Small Stakes and the Effects of Rounding

Amount Today	Future Amount	Horizon in days	AER	Monetary Premium	Amount Today	Future Amount	Horizon in days	AER
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$7	$7.01	3	10%	$0.01	$7	$8	3	1661%
$6	$6.01	7	10%	$0.01	$6	$7	7	813%
$5	$5.02	14	10%	$0.02	$5	$6	14	478%
$3	$3.02	30	10%	$0.02	$3	$4	30	352%
$2	$2.05	91	10%	$0.05	$2	$3	91	163%
$1	$1.05	181	10%	$0.05	$1	$2	181	140%
$14	$14.01	3	10%	$0.01	$14	$15	3	849%
$12	$12.02	7	10%	$0.02	$12	$13	7	420%
$10	$10.04	14	10%	$0.04	$10	$11	14	249%
$8	$8.07	30	10%	$0.07	$8	$9	30	144%
$7	$7.18	91	10%	$0.18	$7	$8	91	54%
$4	$4.20	181	10%	$0.20	$4	$5	181	45%
$24	$24.02	3	10%	$0.02	$24	$25	3	500%
$22	$22.04	7	10%	$0.04	$22	$23	7	233%
$19	$19.07	14	10%	$0.07	$19	$20	14	134%
$17	$17.14	30	10%	$0.14	$17	$18	30	70%
$14	$14.36	91	10%	$0.36	$14	$15	91	28%
$8	$8.41	181	10%	$0.41	$8	$9	181	24%
$39	$39.03	3	10%	$0.03	$39	$40	3	309%
$36	$36.07	7	10%	$0.07	$36	$37	7	143%
$32	$32.12	14	10%	$0.12	$32	$33	14	80%
$28	$28.23	30	10%	$0.23	$28	$29	30	43%
$20	$20.50	91	10%	$0.50	$20	$21	91	20%
$15	$15.76	181	10%	$0.76	$15	$16	181	13%
$80	$80.07	3	10%	$0.07	$80	$81	3	151%
$75	$75.14	7	10%	$0.14	$75	$76	7	69%
$70	$70.27	14	10%	$0.27	$70	$71	14	37%
$65	$65.54	30	10%	$0.54	$65	$66	30	19%
$60	$61.51	91	10%	$1.51	$60	$62	91	13%
$40	$42.03	181	10%	$2.03	$40	$43	181	15%

Figure 1: Random Coefficient Estimates of Risk Attitudes and Discount Rates

Assuming CRRA utility and Exponential discounting; N=198

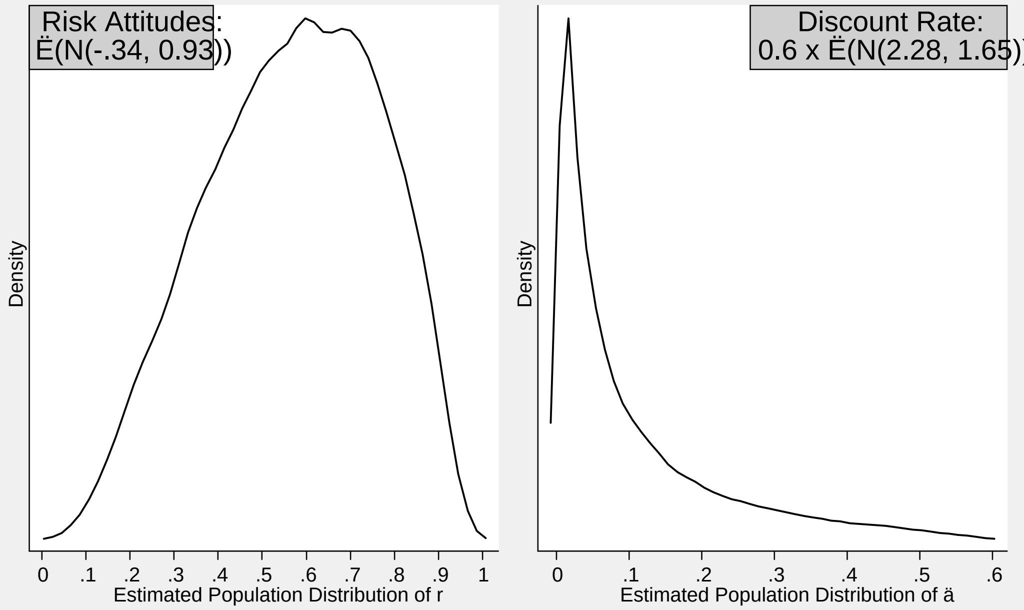
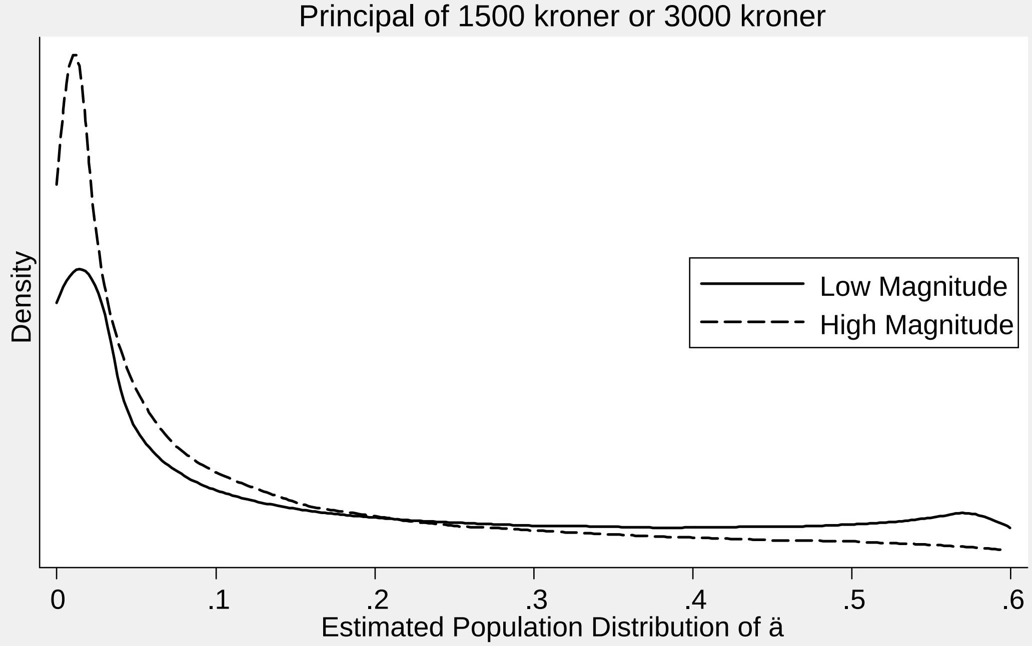


Figure 2: Estimated Population Distribution of Discount Rates by Magnitude of Principal

Principal of 1500 kroner or 3000 kroner



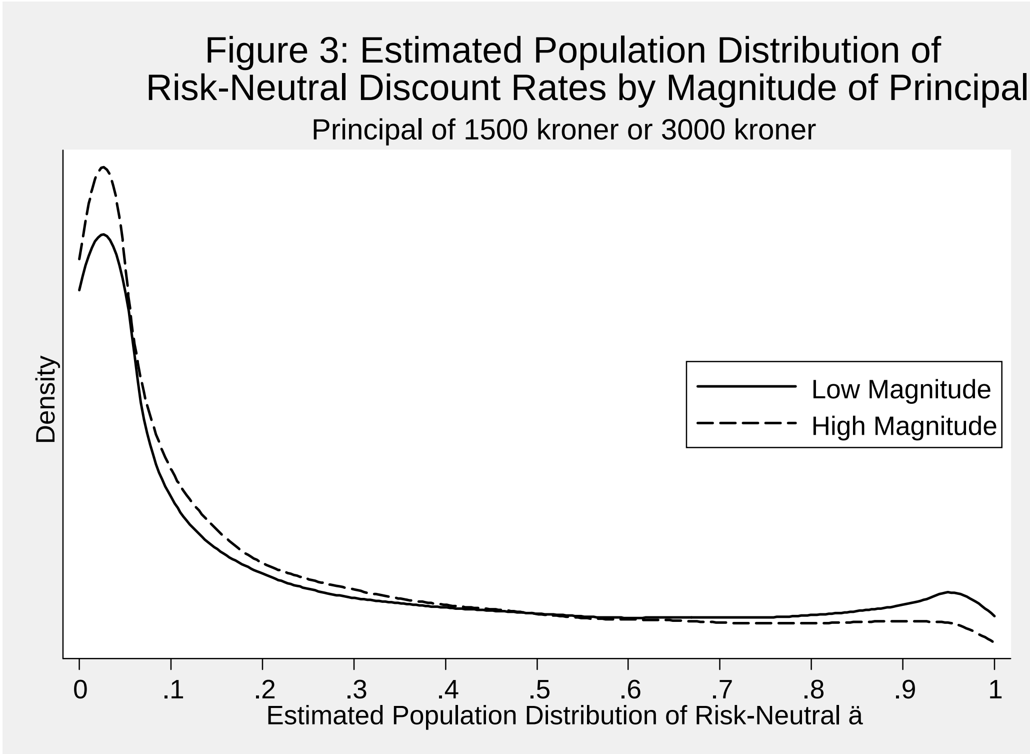


Figure 3: Estimated Population Distribution of
Risk-Neutral Discount Rates by Magnitude of Principal
Principal of 1500 kroner or 3000 kroner
Density
Low Magnitude
High Magnitude
0
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.6
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1
Estimated Population Distribution of Risk-Neutral ä

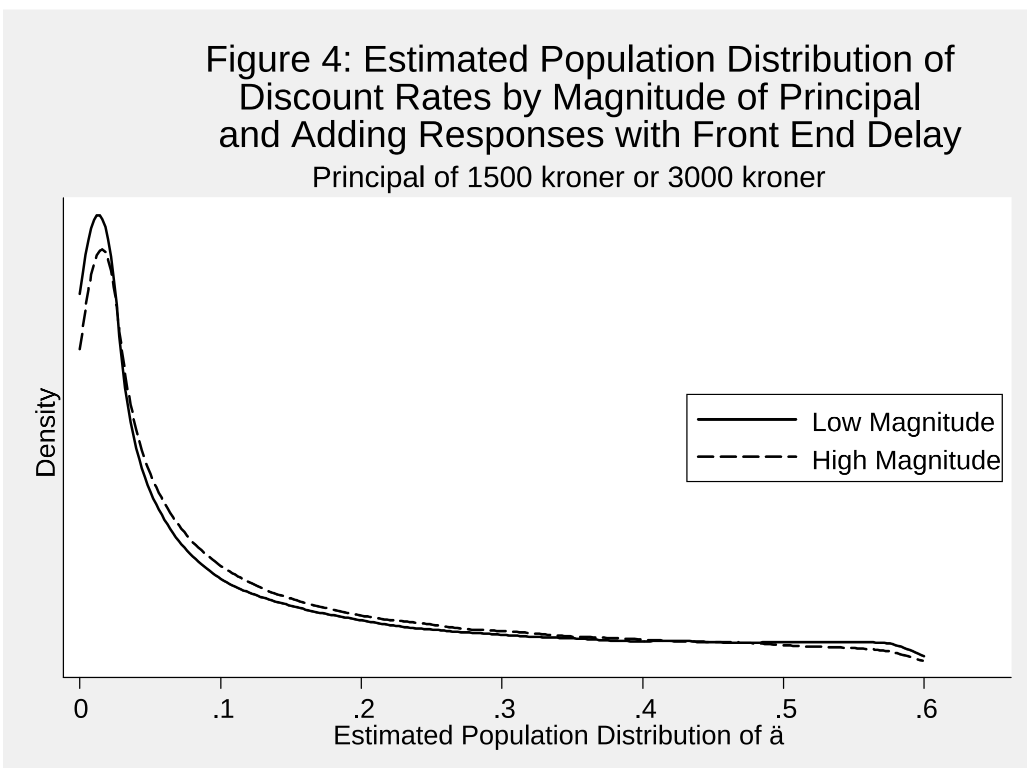
Figure 4: Estimated Population Distribution of
Discount Rates by Magnitude of Principal
and Adding Responses with Front End Delay
Principal of 1500 kroner or 3000 kroner
Density
Low Magnitude
High Magnitude
0
.1
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.6
Estimated Population Distribution of ä

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Appendix A: Literature Review

Thaler [1981] asked each individual to state the delayed amount that they would require to be willing to give up a certain amount today. In each case there was a 3×3 design, varying the initial amount and the time delay. For one group of subjects the amounts were $15, $250 and $3000, and the delays 3 months, 1 year, and 3 years; for another group the amounts were $75, $250 and $1200, and the delays 6 months, 1 year, and 5 years; and for the final group the amounts were $15, $250 and $3000, and the delays 1 month, 1 year and 10 years.[15] All choices were hypothetical, and the sample consisted of U.S. college students. Median responses are reported, with no tests of statistical significance. Table 1 tabulates these results, converted to the annual effective rate assuming daily compounding. No statistical test is typically needed when the median or means are so different, although one would like to know if the distributions had large variances or skewness.

Loewenstein [1987] reports the results of a hypothetical survey of 30 undergraduate students. The only questions that address the magnitude effect involved the subjects stating the delayed amounts that they would pay to avoid a $4 loss now and a $1000 loss now.[16] The time delays were 3 hours, 1 day, 3 days, 1 year and 10 years. Results are reported (Appendix 1, p. 681) as the mean of the fraction of the willingness to pay to avoid the outcome now. So these fractions may be viewed as discount factors, and discount rates inferred assuming an exponential model simply for descriptive purposes. For the $4 amount the continuously compounded rates are 8772%, 3395%, 115%, 62% and 11%, and for the $1000 amount the corresponding rates are 8772%, 1469%, 25%, 9% and 4%. Ignoring the 3-hour horizon, and the decline with horizon, the larger amount clearly

[15] Another group was presented comparable tasks framed as a loss. We focus here solely on choices framed as gains.

[16] There is no mention of the magnitude effect in this study, which addresses other concerns with discounting. The other three questions involved willingness to pay for a $4 gain, willingness to pay for a kiss from a movie star of your choice, and willingness to pay to avoid a non-lethal, 120-volt electric shock.

generated lower discount rates.

Benzion, Rapoport and Yagil [1989] asked hypothetical fill-in-the-blank questions to 282 economics and finance students, of which 204 provided useable responses. They employed four scenarios in which the respondents were asked to imagine a setting in which they had been asked to postpone a receipt or a payment of an amount (hence they were told the amount x now and asked to state the equivalent amount y in the future), or to expedite a receipt or a payment (hence they were told the amount y in the future and asked to state the equivalent amount x now). The time horizons considered were 0.5, 1, 2 and 4 years, and the stated amounts were $40, $200, $1000 and $5000. For present purposes the main results are illustrated by averaging across scenarios, and focusing on the shortest and longest horizons only (Table 2.C, p.278). Average discount rates for each stated amount were 46%, 32%, 29% and 19%, respectively, for the 6-month horizon, and 18%, 15%, 15% and 11%, respectively, for the 4-year horizon. Although considerable noise is present in the data, and no standard errors are reported, one can clearly see the magnitude effect pattern in the shorter-horizon responses, and "faintly" in the longer-horizon data.

Holcomb and Nelson [1992] employed procedures that anticipate the more recent application of modern experimental methods to the elicitation of discount rates, and indeed is heavily cited by Coller and Williams [1999] in their seminal study. They used 101 business students in a series of 41 binary choice tasks, where the sooner and later amounts were presented as given and the subject simply needed to pick one. Treatments included the use of front end delays on the sooner option of 0 days, 1 day or 7 days, the use of a low principal of $5 or a high principal of $17, and the use of a low daily interest rate of 1.5% or a high daily interest rate of 3.0%. One of the 41 choices by each subject was selected for payment at the end of the session, so this appear to be the first study to examine the magnitude effect with real rewards and incentive-compatible elicitation procedures. Focusing on their results with no front end day and the higher interest rate, which are representative in this case, they observe that 23%, 41% and 56% of the subjects chose to delay with the principal of $5 and horizons of 1 day, 1 week and 2 weeks, respectively, whereas 53%, 79% and 87% chose to delay for the corresponding horizons when the principal was $17. Precise discount rates cannot be inferred from this design without strong parametric assumptions, since we only know that the discount rate was in the interval [0%, 1.5%), [1.5%, 3.0%) or [3%, ∞%). But the fractions reported are enough to conclude that there is a magnitude effect at work in these data. Appendix C (available on request) collates the data reported in their tables and demonstrates that there is a statistically significant magnitude effect.

Chapman [1996] is a remarkable study because it is the first to try to estimate discount rates over the utility of time-dated final outcomes. It contains three experiments: the first two are replications and extensions of the basic design of Chapman and Elstein [1995], and the third attempts to correct for concave utility when inferring discount rates. We focus on the elicitation of discount rates over monetary outcomes: a major theme in the overall design is a comparison of discount rates defined over monetary and health outcomes. Experiment 1 posed 32 questions to 40 students fulfilling a course credit. These questions called for a fill-in-the-blank answer for the amount of money in the future equal to a fixed amount today. Horizons of 1, 3, 6 and 12 years were used, and principal amounts of $500, $1000, $2000 and $4000 employed. Geometric means of log-transformed annual discount rates are used in the graphs, which provide the only basis for eyeballing those values. Figure 1 (p.774) shows a clear magnitude effect, with discount rates for 1 year being roughly 115%, 115%, 75% and 75% for each principal amount, respectively; for a 3-year horizon these rates drop to 60%, 50%, 40% and 40%, respectively; this decline and convergence pattern persists for the later horizons. Experiment 2 is similar, and extends the design to consider loss frames as well as gain frames. In this case the instrument contained 48 questions of the same fill-in-the-blank format, administered to 77 students again fulfilling a course credit. The horizons were only 1 year and 9 years, and the principal amounts were $500, $1500 and $4500. The evidence suggests a clear magnitude effect for the 1 year horizon, and none for the 9 year horizon. For the shorter horizon, the discount rates, eyeballed from Figure 2A (p. 778) are 130% for a gain of $500, 55% for gains of $1500 or $4500, 60% for a loss of $500, and 25% for losses of $1500 or $4500. Thus there is evidence of a diminishing magnitude effect in these data.

Experiment 3 of Chapman [1996] is the one that seeks to correct for concave utility. Given the importance this takes on later in the literature, it is worthwhile documenting the clarity of the motivation:

> In the studies so far, discount rates were calculated as the percentage increase in dollars or years of full health needed to offset a delay. An alternative way to calculate discount rates would be the percentage increase in utility needed to offset a delay. These two calculations will be different if utility is not a linear function of dollars or years of full health.

The experimental design contains 38 questions which follow the style of Experiment 2, to elicit discount rates defined over monetary outcomes if one assumes linear utility. The horizons were 1, 3 and 9 years, the principal amounts were $500, $1000 and $4500, and gain and loss frames were again considered. Eyeballing Figure 5A (p. 784) there is striking evidence of a magnitude effect for 1 and 3 years discount rates. This baseline, familiar from Experiments 1 and 2, is important in comparison to the utility-corrected discount rates.

Utility scales were elicited using the following procedure (p. 782):

> In Part 2, participants answered 18 utility assessment questions in which they matched intervals to be of the same subjective magnitude (von Winterfeldt & Edwards [1986; p. 232]). For example, participants considered the interval from $0 to $500 and specified a value *x* such that the interval from $500 to $*x* was of the same subjective magnitude.

> Specifically, participants were asked to consider two intervals; for example,
>
> Interval A: You expected to win $0 but instead will win $500.
>
> Interval B: You expected to win $500 but instead will win ___.
>
> They were asked to specify the amount that would have to appear in the blank for Interval B to make Interval B subjectively the same as Interval A. If a participant answered, say, $1,200, the next question concerned an interval from $500 to $1,200 and a second from $1,200 to *y*, in which the participant must specify *y*. Participants answered four such questions.
>
> If one arbitrarily defines $0 to be 0 utiles and $500 to be 1 utile, then participants' responses are associated with 2, 3, 4, and 5 utiles. To measure utility for monetary losses, I asked participants to match the interval from $500 to $0 to the interval from $0 to *z*, in which *z* was a negative amount. This answer corresponded to −1 utile. Four additional questions specified −2, −3, −4, and −5 utiles.

From these responses power utility functions were estimated, one for each subject. Ignoring the reliability of the determination of these utility functions for the moment, the implied utility discount rates appear to exhibit no quantitatively significant magnitude effect! From her Figure 5E (p. 784) the rates are between 20% and 5% for the 1 year horizon and the various principals, and between 15% and 5% for the 3 year horizon. Although these magnitude effects are *statistically* significant in terms of an ANOVA test (Table 8, p. 785), Chapman [1996; p. 783] notes that, "Because of the very curved utility function for money, utility discount rates for money were so low that differences were hard to detect." No data on the estimated exponents of these utility functions is presented, although the graph of the average value of the estimates (Figure 3, p. 782) does suggest significant concavity.

What about the method for eliciting the utility function? This method is well-known from several older literatures, cited by Wakker and Deneffe [1996; p. 1132], who extend it to eliciting utilities over lotteries. The exposition in von Winterfeldt and Edwards [1986; p.232-5] is clear. If the response to the first question about the amount of money, greater than $500, that makes you indifferent to the gap between $0 and $500, is honest, this method is valid. But there are three problems with this implementation, quite apart from it being hypothetical:

- The questions as posed by Chapman [1996] do not clearly ask the subject to state the increment in money that is the same in subjective utility terms as the baseline increment. The language literally says, "you have $500, what new money amount would make you indifferent to that." There is a colloquial interpretation of the language that is just the "interval" she assumes the subject responds to, but the language contains some ambiguities that one likes to avoid in subject instructions if possible.

- The subject has no incentive to report the blank amount honestly.
- If the subject was motivated by this sequence of tasks, such as if it involved real stakes or the subject kindly behaved as if it was real when it was not, then the rational response is to state a very big number whenever a blank is asked for.

These last two problems are well-known with the Trade-Off Method that is in wide use, and that was proposed by Wakker and Deneffe [1996]: see Harrison and Rutström [2008; §1.5] for an extended discussion.

An additional econometric concern with the utility-adjustments used by Chapman [1996] is that she did not jointly estimate the parameters of the utility function and discounting function. Instead she estimated the former and then assumed the point estimate from that estimation when inferring the latter, ignoring the sampling errors on the former when inferring the latter. Full information maximum likelihood methods for undertaking this joint estimation are now well-known, and reviewed by Harrison and Rutström [2008; §3.4].

For all these problems, and they are each conceptually serious, it is clear that Chapman [1996] was an important advance on the elicitation and modeling of discount rates. Andersen, Harrison, Rutström and Lau [2008] demonstrate how these problems can be avoided or solved.[17]

Kirby and Maraković [1996] undertook a clean experiment in which each subject was asked to make 21 binary choices between a certain amount of money today and a larger amount of money

[17] It is also appropriate to correct our earlier [2008] characterization of this study as not eliciting or estimating risk attitudes. We noted then (p.611) that "Chapman [1996] drew the correct formal link between estimation of individual discount rates and concavity of the utility function, but did not elicit risk attitudes. Instead she used hypothetical questions to elicit individual discount rates over money and health, and then estimated individual discount rates based on various assumptions about the risk attitudes of the subjects." Our current exposition makes it clear that she did much more than that.

in the future. The principal was varied from choice to choice, as they were presented to subjects, as was the larger amount and the horizon. The principal was varied between $16 and $83, the later amount was varied between $30 and $85, and the horizon was varied between 10 and 70 days. This design yielded choices that had annualized discount rates between 128% and 1.2E+13%. The average subject started switching over to the later option when discount rates were above 596%, and only 12% of subjects accepted the lowest discount rate of 128%. It is almost irrelevant if discounting over this horizon was constant or not, since one can undertake thought experiments with longer horizons and rule out such rates being accepted for those choice. One immediate concern with this task is that it is "almost hypothetical." Questionnaires were sent out to every undergraduate student at Williams College, which had a student population of roughly 2000 at the time. Subjects were told that the questionnaires could be returned before one of two days, and that on each day one would be drawn at random and one of the 21 choices played out for real. Despite cheap talk in the instructions that, to "make sure that you get a reward you prefer, you should *assume* that you are the winner, and then make *each choice* as though it were the one that you will win," these are very poor financial incentives. Even if the subject was certain that the lottery with the (delayed) $85 payment would be chosen, this is an expected earning of only $0.085. Although subjects were not told how many of the questionnaires were distributed, this is a small campus and such things are not private. In the event, 672 responded, implying that there was actually an expectation of only $0.25 if the largest prize was then selected. Moreover, even if the largest payment was chosen in all 21 cases, the expected earnings were only $56.19 per lucky subject, and not $85. It is a pity that this clean, transparent task was marred by the use of poor incentives.

Kirby [1997] is a remarkable study: it used real incentives, used payments by subjects out of their own cash, used an incentive-compatible second-price sealed-offer auction to elicit present values, considered the effect of varying the deferred amount ($10 or $20), and considered all odd-numbered horizons between 1 and 29. Each subject entered 30 bids, and was told that one of these bids would be selected at random for payment if the bid was the winning bid. Each auction apparently consisted of the entire sample in an experiment, which does not affect the incentive compatibility of the procedure. Subjects in experiment 1 were "pseudo-volunteers" receiving extra credit in a psychology class for attending, but apart from the show-up rewards all payments were salient. Subjects in experiments 2 and 3 were "people from the Williams College community, including summer students, college staff, and persons unaffiliated with the college," and recruitment was by sign-up fliers and newspaper advertisements. Although it is not possible to infer proximate discount rates without knowing horizons, the daily rates averaged 5.6% for the $10 amount and only 3.6% for the $20 amount in one experiment with 24 students completing a class credit; 2.4% and 1.4%, respectively, for 28 non-students recruited from the field; and 3.5% and 2.4%, respectively, for 20 non-students. These were statistically significantly different from each other (p. 60, 62, 63), and the implied annual rates would be extremely large.

Kirby, Petry and Bickel [1999] implement the procedures of Kirby and Maraković [1996] with 56 heroin addicts and 60 control subjects. The control subjects were recruited by newspaper advertisement, and then matched approximately to the demographics of the sample of heroin addicts. Each subject was given 27 binary choices, and told that there was a 1-in-6 chance that one of the 27 choices would be paid out. Thus these are incentivized tasks. The later amounts of money were grouped into three reward sizes, small ($25 to $35), medium ($50 to $60) and large ($75 to $85), and horizons varied between 7 and 186 days. The results are reported (Table 3, p. 81) in terms of the parameter K in the hyperbolic specification $D(t) = 1/(1+ Kt)$ of the discount factor for horizon t, and the number of subjects that were indifferent at that value (intervals were averaged).

For this specification the implied discount rate is $d(t) = (1 + Kt)^{(1/t)} - 1$. Thus for each discount rate one can infer the fraction of subjects that have a discount rate less than that discount rate, and draw inferences about the discount rates of the sample. In effect, this is an "interval regression" with a constant, and with the sample weighted by the sample size. The raw data from their Table 3 is collated in Appendix C (available on request). A grouped logistic analysis of those data shows that there is a statistically significant magnitude effect, but that it is quantitatively small.

Benhabib, Bisin and Schotter [2010] present subjects with two types of fill-in-the-blank tasks. In one type, the subject was asked 30 questions of the form "what amount of money, \$x, if paid to you today would make you indifferent to \$y paid to you in t days?" In this case the amount \$y and the horizon t would be filled in: $y \in \{10, 20, 30, 50, 100\}$ and $t \in \{$3 days, 1 week, 2 weeks, 1 month, 3 months, 6 months$\}$. The response \$x was incentivized with a Becker, DeGroot and Marschak [1964] (BDM) auction for one of the 30 choices selected at random. A price would be drawn from a uniform distribution between [\$0, \$y], and if the random price was greater than the stated amount \$x then the subject would receive that random price immediately; otherwise the subject would receive \$y in t days. So the upper bound of the BDM auction was the larger amount to be provided in the future. The other type of fill-in-the-blank question involved 30 questions of the form, "what amount of money, \$y, would make you indifferent between \$x today and \$y t days from now? [upper bound = \$z]," where the text in brackets was given to subjects as notation instead of these words. In this case the values of t were the same as the first fill-in-the-blank task, and the values of $x \in \{1, 2, 3, 5, 6, 7\}$ for z=10, $x \in \{4, 7, 8, 10, 12, 14\}$ for z=20, $x \in \{8, 14, 17, 19, 22, 24\}$ for z=30, $x \in \{15, 20, 28, 32, 36, 39\}$ for z=50, and $x \in \{40, 60, 65, 70, 75, 80\}$ for z=100. The same subjects were given both sets of questions on different days. The data were evaluated using a flexible specification, and the model estimated for each individual using non-linear least squares. The individual estimates are very erratic, with a wide range of behaviors being inferred. The general theme is of extremely high discount rates, reported to average 472%, and considerable noise. Although they report evidence of significant magnitude effects (p. 209), they only show data from 3 subjects; presumably, those data are representative.

One major issue in this design is the subject comprehension of the BDM procedure, which is often asserted by experimenters to be understood by subjects but often is not.[18] In this design the upper bound for the BDM procedure was varied with magnitude in a complicated manner, as one can discern from inspection of the parameters listed above. This would not be an issue apart from the fact that it appears to have been a behavioral focal point for elicited valuations. The striking result is that the modal valuation was to bid the upper bound, and the majority were close to that upper bound. This is a serious concern about the subject comprehension of the elicitation procedure.

Additional References

Becker, Gordon M.; DeGroot, Morris H., and Marschak, Jacob., "Measuring Utility By A Single-Response Sequential Method," *Behavioral Science*, 9, July 1964, 226-232.

Harrison, Glenn W., "Theory and Misbehavior of First-Price Auctions: Reply," *American Economic Review*, 82, December 1992, 1426-1443.

Wakker, Peter P., and Deneffe, Daniel, "Eliciting von Neumann-Morgenstern Utilities when Probabilities Are Distorted or Unknown," *Management Science*, 42, 1996, 1131-1150.

von Winterfeldt, Detlof, and Edwards, Ward, *Decision Analysis and Behavioral Research* (New York: Cambridge University Press, 1986).

[18] Benhabib, Bisin and Schotter [2010; p.208] assert that "We had no doubt that the subjects understood the incentive properties of the mechanism." Concerns about the behavioral reliability of the BDM procedure are voiced by Harrison [1992], Plott and Zeiler [2005] and Harrison and Rutström [2008; §1.4, Appendix D].

Appendix B: Instructions (NOT FOR PUBLICATION)

We document the instructions by first listing the "manuscript" that shows what was given to subjects and read to them, and then we document some of the screen displays. The original Danish manuscript is available on request. The originals were in 14-point font, printed on A4 paper for nice page breaks (a horizontal line below indicates a page break), and given to subjects in laminated form. Any experimenter that would like to buy a used laminating machine should contact Steffen Andersen. The manuscript below was for the sessions in which the discount rate task was presented first. After these experimental tasks were completed there were additional tasks in the session that are not relevant here.

A. Experimental Manuscript

Welcome announcement

[Give informed consent form to subjects.]

Thank you for agreeing to participate in this survey. The survey is financed by the Social Science Research Council and the Carlsberg Foundation and concerns the economics of decision making.

Before we begin the survey, let me read out the informed consent form that is handed out to you. This form explains your rights as a participant in the survey, what the survey is about and how we make payments to you.

[Read the informed consent form.]

Is everyone able to stay for the full two hours of the meeting? Before we begin, I will ask each of you to pick an envelope from me. The envelope contains a card with an ID number that we will use to keep track of who answered which questions. All records and published results will be linked to anonymous ID numbers only, and not to your name. Please keep your ID numbers private and do not share the information with anyone else.

[Each subject picks an envelope.]

You will be given written instructions during the survey, but make all decisions on the computer in front of you. Please enter your ID number on the computer in front of you, but keep the card for later use.

You will now continue with the first task. The problem is not designed to test you. The only right answer is what you really would choose. That is why the task gives you the chance of winning money. I will now distribute the instructions and then read it out loud.

[Give IDR instructions to subjects.]
[Read the IDR instructions.]

Task D

In this task you will make a number of choices between two options labeled "A" and "B". An example of your task is shown on the right. You will make all decisions on a computer.

All decisions have the same format. In the example on the right Option A pays 100 kroner today and Option B pays 105 kroner twelve months from now. By choosing option B you would get an annual return of 5% on the 100 kroner.

We will present you with 40 of these decisions. The only difference between them is that the amounts and payment dates in Option A and B will differ.

You will have a 1-in-10 chance of being paid for one of these decisions. The selection is made with a 10-sided die. If the roll of the die gives the number 1 you will be paid for one of the 40 decisions, but if the roll gives any other number you will not be paid. If you are paid for one of these 40 decisions, then we will further select one of these decisions by rolling a 4-sided and a 10-sided die. When you make your choices you will not know which decision is selected for payment. You should therefore treat each decision as if it might actually count for payment.

You will receive the money on the date stated in your preferred option. If you receive some money today, then it is paid out at the end of the experiment. If you receive some money to be paid in the future, then it is transferred to your personal bank account on the specified date. In that case you will receive a written confirmation from Copenhagen Business School which guarantees that the money is reserved on an account at Danske Bank. You can send this document to Danske Bank in a prepaid envelope, and the bank will transfer the money to your account on the specified date.

Before making your choices you will have a chance to practice so that you better understand the consequences of your choices. Please proceed on the computer to the practice task. You will be paid in caramels for this practice task, and they are being paid on the time stated in your preferred option.

[Subjects make decisions in the practice IDR task.]

I will now come around and pay you in caramels for your choice of A or B. Please proceed to the actual task after your earnings are recorded. You will have a 1-in-10 chance of being paid for one of the 40 decisions in the actual task.

Password 1:____

[Subjects make decisions in the actual IDR task.]

I will now come around and ask you to roll a 10-sided die to determine if you are being paid for one of the decisions. If the roll of the die gives the number 1 you will be paid for one of the 40 decisions, but if the roll gives any other number you will not be paid. If you are paid for one of the 40 decisions, then I will ask you to roll a 4-sided and a 10-sided die to select one of the decisions for payment.

Password 2:____

[Roll 10-sided die to determine if they are being paid.]
[Roll 4-sided and 10-sided dice to determine the decision for payment.]

You will now continue with the second task. I will distribute the instructions and then read it out loud.

[Give RA instructions to subjects.]
[Read the RA instructions.]

Task L

In this task you will make a number of choices between two options labeled "A" and "B". An example of your task is shown on the right. You will make all decisions on a computer.

All decisions have the same format. In the example on the right Option A pays 60 kroner if the outcome of a roll of a ten-sided die is 1, and it pays 40 kroner if the outcome is 2-10. Option B pays 90 kroner if the outcome of the roll of the die is 1 and 10 kroner if the outcome is 2-10. All payments in this task are made today at the end of the experiment.

We will present you with 40 such decisions. The only difference between them is that the probabilities and amounts in Option A and B will differ.

You have a 1-in-10 chance of being paid for one of these decisions. The selection is made with a 10-sided die. If the roll of the die gives the number 1 you will be paid for one of the 40 decisions, but if the roll gives any other number you will not be paid. If you are paid for one of these 40 decisions, then we will further select one of these decisions by rolling a 4-sided and a 10-sided die. A third die roll with a 10-sided die determines the payment for your choice of Option A or B. When you make your choices you will not know which decision is selected for payment. You should therefore treat each decision as if it might actually count for payment.

If you are being paid for one of the decisions, we will pay you according to your choice in the selected decision. You will then receive the money at the end of the experiment.

Before making your choices you will have a chance to practice so that you better understand the consequences of your choices. Please proceed on the computer to the practice task. You will be paid in caramels for this practice task.

[Subjects make decisions in the practice RA task.]

I will now come around and pay you in caramels for your choice of A or B. I will ask you to roll a 10-sided die to determine the payment for your choice of A or B. Please proceed to the actual task after your earnings are recorded. You will have a 1-in-10 chance of being paid for one of the 40 decisions in the actual task.

Password 3:____

[Subjects make decisions in the actual RA task.]

I will now come around and ask you to roll a 10-sided die to determine if you are being paid for one of the decisions. If the roll of the die gives the number 1 you will be paid for one of the 40 decisions, but if the roll gives any other number you will not be paid. If you are paid for one of the 40 decisions, then I will ask you to roll a 4-sided and a 10-sided die to select one of the decisions for payment. A third die roll with a 10-sided die determines the payment for your choice of Option A or B.

Password 4:____

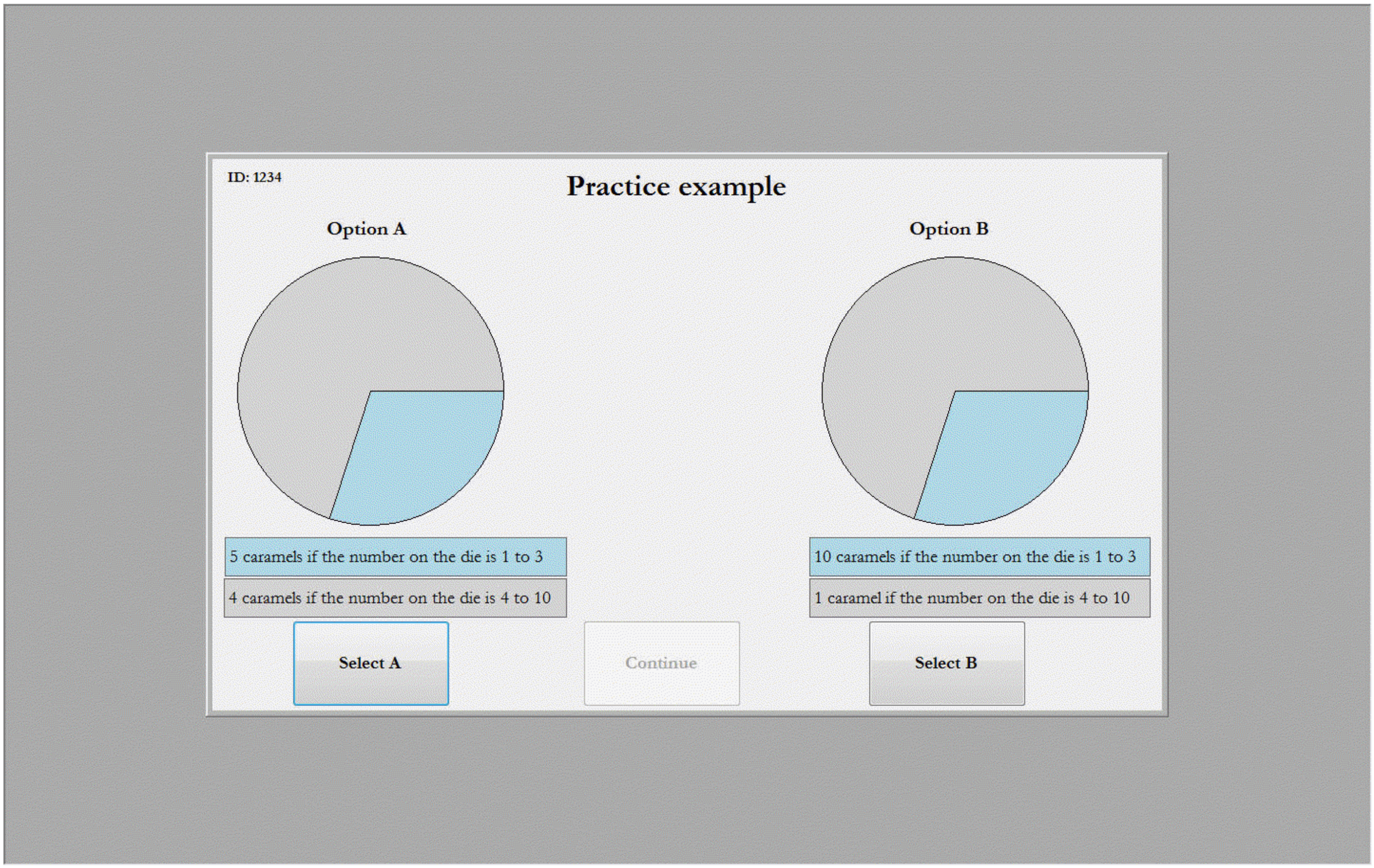
[Roll 10-sided die to determine if they are being paid.]
[Roll 4-sided and 10-sided dice to determine the decision for payment.]
[Roll 10-sided die to determine payment in Option A and B.]

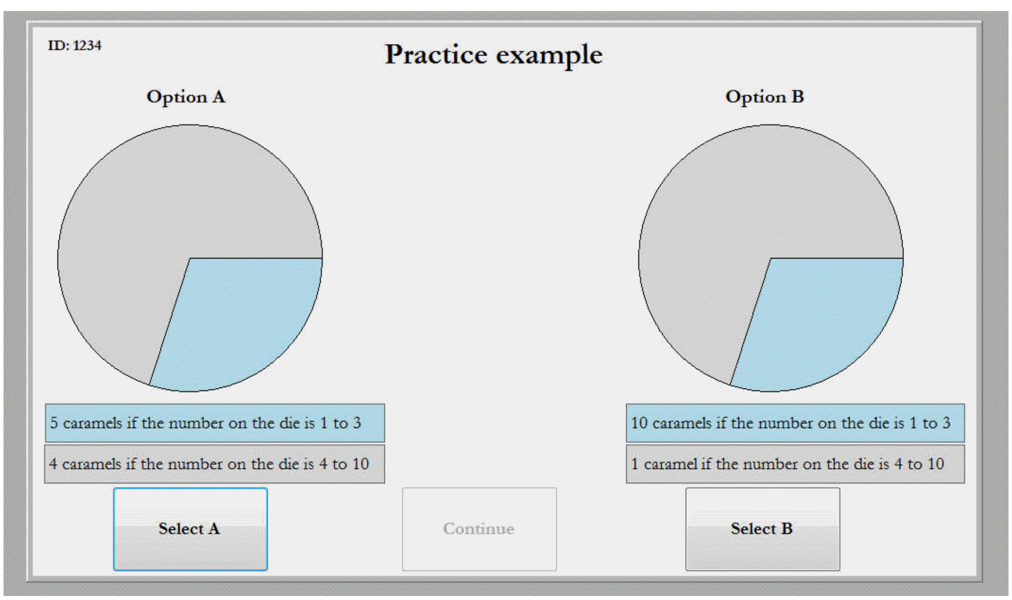
You will now continue with the third task. I will distribute the instructions and then read it out loud.

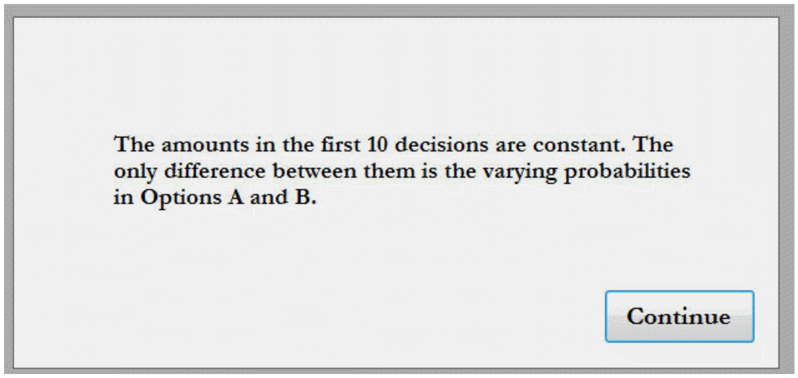
[ADDITIONAL INSTRUCTIONS WERE PROVIDED HERE]

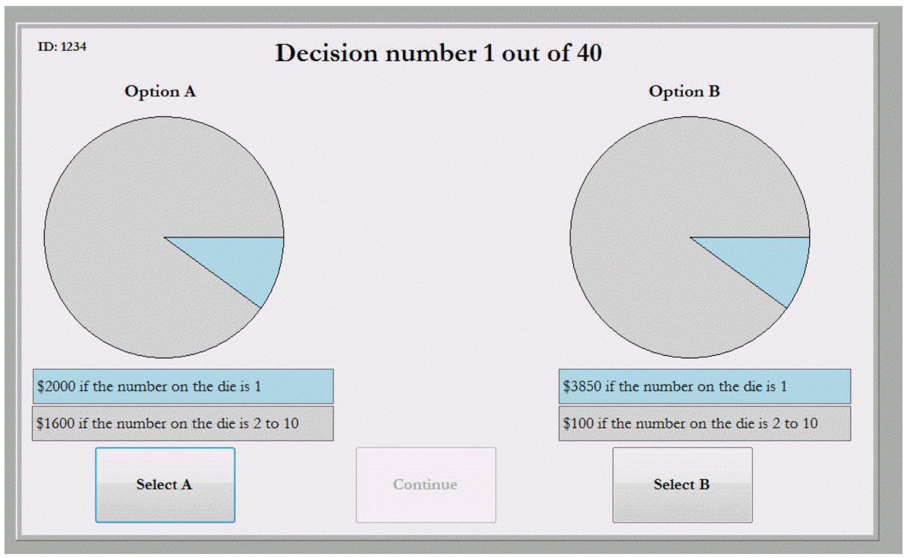
B. Typical Screen Shots for Lottery Choices

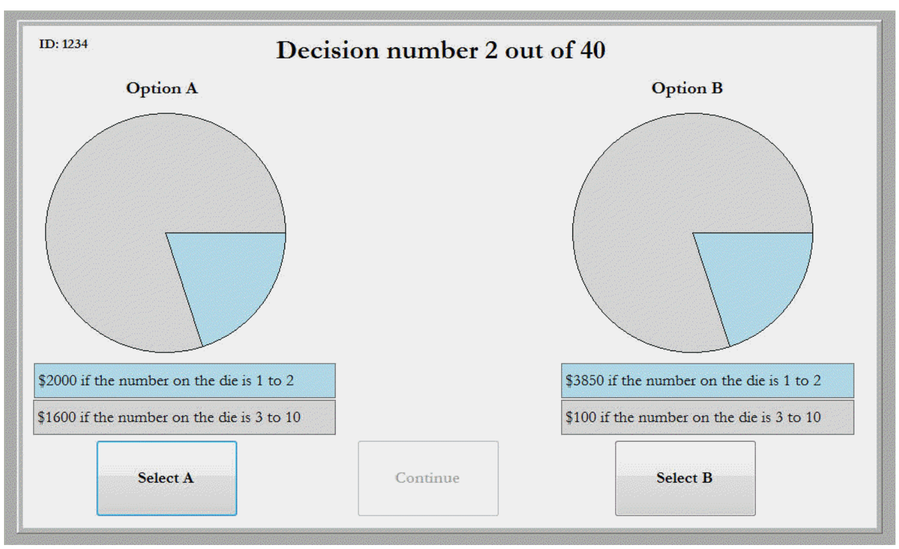
The first screen shot on the next page shows the full screen within which the text box is contained, so that one gets an impression of what the subject encountered in all screen shots. Then we display more detailed screen shots of the practice example and the first few lottery choices. Prior to each block of 10 lottery choices the subject was told that the lottery prizes for the next 10 choices would stay the same and the only thing that would vary would be the probabilities. We then show the sequence of the first two lotteries, and then lottery 11 which uses new prizes.

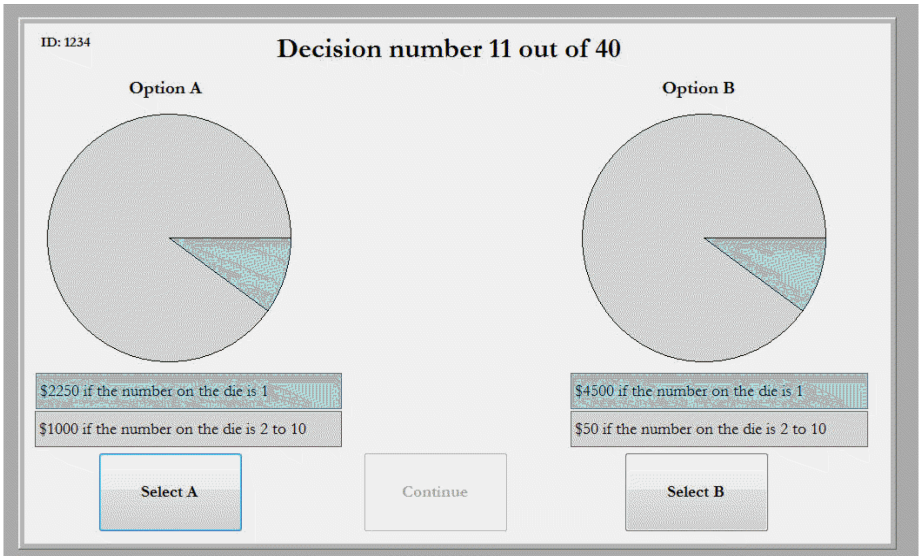

ID: 1234
Practice example
Option A
Option B
5 caramels if the number on the die is 1 to 3
4 caramels if the number on the die is 4 to 10
10 caramels if the number on the die is 1 to 3
1 caramel if the number on the die is 4 to 10
Select A
Continue
Select B


ID: 1234
Practice example
Option A
Option B
5 caramels if the number on the die is 1 to 3
4 caramels if the number on the die is 4 to 10
10 caramels if the number on the die is 1 to 3
1 caramel if the number on the die is 4 to 10
Select A
Continue
Select B


The amounts in the first 10 decisions are constant. The only difference between them is the varying probabilities in Options A and B.
Continue


ID: 1234
Decision number 1 out of 40
Option A
Option B
$2000 if the number on the die is 1
$1600 if the number on the die is 2 to 10
$3850 if the number on the die is 1
$100 if the number on the die is 2 to 10
Select A
Continue
Select B


ID: 1234
Decision number 2 out of 40
Option A
Option B
$2000 if the number on the die is 1 to 2
$1600 if the number on the die is 3 to 10
$3850 if the number on the die is 1 to 2
$100 if the number on the die is 3 to 10
Select A
Continue
Select B

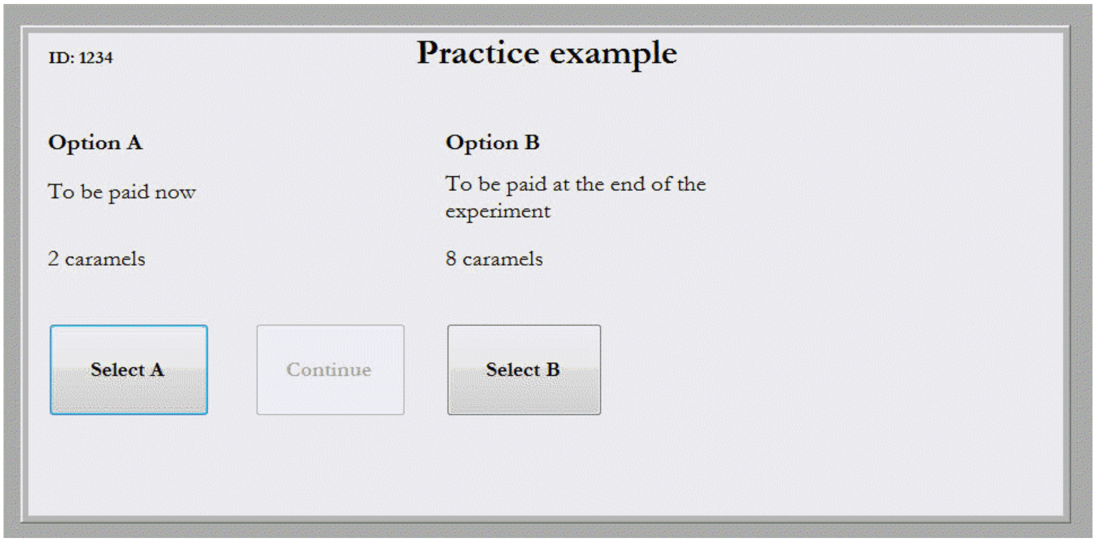


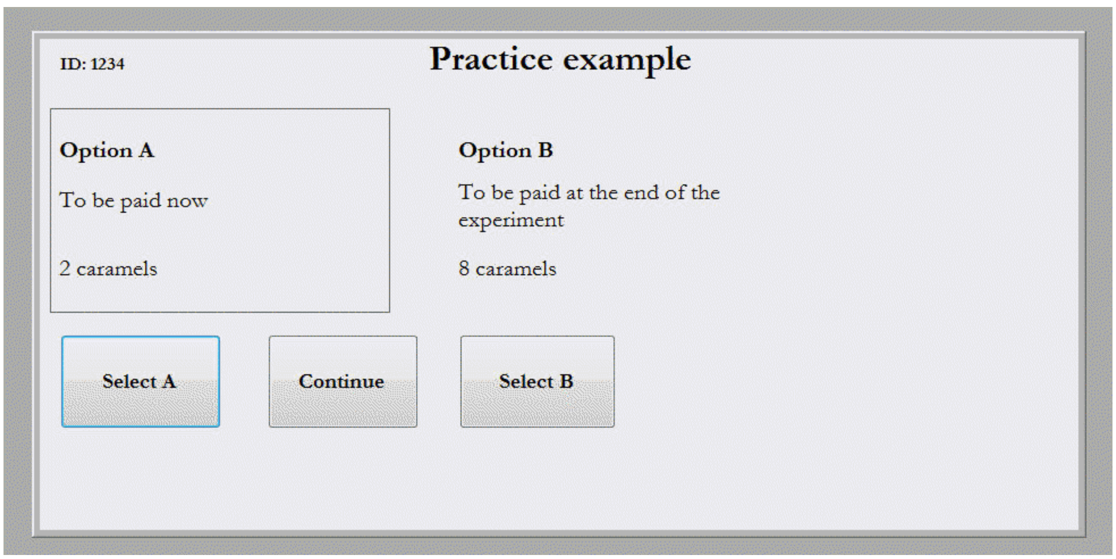
C. Typical Screen Shots for Discounting Choices

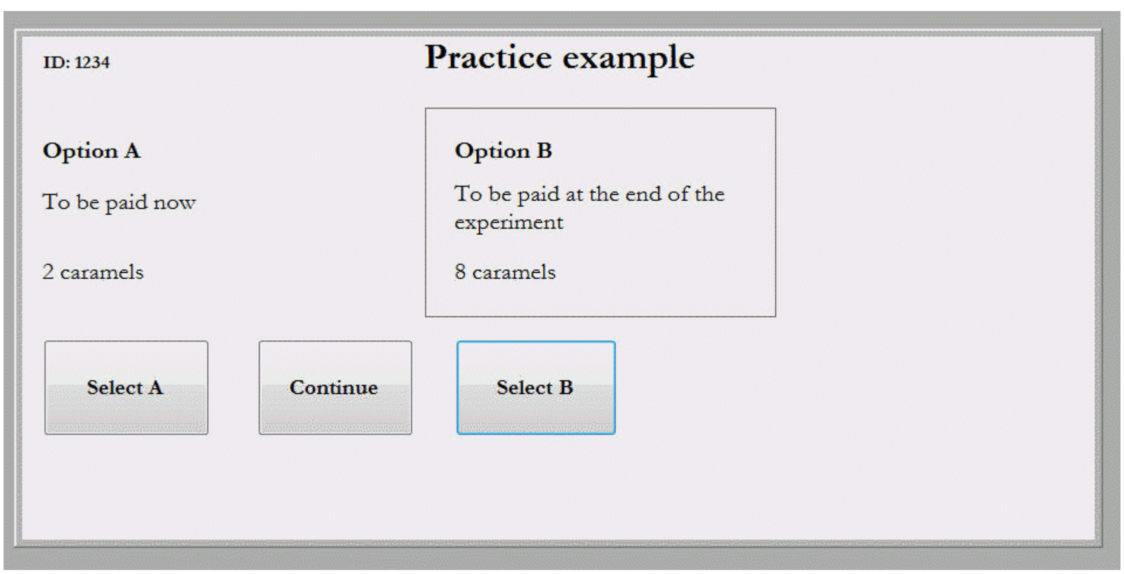
The next page shows the practice example provided at the beginning of these tasks. The top panel shows the initial screen shot, and then the next two panels show how the selected option is highlighted to make it clear to the subject which option is being selected.

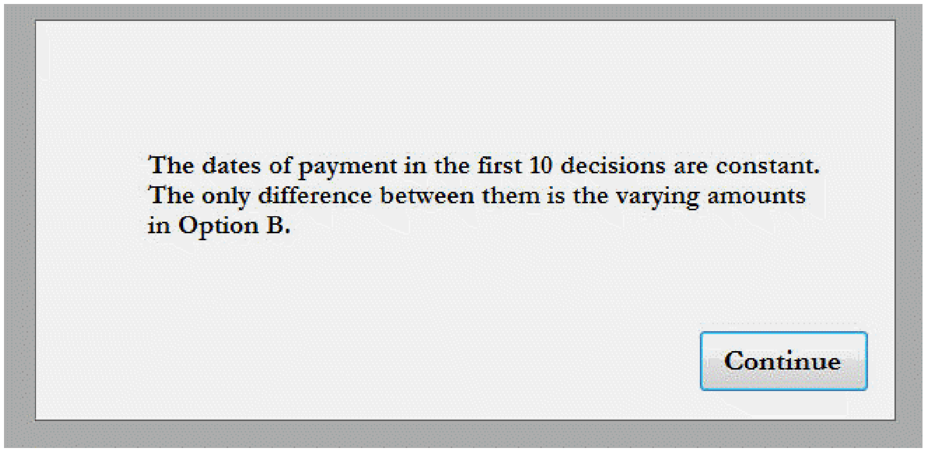
The following page shows the information that was given to each subject prior to each block of 10 choices. This information was that the principal and horizon would remain constant for the next 10 choices, but that the only thing that would change would be the amount in the "later" option. In these displays the implied interest rate is displayed.

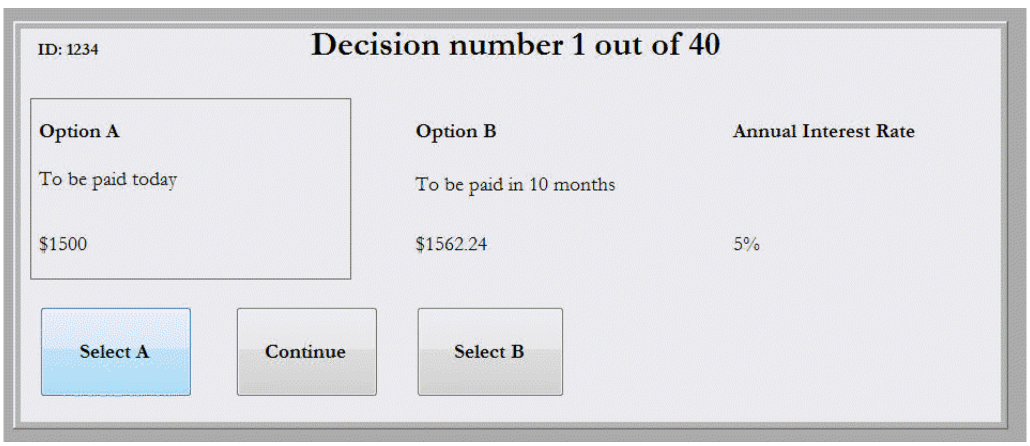
Finally, after the first 10 choices a new horizon was selected for the next 10 choices.

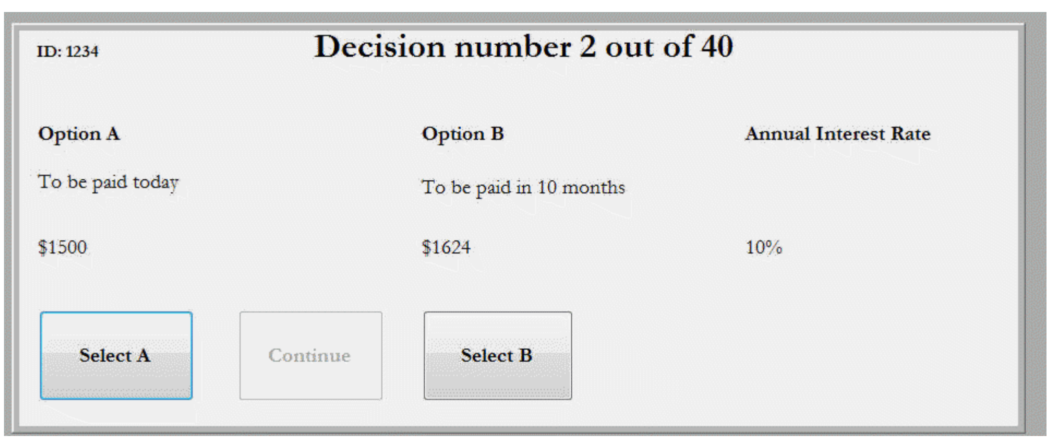

ID: 1234
Practice example
Option A
To be paid now
2 caramels
Option B
To be paid at the end of the experiment
8 caramels
Select A
Continue
Select B

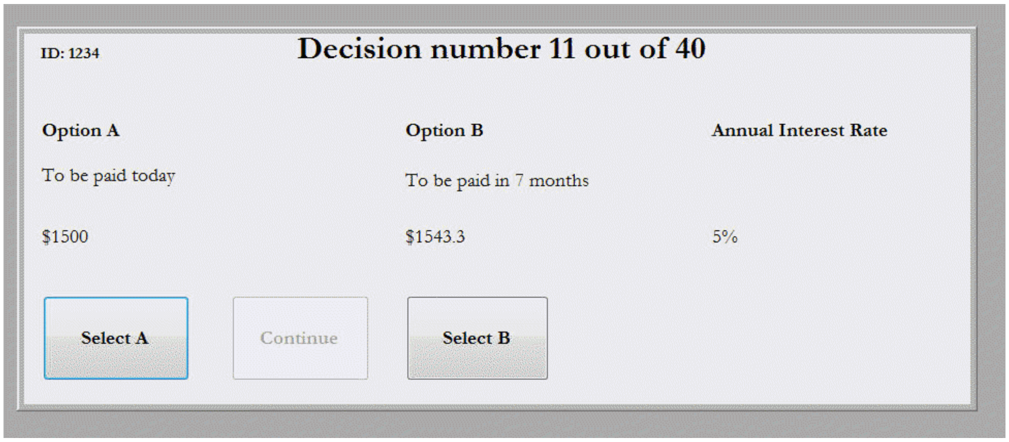

ID: 1234
Practice example
Option A
To be paid now
2 caramels
Option B
To be paid at the end of the experiment
8 caramels
Select A
Continue
Select B


ID: 1234
Practice example
Option A
To be paid now
2 caramels
Option B
To be paid at the end of the experiment
8 caramels
Select A
Continue
Select B


The dates of payment in the first 10 decisions are constant. The only difference between them is the varying amounts in Option B.
Continue


ID: 1234
Decision number 1 out of 40
Option A
To be paid today
$1500
Option B
To be paid in 10 months
$1562.24
Annual Interest Rate
5%
Select A
Continue
Select B


ID: 1234
Decision number 2 out of 40
Option A
To be paid today
$1500
Option B
To be paid in 10 months
$1624
Annual Interest Rate
10%
Select A
Continue
Select B



D. Parameter Values

Table B1 shows the parameters of the lottery choice tasks, and Table B2 shows the parameters of the discounting choice tasks.

In Table B1 the parameters are (1) the decision number, (2) the probability of the high prize in each lottery, (3) the high prize of lottery A, in kroner, (4) the low prize of lottery A, in kroner, (5) the high prize of lottery B, in kroner, (6) the low prize of lottery B, in kroner, (7) the expected value of lottery A, and (8) the expected value of lottery B. The information in columns (7) and (8) was not presented to subjects.

Table B1: Parameters for Lottery Choices

Decision	Probability of High Prize	Lottery A High Prize	Lottery A Low Prize	Lottery B High Prize	Lottery B Low Prize	EV of Lottery A	EV of Lottery B
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1	0.1	1125	750	2000	250	787.5	425
2	0.2	1125	750	2000	250	825	600
3	0.3	1125	750	2000	250	862.5	775
4	0.4	1125	750	2000	250	900	950
5	0.5	1125	750	2000	250	937.5	1125
6	0.6	1125	750	2000	250	975	1300
7	0.7	1125	750	2000	250	1012.5	1475
8	0.8	1125	750	2000	250	1050	1650
9	0.9	1125	750	2000	250	1087.5	1825
10	1	1125	750	2000	250	1125	2000
11	0.1	1000	875	2000	75	887.5	267.5
12	0.2	1000	875	2000	75	900	460

13	0.3	1000	875	2000	75	912.5	652.5
14	0.4	1000	875	2000	75	925	845
15	0.5	1000	875	2000	75	937.5	1037.5
16	0.6	1000	875	2000	75	950	1230
17	0.7	1000	875	2000	75	962.5	1422.5
18	0.8	1000	875	2000	75	975	1615
19	0.9	1000	875	2000	75	987.5	1807.5
20	1	1000	875	2000	75	1000	2000
21	0.1	2000	1600	3850	100	1640	475
22	0.2	2000	1600	3850	100	1680	850
23	0.3	2000	1600	3850	100	1720	1225
24	0.4	2000	1600	3850	100	1760	1600
25	0.5	2000	1600	3850	100	1800	1975
26	0.6	2000	1600	3850	100	1840	2350
27	0.7	2000	1600	3850	100	1880	2725
28	0.8	2000	1600	3850	100	1920	3100
29	0.9	2000	1600	3850	100	1960	3475
30	1	2000	1600	3850	100	2000	3850
31	0.1	2500	1000	4500	50	1125	495
32	0.2	2500	1000	4500	50	1250	940
33	0.3	2500	1000	4500	50	1375	1385
34	0.4	2500	1000	4500	50	1500	1830
35	0.5	2500	1000	4500	50	1625	2275
36	0.6	2500	1000	4500	50	1750	2720
37	0.7	2500	1000	4500	50	1875	3165
38	0.8	2500	1000	4500	50	2000	3610
39	0.9	2500	1000	4500	50	2125	4055
40	1	2500	1000	4500	50	2500	4500

In Table B2 the parameters are (1) the horizon in months, (2) the task number in sequence if this horizon was selected for the subject to make choices over, (3) the principal of 3000 kroner if the subject had the "higher stakes" condition, (4) the principal of 1500 kroner if the subject had the "lower stakes" condition, (5) the annual interest rate presented to the subject if that treatment was applied (this is also the annual effective rate with annual compounding), (6) the delayed payment if the subject had the "higher stakes" condition, (7) the delayed payment if the subject had the "lower stakes" condition, (8) the implied annual effective rate with quarterly compounding, and (9) the implied annual effective rate with daily compounding. The values in columns (8) and (9) were not presented to subjects.

Table B2: Parameters for Discounting Choices

Horizon in months	Task	Principal in high stakes	Principal if low stakes	Annual Interest Rate	Delayed Payment if low stakes	Delayed Payment if high stakes	AER Quarterly	AER Daily
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
0.5	1	3000	1500	5%	3006.10	1503.05	5.1%	5.1%
0.5	2	3000	1500	10%	3011.94	1505.97	10.4%	10.5%
0.5	3	3000	1500	15%	3017.52	1508.76	15.9%	16.2%
0.5	4	3000	1500	20%	3022.88	1511.44	21.6%	22.1%
0.5	5	3000	1500	25%	3028.02	1514.01	27.4%	28.4%
0.5	6	3000	1500	30%	3032.98	1516.49	33.5%	35.0%
0.5	7	3000	1500	35%	3037.75	1518.87	39.9%	41.9%
0.5	8	3000	1500	40%	3042.36	1521.18	46.4%	49.1%
0.5	9	3000	1500	45%	3046.81	1523.40	53.2%	56.8%
0.5	10	3000	1500	50%	3051.11	1525.56	60.2%	64.8%
1	1	3000	1500	5%	3012.22	1506.11	5.1%	5.1%
1	2	3000	1500	10%	3023.92	1511.96	10.4%	10.5%
1	3	3000	1500	15%	3035.14	1517.57	15.9%	16.2%
1	4	3000	1500	20%	3045.93	1522.96	21.6%	22.1%
1	5	3000	1500	25%	3056.31	1528.15	27.4%	28.4%
1	6	3000	1500	30%	3066.31	1533.16	33.5%	35.0%
1	7	3000	1500	35%	3075.97	1537.99	39.9%	41.9%
1	8	3000	1500	40%	3085.31	1542.65	46.4%	49.1%
1	9	3000	1500	45%	3094.34	1547.17	53.2%	56.8%
1	10	3000	1500	50%	3103.10	1551.55	60.2%	64.8%
2	1	3000	1500	5%	3024.49	1512.25	5.1%	5.1%
2	2	3000	1500	10%	3048.04	1524.02	10.4%	10.5%
2	3	3000	1500	15%	3070.70	1535.35	15.9%	16.2%
2	4	3000	1500	20%	3092.56	1546.28	21.6%	22.1%
2	5	3000	1500	25%	3113.67	1556.84	27.4%	28.4%
2	6	3000	1500	30%	3134.09	1567.05	33.5%	35.0%
2	7	3000	1500	35%	3153.87	1576.93	39.9%	41.9%
2	8	3000	1500	40%	3173.04	1586.52	46.4%	49.1%
2	9	3000	1500	45%	3191.65	1595.83	53.2%	56.8%
2	10	3000	1500	50%	3209.74	1604.87	60.2%	64.8%
3	1	3000	1500	5%	3036.82	1518.41	5.1%	5.1%
3	2	3000	1500	10%	3072.34	1536.17	10.4%	10.5%
3	3	3000	1500	15%	3106.67	1553.34	15.9%	16.2%
3	4	3000	1500	20%	3139.91	1569.95	21.6%	22.1%
3	5	3000	1500	25%	3172.11	1586.06	27.4%	28.4%
3	6	3000	1500	30%	3203.37	1601.68	33.5%	35.0%
3	7	3000	1500	35%	3233.74	1616.87	39.9%	41.9%

3	8	3000	1500	40%	3263.27	1631.64	46.4%	49.1%
3	9	3000	1500	45%	3292.03	1646.01	53.2%	56.8%
3	10	3000	1500	50%	3320.05	1660.02	60.2%	64.8%
4	1	3000	1500	5%	3049.19	1524.59	5.1%	5.1%
4	2	3000	1500	10%	3096.84	1548.42	10.4%	10.5%
4	3	3000	1500	15%	3143.07	1571.53	15.9%	16.2%
4	4	3000	1500	20%	3187.98	1593.99	21.6%	22.1%
4	5	3000	1500	25%	3231.65	1615.83	27.4%	28.4%
4	6	3000	1500	30%	3274.18	1637.09	33.5%	35.0%
4	7	3000	1500	35%	3315.63	1657.81	39.9%	41.9%
4	8	3000	1500	40%	3356.07	1678.03	46.4%	49.1%
4	9	3000	1500	45%	3395.55	1697.78	53.2%	56.8%
4	10	3000	1500	50%	3434.14	1717.07	60.2%	64.8%
5	1	3000	1500	5%	3061.61	1530.81	5.1%	5.1%
5	2	3000	1500	10%	3121.53	1560.77	10.4%	10.5%
5	3	3000	1500	15%	3179.89	1589.94	15.9%	16.2%
5	4	3000	1500	20%	3236.78	1618.39	21.6%	22.1%
5	5	3000	1500	25%	3292.31	1646.15	27.4%	28.4%
5	6	3000	1500	30%	3346.55	1673.28	33.5%	35.0%
5	7	3000	1500	35%	3399.59	1699.80	39.9%	41.9%
5	8	3000	1500	40%	3451.50	1725.75	46.4%	49.1%
5	9	3000	1500	45%	3502.34	1751.17	53.2%	56.8%
5	10	3000	1500	50%	3552.16	1776.08	60.2%	64.8%
6	1	3000	1500	5%	3074.09	1537.04	5.1%	5.1%
6	2	3000	1500	10%	3146.43	1573.21	10.4%	10.5%
6	3	3000	1500	15%	3217.14	1608.57	15.9%	16.2%
6	4	3000	1500	20%	3286.34	1643.17	21.6%	22.1%
6	5	3000	1500	25%	3354.10	1677.05	27.4%	28.4%
6	6	3000	1500	30%	3420.53	1710.26	33.5%	35.0%
6	7	3000	1500	35%	3485.69	1742.84	39.9%	41.9%
6	8	3000	1500	40%	3549.65	1774.82	46.4%	49.1%
6	9	3000	1500	45%	3612.48	1806.24	53.2%	56.8%
6	10	3000	1500	50%	3674.23	1837.12	60.2%	64.8%
7	1	3000	1500	5%	3086.61	1543.30	5.1%	5.1%
7	2	3000	1500	10%	3171.52	1585.76	10.4%	10.5%
7	3	3000	1500	15%	3254.83	1627.42	15.9%	16.2%
7	4	3000	1500	20%	3336.65	1668.32	21.6%	22.1%
7	5	3000	1500	25%	3417.06	1708.53	27.4%	28.4%
7	6	3000	1500	30%	3496.14	1748.07	33.5%	35.0%
7	7	3000	1500	35%	3573.96	1786.98	39.9%	41.9%
7	8	3000	1500	40%	3650.59	1825.29	46.4%	49.1%
7	9	3000	1500	45%	3726.08	1863.04	53.2%	56.8%
7	10	3000	1500	50%	3800.50	1900.25	60.2%	64.8%

8	1	3000	1500	5%	3099.18	1549.59	5.1%	5.1%
8	2	3000	1500	10%	3196.81	1598.40	10.4%	10.5%
8	3	3000	1500	15%	3292.96	1646.48	15.9%	16.2%
8	4	3000	1500	20%	3387.73	1693.86	21.6%	22.1%
8	5	3000	1500	25%	3481.19	1740.60	27.4%	28.4%
8	6	3000	1500	30%	3573.42	1786.71	33.5%	35.0%
8	7	3000	1500	35%	3664.46	1832.23	39.9%	41.9%
8	8	3000	1500	40%	3754.39	1877.20	46.4%	49.1%
8	9	3000	1500	45%	3843.26	1921.63	53.2%	56.8%
8	10	3000	1500	50%	3931.11	1965.56	60.2%	64.8%
9	1	3000	1500	5%	3111.81	1555.91	5.1%	5.1%
9	2	3000	1500	10%	3222.30	1611.15	10.4%	10.5%
9	3	3000	1500	15%	3331.54	1665.77	15.9%	16.2%
9	4	3000	1500	20%	3439.59	1719.80	21.6%	22.1%
9	5	3000	1500	25%	3546.53	1773.27	27.4%	28.4%
9	6	3000	1500	30%	3652.40	1826.20	33.5%	35.0%
9	7	3000	1500	35%	3757.26	1878.63	39.9%	41.9%
9	8	3000	1500	40%	3861.16	1930.58	46.4%	49.1%
9	9	3000	1500	45%	3964.12	1982.06	53.2%	56.8%
9	10	3000	1500	50%	4066.21	2033.10	60.2%	64.8%
11	1	3000	1500	5%	3137.22	1568.61	5.1%	5.1%
11	2	3000	1500	10%	3273.89	1636.95	10.4%	10.5%
11	3	3000	1500	15%	3410.05	1705.03	15.9%	16.2%
11	4	3000	1500	20%	3545.72	1772.86	21.6%	22.1%
11	5	3000	1500	25%	3680.91	1840.46	27.4%	28.4%
11	6	3000	1500	30%	3815.66	1907.83	33.5%	35.0%
11	7	3000	1500	35%	3949.97	1974.99	39.9%	41.9%
11	8	3000	1500	40%	4083.87	2041.94	46.4%	49.1%
11	9	3000	1500	45%	4217.37	2108.69	53.2%	56.8%
11	10	3000	1500	50%	4350.49	2175.25	60.2%	64.8%
12	1	3000	1500	5%	3150	1575	5.1%	5.1%
12	2	3000	1500	10%	3300	1650	10.4%	10.5%
12	3	3000	1500	15%	3450	1725	15.9%	16.2%
12	4	3000	1500	20%	3600	1800	21.6%	22.1%
12	5	3000	1500	25%	3750	1875	27.4%	28.4%
12	6	3000	1500	30%	3900	1950	33.5%	35.0%
12	7	3000	1500	35%	4050	2025	39.9%	41.9%
12	8	3000	1500	40%	4200	2100	46.4%	49.1%
12	9	3000	1500	45%	4350	2175	53.2%	56.8%
12	10	3000	1500	50%	4500	2250	60.2%	64.8%

Appendix C: Additional Literature Review (NOT FOR PUBLICATION)

Ainslie and Haendel [1983; p.131-133] report experiments with 18 patients in a substance abuse program that made 66 choices over several weeks. Each subject earned a certain amount of money in an unrelated task during the week, ranging from \$2 up to \$10; call that \$x. They were given a choice between receiving that \$x in 7 days, or receiving \$1.25x in 10 days. Then, on the 7th day, they were given a choice between receiving \$x on that day or receiving \$1.25x in 3 days. It is implied that this second choice was for the same \$x, and not an additional choice, so subjects were allowed to change their minds on the 7th day. Observed behavior was generally consistent with exponential discounting for the majority of choices: 35% of the choices were consistently for the earlier option, and 27% of the choices were consistently for the later option. On the other hand, one-third were consistent with hyperbolic or quasi-hyperbolic preferences, and entailed a shift from the later option to the sooner option. Thus there is, overall, evidence in favor of non-constant discounting, but for a minority of the observed choices.[19] It is not possible to draw any inferences about average discount rates from these data.

Holcomb and Nelson [1992] is an important study that is reviewed in the text. We collated the data reported in the study into a format that is amenable to statistical analysis, and report those data in Table C1 below. The annual rate calculated in Table C1 uses daily compounding, consistent with our other calculations. Table C2 then reports the results of estimating a "grouped" logit estimation on the fraction of the 101 subjects that delayed. The results of the estimation are listed, and then the marginal effect of each covariate on the expected number of subjects to delay. Since the population size is 101, these marginal effects

[19] They also report (p.133) a small follow-up experiment with 5 subjects and a front end delay of two weeks. In that case 4 of the 5 choices entailed a switch from the later payment to the earlier payment.

can be interpreted as the effect of the covariate on the percentage of the sample that delayed. Responses to the "dummy" tasks that offered a 0% interest rate are not included in this analysis (nor do Holcomb and Nelson [1992] report detailed data for them).

Raineri and Rachlin [1993] examined behavior in a series of experiments involving hypothetical choices. Their Experiment 1 is the most relevant for our purposes; the others involved variations in the duration of consumption of the income flows, and the use of non-monetary rewards that were also hypothetical. In Experiment 1 120 students participated as a requirement in an undergraduate psychology course. Each choice task involved a fixed amount y in the future, and an alternative, smaller amount x now. The value of x was varied using the "up-down staircase" or "titration" method: an initial value of x is shown, and if the subject picks x the next alternative x' is lower than x; otherwise, it is higher than x but still lower than y. If these were for real rewards, and the subjects inferred the dependence of future choice options on current choices, then there is an obvious incentive compatibility problem with this procedure; sadly, this does not stop it being used by economists, as reviewed by Harrison and Rutström [2008; §1.5]. Horizons of 1 month, 6 months, 1, 5, 10, 25 and 50 years were used; the amount y varying from \$100 to \$10000 and up to \$1 million; and increasing and decreasing titrations employed for each task. Results are reported descriptively. For each titration choice a simple average of the two values at which the subject switched choice are taken, averages taken of the ascending and descending titration for that subject and choice, and then the median taken across all subjects for each condition. The main results can be illustrated by considering the three shortest horizons. For the \$100 amount the daily-compounded discount rate implied by this statistic is 127%, 71% and 54%, respectively, for the 1-month, 6-month and 1-year horizons; for the \$10000 amount the rates are 12%, 32% and 29%, respectively; and for the \$1 million amount the rates are 12%, 8% and 11%, respectively. For the longest horizons, which are very long indeed, there is no difference in elicited discount rates. Thus we do see a magnitude effect for the smallest amount compared to the two higher amounts, and mixed evidence that favors the magnitude effect for the two higher amounts.

Shelley [1993] replicates and extends the design of Benzion, Rapoport and Yagil [1989]. She poses 96 hypothetical fill-in-the-blank questions to 88 accounting students, and evaluates responses from 74 that completed the questionnaire "consistently." She varied the sign of the monetary amounts (positive or negative), the linguistic frame employed to motivate the context (delay, expedited, and neutral), the horizon (6 months, and 1, 2 and 4 years), and of course the amount ($40, $200, $1000 or $5000). Detailed results are not presented, and the only inferences one can draw about the magnitude effect come from "eyeballing" her Figure 5 (p. 812) to infer discount rates for the amounts and frames; hence these estimates are pooled over horizon. For the neutral frame the discount rates are 24%, 15%, 14% and 14% for each amount in increasing order; for the delay frame the rates are 17%, 14%, 15% and 16%; and for the expedite frame the rates are 17%, 17%, 15% and 15%, respectively. Apart from one cell, the neutral frame and the $40 amount, there is no evidence of a clear magnitude effect in these responses.

Green, Fristoe and Myerson [1994] examine hypothetical choices by 24 undergraduate students that volunteered for the task in return for a $5 flat fee. Their design used a dynamic sequence of questions, explained below, spanning horizons of 1 week, 1 month and 3 months, principals of $20, $100 and $500, and front end delays of 0 days, 1 week, 2 weeks, 1 month and 3 months. The $20 principal was always paired with a $50 later payment; the $100 principal with a $250 later payment; and the $500 principal with a $1250 later payment. Each pair of money amounts were used in a series of questions that had four stages. The initial binary choice might have been between $20 now and $50 in one week, and every subject expected to select the later option. Then the sequences of stages was as follows:

1. a series of binary choice questions in which the delay to the receipt of the later amount was increased steadily until the sooner option was picked once (e.g., $20 now or $50 in one month);
2. the addition of a small and increasing front end delay on the final pair from stage 1 until the subject switched back to the later option twice in a row (e.g., $20 in one week or $50 in 3 months and one week);
3. a single binary choice "check" in which the front end delay on the final pair from stage 2 was removed (the subject should switch back to the sooner option, given the response on the final pair from stage 1); and
4. the addition of a small and increasing front end delay on the pair used in stage 3 until the subject switched back to the later option once.

This process was then repeated for each of the pairs of amounts. The data are not reported in terms of summary statistics of discount rates or discount factors, and no clear conclusions drawn about the effect of magnitude. But one can draw inferences from quadratic polynomial regression equations estimated to fit the observed delays between amounts that were needed to make them equivalent (their Table 1, p. 387). If we assume no front end delay then the intercept terms on these estimated equations tells us the number of weeks, on average, that were needed to make the current amount equivalent in present value to the later amount. This $20 now is equal on average to $50 with a delay of 44.1 weeks, $100 now is equal on average to $250 with a delay of 61.9 weeks, and $500 now is equal on average to $1250 with a delay of 146.0 weeks. These convert into daily-compounded annual rates of 100%, 71% and 30%, respectively. Without any knowledge of the noise associated with these estimates, there does indeed appear to be a magnitude effect as payoffs are scaled up by 5 or 25.

Green, Fry and Myerson [1994] employed hypothetical choices and a titration method to ascertain the amount of money now that would be equivalent to a fixed amount in the future. Focusing on their data with adults, the fixed amounts were $1,000 and $10,000 and the horizons considered were 1 week, 1 and 6 months, 1, 3, 5, 10 and 25 years. The data are not reported in terms of discount rates or discount factors, but there are detailed coefficient estimates of a generalized hyperbolic specification that can be used to infer these (their Table 1, p. 35). For the 12 younger adults evaluated, discount rates were 63%, 54%, 46%, 32% and 25% for the delayed amount of $1,000 and each horizon; for the $10,000 delayed amount the implied discount rates were 40%, 34%, 29%, 19% and 15%. Although there is evidence of a magnitude effect in these point estimates, there is no way to evaluate sampling errors for these values. For the 12 older adults evaluated, discount rates were 12%, 12%, 12%, 12% and 11% for the delayed amount of $1000 and each horizon; for the $10000 delayed amount the implied discount rates were 8%, 8%, 8%, 7% and 7%. Thus there is no evidence of a quantitatively large magnitude effect, nor incidentally any evidence of declining discount rates conditional on the magnitude of the delayed amount. Again, there is no means of ascertaining statistical significance of these results from the reported point estimates.

Kirby and Maraković [1995] used a first-price sealed-*offer* auction between 3 subjects to elicit the present value of a future amount, and acknowledge that an optimal (risk-neutral) bid would be *above* the true valuation (just as an optimal bid for a risk-neutral agent in a first-price sealed-*bid* auction is *below* the true valuation). They also conducted auctions with only 3 bidders, which makes the optimal overstatement more severe than if the auction were for many more bidders: as the number of bidders increase the mis-statement decreases quite rapidly. In principle one can infer the true valuations from observed bids, but only if one estimates the risk attitudes of bidders and assumes some symmetric Bayesian Nash Equilibrium. Furthermore, they deceived subjects and actually had them bid against simulated opponents.

Chapman and Elstein [1995] examine discounting behavior in hypothetical tasks in which the choices are framed as delays in receipt of lottery prize earnings (or health outcomes, ignored here). All subjects were psychology undergraduates fulfilling a course credit requirement. In experiment #1 70 subjects answered 48 questions in which they were asked to state the delayed amount that would be the same as the amount now that was presented to them. The principals were $200, $1000, $5000 and $25000, and the horizons were 6 months, 1, 2 and 4 years. Geometric averages of annualized discount rates can be eyeballed from Figure 1 (p. 378) and exhibit a magnitude effect for the lower amounts. For the 6 month horizon the geometric average discount rate for the $200 principal was roughly 400%, but was only 210% for the $1000 principal and 75% for the two highest principals. The effect diminished for the 1 year horizon, to be roughly 200% for the $200 principal and 125% for the $1000 principal. In experiment #2 similar procedures were used, with principals of $500, $1000, $2060 and $4000 and horizons of 1, 3, 6 and 12 years. Again, geometric means can be eyeballed from Figure 2 (p. 382), and show a smaller magnitude effect than experiment #1. Discount rates for the $500 principal were roughly 150% for the 1-year horizon, 125% for the $1000 principal, and 50% for the higher principals. A sharp decay set in after the 3 year horizon. It is not possible to infer sampling error from these Figures.

Kirby, Petry and Bickel [1999] is an important study that is reviewed in the text. We collated the data reported in their study into a format that is amenable to statistical analysis, and report those data in Table C3 below. We focus solely on their "controls," since the other subjects were known heroin addicts. Table C4 then reports the results of estimating a "grouped" logit estimation on the fraction of the 60 subjects that delayed. The results of the estimation are listed, and then the marginal effect of each covariate on the expected number of subjects to delay. Since the population size is 60, these marginal effects can be scaled up by just less than 2 and then interpreted as the effect of the covariate on the percentage of the sample that delayed.

Chapman and Winquist [1998] study discounting behavior in the context of different frames about the task. In one frame the subjects are told that they have won a lottery, and can defer payment of a larger prize for 3 months; in the other frame they are told that they have received a speeding ticket and can defer payment of a larger fine for 3 months.[20] The subject is asked to fill-in-the-blank for the later amount. The principal amounts used in this case are much smaller than typically used in psychology experiments: $5, $7.50, $10, $15, $20, $30, $40, $60, $80 and $160. The discount rates implied by these choices are not displayed, but can be roughly inferred from Figure 1 (p. 121) and are very, very high. Consider the lottery frame. For the amounts of $20 and less the rates are in excess of 2000% on an annual basis, declining from roughly 5000% at $5 to "just under" 2000% at $30. Even at $160, the rates are in excess of 1000%. Although the implied rates for the speeding ticket frame are lower, all lie between 700% or so and 100% or so, declining with magnitude of the principal. Quite apart from the decline in elicited rates with magnitude, the incredible levels of these rates are an important signal to what might actually be going on in many of these experiments, discussed in §5.

Green, Myerson and McFadden [1997] followed the same procedures as Green, Fry and Myerson [1994]. They used 24 volunteer students, with a fixed delayed amounts and a range of options available now. The fixed delayed amount was either $100, $2000, $25000 or $100000, and the horizons considered were 3 months, 6 months, 1, 3, 5, 10 and 20 years. There were 24 amounts available now, with discount factors between 1% and 99%, with smaller increments closer to either extreme. Each subject either received the sooner options

[20] There are additional tipping frames presented and compared to these standard frames.

in ascending or descending order, and the binary choice questions were terminated when a switch was observed. All choices were hypothetical. Their Table 1 (p.717) reports average discount factors, from which one can infer daily-compounded discount rates. For the $100 principal, average discount rates were 75%, 50%, 39%, 17%, 18%, 10% and 8% for each horizon, respectively; for the $2000 principal the rates were 21%, 19%, 12%, 10%, 12%, 10% and 7%, respectively. So there is a significant decline in discount rates with this change in magnitude, at least for horizons up to 5 years. Of course, no sampling errors are reported in these data. For the $25000 principal the rates were 12%, 10%, 9%, 8%, 8%, 5% and 6%, respectively, so it is only for the two shortest horizons that one sees a continuing magnitude effect.

Du, Green and Myerson [2002] examined the hypothetical discount behavior of 79 Chinese, Japanese and American graduate students. Horizons were 1, 3 and 9 months, and 2, 5, 10 and 20 years; the delayed, fixed amounts were either $200 or $10000. Titration was employed to find the amount to be received now that was equal to the fixed, delayed amount. The first iteration was to offer 50% of the amount, and then adjusted five times based on observed responses in the previous choice task. In principle one could eyeball their Figure 1 (p. 486) and guess at discount factors for different horizons, but the scale of the graphs does not allow any useful precision in this exercise.

Estle, Green, Myerson and Holt [2007] applied titration methods for hypothetical discounting tasks over delayed payments of $40 or $100. Horizons were 1 week, 1 month, 6 months, 1 year and 3 years. The 47 subjects were students completing a course credit. A generalized hyperbolic was fit to the observed data, and the area under the "discount factor curve" used as a measure of the degree of discounting. They report an ANOVA analysis of the effect of magnitude, and that it shows a significant effect. However, it is possible to infer the proximate size of discount rates by eyeballing their Figure 1 (p. 60) to ascertain the discount factor, and then deriving the implied discount rate. Assuming discount factors of 0.98, 0.95, 0.78 and 0.65 for the $40 amount and the first four horizons, and discount factors of 0.99, 0.97, 0.85 and 0.75 for the $100 amount and the same horizons, one can infer annual discount rates with daily compounding of 735%, 62%, 50% and 43% for the $40 amount, and 364%, 37%, 33% and 29% for the $100 amount. Rough as these inferences are, and absent any standard errors, the magnitude effect pattern is clear.

Scholten and Read [2010] administered a web-based survey to 196 individuals, offering them a choice between $250 ($100) [$50] {$25} in 19 months or $295 ($118) [$59] {29.50} in 22 months, or between $250 ($100) [$50] {$25} in 16 months or $340 ($136) [$68] {34} in 22 months. The first set of choices therefore offered a constant 18% nominal rate of return, and the second set of choices offered a constant 36% nominal rate of return. The fraction of subjects taking the sooner option was 0.29 (0.60) [0.62] {0.68} for the first set, and 0.13 (0.38) [0.22] {0.47} for the second set. All choices were hypothetical. The results suggest a magnitude effect as one moves from the $250 principal to the lower amounts, but very little effect for the lower three principal values.

Table C1: Data from Holcomb and Nelson [1992]

Task	FED in Days	Principal	Horizon in Days	Delayed Amount	Fraction Delaying	Daily Rate (%)	Annual Rate (%)
1	0	$5	1	$5.14	0.225	2.8	1008
2	0	$17	7	$18.86	0.423	1.49	538
3	1	$5	1	$5.07	0.113	1.4	504
4	7	$5	7	$5.54	0.282	1.48	531
5	0	$17	14	$20.94	0.704	1.5	540
6	1	$5	7	$5.54	0.268	1.48	531
7	0	$5	14	$6.15	0.408	1.49	536
8	0	$17	7	$20.90	0.789	2.99	1078
9	1	$5	1	$5.14	0.197	2.8	1008
10	1	$17	14	$25.71	0.915	3	1079
11	7	$17	7	$18.86	0.437	1.49	538
12	0	$5	14	$7.56	0.563	3	1079
13	1	$17	7	$18.86	0.408	1.49	538
14	0	$5	7	$6.14	0.409	2.98	1072
15	0	$17	1	$17.26	0.324	1.53	551
16	1	$17	1	$17.51	0.451	3	1080
17	7	$17	14	$20.90	0.803	1.49	535
18	1	$5	14	$6.15	0.281	1.49	536
19	0	$17	14	$25.71	0.873	3	1079
20	1	$17	14	$20.94	0.563	1.5	540
21	1	$5	7	$6.14	0.395	2.98	1072
22	1	$17	1	$17.26	0.31	1.53	551
23	1	$5	14	$7.56	0.563	3	1079
24	0	$5	7	$5.54	0.211	1.48	531
25	7	$5	14	$6.14	0.437	1.48	532
26	0	$5	1	$5.07	0.183	1.4	504
27	0	$17	1	$17.51	0.535	3	1080
28	0	$17	7	$20.90	0.789	2.99	1078
29	7	$17	14	$25.71	0.862	3	1079
30	7	$17	1	$17.26	0.31	1.53	551
31	7	$17	14	$20.94	0.655	1.5	540
32	0	$5	1	$5.00	0	0	0
33	0	$5	7	$5.00	0	0	0
34	0	$17	1	$17.00	0	0	0
35	0	$17	7	$17.00	0	0	0
36	1	$5	7	$5.00	0	0	0
37	7	$5	14	$6.15	0.345	1.49	536
38	7	$5	1	$5.07	0.207	1.4	504
39	7	$5	15	$7.50	0.211	2.74	986
40	7	$5	1	$5.14	0.31	2.8	1008
41	7	$17	1	$17.51	0.558	3	1080

Table C2: Grouped Logistic Analysis of Holcomb and Nelson [1992] Data

```
Logistic regression for grouped data              Number of obs   =       3636
                                                  LR chi2(4)      =     645.92
                                                  Prob > chi2     =     0.0000
Log likelihood = -2180.8345                       Pseudo R2       =     0.1290

------------------------------------------------------------------------------
    _outcome |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
         fed |  -.0023871   .0120324    -0.20   0.843    -.0259702    .0211961
      amount |   .1060896     .00618    17.17   0.000     .0939769    .1182022
     horizon |   3.009402   .2038066    14.77   0.000     2.609949    3.408856
      dailyc |   .0015718   .0001433    10.97   0.000     .0012909    .0018528
       _cons |  -3.373304   .1642249   -20.54   0.000    -3.695179   -3.051429
------------------------------------------------------------------------------

Marginal effects after blogit
      y  = E(cases) (predict)
         =  44.725602
------------------------------------------------------------------------------
variable |      dy/dx    Std. Err.     z    P>|z|  [    95% C.I.   ]      X
---------+--------------------------------------------------------------------
     fed |  -.0594851      .29985   -0.20   0.843  -.647175  .528204   2.63889
  amount |   2.643738      .15365   17.21   0.000   2.34259  2.94488        11
 horizon |    74.9939     5.07048   14.79   0.000   65.0559  84.9319   .258333
  dailyc |   .0391694      .00357   10.97   0.000   .032171  .046167   766.945
------------------------------------------------------------------------------
```

Table C3: Data from Kirby, Petry and Bickel [1999; Table 3]

Task Order	Principal	Delayed Amount	Delay in Days	Percent of Addicts Delaying	Percent of Controls Delaying
13	$34	$35	186	0	0
1	$54	$55	117	4	2
9	$78	$80	162	4	2
20	$28	$30	179	2	0
6	$47	$50	160	0	2
17	$80	$85	157	0	0
26	$22	$25	136	4	0
24	$54	$60	111	0	5
12	$67	$75	119	5	3
22	$25	$30	80	7	3
16	$49	$60	89	9	10
15	$69	$85	91	7	20
3	$19	$25	53	21	22
10	$40	$55	62	13	20
2	$55	$75	61	39	33
18	$24	$35	29	18	30
21	$34	$50	30	34	52
25	$54	$80	30	41	67
5	$14	$25	19	43	72
14	$27	$50	21	55	85
23	$41	$75	20	68	90
7	$15	$35	13	68	90
8	$25	$60	14	82	93
19	$33	$80	14	89	100
11	$11	$30	7	93	100
27	$20	$55	7	91	98
4	$31	$85	7	89	100

Table C4: Grouped Logistic Analysis of Kirby, Petry and Bickel [1999; Table 3] Data

Controls Only (N=60)

```
Logistic regression for grouped data              Number of obs   =       1620
                                                  LR chi2(3)      =    1296.55
                                                  Prob > chi2     =     0.0000
Log likelihood = -446.30914                       Pseudo R2       =     0.5923

------------------------------------------------------------------------------
    _outcome |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
   principal |   .0387394   .0068351     5.67   0.000     .0253428     .052136
     horizon |  -.0436126   .0041953   -10.40   0.000    -.0518352   -.0353899
      dailyc |   .0012892   .0001896     6.80   0.000     .0009177    .0016608
       _cons |  -.4335072   .3226488    -1.34   0.179    -1.065887    .1988728
------------------------------------------------------------------------------

. mfx compute

Marginal effects after blogit
      y  = E(cases) (predict)
         =  19.796768
------------------------------------------------------------------------------
variable |      dy/dx    Std. Err.     z    P>|z|  [    95% C.I.   ]      X
---------+--------------------------------------------------------------------
princi~l |   .5138741      .09088    5.65   0.000   .335746  .692003   38.8889
 horizon |  -.5785163      .03425  -16.89   0.000  -.645637 -.511396   73.1111
  dailyc |   .0171015      .00347    4.92   0.000   .010294  .023909   1091.43
------------------------------------------------------------------------------
```