Eliciting Risk and Time Preferences

Using Field Experiments: Some Methodological Issues

by

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Abstract. We design experiments to jointly elicit risk and time preferences for the adult Danish population. The experimental procedures build on laboratory experiments that have used traditional subject pools. The field experiments utilize field sampling designs that we developed, and procedures that were chosen to be relatively transparent in the field with non-standard subject pools. Our overall design was also intended to be a general template for such field experiments in other countries. We examine the characterization of risk over a wider domain for each subject than previous experiments, allowing more precise estimates of risk attitudes. We also examine individual discount rates over six time horizons, as the first stage in a panel experiment in which we revisit subjects to test consistency and stability of responses over time. Risk and time preferences are heterogeneous, varying by observable individual characteristics. On a methodological level, we implement a refinement of existing procedures which elicits much more precise estimates, and also mitigates framing effects.

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1. Introduction

Utility functions are characterized in three dimensions, reflecting preferences over goods, time and uncertainty. The utility function conventionally characterizes preferences over goods defined by a time period and a state of nature, preferences over the temporal allocation of goods, and preferences over outcomes as realizations of uncertain states of nature. This broad characterization includes most alternatives to conventional expected utility theory.[1] We focus on the utility function for money, collapsing the choice over goods down to just one good so that there is no choice option with respect to goods. We use controlled experiments with field subjects in Denmark to elicit individuals' risk and time preferences.

Information on risk and time preferences is of obvious value for policy, theory and empirical analysis generally. Policy applications include cost-benefit analysis of government programs, which often require welfare calculations to be made over uncertain projects whose impacts are spread over time. Theoretical applications include tests of propositions about the relationship between risk and time preferences and the consistency of time preferences.[2] Empirical applications include the study of savings behavior, insurance decisions, and asset prices.[3]

We evaluate a new field methodology developed to elicit both time and risk preferences from the same respondents. We use relatively simple experimental procedures that have evolved in the recent literature to study each. Indeed, all of the *basic* procedures we use have been applied and evaluated in laboratory experiments, albeit separately for the elicitation of risk and time preferences. This is deliberate, and illustrates the complementarity of lab and field experiments. These experimental procedures are presented in section 2: we build on the risk aversion experiments of

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[1] The major exception for present purposes is the approach of Kreps and Porteus [1978], which allows for preferences over the timing of the resolution of uncertainty.

[2] Deaton [1992; p.20/21] discusses why time consistency is central to debates over the restrictiveness of intertemporal additivity and conventional expected utility theory, with direct implications for the specification of life-cycle models and capital asset pricing models.

[3] It is central to the general understanding of savings behavior (e.g., Hall [1988]), the analysis of insurance decisions by extremely poor households in developing countries (e.g., Townsend [1994]), and the behavior of asset prices over time (e.g., Hansen and Singleton [1983]).

Holt and Laury [2002] (HL) and the discount rate experiments of Coller and Williams [1999] (CW), Harrison, Lau and Williams [2002] (HLW) and Coller, Harrison and Rutström [2003] (CHR). Our design is implemented in the field in Denmark, to obtain a sample that offers a wider range of individual socio-demographic characteristics than usually found in subject pools recruited in colleges, as well as a sample that can be used to make inferences about the preferences of the adult population of Denmark. Our experiments are "artefactual field experiments" in the terminology of Harrison and List [2004], since we essentially take lab experiments to field subjects.

Many of the features of our design were selected to make the experimental task as transparent to the field subjects as possible, and we devote considerable attention to those design issues in section 3. Our goal is also to propose a general experimental design that can be applied, with obvious modifications, in other countries. Results from the first phase of our experiments[4] are presented in section 4, and conclusions drawn in section 5. We focus primarily on the methodological issues but also present some general findings.

We find that adult Danes are generally risk averse over the domains considered here, and that very few exhibit any risk-loving behavior. We estimate average individual discount rates to be 23.8%. Discount rates for the shortest horizon of one month do appear to be higher than for the longer horizons, extending up to 24 months, but only by about 3 to 5 percentage points. Risk and time preferences are heterogeneous, varying by observable individual characteristics. On a methodological level, we implement a refinement of existing procedures which elicits much more precise estimates, and also mitigates framing effects.

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[4] Phase 1 of the project involved the field experiments described here, to elicit information on risk and time preferences. Phase 2 will involve complementary laboratory experiments to assess variations in the experimental design that would be too expensive to evaluate in the field. Phase 3 will involve re-visiting the subject from Phase 1, to generate a panel of data on elicited preferences.

2. General Experimental Procedures for Eliciting Risk and Time Preferences

Many of the experimental procedures employed in this study have been used previously. We do offer important modifications to these procedures that lead to an increase in the precision of the values elicited. In this section we summarize these procedures and discuss the important factors that lead to specific design choices in our field experiments.

A. Risk Preferences: Measuring Risk Aversion

Holt and Laury [2002] (HL) devise a simple experimental measure for risk aversion using a multiple price list (MPL) design.[5] Each subject is presented with a choice between two lotteries, which we can call A or B. Table 1 illustrates the basic payoff matrix presented to subjects. The first row shows that lottery A offered a 10% chance of receiving $2 and a 90% chance of receiving $1.60. The expected value of this lottery, EV^A, is shown in the third-last column as $1.64, although the EV columns were not presented to subjects. Similarly, lottery B in the first row has chances of payoffs of $3.85 and $0.10, for an expected value of $0.48. Thus the two lotteries have a relatively large difference in expected values, in this case $1.17. As one proceeds down the matrix, the expected value of both lotteries increases, but the expected value of lottery B becomes greater relative to the expected value of lottery A.

TABLE 1 ABOUT HERE

The subject chooses A or B in each row, and one row is later selected at random for payout for that subject. The logic behind this test for risk aversion is that only risk-loving subjects would

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[5] The MPL appears to have been first used in pricing experiments by Kahneman, Knetsch and Thaler [1990], and has been adopted in recent discount rate experiments by Coller and Williams [1999]. It has a longer history in the elicitation of hypothetical valuation responses in "contingent valuation" survey settings, as discussed by Mitchell and Carson [1989; p. 100, fn. 14]. The test devised by HL is closely related to one developed by Murnighan, Roth and Schoumaker [1988] to measure the degree of risk aversion of subjects in bargaining experiments.

take lottery B in the first row, and only risk-averse subjects would take lottery A in the second last row. Arguably, the last row is simply a test that the subject understood the instructions, and has no relevance for risk aversion at all. A risk neutral subject should switch from choosing A to B when the EV of each is about the same, so a risk-neutral subject would choose A for the first four rows and B thereafter.

These data may be analyzed using a variety of statistical models. Each subject made 10 responses. The responses can be reduced to a scalar if one looks at the *lowest* row in Table 1 at which the subject "switched" over from lottery A to lottery B[6]. This reduces the response to a scalar for each subject and task, but a scalar that takes on integer values between 0 and 10. Alternatively, one could study the effects of experimental conditions in terms of the constant relative risk aversion (CRRA) characterization,[7] employing an interval regression model. The dependent variable is the CRRA interval that subjects implicitly choose when they switch from lottery A to lottery B. For each row in Table 1, one can calculate the implied bounds on the CRRA coefficient, and these are in fact reported by HL [2002; Table 3]. These intervals are shown in the final column of Table 1. Thus, for example, a subject that made 5 safe choices and then switched to the risky alternatives would have revealed a CRRA interval between 0.14 and 0.41, and a subject that made 7 safe choices would have revealed a CRRA interval between 0.68 and 0.97, and so on.[8] Alternatively, given enough choice observations on each subject it is possible to estimate a flexible, individual utility function following

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[6] Some subjects switched several times, but the minimum switch point is always well-defined. It turns out not to make much difference empirically how one handles these "multiple switch" subjects. We view them as expressing indifference, as explained later when we define the interval response used in our statistical analysis.

[7] The specific functional form used is $U(m) = (m^{1-r})/(1-r)$, where r is the CRRA coefficient. With this parameterization, $r = 0$ denotes risk neutral behavior, $r > 0$ denotes risk aversion, and $r < 0$ denotes risk loving. When $r = 1$, $U(m) = \ln(m)$.

[8] HL also utilize a variant of the Expo-Power utility function proposed by Saha [1993], which is more general than the CRRA characterization. The Expo-Power function is defined as $u(y) = [1-\exp(-\alpha y^{1-r})]/\alpha$, where y is income and α and r are parameters to be estimated. Relative risk aversion (RRA) is then $r + \alpha(1-r)y^{1-r}$. So RRA varies with income if $\alpha \neq 0$. This function nests CRRA (as α tends to 0) and CARA (as r tends to 0). HL estimate this function assuming that every subject has the same risk preference. They rely on a "noise parameter" to accommodate the obvious differences in risk choices across subjects, but do not allow risk preferences to vary with observable socio-demographic characteristics as we do later. It is beyond the scope of this exercise to compare alternative specifications of the utility function.

Hey and Orme [1994] (HO). Nevertheless, in order to make the time requirement on the subjects reasonable we limited the number of tasks to four. We are therefore restricted to estimating a utility function for the sample, conditioning on a number of observable characteristics, and to predict the individual's risk attitude from those sample estimates of the parameters of the function. The main problem with this approach is that it requires the assumption that the observed characteristics of the individual adequately characterize the individual's risk attitudes.

In this study we expand the HL design with some simple modifications to allow a richer characterization of the utility function, although we do not go as far as the design in HO. The HL design called for each subject to be given choices over four lottery prizes and for there to be one major scale change for all real payoffs. In our design we give subjects four similar tasks that each vary the four underlying lottery prizes. Hence we have data for the same subject over more than four prizes and can generate better characterizations of their risk attitudes. This allows us to estimate quite flexible functional forms for the utility function, although here we restrict attention to the common CRRA specification. Future research will explore these flexible specifications further.

We undertake four separate risk aversion tasks with each subject, each with different prizes designed so that all 16 prizes span the range of income over which we seek to estimate risk aversion. Ideally, we would have a roughly even span of prizes so that we can evaluate the utility function for the individual at different income levels and know that there were some response at or near that level. The four sets of prizes are as follows, with the two prizes for lottery A listed first and the two prizes for lottery B listed next: (A1: 2000 DKK, 1600 DKK; B1: 3850 DKK, 100 DKK), (A2: 2250 DKK, 1500 DKK; B2: 4000 DKK, 500 DKK), (A3: 2000 DKK, 1750 DKK; B3: 4000 DKK, 150 DKK), and (A4: 2500 DKK, 1000 DKK; B4: 4500 DKK, 50 DKK). At the time of the first phase of the experiments, the exchange rate was approximately 6.55 DKK per U.S. dollar, so these prizes range from approximately $7.65 to $687.

This set of prizes generates an array of possible CRRA values. For example, set 1 generates CRRA intervals at the switch points of -1.71, -0.95, -0.49, -0.14, 0.15, 0.41, 0.68, 0.97 and 1.37. The

other sets generate different CRRA intervals, such that all four sets span 36 distinct CRRA values between -1.84 and 2.21, with roughly 60% of the CRRA values reflecting risk aversion.[9] Any scaling of the prizes that is common within a set will preserve the implied CRRA coefficients, so this design can also be used in laboratory settings with smaller or larger payoffs.

We ask the subject to respond to all four risk aversion tasks and then randomly decide which one to play out. In addition, the large incentives and budget constraints precluded paying all subjects, so each subject is given a 10% chance to actually receive the payment associated with his decision.[10]

B. *Time Preferences: Measuring Individual Discount Rates*

The basic experimental design for eliciting individual discount rates (IDRs) was introduced in CW and expanded in HLW and CHR. The basic question asked of subjects is extremely simple: do you prefer $100 today or $100+$x$ tomorrow, where x is some positive amount? If the subject prefers the $100 today then we can infer that the discount rate is higher than x% per day; otherwise, we can infer that it is x% per day or less.[11] The format of the CW and HLW experiments modified and extended this basic question in six ways, which we retain here.

First, we pose a number of such questions to each individual, each question varying x by some amount. When x is zero we would obviously expect the individual to reject the option of waiting for no rate of return. As we increase x we would expect more individuals to take the future income option. For any given individual, the point at which they switch from choosing the current

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[9] The second set generates CRRA values of -1.45, -0.72, -0.25, 0.13, 0.47, 0.80, 1.16, 1.59 and 2.21; the third set generates values of -1.84, -1.101, -0.52, -0.14, 0.17, 0.46, 0.75, 1.07 and 1.51; and the fourth set generates values of -0.75, -0.32, -0.05, 0.16, 0.34, 0.52, 0.70, 0.91 and 1.20.

[10] HLW and CHR chose one subject at random to receive payment, but the probability of being selected depended on group sizes, which varied slightly. The procedures used here ensure comparability of incentives across subjects in different group sizes by giving each subject the same probability to receive payment..

[11] We assume that the subject does not have access to perfect capital markets, as explained in CW (p.110) and HLW (p.1607ff.). This assumption is plausible, but also subject to checks from responses to the financial questionnaire that CW, HLW and we ask each subject to complete.

income option to taking the future income option provides a bound on their discount rate. That is, if an individual takes the current income option for all x from 0 to 10, then takes the future income option for all x from 11 up to 100, we can infer that his discount rate lies between 10% and 11% for this time interval. The finer the increments in x, the more precisely we will be able to pinpoint the discount rate of the individual.

Second, the experimental task used an MPL format, simultaneously posing several questions with varying values of x. After all questions had been completed by the individual, one of the questions was chosen at random for actual payment. In this way the results from one question do not generate income effects which might influence the answers to other questions. This feature of the design mimics the format used by HL in their risk aversion experiments: in that case the rows reflected different probabilities of each prize, and in this case the rows reflect different annual effective rates of return.[12]

Third, subjects are provided two future income options rather than one "instant income" option and one future income option. For example, they might be offered $100 in one month and $100+$x$ in 7 months, so that we interpret the revealed discount rate as applying to a time horizon of 6 months. This avoids the potential problem of the subject facing extra risk or transactions costs[13] with the future income option, as compared to the "instant" income option. If the delayed option were to involve such additional transactions costs, then the revealed discount rate would include these subjective transactions costs. By having both options entail future income we hold these transactions costs constant.[14]

Fourth, subjects were asked to provide information to help identify what market rates of interest they face. This information was used to allow for the possibility that their responses in the

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[12] We exploit this similarity of format in the design of our computerized interface to subjects, and in the use of trainers in the risk aversion task as a generic substitute for trainers in the discount rate task.
[13] Including the possibility of default by the experimenter.
[14] The importance of this "front end delay" is identified by CW and CHR.

discount rate task are *censored* by market rates.[15]

Fifth, respondents were provided with information on the interest rates implied by the delayed payment option. This is an important control feature if field investments are priced in terms of interest rates. If subjects are attempting to compare the lab investment to their field options, this feature may serve to reduce comparison errors since now both lab and field options are priced in the same metric.[16]

Sixth, while CW examined a 6-month time horizon only, HLW analyzed questions of time-consistent preferences by eliciting discount rates for four time horizons: 6 months, 12 months, 24 months, and 36 months. Some subjects were randomly assigned a single time horizon, while others were asked to state their preferences for each of the four time horizons, allowing for a test of the effect of asking subjects to consider multiple time horizons.

Subjects in the HLW experiments were given payoff tables such as the one illustrated in Table 2. They were told that they must choose between payment Options A and B for each of the 20 payoff alternatives. Option A was 3000 DKK in all sessions. Option B paid 3000 DKK + X DKK, where X ranged from annual rates of return of 2.5% to 50% on the principal of 3000 DKK, compounded quarterly to be consistent with general Danish banking practices on overdraft

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[15] To explain the censoring problem, assume that you value a cold beer at $3, which is to say that if you had to pay $3 for one beer you would. If I ask you whether or not you are willing to pay $2.50 for a *lab* beer, your response to me will depend on whether or not there is a market price of *field* beer (assumed to be the same as the lab beer) lower than $2.50. If the market price of the field beer is $2.00, and you know that you can buy a beer outside the lab at this price, then you would never rationally reveal to me that you would pay $2.50 for my lab beer. In this case we say that your response is censored by the market price (Harrison [1992; p.1432], Harrison, Harstad and Rutström [2004]). CW and HLW discuss procedures for handling censored responses in the context of discount rate elicitation.

[16] CW suggest that behavior in previous studies may be affected by uncontrolled factors other than time preferences that may help explain observed anomalies. They suggest that subjects may attempt to *arbitrage* between lab and field investment opportunities, but may make mistakes in comparing these opportunities because the lab and field investments are "priced" in different terms. Lab investments are priced in *dollar* terms (the difference between the early and later payments), while field investments are priced in terms of annual and effective interest *rates*. A rational subject should never choose to postpone payment in the laboratory at interest rates lower than those she can receive in the external market, for example, but she may make mistakes in converting dollar interest to an interest rate (or vice versa) for the purposes of comparison. The use of hypothetical or small payments is likely to exacerbate this problem because of the cognitive costs associated with the subject's arbitrage problem; at lower stakes subjects are likely to expend less cognitive effort on getting the comparison right.

accounts. The payoff tables provided the annual and annual effective interest rates for each payment option, and the experimental instructions defined these terms by way of example.

TABLE 2 ABOUT HERE

Across all time horizons considered by HLW, payoffs to any one subject could range from 3,000 DKK up to 12,333 DKK. The exchange rate when the HLW experiments were conducted in mid-1997 was approximately 6.7 DKK per US dollar, so this range converts to $450 and $1,840.

We used the multiple-horizon treatment from HLW. From the perspective of the task faced by the subjects, the only variations are that the instrument is now computerized, and subjects are presented with 6 discount rate tasks, corresponding to 6 different time horizons: 1 month, 4 months, 6 months, 12 months, 18 months, and 24 months.

In addition, there are some minor changes in payment procedures. In the HLW experiments, a certificate for future payment was guaranteed by the Social Research Institute (SFI is the abbreviation of the Danish name), which was redeemable on the payment date for an SFI-issued check. In this study, future payments are guaranteed by the Danish Ministry of Economic and Business Affairs, and made by automatic transfer from the Ministry's bank account to the subject's bank account.[17] This payment procedure is similar to a post-dated check, and automatic transfers between bank accounts are a common procedure in Denmark. We conjecture that this feature will reduce transaction costs and credibility issues associated with future payments. Finally, while CW and HLW randomly select a single "Assignee" from the group of subjects in a given session to actually receive the payment associated with his decision, in these new experiments each subject is given a 10% chance to receive actual payment.

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[17] We are grateful to Sydbank for administrative assistance with the money transfers.

C. Related Literature

Although there have been several experimental studies examining inter-temporal allocations under uncertainty,[18] only two address the elicitation of risk and time preferences directly using procedures familiar to experimental economists.

Anderhub, Güth, Gneezy and Sonsino [2001] (AGGS) use the Becker-DeGroot-Marschak (BDM) procedure to elicit certainty equivalents for lotteries with varied payoff dates. They used undergraduate economic students in Israel as subjects. Each subject provided either a buying or a selling price for each of three lotteries that paid out the day of the experiment, two weeks from the day of experiment, and four weeks from the day of the experiment. The lotteries differ only with respect to the timing of payments.[19] One decision was chosen at random to be played out. AGGS find no statistical difference between certainty equivalents across different time horizons. They find a marginally significant negative relationship between the degree of risk aversion and the discount rates implied by the timing of payments.

The differences between the elicitation tasks in our design and that of AGGS reflect a tradeoff between compactness of experimental procedures and transparency of the task required of subjects. While our elicitation mechanism is logically equivalent to the BDM, we believe the binary decisions in the MPL task are less of a cognitive burden for subjects. Moreover, the AGGS design elicits a single value from subjects that reflects both risk and time preferences, while we examine these preferences separately.

Eckel, Johnson and Montmarquette [2002][2005] (EJM) conduct a field study of time and risk preferences. Their subjects are recruited from low income neighborhoods in Montreal. Subjects in these experiments are given 64 "compensated" questions, one of which is chosen at random for payment. Time preferences are elicited by presenting subjects with choices between payoffs that

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[18] For example, Hey and Dardanoni [1988] and Harrison and Morgan [1990].
[19] AGGS are able to effect delayed payments by distributing post-dated checks the day of the experiment, thereby reducing any differences between immediate and delayed payments due to subject expectations regarding the credibility of future payments. This is a very desirable design feature that is not available to us, as Danish banks do not honor date restrictions on checks.

occur at different times. Time horizons for the later payments ranged from 2 days to 28 days, and most early payments had a front end delay of one day, one week, or two weeks. The value for most questions started at approximately $72 CAD, with a few questions presenting values around $26 CAD. The distribution of annual discount rates implied by the questions was lumpy, with values of 10%, 50%, 200% and 380%.

Risk preferences are elicited by presenting subjects with choices between lotteries, where most choices involved a "less risky" lottery that paid a single amount with certainty. The expected value of the lotteries ranged from $40 CAD to $120 CAD. EJM do not find risk attitudes to vary by subject characteristics, though their analysis does not indicate that they controlled for the age of subjects, one of the factors we report as significant later.

EJM also examine time preferences by presenting subjects with a series of questions of the form "Do you prefer $X in one week or $X + $Y in educational expenses to be reimbursed over the next year." All contexts concerned education or retirement. The elicited discount rates are necessarily imprecise with respect to the context given since it was not possible for EJM to ensure that any funds provided were used for the purposes stated in the question.[20] In addition, since the time horizons for the own education questions are unspecified (though constrained to fall within one year of the experiment), it is not possible to calculate a range of discount rates implied by these tradeoffs. The lowest possible discount rate is approximately 29%, implied by the decision to take $600 in 7 years instead of $100 in a week. From there the rates increase to several hundred percent.

The questionnaire design in EJM is equivalent to an MPL in which the values are arranged in random order, rather than according to some monotonic change in the underlying parameters. Although subjects report they had access to credit market instruments, there is no attempt to control for censoring behavior. Four of the 37 time preference choices had no front-end delay. EJM find

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[20] Payments for investment in education of a family member or own retirement were effected by 5-year or 7-year Certificate of Deposit. Payments for own education were given as reimbursements for "admission fees at an educational institution (professional, collegial, or university) or purchases of didactic material (books, software, or others)."

mixed statistical evidence that the presence of a front end delay affects the likelihood of choosing the later payment. Moreover, EJM manipulate the length of the front end delay and find no effect on implied discount rates.

EJM find some socio-economic characteristics that affect time preferences. In the analysis of the small payment questions, women, students, and older people appear to be more patient, and low income individuals appear to be less patient. In the analysis of the large payment questions, when the later payment is for the individual, older individuals and more risk-averse individuals are more likely to take the earlier cash payment. Level of education has a positive impact on the probability of choosing the delayed payment when framed as an investment in education. Subjects with children are more likely to choose the later payment when it is framed as an investment in the education of a family member, but educated males are less likely to do so.

3. New Methodological Issues

A. The Iterative Multiple Price List

The MPL as employed by CW, HLW, CHR and HL has three possible disadvantages. The first is that it only elicits interval responses, rather than "point" valuations. The second is that some subjects switch back and forth between (lottery or payment option) A and B as they move down the MPL, implying that they may be indifferent between the two options. The third is that it could be susceptible to framing effects, as subjects are drawn to the middle of the ordered table irrespective of their true values. We propose extensions of the basic MPL approach to address each of these concerns.

<u>Interval Responses</u>

The problem of interval responses can be addressed in two ways.

The first is simply to use statistical methods that recognize that the response is interval-censored. These methods are an extension of traditional Tobit models, which recognize that a

dependent variable may be right or left censored at some fixed value.[21] Tobit models can be extended to allow for right or left censoring that varies with the subject. A further extension allows each subject's response to be left-censored *and* right-censored, which is just another way of saying that the subject's response is interval-censored. This is the statistical approach used by CW, HLW and CHR for discount rate applications, and Harrison, Johnson, McInnes and Rutström [2003][2005] for risk aversion applications.[22] Since there is some controversy over the ability to elicit precise valuations using point response methods,[23] it could be that the best one can do anyway is elicit interval responses. For now, we remain agnostic on this issue, although the experiments we undertake can help us address the issue empirically.

The second way to address the interval response issue is to extend the MPL to allow more refined elicitation of the true valuation. We do so, in the form of a computerized variant on the basic MPL format which we call an Iterative MPL (iMPL). Consider three MPL designs:

■   MPL – this is the standard format in which the subject sees a fixed array of paired options and chooses one for each row. It allows subjects to switch back and forth as they like, and has already been used in many experiments.

■   sMPL – Switching MPL varies the standard MPL by asking the subject to simply choose the row at which he wants to first switch from option A to option B, assuming monotonicity of the underlying preferences to fill out the remaining choices. This is an important behavioral bridge to the Iterative MPL below, since the latter implicitly assumes such behavior. In all other respects the sMPL looks just like the standard MPL.

■   iMPL – The iterative MPL extends the Switching MPL to allow the individual to make choices from refined options within the option last chosen. That is, if someone decides at

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[21] In the original context, expenditures could never be negative.
[22] Although CW constructed the likelihood function for the interval-censored regression model "by hand" using *LIMDEP*, it is now a standard option in popular statistical packages including the latest version of *LIMDEP*. For example, *Stata* has an official command INTREG to estimate these models, including variants for complex survey data (SVYINTREG) and panel data (XTINTREG). There is also a user-written command, INTREG2, for multiplicative heteroskedasticity specifications.
[23] See Harrison [1992].

some stage to switch from option A to option B between values of $10 and $20, the next stage of an iMPL would then prompt the subject to make more choices within this interval, to refine the values elicited.[24] Figures 1 and 2 illustrate Level 1 and Level 2, respectively, of an iMPL for a discount rate task. In Level 1 the illustrative subject first chooses B when the interest rate is between 25% and 30%, so that Level 2 presents the subject with 11 more choices within the interval 25% to 30%. The format naturally has some "smarts" built into it: when the values being elicited drop to some specified perceptive threshold (e.g., 0.05 of a percentage point of AR in the discount rate task, and a 1-in-100 die throw in the risk aversion task), the iMPL collapses down to an endogenous number of final rows. When the threshold is met at this minimal interval, the program stops iterating.

The iMPL uses the same incentive logic as the MPL and sMPL. After making all responses, the subject has one row from the first table selected at random by the experimenter. In the MPL and sMPL, that is all there is. In the iMPL, that is all there is if the row selected at random by the experimenter is *not* the one at which the subject switched in Level 1. If it *is* the row at which the subject switched, another random draw is made to pick a row in the Level 2 table. For some tasks this procedure is repeated to Level 3.

FIGURE 1 ABOUT HERE

FIGURE 2 ABOUT HERE

We utilize the iMPL since it provides more refined responses and does not appear to otherwise affect the response. We are also able to undertake some qualified tests of the iMPL procedure by comparing responses at the initial Level 1 step with responses at the final step. This is

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[24] If the subject always chooses A, or indicates indifference for any of the decision rows, there are no additional decisions required and the task is completed.

not the same as the subject facing only an MPL design, but it provides some behavioral check on the new procedure.[25] We use the iMPL both for the risk tasks and the discount rate tasks.

Multiple Switch Points

The problem here is that some subjects switch back and forth as they move down the rows of the MPL. It is quite possible that switching behavior is the result of the subject being indifferent between the options. The implication here is that, in the absence of an explicit indifference option, one could simply use a "fatter" interval to represent this subject, defined by the first row that the subject switched at and the last row that the subject switched at. Few of the existing MPL implementations allow subjects to report indifference.

Our use of the iMPL removes the possibility that subjects can switch back and forth, since we ask them to just state a single switch point. Nevertheless, we also include an explicit indifference option. Hence we allow subjects in the first stage of the iMPL to state if they prefer option A, prefer option B, or if they do not care. The computerized interface handles this possibility in a manner that is consistent with there still being one switch point.

Framing Effects

A natural concern with the MPL is that it might encourage subjects to pick a response in the middle of the table, independent of true valuations. There could be a psychological bias towards the middle, although that is far from obvious *a priori*. More to the point in a valuation setting, the use of specific values at either end of the table could signal to the subject that the experimenter believes that these are reasonable upper and lower bounds.[26] In some tasks, such as risk elicitation tasks, the

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[25] Andersen, Harrison, Lau and Rutström [2004] examine these three institutions in controlled laboratory experiments with college students. The sMPL is implemented because the iMPL changes the decision in two ways: forcing a single switch point in each table, and refining the choice. By comparing MPL and sMPL we can see the pure effect of the first change, and by comparing sMPL and iMPL we can see the pure effect of the second change.
[26] Harrison, Harstad and Rutström [2004] examine the ways in which such information could impact elicited valuations.

values are bounded by the laws of probability between 0 and 1, so this is less likely to be a factor compared to the pure psychological anchor of the middle row.

One solution to this task is to randomize the order of the rows. This is popular in some experimental studies in psychology which elicited discount rates, such as Kirby and Maraković [1996] and Kirby, Petry and Bickel [1999]. We find it unattractive for two reasons. First, if there is a purely psychological anchoring effect towards the middle, this will do nothing but add noise to the responses. Second, the valuation task is fundamentally harder from a cognitive perspective if one shuffles the order of valuations across rows. This harder task may be worthy of study, but is a needless confound for our inferential purposes.

Framing effects can be relatively easily tested for by varying the relative size of theintervals of the basic MPL table. If there is an effect on responses, it will be easy to identify statistically and then to correct for it in the data analysis. We would not be surprised to find framing effects of this kind. They do not necessarily indicate a failure of the traditional economic model, so much as a need to recognize that subjects use all available information to identify a good valuation for a commodity.[27] Thus it is critical to be able to estimate the quantitative effect of certain frames and then allow for them in subsequent statistical analysis.

We devise a test for framing effects by varying the cardinal scale of the MPL used in the risk aversion task. Two asymmetric frames are developed: the *skewHI* treatment offers initial probabilities of (0.3, 0.5, 0.7, 0.8, 0.9 and 1), while *skewLO* offers initial probabilities of (0.1, 0.2, 0.3, 0.5, 0.7, and

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[27] See Harrison, Harstad and Rutström [2004] for a general discussion of the various ways in which such information might impact elicited valuations. CHR discuss the use of a front end delay in discount rate experiments in this manner: the *absence* of a front end delay representing a choice between a "good apple today" and a "bad apple in a week," where the goodness of the apple refers to the probability of actually being paid. In such a frame, it is not clear if the subject is responding to the time delay or the quality of the apple. The psychological literature provides evidence of the effects of framing, such as in the "more is less" setting in which subjects *appear* to be willing to pay less money for more of the good (e.g., Hsee, Loewenstein, Blount and Bazerman [1999]). For example, List [2002] offers subjects 10 high-quality sportscards in one treatment, and the same 10 high-quality sportscards *plus* 3 poor-quality sportscards in another treatment. When valuing these separately, subjects tend to value the second set lower than the first, but when valuing them jointly, they value the second set equal to or higher than the first set. Arguably, the inclusion of 3 poor-quality cards makes the subject wary that there might be quality vagaries in all cards, such that this choice is framed by the subjects as "10 good apples" and "13 good or bad apples" rather than "10 good apples" and "10 good apples, and some apples I can toss or use for cooking."

1). This treatment yields 6 decision rows in Level 1 of the iMPL, as opposed to the 10 rows in the symmetric frame.[28] The distribution of frame treatments across sessions is detailed in Appendix B, which documents the sample design.

B. *Complementary Laboratory Experiments*

It is too costly to examine every possible variant of an experimental design such as ours in the field. Sample sizes in cells would become too small for any reliable statistical inferences. Hence, in an extension of the work presented here, we exploit the complementarity of the laboratory and the field to assess the behavioral effects of design variations.

One issue is the performance of the iMPL institution compared to the traditional MPL institution. In this case we propose examination of the MPL, sMPL and iMPL institutions, with subjects assigned randomly to each. Since framing is a potentially important confound, and could plausibly vary with the use of MPL and iMPL, we will interact the two asymmetric treatments described above with the type of MPL institution. Including controls, this implies a 3×3 design for each elicitation task. Each such design would be implemented for IDR tasks and RA tasks separately, so that we then have a $2 \times 3 \times 3$ design. We also test for the possibility of order effects in the lab by varying the order of the four lottery tasks, further expanding the size of the design.

Because there are important debates over whether EUT is appropriately defined over lab income, lifetime income, lifetime wealth, or non-additive components of lifetime wealth,[29] we also propose one intra-session treatment: the variation of initial endowments that the subject receives. All subjects would receive the same initial show-up fee. In addition, following Rutström [1998], we

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[28] The skewed frames will affect the implementation of the iMPL. In the symmetric frame, all intervals are 10 probability points wide, so that a second level is all that is needed to bring subject choices down to precise intervals of 1 probability point. In the skewed frames, however, because the intervals vary in size, a third level is required to bring choices down to this level of precision, and the number of decision rows in Level 3 depends on the width of the interval in Level 1 at which the subject switches.

[29] See Cox and Sadiraj [2004], Harrison, Johnson, McInnes and Rutström [2003b], Rabin [2000], Rabin and Thaler [2001] and Rubinstein [2002] for discussion of these debates. Of course, these are older issues: see Markowitz [1952], Samuelson [1952; ¶13, p.676] and Quizon, Binswanger and Machina [1984].

would then augment that amount randomly and privately. Thus we can *begin* the assessment of how the elicited risk and time preferences depend on initial wealth, even if this only refers to "lab wealth."[30]

C. Panel Experiments

The first phase of our experimental design involved eliciting risk attitudes and discount rates for a large sample in a series of tasks, four risk tasks and six discount rate tasks. The final phase will involve re-visiting these subjects in the future, and re-estimating the same risk and time preferences. We therefore construct a panel data set over the ten tasks and the two visits.

Several studies, including HLW and CHR, have examined the issue of dynamic consistency by testing whether discount rates elicited at a given point in time are consistent across different time horizons. CHR used a between-subject design and HLW used a within-subject design. Both of them asked questions to a subject at only one point in time, however. Our panel experiments will allow us to look at the dynamic consistency issue in two ways: first we have a within-subject variation in horizons, and second we have a within-subject revisit after some time has past after the initial questions were asked. The revisit experiments will allow for a direct test of whether individuals tend to reverse their preferences as future rewards become more proximate, as suggested by Strotz [1956]. Let T represent the time horizons given to subjects in the first stage. Then the second stage will provide the same tasks and same monetary incentives to subjects at t months after the first stage, but with new time horizons of T-t. If preferences are dynamically consistent, the subject will reveal the same discount rate for horizon T-t in the second stage as he did for horizon T in the first stage.

We will also use the second stage of risk aversion experiments to test whether risk preferences are stable over time. Moreover, in both stages we collect the same information on socio-

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[30] An extension of this treatment would be to evaluate the effects of having subjects *earn* some initial money with a non-trivial task, and then make decisions. Furthermore, one could modify the task itself to refer to their earnings, rather than "found money" provided by the experimenter. Thus the endowment and the task income would better reflect decisions over income that had been acquired by the subject in a more natural manner.

demographic characteristics, financial market activities, and subjects' expectations about their future

economic conditions and their own future financial position. With this information we can check to

see if any of the "states" defining the household have changed, so that we can determine if *state-dependent* preferences are stable over time.

4. Conducting the Experiments in the Field

A. Sampling Procedures

The sample for the field experiments was designed to generate a representative sample of the

adult Danish population. HLW relied on the sample frames developed by the SFI for their sample,

and also used SFI personnel to conduct the field experiments. Given the substantial cost of using

such survey firms, and the desire to have more control over this aspect of the field experiment, we

decided to undertake the sampling and experiments ourselves. There were six steps in the

construction of the sample, with further details provided in Appendix B:

- First, a random sample of 25,000 Danes was drawn from the Danish Civil Registration

    Office in January 2003. Only Danes born between 1927 and 1983 were included, thereby

    restricting the age range of the target population to between 19 and 75. For each person in

    this random sample we had access to their name, address, county, municipality, birth date,

    and sex. Due to the absence of names and/or addresses, 28 of these records were discarded.

- Second, we discarded 17 municipalities (including one county) from the population, due to

    them being located in extraordinarily remote locations. The population represented in these

    locations amounts to less than 2% of the Danish population, or 493 individuals in our

    sample from the civil registry.

- Third, we assigned each county either 1 session or 2 sessions, in rough proportionality to the

    population of the county. In total we assigned 20 sessions. Each session consisted of two

    sub-sessions at the same locale and date, one at 5pm and another at 8pm, and subjects were

    allowed to choose which sub-session suited them best.

- Fourth, we divided 6 counties into two sub-groups because the distance between some

  municipalities in the county and the location of the session would otherwise have been too

  large. A random draw was made between the two sub-groups and the location selected,

  where the weights reflect the relative size of the population in September 2002.

- Fifth, we picked the first 30 or 60 randomly sorted records within each county, depending

  on the number of sessions allocated to that county. This provided a sub-sample of 600.

- Sixth, we mailed invitations to attend a session to the sub-sample of 600, offering each

  person a choice of times for the session.[31] Response rates were low in some counties, so

  another 64 invitations were mailed out in these counties to newly drawn subjects.[32] Everyone

  that gave a positive response was assigned to a session, and our recruited sample was 268.[33]

  These procedures generally followed those used earlier by HLW.

Attendance at the experimental sessions was extraordinarily high, including 4 persons who did not respond to the letter of invitation but showed up unexpectedly and participated in the experiment. Four persons turned up for their session, but were not able to participate in the experiments.[34] The experiments were conducted between June 2 and June 24, 2003, and a total of 253 subjects participated in the experiments.[35] Table 3 summarizes population shares by county,

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[31] The initial letter of invitation included an answer form and a prepaid envelope, and the subject was asked to answer within one week. The same day we received the answer form, a reply letter was sent confirming their participation in the meeting at the given location, date and time. Every recruited subject was reminded by mail or phone within a week of the meeting. Both procedures were used for the first three sessions, and attendance was almost 100% at these sessions. We reminded subjects by mail for the remaining sessions because this procedure is more convenient.

[32] An additional 45 and 19 invitations were sent out in second and third waves of mailings, respectively. The first wave of invitations were sent out four weeks before the first session was scheduled, and we asked people to reply within one week. The second and third waves of invitations were sent out two and three weeks after the first wave, respectively.

[33] The response rate was 42.5% for the first wave of invitations, 20.0% percent for the second wave, and 22.1% for the third wave.

[34] The first person suffered from dementia and could not remember the instructions; the second person was a 76 year old woman who was not able to control the mouse and eventually gave up; the third person had just won a world championship in sailing and was too busy with interviews to stay for two hours; and the fourth person was sent home because too many people showed up (one person came unexpected, and we had only ten laptops available at that session).

[35] Certain events might have plausibly triggered some of the no-shows: for example, 3 men did not turn up on June 11, 2003, but that was the night that the Danish national soccer team played a qualifying game for the European championships against Luxembourg. This game was not scheduled when we picked

distribution of sessions across counties, and final sample frequencies.

TABLE 3 ABOUT HERE

Sample weights for the subjects of the experiment can be constructed using the sample design. Table 4 contains the information needed to see how these were constructed. The population was broken down by county, age group, and sex, as listed in the first 4 columns.[36] The total adult population in these counties in 2002 was 3,909,921, as shown at the bottom. The objective is to construct weights for the sample that "replicate" the sample observation such that the weighted sample reflects the distribution of the Danish population in column 4. There were three sampling stages. The first was the choice of a random sample of people to contact, as described above. This sample of 664 is distributed by county, age group and sex as shown in the "Contacted" column. The second sampling stage was the recruitment stage, in which a subset of this 664 contacted us in response to our letter of invitation and agreed to participate. The sample of recruited subjects was 268, as shown in the "Recruited" column. The third sampling stage was the actual attendance stage, where a subset of the 268 that were recruited actually showed up for the experiment. The final sample of subjects was 253, as shown in the columns "Participated" and "Participated Share".

TABLE 4 ABOUT HERE

The probability of inclusion in the final sample is then given by dividing, for each county, age group and sex, the number of individuals that actually participated by the population. This probability, shown as column "Probability of Inclusion," is just the sample that participated divided by the target population (i.e., column "Sample Participated" ÷ column "Population Size"). The

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session dates.
[36] Counties 30 and 35 were aggregated, due to smaller samples, as were counties 65 and 76. These are each relatively similar areas in Denmark.

sample weight, shown in column "Weight," is the inverse of the probability of inclusion. As a check on this arithmetic, column "Check Frequency" shows the sample in each cell (row) multiplied by the sample weight: the sum of this weighted frequency must, by construction, equal the target population of Denmark. The sample weights in column "Weight" will be used to weight the responses in our statistical analysis, so as to generate unbiased estimates for the adult Danish population. Our approach is to estimate statistical models of observed behavior using un-weighted responses, to use those models to estimate measures of risk aversion or individual discount rates for individuals in the sample, and then to use the sample weights to calculate weighted averages for the population.[37]

B. Conduct of the Sessions

To minimize travel times for subjects, we reserved hotel meeting rooms in convenient locations across Denmark in which to conduct sessions.[38] Because the sessions lasted for two hours, light refreshments were provided. Participants met in groups of no more than 10. To conduct computerized experiments in this field study, we found it was cost-effective to purchase laptop computers and transport them to the meeting sites. It was not necessary to network the computers for these experiments; the program ran independently on each computer and results for each subject were saved onto the laptop that he or she used. Each subject is identified by a unique ID number in the data. For the randomization procedures, two bingo cages were used in each session, one containing 100 balls, and the other containing 3 to 11 balls, depending on the number of decision rows in the iMPL used in different treatments. We found two bingo cages to be the most

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[37] This approach is explained by Harrison and Lesley [1996]. The alternative is to estimate the statistical model using information on the sample design, in order to generate estimates at that level reflecting the population. We plan to examine and compare both methods in subsequent work.

[38] It is possible to undertake experiments over the web with a large sample of subjects drawn from the population. Kapteyn and Teppa [2003] illustrate how one can elicit hypothetical responses to elicit time preferences using a panel of 2,000 Dutch households connected by home computer to surveys. Although not concerned with risk and time preferences directly, Hey [2002] illustrates how one can augment such electronic panel surveys with real experiments. Donkers and van Soest [1999] elicit hypothetical risk and time preferences from pre-existing panels of Dutch households being surveyed for other reasons.

transparent and convenient way to generate random outcomes in the experiments.

To begin the sessions, subjects were welcomed and reminded that they were to be paid 500 DKK for their participation to cover travel costs as long as they were able to stay for the full two hours required for the experiment. Anyone who was not able to stay for the full two hours was paid 100 DKK and excused from the experiment. The experimenters then asked for a volunteer to inspect and verify the bingo cages and number of bingo balls.

Instructions for the experiment were provided on the computer screens, and subjects read through the instructions while the experimenter read them aloud. The experimenters followed the same script for each session; this script is reproduced in the appendix. A complete listing of the screen displays and instructions seen by subjects is also available.[39]

The experiment was conducted in four parts. Part I consisted of a questionnaire collecting subjects' socio-demographic characteristics. Part IV consisted of another questionnaire which elicits information on the subject's financial market instruments, and probes the subject for information on their expectations about their future economic conditions and their own future financial position. The questionnaires are rather long, so we chose to divide them across Parts I and IV in order to reduce subject fatigue and boredom. Both questionnaires are reproduced in Appendix A.

Part II consisted of the four risk aversion tasks, and Part III presented subjects with the six discount rate tasks. Before payments were determined for Part III, we asked subjects what they planned to do with the payments they might receive for Part III.

The four risk aversion tasks incorporate the incentive structure as described in Section 2A, and assigned frames as described in Section 3A. After subjects completed the four tasks, several random outcomes were generated in order to determine subject payments. For all subjects, one of the four tasks was chosen, then one of the decision rows in that task was chosen. For those subjects whose decision at that row led to Level 2, another random draw was required to choose a decision

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[39] See the file "Screen Shots.pdf" in the ExLab digital library. These displays are an English translation of the original Danish.

row in Level 2, and yet another random draw was required should that decision have led a subject to Level 3 in the iMPL. To maintain anonymity we performed the draws without announcing to which subjects it would apply. In the case where a subject indicated *Indifference* for the chosen decision row, another random draw determined whether the subject received the results from Lottery A or Lottery B. At this point all subjects knew whether they were playing Lottery A or Lottery B, and another random draw determined whether subjects were to receive the high payment or the low payment. Finally, a 10-sided die with numbers from 0 up to 9 was rolled for each subject individually. Any subject who received a roll of "0" received actual payment according to that final outcome. All payments were made at the end of the experiment.

A significant amount of time was spent training subjects on the iMPL and the randomization procedures in Part II of the experiment. Subjects were given handouts containing examples of Level 1 and Level 2 of an iMPL that had been filled in. The training exercise explained the logic of the iMPL and verified that subjects were able to correctly fill in an iMPL as shown in the handout. Next, the experimenters illustrated the random procedures necessary to reach a final lottery outcome for each possible choice in the chosen Level 1 decision row. Finally, a single trainer task was conducted in which payments were in the form of candies. The ten-sided die was rolled for each subject, and candies were given to each subject who received a roll of "0."

Finally, the six discount rate tasks, covering the 6 time horizons as described in Section 2B, were conducted. Because this task also used the iMPL format, with the same randomization procedures as the risk aversion task, it was not necessary to repeat the training exercises.

5. Results

We provide an overview of some of our findings from the first round of experiments using field subjects. This overview is only intended as a preliminary analysis of our large data set. All subjects make choices using the iMPL instrument, with 4 RA tasks and 6 IDR tasks. Thus we have a panel consisting of 10 observations per participant. We vary the frame (symmetric, *skewHI* and

skewLO) across sessions for the RA tasks. Other design issues were addressed in the complementary lab experiments and the field revisits, and are not discussed here.

A. Risk Aversion

Figure 3 shows the observed distribution of risk attitudes in our sample, using the raw mid-point of the elicited interval in the *final* iteration stage of the symmetric iMPL. We employ the sample weights such that the distribution reflects the adult Danish population. For this specification of CRRA, a value of 0 denotes risk neutrality, negative values indicate risk-loving, and positive values indicate risk aversion. We see evidence of high risk aversion. The mean CRRA coefficient is 0.67 and the median is 0.67, weighted to represent the Danish population. Out of the 397 observations in the symmetric frame only 52 (13%) indicate risk loving. This distribution is consistent with comparable estimates obtained in the United States, using college students and an MPL design, by HL and Harrison, Johnson, McInnes and Rutström [2003][2005], although indicating a higher average degree of risk aversion. The higher degree of risk aversion observed here may not be surprising given the high stakes used in this experiment. Both HL and Harrison, Johnson, McInnes and Rutström [2003][2005] report that coefficients of risk aversion are increasing in the stakes of the lotteries used.

FIGURE 3 ABOUT HERE

We run a panel interval model on our data to regress the elicited CRRA values on our frame treatments and several of the responses to the questionnaires. The assumption of CRRA is not crucial to any of the conclusions we draw here, but is a transparent and popular specification that allows us to investigate a number of questions about risk attitudes.

Table 5 lists the definitions of the explanatory variables and some summary statistics. We have a fairly broad representation of the Danish population in our sample. Over 65% of our sample

is over the age of 40, 28% of the participants have children, and 69% own their house or apartment. It is clear that our data set is quite different from the standard laboratory set using college students., and much more representative of the target population. Since we also use sample weights based on county, age group, and sex, our findings are likely to be broadly policy relevant for Denmark.

TABLE 5 ABOUT HERE

Table 6 displays a panel interval regression model of the final[40] elicited CRRA values. This model uses panel data since each subject provided four responses, one for each stake condition.[41] Unobserved individual effects are modeled using a random-effects specification The variables *skewLO* and *skewHI* simply control for the frame used, and will be discussed later. We observe that there is an effect on the CRRA coefficient from varying the lottery prizes across the 4 tasks. There is a significant difference between Task 1 (the reference task for this statistical analysis) and the other three tasks. In particular, Task 2 is associated with higher CRRA responses, with a significant coefficient value of 0.28. We therefore confirm the findings reported in HL and Harrison, Johnson, McInnes, and Rutström [2005] that the elicited *relative* risk aversion coefficient is not constant in the stakes, although here we varied the stakes in a non-monotonic manner.[42]

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[40] The term "final" refers to the iterations of the iMPL procedure.

[41] Several checks are undertaken for the specification. First, collapsing the intervals down to their mid-point allows a comparison of random-effects and fixed-effects specifications, and a Hausman test that the random-effects specification is consistent. There is no evidence that the random-effects specification is inconsistent. Second, a Breusch-Pagan test of the null hypothesis that there is no variance in the unobserved individual random effects is convincingly rejected. Third, since potentially fragile numerical quadrature methods are used to estimate this specification, we checked for numerical stability as the number of quadrature points is varied, and there was no evidence of instability in the log-likelihood or any of the individual coefficients. These specification tests are performed for all of our panel models with very similar results.

[42] One can either allow for this effect with a CRRA characterization that conditions on it, as we do here, or explore more flexible specifications than CRRA that might incorporate such variations within a single functional form. Harrison, Johnson, McInnes, and Rutström [2005] also report the presence of order effects in these kind of lottery choice tasks. To separate out order from stake effects for the Danish data we varied the order of the 4 tasks in our complementary laboratory experiments.

TABLE 6 ABOUT HERE

B. Discount Rates

Figure 4 displays the elicited discount rates for our subjects, using the mid-point of the final interval selected and pooling across all horizons. We observe variations of elicited IDR across subjects, with a mean of 24.2%, a median of 24.5% and a standard deviation of 15.7%. These values are close to those reported in the earlier field study by HLW on the Danish population, where the mean is 28.1%[43] They are somewhat higher than the estimated rates found in laboratory elicitation exercises on American students by CW, who report a median of 17.7% using a horizon of 60 days.

FIGURE 4 ABOUT HERE

Figure 5 displays the discount rates by horizon. The distribution of elicited rates for the 1 month and 4 month horizons have a modal response around 30%. The distributions for the other horizons have modal responses at lower rates.

FIGURE 5 ABOUT HERE

Table 7 reports the results from a panel interval regression of the final elicited discount rates, controlling for horizon and individual demographics.[44] These elicited rates are predictions for each

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[43] Elicited discount rates are often criticized because they are so much higher than market interest rates. Nevertheless, the consistency between rates elicited in various settings, including those inferred from actual consumption behavior (Hausmann [1979], Hartman and Doane [1986], Ruderman, Levine and McMahon [1986]), put the burden of proof on the critics to show why private individuals and households should be constrained by rates set on markets that include many institutional traders.

[44] We apply uncensored responses in the present statistical analysis in order to compare interval size across initial and final responses.

individual from the estimated statistical model. This model uses panel data since each subject provided six interval responses, one for each horizon. This regression shows that all horizons have significantly lower discount rates than the reference horizon, which is 1 month. On the other hand, the average drop from the reference horizon is only 3 to 5 percentage points, which is not large in relation to the average for the reference horizon (28.8%). In addition, elicited discount rates do not vary across the horizons beyond 1 month.

TABLE 7 ABOUT HERE

C. Effects of Using the Iterative MPL Procedure

We designed the iterative MPL procedure, iMPL, in order to get more precise responses from subjects than a we would from a procedure using a single table, such as the standard MPL. If subjects do not care much about the differences in their expected outcomes in the refined tables, the refinement should have no effect on the elicited CRRA. With indifference at the more refined levels subjects would either choose indifference or would randomize their choices such that, on average, the estimated responses would be the same.

Figure 6 shows how allowing subjects to iterate over the MPL valuation has an effect on CRRA interval sizes, and therefore on the precision with which we estimate the CRRA coefficients. (Precision depends partly on how risk averse a subject is, since even-sized intervals in probabilities do not map into even-sized intervals in CRRA coefficients.) The top panel in the figure shows the width of the interval within which the subjects switched from the safer to the riskier lottery in the initial stage. In this first stage of the iMPL the subject faces the same, relatively coarse, grid of probabilities used in previous MPL studies to elicit risk aversion. The bottom panel shows the width of the switching interval for the final stage.

FIGURE 6 ABOUT HERE

The top panel of Figure 6 shows that the average subject had CRRA intervals around 0.3 and 0.4 in the initial stage.[45] The fact that the intervals are wider than 0.1 is due to the presence of the indifference option. The bounds on the intervals are computed as the difference between the lower bound of the first B choice and the upper bound of the last A choice, so any expressed indifference would increase this difference. Comparison with the range of elicited CRRA mid-points in Figure 3 provides some perspective on the relative significance of this interval size. The average interval width in the first stage of the iMPL is about equal to the first-stage mean CRRA coefficient. Thus, there is a great deal of uncertainty regarding an individual's risk coefficient when based only on the decision in this first stage.[46]

Allowing iterations in the iMPL has to reduce the interval, since it cannot increase it by design, and the bottom panel of Figure 6 shows that it did lead to a dramatic reduction in the width of the elicited CRRA interval, and therefore in the uncertainty over the CRRA coefficients we elicit for our subjects. The vast majority of intervals for the final decision that subjects made were below 0.1.[47] These reductions in the size of the intervals are highly significant, using a panel Tobit regression controlling for possible confounds such as sample differences in demographics. The dependent variable (crraDIFF) is the difference between the upper and the lower bound of the CRRA interval for the subject. These data are a panel since we have four responses from each subject, corresponding to Tasks 1, 2, 3 and 4 in the experiment. Table 8 shows the estimated model; the coefficient on the dummy variable *Final* (-0.23) captures the large and significant reduction in the

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[45] In the symmetric treatment the average interval was 0.41 and the median interval was 0.32. The average interval was 0.49, and the median interval was 0.40, when all data is included.

[46] Of course, the variation in the distribution in Figure 3 is "between subjects," and the variation in intervals suggested by Figure 4 is "within subjects," but the two go together in a complete analysis to determine overall uncertainty in the estimated CRRA for the sample.

[47] Seventy-seven percent of the sample has an interval below 0.1 in their final iMPL iteration. In the symmetric treatment the average interval was 0.16 and the median was only 0.03. The average interval was 0.17, and the median was 0.03, when all data is included.

interval size.

We conclude that there is much to be gained from using the iMPL format in terms of the increase in the precision of the elicited risk attitude. Subjects do appear to care about choices at the later stages. We further find that the responses in the final stage, at the highest refinement level, are not significantly different from the responses on the initial level. Thus, using a standard MPL format[48] only implies a problem for the variance of the responses, not for the means. Table 9 shows a panel interval regression for the symmetric frame in which the dependent variable is the elicited CRRA interval for the subject, and where the data includes both the initial and the final iteration responses. The coefficient on *Final* is virtually zero.

For the IDR data we similarly find that we get increased precision from using the iMPL response format. The top panel of Figure 7 shows that the average subject had intervals that were 5 to 7 percentage points wide when pooling over all horizons. The bottom panel of Figure 7 shows that the iMPL iterations did lead to a dramatic reduction in the width of the elicited IDR interval, and therefore in the uncertainty over the discount rates we elicit for our subjects. The vast majority of intervals for the final decision that subjects made were below 1.0 percentage point: the average was 1.8 percentage points, with a median of only 0.05 of a percentage point. The reductions in the size of the intervals due to the iterations are highly significant, based on a panel Tobit regression controlling for demographics. The dependent variable (*idrDIFF*) is the difference between the upper and the lower bound of the IDR interval for the subject. Table 10 shows the results of estimating

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[48] We proxy the standard MPL here by investigating the initial responses to an iMPL task.

this model, and the coefficient on the dummy variable *Final* again captures the significant reduction in the interval size. We can also test if allowing iterations has an effect on the mean response. Table 11 regresses the elicited IDR on the dummy variable *Final* and a range of demographics, as well as dummies capturing the horizons. For this model we include both the initial and the final responses. The coefficient on *Final* is small, but statistically significant, implying an increase in the mean response of 3.1 percentage points. These discount rate regressions confirm the conclusion from the risk preference regressions that there is a value in allowing iterations of the iMPL format, since precision is improved, but that the effect on the mean response is small and only significant in the discount rate regressions.

FIGURE 7 ABOUT HERE

TABLE 10 ABOUT HERE

TABLE 11 ABOUT HERE

D. Framing Effects

One design issue that we test directly with the field data is whether subjects appear to be drawn to responses in the middle of the table. We only test this issue for the risk preference elicitation task. The most direct test is to simply compare the row number at which subjects switch to the riskier lottery in the initial task.[49] Figure 8 displays the responses in the initial iteration of the iMPL in a way that allows a comparison of the symmetric and asymmetric treatments. Consider the left panels first, which compare the *skewLO* and symmetric responses. The top left panel simply

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[49] The row number corresponds to the "number of safe choices" in the statistical analysis of HL [2002]. We generally prefer to evaluate the effects of treatments on implied risk aversion measures, since the two are not always the same, but in this case it is informative to do both.

shows the distribution of choices of each row in the *skewLO* experiments. The bottom left panel reports the distribution of responses in the symmetric design, but with the responses "aggregated" to match the *skewLO* design. That is, the first three rows of the *skewLO* and symmetric design represent the same probabilities; the fourth row of the *skewLO* design matches to rows 4 and 5 of the symmetric design; the fifth row of the *skewLO* design matches to rows 6 and 7 of the symmetric design; and the sixth row of the *skewLO* design matches to rows 8, 9 and 10 of the symmetric design. Thus we can directly compare the distributions on the left-hand side. The panels on the right-hand side do the comparable aggregation for the *skewHI* and symmetric responses.

These comparisons confirm the presence of framing effects. If there had been no framing, then the two distributions on the left would look similar, as would the two distributions on the right. We observe that the *skewLO* treatment did generate responses that imply lower CRRA values, since the distribution in the top left is skewed to the left compared to the one below it. Similarly, the *skewHI* treatment shows a difference, with the distribution on the top skewed to the right compared to the one below it.[50]

These initial response biases favoring the middle of the table may not, however, be important in terms of the elicited CRRA coefficients, after allowing subjects to refine their choice in the iMPL. The top and bottom panels of Figure 9 show the CRRA (raw midpoints) for the asymmetric menu treatments, and the middle panel reproduces the symmetric menu from Figure 3. The frame treatments were designed such that, *if subjects anchored on the frame* and were drawn to respond in the middle, we would elicit lower risk aversion estimate in one case (*skewLO*) and higher risk aversion estimates in another (*skewHI*). Inspection of these three panels suggests that there is a slight effect from the asymmetric menu designed to lower elicited risk aversion, but a more sizable one from the asymmetric menu designed to increase elicited risk aversion. The average CRRA coefficient is 0.67 in the symmetric treatment, and 0.43 and 0.91 in the *skewLO* and *skewHI*

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[50] Both the *skewLO* and the *skewHI* distributions are significantly different from the appropriately aggregated symmetric responses using a chi-square test. Both significance levels are less than 1%.

treatments, respectively.[51] The panel interval regression model of final responses shown in Table 6, which controls for demographics, find a significant effect only for the *skewHI* frame, however. The coefficient is 0.27 and is statistically significant at the 1% level.

E. Demographics

Since there are variations in responses across subjects it is of interest to test if these response variations are captured by observable characteristics such as demographics. We collected a wide number of observable characteristics in the questionnaires given to subjects, and some of them are included in the regressions.

Table 6 reports the main results with respect to demographic effects on risk attitudes. We do not find that the elicited risk attitudes are correlated with the sex of the subject. There is, however, an age effect, as shown by the *middle* age variable and the fact that all three age variables are jointly significant at the 1% significance level. We also find that students are more risk averse, displaying a CRRA that is 0.33 higher than non-students. Subjects with some post-secondary education and substantial higher education are also more risk averse, with a CRRA that is 0.28 and 0.34 higher, respectively. Finally, since there are reasons to suspect that risk attitudes may be a function of a person's income level, we also include an income variable in the regressions. Nevertheless, there are no income effects, although this is arguably already captured by other included variables (e.g., education).

Table 7 reports the main results with respect to demographic effects on discount rates. None of the included variables are strongly significant. The only significant variable is substantial higher education, which is significant at the 6% level.

We are delighted to report no apparent effects in either elicitation task from the experimenter that actually conducted the sessions.[52]

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[51] These are significantly different at p<0.001 using Wilcoxon-Mann-Whitney ranksum tests.
[52] The experimenters were Lau and Steffen Andersen. Both had experience in the conduct of these experiments, in a number of trainers conducted and supervised in Copenhagen by Rutström.

6. Conclusions

It is feasible to collect nationally representative estimates of risk and time preferences using controlled experiments. The sampling, recruiting, and experimental procedures we employed in Denmark could be adapted to other countries. The experimental procedures used can also be applied in more traditional laboratory settings with standard, college-recruited subject pools.

We conclude that our recruitment method worked extremely well for this population. We mailed out 664 invitations, generating 268 affirmative responses. This is a response rate of 41%, which can be considered high given that respondents would have to travel to participate in the study. Further, out of these 268 responses, only 15 (less than 6%) did not turn up to the session.

Our basic methodology is adopted from earlier studies. The MPL that we use in both the CRRA and the IDR elicitations has been employed previously in several studies. We implemented a variation of this instrument, the iMPL, in order to generate more precise responses. Our findings confirm that there is a strong improvement in precision from implementing this method. Further, we were concerned about the possibility of framing effects in the MPL. We do find such effects, with the initial responses in our two asymmetric treatments significantly different from the standard, symmetric one. Nevertheless, after iteration using the iMPL instrument, at least one such difference disappears.

Our results show evidence of risk aversion in the Danish population. Individual discount rates do not significantly vary across the horizons considered. Our subject pool is broadly representative of the adult Danish population with a good range of variation in demographic variables. In addition, we construct sample weights based on county, age group, and sex, making our observations particularly policy relevant for Denmark.

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Table 1: Payoff Matrix in the Holt and Laury Risk Aversion Experiments

Default payoff matrix for scale 1

Lottery A				Lottery B				EV^A	EV^B	Difference	Open CRRA Interval if Subject Switches to Lottery B
p($2)		p($1.60)		p($3.85)		p($0.10)					
0.1	$2	0.9	$1.60	0.1	$3.85	0.9	$0.10	$1.64	$0.48	$1.17	-∞, -1.71
0.2	$2	0.8	$1.60	0.2	$3.85	0.8	$0.10	$1.68	$0.85	$0.83	-1.71, -0.95
0.3	$2	0.7	$1.60	0.3	$3.85	0.7	$0.10	$1.72	$1.23	$0.49	-0.95, -0.49
0.4	$2	0.6	$1.60	0.4	$3.85	0.6	$0.10	$1.76	$1.60	$0.16	-0.49, -0.15
0.5	$2	0.5	$1.60	0.5	$3.85	0.5	$0.10	$1.80	$1.98	-$0.17	-0.15, 0.14
0.6	$2	0.4	$1.60	0.6	$3.85	0.4	$0.10	$1.84	$2.35	-$0.51	0.14, 0.41
0.7	$2	0.3	$1.60	0.7	$3.85	0.3	$0.10	$1.88	$2.73	-$0.84	0.41, 0.68
0.8	$2	0.2	$1.60	0.8	$3.85	0.2	$0.10	$1.92	$3.10	-$1.18	0.68, 0.97
0.9	$2	0.1	$1.60	0.9	$3.85	0.1	$0.10	$1.96	$3.48	-$1.52	0.97, 1.37
1	$2	0	$1.60	1	$3.85	0	$0.10	$2.00	$3.85	-$1.85	1.37, ∞

Note: The last four columns in this table, showing the expected values of the lotteries and the implied CRRA intervals, were not shown to subjects.

Table 2: Payoff Table for 6 Month Time Horizon

Payoff Alternative	Payment Option A (pays amount below in 1 month)	Payment Option B (pays amount below in 7 months)	Annual Interest Rate (AR, in percent)	Annual Effective Interest Rate (AER, in percent)	Preferred Payment Option (Circle A or B)	
1	3,000 DKK	3,038 DKK	2.5	2.52	A	B
2	3,000 DKK	3,075 DKK	5	5.09	A	B
3	3,000 DKK	3,114 DKK	7.5	7.71	A	B
4	3,000 DKK	3,152 DKK	10	10.38	A	B
5	3,000 DKK	3,190 DKK	12.5	13.1	A	B
6	3,000 DKK	3,229 DKK	15	15.87	A	B
7	3,000 DKK	3,268 DKK	17.5	18.68	A	B
8	3,000 DKK	3,308 DKK	20	21.55	A	B
9	3,000 DKK	3,347 DKK	22.5	24.47	A	B
10	3,000 DKK	3,387 DKK	25	27.44	A	B
11	3,000 DKK	3,427 DKK	27.5	30.47	A	B
12	3,000 DKK	3,467 DKK	30	33.55	A	B
13	3,000 DKK	3,507 DKK	32.5	36.68	A	B
14	3,000 DKK	3,548 DKK	35	39.87	A	B
15	3,000 DKK	3,589 DKK	37.5	43.11	A	B
16	3,000 DKK	3,630 DKK	40	46.41	A	B
17	3,000 DKK	3,671 DKK	42.5	49.77	A	B
18	3,000 DKK	3,713 DKK	45	53.18	A	B
19	3,000 DKK	3,755 DKK	47.5	56.65	A	B
20	3,000 DKK	3,797 DKK	50	60.18	A	B

Table 3: Sample Design

County	County Code	Population in 2002	Population Share	Number of Sessions	Sample Frequency	Sample Share
Greater Copenhagen area:		**1,199,470**	**22.50**	**4**	**56**	**22.14**
København/Frederiksberg	1	586,026	10.99	2	30	11.86
Københavns amt	15	613,444	11.51	2	26	10.28
Rest of Zeeland and Bornholm:		**1,195,394**	**22.43**	**5**	**59**	**23.32**
Frederiksborg amt	20	365,306	6.85	2	24	9.49
Roskilde amt	25	231,559	4.34	1	12	4.74
Vestsjællands amt	30	295,086	5.54	1	10	3.95
Storstrøms amt	35	259,106	4.86	1	13	5.14
Bornholms amt	40	44,337	0.83	0	0	0
Funen:		**471,974**	**8.86**	**2**	**23**	**9.09**
Fyns amt	42	471,974	8.86	2	23	9.09
Jutland:		**2,463,182**	**46.21**	**9**	**115**	**45.45**
Sønderjyllands amt	50	253,482	4.76	1	12	4.74
Ribe amt	55	224,345	4.21	1	14	5.53
Vejle amt	60	347,542	6.52	1	15	5.93
Ringkøbing amt	65	272,857	5.12	1	12	4.74
Århus amt	70	637,122	11.95	2	22	8.70
Viborg amt	76	233,681	4.38	1	15	5.93
Nordjyllands amt	80	494,153	9.27	2	25	9.88
Total Danish Population in 2002		**5,330,020**	**100%**			
Total # sessions				**20**		
Total sample size					**253**	**100%**

Table 4: Construction of Sample Weights

County	Age	Sex	Population		Sample						Check
			Size	Share	Contacted	Recruited	Participated	Participated Share	Probability of Inclusion	Weight	Frequency
1	Under 45	Male	153,811	0.0393	14	7	7	0.0277	0.00004551	21973	153811
1	Under 45	Female	150,340	0.0385	25	12	12	0.0474	0.00007982	12528.3	150340
1	45 and Over	Male	76,208	0.0195	6	5	5	0.0198	0.00006561	15241.6	76208
1	45 and Over	Female	81,227	0.0208	15	7	6	0.0237	0.00007387	13537.8	81227
15	Under 45	Male	110,556	0.0283	12	6	5	0.0198	0.00004523	22111.2	110556
15	Under 45	Female	110,621	0.0283	15	9	8	0.0316	0.00007232	13827.6	110621
15	45 and Over	Male	107,890	0.0276	15	6	6	0.0237	0.00005561	17981.7	107890
15	45 and Over	Female	118,568	0.0303	18	7	7	0.0277	0.00005904	16938.3	118568
20	Under 45	Male	63,164	0.0162	11	6	5	0.0198	0.00007916	12632.8	63164
20	Under 45	Female	63,848	0.0163	15	7	7	0.0277	0.00010964	9121.1	63848
20	45 and Over	Male	69,795	0.0179	24	7	7	0.0277	0.00010029	9970.7	69795
20	45 and Over	Female	72,972	0.0187	22	6	5	0.0198	0.00006852	14594.4	72972
25	Under 45	Male	42,722	0.0109	8	2	2	0.0079	0.00004681	21361	42722
25	Under 45	Female	42,691	0.0109	9	3	3	0.0119	0.00007027	14230.3	42691
25	45 and Over	Male	43,438	0.0111	8	4	4	0.0158	0.00009209	10859.5	43438
25	45 and Over	Female	44,772	0.0115	11	3	3	0.0119	0.00006701	14924	44772
30 and 35	Under 45	Male	97,408	0.0249	25	10	7	0.0277	0.00007186	13915.4	97408
30 and 35	Under 45	Female	94,362	0.0241	19	3	3	0.0119	0.00003179	31454	94362
30 and 35	45 and Over	Male	108,816	0.0278	20	8	8	0.0316	0.00007352	13602	108816
30 and 35	45 and Over	Female	109,722	0.0281	24	5	5	0.0198	0.00004557	21944.4	109722
42	Under 45	Male	87,916	0.0225	13	5	5	0.0198	0.00005687	17583.2	87916
42	Under 45	Female	85,043	0.0218	13	4	4	0.0158	0.00004704	21260.8	85043
42	45 and Over	Male	85,006	0.0217	19	9	8	0.0316	0.00009411	10625.8	85006
42	45 and Over	Female	87,404	0.0224	15	7	6	0.0237	0.00006865	14567.3	87404
50	Under 45	Male	44,765	0.0114	5	2	2	0.0079	0.00004468	22382.5	44765
50	Under 45	Female	42,673	0.0109	8	3	3	0.0119	0.00007030	14224.3	42673
50	45 and Over	Male	47,136	0.0121	9	4	4	0.0158	0.00008486	11784	47136
50	45 and Over	Female	47,614	0.0122	16	4	3	0.0119	0.00006301	15871.3	47614
55	Under 45	Male	41,846	0.0107	7	1	1	0.004	0.00002390	41846	41846
55	Under 45	Female	39,429	0.0101	9	2	2	0.0079	0.00005072	19714.5	39429
55	45 and Over	Male	39,910	0.0102	16	7	7	0.0277	0.00017539	5701.4	39910
55	45 and Over	Female	39,766	0.0102	8	4	4	0.0158	0.00010059	9941.5	39766
60	Under 45	Male	67,685	0.0173	8	5	5	0.0198	0.00007387	13537	67685
60	Under 45	Female	64,537	0.0165	7	4	4	0.0158	0.00006198	16134.3	64537
60	45 and Over	Male	61,320	0.0157	7	3	3	0.0119	0.00004892	20440	61320

60	45 and Over	Female	62,359	0.0159	8	3	3	0.0119	0.00004811	20786.3	62359
65 and 76	Under 45	Male	94,070	0.0241	12	3	2	0.0079	0.00002126	47035	94070
65 and 76	Under 45	Female	88,441	0.0226	11	6	6	0.0237	0.00006784	14740.2	88441
65 and 76	45 and Over	Male	91,176	0.0233	17	9	9	0.0356	0.00009871	10130.7	91176
65 and 76	45 and Over	Female	90,721	0.0232	20	11	10	0.0395	0.00011023	9072.1	90721
70	Under 45	Male	130,194	0.0333	12	6	5	0.0198	0.00003840	26038.8	130194
70	Under 45	Female	128,962	0.033	17	8	6	0.0237	0.00004653	21493.7	128962
70	45 and Over	Male	107,775	0.0276	12	4	3	0.0119	0.00002784	35925	107775
70	45 and Over	Female	110,524	0.0283	19	8	8	0.0316	0.00007238	13815.5	110524
80	Under 45	Male	94,315	0.0241	17	9	8	0.0316	0.00008482	11789.4	94315
80	Under 45	Female	88,504	0.0226	15	6	6	0.0237	0.00006779	14750.7	88504
80	45 and Over	Male	88,195	0.0226	12	4	4	0.0158	0.00004535	22048.8	88195
80	45 and Over	Female	89,704	0.0229	16	7	7	0.0277	0.00007803	12814.9	89704
All			**3,909,921**	**1.000**	**664**	**271**	**253**	**1.000**		**838804.1**	**3,909,921**

Table 5: List of Variables and Descriptive Statistics

N = 253

Variable	Definition	Estimated Population Mean	Raw Sample Mean
female	Female	0.50	0.51
young	Aged less than 30	0.19	0.17
middle	Aged between 40 and 50	0.27	0.28
old	Aged over 50	0.33	0.38
single	Lives alone	0.21	0.20
kids	Has children	0.31	0.28
nhhd	Number of people in the household	2.54	2.49
owner	Owns own home or apartment	0.68	0.69
retired	Retired	0.13	0.16
student	Student	0.10	0.09
skilled	Some post-secondary education	0.38	0.38
longedu	Substantial higher education	0.36	0.36
IncLow	Lower level income	0.33	0.34
IncHigh	Higher level income	0.36	0.34
copen	Lives in greater Copenhagen area	0.27	0.27
city	Lives in larger city of 20,000 or more	0.41	0.39
experimenter	Experimenter Andersen (default is Lau)	0.47	0.49

Legend: Most variables have self-evident definitions. The omitted age group is 30-39. Variable "skilled" indicates if the subject has completed vocational education and training or "short-cycle" higher education, and variable "longedu" indicates the completion of "medium-cycle" higher education or "long-cycle" higher education. These terms for the cycle of education are commonly used by Danes (most short-cycle higher education program last for less than 2 years; medium-cycle higher education lasts 3 to 4 years, and includes training for occupations such as a journalist, primary and lower secondary school teacher, nursery and kindergarten teacher, and ordinary nurse; long-cycle higher education typically lasts 5 years and is offered at Denmark's five ordinary universities, at the business schools and various other institutions such as the Technical University of Denmark, the schools of the Royal Danish Academy of Fine Arts, the Academies of Music, the Schools of Architecture and the Royal Danish School of Pharmacy). Lower incomes are defined in variable "IncLow" by a household income in 2002 below 300,000 kroner. Higher incomes are defined in variable "IncHigh" by a household income of 500,000 kroner or more.

Table 6: Statistical Model of Risk Aversion Responses

Panel interval regression, with the final CRRA interval as the dependent variable.

N=925 responses, based on 245 subjects.

Variable	Description	Estimate	Standard Error	*p*-value	Lower 95% Confidence Interval	Upper 95% Confidence Interval
Constant		0.26	0.30	0.37	-0.32	0.84
skewLO	Skew towards risk loving	-0.03	0.11	0.80	-0.24	0.19
skewHI	Skew towards risk aversion	0.27	0.11	0.01	0.06	0.49
Task2	Second risk task	0.28	0.06	0.00	0.17	0.39
Task3	Third risk task	0.18	0.06	0.00	0.07	0.29
Task4	Fourth risk task	0.19	0.06	0.00	0.08	0.30
experimenter	Experimenter effect	-0.06	0.09	0.53	-0.24	0.12
female	Female	-0.08	0.09	0.38	-0.26	0.10
young	Aged less than 30	0.15	0.18	0.42	-0.20	0.49
middle	Aged between 40 and 50	-0.29	0.14	0.04	-0.56	-0.01
old	Aged over 50	-0.10	0.16	0.52	-0.43	0.22
single	Lives alone	0.02	0.15	0.92	-0.28	0.31
kids	Has children	0.05	0.14	0.74	-0.23	0.32
nhhd	Number in household	-0.01	0.06	0.94	-0.13	0.12
owner	Owns home or apartment	0.05	0.13	0.72	-0.20	0.29
retired	Retired	-0.10	0.15	0.49	-0.40	0.19
student	Student	0.33	0.18	0.07	-0.02	0.68
skilled	Some post-secondary education	0.28	0.12	0.02	0.04	0.52
longedu	Substantial higher education	0.34	0.13	0.01	0.09	0.59
IncLow	Lower level income	-0.02	0.13	0.86	-0.27	0.23
IncHigh	Higher level income	0.03	0.12	0.83	-0.21	0.26
copen	Lives in Copenhagen area	0.20	0.12	0.10	-0.04	0.45
city	Lives in larger city of 20,000 or more	0.18	0.11	0.11	-0.04	0.4

Table 7: Statistical Model of Individual Discount Rates Responses

Panel interval regression, with the final discount rate interval as the dependent variable.

N=1460 responses, based on 249 subjects.

Variable	Description	Estimate	Standard Error	p-value	Lower 95% Confidence Interval	Upper 95% Confidence Interval
Constant		28.77	6.68	0.00	15.68	41.85
horizon4	4 months horizon	-3.57	1.15	0.00	-5.82	-1.31
horizon6	6 months horizon	-3.83	1.15	0.00	-6.10	-1.57
horizon12	12 months horizon	-5.14	1.15	0.00	-7.40	-2.88
horizon18	18 months horizon	-4.16	1.15	0.00	-6.43	-1.90
horizon24	24 months horizon	-3.32	1.15	0.00	-5.58	-1.06
experimenter	Experimenter effect	-2.09	2.20	0.34	-6.39	2.22
female	Female	1.51	2.19	0.49	-2.79	5.80
young	Aged less than 30	-3.24	4.38	0.46	-11.83	5.35
middle	Aged between 40 and 50	2.84	3.35	0.40	-3.73	9.40
old	Aged over 50	5.82	3.81	0.13	-1.65	13.29
single	Lives alone	2.80	3.59	0.43	-4.23	9.83
kids	Has children	4.62	3.45	0.18	-2.13	11.38
nhhd	Number in household	0.02	1.54	0.99	-3.00	3.04
owner	Owns home or apartment	0.45	2.77	0.87	-4.99	5.88
retired	Retired	-4.43	3.35	0.19	-11.00	2.13
student	Student	-1.74	4.42	0.69	-10.40	6.92
skilled	Some post-secondary education	-3.36	2.81	0.23	-8.86	2.14
longedu	Substantial higher education	-5.46	2.89	0.06	-11.13	0.21
IncLow	Lower level income	2.93	2.93	0.32	-2.80	8.67
IncHigh	Higher level income	-4.14	2.81	0.14	-9.64	1.36
copen	Lives in Copenhagen area	4.79	2.91	0.10	-0.91	10.49
city	Lives in larger city of 20,000 or more	4.06	2.59	0.12	-1.02	9.14

Table 8: Effect of Iterations on Risk Aversion Responses

Panel Tobit model of difference in elicited CRRA estimates.
Initial and final responses pooled, symmetric responses only.

N=700 responses, based on 112 subjects.

Variable	Description	Estimate	Standard Error	p-value	Lower 95% Confidence Interval	Upper 95% Confidence Interval
Constant		0.24	0.11	0.04	0.01	0.46
Final	Final iteration of task	-0.23	0.02	0.00	-0.27	-0.20
Task2	Second risk task	0.07	0.02	0.01	0.02	0.12
Task3	Third risk task	-0.02	0.03	0.54	-0.07	0.03
Task4	Fourth risk task	-0.07	0.03	0.01	-0.12	-0.02
experimenter	Experimenter effect	-0.04	0.04	0.21	-0.11	0.03
female	Female	0.01	0.04	0.88	-0.07	0.08
young	Aged less than 30	-0.08	0.07	0.21	-0.22	0.05
middle	Aged between 40 and 50	0.01	0.05	0.89	-0.10	0.11
old	Aged over 50	0.01	0.06	0.86	-0.11	0.13
single	Lives alone	0.06	0.05	0.24	-0.04	0.17
kids	Has children	-0.12	0.06	0.04	-0.23	-0.01
nhhd	Number in household	0.04	0.02	0.07	-0.00	0.09
owner	Owns home or apartment	0.04	0.04	0.31	-0.04	0.13
retired	Retired	0.08	0.05	0.16	-0.03	0.18
student	Student	0.02	0.08	0.78	-0.13	0.17
skilled	Some post-secondary education	-0.02	0.05	0.69	-0.11	0.07
longedu	Substantial higher education	-0.04	0.05	0.47	-0.13	0.06
IncLow	Lower level income	0.07	0.05	0.13	-0.02	0.17
IncHigh	Higher level income	0.01	0.05	0.75	-0.07	0.10
copen	Lives in Copenhagen area	-0.00	0.05	0.98	-0.10	0.09
city	Lives in larger city of 20,000 or more	0.03	0.04	0.50	-0.05	0.10

Table 9: Additional Analysis of the Effect of Iterations on Risk Aversion Responses

Panel interval regression model of elicited CRRA estimates.
Initial and final responses pooled, symmetric responses only.

N=779 responses, based on 112 subjects.

Variable	Description	Estimate	Standard Error	*p*-value	Lower 95% Confidence Interval	Upper 95% Confidence Interval
Constant		0.63	0.36	0.08	-0.07	1.33
Final	Final iteration of task	-0.04	0.04	0.37	-0.12	0.04
Task2	Second risk task	0.20	0.06	0.00	0.09	0.31
Task3	Third risk task	0.14	0.06	0.01	0.03	0.25
Task4	Fourth risk task	0.23	0.06	0.00	0.12	0.34
experimenter	Experimenter effect	-0.10	0.11	0.38	-0.31	0.12
female	Female	-0.23	0.11	0.04	-0.44	-0.01
young	Aged less than 30	0.19	0.21	0.37	-0.22	0.60
middle	Aged between 40 and 50	-0.48	0.17	0.00	-0.81	-0.16
old	Aged over 50	-0.34	0.19	0.07	-0.71	0.03
single	Lives alone	0.01	0.16	0.95	-0.31	0.33
kids	Has children	-0.07	0.17	0.68	-0.41	0.27
nhhd	Number in household	-0.07	0.07	0.32	-0.21	0.07
owner	Owns home or apartment	0.33	0.14	0.01	0.07	0.60
retired	Retired	0.01	0.17	0.96	-0.32	0.34
student	Student	0.48	0.23	0.04	0.02	0.94
skilled	Some post-secondary education	0.15	0.14	0.29	-0.13	0.42
longedu	Substantial higher education	0.27	0.15	0.07	-0.02	0.56
IncLow	Lower level income	0.01	0.15	0.97	-0.28	0.29
IncHigh	Higher level income	0.03	0.14	0.85	-0.25	0.30
copen	Lives in Copenhagen area	0.27	0.15	0.07	-0.02	0.56
city	Lives in larger city of 20,000 or more	0.05	0.12	0.71	-0.19	0.29

Table 10: Effect of Iterations on Discount Rate Responses

Panel Tobit model of difference in elicited discount rate estimates.
Initial and final responses pooled.

N=2325 responses, based on 243 subjects.

Variable	Description	Estimate	Standard Error	p-value	Lower 95% Confidence Interval	Upper 95% Confidence Interval
Constant		9.22	0.59	0.00	8.08	10.37
Final	Final iteration of task	-6.35	0.16	0.00	-6.66	-6.04
horizon4	4 months horizon	-0.12	0.27	0.65	-0.65	0.41
horizon6	6 months horizon	-0.34	0.27	0.21	-0.87	0.19
horizon12	12 months horizon	-0.21	0.27	0.43	-0.73	0.32
horizon18	18 months horizon	-0.31	0.27	0.26	-0.84	0.22
horizon24	24 months horizon	-0.36	0.27	0.18	-0.90	0.17
experimenter	Experimenter effect	-0.08	0.20	0.70	-0.47	0.31
female	Female	-0.34	0.21	0.11	-0.75	0.07
young	Aged less than 30	-0.52	0.38	0.17	-1.27	0.22
middle	Aged between 40 and 50	-0.47	0.34	0.17	-1.13	0.19
old	Aged over 50	-0.23	0.37	0.53	-0.95	0.49
single	Lives alone	0.17	0.28	0.54	-0.38	0.72
kids	Has children	-0.48	0.36	0.18	-1.20	0.23
nhhd	Number in household	0.19	0.15	0.19	-0.09	0.48
owner	Owns home or apartment	-0.27	0.23	0.23	-0.72	0.17
retired	Retired	-0.37	0.28	0.19	-0.93	0.19
student	Student	-0.70	0.38	0.07	-1.46	0.05
skilled	Some post-secondary education	0.02	0.26	0.94	-0.50	0.54
longedu	Substantial higher education	-0.04	0.27	0.88	-0.58	0.49
IncLow	Lower level income	0.42	0.25	0.10	-0.08	0.91
IncHigh	Higher level income	-0.03	0.26	0.90	-0.53	0.47
copen	Lives in Copenhagen area	-0.28	0.27	0.30	-0.82	0.25
city	Lives in larger city of 20,000 or more	-0.60	0.24	0.01	-1.06	-0.13

Table 11: Additional Analysis of the Effect of Iterations on Discount Rate Responses

Panel interval regression model of elicited discount rate estimates.
Initial and final responses pooled.

N=2468 responses, based on 249 subjects.

Variable	Description	Estimate	Standard Error	p-value	Lower 95% Confidence Interval	Upper 95% Confidence Interval
Constant		26.96	2.21	0.00	22.63	31.30
Final	Final iteration of task	3.07	0.46	0.00	2.17	3.98
horizon4	4 months horizon	-3.27	0.79	0.00	-4.82	-1.72
horizon6	6 months horizon	-3.74	0.79	0.00	-5.29	-2.19
horizon12	12 months horizon	-4.60	0.79	0.00	-6.14	-3.06
horizon18	18 months horizon	-3.93	0.79	0.00	-5.48	-2.38
horizon24	24 months horizon	-3.46	0.79	0.00	-5.01	-1.91
experimenter	Experimenter effect	-7.85	0.73	0.00	-9.28	-6.42
female	Female	-0.32	0.72	0.66	-1.73	1.10
young	Aged less than 30	-5.67	1.36	0.00	-8.33	-3.01
middle	Aged between 40 and 50	3.99	1.20	0.00	1.63	6.34
old	Aged over 50	5.71	1.59	0.00	2.60	8.81
single	Lives alone	6.18	1.02	0.00	4.18	8.17
kids	Has children	2.23	1.04	0.03	0.20	4.27
nhhd	Number in household	0.14	0.41	0.73	-0.67	0.95
owner	Owns home or apartment	1.58	0.87	0.07	-0.13	3.29
retired	Retired	-6.26	1.12	0.00	-8.45	-4.08
student	Student	-2.09	1.25	0.09	-4.54	0.35
skilled	Some post-secondary education	-3.07	0.89	0.00	-4.82	-1.32
longedu	Substantial higher education	-3.19	1.04	0.00	-5.24	-1.15
IncLow	Lower level income	5.62	0.99	0.00	3.67	7.56
IncHigh	Higher level income	-4.85	0.91	0.00	-6.64	-3.06
copen	Lives in Copenhagen area	5.17	0.98	0.00	3.26	7.09
city	Lives in larger city of 20,000 or more	2.89	0.89	0.00	1.14	4.64

Figure 1: First Level of the iMPL Elicitation Format

Decision	Option A: To be paid in 1 month	Option B: To be paid in 7 months	Annual Interest rate	Annual Effective Interest rate	Choice
1	$3,000.00	$3,075.47	5.00 %	5.09 %	⊙ A ○ I ○ B
2	$3,000.00	$3,151.87	10.00 %	10.38 %	⊙ A ○ I ○ B
3	$3,000.00	$3,229.22	15.00 %	15.87 %	⊙ A ○ I ○ B
4	$3,000.00	$3,307.50	20.00 %	21.55 %	⊙ A ○ I ○ B
5	$3,000.00	$3,386.72	25.00 %	27.44 %	⊙ A ○ I ○ B
6	$3,000.00	$3,466.88	30.00 %	33.55 %	○ A ○ I ⊙ B
7	$3,000.00	$3,547.97	35.00 %	39.87 %	○ A ○ I ⊙ B
8	$3,000.00	$3,630.00	40.00 %	46.41 %	○ A ○ I ⊙ B
9	$3,000.00	$3,712.97	45.00 %	53.18 %	○ A ○ I ⊙ B
10	$3,000.00	$3,796.88	50.00 %	60.18 %	○ A ○ I ⊙ B

OK

Figure 2: Second Level of the iMPL Elicitation Format

Decision	Option A: To be paid in 1 month	Option B: To be paid in 7 months	Annual Interest rate	Annual Effective Interest rate	Choice
1	$3,000.00	$3,386.72	25.00 %	27.44 %	⊙ A ○ I ○ B
2	$3,000.00	$3,394.69	25.50 %	28.04 %	⊙ A ○ I ○ B
3	$3,000.00	$3,402.68	26.00 %	28.65 %	⊙ A ○ I ○ B
4	$3,000.00	$3,410.67	26.50 %	29.25 %	⊙ A ○ I ○ B
5	$3,000.00	$3,418.67	27.00 %	29.86 %	⊙ A ○ I ○ B
6	$3,000.00	$3,426.68	27.50 %	30.47 %	⊙ A ○ I ○ B
7	$3,000.00	$3,434.70	28.00 %	31.08 %	⊙ A ○ I ○ B
8	$3,000.00	$3,442.73	28.50 %	31.69 %	⊙ A ○ I ○ B
9	$3,000.00	$3,450.77	29.00 %	32.31 %	⊙ A ○ I ○ B
10	$3,000.00	$3,458.82	29.50 %	32.93 %	○ A ○ I ⊙ B
11	$3,000.00	$3,466.88	30.00 %	33.55 %	○ A ○ I ⊙ B

OK

Figure 3: Distribution of CRRA in Denmark
With Symmetric Menu

Mid-Point of Raw Responses from iMPL

Constant Relative Risk Aversion weighted to population
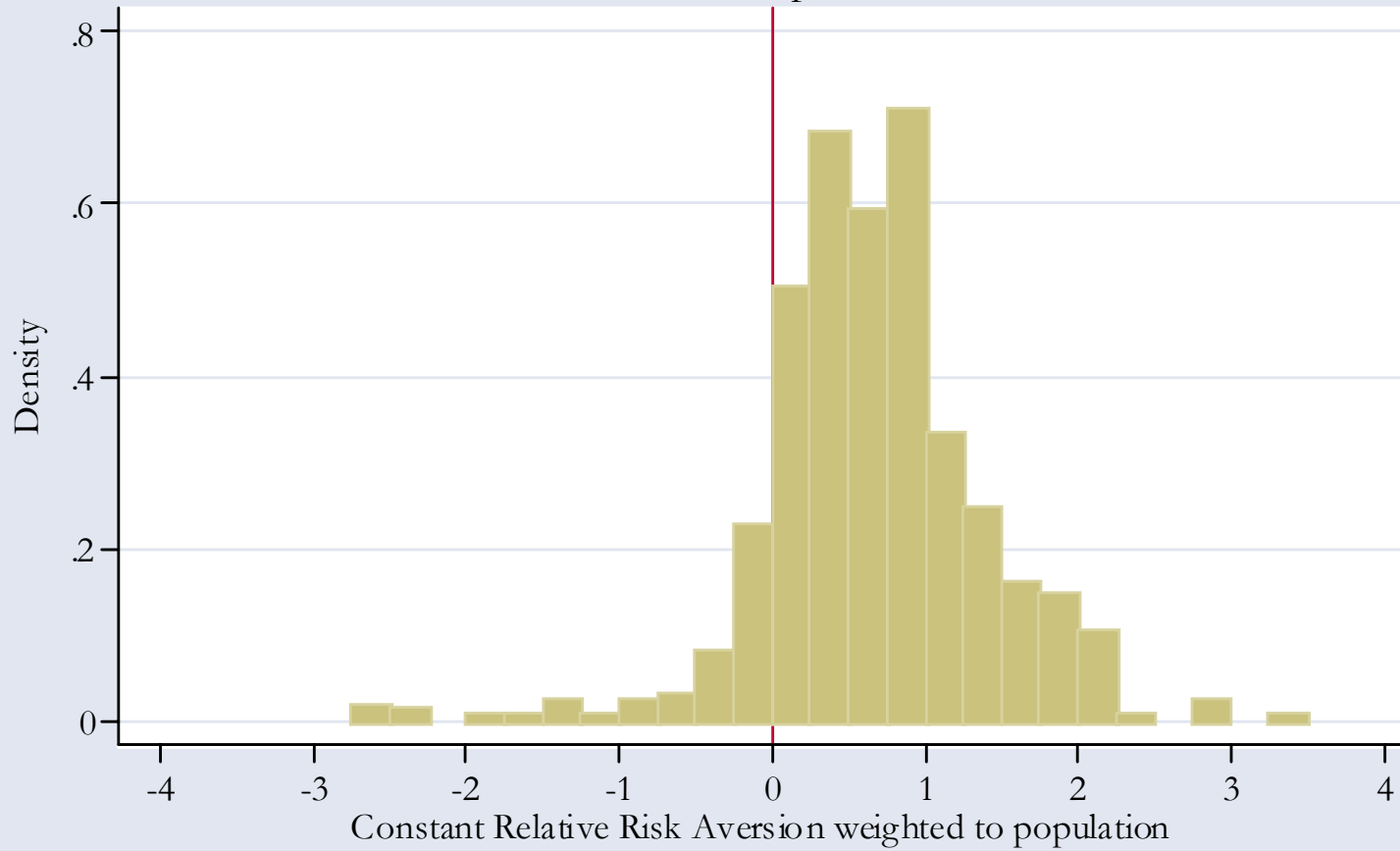
Figure 4: Elicited Discount Rates in Denmark

Mid-Point of interval chosen by subject
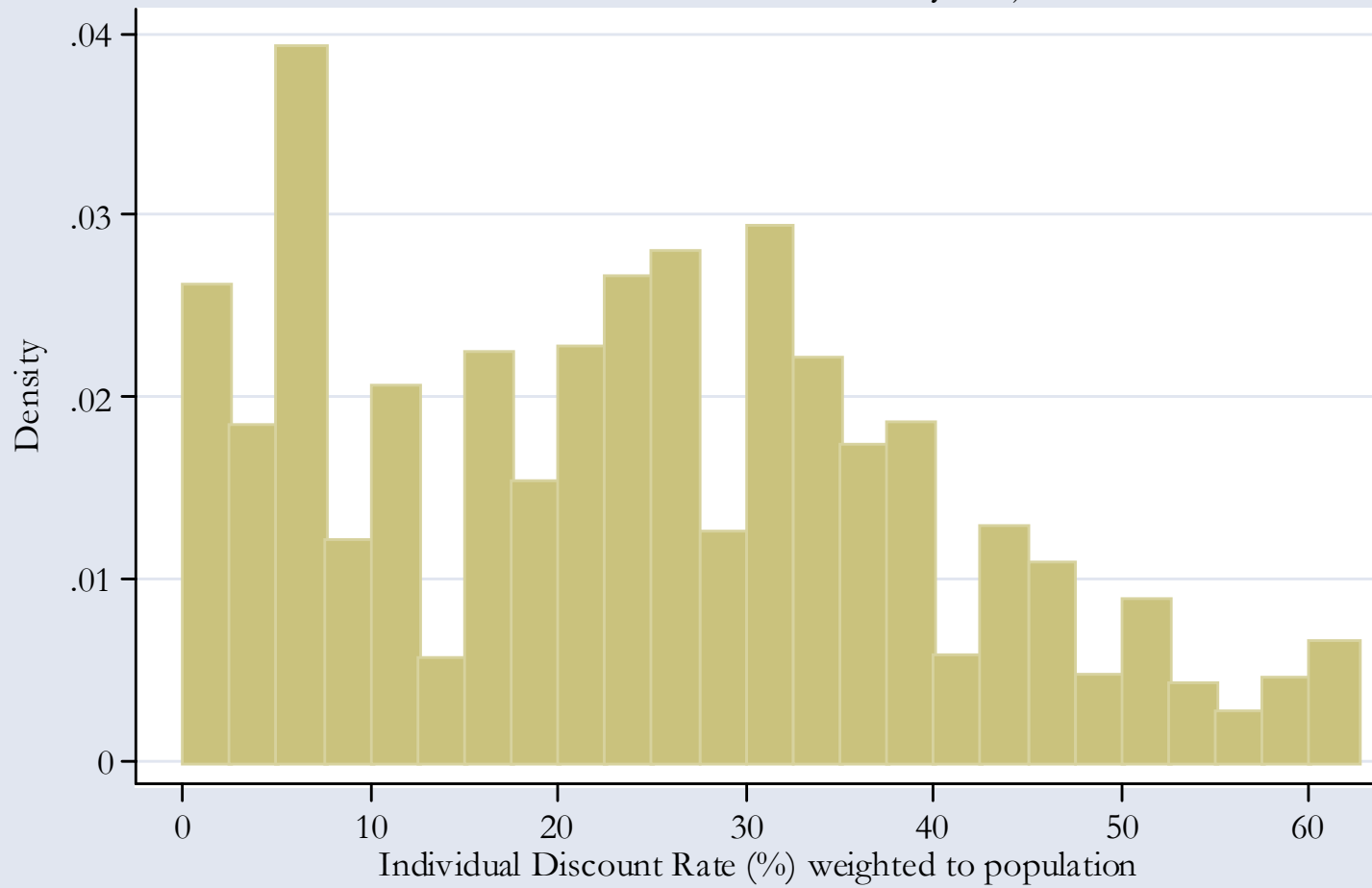
Figure 5: Elicited Discount Rates by Horizon

Mid-Point of interval chosen by subject
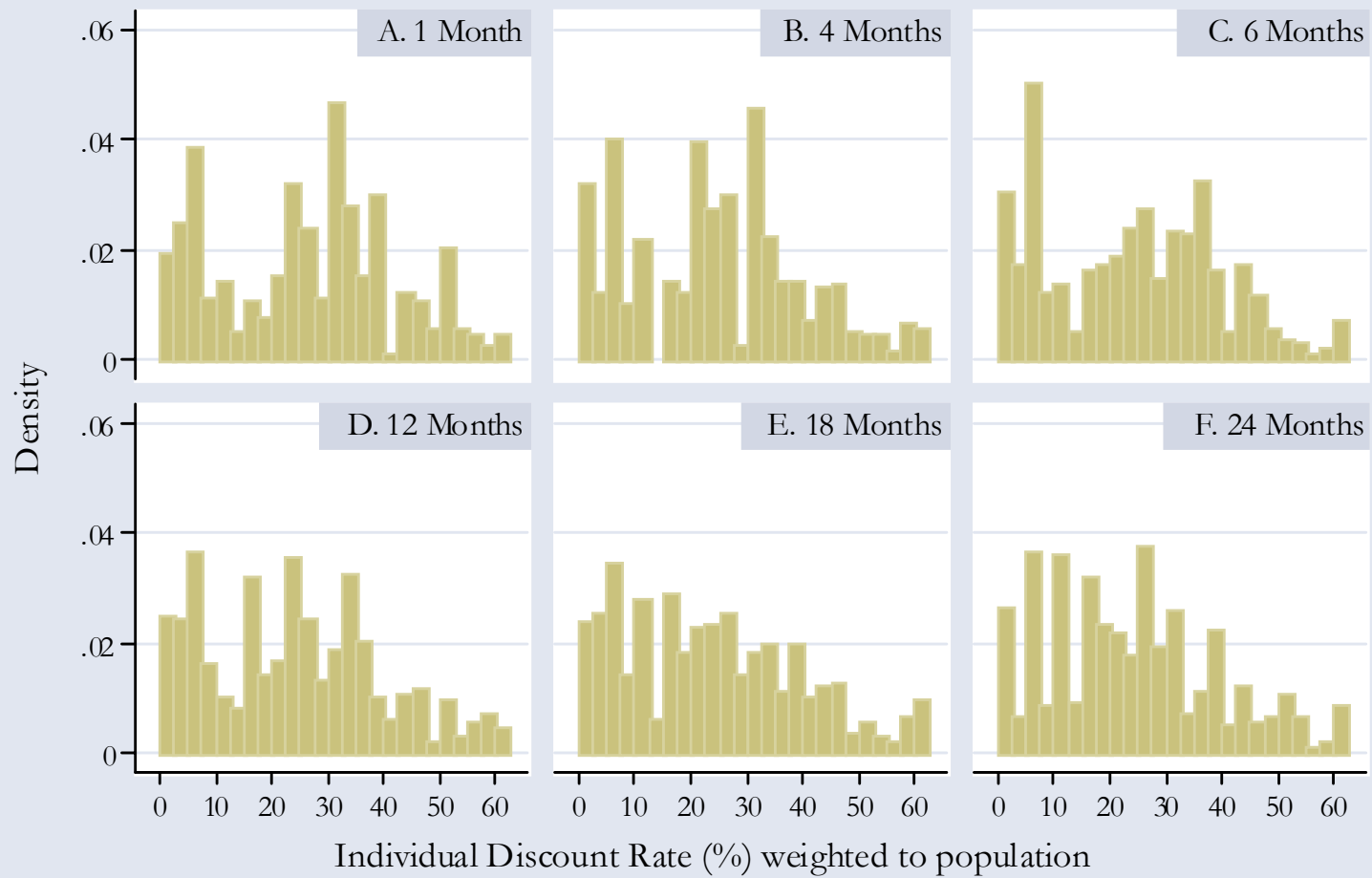
Figure 6: Effect of MPL Iteration on CRRA Interval Sizes

Responses from Symmetric Menu Treatment

Initial Difference in CRRA Intervals
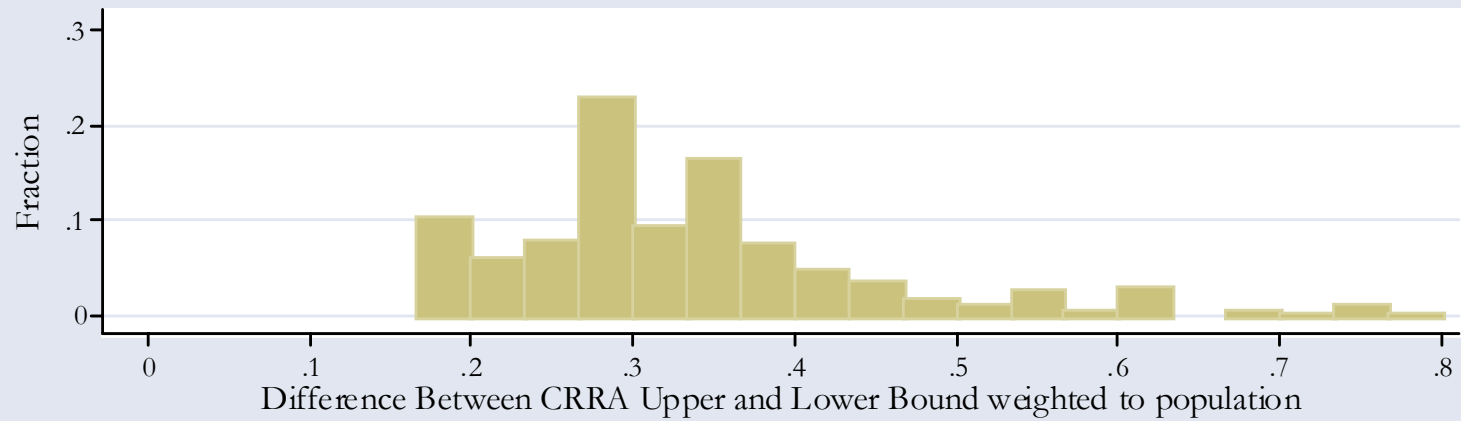
Final Difference in CRRA Intervals
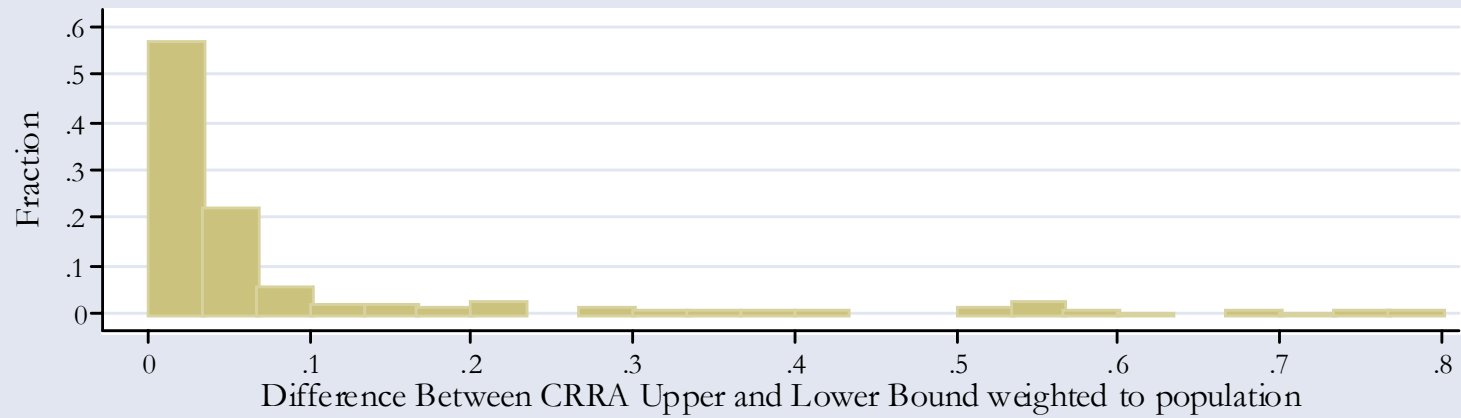
Figure 7: Effect of MPL Iteration on IDR Interval Sizes
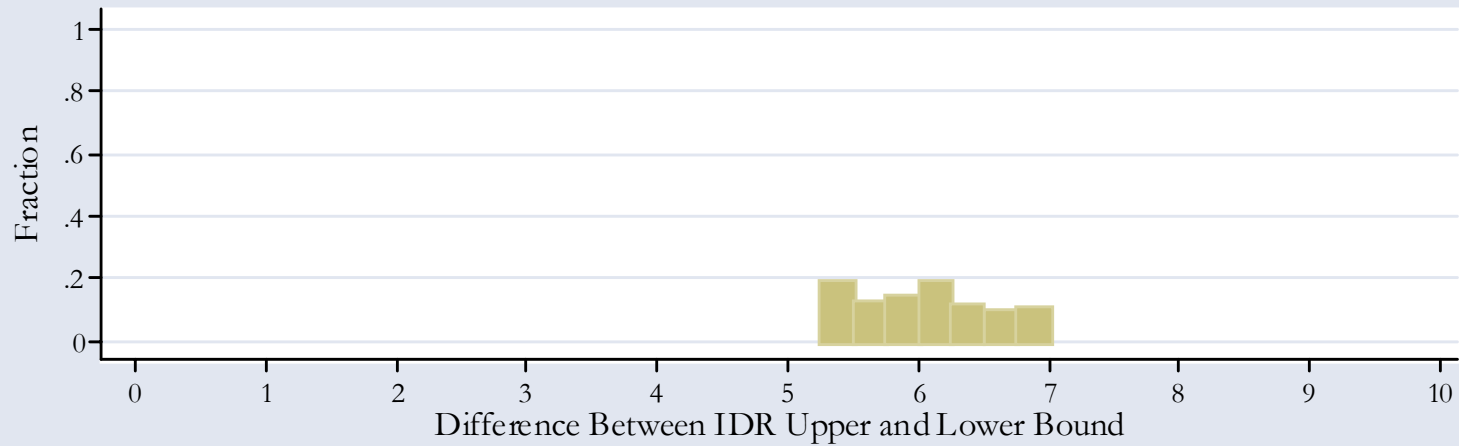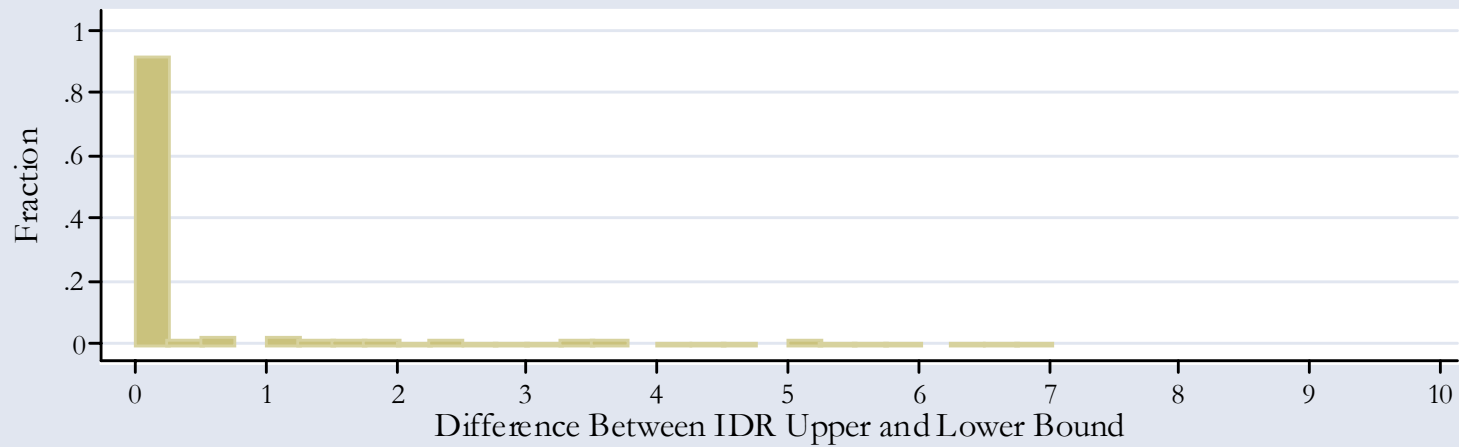
Figure 8: Effect of Frame on Initial Row Selected in Risk Elicitation Task
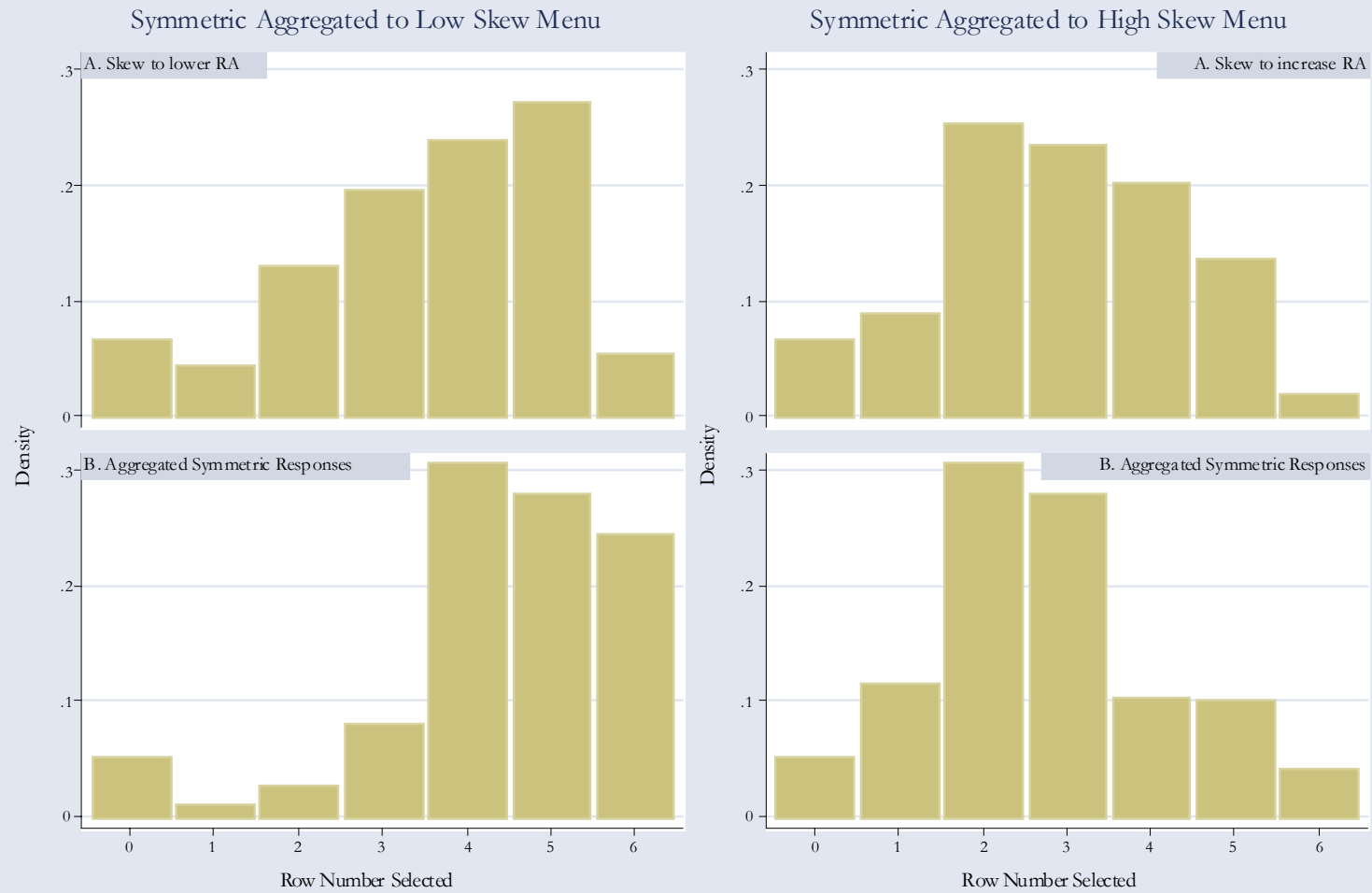
Figure 9: Effect of Frame in Final Task
on CRRA in Denmark

Mid-Point of Raw Responses from iMPL

A. Skew to lower RA

B. Symmetric

C. Skew to increase RA

Density

Constant Relative Risk Aversion weighted to population
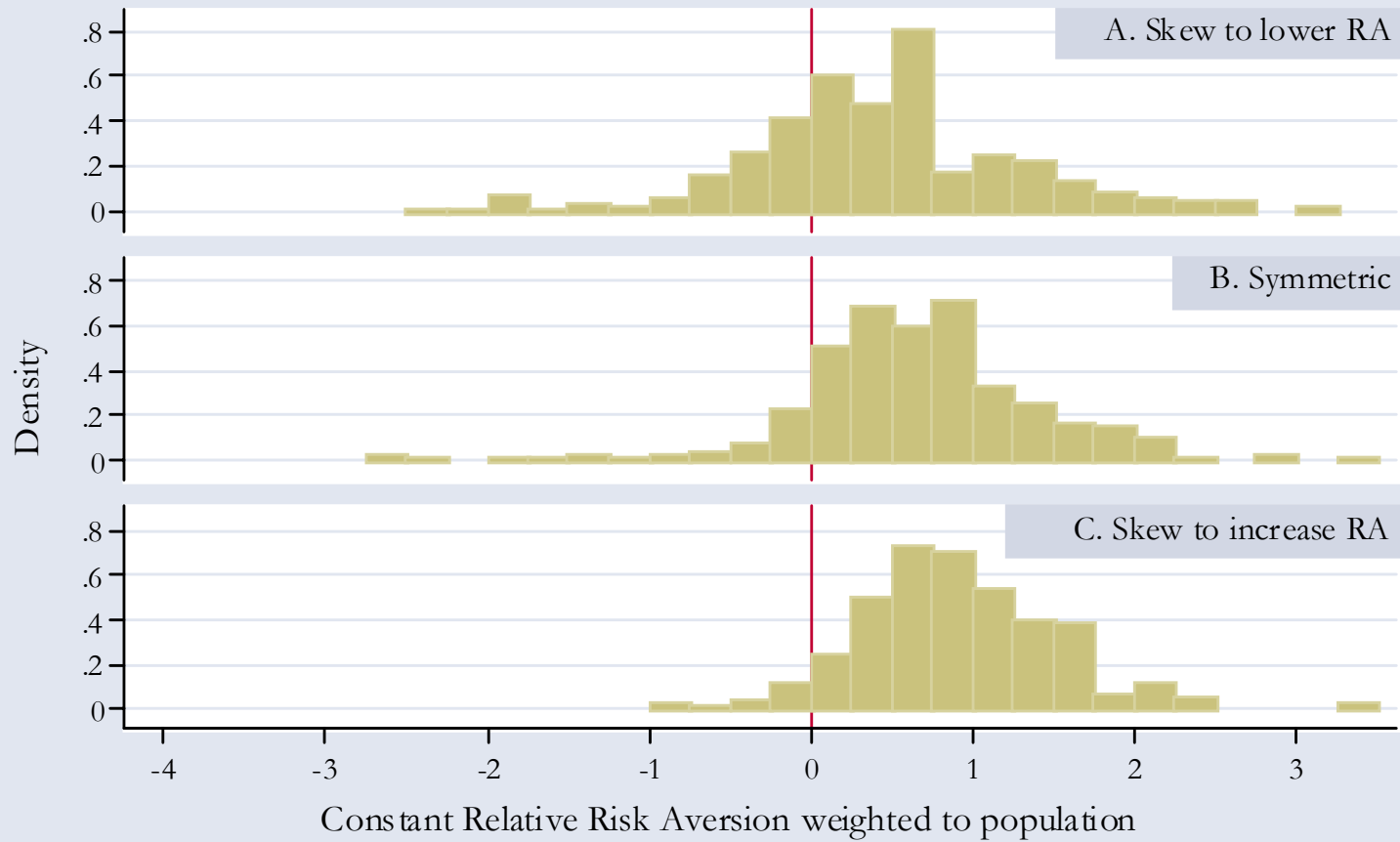
Appendix A: Questionnaires

This appendix presents the survey questions asked of subjects in Parts I and IV of the experiment, as well as the data coding for responses. These are all translations of the original Danish, available on request.

A.1 Part I of the Experiment: Socio-Demographic Questionnaire

In this survey most of the questions asked are descriptive. The questions may seem personal, but they will help us analyze the results of the experiments. Your responses are completely confidential. Please think carefully about each question and give your best answer.

1.   What is your age? _____ years

2.   What is your sex?

    01   Male   02   Female

3.   Where do you live?

    01   Copenhagen including suburbs
    02   Greater Copenhagen area
    03   Municipality with towns of more than 100,000 inhabitants
    04   Municipality with towns of 40,000 – 99,999 inhabitants
    05   Municipality with towns of 20,000 – 39,999 inhabitants
    06   Municipality with towns of 10,000 – 19,999 inhabitants
    07   Other

4.   What type of residence do you live in?

    01   Owner-occupied house
    02   Owner-occupied apartment
    03   Rented house
    04   Rented apartment
    05   Multi-ownership of residence, cooperative
    06   Rented room
    07   Official residence, etc.

5.   What has been your primary occupation during the last 12 months?

[Primary occupation is defined as the type of occupation where you spend most of your working time.]

        01   Farmer
        02   Other self-employed
        03   Assisting spouse
        04   White collar worker
        05   Skilled worker
        06   Unskilled worker
        07   Apprentice
        08   Student
        09   Retired
        10   Unemployed
        11   Other

6.   What is your highest level of education?

        01   Basic school
        02   General upper secondary education
        03   Vocational upper secondary education
        04   Vocational education and training
        05   Short higher education
        06   Medium higher education
        07   Long higher education

A.   *Vocational education and training:*
        01   Commercial and clerical vocational courses
        02   Metal manufacturing vocational courses
        03   Construction vocational courses
        04   Graphic vocational courses
        05   Service-related vocational courses
        06   Food-related vocational courses
        07   Health-related auxiliary programs
        08   Other vocational courses

B.   *Short higher education:*
        01   Social sciences and humanities
        02   Technical and natural sciences
        03   Health-related sciences
        04   Other

C.   *Medium higher education:*
  01   Social sciences
  02   Technical and natural sciences
  03   Health-related sciences
  04   Educational courses and humanities
  05   Officers

D.   *Long higher education:*
  01   Social sciences
  02   Technical and natural sciences
  03   Health-related sciences
  04   Educational courses and humanities
  05   Veterinary and agricultural courses

7.   What are the characteristics of your household?

[A household is an economic unit, and it is defined as a group of persons who live in the same residence and each person contributes to general expenditures.]

  01   Single under 30 years
  02   Single 30 – 59 years
  03   Single older than 59 years
  04   2 adults, oldest person is under 30 years
  05   2 adults, oldest person is 30 – 59 years
  06   2 adults, oldest person is older than 59 years
  07   Single with children, oldest child 0 – 9 years
  08   Single with children, oldest child 10 – 17 years
  09   2 adults with children, oldest child 0 – 9 years
  10   2 adults with children, oldest child 10 – 17 years
  11   Household with at least 3 adults

8.   How many persons (including children) are there in your household?

  01   1 person
  02   2 persons
  03   3 persons
  04   4 persons
  05   5 or more persons

9.   What was the amount of total income before tax earned in 2002 by all members of your household (including children)?

        [Consider all forms of income, including salaries, income from unincorporated business enterprises, pension scheme contributions, interest earnings and dividends, retirement benefits, student grants, scholarship support, social security, unemployment benefits, parental support, alimony, child support, and other types of income.]

        01   Below 150,000 kroner
        02   150,000 – 299,999 kroner
        03   300,000 – 499,999 kroner
        04   500,000 – 799,999 kroner
        05   800,000 kroner or more

10.   How often do you participate in extreme sports?

        [Extreme sports include bungee-jumping, para-gliding, parachute jumping, gliding, rafting, diving and other dangerous sports.]

        01   Never
        02   A few times
        03   Occasionally
        04   Often
        05   Every chance I get

11.   Do you currently smoke cigarettes?

        01   No
        02   Yes

A.   If yes, how much do you smoke in one day? _____ cigarettes

A.2 Questionnaire About Plans with Money in IDR Part

1.   Suppose you win the money today. What do you plan to do with the money you will receive?

    01 Spend 25% or less when you receive the money and save the rest
    02 Spend 26 – 50% when you receive the money and save the rest
    03 Spend 51 – 75% when you receive the money and save the rest
    04 Spend more than 75% when you receive the money and save the rest
    05 Spend 100% when you receive the money

A.3 Part IV of the Experiment: Questionnaire About Finances

In this survey most of the questions asked are descriptive. The questions may seem personal, but they will help us analyze the results of the experiments. Your responses are completely confidential. Please think carefully about each question and give your best answer.

1.   Do you have a checking account?

       01   No
       02   Yes

A.   If yes, what (annual) interest rate does your checking account currently earn?
                _____ %
                _____ Don't Know (88)

B.   What is the current balance on your checking account?

       01   5,000 kroner or less
       02   5,001 – 10,000 kroner
       03   10,001 – 25,000 kroner
       04   25,001 – 50,000 kroner
       05   More than 50,000 kroner
       08   Don't Know

2.   Do you have a line of credit?

       01   No
       02   Yes

A.   If yes, what (annual) interest rate do you currently pay on your line of credit?
                _____ %
                _____ Don't Know (88)

B.   Do you ordinarily carry a balance from month to month on your line of credit?

       01   No
       02   Yes

C.   If yes, what is the balance owed on your line of credit?

          01   1 – 500 kroner
          02   501 – 1,000 kroner
          03   1,001– 5,000 kroner
          04   5,001 – 10,000 kroner
          05   10,001 – 25,000 kroner
          06   25,001 – 50,000 kroner
          07   More than 50,000 kroner
          08   Don't Know

3.   Do you have a credit card?

          01   No
          02   Yes

A.   If yes, what (annual) interest rate do you currently pay on your credit card?

[If you have more than one credit card, please consider the highest interest rate on any credit card with outstanding balances.]

                    _____%
                  _____ Don't Know (88)

C.   What is the balance owed on this credit card?

          01   1 – 500 kroner
          02   501 – 1,000 kroner
          03   1,001– 5,000 kroner
          04   5,001 – 10,000 kroner
          05   10,001 – 25,000 kroner
          06   25,001 – 50,000 kroner
          07   More than 50,000 kroner
          08   Don't Know

3.1   Do you have more than one credit card?

          01   No
          02   Yes

C.   What is the lowest interest rate you currently pay on any credit card with credit left.
                    _____%
                  _____ Don't Know (88)

D.   What is the balance owed on this credit card?

      01   1 – 500 kroner
      02   501 – 1,000 kroner
      03   1,001– 5,000 kroner
      04   5,001 – 10,000 kroner
      05   10,001 – 25,000 kroner
      06   25,001 – 50,000 kroner
      07   More than 50,000 kroner
      08   Don't Know

4.   Do you have outstanding student loan balances?

      01   No
      02   Yes

A.   If yes, what is the (annual) interest rate on your student loan balances?
                    _____ %
                    _____ Don't Know (88)

B.   What is the balance owed on your student loan?

      01   10,000 kroner or less
      02   10,001 – 25,000 kroner
      03   25,001 – 50,000 kroner
      04   50,001 – 100,000 kroner
      05   100,001 – 250,000 kroner
      06   More than 250,000 kroner
      08   Don't Know

5.   Do you have a savings account, excluding contributions to pension schemes?

      01   No
      02   Yes

A.   If yes, what (annual) interest rate does your savings account currently earn?
                    _____ %
                    _____ Don't Know (88)

B.   What is the balance on your savings account?

        01   5,000 kroner or less
        02   5,001 – 10,000 kroner
        03   10,001 – 25,000 kroner
        04   25,001 – 50,000 kroner
        05   50,001 – 100,000 kroner
        06   100,001 – 250,000 kroner
        07   More than 250,000 kroner
        08   Don't Know

6.   Do you have other investment accounts not described above, excluding contributions to pension schemes?

        01   No
        02   Yes

A.   If yes, what (annual) interest rate does your investment account currently earn?

[If you have more than one of these investment accounts, please consider the account currently earning the highest annual interest rate.]

_____%
_____ Don't Know (88)

B.   What is the balance on this investment account?

        01   5,000 kroner or less
        02   5,001 – 10,000 kroner
        03   10,001 – 25,000 kroner
        04   25,001 – 50,000 kroner
        05   50,001 – 100,000 kroner
        06   100,001 – 250,000 kroner
        07   More than 250,000 kroner
        08   Don't Know

7.   If you were to go to the bank to obtain a loan, line of credit, or credit card, what do you think your chances would be of being approved?

        01   At least 90% likely
        02   At least 75% likely
        03   At least 50% likely
        04   Less than 50% likely

8.   How often do you find yourself short of cash between paychecks?

      01   Every time
      02   3 out of 4 times
      03   2 out of 4 times
      04   1 out of 4 times
      05   Almost never

9A.   Would you say that you and your family are better off or worse off financially than you were 1 month ago?

      01   Better now
      02   Same
      03   Worse now
      04   Don't know

9B.   Would you say that you and your family are better off or worse off financially than you were 4 months ago?

      11   Better now
      12   Same
      13   Worse now
      14   Don't know

9C.   Would you say that you and your family are better off or worse off financially than you were 6 months ago?

      21   Better now
      22   Same
      23   Worse now
      24   Don't know

9D.   Would you say that you and your family are better off or worse off financially than you were 12 months ago?

      31   Better now
      32   Same
      33   Worse now
      34   Don't know

9E.   Would you say that you and your family are better off or worse off financially than you were 18 months ago?

        41   Better now
        42   Same
        43   Worse now
        44   Don't know

9F.   Would you say that you and your family are better off or worse off financially than you were 24 months ago?

        51   Better now
        52   Same
        53   Worse now
        54   Don't know

10A.   Now looking ahead, do you expect any major change in your family situation that will lead to higher expenses or lower expenses during the next 1 month?

        01   Higher expenses
        02   No change
        03   Lower expenses
        04   Don't know

10B.   Now looking ahead, do you expect any major change in your family situation that will lead to higher expenses or lower expenses during the next 4 months?

        11   Higher expenses
        12   No change
        13   Lower expenses
        14   Don't know

10C.   Now looking ahead, do you expect any major change in your family situation that will lead to higher expenses or lower expenses during the next 6 months?

        21   Higher expenses
        22   No change
        23   Lower expenses
        24   Don't know

10D.   Now looking ahead, do you expect any major change in your family situation that will lead to higher expenses or lower expenses during the next 12 months?

    31   Higher expenses
    32   No change
    33   Lower expenses
    34   Don't know

10E.   Now looking ahead, do you expect any major change in your family situation that will lead to higher expenses or lower expenses during the next 18 months?

    41   Higher expenses
    42   No change
    43   Lower expenses
    44   Don't know

10F.   Now looking ahead, do you expect any major change in your family situation that will lead to higher expenses or lower expenses during the next 24 months?

    51   Higher expenses
    52   No change
    53   Lower expenses
    54   Don't know

11A.   Do you expect any major change in your family situation that will lead to higher earnings or lower earnings during the next 1 month?

    01   Higher earnings
    02   No change
    03   Lower earnings
    04   Don't know

11B.   Do you expect any major change in your family situation that will lead to higher earnings or lower earnings during the next 4 months?

    11   Higher earnings
    12   No change
    13   Lower earnings
    14   Don't know

11C.   Do you expect any major change in your family situation that will lead to higher earnings or lower earnings during the next 6 months?

       21   Higher earnings
       22   No change
       23   Lower earnings
       24   Don't know

11D.   Do you expect any major change in your family situation that will lead to higher earnings or lower earnings during the next 12 months?

       31   Higher earnings
       32   No change
       33   Lower earnings
       34   Don't know

11E.   Do you expect any major change in your family situation that will lead to higher earnings or lower earnings during the next 18 months?

       41   Higher earnings
       42   No change
       43   Lower earnings
       44   Don't know

11F.   Do you expect any major change in your family situation that will lead to higher earnings or lower earnings during the next 24 months?

       51   Higher earnings
       52   No change
       53   Lower earnings
       54   Don't know

12A.   On balance, do you think that you and your family will be better off or worse off financially 1 month from now?

       01   Will be better off
       02   Same
       03   Will be worse off
       04   Don't know

12B.   On balance, do you think that you and your family will be better off or worse off financially
        4 months from now?

        11   Will be better off
        12   Same
        13   Will be worse off
        14   Don't know

12C.   On balance, do you think that you and your family will be better off or worse off financially
        6 months from now?

        21   Will be better off
        22   Same
        23   Will be worse off
        24   Don't know

12D.   On balance, do you think that you and your family will be better off or worse off financially
        12 months from now?

        31   Will be better off
        32   Same
        33   Will be worse off
        34   Don't know

12E.   On balance, do you think that you and your family will be better off or worse off financially
        18 months from now?

        41   Will be better off
        42   Same
        43   Will be worse off
        44   Don't know

12F.   On balance, do you think that you and your family will be better off or worse off financially
        24 months from now?

        51   Will be better off
        52   Same
        53   Will be worse off
        54   Don't know

13A.   Turning to the economic conditions in the country as a whole, would you say that at the present time economic conditions are better or worse than they were 1 month ago?

        01   Better now
        02   Same
        03   Worse now
        04   Don't know

13B.   Turning to the economic conditions in the country as a whole, would you say that at the present time economic conditions are better or worse than they were 4 months ago?

        11   Better now
        12   Same
        13   Worse now
        14   Don't know

13C.   Turning to the economic conditions in the country as a whole, would you say that at the present time economic conditions are better or worse than they were 6 months ago?

        21   Better now
        22   Same
        23   Worse now
        24   Don't know

13D.   Turning to the economic conditions in the country as a whole, would you say that at the present time economic conditions are better or worse than they were 12 months ago?

        31   Better now
        32   Same
        33   Worse now
        34   Don't know

13E.   Turning to the economic conditions in the country as a whole, would you say that at the present time economic conditions are better or worse than they were 18 months ago?

        41   Better now
        42   Same
        43   Worse now
        44   Don't know

13F. Turning to the economic conditions in the country as a whole, would you say that at the present time economic conditions are better or worse than they were 24 months ago?

    51   Better now
    52   Same
    53   Worse now
    54   Don't know

14A. Do you think that there will be more or less unemployment during the next 1 month?

    01   More unemployment
    02   About the same
    03   Less unemployment
    04   Don't know

14B. Do you think that there will be more or less unemployment during the next 4 months?

    11   More unemployment
    12   About the same
    13   Less unemployment
    14   Don't know

14C. Do you think that there will be more or less unemployment during the next 6 months?

    21   More unemployment
    22   About the same
    23   Less unemployment
    24   Don't know

14D. Do you think that there will be more or less unemployment during the next 12 months?

    31   More unemployment
    32   About the same
    33   Less unemployment
    34   Don't know

14E. Do you think that there will be more or less unemployment during the next 18 months?

    41   More unemployment
    42   About the same
    43   Less unemployment
    44   Don't know

14F.   Do you think that there will be more or less unemployment during the next 24 months?

        51   More unemployment
        52   About the same
        53   Less unemployment
        54   Don't know

15A.   Do you think that interest rates for borrowing money will go up or go down during the next
        1 month?

        01   Go up
        02   Stay the same
        03   Go down
        04   Don't know

15B.   Do you think that interest rates for borrowing money will go up or go down during the next
        4 months?

        11   Go up
        12   Stay the same
        13   Go down
        14   Don't know

15C.   Do you think that interest rates for borrowing money will go up or go down during the next
        6 months?

        21   Go up
        22   Stay the same
        23   Go down
        24   Don't know

15D.   Do you think that interest rates for borrowing money will go up or go down during the next
        12 months?

        31   Go up
        32   Stay the same
        33   Go down
        34   Don't know

15E. Do you think that interest rates for borrowing money will go up or go down during the next 18 months?

      41   Go up
      42   Stay the same
      43   Go down
      44   Don't know

15F. Do you think that interest rates for borrowing money will go up or go down during the next 24 months?

      51   Go up
      52   Stay the same
      53   Go down
      54   Don't know

Appendix B: Sample Design

This appendix adds detail to the documentation of the sample design presented in the body of the paper.

B.1 Overall Design

The sample for the field experiments was designed to generate a representative sample of the adult Danish population.[53] There were six steps in the construction of the sample:

- First, a random sample of 25,000 Danes was drawn from the Danish Civil Registration Office in January 2003. Only Danes born between 1927 and 1983 were included, thereby restricting the age range of the target population to between 19 and 75. For each person in this random sample we had access to their name, address, county, municipality, birth date, and sex. 16 of the records had no name and address and were dropped, and another 12 of the records had no address and were also dropped.
- Second, we dropped 17 municipalities (including one county) from the population, due to them being located in extraordinarily remote locations. The population represented in these locations amounts to less than 2% of the Danish population, and only 493 individuals in our sample from the civil registry.
- Third, we assigned each county either 1 session or 2 sessions, in rough proportionality to the population of the county. In total we assigned 21 sessions. Each session consisted of two sub-sessions at the same locale and date, one at 5pm and another at 8pm, and subjects were allowed to choose which sub-session suited them best.
- Fourth, we divided 6 counties into two sub-groups because the distance between some municipalities in the county and the location of the session would be too large. A random draw was made between the two sub-groups and the location selected, where the weights reflect the relative size of the population in September 2002.
- Fifth, we picked the first 30 or 60 randomly sorted records within each county, depending on the number of sessions allocated to that county. This provided a sub-sample of 600, which we then contacted by mail.
- Sixth, we sent out 600 invitations to attend a session, offering each person a choice of times for the session. Response rates were low in some counties and another 64 invitations were sent out. We signed up everyone that gave a positive response, and our final recruited sample was 268. In the end, we had 253 persons actually turn up for the sessions.

We explain below how we use this information to generate sample weights for the statistical analysis.

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[53] Harrison, Lau and Williams [2002] relied on the sample frames developed by the Danish Social Research Institute (SFI) for their sample, and also used SFI personnel to conduct the field experiments. Given the substantial cost of using such survey firms, we decided to undertake the sampling and experiments ourselves. SFI had a sample of around 5,000 participants from which they picked subjects for the previous experiments. Their show up rate among recruited persons was 85%, which we viewed as quite high for the field, but those persons had previously been interviewed several times by the SFI.

B.2 List of Danish Municipalities and County Codes

Each of the 275 municipalities and 15 counties in the sample has a code, listed below. The 17 municipalities that were dropped due to logistical problems were: 401, 403, 405, 407, 409, 443, 475, 481, 487, 493, 501, 523, 535, 563, 741, 675 and 825. The 6 counties allocated two sessions were: 1, 15, 20, 42, 70 and 80. The 6 counties divided into two sub-groups were: 30, 35, 50, 60, 65 and 76.

Name	County code	Municipality code	Sub-group
København	1	101	
Frederiksberg	1	147	
Københavns Amt	15		
Ballerup	15	151	
Brøndby	15	153	
Dragør	15	155	
Gentofte	15	157	
Gladsaxe	15	159	
Glostrup	15	161	
Herlev	15	163	
Albertslund	15	165	
Hvidovre	15	167	
Høje Taastrup	15	169	
Ledøje-Smørum	15	171	
Lyngby-Taarbæk	15	173	
Rødovre	15	175	
Søllerød	15	181	
Ishøj	15	183	
Tårnby	15	185	
Vallensbæk	15	187	
Værløse	15	189	
Frederiksborg Amt	20		
Allerød	20	201	
Birkerød	20	205	
Farum	20	207	
Fredensborg-Humlebæk	20	208	
Frederikssund	20	209	
Frederiksværk	20	211	
Græsted-Gilleleje	20	213	
Helsinge	20	215	
Helsingør	20	217	
Hillerød	20	219	
Hundested	20	221	
Hørsholm	20	223	
Jægerspris	20	225	
Karlebo	20	227	
Skibby	20	229	
Skævinge	20	231	
Slangerup	20	233	
Stenløse	20	235	
Ølstykke	20	237	
Roskilde Amt	25		
Bramsnæs	25	251	

Greve	25	253	
Gundsø	25	255	
Hvalsø	25	257	
Køge	25	259	
Lejre	25	261	
Ramsø	25	263	
Roskilde	25	265	
Skovbo	25	267	
Solrød	25	269	
Vallø	25	271	
Vestsjællands Amt	30		
Bjergsted	30	301	2
Dianalund	30	303	1
Dragsholm	30	305	2
Fuglebjerg	30	307	1
Gørlev	30	309	1
Hashøj	30	311	1
Haslev	30	313	1
Holbæk	30	315	2
Hvidebæk	30	317	1
Høng	30	319	1
Jernløse	30	321	2
Kalundborg	30	323	1
Korsør	30	325	1
Nykøbing-Rørvig	30	327	2
Ringsted	30	329	1
Skælskør	30	331	1
Slagelse	30	333	1
Sorø	30	335	1
Stenlille	30	337	1
Svinninge	30	339	2
Tornved	30	341	2
Trundholm	30	343	2
Tølløse	30	345	2
Storstrøms Amt	35		
Fakse	35	351	3
Fladså	35	353	3
Holeby	35	355	4
Holmegaard	35	357	3
Højreby	35	359	4
Langebæk	35	361	3
Maribo	35	363	4
Møn	35	365	3
Nakskov	35	367	4
Nykøbing-Falster	35	369	4
Nysted	35	371	4
Næstved	35	373	3
Nørre Alslev	35	375	4
Præstø	35	377	3
Ravnsborg	35	379	4
Rudbjerg	35	381	4
Rødby	35	383	4

Rønnede	35	385	3
Sakskøbing	35	387	4
Stevns	35	389	3
Stubbekøbing	35	391	4
Suså	35	393	3
Sydfalster	35	395	4
Vordingborg	35	397	3
Bornholms Amt	40		
Allinge-Gudhjem	40	401	
Hasle	40	403	
Nexø	40	405	
Rønne	40	407	
Aakirkeby	40	409	
(Uden For Kommuner)		411	
Fyns Amt	42		
Assens	42	421	
Bogense	42	423	
Broby	42	425	
Egebjerg	42	427	
Ejby	42	429	
Faaborg	42	431	
Glamsbjerg	42	433	
Gudme	42	435	
Haarby	42	437	
Kerteminde	42	439	
Langeskov	42	441	
Marstal	42	443	
Middelfart	42	445	
Munkebo	42	447	
Nyborg	42	449	
Nørre Aaby	42	451	
Odense	42	461	
Otterup	42	471	
Ringe	42	473	
Rudkøbing	42	475	
Ryslinge	42	477	
Svendborg	42	479	
Sydlangeland	42	481	
Søndersø	42	483	
Tommerup	42	485	
Tranekær	42	487	
Ullerslev	42	489	
Vissenbjerg	42	491	
Ærøskøbing	42	493	
Ørbæk	42	495	
Årslev	42	497	
Aarup	42	499	
Sønderjyllands Amt	50		
Augustenborg	50	501	6
Bov	50	503	6
Bredebro	50	505	6
Broager	50	507	6

Christiansfeld	50	509	5
Gram	50	511	5
Gråsten	50	513	6
Haderslev	50	515	5
Højer	50	517	6
Lundtoft	50	519	6
Løgumkloster	50	521	6
Nordborg	50	523	6
Nørre Rangstrup	50	525	5
Rødding	50	527	5
Rødekro	50	529	6
Skærbæk	50	531	5
Sundeved	50	533	6
Sydals	50	535	6
Sønderborg	50	537	6
Tinglev	50	539	6
Tønder	50	541	6
Vojens	50	543	5
Aabenraa	50	545	6
Ribe Amt	55		
Billund	55	551	
Blåbjerg	55	553	
Blåvandshuk	55	555	
Bramming	55	557	
Brørup	55	559	
Esbjerg	55	561	
Fanø	55	563	
Grindsted	55	565	
Helle	55	567	
Holsted	55	569	
Ribe	55	571	
Varde	55	573	
Vejen	55	575	
Ølgod	55	577	
Vejle Amt	60		
Brædstrup	60	601	8
Børkop	60	603	7
Egtved	60	605	7
Fredericia	60	607	7
Gedved	60	609	8
Give	60	611	7
Hedensted	60	613	8
Horsens	60	615	8
Jelling	60	617	7
Juelsminde	60	619	8
Kolding	60	621	7
Lunderskov	60	623	7
Nørre Snede	60	625	8
Tørring-Uldum	60	627	8
Vamdrup	60	629	7
Vejle	60	631	7
Ringkøbing Amt	65		

Aulum-Haderup	65	651	9
Brande	65	653	9
Egvad	65	655	9
Herning	65	657	9
Holmsland	65	659	9
Holstebro	65	661	10
Ikast	65	663	9
Lemvig	65	665	10
Ringkøbing	65	667	9
Skjern	65	669	9
Struer	65	671	10
Thyborøn-Harboøre	65	673	10
Thyholm	65	675	10
Trehøje	65	677	9
Ulfborg-Vemb	65	679	10
Videbæk	65	681	9
Vinderup	65	683	10
Åskov	65	685	9
Århus Amt	70		
Ebeltoft	70	701	
Galten	70	703	
Gjern	70	705	
Grenaa	70	707	
Hadsten	70	709	
Hammel	70	711	
Hinnerup	70	713	
Hørning	70	715	
Langå	70	717	
Mariager	70	719	
Midtdjurs	70	721	
Nørhald	70	723	
Nørre Djurs	70	725	
Odder	70	727	
Purhus	70	729	
Randers	70	731	
Rosenholm	70	733	
Rougsø	70	735	
Ry	70	737	
Rønde	70	739	
Samsø	70	741	
Silkeborg	70	743	
Skanderborg	70	745	
Sønderhald	70	747	
Them	70	749	
Århus	70	751	
Viborg Amt	76		
Bjerringbro	76	761	11
Fjends	76	763	11
Hanstholm	76	765	12
Hvorslev	76	767	11
Karup	76	769	11
Kjellerup	76	771	11

Morsø	76	773	12
Møldrup	76	775	11
Sallingsund	76	777	11
Skive	76	779	11
Spøttrup	76	781	11
Sundsøre	76	783	11
Sydthy	76	785	12
Thisted	76	787	12
Tjele	76	789	11
Viborg	76	791	11
Aalestrup	76	793	11
Nordjyllands Amt	80		
Arden	80	801	
Brovst	80	803	
Brønderslev	80	805	
Dronninglund	80	807	
Farsø	80	809	
Fjerritslev	80	811	
Frederikshavn	80	813	
Hadsund	80	815	
Hals	80	817	
Hirtshals	80	819	
Hjørring	80	821	
Hobro	80	823	
Læsø	80	825	
Løgstør	80	827	
Løkken-Vrå	80	829	
Nibe	80	831	
Nørager	80	833	
Pandrup	80	835	
Sejlflod	80	837	
Sindal	80	839	
Skagen	80	841	
Skørping	80	843	
Støvring	80	845	
Sæby	80	847	
Aabybro	80	849	
Aalborg	80	851	
Aars	80	861	

B.3 Map of Denmark

B.4 Recruitment Procedures

We sent out 600 invitations to attend a session in the first recruitment wave, offering each person a choice of two times for the session. The first 30 or 60 randomly sorted records were picked within each county, depending on the number of sessions allocated to that county. Response rates were low in some counties and another 45 and 19 invitations were sent out in the second and third wave, respectively. A total of 664 invitations were sent out.

The first wave of invitations were sent out four weeks before the first session was scheduled, and we asked people to reply within one week. The second and third waves of invitations were sent out two and three weeks after the first wave, respectively.

Number of invitations across counties

County	Wave1	Wave2	Wave3	Total
1	60			60
15	60			60
20	60	12		72
25	30	6		36
30	30	7	1	38
35	30	20		50
42	60			60
50	30		8	38
55	30		10	40
60	30			30
65	30			30
70	60			60
76	30			30
80	60			60
Total	600	45	19	664

We signed up everyone that gave a positive response, and our final recruited sample was 268. The response rate was 42.5 percent for the first wave, 20 percent for the second wave and 22.1 percent for the third wave.

Number of recruited persons across counties

County	Wave1	Wave2	Wave3	Total
1	31			31
15	28			28
20	25	1		26
25	12			12
30	11	1	1	13
35	6	7		13
42	25			25
50	12			12
55	10		3	13
60	15			15
65	13			13

County	Wave1	Wave2	Wave3	Total
70	25			25
76	16			16
80	26			26
Total	255	9	4	268

Attendance at the experimental sessions was extraordinarily high. A total of 253 persons participated in the experiments. Four persons turned up for their session, but were not able to participate in the experiments. The first person suffered from dementia and could not remember the instructions; the second person was a 76 year old woman who was not able to control the mouse and eventually gave up; the third person had just won the world championship in sailing and was too busy with interviews to stay for two hours; and the fourth person was sent home because too many people showed up (one person came unexpected, and we had only ten laptops available at that session). Four persons showed up unexpected and participated in the experiments.

Certain events might have plausibly triggered some of the no-shows: for example, 3 men did not turn up on June 11, 2003, but that was the night that the Danish national soccer team played a qualifying game for the European championships against Luxembourg that had been unscheduled when we picked session dates.

Number of people attending across counties

County	Wave1	Wave2	Wave3	Total
1	30			30
15	26			26
20	23	1		24
25	12			12
30	9		1	10
35	6	7		13
42	23			23
50	12			12
55	10		4	14
60	15			15
65	12			12
70	22			22
76	15			15
80	25			25
Total	240	8	5	253

We assigned each county either 1 session or 2 sessions, in rough proportionality to the population of the county. We assigned initially 20 sessions. Each session consisted of two sub-sessions at the same locale and date, one at 5pm and another at 8pm, and subjects were allowed to choose which sub-session suited them best. Some late sessions had only one or two subjects signed up, and we contacted these subjects by phone and asked them to participate in the early session. One additional session was held on June 24 because midsummer eve on June 23 was unscheduled when we picked the session dates. Subjects scheduled for the June 23 session were contacted by phone and could choose which date suited them best.

The letter of invitation included an answer form and a prepaid envelope, and they were asked to answer within one week. The same day we received the answer form, a reply letter was sent

confirming their participation in the meeting at the given location, date and time. Every recruited subject was reminded by mail or phone within one week before the meeting. Both procedures were used for the first three sessions, and attendance was almost 100 percent at these sessions. We reminded subjects by mail for the remaining sessions because this procedure is more convenient.

Subjects were provided with three treatments in the risk aversion task. The symmetric treatment offered ten initial probabilities of 0.1, 0.2, 0.3, ...,0.9 and 1. In one case the menu was skewed to lower elicited RA and offered six initial probabilities of 0.1, 0.2, 0.3, 0.5, 0.7 and 1. In another case the menu was skewed to increase elicited RA and offered six initial probabilities of 0.3, 0.5, 0.7, 0.8, 0.9 and 1. The same RA treatment was provided to each subject in the same sub-session. Subject #37 was accidently assigned the *SkewLO* treatment instead of the *SkewHI* treatment in the late session #15.

Treatments across sessions

Session	Date	Time	County	Recruitment	Attendance	Reminder	Interviewer	RA
1	3/6	17:00	1	16	16	mail	0	High
1	3/6	20:00	1	9	8	phone	1	Sym
2	2/6	17:00	1	6	6	phone	0	Sym
3	10/6	17:00	15	10	9	mail	1	Sym
3	10/6	20:00	15	3	3	mail	1	High
4	16/6	17:00	15	10	10	mail	1	Low
4	16/6	20:00	15	4	4	mail	1	Sym
5	23/6	17:00	20	5	5	phone	1	High
6	4/6	17:00	20	7	6	phone	1	Sym
6	4/6	20:00	20	6	6	phone	1	Low
7	4/6	17:00	25	8	8	mail	0	Sym
7	4/6	20:00	25	4	4	mail	0	Low
9	11/6	17:00	30	4	3	mail	1	Sym
9	11/6	20:00	30	9	7	mail	1	Low
10	12/6	17:00	35	5	5	mail	1	Sym
10	12/6	20:00	35	8	8	mail	1	High
12	17/6	17:00	42	10	7	mail	1	High
12	17/6	20:00	42	9	10	mail	1	Sym
13	23/6	17:00	42	7	6	phone	0	Low
15	10/6	17:00	50	9	8	mail	0	Sym
15	10/6	20:00	50	3	4	mail	0	High
16	18/6	17:00	55	8	9	mail	1	Low
16	18/6	20:00	55	5	5	mail	1	Sym
17	11/6	17:00	60	8	8	mail	0	Sym
17	11/6	20:00	60	7	7	mail	0	Low
20	19/6	17:00	65	10	9	mail	1	High
20	19/6	20:00	65	3	3	mail	1	Sym
21	12/6	17:00	70	6	5	mail	0	Sym
22	19/6	17:00	70	11	10	mail	0	High
22	19/6	20:00	70	8	7	mail	0	Sym
23	18/6	17:00	76	10	10	mail	0	Low
23	18/6	20:00	76	6	5	mail	0	Sym

25	17/6	17:00	80	6	6	mail	0	High
25	17/6	20:00	80	3	2	mail	0	Sym
26	16/6	17:00	80	10	10	mail	0	Low
26	16/6	20:00	80	7	7	mail	0	Sym
35	24/6	17:00	20	8	7	phone	1	Sym

These documents are translations from the original Danish, available on request. They were sent out under the letterhead of the Ministry of Business and Economic Affairs.

Economic decisions

Dear _____

In daily life you make a number of decisions on how to spend your money. Some decisions concern the future. Should you consume now, or should you save the money and consume later? Should you buy or rent a home? Should you work or get additional education? To find out how Danes respond to these questions, the Ministry of Economic and Business Affairs will carry out a survey. The survey is financed by the Social Research Council and is conducted by researchers from the ministry's research unit and from the United States. This is the second analysis of this kind in Denmark.

You are chosen to participate

200 persons participate from all over the country. We have found the names by random choice from the Central Office of Civil Registration. The survey implies that a small number of people will get together and answer the questions. We would therefore like to invite you to participate in one of these meetings that will take place:

5:00 pm or 8:00 pm, _____ day, ___/___ 2003 at: _____

_____

We will ask you to mark your preferred time for the meeting in the attached answer form.

You can win a significant amount

To cover travel costs, you will receive 500 kroner at the end of the meeting. Moreover, each participant will have a 10 percent chance of receiving an amount between 50 and 4,500 kroner in the first part of the survey, and this amount will also be paid at the end of the meeting. In the second part of the survey, each participant will have a 10 percent chance of receiving at least 3,000 kroner. A random choice will decide who win the money in both parts of the survey. All amounts are subject to personal income taxation and will be recorded at the tax authorities.

It is important that you answer…

But it is voluntary to participate. Your answers will be strictly confidential, and the results will be published in a way that no single person can be identified. The meeting will last at most 2 hours. We ask you to return the attached answer form within a week. Please find attached a stamped envelope.

If you have any questions or would like to know more about the survey, please call Steffen Andersen at 35 46 63 21 or the interview leader, see below. If you have problems with travel expenses, please contact the interview leader and travel arrangements will be made.

With best regards,

Thank you for your help

Morten I. Lau   _____
Interview leader   Interview leader
Tel.: 35 46 62 54

Economic decisions

I, _____, hereby confirm that I would like to participate in the meeting

_____day, the ___/___ 2003 at: _____

_____

17:00 hours _____
20:00 hours _____

Please mark your preferred time for the meeting.

I acknowledge that my travel costs are covered by 500 kroner, and all amounts paid at the meeting are subject to personal income taxation. In case we need to contact you, please provide your phone number below.

Thank you for confirming your participation in the meeting.

Address: _____

Telephone: _____

Economic decisions

Dear _____

Thank you for confirming your participation in the meeting:

17:00/20:00 hours, _____ day, the ___/___ 2003 at: _____

_____

The meeting will begin with a short introduction of the survey, and we will then ask you to answer a number of questions. We will serve coffee, tea and cake. The meeting will last at most 2 hours. The 500 kroner to cover your travel costs and prizes in the first part of the survey will be paid before you leave the meeting.

With best regards,

_____

Morten I. Lau

Interview leader

Economic decisions

Dear _____

We hereby confirm that the meeting will take place:

17:00/20:00 hours, _____ day, the ___/___ 2003 at: _____

_____

The meeting will begin with a short introduction of the survey, and we will then ask you to answer a

number of questions. We will serve coffee, tea and cake. The meeting will last at most 2 hours. The 500 kroner to cover your travel costs and prizes in the first part of the survey will be paid before you leave the meeting.

With best regards,

_____

Morten I. Lau
Interview leader

Appendix C: Experimenter Script

This appendix reproduces the script followed by the experimenters during the experimental sessions. This script contains the instructions on the subjects' computer screens, as well as additional interviewer directions and explanations that were necessary for the conduct of the sessions.

Welcome announcement

[Give letter of invitation to subjects.]

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

Recall from the letter of invitation that you will be paid 500 kroner for your participation to cover travel costs. In order to qualify for this compensation you need to stay the full two hours of this session. Is everyone able to stay for the full two hours? Please make sure your mobile phones are turned off to avoid interruptions during the meeting.

[If somebody is not, take them outside. Give them 100 kroner and send them home.]

You will be given instructions and practice opportunities for the tasks today on the computer screen in front of you.

[Give handouts for Part II to subjects: computer screen examples and practice record sheets.]

Before we start I would like to ask one person to come up here and inspect the two bingo cages that we will use several times during today's session. Please verify that we have here 100 balls numbered from 1 to 100, and here 10 balls numbered 1 to 10. I will now ask you to place these balls into the bingo cages. Please take your seat again.

I will now come around and enter your subject ID numbers on the computers. We will then read through the instructions together. Please wait for me to finish.

WELCOME TO THE EXPERIMENT
THESE ARE YOUR INSTRUCTIONS

This is an experiment in the economics of decision making. Your participation in this experiment is voluntary. However, we think you will find the experiment interesting. You will be paid for your participation *and* you could make a considerable amount of additional money. The instructions are simple and you will benefit from following them carefully. Please take a few minutes to read them through together with me.

In this experiment you may receive some money from us in addition to the guaranteed participation fee. How much you receive will depend partly on chance and partly on the choices you make in series of decision-problems which will be presented to you in a few minutes.

The problems are not designed to test you. What we want to know is what choices you would make in them. The only right answer is what you really would choose. That is why the problems give you the chance of winning real money.

The experiment will proceed in four parts.

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

Part II is a decision problem in which chance may play a part. Your decision problem requires you to make a series of choices between two options. This is described in more detail later.

Part III is a different decision problem in which chance may play a part. We will describe this further after you have completed the second part.

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

At the end we will ask you to step aside for a moment and then call you back in, one at a time, to pay you in private.

At this time we ask that you answer the questions for Part I. Just click the OK button to go on.

Password 1: 1

Instructions for Part II

We will now continue with Part II of the experiment.

Each person in this room will have a chance to receive an additional large sum of money. If you are selected to receive this sum of money, you will have a choice between two payment options; option A or option B. Each person will have a 1-in-10 chance of receiving the money. The selection will be done using a ten-sided die. If the number 0 is drawn you will receive the money at the end of the meeting. If any other number is drawn you will not receive the money.

You will be asked to make a series of choices in a decision problem which may have multiple levels. The table shown on page 1 in the handout is an illustration of what <u>Level 1</u> of the decision problem will look like on your computer screen. This handout contains several other screen images we will mention later.

This screen illustration shows ten decisions listed on the left side, in the column marked **Decision**. Each decision is a paired choice between "Option A" and "Option B." You will be asked to make a choice between these two options in each decision row.

Before you start thinking about your choice, let me explain how your choice affects your earnings. Earnings depend partly on the outcome of a spin of the bingo cage you see in this room. When the bingo cage is spun, a single ball will be randomly picked from all the balls in the bingo cage, and the number on the ball will in part determine your earnings. The bingo cage contains 100 balls which are individually numbered from 1 to 100, so any number between 1 and 100 is equally likely to be chosen.

Please look at decision 1 at the top of the table. Option A pays $100 if the bingo ball is numbered 10 or lower, and it pays $80 if the bingo ball is numbered 11 or higher. This means that there is a 10-in-100 chance of getting $100 and a 90-in-100 chance of getting $80.

Option B yields $170 if the bingo ball is numbered 10 or lower , and it pays $5 if the bingo ball is numbered 11 or higher.

The difference between the two amounts in option A is smaller than the difference between the two amounts in option B.

The other decisions are similar, except that as you move down the table the chances of the higher payoff for each option increase. In fact, for decision 10 in the bottom row, the bingo cage will not be needed since each option pays the highest payoff for sure. So your choice in decision 10 is simply between $100 or $170.

For each of the ten decisions, you will be asked to choose Option A or Option B by clicking on the appropriate button. These buttons are shown on the right of the screen illustration. For some decisions you may not care whether you receive Option A or B, in which case you should click the button labeled "I" for "Indifference."

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

- You may prefer Option A for <u>all</u> decision rows;
- You may be Indifferent between Option A and Option B for <u>all</u> decision rows;
- You may prefer Option B for <u>all</u> decision rows; or
- You may prefer Option A for <u>some</u> decision rows, Option B for <u>some</u> decision rows, and be Indifferent for other decision rows.

Which kind of decision you make is entirely up to you.

If you select Option A for all decision rows, or if you indicate Indifference for any of the decision rows, there will be no further choices to be made by you <u>in this problem</u> before determining your earnings.

If you select Option B for all decisions rows, or if you switch from Option A to Option B at some point, we will give you a <u>Level 2</u> task before determining your earnings. The Level 2 task involves making choices in the Level 2 table illustrated on page 2 in the handout.

The Level 2 table shows you eleven other decisions listed in a similar way. They are arranged in the same way as the ten decisions in the Level 1 table, but they focus in on the decisions that were made in Level 1.

Assume that someone in Level 1 has selected Option A for rows 1-3 and Option B for rows 4-10. This means that this person prefers Option A when the chances of earning the higher amount is 30-in-100 or less, but prefers Option B when the chances of earning the higher amount are 40-in-100 or more. Level 2 then asks this person to choose between Option A and Option B for chances <u>between</u> 30-in-100 and 40-in-100. Thus, row 1 in Level 2 corresponds to a chance of 30-in-100 for the higher amount, and row 2 to a chance of 31-in-100 for the higher amount, and so on until the last row shows a 40-in-100 chance of earning the higher amount. Thus <u>Level 2 just provides more detail in the range of choices this person indicated in Level 1.</u>

Notice that the chance of winning the higher amount in Level 1 increases by intervals of 10-in-100, or 10 percentage points, as you move from decision row 1 to decision row 2. The same increase in chances applies to each row in Level 1. Notice also that the decisions displayed in Level 2 are determined by the row where you first choose Option B over Option A in Level 1. Level 2 simply takes the interval between the point where you last chose A and first chose B, and divides that interval of 10 percentage points into 11 narrower intervals. Thus the chance of winning the higher amount in Level 2 increases by intervals of 1-in-100, or 1 percentage point, as you move from decision row 1 to decision row 2.

As you can see, you have a minimum of 10 decisions to make. You will have 21 decisions if you make the kind of decision in Level 1 that moves you to Level 2. Nevertheless, we will pay you for only one of these decisions. After you have made all of your choices we will use the bingo cage to select which decision will be used to determine your payment. To decide which decision row will determine your payment, we will spin the bingo cage you see in this room and withdraw one ball. The bingo cage contains 10 balls, numbered individually from 1 to 10. The number on the bingo ball determines the decision row you will play out. Thus if the number is 2, you will play out decision row 2. If the number is 9, you will play out decision 9. Each decision row is therefore equally likely to be chosen.

In the example above, if the number "4" bingo ball is withdrawn, that will take this person to Level 2 since this person switched from A to B on row 4 in Level 1. In such a case, we will need to add a number 11 ball to the bingo cage and spin the cage again to determine which decision in Level 2 is binding.

Once we know which choice is binding, we will spin the bingo cage that contains 100 balls to see if you will receive the higher amount or the lower amount for the choice that you made. Thus if you chose Option A, you would be paid the appropriate amount in Option A; if you chose Option B, you would be paid the appropriate amount in Option B.

If the number on the bingo ball corresponds to a row for which you have expressed Indifference, we will first let yet another spin of the bingo cage determine which choice of A or B will determine your earnings. In this case a number between 1 and 50 means that A will be chosen, and a number between 51 and 100 will mean that B is chosen. Hence each option has an equal chance of determining your earnings if you expressed Indifference. This will be done before we spin the bingo cage to determine whether you will be paid the high or the low amount.

Password 2: 2

EXAMPLE 1

To make these procedures very clear to you we are going to go through a few examples. In these examples we will show you how we will spin the bingo cage and how the number on the bingo ball will determine the decision that is binding and then the payment you will receive. You will not be paid for these practice examples, but they will help you understand how the procedures work when you do make decisions for payment.

As you can see on the screen illustration on page 1, we used a decision table that is already filled in. At this time we ask that you fill in answers on your computer that correspond to this illustration. On the computer you will see only the Level 1 table first. The Level 2 table will be shown on a subsequent screen. When you have finished filling in Level 1, but before you click the submit button at the bottom on the screen, please raise your hand and we will come and verify that you have done it correctly.

PLEASE WAIT UNTIL THE EXPERIMENTER ANNOUNCES THAT THE PRACTICE IS CONTINUING.

Since the illustration is for a case where somebody has selected A for some decision rows and then switched to B in decision row 4, the next screen shows you Level 2. In this illustration Option A was selected in Level 1 when the chance for high earnings was 30-in-100 and Option B was selected when the chance was 40-in-100. So Level 2 corresponds to chances between these two values.

Please fill in answers on your computer that correspond to the illustration of the Level 2 table for Example 1 in your handout. When you have finished filling in the table, but before you click the submit button at the bottom on the screen, please raise your hand and we will come and verify that you have done it correctly.

PLEASE WAIT UNTIL THE EXPERIMENTER ANNOUNCES THAT THE PRACTICE IS CONTINUING.

We are now going to illustrate a number of different possible outcomes from the spin of the bingo cage. Remember that we are going to spin the cage both to determine which decision row is the binding one, and also to determine what the payment is for that row, conditional on the choice between Option A, Option B and Indifference that you made.

Password 3: 4

EXPERIMENTER SCRIPT

[SUBJECTS USE PRACTICE RECORD SHEETS, AND EXPERIMENTER USES A PAPER POSTER BOARD.]

We will first spin the bingo cage with 10 balls to determine which decision row in Level 1 is the binding one for payment. This is just an illustration so we are not paying for these decisions.

[SPIN CAGE WITH 10 BALLS.]

[FOR BALL NOT EQUAL TO 4:]
Look at the table on page 1. The number on the bingo ball is X, and it is NOT the first row for which option B is selected. The decisions made in Level 2 will therefore not matter for the payments. Since the number on the bingo ball is X, the choice made for decision row X is the binding choice.

For decision row X the choice was [A OR B].

Now we will spin the bingo cage with 100 balls to determine whether you would receive the higher amount or the lower amount.

[SPIN CAGE WITH 100 BALLS.]

The number on the bingo ball is Z. Since you chose [A OR B] in Decision X, you would be paid [amount].

[SPIN CAGE WITH 100 BALLS 10 TIMES.]

[FOR BALL EQUAL TO 4:]
Since the number on the bingo ball is 4, decision row 4 will determine earnings. However, in this illustration decision row 4 is the first row where Option B is chosen,
which takes us to Level 2. We will therefore spin the cage again to select the row in Level 2 that will determine earnings.

Since there are 11 rows, we will add a bingo ball with number "11" to the bingo cage.

[SPIN CAGE WITH 11 BALLS.]

Since the number on the bingo ball is X, the choice made for decision row X in Level 2 is the binding choice.

For decision row X the choice was [A OR B].

Now we will spin the bingo cage with 100 balls to determine whether you would receive the higher amount or the lower amount.

[SPIN CAGE WITH 100 BALLS.]

The number on the bingo ball is Z. Since you chose [A OR B] in decision row X of Level 2, you would be paid [amount].

[SPIN CAGE WITH 100 BALLS 10 TIMES.]

[REPEAT EXERCISE ONE MORE TIME AND COVER BOTH EXAMPLES]

EXAMPLE 2

We have shown you examples of our procedures for the case where an individual chooses A for some decisions and B for others. We also expect some of you will be indifferent between Option A and Option B for some decisions. How will earnings be determined in that case?

The table for Example 2 on page 4 in your handout illustrates a case where someone is indifferent between Option A and Option B at Decision 7 and 8.

Please fill in answers on your computer that correspond to this illustration of the decision table. When you have finished filling in the table, but before you click the submit button at the bottom on the screen, please raise your hand and we will come and verify that you have done it correctly.

Again, if you indicate Indifference for any of the decision rows, there will be no further choices to be made by you in this problem before determining your earnings. And we will again spin the bingo cage to select the decision row which will determine your payment. This works exactly as before, except in the case where the spin of the bingo cage selects a decision for which you have indicated you don't care whether you are paid under Option A or Option B. In that case, we will have to spin the bingo cage again to choose whether you will be paid under Option A or Option B for that decision. To make sure you clearly understand this situation, we will now illustrate those procedures.

Password 4: 8

EXPERIMENTER SCRIPT

[SUBJECTS USE PRACTICE RECORD SHEETS, AND EXPERIMENTER USES A PAPER POSTER BOARD.]

We will first spin the bingo cage with 10 balls to determine which decision row in Level 1 is the binding one for payment. This is just an illustration so we are not paying for these decisions.

[SPIN BINGO CAGE WITH 10 BALLS.]

[FOR BALL NOT EQUAL TO INDIFFERENCE ROW (7 OR 8):]
Look at the table on page 4. Since the number on the bingo ball is X, the choice made for decision row X is the binding choice.

For decision row X the choice was [A OR B].

Now we will spin the bingo cage with 100 balls to determine whether you would receive the higher amount or the lower amount.

[SPIN CAGE WITH 100 BALLS.]

The number on the bingo ball is Z. Since you chose [A OR B] in Decision X, you would be

paid [amount].

[FOR BALL EQUAL TO INDIFFERENCE ROW (7 OR 8):]
The number of the ball is [7 or 8]. Since the row selected is one for which you made a choice of Indifference, we will need to perform an extra selection before determining whether payments will be based on the high or the low amounts. This extra selection will determine whether Option A or Option B will decide earnings. We will spin the bingo cage with 100 balls, and a number between 1 and 50 means that A will be chosen, and a number between 51 and 100 will mean that B is chosen.

[SPIN CAGE WITH 100 BALLS.]

For decision row X the choice was [A OR B].

Now we will spin the bingo cage with 100 balls to determine whether you would receive the higher amount or the lower amount.

[SPIN CAGE WITH 100 BALLS]

The number on the bingo ball is Z. Since the option selected was [A OR B], you would be paid [amount].

[REPEAT EXERCISE ONE MORE TIME AND COVER BOTH EXAMPLES]

EXAMPLE 3

We will now go through one final example. Please refer to page 6 and 7 in your handouts for screen images of what choices are to be made in this example.

Password 5: 16

EXPERIMENTER SCRIPT

[SUBJECTS USE PRACTICE RECORD SHEETS, AND EXPERIMENTER USES A PAPER POSTER BOARD.]

We will first spin the bingo cage with 10 balls to determine which decision row in Level 1 is the binding one for payment. This is just an illustration so we are not paying for these decisions.

[SPIN CAGE WITH 10 BALLS.]

[FOR BALL NOT EQUAL TO 6:]
Look at the table on page 6. The number on the bingo ball is X, and it is NOT the first row for which option B is selected. The decisions made in Level 2 will therefore not matter for the payments. Since the number on the bingo ball is X, the choice made for decision row X is the binding choice.

For decision row X the choice was [A OR B].

Now we will spin the bingo cage with 100 balls to determine whether you would receive the higher amount or the lower amount.

[SPIN CAGE WITH 100 BALLS.]

The number on the bingo ball is Z. Since you chose [A OR B] in Decision X, you would be paid [amount].

[FOR BALL EQUAL TO 6:]
Since the number on the bingo ball is 6, decision row 6 will determine earnings. However, in this illustration decision row 6 is the first row where Option B is chosen, which takes us to Level 2. We will therefore spin the cage again to select the row in Level 2 that will determine earnings.

Since there are 11 rows, we will add a bingo ball that is numbered with an "11" to the bingo cage.

[SPIN THE CAGE WITH 11 BALLS.]

[FOR BALL NOT EQUAL TO INDIFFERENCE ROW (3):]
Look at the table on page 7. Since the number on the bingo ball is X, the choice made for decision row X in Level 2 is the binding choice.

For decision row X the choice was [A OR B].

Now we will spin the bingo cage with 100 balls to determine whether you would receive the higher amount or the lower amount.

[SPIN CAGE WITH 100 BALLS.]

The number on the bingo ball is Z. Since you chose [A OR B] in Decision X, you would be paid [amount].

[FOR BALL EQUAL TO INDIFFERENCE ROW (3):]
The number of the ball is 3. Since the row selected is the one for which you made the choice of Indifference, we will need to perform an extra selection before determining whether payments will be based on the high or the low amounts. This extra selection will determine whether Option A or Option B will decide earnings. We will spin the bingo cage with 100 balls, and a number between 1 and 50 means that A will be chosen, and a number between 51 and 100 will mean that B is chosen.

[SPIN CAGE WITH 100 BALLS.]

For decision row X in Level 2 the choice was [A OR B].

Now we will spin the bingo cage with 100 balls to determine whether you would receive the higher amount or the lower amount.

[SPIN CAGE WITH 100 BALLS]

The number on the bingo ball is Z. Since the option selected was [A OR B], you would be paid [amount].

[REPEAT EXERCISE ONE MORE TIME]

There is one final detail we need to explain. You will be asked to complete <u>four</u> decision problems as explained above. These four decision problems will be exactly the same except that the high and low amounts will differ. Although you will complete four problems, we will not pay you for all four problems. After you have completed the entire set of decision problems we will need to spin the bingo cage again to determine which of the problems we will use for your payment.

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

Once we have selected that problem, we will then spin the bingo cages as explained above.

It is important to understand that you will have to finish making your choices for all four problems before we start spinning the bingo cages. In addition, in each of the four decision problems there may be up to three levels of tables rather than just two levels.

Are there any questions?

To further illustrate our procedures, we will now continue with an example where the payments are indicated in chocolate kisses. You will be asked to make choices in one problem. After you have completed your choices we will perform all the draws using the bingo cages to determine your payments.

Each person will have a 1-in-10 chance of receiving the chocolate kisses. The selection will be done using a ten-sided die. If the number 0 is drawn you will receive the chocolate immediately. If any other number is drawn you will not receive the chocolate.

Password 6: test

EXPERIMENTER SCRIPT

[EXPERIMENTER USES RECORD SHEETS AND PAPER POSTER BOARD.]

The next two images that appear on the computer screen show the results of your choices and will help us determine your earnings. All records will be linked to an anonymous ID-number only.

We will first spin the bingo cage to determine which decision rows in Level 1 and Level 2 are the binding ones for payment.

[SPIN CAGE WITH 10 BALLS.]
[SPIN CAGE WITH 11 BALLS.]

We will next spin the bingo cage with 100 balls to determine whether Option A or Option B will decide earnings in case you are indifferent. A number between 1 and 50 means that A will be

chosen, and a number between 51 and 100 will mean that B is chosen.

[SPIN CAGE WITH 100 BALLS.]

We will then spin the bingo cage with 100 balls to determine whether you will receive the higher amount or the lower amount.

[SPIN CAGE WITH 100 BALLS.]

Finally, we will now come around and roll the ten-sided die to determine who will receive the chocolate kisses. If the number 0 is drawn you will receive the chocolate immediately. If any other number is drawn you will not receive the chocolate.

[ROLL TEN-SIDED DIE FOR EACH PERSON.]

Password 7: test

This is the end of all the practices. We will now proceed with Part II of the experiment. Recall that you will be asked to make choices in 4 problems, like the ones we have been demonstrating. Each of these four decision problems may consist of up to three levels of tables to fill in. After you have completed all 4 problems, we will perform the draws using the bingo cages to determine your payments for this part.

Each person will have a 1-in-10 chance of receiving the money. The selection will be done using a ten-sided die. If the number 0 is drawn you will receive the money at the end of the meeting. If any other number is drawn you will not receive the money. All payments are made in private so other persons will not know your decisions.

Password 8: ra

EXPERIMENTER SCRIPT

[EXPERIMENTER USES RECORD SHEETS AND PAPER POSTER BOARD.]

We will first spin the bingo cage with 100 balls to determine which of the four problems we will use for your payment.

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

[SPIN CAGE WITH 100 BALLS.]

We will next spin the bingo cage to determine which decision rows in Level 1 and Level 2 are the binding ones for payment.

[SPIN CAGE WITH 10 BALLS.]
[SPIN CAGE WITH 11 BALLS.]

We will next spin the bingo cage with 100 balls to determine whether Option A or Option B will decide earnings in case you are indifferent. A number between 1 and 50 means that A will be chosen, and a number between 51 and 100 will mean that B is chosen.

[SPIN CAGE WITH 100 BALLS.]

We will then spin the bingo cage with 100 balls to determine whether you will receive the higher amount or the lower amount.

[SPIN CAGE WITH 100 BALLS.]

Finally, we will now come around and roll the ten-sided die to determine who will receive the money. If the number 0 is drawn you will receive the money at the end of the meeting. If any other number is drawn you will not receive the money.

[ROLL TEN-SIDED DIE FOR EACH PERSON.]

I will now come around and enter your subject ID numbers on the computers. We will then read through the instructions together. Please wait for me to finish.

[Give handouts for Part III to subjects: computer screen examples.]

Instructions for Part III

We will now begin Part III of the experiment.

Each person in this room will have a chance to receive an additional large sum of money. If you are selected to receive this sum of money, you will have a choice between two payment options; option A or option B. Each person will have a 1-in-10 chance of receiving the money. The selection will be done using a ten-sided die. If the number 0 is drawn you will receive the money. If any other number is drawn you will not receive the money.

As in Part II of the experiment, you will be asked to make a series of choices in a decision problem which may have multiple levels. The table shown on page 1 in the handout is an illustration of what Level 1 of the decision problem will look like on your computer screen. This handout contains another screen image that we will mention later.

This screen illustration shows ten decisions listed on the left side, in the column marked **Decision**. Each decision is a paired choice between Option A and Option B. You will be asked to make a choice between these two payment options in each decision row. In this example each of the 10 decision rows will pay **$100** one month from today (option A) and **$100 + $X** seven months from today (option B), where **$X** differs in each decision row.

In the table there are two columns labeled "*Annual Interest Rate*" and "*Annual Effective Interest*

Rate". To explain these terms, let us consider the following payoff alternative (decision row no. 4 in the table):

Option A pays $100.00 one month from today.
Option B pays $110.25 seven months from today.

In this example, if you choose option B you will earn an annual interest rate of 20.00% on the $100 you choose to receive 7 months from today. Since this is compounded quarterly your annual effective interest rate is 21.55%. (Quarterly compounding is consistent with general banking practices on overdraft accounts.) The annual *effective* interest rate is the rate earned on the initial balance ($100 in this example) plus interest earned on all interest accumulated in the preceding compounding periods.

For each decision row, you will be asked to choose Option A or Option B by clicking on the appropriate button. For some decision you may not care whether you receive Option A or B, in which case you should click the button labeled "I" for "Indifference."

If you select Option A for all decision rows, or if you indicate Indifference for any of the decision rows, there will be no further choices to be made by you in this problem before determining your earnings.

If you select Option B for all decisions rows, or if you switch from Option A to Option B at some point, we will give you a Level 2 task before determining your earnings. The Level 2 task involves making choices in the table illustrated on page 2 in the handout.

The Level 2 table shows you eleven other decisions listed in a similar way. They are arranged in the same way as the ten decisions in the Level 1 table, but they focus in on the decisions that were made in Level 1.

Assume that someone in Level 1 has selected Option A for rows 1-5 and Option B for rows 6-10. This means that this person prefers Option A when the annual interest rate is 25% or less, but prefers Option B when the annual interest rate is 30% or more. Level 2 then asks this person to choose between Option A and Option B for annual interest rates between 25% and 30%. Thus, row 1 in Level 2 corresponds to an annual interest rate of 25%, and row 2 to an annual interest rate of 25.5%, and so on until the last row shows an annual interest rate of 30%. Thus Level 2 just provides more detail in the range of choices this person indicated in Level 1.

Notice that the annual interest rate in Level 1 increases by intervals of 5 percentage points, as you move from decision row 1 to decision row 2. The same increase in annual interest rates applies to each row in Level 1. Notice also that the decisions displayed in Level 2 are determined by the row where you first choose Option B over Option A in Level 1. Level 2 simply takes the interval between the point where you last chose A and first chose B, and divides that interval of 5 percentage points into 11 narrower intervals.

As you can see, you have a minimum of 10 decisions to make. You will have 21 decisions if you make the kind of decision in Level 1 that moves you to Level 2. Nevertheless, we will pay you for only one of these decisions. After you have made all of your choices we will again use the bingo cage to select which decision will be used to determine your payment. These procedures will work in

exactly the same way as in Part II of the experiment.

There is one final detail we need to explain. You will be asked to complete six decision problems as explained above. These six decisions will be exactly the same except that the payment date for Option B will differ. Although you will complete six problems, we will not pay you for all six problems. After you have completed the entire set of decision problems we will need to spin the bingo cage with 6 balls numbered from 1 to 6 to determine which of the problems we will use for your payment.

Once we have selected that problem, we will then spin the bingo cages as explained above.

It is important to understand that you will have to finish making your choices for all six problems before we start spinning the bingo cages. In addition, for each of the six decision problems there may be up to three levels of tables rather than just two levels.

HOW WILL YOU BE PAID?

You will receive a certificate which is redeemable under the conditions dictated by your chosen payment option under the selected payoff alternative. This certificate is issued by the Ministry of Economic and Business Affairs and guarantees that the money is automatically transported from the Ministry's bank account in Sydbank to your personal bank account. You can send the certificate to Sydbank in a prepaid envelope, and the bank will handle the administration of the money transports. Please note that all payments are subject to personal income tax, and information on all payments to participants will be given to the tax authorities by the Ministry of Economic and Business Affairs.

We will now proceed with Part III of the experiment. Recall that you will be asked to make choices in six problems, like the one we have demonstrated. In each of the six problems you may have up to three levels of tables to fill in. After you have completed all six problems, we will perform the draws using the bingo cages to determine your payments for this part.

Each person will have a 1-in-10 chance of receiving the money. The selection will be done using a ten-sided die. If the number 0 is drawn you will receive the money at the agreed date. If any other number is drawn you will not receive the money. All payments are made in private so other persons will not know your decisions.

Password 9: 32

Password 10: idr

EXPERIMENTER SCRIPT

[EXPERIMENTER USES RECORD SHEETS AND PAPER POSTER BOARD.]

We will first spin the bingo cage with 6 balls to determine which of the six problems we will use for your payment.

[SPIN CAGE WITH 6 BALLS.]

We will next spin the bingo cage to determine which decision rows in Level 1, Level 2 and Level 3 are the binding ones for payment.

[SPIN CAGE WITH 10 BALLS.]
[SPIN CAGE WITH 11 BALLS.]
[SPIN CAGE WITH 11 BALLS.]

We will next spin the bingo cage with 100 balls to determine whether Option A or Option B will decide earnings in case you are indifferent. A number between 1 and 50 means that A will be chosen, and a number between 51 and 100 will mean that B is chosen.

[SPIN CAGE WITH 100 BALLS.]

Finally, we will now come around and roll the ten-sided die to determine who will receive the money. If the number 0 is drawn you will receive the money at the agreed date. If any other number is drawn you will not receive the money.

[ROLL TEN-SIDED DIE FOR EACH PERSON.]

Password 10: idr

At this time we ask that you answer the questions for Part IV. This information is for our records only and confidentiality of your responses is assured. Just click the OK button to go on.

This is the end of the survey. When everyone has answered the questions, we will ask you to step aside for a moment and then call you back in, one at a time, to pay you in private.

Thank you for participating in the survey.

Appendix D: Data and Statistical Analysis

Supporting data and statistical analyses are stored in the ExLab Experimental Social Sciences Digital Archive located at http://exlab.bus.ucf.edu. All statistical analyses are undertaken using version 8.2 of *Stata*, documented in StataCorp [2003].