Estimating Individual Discount Rates in Denmark: A Field Experiment

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Glenn W. Harrison, Morten I. Lau and Melonie B. Williams[†]

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Abstract. We estimate individual discount rates with respect to time streams of money using controlled laboratory experiments. These discount rates are elicited by means of field experiments involving real monetary rewards. The experiments were carried out across Denmark using a representative sample of 268 people between 19 and 75 years of age. Individual discount rates are estimated for various households differentiated by socio-demographic characteristics such as income and age. Our conclusions are that discount rates are constant over the 6-month to 3-year horizons used in these experiments, that discount rates vary with respect to several socio-demographic variables, and that they decline with age after middle age. Hence we conclude that it would be *reasonable to assume constant discount rates for specific household types, but not the same rates across all households.*

Subject Areas: Economic Damages/Benefits
Keywords: discount rates, experimental economics, censored dependent variable, interval censored data, time consistency

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Discount rates are often used in cost-benefit analysis. Whenever costs and benefits for a household or individual are spread over time, it is essential that one calculate present value equivalents in order to undertake meaningful comparisons. In most cases welfare analysts use market rates as the basis for these present value calculations. Sensitivity analysis often consists of varying the scalar discount rate up or down in relation to market interest rates.

Economic theory predicts that, in a first-best world, individuals will discount the future at the market rate of interest. Given imperfect markets, however, individual discount rates may be quite different from interest rates observed in financial markets. Moreover, since discount rates are a reflection of subjective time preferences, one would expect that they could differ across different individuals.[1] Nevertheless, standard practice in inter-temporal welfare analyses is to use market interest rates to reflect consumers' rates of discount, and to assume that those rates are (i) the same across households, and (ii) the same for all time horizons. We elicit individual discount rates from subjects in order to test these two hypotheses. The first hypothesis is that discount rates for a *given time horizon* do not differ with respect to socio-demographic characteristics that characterize households in our sample. The second hypothesis is that discount rates for a *given individual* do not differ across time horizons.

We use survey questions with real monetary rewards to elicit individual discount rates and demonstrate the methodological complementarity between lab and field experiments. The survey questions are designed by Coller and Williams [1999], who elicit individual discount rates for university students using controlled laboratory experiments. We apply their experimental procedures, but employ subjects that are normally encountered in field surveys. Our experiments were carried out across Denmark for the Danish government, using a nationally representative sample of 268 people between 19 and 75 years of age.

Our results indicate that discount rates are constant over the 6-months to 3-year horizons used in these experiments, that discount rates vary with respect to several socio-demographic variables, and that they decline with the age of the individual after middle age. *On the basis of these results one can assume constant discount rates for specific household types, but not the same rates across all households.*

In section 1 we review the primary findings from the previous experimental literature,

[1] We elicit discount rates for individuals. To the extent that the characteristics of individuals are used to define "representative households," we can refer to the individual and the household interchangeably. However, we remain agnostic concerning the way in which the individual discount rates of individual household members are aggregated into one household discount rate, akin to a social discount rate for the household as a small society.

and in section 2 review the logic of our experimental design. Section 3 explains the field experiments conducted, and section 4 examines the results.

1. Existing Literature

Numerous studies have employed experimental methods to explore the assumption of constant, or exponential, discounting. Most previous evidence suggests that the rate at which people discount future outcomes tends to rise with the proximity of the outcome and that the effect is most pronounced for short time horizons. In other words, the individual discount rates observed in previous studies appear to follow a general hyperbola-like pattern across time delay.

This delay dependence appears to be robust across several dimensions of experimental and sample design. Hyperbolic discount rates have been observed in experimental studies using hypothetical rewards[2] as well as those using (small) real payments.[3] Although most experimental studies use student subjects, Ainslie and Haendel [1983] and Pender [1996] demonstrate that hyperbolic discounting is not an artifact of the use of student convenience samples.[4] Moreover, the studies by Benzion, Rapoport, and Yagil [1989] and Pender [1996] suggest that we might expect to observe similar behavior in other countries. Delay dependence appears to be robust across question frames: time dependent discounting has been observed across all combinations of gains *vs.* losses and speed up *vs.* delay of outcomes.[5] Loewenstein [1987] and Pender [1996] observe the same pattern of behavior using commodities rather than monetary outcomes. Finally, while most of the experiments cited here vary the delay between early and later payments, delay dependence has also been observed when the time to the early payment (i.e., the "front-end delay") is varied (Ainslie and Haendel [1983] and Winston and Woodbury [1991]).[6]

Coller and Williams [1999] suggest that behavior in these studies may be affected by uncontrolled factors other than time preferences that may help explain observed anamolies.[7]

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[2] Ainslie and Haendel [1983], Benzion, Rapoport and Yagil [1989], Loewenstein [1987], Myerson and Green [1995], Thaler [1981], and Winston and Woodbury [1991]

[3] Ainslie and Haendel [1983], Carlsson and Johnson [1992], Holcomb and Nelson [1992], Kirby [1997], Kirby and Herrnstein [1995], and Pender [1996].

[4] The samples utilized were employees and patients of a veterans hospital, and adult residents of villages in southern India, respectively.

[5] Benzion, Rapoport and Yagil [1989], Carlsson and Johnson [1992], Loewenstein [1987], Shelley [1993]

[6] However, Holcomb and Nelson [1992] observe that manipulating the front-end delay has no impact on behavior in their experiments.

[7] Previous studies have also found implied discount rates that decline with magnitudes of rewards (Ainslie and Haendel [1983], Benzion, Rapoport, and Yagil [1989], Green, Fry, and Myerson [1994], and Kirby and Maraković [1995]). Question frames have been found to systematically affect discounting behavior; for example, discount rates tend to be higher for delayed receipts than for expedited payments (Loewenstein [1987], Benzion, Rapoport, and Yagil [1989], and Shelley [1993]).

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* interest (the difference between the early and later payments), while field investments are priced in terms of annual and effective interest *rates*.[8] 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.

If subjects tend to substantially overestimate exponential functions, as Wagenaar and Sagaria [1975] suggest, then subjects are likely to make mistakes that substantially *overstate* their implied discount rates. Moreover, it is particularly difficult to convert dollars to interest for time periods of less than a year, a scenario that corresponds to most experiments using real payments. These factors may help explain the extraordinarily high short-term discount rates found in most previous studies, as well as the observation of hyperbolic discounting. Observed discount rates are high for short time horizons (when effective rates are difficult to calculate), then tend to level out at horizons longer than a year (at rates close in magnitude to recent market rates, effective interest over a year is very close to simple interest).[9]

Coller and Williams [1999] designed a set of laboratory experiments to address these issues. To address salience, the experiments incorporate real monetary payments over a 6-month time horizon that are substantially larger than those used in previous studies. In addition, consistent with the pricing of field investments, subjects are provided with information on the annual and effective interest rates implied by their choices. In contrast to most previous studies using payments and time horizons of comparable magnitude, Coller and Williams [1999] find implied discount rates in their experiments that are, on average across all treatments, broadly consistent with market interest rates. Moreover, the effect of providing information on implicit

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[8] Effective rates reflect compound interest. It is the effective rate that corresponds to the final dollar interest associated with any investment.

[9] For time horizons of less than a year, the central tendencies of observed discount rates (in annual terms) range from approximately 13% (Carlsson and Johnson [1992]) to billions of percentage points (Kirby [1997], Kirby and Herrnstein [1995], and Kirby and Maraković [1996]). Pender [1996] reports that subjects in a follow-up study revealed constant discount rates over time periods of a year or more. However, since the longer time horizons were used only in the follow-up study, and not in the initial study, this result could also be explained by the experience of subjects in the follow-up study.

rates is to lower revealed discount rates, and their residual variances, substantially. The experiments reported here build on the Coller and Williams [1999] design in important ways by varying the time horizon, examining longer time horizons, and by utilizing a national sample of non-student adults.

2. Eliciting Discount Rates

The basic question used to elicit individual discount rates 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. The format of our experiment modifies and extends this basic question in six ways.

First, we pose a number of such questions to each individual, each question varying x by some amount. When x is zero we would 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 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 their discount rate lies between 10% and 11% for this time interval. The finer the increments in x, the finer will we be able to pinpoint the discount rate of the individual.

Second, we simultaneously pose several questions with varying values of x, selecting one question at random for actual payment after all responses have been completed by the individual. In this way the results from one question do not generate income effects which might influence the answers to other questions. Although one could allow for these effects in the later analysis, they could easily cause more statistical problems than the extra data is worth.

Third, we provide two future income options rather than one "instant income" option and one future income option. For example, we offer \$100 in one month and \$100+x in 7 months, interpreting the revealed discount rate as applying to a time horizon of 6 months. This avoids the potential problem of the subject facing extra transactions costs[10] with the future income option. If the delayed option were to involve greater transactions costs, then the revealed discount rate would include these subjective transactions costs. By having both options entail

[10] Including the possibility of default by the experimenter.

future income we hold these transactions costs constant.

Fourth, we consider four possible time horizons: 6 months, 12 months, 24 months and 36 months. In one series of experiments we assign one of these time horizons to the subject at random, and only elicit discount rates pertaining to that horizon. In another series, with different subjects, we ask the subject to state preferences over all four time horizons, knowing that we will select one time horizon at random for possible payment. A comparison of these two series will allow some evaluation of the effect of explicitly asking subjects to consider multiple time horizons. It is plausible that this could mitigate any tendency for subjects to reveal time-inconsistent discount rates.

Fifth, we elicit information from subjects to help us identify what market rates of interest they face. This information will be used to allow for the possibility that their responses in our surveys are *censored* by market rates. 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[11] 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]). Fortunately, there are simple statistical procedures for allowing for this possibility, and we employ those in our statistical analysis.

It is easy to see how this censoring problem applies here. Consider a subject with a true individual discount rate (IDR) of 30%. In the absence of field substitutes for lab incentives, we would expect this subject to choose to save in the lab when the lab instrument provides a rate of return of 30% or higher. Now assume that this subject can *borrow* in the field at a rate of 14%. Although she demands at least 30% interest to delay consumption and save in the lab, at rates between 14% and 30% she is better off borrowing in the field at 14% and not delaying consumption in the field, leaving the money in the lab earning 14% or more, and repaying the field debt at the time she collects from the experimenter. In this case, the subject should rationally choose to invest in the lab when the lab instrument provides a rate of return of 14% or more. Hence, censoring would imply that the true IDR *could* actually be greater than or equal to the observed borrowing rate when we observe lab investment responses that suggest that the IDR is

[11] Assume further that a beer in the lab is the same product as a beer in the field.

equal to the borrowing rate.[12] In other words, if we ignored the possibility of censoring of lab responses we would incorrectly infer that this subject had an IDR of 14%. Instead, we can only infer from these lab responses that the subject has a true IDR between 14% and ∞. The problem is symmetric for censoring with respect to savings rates, although less significant empirically.[13]

The implication of allowing for censoring is that we cannot presume that the "raw" responses in the lab are unbiased indicators of the true IDR of the subject. Moreover, if we ignored field censoring then we could easily be led to think that we were measuring responses with more precision than would be warranted.

Sixth, we provide respondents with the interest rates associated with 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.

3. The Danish Experiments

3.1 Sample

In 1996 the Danish Ministry of Business and Industry contracted with the Danish Social Research Institute (SFI, after the Danish name *Socialforskningsinstituttet*) to undertake the field surveys.[14] The final surveys were conducted between June 16 and July 8, 1997, throughout Denmark.

The sample population consisted of a random selection from individuals 19-75 years old who had participated all three times in the European Community Household Panel Survey (ECHP) previously conducted by SFI. These persons were chosen because they had some experience with respect to economic surveys, and because we could expect a high response rate. The sample was constructed in two steps.

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[12] When the subject reports an IDR interval that exceeds the borrowing rate that we calculate for the subject, we assume that there are subjective and unobserved transactions costs such that the true (unobserved) market rate for the subject is equal to the lower bound of the reported interval. The subject's responses are then treated statistically as being censored at that inferred borrowing rate.

[13] Consider, for example, a subject with a true IDR of 3%. In the absence of field substitutes for lab incentives, we would again expect this subject to choose to invest in the lab instrument as long as it provides a return of 3% or higher. Now suppose that this subject can *save* in the field at a rate of 10%. Although she would be willing to save at 3%, at rates between 3% and 10% she is better off investing in the field and refusing to invest in the lab. Hence censoring would imply that the true IDR could actually be less than or equal to the observed savings rate when we observe lab investment responses that suggest that the IDR is close to the savings rate.

[14] At the time, Harrison was Director of the MobiDK Project, within the Ministry. Lau was a Senior Researcher with the MobiDK Project.

The 275 municipalities in Denmark were proportionally stratified with respect to the number of persons between 19 and 75 years of age on January 1, 1997. Copenhagen and Aarhus, the two largest municipalities, had their own stratum due to their size. Most of the other municipalities were divided into 23 strata. Some remote municipalities, primarily tiny islands, were not represented in the sample because the population is relatively small and the subjects would spend too much time on traveling to the experimental session.

The 27 sessions were divided equally across geographic locations with 5, 10 and 15 participants in each experiment. In turn, the 27 sessions were located such that the number of participants at the experiments correspond to the relative size of the population in the given stratum. For example, approximately 11% of the population between 19 and 75 years of age live in Copenhagen, and 3 sessions with a total of 30 participants were held in Copenhagen which corresponds to 11.1% of the total sample size.[15]

Most strata consist of several municipalities, and the strata were constructed according to traffic connections. The sessions were held in the evening to facilitate attendance by working subjects. It was important that the participants not spend too much time on traveling in order to join the experiments. In some cases, it was necessary to divide a given stratum into two subgroups, since the distance between some potential participants and the location of the session would otherwise be too great. Accordingly, a random draw from the subgroups was made, weighting the two subgroups with respect to the relative size of the population between 19 and 75 years of age.

The interviewers initially contacted 6, 12 or 17 persons, the number depending on the specific session and assuming a show-up rate of approximately 80%. If a respondent declined to participate, the interviewers contacted a "stand-in" roughly the same age. Hence, either 6, 12 or 17 persons were confirmed before the experiment took place. However, some persons did not show up at the sessions and the actual number of participants varied accordingly.

A total of 268 subjects participated in the experiments. The sample was representative of the adult population of Denmark.

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[15] It is possible that some subjects were confused as to whether they lived in Copenhagen or Greater Copenhagen, so we have tended to lump these together in the statistical analysis. The area called Copenhagen in the survey covers 3 communes: Copenhagen, Frederiksberg and Gentofte. The total population in this area is 600,000 people, which is around 11% of the total population. Three sessions in Copenhagen with 27 subjects in total matches this share well. Some of the sessions referring to Zealand cover some of the suburbs in Copenhagen. The population in Copenhagen, including all suburbs, is 1.35 million, which is around 26% of the total population. We suspect that some subjects who live in the suburbs write that they live in Copenhagen instead of the Greater Copenhagen area.

3.2 Primary Experimental Instructions

Apart from logistical correspondence between SFI and the subject concerning attendance at the session, the only information that the subject received was from the survey instrument administered in the experiment. All correspondence is available in the original Danish via Adobe PDF versions on the web at HTTP://DMSWEB.BADM.SC.EDU/GLENN/DKIDR_DAT.HTM. They include the initial invitation to participate, confirmation that the subject will participate, a letter thanking the subject for agreeing to participate and confirming the time and location, a receipt for travel costs and a statement that the amount earned in the experiment will be taxed, a certificate specifying the payoffs to be received, and a letter accompanying the check that the subject receives.

The initial contact letter to the subjects posed the general nature of the task. A translation of the invitation is as follows:

> Each day you make a number of choices determining how to spend your money. Some of these considerations concern the future. Should you buy now and borrow the money? Or should you save and spend the money later? The Social Research Institute is carrying out a survey to find out how Danes behave in this respect. The survey is carried out for the Danish Ministry of Business and Industry and researchers from the United States. The survey is the first of its kind in Denmark.
>
> 270 persons from all over the country will participate in the survey. All participants are chosen randomly from the survey on the welfare of Danish families, in which you have participated. To conduct the survey, a small group will meet and the participants will answer some questions. We would therefore like to invite you to participate in this meeting, which will take place at...
>
> To cover the costs you may have, you will receive 500 kroner after you have participated in the survey. In addition to that, **one** person from the group will receive <u>at least</u> 3000 kroner. This person will be randomly drawn from the group of participants. All payments are subject to personal income taxes.
>
> It is important that everybody responds, but it is voluntary to participate in the survey. The answers are strictly confidential, and the results will be published in a way that no person can be recognized. The meeting will last at most 2 hours. Please return this letter within a week from now. A postage free envelope is attached.
>
> If you have any questions or want to know more about the survey, please call the interviewers at 33 48 08 00. Thank you.

No other details of the experiment were provided until the subjects arrived at the session.

The main part of the experiment consisted of instructions on the elicitation task. Subjects were taken through a "trial experiment" in which they could practice all of the procedures and in which the commodity was candy instead of money. The following English translation of the instructions[16] explains the main part of the 6-month horizon experiment:

WELCOME TO THE EXPERIMENT
THESE ARE YOUR INSTRUCTIONS

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[16] The original Danish version is available at HTTP://DMSWEB.BADM.SC.EDU/GLENN/DKIDR_DAT.HTM, and consists of all instructions for the 6 month horizon (the other horizons were virtually identical, with payoff tables that are reproduced in full in the multiple-horizons instructions), instructions for the multiple-horizons session, a questionnaire asking about socio-demographic characteristics of the individual, a questionnaire asking about financial characteristics, and the forms used to elicit responses. For the convenience of Danish-challenged readers we also provide on this web site an English translation of all of the instructions and questionnaires.

This is an experiment in the economics of decision making. Funding for this experiment has been provided by the Danish Ministry of Business and Industry.

Your participation in this experiment is voluntary. However, we think you will find the experiment interesting. You will receive 500 DKK to cover your costs of participating *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.

One person in this room will be randomly chosen to receive a large sum of money. If you are the individual chosen to receive this money (the "Assignee"), you will have a choice of *two* payment options; option A or option B. If you choose option B you will receive a sum of money 7 months from today. If you choose option A, you will receive a sum of money 1 month from today, but this option (A) will pay a smaller amount than option B.

Each individual in this room will be asked to choose the payment option he or she would prefer (if chosen to be the Assignee) in *each* of 20 different payoff alternatives. Each individual will receive a table that looks like the table on the next page. Note that each of the 20 payoff alternatives will pay **3,000 DKK** one month from today (option A) and **3,000 DKK + *X* DKK** seven months from today (option B), where ***X* DKK** differs under each payoff alternative. Next to *each* payoff alternative each individual will circle the payment option (A or B) he or she would prefer if chosen to be the Assignee. At the end of the experiment, *one* of the 20 payoff alternatives will be selected at random. The Assignee will be paid based on the payment option he or she chooses under the payoff alternative selected.

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 example payoff alternative (payoff alternative no. 8 in the table):

Option A pays 3,000 DKK 1 month from today.
Option B pays 3,308 DKK 7 months from today.

In this example, if you choose option B you will earn an annual interest rate of 20.00% on the 3,000 DKK 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 (3,000 DKK in this example) plus interest earned on all interest accumulated in the preceding compounding periods.

In a few moments we will ask each individual in this room to choose the payment option he or she would prefer under each payoff alternative *if* chosen to be the Assignee. *All decisions will be written and will not be revealed to any other participants. All decisions will be treated confidentially.*

HOW IS THE PAYOFF ALTERNATIVE SELECTED?

Everyone in this room will receive a payoff table like the one above in duplicate. Note that the payoff alternatives are numbered 1-20. You will be asked to circle your preferred payoff option (A or B) under each payoff alternative. You will then give the original copy of the table to the experimenter, keeping the duplicate (yellow) copy for yourself. After the experimenter has collected everyone's decisions, he will ask one of you to pick one of 20 cards numbered 1 through 20 out of the bag labeled "Payoff Alternative" that you see in the front of the room. Before the cards are placed in the bag, the experimenter will ask one of you to verify that the correct number of cards with the correct labels are being placed in the bag. Note that the bag is constructed so that you cannot see into the bag as you withdraw a card. The payoff alternative whose number coincides with the number on the card will be the payoff alternative under which the Assignee will be paid. Therefore, all payoff alternatives are equally likely to be selected.

Remember, the Assignee will be paid based on the payment option he or she chooses for that payoff alternative. For example, suppose you are chosen to be the Assignee and your copy of your payoff table shows that you chose payment option A under payoff alternative 5. If the card picked reads "5", you will receive 3,000 DKK one month from today (and 0 DKK seven months from today).

HOW IS THE ASSIGNEE DETERMINED?

Each person in this room has received two white index cards and two pink index cards. The pink index cards will be used in a trial experiment and the white index cards will be used in the actual experiment. Pink cards have the same letter written on them and white index cards have the same number. Everyone in the room will have a different letter and number.

Once the experimenter has collected everyone's decisions, he will circulate among you with the bag labeled "Assignee" you see at the front of the room. You will place *one* of the white index cards in this bag. Once everyone's cards are placed in the bag the payoff alternative will be selected. The contents of the Assignee bag will then be shaken and the experimenter will pick a white index card from the bag. Note that the bag is constructed so that the experimenter cannot see into the bag as he withdraws the card. If the number on this white index card matches the number on the white index card in your possession, you are the Assignee. Therefore, all individuals in this room have an equal chance of being the Assignee.

HOW WILL THE ASSIGNEE BE PAID?

The Assignee will receive a certificate which is redeemable under the conditions dictated by his or her chosen payment option under the selected payoff alternative. This certificate is guaranteed by the Social Research Institute. The Social Research Institute will automatically redeem the certificate for a Social Research Institute check, which the Assignee will receive given his or her chosen payment option under the selected payoff alternative. Please note that all payments are subject to income tax, and information on all payments to participants will be given to the tax authorities by the Social Research Institute.

The payoff table referred to in these instructions is reproduced below as Table 1. The instructions for the 12-month, 24-month and 36-month horizon experiments were identical except for the obvious changes. The payoff tables for these experiments are reproduced below as Tables 2 through 4.

The instructions for the multiple-horizons sessions were similar, with the single change that the subject was asked to provide responses for all four time horizons. One time horizon was then selected for possible payment, and the remaining procedures were identical to the single-horizon sessions.

Inspection of the payoff tables indicates that payoffs to any one subject could range from 3,000 DKK up to 12,333 DK. The exchange rate at the time of the experiments was approximately 6.7 DKK per US dollar, so this range converts to $450 and $1,840.

In all sessions the subjects were taken through a "trial experiment" to familiarize themselves with the procedures. To provide a concrete sense of the payoffs the choices were between candies at the beginning of the experiment and candies at the end of the experiment. This English translation covers the salient features of this practice session:

To demonstrate the procedures used in this experiment we will run a short trial experiment. One person in this room will be randomly chosen to receive a quantity of Tom's caramels. If you are the individual chosen to receive this commodity (the "Trial Assignee") you will have a choice of two payment options. If you choose option B you will receive a quantity of caramels at the *end* of today's experiment. If you choose option A, you will receive a quantity of caramels *immediately*, but the number of caramels received under option A will be smaller than under option B.

Each individual will receive a table resembling the payoff tables above, where the payoffs are in Tom's caramels. Each person will choose the payment option (A or B) he or she would prefer (*if* chosen to be the Trial Assignee) in each of 6 different payoff alternatives. Each of the 6 payoff alternatives will pay 5 caramels immediately (option A) and 5+x caramels at the end of the experiment (option B), where x differs under each payoff alternative. *After* everyone has made their decisions and given the original copy to the experimenter, he will circulate among you with the "Assignee" bag. You will place one of your *pink* index cards in the bag. Once all index cards have been collected, the payoff alternative and the Trial Assignee will be chosen. The Trial Assignee will receive the quantity of Tom's caramels coinciding with the payment option he or she chooses under this payoff alternative.

Remember that each payoff alternative is equally likely to be selected, and you are just as likely to be the Trial

Assignee as anyone else. Therefore, it is in your best interest to carefully consider your payment options under each payoff alternative and choose the one which you would truly prefer if you were the Trial Assignee and that payoff alternative were chosen. Once your decisions are given to the experimenter you will not be able to change them.

To non-Danish readers, we note that *Tom's caramels* are a staple of most Danes when they eat candy, akin to the *Hershey's Kisses* used by Coller and Williams [1999] with American subjects.

3.3 Additional Experimental Questionnaires

In addition to the primary elicitation task, we collected information from subjects on a variety of socio-demographic characteristics. Specifically, we collected information on age, gender, size of town the subject resided in, type of residence, primary occupation during the last 12 months, highest level of education, household type (viz., marital status and presence of younger or older children), number of people employed in the household, total household income before taxes, disposable household income, whether the subject is a smoker, and the number of cigarettes smoked per day. We also elicited information on a number of financial variables[17] to help us identify the market circumstances within which the discount rate responses should be viewed.

[17] Specifically, we collected information on whether the subject had a checking account, the annual interest rate on any checking account, and the current balance on the checking account; whether the subject had a line of credit, the annual interest rate on the line of credit, whether or not there was a balance carried from month to month on the line of credit, and the balance owed on line of credit; whether the subject owned a credit card, the annual interest rate on the credit card, and the balance owed on the credit card; the lowest interest rate on any credit card with credit left to use, the balance owed on the credit card with the lowest interest rate; whether there were any outstanding student loan balances, the annual interest rate on student loan balances, and the balance owed on student loans; whether the subject had a savings account, the annual interest rate on the savings account, the balance on savings account; whether the subject had any other investment accounts not described above, the annual interest rate on other investment accounts not described above, and the balance on other investment accounts not described above; the subject's perception of his or her chances of obtaining a loan, line of credit or credit card; how often the subject was short of cash between paychecks; and what were the subject's plans for any money received from the experiment.

Table 1: 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)	**Annual Effective Interest Rate** (AER)	**Preferred Payment Option** (Circle A or B)
1	3,000 DKK	3,038 DKK	2.50%	2.52%	A B
2	3,000 DKK	3,075 DKK	5.00%	5.09%	A B
3	3,000 DKK	3,114 DKK	7.50%	7.71%	A B
4	3,000 DKK	3,152 DKK	10.00%	10.38%	A B
5	3,000 DKK	3,190 DKK	12.50%	13.10%	A B
6	3,000 DKK	3,229 DKK	15.00%	15.87%	A B
7	3,000 DKK	3,268 DKK	17.50%	18.68%	A B
8	3,000 DKK	3,308 DKK	20.00%	21.55%	A B
9	3,000 DKK	3,347 DKK	22.50%	24.47%	A B
10	3,000 DKK	3,387 DKK	25.00%	27.44%	A B
11	3,000 DKK	3,427 DKK	27.50%	30.47%	A B
12	3,000 DKK	3,467 DKK	30.00%	33.55%	A B
13	3,000 DKK	3,507 DKK	32.50%	36.68%	A B
14	3,000 DKK	3,548 DKK	35.00%	39.87%	A B
15	3,000 DKK	3,589 DKK	37.50%	43.11%	A B
16	3,000 DKK	3,630 DKK	40.00%	46.41%	A B
17	3,000 DKK	3,671 DKK	42.50%	49.77%	A B
18	3,000 DKK	3,713 DKK	45.00%	53.18%	A B
19	3,000 DKK	3,755 DKK	47.50%	56.65%	A B
20	3,000 DKK	3,797 DKK	50.00%	60.18%	A B

Table 2: Payoff Table for 12 Month Time Horizon

Payoff Alternative	**Payment Option A** (pays amount below in 1 month)	**Payment Option B** (pays amount below in 13 months)	**Annual Interest Rate** (AR)	**Annual Effective Interest Rate** (AER)	**Preferred Payment Option** (Circle A or B)
1	3,000 DKK	3,076 DKK	2.50%	2.52%	A B
2	3,000 DKK	3,153 DKK	5.00%	5.09%	A B
3	3,000 DKK	3,231 DKK	7.50%	7.71%	A B
4	3,000 DKK	3,311 DKK	10.00%	10.38%	A B
5	3,000 DKK	3,393 DKK	12.50%	13.10%	A B
6	3,000 DKK	3,476 DKK	15.00%	15.87%	A B
7	3,000 DKK	3,560 DKK	17.50%	18.68%	A B
8	3,000 DKK	3,647 DKK	20.00%	21.55%	A B
9	3,000 DKK	3,734 DKK	22.50%	24.47%	A B
10	3,000 DKK	3,823 DKK	25.00%	27.44%	A B
11	3,000 DKK	3,914 DKK	27.50%	30.47%	A B
12	3,000 DKK	4,006 DKK	30.00%	33.55%	A B
13	3,000 DKK	4,100 DKK	32.50%	36.68%	A B
14	3,000 DKK	4,196 DKK	35.00%	39.87%	A B
15	3,000 DKK	4,293 DKK	37.50%	43.11%	A B
16	3,000 DKK	4,392 DKK	40.00%	46.41%	A B
17	3,000 DKK	4,493 DKK	42.50%	49.77%	A B
18	3,000 DKK	4,595 DKK	45.00%	53.18%	A B
19	3,000 DKK	4,700 DKK	47.50%	56.65%	A B
20	3,000 DKK	4,805 DKK	50.00%	60.18%	A B

Table 3: Payoff Table for 24 Month Time Horizon

Payoff Alternative	**Payment Option A** (pays amount below in 1 month)	**Payment Option B** (pays amount below in 25 months)	**Annual Interest Rate** (AR)	**Annual Effective Interest Rate** (AER)	**Preferred Payment Option** (Circle A or B)
1	3,000 DKK	3,153 DKK	2.50%	2.52%	A B
2	3,000 DKK	3,313 DKK	5.00%	5.09%	A B
3	3,000 DKK	3,481 DKK	7.50%	7.71%	A B
4	3,000 DKK	3,655 DKK	10.00%	10.38%	A B
5	3,000 DKK	3,837 DKK	12.50%	13.10%	A B
6	3,000 DKK	4,027 DKK	15.00%	15.87%	A B
7	3,000 DKK	4,226 DKK	17.50%	18.68%	A B
8	3,000 DKK	4,432 DKK	20.00%	21.55%	A B
9	3,000 DKK	4,648 DKK	22.50%	24.47%	A B
10	3,000 DKK	4,873 DKK	25.00%	27.44%	A B
11	3,000 DKK	5,107 DKK	27.50%	30.47%	A B
12	3,000 DKK	5,350 DKK	30.00%	33.55%	A B
13	3,000 DKK	5,604 DKK	32.50%	36.68%	A B
14	3,000 DKK	5,869 DKK	35.00%	39.87%	A B
15	3,000 DKK	6,144 DKK	37.50%	43.11%	A B
16	3,000 DKK	6,431 DKK	40.00%	46.41%	A B
17	3,000 DKK	6,729 DKK	42.50%	49.77%	A B
18	3,000 DKK	7,039 DKK	45.00%	53.18%	A B
19	3,000 DKK	7,362 DKK	47.50%	56.65%	A B
20	3,000 DKK	7,697 DKK	50.00%	60.18%	A B

Table 4: Payoff Table for 36 Month Time Horizon

Payoff Alternative	**Payment Option A** (pays amount below in 1 month)	**Payment Option B** (pays amount below in 37 months)	**Annual Interest Rate** (AR)	**Annual Effective Interest Rate** (AER)	**Preferred Payment Option** (Circle A or B)
1	3,000 DKK	3,233 DKK	2.50%	2.52%	A B
2	3,000 DKK	3,482 DKK	5.00%	5.09%	A B
3	3,000 DKK	3,749 DKK	7.50%	7.71%	A B
4	3,000 DKK	4,035 DKK	10.00%	10.38%	A B
5	3,000 DKK	4,340 DKK	12.50%	13.10%	A B
6	3,000 DKK	4,666 DKK	15.00%	15.87%	A B
7	3,000 DKK	5,015 DKK	17.50%	18.68%	A B
8	3,000 DKK	5,388 DKK	20.00%	21.55%	A B
9	3,000 DKK	5,785 DKK	22.50%	24.47%	A B
10	3,000 DKK	6,210 DKK	25.00%	27.44%	A B
11	3,000 DKK	6,662 DKK	27.50%	30.47%	A B
12	3,000 DKK	7,145 DKK	30.00%	33.55%	A B
13	3,000 DKK	7,660 DKK	32.50%	36.68%	A B
14	3,000 DKK	8,209 DKK	35.00%	39.87%	A B
15	3,000 DKK	8,793 DKK	37.50%	43.11%	A B
16	3,000 DKK	9,415 DKK	40.00%	46.41%	A B
17	3.000 DKK	10,078 DKK	42.50%	49.77%	A B
18	3,000 DKK	10,783 DKK	45.00%	53.18%	A B
19	3,000 DKK	11,532 DKK	47.50%	56.65%	A B
20	3,000 DKK	12,330 DKK	50.00%	60.18%	A B

4. Results

Our null hypotheses are that the discount rates for given time horizons do not differ across households, and that the discount rates for given households do not differ across time horizons.

4.1 Statistical Analysis

The sample consists of 109 observations spread across the four single-horizon sessions, and 115 observations on the multiple-horizon sessions.[18] The statistical analysis takes into account several features of these data.[19] First, we account for the fact that we observe only interval-censored responses, rather than precise values of the IDR. Thus a subject that switched from A to B in option 8 would be viewed as choosing an annual effective rate in the interval (18.68%, 21.55%]. Second, we account for the "panel data" feature of our experiments in which some subjects provided four sets of responses rather than just one. Third, we account for the use of a multi-stage stratified survey design in which 27 strata were employed as explained earlier. Finally, we account for the possibility that market responses are censored by market savings and borrowing rates.[20]

The explanatory variables included in our statistical model are defined as follows:

- T6: binary indicator[21] of the 6-month time horizon;
- T24: binary indicator of the 24-month time horizon;
- T36: binary indicator of the 36-month time horizon;
- FEMALE: binary indicator if the subject was a female;
- YOUNG: binary indicator if the subject was less than 30 years old;
- MIDDLE: binary indicator if the subject was between 40 and 50 years old;
- OLD: binary indicator if the subject was greater than 50 years old;

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[18] The raw data and statistical programs used are all documented in machine-readable form, in a self-extracting archive, at FTP://DMSWEB.BADM.SC.EDU/GLENN/IDRCODE.EXE.

[19] Because of these statistical issues, we refer to the discount rates that are *predicted* by the regression model as the *elicited* discount rates. That is, some statistical analysis is needed to infer the discount rate that is implied by the raw response to the experimental instrument. Although this may seem like semantics, to include the statistical analysis when we refer to the elicited discount rates, it is inevitable given the conceptual issues discussed.

[20] We employ the SVYINTRG procedure of version 6 of *Stata*, documented in StatCorp [1999]. This estimation procedure accounts for the interval-censored nature of the raw data and also allows us to specify that individual responses may be correlated due to the panel structure of the data for multiple-horizon sessions. It also allows us to properly account for the stratification of our overall sample.

[21] As a matter of convention we code all binary indicators with the Boolean interpretation in which a 1 denotes "true" and 0 denotes "false." For example, T6=1 if the observation pertains to the 6-month horizon, and 0 otherwise.

- ❑ LOWER MIDDLE: disposable household income in 1996 between 100,000 and 199,999 Danish kroner;
- ❑ UPPER MIDDLE: disposable household income in 1996 between 200,000 and 299,999 Danish kroner;
- ❑ RICH: disposable household income in 1996 greater than or equal to 300,000 Danish kroner;
- ❑ SKILLED: binary indicator that the subject has completed more than the basic primary and secondary education in Denmark (i.e., completed more than "Basic school, General upper secondary education, and/or Vocational upper secondary education");
- ❑ LONGEDU: binary indicator that the subject has completed some substantial higher education (referred to in Denmark as "medium-cyle or longer-cycle higher education");
- ❑ COPEN: binary indicator that the subject lives in Copenhagen, including "Greater Copenhagen and its suburbs";
- ❑ TOWN: binary indicator that the subject lives in a town with 10,000 or more inhabitants other than Copenhagen;
- ❑ OWNER: binary indicator that the subject lives in an apartment or house that they own;
- ❑ RETIRED: binary indicator that the subject is retired;
- ❑ UNEMP: binary indicator that the subject is unemployed;[22]
- ❑ SINGLE: binary indicator that the subject lives alone, where the subjects were told that a "household is an economic unit, defined as a group of persons who live in the same residence where each person contributes to general expenditures";
- ❑ KIDS: binary indicator that the subject lives with children;
- ❑ MULTIPLE: binary indicator that the subject gave responses in a multiple-horizon session; and
- ❑ GSIZE: variable indicating the size of the group that attended the session that the subject participated in (we also include GSIZE2, the square of GSIZE)

Hence the omitted time horizon is the 12-month case, the omitted age group consists of subjects aged between 30 and 40 years old, and the omitted income category consists of disposable income in 1996 below 100,000 Danish kroner. The characteristics employed in our statistical analysis are

[22] The questionnaire asks subjects to indicate whether they were retired *or* unemployed; there is no collinearity between these two variables.

generally those also used by *Denmarks Statistics* in their household expenditure surveys.[23]

The regression results are presented in Table 5. The overall significance of the regression equation is provided by an adjusted Wald test statistic of the null hypothesis that all coefficients other than the constant are equal to zero. We reject this null hypothesis at any standard level.

The average discount rate elicited over all subjects is approximately 41%. Before examining how these rates vary with the experimental treatments, the absolute level of the elicited rate should be noted. Relative to the extensive experimental literature in which discount rates are elicited with a variety of hypothetical questions, this average is actually quite low. On the other hand, compared to discount rates popularly used in welfare analysis (roughly between 3% and 10%) these rates seem relatively high. Several factors might account for the absolute magnitude of the elicited rates.

First, despite our extensive attempts to encourage credibility, the subjects might have doubted that we would actually follow through on the payments.[24] These are, after all, artificial and constructed payment options. This uncertainty could plausibly have encouraged subjects to view these as "risky" prospects, in turn encouraging them to require a higher rate of return before investing for any longer time period. This particular credibility effect would likely be additive on the elicited discount rates over all time horizons, increasing all elicited discount rates by some fixed amount (e.g., 10 percentage points) to offset the "default risk." The reason that this effect would be constant across time horizons is that the risk of default would not be likely to vary with the time horizon.

Second, since we elicited discount rates over real monetary amounts and operated with a finite budget, we were forced to constrain the amounts of money involved. Compared to many laboratory experiments with real payments, our field experiments use quite large amounts. Nonetheless, the subjects may have perceived these as small amounts of money. Whether or not that leads to a change in revealed discount rates is an open question, but *a priori* folklore amongst experimenters suggests that subjects might not take foregone income seriously if it falls below some subjective threshold. This could lead the subjects not to respond to the incentives offered by foregoing near-term consumption in our experiments.

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[23] These are standard classifications, but also have the advantage of allowing us to map the results into other databases and models that use these classifications for welfare analyses. Specifically, we plan to use these elicited rates to extend the calibration of "generational accounts" for Denmark and computable general equilibrium models for Denmark that represent households as inter-temporal utility maximizers.

[24] It is true that the Ministry of Business and Industry changed it's name to the Ministry of Trade and Industry within the time horizon of the instruments being proffered, but this would not have been known at the time the experiments were conducted, and was largely a superficial change.

We attempt to control for the effect of varying incentives by including the variable GSIZE in our regression model. Payments to subjects varied with the size of the group they participated in, since this (inversely) scaled the probability that the subject would be selected as the one person to actually play out their choices for real payment. By controlling for this variable in the regression model, and generating predictions for the case in which group size was counter-factually assumed to be one, we can ascertain what the regression model predicts would be the elicited discount rate if the probability of being selected was one.

4.2 Elicited Discount Rates

The regression results in Table 5 indicate that individual discount rates vary with a number of important socio-demographic variables. Table 6 uses discount rates predicted from the regression analysis shown in Table 5, and stratifies them by those variables. In each case we implicitly examine the effects on those Danes that do not have the attribute identified, so we are making statements about only one attribute at a time. Qualitative results from Table 5 suggests that (i) middle aged and older people are more patient, (ii) those who own their apartment or house have significantly lower discount rates, and (iii) those that are single appear to be more patient in monetary as well as marital matters.

Some of the individual coefficient results are not significant at popular critical levels and should therefore be interpreted with care. In Table 5 we also report the 90% confidence intervals for the estimated coefficients, to help gauge the extent of the effect of the demographic characteristic on predicted discount rates. Although the results are not significant in the statistical sense that one cannot reject the null hypothesis of no effect at the 10% significance level, the confidence intervals suggest that (i) females may have lower discount rates, (ii) the wealthiest groups in society may have lower discount rates, (iii) those subjects with a significant fraction of their life allocated to completing their formal education may have lower rates, and (iv) people living in Copenhagen may be less patient.

The main hypotheses can be best evaluated by stratifying the elicited discount rates according to the age of the subject and/or the time horizon of the discount rate elicited. These results are presented in Table 6 and Figures 1 through 6.

Figure 1 displays the elicited discount rates averaged over age cohorts, along with 90% confidence intervals for each cohort.[25] The clear tendency, which is statistically significant, is for

[25] Specifically, we calculate the average for age cohorts grouped into 5 intervals: those ages less than 30, those between 31 and 40, those between 41 and 50, those between 51 and 60, and those over 60. Figure 1 then displays these averages for each of the years in the cohort.

discount rates to decline with age. After age 40 they drop significantly.

Figure 2 displays the distribution of elicited discount rates for the four time horizons considered in our experiments. These distributions are shown in terms of "box and whiskers" plots.[26] Although the eyeball may discern a slight drop in elicited discount rates as the horizon becomes longer, this change is not statistically significant.

Figures 3 through 6 report the age-cohort average discount rates for each time horizon, to check that the age effect is robust to the choice of time horizon. Thus we can view Figures 1 and 2 as providing an accurate summary of the results for age and time horizon (i.e., there is no interaction between these two).

With the obvious caveat that we have only elicited discount rates for up to 3 years, we conclude that elicited discount rates do not appear to differ with respect to the time horizon. However, as Figure 1 demonstrates with respect to age, they do differ according to at least one significant demographic characteristic of the individual. In addition, Table 6 shows the detailed results when we stratify the elicited rates according to the other major demographic characteristics. Based on these results we would recommend that welfare analysts employ constant discount rates for specific household types, but that they allow those rates to vary across households.

5. Conclusions

We demonstrate that it is possible to elicit discount rates from individuals in the field using real economic commitments, and that those discount rates are in an *a priori* plausible range. There are variations in discount rates across some socio-demographic characteristics of the Danish population, implying that inter-temporal welfare evaluations for those household groups should take these differences into account. On the other hand, elicited discount rates do not vary with respect to the time horizon used here, consistent with the use of constant discount rates for *given* household types.

[26] These plots seek to display the salient characteristics of the distribution in an efficient manner. The middle line represents the median, or 50th percentile. The box itself identifies the 25th and 75th percentiles. The top whisker extends up to the data point that is equal to the 75th percentile plus 1.5 times the difference between the 25th and 75th percentiles, and symmetrically for the lower whisker. Values outside this range are plotted as appropriate.

Table 5: Results of Regression On Raw IDR Responses

Variable	Coefficient	Standard Error	*t*	Pr. > |*t*|	90% Confidence Interval	
T6	5.10	3.03	1.68	0.09	0.10	10.10
T24	-1.94	2.79	-0.70	0.49	-6.55	2.67
T36	-2.32	3.23	-0.72	0.47	-7.66	3.01
FEMALE	-6.43	5.55	-1.16	0.25	-15.59	2.73
YOUNG	-4.92	9.39	-0.52	0.60	-20.43	10.59
MIDDLE	-11.87	6.97	-1.70	0.09	-23.37	-0.36
OLD	-16.83	8.81	-1.91	0.06	-31.39	-2.27
LOWER MIDDLE	5.65	7.30	0.77	0.44	-6.41	17.70
UPPER MIDDLE	-1.60	8.77	-0.18	0.86	-16.08	12.89
RICH	-8.53	10.60	-0.81	0.42	-26.05	8.98
SKILLED	2.60	7.13	0.37	0.72	-9.17	14.37
LONGEDU	-8.62	7.38	-1.17	0.24	-20.80	3.57
COPEN	10.78	8.10	1.33	0.19	-2.60	24.17
TOWN	1.48	6.58	0.23	0.82	-9.39	12.35
OWNER	-20.57	6.98	-2.95	0.00	-32.10	-9.03
RETIRED	1.90	9.37	0.20	0.84	-13.58	17.38
UNEMP	11.99	11.91	1.01	0.32	-7.68	31.66
SINGLE	-12.06	6.52	-1.85	0.07	-22.83	-1.29
KIDS	2.27	7.02	0.32	0.75	-9.32	13.86
MULTIPLE	-3.47	5.52	-0.63	0.53	-12.59	5.65
GSIZE	3.36	3.87	0.87	0.39	-3.04	9.77
GSIZE2	-0.13	0.18	-0.69	0.49	-0.43	0.17
CONSTANT	50.70	23.73	2.14	0.03	11.50	89.90

Note: The regression model is interval-censored, with allowance for the stratification of the sample and the non-independence of observations for individuals in multiple-horizon sessions. See text for details.

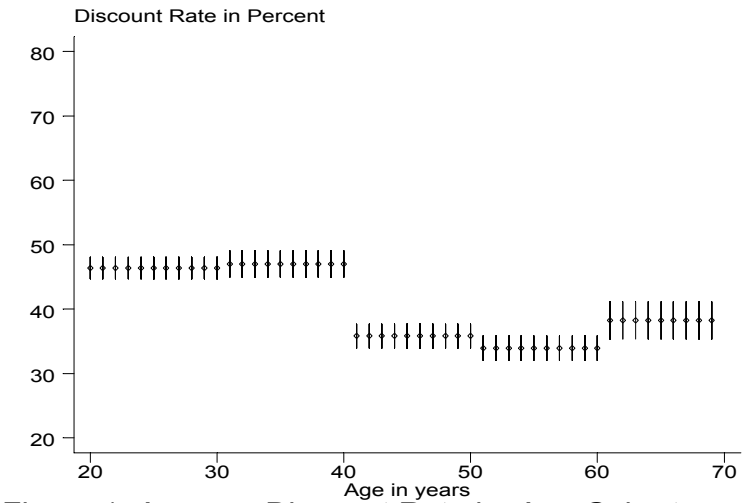

Figure 1: Average Discount Rate by Age Cohort

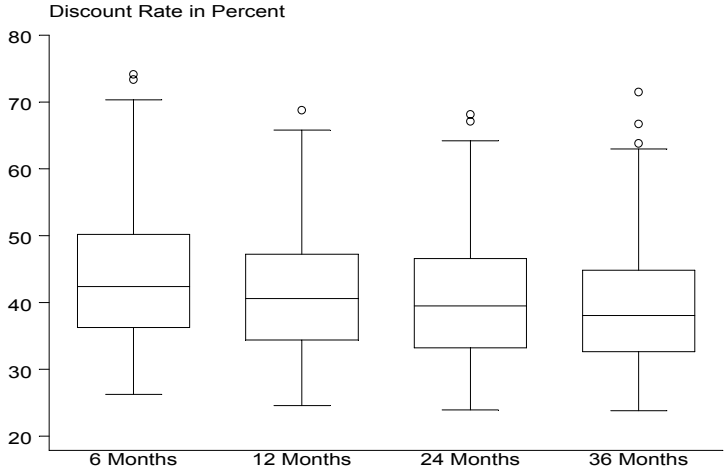

Figure 2: Elicited Discount Rates by Horizon

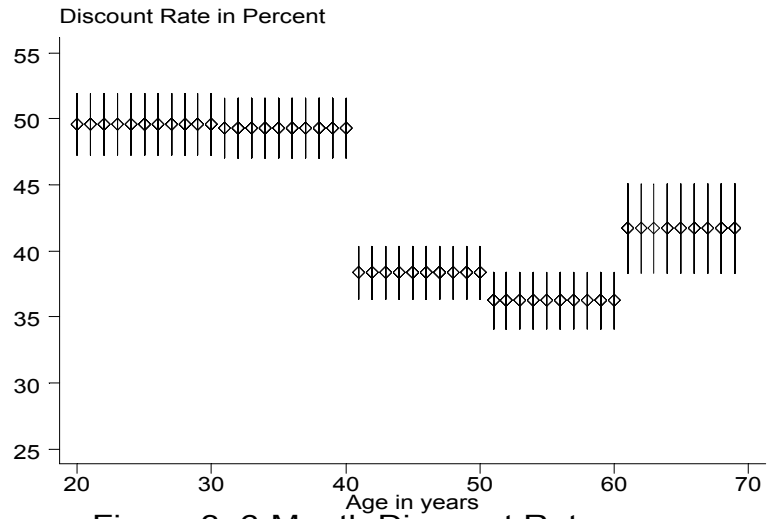

Figure 3: 6-Month Discount Rate

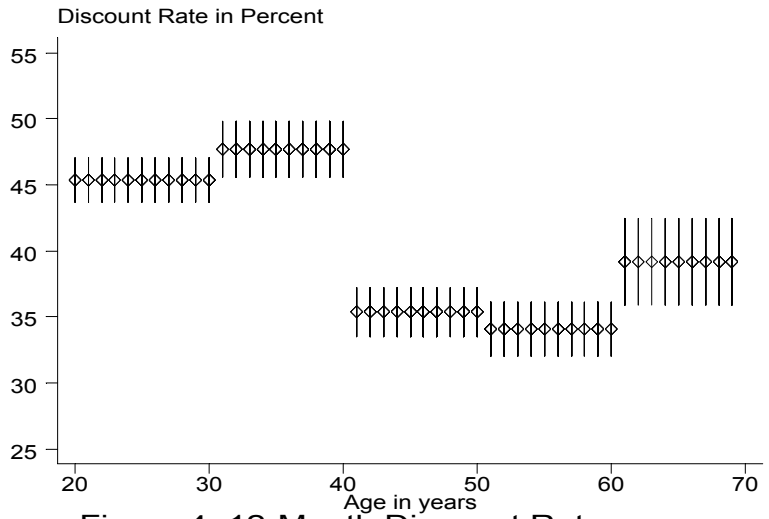

Figure 4: 12-Month Discount Rate

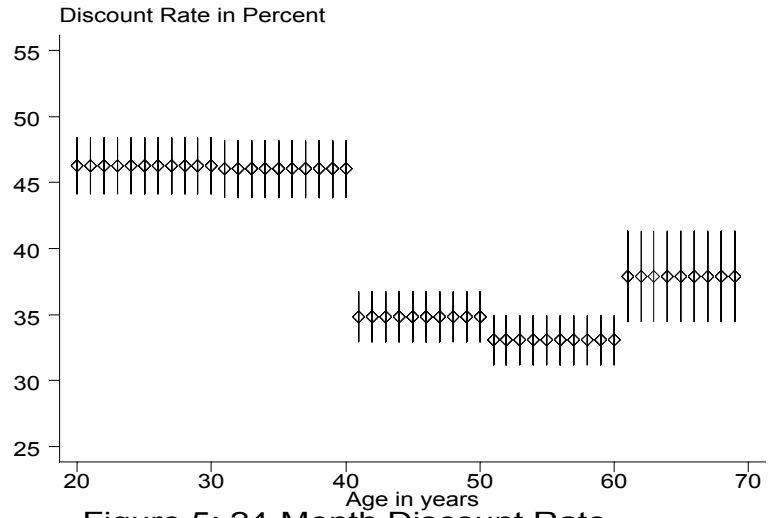

Figure 5: 24-Month Discount Rate

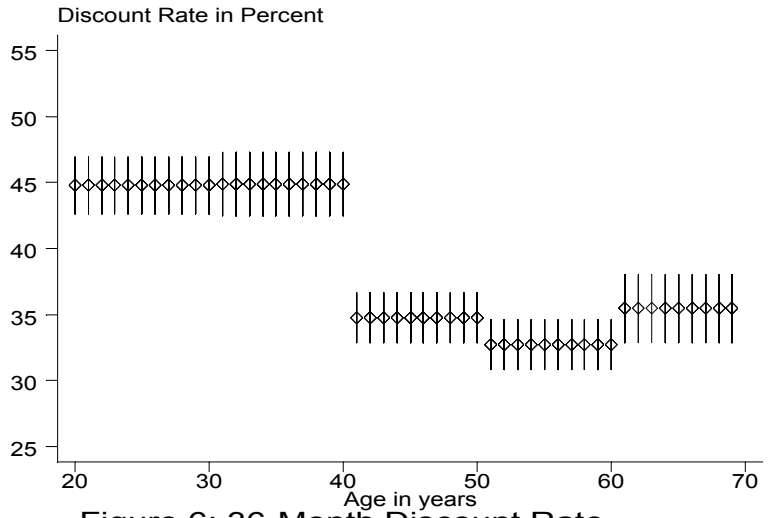

Figure 6: 36-Month Discount Rate

Table 6: Average Elicited Discount Rates Stratified by Major Demographics

Demographic Characteristic	Average	Standard Error	90% Confidence Interval		Sample
all	41.24	0.66	39.94	42.55	646
Male	43.17	0.98	41.24	45.09	317
Female	39.39	0.91	37.59	41.18	329
Poor	42.69	1.20	40.33	45.05	166
Lower Middle Income	44.24	1.03	42.21	46.27	276
Upper Middle Income	37.84	1.59	34.71	40.96	121
Rich	33.34	1.13	31.12	35.57	83
Renter	49.03	0.84	47.37	50.69	272
Owner	35.58	0.60	34.39	36.76	374
No Children	39.95	0.82	38.35	41.56	399
Children	43.32	1.13	41.10	45.54	247
Active	41.41	0.73	39.96	42.85	564
Retired	40.12	1.74	36.69	43.56	82
Working	40.98	0.68	39.64	42.32	616
Unemployed	46.60	2.13	42.41	50.80	30
Not a Student	40.58	0.70	39.20	41.95	576
Student	46.71	2.17	42.43	51.00	70
Not in Copenhagen	40.00	0.74	38.54	41.46	487
Copenhagen	45.03	1.49	42.09	47.97	159
Unskilled	42.63	1.01	40.65	44.62	270
Skilled	40.24	0.90	38.47	42.02	376
Less Education	43.06	0.79	41.51	44.61	460
More Education	36.76	1.17	34.45	39.07	186

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