Estimating Individual Discount Rates in Denmark: A Field Experiment

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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.

Since discount rates are a reflection of subjective time preferences, one would expect a priori that they could differ across different individuals.[1] However, standard practice in intertemporal welfare analyses is 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 sociodemographic 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 Manbeth Coller and Williams (1999), who elicit nominal individual discount rates for university students using controlled laboratory experiments.[2] 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 nominal[3] discount rates are constant over the one-year to three-year horizons used in these experiments, and that discount rates vary significantly with respect to several sociodemographic variables. *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 I we review the logic of our experimental design. Section II explains the field

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[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.

[2] Coller and Williams (1999) explain how their design relates to findings in the extant experimental literature. We review this discussion in our working paper, available at ⟨http://dmsweb.badm.sc.edu/glenn/idr/dkidr.htm⟩. This web page also contains links to all experimental instructions, data, and software to replicate our results. For the convenience of Danish-challenged readers we also provide on this web site an English translation of all instructions and questionnaires.

[3] At the time of the experiments the inflation rate in Denmark was just under 2 percent per annum, and had been steady for several years. It rose to 3 percent per annum by the end of the longest horizon used in our experiments. The realized rates of inflation, taking the front-end delay into account, were 0.3 percent, 1.2 percent, 3.2 percent, and 6.1 percent for the 6-, 12-, 24-, and 36-month horizons, respectively.

experiments conducted, and Section III examines the results and relates them to those found in the existing literature.

I. Experimental Design

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 percent per day; otherwise, we can infer that it is x percent 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 he switches from choosing the current income option to taking the future income option provides a bound on his discount rate. That is, if an individual takes the current income option for all x from 0 to 10, then takes the future income option for all x from 11 up to 100, we can infer that his discount rate lies between 10 percent and 11 percent 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[4] 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 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 subjects were randomly assigned to a session in which they were asked to consider one of these time horizons. In these sessions we only elicited 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[5] 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.

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

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

It is easy to see how this censoring problem applies here. Consider a subject with a true individual discount rate (IDR) of 30 percent. 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 percent or higher. Now assume that this subject can *borrow* in the field at a rate of 14 percent. Although she demands at least 30 percent interest to delay consumption and save in the lab, at rates between 14 percent and 30 percent she is better off borrowing in the field at 14 percent and not delaying consumption in the field, leaving the money in the lab earning 14 percent 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 percent 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 equal to the borrowing rate.[6] 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 percent. Instead, we can only infer from these lab responses that the subject has a true IDR between 14 percent and ∞. The problem is symmetric for censoring with respect to savings rates, although less significant empirically.[7]

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.[8]

II. The Danish Experiments

A. *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.[9] 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

[6] 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.

[7] Consider, for example, a subject with a true IDR of 3 percent. 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 percent or higher. Now suppose that this subject can *save* in the field at a rate of 10 percent. Although she would be willing to save at 3 percent, at rates between 3 percent and 10 percent 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.

[8] 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 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* interest (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.

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

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.

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 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 percent of the population between 19 and 75 years of age live in Copenhagen, and three sessions with a total of 30 participants were held in Copenhagen, which corresponds to 11.1 percent of the total sample size.[10]

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, weighing the two subgroups with respect to the relative size of the population between 19 and 75 years of age.

The interviewers initially contracted 6, 12, or 17 persons, the number depending on the specific session and assuming a show-up rate of approximately 80 percent. 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 designed to be equally split between single-horizon and multiple-horizon treatments, and then equally split by time horizon within the single-horizon treatments. All subjects were randomly assigned to treatment condition.[11] The sample was representative of the adult population of Denmark, due to the stratified sampling methods employed.

B. *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. The initial contact letter to the subjects posed the general nature of the task, and informed subjects that they would be paid 500 DKK after participating in the survey and that one subject would receive at least 3,000 DKK. No other details of the experiment were provided until the subjects arrived at the session.

[10] 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 three communes: Copenhagen, Frederiksberg, and Gentofte. The total population in this area is 600,000 people, which is around 11 percent 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 percent 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.

[11] Due to the vagaries of no-shows, the actual sample differs slightly from this design. There were 118 subjects in the 15 single-horizon experiments, and 150 subjects in the 12 multiple-horizon experiments. Within the single-horizon experiments there were 26, 32, 31, and 29 subjects, respectively, in the 6-month, 12-month, 24-month, and 36-month treatments.

TABLE 1—PAYOFF TABLE FOR THE 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

Upon arrival at the experimental session, subjects were given the following information:

> *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.

Subjects were given payoff tables as illustrated in Table 1. They were told that they must choose between payment Options A and B for each of the 20 payoff alternatives. Option A was 3,000 DKK in all sessions. Option B paid 3,000 DKK + X DKK, where X ranged from annual rates of return of 2.5 percent to 50 percent on the principal of 3,000 DKK, compounded quarterly to be consistent with general Danish banking practices on overdraft 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. Subjects were then told that a single payment option would be chosen at random for payment, and that a single subject would be chosen at random to be paid his preferred payment option for the chosen payoff alternative. The payment mechanism was explained as follows:

> 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.

Finally, prior to the choice task, the experimenter illustrated the randomization devices in a trial

experiment which utilized different quantities of candies as payoffs. The trial Assignee was paid his candies at the end of the trial experiment, to illustrate the concrete nature of the payoffs.

The instructions for the 12-month, 24-month, and 36-month horizon experiments were identical except for the obvious changes. 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. All four time horizons were presented simultaneously to the subject, who could respond to them in any order.[12] One time horizon was then selected for possible payment, and the remaining procedures were identical to the single-horizon sessions.

Across all time horizons, payoffs to any one subject could range from 3,000 DKK up to 12,333 DK. The exchange rate in mid-1997 was approximately 6.7 DKK per U.S. dollar, so this range converts to $450 and $1,840.

C. *Additional Experimental Questionnaires*

In addition to the primary elicitation task, we collected information from subjects on a variety of sociodemographic 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 to help us identify the market circumstances within which the discount rate responses should be viewed. Specifically, we collected information on whether the subject had various accounts (e.g., checking account, credit card, line of credit), the annual interest rate on those accounts, and the current balance. We also collected information on the subject's perception of his or her chances of obtaining a loan, line of credit, or credit card.

[12] The literal sequence of the time-horizon payoff tables in the survey instrument was the natural one, with the 6-month horizon coming first.

III. 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.

A. *Statistical Analysis*

After removing subjects that gave incomplete or inconsistent responses, the final sample consists of 109 observations spread across the four single-horizon sessions, and 132 observations on the multiple-horizon sessions.[13] The statistical analysis takes into account four features of these data.[14] 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 percent, 21.55 percent]. Second, we account for the stratification of our national sample, as described earlier. Third, we account for the "panel data" feature of our experiments in which some subjects provided four sets of responses rather than just one.[15] Finally, we account for the possibility that market responses are censored by market savings and borrowing rates.[16]

[13] An inconsistent response is one in which the subject switched between A and B more than once. This occurred in only 3 percent of the responses, reflecting 4 percent of the subjects. The remaining sample reductions are from subjects that neglected to answer some core demographic question.

[14] 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.

[15] This feature amounts to a multistage sampling design in which there are up to four observations for each "primary sampling unit," which in our case is the individual subject. The regression procedure we use allows for any amount of correlation within the observations for each primary sampling unit. See StataCorp (2001, User's Guide, p. 324).

[16] We estimate an interval regression model recognizing the features of the complex survey design used, employing version 7 of *Stata* documented in StataCorp (2001).

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

T6, T12, T24, and T36: binary indicators[17] of the 6-month, 12-month, 24-month, and 36-month time horizons, respectively;
MULTIPLE: binary indicator that the subject gave responses in a multiple-horizon session;
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;
MIDDLE1: disposable household income in 1996 between 100,000 and 199,999 Danish kroner;
MIDDLE2: 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");
STUDENT: binary indicator that being a student was the primary occupation in the last year;
LONGEDU: binary indicator that the subject has completed some substantial higher education (referred to in Denmark as "medium-cycle 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 the subject owns;
RETIRED: binary indicator that the subject is retired;
UNEMP: binary indicator that the subject is unemployed;
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;
GSIZE: variable indicating the size of the group that attended the session that the subject participated in;
BALANCE: binary indicator that the subject carries a positive balance in a line of credit[18] or credit card; and
CHANCES: binary indicator that the subject believes that the chances of getting a line of credit or credit card approved if the subject went to a bank are poor (less than 75 percent likely).

The characteristics employed in our statistical analysis are generally those also used by *Denmarks Statistics* in its household expenditure surveys.[19]

The regression results are presented in Table 2. 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 28 percent. Before

[17] 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.

[18] It is common for Danes to carry a prearranged personal line of credit at a bank, so we view this as being similar to the credit card balances that Americans might carry in terms of convenience of access.

[19] 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 intertemporal utility maximizers.

TABLE 2—REGRESSION ANALYSIS OF DISCOUNT RATE RESPONSES

Variable	Coefficient	Standard error	t	$P > \|t\|$	90-percent confidence interval	
T6	34.86076	7.908359	4.41	0.000	21.8014	47.92012
T12	28.95233	7.976701	3.63	0.000	15.78012	42.12454
T24	27.44078	8.018661	3.42	0.001	14.19928	40.68228
T36	27.87162	8.046035	3.46	0.001	14.58491	41.15832
MULTIPLE	0.8359218	2.228436	0.38	0.708	−2.843975	4.515818
FEMALE	1.014945	2.713695	0.37	0.709	−3.466278	5.496168
YOUNG	−1.094671	3.934629	−0.28	0.781	−7.592065	5.402722
MIDDLE	0.1785973	3.446215	0.05	0.959	−5.512261	5.869455
OLD	−0.4595653	3.754661	−0.12	0.903	−6.659771	5.740641
MIDDLE1	−1.305936	3.674648	−0.36	0.723	−7.374014	4.762143
MIDDLE2	−3.214197	4.309141	−0.75	0.456	−10.33004	3.901641
RICH	−5.341135	4.102213	−1.30	0.194	−12.11527	1.432997
SKILLED	0.7426614	3.275909	0.23	0.821	−4.666965	6.152288
STUDENT	4.204929	5.285858	0.80	0.427	−4.523798	12.93366
LONGEDU	−9.202757	3.174322	−2.90	0.004	−14.44463	−3.960884
COPEN	−1.13076	3.209827	−0.35	0.725	−6.431263	4.169742
TOWN	3.171888	2.845343	1.11	0.266	−1.52673	7.870505
OWNER	−3.764708	3.030948	−1.24	0.215	−8.769821	1.240406
RETIRED	12.37832	5.048285	2.45	0.015	4.041905	20.71473
UNEMP	−7.769304	4.437314	−1.75	0.081	−15.0968	−0.4418082
SINGLE	−2.401655	3.009327	−0.80	0.426	−7.371065	2.567755
KIDS	0.2497801	3.11824	0.08	0.936	−4.899481	5.399041
GSIZE	0.0238708	0.3650134	0.07	0.948	−0.5788889	0.6266305
BALANCE	1.829445	2.61292	0.70	0.485	−2.485364	6.144253
CHANCES	7.648062	3.996732	1.91	0.057	1.048115	14.24801

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 analyses (roughly between 3 percent and 10 percent) 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.[20] 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 forgone income seriously if it falls below some subjective threshold. This

[20] 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.

could lead the subjects not to respond to the incentives offered by forgoing near-term consumption in our experiments.

We attempt to control for the effect of varying incentives by including the variable GSIZE in our regression model. Expected 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 his choices for real payment. By controlling for this variable in the regression model, and generating predictions for the case in which group size was counterfactually 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.

B. *Elicited Discount Rates*

The regression results are presented in Table 2. Each of the four time horizon treatments (denoted T6, T12, T24, and T36) generates an equation intercept, while the remainder of the coefficients can be directly interpreted as the marginal effect of each variable. An alternative way to view the effects of demographics is to generate predicted discount rates for everyone in the sample and then to stratify these predicted rates. These results are shown in Table 3. The demographic results in Table 3 show the effect of varying the indicated variable and all other characteristics that are associated with it. Thus, if women are better educated on average than men in Denmark, the effect of sex in Table 3 will include the effect of this difference in education whereas the marginal effect on that coefficient in Table 2 will not. We report both sets of demographic breakdowns since each is of policy interest.

Table 2 indicates that there was some difference in the estimated discount rates for the 6-month horizon compared to the others. Varying the time horizon appears to have no effect on discount rates for the 12- to 36-month time horizons, while rates for the 6-month time horizon are roughly 6 percentage points higher.[21] An F-test confirms these claims. The only demographic characteristics that appear to matter in Table 2 are (i) the length of education, which is associated with a discount rate over 9 percentage points lower than otherwise; (ii) retirement, which is associated with a discount rate over 12 percentage points higher than otherwise; and (iii) unemployment, which is associated with a discount rate just over 7 percentage points lower than otherwise.[22] In addition, if the individual perceives that they have a *poor* chance of getting a loan or credit card approved at a bank, their discount rate is over 7 percentage points higher.

Although the individual coefficients do not indicate statistical significance at the conventional levels, we also observe a lowering of estimated discount rates as incomes rise. However, this marginal effect could be correlated with investments in education. For this reason it is appropriate to examine the fully stratified results in Table 3, which show the joint effects of each demographic characteristic and those other characteristics correlated with it.

Table 3 generates several interesting results,[23] complementing the marginal effects of Table 2:

- The overall individual discount rate in Denmark is estimated to be 28.1 percent. This reflects the stratification of our sample in order to obtain an efficient estimate of the national average. Figure 1 displays the distri-

[21] The standard error of prediction from this statistical model is 6.5 percentage points. The median is very close to the mean, since the distribution of estimated discount rates is relatively symmetric. Hence we refer to mean estimates throughout.

[22] In each case we can plausibly entertain hypotheses that allow the causality to go both ways. In fact, one of the motivating policy forces behind our survey was a concern that Danes did not invest enough in education. Our results suggest that those that do invest in education may do so because they simply have a lower discount rate, and are more willing to trade off near-term costs for longer-term payoffs.

[23] The total sample in Table 3 is listed as 696, even though some observations were deleted in the regression analysis in Table 2. The reason is that the complete sample is utilized when adjusting the standard errors for the sample stratification.

TABLE 3—AVERAGE ELICITED DISCOUNT RATES STRATIFIED BY MAJOR DEMOGRAPHICS

Demographic characteristic	Estimate	Standard error	90-percent confidence interval		Observations
ALL	28.1464	0.53537	27.26233	29.03048	696
Male	28.06626	0.76262	26.80692	29.3256	336
Female	28.22121	0.7667374	26.95507	29.48735	360
Young	28.71521	0.9551633	27.13791	30.2925	146
Middle (30–40)	28.35924	0.8708021	26.92125	29.79722	199
Middle (41–50)	25.05474	1.065985	23.29444	26.81503	158
Old	30.02767	1.256172	27.95331	32.10203	193
Poor	32.92452	1.014352	31.24948	34.59955	171
Lower middle	30.08146	0.676202	28.96482	31.19809	280
Upper middle	22.68201	0.7520371	21.44014	23.92387	126
Rich	22.51315	1.251744	20.4461	24.5802	119
Unskilled	31.42633	0.7387784	30.20636	32.6463	295
Skilled	25.73349	0.6889163	24.59586	26.87113	401
Not a student	27.48244	0.5661343	26.54756	28.41732	621
Student	33.64402	1.291917	31.51063	35.7774	75
Less educated	30.9838	0.547016	30.0805	31.88711	506
More educated	20.58996	0.7659382	19.32514	21.85479	190
Not Copenhagen	28.50351	0.5887187	27.53133	29.47568	531
Copenhagen	26.99719	1.236626	24.9551	29.03927	165
Not in a town	26.79067	0.7654368	25.52668	28.05466	388
Town	29.85428	0.7091534	28.68323	31.02533	308
Not an owner	31.66546	0.7322497	30.45627	32.87465	291
Owner	25.6179	0.6893576	24.47953	26.75626	405
Active	26.52264	0.4946091	25.70587	27.3394	603
Retired	38.67471	1.029985	36.97386	40.37557	93
Working	28.38739	0.5465463	27.48486	29.28992	655
Unemployed	24.29656	1.699674	21.48983	27.1033	41
Married	27.47882	0.7236279	26.28387	28.67377	453
Single	29.39091	0.8189498	28.03855	30.74328	243
No children	28.89642	0.7119442	27.72076	30.07208	431
Have children	26.92657	0.8296289	25.55658	28.29657	265
No balance	28.19078	0.7941139	26.87943	29.50213	387
Carries a balance	28.09083	0.7535453	26.84647	29.33518	309
Good chances	27.10611	0.5349895	26.22266	27.98956	611
Poor chances	35.62428	1.365615	33.36919	37.87937	85

bution of estimated discount rates, which is roughly normal.

- The discount rates for men and women appear to be identical, confirming the marginal effects in Table 2. This result is particularly notable since many other characteristics vary with sex.
- Discount rates appear to decline with age, at least after middle age.
- There does appear to be a significant lowering of the discount rate for higher income individuals, when we allow these individuals to "carry with them" the other characteristics they typically have, such as more education.

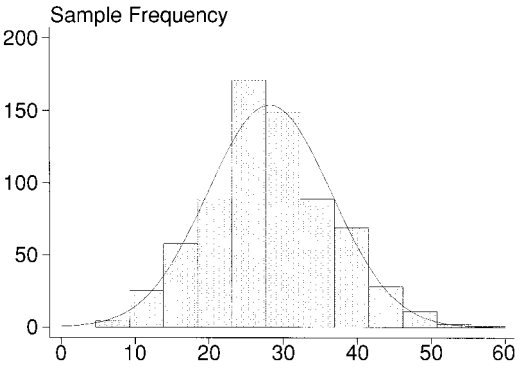

FIGURE 1. ESTIMATED DISCOUNT RATES FOR THE DANISH POPULATION

Households in the highest income range have discount rates that are over 10 percentage points lower than those in the lowest range. This difference between Table 3 and Table 2 illustrates the potential importance of examining demographic effects both ways.

- Table 3 also shows a large difference between the discount rates of skilled and unskilled individuals, with those that have skills having a significantly lower discount rate.
- Perhaps surprisingly, students have a higher discount rate than nonstudents.
- The importance of the extent of education from Table 2, measured by variable LONGEDU, is confirmed in Table 3: those with longer investments in education are also those with substantially lower discount rates.
- Ownership of a house is associated with having a lower discount rate, perhaps because home ownership is correlated with other demographics associated with lower discount rates, such as income and having children.
- Retired individuals have higher discount rates, confirming the marginal effect from Table 2.
- The unemployed have lower discount rates than the employed.
- Finally, poor perceived chances of being turned down for a loan or credit card by a bank are associated with the individual having much higher discount rates, as one would expect. This result also appeared in Table 2, and seems to cut across other demographics.

C. *Comparison to the Literature*

There have been several attempts to estimate discount rates for individuals in field settings using financial instruments.[24] All of them find relatively high discount rates.

Lawrence M. Ausubel (1991, Table 11, p. 70) shows that nearly three-quarters of those holding credit cards in banks he surveyed do not pay off their balance on time and avoid finance charges, despite the fact that those finance charges amount to roughly 19 percent per annum. We find that subjects in our experiments that hold comparable balances in Denmark have essentially the same discount rates as those that do not hold such balances (see Tables 2 and 3).

John T. Warner and Saul Pleeter (2001) estimate individual discount rates for a large number of U.S. military personnel who were offered voluntary separation options. One option was an initial lump-sum payment, and the other was an annuity. They estimate (Table 6, p. 48) that officers had an average discount rate of between 10 percent and 19 percent, depending on the statistical specification assumed, and that enlisted personnel had discount rates between 35 percent and 54 percent.

Although these findings refer to selected segments of the population, albeit large segments with a diverse range of sociodemographic characteristics, they suggest that the level of discount rates that we find for the Danish population is consistent with other field evidence.

IV. Conclusions

We demonstrate that it is possible to elicit discount rates from individuals in the field using real economic commitments, and that those dis-

[24] There are also numerous studies estimating large discount rates implicit in the purchase of alternative consumer durables, and numerous laboratory studies using student subjects that utilize financial instruments also find large discount rates. Henry Ruderman et al. (1986) review the former, and Coller and Williams (1999) review the latter. The only laboratory experiments with lower discount rates, that we are aware of, are those of Coller and Williams (1999), whose design we employed here. They find annual rates for American college students in the 15-percent to 18-percent range.

count rates are in an a priori plausible range. There are variations in discount rates across some sociodemographic characteristics of the Danish population, implying that intertemporal 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 beyond one year, consistent with the use of constant discount rates for *given* household types for those horizons.

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