Dynamic Consistency in Denmark:

A Longitudinal Field Experiment

by

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Abstract. Evidence that individuals have dynamically consistent preferences is usually generated by studying the discount rates of the individual over different horizons, but where those rates are elicited at a single point in time. If these elicited discount rates vary by horizon the individual is typically claimed to have preferences that imply a dynamic inconsistency, although this inference requires additional assumptions such as intertemporal separability. However, what one really wants to know is if the same subject has the same discount rate function when that individual is asked at a later point in time. Such panel tests then require than one allow for possible changes in the states of nature that the subject faces, since they may confound any in-sample comparisons of discount rate functions at different points in time. We report the results of a large-scale panel experiment undertaken in the field that allows us to examine this issue. In June 2003 we elicited subjective discount rates from 253 subjects, representative of the adult Danish population. Between September 2003 and November 2004 we re-visited 97 of these subjects and repeated these tasks. In each visit we also elicited information on their individual characteristics, as well as their expectations about the state of their own economic situation and macroeconomic variables. We find evidence in favor of dynamic consistency.

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Evidence that individuals have dynamically consistent preferences is usually generated by studying the discount rates of the individual over different horizons, but where those rates are elicited at a single point in time. Thus a subject might be asked to state their discount rate over an 18-month period starting at some reference point in time, and then state their discount rate over a 24-month period starting from the same reference point. If these elicited discount rates vary by horizon the individual is typically claimed to have preferences that imply a "dynamic inconsistency," although this inference requires additional assumptions such as inter-temporal separability and an assumed temporal stability in the discount rate function elicited at some point in time. If these assumptions are made, then the individual could behave in a dynamically inconsistency manner, by holding and acting on preferences at one point in time that contradict the preferences of the same individual at a later date.

Following Strotz [1955-56], there have been two broad theoretical responses to this possibility. The first is to explore alternative ways in which the decision-maker might *look ahead* today and take deliberate actions today in awareness of the potential future problem. For example, the agent might knowingly choose to engage in addictive behavior, despite the known consequences (which could be probabilistic). Or the agent might undertake some pre-commitment strategy by constraining the feasible set of future choices. The second broad approach is to explore the implications of the agent *looking backwards* when confronted with the later choice, and taking the earlier choice into account with some non-separable multi-period utility function (e.g., Machina [1989] and McClennan [1990]). Thus, even if there were evidence that the decision-maker holds preferences that *might* lead to dynamically inconsistent choices, it does not follow that such choices will ever be observed in the sense that the decision-maker would want to change behavior over the entire time period.

However, before worrying about ways that the individual could address possible dynamic inconsistencies, we need to be sure that the behavioral premiss is valid. What one really wants to know is if the same subject has the same discount rate *function* when that individual is asked at a *later* *point in time*. To take the earlier example, the issue is whether the 6-month discount rate of the individual in our earlier example changes in a year's time, when it spans the remaining time of the original 18-month discount rate; or whether the 6-month discount rate of the individual changes in 18 months time, when is spans the remaining time of the original 24-month discount rate. Only if these "overlapping" discount rates differ can we say that there is a possibility of a dynamic inconsistency. As noted, this is typically tested by eliciting a "snapshot" of the discount rate function at some point in time, and then just assuming the (popular) structure that causes problems.

Remarkably, there have been few direct tests of this empirical premiss of the dynamic inconsistency literature using real rewards. Such longitudinal tests require that one allow for possible changes in the states of nature that the subject faces, since they may confound any in-sample comparisons of discount rate functions at different points in time.

We report the results of a large-scale longitudinal experiment undertaken in the field that allows us to examine this issue.[1] In June 2003 we elicited subjective discount rates from 253 subjects, representative of the adult Danish population. Between September 2003 and November 2004 we re-visited 97 of these subjects and repeated these tasks. In each visit we also elicited information on their individual characteristics, as well as their expectations about the state of their own economic situation and macroeconomic variables. We find evidence in favor of dynamic consistency.

We review our experimental design in section 1, and discuss our procedures in section 2. We present the evidence in section 3, using in-sample comparisons and out-of-sample comparisons. We also consider the effects of allowing for changes in the states of nature that might confound any inferences. In section 4 we review previous evidence on these issues, and in section 5 we draw conclusions.

[1] Our experiments are "artefactual field experiments" in the terminology of Harrison and List [2004]. List [2003] appears to have reported the first longitudinal field experiment.

1. Experimental Design

A. The Basic Elicitation Procedure

The basic experimental design for eliciting individual discount rates (IDRs) was introduced in Coller and Williams [1999] (CW) and expanded in Harrison, Lau and Williams [2002] (HLW). The basic question asked of subjects is extremely simple: do you prefer \$100 today or \$100+x tomorrow, where x is some positive amount? If the subject prefers the \$100 today then we can infer that the discount rate is higher than x% per day; otherwise, we can infer that it is x% per day or less. The format of the previous experiments modified and extended this basic question in six ways, which we retain here.

First, we pose a number of such questions to each individual, each question varying x by some amount. When x is zero we would obviously expect the individual to reject the option of waiting for no rate of return. As we increase x we would expect more individuals to take the future income option. For any given individual, the point at which they switch from choosing the current income option to taking the future income option provides a bound on their discount rate. That is, if an individual takes the current income option for all x from 0 to 10, then takes the future income option for all x from 11 up to 100, we can infer that his discount rate lies between 10% and 11% for this time interval. The finer the increments in x, the more precisely we will be able to pinpoint the discount rate of the individual.

Second, the experimental task used an Multiple Price List (MPL) format, simultaneously posing several questions with varying values of x. After all questions had been completed by the individual, one of the questions was chosen at random for actual payment. In this way the results from one question do not generate income effects which might influence the answers to other questions. This feature of the design mimics the format used by Holt and Laury [2002] in their risk aversion experiments: in that case the rows reflected different probabilities of each prize, and in this case the rows reflect different annual effective rates of return.[2]

Third, subjects are provided two future income options rather than one "instant income" option and one future income option. For example, they might be offered \$100 in one month and \$100+x in 7 months, so that we interpret the revealed discount rate as applying to a time horizon of 6 months. This avoids the potential problem of the subject facing extra risk or transactions costs[3] with the future income option, as compared to the "instant" income option. If the delayed option were to involve such additional transactions costs, then the revealed discount rate would include these subjective transactions costs. By having both options entail future income we hold these transactions costs constant.

Fourth, subjects were asked to provide information to help identify what market rates of interest they face. This information was used to allow for the possibility that their responses in the discount rate task are *censored* by market rates.[4]

Fifth, respondents were provided with information on the interest rates implied by the delayed payment option. This is an important control feature if field investments are priced in terms of interest rates. If subjects are attempting to compare the lab investment to their field options, this feature may serve to reduce comparison errors since now both lab and field options are priced in the same metric.[5]

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[2] We exploit this similarity of format in the design of our computerized interface to subjects, and in the use of trainers in the risk aversion task as a generic substitute for trainers in the discount rate task.

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

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

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

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

Subjects in the HLW experiments were given payoff tables such as the one illustrated in Table 1. They were told that they must choose between payment Options A and B for each of the 20 payoff alternatives. Option A was 3000 DKK in all sessions. Option B paid 3000 DKK + X DKK, where X ranged from annual rates of return of 2.5% to 50% on the principal of 3000 DKK, compounded quarterly to be consistent with general Danish banking practices on overdraft 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.

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

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

In addition, there are some minor changes in payment procedures. In the HLW experiments, a certificate for future payment was guaranteed by the Social Research Institute, which was redeemable on the payment date for a check issued by that Institute. In this study, future payments are guaranteed by the Danish Ministry of Economic and Business Affairs, and made by automatic transfer from the Ministry's bank account to the subject's bank account.[6] This payment procedure is similar to a post-dated check, and automatic transfers between bank accounts are a common

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

procedure in Denmark.[7] We conjecture that this feature will reduce transaction costs and credibility issues associated with future payments. Finally, while CW and HLW randomly select a single "Assignee" from the group of subjects in a given session to actually receive the payment associated with his decision, in these new experiments each subject is given a 10% chance to receive actual payment.

We implement one extension of the basic MPL procedure in order to elicit more refined intervals of IDRs, but maintaining the transparency of the incentives of the basic MPL. We do so in the form of a computerized variant on the basic MPL format which we call an Iterative MPL (iMPL).

The basic MPL is the standard format in which the subject sees a fixed array of paired options and chooses one for each row. It allows subjects to switch back and forth as they like, and has already been used in many experiments. The iMPL format extends this by first asking the subject to simply choose the row at which he wants to first switch from option A to option B, assuming monotonicity of the underlying preferences to automatically fill out the remaining choices. The second extension of the MPL format is to then allow the individual to make choices from refined options within the option last chosen. That is, if someone decides at some stage to switch from option A to option B between probability values of 0.1 and 0.2, the next stage of an iMPL would then prompt the subject to make more choices *within* this interval, to refine the values elicited.[8]

The iMPL uses the same incentive logic as the MPL. After making all responses, the subject has one row from the first table selected at random by the experimenter. In the MPL that is all there is. In the iMPL, that is all there is if the row selected at random by the experimenter is *not* the one that the subject switched at in the first table. If it *is* the row that the subject switched at, another

[7] Anderhub, Gneezy, Güth and Sonsino [2001] use post-dated checks for deferred payments in their study of individual risk and time preferences, a practice that is available in Israel. The early payment is due immediately, and they find that individual discount rates are constant over the 4- to 8-week periods considered in the study.

[8] If the subject always chooses A, or indicates indifference for any of the decision rows, there are no additional decisions required and the task is completed. Furthermore, the iterative format has some "smarts" built into it: when the values being elicited drop to some specified perceptive threshold (e.g., 0.05 of a percentage point), the iMPL collapses down to an endogenous number of final rows and the elicitation task stops iterating after those responses are entered.

random draw is made to pick a row in the second table that the subject was presented with, and so on.

As the subject iterates in the iMPL the choices become more and more alike, by design. Hence one would expect that greater cognitive effort would be needed to discriminate between them. At some point we expect the subject to express indifference, which we account for in our analysis by only considering the interval over which the subject could (strictly) discriminate. In fact, one possible explanation to why subjects have been observed switching back and forth between choices in MPL is that they are indifferent. If so, explicitly including an indifference option, as we do here, may be a cleaner way to capture this behavior.

Andersen, Harrison, Lau and Rutström [2004] report results from a complementary series of laboratory experiments in Copenhagen which were designed explicitly to test the properties of the iMPL procedure in comparison to the MPL procedure.[9] They conclude that the iMPL format has no discernible effect on elicited discount rates, that there is no effect from framing on discount rates, but that there is a small order effect on the second task after the initial task (around 3½ percentage points).

B. Panel Experiments

Table 2 displays the panel design of our experiments. We conducted five series of experiments, beginning in June 2003. Series 1 was the "base camp," where we interviewed 253 subjects. In this experiment we elicited responses for six time horizons, as indicated by the first row: 1-month, 4-months, 6-months, 12-months, 18-months and 24-months. We also elicited responses to a number of questions about the recent and prospective well-being of the individual, including his perception of the future state of the economy. In all cases we used a front end delay of 1 month.

Across series 2 through 5 we re-visited 97 of these 253 subjects. The objective was to re-visit

[9] Their design also considered an intermediate institution, the Sequential Multiple Price List, which enforces monotonicity of choices within a given ordered list of choices but does not undertake iterations to refine the interval selected. The iMPL combines enforced monotonicity and iterations to refine choices.

100 of them, split roughly equally in time. The actual sample sizes were close to this, with 26, 23, 23 and 25 in each of the four series. These experiments were conducted in September 2003 (3 months after Series 1), November 2003 (5 months after), May 2004 (11 months after), and November 2004 (17 months after).

To see the logic of the design, consider the horizons for which we elicited an IDR in Series 2, and compare those to Series 1. Series 2 occurred 3 months after Series 1. We elicited an IDR for a 1-month horizon in Series 2, which overlaps the IDR elicited for the 4-month horizon in Series 1. Similarly, the 3-month horizon in Series 2 overlaps the 6-month horizon in Series 1; the 9-month horizon elicited in Series 2 overlaps the 12-month horizon elicited in Series 1; the 15-month horizon elicited in Series 2 overlaps the 18-month horizon elicited in Series 1; and finally the 21-month horizon elicited in Series 2 overlaps the 24-month horizon elicited in Series 1. Series 3, 4 and 5 provide comparable series of horizons vis-a-vis the horizons for which we elicited discount rates in Series 1.

2. Experimental Procedures

A. Sampling Procedures for Series 1

The sample for the field experiments in Series 1 was designed to generate a representative sample of the adult Danish population. There were six steps in the construction of the original sample,[10] essentially following those employed in Harrison, Lau and Williams [2002]:

- First, a random sample of 25,000 Danes was drawn from the Danish Civil Registration Office in January 2003. Only Danes born between 1927 and 1983 were included, thereby restricting the age range of the target population to between 19 and 75. For each person in this random sample we had access to their name, address, county, municipality, birth date, and sex. Due to the absence of names and/or addresses, 28 of these records were discarded.
- Second, we discarded 17 municipalities (including one county) from the population, due to

[10] Further details are provided in Harrison, Lau, Rutström and Sullivan [2005].

them being located in extraordinarily remote locations. The population represented in these locations amounts to less than 2% of the Danish population, or 493 individuals in our sample of 25,000 from the Civil Registry.

- Third, we assigned each county either 1 session or 2 sessions, in rough proportionality to the population of the county. In total we assigned 20 sessions. Each session consisted of two sub-sessions at the same locale and date, one at 5pm and another at 8pm, and subjects were allowed to choose which sub-session suited them best.
- Fourth, we divided 6 counties into two sub-groups because the distance between some municipalities in the county and the location of the session would be too large. A weighted random draw was made between the two sub-groups and the location selected, where the weights reflect the relative size of the population in September 2002.
- Fifth, we picked the first 30 or 60 randomly sorted records within each county, depending on the number of sessions allocated to that county. This provided a sub-sample of 600.
- Sixth, we mailed invitations to attend a session to the sub-sample of 600, offering each person a choice of times for the session. Response rates were low in some counties, so another 64 invitations were mailed out in these counties to newly drawn subjects. Everyone that gave a positive response was assigned to a session, and our recruited sample was 268.

Attendance at the experimental sessions was extraordinarily high, including 4 persons who did not respond to the letter of invitation but showed up unexpectedly and participated in the experiment. Four persons turned up for their session, but were not able to participate in the experiments.[11] The experiments in series 1 were conducted in June of 2003, and a total of 253 subjects participated in the experiments.[12] Sample weights for the subjects in the experiment can be

[11] The first person suffered from dementia and could not remember the instructions; the second person was a 76 year old woman who was not able to control the mouse and eventually gave up; the third person had just won a world championship in sailing and was too busy with media interviews to stay for two hours; and the fourth person was sent home because they arrived after the instructions had begun and we had already included one unexpected "walk-in" to fill their position.

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

constructed using this experimental design, and are used to calculate weighted distributions and averages that better reflect the adult population of Denmark.

B. Conduct of the Sessions in Series 1

To minimize travel times for subjects, we reserved hotel meeting rooms in convenient locations across Denmark in which to conduct sessions.[13] Because the sessions lasted for two hours, light refreshments were provided. Participants met in groups of no more than 10. To conduct computerized experiments in the field, it was cost-effective to purchase laptop computers and transport them to the meeting sites. Each subject was identified by a unique ID number. For the randomization procedures, two bingo cages were used in each session, one containing 100 balls and the other containing 3 to 11 balls, depending on the number of decision rows in the iMPL used in different treatments. We found two bingo cages to be the most transparent and convenient way to generate random outcomes in the experiments.

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

Instructions for the experiment were provided on the computer screens, and subjects read through the instructions while the experimenter read them aloud. The experimenters followed the same script and procedures for each session, documented in Harrison, Lau, Rutström and Sullivan [2005].

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

The experiment was conducted in four parts. Part I consisted of a questionnaire collecting subjects' 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, whether the subject is a smoker, and the number of cigarettes smoked per day. Part IV consisted of another questionnaire which elicits information on the subject's financial market instruments, and probes the subject for information on their expectations about their future economic conditions and their own future financial position. The questionnaires are rather long, so we chose to divide them across Parts I and IV in order to reduce subject fatigue and boredom. Part II consisted of four risk aversion tasks, and Part III presented subjects with the six discount rate tasks similar to those developed in Harrison, Lau and Williams [2002]. We will not discuss the risk aversion tasks here.

The six discount rate tasks incorporate the incentive structure described earlier. After subjects completed the six tasks, several random outcomes were generated in order to determine subject payments. For all subjects, one of the six tasks was chosen, then one of the decision rows in that task was chosen. For those subjects whose decision at that row led to the second level of the iMPL table, another random draw was required to choose a decision row in the second level, and yet another random draw was required should that decision have led a subject to the third level in the iMPL. To maintain anonymity we performed the draws without announcing to which subjects it would apply. In the case where a subject indicated indifference for the chosen decision row, another random draw determined whether the subject received the early payment option A or the later payment option B. Finally, a 10-sided die was rolled for each subject. Any subject who received a roll of "0" received actual payment according to that final outcome.

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

C. Procedures for Series 2 Through 5

Between September 2003 and November 2004 we re-visited 97 of the 253 subjects who participated in Series 1 and repeated the individual discount rate tasks. Each subject was interviewed in private in the four new series of experiments, because attendance at the experiments otherwise would have been too low. To minimize travel times for subjects and encourage higher attendance, we offered to conduct the experiment at their private residence, or at another convenient location of their choice.

There were four steps in the construction of this sample. First, we assigned each of the 14 counties either 1 number or 2 numbers, in rough proportionality to the population of the county. In total we assigned 20 numbers that also reflect the distribution of the 20 sessions in the sampling procedures for Series 1. Second, although Denmark is a relatively small country, it was necessary to consider logistical constraints, and we randomly picked 4 of the 20 numbers for each Series 2 through 5. This procedure implied that we at most should re-visit subjects from four counties in each new series of experiments. Third, we picked the first 50% or 25% of the randomly sorted records within each county, depending on the one or two numbers allocated to each county. This provided a sub-sample of 100 subjects. Fourth, we contacted subjects by phone and invited them to participate again in the experiments. It was difficult to get in contact with some subjects, and other subjects did not want to participate again in the experiments, so we invited the next subjects on the randomly sorted list of former participants until 26 people were signed up for each new series of experiments.[14]

The interviews lasted about one hour, and each subject was paid 300 DKK for their participation. For the randomization procedures, we used two 10-sided die (one numbered from 0 to 9 and the other numbered from 00 to 90), and eleven playing cards, numbered from 1 to 11. These procedures were the most convenient way to generate random outcomes, and allowed us to avoid bringing two large bingo cages to the meeting. The experiments were computerized, and all the experimental procedures, tasks and monetary incentives were similar to those used in Series 1. Since the subjects had prior experience with the experimental procedures, we did not spend time on training subjects on the iMPL and randomization procedures. However, we maintained the trainer task in which payments were in the form of candies.

Table 3 provides the definitions of the explanatory variables used in the statistical analysis and summary statistics. It is clear that our data set is quite different from the standard laboratory set using college students, and that it is much more representative of the target population. There are no significant differences in the composition of the two samples, although there were relatively more subjects with substantial higher education and higher level income who participated in Series 2-5 of the experiments compared to Series 1.

3. Results

Figure 1 collects the raw results of our experiments. It displays the distribution of elicited discount rates by horizon, using box plots for each horizon.[15] Panel A shows the raw results of the Series 1 experiments and Panel B illustrates the raw results of the Series 2 through 5 experiments. Panel C collates these results into one graph, for ease of comparability, with an asterisk indicating that the data were generated in Series 1. The raw data here is the mid-point of the elicited discount

[14] We anticipated that a few subjects would cancel the meeting and thus signed up one extra subject for each new series. Only 16% of the subjects refused or were not able to participate again, and we were unable to get in contact with another 15% of the subjects.

[15] A box plot shows the median as a solid dot, the inter-quartile range as a shaded rectangle, and the range in the outer "whiskers." The interquartile range is the 25th and 75th percentile.

rate interval, after all of the iterations of the iMPL have been completed. These comparisons across horizons mix between-subject and within-subject comparisons; we examine these in a more controlled statistical manner below. All discount rates reported here refer to annual effective interest rates, for comparability across horizons.

We observe variations of elicited IDR across subjects, with a mean and median of 24.2% and a standard deviation of 15.7%. These values are close to those reported in the earlier field study by Harrison, Lau and Williams [2002] on the Danish population, where the mean is 28.1%[16] They are somewhat higher than the estimated rates found in comparable laboratory elicitation exercises on American students by Coller and Williams [1999], who report a median of 17.7% using a horizon of 60 days.[17]

The raw results indicate that there is considerable variation in discount rates across the sample. The inter-quartile range generally varies from around 15% per annum to 35%, although some subjects exhibit wider variation. Of course, this variation could just reflect heterogeneity in subjective preferences. The raw results also point to elicited discount rates being relatively stable across horizons. The only possible outlier to the eyeball is the median 1-month discount rate elicited in Series 1, which is higher than the rest, but the difference is not large in relation to the variation within each horizon.

There are limits to what one can infer from these raw data. For example, within the inter-quartile range of Figure 3 there could be some subjects with sharply declining discount rate functions by horizon, and others that are simply high-variance over the horizon. To better evaluate the hypotheses of interest we must turn to in-sample comparisons across horizons, and then control

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

[17] They are spectacularly lower than the rates reported in other field experiments by Eckel, Johnson and Montmarquette [2005; p.258], who find short-term discount rates averaging 289% per annum. Rates as high as this are actually quite common in the extensive psychology literature on discount rates that "scrambles" choices so that subjects get different principals, horizons and/or front end delays on each choice (e.g., Kirby and Maraković [1996] and Kirby, Petry and Bickel [1999]). Following Coller and Williams [1999], who also review earlier economics experiments of the same format, it is now common to present subjects with an ordered series of choices to reduce simple confusion.

for possible changes in the state of nature over time.

A. Within-Subject Comparisons

Figure 2 displays the data for the within-subjects comparisons that our experimental design was constructed to allow.[18] We show the difference between the IDR elicited from the same subject at two different points in time. If there was no change in that elicited IDR then the data point underlying the histogram in Figure 2 would be zero. If the IDR had increased in Series 2, the data point would have been positive.

The differences in discount rates illustrated in Figure 2 do not have any apparent tendency to be positive or negative. However, these differences in discount rates reflect all possible within-subjects comparisons. A direct test of the hypothesis that discount rates are constant for each time horizon considered in Series 1 is possible with a regression model in which the difference in discount rates for any two comparable time horizons is the dependent variable and treated as a binary dummy variable.[19] We also control for individual characteristics of the subject, and the possibility of an experimenter effect.

Table 4 reports the results of the analysis of discount rates using censored values. There is no statistically significant change in any of the elicited discount rates as one gets closer to a given horizon in the Series 1 experiments. We can directly test for constancy of discount rates using the statistical model in Table 4, by testing if the coefficients on each horizon are jointly equal to zero. An adjusted Wald test of this hypothesis has a p-value of 0.855 Hence *we cannot reject the hypothesis that elicited discount rates are the same* as the horizon they refer to is approached.

The results in Table 4 do not even point to any identifiable demographic segment of the

[18] We assume that the subjects have access to capital markets and use censored discount rates for all statistical analyses. Market prices are relevant to the extent that subjects attempt to arbitrage between the investment instrument provided in the experiment and field investment opportunities, even if utility is defined over income earned in the experiment rather than over terminal lifetime wealth.

[19] This model allows for the deliberate survey design we employed. In particular, we allow for the fact that subjects in one county were selected independently of subjects in other counties, as well as the possible correlation between responses by the same subject. Comparable results are obtained if one uses uncensored discount rates instead of censored discount rates, and if one controls for observed and unobserved individual characteristics using random-effects panel regression, or adds controls for multiplicative heteroskedasticity.

Danish population that might have higher discount rates as the horizon approaches. Older Danes do tend to exhibit significantly *lower* discount rates as the horizon approaches, but that is in the opposite direction than predicted by the usual dynamic inconsistency story.

The only group that comes close to having higher discount rates are students, who have discount rates that are on average 17.7percentage points higher as the horizon approaches. The 95% confidence interval for this marginal effect is between -3.7 and + 39.2, and the effect has a *p*-value of 0.104. Thus it could be that the only subjects to exhibit behavior remotely consistent with the usual presumption happen to be those most likely to be used in traditional laboratory experimental environments. This would suggest that evidence from those environments and convenience samples is unrepresentative of the population as a whole.

We can examine the possibility of an interaction between horizon length and consistency by re-estimating the regression model in Table 4 separately for the 4-month, 6-month, 12-month, 18-month and 24 month horizons in Series 1. The detailed results are reported in an Appendix (available on request), and do point to some significant interactions. As our design would suggest, we have larger sub-samples for the longer horizons in Series 1, since here are more opportunities to observe comparable discount rates. Specifically, for the 4-month, 6-month, 12-month, 18-month and 24-month sub-populations, we have samples of 26, 49, 72, 97 and 97, respectively.

For the 4-month horizon we can readily identify some segments of the population that are more likely on average to exhibit dynamic inconsistency. People with children have discount rates that are 34.3 percentage points higher (*p*-value of 0.086); those who live in a larger city of 20,000 or more have discount rates that are 19.9 percentage points higher (*p*-value of 0.056), and students have discount rates that are 29.1 percentage points higher (*p*-value of 0.042). These results are particularly relevant since virtually all of the laboratory experiments with convenience student samples focus on horizons of 4-months or shorter, to ensure credibility of payment within a give academic year. One could easily envisage a sample of students generating significant evidence of dynamic inconsistency in laboratory experiments.

On the other hand, several demographic characteristics generate significant effects in the opposite direction. Subjects that have larger households, longer education and have higher incomes generate discount rates that are 15.9, 24.3 and 30.1 percentage points lower as the horizon approaches. In each case these are also statistically significant at the 7.5% level or better. In addition, there is a large effect for retired subjects, who have discount rates that are 64.8 percentage points lower as the horizon approaches (*p*-value of 0.01). Taken together, these results point to considerable heterogeneity in the population as a whole for the 4-month horizon: there are clearly some segments that can be identified as behaving in the customary dynamically inconsistent manner, there are some that can be identified as behaving in a dynamically inconsistent manner that is not expected, and there are many that exhibit no evidence of dynamic consistency. *This is not mixed evidence for or against dynamic inconsistency: it is evidence that statistical inferences that fail to control for observable heterogeneity will fail to identify these different preference patterns.*

The sensitivity of this heterogeneity is evident if one moves to consider the 6-month horizon results. Here we observe the none of the individual characteristics of the subjects are significant at any conventional level, and there is no significant effect for students over this horizon. It might be convenient if we could attribute this difference in estimated effects to the smaller sample size of the 4-month sub-population, but the smaller sample would actually lead to larger standard errors instead of smaller standard errors.

For the 12-month horizon we find that average discount rates are higher as the horizon approaches. The average discount rate is 24.8 percentage points higher for the 7-month horizon in Series 3 (*p*-value of 0.038) and 21.7 percentage points higher for the 1-month horizon in Series 4 (*p*-value of 0.095). These results suggest that subjects do behave in a dynamically inconsistent manner over the one-year period, but the results do not condition on changes in the state of nature during this time horizon, which are discussed below. Looking at individual characteristics, we find that older subjects and those with children have lower discount rates as the 12-month horizon approaches. Older subjects have discount rates that are 23.7 percentage points lower (*p*-value of

0.028), and subjects with children have discount rates that are 12.7 percentage points lower (*p*-value of 0.082). The negative coefficient for subjects with children is qualitatively different from the positive effect for the 4-month horizon, and points to considerable interaction between the horizon and subjects with children.

There are no statistically significant changes in elicited discount rates as one gets closer to the 18-month horizon. The only individual characteristics that are significant is students and subjects with children. Discount rates are 23.9 percentage points higher for students (*p*-value of 0.033), and this time subjects with children have discount rates that are 14.6 percentage points *higher* when the 18-month horizon gets closer (*p*-value of 0.045).[20]

Finally, for the 24-month horizon we find that students have discount rates that are 17.3 percentage points higher than non-students as the horizon approaches (*p*-value of 0.036), and none of the other individual characteristics of the subjects are statistically significant. The overall results thus suggest that individual discount rates are dynamically consistent, with the possible exception of the 12-month horizon, and students are more likely on average to exhibit dynamic inconsistency over the 4- to 24-month horizons considered here.

B. Controlling for Changes in the States of Nature

Although we visited the same person at a later date, the fact that a certain amount of time had to pass means that there may have been changes in the preferences of the individual. If we are prepared to just assume that preferences are stable over the 17-month time difference between our first and last experimental sessions, then we can rest on the statistical analysis presented above. Although there is some evidence that risk attitudes are temporally stable over time, including some evidence for our sample, common sense indicates that preferences could be state-dependent.[21] We

[20] We also observe the only statistically significant experimenter effect for any horizon, resulting in discount rates that are 12.5 percentage points *lower* when Steffen Andersen conducted the session instead of Morten Lau. Our inferences control for this effect, and remain valid as long as we assume that there is no interaction between the experimenter and demographics.

[21] Harrison, Johnson, McInnes and Rutström [2005] demonstrate the temporal stability of risk attitudes in lab experiments over a period of 4 months.

therefore evaluate the extent to which observable changes in states of nature might change our conclusions.

In each series we asked subjects to respond to seven questions about their perception of the state of the economy in general and their own personal financial situation. In each case we asked for their perception for horizons, denoted X below, of 1, 4, 6, 12, 18 and 24 months:

- Would you say that you and your family are better off or worse off financially than you were X months ago?
- Now looking ahead, do you expect any major change in your family situation that will lead to higher expenses or lower expenses during the next X months?
- Do you expect any major change in your family situation that will lead to higher earnings or lower earnings during the next X months?
- On balance, do you think that you and your family will be better off or worse off financially X months from now?
- Turning to the economic conditions in the country as a whole, would you say that at the present time economic conditions are better or worse than they were X months ago?
- Do you think that there will be more or less unemployment during the next X months?
- Do you think that interest rates for borrowing money will go up or go down during the next X months?

We readily concede that these questions do not exhaust the set of conceivable events that could occur over the horizon of interest,[22] but they are certainly a good general place to start looking for possible effects from changes in states of nature.

We construct a variable for each subject using their responses to these questions. For each question we asked if they thought that there would be an improvement, a worsening, no change, or whether they did not know. We coded improvements as 1, a worsening as -1, no change as 0, and don't know as missing. For each question and horizon we can then calculate the change from the

[22] In the demographic survey conducted in Harrison, Lau and Williams [2002] we literally had one subject ask us if he wanted us to state his current sex or the sex he would be at the end of the longest horizon.

response in the Series 1 session to the comparable response in the later session. Thus for any question-horizon-subject the difference could be +2, +1, 0, -1, -2 or missing. We then calculate the sum for each individual over all horizons of a given question; as it happens, these tended to be positively correlated. Thus each individual had a value which reflected the extent to which they expected improvement or worsening in each state over the various horizons considered in Series 1. There are many ways to summarize these data, but this statistic seems sensible here.

Table 5 describes these statistics of the changes in states of nature. A positive value means that subjects believe that the specific state of nature is improved at the time of the later sessions compared to Series 1, and vice versa for negative values. In general, subjects are more optimistic about the state of the economy and their own personal financial situation at the later session compared to the base camp, although they seem to have become a bit more pessimistic about future personal income and expenditures. The subjects have a more positive impression of the current state of the economy compared to the past, and they have become more optimistic about the general level of unemployment during the next two years. Finally, we observe that subjects are more pessimistic about changes in interest rates and they are more inclined to believe that the interest rate for borrowing money will go up in the near future.

Table 6 extends the regression model of Table 4 by including these measures of the change in each state of nature for each individual. There are some interesting changes in the estimated effects of the explanatory variables. First, we find that the states of nature are generally statistically insignificant, with the exception of the variable reflecting beliefs about the present state of the economy compared to the past (p-value of 0.002). On the whole the states of nature do not have a statistically significant joint effect (p-value of 0.125). Second, the size of the estimated change in the discount rate does not change. In other words, at the risk of some double negatives, there is no change in the lack of evidence of an increase in discount rates as the horizon approaches. Moreover, a test of the joint hypothesis that all of the horizon dummies are zero again leads to the null hypothesis not being rejected (p-value of 0.809). Finally, we observe comparable demographic effects from students. Conditional on changes in the states of nature, we find that students have discount rates that are on average 19.2 percentage points higher as the horizon approaches. The 95% confidence interval for this marginal effect is between -2.2 and + 40.5, and the effect has a *p*-value of 0.077.

We can again examine the possibility of an interaction effect between horizon length and consistency by estimating the model in Table 6 separately for the 6-month, 12-month, 18-month and 24-month horizons in Series 1, conditional on experimenter effects, individual characteristics and changes in states of nature.[23] The results again point to some significant interaction effects.

For the 6-month horizon, we find that none of the individual characteristics of the subjects are significant at the 10% confidence level or better, which is consistent with the results from the comparable model without control for changes in states of nature. We also observe that two of the variables that reflect the states of nature are statistically significant, but generate effects in opposite directions. The variable that reflects beliefs about the present state of the economy compared to the past is negative with a coefficient that is equal to -2.4 percentage points (*p*-value of 0.096), whereas the variable that elicits beliefs about the general unemployment outlook is positive and equal to 2.7 percentage points (*p*-value of 0.059).

Moving to the 12-month horizon, we find that discount rates are higher as the horizon gets closer, but the coefficients with respect to the horizon dummies are now statistically insignificant when we control for changes in states of nature. The results also show that the discount rate is 10.2 percentage points higher for subjects who live in the greater Copenhagen area (*p*-value of 0.057), whereas middle-aged and older subjects have discount rates that are 17.8 and 19.8 percentage points lower with *p*-values of 0.047 and 0.066, respectively. Turning to state dependency, the variable that reflects beliefs about the present state of the economy compared to the past is again negative and statistically significant (*p*-value of 0.003).

For the 18-month horizon, we find that students and subjects with children have higher

[23] The sample size for the 4-month horizon in which we include changes in the states of nature does not have sufficient degrees of freedom for reliable estimation, and is therefore not reported here.

discount rates when the horizon approaches, and these effects are similar to those in the regression that does not control for state dependency. The average discount rate is 23.8 percentage points higher for students than for non-students (*p*-value of 0.040), and subjects with children have discount rates that are 17.1 percentage points higher than otherwise (*p*-value of 0.018). We again observe that the variable that reflects beliefs about the present state of the economy compared to the past is negative and statistically significant (*p*-value of 0.007).

Finally, the results for the 24-month horizon show that students again have higher discount rates as the horizon get closer (the coefficient is equal to 20.3 percentage points with a *p*-value of 0.019), and the only significant variable with respect to state dependency is beliefs about the present state of the economy compared to the past, which again is negative and equal to -1.8 percentage points (*p*-value of 0.032). Taken together, the results suggest that individual discount rates are dynamically consistent over the time horizons considered here, and none of the horizon dummies are statistically significant when the regression models condition on changes in the states of nature. We also observe that students continue to behave in a dynamically consistent manner over the 18- to 24-month horizons when we control for changes in states of nature, which is remarkable since we also control for other characteristics that students normally "carry" with them, such as age.

4. Previous Evidence

Our design differs in several ways from those used in previous literature. Quite apart from the panel nature of our design, which is the main contribution, we employ procedures that have evolved in recent studies to mitigate potential confounds.[24] An appendix discusses these design features in detail, with citation to the literature, but the basic points are easy to follow.

First, we use real rewards instead of hypothetical rewards. There is debate about the importance of using real rewards, with many "behavioral economists" defending the use of

[24] Excellent reviews of the literature, with a critical eye to the potential for such confounds to affect behavior, can be found in Coller and Williams [1999] and Frederick, Loewenstein and O'Donoghue [2002; §6].

hypothetical surveys and many "mainstream experimental economists" insisting on real rewards. We believe that the evidence supports the latter camp, but simply do not see this issue as fundamental. If the claims of the behaviorists do not survive when one uses real rewards, then their claims are extraordinarily fragile and not worth taking seriously. Since there is evidence that real and hypothetical tasks differ in *some* task domains, why risk adding a confound by failing to ensure the control over subject motivation that requires real rewards as a necessary[25] condition?

Second, we do not "scramble" tasks that involve different horizons, principals and premia to delay. Our MPL is deliberately ordered, to provide subjects with a transparent task that leads to them revealing their discount rate. Many experiments have used tasks presented to subjects in a random order, but this adds potential confounds due to computational complexity that we prefer to avoid. In a related vein, we provide information to the subject about the implied annual effective interest rate, to facilitate comprehension of the task in a relatively familiar manner.

Finally, we provide a FED of one month on all options. This FED is intended to mitigate the possible effects of differential credibility of payment between the two horizons. Frederick, Loewenstein and O'Donoghue [2002; p.382] explain the problem well:

> In experimental studies, subjects are typically instructed to assume that delayed rewards will be delivered with certainty. It is unclear whether subjects do (or can) accept this assumption, because delay is ordinarily – and perhaps unavoidably – associated with uncertainty. A similar problem arises for field studies, in which it is typically assumed that subjects believe that future rewards, such as energy savings, will materialize. Because of this subjective (or "epistemic") uncertainty associated with delay, it is difficult to determine to what extent the magnitude of imputed discount rates (or the shape of the discount function) is governed by time preference *per se*, versus the diminution in subjective probability associated with delay.

The implication of using a non-trivial FED, as emphasized by Coller, Harrison and Rutström [2003], is that one cannot discriminate between exponential preferences and quasi-hyperbolic preferences. On the other hand, it is difficult to imagine public policy investments and major personal decisions that do not entail some legal or contractual FED option.

Our sense from the literature that examines these design features is that it is the second and

[25] Necessary but not always sufficient: see Harrison [1989].

third that makes a major difference to elicited discount rates, and hence to the likely stability of elicited rates over time. Scrambling is generally associated with annual discount rates that are extraordinarily high, as noted earlier. Similarly, the absence of a FED increases elicited rates by hundreds of percentage points in most designs.

5. Conclusions

We have shown that subjects in Denmark behave as if they have constant discount rates over time when the front end delay is one month. This is essentially true if we consider discount rates over different planning horizons that are elicited at a single point in time,[26] as well as over time when one revisits the same subject. Comparable laboratory experiments for significantly shorter planning horizons suggest that this conclusion generalizes to settings in which the front end delay is as short as one week, but that it does not generalize to settings in which the initial option is available immediately (Coller and Williams [1999] and Coller, Harrison and Rutström [2003]). When there is no front end delay, subjects in the laboratory behave as if they have extremely high discount rates for shorter time horizons of two or three weeks, and that these discount rates decline for longer planning horizons.

Taken together, these results focus attention squarely on the interpretation of the front end delay feature of intertemporal choice experiments. This design feature controls for plausible contaminants of behavior in choice settings such as the asymmetric credibility of a payment by the experimenter "today" rather than "in the future" and asymmetric transactions costs to the subject of collecting the payoffs. It also controls for any other asymmetric subjective costs that the subject may attach to having to face a delay of any length. Any such subjective costs would result in the demand for additional compensation in the form of a possible fixed premium for any delay. Although we can not point to evidence as to why subjects attach a premium (in addition to their required rate of

[26] See Harrison, Lau and Williams [2002] and Harrison, Lau, Rutström and Sullivan [2005]. The qualifier "essentially" is added to acknowledge that there is some evidence that the average elicited discount rates for the 1-month horizon are slightly higher than those for longer horizons. The effect is very small, in the order of a few percentage points, and hardly the stuff of major dynamic inconsistency.

interest) to deferred payment options, there is evidence that this premium affects the compensation demanded only in choices made in the presence of no front end delay.

There are two ways to interpret the front end delay:

- If the front end delay is viewed as a necessary device to control for the subjective cost confounds of earlier experiments, then we have shown that one cannot reject the hypothesis that discount rates are constant over the time horizons considered.
- However, if the front end delay is viewed as controlling for the "passion for the present" that is presumed in quasi-hyperbolic accounts of intertemporal preference, then our results show (a) that the "present" does not last longer than one month (or one week in the lab), and (b) that the discount rates for the "future" are indeed constant.

Which is it? We do not know. Nor do we believe that it is possible to operationally differentiate between the two.[27] Our priors are that it is the former, hence we reject the alternative hypothesis of continuously declining discount rates. But we stress, honestly, that this is a matter of our *a priori* judgement, just as claims that it is a "passion for the present" are priors that others might hold.

Thus the deciding factor must be the field relevance of the notion of a front end delay. In public policy settings, we believe it entirely appropriate. In private settings, we can immediately think of some settings in which it is not appropriate (e.g., diet, or consumption of habituating products such as smoking), just as we can think of many setting in which it is appropriate (e.g., educational decisions, housing decisions, family composition decisions, and retirement decisions). Thus, to the extent that many significant field choices or policy decisions do not involve choices with immediate payoffs, the use of constant discount rates remains appropriate.

[27] The difficulty in designing an experiment to separate these possibilities is clear. To do so would require a decision task that involves "now vs. later," allowing for present-biased preferences to manifest themselves, while keeping other costs constant between the two choices. Clearly any choice that involves "now vs. later" must involve at least some transactions cost (if nothing more than the effort of keeping track of the arrangements needed to receive payment) and some (however minimal) risk of default in payment. These difficulties call into question the relevance of drawing such a distinction in the first place.

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

Table 2: Experimental Design

Date of Experiments	Horizon in Months						Sample Size
Series 1, June 2003 (Base Camp)	1	4	6	12	18	24	253
Series 2, September 2003 (+3 Months)		1	3	9	15	21	26
Series 3, November 2003 (+5 Months)			1	7	13	19	23
Series 4, May 2004 (+ 11 Months)				1	7	13	23
Series 5, November 2004 (+17 Months)					1	7	25

Table 3: List of Variables and Descriptive Statistics

Variable	Definition	Phase I Sample Mean	Phase II Sample Mean
female	Female	0.51	0.53
young	Aged less than 30	0.17	0.14
middle	Aged between 40 and 50	0.28	0.29
old	Aged over 50	0.37	0.39
single	Lives alone	0.20	0.16
kids	Has children	0.28	0.32
nhhd	Number of people in the household	2.49	2.50
owner	Owns own home or apartment	0.69	0.67
retired	Retired	0.16	0.17
student	Student	0.09	0.09
skilled	Some post-secondary education	0.38	0.31
longedu	Substantial higher education	0.36	0.47
IncLow	Lower level income	0.34	0.31
IncHigh	Higher level income	0.33	0.40
copen	Lives in greater Copenhagen area	0.27	0.27
city	Lives in larger city of 20,000 or more	0.39	0.42
experimenter	Experimenter Andersen (default is Lau)	0.49	0.54
	Number of subjects	253	97

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

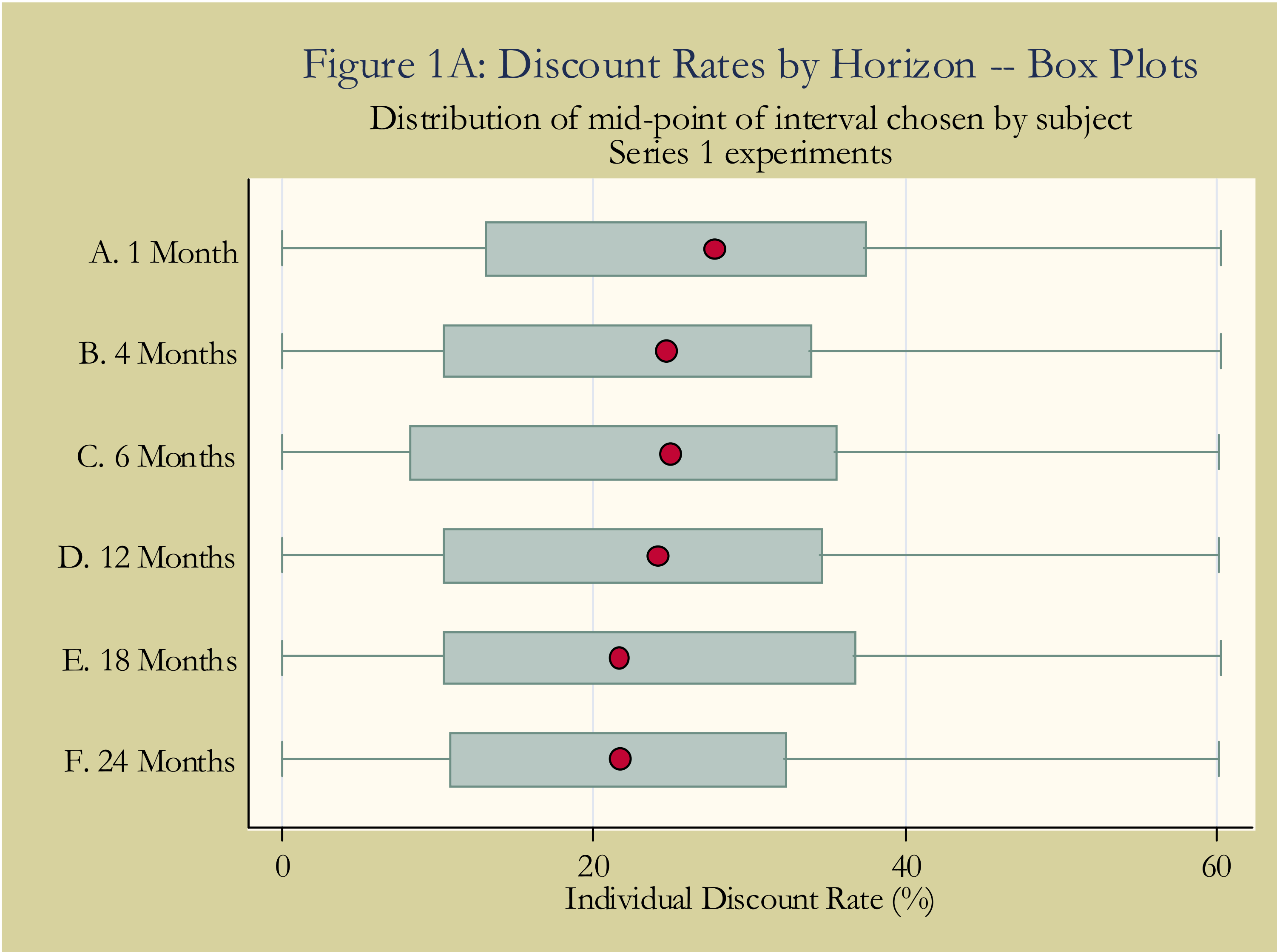
Figure 1A: Discount Rates by Horizon -- Box Plots
Distribution of mid-point of interval chosen by subject
Series 1 experiments
A. 1 Month
B. 4 Months
C. 6 Months
D. 12 Months
E. 18 Months
F. 24 Months
0
20
40
60
Individual Discount Rate (%)

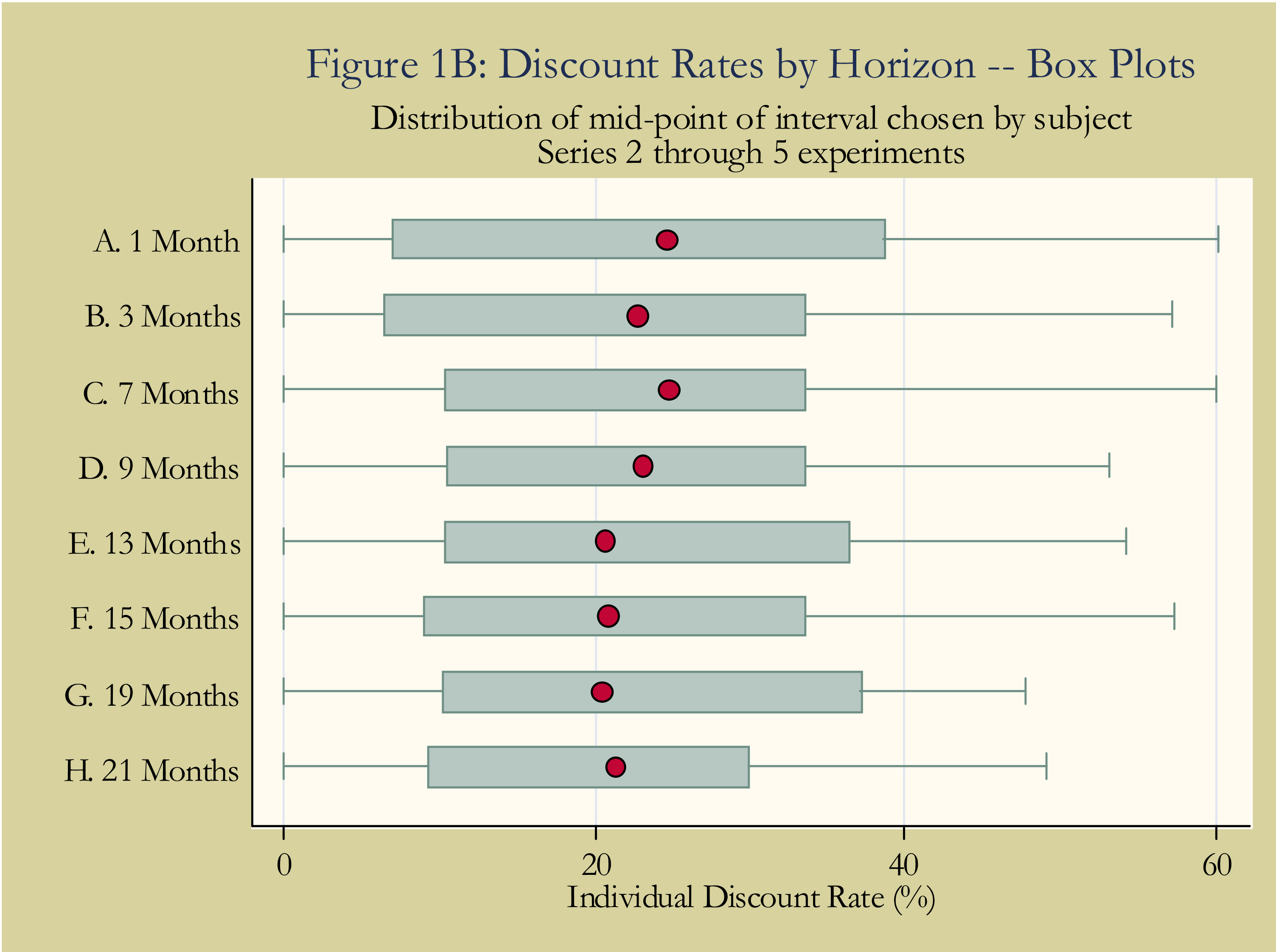

Figure 1B: Discount Rates by Horizon -- Box Plots
Distribution of mid-point of interval chosen by subject
Series 2 through 5 experiments
A. 1 Month
B. 3 Months
C. 7 Months
D. 9 Months
E. 13 Months
F. 15 Months
G. 19 Months
H. 21 Months
0
20
40
60
Individual Discount Rate (%)

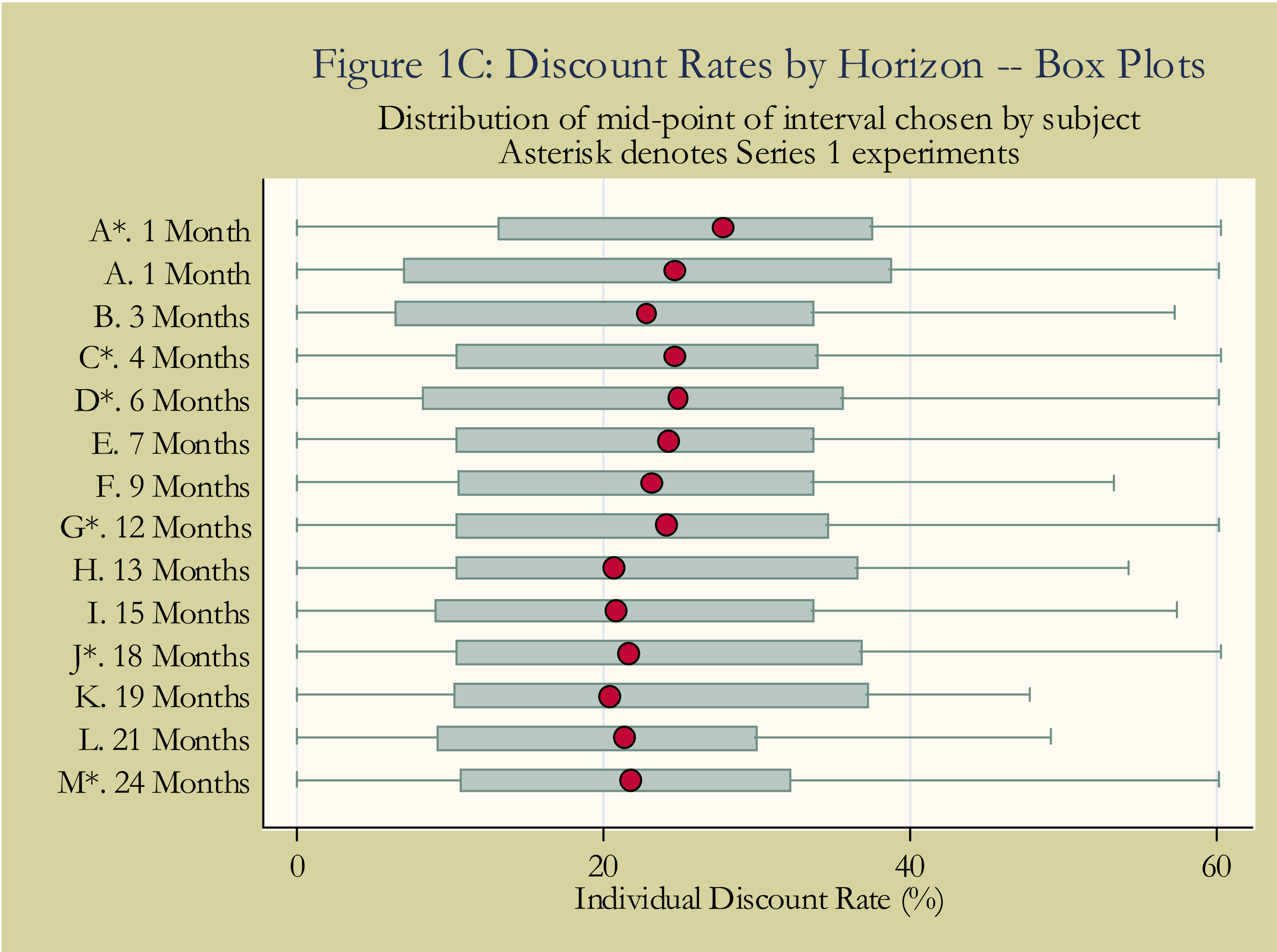
Figure 1C: Discount Rates by Horizon -- Box Plots
Distribution of mid-point of interval chosen by subject
Asterisk denotes Series 1 experiments
A*. 1 Month
A. 1 Month
B. 3 Months
C*. 4 Months
D*. 6 Months
E. 7 Months
F. 9 Months
G*. 12 Months
H. 13 Months
I. 15 Months
J*. 18 Months
K. 19 Months
L. 21 Months
M*. 24 Months
0
20
40
60
Individual Discount Rate (%)

Figure 2: Within-Subject Differences in Discount Rates

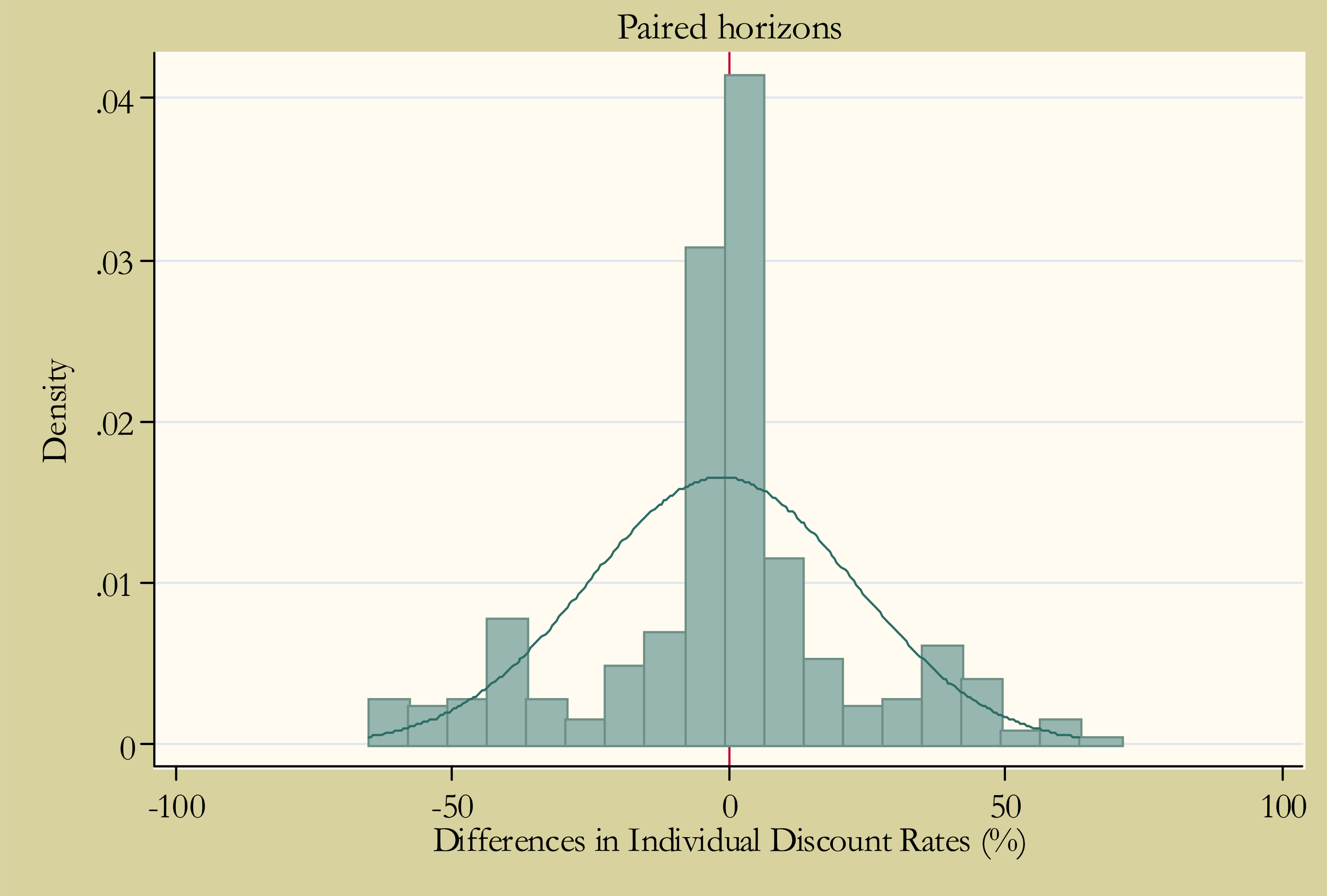

Table 4: Regression Model of Within-Subject Differences in Elicited Discount Rates

```
pweight:  weight                                    Number of obs    =       341
Strata:   county                                    Number of strata =        11
PSU:      id                                        Number of PSUs   =        97
                                                    Population size  = 5473963.2
                                                    F(  31,     56)  =      0.78
                                                    Prob > F         =    0.7670
                                                    R-squared        =    0.1589

------------------------------------------------------------------------------
 cenidr_diff |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
   Hpair_2_4 |   6.115897   12.52979     0.49   0.627    -18.79251     31.0243
   Hpair_2_6 |   5.334726   13.54317     0.39   0.695     -21.5882    32.25765
  Hpair_2_12 |   .4992039   13.39002     0.04   0.970    -26.11928    27.11769
  Hpair_2_18 |   1.518972   12.67531     0.12   0.905     -23.6787    26.71665
  Hpair_2_24 |   .6400804   12.13372     0.05   0.958    -23.48096    24.76112
   Hpair_3_6 |   13.07173   12.27764     1.06   0.290    -11.33542    37.47887
  Hpair_3_12 |   7.802696   11.73943     0.66   0.508    -15.53452    31.13992
  Hpair_3_18 |   3.313218   12.34078     0.27   0.789    -21.21944    27.84588
  Hpair_3_24 |   2.394551   11.79224     0.20   0.840    -21.04765    25.83675
  Hpair_4_12 |   16.42445   13.97168     1.18   0.243    -11.35034    44.19923
  Hpair_4_18 |   13.12725   13.29627     0.99   0.326    -13.30485    39.55935
  Hpair_4_24 |   11.26758   13.55396     0.83   0.408    -15.67679    38.21195
  Hpair_5_18 |   3.895462   12.79405     0.30   0.762    -21.53827    29.32919
  Hpair_5_24 |   2.679303   12.66186     0.21   0.833    -22.49165    27.85025

experimenter |  -7.199596    5.56285    -1.29   0.199    -18.25818    3.858983

      female |  -.9648553   4.358901    -0.22   0.825    -9.630064    7.700353
       young |    1.55538   8.358535     0.19   0.853    -15.06084     18.1716
      middle |   1.642957   6.667744     0.25   0.806    -11.61208    14.89799
         old |  -7.318705   7.710185    -0.95   0.345    -22.64605    8.008635
      single |   1.531458    8.42581     0.18   0.856     -15.2185    18.28141
        kids |   4.959648   6.065616     0.82   0.416    -7.098398    17.01769
        nhhd |  -1.723073   2.512552    -0.69   0.495    -6.717861    3.271715
       owner |  -1.237635   4.863425    -0.25   0.800     -10.9058    8.430535
     retired |  -2.348029   7.577629    -0.31   0.757    -17.41186     12.7158
     student |   17.73226   10.79603     1.64   0.104    -3.729544    39.19407
     skilled |   3.705849   7.032024     0.53   0.600    -10.27335    17.68505
     longedu |    .224232   6.759982     0.03   0.974    -13.21417    13.66263
      IncLow |  -3.844035   7.418023    -0.52   0.606    -18.59058    10.90251
     IncHigh |    .936288   6.866147     0.14   0.892    -12.71316    14.58574
       copen |   2.210342   5.353161     0.41   0.681     -8.43139    12.85207
        city |  -1.723969   5.647373    -0.31   0.761    -12.95057    9.502636
------------------------------------------------------------------------------
```

Table 5: Descriptive Statistics for the Changes in States of Nature

Number of observations: 341
Number of individuals: 97

Variable		Mean	Std. Dev.	Min	Max	Observations
ds_fin0	overall	.1554252	3.176551	-12	10	N = 341
ds_exp	overall	-.4281525	2.599485	-7	9	N = 341
ds_inc	overall	-.3343109	2.007258	-6	6	N = 341
ds_fin1	overall	.2932551	2.769941	-6	12	N = 341
ds_eco	overall	.4868035	3.797519	-12	12	N = 341
ds_emp	overall	.5483871	2.84525	-10	7	N = 341
ds_int	overall	-1.108504	3.577292	-12	7	N = 341

Legend: Variable "ds_fin0" indicates if the subject is better off or worse off financially than he were X months ago, variable "ds_exp" indicates if the subject expects any major change that will lead to higher expenses or lower expenses during the next X months, variable "ds_inc" indicates if the subject expects any major change that will lead to higher earnings or lower earnings during the next X months, variable "ds_fin1" indicates if the subject thinks that he will be better off or worse off financially X months from now, variable "ds_eco" indicates if the subject would say that at the present time economic conditions are better or worse than they were X months ago, variable "ds_emp" indicates if the subject thinks there will be more or less employment during the next X months, and variable "ds_int" indicates if the subject thinks that interest rates for borrowing money will go up or down during the next X months.

Table 6: Regression Model Including Changes in States of Nature

```
pweight:  weight                                    Number of obs    =       341
Strata:   county                                    Number of strata =        11
PSU:      id                                        Number of PSUs   =        97
                                                    Population size  = 5473963.2
                                                    F(  38,     49)  =      1.14
                                                    Prob > F         =    0.3312
                                                    R-squared        =    0.2329
```

cenidr_diff	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
Hpair_2_4	-2.566186	13.5208	-0.19	0.850	-29.44465	24.31228
Hpair_2_6	-3.347357	14.19669	-0.24	0.814	-31.56945	24.87474
Hpair_2_12	-8.182879	14.20774	-0.58	0.566	-36.42692	20.06117
Hpair_2_18	-7.163111	13.60495	-0.53	0.600	-34.20886	19.88263
Hpair_2_24	-8.042003	13.10722	-0.61	0.541	-34.09828	18.01428
Hpair_3_6	4.228016	13.16616	0.32	0.749	-21.94544	30.40147
Hpair_3_12	-1.041014	12.89051	-0.08	0.936	-26.66651	24.58448
Hpair_3_18	-5.530493	12.96755	-0.43	0.671	-31.30913	20.24814
Hpair_3_24	-6.449159	12.78042	-0.50	0.615	-31.85579	18.95747
Hpair_4_12	6.276777	13.81552	0.45	0.651	-21.18757	33.74112
Hpair_4_18	2.979578	13.86568	0.21	0.830	-24.58448	30.54363
Hpair_4_24	1.119912	14.14586	0.08	0.937	-27.00112	29.24094
Hpair_5_18	-4.029808	12.81841	-0.31	0.754	-29.51197	21.45235
Hpair_5_24	-5.245966	12.78378	-0.41	0.683	-30.65927	20.16734
experimenter	-2.199065	4.898299	-0.45	0.655	-11.93656	7.538431
female	-1.13321	4.225104	-0.27	0.789	-9.532439	7.266019
young	4.949411	10.51914	0.47	0.639	-15.96195	25.86077
middle	-1.610132	6.907248	-0.23	0.816	-15.34129	12.12102
old	-3.571471	7.067035	-0.51	0.615	-17.62027	10.47733
single	3.99864	8.224598	0.49	0.628	-12.35132	20.3486
kids	9.541866	6.064591	1.57	0.119	-2.514142	21.59787
nhhd	.3598842	2.312471	0.16	0.877	-4.237156	4.956925
owner	-.9765377	5.552201	-0.18	0.861	-12.01395	10.06087
retired	-2.089204	7.621345	-0.27	0.785	-17.23994	13.06153
student	19.17504	10.73014	1.79	0.077	-2.155777	40.50586
skilled	2.667989	6.675985	0.40	0.690	-10.60343	15.93941
longedu	-2.030967	6.035611	-0.34	0.737	-14.02936	9.96743
IncLow	-2.771109	7.298713	-0.38	0.705	-17.28047	11.73825
IncHigh	2.528066	7.107635	0.36	0.723	-11.60144	16.65758
copen	3.419973	4.835127	0.71	0.481	-6.191941	13.03189
city	-1.882499	4.885864	-0.39	0.701	-11.59528	7.830277
Ds_fin0	.4817763	.8327758	0.58	0.564	-1.173727	2.13728
Ds_exp	.5190432	.9958566	0.52	0.604	-1.460654	2.498741
Ds_inc	.8953618	.9002528	0.99	0.323	-.8942814	2.685005
Ds_fin1	-.6854827	.8260446	-0.83	0.409	-2.327605	.9566397
Ds_eco	-1.962495	.6102994	-3.22	0.002	-3.17573	-.7492602
Ds_emp	1.039194	.7074027	1.47	0.145	-.3670756	2.445464
Ds_int	.3863497	.6568838	0.59	0.558	-.919492	1.692191

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Appendix A: Additional Statistical Results (NOT FOR PUBLICATION)

These regression results should be compared to those in Table 4. They represent the same model, but estimated on the sub-population indicated. Given the survey design employed, the estimation procedure employed here actually utilizes data from the entire sample to correctly calculate the coefficients and standard errors for the sub-population. The sample size for the 4-month horizon in which we include changes in the state of nature does not have sufficient degrees of freedom for reliable estimation, and is therefore not reported below.

Table A1: Regression Analysis for 4-Month Horizon Only

pweight: weight
Strata: county
PSU: id

Subpopulation no. of obs = 26
Subpopulation size = 432496.9

Number of obs = 142
Number of strata = 4
Number of PSUs = 32
Population size = 2342566.7
F(17, 12) = .
Prob > F = .
R-squared = 0.6712

cenidr_diff	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
Hpair_2_4	43.26093	41.94996	1.03	0.311	-42.66967	129.1915
experimenter	7.521802	14.12427	0.53	0.599	-21.41045	36.45406
female	2.638318	8.205368	0.32	0.750	-14.16962	19.44625
young	-16.94052	47.47879	-0.36	0.724	-114.1964	80.31537
middle	3.43273	11.77881	0.29	0.773	-20.69506	27.56052
old	19.29943	18.71664	1.03	0.311	-19.03986	57.63873
single	-5.027227	20.70388	-0.24	0.810	-47.43721	37.38276
kids	34.27513	19.28947	1.78	0.086	-5.237558	73.78781
nhhd	-15.86026	7.332798	-2.16	0.039	-30.88081	-.839701
owner	8.077212	13.42501	0.60	0.552	-19.42268	35.5771
retired	-64.78028	23.43401	-2.76	0.010	-112.7827	-16.77788
student	29.14113	13.66524	2.13	0.042	1.149156	57.13311
skilled	-9.495323	29.53642	-0.32	0.750	-69.99793	51.00729
longedu	-24.30824	10.90373	-2.23	0.034	-46.64352	-1.972962
IncLow	-.8603927	28.11752	-0.03	0.976	-58.45652	56.73573
IncHigh	-30.14416	16.30625	-1.85	0.075	-63.54599	3.257678
copen	16.24521	10.77708	1.51	0.143	-5.830644	38.32107
city	19.9449	10.00515	1.99	0.056	-.5497313	40.43953

Table A2: Regression Analysis for 6-Month Horizon Only

```
pweight:  weight                                   Number of obs    =        282
Strata:   county                                   Number of strata =          8
PSU:      id                                       Number of PSUs   =         73
                                                   Population size  = 4844778.6
                                                   F(  19,      47) =       1.10
Subpopulation no. of obs =         49              Prob > F         =     0.3794
Subpopulation size       =   822780.2              R-squared        =     0.2837
```

cenidr_diff	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
Hpair_2_6	3.906378	36.71105	0.11	0.916	-69.41065	77.22341
Hpair_3_6	10.21446	37.76121	0.27	0.788	-65.1999	85.62881
experimenter	-2.935683	8.884454	-0.33	0.742	-20.67916	14.8078
female	-10.2585	8.051776	-1.27	0.207	-26.33901	5.822008
young	37.66837	36.87318	1.02	0.311	-35.97245	111.3092
middle	4.924201	18.81122	0.26	0.794	-32.6444	42.4928
old	-8.552866	26.12454	-0.33	0.744	-60.72718	43.62144
single	5.535708	19.29223	0.29	0.775	-32.99353	44.06495
kids	1.975745	17.29325	0.11	0.909	-32.56126	36.51275
nhhd	-3.620084	5.634561	-0.64	0.523	-14.87308	7.632912
owner	8.800345	13.27003	0.66	0.510	-17.70174	35.30243
retired	-16.49871	10.98994	-1.50	0.138	-38.44713	5.44971
student	.5229717	25.64481	0.02	0.984	-50.69325	51.73919
skilled	7.463489	15.48185	0.48	0.631	-23.45591	38.38289
longedu	1.086897	12.69666	0.09	0.932	-24.27008	26.44387
IncLow	-15.09036	12.90897	-1.17	0.247	-40.87136	10.69065
IncHigh	5.971815	12.1654	0.49	0.625	-18.32418	30.2678
copen	2.609906	11.12138	0.23	0.815	-19.60103	24.82084
city	4.115909	10.36433	0.40	0.693	-16.58308	24.8149

Table A3: Regression Analysis for 12-Month Horizon Only

pweight:	weight	Number of obs =	315
Strata:	county	Number of strata =	10
PSU:	id	Number of PSUs =	84
		Population size =	5198001
		F(20, 55) =	1.16
Subpopulation no. of obs =	72	Prob > F =	0.3215
Subpopulation size =	1169386.5	R-squared =	0.2835

cenidr_diff	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
Hpair_2_12	17.42383	12.1625	1.43	0.156	-6.810494	41.65815
Hpair_3_12	24.8416	11.73903	2.12	0.038	1.451076	48.23213
Hpair_4_12	21.7271	12.84187	1.69	0.095	-3.860888	47.31508
experimenter	-5.18943	5.889797	-0.88	0.381	-16.92511	6.546247
female	3.402451	5.051478	0.67	0.503	-6.662837	13.46774
young	-10.89186	8.804013	-1.24	0.220	-28.43424	6.65052
middle	-12.64597	8.308959	-1.52	0.132	-29.20194	3.90999
old	-23.73089	10.55791	-2.25	0.028	-44.768	-2.693789
single	-2.845906	8.850657	-0.32	0.749	-20.48122	14.78941
kids	-12.6746	7.193425	-1.76	0.082	-27.00781	1.658612
nhhd	-1.780957	2.358695	-0.76	0.453	-6.48076	2.918846
owner	-3.343441	4.978669	-0.67	0.504	-13.26366	6.576772
retired	-1.29042	8.250914	-0.16	0.876	-17.73073	15.14989
student	-8.791733	15.60392	-0.56	0.575	-39.88322	22.29976
skilled	7.433238	7.574066	0.98	0.330	-7.658419	22.52489
longedu	-2.894381	7.667932	-0.38	0.707	-18.17307	12.38431
IncLow	8.709371	8.037011	1.08	0.282	-7.304723	24.72346
IncHigh	3.94359	7.575723	0.52	0.604	-11.15137	19.03855
copen	7.872946	5.634084	1.40	0.166	-3.353211	19.0991
city	-7.32094	5.04133	-1.45	0.151	-17.36601	2.724128

Table A4: Regression Analysis for 18-Month Horizon Only

```
pweight:  weight                                  Number of obs    =       341
Strata:   county                                  Number of strata =        11
PSU:      id                                      Number of PSUs   =        97
                                                  Population size  = 5473963.2
                                                  F(  21,     66)  =      1.00
Subpopulation no. of obs =        97              Prob > F         =    0.4714
Subpopulation size       = 1524649.8              R-squared        =    0.1840
```

cenidr_diff	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
Hpair_2_18	-11.09285	16.01968	-0.69	0.491	-42.93891	20.75322
Hpair_3_18	-6.795508	14.3993	-0.47	0.638	-35.42037	21.82936
Hpair_4_18	6.706997	16.47549	0.41	0.685	-26.04519	39.45919
Hpair_5_18	-3.696854	15.94833	-0.23	0.817	-35.40107	28.00737
experimenter	-12.45005	7.010052	-1.78	0.079	-26.38557	1.48547
female	-2.520309	5.327792	-0.47	0.637	-13.11161	8.070992
young	4.491953	9.61185	0.47	0.641	-14.61577	23.59968
middle	5.024171	7.803481	0.64	0.521	-10.48864	20.53698
old	-3.062801	9.106846	-0.34	0.737	-21.16661	15.04101
single	4.270913	10.49719	0.41	0.685	-16.59681	25.13864
kids	14.58196	7.184542	2.03	0.045	.2995666	28.86436
nhhd	-.7939397	3.078095	-0.26	0.797	-6.91299	5.32511
owner	1.627256	6.312898	0.26	0.797	-10.92237	14.17688
retired	5.371876	9.697708	0.55	0.581	-13.90653	24.65028
student	23.86789	10.9962	2.17	0.033	2.008172	45.7276
skilled	6.508764	8.143582	0.80	0.426	-9.680141	22.69767
longedu	2.997287	8.117473	0.37	0.713	-13.13972	19.13429
IncLow	-7.54346	8.978882	-0.84	0.403	-25.39289	10.30597
IncHigh	-.2187955	8.446397	-0.03	0.979	-17.00968	16.57209
copen	.7280599	6.99486	0.10	0.917	-13.17726	14.63338
city	.0833522	7.208419	0.01	0.991	-14.24651	14.41322

Table A5: Regression Analysis for 24-Month Horizon Only

```
pweight:  weight                                    Number of obs    =       341
Strata:   county                                    Number of strata =        11
PSU:      id                                        Number of PSUs   =        97
                                                    Population size  = 5473963.2
                                                    F(  21,      66)  =      0.88
Subpopulation no. of obs =        97                Prob > F         =    0.6169
Subpopulation size       = 1524649.8                R-squared        =    0.1987
```

cenidr_diff	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
Hpair_2_24	-4.211588	13.91491	-0.30	0.763	-31.87352	23.45034
Hpair_3_24	-.1300271	12.74506	-0.01	0.992	-25.46636	25.20631
Hpair_4_24	7.238356	14.93551	0.48	0.629	-22.45245	36.92916
Hpair_5_24	.2684235	14.0477	0.02	0.985	-27.65747	28.19432
experimenter	-8.660197	5.836121	-1.48	0.141	-20.26202	2.941627
female	.812927	4.832367	0.17	0.867	-8.7935	10.41935
young	3.063827	9.797615	0.31	0.755	-16.41319	22.54084
middle	2.185824	6.432108	0.34	0.735	-10.60078	14.97243
old	-6.523605	8.385883	-0.78	0.439	-23.19419	10.14698
single	1.359593	9.130053	0.15	0.882	-16.79035	19.50954
kids	5.122729	6.353972	0.81	0.422	-7.508549	17.75401
nhhd	-.5256916	2.490982	-0.21	0.833	-5.477599	4.426216
owner	-5.555975	5.686504	-0.98	0.331	-16.86037	5.74842
retired	4.819042	9.507818	0.51	0.614	-14.08187	23.71996
student	17.28603	8.11339	2.13	0.036	1.157147	33.41492
skilled	5.157873	7.387551	0.70	0.487	-9.528091	19.84384
longedu	2.280192	7.744357	0.29	0.769	-13.11508	17.67547
IncLow	-.1115703	8.027746	-0.01	0.989	-16.0702	15.84706
IncHigh	4.824554	7.005536	0.69	0.493	-9.10199	18.7511
copen	-1.72816	5.679515	-0.30	0.762	-13.01866	9.562342
city	-5.408757	6.136521	-0.88	0.381	-17.60776	6.790243

Table A6: Regression Analysis for 6-Month Horizon With Changes in States of Nature

```
pweight:  weight                                    Number of obs    =       282
Strata:   county                                    Number of strata =         8
PSU:      id                                        Number of PSUs   =        73
                                                    Population size  = 4844778.6
                                                    F(  26,      40)  =      2.49
Subpopulation no. of obs =         49               Prob > F         =    0.0046
Subpopulation size       =   822780.2               R-squared        =    0.3878
```

cenidr_diff	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
Hpair_2_6	-8.240439	40.45173	-0.20	0.839	-89.02813	72.54725
Hpair_3_6	-4.00827	40.21823	-0.10	0.921	-84.32961	76.31307
experimenter	10.73071	8.528238	1.26	0.213	-6.30136	27.76277
female	-6.951046	7.434566	-0.93	0.353	-21.7989	7.896808
young	50.49849	32.49228	1.55	0.125	-14.39307	115.3901
middle	1.457331	19.38843	0.08	0.940	-37.26404	40.1787
old	.6449682	22.89446	0.03	0.978	-45.07842	46.36836
single	9.444289	20.41847	0.46	0.645	-31.3342	50.22278
kids	20.36114	14.30898	1.42	0.160	-8.215869	48.93814
nhhd	-.3130829	6.643646	-0.05	0.963	-13.58136	12.95519
owner	3.879523	11.82336	0.33	0.744	-19.73336	27.4924
retired	-8.788195	12.72312	-0.69	0.492	-34.19801	16.62162
student	5.116553	27.10502	0.19	0.851	-49.01591	59.24902
skilled	-2.791227	15.16062	-0.18	0.855	-33.06908	27.48663
longedu	-2.411531	11.64303	-0.21	0.837	-25.66426	20.8412
IncLow	-16.12484	13.38516	-1.20	0.233	-42.85684	10.60717
IncHigh	.0925229	14.65526	0.01	0.995	-29.17605	29.36109
copen	-3.542016	11.35983	-0.31	0.756	-26.22917	19.14513
city	-2.08904	9.385989	-0.22	0.825	-20.83416	16.65607
Ds_fin0	.8912056	1.864084	0.48	0.634	-2.831627	4.614038
Ds_exp	2.357029	2.34211	1.01	0.318	-2.320488	7.034545
Ds_inc	-2.036687	2.011817	-1.01	0.315	-6.054564	1.981189
Ds_fin1	-2.162412	1.40061	-1.54	0.127	-4.959623	.6347987
Ds_eco	-2.381932	1.410246	-1.69	0.096	-5.198387	.4345236
Ds_emp	2.73692	1.421553	1.93	0.059	-.1021179	5.575958
Ds_int	.1842693	.8806872	0.21	0.835	-1.574584	1.943123

Table A7: Regression Analysis for 12-Month Horizon With Changes in States of Nature

```
pweight:  weight                                   Number of obs    =       315
Strata:   county                                   Number of strata =        10
PSU:      id                                       Number of PSUs   =        84
                                                   Population size  =   5198001
                                                   F(  27,     48)  =      1.22
Subpopulation no. of obs =        72               Prob > F         =    0.2666
Subpopulation size       = 1169386.5               R-squared        =    0.3925
```

cenidr_diff	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
Hpair_2_12	7.357398	11.89146	0.62	0.538	-16.33686	31.05166
Hpair_3_12	13.83922	12.42315	1.11	0.269	-10.91444	38.59288
Hpair_4_12	8.204935	12.56853	0.65	0.516	-16.8384	33.24827
experimenter	3.78884	6.009445	0.63	0.530	-8.18524	15.76292
female	2.596878	4.435297	0.59	0.560	-6.240644	11.4344
young	-4.430641	12.16506	-0.36	0.717	-28.67005	19.80876
middle	-17.80669	8.834563	-2.02	0.047	-35.40994	-.2034393
old	-19.43788	10.41941	-1.87	0.066	-40.199	1.323241
single	-4.206653	9.458126	-0.44	0.658	-23.05238	14.63908
kids	-8.091924	7.532703	-1.07	0.286	-23.10116	6.917315
nhhd	.0123156	2.104338	0.01	0.995	-4.180669	4.2053
owner	-.5472022	5.638873	-0.10	0.923	-11.7829	10.6885
retired	-1.523601	8.595919	-0.18	0.860	-18.65134	15.60414
student	-8.512197	14.36456	-0.59	0.555	-37.1342	20.10981
skilled	4.715012	7.278986	0.65	0.519	-9.788684	19.21871
longedu	-6.85413	6.284414	-1.09	0.279	-19.3761	5.667838
IncLow	10.35054	7.348295	1.41	0.163	-4.291262	24.99233
IncHigh	6.206864	8.095224	0.77	0.446	-9.923222	22.33695
copen	10.18758	5.268482	1.93	0.057	-.3100975	20.68526
city	-5.266468	4.226605	-1.25	0.217	-13.68816	3.155227
Ds_fin0	1.178388	1.073133	1.10	0.276	-.9598767	3.316653
Ds_exp	-.2549892	1.092395	-0.23	0.816	-2.431634	1.921655
Ds_inc	1.79875	1.287663	1.40	0.167	-.7669743	4.364475
Ds_fin1	-.7321967	.854729	-0.86	0.394	-2.435281	.970888
Ds_eco	-1.940203	.6263944	-3.10	0.003	-3.188321	-.6920848
Ds_emp	1.105468	.9216863	1.20	0.234	-.7310325	2.941968
Ds_int	1.198202	.7739887	1.55	0.126	-.3440044	2.740408

Table A8: Regression Analysis for 18-Month Horizon With Changes in States of Nature

```
pweight:  weight                                   Number of obs    =        341
Strata:   county                                   Number of strata =         11
PSU:      id                                       Number of PSUs   =         97
                                                   Population size  = 5473963.2
                                                   F(  28,      59) =       1.09
Subpopulation no. of obs =         97              Prob > F         =     0.3853
Subpopulation size       = 1524649.8               R-squared        =     0.2876
```

cenidr_diff	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
Hpair_2_18	-22.39274	17.53402	-1.28	0.205	-57.24922	12.46374
Hpair_3_18	-18.38869	16.01675	-1.15	0.254	-50.22894	13.45156
Hpair_4_18	-6.012663	18.06488	-0.33	0.740	-41.92446	29.89913
Hpair_5_18	-13.08274	16.64557	-0.79	0.434	-46.17304	20.00756
experimenter	-8.303125	6.37873	-1.30	0.196	-20.98362	4.37737
female	-2.930868	5.318048	-0.55	0.583	-13.5028	7.641062
young	7.024645	11.65678	0.60	0.548	-16.14827	30.19756
middle	3.191914	7.938113	0.40	0.689	-12.58853	18.97236
old	2.127491	8.163698	0.26	0.795	-14.1014	18.35639
single	6.170962	10.3807	0.59	0.554	-14.46519	26.80711
kids	17.10762	7.067306	2.42	0.018	3.058281	31.15696
nhhd	1.735016	2.919159	0.59	0.554	-4.068081	7.538113
owner	1.680719	7.49698	0.22	0.823	-13.22278	16.58422
retired	4.084491	9.305268	0.44	0.662	-14.41377	22.58275
student	23.81778	11.41839	2.09	0.040	1.118773	46.51679
skilled	5.66358	8.183126	0.69	0.491	-10.60394	21.9311
longedu	2.275006	7.599125	0.30	0.765	-12.83156	17.38157
IncLow	-3.05717	9.205904	-0.33	0.741	-21.3579	15.24356
IncHigh	1.928781	8.602443	0.22	0.823	-15.17231	19.02987
copen	2.882345	6.160513	0.47	0.641	-9.364351	15.12904
city	.38131	6.689746	0.06	0.955	-12.91747	13.68009
Ds_fin0	.8637979	.9754115	0.89	0.378	-1.075256	2.802852
Ds_exp	.1097021	1.266488	0.09	0.931	-2.407992	2.627396
Ds_inc	1.130173	1.112439	1.02	0.313	-1.081283	3.341628
Ds_fin1	-.8094637	1.050446	-0.77	0.443	-2.897681	1.278754
Ds_eco	-2.374397	.8604921	-2.76	0.007	-4.084998	-.663795
Ds_emp	.71599	.8554393	0.84	0.405	-.9845671	2.416547
Ds_int	.3849213	.9067492	0.42	0.672	-1.417636	2.187479

Table A9: Regression Analysis for 24-Month Horizon With Changes in States of Nature

```
pweight:  weight                                  Number of obs    =       341
Strata:   county                                  Number of strata =        11
PSU:      id                                      Number of PSUs   =        97
                                                  Population size  = 5473963.2
                                                  F(  28,     59)  =      0.99
Subpopulation no. of obs =        97              Prob > F         =    0.4960
Subpopulation size       = 1524649.8              R-squared        =    0.2813

------------------------------------------------------------------------------
 cenidr_diff |      Coef.   Std. Err.       t     P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
  Hpair_2_24 |  -7.733469    15.8409     -0.49   0.627     -39.22414     23.7572
  Hpair_3_24 |  -1.939289   14.46224     -0.13   0.894     -30.68927    26.81069
  Hpair_4_24 |   2.606948   16.91827      0.15   0.878     -31.02547    36.23936
  Hpair_5_24 |  -3.210422   15.01701     -0.21   0.831     -33.06325     26.6424

experimenter |  -6.877976   5.015439     -1.37   0.174     -16.84834    3.092386

      female |  -.7193629    4.97992     -0.14   0.885     -10.61912    9.180391
       young |  -.1100479   10.22068     -0.01   0.991     -20.42809    20.20799
      middle |  -1.304833   6.406563     -0.20   0.839     -14.04066    11.43099
         old |  -4.360163   7.636342     -0.57   0.570     -19.54071    10.82038
      single |   3.956499   9.017731      0.44   0.662     -13.97016    21.88316
        kids |   6.705362   6.269981      1.07   0.288     -5.758948    19.16967
        nhhd |   .8510131   2.493798      0.34   0.734     -4.106493    5.808519
       owner |  -5.463285   6.396119     -0.85   0.395     -18.17835    7.251779
     retired |   2.302383   9.363704      0.25   0.806     -16.31205    20.91681
     student |   20.29348    8.50889      2.38   0.019      3.378363    37.20859
     skilled |   5.432709    7.74988      0.70   0.485     -9.973543    20.83896
     longedu |   .8266985   7.968691      0.10   0.918     -15.01453    16.66793
      IncLow |    .861093    8.86761      0.10   0.923     -16.76713    18.48932
     IncHigh |   6.963735   6.819852      1.02   0.310     -6.593683    20.52115
       copen |  -.5919685   5.226561     -0.11   0.910     -10.98203     9.79809
        city |  -5.766352   5.719256     -1.01   0.316     -17.13586    5.603154

     Ds_fin0 |   .1443199    .883515      0.16   0.871      -1.61205     1.90069
      Ds_exp |  -.0563692   .9775722     -0.06   0.954     -1.999718     1.88698
      Ds_inc |   1.581726   1.088793      1.45   0.150      -.582723    3.746175
     Ds_fin1 |  -.1006795   1.045412     -0.10   0.924      -2.17889    1.977531
      Ds_eco |  -1.768978     .81316     -2.18   0.032     -3.385487   -.1524699
      Ds_emp |   1.320741   .7757092      1.70   0.092      -.221318      2.8628
      Ds_int |   .0905562   .6668893      0.14   0.892     -1.235176    1.416288
------------------------------------------------------------------------------
```

Appendix B: Additional Discussion of Experimental Design Choices

(NOT FOR PUBLICATION)

Our goal is to place the existing empirical findings in context, so that we can see when and why non-constant discount rates emerge, and the implications for dynamic consistency. The panel feature of experimental design is, we believe, novel in the setting of discount rate experiments that involve real rewards. Our single-stage experimental design differs from much of the previous literature in some respects, although each of the key features has been employed previously in other experiments. We review each of these features.

A. Use of a Front End Delay

The most important design feature is that we employ a FED on the choices presented to subjects in order to control for any confounding effects from fixed premia due to transactions costs. It is important to recognize that the use of this FED means that we cannot differentiate between "quasi-hyperbolic preferences" and "exponential preferences," and that we do not believe that any credible design can do so.

O'Donoghue and Rabin [1999; p.103] motivate their analysis of time-inconsistent preferences with the following passage:

> People are impatient — they like to experience rewards soon and to delay costs until later. Economists almost always capture impatience by assuming that people discount streams of utility over time exponentially. Such preferences are time-consistent: A person's relative preference for well-being at an earlier date over a later date is the same no matter when she is asked.
>
> Casual observation, introspection, and psychological research all suggest that the assumption of time consistency is importantly wrong. It ignores the human tendency to grab immediate rewards and to avoid immediate costs in a way that our "long-run selves" do not appreciate. For example, when presented a choice between doing seven hours of an unpleasant activity on April 1 versus eight hours on April 15, if asked on February 1 virtually everyone would prefer the seven hours on April 1. But come April 1, given the same choice, most of us are apt to put off the work until April 15. We call such tendencies present-biased preferences: When considering trade-offs between two future moments, present-biased preferences give stronger relative weight to the earlier moment as it gets closer. (Footnotes omitted)

This passage seems to confound two things, each of which are important. The first is whether the length of the time horizon over which the discount rate is being elicited affects the discount rate, such as it does with continuously hyperbolic preferences. The second is whether the discount rate elicited with a FED is different than a discount rate elicited with no FED, i.e. whether preferences are quasi-hyperbolic. For an experimenter, and for subjects evaluating the credibility of being paid, these are very different questions. The formal analysis of O'Donoghue and Rabin [1999] employs quasi-hyperbolic preferences, which do not suffer from these confounds.

Building on Coller and Williams [1999] and Harrison, Lau and Williams [2002], our single-stage design is intended to separate the effects of the FED from the pure effects of the length of the time horizon. The potential importance of this distinction seems to have been first noticed by Benzion, Rapoport and Yagil [1989].[28] It was also highlighted by Roberts [1991; p.344], in the context of comments on Ainslie and Haendel [1983] and Winston and Woodbury [1991]:

> There is a bias toward choosing the small-early reward, particularly in the study by Ainslie and Haendel [1983] where real money changed hands. Ainslie and Haendel find ridiculously high implicit interest rates. An individual who prefers \$10 today to \$12.50 three days from now is turning down a rate of return that is a compounded annual rate of well over a trillion percent. Ainslie and Haendel, in a passage quoted by Winston and Woodbury, suggest that this is how people behave in one-time events and is not inconsistent with people willing to put money in a savings account at 5 percent - a repetitive activity.
>
> An alternative explanation is that Ainslie and Haendel have revealed the role of uncertainty in experiments lasting more than a single day. A student is likely to prefer \$10 today to \$12.50 in three days if there is some uncertainty about whether the experimenters will return in three days. I suspect that if respondents saw the experimenters consistently handing out money three days later at 25 percent interest rates so that they could be convinced that the experimenters were a going concern, they would take the larger return and not act as if they required scientific notation to express their discount rate.

One important reason that the FED design was introduced into discount rate experiments is the concern about differential credibility. While it may not completely solve the potential credibility problem,[29] it arguably mitigates it. For example, consider a FED of 30 days such that subjects choose

[28] Holcomb and Nelson [1992] re-examine the role of a FED with monetary payoffs, motivated by a concern that Benzion, Rapaport and Yagil [1989] only studied hypothetical choices. Their FED was only one day, so it is not obvious that the subjects viewed this as substantially different from there being no FED. They observed no apparent effect of the one-day FED on behavior.

[29] It could be that credibility in the mind of the subject is a decreasing function of the time between the experiment and the next payoff. Hence an experiment with a FED of 10 years would be tantamount to a hypothetical

between receiving different payment amounts in 30 days and in 60 days. While the subject may have some doubt about actually receiving payment in 30 days, this doubt is not likely to differ much from the doubt about receiving payment in 60 days. Similarly, the FED equalizes transactions costs between the two payment options. While there are some costs to returning at a later date to receive payment, these costs are not likely to differ between returning in 30 days vs. returning in 60 days.[30] The FED also serves to equalize any other unspecified differences subjects may perceive between the two payment options. For example, if subjects have a "passion for the present," they demand a premium in order to accept a delay of any length. In a choice between immediate payment and delayed payment, this premium is attached only to the delayed payment. However, if both payments are delayed, the premium applies to both choices and thus becomes irrelevant in choosing between them.

Having said this, there are many field settings in which the relevant issue is what the discount rate is for "money today" versus "money in the future."[31] Even if the experimenter faces the inferential problem of having to then tease apart the effects of time horizon from credibility, transactions, or other subjective costs, it is entirely appropriate that experiments with no FED be considered. If there is a finding that discount rates are not constant when there is no FED, then it is a matter for interpretation as to whether this is a subjective differential cost effect or a time-inconsistency effect (or both).

Evidence for the behavioral importance of a 30-day FED was provided by Coller and Williams [1999]. In one of their experimental treatments they had no such delay, and the results from those experiments can be directly compared to their other experiments. After some minor modifications to their statistical analysis, their results provide evidence that the use of a FED decreases elicited rates by a large amount. The average effect of having no FED is to increase elicited rates by 28 percentage points, with a 95% confidence interval between 52 percentage points and 3 percentage points; the

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experiment in most settings. Conversely, a FED of 5 minutes would be the same as having no FED.

[30] This assumes no dramatic change in circumstances that makes collecting the payment more difficult later. For this reason, we are careful to only offer choices that pay off on a week day during the current teaching semester.

[31] Such settings might include individual decisions of whether to consume now or save for future consumption, or to purchase a more expensive but energy efficient appliance. We believe that individual decisions involving more significant sums of money or public policy decisions are better characterized as having a FED.

coefficient on the dummy variable denoting this treatment is statistically significant at the 4.8% level.

Read [2001] develops an experimental design which includes a FED, and finds that it *increases* the elicited discount rate (reported as a decline in the discount factor) in the one experiment in which he used salient monetary rewards. Unfortunately, the method he uses to elicit discount rates is not incentive compatible, and he was obliged to drop some subjects that appeared to have exploited this flaw by claiming that they always preferred the shorter horizon option. Even if some subjects did not exploit the flaw to the point where they were dropped, it is possible that they were aware of it and inflated their responses to a point that they thought would not be detected. Either way, there are obvious problems of control over incentives to truthfully reveal discount rates. His design also examined only one horizon (8 months for the non-salient experiments, and 6 months for the salient experiment), making it difficult to tell if the effect of the FED persisted across other time horizons, which is the focus of our design; the same limitation applies to the design of Coller and Williams [1999].

Kirby and Santiesteban [2003] use a FED of 1 day in an experiment in which they elicited the individual discount rates of financially-motivated subjects for horizons of between 1 and 43 days. They compare results with comparable experiments using no FED, and find that there is essentially no difference in the pattern: discount rates decline with time horizon. These results are valuable, and accord with our priors. They show that 1 day appears to be insufficient to overcome the subjective costs and resulting fixed premium we hypothesize.

B. Hypothetical Responses

One major difference in experimental design between the experiments reported here and most of the existing literature is our use of real, rather than hypothetical, payments. One of the hallmarks of experimental economics is the use of instructions and payoffs designed to ensure control over the incentives faced by individuals. This control facilitates interpretation in terms of existing theory. One aspect of control is the use of payoffs to the subject that vary with the responses made by the subject, and in a way that the subject understands. This is called "salience" in the terminology of Smith [1982].

It is also important that these payoffs be observable and measurable by the experimenter, thus ruling out "intrinsic motivation" as a candidate explanation for observed behavior. We cannot rely on subjects' personal desires to do the task correctly for their own satisfaction, since that is not observable and subjects may differ in this regard. Hence monetary rewards are customarily used by experimental economists to motivate subjects.

The existing literature relies heavily on responses to hypothetical choice situations which do not employ salient monetary incentives.[32] While hypothetical scenarios may be entirely appropriate in some contexts, the argument offered in favor of using hypothetical scenarios in many studies is simply convenience. The notion here is that there are certain "realistic" questions which one cannot feasibly ask within the usual constraints of budgets and ethical review boards.

Loewenstein and Thaler [1989; p. 184] offer the following argument in favor of using hypothetical choice scenarios:

> In this study, and some others described here, the questions asked were hypothetical. Of course, all things being equal it would be better to study actual choices. However, there are serious trade-offs between hypothetical and real money methods. Using hypothetical questions one can ask subjects to consider options that incorporate large amounts of money, both gains and losses, and delays of a year or more. In studies using real choices, the experimenter must reduce the size of the stakes and the length of the delay, and it is difficult to investigate actual losses. Also, in a hypothetical question, one can ask the subject to assume that there is no risk associated with future payments, while in experiments using real stakes, subjects must assess the experimenter's credibility.

The flexibility made possible by using hypothetical payments prompt Loewenstein and Thaler [1989] to utilize a hypothetical scenario in their study. However, they acknowledge that real choices made in the presence of economic incentives are more credible. It is only when the research question necessitates the use of unaffordably large prizes, or runs counter to ethical constraints which prohibit the imposition of losses, that hypothetical responses are reasonable substitutes for real ones. Because sufficient evidence[33] exists that hypothetical responses *can* be misleading in valuation and choice settings of interest to economists, we choose not to rely on inferences made from hypothetical

[32] Frederick, Loewenstein and O'Donoghue [2002] review the literature.
[33] See the review in Harrison and Rutström [2005] of valuation tasks designed to test for hypothetical bias.

choices.

C. Eliciting Truthful Responses

An important aspect of salience is the understanding that subjects have for the way in which payoffs are affected by their decisions. Furthermore, for payoffs to be salient, subjects must feel confident that the earned payoffs will actually be received. A second important design consideration that distinguishes the experiments reported here from many others is the effort to ensure credibility and to provide a simple, incentive compatible payment mechanism.

Problems with credibility and subject understanding can greatly limit the inferences drawn from any experiment. For example, Horowitz [1991; p.320] honestly reports problems with experimental control that should cause some pause about accepting his results:

> If anything, the results reported in this paper understate how unusual the behavior was in our experiment. [...] First, the winners of Auctions 1 and 2 were extremely reluctant to pay for their bonds. Two out of six of the bond purchasers refused to pay for the bonds they were supposed to buy; the four other winners all had elaborate excuses for their inability to pay for the bonds immediately and took as long as a week to pay eventually. This reaction was *especially* pronounced in Auction 2.
>
> Second, a dramatic change in bidding behavior was observed between Auction 1 and Auction 2. Over 55 percent (40 out of 70) of the participants were willing to pay less for the second bond than they were for the first one, despite the fact that the payoff date was 30 days closer. The four winners of the first auction bid especially low in the second. Two of them submitted bids of $0 and one submitted a bid of $5.00; the fourth winner dropped the class between the two auction dates. In scrutinizing the reliability of our data, one might argue that a single auction (Auction 1) is insufficient to familiarize individuals with the auction mechanism or the tradeoffs involved with our bonds. But the striking change in behavior between Auctions 1 and 2 suggests that individuals learned a lot from Auction 1.

The credibility of the payoffs is an issue that was also recognized by Neill, Cummings, Ganderton, Harrison and McGucken [1994] in experiments designed to elicit real payments for a physical good (an art object). They addressed it by modifying the experimental procedures for their Vickrey auction by requiring subjects to include the cash or a check for their bid in an envelope, to make their bid effective. The losers would have their envelopes returned unopened, and the winner would simply be refunded the difference between their bid and the second-highest bid. Quite apart from ensuring that the experimenter received some real payment from the winner for the object sold, this procedural device undoubtedly clarified for all subjects that there were real consequences of their bid. In this manner, control over incentives was regained.

Kirby and Maraković [1995] used real rewards for their subjects in discount rate experiments in which the subjects were asked to state an amount of money they would accept immediately in exchange for a deferred payment. The auction institution used was a first-price sealed-offer auction, in which the subjects were told that the winner would be the person that offered the smallest amount and that the winner would receive that amount instead of the deferred payment. The first-price auction is not one in which subjects have a rational incentive to reveal their true value, although it could be inferred by the experimenter with some strong auxiliary assumptions.[34] Another problem was that the actual procedures for payments were quite different than the procedures explained to subjects: "In fact, the auction was entirely simulated and subjects' bids were never really compared. The computer randomly determined whether the subject "won" the bid with a probability inversely related to the size of the bid." (p. 24). One might say that this is immaterial since the subjects did not know this, but such deception calls into question the credibility of anything that the experimenter has to say to the subject.

Both of these concerns were addressed in an important follow-up study by Kirby [1997]. This study does use procedures that meet the salience standards of experimental economics. The subjects were provided real rewards, and were required to come to the experiment with $20 in cash to use for bidding (p.59). He also employed an example, patterned after Neill et al. [1994], in which an auction for a used car was used to explain the notion that truthful bidding is a dominant strategy. Finally, he

[34] Specifically, the experimenter would have to assume that the subject followed a symmetric Nash Equilibrium offer function, relating his offer to the number of active subjects in the auction and the range of possible valuations that individuals could have drawn. In addition, some assumptions about risk attitudes would need to be made. In the simplest case of a first-price sealed-*bid* auction with risk neutral bidders and valuations drawn from the unit interval, the Nash Equilibrium bidding rule is for the subject to bid a fraction of his valuation, equal to the number of bidders minus one, divided by the number of bidders. Hence one could just invert this fraction, multiply it by the observed bid, and infer the true valuation. Quite apart from the plausibility of these auxiliary assumptions being true for these subjects, the first-price auction is known to provide weak incentives to the subject to reveal the optimal bid accurately.

studied the effect of not providing feedback on winning bids during repetitive trials.[35]

Although Vickrey auctions are in general demand-revealing, it is possible that we may not elicit truthful responses with this institution if subjects do not understand the dominant strategy logic.[36] An alternative procedure, known as the Multiple Price List (MPL) auction and shown in Table 1, has been employed by Kirby and Maraković [1995], Kirby, Petry and Bickel [1999], Coller and Williams [1999] and Harrison, Lau and Williams [2002]. The idea is to offer subjects a series of choices between a short-term reward and a longer-term reward, and to vary the parameters of the reward in the series offered to any one subject. One could vary the principal amount, the time horizon between the two rewards, the FED, the rate of return implied by the choices, the way in which the alternatives are ordered when presented to subjects, or any combination of these. The logic of telling the truth is even more transparent than in the bidding setting, since the subject literally gets the binary choice he or she makes.

One difference between the implementation of the MPL procedures across these studies is the ordering of the payoff choices. Kirby and Maraković [1995] and Kirby, Petry and Bickel [1999] presented the options to each subject in a random order, whereas Coller and Williams [1999] and Harrison, Lau and Williams [2002] presented them in increasing order of the implied rate of return. The latter ordering was employed to make the trade-off to subjects transparent, and hence to focus on the trade-off rather than on the computational difficulty of comparing the alternatives. Moreover, Coller and Williams [1999] and Harrison, Lau and Williams [2002] presented the annual rate of return and the annual effective rate of return for each option, whereas the other studies did not. In addition to achieving consistency with the informational requirements of "fair lending laws" in most developed countries, these procedures likely minimize the informational burden of the task and focus the subject on the trade-offs entailed by the alternatives.

[35] His experiment 3 provided this information to 2 of the 4 bidders in each trial; neither of his previous experiments provided any feedback. The concern with providing feedback is that it *may* lead to affiliation of beliefs about the valuation of the object for sale, as explained by Harrison, Harstad and Rutström [2004].

[36] See Rutström [1998] and Kagel, Harstad and Levin [1987] for discussions of this problem with Vickrey auctions.

Another difference across studies is whether the principal amount was varied. Coller and Williams [1999] and Harrison, Lau and Williams [2002] maintained the same principal amount for all of the short-term options, so that the only thing that varied for each subject was the rate of return in the longer-term reward. Again, the objective was to make the trade-off transparent, rather than additionally test if the subject was able to make these calculations. To illustrate the potential importance of this simple design feature, Table B1 lists the options offered in Kirby and Maraković [1995] and Kirby, Petry and Bickel [1999] in order of the implied annual effective rate of return on each choice: the actual order presented to the subjects is listed in the first column. Table B1 also shows the percentage of subjects that chose the deferred payment option.[37] It is obvious from Table B1 that the change in the percentage of subjects choosing the deferred option is not monotonic, a fact which may reflect the additional computational burden of the task.

Yet another difference is whether individual subjects faced differing choice horizons. Coller and Williams [1999] and Harrison, Lau and Williams [2002] provided the subjects with only one time horizon in each payoff table, while Kirby and Maraković [1995] and Kirby, Petry and Bickel [1999] had many different time horizons in the same payoff table. Although Harrison, Lau and Williams [2002] vary the time horizon across subjects and ask some of their subjects to fill in responses for all four horizons, within each MPL payoff table the subject had only one time horizon to consider.

There may be legitimate reasons for using these "jumbled up" tasks, such as when there are strong reasons to suspect framing effects,[38] but such tests make any inferences about discount rates

[37] Kirby and Maraković [1996] deleted some subjects due to inconsistent responses, and we summarize their final sample of 621. Kirby, Petry and Bickel [1999] used heroin addicts as well as 60 control subjects that were not heroin addicts, and we report results for the latter sample only. The financial incentives for the Kirby and Maraković [1996] study were also relatively weak: 2,000 students were asked to return the questionnaire, knowing that one of those that returned the survey would be eligible to receive *up to* $85 based on their responses. If the students correctly guessed the potential sample size of 2000, and assumed optimistically that they were awarded the $85 prize, the expected payoff from returning the questionnaire was only 4.25 cents. If they rationally estimated that only 672 would return the questionnaire, and continued to be optimistic with respect to the $85 prize, the expected payoff changes to 13 cents.

[38] One of the intentions behind such "jumbled up" payoff matrices is to avoid framing effects which may occur when subjects evaluate options in an ordered sequence. These framing effects emerge when subjects anchor on the first option and evaluate all subsequent offers in relation to the first. Since the MPL presents all options simultaneously to subjects, in one payoff matrix, we do not believe that the choice between jumbling or ordering will significantly affect framing in our setting. One concern with the MPL procedures used in Coller and Williams [1999] and Harrison, Lau and Williams [2002] is that the use of a fixed array of alternatives might "cue" subjects to switch their payment preference somewhere in the middle of the table. This hypothesis would be easy to test by varying the number of rows or changing the numerical values in the same number of rows. However, our design is focused on a comparison across FED treatments that all use the MPL method, so any effect from that method would be applicable to all of our FED

joint hypothesis tests, that the subject is able to make these comparisons despite their computational difficulty, and that the subject chooses the preferred alternative in accord with his true discount rate.[39] In the experiments reported here, we choose to simplify the computational aspect of the choice we ask subjects to make. We employ the MPL mechanism using a constant principal amount with choices ordered in terms of increasing rates of return. We provide subjects with both the annual interest rate and the annual effective rate implied by each choice. We also implement the different time horizons on a between subject basis.

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treatments.

[39] Such joint tests are quite interesting, and of some importance for policy settings in which there are no regulatory constraints on the information that must be provided in financial transactions, or where decision-makers might not understand the import of the information provided, but they are tests that are best undertaken after we have resolved how to elicit discount rates in the most transparent setting possible. We would add to this list tests of the effects of elicitation mode: for reasons described in Harrison, Harstad and Rutström [2004], we are not surprised that there is considerable variation in elicited responses to discount rate questions in the psychology literature when the choices are framed differently.

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Table B1: Aggregate Results from Comparable Psychology Experiments

Question Order	Amount Today	Amount in the Future	Difference in Amounts	Delay in Days	Annual Effective Rate	Percent of Subjects Choosing Future Payoff
			A. KIRBY AND MARAKOVIĆ [1996]			
15	$53	$55	$2	55	27%	12
4	$34	$35	$1	43	27%	12
7	$83	$85	$2	35	28%	12
12	$65	$75	$10	50	180%	44
20	$27	$30	$3	35	196%	17
9	$48	$55	$7	45	197%	34
8	$21	$30	$9	75	454%	36
16	$47	$60	$13	50	480%	57
18	$50	$80	$30	70	1021%	74
3	$67	$85	$18	35	1056%	70
10	$40	$65	$25	70	1114%	67
14	$30	$35	$5	20	1503%	44
19	$45	$70	$25	35	9312%	90
2	$40	$55	$15	25	9708%	71
11	$25	$35	$10	25	12613%	68
21	$16	$30	$14	35	64171%	86
6	$32	$55	$23	20	1713182%	94
17	$40	$70	$30	20	2369554%	97
1	$30	$85	$55	14	42708226751144%	99
13	$24	$55	$31	10	923532365791074%	99
5	$15	$35	$20	10	1766705945627180%	99
			B. KIRBY, PETRY AND BICKEL [1999]			
13	$34	$35	$1	186	6%	0
9	$78	$80	$2	162	6%	2
1	$54	$55	$1	117	6%	2
20	$28	$30	$2	179	15%	0
17	$80	$85	$5	157	15%	0
6	$47	$50	$3	160	15%	2
26	$22	$25	$3	136	40%	0
12	$67	$75	$8	119	41%	3
24	$54	$60	$6	111	41%	5
16	$49	$60	$11	89	127%	10
22	$25	$30	$5	80	127%	3
15	$69	$85	$16	91	128%	20
2	$55	$75	$20	61	524%	33
10	$40	$55	$15	62	535%	20
3	$19	$25	$6	53	545%	22
21	$34	$50	$16	30	10130%	52
18	$24	$35	$11	29	10716%	30
25	$54	$80	$26	30	11078%	67
14	$27	$50	$23	21	3868337%	85
23	$41	$75	$34	20	5259992%	90
5	$14	$25	$11	19	5904279%	72
8	$25	$60	$35	14	598219411260%	93
19	$33	$80	$47	14	774637209375%	100
7	$15	$35	$20	13	1549267326926%	90
11	$11	$30	$19	7	2563880995240443200000000%	100
4	$31	$85	$54	7	3377984399504987200000000%	100
27	$20	$55	$35	7	3928735811211553600000000%	98