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 temporal stability. Nevertheless, 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 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 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 no evidence that allows us to reject the hypothesis of dynamic consistency, although we are able to identify narrow segments of the population that appear to suffer from dynamic inconsistency.

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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.[1] Thus subjects might be asked to state their discount rate over a 1-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," holding and acting on preferences at one point in time that contradict the preferences of the same individual at a later date.

This methodology for testing dynamic consistency relies on important assumptions of inter-temporal separability (Machina [1989]) and temporal stability in the discount rate function. If either of these assumptions are violated, inferences about dynamic inconsistency cannot be drawn from non-constant discount rates in the usual way. An alternative way to test for dynamic consistency is to elicit the discount rate for an individual at different points in time holding the maturation date of the assets constant but allowing the horizon to vary. Thus, subjects would be asked to state their discount rate over a 24 month period starting at some reference point in time, and then be asked again 23 months later for their discount rate for the remaining 1 month period. Ceteris paribus, if the two are not the same one can claim that preferences are not dynamically consistent. In particular, hyperbolic discounting would require that the latter is significantly higher than the former.

We report the results of a large-scale longitudinal experiment undertaken in the field that allows us to directly examine the stability of preferences over time.[2] The experiment is designed precisely for the purpose of eliciting discount rates for assets of different horizons while holding the maturation dates constant. 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-

[1] See, for example, Thaler [1981], Loewenstein [1988], Benzion, Rapaport and Yagil [1989], Horowitz [1991], Winston and Woodbury [1991], Holcomb and Nelson [1992], Lazo, McClelland and Schulze [1992], Shelley [1993] and Pender [1996].

[2] Our experiments are "artefactual field experiments" in the terminology of Harrison and List [2004].

visited 97 of these subjects and repeated these tasks, but using horizons that matched the remaining horizons of the original 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. The latter information is used to control for the possible confound generated when other economic circumstances that affect their discount rates change.[3] *We find no evidence that discount rates decline with the length of the horizon.* Nevertheless, we can identify narrow segments of the population that tend to exhibit dynamic inconsistencies.

Remarkably, there have been few direct tests of this empirical premise of the dynamic inconsistency literature using real rewards. Oxoby and McLeish [2004] conducted a longitudinal lab experiment in which subjects participated in four sessions over a seven-week time period. Apart from using a much shorter time period their design also differ from ours in other respects. In particular, they do not control for the effect on preferences from changes in the front end delay or from changes in the maturation date.

We review our experimental design in section 1, and discuss our procedures in section 2. We present the longitudinal evidence in section 3, using within-subject comparisons of individual discount rates over a two-year time frame. In section 4 we draw conclusions.

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).[4] Subjects were given payoff tables such as the one illustrated in Table 1. In this example, Option A offered 3000 DKK in one month and Option B paid 3000 DKK + x DKK in seven months, where

[3] Another approach is to introduce uncertainty about when the payoffs are realized. In this case, the corresponding preferences may entail hyperbolic discounting, giving rise to (rational) preference reversals (Dasgupta and Maskin [2005]).

[4] Our experimental procedures are documented in detail in Harrison, Lau, Rutström and Sullivan [2005], so we focus here just on the basics. The longitudinal responses from this experiment have not been evaluated before.

x ranged from annual rates of return of 5% to 50% on the principal of 3000 DKK, compounded quarterly to be consistent with general Danish banking practices on overdraft accounts.[5] The payoff tables provided the annual and annual effective interest rates for each payment option, and the experimental instructions defined these terms by way of example. Subjects were asked to choose between Option A and B for each of the 10 payoff alternatives, and one decision row was selected at random to be paid out at the chosen date. If a risk-neutral subject prefers the 3000 DKK in one month then we can infer that the annual discount rate is higher than x%; otherwise, we can infer that it is x% per day or less.[6]

We use a multiple-horizon treatment employed by 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 Series 1 of the experiment, corresponding to 6 different time horizons: 1 month, 4 months, 6 months, 12 months, 18 months, and 24 months. In each task subjects are provided two future income options rather than one "instant income" option and one future income option. We follow HLW and use a front end delay (FED) of one month in all tasks. For example, they were offered 3000 DKK in one month and 3000 DKK + x DKK 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 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

[5] At the time of the first phase of the experiments the exchange rate was approximately 6.55 DKK per U.S. dollar, so the principal of 3,000 DKK corresponded to 458 USD.

[6] We assume that the subject does not have access to perfect capital markets, as explained in CW (p.110) and HLW (p.1607ff.). This assumption is plausible, but also subject to checks from responses to the financial questionnaire that CW, HLW and we ask each subject to complete. We also assume that subjects consume the monetary amounts in options A and B at the time stated in the instrument, and do not smooth consumption as the result of the outcomes from these tasks. These are common assumptions in the discounting literature, as noted by Frederick, Loewenstein and O'Donoghue [2002; p.380]. The effects of allowing for field borrowing and lending opportunities on elicited discount rates for risk neutral subjects are discussed by CW and HLW; Harrison, Harstad and Rutström [2004] discuss the general implications of allowing for extra-experimental trading opportunities on inferences from experimental responses. The effects of allowing for consumption smoothing are harder to identify, since they require a more elaborate specification of the intertemporal choices of the individual. The upshot is that we are eliciting time preferences over money endowments, and these can only be interpreted as time preferences over money consumption when these assumptions are imposed.

constant.

Each subject responded to all six discount rates tasks and one task and row was chosen at random to be played out. Future payments to subjects were 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. This payment procedure is similar to a post-dated check, and automatic transfers between bank accounts are a common procedure in Denmark.[7] Finally, each subject was given a 10% chance to receive actual payment.

We implement one extension of the basic multiple price list (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 the underlying preferences are monotonic 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 annual interest rates of 20% and 25%, 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

[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.

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 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 [2006] 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] The experiments were conducted in October 2003 and they used similar monetary incentives and random devices in the discount rate tasks. They conclude that the iMPL format has no discernible effect on elicited discount rates, that there is no effect from using asymmetric frames on discount rates, but that there is a small order effect on the second task after the initial task (around 3½ percentage points higher). The possibility of this small order effect has no effect on our overall conclusions, as it turns out.

Our design differs in several ways from many of those used in previous literature where evidence of dynamic consistency has been reported. 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.[10]

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[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.

[10] 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].

First, we use real rewards instead of hypothetical rewards. There is debate about the importance of using real rewards, but since there is evidence that real and hypothetical responses differ in *some* task domains, we think it prudent not to risk adding a confound by failing to ensure the control over subject motivation that requires real rewards as a necessary condition. Second, we do not "scramble" tasks that involve different horizons, principals and premia to delay. Scrambling is generally associated with annual discount rates that are extraordinarily high. Our MPL is deliberately ordered in terms of return on principal, to provide subjects with a transparent task. 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. In some instances, the absence of a FED has increased elicited rates by hundreds of percentage points. 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.

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 FED of 1 month.

Across series 2 through 5 we re-visited 97 of these 253 subjects. The objective was to re-visit 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).[11]

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. To be precise, this means that the 1-month IDR elicited in Series 2 corresponds in "real time" to the last month of the 4-month IDR elicited in Series 1. This is *exactly the type of paired comparison that is referenced in the thought experiments underlying statements about dynamic consistency*. 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.

Thus our design provides 14 paired comparisons and 341 observations, with time horizons varying from 1 month up to 21 months. We also have some repetition of three of the horizons across the re-visits. The 1-month horizon is tested in Series 2, Series 3, Series 4 and Series 5. The 7-

[11] Financial constraints prevented us from re-visiting every single subject in Series 1 of the experiment. The cost of conducting Series 2 through 5 of the experiment was approximately 8,000 dollars per series (including fixed participation fees and earned income in additional risk aversion tasks), i.e. 32,000 dollars in total. Re-visiting every person 4 times and providing the same monetary incentives in each series would exceed our otherwise substantial data budget.

month horizon is tested in Series 3, Series 4 and Series 5. Finally, the 13-month horizon is tested in Series 3 and Series 4. Thus in these instances we have several opportunities at different points in time to make paired comparisons for a given horizon.

C. Sampling Procedures and Data

The sample for the field experiments was designed to generate a representative sample of the adult Danish population. Series 1 of the experiment was conducted between June 2 and June 24, 2003, and the 253 subjects participated in 20 sessions spread across the country. In Series 2-5 we repeated the individual discount rate tasks and provided similar monetary incentives and payment methods. Each subject was interviewed in private in Series 2-5, because attendance otherwise would have been too low.

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.

3. Results

A. Raw Results

Figures 1 and 2 collect the raw results of our experiments. Each displays the distribution of elicited discount rates by horizon, using box plots for each horizon.[12] Panel A of Figure 1 shows the raw results of the Series 1 experiments and Panel B of Figure 1 illustrates the raw results of the Series 2 through 5 experiments. Figure 2 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 rate interval, after all of the iterations of the iMPL have been

[12] 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.

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 observations, 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%[13] 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.[14]

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 declining with the length of the horizon, although not dramatically so. The most prominent 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 still 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 2 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

[13] 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. We also performed all of our tests on discount rates that are corrected for the respondent's potential access to financial markets. Making these corrections, and censoring to market rates, has the effect of substantially increasing the average rates due to increased uncertainty about the true upper end of the possible range. We prefer to present the results based on the raw elicited rates here. None of our qualitative results are weakened by using censored rates.

[14] They are well below 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.

the hypotheses of interest we must turn to in-sample comparisons across horizons and implied maturation dates, and then control for possible changes in the state of nature over time.

Figure 3 displays the data from the within-subjects comparisons that our experimental design was constructed to allow. We show the difference between the IDR elicited from the subject at two different points in time for paired assets with the same maturation date. If there was no change in the elicited IDR the data point underlying the histogram in Figure 3 would be zero. If the IDR had increased in the later time period (for the shorter horizon with the same maturation date), the data point would be positive. The differences in discount rates illustrated in Figure 3 do not show any apparent tendency to have a positive or negative bias. Nevertheless, these differences in discount rates reflect all possible within-subjects comparisons.

It is also possible to classify subjects according to four groups based on the same paired assets: (i) subjects with no change in discount rates, (ii) subjects with increasing (hyperbolic) discount rate functions for the shorter horizons, (ii) subjects with declining discount rate functions, and (iv) subjects who reveal both higher and lower discount rates for the shorter horizons. Figure 4 displays the shares of subjects that fall into those four categories conditional on a noise parameter. The noise parameter defines a symmetric interval around the elicited difference in IDR. When the noise parameter is equal to zero, we find that 27% of the subjects have consistently increasing discount rates functions over all comparable horizons, 26% of the subjects have consistently declining discount rate functions, and 47% of the subjects display an inconsistent pattern. The share of subjects with no difference in elicited IDR increases with the size of the noise parameter by definition, while the share of subjects with increasing or declining discount rate functions falls to zero with a sufficiently high level of noise.

A direct test of the hypothesis that discount rates are stationary as the time horizon shrinks for the same maturation date is possible with a regression model. We define the dependent variable as the difference in discount rates between the shorter and the longer time horizon for two assets with the same maturation date for a subject. The longer time horizon always corresponds to one of

the horizons in series 1 and the shorter horizons are always from one of series 2-5. Recall that each subject only participates in one of series 2-5. The independent variables are binary dummy variables for each of the assets in the later series. The statistical model allows for the deliberate survey design we employed; see Harrison, Lau, Rutström and Sullivan [2005] for further details. 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.[15] The model needs to also control for possible changes in the states of nature facing our subjects over the time frame between experiments.[16] The next section discusses these controls.

B. Controlling for Changes in the States of Nature

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:

1. Would you say that you and your family are better off or worse off financially than you were X months ago?
2. 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?
3. Do you expect any major change in your family situation that will lead to higher earnings or lower earnings during the next X months?
4. On balance, do you think that you and your family will be better off or worse off financially X months from now?
5. 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?
6. Do you think that there will be more or less unemployment during the next X months?
7. Do you think that interest rates for borrowing money will go up or go down during the next X months?

[15] The use of clustering to allow for "panel effects" from unobserved individual effects is common in the statistical survey literature. Clustering commonly arises in national field surveys from the fact that physically proximate households are often sampled to save time and money, but it can also arise from more homely sampling procedures. For example, Williams [2000; p.645] notes that it could arise from dental studies that "collect data on each tooth surface for each of several teeth from a set of patients" or "repeated measurements or recurrent events observed on the same person." The procedures for allowing for clustering allow heteroskedasticity between and within clusters, as well as autocorrelation within clusters. They are closely related to the "generalized estimating equations" approach to panel estimation in epidemiology (see Liang and Zeger [1986]), and generalize the "robust standard errors" approach popular in econometrics (see Rogers [1993]).

[16] Horowitz [1992; p.177] collects information on financial characteristics of the individual to control for changes in states of nature, but does not report if it changed the statistical inference about temporal stability of risk attitudes over a 2-month period.

We readily concede that these questions do not exhaust the set of conceivable events that could occur over the horizon of interest,[17] 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 response in the Series 1 session to the comparable response in the later session. Thus for any question-horizon-subject combination 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.[18] 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 4 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

[17] In the demographic survey conducted in Harrison, Lau and Williams [2002] one subject ask if he should state his current sex or the sex he would be at the end of the longest horizon. Generally, however, we find very little variation in individual characteristics over the 17 months time span considered here.

[18] There is no indication of strong linear associations between the seven variables that reflect changes in states of nature. The highest positive correlation coefficient it 0.35 and the lowest negative correlation coefficient is -0.18.

borrowing money will go up in the near future.[19]

C. Statistical Controls for Horizon Effects

Table 5 uses these measures of the change in each state of nature to control for possible confounding changes in the elicited discount rates for each individual. We estimate a survey regression model and allow for the fact that subjects in one county were selected independently of subjects in other counties, as well as the possibility of correlation between responses by the same subject. We also control for the fact that there may have been a change in the experimenter between the two times the subject was interviewed. Of course, the main controls are binary dummies for each of the horizons considered, as shown in our experimental design in Table 2.

Only one of the horizons has a coefficient estimate indicating a large increase in elicited discount rates that is statistically significant. The discount rates for the 1-month horizon elicited in Series 4 were 10.5 percentage points larger than the discount rate for the corresponding 12-month horizon elicited in Series 1 (recall Table 2) with a *p*-value of 0.03. None of the other 13 pair-wise comparisons provides evidence that discount rates increase as the horizon approaches. In fact, we even find one case where the discount rate for a shorter horizon (13 months for a corresponding 18 months in Series 1) is lower by 9.6%. Most of the estimated effects are small in size, and statistically insignificant. The *p*-values for each individual series-horizon pair are shown in Table 5. The hypothesis that all horizons jointly have an estimated effect that is zero cannot be rejected, with a *p*-value of 0.87.[20] The overall conclusion is that, even though we observe some instances of variation

[19] The Danish economy was very stable from June 2003 to November 2004 when we conducted the experiments. The interest rate set by Danmarks Nationalbank was constant and equal to 2%, and the seasonally adjusted unemployment rates also remained unchanged at 6.3%. The lowest unemployment rate in this period of time was 6.1% (August 2003) and the highest rate was 6.7% (January 2004).

[20] We also ran regressions including interaction effects between the variables capturing changes in the states of nature and the binary horizon dummies. Further, as discussed in Andersen, Harrison, Lau and Rutström [2006], there is a possibility of an order effect in the second task possibly increasing the elicited rate by 3% on average. In this experiment the second task corresponds to the 4-month horizon in series 1, the 3 month horizon in series 2, and the 7-month horizon in series 3-5. This implies that the coefficient on Hpair_2_4 could be underestimated by 3%, while the coefficients on Hpair_2_6, Hpair_3_12, Hpair_4_18, and Hpair_5_24 could be overestimated by 3%. Given the confidence intervals, however, only for two of these do we suspect that the absence of an order effect would have resulted in a significant coefficient, and only one of those would be consistent with increasing discount rates. The qualitative conclusions we draw here remain the same: we find no support for the claim that discount rates generally

in discount rates as the horizon gets shorter, there is no consistent evidence that they increase. The variation in the rates that we estimate here is consistent with our earlier inference about the presence of small segments of the population that exhibit dynamic inconsistency both in the direction of increasing and decreasing discount rates, and a larger one with a mixed pattern.

We find that the states of nature are generally statistically insignificant, with the possible exception of the variable reflecting beliefs about the present state of the economy compared to the past (*p*-value of 0.08). The only other state to exhibit some effects is the variable representing beliefs about future personal earnings (*p*-value of 0.12). On the whole the states of nature do not have a statistically significant joint effect (*p*-value of 0.57).

We can also use a model such as the one presented in Table 5 to study the effects of demographic characteristics on the tendency to report higher discount rates as the horizon draws near. In this case we substitute the demographic characteristics shown in Table 3 for the horizon-series dummies in Table 5. The only major effect is for the older participants (over 50 years of age): on average, they report discount rates that are 8.9 percentage points lower than younger participants (*p*-value of 0.05). Students have a weakly significant coefficient indicating a discount rate that is 13 percentage points higher than non-students, although the *p*-value is only 0.10. This result cautions that comparable experiments undertaken solely with student subjects (who are also classified as younger) might provide a misleading estimate of the likelihood of dynamic inconsistency in the broader population.

D. Allowing for Sample Selection Effects

Finally, our design allows a check for the possibility that sample selection effects could be biasing inferences about the constancy of discount rates. If we take the sample of 253 in Series 1 as the population for present purposes, the sub-sample of 97 in Series 2 through 5 can be viewed as the result of exogenous and endogenous selection processes. The exogenous processes were the ones

increase as the horizon gets shorter.

we used to invite subjects, described earlier. No attempt was made to identify subjects to recruit for the follow-up experiments based on their initial responses. However, we could not control who would accept our invitation, and therein lies the potential for sample selection.[21]

To check for possible effects of sample selection, we extend the statistical analysis presented in Table 5 to include a formal correction for sample selection. The demographic characteristics summarized in Table 3, at the time of recruitment for later series, were used to explain participation. We also included a binary measure of the experimenter in Series 1 for that subject, binary indicators for whether the subject received income in the two main tasks of Series 1, and three dummy variables indicating the region where they lived at the time of the first interview.[22]

We find *no strong evidence that our estimates of the effects of horizon on elicited discount rates are biased due to sample selection.* The only sample selection effect we see is that the coefficient on the 7 month horizon elicited in Series 3 is no longer significant. Nevertheless, the only instance in which we found an increasing discount rate in Table 5 is also strengthened when we control for sample selection. Again, none of the other 12 paired comparisons approach statistical significance, and the point estimates are not uniformly positive. Thus there is no change in the qualitative conclusions drawn from Table 5.

We also confirm the finding of a significant difference between older and younger participants, although the *p*-value is now 0.07. The difference we saw between students and other subjects appears to be there, but is also weakened to a *p*-value of 0.12. Nevertheless, when we test for the total effect on changes in discount rates from the student status, by allowing the coefficient to be influenced by any other characteristics that might be correlated with student, such as age, the

[21] Strictly speaking this is a case of sample attrition, but the standard methods of sample selection apply immediately in this instance, as noted by Heckman [1979; p.154]. If we were analyzing behavior in each of the series as a panel, rather than the difference between responses in the two series that a given subject participated in, one could use comparable methods developed by Hausman and Wise [1979]. We might also restrict the possibility for sample selection to the sub-sample of 141 of the original 253 that we actually attempted to recruit. Doing so does not change our results in any significant manner; we elect to treat the original 253 as the population from which sample selection biases may have arisen, to provide an additional robustness check that our exogenous selection process had no bias.

[22] We aggregate the 12 counties into three regions: (i) greater Copenhagen area, (ii) rest of Zealand and Funen and (iii) Jutland.

p-value is 0.06. We therefore conclude that using student participants in testing dynamic consistency may not lead to results that generalize to broader populations.

4. Conclusions

We have shown that subjects that are representative of the general population in Denmark do not significantly increase their discount rates as the horizon of an asset gets shorter. This conclusion is important in light of reported findings of hyperbolic discounting elsewhere in tests where subjects are asked for their discount rates over different horizons, but where those rates are elicited at a single point in time. Our panel method generates a more direct way of testing since it does not rely on assumptions of stationarity over time.

Small identifiable segments of the population do appear to suffer from some systematic biases even when there is some front end delay: students. This is consistent with the fact that much of the literature that has documented declining discount rates have relied on laboratory experiments that predominantly use student participants. Our findings point to the need for a continued testing of dynamic inconsistency on broader populations in order to establish more general results.

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	3,075 DKK	5	5.09	A	B
2	3,000 DKK	3,152 DKK	10	10.38	A	B
3	3,000 DKK	3,229 DKK	15	15.87	A	B
4	3,000 DKK	3,308 DKK	20	21.55	A	B
5	3,000 DKK	3,387 DKK	25	27.44	A	B
6	3,000 DKK	3,467 DKK	30	33.55	A	B
7	3,000 DKK	3,548 DKK	35	39.87	A	B
8	3,000 DKK	3,630 DKK	40	46.41	A	B
9	3,000 DKK	3,713 DKK	45	53.18	A	B
10	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
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 1: Elicited Discount Rates by Series and Horizon

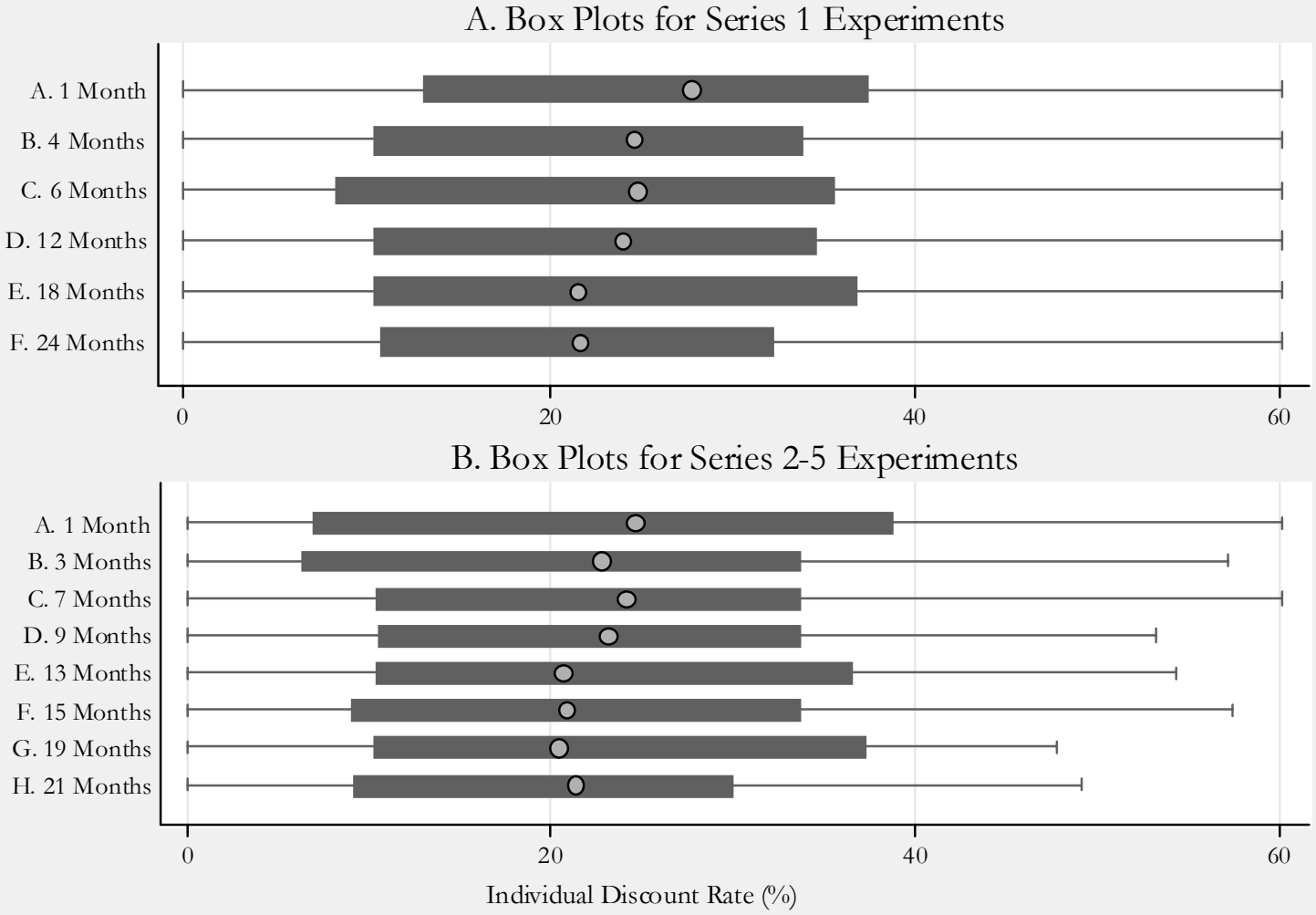

Figure 2: Discount Rates by Horizon

Box Plots of mid-point of interval chosen by subject
Asterisk denotes Series 1 experiments

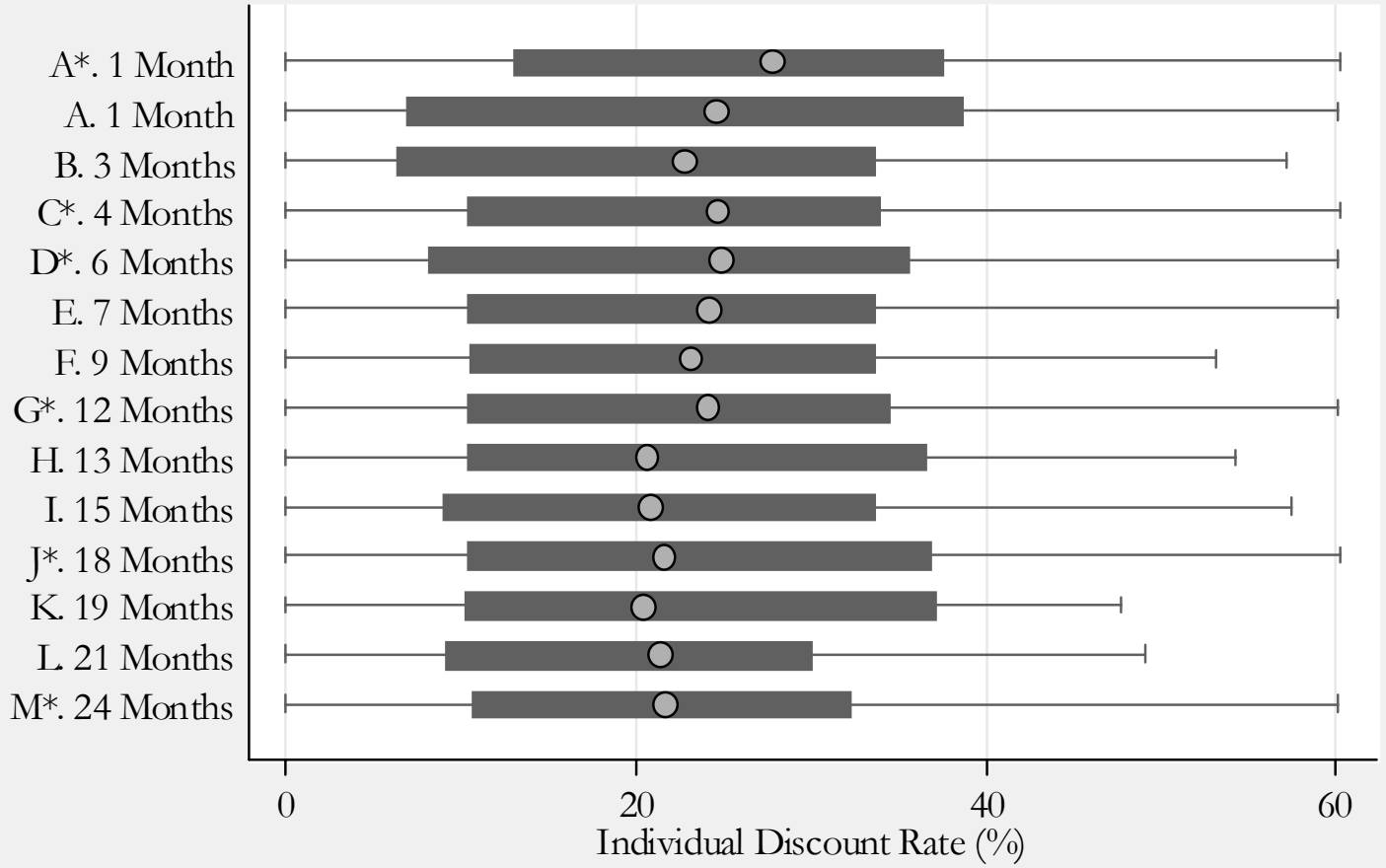

Figure 3: Within-Subject Differences in Elicited Discount Rates for Paired Horizons

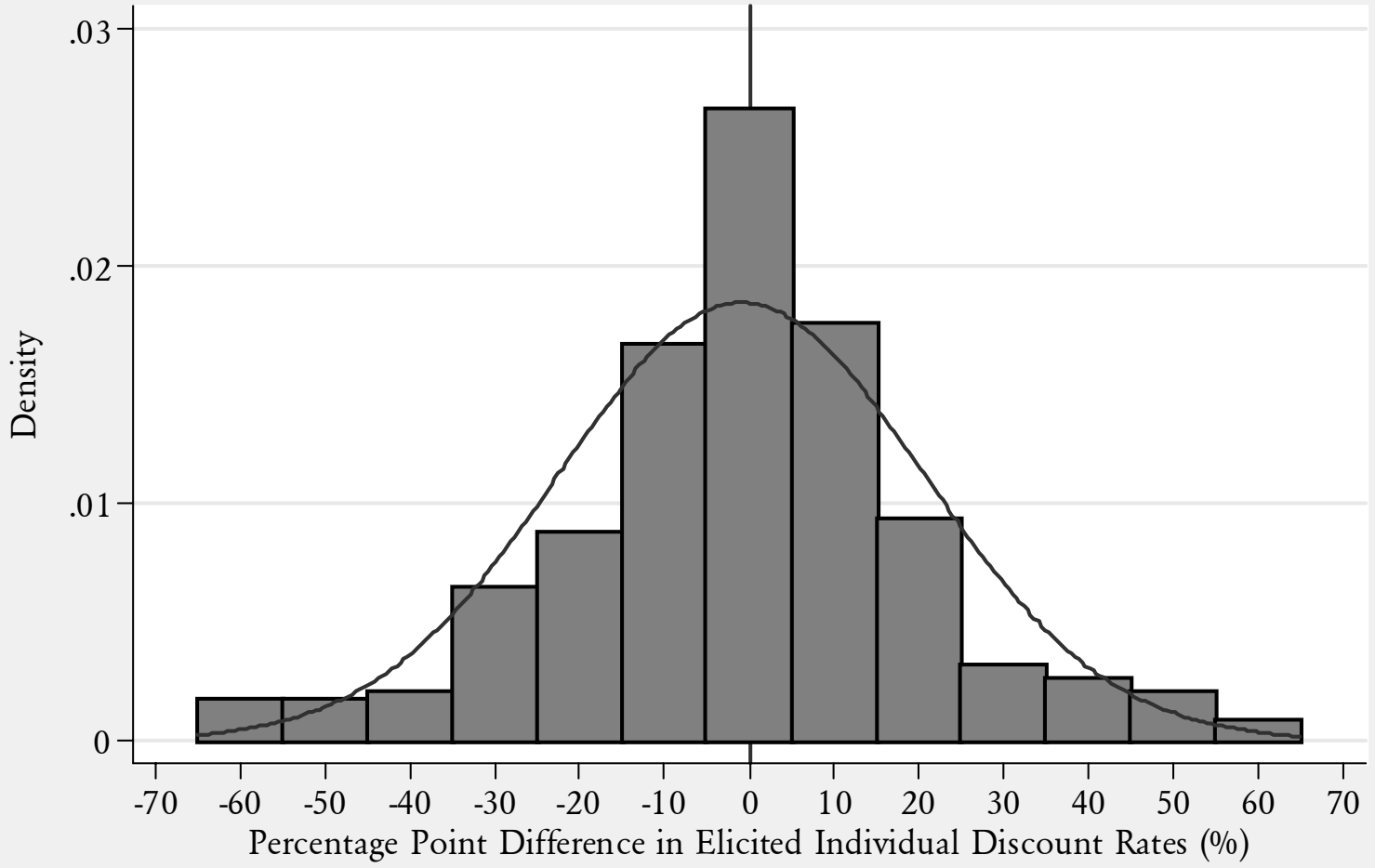

Figure 4: Subjects With Biased Differences in Elicited Discount Rates

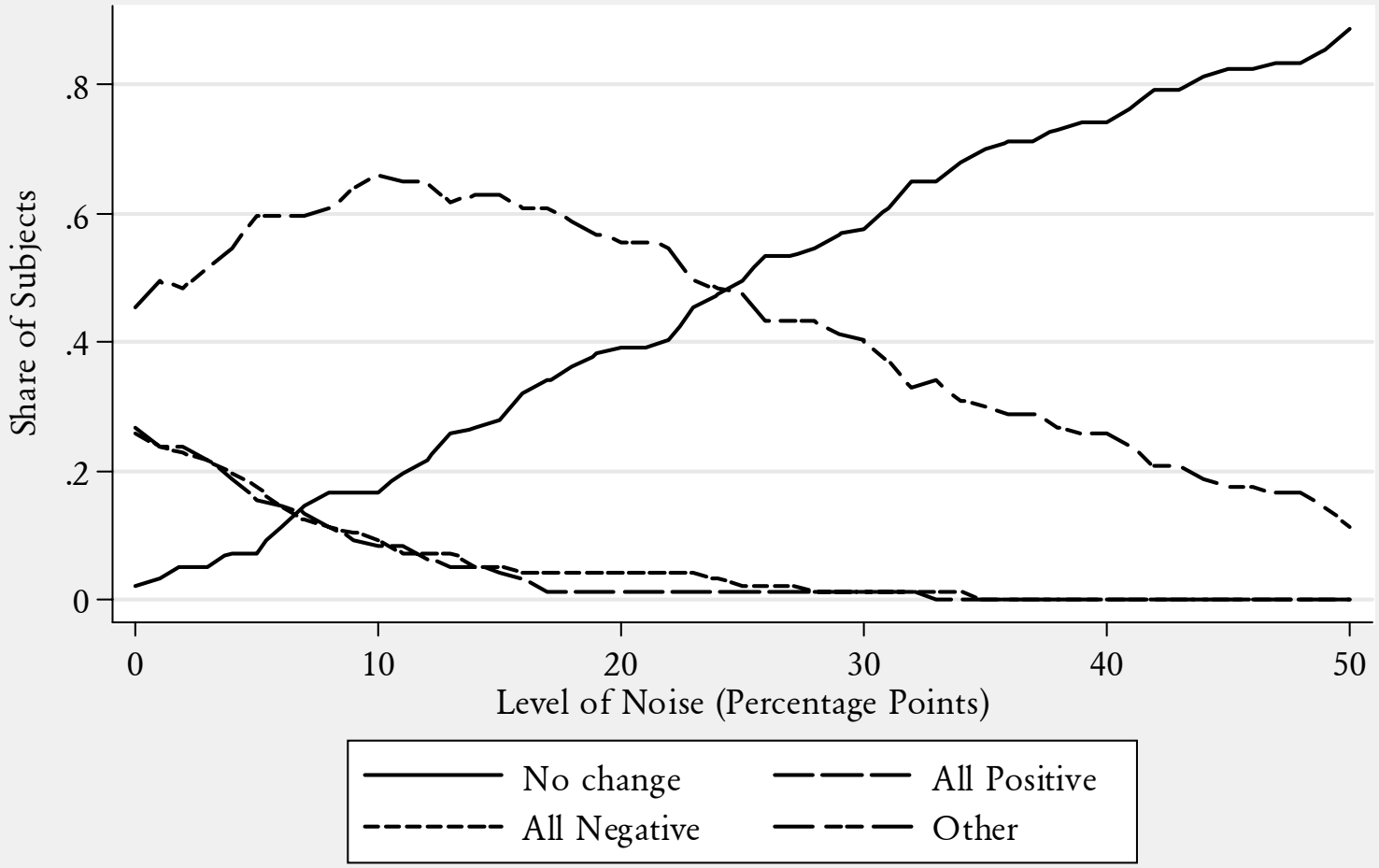

Table 4: Descriptive Statistics for the Changes in States of Nature

N=341, based on 97 subjects

Variable	Description	Mean	Standard Deviation	Min	Max
Ds_fin0	Financial situation compared to the past	0.16	3.18	-12	10
Ds_exp	Future personal expenditures	-0.43	2.60	-7	9
Ds_inc	Future personal earnings	-0.33	2.01	-6	6
Ds_fin1	Future financial situation	0.29	2.77	-6	12
Ds_eco	Economic situation compared to the past	0.49	3.80	-12	12
Ds_emp	Future general employment level	0.55	2.85	-10	7
Ds_int	Future general interest rate level	-1.11	3.58	-12	7

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. "X" months refer to 1 month, 4 months, 6 months, 12 months, 18 months and 24 months.

Table 5: Regression Model of Within-Subject Differences in Elicited Discount Rates

Survey linear regression model of difference in elicited individual discount rates
N=341, based on 97 subjects

Variable	Description	Estimate	Standard Error	*p*-value	Lower 95% Confidence Interval	Upper 95% Confidence Interval
Hpair_2_4	Series 2, 1 month horizon	4.94	4.51	0.28	-4.01	13.89
Hpair_2_6	Series 2, 3 months horizon	-2.39	5.13	0.64	-12.57	7.79
Hpair_2_12	Series 2, 9 months horizon	-3.33	3.35	0.32	-9.98	3.31
Hpair_2_18	Series 2, 15 months horizon	-1.52	4.35	0.73	-10.15	7.11
Hpair_2_24	Series 2, 21 months horizon	-2.53	3.55	0.48	-9.58	4.52
Hpair_3_6	Series 3, 1 month horizon	5.56	5.16	0.28	-4.69	15.80
Hpair_3_12	Series 3, 7 months horizon	-0.23	3.05	0.94	-6.27	5.82
Hpair_3_18	Series 3, 13 months horizon	-9.61	4.61	0.04	-18.76	-0.46
Hpair_3_24	Series 3, 19 months horizon	-4.81	4.09	0.24	-12.93	3.31
Hpair_4_12	Series 4, 1 month horizon	10.45	4.80	0.03	0.93	19.98
Hpair_4_18	Series 4, 7 months horizon	5.07	5.10	0.32	-5.06	15.20
Hpair_4_24	Series 4, 13 months horizon	1.84	4.36	0.67	-6.81	10.50
Hpair_5_18	Series 5, 1 month horizon	2.80	6.79	0.68	-10.68	16.29
Hpair_5_24	Series 5, 7 months horizon	-5.14	3.78	0.18	-12.64	2.36
D_experimenter	D_experimenter	1.13	3.69	0.76	-6.20	8.45
Ds_fin0	Financial situation compared to the past	0.96	0.86	0.27	-0.75	2.66
Ds_exp	Future personal expenditures	-0.21	0.69	0.76	-1.58	1.17
Ds_inc	Future personal earnings	1.20	0.77	0.12	-0.33	2.74
Ds_fin1	Future financial situation	-0.05	0.61	0.93	-1.26	1.15
Ds_eco	Economic situation compared to the past	-0.73	0.41	0.08	-1.55	0.08
Ds_emp	Future general employment level	0.13	0.66	0.84	-1.19	1.45
Ds_int	Future general interest rate level	0.10	0.56	0.85	-1.00	1.21

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