CHANGING DECISION RULES:
Uncovering Behavioral Strategies Using Estimation/Classification (EC)*

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ABSTRACT

We use the Estimation Classification (EC) estimator first introduced and utilised in El-Gamal and Grether (1995) to study the extent to which individual decision making under uncertainty is shaped by the simplicity of application of various heuristics. In particular, we consider the representativeness heuristic, which figured prominently in earlier empirical results. We study two sets of data from two experiments conducted recently at the University of Wisconsin at Madison, where we employed two designs. One of the designs was used in our previous research and makes the representativeness heuristic readily available to the subjects, whereas the other design does not. In one experimental session, we started with the first design and switched to the second, and in the other session the order of the designs was reversed. We find strong evidence that the ease with which subjects can use the representativeness heuristic influences their tendency to use it. This is evidence for a long-held view in the bounded rationality literature that – other things constant – individuals tend to use heuristics which are more readily available to them.

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INTRODUCTION

In previous papers (El-Gamal and Grether (1995), (1996), (1998a,b)) we introduced and applied the Estimation/Classification (EC) estimator and algorithm to experimental data on individual decision making under uncertainty. This estimation procedure allows one to uncover heterogeneity of unknown kinds in panel models. The types of decision rules we uncovered in the different studies – using different data sets and models of behavior – suggested that the experimental design may play a large part in determining the behavioral patterns that we uncover in the data. The experiments studied in this chapter were constructed to study this phenomenon in greater detail in a within-group and between-group framework, keeping other variables constant.

In El-Gamal and Grether (1995) we analyzed data on 257 subjects from four southern California colleges and universities, each of whom made approximately 20 probability updating decisions. It seemed plausible to us that different subjects might follow different strategies in reaching their conclusions, but for reasons of parsimony we did not wish to fit a different model for each subject. The treatment of heterogeneity in panel data has been a major area of interest in econometrics and applied economics for years and there exists a huge literature on the subject. (For excellent introductions, the interested reader should see Hsiao (1986) or Baltagi (1995) and the references cited therein.) However, the standard methods used in that literature such as fixed and random effects models did not seem appropriate for our application. Based upon the structure of the inference problems that the subjects faced we posited a family of 512 different decision rules we felt encompassed all reasonable rules that subjects might use in solving these problems. We also allowed for the possibility that in applying their chosen rules, subjects may occasionally make mistakes. We used the EC estimator to uncover the number of rules that the subjects were using, as well the specific rules being employed, and classified the subjects by the type of rule they were using. In other words we treated the problem of heterogeneity in our data set as a classification or clustering problem.

We found evidence that virtually all of the subjects were using one of two decision rules. The majority of the subjects (156) made decisions most consistent with Bayes's Rule, i.e. textbook probability theory. The second largest group (92) were classified to a rule we called representativeness due to its similarity to the representativeness heuristic introduced by Kahnemann and Tversky (1972). The remaining nine subjects made decisions consistent with what psychologists call conservatism (Edwards (1982)).

In El-Gamal and Grether (1998b) we analyzed the same data, but rather than prescribing a fixed family of rules that the subjects must follow we posited a model for the subjects' subjective log odds and estimated a separate probit models for each of the types. Let y^*_{it} denote the subjective log posterior odds of subject i in period t. Of course, we do not observe y^*_{it}, but rather observe

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$$y_{it} = \begin{cases} 1 & if \ y^*_{it} > 0 \\ 0 & otherwise \end{cases}$$

We then assume that the subjective log posterior odds each subject holds is governed by the (probit) equation:

$$y^*_{it} = \beta_0 + \beta_1 \log prior\ odds + \beta_2 \log likelihood\ ratio + \varepsilon_{it}$$

where ε_{it} is assumed to be distributed i.i.d. *N(0,1)*.

In this model, $\beta_1 = \beta_2$ corresponds to Bayesian behavior, while $\beta_1 < \beta_2$ indicates that the subjects are giving more weight to the likelihood ratio than to the prior odds. This over-weighting of the evidence is a generalization of the representativeness heuristic introduced by Kahnemann and Tversky (1972). Alternatively, reversing the inequality shows that subjects are over weighing the prior odds and corresponds roughly to conservatism (c.f. Edwards (1982)). The results obtained were quite similar to those reported in the earlier paper. For each of the four schools we found that the number of types was either one (two schools), two or three (one of each). Based upon the statistics, 147 of the subjects would be classified as Bayesian, 100 classified as representative with 10 classified as conservative. These are nearly the same totals as before, but we note that the subjects are not necessarily classified in the same way. For example, the nine subjects identified as conservative in the earlier paper were from Occidental College while the ten so classified in the probit analysis were from California State University at Los Angeles.

El-Gamal and Grether (1998b) studied experimental data generated by a different set of subjects faced with multiple updating tasks. In that design, subjects observed one to four samples from a given population, and then the experimenter elicited (subjective posterior) probability judgements after observing each sub-sample. The model we used to analyze the elicited sequential posterior probability responses was a simple generalization of the probit model, with y^*_{it} observed, and with multiple terms replacing the single term for log likelihood ratio.

$$\begin{aligned} y^*_{it} &= \beta_0 + \beta_1 \log prior\ odds + \beta_2 \log likelihood\ \ ratio \\ &+ \beta_3 \log incremental\ likelihood\ ratio + \varepsilon_{it} \end{aligned}$$

The models were estimated using a two sided Tobit procedure with truncation of the observations in which subjects responded with probabilities of zero or unity. The results obtained were quite different from those obtained in the other studies. Rather than being predominantly Bayesian, the subjects were found to be mostly conservative. In addition, most of our estimates in this data set showed recency effects. In other words, the most recent batches of observations received more weight than past observations. We note that in addition to using a multiple updating design, these experiments were conducted with different subject pools, and the inference problems were structured to make the representativeness heuristic unavailable. Thus it is not possible to say what factor or combination of factors caused the different results.

In this paper we present the results of experiments designed to further investigate the differences found between the earlier sets of results. In the new experiments, subjects were presented with two different problems; one very similar to the one used in El-Gamal and Grether (1995), and another for which the representativeness heuristic as defined by Kahnemann and Tversky (1972) does not apply. After 24 repetitions with one problem, subjects were switched to the other problem for another 24 repetitions. Data were collected in two experimental sessions, where the order of the two designs was reversed. The results reported in this paper are a first step in an effort to understand how individuals adapt their decision strategies to changing decision environments.

THE EXPERIMENTS

Eighty-one undergraduates at the University of Wisconsin at Madison were recruited for the experiments. The subjects were recruited from introductory economics classes, and told that they will participate in a decision-making experiment, where they can earn money based on the number of correct decisions they make. To minimize the contamination of the subject pool for the second experimental session, we recruited the subjects for the two sessions at different classes.

The equipment used in the experimental sessions consisted of two identical bingo cages and one 10-sided die. The die was used to determine which of the two cages was used to generate draws. The rules were all of the form: “If the die shows one, two, or three, we shall use Cage A. If it shows four, five, … or ten, we shall use Cage B.” In both experimental session, the rules implied prior probabilities for Cage A of 0.3, 0.4, 0.6 or 0.7. The subjects were not informed of the outcome of the roll of the die, but one subject chosen at the beginning of the experiment monitored the activities of the experimenters to ensure that they followed the design which they explained to the subjects. Once a cage was chosen at random, it was used to generate draws with replacement. The subjects observed the draws, but could not identify the cage since the two cages looked identical.

Under the first design, both cages contained six balls, with Cage A containing four balls labeled “N” and two labeled “G”, and Cage B containing three Ns and three Gs. Under this design, subjects observed in each trial the results of six draws (with replacement) from the cage randomly selected by the roll of the die. The subjects were then asked to indicate which cage they believe generated the data. Note that both cages are likely to produce samples with either three or four Ns; that is, samples that look like – or are representative of – one of the cages.

Under the second design, each cage contained ten balls. Cage A contained four Ns and six Gs, and Cage B contained six Ns and four Gs. Under the second design, the subjects observed in each trial the outcomes of 7 draws with replacement out of the randomly selected cage. Again, the subjects were asked after observing those outcomes for each trial which cage they thought generated the observations. Note that with this configuration it is not possible to observe samples which mimic the population proportions. However, since the sample size is odd there will always be a majority of either Ns or Gs, and the data in each trial will favor one of the two cages.

All subjects were paid a fixed participation fee. In addition three of their decisions were selected randomly at the end of the experiment, and each subject was paid an additional $20 for each correct decision of the three. A decision is correct if the balls were drawn from the cage that the subject picked (as being most likely). The experiments were conducted in two sessions. The sessions differed only in the order in which the two designs were used. The first session began with the 4-2, 3-3 design and switched to the 4-6, 6-4 design in the middle. Forty-one subjects (plus one subject who was chosen to monitor the experimental procedures) participated in the first day and thirty-eight (plus one monitor) in the second day.

METHOD OF DATA ANALYSIS: EC

Let k be the number of types in the population. We do not know k, and we do not know the actual k types, or which subjects belong to which type. For k=1, we estimate the probit parameters $(\beta_0, \beta_1, \beta_2)$ via maximum likelihood. However, since we wish to estimate k, and the k types $(\beta_0, \beta_1, \beta_2)_1, \ldots, (\beta_0, \beta_1, \beta_2)_k$, and the classification of subjects to types, we use the EC algorithm of El-Gamal and Grether (1995, 1996). We refer the reader to the cited papers for technical details of the algorithm, and analysis of the asymptotic behavior of the obtained estimates. Simply stated, the EC-estimator proceeds in two stages:

1. Use the EC algorithm to estimate types $(\beta_0, \beta_1, \beta_2)_1, \ldots, (\beta_0, \beta_1, \beta_2)_k$, for k=1,2,…
2. Calculate an information criterion (IC) for each k, possibly using log prior on the estimated parameters (including k, and the classifications) as a penalty function, thus giving the IC an interpretation as log posterior. Choose the model (k, k-tuple of rules, and classifications), which maximize IC(k).

For any given k, the likelihood function of the data evaluated at $(\beta_0, \beta_1, \beta_2)_1, \ldots, (\beta_0, \beta_1, \beta_2)_k$ can be written thus:

$$F_k = \sum_{k'=1}^{k} \sum_{i=1}^{n} \delta_{ik'} f\left(data_i ; \left(\beta_0, \beta_1, \beta_2\right)_{k'}\right)$$

where $f(data_i, (\beta_0, \beta_1, \beta_2)_{k'})$ is the standard probit log likelihood function for individual i's data evaluated at the parameter vector $(\beta_0, \beta_1, \beta_2)_{k'}$, and δ_{ik} is a 0 or 1 variable indicating whether individual i belongs to group k'. For each k, the EC algorithm maximizes F_k thus:

- For any candidate k-tuple of rules $(\beta_0, \beta_1, \beta_2)_1, \ldots, (\beta_0, \beta_1, \beta_2)_k$:
  - Loop over individuals $i=1,\ldots,n$.
  - For each individual, calculate $f(data_i,, (\beta_0, \beta_1, \beta_2)_{k'})$ for $k'=1,\ldots,k$.
  - Set $\delta_{ik'} = 1$ for the k' which maximizes $f(data_i,, (\beta_0, \beta_1, \beta_2)_{k'})$ over $k'=1,\ldots,k$.
- Return the value of F_k calculated with those $\delta_{ik'}$'s to optimization search routine.
- Let search routine find $(\beta_0, \beta_1, \beta_2)_1, \ldots, (\beta_0, \beta_1, \beta_2)_k$ which maximize F_k .

In El-Gamal and Grether (1996), we prove that if the data generating process satisfies the standard assumptions to obtain consistency and asymptotic normality (CAN) in the case k=1, the CAN property extends to our EC estimator which simultaneously estimates k, the k-tuple of parameters, and the classifications. This result is contingent on the penalty function we choose meeting minimal requirements.

EMPIRICAL RESULTS

Tables 1-4 report the results of conducting our EC analysis for the four experimental data sets described above. Tables 1 and 2 analyze the data from the first day under the two designs, and Tables 3 and 4 analyze the data from the second day. Each table reports for each k=1,2,3 the number of subjects classified to each "decision rule" (summarized by the three parameters), the t-statistic for $(\beta_1-\beta_2)$ and its qualitative significance as Bayesian, Representativeness, or Conservatism, and the likelihood and IC. For the IC's reported in this table, the penalty function was based solely on the prior on k (chosen as $1/2^k$) and the classifications of n subjects to k types (all of the approximately $k^n/k!$ possible configurations given equal a priori weight). In most cases, the IC suggests that we have three types in the population, except for the second part of the second day, where the IC refused to "turn", but for comparison we still limit attention to 3 types. Before we proceed to analyze the output from the multiple probits, we provide a count-based first analysis of the data, taking into account only the number of deviations for each subject from the ideal rules based on a cutoff on the number of N's observed.

A First Analysis of the Results

In discussing the results we shall refer to the design with six balls interchangeably as "the first" design and "the old" design (due to its similarity to the one analyzed in our previous papers) and refer to the other as "the second" or "the new" design. If we classify subjects as Bayesians, representative heuristic users, or conservatives, by counting the number of deviations each subject makes from the prescriptions of each rule, the two designs produced similar results. In the first day, using the old design, a simple count of deviations can classify 22 subjects as Bayesians, 13 as representative types, and 14 as conservatives. For the new design the corresponding numbers are 22, 10 and 11 respectively. Note that the count numbers add to more than 41 because of ties. During the second day the old design had 19 Bayesians, 15 representative types and 4 conservatives (no ties), while the old design gave 33, 14 and 2, respectively.

Note that virtually all of the changes in count classifications between the two designs are due to ties between representativeness and Bayes's Rule. However, the count analysis shows that the new design fit significantly better than the old design in both days. For example the Chi-square statistics for testing equality of error rates for Bayes's Rule in the two designs are 11.7 and 14.3 (one degree of freedom). The sole exception is that conservatism fits equally well (or badly) for both designs on the second day. It is also true that the error rates were lower in general on the second day. Classifying each subject to the rule which best fits their data, and comparing the results, give Chi-squares of 11.1 for the old design and 4.9 for the new. If instead we classify subjects to Bayes's Rule or to representativeness (the two best overall) the figures are 12.9 and 6.7, confirming that the better fits on the second day are significant.

Probit Analysis Results

We can represent the representativeness heuristic / Bayes’s rule / conservatism trichotomy on a Real line, with representativeness at the extreme right and conservatism at the extreme left-hand side. Thus, movements to the right would indicate giving more weight to the data, and movement to the left would indicate giving more weight to the prior.

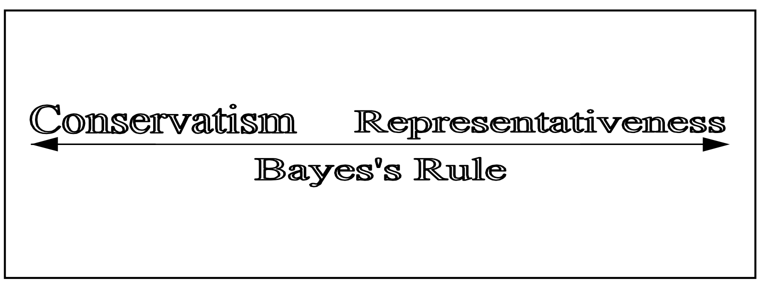

Figure 1. The representativeness to conservatism spectrum

Using the results in Tables 1 and 2, we can observe the groups to which each individual was classified under the first and second designs. We can then assign a score of +1 to each individual qualitatively moving in the direction of representativeness (i.e. changing from conservatism to Bayes, or from Bayes to representativeness), and a score of -1 to each subject moving in the opposite direction. Adding those scores (with subjects qualitatively remaining the same being assigned a score of zero), we can measure the qualitative effect on the subjects in the first day of the design change. The result is summarised in the following figure:

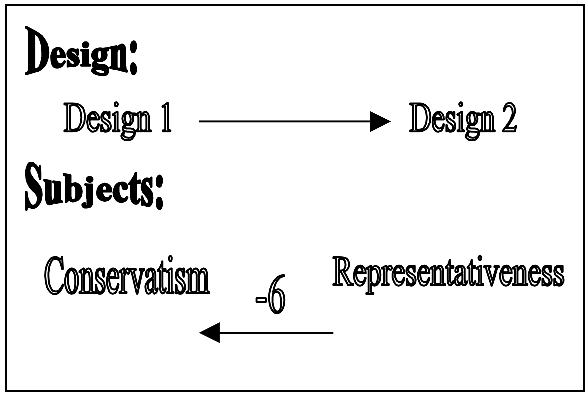

Figure 2. Day 1: Qualitative design and classification change

For the second day, using the results from Tables 3 and 4, we can construct a similar aggregate measure of the qualitative movement of the subjects, and the result is summarised in the following figure:

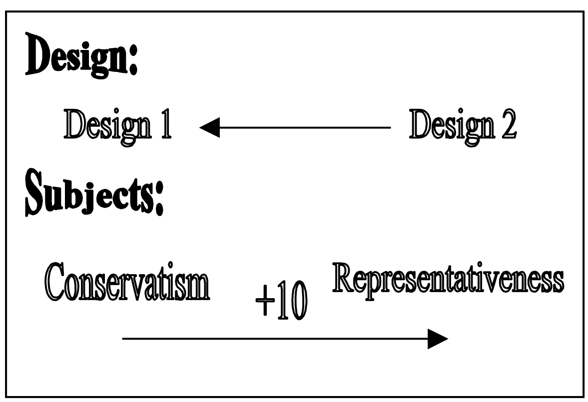

Figure 3. Day 2: Qualitative design and classification change

Figures 2 and 3 reveal a strong trend for Design 1, which makes the representativeness heuristic more readily available to the subjects, to induce movement in the direction of representativeness, and vice versa.

This pattern is even more pronounced when we look at the estimates for k=2 (which is selected for most cases by our IC if we incorporate priors on the parameter estimates in the penalty function). For that case, we see on day 1 under design 1 that more than half the subjects are classified to the representativeness heuristic; whereas not a single subject is classified to that rule after we switch to design 2. On day 2, we see the same pattern in reverse sequence, with no subjects using the representativeness heuristic being detected under design 2 with k=2, but when we switch to design 1, almost half the subjects are classified to the representativeness heuristic.

As in El-Gamal and Grether (1995,1996), we see that ignoring heterogeneity can be quite deceptive, since in all four tables with k=1 we fail to reject the hypothesis that all subjects are Bayesian. This reflects the sense in which the question "are people Bayesian" asked in the title of El-Gamal and Grether (1995) was shown to be ill-posed: if we assume that all the subjects are the same, then we fail to reject that they are "Bayesian", but if we allow for heterogeneity (k>1) we strongly reject that hypothesis for three out of the four cases studied here. The conclusions we reach depend crucially on the degree of heterogeneity we allow. It is, therefore, comforting that – leaving aside the penalty function in our IC temporarily – the same pattern is detected for both k=2 and k=3. This pattern is also consistent with the guess which motivated our experimental design: that the ease with which subjects can use heuristics is one of the main determinants of whether or not they use them.

CONCLUDING REMARKS

It seems obvious that the strategies that individuals adopt depend upon the problems they face. Buying a house or a car is a more consequential decision than selecting a brand of cola to consume with lunch. We would not expect to find people exhibiting the same behavior in these different situations. In addition we know that problems with identical structure and importance will be treated quite differently depending upon the context in which they are presented (Wagenaar, et al (1988)). The "framing" of a decision problem in terms of – for example – gains versus losses can change the way people evaluate alternatives (Tversky and Kahnemann(1986)).

The results presented in this paper represent one step towards understanding how the rules or strategies that people adopt depend upon the detailed structure of the problems. All the problems subjects faced in these experiments involved observing draws from one of two known populations with public knowledge priors on the populations. In all cases the populations consisted of known proportions of two types of objects (balls labeled with one of two letters). The problems differed only in the sample sizes and in the proportions of the types.

We analyzed the data from our experiments using the EC estimator and algorithm first introduced in El-Gamal and Grether (1995). This estimator allows for subjects to be using different strategies and allows us to estimate the number of rules, which rules are being used and the number of subjects using each of the estimated rules. We use an information criterion (penalized likelihood) to determine the number of rules. The rules are estimated by maximum likelihood. Allowing for heterogeneity in subjects' behavior makes a substantive difference in the conclusions drawn.

Assuming all subjects are using the same strategy yields results consistent with Bayes's Rule. However, allowing for more than one strategy leads to the conclusion that some subjects' actions are consistent with using a generalization of the representativeness heuristic, especially when it is easily available.

We find evidence that when the samples were structured so that the representativeness heuristic was easily and frequently available subjects tended to use it and deviate from Bayes's Rule. When the problem was changed so that samples could not exactly replicate the populations from which they were drawn, subjects were more likely to exhibit behavior consistent with Bayes's Rule. The order of presentation of the problems did not seem to affect this pattern. Clearly, more research is needed to understand how the structure of decision problems changes the choices of decision strategies. Our results show that it is extremely important to allow for heterogeneity of behavior when analyzing decision-making data at the individual level.

RESULTS – DAY 1

Table 1. First day, first design (representativeness readily available)

k	*N*	T	Const.	Ln(PO) [β_1]	ln(LR) [β_2]	t(β_1-β_2)	Rule	Mean lnlik	IC
1	41	24	0.040 (0.058)	1.626 (0.111)	1.579 (0.093)	0.571	Bay	-.412	-406.1
2	22	24	0.129 (0.083)	1.922 (0.213)	2.791 (0.220)	-5.215	Rep	-.364	-288.3
	19	24	0.057 (0.072)	1.651 (0.148)	1.048 (0.114)	4.940	Con		
3	**10**	24	-.106 (0.091)	1.028 (0.176)	1.264 (0.150)	1.021	**Bay**	-.348	**-387.8**
	18	24	0.293 (0.105)	2.809 (0.400)	3.681 (0.402)	-4.360	**Rep**		
	13	24	0.121 (0.098)	2.097 (0.210)	1.082 (0.154)	6.086	**Con**		
4								-.337	-388.0

Table 2. First day, second design (representativeness **not** readily available)

k	*N*	T	Const.	ln(PO) [β_1]	ln(LR) [β_2]	t(β_1-β_2)	Rule	Mean lnlik	IC
1	41	23	0.196 (0.056)	1.302 (0.088)	0.969 (0.638)	0.519	Bay	-.363	-343.0
2	27	23	0.321 (0.099)	2.555 (0.274)	2.283 (0.207)	1.690	Bay	-.296	-308.2
	14	23	0.034 (0.084)	0.923 (0.123)	0.445 (0.076)	3.371	Con		
3	**20**	23	0.487 (0.139)	2.249 (0.297)	2.724 (0.275)	-2.014	**B/R**	-.265	**-295.2**
	11	23	0.066 (0.092)	0.657 (0.133)	0.463 (0.084)	1.233	**Bay**		
	10	23	0.141 (0.161)	3.485 (0.506)	1.449 (0.285)	6.025	**Con**		
4								-.337	-298.5

RESULTS – DAY 2

Table 3. Second day, first design (representativeness **not** readily available)

k	*n*	T	Const.	ln(PO) [β_1]	ln(LR) [β_2]	t(β_1-β_2)	Rule	Mean lnlik	IC
1	38	24	-0.088 (0.066)	1.415 (0.109)	1.459 (0.091)	-0.420	Bay	-.294	-268.8
2	26	24	-0.118 (0.110)	2.308 (0.216)	2.448 (0.197)	-0.914	Bay	-.250	-255.0
	12	24	-0.155 (0.099)	0.937 (0.146)	0.848 (0.113)	0.533	Bay		
3	**18**	24	-0.115 (0.096)	1.026 (0.153)	1.763 (0.149)	-4.283	**Rep**	-.227	**-249.1**
	13	24	0.290 (0.247)	4.681 (0.837)	3.414 (0.561)	3.009	**Con**		
	7	24	-0.287 (0.139)	1.344 (0.207)	0.703 (0.149)	2.894	**Con**		
4								-.218	-251.8

Table 4. Second day, second design (representativeness readily available)

k	*n*	T	Const.	ln(PO) [β_1]	ln(LR) [β_2]	t(β_1-β_2)	Rule	Mean lnlik	IC
1	38	24	0.008 (0.057)	1.408 (0.106)	1.559 (0.102)	-1.312	Bay	-.350	-319.9
2	17	24	-0.241 (0.080)	0.738 (0.132)	1.436 (0.130)	-4.177	Rep	-.289	-290.6
	21	24	0.475) (0.107)	3.401 (0.353)	2.677 (0.279)	3.434	Con		
3	**15**	24	0.784 (0.15)	4.050 (0.565)	3.845 (0.498)	0.664	**Bay**	-.260	-279.2
	6	24	-0.200 (0.203)	4.058 (0.722)	1.738 (0.385)	3.796	**Con**		
	17	24	-0.191 (0.080)	0.667 (0.133)	-1.544 (0.139)	-4.827	**Rep**		
4								-.241	-272.8

(Note: the last design (second session, second design) did not turn at *k*=3, but for comparison, we restrict attention to this case**)**

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