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Are Groups More (or Less) Consistent Than Individuals?*

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Abstract

There is now overwhelming experimental evidence that individuals systematically violate the axioms of Expected Utility theory. In reality, however, many economic decisions are taken by, or on behalf of, groups whose members have a joint stake in those decisions. This paper reports on an experiment in which pairs of individuals are tested for Common-Ratio inconsistencies. We find that the agreed choices of subject-pairs follow a pattern of inconsistency very close to that of individuals' choices. We also look for evidence that group participation increases the consistency of the individuals themselves. With one solitary exception, we find none.

Key words: Expected Utility theory, Common-Ratio Effect, experiments, groups

JEL Classification: D81, C91, C92

1. Introduction

The experimental evidence on the violation of Expected Utility (EU) theory, by individuals in properly motivated experiments, is now overwhelming.[1] Of course, there is always room for debate about whether such experimental results are representative of behaviour in real economic settings. The central issue here is the extent to which experimental conditions reproduce the essential features of the real environment in which agents operate. But one related issue that is largely overlooked, in the context of these results, concerns the agents themselves. In reality, many economic decisions are taken by, or at least on behalf of, groups rather than isolated individuals.

This paper reports on an experimental investigation into collective decision-making under uncertainty. We asked pairs of individuals to agree choices between financial prospects in which they had a joint stake. Our primary aim was to see whether, in experimental conditions, groups violate EU theory in the same manner, and to the same extent, as individuals appear to do.

But we also had a secondary, parallel aim. One obvious way in which groups differ from individuals is that group members are able, and indeed have an incentive, to argue and discuss with each other. Whatever influence this may have on the group's agreed choices, there are some grounds for supposing that it might increase the EU-consistency of the individual members concerned. Our experiment provided an opportunity of testing this ideas as well.

The specific form of EU-violation which we examined was the Common-Ratio Effect. In Section 2 we define and explain this. Section 3 then elaborates our main empirical concerns, and discusses some of the conceptual background. The experiment itself is detailed in Section 4, and the results examined in Section 5. Section 6 provides a summary of the results, and some concluding observations.

2. The Common-Ratio Effect

The Common-Ratio Effect is a simple form of violation of EU theory. A typical construction involves a positive scalar $\alpha < 1$, three monetary (£) prizes with values X > Y > Z, and m distinct non-zero probability values $p_1 > p_2 > ,\ldots, > p_m$. These define a set of m prospect-pairs {**R*i***, **S*i***}, each pair corresponding to some p_i, and comprising one "Risky" and one "Safe" prospect:

R*i* £X with probability αp_i; £Z otherwise
S*i* £Y with probability p_i; £Z otherwise

By construction, α is the common ratio of the winning probabilities in each prospect-pair. We describe as a *CR-set* any set of prospect-pairs {{**R1**,**S1**}, …,(**R*m***,**S*m***}} with this structure. In our experiment each CR-set comprised three pairs ($m = 3$), and is therefore called a *CR-triple*.

The standard EU axioms require that, within a given CR-set, the preference between each prospect-pair be the same. That is, **R1** is preferred to **S1** only if **R*i*** is preferred to **S*i*** for every pair {**R*i***,**S*i***} in that CR-set, and likewise **S1** is preferred to **R1** only if **S*i*** is preferred to **R*i***.

Firstly, the Reduction axiom implies that {**R*i***,**S*i***} may be equivalently described as:

R*i* **R1** with probability p_i/p_1; £Z otherwise
S*i* **S1** with probability p_i/p_1; £Z otherwise

These two (compound) prospects differ only in the first outcome, which occurs in each case with probability p_i/p_1. So, secondly, the Independence axiom requires that the preference over these two prospects {**R*i***,**S*i***} correspond to that over {**R1**,**S1**}.

Nevertheless, in experimental conditions individuals are often found to have preferences which differ across pairs in a given CR-set. Specifically, there is an observed tendency to prefer $\mathbf{S}i$ for high values of p_i and $\mathbf{R}i$ for low values of p_i. This is known as the *Common-Ratio Effect*.

3. Two empirical questions

In this study, we are interested in two broad empirical questions:

1 How does the EU-consistency of groups compare with that of individuals?
2 Do individuals learn, through group interaction, to be more EU-consistent?

Economic theory customarily classifies the main decision-making agents as *households* and *firms*. In reality, most households and firms are not single individuals but groups of people with a joint stake in economic decisions. This is in addition to groups, such as investment or gambling syndicates, formed explicitly to undertake uncertain financial ventures. Although groups may delegate decision-making to individual representatives, their choices must ultimately be agreed, in some way, by group members. So it is of empirical interest to know whether choices agreed by groups are more, or less, EU-consistent than are those made by individuals.

This is also a theoretical question, of course. In theory, the simplest situation is where group members are individually EU-consistent, and have identical preferences over the prospects in which they share a joint stake. Then, so long as their agreed choices are efficient, these too will conform to EU theory. But group members, however EU-consistent as individuals, will in general have some divergent preferences. Agreement will then require compromises by individual members, and group choices will depend on the bargaining principles by which such compromises are reached.

Economists commonly view such principles in one of two ways.[2] The *strategic* approach is to analyse the bargaining process as a non-cooperative game. On this view, there is no reason to suppose that agreed decisions will be in any way collectively rational. More promising, in this sense, is the *axiomatic* approach of Nash and others, which explicitly applies axioms of rationality to group choices. But typically these are just the basic axioms (e.g., Transitivity) of sure choices. Indeed, we know from Harsanyi (1955) that the only arbitration principle guaranteed to produce fully EU-consistent choices is sum-utilitarianism, i.e., maximises a linear function of individual vNM utilities.

In the context of financial prospects in which group members have a joint stake, there is a specific reason why group choices may be EU-inconsistent. This relates to the (re-)allocation of joint prizes between group members, and is explained in Section 4.

So there seems to be a clear theoretical presumption that groups will be EU-inconsistent. But then empirically, of course, so too are individuals. Given this, the question remains as to whether groups will be more, or less, inconsistent than are individuals. Theory does not seem to help here, most obviously because it does not give any guidance as to the degree of group inconsistency, but also because it generally pre-supposes individuals to be consistent in the first place. Almost all theoretical work on bargaining, whether strategic or axiomatic, assumes each group member to have a cardinal utility function defined over final (sure) outcomes, and Harsanyi's theorem explicitly assumes individuals to be EU-consistent. We therefore view this as an open empirical question, of which, we distinguish two aspects. The first is the relative incidence of EO-inconsistency, as between groups and individuals. The second is the type of inconsistency involved.

In order to investigate this question, we constructed an experiment in which pairs of subjects were asked to agree choices from CR-triples, given a joint stake in the prospective prizes. Of courses, for a controlled comparison of groups and individuals, we had separately to measure the EU-consistency of our subjects as individuals. One potential ambiguity here is that the group encounter itself may have some influence on members' individual preferences. Indeed, it is not implausible that, through group discussion, members might become more EU-consistent as individuals.

The idea that people might learn to be more EU-consistent is sometimes associated with Savage, following his remarks on the Allais paradox (Savage, 1972, pp. 101–104). Savage was here defending EU theory on normative grounds. But since his position entails that EU-violations are errors of some kind, it leads naturally to a positive hypothesis that individuals can recognise, and rectify, any such errors.

Even if true, this leaves open the question of what conditions might facilitate this. Discussion, however, would seem to be a promising candidate, especially where the individuals concerned each have a mutual interest in its resolution, as in our experiment. The most obvious scenario involves a teacher/learner relationship, where one partner who is already EU-consistent persuades the other, possibly along the lines of the argument in Section 2. An alternative scenario has both partners starting from a position of EU-inconsistency, but then discovering their mutual error through constructive discussion. Perhaps this would be most likely, if at all, in cases where their individual preferences are (initially) in conflict, this being the trigger for discussion.

We decided, therefore, to elicit our subjects' individual preferences both before and after their group encounters. This would give us not only two standards of comparison for the group choices, but also information relevant to the Savage hypothesis. Evidence of increased EU-consistency would provide suggestive support for the hypothesis. Of course, the lack of such evidence would not falsify it; in this sense our experiment did not constitute a scientific test.

So the experiment comprised three consecutive stages, in each of which subjects were given the same set of choice problems. It might be thought that this repetition could itself induce EU-learning, independently of any group discussion. It is certainly plausible that repetition can induce adaptive learning, where the experience of (suboptimal) outcomes prompts subjects to modify their behaviour. Indeed, there have been many experimental investigations of this idea.[3] But, as described in the following section, our experiment provided no outcomes until all choices had been completed. So adaptive learning of this type could not have occurred.[4] Of course, it may be that repeated experience merely of the choice task, regardless of outcome, provides some opportunity for learning. This idea seems rather more speculative, but in any case the above 'model' of learning through discussion provides a rudimentary framework for the separate measurement of any such effects. We return to this in Section 5.

4. The experiment

Our subjects registered choices from four CR-triples, giving a total of 12 prospect-pairs in all. Following the notation given in section 2, let $\{\mathbf{R}i_t, \mathbf{S}i_t\}$ denote the ith prospect-pair (i = 1, 2, 3) of triple t (t = 1, 2, 3, 4).

The triples all had in common the probability parameters:

$$\alpha = 0.5;\ p_1 = 1,\ p_2 = 0.5,\ p_3 = 0.2$$

and were distinguished only by their prospective prizes, as shown in Table 1.

The final column of Table 1 gives the ratio (ρ) of the expected monetary values of $\mathbf{R}i_t$ and $\mathbf{S}i_t$ in a given triple. It is a measure of the attractiveness of $\mathbf{R}i_t$ relative to the corresponding $\mathbf{S}i_t$. Of course, it is not the definitive measure; a risk-averse individual may nevertheless prefer $\mathbf{S}i_t$ to $\mathbf{R}i_t$ in any of these triples, and will do so in the case of triple 1. But (ρ) nevertheless provides a convenient and natural ordering of the four triples.

The experiment consisted of three consecutive stages. At each stage, the twelve prospect-pairs were given to each subject, each time in an independently randomised order. Stage 2 differed from stages 1 and 3 in that (immediately prior to it) the subjects were paired-up, at random. Each partnership was given the twelve prospect-pairs, as above except that the prizes were all doubled, and asked to agree a choice from each.

Table 1. Prospective (£) prizes for the four triples

	X	Y	Z	ρ
triple 1	30	15	0	1
triple 2	35	15	0	1.17
triple 3	30	12	0	1.25
triple 4	35	12	5	1.67

At stage 2, therefore, each prospect-pair took the form:

$\mathbf{R}i_t$ £2X with probability αp_i; £2Z otherwise
$\mathbf{S}i_t$ £2Y with probability p_i; £2Z otherwise

If these prizes were divided equally, then for each partner the prospect-pairs would correspond exactly to those in stages 1 and 3. However, we did not attempt to impose any specific division of the prizes, instead leaving this for the partners to decide for themselves. In fact, we required them explicitly to register not only their agreed choice of prospect, from each prospect-pair, but also their agreed division of its prospective prizes.

To illustrate why we did this, consider triple 2. Given an equal division of prizes, the second prospect-pair of this triple can be depicted as:

$\mathbf{R2_2}$	J	K
0.25	£35	£35
0.75	£0	£0
v	13.65	3.03

$\mathbf{S2_2}$	J	K
0.5	£15	£15
0.5	£0	£0
v	13.03	3.07

where the partners are denoted {J,K}, and their expected utility values (v) are based on the illustrative vNM utility functions:

$$u_j = (10 + x_j)^{0.9} \qquad u_k = (10 + x_k)^{0.4}$$

In this example, K is the more risk-averse of the two individuals, as reflected in the fact that he prefers the "safe" prospect $\mathbf{S2_2}$ while J prefer $\mathbf{R2_2}$.

However, J might be persuaded to agree to $\mathbf{S2_2}$, given suitable accompanying side-payments. For example, suppose that K agrees to pay J £6 in the event that they win, and J to pay K £3.50 otherwise. This, in effect, gives them the joint prospect:

$\mathbf{S2'_2}$	J	K
0.5	£21	£9
0.5	−£3.5	£3.5
v	13.69	3.04

This is a prospect which, *ex ante*, Pareto-dominates $\mathbf{R2_2}$. So, although she prefers $\mathbf{R2_2}$ to $\mathbf{S2_2}$, J might nevertheless agree to the latter, given contingent side-payments of this type.

But now consider the third prospect-pair {$\mathbf{R3}_2$,$\mathbf{S3}_2$}. Evidently, the correspondingly re-allocated version of $\mathbf{S3}_2$ does *not* Pareto-dominate $\mathbf{R3}_2$:

$\mathbf{R3}_2$	J	K
0.1	£35	£35
0.9	£0	£0
v	10.22	2.72

$\mathbf{S3'}_2$	J	K
0.2	£21	£9
0.8	−£3.5	£3.5
v	8.71	2.92

This illustrates an important proposition: the structure of Pareto-dominance relations between correspondingly-allocated joint prospects is not identical across different prospect-pairs within a given CR-set. It follows that if partners are able freely to (re-)allocate prospective prizes, then their choices among joint prospects may appear as inconsistent. Thus, the agreed choice of $\mathbf{S2}_2$ from {$\mathbf{R2}_2$,$\mathbf{S2}_2$}, but of $\mathbf{R3}_2$ from {$\mathbf{R3}_2$,$\mathbf{S3}_2$}, could actually reflect the Pareto-dominance of $\mathbf{S2'}_2$ over $\mathbf{R2}_2$, there being no corresponding Pareto-dominance of $\mathbf{S3'}_2$ over $\mathbf{R3}_2$. Let us call this an *allocation effect*.[5]

Since we felt unable to prevent covert side-payments, we decided instead to ask partnerships explicitly to record their agreement on how their prizes were to be divided. This would at least give us some information on the extent of any allocation effects.

There was another reason for allowing partners freely to distribute the prizes between themselves. The situation thereby corresponds more closely to that facing groups in the real world, where there is no externally-imposed division of financial prospects in which members have a joint stake.

The experiment proceeded as follows. On recruitment, each subject received a copy of the instructions (see Appendix) together with a booklet containing answer forms for one set of 12 prospect-pairs. In each booklet both the order of the prospect-pairs, and also the (left/right) order of the two prospects in each pair, were individually randomised. Subjects completed these (stage 1) booklets unsupervised, and in their own time, before reporting to an appointed session for stages 2 and 3. Here, having first submitted their completed booklets, subjects were paired-up at random, by drawing coloured counters from an opaque bag.

Each partnership was given a new booklet for stage 2, containing randomised answer forms for the 12 joint prospect-pairs. The session took place in a large hall, within which partnerships were dispersed so that they could each discuss their choices in privacy. When completed, they submitted the booklet to the experimenter, who issued each partner with a new (stage 3) booklet, containing randomised answer forms for the 12 individual prospect-pairs. Subjects then completed and submitted these booklets, again in privacy.

The payment mechanism, specified in the advance instructions, was as follows. Over the three stages of the experiment, each subject was presented with 36 prospect-pairs, either as an individual (stages 1 and 3) or in a partnership (stage 2).

On completion, one of these 36 prospect-pairs was selected at random, independently for each partnership, for playing-out. Both the selection of the prospect-pair, and the playing-out of the chosen prospect, were done by the subject(s) concerned, again by drawing counters from a bag.

If the selected prospect-pair was from stage 1 or 3, then each partner had his chosen prospect independently played out, his payment then being the corresponding prize. If it was from stage 2, then the jointly chosen prospect was played out as a single event, each partner then receiving his agreed division of the joint prize. Had the partnership failed to register an agreed choice (or prize division) from the selected prospect-pair, then each partner would be paid £Z, the lowest prize corresponding to that prospect-pair.

Likewise, of course, an individual making choices at stage 1 or 3 might also have been curious to know the default outcome, i.e., the consequence of failing to register a choice from any given prospect-pair. Unlike for partnerships, however, we would not expect this knowledge to have any bearing on his choice (unless he prefers the default outcome to each prospect in that pair). So, following convention, we did not specify such defaults for stages 1 and 3. Neither did we specify time limits on registering choices at stages 2 and 3. As anticipated, this did not appear to cause any problems; all individuals and partnerships completed their choices in good time.

5. The results

Subjects were undergraduate and graduate students at York University, being largely but not exclusively economists. There were 46 individual subjects, and thus 23 partnerships. So there was a total of 276 responses (choices) at stage 2, and 552 at each of stages 1 and 3.

Let us look first at individuals' choices. Table 2 records the number of individuals who gave EU-consistent responses in n of the four triples, at each of stages 1

Table 2. Number of individuals EU-consistent in n triples

		stage 3					
l	n	0	1	2	3	4	total
stage 1	0	4	2	0	1	0	7
	1	2	9	2	2	0	15
	2	1	3	1	1	0	6
	3	0	5	4	5	1	15
	4	0	2	0	0	1	3
	total	7	21	7	9	2	46

and 3. Only one individual, whom we shall call Leonard, was fully EU-consistent ($n = 4$) at each stage. Leonard was one of only seven individuals who were consistent in at least three triples at each stage.

As measured by n, the overall level of EU-consistency actually fell between stages 1 and 3. There were 9 individuals whose responses improved (above the diagonal in Table 2), but 17 whose responses deteriorated (below the diagonal). The median individual was consistent in two triples at stage 1, but in only one at stage 3. The corresponding mean values were, respectively, 1.83 and 1.52 triples. As a proportion of full-consistency ($n = 4$), this mean score was 0.46 at stage 1, and 0.38 at stage 3.

This overall deterioration in EU-consistency might conceal some instances of improvement through group interaction. Following the discussion in Section 3, let us identify as potential teachers those seven individuals scoring $n \geq 3$ throughout, of whom Leonard was uniquely the best-qualified. Sure enough, his partner was among the improvers, registering one EU-consistent triple at stage 1, and three at stage 3. However, none of the other six potential teachers had partners who improved. Alternatively, we can look for common improvement in partnerships without a teacher. There was none. In fact, in none of our 23 partnerships did both partners improve. So Leonard (or rather his partner) is our sole piece of evidence in support of the Savage hypothesis.

Our primary empirical question concerns the EU-consistency of the partnerships themselves. Before examining this we should report on one very clear result. As discussed in Section 4, the allocation effect provides a possible source of EU-inconsistency for partnerships, and in an attempt to monitor this we asked them to record their agreed division of joint prizes. Of the 23 partnerships in our experiment, all but one agreed to divide all prizes equally. Furthermore, the pattern of this one partnership's prospect choices was very typical. So we can conclude that allocation effects generally played no part in our partnerships' choices.

The prevalence of equal-division agreements may be somewhat surprising. Partners seemed to be overlooking even the opportunity of efficiency gains available through differential risk-sharing. This was not, of itself, a direct concern of the experiment. But it does call for some comment, which we offer in the concluding section.

As regards their prospect choices, only one of the partnerships was fully EU-consistent ($n = 4$). This was Leonard and his partner who, interestingly, agreed choices which coincided exactly with Leonard's own. Throughout the experiment Leonard, accompanied or otherwise, chose $\mathbf{S}\boldsymbol{i}_t$ from each prospect-pair in triples 1–3, and $\mathbf{R}\boldsymbol{i}_t$ in triple 4. We should note in passing that his was not the unequal-division partnership.

Of the remaining 22 partnerships, 13 registered $n = 1$, which was therefore the median value. The mean score was 1.48 triples, which as a proportion of full-consistency is 0.37. So the average incidence of EU-consistency was remarkably similar to that for individuals at stage 3.

We found, therefore, that groups and individuals were closely comparable in the extent of their EU-inconsistency. But, as suggested in Section 3, we might also ask whether the type of inconsistency differs between groups and individuals.

For a given triple t, let **RRS** (for example) denote a respondent's choice of $\mathbf{R1}_t$, $\mathbf{R2}_t$, and $\mathbf{S3}_t$. There are eight possible response patterns, which may be categorised as follows:

EU: either **SSS** or **RRR**
CR: either **SSR** or **SRR** (the Common-Ratio Effect)
other: any of {**SRS, RSS, RSR, RRS**}

Table 3 records the proportion of responses falling into each of these three categories, by triple and in total. The breakdown by triple is informative. Firstly, the incidence of EU-consistency, at all stages, was markedly greater in triple 4 than in other triples. This perhaps not surprising, given the relative attractiveness (ρ) here of **RRR**. Indeed, only one respondent (an individual at stage 1) chose **SSS** in triple 4. Secondly, partnerships were somewhat more EU-consistent than were individuals in triple 4, and somewhat less so in triples 1–3. These facts are connected, as will shortly be explained.

As regards the type of inconsistency, of the non-EU responses (individual or partnership) almost all were CR. Across all triples and respondents, around 90% of responses were either EU or CR, although there are some variations in this by triple. It therefore seems useful to identify a class of response pattern of which EU and CR are (exhaustive) special cases. We call this a General Common-Ratio (GCR) pattern, defined by the absence of any (re-)switching from $\mathbf{R}i_t$ to $\mathbf{S}i_t$, as p_i

Table 3. EU and CR responses by triple

		stage 1	stage 2	stage 3
triple 1	EU	0.48	0.22	0.28
	CR	0.52	0.74	0.61
	other	0.00	0.04	0.11
triple 2	EU	0.41	0.22	0.33
	CR	0.54	0.61	0.57
	other	0.04	0.17	0.11
triple 3	EU	0.30	0.17	0.37
	CR	0.61	0.61	0.54
	other	0.09	0.22	0.09
triple 4	EU	0.63	0.87	0.54
	CR	0.28	0.13	0.37
	other	0.09	0.00	0.09
total	EU	0.46	0.37	0.38
	CR	0.49	0.52	0.52
	other	0.05	0.11	0.10

falls. Within a CR-triple there are four such patterns, and we can order them as follows:

SSS **SSR** **SRR** **RRR**

i.e., according to the point, within the triple, at which the **S** → **R** switch (if any) occurs. In terms of the Machina-Marschak triangle, these are the four response patterns consistent with the fanning-out hypothesis (Machina, 1982).

The distribution of responses across the four GCR patterns, as ordered above, was unimodal for most triple:stages. The exceptions were 4:2, 2:3, 3:3, only the last of which was markedly bi-modal. Given this, and given that overall around 90% of responses were GCR, we can perhaps think in terms of a 'representative respondent'. For each triple, the median response pattern (of those conforming to GCR) was the same at each stage. For triples 1 and 2 it was **SSR**, for triple 3 it was **SRR** and for triple 4 it was **RRR**. Thus, the median (i.e., representative) GCR switch point was monotonically related to the value of ρ, and was the same for partnerships and individuals.

If most responses are GCR then we would expect that, within each triple, the proportion of respondents choosing $\mathbf{R}\boldsymbol{i}_t$ increases with i, i.e., as p_i falls. Table 4 confirms this. It records this proportion for each prospect-pair, at each stage. In addition to confirming the GCR response pattern, the data shows that for any given i there was a general tendency for the proportion choosing $\mathbf{R}\boldsymbol{i}_t$ to increase with t, i.e., as ρ increased. There were a few exceptions to this, however.

Table 4 shows also that partnerships were more inclined to choose $\mathbf{R}\boldsymbol{i}_t$, both in total and for most prospect-pairs, than were individuals. This is consistent with the evidence (Table 3) that on average groups were more EU-consistent than individuals in triple 4, and less so in other triples. For both individuals and partnerships, as ρ increases the distribution of GCR responses shifted away from **SSS** and towards

Table 4. Proportional aggregate responses ($\mathrm{R}i_t$) by prospect-pair

	stage 1	stage 2	stage 3	1 and 3
$\mathbf{R1_1}$	0.07	0.00	0.04	0.02
$\mathbf{R2_2}$	0.22	0.22	0.39	0.11
$\mathbf{R3_3}$	0.59	0.74	0.65	0.48
$\mathbf{R1_2}$	0.13	0.26	0.07	0.02
$\mathbf{R2_2}$	0.33	0.52	0.41	0.17
$\mathbf{R3_2}$	0.67	0.83	0.59	0.48
$\mathbf{R1_3}$	0.17	0.22	0.13	0.07
$\mathbf{R2_3}$	0.54	0.52	0.52	0.41
$\mathbf{R3_3}$	0.76	0.78	0.67	0.59
$\mathbf{R1_4}$	0.65	0.87	0.63	0.52
$\mathbf{R2_4}$	0.89	0.91	0.91	0.83
$\mathbf{R3_4}$	0.91	1.00	0.93	0.87
total	0.49	0.57	0.50	0.38

RRR, the EU-consistent endpoints of the GCR range. But for any given triple the distribution of partnerships' responses was positioned more towards **RRR** than was that of individuals. This difference is only slight since, as already noted, the median GCR response for any given triple was the same for individuals and partnerships. But for each of triples 1–3 individuals' responses were sufficiently closer to **SSS** for their overall EU-consistency to be higher, while for triple 4 the partnerships' responses were sufficiently closer to **RRR** for the reverse.

We can therefore summarise our findings on group choices as follows, with reference to Table 3. The average level of EU-consistency, at 0.37, was close to that for individuals, especially at stage 3. As regards the type of inconsistency, the similarity was also very close. Responses exhibiting the Common-Ratio effect accounted, on average, for a further 0.52 of partnerships' overall responses, again just as for individuals. Both for partnerships and for individuals, the representative respondent had a GCR response pattern which shifted rightwards (away from **SSS** towards **RRR**) as the value of ρ increased. Together with the general tendency for partnerships to favour $\mathbf{R}i_t$ more than did individuals, this largely accounts for the differences, by triple, in EU-consistency between partnerships and individuals.

These are our main findings. However, our data contains further information which may be of interest, in respect of both individual and group choices. Firstly, the final column in Table 4 records the proportion of individual respondents who chose $\mathbf{R}i_t$ at each of stages 1 and 3. This gives some indication of the degree of 'repetition-consistency' between the two individual stages. For example, while 0.67 of individuals chose $\mathbf{R3}_2$ at stage 1, and 0.59 at stage 3, only 0.48 chose $\mathbf{R3}_2$ at both stages. Simple calculation reveals that 0.19 must have changed from $\mathbf{R3}_2$ to $\mathbf{S3}_2$, and 0.11 changed in the reverse direction. So 0.30 of responses, for this prospect-pair, changed between the two stages. It follows that 0.70 of responses remained unchanged (and therefore that 0.22 of individuals repeatedly chose $\mathbf{S3}_2$).

Table 4 shows that in total, i.e., over all prospect-pairs, 0.38 of responses were $\mathbf{R}i_t$ at both stages. So by a similar calculation just over three-quarters (0.77) of all responses were unchanged. Aggregated by triple, the repetition-consistency rate ranged from 0.71 in triple 2 to 0.84 in triple 4.

Secondly, for any prospect-pair in which both partners registered repeated and identical choices, we would expect their joint choice to coincide with this. Table 5 records the instances of Pareto-inefficiency, where this was not the case. It does so

Table 5. Pareto-inefficiencies

	both **R**	both **S**
triple 1	0^{7}	0^{26}
triple 2	0^{7}	5^{22}
triple 3	1^{13}	2^{18}
triple 4	1^{39}	1^{2}
total	2^{66}	8^{68}

according to the triple containing the prospect-pair in question, and according to whether each partner's individual preference was for $\mathbf{R}i_t$ or $\mathbf{S}i_t$. The superscript figure, in each cell, shows the total number of instances (within that triple) in which partners registered such preferences repeatedly and unanimously. For example, within triple 3 there were 13 instances where, for some given i, both partners repeatedly chose $\mathbf{R}i_3$. In one of these, the partnership nevertheless chose $\mathbf{S}i_3$.

The general incidence of Pareto-inefficiency, defined thus, appears to be quite low. The obvious exception is in triple 2, where there were five instances of partnerships choosing $\mathbf{R}i_2$ despite their partners' repeated preference for $\mathbf{S}i_2$. This is consistent with the finding that, overall, partnerships tended to favour $\mathbf{R}i_t$ more than did individuals, although of course we would not have expected this to over-ride unanimous individual preferences.

Thirdly, in section 2 it was noted that the Common-Ratio Effect involves a violation of at least one of the Reduction and Independence axioms of EU theory. Generalisations of EU theory, to accommodate such deviant preferences, normally involve some weakening of the Independence axiom. An example of this is the fanning-out hypothesis mentioned above, which rationalises GCR responses. But other approaches are possible.[6]

Although such questions were not our central concern, our data does contain some indirectly relevant information. Assuming that a larger prize is preferable to a smaller one, then there are some response patterns, across prospect-pairs in different triples, which would specifically violate Independence. For example, if £35 is preferable to £30 then according to Independence $\mathbf{R3}_2$ is preferable to $\mathbf{R3}_1$. Since $\mathbf{S3}_2$ is the same as $\mathbf{S3}_1$, it would therefore be inconsistent (given Transitivity) to choose both $\mathbf{R3}_1$ and $\mathbf{S3}_2$. Indeed, there is one response pattern which would simply violate the assumption that a larger prize is preferable. This is the choice of $\mathbf{R1}_1$ and $\mathbf{S1}_3$.

Table 6 shows the set of similarly inconsistent response patterns, and records their incidence. Each respondent (individual or partnership) could have committed up to 6 such violations. Thus, up to 276 violations could have occurred at each of stages 1 and 3, and up to 138 at stage 2. Given this, the overall incidence seems to

Table 6. Cross-triple violations of Independence

		stage 1	stage 2	stage 3
$\mathbf{R1}_1$ &	$\mathbf{S1}_2$	1	0	1
	$\mathbf{S1}_3$	1	0	0
$\mathbf{R2}_1$ &	$\mathbf{S2}_2$	0	1	3
	$\mathbf{S2}_3$	2	0	2
$\mathbf{R3}_1$ &	$\mathbf{S3}_2$	1	1	5
	$\mathbf{S3}_3$	2	1	3
$\mathbf{S1}_4$ &	$\mathbf{R1}_2$	1	1	0
	$\mathbf{R1}_3$	1	0	2
total		9	4	16

be quite low. It is at its highest at stage 3, in respect of $\mathbf{R3}_1$, as described above. Interestingly, one of the culprits here was Leonard's erstwhile and impressionable partner. In fact, his selection of the prospect $\mathbf{R3}_1$ constituted the only difference between his stage 3 choices and Leonard's.

6. Summary and conclusions

Leonard apart, we found no support for the hypothesis that individuals learn to be more EU-consistent either through discussion or through repetition. Indeed, between stages 1 and 3 there is a fall in the overall incidence of EU responses. However, the date does suggest that individuals' choices are far from arbitrary. There is strong evidence of a systematic GCR response pattern in all triples, although again the overall incidence of this falls slightly between stages 1 and 3.

Partnership choices, likewise, appear not to be arbitrary. The incidence of Pareto-inefficiency, and of cross-triple Independence violations, is low. As with individuals, there is a strong GCR response pattern in all triples. For partnerships, the overall incidence of both EU and GCR response patterns is very similar to that for individuals, although there are some differences by triple.

Finally, we note that non-EU responses, in the case of partnerships, cannot be attributed to allocation effects, since in all cases except one the partners agreed to divide all prizes equally. This requires further comment. Partners may have been explicitly concerned with fairness, an idea which has been explored in other experimental work. However, the focus of such work is typically on zero-sum distributional issues, such as in bargaining or ultimatum games.[7] If our subjects were guided solely by a fairness principle, it was strong enough to steer them not only away from distributionally unfair shares, but also towards shares which we can presume to be allocatively inefficient.

This can be seen by pursuing the example in section 4. Consider the joint prospect $\mathbf{S2}_2$, with prizes divided as follows:

$\mathbf{S2}_2^+$	J	K
0.5	£21	£9
0.5	−£5	£5
v	13.12	3.10

Although unequal, this is an *ex ante* envy-free allocation, which J and K each prefer to $\mathbf{S2}_2$ with equal division. It illustrates the possibility of efficient risk-sharing under which, roughly, the less risk-averse individual (here J) bears proportionately more of the risk. Of course, without knowing subjects' individual degrees of risk-aversion, we could not be sure in any given case that equal division was Pareto-inefficient. But we can be fairly confident that it was so in more than 1/23 of our partnerships.

Our own conjecture is that partners would have agreed to efficient (unequal) divisions, had they perceived this possibility for mutual gain, but that they simply failed to do so. Perhaps this was because the prospect, or its representation, was sufficiently complex to hide the possibility, or perhaps because the number of choices diverted attention from it. We are currently engaged in a new series of experiments designed specifically to explore this conjecture. Preliminary results appear to support it.

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Notes

1. Camerer (1995) provides an excellent overview.
2. A succinct and accessible treatment can be found in chapter 11 of Rasmusen (1994).
3. Experiment work in this area has, to our knowledge, largely concentrated on strategic interaction, although for a recent exception see Friedman (1998). In the strategic context, repetition also permits dynamic strategies through which, in particular, collusion may be facilitated. It is important to distinguish this from learning as such. Ledyard (1995) provides a discussion of these issues in the context of public good contributions. For an experimental investigation of strategic learning in the proper sense, see Erev and Roth (1995).
4. Even had we provided interim outcomes, any suboptimality experienced as a result of Common-Ratio inconsistency is, to say the least, of an ethereal type.
5. This is only a suggestive analysis. For a more complete account see Bone (1998).
6. Carlin (1992) offers some experimental evidence that the problem lies with Reduction rather than with Independence as such. Cubitt, Starmer and Sugden (1998) explore, experimentally, an analogous decomposition in terms of dynamic choice axioms.
7. A seminal reference is Ochs and Roth (1989). Roth (1995) provides a survey of strategic bargaining experiments, including an extended discussion of this issue.

Appendix: A Group Pairwise Choice Experiment

Welcome to this experiment. The instructions are simple, and if you follow them carefully, you may make a considerable amount of money which will be paid to you in cash immediately after the experiment.

The experiment is in three parts:

1. the first you will do in private before attending the session into which you have been booked;
2. the second you will do in conjuction with one other participant when you attend for this session;
3. the third you will do in private at the end of the session.

You must complete all three parts of the experiment. We anticipate that the session into which you have been booked will last no more than one hour in total.

All three parts of the experiment involve a number of Choice Problems. On each of these you will be asked to choose one or other of the two Options on offer. In the first part of the experiment, you will be choosing as an individual, as indeed in the third part, so you should choose which of the two you personally prefer. In the second part you and the other participant have to jointly choose one or other of the two Options on offer. This makes this second part of the experiment somewhat different from the first and third parts—we will have more to say on this shortly.

In order to provide an incentive for you to consider your choices carefully, we will be using the following payment scheme for your participation in the experiment. After you have completed the experiment, we will pick *one* of the Choice Problems at random: that is *one* picked randomly from the Choice Problems in the first, second and third parts of the experiment. If the randomly picked Choice Problem is from the first or third part of the experiment, we will play out for real the Option you chose on that Problem—and pay you accordingly.

If the randomly picked Choice Problem comes from the second part of the experiment, the procedure we will use will be slightly different—though the basic idea is the same. Once again, we will play out for real the Option you chose on that question; to then decide how to pay you and the other participant off we will follow decisions you made earlier about the division of the possible payoffs. Thus, in the second part of the experiment, we will not only ask you and the other participant to come to a joint decision as to which Option you are choosing but also to make a statement about how the various possible payoffs would be divided up between you.

Option A:
£20 with probability 1.0

outcome	1's payoff	2's payoff	total payoff
£20			£20

Option B:
£40 with probability 0.75
£2 with probability 0.25

outcome	1's payoff	2's payoff	total payoff
£40			£40
£2			£2

Let us give an example. Consider the following Choice Problem:

There are various possibilities for you and the other participant. For example:

1. Choose (A) and split the proceeds £10 to you and £10 to him/her
2. Choose (A) and split the proceeds £12 to you and £8 to him/her
3. Choose (B) and if £2 is the outcome, both of you get £1; whilst if £40 is the outcome, you get £19 and he/she gets £21
4. Choose (B) and you get £2 whatever happens, while he/she gets £0 if £0 is the outcome, and gets £38 if £40 is the outcome
5. Choose (B) and he/she gets £12 whatever is the outcome, while you lose £10 if £0 is the outcome (i.e. you pay him/her £10) and get £28 if £40 is the outcome

More generally, your choices will be one of the following:

6. Choose (A), and you get £x while he/she gets £$(20 - x)$
7. Choose (B), and you get £y and he/she gets £$(2 - y)$ if £0 is the outcome, while you get £z and he/she gets £$(40 - z)$ is £40 is the outcome.

You are free to choose either 6. and any value of x that you agree or 7. and any values of y and z that you agree.

If you have not come to an agreement on a Choice Problem and that Problem is chosen at the end to be played out for real, then we will take the smallest amount in that Choice Problem and split it equally between you.

In the second part of the experiment, you will be asked to compete a box like the one above for each of the Choice Problems. Note that the total of the second and third columns must be the same as the entry in the first and last columns. So, for example, if you chose 1. above, you would do the following:

Option A:	
	£20 with probability 1.0
	CHOSEN

outcome	1's payoff	2's payoff	total payoff
£20	£10	£10	£20

Option B:	
	£40 with probability 0.75
	£2 with probability 0.25

outcome	1's payoff	2's payoff	total payoff
£40			£40
£2			£2

If, however, you chose 4. above you would do as illustrated below:

Option A:	
	£20 with probability 1.0

Option B:	
	£40 with probability 0.75 £2 with probability 0.25 CHOSEN

outcome	1's payoff	2's payoff	total payoff
£20			£20

outcome	1's payoff	2's payoff	total payoff
£40	£2	£38	£40
£2	£2	£0	£2

To summarise, the experiment involves the following:

Part 1: Before you come to attend the experimental session, you will do the following. On each of the Choice Problems (appended to these instructions) you must choose either Option A or Option B.

Part 2: You will attend the experimental session, hand in your answers to Part 1, and be paired with some other participant. You will be presented with a sequence of Choice Problems. On each of these Choice Problems you and the other participant with whom you are paired must choose either Option A or Option B as well as indicating how the various possible payoffs would be divided up between you.

Part 3: After the experimental session, you—again as an individual—will be presented with a sequence of Choice Problems. Once again on each of the Choice Problems you must choose either Option A or Option B.

At the end of the experiment, *one* of the Choice Problems from Parts 1, 2 and 3 will be picked at random. If the randomly picked Choice Problem is from Parts 1 or 3, then your chosen Option will be played out for real and you will be paid accordingly. If the randomly picked Choice Problem is from Part 2 then the Option you (jointly) chose earlier will be played out for real—and the two of you paid accordingly—according to the division of the payoffs that you specified earlier. If you had not reached any agreement then the smallest amount of money in that Choice Problem will be split equally between you.

If there is anything in these instructions that you do not understand please ask one of the experimenters.

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