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Are bygones bygones?

by

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Abstract:
The paper reports an experiment which tests the principle of separability, i.e. that behaviour in a dynamic choice problem is independent of history and of unreachable eventualities. Although this is a well-known principle of orthodox decision theory and central to conventional economic modelling, it has been questioned on grounds suggested by non-expected utility models of choice under risk and by the psychology of affective influences on risk-taking. Our experimental design, which provides between-subjects tests of separability using three treatments in which the history preceding a decision is manipulated, is inspired by these concerns. We expose separability to a clean and harsh test, but find no evidence that it is violated.

Keywords:
Separability; history-independence; non-expected utility; risk and affect.

Section 1: Introduction

The maxim that "bygones are bygones" expresses classic folk wisdom. It is also a standard principle of normative decision theory, often formalised as a principle of 'separability'. Separability requires agents to take decisions by comparing the available options in eventualities that can still occur, uninfluenced by how the current situation was reached or by eventualities that are precluded by that history. In the language of decision trees, it requires the agent's choice at a particular choice node to be independent of unreachable parts of the tree. This formulation makes clear how central the principle is to standard economic theory by, for example, founding the folding-back algorithm for individual sequential decisions and game theoretic concepts such as subgame perfection.

Yet, despite this, there are important conceptual and empirical reasons to wonder whether agents will obey separability. For example, Machina (1989) famously put violation of the principle at the heart of his defence of non-expected utility models of choice under risk which were inspired by well-known experimental violations of traditional theory (Machina, 1987; Camerer, 1995; Starmer, 2000). A different set of reasons for expecting separability to fail is provided by the psychological literature on affect (Isen, 1999; Slovic *et al* 2002). Perhaps partly because of suspicions about the empirical validity of separability, theorists have recently developed models which allow experience acquired as a decision problem unfolds to influence subsequent behaviour in it (see for example, Cohen *et al,* 2008).

In this paper, we report an experimental test of separability motivated by these concerns.[1] One virtue of an experimental approach is that it allows a clean and direct test, in which subjects face decision problems that are essentially identical apart from controlled variation in what precedes the choice. It is difficult to find this feature in field data, even where it is drawn from a highly structured environment. For example, a striking field study that seems to cast doubt on separability is Post *et al* (2008)'s analysis of behaviour in the TV game show *Deal or No Deal?*[2] Their analysis suggests a tendency for participants' willingness to take risks to vary, depending on whether they have been lucky or unlucky earlier in the game. While this is a notable finding, its implications for separability are not conclusive because the data set does not provide observations of the behaviour of participants facing essentially the same decision problem after different histories.[3]

There has been some experimental research on dynamic choice.[4] Previous studies reported by Cubitt *et al* (1998a), Busemeyer *et al* (2000), and Cubitt and Sugden (2001) were each designed to test sets of dynamic choice principles, of which separability was one; and each found more evidence that other dynamic choice principles were violated than that separability itself was. While this is re-assuring for some aspects of conventional theory, it is surprising from the perspective of non-EU decision theory and the psychology of affect. In our view, this justifies a further test with a combination of design features motivated specifically by these concerns.

The remainder of our paper is organised as follows: Section 2 explains the theoretical background; Section 3 describes our experimental design; Section 4 presents the results; and Section 5 concludes.

Section 2: Theoretical background

2.1: Formulating the separability principle

We assume a single agent and work initially with a framework in which the choices she faces are represented by decision trees. A tree consists of choice nodes (drawn as squares) at which the agent makes a choice between two or more options; chance nodes (drawn as circles) at which nature resolves uncertainty between two or more possibilities according to well-defined probabilities; and terminal nodes, at which the agent receives a final consequence. Every option (resp. possibility) at every choice (resp. chance) node corresponds to some immediately succeeding node; and every node in the tree immediately succeeds exactly one other, with the exception of a unique initial node. We assume a set X of final consequences, a unique element of which is associated with each terminal node.

For any choice node n in any tree T, T*(n) denotes the free-standing tree that is identical to the sub-tree of T commencing at n; and $n_0(T^*(n))$ denotes the initial node of T*(n). We postulate an action-choice function that, for every choice node, picks out as the agent's chosen option one of the options available at that node. In this framework, the principle of separability can be formulated as a condition on the action-choice function:

> *SEP:* Consider any choice node n in any tree T. The chosen option at n corresponds to the chosen option at $n_0(T^*(n))$.

As an example, let $X = \{x_1, x_2, x_3\}$, where these consequences are monetary amounts with $x_1 > x_2 > x_3 \geq 0$. (We assume throughout that more money is preferred to less.) Consider the following trees:

Figure 1: T1

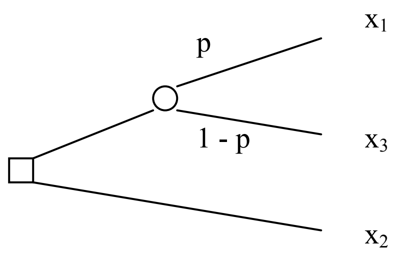
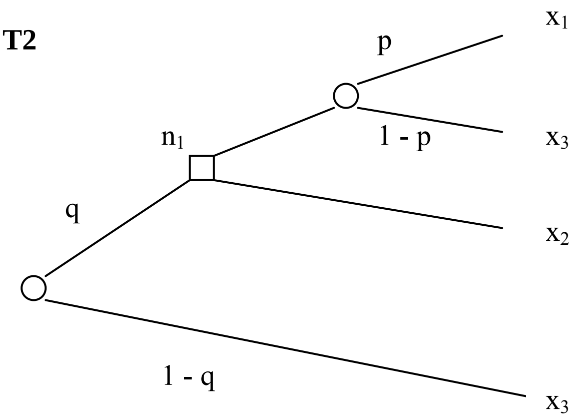


Figure 2: T2



T1 represents a choice between the lottery (x_1, p; x_3, 1 – p), where 1 > p > 0, and the certainty (x_2, 1). T2 represents a situation in which, with probability 1 - q (with 1 > q > 0), the agent receives (x_3, 1) and with probability q, she must choose between (x_1, p; x_3, 1 – p) and (x_2, 1). Note that T1 is the free-standing tree identical to the sub-tree of T2 which commences at the choice node n_1. Thus, SEP requires that, if n_1 is reached in tree T2, the agent makes the same choice there as she would make in T1.

2.2: Non-EU preferences

There is substantial evidence, especially from experiments on Allais paradoxes and related phenomena, of violation of the independence axiom of expected utility theory. Yet, if an agent's preferences over lotteries do violate that axiom, that makes it controversial whether she should make the same choice in T1 and T2. Any agent with non-expected utility preferences *must* violate at least one out of a small set of principles of dynamic decision-making of which separability is one (Cubitt *et al*, 1998a, 2004, and references therein). The best-known argument that picks out one of these principles as a particular candidate for violation is that due to Machina (1989); and it picks out separability.

To see why, consider an agent who prefers (x_2, 1) to (x_1, p; x_3, 1 – p) and (x_1, qp; x_3, 1 – qp) to (x_2, q; x_3, 1 - q). Such an agent displays the classic violation of expected utility theory known as the *common ratio effect*,[5] which in turn can be explained by indifference curves in the unit probability triangle with some tendency to "fan out".[6] If faced with T1, this agent will choose Down to obtain (x_2, 1) rather than (x_1, p; x_3, 1 – p). Given this, SEP requires her to choose Down in T2, if node n_1 is reached. However, for a tree of this type, Machina argues that the agent will instead employ a *back-tracking decision procedure* that identifies with each available option at node n_1 the lottery implied by the tree *as a whole*, if that option is taken. Given this procedure and reduction of compound lotteries, the lottery identified with Up at n_1 is (x_1, qp; x_3, 1 – qp) and that identified with Down at n_1 is (x_2, q; x_3, 1 - q). Thus, given her preferences, the agent will choose Up in T2, if she has a choice to make. Although it implies that the agent violates SEP, this account leaves the agent dynamically consistent, in the sense that her behaviour at the choice node accords with the plan she would have made at the start of the tree, despite her non-EU preferences. In this respect, Machina's analysis accords with the model of resolute choice (McClennen, 1990) and with the normal form approach to decision-making.

Machina's back-tracking procedure provides one example of how an agent with non-EU preferences might violate separability. Another, quite different, possibility is suggested by Post *et al's* (2008) prospect theoretic analysis of their *Deal or No Deal?* data. They propose a model[7] of sequential decision-making in which, when required to make a choice, the agent evaluates the monetary outcomes that are still possible as gains and losses relative to some reference-point. An initial reference-point is formed by the agent considering the decision problem as a whole, viewed from its start; and the reference-point is then adjusted as the sequential problem unfolds. However, crucially, the adjustment process is sticky. Thus, for example, the reference-point that the agent employs at the choice node in T2 will be a weighted average of her initial reference-point, formed at the start of T2, and of whatever reference-point would be appropriate to the remaining sub-tree, if faced in isolation. Since the latter is precisely the reference-point she would employ in T1, sticky adjustment drives a wedge between the reference-points employed at the choice nodes of T1 and T2. Even though the choice-sets at these two nodes are identical, viewed in a forward-looking way and in terms of absolute monetary consequences, comparison of the monetary prizes with different reference-points allows SEP to be violated, if (as in prospect theory) the agent's value function displays diminishing sensitivity to gains and losses.[8] Note that, for this *sticky* *reference-point* model to be compatible with a common ratio effect in the agent's preferences, any violation of SEP would have to be in the same direction as suggested by Machina's hypothesis.[9] The stickier the reference-point, the more likely SEP violation would be.

Thus, non-EU sequential decision theory provides two distinct reasons for expecting the agent to be more attracted to the risky option Up at the choice node of T2 than at the choice node in T1, if her preferences display a common ratio effect.

2.3: The psychology of affect

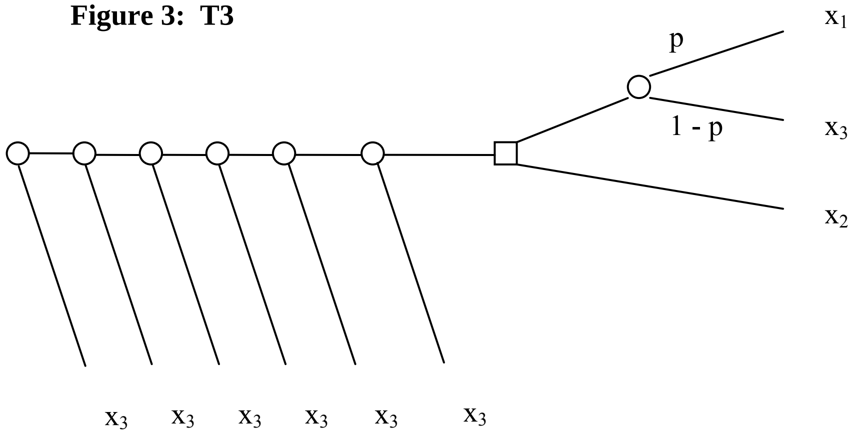
An entirely different reason for doubting the separability principle can be distilled from the possibility that agents endorse another popular maxim: "Don't push your luck" (DPYL). Since the agent at n_1 in T2 cannot do worse than receive x_3 and can guarantee herself x_2 if she chooses to do so, having to make the choice in T2 must be construed as good luck compared with the alternative possible outcome of the initial chance. The maxim DPYL gives the agent at n_1 in T2 a reason to act cautiously that the agent at the initial node of T1 does not have. Thus, an agent who picks Up in T1 and is swayed sufficiently strongly by DPYL, would violate SEP.

Grounds for expecting agents to behave in accordance with DPYL, at least in certain circumstances, can be found in the psychological literature on the influence of affect on judgement and decision-making (Isen, 1999; Slovic *et al*, 2002). Experience of positive affects can lead to changes in: probability assessments (Johnson and Tversky, 1983); valuations of outcomes (Isen *et al*, 1988); or relative weights in decision-making on outcome and probability dimensions of risk (Nygren *et al*, 1996). According to Isen (1999), it is a stylised fact that positive affect tends to increase risk aversion in the context of decisions that are perceived as involving significant risk. We will call this the *affect hypothesis*. Support for this hypothesis has typically been obtained in studies in which positive affect is manipulated by giving subjects small gifts. However, it is plausible that experience of positive outcomes of risks could itself be a source of positive affect. For example, given the argument that reaching n_1 in T2 is a lucky outcome compared with the alternative, the agent might be expected to experience positive affect on reaching that point. If so then, provided the choice faced there is perceived as a serious one, the influence of affect would be expected, according to the argument of Isen (1999), to induce more risk averse behaviour among agents reaching n_1 in T2 than among agents at the initial node of T1. This is the opposite direction of violation of SEP as that predicted, in the presence of a common ratio effect, by the non-EU sequential decision theories discussed in the previous sub-section.

2.4: A variant on T2

Finally, consider T3 in which the agent faces a series of chance nodes before, possibly, reaching a point at which she must choose between (x_1, p; x_3, 1 – p) and (x_2, 1) and in which failure to reach that point always results in (x_3, 1). From the perspective of separability, it makes no difference what the probabilities are at the chance nodes that precede the choice node in T3. SEP implies that, if the choice node is reached, the subject makes the same choice as she would in T1 (and, therefore, T2). But, now suppose that the overall probability of reaching the choice node in T3 is q, just as in T2.

Figure 3: T3



Given reduction of compound lotteries, Machina's back-tracking argument suggests that the agent will make the same choice (if reached) in T2 and T3, since the probabilities of making a choice, the outcome if she does not, and the forward-looking aspects of the problem are the same in the two trees. In contrast, if positive affect is at least partly responsive to the number of successful risk outcomes (and not just to probability), the affect hypothesis would lead one to expect a greater propensity to take the risky option in T2 than in T3. The comparison of these trees, from the perspective of the sticky reference-point model, turns on which produces stronger adjustment of the reference-point between the start of the tree and the choice node. Given reduction of compound lotteries, one would expect the initial reference-point to be the same in T2 and T3; and that each reference-point would rise, as the choice node is approached, in the direction of that used in T1. On this view, in which ever case adjustment is stronger, the propensity to take the risky option will be closer to that in T1.

Section 3: Experimental Design

The theoretical background set out in the previous section motivates the most important features of our experimental design. As we will explain, these are a single-task design,[10] with significant monetary incentives; a control which tests for the presence of a "common ratio" violation of expected utility theory in subjects' preferences over static gambles; experimental manipulation of the pre-decision phase across more than one prior history treatment; and qualitative data on subjects' reasoning. While there are previous studies with some of these features, we are not aware of any with this combination.[11]

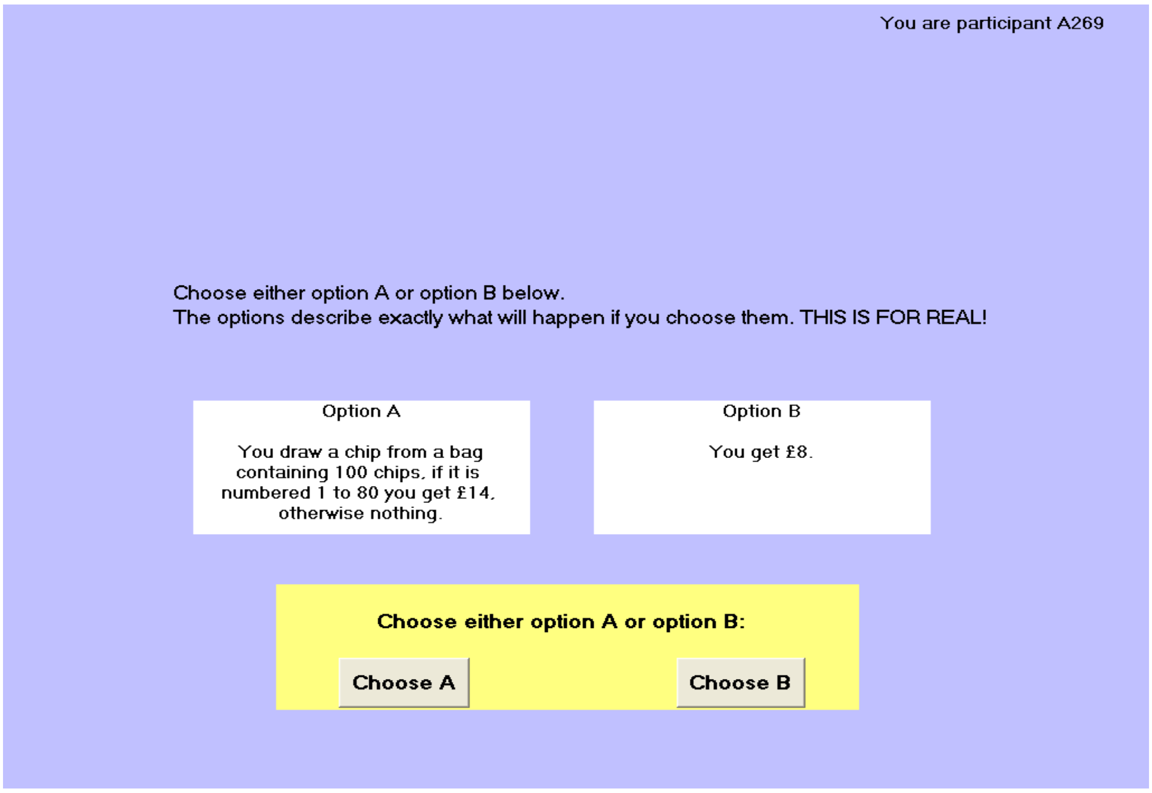
The experiment has three treatments; and, in each one, individual subjects faced just one task for real, with that task corresponding to either T1, T2 or T3. We refer to these as the 'main' task for each subject. Subjects also responded to some additional tasks in a questionnaire which we describe below, but, in each case, the main task was completed before the questionnaire and was the only incentivised task that each subject faced.

Relative to other incentive schemes, this single-task design is costly to implement because it generates so little incentivised data from each subject, but it has considerable advantages compared with alternatives, particularly when attempting to test dynamic choice principles such as separability. There are essentially two ways in which multiple tasks can be incentivised. The first is the 'all pay' approach in which subjects are rewarded according to the outcome of each task that they complete. A major disadvantage of this design, however, is that it creates the possibility of confounding wealth effects across tasks.[12] A more widely used alternative, intended partly to avoid such effects, is the random lottery incentive system.[13] In applications of this procedure, subjects face multiple tasks knowing that their payoff will depend on their responses to one of the tasks they complete, but they do not know which of the tasks is for real until the end of the experiment. However, given our purposes, each of these alternatives to the single-task design has an inherent weakness. Our experimental objectives require us to compare behaviour in problems whose trees differ in specific ways under our control. Use of either a random lottery incentive mechanism or an all pay regime would undercut this objective because it would imply that the incentivised part of the experiment corresponds to a more complex tree. Moreover, we cannot predict in advance how behaviour in each main task would be affected by this added complexity without invoking particular dynamic choice principles (Cubitt *et al*, 1998a, section 2). As separability is just such a principle, this would be a significant disadvantage. Hence we implement the more costly, but cleaner, single-task design.

The trees for the three main decision problems are T1, T2 and T3, with x_1 = £14, x_2 = £8, x_3 = 0, p = 0.8 and q = 0.25. These parameters were chosen as they are typical of previous designs with real incentives in which the common ratio effect has been found. In the baseline *no history treatment* (NH), which corresponds with T1, each subject faced a single choice between a certainty of £8 and a lottery that would yield £14 with probability 0.8 and zero otherwise. All tasks were presented on computer screens and the choice task for treatment NH appeared as shown in the screen capture of Figure 4.[14] Subjects who chose the certain option were paid a task-reward of £8. For subjects who chose the risky option, this was played out by a draw from a bag of chips that subjects knew to be numbered from 1 to 100. Subjects who won were then paid £14 for this task.

The other two treatments involved *prior history* of a specific kind, featuring one or more risks. In these two treatments, subjects began by facing a random process with two possible outcomes, one of which was "losing", i.e. leaving the experiment with no reward from this task, and the other of which was "surviving" to face a choice between the same two options as in NH. In the *single prior risk treatment* (SPR), subjects had to survive a single prior risk to reach the decision (this task corresponds with tree T2); in the *multiple prior risk treatment* (MPR), subjects had to survive six prior risks to reach the decision (this task corresponds with tree T3).

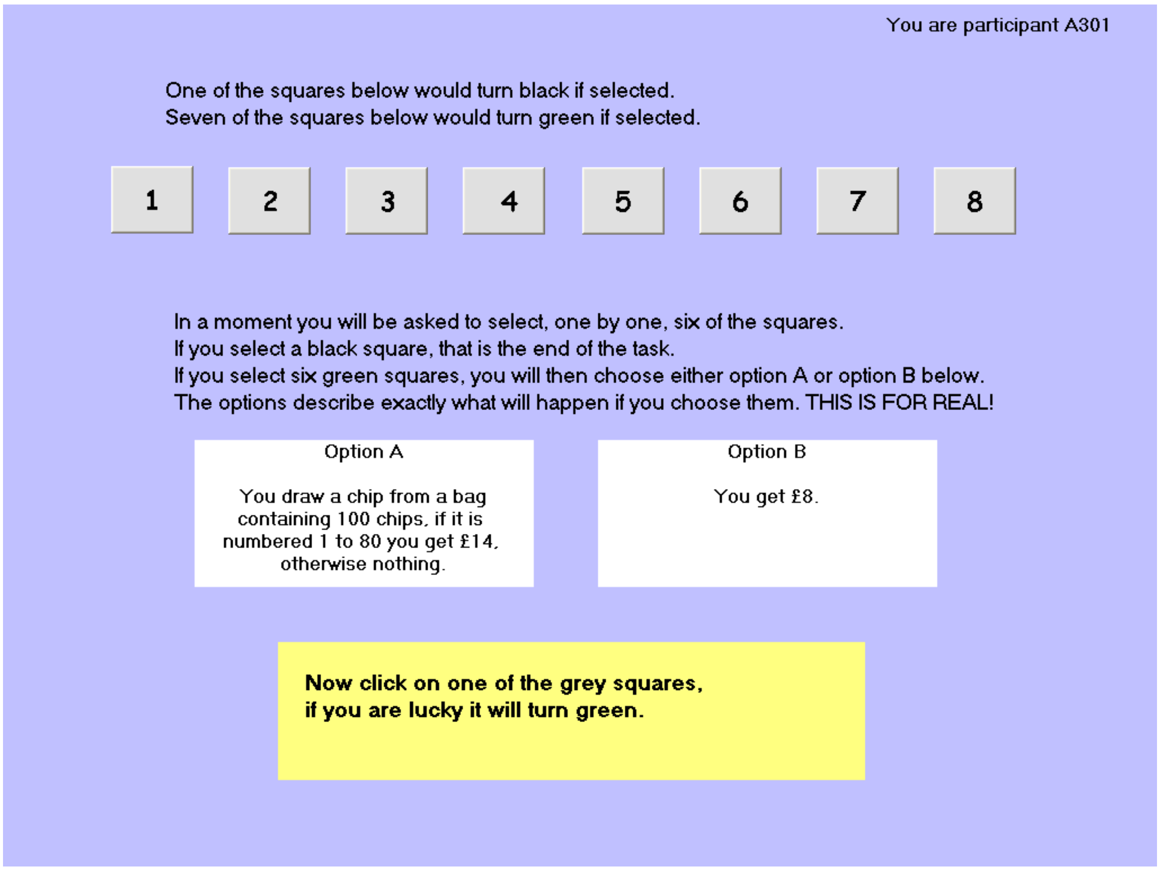
Figure 4: Task display for the No History Treatment



In the SPR and MPR treatments, the prior risks (single or multiple) were operationalised by presenting subjects with a screen containing eight grey squares some of which they were required to select. This is illustrated in Figure 5, using the display for the subjects in the MPR treatment. In that treatment, subjects were required to select six different squares knowing that one of the eight squares would lose. Whether or not a particular square was a losing one was only revealed to the subject when they selected it. Losing squares turned black on selection;[15] non-losing squares turned green. In the SPR treatment, subjects were required to select just one square, knowing that six of the eight squares were losing ones.[16] If the subject picked a losing square for any selection, the task would end and the subject would receive zero for the task. Subjects who survived the prior risks in either prior history treatment then faced a choice between the same two options as in NH, as shown in Figure 5.

It is a deliberate feature of our design that the overall probability of surviving the prior risks is the same (i.e. 0.25) in each prior history treatment. However, we conjectured that surviving the selection of six separate squares, one after another, would be likely to produce stronger positive affect than surviving the selection of a single square especially as, in the MPR treatment, the probability of picking a losing square rises with every square selected. The design tests this conjecture, whilst controlling overall survival probability.

Figure 5: Task display for the Multiple Prior Risk Treatment



Separability implies the following null hypothesis, in relation to the main tasks of our design:

H_0: f(NH) = f(SPR) = f(MPR).

where f(t) is the probability that a randomly selected individual, from the population from which subjects were drawn, would choose the risky option when faced with the main task in treatment t. Such probabilities may be interpreted in terms of a theory of preferences according to which, for each individual, preferences are non-stochastic and random variation arises from the random allocation of subjects to treatments. Alternatively, the probabilities can be interpreted using an assumption that each individual has preferences that are subject to random variation, as discussed by Loomes and Sugden (1995, 1998).[17]

Conditional on the existence of a common ratio effect in subjects' preferences, Machina's argument implies the following alternative hypothesis:

H_1: f(SPR) = f(MPR) > f(NH).

Given the same condition, the sticky reference-point model accords with Machina's argument in predicting f(MPR) > f(NH) and f(SPR) > f(NH), but it makes no specific prediction about the comparison of f(MPR) and f(SPR).

In contrast, conditional on the conjectures that positive affect would be stimulated by survival of the prior risk stages, and most strongly so in the multiple prior risk treatment, the affect hypothesis implies:

H_2: f(NH) > f(SPR) > f(MPR).

After completing one of the three main tasks, subjects were asked to respond to a brief questionnaire with two elements: (a) a qualitative response to a question concerning the main task followed by (b) hypothetical responses to some other binary choice tasks. Subjects were paid a flat fee of £2 for completing these parts of the experiment.

For part (a) of the questionnaire, all subjects were asked a single question, the form of which depended on the main task which they had faced. Subjects in the NH treatment were asked: *"In the task you just completed, you had a choice to make. What did you choose and why?"*; whereas subjects in the prior risk treatments were asked: *"In the task you just completed, you may have had a choice to make. If you did have the choice, what did you choose and why? If you didn't have the choice, what do you think you would have chosen (if you had had it) and why?"*. These questions were intended to provide a non-behavioural indicator of subjects' reasoning. In particular, they allow us to explore two issues. First, to what extent do subjects who have survived a risky prior history refer back to that history in explaining their decisions? Second, whether or not they refer back, does the pattern of forward-looking reasons that they give vary according to the treatment they have faced in our experiment? An affirmative answer to either question would suggest that factors inconsistent with separability are observable at least at the level of reasoning. Also, if separability fails to be violated in choices because of offsetting effects, for example because the tendencies suggested by **H_1** and

$\mathbf{H_2}$ go in opposite directions, one would nevertheless expect those effects to leave some footprint in subjects' qualitative responses.

Part (b) of the questionnaire was the same for all subjects and consisted of a set of hypothetical binary choice questions. A subset of these tasks were designed to provide a check on the existence of a common ratio effect for our subjects[18]. The use of hypothetical tasks for this purpose is supported by two considerations. First, as we have already explained it is an important feature of our strategy for testing separability that, in each treatment, subjects faced just a single decision (the main task) for real. Secondly, we have a good empirical basis for expecting that hypothetical choice tasks will provide a reliable test for the presence of a common ratio effect because previous research has examined the influence of real versus hypothetical incentives on the incidence of the common ratio effect and found no evidence of any impact on qualitative findings (see Beattie and Loomes, 1997; and Cubitt *et al* 1998b).

Section 4: Results and Discussion

The experiment was conducted at the University of Nottingham. Subjects were recruited randomly from the CeDEx database of registered volunteers. A total of 377 subjects took part, mainly undergraduate and postgraduate students from a range of disciplines. The experiment was run across 26 sessions with the treatment determined randomly for each session.

Table 1 summarises the choices made in the main task. Although 162 and 167 subjects respectively took part in the SPR and MPR treatments, the numbers surviving the prior lotteries to make a choice were 50 in the SPR treatment and 46 in MPR.[19] Thus, the realisations of the prior lotteries yielded approximately the same number of choices in all three treatments.

Table 1

Summary of Data from Main Task

Treatment	Total Subjects	Total Choices	Certainty Choices		Risky Choices	
			Number	(%)	Number	(%)
NH	48	48	21	43.75	27	56.25
SPR	162	50	26	52	24	48
MPR	167	46	22	47.83	24	52.17
Total PR	329	96	48	50	48	50
Totals	377	144	69	47.92	75	52.08
Null Hypothesis Test			$\chi^2 = 0.67$		Not rejected	

There is a straightforward conclusion from Table 1. The last row of the table reports the chi-squared statistic from a test of the null hypothesis of separability. With $\chi^2 = 0.67$, the null is not rejected. In addition, the tendencies observed in individual's willingness to take risks, across treatments, oppose both $\mathbf{H_1}$ and $\mathbf{H_2}$. Contrary to Machina's argument and the sticky reference point model, a higher percentage of subjects chose the risky option in the NH treatment than in the prior risk (PR) treatments. Although the direction of this difference is consistent with the affect hypothesis, it is not statistically significant and, contrary to the affect hypothesis, there is more risk taking among MPR subjects than among SPR subjects.

Although the most straightforward interpretation of the findings reported in Table 1 is that subjects satisfy the principle of separability, some other possible interpretations may occur to the reader. For instance, notice from Table 1 that, aggregating across all treatments, subjects were quite finely balanced between options with approximately half of them choosing the certainty over the risky option. Achieving a non-extreme split between the options was in fact a design objective because this makes it possible for us to observe violations of separability in either direction, which is important as $\mathbf{H_1}$ and $\mathbf{H_2}$ predict violations in different directions. Nevertheless, one might wonder whether the roughly even split between safe and risky options, in our main task, is evidence that subjects simply chose at random. Analysis of our questionnaire responses, however, leads us to discount this possibility. As we will shortly see, even though these tasks were hypothetical, responses were highly systematic. Given this, it is implausible to interpret behaviour in our main, incentivised, tasks as dominated by randomness.

The questionnaire responses also allow us to examine another possible interpretation of our results. Specifically, on Machina's hypothesis and the sticky reference point model, violation of separability is to be expected *among agents who violate the independence axiom of expected utility theory*. So, one potential explanation of our failure to reject separability is that our subjects obey that axiom. If this were so, we would have failed to provide a harsh test of separability, from the non-EU perspective.

The choice tasks in our questionnaire included[20] two pairs of problems very similar to those used in a range of previously reported tests for the common ratio effect. These tasks are presented in Table 2 where each row describes a choice between a "Risky Option" and a "Safer Option". Since all of the options are binary lotteries with just one non-zero prize, we represent each one compactly by its potential prize (the first bracketed number for each cell) and the probability of winning it (the second bracketed number). Hence Task 1 is a choice between a 75% chance of £30 versus a sure amount of £18. Task 2 is identical to Task 1 except that the probability of winning in each option has been scaled down by a common factor of 0.2. Tasks 1 and 2 are a typical pair of problems used to test for the common ratio effect in previous literature (see, for example, Beattie and Loomes (1997) and Cubitt *et al* (1998b) who use problems very similar to these). Tasks 3 and 4 form another pair of classic common ratio problems: Task 3 is a choice between an 80% chance of £12 versus £8 for sure; Task 4 is the same except that a (more extreme) scaling factor of 0.05 was applied.[21] For each pair of tasks, a common ratio effect would be a tendency for more risk-seeking behaviour when probabilities are scaled down.

Table 2

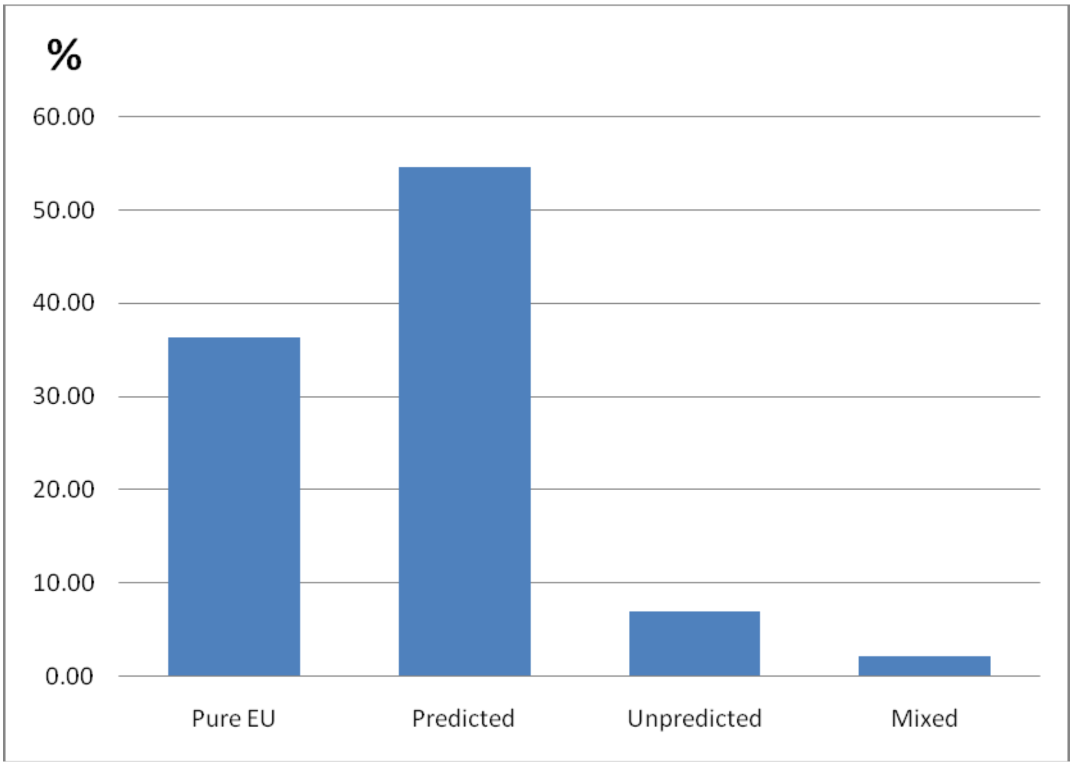
Summary of Independence Tests

Task	Risky Option	Safer Option	% Risky	Z
1	(30, 0.75)	(18,1)	53.9	11.24
2	(30, 0.15)	(18, 0.2)	88.4	
3	(12, 0.8)	(8,1)	52.8	11.48
4	(12, 0.04)	(8, 0.05)	88.1	

The penultimate column of Table 2 reports the percentage of subjects choosing the risky option in each task.[22] Notice that, in the choices that feature certainties (Tasks 1 and 3), there is a near equal split between safe and risky options, much as in our main tasks. But when probabilities are scaled down, and the choice is between two relatively low-probability bets, almost 90% of subjects select the risky alternative. The final column of Table 2 reports tests of the null hypothesis that choice proportions are unaffected by the scaling of probabilities (reported statistics are Z-values based on the normal distribution). The null hypothesis is confidently rejected at the 1% level in both cases. This is very clear evidence of a common ratio effect in aggregate behaviour.

We also classify individuals according to their behaviour across the two pairs of common ratio problems in the questionnaire. We partition into four groups. One group contains individuals whose four choices are entirely EU consistent: we label these ‘Pure EU’ individuals. A second category contains individuals who violate EU (once or twice) but only ever in the direction of the common ratio effect: we call these ‘predicted’ violators. A third category, ‘unpredicted’, contains individuals who violate EU at least once, but only ever in the opposite direction of the common ratio effect. The final category, ‘mixed’ contains individuals who violated expected utility theory twice but in different directions. Figure 6 reports the percentages of individuals in each of these four categories. The majority of subjects (55%) are predicted violators and most of the rest are consistent EU. The scale and consistency of the common ratio effect in these data support the supposition that a substantial proportion of our subjects were prone to violate the independence axiom of expected utility theory, in the usual common ratio effect direction, in contexts where it is typically found. Since our main task is built around typical common ratio effect parameters, this leads us to be very sceptical of the interpretation that our failure to reject separability in the main tasks arose from a preponderance of subjects who satisfy the independence axiom.

Figure 6: Individual 'types' based on common ratio responses



Two further diagnostic possibilities relate to the role that affective reactions might play in our data.

One is to suppose that, although there is potential for the effect postulated by the affect hypothesis, we have failed to induce affective reactions in our experiment (and the effect postulated by non-EU decision theory are also absent). Would it be a reasonable conjecture that our prior risks do not generate the potential for affective reaction? One difference between our experiment and those in the existing experimental psychology literature is that the latter have typically used small gifts (Isen, 1999), such as a bag of candy, to stimulate positive affect. Accordingly, we conjectured that surviving a prior lottery that entitles one to get a certainty of £8 as a prize would be a more effective stimulant of positive affect. Thus, if positive affect is driven by values of consequences, we have good reason to expect that our design is at least as likely to induce an affect effect as previous studies.[23]

A second possibility is to suppose that the effects postulated by non-EU theory and by the affect hypothesis are both present, but they offset each other. This view looks less promising when reminded that the comparison of the two prior risk treatments revealed no significant difference and the difference that there was went in the 'wrong direction' for the affect hypothesis.[24] For this interpretation to go through we need to posit that there is an effect of affect caused by experience of past risk, and consistent with the maxim DPYL, which roughly cancels out the non-EU effect but is not sensitive to the number of past risks.

To shed further light on these issues, we now consider responses to the qualitative question about the main tasks, in which subjects were asked about their reasoning. For present purposes, we confine our analysis to the responses of the 144 individuals who actually made a choice (i.e. all of the NH subjects plus those who survived to choose in either of the prior history treatments), because we cannot be sure that subjects who failed to survive prior risks could accurately predict what they would have done if they had (Loewenstein and Adler, 1995; Loewenstein *et al*, 2003). Recall that the purpose of this question was to shed light on whether or not subjects give backward-looking reasons, on the nature of any such backward-looking reasons, and on the impact of treatment on any forward-looking reasons given. To address these questions, we classified an individual as giving a backward-looking reason if his or her answer contained *any* reference to the past. Examples of backward-looking answers we observed are: *"I don't believe I'm lucky enough to get it right twice"*, and *"I was lucky enough to make the choice"*. All other reasons were treated as forward-looking.

The questionnaire responses lead to two very clear-cut observations. Firstly, the reasons our subjects gave for their choices contained almost no references to the past: only 4 subjects (2.8%) gave a backward looking reason. Secondly, we could find no evidence that the distribution of forward-looking reasons varied between groups according to the task history.[25] The appendix gives details of the distributions of forward-looking responses. Thus, the qualitative data part of our study provides no reason for doubting separability and tells against the interpretation that its failure to be violated arises from offsetting effects.

Section 5: Conclusion

To sum up, neither the choice data from our main tasks nor our qualitative data suggest any tendency for separability to be violated, despite the fact that there is clear evidence from the questionnaire choices that a significant proportion of subjects were systematically non-EU. It seems that, as far as our subjects were concerned, bygones were bygones.

Appendix: distributions of forward-looking reasons

Table 4 provides a simple classification of forward-looking reasons given by subjects who chose the lottery in the main decision task. Table 5 provides a corresponding breakdown for subjects who chose the certainty. Note that in each of the two tables a given subject may have more than one code.

Table 4

Distribution of Forward-Looking Reasons: Lottery Choosers

Forward-Looking Reasons Pro Lottery	**Lottery Choosers:** Frequency of reasons			
	SPR (24 subjects)	**MPR (24 subjects)**	**NH (27 subjects)**	**Total (75 subjects)**
High Probability and Prize	15	14	19	48
Explicit Expected Value	2	2	2	6
More money	1	3	3	7
High Probability	2	2	3	7
Nothing to Lose	6	7	4	17

Table 5

Distribution of Forward-Looking Reasons: Certainty Choosers

Forward-Looking Reasons Pro Certainty	**Certainty Choosers:** Frequency of reasons			
	SPR (26 subjects)	**MPR (22 subjects)**	**NH (21 subjects)**	**Total (69 subjects)**
Certainty	20	16	18	54
Extra £6 not worth risk	3	2	3	8
Disappointment avoidance	-	-	1	1

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Notes

[1] Our concern is the descriptive adequacy of the separability principle. For debate about its normative status, see Bratman (1987), Machina (1989) and McClennen (1990).

[2] See Bardsley *et al* (2010, section 6.4.5) for further discussions of issues arising from analysis of game show data and references to other studies of *Deal or No Deal?*.

[3] Post *et al* (2008) use econometric techniques to infer an impact of history, in the form of which prizes have been eliminated, on subsequent willingness to accept the "banker"'s sure thing offers in the TV game. But a participant facing a particular such choice after a lucky history receives a different bank offer and faces a different set of remaining possible prizes, compared with another participant who reaches the corresponding stage with an unlucky history. Nor is there is there any analog in the game of facing a given decision in isolation from history, analogous to "snipping off" a sub-tree from some larger tree. (As we will explain, a snipped-off task plays an important part in our design.) Other possible concerns about game show data, as evidence for separability violation, include the possibility that participants are selected for having particular characteristics (e.g. likely extreme responses to the unfolding of the game) or the possibility of uncontrolled variation in social aspects of history induced by either the studio audience or the show's host

[4] See Cubitt *et al* (2004) for a survey.

[5] To see that this is a violation, note that expected utility theory implies the existence of a function u(.) defined on consequences, maximisation of the expectation of which represents preferences. Assuming that more money is preferred to less, it also permits normalisation of $u(x_1)$ to unity and of $u(x_3)$ to zero. Then, if $(x_2, 1)$ is strictly preferred to $(x_1, p; x_3, 1 – p)$, we must have $u(x_2) > p$. But, if $(x_1, qp; x_3, 1 – qp)$ is strictly preferred to $(x_2, q; x_3, 1 – q)$, we must have $qp > qu(x_2)$. The two inequalities are inconsistent, since q is positive.

[6] See Machina, (1987), Camerer (1995) and Starmer (2000) on fanning-out and for surveys of studies of the common ratio effect and other violations of expected utility theory.

[7] Post *et al* (2008) do not attempt to present their model in fully general terms. Thus, we are taking a liberty in applying it to a different set of problems from those they consider, but we believe our rendition is faithful to the spirit of their model.

[8] With linear (or piece-wise linear) functional forms, different reference-points would merely induce distinct, but parallel, value functions, leaving differences between valuations of the relevant gains and losses unaffected, so preventing a violation of SEP. However, diminishing sensitivity is a key element of prospect theory.

[9] Without a common ratio effect, the sticky reference-point model could in principle induce a violation of separability in either direction. One would expect the reference point employed at the choice node of T1 to be higher than that employed at the choice node of T2 but, without the discipline imposed by accounting for a common ratio effect, this difference in reference-points might, in principle, induce a difference in risk-taking in either direction.

[10] See Cubitt et al (2001) for discussion of this class of designs.

[11] Cubitt *et al* (1998a) and Cubitt and Sugden (2001) used single-task designs. Cubitt *et al* (1998a) controlled for violations of expected utility theory in static choice but found only relatively weak evidence of a common ratio effect among their subjects, whereas we will report below strong evidence of such an effect. Johnson and Busemeyer (2001) report an experiment that manipulates the length of the pre-decision phase across prior history treatments, but does not report a test of separability. None of these studies reports qualitative data on subjects' reasoning.

[12] This problem is accentuated if subjects do not integrate their experimental winnings with other assets.

[13] See Cubitt *et al* (1998a,b) and Bardsley *et al* (2010, section 6.5) for discussion.

[14] For all treatments, the positioning of the safe and risky options varied randomly from left to right across subjects. So, for some subjects, the safe option appeared as option A (on the left); for others, it appeared as option B (on the right).

[15] Subjects were required to confirm selections of squares they indicated their intention to pick; confirmed selections could not be de-selected; nor could they be picked for further selections.

[16] The experimenters had a pre-printed record of the subject-specific distribution of the losing squares and subjects were told that they could corroborate that this matched what they had observed in their task, if they wished, at the end of the experiment.

[17] Loomes (2005), Wilcox (2008) and Bardsley *et al* (2010, chapter 7) provide further discussion of this "random preference" specification and comparison of it with alternative models of stochastic choice.

[18] We give more details of these tasks when we report the results from them.

[19] 233 subjects did not reach the choice node in their main task and therefore received a payment of zero for this task. This is an essential feature of the design, if subjects who do survive to reach the choice node in the SPR and MPR treatments are to have faced a genuine risk of not surviving.

[20] The questionnaire included eleven binary choice tasks in total. Those not described here were intended to mask from subjects the structure of the two pairs of choices described here.

[21] We also included another task generated by applying a scaling factor of 0.5 to the parameters of Task 3. This intermediate scaling factor is rather un-typical of common ratio problems reported in the literature and, in fact, this manipulation produced no significant change in behaviour when compared with Task 3.

[22] Since the same qualitative pattern is observed for each of the subgroups, we report aggregate data here.

[23] It is possible that positive affect could be more sensitive to the gift-framing than to the value of the gift, in which case separability might hold in our experiment where no explicit gift is involved even if it fails in other cases. However, if the gift framing is crucial for affect, the significance of the effect for economic settings would be diminished.

[24] Although the difference is not significant, the direction of the difference between NH and the two prior risk treatments suggests, if anything, that the affect hypothesis dominates non-EU influences. But, this is difficult to reconcile with a small difference in what, for the affect hypothesis, is the wrong direction between SPR and MPR.

[25] Since there were so few backward-looking reasons, there is little point in analysing how they vary across groups.