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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 standard principle of decision theory, it can be 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. But, we find no significant evidence of violation of separability.

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

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Section 1: Introduction

The maxim that "bygones are bygones" expresses one of the most widely endorsed principles of normative decision theory. It requires rational agents to take decisions by comparing the available options in eventualities that can still occur, ignoring the history of how the current situation was reached and any eventualities that are precluded by it. Expressed in the language of decision trees, it requires rational decision at any choice node to be independent of unreachable parts of the tree. In this paper we discuss, not the normative force of "bygones are bygones", but the question of whether economic agents act in accordance with it.[i] We refer to the claim that individual decision-making does satisfy "bygones are bygones" as the *separability principle.* This principle is fundamental to economic theory because of the commitment of (conventional) economics to the explanation of actual behaviour as rational choice, combined with general acceptance of the normative force of "bygones are bygones". Although (presumably) not the primary motivation for it, this combination also has the convenient property of permitting situations of interest in economics to be modelled without reference to how they arose.

This paper reports an experimental test of the principle of separability motivated by the fact that, notwithstanding its central place in economics, there are grounds for doubt about its empirical validity. Section 2 describes the two sources of doubt – one derived from non-expected utility models of choice under risk, the other from the psychology of affect - that inspired our experimental design. Section 3 describes that design. Section 4 reports and discusses the results. Section 5 concludes.

Section 2: Theoretical background

We work initially with a tree framework for representing individual choice under risk. A tree consists of choice nodes (represented by squares) at which the decision-maker must make a choice between two or more options; chance nodes (represented by circles) at which nature resolves uncertainty between two or more possibilities according to well-defined probabilities; and terminal nodes, at which consequences are received. Every option (resp. possibility) at every choice (resp. chance) node n corresponds to some node which immediately succeeds n; every node in the tree immediately succeeds exactly one other node, with the exception of a unique initial node, which does not succeed any other. We assume a set X of consequences; a unique element of X 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 n, picks out as the chosen option at n 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 the elements of X 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:

INSERT Figure 1 HERE.

INSERT Figure 2 HERE.

T1 represents a choice between the lottery (x_1, p; x_3, 1 – p), where 1 > p > 0, and (x_2, 1), i.e. a certainty of x_2. 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). T1 is the free-standing tree identical to the sub-tree of T2 which commences at the choice node n_1. Thus, SEP implies that, if n_1 is reached in tree T2, the agent makes the same choice there as she would make in T1. Notice that the same implication of SEP would hold, regardless of how one varied the structure of those parts of T2 that do not succeed n_1. Generalising this point, SEP is a strong principle which implies the independence of choice at any choice node n of the number of nodes that precede n, of their nature (i.e. choice or chance), and of the options and possibilities foregone at them (including all of the further nodes that could have been reached down other paths). Our choice of which predictions of separability to test was motivated by particular factors that cast doubt on it which we now explain.

If the agent's preferences over lotteries violate the independence axiom of expected utility theory, it is not uncontroversial that 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). Probably the most well-known argument that picks out one of these principles as a candidate for violation is due to Mark Machina. Machina (1989) argues that an agent with non-expected utility preferences should be expected sometimes to violate 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*,[ii] which in turn can be explained by indifference curves in the unit probability triangle with some tendency to "fan out".[iii] 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, Machina's analysis accords with the model of resolute choice (McClennen, 1990) and with the strategic, or normal form, approach to decision-making.

An entirely different reason for doubting separability 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.[iv] 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 the DPYL maxim, 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 to induce more risk averse behaviour among agents reaching n_1 in T2 than among agents at the initial node of T1. Notice that testing this extension of the affect hypothesis avoids a problem associated with designs that have used gifts to stimulate positive affect, namely that they may confound the influences of affect and wealth.

The two reasons given above for why separability might be violated in decision problems whose trees are T1 and T2 suggest violations in opposite directions. To the extent that the agent's preferences over lotteries violate expected utility theory in the way suggested by the common ratio effect, Machina's argument suggests a greater propensity to choose the risky option at n_1 in T2 than at the initial node of T1. But, on the affect hypothesis, one would expect a greater propensity to select the risky option in T1 than at n_1 in T2.

Now, 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 $(x_3, 1)$. Suppose that the overall probability of reaching the choice node in T3 is q, just as in T2.

INSERT Figure 3 HERE

SEP requires that, conditional on reaching the choice node, the agent makes the same choice in each of T1, T2 and T3. Given reduction of compound lotteries, Machina's argument suggests that the agent will make the same choice (if reached) in T2 and T3. If positive affect is at least partly responsive to the number of successful risk outcomes (and not just to probability) then the affect hypothesis would lead one to expect the greatest propensity to take the risky option in T1, followed by T2, and then by T3.

Section 3: Experimental Design

A key feature of our experiment is that it exploits the single-task design of the kind discussed by Cubitt *et al* (2001). Specifically, our experiment involves three treatments; in each one, individual subjects faced just one task for real corresponding with 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 it was the only incentivised task that each subject faced (all of the questionnaire tasks were hypothetical).

Relative to other widely used incentive schemes, the single-task design is costly to implement, because it generates so little (incentivised) data from each subject, but it has considerable advantages 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. A more widely used alternative, designed partly with the aim of avoiding such effects, is the random lottery incentive system.

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.

Relative to our purposes, however, 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. But, the use of a random lottery incentive mechanism, or an all pay regime, undercuts this objective because it implies that the incentivised part of the experiment corresponds to a more complex tree. Moreover, we cannot predict how behaviour, relative to each main task, will be affected by this added complexity without invoking particular dynamic choice principles (Cubitt *et al*, 1998, Section 2). This is clearly a major disadvantage given our objective of testing separability, hence we implement the more costly, but cleaner, single-task design.

So, in all treatments subjects made a single real decision involving risks. 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 C of £8 and a lottery R 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.[v] 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 C and R. 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).

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[vi]; 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[vii]. 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 treatment then faced a choice between C and R.

An important feature of our design is 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 affects than surviving the selection of a single square, especially as in the multiple prior risk treatment the probability of a losing square rises with every one selected. The design tests this conjecture, whilst controlling overall survival probability.

After completing one of the three main tasks, subjects were asked to respond to a brief questionnaire requiring (a) hypothetical responses to a number of supplementary binary choice tasks, preceded by (b) a qualitative response question concerning the main task. Subjects were paid a flat fee of £2 for completing these parts of the experiment. We have already explained why it is a crucial feature of our design that, in each treatment, subjects faced just a single task (the main task) for real. Even in the absence of financial incentives, however, we have a good basis for expecting that the responses to the hypothetical choice tasks will provide meaningful data. The function of these tasks was to provide a check on the existence of a common ratio effect for our subjects. The use of hypothetical incentives in relation to this function of the questionnaire is supported by the findings of Beattie and Loomes (1997) and Cubitt *et al* (1998b, experiment 3), who report controlled tests of the influence of real versus hypothetical incentives on the common ratio effect and find no evidence of any such influence on qualitative findings. We return below to the qualitative response part of the questionnaire. First, we make precise the hypotheses to be tested in relation to the main task.

In the context of our design, separability implies the following null hypothesis:

$\mathbf{H_0}$: 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 R when faced with the main task in treatment t. Such probabilities may be interpreted in terms of a deterministic 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) and Loomes (forthcoming).

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).

Finally, 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).

Section 4: Results and Discussion

The experiment was conducted at the University of Nottingham during December 2004. 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 twenty six sessions with the treatment determined randomly for each session. We present the results of the main task first, deferring till later the results from the questionnaire.

Table 1 summarises the choices made in the main task. Although 162 and 167 subjects took part, respectively, in SPR and MPR treatments, just 50 and 46 individuals, respectively too, survived the prior lotteries to make a choice, yielding approximately the same number of choices in each treatment. A reassuring feature of the results is that, aggregating across all treatments, subjects were finely balanced in that approximately half of the subjects chose the certainty and approximately half the risky option. This makes it possible for us to observe violations of separability in either direction. This is important as H_1 and H_2 suggest tendencies for separability to be violated in different directions.

INSERT TABLE 1 HERE

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 alternative hypotheses. Contrary to Machina's argument, there is a higher percentage of subjects choosing the risky option in the NH treatment than in the prior risk (PR) treatments. The direction of this difference is consistent with the affect hypothesis but the difference is not statistically significant and, contrary to the affect hypothesis, there is more risk seeking behaviour among the MPR subjects than the SPR individuals. Hence, results from the main tasks lend little support to either of the arguments against separability that motivated our tests, taken individually.

The most straightforward candidate interpretation of our findings is that subjects satisfy the principle of separability. However, another possible interpretation is that, although subjects do not in general satisfy separability, we have failed to create conditions that would be necessary for observing violations of it. On Machina's hypothesis, violation of separability is to be expected among agents who violate the independence axiom of expected utility theory. One potential explanation of our main task findings is that our subjects typically obey that axiom. The affect hypothesis conditions an increase in revealed risk aversion on positive affective experiences. Another potential explanation of our main task findings is that the prior history stages of the tasks in the SPR and MPR treatments failed to stimulate sufficient positive affect. Yet a third possibility is that both effects do in fact operate in our experiment but cancel each other out. In the rest of this section, we discuss the tenability of these alternative candidate explanations, drawing on the data from the questionnaire part of the experiment.

The clearest evidence we have with regards to these possible interpretations relates to Machina's hypothesis. The questionnaire contained twelve hypothetical binary choice questions which were identical for each treatment[viii]. Five of these tasks were designed to address whether or not subjects display a common ratio effect. They involved choices of the form (x_2, 1) versus (x_1, p; x_3, 1-p); or of the form (x_2, q; x_3, 1-q) versus (x_1, qp; x_3, 1-qp). Note that the form of the second pair of lotteries results from scaling-down the first pair by multiplying the probabilities of a prize better than x_3 by a common ratio q, with $0 < q < 1$. Table 2 describes the five problems (P1 – P5). The first set is a pair of problems very similar to those in Beattie and Loomes (1997) and Cubitt *et al* (1998b), where x_1 = £30, x_2 = £18, x_3 = 0, p = 0.75 and q = 0.2. The second set consists of three problems where x_1 = £12, x_2 = £8, x_3 = 0 and p = 0.8. The monetary amounts used in the second set of problems are very close to those in the main tasks. However, we deliberately chose a value of x_1 that did not exactly match the main tasks to prevent them from being identical. Further, rather than using q = 0.25, as in the main tasks, we adopted a straddling policy of using q = 0.5 to construct P4 and q = 0.05 to construct P5.

INSERT TABLE 2 HERE

Taken together, these five problems allow four tests of expected utility theory: P1 vs P2; P3 vs P4; P3 vs P5; and P4 vs P5.[ix] For each pair of problems, the null hypothesis is that an equal proportion of subjects choose the risky option in both problems. To test these hypotheses, we use a test of proportions based on the normal distribution. (The critical values of the test statistic z are ± 2.58 and ± 1.96, for 1% and 5% significance levels, respectively.)

Results of these tests are summarised in Table 3. Looking first at the final row of the table we see very clear evidence of a common ratio effect: there is a marked tendency towards selecting risky options as the chances of winning are 'scaled down' (i.e. as q falls). The null hypothesis is rejected at the 1% level in three out of the four tests. The one in which it is not rejected, P3 versus P4, is the one in which we had the lowest prior expectation that it would be, because q = 0.5 is unusually high, relative to previous studies. The same qualitative pattern is observed for each of the subgroups. This provides strong support for the supposition that subjects were prone to violate the independence axiom of expected utility theory. Since our main task is built around typical common ratio effect parameters, this leads us to discount the interpretation that our failure to reject separability in the main tasks arose from a preponderance of subjects who satisfy the independence axiom.

INSERT TABLE 3 HERE

A further possibility is that there is an effect of the kind implied by Machina's hypothesis but that this is outweighed by an affective reaction in the opposite direction. This 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' relative to the affect hypothesis). 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 Machina effect and is not sensitive to the number of past risks.

A remaining possibility is that there is no Machina type effect (even among subjects who violate independence), that there is an effect as postulated by the affect hypothesis, but that we have failed to induce it in our experiment. Would this be a reasonable conjecture? 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. However, 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 would hold in our experiment where no explicit gift is involved even if it fails more generally. We note, however, that to the extent that the gift framing is crucial, the significance of the effect for economic settings would be correspondingly diminished.

While we cannot rule out these other interpretations on the basis of a single experiment, the simplest interpretation of our data, requiring least in the way of supplementary hypotheses, is that subjects behave as if bygones are bygones. This face value interpretation is also consonant with responses that subjects gave to an open-ended question presented at the start 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 the question:

> *"In the task you just completed, you had a choice to make. What did you choose and why?"*

Subjects in the prior risk treatments were asked the question:

> *"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?"*

In the spirit of attempting to expose separability to the hardest tests we can construct, we use the qualitative responses to explore two questions. First, to what extent do subjects who have survived a history of risk refer back to that history as an account of their decisions? If they do refer back, that might provide some reason to doubt separability. Conversely, if the failure to reject separability in the aggregate choice data arises from a conjunction of offsetting effects, one would expect those effects to leave some footprint in the qualitative responses. Of course, it is also possible that that reasoning could be history dependent even in the absence of any self-consciously backward looking reasoning. With this in mind, the second question we investigate is whether the patterns of any forward looking reasons that subjects give vary according to the history they have faced in our experiment.

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"*. Forward looking reasons were subdivided into those favouring the riskier choice versus those favouring the certainty[x].

While all subjects were asked these questions, for present purposes we confine the 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). We focus on these subjects largely because we cannot be sure that the excluded subjects could accurately predict what they would have done had they survived the risk to make a choice (Loewenstein and Adler, 1995; Loewenstein *et al*, 2003). While qualitative data can be difficult to analyse, our data lead to two very clear-cut observations pertinent to the investigation of separability. The first is that 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. A second observation is based on the data in Tables 4 and 5. Table 4 provides a simple classification of 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. Statistical analysis confirms the eyeball impression that the distribution of reasons across categories is independent of the task history.

INSERT TABLES 4 AND 5 HERE

Section 5. Conclusion

The principle of separability asserts that individuals obey the maxim that bygones are bygones. The fundamental role played by this principle in conventional decision theory justifies a sustained programme of empirical testing. This is especially so as doubts can be raised about separability on the basis of indirect evidence from the psychology of affect and from tests of non-expected utility models of choice under uncertainty. At a theoretical level, prominent authors such as Machina (1989) and McClennen (1990) have put violation of separability at the heart of their defences of non-expected utility theory.

Previous tests of separability in the context of choice under uncertainty, reported by Cubitt *et al* (1998a), Busemeyer *et al* (2000) and Cubitt and Sugden (2001), have found little evidence of violation of it. But, clearly, the failure of a few studies to reject the null hypothesis that separability holds cannot establish the general validity of the principle: investigators may simply have ‘looked in the wrong place’ to find violations. Each of the studies listed was designed to test a set of dynamic choice principles, of which separability was only one. Their designs were effective at detecting other violations of conventional dynamic decision theory, but not of separability. This consideration motivates further studies with new designs.

Our design is distinguished by the following combination of features, intended to provide a particularly clean and harsh test of separability in the light of the alternative hypotheses provided by the non-EU and psychology literatures: a single-task design, with significant monetary incentives; a control which establishes 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. Some of the previously mentioned studies have some of these features,[xi] but none has this combination.

Although we discussed alternatives in Section 4, the most straightforward explanation of our subjects' behaviour is that, as far as they were concerned, bygones were bygones.

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Figure 1: T1

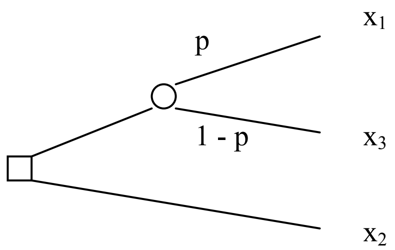

Figure 2: T2

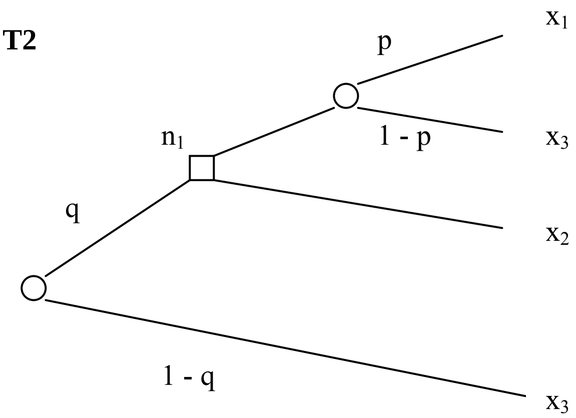

Figure 3: T3

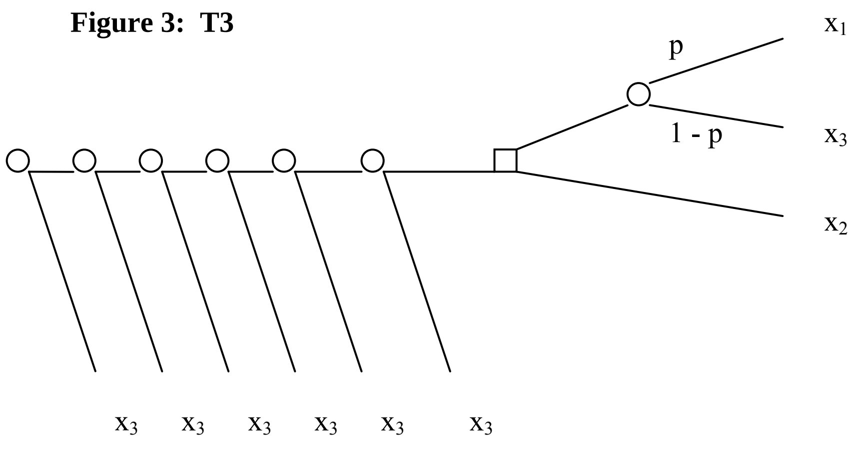

Figure 4: Task display for the No History Treatment

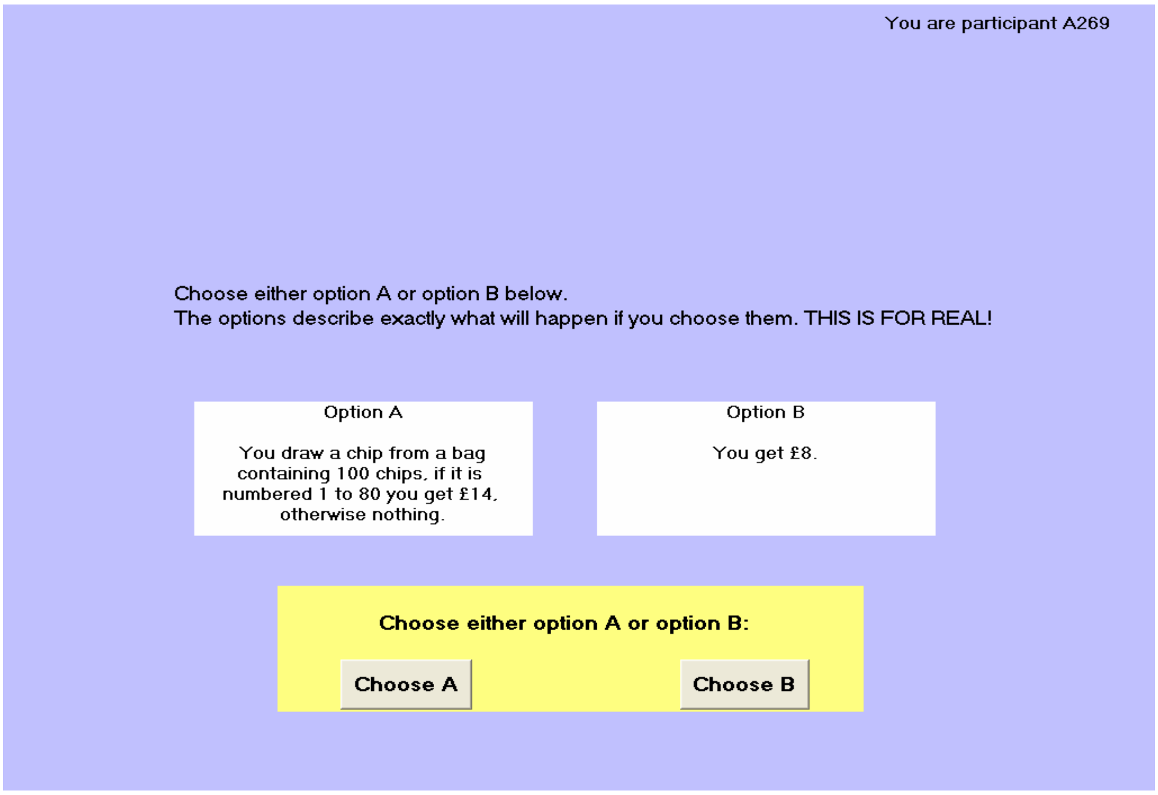

Figure 5: Task display for the Multiple Prior Risk Treatment

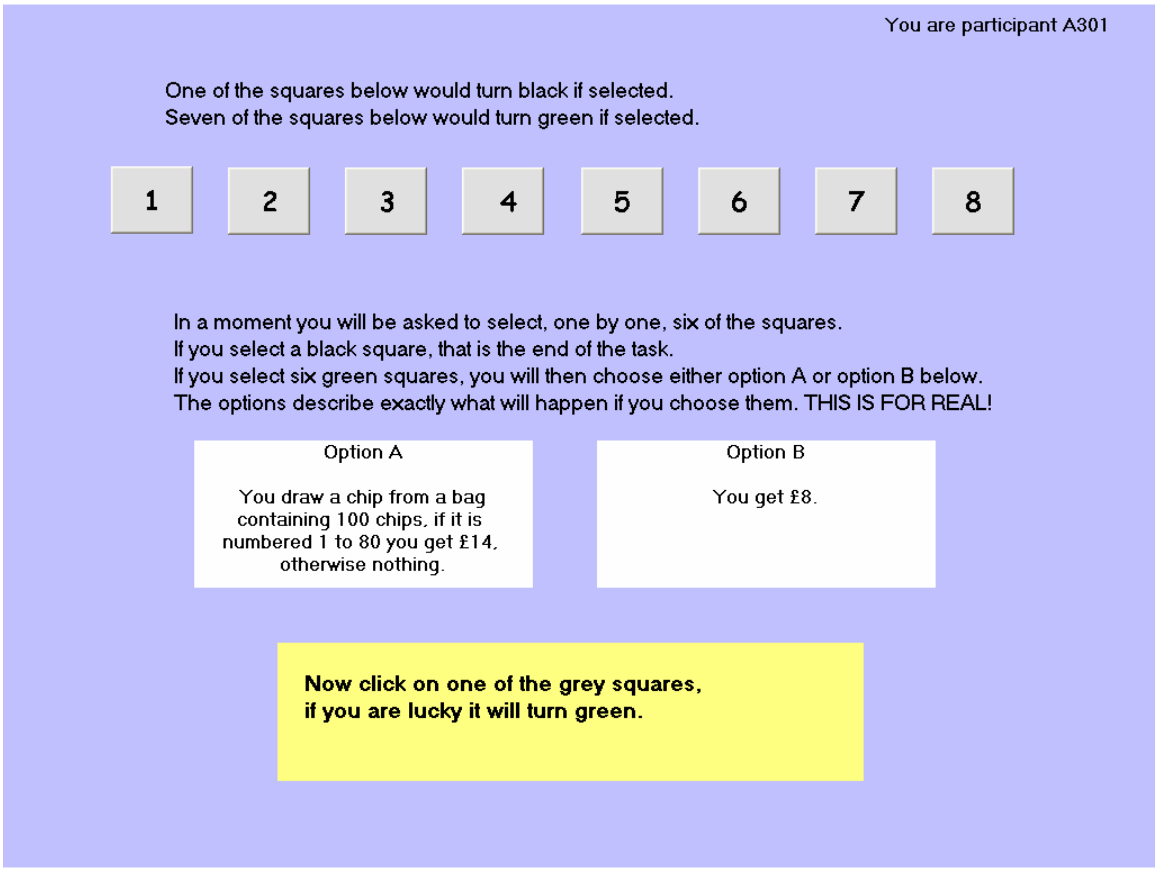

Table 1

Summary of Data from Main Task

Treatment	Total Subjects	Decisions	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	

Table 2

Summary of Common Ratio Problems

	Problem	Risky Option	Safer Option
First set	**1**	(30, 0.75; 0, 0.25)	(18,1)
	2	(30, 0.15; 0, 0.85)	(18, 0.2; 0, 0.8)
Second set	**3**	(12, 0.8; 0, 0.2)	(8,1)
	4	(12, 0.4; 0, 0.6)	(8, 0.5; 0, 0.5)
	5	(12, 0.04; 0, 0.96)	(8, 0.05; 0, 0.95)

Table 3
Summary of Choices in the Common Ratio Problems

Treatment	% Risky Option Choices (z statistics)				
	1	**2**	**3**	**4**	**5***
NH (48 subjects)	48.94	93.75 (5.54)	45.83	70.83 (-2.57)	95.83 (-6.45) (-3.49)
SPR (162 subjects)	57.41	88.27 (6.66)	53.70	52.47 (0.22)	89.51 (-7.79) (-6.26)
MPR (167 subjects)	48.15	87.04 (8.22)	53.94	48.50 (0.99)	84.43 (-6.37) (-5.52)
Total (377 subjects)	**53.91**	**88.44 (11.24)**	**52.8**	**53.05 (-0.69)**	**88.06 (-11.48) (-6.56)**

**The first statistic z refers to the comparison to problem 3; the second, to problem 4.*

Table 4

Distribution of Forward-Looking Reasons: Lottery Choosers

Forward-Looking Reasons Pro Lottery	Lottery Choosers Frequency#			
	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#			
	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

#Note to tables 4 and 5: note that each subject could have more than one code.

Notes

[i] On the normative question see, for example, Bratman (1987), Machina (1989) and McClennen (1990).

[ii] 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.

[iii] See 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.

[iv] Except for an agent averse to choice itself.

[v] 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).

[vi] 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.

[vii] 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.

[viii] The only difference between treatments was in the main task which preceded it.

[ix] Each of these pairs of problems poses a common ratio test, in the sense in which we have defined it, except for Problem 4 versus Problem 5 since neither of these involves a certainty. However, it is straightforward to show that expected utility theory implies that corresponding choices must be made in these two problems.

[x] The items favouring the lottery choice are *high probability and prize; explicit expected value; more money; high probability;* and *nothing to lose*. Examples of subjects' answers corresponding to these categories are, respectively: "there was a high probability of getting more money"; "the lottery has a bigger expected value-11.2 > 8-"; "£14 is considerably bigger than £8"; "80% is a high chance of winning"; and "I came with nothing"; among others. From the certainty choice perspective, the categories are *certainty*; *extra £6 not worth the risk;* and *disappointment aversion*. The corresponding examples are, respectively, "it was a guarantee amount"; "the extra £6 are not worth the risk"; and "to avoid disappointment if I had chosen the gamble and I had lost".

[xi] 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. 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.