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Reconsidering the common ratio effect: The roles of compound independence, reduction, and coalescing

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Reconsidering the common ratio effect:
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by **Ulrich Schmidt,
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Reconsidering the common ratio effect: The roles of compound independence, reduction, and coalescing*

Ulrich Schmidt and Christian Seidl

Abstract:

Common ratio effects should be ruled out if subjects' preferences satisfy compound independence, reduction of compound lotteries, and coalescing. In other words, at least one of these axioms should be violated in order to generate a common ratio effect. Relying on a simple experiment, we investigate which failure of these axioms is concomitant with the empirical observation of common ratio effects. We observe that compound independence and reduction of compound lotteries hold, whereas coalescing is systematically violated. This result provides support for theories which explain the common ratio effect by violations of coalescing (i.e., configural weight theory) instead of violations of compound independence (i.e., rank-dependent utility or cumulative prospect theory).

Keywords: common ratio effect, coalescing, reduction, compound independence, event splitting, branch splitting, isolation effect, Allais paradox.

JEL classification: C91, C44, D81.

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1 Introduction

The common ratio effect became one of the prime examples of the failure of expected utility theory and has motivated a substantial amount of literature analyzing the descriptive validity of expected utility (see, e.g., Hey and Orme (1994)).¹ For two monetary prizes, $a > b$, and a probability p , $1 \geq p \geq 0$, of winning a and $(1 - p)$ of winning b , we denote the respective lottery by $\Phi = (a, p; b, (1 - p))$. Suppose that there is also another lottery $\Psi = (c, q; b, (1 - q))$, $c > a$, $p > q \geq 0$, at choice. Reducing now the winning probabilities to λp and λq , $1 > \lambda > 0$, defines new lotteries $\Phi' = (a, \lambda p; b, (1 - \lambda p))$ and $\Psi' = (c, \lambda q; b, (1 - \lambda q))$. Let \succsim denote a subject's preference ordering among lotteries and suppose $\Phi \succsim \Psi$. Then this subject acts in conformity with expected utility theory, if

$$(1) \quad \Phi \succsim \Psi \Leftrightarrow \Phi' \succsim \Psi'.$$

However, experimental research, using both hypothetical and real payoffs, showed that many subjects decide according to

$$(2) \quad \Phi \succ \Psi \text{ and } \Phi' \prec \Psi'.$$

This is the *common ratio effect*. It holds in particular for $p = 1$ and $q < 1$.²

¹Allais (1953a,b, 1979a,b) and Morlat (1953) were the first to develop lotteries which revealed that subjects violate the axioms of expected utility, primarily the independence axiom and the sure-thing principle. Allais (1979b, p. 533) reported: "During the 1952 Paris Colloquium, I had Savage respond over lunch to a list of some 20 questions. His answers to each was incompatible with the basic axioms of his own theory." In the second edition of his *Foundations of Statistics*, Savage (1972, p. 103) explained that his immediate reaction was based on error. He argued that a different presentation of Allais' lotteries as shown in his Table 1 would not have trapped him in his immediate error.

For other literature on such anomalies which were summarized under the headlines *common ratio effect* and *common consequence effect*, see MacCrimmon (1967), Morrison (1967), Moskowitz (1974), Slovic and Tversky (1974), MacCrimmon and Larsson (1979), Kahneman and Tversky (1979), Conlisk (1989), MacDonald and Wall (1989), Battalio et al. (1990, p. 37), Starmer and Sugden (1991, 1993), Harless (1992a,b), Harrison (1994, p. 231), Burke et al. (1996), Beattie and Loomes (1997), Cubitt et al. (1998a), Fan (2002), Birnbaum (2004), and Blavatsky (2010).

²The common ratio effect was explained by way of the fanning-out hypothesis in a Marschak-Machina

Obviously, Φ' and Ψ' can also be established by two-stage lotteries $\Phi'' = (\Phi, \lambda; b, (1-\lambda))$ and $\Psi'' = (\Psi, \lambda; b, (1-\lambda))$. In the first stage, a lottery is played which accords a payoff of b with probability $(1-\lambda)$ and a lottery Φ or Ψ with probability λ .³ If the subject ignores the first stage, then the *isolation effect* or the *pseudocertainty effect*⁴ are at work and the subject decides as if only Φ and Ψ were at choice. The isolation effect and the pseudocertainty effect then induce subjects to satisfy *compound independence*

$$\Phi \succsim \Psi \Leftrightarrow \Phi'' \succsim \Psi'',$$

which was introduced by Segal (1990) and demands the independence axiom only for two-stage lotteries.

In general terms, the reduced form of Φ'' is given by the three-part lottery

$$\Phi''' = (a, \lambda p; b, \lambda(1-p); b, (1-\lambda)).$$

Reduction of compound lotteries holds if we always have $\Phi''' \sim \Phi'$.

Event splitting or *branch splitting* means that the probabilities of some payoffs are split up, so that the respective payoff is accorded with two or more probabilities which triangle. In particular, it is observed in the bottom right corner of the Marschak-Machina triangle. Some observations located it at the boundary of a Marschak-Machina triangle. This comes up to the *certainty effect*, which results from overweighing payoffs which are obtained with certainty; see Kahneman and Tversky (1979, p. 265), Tversky and Kahneman (1986, p. S266), and Conlisk (1989, p. 397). Having investigated both explanations, Conlisk (1989, p. 401) concluded: “The direction of the systematic effect favors the certainty effect hypothesis over at least the linear version of the fanning-out hypothesis.” Our paper shows that the common ration effect can be explained by coalescence.

³In his *Gains-Decomposition-Utility* (GDU) model, Luce (2000, pp. 200-2) proposed to arrange multiple-branch decision problems in terms of multiple-stage trees.

⁴Kahneman and Tversky (1979, p. 271) argue: “In order to simplify the choice between alternatives, people often disregard components that the alternatives share, and focus on the components that distinguish them ... This approach to choice problems may produce inconsistent preferences, because a pair of prospects can be decomposed in more than one way, and different decompositions sometimes lead to different preferences. We refer to this phenomenon as the *isolation effect*.”

If one of the second-stage lotteries has a payoff under certainty, we encounter a particular species of the isolation effect, viz. the *pseudocertainty effect*. This terminology was coined by Tversky and Kahneman (1986, pp. S267-68), “because an outcome that is actually uncertain is weighted as if it were certain”.

sum up to the original probability. Suppose $1 \geq p > r \geq 0$, $1 \geq p + r \geq 0$, then event splitting of payoff b is given by the three-part lottery

$$(a, p; b, r; b, (1 - p - r)).$$

The inverse operation, viz. unifying the split probabilities to a single probability for the particular payoff, is called *coalescing*. Note that there are many ways of event splitting, but only one way to coalesce events to one event with the respective compound probability. If coalescing holds, we have for this example

$$\Phi \succsim \Psi \Leftrightarrow (a, p; b, r; b, (1 - p - r)) \succsim \Psi.$$

Summarizing, we have:

1. Compound independence holds if $\Phi \succsim \Psi \Leftrightarrow \Phi'' \succsim \Psi''$.
2. Reduction of compound lotteries holds if $\Phi'' \succsim \Psi'' \Leftrightarrow \Phi''' \succsim \Psi'''$.
3. Coalescing holds if $\Phi''' \succsim \Psi''' \Leftrightarrow \Phi' \succsim \Psi'$.

If all three axioms hold, we have

$$\Phi \succsim \Psi \Leftrightarrow \Phi'' \succsim \Psi'' \Leftrightarrow \Phi''' \succsim \Psi''' \Leftrightarrow \Phi' \succsim \Psi'.$$

Logical ratiocination implies

$$\Phi \succsim \Psi \Leftrightarrow \Phi' \succsim \Psi',$$

and common ratio effects would be ruled out.

In other words, at least one of these axioms should be violated in order to generate a common ratio effect. The experiment in the present paper analyzes which failure of these axioms is concomitant with the empirical observation of common ratio effects. Similar analyses were performed by Cubitt et al. (1998a) and by Birnbaum (2004). Cubitt et al. (1998a, pp. 1364-5) investigated a five-step decomposition of the common ratio effect. Whereas our decomposition is basically static, their decomposition contains inter alia a precommitment lottery, i.e., a two-stage lottery such that subjects were required to choose

their option for the second stage before the initial lottery was resolved; they called that *dynamic choice*. They (p. 136) referred to orderly precommitment as *timing independence*. They tied down violation of timing independence as the major cause of the common ratio effect. According to Cubitt et al. (1998a, p. 1378), subjects may be tempted to make more risky decisions under precommitment or they may experience endogenous preference shifts which they failed to anticipate. Note that Cubitt et al. (1998a) had no lotteries with event splitting (and coalescing) in their experimental design. Also note that all theories for explaining the common ratio effect which they discuss in their Table 1 cannot explain splitting effects and, therefore, cannot reconcile our results. Birnbaum (2004) performed a similar analysis for common consequence effects and identified violations of coalescing as their major source. We will, in particular, investigate whether his results translate into common ratio effects.

Event splitting and coalescing effects were observed and investigated by Starmer and Sugden (1993), Neilson (1992), Humphrey (1995, 1996, 1998, 2001), and Birnbaum (1999,a, b, 2004). Birnbaum (2004) provided comprehensive theoretical and empirical analyses of the common consequence effect, in which he, inter alia, studied event splitting. Different evaluation of split events must be either traced to the associated probabilities or the utility of multiple events has to be adapted accordingly. Explaining the effects of event splitting thus presupposes either probability weighing or utility dependence on event frequency. In an early study on price determination, Birnbaum and Stegner (1979) found that buyers place more weight on the lower estimates and sellers on the higher estimates of value. These are expressions of risk aversion to prevent determining faulty prices.

Before embarking on our experiment, some remarks on experimental incentives are appropriate. Smith (1982) proclaimed a list of sufficient conditions for microeconomic experiments. We single out saliency and dominance. *Saliency* demands that subjects' rewards are increasing (decreasing) in good (bad) outcomes, and *dominance* demands that the rewards should dominate any subjective costs associated with participation in the activities of an experiment. This requires real rather than hypothetical payoffs unless subjects are interested in the substance of the experiment. As applied to Allais' experiments, this would mean in the strict sense that only persons like Bill Gates, Warren

Buffett, or George Soros could afford making respective experiments. There are several escapes from this impediment: first, the experimenter can rely on subjects who are so much interested in the particular experiment that they appreciate participation in the experiment higher than the subjective costs of it. Then hypothetical payoffs are appropriate. Second, the experimenter scales down the payoffs, and, third, the experimenter uses somewhat higher payoffs but resorts to the random-lottery incentive system.

With respect to Allais' lotteries all three methods were applied. Each one has pros and cons, but ideological attitudes are misplaced in this respect.⁵ We decided in favor of the second method following Conlisk (1989), Battalio et al. (1990, p. 37), Starmer and Sugden (1991), Harrison (1994, p. 231)⁶, Burke et al. (1996), Beattie and Loomes (1997), Cubitt et al. (1998a), and Fan (2002), to mention only some authors. Conlisk, Harrison, Burke et al., and Fan observed a dramatic reduction of Allais-type responses for comparatively small real payoffs. Conlisk (1989, pp. 401-3) provided two explanations for that: (i) subjects did "reason more carefully and thus discover the appeal of responses consistent with expected utility theory" (for this view cf. also Slovic (1969)), (ii) subjects

⁵It seems that Smith' (1982) precepts for orderly experiments had triggered a heated debate about financial incentives, and, among them, about the random-lottery incentive system. Comprehensive investigations by Camerer and Hogarth (1999) for 74 experiments showed that the superiority of financial incentives is not apodictic. Rather it depends on the subject matter of the respective experiments and the particular circumstances on whether financial or hypothetical payoffs are superior. Camerer (1995, p. 635) remarked:

The effect of paying subjects is likely to depend on the task they perform. In many domains, paid subjects probably *do* exert extra mental effort, which improves their performance, but in my view choice over money gambles is *not* likely to be a domain in which effort will improve adherence to rational axioms. Subjects with well-formed preferences are likely to express them truthfully, whether they are paid or not.

Davis and Holt (1993, p. 450) tend to endorse financial incentives. Even Smith and Walker (1993, p. 246), who carefully surveyed 31 experimental studies, are less apodictic than Smith (1982) in unconditionally endorsing financial incentives: "neither of the polar views—only reward matters or reward does not matter—are sustainable across the range of experiments."

⁶Beattie and Loomes (1997, p. 158) rightly remark that Harrison's "sample sizes are too small for statistical tests to discriminate between the differences."

tend to switch to the maximization of expected value, since there are no great fortunes at stake such as one million for sure. Alas, when administering hypothetical payoffs to his pilot subjects, Conlisk (1989, p. 406) observed the same drop in Allais-type behavior as for the treatment with real payoffs. This seems to suggest that it is small rather than real payoffs which caused Allais-type behavior to disappear.

As for our experiment, comparison of Experiments 1 and 3 shows distinct Allais-type behavior in spite of low and real payoffs as used in our experiment, which is in line with the results of Battalio et al. (1990, p. 37), Starmer and Sugden (1991), Beattie and Loomes (1997), and Cubitt et al. (1998a).

The third method was applied inter alia by Birnbaum (2004), who also observed Allais-type behavior.

Seminal work in comparing these methods was done by Starmer and Sugden (1991) and Beattie and Loomes (1997). Starmer and Sugden (1991) investigated the common consequence effect for real payoffs for all lotteries and for the random-lottery incentive system. For both treatments they evidenced significant Allais-type behavior. Beattie and Loomes (1997) investigated the common ratio effect (their Questions 1 and 3) under three treatments, viz. hypothetical payoffs (subjects received only a modest show-up fee), real payoffs under the random-lottery incentive system, and real payoffs for all lotteries. For all three treatments Beattie and Loomes (1997, p. 163, Table 1) observed a substantial common ratio effect with no significant differences among the three treatments. This result allowed them to state (p. 164) that “the salience hypothesis appears to be comprehensively rejected.”

2 Experimental Design

In the time between June 6th and June 11th, 2013, we distributed four types of questionnaires during the classes to 162 undergraduates of Kiel University. The questionnaires were entitled “Experiment 1” to “Experiment 4”. Subjects were told that they had to cross one of two options which they preferred and that they could earn up to €4.00 in

some ten minutes.⁷ After collection of the completed questionnaires, two marbles (with replacement after the first draw) were drawn from an urn containing 100 numbered marbles⁸ by two participants of the respective course, and payment in cash was immediately effectuated.

Each subject answered only one questionnaire. This means that we used a 1-in-1 payment protocol (using the terminology of Harrison and Swarthout (2012, p. 2)). This between-subjects method was vividly endorsed by Cubitt et al. (1998a, p. 1372). It presupposes that “risk preferences across subjects are the same” (Harrison and Swarthout (2012, p. 7)). This proviso gathers momentum if lotteries are administered to obviously different groups of subjects, e.g., youngsters and seniors, males and females, left- and right-wing sympathizers, etc. Then extensive pilot experiments would have to be conducted for making sure that risk preferences do not differ across subjects.⁹ However, such pre-tests

⁷At the time of the experiment one euro (€) amounted to some \$1.30. Our payments might appear moderate but €4.00 in ten minutes comes up to €24.00 per hour. When students job in Germany, they can usually make €10.00 per hour. Moreover, we distributed our questionnaires during undergraduate classes so that students had neither travel expenditures nor costs of time involved with the experiment. Since we addressed only economics classes, the majority of students showed great interest in economic experiments.

⁸We used this arrangement instead of differently colored marbles, as Birnbaum (2004) did, to avoid framing effects, although they are minor as evidenced by Birnbaum (2004, p. 99).

⁹As for respective methods, see, e.g., the impressive experiment carried out by Holt and Laury (2002). They started asking subjects to choose a lottery from the pair $L_1 = (\$2.00, 0.1; \$1.60, 0.9)$, $G_1 = (\$3.85, 0.1; \$0.10, 0.9)$. Obviously L_1 will be chosen. Then they increased the probability of gaining the higher payoff in both lotteries stepwise by 10 percent and decreased the probability of gaining the lower payoff by 10 percent, ending with $L_{10} = (\$2.00, 1; \$1.60, 0)$, $G_{10} = (\$3.85, 1; \$0.10, 0)$. Obviously G_{10} will be chosen. A risk-neutral subject will choose L four times before switching to G . Greater lottery indices of the switching point indicate greater risk aversion. For a more refined approach along these lines cf. Harrison et al. (2007, pp. 88-92). A similar method was earlier employed by Loomes and Sugden (1998). They arranged model lotteries taking expected utility as their core model in a series of Marschak-Machina triangles so that their gradients steadily increased rendering the riskier lotteries more attractive. Then they used the reversal frequencies as indicators of risk preferences.

Curiously enough, although these experiments *could* have been used to check homogeneity of risk preferences of subjects in different groups, to the best of our knowledge this research was never used for this purpose. Rather it was carried out under the implicit assumption that risk preferences were

“imply massive sample sizes for reasonable power, well beyond those of most experiments” (Harrison and Swarthout (2012, p. 7)). But such involved pre-tests seem dispensable if the groups of subjects were sampled at random from a homogenous population such as economics undergraduates in our experiment. Of course, we do not deny that risk preferences differ across subjects, but the probability that random sampling will in such cases create groups with systematically sharply different risk preferences is negligible although admittedly different from zero. Hence, we felt entitled to assume by and large identically distributed risk preferences across the four groups of our subjects. Using the 1-in-1 payment protocol ruled out possible distortions induced by random-lottery incentive mechanisms.¹⁰

We applied the following experimental design:¹¹

Experiment 1

□ Option A: If a marble numbered 1 to 100 is drawn, you receive €3.00.

□ Option B: If a marble numbered 1 to 80 is drawn, you receive €4.00, and if a marble numbered 81 to 100 is drawn, you receive €0.00.

the same across the group of all subjects used for these experiments. The aim of this research was to investigate how risk preferences vary as the payoffs used in the respective experiments are scaled up (cf. also Footnote 14).

¹⁰Such distortions have been recently reported by Cox et al. (2014a), Cox et al. (2014b), and Harrison and Swarthout (2012). For contrary results see Starmer and Sugden (1991), Beattie and Loomes (1997), Cubitt et al. (1998b), and Hey and Lee (2005).

¹¹Note that the following four experiments are based on the experimental design of Kahneman and Tversky (1979, p. 266, Problems 3 and 4, and p. 271, Problem 10). We divided their payoffs, which were stated in contemporary Israeli Pounds (at that time, the median net monthly income of a family was 3,000 Israeli Pounds) by 1,000 to receive the payoffs of our experiments in terms of €. The original paper by Allais (1953b, pp. 527) covered only the common consequence effect, not the common ratio effect.

Our experiment differs from the Kahneman and Tversky (1979) experiment in three important aspects: first, instead of presenting the probabilities straight in terms of numbers, we presented them in terms of draws of a marble from an urn. Carlin (1990) showed that this seemingly minor move has marked effects. Second, we used real rather than hypothetical payoffs. Third, we analyze the role of event-splitting effects.

Experiment 2

- Option A: If a marble numbered 1 to 25 is drawn, you receive €3.00, and if a marble numbered 26 to 100 is drawn, you receive €0.00.
- Option B: If a marble numbered 1 to 20 is drawn, you receive €4.00, if a marble numbered 21 to 25 is drawn, you receive €0.00, and if a marble numbered 26 to 100 is drawn, you receive €0.00.

Experiment 3

- Option A: If a marble numbered 1 to 25 is drawn, you receive €3.00, and if a marble numbered 26 to 100 is drawn, you receive €0.00.
- Option B: If a marble numbered 1 to 20 is drawn, you receive €4.00, and if a marble numbered 21 to 100 is drawn, you receive €0.00.

Experiment 4

For this experiment a first marble will be drawn, and, after replacement, a second marble will be drawn.

- Option A: If a marble numbered 1 to 75 is first drawn, you receive €0.00 and the experiment is terminated.

If a marble numbered 76 to 100 is first drawn, and the second marble drawn is numbered 1 to 100, you receive €3.00.

- Option B: If a marble numbered 1 to 75 is first drawn, you receive €0.00 and the experiment is terminated.

If a marble numbered 76 to 100 is first drawn, and the second marble drawn is numbered 1 to 80, you receive €4.00, and if the second marble drawn is numbered 81 to 100, you receive €0.00.

The focus of our investigation is subjects' response to the common ratio effect under the conditions of event splitting, coalescing, and compound lotteries. Moreover, we wanted to compare our experimental results with the results of other similarly structured experiments. Hence, we stuck to probability 1 for the scaled-up 'safer' option, since this was also characteristic for Allais' original lottery proposal and for the experimental design of the experiments of Kahneman and Tversky (1979), Conlisk (1989), Beattie and Loomes (1997), and Cubitt et al. (1998a), and because the common ratio effect was well documented for this case. We also availed ourselves of low and real payoffs to investigate whether the common ratio effect largely disappears as suggested by the experiments of Conlisk (1989), Harrison (1994, p. 231), Burke et al. (1996), and Fan (2002). This renders our results independent of whether the common ratio effect is mainly due to the certainty effect or to the bottom right corner of the Marschak-Machina triangle.

Comparison of the results of these experiments allows us six tests:

1. Comparing Experiments 1 and 2 tests event splitting for the common-ratio case (because Experiment 2 differs from Experiment 3 only by event splitting of Option B).
2. Comparing Experiments 1 and 3 tests coalescing for the common-ratio case (because Option B in Experiment 3 is just the coalesced Option B of Experiment 2; note that this case is the default case of the common ratio effect).
3. Comparing Experiments 1 and 4 tests compound independence (because Experiment 1 is the isolation of the first lottery in Experiment 4).
4. Comparing Experiments 2 and 3 tests coalescing (because Option B in Experiment 2 is the coalesced Option B in Experiment 3).
5. Comparing Experiments 2 and 4 tests event splitting and reduction of compound lotteries.
6. Comparing Experiments 3 and 4 tests coalescing and reduction of compound lotteries.

If coalescing and reduction of compound lotteries hold, then Experiments 2 to 4 are equivalent. If coalescing is violated, as the results of other studies suggest, then all comparisons involving coalescing, i.e., Experiment 3, should exhibit markedly different results from the other comparisons.

3 Empirical Results

It is interesting to see that after the bombshell of Allais (1953a,b), apart from more anecdotic amusement, it took more than a quarter of a century for systematic experimental work to come about. Indeed there was an early bird, viz. Ole Hagen's Heidelberg seminar paper of 1971, but the experimental age started only with Kahneman and Tversky (1979) and MacCrimmon and Larsson (1979) using hypothetical payoffs. Real payoffs were not used before Conlisk's (1989) work.

It is tempting to consider first the results of other experiments. Because of the congruence of the Kahneman and Tversky (1979, pp. 266, 268, and 271) experiments with Experiments 1, 3, and 4 dealt with in this paper, their results are particularly interesting. Translated into our notation, 80% of subjects preferred Option A in Experiment 1 to Option B, whereas 65% preferred Option B in Experiment 3 to Option A, which is strong evidence for the prevalence of the common ratio effect. For the analogue of Experiment 4, 78% preferred Option A to Option B, which evidenced the working of the isolation and pseudocertainty effects.

Let us also have a look at the Beattie and Loomes (1997, p. 163, Table 1) results. Recall that they investigated the common ratio effect under three treatments (hypothetical, random-lottery, and real payoffs) yielding quite similar results. Their design corresponds to the comparison of our Experiments 1 and 3. Translated to our Experiment 1, they observed between 85 and 88 percent choices of Option A and between 45 and 54 percent choices of Option B as translated to our Experiment 3. This documents a considerable common ratio effect. Note that, whereas the qualitative structure of the Beattie and Loomes (1997) results corresponds to our results, the shift in percentages may be due to

the higher level (in terms of 1997 purchasing power some five times of our payoffs) and the greater relative discrepancy of the Beattie and Loomes payoffs. This example illustrates that a statement such as “an axiom is rejected if more than x percent of the subjects do not behave in conformity with it” does not make much sense, since this depends also on the level of the payoffs. For instance, Battalio et al. (1990) worked with mean-preserving spreads for their experiment, and, in spite of that, observed Allais-type behavior. What matters is a statistically significant deviation of the respective choices in comparison to other choices, not some arbitrarily chosen value of x .

Table 1: Results of the Common Ratio Experiments

Experiment	Experiment 1		Experiment 2		Experiment 3		Experiment 4	
Option	#	%	#	%	#	%	#	%
A	20	48.78	18	45.00	11	26.83	20	50.00
B	21	51.22	22	55.00	30	73.17	20	50.00
Sum	41	100.00	40	100.00	41	100.00	40	100.00

The results of our experiments are presented in Table 1. Before interpreting these results, we have to test them for statistical significance of the pairwise equality or inequality of the choice probabilities.

For this purpose we employed a test for the equality or inequality of probabilities based on the normal distribution which is standard in statistical textbooks; it was also used by Conlisk (1989, pp. 393 and 404). He called it D statistic. For a large number of subjects, D approaches a standard normal distribution. Let V_i and V_j denote the fraction of subjects who chose Option B in Experiment i and j , respectively, and N_i and N_j the number of subjects who participated in Experiment i and j , respectively. Then the D statistic is computed by

$$(3) \quad D_{ij} = \frac{V_i - V_j}{\sqrt{\frac{V_i(1-V_i)}{N_i-1} + \frac{V_j(1-V_j)}{N_j-1}}},$$

where D_{ij} denotes the D statistic for comparing Experiments i and j . It is easily seen from (3) that the denominator of (3) is positive and symmetric for Options A and B. Moreover,

when changing the order of comparison, D changes sign, i.e., $D_{ij} = -D_{ji}$. Therefore, it is only $|D|$ which matters for the pairwise comparison of experimental results.

For instance, comparing V_1 and V_3 in this order, we get

$$D_{13} = \frac{0.5122 - 0.7317}{\sqrt{\frac{0.5122(1-0.5122)}{40} + \frac{0.7317(1-0.7317)}{40}}} = -2.08.$$

The values of the D test and the associated probability values for our experiments are shown in Table 2 for the one-sided test and in Table 3 for the two-sided test.

Table 2: Values of the D statistic and p values: one-sided test

	Experiment 2		Experiment 3		Experiment 4	
	D	p	D	p	D	p
Experiment 1	-0.34	0.367	-2.08	0.019	0.11	0.456
Experiment 2			-1.71	0.043	0.44	0.330
Experiment 3					-2.18	0.015

$|D|=1.65$ for the 5% significance level, $|D|=2.33$ for the 1% significance level.

Table 3: Values of the D statistic and p values: two-sided test

	Experiment 2		Experiment 3		Experiment 4	
	D	p	D	p	D	p
Experiment 1	-0.34	0.734	-2.08	0.038	0.11	0.912
Experiment 2			-1.71	0.087	0.44	0.660
Experiment 3					-2.18	0.029

$|D|=1.96$ for the 5% significance level, $|D|=2.575$ for the 1% significance level.

Table 1 suggests two hypotheses, viz. first $V_1 = V_2 = V_4$, and second $V_1 < V_3$, $V_2 < V_3$, $V_4 < V_3$. Testing these hypotheses requires a two-sided test for the first hypothesis and a one-sided test for the second hypothesis. Both tests have the same values for the test statistic, but different probability values associated with the values of the test statistic. Table 2 shows that the second hypothesis, $V_1 < V_3$, $V_2 < V_3$, $V_4 < V_3$, cannot be rejected

at the 5% significance level, and Table 3 shows that the first hypothesis, $V_1 = V_2 = V_4$, cannot be rejected at the 5% significance level. However, Table 3 shows also that $V_2 = V_3$ cannot be rejected at the 5% significance level.¹² Hence, by applying the appropriate tests for our hypotheses, we may consider both hypotheses as not rejected at the 5% significance level.

Comparing Experiment 1 with Experiment 3 documents the conventional common ratio effect: 48.78% of the subjects chose Option A in Experiment 1, but only 26.83% chose Option A in Experiment 3; in other words, 51.22% of subjects chose Option B in Experiment 1 and 73.17% of the subjects chose Option B in Experiment 3.¹³ These figures are not as impressive as the respective results of Kahneman and Tversky (1979), but we have to take into account that we worked with real and small payoffs rather than hypothetical and sizeable payoffs, which has possibly attenuated the strength of the common ratio effect.¹⁴ Moreover, rather than presenting the probabilities in terms of numbers, we presented them in terms of draws from an urn; Carlin (1990) observed that this framing, too, attenuates the common ratio effect. But our results show that the conventional common ratio effect cannot be rejected at the 5% significance level.

When comparing Experiments 1 and 2, it is interesting to see that the common ratio

¹²We also conducted a χ^2 -test (not reported here) which (as a two-sided test) fully confirmed the results of Table 3.

¹³Note that, as translated into our experiment, Cubitt et al. (1998a, p. 1375) observed that 38% of their subjects chose Option B in Experiment 1 and 48.1% chose Option B in Experiment 3. Obviously the majority of their subjects appreciated a 80% chance of getting £16 more than a certain payoff of £10, whereas only 51.22% of our subjects considered a 80% chance of getting €4 as preferable to a certain payoff of €3. This difference is well explained by the different level and spread of rewards. Note, however, that the response pattern of subjects is similar: whereas Cubitt et al. (1998a) observed 27% more B responses in Experiment 3 than in Experiment 1, our figures amount to 43% more B responses (see also Cubitt et al. (1998a, p. 1376, *H11-H12*)).

¹⁴Holt and Laury (2002, pp. 1648-50) observed major increases in risk aversion as the real payoffs of their model lotteries are scaled up. A follow-up experiment by Harrison et al. (2005) controlling for order effects confirmed Holt and Laury's results, but at a lower level of risk aversion. For further follow-up work see Holt and Laury (2005). Smith and Walker (1993, p. 259) found that real rewards reduce the variance of data around the predicted outcome.

effect by and large vanishes, although Experiment 2 differs from Experiment 3 only by a small event splitting of the worst payoff. Notwithstanding that slightly less (more) subjects chose Option A (B) in Experiment 1 than in Experiment 2, viz. 45% versus 48.78% (55% versus 51.22%), equality of these percentages cannot be rejected at the 5% significance level. Hence, coalescing provokes the common ratio effect in our experiment.

Comparing the results of Experiments 1 and 4 shows us that the percentages of subjects having chosen Options A and B, respectively, are virtually identical in both experiments.¹⁵ Equivalently to the Kahneman and Tversky (1979) and Carlin (1992) results, this shows us that the isolation and the pseudocertainty effects, too, work in our experiment perfectly: compound independence holds and, thus, the common ratio effect vanishes for the two-stage lottery of Experiment 4.

Comparing Experiments 2 and 4 shows that reduction of compound lotteries holds with our definition.

Comparing Experiments 3 and 4 shows that reduction of compound lotteries is significantly violated due to a failure of coalescing (since reduction of compound lotteries holds if coalescing is absent). Note that other authors implicitly assume coalescing and would test reduction by comparing Experiments 3 and 4.

It is interesting to see that event splitting produces an effect comparable to the isolation and the pseudocertainty effect: the probability split in Experiment 2 for the zero-payoff event into two portions, viz. into 5% and 75% probabilities, by and large caused the common ratio effect to disappear. Subjects seem to perceive multiple mention of unsatisfactory events to loom larger than unique mention of unsatisfactory events, even if their incidence happens to be the same. Hence, coalescing causes unsatisfactory events to be perceived as “less bad” as compared to their multiple appearance, even if their total effect is the same. This perceptual underestimation of unique, i.e. coalesced, bad events

¹⁵Note that there is a major difference between our results and the Cubitt et al. (1998a) results. Whereas we observe nearly 50% choices of Option B in Experiments 1 and 4, Cubitt et al. (1998a, p. 1375) (as translated into our experiments) observe 38% choices of Option B in Experiment 1 and 66% in Experiment 4. We have not explanation for the marked prevalence of risky choices in Experiment 4 in the Cubitt et al. (1998a) experiment.

seems to have caused the common ratio effect in our experiment. Without coalescing, we could not observe a common ratio effect. Combining our evidence with that of Birnbaum (2004), coalescing of split events explains Allais' paradoxes in both experiments.

4 Discussion

In his proposal of dual utility theory, Yaari (1987) showed that probability weighing is by and large equivalent to expected utility by replacing weighing of payoffs with weighing of probabilities.¹⁶ Although Yaari did not deal with event splitting, probability weighing is one way to explain Allais-type paradoxes due to event splitting. The other way of explaining Allais-type paradoxes due to event splitting is sticking to linearity in probabilities and considering the utility values of events to vary with the number of mentionings. This other way to explain Allais-type paradoxes was proposed by Neilson (1992) in his *expected cardinality-specific utility*. He explains that with peoples' preferences for fewer probable outcomes, i.e., reduction of complexity. We will refer to this approach as *frequency theory*.

Both approaches can command of rationales for their support. The case for probability weighing can be illustrated with an example due to Zeckhauser which was taken up by Kahneman and Tversky (1979, p. 283): you are compelled to play Russian roulette, but “are given the opportunity to purchase the removal of one bullet from the loaded gun. Would you pay as much to reduce the number of bullets from four to three as you would to reduce the number of bullets from one to zero?” Although in both cases the probability of death is reduced by one sixth, most people would agree that certainty of life deserved a higher price than just reducing the probability of death by one sixth without eliminating it altogether. The case for the frequency theory can be illustrated by reference to the habit of insurance companies to split very similar risks into sub-risks in their description of policies (Humphrey (2001, p. 92)). Mentioning the dangers of many instances of similar risks and silencing their small probabilities can lure people to enter into insurance contracts. The

¹⁶Seidl (2013) showed that probability weighing may, equivalently to payoff weighing, be used to “solve” or regain St. Petersburg paradoxes.

same applies to commercial lotteries which hype up exorbitant peak prizes and hush up their tiny probability. Outcomes rather than probabilities are moved into the forefront of the decision problem.

The investigation of probability preferences started with work by Edwards (1953, 1954a,b,c). Based on this earlier work, Edwards (1962, p. 127) proposed *subjectively weighed utility theory* by attaching weights to probability, which express “the relative desirability of undesirability of the probability displayed by that event”. Probability weighing was further employed by Kahneman and Tversky (1979). Birnbaum and associates developed *configural weight models*,¹⁷ of which the most advanced varieties are the *Rank-Affected Multiplicative Weights* (RAM) and the *Transfer of Attention Exchange* (TAX) models (see Birnbaum (1997, 1999a,b)). Birnbaum (1999b, 2004) showed that configural weight models may well be used for analyzing event splitting and coalescing effects to explain Allais-type paradoxes.¹⁸ Probability weighing for analyzing Allais-type paradoxes was also employed by Starmer and Sugden (1993) and Humphrey (1995, 1996).¹⁹

The frequency theory of explaining event-splitting effects was further developed and experimentally tested by Humphrey (1998, 2001). It has also shown ability of explaining Allais-type anomalies.

Note that these explanations of Allais-type anomalies are immaterial for the interpretation of the results of our experiment. They can as well result from negatively accelerated (i.e., subadditive) probability weighing functions, such that $w(p_1) + w(p_2) > w(p_1 + p_2)$, or from event utilities which depend on the frequency of equal outcomes. Under both approaches, low-payoff branches are more (less) heavily weighed, which lets the split (coalesced) shape of the gamble appear less (more) attractive than the gamble with coalesced

¹⁷Cf., e.g., Birnbaum et al. (1971, 1992), Birnbaum (1974), Birnbaum and Stegner (1979), and Birnbaum and Chavez (1997).

¹⁸Note that decision models based on weighed cumulative probabilities (e.g., rank dependent and rank and sign dependent utility theories as developed by Quiggin (1982, 1985, 1993), Luce and Fishburn (1991, 1995), and Tversky and Kahneman (1992)) cannot deal with event splitting because identical terms of cumulative probabilities for the same events cancel. see also Birnbaum (2008).

¹⁹Seminal work on probability weighing functions was done by Prelec (1998) and Gonzalez and Wu (1999).

(split) probabilities. By event splitting (coalescing), high-payoff branches are more (less) heavily weighed, which lets the split (coalesced) shape of the gamble appear more (less) attractive than the gamble with coalesced (split) probabilities. Note that this holds by virtue of the utility function $u(\cdot)$ which is obviously an increasing function of payoffs and possibly also of the number of equal outcomes. Negatively accelerated probability weighing or utility functions which depend on the frequency of identical events suffice to produce the results observed in this paper.

As applied to our experiment, event splitting of the worse outcome in Experiment 2 rendered Option B less attractive due to double mentioning of the worse event. This caused the common ratio effect to disappear. In our experiment, the common ratio effect is caused by coalescing the probabilities of split events. Birnbaum (2004, p. 105) concluded that splitting or coalescing of branches appears to give a good explanation of Allais' paradoxes.

5 Conclusion

This paper decomposed the common ratio effect into three separate properties, compound independence, reduction of compound lotteries, and coalescing, and tested these properties by a simple experiment with 162 undergraduates of Kiel University.

Our results show that the classical common ratio effect can be also observed in our data. While compound independence and reduction of compound lotteries hold, we observe a clear violation of coalescing. Combining our evidence with that of Birnbaum (2004), coalescing of split events seems to be a major explanation of violations of the independence axiom as manifested by Allais paradoxes. The same observation can be also extended to violations of reduction, as reduction clearly holds in our data if no coalescing is involved. Because of multiple mentioning, splitting of worse events causes these results to loom larger as compared to coalescing them.²⁰

²⁰As a résumé of their work, Starmer and Sugden (1993, p. 253) remarked: “Perhaps the most significant feature of our results is that they provide evidence of event-splitting effects that are inconsistent with almost all current theories of choice under [un]certainty.” Our research has hopefully added another piece

Reviewing theories of risky decision making, we found that configural weight theories or frequency theories are able to explain common ratio effects by violations of coalescing. Other popular alternatives to expected utility like rank-dependent utility and cumulative prospect theory explain common ratio effects by violations of compound independence. Such violations could, however, not be observed in our experiment.

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