Experiments on compound risk in relation to simple risk and to ambiguity*

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Abstract

We conduct experiments measuring individual behavior under compound risk, simple risk and ambiguity. We focus on: (1) treatment of compound risks relative to simple risks; and (2) the relationship between compound risk attitude and ambiguity attitude.

We find that compound risks are valued differently than corresponding reduced simple risks. These differences measure compound risk attitudes. These attitudes display more aversion as the reduced probability of the winning event increases.

Like Halevy (2007), we find an association between compound risk reduction and ambiguity neutrality. However, in contrast to the almost perfect identification in Halevy (2007)'s data, we find a substantially weaker relation in both directions. First, a majority of our ambiguity neutral subjects fail to reduce compound risk. Second, almost a quarter of our subjects who reduce compound risk are non-neutral to ambiguity. All of the latter come from the more quantitatively sophisticated part of our subject pool.

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1 Introduction

We experimentally investigate behavior toward compound risk and its relation with behavior toward simple risk and toward ambiguity (i.e., subjective uncertainty about probabilities). In

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classical theories of decision making, such as expected utility (Savage (1954)), compound risk is reduced to simple risk by multiplication of probabilities and ambiguity is reduced to simple risk through assignment of subjective probabilities. Thus, behavior toward all three types of uncertainty is tightly linked in classical theory. We examine binary prospects and focus on two issues: (1) the treatment of compound risks relative to the corresponding reduced simple risks and how this varies with the probability of the winning event; and (2) the relationship between compound risk attitude and ambiguity attitude, especially between reduction of compound risk and ambiguity neutrality and whether this relationship depends on the quantitative background of the subjects.

Regarding the first issue, there has been little direct examination of how aversion toward compound risk relative to simple risk changes with the reduced probability of the winning event. In our view, the lack of focus on behavior toward compound risk is surprising in light of the low (if any) cost of presenting risk as compound versus simple to an economic agent. To the extent that behavior differs substantially toward compound risks as compared to simple risks, the marketing, policy and economic implications of understanding this behavior may be large. For example, if individuals dislike compound risk, then sellers of risky assets or products would benefit from presenting the risk in its reduced form.

We compare the certainty equivalents recorded from subjects' choices under simple risk and two types of compound risks. We find that compound risks are valued differently than corresponding reduced simple risks. These differences measure compound risk attitudes. A main novel finding is that individuals become more compound risk averse as the reduced probability of the winning event increases. Such increasing aversion is consistent with greater insensitivity to changes in probability under compound risk compared to simple risk. Likelihood insensitivity is a central concept in the behavioral decision theory literature building on prospect theory (Fox and Tversky (1995), Tversky and Wakker (1995), Wakker (2010)). Comparative insensitivity across sources has been investigated both theoretically and experimentally (Einhorn and Hogarth (1985), Abdellaoui et al. (2011a)), but not for compound risk.

Turning to the second issue, most theories of decision making under ambiguity do not have any particular implication for the connection between behavior toward ambiguity and behavior toward compound risk. However, some prominent recent theories explicitly connect the two. For example, Segal (1987) and Seo (2009) present theories equating compound risk reduction with ambiguity neutrality.[1] Halevy and Ozdenoren (2008)'s theory does not equate the two but says that compound risk reduction implies ambiguity neutrality. This implication, if valid, may have a large impact on the way ambiguity attitudes are viewed. If one sees failure to reduce compound risk as a departure from rationality, then according to these theories, ambiguity non-neutrality must be such a departure as well. In contrast, if compound risk and ambiguity are less tightly linked, there is room to reach separate normative and prescriptive conclusions concerning each.

Few papers have empirically examined the link between attitude towards ambiguity and attitude towards compound risk. There appear to be two main (and contradictory) empirical

[1] An early precursor to these theories is Kahneman and Tversky (1975, pp.30-33). As well, early empirical literature on ambiguity commonly assumed it to be operationally identical to compound risk (Becker and Brownson (1964), Yates and Zukowski (1976), Kahn and Sarin (1988)).

contributions dealing with this relationship. Bernasconi and Loomes (1992) use a compound risk version of Ellsberg (1961)'s three color urn and, based on their finding of less Ellsberg type behavior than typically found under ambiguity, conclude:

> "(...) Our findings that 'ambiguous lotteries' in the sense of Ellsberg cannot be fully characterized by 'distributed lotteries' as suggested by Segal also undermine the possibility of viewing ambiguity aversion and risk aversion as 'the two sides of the same coin'." (p. 91)

More recently, Halevy (2007) finds that the two attitudes are related. In fact, Halevy goes even further and suggests that non-reduction of compound risk is *necessary* for non-neutral attitude towards ambiguity:

> "(...) subjects who reduced compound lotteries were almost always ambiguity neutral, and most subjects who were ambiguity neutral reduced compound lotteries appropriately." (p. 531)
>
> "(...) The results suggest that failure to reduce compound (objective) lotteries is the underlying factor of the Ellsberg paradox, ..." (p. 532)

An advantage of Halevy (2007)'s design is its allowance for within-subject comparisons. We build on this important prior work and further explore the relationship between attitudes towards ambiguity and compound risks using a design that similarly allows for within-subject comparisons. Additionally, to test for possible influence of subjects' quantitative sophistication and background on the relationship, our subject pool was divided among advanced engineering students at an elite institution and students from a cross-section of non-engineering fields and institutions.

Like Halevy (2007) and Dean and Ortoleva (2012), we find an association between compound risk reduction/non-reduction and ambiguity neutrality/non-neutrality. However, unlike the almost perfect identification of compound risk reduction with ambiguity neutrality in Halevy (2007)'s data, we find a substantially weaker relation in both directions. With regard to ambiguity neutrality implying reduction of compound risk (as in the theory of Seo (2009) and as supported in Halevy (2007)'s data), we find less support among both engineers and non-engineers for this hypothesis. In fact, the majority of ambiguity neutral subjects do not reduce compound risk. With regard to reduction of compound risk implying ambiguity neutrality, we find less support for this among engineers while obtaining support similar to that in Halevy (2007) among non-engineers. For the engineers, compound risk reduction appears compatible with non-neutral attitudes toward ambiguity.

Section 2 details the experimental design of the two main studies (Studies 1 and 2). Section 3 describes and analyzes the observed behavior of subjects toward compound risk in relation to simple risk using Study 1. Section 4 discusses the findings from Study 2 on the relationship between ambiguity and compound risk attitudes. It also includes comparison with an additional study (Study 3), to address whether the type of compound risk considered affects the nature of the relationship. Section 5 concludes. Appendices contain additional detail concerning the experimental procedures and some supplementary analysis.

2 Experiments

2.1 Procedure

Each study involves binary bets with a winning payoff of 50 euros and a losing payoff of 0 euros. In Study 1, we consider bets under simple risk and two types of compound risk. For each type of bet we examined three different probabilities of winning – 1/12, 1/2 and 11/12. In Study 2, we consider bets under simple risk, three compound risks and ambiguity. All of the bets in Study 2 are symmetric (probability 1/2 of winning for simple and compound risks; symmetric information about two colors for ambiguity).

Bets were displayed on a computer monitor one at a time. Simple risk, compound risk and ambiguity were each represented by Ellsberg-like urns containing colored balls. Subjects had to consider bets whose outcome depended on which color(s) were drawn from the urn(s). For the simple (resp. compound) risk, the subjects saw the color of the balls in one (resp. two successive stages of) urn(s), and thus could infer the probability of winning the bet. Section 2.3 describes and motivates the different types of compound risks. For ambiguity, the colors of the two balls in the urn were hidden by making the urn opaque. Section 2.2 describes how the composition of the ambiguous urn was determined. The bets used in Study 1 are displayed in Figure 1. Similarly, Figure 2 displays the bets used in Study 2. We elicited certainty equivalents for each bet using an iterative choice list procedure adapted from Abdellaoui et al. (2011a). For each bet, subjects make choices between the bet and (an ascending range of) sure payments. See Appendix C.1 for details and sample screen images of this choice list procedure.

The experiments were organized as follows: A group of subjects entered the room and each subject was assigned a cubicle with a computer monitor and mouse. Once everyone in the group was seated, the instructions were displayed on each screen and the experimenter read them aloud. After the instructions, the subjects were allowed to ask questions of the experimenter in case there was anything they did not understand. The computer then randomly assigned each subject to an order treatment. There were two order treatments for Study 1, determining whether a subject faced risk first or compound risk first, and six order treatments for Study 2, determining the sequence among risk, ambiguity and compound risk.[2] Within each type of bet, the order of presentation was also randomized. Next came a training phase (see Appendix C.2) in which each subject is presented with the first stimulus she will face and guided through the choice list comparisons she will have to make and how payment will be determined, but not yet making choices that count. After this training, the experiment proper began, with bets presented sequentially and choices recorded. Copies of the instructions for Studies 1 and 2 are provided in Appendix A and Appendix B, respectively. At the end of the experiment, payments were made using a random incentive system described in Section 2.4. Subjects were additionally paid a show-up fee of 5 euros.

[2]Although the order was intended to be assigned in an exactly balanced way, we ended up with a slight imbalance of subjects across order treatments due to the fact that for a few subjects the computer froze at the instruction screen and needed to be rebooted thus assigning them to the next order treatment.

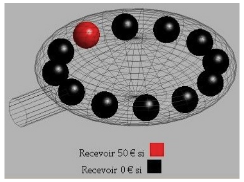
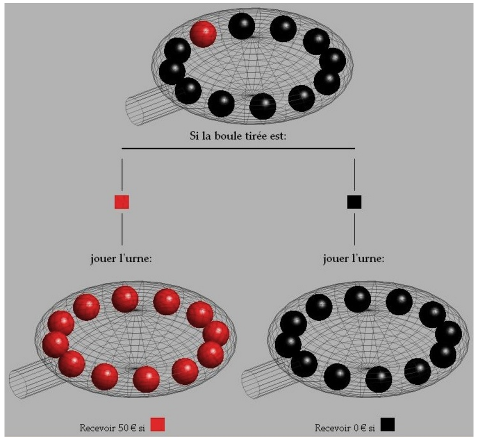
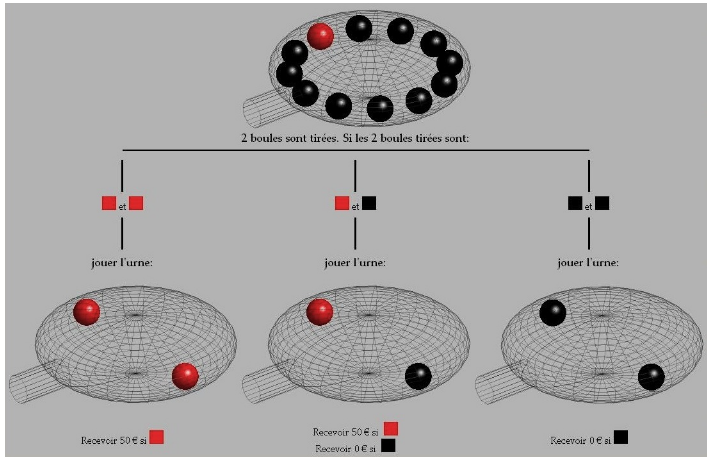
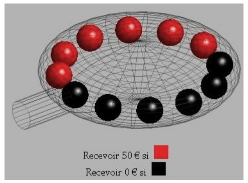
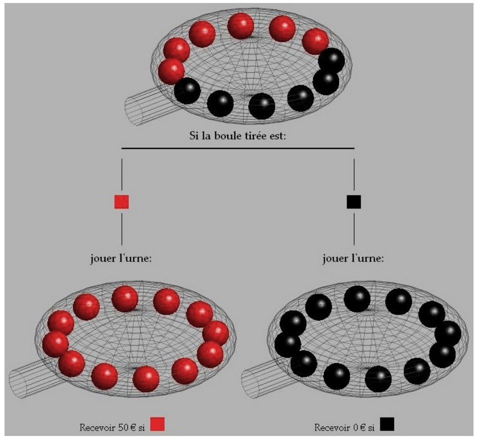
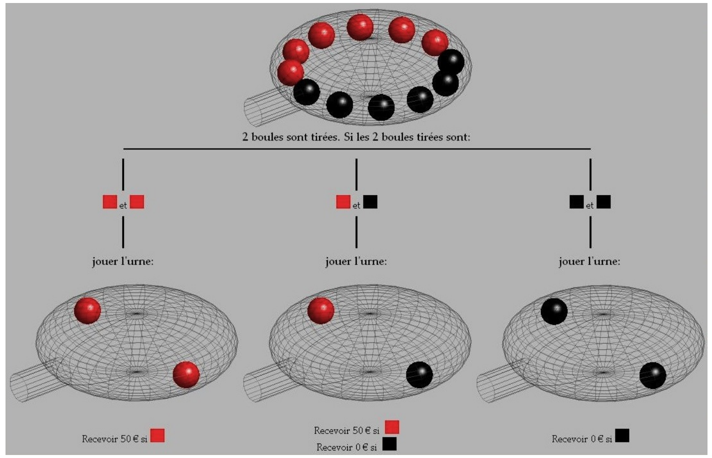
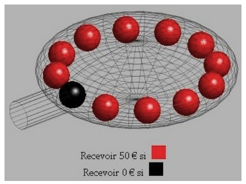
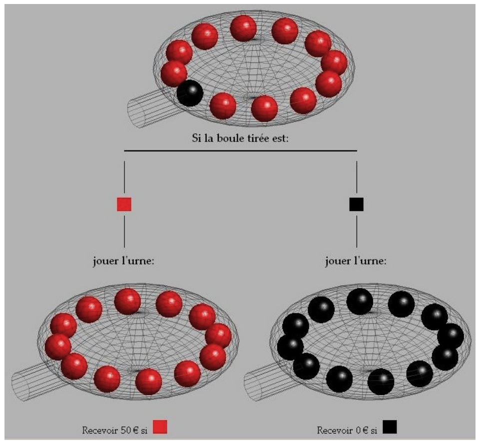
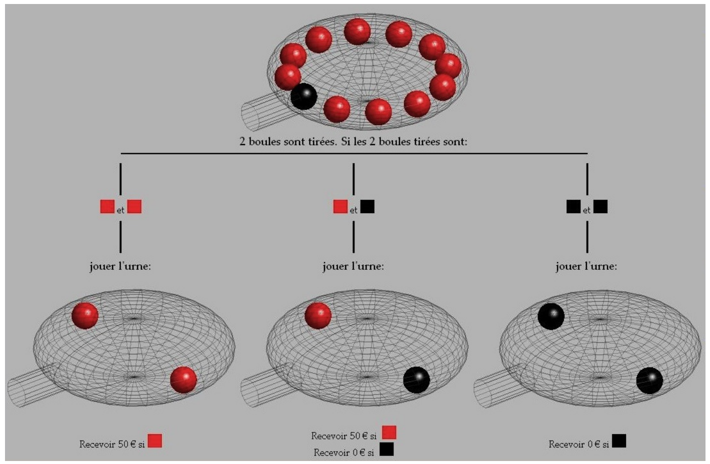
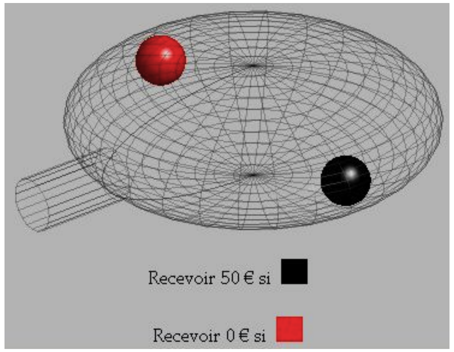
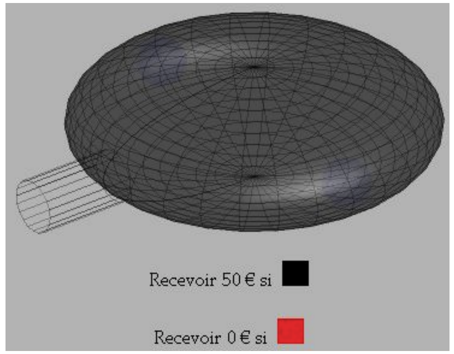
Probability of winning	Simple risk (R)	Compound risk with degenerate second stage (CRD)	Hypergeometric compound risk (CRG)
1/12			
1/2			
11/12			

Figure 1: Bets faced by a subject participating in Study 1 (shown here with red as the winning color) organized by probability of winning and by type of risk

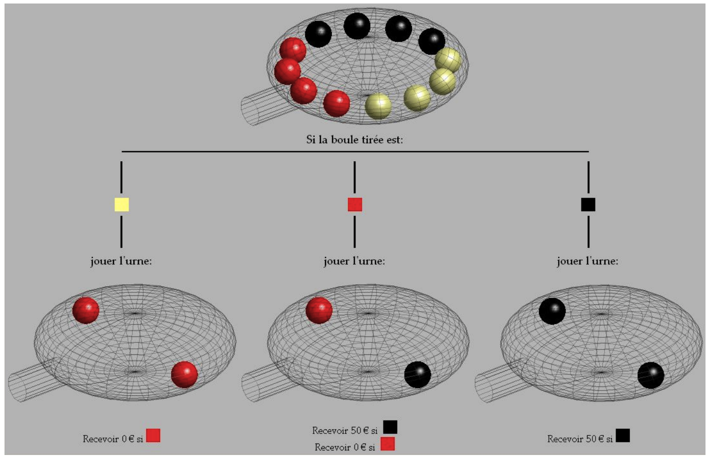
Simple risk (R)



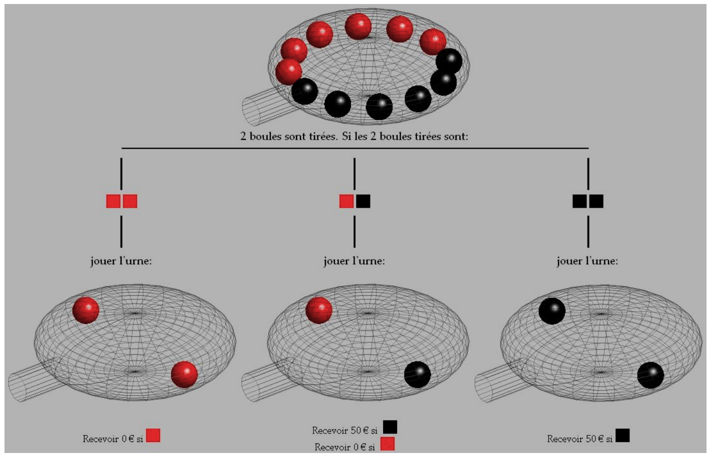
Ambiguity (A)



Diverse uniform compound risk (CRU)



Hypergeometric compound risk (CRG)



Compound risk with degenerate second stage (CRD)

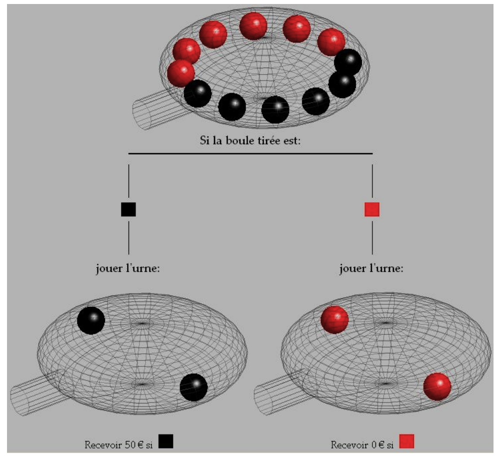


Figure 2: Bets faced by a subject participating in Study 2 (shown here with black as the winning color)

2.2 Ambiguity implementation

The ambiguous urn for Study 2 was generated through a two-stage process involving a physical bag of balls. During the reading of the instructions, one of the participants is asked to generate the ambiguous urn by drawing (in front of everyone) two balls from a bag containing 12 balls. The subjects were told that the 12 balls could be of the two possible colors (red and black), but were not informed of the distribution of colors (the actual distribution was 6 red and 6 black). The balls were covered so that the colors of the two drawn balls could not be seen by either the experimenter or the subjects until the end of the experiment. These two balls are placed in a bag representing the ambiguous urn. Near the end of the instructions for Study 2, the computer prompts each subject to choose the color that will be associated with the 50 euro prize for the bets they will be presented with.

This process has three main advantages. First, the final color composition of the ambiguous urns is unknown to both the subject and the experimenter, reducing possible comparative ignorance effects (Fox and Tversky (1995)). Second, this process may induce a two-stage representation of ambiguity in subjects (however, it does not guarantee that subjects used such a representation); hence, one cannot argue that possible similarities in attitudes towards two-stage compound risk and ambiguity are undermined by the way ambiguity has been implemented. If anything, our set-up may be biased in the direction of making such a connection stronger. Third, allowing subjects to choose which color is associated with the winning payoff reduces possible suspicion effects (Hey et al. 2010). A possible disadvantage of allowing the choice of color is that it may bias findings against ambiguity aversion for those subjects who view randomizing over choice of color as a hedge against ambiguity (Raiffa (1961), Dominiak and Schnedler (2011)). Color choice may also bias upward valuation of all bets through an illusion of control (Langer (1975)).

2.3 Choice of compound risks

The types of compound risk used in our studies and our motivation for including them are explained here. In compound risk with degenerate second stage (CRD), all risk resolves in the first stage since each possible second stage urn contains only one color. CRD was included in Study 1 to test for violations of time neutrality (Segal (1987)), i.e., for a pure effect of risk resolving in the first stage versus the second stage. In diverse uniform compound risk (CRU), the first stage generates a uniform distribution over all possible second stage urns. Both CRU and CRD were included in Study 2 to mimic the two compound risks used in Halevy (2007). In hypergeometric compound risk (CRG), two balls are drawn (without replacement) from the first stage urn to form the contents of the second stage urn. We refer to such risk as hypergeometric because the number of red balls in the second stage urn follows a hypergeometric distribution. CRG (included in all studies) was designed to be similar to the process used to generate the ambiguous urns in our study – in both cases balls are drawn (without replacement) from a larger urn and placed in a new urn, a draw from which determines the outcome of the bet. This makes CRG an interesting candidate as a potential model a subject might associate with the ambiguous urn.

2.4 Incentives

Subjects performed the choice tasks knowing that, at the end of the session, one task for each subject would be randomly drawn and the choice the subject had indicated for this task would be implemented and paid. If the subject had chosen a (simple or compound) risk, the corresponding urn(s) were created and the subject physically selected the ball(s) that determined the payment. If the subject had chosen an ambiguous urn, he/she physically drew the ball that determined the payment from the 2-ball urn which was generated at the beginning of the experiment. If the subject had chosen a sure amount, that amount was paid. Thus, each subject's total payoffs ranged between 0 and 50 euros plus the 5 euro show-up fee.

It is worth noting that paying according to a random-lottery incentive system (RIS) has been theoretically criticized (Holt (1986)). A key issue is whether subjects treat the entire experiment as one grand compound lottery/ambiguity, and so, because they may violate independence and/or compound reduction properties, choose differently in sub-problems than they would if presented with that sub-problem alone. If subjects, instead, treat each choice in isolation, then a RIS introduces no distortions. Experimental evidence on these points is mixed, but is generally supportive of RIS in practice and the use of RIS is widespread. See Starmer and Sugden (1991) and Cubitt et al. (1998) for supportive evidence and Harrison et al. (2013) for a finding of some distortion. Wakker (2007) is an informative discussion of the issue.

2.5 Sample

As indicated in Table 1, 209 subjects are included in our sample – 124 were students from Arts et Métiers ParisTech (an elite french graduate engineering school) and 85 were university students recruited from across non-engineering fields through the lab recruitment system (LEEP: Laboratoire d'Economie Expérimentale de Paris).[3] What distinguishes engineers and non-engineering students is that the former are intensively quantitatively trained and highly selected on academic ability while the latter are less quantitatively trained and vary in the selectivity of their courses of study and institutions. Each subject participated in only one of the two studies.

Sample	Study 1	Study 2	Total
Engineers	49	75	124
Non-engineers	45	40	85
Total	94	115	209

Table 1: **Pool of subjects**

[3]These numbers do not include those subjects who ever violated dominance by preferring 0 euros to a bet or preferring a bet to 50 euros, or who ever violated monotonicity by preferring a lower sure amount to the bet but the bet to a higher sure amount. In Study 1, there were 8 such subjects, while in Study 2, there were 2. These 10 subjects were not included in any of our calculations or results.

2.6 Study 3

An additional experiment (Study 3) is described in Appendix E. Data from this study contributes to the analysis in Section 4.1.3.

3 Valuation of compound risk in relation to simple risk

3.1 Attitudes toward compound risk

One of the main assumptions of most economic models of decision making under risk is that the nature and the complexity of a lottery should not affect its evaluation. Most models implicitly or explicitly incorporate a reduction of compound lotteries axiom requiring a decision maker to value a simple lottery exactly as any compound lottery with the same reduced probabilities. Table 2 reports statistics on the observed certainty equivalents for the simple and compound risks in Study 1.

According to theories assuming reduction, within each winning probability treatment, and for each subject, the certainty equivalents for the three bets should be equal. A repeated measures MANOVA analysis rejects even the weaker hypothesis that within each probability treatment the average certainty equivalents for the three bets are equal ($p < 0.05$). Thus, our data rejects reduction of compound lotteries. Miao and Zhong (2012) also find evidence of non-reduction of compound lotteries whereas Harrison et al. (2013) find mixed evidence on reduction.

Given non-reduction, it is interesting to investigate attitudes toward compound risk by comparing certainty equivalents between compound risk treatments and a simple risk treatment giving the same reduced probability, as such a comparison controls for attitudes toward simple risk. For this purpose, define the compound risk premium (for a given compound lottery) as the certainty equivalent for the corresponding reduced lottery minus the certainty equivalent for the compound lottery. We say a subject is compound risk averse/neutral/seeking when the compound risk premium is positive/zero/negative. On average (across probability treatments and the two compound risks), our subjects are compound risk averse (mean = 1.27, SD = 8.55, MANOVA $p < 0.01$). Average time neutrality (indifference to risk resolving entirely in the first stage of two rather than in one stage) is not rejected (the average compound risk premium for CRD is 0.663, t-test $p = 0.17$).

Importantly, there is systematic variation in compound risk premia across probability treatments. The first row of Table 3 reports the average compound risk premia by probability treatment (pooled across the two compound risks). Notice that these average premia appear to increase with the probability of winning. Furthermore, using within-subject variation, there is strong evidence that compound risk premia increase with the probability of winning (Page's L test for increasing trend, $p < 0.01$).[4]

[4] Page's L test (Page, 1963) has as its null hypothesis that, for each subject, all orderings of premia across treatments are equally likely (this description ignores treatment of ties in orderings, which the test itself accounts for). The alternative is that higher treatments tend to be assigned higher ranks. An advantage of this test is that it does not make parametric distributional assumptions and, most importantly, accommodates heterogeneity in distributions of premia across subjects since only within-subject rankings are used.

Winning probability ⟶	Sample size (n)	**1/12**			**6/12**			**11/12**		
Expected value (EV)		4.17			25			45.83		
Certainty equivalents (in euros) ↓		Mean	Median	SD	Mean	Median	SD	Mean	Median	SD
Risk (**R**)	94	9.91	5.50	9.97	22.65	24.50	9.23	37.74	40.50	11.29
Compound risk with degenerate second stage (**CRD**)	94	9.85	5.50	9.35	22.17	24.50	9.85	36.30	40.50	12.39
Hypergeometric compound risk (**CRG**)	94	9.25	5.50	8.30	20.72	22.50	9.34	34.72	39.00	12.36
Pooled across R, CRD and CRG	282	9.67	5.50	9.20	21.85	24.50	9.47	36.26	40.00	12.04

Table 2: **Certainty equivalents of simple risk and compound risks**

Recent literature has not investigated the relationship between probability of winning and compound risk aversion. An older experimental literature examining ambiguity operationalized as compound risk could be interpreted as providing some evidence for the pattern of more compound risk aversion for high probabilities than for low probabilities (Kahn and Sarin (1988)) and some not supporting that pattern (Larson (1980)). Evidence that compound risk seeking is common for low probability levels is found in Friedman (2005) who examines only low probability compound lotteries (with winning probabilities ranging from 0.0625 to 0.5625) and finds that individuals tend to value the compound lotteries more than their reduced simple risks. It is also found in Kahn and Sarin (1988). See Budescu and Fischer (2001) for additional studies and discussion of behavior toward compound lotteries.

This pattern of becoming more averse as winning probabilities increase has been previously identified for other types of uncertainty including simple risk (See the references in Wakker, 2010, footnote 2, p.204 as well as Table 9 in Appendix D showing that Study 1 finds this pattern for simple risk) and ambiguity (Tversky and Fox (1995), Abdellaoui et al. (2011a)). The most prominent explanation in the literature of such patterns is based on insensitivity to changes in probabilities (Wakker, 2010, chapter 7). In this light, our findings could be interpreted as evidence that when moving from simple to compound risk, subjects become more insensitive to changes in the winning probability and this leads them to effectively discount the increases from 1/12 to 1/2 and from 1/2 to 11/12.

3.2 Sensitivity analysis: Effects of background, order and type of compound risk

How sensitive are these patterns in compound risk aversion/insensitivity to different varieties of compound risk and to difference in the technical sophistication of subjects? Relatedly, do these patterns depend on whether one is first presented with simple risk or not?

Starting from the average compound risk premia pooled across the two types of compound risk, across order and across engineering and non-engineering subjects, and then disaggregating, the average premia in Table 3 may seem to suggest that the pattern of increasing compound risk aversion with probability treatments is driven primarily by the less quantitatively sophisticated (non-engineering) subjects who were presented with compound risk before simple risk. However, these are only cross-subject averages. The more important pattern of increasing compound risk aversion is the within-subject pattern. Here, Page's L test for increasing trend, which strongly indicated an increasing within-subject trend on the pooled data, continues to show within-subject increasing trend for both engineers and non-engineers ($p < 0.05$ and < 0.01, respectively). Within both groups of subjects and for both compound risks, however, the increasing trend appears significant only for subjects who were presented with compound risk first ($p < 0.05$, < 0.05, < 0.01, < 0.1, respectively).

Winning probability ⟶	**Sample size** n	**1/12**	**1/2**	**11/12**	**Page's L test p-value for within-subject increasing trend**
Average compound risk premia[a]	94	0.36	1.20**	2.23**	**0.000*** **
Average premia for CRG		0.66	1.93***	3.02**	**0.006*** **
engineers	49	1.18	0.29	1.92	**0.133**
non-engineers	45	0.09	3.72***	4.23**	**0.007*** **
Average premia for CRD		0.06	0.48	1.45	**0.162**
engineers	49	-0.51	0.00	0.31	**0.059* **
non-engineers	45	0.69	1.00	2.69	**0.583**
Average CR premium engineers		0.34	0.14	1.11	**0.012** **
R presented first	27	1.06	1.57*	0.31	**0.170**
CR presented first	22	-0.55	-1.61	2.09	**0.001*** **
Average CR premium non-engineers		0.39	2.36***	3.46**	**0.007*** **
R presented first	22	1.84	2.45**	1.36	**0.440**
CR presented first	23	-1.00	2.26*	5.46*	**0.000*** **
Average CR premium if R presented first		1.41**	1.97***	0.79	**0.210**
engineers, CRG	27	2.74**	2.04	2.37	**0.500**
engineers, CRD	27	-0.63*	1.12*	-0.29	**0.472**
non-engineers, CRG	22	1.59	4.05***	3.32	**0.225**
non-engineers, CRD	22	2.09*	0.86	-1.74	**0.959**
Average CR premium if CR presented first		-0.78	0.37	3.81**	**0.000*** **
engineers, CRG	22	-0.73	-1.86	1.36	**0.048** **
engineers, CRD	22	-0.36	-1.36	2.82	**0.012** **
non-engineers, CRG	23	-1.35	3.39**	5.09	**0.003*** **
non-engineers, CRD	23	-0.65	1.13	5.83*	**0.081* **

Table 3: **Average compound risk premia by probability treatment, sophistication and order effect**

[a]***, ** and * represents significance at a 1, 5 and 10% level respectively.

When a pattern occurs with one ordering but not the other, a natural question is whether the pattern remains evident in a setting where just one type of bet is presented. Arguably, the only certainty equivalents in our study reflecting such an evaluation are those for the type of bets the subjects see first. Is the same pattern of increasing compound risk premia present if only these certainty equivalents are used? Since it is impossible for any subject to see both simple and compound risk first, some cross-subject comparisons are necessary to address this question. With this caveat in mind, Table 4 shows the compound risk premia based on differences in average certainty equivalents (average certainty equivalent for simple risk when simple risk is seen first minus average certainty equivalent for compound risks when compound risk is seen first) by probability treatment. The increasing pattern is strongly evident (one-tailed t-test for

1/2 vs 1/12, $p < 0.01$; one-tailed t-test for 11/12 vs 1/2, $p < 0.01$).[5] This supports the notion that insensitivity/increasing compound risk aversion across probability treatments is a genuine aspect of subjects' compound risk evaluations and not merely a product of order effects.

	1/12	**1/2**	**11/12**
Average cross-subject premia for compound risk	-0.92	3.34	8.33

Table 4: **Average certainty equivalent for simple risk when simple risk is seen first minus average certainty equivalent for compound risks when compound risk is seen first**

3.3 Summary

A key finding is that increases in the probability of the good outcome increase aversion toward compound risk. In other words, raising the probability of winning increases the valuation of compound risk by less than it does for the corresponding simple risk. This, together with prior results relating to simple risk, highlights the importance of controlling for probability when measuring risk aversion. It also suggests that any descriptive model intended to apply to the full range of risky situations must allow aversion to increase with probabilities.

4 The relationship between compound risk attitudes and ambiguity attitudes

4.1 Relating reduction of compound risk and ambiguity neutrality

Segal (1987) (as well as antecedents such as Becker and Brownson (1964), Kahneman and Tversky (1975, pp. 30-33), Yates and Zukowski (1976) and others) suggests that ambiguous bets can be represented as a two-stage risk, where the first stage lottery describes the probabilities of getting various lotteries in the second stage. These models rely on the hypothesis of non-reduction of two-stage lotteries to generate ambiguity sensitive behavior. More recent theories explicitly using violation of reduction of compound lotteries to model ambiguity attitudes include Halevy and Ozdenoren (2008) and Seo (2009).[6] In these papers, reduction of compound risk implies neutrality to ambiguity. Non-reduction of compound lotteries is this strand of literature's explanation of Ellsberg type behavior.

[5]These tests use the within-subject increases in certainty equivalents across probability treatments for the type of bet seen first. The alternative hypothesis is that the population average within-subject increases are smaller for the population seeing compound risk first. This is equivalent to the population versions of the premia in Table 4 increasing with probability treatment.

[6]We highlight these papers as they explicitly include objective compound lotteries and have clear implications for the relationship between behavior toward compound lotteries and behavior toward ambiguity. A number of related models, including Klibanoff et al. (2005), Nau (2006), Ergin and Gul (2009) and Neilson (2010) do not include objective compound lotteries among the objects of choice and do not rely on non-reduction of such lotteries to generate ambiguity sensitivity.

4.1.1 A comparison with Halevy (2007)

The strongest and most striking evidence in Halevy (2007) for the identification of ambiguity attitude with compound risk attitude is a contingency table relating neutrality/non-neutrality towards ambiguity and reduction/non-reduction of compound risk. All bets in Halevy (2007) either have objective probability 1/2 of winning or, in the case of ambiguity, win if one of the two possible colors is drawn. Accordingly, in Study 2, we present subjects with a 2-ball risky urn, a 2-ball ambiguous urn, and three compound urns each having reduced probability one-half of winning. Four of the five bets are analogous to the four bets used by Halevy (2007) (a minor difference is that our final stage urns contain two balls rather than ten). The fifth (hypergeometric compound risk) was included because it is a plausible model of the actual process used to generate and draw from the ambiguous urn.

In Table 5, we construct a contingency table for our data from Study 2 alongside the table reported by Halevy. Subjects are classified as reducing compound risk if the reported certainty equivalents for the simple risk and each of the compound risks are equal. Subjects are classified as ambiguity neutral if the reported certainty equivalents for the simple risk and for the ambiguous bet are equal. In the table, "Count" indicates the number of subjects in each category while "Expected" indicates the number of subjects in each category expected if the joint distribution over cells is equal to the product of the observed marginal distributions (e.g., $4.5 = (28/142) * (23/142) * 142$).

			Compound risk attitudes					
			Halevy (2007)			**Study 2**		
			Reduce	**Do not reduce**	**Total**	**Reduce**	**Do not reduce**	**Total**
Ambiguity attitudes	**Neutral**	Count	22	6	28	13	17	30
		Expected	4.5	23.5		4.4	25.6	
	Non-neutral	Count	1	113	114	4	81	85
		Expected	18.5	95.5		12.6	72.4	
		Total	23	119	142	17	98	115
Fisher's exact test p-value (2-tailed)			0.000			0.000		

Table 5: **Contingency table relating ambiguity and compound risk attitudes**

Both sets of data show a relationship between reduction/non-reduction and ambiguity neutrality/non-neutrality. While Halevy's data suggests something close to identification in the direction of reduction implying neutrality – conditional on reducing compound risk, one out of 23 subjects is non-neutral toward ambiguity, while conditional on ambiguity neutrality, 6 out of 28 subjects fail to reduce the compound risks – our data is less extreme – conditional on reducing the compound risks, 4 out of 17 subjects are non-neutral toward ambiguity (all 4 are ambiguity averse), while conditional on ambiguity neutrality, 17 out of 30 subjects fail to reduce the compound risks. Note that the difference between 1 out of 23 and 4 out of 17 is statistically significant (two-sample test of proportions, $p < 0.1$), as is the difference between 6 out of 28 and 17 out of 30 ($p < 0.01$). Similar conclusions hold when reduction in Study 2 is defined without the hypergeometric compound risk (which was not present in Halevy (2007)).

One difference between our observed certainty equivalents and Halevy's is that ours are observed on a discrete scale with one euro increments while his are to the nearest US cent. The fineness of scale could potentially increase or decrease correlation. See Appendix D for a comparison with a "coarsened" version of Halevy's data.

4.1.2 Variation with subjects' background

In Study 2, as in Study 1, some of our subjects were advanced engineering students from an elite program while others were from a broader cross-section of non-engineering fields and institutions. In part, this was motivated by questions from reviewers about data we collected from an earlier sample (Study 3) composed almost entirely of advanced engineering students. That data resulted in contingency tables even less similar to Halevy's (Abdellaoui et al. (2011b)) and one hypothesis put forward was that the quantitative sophistication of the subjects was instrumental in generating that difference. To this end, Table 6 splits our data from Table 5 into separate contingency tables for the advanced engineers and non-engineers.

			Compound risk attitudes					
			Non-engineers			**Engineers**		
			Reduce	**Do not reduce**	**Total**	**Reduce**	**Do not reduce**	**Total**
Ambiguity attitudes	**Neutral**	Count	6	9	15	7	8	15
		Expected	2.2	12.8		2.2	12.8	
	Non-neutral	Count	0	25	25	4	56	60
		Expected	3.8	21.2		8.8	51.2	
		Total	6	34	40	11	64	75
Fisher's exact test p-value (2-tailed)			0.001			0.001		

Table 6: **Contingency table relating ambiguity and compound risk attitudes by background**

Interestingly, almost the same proportion of engineers and non-engineers reduce compound risks (14.7% versus 15%, $p = 0.96$). Fewer engineers are ambiguity neutral (20% versus 37.5%, $p < 0.05$). Thus, considering our advanced engineers as the more "sophisticated" subjects, we find that more sophistication does not result in more ambiguity neutrality nor in more reduction of compound risks.

As is apparent from the counts, more engineers who reduce compound lotteries are ambiguity non-neutral compared to the non-engineers, to Halevy's data and to the "coarsened" Halevy data (two-sample test of proportions, $p < 0.1$, $p < 0.05$ and $p < 0.05$, respectively). Such a contrast between engineers and non-engineers is not evident with regard to the proportion of those who are ambiguity neutral failing to reduce compound risk (two-sample test of proportions, $p = 0.71$). Thus, it appears that the tendency for those who reduce compound risks to be ambiguity neutral is much weaker among the quantitatively sophisticated population of engineers than in a broader cross-section of students such as our non-engineers or Halevy's subjects.[7] On the other hand, the tendency in the other direction (ambiguity neutrality implying reduction of compound risk) is much weaker in our data than in Halevy (2007) and almost identical across engineers/non-engineers.

4.1.3 Does the type of compound risk matter for the relationship?

A plausible cognitive process behind the hypothesis, as in Halevy (2007) and Halevy and Ozdenoren (2008), that reduction of compound risk implies ambiguity neutrality is the following: when faced with ambiguity, an individual forms a "mental model" of the ambiguity in the form of a compound risk and then evaluates this model as they would an objective compound risk. Given such a hypothesis, an implication is that evaluation of compound risks that are more plausible mental models of a given ambiguous bet should be more strongly related to evaluation of the ambiguity than compound risks that are unlikely to be seen as similar to the ambiguous bet. In both Halevy (2007) and our Study 2, the compound risks used were chosen in large part because they are at least plausible models of the ambiguous bet. In Study 3 (see Appendix E), we used a greater variety of compound risks, some of which were far less plausible as mental models of the ambiguity. For example, the compound risks CR low and CR high (see Figure 7 in Appendix E) would be unlikely to be models of the ambiguous bet. Do we see reduction of these compound risks having a weaker association with ambiguity neutrality? To make this comparison cleanly, we compare the advanced engineering students in Study 2 to those in Study 3.[8] This gives rise to the following contingency table:

[7] Halevy (2012) finds 20% to 30% of subjects maintain ambiguity non-neutrality after being taught to reduce compound risk. Our findings support the hypothesis that subjects with greater quantitative sophistication will be overrepresented in this 20-30%, as the advanced engineers are the subjects for whom the link between compound risk reduction and ambiguity neutrality is weakest in our data.

[8] As there was only a small proportion of non-engineers in Study 3, and their background was quite different from the non-engineers in Study 2, comparing non-engineers across these studies seems less appropriate. For completeness, we report here that of the non-engineers reducing compound risks, in Study 2 (from Table 6) 6 out of 6 were ambiguity neutral while in Study 3, considering compound risks CR low and CR high, 0 out of 1 were ambiguity neutral.

			Compound risk attitudes					
			Engineers Study 2			**Engineers Study 3**		
			Reduce means R=CRU=CRD=CRG			**Reduce means R=CR low=CR high**		
			Reduce	**Do not reduce**	**Total**	**Reduce**	**Do not reduce**	**Total**
Ambiguity attitudes	**Neutral**	Count	7	8	15	3	13	16
		Expected	2.2	12.8		1.8	14.2	
	Non-neutral	Count	4	56	60	3	33	36
		Expected	8.8	51.2		4.2	31.8	
		Total	11	64	75	6	46	52
Fisher's exact test p-value (2-tailed)			0.001			0.357		

Table 7: **Contingency table relating ambiguity and compound risk attitudes in Study 2 vs Study 3, engineers only**

Contingent on reducing the less plausible compound risks, 3 out of 6 engineers in Study 3 are non-neutral to ambiguity. This compares to the ambiguity non-neutral 4 out of 11 engineers in study 2 who reduce compound risks. Thus, the observed proportion moves in the direction predicted by the "less plausible models" hypothesis, but not significantly so (two-sample test of proportions, $p = 0.59$). With regard to the other direction of the association, entailed in Seo (2009), contingent on ambiguity neutrality, 3 out of 16 engineers reduce the less plausible compound risks in Study 3 while 7 out of 15 engineers reduce the compound risks in Study 2 (two-sample test of proportions, $p < 0.1$). This indicates a weaker association with the less plausible compound risks. Further comparison of Studies 2 and 3 may be found in Appendix D.

4.1.4 Order effects

Are the results discussed above relating ambiguity and compound risk attitudes sensitive to the order in which bets are presented? To address this, we make use of the fact that subjects in Study 2 were placed in one of six order treatments corresponding to the six possible orderings of simple risk, compound risk and ambiguity. Arguably, since simple risk is not directly involved, the purest measurement of the association between ambiguity and compound risk attitudes comes from the orders in which simple risk is presented last. Thus we begin by examining a pooled contingency table using only these orders (Table 8). Comparing this to the overall results from Study 2 in Table 5, conditional on reducing compound risks, 2 out of 4 subjects are ambiguity neutral versus 13 out of 17 (two-sample test of proportions, $p = 0.29$). Therefore, there is no evidence that our finding of a weaker association than in Halevy (2007) is driven by simple risk interfering with the relationship between ambiguity and compound risk. Furthermore, within the "simple risk last" orderings, we find no evidence that ambiguity first versus compound risk first makes a difference.

			Compound risk attitudes when risk is last		
			Reduce	**Do not reduce**	**Total**
Ambiguity attitudes	**Neutral**	Count	2	7	9
		Expected	1.0	8.0	
	Non-neutral	Count	2	26	28
		Expected	3.0	25.0	
		Total	4	33	37
Fisher's exact test p-value (2-tailed)			0.244		

Table 8: **Contingency table relating ambiguity and compound risk attitudes when simple risk is last, pooled**

Another order effect we investigate is whether orders where compound risk comes before ambiguity are much different than those where ambiguity comes before compound risk. The answer is no, they are not. Specifically, when compound risk comes before ambiguity, conditional on reducing compound risk, 2 of 7 subjects are ambiguity non-neutral, while under the reverse ordering, 2 of 10 are non-neutral (two-sample test of proportions, $p = 0.68$). In the other direction, conditional on ambiguity neutrality, 5 of 13 reduce compound risks, while under the reverse ordering, 8 of 17 reduce (two-sample test of proportions, $p = 0.64$).

The examination of order effects above was targeted precisely at evaluating whether our main findings are artifacts of order. We conclude this section by testing for order effects in the more general sense of effects on the central tendency of reported certainty equivalents. Specifically, for each of the five bets in Study 2, we tested whether the mean and/or median certainty equivalents varied by order and found that none did (all five MANOVA p-values for equality of means are at least 0.69, all five k-sample ranksum p-values for equality of medians are at least 0.33).

5 Conclusion

We provide new evidence on behavior toward compound risk in relation to simple risk and ambiguity. We confirm that reduction of compound risks generally fails, i.e., non-neutrality toward compound risk is typical. Specifically, behavior toward compound risk relative to simple risk displays systematic variation as the (reduced) probability of winning a binary bet increases. We find that the predominant pattern is increasing aversion to compound risk. This behavior is consistent with more likelihood insensitivity for compound risk than for simple risk. This pattern is strongest when compound risk is evaluated before seeing the comparable simple risk. These results suggest that a descriptively valid theory of decision making under uncertainty should account for compound risk attitude and simple risk attitude as distinct aspects of preference and allow each to display aversion increasing in the probabilities of winning as, for example, through source-dependent likelihood sensitivity.

In regard to ambiguity, we investigated the relationship between reduction of compound risk and ambiguity neutrality, finding, for a quantitatively sophisticated population (advanced engineers), a weaker relation between the two than prior literature did. Even for non-engineer subjects, conditional on ambiguity neutrality, the tendency to reduce compound risk was weaker than previously found. These findings caution against modeling ambiguity non-neutrality through non-reduction of objective compound lotteries (Segal (1987), Halevy and Ozdenoren (2008), Seo (2009)) when applying such models to sophisticated actors. We hope that they also contribute productively to further investigations of individual sensitivity to forms of uncertainty (e.g., simple risk, compound risk and ambiguity) and the role educational background, especially quantitative sophistication, may play.

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Appendix A

Instructions for Study 1

— Instructions —

AIM OF THE STUDY

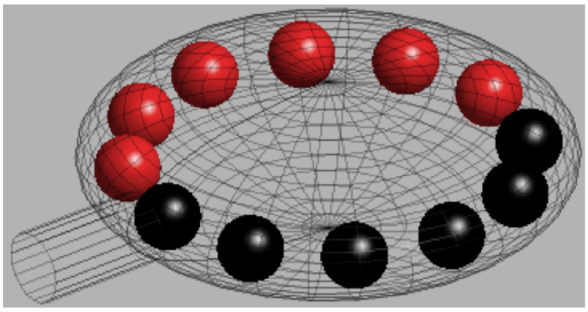
You are participating in a study on decision making under uncertainty.

PROCEDURE

There are nine different uncertain scenarios; for each scenario you are asked to make choices between two options:

- Option 1 is "Having the amount of money you receive determined by the outcome of the uncertain scenario,"
- Option 2 is "Receiving a given amount of money for sure."

Each uncertain scenario consists of an urn (or a combination of urns) from which one or two balls is (are) drawn resulting in either 0 or 50 euros depending on whether the color of the ball is black or red (or resulting in an additional draw from another urn and 0 or 50 euros depending on whether the color of the ball on this second draw is red or black).



Example of an urn

The sure amounts of money in **Option 2** also range between 0 and 50 euros.

For each of the 9 uncertain scenarios and each whole euro amount between 0 and 50, you must choose between Option 1 or Option 2. To help you decide between Option 1 and Option 2, we proceed iteratively, in three steps, the uncertain scenario remaining the same during these steps.

Step 1 - For each amount 0, 10, 20, 30, 40 and 50 euros (Option 2), you have to ask yourself whether you prefer to be paid according to the uncertain scenario or whether you prefer to be given this amount of money for sure. For instance, you can begin to ask yourself the following questions:

- For 0 euro, do I prefer to be paid according to the uncertain scenario resulting in 0 euro or 50 euros, or to be given 0 euros?
- For 10 euros, do I prefer to be paid according to the uncertain scenario resulting in 0 euro or 50 euros, or to be given 10 euros?

- ...
- For 50 euros, do I prefer to be paid according to the uncertain scenario resulting in 0 euro or 50 euros, or to be given 50 euros?

Step 2 – You are asked to refine your choice to the nearest euro: Imagine you chose Option 1 for 10 euros and Option 2 for 20 euros. You will be asked to assess your preference between Option 1 and Option 2 for the amounts ranging between 10 and 20 euros (11, 12,. . . ,19 euros). If you choose Option 1 from 0 to x euros, and Option 2 from x+1 to 50, we will say that x is your switching point.

Step 3 – This step summarizes all of your choices between one uncertain option and each of 51 sure amounts, illustrating your switching point. If you wish to go back and revise some of these choices at this stage, you may. Otherwise, simply confirm that these are the choices you would like to submit.

PAYMENT

After the experiment, you will be paid a 5 euros show-up fee plus an amount of money that depends on the choices you have made in the experiment. For each participant, first, one of the four uncertain scenarios from the experiment is randomly selected with an equal chance of each one and the choices you made for this scenario are displayed. Next, a number between 0 and 50 is randomly selected with equal chance for each one. This number is compared with your switching point x:

- If the number selected is between 0 and x, you will draw a ball from the corresponding physical urn(s) and learn whether the result is 0 euro or 50 euros depending on the color of the ball drawn.
- If the number selected is between x+1 and 50, as you chose the sure monetary amount, the result is the number selected in euros. If the result is z euros, you will be paid z (in addition to the show-up fee). Only you and the experimenter will know your final payment.

— End of instructions —

Appendix B

Instructions for Study 2

— Instructions —

AIM OF THE STUDY

You are participating in a study on decision making under uncertainty.

PROCEDURE

There are five different uncertain scenarios; for each scenario you are asked to make choices between two options:

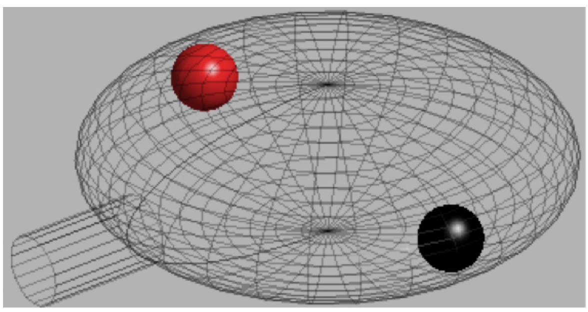
- Option 1 is "Having the amount of money you receive determined by the outcome of the uncertain scenario,"
- Option 2 is "Receiving a given amount of money for sure."

Each uncertain scenario consists of an urn (or a combination of urns) from which one or two balls is (are) drawn resulting in either 0 or 50 euros depending on whether the color of the ball is black or red (or resulting in an additional draw from another urn and 0 or 50 euros depending on whether the color of the ball on this second draw is red or black).

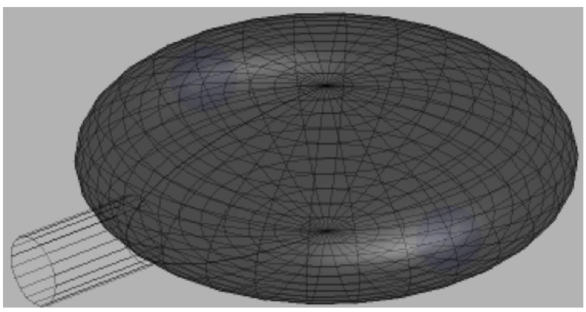
For four scenarios, the urn(s) is (are) transparent and you can observe how many balls there are of each color in the urn(s). For one scenario, the urn is opaque and you cannot observe how many balls there are of each color in the urn. The balls in the opaque urn are determined by a draw of 2 balls from a larger urn containing 12 red and black balls in unknown proportion. The experimenter will blindly perform this draw before the experiment begins so that neither you nor the experimenter knows how many of the 2 balls in the opaque urn are red and how many are black.

[At this point, the experimenter asks one subject to come up and, in view of everyone, carry out the blind draws to determine the two balls in the opaque urn.]

After the composition of the opaque urn is determined, you will be allowed to choose whether you want black or red to be the color associated with 50 euros when drawing from that urn.



Transparent urn: it contains 2 balls and you observe its composition



Opaque urn: it contains 2 balls but you cannot observe its composition

The sure amounts of money in **Option 2** also range between 0 and 50 euros.

For each of the five uncertain scenarios and each whole euro amount between 0 and 50, you must choose between Option 1 or Option 2. To help you decide between Option 1 and Option 2, we proceed iteratively, in three steps, the uncertain scenario remaining the same during these steps.

Step 1 - For each amount 0, 10, 20, 30, 40 and 50 euros (Option 2), you have to ask yourself whether you prefer to be paid according to the uncertain scenario or whether you prefer to be given this amount of money for sure. For instance, you can begin to ask yourself the following questions:

- For 0 euros, do I prefer to be paid according to the uncertain scenario resulting in 0 euros or 50 euros, or to be given 0 euros?
- For 10 euros, do I prefer to be paid according to the uncertain scenario resulting in 0 euros or 50 euros, or to be given 10 euros?
- ...
- For 50 euros, do I prefer to be paid according to the uncertain scenario resulting in 0 euros or 50 euros, or to be given 50 euros?

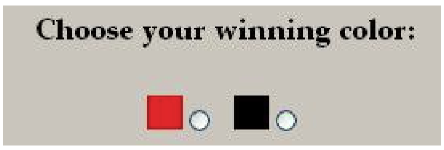
Step 2 – You are asked to refine your choice to the nearest euro: Imagine you chose Option 1 for 10 euros and Option 2 for 20 euros. You will be asked to assess your preference between Option 1 and Option 2 for the amounts ranging between 10 and 20 euros (11, 12,. . . ,19 euros). If you choose Option 1 from 0 to x euros, and Option 2 from x+1 to 50, we will say that x is your switching point.

Step 3 – This step summarizes all of your choices between one uncertain option and each of 51 sure amounts, illustrating your switching point. If you wish to go back and revise some of these choices at this stage, you may. Otherwise, simply confirm that these are the choices you would like to submit.

PAYMENT

After the experiment, you will be paid a 5 euros show-up fee plus an amount of money that depends on the choices you have made in the experiment. For each participant, first, one of the five uncertain scenarios from the experiment is randomly selected with an equal chance of each one and the choices you made for this scenario are displayed. Next, a number between 0 and 50 is randomly selected with equal chance for each one. This number is compared with your switching point x:

- If the number selected is between 0 and x, you will draw a ball from the corresponding physical urn(s) and learn whether the result is 0 euros or 50 euros depending on the color of the ball drawn.
- If the number selected is between x+1 and 50, as you chose the sure monetary amount, the result is the number selected in euros. If the result is z euros, you will be paid z divided by 5 (in addition to the show-up fee). Only you and the experimenter will know your final payment.



— End of instructions —

Note: the words in square brackets above were not included in the instructions, but are intended to document to the reader the point where an action is performed.

Appendix C

C.1 Description of the iterative choice list method and calculation of certainty equivalents

The iterative choice list procedure for eliciting certainty equivalents is borrowed from Abdellaoui et al. (2011a). For each bet, three screens were presented sequentially to the subject. On the first screen they chose, for each of six amounts evenly spaced in ten euro increments between 0 and 50 euros, between the bet and the sure amount (see Figure 3). On the second screen, for the same bet, they chose, for each of eleven amounts in one euro increments between the highest amount for which they chose the bet on the first screen and the lowest amount for which they chose the sure amount on the first screen, between the bet and the sure amount (see Figure 4). On the third screen, the choices between the bet and sure amounts in one euro increments between 0 and 50 euros implied by the choices from the first two screens and monotonicity are displayed for the subject (see Figure 5). At that point the subject is given the opportunity to modify any of these 51 choices on the third screen if desired and then the final response for that bet is recorded. Notice that the first two screens are simply a method to allow the subject to initially populate the choices on the third screen through refinement – it is only the 51 choices on the third screen that are recorded and eligible to be selected and paid through the random incentive system. We calculate the certainty equivalent for the bet by taking the midpoint between the highest sure payment rejected and the lowest sure payment accepted in the third step. In the example below, this recorded certainty equivalent is 8.5 euros.

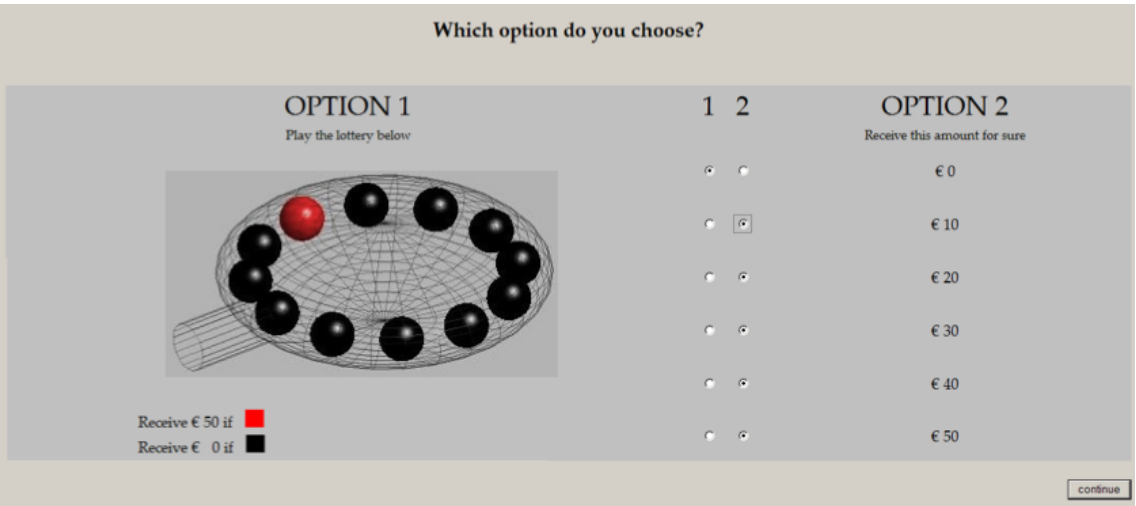


Figure 3: Simple risk for probability 1/12 treatment (first list)

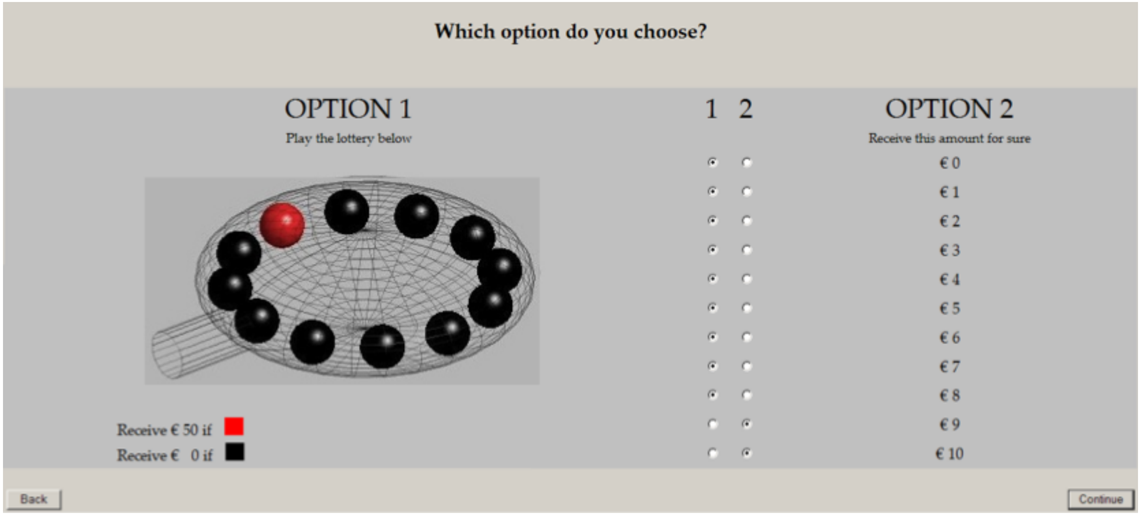


Figure 4: Simple risk for probability 1/12 treatment (second list: refinement of the first)

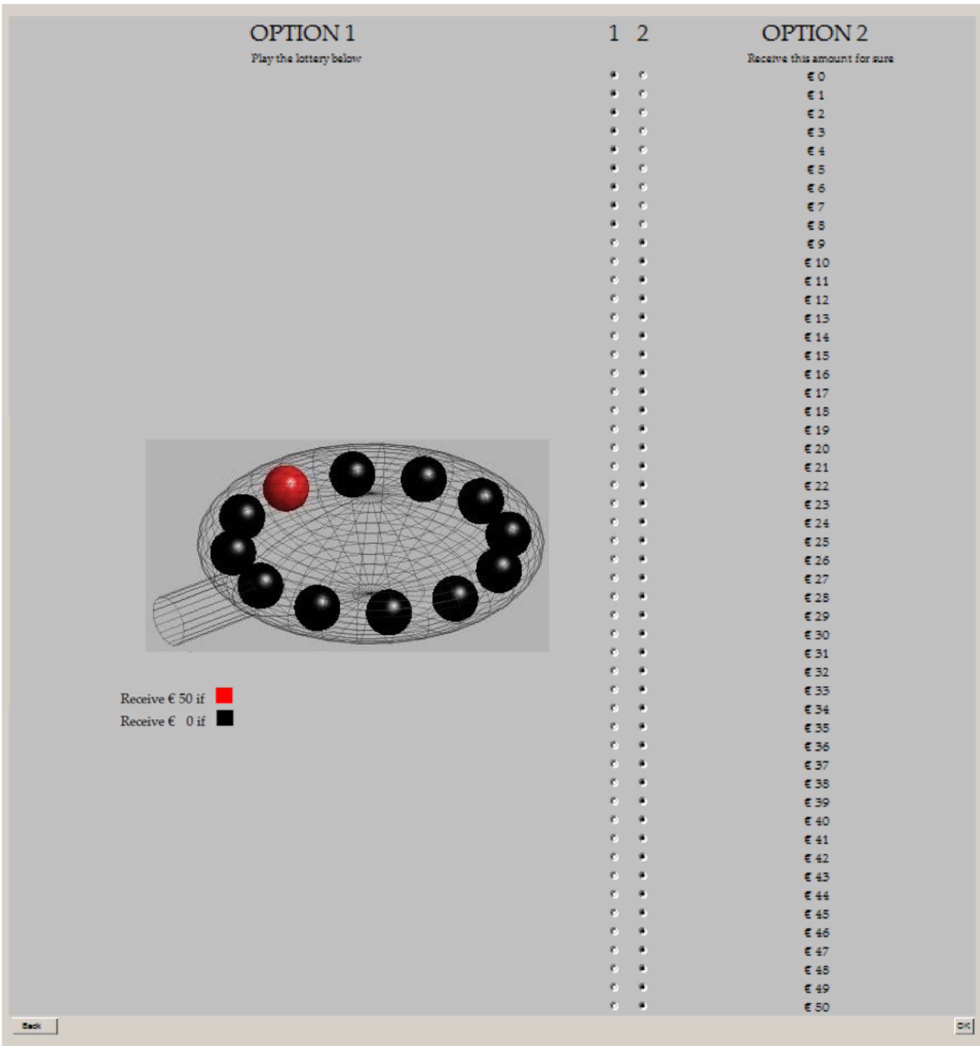


Figure 5: Simple risk for probability 1/12 treatment (third step: confirmation)

C.2 Description of the training phase

A training phase is launched by the experimenter after the reading of the instructions. At the beginning of the training phase, each subject is presented with a screen like Figure 3, except that Option 1 is replaced by the first bet that they will see as determined by their study, order treatment and any prior color choice and the word "Training" appears prominently at the top of the screen. Since each subject may be presented with a different bet, the experimenter, so as to be able to guide all subjects, projected on a screen at the front of the room a version of Figure 3 which replaces the bet and colors with rectangles containing the words "urn(s)" and "color 1" and "color 2" written on them. Then each screen of the iterative choice list (Figures 3-5) is explained by the experimenter and subjects cannot move to the next training screen until the experimenter does so. Once subjects complete the three screens, they click on a button and one choice from those on their third screen is randomly selected by the software and highlighted in red. The experimenter then explained based on the choice selected whether the bet or the sure amount would determine payment if this were the real experiment and not the training phase. Note that no bets had their outcomes revealed during the training phase. After again giving subjects the opportunity to ask questions, the experimenter ended the training phase and instructed the software to start the experiment proper on each subject's computer.

Appendix D

Additional statistics for Study 1 and 2

D.1 Study 1

Pattern of moving from seeking to aversion for simple risk when the winning probability increases

Winning probability $\longrightarrow$	**Sample size n**	**1/12**	**1/2**	**11/12**	**Page's L test p-value for within-subject increasing trend**
Average risk premia	94	-5.75	2.35	8.09	0.000***
R presented first	49	-5.68***	0.93	5.93***	0.000***
CR presented first	45	-5.82***	3.90**	10.45***	0.000***

Table 9: **Average risk premia by probability treatment and order effect**

D.2 Study 2

Comparison with coarsened data

Individuals exhibiting slight non-reduction at a finer scale might be classified as reducing at another scale, while individuals classified as slightly ambiguity non-neutral might be ambiguity neutral according to another scale. To test the effect, if any, we went back to Halevy's data reconstructed his table as it would have appeared if he had used a similarly coarse classification (in theory, the maximum difference in certainty equivalents that our measure might classify as the same is 1 euro, i.e., 2% of 50 euros. Since Halevy's winning prizes were either \$2 or \$20, 2% is \$0.04 and \$0.40 respectively). The result is recorded in Table 10. This does move his data closer to our pooled data, however, the evidence for neutrality given reduction in his data remains extreme – conditional on reducing compound risk, 2 out of 27 subjects are non-neutral toward ambiguity. The evidence for reduction given neutrality is less stark in his data under coarsening – conditional on ambiguity neutrality, 11 out of 36 subjects do not reduce compound risk. Finally, note that these calculations reflect a "worst-case scenario" in coarseness as coarse measurement will typically pick up some of the differences of less than 2%. At worst, the p-values for the results reported after Table 5 increase to $p = 0.13$ and $p = 0.033$ respectively.

			Compound risk attitudes Reduce all compound risks		
			Reduce	**Do not reduce**	**Total**
Ambiguity attitudes	**Neutral**	Count	25	11	36
		Expected	6.8	29.2	
	Non-neutral	Count	2	104	106
		Expected	20.2	85.8	
Total			27	115	142
Fisher's exact test p-value (2-tailed)			0.000		

Table 10: **Coarsening the Halevy (2007) Data: contingency table**

Additional comparison between Studies 2 and 3

Some evidence that comparing across Studies 2 and 3 is reasonable may be found in Table 11. As may be seen there, when considering only the hypergeometric compound risk and the ambiguous bet common across the two studies, both the proportions of engineers reducing the hypergeometric compound risk and the proportion of those reducing who are ambiguity non-neutral are virtually the same across studies. Moreover, equality of the proportion of those who are ambiguity neutral who fail to reduce hypergeometric compound risk is not rejected (two-sample test of proportions, $p = 0.35$).

			Compound risk attitudes					
			Engineers Study 2			Engineers Study 3		
			Reduce means R=CRG					
			Reduce	**Do not reduce**	**Total**	**Reduce**	**Do not reduce**	**Total**
Ambiguity attitudes	**Neutral**	Count	10	5	15	8	8	16
		Expected	4.0	11.0		4.6	11.4	
	Non-neutral	Count	10	50	60	7	29	36
		Expected	16.0	44.0		10.4	25.6	
Total			20	55	75	15	37	52
Fisher's exact test p-value (2-tailed)			0.000			0.044		

Table 11: **Contingency table relating ambiguity and hypergeometric compound risk attitudes in Study 2 vs Study 3, engineers only**

Appendix E

Study 3

Study 3 was conducted for an earlier version of the present paper prior to Studies 1 and 2.

Sample

64 subjects were recruited, 51 from Arts et Métiers ParisTech (an elite french graduate engineering school) and 13 from a Masters degree program in quantitative economics at Université Paris 1.

Procedure

The procedure was much like that in Studies 1 and 2, thus we describe here mainly the differences. First, subjects in Study 3 participated one at a time and the subject and the experimenter sat in front of a computer together. For each screen where choices were needed, the subject verbally indicated his choices to the experimenter who then entered them onto the screen. Second, subjects were faced with thirty-two bets, including simple risks, ambiguities and compound risks. There were no order treatments in this study, and all subjects saw simple risks first, followed by ambiguities and then compound risks. Also, the color(s) associated with the high (50 euro) payoff varied from bet to bet, were the same for all subjects, and were fixed in advance rather than chosen by subjects. A training phase using the first bet from the category coming next was used at the beginning of each of these three sections of the experiment to check whether subjects had a correct understanding of the design and of the type of uncertainty faced.

We conducted three probability treatments – for most types of uncertainty, certainty equivalents were elicited for 3 different probabilities $(1/12, 1/2, 11/12)$ of winning 50 euros. For ambiguity, instead of probability levels, we varied the fraction of winning colors: $(1/12, 1/2, 11/12)$. The specific simple risk, ambiguous and compound risk bets used for each probability treatment are listed in Tables 12 and 13. The notation $(p, 50; 0)$ represents a simple lottery with probability p of winning 50 and $(1-p)$ of winning 0. Similarly, $(q_1, (r_1, 50; 0); ...; q_m, (r_m, 50; 0))$ represents a two-stage compound lottery with first stage probability q_i of a second stage lottery giving 50 with probability r_i and 0 with probability $1 - r_i$. Additionally, $(q, (r, 50; 0); c)$ represents a two-stage compound lottery with first stage probability q of a second stage lottery giving 50 with probability r and 0 with probability $1 - r$ and first stage probability $1 - q$ of giving amount $c \in \{0, 50\}$. Finally, $(k\ colors, 50; n - k\ colors, 0)$ represents a bet on an ambiguous urn that yields 50 if one of k colors is drawn and 0 if one of the other $(n - k)$ colors is drawn. Figures 6-7 present the visual depiction of the full set of bets for the probability level 1/2. Those for the other probability treatments are analogous.

Probability ↓	**Simple risk**		**Ambiguity**	
Urn(s)⟶	12 ball	2 ball	12 ball	2 ball
1/12	(1/12, 50; 0)	-	(1 *color*, 50; 11 *colors*, 0)[a]	-
1/2	(1/2, 50; 0)	(1/2, 50; 0)	(6 *colors*, 50; 6 *colors*, 0)	(1 *color*, 50; 1 *color*, 0)[b]
11/12	(11/12, 50; 0)	-	(11 *colors*, 50; 1 *color*, 0)	-

Table 12: **Simple risk and ambiguity bets**

[a]Subjects faced this bet three times with the winning color varied.
[b]Subjects faced this bet two times with the winning color varied.

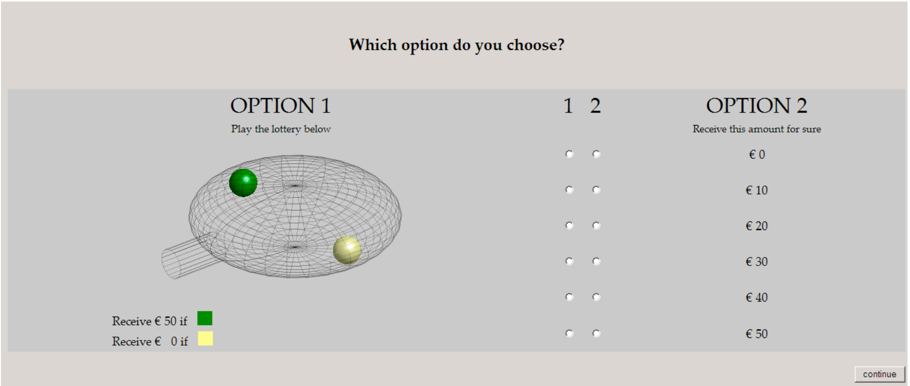
Probability ↓	**Diverse uniform CR**	**Degenerate uniform CR**	**Hypergeometric CR**
Urn(s)⟶	12 ball in the first stage and 2 ball in the second stage		
1/12	-	-	(1/6, (1/2, 50; 0); 5/6, (0, 50; 0))
1/2	(1/4, (1, 50; 0); 1/4, (1/2, 50; 0); 1/4, (1/2, 50; 0); 1/4, (0, 50; 0))	(1/2, (1, 50; 0); 1/2, (0, 50; 0))	(5/22, (1, 50; 0); 12/22, (1/2, 50; 0); 5/22, (0, 50; 0))
11/12	-	-	(5/6, (1, 50; 0); 1/6, (1/2, 50; 0))

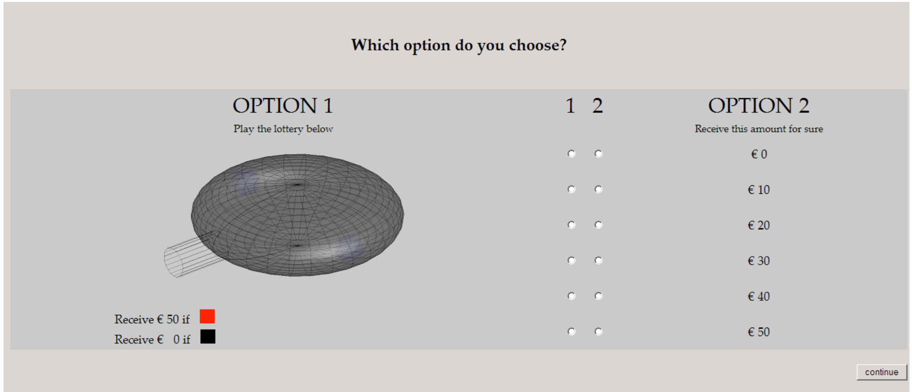
Probability ↓	**CR high**	**CR low**	**CR high with explicit degenerate urn**	**CR low with explicit degenerate urn**
Urn(s)⟶	12 ball in both the first and second stage			
1/12	(1/2, (1/6, 50; 0); 0)	(1/6, (1/2, 50; 0); 0)	(1/2, (1/6, 50); 1/2, (1, 0))	(1/6, (1/2, 50; 0); 5/6, (1, 0))
1/2	(3/4, (2/3, 50; 0); 0)	(2/3, (3/4, 50; 0); 0)	(3/4, (2/3, 50; 0); 1/4, (1, 0))	(2/3, (3/4, 50; 0); 1/3, (1, 0))
11/12	(5/6, (1/2, 50; 0); 50)	(1/2, (5/6, 50; 0); 50)	(5/6, (1/2, 50; 0); 1/6, (1, 50))	(1/2, (5/6, 50; 0); 1/2, (1, 50)

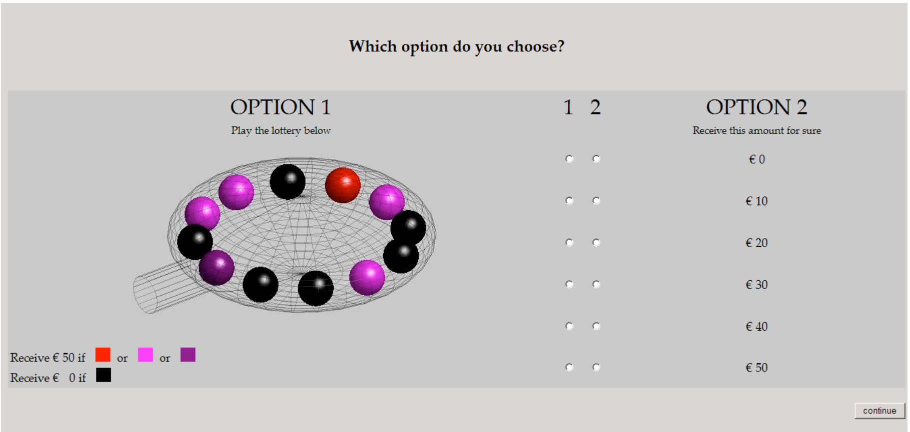
Table 13: **Compound risk bets**

Below, the bets corresponding to probability 1/2 are displayed in urn form (the form in which subjects saw them).

Simple risk	Ambiguity







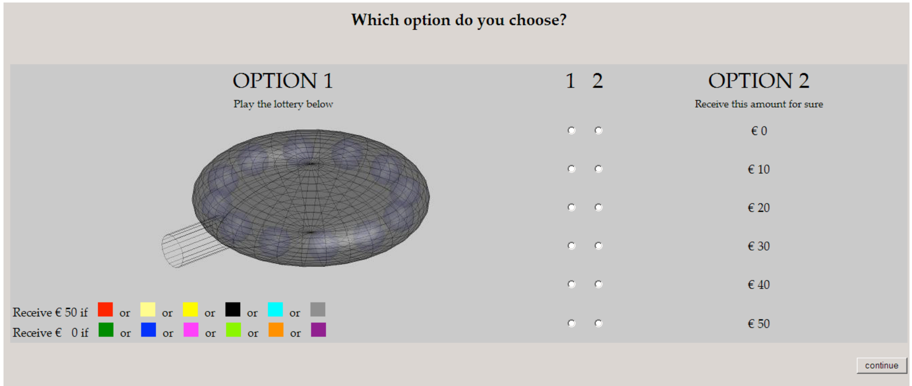


Figure 6: Urn representation of the bets described in Table 12 for probability one-half

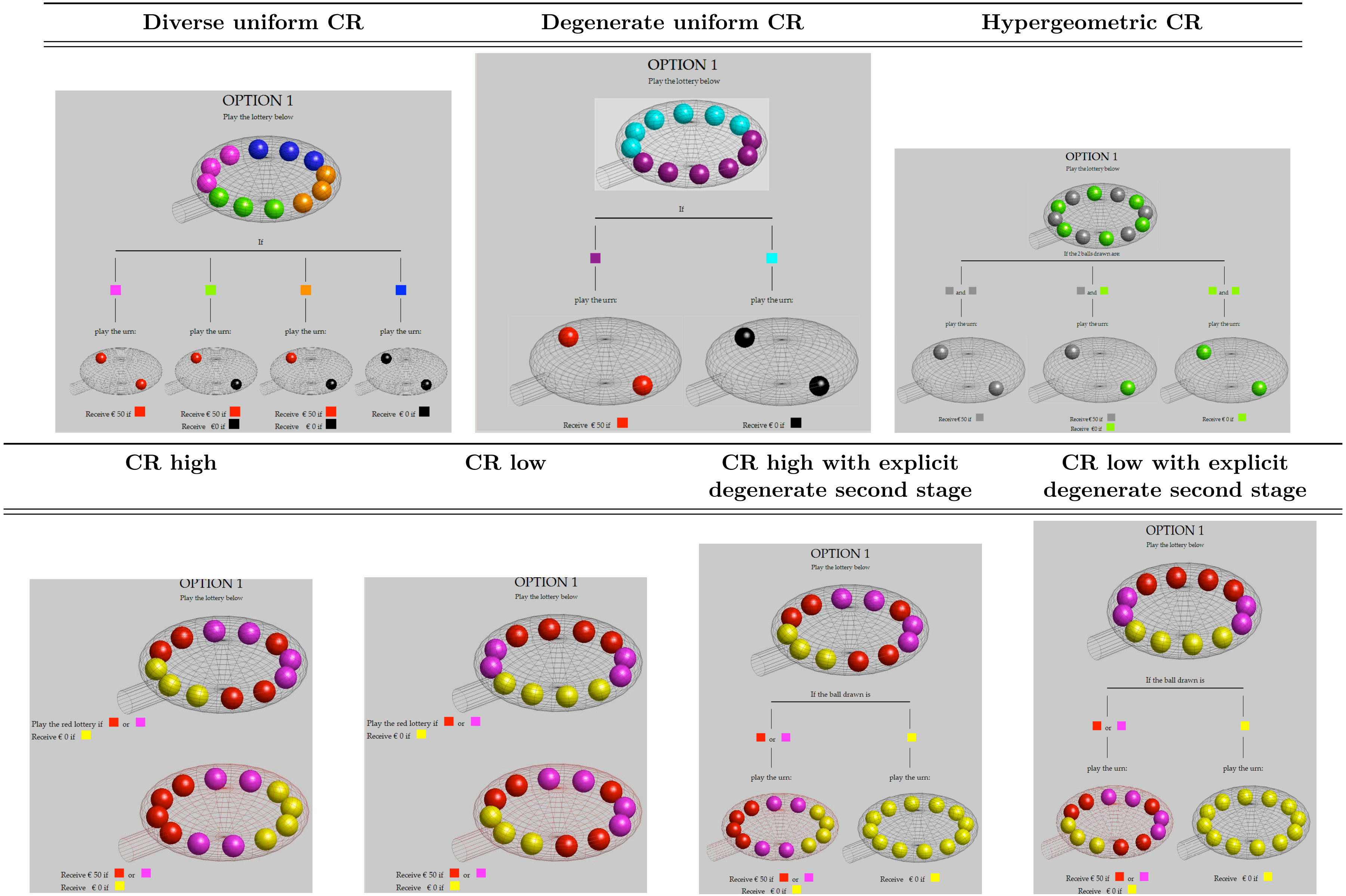


Figure 7: Urn representation of the bets described in Table 13 for probability one-half