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Preferences for information precision under ambiguity

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Abstract

This paper presents an experiment designed to measure the effect of information precision on ambiguity attitudes. The Ellsberg's two-urns experiment is adapted so that the subjects are provided with sets of observations informing on the composition of the ambiguous urn. The central feature of the design consists in keeping the frequencies of observations constant across datasets, which allows to isolate the influence of information precision by varying the number of observations. The experimental results suggest that the availability of information does not eliminate Ellsberg-type preferences, since most subjects prefer the risky urn to the ambiguous urn to bet on both colors, but it does not translate into significantly different valuations for the risky and ambiguous prospects. Moreover, I do not find evidence that the increase in information precision is associated with higher valuation of the ambiguous prospect.

Keywords: Preferences for information precision, Ambiguity, Ellsberg paradox, Certainty Equivalence, Preference Reversals, Experiment, Prince.

JEL Classification Codes: C91, D81, D83.

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1 Introduction

In real-life decision situations, exact probabilistic information about the outcomes of actions is usually unavailable. However, most decision makers are reluctant to chose ambiguous prospects and hence, are more likely to bet on events with known rather than unknown probabilities. This pattern of choices, known as ambiguity aversion, has found considerable empirical support since the pioneer paper by Ellsberg (1961)[1]. In this article, Ellsberg describes the following thought-experiment: 2 opaque urns are filled with 100 balls each and balls can be either black or red. Urn I contains exactly 50 black and 50 red balls while the composition of Urn II is unknown. What is the preferred urn to bet on black? And to bet on red? Most agents strictly prefer the known urn (Urn I) for both bets. These choices cannot be reconciled with Savage's (1954) axiomatization of the Subjective Expected Utility theory and consequently, are known as the Ellsberg paradox. Subsequent influential works have provided various theoretical frameworks to account for this type of preferences (Gilboa and Schmeidler, 1989; Ghirardato *et al.*, 2004; Klibanoff *et al.*, 2005).

However, compared to the limit cases of risk (where probabilites are objectively known) and ambiguity (where no information on probabilites is known), decision makers often own partial information about the decision process at hand: they refer to weather forecasts before setting an outside meeting, they compare customer ratings and reviews before an expensive investment, they purchase medical trials before undergoing an innovative treatment... In our modern societies, information has become one of the most traded good and, from global interests to micro motives, the issue of information acquisition is paramount in decision under uncertainty. Indeed, learning allows to mitigate ambiguity (Marinacci, 2002; Epstein and Schneider, 2007; Zimper and Ma, 2017). In particular, regarding the proportions of colored balls in Ellsberg urns, Nicholls *et al.* (2015) observe that "subjective estimates converge to the true proportions of differently colored balls (='objective' probabilities) if the respondents can observe large data

[1] Surveyed by Camerer and Weber (1992) and most recently by Trautmann and Van De Kuilen (2015).

samples from multivariate Bernoulli trials." (pp.103–104). Consequently, information that reduces ambiguity has a positive value for ambiguity-averse decision makers (Snow, 2010; Attanasi and Montesano, 2012). In an experiment, Ambuehl and Li (2018) elicit subjects' Willingness-to-Pay (WTP) for useful information (a single observation) using uncertainty equivalents. They especially find that agents have a strong preference for information that resolves uncertainty.

In the present article, I report the results of an experimental investigation on the role of information in ambiguous settings. This paper explores the following question: how do agents value partially ambiguous bet, i.e., when some information describing the decision situation is available? Since information allows to mitigate ambiguity, additional information may render the gamble more attractive for the ambiguity-averse decision maker. Moreover, I investigate the extent to which preferences are robust to different elicitation procedures. Considering two choice options, preference reversals describe situations where an individual provides contradictory preference orders under different elicitation mechanisms. As initially demonstrated by Lichtenstein and Slovic (1971), it is pretty common that one gamble, characterized by a high probability of winning a small gain (usually called the P-bet), is strictly preferred to a second gamble, featuring a small probability of winning a high gain (usually called the $-bet). However, the preferred gamble is often assigned a lower selling price. Both decisions appear to be mutually inconsistent and this is known as the standard preference reversal phenomenon. Indeed, most of the standard theories of rational choice do not admit the existence of preference reversals. This raises the question of what true preferences are since choices may depend on how the task is framed.

While preference reversals have been widely studied in riskless and risky environments[2], there are comparatively few papers addressing the occurrence of such paradox in ambiguous frameworks. Maafi (2011) investigates standard preference reversals and finds that agents do reverse their preferences under ambiguity, and the effect is even more salient than under risk. In her experiment, although the share of subjects pre-

[2] Surveyed by Seidl (2002).

ferring the P-bet is roughly constant across the risky and the ambiguous conditions, ambiguity increases the gap between the prices of the ambiguous $-bets and their corresponding ambiguous P-bets, so that reversals occur more frequently. Closely related to my paper is the Trautmann *et al.* (2011) study of preference reversals within only one attribute: contrary to standard design, the payments of the 2 lotteries are identical and the lotteries only differ in their likelihood. In several Ellsberg 2-color experiments, they report a minor but significant share of preference reversals under ambiguity when the price is elicited via WTP; however, this finding is mitigated when gambles are evaluated via Certainty Equivalents (CE). They show that WTP measurements entail a general overestimation of ambiguity aversion due to a reference point effect and the resulting loss aversion. Therefore, this study asks whether preference reversals occur under ambiguity. My design focuses on preference reversals exclusively within the likelihood attribute as in Trautmann *et al.* (2011), but I enable agents to observe informative signals whereas they focus on complete ambiguity.

In a lab experiment, subjects are asked (1) to provide their CE for an unambiguous (hence, risky) bet and an ambiguous bet and (2) to select their most preferred bet between the two. The experimental design draws on Ellsberg's two-color urns and the bets consist of gambling on the color of the ball to be drawn from an urn. In the risky bets, the participant is asked to consider gambling on blue and gambling on red in the known urn containing as much blue balls as red balls; while in the ambiguous bets, the exact composition of the urn is unknown but the urn is described by a dataset containing random draws with replacement. Hence, the degree of precision of information depends on the number of reported draws. This experiment consists of 5 treatments for 5 distinct ambiguous urns. Depending on the treatment, the number of random draws performed in the ambiguous urn spans from 0 to 500.

One key feature of the design relies on the symmetry of draws reported in the dataset: half of them are blue, the other half being red. Without information, the agent is likely to adopt a symmetric prior about the composition of the ambiguous urn (Gilboa and Marinacci, 2016, p392). Thereafter, all commonly used updating rules predict that symmetric information about the color of draws result in symmetric revised beliefs. Hence, the symmetry of draws allows for a direct comparison of the CEs and the preferences between the risky urn and the ambiguous urn, since the risky urn contains as much blue balls as red balls. Indeed, if the symmetry of prior and posterior beliefs is verified, the comparison between the CEs depends only on the individual perception of ambiguity and the individuals' attitude towards ambiguity. Besides, the symmetry of signals allows to relate this study to other experimental replications of Ellsberg's two-urns problem in similar symmetrical frameworks.

Further, this experiment can be used to investigate the strength of Ellsberg-type preferences: the direct choices between urns are used to evaluate the incidence of ambiguity aversion in the case of partial ambiguity and the CE measurements provide an estimation of the magnitude of the preference for one urn compared to the other (high differences in CE indicate strong preferences whereas small differences suggest weak preferences). In the last stage of the experiment, the participant is asked to explicitly state his individual beliefs on the composition of the ambiguous bag. More specifically, his estimation of the number of blue balls in bag B is collected and the respondent has to provide lower and upper bounds for the proportion of blue balls in the bag. This non-incentivized task serves to study how the precision of information influences beliefs and perceived ambiguity.

In terms of payment device, the choices are incentivized via the Prince mechanism, recently proposed by Johnson *et al.* (2015). Compared to standard methods, the Prince mechanism allows to clarify consequences of decisions and makes incentive compatibility completely transparent to subjects. To the best of my knowledge, this paper is the first to implement this method to incentivize preferences for information precision in an ambiguous framework.

This experiment yields three key findings:

1. Within treatments, despite the fact that the majority of participants prefer the risky urn to the ambiguous urn for both bets (in conformity with the Ellsberg paradox), they do not place a higher value on the risky gamble compared to the

ambiguous one (even in the no-information treatment).

2. Between treatments, although one would have expected a positive correlation between the precision of information and the value attributed to the ambiguous bet (the more information, the more appealing the ambiguous gamble), there is no significant relationship between the number of draws in the dataset and the CE of the ambiguous bet. Moreover, the comparison of standard deviations of the distributions of CE by treatments does not indicate more dispersion in the less informative treatments.
3. On average, 1/4 of the subjects exhibit preference reversals. Hence, CE measurements do not eliminate preference reversals in this framework, as opposed to the findings of Trautmann *et al.* (2011).

I can conclude from these results that ambiguity-averse preferences expressed by the subjects are relatively weak since ambiguity-aversion deduced from direct choices does not translate into significant CE differences between the risky and the ambiguous gambles. The reason is that it might be easier to evaluate attributes jointly than separately. In particular, my finding can be related to the "coherent arbitrariness" explanation proposed by Ariely *et al.* (2003). The authors argue that DMs experience difficulties to value goods and they show in a series of experiments that subjects' valuations are notably arbitrary and unstable since individual prices can be manipulated by normatively irrelevant factors (such as framing and anchors). Furthermore, the similarity of valuations between treatments suggests that the increase in informativeness does not significantly modify the perception of ambiguity and this tends to prove that subjects form a confident "fifty–fifty" probability judgment for the composition of the ambiguous urn even when no or very little information is available. This is confirmed by individual estimates on the composition of the ambiguous bag.

The type of information considered here is fundamentally different from Eliaz and Schotter (2010) who study demand for non-instrumental information (information that may not change the final decision). In a risky environment, Eliaz and Schotter report that experimental subjects assign a positive value to non-instrumental information. This may be due to what they call the "confidence effect", i.e., an intrinsic preference for being confident in choosing the right decision. This study departs also from Ambuehl and Li's (2018) experiment for two main reasons: First, while they estimate WTP for information, I choose to focus on CE since WTP measurements have been blamed for artificially inflating the number of ambiguity averse agents and subsequent estimates of preference reversals (Trautmann *et al.*, 2011). Second, their sets of observations contain only one draw from the urn whereas, in this paper, the datasets describing the ambiguous bag are of different lengths depending on the treatment. Hence, I am able to generate a wide range of degrees of information precision, ranging from 2 observations (weakly-informative treatment) to 500 observations (highly-informative treatment), whereas the respondents of the control group (no-information treatment) do not receive any statistical feedback. Contrary to Trautmann *et al.* (2011) who impose that CEs lie between the minimum payoff and the Expected Value of the risky lottery (or the mean of the payoffs of the ambiguous lottery), the CE elicited in my experiment can take any value between the minimum and the maximum payoffs of the gamble. Indeed, 1/3 of estimated CE in this experiment are higher than the Expected Value of the lottery.

This paper proceeds as follows. Section 2 presents the experimental design. Results of the experimental sessions are discussed in section 3. Section 4 concludes. Appendix A provides supplementary material for the experiment (including the instructions of the experiment). Additional tables and figures appear in Appendix B.

2 Experimental Design

2.1 Stimuli

The experimental design draws on Ellsberg's two-color urns experiment. Subjects are shown two opaque bags: bag A and bag B. They are informed that both bags contain 100 balls each and that balls can be either blue or red. The composition of bag A is perfectly known: it contains exactly 50 blue balls and 50 red balls. The proportions of blue and red balls contained in bag B are unknown but some information on its composition is sometimes available: subjects are told that previous random draws with replacement from bag B have been been carried out and are informed of the results of the draws.

The gambles presented to the participant are of the following type: if you draw a ball of a prespecified color, you win 15€, otherwise you win 5€[3]. There are 4 different possible gambles: betting on a blue ball from bag A and betting on a red ball from bag A (risky bets), betting on a blue ball from bag B and betting on a red ball from bag B (ambiguous bets). For each of these gambles, the subject's Certainty Equivalents (CE, defined as the sure amount equally desirable as the gamble) are elicited.

There exist two alternative experimental strategies to elicit prices such as CE: direct matching and choice-based procedures. The direct matching method consists of determining the CE by directly asking the subject to find the monetary outcome that represents the switching opoint[4]. In contrast, choice-based procedures refer to strategies based on outright choices, where the value of CE is deduced from a series of choices. The Multiple-Price List (MPL) technique belongs to this last category[5]: it consists of binary decisions between the prospect and (usually equally spaced) sure amounts (e.g., Bruhin *et al.*, 2010, Experiment 4 in Trautmann *et al.*, 2011, Chew *et al.*, 2017). The seminal paper of Holt and Laury (2002) popularized the MPL procedure. Thereafter several recent experimental studies on decision theory estimate prices through MPL. Indeed, this method presents several advantages. First, it is the easiest method to deal with: (*i*) for experimenters, because instructions and implementation are clear and straightforward; (*ii*) for participants, because it is easy to understand. Although direct matching is undoubtedly the most time-saving procedure, it is cognitively demanding since subjects are asked to find their indifference point which is not a natural inquiry. In contrast, the MPL method interrogates subjects about their preferences, which is more intuitive. Second, the simplicity of the MPL procedure (together with the Prince incentive method, see

[3]Given that there is a 5€ show-up fee, the minimum reward in this experiment does amount to 5€. This fee is included in the gambles payoff because agents seem to pay more attention to their decisions and make less errors when the monetary incentives are high (see for instance Lévy-Garboua *et al.*, 2012).

[4]For instance, individual Willingness-To-Accept is elicited via direct matching procedure in Maafi (2011).

[5]Along with the bisection method (Abdellaoui, 2000). See also pages 555-556 of Glimcher and Fehr (2013) for a discussion of the different elicitation techniques.

Section 2.6 on incentives) makes it relatively obvious for participants that truthful revelation of preferences is in their best interest. Third, experimental evidence shows that choice-based procedures are more consistent than direct matching. Indeed, preference reversals occur more often with matching techniques (Bostic *et al.*, 1990). Since there is a particular concern with the prevalence of preference reversals in this paper, I selected the method that is the least prone to such paradoxical choices to avoid artificial inflation of preference reversals occurrences. For a review on the most recent experimental literature on Ellsberg-urns tasks and on the different valuation and incentive methods, see Trautmann and Van De Kuilen (2015) (Table 1).

Therefore, in this experiment, the CEs are reported using MPL. Precisely, for the 4 gambles, subjects are asked to consider 10 choices between playing the gamble and receiving a sure amount. In each row of the MPL, the participant has to single out his preferred option between: "Bet on *color* in *bag*" or "x€", with *color* being replaced by blue or red, *bag* being replaced by bag A or bag B and x taking the 10 equally spaced values between 5.50 and 14.50. In each MPL, the monetary amounts rise moving down the list while the bet remains the same. By linear interpolation, the CE is taken as the mid-point of the two sure amounts for which the subject switched preferences. However, several experiments show that people often report multiple switching points, which violates monotonicity of preferences. For instance, Lévy-Garboua *et al.* (2012) replicate Holt and Laury's (2002) seminal procedure for measuring risk aversion, they report a substantial rate of inconsistencies under various frames (in their experiment, 30% of subjects violate monotonicity when presented with 10 simultaneous ranked choices). In Cohen *et al.* (2011), most participants violate monotonicity at least once in their online experiment. To avoid this type of issues in my experiment, I wrote a computer program that enforces the monotonicity of revealed preferences by allowing at most one switching point from the gamble to the sure amount. More concretely, once the subject ticks a given option on the MPL, the computer fills in the lines above and below so as to ensure monotonicity (see the Instructions in Appendix A for an exhaustive description of the mechanism). This convenient technique has been implemented in numerous cases since the influential paper of Gonzalez and Wu (1999)[6]. Moreover, in an experimental study comparing different MPL filling designs, Andersen *et al.* (2006) finds no systematic effect of monotonicity-enforcement on subjects responses. Finally, this method allows to keep the experiment as short as possible to maintain subjects aware and concentrated since the tedious task of filling rows one by one can be boring.

In a second step, the participant is asked to select his most preferred bag to bet on blue and his most preferred bag to bet on red.

Table 1 summarizes the 6 different tasks performed by the participant.

Task 1	Fill out the MPL for "Bet on blue in bag A".
Task 2	Fill out the MPL for "Bet on red in bag A".
Task 3	Fill out the MPL for "Bet on blue in bag B".
Task 4	Fill out the MPL for "Bet on red in bag B".
Task 5	Choose between "Bet on blue in bag A" and "Bet on blue in bag B".
Task 6	Choose between "Bet on red in bag A" and "Bet on red in bag B".

Table 1: Incentivized tasks

Valuation tasks (Tasks 1-4 in Table 1) are run before choice tasks (Tasks 5-6)[7]. The two types of tasks are performed one after the other. Indeed, in the literature on preference reversals, there is a substantial number of works arguing that valuation tasks and choice tasks call forth different heuristics and different cognitive processes[8] (see recently Loomes and Pogrebna, 2016). Thus, I decide to not randomize between types of tasks to make the experiment easy to understand and to avoid confusion. Similarly, since it is much more straightforward to mentally represent risky prospects than ambiguous ones, the risky bag is presented first in the instructions and the description of the ambiguous bag follows. Then, when performing the tasks, the participant evaluates first the bets

[6]See recently for instance: Csermely and Rabas (2016), Gerhardt *et al.* (2017), Ambuehl and Li (2018). Cubitt *et al.* (2014) proposes an alternative but similar framework: the participants can freely complete the choice-list but the computer program only accepts answers with a single switching point.

[7]There is no particular agreement relative to the presentation order of tasks: for instance, in Maafi (2011), participants answer valuation questions before choice questions whereas in Trautmann *et al.* (2011) the participants perform choice tasks first.

[8]Besides, Grether and Plott (1979) show that the presentation order between valuation and choice tasks does not significantly influence the pattern of reversals under risk.

on blue/red in bag A (Tasks 1 and 2) and second, the bets on blue/red in bag B (Tasks 3 and 4).

2.2 Treatments

There are 5 different treatments for 5 different bags B. For the sake of clarity, these 5 distinct bags are labelled bag B1, bag B2, bag B3, bag B4, bag B5 in this paper but note that the partially ambiguous bag is always named bag B during the experimental sessions. In each treatment, the participant faces only 2 bags: the risky bag (bag A) and the ambiguous bag (bag B). The treatments differ in the number of draws contained in the dataset describing the ambiguous bag. Indeed, different lengths of datasets correspond to different degrees of information precision. One key feature of the design relies on the symmetry of draws reported in the dataset: in each treatment, half of the draws are blue, the other half being red. Since the alternative bag (bag A) contains as much blue balls as red balls, the symmetry of draws in the dataset describing bag B greatly simplifies the comparison of preferences for bets on the ambiguous bag B to the situation of objective risk in bag A. Indeed, if initial beliefs on the composition of bag B are symmetric and if the symmetry of draws implies the symmetry of revised beliefs, the comparison between CEs depends only on the individual's perception of ambiguity and his attitude towards ambiguity.

Table 2 describes the different datasets associated to the 5 ambiguous bags. For example, consider treatment 1: in this treatment, the subject is informed that 500 random draws with replacement have been carried out in bag B. We have observed 250 blue balls and 250 red balls. Depending on the treatment, the number of previous draws spans from 0 to 500 where the no-information treatment (treatment 5) mimics the classical Ellsberg's experiment and serves as a benchmark for the impact of information precision on ambiguity perception.

Treatment	Bags	Dataset		
		Total number of draws (d)	Blue draws	Red draws
1	B1	500	250	250
2	B2	50	25	25
3	B3	10	5	5
4	B4	2	1	1
5	B5	0	0	0

Table 2: Number of draws describing the ambiguous bag

2.3 Non-incentivized task

After the valuation and choice questions, an additional belief task is added to the experiment: the subject is asked about his individual belief about the composition of the ambiguous bag. In particular, he has to explicitly report his estimation of the number of blue balls in bag B. Then, he reports his lower and upper bounds for the proportion of blue balls in the bag which provides a measure of his degree of confidence in his estimate[9].

This task allows to study the influence of precision of information on learning. Learning occurs when the subjective estimates converge to the objective probabilities. In my framework, given the symmetry of draws reported in the datasets, it is likely that respondents state equal proportions for both colors. Moreover, increasing the number of observations raises the precision of information which might lead the agent to behave as if he knew the true composition of the bag in the limit. For instance, one might feel more confident in one's 50-50 probability judgment with the observation of 250 blue draws and 250 red draws (treatment 1) than with the realization of 1 red draw and 1 blue draw (treatment 4). Therefore, subjects belonging to the most informative treatments are expected to report less dispersed estimates as compared to subjects in the less informative treatments. The control group is given by the no-information treatment (treatment 5).

[9]The questions are framed as follows: "In your opinion, how many blue balls are contained in bag B? Indicate also your minimal estimate and your maximal estimate of blue balls contained in bag B.".

2.4 Participants[10]

The experiment was conducted at LEEP (Laboratoire d'Economie Expérimentale de Paris[11]) and consisted of 10 sessions in July, 2017. For each of the 5 treatments, 40 participants were recruited using ORSEE (Greiner, 2015) and no one was allowed to participate in more than one treatment. None of the subjects had previously participated in a similar economic experiment. 181 participants showed up and took part in the experiment: 60% of them are female, ages vary between 18 and 77 (median age being 28), and half of them have completed at least 3 years of higher education. Table 3 details the sample size and the average gain by treatment (see also table B1 in Appendix B for additional figures on participants). The experiment lasted about 50 minutes (including payment). The average payment amounts to 10.32€ (including the 5€ show-up fee), compared to the french minimum hourly wage which is less than 7.60€ in 2017.

Treatment	Number of participants	Average gain
1	37	10.55€
2	37	10.32€
3	34	10.02€
4	36	9.90€
5	37	10.56€
Total	*181*	*10.32€*

Table 3: Sample size and average gain by treatment

2.5 Procedures

The experiment was programmed and conducted with the experiment software z-Tree[12] (Fischbacher, 2007). No particular skill is required to participate, except the understanding of french language. There is no time limit to answer the questions.

[10] I am grateful to undergraduate Economic students from the University of Cergy-Pontoise for their participation to non-incentivized pilot sessions.

[11] `http://leep.univ-paris1.fr/accueil.htm`. Maison des Sciences Economiques, 106 - 112 boulevard de l'Hôpital, 75647 Paris cedex 13.

[12] `http://www.ztree.uzh.ch/en.html`

A typical session is structured as follows: after an oral presentation of the experiment[13], the participant is asked to perform the 6 tasks using the computer in front of him. Then the subject reaches the experimenter's office for payment: one question of the experiment is played for real according to the Prince incentive mechanism (see section 2.6). If the preferred option is a sure amount, he receives the sure gain; if the preferred option is a bet, he randomly draws one ball from the bag involved in the bet. Besides, the subject may implement real draw in real bag in order to persuade him that the procedure is truly not-manipulated by experimenters. The detailed script of the experiment is provided in Appendix A.2.

The experimental design is as follows: the participant is presented with only 2 different bags. Both bags are fully described in the instructions so that when evaluating one bag, the subject knows about the existence of the other. There are only 6 tasks to perform. This short and easily understandable design allows mainly for between-subject analysis.

Regarding the technical aspects of the composition of the bags, bag A contains 50 blue balls and 50 red balls as announced. The composition of bags B1-B4 follows from a 3-steps program on MATLAB: first, I generate all the possible bags containing 100 (blue or red) balls, this yields 101 different bags with composition ranging from (0 blue, 100 red), (1 blue, 99 red), ... to (100 blue, 0 red); second, I randomly draw d balls with replacement from all the bags ($d \in \{2, 10, 50, 500\}$) and I keep the bags for which I obtain the dataset with frequencies of interest, i.e., $d/2$ blue and $d/2$ red balls; third, for each d, I randomly select one of these urns to be presented to the participants. Lastly, since no balls have been drawn from bag B5, bag B5 is randomly drawn among the 101 possible bags. The detailed bags composition is provided in Appendix B (Table B2).

2.6 Incentives

One commonly known challenge of experimental economics lies in the choice of the adequate method to incentivize decisions made in the laboratory. Studies usually involve

[13] See Appendix A.1 for a full set of instructions.

more than one decision and paying every choice results in wealth effects. Thus, in most experiments, only one round is chosen by a specified random device at the end of the experiment (*ex post*) and implemented for real. Such design, popularized by Savage (1954) under the name Random-Incentive-System (RIS), produces valid results only if participants consider each experimental decision in isolation (i.e., as a single real choice). Otherwise, the method violates incentive-compatibility (IC). An experimental design is said to be IC when it ensures that *all* participants act according to their true preferences.

Most experimental studies in the literature on decisions under uncertainty uses RIS, often combined with the Becker-DeGroot-Marschak (BDM) mechanism (1964), to elicit preferences for ambiguous lotteries. However, these devices have been criticized for their inability to motivate truthfull-telling for non-expected-utility maximizers. It has been shown that BDM does not ensure IC in experiments involving risky lotteries (Karni and Safra, 1987), and even in experiments involving non-random goods (Horowitz, 2006). Besides, Bade (2015) argues that without the assumption of expected utility preferences, a participant's behavior in the RIS needs not reflect his behavior in single choice experiments. The reason for this is that ambiguity-averse agents can use random device to hedge across experimental tasks and hence may not act according to their true preferences. Baillon *et al.* (2015) also demonstrate that usual implementations of the RIS yield violations of IC for ambiguity-averse DMs. The underlying argument is that, in case of ex-post situation determination, agents may represent the choice problem as a meta-lottery, leading to violations of isolation. They show how IC may be restored with a slight modification of the device in which the randomization takes place before the decisions are made[14].

I thus choose to incentive the decisions in this experiment using the innovative Prince mechanism proposed by Johnson *et al.* (2015). Compared to standard methods, this mechanism allows to clarify consequences of decisions and makes incentive-compatibility completely transparent to subjects. In concrete terms, this device relies on the following

[14]See also Oechssler and Roomets (2014) and Kuzmics (2017) for further discussions on hedging and misclassifications of ambiguity-sensitive subjects in experiments.

main features contained in the acronym PRINCE:

> "(1) the choice question implemented for real is randomly selected PRior to the experiment; (2) subjects' answers are framed as INstructions to the experimenter about the real choice to be implemented at the end; (3) the real choice question is provided in a Concrete form, e.g., in a sealed envelope; (4) the Entire choice situation, rather than only one choice option, is described in that envelope." (Johnson *et al.*, 2015, p3)

Prince allows to overcome the inherent drawbacks of other standard techniques by enhancing isolation between experimental tasks. Consequently, participants are ensured that truthful revelation is in their best interest and the internal validity of the experiment is improved. Finally, in an experiment, in which Johnson *et al.* (2015) elicit the Willingness-to-Accept for a mug, Prince has been found to produce similar results under choice and matching strategies. Here, I elicit the CE of several gambles which are more complex to mentally represent than a concrete object such as a mug[15]. Moreover, there is no empirical evidence that both strategies are equivalent to estimate CE of lotteries under Prince. For this reason and the ones surveyed in section 2.1, I chose to provide subjects with MPLs.

In my experiment, before performing the tasks, each subject receives a closed envelope containing the question that will be implemented for real. Each question describes two choice options (e.g., a lottery versus a certain amount of money, see Figure 1). The goal of the participant is to obtain the most preferred option. For each session, there are 60 envelopes, numbered 1-60 in random order. Besides, each of the 6 tasks is associated to a type and referred to by a Greek letter. There are 10 envelopes of each type (e.g., envelope contents of type α correspond to the rows of the MPL of Task 1[16]). Hence, each question of the experiment is contained in one envelope at least. The type of the envelope remains secret until the opening of the envelope and it helps improving

[15] Proposing a matching strategy here is equivalent to asking subjects to determine the amount that makes them indifferent between playing the lottery and receiving the amount for sure, which is not a straightforward task.

[16] See the table in the instructions (p35) for the complete list of the types of envelopes.

and speeding up the payment phase. During the experimental session, all the possible contents of envelopes are presented to the subjects. Each of these choices is made particularly salient with the envelope at hand. The subject is informed about all the possible contents of the envelopes in the experiment but does not know the content of his own in particular. At the end of the session, the envelope is opened and the choice of the subject is performed recalling the instructions he gave on the computer: If the preferred option was a sure amount, the subject receives the sure gain; if the preferred option was a bet, he randomly draws one ball from the bag involved in the bet and receives the gain associated with the color of the drawn ball.

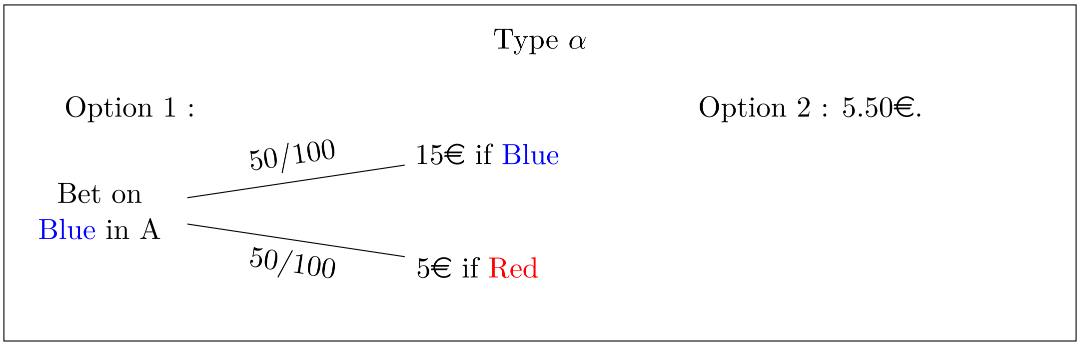

Figure 1: An example of envelope content

The rules of the experiment are made explicitly clear from the beginning of the experiment: as soon as they get into the room, the participant is asked to sign a consent form where it is specified that the gain is conditional upon the respect of the instructions and especially upon the envelope remaining closed until the end of the experiment (See Appendix A.4). Moreover, to render the experiment completely transparent, the participant gets verification lists at the end of the session. These lists describe the contents of all the envelopes in the experiment and each participant can check that they were truthfully described, avoiding deception (See Appendix A.3).

3 Results

3.1 Certainty Equivalents

Descriptive statistics on CEs are reported in table 4. As in all subsequent tables, the numbers of draws describing the ambiguous bag (in parentheses) are listed next to the treatment. N gives the sample size. In the following, the CE for the bet on blue (red) in bag A is denoted $CE_A(blue)$ ($CE_A(red)$) and similar notation applies for bag B. Normal distributions of CEs cannot be rejected[17]. However, whenever possible, I favour nonparametrical analysis over parametrical tests because the sample sizes are relatively small (less than 40 subjects in each treatment).

First, the experimental data do not provide evidence on substantial difference of valuation of bets depending on the color of the ball to be drawn. The differences between the CEs in bag A and the CEs in bag B are not statistically significant (paired Wilcoxon Signed-rank test[18], all $p > .05$), except for the treatment 5 where $CE_B(blue)$ differs significantly from $CE_B(red)$ (paired Wilcoxon Signed-rank test, $z = -1.970$ and $p = .0488$).

Apart from treatment 1, the means and the medians of CEs on bag A are between 9 and 10 euros and are not significantly different from 10, which is the expected value of the risky prospect (one-sample t-tests: all $p > .05$ and one-sample Wilcoxon tests: all $p > .05$)[19]. This finding suggests, on average, relative risk-neutrality for participants in treatments 2-5. In treatment 1, $CE_A(blue)$ differs significantly from 10 (one-sample Wilcoxon, $z = 1.99$, $p = .0458$) but the null hypothesis of equality of mean with 10 cannot be rejected with a one-sample t-test ($p > .05$). Identical analysis on $CE_A(red)$

[17] I performed a series of statistical tests for normality of variables that preclude the rejection of the null hypothesis that CEs are drawn from a normal distribution: Shapiro-Wilk (all $p > .05$), Shapiro-Francia (all $p > .05$ except for $CE_A(blue)$ in treatment 2: $z = 1.667$ and $p = 0.04779$), Jarque-Bera (all $p > .05$).

[18] Under the assumptions that (1) data are paired and come from the same population, (2) paired observations are independent and (3) paired differences can be ranked, the paired Wilcoxon-Signed rank test (non-parametric) tests the null hypothesis that the median of the paired differences is zero.

[19] Under the assumption of the variables being approximately normally distributed, the one-sample t-test is used to test the null hypothesis that the mean is equal to 10. In the absence of such normal pattern, one-sample Wilcoxon test allows to test the hypothesis of equality of median with 10.

		Treatment				
		1 (500)	2 (50)	3 (10)	4 (2)	5 (0)
		$N = 37$	$N = 37$	$N = 34$	$N = 36$	$N = 37$
Bag A (risky)						
$CE_A(blue)$	mean	10.84	9.76	9.86	9.58	9.95
	med	10	10	10	10	9
	var	7.08	7.13	7.28	7.91	9.83
	std	2.66	2.67	2.70	2.81	3.14
$CE_A(red)$	mean	10.54	9.51	9.24	9.78	9.73
	med	10	10	9.5	10	10
	var	7.81	6.26	8.37	6.92	8.70
	std	2.79	2.50	2.89	2.63	2.95
Bag B (ambiguous)						
$CE_B(blue)$	mean	10.32	9.68	9.44	9.36	9.21
	med	10	10	10	9	9
	var	8.95	5.95	11.10	7.89	7.73
	std	2.99	2.44	3.33	2.81	2.78
$CE_B(red)$	mean	10.57	9.27	9.29	9.31	9.95
	med	10	10	9	9.5	10
	var	7.53	6.09	8.82	5.99	7.72
	std	2.74	2.47	2.97	2.45	2.78

Table 4: CEs - Descriptive statistics

Note: med, var and std stand for median, variance and standard deviations, respectively.

suggests that it is not significantly different from 10.

Figure 2 depicts the proportions of choices in favour of the sure gain in bag A and in bag B. It shows that the percentage choosing the sure gain falls as the sure amount increases. There is little graphical evidence of stochastic dominance of one distribution over the other. Indeed, for each treatment, the differences between $CE_A(blue)$ and $CE_B(blue)$ are not statistically significant (Wilcoxon-Signed rank test: all $p > .05$). The same results apply for the differences between $CE_A(red)$ and $CE_B(red)$. These findings are summarized in Result 1.

Result 1 *Within treatments, for both color bets, there is no significant difference between the valuation of bets on the risky bag and the valuation of bets on the ambiguous bag.*

This result is in line with Maafi's (2011) study of preference reversals in Ellsberg's two-color framework. My design is closely related to a particular condition in her experiment where the probability of winning from the risky urn equals .5 while the probability of winning from the ambiguous urn is unknown (Maafi, 2011: $-bet of Pair V in Table 1). In case of success, the participant wins 12€ (+ 5€ of show-up fee) and 0€ (+ 5€) otherwise, which is similar to the stakes in my experiment. The Willingness-To-Accept for both bets is not significantly different and is close to the Expected Value of the risky urn, as reported in the present paper with CE measurements. Maafi's analysis falls within the framework of Prospect Theory due to Kahneman and Tversky (1979, 1992), and she argues that her results may be due to the particular form of the probability-weighting function, which is S-shaped and has an inflexion point in the middle region. According to this explanation, while DMs tend to overweigh small probabilities and underweigh large probabilities, they are relatively insensitive to probability changes in the middle zone. This implies ambiguity-aversion for likely-events and ambiguity-loving for rare events, whereas DMs exhibit ambiguity-neutrality for the events that are assigned intermediate probabilities of occurrence. This explains hence that the risky and the ambiguous prospects are valued almost equally.

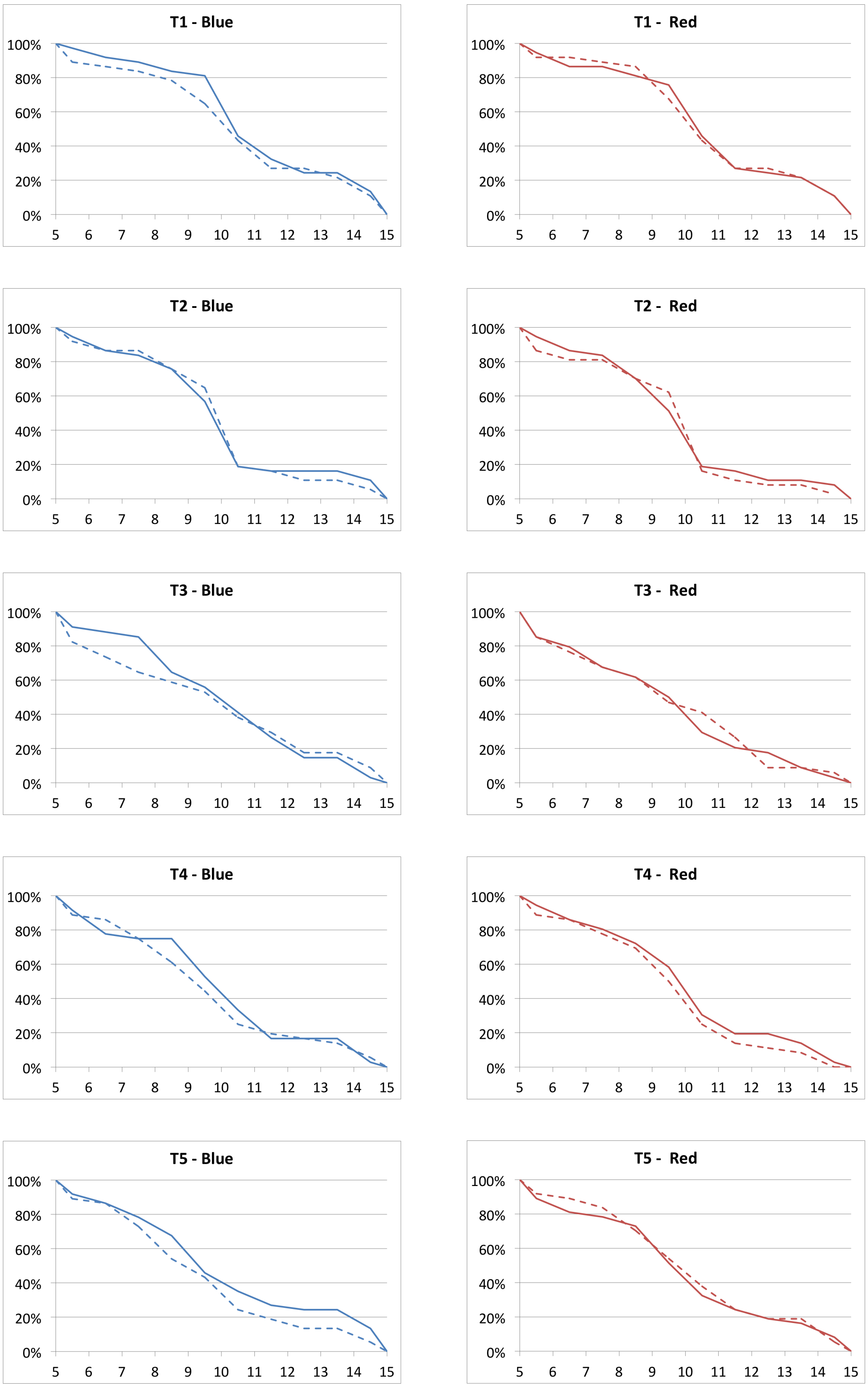

Figure 2: Proportions of choices in favour of the sure amount, by treatment

Note: bag A (solid line) versus bag B (dashed line). x-axis: sure amount, y-axis: percentage of respondents.

Between treatments, the valuation of the bet on blue in the ambiguous bag is increasing in the number of observations describing the bag. This is in line with the intuition that more information renders the bet more attractive. The valuation of the bet on red does not exhibit such trend. However, Kruskal-Wallis (K-W) tests[20] suggest that, for both bets, the CE differences *between* treatments is statistically insignificant (all $p > .05$). Moreover, the dispersion of the results across treatments is never statistically significant according to a Levene's test[21] (all $p > .05$). This yields to formulate Result 2.

Result 2 *Between treatments, more information does not induce agents to value significantly more the ambiguous bet.*

This is confirmed by the analysis of the difference between the CEs for the bets on blue and the difference between the CEs for the bets on red in the risky bag and in the ambiguous bag. Table 5 provides descriptive statistics of both differences (histograms are plotted in Figures B1 and B2 in Appendix B).

		Treatment				
		1 (500)	2 (50)	3 (10)	4 (2)	5 (0)
		$N = 37$	$N = 37$	$N = 34$	$N = 36$	$N = 37$
$CE_A(blue) - CE_B(blue)$	mean	0.51	0.08	0.41	0.22	0.73
	med	0	0	0	0	0
	var	4.98	6.47	12.01	4.98	14.65
	std	2.64	2.54	3.47	2.23	3.82
$CE_A(red) - CE_B(red)$	mean	-0.03	0.24	-0.06	0.47	-0.22
	med	0	0	0	0	0
	var	4.42	5.19	7.39	3.80	10.23
	std	2.10	2.28	2.72	1.95	3.20

Table 5: CE differences - Descriptive statistics

Note: med, var and std stand for median, variance and standard deviations, respectively.

[20] Under the assumptions that the observations across groups are independent and that the scale of measurement is at least ordinal, the Kruskal-Wallis test (non-parametric) tests the null hypothesis that the medians of all groups are equal.

[21] Under the following assumptions: (1) observations across groups are independent, (2) the scale of measurement is at least ordinal and (3) the samples are approximately normally distributed, the Levene's test (parametric) tests the null hypothesis of equality of sample variances (homoscedasticity).

Since CEs lie in the interval $[5; 15]$, the differences can take values from -10 to 10. As explained previously, the experimental design allows to examine agent's attitude towards ambiguity by directly comparing the CEs in both bags because: (i) bag A contains as much blue balls as red balls, and (ii) the symmetry of draws in the datasets describing bag B should induce the symmetry of beliefs. Hence, positive differences are associated to ambiguity-averse preferences (henceforth denoted *CE-ambiguity-aversion*) and negative differences reflect ambiguity-loving preferences (*CE-ambiguity-loving*). All means and medians are not significantly different from 0 (one-sample t-tests: all $p > .05$, one-sample Wilcoxon tests: all $p > .05$). Moreover, for both colors, I cannot reject the hypothesis that all differences originate from the same distribution (K-W tests: all $p > .05$). At last, although one would have expected more dispersion among the less informative treatments, this is not supported by experimental data since the variances do not significantly differ across treatments according to Levene's tests (all $p > .05$).

3.2 Direct choices

Table 6 reports the decisions of subjects in the direct-choice tasks.

	Treatment					
	1 (500) $N = 37$	2 (50) $N = 37$	3 (10) $N = 34$	4 (2) $N = 36$	5 (0) $N = 37$	*Total* $N = 181$
Bet on blue						
Bag A (risky)	76	65	71	61	73	*69*
Bag B (ambiguous)	24	35	29	39	27	*31*
Bet on red						
Bag A (risky)	70	78	79	75	81	*77*
Bag B (ambiguous)	30	22	21	25	19	*23*
Bet on blue and on red						
Bag A (risky)	59	59	65	52	68	*61*
Bag B (ambiguous)	14	16	15	17	14	*15*

Table 6: Direct choices (in %)

To bet on a particular color, there are, on average, more than 2/3 of subjects that prefer the risky bag over the ambiguous one. When combining the two direct choices, 61% of participants exhibit Ellsberg-type behavior and prefer the risky bag for both bets (henceforth denoted *choice-ambiguity-aversion*). This is even more salient in the no-information treatment where the share of ambiguity-averse agents is maximum (68%), although the differences between treatments are not statistically significant (Kl-W test: $p > .05$). This finding is in line with the experimental evidence on Ellsberg-type problems. Oechssler and Roomets (2015) surveyed 40 Ellsberg's experiments and they report that, on average, slightly more than half of subjects are classified as ambiguity averse.

On the other hand, there is little evidence of *choice-ambiguity-loving* in my experiment since, on average, only 15% of subjects choose the ambiguous bag twice. The remaining subjects (24%) whose preferences switched from one bag to the other are classified as ambiguity-neutral. These effects are summarized in Result 3.

Result 3 *In all treatments, the majority of subjects exhibit Ellsberg-type preferences by preferring the risky bag to the ambiguous bag to bet on both colors.*

Hence, the information on the ambiguous bag does not allow to remove the Ellsberg paradox from the analysis of patterns of preferences as ambiguity-aversion is the behavior most often reported in all information conditions. Hence, I report ambiguity-averse preferences when subjects are comparing the bets (Result 3), but the valuations are not significantly different when prospects are priced in isolation (Result 2). Therefore, choice-ambiguity-aversion seems to be an unstable trait of preferences since it does not translate into CE-ambiguity-aversion. One rationale for this finding can be found in Ariely *et al.* (2003) argument that subjects' absolute valuations of goods are arbitrary and manipulable. In my experiment, DMs may have a hard time pricing gambles in isolation whereas comparative tasks might be easier to deal with. Henceforth, the Ellsberg paradox prevails especially when the DM directly expresses preferences between risky and ambiguous prospects.

3.3 Preference reversals

For each color bet, two types of preference reversals can occur: either bag A is preferred to bag B but the value attributed to the bet on bag B exceeds the value associated with the bet on bag A (choice-ambiguity-aversion and CE-ambiguity-loving), or the converse, i.e., when bag B is preferred to bag A but the value attributed to the bet on bag A exceeds the value associated with the bet on bag B (choice-ambiguity-loving and CE-ambiguity-aversion). Table 7 reports the proportions of choices that reverse preferences.

As it has been previously found in Trautmann *et al.* (2011) with WTP measurements, I report more preference reversals that result from the combination of choice-ambiguity-aversion and CE-ambiguity-loving (17%) than choice-ambiguity-loving and CE-ambiguity-aversion (7%). In particular, the results of their Experiment 4 can be compared to the control group (treatment 5): in their experiment, the agents consider one risky bag containing half winning chips and one ambiguous bag with unknown composition. The gamble consists in choosing the color on which to bet for the ball to be drawn from the bag, and the individual valuation for the bet is given by a CE measurement. They observe a majority of ambiguity-averse subjects (2/3 of subjects prefer the risky bag to bet on the color of a random draw). However, they find no credible evidence of preference inconsistencies with CE estimations (8%), whereas 28% of subjects reverse preferences in my no-information condition. Moreover, in my experiment, when more information is provided about the ambiguous bag, the frequency of preference reversals tends to decrease without disappearing, although the differences between treatments do not reach statistical significance (K-W tests: all $p > .05$). This yields to formulate Result 4.

Result 4 *In all treatments, a small but statistically significant share of subjects exhibit preference reversals with CE measurements.*

	Treatment					
	1 (500)	2 (50)	3 (10)	4 (2)	5 (0)	*Total*
	$N = 37$	$N = 37$	$N = 34$	$N = 36$	$N = 37$	$N = 181$
Choice-ambiguity-aversion and CE-ambiguity-loving						
Blue	14	8	15	17	27	*16*
Red	16	19	26	14	22	*19*
Total	*15*	*14*	*19*	*15*	*24*	*17*
Choice-ambiguity-loving and CE-ambiguity-aversion						
Blue	11	5	6	19	5	*9*
Red	5	5	3	8	3	*5*
Total	*8*	*5*	*4*	*14*	*4*	*7*

Table 7: Preference reversals (in %)

		Treatment				
		1 (500)	2 (50)	3 (10)	4 (2)	5 (0)
		$N = 37$	$N = 37$	$N = 34$	$N = 36$	$N = 37$
Min_Blue	mean	38.3	26.8	26.5	24.6	28.3
	med	40	25	25	27.5	30
	var	464.4	130.8	263.2	300.8	361.2
	std	21.6	11.4	16.2	17.3	19.0
Estim_Blue	mean	53.5	46.2	51.7	47.5	51.5
	med	50	50	50	50	50
	var	440.7	265.8	405.3	492.1	308.2
	std	21.0	16.3	20.1	22.2	17.6
Max_Blue	mean	63.4	57.7	64.5	60.5	68.1
	med	60	60	60	60	65
	var	529.4	358.2	382.1	567.3	643.2
	std	23.0	18.9	19.6	23.8	25.4

Table 8: Estimates and limit bounds of the number of blue balls in bag B - Descriptive statistics

Note: med, var and std stand for median, variance and standard deviations, respectively.

3.4 Estimates on the composition of the ambiguous bag

Descriptive statistics of the individual beliefs of the balls' proportions are reported in Table 8 (histograms are plotted in Figures B3-B5 in Appendix B). The individual estimate (*Estim_Blue*) is bounded by a lower limit (*Min_Blue*) and an upper limit (*Max_Blue*), all reported by the participant. The estimates are not significantly different from 50 (one-sample Wilcoxon tests: all $p > .05$) and not different across treatments (K-W test: $p > .05$).

On average, the mean of the lower bounds is the highest in the most informative condition (treatment 1) and exceeds the mean in the control group by 10 points (K-W test: $p > .05$). Regarding the upper bounds, the difference between the means of treatment 1 and treatment 5 is weaker and equals -4.7 points (K-W test: $p > .05$). Moreover, I measure the size of the range of beliefs (dispersion) by calculating the difference between the limit bounds: *Max_Blue* - *Min_Blue*. Descriptive statistics are reported in Table 9. As expected, the difference is the largest in the control group (treatment 5) and the weakest in the treatment 1[22]. This denotes higher confidence in the 50-50 belief regarding the composition of the bag in the most-informative condition than in the less-informative treatments although the differences between all treatments do not reach statistical significance (K-W test: $p > .05$).

	Treatment				
	1 (500) $N = 37$	2 (50) $N = 37$	3 (10) $N = 34$	4 (2) $N = 36$	5 (0) $N = 37$
mean	25.1	30.9	38	35.9	39.8
med	20	25	37.5	30	40
var	557.15	609.0	263.2	775.1	850.7
std	23.6	21.8	24.7	27.8	29.2

Table 9: Difference between *Max_Blue* and *Min_Blue*

Note: med, var and std stand for median, variance and standard deviations, respectively.

This result can be related to Nicholls *et al.* (2015) who observe statistical learning in a 3-colors urn framework. Their urn contained 60 blue, 20 red and 20 yellow balls. In the absence of information, the respondents of the control group report roughly equal proportions for all colors (36-33-31 respectively, on average), whereas answers of participants who receive statistical feedback (54-25-21) converge to the true proportions.

[22] According to (two-sample) K-W tests, the differences between dispersions are statistically significant at the 5% level for treatment 1 and treatment 5 and for treatment 1 and treatment 3, at the 10% level for treatment 1 and treatment 4, and not significant for treatment 1 and treatment 2.

In my experiment, the subjects of the control group report 50-50 probability judgment as a reflection of their ignorance. This prior is confirmed by the information and as the number of draws rises, the size of the range of beliefs decreases which suggests that subjects feel more confident about their estimation. These findings are summarized in Result 5.

Result 5 *Increasing the precision of information tends to reduce the dispersion of beliefs on the composition of the ambiguous bag (without reaching statistical significance).*

4 Conclusion

In this paper, I present an experiment designed to measure the effect of information precision on ambiguity attitudes and preference reversals. To this end, subjects are provided with sets of observations that inform on the ambiguous alternative. The informative signals differ within only one attribute: their precision (the number of observations varies across treatments), while the reported frequencies are kept constant.

Although most subjects exhibit Ellsberg-type preferences (Result 3), the valuations of the risky and ambiguous prospects do not differ significantly (Result 1), suggesting only weak ambiguity-aversion. An alternative explanation might be that DMs experience difficulties to provide absolute valuation for prospects whereas comparing alternatives is an easier task. Moreover, I did not find evidence that increasing the information precision increases the attractiveness of the ambiguous prospect (Result 2) although respondents seem to feel more confident in their estimation as the number of observations rises (Result 5). Finally, contrary to what was found by Trautmann *et al.* (2011), preference reversals are not eliminated even when using CE estimations (Result 4).

Several tests reported in this experiment do not provide statistically significant results. This has to be seen in the context of the small sample sizes of each treatment. Therefore, it may be worthwhile to conduct additional experiments with a larger pool of participants to gain further insights into the link between information precision and ambiguity attitudes. Moreover, the increase of information considered here is not valuable for frequentist DMs. Indeed, a frequentist is indifferent between any two datasets with different precision but identical frequencies. Therefore, the study of the relation between ambiguity perception and the informativeness of signals that differ within the two attributes (precision and frequencies) might exhibit different and more significant effects. This question will be addressed in Chapter 3.

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A Supplementary material for the experiment

A.1 Instructions (Treatment 1, translated from French)

Note: The sentences in brackets are not included in the instructions but inform the reader on the moment an action is performed.

— Start of Instructions —

Hello everybody, and thank you for accepting our invitation to participate in this experiment. My name is Roxane Bricet and I am working at the Economics lab of the University of Cergy-Pontoise. I am going to briefly present the outline of the experiment, please pay close attention to this short presentation.

First, some important rules which must be respected: It is important that the experiment takes place in silence, you are not allowed to communicate with other participants. The use of mobile phones and calculators is forbidden. No particular preliminary knowledge is needed to participate. As soon as the experiment starts, if you have any questions, please raise your hand and I will assist you.
Thank you in advance for your cooperation.

What do we do?
You are going to participate in an economic experiment on decision theory.
In this experiment, there are two opaque bags, A and B, each containing 100 balls. These balls can be either blue or red.

The composition of bag A is perfectly known. It contains exactly 50 blue balls and 50 red balls.

The composition of bag B is not known. However, partial information on the content of this bag is provided. Indeed, we have randomly drawn one ball from bag B and reported its color, then we have replaced it in bag B. This operation has been replicated 500 times and we have observed: 250 blue balls and 250 red balls. [This phrase was adjusted to reflect the information in the specific treatment (2, 3 and 4). In treatment 5, the last sentences are replaced by: "The composition of bag B is unknown.".]

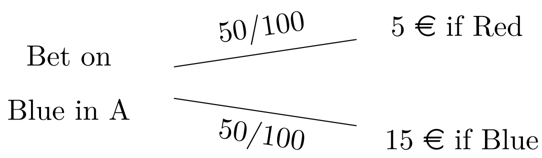
Note that nothing indicate that bag B contains as many blue balls as red balls!

The gambles:

During the experiment, you will be asked to consider gambles on bags A and B. Here is an example of gamble on bag A:

You are going to proceed to draw a ball at random from bag A. If the color of the ball is blue, you win 15€, if the ball is red you win 5€.

The composition of bag A being known, these gambles are presented in the following way:

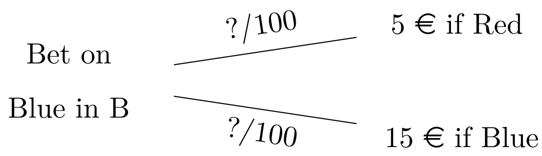


In other words, since 50 of 100 balls contained in bag A are blue, you have a chance of 50 out of 100 to win 15 € and a chance of 50 out of 100 to win 5 €.

Here is an example of a gamble on bag B:

You are going to proceed to draw a ball at random from bag B. If the color of the ball is blue, you win 15€, if the ball is red you win 5€.

The composition of bag B being unknown, these gambles are presented in the following way:

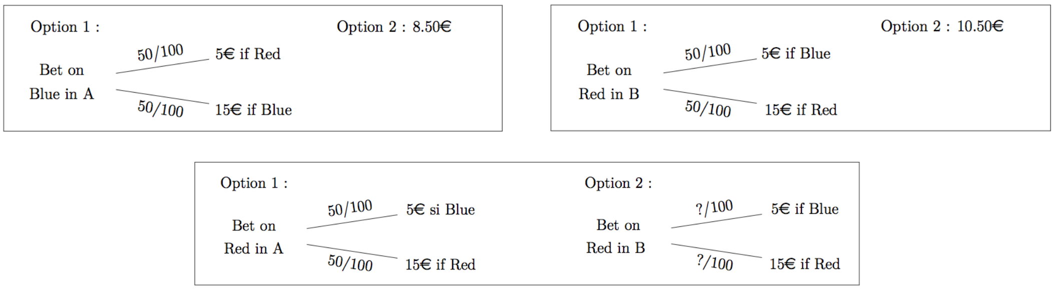


The envelopes:

Before the beginning of the experiment, each of you will receive a **closed** envelope. Each

envelope contains two options. These options differ across the envelopes. Your goal is to tell us which of the two options you prefer for each envelope in the experiment. At the end of the experiment, you will be rewarded depending on the content of your own envelope and your choices of options.
Important: There are no "right" or "wrong" answers in this experiment, it is only a matter of preferences.

Here are some examples of envelope content:

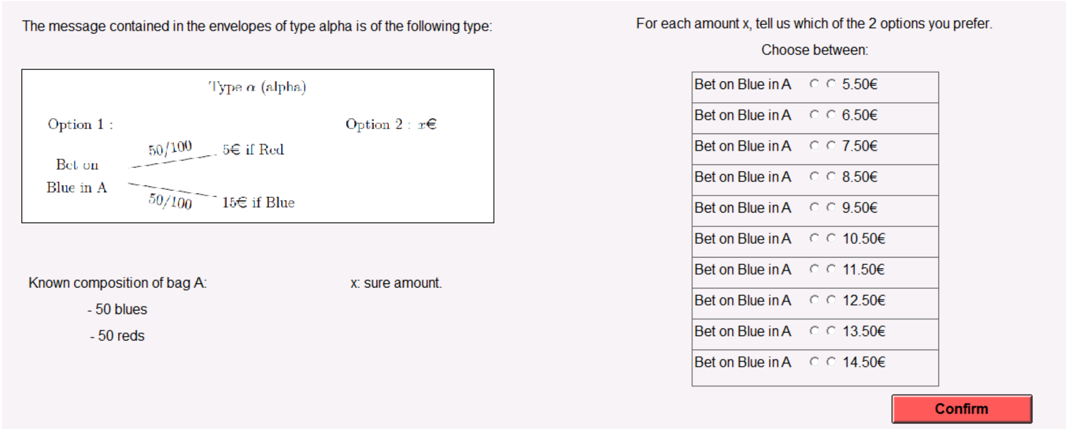


In concrete terms, there are 6 types of envelopes: type α (alpha), type β (beta), type γ (gamma), type δ, type ϵ and type ϕ (phi), corresponding to 6 different questions.

The options contained in the different types of envelopes are given in the following table:

envelopes	option 1	option 2
α	bet on Blue in A	x€
β	bet on Red in A	x€
γ	bet on Blue in B	x€
δ	bet on Red in B	x€
ϕ	bet on Blue in A	bet on Blue in B
ϵ	bet on Red in A	bet on Red in B

During the experiment, the contents of the envelopes will be presented to you as follows:



This is the message contained in the envelopes of type alpha. Option 1 corresponds to the bet on the draw of a blue ball from bag A. If you draw a blue ball, you win 15€, if you draw a red ball, you win 5€. Option 2 corresponds to a sure amount x in euros.

For each amount x, tell us which of the 2 options you prefer. To this end, fill out the following table. In this table, each line describes the content of an envelope.

The filling of this table is automated. When you tick a box of a line of the table, option 1 is automatically ticked for the lines above and option 2 is automatically ticked for the lines below.

Thus, it is enough for you to tick a single box of the table to fill out every lines of the table.

Indeed, if for the first line of the table you choose option 1 "Bet on Blue in A", tick option 1 on the first line of the table. Then, option 2 will be automatically ticked for the rest of the decisions, that is to say for the lines 2 to 10. If for the first line of the table, you choose option 2 "5.50€", tick option 2 on the first line of the table. Then, option 2 will be automatically ticked for the rest of the decisions, that is to say for the lines 2 to 10. [Each pattern of answers described in the instructions is illustrated with an example of a filled table.]

You may also choose option 1 for the second line of the table. In this case, tick option 1 on the second line of the table. Option 1 will be automatically ticked for the line 1 of the table, and option 2 will be automatically ticked for the lines 3 to 10 of the table.

Otherwise, you may choose option 2 for the second line of the table. In this case, tick option 2 on the second line of the table. Option 1 will be automatically ticked for the line 1 of the table, and the option 2 will be automatically ticked for the lines 3 to 10 of the table.

Again, you may choose option 1 for the third line of the table. In this case, tick option 1 on the third line of the table. Option 1 will be automatically ticked for the lines 1 and 2 of the table, and option 2 will be automatically ticked for the lines 4 to 10 of the table. Otherwise, you may choose option 2 for the third line of the table. In this case, tick option 2 on the third line of the table. Option 1 will be automatically ticked for the lines 1 and 2 of the table, and option 2 will be automatically ticked for the lines 4 to 10 of the table.

And so on until the end of the table. You can choose option 1 for the tenth line of the table. In this case, tick option 1 on the tenth line of the table. Option 1 will be automatically ticked for the rest of the decisions, that is to say for the lines 1 to 9.

Note that you can revise your choice as many times as needed before confirming your choice and proceeding to the following question.

There are 60 envelopes, 10 of each type.

These envelopes are numbered from 1 to 60. Three of you are now asked to check their numbering.

With this established, I am going to walk among you and each of you will randomly draw one of the envelopes.

DO NOT OPEN YOUR ENVELOPE!

Anyone who opens his envelope will be immediately excluded from the experiment and will not receive any financial reward.

YOUR PAYMENT:

In this experiment, the minimum gain is 5 euros and the maximum gain is 15 euros.

Reminder: There are no "right" or "wrong" answers in this experiment, it is only a matter of preferences.

Plan of the experiment:

In practice, you will answer the questions of the experiment using the computer in front of you.

First, you will be asked some comprehension questions to check your understanding of the instructions. *Your answers to these questions do not affect your payment.*

Then, the experiment on your choices of options will start. The experiment consists of 6 successive screens for the 6 types of envelopes (6 types of different questions).

Finally, you will be asked to fill out a short complementary questionnaire to get to know you better. *Your answers to these questions do not affect your payment.*

The last screen will invite you to reach the experimenter's office to proceed to the payment.

In the office of the experimenter:

I will open the envelope in front of you and I will just implement what you chose during the session. If the option chosen during the session is a bet: you will proceed to draw a ball at random from the bag involved in the bet. If the drawn ball matches the winning color, you win 15€; otherwise, you only win 5€. If the option is a sure amount x: you get the x€ gain.

Because you do not know the content of your envelope and because I will implement your choices, it is in your best interest to tell us your preferred option at each question. Indeed, if you tell us what you want, your preferred option will be the one implemented!

Verification:

At the end of the experiment, you will get a list describing the contents of all envelopes. This list describes the content of each envelope: number of the envelope, type of the envelope, option 1, option 2.

You will then check that our description of the numbered envelopes was truthful.

You will also be able to check that the list does contain the 6 mentioned tasks.

Your answers will be kept strictly confidential and anonymous, henceforth, feel free to answer as you like. Moreover, there is no time limit, so take all the time you need to read the instructions and answer the questions.

Do you have any questions?
If everything is clear, you can now start the experiment!

— End of Instructions —

A.2 Detailed script

0. *Preliminaries:* The 60 envelopes are grouped in 20-tuples (1-20; 21-40; 41-60). The envelopes and the opaque bags A and B are placed on the table in the front of the room.

1. The participants are welcome at the doorstep of the lab. Before entering the room, they randomly pick a number that determines where they sit in the room. They are also asked to sign a participation consent before the start of the session.

2. The experimenter gives the instructions in the form of an oral presentation with slides. At the relevant points during the explanations:
   - The bags A and B are showed.
   - Each 20-tuple of envelopes is checked for the presence of every number by a volunteering subject, and then thrown into an opaque bag. Each participant then randomly picks one envelope from the bag.

3. *Computerized:* The participants are asked to answer a short comprehension test to check their understanding of the experiment. No one can reach the next stage before everybody has answered correctly.

4. *Computerized:* The participants perform the 6 tasks of the experiment.

5. *Computerized:* Stage 5 consists of filling out a short complementary questionnaire. This form consists of:
    - individual estimations of the composition of bag B;
    - socio-demographic variables: sex, age, level of education.
6. When they are done, the participants reach the office of the experimenter. The payment phase proceeds individually:
    - 6.1 The experimenter opens the envelope of the subject and reads aloud the note contained inside.
    - 6.2 The experimenter recalls the answer of the participant via the computerized experimental data collection.
    - 6.3 The option chosen by the participant is implemented: if it is a sure amount, the participant gets the sure gain; if it is a bet in a bag, the participant proceeds to draw a ball at random from the bag in question.
    - 6.4 The participant receives the corresponding payment and signs a receipt.
    - 6.5 The participant gets the verification lists describing all envelopes in order to check that the envelopes were truthfully described.
7. The participants leave the room.

A.3 Verification lists (Treatment 1, session 1)

List of envelopes by type

Envelope nr.	Type	Option 1	Option 2
45	Type α (alpha)	Bet on Blue in A	5.50 €
57	Type α (alpha)	Bet on Blue in A	6.50 €
24	Type α (alpha)	Bet on Blue in A	7.50 €
1	Type α (alpha)	Bet on Blue in A	8.50 €
28	Type α (alpha)	Bet on Blue in A	9.50 €
16	Type α (alpha)	Bet on Blue in A	10.50 €
32	Type α (alpha)	Bet on Blue in A	11.50 €
41	Type α (alpha)	Bet on Blue in A	12.50 €
27	Type α (alpha)	Bet on Blue in A	13.50 €
25	Type α (alpha)	Bet on Blue in A	14.50 €
58	Type β (bêta)	Bet on Red in A	5.50 €
59	Type β (bêta)	Bet on Red in A	6.50 €
26	Type β (bêta)	Bet on Red in A	7.50 €
29	Type β (bêta)	Bet on Red in A	8.50 €
17	Type β (bêta)	Bet on Red in A	9.50 €
18	Type β (bêta)	Bet on Red in A	10.50 €
13	Type β (bêta)	Bet on Red in A	11.50 €
30	Type β (bêta)	Bet on Red in A	12.50 €
39	Type β (bêta)	Bet on Red in A	13.50 €
14	Type β (bêta)	Bet on Red in A	14.50 €
3	Type γ (gamma)	Bet on Blue in B	5.50 €
11	Type γ (gamma)	Bet on Blue in B	6.50 €
20	Type γ (gamma)	Bet on Blue in B	7.50 €
5	Type γ (gamma)	Bet on Blue in B	8.50 €
42	Type γ (gamma)	Bet on Blue in B	9.50 €
36	Type γ (gamma)	Bet on Blue in B	10.50 €
22	Type γ (gamma)	Bet on Blue in B	11.50 €
7	Type γ (gamma)	Bet on Blue in B	12.50 €
40	Type γ (gamma)	Bet on Blue in B	13.50 €
44	Type γ (gamma)	Bet on Blue in B	14.50 €
49	Type δ (delta)	Bet on Red in B	5.50 €
50	Type δ (delta)	Bet on Red in B	6.50 €
4	Type δ (delta)	Bet on Red in B	7.50 €
31	Type δ (delta)	Bet on Red in B	8.50 €
46	Type δ (delta)	Bet on Red in B	9.50 €
55	Type δ (delta)	Bet on Red in B	10.50 €
34	Type δ (delta)	Bet on Red in B	11.50 €
23	Type δ (delta)	Bet on Red in B	12.50 €
8	Type δ (delta)	Bet on Red in B	13.50 €
21	Type δ (delta)	Bet on Red in B	14.50 €
38	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
54	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
12	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
43	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
47	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
6	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
35	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
60	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
53	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
2	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
10	Type θ (theta)	Bet on Red in A	Bet on Red in B
52	Type θ (theta)	Bet on Red in A	Bet on Red in B
56	Type θ (theta)	Bet on Red in A	Bet on Red in B
48	Type θ (theta)	Bet on Red in A	Bet on Red in B
19	Type θ (theta)	Bet on Red in A	Bet on Red in B
51	Type θ (theta)	Bet on Red in A	Bet on Red in B
15	Type θ (theta)	Bet on Red in A	Bet on Red in B
33	Type θ (theta)	Bet on Red in A	Bet on Red in B
9	Type θ (theta)	Bet on Red in A	Bet on Red in B
37	Type θ (theta)	Bet on Red in A	Bet on Red in B

List of envelopes by number

Envelope nr.	Type	Option 1	Option 2
1	Type α (alpha)	Bet on Blue in A	8.50 €
2	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
3	Type γ (gamma)	Bet on Blue in B	5.50 €
4	Type δ (delta)	Bet on Red in B	7.50 €
5	Type γ (gamma)	Bet on Blue in B	8.50 €
6	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
7	Type γ (gamma)	Bet on Blue in B	12.50 €
8	Type δ (delta)	Bet on Red in B	13.50 €
9	Type θ (theta)	Bet on Red in A	Bet on Red in B
10	Type θ (theta)	Bet on Red in A	Bet on Red in B
11	Type γ (gamma)	Bet on Blue in B	6.50 €
12	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
13	Type β (bêta)	Bet on Red in A	11.50 €
14	Type β (bêta)	Bet on Red in A	14.50 €
15	Type θ (theta)	Bet on Red in A	Bet on Red in B
16	Type α (alpha)	Bet on Blue in A	10.50 €
17	Type β (bêta)	Bet on Red in A	9.50 €
18	Type β (bêta)	Bet on Red in A	10.50 €
19	Type θ (theta)	Bet on Red in A	Bet on Red in B
20	Type γ (gamma)	Bet on Blue in B	7.50 €
21	Type δ (delta)	Bet on Red in B	14.50 €
22	Type γ (gamma)	Bet on Blue in B	11.50 €
23	Type δ (delta)	Bet on Red in B	12.50 €
24	Type α (alpha)	Bet on Blue in A	7.50 €
25	Type α (alpha)	Bet on Blue in A	14.50 €
26	Type β (bêta)	Bet on Red in A	7.50 €
27	Type α (alpha)	Bet on Blue in A	13.50 €
28	Type α (alpha)	Bet on Blue in A	9.50 €
29	Type β (bêta)	Bet on Red in A	8.50 €
30	Type β (bêta)	Bet on Red in A	12.50 €
31	Type δ (delta)	Bet on Red in B	8.50 €
32	Type α (alpha)	Bet on Blue in A	11.50 €
33	Type θ (theta)	Bet on Red in A	Bet on Red in B
34	Type δ (delta)	Bet on Red in B	11.50 €
35	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
36	Type γ (gamma)	Bet on Blue in B	10.50 €
37	Type θ (theta)	Bet on Red in A	Bet on Red in B
38	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
39	Type β (bêta)	Bet on Red in A	13.50 €
40	Type γ (gamma)	Bet on Blue in B	13.50 €
41	Type α (alpha)	Bet on Blue in A	12.50 €
42	Type γ (gamma)	Bet on Blue in B	9.50 €
43	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
44	Type γ (gamma)	Bet on Blue in B	14.50 €
45	Type α (alpha)	Bet on Blue in A	5.50 €
46	Type δ (delta)	Bet on Red in B	9.50 €
47	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
48	Type θ (theta)	Bet on Red in A	Bet on Red in B
49	Type δ (delta)	Bet on Red in B	5.50 €
50	Type δ (delta)	Bet on Red in B	6.50 €
51	Type θ (theta)	Bet on Red in A	Bet on Red in B
52	Type θ (theta)	Bet on Red in A	Bet on Red in B
53	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
54	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B
55	Type δ (delta)	Bet on Red in B	10.50 €
56	Type θ (theta)	Bet on Red in A	Bet on Red in B
57	Type α (alpha)	Bet on Blue in A	6.50 €
58	Type β (bêta)	Bet on Red in A	5.50 €
59	Type β (bêta)	Bet on Red in A	6.50 €
60	Type ϵ (epsilon)	Bet on Blue in A	Bet on Blue in B

A.4 Participation consent form

PARIS SCHOOL OF ECONOMICS
ECOLE D'ECONOMIE DE PARIS

CONSENTEMENT A PARTICIPER A UNE SESSION EXPERIMENTALE EN ECONOMIE

Nom, Prénom : ....

Adresse : ....

Adresse Email * : ....

(dénommé par la suite « le participant »)

Le participant consent librement à prendre part à la session expérimentale suivante :
Dossier N°2016-2017 n° 18

Date de la session expérimentale : 20/07/2017..
Durée maximum (en heures) : 1h..

Le montant de la compensation reçue à l'issue de la session expérimentale par le participant dépend des résultats obtenus durant cette expérience. Ce montant est au minimum de 5 € (Euros) à condition de respecter les règles de l'expérience (notamment ne pas ouvrir l'enveloppe fermée reçue au début de la session).
Les données issues de cette expérience seront traitées de façon anonyme

Fait à Paris, le 20/07/2017

Signature du participant contractant

*** Votre adresse Email et adresse postale <u>ne seront pas</u> communiquée. Elles ont pour but de nous permettre de vous contacter en cas de contrôle afin que vous nous confirmiez le montant reçu au titre de l'expérience.**

B Complementary tables and figures

Treatment	Female	Age	Education
1	54%	26	3 years
2	65%	27	3 years
3	53%	25	2 years
4	72%	31.5	3 years
5	59%	30	3 years

Table B1: Socio-demographic variables

Note: *Age* and *Education* correspond to median values. *Education* stands for the number of completed years in higher education.

Treatment	Bag	Draws			Bag composition	
		Total	Blue	Red	Blue	Red
1	B1	500	250	250	47	53
2	B2	50	25	25	43	57
3	B3	10	5	5	38	62
4	B4	2	1	1	10	90
5	B5	0	0	0	90	10

Table B2: Bags composition resulting from MATLAB programming

Lecture: Participants of treatment 1 are informed that 500 balls have been randomly drawn with replacement from bag B1, 250 were blue and 250 were red (public information). The bag B1 contains actually 47 blue balls and 53 red balls (non-public information).

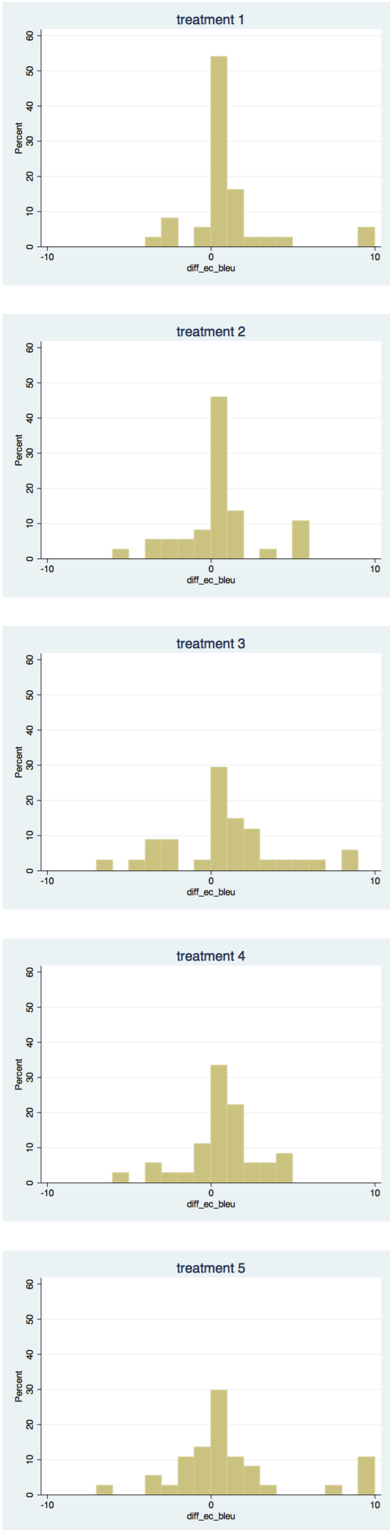


Figure B1: Histograms of the differences between CEs on blue ($CE_A(blue) - CE_B(blue)$), by treatment

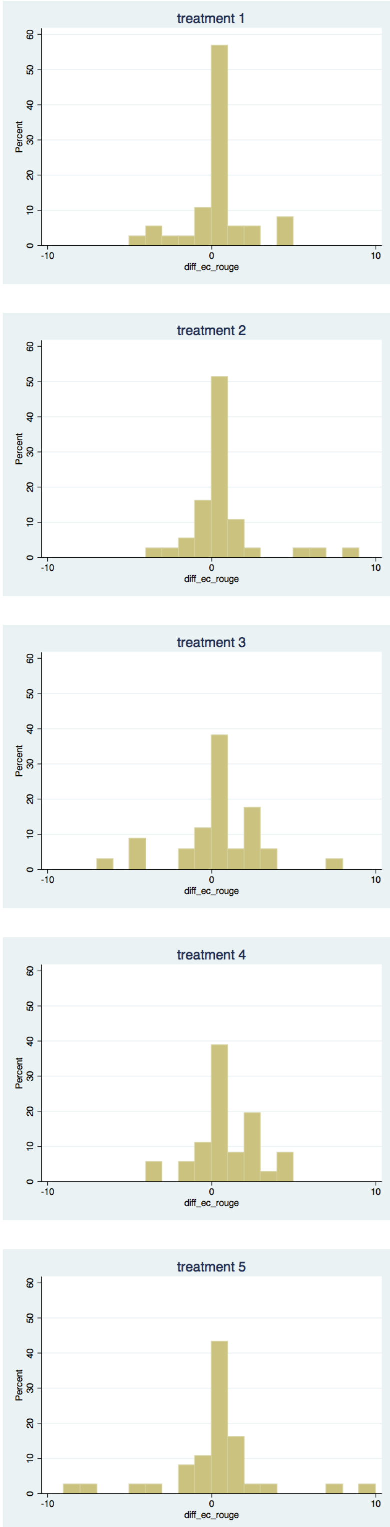


Figure B2: Histograms of the differences between CEs on red ($CE_A(red) - CE_B(red)$), by treatment

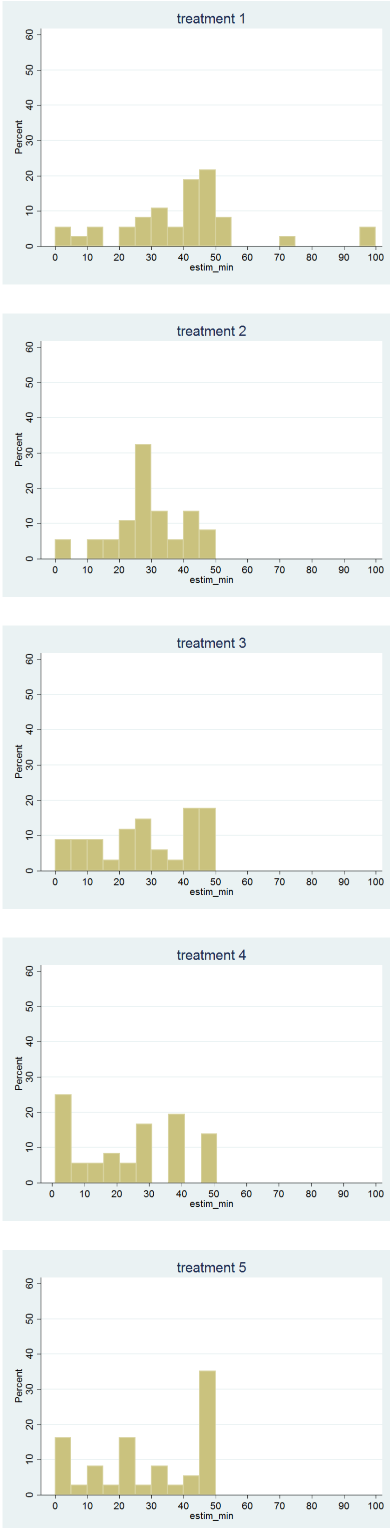


Figure B3: Histograms of the estimates on the proportion of blue balls in the ambiguous bag, by treatment

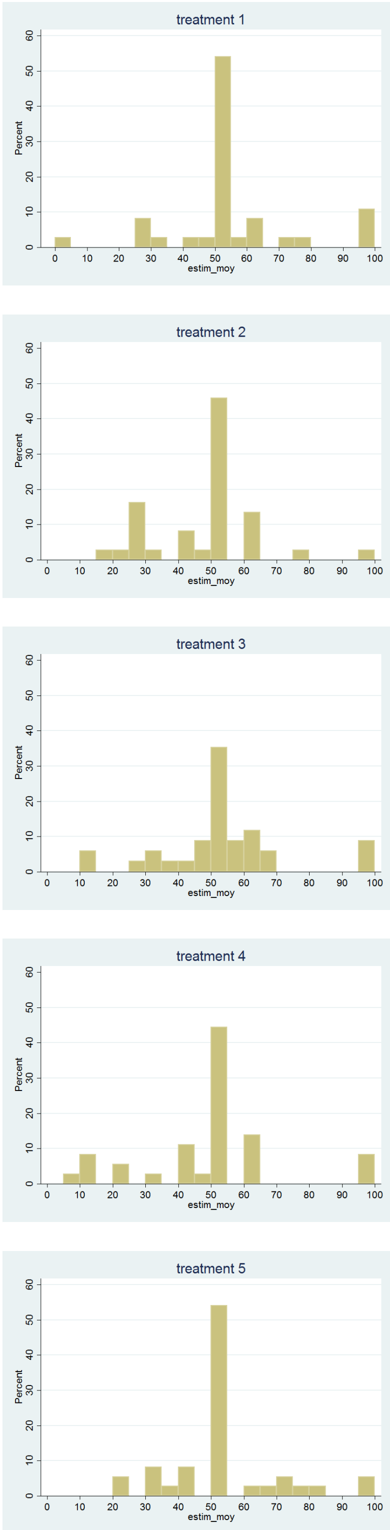


Figure B4: Histograms of the estimates on the proportion of blue balls in the ambiguous bag, by treatment

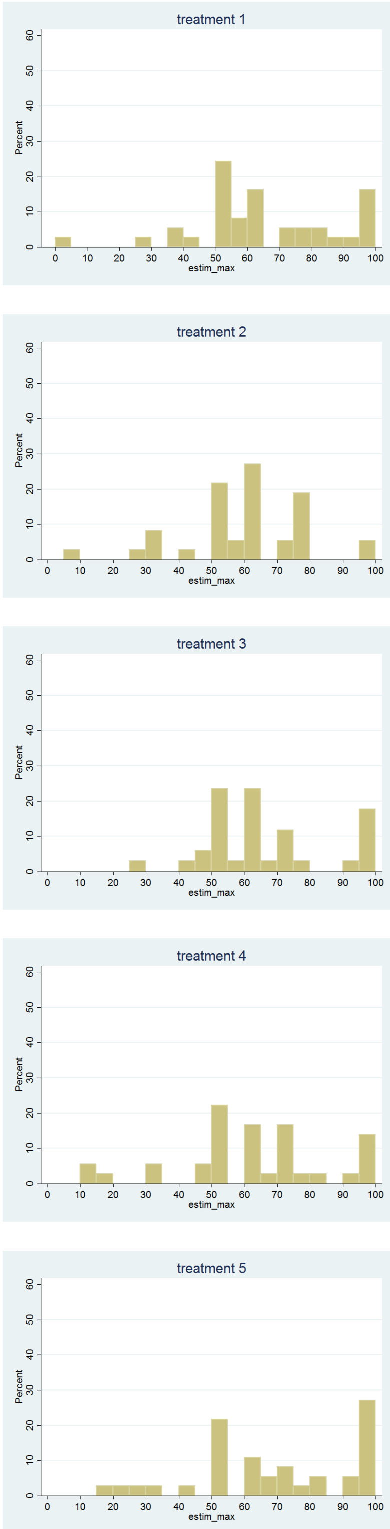


Figure B5: Histograms of the estimates on the proportion of blue balls in the ambiguous bag, by treatment